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APPROXIMATION VERSUS IDEALIZATION: THE KEPLER-NEWTON CASE*

1. Introduction: approximation versus idealization

In general, the laws and theories of science are not exactly true. That much is agreed upon by almost all philosophers of science. There is, however, a difference in emphasis. The stress is commonly laid on "exactly": scientific laws are not *exactly* true, but they do not — or should not — deviate too much from the phenomena observed. That is, scientific laws are *approximations* to the truth. On the other hand, one also can italicize the thesis this way: scientific laws are *not* exactly *true*. The virtues of scientific laws then lie in the fact that they tell us what would be the case if such-and-such simplifying assumptions could be made. As a rule, the initial or boundary conditions described by these assumptions do not obtain in fact, they are *counterfactual* and thus are responsible for the falsity of the laws. It is in this sense that I will call scientific laws and theories *idealizations* in this paper.

The two views on scientific laws and theories, which on the face of it are not connected in any cogent way, are not kept apart in the literature as nicely as the subject would seem to require. With few exceptions[1], philosophers of science tend to mix up arguments from the approximation view with those from the idealization view. It is true that many idealizations get their heuristic and practical value by being approximately true, but there is no reason to suppose that all, or only the majority of, idealizing assumptions lead to predictions close to the truth or that all, or most, approximately true laws can be construed as being based on counterfactual conditions. The conceptual relationship between idealization and approximation seems to be delicate, and certainly there is much to be done in this field.

In this paper I do not try to collect abstract arguments for my understanding of idealizations, nor will I advance general philosophical reasons and consequences of their detachment from approximations. My aim is rather to illustrate the difference by means of a simple and well-known example: the relation between *Kepler's laws of planetary motion* (KLP) and *Newton's theory of universal gravitation* (NTG). Since the works of Duhem [1906], Popper [1957], and Feyerabend [1965] it is commonly acknowledged that KLP and NTG are, strictly speaking, incompatible[2]. The various claims found in popular accounts of the history of science and in textbook introductions that NTG was "derived from" KLP or conversely that KLP "follow from" NTG, therefore cannot be accurate. If NTG is true, then KLP are false, and *vice versa*. But of course there is a close relationship between KLP and NTG, so it is reasonable to suppose that here we are facing a case of intertheoretic approximation and/or intertheoretic idealization[3]. In fact, KLP come very close to what follows from NTG for the solar system, and KLP, or rather a modified form thereof, will turn out to be an idealization if viewed from the standpoint of NTG. The purpose of this paper is to disentangle and contradistinguish the approximation view and the idealization view on the relation between KLP and NTG.

In the next section, a number of proposals to establish the Kepler-Newton case as a paradigm of intertheoretic approximation — all of them in the wake of Scheibe's seminal paper [1973a; also see 1973b] — will be discussed and criticized. Section *3* offers a qualitative account of how idealizing assumptions figure most importantly in the simultaneous explanation of both the significance and the failure of a predecessor theory by a superior successor theory. Section *4* then proceeds to analyze the relation between KLP and NTG as a case of idealization. Two attempts are made to show that NTG is in fact a superior theory to KLP. The first idealization referring to "one-body systems" is found to be wanting since it is hardly interpretable from the point of view of NTG. Two-body systems, however, will serve our needs and show that Kepler's third law has to be modified. It is argued in conclusion that the picture of the Kepler-Newton case provided by the idealization view is complementary and perhaps preferable to that provided by the approximation view.

2. The Kepler-Newton case as in instance of approximation

Erhard Scheibe [1973a] is the author of a trail-blazing investigation into the Kepler-Newton case. He considers state descriptions of *n*-body systems ($n \geqslant 2$), together with constants $m_i \in \Re^+$ ($i = 1, ..., n$) or $k \in \Re^+$ satisfying the kinematic equations

$$\text{N} \quad \ddot{\mathbf{r}}_i = - \sum_{j = 1,\ldots, n;\, j \neq i} (m_j/|\mathbf{r}_i - \mathbf{r}_j|^3) \times (\mathbf{r}_i - \mathbf{r}_j), \quad i = 1,\ldots,n,$$

or respectively,

$$\text{K} \quad \text{(a)} \; \ddot{\mathbf{r}}_i = -(k/|\mathbf{r}_i - \mathbf{r}_1|^3) \times (\mathbf{r}_i - \mathbf{r}_1), \quad i = 2,\ldots,n,$$
$$\text{(b)} \; \ddot{\mathbf{r}}_1 = 0,$$
$$\text{(c)} \; 1/2|\mathbf{r}_i - \mathbf{r}_1|^2 - k/|\mathbf{r}_i - \mathbf{r}_1| < 0, \quad i = 2,\ldots,n.$$

Here the $\mathbf{r}_i$'s are the postitions of the bodies with respect to an inertial reference frame (bold types denote vectors), the dot notation denotes time derivatives, and the m_j's are gravitational masses. I shall assume some familiarity with NTG and KLP. so it should be obvious that systems satisfying N are called *Newtonian systems*. It is less plain that systems satisfying K(a)-(c) are called *Keplerian systems*, since Kepler stated his laws in a very different manner and K (a), K (b) and K (c) do of course not at all correspond to Kepler's first, second and third laws respectively. Conditions (a) and (b) are easily interpreted only in the light of N, and (c) is a total energy condition that guarantees closed orbits (as opposed to open parabolic or hyperbolic orbits). In this section, we will accept Scheibe's move to present K (a)-(c) as the modern, Galileo invariant formulation of Kepler's laws. Note that the laws put in this way are not about "planetary motion" but about the motions in an arbitrary n-body system with the particle $i = 1$ distinguished as "sun". A kinematical state description is called *Newtonian* (*Keplerian*) if and only if there are appropriate constants m_i (k) such that the state description together with the constant(s) is a Newtonian (Keplerian) system.

