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The birth of mathematical physics -

Kepler's proof of the planets' elliptical orbits from causal hypotheses

Sigurd Tønnessen

A dissertation for the degree of
Philosophiae Doctor

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“If you want to know something about scientists and their methods you should stick to one principle: don’t listen to what they say, but look to what they are doing.”

Albert Einstein

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Summary

Johannes Kepler (1571-1630) was an iconographic scientist and one of the forefathers of the scientific revolution. His ground-breaking work on astronomy has been extensively used in the study of scientific progress. I have continued this tradition in this thesis, and have studied Kepler’s original work in order to understand his theory development. More specifically, I have studied Kepler’s analysis on planetary orbits, *how* he deduced the correct planetary orbits from his analyses and *what* exactly his theories stated.

Kepler has been regarded as a great mathematician, but his work on the planetary orbits has been considered as being mixed with lucky guesses and mysticism. I have proved in this thesis that these prejudices are unfounded. I have demonstrated that Kepler managed to develop a coherent theory that connected force models to specific kinematical expressions, and that these expressions produced the correct elliptical orbit. I claim that Kepler proved the elliptical orbit on the basis of mathematical - physical models that introduced basic elements of classical mechanics, which were in many ways equivalent to the later Newtonian mechanics. I have also revealed several features of the sophisticated scientific method Kepler used to achieve this goal in this thesis, and I claim that Kepler´s new astronomy was not merely an introduction of a new theory, but a new way of systematically analysing dynamical systems in order to reveal the underlying causes for the behaviour of the system.

In the process of his scientific work, Johannes Kepler revolutionized the natural sciences by developing some of the most elementary parts of mathematical physics and the foundations for the modern scientific method. In many ways, Johannes Kepler gave birth to mathematical physics.

Acknowledgements

Writing this thesis has been a long process, and a bit of an uphill battle. It has not been easy, trying to combine philosophy with the hard sciences, i.e. mathematics and physics. The difficulty has not necessarily lain in the work itself, but in the communication of the work. The current scientific culture differs so vastly between philosophy and the hard sciences, that one not only meets little understanding for the combining approach, but one also often ends up defending the approach as well - in both camps. The other downside of this approach is that one ends up working pretty much alone.

My experiences on the matter can be aptly summarized by the small cartoon below, and by a short excerpt from discussions with my wife, when I was considering the openings words of these acknowledgements. I suggested to her that I should start by quoting my case study Johannes Kepler himself; “ I much prefer the sharpest criticism of a single intelligent man to the thoughtless approval of the masses”, whereas my wife replied “Darling, I don´t think your thesis will ever have the *option* of the thoughtless approval of the masses”.

Nevertheless, I do have gotten a lot of support and encouragement along the way, and I have a great many people to thank for finally reaching the end of a seemingly endless journey.

First of all, I would like to thank my supervisor Professor Arnt Myrstad for his unwavering enthusiasm and interest in the subject of Kepler's theory development, and his willingness follow all threads of thought. He truly has been a dedicated supervisor.

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Since only parts of Kepler's work have been translated from Latin to English or German, I have been dependent on help with translations. Per Pippin Aspaas has been an invaluable help in translating the foreword of Kepler's *Epitome* and discussing various other passages of the original text.

An essential part of being a happy researcher is the academic activities outside of the office. I want to thank my colleagues Sami Paavola, Pasi Pohjola and Torgeir Knag Fylkesnes for turning up at the same conferences as I have. They share an admirable skill of combining lively social life with lively academic discussions on anything relating to philosophy of science - which covers pretty much everything. I would also thank Michael "How to get it" Hoffmann who showed us just that.

I'd sincerely like to thank the colleagues I have met under my research stays, both at Dibner Insitute of History of Science and Technology at MIT, and at the Department of Philosophy at UBC. Especially, I want to thank James R. Voelkel and George E. Smith at Dibner and Owen Gingerich at Harvard University for their hospitability and for their inspiring and encouraging feedback. For once, I felt that I was *not* working with a marginal subject of little interest, but indeed, that I was too, standing upon the shoulders of giants. I'd also like to thank Paul Bartha and Steven Savitt at UBC for taking interest in my work. I got to appreciate the importance of a good supportive scientific community during both of my research stays.

Fortunately, friends have helped me to keep my perspective during my studies, by inviting me out of the office and reminding me of the other important things in life. I would especially like to thank Martin, not only for sharing an interest in physics and mathematics and his eagerness to discuss problems considering both, but also because he has been among the few important individuals that refreshingly *have not cared* if I ever should finish or not.

I hope reading these words will appease my family of slightly worried engineers. I finally made it, and the reason why it took so long can be summarized by the following: In research if you know what you are doing, you should not be doing it, whereas in engineering if you don't know what you're doing you should not be doing it.

Last, I'd like to thank my lovely wife Elina, for the unprecedented patience and moral support she has shown me throughout my studies – more than any man could expect from a wife. She might not have had the interest in listening to my ruminations on Kepler every night for the last eight years, but in the last few months she has diligently read through, and commented on the legibility of, my whole thesis. If the text flows with any ease it is due to her editorial touch.

Finally, I'd like to thank the Research Council of Norway and my wife for funding the project.

List of original papers

The thesis is based on the following original papers, which are referred to in the text by their numerals:

1. Tønnessen, S. Re-analysis of Kepler’s Distance Laws and the birth of mathematical physics (manuscript).

2. Tønnessen, S. Kepler’s primary measure of time in planetary orbits (manuscript).

3. Tønnessen, S. 2010. Kepler’s Analysis of the Dynamics of Planetary Motion. In *Kepler’s Heritage in the Space Age (400th Anniversary of Astronomia nova)*, eds. Alena Hadravová, Terence J. Mahoney, and Petr Hadrava. Prague: National Technical Museum in Prague.

4. Tønnessen, S. A new edifice for physical science – Kepler’s proof of the elliptical orbit from physical principles (manuscript).

In addition, parts of the introduction to the thesis (Sections 1.2 and 1.3) are based on Tønnessen (2009).

1 Introduction

1.1 What is this study about?

The main questions dealt with in philosophy of science are to understand *how* and *why* science produces knowledge. In order to understand the production of new knowledge, we must aim to grasp the reasoning of the scientist as presented by the scientist himself, and use the reasoning process as a basis to develop tools for analysing the on-going epistemological[1] processes. This line of analysis relies to a high degree on studies of actual scientific practice, i.e. reproducing the steps that are indispensable for the scientist themselves. Therefore historical case studies of scientific discoveries have a prominent role to play in our quest to understand the process of developing new hypotheses and theories.

This PhD thesis is dedicated to studying the reasoning processes of an iconographic historical scientist, Johannes Kepler (1571-1630). The overall aim of this thesis is to thoroughly analyse Kepler's reasoning processes and arguments leading up to his theory of planetary motions, in order to understand the historical development of modern science and theory development.

[1] Epistemology; from Greek *ἐπιστήμη (epistēmē)*, meaning "knowledge, science", and *λόγος (logos)*, meaning "study of") is the branch of philosophy concerned with the nature and scope (limitations) of knowledge.

1.2 Background for the study - the Case Study Method

In the last 20 years there has been growing interest in the study of discovery processes within science. The method of this study has largely followed the ideas of the cognitive psychologist Howard Gruber (Gruber 1980, 1981). Gruber suggested that close readings of research notes and a thorough study of the whole work of a researcher should be more prominent in bringing forth new information on discovery processes. This is called the Case Study Method (Gruber 1980), and although this method is all well and good both in intention and actual practice, the relation between history and philosophy (and in Gruber's case cognitive psychology) is not clear cut. My own attitude is that the Case Study Method cannot be a well-defined methodology. The methodologies we employ to study our subjects very much define what we will find. This is hardly a surprise. Our choice of research strategy and methodology should be based on the knowledge of previous research strategies, their findings and their shortcomings. But most important of all is that the methodology must be tailored to the goals of our own research.

The philosophical motivation for the case study presented in this thesis grew out of a discipline within philosophy of science labelled Model-based Reasoning. This discipline's main interest lies in studying and developing theories on theory development. The methodological foundation of this discipline corresponds to Gruber's ideas and Nancy Nersessian's method of Cognitive Historical Analysis. Nersessian presents the main ideas of this method in her paper "How Do Scientists Think". The paper consists of both a historical and a psychological dimension. The historical dimension of the Cognitive Historical Analysis is in line with Gruber's Case Study method:

> The historical dimension of the method has its origins in the belief that to understand scientific change the philosophy of science must come to grips with the historical processes of knowledge development and change. This is the main lesson we should have learned from the "historicist" critics of positivism (Nersessian 1992).

In other words, Nersessian claims that any theory that purports to explain how scientific change occurs and how knowledge is gained, must take *actual practice* as its basis. It is of course possible to develop theories on knowledge development (or theories on the impossibility of explaining theory development) based on various philosophical theories, but we will not know if our theory has anything to do with actual theory development. Furthermore, Nersessian states: “The cognitive dimension of the method reflects the view that our understanding of scientific social practices needs to be psychologically realistic”. Thus, this method of Cognitive Historical Analysis emphasizes that a formal interpretation must be able to account for real cases of theory development: “Equally as important as problems concerning the rationality of acceptance – which occupy most philosophers concerned with scientific change – are problems about the construction and the communication of new representational structures.” That is, the focus in the historical studies should be on types of representations used and how these are communicated.

Facing the question of how historical studies and the philosophical or psychological analysis of them relate, makes the real methodological problems surface. Nersessian is fairly straightforward on this: “We need to find out how human cognitive abilities and limitations constrain scientific theorizing *and this cannot be determined a priori*” (Nersessian 1992). And furthermore: “The challenging methodological problem is to find a way to use the history of scientific knowledge practices as the *basis* from which to develop a theory of scientific change” (Nersessian 1992). Nersessian’s goal is to integrate insights from cognitive psychology and “the historical findings about the representational and problem-solving practices that have actually brought about major scientific changes”. Such an attempt will inevitably lead to an “essential tension” between the aims of the historian and the psychologist or philosopher. Whose aim is going to steer the study? Should the psychologist apply cognitive theories to historical cases? Or should theory on cognition be developed from scratch based on the historical cases? The first scenario is possible but not viable, since all we do is to exemplify our theory. The theory has already defined what theory change is, and all we do is to search for instances that match the theory. This is a rather trivial Popperian verification problem and is not the attitude of Nersessian. The second scenario is not viable either. To use a historical case study to be the sole basis for an epistemological theory would require a sort of *tabula rasa* attitude towards the actual aim. But of course, we have some ideas of what an epistemological theory should look like. Furthermore, our aim is not to reinvent the wheel. The reason for taking the actual processes of theory change as a serious source for psychological and philosophical analysis is that they can provide novel insights on the epistemology of these processes. The tension in the Case Study Method lies in finding a manner in which we can allow the case study to inform us, and not only supply *examples* validating existing theories on theory change. Nersessian's solution is this:

> Cognitive historical analysis is reflexive. It uses cognitive theories to the extent that they help interpret the historical cases – at the same time it test to what extent current theories of cognitive processes can be applied to scientific thinking and indicates along what lines these theories need extension, refinement, and revision (Nersessian 1992: 7).

This is also the basis for my own attitude. The real crux lies in finding a procedure that provides us with novel information from our case study. I think this procedure cannot, be settled once and for all before starting the analysis of the case in question.

Although I appreciate many aspects of Nersessian's methodology, I find it necessary to attempt to establish an even closer connection between Philosophy of Science and History of Science. The reason for this is that I not only want to analyse the history of conceptual and representational change, but also the scientists` *arguments* causing these changes. My view is that the philosophy of science should become more of an empirical science where the "argument" is the object of study. This view poses a series of problems that all empirical sciences face concerning observation and theory. We do not observe the 'facts' around us objectively. Facts, what we see and study, are always theory laden. But there is a significant difference here: an argument is not studied as an object. An argument is inter-subjective. An argument is presented with the intention that those who read it will follow a certain reasoning pattern, in order to convince the audience about a certain state of affairs. Hanson writes:

> Logicians are concerned with *arguments*, logicians of science with scientific arguments. Their enquiries presuppose answers to worries about the conceptual "stuff" of arguments: unless you know *what is being* argued you cannot determine the argument's soundness (Hanson 1962).

That is, if one has the intention of analysing an argument, one must first of all understand what it purports to argue. This is not as easy as it seems. Earlier arguments are not only expressed with concepts that have afterwards undergone a considerable change, they are also often expressed in a language not familiar to scientists today (let alone philosophers). For example, Newton did not use the newly developed differential calculus in his *Principia Mathematica* (1687). The mathematical reasoning in this revolutionary book is expressed in a geometrical language that is very different from the new calculus. This means that, to be able to understand the reasoning, it is essential to understand the language it is expressed in.

