Theory Assessment in the Absence of Empirical Evidence

A Philosophical Inquiry into an Increasingly Pressing Problem in Modern Physics

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Abstract

The characteristics of theoretical statements in modern physics make that there is an increasing amount of energy needed in order to evaluate those statements by using empirical data. An amount which is very hard or maybe even impossible to provide. Therefore it is not easy or not possible to investigate and support the statements with empirical evidence, while this is an important part of conventional scientific methods. Without the important input of empirical data, science, according to these conventional methods, is strictly speaking not possible. In this Bachelor's thesis, solutions to this problem will be explored. One of the possible solutions, a change in the way in which science is normally done in the form of non-empirical theory assessment, turns out to be promising, but also unreliable and uncertain. Therefore this solution is investigated in more detail. It will be concluded that although a solution based on empirical theory assessment will probably always be the most desirable (but also unrealistic for the problem in modern physics), non-empirical theory assessment might actually provide a better, more plausible, solution than you would initially expect.

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Introduction

Ask a random person what she or he thinks distinguishes science from myths, and they will probably answer: proof. The concept of proof is closely intertwined with modern ideas about science and often it will refer to empirical evidence: a type of proof pre-eminently important for physics.

But what if there could be no empirical evidence found any more?

This might seem a remote situation, something for the future perhaps. But nothing could be further from the truth. In modern physics, acquiring empirical data becomes more and more difficult, which makes it really hard or maybe even impossible to support theoretical statements: an increasingly pressing problem.

In this Bachelor's thesis I will explore some possible solutions to this problem. Special attention will be given to the solution of non-empirical theory assessment, because it turns out that this is both the most promising as the most unreliable and controversial solution.

Of course, in the first place the solution to the problem of lacking empirical data should come from physicists working in fields which encounter this problem themselves. They know the specific situations best and are the ones who will eventually use the solution. However, philosophical inquiries to the issue could provide new ways of thinking, investigate the plausibility of proposed solutions and provide a framework for thinking about questions raised by the problem. But obviously, it is of course also an intrinsically interesting case to study philosophically.

We will start our exploration with a closer look to the introduced problem. It will be discussed why it becomes harder to collect empirical data and why it is problematic that theoretical statements could not be supported by empirical evidence. This will be done in Chapter I. Then, in Chapter II, an overview of possible solutions to the problem is sketched. As already mentioned, one of those solutions, a change in the way in which science is normally done in the form of non-empirical theory assessment, turns out to be promising, but not free of problems. Therefore we will investigate this solution in more detail in Chapter III and try to raise its plausibility by a Bayesian analysis in Chapter IV. Thereafter Chapter V will provide a look into the current discussion about the problem in modern physics and the possible role of non-empirical theory assessment as solution. At the end we will take stock and discuss some considerations, expectations and conclusions of which probably the most important one is that although non-empirical theory assessment might never win from empirical theory assessment, in times of empirical crisis, it might actually provide a better solution than you would initially expect.

Chapter I - *The Problem*

This chapter consists of five sections in which the problem will be sketched. The first section is devoted to the current situation in modern physics in which theoretical and empirical inquiries are growing apart. In this section it will be explained that a problematic aspect of this situation is that there are increasingly less empirical data available for the evaluation of theoretical statements. Then, the major cause of this situation is explained: the even higher amount of energy required by experiments. Thereafter we will go into some detail about why one of the aspects of the situation described in the first section is in fact problematic. Lastly, it will be shown how this problem could be understood in a broader philosophical context, namely as a case of a familiar concept in the philosophy of science: underdetermination of theory by data.

1. Empirical Inquiries Lagging Behind

'Particle zoo', that is the slightly derogative term theoretical physicists used to express their dissatisfaction with the state of physics in the mid 1960s. At that time, more than 60 particle types had been found by scattering experiments at high energies, but a theoretical account was missing. Theorists could only watch from the sidelines while the experimental spectacle carried on. Theory seemed to have lost its structuring power.[1]

Nowadays, the roles are reversed. No longer empirical considerations, but theoretical ones play the leading role in theory dynamics.[2] The change started when the theorists were finally able to keep up with the experimentalists by providing a theoretical account of the newly discovered particles in the form of the standard model.[3] But the theoretical efforts did not stop at this point and in the last forty years a lot of new hypotheses emerged, a major part of them not inspired by empirical results, such as the wish to explain empirical anomalies or to investigate new phenomena, but on theoretical considerations about consistency, theory unification and increased explanatory power.[4]

But that is not the only way in which theoretical and empirical inquiries grew apart. Not only, as mentioned above, leave experimental signatures increasingly wide spaces for entirely theoretical reasoning with little or no empirical interference, those signatures are also moving towards the fringes of the phenomenal world,[5] which leads to the problem that there are increasingly less empirical data available for the evaluation of theoretical statements.

2. The Role of Energy

But why exactly does the fact that empirical signatures move towards the fringes of the phenomenal world lead to the above stated problem? This is because in order to gain knowledge about the fringes of the phenomenal world, which is

[1]Dawid, *Marginalization*, 4
[2]Ibidem, 11
[3]Ibidem, 6
[4]Ibidem, 7-8
[5]Ibidem, 8

crucial for the assessment of modern physical hypotheses, more and more energy is required: the secret world of the smallest constituents of our Universe will only reveal itself by our experiments at specific, very high, energy levels. Energy levels which could not be reached in our current experiments and are unlikely to be reached by experiments in the near future, what is more, they might never be reached at all.[6] And so while the theorists are already continuing their theoretical research, the experimentalists are still struggling with energy problems.

The reason for the ever larger difficulty of reaching higher energy levels shows itself in the history of experimental devices, especially in the one of particle accelerators. In order to reach a higher energy level, a longer accelerator tube is needed. And so the size of the largest existing accelerator rose from 24cm in 1931 to 5m in 1942, 72m in 1953, 600m in 1959 and 3km in 1966. In the meantime, the spectacular length of 27km is achieved with the Large Hadron Collider (LHC) at CERN.[7] But still, 'only' energies of 14 TeV could be accomplished[8], while for example string theory needs to be investigated at an energy scale quite close to the Planck scale,[9][10] which is around 12×10^{15} TeV, 10^{15} times higher than the energies accessible at the LHC. Now imagine what an incredible complicated task it would be to develop an apparatus to investigate at that level of energy, remembering that the LHC already took 16 years to build (and that while the tunnel was already there from a previous experiment), cost about 3 billion Euro for the machine alone and provides work for 8000 physicists.[11][12]

So, no new experiments without a lot of money, time, material, space, ingeniousness, technology and labor. Things we might not be able[13] or willing to provide. And that makes acquiring new empirical data very difficult, maybe in some cases even impossible. But why exactly is this a problem?

In order to see this, we need to take a look at ideas on scientific methodology.

3. Scientific Methods

There are a couple of differing visions on what is a good way to do science.[14] But all those visions have one thing in common: they ascribe a pivotal role to empirical data. One of those visions is the well-known hypothetico-deductive method, a vision for instance shared by the philosopher Popper. To give an idea of how such a proposed scientific method looks and how empirical data is used, we will now discuss this hypothetico-deductive method in more detail.

The method, depicted in a strongly simplified version in Figure 1, starts with asking a question or establishing a problem. The inspiration for the question or

[6] Dawid, *Marginalization*, 9

[7] Ibidem, 4, 6

[8] Communication Group (CERN), *CERN faq*, 3

[9] However, there are also some models of string theory which require a much lower amount of energy for testing, low enough to become (almost) observable at the LHC. Those models propose large extra dimensions. (Dawid, *String Theory*, 17)

[10] Dawid, *String Theory*, 16

[11] Communication Group (CERN), *CERN faq*, 17

[12] Dawid, *String Theory*, 66

[13] It is important to have in mind that this could be due to human shortcomings, but also to the encounter of physical boundaries or the limitations imposed by the properties of the earth.

[14] And of course even more ways in which science is actually done.

the problem may come from very different sources, such as empirical data and certain phenomena, but also theoretical considerations and even a dream or a poem could be an inducement.[15][16] Next, an answer to the question or a solution to the problem will be proposed: the hypothesis. Based on this hypothesis predictions are made and the next step is to verify or falsify the hypothesis by subjecting the predictions to empirical tests. In other words, to conduct experiments and to compare found empirical data with the predictions. If the predictions contradict the data, the hypothesis gets falsified and the chance is small that it will get (part of) a theory one day.[17] But if the predictions correspond with the data, the hypothesis is confirmed[18] and this will increase the chance that it will be added to the collection of scientific knowledge.

In this system, there are two moments when empirical information plays a role. The first one is what Popper called the 'context of discovery', or in other words the moment that through the usage of all sorts of sources of inspiration a problem or question is formulated. Empirical findings are namely perfectly suited to serve as a source of inspiration. The second moment that empirical information enters the scientific process, is Popper's 'context of justification',[19] or the step of testing hypotheses and thus, when a hypothesis seems correct, providing a justification for the new-born theory.[20]

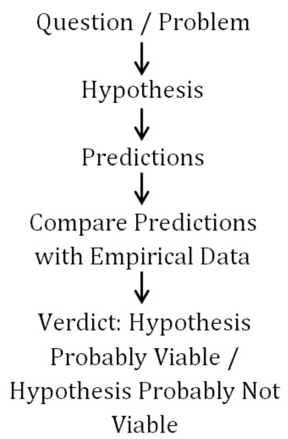


Figure 1: A strongly simplified version of the hypothetico-deductive method.

[15]Especially because of the wide range of possible sources of inspiration, one should not marginalize the influence of bias in this and further stages of the method.

[16]Dieks and Pasveer-de Vries, *Filosofie/Grondslagen van de Natuurkunde*, 66

[17]However, the chance is not directly zero, maybe there were made some mistakes with the collection of the data for example.

[18]Sprenger, *Hypothetico-Deductive*, 1,2 (In this article, also some interesting examples of the hypothetico-deductive method in action could be found.)

[19]Popper was not the first to use the terms 'context of discovery' and 'context of justification', however, the character he gave this distinction was the one which influenced the discussion about the two contexts in the previous century the most. (Hoyningen-Huene, *Context*, 503)

[20]It is important to notice the nuance the word 'justification' brings to this sentence. Because although a confirmation of a hypothesis could explain someone's belief in it, it could not prove its general truth. The reason for this is connected with the induction problem of Hume. The fact that (continuous) verifications of a hypothesis do not lead to a general established truth, explains Popper's emphasis on falsification and the crucial role of it in scientific progress. (Dieks and Pasveer-de Vries, *Filosofie/Grondslagen van de Natuurkunde*, 66, 67)

In the 'context of discovery' the lack of empirical input is a pity, but certainly not a disaster. As mentioned above, empirical information is not the only source of inspiration for coming up with a new hypothesis. So theoretical considerations and other ways to get inspired are able to fill the gap empirical information is leaving behind. Something that already happened in the years after the development of the standard model, where, as described above, theoretical considerations about consistency, theory unification and increased explanatory power were the dominant sources of inspiration.

However, in the 'context of justification' the disappearance of empirical data is a problem. According to the hypothetico-deductive method, the only 'official and accepted' way to promote a hypotheses to a theory is by extensive comparisons of predictions made by the respective hypotheses with empirical data. So when these data are missing, the confirmation of hypotheses and the justification of theories is not possible anymore.

This means a step of this scientific method has become unrealisable and therefore it is, with this method, strictly speaking no longer possible to add new knowledge to the scientific body of knowledge. The cycle of the hypothetico-deductive method is stopped and thereby, according to this method, science is too.

4. Underdetermination

Still, it is not the first time problems emerged because of an absence of empirical evidence. The fact that such a lack of empirical evidence has already occurred earlier, is evident from the fact that the above described problem is a case of a familiar concept in the philosophy of science: underdetermination of theory by data, or simply 'underdetermination'.

Although there are a couple of different coexisting interpretations (with corresponding nuances) of the term 'underdetermination' in the philosophy of science, something which has often caused confusion,[21] for the purposes of this thesis the following, simple, definition suffices:

> The underdetermination of scientific theory by evidence is in essence the simple idea that the evidence available to us at a given time may be insufficient to determine what beliefs we should hold in response to it.[22]

An example as illustration of the above definition:

Imagine yourself in a wheelchair on the foot of a mountain in a country where there is a currency (⋆) with only 'one' and 'two' valued coins. You know that on the top of the mountain there is a little fruit and vegetable stall where they sell bananas, white radishes and red radishes. The prices of the goods are known to you and you also know that customers always have to pay with the exact amount. Now you see an old man climbing a steep mountain trail on his way to the stall. When he comes back, you notice two bananas in his bag, and with your knowledge of the price of a banana you are sure he spend 2⋆. You are

[21] Stanford, *Underdetermination*
[22] Ibidem

wondering in which way he paid and you come up with two hypotheses: a) he spent 2 coins of 1⋆ or b) he spent 1 coin of 2⋆. But on the basis of just knowing that he bought two bananas, it is impossible to know which of the theories is the correct one.

This is a classical case of underdetermination. Now the example will be expanded to come closer to the situation in modern physics:

Informed as you are, you know that the old man really likes structure in his life and therefore always pays in the same way. So to decide which of your hypotheses is true, the only thing you need to do is to get yourself up the mountain and just watch the man the next time he pays for his fruit. But, poor you, that will never happen. Because it does not matter how hard you try, you will never have enough strength and stored energy in your body to roll yourself to the top of the steep mountain. And so, unless you think of a trick, you will never know in which manner the man paid.

The example is similar with the situation in modern physics, because in the context of discovery it is also often possible to think of a couple of theories not contradicting with the available empirical evidence, wherein the context of justification it is then impossible to add new evidence in the form of results of experimentation to the collection of evidence upon which it could be decided which of the in the context of discovery formulated hypotheses is 'the right one'. In other words, the total sum of empirical evidence is not able to determine which theory gives the right description of it, and so the described problem in modern physics is a case of underdetermination.

Different from occurrences of underdetermination in the past, however, is the expected endurance of the interruption of scientific reasoning by the lack of empirical data. The time between the formulation of the hypothesis and the disappearance of the interruption, the moment you can evaluate the hypothesis, was much shorter in the past. For in the most cases scientists did not have to wait many years for technology to improve and reach the level of advancement needed for the investigation of their ideas.[23] And they knew that, which is why estimates of the possibility of a test in the future and therefore overcoming the interruption were quite positive. Which is a second difference, because contemporary scientists see the limits of technology looming, and therefore are not so positive with respect to the possibility of solving interruptions with new technology in the near future.[24]

Before we continue, there is the following important remark regarding underdetermination I would like to make. Without specifying what we consider to be different theories, we could strictly speaking always say that there is an unlimited amount of alternatives for a certain theory. This could for instance be done by introducing unimportant, meaningless dummy variables, so that the alternatives are a little bit different, but fit the available empirical data equally well. And this implies a serious case of underdetermination, because how could we ever decide which of all those possible theories is 'the right one'?

[23]Advancements in technology as a solution for the problem of underdetermination will be further discussed in Chapter II.

[24]Dawid, *String Theory*, 79, 80

To avoid these kind of problems, the following constraints for the individuation of scientific theories will be used:[25]

- Different theories make different predictions. If two theories make exactly the same predictions, then we consider them to be identical.[26]
- Different theories provide different solutions to a given scientific problem. That is, theories which only differ in a detail, such as the precise value of a parameter or the existence of a physically meaningless dummy variable, do not count as different theories.

5. Summary

In this chapter it has been made clear that there is a problem in (future) modern physics, a case of underdetermination, namely the following:

The characteristics of theoretical statements in modern physics make that there is an increasing amount of energy needed in order to evaluate those statements by using empirical data. An amount which is very hard or maybe even impossible to provide. Therefore it is not easy or not possible to investigate and support the statements with empirical evidence, while this is an important part of conventional scientific methods. Without the important input of empirical data, science, according to these conventional methods, is strictly speaking not possible.

