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IN DEFENCE OF DISCRETE SPACE AND TIME

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1. *Problems with Infinite Divisibility*

It is a rather safe statement to claim that the standard paradoxes of Zeno, e.g., the dichotomy, does not really pose any problem (any more). If we have an interval [0,1] that is divided up into an infinite number of intervals, $[0,1/2[$, $[1/2,1/2+1/2^2[$, ..., $[1-1/2^n,1-1/2^{n+1}[$, ..., then we know (though not so long ago) that the sum of all the intervals gives the neat result 1, as we would (now) expect. As far as speed is concerned, no problem there as well. For if we set the time interval equal to [0,1], then we have the same subdivision and obviously in any interval the value of the speed is 1, as it should be (now). So why do some philosophers, including the author of this paper continue to worry about infinite divisibility of space and time and to look for alternatives?

The basic answer is this: although it is a fact that in some cases infinite divisibility does not pose a problem, this does not entail that it does so in all cases. At this stage, what I should have to do, is to present an overview of the literature on supertasks and the problems connected with them, but I will restrict myself to two special cases that can be considered paradigmatical[1]. The first example is Ross' paradox, the second example is (what I call) Faris' paradox. Both examples presuppose a unit time interval, say one minute, divided up into an enumerable series as shown above. I will label them T_1, T_2, ... such that $T_{n+1} = [1-1/2^n,1-1/2^{n+1}[$.

1.1. *Ross' Paradox*

Suppose you have an urn and an enumerable number of labeled balls 1, 2, ..., *n*, ... In the first interval T_1, you put ball 1 to 10 in, and take ball 1 out. In the second interval T_2, 11 to 20 go in, and 2 goes out. In general in the *n*-

[1] A paper that can be considered both an excellent summary and a critical evaluation of the state of the art concerning supertasks, is Earman & Norton [1996]. One tiny comment: without going into the details, I do not agree with the view of both authors that the discussion about supertasks is to be considered, save a few exceptions, over and finished.

th interval T_n, the balls labeled 10.(n-1)+1 to 10.n go in, and ball n goes out.

The question is quite simply this: what is the state of the urn after one minute?

The rather surprising answer is: the urn has to be empty. And there is an excellent and straightforward argument: suppose the urn is not empty. Then there has to be a ball in it. This ball must have a label, say n. But in the n-th interval T_n, this ball was removed from the urn, so it cannot be in it. Hence the urn is empty.

It must be clear that, from the mathematical point of view, there is no problem. One might argue that results of this kind are no more (or just as) surprising as, say, Hilbert's Hotel[2] and similar phenomena, illustrating thereby how strange the infinite is. But *as a physical process*, the matter is not so straightforward. In my paper [1994a] I have tried to argue that any physical execution - that is, within the limits of the appropriate physical theories that can produce a description of such an execution - of Ross' paradox is impossible. I have to add straightaway that I wrote that paper in reply to Allis and Koetsier [1991] who held the opposite view. They replied to my paper in their [1995] with some very good arguments to refute my reasoning. Hence my careful choice of words: "not so straightforward". The argument is the following: in terms of physical actions, the only type of action that takes place, is to move a ball from one location to another (either in or out of the urn). Nothing else is happening. But how did the urn end up empty? Not because there was a last ball in the urn and because that was removed from the urn, for there isn't one. But that is the only physical action "available" that could have led to an empty urn.

I realize fully that this reasoning does not sound particularly convincing. Hence I will present another supertask that, to my mind, makes the problem clearer.

1.2. *Faris' Paradox (or the Faris sheet)*

This paradox is based on a description given by J.A. Faris in his [1996], pp. 13-15. The version I am presenting here, is a simplification of the full

2 In a nutshell: Hilbert's Hotel has a denumerable number of rooms which allows for the possibility that, even if the hotel is full (i.e., for every n, there is someone in room n), more and more guests can be accommodated by moving those present around. The most dramatic case is that where a denumerable number of guests arrives. Every person already in the hotel moves to room 2.n, if (s)he was staying in room n, thus all odd-numbered rooms are free!

story[3].

Before you lies a sheet of paper. You have a pencil and an eraser. The supertask to perform is quite simple: in the *n*-th interval, write down the numeral *n* on the sheet and erase it after that (in the same interval).

The question is the same as in the previous case: what is on the sheet after one minute? And the answer is the same: nothing. Even the argument is the same: if there were any number on the sheet, say *n*, then obviously it was erased immediately after writing it down in the same interval.

Consider now the supertask in terms of physical processes. It is clear that only two physical actions are taking place: writing down numerals and erasing them. No other action is involved. At the end of the one minute, the sheet is empty. As there have been numerals on it, the only physical action that could have led to an empty paper is an erasure. But, as there is no last numeral, there cannot have been a last numeral on the sheet, hence the empty sheet after one minute is *not* the result of an erasure. According to this analysis, the following set of statements is indeed inconsistent:

(a) two physical actions are possible
(b) during the one minute interval, (a) applies,
(c) the situation at the end of the interval, is not the result of (a).

This extremely simple situation may help to clarify the strangeness of the Ross' paradox. Duplicating this argument, it says that the empty urn cannot be the result of moving the balls around.

Actually, if one is not convinced by this argument, try this observation. The well-known Thomson lamp - running through the intervals, the lamp is alternately on and off - leads to an indeterminate state at the end of the one minute interval. There is general agreement about that. Here is an easy way to connect a Thomson lamp to a Faris sheet. In order to have the situation as physically concrete as possible, suppose that the sheet lies on top of the switch of the Thomson lamp. When a numeral is written on the sheet, the lamp is on, when it is erased, the lamp is off. In semi-formal language, we can write the connection as a couple that is of the form (write numeral *n*, Thomson lamp on) or $(W(n),T(1))$ or that is of the form (erase numeral *n*, Thomson lamp off) or $(E(n),T(0))$. This means that with every erasure cor-

[3] The focus of Faris's work is not on supertasks as such, but as an element of an investigation with the aim to arrive at an interpretation of Zeno's paradoxes as it appears in the original texts. It is worth mentioning however that Faris reaches a conclusion that is not the conclusion I arrive at. Without going into the details, I believe in fact that his solution is not correct, illustrating once more that supertasks are the subject *par excellence* about which to fully distrust your intuitions.

responds an off-state of the Thomson lamp. Suppose finally that the end-state of the Faris sheet is due to an erasure, then it must correspond to the off-state of the lamp. This would determine the state of the Thomson lamp at the end of the one minute interval. But it is undetermined. Thus the end state of the sheet cannot be the result of an erasure.

In short, something has happened, some physical action has taken place, whereof we have no idea what it is. For that matter, one might actually argue that at the end of the one minute interval, your name is on the sheet. As there is no physical action that produced the empty sheet, likewise there cannot be a physical action that prevents an arbitrary state from manifesting itself.

