CHARACTERIZING THE ROBUSTNESS OF SCIENCE

After the Practice Turn in Philosophy of Science

Edited by

LÉNA SOLER

Archives H. Poincaré, Laboratoire d'Histoire des Sciences et de Philosophie, Nancy, France

EMILIANO TRIZIO

Archives H. Poincaré, Laboratoire d'Histoire des Sciences et de Philosophie, Nancy, France

THOMAS NICKLES

University of Nevada, Reno, NV, USA

and

WILLIAM C. WIMSATT

University of Chicago, Chicago, IL, USA

Springer

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Chapter 1
Introduction: The Solidity of Scientific Achievements: Structure of the Problem, Difficulties, Philosophical Implications

Léna Soler

The disciplines whose scientific status is not in question, such as physics, are characterized by what is commonly described as the 'reliability' and 'successfulness' of their theoretical, experimental or technical accomplishments. Today, philosophers of science often talk of "robustness".

At first sight, robustness seems to be an intuitively clear concept. Yet, it is far from easy to give a precise account of just what is implied by this notion. Major fluctuations and vagueness in the use of robustness terminology only accentuates this substantial difficulty. As stressed by one of the contributors to this book, Thomas Nickles: "In recent years the term 'robustness', its cognates and neighbors (solidity, persistence, hardiness, reliability, resilience, viability, flexibility, healthiness, etc.) have been applied to just about everything. In fact, 'robust' has become a buzzword in popular culture that can be applied to anything that exhibits strength of some sort." Indeed, the word is commonly applied, in addition to achievements in the scientific field (scientific propositions, scientific theories, experimental devices, experimental results and the like), also to complex technological systems beyond the scientific field (aircraft, electrical grids, nuclear plants, the Internet and the like), as well as to biological organisms and to social networks. So there is a need for both substantive investigation and terminological clarification.

At the most general level, central questions await further exploration: What exactly lies at the basis of the robustness of the various sciences? How is robustness historically generated and improved upon? What does it mean that a scientific result or a developmental stage of science is 'more robust' than another? Behind such questions lurk crucial epistemological issues: indeed, nothing less than the very nature of science and its specificity with respect to other human practices, the nature of rationality and of scientific progress, and (last but not least) science's claim to be a truth-conducive activity.

L. Soler (✉)
Archives H. Poincaré, Laboratoire d'Histoire des Sciences et de Philosophie,
UMR 7117 CNRS, Nancy, France
e-mail: l_soler@club-internet.fr

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In relation to these questions, William Wimsatt has been a pioneer, and his writings constitute a fundamental reference point. The first chapter of the present volume reprints his seminal 1981 paper, “Robustness, Reliability, and Overdetermination”, which has been the point of departure for work on robustness since that time.[1] In a new chapter that appears in this volume for the first time, Wimsatt provides a personal account of the peculiar situation in evolutionary biology that he experienced as a young professor in the 1970s, which sparked his lifelong concern with robustness and which had already motivated authors such as Richard Levins and Donald Campbell to introduce notions related to robustness.

In the 1981 article reprinted in the present book, Wimsatt developed a systematic general analysis inspired by these attempts and similar ones, that he called “robustness analysis”. Through this article, he introduced into philosophy of science the informal robustness vocabulary already widely appearing in scientists’ own talk and proceeded to develop a more specific and technical one that, while preserving the common association with the ideas of reliability and successfulness, allows a more precise characterization. Robustness is defined as the use of “multiple means of determinations” to “triangulate” the existence and the properties of a phenomenon, of an object or of a result. The fundamental idea is that any object (a perceptual object, a physical phenomenon, an experimental result, etc.) that is sufficiently invariant under several independent derivations (in a wide sense of the term ‘derivation’, including means of identification, sensorial modalities, measurements processes, tests, models, levels of description, etc.) owes its strength (i.e. its robustness) to this situation.

Historically, the question of the reliability of science was first formulated, within philosophy of science, as a problem concerning the relations between statements. The so-called “practice turn” of Science Studies that began in the 1980s has produced a shift in focus. The practice turn has led to an enlarged characterization of science that includes several aspects previously ignored or underplayed on the grounds of their alleged epistemological irrelevance (tacit knowledge, local norms and standards, instrumental resources and the like). Since the practice turn showed that these aspects are often not anecdotal, the investigation of the issue of robustness, and the resulting characterization, must take them into account. This is what the reference to “the practice turn” in the sub-title of this book is intended to mean.

To open this volume on robustness in science I would like, starting from William Wimsatt’s writings on robustness, to propose a general analysis of what will be called ‘the problem of the solidity of scientific achievements’. (The reason why the term ‘solidity’ replaces the more common term ‘robustness’ will appear in the next section.) First the problem and its structure, as well as terminological clarifications and suggestions, are introduced. Then several difficulties that any robustness analysis inspired by Wimsatt’s framework will have to face, are identified and characterized. The philosophical issues related to the solidity problem are also clarified.

[1] Thanks to William Wimsatt for offering to re-print this fundamental chapter in the present book. It is all the more important because this chapter is a pivotal source of inspiration for most of the contributions of this book, and is very often quoted or exploited. So the reader will have the original chapter at hand.

Along the way, I shall indicate which chapter of this book contributes to the investigation of these difficulties and philosophical issues. Finally, a more systematic and sequential overview of the different chapters of this book will be provided.

1.1 Robustness. . . That Is to Say?

Let me begin with some terminological remarks, which, clearly, are not just a matter of words.

As already suggested above, the term 'robustness', central to the volume title, is, today, very often employed within philosophy of science in an intuitive, non-technical and flexible sense that, globally, acts as a synonym of 'reliable', 'stable', 'effective', 'well established', 'credible', 'trustworthy', or even 'true'. Correlatively, the more precise and technical sense developed by Wimsatt refers to the idea of the invariance of a result under multiple independent determinations. However, as Wimsatt himself explicitly admits, the scheme of invariance under independent multi-determinations does not exhaust the ways in which an element of scientific practices can acquire the status of a 'robust' ingredient in the broad sense of the term.

In order to avoid confusion between the intuitive sense and Wimsatt's technical sense, and to make room for other possible schemes of constitution of reliability, it would be desirable to have at hand an appropriate specific standard terminology shared by the members of the Science Studies community. To take a step in this direction, I suggest the following terminological options.

I will restrict the term 'robustness' to the specific sense introduced by Wimsatt, that is:

> X is robust $=$ X remains invariant under a multiplicity of (at least partially) independent derivations.

In order to capture the broader, more general and largely indeterminate sense found in common use, I will employ the term 'solidity'.

On the basis of these decisions, robustness appears as a particular case of solidity[2]:

> X is robust $=$ X is solid, *for* X remains invariant under partially independent multiple determinations.

[2] Several authors of this book have adopted my terminological proposal, but of course, alternative terminological options are possible, as soon as they are clearly specified. For example Mieke Boon, in her paper, chooses to retain the expression "robustness notions" to encompass all the various uses connected to the intuitive idea of robustness. Then she decomposes this umbrella expression into different but related features (such as reality, reproducibility, stability etc.) that can be attributed to specified kinds of things (independent reality, physical occurrences, observable and theoretical objects, etc.), features that may differ in status (metaphysical, ontological, epistemological, etc.). In such a framework robustness is a generic notion, and we then have different species of robustness (robustness in the sense of reliability, robustness in the sense of multiple determinations, etc.).

However, other schemes could be involved in the process of constituting solidity. For instance, Wimsatt has also insisted on another scheme, different from that of robustness, that he has called "generative entrenchment"[3] (hereafter, GE):

> X is GEed $=$ X is solid, *for* X is involved in an essential way (X plays a quasi-foundational role) in the generation of a huge number of ingredients constituting scientific practices.

I propose the term of 'solidity' for two reasons. First, it is not already associated, as far as I know, with any specific, technical sense in Science Studies. Second, its meaning in ordinary usages is very broad: the term is currently applied to very heterogeneous kinds of things, either material or intellectual. This second characteristics is, as we will see (Section 1.7.3), a desirable feature if we want to be in a position to think of the robustness of science in relation to scientific *practices*. Indeed, many heterogeneous kinds of elements are involved in these practices and can be implicated, in one way or another, in the generation of something that we are intuitively inclined to call 'robust' or 'solid'. On the basis of the terminology just proposed, the title of this volume should be changed to: *Characterizing the* Solidity *of Science After the Practice Turn in Philosophy of Science*. The editors nevertheless choose to retain the initial title, since 'robustness' is a more familiar descriptive term for people who work in Science Studies.

No doubt there exist other schemes of the acquisition of solidity besides robustness and generative entrenchment that await characterisation. Several chapters of this book will help to take a step in this direction: consult those of Mieke Boon, Hubertus Nederbragt, Thomas Nickles, Andrew Pickering, Léna Soler, Emiliano Trizio and Frédéric Wieber. They will either point to schemes that can be seen as variations of the Wimsattian scheme: as refined or more complex versions of this scheme (see especially in this respect the more complex structural schemes designed by Trizio in Chapter 4, Fig. 4.4). Or they will exhibit alternative processes through which the status of 'solid' can come to be attributed to an element of scientific practices through time. In particular, in my contribution, I propose a complex architecture as a schematic reconstruction of the historical situation under scrutiny (see Chapter 10, Fig. 10.9). On the basis of this reconstruction, taken as having a general value beyond the particular case under study, I suggest regarding the Wimsattian scheme as a simple unit of analysis, an "elementary scheme" involved as a building block in more complex argumentative structures.

To see it as a building block could be one reason why, even if alternative schemes to the robustness one are involved in scientific practices, the robustness scheme is nevertheless especially fundamental. Other reasons can be mentioned. Trizio's article points to another very important one: even when the robustness scheme is not instantiated in a given episode of the history of science, it still works for practitioners as a regulative ideal or, as Trizio nicely expresses it, as a "methodological

[3] See Wimsatt (2007a), especially Chapter 8.

attractor". Indeed, the robustness scheme seems to incarnate the prototypical way through which practitioners of the empirical sciences think they can secure, if not justify, their experimental propositions, theories, models etc. We find incredibly numerous examples, in very different contexts of scientific practice, where scientists explicitly look for something akin to a robustness scheme and explicitly ask for such a scheme when it is not realized. As illustrated in Trizio's case study, reviewers of scientific papers often suspend publication of a submission until a supplementary, convergent derivation is obtained. Another common example occurs when scientists are convinced of a result *X*, and think their colleagues will be convinced too, because they think *X* is indeed involved in a robustness scheme (which also explains why scientific papers so often manifest robustness schemes, as Allamel-Raffin and Gangloff argue and illustrate in Chapter 7[4]). So, without doubt, the robustness scheme plays an effective and important role in scientific practices. Critics cannot reproach it for being an invention of the philosopher of science. In Stegenga's words, it is "an exceptionally important notion", "ubiquitous in science", and a "(trivially) important methodological strategy which scientists frequently use".

As a consequence, reflexive studies interested in scientific practices should analyze this role closely. Now, some of the existing studies seem to have followed what scientists themselves say uncritically and without philosophical arguments in their high valuation of robustness. Correlatively, some philosophers of science have invested very strong hope in robustness. In his chapter, Stegenga lists the many (themselves highly valued) epistemic tasks that robustness has been assumed to be able to achieve: to demarcate experimental artifacts from real phenomena and objective entities, to stop the experimenter's regress, to secure appropriate data selection, to help to recognize the best hypotheses (the most explanatory, empirically adequate, objective, otherwise promising), to provide a strong argument in favor of scientific realism, and more.

Given these hopes and the apparently pervasive involvement of the strategy of robustness in scientific practices, it is surprising that so few systematic historical, philosophical or sociological studies have been devoted to the topic: robustness remains poorly understood. The present book is intended to constitute a set of resources in order to improve this understanding. The contributors hope that their efforts will stimulate new investigations from others.

In the remaining part of this introduction, I will offer some systematic reflections that, although primarily directed toward robustness, are intended to be relevant to solidity more generally. So I see these reflections – which owe a great deal to the writings of two participants of this book, William Wimsatt and Andrew Pickering – as a contribution to the problem of solidity in science.

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[4] The episode of the 'discovery of the weak neutral currents' on which I rely in my paper would also be a good example. See notably the quotations of scientists given in Schindler (201X).

1.2 Solidity, a Relational Status: Between Holism and Modularity

Solidity is a relational attribute: to give an account of the solidity of *X* amounts, inevitably, to invoke a multitude of elements *other than X*.

For example:

- To give an account of the *robustness* of *X* = to invoke a *multitude* of partially independent derivations leading to *X*.
  Schematically and visually, the representation that first comes to mind is an arrows-node scheme, in which a series of upward arrows representing the derivations *converge* on the node *X* (see Fig. 1.1).

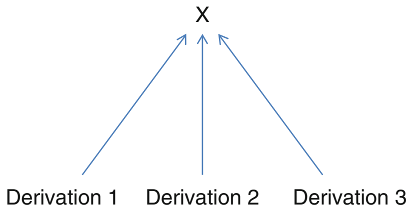

Fig. 1.1 The robustness scheme

- To give an account of the *GE* of *X* = to invoke a *multitude* of practices in which *X* plays a quasi-foundational role (that is, for which *X* is required at a given historical moment, for which it appears impossible or very difficult to proceed without *X*).
  In terms of the arrows-node scheme, one is inclined to translate the situation by drawing a series of divergent upward arrows starting from the node *X* (see Fig. 1.2).

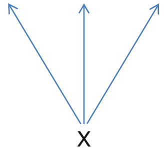

Fig. 1.2 The generative entrenchement scheme

Thus, solidity is not an intrinsic attribute of the *X* that is declared solid. The problem of solidity attributions has an irreducibly relational, holistic structure.

At the same time, we are not inclined to say that everything depends on everything else in science. Intuitively, we feel entitled to decompose the situation in terms of relatively autonomous modules, at least with respect to certain aims; and I think we must do justice to this intuition. The tension between holism and modularity is a crucial and difficult aspect of the problem of solidity in science. It will be encountered again below (Sections 1.6 and 1.7).

1.3 Counting and Weighing the Arrows of a Solidity Scheme

At first glance one expects that, all other things being equal, the solidity of a given node increases with the number of the associated arrows.

– With the number of arrows leading to the node, in the case of a robustness scheme;
– With the number of arrows starting from the node in the case of GE.

So analysts of a given historical scientific episode (among whom are scientists) must count the derivations: the higher the number of (independent) derivations, the more solid will be the *X* which lies at their intersection. But that is not all. Indeed, the different arrows are not necessarily on the same level. They do not always have the same strength. So the analyst must also weigh the derivations.

Let us begin with this second requirement and think about the robustness of an experimental fact in physics or in biology, for example – a kind of example that will occupy an important place in the contributions of this volume.

- While examining the various experiments that, through history, have contributed to the establishment of this fact as a fact, we are often led to introduce a hierarchy among the different experiments and to deem some of them more important, reliable and conclusive than others.

  Sometimes new derivations appear fragile because they are new, for instance if they involve innovative and relatively untried techniques or instruments that have to be checked. Catherine Allamel-Raffin and Jean-Luc Gangloff's chapter offers an illustration in the case of a new survey in astronomy. Sometimes new derivations are viewed as improvements on the old ones. The scientific episode in the field of quantum physics investigated by Catherine Dufour is a case in point. Here we find generations of successive experiments aiming at testing the same claim *X*, and each new experiment is seen as an improvement on previous ones with respect to this or that loophole in the experimental derivation.[5]

  Whatever the details of the particular historical configuration, it is clear that the different experimental derivations, past or present, that speak in favour of a given *X* in a given stage of scientific development are usually perceived as differing in strength. In her chapter, Dufour proposes a graphical representation of the situation by means of more or less large arrows according to the strength of the derivation.

[5] Admittedly, this is not a typical case of robustness, because as soon as real experiments are conceived as improvements on one and the same experiment, their independence is obviously discussable, and in any case they are not independent in the same sense as multiple experiments of different kinds. (For more details on this point, see below Section 1.9.2 the presentation of Dufour's contribution.) But as we will see, the independence is always problematic, and, anyway, the configuration involved in Dufour's case study is a widespread situation that must be considered in relation to the robustness issue.

- Moreover, the analysis of the solidity of an experimental fact should not just take into account *only* the experiments that have *supported* the result finally established as a fact, but *also* the possible experiments that have played *against* this result. For, in the controversial situations at least, the status of a solid experimental fact emerges through an assessment that is not based only on positive elements.

  Stegenga refers to this problem as "discordance", which he divides in two types, "inconsistency" and "incongruity". According to him, discordance is "ubiquitous" whereas concordance is not. Moreover, artificial concordance is often produced by retrospective readings of past science, through which all that is not congruent with the present science is 'forgotten' or dismissed. In the light of such considerations, philosophers of science should be more attentive to discordance in their analyses of robustness. Dufour's study of quantum physics also raises the issue of discordant evidence and its consequences, in a situation where there exists a high multiplicity of concordant derivations against a unique discordant derivation which finally counts for nothing in the physics community.

  This difference between concordant and discordant arguments could be taken into account in more complex and enriched robustness schemes, and could be expressed graphically, by introducing, for instance, arrows with a (+) and arrows with a (–), or else by using different colours (with possibly a set of different shades indicating the relative strength of the supportive derivations on the one hand and of the 'fragility-making' ones on the other).
- Finally, the equation 'robustness = invariance under multiple independent derivations' only holds if the derivations involved are *genuine* derivations. In other words, it is related to a certain conception of what is an argument worthy of the name: it presupposes a set of norms about what is scientific/pseudo-scientific/non scientific.