What to do now? Should we just stop doing modern physics? The disappearance of empirical evidence is one of the biggest changes coming up in modern physics and it will form one of the greatest methodological challenges, but we should not give up. There are numerous reasons thinkable why one would not wish to let modern physics go. Reasons such as job opportunity, prestige, money, or love for mathematical puzzles but above all boundless curiosity and the urge to expose the deepest structures of reality. So, when stopping with modern physics is not an option, a solution to the problem is needed. Therefore Chapter II will be devoted to solutions.

[25]These constraints are taken from: Dawid, Hartmann and Sprenger, *No Alternatives*

[26]This means that in the first place we only demand empirically adequacy of theories, and not necessarily truth. This is to prevent that different interpretations of a empirical adequate theory also count as alternatives, which would be needed to talk about truth, but could cause a serious underdetermination problem if there are a lot of interpretations. However, in the following chapters I have been a bit sloppy with these terms (empirical adequacy and truth), this was sometimes necessary for clarity, and because I did not want to completely exclude truth and the quest for true theories from the discussion. The occurrence of references to truth are however minimal, furthermore if it might seem that the mention of truth may cause problems because it would imply a huge amount of alternatives, this could be solved by creating constraints for the possible interpretations.

Chapter II - *Solutions*

In this chapter, solutions to the problem discussed in Chapter I will be put forward.[27] First we will take a look at a pragmatic solution and thereafter three theoretical ones will be explained. Also their weaknesses will be discussed in some detail. For these solutions empirical data is still very important, but in the penultimate section of this chapter, we will also encounter a solution that does without these data. At first sight, this might seem unscientific and wrong, but considering the nature of the problem, the chance exists that it is the only option we have.

1. Pragmatic Solution

This is actually the simplest and most trivial solution and comes down to the following thought: 'When there are problems with underdetermination because of a lack of empirical data, we simply need to create more empirical data.' In the situation in modern physics that we described in Chapter I, we saw that the lack of empirical data results from the high amount of energy needed to acquire these data. So, according to this solution, to solve the problem of underdetermination, the only thing we need to do is wait until there are enough investors, enough advancements in technology, enough scientists and enough other contributors to set up new experiments which are able to create and use the necessary energy.

Now let us see how the example of underdetermination in Section I.4 would be solved by using the pragmatic solution:

Although you were not able to roll yourself to the top, you did not give up and now you have come with a solution to get up the mountain: you are building an elevator. It is a lot of work and costs a lot of money but in a couple of months you will be able to go to the fruit and vegetable stall and look in which way the man pays for his bananas.

The solution to just develop new technology or wait until someone else makes technological progress was already discussed in Section I.4 as a solution often used in the past. But there, and more explicitly in Section I.1, it was also already noted that it is not a realistic solution in modern times.

2. Theoretical Solutions

Now, three solutions with a theoretical base will be discussed.

a) A New Theory

Instead of solving the underdetermination problem of your earlier developed theory (or theories), it is also possible to come up with a new theory, one that is not subject to the underdetermination problem. This means that the new theory should for example explain the same phenomenon, or could provide the same unification of other theories, but in a different way, that is, a way which is testable by executable experiments.

[27] It is certainly not claimed that this list of solutions is complete, but I think it gives a nice overview of the kind of directions in which possible solutions could be found.

Of course, when the second theory turns out to be viable, this does not automatically mean that the first theory is complete nonsense. Maybe there are two possible explanations for a phenomenon, for instance because of two different separate causes for the same observation. But in most cases it is probably more likely that there just exists one cause, or one way to unify two theories and so when the second theory is accepted, the first one could not be accepted. In addition, it is also not unthinkable that your two theories are contradictory and could therefore not both be true at the same time. So when you know that your second theory seems right, you could automatically get rid of your first one.

But even when it is not the case that there is just one explanation possible or that they are contradictory (so that you could not reject the first theory when knowing the second one seems right), then you still have your second one to use in further scientific inquiries. So maybe you are not aware of the full 'truth' yet, you could at least prevent a standstill of scientific research in your area.

With this solution, the example from Chapter I could be continued as follows:

When you were wondering in which way the man paid for his bananas, you came up with two theories: a) he spent 2 coins of 1⋆ or b) he spent 1 coin of 2⋆. But it was impossible to decide which one of the theories was correct. Now you come up with a new idea: maybe he did not pay for the fruit at all! You know that the owner of the stall gives free fruit and vegetables to her family. And to check if the old man is family or not, you do not have to get up the mountain, you just need to ask someone, so the test is executable. When the man indeed turns out to be family, you know the way the man 'pays' and the underdetermination problem is solved.

At first sight, this solution seems quite suitable for the problem with underdetermination in modern physics. However, one should ask themselves how likely it actually is that a) a new theory will be developed and b) on top of that, the new theory does not require experiments at high energy levels, while all the other comparable theories do. Taking this into account, it does not seem like a bad idea to explore other possibilities for a solution.

b) Same Theory, New Predictions

Although this solution has some similarities with the previous one, it is still quite different. In the case of underdetermination in modern physics described in Chapter I, there was not enough empirical data to test the predictions the different hypotheses made. So an obvious thought would be to create extra data. However, this solution proposes the opposite, namely to try to develop new predictions, predictions which do not need the huge amount of energy to be tested, the characteristic of the earlier predictions that caused the underdetermination problem.

To develop new predictions, one could look into unexplored parts of a theory, combine the theory with other knowledge or apply the theory to a new situation. Cosmological phenomena for example, make interesting test cases. And so instead of trying to artificially create high energies, one could also register and investigate what is happening in the Universe.[28]

[28] An interesting example of this kind of obtaining predictions for string theory could be

Back again to the example set out in Chapter I:

Until now, you have only made one prediction per hypotheses: 'When I watch the old man paying for the bananas, I will see that he pays with 2 coins of 1⋆' and 'When I watch the old man paying for the bananas, I will see that he pays with 1 coin of 2⋆'. But after some thinking and research you find out that people who pay with 2 coins of 1⋆ will get a blue label on the bananas and the customers who pay with 1 coin of 2⋆ get a red one. So now you have two extra predictions: 'If I check, I will see that the bananas have a blue label on them.' and 'If I check, I will see that the bananas have a red label on them.', respectively. But these two new predictions both have a great advantage compared to the other two: you can check them while staying at the foot of the mountain. This solves the underdetermination problem, because the only thing that withheld you from knowing which one of the hypotheses is correct, was the trip to the top of the mountain.

This solution might be helpful, there are however some concerns one should not forget. The first one is that it might not be possible for every theory to find new predictions, thereby it is also not certain that the new predictions found are more easy to test. But more important, one should anticipate the fact that confirmation of the new predictions in some cases (dependent on the character of the old and new predictions) does not bring the same level of confirmation to the theory as the older ones.[29]

c) Connections between Large and Small Length Scales
This solution is all about the idea that it might be possible that certain theoretical structures could be discovered which indicate connections between large and small length scales in nature through which observations at large length scales could be translated to knowledge about small length scales, without the observations at large length scales being consequences of processes at small length scales.[30] By making use of those connections and observations

found in: Phys.org, *Practical Test*.

[29]This is because it is more likely that the older predictions need to be tested by direct observation, since this type of prediction is easier to think of. Direct observation most of the time is considered to bring the greatest level of trust to the connected hypothesis. An example of an 'old' prediction connected to the hypothesis 'This sample contains type-C-cells.' could be: 'When I look with a microscope at the sample, I will see type-C-cells.', while a 'new' prediction (a prediction after more thinking) could be 'When I add chemical D to the sample it will turn pink.' It must be acknowledged however, that since the time of the discussion around the existence of the atom, at the end of the nineteenth century, the second kind of prediction, the indirect one, gained some more credit. (Dieks and Pasveer-de Vries, *Filosofie/Grondslagen van de Natuurkunde*, 53)

[30]Of course, observations with a resultative or emergent character (such as the temperature of something, which originates from the movement of constituting particles), could also provide useful experimental data suitable to say something about smaller length scales. But this type of reasoning is already covered by solution 2.b, because these kinds of observations are the ones you could compare with predictions made by the theory which is tested. Whereas in this solution, solution 2.c, the described connections indicated by theoretical structures are general, and not per se predicted by the theory which is tested itself. So the observations talked about in 2.c are not providing information about the small length scales because they are a confirmation of predictions made by a theory about the small length scales, instead they serve as input for the discovered theoretical structures which could translate these observational

at large length scales, it is possible to gain knowledge about the small length scales with empirical research, but without the need of a large amount of energy.

Now two possible ways in which events at small and large length scales could be connected will be discussed.[31]

- Reality is constructed in such a manner that always when event A (an event at a large length scale) happens, event B (an event at a small length scale) happens too. As already mentioned, event A does not *cause* event B or vice versa, but when for instance something at large length scales let A come into existence, B emerges immediately. It might even be better to take A and B as one event, AB, that just happens to take place partly at a large length scale and partly at a small length scale. Now, if you need to know if event B exists to confirm a theory, but the length scale at which event B happens is unapproachable for your experiment, you could look for event A at an accessible length scale. When event A is observed, you automatically know event B has to exist.

  To give an illustration of the previous, we will again take a look at the example introduced in Chapter I, this time it will be a bit more unrealistic, but for the purposes of illustrating that is not a problem.

  Still wanting to solve your underdetermination problem, you find yourself in the library looking in old books to find some leads, when finally you come across a passage about fruit stalls. You are lucky, besides all the old wisdom about bananas and radishes there is also a part dedicated to the mysterious relation between the way old men are paying for bananas and the colour of a ball falling on one-thinking-about-that-paying-process' head. Although still wondering how it is possible that you never happened to notice the ball falling on your head before, you are very happy that you now know that when a yellow ball falls on your head when the man pays, hypotheses a) is true, and when a green ball falls on your head hypotheses b) is true.

- Reality is constructed in such a manner that certain observations could be explained in two ways. This should not be understood as a case of underdetermination, because the two ways are completely equivalent. One way to explain the observation uses the larger length scale in its explanation and the other way the smaller one. Also it is known how to convert the explanations into each other. Therefore, you are able to gain knowledge about smaller length scales by investigating phenomena, receptive for such a double explanation, at larger length scales and subsequently converting the description made to the equivalent smaller length description. This will be the same description but just 'written' with other numbers and terms. But the numbers and terms of the smaller length description do

findings to information about small length scales.

[31] It is not claimed that these are the only two possibilities, thereby it is also important to have in mind that the possibilities are not more than just proposals for what might be found in nature one day, not descriptions of connections that nowadays are actually known to exist.

happen to give you (parts of) the information you need about the small, inaccessible part of reality. Questionable remains however, how useful this information about sole observations is. But maybe after many observation descriptions certain patterns will emerge, which could lead to the confirmation of a prediction of a hypothesis.

This is all still quite vague, perhaps an example will be enlightening. This time the example will not be a continuation of the example from Chapter I, but a similar case as the above, obtained from string theory.

An important feature of string theory is the occurrence of string dualities. The string world shows a remarkable tendency to link seemingly different string scenarios by so-called duality relations. The linked scenarios are equivalent concerning their observational signatures. One of those relations is T-duality. T-duality asserts that a model(1) where a string with characteristic length I is wrapped n times around a dimension with radius R and has momentum eigenvalue m is dual to a model(2) where a string is wrapped m times around a dimension with radius I^2/R and has momentum eigenvalue n.[32][33] *So, based on an observation you could try to determine I, n, R and m in model(1) and then convert these values to the ones in model(2). So even when for a particular reason the values of, for example, model(2) are hard to determine,*[34] *you could still acquire them via model(1).*

This is similar with the above described solution, but then one of the models would have to be an account at a large length scale and the other one an account at a small length scale.

Also this solution has some weaknesses. The first and most problematic one is that before it is even possible to use the connections indicated by theoretical structures as a tool to facilitate the confirmation of hypotheses, those connections themselves must be confirmed. Because of the part of the connection at small length scales, they will also need to be tested at high energies, but it is exactly the non-testability at high energies you are trying to solve with the connections. Furthermore, just as was the case with the previous solution, the evidence collected with this solution could get lesser 'confirmation credit' than evidence acquired by more direct observation. At last we should certainly not forget to ask ourselves how likely it actually is that these kind of connections exist at all and, if they exist, that they will be discovered by us.

3. Methodological Solution

In Chapter I it was mentioned that all the visions on scientific methodology nowadays attribute an important role to empirical data as a way to verify and justify theories. And therefore it also became clear that these proposed types of methods inevitably encounter a problem when empirical data is no longer

[32]A description of string theory and the concepts used in this example is not given, because this is not necessary for understanding the point the example tries to make.

[33]Dawid, *String Theory* 18, 19

[34]Maybe lower momentum eigenvalues are harder to determine and is $n < m$.

available. All the previous solutions for this had one thing in common: they tried to solve the problem by removing the cause, in other words, by removing the lack of empirical data. They did this either by proposing a way to gain more data, (solution 1 and 2.c) or by making other data more important (solution 2.a and 2.b). But we could also try to solve the problem in a different way, namely by proposing a scientific method that does not need empirical data: a post-empirical method.

In a post-empirical method justifying a theory should be based upon other reasons than conformity between prediction and observation. One way to construct such a method, is to use so-called non-empirical theory assessment. An important advantage of this approach is that a major part of the 'old' scientific methods can remain the same,[35] only if it is necessary the phase of empirical theory assessment could be replaced by non-empirical theory assessment. It is a little detour in times of need. In Figure 2 it is depicted how this would look for the hypothetico-deductive method.

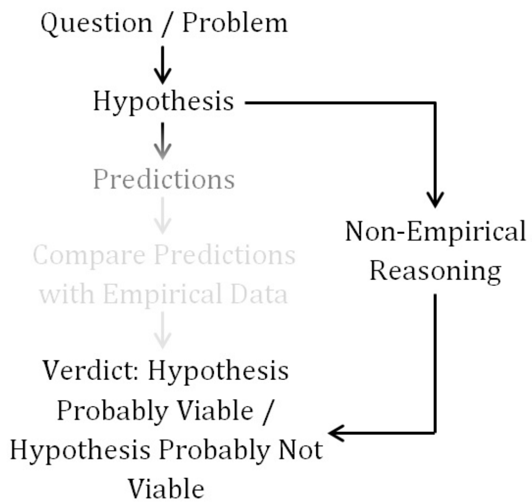


Figure 2: The hypothetico-deductive method with non-empirical detour. In light grey is the step that becomes impossible when empirical data is missing. 'Predictions' is also grey (but darker), because non-empirical reasoning could also be used for hypotheses that do not make predictions. However, one could ask to the status of such hypotheses, as they do not satisfy the conventional falsification criterion.

4. Summary

In this chapter we encountered several solutions for the underdetermination problem introduced in Chapter I. The pragmatical and theoretical solutions still emphasize the role of empirical data in theory verification and justification, either by providing a way to create new data (solution 1 and 2.c), or by making other data more important (solution 2.a and 2.b). The methodical solution however, solves the problem by abandoning the thought that a basis for theory justification lies in empirical data exclusively.

[35] Another advantage is that the construction of a post-empirical method in this way is not completely free. Without the boundary conditions 'old' methods offer (or even more general: without the wisdom of what does and does not work in science), a method such as 'form entirely random stories and then throw a dice to determine which of the stories you will call scientific knowledge' would also be an acceptable post-empirical method.

Most people probably consider the pragmatical and theoretical solutions as the best ones, they are still based upon empirical considerations and these have already proven themselves in the past. However, in this chapter we saw that they all have some weaknesses which could make them less helpful in the problematic situation in modern physics. Therefore, the third solution, the one of non-empirical theory assessment, will become increasingly important. Non-empirical considerations also play a role in 'old' ways to do science, albeit perhaps unwillingly, unconsciously or indirectly. But because of the underdetermination problem they might become indispensable, and highly influential on the amount of trust one grants a theory if we are willing to accept these kind of considerations in science, of course. But because we might not have an other choice, it seems like a good idea to explore non-empirical theory assessment in some more detail. This will be done in Chapter III.