The result of these considerations is not (unfortunately) that infinite divisibility is a nonsensical concept, but (merely) that it is as problematic as its counterpart, namely discreteness. However as far as the notion of discreteness is concerned, it has always been assumed that that is really and truly a problematic if not nonsensical concept. The remainder of this paper is a plea to reject this assumption.

2. *Two approaches to discreteness*

2.1. *The Pythagorean problem*

The basic argument why a discrete interpretation of space and/or time does not make sense has been formulated quite clearly by Hermann Weyl[4]. The argument runs as follows. Assume, for simplicity, that the plane consists of small squares. Suppose we have an origin, a vertical and horizontal axis. Thus, each square has integer coordinates (i,j). It seems straightforward to define the distance d between two points $p(i,j)$ and $q(i',j')$ as $d(p(i,j), q(i',j')) = | i - i' | + | j - j' |$. Consider now the three points $p(0,0)$, $q(3,0)$ and $r(0,4)$. In this right-angled triangle, Pythagoras fails miserably as $d(q(3,0),p(0,0)) + d(p(0,0),r(0,4)) = d(q(3,0),r(0,4))$. The problem is that the distance function d is, on the one hand, the most "natural" choice possible in a discrete geometry, but, on the other hand, it does not correspond with observable geometrical objects and their properties, in particular, with a Euclidean distance function d_E (applicable when we are talking about human-scale environment[5]).

[4] Weyl [1949], p. 43: "If a square is built up of miniature tiles, then there are as many tiles along the diagonal as there are along the sides; thus the diagonal should be equal in length to the side".

Actually, the situation seems less dramatic than expected. After all, the viewpoint that a discrete geometry has *necessarily* a unique and "natural" distance function, needs to be argued for. As I will claim at a later stage in this paper, there are good arguments against the "naturalness" of the function d, mentioned above. Thus there is a choice either to accept d or to reject d. In the latter case, other distance functions are admissible. Why not, along the same lines as classical geometry, *impose* a distance function? Or, if such is possible, why not have two (or more) distance functions? At the micro-scale we have d, at the macro-scale we have d_E. And, if such is possible, suppose that there is a nice connection between d and d_E? Would that not constitute a neat solution? As one might expect, this is the approach favoured in this paper.

The remainder of this paragraph discusses two already existing solutions and presents a new framework where both can be integrated.

2.2. *First solution: width as a relevant concept*

What follows is a brief summary of my proposal as presented in my [1987]. A possible way to solve the Hermann Weyl problem is to introduce the width N_D of a line in a discrete plane. Thus N_D represents a large (finite) number, corresponding to the number of squares that form N_D. Given a line L, the width is always defined as perpendicular to that line. Now suppose that the line has an orientation corresponding to an angle a between the line and the x-axis. Then the width N_D of that line, when projected on the x-axis will be $N_D/\cos\alpha$.

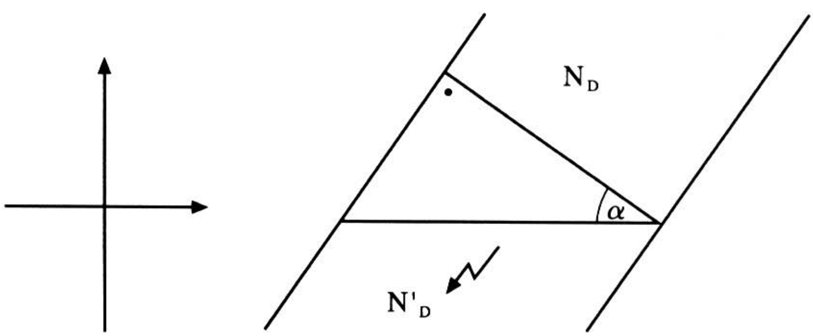


figure 1

5 Throughout this paper I will confront any discrete model of space and time with the classical Euclidean model. This is, of course, a severe restriction, but once the Euclidean connection is established, we have at least a starting point for further developments.

Now comes the definition of a distance function. The distance d between two points p and q is the number of squares in the rectangle formed by the line from p to q and the width N_D, divided by N_D. The idea is that, although in a discrete geometry, lines must necessarily have a width, this is not an essential feature, so it can be divided out. Hence:

$$d(p,q) = [(N_L.N_D)/\cos\alpha] \ (\text{div } N_D).$$

N_L here corresponds to the number of layers parallel to the x-axis between p and q.

As an illustration, consider the Weyl problem. We have a right-angled triangle pqr such that for simplicity the right sides pq and qr are equal to one another and are aligned with the axes of the grid. Suppose that the number of squares in the right sides is N_L. Then

$$d(p,q) = d(q,r) = [N_L.N_D] \ (\text{div } N_D) = N_L.$$

However, the hypotenuse has an angle of $\pi/4$. Hence $\cos\alpha = \sqrt{2}/2$. Thus

$$\begin{aligned} d(p,r) &= [(N_L.N_D)/\cos\alpha] \ (\text{div } N_D) \\ &= [\sqrt{2}.N_L.N_D] \ (\text{div } N_D) \\ &= [\sqrt{2}.N_L]. \end{aligned}$$

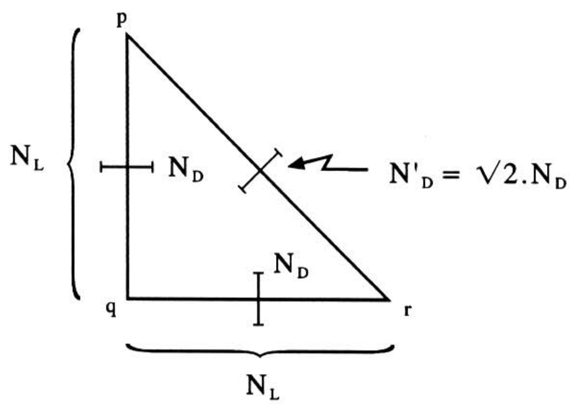


figure 2

No calculations are needed to show that the Pythagorean theorem holds, i.e., $d^2(p,q) + d^2(q,r) = d^2(p,r)$. Finally, there is an easy explanation why the Weyl problem occurs: it corresponds to the limiting case $N_D = 1$.

Although the introduction of a width N_D apparently solves the problem, it is equally clear what the drawbacks are. Without Euclidean geometry in the background, there is really no way to get the construction going. Thus I did not have, e.g., a definition of a line in terms of the discrete geometry, and, above all, the projected width on the x-axis of a line L is calculated according to a Euclidean distance function that is not explicitly mentioned. In short, there is a mixture of two distance functions here that needs to be resolved.