Roughly, Scheibe's main claims, which are proven for $n = 2$ and conjectured for $n \geqslant 2$, are the following. Let $\varepsilon > 0$ be given. For any Keplerian state description then, there is a Newtonian state description deviating from the former "at most by ε", i.e., $|\mathbf{r}_i^N(t) - \mathbf{r}_i^K(t)| < \varepsilon$ for all $i = 1, ..., n$ and all $t \in \Re$, where $\mathbf{r}_i^N$ ($\mathbf{r}_i^K$) is the position function for the i-th body in the Newtonian (Keplerian) state description. Conversely, there is a "specialization" of NTG (i.e., NTG plus a set of additional assumptions) such that for any state description compatible with this specialization, there is a Keplerian state description deviating from the former at most by ε. The additional assumptions necessary to effect a suitable specialization consist in a counterpart to condition K(c) and a condition requiring the ratios m_i/m_1 (i = 2, ..., n) to be small, small enough at least to satisfy a certain inequality. We shall neglect the former requirement in what follows. Concerning the latter requirement, which implies that the masses m_i are (relatively) small as well, it should be noted that it is essentially dependent on ε.

Identifying approximative explanation with inclusion of blurred model sets, Scheibe concludes that, surprisingly, there is a direct approximative explanation of NTG by KLP, while the expected approximative explanation of KLP by NTG is only conditional, that is, must have recourse to additional premises. The intuitive superiority of NTG appears when the "ranges of application" [Scheibe, 1973b, p. 938] are considered: the specializations of NTG are still powerful enough to capture every Keplerian state description (with the accuracy ε allowed to be arbitrarily chosen), whereas KLP only cover a tiny fraction (the exact quantity is dependent on ε) of the Newtonian state descriptions. Scheibe [1973a, p. 117] therefore tentatively defines the *approximative explanation* of a theory T by a theory T' (up to degree ε) to consist of two parts: first the models of a specialization T'' of T' (or T' itself) must form a subset of the blurred (to degree ε) set of T-models, and second it must be possible to blur every T-model (to degree ε) in such a way that the blurred system is a model of T'' (and thus T' itself).

I want to touch on four more approaches to the Kepler-Newton case within the "approximation view" in this section: the reference is to Moulines [1980], Mayr [1981], Pearce and Rantala [1984], and Balzer, Moulines and Sneed [1987, section VII.3]. All of these papers are basically attempts to fit Scheibe's more informal account into a rigorous mathematical and metatheoretical framework, and as such they seem to be of minor importance for the point of this essay. But there are some interesting variations on Scheibe, and in order to get an up-to-date survey of the approximation approach to the Kepler-Newton case we must take cognizance of them anyway.

C. U. Moulines was the first to transfer Scheibe's analyses into a precise technical environment. He is an advocate of the Sneed-Stegmüllerian "structuralist view" of theories, and he chooses uniform structures, or simply uniformities, as his central topological concepts. While Scheibe set out from the idea of a deductive-nomological explanation à la Hempel, Moulines' central concern is to show that the

Kepler-Newton case fits in a pattern of intertheoretic approximation based on the Adams-Sneedian notion of reduction[4]. He shows as a theorem that three central results of Scheibe follow from the assumptions that (a) we have to deal only with two-body systems, (b) Newton's theory is correct, and (c) the Kepler-Newton case is an instance of "strict ρ^1_2-approximation" [see assumptions P1-P3 in Moulines, 1980, pp. 406-411]. This appears to lend good evidence in support of Moulines' claim that KLP and NTG in fact stand in a relation of strict ρ^1_2-approximation[5].

Unfortunately, there are a few inaccuracies that impair the presentation of Moulines' idea. As he takes over Scheibe's topological base for his analysis, it can quite easily be verified that the Kepler-Newton case is no case of *strict* approximation in the sense of Moulines[6]. Furthermore, his second restatement of Scheibe's results [1980, p. 407, (S2); cf. Scheibe 1973a, pp. 114-115] is not correct. We need not enter into details here, but the reason for Moulines' errors is interesting from the perspective of this paper: There is no single specialization of Newton's theory that works for any degree of accuracy ε. For an arbitrarily chosen ε, we can find additional conditions such that all systems satisfying NTG and these conditions are ε-close to a Kepler system. One of these conditions [cf. Scheibe 1973a, p. 115, (24)] which tampers with the ratio of the planetary and solar masses is essentially dependent on the externally given ε. For that reason the "specialization" of NTG effected by these conditions must be considered as an *ad hoc* theory.

D. Mayr's dense paper [1981] comes next in succession to the lines taken by Scheibe. Mayr's metatheoretical foundation is G. Ludwig's general philosophy of science with the intertheoretic relation of "blurred embedding", and the mathematical concepts he employs are (separating) uniform spaces and completions thereof. Again we need not inquire into the niceties of Mayr's approach. We shall only take notice of a correction of Scheibe's conjectures. Mayr gives an example illustrating that if we consider n-body systems with $n > 2$, then there are Keplerian models which cannot be approximated by Newtonian models to all eternity. It is reasonable therefore, and in accordance with the physicist's practice, to restrict oneself to *compact* intervals of time, i.e., to use the inequality $|\mathbf{r}^N_i(t) - \mathbf{r}^K_i(t)| < \varepsilon$ for all $i = 1,\ldots, n$ and all t in a compact interval $T \subseteq \mathfrak{R}$, as a base for the topology in the space of kinematical state descriptions. For any ε and any compact interval T, Mayr's central theorem states, every Keplerian state description of n

bodies can be approximated up to ε by a Newtonian state description *during T*. As n-body systems are extremely hard to calculate (where they are calculable at all), Mayr's proof is naturally non-constructive, by contrast to Scheibe's treatment of the 2-body case. The approximation is once again accomplished "by letting the masses become small" [Mayr 1981, p. 68].