  Indeed, imagine that the arrows of a robustness scheme represent derivations that have been rejected in the course of the history of science (for instance: the famous experiments invoked by Blondlot in favour of the existence of *N* rays). Or worse, imagine that the arrows symbolize derivations viewed as pseudo-derivations by any contemporaneous physicist. For instance: the *N* rays exist, because it is impossible that God, who is omnipotent, restricted himself to the narrow set of the rays so far discovered by physicists (electromagnetic rays, X rays and the like). Another amusing illustration is given in Hubertus Nederbragt's contribution. Nederbragt mentions a situation imagined by Collins in which a scientific hypothesis under discussion would be tested, among other ways, by examining the entrails of a goat. This 'method' is indeed truly independent to the other scientific methods involved (such as the recourse to experiments), "But," Nederbragt stresses, "here we reject immediately and with force the whole body of background knowledge of this method".

  In cases of this kind, the derivations will only be considered as 'derivations' with inverted commas. Even if they are indeed independent of one another, they will not appear as potential arguments in favour of *X*, and the status of 'robust' will be refused to the node on which they converge.

These remarks point to the need for an analysis of what confers its strength (or degree of solidity) to the derivations involved in a robustness scheme. I will come back to this topic below in Section 1.6.2. In the meanwhile, we can conclude that the analysis of the solidity of an *X* will call for, together with the quantitative evaluation of the number of arrows, a prima facie more qualitative estimate of the strength of each arrow.[6] Not only must the arrows be counted, but moreover they must be weighed.

Actually the situation is even more complicated. Although at first glance an analyst's task of counting the derivations under which an *X* remains invariant seems straightforward, under examination it is not. Several reasons can be given for expecting that (and explaining why) different analysts (including scientists) interested in one and the same targeted scientific situation may represent it as involving different numbers of derivations in favour of *X*.

First, we can deduce from the previous reflections about the strength of a derivation that the two variables 'strength of derivations' and 'number of derivations' are not independent. As we have seen, a 'too weak' derivation, either positive or negative, will count for nothing. Clearly, generalizing, judgments of strength will have consequences for the number of derivations that will figure in the robustness scheme supposed to represent a given real situation.[7]

Second, there is the hard problem of assessing what should be counted as two truly different derivations or, rather, as two versions of *one and the same* derivation. This is what Jacob Stegenga calls the "individuation problem" (in his framework: for "multimodal evidence"; in my terminology: for multiple derivations): According to what criterion are we going to individuate a singular mode? The answer to the previous question is crucial, since it will determine the *number* of different modes

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[6] Bayesians might contest the claim that the estimations of the arrow strengths are indeed "more qualitative", on the basis that they are able to quantify the weight of each arrow through numerical measures of evidential support. (I thank Stegenga to have drawn my attention to this point.) But, to my eyes, such kinds of attempts are at least useless with respect to the aim of characterizing real, ongoing scientific practices, and sometimes pernicious with respect to the realistic pretention of the numerical values they put forward and the algorithmic-transparent model of rationality they suggest. (see below, the end of Section 1.4, and Chapter 10, especially the conclusion.) So, at the end of the day, I think that robustness is indeed a qualitative notion (as is any human judgment), even if quantified modelisation can sometimes be clarifying. As Stegenga concludes, after having noticed that "Philosophers have long wished to quantify the degree of support that evidence provides to a hypothesis" and having regretted that in such context they "developed confirmation theory 'given a body of evidence *e*', without worrying about what constitutes a 'body of evidence' ": "at best, the problem of discordance suggests that robustness is limited to a qualitative notion." I agree with Stegenga's conclusion, perhaps more strongly than he does himself, given his cautious formulation. In any case, the study of real, ongoing practices clearly shows that different practitioners very often weight differently the multiple derivations for and against a given hypothesis in a historical situation (see for example Dufour's paper as an illustration among others). So if weights could be meaningfully attributed to derivations, they would likely be different from one scientist to another one – which is of course not in the 'objectivist' spirit of the Bayesianist enterprise.

[7] For further developments on this issue, see also Section 1.5, third and fourth points.

that speak in favor of the hypothesis under discussion, that is, *how multiple* is the *multi*modal concordant favorable evidence for an *X*, and where lies the boundary between a *multi*modal configuration and a *mono*modal configuration.

1.4 Solidity, a Status That Comes in Degrees

'To be solid', as an attribute of an *X*, is not a binary property admitting a clear-cut application of the form 'yes or no'. *X* is not either solid or not solid: *X* is *more or less* solid. And, as a particular case, *X* is more or less robust, more or less GEed.

All authors who reflected on robustness have stressed this feature, among whom are Wimsatt himself, and, following him, many contributors to the present book. "Robustness may come gradually and in degrees", writes Nederbragt, for example, after having quoted Wimsatt.

But the next, more delicate and less explored question is: What does this 'more or less' depend on?

Let us consider the particular case of robustness. It already follows from the last section that in the assessment of the degree of robustness of an *X*, both a number of arrows and the strength of each arrow are involved. In addition, at least two other things must be taken into account.

- The *degree of independence* of the derivations, one from the others

  I will return to independence below, Section 1.8, but just to approach the idea intuitively at this point, imagine that the derivations turn out to be hardly independent, or not independent at all. For instance, imagine that they turn on a large number of hidden common assumptions. In such a case the convergence of their results will not provide additional support to the final result (additional with respect to the situation where only one of them would be available). The convergence would be artificially produced by the elements shared by the different derivations.
- The *more or less satisfying quality of the convergence* of the results derived from the different derivations

  At this level, what is needed is an analysis of the grounds on which it is possible to say that the multiple derivations lead to the *same* result. As several chapters of this book will clearly illustrate, the identity is rarely, if ever, immediately given as such. This is a very important point and, I think, one innovative contribution of this book. As far as I know, the reflections previously devoted to robustness topic did not really pay attention to it. As Trizio notes about experimental derivations in his case study in cell biology:

  > (...) different techniques often yield completely different experimental outputs. For instance, a colored film realized with *fluorescence microscopy* will have, at first sight, little in common with a black and white picture obtained by means of an electron microscope. In most cases, the identity is achieved only at the level of the judgments expressing the final results based on the interpretation of the experimental outputs.

Although not explicitly stressed by Allamel-Raffin and Gangloff, their case study offers an especially striking illustration of the idea that the multiple derivations of a robustness scheme lead to *strictly speaking different* derived results and not to *one and the same invariant* result. In some of the argumentative modules constitutive of the astrophysical paper they study, the robustness scheme extracted by Allamel-Raffin and Gangloff clearly involves *perceptually different* images obtained by different kinds of telescopes and different techniques of noise evaluation. Going from these different pictures to the conclusion that all of them converge (give the same information and hence can be considered as *one and the same* map), there is an inferential road and an act of synthesis: practitioners have to work on the initial images, they have to build ways (described in the chapter) to make them comparable and to be in a position to conclude that they converge. So what is at stake with robustness is constructing a convergence of a more or less good quality, rather than an identity that is simply given.

Mieke Boon also draws attention on this point and states it at a more general level. Contrasting "multiple means of determination" with repetitions of the same experiment, she stresses that the former

> usually does not produce the same results, at least not at the level of our observations or measurements. Yet, in his examples of multiple determination Wimsatt suggests that the point of it is producing the *same* results: "to detect the *same* property or entity", "to verify the *same* empirical relationships or generate the *same* phenomenon", etc. (Wimsatt 2007a, 45, MB's italics). This way of phrasing how multiple determination works suggests (. . .) that phenomena are like grains of sand on the beach. They are clearly identifiable objects in whatever circumstances: they remain as exactly the same identifiable entities whether on the beach, at the bottom of the sea, in the belly of a fish or in my shoes. (. . .) this is often not the case.

Stegenga, for his part, presents this problem as one of "the 'hard' problems of robustness", in the context of his discussion of discordant configurations, noting that these are often "incongruities" rather than clear inconsistencies.

> Evidence from different types of experiments, he emphases, is often written in different 'languages'. (. . .) To consider multimodal evidence as evidence for the same hypothesis requires more or less inference between evidential modes.

So, very often, the invariance, or rather the convergence, has to be built from the end-products of the multiple co-present derivations, and these end-products are initially different, strictly speaking. In my chapter, I analyze this building process for a case in which the operations of transformation are 'quasi invisible' and nevertheless – I argue – indeed still present, and I call the operations involved a (more or less creative according to the case) "calibrating re-description" of the end-products of the multiple arrows.

To sum up at this stage: *X* can be more or less robust, and this ‘more or less’ depends on at least four (not independent[8]) variables:

> *X* is (more or less) robust = a (greater or smaller) number of (more or less reliable) derivations that are (more or less) independent lead to a result (more or less close) to *X*.

This characterization leads to difficulties, the structure of which is admittedly familiar to philosophers of science at least since Thomas Kuhn.

How can we determine the ‘threshold’ beyond which *X* can be said to be solid, below which *X* must be considered as fragile? It is well-known that significant disagreements can arise at the level of the judgements of solidity/fragility of any element *X*, both within the members of the scientific community and within the members of the Science Studies community.

This is already the case if we consider in isolation each of the four degree-judgments involved in the robustness equation above. And it is a fortiori the case for the global judgment in terms of the degree of robustness, which, in addition, involves a balance judgment. The latter description is, in my view, an analytical decomposition and reconstruction of the situation, provided for the purpose of clarification. In practice it is more than dubious that scientists proceed by composition from the parts to the whole, that is, first by assessing the strength, independence, degree of convergence, and number of convergent derivations, and, second, by constructing, on the basis of these ‘atomic’ judgments, a ‘molecular’ judgment about the resulting robustness of *X*.

Moreover, the global judgment about the robustness of an *X* can vary, ‘all other things being equal’, with contextual conditions ‘external’ to the robustness scheme itself. For example, and as illustrated by Trizio’s case study, if an experimental result *X* under discussion is, according to the theories admitted at the time, highly implausible, then a higher number of convergent experimental derivations will be demanded to in order to conclude that *X* is sufficiently robust – higher than if this *X* were expected according to these theories. Or, again, and as illustrated by Dufour’s case, if *X* appears as an especially crucial and important proposition, even a very high number of concordant experimental derivations will seem still insufficient to some scientists.

This is not to say that, as philosophers of science, we cannot try to propose criteria, or at least to reflect on what could be the criteria, to evaluate what is a sufficiently high number of sufficiently independent derivations which show, taken one by one, a sufficient degree of strength and, taken all together, a sufficiently good degree of convergence, and on this basis, criteria to assign a sufficient degree of robustness. In this vein Stegenga introduces what he calls “the amalgamation problem”, that

[8] In Section 1.8, we will see that the variable ‘number of derivations’ is no more independent of the variable ‘degree of independence’ than of the variable ‘strength of the derivation’.

is, the problem of "*how* multimodal evidence should be assessed and combined to provide systematic constraint on our belief in a hypothesis" *X*, and he looks for an "amalgamation function" to do the job. But his analysis points to deep difficulties associated with the task.

So at the level of all of the degree-judgments involved, we face the difficulty, not to say the impossibility, of specifying universally compelling criteria and of pointing at a universally compelling threshold beyond which it would be de jure irrational not to uphold the solidity of *X*. This difficulty becomes particularly tangible, and its epistemological implications become clearly visible, when we study moments of the history of science during which important controversies about solidity judgements took place among the practitioners themselves. However, this difficulty is in principle also present for episodes in which a good consensus exists among the members of a scientific community. And it is a difficulty with respect to which a philosopher of science willing to analyze the solidity of what is taken as a scientific accomplishment has, sooner or later, to take sides, even if it is by entrenching himself behind the practitioners' judgements.

1.5 Arrows-Node Schemes of Solidity and Scientific Practices

The volume title refers to the practice turn. What then is the relation between a solidity scheme – such as the one of robustness or generative entrenchment as defined above – and the level of scientific practices?

Solidity schemes can be said to be *emergent* with respect to scientific practices, in the following sense:

- The scheme aims at grasping what has emerged during certain 'conclusive moments', at a given point of the historical process.
- The arrows and the nodes that constitute the scheme belong, therefore, to the level of the 'finished' products of science, rather than to the level of the ongoing process of scientific investigation;
- The arrows and the nodes refer most of the time to what appears in scientific publications, rather than to what is actually done in the laboratories.

This doesn't diminish the interest and relevance of these schemes, for the vast majority of the scientists themselves rely on what appears in the publications in order to judge the solidity of a procedure or of a scientific result. But this makes us suspect that the robustness and GE schemes do not exhaust the analysis of the constitution of solidity. I will shortly get back to this when I address the issue of the independence of the derivations (Section 1.8).

Meanwhile, I would like to specify further in what way the solidity schemes pictured by means of the arrows-nodes representation belong to an emergent level with respect to the one of science in action and in real time.

I will specify that point by relying once more on the case of robustness.

- First, the robustness scheme already presupposes the stabilization of the 'derivation-result' connection. Whereas in real and developing laboratory practices, this connection emerges from a more or less delicate and more or less problematic trial and error process, the outcome of which is of course not determined in advance for practitioners.
- Second, the scheme offers a panoramic description that treats each derivation as an unproblematically individuated entity and as a black box. Whereas with reference to laboratory practices, a derivation involves multiple sequences of actions, whose individuation and re-individuation are not obvious and may be problematic. Moreover, even once these sequences of action have been conceptually grasped as *one* sufficiently well-defined *kind* of procedure symbolized and black-boxed as an arrow, this arrow actually refers to a complex reality combining multiple components of heterogeneous types (material-conceptual-functional objects such as instrumental devices, practical skills required for the successful use of the instrumental devices, various kinds of operations and reasoning...).
- Third, and as already stressed Section 1.3, the scheme only takes into account the supportive arrows (the 'confirmations'). It says nothing about the argumentative lines that could weaken the node *X*, if not refute *X*. In other words, the scheme holds after an assessment of the importance of the negative arguments possibly involved in the historical situation. Such an assessment can be either the conclusion of a deliberate and systematic discussion, or – maybe more often – an intuitive judgment associated with implicit reasons. But in any case, the robustness scheme, which does not even mention, and thus ignores, the negative arguments, presupposes that these negative arguments are not significant or of very weak weight.[9]
- Fourth and finally, even with respect to the supportive elements alone, the scheme presupposes a preliminary choice, related to the more or less significant character of the available positive arguments. As already stressed and illustrated in Section 1.3 above, only those deemed to be the most significant ones will appear as arrows in a robustness scheme.

Clearly, a solidity scheme is an idealized representation of a real scientific configuration, which depends on and reflects multiple judgments and decisions on the side of the analyst who proposes it.

[9] This point is closely related to what has been developed above, Section 1.3, about the issue of the weights associated with the derivations. An alleged negative argument once proposed in the history of science against an *X* but taken by a given analyst to be very weak if not totally dismissed as an argument (equated with something that counts for nothing, or if one prefers, associated with a weight equal to zero), will *not appear at all* in the robustness scheme that this analysis proposes as a reconstitution of the historical situation.

1.6 About the Nature of the *X* Appearing in the Judgment '*X* Is Solid'

1.6.1 Solidity of the Nodes

I would like, now, to say a few words about the nature of the *X* that could appear in the judgment '*X* is solid'.

Actually, this *X* can be of quite various kinds. This variety will be exemplified by the diverse types of *X* candidates for robustness attributions that are involved in the different chapters of the book. Several contributions also directly address the issue of this variety and propose typologies or classifications. For example, Boon's chapter attempts to list systematically and to categorize the multiplicity of heterogeneous 'things' that are commonly called "robust" and to explain the different senses involved.

A. Under the influence of a long tradition, philosophers of science first think of *X* as a theoretical or experimental result that could appear in a descriptive sentence about the natural world. More generally, they think to *X* as a proposition (or a set of propositions).

   When ***X* = a propositional entity**, '*X* is solid' (typically) means:

   X is trustworthy, reliable, effective, believable, if not true;

   The entities and processes referred to by the proposition exist, are objective, are real.

   For instance, *X* can be a theoretical hypothesis, a whole theory, a theoretical analysis or explanation, an experimental fact, a scientific model, a certain belief, and so on. Many chapters of this volume will consider the solidity of this kind of *X*, as the examples invoked up to now in this introduction already show. In most of these examples, *X* was a proposition, and, in addition, all those examples came from the empirical sciences. But the mathematical sciences are also considered in this volume. In Chapter 8, Ralf Krömer examines the peculiar case of the robustness of mathematical propositions (and more generally if, and to what extent, the robustness scheme plays a role in mathematical practices).

B. Besides propositions, we can also think of other types of scientific results that can take the place of nodes in a solidity scheme.

   For instance, ***X* can be a scientific image** in a broad sense of the term "image" (a photograph, a picture, a map, a pictorial scheme[10]. . .). In their chapter, Allamel-Raffin and Gangloff analyze a case of this kind in the field of astronomy.

[10] Some might claim that images or maps are reducible to complex networks of propositions and hence do not correspond to a *different type* of node than in the preceding case. This might be a controversial point. In their paper, Allamel-Raffin and Gangloff argue that images are not reducible to propositions and that they play a specific role in scientific argumentation. I am inclined to think they are right. Anyway, it is not my problem here to discuss this point. My intention is only to illustrate the diversity of the scientific items that can possibly constitute a node in a solidity scheme.

