Junior Mathematics Schedule

rev. August 9, 2024

Overview

1. Selections from Galileo's *Two New Sciences*, NE 68–93.
2. Essays of Leibniz, with notes:
   (a) "An Approach to the Arithmetic of Infinites."
   (b) "A New Method."
   (c) "On Recondite Geometry."
   (d) "True Proportion" (optional).
   (e) Two essays on the Hanging Chain (optional—not in manual).
3. Dedekind, *Continuity and Irrational Numbers*
4. "Cantor's Transfinite Set Theory" (optional).
5. Newton, *Principia*:
   (a) Book I, Sections 1–3.
   (b) Book I, Section 11: Propositions 57–69 (optional).
   (c) Book III: Preface, Phenomena, Propositions 1–13.
   (d) General Scholium.

Notes: Materials needed for Junior Mathematics include Galileo's *Two New Sciences*, Newton's *Principia*, and a first semester manual (which includes the Leibniz essays and notes, the Dedekind, and the account of Cantor's set theory). Many have also found notes on the Newton helpful, even at times necessary. The most commonly consulted works include Dana Densmore's *Central Argument*, Robert Bart's *Notes*, and Percival Frost's *Principia*. The Section 11 propositions are usually done with "Two, Three, and Multiple Body Problems", written by Chester Burke. In the more detailed schedule below, there is also occasional mention of a supplement, which includes additional documents that tutors and students may find useful over the course of the year.[1] A more extensive collection of documents was put into an online folder by Michael Dink back in 2016–17.[2]

Some tutors have found it useful to consult the tutor-written manual we used for calculus before adopting Leibniz—the so-called Kutler manual.

For a list of writing assignments from recent archon reports, see page 106 of the supplement. For a list of topics for Leibniz papers, see page 58 of the supplement.

[1] A copy of the supplement is also hyperlinked to this schedule. If you are reading this version, then the word "supplement" should appear in a different color. Pagination of the supplement appears on the upper-right corner of each page.

[2] A zip file of the folder is available at the hyperlink above, but the folder can also be accessed at the link in the digital archives copy of Michael's 2016–17 archon report.

Note: The following schedule lists thirty-six assignments for an expected forty-two classes. Nineteen additional assignments are listed and flagged as optional.

Two New Sciences (Galileo)

> *Note*: What follows is only one approach to the Galileo. It does not include the so-called "wheel paradox" or as much on the phenomenon of motion as older schedules did. For a two-day schedule that includes the wheel paradox, along with a four-day schedule that includes more on motion, see page 5. In another option, one might simply skip the Galileo, to allow more time for other first semester readings.

1. Four excerpts from Aristotle's *Physics* (in manual) and NE 77–82 (through "than its division into a thousand parts.").[1]
2. NE 82–89 (through "even after a thousand discussions?") and NE 92–93 ("But it is time now" through "by assuming the said composition of indivisibles.").

Note: On older schedules the Galileo was followed by Lemmas 1–11 of the *Principia*, which perhaps makes a better fit with Galileo than Leibniz, especially for classes that spend more time on Galileo. In a more recent order of the past, the Galileo was followed by Leibniz, but Leibniz was followed by Newton. Either order means reading the Dedekind second semester.(See page 7 for the start of the *Principia* schedule.)

An Approach to the Arithmetic of Infinites (Leibniz)

> *Note*: In the original sequence of Leibniz readings proposed, this paper was read in tandem with a letter Leibniz wrote to Ehrenfried Walther von Tschirnhaus. See page 86 of the supplement.[2]

3. Title, from the first paragraph through the paragraph ending next to Note 6. Notes 2–3, 6.[3]
4. From the first paragraph after Note 6 to the paragraph ending next to Note 7.[4]

A New Method (Leibniz)

5. Title and first paragraph. Notes 1–3.
6. Second and third paragraphs ("Let a be a given constant quantity" through "find ways to make it easier."). Notes 4–6.
7. Note 7, Part 3. Omit proofs for negative exponents and root rule.[5]
8. Note 7, Part 1, Examples 1–4; Part 2, Problems 1–7 odd.

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[1] Pagination from "National Edition" (NE) found in the margins of the Drake-edited text.

[2] The part of the letter relevant to the Leibniz paper begins at the paragraph "Furthermore, as I go over the rest of your letter. . . ." See page 92 of the supplement.

[3] Notes 1, 4–5 are optional. In the first Leibniz schedule for this reading, the instruction was to have students go to the board to work through the arguments in Notes 2, 3, and 6; and to have students prepare problem 1 in Note 3, and problems 2 and 3 in Note 6.

[4] This omits the demonstrations of the axioms following the last paragraph read.

[5] Doing Part 3 of Note 7 before Parts 1 and 2 makes derivation of the rules prior to their application. An alternative approach would follow the manual in doing Parts 1 and 2 before Part 3.

9. Note 7, Part 1, Examples 5–7; Part 2, Problems 9–23 odd.
10. Note 7, Parts 4 and 5.[1]
11. Fourth paragraph ("Once we have learned this *Algorithm*" through "we could find it from a given property of the tangents of a circle"). Notes 8–9.[2]
12. Last sentence of fifth paragraph ("Let me now give some easier examples") through middle of sixth paragraph ("things that someone skilled in our calculus will henceforth be able to produce in three lines"). Notes 12–19.[3]
13. Later in sixth paragraph ("As an appendix") to the end of the paper. Note 22, Parts 1–5.[4]
14. Note 22, Part 6.

On Recondite Geometry (Leibniz)

15. Title, first three sentences of first paragraph (through "those who try to prove the impossibility of quadrature neglect this distinction"). Notes 1–2.
16. Fourth and fifth sentences of first paragraph ("He recognizes with me that the figures" to "since it supplies the best remedy against irrationalities"). Third, fourth, and fifth paragraphs ("Further, to say something more useful here" to "thus are not comprehended by means of algebraic equations"). Notes 3–10.[5]
17. Sixth paragraph ("Furthermore, because hardly anything can be imagined that is more useful" through "innumerable other things are also derived from this"). Notes 11–17.
18. Seventh and eighth paragraphs ("Finally, so that I may not seem to ascribe too much to myself" to "innumerable transfigurations and equipotencies of figures may arise from this very same thing"). Note 18.[6]
19. Note 19, Parts 1 and 2.[7]
20. Note 19, Part 3.[8]

[1] Parts 6 and 7 of Note 7 are omitted, which involve the finding of tangents. An alternative approach would take a day or two to do them.

[2] A tutor-written note on the fourth paragraph can be found on page 109 of the supplement. A copy of the Fermat paper mentioned in footnote 7 on page 33 can be found on page 112 of the supplement.

[3] The first example of the calculus that Leibniz gives (fifth paragraph) is skipped.

[4] An example in paragraph six ("I will show this with yet another example") is skipped. Also, Parts 1–5 may be too much for a single day. An alternative approach might divide this assignment over two days: Parts 1–2; 3–5. One might also replace Note 22 with a shorter tutor-written note, "De Beaune's problem to Descartes, solved by Leibniz"—see page 14 of the supplement. On page 20 of the supplement can also be found "How to approach an exact value for e," based on Theorem 3 in Part 3.

[5] The rest of the first paragraph and the second paragraph are skipped. A brief elaboration of Note 8 can be found on page 21 of the supplement; and on page 22, a fuller treatment of steps in Notes 8 and 9, from an older version of the notes.

[6] The seventh paragraph might be skipped. And the final paragraph of the paper goes unread.

[7] These parts of Note 19 are sometimes found wanting, especially the formulation and proof of the first fundamental theorem. For a different approach to both theorems, see page 28 of the supplement.

[8] See page 30 of the supplement for an alternative account of algebraic quadratrices, which existed in the manual for two years, is closer to what is found in calculus textbooks, and is

21. Note 19, Part 4 through Example 4.
22. Note 19, rest of Part 4.
23. Note 19, Part 5 through "Transcendence of the logarithmic line."
24. Note 19, rest of Part 5.

Note: Given time and motivated students, at this point one might study the hanging chain problem in two additional papers of Leibniz (not in manual). See page 6 for a four-day schedule of this. A shorter option: Leibniz papers that used to be read on the isochronic line (not in manual; consists of separate readings and comments). See page 6 for a two-day schedule.

Mathematics for Newton's physics (tutor-written)

"Functional Notation and Calculus"

> *Note*: A related text on functions, sometimes read in the past, is Frege's "What is a Function?"

25. First two sections: "Functions and derivatives"; "Finding derivations in functional notation." Problem 1 optional.
26. Last two sections: "The method of substitution and the chain rule"; "Functions of more than one variable and partial derivatives." Problems 2–10 optional.

"Calculus and Newtonian Physics"

27. First paragraph and first two sections: "Velocity and force"; "The phenomenon and force of one falling body." Problems 1–5 optional.
28. Last section: "Projectile motion". Problems 1–4 optional.

Note: Given time and interest, at this point one might read another Leibniz paper (in manual): "On the True Proportion, Expressed in Rational Numbers, of a Circle to a Circumscribed Square." See page 5 for a two-day schedule for this. A one-day alternative is to read the defense of his calculus given by Leibniz in two letters, or in an early manuscript; see page 46 of the supplement for the first and page 51 for the second.

Continuity and Irrational Numbers (Dedekind)[1]

> *Note*: Another option, found on older schedules, is to read Dedekind second semester, after Newton. This means starting *Principia* first semester, either before or after Leibniz. (The *Principia* schedule begins on page 7.)

29. Untitled Preface. Note 1.[2]
30. Chapters I–II.
31. Chapter III.

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preferred by some tutors.

[1] Thanks to André Barbera, the supplement contains two articles that appeared in *Energeia* in 1965 about Dedekind and Euclid. The first, written by Sam Kutler, can be found on page 100; the second, by Eva Brann, can be found on page 102. There is also a longer Kutler essay on Dedekind.

[2] Note 1 is long and not directly connected to Dedekind. An alternative approach would delay or even skip this note, and combine this day with the following one. If delayed one might read it second semester, after the first two lemmas of *Principia*. (See page 7 for more on this.)

32. Chapter IV: first through beginning of sixth paragraph ("Hence the square of every rational number x is either $< D$ or $> D$"). Note 2 through step (16).
33. Chapter IV: sixth through ninth paragraph (stopping just before paragraph that begins "In order to obtain a basis for the orderly arrangement of all *real*, i.e., of all rational and irrational numbers"). Rest of Note 2.
34. Rest of Chapter IV.
35. Chapter V.
36. Chapter VI.[1]

Note: Given time and interest, at this point one might study Cantor's set theory. Alternatively, this study might be done second semester, before or after the Newton. See page 6 for a five-day schedule. One way to end the Leibniz-Dedekind-Cantor sequence is through a brilliant essay by José Benardete, "Continuity and Theory of Measurement."

First semester: optional readings

Two New Sciences

1. NE 68 to middle of 72 (through "inappropriately did add") and from just before 92 to just before 97 ("But it is now time" to "and natural materials.").
2. Four excerpts from Aristotle's *Physics* (in manual). NE 77–82 (through "than its division into a thousand parts.") and NE 92–93 ("But it is time now" through "by assuming the said composition of indivisibles.").

Two New Sciences and *Physics*

1. Three excerpts from the *Physics* (not in manual—see page 1 of supplement): 232b20–233a32; 239b5–240a18; 263a11–263b9. Definition of motion from *Physics* III:1.
2. Optional: Excerpts from Bergson's *Creative Evolution* and *Matter and Memory* (not in manual—see page 7 of supplement).
3. Optional: Excerpt from Aristotle's *Mechanical Problems* (not in manual—see page 4 of supplement).
4. Four (or more) days on NE 68–96, along with excerpts from Aristotle's *Physics* in manual.

On the True Proportion

1. First five paragraphs, ending with "This is the kind of quadrature I am presenting here." Notes 2–9.
2. Continuing from sixth paragraph through middle of ninth paragraph (from "Accordingly I found" through the indented and italicized "If the area of the inscribed square is 2/4, etc."). Notes 11–12.[2]

[1] Chapter VII is omitted but could be done with an additional class.
[2] The rest of the paper is omitted.

The Hanging Chain problem (not in manual)

"On the Line in which a Heavy Body Bends by its own Weight"

1. Title. First through third paragraphs. The italicized "primary Problems" 1–6 but not their solutions. Notes.

"A Solution of the Problem First Proposed by Galileo"

2. Skip first two paragraphs; begin with "An analysis of the chain problem." Read through equation 11. Notes 1–11.
3. Read through equation 20. Notes 12–18.
4. Read to the end. Remaining Notes.

The Isochronic Line Problem

Not in manual—separate readings and comments. Schedule follows pagination in text.

1. "A Brief Demonstration" pages 41–43. "On the Isochronic Line," pages 44–47. Comment 1, pages 145–146. The other comments are optional.
2. Leibniz, "An undated manuscript on the isochronic line," pages 48-51. Omit the "Problem" and its demonstration.

Cantor's Transfinite Set Theory

Note: What follows is a five-day schedule using the tutor-written account in the manual. One could reduce this to one or two days, devoted only to the proofs for the denumerability of the rationals and non-denumerability of the reals, using either Sections 9 and 12 in this treatment or a different text for the purpose (such as the sections from Courant and Robbins, *What is Mathematics?* found on page 59 of the supplement). There is also a separate copy of the text with a supplement of notes.

1. Sections 1–7.
2. Sections 8–11.
3. Sections 12–13.
4. Sections 14–15.
5. Sections 16–17.

End of First Semester

Note: The following schedule lists thirty-six assignments for an expected forty-two classes. Five additional assignments are listed and flagged as optional.

Principia (Newton)

Note: There is no assigned place on this schedule for a helpful, even necessary, review of the definitions and laws that precede the lemmas and read in Junior Laboratory first semester. This might be assigned over winter break along with Lemmas 1 and 2. For notes on this material, from the Junior Laboratory manual, see page 64 of the supplement. There is also a website associated with the Integral Program of St. Mary's College of California that permits viewers to interact with various lemmas and propositions of the *Principia*. The current address of the website is: https://www.geogebra.org/m/YlONul2N.

1. Lemmas 1 and 2.

   Note: At this point, in a practice of long-standing, one might devote the next class (or even two classes) to Archimedes' *On the Measurement of the Circle* (in manual) and Euclid's *Elements* X.1 and XII.2, along with the second paragraph of the scholium after Lemma 11. One might also read or review Note 1 to the Dedekind.

2. Lemma 3 with corollaries.
3. Lemma 4 with corollary. Lemma 5.
4. Lemmas 6 and 7.[1]
5. Corollaries to Lemma 7. Lemma 8.
6. Lemma 9.
7. Lemma 10 with Corollaries 3 and 4.[2]
8. Lemma 11 through Case 1.[3]
9. Lemma 11, Cases 2 and 3. Begin corollaries.
10. Finish corollaries and read entire scholium to Lemma 11.[4]

    Note: More than one archon report suggests spending more time on the Book I propositions, especially the earliest ones, than indicated below (although in earlier times 7–9 were skipped). There is also disagreement about doing or omitting "same otherwise" proofs in general; this schedule draws no attention to them. Unlisted corollaries and scholia are also assumed optional.

11. Proposition 1.
12. Corollaries 1, 2, and 4 of Prop. 1.[5]
13. Proposition 2.

[1] One might assign a review of *Elements* III.16 with Lemma 6 and allow two days here.

[2] Corollary 4 is invoked in I.6 and relevant to III.4. This is also sometimes assigned with a review of Prop. II of "On naturally accelerated motion" from *Two New Sciences* (NE 208 ff).

[3] A supplement here is useful that discusses finite curvature. Possible texts: "Note on Curvature" in Junior Lab manual (see page 74 of supplement); Bart's *Notes* (36 ff); Densmore's *Central Argument* (74; 109); Comenetz's *Calculus* (332 ff). Henry Higuera has also written notes that start with Lemma 11 and end with Prop. 17.

[4] Corollaries 4 and 5 not in Densmore.

[5] Corollary 1 of the Laws might be reviewed here.

14. Proposition 3. Scholium after 3.
15. Proposition 4. Corollaries 1–7, 9.[1]
16. Proposition 5.[2]
17. Proposition 6 and Corollary 1.[3]
18. Remaining corollaries to Prop. 6.[4]
19. Proposition 7.[5]
20. Propositions 8. Scholium after 8. Proposition 9.[6]
21. Lemma 12. Proposition 10 with corollaries. Scholium after 10.[7]

    Note: More than one archon suggests spending less time on the remaining propositions of Bk I than indicated here (11 and 12 are sometimes combined into a single day; while 14–16 are sometimes only enunciated). A usual practice in earlier years omitted 16 and 17, and some tutors have doubted the necessity even of 14 and 15, which go unused in (what we read of) Bk III. Another time-saving suggestion of the past: treat 12 and 13 lightly, or even omit.

22. Proposition 11.[8]
23. Proposition 12.[9]
24. Lemmas 13 and 14. Proposition 13 with corollaries.[10]
25. Propositions 14 and 15, with their corollaries.
26. Proposition 16.
27. Proposition 17.[11]
28. Review and discussion of Propositions 1–17.

    Note: This schedule follows Newton's advice at the beginning of Book III, moving straight to Book III after Proposition 17 of Book I. But a common alternative—and some tutors would argue for its importance—is to precede Book III with a study of Propositions 57–69, since 69 is needed for III.7 and 57–69 form a sequence in itself (section 11 of Book I). A five-day schedule for this sequence can be found on page 9.

29. Preface. Rules.
30. Phenomena. Propositions 1 and 2.[12]

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[1] Corollary 9 is used in III.4, an important proposition.

[2] This proposition is regarded as optional on some older schedules. It is omitted from Densmore.

[3] A shorter alternative would be a class on Proposition 6 with Corollaries 1 and 5, without a second class.

[4] In his 2018–19 archon report, André Barbera notes that in Prop. 6, Cor. 3, "Newton seems to have Euclid III.32 in mind. I do not see this indicated in either Bart or Densmore."

[5] *Elements* III.32 and III.36 might be reviewed here.

[6] Proposition 8 is regarded as optional on some older schedules.

[7] *Conics* I.15, I.21, and I.37 might be reviewed here. For an interesting tutor note (Joe Sachs) on the sequence of propositions from 4 to 10, see page 77 of the supplement.

[8] *Conics* III.48 and III.52 might be reviewed here.

[9] *Conics* III.51 might be reviewed here. Also, to save time, one could simply draw the diagram to show the analogy with the proof in 11 for the ellipse.

[10] *Conics* III.45, I.46, and I.49 (with the translator's note for 49) might be reviewed here. Two notes on Corollary 1 can be found on page 96 and 98 of the supplement.

[11] A note on Prop. 17 can be found on page 99 of the supplement.

[12] This schedule gives perhaps the most compressed assignment possible for the six phenomena that precede the propositions in Book III. One strategy is refer back to the phenomena as they are invoked in the proofs for the propositions. But older schedules devoted three days to

31. Propositions 3 and 4. Scholium.[1]
32. Proposition 5. Corollaries. Scholium.
33. Proposition 6. Corollaries.[2]
34. Proposition 7.
35. Propositions 8–13. Hypothesis after 10.[3]
36. General Scholium after Book III (at end of *Principia*).

> *Note*: At this point, given time (or even with the General Scholium in a single asssignment), one might read an excerpt from Newton's *Optics* (Query 31) that bears on what he writes in the General Scholium. See page 79 of the supplement.

Second semester: optional readings

Principia, Book I, Section 11

> *Note*: In addition to commentary on certain of these propositions in both the Densmore guidebook and the Bart manual, there is a separate set of notes for this sequence written by Chester Burke.

1. Introduction. Proposition 57.
2. Proposition 58.
3. Proposition 64. Read through Proposition 65.
4. Proposition 66. Corollaries 1–6. Browse through all the corollaries.
5. Proposition 69 with its corollaries. Scholium after 69.

End of Second Semester

the phenomena alone (grouping the six in pairs). And a look at Densmore's treatment in her *Central Argument* (devoting nearly fifty pages to them) gives a sense of how much one can make of the phenomena on their own.

[1] Proposition 4 would be the focus this day, and arguably deserves a day of its own.

[2] On older schedules, Propositions 5 and 6 were done together, with all the corollaries of 5 and Corollaries 1, 2, and 5 of 6.

[3] A strategy for this assignment is to restrict the reading and discussion to the enunciations of the propositions. Another approach would focus on Propositions 8 and 13. Still another approach might spend a few days on the sequence of propositions in Book I (71–76) that lead to III.8, and as if culminating in 8.

Other Readings

Aristotle: Three excerpts from the *Physics* on Zeno's paradoxes

232b20–233a32

Since every motion is in time and in every time something can be moved, and since every moved thing can be moved faster and more slowly, then in every time the faster can also be moved more slowly. This being so, time must also be continuous. I call continuous that which is always divisible into divisibles; and since this is the assumption about the continuous, time must be continuous. For since it has been shown that the faster traverses the equal in less time, let there be A, a faster, and B, a slower, and let the slower be moved the magnitude CD in the time FG. Obviously the faster in less of this will be moved the same magnitude; let it have been moved in FH. Again, since the faster in time FH will have traversed the whole CD, the slower in the same time traverses a less; let it be CK. But since the slower, B, in time FH, will have traversed CK, the faster traverses it in less, so that again the time FH will be divided. And this having been divided, also the magnitude CK will be divided in the same ratio. And if the magnitude, also the time. And this will always be the case when anyone alternates from the faster to the slower and from the slower to the faster and uses the thing deduced; for the faster will divide the time, and the slower the length. Then if each alternating step is true, and if at each step a division always occurs, it is clear that every time will be continuous. And at the same time it is clear that every magnitude is continuous; for into the same and equal divisions the time and the magnitude are divided.

And besides, it is clear from the ordinary arguments that if the time is continuous, so also is a magnitude, as long as in the half time a half is traversed, or simply in the less a less; for there will be the same divisions of the time and of the magnitude. And if one of the two is infinite, so also is the other, and *as* the one, so the other; for example, if the time is infinite at the ends, also the length at the ends, if by division, also the length by division, and if in both, also in both the magnitude.

On which account the argument of Zeno assumes falsely the impossibility of traversing the infinite or of touching each of an infinitude in a limited time.

For in two ways is a length or a time, or generally any continuous thing, said to be infinite, with respect either to division or to the ends. Things infinite with respect to quantity, then, it is not possible to touch in a limited time, but those with respect to division, it is possible; for even the time itself is infinite in that way. So that in the infinite and not in the limited time, the infinite turns out to be traversed, and an infinitude to be touched in an infinitude and not in a limited number.

239b5–240a18

Zeno's paradoxes are paralogisms; for if a thing is always at rest, he says, whenever it is in a place equal to itself, and a thing on the move is always under way in the now, then the flying arrow is motionless. But this is false; for time is not composed of indivisible nows, just as no other magnitude at all is. There are four arguments of Zeno about motion, which give stomach aches (δυσκολίας) to those who analyze them: first, the one about there being no motion because the thing under way must reach the half before the end, which *we* have gone all the way through in the passage above. Second, the so-called Achilles, is this: that the slowest, running, will never be left behind by the fastest, for before that the pursuer must come to the place the pursued set off from, so that the slower must always be ahead by some amount. But this is the same argument as the bisecting, but differs in that the dividing of the magnitude taken in addition is not in two. The not catching up with the slower comes out of the argument, but comes about by means of the same thing as in the bisection (for in both, the magnitude being somehow divided, the not reaching the end follows, though in this one there is piled on that not even will the one fastest in the tragedies reach, in pursuit, the slowest), so that the analyses must also be the same. And holding that the one ahead will never be caught is false; for when he is ahead he will not be caught, but he will nevertheless be caught as long as one grants that he will traverse some definite distance. These then are two arguments, and the third is the one mentioned here, that the flying arrow stands still. And it follows from taking time to be composed of nows; this not being granted, there will be no syllogism.

And the fourth is the one about the things moved opposite ways in the stadium, of equal size, past equals, some from the ends of the stadium and some from the middle, equal in speed, in which he thinks it follows that the half time is equal to its double. But the paralogism is in holding that something will be borne in an equal time past one equal magnitude moving at the same speed and past another equal magnitude at rest; but this is false. For example, let there be placed a row of stationary equal bodies marked A, then a row of B's beginning from their middle, being equal in number and magnitude to these, and then a row of C's from the end [of the B's, that is from the middle of the A's but in the opposite direction], being equal in number and magnitude to these and equal in speed to the B's. Now it follows that when these have moved past each other, the first B will be at the end at the same time as the first C. And it follows that row C will have gone past the whole length [of the B's], but row B past the half length [of the A's]; and so the time [for B] will be half, since each row takes the same time to pass each body. But at the same time it follows that the first B has passed all the C's, for at the same time the first C and the first B will be at opposite ends, because both come to be past the A's in an equal time. This then is the argument, but it follows by means of the thing said to be false.

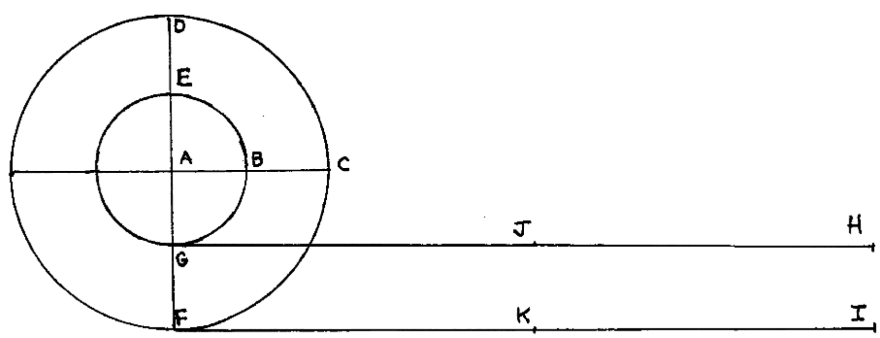
263a11–263b9

In our first discussions of motion we analyzed [Zeno's argument about the bisection] by way of time's having an infinity in itself; for there is nothing strange if someone traverses an infinity in an infinite time, and the infinite is present in the same way both in the length and in the time. But while this analysis holds sufficiently for the one who raises the question (for he asks if in a limited time it is possible to traverse or count an infinite), with respect to our concern and the truth it is not sufficient; for should someone, leaving aside length and the question whether in a limited time it is possible to traverse an infinite, make the same inquiries about time itself (for time has infinitely many divisions), no longer will this analysis be sufficient, but one must articulate the truth, the very thing we were speaking of in the passage just preceding. For if someone should divide the continuous into two halves, he would use the one point as two; for he makes it a beginning and an end. Thus do both the one counting and the one dividing into halves. And having been thus divided, neither the line nor the motion will be continuous; for the continuous motion is through something continuous, and while there are present in the continuous infinitely many halves, they are present not actually but potentially. And if one makes them actual he will take apart the continuity traversed, and will stop the continuous motion, and it is this very thing that clearly happens to the one who counts the halves; for it is necessary for him to count the one point as two, since it will be the end of one half and the beginning of the other, whenever one does not count the continuous as one but as two halves. So that one must say to the one asking if it is possible to traverse an infinite either in time or in length, that in a certain way it is but in a certain way it is not. For if things are infinitely many actually, it is not possible to go through them; if potentially, it is possible. For what is moved continuously traverses an infinite accidentally, not simply; for it befalls the line by accident to be an infinity of halves, but the being of the line, what it is, is other than that.

Aristotle[1]: *Mechanical Problems* 24: Another paradox — "Aristotle's Wheel"

It stumps one why in the world a larger circle rolls out a line equal to a smaller circle when they are about the same center. When they are rolled out separately their paths come out as their magnitudes are to one another. And when both have one and the same center, sometimes the line they roll out is as long as the one the smaller circle rolls out by itself, but sometimes as long as the larger. Now that the greater circle unrolls a greater line is clear. For it appears to sight that the angle each circumference makes with its own diameter [not the horn angle but is "complement"] is greater in the greater circle and less in the less, so that this same ratio must hold between the lines along which they are unrolled, just by sight. But that they also unroll an equal distance whenever they lie about the same center is clear; and thus it happens that this distance is equal sometimes to the line the greater circle unrolls, sometimes to the less.

For let there be the greater circle DFC, the less EGB, and A the center of both; and let there be the line FI which the big circle rolls out by itself, with FK cut off on it equal to the line GJ which the smaller circle rolls out by itself. If I roll the smaller circle, I displace the common center A; and let the big circle be attached. When, then, AB turns perpendicular to GJ, at the same time AC becomes perpendicular to FK, so that there will always be equal elapsed distances, along GJ by the circumference GB, and along FK by FC. And if the quadrant rolls out an equal line, it is clear that the whole circle will have rolled out one equal to that of the other whole circle, so that when the circle BG comes to rest at point J, circumference FC will be on point K, and the whole circle will have unrolled.



Likewise also, if I roll the big circle, having fitted the small one in it with the same center, AB will be pointing straight down at the same time as AC, perpendicular to GH when the latter is perpendicular to FI. So that when

[1]It is not certain that Aristotle is the author of the *Mechanical Problems*, but it was traditionally attributed to him.

the one will have completed the distance GH, the other FI, and FA has again become perpendicular to FK, and AG to GJ, the initial position will be restored on the points HI. But since the greater circle does not stand still for the less, so as to remain for some time on the same point (for both move continuously on both hypotheses), and since the less does not jump over any point, for the greater to go through a distance equal to the less, or the less to the greater, is strange. Moreover, for the displaced center, with a motion that is always one, to be rolled along sometimes a greater distance and sometimes a less, is amazing. For the same thing moved with the same speed naturally covers the same distance; but the center is to move with the same speed for an equal time on both hypotheses.

One must take this beginning about the cause of these things, that the same power, or an equal one, moves one magnitude more slowly, another faster. If there should be anything which is not by nature moved by itself, and something naturally moved should move both that thing and itself, it would be moved more slowly that way than if it had moved alone. And if it were moved by nature, and nothing were moved with it, the same thing holds. It is impossible for anything to be moved more than what moves it; for it is not moved by its own motion, but by whatever moves it.

There could be two circles, a bigger A and a smaller B. If the smaller were to shove the larger, while not itself rolling, it is clear that the larger would go through just so much of a straight path as it had been pushed by the smaller. And it would be pushed just so far as the small one had been moved. Therefore they would have completed an equal amount of the straight path. And so, necessarily, if the smaller while rolling were to push the larger, it would be rolled at the same time, by what pushed it, and just so much of its length would unroll as unrolled from the smaller, if it were not to move by any motion of its own. For in the manner and to the extent that the thing moved it moved, so much must the thing moved have been moved by it. But surely the circle did move the same, in a circle and for a foot (for let this be how much it was moved), and the big one was therefore moved just that much. Likewise also, if the big one should move the little one, the little one will be moved as the big one. Whichever of the two is moved by itself, and whether fast or slowly, the other will straightway, naturally, with the same speed, cover just as long a line. And this very thing produces the impasse, that no longer do they do the same when they are fitted together. In that case the one is moved by the other neither as is natural nor with its own motion. For it makes no difference whether the smaller is nested inside the larger or attached side-by-side; similarly, whenever the one moves and the other is moved by it, however much the one moves, the other will be moved that much. Now when one circle drives another edge-against-edge, or by means of a cord or belt, it does not always cause it as much rolling as it undergoes; but whenever they are placed about the same center, the one must always be rolled together with the other by it. But nonetheless, the one is moved not with its own motion, but just as if it had no motion at all. And if it has it, but does not use it, the same thing happens. Whenever, then, the big circle moves a built-in small one, the small one gets all its motion from this big one;

and whenever the small one moves a built-on large one, again the large one gets all its motion from this small one. But if they are separate, then [no matter what motion one contributes to the other], each one moves itself, to some extent, itself. But the perplexed one mis-reasons sophistically that with the same center and moving at the same speed, they describe a line unequally. For the center of both is the same, but by accident, like educated and white.[2] For to be the center of a small circle is not as useful [to a point at which a twisting motion is applied] as to be the center of a large circle. Whenever the moving circle is the small one, the center and the source is as from it, but whenever the larger, as from it. Therefore the same thing is doing the moving, but only in a certain sense.