Like Moulines and Mayr, D. Pearce and V. Rantala start with Scheibe's formulations of NTG and KLP. They have developed a general metatheory of their own, making use of abstract logic and category theory. The most general intertheory relation they introduce is the relation of "correspondence" composed of a structural correlation and a translation fitting together. A most important subspecies is limiting case correspondence, which is the pattern to be matched by NTG and KLP. The role of Moulines' and Mayr's topological instruments is taken over by non-standard analysis in the approach of Pearce and Rantala. Very roughly, their main results concerning NTG and KLP are the following: For any non-standard closed-orbit model x of NGT such that the "sun" has a *finite but non-infinitesimal* mass and the planets have *infinitesimal* masses, the standard part of x is a (standard) model of KLP. Conversely, they show it to be a consequence of Mayr's [1981] theorem that there is such a non-standard model x of NTG for every standard model of KLP[7]. These results, together with the existence of an appropriate translation describing the process of forming standard approximations, essentially guarantee that there is in fact a limiting case correspondence of KLP to NTG (relative to some infinitary logic) in the sense of Pearce and Rantala.

Up to now we have always had to face the difficulty that there is no single set of additional assumptions for NTG that can achieve the desired intertheory relation for any degree of accuracy. Pearce's and Rantala's account is no genuine exception. The use of non-standard analysis always replaces and eliminates the ε-δ-method from mathematics and has no substantial bearing on the particular problem of the Kepler-Newton case. The situation changes drastically when we have a look at the most recent proposal of W. Balzer, C. U. Moulines and J. D. Sneed. They, too, begin with Scheibe's formulations N and K[8]. But their presentation purports to have *one* exact specialization of NTG, a theory called "special gravitational classical particle mechanics" (GCPM*) [1987, pp. 378-379], that is appropriate to do the job for all ε's. Moreover, their reduction relation is extremely simple. It is restricted to two-body situations and relates Keplerian and Newtonian systems whose kinematical state descriptions are identical while the Kepler constant is equal to the mass of the "sun". There is no requirement on the mass of the "planet" in the Newtonian systems. Finally, on checking the proof of their central theorem [1987, pp. 379-381] we discover that they even seem to dispose of an *exact* reduction relation ρ, in the sense that the (unique) ρ-correlate of every NTG-model is already a model of KLP — without any blurs being necessary.

Alas, this is too good to be true. The authors' GCPM* is no specialization of NTG, and in fact their third requirement for GCPM* that the sun move in an inertial orbit is inconsistent with N — unless the planet has a zero mass (which is justly forbidden by the authors' definitions). It is an immediate consequence of the conservation of momentum (which in turn follows from Newton's third law) that the center of mass $(m_1\mathrm{r}_1 + m_2\mathrm{r}_2)/(m_1 + m_2)$, and not the sun, moves uniformly in a straight line in closed Newtonian two-body systems[9]. Hence no two-body system can be a model of both NTG and GCPM*. We conclude our review by saying that the contribution of Balzer, Moulines and Sneed is no advancement of the approximation view of the Kepler-Newton case.

Having seen the ways the Kepler-Newton relationship is treated by approximation theorists, we now are in a position to judge the merits and deficiencies of this point of view. I have three points to the effect that the approximation view is not wholly satisfactory and not wholly complete.

First, one of the most striking features was that we could not relate the exact formulations of NTG to KLP directly, nor effect an exact relationship by supplementing NTG with some unique set of reasonable additional conditions. Any set of supplementary assumptions for NTG was suitable only for a given degree of accuracy ε and a given time interval T, and in general we must accomodate the choice of this set to any new $\varepsilon' < \varepsilon$ or any new $T' \supseteq T$. Since neither ε nor T is an intrinsic feature of one of the theories KLP and NTG, the assumptions cannot be regarded as characterizing "natural" domains of application for KLP from the perspective of NTG.

Second, the approximationist's assumptions clearly take us away from truth. They all require the masses of the planets to be smaller and smaller (according to the values of ε and T). But it should always be kept in mind that KLP are about *our* solar system, and that NTG is, among other things, designed to explain, to reduce, or at least to replace, *these* laws about *this* solar system. Prior to both of these theories there are observations of the orbits of stars, planets, satellites etc. Tycho de Brahe's excellent records for instance were most relevant. Given these observations and NTG, however, it is evident that the masses of the planets *cannot be* arbitrarily small. Using the Newtonian interpretation of Kepler's third law (see section 4.1), it is easy to calculate the mass of the earth from the motion of the moon around it[10]. Even if such computations are not entirely exact, they certainly suffice to set definite lower bounds to the masses of the planets. The real world's boundary conditions render the assumptions which are requisite for the approximation processes counterfactual.

Third, and most significant, it is true that a kind of "correspondence principle" has been operative in the transition from Kepler to Newton: NTG in a sense really explains Kepler's laws, they can somehow be reduced to or embedded in NTG, they are, in some way, a limiting case of NTG. But it is of equal importance that NTG modifies KLP, corrects them, and thus contradicts them. To mention an example, one of the greatest triumphs of modern science would not have been possible without Newton's rectification of KLP: Adams' and Leverrier's prediction of the existence and position of Neptune from the perturbations of Uranus' path in 1846. It is desirable to put stress on the fact that Newton's theory of gravitation not only explains why Kepler's laws are nearly correct, but also why they do fail in the end. The approximation theorists would not seem to give due care to the latter question.

These are the three reasons for my attempt to supply an alternative account of the Kepler-Newton case by viewing it as an instance of idealization. Instead of making various additional assumptions, which are *both* dependent on externally given ε and T *and* counterfactual at the same time, I shall face up to, and indeed welcome, counterfactuality alone. My aim is to show that *one* natural idealizing assumption will suffice to accomplish a *strict* relationship between KLP and NTG. This assumption, however, will turn out to be different from the one suggested by the approximation view, viz., the assumption that the masses of the planets were zero. By contrasting the counterfactual conditions necessary to get KLP as a Newtonian idealization with the factual circumstances in the solar system, we shall be able to explain the evident significance as well as the final failure of Kepler's laws.

