We go to the end of the interval $(\alpha \ldots \beta)$, which has been given to us arbitrarily and in which $\alpha < \beta$; the first two numbers of our sequence (4) which lie in the interior of this interval (with the exception of the boundaries), can be designated by α', β', letting $\alpha' < \beta'$; similarly let us designate the first two numbers of our sequence which lie in the interior of $(\alpha' \ldots \beta')$ by α'', β'', and let $\alpha'' < \beta''$; and in the same way one constructs the next interval $(\alpha''' \ldots \beta''')$, and so on. Here therefore α', α'' ... are by definition determinate numbers of our sequence (4), whose indices are continually increasing; the same goes for the sequence β', β'' ... ; furthermore, the numbers α', α'', ... are always increasing in size, while the numbers β', β'', ... are always decreasing in size. Of the intervals $(\alpha \ldots \beta)$, $(\alpha' \ldots \beta')$, $(\alpha'' \ldots \beta'')$, ... each encloses all of those that follow.—Now here only two cases are conceivable.

Either the number of intervals so formed is finite; in which case, let the last of them be $(\alpha^{(\nu)} \ldots \beta^{(\nu)})$. Since in its interior there can be at most one number of the sequence (4), a number η can be chosen from this interval which is not contained in (4), thereby proving the theorem for this case.—

Or the number of constructed intervals is infinite. Then the numbers α, α', α'', ..., because they are always increasing in size without growing into the infinite, have a determinate boundary value α^∞; the same holds for the numbers β, β', β'', ... because they are always decreasing in size. Let their boundary value be β^∞. If $\alpha^\infty = \beta^\infty$ (a case that constantly occurs with the set (ω) of all real algebraic numbers), then one easily persuades oneself, if one only looks back to the definition of the intervals, that the number $\eta = \alpha^\infty = \beta^\infty$ *cannot* be contained in our sequence;[1] but if $\alpha^\infty < \beta^\infty$ then every number η in the interior of the interval $(\alpha^\infty \ldots \beta^\infty)$ or also on its boundaries satisfies the requirement that it not be contained in the sequence (4).—

The theorems proved in this article admit of extensions in various directions, of which I shall mention only one here:

'If $\omega_1, \omega_2, \ldots \omega_n, \ldots$ is a finite or infinite sequence of numbers which are linearly independent from one another (so that no equation of the form $a_1\omega_1 + a_2\omega_2 + \ldots + a_n\omega_n = 0$ is possible with integral coefficients which do not all vanish) and if one imagines the set (Ω) of all those numbers Ω which can be represented as rational functions with integral coefficients of the given numbers ω, then in every interval $(\alpha \ldots \beta)$ there are infinitely many numbers which are not contained in (Ω).'

In fact one persuades oneself through a method of proof similar to that in §1 that the set (Ω) can be conceived in the sequential form

$$\Omega_1, \Omega_2, \ldots \Omega_\nu, \ldots$$

from which, in view of §2, the correctness of the theorem follows.

[1] If the number η were contained in our sequence, then one would have $\eta = \omega_p$, where p is a definite index. But this is not possible, for ω_p does *not* lie in the interior of the interval $(\alpha^{(p)} \ldots$

A quite special case of the theorem cited here (in which the sequence $\omega_1, \omega_2, \ldots \omega_n, \ldots$ is finite and the degree of the rational functions, which yield the set (Ω), is stipulated in advance) has been proved, with recourse to Galoisian principles, by Herr B. Minnigerode (See *Math. Annalen*, Vol. 4, p. 497).

B. THE EARLY CORRESPONDENCE BETWEEN CANTOR AND DEDEKIND

Cantor and Dedekind exchanged letters over a period of many years. The mathematical portions of some of their letters were published by Emmy Noether and Jean Cavaillès in 1937; the mathematical portions of others were published (with many errors of transcription) by Ernst Zermelo in his edition of Cantor's writings (*Cantor 1932*). The Cantor *Nachlass*, containing the original letters from Dedekind, appears to have been destroyed in the Second World War. The letters from Cantor and drafts of some of the letters from Dedekind were taken to America by Emmy Noether, and are now kept at the library of the University of Evansville, Evansville, Indiana. (For an account of the history and the contents of the Evansville collection, see *Grattan-Guinness 1974*.) Portions of the non-mathematical correspondence, not published by Noether and Cavaillès or by Zermelo, are reproduced in *Grattan-Guinness 1974* and in the appendices to *Dugac 1976*.