[2] "Someone educated might be white; but since neither from necessity nor for the most part does this happen, we call it an accident." (*Metaphysics*, 1025a 19–21).

Henri Bergson: *Creative Evolution*[1]

Suppose we wish to portray on a screen a living picture, such as the marching past of a regiment. There is one way in which it might first occur to us to do it. That would be to cut out jointed figures representing the soldiers, to give to each of them the movement of marching, a movement varying from individual to individual although common to the human species, and to throw the whole on the screen. We should need to spend on this little game an enormous amount of work, and even then we should obtain but a very poor result: how could it, at its best, reproduce the suppleness and variety of life? Now, there is another way of proceeding, more easy and at the same time more effective. It is to take a series of snapshots of the passing regiment and to throw these instantaneous views on the screen, so that they replace each other very rapidly. This is what the cinematograph does. With photographs, each of which represents the regiment in a fixed attitude, it reconstitutes the mobility of the regiment marching. It is true that if we had to do with photographs alone, however much we might look at them, we should never see them animated: with immobility set beside immobility, even endlessly, we could never make movement. In order that the pictures may be animated, there must be movement somewhere. The movement does indeed exist here; it is in the apparatus. It is because the film of the cinematograph unrolls, bringing in turn the different photographs of the scene to continue each other, that each actor of the scene recovers his mobility; he strings all his successive attitudes on the invisible movement of the film. The process then consists in extracting from all the movements peculiar to all the figures an impersonal movement abstract and simple, movement in general, so to speak: we put this into the apparatus, and we reconstitute the individuality of each particular movement by combining this nameless movement with the personal attitudes. Such is the contrivance of the cinematograph. And such is also that of our knowledge. Instead of attaching ourselves to the inner becoming of things, we place ourselves outside them in order to recompose their becoming artificially. We take snapshots, as it were, of the passing reality, and, as these are characteristic of the reality, we have only to string them on a becoming, abstract, uniform and invisible, situated at the back of the apparatus of knowledge, in order to imitate what there is that is characteristic in this becoming itself. Perception, intellection, language so proceed in general. Whether we would think becoming, or express it, or even perceive it, we hardly do anything else than set going a kind of cinematograph inside us. We may therefore sum up what we have been saying in the conclusion that *the mechanism of our ordinary knowledge is of a cinematographical kind.*. . .

I take of the continuity of a particular becoming a series of views, which I connect together by "becoming in general." But of course I cannot stop there. What is not determinable is not representable: of "becoming in general" I have only a verbal knowledge. As the letter x designates a certain unknown quantity,

[1] *Creative Evolution*, translated by Arthur Mitchell. New York: Henry Holt and Company, 1911. Pp. 304–6, 307–14

whatever it may be, so my "becoming in general," always the same, symbolizes here a certain transition of which I have taken some snapshots; of the transition itself it teaches me nothing. Let me then concentrate myself wholly on the transition, and, between any two snapshots, endeavor to realize what is going on. As I apply the same method, I obtain the same result; a third view merely slips in between the two others. I may begin again as often as I will, I may set views alongside of views for ever, I shall obtain nothing else. The application of the cinematographical method therefore leads to a perpetual recommencement, during which the mind, never able to satisfy itself and never finding where to rest, persuades itself, no doubt, that it imitates by its instability the very movement of the real. But though, by straining itself to the point of giddiness, it may end by giving itself the illusion of mobility, its operation has not advanced it a step, since it remains as far as ever from its goal. In order to advance with the moving reality, you must replace yourself within it. Install yourself within change, and you will grasp at once both change itself and the successive states in which it might at any instant be immobilized. But with these successive states, perceived from without as real and no longer as potential immobilities, you will never reconstitute movement. Call them *qualities*, *forms*, *positions*, or *intentions*, as the case may be, multiply the number of them as you will, let the interval between two consecutive states be infinitely small: before the intervening movement you will always experience the disappointment of the child who tries by clapping his hands together to crush the smoke. The movement slips through the interval, because every attempt to reconstitute change out of states implies the absurd proposition, that movement is made of immobilities.

Philosophy perceived this as soon as it opened its eyes. The arguments of Zeno of Elea, although formulated with a very different intention, have no other meaning.

Take the flying arrow. At every moment, says Zeno, it is motionless, for it cannot have time to move, that is, to occupy at least two successive positions, unless at least two moments are allowed it. At a given moment, therefore, it is at rest at a given point. Motionless in each point of its course, it is motionless during all the time that it is moving.

Yes, if we suppose that the arrow can ever *be* in a point of its course. Yes again, if the arrow, which is moving, ever coincides with a position, which is motionless. But the arrow never *is* in any point of its course. The most we can say is that it might be there, in this sense, that it passes there and might stop there. It is true that if it did stop there, it would be at rest there, and at this point it is no longer movement that we should have to do with. The truth is that if the arrow leaves the point A to fall down at the point B, its movement AB is as simple, as indecomposable, in so far as it is movement, as the tension of the bow that shoots it. As the shrapnel, bursting before it falls to the ground, covers the explosive zone with an indivisible danger, so the arrow which goes from A to B displays with a single stroke, although over a certain extent of duration, its indivisible mobility. Suppose an elastic stretched from A to B, could you divide its extension? The course of the arrow is this very extension; it is equally simple and equally undivided. It is a single and unique bound. You fix a point C in the interval passed, and say that at a certain moment the arrow was in C. If it had been there, it would have been stopped there, and you would no longer have had a flight from A to B, but two flights, one from A to C and the other from C to B, with an interval of rest. A single movement is entirely, by the hypothesis, a movement between two stops; if there are intermediate stops, it is no longer a single movement. At bottom, the illusion arises from this, that the movement, *once effected*, has laid along its course a motionless trajectory on which we can count as many immobilities as we will. From this we conclude that the movement, *whilst being effected*, lays at each instant beneath it a position with which it coincides. We do not see that the trajectory is created in one stroke, although a certain time is required for it; and that though we can divide at will the trajectory once created, we cannot divide its creation, which is an act in progress and not a thing. To suppose that the moving body is at a point of its course is to cut the course in two by a snip of the scissors at this point, and to substitute two trajectories for the single trajectory which we were first considering. It is to distinguish two successive acts where, by the hypothesis, there is only one. In short, it is to attribute to the course itself of the arrow everything that can be said of the interval that the arrow has traversed, that is to say, to admit *a priori* the absurdity that movement coincides with immobility.

We shall not dwell here on the three other arguments of Zeno. We have examined them elsewhere. It is enough to point out that they all consist in applying the movement to the line traversed, and supposing that what is true of the line is true of the movement. The line, for example, may be divided into as many parts as we wish, of any length that we wish, and it is always the same line. From this we conclude that we have the right to suppose the movement articulated as we wish, and that it is always the same movement. We thus obtain a series of absurdities that all express the same fundamental absurdity. But the possibility of applying the movement *to* the line traversed exists only for an observer who keeping outside the movement and seeing at every instant the possibility of a stop, tries to reconstruct the real movement with these possible immobilities. The absurdity vanishes as soon as we adopt by thought the continuity of the real movement, a continuity of which every one of us is conscious whenever he lifts an arm or advances a step. We feel then indeed that the line passed over between two stops is described with a single indivisible stroke, and that we seek in vain to practice on the movement, which traces the line, divisions corresponding, each to each, with the divisions arbitrarily chosen of the line once it has been traced. The line traversed by the moving body lends itself to any kind of division, because it has no internal organization. But all movement is articulated inwardly. It is either an indivisible bound (which may occupy, nevertheless, a very long duration) or a series of indivisible bounds. Take the articulations of this movement into account, or give up speculating on its nature.

When Achilles pursues the tortoise, each of his steps must be treated as indivisible, and so must each step of the tortoise. After a certain number of steps, Achilles will have overtaken the tortoise. There is nothing more simple. If you insist on dividing the two motions further, distinguish both on the one side and on the other, in the course of Achilles and in that of the tortoise, the *sub-multiples* of the steps of each of them; but respect the natural articulations of the two courses. As long as you respect them, no difficulty will arise, because you will follow the indications of experience. But Zeno's device is to reconstruct the movement of Achilles according to a law arbitrarily chosen. Achilles with a first step is supposed to arrive at the point where the tortoise was, with a second step at the point which it has moved to while he was making the first, and so on. In this case, Achilles would always have a new step to take. But obviously, to overtake the tortoise, he goes about it in quite another way. The movement considered by Zeno would only be the equivalent of the movement of Achilles if we could treat the movement as we treat the interval passed through, decomposable and recomposable at will. Once you subscribe to this first absurdity, all the others follow.[2]

Nothing would be easier, now, than to extend Zeno's argument to qualitative becoming and to evolutionary becoming. We should find the same contradictions in these. That the child can become a youth, ripen to maturity and decline to old age, we understand when we consider that vital evolution is here the reality itself. Infancy, adolescence, maturity, old age, are mere views of the mind, *possible stops* imagined by us, from without, along the continuity of a progress. On the contrary, let childhood, adolescence, maturity and old age be given as integral parts of the evolution, they become *real stops*, and we can no longer conceive how evolution is possible, for rests placed beside rests will never be equivalent to a movement. How, with what is made, can we reconstitute what is being made? How, for instance, from childhood once posited as a *thing*, shall we pass to adolescence, when, by the hypothesis, childhood only is given? If we look at it closely, we shall see that our habitual manner of speaking, which is fashioned after our habitual manner of thinking, leads us to actual logical deadlocks — deadlocks to which we allow ourselves to be led without anxiety, because we feel confusedly that we can always get out of them if we like: all that we have to do, in fact, is to give up the cinematographical habits of our intellect. When we say "The child becomes a man," let us take care not to fathom too deeply the literal meaning of the expression, or we shall find that, when we posit the subject "child," the attribute "man" does not yet apply to it, and that, when we express the attribute "man," it applies no more to the subject "child." The reality, which is the *transition* from childhood to manhood, has slipped between our fingers. We have only the imaginary stops "child" and "man," and we are very near to saying that one of these stops is the other, just as the arrow of Zeno is, according to that philosopher, at all the points of the course. The truth is that if language here were molded on reality, we should not say "The child becomes the man," but "There is becoming from the child to the man." In the first proposition, "becomes" is a verb of indeterminate meaning, intended to mask the absurdity into which we fall when we attribute the state "man" to the subject "child." It behaves in much the same way as the

[2]Here we omit a footnote where Bergson dismisses as irrelevant a certain mathematical treatment of Zeno's paradox.

movement, always the same, of the cinematographical film, a movement hidden in the apparatus and whose function it is to superpose the successive pictures on one another in order to imitate the movement of the real object. In the second proposition, "becoming" is a subject. It comes to the front. It is the reality itself; childhood and manhood are then only possible stops, mere views of the mind; we now have to do with the objective movement itself, and no longer with its cinematographical imitation. But the first manner of expression is alone conformable to our habits of language. We must, in order to adopt the second, escape from the cinematographical mechanism of thought.

We must make complete abstraction of this mechanism, if we wish to get rid at one stroke of the theoretical absurdities that the question of movement raises. All is obscure, all is contradictory when we try, with states, to build up a transition. The obscurity is cleared up, the contradiction vanishes, as soon as we place ourselves along the transition, in order to distinguish states in it by making cross cuts therein in thought. The reason is that there is *more* in the transition than the series of states, that is to say, the possible cuts — *more* in the movement than the series of positions, that is to say, the possible stops. Only, the first way of looking at things is conformable to the processes of the human mind; the second requires, on the contrary, that we reverse the bent of our intellectual habits. No wonder, then, if philosophy at first recoiled before such an effort. The Greeks trusted to nature, trusted the natural propensity of the mind, trusted language above all, in so far as it naturally externalizes thought. Rather than lay blame on the attitude of thought and language toward the course of things, they preferred to pronounce the course of things itself to be wrong.

Such, indeed, was the sentence passed by the philosophers of the Eleatic school. And they passed it without any reservation whatever. As becoming shocks the habits of thought and fits ill into the molds of language, they declared it unreal. In spatial movement and in change in general they saw only pure illusion. This conclusion could be softened down without changing the premisses, by saying that the reality changes, but that it *ought not* to change. Experience confronts us with becoming: that is *sensible* reality. But the *intelligible* reality, that which *ought* to be, is more real still, and that reality does not change. Beneath the qualitative becoming, beneath the evolutionary becoming, beneath the extensive becoming, the mind must seek that which defies change, the definable quality, the form or essence, the end. Such was the fundamental principle of the philosophy which developed throughout the classic age, the philosophy of Forms, or, to use a term more akin to the Greek, the philosophy of Ideas.

Henri Bergson: *Matière et Mémoire*: An excerpt from chapter iv[3]

Every movement, insofar as it is a passage from rest to rest, is absolutely indivisible.

What is in question here is not an hypothesis, but a fact, which is generally concealed by an hypothesis. Here, for example, is my hand, positioned at point A. I carry it to point B, passing through the interval at one stroke. There are in this movement, both at the same time, an image which strikes my sight and an act which my muscular consciousness grasps. My consciousness gives me the inward sensation of a simple fact, for in A was rest, in B is rest again, and between A and B is placed an indivisible or at least an undivided act, a passage from rest to rest, which is the movement itself. But my sight perceives the movement under the form of a line AB which is passed through, and this line, like all space, is indefinitely decomposable. It seems then, at first, that I may at will take this movement to be multiple or indivisible, according as I envisage it in space or in time, as an image which takes shape outside of me or as an act which I accomplish myself.

Yet, by putting aside all preconceived ideas, I quickly perceive that I do not have this choice, that my sight itself grasps the movement from A to B as an indivisible whole, and that if it divides something, it is the line supposed to have been passed through, and not the movement passing through it. It is indeed true that my hand does not go from A to B without traversing the intermediate positions, and that these intermediate points resemble stages, as numerous as one wishes, placed all along the route; but between the divisions so marked and stages properly so called there is this capital difference, that at a stage one stops, while here the moving body passes. Now a passage is a movement, and a stop is an immobility. The stop interrupts the movement; the passage is one with the movement itself. When I see the moving body pass a point, I no doubt conceive that it *could* stop there; and even though it does not stop there, I am inclined to consider its passage as an infinitely short stop, because I must have at least the time to think there; but it is my imagination alone which rests there, and the role of the moving body is, on the contrary, to move. As every point of space necessarily appears to me as fixed, I am hard put not to attribute to the moving body itself the immobility of the point with which, for a moment, I make it coincide; it seems to me, then, when I reconstitute the total movement, that the moving body has stood for an infinitely short time at all the points of its trajectory. But it would not do to confound the data of the senses, which perceive the movement, with the artifices of the mind which recomposes it. The senses, left to themselves, present to us the real movement, between two real stops, as a solid and undivided whole. The division is the work of the imagination, whose function is rightly to fix the moving images of our ordinary experience, like the instantaneous flash which at night illuminates the

[3]First published in 1896. (7th ed., Paris: Quadrige/Presses Universitaires de France, 1939, p. 211). Trans., Adam Schulman.

scene of a storm.

We grasp here, at its very origin, the illusion which accompanies and conceals the perception of real movement. Movement visibly consists in passing from one point to another, and consequently in traversing space. Now the space traversed is divisible to infinity; and as the movement is, so to speak, applied along the line through which it passes, it appears integral with (*solidaire de*) this line and, like it, divisible. Has it not itself drawn the line? Has it not traversed, in turn, the successive and juxtaposed points? Yes, without doubt, but these points have no reality except in a line traced, that is to say immobile; and only because you represent the movement, in turn, at these different points, do you necessarily stop it there; your successive positions are, at bottom, nothing but imaginary stops. You substitute the trajectory (*la trajectoire*) for the passage (*la trajet*), and because the passage is subtended by the trajectory, you believe that they coincide. But how could a *progress* coincide with a *thing*, a movement with an immobility?

What facilitates the illusion here is that we distinguish moments in the course of the duration, like positions in the passage of the moving body. Supposing that the movement from one point to another forms an undivided whole, this movement still fills a determinate time; and it suffices to isolate an indivisible instant from this duration, for the moving body to occupy at that precise moment a certain position, which is thus detached from all the others. The indivisibility of the movement implies then the impossibility of the instant; and a very summary analysis of the idea of durations will show us both why we attribute instants to duration and why it cannot have any. Let there be a simple movement, like the passage of my hand when it is displaced from A to B. This passage is given to my consciousness as an undivided whole. No doubt it endures; but its duration, which also coincides with the inward aspect which it has for my consciousness, is, like it, compact and undivided. Now while it presents itself, *qua* movement, as a simple fact, it describes in space a trajectory which I may consider, for simplicity's sake, as a geometric line; and the extremities of this line, *qua* abstract limits, are no longer lines but indivisible points. Now if the line which the moving body has described measures for me the duration of its movement, how could the point where the line ends not symbolize an extremity of this duration? And if this point is an indivisible of length, how can the duration of the passage not be terminated by an indivisible of duration? As the total line represents the total duration, the parts of this line must correspond, it seems, to the parts of this duration and the points of the line to the moments of the time. The indivisibles of duration or moments of time are thus born of a need for symmetry; one arrives at them naturally when one demands of space an integral representation of time. But precisely here is the error. While the line AB symbolizes the elapsed duration (*la durée écoulée*) of the accomplished movement from A to B, in no way can it, immobile, represent the movement being accomplished, the duration flowing (*la durée s'écoulant*); and from the fact that this line is divisible into parts and that it is terminated by points, one may not conclude either that the corresponding duration is composed of separate parts or that it is limited by instants.

De Beaune's problem to Descartes, solved by Leibniz

In the final paragraph of "A new method", we learn that Leibniz has solved De Beaune's problem: De Beaune's line is a logarithmic line. There are a number of important claims that are made or suggested in these last lines.

First, Leibniz points out that Descartes failed to find an equation for De Beaune's curve. Implicit in his remark is that Descartes was dealing with the sort of problem Leibniz has just described—a problem "which no one will be able to deal with easily by proceeding blindly without our differential calculus or something like it"(16). Leibniz is pointing out both the limits of Descartes' *Geometry* and the power of the differential calculus.

Second, the last part of "A new method" shows us that Leibniz can find a differential equation for a curve even when the rules formulated in the first part of Leibniz's paper do not apply. Leibniz does this by showing that a differential equation can be derived directly from the geometric representation of the curve and its tangent properties. In considering the geometric representation of a non-algebraic curve, Leibniz seems to have derived an additional rule for certain kinds of non-algebraic curve. However, Leibniz never gives us this new differential equation as an explicit rule to associate with curves like de Beaune's. In this handout, we will try to formulate such a rule. However, we should note that Leibniz might want to distinguish between **rules** that one can apply to algebraic equations to find differential equations and a more general **method** that one can use to find a differential equation for non-algebraic curves (notice that the paper is entitled "A new method…" but when he finds the differential equations for algebraic curves he uses the "rules of the calculus"). By showing that his calculus transcends a particular set of rules applied to equations, Leibniz may be demonstrating the power of his method to transform geometric curves directly into differential equations bypassing not only algebraic representations of the curve but other analytic representations of the curve. In formulating a rule, we will introduce an analytic representation of de Beaune's curve which will mediate between the geometric representation and the differential equation. This may, in a way, blunt the point Leibniz is trying to make about the power of his method.

Third, and most specifically, after Leibniz gives a differential equation for De Beaune's curve he claims that this particular differential equation reveals that De Beaune's curve is a logarithmic curve. In order to understand this claim we will need to understand what a logarithmic curve is and how a differential equation could lead us to know a curve is logarithmic.

In the notes below, we will expand on all three claims.

1 Finding a differential equation for a curve when there is no algebraic expression for that curve

The techniques of Descartes' *Geometry* won't work to help one find an equation for De Beaune's curve, but Leibniz can easily find an equation for De Beaune's curve with the differential calculus.

You can do this for yourself. Your goal is simply to find a way of relating dx to dw for fig. 13 on p. 16 and thus write a differential equation. (Hint: the distance between x and x_1 is a finite not an infinitesimal distance (fig. 13 p. 16).)

You should come up with the same thing that Leibniz did.

$$\frac{w}{XC} = \frac{dw}{dx}$$

Or in Leibniz's words: "Now XW (or w) is to XC (or a) as dw is to dx".

Exercise: Leibniz has shown that it is possible to go immediately from a curve and the properties of its tangent lines to a differential equation. As an exercise, one might do this for the parabola. In Apollonius, we learned that for any given ordinate and a line tangent to the curve at that ordinate, the abscissa is equal to the section of the axis between the vertex and the point of intersection of the tangent line and the axis. Find a differential equation for the parabola based on this tangent property. By bypassing an algebraic equation your final differential equation will look a bit different than the differential equation for the curve $y = x^2$ or $x = y^2$. What is needed to show that the two differential equations are equivalent?

2 Is Leibniz introducing a new rule for the differential calculus?

After finding the differential equation, Leibniz rearranges and simplifies the equation to make it clear that "These w's will thus themselves be proportional to their own increments or differences"(16).

We begin with the differential equation:

$$\frac{w}{XC} = \frac{dw}{dx}$$

and rewrite it thus:

$$w = \frac{XC}{dx}dw.$$

XC and dx are constants; Leibniz renames XC, a, and dx, b. Thus we have,

$$w = \frac{a}{b}dw.$$

This form of the equation makes it clear that the w's are proportional to their own differences; that is, w is always equal to dw times some constant.

Where Descartes' *Geometry* failed to give an algebraic rule for logarithmic curves, and Leibniz' rules at the outset of his paper failed to give us a differential equation, Leibniz has been able to use the method of tangents to find a differential equation for the logarithmic line.

Is there a way to generalize the method that Leibniz used to get this differential equation so that we can derive a new rule for all curves like De Beaune's curve?

According to our equation, the ordinates of our curve are proportional to the differences of these ordinates. Leibniz says that this means that the x's are the logarithms of the w's (we will try to understand this claim in the next section).

So, we can write the following non-algebraic equation for the curve:

$$x = \log_k w$$

Or:

$$w = k^x$$

We know that:

$$w = \frac{a}{b} dw$$

$$dw = \frac{b}{a} w$$

And, since $w = k^x$ and $b = dx$, we can derive an *Exponential Rule:*

$$d(k^x) = \frac{k^x dx}{a}$$

Recall that a is the length cut off on the axis between the tangent line and the ordinate at a given point. The value of k will determine a (and conversely). If $a = 1$, then k is the familiar constant, e. So we end up with a special case of the *Exponential Rule:*

$$d(e^x) = e^x dx$$

(There is a method of approximating e that is left as a guided exercise in the manual.)

We also could have rewritten our differential equation so as to find out the differential of a logarithm. In this case, our *Logarithm Rule* is:

$$d(\log_k w) = a \frac{dw}{w}$$

Here too, we can consider the special case when $a = 1$ and $k = e$. When $k = e$, it is sometimes written as "$\ln w$" in place of "$\log_e w$" and we say "the natural log of w". In this special case we have:

$$d(\ln w) = \frac{dw}{w}$$

We have been able to generalize Leibniz's method to derive two rules for finding differential equations for exponential and logarithmic curves. But in doing so we allowed ourselves to accept Leibniz's claim that De Beaune's curve was, in fact, a logarithmic curve. Our derivations used Leibniz's claim that the x's are the logarithms of the w's. How did Leibniz get this claim?

3 WW is a logarithmic curve

Let's return now to Leibniz's claims that De Beaune's curve is, in fact, a logarithmic curve. After simplifying the expression to show that, "These w's will thus themselves be proportional to their own increments or differences," Leibniz continues, "if the x's are in an arithmetic progression, then the w's will be in a geometric progression. In other words, if the w's are numbers, then the x's will be logarithms. WW is therefore a logarithmic line"(17).

Leibniz is making two claims in these final lines that we might want to better understand.

Claim 1: If the w's are proportional to their own differences ($w = \frac{a}{dx}dw$) and if the x's are in an arithmetic progression ($dx = b$, where b is a constant), then the w's will be in a geometric progression.

Claim 2: If the x's are in an arithmetic progression, and the w's are numbers in a geometric progression then WW is a logarithmic line.

If we understand what a logarithmic curve is, perhaps both claims will become clear. The word *logarithm* comes from the Greek word *logos* (ratio) and *arithmos* (number): a logarithm is a number of a ratio.

Consider the following geometric series: $1, 2, 4, 8, 16, \ldots$. In any geometric progression, the ratio of any term to its subsequent term is the same. In our geometric series:

$$g_n : g_{n+1} :: 1 : 2 \tag{10}$$

Another ratio we might consider is that between the first term, g_0, and any mth term, g_m. For example, we might want to know the ratio between g_0 and g_2. From equation (10) we know that:

$$g_0 : g_1 :: g_1 : g_2 :: 1 : 2$$

The ratio of $g_0 : g_2$ is therefore the duplicate ratio of $1 : 2$; in other words it is $1 : 4$. Similarly the ratio of $g_0 : g_3$ is going to be the triplicate ratio of $1 : 2$; in other words it is $1 : 8$. We can find the nth term of the geometric series by replicating the characteristic ratio of a geometric series n times. The nth term will have this ratio to the first term of the series. Alongside our geometric series, then, we can put an arithmetic series that represents the number of times a ratio has been replicated to produce that term:

$$1,2,4,8,16,\ldots$$
$$0,1,2,3,\ \ 4,\ldots$$

If one lets the value of the abscissa along an axis represent the arithmetic progression, and the corresponding ordinates the geometric progression, one will have a series of points that looks like they might be interpolated to make a curve similar in shape (though not orientation) to De Beaune's curve.

But we merely have a set of points, not a curve. To get a curve, we need these discrete points to be a part of a continuous curve. Let's revisit the geometric progression, but let's rewrite it like this:

$$2^0, 2^1, 2^2, 2^3, 2^4, \ldots$$

The geometric series can then be represented as some base k (in our case 2), raised to the nth value, where n takes on integer values. If we allow n to to take all real values of n, then we will get a smooth curve composed of the series of points (n, k^n). But how do we know, for example, the value of $2^{1/2}$? This is the value of the subduplicate ratio of 2^0 and 2^1; or, in other words, it is the mean proportional between 2^0 and 2^1. What would $2^{1/3}$ be? $2^{2/3}$? We would need to include all these points and in addition, 2 raised to irrational quantities. Leibniz will talk about this sort of problem in the next paper where he argues that in order to solve it one would need an algebraic equation of a different degree to find each value; the more general problem, to find any possible number of mean proportionals, will be of indefinite degree and transcend every algebraic equation. Leibniz claims that even though solving these problems will require equations of indefinite degree, these problems should be included in the realm of geometry—indeed, they should be considered to be among its leading problems (On Recondite Geometry and the Analysis of Indivisibles and Infinites).

Leibniz observes that if arithmetic intervals along the axis correspond to ordinates that are in a geometric progression then the line itself is logarithmic. Such a curve can be expressed two ways:

$$w = k^x$$

or

$$x = \log_k w.$$

At this point, we should have a pretty good understanding of Leibniz's second claim: if the x's are in an arithmetic progression, and the w's are numbers in a geometric progression then WW is a logarithmic line.

But Leibniz also claims that if the w's are always proportional to their own differences while x is changing uniformly (that is the x's are in an arithmetic progression) then w is in a geometric progression which is tantamount to saying (we now know) that x is the logarithm of w (Claim 1).

Ideally, we would like to show that our given, that $w = \frac{a}{dx} dw$ and that x is increasing in an arithmetic progression, entails either that $x = \log_k w$ or $w = k^x$.

One difficulty is that our given concerns infinitesimals while what we want to prove does not. One way that we might approach this demonstration is to show that an analogous finite claim (that the w's are proportional to their finite differences) entails that WW is a logarithmic curve. We could then try to understand why it is the case that if the w's are proportional to their infinitesimal differences the w's would also be proportional to their finite differences.

So, is it the case that if the w's are proportional to their finite differences and the x values are in an arithmetic progression, then we have a logarithmic curve?

If the w's are proportional to their finite differences, then the ratio of any w to its difference is the same as the ratio of any other w to its difference provided that the corresponding difference in x is the same for each. So if we take the w value for $x = a, b, c, \ldots$ where $a, b, c, \ldots$ are equal finite distances apart, and we call these w values $w_a, w_b, w_c, \ldots$ respectively, then these w's will all have the same ratio to their differences:

$$w_b - w_a : w_a :: w_c - w_b : w_b :: w_d - w_c : w_c :: w_e - w_d : w_e$$

But, if this is true, then (by *componendo*)

$$w_b : w_a :: w_c : w_b :: w_d : w_c :: w_e : w_d$$

And thus,

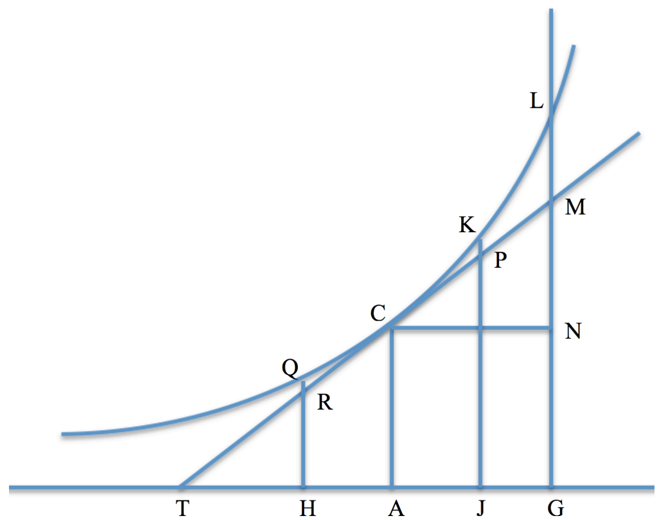
$$w_a : w_b :: w_b : w_c :: w_c : w_d :: w_d : w_e.$$

But this is just to say that if the x's are in an arithmetic progression, the w's will be in a geometric progression. That is, that WW is a logarithmic curve.

So we can now see that if this property of being proportional to one's own difference is true in the finite case, then the curve is logarithmic.

Exercise: You can do the exercises on p. 77, #'s 1–6 (you may assume that all logarithms are natural logarithms).

How to approach an exact value for e (the base of the natural logarithm)



The *Notes* (Theorem 3, pp. 70-72) prove that $2 < e < 4$ and mention (p. 72) that the same methods can be generalized to find more exact approximations for e. Let us use Figure 37 (p. 71) to see how this can be done.

In the figure, the curve represents $w = e^x$; line TM is tangent to the curve at point C; point A is the *origin* (where the abscissa $x = 0$ and where the ordinate $w = CA = e^x = e^0 = 1$); also, if we assume that AG = 1, LG = $e^1 = e$.

By equation 3 on p. 70, $dx = dw/w$, or $dw/dx = w$. Since dw/dx is the slope of a line tangent to the curve, it follows that, at point C, where the ordinate $w = 1$, the slope of the tangent is $dw/dx = 1$ as well, so the line TM tangent to the curve at C has slope 1. Therefore, in the figure, all of the following line segments have unit length: CA, NG, MN, TA, CN and AG. It follows that MG = 2, from which it is obvious that $e > 2$.

To prove that $e < 4$, the *Notes* added the ordinate QH *to the left* of CA, and assumed that HA = ½, so that point H is half a unit *to the left* of the origin A, so the abscissa there is $x = -½$, while the ordinate QH is $w = e^{-½}$, and RH = TH = ½. Then, from the fact that RH < QH, it follows that $½ < e^{-½}$, or $e < 4$.

