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G. KREISEL

A NOTION OF MECHANISTIC THEORY

I. INTRODUCTION

The notion in question is suggested by the words 'mechanism' or 'machine'. Unlike the usual meaning of 'mechanistic', that is, *deterministic* in contrast to *probabilistic*, the notion here considered distinguishes *among* deterministic (and among probabilistic) theories.

The general idea is this. We consider theories, by which we mean such things as classical or quantum mechanics, and ask *if every sequence of natural numbers or every real number which is well defined* (observable) **according to the theory** *must be recursive* or, more generally, *recursive in the data* (which, according to the theory, determine the observations considered). Equivalently, we may ask whether any such sequence of numbers, etc., can also be generated by an ideal *computing* or *Turing machine* if the data are used as input. (This formulation explains our terminology 'mechanistic'.) The question is certainly not empty because most objects considered in a (physical) theory are not computers in the sense defined by Turing; in fact, so-called analogue computers are not Turing machines; at best their behavior may be simulated by Turing machines. They will be, according to theory, if the particular theory of the behavior of the analogue computers considered happens to be mechanistic in the sense described above.

The stress on the proviso 'according to theory' in the preceding paragraph is intended as a warning: We are here primarily interested in a distinction between classes of theories, not classes of phenomena. The reader should not allow himself to be confused at this stage by doubts about the validity of a theory with regard to the phenomena for which it is intended. Naturally such doubts imply doubts about the relevance (to those phenomena) of any results about the mechanistic character of the theory. It remains to be seen whether the notion of *mechanistic theory*, that is, the division into mechanistic and nonmechanistic theories, will be useful for such physical theories as classical

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or quantum mechanics. It would be a bit odd if it were not since the distinction is useful in such mathematical theories as geometry and topology which, after all, are also theoretical idealizations.

Over the last decade, I have published scattered remarks concerning the question whether existing (physical) theories are mechanistic, with special emphasis on specific familiar problems which are reasonable candidates for counterexamples. The most recent publication is Kreisel (1972) where back references are to be found in the third paragraph on p. 321.[1] The results are not conclusive, except for showing convincingly that the subject lends itself to precise formulations. For what it is worth: I still have the impression that some (of the unsolved) problems of current physical theories have nonmechanistic solutions.

The purpose of the present note is to discuss a quite specific aspect of the *extension of theoretical knowledge*, which is liable to introduce nonmechanistic elements in a perhaps not altogether trivial way. One of the most striking features of the whole business of (fundamental) extensions of the sphere of theory consists in this: *constants are calculated theoretically which were previously obtained by 'empirical'* usually *approximate measurement*. In particular – and this case seems most relevant for our purposes – according to theory, some quantities may have to be *integral* (multiples); in this case quite rough measurements are sufficient to fix a precise 'quantized' value. In this situation, a famous principle, due to Hadamard, which restricts the class of meaningful theories, loses much of its (restrictive) force. The principle requires that theoretical relations corresponding to *functions*, mapping data to (other) observables, must be *continuous* in the data; this condition becomes empty if the theoretically permissible values for the data are 'quantized' (discrete) since then every function is continuous.

Superficially the kind of quantization mentioned would seem to be most relevant in connection with the quantum theory. But I do not know enough about the subject to judge with confidence the significance of (mathematical) examples. So I have used examples from geometry and celestial mechanics to *illustrate the state of affairs* described in the last paragraph. This is done in Section IV. The examples use the *Background Information* in Section II, but not the discussion of Section III which is intended to remove some sources of *malaise* (concerning the notion of mechanistic, or rather of nonmechanistic theories) which I have found

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in the literature and in conversations with logicians. The (pedagogically) most important part of Section III concerns our experience with theories of (a) familiar mathematical objects as conceived in so-called classical mathematics and of (b) less familiar abstract constructions considered in so-called intuitionistic mathematics. There is a good deal of literature on the question whether those theories are mechanistic: we point out a number of *systematic errors* which slowed up progress.