C. We can also envisage the case in which the node X of a robustness scheme refers to a measuring instrument, or more generally to any technological device.

When $\boldsymbol{X} =$ **a measuring instrument**, 'X is solid' (typically) means:

X fulfills, reliably and durably, the function for which it has been conceived (the measurement of this or that quantity or phenomenon);

The instrumental outputs of X provide information about the 'true values' of the variables, about characteristics actually possessed by the measured objects.

At this level and more generally, the task is to investigate what solidity may mean when $X =$ a technological means, and how exactly the solidity of these hybrid entities is constituted, given that they are, at the same time, material, conceptual, and intentional, that is, designed in the intention to fulfill definite practical functions with respect to human life.

In Chapter 13, Andrew Pickering reflects on the solidity of technological material achievements (what he calls "free-standing machines and instruments"). Faithful to the fundamental intuition that underlies all his work, he focuses on the level of the most concrete actions and material aspects of science, holding that "material performance and agency" is "the place to start in thinking about the robustness of science".

The contribution of Nickles also touches on the solidity of technological achievements, but here the X under discussion is not just a simple localized object such as a measuring instrument; it is, rather, a highly complex "epistemic system" which involves, among other dimensions, technological features.

D. We can add this kind of complex X to our repertory. True, contrary to what the title of the present section suggests, we are not intuitively inclined, initially, to see this kind of X (a complex epistemic *system*) as a node. But this could be said as well about the X considered just above section C, for a measuring instrument can itself be viewed as a (more or less) complex system and represented as an extended network constituted by multiple nodes and arrows. And after all, any complex system can be seen, at a certain scale, as one unitary entity, and its solidity can then be discussed as such. Anyway, even if the kind of X in question is not at its ideal place in the present section, the discussion of the solidity of $X =$ a complex hybrid system is an important aspect of the solidity problem.

When $\boldsymbol{X} =$ **a complex hybrid system**, 'X is solid' can mean very different things, beyond the idea of its ability to fulfill, reliably and durably, the function(s) for which it has been designed. It depends on what the system is made of, on its intended use and on what the users want to avoid. Admittedly, the solidity will be differently conceived if the complex system is an epistemic system like a scientific paradigm, an industrial system like a nuclear power plant or an information system like the Internet. But at a general level we can nevertheless stress, following Nickles in Chapter 14, that for complex hybrid systems, to specify what solidity is, requires us to specify the *kinds* of anticipated threats or failures we want to avoid, and to *relativize* solidity *to* these kinds. A complex system is never solid in all respects. It may be solid in certain respects but fragile in others. Moreover, as Nickles argues in his chapter, attempts to increase the solidity in one direction often, as far as we can know, create new, unpredicted fragilities, sometimes worse than the ones eliminated or attenuated by previous design modifications. In these cases a "robustness-fragility tradeoff" is inevitable and we must renounce once and for all the optimistic idea of a "cumulative fragility-reduction".

With respect to complex systems, more especially technical systems, Nickles, in his chapter, refers to a useful distinction between two kinds of robustness (or rather, solidity, according to my terminological decisions) that we can fruitfully add to our toolbox in the context of this introduction. The first is "simple robustness", referring to a situation in which a system is robust because it is made of a few very reliable components (the robustness of the totality is the sum of the robustness of the parts). The second is "complex robustness", related to a situation in which the robustness is an emergent property of a complex system made of multiple 'sloppy' and cheap *but redundant* parts and of diverse processes of control.

E. Finally, outside of the field of studies devoted to scientific achievements, robustness is also currently applied by historians and philosophers of evolutionary biology to living beings 'designed by nature' (by reference to and by analogy with technological devices designed by humans), or more generally to aspects of the natural (possibly nonliving) world, such as physical phenomena or the behavior of physical objects. This dimension of robustness will not be systematically investigated in the present volume which is primarily interested in the robustness *of scientific achievements*. Nevertheless, Wimsatt's contribution will consider this kind of "material robustness" (as he calls it, by contrast to "inferential robustness"), and will illustrate it through the example of the robustness of some phenotypic properties required in order for the sexual reproduction to be possible.

1.6.2 Solidity of the Arrows

The previous examples were examples of an *X* that would appear *as a node* in a solidity scheme. Now I would like to say a few words about the solidity of the *arrows* of the scheme.

I will consider again the case of robustness: the case of an arrow that denotes a procedure (a derivational means, whatever it is) aimed at establishing the robustness of *whatever type* of scientific result. At this level, the typical examples are:

- A theoretical argument in the empirical sciences;
- A mathematical argument;

- A protocol of any kind, for instance:
    - A technique (of fabrication, of production of samples, of preparation…)
    - An experimental procedure, with its characteristic set of instrumental apparatuses and techniques…

When ***X* = a procedure-arrow**, '*X* is solid' can be further characterized as: *X* is reliable, trustworthy, efficient, stable.[11]

Several chapters of this book deal with the issue of the solidity of a procedure in an indirect or peripheral manner, but this question is the central topic of Frédéric Wieber's contribution (Chapter 11), which describes and discusses how a modeling procedure developed in the 1960s and 1970s in the field of biochemistry has been progressively taken as a solid procedure. Dufour also directly addresses this issue, through the characterization of two different "loopholes" in the available derivations in favor of the violation of Bell's inequalities, and insistence on the fact that practitioners assess the consequences of these loopholes differently. This is also the topic of my own chapter, where I open the black box of an experimental derivation in favor of the existence of weak neutral currents in the 70s, and give a structural characterization and a visual representation of this derivation, on the basis of which I discuss what the solidity of a derivation is made of.

If we ask: 'What types of reasons can be invoked to support the reliability of $X =$ a procedure?', the answer can be explored along two paths and divided in two parts.

- A first part of the answer doubtlessly lies in a sort of reversed formulation of the Wimsattian robustness scheme: the solidity of a procedure will increase with its being involved in the derivation of the greatest possible number of independent results already established as solid (say the *R*is). For instance, the reliability of an experimental procedure will increase with the number of already-robust results that it yields.

  Does this first part of the answer lead us in a circle?

  Yes, no doubt. But the circle is not necessarily vicious. On the face of it, we are tempted to avoid the vicious circle by pointing out that the results and methods that play the role of 'what establishes solidity' on the one side, and the role of 'what acquires solidity' on the other side, are not the same. The

[11] Note that the same terms were already employed in the previously considered case, '$X =$ a propositional entity'. Actually, these terms, and especially the reliability vocabulary, have a very broad scope and can be applied to propositional results as well as to procedures. (In this respect, the lexicon of 'reliable' could have been a good alternative to the vocabulary of 'solidity'). But note also that the reverse does not hold: all the terms used to name the solidity of a *propositional* result do not automatically apply to *procedural* achievements. In particular, talk of 'truth', 'objectivity' and what is 'real' are restricted to propositional entities. They are not, in ordinary usage at least, employed to pick out procedures, methods, derivations and the like. These differences at the level of the vocabularies reflect a difference of epistemic kinds.

already-solid methods act like springboards to establish the solidity of *new results*, and the already-solid results act like springboards to establish the solidity of *new methods*. We find, therefore, rather than a circle, a helix.

These reflections lead to several suggestions.

First, they clearly show that a solidity scheme only holds with respect to certain historical coordinates, and, therefore, that any solidity scheme should specify these coordinates. Indeed, these coordinates will determine what can be taken as already solid – what works as an already established ingredient – and can therefore serve as a source of solidity for the *X* under scrutiny.

Second, even if we believe that this helix is not vicious, we are pushed in an endless journey back trough the axis of the time of the history. For the 'what has been *newly* established as solid' always refers to 'what has been established as solid *before*', and so forth. We have a temporal regression without an end.

This can make us suspect that the historical order is far from being indifferent. . . That science might be path-dependent in a highly constitutive way. It raises the problem of knowing whether we can leave the historical order behind, by considering, as is almost always done, that at least some of our robust scientific results enjoy autonomy with respect to this order, and were, in a sense, inevitable independently of this order. This is a delicate question. The epistemological implications are the degree of contingency associated with what is taken as a scientific result, and the plausibility of a science that would be, *at the same time*, *as solid as our* science, *but* coordinated with an *irreconcilable* ontology – an ontology either contradictory, or, more plausibly, incommensurable to ours. This is the contingency issue, to which Andrew Pickering has made an original, if controversial, contribution, and to which I shall return at the end of this introduction.

- The answer that has just been given to the question of what provides its solidity to a derivation leaves us unsatisfied. It seems to be only a part of the answer. Indeed, intuitively, this answer appears too 'extrinsic' with respect to the procedure *X* whose solidity is under discussion. For it depends on historical circumstances external to *X*, that is, to the *empirically contingent* development of a set of *other* procedures involved in the establishment of the solidity of the already-established-as-solid *R*is.

  Intuitively, we would like to link the solidity of a procedure to some of its 'internal' properties, to more 'intrinsic' features. This would be the second part of the answer to our question. In my chapter, I will examine the nature and the role of these possible intrinsic features that are expected to give its solidity to a procedure 'from the inside'. But to anticipate, I can emphasize the fact that these features – that one may be inclined to consider, at first sight, as 'intrinsic' – can be said to give its solidity to a procedure only *by referring to many other things besides themselves.*

On the whole, we have the quite vertiginous impression that every time we ask about the solidity of an ingredient of the robustness scheme, we start a cascade involving more and more ingredients, horizontally in the synchronic space, and vertically along the diachronic axis.

I will now have a closer look at what there is behind this impression. This will lead us back to the tension between holism and modularity introduced above in Section 1.2.

1.7 From the Pyramidal-Foundational Model to the Holistic-Symbiotic Model, and Back

In order to understand better the 'mechanism' that underlies the attributions of solidity, I will discuss the simple case of a four ingredients robustness scheme in which three arrows converge on a node *R*, and I will do the following exercise: to vary the 'solidity values' (an expression framed on the model of the widespread expression 'truth value') associated to the different ingredients of the scheme, and to examine the various resulting readings.

1.7.1 A Thought Experiment Playing with the Solidity Values of the Elements of a Robustness Scheme

A. Let us first imagine that, at a given moment of the history of science, the three kinds of methods involved in each of the three derivations are – independently and before they come to be seen as able to derive the result *R* and as a supportive argument in favor of *R* – already standard and well-mastered methods, and thus methods seen as reliable and solid. Let us assume moreover that the three arrow-methods are of *equal* solidity.[12]

   On the basis of such 'initial'[13] solidity values, the scheme of robustness should be read as follows: the result *R* becomes robust (or: acquires a strong

[12] This is of course just a thought experiment in order to show how the solidity scheme works, not a positive claim about the possibility of 'measuring' the solidity values that in fact hold or should have held in a given empirical situation. As already suggested in note 6, this possibility is, to say the least, highly problematic.

[13] The quotes are intended to prevent the reader from equating this 'initial' to a given moment of the history of science at which one would have succeeded in measuring the actual, objective solidity values of some existing scientific derivations. This is not at all the sense of the present exercise. The 'initial' and the 'final' name two *logical* moments of the proposed thought experiment. The thought experiment *imposes by fiat* such and such solidity values to certain elements of the robustness scheme (which are the 'initial' values) and then discusses, on this basis, what consequences result 'next' (i.e., what are the 'final' solidity values and how they result from the 'initial' state). Of course, this thought experiment is intended to help us to understand what happens in real historical situations. But this does not imply that in real historical situations anyone is able to associate explicit objective measures to scientific derivations. The relation with the history of science, and the way the thought experiment is used to understand real situations, will be sketched below Sections 1.7.2 and 1.7.3.

degree of robustness), because three independent methods converge on it. Here, the 'flux of robustness' is *oriented* from some ingredients of the scheme (the three derivations taken altogether) to others (the result *R*): namely *from* the more robust ingredients *to* the less robust ones. Intuitively, one is easily inclined to characterize such a situation by means of a pyramidal-hierarchic (down-up) model, if not by means of a foundational one: a model according to which an 'intrinsically solid' basis works as a foundation for the edification of upper, 'intrinsically more fragile' floors.

B. But let us now modify, in the preceding scenario, the 'initial' solidity value of one of the arrow-methods. Imagine that one of the three methods is controversial, that it has a low degree of solidity. In such a configuration, the robustness scheme will lead to a reading notably different than the one in the previous 'democratic' scenario (same weight attributed to each arrow).

   It will be read as follows: two equally solid derivations (based on solid methods) lead to the result *R*, thus this result acquires a certain degree of robustness. In parallel, a method so far considered as fragile leads to the same result *R*. Thus, the method in question acquires solidity. Therefore, on the whole, we can say that the result *R* is at the point of convergence of *not just two* solid derivations, *but three* solid derivations, and thus, the degree of robustness of *R* still increases (compared with the configuration in which *only two* arrows converged on it).

   The description just proposed to describe this second scenario appears a little bit artificial and unsatisfactory. This is mainly because a verbal presentation is ineluctably sequential, joined to the circumstance that, for the sake of didactic purposes, we are led to decompose the situation into a maximum of 'partial fluxes' of robustness. In such a verbal presentation, we cannot but consider *one after the other*, and hence in a certain order, the multiple partial fluxes of robustness directed from some ingredients to some others. Whereas, in fact, the robustness attributions result from a global, 'instantaneous' equilibrium, from an unordered mutual reinforcement.

C. Actually, the most suitable configuration to show that the attributions of solidity result from a global equilibrium is the configuration in which all the ingredients of the scheme are 'initially', when considered in isolation from one another, all taken to be fragile (or associated with a weak degree of robustness).

In that case, the scheme will be read as follows: the robustness of the result *R*, the solidity of the first derivation, the solidity of the second derivation, and the solidity of the third derivation, are each *mutually and equally* reinforced, due to the global configuration in which they are involved. *Although 'at the start', none of the ingredients were solid, all of them gain solidity from their conjunction in a network*.

In such a configuration, the 'solidity fluxes' are no longer unidirectionally oriented gradients: they are uniformly distributed. The reinforcements act in both ways and in a symmetrical manner between all the elements, a situation that we can represent by means of double arrows. Each ingredient equally contributes to the solidity of all the others.

In such a configuration, one is no longer inclined, intuitively, to appeal to a hierarchic-foundational model. Rather, one is inclined to say that the solidity is co-constituted, co-stabilized. One is inclined to describe this situation by referring to a network structure, to a holistic equilibrium, to reciprocal stabilizations, or again – to borrow a term often used by Pickering in his analysis of scientific practices – to a "symbiosis".[14]

Here not only are we back to the relational nature of solidity attributions noted above, in Section 1.2, but we can also go a step further. Relying on the previous analysis, and taking into account the fact that each element of the network owes its strength to its connection with the others, we are led to conclude that the *X* involved in the judgment '*X* is solid' can be identified – and perhaps is more adequately identified – with a more or less complex structure or network or symbiosis, rather than with one particular ingredient of this structure (as is commonly done and as I have treated it in this introduction up to this point). In his chapter, Nickles will approach robustness by taking up this perspective, that is, from the angle of the general characteristics that complex *systems* might or should satisfy in order to be solid in this or that desired respect.

1.7.2 What Hides the Holistic-Symbiotic Working of a Robustness Scheme in a Given Historical Configuration

When we analyze a particular case, what is it that hides the holistic-symbiotic character, or even leads to reject as invalid the holistic-symbiotic model and favors, instead, a hierarchic-foundational reading? It is the operation that, 'initially', remains fixed, and assumes as unproblematic the solidity of some of the ingredients of the scheme considered in isolation.

What can we say about this operation?

First, that it obviously reflects what is the case in the history of science and in real practices. Indeed, each practitioner, at a given period of the research, does not place on the same level all the ingredients involved in the developmental stage in question.

Second, we should not deplore such a situation, since it seems to be a necessary condition of the possibility of our science. If the attributions of solidity were all the time and all together questioned, practitioners could not rely on anything and science could no longer progress – or in any case, would not look like *our* science. We are led here to a variant of the famous metaphor of Neurath's boat.

Third, the question nevertheless arises of the origin of the attributions of solidity that are 'initially' associated to the multiple ingredients of the robustness scheme.

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[14] Pickering's symbioses apply to real time dynamic scientific practices and their various (material, intellectual and social) ingredients, rather than to elements of an idealized and 'synchronic' robustness scheme as is the case in the reasoning just above. For more about the symbiotic conception of scientific development and references, see Section 1.7.3.

And to this question, the answer seems to be: the initial solidity of an ingredient considered in isolation is related to its past history, and more precisely, to the way this ingredient has been, in the past, involved in other holistic equilibriums or symbioses. We are back to the open-ended helical process we met before. We have the impression that the holistic modules represented in the plane of the sheet of paper must always be connected, 'in depth' (in a perpendicular plane representing the temporal dimension), to an undefined multiplicity of structurally analogous holistic modules.

1.7.3 Structural Homologies and Substantial Differences Between the Robustness Analysis of Real-Time Scientific Practices and Retrospective Consideration of Past Science

What has just been said about the holistic-symbiotic way of functioning applies, as I stressed before in Section 1.5, to a level which is emergent with respect to scientific practices. In Section 1.5, I specified in what sense and for what reasons the robustness scheme is emergent. We can now reformulate the point in terms of holistic units, now seeing any solidity scheme as a holistic unit.