Chapter III - *Non-Empirical Theory Assessment*

In the previous chapter the importance of a post-empirical method as a solution to the underdetermination problem in modern physics was noticed. It was also mentioned that it seems like a good idea that this method should mean that empirical theory assessment would be replaced by non-empirical theory assessment if necessary. In this chapter we will get in some more detail about non-empirical theory assessment. Of course, many forms of non-empirical theory assessment are possible because it just comes down to judging a theory based on something else than empirical data. So, even 'always trust the third theory conceived' strictly speaking is a kind of non-empirical theory assessment. But in this chapter some forms will be discussed which hopefully could be made more plausible.

The first section of this chapter will be devoted to a short introduction of two important concepts in the discussion of non-empirical theory assessment presented in this chapter. Namely 'setting limitations to underdetermination' and 'assessing limitations to underdetermination'. Thereafter we will discuss three ways of non-empirical theory assessment, namely non-empirical theory assessment based on non-empirical virtues of theories, on inter-theoretical connections and on considerations about the research process. They will be discussed in the Sections 2, 3 and 4 respectively.

1. Setting or Assessing Limitations to Underdetermination

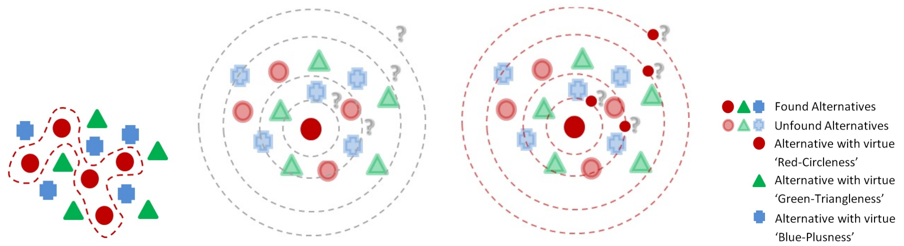
When there is a case of underdetermination and you wait for enough new empirical data, this data will most likely lead to an unambiguous preference for one of the theories. However, with non-empirical theory assessment such an unambiguous answer to underdetermination is not always possible. This does not mean that we cannot say anything about the problem though. Because although in some cases we might not be able to point out one theory as the one to be chosen,[36] we could still try to indicate how likely a certain theory is.

Two strategies that could be used by non-empirical theory assessment methods that are related to the likelihood of a theory are 'setting limitations to underdetermination' and 'assessing limitations to underdetermination'.[37]

Non-empirical theory assessment by setting limitations to underdetermination is possible because in a case of underdetermination (the moment you

[36]And I think this are the most cases where non-empirical theory assessment is applied seriously. (A method such as 'always choose the third theory conceived' does point out one theory, but the question is how reliable such a method is.)

[37]The basic idea to use limitations to underdetermination as a way to assess theories non-empirically stems from Dawid (a physicist turned to philosophy), he elaborates on this in his book *String Theory and the Scientific Method*, although he presents limitations to underdetermination more as an explanation of the validity of certain non-empirical arguments than as a 'tool', which comes closer to the way I will present it. However, this is just a difference in approach, content wise there is not much of a difference. Nevertheless, although some parts of this chapter stay close to his ideas, there are also some deviations. For example, among other things, the distinction between setting and assessing limitations to underdetermination and a greater attention for non-empirical virtues and inter-theoretical relations are not or to a lesser extent to be found in Dawid's work.



(a) (b) (c)

Figure 3: (a) Setting limitations to underdetermination: you know which alternatives there are in principle, but you only select some of them as relevant, in this case the ones with the virtue 'Red-Circleness'. (b) Assessing limitations to underdetermination: you have found one hypothesis and you estimate the size of the set alternatives in principle, or in other words, you estimate how many possible alternatives there are. (c) A combination of setting and assessing limitations to underdetermination: you have found one hypothesis and estimate how many comparable (read: with the same virtue) alternatives there are.

need non-empirical theory assessment) setting limitations to underdetermination makes the set of relevant hypotheses[38] smaller and this means that the competition for a single hypothesis to become the 'winning' theoretical description decreases. Therefore the trust in a hypothesis increases.[39] Setting limitations to underdetermination for instance happens when you impose certain criteria on the set of hypotheses in the form of minimal amounts of specific non-empirical virtues (see Figure 3a) and you could also use it when considering inter-theoretical relations. This will be discussed in Section 2 and 3 respectively.

'Assessing limitations to underdetermination' means that you estimate where the limits of the underdetermination are, or in other words, how many alternative theories there are in principle possible (see Figure 3b). The trust in a theory enhances when you estimate that underdetermination is highly limited, or in other words, when there are not many alternatives for the theory. The fact that there are not many alternatives for a theory increases the trust in a theory in the way described above. Assessing limitations to underdetermination could for instance be done by analysing certain aspects of the scientific context in which the theory is formed. This will be discussed in more detail in Section 4.

It is important to notice the difference between setting limitations to underdetermination and assessing limitations to underdetermination. Setting them means that you manually put a stop to underdetermination, for instance by demanding certain things of theories, while assessing limitations to underdetermination means giving an estimate of the number of alternatives for a theory

[38]The set of relevant hypotheses is the spectrum of theoretical alternatives worthy of scientific examination and pursuit. (Carrier, *Values*, 1540)

[39]It is implicitly assumed that the chance for a hypothesis to turn out to be the best (if you like: the true) theory goes approximately as $1/N$ with N the number of relevant hypotheses. To attribute to all the candidates approximately the same chance seems acceptable taking into account the fact that they must be comparable, otherwise they would not be in the set of equivalent theories after the limitations to underdetermination are set.

which are possible in principle. When you set limitations to underdetermination you already know (approximately) this number of alternatives possible in principle,[40] but by choosing some constraints, you make a selection of alternatives you will take into consideration when assessing the theory in question. So the number of relevant alternatives to the theory is reduced from all alternatives possible in principle to just the selected ones and so the underdetermination has become smaller. With assessing limitations to underdetermination you do not reduce underdetermination yourself, you are just lucky when you estimate that it is probably low (so when you estimate that there are not many alternatives possible in principle). However, the difference between the two is not always clear and sometimes they also occur together (see Figure 3c).[41]

2. Non-Empirical Virtues of Theories

Non-empirical virtues are qualities theories can possess which could support the choice for one theory over another when this is not possible on the sole grounds of empirical data, in other words, when there is an underdetermination problem. These qualities are based upon what scientists consider as valuable characteristics for a theory.[42] There are many kinds of non-empirical virtues. They could have an epistemic basis,[43] such as scope and internal consistency,[44] a mathematical one, like symmetry or convergence, or a more pragmatic basis, such as understandability or applicability. But even for example religious (accordance with religious doctrines), social (decentralizing power[45]), esthetical (beauty, order, harmony) or political (accordance with political conviction) ideas could be used as decision makers.[46] This of course leads naturally to questions about the general agreement of scientists on reliable virtues and the status of decisions made based on such virtues, something we will come back to later on.

Underdetermination is solved by means of non-empirical virtues by choosing

[40]Of course, you could also set limitations to a group of alternatives of which you know, or suspect, that it is not complete. However, in the main text I emphasized a complete group, because if you are not certain that you have found all the possible alternatives, the chance for a hypothesis to be viable you calculate based on the amount of alternatives after setting limitations, is not reliable, because there could be other suitable alternatives among the ones you did not find.

[41]This happens for example when estimating the number of *equally satisfying* alternatives to a theory. Because before assessing the number of alternatives, you have already set limitations by demanding that the alternatives are equally satisfying (or: posses a sufficient amount of certain non-empirical virtues).

[42]Non-empirical virtues of theories are resemblances of non-empirical values of scientists. Therefore non-empirical values are very important for determining the requirements for the confirmation of knowledge claims. But non-empirical values actually serve two chief purposes in science: they also express requirements for the significance of knowledge claims. This means that they are influential on the choice of research problems and the pursuit of theories. Values influencing the significance could be epistemic ones, but also non-epistemic values such as pragmatic, ethical, utilitarian, or social values could shape the line of research. (Carrier, *Values*, 1538, 1539)

[43]Epistemic virtues delineate the goals attributed to science seen as a knowledge-seeking enterprise. They distinguish characteristics of knowledge intrinsically worth knowing. (Carrier, *Values*, 1539)

[44]Internal consistency is consistency within a theory itself.

[45]Longino, *Gender*, 389

[46]More about different kind of empirical virtues and the role they play in science could for instance be found in Longino's article *Gender, politics, and the theoretical virtues* or Kuhn's article *Objectivity, Value Judgment, and Theory Choice.*

the theory that possesses the highest amount of the most important virtues, or it is limited by setting some standards for the minimal amount of certain virtues, standards hypotheses need to satisfy in order to be a serious candidate theory. Important to realize is that this way to limit underdetermination only focusses on characteristics of the theories themselves and not for example on relations with other theories. Other theories are only necessary to cooperate in creating a frame of reference,[47] but when this is done, the chances of a hypothesis could be assessed purely based on the hypothesis alone.

The use of such virtues as a way to solve underdetermination is not new, it is even the solution most of the times proposed in cases of underdetermination. And not only in explicit cases of underdetermination they are important. Kuhn for example acknowledged the influence non-empirical values associated with a paradigm exert on theory conceiving, assessment, confirmation and acceptance in general.[48] His ideas are in line with the famous Duhem-Quine thesis, which also emphasizes the indispensable role of non-empirical virtues in science and reads:

> The agreement of the empirical consequences of a theory with the available observations is not a sufficient reason for accepting a theory, the acceptance must always be understood in a wider conceptual framework (including the non-empirical values and virtues). Duhem emphasizes the role of intuition based on experience in the choice which theory to accept, while Quine underscores that every individual statement could be made compatible with any empirical data by making changes within the existing framework.[49][50]

Laudan also agrees that these virtues constitute an important part of science, as is shown by the following quote:

> 'Such values are constitutive of science in the sense that we cannot conceive of a functioning science without them, even though they fail to be intelligible in the terms of the classical theory of knowledge'[51]

In this quote Laudan also mentions the problematic character of non-empirical virtues, something which will be explored after a worked out example of a non-empirical virtue.

[47] One could for instance base the standard for the minimal amount of a virtue on the amount of that virtue possessed by another (standard)theory (quantum mechanics for example).

[48] Bird, *Thomas Kuhn*

[49] Actually, here we encounter the problem of different interpretations of the term underdetermination which was mentioned before. Because the Duhem-Quine thesis also describes a kind of underdetermination, but not the one used in other parts of this thesis. The difference is that in the Duhem-Quine kind choosing a hypothesis based purely on empirical data is logically impossible, while in the one used in this thesis choosing a hypothesis based purely on empirical data is not possible due to specific circumstances. The first kind is therefore also more general, it applies to all the choosing moments, while the second kind only applies to certain problematic cases. The definition of underdetermination given in Section I.4 is however loose enough to encompass both the types. More about the different definitions of underdetermination can be found in Dawid, *String Theory*, 42-45.

[50] Carrier, *Values*, 1538 and Dawid, *String Theory*, 43

[51] Laudan, *The epistemic*, 19

A. Example: Simplicity
It is hard to put simplicity in one of the categories for non-empirical virtues mentioned above. Simplicity could namely both be considered as an epistemic virtue, a pragmatic and an esthetic virtue. When it is understood as equivalent with parsimony, then it is epistemic, when associated with mathematical easiness it is pragmatic and when connected with elegance it is esthetic. The parsimony type could also be called *ontological simplicity* and measures the number of kinds of entities postulated by the theory.[52] Simplicity seen as connected with and contributor to elegance could also be called *syntactic simplicity* and measures the number and conciseness of the theory's basic principles.[53] For the mathematical easiness type there is no other established word, but *mathematical simplicity* probably suffices and it measures the difficulty to understand and use the mathematical content (if present) of a theory.

The concepts of simplicity are virtues of theories because of the principle of Occam's Razor. This principle is basically the idea that simple theories are better theories, wherein simple usually refers to ontologically or syntactically simple. In the context of ontological simplicity the principle of Occam's Razor comes down to not adding extra (theoretical) entities to a theory when this is not strictly necessary. This will be illustrated with an example.

*Back to the problem with the old man and the bananas. You came up with two hypotheses when you thought about the way in which the old man paid. But a friend of yours also had an idea. He mentioned the possibility that there might exist another type of coin, namely one of 2⋆cent and that the old man is the only one who is allowed to use that type of coin, which explains why you assumed it did not exist. And so the following hypothesis must be added: the man had not paid with 2 coins of 1⋆ or 1 coin of 2⋆, but with 100 pieces of 2⋆*cent*. However, to accept this hypothesis also means that you should accept a new type of coin (2⋆*cent*) and a new rule (the old man is the only one who could use the 2⋆*cent* coin) Something which seems highly artificial and superfluous considering the other two suitable hypotheses. But to be honest, refuting the new hypothesis based purely on empirical data (your observation that the man bought bananas) is not possible. But when you need to make a choice between the three hypotheses you will rely on non-empirical virtues and go undoubtedly for the simpler hypotheses: one of the first ones. It seems unnecessary to propose new rules or new entities in the form of a new type of coin.*

B. Problems
As already announced, now some problems with non-empirical virtues will be discussed.

a) Different Definitions
One of the problems with non-empirical virtues of the kind discussed in the foregoing is that they are imprecise. Different scientists could use different definitions for the same virtues, this could lead to the situation that individual scientists may legitimately differ about the application of certain non-empirical

[52] Baker, *Simplicity*
[53] Ibidem

values to concrete cases.[54]

b) Different Ranking

Even when there is general agreement about the definitions, it is not unlikely that the non-empirical virtues conflict with one another in concrete cases. Scientists, moreover, need not to agree on what they consider as the most important non-empirical virtues and therefore an objective ranking of non-empirical virtues may not exist. This means that it might not be decidable which non-empirical virtue should be the decisive factor in case of conflict, and so non-empirical virtues fail to guide theory choice unambiguously. As a result, one of the competing theories may appear superior according to one ranking and inferior according to another. This problem is known as *Kuhn-underdetermination* or *methodological incommensurability*.[55] The problem of different ranking and the problem of different definitions are also mentioned by Kuhn in his 1977 article *Objectivity, Value Judgment, and Theory Choice*, where he says the following:

> When scientists must choose between competing theories, two men fully committed to the same list of criteria for choice may nevertheless reach different conclusions. Perhaps they interpret simplicity differently or have different convictions about the range of fields within which the consistency criterion must be met. Or perhaps they agree about these matters but differ about the relative weights to be accorded to these or to other criteria when several are deployed together.[56]

c) No A Priori Connection between Virtue and Truth or Empirical Success

There is no necessary logical connection between a non-empirical virtue and the truth or empirical success of a theory: the factors that most of the time determine how successful a theory is.[57] Nothing *in the concept of a non-empirical virtue itself* indicates a relation with truth or empirical success and as far as we know there is no a priori preference to explain the Universe with theories supported by certain non-empirical virtues. And so a link is missing between non-empirical virtue and a theory's success. A link that is however assumed to exist when the choice for a theory is made based on an epistemic non-empirical virtue and sometimes also when based on a non-epistemic non-empirical virtue.[58]

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[54] Kuhn, *Objectivity*, 357

[55] Carrier, *Values*, 1540

[56] Kuhn, *Objectivity*, 358

[57] What you consider as a successful theory depends on whether you hold a realist, an empiricist, an instrumentalist or other position in the scientific realism debate. However, from the instrumentalist point of view a virtue like simplicity *does* have a connection with a theory's success, because success in this view is how useful the theory is as an instrument for prediction and systematizing, and a simpler theory is more useful. (Chakravartty, *Scientific Realism*) So, it is important to have in mind which kind of success is meant. In this paragraph a theory's success is particularly understood as realist type of success (truth in the sense of the correspondence with reality) or empiricist type of success (successful prediction and correspondence with observations).

[58] When someone does not assume the existence of this link when choosing between theories on the basis of a non-epistemic non-empirical virtue, one may wonder if the intentions of that person where purely scientific, and not for instance political.

Despite all the criticism, non-empirical virtues could still be useful for putting limitations to underdetermination when combined with the soon to be discussed *Meta-Inductive Argument*. Because although the non-empirical virtues in itself could not indicate truth or empirical success, experience could learn which of the non-empirical virtues happen to be present in theories that turn out to be successful. In this way experience could give guidelines for refining the definitions, for ranking the virtues and it gives a connection between virtue and success based on induction. And so MIA solves, or at least reduces, the problems.