The selection that follows is a translation of the correspondence published by Noether and Cavaillès, omitting only Cantor's letter to Dedekind of 22 December 1879. That letter gives a counterexample to a theorem of Appell in trigonometric series, and is of little importance today. (Cantor's late correspondence with Dedekind in 1899 is translated below as Selection E.) The correspondence spans the period from April 1872 (immediately after the publication of the theories of Dedekind and Cantor on the irrational numbers) to November 1882 (immediately before the publication of Cantor's *Foundations of a general theory of manifolds*). This was the period during which Cantor made his greatest discoveries in the theory of sets, and his correspondence with Dedekind provides rare documentation of the way in which a new mathematical theory was discovered.

The translation is by William Ewald; references should be to the dates of the correspondence.

Cantor to Dedekind

Halle a/S, 28 April 1872

I thank you most warmly for kindly sending me your treatise on continuity and been training myself in a particular conception of this subject, and I have convinced myself that my conception substantially agrees with yours; the only difference is in the *conceptual introduction* [*begrifflichen Einführung*] of the numerical quantities. I agree wholeheartedly that the essence of continuity lies in what you emphasize.

Halle, 29 Nov. 73

Allow me to put a question to you. It has a certain theoretical interest for me, but I cannot answer it myself; perhaps you can, and would be so good as to write me about it. It is as follows.

Take the totality [Inbegriff] of all positive whole-numbered individuals n and designate it by (n). And imagine say the totality of all positive real numerical quantities x and designate it by (x). The question is simply, Can (n) be correlated to (x) in such a way that to each individual of the one totality there corresponds one and only one of the other? At first glance one says to oneself no, it is not possible, for (n) consists of discrete parts while (x) forms a continuum. But nothing is gained by this objection, and although I incline to the view that (n) and (x) permit no one-to-one correlation, I cannot find the explanation which I seek; perhaps it is very easy.

Would one not be inclined at first glance to maintain that (n) cannot be correlated one-to-one with the totality $\left(\frac{p}{q}\right)$ of all positive rational numbers $\frac{p}{q}$? And yet it is not difficult to show that (n) can be correlated one-to-one not only with this totality, but with the more general

$$(a_{n_1, n_2, \ldots n_\nu})$$

where $n_1, n_2, \ldots n_\nu$ are unrestricted positive integer indices in arbitrary number ν.

Halle, 2 December 73

I was exceptionally pleased to receive your answer to my last letter. I put my question to you because I had wondered about it already several years ago, and was never certain whether the difficulty I found was subjective or whether it was inherent in the subject. Since you write that you too are unable to answer it, I may assume the latter.—In addition, I should like to add that I have never seriously occupied myself with it, because it has no special practical interest for me. And I entirely agree with you when you say that for this reason it does not deserve much effort. But it would be good if it could be answered; e.g. if it could be answered with no, then one would have a new proof of Liouville's theorem that there are transcendental numbers.

Your proof that (n) can be correlated one-to-one with the field of all algebraic numbers is approximately the same as the way I prove my contention in the last letter. I take $n_1^2 + n_2^2 + \ldots + n_\nu^2 = \mathfrak{N}$ and order the elements accordingly.

Is it not excellent that, as you have characteristically stressed, one can speak of the nth algebraic number, so that every one appears once in the sequence?

As you quite rightly remark, our question admits the following formulation: 'Can (n) be one-to-one correlated with a totality:

$$(a_{n_1, n_2, \ldots})$$

where $n_1, n_2, \ldots$ are unrestricted, positive integral indices in infinite number.'

Halle, 7[th] December 73

In the last days I have had the time to pursue more thoroughly the conjecture I spoke to you about; only today do I believe myself to have finished with the thing; but if I should be deceiving myself, I should certainly find no more indulgent judge than you. So I take the liberty of presenting to your judgement what I have just written down in the imperfection of the first draft.