In any case, we have to understand the reasoning *as it is actually performed in the language it is expressed in* to be able to analyse it and to assess if the logical analysis provides valuable and/or relevant information. Our primary aim, either as historians or philosophers of science, is not merely to find instances of inductive, deductive or abductive reasoning, but to understand *what* is argued for and *how* it is argued for. In this quest, the adequacy of our own tools for analysing this process must also be under scrutiny. But what can then be the foundation whereupon we judge our tools as adequate or inadequate? One such foundation should be the actual reasoning processes. The attentive reader could now think that I have trapped myself in a circular argument. If our aim is to gain philosophical understanding of the scientist's reasoning processes, then how can these same reasoning processes work as a foundation for judging our philosophical tools? The way out of a circular argument is appreciating the difference between having an analytical understanding of reasoning processes (their syntax) and understanding the scientific arguments (their semantic content). It is a (philosophical) myth that one cannot understand, follow and even judge an argument if one does not have adequate tools of formal logic for analysing the argument. An astronomical argument for instance, is first of all an *astronomical* argument. This means that if we read the argument with the skills of an astronomer, we should be able to follow, understand and judge it. This does not mean that there is no distinction between the argument and the analysis of it. But it is essential that we are able to see that all the important aspects of the argument, read as a colleague of the scientist, are captured by the analysis.

Norwood Russel Hanson argues in his paper "The Irrelevance of History of Science to Philosophy of Science" (Hanson 1962) that the relationship between Philosophy of Science and History of Science is asymmetric. He argues that history of science has no insights to offer relevant to the understanding of valid scientific reasoning: "The logical relevance of history of science to philosophy of science is nil" (Hanson 1962: 585). Hanson implies in his analysis that logic is a non-empirical analytic a priori science, while history of science is an empirical science. In other words, Hanson considers philosophy of science to be a science that applies tools developed in an analytic a priori science (philosophy) on empirical subject matters (e.g. history). Hanson's objective, like Nersessian's and my own, is to understand how science develops. Furthermore, I share Hanson's ambition of understanding the role of different forms of reasoning and arguments in the process of formulating and approving new hypotheses and theories. However, the tools for analysing such arguments must guarantee that they answer these objectives, and .in my opinion, there is no manner of validating that the tools of logic can handle these objectives satisfactorily. If a logical analysis of a scientist's arguments does not manage to explain if and how the arguments work, it would be fallacious to conclude that it is something wrong with the scientist's reasoning. The study of real arguments bringing forth scientific progress must therefore have priority over preconceived conceptions of the logical structure of (scientific) arguments in general.

1.3 Why use Johannes Kepler as a case study?

Johannes Kepler's astronomical work is a classical study object in both history and philosophy of science. The studies on Kepler's work concern his two famous laws concerning the orbits of planets developed in his book *Astronomia Nova* (1609, henceforth *AN*). The first law states that the planets move in ellipses, and the second law states that the radial line sweeps out equal areas in equal times. His popularity as a study object stems both from his introduction of the two planetary laws and his exceptionally meticulous notations on his own reasoning processes. Also, he is an interesting study object because of his purported infamous preconceptions, and many historians have claimed that he let himself be restricted by his mystic beliefs (Kuhn 1962: 152-153).[2] Many historians and philosophers of science have also found him confused (Koyré 1973: 271; Koestler 1961: 334-336), and his conclusions have often been interpreted as lucky guesses (Gingerich 1989: 68; Koyré 1973: 271). One of the reasons why he has been considered as confused was his tendency to move seemingly erratically among different methods of analysis, such as analysis of observation data, motion, and geometrical form, and speculation on the causes.

Although Kepler's research on the planetary orbits has been extensively studied both philosophically (Hanson 1958; Peirce 1931; Mittelstrass 1972; Westman 1972; Duhem 1969) and mathematically (Whiteside 1974; Davis 1989; Stephenson 1994; Aiton 1969), I claim that many important parts of his reasoning process have been overlooked and misinterpreted. I claim to have been able to fill these gaps through close reading of his original work and through a reconstruction and analysis of his arguments, concepts and reasoning process. I have found that the steps in Kepler's reasoning that have been labelled confused or false, are the very steps that drove his theory forward. After learning Kepler's mathematics and following his reasoning, it was possible for me to understand the process as an *astronomer*, see how his reasoning steps made good mathematical and physical sense, and ultimately understand how these steps led to the development of new concepts and hypotheses. The fact that I was able to follow and acknowledge Kepler's reasoning is a sign that

[2] Thomas S. Kuhn writes: "Individial scientists embrace a new paradigm for all sorts of reasons and usually for several at once. Some of these reasons – for example, the sun worship that helped make Kepler a Copernican – lie outside the apparent sphere of science entirely" I hope that the present work will convince the reader that this kind of metaphysical speculation was not the final arbiter of Kepler's commitment to the Copernican system.

theoretical knowledge is developed. This knowledge is not merely *historical* as in reporting the reasoning process, but *conceptual* as in becoming convinced of an astronomical state of affairs.

So what was more intelligible to me as a “contemporary” to Kepler, than to me as a modern philosopher? It was the fact that Kepler tried to develop a new *theoretical foundation* for astronomical theories and aimed to base this new foundation on *causal explanations* of planetary motions. Astronomers before Kepler constructed geometrical models and checked them with observational data, but Kepler demonstrated that one could in principle construct an infinite number of geometrical models that would fit the data. Kepler thought bigger than his contemporaries, and tried to integrate data, mathematical representations and causes in one unifying theory, and in doing so he revealed fundamental aspects of dynamical phenomena, and developed means to represent these aspects through mathematics. The analyses of Kepler’s arguments in *AN* cannot be accounted for solely by formal logic, but his reasoning is nevertheless undoubtedly scientifically sound. In this thesis I will demonstrate that it is perfectly possible to analyse, reproduce and explain the role of the different steps of reasoning in Kepler’s work.

2 Kepler's theory of science and its influence on his new astronomy

Kepler included an excerpt of Ramus' *Scholae Mathematicae* (1569) on the very title page of his *Astronomia Nova.* In the excerpt, Ramus reacts to astronomical theories in general and to Copernicus' theory specifically: " In later times, on the other hand, the tale is by far the most absurd, the demonstration of the truth of natural phenomena through false causes". Ramus then challenges astronomers to construct astronomy without hypotheses, an astronomy that was to be based solely on observations, arithmetic and geometry. By 'hypotheses' Ramus referred to models of the planetary system using mechanisms for representation that cannot be observed. This included, in Ramus' understanding, epicycles and deferents, eccentrics and equants etc. (see explanations of concepts in Appendix 1), together with preconceived assumptions on the organization of the planetary system like the Copernican model, which places the Earth in the centre, seemingly without empirical justification (Aiton 1975). Ramus offered his own professorship as a prize to the person who could fulfil these criteria.

Kepler's jocular response to Ramus's challenge was: "Conveniently for you Ramus you have abandoned this surety by departing both life and professorship. Had you still held the latter, I would in my judgement, have won it indeed, inasmuch as, in this work, I have at length succeeded, even by the judgement of your own Logic" (Kepler 1609/1992: 28). This response is fairly bold, considering that Kepler introduced a whole array of new geometrical and causal hypotheses in *AN*. Why did he then claim that he had fulfilled Ramus' criteria?

The explanation is found in Kepler's theory of science. In the following sections, I will present this theory of science and his view on the type of hypotheses that astronomy had to encompass. Kepler discussed these issues explicitly and extensively in *Apologia pro Tychone contra Ursum*.[3] This book entails detailed

[3] The work is presented as a "defence" of Tycho Brahe in a priority dispute. In 1588 Reimarus Ursus (Nicolaus Reimers Bär, 1551-1600) published his work called *Fundamentum Astronomicum*, in which he presents a geo-heliocentric model. This provoked Tycho Brahe to accuse Ursus of plagiarism. Kepler had, unfortunately, sent a letter to Ursus in 1595 praising him of his theory and his skills as a mathematician. Ursus presented this letter without Kepler's consent in his *De hypothesibus astronomicis tractatus* (1597), where Ursus strikes a counter attack on Brahe. This became a serious embarrassment for Kepler, especially since Kepler started working for Brahe in 1600 in Prague. Brahe had planned to write a two-volume refutation of Ursus' claim. When Kepler came to Prague Brahe managed to convince Kepler to write the second volume, forcing him to do this by exploiting Kepler's embarrassment over the letter to Ursus. As it turned out, Kepler used this opportunity to write a treatise more on epistemological and methodological issues, using Ursus as an opponent, than on the defence of Brahe. *Apologia* was written around Christmas 1600, but was not published until 1858 (Kepler and Jardine 1984: 1-28).

accounts of several topics on philosophy of science, analyses of some foundational problems of astronomical science at the time, and a historical account of various planetary models. Further, discussions on how astronomical theories are built up are also found in the prefaces of Kepler's *Astronomia Pars Optica* (1605, *Optics*) and *Epitome of Copernican Astronomy* (1618-1621, *Epitome*). Even though there exist several thorough accounts of Kepler's analyses on the status of astronomy and its theoretical problems,[4] few have demonstrated how his philosophy of science actually *directed* his analyses in *AN*. With this introduction, I aim to place the papers included in this thesis within the right methodological and theoretical context of Kepler's own theory of science.

[4] See especially Jardine's comments to his own translation of the *Apologia* in the *The Birth of History and Philosophy of Science* (Kepler and Jardine 1984).

2.1 *The sceptic's view on astronomical theories*

Kepler's philosophy of science was developed as a response to the instrumentalist or sceptical view of astronomy that evolved in the 16th century. Especially two factors led Kepler's immediate predecessors and contemporaries to adopt this view. The first was the rejection of Aristotelian cosmology, which had hitherto provided the sole metaphysical explanation of the organization of the world. The second factor was the acceptance that many competing planetary models, e.g. Copernicus', Ptolemaiios' and Tycho Brahe's models, saved the same phenomena equally well for selected observations.[5] Kepler actually referred to Osiander's notoriously sceptic preface of Copernicus' *Revolutionibus Orbium Celestium* (1543)[6] in his answer to Ramus. Kepler believed, that Ramus' naïve realist position was founded on the same faulty view of astronomical practice and theories that he considered the sceptics to hold. For Kepler, the fact that several kinematical-geometrical models of the solar system saved the phenomena did not mean that there were no true theories of the planetary system. We will come back to Kepler's arguments, but let us first take a closer look at the sceptics' view of astronomy.

In the first half of the 16th century, the German universities went through important reforms, inspired by the growing humanist ideas (Westman 1975). One of the important scholars behind these academic changes was Philipp Melanchthon (1497-1560). Melanchthon was an active leader in the humanist movement both at the Universities of Tübingen and Wittenberg. In Wittenberg, he became a well-known scholar, teacher, and the intellectual origin of the so-called "Wittenberg interpretation". However, the term "Wittenberg interpretation" refers more to the view of Melanchthon's students, than to the view of Melanchthon himself. His students tended to embrace Copernicus' model to a greater extent than Melanchthon himself did. But this is not to say that they embraced Copernicus' theory as a true depiction of the planetary system. It was recognized that Copernicus' theory was in many respects geometrically equivalent to Ptolemy's theories, and Copernicus' theory became part of the astronomy curriculum alongside Ptolemy's theories. The reason

[5] The equivalencies were not exact, but neither of the models seem to be fundamentally better at explaining the data and for several observations the models did not differ significantly.
[6] The preface of Copernicus' *Revolutionibus Orbium Celestium* was written anonymously and added after Copernicus' death. Owen Gingerich describes in his book *The Book Nobody Read* how Kepler came to know that Andreas Osiander was the writer, and that Kepler was the first to state this in print in *AN* (on the aforementioned front piece) (Gingerich 2004).

the Wittenberg circle embraced Copernicus' theory was its technical features, especially the fact that Copernicus had managed to get rid of the dreaded equant, and preserved the circular uniform motion for all the planets. Copernicus' theory was actually seen as a restoration of astronomy to its roots, rather than representing a revolutionary new world-view. What is interesting here, is that most members of the Wittenberg circle did not express any cosmological preferences.[7]

As a generalization of the attitudes among intellectuals at German universities around 1540-1560, we can say that the usefulness of some of Copernicus' techniques was fully acknowledged, but the attitude towards astronomy as a science delivering true theories of the world, was at best undecided. Nicholas Jardine (1979) makes an interesting and thorough re-analysis of the different positions in the 16th century in his paper "The Forging of Modern Realism". From Jardine's analysis it is obvious that many of the so-called instrumentalists are better viewed as *sceptical realists*. An instrumentalist is one who asserts that theories in principle cannot be determined as true or false, thus he views theories merely as tools of representation and prediction. A sceptic, on the other hand, is a realist who has developed a sceptical attitude towards the theories of his profession. Those who held a sceptical view on astronomy in the 16th century claimed that there were certain constraints on astronomy that made it difficult or impossible to justify the theories.

These constraints can be exemplified with Melanchthon's criteria for gaining true knowledge: 1) *experientia universalis* – "by applying the senses to their proper objects under normal circumstances we are able to obtain knowledge of the properties of the various kinds of terrestrial objects", 2) *noticia principiorum* – with the mind gain (a priori) knowledge of mathematical truths and certain general principles (the whole is greater than its proper part), 3) *intellectus ordinis in syllogismo* – we are able to grasp syllogistic form, which help us to extend our initial stock of knowledge by way of syllogistic inference (Jardine 1979).