3. *Idealization and double explanation*

Before laying abstract foundations, let us briefly restate the problems posed by the Kepler-Newton case. NTG somehow explains KLP. But, as we all know, NTG also gives an explanation why KLP are false. To put it differently, NTG is not only a *conservative* or *good* successor theory to KLP but, what is more, also a *progressive* or *superior* successor theory of KLP. Still another way to express this state of affairs is to say that the transition from Kepler to Newton exemplifies the crucial roles of continuity as well as contradiction in science[11].

Returning to the first formulation, the question of course is: What concept of explanation can fulfill the double-edged task set by the Kepler-Newton case? I get my answer from two happily conspiring sources. On the one hand, Clark Glymour [1970] argues convincingly in favour of his following thesis:

> Inter-theoretical explanation is an exercise in the presentation of counterfactuals. One does not explain one theory from another by showing why the first is true; a theory is explained by showing under what conditions it *would be* true, and by contrasting those conditions with the conditions which actually obtain [Glymour 1970, p. 341].

On the other hand, it can be argued that 'because'-sentences are paradigm formulations of explanations. It has often been noticed that the counterfactual 'if' and the factual 'because' have closely related meanings. Rott [1986] offers an exact analysis of both connectives which is based on Peter Gärdenfors' fertile model of the dynamics of belief or "theory change"[12].

The phrase 'something is explained by a given theory *T*' is mostly understood in the sense that it is possible to specify additional or applicability conditions *A* (in the language of *T*) such that *T* and these conditions taken together entail the explanandum. To that I agree. The pivotal point of my explication is that I do not consider it advisable to put any restrictions concerning truth values on the applicability conditions. The term 'theory' is understood in a comprehensive sense so as to include also knowledge about the initial or boundary conditions that describe the circumstances obtaining in the real world. Three cases can then be distinguished. First, the applicability conditions *A* may be known to be true (realistic), or second, they may be known to be false (idealizing) from the standpoint of *T*. In the former case I shall say that

T provides a *real* explanation, in the latter case the explanation will be called *idealizational*. The third possibility arises when it is not known in T whether A is true, or when A is known to be true on some occasions but false on others. Here we can speak of a *conditional* explanation[13].

Putting together the pieces introduced up to now and formulating real, idealizational and conditional explanations with the help of 'because', subjunctive 'if' and indicative 'if' respectively, we find that there are three conceivable ways to express the fact that T_2 is a *superior successor theory* for T_1:

Either

(R) 'Because A is true, T_1 is correct;
but if A were not true, T_1 would not be correct.'

or

(I) 'If A were true, T_1 would be correct;
but because A is not true, T_1 is not correct.'

or

(C) 'If A is true, T_1 will be correct;
but if A is not true, T_1 will not be correct.'

is acceptable from the standpoint of T_2, where A stands for the applicability conditions of T_1. (The halves before the semicolons are the requirements for *good* successor theories). Note that 'correct' here is intended to mean *strictly* correct, not only approximately correct. As a consequence of the analyses in Rott [1986], we get the following

Theorem: T_2 is a superior successor theory for T_1 if and only if there are applicability conditions A for T_1 (as viewed from T_2) such that the following four conditions are simultaneously satisfied:

the minimal revision of T_2 needed to incorporate $\left\{\begin{array}{l} A \\ T_1 \\ non\text{-}A \\ non\text{-}T_1 \end{array}\right\}$ into T_2

$$\text{includes} \left\{ \begin{array}{l} T_1 \\ A \\ non\text{-}T_1 \\ non\text{-}A \end{array} \right\}^{14}.$$

I do not want to engage in a discussion of requirements on minimal revisions; for this see [Gärdenfors, 1988]. Only one restriction will be imposed in the sequel: certainly we cannot speak of a "minimal revision" of T_2 at all if the assumptions or theories we are to incorporate are at variance with the most fundamental axioms of T_2. Thus T_2 can only be a superior successor theory for T_1 if neither A nor T_1 nor *non-A* nor *non-*T_1 overthrow the fundamental axioms of T_2.

It should be beyond dispute that intuitively NTG is a superior successor theory for Kepler's laws. And it is equally obvious from the previous discussion that NTG can strictly explain KLP at most by way of an idealization, while what it explains in fact is the failure of KLP. So assuming the alternatives (R), (I), and (C) above, (I) is the relevant option. It is convenient, however, to refer to the conditions stated in the theorem in what follows. Our first duty will be to find the right applicability condition A for the Kepler-Newton case. The "real parts" (the parts concerning *non-A* and *non*-KLP) then are trivial: already Newton knew that Kepler's laws must be wrong and that the applicability conditions for them are not met by the solar system. So we need not revise NTG at all to have *non-A* and *non*-KLP incorporated. The "idealizational parts" (the parts concerning A and KLP) are interesting and are in fact dealt with in Newton's *Principia* as well as in modern textbooks. Let us now return to the case study and try to show that the Kepler-Newton relationship really is an instance of idealization and double explanation.

4. The Kepler-Newton case as an instance of idealization

There are some principal respects in which my approach to the Kepler-Newton case differs from the approximation theorists' analysis. First of all, starting with Scheibe's formulation K of Kepler's law runs the danger of passing over the most instructive aspects of the relation between KLP and NTG. It does not require too much goodwill to give credence to the claim that the solutions of K(a)-(b) will for some time come close to those of N if m_1 is very close to k and the m_i's are very close to zero for $i = 2, ..., n$. (Note that $|\mathbf{r}_i - \mathbf{r}_j|$ is bounded for closed orbits). However, it takes a good deal of mathematical work and is a considerable cognitive achievement to recognize that K(a)-(c) are in fact equivalent to the original formulation of Kepler's laws. The most interesting part of the explanation of KLP (and their failure) afforded by NTG may as well be supposed to lie in this step. So let us repeat KLP in their usual form for a beginning:

K1 The planets move in elliptical orbits, with the sun in one focus of the ellipse.
K2 The radius vector from the sun to a planet sweeps out equal areas in equal times.
K3 The ratio of the square of the period to the cube of the semimajor axis of the ellipse (i.e., to the cube of "the mean distance" from the sun[15]) is the same for all planets.