But let us, more generally, assume that HA = $1/m$ for any positive integer m. Then RH = TH = TA – HA = $1 – 1/m = (m - 1)/m$, and the abscissa at H is $x = -1/m$, so the ordinate QH = $e^{-1/m}$. Then RH < QH leads to $(m - 1)/m < e^{-1/m}$, or $e^{1/m} < m/(m - 1)$, or $e < [m/(m - 1)]^m$ for any positive integer $m \neq 1$. Or, setting $n = m - 1$, $e < [(n + 1)/n]^{(n + 1)}$ for any positive integer n.

Note that, the larger n gets, the closer RH approaches QH *from below* (as n approaches ∞, H merges into A, and RH and QH both ultimately coincide with CA where the tangent meets the curve).

If we modify figure 37 by adding ordinate KJ to the *right* of CA, and letting AJ = $1/n$ for any positive integer n, then PJ = TJ = TA + AJ = $1 + 1/n = (n + 1)/n$, and the ordinate KJ = $e^{1/n}$. Then KJ > PJ leads to $e^{1/n} > (n + 1)/n$, or $e > [(n + 1)/n]^n$. Note that, the larger n gets, the closer KJ approaches PJ *from above* (as n approaches ∞, J merges into A, and KJ and PJ both ultimately coincide with CA where the tangent meets the curve).

Combining the two inequalities, this means that, for any positive integer n, $[(n + 1)/n]^n < e < [(n + 1)/n]^{(n + 1)}$. That is, e always lies *between* the n^{th} and the $(n+1)^{th}$ powers of $(n + 1)/n$. So, for $n = 1$, e lies between 2 and 4; for n = 2, e lies between $(3/2)^2 = 2.25$ and $(3/2)^3 = 3.375$; for $n = 3$, e lies between $(4/3)^3 \approx 2.37$ and $(4/3)^4 \approx 3.16$; for $n = 1{,}000{,}000$, e lies between $(1{,}000{,}001/1{,}000{,}000)^{1{,}000{,}000} \approx 2.718280$ and $(1{,}000{,}001/1{,}000{,}000)^{1{,}000{,}001} \approx 2.718283$. To fifty places past the decimal point, e has been calculated to be approximately 2.71828182845904523536028747135266249775724709369995…

—Adam Schulman 9-22-15

Brief supplement to Note 8 of Leibniz's "Recondite Geometry"

Here again is equation 2:

$$0 = a + bx + cy + exy + fx^2 + gy^2 + \cdots .$$

We then find its differential equation:

$$0 = bdx + cdy + exdy + eydx + 2fxdx + \cdots .$$

Now we compare our differential equation to the tangent property $dy = xdx$ (equation 1 in Note 8). dy and xdx are its only terms, and their coefficients in the differential equation are c and f. These are the only terms in the equation, then, with non-zero coefficients: $c = -1$ and $f = 1/2$.

So now we put these coefficients back into the original non-differential equation (2) in order to get the equation which, if differentiated, gives us our differential equation:

$$0 = -y + 1/2x^2.$$

And this corresponds to equation 3 in Note 8.

Comment 6

Leibniz claims here, without proof, that finding quadratrices is a special case of an inverse tangent problem. An inverse tangent problem is a problem where we are given a property that the tangents of a curve must have, and we have to find the curve. Here, instead of immediately demonstrating that the problem of finding quadratrices is a special case of the inverse tangent problem, Leibniz first goes on to show (in this and the following paragraph) how the inverse tangent problem can be approached by a method that is closely analogous to the one presented in an earlier paper, "On finding measurements of figures," a method which in turn follows Tschirnhaus's method.

Comment 7

Here is an example of the solution to an inverse tangent problem, using the method Leibniz sketches here. Suppose we are looking for a curved line AEB (Figure 30) whose ordinates are EF, whose abscissas are CF, and whose tangents EG have the property that

$$\frac{EF}{FG} = CF.$$

If we set $CF = x$ and $EF = y$, and we draw a characteristic triangle EE_1H, then $EH = dx$, and $E_1H = dy$. Since triangle EE_1H is similar to triangle GEF,

$$\frac{EF}{FG} = \frac{dy}{dx},$$

and therefore, because of our property of tangents,

$$\frac{dy}{dx} = x. \tag{6}$$

To find the curve AEB that has equation 1, we first write down a "general or indefinite equation" for it:

$$0 = a + bx + cy + exy + fx^2 + gy^2 + \text{ etc.} \tag{7}$$

This equation is general or indefinite insofar as its coefficients (a, b, c etc.) are not definite numbers, but general constants, each of which could represent any number. Such an equation can represent *any* curve that has an algebraic equation, and so, in particular, it can represent the curve AEB we are looking for *if* it has an algebraic equation.

We then use this general equation to find the tangent of the line. Our first step is to use the differential calculus to find a differential equation for the

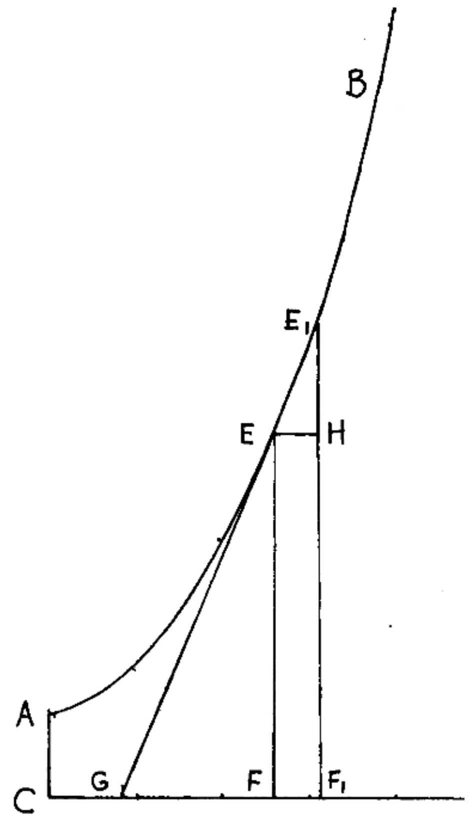


Figure 30

curve:

$$\begin{aligned}
0 &= d\left(a + bx + cy + exy + fx^2 + gy^2 + \text{ etc.}\right) \\
&= d(a) + b\,dx + c\,dy + e\,d(xy) + f\,d(x^2) + g\,d(y^2) + \text{ etc.} \\
&= 0 + b\,dx + c\,dy + e(x\,dy + y\,dx) + f(2x\,dx) + g(2y\,dy) + \text{ etc.} \\
&= b\,dx + c\,dy + ex\,dy + ey\,dx + 2fx\,dx + 2gy\,dy + \text{ etc.}
\end{aligned}$$

We can then solve this equation for the ratio of dy to dx, that is, the ratio of EF to FG, and thereby find the tangent. For (gathering terms)

$$0 = (b\,dx + ey\,dx + 2fx\,dx + \text{ etc.}) + (c\,dy + ex\,dy + 2gy\,dy + \text{ etc.,})$$

and therefore

$$0 = (b + ey + 2fx + \text{ etc.})\,dx + (c + ex + 2gy + \text{ etc.})\,dy,$$

and therefore

$$-(b + ey + 2fx + \text{ etc.})\, dx = (c + ex + 2gy + \text{ etc.})\, dy,$$

and therefore

$$\frac{-(b + ey + 2fx + \text{ etc.})}{(c + ex + 2gy + \text{ etc.})} = \frac{dy}{dx}. \tag{8}$$

Next we can "compare what [we found] with the given property of the tangents," by using equation 3 to substitute for

$$\frac{dy}{dx}$$

in equation 1, to get equation 4:

$$\frac{-(b + ey + 2fx + \text{ etc.})}{(c + ex + 2gy + \text{ etc.})} = x. \tag{9}$$

Equation 4 expresses the conditions that a, b, c, etc. must satisfy in order for equation 1 to hold and for the tangents of AEB to have the given property. We use this equation to "find out the value of the assumptive letters a, b, c, etc. For it follows from equation 4 that

$$-(b + ey + 2fx + \text{ etc.}) = x(c + ex + 2gy + \text{ etc.}),$$

and therefore

$$-(b + ey + 2fx + \text{ etc.}) = (cx + ex^2 + 2gyx + \text{ etc.}),$$

and therefore

$$0 = (b + ey + 2fx + \text{ etc.}) + (cx + ex^2 + 2gyx + \text{ etc.,})$$

and therefore

$$0 = b + (c + 2f)x + ey + ex^2 + 2gxy + \text{ etc.}$$

Now since x and y are variables and can take on any value, the only way that

$$b + (c + 2f)x + ey + 2gxy + ex^2 + \text{ etc.}$$

can always be equal to 0 is for each of its coefficients to be equal to 0, that is, for

$$\begin{aligned} b &= 0, \\ c + 2f &= 0, \\ e &= 0, \text{ etc.} \end{aligned}$$

It follows that

$$f = -\frac{c}{2},$$

and

$$b = e = g = h = \ldots = 0.$$

Note that there is no restriction on a. Substituting these values back into the general equation (equation 2), gives us a *definite* equation for AEB, that is, it enables us to "define the equation of the line sought":

$$\begin{aligned} 0 &= a + 0x + cy + 0xy - \frac{c}{2}x^2 + 0y^2 + \text{ etc.} \\ &= a + cy - \frac{c}{2}x^2. \end{aligned}$$

Simplifying this equation to solve for y gives us

$$cy = \frac{c}{2}x^2 - a, \text{ and}$$

$$y = \frac{1}{2}x^2 - \frac{a}{c}. \tag{10}$$

This is the equation for line AEB. "Some things remain arbitrary," namely, the constant,

$$\frac{a}{c},$$

because innumerably many lines solve the problem. In fact, the length of the line AC in Figure 30 is arbitrary, and corresponds to

$$-\frac{a}{c}.$$

Comment 8

The problem at the end of "A New Method" (pages 24–25) is an inverse tangent problem where "the comparison does not succeed" and the line is transcendent. Recall that in that problem we were looking for a line AEB (Figure 31) whose tangents had the property that the line GF between the tangents EG and ordinates EF was always equal to a constant line k. For simplicity, let us suppose $k = 1$. If we again set $CF = x$ and $EF = y$, then because the characteristic triangle EHE_1 is similar to triangle GFE,

$$\frac{EH}{E_1H} = \frac{EF}{GF},$$

and therefore

$$\frac{dy}{dx} = y. \tag{1}$$

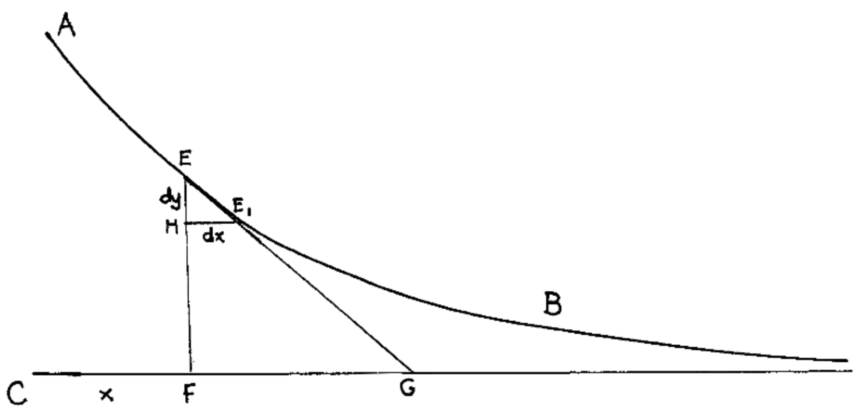


Figure 31

If, following Leibniz's method, we try to find a solution by using a general or indefinite equation

$$0 = a + bx + cy + exy + fx^2 + gy^2 + \text{ etc.}, \tag{2}$$

we will not succeed. For, proceeding as in the previous comment, we will find that

$$\frac{-(b + ey + 2fx + \text{ etc.})}{(c + ex + 2gy + \text{ etc.})} = \frac{dy}{dx}. \tag{3}$$

We can again use equation 3 to substitute for

$$\frac{dy}{dx}$$

in our new equation 1 to get a new equation 4

$$\frac{-(b + ey + 2fx + \text{ etc.})}{(c + ex + 2gy + \text{ etc.})} = y. \tag{4}$$

We use this equation to try to "find out the value of the assumptive letters a, b, c, etc. For it follows from equation 4 that

$$-(b + ey + 2fx + \text{ etc.}) = y(c + ex + 2gy + \text{ etc.}),$$

and therefore

$$-(b + ey + 2fx + \text{ etc.}) = (cy + exy + 2gy^2 + \text{ etc.}),$$

and therefore

$$0 = (b + ey + 2fx + \text{ etc.}) + (cy + exy + 2gy^2 + \text{ etc.,})$$

and therefore

$$0 = b + 2fx + (e + c)y + exy + 2gy^2 + \text{ etc.}$$

Now since x and y are variables and can take on any value, the only way that

$$b + 2fx + (e + c)y + exy + 2gy^2 + \text{ etc.}$$

can always be equal to 0 is for each of its coefficients to be equal to 0. Now we are looking for values of a, b, c, etc., such that equation 2,

$$0 = a + bx + cy + exy + fx^2 + gy^2 + \text{ etc.},$$

becomes a meaningful algebraic equation. Therefore at least one of a, b, c, etc., must not be equal to 0 (otherwise equation 2 becomes the meaningless equation $0 = 0$), and only finitely many of a, b, c, etc., can be non-zero (otherwise equation 2 would not be algebraic, but would be of infinite degree). It turns out that it is impossible in this case to find finitely many non-zero values for a, b, c, etc., such that

$$0 = b + 2fx + (e + c)y + exy + 2gy^2 + \text{ etc.}$$

(the argument is too complex to go into here). Therefore the line AEB cannot be algebraic, but must be *transcendent*.

Comment 9

When the solution to a problem is transcendent, we would like to be able to say what kind of transcendent it is. To do this, Leibniz chooses a simple given transcendent v, and tries to express the solution in terms of x and y, and this new transcendent v. For example, the solution to the problem might have the equation

$$1 + v^2 + 2vx - 3xyv + y^3v = 0.$$

The transcendent v might "depend on the general cutting of a ratio;" we will see below that such a transcendent would be a logarithm or exponential,

$$v = \log y$$

or

$$v = e^x.$$

The transcendent v might also depend on "the general cutting of an angle;" we will see below that such a transcendent would be a sine,

$$v = \sin x.$$

The transcendent v could also depend on more complex problems. Leibniz claims here that his method can show how transcendent solutions to problems are related to these simpler transcendents.

Alternative Proof of the First Fundamental Theorem

To prove:

$$\int dv = v.$$

Let there be a curve Q, with ordinates y and abscissas x, where $y = 0$ when $x = 0$. Let there be another curve R, with ordinates v and abscissas x, where $v = 0$ when $x = 0$. And let every tangent to R have an inclination defined by y, such that

$$y = \frac{v}{XA}, \tag{1}$$

where XA is the distance on x between v and the corresponding tangent. In other words, let every tangent to R at point (x, v) on the ordinate intersect point $(x - v/y, 0)$ on the abscissa.

From Leibniz, we know in general that

$$\frac{v}{XA} = \frac{dv}{dx}.$$

And from (1), it follows for Q and R that

$$ydx = dv. \tag{2}$$

From Leibniz, we can read (2) to mean that, along abscissa x, any increment ydx of the area under Q will equal some increment dv of the ordinate under R, and vice versa.

Taking sums of both of sides of (2),

$$\int ydx = \int dv. \tag{3}$$

From Leibniz, we can read (3) to mean that, along abscissa x, any sum of area-increments under Q will equal a sum of ordinate-increments under R, and vice versa.

But $v = 0$ when $x = 0$. So the sum of ordinate-increments $\int dv$ begins where the ordinate begins, and therefore equals an ordinate v under R. It would follow that $\int dv = v$ if $\int ydx = v$. That is, the conclusion would follow if the sums $\int dv$ and $\int ydx$ equal the same v rather than different v's.

But $y = 0$ when $x = 0$. So the sum of area-increments $\int ydx$ begins where the area begins, and therefore equals an area under Q. This area, in turn, equals the ordinate v under R, since it too began at $x = 0$, as did the sum of its increments equal by (3) to the area's sum.

So,

$$\int ydx = v.$$

Therefore:

$$\int dv = v. \tag{C}$$

Note: Since Q is thus a quadranda, and R its quadratrix, the first theorem implies that (1) is a sufficient condition of quadrature.

Alternative Proof of the Second Fundamental Theorem

To prove:

$$d\int dv = dv.$$

Let there be a curve Q, with ordinates y and abscissas x. Let there be another curve R, with ordinates v and abscissas x. And along abscissa x, let the area under Q always equal the ordinate under R. From Leibniz, it follows that

$$\int y dx = v. \tag{4}$$

Taking differences of both sides of (4),

$$d\int y dx = dv. \tag{5}$$

From Leibniz, we can read (5) to mean that the increment of a sum of area-increments under Q will equal some increment of the ordinate under R, and vice versa. But since the sum in this case consists of area-increments, the increment of this sum will be an area-increment. That is, in this case $d\int ydx = ydx$, and thus

$$ydx = dv. \tag{6}$$

It would follow from (5) and (6) that $d\int dv = dv$, if $\int dv = v$. That is: the conclusion would follow if (6) could be read generally, where *any* area-increment in $\int ydx$ equals some ordinate-increment in v, and not just the one area-increment ydx we got from $d\int ydx$ to get (6). For then $\int dv = v$ from (4).

But we know from Leibniz that dv/dx is a general feature of any curve. For all curves in general,

$$\frac{dv}{dx} = \frac{v}{XA},$$

where XA is the distance on x between v and the corresponding tangent. And from (6), then, we can infer that for Q and R in general,

$$\frac{dv}{dx} = \frac{v}{XA} = y. \tag{7}$$

That is, since y is determined by dv and dx in (6), y is general, making ydx general, and its equality with dv general. So, taking the sums of both sides of (6),

$$\int ydx = \int dv.$$

And from (4):

$$\int dv = v.$$

Therefore, from (5):

$$d\int dv = dv. \tag{C}$$

Note: Since Q was assumed a quadranda, and R its quadratrix, the second theorem implies that (7), i.e. (1), is a necessary condition of quadrature.

and

$$dv = v_1 - v = ACF_1E_1 - ACFE = EFF_1E_1.$$

But since E and E_1 are infinitely close, we may take $EF = E_1F_1$ and treat EFF_1E_1 as a rectangle. Its area is therefore equal to EF times FF_1, that is, to $y\,dx$. Therefore

$$dv = y\,dx,$$

that is,

$$d\int y\,dx = y\,dx.$$

Q. E. D.

The second fundamental theorem says that any infinitely small variable quantity $y\,dx$, is equal to the difference of its sums.

3. Examples of determining algebraic quadratrices

In all the following examples we are given a curved line ADB (Figure 22) whose axis is AEC and whose ordinates are DE. Let $AE = x$ and $DE = y$. Let the curve AFG be the quadratrix of the curve ADB (the quadranda), that is, let the ordinate EF of AFG always be equal to the curvilinear area ADE. Let $v = EF$, so that v is always equal to the area ADE. Then

$$v = \int y\,dx.$$

To find an equation for the quadratrix AFG we have to find an equation relating v to x, that is we have to find the area $\int y\,dx$ in terms of x.

To find v, we use the second fundamental theorem. If

$$v = \int y\,dx,$$

then, according to the second fundamental theorem,

$$dv = y\,dx.$$

Therefore, given y, we need to find a quantity v such that dv/dx is equal to y; that is, we need to find a quantity v whose differences are equal to $y\,dx$. Thus finding sums is in general much more difficult than finding differences. To find differences we simply have to follow mechanically the rules Leibniz has given us in "A New Method." But there is no complete set of rules for going backwards and, given a quantity y, to find a quantity v whose differences are equal to $y\,dx$. In an earlier paper Leibniz writes that

> But this is the labor, this is the task: given a Quadranda, to find some Quadratrix for it; this is especially difficult because sometimes it is impossible to find a quadratrix (at least one that can be expressed algebraically).[7]

[7] "On Finding Measurements of Figures," published in the *Acts* in May of 1684. It is on pages 123–6 in Volume V of Gerhardt's edition.

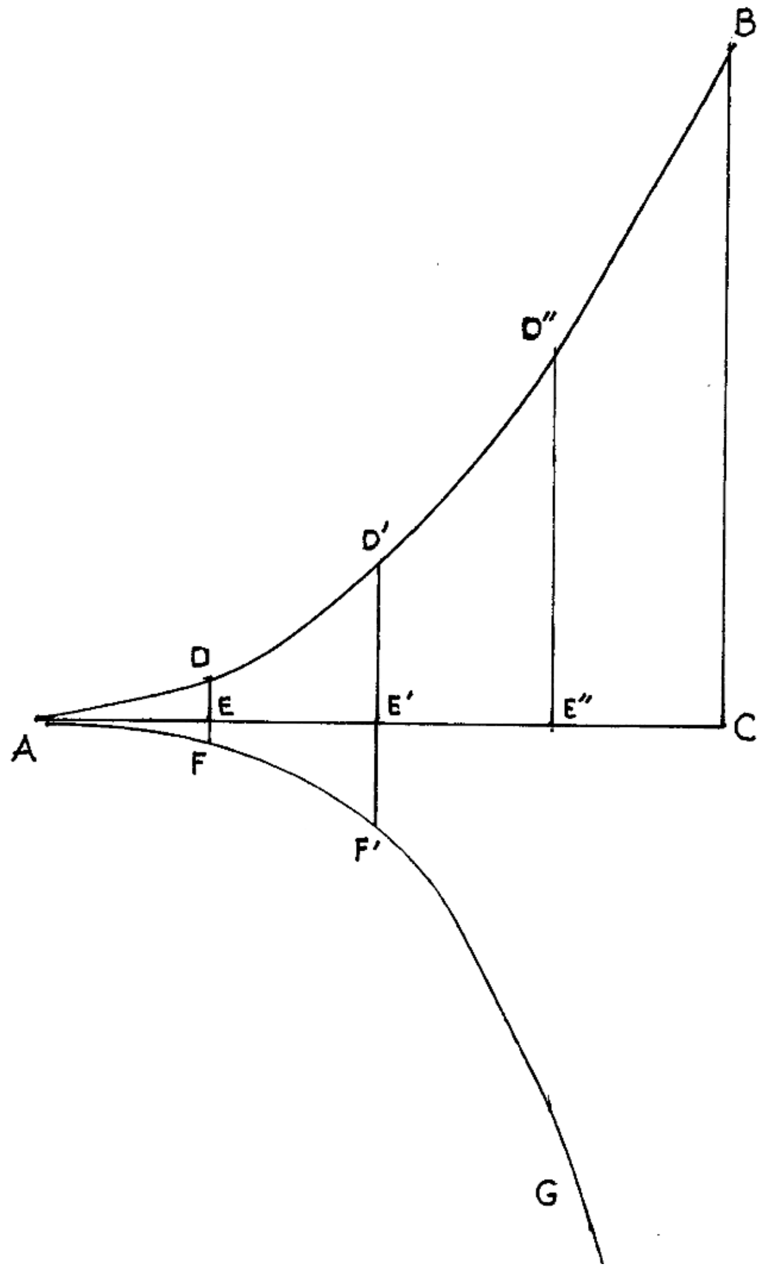


Figure 22

He is alluding here to Book VI of Virgil's *Aeneid* where the Cumaean Sybil says to Aeneas

> Born of the blood
> of gods and son of Troy's Anchises, easy —
> the way that leads to into Avernus: day
> and night the door of darkest Dis is open.
> But to recall your steps, to rise again
> into the upper air: that is the labor;
> that is the task.[8]

While there is no general method for finding sums, there is one difficulty that always arises and may be treated methodically. Let

$$v = \int y\,dx,$$

so that, by the second fundamental theorem,

$$dv = y\,dx.$$

Suppose we have managed to find an expression for a quantity w such that $dw = y\,dx$. Then $dv = dw$, and therefore $d(v - w) = 0$, and $v - w$ must be constant. Let $v - w = C$. Then $v = w + C$. To complete the solution of the problem, we need to find C. We can do so by setting $w + C = 0$ at the point where the sum begins.

Here are some examples.

1. Let $y = x^2$, so that $v = \text{area } ADE = \int y\,dx = \int x^2\,dx$. According to the second fundamental theorem, $dv = x^2\,dx$. So let us find an expression w such that $dw = x^2\,dx$, then set $v = w + C$ for some constant C, and, finally, solve for C.

   First we need to find w such that $dw = x^2\,dx$. We know from the rules of the calculus that

$$\begin{aligned} d\left(\frac{x^3}{3}\right) &= \frac{d(x^3)}{3} \\ &= \frac{3x^2\,dx}{3} \\ &= x^2\,dx \\ &= y\,dx. \end{aligned}$$

   Therefore let us set $w = \dfrac{x^3}{3}$, so that $v = w + C = \dfrac{x^3}{3} + C$.

[8] Lines 174–180 of Allen Mandelbaum's translation. Lines 125–129 of R. A. B. Mynors's Latin text.

Finally, to find C, we set $w + C = 0$ at the point where the sum begins. Here the sum begins where $x = AE = 0$. Therefore, when $x = 0$,

$$\frac{x^3}{3} + C = 0,$$

that is, $0 + C = C = 0$. Therefore

$$v = w = \frac{x^3}{3}.$$

We conclude that

$$\text{area } ADE = v = \frac{x^3}{3}.$$

Therefore the equation of the quadratrix AFG is in this case

$$v = \frac{x^3}{3}.$$

2. Let $y = x^3$, so that $v = \int y\,dx = \int x^3\,dx$. According to the second fundamental theorem, $dv = x^3\,dx$. So let us find an expression w such that $dw = x^3\,dx$, then set $v = w + C$ for some constant C, and, finally solve for C.

   First we need to find some w such that $dw = x^3\,dx$. We know from the rules of the calculus that

$$\begin{aligned} d\left(\frac{x^4}{4}\right) &= \frac{d(x^4)}{4} \\ &= \frac{4x^3\,dx}{4} \\ &= x^3\,dx \\ &= y\,dx. \end{aligned}$$

   Therefore let us set $w = \dfrac{x^4}{4}$, so that $v = w + C = \dfrac{x^4}{4} + C$.

   Finally, to find C, we set $w + C = 0$ at the point where the sum begins. Here the sum begins where $x = AE = 0$. Therefore, when $x = 0$,

$$\frac{x^4}{4} + C = 0,$$

   that is, $0 + C = C = 0$. Therefore

$$v = w = \frac{x^4}{4}.$$

   We conclude that

$$\text{area } ADE = v = \frac{x^4}{4}.$$

Therefore the equation of the quadratrix AFG is in this case

$$v = \frac{x^4}{4}.$$

3. Let $y = x^n$, where n is any nonnegative number. We proceed just as in the previous two examples, finding a quantity w such that $y\,dx = dw$, setting $v = w + C$, an solving for C by setting $w + C = 0$ when $x = 0$.

   We know from the rules of calculus that

$$\begin{aligned} d\left(\frac{x^{(n+1)}}{n+1}\right) &= \frac{d(x^{(n+1)})}{n+1} \\ &= \frac{(n+1)x^n\,dx}{n+1} \\ &= x^n\,dx \\ &= y\,dx. \end{aligned}$$

   We therefore set

$$w = \frac{x^{(n+a)}}{n+1}.$$

   so that

$$v = \frac{x^{(n+1)}}{n+1} + C.$$

   Finally, to find C, we set $w + C = 0$ at the point where the sum begins. Here again the sum begins where $x = AE = 0$. Therefore, when $x = 0$,

$$\frac{x^{(n+1)}}{n+1} + C = 0,$$

   that is, $0 + C = C = 0$. Therefore

$$v = w = \frac{x^{(n+1)}}{n+1}.$$

   We conclude that

$$\text{area } ADE = v = \frac{x^{(n+1)}}{n+1}.$$

   Therefore the equation of the quadratrix AFG is in this case

$$v = \frac{x^{(n+1)}}{n+1}.$$

4. Let $y = x^3 + x^2$. Then, if we set

$$w = \frac{x^4}{4} + \frac{x^3}{3},$$

according to the rules of calculus,

$$dw = x^3\,dx + x^2\,dx = y\,dx.$$

Therefore

$$w + C = \int y\,dx.$$

The quantity $w + C$ must be equal to zero when $x = 0$, and therefore

$$\frac{0^4}{4} + \frac{0^3}{3} + C = C = 0.$$

Therefore

$$\int y\,dx = w + 0 = \frac{x^4}{4} + \frac{x^3}{3}.$$

Note that here it turns out that

$$\int (x^3 + x^2)\,dx = \int x^3\,dx + \int x^2\,dx.$$

This is generally true: for any variable quantities t and u,

$$\int (t + u) = \int t + \int u.$$

For if $t = dv$ and $u = dw$, then

$$d(v + w) = dv + dw = t + u,$$

and if we begin the sums when $v = 0$ and $w = 0$ then we will also begin the sums where $v + w = 0$, and according to the first fundamental theorem,

$$\begin{aligned}\int (t + u) &= \int d(v + w)\\ &= (v + w)\\ &= \int dv + \int dw\\ &= \int t + \int u.\end{aligned}$$

We might call this the *addition rule for sums*. We could likewise show that for any constant a and any variable t

$$\int at = a \int t.$$

This could be called *constant multiple rule for sums*.

We can use these rules, and the rule from the third example, to find sums for many algebraic expressions, as in the following example.