As Jardine has interpreted Melanchthon's first criteria, it is impossible to grasp the general characteristics of all non-terrestrial phenomena by normal sensory experience. By requiring that the senses should be applied "to their proper objects" and "under normal circumstances" celestial phenomena are effectively ruled out. We

[7] The only person who took a stance was Rheticus. He was drawn towards the fact that Copernicus' theory was a comprehensive model for the universe. It demonstrated, for him, that the world was harmonious and written in the language of geometry (alluding here to Plato).

will come back to Kepler’s view on astronomical observations in Section 2.4, but Kepler had at least a nascent awareness that even everyday experiences are theory laden. In a sarcastic reply to Patricius’ view that truth is simply what you see, Kepler referred to a ‘miracle’ seen by a friend. A spider hanging in front of a window had come between the eyes of the friend and a cow grazing outside the window, giving the appearance of a multi-legged cow (Westman 1972: 241). Clearly, no observation is independent of point of reference.

Melanchthon’s second criterion captures the mathematical part of astronomy, and as such, makes it an important exercise for abstract reasoning. But mathematical models were not anchored in ordinary perception. They were not facts of perceptual experience (Erfahrung), but the hypothetical saving of the phenomena.

The third of Melanchthon’s criteria concerns valid scientific reasoning. The syllogistic form secures that a true conclusion follows from true premises. But true conclusions can also follow from false premises. If the premises in question save the phenomena, it does not follow that the premises are true. Such reasoning is to commit the fallacy of affirming the consequent. Since both Ptolemy’s models and Copernicus’ model could describe the same facts equally well, and since it was agreed that both could not be true at the same time, it seemed to follow that at least one of the models had to be false. That is, from one of the false models the true appearances could be deduced. The fact that seemingly competing models saved the same phenomena had been a fundamental cosmological problem since Averroes’ (1126 – 1128) time (Jardine 1979). After the sceptics’ refutation of Aristotelian cosmology as a blueprint for astronomical models, there did not seem to be any other possibility than giving up the notion that astronomy could and should render true models of the world.

2.2 Kepler's refutation of the sceptical stance

Kepler's response to this situation was quite curious. He wrote: "for the truth to be legitimately inferred the premises of a syllogism, that is, the hypotheses, must be true" (Kepler and Jardine 1984: 139). But how do we know the truth of hypotheses if the syllogism cannot guarantee it? Kepler's answer was as follows:

> If a man assesses everything according to this precept, I doubt indeed whether he will come across any hypothesis, whether simple or complex, which will not turn out to have a conclusion peculiar to it and separate and different from all the others. Even if the conclusions of the two hypotheses coincide in the geometrical realm, each hypothesis will have its own peculiar corollary in the physical realm (Kepler and Jardine 1984: 141).

This quote entails two claims. The first claim is the idea that "each hypothesis will have its own peculiar corollary in the physical realm." At the time Kepler wrote this, he had not developed his physical theory. Indeed, he probably did not know what the physics explaining the planetary motions would look like. He refers to the "physical realm" merely as a collective term for hypotheses presenting causes of the planetary motions and organization. Nevertheless, if it is assumed that the various kinematical artefacts, like epicycles an equants, represent *effects* of causes, then the various kinematical models (Ptolemaiian, Copernican or Brahean) must each have their very own physical models.

The second claim in the quote above is that the hypotheses coincide in the geometrical realm. The argument for this claim, and the meaning of the claim, demonstrates a significant insight on kinematics and relative motion:

> Well then, isn't it necessary for one of the two hypotheses about the primary motion (to take an example) to be false - either the one that says that the earth moves within the heavens, or the one which holds that the heavens are turned about the earth? Certainly if contradictory propositions cannot both be true at once, these two will not both be true at once: rather one of them will be altogether false. But is not the same conclusion about the primary motion demonstrated by both means? Do not the same emergences of the signs of the zodiac follow, the same days, the same risings and settings of the stars, the same features of the nights? Does what is true follow equally from what is false and what is true then. Far from it! For the occurrences listed above, and a thousand others, happen neither because of the motion of the heavens, nor because of the motion of the earth, insofar as it is a motion of the heaven or of the earth. Rather, they happen insofar as there occurs a degree of separation between the earth and the heaven along a path which is regularly curved with respect to the path of the sun, by whichever of the two bodies that separation is brought about. So the above-mentioned things are demonstrated from two hypotheses insofar as they fall under a single genus, not insofar as they differ. Since, therefore, they are one for the purpose of the demonstration, for the purpose of demonstration they certainly are not contradictory propositions. And even though a physical contradiction inheres in them, that is still entirely irrelevant to the demonstration. So this example certainly does not

> show that what is true can follow both from what is true and from what is false (Kepler and Jardine 1984: 142).

The two hypotheses mentioned in the quote above, “the earth moves within the heavens” and “heavens are turned about the earth” are observationally equivalent hypotheses, or with Kepler’s own words *equipollent* hypotheses. Both hypotheses have the ability of representing the same observations equally well: “Do not the same emergences of the signs of the zodiac follow, the same days, the same risings and settings of the stars, the same features of the nights?” Hence, the observations themselves are incapable of rendering any difference in the ability of various models to account for the appearances.

However, Kepler’s insight is not that different models “save the phenomena”, but that they are essentially *the same model*. With the benefit of hindsight, we can see that the excerpt “there occurs a degree of separation between the earth and the heaven along a path which is regularly curved with respect to the path of the sun, by whichever of the two bodies that separation is brought about” refers to what we today call relative motion. This interpretation is corroborated by the treatment of the same problem in Kepler’s earlier *Mysterium Cosmographicum*:

> For it can happen that the same [conclusion] results from two suppositions which differ in species, because the two are in the same genus and it is in virtue of the genus primarily that the result in question is produced. Thus Ptolemy did not demonstrate the risings and settings of the stars from this as a proximate and commensurate middle term: ‘The earth is at rest in the centre’. Nor did Copernicus demonstrate the same things from this as a middle term: ‘The earth revolves as a distance from the centre’. It sufficed for each of them to say (as indeed each did say) that these things happen as they do because there occurs a certain separation of motions between the earth and the heaven, and because the distance of the earth from the centre is not perceptible amongst the fixed stars [i.e**.** there is no detectable parallax effect] (Kepler Gesammelte Werke, I: 15-16, translated in Kepler and Jardine 1984: 216).[8]

Thus, the reason that both the Copernican and Ptolemaiian models predict the correct data is that they both deduce the data from the exact same relative motion (“a certain separation of the motions between the earth and heaven”).[9] In other words, the two models are equivalent with respect to representing the same true relative motion. The only difference between them is that the relative motion between the sun and the Earth are described from two different perspectives or reference points. This

[8] The term “genus” is important in the quote above. It refers to the classification under which we choose to model the phenomenon. Ptolemy’s, Copernicus’ and Brahe’s models all classified the phenomenon as a kinematical phenomenon - *qua* kinematics they are equal.

[9] Jardine (in Kepler and Jardine 1984: 217) points out that although the principle of kinematic relativity and the connected vocabulary of absolute and relative motion did not exist until the mid 17th century, many 16th century astronomers show a clear grasp of cases of relative motion.

argument is essentially different from an argument of under-determination of hypotheses by data.

Kepler discussed the equivalencies of hypotheses again in the first and second of total five parts of *AN*. In the first part of *AN* (Chapters 1-6), he kept a sharp focus on the various appearances of the motion from different perspectives, and discussed in detail whether, and to what degree, the various models and tools of representation were equivalent (e.g. helio- versus geocentric systems, epicycles versus an eccentric with an equant, and real sun versus the mean sun). This discussion on the equivalency of kinematical models was strategically important in *AN*, since it introduced epistemological reasons to abandon the misguided tendency to save the phenomena with various geometrical tools.

2.3 In imitation of the ancients – the Vicarious Hypothesis

Kepler developed a new model “in the imitation of the ancients” in the second part of *AN* (Chapters 7-21). This model was called the Vicarious Hypothesis, since it ultimately failed to predict both longitudes and correct distances. Kepler developed the Vicarious Hypothesis in Chapter 16, and based it on two assumptions inherited from the ancients; the planet moves in a perfect circle and the equant point is at a fixed position on the line of apsides (Kepler 1609/1992: 284). The equant point was the main kinematical device among the ancients, and is merely defined as the point around which the angular motion of a planet (or the sun in a geocentric model) is uniform. The Vicarious Hypothesis differed from all other existing models in that it employed the sun as a reference point for describing the planets’ motions. Even Copernicus’ theory was not strictly heliocentric. [10]

Kepler constructed the Vicarious Hypothesis on the basis of four observations of Mars taken at opposition. Observing Mars at opposition removed the effect of the second inequality, i.e. the motion of the Earth.[11] The resulting model was an eccentric circle (i.e. the sun is placed off centre), where the equant is opposite to the sun relative to the centre of the apsidal line, but closer to the centre than the sun.

The Vicarious Hypothesis predicted the correct longitudes well within the observational accuracy. However, by using latitude observations (where Mars’ orbital plane is inclined to the orbital plane of the Earth) Kepler found that the model produced incorrect distances to the sun. Moreover, these latitude measurements indicated that the orbit should be close to bisected, i.e. that the sun and the equant point are opposite of, and equally spaced from, the centre of the orbit (Chapter 19, *AN*). When adjusting the Vicarious Hypothesis to a bisection of the eccentricity, he found that the new model predicted an error of 8’ in the octants (Kepler 1609/1992:

[10] In the other models, even in Copernicus’ heliocentric model, the reference point was not the sun, but a point called the ‘mean sun’. The mean sun was the point around which the Earth (in Copernicus’ heliocentric model) moved with uniform angular motion. Copernicus also believed that the centre of the Earth’s orbit was the mean sun. Kepler had earlier shown that the intersection of all the planets’ orbital planes went through the sun. He therefore saw this as a clear indication that any model should take the true sun as the reference point for describing the planets’ motions.

[11] An important pair of concepts was the ancients’ distinction between first and second inequality. The first inequality is the variation in the speed of the planet (as seen against the fixed stars), ignoring the effect of the second inequality. The second inequality is the apparent (from earth) speeding up of a planet that is in conjunction with the sun, and the slowing down or backwards motion of a planet that is in opposition with respect to the sun. In a heliocentric reference frame the second inequality arises from the motion of the observer’s location on a moving earth.

286). Kepler concluded that the assumptions for developing the model had to be abandoned:

> But what was assumed was: that the orbit upon which the planet moves is a perfect circle; and that there exists some unique point on the line of apsides at a fixed and constant distance from the centre of the eccentric about which point Mars describes equal angles in equal times. Therefore, of these, one or the other or perhaps both are false, for the observations used are not false (Kepler 1609/1992: 284).

Thus, the geometrical devices of the ancients, the circle and the equant, were not fitted to describe the plants' motions given that the sun was used as the point of reference. They must therefore be viewed as unfounded preconceptions of how to represent the motions of the planets.

One of the central claims I make in this thesis is that Kepler introduced a *new way* of doing astronomy. This *new astronomy* was based on an epistemological view that has specific methodological implications. This epistemological view is argued for in Chapter 21 in *AN*, and marks the transition from the methods in 'imitation of the ancients' to the method of analyses instigating the new astronomy.

2.4 The truth in false hypotheses

Kepler reiterates his view on “false” models at the start of Chapter 21: “I particularly abhor that axiom of the logicians, that the truth follows from the false” (Kepler 1609/1992: 294). He extended here his former criticism of this type of reasoning to all types of seemingly false hypotheses that give (some) correct predictions. The immediate relevant cases were the Vicarious Hypothesis and the equant and circularity, but the point was evidently meant to be general. In the subsequent argument he applied the same basic geometrical devices (“principles”) as in the Vicarious Hypothesis (i.e. circles and an equant) in order to demonstrate that a model based on these principles can be made to save the data. By successively adjusting the model, he demonstrated that he could produce a model that accounted for longitudes at the apsides, quadrants and octants within the observational accuracy.

This analysis demonstrated two insights concerning epistemology and method of scientific research. The first insight is:

> Further, as these false principles are fitted only to certain positions throughout the whole circle, it follows that they will not be entirely correct outside those positions, except to the extent (as shown in this example) that the difference can no longer be appraised by the acuteness of the senses (Kepler 1609/1992: 298).

That is, it is always possible to construct a posteriori a model of the phenomena that will fall within the observational accuracy. The model might have some peculiar empirical results to it, but it deviates so little from other models that it cannot be detected by observation. Thus, a method that proceeds by developing models that fit the data is not a sensible scientific method. Neither is it a very convincing strategy for validating a model. This view is corroborated by his comment on a later occasion, when a model he constructed on the basis of two physical principles failed to predict the correct data:

> You will say that we have come out worse, since in ch. 48 we came nearer the truth in our results. But, my good man, if I were concerned with results, I could have avoided all this work, being content with the vicarious hypothesis. (Kepler 1609/1992: 494).