II. BACKGROUND INFORMATION

We need a few facts about recursive functions of natural numbers, recursive real numbers, and recursive functions of (not necessarily recursive) real numbers. (a) Most modern texts on mathematical logic contain enough information for our purposes about recursive functions of natural numbers. A reader who has not met the notion should replace 'recursive' by – what he imagines to be – *systematically computable.* (b) Recursive real numbers will be approximated by recursive sequences of rational numbers p_n/q_n or equivalently of pairs (p_n, q_n) of natural numbers. The principal point to remember here is that the class of recursive functions is sufficiently flexible to satisfy the following condition, for any of the familiar styles of approximation (by general Cauchy sequences, decimal or binary expansions, etc.): If everything in sight is restricted to be recursive and if a real number ξ is recursive for one style of approximation, ξ is also recursive for the other styles. For example, if ξ has a recursive approximation p_n/q_n with a *recursive* modulus of convergence, that is, a recursive v such that, for all m and n,

$$n > v(m) \rightarrow |p_n/q_n - p_{v(m)}/q_{v(m)}| < m^{-1},$$

(where m^{-1} replaces the usual ε) then ξ also has a recursive binary expansion. This part of the subject is also easily accessible.

(c) A little care is needed in connection with recursive functions of real numbers, for two reasons. First of all, the equivalences mentioned in (b) are simply *discontinuous*: there is no $\bar{n}$ such that for all ξ and all its Cauchy expansions (equipped with a modulus of convergence) the nth binary digit of ξ is determined by the first $\bar{n}$ terms of the expansions; neither if we decide to use the expansion of $p2^{-q}$ with a tail of 0's nor if we use the *different*, but 'equivalent' expansion with a tail of 1's. Sec-

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ond, some functional equations can be solved by continuous functions (defined on the representations) which do *not* preserve *equivalence*, but cannot be solved by functions which do. The standard example, familiar from the theory of separating roots of polynomials, is provided by

(*) $\qquad x^3 - 3x = c.$

For each real number c there is an x which satisfies (*), and for each recursive c there is a recursive x (in fact, for recursive c, all x which satisfy (*) are recursive). But there can be no $\xi : c \mapsto x$ which is continuous for the topology usually associated with the real numbers. For when $c=0$, (*) has three solutions $(-\sqrt{3}, 0, \sqrt{3})$; if $|c|$ is large and c is negative, there is only one solution, say $\xi^-(c)$ and $\xi^-(c) \to -\sqrt{3}$ as $c \to 0$, while for the corresponding ξ^+, $\xi^+(c) \to +\sqrt{3}$ as $c \to 0$. However, it is clear how to define ξ if c is given by binary expansions, say ξ_b, making use of the fact that if $c = p/2^q$, then c has two binary expansions, for which ξ_b may take different values; such a ξ_b will still be continuous In the binary approximations.[2] – Other defects of the usual topology for computational purposes arise with Yes-No questions (or, generally, functions with values in a discrete space); cf. III.2(b) and IV.2 below.

Pedagogic remark. The reader is recommended to look carefully at the example above and to make up some other examples for himself (e.g., concerning the *in*adequacy of binary expansions for the *addition* of real numbers). He should not allow himself to get paranoid about (needing) 'general definitions' or even general 'criteria' to decide which representations are to be used in different situations. He should take it on trust that a *few* representations, that is, styles of approximations, will be sufficient in *most* applications (and that in any case the most interesting problems will no doubt require representations specially adapted to the particular problem, as illustrated by the example at the end of IV.1(a) below). Where the literature referred to below does not state explicit conditions on the style of approximation for which the work is valid, the reader should give a moment's thought to the matter. He should take it on trust that, in *most* cases, an author clever enough to give an interesting construction on approximations will also have been clever enough not to have relied on some peculiar (or, as one says, accidental) feature of the approximation.