5. Let $y = 3x^5 - 8x^2 + 4$. Then

$$\begin{aligned}\int y\,dx &= 3\int x^5\,dx - 8\int x^2\,dx + 4\int x^0\,dx \\ &= 3\frac{x^6}{6} - 8\frac{x^3}{3} + 4\frac{x^1}{1} \\ &= \frac{x^6}{2} - \frac{8x^3}{3} + 4x.\end{aligned}$$

6. Let

$$y = 2\sqrt{x} - 8x^{\frac{5}{3}}.$$

Then

$$\begin{aligned}\int y\,dx &= 2\int x^{\frac{1}{2}}\,dx - 8\int x^{\frac{5}{3}}\,dx \\ &= 2\left(\frac{x^{\frac{3}{2}}}{\frac{3}{2}}\right) - 8\left(\frac{x^{\frac{8}{3}}}{\frac{8}{3}}\right) \\ &= \frac{4x^{\frac{3}{2}}}{3} - 3x^{\frac{8}{3}}.\end{aligned}$$

Now suppose that we are interested not in the area ADE, but in the area DEE_1D_1 (see Figure 23) between two definite ordinates, DE and DE_1. Let $AE = a$ and $AE_1 = b$, where a and b are constants. This area is equal to the sum of $y\,dx$ between $x = a$ and $x = b$, which we denote by

$$\int_a^b y\,dx.$$

This sum is called a *definite integral*, because, unlike the sums in the previous examples, it represents a single constant area, and not a variable area. To find the area DEE_1D_1, we take the difference of the area AD_1E_1 (this area is equal to the value of v when we set $x = b$, which we will call v_b) and the area ADE (this area is equal to the value of v when we set $x = a$, which we will call v_a):

$$\begin{aligned}\text{area } DEE_1D_1 &= \text{area } AD_1E_1 - \text{area } ADE \\ &= v_b - v_a.\end{aligned}$$

In finding definite integrals, we proceed in the same way as before, first finding a quantity w such that $dw = y\,dx = dv$. It follows that $v = w + C$ for some C. Therefore the definite integral

$$\begin{aligned}\int_a^b y\,dx =& v_b - v_a \\ =& w_b + C - w_a + C \\ =& w_b - w_a.\end{aligned}$$

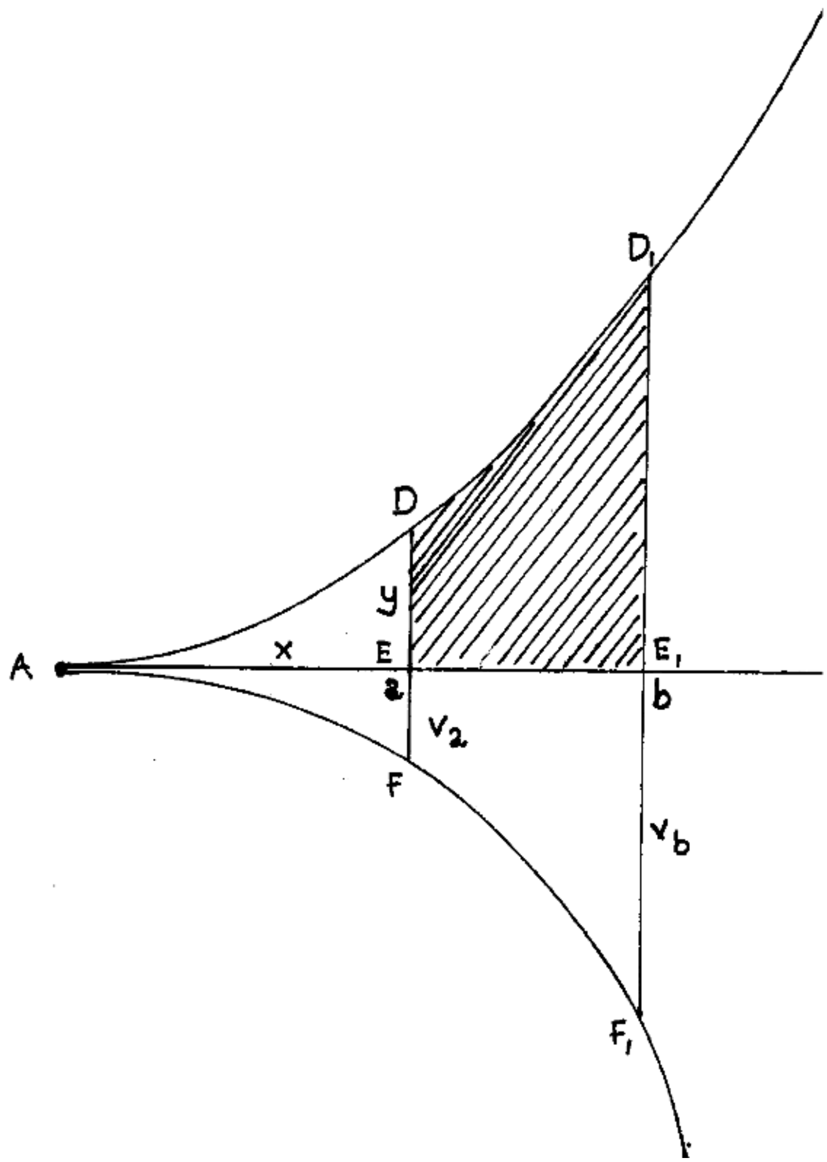


Figure 23

Thus we can skip here the step of finding C: we simply find any quantity w whose differences are equal to the quantity $y\,dx$ we want to integrate, and take the difference of its value at the last point of the sum and its value at the first. Of course, if it is convenient, we can always set $w = v$. It usually will be convenient to do so when y is a simple algebraic quantity, but when y is transcendent this is generally not the case, as we will see below.

Here are two simple examples of definite integrals where y is an algebraic quantity.

7. Let $y = x^2$. Then, as we saw above (page 113), if

$$w = \frac{x^3}{3},$$

then $dw = x^2\,dx$. (Here $w = v$, but we could use $w = v + C$ for any C.) Now if $a = AE = 2$ and $b = AE_1 = 4$, then

$$\begin{aligned} \text{area } DEE_1D_1 &= \int_2^4 y\,dx \\ &= w_4 - w_2 \\ &= \frac{4^3}{3} - \frac{2^3}{3} \\ &= \frac{64}{3} - \frac{8}{3} \\ &= \frac{56}{3}. \end{aligned}$$

8. Let $y = 3x^2 + 7x$. Then

$$\begin{aligned} v &= \int y\,dx \\ &= 3\int x^2\,dx + 7\int x\,dx \\ &= 3\frac{x^3}{3} + 7\frac{x^2}{2} \\ &= x^3 + \frac{7x^2}{2}. \end{aligned}$$

Here we could take $w = v + C$ for any value of C, but it is simplest to set

$C = 0$ and use v by itself. Now if $a = AE = 1$ and $b = AE_1 = 5$, then

$$\begin{aligned}
\text{area } DEE_1D_1 &= \int_1^5 y\,dx \\
&= v_5 - v_1 \\
&= \left(5^3 + \frac{7(5^2)}{2}\right) - \left(1^3 + \frac{7(1^2)}{2}\right) \\
&= \left(125 + \frac{175}{2}\right) - \left(1 + \frac{7}{2}\right) \\
&= \frac{425}{2} - \frac{9}{2} \\
&= \frac{416}{2} \\
&= 213.
\end{aligned}$$

Some problems on sums of algebraic quantities

Find the following sums.

1. $$\int (2x^3 - x + 4)\,dx.$$

2. $$\int (3x^5 - 2x^2 + 1)\,dx.$$

3. $$\int (\sqrt{x} + (\sqrt{x})^3)\,dx.$$

4. $$\int (x^3 + \sqrt[3]{x})\,dx.$$

5. $$\int_1^3 x^3\,dx.$$

6. $$\int_{-1}^1 x^4\,dx.$$

7. $$\int_1^2 (5x^4 - 2x)\,dx.$$

8. $$\int_{-1}^2 (2x^2 + 1)\,dx.$$

4. Let

$$z = \sin(\omega t),$$

where ω is some constant. To find dz in terms of t. Let $v = \omega t$, so that

$$z = \sin v.$$

Therefore, by the first example, above,

$$dz = \cos v\, dv;$$

and

$$dv = \omega\, dt.$$

Therefore

$$\begin{aligned} dz &= \cos v\, dv \\ &= \cos(\omega t)\, dv \\ &= \cos(\omega t)\, \omega\, dt \\ &= \omega \cos(\omega t)\, dt. \end{aligned}$$

5. Let

$$z = \cos^2(4a).$$

To find dz in terms of a. Here we let $v = \cos(4a)$, so that $z = v^2$ and

$$dz = 2v\, dv.$$

To find dv , we let $u = 4a$, so that $v = \cos u$ and

$$dv = -\sin u\, du = -\sin(4a)\, du$$

by the second example, above. Finally, according to the constant multiple rule,

$$du = d(4a) = 4\, da.$$

Therefore

$$\begin{aligned} dz &= 2v\, dv \\ &= 2(\cos(4a))(-\sin(4a)\, du) \\ &= 2(\cos(4a))(-\sin(4a))(4\, da) \\ &= -8\cos(4a)\sin(4a)\, da. \end{aligned}$$

6. To find $\int \sin a\, da$, that is, area FLM in Figure 25, we proceed as we did for algebraic quantities (page 112 and what follows). First, we need to find a quantity w such that

$$dw = \sin a\, da.$$

It follows from the second example, above, that

$$d(-\cos a) = \sin a\, da.$$

Therefore we set $w = -\cos a\, da$. Then we know by the second fundamental theorem that

$$dw = d\int \sin a\, da,$$

and therefore

$$w = \int \sin a\, da$$

is constant, that is, there is some constant C such that

$$\begin{aligned}\int \sin a\, da =& w + C \\ =& -\cos a + C.\end{aligned}$$

To find the value of C here, note that our sum begins when $a = 0$. Therefore when $a = 0$,

$$\int \sin a\, da = -\cos a + C = 0.$$

Therefore

$$-\cos 0 + C = 0.$$

Since $\cos 0 = 1$, it follows that $C = 1$ and

$$\int \sin a\, da = 1 - \cos a.$$

7. To find the definite integral

$$\int_{\frac{\pi}{4}}^{\frac{\pi}{2}} \sin a\, da.$$

(See page 116, above, for a discussion of definite integrals.)

This sum is equal to the area LMM_1L_1, where

$$FM = \frac{\pi}{4}$$

and

$$FM_1 = \frac{\pi}{2}$$

(see Figure 26). This area is equal to the difference of area FM_1L_1 and area FML .

Here we proceed in the same way as we did in finding definite integrals of algebraic quantities: we find a w such that $dw = y\, dx$. Then for some

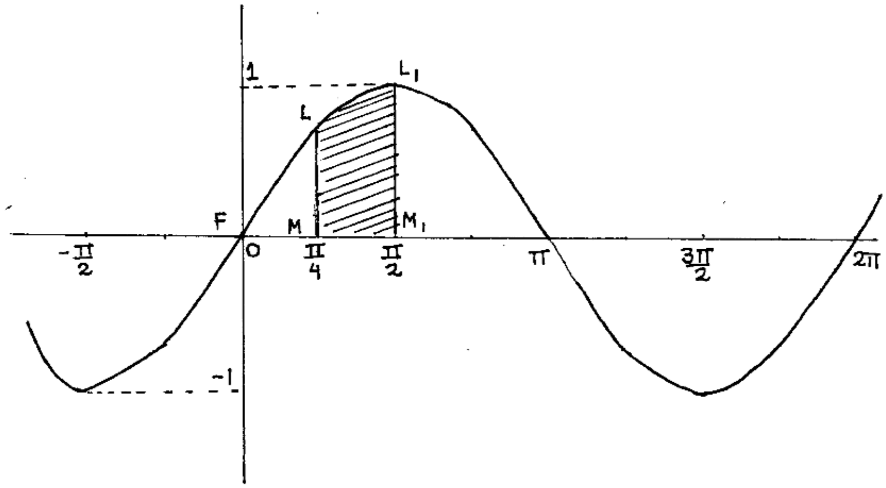


Figure 26

constant C the area FML is equal to $w+C$ evaluated when $a = \pi/4$, while the area FM_1L_1 is equal to $w + C$ evaluated when $a = \pi/2$. The area LMM_1L_1 is therefore the difference between $w + C$ evaluated at $a = \pi/2$ and $w + C$ evaluated at $a = \pi/4$, that is, the difference

$$w_{\frac{\pi}{2}} - w_{\frac{\pi}{4}}.$$

As we saw in the previous example, if $w = -\cos a$ then $dw = \sin a\, da$. Therefore the definite integral

$$\int_{\frac{\pi}{4}}^{\frac{\pi}{2}} \sin a\, da$$

is equal to the difference

$$\begin{aligned} w_{\frac{\pi}{2}} - w_{\frac{\pi}{4}} &= -\cos\left(\frac{\pi}{2}\right) - (-\cos\left(\frac{\pi}{4}\right)) \\ &= \cos\left(\frac{\pi}{4}\right). \end{aligned}$$

(It turns out that, by a trigonometric argument that is not worth going into here,

$$\cos\left(\frac{\pi}{4}\right) = \frac{\sqrt{2}}{2},$$

so that

$$\text{area } LMM_1L = \frac{\sqrt{2}}{2} \approx .7071.)$$

Sums of quantities involving logarithms

1. Let $y = e^x$. To find $\int y\,dx$, that is, area $FAED$ in Figure 27 (page 129), in terms of x. To do this, we first need to find a quantity v such that $dw = e^x\,dx$. According to equation 4 on page 72, above,

$$d(e^x) = e^x\,dx.$$

Therefore we let $w = e^x$, and it follows that

$$\int e^x\,dx = w + C = e^x + C.$$

To find C, we set $w + C = 0$ at the beginning point of the sum, where $x = 0$:

$$e^0 + C = 1.$$

Since $e^0 = 1$, it follows that $C = -1$ and

$$\int e^x\,dx = e^x - 1.$$

2. Let $y = e^{2x}$. To find $\int y\,dx$ in terms of x. To do this, let

$$u = 2x.$$

Then

$$y = e^u$$

and, by the constant multiple rule,

$$du = 2\,dx.$$

Therefore

$$dx = \frac{1}{2}\,du.$$

Therefore

$$\begin{aligned}
\int y\,dx &= \int (e^u)\left(\frac{1}{2}\,du\right) \\
&= \frac{1}{2}\int e^u\,du \\
&= \frac{1}{2}(e^u - 1) && \text{(previous example)} \\
&= \frac{1}{2}(e^{2x} - 1).
\end{aligned}$$

The method we have used in this example is called *integration by substitution.* It is useful whenever we can find another variable u such that

$$\int y\,dx$$

can more easily be found when it is expressed in terms of u. There are no universal rules for determining what new variable u to substitute, and it can even be difficult to see in advance that substitution is a useful method for a given problem.

3. Let $y = x\sin(x^2)$. To find $\int y\,dx$ in terms of x. To do this, let

$$u = x^2.$$

Then

$$du = 2x\,dx$$

and

$$\begin{aligned}\frac{1}{2}\sin(u)\,du &= \frac{1}{2}\sin(x^2)(2x\,dx)\\ &= \sin(x^2)\,x\,dx\\ &= x\sin(x^2)\,dx\\ &= y\,dx.\end{aligned}$$

Therefore

$$\begin{aligned}\int y\,dx &= \int \frac{1}{2}\sin(u)\,du\\ &= \frac{1}{2}\int \sin(u)\,du\\ &= \frac{1}{2}(1-\cos(u)) \qquad \text{(example 6, page 125)}\\ &= \frac{1}{2}(1-\cos(x^2)).\end{aligned}$$

4. Let $y = xe^x$. To find $\int y\,dx$ in terms of x.

Let $u = x$ and $v = e^x$. Then $dv = e^x\,dx$, and therefore

$$y\,dx = u\,dv.$$

Now, according to the multiplication rule,

$$d(uv) = u\,dv + v\,du,$$

and therefore

$$u\,dv = d(uv) - v\,du.$$

Therefore

$$\begin{aligned}
\int y\,dx &= \int u\,dv \\
&= \int (d(uv) - v\,du) \\
&= \int d(uv) - \int v\,du \\
&= uv - \int v\,du \qquad \text{(first fundamental theorem)} \\
&= xe^x - \int e^x\,dx \\
&= xe^x - (e^x - 1) \qquad \text{(first example)}
\end{aligned}$$

The method we have used in this example is called *integration by parts*: for any sum $\int y\,dx$, if we can find u and v such that $y\,dx = u\,dv$, then

$$\int y\,dx = uv - \int v\,du.$$

This method is useful when it is easier to find $\int v\,du$ than it is to find $\int u\,dv$.

5. Let $y = x\cos x\,dx$. To find $\int y\,dx$ in terms of x.

   Let $u = x$ and $v = \sin x\,dx$. Then $dv = \cos x\,dx$, and therefore

   $$y\,da = u\,dv.$$

   As in the previous example, it follows that

   $$\int y\,dx = uv - \int v\,du.$$

   In this case,

   $$\begin{aligned}
   \int y\,dx &= uv - \int v\,du \\
   &= x\sin x - \int \sin x\,dx \\
   &= x\sin x - (1 - \cos x\,dx) \\
   &= x\sin x + \cos x - 1.
   \end{aligned}$$

Some problems on sums involving logarithms

Using the first fundamental theorem and the rules of the differential calculus, find the following sums.

1. $$\int (3e^x + x^2)\,dx.$$

G. W. Leibniz
Philosophical Papers & Letters,
Loemker. Reidel

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LETTER TO VARIGNON, WITH A NOTE ON THE 'JUSTIFICATION OF THE INFINITESIMAL CALCULUS BY THAT OF ORDINARY ALGEBRA'

1702

The criticisms to which the new infinitesimal calculus was subjected during the last decade of the 17th century were often grounded upon nothing more than a failure to understand its value or a distrust of novelty. Thus Huygens himself, and the Abbé Gallois, an editor of the Journal des savants, *seem not at once to have grasped the importance of the new instrument. With more veneration for Descartes than insight into mathematics, the Abbé Catelan once more rushed into print. Among the criticisms, however, was one aimed at an unclear foundation of the calculus itself, the uncertain status and nature of the infinite and the infinitesimals which were used. In 1694 and again in 1695 this issue was raised by Bernard Nieuwentijt in two criticisms of the calculus; later the criticism was developed more convincingly by Michel Rollé, an abler mathematician. Among the defenders of the new methods were the Bernoulli brothers (cf. No. 53); the Marquis de l'Hospital, whose* Analyse des infiniment petits *(1696) was the first textbook in the field (cf. p. 420, note 8); Jacob Hermann (1678–1733); and Pierre Varignon (1654–1722), who had already made contributions to statics and was later to develop polar coordinates.*

Varignon was engaged at the time of this letter in replying to Rollé's criticisms and had asked Leibniz what he meant precisely by the infinitely small. The latter's reply was published in the Journal des savants, *March 20, 1702; it suggested several levels of interpretation upon which the user of the calculus might stand. The supplement (II) had been sent to Pinson and Varignon sometime earlier and appeared in the* Mémoires de Trévoux, *January, 1701.*

I. LETTER TO VARIGNON

[GM., IV, 91–95]

Hanover, February 2, 1702

I am a little late in replying to the letter with which you honored me on November 29 of last year but which I did not receive until today. After Mr. Bernoulli sent it to me from Groningen, it did not arrive in Berlin until after I had left to return to Hanover with the Queen of Prussia, Her Majesty having been so gracious as to ask that I be in her suite. This had delayed my return.

I am greatly obliged to you, Sir, and to your learned men who have done me the honor of reflecting upon what I wrote to one of my friends in reply to the criticisms against the calculus of differences and sums which were published in the *Journal de Trévoux*. I do not recall exactly what expressions I may have used, but my intention was to point out that it is unnecessary to make mathematical analysis depend on

metaphysical controversies or to make sure that there are lines in nature which are infinitely small in a rigorous sense in contrast to our ordinary lines, or as a result, that there are lines infinitely greater than our ordinary ones (yet with ends; this is important inasmuch as it has seemed to me that the infinite, taken in a rigorous sense, must have its source in the unterminated; otherwise I see no way of finding an adequate ground for distinguishing it from the finite).[1] This is why I believed that in order to avoid subtleties and to make my reasoning clear to everyone, it would suffice here to explain the infinite through the incomparable, that is, to think of quantities incomparably greater or smaller than ours. This would provide as many degrees of incomparability as we may wish, since that which is incomparably much smaller has no value whatever in relation to the calculation of values which are incomparably greater than it. It is in this sense that a bit of magnetic matter which passes through glass is not comparable to a grain of sand, or this grain of sand to the terrestrial globe, or the globe to the firmament. It was to make this point that I once submitted some lemmas on incomparables to the Leipzig *Acts*, which could be understood as applicable either to infinites in the rigorous sense or merely to magnitudes which do not need to be considered in relation to others.

But at the same time we must consider that these incomparable magnitudes themselves, as commonly understood, are not at all fixed or determined but can be taken to be as small as we wish in our geometrical reasoning and so have the effect of the infinitely small in the rigorous sense. If any opponent tries to contradict this proposition, it follows from our calculus that the error will be less than any possible assignable error, since it is in our power to make this incomparably small magnitude small enough for this purpose, inasmuch as we can always take a magnitude as small as we wish. Perhaps this is what you mean, Sir, when you speak of the inexhaustible, and the rigorous demonstration of the infinitesimal calculus which we use undoubtedly is to be found here.[2] It has the advantage of giving such a proof visibly and directly and in a way well fitted to reveal the source of the invention, while the ancients, like Archimedes, gave it indirectly in the form of their reductions to the absurd; but they were unable to arrive at complicated truths or solutions in default of such a calculus, though they possessed the foundation of the invention. It follows from this that even if someone refuses to admit infinite and infinitesimal lines in a rigorous metaphysical sense and as real things, he can still use them with confidence as ideal concepts which shorten his reasoning, similar to what we call imaginary roots in the ordinary algebra, for example, $\sqrt{-2}$. Even though these are called imaginary, they continue to be useful and even necessary in expressing real magnitudes analytically. For example, it is impossible to express the analytic value of a straight line necessary to trisect a given angle without the aid of imaginaries. Just so it is impossible to establish our calculus of transcendent curves without using differences which are on the point of vanishing, and at last taking the incomparably small in place of the quantity to which we can assign smaller values to infinity. In the same way we can also conceive of dimensions beyond three, and even of powers whose exponents are not ordinary numbers – all in order to establish ideas fitting to shorten our reasoning and founded on realities.

Yet we must not imagine that this explanation debases the science of the infinite and reduces it to fictions, for there always remains a 'syncategorematic' infinite, as the Scholastics say.[3] And it remains true, for example, that $2=\frac{1}{1}+\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{16}+\frac{1}{32}+\cdots$,

For references see p. 546

which is an infinite series containing all the fractions whose numerators are 1 and whose denominators are a geometric progression of powers of 2, although only ordinary numbers are used, and no infinitely small fraction, or one whose denominator is an infinite number, ever occurs in it. Furthermore, imaginary roots likewise have a real foundation [*fundamentum in re*]. So when I told the late Mr. Huygens that $\sqrt{1+\sqrt{-3}}+\sqrt{1-\sqrt{-3}}=\sqrt{6}$, he found this so remarkable that he replied that there is something incomprehensible to us in the matter. So it can also be said that infinites and infinitesimals are grounded in such a way that everything in geometry, and even in nature, takes place as if they were perfect realities. Witness not only our geometrical analysis of transcendental curves but also my law of continuity, by virtue of which we may consider rest as infinitely small motion (that is, as equivalent to a particular instance of its own contradictory), coincidence as infinitely small distance, equality as the limit of inequalities, etc. This law I once explained and applied in Mr. Bayle's *Nouvelles de la république des lettres* and applied to the rules of motion of Descartes and Father Malebranche. I have since observed, by the second edition of the latter's rules which has since appeared, that the entire force of this principle is not yet understood.[4]

Yet one can say in general that though continuity is something ideal and there is never anything in nature with perfectly uniform parts, the real, in turn, never ceases to be governed perfectly by the ideal and the abstract and that the rules of the finite are found to succeed in the infinite – as if there were atoms, that is, elements of an assignable size in nature, although there are none because matter is actually divisible without limit. And conversely the rules of the infinite apply to the finite, as if there were infinitely small metaphysical beings, although we have no need of them, and the division of matter never does proceed to infinitely small particles. This is because everything is governed by reason; otherwise there could be no science and no rule, and this would not at all conform with the nature of the sovereign principle.

For the rest, when my reading of the *Journal de Trévoux* brought me to write about the attacks made there against the differential calculus, I assert that I was not thinking of the controversy which you, or rather those who use the calculus, are having with Mr. Rollé. It is only since your last letter, too, that I learned that the Abbé Gallois, whom I always honor greatly, has taken part. Perhaps his opposition comes only from his belief that we have founded the demonstration of this calculus on metaphysical paradoxes which I myself believe we can well discard. ... I even find that it means much in establishing sound foundations for a science that it should have such critics. It is thus that the skeptics, with as much reason, fought the principles of geometry; that Father Gottignies, a Jesuit scholar, tried to throw out the best foundations of algebra; and that Mr. Cluver and Mr. Nieuwentijt have recently attacked our infinitesimal calculus, though on different grounds. Geometry and algebra have survived, and I hope that our science of infinites will survive also. But it will always owe you a great debt for the light which you have shed upon it. I have often thought that a reply by a geometrician to the objections of Sextus Empiricus and to the things which Francis Sanchez[5], author of the book *Quod nihil scitur*, sent to Clavius, or to similar critics, would be something more useful than we can imagine. This is why we have no reason to regret the pains which are necessary to justify our analysis for all kinds of minds capable of understanding it. ...

II. JUSTIFICATION OF THE INFINITESIMAL CALCULUS BY THAT OF ORDINARY ALGEBRA

[GM., IV, 104–6]

Let two straight lines AX and EY meet at C, and from points E and Y drop EA and YX perpendicular to the straight line AX. Call AC, c and AE, e; AX, x and XY, y (Figure 36). Then since triangles CAE and CXY are similar, it follows that $(x-c)/y=c/e$.

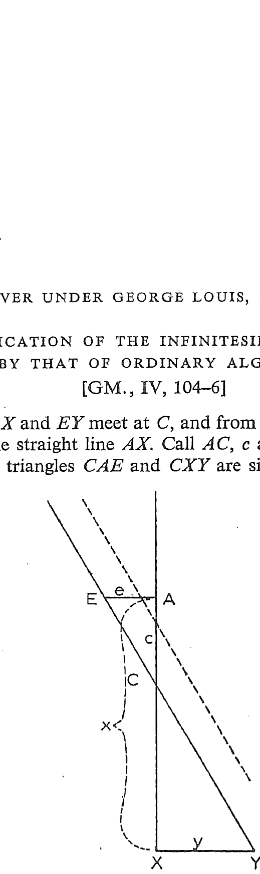

Fig. 36.

Consequently, if the straight line EY more and more approaches the point A, always preserving the same angle at the variable point C, the straight lines c and e will obviously diminish steadily, yet the ratio of c to e will remain constant. Here we assume that this ratio is other than 1 and that the given angle is other than 45°.

Now assume the case when the straight line EY passes through A itself; it is obvious that the points C and E will fall on A, that the straight lines AC and AE, or c and e, will vanish, and that the proportion or equation $(x-c)/y=c/e$ will become $x/y=c/e$. Then in the present case, assuming that it falls under the general rule, $x-c=x$. Yet c and e will not be absolutely nothing, since they still preserve the ratio of CX to XY, or the ratio between the entire radius and the tangent of the angle at C[6], the angle which we assumed to remain always the same as EY approached the point A. For if c and e were nothing in an absolute sense in this calculation, in the case when the points C, E, and A coincide, c and e would be equal, since one zero equals another, and the equation or proportion $x/y=c/e$ would become $x/y=0/0=1$; that is, $x=y$, which is an absurdity, since we assumed that the angle is not 45°. Hence c and e are not taken for zeros in this algebraic calculus, except comparatively in relation to x and y; but c and e still have an algebraic relation to each other. And so they are treated as infinitesimals, exactly as are the elements which our differential calculus recognizes in the ordinates of curves for momentary increments and decrements. Thus we find in the calculations of ordinary algebra traces of the transcendent dif-

For references see p. 546

ferential calculus and the same peculiarities about which some scholars have scruples. Even algebraic calculation cannot avoid them if it wishes to preserve its advantages, one of the most important of which is the universality which enables it to include all cases, even that where certain given lines disappear. It would be ridiculous not to accept this and so to deprive ourselves of one of its greatest uses. All capable analysts in ordinary algebra have made use of this universality in order to make their calculations and constructions general. And when this advantage is applied to physics as well, particularly to the laws of motion, it reduces in part to what I call the *law of continuity*, which has long served me as a principle of discovery in physics and also as a convenient test to see if certain proposed rules are good. Some years ago I published an example of this in the *Nouvelles de la république des lettres*, in which I take equality as a particular case of inequality, rest as a special case of motion, parallelism as a case of convergence, etc., assuming not that the difference of magnitudes which become equal is already zero but that it is in the act of vanishing; and similarly in the case of motion, not that it is already zero in an absolute sense but that it is on the point of becoming zero. And anyone who is not satisfied with this can be shown in the manner of Archimedes that the error is less than any assignable quantity and cannot be given by any construction. It is in this way that a mathematician, and a very capable one besides, was answered when he criticized the quadrature of the parabola on the basis of scruples similar to those now opposed to our calculus. For he was asked whether he could, by means of any construction, designate any magnitude that would be smaller than the difference he claimed existed between the area of the parabola given by Archimedes and its true area, as can always be done when the quadrature is false.

Although it is not at all rigorously true that rest is a kind of motion or that equality is a kind of inequality, any more than it is true that a circle is a kind of regular polygon, it can be said, nevertheless, that rest, equality, and the circle terminate the motions, the inequalities, and the regular polygons which arrive at them by a continuous change and vanish in them. And although these terminations are excluded, that is, are not included in any rigorous sense in the variables which they limit, they nevertheless have the same properties as if they were included in the series, in accordance with the language of infinites and infinitesimals, which takes the circle, for example, as a regular polygon with an infinite number of sides. Otherwise the law of continuity would be violated, namely, that since we can move from polygons to a circle by a continuous change and without making a leap, it is also necessary not to make a leap in passing from the properties of polygons to those of a circle.

REFERENCES

1 The section enclosed in parentheses was to be omitted in the letter as sent.

2 If Leibniz had more clearly combined his conception of the infinitesimal as a quantity to be taken at will as less than any assignable quantity whatever with his own analysis of series and his functional conception of the law of continuity, he should have been led to the critical concept of limits upon which the calculus was at last theoretically grounded in the nineteenth century by Weierstrass and Cauchy.

3 I.e., a 'potential infinite'; see also the letters to John Bernoulli (No. 54) and p. 514, note 1.

4 See No. 37.

5 GM. has Suárez.

6 That is, assuming the angle at C to be at the center of a unit circle with radius CX.

57

ON WHAT IS INDEPENDENT OF SENSE AND OF MATTER

(Letter to Queen Sophia Charlotte of Prussia, 1702)

This and the following two selections (Nos. 58 and 59) show Leibniz's ability to popularize his ideas. The first two were certainly written at the instigation of his pupil, the Queen of Prussia, and the third, on ethics and law, belongs by style at least to the same group.

This letter of Leibniz's was probably in reply to one by John Toland, who had visited the courts of Hanover and Berlin and had there had opportunity to expound his own empiricism and materialism. In any case, Sophia Charlotte submitted a letter of Toland's to Leibniz for criticism, and a rather short and unsatisfactory correspondence between the two men followed. Leibniz's paper is at certain points clearly aimed at Toland and probably also at Locke, whose Essay he was engaged in criticizing, but who avoided being drawn into correspondence.

[G., VI, 499–508]

I found the letter truly ingenious and beautiful which was sent some time ago from Paris to Osnabrück, and which I recently read by your order at Hanover. Since it deals with two important questions on which I admit I do not entirely share the opinion of its author – whether there is something in our thoughts which does not come from sense and whether there is something in nature which is not material – I wish I were able to explain myself with the same charm as his, in order to obey Your Majesty's orders and satisfy Your Majesty's curiosity.

To use the analogy of an ancient writer, we use the external senses as a blind man uses his stick, and they help us to know their particular objects, which are colors, sounds, odors, tastes, and tactual qualities. But they do not help us to know what these sensible qualities are or in what they consist. For example, if red is the whirling of certain small globes which, it is claimed, make light; if heat is an eddy of very fine dust; if sound is made in the air as are circles in the water when a stone is thrown in, as some philosophers hold, we at least do not see this, and we cannot even understand how this whirling, these eddies and circles, if they are real, should bring about just the particular perceptions which we have of red, of heat, and of noise. So it can be said that *sensible qualities* are in fact *occult qualities* and that there must be others *more manifest* which could render them understandable. Far from understanding sensible things only, it is just these which we understand the least. And even though we are familiar with them, we do not understand them the better for that, just as a pilot does not understand the nature of the magnetic needle, which turns to the north, better than other men, although he has it constantly before his eyes in the compass, and as a result scarcely even has any more curiosity about it.

I do not deny that many discoveries have been made about the nature of these

For references see p. 553

1698, to all the objections contained in a memoir which a scholarly man published in the same journal in 1697, and which Mr. Bayle cites in the margin, letter S.[8] I think I have even demonstrated there that without an active force in the body there would be no variety in phenomena, which amounts to the same thing as there being nothing at all. It is true that my learned adversary replied to this (May, 1699), but he really only further explained his own opinion without adequately touching upon the reasons which I opposed to it. As a result, he did not remember to reply to this demonstration, especially since he considered it futile to try to arrive at an agreement or to clarify this matter further; he even regarded it as capable of injuring our mutual understanding. I admit that this is commonly the result of such debates, but there are exceptions, and this discussion between Mr. Bayle and myself seems to be of another kind. For my part, I try always to take proper measures to preserve moderation and to push forward the clarification of problems, so that the dispute will not only be harmless but may even become fruitful. I do not know if I have achieved this last aim, but although I cannot flatter myself to give entire satisfaction to a mind as penetrating as Mr. Bayle's in a matter as difficult as the present one, I shall always be content if he finds that I have made some progress in so important an investigation.