The second insight concerns the aforementioned “axiom of the logicians”:

> It is at least now clear to what extent and in what manner the truth may follow from false principles: whatever is false in these hypotheses is peculiar to them and can be absent, while whatever endows truth with necessity is in general aspect wholly true and nothing else (Kepler 1609/1992: 298).

In other words, the reason a false hypothesis predicts something true is not necessarily accidental. The parameters of the model that produces the *correct result* represent some of the features of the phenomenon correctly. Likewise, the parameters of the model that produce *incorrect results* do not do the predictive work. The models that produce true predictions are therefore often only partly true. The epistemological point is that one has to acknowledge and preserve the aspects of the models that *are* true of the phenomenon. One should never discard all features of the model even though the model as a whole is not physically viable. Rather, the ‘false’ model can be an important guiding light in the search for a correct understanding of the planets’ motions.

These two epistemological insights played an essential role in laying the *methodological basis* for Kepler’s research in *AN*. According to Kepler, saving the appearances was not a viable method for developing astronomical theories. However, ‘false’ models could expose kinematical aspects that the data themselves did not show.

2.5 The key to a causal hypothesis

The insight on "false" models from Part 2 of *AN* (Chapter 21) was put into practice in Part 3. In this part Kepler started his foray into new territory and his search for "the key to a deeper astronomy", by using the false equant to search for a common kinematical characteristic among the planets.

All the existing major planetary models assumed that the Earth's orbit (or the sun's motion about the Earth for geocentric models) was fundamentally different from the other planets' orbits. These models accounted for the fact that the planets, except the Earth, had a point of uniform motion outside of the geometrical centre of their orbits, i.e. they had an equant. The earth-sun system, on the other hand, was assumed to have the point of uniform motion at the centre of the orbit, i.e. it was assumed not to have an equant. [12]

Kepler had for a long time assumed that the cause of the planets' motions was common to all. This meant that if the other planets had some kinematical characteristics in common, one would also expect to find this characteristic in the Earth's orbit. He had already in his *Mysterium Cosmographicum* (1596) suspected that the Earth might have an eccentricity like all the other planets:

> In chapter 22 of the *Mysterium Cosmographicum*, when I was giving the physical cause of the Ptolemaic equant or of the Copernican-Tychonic second epicycle, I raised an objection against myself at the end of the chapter: if the cause I proposed was true, it ought to hold universally for all planets. But since the earth, one of the celestial bodies (for Copernicus), or the sun (for the rest) had not hitherto required this equant, I decided to leave the speculation open, until the matter were clearer to astronomers. I nevertheless entertained a suspicion that this theory might perchance also have its equant (Kepler 1609/1992: 305-306).

We have seen that Kepler demonstrated that Mars' eccentricity was bisected, i.e. that the geometrical centre of the orbit cut the distance between the sun and equant point in half. *If* it could be shown that also the Earth's orbit was bisected he could be quite confident that this eccentricity had a common cause. This would be the *key* to his physics:

> In this third part I first approach the second inequality. Here I shall use unquestionable observations to demonstrate, with either a confirmation or a refutation, all that I have hitherto supposed as principles but had doubts about. Once this is found it will be like a key: the rest will be opened up (ibid.: 305).

[12] See e.g. Pannekoek (1961) for a comprehensive description of the various models.

Kepler therefore set out to analyse the orbit of the Earth in order to figure out whether it has an equant and, if so, where it should be placed.[13]

To make measurements of the Earth's orbit is no simple task, since the observer is located on the Earth itself. Kepler used measurements of Mars, which he turned into measurements of the Earth. In order to do that, he had to know the exact position of Mars relative to the Earth and to the Earth's equant point (mean sun). One of the major problems he faced was that he did not have the exact distances of Mars, only the longitudes. He solved this by using observations at exactly one Mars year (687 earth days) apart, thus he knew that Mars would be located at the exact same spot each time. With a clever use of Tycho Brahe's model and data along with the Vicarious Hypothesis model (both models providing correct longitudes) he could fairly accurately position the Earth's equant point (Small 1804/1963; Stephenson 1994).

Another important aspect of Kepler's analysis is that he performed the calculations in all Copernican, Ptolemaian and Tychonic frameworks, i.e. by assuming the organization of the planetary system in accordance with these hypotheses. In the earlier chapters Kepler had discussed in detail how certain features of the various models were model-specific. The present calculation of the eccentricity was proved to be evident in all three types of models. Kepler could therefore conclude that the calculations gave the real placement of the equant, and that the result was not merely an accidental effect of one model of representation.

The result of the analyses was that the centre of the orbit of the Earth, as was previously proven for Mars, is bisecting the line between the sun and equant point (Kepler 1609/1992: 323-324). Thus, the bisection was proven to be a general kinematical aspect for all the planetary orbits.[14] In a bisected eccentric orbit we have that the motion at the apsides is *inversely* proportional to the distance from the sun. This correlation between distance between the sun and planets' and the planets' motions at the apsides (the starting point of Paper 1 in this thesis) was the *key* that

[13] Lucid accounts of Kepler's proof that the Earth indeed had a bisected eccentricity have been given by Small (1804/1963), still remarkably relevant, and later by Stephenson (1994: 49-61). For the present, we do not need to go into the technical details, but some of the aspects of the reasoning process reveal some features of Kepler's method of analysing data.

[14] The bisection had previously been only proved for Mars' orbit, but the effect of the incorrect assumption that the Earth did not have an equant was that all the other planets' equants seemed to oscillate. By taking into account that the Earth in fact did have an equant Kepler could conclude that the other planets' equants were fixed (or nearly fixed) (Small 1804/1963: 198).

would expose the underlying causes of the motions, and the basis for Kepler's new astronomy.

2.6 What was Kepler's new astronomy?

2.6.1 Kepler's view on theories and their validation

Ramus' challenge to the astronomers quoted above, was to develop an astronomy "without hypotheses." Both the naïve realist (Ramus) and the sceptics viewed the geometrical models, with epicycles and equants, as fictitious hypothesis. The naïve realist would have liked to get rid of these devices, while the sceptics believed they were necessary in order to save the phenomena. Kepler's reaction to both of these views was that they misunderstood the epistemic role of hypotheses in astronomy.

Kepler presented his arguments against these views in his critique of *Ursus* scepticism in *Apologia*. He criticized Ursus for equating 'hypothesis' with 'aitema', which in the Aristotelian sense can mean 'illegitimate or false assumption'.[15] Kepler's explanation for his criticism was based on the manner the term 'hypothesis' is used in geometry. The geometer classifies his propositions into three different kinds: axioms, postulates and hypotheses. Axioms are propositions acknowledged as certain and postulates are propositions that are not certain, but regarded as true for the sake of argument (e.g. a drawing of a straight line is seldom exactly straight, but for the sake of argument the line is postulated to be straight). In the course of a demonstration both postulates and axioms can play the role of hypotheses if these were the assumptions to be demonstrated. Thus, a hypothesis is the status an assertion has when it is under scrutiny. Kepler applied this view to astronomy:

> We, however, call 'a hypothesis' generically whatever is set out as certain and demonstrated for the purpose of any demonstration whatsoever (Kepler and Jardine 1984: 138).

Kepler's claim is categorical. *Everything* that is assumed for the sake of a demonstration is a hypothesis. Kepler also meant by this that these things we assume are *hypothetical*. That is, they are assumed to be true for the purpose of demonstration, but otherwise the beliefs might as well be false. Earlier in *Apologia,*

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[15] In *Posterior Analytics* Aristotle defines 'aitema' both as 'an assumption capable of proof but assumed without proof' and 'a false or illegitimate assumption'. It is the latter definition Ursus uses (see Kepler and Jardine 1984: 42, fn. 38).

Kepler likened this attitude to that of the mathematician who wants to demonstrate some characteristics of a circle. He does not need to draw a perfect circle, it suffices to *assume* the figure to be circular and then see what follows from the assumption. That is, there might not exist any circular object in the world, but given that some object is circular it would have this or that characteristic *qua* circular. In this manner these propositions and premises are better labelled ‘hypotheses’.

> Thus in every syllogism the hypotheses are what we otherwise call ‘propositions’ or ‘premises’. But in a longer demonstration, which includes many subordinate syllogisms, the premises of the initial syllogisms are called ‘hypotheses’. That is, in the ordinary use ‘hypotheses’ are only part of the first syllogisms in a whole array of syllogisms (Kepler and Jardine 1984: 139).

As a consequence of his attitude, Kepler claimed that everything we set as certain in a demonstration is a hypothesis:

> Thus in astronomy suppose we demonstrate with the help of numbers and figures some fact about a star we have previously observed, from things we have seen when carefully and meticulously examining the heavens. Then in the demonstration we have set up, the above-mentioned observation constitutes a hypothesis upon which that demonstration chiefly rests (ibid.).

Thus, the ‘facts’ are both what we set out to demonstrate (“with the help of numbers and figures”), and at the same time they constitute a hypothesis that the demonstration rests upon. In the spider-cow example mentioned above (Section 2.1.) we saw that Kepler criticized the notion of ‘objective’, theory independent, observation.

Kepler continued:

> We thereby designate a certain totality of the views of a practitioner, from which totality he demonstrates the entire basis of the heavenly motions. All premises, both physical and geometrical, that are adopted in the entire work undertaken by the astronomer are included in that totality (ibid.).

He then went on to explain where this totality of views might come from:

> They are included if the practitioner has for his convenience borrowed them from elsewhere. And they are likewise included if he has already demonstrated them from observations, and now in the reverse manner, requires that what he has demonstrated should be conceded to him by the learner as hypotheses: from which hypotheses he promises to demonstrate by syllogistic necessity both those observed positions of the stars (which had in the first place been used by him as hypotheses) and also, so he hopes, those which are about to appear in the future (ibid.).

Kepler’s criteria for a plausible theory lay in the status of the ‘totality of views’ and the testing of this totality of views. Jardine points out in an essay accompanying his translation of *Apologia* (Kepler and Jardine 1984: 283-286) that the concept of a ‘theory’ was not developed at this point. Kepler seems to use the term ‘hypothesis’ sometimes as we do it today, and sometimes as an equivalent to ‘a theory’. But Kepler’s reference to ‘totality of views’ is here equivalent to what we would call a theory, with the exception that when the theory is assumed for the sake of demonstration, it is to be taken as a hypothesis. However, Kepler’s point here is that astronomical theories must constitute a whole set of causal explanations and representations, and even the data entered into the formulation of hypotheses. Again I would like to stress Kepler’s epistemological point that data alone cannot prove the truth of a kinematical model or distinguish between two kinematical models. Many scholars seem to believe that Kepler tried to fit geometrical models to data, but Kepler considered it to be a methodological mistake to lay much weight on verifying kinematical models on data. He wanted to *prove* that the data followed from the ‘totality of views”. But what should this totality of views consist of in astronomy and how should such a theory prove the phenomena?

2.6.2 What the totality of views consists of in astronomy

The totality of views, or 'the edifice', of astronomy had to account for several levels of representations and explanations, according to Kepler. These levels of representations, or 'parts', are described in his *Apologia* (1600) and in the forewords of his *Optics* (1604) and *Epitome* (1618-1625) (see Appendix 3 for translation). I will focus here on the *Optics* and the *Epitome*, since these entail the most detailed and precise descriptions.[16]

In the spring of 1602 Kepler realized that the path of Mars could not be perfectly circular. He understood that his book on Mars' orbit, i.e. the *AN*, would not be finished as early as he had hoped. Since it was expected of him to publish, he took a break from the work on Mars to write the *Optics,* which was published in 1604. In the foreword of the *Optics* Kepler presented a thorough and intricate exposition of the various parts that enter into astronomy:

> Astronomy, which deals with the motions of the heavenly bodies, principally has two parts: One consists of the investigation and comprehension of the forms of the motions, and is mainly subservient to philosophical contemplation. The other, arising from it, investigates the positions of the heavenly bodies at any given moment, and has practical orientation, laying the foundations for prognosis (Kepler 1604/2000: 13).

In Kepler's view, astronomy has as a goal to *explain* the heavenly motions and this explanation provides the ability to *predict* the planet's positions. Furthermore, astronomy is dependent on two types of demonstrations in order to fulfil its purpose: "in astronomical demonstrations there are two kinds of principles: one, the *observations,* and the other, the *physical or metaphysical axioms*" (Kepler 1604/2000: 13). The observations are divided into three parts:

> The first is the mechanical part, dealing with instruments, suitable for observing the celestial motions, and the way of using them, which that phoenix of astronomers, the late Tycho Brahe, published five years ago. The second is the historical part, comprising the observations themselves. […] the third, optical part of astronomy, which I am treating now, through a brief recounting, as if included among the principles, of the old things that Witelo treated methodically, or the new things that Tycho Brahe treated here and there, on this subject (Kepler 1604/2000: 13).

The fourth part of astronomy Kepler concerns the second principle, the physical or metaphysical axioms:

> The other kind of principles in astronomical demonstrations, the physical or metaphysical principles, together with the subject matter itself of astronomy, which is

[16] For the account in *Apologia* see Jardine and Kepler 1984: 154.