It is not advisable to weld these laws together into a single block, for K3 will turn out to be "more idealized" than K1 and K2[16].

The approximation view seems to indicate a plain strategy for the idealization analysis. Approximate validity of KLP can be obtained by "letting the masses of the planets go to zero". This, we noted, means straying from truth. So *prima facie* it does not seem worse to simply put the planetary masses to zero. One could argue that the idealizing condition

$A_0 \quad m_i = 0 \text{ for } i = 2,\dots,n$

is just an extrapolation and full acknowledgement of the counterfactuality involved in the approximation process. This move would have the advantage of replacing the various ε-dependent assumptions of the approximation view by a single idealizational assumption that strictly reduces N to K(a)-(b) (provided that m_1 is chosen identical with k). But of course this is illegitimate! There is no sense to be made of planets, or of any other particles whatsoever, with a zero mass in the context of Newton's central axioms. Gravitation cannot explain the paths of massless particles, since according to the distinctive law of NTG such particles are not attracted by anything. Still worse, Newton's second law of motion, which is presupposed by NTG, cannot be adapted for application to massless "bodies": no force of any kind could be thought of as acting on such "bodies" (at least if we exclude infinite accelerations). It is evident then that the idealizing condition A_0 is irreconcilable with the very fundamentals of NTG[17]. Hence no minimal revision of NTG with a view to assimilating A_0 is possible, hence A_0 is no serious candidate for establishing NTG as a superior successor theory of KLP.

We have to look for new strategies from other sources than the approximation view. The material from which we can get the right hints is not hard to come by: it can be found in Newton's *Principia* and in modern elementary textbooks like the *Berkeley Physics Course* [Kittel et al., 1973]. We shall discriminate two "grades of idealization". The first idealization involves Kepler's laws proper, the second, "more realistic" idealization involves K1, K2 and a corrected version of Kepler's third law. In both cases we shall briefly discuss the two directions the idealization must take: from KLP to their applicability conditions in NTG, and conversely from these applicability conditions to KLP. Recall that the theorem of section 3 prescribes that we must go both ways if Newton's theory is to be considered a superior successor theory of Kepler's laws according to our definitions.

4.1. First idealization: one-body systems. The first attempt to justify KLP from the standpoint of NTG by means of an idealization leaves KLP totally unaffected. Indeed, it starts from the very laws of Kepler.

4.1.1. From Kepler's laws to their Newtonian applicability condition: one-body systems. Kepler's laws – interpreted as laws of the motion of "bodies" – figure most importantly right in the first sections of Newton's *Principia*. Roughly, propositions I-III in book 1 prove that a planet's motion in a plane according to the area law K2 (with the sun being the point of reference) is equivalent to the existence of a centripetal force (with the sun being the unaccelerated point of reference). On this basis, proposition XI shows that the law of elliptic orbits K1 implies that the central force varies inversely as the square of the distance from the sun, but this only for (the orbit of) each planet separately. Corollary VI of proposition IV, dealing with K3 in the special case of uniform circular motion, demonstrates a universal inverse-square force to be at work[18]. A modern account of the implication of a centripetal inverse-square force by K1-K3 is given in Born [1949, pp. 129-133], where in particular it is shown how K3 as applied to ellipses removes the dependence of the inverse-square law on special features (e.g. in Born, area constant and semi-latus rectum) of a planet's elliptical path.

In the first idealization therefore, the applicability condition for KLP in NTG views planets as special kinds of *one-body systems*:

A_1 Planets are single bodies moving under the action of a centripetal inverse-square gravitatational force, the center of which is in an unaccelerated motion.

Note that A_1 puts no restrictions on the mass of the planet under consideration — save that it be non-zero.

We have seen that it is of course not, as often claimed, Newton's theory which can be derived from KLP. But *within* NTG, with its concept of force available, KLP imply A_1 in the sense that *if* the planets really moved in conformity with Kepler's laws, then they *would be* objects in one-body systems as described by A_1. We have just revealed how NTG, to use Popper's words [1957, p. 33], "would have to be adjusted — what false premisses would have to be adopted, or what conditions stipulated" in order to make KLP true.

4.1.2. From one-body systems to Kepler's laws. Assume now that planets are moving under the undisturbed action of a centripetal inverse-square force

$$[1]\ \mathbf{F} = m\ddot{\mathbf{r}} = -(GMm/|\mathbf{r}|^2) \times \hat{\mathbf{r}}.$$

Here $\mathbf{r}$ denotes the position of the planet with respect to the center of force, $\hat{\mathbf{r}} = \mathbf{r}/|\mathbf{r}|$ is the unit vector in the direction of $\mathbf{r}$, and G is the constant of gravitation; m designates the planetary mass, and M represents the "solar mass". But as assumption [1] is to formalize the one-body system condition A_1, it should be borne in mind that M is not intended to be the real mass of a real body here, but a mere constant (let us call it "quasi-mass") of the solar system.

It is a subject of most introductory textbooks on mechanics that the paths of particles[19] moving under the sole action of such a centripetal inverse-square force are conic sections. The radius vector drawn from the center of force to the particle sweeps out, as is readily shown, equal areas in equal time intervals for centripetal forces of arbitrary magnitude. Finally, Kepler's third law is found to hold, with the ratio mentioned in K3 coming out as $4\pi^2/GM$ [for all this, see Kittel et al., 1973, pp. 279-288].

Hence, *if* planets *were* as described in A_1, then they *would be* perfectly Keplerian bodies. It has now been ascertained that the last of the four conditions of the theorem is satisfied, and hence the second clause of double explanation according to (I) may be added: *because* planets are *not* as described in A_1, Kepler's laws are *not* true.