2.3. *Second solution: points with extensions*

Peter Forrest in his [1995] presents a solution, that is quite definitely an improvement over my approach. Let me first try to summarize his ideas. He starts by introducing a family of discrete spaces $E_{n,m}$, where n corresponds to the "classical" dimension of space and m is a scale factor, to be understood as follows: m is a parameter to decide when two points are or are not adjacent, which is the basic (and sole) concept of his geometry.

Thus in the case $n = 2$, points are labeled by couples of integers (i,j) and two points (i,j) and (i',j') are adjacent if (a) they are distinct and (b) $(i - i')^2 + (j - j')^2 \leq m^2$. Once adjacency has been stipulated, a distance function can be easily derived: the distance between p and q, $d(p,q)$, is the smallest number of "links" in a chain of points connecting p and q such that each one is adjacent to the previous one. Next there is no problem to show that a straight line passing through two points is that chain of points that has the shortest distance.

If the parameter m has a small value, then the resulting distance function is not Euclidean. More specifically, if $m = 1$, then we have, once again, the situation presented by Weyl. But if, say, $m = 10^{30}$ (the figure proposed by Forrest himself), then the situation changes. Then it is possible to show that the distance function on the discrete space will approximate the Euclidean distance function as close as one wants for sufficiently separated points. Without presenting all the details, one can show that a Euclidean distance function d_E and the discrete distance function d are related by a scale factor, i.e.,

$$d_E(p,q)/d(p,q) = \text{constant}(m),$$

where the constant is determined by the value of m. No calculations are needed once again, to show that the original distance function d satisfies the Pythagorean theorem.

If one is looking for a weak point in this approach, then inevitably one must end up with the basic notion of adjacency. What is the reason for defining adjacency in Euclidean terms (apart from the fact of course that Forrest is, as he clearly states, looking for discrete approximations to Euclidean spaces)? For, after all, a condition such as $(i-i')^2 + (j-j')^2 \leq m^2$

looks as Euclidean as can be. Of course, Forrest is absolutely right that, once you have your notion of adjacency, all the other stuff follows in a straightforward way. But why this particular notion of adjacency? I should add here - and I will return to the topic in paragraph 3.6 - that Forrest calls into question the whole idea of a specific representation of a discrete space, e.g., as built up from tiny squares. Hence, trying to derive a distance function from properties of a specific representation is, according to Forrest, quite literally besides the point.

2.4. *Overcoming the difficulties*

The idea of this paragraph is to present a discrete geometry from scratch, making references to Euclidean notions as minimal as possible (preferably not at all).

Let us start with the plane, built up with squares (or tiles). To make the distinction clear, I will call the squares *t-points*. It is straightforward to define coordinates for the t-points. Choose an arbitrary tile as the origin, label it (0,0) and label the others (i,j), where i and j are integers in the usual fashion. The next thing we need are *t*-lines. Actually, there are several possibilities (although the opposite might seem to be the case). I will use the simplest one[6].

Definition 1. Given two *t*-points $p(i_1,j_1)$ and $q(i_2,j_2)$, then the *t-line passing through p and q* is defined by the set of *t*-points $r(i,j)$ that satisfy:

$$j - j_1 = [((j_2 - j_1)/(i_2 - i_1)).(i - i_1)],$$
$$\text{or} \quad i - i_1 = [((i_2 - i_1)/(j_2 - j_1)).(j - j_1)],$$

if defined. There are two special cases: (i) $i_1 = i_2$, and (ii) $j_1 = j_2$. In case (i), the equations reduce to $i = i_1$, and in case (ii) to $j = j_1$. From now on, I will only deal with the general case, as the special cases do not generate any

[6] One other possibility I had in mind is the following. This solution avoids the use of algebraic equations altogether. First you define connectedness in the usual way. Given two t-points $p(i_1,j_1)$ and $q(i_2,j_2)$, define the rectangle formed by (i_1,j_1), (i_1,j_2), (i_2,j_1) and (i_2,j_2).

It consists of $|(i_2 - i_1 + 1).(j_2 - j_1 + 1)|$ *t*-points. A *t*-line is defined as a connected series of *t*-points such that (a) it contains the least number of *t*-points, (b) it divides the rectangle in two parts P_1 and P_2, and (c) the number of *t*-points in P_1 is as close as possible to the number of *t*-points in P_2. The underlying idea is to have a discrete notion of a diagonal. Two remarks: (i) this definition does not guarantee a unique solution, however it is not difficult to show that any of them will do as they differ marginally (in terms of *t*-points), (ii) this definition is not equivalent to the one discussed in this paper (as the definition in the paper produces a unique solution).

specific problems. The square brackets indicate integer rounding off. The "or" in the definition is important. I am not looking for values that satisfy both equations, but at the same time I do not wish to exclude that some values do satisfy both equations. Thus, as an example, a *t*-line passing through $p(2,0)$ and $q(4,3)$ will pass through the following *t*-points: $(2,0)^{*,**}$, $(2,1)^{**}$, $(3,1)^{*}$, $(3,2)^{**}$, $(4,3)^{*,**}$. The superscripts * and ** indicate that the value satisfies the first equation, resp., the second equation.

Definition 2. If a *t*-line is defined by two *t*-points $p(i_1,j_1)$ and $q(i_2,j_2)$, then the ratio $\omega = (j_2 - j_1)/(i_2 - i_1)$, if defined, will be called the *direction* of the line.

Note: I assume here that the rational numbers are available. This is hardly a trivial remark and I will return to it in 3.5.

Definition 3. Two *t*-points $p(i_1,j_1)$ and $q(i_2,j_2)$ are *connected* if
$i_2 = i_1 \pm 1$ and $j_2 = j_1 \pm 1$.

Definition 4. A *t*-line is *connected* if, given two *t*-points p_1 and p_n, there is a series of *t*-points $p_1, p_2, ..., p_k, ..., p_n$ on the *t*-line, such that for every k, p_k and p_{k+1} are connected.

Lemma: Given two *t*-points (i_k,j_k) and (i_{k+1},j_{k+1}) on a *t*-line, such that $\omega > 0$. Then, if $j_{k+1} > j_k$, then $i_{k+1} \geq i_k$.

Proof: Suppose that $j_{k+1} > j_k$, and $i_{k+1} < i_k$. Then
$i_{k+1} - i_1 < i_k - i_1$. Then
$\omega .(i_{k+1} - i_1) < \omega .(i_k - i_1)$. Then
$[\omega .(i_{k+1} - i_1)] \leq [\omega . (i_k - i_1)]$. Then
$j_{k+1} - j_1 \leq j_k - j_1$. Thus
$j_{k+1} \leq j_k$. Contradiction. □

Note: the proof is quite similar for $\omega < 0$. The special case $\omega = 0$ needs no comment.

Theorem 1: Every *t*-line is connected.