Assume that all positive [real] numbers $\omega < 1$ can be brought into the sequence:

(I) $$\omega_1, \omega_2, \omega_3, \ldots, \omega_n \ldots.$$

Let ω_α be the next largest term following ω_1 in the sequence, ω_β the next largest, and so on. One sets: $\omega_1 = \omega_1^1$, $\omega_\alpha = \omega_1^2$, $\omega_\beta = \omega_1^3$ etc. One extracts from (I) the infinite sequence:

$$\omega_1^1, \omega_1^2, \omega_1^3, \ldots, \omega_1^n, \ldots$$

In the sequence that remains, one designates the first term by ω_2^1, the next greater following term by ω_2^2, etc. One extracts the sequence:

$$\omega_2^1, \omega_2^2, \omega_2^3, \ldots \omega_2^n, \ldots$$

If one continues in this way, one sees that the sequence (I) can be decomposed into the infinitely many:

(1) $$\omega_1^1, \omega_1^2, \omega_1^3, \ldots, \omega_1^n, \ldots$$

(2) $$\omega_2^1, \omega_2^2, \omega_2^3, \ldots, \omega_2^n, \ldots$$

(3) $$\omega_3^1, \omega_3^2, \omega_3^3, \ldots, \omega_3^n, \ldots$$

in each of which the terms increase continuously from left to right; we have:

$$\omega_k^\lambda < \omega_k^{\lambda+1}.$$

Now take an interval $(p \ldots q)$ so that no term of the sequence (1) lies in it; say within $(\omega_1^1, \ldots, \omega_1^2)$. Now, all terms of say the second sequence or the third could lie outside $(p \ldots q)$; however, there must be some sequence, which I shall call the k^{th}, such that not all of its terms lie outside $(p \ldots q)$ (for otherwise the numbers within $(p \ldots q)$ would not be contained in (I), contrary to the hypothesis). Then one can fix an interval $(p' \ldots q')$ within $(p \ldots q)$ so that all terms of the k^{th} sequence lie outside it. But then $(p' \ldots q')$ behaves in the same way with respect to the previous sequences; so eventually there must appear a k'^{th} sequence not all of whose members lie outside $(p' \ldots q')$, and then one takes inside $(p' \ldots q')$ a third interval $(p'' \ldots q'')$ so that all members of the k'^{th} sequence lie outside it.

So one sees that it is possible to form an infinite sequence of intervals:

$$(p \ldots q),\ (p' \ldots q'),\ (p'' \ldots q''),\ \ldots$$

such that each includes its successors, and such that they are related as follows to our sequences (1), (2), (3) . . . :

The members of the 1^{st}, 2^{nd}, . . . $k-1^{st}$ sequence lie outside $(p \ldots q)$

The members of the k^{th} . . . $(k'-1)^{st}$ sequence lie outside $(p' \ldots q')$;

The members of the k'^{th} . . . $(k''-1)^{st}$ sequence lie outside $(p'' \ldots q'')$.

Now, we can always find *at least* one number which lies inside each of these intervals; I shall call it η. One sees at once that this number η, which is clearly $\substack{>0\\<1}$, cannot be contained in any of our sequences (1), (2), . . ., (n). Thus from the assumption that all numbers $\substack{>0\\<1}$ are contained in (I), one arrives at the contrary conclusion that we can find a definite number $\eta \substack{>0\\<1}$ that is *not* in (I); consequently the assumption was incorrect.

So I finally believe myself to have found the reason why the totality designated by (x) in my earlier letters *cannot* be correlated one-to-one with the totality designated by (n).

Halle, 9 December 73

I have already found a simplified proof of the theorem just proved, so that the decomposition of the sequence (I) into (1), (2), (3), . . . is no longer necessary.

I show directly that if I start with a sequence

(I) $$\omega_1,\ \omega_2,\ \ldots,\ \omega_n,$$

then in *every* given interval $(\alpha \ldots \beta)$ I can determine a number η that is not contained in (I). From this it follows at once that the totality (x) cannot be correlated one-to-one with the totality (n); and I infer that there exist essential differences [Wesensverschiedenheiten] among the totalities and value-sets [Inbegriffen und Werthmengen] that I was until recently unable to fathom.

Now I must ask your forgiveness for having taken so much of your time with this question.