4.2. Second idealization: two-body systems. Like every idealization, A_1 is counterfactual. This was clearly seen by Newton who opens section XI of book 1 of the *Principia* with the following sentences:

> I have hitherto been treating of the attractions of bodies towards an immovable centre; though very probably there is no such thing existent in nature. For attractions are made towards bodies, and the actions of the bodies attracted and attracting are always reciprocal and equal, by Law III; so that if there are two bodies, neither the attracted nor the attracting body is truly at rest, but both (by Cor. IV of the Laws of Motion), being as it were mutually attracted, revolve about a common centre of gravity [1934, p. 164].

Newton indicates that the whole enterprise of his first ten sections must perhaps be regarded as conflicting with the third of his fundamental axioms (action = reaction). Indeed, the price we had to pay for saving Kepler's laws in the last section has been very high. Condition A_1 causes us some embarrassment. We have to deny not only the existence of the other planets (interplanetary interactions would perturb the centripetal force) but also the existence of the sun, because the sun as a massive body would have to be attracted by the planet under consideration. (Notice we are not allowed to strengthen A_1 by adding that the sun *is* located at the center of the force, which is another consequence of K1-K3). Thus A_1 calls for an answer to the question what the central gravitational field originates from. Then again, how are we to interpret the quasi-mass M in the Newtonian formula [1] for the gravitational force? I do not know of any reasonable answers to these queries, and I wonder whether there really are "minimal revisions" of NTG suitable to assimilate A_1. Hence, it is very doubtful whether Kepler's laws K1-K3 can be accounted for in NTG even as an idealization.

Does this mean a defeat to the idealization analysis? No. In my submission, the idealization shows quite precisely that a defeat is suffered by Kepler's original set of laws. Their approximate validity and their being approximately explained by NTG notwithstanding, KLP are suggested by NTG to be in need of modification, even though we already allowed a revision of NTG by idealizing, contrary-to-fact assumptions. The argument runs as follows:

Intuitively, Newton's theory is beyond doubt a superior successor theory for Kepler's laws, so we should be able to mirror this on a more formal level. In section 4.1 we attempted to verify that NTG is a superior successor theory for KLP in the sence of section 3. We found out that A_1 is *the* applicability condition suitable for use in the criterion provided by the theorem, i.e. that A_1 is *the* right applicability condition of KLP within the Newtonian framework. But A_1 exhibits a distinctive air of incompatibility with Newton's "Law III", and it is forbidden to add assumptions to NTG which are at variance with its most fundamental axioms. It thus appears that we have disqualified K1-K3 from operating as an idealized predecessor of NTG, or – to put it the other way round – disqualified NTG from operating as a superior successor theory for K1-K3. So it is not KLP proper that fits the scheme of double explanation by NTG. Fortunately a slight modification of KLP will do. And this modification will ensue as a matter of course if we follow Newton's own line of reasoning. Let us now consider two-body systems.

4.2.1. From two-body systems to Kepler's laws modified. In a second, alternative idealization, the applicability condition for KLP in NTG views planets as parts of *two-body systems*:

A_2 Planets are single bodies revolving about the sun.

Let m and M be the Newtonian masses of the planet and the sun respectively. It is well-known that the problem of two-body motion can mathematically be reduced to the following equation [see Kittel et al., 1973, pp. 289-292]:

[2] $\mu\ddot{\mathbf{r}} = -(GMm/|\mathbf{r}|^2) \times \hat{\mathbf{r}}.$

Here r denotes the position of the planet with respect to the sun, and $\mu = Mm/(M + m)$ is known as the *reduced mass* in celestial mechanics.

A simple transformation makes equation [2] exactly look like the one-body equation [1]:

[2'] $\mu \ddot{\mathbf{r}} = -(G[M + m]\mu/|\mathbf{r}|^2) \times \hat{\mathbf{r}}$.

Thus a planet's motion can be treated *as though* it had the reduced mass μ and moved in a centripetal inverse-square field, the center of force being located in the actual center of the sun and the quasi-mass of the system being $M + m$. But note that this is a genuine *as though* construction because, as was already mentioned in section 2, by force of NTG the sun *cannot be* the origin of an inertial reference frame in two-body systems (but revolves around the common center of gravity) and because its mass is not $M + m$ (but M). There is no true force defined by the left hand of [2'].

Equation [2'] nevertheless is most helpful in investigating the validity of Kepler's laws. Thanks to its structural identity with [1] and thanks to the fact that **r** indicates the position relative to the sun, it is clear that K1 and K2 *strictly hold*: in two-body systems, planets do move in ellipses around the sun with the sun in one focus, and the radius vectors do cover equal areas in equal times. In contradistinction to these laws, in which shape is relevant and size is not, the harmonic law K3 is no longer accurate. On substituting $M + m$ for M, and μ for m, in the argument in [Kittel et al., 1973, pp. 287-288], we have that the ratio mentioned in K3 is equal to $4\pi^2/G(M + m)$. We must thus modify K3 into

K3* The ratio of the square of the period to the cube of the semimajor axis of the ellipse (to the cube of "the mean distance" from the sun) varies inversely as the sum of the masses of the sun and the planet.

This law is often presented as "Kepler's third law"[20]. Let us call the conjunction of K1, K2 and K3* KPL*. It is this variation on Kepler's laws that is the right "predecessor theory" for the superior theory of Newton. Notice that on the idealizing assumption A_2 there is no need for making any approximations to get KLP*. But the idealizational laws KLP* show exactly why Scheibe's [1973a, pp. 114-115] approximations (which only work for two-body systems) are necessary: in order to correct K3. And here is a precise and proper place where approximation enters in the Kepler-Newton case: K3 is approximately valid, because the sums $M + m$ mentioned in K3* have approximately the same value for all planets.