A solidity scheme reflects and carries some options about the content and the extension of the holistic-symbiotic units within which, and at the scale of which, the interactive stabilizations are supposed to apply. In other words, it presupposes options about the content-extension-frontiers of the network on the basis of which it is relevant to think about the equilibrium of the solidity distributions. That is, the analyst cuts through the infinitely rich historical reality where, strictly speaking, everything could be thought to be linked with everything in a certain respect; and he extracts a holistic module considered as sufficiently autonomous with respect to the aim of solidity analysis. In other words, he assumes *local* holism[15] and it remains for him to decide about the 'size' of the local. He has to face the tension between holism and modularity and to overcome it in one way or another.

That said, *although emergent*, the analysis proposed above concerning the way the holistic modules function, is, I believe, *still relevant*, in its general principle, for understanding what is at stake in *real practices* such as laboratory practices. This is so, since something like the same structural scheme is involved in both cases: at the level of the emergent scheme as well as at the level of the ongoing practices, we find interactive co-stabilizations. When we consider science from the standpoint of

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[15] Thomas Kuhn introduces the idea of "local holism" in a 1983 paper in which he tries to understand the incommensurability of scientific theories in terms of the (locally) different linguistic structures of these theories (On Kuhn, robustness and incommensurability, understood in a structural framework inspired by network theories, see Nickles' paper, Chapter 14.) To decide how local is this local holism (and hence how 'extended' or 'spread' is the incommensurability), or to decide the size of the network relevant to robustness assessments, is the same kind of problem and leads us to back to the tension between holism and modularity stressed Section 1.2.

practices, we can re-describe it as an attempt to obtain good "symbioses"[16] between the ingredients involved in these practices. When the attempt is successful, what is obtained is a modular (more or less extended) holistic unit endowed with a certain quality of stability and autonomy. Ian Hacking once described it as a "closed system" and a "self-vindicating structure" (Hacking 1992, 30), Andrew Pickering as a "self-containing, self-referential package" (Pickering 1984, 411). It is such a unit which can be represented at an emergent level, through considerable simplification and inevitable distortions, by an arrow-node scheme of solidity.

Now, if there are structural homologies between what holds in real historical situations and what holds at the level of the very simplified representation of solidity schemes, there also exist substantial differences.

A first striking difference, to which an allusion has already been made at the very beginning of this introduction, lies in the *nature of the ingredients* involved in the inter-stabilizations under scrutiny. When we consider science in action, for example laboratory practices, the interactive stabilizations or good symbioses that eventually emerge arise, as the practice turn showed, among ingredients *of various heterogeneous types*, possibly including know-how and professional skills, local norms and standards guiding the production of results, instrumental and material resources, geometry of the laboratory and short-term concrete feasibility, if not available institutional organization, the power of convincing peers, decision-makers, sponsors, and so forth.

If we want to continue to use an arrow-node diagram in order to represent living practices and the process of constitution of solidity in these practices, we will have to distinguish and to define the possible kinds of ingredients potentially involved and to find a specific representation for each kind.

On this path, the task and the difficulty are:

- To specify and classify the items that may constitute the robust scientific symbioses (taxonomy of the constituents that are likely to intervene)[17]
- And at the same time, to specify the nature of the 'glue' that may be able to 'hold together' the various ingredients of scientific practices (typology of the relations that are likely to contribute to a solid symbiosis). We need to find a way to think

[16] Pickering (1984, 1995). Hacking sometimes also uses this terminology (see Hacking 1992), but more 'in passing'. In Pickering's writings, the idea of a symbiosis is more developed and is a central conceptual tool.

[17] This is actually a very difficult task. Hacking made an attempt in this direction in Hacking (1992). He distinguishes three main categories ("things", "ideas" and "marks") and fifteen elements distributed on them (See the quotation note 21 below for examples of these elements. See also Mieke Boon's paper, Section 1.2, for a presentation and brief discussion of Hacking's typology). More generally, and as already noted, Boon's paper itself aims at building a typology of the ingredients involved in scientific practices that might be candidate to robustness attributions. Further work on this issue is, in my opinion, strongly needed. Clearly, the general – highly criticized but still in use – distinctions such as cognitive/social and internal/external factors are completely non-operative for the analysis of detailed case studies.

about the nature of this 'global hanging together' on the basis of something different from the classic idea of logical coherence. We need to develop something akin to what Hacking called an "enlarged coherence" between "thoughts, actions, materials and marks" (Hacking 1992, 58).[18]

A second important difference between a characterization at the emergent level of solidity schemes and a characterization at the level of real practices[19] is that, in the latter case, we must take into account a sense in which at least some of the ingredients involved in real time practices are in a relation of co-*maturation* (this is one important aspect that the idea of a symbiosis is intended to convey).

This means that, at a given moment of the history of science, several coexisting traditions (typically, some theoretical and some experimental) reciprocally feed the others with relevant and interesting subjects of research and mutually influence the investigation of the others in certain directions, thus at the same time moving them away from different possible directions. Each pole favours what is 'in phase' with it within the other (where 'in phase' means at least 'provides relevant exploitable elements to it', if not 'gives positive support to it'). Such a picture motivates the symbiosis metaphor: each pole gives life to, or sustains, the survival of what is 'in phase' with it. Correlatively, each pole neglects or dismisses what is not 'in phase', thus contributing to its extinction. When, as a result of this process, a better symbiosis than before is achieved at a point of the history of science – as sometimes happens –, that is, when practitioners feel that more things than before nicely fit together or are more strongly connected, this in turn conditions what happens next – what is taken as an interesting question, as an already solid and as a still fragile ingredient, etc. – and so on from point to point along the temporal axis of the history of science.

As a consequence, an arrows-nodes scheme designed to be a model of real scientific practices should represent the various equilibriums *in 'real time'* and show how they have been restructured along the historical path. This means that each arrow-node configuration should only involve *contemporaneous* ingredients – rather than juxtaposing in the same space, as is often the case in an *emergent* scheme, derivations that have been stabilized at very distant moments of the history of science. More precisely: if the scheme juxtaposes derivations that have been stabilized at distant moments, the arrows should represent, not these derivations as they appeared to practitioners at the time they had first been stabilized, but – and this is almost always very different – these derivations *as they appear to scientists at one and the same later moment of the history of science* (which coincides either with the period

[18] According to the kinds of 'ingredients' and kinds of 'glue' one is ready to associate to a good/better scientific symbiosis, the idea of a symbiosis can carry different philosophical implications, and in particular, stronger or weaker relativistic implications. See Soler (2008a, 2006b).

[19] The degree of complexity and the opacity of the configurations could also be mentioned here, but I will leave these aspects aside.

of the oldest derivation involved in the scheme or with a period posterior to all the derivations involved).

In this book, several authors – usually inspired by the work of Pickering and by the 1992 paper of Hacking on the self-vindication of science – develop their reflections on robustness (and the dynamical arising of robustness through time in the history of science) in terms of mutual adjustments and interactive stabilizations.

First of all, Pickering himself does this, of course. In his framework, scientific development appears as a "dance of agency" with passive phases where the "otherness" of the world manifests itself through material performances, and active phases where human beings try multiple accommodations and adjustments. After many iterations of such phases, sometimes a point is reached where practitioners feel that a good "interactive stabilisation" (or good symbiosis) has been achieved: all the pieces of knowledge nicely fit one with the others and some pieces of this system nicely fit with the material performances of available material means, so that at the end of the day, all items reinforce one another. At this point the dance extinguishes itself and a duality is produced between the human and the non human (natural or technical) world. At this point some nonhuman ingredients appear to be solid scientific results (supported by other ingredients of the structure) and can be published. As I understand Pickering's picture, on the epistemological side,[20] the robustness of an ingredient of scientific practice is due to the good 'hanging together' of the multiple conceptual and material items involved in a given stage of scientific development.

Boon, following Hacking, also endorses the conception that what practitioners of science strive to achieve is the mutual adjustment and the co-stabilization of multiple elements of different kinds. All the ingredients involved in such self-vindicating structures deserve, according to Boon, the status of scientific results – not just facts and theories as in traditional accounts. As a consequence, many different sorts of scientific results ("data, physical phenomena, instruments, scientific methods and different kinds of scientific knowledge") can be said to be robust, in different senses of the word. In this framework, Boon's chapter aims at specifying how the different kinds of robustness attributions are acquired and what are the interrelations between them.

The same kind of framework underlies my own article. Indeed, my analysis of an experimental derivation makes this derivation appear, at the end of the day, as a complex architecture and a global equilibrium between a great multiplicity of elements that fit altogether.

I think something of the same kind is involved in Nickles' reflection on complex epistemic systems, despite the fact that his reflection is framed in different categories and refers to a different background literature (network theory, theories of complex systems and risk analysis) than the works just mentioned.

[20] In his contribution to this book, Pickering also considers (and primarily focuses on) the *ontological* side of robustness (see below, Section 1.9.5).

1.8 Independent Derivations . . . In What Sense?

The last point I would like to examine in this introduction concerns the independence of the derivations mentioned in a robustness scheme. Several contributions to the present volume encounter this issue at one point or another, and three of them, namely Nederbragt's, Stegenga's and Trizio's, address it extensively.

This is a delicate but highly important issue for at least two reasons: first because genuine independence seems to be a condition of possibility of genuine robustness; second because the independence of convergent derivations is rarely explicitly analyzed by scientific practitioners. As Nederbragt stresses in his contribution, if, "in daily practice scientists discuss experimental methods and their results", in these, "evaluations independence is (. . .) a hidden criterium, applied intuitively."

In order for the *N* derivations represented in the robustness scheme to be stronger than just one of them (or stronger than a number smaller than *N* of them), there have to be, among them, some differences *that make a difference*. The plurality must be real and not just an illusion. The derivations shouldn't, after scrutiny, turn out to be, in some sense, derivable from one another. If this were the case, the 'at first sight multiple' *N* derivations would reduce, under examination, to a number of derivations inferior to *N*, and possibly to one unique derivation. Clearly, the 'degree of independence' and the 'true number' of derivations are not two independent variables.

In order to describe this requirement, Wimsatt resorts to the vocabulary of (partial) independence: The derivations must be, two by two, at least partially independent. The notion of independence that is involved here is far from being straightforward and unproblematic. It would require a thorough analysis based on particular cases. Different types of independence, worthy of careful mapping, are likely to be involved. Now intuitively, we perceive that the independence clause covers at least two different requirements:

- On one side the requirement that the multiple derivations, as far as their *content* is concerned, be different enough (common assumptions should be as few as possible, the derivations should not be logically derivable from one another. . .).
- On the other side the requirement that, as far as their *historical origin* is concerned, the multiple derivations supporting the result *R* do not entertain a relation of mutual generation.

The independence of the derivations mentioned in the robustness scheme has, therefore, two sides:

- Independence of the historical processes corresponding to the empirical development of each derivation as an argument in favor of *R* (origin, maturation and stabilization);
- Independence of the argumentative contents of the emerging stabilized derivations.

Historical or genetic independence is rarely distinguished as such, let alone discussed, in the literature devoted to robustness. Content independence corresponds to what scholars most of the time understand and examine under the independence that is at issue in a robustness scheme or a robustness strategy. This is the kind of independence primarily discussed in Nederbragt's, Stegenga's and Trizio's chapters. Nederbragt nevertheless mentions the possibility of "the validation of one method by the other", which is a particular case of historical dependency of derivations. And his conclusion suggests a distinction similar to the one I just introduced between content and genetic independences: "absence of overlap in background knowledge of both methods and no validation of one method by the other seem to be crucial to decide on independence."

Let us further examine content independence.

1.8.1 Content (or Logico-semantic) Independence

I use the word 'content' in a very large sense: assumptions, principles, concepts, forms of argument, mathematical formula, techniques at work in each derivation...

At the level of contents, the discussion about independence will be based on differences that can be characterized as 'logico-semantic', or 'theoretical' in a very broad sense (e.g. not only what belongs to high level theories, but also very local, possibly implicit assumptions, etc.).

On the logico-semantic side, we have to compare the derivations with regard to symbolic resources (vocabularies, concepts, mathematical tools...) and the assumptions expressed with these resources. We also have to compare relations of all kinds (deduction, inclusion, analogies, reasoning schemes...) existing between the contents of each.

It is possible to envisage logico-semantic differences that are *more or less important:* related to more or less strong degrees of independence. Two derivations of the same result belonging to *different disciplines* (physics and biology for instance) will certainly be perceived as implying more radical differences and a stronger independence than two derivations belonging to the *same discipline* (physics for instance), and than two derivations belonging to the same specialty (particle physics for example).

At the level of the analysis of content independence, using the word 'content' in the broad sense, one could also take into account differences of (what I would call) 'epistemic spheres'. For example and typically, the fact that one derivation is seen as experimental and another is viewed as purely theoretical. Intuitively, this has an impact on the independence judgments. Intuitively and without taking into account what the derivations 'say', an experimental derivation and a purely theoretical derivation owe a part of their independence *precisely to the fact that they belong to two different spheres*. In this vein we should also examine what happens in hybrid cases such as the derivations involving simulations.

Various illustrations can be found in the different chapters of this book. In Nederbragt's chapter, in particular, the independence of the derivations involved in robustness strategies is investigated in terms of a rich panel of examples. The discussion of these examples offers many concrete instanciations of content-independence of the multiple derivations. Nederbragt analyzes content-independence in terms of differences at the level of "theoretical and methodological" "aspects", "assumptions", "background" and "principles" (my logico-semantic differences). We can also trace in his examples something akin to differences related to the epistemic sphere (experimental/purely theoretical investigations). Moreover, Nederbragt's contribution points to other, finer-grained differences of this type, such as the difference between the three following kinds of study: investigation of *naturally occurring* situations ("studies of spontaneous disease cases or spontaneous exposures"); investigations of *deliberately modified natural* (in vivo) situations ("an intervention study, performed in a population in which an exposure is modified"); and investigations of *artificially-created and laboratory controlled* situations ("an experiment under controlled circumstances with animals or volunteers"). As stressed by Nederbragt, when the results of such studies converge, this increases the robustness of the convergent conclusion. I would like to add that it increases the robustness of the convergent *X*, *all other things being equal*, *specifically due to* differences concerning the *modality* of the investigation (purely observational *vs*. modifications in natural situations *vs.* experimentally created configurations). Of course, the latter dissection of the situation is for the sake of analytical clarification. In the discussion of a given scientific configuration, the different kinds of content-independence of the derivations have to be taken into account *all together* and balanced in view to an assessment of the robustness of the derived result.

1.8.2 Building an Independence Scale

As already stressed in this introduction, Section 1.4, the independence of the derivations involved in a robustness scheme comes in degrees. Moreover, intuitively, we relate degrees of independence and judgments of difference in the following way: a judgment of independence includes a judgment in terms of differences; the 'threshold' corresponding to independence is crossed when the derivations are *sufficiently* different; and beyond this point, higher and higher degrees of independence are involved.

With respect to this intuition, Nederbragt, in Chapter 5, provides a useful hierarchic classification of the robustness strategies according to the *kind of difference* and *degree of independence* of the derivations (see also Nederbragt (2003)). Trizio adapts Nederbragt's taxonomy and exploits Nederbragt's classification in his own analysis of content-independence (applied to a contemporary case study in experimental cell biology). Let me summarize this classification and reconsider it in the light of my distinction between content- and genetic-independences. In this perspective, I will categorize it as an 'independence scale'.

Nederbragt's hierarchy comprises four levels. The lowest, more local level refers to "confirmation of the observation when it is made for the first time and is new and surprising". Here the experimenter's aim is to delimit some initial conditions that will lead to stable and reproducible final conditions, and "to exclude the possibility that the observation is a coincidental artefact of the experimental manipulation of the objects of study". So the variations are minimal: the instruments and experimental configurations essentially remain the same. As a consequence, the reproducible final conditions are related to particular and local conditions. Nederbragt names this first hierarchical level "reliable process reasoning (in the experimental set-up)". Reframed in terms of my categories: at this level, we have no content-independence (the content-differences are too small); nevertheless, a certain kind of minimal historical independency is assumed, which here means that all the individual experiments involved count as truly different experiments in the sense of 'performed at different moments' (and possibly: by different experimenters).

The next level up consists in more variations of the parameters of the experiment while conserving the same kind of instrumental means. In other words, the theoretical principles involved in the conception of the instruments are not deeply modified. Nederbragt calls this "variation of independent methods", in which we have to understand "methods" as ways of playing with one and the same kind of experimental set-up: "modifications of procedure within a fixed theoretical background". At this level, the aim of the experimenter is, in Nederbragt's words, "directed at confirming the observation, to ensure that it is generalisable and that it can be made under different circumstances." Here, as at the lowest level, we still have no content-independence (although the differences in content involved are more important than at the lower level); but we do have a historical independency in the same sense as specified just above (clearly, the multiple content-similar but temporally-different individual derivations *count as more than just one of them*). Since the derivations are independent only in the sense they are conducted at different moments, and not as far as their background theories are concerned, Trizio prefers to call this second hierarchical level "variation of experimental techniques".