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III. EMPIRICAL EVIDENCE AND SOURCES OF SYSTEMATIC ERROR

We give *examples* to avoid possibly premature generalities, and informal *comments* to put the examples in perspective. (The reader should here think of some substantial theory he happens to know, such as classical or quantum mechanics.) Our main question is this:

> What evidence do we have, from our experience with the theory in question, for supposing that the theory is mechanistic?

1. Large parts of the theory are bound to be mechanistic – in fact, all those which, naively, we regard as worked out! If the theoretical physicist has obtained, say, a differential equation for some physical set-up, the traditional mathematician will regard the theory as worked out only if he has a *systematic method* for computing approximations to the solution of the equation. But in the sense of 'method' used in mathematics – and surely correctly analyzed by Turing – this just means that the physicist's theory is mechanistic (at least as far as the context covered by the differential equation is concerned). The reverse procedure, to use the physical setup as an *analogue computer* which, according to theory, computes approximations to the solution, is not a mathematical method.

Two obvious points stand out. On the one hand, (partial) differential equations which were set up in the eighteenth or even back in the seventeenth century, for example, in continuum mechanics (so to speak, the opposite extreme to discrete digital computers), turned out to be mechanistic: in this century methods of approximation were found to determine solutions recursively from the parameters (data). Or, when it was shown that there are no such solutions for certain ranges of the parameters, *discontinuities* were discovered of the kind violating Hadamard's principle mentioned in the introduction. On the other hand, the subject contains a lot of unsolved problems (and we know that counterexamples to the mechanistic character of the theory must be sought among such problems). Here it should perhaps be mentioned that the American Mathematical Society recently solicited a list of open problems from distinguished mathematicians in connection with its Symposium *Mathematical developments arising from the Hilbert problems*, at De Kalb, May 1974, and that several problems proposed by Ar'nold (one of the

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leading workers in mechanics) concerned the possibility of *algorithmic*, that is, what we call 'mechanistic', solutions to questions in *stability theory*, a particularly recalcitrant subject.[3]

I have no good idea for evaluating this situation, especially in view of the experience discussed in III.3.

2. There is a whole class of theoretical results which *either* provides evidence for supposing that familiar theories are mechanistic *or* at least excludes a whole lot of candidates for counterexamples. The results have the following character.

> We do have examples of (familiar) properties, say $P(\xi)$, of real numbers ξ which are satisfied by some ξ, but in general by no *recursive* ξ. However, if ξ is *isolated* it is automatically recursive.

The following two cases give the flavor of such results.

(a) Let P_f be the property: the continuous function f *attains* its maximum in, say, $[0, 1]$. Suppose that the function is *recursively* continuous; that is, it is supplied with a recursive modulus of continuity. Inspection of the proof of Specker (1959) shows that the function f constructed there satisfies the conditions above, but f does not attain its maximum at any recursive ξ. (We shall make further uses of this fact in Section IV.)

But clearly, under the conditions on f stated above, if the set $\{\xi: P_f(\xi)\}$ has an *isolated* point, say ξ_0, this can be recursively computed by trial and error. If ξ_0 is isolated there is a rational interval $[a, b]$ where $a<b$ and ξ_0 is the only element of $\{\xi: P_f(\xi)\} \cap [a, b]$. We *tri*sect $[a, b]$ and compute approximations to the maximum *value* of f in each of the three parts: this requires only a modulus of continuity. After a finite number of steps, the approximations to those values will be close enough to determine at least one of the three parts of $[a, b]$ in which ξ_0 can*not* lie. By continuing this procedure one locates ξ_0.

We shall return to physical applications of this and related examples in IV.1.