I have been unable to prevent myself from renewing the pleasure I once had in reading, with particular attention, a number of articles from his excellent and rich *Dictionary*, among others those concerned with philosophy, like the articles on the Paulicians, Origen, Pereira, Rorarius, Spinoza, and Zeno. I was surprised anew at the fecundity, force, and brilliance of his thoughts. No ancient academician, not even Carneades, could have brought out the difficulties better. Although Mr. Foucher was most able in such studies[9], he does not approach these, and I myself find nothing in the world more useful in solving these same old difficulties. It is this that pleases me so in the criticisms of able and moderate persons, for I feel that they give me new powers, like Antaeus hurled to earth in the fable. If I speak with some confidence, it is because my own views have become fixed only after my considering all sides and weighing them well, so that I can perhaps say, without vanity, *omnia praecepi atque animo mecum peregi*.[10] But criticisms put me back on the track and save me much trouble, for it is no small matter to have to retrace all the bypaths to anticipate and foresee everything that others may find to criticize, inasmuch as our points of view and inclinations are so different that there have been very penetrating persons who have accepted my hypothesis from the start and even urged it upon others, while other very able men have pointed out to me that they have in fact already held this view, and still others have even said that they understand the hypothesis of occasional causes in this same sense and cannot distinguish it from mine, which well satisfies me. But I am no less pleased when I see someone set out to examine it as it should be done.

To say something about the articles of Mr. Bayle which I have just mentioned and whose subject matter has many connections with this question, it seems that the reason for permitting evil rests in the eternal possibilities, in accordance with which this kind of universe which allows evil and has admitted it to actual existence is found to be more perfect, considering the whole, than all other possible kinds. But we go astray in trying to show in detail the value of evil in revealing the good, as the Stoics do – a value which St. Augustine has well recognized in general, and according to which we step back, so to speak, in order to leap forward better.[11] For can we enter

Leibniz, evil & ~~[illegible]~~ Reply to a criticism in P. Bayle's Dictionary. Loemker

into the infinite particulars of the universal harmony? If it were necessary, however, to choose reasonably between the two, I should always favor the Origenists and never the Manichees.[12] It does not seem necessary to me to deny action or force to creatures on the pretext that they would be creators if they produced their modifications. For it is God who conserves and continuously creates their forces, that is, who establishes a source of changing modifications in the creatures, or a state by which we can conclude that there will be a change of their modifications. Otherwise I find, as I have shown in the work cited above, that God would produce nothing and that there would be no substances beyond his own – a view which would lead us back into all the absurdities of Spinoza's God. It also seems to me that Spinoza's error comes entirely from his having pushed too far the consequences of the doctrine which denies force and action to creatures.

I acknowledge that time, extension, motion, and the continuum in general, as we understand them in mathematics, are only ideal things – that is, they express possibilities, just as do numbers. Even Hobbes has defined space as a phantasm of the existent. But to speak more accurately, *extension* is the order of possible coexistence, just as *time* is the order of possibilities that are inconsistent but nevertheless have a connection. Thus the former considers simultaneous things or those which exist together, the latter those which are incompatible but which we nevertheless conceive as all existing; it is this which makes them successive. But space and time taken together constitute the order of possibilities of the one entire universe, so that these orders – space and time, that is – relate not only to what actually is but also to anything that could be put in its place, just as numbers are indifferent to the things which can be enumerated. This inclusion of the possible with the existent makes a continuity which is uniform and indifferent to every division. It is true that perfectly uniform change, such as the mathematical idea of motion, is never found in nature any more then are actual figures which possess in full force the properties which we learn in geometry, because the actual world does not remain in this indifference of possibilities but arises from the actual divisions or pluralities whose results are the phenomena which are presented in practice and which differ from each other down to their smallest parts. Yet the actual phenomena of nature are arranged, and must be, in such a way that nothing ever happens which violates the law of continuity, which I introduced into philosophy and first mentioned in Mr. Bayle's *Nouvelles de la république des lettres*[13], or any of the other most exact rules of mathematics. On the contrary, things can be rendered intelligible only by these rules, for they alone are capable, along with the rules of harmony or perfection which the true metaphysics provides, of leading us to the reasons and intentions of the Author of things. The result of this very great multitude of infinite compositions is that we are finally lost and are forced to stop in our application of metaphysical principles, and of mathematical ones as well, to physics. Yet these applications are never in error, and when a miscalculation appears after an exact chain of inference, it is because we cannot adequately examine the facts and because there is an imperfection in our assumption. We are even more able to advance in this application to the degree that we are able to make use of our thoughts of infinity, as our newest methods have shown us. Although mathematical thinking is ideal, therefore, this does not diminish its utility, because actual things cannot escape its rules. In fact, we can say that the reality of phenomena, which distinguishes them from dreams, consists in this fact. However, mathematicians

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do not need all these metaphysical discussions, nor need they embarrass themselves about the real existence of points, indivisibles, infinitesimals, and infinites in any rigorous sense. In my reply to a notice in the *Mémoires de Trévoux* for May and June, 1701, which Mr. Bayle cites in the article on Zeno, I pointed this out, and in the same year I suggested that the mathematicians' demand for rigor in their demonstrations will be satisfied if we assume, instead of infinitely small sizes, sizes as small as are needed to show that the error is less than that which any opponent can assign, and consequently that no error can be assigned at all.[14] So even if the exact infinitesimals which end the decreasing series of assigned sizes were like imaginary roots, this would not at all injure the infinitesimal calculus, or the calculus of differences and sums, which I have proposed and which excellent mathematicians have cultivated so fruitfully. Error is impossible in this calculus except through a failure to understand it, or a false application, because it contains its own demonstration. It was later acknowledged in the *Journal de Trévoux*, in the same place, that the objections which had been raised before do not apply to my explanation. It was still maintained, it is true, that these objections do apply to the calculus of the Marquis de l'Hospital, but I believe that he has no more desire than I to burden geometry with metaphysical problems.

I almost laughed at the airs which the Chevalier de Méré gave himself in the letter to Pascal which Mr. Bayle describes in the same article. But I can see that the chevalier knew that this great genius had his imperfections, which sometimes made him too susceptible to extreme spiritualistic impressions and even made him disgusted at intervals with sound knowledge. The same thing has since happened to Steno and Swammerdam, but without any restoration, because they failed to combine the true metaphysics with physics and mathematics.[15] De Méré took advantage of this fact to talk down to Pascal. It seems to me he was talking a little too frivolously, as is common with people of the world who have much spirit but mediocre knowledge. They try to convince us that what they do not fully understand is of slight importance; they should be sent to school with Mr. Roberval. Yet it is true that the chevalier had an unusual talent, especially for mathematics, and I have learned from Mr. des Billettes, a friend of Pascal's who excels in mechanics, what the discovery was of which the chevalier boasts in his letter. Being a great gambler, he made the first ventures into the calculation of wagers, and it was this that led to the beautiful studies of the game of hazard [*de alea*] by Fermat, Pascal, and Huygens which Roberval could not or would not understand.[16] The Pensionaire de Witt has pushed this study still further and applied it to other more important uses related to life insurance. Mr. Huygens told me that Mr. Hudde also had excellent ideas on the problem and that it is unfortunate that he suppressed these along with so many others. Games themselves deserve study, and if a penetrating mathematician were to investigate them, he would find many important truths, for men never show more spirit than when they are jesting.

Let me add in passing that not only Cavalieri and Torricelli, of whom Gassendi spoke in the passage cited by Mr. Bayle, but also I myself and many others have found figures of infinite length whose areas are finite. There is nothing more extraordinary about this than about infinite series, where we find that $\frac{1}{2}+\frac{1}{4}+\frac{1}{8}+\frac{1}{16}+\frac{1}{32}+\text{etc.}=1$. It may be, however, that the chevalier also experienced some fine enthusiasm which transported him "into that invisible world and into that infinite extension" of which he speaks and which I believe to be that of the ideas or forms of which certain Scholastics also speak in raising the question of whether "there is a vacuum of forms". For he says that "one can here discover the principles of things, the most hidden truths, the harmonies, the justice, the propositions, the true originals and perfect ideas of everything that one seeks".

This intelligible world of which the ancients speak so much is in God and in some way also in us. But what the letter says against division to infinity makes it clear that its author was still too much of a stranger in this superior world and that the charms of the visible world of which he wrote did not leave him the time which he needed to acquire the rights of citizenship in the other. Mr. Bayle is right in saying, with the ancients, that God uses geometry and that mathematics makes up a part of the intelligible world and is therefore the more fit to be an entrance into it. But I myself believe that its interior is something more. I have suggested elsewhere that there is a calculus more important than those of arithmetic and geometry which depends on the analysis of ideas. This would be a universal characteristic, and its formation seems to me one of the most important things that can be undertaken.

REFERENCES

1 See No. 47.
2 See No. 52.
3 Violent action, in the Scholastic sense, is action proceeding not from an internal cause but from an extraneous source.
4 See No. 36, I; No. 38.
5 *Phaedo* 60 *b,f*.
6 See Nos. 30 and 35 (*Discourse*, Sec. 9); also p. 271, note 7.
7 François Lami had attacked various aspects of the theory of pre-established harmony in the *Connoissance de soy-même*, Paris 1699; Leibniz finally published a partial reply in the *Supplément du Journal des savants* in 1709 (G., IV, 572–95).
8 *De ipsa natura* (No. 53), the reply to John Chr. Sturm.
9 For Simon Foucher see No. 11, and the introduction to No. 47. The allusion is probably primarily to his *Histoire des académiciens* (1690). He had died in 1697.
10 "I have anticipated everything and gone through it in my mind."
11 *Reculer pour mieux sauter*. This is a favorite figure of Leibniz in interpreting the role of evil in history.
12 In his article on Manicheism, Bayle had formulated a dualistic theory of good and evil; much of the discussion in the *Theodicy* later is directed at Bayle's further development of this position. Jean le Clerc, French refugee and editor of the *Bibliothèque choisie*, had replied to the article on Manicheism in a chapter of his *Parrhasiana* (1699), putting his answer in the mouth of an Origenist.
13 See No. 37.
14 See No. 56, II, and GM., V, 350ff.
15 On Steno see No. 23, introduction and p. 220, note 1. John Swammerdam became an enthusiast and follower of Mme de Bourignon.
16 See No. 49. Most of De Méré's letter to Pascal is reprinted in Bayle's article on Zeno.

Leibniz: A Manuscript on the Fundamental Principles of the Calculus[1]

When my infinitesimal calculus, which includes the calculus of differences and sums, had appeared and spread, certain over-precise veterans began to make trouble; just as once long ago the Sceptics opposed the Dogmatics, as is seen from the work of Empicurus against the mathematicians (i.e., the dogmatics), and such as Francisco Sanchez, the author of the book *Quod nihil scitur*, brought against Clavius; and his opponents to Cavalieri, and Thomas Hobbes to all geometers, and just lately such objections as are made against the quadrature of the parabola by Archimedes by that renowned man, Dethlevus Cluver. When then our method of infinitesimals, which had become known by the name of the calculus of differences, began to be spread abroad by several examples of its use, both of my own and also of the famous brothers Bernoulli, and more especially by the elegant writings of that illustrious Frenchman, the Marquis d'Hospital, just lately a certain erudite mathematician, writing under an assumed name in the *Journal de Trevoux*, appeared to find fault with this method. But to mention one of them by name, even before this there arose against me in Holland Bernard Nieuwentijt, one indeed really well equipped both in learning and ability, but one who wished rather to become known by revising our methods to some extent than by advancing them. Since I introduced not only the first differences, but also the second, third and other higher differences, inassignable or incomparable with these first differences, he wished to appear satisfied with the first only; not considering that the same difficulties existed in the first as in the others that followed, nor that wherever they might be overcome in the first, they also ceased to appear in the rest. Not to mention how a very learned young man, Hermann of Basel, showed that the second and higher differences were avoided by the former in name only, and not in reality; moreover, in demonstrating theorems by the legitimate use of the first differences, by adhering to which he might have accomplished some useful work on his own account, he fails to do so, being driven to fall back on assumptions that are admitted by no one; such as that something different is obtained by multiplying 2 by m and by multiplying m by 2; that the latter was impossible in any case in which the former was possible; also that the square or cube of a quantity is not a quantity or zero.

In it, however, there is something that is worthy of all praise, in that he desires that the diferential calculus should be strengthened with demonstrations, so that it may satisfy the rigorists; and this work he would have procured from me already, and more willingly, if, from the fault-finding everywhere interspersed, the wish had not appeared foreign to the manner of those who desire the truth rather than fame and a name.

It has been proposed to me several times to confirm the essentials of our calculus by demonstrations, and here I have indicated below its fundamental principles, with the intent that any one who has the leisure may complete the

[1]This translation is by J. M. Child, and appears in *The Early Mathematical Manuscripts of Leibniz*. It probably dates from around 1701.

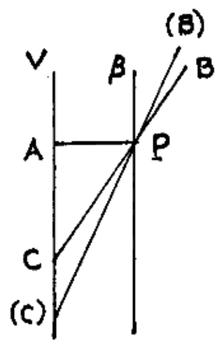
work. Yet I have not seen up to the present any one who would do it. For what the learned Hermann has begun in his writings, published in my defence against Nieuwentijt, is not yet complete.

For I have, beside the mathematical infinitesimal calculus, a method also for use in Physics, of which an example was given in the *Nouvelles de la République des Lettres*; and both of these I include under the Law of Continuity; and adhering to this, I have shown that the rules of the renowned philosophers Descartes and Malebranche were sufficient in themselves to attack all problems on Motion.

I take for granted the following postulate:

In any supposed transition, ending in any terminus, it is permissible to institute a general reasoning, in which the final terminus may also be included.

For example, if A and B are any two quantities, of which the former is the greater and the latter is the less, and while B remains the same, it is supposed that A is continually diminished, until A becomes equal to B; then it will be permissable to include under a general reasoning the prior cases in which A was greater than B, and also the ultimate case in which the difference vanishes and A is equal to B. Similarly, if two bodies are in motion at the same time, and it is assumed that while the motion of B remains the same, the velocity of A is continually diminished until it vanishes altogether, or the speed of A becomes zero; it will be permissible to include this case with the case of the motion of B under one general reasoning. We do the same thing in geometry when two straight lines are taken, produced in any manner, one VA being given in position or remaining in the same site, the other BP passing through a given point P, and varying in position while the point P remains fixed; at first indeed converging toward the line VA and meeting it in the point C; then, as the angle of inclination VCP is continually diminished, meeting VA in some more remote point (C), until at length from BP, through the position $(B)P$, it comes to βP, in which the straight line no longer converges toward VA, but is parallel to it, and C is an impossible or imaginary point. With this supposition it is possible to include under some one general reasoning not only all the intermediate cases such as $(B)P$, but also the ultimate case βp.



Hence also it comes to pass that we include as one case ellipses and the parabola, just as if A is considered to be one focus of an ellipse (of which V is the given vertex), and this focus remains fixed, while the other focus is variable as we pass from ellipse to ellipse, until at length (in the case when the line BP, by its intersection with the line VA, gives the variable focus) the focus C becomes evanescent[2] or impossible, in which case the ellipse passes into a parabola. Hence it is permissible with our postulate that a parabola should be considered with ellipses under a common reasoning. Just as it is common

[2]The term is here used with the idea of "vanishing into the far distance."

practice to make use of this method in geometrical constructions, when they include under one general construction many different cases, noting that in a certain case the converging straight line passes into a parallel straight line, the angle between it and another straight line vanishing.

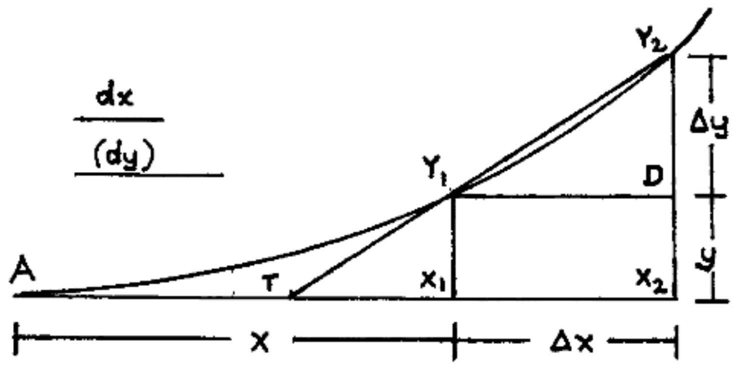
Moreover, from this postulate arise certain expressions which are generally used for the sake of convenience, but seem to contain an absurdity, although it is one that causes no hindrance, when its proper meaning is substituted. For instance, we speak of an imaginary point of intersection as if it were a real point, in the same manner as in algebra imaginary roots are considered as accepted numbers. Hence, preserving the analogy, we say that, when the straight line BP ultimately becomes parallel to the straight line VA, even then it converges toward it or makes an angle with it, only that the angle is then infinitely small; similarly, when a body ultimately comes to rest, it is still said to have a velocity, but one that is infinitely small; and, when one straight line is equal to another, it is said to be unequal to it, but that the difference is infinitely small; and that a parabola is the ultimate form of an ellipse, in which the second focus is at an infinite distance from the given focus nearest to the given vertex, or in which the ratio of PA to AC, or the angle BCA, is infinitely small.

Of course it is really true that things which are absolutely equal have a difference which is absolutely nothing; and that straight lines which are parallel never meet since the distance between them is everywhere the same exactly; that a parabola is not an ellipse at all, and so on. Yet, a state of transition may be imagined, or one of evanescence, in which indeed there has not yet arisen exact equality or rest or parallelism, but in which it is passing into such a state, that the difference is less than any assignable quantity; also that in this state there will still remain some difference, some velocity, some angle, but in each case one that is infinitely small; and the distance of the point of intersection, or the variable focus, from the fixed focus will be infinitely great, and the parabola may be included under the heading of an ellipse (and also in the same manner and by the same reasoning under the heading of a hyperbola), seeing that those things that are found to be true about a parabola of this kind are in no way different, for any construction, from those which can be stated by treating the parabola rigorously.

Truly it is very likely that Archimedes, and one who has seemed so to have surpassed him, Conon, found out their wonderfully elegant theorems by the help of such ideas; these theorems they completed with *reductio ad absurdum* proofs, by which they at the same time provided rigorous demonstrations and also concealed their methods. Descartes very appropriately remarked in one of his writings that Archimedes used as it were a kind of metaphysical reasoning (Caramuel would call it metageometry), the method being scarcely used by any of the ancients (except those who dealt with quadratrices); in our time Cavalieri has revived the method of Archimedes, and afforded an opportunity for others to advance still further. Indeed Descartes himself did so, since at one time he imagined a circle to be a regular polygon with a infinite number of sides, and used the same idea in treating the cycloid; and Huygens too, in his work on the pendulum, since he was accustomed to confirm his theorems by rigorous demonstrations; yet at other times, in order to avoid too great prolixity, he made use of infinitesimals; as also quite later did the renowned La Hire.

For the present, whether such a state of instantaneous transition from inequality to equality, from motion to rest, from convergence to parallelism, or anything of the sort, can be sustained in a rigorous or metaphysical sense, or whether infinite extensions successively greater and greater, or infinitely small ones successively less and less, are legitimate considerations, is a matter that I own to be possibly open to question; but for him who would discuss these matters, it is not necessary to fall back upon metaphysical controversies, such as the composition of the continuum, or to make geometrical matters depend thereon. Of course, there is no doubt that a line may be considered to be unlimited in any manner, and that, if it is unlimited on one side only, there can be added to it something that is limited on both sides. But whether a straight line of this kind is to be considered as one whole that can be referred to computation, or whether it can be allocated among quantities which may be used in reckoning, is quite another question that need not be discussed at this point.

It will be sufficient if, when we speak of infinitely great (or more strictly unlimited), or of infinitely small quantities (i.e., the very least of those within our knowledge), it is understood that we mean quantities that are indefinitely great or indefinitely small, i.e., as great as you please, or as small as you please, so that the error that anyone may assign may be less than a certain assigned quantity. Also, since in general it will appear that, when any small error is assigned, it can be shown that it should be less, it follows that the error is absolutely nothing; an almost exactly similar kind of argument is used in different places by Euclid, Theodosius and others; and this seemed to them to be a wonderful thing, although it could not be denied that it was perfectly true that, from the very thing that was assumed as an error, it could be inferred that the error was non-existent. Thus, by infinitely great and infinitely small, we understand something indefinitely great, or something indefinitely small, so that each conducts itself as a sort of class, and not merely as the last thing of a class. If anyone wishes to understand these as the ultimate things, or as truly infinite, it can be done, and that too without falling back upon a controversy about the reality of extensions, or of infinite continuums in general, or of the infinitely small, ay, even though he think that such things are utterly impossible; it will be sufficient simply to make use of them as a tool that has advantages for the purpose of the calculation, just as the algebraists retain imaginary roots with great profit. For they contain a handy means of reckoning, as can manifestly be verified in every case in a rigorous manner by the method already stated. But it seems right to show this a little more clearly, in order that it may be confirmed that the algorithm, as it is called, of our differential calculus, set forth by me in the year 1684, is quite reasonable. First of all, the sense in which the phrase "dy is the element of y," is to be taken will best be understood by considering a line AY referred to a straight line AX as axis. Let the curve AY be a parabola, and let the tangent at the vertex A be taken as the axis. If AX is called x, and XY, y, and the latus-rectum is a, the equation to the parabola will be $xx{=}ay$, and this holds good at every point. Now, let $A\ X_1{=}x$, and $X_1Y_1{=}y$, and from the point Y_1



let fall a perpendicular Y_1D to some greater ordinate X_2Y_2 that follows, and let X_1X_2 the difference between A, X_1 and A, X_2 be called ΔX;[3] and similarly, let DY_2 the difference between X_1Y_1 and X_2Y_2 be called ΔY.

Then, since

$$y = x^2 : a,$$

by the same law, we have

$$y + \Delta y = (x^2 + 2x\,\Delta x + (\Delta x)^2) : a$$

and taking away the y from the one side and the $x^2 : a$ from the other, we have left

$$\Delta y : \Delta x = (2x + \Delta x) : a;$$

and this is a general rule, expressing the ratio of the difference of the ordinates to the difference of the abscissae, or, if the chord Y_1Y_2 is produced until it meets the axis in T, then the ratio of the ordinate X_1Y_1 to TX_1, the part of the axis intercepted between the point of intersection and the ordinate, will be as $2x+\Delta x$ to a. Now, since by our postulate it is permissible to include under the one general reasoning the case also in which the ordinate X_2Y_2 is moved up nearer and nearer to the fixed ordinate X_1Y_1 until it ultimately coincides with it, it is evident that in this case Δx becomes equal to zero and should be neglected, and thus it is clear that, since in this case TY_1 is the tangent, X_1Y_1 is to TX_1 as $2x$ is to a.

Hence, it may be seen that there is no need in the whole of our differential calculus to say that those things are equal which have a difference which is infinitely small, but that those things can be taken as equal that have not any difference at all, provided that the calculation is supposed to be general, including both the cases in which there is a difference and in which the difference

[3] [Here and below Leibniz's notation has been altered from dx, dy to Δx, Δy and from (dx), (dy) to dx, dy.]

is zero; and provided that the difference is not assumed to be zero until the calculation is purged as far as is possible by legitimate omissions, and reduced to ratios of non-evanescent quantities, and we finally come to the point where we apply our result to the ultimate case.

Similarly, if

$$x^3 = a^2 y,$$

then we have

$$x^3 + 3x^2\Delta x + 3x(\Delta x)^2 + (\Delta x)^3 = a^2 y + a^2 \Delta y,$$

or cancelling from each side,

$$3x^2\Delta x + 3x(\Delta x)^2 + (\Delta x)^3 = a^2 \Delta y,$$

or

$$(3x^2 + 3x\Delta x + (\Delta x)^2) : a^2 = \Delta y : \Delta x = X_1 Y_1 : T X_1.$$

Hence, when the difference vanishes, we have

$$3x^2 : a^2 = X_1 Y_1 : T X_1.$$

But if it is desired to retain Δy and Δx in the calculation, so that they may represent non-evanescent quantities even in the ultimate case, let any assignable straight line be taken as dx, and let the straight line which bears to dx the ratio of y or $X_1 Y_1$ to $X_1 T$ be called dy; in this way dy and dx will always be assignables bearing to one another the ratio of DY_2 to DY_1, which latter vanish in the ultimate case ...

Note: *As Δx is made smaller, the ratio $\Delta y/\Delta x$ is usually changing. Consequently, keeping the "assignable" line dx fixed at an arbitrary length, the other assignable line, dy, must vary so as to keep dy/dx equal to $\Delta y/\Delta x$. Since, however, in most of our usual cases the secant will approach coincidence with the tangent, the slope of the secant*

$$\Delta y : \Delta x$$

will approach the slope of the tangent. Consequently,

$$dy : dx$$

will approach a limiting ratio, namely the slope of the tangent, and since we are holding dx constant, dy will approach a limiting length. For conceptual clarity, one might wish to distinguish the varying lengths of dy from the limiting length by using Leibniz's own '(dy)' for the former and reserving 'dy' for the latter. With this in mind, we have

$$\Delta y : \Delta x = (dy) : (dx)$$

always and both

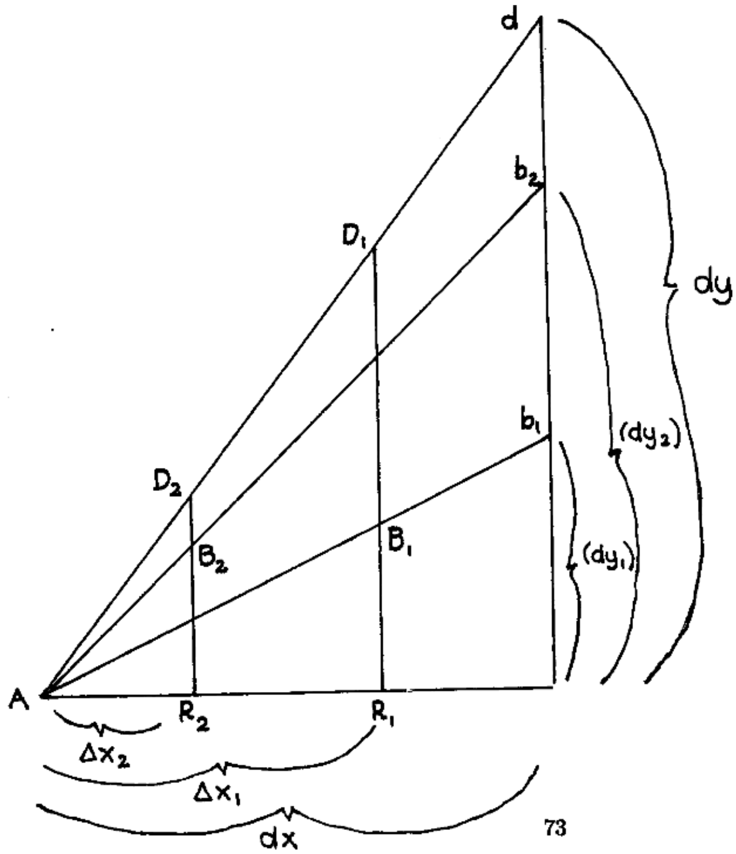
$$\Delta y\!:\!\Delta x \to dy\!:\!dx$$

and

$$(dy)\!:\!dx \to dy\!:\!dx,$$

(here the arrow means that the quantities on the left approach the quantities on the right and are ultimately equal to them) or the limiting ratio of $\Delta y : \Delta x$ is the limiting ratio of $(dy)\!:\!dx$ and this limiting ratio equals $dy\!:\!dx$.

This figure shows the similarities between Newton's microscope technique of Lemmas VI, VII, and VIII, and Leibniz's assignable differentials.



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Leibniz Paper
Possible Topics

1. What is the calculus for Leibniz? How does it go beyond the mathematics of the ancients and of Descartes? Wherein lies its power? Are there problems in any of Leibniz' assumptions or methods?

2. Why are transcendent curves so important? Discuss this with reference to the logarithmic curve, or cycloid, or sine curve.

3. What is dx? What is a characteristic triangle? Is it a mathematical fiction or a real triangle? Discuss the problem of using infinitessimals. How can there be degrees of infinitely small differences? How can we calculate with something infinitely small? Once we have an equation for a dv, what do we now know that we didn't know before? Why is this knowledge important?

4. How is the calculus related to final causes? Discuss this with reference to Snell's Law. What exactly is Leibniz claiming about how final causes function in nature and in the study of nature?

5. Leibniz develops two kinds of calculus: differential and integral. The first is the calculus of differences, the second that of rectification or area (the quadratrix). We know from the two Fundamental Theorems that the difference and the integral are inverses of each other. What is the meaning, and what are the implications, of this inverse relation? Discuss this with reference to an example.

6. The logarithmic curve is the quadratrix of the hyperbola. Why is this important? What does it reveal about the two curves, and about the calculus?

7. Does the existence, and apparent success, of the calculus mean that human beings can in some way *know* the infinite?

some number n of elements and no more, *then it cannot be equivalent to any one of its proper subsets*, since any proper subset could contain at most $n - 1$ elements. But, *if a set contains infinitely many objects*, then, paradoxically enough, *it may be equivalent to a proper subset of itself.* For example, the coördination

$$\begin{array}{cccccccc} 1 & 2 & 3 & 4 & 5 & \cdots & n & \cdots \\ \updownarrow & \updownarrow & \updownarrow & \updownarrow & \updownarrow & & \updownarrow & \\ 2 & 4 & 6 & 8 & 10 & \cdots & 2n & \cdots \end{array}$$

establishes a biunique correspondence between the set of *positive integers* and the proper subset of *even integers*, which are thereby shown to be equivalent. This contradiction to the familiar truth, "the whole is greater than any of its parts," shows what surprises are to be expected in the domain of the infinite.

2. The Denumerability of the Rational Numbers and the Non-Denumerability of the Continuum

One of Cantor's first discoveries in his analysis of the infinite was that the set of *rational numbers* (which contains the infinite set of integers as a subset and is therefore itself infinite) is equivalent to the *set of integers*. At first sight it seems very strange that the dense set of rational numbers should be on the same footing as its sparsely sown subset of integers. True, one cannot arrange the positive rational numbers *in order of size* (as one can the integers) by saying that a is the first rational number, b the next larger, and so forth, because there are infinitely many rational numbers between any two given ones, and hence there is no "next larger." But, as Cantor observed, by disregarding the relation of magnitude between successive elements, it is possible to arrange all the rational numbers in a single row, $r_1, r_2, r_3, r_4, \cdots$, like that of the integers. In this sequence there will be a first rational number, a second, a third, and so forth, and every rational number will appear exactly once. Such an arrangement of a set of objects in a sequence like that of the integers is called a *denumeration* of the set. By exhibiting such a denumeration Cantor showed the set of rational numbers to be equivalent with the set of integers, since the correspondence

$$\begin{array}{ccccccc} 1 & 2 & 3 & 4 & \cdots & n & \cdots \\ \updownarrow & \updownarrow & \updownarrow & \updownarrow & & \updownarrow & \\ r_1 & r_2 & r_3 & r_4 & \cdots & r_n & \cdots \end{array}$$

is biunique. One way of denumerating the rational numbers will now be described.