Proof: Let $p_1(i_1,j_1)$ and $p_n(i_n,j_n)$ be given. First note that, according to the definition of a *t*-line, there must be a sequence of *t*-points, such that $i_1 = m$ and $i_n = m+(n-1)$. Take two neighbouring *t*-points, say, p_k and p_{k+1}, such that $i_{k+1} - i_k = 1$. Suppose that $j_{k+1} > j_k + 1$ (the case $j_k - 1$ is similar). Then there is at least one j such that $j_k < j < j_{k+1}$. But according to the lemma, this means that $i_k \leq i \leq i_{k+1} = i_k + 1$. Thus either $i = i_k$ or $i = i_{k+1}$. Repeat the argument with j_{k+1} replaced by j. Eventually we must reach j_k and thus we get a connected series of t-points going from j_k to j_{k+1}. □

We now have the basic ingredients, t-points and t-lines, to get a geometry going. But do note that, up to this point, no mention has been made of distances. Actually, in this proposal, I will simply avoid talking about a distance defined in terms of t-points and/or t-lines. Rather I want to arrive at a definition of points and lines, that is, the macroscopic geometrical units that will have to approximate Euclidean notions as close as possible. The core of the problem is rotations. Consider a macroscopical geometrical object, say a rectangle. If the rectangle is translated across the square grid, no problem arises. But, if it is rotated, the number of squares making up the triangle does not remain the same. Thus, in terms of micro-distance, rotations are funny things. It is therefore understandable that it seems unlikely to be able to define rotations on the basis of micro-concepts. However!

Talk about rotations equals talk about angles. Now angles in a discrete geometry are nasty business as well. But there is one exception: the right angle. It is perfectly possible to define a right-angle in geometrical micro-terms.

Definition 5. Suppose that a t-line L with equations

$$j - j_1 = [\omega(i - i_1)],$$

or $$i - i_1 = [(1/\omega).(j - j_1)]$$

is given. Then the t-line perpendicular to L in (i_1, j_1) is the t-line defined by the equations:

$$j - j_1 = [-(1/\omega).(i - i_1)],$$

or $$i - i_1 = [-(\omega).(j - j_1)].$$

Note: If asked why this particular definition "works", it helps to see what happens if one asks for the line perpendicular to the line perpendicular to L. That happens to be L itself.

The last element we need is based on the following observation. Objects that in Euclidean geometry can be described in two equivalent ways, say D_1 and D_2, need not be identical in terms of t-points and t-lines. For what is the problem? If we talk about rotations, then it would be a great help if we could define a circle such that the result is a Euclidean circle. Of course, the first definition that comes to mind, is: the set of t-points that are equally far away from a fixed t-point. This requires a notion of distance and, whatever you try, it will not do (see further). But there are other ways to characterize circles, such that even in terms of t-points and t-lines, the result is a Euclidean looking circle.

Definition 6. Suppose a t-line segment is given with end-t-points, $p(i_1,j_1)$ and $q(i_2,j_2)$. A t-circle C is the set of all t-points, such that each t-point is the intersection of a pair of t-lines L_1 and L_2, such that (i) L_1 and L_2 are perpendicular, (ii) L_1 goes through p, and (iii) L_2 goes through q.

Note that in this definition no reference at all is made to Euclidean notions. This is an entirely microscopic definition that produces a Euclidean-looking circle.

Definition 7. Given a t-circle based on a t-line segment with end-t-points, $p(i_1,j_1)$ and $q(i_2,j_2)$. Then the t-point c with coordinates $([(i_1 + i_2)/2], [(j_1 + j_2)/2])$ is the *center* of the circle.
I will abbreviate these numbers by (c_1,c_2).

Theorem 2. t-points $p(i,j)$ on a t-circle with center (c_1,c_2) (roughly) satisfy the equation $(i - c_1)^2 + (j - c_2)^2 = \text{constant}$.

Proof: I will look at a special case (without loss of generality), namely, suppose we have a t-line segment with, on the one end, a t-point $p(-K,0)$ and, at the other end, $q(K,0)$. The coordinates of the center are then (0,0). When the theorem says "roughly", that is because I will allow myself to reason without the square brackets. Thus a line going through $p(-K,0)$ will satisfy (roughly) the equation $j = \omega.(i + K)$, whereas the perpendicular going through q will have as equation $j = (-1/\omega).(i - K)$. The t-point of the circle has to satisfy both equations, thus:

$$j = \omega.(i + K)$$
and $$j = (-1/\omega).(i - K).$$

From the first one follows $\omega = j/(i + K)$, substituted in the second one, this gives:

$$j = (-(i + K)/j).(i - K)$$
or $$j^2 = -(i^2 - K^2)$$
or $$i^2 + j^2 = K^2.$$ □

As to be expected, I will call K the *radius* of the t-circle.

From here on, there are several routes to continue. The idea is to define a Euclidean distance function on the basis of t-circles. Because theorem 2 is a "rough" proof, it is safe to define a point (not a t-point) as having a minimum extension.

Definition 7. Suppose we fix a (large) natural number K. Then a *point* is the region enclosed by a t-circle with radius K.

Definition 8. The *t-point p related to a point P* is the *t*-point that is the center of the *t*-circle that defines the point.

Note: To avoid confusion, I will use capital letters *P*, *Q*, ... to indicate points. Sometimes I will attach coordinates to them, thus $P(i,j)$ means that the *t*-point *p* related to *P* has coordinates (i,j).

Definition 9. Two points *P* and *Q* are *connected* if the corresponding *t*-circles either overlap or touch, i.e. have at least one *t*-point in common.

Definition 10. Given two points *P* and *Q* with related *t*-points *p* and *q*. The *line* $\mathcal{L}$ (not a *t*-line) connecting *P* and *Q* is a set of pair-wise connected points such that the *t*-points related to them lie on the *t*-line going through the *t*-points *p* and *q*.

Note: I use the notation $\mathcal{L}$ for a line to avoid confusion.

The implicit idea in what follows, is quite simply to define the Euclidean distance between two points *P* and *Q* as the least number of connected points between *P* and *Q*. I will treat a special case first: suppose we want to know the distance between the point $P(0,0)$ and $Q(i,j)$. Consider then the *t*-circle that has $p(0,0)$ as its center and goes through $q(i,j)$. Its equation is $i^2 + j^2 = M^2$, for some number *M*. Thus the radius is *M* and, as every point has a radius *K*, it seems absolutely "natural" to define

$$d_E(P(0,0),Q(i,j)) = M/K.$$

In terms of a picture, to measure the distance, what you do is to see how many circles of radius *K* can be fitted in on a radius of a circle of radius *M*.

Theorem 3. d_E is a Euclidean distance function.