(b) Let P_f be the property (of the continuous mapping f of say the unit circle into itself): the point ξ of the plane is a *fixed* point of f. By Brouwer's fixed-point theorem there is such a ξ. – As Brouwer knew, though he stated the result in different terms, ξ does *not* depend con-

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tinuously on f with regard to the uniform convergence topology. An even sharper counterexample was constructed in Orevkov (1964), for a particular *recursively* continuous f which has an additional property (needed for bisection arguments and the like): f is supplied or, as one sometimes says, 'equipped', with a recursive f^* where the *arguments* of f^* are *pairs*, of rational intervals I and rational points r, and the *values* of f^* are pairs, of rational points s and $v \in \{-1, 0, +1\}$ such that, if $f^*(I, r)=(s, v)$

$$s \in I; \quad f(s)<s \;\&\; v=-1 \quad or \quad f(s)=s \;\&\; v=0 \quad or \quad f(s)>s \;\&\; v=+1$$

(where $<$ is the relevant partial ordering of points in the plane). – N.B. In general, f^* cannot be extended *continuously* (on the usual topology for $\mathbb{R}$) to all real values of r since then the discrete-valued component v would have to be constant. We could extend f^* continuously (for the Baire space topology) to binary sequences in the sense of II(c), just before the *Pedagogic remark*.

Orevkov's particular f has no recursive fixed point.[4]

However, the isolated fixed points of any recursively continuous f, supplied with a recursive f^* as above, must be recursive. This will be clear to anyone who knows a standard proof of Brouwer's theorem which involves the calculation of the so-called Brouwer *degree* of a point (if $f(x) \neq x$ the degree of x with regard to f can be calculated from our data in a recursive manner, for curves sufficiently close to x).

The significance of (b), for our problem, depends on the fact that, first of all, many differential equations occurring in physical theories are solved by means of (generalizations of) Brouwer's fixed-point theorem and, second, *stability* of the solution tends to require that the solution be *isolated* (in the relevant spaces).

3. Finally, we shall try to see what can be learned from experience with the best-known examples of, demonstrably, *nonmechanistic* theories, namely, the axiomatic theories of Frege and Dedekind of specific mathematical structures, such as those of the natural numbers with the successor relation or the ordering of the real numbers. The theories consist of familiar so-called second-order axioms, known as 'Peano's axioms' for arithmetic and 'Dedekind's axioms' for the continuum. The theories are nonmechanistic because, on the one hand, they determine the struc-

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tures uniquely and, on the other hand, some of the familiar predicates of natural numbers are, demonstrably, not recursive.

Remark. For a reader who feels bothered at this point by orthodox 'doubts' about the objectivity of mathematics or about the existence of mathematical objects. Whatever the merits of those doubts (in other contexts), they do not discredit our proposed use of (experience with) the axiomatic theories mentioned. We want to use them for *orientation*, in connection with analyzing the mechanistic character of *existing* theories, say in physics. Whether we like it or not, existing theories use freely concepts from the arithmetic of natural and real numbers which have nonmechanistic theories. Furthermore, our requirements on counterexamples are very *sharp*. We do not merely ask whether existing theories are *intended*[5] to be nonmechanistic (an intention which one may like or dislike; actually they are, at least in the sense that they are not intended to be *only* about digital computers). We want to know whether, according to existing theories, there are observable sequences which are not recursive in the data, in other words, sequences which simply do not possess *any* mechanistic theory at all.

Returning then to the axiomatic theories of the mathematical structures mentioned before the *Remark*, we have a situation which is, at least superficially, quite similar to that described in III.1. The huge bulk of the mathematical problems that were regarded as solved, had formal, that is, mechanically computable, solutions. There were *formal* systems (replacing Frege's and Dedekind's second-order axioms) which were proposed as mechanical means of proving *all* theorems, in elementary (also called: first-order) logic, the field of real numbers, number theory. A great deal of 'evidence' for these proposals was said to be provided, for example, by PM.

We all know that, within a couple of years around 1930, Gödel's completeness theorem supported the proposals in the case of logic, Tarski's elimination of quantifiers did the same in the case of (the first-order theory for) the field of real numbers, and Gödel's incompleteness theorems refuted the proposals in the case of number theory.

Even using hindsight, it is not at all clear (to me) how the 'empirical evidence' available in the twenties could properly be used to prepare us for the results. In particular, in connection with number theory, would it have been more reasonable for Gödel to have tried *first* to prove completeness?