> principally the motions of the heavens, a physical matter, makes up *the fourth part of astronomy*, namely, *the physical part*…(Kepler 1604/2000: 13-14)

In the *Epitome*, which was a series of books published between 1618 and 1621, Kepler presented a very similar division:

> The astronomical task consists chiefly of five parts, the Historical, which has to do with Observations, the Optical, which has to do with Hypotheses, the Physical, which has to do with the causes of Hypotheses, the Arithmetical, which has to do with Tables and Calculation, [and] the Mechanical, which has to do with Instruments (Kepler 1953: 23).

The only difference here is that in the latter version arithmetic (or setting up astronomical tables) is included as a fifth part.

In and of itself, it is interesting that Kepler emphasized that doing astronomy incorporates all these subjects. A main focus in this thesis is on the parts that enabled him to *explain* the planetary motions. Let us therefore take a closer look at this part of astronomy. We continue the quote from *Optics* above:

> …*the physical part*, which deals with the efficient causes of the motions, or the movers; the formal causes, or figures that the movers strive for; the material cause or orbs; and the physical intension or remission of motions; which part, if God should grant me life, I shall encompass by the means of *Commentaries* on the motions of *Mars* …which I think I can call *the key to a deeper astronomy* (Kepler 1604/2000: 13-14).

Hence, Kepler's focus in *AN* was on the physical part, which entailed the key to a deeper astronomy. The *Epitome* was written after the *AN*, so his description of the physical part was based on his finished theory. Kepler discussed the physical part under the heading *On the causes of Hypotheses* in the *Epitome*:

> The third part, the Physical, is commonly held to be unnecessary for the Astronomer, even though it is most essential in order to reach the aim of this part of Philosophy, and cannot be resolved by anyone but the Astronomer. For Astronomers should not be given pure licence to make whatever speculation they like without reason; indeed, you should be able to render probable also the causes of your Hypotheses, which you present as the true causes of the Appearances, and thus stabilise [establish] the principles of your Astronomy in the higher science, namely Physics or Metaphysics; without being shut out from those Geometrical arguments, Physical or Metaphysical, that are provided for you by the very stepping out from the discipline proper, concerning things pertaining to the higher disciplines, as long as you avoid meddling with any petition of the Beginning (Kepler 1953: 25).

Again, Kepler professed that the physical part is *necessary* in order for the astronomer to reach his goal.

In the *Epitome*, Kepler made a subtle shift in his notion of the primary goal of astronomy from revealing the true motions (for which you need to study the causes of these motions) to the causes themselves. Under the heading *What is Astronomy?*

Kepler stated: “It is a science that provides the causes behind the things in the sky and amongst the stars that appear to us down on Earth, things that give rise to the changes of the times: when these things are understood, we may be able to predict the face of the sky, that is, the appearances that have taken place in the past” (Kepler 1953: 23). Let us bear in mind though, that Kepler’s ‘cause’ is not restricted to efficient causes. For example, the cause of the apparent retrograde motion of the outer planets on the sky is that the observer is placed on the moving Earth. This cause would be a ‘formal’ cause. While the cause of the actual motion of the planets is an ‘efficient’ cause.

Kepler explained the relation between physics and astronomy under the heading *What is the relationship between this science and the other sciences?*: “It is a part of Physics, because it searches for the causes of natural things and events: and because the movements of objects in the sky are among its subjects: and because its only aim is this, to establish how the edifice of the world and its parts are pieced together” (Kepler 1953: 23). That is, astronomy is a discipline within physics, not merely a discipline that invokes physical considerations.

We must be careful to not assume our modern meaning of the concept “physics” when interpreting what he meant, but likewise we must not superimpose meanings from earlier scientists and philosophers without reference to Kepler himself. Kepler demonstrated time and again in *Apologia* and *Optics* an aptitude to sharp criticism of the tradition. It is clear however, that “physics” for Kepler involved accounting for the ‘efficient’ causes of the planet’s motions.

An important aspect of mathematical physics is that cause and effect are formalized by mathematics, and this has been a central theme in the debate regarding Kepler’s work. Did Kepler express and analyse the physical causes he developed appropriately with mathematics? This subject will be discussed thoroughly in all the papers included in this thesis. But Kepler’s own account of the relation between physics and mathematics is found in the last statement under the same heading of *Epitome* as above (*What is the relationship between this science and the other sciences?*):

> It [astronomy] is, however, subordinated to the *genus* of Mathematical disciplines, and it uses Geometry and Arithmetic as two wings; taking into account the quantities and figures of the objects and movements of the world, measuring and counting time, and using time to establish its demonstrations: and bringing speculation in its entirety into use, or *praxis* (Kepler 1953: 23).

To paraphrase Kepler: geometers prick up your ears! Here is a statement that encapsulates Kepler's view on the role of mathematics in astronomy. As a discipline astronomy is subordinated to the *genus* of mathematics. Read in connection to the previous points, especially where he stated that physics needed to be included, we see that Kepler did not *reduce* astronomy to a mathematical discipline. Rather, he required that the analysis on causation (and all other aspects of the phenomenon) had to be expressed in mathematics. That is, he set out the requirements for a "mixed science" view of astronomy. This view does not cohere with the interpretation that Kepler's mathematical analyses of the planetary systems was restricted to the kinematical aspects of the phenomena. It is a statement that is in full accordance with modern mathematical physics where the cause-effect relation should be expressed mathematically.

2.7 Summary of Kepler's philosophy of science

Kepler's main critique of the sceptics and realists was that they had misunderstood the status of the seemingly competing models of the planetary system. Kepler pointed out that the different models were not rivals; they were actually all true geometrical representations of the same relative motion. He understood that the problem with the existing models was that they all were built on unfounded preconceptions (hypotheses) on *how* to represent the planetary motions (theoretical devices like equants, circles etc.). Kepler demonstrated that constructing a model on the basis of these preconceptions could not represent all relevant kinematical features. But, such models can often save the phenomena so well that their inaccuracies cannot be detected. He demonstrated that it is always possible to save the phenomena by unfounded geometrical devices.

This problem called for a new *methodology;* Kepler claimed that the aim of analysing data was not to save the phenomena, but to expose and explain the true motions. He postulated that only the search for the causes of the motions would produce accurate and true descriptions of the planets' motions. However, Kepler also pointed out that there is always something true in accurate, but false models. He applied the false idea of the equant hypothesis (the Vicarious Hypothesis) to demonstrate that a universal kinematical feature is found in a bisection of the orbits. This bisection is equivalent to a correlation between the distance to the sun and the planets' motions. It was this very correlation that exposed the planetary motions to an exploration of their causes, and paved the way for a new astronomy.

Kepler's new way of analysing the planetary motions (his new astronomy) included a view of theories as a collection of causal and representational models. He stressed that it was essential that all the elements in the theory had to be expressed in mathematics in order for the theory to explain the appearances.

3 Presentation of papers

The four original papers included in this thesis describe (i) the building blocks of the edifice of the new astronomy (Papers 1 and 3), (ii) the method by which Kepler develops the building blocks (Papers 1 and 3), (iii) how the edifice is erected on these building blocks (Papers 2 and 4) and finally, (iv) the various conceptual and mathematical analyses and inventions that the whole enterprise is dependent on (all papers). I will first discuss the main findings of these four papers through four central themes, and then attempt to place my work in the context of existing literature on Kepler's work. All the mathematical expressions used in the papers are reproduced in Table 1 of Appendix 2, along with their different kinematic and physical interpretations.

3.1 Theme 1; the kinematical basis for the correct orbit

The kinematical basis for the correct elliptical orbit is described in Papers 1 and 3. I demonstrate in these papers, that there are in total four expressions describing the planets' motion.

The first two expressions, discussed in Paper 1, were developed on the basis of the correlation discussed in Section 2.5 of this introduction (Chapters 32 to 39 in *AN*). This correlation is commonly referred to as the "distance law" in the literature. I demonstrate, that the correlation in question is actually the source of *two* laws of motion, i.e. Distance Law (1) and (2) (Table 1 of Appendix 2). Distance Law (1) expresses the motion of the planet as an angular motion, while Distance Law (2) expresses a component transverse to the sun-planet radius. However, as these two distance laws did not fully describe the planets' motions, Kepler needed one more component in order to explain why the planets moved in an eccentric orbit. At this point, Kepler had also found that the orbit had to be some sort of oval. Therefore, the missing component had to account for both the eccentricity and the oval nature of the orbit.

Kepler's first attempt at introducing the missing component and his first attempt at describing an oval orbit, are not treated in detail in this thesis, but they are shortly presented in Paper 2 (Chapters 45 to 50 in *AN*, see also Section 4 on future research needs below). Kepler's second attempt at introducing the missing component, i.e. Kepler's introduction of the correct distances (Chapter 56), is discussed shortly in Paper 1 and at length in Paper 3. In this attempt, Kepler suggested a component describing a sinusoidal motion along the radius between the sun and planet. I demonstrate that Kepler developed two expressions for this component in Paper 3 (Equations (3)/(4) and (5) in Table 1, Appendix 2).

Equations (1), (2), (4) and (5) are given as differential equations (expression (4) is the differential of (3)). Kepler himself represented the relations as ratios of increments. I demonstrate in Paper 1 that these ratios are tantamount to differential equations. As such, these five expressions represent the dynamics of the planets' motion in Kepler's theory.

3.2 Theme 2; the physical foundation, method and mathematics

The differential equations all have different validations and physical interpretations. These differences are a consequence of Kepler's method of analysis. One of the overall claims in this thesis is that there is a close connection between the method of analysis, the validation of the equations and the physics.

Kepler developed Distance Law (1) "empirically" by assuming that the correlation between the sun and planet found at the apsides held throughout the orbit (see Section 2.5). Equation (3) for the radial distance was developed by a similar criterion; Kepler found that the distance expressed by (3) fell within the observational accuracy and could be expressed as a simple sinusoidal function of the so-called 'eccentric variable'. I suggest that Kepler sought correlations between variables that could express the motion of the planet as a simple function of a parameter or parameters. Subsequently, the reason why the parameter(s) in question could express the motion had to be explained by a model of forces. I argue in Paper 1, that this manner of systematic search for correlations represents a significant turn towards modern physics and modern science in general.

Kepler's subsequent search for the causes of (1) and (3) was based on the fact that they expressed motions as a function of parameters of the orbit. I describe in Paper 1 how Kepler developed a force model explaining why the distance to the sun was correlated to the planet's motion. The features of this force model were all taken from well-known natural phenomena, notably light and magnetism. These model features defined the quantitative aspects of the force in question in such a manner, that the effect of this force described a transverse component of motion, i.e. Distance Law (2).

I discuss the second force model purported to explain the motion along the radius (Chapter 57 in *AN*) in Paper 3. As with the first force model, Kepler borrowed several features from magnetism. The second model described how two forces, one pulling and one pushing the planet along the sun-planet radius, interact to create a sinusoidal motion. Kepler introduced the law of the balance as a dynamical principle for interacting forces in the analysis of this model. I argue that the development and the analysis of the model is sound and that the result represents a sound theoretical causal hypothesis on how a sinusoidal motion can be generated.

Equation (5) is the quantitative expression for the force acting on the planet. Given Kepler's principle of inertia, this expression also represents the motion the forces cause. However, this equation employs variables that are not employed in the expression he set out to explain in Equations (3)/(4). The validation that the two expressions for the radial motion are equivalent can only be done for the correct orbit (Paper 4).

In sum, Distance Law (1) and Equations (3)/(4) were supposed to be true on the basis of simple correlations pointing towards a probable causal basis, while Distance Law (2) and expression (5) were deduced from the parameters of force models. To the best of my knowledge, these reasoning processes have so far been overlooked in the literature.

3.3 Theme 3; computing time on the basis of the kinematics

A central problem for establishing the correct orbits of the planets is to determine the time it takes for a planet to reach a certain position. What we know as “Kepler’s second law” is such a measure of time, stating that the radius between the sun and planet sweeps out equal areas in equal times.

I demonstrate in Paper 2 that Kepler used the previously mentioned Distance Laws (1) and (2) to compute time. The primary time measure for Kepler, called the Keplerian time measure, was the one based on Distance Law (1), while the secondary time measure, called the Newtonian time measure, was based on Distance Law (2). The secondary time measure is the well-known “area law”, but this time measure is merely hinted at and applied only occasionally in *AN*.

The two time measures are found by solving integrals on the basis of the two distance laws (Table 2, Appendix 2). I demonstrate in Paper 2 that Kepler solved summations tantamount to integration on the basis of Distance Law (1). Many historians have interpreted his measure of time as being somewhat qualitative or imprecise (see Section 3.5). I argue to the contrary that his calculation of time is precise. However, the Keplerian time measure merely related time to certain parameters of the orbit, not to the parameters determining the actual position of the planet (the true anomaly, see Appendix 1).

Furthermore, Kepler demonstrated that on the assumption of a circular orbit, sector areas of the orbit do not measure the Keplerian time measure. I show in Paper 2 that the two time measures are only equivalent for the correct ellipse.