3. Inter-Theoretical Connections

This way to give a non-empirical assessment of theories is based on inter-theoretical connections and is actually an extended version of setting standards for theories by the non-empirical virtue of external consistency.[59] It uses the fact that the status of a theory could be influenced by the status of connected theories and the idea to impose the requirement of a minimal amount of support by other theories on the set of equivalent hypotheses.

When there is for example a case of underdetermination, so that there is a set of empirical equivalent hypotheses, it might be possible to encompass one or a couple of them in a more general theory while the others cannot be so encompassed. Then the evidential support of the general theory flows only to the encompassed (and therefore with the general theory consistent) ones, which makes that these theories receive larger confirmational support than the rivals.[60] In other words, the evidence for the general theory serves as indirect evidence for the encompassed ones. And now it is possible to limit underdetermination by excluding the theories which do not have enough indirect confirmational support compared to the others.

Also, the possibility exists that one of the equivalent hypotheses gets at odds with (new) background knowledge. When the conflict is serious enough, so when we could speak of a serious inconsistency, this could mean a refusal of that hypothesis.[61] Through this, the set of relevant hypotheses also becomes smaller.

Now it is time for another little example before moving to some remarks about the usefulness of this type of setting limitations to underdetermination in the context of our central problem.

You had tried everything but still had not figured out if hypothesis a) or b) is true and so you gave up for the moment. But now, a few months later, you are again investigating the old man, although now in a broader context. The following hypothesis from your current research has caught your attention: 'Always when the old man pays, he pays with the least amount of coins possible.' This hypothesis encompasses hypothesis b), and so when you find evidence for your new hypothesis, you also enhance the trust in hypothesis b). When the difference in trust between b) and a) gets large enough, it could support the choice to reject hypothesis a).

[59] External consistency is consistency with other theories.
[60] Acuña, *Empirical Equivalence*, 38
[61] Ibidem

So, now we understand how to set limitations to underdetermination by using inter-theoretical relations, but how useful is this kind of limiting underdetermination in the problematic situation in modern physics? A great advantage of this way to set limitations is its strong connection with empirical findings (although indirect),[62] because this suits the ordinary ideas about theory assessment. But this advantage is at the same time also the greatest disadvantage in the context of the problem in modern physics. For it is exactly obtaining empirical data that is the problem. So when the more general theory or the (new) background knowledge needs to be supported by empirical data about small length scales (or when supportive empirical data is not available for another reason), we still cannot make any progress.

Acuña states in his master thesis that if inter-theoretical relations with a more general theory or with (new) background knowledge could not solve the underdetermination problem by breaking the equivalence, one should take into account the recursive nature of these two ideas for solving underdetermination, which means that one should make an even more general theory and more background knowledge.[63] Normally, this would indeed be a good solution. But with our problem in modern physics it is definitely not certain that this will work. Because it is really thinkable that the even more general theory and the extra background knowledge also need empirical data from small length scales. And this applies to every further necessary recursive step. This method will therefore result in a complex web of theories and inter-theoretical relations, waiting for some of the theories on the fringes of the web to get some empirical support, so that other theories will also gain more trust. In short, setting limitations to underdetermination based on inter-theoretical relations is a promising way to solve underdetermination, but unfortunately it seems not very suitable for our problem in modern physics.

4. Considerations about the Research Process

In the previous sections we saw that it is possible to enhance the trust in a theory by setting limitations to underdetermination. Now we will focus on assessments of limitations to underdetermination and their power to influence the trust in a theory. As already mentioned, when the estimated amount of underdetermination turns out low, so when underdetermination is highly limited, or in other words, when there probably exist not many alternatives for a theory, this increases the trust in this theory.

In this section, two indications for a small set of theories that describe the same data as a specific theory will be discussed. The indications are observed characteristics of the research process and of opinions of scientists. The two indications for the existence of a small amount of alternatives to a theory are the observation that scientists have not found many alternatives to the theory yet and the observation that the theory for which you assess the number of

[62]One could wonder if this method should still be called non-empirical theory assessment when relying in such significant part on empirical data. But the fact is that this data does not support the hypothesis in question directly. The indirect character of the support makes the assessment non-empirical: not the empirical data provides the support, but the status of connected theories.

[63]Acuña, *Empirical Equivalence*, 39

alternatives delivers unexpected explanations for phenomena. They will now be discussed separately.

A. Scientists have not Found many Alternatives

The observation that scientists have not found many alternatives is an indication for limited underdetermination, because one could conjecture a connection between the spectrum of theories scientists came up with and the spectrum of all possible scientific theories that fit the available data.[64] This is not an illogical presumption: when there are a lot of theories possible in principle, there are also a lot of theories to stumble upon and vice versa. So the fact that one observes that no or less equally satisfying alternatives to a specific theory have been discovered by scientists even after a long and careful search, is a sign that not too many alternatives are possible in principle: underdetermination is significantly limited.

Of course, one should be aware of the fact that the lack of alternatives could also be caused by insufficient depth and scope of the scientific analysis.[65] Therefore it was emphasized in the previous paragraph that concluding a small spectrum of possible alternatives from the observation of less or no alternatives found by scientists, is only possible when you are sure of the occurrence of a long and careful search.

But even then, the possibility always exists that humans are just not smart enough to come up with certain possible alternatives or that there are other (unknown) restrictions that inhibit the finding of alternatives. This shows that one should be careful with conclusions based upon the fact that there are not many alternatives found, the danger of a false, excessive amount of trust in a theory is lurking. But this does not mean that the observation of not many found alternatives is useless, on the contrary, with the right assumptions, a healthy dose of optimism, some confidence in human competence and by combining it with other indications and other arguments for the theory, it could really help assessing a theory, as long as one takes the known restrictive factors and the possibility of unknown ones into account.[66]

When one uses the fact that there are not many alternatives found to a theory as an argument for the viability of a theory, he or she uses the so called *No Alternatives Argument* (NAA). This term is proposed by Dawid in his *String Theory and the Scientific Method*,[67] and although the contents of the argument would stay the same, I personally prefer a name such as the *No Alternatives Found Argument* (although this sounds a bit less impressive), to prevent confusion about the word 'Alternatives', for it could refer both to alternatives found by scientists and alternatives that are in principle possible. The argument has to refer to the first kind of alternatives, because they are the ones indicating limited underdetermination when lacking, while 'No Alternatives' in the context of the second kind already means the same as 'limited underdetermination'.[68]

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[64] Dawid, *String Theory*, 46, 47

[65] Ibidem, 47

[66] Such as the difficulty of the scientific problem one tries to solve.

[67] Dawid, *String Theory*, 31

[68] Of course, in the end it is the fact that there are less alternatives in principle that enhances the trust in a theory. But that applies to all the arguments discussed in this chapter that work because of their connection with limitations to underdetermination. And so if you use the second interpretation of 'Alternatives', all those arguments could be called a no alternatives

Before we move on to the next indication, let us first take a look at an example:

There is a crisis in your country, all the 1⋆ coins have disappeared. However, you do not worry about it at all, because now you are finally able to say something more about the way in which the old man pays for bananas. When you are sure the man had to pay, so when there is no mention of, for instance, a family relation with the owner which could have meant the old man gets them for free, the only way to pay is with a 2⋆ coin. There is no alternative you could think of. And even when there are some alternatives possible which you just did not think of, there could not be a lot of them, otherwise you should have at least found some of them. On top of that, those possible unconceived alternatives have to be special exceptions in paying made for the man, and, as the word exception already implies, you can safely assume there not many of them. Of course, it is still not possible to see the paying process with your own eyes, and so you will not be a hundred percent certain, but the fact that there are no or less alternatives for hypothesis b) makes that it is safe to grant the hypothesis at least a major amount of trust, and maybe even accept it as the correct hypothesis.

B. Unexpected Explanations

The next indication for limited underdetermination is the observation that a theory provides unexpected explanations. Which means that it gives information about phenomena that were not intended to be touched on by the theory and so that it provides explanatory connections which were not aimed at during the construction of the theory.[69][70] The idea that confirmation of a theory could be based on achievements of a theory that had not been foreseen at the time of its construction is widely held. But normally those achievements refer to empirical predictions which are later confirmed by experiment. However, now we do not talk about empirical predictions of course, but about theoretical explanations and the surprise that a new theoretical principle solves more problems than it was intended to do, the principle provides an unexpected more coherent theoretical picture.[71]

The main reason that such unexpected explanations could serve as an indication for limited underdetermination is the following: imagine that there are some seemingly independent scientific problems, for which you assume that solutions to each problem are abundant. Finding a theory that solves one of the problems does not imply that this solution is necessarily viable, for we assumed that the solutions are abundant and so there is a significant chance that the 'real' solution is among the other possibilities. However, the fact that a theory solves multiple problems at once could enhance the trust in that theory considerably. It is namely to be expected that theories that solve multiple problems at once are rare, and so there are not many equally satisfying alternatives,[72] or

argument too, and that is not what is meant.

[69]The requirement that the explanations need to be unexpected is important to exclude the case that someone sets out to find an explanation for a certain phenomenon and subsequently uses the fact that the resulting theory provides this explanation as an argument for its viability, which would make no sense, since that was just what she or he was searching for.

[70]Dawid, *String Theory*, 47

[71]Ibidem, 33

[72]Which here means: theories that could at least solve the same amount of problems.

in other words: underdetermination is limited.[73] Finding a theory that solves multiple problems at once is exactly what the observation that a theory provides unexpected explanations comes down to. Because to say that you could give unexpected explanations with a theory is the same as to say that you have found out that the theory solves more problems than you expected. And so unexpected explanations are an indication for limited underdetermination.

The way in which unexpected explanations are connected with limited underdetermination is almost the same as the type of connection non-empirical virtues have with it. When considering unexpected explanations you also investigate a property of a theory and then you demand a certain amount of that property from other theories in order to be a satisfying alternative.[74] In other words, you set a limit to underdetermination by demanding a certain amount of unexpected explanations a theory should provide, just as you do with non-empirical virtues. There is however a difference between unexpected explanations as a non-empirical virtue and other non-empirical virtues: it is more dependent on the opinions scientists have about the research process. Because in order to reason based upon the observation that a theory provides unexpected explanations one must first determine what exactly was expected and what was not, and in some cases that might differ from scientist to scientist.[75]

We just saw that the main reason that unexpected explanations given by a theory enhance the trust in that theory was because they implied a small group of other rare theories as alternatives for the theory, also it was mentioned that this was resembling a case of setting limitations to underdetermination. Now another reason for enhancement of trust by unexpected explanations will be discussed, namely that the occurrence of unexpected explanations could support a claim of limited underdetermination already made by other arguments based on setting or assessing limitations to underdetermination.[76] Because when one already assumes that underdetermination is limited, and so that the theory in question is maybe the only theory possible in principle, the expectation of unexpected explanations the theory was not constructed to provide comes naturally. This is because of the principle of scientific optimism, which assumes that there is an empirically fully adequate scientific theory that covers all problems in a specific research field. And when you think that your theory is the only theory possible, it must be this empirically fully adequate one. So it should not come as a surprise that it indeed covers all the problems, or in other words: that it provides unexpected explanations. Therefore the fact that the unexpected explanations really occur supports the idea that you have found this one empirically fully adequate theory. However, when you still believed in an abundant set of alternatives, the unexpected explanations come unnaturally and are a seemingly inexplicable mystery.[77]

[73] Outlines from Dawid, *String Theory*, 47, 48

[74] Unexpected explanations as non-empirical virtue is better known as the virtue of fruitfulness.

[75] Opinions of scientist do also play a role in thinking about other non-empirical virtues of course, but those opinions are on the definition, ranking or occurrence of a non-empirical virtue and are not (or at least less) connected with the research process.

[76] Here I deviate from Dawid, because he actually only describes the reason discussed in this paragraph explicitly. However, the preparation he uses for giving this reason is what inspired me to think of the, in my opinion, main reason.

[77] Dawid, *String Theory*, 48

Before moving to an example, first a short remark: when one gives the fact that a theory provides unexpected explanations as an argument for a theory, we could say he or she uses the *Argument of Unexpected Explanatory Coherence* (UEA), again this term is introduced by Dawid in his *String Theory and the Scientific Method*.[78] And now the example.

On Sundays the fruit stall has a special offer: two bananas for the price of one. So two bananas cost just 1⋆ and four bananas cost 2⋆. When the old man buys bananas on a Sunday, you always notice that he bought four bananas instead of the usual two. Which is really weird, because you know for sure that the man always wants just two. So this is a problem. However, one of the hypothesis about the way in which the man pays on the other days could offer a solution, assuming that the man holds on to his tradition to always pay in the same way. The hypothesis that offers the solution is hypothesis b), the one that proposes that the man pays with one coin of 2⋆. This is because we assumed that the stall does not offer any change. When the man comes to the stall on a Sunday and hears that the two bananas are just 1⋆, he still has no other option than to hand over the 2⋆ coin. But, it is likely that he will regret loosing 1⋆ for nothing and therefore decides to then just get four bananas. Hypothesis a) could not solve the problem of the four bananas on Sunday, because then the old man has the option to just give one of the two 1⋆ coins. The fact that hypothesis b) could give this unexpected explanation of the buy behaviour of the man on Sundays (unexpected because the hypothesis was only made to explain behaviour on days without special offers) makes that it gains more trust.

Now we have discussed two indications for limited underdetermination. A great advantage of these indications is their empirical character, or even better: their *meta-empirical* character, because they do not serve as empirical data to test empirical predictions of a theory, but they do increase the trust in a theory based on observations about the research process and therefore support the theory on a meta-level. So when direct empirical evidence for a theory is missing, we could still assess a theory based on empirical information, only now with the great advantage that observations about the research process do not require an enormous amount of energy. The empirical character is an advantage because it reminds us of empirical theory assessment and it seems to provide more objectivity to non-empirical theory assessment than for example non-empirical theory assessment based on non-empirical virtues.

However, although we have seen it is quite understandable why the indications point to limited underdetermination, it is still a bit strange that considerations and observations about the research process could contribute to the trust in a theory. This step from observations about present human perspectives towards a conclusion about the overall spectrum of possible scientific theories is by no means trivial.[79] However, we have also seen that in times of a shortage of empirical evidence, these type of indications could be really necessary and therefore it seems like a good idea to make the connection between human perspectives and the spectrum of possible theories a bit more plausible and intuitive. This will be done in two ways, namely with the

[78] Dawid, *String Theory*, 33
[79] Ibidem, 47

Meta-Inductive Argument and with a Bayesian analysis of the *No Alternatives Argument*. The Bayesian analysis will be given in the next chapter, the *Meta-Inductive Argument* will now be discussed.

C. The Meta-Inductive Argument

The *Meta-Inductive Argument*, or fully the *Meta-Inductive Argument from the Success of Other Theories in the Research Program* (MIA),[80] is basically the idea that the validity of NAA and UEA is supported by successful argumentations based on them in the past, where successful means that theories which where highly trusted because of NAA and UEA actually turned out to be the best theories.[81] In other words, MIA is an empirical argument that instead of testing a theory, provides empirical tests of the strategies of non-empirical theory assessment, which become more trustworthy if found to be regularly successful.[82] So MIA makes the connection between human perspective and the spectrum of possible theories more plausible by showing that in the past assuming such a connection was indeed justified.

There is however one major objection against MIA, and that is the classic induction problem: there is no logical connection between the fact that something happened a couple of times in the past under certain circumstances and the fact that it will happen again in the future under the same circumstances. However, this problem applies to all the cases on induction, which are actually even all the completely accepted laws of physics and so it seems like this problem should not be the decisive factor for refusing MIA, although it is wise to be aware of it when using MIA.

D. Solving the Underdetermination Problem with Assessments of Limitations to Underdetermination?

As was already mentioned, the problem with underdetermination could easily be solved by setting limitations to underdetermination. This is logical, because strictly speaking you could just set as much limitations to underdetermination as needed to end up with one theory. When you, for example, use non-empirical virtues to set the limitations, you could just demand certain amounts of virtues in such a way that only one theory suffices. However, the question is of course if we want to use such an active and seemingly artificial interference.