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Finally – and this is perhaps most directly relevant to the role of 'empirical' evidence – even today we do not have any theorem in ordinary number-theoretic practices which cannot be proved in PM. And though this empirical fact does not contradict incompleteness (since there are many open problems in ordinary number theory), it may seem to support the following hypothesis (which is of course also refuted by Gödel's argument): if a number-theoretic proposition can be proved at all, it can be proved in PM. In short, the nonmechanistic nature of the axiomatic theory of natural numbers was discovered, *not by sifting existing applications which accumulated in the course of nature* (here: in number-theoretic practice) *but by looking for unusual or neglected applications* (here: to metamathematical questions); applications specifically chosen for their relevance to questions of mechanization or, equivalently, formalization. This – it seems to me – is the principal lesson to be learned from our experience with axiomatic theories of mathematical object; for use with our present problem concerning the mechanistic character of (other) scientific theories.

Perhaps it is worth adding (at least for the reader familiar with the subject of constructive mathematics) that an apparently *systematic* error was introduced even in those metamathematical studies which made the mechanistic character of constructive theories a principal subject of research! Specifically, the question whether there is a proposition $\forall n \exists m R(n, m)$ which is constructively provable, but $\forall n R(n, f(n))$ is not provable if f is recursive. The systematic error which precluded the *possibility* of finding such an example was this: people insisted on considering systems Σ with the property that if $\Sigma \vdash \exists m R(\bar{n}, m)$ (where $\bar{n} = 0, 1, \ldots$) then for some $\bar{m}$, $R(\bar{n}, \bar{m})$ is provable in Σ itself. The error lies in this. Of course, if $\Sigma \vdash \exists m R(\bar{n}, m)$ and Σ is constructively sound, there is some $\bar{m}$ for which $R(\bar{n}, \bar{m})$ can be proved constructively in some Σ_n, e.g., in $\Sigma \cup \{R(\bar{n}, \bar{m})\}$. But it would be a *petitio principii* to *assume that Σ_n depends recursively on n*; cf. p. 328 of Kreisel (1972), where this kind of error is analyzed.

IV. PRINCIPAL EXAMPLES

The examples concern mathematical *definitions of nonrecursive objects* and the *requirements imposed by Hadamard's principle.* The first examples

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of nonrecursive objects, such as sequences of natural numbers, or, equivalently, examples of recursively unsolvable problems, came from logic; the decision problems for first-order arithmetic and for first-order predicate logic were the easiest examples to *find*. As time went on, problems with a more *mathematical look* were considered, and the matter of recursiveness became more interesting for mathematics (even though some of the interesting examples were 'easily' reduced to the logical ones; that is, without any new mathematical construction after some imagination was applied to spotting an interesting case). The mathematically most powerful result in this area is due to Matyasevic who found diophantine equations in two parameters, which define an enumeration of all recursively enumerable sets. (This enumeration is of course not recursive.) However, diophantine equations seem far removed from contemporary mathematical physics, and so one tries to spot consequences with a more *physical look*. – We shall first describe some of those consequences with a physical or at least geometrical look and then discuss the advantages of using Matyasevic's result, compared with earlier unsolvability results.

1. (a) One example[6] comes from the geometry of continuous curves (defined by continuous functions on, say, [0, 1]). Such a curve *attains* its maximum value, and there is a *left-most point at which this value is attained*. Several examples of such *curves determined by recursive data* are known, for example, in Specker (1959); they are recursively continuous curves which do not attain their maximum at any recursive point. A fortiori, the left-most point is not recursive.