3.4 Theme 4; the synthesis of the new astronomy

What induced Kepler to embrace the elliptic form of the orbit? What was the basis of the proof? Was the proof inferred as the best fit to data? Or was it a mixture of the best fit to data and “qualitative physical principles”? I claim in Paper 4, that the physical foundation Kepler laid down in *AN* (presented in Papers 1 and 3), is exactly sufficient to describe why the planets move in elliptical orbits.

Kepler demonstrated this by constructing the ellipse and then proving that the physics described the elliptical orbit. First he proved that the Keplerian and Newtonian time measures were satisfied, and then that the two distance laws were satisfied at each instant. This second part is particularly important, since it involves the proof that the instantaneous motion in the ellipse is a consequence of the components of motions described by the aforementioned differential equations. The interpretation of this proof is challenging, since it does not apply components of motion directly, but relies on coordinating Distance Laws (1) and (2) and the distances produced by (3).

Kepler’s new astronomy is not merely another theory of the planets’ orbits; it represents a new approach to astronomy. The final proof of the correct orbit depended on a well-built edifice. The different parts of this edifice involved models of forces, equations describing the motions, mathematical methods for representing these motions, and computing values on the basis of these equations.

3.5 Previous interpretations of Kepler's work

Previous interpretations of Kepler's work on the planetary motions have been remarkably varied. In this section I will shortly present some of the areas where my interpretations both differ from and coincide with other interpretations.

3.5.1 Interpretations of Kepler's geometry and metaphysical commitment

One of the overarching reasons for the varied interpretations of Kepler's scientific work concerns Kepler's guiding principles. What exactly convinced Kepler that he was on the right track or that his models were correct?

A common view among historians of science has been that Kepler sought geometrical forms or symmetrical curves. The general assumption has been that the geometrical form and fit with data were final arbiters for Kepler (Whiteside 1974: 14; Davis 1989: 95).

I agree to a certain extent that Kepler had "metaphysical" guiding principles, but that these did not differ in any essential way from guiding principles of modern scientists. Kepler believed that God had created a harmonious well-ordered universe, but this does not necessarily mean that *geometrical* symmetry was his final arbiter. I claim in my papers that he considered the geometry as a tell-tale of the causal principles *generating* the orbit, but the form of the orbit by itself was never the final arbiter, nor were the data.

3.5.2 Interpretations of Kepler’s mathematics, physics and distance laws

Interpreting Kepler’s reasoning is a daunting task when it comes to his mathematics expressing the motions. The reason for this is that Kepler did not have the benefit of formalized modern calculus. Instead, he tweaked geometry to perform the necessary tasks. I owe much of my growing understanding of Kepler’s mathematics to Davis’ works (Davis 1989, 1992). Davis has done a tremendous job of analysing the increment ratios and generally putting the focus on Kepler’s mathematical precision, and her work has had very important influence on my work on Kepler. The central differences that set my analyses on Kepler’s work apart from Davis’, concern the relation between the kinematical expressions of the planet’s motions and their causal foundation (Paper 1 and 3), and the time measurement deduced on the basis of the distance laws (Paper 2).

I have found the clearest exposition of the mathematical relations that introduces the distance laws in Small’s and Davis’s works (Small 1804/1963; Davis 1992). They both recognize that (1) and (2’) are possible to deduce for the apsides of the bisected eccentric orbit. Nevertheless, my interpretation of the distance laws deviates from Davis’ and many others (for details see Paper 1). To my knowledge, the force model developed in Chapters 33 to 38 in *AN* has not been interpreted as having any deductive significance for the establishment of the motion of the orbit before. The various accounts on the matter all seem to view the physical model as an attempt at saving Distance Law (1). I on the other hand claim that the force model explicates a specific component of motion transversely to the radius, expressed as Distance Law (2) (Paper 1).

The deviation of interpretations has consequences for several of the following analyses. I will here mention three central discussions where the different interpretations have profound consequences.

If the transverse component (Distance Law (2)) is not assumed to express one specific component, then the problem of deciding the variation in distances does not become a problem of combining specific components to generate an orbit. Other scholars, with the notable exception of Wilson (1974), have concluded that the discussion on the distances was to adjust the geometrical form of the path from an eccentric circle to an oval. According to my interpretation, the force model explained a *concentric* motion about the sun, while the fact that the orbit was eccentric and oval called for another specific component of motion (Papers 1 and 3).

The other consequence of the different interpretations of the distance laws that I would like to mention concerns the computation of time (Paper 2). Most scholars (Kepler 1609/1929; Stephenson 1994; Aiton 1969) have recognized that the sums of distances are not equivalent to areas of the orbit, given a circular orbit. Caspar (in Kepler 1609/1929: 45) has pointed out that the three different time measures (the equant hypothesis, the area law and the distance sum) are not equivalent, but he viewed these three time measures as competing hypotheses. Furthermore, he believed that Kepler had to decide which of the time measures were correct on the basis of observations. However, if my analysis is correct, Kepler assumed that both the distance sum (associated with Distance Law (1)) and the area-law (associated with Distance Law (2)) were true of the correct orbit while the equant hypothesis was altogether abandoned as a viable kinematical description.

The last consequence concerns the final proof of the orbit. I argue in Paper 4 that the proof of the correct orbit relies on the assumption that the two time measures are equivalent. The part that has vexed most scholars (Kepler 1609/1929; Aiton 1969) concerns the coordination of the distance laws expressing the instantaneous motion(s) of the planets. It is not possible to make sense of Kepler's proof if the whole set of equations (presented in Table 1, especially Distance Laws (1) and (2)) is not assumed.

3.6 Concluding remarks on the presentation of the papers

I have demonstrated that Kepler managed to develop a coherent theory that connected force models to specific kinematical expressions, and that these expressions produced the correct elliptical orbit. An essential factor for Kepler's success in consolidating the coherence of the theory was that he subsumed all the causal features of the system under mathematics. I claim that Kepler proved the elliptical orbit on the basis of mathematical - physical models that introduced basic elements of classical mechanics, which were in many ways equivalent to later Newtonian physics.

I have also revealed several features of the sophisticated scientific method Kepler used to achieve this goal in this thesis, and I claim that Kepler´s new astronomy was not merely an introduction of a new theory, but a new way of systematically analysing dynamical systems in order to reveal the underlying causes for the behaviour of the system. I hope that the conclusions from the case study presented in this thesis can be a source for revising both our understanding of the historical development of modern science and our philosophical theories on theory development.

4 Future research needs

There are two important parts of Kepler's work that I have not included in this thesis that require further analysis.

The first part is the complete analysis of Kepler's physical model of the radial motion. Paper 3 is somewhat incomplete with regard to the full analysis of the physical aspects of this model. The physics of the model require that the force would also vary inversely to the distance. Kepler gives an explanation for why the distance is cancelled out both in *AN* and in the *Epitome*. My preliminary analyses of this explanation indicate that Kepler's physics is challenged by the Keplerian inertia principle. I hope to be able to complete this analysis in the near future.

The other part concerns Kepler's first suggestion of how an oval orbit could be generated in Chapters 45 to 50 of *AN*. The analyses Kepler performed in these chapters did not introduce any physical models or kinematical expressions that are included in his final edifice. However, I assert that this part has to be re-analysed in the light of the findings of this thesis.

Philosophical analysis on theory development has relied heavily on case studies, and Kepler's work has been among the most referred cases. In recent years there has been a growing interest in fields studying theory development, such as abduction (reasoning to new hypotheses) (Paavola 2004; Aliseda 2004; Magnani 2004), model-based reasoning (Nersessian 1995; Cartwright 1994; Portides 2005) and the use of analogies in science (Gentner 1983; Gentner 2002; Abrantes 1999; Holyoak and Thagard 1995). It will be fruitful to use the findings in this study to further develop these fields of philosophy of science. At the same time an analysis of Kepler's methods of scientific enquiry would benefit greatly from the philosophical insights these fields can offer, and I hope to be able to take part in this exchange in the future.

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Paper 1

Paper 2

Paper 3

Paper 4

Appendix 1

Appendix 1 – Glossary

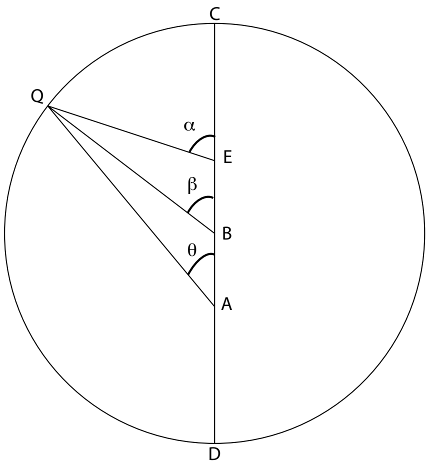

Fig. 1. Bisected eccentric circle

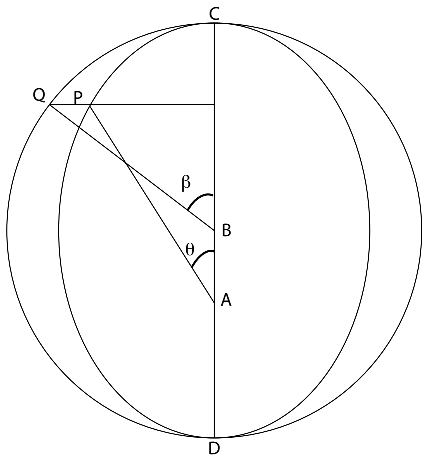

Fig. 2. Ellipse correlated to auxiliary circle

The geometrical references in the list of concepts and variables below refer to both Figures 1 and 2 above, unless otherwise stated. The concepts that originate from Kepler's work are marked with a (K). For conventional concepts that Kepler used in an unconventional way I give both the conventional and Kepler's meaning[1]. Kepler's work *Astronomia Nova* (1609) is referred to as *AN*.

[1] The definitions are largely based on "A Glossary of Technical Terms" in *The General History of Astronomy Volume 2: Planetary astronomy from the Renaissance to the rise of astrophysics Part A: Tycho Brahe to Newton* (Taton and Wilson 1989) and the "Glossary" in *Kepler´s Physical Astronomy* by Bruce Stephenson (1994).

Anomaly: the position of a heavenly body in the heavens, often measured from the aphelion or perihelion/perigee in terms of degrees, minutes and seconds.

Aphelion: the point farthest from the sun (in a heliocentric system, *D*).

Apsidal line: the line connecting the apsides (line *AC*).

Apsides: the two points a planet occupy in its path that are respectively closest to (*D*) and farthest from (*C*) the body (or point in space) it is believed to revolve around.

Arc: part of a concentric circle. Kepler customarily used ‘arc’ to designate the corresponding angle it subtends. Thus, arc should not be treated as part of the actual path of an orbit (Davis 1992).

Bisection of eccentricity (or only *bisection*): the distance between the centre (*B*) and the equant point (*E*) is equal and opposite in direction to the distance from the centre (*B*) to the central body (*A*).

True (coequated) anomaly: angle about the sun measured from the line of apsides to the point where the planet actually is ($\angle CAQ$). It measures the longitudes of a planet.

Deferent: the circle an *epicycle* travels along. The epicycle’s centre follows a circular path around the sun (e.g. in some of Kepler´s early models) or the Earth (e.g. Ptolemaiian models), this path is called the deferent.

Eccentric or *eccentric circle*: a circular orbit about some central astronomical body (i.e. the Earth in geocentric systems or the sun in heliocentric systems) where the central body is not located in the centre of the circle. References to an ‘eccentric’ often implies that the *equant* concur with the centre of the circle. This is not the case for the orbits discussed in the papers included in this thesis, where eccentric merely refers to the fat that the sun is located outside the centre (at *A*)

Eccentric anomaly (K): angle about the centre of the ellipse measured from the line of apsides to the point the planet would have occupied if the path were a circle ($\angle CBQ$). The measurement can also be the length of the eccentric arc from the apside to the planet. In an unknown orbit the eccentric anomaly is undefined with regard to longitudinal position. In the correct ellipse (*CPD*, Figure 2) the eccentric anomaly is defined on the auxiliary circle (*CQD*) by projecting the position of the planet *P* onto the circle via the normal to the apsidal line (*QH*).

Eccentricity: the distance between the centre (*B*) and the central body (*A*).

Ecliptic plane: plane of the Earth's orbit.

Epicycle: a small circular orbit whose centre moves on the *deferent*. Kepler used an epicycle to represent the radial motion in Chapter 56. This was merely a device used to represent the distance and was not intended to represent the actual motion in an epicycle in the traditional way.

Equant (or *equant point*): the point around which a body moves with uniform angular velocity.

Optical equation: difference between eccentric anomaly and true anomaly ($\angle BQA$ in Figure 1). Typically applied to change angular reference point from the centre (B) to the sun (A), or *vice versa*. In chapter 40 Kepler referred to the 'optical equation' as an area, this area being the isosceles triangle QBO since the area and the angle are proportional.