Halle, 10 December 73

Confirming the receipt of your friendly lines of the 8^{th} of December, allow me to assure you that nothing can give me more pleasure than to have been lucky enough to arouse in you an interest for certain questions of analysis. And permit me to add that nothing can spur me to further efforts more than this; I should like to ask that you send me your comments in the future as well. Won't the general idea of $\omega_1, \omega_2, \ldots, \omega_\nu, \ldots$ yield good results for your theory of algebraic numbers[a]

Berlin, 25 December 73

Although I did not yet wish to publish the subject I recently for the first time discussed with you, I have nevertheless unexpectedly been caused to do so. I communicated my results to Herr Weierstrass on the 22^{nd}; however, there was no time to go into details; already on the 23^{d} I had the pleasure of a visit from him, at which I could communicate the proofs to him. He was of the opinion that I must publish the thing at least in so far as it concerns the algebraic numbers. So I wrote a short paper with the title: *On a property of the set of all real algebraic numbers*,[b] and sent it to Professor Borchardt to be considered for the *Journal für Math.*

As you will see, your comments (which I value highly) and your manner of putting some of the points were of great assistance to me. I wished to communicate this to you.

Berlin, 27 December 73

As a result of our recent correspondence, writing to you has become so natural that, without thinking about it, I jot down my replies and almost forget to apologize for their frequency. Perhaps it is caused by the similarity of our interests, and because the publicly beneficial development of science is dear to both our hearts.

The restriction which I have imposed on the published version of my investigations is caused in part by local circumstances (about which I shall perhaps later speak with you orally) and in part because I believe that it is important to apply my ideas at first to a single case (such as that of the real algebraic

[a] [Indeed, in his work *Über die Permutationen des Körpers aller algebraischen Zahlen* (*Ges. Werke*, Vol. ii, p. 278) Dedekind applies the theorem; but the existence of a finite basis makes well-ordering unnecessary for finite algebraic number fields.—Noether and Cavaillès.]

[b] [*Cantor 1874*, translated above.]

numbers); the extensions (whose possibility I can already see in plenty) ought then to cause no great trouble, and it will not much matter whether I make them or somebody else. So after a short introduction I have written two §§; in the first it is shown that the totality of real algebraic numbers can be correlated one-to-one with the totality of positive integers; in the second that when we have a sequence $\omega_1, \omega_2, \ldots, \omega_n, \ldots$ one can define numbers η in every interval that are not contained in it.

Dedekind's notes on the letters of 1873

1873.11.29

Herr G. Cantor (Halle) presented me with the question, whether the totality (n) of all positive whole-numbered individuals n (natural numbers) can be correlated with the totality (x) of all positive real numerical quantities x in such a way that to each individual of the one totality there corresponds one and only one of the other?—He ends with the words:

'Would one not be inclined at first glance to maintain that (n) cannot be correlated one-to-one with the totality $\left(\frac{b}{a}\right)$ of all positive rational numbers $\frac{b}{a}$? And yet it is not difficult to show that (n) can be correlated one-to-one not only with this totality, but with the more general

$$(a_{n_1, n_2, \ldots n_\nu})$$

where $n_1, n_2, \ldots n_\nu$ are unrestricted positive integer indices in arbitrary number ν.'

To this I replied by return of post that I could not answer the first question, but at the same time stated and fully proved the theorem that even the totality of all algebraic numbers can be correlated in the stated manner with the totality (n) of all natural numbers. (Shortly thereafter, this theorem and proof appeared almost word-for-word in Cantor's paper in *Crelle*, Vol. 77, even with the use of the artificial expression height [*Höhe*], but with the weakening, against my recommendation, that only the totality of all *real* algebraic numbers is considered.) But the opinion I expressed that the first question did not deserve too much effort because it has no particular practical interest was conclusively refuted by Cantor's proof of the existence of transcendental numbers (*Crelle*, Vol. 77).

1873.12.2

C. points out the importance of the first question: if it is answered negatively then the existence of transcendental numbers (Liouville) can be proved in a new way. He continues: 'Your proof that (n) can be one-to-one correlated with the field of all algebraic numbers is approximately the same as the way I prove my contention in the last letter. I take $n_1^2 + n_2^2 + \ldots + n_\nu^2 = \mathfrak{N}$ and order the elements accordingly. Is it not excellent that, as you have characteristically stressed, one can speak of the n^{th} algebraic number, so that every one appears once in the sequence? As you quite rightly remark, our question admits the following formulation: "Can (n) be one-to-one correlated with a totality:

$$(a_{n_1, n_2, \ldots})$$

where $n_1, n_2, \ldots$ are unrestricted, positive integral indices in infinite number." '

1873.12.7

C. communicates to me a rigorous proof, found on the same day, of the theorem that the totality of all positive numbers $\omega < 1$ *cannot* be one-to-one correlated with the totality (n).