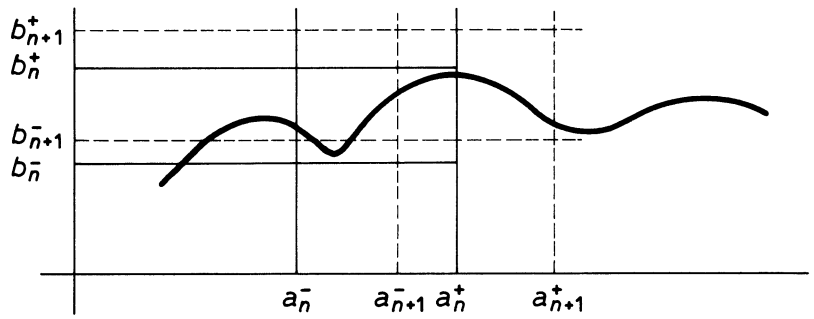
The proof in Specker (1959) uses functions defined by primitive recursive schemata. By use of Matyasevic's results the same idea of proof allows one to find examples of such functions defined by more restricted means (more familiar from ordinary mathematical practice).

The principal obstacle to using this example for our topic (of showing that existing theories are not mechanistic) is not connected with the difficulty of measuring the position of the left-most point (by means of the first method of measurement that comes to mind). For contrary to simple-minded positivistic accounts it is a principal task of theory to *determine appropriate means of measurement*, and, in particular, appropriate (statistical) evaluation of *probable errors*. In the present context, of the *geometry* of curves, the principal issue is this:

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By which data will the curve be given?

If the only way we know of describing the curve is by the usual approximations (made explicit in the uniform convergence topology) the *left-most position for a maximal value is simply not determined approximately by approximate data.* Specifically, the approximations consist of finitely many overlapping rectangles between the abscissas (a_n^-, a_n^+), and the ordinates (b_n^-, b_n^+) where $a_{n+1}^- < a_n^+$, and $b_{n+1}^- < b_n^+$:



The method of determining the data which is summarized by the picture above, involves the following *theoretical hypothesis.* Theory restricts the *modulus of continuity* in such a way that a single measurement of a point on the curve inside the *n*th rectangle is *interpreted* to mean that the part of the curve between the abscissas (a_n^-, a_n^+) lies wholly within the rectangle. ('Theoretical' is taken in the wide sense here, including global visual *impressions* of the look of the curve.)

Clearly, theory could go much farther, in some particular situation in which continuous curves arise. The class of *theoretically possible* curves may be so much restricted that a few *rough measurements are interpreted to mean that the curve in question is precisely the one defined in Specker* (1959). In this case we have a quite new (valid) method of 'measuring' the left-most position at which the curve attains its maximum: the calculation from the data. It would simply be a *better method* than the familiar 'direct' method (which will of course still be required to be *consistent* with the sophisticated method).

A hackneyed, but perhaps useful, illustration of the situation considered can be found within pure mathematics too. Suppose the function f is *regular* in, say, the circle C of the plane, $|f(z)| > N^{-1}$ for some N on C

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and f is bounded, say, by M, in some concentric circle C' containing C in its interior. In particular, suppose we are given a definition of f, say, by power series or Dirichlet series (which permits the calculation of f on C to any desired degree of accuracy). To calculate

$$\frac{1}{2\pi i}\int_C \frac{f'(z)}{f(z)}\,dz$$

we need very little on the basis of the data above! Approximate values of $f(z_n)$ for $z_n \in C$ for suitably many z_n will give us enough to approximate the values of f' on C and to calculate the integral to accuracy $<\frac{1}{2}$. This is enough to get a *precise* value since, *by complex function theory, the value* must *be among* 0, 1, 2,.... (This is a better method of calculation than, say, getting approximate values of f' from the definition and calculating the integral numerically to accuracy $2^{-100\,000}$. Put differently, it is better to operate directly on the *definitions* of f, given by (approximations to a suitable finite number of) the *coefficients* of the power series or Dirichlet series expansions, than to operate on (approximations to) the numerical values of f. This is the illustration referred to in the *Pedagogic remark* in II(c) concerning 'specially adapted' representations.)