Proof: The proof is fairly trivial once one realizes that

$$d_E(P(0,0),Q(i,j)) = M/K = (i^2 + j^2)^{1/2}/K.$$

Up to the scale factor *K*, this is nothing but a Euclidean distance function. □

Note that, as expected, distance is invariant under translation. Thus, if d_E is the distance between two points $P(i_1,j_1)$ and $Q(i_2,j_2)$, it is easy to check that:

$$d_E(P(i_1,j_1),Q(i_2,j_2)) = d_E(P'(i_1 + m,j_1 + n),Q'(i_2 + m,j_2 + n)),$$

where m and *n* are arbitrary integers.

I will not spend too much effort to show how to integrate the two proposals that have been discussed. It is obvious that if a line $\mathcal{L}$ is made up of points that have a radius K, then however the line is orientated compared to the squares, it will have a constant width (perpendicular to the line) that is $2K$. Thus the idea of a constant width comes out naturally. As far as the solution of Forrest is concerned, this solution presented here is pretty close to his approach, but with the bonus that now there is a neat explanation for his notion of adjacency in terms of *t*-points and *t*-lines.

Two remarks. Firstly, as has been remarked before, Forrest claims that the advantage of his proposal is that exactly one simple primitive notion is sufficient, namely *adjacency*, to construct a geometry - an advantage lacking in my proposal - but as the outline above shows, the strong point of this framework is that it neatly integrates both my and Forrest's proposal. Secondly, the solution presented above is clearly just one of a family of solutions. It is, e.g., perfectly possible to continue to talk about *t*-points and *t*-lines (as, indeed, Forrest does). There is no real need to introduce points and lines. This is a rather comforting thought: the project does not stand or fall with one unique solution.

All this being said and done, it is worthwhile to have a look at other objections that have been raised against a discrete approach. Most of these problems will have easy answers, with one notable exception.

3. *Further objections*

3.1. *The "natural" distance argument.*

When discussing the Weyl argument, I wrote that the distance function $d(p(i,j),q(i',j')) = |i - i'| + |j - j'|$ is the most "natural" one possible. If it is possible to find other distance functions, equally well defined in terms of the squares (or *t*-points, in my terminology), should the conclusion then not be that there is nothing really "natural" about it? With the proposal outlined above, this task is rather trivial.

Think about the definition of a *t*-line connecting two *t*-points $p(0,0)$ and $q(i,j)$, where for simplicity p is supposed to be the origin. If we define the distance d from p to q as equal to the number of *t*-points between p and q, then one notes the following:

(i) in some cases, it is equal to the sum $i + j$, thus corresponding to the "natural" distance,
(ii) in some cases it is equal to the largest of i and j, $\max(i,j)$,
(iii) for most values, it is something in between these two, thus $\max(i,j) \leq d(p,q) \leq i + j$.

This is based on the following observation. The two equations that define a *t*-line, namely

$$j - j_1 = [\omega.(i - i_1)],$$
$$\text{or} \quad i - i_1 = [(1/\omega).(j - j_1)]$$

have the following property. For every value of i, there is a unique value of j, so we have at least i possible solutions. The same holds for the second equation, for every j, there is a unique value of i. So we have j possible solutions. In some cases, for every i-solution, there is a corresponding j-solution, then we have that $d(p,q) = i = j = \max(i,j)$. Think about the case when $\omega = 1$. Then we have the equations $j = [i]$ or $i = [j]$; they reduce to the same, namely, $j = i$. Hence the number of *t*-points between p and q is $i = j$, i.e., $\max(i,j)$. But in most cases, there will be i-solutions without corresponding j-solutions and vice versa. Example: take the *t*-line going through (0,0) and (2,3). Then $\omega = 3/2$. The first equation, for $i = 1$, gives $j = 1$. So (1,1) is on the line, but, for $j = 1$, the second equation says $i = 0$, so (0,1) is on the line. So, there will be more solutions than $\max(i,j)$, but not necessarily as much as $i + j$ (as there are overlaps).

In addition, note that the following inequalities are always the case:

$$\max(i,j) \leq (i^2 + j^2)^{1/2} \leq i + j.$$

There is a nice picture to illustrate this. If we talk about circles in the standard way, then a circle is the set of all *t*-points that are equally far away from a fixed point. I leave it to the reader to reflect on the following picture[7]:

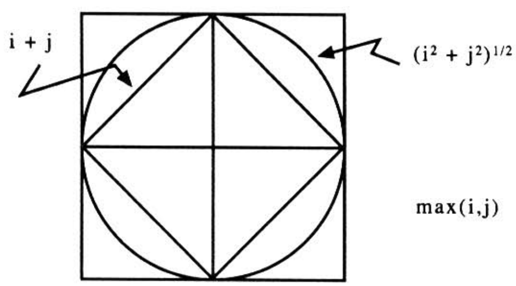


figure 3

If we consider max(i,j) and i + j as "natural", is it not plausible to consider the distance functions "in between" as natural as well. In that case, there is nothing "unnatural" about the Euclidean distance function (that is, on the level of *t*-points and *t*-lines).

[7] In this sense, the square circle does exist!

In short, I cannot be terribly impressed by the "natural" distance argument. If, however, the claim is that in discrete geometry the distance function should somehow be a function of the *t*-points, then it is clear that (i) there are many possibilities, (ii) the proposal outlined here satisfies that criterion.

3.2. *The smallest is not the smallest*

This is surely one of the best known counter-arguments. Faris ([1996], pp. 54-55) gives a beautiful example that tells it all: suppose a curved line connecting *A* and *B*. Suppose that the distance from *A* to *B* is the shortest distance possible. Now draw the chord that connects *A* and *B*. Surely the length of the chord must be shorter than the length of the curve.

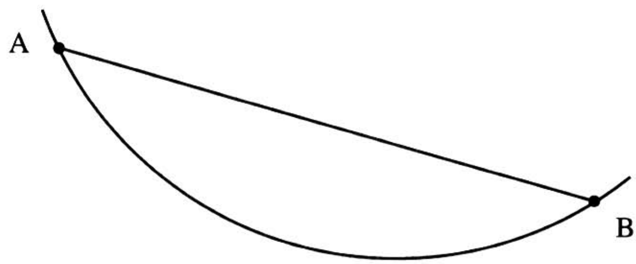


figure 4

This would indeed be the case if the Euclidean distance function is applicable on all levels, i.e., both on the level of *t*-points and points. But it doesn't. If we move down to the level of the *t*-points, it is very well possible that the chord and the curve have the same length in terms of distance functions, such as the max(i,j), discussed above. In a nutshell, in the proposal outlined above, the following statement is perfectly acceptable: at the level of points, you have the Pythagorean theorem, on the level of *t*-points, you do not.