I answer this letter, received on 8 December, on the same day with congratulations for the fine success; at the same time, I rephrase much more simply the core of the proof (which was still quite complicated). This presentation too went into Cantor's paper (*Crelle* Vol. 77) almost word-for-word; to be sure, the phrase I use, 'according to the principle of continuity', is avoided at the spot in question (p. 261, ll. 10–14)!

1873.12.9

C. writes to me in a hurry that he has found a simplified proof of the theorem. Since he does not mention my letter, it must have arrived later.

1873.12.10

C. confirms the receipt of my letter of 8 December, without mentioning the simplified presentation of the proof contained therein; thanks me for my interest in the subject.

1873.12.25

C. writes (from Berlin) that he has (on the prompting of Weierstrass) written a short article with the title: *On a property of the set of all real algebraic* *numbers*. 'As you will see, your remarks (which I value highly) and your manner of putting some of the points were of great assistance to me.'

I answer by return of post with the advice to drop the restriction to the field of all *real* algebraic numbers.

1873.12.27

C. writes (from Berlin): 'The restriction which I have imposed on the published version of my investigations is caused in part by local circumstances (about which I shall perhaps later speak with you orally) and in part because I believe that it is important to apply my ideas at first to a single case (such as that of the real algebraic numbers).'

I have never received an explanation of the 'Berlin circumstances'; we also later never dealt with the article (*Crelle*, Vol. 77).

Cantor to Dedekind

Halle, 5 January 74

As for the question with which I have recently occupied myself, it occurs to me that the same train of thought also leads to the following question:

Can a surface (say a square including its boundary) be one-to-one correlated to a line (say a straight line including its endpoints) so that to every point of the surface there corresponds a point of the line, and conversely to every point of the line there corresponds a point of the surface?

It still seems to me at the moment that the answer to this question is very difficult—although here too one is so impelled to say *no* that one would like to hold the proof to be almost superfluous.

Halle, 28 Jan. 74

In the *Comptes rendues* of last year I find an article by Hermite in which a simultaneous development of n exponential quantities e^{ax}, e^{bx}, ... e^{nx} is treated with the most skilful handling of the analysis; and what seems most important, he bases on this a completely rigorous proof of the transcendence of the number e. Hermite confesses to have spent a great deal of time on the proof of the transcendence of π, but for this number he surrenders with the remark that he would be delighted if somebody else were to succeed.

Halle, 18 May 74

The wish to speak with you about scientific subjects and to have more personal contact with you makes me want to visit you occasionally in Brunswick this summer.

... When you get around to answering me, I should be grateful to hear whether you had the same difficulty as I in answering the question I sent to you in January about the correlation of a line and a surface, or whether I am deceiving myself. In Berlin a friend to whom I presented the same problem told me the subject was somewhat absurd, because it is self-evident that two independent variables cannot be reduced to one.

Dedekind to Cantor

... The objection of Herr I. which you communicated to me cannot, if I have correctly understood it, affect my presentation; the principle of continuity set forth in §3, p. 18 [of *Continuity and irrational numbers*] is of course only to be conceived as the necessary *completion* of the laws I, II, III stated in §2, p. 15; and II states exactly what you or Herr I. seem to miss. The objection would probably not have been made had I placed number IV before the principle on p. 18, as I do with the analogous laws in §5, p. 25. Or have I misunderstood the objection? Then I should be grateful for an explanation.

Brunswick, 11 May 1877

Cantor to Dedekind

Halle, 17 May 1877

Thank you for your answer. I must confess that on page 25 of your paper on irrational numbers you adduce, with I, II, III and IV,[c] properties that are thoroughly characteristic for the domain of all real numbers, so that *no* other value-system of real numbers simultaneously shares all these properties with that system.

However, allow me to remark that perhaps the stress which at various points in your paper you *expressly* lay on property IV as being the *essence* of continuity must lead to misunderstandings which, in my opinion, without that emphasis on IV (as the proper essence) could not accrue to your theory. In particular you say in the preface that my axiom fully agrees with what you state in §3

[c] [I: Order; II: Density; III: Cut; IV: To every cut there corresponds a number. *Dedekind 1872*, §5.]

as the *essence* of continuity. But by that you understand the same property that is designated by IV on p. 25; but this property also holds of the system of all integers, which can however be regarded as a prototype of discontinuity.