The third hierarchical level corresponds to "multiple derivability". Here, the theories of the experimental set-up are completely different: each derivation involves "independent methods, different in theoretical and technical background" (e.g., electron microscopy and light microscopy). Multiple derivability, as defined by Nederbragt, "is a strategy for *local* theories" (my italics, LS), for example a specific theory of bacterial invasion applied to the limited domain of particular types of cultural cells and particular bacteria. Following Trizio's exploitation of Nederbragt's scale, only at this third level do we reach a true content-independence. Clearly, a conceptual decision underlies such kinds of judgments (a decision concerning the level at which the independence threshold is situated). Indeed, nothing forbids us from saying that, at the second level of the scale, a low degree of independence is already involved.

Finally, Nederbragt's fourth level corresponds to "triangulation", which is the more general strategy, since "Multiple derivability goes from methods to theories, triangulation goes from theories to theory complexes". In triangulation, the derivations use different and independent theories, and the robust *X* so-produced is a theory complex.

Trizio provides diagrammatic symbols to distinguish the three bottom levels of this independence scale (as far as content-independence is concerned). He distinguishes diagrammatically the three following cases (here re-described in my terminology): *N* truly content-independent derivations (represented as *N convergent* arrows); *N* different but not content-independent derivations (represented as *N parallel* arrows); so minor variations that the result is classified, regarding its content, as one and the same derivation (representation: *one* arrow).

Although such an independence scale is highly clarifying from an analytical point of view, I would like to stress that it is not always easy, faced with a given scientific practice, to decide to which category it corresponds. The reasons are multiple. In real scientific practices, we find something like a continuum of differences rather than sharp distinctions. Correlatively, judgments of independence and 'degrees of independence' are subject to exactly the same problems than judgments of 'degrees of strength' of the derivations put forward above in Section 1.3 and generalized in Section 1.4. They are largely intuitive and qualitative judgments, in part opaque to practitioners themselves. They can vary from one individual to another one. And there is no 'objective threshold' or clear-cut demarcation between a case of independence on the one hand (i.e. a *sufficient* degree of independence, with the consequence that *two* distinct derivations should appear in our robustness scheme), and a case of dependency on the other hand (i.e., an *insufficient* degree of independence, with the consequence that the prima facie duality reduces to the unity, so that *just one* derivation should be represented in our robustness scheme). Once again, we have to face an "individuation problem". In this respect no algorithm can be applied. The analysts must make the decision based on their own judgment and experience, and here differences frequently arise.

In Chapter 9, Stegenga provides a systematic discussion of the difficulties involved in the independence problem (more precisely in the analysis of *content* independence), which clearly and extensively shows how deep these difficulties are as soon as we seek to go beyond the level of intuitions.

1.8.3 Historical (or Empirico-genetic) Independence

I now turn to the *historical* (or 'empirico-genetic') independence of the processes of emergence of the derivations.

Which kinds of scenarios can be called for in order to make sense of and to support the possibility of a genetic dependency between the convergent derivations? Several scenarios are possible, but here I will mention only one of them. This scenario seems to me, on the one hand, the best able to give some plausibility to the rather counter-intuitive idea of empirico-genetic dependency of scientific derivations, and, on the other hand, the most interesting from an epistemological point of view.

In order to grasp the principle of what is at stake, I will rely on an example carefully analyzed by Andrew Pickering in *Constructing Quarks*, one that I will here sum up in an inevitably crude and schematic way: the discovery of the weak neutral currents in the 1970s. This discovery can be explained, and is commonly explained, as the convergent result of several experiments that involve instrumental devices seen as sufficiently different, from a theoretical point of view, to be considered as content-independent. In order to capture this difference in a concise manner, I will talk about 'visual' and 'electronic' experiments.

Multiple interactions have existed between the two teams of physicists that have performed and analyzed the visual experiments on the one side and the electronic experiments on the other side. This is uncontroversial. To jump from this fact to the idea of a genetic dependence between the two experimental derivations, a supplementary thesis must be assumed: the thesis of a non-negligible "plasticity" of scientific practices.

We can understand this plasticity as a variant and an extension (as Hacking says) of the Duhem-Quine thesis. Referred to real-time practices, the thesis is *extended* in the sense that the relevant holistic units are possibly made of *heterogeneous kinds* of ingredients involved in these practices[21] – not just propositions as is the case with the Duhem-Quine thesis as traditionally presented. Plasticity is the idea that scientists looking for solidity and good symbioses can play on numerous factors and try many alignments, with the consequence that several different emergent co-stabilizations are possible – and are possibly associated with incompatible descriptions of the world.

If we are ready to concede this plasticity, it becomes plausible that the interactions between two teams involved in the resolution of similar problems with different experimental means can influence the maturation process of the derivations on each side, and therefore, at the end of the day, will also influence the stabilized couple 'derivation-result' that will eventually emerge on each side and will then be the object of an explicit description in published papers.

For example, suppose that the visual experimenters think they have succeeded in deriving the existence of weak neutral currents from their visual experiment, and that they announce the news to the electronic experimenters.[22] This will strongly encourage the latter to look for solutions that go in the same direction, to try hard to find conceptual interpretations and material modifications of their own electronic experiments that favor the convergence of the electronic conclusion with the visual conclusion.

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[21] "Let us extend Duhem's thesis to the entire set of elements (1)–(15). Since these are different in kind, they are plastic resources (Pickering's expression) in different ways. We can (1) change questions, more commonly we modify them in mid-experiment. Data (11) can be abandoned or selected without fraud; we consider data secure when we can interpret them in the light of, among other things, systematic theory (3). (. . .) Data assessing is embarrassingly plastic. That has been long familiar to students of statistical inference in the case of data assessment and reduction, (12) and (13). (. . .) Data analysis is plastic in itself; in addition any change in topical hypotheses (4) or modelling of the apparatus (5) will lead to the introduction of new programs of data analysis" (Hacking 1992, 54). "Far from rejecting Popperian orthodoxy, we build on it, increasing our vision of things that can be 'refuted' " (Hacking 1992, 50). "The truth is that there is a play between theory and observation, but that is a miserly quarter-truth. There is a play between many things: data, theory, experiment, phenomenology, equipment, data processing" (Hacking 1992, 55).

[22] This is what happened historically. For references about this case study, see my contribution to this volume, in particular notes 1 and 6.

Of course, nothing guarantees that they will succeed (in no way does plasticity mean that experimenters can do anything they want). Nevertheless, we grasp here the possibility of a certain kind of influence that could potentially work as (what I would call a) ‘convergence inducer’.

1.8.4 Robustness, Historical Dependency, Scientific Realism and Contingentism

This possibility is interesting from an epistemological point of view, since the robustness of the convergent result *R* is not at all illusory. Practitioners have indeed found a mutual adjustment of the ingredients involved in the historical situation, which institute a stable connection between a certain experimental procedure and a certain result. But, nevertheless, the maturation processes of the two arrow-node connections involved in the emergent scheme of robustness, namely ‘visual derivation-*R*’ on the one side and ‘electronic derivation-*R*’ on the other side, cannot be said to be independent as far as their historical genesis is concerned.

From an epistemological point of view, this has implications for the realist/constructivist issue. The robustness scheme is currently described as a *criterion* for realist attributions, objectivity and the like. In this vein Wimsatt writes: “Robustness is widely used as a criterion for the reality or trustworthiness of the thing which is said to be robust” (Chapter 2, p. 74). “Robustness is a criterion for the reality of entities” (Chapter 2, p. 76), “a criterion for objecthood” (Chapter 2, p. 75). As I develop in Chapter 10, Section 10.3, the (often implicit) argument that lurks behind the inference from robustness to truth or reality attributions is one version or another of the well-known “no-miracle argument” commonly invoked by realists. Or, in Stegenga’s words:

> one way to understand robustness is as a no-miracles argument: it would be a miracle if concordant multimodal evidence supported a hypothesis and the hypothesis were not true; we do not accept miracles as compelling explanations; thus, when concordant multimodal evidence supports a hypothesis, we have strong grounds to believe that it is true.

As a description of the frequent ‘tacit inferences’ followed by real scientists, I take Wimsatt’s quotations above, and similar claims, to be undeniable. But *as a philosophical thesis about the realist pretention of our science* and the fact that robustness is truly a strong argument in favor of the realism of scientific results, we have to be more cautious.

Indeed, if, historically, the multiple derivations and their convergent result can be seen as co-adjusted and co-stabilized (in senses that should be specified case by case), the miracle of the convergence appears definitely less miraculous. Or rather, it appears to be a miracle of a different kind from the one involved in the traditional so-called “no-miracle argument” of the realist (see Chapter 10, Section 10.3 and the last part of Section 10.16). The miracle would rather be that practitioners have succeeded – under the various constraints, perceived as extremely strong, which were present in a given historical situation – to obtain not only one stabilization, but, moreover, a stabilization that converges with other, already available ones.

I think we should continue to speak of a miracle, in order to point to the extreme difficulty of what is at stake and to the effort and shrewdness it might require. But, obviously, this type of miracle no longer feeds realist intuitions. We are less inclined, not to say no longer inclined, to call for the external pressure of reality as the origin, if not the cause, of the convergence – even if one might choose to conserve the idea of reality as a global source of non-isolable constraints and as a regulative ideal.

What is weakened, in this scenario, is the almost irresistible explanation of robustness in terms of realism: the equation 'it's robust, therefore it is true' (in the sense of correspondence truth). What is moreover weakened – although I cannot really argue this point here – is the equation 'it's robust, therefore it was inevitable' (given certain conditions of possibility working as initial conditions).

So the philosophical questions lurking behind all of this are the realism *versus* antirealism issue and the "inevitabilism" *versus* "contingentism" issue (in Hacking's terminology[23]). Several chapters of the present book approach these questions.

Regarding the contingentism/inevitabilism antagonism, Pickering is one of the analysts of scientific practices who has gone the furthest in the discussion of the contingency thesis. In previous works (see especially Pickering (1995, 201X)), he has articulated the idea of a genuine contingency of scientific achievements and advocated it in relation to various historical configurations. In his contribution to the present volume, he reaffirms the contingency of scientific achievements, be they experimental facts, material devices or more abstract conceptual systems, at the very same time that he vindicates their very robustness. On the basis of his account of scientific development in terms of symbioses, what could forbid several very different and conceptually irreconcilable global fits, all of them achieving a good machinic grip on reality? And why would this multiplicity be a problem? This is how I understand the moral of Pickering's chapter: robust but not unique. The history of science is a contingent process. The results achieved along the way depend on this process, since they acquire the status of results on the basis of their inter-stabilization with other co-present ingredients, on the basis of "contingent and emergent productive alignments". They cannot be detached from this process: they are "path-dependent". Hence, scientific results, although robust, both epistemologically (in the sense that a satisfying symbiosis has indeed been achieved) and ontologically (in the sense that the world or a true "otherness" indeed enter in them constitutively), are nevertheless contingent (in the sense that they could have been different and incompatible).

Nickles and I, both inspired by Pickering's work, will also approach the contingency issue in our contributions.

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[23] See Hacking (1999, 2000). For further consideration of the inevitabilitist/contingentist issue and relevant references see Soler (2008b) (for the presentation of the problem and its situation in the landscape of the Science Studies) and Soler (2008c) (for an analysis of the structure of the problem and its internal difficulties). See also Soler (201X) for a general argument in favor of contingentism. In french, see Soler (2006a).

As for the realist issue, and especially the passage from robustness to truth, many chapters will discuss it: the three just-mentioned ones of Pickering, Nickles, and my own, where the realist issue is mobilized in close relation to the contingentist issue; Stegenga's chapter, as already noted and quoted at the beginning of this section; and Boon's article, in which the author argues that the jump from robustness to the truth of theories and the reality of scientific entities is not legitimate.

1.9 Sequential Overview of the Contents of This Book

1.9.1 Chapters 2 and 3: Wimsatt on Robustness, Past and Present

As already noted at the beginning of this introduction, William Wimsatt is one of the pioneers of the topic of robustness in general and the person most important for making it a topic for philosophy of science. His seminal 1981 paper, "**Robustness, Reliability, and Overdetermination**", is reprinted in this volume as Chapter 2. Since then he has added material in other papers and in his 2007 book, *Re-Engineering Philosophy for Limited Beings*. Wimsatt deserves credit not only for introducing the topic but also for providing an analysis of, and a schema for, robustness that has been, along with Richard Levins' 1966 paper, "The Strategy of Model Building in Population Biology", the main focus of ensuing discussions of robustness. The contributors to this volume maintain that focus.

In his new chapter for this volume, **"Robustness: Material, and Inferential, in the Natural and Human Sciences", Wimsatt** recounts the origins of his concern with robustness, as he experienced it at the beginning of the 1970s, in the context of attempts to connect more closely the domains of population genetics and community ecology. The fact that domains so closely related in topic were employing quite different kinds of models was puzzling. Wimsatt describes the complex theoretical situation at the time and the inadequacy of the methods developed by philosophers of science with respect to the biologists' aims. All this pushed practitioners of the field to elaborate strategies related to robustness. Among others, it motivated Richard Levins to develop the idea of robust theorems that are stable across multiple models, and it led Donald Campbell to introduce the related notion of "triangulation". Inspired by these different approaches, Wimsatt formulated a unified and systematized analysis of robustness. This permitted him to identify various kinds of failures of robustness, such as the "pseudo-robustness" that results when the independence of convergent approaches first assumed proves to be illusory.

Wimsatt's new contribution for this volume provides useful tools toward the recognition and differentiation of types of robustness. In particular, he distinguishes two main types of robustness, inferential and material.

- In inferential robustness the target is what I would call a symbolic object, typically a scientific proposition. In this case, establishing robustness consists in designing multiple inferential means to the target proposition. Wimsatt proceeds to distinguish three subcases, according to the kind of inferential means involved:

(1) empirical means available to the empirical sciences (“sensory modalities”, “instrument mediated” derivations, etc.); (2) empirical tools employed by the social and “human intentional sciences” “that make intensive use of intentional responses” such as questionnaires; and (3) analytical means consisting of “multiple independently motivated and constructed models”, mathematical derivations, and the like.

- Material robustness concerns robustness attributions to material systems – typically biological organisms designed by nature, but possibly including technological systems or non-living objects and their behaviors – that acquire their robustness through natural processes. Wimsatt’s chapter mentions other classification attempts, some very recent, and situates them with respect to his proposal.

In addition to these analytical tools, Wimsatt analyzes a case of material robustness through a puzzling example. He calls it “the paradox of sex” and takes it to be “the most interesting (and indeed most focal) problem involving robustness in biological organisms”. The problem involves two levels: on the one hand the inferior genetic level and the important variation it manifests (genetic variability, genetic recombinations and the like), and on the other hand the superior level of phenotypic properties. The enigma is the following: how is the stability at the superior level possible (leading to members of the species that are sufficiently similar to be able to have sex together), given the high variability of the inferior level? In response to this puzzle, Wimsatt discusses what he calls a “sloppy gappy robustness” or “statistical robustness” in which the desired result at the higher level must only occur sufficiently frequently, although not in all cases.

1.9.2 Chapters 4, 5, 6, 7 and 8: Case Studies of the Robustness of a Single Node

The book continues with five chapters centered on (more or less simple and more or less prototypical) case studies devoted to the robustness of a relatively simple node X (a scientific proposition or a scientific image), both in the domain of the empirical sciences (Chapters 4, 5, 6 and 7) and in mathematics (Chapter 8).

A. In Chapter 4, **“Achieving Robustness to Confirm Controversial Hypotheses: A Case Study in Cell Biology”, Emiliano Trizio** takes up a relatively simple case, although not completely prototypical of the Wimsattian robustness scheme, and a little bit more complicated than this scheme. The analysis of the case by Trizio enables us, both to meet the shift between real practices and idealized schemes, and, in feedback, to understand better the prototypical simple scheme. Moreover, it provides a clear illustration of the degrees of difference that might be involved between the multiple derivations constitutive of a robustness scheme.

   The case concerns bacterial endocytosis in mammalian cells. Endocytosis is a process by which cells engulf and absorb external material such as proteins and bacteria. The question at issue is how large these external "particles" can be. In 2005, some scientists claimed to have shown experimentally, by fluorescence microscopy, that endocytosis can involve particles of a very large size. This claim was in tension with the theoretical beliefs at the time. Many scientists reacted with skepticism to it. They asked for further experiments that use different techniques from fluorescence microscopy. This amounts to a demand for robustness, in a situation where the experimentally derived results challenged previous theoretical postulates about the size of the particles involved. The demand for robustness is a demand that the results of both practices of fluorescence microscopy and electronic microscopy be brought in line with one another, that is, that they converge toward a common result.

The case study presents several interesting features in addition to the abovementioned ones. For one thing, it is an investigation of ongoing research that is still open. Trizio's chapter analyzes a contemporary scientific practice in realtime, thus avoiding the risk of a posteriori reconstructions of robustness schemes that ignore the uncertainties or forget the messiness of discordant evidence. It is moreover a fascinating example of broadly wimsattian robustness worries manifesting themselves in the heated action of scientific research. The demand of robustness appears as an explicit criterion for the acceptance of experimental results: the referees of the journal *Nature* ask for it to be satisfied and reject the paper that lacks it.