With assessing limitations to underdetermination, solving underdetermination is not that easy. Underdetermination is actually only solved when it turns out that there are no alternatives to a theory possible in principle, or in other words: when you estimate that underdetermination is completely limited.[83] Because when there are more alternatives possible, underdetermination is of course not solved. The only effect the passive process of estimating the number of alternative theories then has, is that it will increase the trust in all the alternatives with just the same amount. And because we need relative and not absolute

[80] This is again a term proposed by Dawid. (Dawid, *String Theory*, 35)

[81] With what are 'the best theories' depending on your expectations of science. For some people it could be for instance 'the true theories'.

[82] Dawid, *String Theory*, 48

[83] Warning: it is still just an estimate and 'solving underdetermination' in this context could therefore maybe better be understood as 'accepting the theory that is granted a specific high amount of trust', where in this case the trust is gained by probably being the only theory possible.

differences in trust to solve underdetermination, this will not be helpful. Well, actually that is not completely true, it could be useful for solving underdetermination when combined with other non-empirical considerations and it could also be useful for other purposes than solving underdetermination, for instance when you want to compare the trust in (members of) two different sets of alternatives for a mysterious reason.[84]

5. Summary

In this chapter we discussed several kinds of non-empirical theory assessment. For our discussion we used two concepts: setting limitations to underdetermination and assessing limitations to underdetermination. Two methods that mostly used setting limitations were using non-empirical virtues and using inter-theoretical relations, while the possibility of using considerations about the research process in non-empirical theory assessment is mostly based on assessing limitations to underdetermination. All the methods had some problems, but in the context of the problem in modern physics the third method's problems seem to be the least harmful and on top of that this method also has the major benefit that it has a (meta-)empirical character. However, the validity of arguments based on considerations about the research process is not self-evident. It has already been tried to enhance their plausibility by MIA. The plausibility and the clarity will tried to be further raised in the next chapter by a Bayesian analysis of NAA.

This chapter also raises some questions, such as the question to the status of solutions for the underdetermination problem proposed by non-empirical reasoning. Furthermore, we could also ask ourselves which of the proposed methods, or which combination of methods will be most successful for solving or reducing underdetermination. Also the question arises what exactly the connection is between the trust in the truth[85] of a theory and the actual truth of that theory. A high amount of trust in a theory or in other words a strong presumption that the theory is right still seems not enough to conclude the actual truth of a theory. But then, how does a very strong presumption actually differs from the strong feeling of certainty caused by verifying an empirical prediction? Or in other words: does this problem of trust and truth not also applies to empirical theory assessment as well? The discussion needed to do justice to these questions about the connection between truth and trust will be too extensive for this thesis. The first two questions however will (shortly) come across in the following chapters. But for now we will turn our attention to the Bayesian analysis of NAA coming up in Chapter IV.

[84]Perhaps to determine on which theory you would like to work, a theory with just three alternatives seems to be more promising than one with six hundred alternatives.

[85]I am aware of the problems with using a word such as 'truth', but I do not think an extensive discussion of the definition of truth is necessary here. But for clarity, I mean something like empirical adequate and corresponding with reality.

Chapter IV - *A Bayesian Analysis of NAA*

As already announced, this chapter will be devoted to a Bayesian Analysis of NAA, the *No Alternatives Argument.* First there will be a short introduction to Bayesianism. Which will thereafter be used to prove that NAA could indeed enhance the chance for a theory to be empirically adequate, or in other words, that NAA could contribute to the confirmation of a theory. This will hopefully take away some reservations against NAA and raise the plausibility of it. The proof of NAA given in this chapter was constructed by Dawid, Hartmann and Sprenger. They used five assumptions for this proof, one of them will be criticised by me after the proof. Thereafter I will investigate new ways to prove NAA disregarding or modifying the, in my eyes, problematic assumption.

1. Introduction

Bayesianism constitutes currently the most popular formalization of scientific theory confirmation and is based on the mathematical theory of probability.[86] It provides an expression of the probability of certain propositions and the ways in which the probabilities of those propositions influence each other.

At the core of Bayesianism is Bayes' theorem, which reads:

$$P(A|B) = \frac{P(B|A)P(A)}{P(B)}$$

Where $P(A)$ and $P(B)$ denote the probability of A respectively B. And $P(A|B)$ is the probability of A when B has been taken into account and $P(B|A)$ is the probability of B when A has been taken into account.

Before we will get to the Bayesian representation of non-empirical theory confirmation in this chapter, let us first take a look at empirical theory confirmation as a little warming-up. When empirical data is denoted with E, and T denotes the viability of a theory, then this theory gets confirmed by the empirical data iff[87]

$$P(T|E) > P(T)$$

Or in other words: when the chance for the theory to be viable is higher when the empirical data is taken into consideration than when it is not.

Now we are ready to direct our attention to the Bayesian picture of the non-empirical theory confirmation of NAA.

2. A Proof of NAA

First a little recap of NAA: the *No Alternatives Argument* is the argument that it is more likely that a theory is empirically adequate when the observation is made that no alternatives to the theory have been found, despite considerable efforts to do so. In their article *The No Alternatives Argument*, Dawid, Hartmann and Sprenger show that the fact that NAA applies to a theory indeed enhances the

[86]Dawid, *String Theory*, 39 and Hartmann, Sprenger, *Bayesian*, 1
[87]Dawid, *String Theory*, 39

chance for that theory to be empirically adequate.[88] But before we take a look at the actual proof given by them, we first need to agree on terminology and do some preparations.[89]

In order to formalize NAA we consider a theory or hypothesis H and the observation that scientists have not yet found an alternative to H: the potential non-empirical evidence for H which we call $\mathrm{F_A}$. This is potential non-empirical evidence, because, as we already saw, this observation is often taken to indicate that there are actually not too many alternatives in principle possible to H, and therefore, because of this restricted competition to H, it is indirectly an argument for the viability of H.

To find out if $\mathrm{F_A}$ really confirms[90] H, as NAA suggests, we need more tools. To express the competition to H, we base the reconstruction of NAA on the notion that there exists a specific but unknown number k of possible suitable scientific theories, of which H is one. These theories have to satisfy the constraints C (whose nature is left to the scientific community), explain data P and predict the outcomes of experiments E to be suitable. Furthermore, we also introduce the binary propositional variable T and we specify the earlier introduced $\mathrm{F_A}$ as part of the binary propositional variable F_A. T takes the following values:

T The hypothesis H is empirically adequate.

¬T The hypothesis H is not empirically adequate.

and F_A takes these values:[91]

$\mathrm{F_A}$ The scientific community has not yet found an alternative to H that fulfils C, explains P and predicts the outcomes of E.

$\neg\mathrm{F_A}$ The scientific community has found an alternative to H that fulfils C, explains P and predicts the outcomes of E.

Another variable we introduce is the variable Y, which mediates the connection between T and F_A, as it is measuring the number of alternatives to H. It could take on values in the natural numbers, and we interpret the set of associated propositions $\mathrm{Y_k} := \{Y = k\}$ in the following way:

$\mathrm{Y_k}$ There are exactly k hypotheses that fulfil C, explain P and predict the outcomes of E.

[88] Dawid, Hartmann, Sprenger, *No Alternatives*

[89] The terminology and the preamble to the proof presented in this section resemble to a large extent contents of the mentioned article of Dawid, Hartmann and Sprenger (pages 5-9). Some knowledge of what is mentioned there is namely necessary to understand the proof and to understand the criticism on one of the assumptions presented in the next section. For more details about a.o. terminology, justification and approach I would like to refer to their article.

[90] Note that you should interpret 'confirms' as 'supports' and 'enhances the probability of' and not as 'totally accepts' and 'establishes'.

[91] I think this way of constructing F_A by Dawid, Hartmann and Sprenger contains a weakness, namely that it is not possible to distinguish the case that there is one alternative found from the case that there are more than one, or even a lot of alternatives found. Because not only the fact *that* an alternative is found, but also the amount of alternatives found could influence other aspects of the Bayesian analysis.

We are still missing one variable, because we should notice that the value of F_A does not only depend on the number of available alternatives in principle, but also on the difficulty of the problem, the cleverness of the scientists, or the available computational, experimental, and mathematical resources. It is for example not hard to imagine that a very difficult problem will lead to the observation that scientists have not found many suitable alternatives yet, and in this case the conclusion that the struggle to find alternatives indicates a low amount of alternatives in principle would be wrong. This shows the importance of the complementary factors which we will denote as D, with values in the natural numbers and with $\mathrm{D_j} := \{D = j\}$, the higher the values of D (or j of course), the more difficult the problem. Or, stated a little differently, we could interpret $\mathrm{D_j}$ as follows:

$\mathrm{D_j}$ The difficulty of the scientific problem could be identified with the magnitude of the number j.

Now we have all the variables we need to proof NAA. The variables are, together with the relations between them, depicted in the Bayesian network in Figure 4.[92]

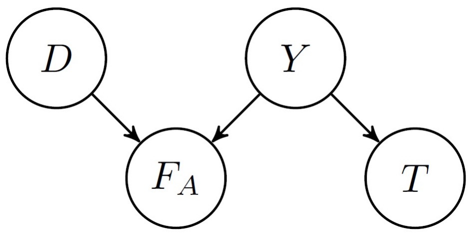

Figure 4: Bayesian Network 1 - The network as used in the article *The No Alternatives Argument.*

However, we are still not ready to understand the proof of NAA Dawid, Hartmann and Sprenger have given, because we also need to take into account the five assumptions they used. I have listed their assumptions below, they are taken literally from their *The No Alternatives Argument* article.[93]

A. Assumptions

A1 The variable T is conditionally independent of F_A given Y:

$$T \perp\!\!\!\perp F_A | Y$$

Hence, learning that the scientific community has not yet found an alternative to H does not alter our belief in the empirical adequacy of H if we already know that there are exactly k viable alternatives to H.

[92]Figure 4 from: Dawid, Hartmann, Sprenger, *No Alternatives*, 11

[93]The reason I included them in the main text and not as an appendix is because I think that they give a nice insight in the dynamics between the variables and that they serve as a good warming-up for the actual proof.

A2 The variable D is (unconditionally) independent of Y:

$$D \perp\!\!\!\perp Y$$

Recall that D represents the aggregate of those context-sensitive factors that affect whether scientists find an alternative to H, but that are not related to the number of suitable alternatives. In other words, D and Y are orthogonal to each other by construction.[94]

A3 The conditional probabilities

$$f_{kj} := P(\mathrm{F_A}|\mathrm{Y_K}, \mathrm{D_j})$$

are non-increasing in k for all $j \in \mathbb{N}$ and non-decreasing in j for all $k \in \mathbb{N}$.

The (weak) monotonicity in the first argument reflects the intuition that for fixed difficulty of a problem, a higher number of alternatives does not decrease the likelihood of finding an alternative to H. The (weak) monotonicity in the second argument reflects the intuition that increasing difficulty of a problem does not increase the likelihood of finding an alternative to H, provided that the number of alternatives to H is fixed.

A4 The conditional probabilities

$$t_k := P(\mathrm{T}|\mathrm{Y_k})$$

are non-increasing in k.

This assumption reflects the intuition that an increase in the number of alternative theories does not make it more likely that scientists have already identified an empirically adequate theory.

A5 There is at least one pair (i, k) with $i < k$ for which (i) $y_i y_k > 0$, (ii) $f_{ij} > f_{kj}$ for some $j \in \mathbb{N}$, and (iii) $t_i > t_k$.

In particular, this assumption implies that $y_k < 1$ for all $k \in \mathbb{N}$ because otherwise, a pair satisfying (i) could not be found.

Now we are finally ready to turn our attention to the proof, which could be found in Appendix A.[95] The proof shows that:

Theorem 1 *If Y takes values in the natural numbers $\mathbb{N}$ and assumptions* **A1** *to* **A5** *hold, then* $\mathrm{F_A}$ *confirms* T*, that is,* $P(\mathrm{T}|\mathrm{F_A}) > P(\mathrm{T})$.

And so it is indeed true that $\mathrm{F_A}$ confirms H.

As we see, Theorem 1 only applies if Y takes values in the natural numbers. But in their article, Dawid, Hartmann and Sprenger show that by generalizing some of the assumptions, the case that there are infinitely many alternatives available to H ($\mathrm{Y}_\infty := \{Y = \infty\}$) is also allowed.

[94]Strictly speaking, this is not true, they are not orthogonal *by construction* because in the article there was no mention of a constraint imposed on the complementary factors that involves that the factors need to be unrelated to the number of suitable alternatives, as is implied in these two sentences. The only thing that was said about the relation between D and Y before this assumption, was the following: 'It is clear that D has no direct influence on Y and T (or vice versa)', but stating that something seems clear, is totally different than imposing it as a requirement. However, assumption **A2** could of course still be made.

[95]Proof by: Dawid, Hartmann, Sprenger, *No Alternatives*, 24, 25

3. A Note on Assumption A1

In this section I will discuss some remarks about and criticism on assumption **A1**.

A. The Assumption
Assumption **A1** tells us that variable T is conditionally independent of F_A given Y. So if we already know that there are k suitable alternatives for H, it does not matter for the chance of H to be empirically adequate that those alternatives have not yet been found (despite the sufficient effort and time put into the finding of alternatives).

B. Role of the Assumption
This assumption is used in the proof of Theorem 1. If I have understood it correctly, this assumption is necessary in the third line of the proof. Here $P(\mathrm{F_A}, \mathrm{T}|\mathrm{Y_i})$ is namely written as $P(\mathrm{F_A}|\mathrm{Y_i})P(\mathrm{T}|\mathrm{Y_i})$, which is only possible if T and $\mathrm{F_A}$ are statistically independent (given $\mathrm{Y_i}$) and that is exactly what assumption **A1** guarantees. $P(\mathrm{F_A}|\mathrm{Y_i})$ and $P(\mathrm{T}|\mathrm{Y_i})$ could then be replaced with the abbreviations that where earlier used in the proof, which is important for the remainder of the proof.

C. Questioning the Assumption
So, assumption **A1** is important for the proof of Theorem 1, but one may wonder if the assumption is really as logical as was sketched in the article, where it was stated that the authors take the assumption to be 'eminently sensible'.[96] Suppose for example that I am sure that there are five possible alternatives for my found hypotheses H, does the fact it costs a lot of time and effort to find those alternatives[97] then really not matter for the chance of H to be empirically adequate? I think it *does* matter.

When it is observed that scientists have not found the five alternatives yet, and so it is the case that it costs a lot of time and effort to find those five alternatives to H after I have already found H, this might be an indication for the fact that H is simpler[98] than the five alternatives I am trying to find. And, as was already mentioned in Section III.2 about non-empirical virtues, a high amount of simplicity possessed by a theory could enhance the chance for a theory to be viable. So in this way, the observation that scientists have not yet found alternatives to H could have an influence on the chance for H to be viable, despite the fact that you already know how much alternatives there are. And so according to this reasoning F_A and T are not statistically independent, given Y.

Of course, we also already encountered the problems of supposing such a relation between a non-empirical virtue and the chance for viability. But those problems could be considerably reduced by MIA, and there are indeed some examples from the past in which simple theories turned out to be better theories, which makes the connection between simplicity and empirical adequacy more

[96] Dawid, Hartmann, Sprenger, *No Alternatives*, 7

[97] Which is indicated by the observation that the scientist have not yet found the alternatives yet.

[98] Assuming that a simple theory is easier to find (for example because they imply less complex processes and less new unknown concepts).

plausible. Still, this connection would be too weak to completely ground the choice for a theory upon, but that is also not what happens here, we merely *increase the chance* for a theory to be empirically adequate. And even when we find out that a simple theory just a couple of times (maybe just one or two times) more than a complex theory turned out to be the better one, this would already create a little connection between simplicity and empirical adequacy, enough to increase the chance for H to be empirically adequate a little bit, and just that little bit of increasement is already enough to make **A1** not true, because according to **A1** no increasement (or decreasement) would be possible whatsoever.