(b) Other examples of nonrecursive objects with a physical look are to be found in the papers by Scarpellini (1963) and Richardson (1968). They consider classes $\mathfrak{F}_S$ and $\mathfrak{F}_R$ resp. of quite elementary[7] functions of a real variable such that

each element of $\mathfrak{F}_S$ and $\mathfrak{F}_R$ is given by finitely many rational numbers, and the following decision problems are recursively unsolvable:

$$\begin{array}{ll} \text{for} \quad f\in\mathfrak{F}_S, & \int f(\xi)\cos n\xi\,d\xi > 0, \\ \text{for} \quad f\in\mathfrak{F}_R, & \exists\xi[f(\xi)<0], \quad \text{etc.} \end{array}$$

The results have a physical look since certainly, for example, stability questions are directly affected by whether or not some parameter, in particular, a Fourier coefficient, can take a *negative* value. – The obstacle to a convincing application is of course parallel to case (a): where do we find a situation involving $f\in\mathfrak{F}_S\cup\mathfrak{F}_R$ with *data* permitting us to *fix* f? Otherwise we have a violation of Hadamard's principle since the two properties of f considered above are certainly not continuous if the data are subject to the topology of uniform convergence.

2. Besides simply recognizing that some questions with a physical look

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violate Hadamard's principle (a fact illustrated in IV.1), it is necessary to say a word about suitable *reformulations* of questions, to make them *continuous* in the data – but possibly leaving open whether the reformulation has a *recursively* continuous solution. Here, analogues to the distinctions in I(c) will be essential to avoid trivialities (connected with the fact that a Yes-No question which is continuous on the *usual* topology for the reals has always the same answer since the data space is connected). The well-known *three-body problem* seems to provide a good illustration, even apart from its glamor.

We consider three point masses m_1, m_2, m_3 at time $t=0$, at points ξ_1, ξ_2, ξ_3, respectively (in three dimensions), with velocity vectors v_1, v_2, v_3 moving subject to the inverse square law. The scales are so chosen that the force of attraction is $m_i m_j/(r_{ij})^2$. The problem is to determine the positions and velocities of the three point masses at time t_1.

Clearly, (given the standard theory of differential equations) there is no problem if we are given the information that there is *no collision* for times $\leqslant t_1$. In fact there will then be neighborhoods U_i in phase space (that is, masses near m_i, positions and velocities near ξ_i and v_i at time $t=0$) such that there is no collision before time t_1 for initial conditions in U_i. So the principal question is to decide whether or not there is a collision for $t \leqslant t_1$. This question has a Yes-No answer, which clearly cannot be continuous in the data (for the ordinary metric on masses, points, and velocities).

To *reformulate* the problem in accordance with Hadamard's principle we take as data

> intervals U_i in phase space with rational end points (instead of m_i, ξ_i, v_i lying in U_i)

and

> an interval T with rational end points t', t'' (instead of t_1, where $t' < t_1 < t''$).

The *collision problem* is now formulated in terms of a bound n^{-1} for error (or, if preferred, as $\varepsilon > 0$ where $\varepsilon > n^{-1}$):

> To determine recursively in (n, T, U_1, U_2, U_3): 3 neighborhoods V_1, V_2, V_3 and $t \in T$ and a 'decision', say in $\{0, 1\}$ such that

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either the 'decision' is 0; each $V_i \subset U_i$ $(i=1, 2, 3)$ and for *every* initial position in V there is no collision at times $\leqslant t$,
or the 'decision' is 1; the distances between V_i and U_i are $<n^{-1}$ and for *some* initial position in V there is a collision before time t.

The reformulation satisfies Hadamard's criterion (simply because the rational data are taken to be known or 'given' precisely). But for fixed n and U_i, a 'small' change in T may, for example, flip the decision from 0 to 1 and place V_i outside U_i instead of inside U_i.

I do not know if the reformulated collision problem has a *recursive* solution. (The results, e.g., by Moser, 1973, do not seem to settle the matter.) If it does not, classical mechanics of *point* masses is not likely to be mechanistic in our sense. This notion of 'mechanistic' has led (one) in a, I believe, completely straightforward way to (re)formulate the collision problem as above. So inasmuch as the formulation has intrinsic appeal as judged by the light of nature, the notion has the kind of uses envisaged at the end of the Introduction.