Physical equation: also physical equation of centre: the part of the equation that arose from the fact that the motion around the centre varied. Kepler explained the physical equation by the planets radial component of motion (Chapter 38/39 of *AN*). If the orbit had moved uniformly in a concentric, the physical equation would be zero. In Chapter 40 of *AN* the concept is used as the excess of the mean anomaly over the eccentric anomaly.

Mean anomaly: measures the time elapsed since the planet was in aphelion. The measure is given in degrees, one period equals $360°$, or in radians. The mean anomaly is given by $\angle CEQ$ in models using an equant. Kepler abandoned the equant and developed other measures for the mean anomaly (see Paper 2 in thesis).

Mean sun: for a geocentric model, it is the positions on the sky where the sun would have been if it's apparent motion had been uniform. Conversely, in Copernicus' model it is the point around which the earth moves with uniform angular motion. It should be noted that Copernicus also believed that the centre of the Earth's orbit was in the mean sun.

Perihelion: the point in the path that is closest to the sun (only for heliocentric models).

True sun: the point where the sun is actually located.

Inequality, first and second: Change in the speed an/or direction of the planet’s motion. Kepler inherited the pair “first inequality” and “second inequality” from the ancients. The first inequality is the variation in speed of the planet, ignoring the effect of the second inequality. The second inequality is the apparent (from Earth) speeding up of a planet that is in conjunction and slowing down or backwards motion of a planet that is in opposition. In the heliocentric models the second inequality arises from the fact that the observer is located on the moving Earth.

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Appendix 2

Appendix 2 - Mathematical relations and deductions

The mathematical relations, deductions and equations presented below are the found in Johannes Kepler's correct solution of the elliptical orbit, referred to in Papers 1-4 in this thesis. Please note that at the time Kepler established these various relations he did not know for certain that they were true descriptions of the states of the planet. The trueness of the equations relied on whether they could be proved to work in union to generate an orbit within the limits of observational accuracy. This proof is found in Chapter 59 of *Astronomia Nova* (1609, henceforth *AN*) (see Paper 4).

Equation (1), Paper 1: Equation (1) is a fairly straightforward deduction from the bisected eccentric circle, deduced in Chapter 32. However, the deduction holds only at the apsides. Kepler simply assumed that this relation will hold for the rest of the orbit.

Equation (2)/(2'), Paper 1: This equation is developed from a causal model in Chapter 33, and further analysed in the subsequent Chapters 34 to 38. It represents one component of motion driving the planet around the sun perpendicular to the radius, i.e. the line connecting the planet and the sun.

Equation (3), Paper 3: The distance of the planet from the sun, presented in Chapter 56. The distance is based on the natural motion this represents (see it's derivation in Equation 4) besides it's fit with observations.

Equation (4), Paper 3: The derivative of (3). Kepler represents the incremental distances, *dr*, geometrically via an epicycle in the *AN*. The epicycle is not meant to represent a motion at this point, only the relation between the increments of distances and eccentric anomalies. From the geometrical figure Kepler recognised the "natural" relation between increments of distance and eccentric anomaly pointing towards a probable causal mechanism. The expression is derived more accurately in the *Epitome of Copernican Astronomy*, 1618-1621 (p. 134).

Equation (5), Paper 3: Developed from a causal model consisting of two interacting forces where the interaction at each point is determined by the eccentric anomaly.

Figure and main identities of the circle and ellipse

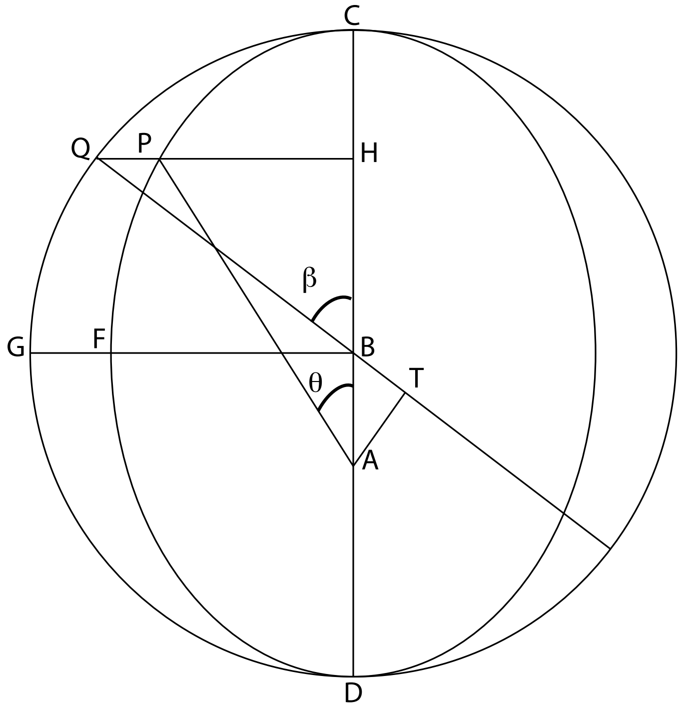

Fig. 1. Auxiliary circle and ellipse related by the ordinate *QH*.

A	is the sun
B	is the centre of the orbit
C and *D*	are the apsides
G	is the position at the quadrant of the ellipse
F	is the position at the quadrant of the circle
Q	are the intermediate positions on the circle
P	are the intermediate positions on the ellipse
$\angle CAQ = \theta$	the true anomaly
$\angle\ CBQ = \beta$,	the eccentric anomaly
$BC = BD = a$	radius of circle and later major axis of ellipse
$BF = b$	minor axis of ellipse
$AB = ae$	where e is the eccentricity of the orbit
QT	called the ‘diametral distance’ by Kepler in Chapter 40.

On the assumption that the orbit is a circle we have the distance
$r = AQ$

On the assumption that the orbit is an ellipse we have the distance

$r = AP$

Differential equations and integrals

Table 1: Differential equations and physical interpretations

Nr.	Relations	Ch.	Kinematical description	Causal description
(1)	$\frac{dt}{d\beta} \propto r$	32	Angular motion around eccentric anomaly. Implicit expression of resultant motion. Form: differential.	Implicit expression of the sum of forces. Instantaneous.
(2)	$\frac{ds_t}{dt} \propto \frac{1}{r}$	33	Motion transverse to the radius. Form: differential.	Explicit expression of force transversal to radius. Instantaneous.
(2')	$\frac{dt}{d\theta} \propto r^2$	33	Angular motion around true anomaly. Form: differential	Implicit expression of force acting transverse to the radius. Instantaneous.
(3)	$r(\beta) = a(1 + e\cos\beta)$	56	Geometrical description*: The distance of the planet at certain eccentric anomalies. Form: integral.	
(4)	$\frac{dr}{d\beta} \propto ae\sin\beta$	57	"Motion"** along the radius. Form: differential.	Unspecified forces. Instantaneous**.
(5)	$\frac{dr}{dt} \propto \sin\theta$	57	Motion along the radius. Form: differential.	Causal factors implied in the expression 1. One repulsive and one attractive force (effective cause). 2. Resultant dependent on the angle the axis of forces makes with the sun.
(oc)	$\frac{d\theta}{d\beta} = c\frac{1}{r}$			

Nr. = Number. Ch. = Chapter in *AN*.* (3) is not a kinematical description at all. It merely describes the length given the eccentric anomaly.** The motion described here is unspecified with regard to time. But it *is* motion since the progression of β is dependent on time. Furthermore, 'instantaneous' is not applied in the modern form where the independent variable time is thought to approach zero. Here it is used generally where the independent variable, either describing time or space, is thought to approach zero or being "as small as you want". This concerns any instance where time is not the independent variable (as in (1) and (2')). (3*) a*e* versinβ; distance covered since the planet was at aphelion.

Table 2. Differential equations and integral solutions

Number	Incremental time	Integral	Integral solutions
(1)	$dt(\beta) \propto r d\beta$	1	Keplerian time measure $t \propto \int r\, d\beta$
(2)	$dt(\theta) \propto r^2 d\theta$	2	Newtonian time measure: $t \propto \int r^2 d\theta$

Elementary geometrical relations of the ordinate construction

(See Paper 3 and 4)

$$\frac{b}{a} = \frac{PH}{QH} \qquad \text{(i)}$$

Various lines are given

$$PH = r\sin\theta \qquad \text{(ii)}$$

$$QH = a\sin\beta \qquad \text{(iii)}$$

$$\begin{aligned} AH &= r\cos\theta \qquad \text{(iv)} \\ &= a(\cos\beta + e) \end{aligned}$$

The ratios of the sines of the true and eccentric anomalies can then be found

$$\frac{b}{a} = \frac{PH}{QH}$$

$$\frac{b}{a} = \frac{r\sin\theta}{a\sin\beta}$$

$$\frac{b}{r} = \frac{\sin\theta}{\sin\beta} \qquad \text{(v)}$$

Relation between sector areas of the circle and ellipse. From (i) we have

$$PH = \frac{b}{a}QH$$

From proposition 5 of Archimedes' *on Spheroids* Kepler could state

$$areaCGD = \frac{b}{a}areaCQD$$

Again from Archimedes proposition 5 and a proof by Commandino Kepler deduces in the following

$$\text{Segment } CPH = \frac{b}{a}\text{Segment } CQH \qquad \text{a.}$$

Since the triangles HPA and HQA share the same height AH, while their bases are HP and HQ respectively, we get that

$$\Delta PHA = \frac{b}{a}\Delta QHA \qquad \text{b.}$$

From a and b we get that

$$areaCPA = \frac{b}{a}areaCQA \qquad \text{(vi)}$$

Additional relations and deductions

Optical cause or Requirement 1 (Paper 4)

$$\frac{d\beta}{d\theta} \propto r \qquad \text{(oc)}$$

Deduction 1 – from distance-laws: given that Distance Laws (1) and (2) are true for the correct orbit, they determine a specific property of the correct path. From distance-law (1) we get an expression of increments of time

$$dt \propto r d\beta$$

If we substitute the increments of time in Distance Law (2) for the expression determined by (1) we get a specific property that the correct orbit of the planet must satisfy, i.e. the optical

$$\frac{r d\beta}{d\theta} \propto r^2$$

$$\frac{d\beta}{d\theta} \propto r$$

Q.E.D.

The optical cause is deduced directly from the differential equations describing the motion of the planet.

Deduction 2 – differentiation of ellipse

If we *know* that the correct orbit is the sun-focused ellipse and that ordinates to the apsidal line determine the relation to the circumscribing circle we can also deduce the optical cause. A characteristic of the ordinate constructed ellipse is

$$\frac{b}{r} = \frac{\sin\theta}{\sin\beta} \qquad \text{(v)}$$

Inserting for distance r expressed by the eccentric anomaly ((3) in table 1) we get

$$\sin\theta = \frac{b\sin\beta}{a(1+e\cos\beta)}$$

Differentiating with respect to β and using the chain rule on the left side of the equation (we suppose that the true anomaly can be exressed as a function of the eccentric anomaly):

$$\frac{d\sin\theta}{d\theta}\frac{d\theta}{d\beta} = \frac{b}{a}\frac{d}{d\beta}\left(\frac{\sin\beta}{1+\cos\beta}\right)$$

$$\cos\theta\frac{d\theta}{d\beta} = \frac{b}{a}\frac{\cos\beta(1+e\cos\beta)+e\sin^2\beta}{(1+e\cos\beta)^2}$$

Using (iv) we get

$$\frac{d\theta}{d\beta} = \frac{b}{a}\frac{r}{a(\cos\beta+e)}\frac{\cos\beta+e(\cos^2\beta+\sin^2\beta)}{(1+e\cos\beta)^2}$$

$$= b\frac{r}{r^2}\frac{\cos\beta+e}{\cos\beta+e}$$

Hence,

$$\frac{d\theta}{d\beta} = \frac{b}{r}$$

Q.E.D.

Correlation of arcs and increments of areas on the basis of Requirement 1

(A different version is found in Paper 4)

Increments of transverse arc, ds_T, is given by

$$ds_T = rd\theta$$

Using the optical equation (Equation oc) we get

$$rd\theta = bd\beta \;\Rightarrow\; ds_T = bd\beta$$

Arcs on the auxiliary circle is given by

$$ds_c = ad\beta$$

where ds_c is arcs on the circle. For equal incremental arcs on the auxiliary circle (corresponding to equal increments of eccentric anomaly) we have

$$\frac{ds_T}{ds_c} = \frac{b}{a}$$

Adding ½ r to the fraction of arcs (by the rule of expansion of fractions) gives

$$\frac{\tfrac{1}{2}rds_T}{\tfrac{1}{2}rds_c} = \frac{b}{a}$$

$$\frac{dA_e}{dA_c} = \frac{b}{a}$$

Or

$$dA_e \propto dA_c$$

where dA_e is an incremental sector area of the ellipse and dA_c is the corresponding incremental sector area of the circle, both with vertices at A.