A curious feature of this case is that the old understanding of the limit on size of particles that can be absorbed by means of endocytosis had gained a kind of theoretical status, informing much other research, despite its not being part of a well-confirmed theoretical mechanism or model in biochemistry. Rather, its provenance is related to technical contingencies: previously used detection techniques never turned up cases of large particles being absorbed. Some of the scientists involved in the debate therefore regard it as a "dogma" produced by a contingent prior history of observation based on older detection methods.[24]

After laying out the endocytosis controversy, Trizio applies to it an adapted version of Nederbragt's classification of the experimental derivations according to the kind of difference and degree of independence they manifest (see above Section 1.8.1). As a result, three kinds of experimental derivations appear to be involved in the historical situation. Some of the experimental actions are categorized as variations in the parameters of the experiment without essentially altering it. Other experimental actions are analyzed as cases of significant differences in experimental techniques but without altering the underlying theoretical principles. In such a case we are intuitively inclined to say that different but not independent derivations are involved. Finally, a third configuration is also found, which corresponds to Wimsatt's definition of robustness: experimental

[24] If they are right we can suspect (although this is not articulated in Chapter 4) that there is a kind of historico-social entrenchment in operation here. Wimsatt's own analysis suggests that entrenchment possesses a historical dimension. On social entrenchment, see Chapter 5.

actions for which the theoretical principles underlying the experiment and its techniques are "entirely different" (for example fluorescence microscopy and electron microscopy).[25] In that case we are inclined to say that genuinely independent derivations are in presence. Trizio provides diagrammatic symbols to distinguish these three cases, and using them, he proposes a schematic arrows-nodes representation of the situation which immediately and very clearly shows in what respects this case is more complex than the Wimsattian configuration.

Although Trizio bases his analysis on a particular case, it would seem to generalize quite easily. As he says, "This complexity is likely to be widespread across different scientific disciplines, and the neat, idealized convergence described by the classical robustness scheme should be an exception rather than the rule (. . .)". Most often, the wimsattian scheme is useful as a regulative ideal or (in Trizio's suggestive phrase) as a "methodological attractor" for practitioners.

B. Like Trizio, **Hubertus Nederbragt**, "**Multiple Derivability and the Reliability and Stabilization of Theories**," discusses the problem of robustness in the biomedical sciences. His central case study is the invasion of cultured cells by bacteria, but he employs many other examples.

Nederbragt begins by distinguishing three logical situations that are often lumped together: consilience of inductions, multiple derivability, and triangulation. He argues that these serve different purposes in scientific research. He spells out the differences he sees, then focuses on multiple derivability and triangulation in his discussion of cases.

Multiple derivability and triangulation are two different robustness strategies. More precisely, both are *inductive* strategies for the purpose of theory improvement. Both strategies make the theory more reliable when they succeed. The difference lies: (a) in the degree of generality of the targeted theory at stake: for multiple determination, local theories (for example a specific theory of bacterial invasion applied to the limited domain of particular types of cultural cells); for triangulation, "interdisciplinary theories or theory complexes", which "may even lead to new disciplines". (b) In the nature of the independent derivations involved: theoretically and technically independent methods for multiple determination; theories for triangulation.

With this categorization, Nederbragt adopts a narrower, more restricted sense of "multi-determination" than the one involved in Wimsatt's terminology (or in my own taxonomy as presented above in this introduction – not surprisingly since my taxonomy has been built from and in continuity with Wimsatt's). In Wimsatt's framework as well as in my conceptualization, Nederbragt's triangulation would be described as a case of multi-determination. Both Nederbragt's multi-determination and Nederbragt's triangulation would instantiate a robustness scheme defined as 'convergence under multiple determinations'. Indeed, in both cases we have several taken-as-solid derivations-arrows converging on one and the same result R taken-as-robust in virtue of this structural configuration.

[25] For problems of individuation that arise here, see Chapter 9.

The difference lies in the ingredients of the scheme (and hence in the aim of the strategy if we consider the situation dynamically): *R* refers either to a local theory or a theory complex; and the arrow-derivations refer either to material-experimental methods or to intellectual-theoretical derivations.

Nederbragt's chapter continues with an analysis of the independence of the derivations involved in robustness strategies, and develops in this context a hierarchical classification of different types of derivations and robustness strategies (summarized and commented above Section 1.8.1). "A hierarchy of various types of derivations may be discerned in scientific practice", Nederbragt writes, from very particular ones to more and more general ones, which, respectively, lead to more and more robust derived theories. Briefly put: "a hierarchy of derivabilities" is conceptualized into a "hierarchical model of theory making".

Finally, Nederbragt's chapter discusses what might happen in historical situations in which robustness strategies like multiple determination and triangulation fail to work, either because no more than one derivation is available, or because the multiple available derivations are too weak to achieve genuine robustness. In such situations there are schematically two possibilities according to Nederbragt. In the first, the *X* that was a candidate to robustness is finally abandoned or rejected as fragile. In the second, the *X* that was a candidate for robustness is stabilized through "social anchoring", that is, "because of social interaction between method, theory and scientists of the research group". The contrast between the two possibilities leads Nederbragt to introduce a distinction between reliability and stability. An element *X* of scientific practices can be historically stabilized because of different kinds of factors: different "ways of anchoring of knowledge may be distinguished, leading to its stabilization". Reliability (and robustness is a case in point) is stabilization "by epistemological arguments and factors" or "epistemological anchoring". But stabilization can also occurs "by social, i.e. non-epistemological, factors". As a consequence, stability is a more general category than reliability. According to Nederbragt, all kinds of combinations can be found among actual historical cases: "Theories may be reliable but not stable because they do not fit in a reigning paradigm. (. . .) Theories may be stable, although they are epistemologically unreliable."

C. In **"Robustness of an Experimental Result: The Example of the Tests of Bell's Inequalities", Catherine Dufour** case study concerns experimental tests of the inequalities following from Bell's theorem of 1964. This theorem states that if standard quantum mechanics (SQM) is correct, then quantum entanglement experiments of the kind first proposed by Einstein, Podolsky, and Rosen in 1935 should show that physical nature violates the inequalities in specified circumstances; whereas if local hidden-variable theories (LHVT) is correct, then nature should satisfy the inequalities. So the experimental tests of Bell's inequalities amount to tests of SQM over LHVT (or Bohr against Einstein as it is often said).

Dufour argues that even the apparent convergence of the so many experimental outcomes supporting SQM over LHVT is not quite enough to meet the wimsattian robustness ideal.

Over the past several decades, physicists have performed a long series of such tests of gradually increasing sophistication. However, two notable loopholes remain: the so-called "locality loophole" and the "detection loophole". The locality loophole can be closed only by guaranteeing that the two separating 'particles' have no means of 'communicating' with one another, even by a speed-of-light signal. The detection loophole is a specific version of a general problem that affects many kinds of experiments in the sciences. In a typical test of Bell's inequalities, only a sampling of the particles (usually ranging from 20 to 80%) are actually detected.

While physicists and technicians have made significant progress in closing both loopholes, it is exceedingly difficult, perhaps even impossible, to close both at once. Dufour emphasizes that even the most recent sophisticated experimental designs still fall short of the EPR ideal. Many, perhaps most, physicists who care about this issue believe that the tests decisively support SQM, but doubters remain able to contrive LHVT arrangements that evade this conclusion. Leading physicists themselves disagree about how much robustness is enough (that is, about the desirability or the necessity of supplementary experimental derivations). Accordingly, the robustness debate remains somewhat open in this case. In other words, it would be premature, according to Dufour, to conclude that LHVT has been empirically refuted, because questions of robustness remain unresolved. An element of fragility remains.

The analysis of the case shows how judgments about the robustness of an experimental *result* are dependent on and intertwined with judgments about the solidity of the *derivations*.

In such a case we can ask why, despite the concordance of the so many experimental outcomes obtained, physicists did not stop and a so high number of experiments have been performed. As noted by Dufour, part of the answer lies in the importance of the theoretical implications for our scientific worldview. Beyond the particular case, the general moral is, I think, that the number of derivations deemed sufficient to provide enough robustness to a result on which they converge depends on contextual factors, 'external' to the robustness scheme itself (here the circumstance that the experimental result is perceived as especially important).

Another interesting feature of the Bell test experiments is that one experimental derivation *against* SQM was developed but never published. Even its authors (Holt and Pipkin) rejected its conclusion. This result has been set aside almost as if it never existed. The methodological-philosophical question of how to handle 'outliers' is a pervasive problem in the sciences. Given the universal significance of the debate between SQM and LHVT, the problem is all the more interesting in this case.

Finally, the historical situation under scrutiny exemplifies a non-prototypical robustness scheme, non-prototypical but interesting because it is so frequently instantiated in the history of science. It is a situation in which the multiple experimental derivations involved in the temporal sequence are thought as several classes of successive generations of improved experiments of one and a same kind. Although beyond the scope of Dufour's chapter, she provides material toward a future, finer-grained reflection on the kinds of independence required in a solidity scheme. Clearly, successive experiments thought as improvements of one and a same kind of experiment are not independent in the same sense as multiple experiments of different kinds. Nevertheless, there is a sense in which the experiments involved in the first configuration are considered as independent (this sense is related to the "empirico-genetic independence" introduced above Section 1.8). One manifestation of this independence is that an improved experiment does not completely 'cancel' the anterior ones, as if just the last one counted. Although the new and old experiments are clearly not on the same footing, although the new-and-improved one is clearly more important and compelling than the preceding ones, the fact that *two* (or more) such experiments led to the same conclusion counts for more than does the last experiment in the series, standing alone. There is a gain provided by the very fact of a convergent multiplicity. In a sense the more sophisticated (and often more precise) experiments later in the series confirm the promise indicated by the earlier, cruder efforts. Scientists get the sense that the entire series is thus 'on the right track.'

D. In Chapter 7, **Catherine Allamel-Raffin and Jean-Luc Gangloff**, informed by the ethnographic work of the first author at the Harvard-Smithsonian Center for Astrophysics, analyze in detail the argumentative structure of a single, recent (2001), frequently cited radio-astronomy paper. The scientific paper presented two large maps of the CO distribution in the Milky Way Galaxy and the argumentative justification of their accuracy.

As the title, "**Scientific Images and Robustness**" of their contribution suggests, Allamel-Raffin and Gangloff argue that images and other nonverbal features of scientific articles play a more than illustrative role. Their second major claim is that the procedures employed by the radio astronomers to convince themselves and their readers that the Milky Way maps are accurate satisfy Wimsatt's strategy for achieving robustness.

The originality of their case study consists in showing that nonverbal entities – Images, maps, diagrams, etc. – can be the objects of robustness claims, that is, the conclusions of converging series of arguments, each constituting a derivation in the sense defined in this introduction. Moreover, these derivations sometimes themselves employ images as well.

It is noteworthy that the scientific paper in question is only ten pages long yet contains twenty images of various kinds. Allamel-Raffin and Gangloff emphasize the importance of images in the economy of scientific argumentation. A map containing many points, for instance, is worth more than a thousand words, for the information it contains could only be expressed in terms of an incredibly large number of sentences, in which form it would hardly be intelligible.

All this led the authors to see Wimsattian robustness analysis as a bridge between philosophy and the sciences. They conclude that

> (. . .) the concept of robustness analysis (which is in fact a philosophical concept) gives a perfect account of the procedures used in the day-to-day activities of a lab to prove scientific assertions. (. . .) With the robustness concept, we have a perfect example of a 'working' concept that can build a bridge between the scientists and the philosophers.

E. Finally, in Chapter 8, **"Are we still Babylonians? The structure of the foundations of mathematics from a Wimsattian perspective," Ralf Krömer** discusses an important but rarely investigated issue, namely, whether and how robustness is involved in mathematical practice. Does robustness analysis have anything to contribute to this domain?

A preliminary indication that it does is the fact that several important mathematical results have been proved in multiple ways.[26] To be sure, in some cases the importance of a mathematical proof lies in the connection it forges between two different domains. But in some of these same, as well as other, cases, additional proofs apparently add solidity to the theorem asserted and, indirectly, to the methods by which it was previously proved.

Yet, as Krömer indicates, according to "the mainstream view of the epistemology of mathematical proof", the answer to our opening question seems to be a clear 'no'. This is because a mathematical proof, typically conceived as a deductive proof from a set of explicit axioms and previously proved theorems, is taken to be absolutely certain and inescapable – quite in contrast to inductive inferences in the empirical sciences. If you possess such a proof, the resulting theorem is necessarily established, once and for all, irreversibly. Thus no additional support is needed: the maximum is already achieved. Adding anything else would be wasted effort.

If this conception of mathematics does exhaust the practice of mathematical demonstration, then a robustness strategy, defined as developing multiple independent mathematical derivations of one and the same result, gains no foothold in mathematics. Robustness would neither describe a desired aim of real mathematical practitioners, nor would it be a fruitful site for the epistemological analysis of mathematical practice. Thus we could point to an essential difference between demonstrational practices in the formal sciences on the one hand and in the empirical sciences on the other hand. And, as Krömer notes at the beginning of his chapter, that is precisely the traditional conception of these two enterprises.

Krömer's contribution clearly shows that this common view, at the very least, does not exhaust the situation, which turns out to be more complicated and interesting than anticipated. He advances several challenges to the mainstream view. First, as soon as one examines actual practices rather than traditional idealizations, it appears, as a matter of fact, that mathematicians do indeed strive to re-prove theorems. And when the analyst of science asks why they do so – as

[26] More generally, students of history should be curious why, even in wider contexts, such as 'proofs' of the existence of God, thinkers thought it necessary to provide more than one proof, each one supposedly decisive in itself.

John Dawson did independently of any connection to the robustness issue – one important part of the answer involves a robustness scheme essentially similar to the one valued by practitioners of the empirical sciences. In fact, the idea of an absolute necessity attached to mathematical proofs has sometimes appeared to be an illusion. As the history of mathematics shows, "Proofs can be false, and errors can pass undetected for a long time". Thus mathematicians historically and today experience *degrees* of confidence in mathematical derivations, hence their search for different derivations that, taken together, would enhance the confidence in a given mathematical result. As a consequence – and as already suggested by Wimsatt (1981), himself inspired by Richard Feynman – the edifice of mathematics is less "Euclidean" and more "Babylonian" than is commonly thought. In other words, there exist over-connected mathematical structures: structures in which many propositions are at the centre of multiple derivations; in which multiple, non-hierarchical paths are possible from one proposition to the other; in which no special set of propositions is identified as *the* ground of the whole edifice.

From this opening line of thought, Krömer draws a first – in my opinion very important – conclusion with respect to the desirable re-orientation of the philosophy of mathematics: "we might (and should) be led to feel the need for a more subtle, and more appropriate, epistemological conception of mathematical proof and its role for conviction (eventually making use of the concept of robustness)."[27] Just as many philosophers now agree that good philosophy of the empirical sciences requires close attention to actual scientific practices, so, philosophers of mathematics need to pay more attention to the actual practices manifested in mathematical work-in-progress.

Second, there are interesting mathematical propositions for which mathematicians do not possess a formal deductive proof and for which they doubt that they will ever have one. Typically, such claims are either taken to be independent of a preferred axiomatic base or else (in some cases) being considered as an addition to an existing base, whence worries about the consistency of the system arise. Krömer considers two mathematical propositions of this kind, one in the well-known context of century-old set theory, the other in the context of a less well-known and much younger mathematical framework, namely category theory.

What processes could secure, or at least enhance, the reliability of such propositions in the absence of deductive proofs? Schemes akin to robustness are, or could be, involved in the answer. In the case of set theory, Krömer considers an answer proposed by Bourbaki: the proposition can be taken as robust in the absence of a deductive proof since it has been applied so many times in so many branches of mathematics without producing any contradiction. Krömer discusses

[27] In this respect, see Van Bendegem et al. (2012). Krömer's conclusion just above exactly expresses the spirit in which this book, entitled *From Practice to Results in Logic and Mathematics*, has been impulsed, following a Conference organized by the PratiScienS group and myself in June 2010. See http://poincare.univ-nancy2.fr/PratiScienS/Activites/?contentId=6987.

the similarities and differences between such a proposal and the Wimsattian robustness scheme as the latter applies to the empirical sciences. He also points to difficulties, some of which are specific to the mathematical field, and some shared with empirical science (for example, the hard problem of the independence of multiple derivations). In the case of category theory, where a solution à la Bourbaki is unavailable because the theory is still in the early stages of its development, Krömer furnishes reasons why the search for multiple derivability could be a valuable strategy for mathematicians. This is an especially important suggestion, given the present situation in which category theory presents us with foundational problems yet is employed as the only available demonstration of some important propositions.

1.9.3 Chapter 9: A Systematic Panoramic Analysis of the Robustness Notion

In Chapter 9 **"*Rerum Concordia Discors:* Robustness and Discordant Multimodal Evidence", Jacob Stegenga** offers a systematic and rigorous panoramic analysis of the robustness idea and the main difficulties associated with evaluating robustness, and provides multiple illustrations relevant to diverse empirical sciences. Hopefully, such an analysis will be better understood, it's relevance and usefulness will appear more clearly, once having in mind the multiple concrete cases considered in the previous chapters.

After having investigated some uses of "robustness" in the philosophy of science literature, Stegenga builds his own general definition and terminology: "A hypothesis is robust if and only if it is supported by concordant multimodal evidence." In this definition, "hypothesis" is used in a broad, generic sense, intended to cover all kinds of scientific knowledge components about which robustness can be predicated: experimental results, statistical analyses, models or any kind of scientific method. In the same spirit, "multimodal evidence" aims to encompass a great diversity of "way[s] of finding out about the world": human sensory modalities, experiments, statistical analyses, comparative observations, mathematical models, heterogeneous techniques and so on.