Remark. Whatever the mathematical merits of the formulation (of the collision problem) given above may be, further work is needed to see if it can be used to establish the nonmechanistic character of classical celestial mechanics. Specifically it is necessary to describe (an ensemble of) experiments and their statistical analysis for which the most probable outcome of the experiments is determined by the solution to our problem. In other words, if our problem has no recursive solution the most probable outcome of the experiments should be nonrecursive too. The difficulty consists in the fact that our formulation is *asymmetric.* It distinguishes between (a) the actual absence of a collision (if the initial data are suitably sharpened to lie in V) and (b) the possibility of a collision (if the initial data are fuzzy to degree n^{-1}); 'possibility' in the sense that a collision *cannot be excluded on the basis of those data.* It is by no means clear (to me) what principles of statistical analysis are applicable here, especially since the treatment of the data above is quite unrealistic, not at all related to any particular distribution of errors of measurement.[8]

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NOTES

[1] Proofs of the results at the end of the review of Kreisel (1972) appeared in Mints (1974).
[2] In fancy language, continuous in the real number *generators* (for c) or, again, continuous for the totally disconnected Baire space topology on binary sequences. The reader may like to write down a system of neighborhoods for binary sequences which corresponds to the usual topology on $\mathbb{R}$ (but evidently the two *distinct* sequences $0 \cdot \dot{1}$ and $1 \cdot \dot{0}$ *cannot be separated* here).
[3] It is, perhaps, not surprising that active workers in the subject are not tempted to commit the *petitio principii*, fairly familiar among logicians, of supposing that a theory is not 'precise' just because it is not mechanistic, cf. the end of Gandy (1969) – hardly a helpful first step for answering the question *whether* a given theory is mechanistic.
[4] The reader may wish to verify that, in dimension 1, that is, for a recursively continuous function f (even if not equipped with the analogue to f^* but) satisfying $|f(x)| \leqslant 1$ for $|x| \leqslant 1$, f does have a recursive fixed point ξ. However, ξ does not in general depend continuously on the data (f and its modulus of continuity).
[5] Trivially, if one states Newton's laws in terms of the usual notions of space and time, the *intention* is that they hold for objects and distances of arbitrary real measures – not merely as some kind of shorthand concerning (rational) approximations. But we *discover* that, in most applications of Newtonian mechanics, the consequences of the theory concerning approximations have a recursive, that is, mechanistic character. Put differently, there exists an 'autonomous' mechanistic theory for those approximations, albeit not the intended theory. (Here the logician naturally thinks of the parallel provided by the intended abstract notion of logical validity and the mechanical enumeration of the set of formulas in the language of first-order predicate logic which are, abstractly, valid.)
[6] Already mentioned in Section III, and an apparent exception to III.2 where, roughly speaking, *uniqueness implies recursiveness* (when the data are recursive). The reader interested in the matter may wish to see whether uniqueness in a wider 'sample' space implies recursiveness.
[7] For a precise description, see the papers loc. cit. The functions are included among rational, trigonometric, exponential functions, and possibly their inverses. (The papers were written before Matyasevic's work, and the results can be sharpened, roughly speaking, by suppressing closure of $\mathfrak{F}$ under exponentiation to the base 2.) – It should perhaps be mentioned that Richardson (1969) provides, as a foil to Richardson (1968), some decidability results for the *integral* case which have been simplified in Ehrenfeucht (1973).
[8] The difficulty may be related, as P. Suppes suggested in conversation, to the weakness of mathematical formulations in *stability theory* which also, usually, neglect the statistical aspects of errors of measurement. – It should perhaps be mentioned that there are open problems in the quantum theory, nonrecursive solutions of which would be easier to interpret (even if perhaps more difficult mathematically). Suppose we find a Schrödinger equation of a – presumably large – molecule such that the (dimensionless) ratio λ_2/λ_1 of its second to its first eigenvalue is not recursive (in the data). Then there is no difficulty in finding a corresponding experimental setup to show that the quantum theory is non-mechanistic in the sense of this note.

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