3.3. *Problems with motion*

Of all the problems a discrete viewpoint has to deal with, this is no doubt the hardest problem to tackle. If space is discrete and if time is discrete (although for some of the objections that follow this is not even required), then movement, whatever it is, must be discrete as well. It obviously cannot be continuous. But discrete motion raises all kinds of strange problems (I will now talk about *hodons* (or of *t*-points) and *chronons* for the smallest space and time units):

(i) There can be no motion at all, because of the following argument: "If motion is the time derivative of position, then position must be differentiable, hence continuous, function of time, and so there can be no motion in a discrete space."(Harrison, [1996], p. 279).
(ii) If there is motion, then it has to be a "jerky" motion. In other words, there is no genuine change. Worse, it seems that from chronon to chronon, things are annihilated and recreated.
(iii) If there is motion, it is possible that objects can cross each other, without aligning. In short, what we have here, is Zeno's stadium paradox, once more. As a side effect, this also means that two lines that are not parallel, do not intersect, if the intersection point happens to be the cross-over point.

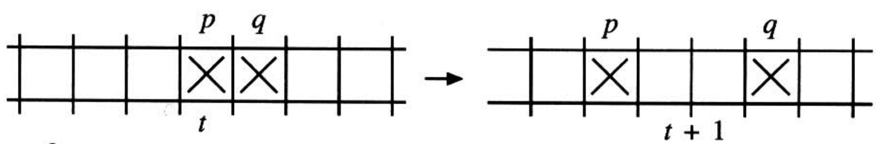
As must be obvious, such problems must receive answers, for otherwise, discrete geometry (together with discrete time) will not do as a basis for differential and integral analysis and hence will not do as a basis for physics, which after all, is one of the most important goals we have in mind. Now (i) and (iii) are easy to refute, but (ii) is a deep philosophical problem that is not easy to solve at all (and I do not pretend to have a solution, merely some plausibility arguments).

Ad (i): When we are talking about derivatives, we are talking about analytical concepts that are related to geometrical concepts. But then we are on the level of Euclidean geometry, that is on the level of points and lines and not on the level of *t*-points and *t*-lines. Hence this cannot be a counterargument for motion in a discrete space. This being said, it is not trivial to match geometry and analysis in an easy and straightforward way (see 3.5).

Ad (ii): The "jerky" motion argument says that if hodon/chronon defines the largest speed possible, then what is an object occupying one hodon supposed to do if it is to travel at half that speed. The only scenario's one can imagine, are those wherein the point moves one hodon, does nothing the next chronon, moves one hodon the next, and so on. Thus one gets a jerky motion. Actually, that we have this kind of strange behaviour does not really worry me. After all, we are talking here about the ultimate constituents of this world, space and time. What expectations I am supposed to have concerning the behaviour of space and time at that level? If I look at our best (continuous) theories today, these too present me a strange picture of the world as far as its ultimate elements is concerned. But, do note that at the level of points and lines, once again, there will be no jerky motion to observe[8].

The same applies to the creation and annihilation idea. I will not use the weak, if not misleading argument that in quantum mechanics, creation and annihilation operators are essential ingredients. Apart from the apparent name likeness, not much else is to be found there. Rather I would defend the following idea. Suppose for simplicity that we have a grid n by n. There are n^2 squares. Suppose at chronon t we have a certain distribution of points over the grid. For the next chronon, there are 2^{n^2} possibilities, for each hodon can be occupied or not. We could imagine a universe where each of these is a real possibility. But what we note is that not all possibilities occur. Some are excluded. And, in some cases, these exclusion rules are detectable. A simple example may illustrate this process:

case 1:



case 2:

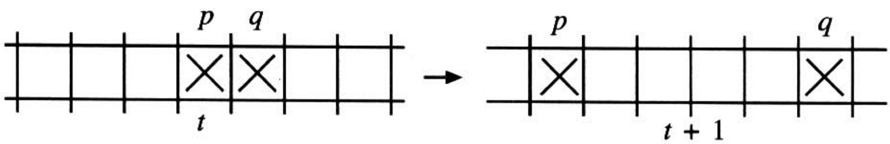


figure 5

If one hodon per chronon is the maximum speed, then case 1 is possible — p moved, one step to the left, q one step to the right – but case 2 is impossible. This is easy to check because, after all, the number of possibilities is finite.

In short, what links the situation at one chronon with the situation at the next chronon is a sort of embryonic form of natural law(s). Of course, one might still just be unhappy with the idea of going out of and back into existence a billion billion times a second. But then again, according to our best physical descriptions at the moment, I cannot even start to describe my own body, let alone "myself" (if any difference), in quantum-mechanical terms at the level of quarks, electrons and all the other stuff that is "me". So, to claim that continuity solves the problem that discreteness is not able to solve, is a rather empty statement.

[8] Although I should add here that not everyone shares this opinion. Forrest's [1995] is titled "Is Space-Time Discrete or Continuous?" and the subtitle says "An Empirical Question". He, for one, believes firmly that the question can be settled empirically.

Ad (iii): Let me, first of all, formulate the argument in all its simplicity. Suppose we have three rows consisting of *t*-points or hodons:

*A*1	*B*1	*C*1
*A*2	*B*2	*C*2
*A*3	*B*3	*C*3

During one chronon row two moves one hodon to the left and row three one hodon to the right. Thus we find:

	*A*1	*B*1	*C*1	
*A*2	*B*2	*C*2		
		*A*3	*B*3	*C*3

If one now looks at row 2 and 3, one sees that they are two hodons apart. Hence the question: at what moment did we have the following situation:

*A*2	*B*2	*C*2	
	*A*3	*B*3	*C*3

The answer "half a chronon" contradicts the discreteness and the answer "at no moment" seems bizarre, for at no moment did *B*2 align with *A*3 and *C*2 with *B*3. So, out goes the idea of a discrete solution, as most philosophers and scientists are very willing to accept. This probably explains why the problem is not so often discussed. Actually in the second edition [1995] of Sainsbury's classic on paradoxes, it is even missing.

Nevertheless, there are a number of philosophers who will stress that the "at no moment" response is at least logically acceptable. Thus Salmon in his [1980]: "This is strange perhaps, but again, it is hardly logically impossible" (p. 65). Whitrow goes even further and claims that Zeno makes a logical error one has asks for what has happened *in between*: "Indeed, Zeno is in fact guilty of a logical error himself when he makes this appeal, for he is tacitly invoking a postulate of continuity which is incompatible with the hypotheses adopted at the beginning of the argument." ([1980], p. 191). However, logical possibility does not imply plausibility, let alone acceptability.

Note, once again, that we are talking at the level of hodons and chronons. At the level of Euclidean geometry, these phenomena will not happen, as points and lines have extensions and are capable of overlapping.