Beyond the general definition, "robustness-style arguments" will possibly differ in many respects: with respect to the kind of modes and the kind of hypotheses they involve; the number of different modes involved; the strength of the evidence that each provides; the kind and degree of independence that the multiple modes, considered together, show; the quality of the concordance they manifest; and how to handle the existence of discordant evidence. With the elucidation of these different variables, Stegenga greatly clarifies what robustness-style arguments are made of, and exhibits by the way all the complexity of the evaluation involved. Moreover, he distinguishes robustness from "Another way in which multimodal evidence is said to be valuable", called "security", which "is the use of one mode of evidence to support an auxiliary hypothesis for another mode of evidence, which is itself evidence for the main hypothesis of interest".

Stegenga points to the hopes invested in robustness-style arguments and lists the numerous valued epistemic tasks that they have been credited to accomplish (see above Section 1.1). But immediately after, he stresses that these hopes and tasks are in need of philosophical analysis and justification. And it is clear from Stegenga's incisive critical examination that many of them raise questions, to say the least. Stegenga articulates several difficulties that I take to be deep and often ignored or minimized.

Although it is indeed a "hard problem", I pass rapidly over what Stegenga presents, rightly I think, as two empirical facts, namely, that multimodal concordant evidence is rare and that most of the time multimodal evidence is discordant, producing various sorts of incongruity or outright inconsistency. Rather, I shall concentrate on the analytical difficulties.

First, when we attempt to get beyond vague intuitions, it is not at all easy to specify what multimodal evidence is. What defines an individual mode as *one* mode and distinguishes it as sufficiently independent from another one? Stegenga calls this difficulty "the *individuation problem* for multimodal evidence". We could call it as well the independence problem. As already noted Section 1.3, this problem is crucial since its solution determines the *number* of different modes (or derivations-arrows) involved in the robustness scheme associated with a given historical situation, and since the evaluation of this multiplicity is the first logical step of robustness assessments. (I speak here of a 'logical step', not 'temporal step', because I think that, in practice, the different difficulties analytically distinguished by Stegenga are not treated one after the other but mixed all together). This is the problem of describing the nature of independence, as well as of justifying, on the one hand the degree of independence the modes should possess in order to count as providers of robustness (total? partial?), and on the other hand the criteria according to which the independence between modes can be assessed. To belong to independent modes, is it sufficient that the arguments differ only in a single background assumption, while sharing the rest? At the other extreme, must they have no background assumptions in common? Or is it enough that they differ in at least one problematic assumption? These are the kinds of questions that must be addressed. In addition to the *choice* of the criteria, their *application* to concrete cases presupposes that we are able to *recognize* all the background assumptions involved in the configuration under scrutiny, which is a strong – and I would say, in agreement with Stegenga, not very realistic – presupposition (just think of the related problem, to which Pierre Duhem called attention, of identifying all the auxiliary assumptions involved in a typical experimental test).

I see the independence problem as a part of a second, more general problem put forward by Stegenga, "the amalgamation problem" ("*how* multimodal evidence should be assessed and combined to provide systematic constraint on our belief in a hypothesis"). This is a balance problem, the solution to which would determine a global judgment about the robustness of the hypothesis under discussion. Stegenga sees the task as the determination of an “amalgamation function” and sketches what its task would be:

> [A]n amalgamation function for multimodal evidence should do the following: evidence from multiple modes should be assessed on prior criteria (quality of mode), relative criteria (relevance of mode to a given hypothesis) and posterior criteria (salience of evidence from particular modes and concordance/discordance of evidence between modes); the assessed evidence should be amalgamated; and the output of the function should be a constraint on our justified credence.

One important problem facing the task of constructing such an amalgamation function is that, as a matter of fact, practitioners often disagree about these evaluations.

> The basis of many scientific controversies can be construed as disputes about differential assessments of these desiderata: one group of scientists might believe that evidence from some techniques is of higher quality or is more relevant to the hypothesis or has greater confirmational salience than other techniques, while another group of scientists might believe that evidence from the latter techniques is of higher quality or is more relevant or salient.

Stegenga seems to suggest that this kind of problem might be overcome, when he concludes: “The construction and evaluation of such schemes should be a major task for theoretical scientists and philosophers of science.” I am not so confident and suspect, rather, that there is no such general and systematic function. I cannot see how we could get rid of the intuitive, judgmental, non-computational character of global estimations of scientists and the historical fact of individually varying appreciations regarding these matters. Although Stegenga’s analysis is immensely helpful with respect to our conceptualization of the robustness problem, and although such general schemes as the Wimsatt robustness scheme are analytically clarifying, I am afraid they will not give us anything like a general, uniform decision algorithm. In my view, individually-variable intuitions and assessments are an ineliminable part of real scientific practices.

1.9.4 Chapters 10 and 11: The Solidity of Derivations

A. **Léna Soler’s** chapter,[28] “**Robustness of Results and Robustness of Derivations: the Internal Architecture of a Solid Experimental Proof**”, analyzes in detail an influential scientific paper usually considered as one of those which have contributed to the discovery of weak neutral currents (one of the developments discussed by Pickering in *Constructing Quarks*). The chapter is based on work done at Gargamelle, the giant bubble chamber at CERN. This work was important, since the discovery of weak neutral currents (weak NCs) was consistent with the so-called Standard Model of Glashow, Salam, and Weinberg, the particle theory that unites the electromagnetic with the weak and strong nuclear forces. Soler’s focus is on derivations more than final results, that is, on the “argumentative line” that leads to the result. The Gargamelle

[28] This chapter is here presented by Thomas Nickles.

work was one of three different experimental argumentative lines that converged to robustly constitute the 'discovery' of weak neutral currents in the early 1970s, the other two coming from Aachen and Fermilab National Accelerator Laboratory. Each of these argumentative lines is represented by a single arrow in Wimsatt's scheme, with the arrows converging on the same result: neutral currents exist.

Soler's strategy is to take Wimsatt's robustness "panoramic" scheme as her starting point and then to zoom in on the Gargamelle arrow, with the intention to look at the detailed practices necessary to produce it. This investigation shows that, while Wimsatt's scheme remains useful as an idealization, appreciation for the detailed scientific practices involved in realizing it (themselves frequently employing that scheme in micro-contexts) somewhat undercuts the common claim that robustness considerations diminish the contingency of scientific work to the point that we can regard mature scientific results as inevitable. In short, there remains a leap from 'robust' to 'true'.

Soler shows that, as we zoom in on the Gargamelle arrow, we find that this simple representation conceals a multitude of black boxes or modules involving solidity/robustness arguments, some inside the others like a set of Russian dolls (although with interactions among them). Her purpose is to open these black boxes. The resulting story (which she insists is still greatly simplified) becomes a dizzying spiral of complex stories within stories, reaching back into scientific history, and shows how naïve are familiar philosophical accounts of scientific experimentation and the corresponding formal confirmation theories. Although introducing levels of complexity hidden from most philosophers in their accounts (and from some of the scientists themselves), Soler points out that her analysis is still located at "a level of scientific practices that is emergent with respect to laboratory practices themselves."

The Gargamelle experiment involved making and interpreting photographs of particle interactions inside the large bubble chamber. The general point to be made here is that the sought-for weak neutral current reaction, $\nu + nucleon \rightarrow \nu + hadrons$, where ν is a neutrino, was very far from something 'given' by observing nature. Soler outlines the constructive steps and expert judgments required at every step of the way. The raw data consisted of 290,000 photographs of events of possible interest. But how was this data to be analyzed and interpreted? One problem is that neutral particles leave no tracks in bubble chambers or on film, and neutrinos are neutral. Another (related) problem, the neutron-background problem, was to distinguish pseudo-neutral-current events from those produced by high-energy neutrons (also neutral particles). Handling this difficulty required a problem shift: to determine the *ratio* of NC events to charged-current events. And this is just the beginning of the story. To make sense of the complexities, Soler provides an architectural metaphor with accompanying figures: four floors of data analysis.

The story of the work done on each of these 'floors' is, again, quite complicated, with robustness considerations often central. For example, the ground floor involved four kinds of data filtering, one of them being an energy "cut" at 1 GeV to weed out many pseudo-events among the tracks too ambiguous to decipher.

> The filtering operations aim at eliminating some confusions, but they can themselves be sources of mistakes. For example, if the energy cut at 1 GeV is too severe, real NC-events might be artificially eliminated, and the risk is to conclude mistakenly that weak neutral currents do not exist. But if the cut is too permissive, too many pseudos might be taken for authentic NC-events, and the risk is, this time, to conclude mistakenly that weak neutral currents do exist.

So the attempt to make the argumentative line solid can actually introduce new elements of fragility (a theme that Nickles takes up in a different context). A kind of compromise or equilibrium is sought that makes the result solid *enough* to proceed.

On the third floor, the way scientists dealt with another problem, the muon noise problem, looks at first like it fits the Wimsatt model of robustness, with three lines of argument converging on the final conclusion that then became input data to the next level of analysis. But, as is typical in actual research, the three results were not identical. Finding sufficient concordance required further work (which in this case is relatively invisible but nevertheless present), work involving what Soler calls "calibrating re-descriptions" and inferential moves, based on scientific judgment, that were not logically obligatory. Here Soler's chapter intersects Stegenga's and others. Again at this level of description, the concordance of results is not simply "given." Rather, it must be constructed. There are severe constraints on these constructions, to be sure, but there is also room for flexibility.

Soler concludes that the solidity of a derivation consists of two components. One is the internal solidity of that argumentative line, based on the remarkable amount of detailed work from which it emerges. The other is its relation to external or extrinsic circumstances such as the existence of other lines of argument. Here the solidity comes from something like Wimsatt's elementary robustness scheme rather than from the internal detail of a single argumentative line.

In the final sections of her chapter, Soler points to the implications of her work. One, already mentioned, is the role of expert judgment. She clearly believes that formal algorithms are insufficient to model scientific practice. "To account for such judgments (as far as this can be done), the philosopher will have to take into account the particular content to which the robustness skeleton is associated in each case." This point applies also to any attempt to include an algorithmic version of Wimsatt's scheme in a formal confirmation theory. There is no getting around the need for expert judgment – human decision, human agency – at many stages of research (again, an intersection with Pickering's chapter).

The most important implication, in her eyes, is the probable historical path-dependence of science and the resulting contingency of even the most mature scientific results. Soler's picture of the development of science, which she believes the Gargamelle case supports but does not prove, is that at many,

many points in their research, scientists manage to establish some degree of stability, a kind of equilibrium, among the multitude of constraints and contingencies they face; yet we can easily imagine that their decisions at various points might well, and perfectly legitimately, have been different. Since these historical decisions become planks in the platform of ongoing science, their influence ramifies through all future work that depends upon them (here some generative entrenchment is at stake). The contingent nature of previous decisions and the reservations then felt are largely forgotten, concealed by later work, erased. Just as Stephen Jay Gould imagined that the tree of life on earth would look different each time the tape of biological evolution were replayed (a image that Pickering himself invokes in his chapter), so Soler imagines that the tree of scientific developments would look different than it does now, given our same universe and equally good science, if the tape of the historical evolution of science were replayed.

Soler takes the Gargamelle case to be emblematic of sophisticated scientific research. Insofar as this is true, her analysis appears to undercut the inference from robustness, and solidity more generally, to historical inevitability. A related issue is scientific realism. Beginning with Campbell, Levins and Wimsatt themselves, robustness and its relatives such as triangulation have often been made the basis of an argument for scientific realism, along the lines of the so-called miracle argument. According to this argument, were realism false, it would be a miracle that these independent lines of research should lead to exactly the same result. Soler argues here (and above in this introduction, Section 1.8) that the kind of flexibility-exploiting construction involved in making Wimsattian robustness claims possible in the first place undercuts this argument for strong realism. "(. . .) if there is a 'miracle' here, it seems to be of a different kind than the one involved in the realist argument". She refers to the perhaps surprising fact that scientists are so often able to establish the aforementioned stabilities within the Jamesian "blooming, buzzing confusion" of historical contingencies and constraints. But once we appreciate the amount of co-adjustment involved in somewhat forcing results to cohere, the sense of miracle begins to dissipate. As Kuhn put his version of the point, to some degree normal scientists have to "beat nature into line." This point feeds back into the contingency thesis.

B. **Frédéric Wieber's case study ("Multiple means of determination and multiple constraints on construction: robustness and strategies for modeling macromolecular objects")** examines the emergence and stabilization of a new scientific practice in protein chemistry, namely, a procedure developed in the 1960s and 1970s for modeling the structure of proteins. The problem is a formidable one, for proteins – those building blocks of all the life forms that we know – are exceedingly complex macromolecular objects, so complex that their structure must be described at four different, interacting levels: the amino acid sequence, the regular subunit arrangement (as in the alpha helix), the three-dimensional folding that is so crucial to chemical function, and the higher-order organization of multiple proteins (dimers, trimers, etc.). In principle, quantum theory can explain the bonding, protein folding, and so on; but the application of quantum theory was (and largely remains today) so far beyond our computational capability as to be completely intractable.

Given that human scientists are limited beings with limited resources, how did they go about tackling the problem of modeling proteins back in the 1960s? Why did the protein chemists develop, and how did they stabilize, a particular modeling strategy rather than pursue other options? What did it take to convince the community that a particular modeling procedure was reliable, and how well do the scientific practices involved fit Wimsatt's robustness analysis versus an alternative account of solidity?

Like Wimsatt himself, Wieber begins from population biologist Richard Levins' choice of modeling strategy from the 1960s, but Wieber eventually concludes that the solidity of the protein modeling procedure has more to do with Wimsatt's concept of generative entrenchment than with the robustness scheme. What is involved is "a mutual and iterative adjustment" of "the three limited resources" available to proteins scientists, namely "theoretical, empirical and technological" (especially computational) resources. Levins had argued that it is humanly scientifically impossible to provide population models of complex biological systems that are equally faithful to the demands of generality, realism and precision. Any model must partially sacrifice one of these dimensions. Levins also argued that robust results may nevertheless be obtained by considering multiple models (by varying parameters) and looking for convergence of results. As he famously summed up this strategy, "Our truth is the intersection of independent lies."

Since the investigation of protein structure in the 1960s and 1970s presented a similar challenge to scientists, Wieber uses Levins' framework as a tool to get a clearer understanding of the protein case. He shows why and how, in this case, scientists deliberately sacrificed the representational accuracy of their model to practical, computational imperatives, while nevertheless maintaining the hope that sufficient accuracy of the relevant predictions derived from the model would emerge from their work. Scientists used a patently inaccurate (by their own theoretical standards) formula that they had to feed with a high number of empirical parameters in order to build the model of one particular protein. This is the theoretical side of the model. Moreover, since the parameters are different from one molecule to the other, and since only a few of these parameters for a restricted number of proteins were experimentally determined (sometimes with discrepancies from one study to the other), scientists had to "exploit creatively" the existing empirical resources in order to estimate the unknown values. They did this by means of extrapolations and analogies from one kind of molecule to the other, and then, for each particular molecule, by adjusting the different parameters to one another in order to produce the so-called "force field" that (unlike single parameters in isolation) has chemical meaning. Next, these structural hypotheses had to be tested against the data. This is the empirical side of the modeling procedure. Finally, in order actually to use a given protein model, the scientists had to minimize the potential energy of the molecule, by intensively calculating a large number of conformations. Had computers not been available, the task would have been out of reach – which shows the historical dependency of this modeling procedure on the development of computer technology. This is the technological, computational side of the model.

Even with the new computational resources, the scientists remained almost overwhelmed by the complexity of protein chemistry. (This provides another illustration of Wimsatt's point that philosophers of science must treat human investigators (and communities of same) as limited beings, far from perfectly rational, let alone omniscient. Accordingly, they must make compromises and develop special methods for dealing with them. That is one of the reasons why the neglected issues of robustness and of solidity in general are so scientifically important and so philosophically interesting).

Wieber's case illustrates and clarifies the central role often played by computers in the solidification of a modeling procedure. In his protein case, recourse to computers greatly improved computational efficiency, but not only that. More fundamentally, it also modified the very content of the modeling procedure. For storing data in large databanks and partially black-boxing programs in the form of computer packages made available an increasingly large number of common parameters to more and more specialists. As a result, a more and more systematic and uniform process for the choice and estimation of the different parameters emerged and crystallized, replacing the human skill-dependent and more heterogeneous local solutions. Correlatively, an enlarged community of scientists began to use the procedure to interpret their experimental data about proteins in terms of molecular structures. This contributed to the refinement and optimization of the model parameters themselves through a back-and-forth movement between the experimental results obtained for more and more molecules on the one side, and the improvements introduced in the data base and computer program packages on the other side. As Wieber writes, "The computerization of the procedure of modeling is then really fundamental: with more and more models [of new proteins] constructed and effective calculations executed, scientists have been able to increasingly test the results produced against empirical data in order to *iteratively optimize* the parameters chosen for modeling."

1.9.5 Chapters 12, 13 and 14: Robustness, Scope, and Realism

The three last chapters widen the scope with respect to the robustness scheme *à la Wimsatt*. All three also question strong realism.