Another possible point of criticism of the idea that the observation that scientists have not yet found alternatives for H could be explained by a significant difference in simplicity of H with respect to the possible alternatives, is that such a difference could not be possible because of the constraints C: the constraints that every theory has to fulfil. One then alludes to a certain type of constraint that guarantees the same amount of simplicity for every theory. But it is unlikely that such a constraint would really be used by the scientific community. Such a constraint would namely rule out theories which have a lower amount of simplicity (but still have enough to be manageable) but do have other great assets and high amounts of other virtues. Furthermore, it remains to be seen if theories with the exact amount of simplicity are likely to exist at all and if they exist, that they could be qualified as such by scientists. The constraint of a minimal amount of simplicity on the other hand is expectable, but above that minimum, fluctuations in simplicity are of course still possible.

Furthermore, one could object against introducing the amount of simplicity of alternatives as a factor of influence on F_A by stating that the influence of the simplicity of the alternatives on F_A is already covered by D. This however, is not true. Because D is about the difficulty of the *problem*, not of the solutions (H and the alternatives). Thereby it was assumed that D is statistically independent of T, something which we take not to be true for the simplicity factor.

But there is also another way through which F_A and T could be statistically dependent, given Y.[99] Suppose that I have found hypothesis H and the theoretical framework where H is also part of predicts that there are another eighty possible empirical adequate hypotheses, but I have found none or an extremely small amount of those eighty alternatives. That seems really unlikely, even if they are complex theories and therefore hard to find, I should still be able to find at least more than a few of them, am I not? An explanation could be that the used theoretical framework has an inconsistency. For example the way the framework predicts how many alternatives there are could be at odds with the rest of the framework, because it does not gives the right prediction. Or maybe the way of predicting suits the rest of the framework, but the predicted number of alternatives could not be found because some other parts of the theoretical framework are (unknowingly) inconsistent, which inhibits the construction of (some) alternatives. The suspicion of an inconsistent framework could be really problematic, because it indicates that you might be looking in the wrong way to reality, an inconsistent framework could namely never be the framework

[99] Important for this way is that Y must be given by a certain theoretical framework and not 'magically revealed by reality'.

that structures reality accurately,[100] and so you are on the wrong track. And since H is also part of your probably wrong theoretical framework, H itself also becomes less probable. Also in this case the observation that scientists have not found alternatives yet indicates a hidden factor that influences the chance for H to be empirically adequate, even if the number of alternatives is already known. Again T and F_A are not statistically independent, given Y.

D. Modifying the Bayesian Network

Below (Figure 5) I present a modified version of the earlier encountered Bayesian network (Figure 4), which I think gives a more accurate representation of non-empirical theory assessment in reality.

The added variables in the second network have the following meanings:

S^a ('Simplicity a') is a measure of the simplicity of H and has values in the natural numbers, with $\mathrm{S^a_g} := \{S^a = g\}$. So the interpretation of $\mathrm{S^a_g}$ is as follows:

$\mathrm{S^a_g}$ The simplicity of hypothesis H could be identified with the magnitude of the number g.

S^b ('Simplicity b') is a binary variable which could take the following values:

$\mathrm{S^b}$ Hypothesis H is much simpler than its k alternatives.[101]

$\neg\mathrm{S^b}$ Hypothesis H is not much simpler than its k alternatives.

W ('Suitability Frame$W$ork') is also a binary variable and has the values:

W The framework to which H belongs is not suitable.[102]

$\neg$W The framework to which H belongs is suitable.

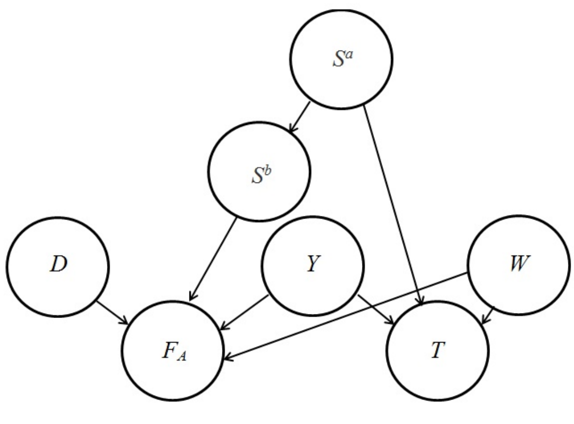

Figure 5: Bayesian Network 2 - Network 1 to which S^a, S^b and W are added.

[100] Assuming reality is consistent of course, which will not be subjected to many objections I suspect.

[101] Alternative formulation: The k alternatives are a lot more complex than H.

[102] A suitable framework is a framework that has no inconsistencies and so could possibly be an accurate ('true') picture of reality.

Before we move on, two short remarks: often it will be useful to understand 'Simplicity a' and 'Simplicity b' as just one factor named 'Simplicity' and because that what 'Simplicity' and 'Suitability Framework' have in common is that they both give an other explanation for the value of F_A than Y *and* are capable of influencing T, these two (and eventual other factors that are able to do this) could be taken together under R: the group of all possible factors that make an influence on T by F_A possible even when the value Y is known (see Figure 6).

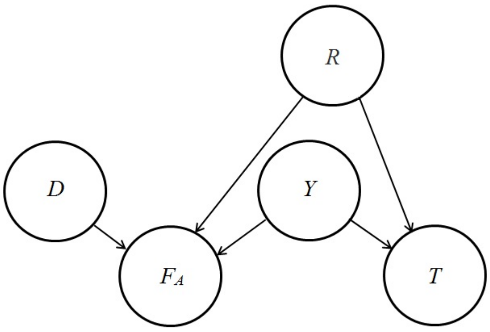

Figure 6: Bayesian Network 3 - Network 1 to which R is added.

E. Is Theorem 1 still Valid? Intuitive

So, through the 'detour' via the factors 'Simplicity' (S) and 'Suitability Framework' (W) F_A could still have influence on T despite the fact that Y is known. And therefore assumption **A1**, $T \perp\!\!\!\perp F_A|Y$, might fit the reality of the research practice less than thought. But this immediately raises the question what will happen with the proof of Theorem 1, since assumption **A1** played a crucial role in it. Could $P(\mathrm{T}|\mathrm{F_A}) > P(\mathrm{T})$ still be proven in Bayesian Network 2?

Let us first have a look at the changes intuitively. If only 'Simplicity' would have been added to the model, our intuition tells us that Theorem 1 is probably still valid: the fact that it costs the scientific community a lot more effort to find alternatives for H still supports the empirical adequacy of H. It does namely not matter if the great difficulty to find the alternatives comes from a lack of alternatives or from the fact that the possible alternatives are a lot more complex than H, both reasons enhance the chance for H to be empirically adequate. What is different in this case however, is that in an explanation of the 'No Alternatives Argument' one should not only mention the assumed connection between F_A and Y as reason for the effectiveness (=the power to enhance the chance for H to be empirically adequate) of NAA, but also the connection between F_A and S^b. To distinguish NAA with this explanation from the 'classical NAA', we could call this the 'No (Simple) Alternatives Argument', if we like. But I suspect that just using the name NAA for both the cases does not cause any problems.

If 'Suitability Framework' is also added to the model it becomes quite more difficult to say something intuitively about the effect of F_A on T. Because while $\mathrm{F_A}$ via the path 'Simplicity' and the path 'Number of Alternatives' (Y) enhances the chance for H to be empirically adequate, via the path 'Suitable Framework' it is exactly reversed: $\mathrm{F_A}$ lowers the chance.[103] A way to

[103] A little reminder: this was because the observation that scientists have not found alter-

nevertheless find out more about if the chance for H to be empirically adequate will raise or lower, is by giving an estimate of how probable the occurrence of the possible paths is. If it turns out that the reason for the lack of found alternatives is more likely to be a small set of alternatives possible in principle or the fact that the alternatives are a lot more complex than H, in other words, if the chance for the paths 'Number of Alternatives' and 'Simplicity' is higher than the chance for the path 'Suitability Framework', then the chance for empirical adequacy of H increases when $\mathrm{F_A}$ is the case, and so Theorem 1 is still valid.

F. Is Theorem 1 still Valid? A New Proof

Assumption **A1** was important for the Bayesian proof of Theorem 1, so it will be interesting to see if a proof of Theorem 1 could still be given now assumption **A1** is no longer available in its original form. For such a new proof we need a new assumption. I think there are three ways to modify assumption **A1**:

1. *F_A and T are not conditionally independent, given Y: $F_A \not\perp\!\!\!\perp T|Y$*
   This seems, considering what is discussed before, on first sight the most logical choice.

2. *F_A and T are conditionally independent, given Y, W, S^a:*
   $F_A \perp\!\!\!\perp T|Y, W, S^a$
   This assumption is in Bayesian Network 2 the equivalent of assumption **A1** in Bayesian Network 1. This assumption is appealing, because now F_A could no longer influence T via S^a or W and so, in contrast to **A1**, F_A and T are now really conditionally independent.[104]

3. *Total disposal of **A1** and coming up with completely new assumption*

I have already made a start with constructing a new proof, but unfortunately, my attempts were not very successful. I wanted to start with picturing how the new assumptions would change the course of the original proof of Theorem 1. So I followed the line of thought of the original proof, but used a modified assumption and the variables associated with the extended Bayesian network (Figure 5).

A proof following the outset of the original proof and with modified assumption 1 failed, because, as could be expected, it is a problem when some expressions could not be expanded (see subsection B in this section). Also the attempt with modified assumption 2 was not really successful, because the proof became really fast really complex. This is due to the fact that there should be many variables included in the proof. I already considered S^a and S^b as just one variable S, but it remained very complex. Also the fact that W lowers the chance of T while S and Y raise it, provided an extra challenge.

However, the fact that my attempts seem not very promising, does not mean that proving NAA in Bayesian Network 2 is impossible. I suspect that some of

natives yet could indicate an erroneous framework.

[104]If it might be the case that another factor could be thought of that influences F_A and also influences the chance that H is empirically adequate, in other words, a factor that could make that knowledge of the value of F_A, despite the fact that Y, W and S^a are known, could be an influence to T (and so F_A and T are not conditionally independent), then it seems obvious to also include this new factor to the list of given factors. By adding a factor to this list, you namely prevent that the factor enables 'via a detour in the Bayesian network' influence of F_A on T. (So the assumption could also be written as: $F_A \perp\!\!\!\perp T|Y, R$.)

the encountered problems could not be solved by me because of my inexperience with Bayesian analysis, furthermore, I have not investigated in detail proofs that do not follow the set-up of the original proof, but start from scratch and are exclusively based on the extended network and a modified assumption. An idea of such a kind of proof was already mentioned at the end of subsection E of this section, which could be formalized by first proving individually for the added variables that they influence the chance of T and subsequently, in order to prove NAA, that Y and S raise the probability of T more than W lowers it. This, however, would require specific values of the probabilities, which is a problem, because we want a general proof. But maybe it could be shown (by investigating some actual cases) that always when we observe $\mathrm{F_A}$, Y and S are much more likely to be the cause of it, processing this finding in the Bayesian analysis would however imply imposing another constraint.

I also would have liked to proof that $P(\mathrm{T}|\mathrm{F_A},\mathrm{Y}) > P(\mathrm{T}|\mathrm{Y})$[105] in Bayesian Network 2, formally showing that T and F_A are not independent given Y. This would, together with enough justification for adding S, W and eventual other factors, justify my claim that assumption **A1** might not be correct. This proof was however also not (fully) found by me, but again, that does not mean that it does not exist.

4. Summary

The goal of this chapter was to make NAA more plausible, and I hope it is achieved. It might still seem a bit odd that an observation about the research process could confirm a theory, but on the other hand, this is exactly what has been proven to be possible in this chapter, at least in a Bayesian model of theory confirmation.[106]

For this proof five assumptions were needed, one of them was criticised. In short, this criticism came down to the fact that the original proposed Bayesian model was not complete and did not resemble the real confirmation process enough. Therefore a more extensive Bayesian picture was presented.

At last we have taken a look at some ideas on proving NAA again in the more extensive picture, there were also made some proposals for a modification of assumption **A1**.

Before we continue, let us pause and take a look at what we have done up to now. First we had discussed the problem and its possible solutions. One

[105]Or: $P(\mathrm{T}|\mathrm{F_A},\mathrm{Y}) < P(\mathrm{T}|\mathrm{Y})$.

[106]It is important to remain critical towards this result. Because the fact that NAA could be proved in the used Bayesian network, does not automatically mean that it is therefore a completely reliable and uncontroversial argument. This is because it is not proven independent of the network, and the used assumptions and the construction of the network could be wrong. Maybe there is for instance no connection between the amount of alternatives found by scientists and the alternatives possible in principle. With proving NAA in the used Bayesian network we only show that in what we consider is (approximately) a reliable model of the confirmation process, NAA is indeed a viable argument. In other words: we prove that it is possible to provide a Bayesian model of a way people think, not that this way of thinking is necessarily good and justified. However, being able to capture NAA in a Bayesian picture does raise the plausibility of NAA because it provides a better insight in the argument and it satisfies the possible demand that in order to be good, an argument must be able to get represented in a Bayesian way.

of those solutions, non-empirical theory assessment was then discussed in some more detail, because it is both the most unreliable and uncertain solution as it is the most promising one in times of empirical crisis. We saw its advantages and disadvantages, its problems and its supports, of which the proof of NAA might be the strongest. Now we have collected enough material to dive in some interesting details about the current situation in physics and the current philosophical debate about the described problem and non-empirical theory assessment. This will be done in Chapter V.

Chapter V - *A New Way of Doing Science?*

We have seen that non-empirical theory assessment might provide a solution in times of empirical data shortage. We discussed how this method works, encountered its weaknesses but also saw that it is maybe more plausible than initially thought. But do we really want it? Do we really want non-empirical theory assessment to enter science? Is it reliable enough to give us a trustworthy insight in nature? Does it fit our ideas about science? And if not, does it actually have to? In Chapter II it was mentioned that using non-empirical theory assessment means an adjustment of the empirical methods conventionally used in science, do we really want to go as far as changing the way of doing science in order to solve the problem?

In this chapter we will discuss some answers to these questions and other interesting ideas taken from the young but important and intriguing debate on non-empirical theory assessment and the growing problem of the lack of empirical data. It will of course not be possible to discuss all the ideas presented in this debate, also some ideas might not always get the full attention they deserve, simply because that would take up too much space and because it is not necessary for the purpose of this chapter, which is giving a glimpse of what is currently going on in the debate.

But before we will come to these ideas, first something else will get our attention. It was already mentioned a couple of times before that (elements of) non-empirical theory assessment always played a role in science, but this role was taken to be a lot smaller than the role it might get in the future. But what was not mentioned before, is that this future might have already started. Because in some fields of physics, some of the discussed non-empirical types of reasoning are already used more often and more explicit than one might expect. A short discussion of this is what we will start with in the first section.

1. A Non-Empirical Method is Already Used

One of the physical fields in which the use of a non-empirical method is the most evident, is string theory. String theory is highly trusted by a substantial group of physicists, while empirical evidence for the theory is absent. According to conventional methods of science, such a theory should not be granted such an amount of trust. It turns out that some of the main reasons physicists give for their trust come down to NAA, UEA, and MIA.[107] The fact that some scientists apparently use these arguments should however not be seen as evidence for the viability of them. Because it were exactly the reasons given by string theorists for their trust that was the source of inspiration for the formulation of NAA, UEA, and MIA in the first place. Dawid, who proposed the concepts, had namely as goal to find the reasons for the trust and to give them a firmer footing.[108]

To get an idea of how the non-empirical arguments[109] NAA, UEA and MIA

[107]Dawid, *String Theory*, 31-37
[108]Ibidem, 9-11
[109]Keep in mind that NAA, UEA and MIA *are* empirical on a meta-level, as they convert an *observation* about the research process to trust in a theory. However, when saying that they are non-empirical arguments, the word 'empirical' refers to observations related to the theory *itself*.

are made concrete for a real scientific case, it will now be shown why they could be applied to string theory according to the confident group of scientists. This will serve as an illustration of non-empirical reasoning already applied in science.