In short, the overall answer to all these problems is that, on the level of the *t*-points and *t*-lines, we indeed do have a rather strange world. It is a world where things can cross without meeting, where things can go in and out of existence, but, on the level of points and lines, everything looks Euclidean (as close as possible). If a comparison can be made, what is happening here is quite similar to Conway's game of life (or any cellular automaton for that matter). The rules on the level of the squares are elementary and concern only local properties, but, once the thing starts moving, all kind of strange creatures appear, with their own properties, that are not easily reducible to the behaviour of the elementary parts. Finally, it also worth stressing that the continuum view is about as good as the discrete view in terms of strangeness. As Richard Gale puts it in his [1978], more precisely in the introduction to Zeno's paradoxes: "... there is no way in which we can picture or intuit a continuous time. Any attempt to reduce it to common-sense notions will result in absurdities." (p. 394). Quite so.

3.4. *Anisotropy problem*

This problem states that a discrete space favours certain directions, hence is anisotropic, whereas Euclidean space is isotropic. I follow here, up to the last detail as it were, the analysis of Peter Forrest [1995]. The anisotropy is a microgeometrical feature that disappears at the macrogeometrical level. In the framework I propose here, this is quite clear: the transition from *t*-points and *t*-lines to points and lines requires the circle, the isotropic object *par excellence*. One might even argue that the specific shape of the hodons is actually more or less irrelevant. In fact, and here too I fully agree with Forrest, a fuzzy or vague approach would be best (see 3.6).

3.5. *Getting the numbers right*

As stated before, a discrete geometry that is supposed to be mathematically (and physically) interesting, should allow us to develop the concepts and notions needed for differential and integral analysis. Surely, one of the most important connections is that between geometry and the real numbers. But here we find a deep problem. At the side of the discrete geometry, we do find the integers, we add them, multiply them, divide them, take squares and square roots, and so on, in short, what we get are (at best) the algebraic numbers. No hope of ever arriving in this way at the rest of the reals (actually, the continuum, classically speaking, of the transcendentals). So, does this mean that most of the reals have to go? Even if one is brave and prepared to answer yes, this is unfortunately not the end of the story.

In order to illustrate the problem that follows, let me just say a few words about finding discrete theories or models for classical (possibly) infinite theories or models[9]. The procedure, roughly outlined, is that the domain of the original classical model is cut up in a finite number of pieces $P_1, ..., P_k$, and, given a statement $A(x_1, x_2, ..., x_n)$, its truth value is determined as follows (I use capital letters for the new truth values and small letters for the classical values):

(i) True, if, for all i, if x_i (classically) belongs to some P_j, then for all other choices of x_i in P_j, A is true,
(ii) False, if, for all i, if x_i (classically) belongs to some P_j, then for all other choices of x_i in P_j, A is false,
(iii) Undecided, if for some choice A is true and for some choice A is false.

As an example, take a classical real model for the plane, R^2. Split R up in a finite number of pieces, each of length e (in order to be as concrete as possible, say, 0,001). I will label a piece $n.e$ (including the piece $0.e$), just in case it can be represented as $\{x|n.e \leq x < (n+1).e\}$. A piece of R^2 will be represented by $(n.e, m.e)$. It is easy to see that in R:

(i) The statement "$(\forall x)(\exists y)(x \neq y)$" is True. Suppose x belongs to a piece $n.e$. Then it is sufficient to pick for y a piece that is different from it. Because then, no matter what x we pick $n.e \leq x < (n+1).e$, x will be different from y.
(ii) The statement "$(\forall x)(\exists y)(y = x/2)$" is Undecided. This may sound strange at first, but, applying the definitions leaves no other choice. Suppose that x corresponds to a piece $\{x|n.e \leq x < (n+1).e\}$, then it follows that $(n/2).e \leq x/2 < ((n+1)/2).e$. According to whether n is even or odd, $x/2$ will end up in a specific piece, say P. Thus one would think the statement is true. But, it has to be true for *every* choice in $n.e$ and P, so, it is sufficient to pick a value of y, different from $x/2$ (classically speaking), to have the statement false. Hence, the statement is Undecided.

Thus, one has also to be prepared to accept that many statements, even restricted to part(s) of the reals, will be Undecided. The conclusion seems to be that we are left with a very weak theory. However, I want to leave room for another conclusion (albeit without argumentative support): the division of R into equal pieces is not the best choice there is. There is perhaps one

[9] The full details can be found in my [1994b]. The basic technique has been developed by Graham Priest in his [1991].

small argument: in the construction outlined above, points (not *t*-points) are not fixed objects, that is, any *t*-point can be the center of a point. It does not make sense to carve up the plane into fixed regions that are to be interpreted as points. As a matter of fact, the whole thing seems to be much more (discretely) fluid than expected. What we therefore must look for - and I present it here as an open question - is a carving up of the reals that can take this feature into account.

3.6. *Getting vagueness in*

Peter Forrest in his [1995] writes the following when discussing the problem of finding a *representation* of a discrete space:

> "If we take it literally [i.e., the regular tile paradigm, my addition] it would require the points to have extension. But that seems to imply that part of a point is some distance from some other part of a point. But we are defining distance in terms of a relation between points. We should not therefore take the tile paradigm literally, but rather as just a way of *representing* one possible discrete geometry using Euclidean space. While I do not know the precise diagnosis of Pythagoras trouble, it shows at least that the true discrete geometry cannot be represented *both* so that points get represented by regularly shaped regions, *and* so that adjacent points are represented by regions sharing a line boundary." (p.331)

The argument may sound familiar. If a hodon h of a discrete space is a truly specified entity, say a square tile, then is it not unavoidable that all kinds of questions can be asked: Does it have sides? Does it have a diagonal? Does it have parts? Can two squares overlap? Now either one states that such kinds of questions cannot be asked, or one makes sure that the answers are undecided. Now this sounds remarkably like the way we think about vagueness. To put it otherwise, what would happen if we were to treat the predicate "being a hodon", applied to regions of space, call it H, as a vague predicate. That is, for regions X of space that are sufficiently large, we will gladly state that "It is not the case that $H(X)$". Or suppose we can divide a region in two parts: X_1 and X_2. Then again we will gladly state that "It is not the case that $H(X_1)$ and it is not the case that $H(X_2)$." But, eventually, this process has to stop, though it seems indeed very strange to say that at that or that exact level, no parts can be found and we have therefore reached a hodon.

What I am claiming, is the following. Let us call the theory as presented above, $T(h)$, where h is the size of the hodon in that theory. We can now imagine a set of such theories $T(h_i)$, were $h_i \neq h_j$, for $i \neq j$ and there are M

and N such that $M < h_i < N$, for all i. In short, we have a set of similar theories based on hodons of different sizes. Now apply the well-known supervaluation technique as developed by van Fraassen (and quite similar to the procedure outlined in 3.5). To find out whether a statement A is True, False or Undecided, look at all the separate theories $T(h_i)$. If A is true in all of them, then A is True; if A is false in all of them, then A is False; if A is true in some of them and false in others, then A is Undecided. As an example, if A is the statement "hodons have size a" (where a is a specific number), this will be Undecided, if a corresponds to at least one of the h_i. For in the theory $T(h_i)$, this statement will be true, but in a theory $T(h_j)$, where the size of h_j is smaller than the size of h_i, this will be false. Hence it is Undecided.