A. **Mieke Boon's chapter, "Understanding Scientific Practices: The Role of Robustness-Notions,"** deals with engineering sciences and, more broadly, with scientific research in the context of practical and technological applications. Boon is especially concerned with "the production and acceptance of physical phenomena as ontological entities; the role of instruments and experiments in their production; and the rule-like knowledge that is produced simultaneously".

Her aim is to understand what robustness might mean and how robustness is achieved in such scientific practices.

One overarching thesis of the whole analysis is that it is illegitimate to jump from robustness attributions to truth or reality attributions. "robustness, in the sense of multi-determination, cannot function as a truth-maker". What scientists achieve when they are successful is reliability. Hence a philosophy of science that cares about actual scientific practices must replace truth by reliability. Nevertheless, Boon endorses a minimal metaphysical realist belief, required, according to her, "in order to explain why scientific results can travel to other scientific fields or technological applications". This minimal realism implies the existence of an external, independent world that "stably sets limits" "to what we can *do* with it and to the regularities, causal relations, phenomena and objects that can possibly be determined". This realism is "minimal because it avoids the idea of a cognizable independent order or structure in the real world".

Boon distinguishes several "robustness-notions" that correspond to different uses of the term "robustness". According to a first, important use, "robust" is applied to the independent world as a whole. Here "robust" means "real", "stable" or the like. It points to an existing something which is supposed to be what it is once and for all, to resist us and to impose constraints on what we can do and think about it. This "robustness-notion" is a metaphysical category.

A second crucial robustness-notion that Mieke Boon applies to scientific practices points to the presupposition (when this sort of robustness obtains) that the same initial conditions will be followed by the same final conditions. Here, to say that our scientific practices are "robust" means that they are governed by the principle "Same conditions – same effects". This assumption has the status of a regulative principle: scientists cannot prove it, cannot "find out whether this principle is an empirical or metaphysical truth"; but practitioners indeed assume it and need it as a "condition of possibility" and as "a guiding principle" of any scientific research in the empirical sciences.

This latter assumption (robustness in the sense of "Same conditions – same effects") is related to the former assumption that there is an independent, real, stable world (robustness in the sense of reality and stability) imposing constraints on our experimental activities. But these regularities and reproducible connexions are not given as such, for they must be explored and constructed as technological achievements. "Experimental interventions with technological devices will (...) produce knowledge of conditions that are causally relevant to the reproducible production of a phenomenon described by $A \rightarrow B$, which is presented in 'rule-like' knowledge in the form: , unless (K and/or X).

How is this achieved? At this stage, a third robustness-notion (familiar to the readers of Wimsatt) enters the scene: multiple-derivability. Under the regulative idea of the robustness of scientific practices in the sense of "Same conditions – same effects", practitioners look for such kinds of practical recipes (my terminology). By repeating experiments, they vary their conditions and even the kinds of experiments. In this process they sometimes succeed in achieving a practical recipe of the kind "Same conditions – same effects". When this happens, the ‘something’ corresponding to the effects is said to be reproducible, stable, invariant. From this analysis of scientific practices, Boon puts forward a third robustness-notion: repetition and multiple-determination, which is a methodological category.

Now, the scientific results that, through this robust method, are built and recognized to be reproducible, stable and invariant, are also taken to be robust achievements in a different (although related) sense. In Boon’s terminology, when the scientific results involved are measured or observed physical occurrences, “robust” means “reproducible”, and when the scientific results involved are interpreted as phenomena described by $A \rightarrow B$, robust means “stable” and “invariant”. This is the fourth robustness-notion of Boon’s taxonomy. It is an ontological category, because when a result is reproducible, stable or invariant under multiple determinations, not only is it taken as acceptable, but also it acquires an ontological status.

In scientific practices, reproducibility, stability and invariance work as criteria for the acceptance that a ‘real something’ has been found. But that is still not all. As soon as an effect B has been recognized reproducible by the robust method of multi-determination, the rule-like knowledge of the form: “$A+C_{\text{device}}$, will produce the same effects, B, unless (K and/or X)” is also recognized to be reliable. Here we encounter the fifth and last of Boon’s robustness-notion. Here “robust” means “reliable”, applies to rule-like knowledge, and is an epistemological category. (This robustness notion also applies, in other ‘more theoretical’ contexts, to phenomenological laws, scientific models and even to fundamental theories – but in that latter case Boon prefers to talk about “empirical adequacy,” here borrowing van Fraassen’s expression).

These robustness-notions must be analytically distinguished, but in practice, the robustness attributions of the different kinds are essentially related and entangled: “Regulative, methodological, and epistemological or ontological criteria are used in a mutual interplay”. Robustness in the sense of reproducibility, stability and invariance works as an ontological criterion (a criterion for the acceptance of a phenomenon described by $A \rightarrow B$ as a real phenomenon: ontological robustness). It also works as an epistemological criterion (a criterion for the acceptance of the rule-like knowledge required for the (re)production of the phenomenon *as reliable* rules: epistemological robustness). These two kinds of robustness are essentially related to robustness in the sense of multi-determination (methodological robustness), since multiple-determination works as a methodological criterion for justifying the attributions of invariance (from which the ontological and reliable statuses are in turn attributed). Methodological robustness rests in turn on the regulative principle ‘Same conditions – same effects’, since the presupposition that our scientific practices are governed by such immutable regularities justifies the method of repetitions and variations as the way to delimitate such regularities. Finally, this regulative robustness is related to the metaphysical robustness, through the presupposition of the existence one real independent immutable world (a robust world in this sense) which imposes fixed constraints, and hence fixed regularities, to our experience and to what we can do and think about the world.

B. **Andrew Pickering's chapter, "The Robustness of Science and the Dance of Agency"**, is primarily driven by an ontologically-oriented interest rather than an epistemologically-oriented one. He understands the use of robustness terminology in Science Studies as a means to point to "the otherness of the world", with the intention to re-affirm and try to vindicate the existence of a non-human contribution of this world to our science. This use has to be situated in a given intellectual context. It expresses the attempt to find a viable middle way between two antagonist positions: on the one hand, the strongly counterintuitive relativist thesis that science is merely a social construction (understood as the claim that human beings can say whatever they want about the world, so that "the otherness of science vanishes"); and on the other hand, the equally strong and untenable realist thesis that our physical theories 'correspond' to a unique external world that is what it is once and for all, a conception in which scientists have no choice and science is "absolutely *other* to its producers and users".

So the question is: Does the world constitutively enter into science, how and in what sense? Or in other words: Is our science ontologically robust? Pickering's aim is to articulate his own position with respect to this "ontological sense of the robustness of science". To do so, Pickering first focuses on concrete actions and material aspects of science, convinced that "If there is a certain nonhuman toughness about scientific knowledge, it is grounded in performative (not cognitive) relations with the material world". But after having discussed the case of the first bubble chamber, he enlarges the scope: he examines the case of our conceptual knowledge about the material performances of instruments (in other words the case of experimental facts) and makes comments on the conceptual knowledge issuing from "purely conceptual practice".

At the end of the day, Pickering's answer to the initial question is the same for all of these kinds of scientific achievements, and it is positive: yes, ontologically robust are both our technological material achievements (the "free-standing machines and instruments" that "stand apart" from humans, "operate reliably" and reproduce the same performances independently of the individuals involved); and yes, ontologically robust is also our conceptual knowledge (experimental statements about physical phenomena or "purely conceptual systems"). In other words, the otherness of the world constitutively enters at all levels of our science.

Typically, the otherness of the world manifests itself, in some phases of research categorized as "phases of passivity", through material, instrumental performances. These manifestations are unpredictable and uncontrolled by humans. Practitioners do not know in advance what they will be. They have to wait for their otherness and to deal with it, and dealing with it, sometimes they succeed to obtain free standing-machines able to produce and reproduce stable material performances. This is "the primary sense in which the world enters constitutively into science – and the primary sense in which science is a robust enterprise and not a mere construction". Something similar holds for human attempts of conceptualizations: scientists cannot know in advance where this or that theoretical assumption will lead them. This motivates Pickering to nuance the metaphor of the plasticity of science he used in previous works: "at the conceptual as well as the material level the plasticity metaphor fails, precisely in that (. . .) scientists (. . .) have genuinely to find out what the upshot of that will be."

But if the otherness of the world constitutively enters at all levels of our science, the ontological robustness of our experimental recipes and scientific knowledge does not allow us to extract from this knowledge, and to contemplate apart of it, anything like a 'pure' bit of this external and independent world. The contribution of the world cannot be extracted and separated from the contribution of the human beings who make science and of the societies in which they evolve. The human and nonhuman contributions are irreducibly "mangled". "The material performance of instruments is indeed constitutive of the knowledge they produce, though prior scientific conceptualisations of the world are constitutive too, and this in an irrevocably intertwined fashion." Scientists deal with what Pickering elsewhere calls "symbioses", that is, complex structures made of irreducible intertwined human and nonhuman elements. All strive for "interactive stabilisations"; nobody knows in advance whether the material performances and the conceptual hypotheses will "fit together and interactively stabilise one another" and with the other scientific pieces already in place; and sometimes, good interactive stabilisations associated with a powerful "*machinic grip* on the world" are achieved. Here lies the manifestation of the otherness of the world and the robustness of science. But this otherness is not an absolute, "unsituated otherness", and this robustness does not mean any inevitability of the content of human knowledge.

However successful our scientific knowledge is, however satisfying is the symbiosis, however impressive is our machinic grip on the world and control on phenomena, we cannot "factor out the human side of the dance of agency": *it remains* "*our knowledge" and can never be equated with "something forced upon us by nature itself*" (emphasis added, LS). "We can indeed specify the source of science's robustness in dances of agency, especially with the material world, and in the production of free-standing machines and instruments", but the material performances always occur in the framework of some human questions, material and cultural resources, beliefs and values (all of which could have been different). The human reactions to these manifestations in the "active phases" which follow the passive ones are by no mean something that could be considered as pre-determined and unique. Different individuals or groups act differently in the same configuration and often favour different "accommodations", as the history of science so often shows. The material performances can be accommodated in different ways at each stage of the scientific development. And it happens, according to Pickering who gives examples, that several good inter-stabilisations and machinic grips are achieved, which all are *at the same time* robust *and* very different in content (sometimes even incommensurable).

So scientific development is an open, not pre-determined process, and scientific achievements of all kinds, although truly robust, are genuinely path-dependent and contingent. In the iterative process of passive and active phases, any kind of ingredient and dimension can be transformed and reconfigured: the individual scientists involved; the devices and their tangible performances; the ideas and practices of proof; the scientific forms of life; the idea of science and the place of science in our culture; as well as our ideas and commitments about the world and the kind of place the world is.

Pickering concludes that his account, by providing an alternative explanation, "somehow *defangs* realism and makes it a less pressing topic" and "undermine the intuition of uniqueness that goes along with" realism. The alternative 'symbiotic explanatory scheme' of solidity (as I would call it) is very general, if not universal (as Pickering writes: "the mangle is a sort of *theory of everything*"). It is assumed to apply to any kind of scientific practice (instrumental, experimental, purely theoretical, mixed kind of scientific practices). It is assumed to apply to all scales of scientific research (at the micro-, meso- and macro-levels). And it is moreover assumed to apply at an even more panoramic scale, since science, its knowledge and instruments have also to be in symbiosis with the rest of the society: with respect to different social values, our most beautiful theories could reduce to noise and our most efficient scientific instruments to "a pile of useless junk".

C. **Tom Nickles** opens his chapter, "**Dynamic Robustness and Design in Nature and Artifact**", by broadening the scope of robustness considerations to include apparently heterogeneous complex systems, both natural and artificial. Examples are large-scale industrial systems such as nuclear power plants and the electric power grid, information technological systems such as the Internet, but also epistemic systems such as a scientific specialty area and its products. Nickles is concerned with all "humanly constructed (explicitly or implicitly designed or engineered), evolved and evolving complex technological systems of *inquiry* and their products", especially "at innovative frontiers", where the systems in question are undergoing change at any of several levels (experimental, theoretical, instrumental, methodological, axiological). The stability of a Kuhnian paradigm could serve as an example, as we shall see.

Nickles' central question is whether we can normally hope to obtain an ever-increasing and cumulative robustness in either our epistemic systems or our complex material systems, up to something close to a zero-risk stage. And his answer is no: every attempt to increase robustness in one identified respect may, as far as we can usually know, create new fragilities in another respect. These new fragilities are often unexpected, unpredicted, in practice largely unpredictable, especially for innovative and bound-to-evolve systems. Moreover, the resulting failures can be worse than those prevented by the new robustness measures.

According to Nickles, this situation is inescapable because it is due to the very nature of the systems involved, especially owing to their complexity, high degree of connectivity, "extreme non-linearity" and designed-to-be-dynamic character.

Hence the new fragilities, accidents or failures subsequent to the robustness "improvements" should be considered as normal and endogenous, and not, as is commonly the case, as exogenous avoidable attacks from the outside or consequences of merely human errors.

Thus the "cumulative fragility-reduction thesis" or "convergent risk-reduction thesis is false when applied to epistemic systems". The hope that robustness analysis will gradually eliminate more and more errors and will bring us closer and closer to the ideal of invulnerability to major failures must be abandoned, even as a regulative ideal. Rather, we have to recognize and to prepare for "a direct coupling of robustness to fragility" and the inevitability of a "robustness-fragility tradeoff". "nothing that we can humanly do can prevent occasional, surprising avalanches of failure".

For the analysis of the robustness of epistemic systems and the argumentation of his "robustness-fragility tradeoff" thesis, Nickles exploits recent developments related to complex systems – all the more interesting for the completeness of a volume on robustness because these important resources are not represented elsewhere in the book.

Inspired by social scientist Charles Perrow's *Normal Accidents: Living with High-Risk Technologies* (1984), Nickles extends Perrow's thesis to epistemic systems while keeping also in view material and organizational networks such as the Internet and the commercial airline hub system. Perrow's qualitative risk analysis is complemented by more quantitative and technical literature on complex systems, e.g., network theory, the highly optimized tolerance (HOT) model of physicists Jean Carlson and John Doyle, and the study of system behaviors that can be described by distributions with so-called "heavy tails".

Heavy-tailed distributions are potentially relevant to robustness analysis because some such distributions signal the existence of a so-called "power law", and power law distributions have supposedly been found to characterize relevant aspects of systems of various kinds, including the distribution of failures. Insofar as this is true, it is frightening, because the incidence of failures does not drop off exponentially with the size of the failure as with a Gaussian distribution. (Earthquakes have a power law distribution.) Some investigators hope to discover mechanisms underlying these distributions that can be parlayed into a general trans-disciplinary "science of order and connectivity" or general complexity theory that would, among other things, help us understand robustness and fragility in a more theoretically sophisticated manner. Other experts are not so sanguine.

I should like to mention three other points from Nickles' chapter.

One that is very important to my eyes, but not often stressed, is that robustness must be relativized to specific kinds of potential threats or failures. It makes no sense to ask: Is a system robust (*tout court*)? We must specify: robust *with respect to what feature*? What do we want to avoid? For instance (and this also illustrates the tradeoff between robustness and fragility), an epistemic system of the axiomatic kind is robust to failures of logical entailment but is highly fragile to problems at the level of its starting principles, since any failure at this level will propagate instantaneously through the whole system and produce “a disastrous cascade of failure” and a collapse. Another example is this. (I choose this example since, at the end of the chapter, Nickles applies his ideas to Kuhn’s model of science; although my illustration is freely inspired by his developments.) If the complex system is a scientific paradigm *à la Kuhn*, then, among the kinds of potential failure, we typically think, as Nickles stresses, to “empirical failure”, that is, refuted predictions. But I think we could also count as a failure any apparently insurmountable barrier to fulfilling the main desiderata that the adherents of a given paradigm feel it crucial to satisfy, from Kuhn’s “big five” to more specific requirements such as ‘explain everything in mechanical terms only’. In sum, robustness is relative to various dimensions, in addition to be a matter of degree along each dimension.

Nickles also insists that “there is a prospective dimension of robustness” and that we should take into account “prospective robustness” in addition to the “purely retrospective conception of robustness”. The retrospective conception assesses the robustness at a given point on the basis of what has happened up to now, examining the way the epistemic system has in fact resisted failures and has been fruitful. (In the case of scientific theories, this is often measured by philosophers in terms of the degree of justification at a given time.) But because “human designers of epistemic systems possess a degree of lookahead”, retrospective assessments do not exhaust what human designers take into account when they evaluate the robustness of their epistemic systems. Their evaluations also incorporate judgments about “prospective fertility”, intuitions and bets about the future fruitfulness of the system, its ability to resist to multiple threats and to fulfill the most important desiderata imposed on it. A robust system at the research frontier is one that possesses “a strong heuristic promise”. This prospective dimension of robustness is “crucial for decision-making”, especially when choices have to be made between several competing options, including research proposals. A robust system is a system that has proved to be robust up to now *and* that is perceived as promising future robustness. So both past-informed and future-oriented perspectives must be taken into account in evaluations of robustness.

Lastly, Nickles provides an interesting insight into ways in which his extension of Perrow’s thesis and general theories of networks could help us to understand better some aspects of Kuhn’s famous conception of mature science. In particular, this approach illuminates Kuhn’s claims that the very nature of normal science “prepares the way for its own change” (in Kuhn’s words). So here, too, we find the tradeoff put forward by Nickles between robustness and fragility, in this case in relation to scientific paradigms.

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