4.2.2. From Kepler's laws modified to their Newtonian applicability condition: two-body systems imitated. There is still one point to deal with if NTG is to be a superior successor theory for KLP*. Suppose that our solar system obeyed Kepler's modified laws KLP*[21]. Note that this supposition rests on the background of NTG. K3* cannot be formulated without the Newtonian concept of mass[22]. Can we get from KLP* to their Newtonian applicability condition A_2, and if so, how? In order to find an answer to this question, we can take over the chief part of Born's way from KLP to A_1. Only the last portion of his proof [Born, 1949, p. 132] is affected by the substitution of K3* for K3. And even this is easily accomodated if we replace M by $M + m$. The result is that KLP* imply

$$[2''] \quad \ddot{\mathbf{r}} = (G[M + m]/|\mathbf{r}|^2) \times \hat{\mathbf{r}},$$

which is just equation [2'] with μ cancelled on both sides.

What does this result show? It shows that according to KLP*, each planet moves *as though* it revolved in a central inverse-square field, the center of which coincides with the center of the sun and the quasi-mass of which equals $M + m$. But this must not be taken literally, since the sun *is* in the center but its mass is unique and equals M. So a literal interpretation of [2''] in terms of (disembodied) central forces would risk running counter Newton's fundamental axioms even more than the construction in section 4.1. did.

We learned in the last subsection that planets in all two-body systems in which the masses of sun and planet add up to $M + m$ obey [2'']. But even if we take the pertinent masses as given, we may not counterfactually jump from [2''] to A_2, since apparently there is no way to derive the number of bodies involved from a bare differential equation. It therefore seems that we must weaken A_2 by admitting kinematically equivalent systems[23]:

A'_2 Planets move like single bodies revolving about the sun.

This is, I grant, not a pleasant result. It is ugly to have a 'like' phrase inserted in the already idealizing, i.e., counterfactual applicability condition A_2. As long as we are not able to demonstrate that [2"] can only apply to two-body systems, however, I see no way to get round this move. After all, it is not pernicious, since the derivation of KLP* within NTG is not disturbed if A_2' stands in the place of A_2. And plainly the purely kinematical assumption A_2' does no harm to the fundamental axioms of Newton's theory of gravitation.

Thus we have found an efficient condition which shows the significance of Kepler's laws from the standpoint of NTG. In other words and more precisely, it shows that NTG is a superior successor theory for the emended version KLP* in the sense that the minimal (counterfactual!) revision of NTG needed to incorporate A_2' includes KLP* (section 4.2.1.) und conversely the minimal (counterfactual!) revision of NTG needed to incorporate KLP* includes A_2' (section 4.2.2). Equivalently, I claim that the proponents of NTG subscribe to the idealizing explanation of KLP* in the sense of (I): *If* planets *moved* like single bodies revolving about the sun, KLP* would be entirely accurate; but *because* they *don't*, KLP* in fact fail.

5. *Conclusion*

The present paper has shown, I believe, that idealizations are different from, complementary to, and perhaps preferable to approximations for the purpose of analyzing the Kepler-Newton case. They are different, because giant planets would disable KLP as approximations but not as idealizations, while Cartesian physics would probably obstruct KLP as idealizations but not as approximations. They are complementary in the sense that the approximation view considers quantitative relations between models satisfying kinematic differential equations like N and K, whereas the idealization analysis is qualitative and concentrates on how such equations are reached from Kepler's laws and how they can be interpreted in NTG. Let me now summarize why I consider the idealization view to be more instructive than the approximation view.

What are the virtues of Kepler's laws? It cannot be the point that they are approximations to the truth. They were not empirically established beyond doubt, nor were they unrivalled in the mid-seventeenth century. Cassini's ovals were serious competitors of Kepler's ellipses. Assuming ellipses, on the other hand, one gets quite as good results from the "simple elliptic hypothesis" (taking the planets to move at constant angular velocity with respect to the empty focus) as from

Kepler's area law. Finally, Newton urged Flamsteed in 1684 to look at the aberration of Saturn's path from Kepler's sesquialterate proportion. Historians of science are quite ready to concede the insignificance of KLP as "empirical premises" for Newton [see Wilson 1969/70 and Baigrie 1987].

The point is taken better if one dwells on the fact that KLP are approximations to NTG. But as I argued at the end of section 2, the approximation process gratuitously requires auxiliary assumptions which both depend on degrees of accuracy (and on time intervals, if $n > 2$) and depart from the truth at the same time, and it leaves open the question *why* Kepler's laws were found wanting at last.

Idealization, in the sense employed here, seems to improve upon this. Approximations cannot be obtained without counterfactual assumptions, but one can specify counterfactuals which dispense with the necessity of approximations. In section 4 I took idealizing, or counterfactual, assumptions seriously. It has strongly been suggested that Kepler's original laws of planetary motion cannot be justified from the perspective of NTG, for the existence of one-body systems is hardly conceivable in the latter theory. On this account we had to modify slightly KLP, arriving at their "theoretical concretization" [the term is due to Kuipers 1985, p. 186][24] KLP*. We have shown that KLP* would be *exactly* right, *if* our planets *were* – or rather *moved like* – bodies forming two-body systems with the sun. The idealization analysis pointed out a culprit – K3 must be replaced by K3* – and at the same time made clear that the "wrong" third law approximates the "right" one in a very natural and straightforward way.

I have tried to show by means of counterfactuals how NTG can be perceived to be a superior successor theory for KLP*, that is, how it can explain both KLP* and its final failure. This agrees well with the analyses of the distinguished historian of science and Newton scholar I. Bernard Cohen: "Newton's 'theory' first displays the hypothetical circumstances under which ... Kepler's laws are valid, and then shows the modified form in which all three of these laws occur in Newtonian dynamics" [1974, p. 319]. Double explanation as introduced in section 3 contrasts idealizing with actual boundary conditions. As Cohen puts it: "In this 'real world', as opposed to the hypothesized world presented in the earlier sections of the *Principia*, Kepler's laws will no longer hold exactly" [1974, p. 315][25]. The essential point of the Kepler-Newton case is the opposition of hypothetical truth and actual falsity, not the opposition of approximate and strict truth. I do not wish to deny the heuristic or practical value of the fact that KLP come near to both the empirical truth and NTG, but in so far as we are concerned with theoretical explanation and abstract intertheory relations, this becomes a secondary phenomenon.