Requirement 2

Deduction 1 – deducing Requirement 2 (Paper 4)

(5) –Radial motion deduced from force model (Paper 3)

$$dr \propto \sin\theta dt \qquad (5)$$

(4) – Radial motion justified from 'natural' oscillation (Paper 1 and 3)

$$dr \propto ae \sin\beta d\beta$$

We want to demonstrate that these radial increments correspond in the same time interval. (4) does not express time, but we have that time is related to the eccentric anomaly in distance-law (1). Premise: Time from Distance Law (1)

$$dt \propto r d\beta$$

(1) in (5) gives:

$$dr \propto \sin\theta (r d\beta)$$

We can now find the condition for an orbit where the two expressions for radial increments are equivalent:

$$\sin\theta (r d\beta) \propto \sin\beta d\beta$$

$$\Rightarrow \frac{\sin\beta}{\sin\theta} \propto r$$

From (v) we know that the ordinate related ellipse satisfies this condition.

Hence, (4) and (5) is proved to be equivalent in the elliptic orbit.

Time measures

Newtonian time measure

Increment of time based on Distance Law (2):

$$dt(\theta) \propto r^2 d\theta$$

Increment of time to incremental areas:

$$\begin{aligned} dt(\theta) &\propto r^2 d\theta \\ &\propto r ds_T \\ &\propto dA \end{aligned}$$

since $dA = ½\, r^2 d\theta$. Integral measure of time (Integral 2, Paper 2)

$$t(\theta) \propto \int r^2\, d\theta \qquad \text{Integral 2}$$

Areas swept out by the radius measure the time. This holds for *any* path as long as the angular motion around the sun is given by (2)/(2’).

Keplerian time measure

Increment of time based on Distance Law (1)

$$dt(\beta) \propto r d\beta$$

Integral measure of time (Integral 1, Paper 2)

$$t(\beta) \propto \int r\, d\beta \qquad \text{Integral 1}$$

Requirement 1*

Given that the both Integral 1 and Integral 2 measure the times we can formulate a requirement on the areas of the orbit

$$t(\theta) = t(\beta)$$

$$\text{Integral 2} \propto \text{Integral 1}$$

The circle

On the assumption that the orbit is a circle we have that the sector areas is not a solution to Integral 1.

The distance in the circle is given as

$$AQ = a\sqrt{1+e^2+2e\cos\beta}$$

Kepler developed Figure 2 as a measure tantamount to Integral 1. He could not compute the area of this figure.

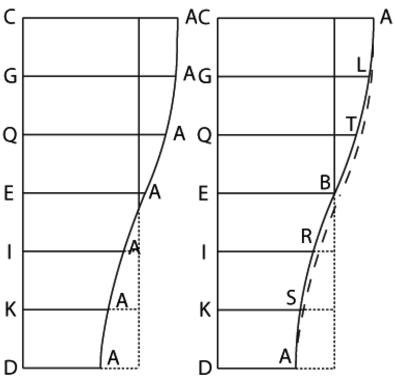

Fig. 2. Geometrical solution to Integral 1 on the assumption of a circular orbit

The ellipse

In the correct ellipse the distance is given by

$$r(\beta) = a(1+e\cos\beta)$$

The solution to Integral 1

$$\begin{aligned} t(\beta) &\propto \int r(\beta)d\beta \\ &= \int a(1+e\cos\beta)d\beta \\ &= a(\beta + e\sin\beta) \end{aligned}$$

The solution to Integral 2 is trivially areas. Sector areas of the circle is given by

$$\text{area } CQB = \tfrac{1}{2}\, a^2\beta$$

$$\text{area } QBA = \tfrac{1}{2}\, a^2 e\sin\beta$$

$$\Rightarrow \qquad \text{area } CQA = \tfrac{1}{2}\, a^2(\beta + e\sin\beta)$$

Hence, the solution to Integral 1 is proportional to Integral 2 by a factor of ½ a^2, and we can conclude that Requirement 1* is satisfied in the ellipse.

Appendix 3

THE FIRST BOOK OF THE

EPITOME OF COPERNICAN ASTRONOMY [1]

ON THE PRINCIPLES OF ASTRONOMY AS A WHOLE, AND OF SPHERICAL DOCTRINE IN PARTICULAR

What is Astronomy?

It is a science that provides the causes behind the things in the sky and amongst the stars that appear to us down on Earth, things that give rise to the changes of the times: when these things are understood, we may be able to predict the face of the sky, that is, the appearances in the sky before they take place, and to identify the exact time of appearances that have taken place in the past.

How did it receive the name astronomy?

From the law or regime of the stars, that is of the movements that the stars are making, just like Economy from the supervision of domestic matters, or Paedonomus from the supervision of children.

What is the relationship between this science and the other sciences?

1. It is a part of Physics, because it searches for the causes of natural things and events: and because the movements of objects in the sky are among its subjects: and because its only aim is this, to establish how the edifice of the world and its parts are pieced together.

2. Astronomy is the soul of Geography and of Hydrography, or seamanship. For the various things that take place in the sky [as seen] at various places and positions on land and at sea, can only be singled out from each other by means of Astronomy.

3. It is superior to Chronology, because the movements in the sky categorise time as well as the political years, and they place their signature on historical events.

4. It is superior to Meteorology. For the stars are moving and influencing Nature below the moon, and even the human beings themselves in a certain sense.

5. It comprises a substantial part of Optics, because it has a subject in common with it, [namely] the light of objects in the sky: and because it reveals many illusions of vision as regards the form of the world and of movements.

6. It is, however, subordinated to the *genus* of Mathematical disciplines, and it uses Geometry and Arithmetic as two wings; taking into account the quantities and figures of the objects and movements of the world, measuring and counting time, and using time to establish its demonstrations: and bringing speculation in its entirety into use, or *praxis.*

How many-sided, then, is the task and toil of the Astronomer?

The astronomical task consists chiefly of five parts, the Historical, which has to do with Observations, the Optical, which has to with Hypotheses, the Physical, which has to do with the causes of Hypotheses, the Arithmetical, which has to do with Tables and Calculation, [and] the Mechanical, which has to do with Instruments.

How do these parts differ?

Although none of these parts can cope without Geometrical demonstrations, which contribute to Theory, or Numbers, which contribute to *Praxis*, since there are certain numbers[2] that are, so to speak, the language of the Geometricians: the three former have, however, more to do with Theory, whereas the two latter have more to do with *Praxis*.

On Observations

Describe for me the first of these parts, the Historical.

The Historical part describes, to begin with, how the face of the world appears to us, and [singles out] what is changing during a day, what during a year, or in the course of longer periods of time: what appears differently and what stays the same at various places on land or at sea. Really rare or notable events, such as eclipses of the Sun or Moon, or spectacular conjunctions, it culls from historical sources. More subtle observations of single stars, however, it collects from the books of credible authors, namely, *Hipparchus*, *Ptolemy*, *Albategnius*, *Arzacheles* as well as other authors cited by these, and brings them together, while adding even further observations provided by the present generation of observers: in this respect the incredible diligence of *Tycho Brahe* surpasses that of all others, with his series of copious and most trustworthy observations, made personally and almost without interruption over a period of altogether 38 years.

Thus, observations of this kind ought to be compared with each other in a sophisticated manner, and to be singled out from each other in well defined groups, and in well defined periods of time, so that similar things fit similar things: in about the same manner that *Aristotle*, when describing the History of Animals, started out by establishing a most sophisticated history of animals, by singling out, for all species that had been defined to belong to the same *genus*, those traits that these had in common with the rest.

On Hypotheses

Describe for me also the second part of the Astronomical Task.

The second part, the Optical, strives, after having considered these varieties of Observations and the belonging together of certain observations in certain groups, to penetrate into the causes that make *species* that are so utterly at variance with the truth take place before the eyes of human beings, *species* that Astronomers call Appearances, [or] in Greek *phaenómena.* Here, the intellectual capacity of each person decides how many types of appearances he saves and establishes by means of some single form of movement that perpetually remains similar to itself, or by means of some single figure of bodies; by accommodating the entire method of his demonstration to laws and Theorems, be they Geometrical or Optical, a method that is subordinated to Geometry: and the result is that one person, by taking forms of movements of this kind into account, comes closer than another to the very Nature of things. Thus, although he in this strenuous and blind grappling for causes touches upon the laws of Nature, [while] being led astray from the truth in some constituents of his Opinions, they [i.e., the Astronomers?] may nonetheless save the appearances of the sky by means of these: conventionally, we call the opinion that each famous author uses in order to explain the causes of the Appearances of the sky, Hypotheses: because the Astronomer has the habit of saying: when this or that, claimed by himself to be true about the World, is supposed (*hypotithéntos*), it follows by the necessity of the Geometrical demonstrations, that the appearances that are contained in the above-mentioned historical signature, have occurred on precisely the number of occasions and at the time they did.

Nowadays, there are three such forms of Hypotheses, called the *Ptolemaean*, the *Copernican*, and the *Tycho-Brahean*.

The contemplation of nature and of the properties of light, or the *praxis* of the doctrine of Refractions, also pertains to these two first parts.

On the causes of Hypotheses

What exactly is the third part of the Astronomical Task?

The third part, the Physical, is commonly held to be unnecessary for the Astronomer, even though it is most essential in order to reach the aim of this part of Philosophy, and cannot be resolved by anyone but the Astronomer. For Astronomers should not be given pure licence to make whatever speculation they like without reason; indeed, you should be able to render probable also the causes of your Hypotheses, which you present as the true causes of the Appearances, and thus establish the principles of your Astronomy in the higher science, namely Physics or Metaphysics; without being shut out from those Geometrical arguments, Physical or Metaphysical, that are provided for you by the detailed exposition[3] of the discipline proper alone, concerning things pertaining to the higher disciplines, as long as you avoid meddling with any appeal to the Beginning[4]. Through this pact the Astronomer (made the master of his ambition insofar as he created hypotheses concerning the causes of movements, causes that were in agreement with reason and fit to make everything that is found in the history of Observations happen) at last brings together in one presentation everything that he previously had [only] stated separately. He then pretends that the aim stated so far (that is, the demonstration of phenomena, and the benefits for the common good resulting from such demonstrations) is no longer at stake, and strives with praise from the philosophers to reach a higher aim, and to this aim he now turns all his pleasurable attention, employing all his arguments, be they Geometrical or be they Physical: in order, of course, to place before the eyes the genuine form and disposition, or ornamentation of the entire World: and this is precisely that very Book of Nature, in which Good the creator in part made known and depicted His essence and His will towards man, in a kind of wordless language.

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[1] Translated by Per Pippin Aspaas, University Library of Tromsø (per.pippin.aspaas@uit.no). The translation follows Max Caspar's edition in *Gesammelte Werke*, vol. VII, 1953. The Johannes-Plancus edition of 1618 (copy digitised by Universiteit Gent, retrieved from Google Books 14 August 2009) has also been consulted. The original's use of italics has been retained, as has the widespread use of initial capital letter. A few Greek and Latin terms have been retained in translation. These are italicised as well.

[2] Both the Johannes-Plancus edition of 1618 and *Gesammelte Werke* has *cum sint quidam quasi sermo Geometrarum*, which is somewhat ambiguous. I see three possible solutions: 1. "since there are certain numbers that are, so to speak, the language of the Geometricians" (*quidam* ['certain'] can only refer to *Numeris* ['numbers'], the only word in masculine plural in the preceeding part of the sentence). 2. "although there are certain numbers that are, so to speak, the language of the Geometricians" (*cum* used in the concessive sense

[‘although’ instead of ‘since’]). 3. *quidam* (‘certain’) is a misprint for *quidem* (‘indeed’), in which case the phrase should be translated: “since these [viz., numbers in general] are indeed so to speak the language of the Geometricians”.

[3] “that are provided for you by the detailed exposition (*diexodo*) of the discipline proper alone”. *Diexodus* has several meanings in classical Greek, where it is attested in both a literal (‘outlet, passage’ etc.) and figurative sense (‘detailed narrative or description’ etc.); cf. Henry George Liddel & Robert Scott, *Greek-English Lexicon*, 9th edn, Oxford 1996, s.v. διέξοδος. The question is how it was used in the learned Latin of Kepler’s age. A parallell might be the theological work of Johann Forster, *Diexodus exodi Daß ist / Gründliche…Erklerung oder Außlegung vber das Ander Bvch Mose*, Wittenberg 1614 (‘Diexodus of the Exodus, or: Thorough Explanation or Exposition of the Second Book of Moses’); cf. Martin Bircher, *Deutsche Drucke des Barock 1600-1720 in der Herzog August Bibliothek Wolfenbüttel*, vol. I, Munich 1977, p. 143. Also the philosopher/mathematician Joachim Jungius (1587-1657) described detailed, copious treatises and commentaries as *diexodi*; cf. Clemens Müller-Glauser’s introduction to his critical edition of Joachim Jungius, *Disputationes Hamburgenses*, Göttingen 1988, p. xxii.

[4] “appeal to the Beginning” (*Principij petitionem*) is ambiguous. *Principium* may be used in the sense ‘principle’, but I suggest that Kepler here alludes to the Biblical sense, as in the opening words of John’s Gospel: *In principio erat verbum ...* This would imply that Kepler distinguishes the astronomer’s task from that of the theologian.

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