Every rational number can be written in the form a/b, where a and b are integers, and all these numbers can be put in an array, with a/b in the ath column and bth row. For example, 3/4 is found in the third column and fourth row of the table below. All the positive rational numbers may now be arranged according to the following scheme: in the array just defined we draw a continuous, broken line that goes through all the numbers in the array. Starting at 1, we go horizontally to the next place on the right, obtaining 2 as the second member of the sequence, then diagonally down to the left until the first column is reached at the position occupied by 1/2, then vertically down one place to 1/3, diagonally up until the first row is reached again at 3, across to 4, diagonally down to 1/4, and so on, as shown in the figure. Travelling along this broken line we arrive at a sequence 1, 2, 1/2, 1/3, 2/2, 3, 4, 3/2, 2/3, 1/4, 1/5, 2/4, 3/3, 4/2, 5, $\cdots$ containing the rational numbers in the order in which they occur along the broken line. In this sequence we now cancel all those numbers a/b for which a and b have a common factor, so that each rational number r will appear exactly once and in its simplest form. Thus we obtain a sequence

1	2	3	4	5	6	7	$\cdots$
$\frac{1}{2}$	$\frac{2}{2}$	$\frac{3}{2}$	$\frac{4}{2}$	$\frac{5}{2}$	$\frac{6}{2}$	$\frac{7}{2}$	$\cdots$
$\frac{1}{3}$	$\frac{2}{3}$	$\frac{3}{3}$	$\frac{4}{3}$	$\frac{5}{3}$	$\frac{6}{3}$	$\frac{7}{3}$	$\cdots$
$\frac{1}{4}$	$\frac{2}{4}$	$\frac{3}{4}$	$\frac{4}{4}$	$\frac{5}{4}$	$\frac{6}{4}$	$\frac{7}{4}$	$\cdots$
$\frac{1}{5}$	$\frac{2}{5}$	$\frac{3}{5}$	$\frac{4}{5}$	$\frac{5}{5}$	$\frac{6}{5}$	$\frac{7}{5}$	$\cdots$
$\frac{1}{6}$	$\frac{2}{6}$	$\frac{3}{6}$	$\frac{4}{6}$	$\frac{5}{6}$	$\frac{6}{6}$	$\frac{7}{6}$	$\cdots$
$\cdots$	$\cdots$	$\cdots$	$\cdots$	$\cdots$	$\cdots$	$\cdots$	$\cdots$

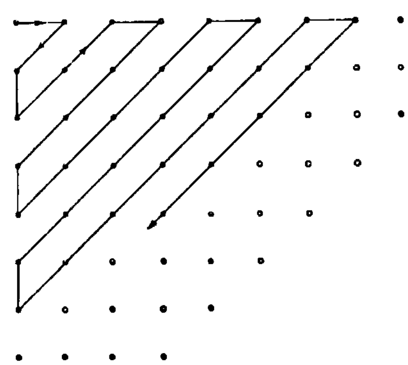


Fig. 19. Denumeration of the rational numbers.

1, 2, 1/2, 1/3, 3, 4, 3/2, 2/3, 1/4, 1/5, 5, $\cdots$ which contains each positive rational number once and only once. This shows that the set of all positive rational numbers is denumerable. In view of the fact that the rational numbers correspond in a biunique manner with the rational points on a line, we have proved at the same time that the set of positive rational points on a line is denumerable.

Exercises: 1) Show that the set of all positive and negative integers is denumerable. Show that the set of all positive and negative rational numbers is denumerable.

2) Show that the set $S + T$ (see p. 110) is denumerable if S and T are denumerable sets. Show the same for the sum of three, four, or any number, n, of sets, and finally for a set composed of denumerably many denumerable sets.

Since the rational numbers have been shown to be denumerable, one might suspect that *any* infinite set is denumerable, and that this is the ultimate result of the analysis of the infinite. This is far from being the case. Cantor made the very significant discovery that *the set of all real numbers*, rational and irrational, *is not denumerable*. In other words, the totality of real numbers presents a radically different and, so to speak, higher type of infinity than that of the integers or of the rational numbers alone. Cantor's ingenious indirect proof of this fact has become a model for many mathematical demonstrations. The outline of the proof is as follows. We start with the tentative assumption that all the real numbers have actually been denumerated in a sequence, and then we exhibit a number which does not occur in the assumed denumeration. This provides a contradiction, since the assumption was that *all* the real numbers were included in the denumeration, and this assumption must be false if even one number has been left out. Thus the assumption that a denumeration of the real numbers is possible is shown to be untenable, and hence the opposite, i.e. Cantor's statement that the set of real numbers is not denumerable, is shown to be true.

To carry out this program, let us suppose that we have denumerated all the real numbers by arranging them in a table of infinite decimals,

$$\begin{array}{ll} \text{1st number} & N_1.\,a_1a_2a_3a_4a_5\cdots \\ \text{2nd number} & N_2.\,b_1b_2b_3b_4b_5\cdots \\ \text{3rd number} & N_3.\,c_1c_2c_3c_4c_5\cdots \\ \cdots\cdots\cdots & \cdots\cdots\cdots\cdots\cdots \end{array}$$

where the N's denote the integral parts and the small letters denote the digits after the decimal point. We assume that this sequence of decimal fractions contains *all* the real numbers. The essential point in the proof is now to construct by a "diagonal process" a new number which we can show to be not included in this sequence. To do this we first choose a digit a which differs from a_1 and is neither 0 nor 9 (to avoid possible ambiguities which may arise from equalities like $0.999\cdots = 1.000\cdots$), then a digit b different from b_2 and again unequal to 0 or 9, similarly c different from c_3, and so on. (For example, we might simply choose $a = 1$ unless $a_1 = 1$, in which case we choose $a = 2$, and similarly down the table for all the digits $b, c, d, e, \cdots$.) Now consider the infinite decimal

$$z = 0.abcde \cdots .$$

This new number z is certainly different from any one of the numbers in the table above; it cannot be equal to the first because it differs from it in the first digit after the decimal point; it cannot be equal to the second since it differs from it in the second digit; and, in general, it cannot be identical with the nth number in the table since it differs from it in the nth digit. This shows that our table of consecutively arranged decimals does *not* contain all the real numbers. Hence this set is not denumerable.

The reader may perhaps imagine that the reason for the non-denumerability of the number continuum lies in the fact that the straight line is infinite in extent, and that a finite segment of the line would contain only a denumerable infinity of points. This is not the case, for

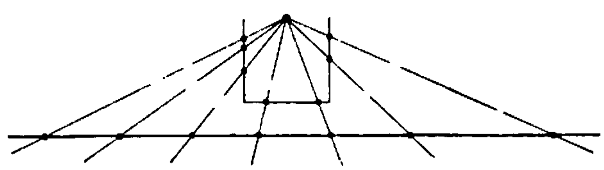


Fig. 20

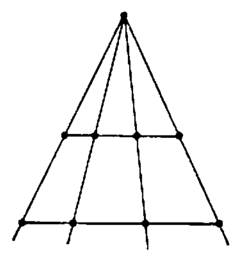


Fig 21

Fig. 20. Biunique correspondence between the points of a bent segment and a whole straight line.
Fig. 21. Biunique correspondence between the points of two segments of different length.

it is easy to show that the entire number continuum is equivalent to any finite segment, say the segment from 0 to 1 with the endpoints excluded. The desired biunique correspondence may be obtained by bending the segment at $\frac{1}{3}$ and $\frac{2}{3}$ and projecting from a point, as shown in Figure 20. It follows that even a finite segment of the number axis contains a non-denumerable infinity of points.

Exercise: Show that any interval $[A, B]$ of the number axis is equivalent to any other interval $[C, D]$.

It is worthwhile to indicate another and perhaps more intuitive proof of the non-denumerability of the number continuum. In view of what we have just proved it will be sufficient to confine our attention to the set of points between 0 and 1. Again the proof is indirect. Let us suppose that the set of all points on the line between 0 and 1 can be arranged in a sequence

$$(1) \qquad a_1, a_2, a_3, \cdots.$$

Let us enclose the point with coördinate a_1 in an interval of length $1/10$, the point with coördinate a_2 in an interval of length $1/10^2$, and so on. If all points between 0 and 1 were included in the sequence (1), the unit interval would be entirely covered by an infinite sequence of possibly overlapping subintervals of lengths $1/10, 1/10^2, \cdots$. (The fact that some of these extend beyond the unit interval does not influence our proof.) The sum of these lengths is given by the geometric series

$$1/10 + 1/10^2 + 1/10^3 + \cdots = \frac{1}{10}\left[\frac{1}{1-\frac{1}{10}}\right] = \frac{1}{9}.$$

Thus the assumption that the sequence (1) contains all real numbers from 0 to 1 leads to the possibility of covering the whole of an interval of length 1 by a set of intervals of total length 1/9, which is intuitively absurd. We might accept this contradiction as a proof, although from a logical point of view it would require fuller analysis.

The reasoning of the preceding paragraph serves to establish a theorem of great importance in the modern theory of "measure". Replacing the intervals above by smaller intervals of length $\epsilon/10^n$, where ϵ is an arbitrary small positive number, we see that any denumerable set of points on the line can be included in a set of intervals of total length $\epsilon/9$. Since ϵ was arbitrary, the latter number can be made as small as we please. In the terminology of measure theory we say that a denumerable set of points has the *measure zero.*

Exercise: Prove that the same result holds for a denumerable set of points in the plane, replacing lengths of intervals by areas of squares.

3. Cantor's "Cardinal Numbers"

In summary of the results thus far: The number of elements in a *finite* set A cannot equal the number of elements in a finite set B if A contains *more* elements than B. If we replace the concept of "sets with the same (finite) number of elements" by the more general concept of *equivalent sets,* then with infinite sets the previous statement does not hold; the set of all integers contains more elements than the set of even integers, and the set of rational numbers more than the set of integers, but we have seen that these sets are equivalent. One might suspect that *all* infinite sets are equivalent and that distinctions other than that between finite numbers and infinity could not be made, but

Reading: Newton, "Principia"

Book I, Author's Preface to the Reader, Definitions, Scholium on Absolute Space and Time, Axioms or Laws of Motion, Corollaries to the Laws, and Scholium.

Translations:

- Cohen and Whitman, pp. 381-430;
- Densmore and Donahue, pp. 1-16 (you will also need a translation of the proofs of the Corollaries and the Scholium which follows);
- Motte-Cajori, Volume I, pp. xvii-xviii and pp. 1-28.

Demonstration: Rotating bucket, with Newton's argument in the Scholium on Absolute Space and Time.

Notes to the Reading

Galileo's science of motion began by taking speed as a Euclidean magnitude, and finding its relation to other magnitudes, better known and more accessible to measurement. Newton's enterprise begins further back, with a definition of body as one magnitude among others. Body so understood is called mass and measured, in the system of units to which we will adhere, in grams. Weight, to which mass is proportional, is a separate quantity, defined by the expositions under Definitions 5, 7, and 8, and is measured in dynes.[27] Body as such is known through the product of bulk (volume) and density. (To say that density is "weight over volume" is thus mathematically correct, but unintelligible as a definition. How must Newton understand density?) Body is evident in the world first of all as a power of resistance (Definition 3), but the whole Principia constitutes a proof that body as such has a second power, of which ordinary experience gives us no clue. This second power will be named universal gravitation, but to begin with, Newton means by gravity only the force by which bodies tend to the center of the earth (Definition 5).

Law I rejects Galileo's notion of inertia (as being only horizontal and hence following the curve of the earth) in favor of Descartes'. Law II rejects the conception of motive force common to Descartes and Leibniz, that it resides in any moving body, instead locating force only in the transfer of motion (see Definition IV). Law III rejects the possibility of an unmoved mover. Laws I and II together reject the principle that primary motions must be circular, in favor of one that decrees that they must be in straight lines. Corollary 1 is the whole science of vectors. Corollary 2 shows that the three laws and first corollary contain "the whole doctrine of mechanics variously demonstrated by different authors," and the scholium following the corollaries traces all the discoveries of the new science to those sources.

Law II must be understood as qualified by the phrase "other things being equal," since the same change of motion (Δmv) could be produced by a large force in a short time or by

[27] Thus the dyne is a unit of force. It is equal to the amount of motive force possessed by a body with a mass of one gram which is accelerating at a rate of 1 cm/sec/sec (1 cm/sec^2). A 10 gram body (near the surface of the earth) weighs 9800 dynes; for if it were to fall freely, it would accelerate at the rate of 980 cm/sec^2. If we measure mass in terms of kilograms (1 kg = 1000 grams) and distance in terms of meters (1 m = 100 cm), then the unit of force is the Newton. One Newton = 1 kg $\times$ m/sec^2.

a small one in a long time. Hence to have an expression for motive force we must write

$$f \propto \Delta(mv)/\Delta t.$$

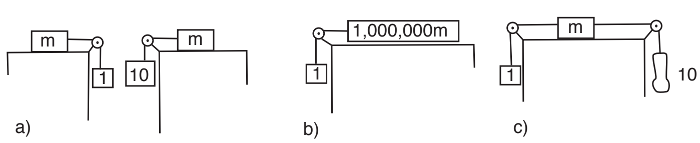
We can refine the expression in three steps. Since the mass does not change when the motion does, we may take m as a constant coefficient multiplied by the rest of the formula. Since the expression we have would apply only to constant or average forces, we must take the limit of the difference quotient to gain generality and precision. Finally, since the unit dyne is defined as that measure of motive force which results from measuring all masses, lengths, and times in grams, centimeters, and seconds (1dyne = 1gram cm/sec^2), we may change our proportionality statement to an equation with no new constants,

$$f = m(\frac{dv}{dt}) = ma, \text{ where } a \text{ is acceleration.}$$

If the acceleration due to gravity is called g, then weight is $f = mg$, the motive force impressed on a falling body.

The laws and first corollary form the framework for analyzing any event. The questions to be asked are always, Where are the forces? and, What are the equal and opposite forces opposed to them? Consider the stone whirled in a sling discussed under Definition 5. The stone pulls the string by what we ordinarily call centrifugal force, a misnomer unless we are very careful about how we are using Newton's language. It is the inertia of the stone (its "force of inactivity") that applies a motive force at the end of the sling. The equal and opposite reaction is a centripetal force resulting from the cohesion of the parts of the sling. And we may carry the analysis further, to the interaction between the sling and the hand. We say that each is pulled by the other, but suppose there were not friction between them. Are not the pulls then really pushes, with bits of skin and of the rough surface of the sling getting behind each other? (A pull which cannot be reduced to a push is a very strange notion.) And clearly, the analysis might continue into the body to which the hand is connected, as the end of Corollary 2 suggests. Let us begin with some simpler situations.

Let m represent a massive body, sliding on a frictionless surface, connected by a string of negligible mass through a frictionless pulley to a freely hanging mass of either 1 or 10 grams. What will happen in (a)? How will the two cases differ? In (b), Newton's Laws and Definitions guarantee that any hanging mass, however small, will move any mass on the surface, however large. Why? Describe the motion mathematically. Why does ordinary



experience seem to contradict your conclusion? What will happen in (c)? Finally, consider what would happen in (a) if the strings were cut above the two hanging masses.

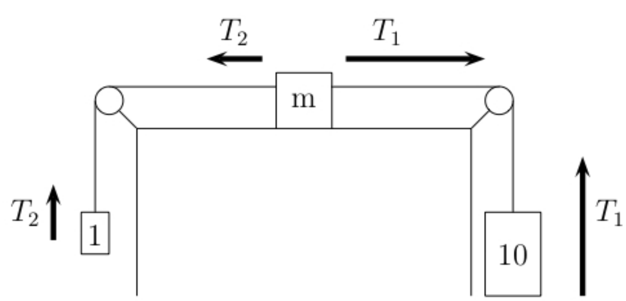
To assist you with these questions, consider the following: In the example on the left in (a), a downward force of 1 gm times g ($f = 1g$; where g is the acceleration due to gravity) is pulling a combined mass of $(1 + m)$ grams. If the 1 gm body were not connected to m, its acceleration would be equal to g. We intuitively feel that the two bodies will accelerate at a rate less than g – and we are correct. *A* force equal to $1g$ dynes is accelerating a mass of $(m+1)$ grams. Since force equals mass times acceleration,

$$f = (m+1)a = 1g$$

$$a = g/(m+1);$$

where the units of acceleration are dynes/grams = cm/sec^2. The greater the value of m, the smaller the acceleration a of the two bodies. Do you see that the acceleration a in the case on the right of (a) equals $10g/(m+10)$. Again, the acceleration will always be less than g.

In example (c), mass m is pulled by mass 1 to the left, and by mass 10 to the right. Our intuition tells us that it will move to the right. What will be its acceleration? Left to



fall independently, masses 1 and 10 will accelerate downward with motive forces equal to $1g$ and $10g$, respectively. If we connect the two masses with a massless string across a pair of frictionless pulleys (ignoring mass m for the moment), Newton's Second Law tells us that they will be accelerated by a motive force equal to $(10-1)g = 9g$. We subtract the two forces because they pull on the attached string in opposite directions. Dividing by the sum of the two masses, 1 and 10, gives us the acceleration of the two-body system: $\dfrac{9}{10+1}g = \dfrac{9}{11}g$. At this point there is a single tension in the string, reducing the downward acceleration of mass 10 by $\left(1 - \dfrac{9}{11}\right) g = \dfrac{2}{11}g$, and increasing the acceleration of mass 1 by $\left(\dfrac{9}{11} + 1\right) g = \dfrac{20}{11}g$. (Again, we add the accelerations in the latter case because we consider mass 1's original acceleration as negative.) Notice that the ratio of changes in acceleration (1:10) is inversely proportional to the ratio of the accelerated masses (10:1). This is in accordance with Newton's Third Law. Adding mass m to the system increases the accelerated mass of the system by m, resulting in an acceleration of $\dfrac{9}{10+1+m}g = \dfrac{9}{11+m}g$. All three bodies will accelerate at this rate.

One interesting thing about this situation is that the tensions in the two strings are different. Thus we have two instances of the Third Law combining to move three connected bodies with the same acceleration. Let us calculate the tension in each string to show that the resultant tension is equal to a motive force which accelerates mass m by an amount equal to $\dfrac{9}{11+m}g$.

1) **Calculating T_1 between mass 10 and mass m.** Due to the addition of mass 1 and mass m, mass 10's acceleration has been diminished by

$$1g - \frac{9}{m+11}g = \frac{m+11-9}{m+11}g = \frac{m+2}{m+11}g.$$

Thus there is a tension T_1 in the string pulling up on mass 10, equal to $10\frac{m+2}{m+11}g$, which by Law 3 is met with an opposite and equal tension T_1, pulling mass m to the right.

2) **Calculating T_2 between mass 1 and mass m.** Mass 1's acceleration has been increasing from -1g to $\frac{9}{11}g$, an increase of

$$1 + \frac{9}{m+11}g = \frac{m+11+9}{m+11}g = \frac{m+20}{m+11}g.$$

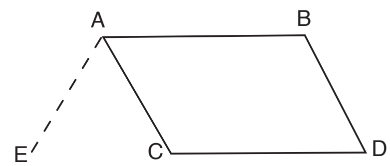
Thus there is a tension T_2 in the string pulling up on mass 1, equal to $1\frac{m+20}{m+11}g$ which by Law 3 is met with an opposite and equal tension T_2, pulling mass m to the left.

3) The difference in tensions $T_1 - T_2$ is equal to

$$\left(\frac{10(m+2)}{m+11} - \frac{1(m+20)}{m+11}\right)g = \frac{(10m+20-m-20)}{m+11}g = m\frac{9}{m+11}g.$$

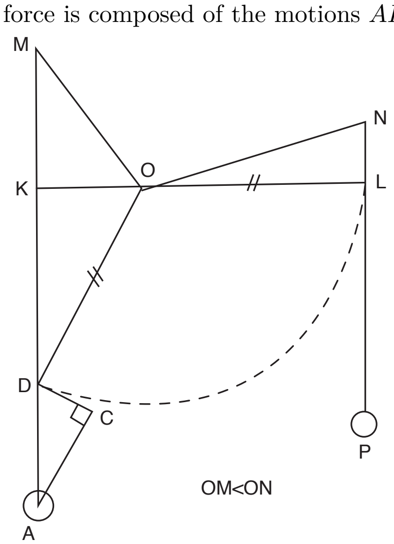
This is the net force acting on mass m which by Law 2 will accelerate a mass of m to the right by the quantity $\frac{9}{m+11}g$. This is what we predicted at the beginning of this note.

Notes to the Corollaries to the Laws of Motion: The implications of Newton's laws are numerous and profound. Perhaps the most important part of studying the corollaries is to see exactly how they follow from the laws. It is with this in mind that these notes were written.

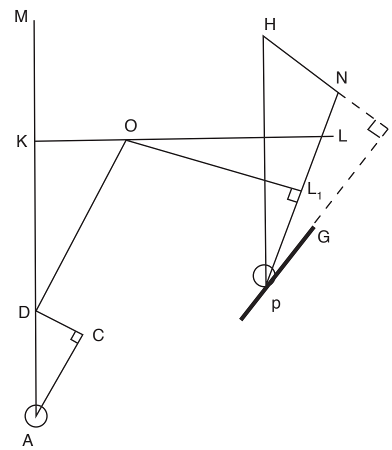


Corollary 1: The forces M and N are impressed on a body at place A, after which no more force is impressed and the body moves with uniform motion. What is its path? Newton's proof falls out directly from Laws I and II. Each impressed force results in a uniform motion (Law I) in the direction of the force (Law II), force M causing motion from A to B, force N causing motion from A to C, both in a given time t. Force N cannot prevent the body from reaching some point along BD (parallel to N's direction AC) in time t; likewise, M cannot prevent the body from reaching some point along CD (parallel to M's direction AB) in time t. Therefore, in time t the body will be found at the intersection of BD and CD, that is, at D. Note that AD gives the direction resulting from the united forces.

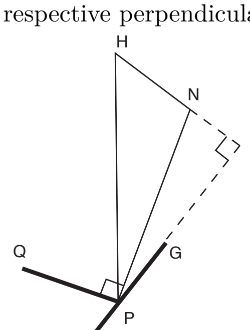
How would you determine the motion if more than two forces were acting at A? In one edition (E_2i) of the Principia, Newton draws the following figure, saying "If the body by a third force impressed at A is carried from A to E in that given time, the resulting impressed force is composed of the motions AD and AE. And so on ad infinitum."



Corollary 2: Part (a): We should imagine points M, K, D, N, and L as being located on a wheel (situated vertically) which rotates about center O. Weights may be suspended from the previously mentioned points. A given weight suspended from M, K, or D has the same power to turn the wheel counterclockwise; another given weight suspended from N or L has the same power to turn the wheel clockwise. Now represent the whole force of weight A by the line AD. Resolve AD into two forces perpendicular to one another: 1) force AC which pulls along radius OD (it was drawn parallel to OD) and thus causes no rotation of the wheel; and 2) force DC which pulls radius OD perpendicularly. What weight does P have to be in order that the wheel not move? Since $OD = OL$, if $P : A :: DC : DA$, then force DC pulling OD will have the same effect as P pulling OL. But since triangles ADC and DOK are similar, then $DC : DA :: OK : OD :: OK : OL$. And since it doesn't matter whether A is suspended from K or from D, the wheel will remain in equilibrium if $P : A :: OK : OL$. And thus the weights are inversely as the radii placed in a straight line. Therefore Newton believes he has revealed the well-known property of the balance, lever, and wheel as resulting from the resolution of force AD into oblique forces DC and AC.



Part (b): Our intuition tells us that if weight P, hanging from N, is partly supported by a plane pG, that the wheel (previously in equilibrium) will turn counterclockwise unless weight P is increased. In order to calculate the new weight of P (now called p), we draw pH perpendicular to the horizon and NH perpendicular to the supporting plane pG. Let Hp represent the downward tending force of p. By Corollary 1, Hp can be resolved into forces HN and pN. Finally, imagine a plane pQ perpendicular to pN such that the resolve forces HN and pN are entirely supported by planes pG and pQ, respectively. These two oblique planes form the wedge Newton will talk about in the last paragraph of the corollary. If we now take away plane pQ, the weight will stretch the chord with force (tension) pN. Weights A and p will have the same power of moving the wheel if their ratio is inversely as their respective perpendicular distances from the wheel's center, O, that is, if



tension pN : tension (or weight) $A :: OK : OL_1$. But,

weight p : tension $pN :: pH : pN$. So if, as just stated,

tension pN : weight $A :: OK : OL_1$, then, by compounding ratios,

weight p : weight A comp. $OK : OL_1, pH : pN$.

Note: Newton incorrectly gives the first ratio as $OK : OL$ and does not have line OL_1 on his diagram.

Note that p must be increased from its former value P, both because its effective weight has been reduced by the supporting plane pG, and because this weight is acting on a shortened lever arm OL_1 (which is less than OL).

Corollary 3: Conservation of what Leibniz called "progress" (mass times directed speed, or directed motion) immediately follows from Law III. If the bodies collide while moving in the same direction, the one following will lose as much progress as is added to the one in front. (Note that Newton can speak of motion being added or subtracted from motion, as the word now has a precise, technical sense as given in definition II.) If the bodies collide while moving in opposite or contrary directions, the same amount of motion will be subtracted from both. Newton then gives an example of a collision and "investigates" a variety of outcomes.

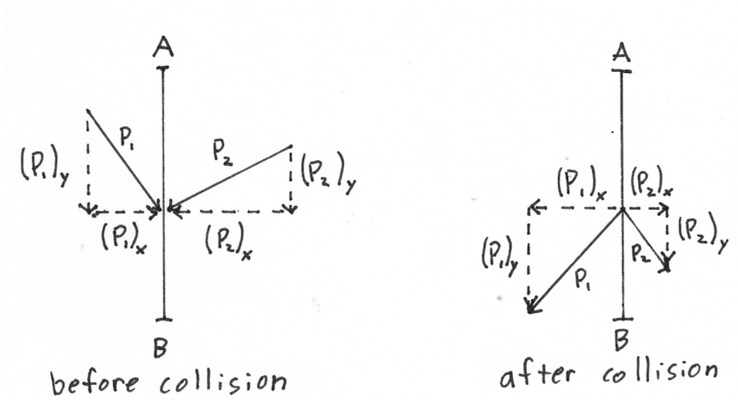
mass$A$: mass$B$ :: 3 : 1

A moves with speed $AD = 2$

B moves with speed $BD = 10$

We can use the method in Huygens' Prop. IX (finding the center of gravity C and making $CE = CD$) to determine the outcome in the case of perfectly hard bodies. After collision, A should move forward with speed $EA = 6$, B backward with speed $EB = 2$. Note that the relative speed (8) is also conserved (Huygens' Prop. IV).

But Newton is interested in the conservation of motion. Before collision, the quantity of motion is $(3\times2)+(1\times10) = 16$. After collision, the quantity of motion is $(3\times6)-(1\times2) = 16$. Body A has acquired $18 - 6 = 12$ parts of motion, B has gained $-2 - 10 = -12$ parts, that is lost 12 parts. Oddly enough, this is the last case Newton considers. Perhaps this is because he is interested in the universal applicability of the corollary, that is, in the cases where bodies are not perfectly hard and where *vis viva*, measured by mv^2, is not conserved. We will witness this same interest in the Scholium which follows.



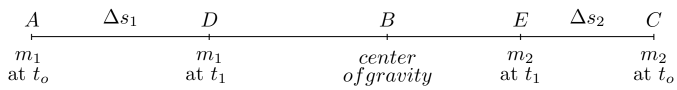
In the last part of the corollary, Newton investigates oblique collisions, those in which the bodies are not traveling along the same straight line. The solution is ingenious: we need only to establish the plane tangent to the bodies at the point of impact, and then resolve (Corollary 2) their respective motions into motions parallel and perpendicular to this plane. P_1 and P_2 represent the quantities and directions of two "motions" colliding at a point. The two motions have been resolved into motions parallel and perpendicular to the plane tangent to the colliding bodies (represented by AB) at their point of impact. The motions parallel to the plane (in the y-direction), $(P_1)_y$ and $(P_2)_y$, are not changed by the impact. The motions perpendicular to the plane (in the x-direction), $(P_1)_x$ and $(P_2)_x$, change after collision. According to Corollary 3, the difference $(P_2)_x - (P_1)_x$ of the motion to the left and the motion to the right before the collision is the same as the difference $(P_1)_x - (P_2)_x$ after the collision.

Corollary 4: In other words, no matter how complicated the actions of various bodies are in relation to one another, the center of gravity of the universe remains either at rest or moves uniformly in a straight line. To build up to this large thought, Newton takes two bodies which are free both from any external forces and from any actions upon one another. By Law I, they must either be at rest or moving in uniform straight line motion. Newton considers the latter case. By the use of a purely mathematical lemma (XXIII), he can show that the center of gravity (a point dividing the distance between the two bodies in a given ratio) must either remain at rest or move uniformly in a straight line. It follows naturally that the center of gravity of the two original bodies and some third body themselves have a center of gravity which must either remain at rest or move uniformly in a straight line. It is an easy move from three bodies to four, five, and so on, *in infinitum*.

Next Newton considers the more realistic case where two or more bodies do indeed act on one another. "Since the distances between their centers and the common center of gravity are reciprocally as the bodies, the relative motions of these bodies, whether of approaching to or of receding from that center, will be equal among themselves."

Take two bodies with masses m_1 and m_2, moving toward one another with their common center of gravity at B (B itself can also be moving). Newton's claim is that at every moment,

$m_1v_1 = m_2v_2$, *if the speeds are measured in relation to B.*



For, at t_0, $AB : CB :: m_2 : m_1$,
and at t_1, also $DB : EB : m_2 : m_1$ [Archimedes],
so $AB : CB :: DB : EB :: m_2 : m_1$.

The speeds of m_1 and m_2, measured in relation to B, are proportional to the changes in distance from center B traversed in the time interval from t_0 to t_1. These distances are $AB - DB$ and $CB - EB$.

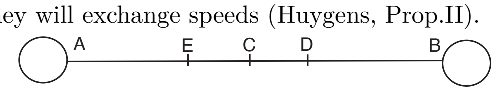
And $(AB - DB) : (CB - EB) : m_2 : m_1$ [Euclid V.19]
that is, $\Delta s_1 : \Delta s_2 :: m_2 : m_1$.
But this is true at every single moment after time t_0, so necessarily at every moment
$v_1 : v_2 :: m_2 : m_1$, measured from B,
and $v_1m_1 = v_2m_2$ with speeds measured from B. Q.E.D.

"Therefore this center (by equal changes directed to contrary parts, and this by the actions of these bodies among themselves) is neither accelerated nor retarded nor suffers any change as to its state of motion or rest."

Relative to the center of gravity, $m_1v_1 = m_2v_2$ at all times, as has just been shown. Therefore, when bodies exert forces upon one another "the changes (in m_1v_1 and m_2v_2) are equal and directed to contrary parts" relative to the center of gravity. By Law III, this is true relative to all points which are not accelerating. Now from an accelerated point, both bodies would appear to be impelled by a force from without in a direction opposed to the acceleration of the point. And it follows that the total quantity of motion, mv, of both bodies together would be continually changing. Therefore the changes in m_1v_1 and m_2v_2 would not be equal and opposed, relative to an accelerated point. But they were just shown to be equal and directed to contrary parts, relative to the center of gravity. Therefore "the common center. . . is neither accelerated nor retarded."

Corollary 5: Huygens' boat, at rest or in motion, turns out to be an image of space itself. Imagine equal bodies A and B moving toward one another with speeds 4 and 2, respectively. After colliding we know that they will exchange speeds (Huygens, Prop.II).