In the interest of the subject, which has become important to me as well, I ask you, when you have the time, to go into my doubts more closely.

P.S. I tell myself that you lay special emphasis on IV because this property distinguishes the complete domain of numbers from the domain of all rational numbers; however it seems to me for the above reasons that one cannot give property IV the name 'essence of continuity' as you do.

Dedekind to Cantor

After your last letter it appears to me that we run the danger of arguing more about words than about things. Every attentive reader of my paper will certainly understand that my view about continuity is as follows: Domains with an opposition [Gegensätzlichkeit] and completeness of their elements—as completeness is expressed by I and II in §1 p. 14, §2 p. 15, §5 p. 25 (III is a consequence of I and only introduced to prepare the way for IV)—are not yet necessarily continuous domains; such domains obtain the property of continuity from the addition of property IV (on p. 18 (unnumbered) and on p. 25) and only from this property. And so this property is called the essence of continuity.

You tell me in your card of the 10th that my definition of continuity is not complete, and make a suggestion for removing this defect. I decline this suggestion, and call your attention to II, which contains what you are seeking. You concede in your last letter that my definition in fact overlooks nothing; e.g. when I say, 'Domains which possess properties I and II are called continuous if they also possess property IV' you have (if I understand your last letter correctly) nothing to object against the *completeness* of such an explanation. But it seems that you would prefer it if property II were moved from the relative clause into the conditional clause: 'Domains, whose elements possess an opposition [Gegensätzlichkeit] of the type defined by I are called continuous if they also possess properties II *and* IV'; and you worry that my exclusive *stressing* of IV as *the* property in which the *essence* of continuity is expressed could lead to misunderstandings. I do not share this concern; I am firmly convinced that every attentive reader of my paper understands my opinion as I have expressed it above; and so the example you adduce of the system of all rational integers fails to provide the motive for an objection. Moreover, as for the above rearrangement of the definition, I cannot say that it pleases me. And I infer from your postscript that you too, as soon as you were to attempt to *rework* my paper in this way, would certainly concede that this paper (which has as its chief object the advance of arithmetic from rational to irrational) would, with regard to precise exposition, lose its proper point—which consists solely in the emphasis on IV, since II is already available in the non-continuous rational domain. But if the

revised definition pleases somebody better, I have nothing to say against its *legitimacy*—that is, not if it should be of advantage for certain investigations. But my original formulation pleases me much better, and I think it is more expedient in treating the essence of continuity to lay the emphasis solely on IV and to discuss property II earlier, before continuity or discontinuity is at issue. In any case, I utterly dispute the *necessity* of the reformulation of the definition; if one were really to demand this, one could just as well raise the question (with which I have already occupied myself) whether it is not expedient to move property I from the relative clause into the conditional clause as well. This is not at all an uninteresting question, but it would lead me too far astray if I were to go into it. I genuinely believe that our opinions diverge at most about expediencies, not about necessities; so a continuation of the debate will probably not yield very much.

Brunswick, 18 May 1877

Cantor to Dedekind

Halle, 20 June 1877

Thank you for your letter of 18 May. I completely agree with its contents; and I acknowledge that the difference in our points of view was merely external. And now I approach you again today with a request. (You see that our shared theoretical interests have a disadvantage for you: perhaps I bother you more than you would like.)

I should like to know whether you consider an inference-procedure that I use to be arithmetically rigorous.

The problem is to show that surfaces, bodies, indeed even continuous structures of ρ dimensions can be correlated one-to-one with continuous lines, i.e. with structures of only *one* dimension—so that surfaces, bodies, indeed even continuous structures of ρ dimensions have the same *power* [*Mächtigkeit*] as curves. This idea seems to conflict with the one that is especially prevalent among the representatives of modern geometry, who speak of simply infinite, doubly, triply, . . . ρ-fold infinite structures. (Sometimes you even find the idea that the infinity of points of a surface or a body is obtained by as it were squaring or cubing the infinity of points of a line.)

Since structures of the same number of dimensions can be related to one another *analytically*, it seems to me that those more general questions should be put in the following purely arithmetical form:

'Let $x_1, x_2, \ldots x_\rho$ be ρ independent real variables such that each can assume all values ≥ 0 and ≤ 1. Let y be a $\rho + 1^{th}$ real variable with the same range $\begin{pmatrix} y \geq 0 \\ \leq 1 \end{pmatrix}$.