The confident string theorists use NAA because they believe string theory is the only theory that provides a viable option for constructing a unified theory of microphysics and general relativity, or in other words: of elementary particle interactions and gravity. String theory is not the only theory dealing with questions of quantum gravity, but other approaches only focus on a unification of quantum mechanics and general relativity, while string theory also integrates into the overall theory some important concepts from high energy physics and cosmology, which string theorists take to be crucial for a truly unified description. So for such a truly unified description, the other approaches do not constitute alternatives. Furthermore, they believe that if the other approaches would find viable results, they would blend into the string theory research program if put into the context of contemporary particle physics.[110]

UEA could be used, because the basic posit of string theory provided far more insights and explanation than was expected when it was formulated. The basic postulate of string theory is actually very simple, it states that elementary objects are extended (instead of point-like). It was introduced to solve a technical problem, but later it turned out that it provided all kinds of surprising deeper explanations, such as that it did not just facilitate a description of gravity, but that it actually *implied* it. This and other explanations of seemingly unconnected facts or theoretical concepts shows that the introduction of the new theoretical principle in the form of the extendedness of elementary objects surprisingly provided a more coherent theoretical picture. This is what string physicists take as a sign that they are on the right track.[111]

MIA is used by string theorists to support their trust, because string theory is developed within the conceptual context of high energy physics, wherein a trusted but at first empirically unconfirmed theory already occurred, namely the theory of the standard model. Just like in the case of string theory, there were no alternatives found that were equally satisfactory at a theoretical level. In addition, unexpected explanations and interconnections emerged. The fact that the standard model was at the end impressively confirmed by experiments enhances the trust that this will happen with string theory too. [112]

Of course not everyone agrees on the applicability of the arguments to string theory, or they make some critical notes on the reasons why some string theorists think it is possible to apply the arguments. Especially the question which cases could serve as input for MIA[113] is open for much discussion. The debate on the applicability of the arguments focusses mainly on physical details and is for our current discussion not very important, for which it was only interesting to show *that* non-empirical discussions and considerations already happen in physics, probably more than expected. Therefore we will not go into further detail about specific details on the case of the non-empirical confirmation of string theory.

[110]Marshall, *post-empiricism* and Dawid, *String Theory*, 31, 32
[111]Marshall, *post-empiricism* and Dawid, *String Theory*, 33
[112]Marshall, *post-empiricism* and Dawid, *String Theory*, 35
[113]In other words: which theories were empirically unconfirmed and supported by NAA and/or UEA, and eventually turned out to be viable.

However, before we continue, I would nevertheless like to mention one of the people that provides criticism related to the contents of the non-empirical arguments applied to string theory, that is theoretical physicist Peter Woit, who for example disagrees with Dawid on the suitability of the discovery of the Higgs-particle as input for MIA.[114] Another interesting comment from Woit directed to the work of Dawid is that in Dawid's description of string theorists and their trust in the theory 'string theorists are still partying like it is 1999',[115] in other words, Dawid's description and observation of happenings in string theory he tries to explain are out-dated. As promised, no further elaboration on the string theory case will follow, but this is probably a starting point for some further research, because if the string theorists' trust really changed as Woit suggests, it is interesting and important to analyse the reasons for this, as this might does or does not support the earlier used arguments (when it was still 1999). For instance, when it turns out that presently the trust in string theory has decreased and there are also (more) satisfactory alternatives found, this supports the idea that in the past the low(er) amount of alternatives indeed caused the trust. For now we will leave this case, for our discussion it is not so important when exactly the outburst of trust happened, also when it happened earlier than thought it still shows that a major role for non-empirical theory assessment in scientific reasoning is not just something for the future.

String theory is not the only example we could give of a research field in which non-empirical reasoning is used more than we might had expected for contemporary research. According to Dawid, another moment where non-empirical arguments were used to a large extent, was during the discussions around atomism. But non-empirical reasoning could also be found outside physics, where it often comes more naturally, is sometimes perfectly reputable and has already been used for a long time, this applies for instance to historical sciences such as paleontology or archeology, where they always have to deal with a shortage of data, because a lot of it has already decayed.[116]

So now we have seen that non-empirical theory assessment instead of empirical theory assessment is sometimes already used in science. We must be careful however not to settle with the idea that this non-empirical reasoning is just the type of non-empirical influence that is always present in science, because the role of it is much bigger now: it is the crucial factor where the choice for a theory is based upon, as it serves as a replacement for empirical evidence. But, the fact that non-empirical theory assessment, and therefore a post-empirical method, is already used sometimes in science, does not mean that it is therefore also a perfectly fine, reliable method in every context. So before the method could be applied on a larger scale, it needs some thorough investigation. We already made a start with that in the previous chapters, but the current philosophical discussion stayed out of sight. That is why we will now turn to some ideas of philosophers and physicists on the use of a post-empirical method, especially the possible use of it to solve the problem of the lack of empirical data in modern physics.[117]

[114]Woit, *String theory and post-empiricism*

[115]Woit, *Scientific Method*

[116]Marshall, *post-empiricism* and Dawid, *String Theory*, 98-101

[117]A lot of the ideas stem from the debate on using non-empirical theory assessment in string theory, this should be no surprise, since that is at the moment the research field with the most problems related to a shortage of empirical data. However, in the upcoming section, I tried

2. Philosophers and Physicists on Non-Empirical Theory Assessment

As could have been expected, Dawid is a proponent of using a post-empirical method if necessary. He talks about this in the book we have already mentioned a couple of times now (*String Theory and the Scientific Method*), but he also brings it up in an interview, where he says the following: 'I argue that observations which do not constitute empirical evidence for a given scientific theory can still make it more likely that this theory will eventually turn out empirically viable.'[118] Which is exactly what a post-empirical method that uses non-empirical theory assessment entails.

However, not everyone agrees. The theoretical physicist Smolin sees the use of non-empirical arguments in science as no more than a symptom of 'groupthink':[119][120] a desperate attempt to uphold a theory, if not by the characteristics of a theory itself, then by setting new standards to the way of doing science.

Although I do think it is important to watch out for groupthink, especially in times when we might change conventional ways of doing things, I think it is too easy to just dismiss the whole idea of using non-empirical reasoning as just a form of groupthink. It marginalizes the substantial argumentation behind it, it neglects the competence of the group in question and above all, it is not possible to criticise the idea of this groupthink, because every attempt to deny the occurrence of groupthink could again just be dismissed as another example of it, as 'defensiveness' is often taken as a sign of groupthink.

Dawid has also said some things in reaction to the idea of groupthink of Smolin. One of them is that for the group of string theorists, a group that, as we saw, uses non-empirical arguments, this would be a grossly one-sided and inadequate description, or as he puts it: 'The picture of a sheepish group following the directives of a few prophets would be an obvious misrepresentation of the actual situation in the field.'[121] Which shows a situation wherein non-empirical reasoning could not be attributed to groupthink and this also brings attention to the fact that it is apparently hard to objectively determine what counts as groupthink, as these two persons, Smolin being a real proponent of the idea of groupthink in string theory,[122] already disagree about it.

Furthermore, Dawid has also some more general objections against arguments of critics on non-empirical theory assessment such as Smolin, two of them will now be discussed. First, he deals with the remark from critics that non-empirical theory assessment is a deviation from the path of legitimate scientific reasoning. Against this he argues that is not enough to point out that a certain method does not agree with a more traditional notion ('the path of legitimate scientific reasoning') in order to criticize that strategy, because then you would

to present the ideas more generally, so that they could also be used for forming an opinion on non-empirical theory assessment in general and for the use of it in possible other cases than string theory in the future.

[118]Marshall, *post-empiricism*

[119]He actually speaks about this in the context of string theory, but I think it is safe to assume that he will also take the use of non-empirical arguments in another field of physics as a result of groupthink, because of his strong emphasis on empirical evidence as the only reliable type of evidence.

[120]Marshall, *post-empiricism* and Smolin, *Response*

[121]Dawid, *String Theory*, 27, 28

[122]Smolin, *Response*

'implicitly presume that there is an unchanging conception of theory confirmation that can serve as an eternal criterion for sound scientific reasoning.' And something like that does not exist, a conception of theory confirmation is not god-given: it will always vary based on the scientific process.[123]

The second objection is that some of the comments of critics show that they misunderstood the nature of the non-empirical arguments. It is namely sometimes (implicitly) suggested by the comments that the arguments are arbitrarily and uncritical, also the meta-empirical character of the arguments is sometimes forgotten.[124] While this meta-empirical character, which means that the arguments are based on meta-level *observations* about the research process, is also just what makes the arguments not arbitrarily, because of the inspiration from the research process, and not uncritical, because of the constant feedback on them provided by MIA, which uses meta-level observations on the success of theories.

I think that with the first objection Dawid has a good point, but we still need to be very careful not to refute old methods and conceptions too easily.[125] However, the second objection might not apply to every critic. Of course there are probably indeed some that did not understand, or are not aware enough of the nature of the arguments, but for instance Woit already mentions that there is nothing wrong with his ability to understand. What is more, he mentions in response to Dawid's objection that he dedicated a whole chapter of his book to a version of NAA.[126] Though, I do have a suspicion where the claim from Dawid about the non-understanding by a critic like Woit comes from, because besides some minor differences in what they take to be the content of the argument, there is another difference between Woit and Dawid: where Woit seems to take exclusively the contents of the argument to be the source of the power of it, it is Dawid who emphasises that this should be complemented by the fact that there is a connection between the applicability of (any version of) NAA and the viability of a theory on a meta-level.[127] So that is a point for Dawid after all.

However, Dawid can not always win. Because although it is not really an argument about whether you should or should not use a non-empirical method (it is rather about the form of it), Woit touches on a good point by saying that during the formulation of his version of non-empirical theory assessment[128] Dawid naively accepted all non-empirical arguments used by string theorists.[129] I do not think I agree that Dawid's acceptance was naive, because he devoted a substantial part of his book to investigating and supporting the arguments.[130] Nevertheless, it is true that the non-empirical arguments came from string theorists and that it appears that Dawid did not really try to find other post-

[123]Marshall, *post-empiricism*

[124]Ibidem

[125]After all, if we may use MIA for non-empirical theory assessment, we may also use it for empirical theory assessment, and then we will see that empirical theory assessment will get much support from successful cases in the past. So non-empirical theory assessment will really have to work hard in order to come close to being some sort of a replacement.

[126]Woit, *String theory and post-empiricism*

[127]A connection established by using MIA.

[128]A reminder: this formulation resembles to some extent the non-empirical theory assessment discussed in Chapter III, especially the section about considerations about the research process was based on Dawid's work.

[129]Woit, *String Theory and post-empiricism*

[130]Dawid, *String Theory*

empirical approaches or other non-empirical arguments than the ones he had heard of in the field. An unmentioned but interesting approach is for example Woit's idea that physicists should look carefully at how mathematicians make progress, because mathematics is also 'post-empirical' and has 'a long history and a deeply-ingrained culture that helps mathematicians figure out the difference between promising and empty speculation.'[131]

Another remark of Woit on Dawid's proposal for non-empirical theory assessment is that it does not come with (enough) protections against 'all-too-human failings', with which he means failings such as wishful thinking and the earlier discussed groupthink.[132] I think Woit is right, because although Dawid is more careful than Woit suggests, I suspect that the system he proposes is more susceptible for groupthink than what is desirable. This suspicion comes from the fact that MIA has such an unrestricted and open character that it could be both used to support and to weaken NAA and UEA, it just depends on the choice of which former theory assessments you take into consideration and the way you interpret them. This could also clearly be seen in the current philosophical debate on non-empirical theory assessment, both proponents and opponents use former theory assessments to support their convictions. However, I think that this should not have to be an insurmountable problem for Dawid's non-empirical theory assessment, as long as there will be clear agreements and an as objective as possible research to former cases and the arguments used there.

Woit's caution is really understandable. Because during all the talk about the details of non-empirical theory assessment it is sometimes easy to forget what it is really all about: finding true, or viable, theories about nature. And could that really be done with non-empirical reasoning? Is it really possible to establish the existence of complete extra dimensions and universes just based on the fact that we humans were not able to think of something else? Of course this all needs some more nuance and of course we have already investigated some reasons that make such a relation between the research process and the viability of theories more plausible, but it is important to take a step back sometimes and ask ourselves if we were not too much caught by, for instance, wishful thinking.

This type of reflection by the proponents is however not completely absent, as for example is shown by Dawid when he talks about the reliability of non-empirical theory assessment during an interview.[133] But I think it would not be a bad idea if there was a little bit more carefulness, reflection and protection against wishful thinking on the proponents' side. Also, proponents might get so carried away by post-empiricism sometimes that they might forget some obvious objections and consequences. But also the opponents could be criticised, namely because of the fact that it sometimes seems that they knowingly or unknowingly are not even open to the possibility of non-empirical theory assessment in advance. Furthermore, as we have already discussed, critics sometimes make mistakes in their understanding of non-empirical theory assessment. We have already seen some examples of the above mentioned behaviour of proponents and opponents, but now it will be extended with some other examples taken from a recent article by the opponents George Ellis (mathematician and physicist) and Joe Silk (physicist).

[131] Woit, *String Theory and post-empiricism*
[132] Ibidem
[133] Marshall, *post-empirical*

The first example will be what I think is a mistake in understanding from the opponents side. Ellis and Silk state in their article that proponents 'began to argue explicitly that if a theory is sufficiently elegant and explanatory, it need not be tested experimentally.'[134] But that is not true. Proponents do not claim that the theory does not need to be tested experimentally if it is sufficiently elegant and explanatory, on the contrary, it does need experimental testing. But if this testing is not possible for a long period of time, then, they claim, it is possible to already give a prediction of how viable the theory will probably turn out. This does not mean however, that when new experimental data becomes available after such a prediction, the data is not necessary any more. Empirical testing will always remain the ultimate goal.[135] And when, before the data became available, an assessment was already made with non-empirical reasoning, the empirical data will also provide a test of how good this reasoning was. Furthermore, by saying that if a theory is sufficiently elegant and explanatory, it needs no empirical testing, one neglects that the possibility to use elegance and explanatory power as an argument for a theory also needs to have some sort of empirical basis. Because there needs to be an established connection between those virtues and the viability of theories based on earlier (successful) assessments (MIA) in order to use them as an argument.

Another mistake Ellis and Silk seem to make, is that they did not really understand the purpose and meaning of NAA.[136] Because in their article they say, apparently as counterargument for NAA: 'We cannot know that there are no alternative theories. We may not have found them yet.'[137] And while this statement is true, it is not a counterargument for NAA. Because NAA does not claim that it is certain that there are no alternatives in principle, and it also does not rule out the possibility of undiscovered alternative theories. NAA actually states that at a certain moment there are no alternatives found, indicating that there are probably not so many alternatives in principle, so that if there are alternatives found eventually, it are probably just a few of them. So, it is true that we cannot know that there are no alternative theories, but at least we could give an estimate of the number of alternatives with the help of a.o. NAA.

Furthermore, by using a sentence as 'post-empirical science is an oxymoron',[138] it seems that Ellis and Silk are not even really open to the possibility of post-empiricism. It just seems a bit too easy just to call something contradictory. Especially when there is a problem such as a lack of empirical data which old ways to do things cannot solve, every option needs to be taken seriously, and also old ideas such as your ideas about science should be available for questioning. Thereby, also the solution they propose for the problem in modern physics: 'physicists, philosophers and other scientists should hammer

[134] Ellis and Silk, *Defend*

[135] In their article, Ellis and Silk say that 'a theory must be falsifiable in order to be scientific' (Ellis and Silk, *Defend*), implying that proponents of non-empirical methods would not agree with them. But that does not have to be the case. Now that we have emphasised the important role empirical findings still have, it is easy to see that a proponent of non-empirical theory assessment could also demand falsifiability, because that demand does not conflicts with non-empirical theory assessment, it is perfectly possible to give an estimate of the viability of a theory and demand that the theory could in principle be tested with experiments.

[136] So this is again an example of Dawid's objection to the address of critics that they do not always understand the nature of the non-empirical arguments correctly.