Let $A = B = 1$
$AD = 4$, $BD = 2$

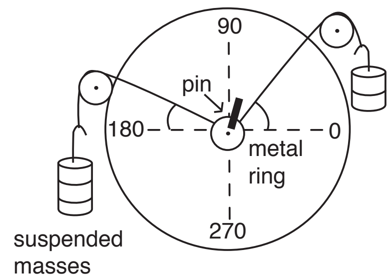


The sum of the two motions toward contrary parts is 4+2=6, both before and after the collision. Now, imagine the space itself to move from left to right with a uniform speed of 7. Since both bodies will now be traveling in the same direction, body A with a motion of 7+4 = 11, B with a motion of 7-2=5, we find the *difference* in their motions, 11-5=6. Therefore, by Law II, the "same" collision is occurring. And indeed, when we subtract the motions after collision, $(7 + 4) - (7 - 2) = 6$. Newton says "this very same thing was established by a brilliant [*lucento*] experiment. All motions occur in the same manner on a boat, whether that boat be at rest or moving uniformly in a straight line."

Corollary 6: Can you see why this corollary does not apply to bodies urged by equal accelerative forces in the direction of lines which are *not* parallel?

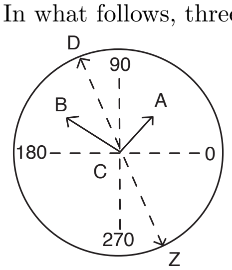
Experiment: Force Tables (with Corollary 1)

In this experiment we will practice adding forces which act in different directions at a given point. Our forces will be the weights of masses hung on metal stands (make sure to include the weight of the stands in your calculations), connected by strings to a metal ring. Since weight (mg) is proportional to mass (m), we can substitute mass for weight in all of the calculations.



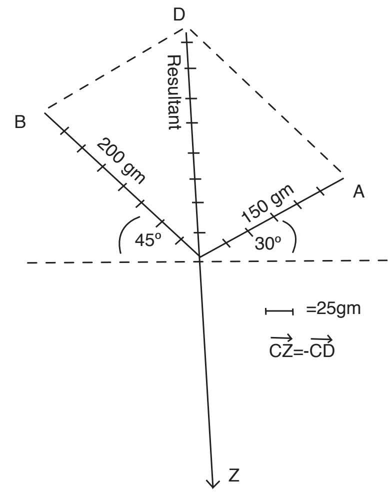
With the pin in place, select masses to hang from two different angles on the force table. In order that we may use the scale of angles on the table, the forces must act along lines passing through the table's center. Thus the strings should be adjusted on the metal rings such that if extended they would pass through the center of the table.

The pin prevents the system from moving. Try to guess the direction of the motion that will occur when the pin is removed. While holding the metal ring, remove the pin and allow the system to move slowly. Both **CA** and **CB** represent forces whose direction is from the center of the table and whose magnitude is the product of the mass m and the gravitational acceleration g. When the pin is removed the system will be accelerated by force **CD**, whose direction and magnitude are determined by the masses and angles selected. To achieve equilibrium, a force **CZ** must be selected which is equal to **CD** in magnitude but opposite in direction. In what follows, three ways are given for selecting **CZ**.



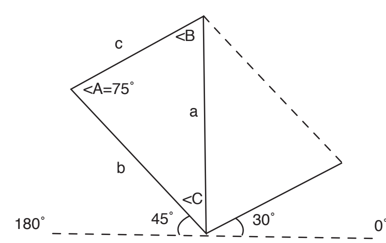
Forces CB+CA=Force CD
Force CZ balances Force CD

1. The first method is the one of trial and error. Select a third mass and a direction which you think will balance forces **CA** and **CB** and, with the same precaution as before, remove the pin. Continue to change both the mass and the direction until the ring does not move when the pin is removed. You may notice that adding or subtracting one or two grams and changing the direction of the force by half a degree to either side has no effect on the motion of the ring. This seems to be the limit of sensitivity for most of our tables.



2. The second method involves drawing a force parallelogram. Before hanging the initial masses, attach a blank sheet of paper to the force table. Draw two straight lines in any direction from the center and let their respective lengths be determined by the masses you are going to suspend. Choose a convenient scale (for example, 1 cm = 25 gm) and draw the lengths. Then complete the force parallelogram and measure the diagonal which represents the magnitude and direction of the resultant force. To verify this experimentally, hang masses along the chosen directions, and on the third stand hang a mass equal in magnitude and opposite in direction to the resultant force. It is important to realize that we could use this method to calculate the force resulting form the combination of any number of forces.

3. (optional) The third method is the same as the second, except that after drawing the force parallelogram, we calculate the length of the diagonal and its direction (relative to the scale on the force table) by using the law of cosines instead of by measuring them directly.[28]



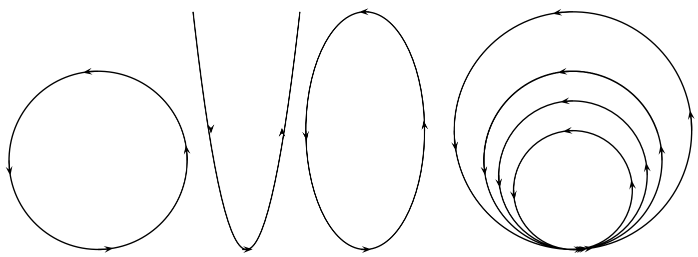
Example: In triangle ABC, $a^2 = b^2 + c^2 - 2bc\cos A$, $B = 200$ gm, $C = 150$ gm, and $\angle A = 75^o$ (why?); Thus $a = 217$ gm, very nearly.

Solving for $\angle C, c^2 = a^2 + b^2 - 2ab\cos C$, Thus $\angle C = 42^o$, very nearly, And the resultant force will be pulling at an angle of $180 - (45 + 30 + 42) = 93^o$ (as measured on the table).

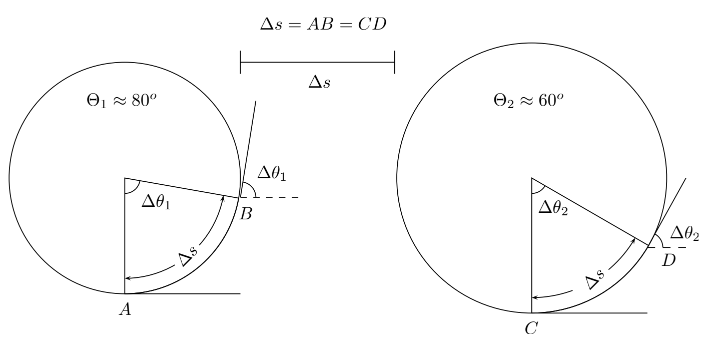
[28]The law of cosines can be derived form Euclid's *Elements* Book II, Propositions 12 and 13.

What does $1/PR$ represent? Taylor has constructed line PR equal to the radius of a circle which shares points p, P, and π with the string whose shape need not be a circle. Such a circle is called the circle of curvature to the curve of the string at point P. We will show in the following note that the curvature at P is measured by the reciprocal of the radius of the circle of curvature at P, that is, the curvature at P is equal to $1/PR$.

Note on Curvature



Compare the curvature of a circle with that of a parabola or ellipse. The circumference of a circle does not become more or less curved as we move along it in thought. Its curvature is constant. But the circumference of a smaller circle is more curved than that of a large circle. How might we quantify this difference? One way is to assume some unit distance Δs, mark off an arc equal to that distance, draw the tangents at the beginning and end of that arc, measure the difference in orientation of these tangents in degrees, and then measure the curvature of a circle in terms of degrees of orientation-change per distance.



Supposing $\Delta s = AB = CD$ (in length), and that θ_1 is about 80^o, while θ_2 is about 60^o, we may say that the ratio between the two curvatures is about $4:3$. Let curvature be represented by κ. Then in this case, $\kappa_1 : \kappa_2 :: 4:3$ very nearly.

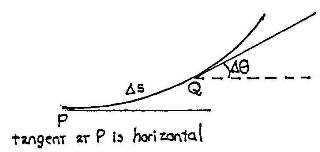
To remove some arbitrariness in this way of measuring curvature, let the distance Δs be equal to the circumference of the circle $= 2\pi r$, and let the angle θ be measured in radians instead of degrees ($360^o = 2\pi$ radians). Then, for any given circle,

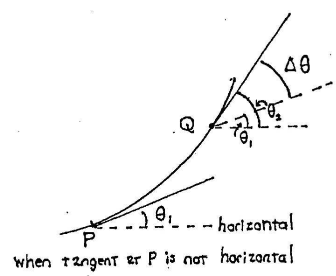
$$\text{Curvature } \kappa = \text{ angular change measured in radians/circumference of circle}$$

$$= 2\pi/2\pi r = 1/r.$$

We increase κ by decreasing the radius r, and decrease κ by increasing the radius r. Thus the curvature of a given circle is the same at every one of its points and is equal to 1/radius.

Having established that the curvature at any one of its points on a circle is equal to 1/radius, we can consider the curvature at points on other lines. For example, the curvature of a straight line (more precisely, at any one of its points) is always equal to zero. That's easy. Harder is the curvature of a non-circular curved line. Let the following be such a line. Let it be smoothly curved between P and Q. And let the tangents have been ascertained, as shown.





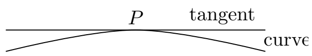
Angle $\Delta\theta$ measures the total curvature from P to Q. Let Δs be the length of the arc PQ. Then $\Delta\theta/\Delta s$ equals the average curvature of the arc PQ. At the point P the curvature is the limit of the average curvatures as the length of the arc Δs vanishes:

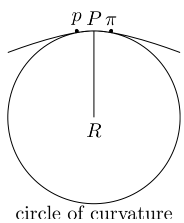
$$\kappa_P = \lim_{\Delta s \to 0} \frac{\Delta\theta}{\Delta s},$$

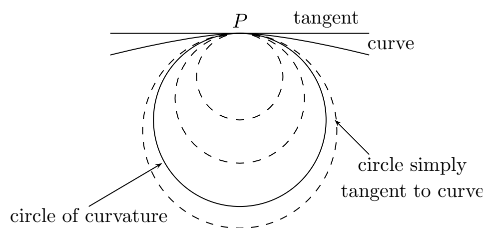
assuming this limit exists; that is,

$$\kappa_P = \frac{d\theta}{ds}.$$

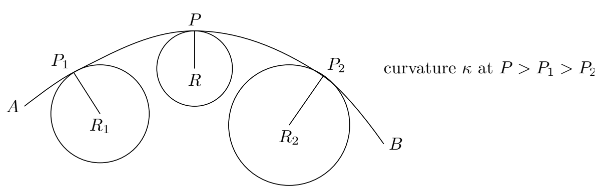
Let's now go back to Taylor's stretched string. Lemma 2 proved that the acceleration of an infinitely small particle pP on the string was proportional to $1/PR$, PR being the radius of the circle of curvature to the string at point P. This circle is called the circle of curvature at P precisely because it has the very same curvature which the string has at point P; and this circle is said to measure the curvature of the string at P. Unlike a tangent to the curve which touches it only at point P, the circle of curvature "touches" the curve at p, P, and π as the distances pP and $P\pi$ become infinitely small. Such a circle was named an "osculating (kissing) circle."



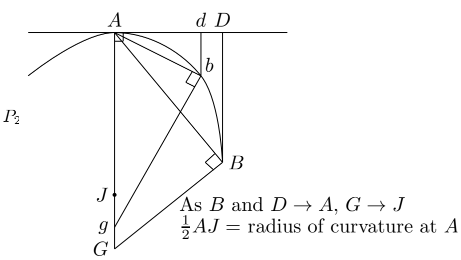




While there are many circles which are tangent to the curve right at P, only one of them is an osculating circle, that is, only one of them measures its curvature.[4] And as different points along the stretched string have different curvatures (resulting in different accelerations), they are measured by different circles of curvature.



different curvatures at different points on curve AB



Although any three non-collinear (not in a straight line) points determine a circle, it is perhaps awkward to talk about a curve and its circle of curvature as sharing three points p, P, and π, when there is clearly no finite distance between the points. In Lemma XI, Newton gets around this problem by the method of limits, showing that for any curve AB having finite curvature at A, as points D and B approach A, lines BG and AG intersect ultimately at point J, determining AJ as the diameter of the circle of curvature, and thus $\frac{1}{2}AJ$ as the radius of this circle.[5]

[4]"Any two curves are said to "*osculate*" at point P to have "*contact of order two*", if they pass through P, have the same tangent at P, and also have the same curvature." Courant and John, *Introduction to Calculus and Analysis*, Volume I, pages 355 ff.

[5]You can see that we have introduced two ways of measuring the curvature κ at a point P on a curve: 1) by $1/r$, the reciprocal of the radius of the *circle of curvature* at point P; and 2) by $d\theta/ds$, the instantaneous rate of change of the *direction* of the curve with respect to the *distance traveled* along the curve at P.

 JK MP

A note on Propositions 7-10 of Book I of Newton's <u>Principia</u>

Joe. Sachs

Newton's laws replace the classical principle of regular circular motion with a principle of regular straight-line motion. The latter has arguments to justify it in Descartes' <u>Le Monde</u>, as the former has in Aristotle's <u>Physics</u>, but the two assumptions are equally arbitrary. Under Newton's assumptions, then, a circular motion would be a derived one, a complex result of elementary causes which do not act in circles.

Proposition 4 shows that a circular orbit is possible in a Newtonian world under any force law. If a body happens to be moving at right angles to the line joining it to a center of centripetal force, and if its inertial speed is just such as to make the center of its circle of curvature coincide with the center of force, then the original situation will be continually reproduced and the body will move always at the same distance from the center, at right angles to its radius, and at the same speed. Since Newton assumes accelerative force always to be the same at the same distance, and that distance does not change here, the particular law of variation of that force is irrelevant.

But the conditions of Prop. 4 are infinitely unlikely. What would happen if any of them were varied slightly? The device of Prop. 6 gives a way to begin exploring that question, and Newton's first use of it is in application to the eccentric circle. Is an eccentric circular orbit possible under Newton's laws? Prop. 7 says yes, if and only if the centripetal force varies jointly as the inverse square of the distance and the inverse cube of the <u>chord</u>. If the orbit is to be an eccentric circle, it is no longer determined solely by the absolute force at the center and distance, direction, and speed of the body. The center of force must somehow sense and adjust itself not only to where the body is but also to where it is going to be at some later time.

Newton states the force law of Prop. 7 deadpan. As always, his largest conclusions are between the lines. In this case, Corollary 1 and Prop. 8 point the way to what he is doing. Why does Newton move the center of force first into the circumference and then to an infinite distance? In neither case could a whole orbit be completed once, but only in those cases is the force law of Prop. 7 transformed into an intelligible one. Props. 7 and 8 together, then, form a <u>reductio</u> proof. Under Newton's assumptions, an eccentric circle is an intelligibl possibility only when it is a physical impossibility.

Circular motion, then, seems effectively banished from the world. It is too pure a ~~prix~~ principle, requiring exact centricity and suffering no approximation.

Propositions 9 and 10 similarly form a pair, with an inexplicit intention. The logarithmic spiral results from an inverse cube force law, and the central ellipse from a force varying directly as the distance. In the former case the force falls away too rapidly to hold an orbit together. In the latter, where it increases, the orbits are held so tightly that there is only one periodic time for them all. Under such a law, the planets would appear as one more constellation moving together among the fixed stars. In fact our world has closed orbits and presents the beautiful variety of planetary motion, but the two force laws in Props. 9 and 10 seem to violate more than the facts. The very notion of a world seems to involve some combination of stability and variety. If there is to be a world at all then, the two force laws presented will form the bookends or boundary-posts of its possibility. Centripetal force must fall away with distance, but not so rapidly as with the cube.

A brief note on Prop. 2, Book III ("the quiescence of the aphelion points")

In props. 43-5 of Book I, Newton develops a theory of moving orbits, and discovers that the only way the effect can be produced of a closed orbit rotating as a whole is by a centripetal force varying nearly as the inverse square, or by an inverse square force slightly perturbed. In fact, the line of apsides of every planet does rotate, slowly, in the direction that would result from a force slightly greater than the inverse square. None has a motion as great of that of the moon, discussed in the next proposition, where the perturbation might be expected to be greater.

Engraved by W.T.Fry.

OPTICKS

OR

A Treatise of the Reflections,
Refractions, Inflections
& Colours of Light

SIR ISAAC NEWTON

BASED ON THE FOURTH EDITION LONDON, 1730

With a Foreword by
ALBERT EINSTEIN
An Introduction by
SIR EDMUND WHITTAKER
A Preface by
I. BERNARD COHEN
And an Analytical Table of Contents
prepared by
DUANE H. D. ROLLER

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NEW YORK

tance, so as sometimes to take up above a Million of Times more space than they did before in the form of a dense Body. Which vast Contraction and Expansion seems unintelligible, by feigning the Particles of Air to be springy and ramous, or rolled up like Hoops, or by any other means than a repulsive Power. The Particles of Fluids which do not cohere too strongly, and are of such a Smallness as renders them most susceptible of those Agitations which keep Liquors in a Fluor, are most easily separated and rarified into Vapour, and in the Language of the Chymists, they are volatile, rarifying with an easy Heat, and condensing with Cold. But those which are grosser, and so less susceptible of Agitation, or cohere by a stronger Attraction, are not separated without a stronger Heat, or perhaps not without Fermentation. And these last are the Bodies which Chymists call fix'd, and being rarified by Fermentation, become true permanent Air; those Particles receding from one another with the greatest Force, and being most difficultly brought together, which upon Contact cohere most strongly. And because the Particles of permanent Air are grosser, and arise from denser Substances than those of Vapours, thence it is that true Air is more ponderous than Vapour, and that a moist Atmosphere is lighter than a dry one, quantity for quantity. From the same repelling Power it seems to be that Flies walk upon the Water without wetting their Feet; and that the Object-glasses of long Telescopes lie upon one another without touching; and that dry Powders are difficultly made to touch one another so as to stick together, unless by melting them, or wetting them with Water, which by exhaling may bring them together; and that two polish'd Marbles, which by immediate Contact stick together, are difficultly brought so close together as to stick.

And thus Nature will be very conformable to her self and very simple, performing all the great Motions of the heavenly Bodies by the Attraction of Gravity which intercedes those Bodies, and almost all the small ones of their Particles by some other attractive and repelling Powers which intercede the Particles. The *Vis inertiæ* is a passive Principle by which Bodies persist in their Motion or Rest, receive Motion in proportion to the Force impressing it, and resist as much as they are resisted. By this Principle alone there never could have been any Motion in the World. Some other Principle was necessary for putting Bodies into Motion; and now they are in Motion, some other Principle is necessary for conserving the Motion. For from the various Composition of two Motions, 'tis very certain that there is not always the same quantity of Motion in the World. For if two Globes joined by a slender Rod, revolve about their common Center of Gravity with an uniform Motion, while that Center moves on uniformly in a right Line drawn in the Plane of their circular Motion; the Sum of the Motions of the two Globes, as often as the Globes are in the right Line described by their common Center of Gravity, will be bigger than the Sum of their Motions, when they are in a Line perpendicular to that right Line. By this Instance it appears that Motion may be got or lost. But by reason of the Tenacity of Fluids, and Attrition of their Parts, and the Weakness of Elasticity in Solids, Motion is much more apt to be lost than got, and is always upon the Decay. For Bodies which are either absolutely hard, or so soft as to be void of Elasticity, will not rebound from one another. Impenetrability makes them only stop. If two equal Bodies meet directly *in vacuo*, they will by the Laws of Motion stop where they meet, and lose all their Motion, and remain in rest, unless they be elastick, and receive new Motion from their Spring. If they have so much Elasticity as suffices to make them re-bound with a quarter, or half, or three quarters of the Force with which they come together, they will lose three quarters, or half, or a quarter of their Motion. And this may be try'd, by letting two equal Pendulums fall against one another from equal heights. If the Pendulums be of Lead or soft Clay, they will lose all or almost all their Motions: If of elastick Bodies they will lose all but what they recover from their Elasticity. If it be said, that they can lose no Motion but what they communicate to other Bodies, the consequence is, that *in vacuo* they can lose no Motion, but when they meet they must go on and penetrate one another's Dimensions. If three equal round Vessels be filled, the one with Water, the other with Oil, the third with molten Pitch, and the Liquors be stirred about alike to give them a vortical Motion; the Pitch by its Tenacity will lose its Motion quickly, the Oil

being less tenacious will keep it longer, and the Water being less tenacious will keep it longest, but yet will lose it in a short time. Whence it is easy to understand, that if many contiguous Vortices of molten Pitch were each of them as large as those which some suppose to revolve about the Sun and fix'd Stars, yet these and all their Parts would, by their Tenacity and Stiffness, communicate their Motion to one another till they all rested among themselves. Vortices of Oil or Water, or some fluider Matter, might continue longer in Motion; but unless the Matter were void of all Tenacity and Attrition of Parts, and Communication of Motion, (which is not to be supposed,) the Motion would constantly decay. Seeing therefore the variety of Motion which we find in the World is always decreasing, there is a necessity of conserving and recruiting it by active Principles, such as are the cause of Gravity, by which Planets and Comets keep their Motions in their Orbs, and Bodies acquire great Motion in falling; and the cause of Fermentation, by which the Heart and Blood of Animals are kept in perpetual Motion and Heat; the inward Parts of the Earth are constantly warm'd, and in some places grow very hot; Bodies burn and shine, Mountains take fire, the Caverns of the Earth are blown up, and the Sun continues violently hot and lucid, and warms all things by his Light. For we meet with very little Motion in the World, besides what is owing to these active Principles. And if it were not for these Principles, the Bodies of the Earth, Planets, Comets, Sun, and all things in them, would grow cold and freeze, and become inactive Masses; and all Putrefaction, Generation, Vegetation and Life would cease, and the Planets and Comets would not remain in their Orbs.

All these things being consider'd, it seems probable to me, that God in the Beginning form'd Matter in solid, massy, hard, impenetrable, moveable Particles, of such Sizes and Figures, and with such other Properties, and in such Proportion to Space, as most conduced to the End for which he form'd them; and that these primitive Particles being Solids, are incomparably harder than any porous Bodies compounded of them; even so very hard, as never to wear or break in pieces; no ordinary Power being able to divide what God himself made one in the first Creation. While the Particles continue entire, they may compose Bodies of one and the same Nature and Texture in all Ages: But should they wear away, or break in pieces, the Nature of Things depending on them, would be changed. Water and Earth, composed of old worn Particles and Fragments of Particles, would not be of the same Nature and Texture now, with Water and Earth composed of entire Particles in the Beginning. And therefore, that Nature may be lasting, the Changes of corporeal Things are to be placed only in the various Separations and new Associations and Motions of these permanent Particles; compound Bodies being apt to break, not in the midst of solid Particles, but where those Particles are laid together, and only touch in a few Points.

It seems to me farther, that these Particles have not only a *Vis inertiæ*, accompanied with such passive Laws of Motion as naturally result from that Force, but also that they are moved by certain active Principles, such as is that of Gravity, and that which causes Fermentation, and the Cohesion of Bodies. These Principles I consider, not as occult Qualities, supposed to result from the specifick Forms of Things, but as general Laws of Nature, by which the Things themselves are form'd; their Truth appearing to us by Phænomena, though their Causes be not yet discover'd. For these are manifest Qualities, and their Causes only are occult. And the *Aristotelians* gave the Name of occult Qualities, not to manifest Qualities, but to such Qualities only as they supposed to lie hid in Bodies, and to be the unknown Causes of manifest Effects: Such as would be the Causes of Gravity, and of magnetick and electrick Attractions, and of Fermentations, if we should suppose that these Forces or Actions arose from Qualities unknown to us, and uncapable of being discovered and made manifest. Such occult Qualities put a stop to the Improvement of natural Philosophy, and therefore of late Years have been rejected. To tell us that every Species of Things is endow'd with an occult specifick Quality by which it acts and produces manifest Effects, is to tell us nothing: But to derive two or three general Principles of Motion from Phænomena, and afterwards to tell us how the Properties and Actions of all corporeal Things follow from those manifest Principles, would be a very great step in

Philosophy, though the Causes of those Principles were not yet discover'd: And therefore I scruple not to propose the Principles of Motion above-mention'd, they being of very general Extent, and leave their Causes to be found out.

Now by the help of these Principles, all material Things seem to have been composed of the hard and solid Particles above-mention'd, variously associated in the first Creation by the Counsel of an intelligent Agent. For it became him who created them to set them in order. And if he did so, it's unphilosophical to seek for any other Origin of the World, or to pretend that it might arise out of a Chaos by the mere Laws of Nature; though being once form'd, it may continue by those Laws for many Ages. For while Comets move in very excentrick Orbs in all manner of Positions, blind Fate could never make all the Planets move one and the same way in Orbs concentrick, some inconsiderable Irregularities excepted, which may have risen from the mutual Actions of Comets and Planets upon one another, and which will be apt to increase, till this System wants a Reformation. Such a wonderful Uniformity in the Planetary System must be allowed the Effect of Choice. And so must the Uniformity in the Bodies of Animals, they having generally a right and a left side shaped alike, and on either side of their Bodies two Legs behind, and either two Arms, or two Legs, or two Wings before upon their Shoulders, and between their Shoulders a Neck running down into a Back-bone, and a Head upon it; and in the Head two

Ears, two Eyes, a Nose, a Mouth, and a Tongue, alike situated. Also the first Contrivance of those very artificial Parts of Animals, the Eyes, Ears, Brain, Muscles, Heart, Lungs, Midriff, Glands, Larynx, Hands, Wings, swimming Bladders, natural Spectacles, and other Organs of Sense and Motion; and the Instinct of Brutes and Insects, can be the effect of nothing else than the Wisdom and Skill of a powerful ever-living Agent, who being in all Places, is more able by his Will to move the Bodies within his boundless uniform Sensorium, and thereby to form and reform the Parts of the Universe, than we are by our Will to move the Parts of our own Bodies. And yet we are not to consider the World as the Body of God, or the several Parts thereof, as the Parts of God. He is an uniform Being, void of Organs, Members or Parts, and they are his Creatures subordinate to him, and subservient to his Will; and he is no more the Soul of them, than the Soul of Man is the Soul of the Species of Things carried through the Organs of Sense into the place of its Sensation, where it perceives them by means of its immediate Presence, without the Intervention of any third thing. The Organs of Sense are not for enabling the Soul to perceive the Species of Things in its Sensorium, but only for conveying them thither; and God has no need of such Organs, he being every where present to the Things themselves. And since Space is divisible *in infinitum*, and Matter is not necessarily in all places, it may be also allow'd that God is able to create Particles of Matter of several Sizes and

Figures, and in several Proportions to Space, and perhaps of different Densities and Forces, and thereby to vary the Laws of Nature, and make Worlds of several sorts in several Parts of the Universe. At least, I see nothing of Contradiction in all this.

As in Mathematicks, so in Natural Philosophy, the Investigation of difficult Things by the Method of Analysis, ought ever to precede the Method of Composition. This Analysis consists in making Experiments and Observations, and in drawing general Conclusions from them by Induction, and admitting of no Objections against the Conclusions, but such as are taken from Experiments, or other certain Truths. For Hypotheses are not to be regarded in experimental Philosophy. And although the arguing from Experiments and Observations by Induction be no Demonstration of general Conclusions; yet it is the best way of arguing which the Nature of Things admits of, and may be looked upon as so much the stronger, by how much the Induction is more general. And if no Exception occur from Phænomena, the Conclusion may be pronounced generally. But if at any time afterwards any Exception shall occur from Experiments, it may then begin to be pronounced with such Exceptions as occur. By this way of Analysis we may proceed from Compounds to Ingredients, and from Motions to the Forces producing them; and in general, from Effects to their Causes, and from particular Causes to more general ones, till the Argument end in the most general. This is the Method of Analysis: And the

Synthesis consists in assuming the Causes discover'd, and establish'd as Principles, and by them explaining the Phænomena proceeding from them, and proving the Explanations.

In the two first Books of these Opticks, I proceeded by this Analysis to discover and prove the original Differences of the Rays of Light in respect of Refrangibility, Reflexibility, and Colour, and their alternate Fits of easy Reflexion and easy Transmission, and the Properties of Bodies, both opake and pellucid, on which their Reflexions and Colours depend. And these Discoveries being proved, may be assumed in the Method of Composition for explaining the Phænomena arising from them: An Instance of which Method I gave in the End of the first Book. In this third Book I have only begun the Analysis of what remains to be discover'd about Light and its Effects upon the Frame of Nature, hinting several things about it, and leaving the Hints to be examin'd and improv'd by the farther Experiments and Observations of such as are inquisitive. And if natural Philosophy in all its Parts, by pursuing this Method, shall at length be perfected, the Bounds of Moral Philosophy will be also enlarged. For so far as we can know by natural Philosophy what is the first Cause, what Power he has over us, and what Benefits we receive from him, so far our Duty towards him, as well as that towards one another, will appear to us by the Light of Nature. And no doubt, if the Worship of false Gods had not blinded the Heathen, their moral Philosophy would have gone farther than to the four Cardinal Virtues; and instead of teaching the Transmigration of Souls, and to worship the Sun and Moon, and dead Heroes, they would have taught us to worship our true Author and Benefactor, as their Ancestors did under the Government of *Noah* and his Sons before they corrupted themselves.