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NOTES

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1Notably M. Scriven, D. Shapere, F. Suppe, and R. Laymon. Nancy Cartwright is famous for her doctrine that the fundamental laws of physics, for instance Newton's law of universal gravitation and Coulomb's law, "are not true; worse, they are not even approximately true" [1983, p. 57].

2Incompatible, that is, when conjoined with one element of the overwhelming majority of all conceivable initial conditions, and certainly when conjoined with the initial conditions pertaining to our solar system. Without this precaution, Scheibe [1973a; 1973b] is right to reject the Duhemian incompatibility thesis with respect to KLP and NTG.

3Since scientists do not possess "the truth" but at most better and better theories, it is perhaps more rewarding to study this intertheoretic versions than the notions of approximation and idealization *simpliciter*.

4This notion has not gone unchallenged even within the structuralist school of philosophy of science. For an overview of the criticisms of Adams-Sneedian reduction, as well as for its relation to deductive-nomological explanation see [Rott, 1987a]. Cf. also footnote 9 of that paper, where it is pointed out that Moulines' [1980, pp. 400-403] approximation is, in a sense, just the reverse of this reduction relation.

5Moulines' claim does of course not *follow* – as he contends [1980, p. 411] – from the theorem mentioned.

6This claim presupposes the concept of strict approximation as used by Moulines in the proof of his Theorem 2 [1980, p. 410, lines 1-3]. His definition of strict approximation (p. 403), which is highly condensed and extremely difficult to unravel, is perhaps not quite equivalent.

7Pearce and Rantala forgot to write down Mayr's compactness condition for the time intervals.

8The authors claim [1987, pp. 375-376] that K is only a representation of Kepler's first two laws. This is not correct. Kepler's third law is essential in the derivation of K, and conversely it is entailed by K. See section 4.

9See [Kittel et. al., 1973, pp. 82-84, 175-176].

[10]This claim naturally needs some qualifications. First, it is necessary to know the value of the constant of gravitation, which can also be drawn from the motions of heavenly bodies, for example the sun, the earth and the moon [see Stumpff 1973, §18]. Second, it is an effect of Newton's correction of Kepler's third law (see section 4.2) that actually only the sum of the masses of the earth and the moon can directly be obtained, but the earth's mass can be determined by considering the orbits of the earth-moon system around the sun. Third, all these calculations of course abstract from any "perturbation" by other celestial bodies.

[11]This section is an adaptation of Rott [1987b], where a slightly more detailed and slightly more formal attempt to reconcile continuity with incompatibility in scientific change can be found.

[12]An authoritative and topical treatise on this subject is Gärdenfors [1988].

[13]In Rott [1987b], this trichotomy is called factual, counterfactual, and potential explanation.

[14]The meanings of the abbreviations *non*-A and *non*-T_1 should be evident. For limitations of space it is not possible to give the proof of the theorem here. More details about it can be found in [Rott, 1987b, section 4].

[15]It is customary but somewhat artificial to identify a planet's mean distance from the sun with the semimajor axis of its elliptical path. For a more discriminating treatment of that matter, see [Stumpff, 1973, §25].

[16]As Felix Mühlhölzer has pointed out to me, the principal difference between Kepler and Newton may perhaps be viewed to consist in the fact that Kepler thought the sun to be at rest, while Newton regarded it as moving. Kepler's *laws*, however, make no claim about the absolute motion of the sun. We now see, by the way, that K(b) is an addition to KLP which is Newtonian rather than Keplerian in spirit.

[17]This has already been stressed in Popper [1957] and is without doubt clearly seen by the approximation theorists.

[18]I could not find a derivation of the universal inverse-square force from K-3 *as applied to ellipses* in Newton's *Principia*. (The converse implication is stated in proposition XV). This may be due to the fact that Newton considered the inverse-square law to be better established by the quiescence of the planetary aphelion points. Cf. his comment on the proof of proposition II of book 3.

[19]Newton took considerable pains to show that it makes no difference – and thus is a harmless idealization – if planets are treated as particles instead of solid sphers [cf. Newton, 1934, section XII of book 1; Kittel et al., 1973, pp. 271-275].

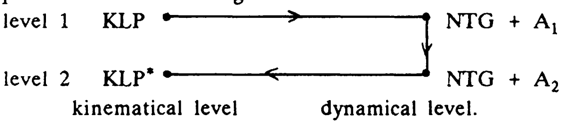
[20]In the first edition of the *Berkeley physics course*, it is $K3^*$ which is proven from the premises of the two-body model, and this adjustment of KLP is not even commented on. Kepler's laws are discussed in the context of one-body systems only in the second edition [Kittel et al., 1973, pp. 287-288].

[21]Newton knew that this is contrary-to-fact. See for instance his comment on proposition XIII of book 3 of the *Principia*.

[22]In [Rott, 1987b] it is supposed that, intuitively, predecessor theories are always 'theoretical' and applicability conditions are always 'non-theoretical'. It is interesting that this assumption seems to be confirmed by the second idealization, while reversed by the first idealization.

[23]'Kinematically equivalent' is intended to mean identical spatiotemporal state descriptions of corresponding particles (relative to suitable reference frames), while any constants, masses and forces in the systems may differ. For example, every two-body system with masses *M'* and *m'* is kinematically equivalent to the two-body system with masses *M* and *m* if $M' + m' = M + M$ (as [2'] bears witness).

[24]It means that the transition from KLP to KLP* proceeds on the basis of theoretical background considerations in NTG, not on the basis of empirical evidence. We can put this in a scheme which is parallel to Kuipers's suggestive scheme for the much more complex case of the ideal gas law and the law of van der Waals:



[25]Also see the last paragraph of Cohen's footnote 30. Incidentally, Cohen even maintains that "the presentation in the *Principia* more or less follows the line of discovery" [1974, p. 332].

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