[137] Ellis and Silk, *Defend*

[138] Ibidem

out a new narrative for the scientific method that can deal with the scope of modern physics.'[139] is a little bit non-committal in my eyes, because they do not come up with a proposal or directions for this and so it is not really clear what they mean by 'a new narrative.'

So now we have seen some examples of criticism on the reasoning of opponents, but as promised, we will also discuss some examples of comments on the behaviour of proponents. A little reminder: the main problems on the proponents' side were that they are not always careful enough, and, connected with this, that they might forget some objections and consequences of non-empirical reasoning sometimes.

One of the examples is that Ellis and Silk justly note that the premises proponents use might be wrong, which could undermine some non-alternative arguments.[140] If you, for instance, want to use NAA or UEA for string theory, you must already assume that there exists an overarching, unified theory. Otherwise it is not possible to say that the chance for a specific theory to be 'the viable one' increases when it turns out that there are not many alternatives in principle for that theory. Because without the assumption, there might not even be 'a viable one' at all.[141] But this assumption, this premise, could of course be wrong, there might be no need for an overarching theory. This could also apply to other explicitly or implicitly used premises by proponents.

Another objection of Ellis and Silk on non-empirical reasoning which I think is not really noticed or at least not extensively discussed by proponents, is that it 'could open the door for pseudoscientists to claim that their ideas meet similar requirements'[142] (as 'normal' theories). I think this is a serious problem, for which there is no easy solution.[143] However, I do not think that the problem is severe enough to reject non-empirical reasoning for, but it will bring some interesting challenges to the demarcation debate.

At last, Ellis and Silk also make another interesting remark, a remark that is not really an objection to the proponents' reasoning, but still important to mention. In their article they warn for the 'potential damage to public confidence in science' in times where 'scientific results - in topics from climate change to the theory of evolution - are being questioned by some politicians and religious fundamentalists.'[144] I think it is indeed important to be a bit cautious, as long as it does not influence the debate between scientists and philosophers and as long as the proponents will still be taken seriously and not get depicted as reckless rebels damaging the reputation of science.

3. Kuhnian Paradigm Shift?

Dawid characterises the dispute between proponents and opponents of non-empirical theory assessment 'as a discussion that fails to be productive due to a paradigmatic rift between the two disputants: each side bases its arguments

[139] Ellis and Silk, *Defend*

[140] Ibidem

[141] In other words: you must first assume that there exists a viable unified theory, before you could determine the chance for a hypothesis to be that theory.

[142] Ellis and Silk, *Defend*

[143] Perhaps it is a start to demand that theories have to be firmly rooted in other extensively empirically tested theories and research programs in order receive non-empirical support.

[144] Ellis and Silk, *Defend*

on a different set of fundamental preconceptions.'[145][146] Or in other words: the debate shows aspects of a Kuhnian paradigm shift.

I will not try to summarize what Dawid has said about this interesting topic, because it is written very clearly in his book and it could not be shortened without missing some important details.[147] However, I would nevertheless like to mention the points in which this case *differs* from a classical Kuhnian case. Dawid already mentioned two of the reasons, namely that the shift did not happen in a revolutionary way and that it is a shift on a meta-level, because it is not about certain scientific theories, but about the methods used to do science (you could call it 'meta-paradigmatic').[148] Another point in which the case in my opinion differs from a 'normal' Kuhnian paradigm shift, is that, although the proponents might not always really understand why the opponents do not understand them, the proponents do still understand *and* use (if possible) the 'old' scientific method that the opponents strongly defend, namely the one of empirical theory assessment. Using elements from old paradigms is not new, think for example about Newtonian physics which is still used, but what is special is that in the current case an element from the older paradigm, the 'older' empirical method, is considered better and more trustworthy than the non-empirical method which belongs exclusively to the new paradigm.

It actually might be a bit too early to determine with complete certainty how the debate should be characterised, it is however interesting to think about, and maybe in a few years, when the debate has progressed, we could say more.

4. A Different Kind of Science?

It is not easy to give a good definition of science, such a conception of what science is varies from time to time and from context to context. However, the current conception of science most people have will probably at least contain a reference to the importance of empirical findings and the fact that theories must be tested against nature (at least in physics). So how will that conception change if we allow non-empirical theory assessment in science? Could it still be called 'science' then?

For now we will focus on the future of physics. Ellis and Silk argue that 'theoretical physics risks becoming a no-man's-land between mathematics, physics and philosophy that does not truly meet the requirements of any'[149] if we accept theories that could not be empirically proven by us, as serious candidates. But I think that when the moment is there that we have found and investigated all empirical data possible for us humans to gather, then physics becoming a no-man's-land is always better than no physics at all. Although the flow of empirical data could stop someday, our human curiosity will not. And at that moment it is better to make 'highly educated' and by 'classical science' inspired guesses for the answers on our remaining questions than random, unfounded or no guesses at all. I think it is true when Ellis and Silk say that physics will be-

[145]This is actually about the debate on non-empirical theory assessment specifically in string theory, but I think it could easily be generalized.
[146]Dawid, *String Theory*, 28
[147]Ibidem, 28-31
[148]Ibidem, 28-29
[149]Ellis and Silk, *Defend*

come some kind of a mixture between mathematics, physics and philosophy.[150] But I do not think that it is a problem, as long as we do not see it as a failed or deformed variation of the current science, but as something new and different: the type of investigating nature we stumbled upon when we made the best out of the situation when 'classical, empirical science' was not possible any more.

5. Summary

In this last chapter we first saw that non-empirical theory assessment is not just something for the future, but that it is already used more explicitly than expected today. Thereafter we encountered a lot of different ideas about non-empirical theory assessment. Both the opponents and the proponents make some interesting remarks about this type of assessment and its possible role as solution of the problem of the lack of empirical data in modern physics. It was noticed that proponents had some problems with carefulness, forgotten consequences and objections, while opponents struggle with their understanding of the non-empirical arguments and the fact that they do not always show an open attitude towards non-empirical reasoning. But this might be due to the fact that there is a paradigmatic rift between the two disputants, making it hard for opponents to fully understand what proponents mean and (maybe to a lesser extent) vice versa, something we discussed in the fourth section. The last section was devoted to some ideas on the possible changes of our conception of science, concluding that a different form of science, something which we might not call science according to our current definition, is always better than no science at all.

With this we conclude our investigations to possible solutions for the problem of the (threatening) lack of empirical data in modern physics, especially to the promising but uncertain solution in the form of non-empirical theory assessment. Now we will turn to the conclusion to take stock and give some recommendations and expectations considering the solution of the mentioned problem.

[150] At the moment when a lack of empirical data obligates us to convert to non-empirical theory assessment.

Conclusion

Let us take a look again at the description of the problem in modern physics given in Chapter I:

The characteristics of theoretical statements in modern physics make that there is an increasing amount of energy needed in order to evaluate those statements by using empirical data. An amount which is very hard or maybe even impossible to provide. Therefore it is not easy or not possible to investigate and support the statements with empirical evidence, while this is an important part of conventional scientific methods. Without the important input of empirical data, science, according to these conventional methods, is strictly speaking not possible.

So, how are we going to solve this problem?

Again, I want to emphasize that eventually, the solution should of course come from physicists working in fields encountering this problem. But this thesis hopefully already shows that philosophy could also say some interesting things about the issue by exploring possible solutions and assessing their plausibility, and usefulness in the problem. So no instant action plan will be given, but the results from our exploration.

First it must be noticed that empirical theory assessment will probably always remain the best type of assessment, this type of assessment has namely already abundantly proven itself. And so solutions as waiting for technological improvement, developing new theories or making new predictions are the most desirable. However, we already saw that these solutions are not really realistic for the problem in modern physics. Although maybe some sort of theoretical solution, for instance in the form of the discovery of a certain relation between small and large length scales, could some day provide a surprising way out.

The most promising, but at the same time the most unreliable and uncertain solution is non-empirical theory assessment. As the name already suggests, this solution does not encounter problems from a lack of empirical data. However, this solution means a deviation from the conventional methods in science, which is why extra attention was given to it. It turned out that non-empirical theory assessment might be more plausible than initially thought, because some non-empirical methods, including the ones we discussed, still have some sort of (meta-)empirical component, are based on earlier experience, could be captured in a Bayesian picture and it is possible to philosophically explain why the methods could support a theory. Where we should mention that the type and amount of justification eventually needed for non-empirical theory assessment is depending on the goals you attribute to science. Non-empirical theory assessment is for instance probably easier to accept for an instrumentalist than for a realist. Concluding, empirical theory assessment will probably stay the ultimate goal, but in times of empirical crisis, and if there are no other solutions, non-empirical theory assessment might be a quite suitable option.

But, we must not forget, as we saw in Chapter V, that non-empirical theory assessment should come with some good agreements and the necessary protections against wishful thinking, group think and too much influence of personal preferences of physicists. So there is still plenty of philosophical work to do. Also because the acceptance of it will raise some questions about demarcation and it will bring some changes to our conception of physics. And more interesting questions regarding non-empirical theory assessment could be thought of. We could for instance explore some case studies, or try to find out how it is determined in physics which hypotheses qualify for empirical testing (if possible), because, as testing is expensive and labour-intensive, choices must be made, and so it will be interesting to see which indications are used by experimentalists (and theorists perhaps) to determine which hypotheses are promising enough to test, indications that must be non-empirical 'per definition', as they are used before experiments are done and new data is gathered.

Let us now turn to some expectations. Could we actually already make a prediction of which *type* of non-empirical theory assessment will be used, if it is used? I expect that it is likely that it will be a combination of different non-empirical methods, perhaps with different ascribed importance. The three methods discussed in this thesis, the ones based on non-empirical virtues, inter-theoretical connections and considerations about the research process, have a good chance to be part of the combination, I think. It is however very difficult, if not impossible, to give a reliable prospect, because experience and practice are also necessary to show what works and what does not.

It is of course also very difficult to envision *how* non-empirical reasoning will be actually used in physics if it is accepted in times of absence of empirical data. But, in order to still give an indication, I would like to discuss something that might be a potential scenario.

In this scenario, physicists *assume* that there is just one overall structure of theories that could describe nature best. This assumption is not ad hoc. When we take it for granted that there is just one 'true way' in which things happen in nature, we could come to such an assumption of one overall theoretical structure by recognizing that the 'real' structure in nature is very complex, and so it would be quite remarkable if we are able to find a perfectly not contradictory description that is not the true one.[151] Finding this one overall theoretical structure could then just be done with trial-and-error: just trying long enough until it is found. However, already found theories based on empirical data make the search easier, they reduce the possibilities by serving as boundary conditions. It is as if they are the borders of a jigsaw, or the edges of the theoretical web. And now also non-empirical theory assessment becomes important, because this type of assessment could, partly due to the strong connection some methods have with earlier scientific research, serve as guidance for finding the theoretical structure, making the search more efficient, as it helps to detect wrong tracks and point out promising ways. A combination of the in this thesis discussed non-empirical methods could be quite suitable for this goal. (Finding the unique overall theoretical structure in this way, could be somewhat compared with solving a Sudoku by just trying different configurations of numbers, with the already filled-in numbers as the constraint empirical data imposes, and rules such as no sevens next to each other and the numbers one to nine in a box as the rules that you gradually discover about what works in doing science: the rules you could use when assessing a theory non-empirically).

[151] Of course, much more could be said about this, but because it is just about an assumption in a possible scenario, and not a proposal of how to do science, this will not be done.

Let us end where we began: proof. Could we really speak about non-empirical proof? Probably not, according to our (current) conception of proof, it refers to something that brings more certainty. However, although non-empirical proof might strictly speaking not exist, at least, as we saw, non-empirical reasoning could provide some strong (and weaker) indications for and guidance to the viability of a theory.

So, about non-empirical reasoning. Could it provide proof in the way we are used to? Probably not. Is it good guidance? Most likely, yes. Is it worth giving it a chance in times of empirical crisis? Absolutely.

Appendix

1. Proof of Theorem 1

$\mathrm{F_A}$ confirms T if and only if $P(\mathrm{T}|\mathrm{F_A}) - P(\mathrm{T}) > 0$, that is, if and only if

$$\Delta := P(\mathrm{T},\,\mathrm{F_A}) - P(\mathrm{T})P(\mathrm{F_A}) > 0.$$

We now apply the theory of Bayesian networks to the structure depicted in Figure 4, using assumption **A1** ($T \perp\!\!\!\perp F_A|Y$) and **A2** ($D \perp\!\!\!\perp Y$):

$$\begin{aligned}
P(\mathrm{F_A}) &= \sum_{i=0}^{\infty}\sum_{j=0}^{\infty} P(\mathrm{F_A}|\mathrm{Y_i},\mathrm{D_j})P(\mathrm{Y_i},\mathrm{D_j}) = \sum_{i=0}^{\infty}\sum_{j=0}^{\infty} d_j y_i f_{ij} \\
P(\mathrm{T}) &= \sum_{k=0}^{\infty} P(\mathrm{T}|\mathrm{Y_k})P(\mathrm{Y_k}) = \sum_{k=0}^{\infty} t_k y_k \\
P(\mathrm{T},\mathrm{F_A}) &= \sum_{i=0}^{\infty} P(\mathrm{F_A},\mathrm{T}|\mathrm{Y_i})P(\mathrm{Y_i}) = \sum_{i=0}^{\infty} y_i P(\mathrm{F_A}|\mathrm{Y_i})P(\mathrm{T}|\mathrm{Y_i}) \\
&= \sum_{i=0}^{\infty} y_i t_i \left(\sum_{j=0}^{\infty} P(\mathrm{F_A}|\mathrm{Y_i},\mathrm{D_j})P(\mathrm{D_j}|\mathrm{Y_i}) \right) \\
&= \sum_{i=0}^{\infty}\sum_{j=0}^{\infty} d_j y_i t_i f_{ij}
\end{aligned}$$

Hence, we obtain, using $\sum_{k\in\mathbb{N}} y_k = 1$,

$$\begin{aligned}
\Delta &= \left(\sum_{i=0}^{\infty}\sum_{j=0}^{\infty} d_j y_i t_i f_{ij}\right) - \left(\sum_{i=0}^{\infty}\sum_{j=0}^{\infty} d_j y_i f_{ij}\right)\left(\sum_{k=0}^{\infty} y_k t_k\right) \\
&= \left(\sum_{i=0}^{\infty}\sum_{j=0}^{\infty} d_j y_i t_i f_{ij}\right)\left(\sum_{k=0}^{\infty} y_k\right) - \left(\sum_{i=0}^{\infty}\sum_{j=0}^{\infty} d_j y_i f_{ij}\right)\left(\sum_{k=0}^{\infty} t_k y_k\right) \\
&= \sum_{i=0}^{\infty}\sum_{j=0}^{\infty}\sum_{k=0}^{\infty} (d_j y_i y_k t_i f_{ij} - d_j y_i y_k t_k f_{ij}) \\
&= \sum_{j=0}^{\infty} d_j \sum_{i=0}^{\infty}\sum_{k\neq i=0}^{\infty} y_i y_k f_{ij}(t_i - t_k) \\
&= \sum_{j=0}^{\infty} d_j \sum_{i=0}^{\infty}\sum_{k>i} (y_i y_k f_{ij}(t_i - t_k) + y_k y_i f_{kj}(t_k - t_i)) \\
&= \sum_{j=0}^{\infty} d_j \sum_{i=0}^{\infty} \frac{1}{2} \sum_{k\neq i=0}^{\infty} y_i y_k (f_{ij}(t_i - t_k) + f_{kj}(t_k - t_i)) \\
&= \frac{1}{2}\sum_{i=0}^{\infty}\sum_{j=0}^{\infty}\sum_{k\neq i=0}^{\infty} d_j y_i y_k (t_i - t_k)(f_{ij} - f_{kj}) \\
&> 0
\end{aligned}$$

because of **A3-A5** taken together: **A3** entails that the difference $(f_{ij} - fkj)$ is non-negative, **A4** does the same for the $(t_i - t_k)$, and **A5** entails that these differences are strictly positive for at least one pair (i, k). Hence, the entire double sum is strictly positive. □

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