Summarizing, there is no intrinsic difficulty to have a discrete geometry where the hodons are vague entities. From this perspective, in a particular theory $T(h)$, h need not be the "true" hodon.

4. *Afterthoughts*

I may have given the wrong impression at the beginning of this paper. I made it seem as though there are only two viewpoints possible: either you go for the continuum, or else you go discrete. This is, of course, absolutely not the case. In fact, there have been some writings lately, that suggests other approaches. I will not discuss these here, but restrict myself to one comment. Some of these solutions make use of sophisticated techniques, such as non-standard analysis and the like. A beautiful example is to be found in Harrison [1996]. Basically and extremely simplified, one starts with a model of the real numbers R. Then, non-standard numbers are added (typically, infinitely large numbers ω) that lead to infinitesimals, by taking the reciprocals, i.e., $1/\omega$. On the one hand, it allows you to talk about the continuum as made up of infinitesimals which seems rather pleasing as a solution to Zeno's paradoxes[10]. But, on the other hand, one should not forget that these infinitesimals have strange properties. To mention but one: they are smaller than *any* positive real number. It is rather typical for approaches of this kind, that quotation marks are often used: do not say finite, but say "finite". I therefore agree with Alper & Bridger in their [1997] that this is improper use of language: "They use the word "finite" in its technical sense, but leave the impression that it retains its intuitive meaning." (p. 152) To be precise, sets that are internally finite contains at least all the real numbers.

An issue I have not dealt with in this paper - and that is treated in extenso by Forrest in his [1995] - is the question of the reality of a discrete

[10] A very accessible presentation of this approach is to be found in McLaughlin [1994].

geometry. Suppose that nature indeed is built up from elementary geometrical units, such as squares or tiles in a plane. Two questions immediately pop up: (i) is it possible to determine their size, (ii) do they carry any direct physical meaning? Let me just say a few words about the second question. I am tempted to say that all attempts to equate the tiles *directly* to some physically meaningful concept are to be avoided. In 3.3 I already expressed my doubts about the interpretation of the ratio hodon/chronon as a speed on the basis of the argument that, when we talk physics, we talk about points and lines, not about *t*-points and *t*-lines. But hodons and chronons are on the level of *t*-points and *t*-lines. If this argument holds good, then it follows necessarily that I must have even more doubts about equating the ratio hodon/chronon with the velocity of light.

There is, in addition, a historical argument to support these doubts: when one glances through the historical study of Kragh and Carazza [1994], one sees that all such attempts have failed. Especially, the idea to equate the speed of light with one hodon per chronon, seems to lead to all kinds of difficulties.

A third remark is that the proposal outlined here, has an interesting intuitionistic feature about it. Given two points P and Q, then it is not the case that either $P = Q$ or $P \neq Q$, as it is possible that P and Q partially overlap. This sounds awfully familiar to the intuitionists' treatment of the reals, where it is also not the case in general that for two real numbers r and r' either $r = r'$ or $r \neq r'$. I am unable at the present moment to decide whether this is more than a superficial similarity.

As a concluding remark, it is worth emphasizing that, even if basic flaws are to be found in this proposal, it does show that a discrete approach is both conceptually possible and mathematically non-trivial, which is, philosophically speaking, more important. In the words of Salmon ([1980], p. 66): "..., in order to make good on the claim that space and time are genuinely quantized, it would be necessary to provide an adequate geometry based on these concepts. I am not suggesting that this is impossible, but it is no routine mathematical exercise, ...".

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REFERENCES

ALLIS, V. & T. KOETSIER: On Some Paradoxes of the Infinite. *The British Journal for the Philosophy of Science*, Vol. 42, 1991, pp. 187-194.

ALLIS, V. & T. KOETSIER: On Some Paradoxes of the Infinite II. *The British Journal for the Philosophy of Science*, Vol. 46, 1995, pp. 235-247.

ALPER, Joseph S. & Mark BRIDGER: Mathematics, Models and Zeno's Paradoxes. *Synthese*, vol. 110, 1997, pp. 143-166.

EARMAN, John & John D. NORTON: Infinite Pains: The Trouble With Supertasks. In: Adam MORTON & Stephen P. STICH (eds.), *Benacerraf and his Critics*. Blackwell, Oxford, 1996, pp. 231-261.

FARIS, J.A.: *The Paradoxes of Zeno*. Ashgate (Avebury), Aldershot, 1996.

FORREST, Peter: Is Space-Time Discrete or Continuous? - An Empirical Question. *Synthese*, Vol. 103, no.3, 1995, pp. 327-354.

GALE, Richard M. (ed.): *The Philosophy of Time. A Collection of Essays*. Harvester Press, Sussex, 1978.

HARRISON, Craig: The Three Arrows of Zeno. Cantorian and Non-Cantorian Concepts of the Continuum and of Motion. *Synthese*, Vol. 107, 1996, pp. 271-292.

KRAGH, Helge & Bruno CARAZZA: From Time Atoms to Space-Time Quantization: the Idea of Discrete Time, ca 1925-1936. *Studies in the History and the Philosophy of Science*, Vol. 25, no. 3, 1994, pp. 437-462.

McLAUGHLIN, William I.: Resolving Zeno's Paradoxes. *Scientific American*, vol. 271, no. 5, 1994, pp. 66-71.

PRIEST, Graham: Minimally Inconsistent LP. *Studia Logica*, Vol. L, n. 2, 1991, pp. 321-331.

SAINSBURY, R.M.: *Paradoxes (second edition)*. Cambridge University Press, Cambridge, 1995.

SALMON, Wesley C.: *Space, Time, and Motion. A Philosophical Introduction*. (second edition, revised). University of Minnesota Press, Minneapolis, 1980.

VAN BENDEGEM, Jean Paul: Zeno's Paradoxes and the Weyl Tile Argument, *Philosophy of Science*, 54, 2, 1987, pp. 295-302.

VAN BENDEGEM, Jean Paul: Ross' Paradox is an Impossible Super-Task. *The British Journal for the Philosophy of Science*, Vol. 45, 1994a, pp. 743-748.

VAN BENDEGEM, Jean Paul: Strict Finitism as a Viable Alternative in the Foundations of Mathematics. *Logique et Analyse*, vol. 37, 145, 1994b (date of publication: 1996), pp. 23-40.

WEYL, Hermann: *Philosophy of Mathematics and Natural Sciences*. Princeton University Press, Princeton, 1949.

WHITROW, G.J.: *The Natural Philosophy of Time*. (second edition). Oxford University Press, Oxford, 1980.