A CATALOGUE OF SELECTED DOVER BOOKS
IN ALL FIELDS OF INTEREST

A letter from Leibniz to Ehrenfried Walther von Tschirnhaus in Rome[22]

My Friend,

Since I wrote to you I have received two of your letters; first a long one in which you describe a method for finding roots of equations; then a short one in which you mention your journey. I would have answered the first right away, if you had not forbidden it because of your impending journey. I answer now, because you told me to in your second letter. I hope you have returned safely from your Sicilian journey, or will return soon. Now although I know you are careful enough in journeys, nevertheless because travelers are exposed to many chances I shall not stop worrying about you until I hear that you have returned. Our Schiller writes that he has received nothing from you for more than six months now, and I am therefore afraid that your letters to me, which you say that you included with those for Schiller, have been lost along with them. I certainly have not received the one you write about, where you expound a method for expressing any ratio or proportion through an infinite series; consequently I also have not seen the method you describe there for investigating the number of all curves. Because you wished to describe for me so amply and clearly, and not without some labor, your method for finding roots of equations, I acknowledge that I owe you much. I have read carefully and, if I am not mistaken, understood and finally grasped that the problem is not altogether solved; if anything, I believe that the problem *cannot* be solved, at least in this way. The whole thing must come down to this: let x^4+qx^2+rx+s eq. 0. x is supposed eq. to $a+b+c$; from here you construct another equation, which you put together with the first equation to make $ab+ac+bc$ eq. m, abc eq. n, and $a^4+b^4+c^4$ eq. l. Next you wish to derive the following equations: $a^4+b^4+c^4$ eq. l, $a^4b^4+a^4c^4+b^4c^4$ eq. e, and $a^4b^4c^4$ eq. n^4. And after obtaining these three equations I admit you have a, b, and c, and therefore also x. But I say it is impossible to deduce these equations, and in particular the penultimate equation (or the quantity e being sought, namely, the known value of $a^4b^4+a^4c^4+b^4c^4$) by starting from the equations assumed earlier, unless perhaps it is to be resolved through an equally difficult equation. I have explained the demonstration of my opinion about this on the attached sheet, and I have also shown the way that it does seem possible to me to arrive at the roots of equations, and added a demonstration that it will succeed. I look forward to your opinion on this. This is certainly the only way known to me that brings with it a demonstration of its success. You do understand that many of

But

$$\text{rectangle } ABCD = (AB)(AD) = (d^3)(d) = d^4.$$

Therefore

$$\text{area } ADCA = \text{rectangle } ABCD - \text{area } ABCA = d^4 - \frac{3}{4}d^4 = \frac{d^4}{4}.$$

[22] A III ii 440-452. The letter was sent at the end of May or the beginning of June in 1678.

the things you have set forth in your letter were once also explored or tried by me, and especially that method of yours for finding e, and consequently x, from the equations $ab+ac+bc$ eq. m, abc eq. n, and $a^4+b^4+c^4$ eq. l, a method which was once wonderfully pleasing to me, too, but which I afterwards understood to be useless. You have many beautiful theorems about forms arising from similar arrangements of letters; I, too, have worked much in this kind of thing, as I remember writing about in my earlier letter. For example, I have a table through which it appears at once how many examples there are of any form in a given number of letters; e.g., there are 3 examples of the form ab, given three letters. This table is built by a certain rule, which, it appears from your letter, has been sufficiently noticed by you, if I am not mistaken. I have another, more important, rule, about the multiplication of a form by a form, e.g, a^2b by ab, supposing 4 letters: a, b, c, d. Here it is:

$$\begin{array}{lllllll}
 & ab & ac & ad & bc & bd & cd \\
a^2b) & a^3b^2 & a^3bc & a^3bd & a^2b^2c & a^2b^2d & a^2bcd \\
 & \left.\frac{12}{12}\right\}1 & \multicolumn{2}{l}{\underbrace{\left.\frac{12}{12}\right\}1 \quad \left.\frac{12}{12}\right\}1}} & \multicolumn{2}{l}{\underbrace{\left.\frac{12}{12}\right\}1 \quad \left.\frac{12}{12}\right\}1}} & \left.\frac{12}{4}\right\}3
\end{array}$$

that is, a^2b by ab gives

$$a^3b^2 \qquad +2a^3bc \qquad +2a^2b^2c \qquad +3a^2bcd;$$

I not only multiply all the examples of one form by one example of another (as you also have noted) in order to find the resulting forms, but also, in order to attach numbers to the front of the resulting forms, I write under each resulting form the quotient obtained by dividing the number of examples of the multiplying form (here a^2b, whose number is 12) by the number of examples of each resulting form (such as a^2bcd, of which there are 4 examples in 4 letters, and 12 divided by 4 is 3). If a form results more than once, then the quotient should be multiplied by the number of repetitions. It is worth noting that in some cases the quotient is a fraction, but it can always be eliminated afterwards by multiplication by the number of repetitions. Next, by means of this rule I began to build a table by means of which the product of a form by a form could immediately be known at first glance, and that table has truly beautiful progressions, the greater part of which I now see. Moreover, this table will also have very beautiful uses in algebra, not only in those problems where the unknown letters are in the same arrangement, but also in all others, since all problems might finally be reduced to problems of unknowns in similar arrangements. This is extremely useful, since then beautiful abbreviations usually emerge, and (what is truly of great importance) one equation serves to find all those unknowns at the same time, that is, those different unknowns are roots of this last equation. From here their sums, the sums of the rectangles from them, and the sum of their parallelepipeds (or rectangular solids) can be found — from that same completed equation, of course. For in that equation the second term is equal to the sum of the roots, the third term to the sum of the rectangles, etc.; and since the roots of the completed equation are the same as the many indeterminates or unknowns that are applied in solving the problem, it is clear that their sum and the sum of their rectangles, etc. are obtained. For example, if $x + y$ eq. a and xy eq. b^2, the equation becomes $y^2 - ay + b^2$ eq. 0, and one root of this equation will be y, the other x. From this it is also clear that, since all problems may be reduced (either by calculation or by drawing lines) to unknowns in similar arrangements, and in problems in similar arrangements, when reduced to a completed equation, the sum of all the unknowns and the sum of their rectangles, etc., may be obtained; it follows that all problems will be resolved by applying only these; if I may use your letters, supposing the unknowns we are looking for in the problem are a, b, c, d, e, they will be found by means of x, y, z, etc if we set x eq. $a + b + c+$, y eq. $ab + ac+$ etc., z eq. $abc+$ etc. [But x, y, z should not be used to find x in turn, as happens in finding the roots of equations, and therefore the values of y, z, etc. should not be sought first, but rather the values of a, b, c, etc. through $a^4 + b^4 + c^4$ eq. $\ldots$, $a^4b^4c^4$ eq. $\ldots$] And it seems to me that this should be held to be one of the greatest secrets in all of algebra, since by means of it all problems may be reduced to a few; and tables can be built through which everything may be found without calculation. These tables also seem to be the true way of finding elegant geometric constructions. I wanted to write these things out clearly for you, because I expect that both they and many other things may be completed by you. Our analysts (if you leave out Viète) have cared too little about elegant constructions, being content to show a calculation; but since such problems are for the most part investigated for the sake of the mind rather than for practical use, it seems to me that an elegant solution should also be sought to the extent that it is possible. Huygens told me that he has thought some about making demonstrations from calculation elegant using the ancients' way, a way very different from Schooten's; I have noted many things about the art of finding elegant constructions, but nevertheless they are not yet perfect; the main secret, however, consists, as I have said, in seeking more unknowns that are arranged similarly, and also have fewer roots. For often the reason why problems ascend to a higher degree than they should arises not so much from their own nature as from the nature of the assumed unknown, which has many roots, although the problem may be resolved through another unknown having fewer. For example, if you take as an unknown the distance of a point in question from the center of a conic section, fewer roots arise than if you take the distance from the focus; for there are two foci, and therefore two distances satisfy the equation, instead of one. But I mention these things only in passing.

I come to the other parts of your letter. I had written that certain traces of the method you use for quadratures were extant in Fabri and Pascal, but that I too had tried something similar from time to time. You seem to interpret this as if I suspected you of having drawn from other sources; but this did not come into my mind even in a dream. For I know the power of your mind is so great that you can think up even more outstanding things. The whole thing comes down to this, if I am not mistaken: just as I can give a quadrature in a plane figure either by seeking the sum of all the ys or by seeking the sum of all the xs, so also in a solid, where there are three indeterminates, x, y, and z, the solid can be resolved into planes in three ways, and hence tetragonistic equations arise by comparing the values of the whole content (which is always the same) with each other. Moreover, various quadratures arise from these tetragonistic equations, as you explained. *I* have not only this method, but infinitely many other ways of obtaining tetragonistic equations through a calculus; those proposed by you are only special cases of this. Moreover, I now carry out this calculus by means of certain new and wonderfully convenient signs; when I had written to you recently about them, you answered that your way of explaining them is more ordinary and intelligible, and that you avoid *novelty in definitions* of things as much as possible; *for this*, you say, *is nothing but to make the sciences more difficult.* But the ancient arithmeticians could have made the same reply when other later arithmeticians introduced Arabic characters in place of Roman ones, as could the ancient algebraists when Viète used letters in place of numbers. In signs, fitness for finding should be considered, and this fitness is greatest whenever they briefly express the innermost nature of the thing, and as it were paint; for in this way the labor of thinking is wonderfully diminished. Such indeed are the signs applied by me in the calculus of tetragonistic equations, whereby I often solve the most difficult problems in a few lines. For example, by applying my characters I solve in three or four little lines a problem which Descartes took up in vain in his letters: to find a curve such that the interval AT, taken on the axis between the tangent CT and the ordinate EA, is a constant straight line. For I have the same calculus and the same signs for the inverse method of tangents as for the tetragonistic method. I remember speaking to you once about all these things, but you were not paying enough attention; and thus I am so far from saying that I think that you drew your method of quadratures from me, that, on the contrary, I rather reproached you for not paying sufficient attention to many things I showed you; for you always suspected my methods, by I know not what prejudice, of being only particular and too unnatural, so that after neglecting them you looked for other methods by yourself, and often after you had finally come on your own to those very methods which I had used, they then seemed universal and natural enough to you, and appeared clearly different from the ones I had proposed earlier, both because you expressed them in a different way than I did, and because the ways and procedures by which (as far as I am aware) you arrived at them, pleased you more than when they were shown by me, because I had not shown you (you were in fact paying little attention) my procedure and way of finding them. I admit that you gained from this: both because these things became your own, and because you thus trained yourself in the art of finding; however you might have been able to spare yourself the effort and time, and to practice the art of finding on other, untouched problems. For, if I may say in general what I think, if you had listened to me at that time, you should have spent most of the time you have spent on quadratures and roots of equations on other things. For you have not yet advanced beyond what could have been taken from our meetings in Paris. For in Paris I, too, referred all comparison to the rectangles ab, abc, etc., for the sake of finding roots; and, as far as quadratures are concerned, I prefer the method through differences, which I once showed you, to all others.

For every differential figure is squarable, and conversely, every squarable figure is differential. I call a figure differential whose series of ordinates coincides with the series of differences from another series; therefore we need only investigate whether a given figure is differential; however, we find this out by comparing the equation of the given figure with a general equation of a differential figure; for by means of this general differential equation all special equations that belong to squarable figures can be enumerated, so that a table of all squarable figures might easily be built. Nevertheless, I still lack two things in this method. First, it only exhibits those figures whose quadratrices are analytic (a quadratrix is a figure whose differential is the given figure, that is, one that is to the given figure as some series is to its differences), but not those figures whose quadratrices are transcendent. For example, it could not show that the quadratrix of the hyperbola is logarithmic. And so the area of the proposed figure cannot be found by this method since it cannot be expressed through an equation — I mean a common equation; but transcendent or (if you prefer to call them this) non-analytic quantities might be expressed through equations in other ways, so long as the equations are transcendent ones (in which the unknown enters the exponent). And so although this method shows that the circle and hyperbola do not have an analytic quadratrix, none the less it does not show what sort of quadratrix they do have; and this method cannot show whether perhaps the quadrature of some figure may be found, if not absolutely, then at least if we assume the quadrature of another figure, such as the circle or hyperbola. For example, it cannot show whether the curve of an ellipse can be found if we assume the quadrature of the circle or of the ellipse or of both, just as I have found the curve of an the equilateral hyperbola by assuming the quadrature of the hyperbola. But I have various arts by which it is possible to cure this defect. The *other* defect of this method is that although it may show that some figure is not analytically squarable by a universal quadrature common to all its portions, that is, that it does not have an analytic quadrature, nevertheless it does not show whether or not some portion may be squared in some special way. And so we do not know today whether it may be possible to find a special quadrature of some certain sector or segment of a circle, or even of the entire circle. I certainly have various ways by which I find special quadratures of this sort, but I have not found any that can be used to determine whether some proposed special quadrature, e.g., of the entire circle, is possible (through an ordinary, that is, non-transcendent, quantity) or indeed impossible. And so I would wish that you look into difficulties of this kind, which we do not yet have in our power. *I* do not yet see any other way of demonstrating such impossibilities of finding quadratures of special portions, than through solving transcendent equations, that is, those where the unknown enters the exponent. For if anyone can solve an equation like $x^y + \overline{ya}^x +$ etc. eq. 0 or change it into another, ordinary equation, or show the impossibility of changing it into an ordinary equation, he can also fully find all quadratures, or demonstrate their impossibility; for all quadratures may be expressed by transcendent equations of this kind. And so such a transcendent analytic remains to be completed, an analytic by which we shall have absolutely all things that we are now seeking in this area. I wanted to call attention to this, since you write that you have not needed these equations: I am not surprised at this, since no one else has examined their use or treated them analytically in sufficient generality. My method of solving quadratures through logarithms coincides with this method of reducing them to transcendent equations. Moreover, it is at any rate extremely general, since it is common to all curves, analytic as well as transcendent. Because I cannot describe it briefly enough, and this letter is already very lengthy, I save it for another time. You say that there are three methods of quadrature counted by you: one which Heurat, Barrow, and others use in various places, by means of tangents; another my method of changing one figure into another by means of calculation; and third, your method through different sections of the same solid. I, however, consider all three of these methods as parts of my general tetragonistic calculus; the transmutative method which I shared with you was only an example of this. For I also accomplish all the rest through a certain calculus that is always the same, so that I do not put much value on the very many theorems and methods given by Gregory and others: for they were among my first attempts; indeed, afterwards I learned how to obtain all such things by the calculus, and at the same time I came upon much greater things. And so not without reason do I wish that you would look instead into other more important things, which are not yet in our power, such as (1) a demonstration of the impossibility of a special quadrature of some portion, e.g., of an entire circle; (2) the inverse method of tangents, when the curve sought is transcendent; for when it is analytic I have the inverse method of tangents universally in my power; however, although in transcendents my tetragonistic calculus is very often satisfactory, nevertheless there remain some things which I have not examined well enough yet, and therefore I cannot yet declare that they are in my power. (3) The solution of equations in which the unknown enters the exponent, and consequently the finding of a root, either through a transcendent value (exhibiting letters or irrational numbers in the exponent) or through a common value (where only rational numbers enter the exponent), when this is possible. If this third thing is done, the two previous ones, namely, quadrature and the inverse method of tangents, will immediately be accomplished. (4) The solution of the problems of common geometry (namely, those problems which are referred to ordinary equations) through the briefest possible calculations and the most elegant linear constructions; pertaining to this are: the art of contracting the calculation from the beginning, so there is no need for depression afterwards; the art of making elegant linear constructions by using a calculation, or even better, the art of arriving at constructions without calculation, through an analysis not so much of magnitude (which pertains to calculation) as of location; the ancients seem to me to have had something of this which now has been lost and I have restored, and which perhaps might be advanced further. Although this work may be less grand sounding than the previous ones, nevertheless it is more suited to the common understanding, and it requires genius not less than any of the others; but I have already touched on something of this above. (5) The solution of Diophantine numerical problems; but here I am at a point where I may want to cross out this number. For I believe that some seven years ago I discovered the easiest and most general way of solving these problems and, when this cannot be done in rational numbers, of demonstrating the impossibility. At any rate, I might wish that you examine these four or five things. For you would no doubt uncover many very important things in them; but I would urge you to be more sparing of yourself in other things which are already in our power, unless perhaps you will discover elegant advances and beautiful theorems: for even when we have something in our power, we nevertheless do not therefore dig up and pay attention to the beautiful theorems hiding in it. And so, although it is obvious that we have in our power the common enumeration of all analytical curves, because unless the calculation needed for this is handled with art it is rather extensive, you will give us something beautiful and difficult if you uncover for us the true infinite progression of curves of this sort. For it often happens that different equations nevertheless belong to the same kind of curve; e.g., xy eq. a^2 and x^2y^2 eq. b^2 are both equations for a hyperbola; and to be able to uncover this in higher equations by a constant and easy method would be very important. I would also like to know how you are sure, as you say, that if you can determine only thirty quadratures you can show all the others.

Furthermore, as I go over the rest of your letter I notice in passing that you write: *there are many who falsely believe that the combinatoric art is a science so separated from and prior to algebra, that it must be learned in addition to the other sciences; indeed, there are those who believe that the combinatoric art contains more things in itself than the art that is commonly called algebra, that is, that the daughter knows more than the mother, for in truth, if by nothing else, then by the composition of powers alone it is obvious that the combinatoric art is learned from algebra.* Such are your words, which are no doubt directed at me. For the many who, so you say, think thus, are, I believe, few besides be. However I think you are right, because you do not seem to have understood me: for if you consider combinatorics to be the science of finding numbers of variations, I will willingly admit to you that it is subordinate to the science of numbers (and consequently to algebra, since the science of numbers is subordinate to algebra), for at any rate you will not find those numbers except by adding, multiplying, etc. And the art of multiplying is descended from the general science of quantity, which some call algebra. But the *combinatoric art* is something very different for me, namely: the science of *forms* or of the *similar* and *dissimilar*, just as algebra is the science of magnitude or of the equal and the unequal. Indeed, combinatorics seems to differ little from the general characteristic science, by means of which fitting characters for algebra, music, and even logic have been thought up or can be thought up. Cryptography is also a part of this science, although the difficulty in it is not so much in putting things together as in taking apart what has been put together and, so to speak, finding roots. For a root in algebra is like a key in divinatory cryptography. Algebra by itself has rules for equalities and proportions, but when problems are more difficult and the roots of equations are very involved, it is forced to borrow some things from the higher science of the similar and the dissimilar, that is, from combinatorics. For the artifice of comparing similar equations (or equations of the same form) was already known to Cardano and others, and it was very distinctly described by Viète; it is properly sought in the combinatoric art, and it can and should be applied not only when it is a matter of formulas expressing magnitudes and solving equations, but also when we have to unfold a complicated key for other formulas having nothing to do with magnitude. The art of looking for progressions and of building tables of formulas is also purely combinatoric, and it has a role to play not only for formulas expressing magnitude but also for all others. It is also certainly possible to find formulas expressing location and the drawing of lines and angles, without considering magnitudes, and by this means more elegant constructions will be found, or at least more easily found, than by a calculation of magnitudes. That the sides of triangles having the same angles are proportional can far more naturally be demonstrated by means of combinatoric theorems (or theorems of the similar and the dissimilar) than in the way Euclid did it. I admit that sometimes there are no more beautiful examples of the combinatoric art or the general characteristic than those given in algebra, and consequently whoever knows algebra will more easily establish general combinatorics, for it is always easier to arrive at general sciences *a posteriori* from special examples than *a priori*. But it should not be doubted that combinatorics or the general characteristic contains far greater things than those algebra has given; for by means of it all our thoughts can be, as it were, painted and fixed, and contracted and put in order: *painted*, so that they may be taught to others; *fixed* for us so that we may not forget them; *contracted* so that they may be expressed in few words, *put in order* so that they may all be held in view by those who contemplate them. Moreover, although I know that you, prevented by I know not what cause, have been rather a stranger to these contemplations of mine, I nonetheless believe that when you have examined the matter seriously you will agree with me that this characteristic is going to be unbelievably useful, since by means of it a language or writing may be thought up which could be learned in a few days and would suffice to express everything that occurs in common use, and would be wonderfully valuable for judging and finding, following the example of numerical characters. For at any rate, we calculate much more easily with arithmetic characters than with Roman ones, whether we use a pen or our mind — no doubt because Arabic characters are more fitting, that is, they better express the genesis of numbers. Moreover, no one should fear that the contemplation of characters may lead us away from things; on the contrary, it will lead to the innermost parts of things. For today we often have confused notions on account of badly ordered characters, but then we shall easily have very distinct notions by means of characters; in fact a mechanical (so to speak) thread of thinking will be at hand, by means of which any idea can very easily be resolved into the the others from which it is put together, or rather, when the character of any concept is attentively considered the simpler concepts into which it is resolved will occur at once to the mind; from which it follows that because the resolution of a concept corresponds exactly to the resolution of a character, when the characters are only looked at by us, adequate notions will on their own and without any effort press on our minds; no greater help than this can be expected for the perfection of our mind. I wanted to write these things down a little more fully, my friend, so that I might test whether reasons are stronger for you than prejudiced opinions; if you say the matter is splendid but difficult, I will have obtained enough from you. For difficulties do not frighten me, since I see quite certain, and, if I am not mistaken, very convenient ways of overcoming them.

You will not be unaware that Spinoza's *posthumous works* have come out. There is in them a fragment on the emendation of the intellect, but at the point where I was most hoping for something, it ends. In the *Ethics* he does not sufficiently explain his opinions everywhere, which I sufficiently censure thus. Sometimes he commits paralogisms, and this comes from the fact that he departs from rigorous demonstration. I certainly think it is useful to depart from rigor in geometric demonstrations, because errors are easily avoided in them, but I think that the highest rigor of demonstration should be followed in metaphysics and ethics, because slipping up is easy in them; nevertheless, if we were to have an established characteristic, we would reason equally safely in metaphysics as in mathematics. You say that it is hard to give definitions of things; perhaps you mean the simplest possible, and so to speak, original concepts, which I admit are hard to give. However, you should note that for one thing there are many definitions, that is, reciprocal properties distinguishing the thing from all others, and that from each of them we can bring out all the other properties of the thing, as you are also not unaware; but some of these definitions are more perfect than others, that is, more suited to first and adequate notions. And indeed I consider it a certain sign of a perfect definition that *if the definition is once understood, it is no longer possible to doubt whether the thing comprehended by the definition is possible or not.* Besides, whoever wishes to establish the universal characteristic (or analytic), can use any definitions at all at the beginning, since by continued resolution they always end up in the same place. When you say that in very complicated things calculation is needed you clearly agree with me: it is the same as if you had said that characters were needed, since *calculation* is nothing other than an operation through characters, which has a place not only in quantities but also in every other reasoning. When it is possible I greatly value those things that can be done without an extended calculation, that is without paper and with the mind's power for a pen, because they depend least on external things, and are also in the power of a prisoner to whom a pen is denied or whose hands are bound. And so we should train ourselves both in calculating and in thinking, and those things we have found by calculation we should also try afterwards to demonstrate by thought alone, which is something I have often succeeded at.

But I do not doubt that we have the same opinions about many things, although we may differ in the way we follow them; I would not want this to be a cause of disagreement between us, inasmuch as I would not want disagreement to diminish our friendship. I therefore hope that my sincerity in setting forth my opinion on your extraction of roots from equations will not be unwelcome, since indeed I thought that you were off the mark and wanted to indicate this to you so that you could save yourself some trouble. I look forward in turn to your judgment about my opinion, a judgment to which I attribute much, and I do not doubt that I am benefited and have learned many things from you and still can, and that you are capable of outstanding discoveries, and that those that have already been exhibited by others as well as by me could also be shown by you if you should turn your mind to them. Nevertheless, I would prefer that for the sake of the public good you apply your mind instead to untouched things and things we do not yet have in our power; I also hope that some of the prejudices you seem to have against certain opinions of mine will be wiped out more and more. For the rest, farewell and let me know both the state of your health and the progress of your extraordinary studies.

Your Most Devoted,

Gottfried Wilhelm Leibniz

A Note on the First Part of Cor. I, Prop. XIII

The argument as a whole is a kind of semi-reductio-- that is, he first constructs a conic With a focus at S, where the assumed center of force is, a velocity at P equal and in the same direction as the assumed velocity at P, and produced by an accelerative force exactly equal in magnitude to that assumed to be acting on the body at P.

How does he know how to do this? Well, first, he can determine what curvature the conic must have at P: for, knowing the force and the velocity in absolute terms, he can use the expression

$$PV = \frac{2v^2}{f} \qquad \text{(PV is the chord of curvature).}$$

As he says, "the curvature is given from the centripetal force and the velocity of the body being given."

The above relation can be derived as follows. According to calculus, if the absolute value of a force is expressed in distance-units over time-units squared, i.e. if it is measured by the second derivative of the displacement with respect to time, then for a constant force, $s=\frac{1}{2}ft^2$, where f is that measure. Therefore, ultimately for any force

$$f = \frac{2s}{t^2} \qquad \text{by Lemma X.}$$

I must now revert to Newton's familiar point-labelling. In the ultimate case

$$s = QR \qquad \text{and } t = \frac{QP}{v}$$

substituting,

$$f = \frac{2QR}{QP^2}\, v^2$$

but as mentioned in Cor. III, Prop. VI and proved in the Bart Manual, p. 40,

$$\frac{QP^2}{QR} = PV$$

, and this is equality, not mere proportionality. So, subsituting,

$$f = \frac{2v^2}{PV} \qquad \text{and} \qquad PV = \frac{2v^2}{f}$$

OK, so now he knows what curvature he needs at P, and he says, "the focus, the point of contact, and the position of the tangent, being given, a conic section may be described, which at that point shall have a given curvature." How does he know how to do this? Well, it is easy to reduce this construction problem to one in which the latus rectum is given instead of the curvature; and since Newton is about to show us, in Prop. XVII, how to construct a conic given a focus, point, tangent and latus rectum, once we perfom this reduction we're in business.

So the question is, how to determine the pricipal latus rectum when we know the chord of curvature at a point and also the position of the tangent. Let us again revert to Newton's point labels.

$QT = PQ\sin\S$, where § is the angle between the tangent and PS.

but $\frac{PQ^2}{QR} = PV$;, so $\frac{QT^2}{QR} = \frac{PQ^2\sin^2\S}{QR} = PV\sin^2\S$

but as Newton proves and states as Cor. II, Prop. XIII,

$$\frac{QT^2}{QR} = L$$, the principal latus rectum.

Therefore $$L = PV\sin^2\S$$;

and since both PV and § are known, L is now known, and we can flip to XVII for how to complete the construction.

An Expansion of Newton's Argument in the Last Sentence of Cor. I, Prop. XIII

Let us assume that we can construct the conic which Newton calls for. Applying either Prop. XI, XII, or XIII to it, it follows that this conic would be the orbit produced by an inverse-square force from S; and we have, furthermore, managed to construct this conic so that both the force and the velocity at P are identical to the ones assumed as given.

At this point Newton says, "Two orbits, touching one the other [i.e., having a common tangent at P] cannot be described by the same centripetal force and the same velocity."

He seems to have assumed that his readers would find this claim fairly well self-evident; but it is possible to further belabor the point, as follows.

In the very first instant of the motion, the bodies move the same distances along the common tangent, since their velocities are assumed equal. Furthermore, the forces are assumed equal and in the same direction, so that in the first instant the deviations <u>from</u> the tangents caused by the forces are also equal, by Cor. III, Lemma X, and in the same direction, by Law II. Thus in the first instant the bodies are tending towards the same point. Furthermore, in this first instant the changes in the <u>velocities</u> are also equal and in the same direction, also by Law II. Thus not only do the bodies arrive at the same point, they also arrive with equal velocities in the same direction. Finally, both bodies are assumed to be under an inverse-square force law and to have started under equal forces, and have just been shown to arrive at equal distances from S: therefore the <u>forces</u> will be equal here, too, since each will have the same ratio to an equal force.

Now the same reasoning applies from this point, since the bodies have retained equal forces and velocities, a common tangent, and equal distances from the center. This will be true of every point: the bodies never have any tendency to deviate, and thus will never deviate, from each other's paths. Therefore there cannot be two orbits here, but only one.

CW

Can Newton Prove that the Inverse-Square Law implies a Conic-Section Orbit?

From Cor.2 of Prop.13 we know that

$$L = \lim_{Q \to P} (QT^2/QR) \qquad (1)$$

The accelerative centripetal force f is given in two ways:

$$f = \lim_{Q \to P} (QR/t^2) = k/SP^2, \qquad (2)$$

where t is time and k is a constant. We can eliminate QR from (1) if we rewrite it in such a way that we can substitute for QR/t^2 under the limit sign:

$$L = \lim_{Q \to P} \left(\frac{QT^2/t^2}{QR/t^2} \right) = \lim_{Q \to P} \left(\frac{(QT.SP)^2}{k.t^2} \right) \qquad (3)$$

But we have seen that $QT.SP = PR.SY$, $(PR \times SY)$ and $\lim_{Q \to P}(PR/t) = v$, the velocity. Making these substitutions in (3), we shall have

$$L = \lim_{Q \to P} (v.SY)^2/k = (v.SY)^2/k \qquad (4)$$

Thus, for a body moving in the field of an inverse-square centripetal force, from the initial velocity, the perpendicular distance from the center of force onto the tangent, and the constant k, we may determine the latus rectum.

In Prop.17, Newton instead of introducing the constant k supposes a conic-section orbit already known; its sole function in the proof is to permit determination of the latus rectum of the new orbit. Given the latus rectum as well as the angle that the tangent PR makes with SP, Newton then shows how to construct a conic section in which a body might move so as to have the given velocity at the place P. If we agree that with given initial conditions there can be only one orbit, then the only possible orbit is the one so found.

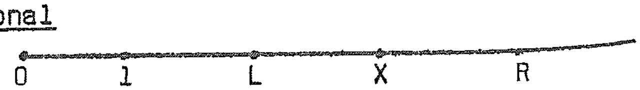
FROM EUCLID TO DEDEKIND

Samuel Kutler

Dedekind "creates" the system of real numbers by postulating that every cut (A_1, A_2) of rational numbers such that any number in A_1 is less than any number in A_2 is effected by some number, be it one of the given rationals or a new irrational. In this property of the real number system Dedekind finds the "essence of continuity."[1] Dedekind assumes that "every one will at once grant the truth of this statement,"[1a] when the expression is interpreted geometrically. His object is to place the differential calculus on a perfectly rigorous foundation by giving a purely arithmetic development of the real number system.

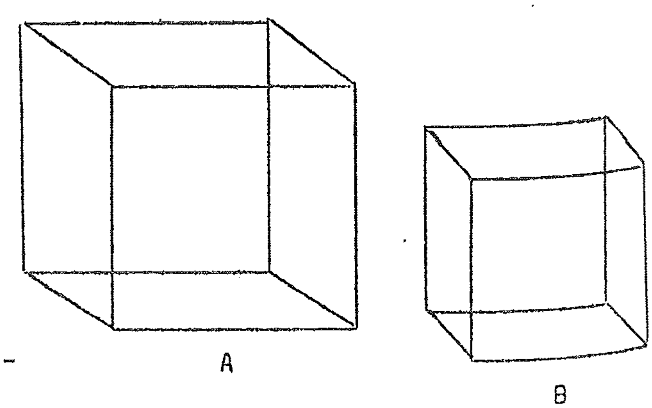
It is often said that the Dedekind cut is prefigured by Eudoxus' definition of the same ratio given in Euclid's Elements as the fifth definition of Book V. Dedekind himself calls "this manner of determining the irrational number as the ratio of measurable quantities . . . the source of my theory."[2] What follows is an attempt to present the manner in which this definition is the source of Dedekind's theory.

Let 01 be any straight line whatsoever. The point P on the line 01 or on 01 extended is defined to be a rational point (with respect to 01) whenever OP and 01 are commensurable or an irrational point whenever they are incommensurable.



Let A and B be incommensurable magnitudes. Let X be the irrational point on the line 01 such that

A:B :: OX : 01



Then, geometrically, Dedekind determines the irrational point X by the

1) Essays on the Theory of Numbers, R. Dedekind (trans. by W. W. Beman) Open Court 1948, page 11.
1a) Ibid, page 11
2) Ibid, pages 39-40

separation of the line into two classes such that every point of the first class lies to the left, (of X and hence) of every point of the second class.

We now seek the arithmetic determination by means of Euclidean definitions:

By Euclid V, Def. 4, there must be numbers n, m, s, and r for which

$nA > mB$ and $sA < rB$.

Then from Euclid V, Def. 5, it follows that

n.OX $n \cdot \theta X > m \cdot 01$ and $s \cdot 0X < r \cdot 01$.

From which it follows by Euclid V, Def. 7 that

$m:n < A:B$ and $r:s > A:B$.

$m:n < 0X:01$ and $r:s > 0X:01$.

On the line 01 let L and R be points such that

$m:n::0L:01$ and $r:s::0R:01$.

Then $L < X < R$.

Clearly just as the ratios m:n and r:s correspond to the rational points L and R, so given any rational point P on 01, there will correspond some ratio say XL p:q where

pA greater than qB whenever P is to the left of X and

pA less than qB whenever P is to the right of X.

Thus, just as in the geometric definition of the irrational number X as the separation of all the rational points into two classes such that every rational in the first class - such as L - lies to the left of every rational in the second class - such as R - we have effected a separation of all <u>ratios</u> into two classes such that given any ratio in the first class - such as m:n - and any ratio in the second class - such as r:s -

$m:n < r:s$.

We are now free to abstract from (forget) the geometric representation and to simply define the positive irrational number X as this separation or cut. Finally, if we descend to the correspondence between a ratio p:q and a "rational number" $\frac{p}{q}$, we have created the irrational number X as a cut in the rational numbers. Moreover, at the root of this analysis is the "celebrated definition which Euclid gives of the equality of two ratios."[3]

3) Ibid, pages 39-40

FROM EUCLID TO DEDEKIND - SEQUEL

Eva Brann

In the last COLLEGIAN (June, 1965) Mr. Kutler gave a precise Euclidean construction which might warrant the assertion, made by Dedekind himself and often repeated,[1] that Definition 5 of Book V of the Elements, the definition of "same ration," is the "source" of the theory of irrational numbers in terms of cuts. In working over Mr. Kutler's presentation in the Senior mathematics tutorial, the question naturally arose whether or not Euclid himself might have agreed that the definitions of Book V could be used in this way. Dark suspicions voiced in class led me to try to pin-point the junctures where the difficulties arise. I came to the conclusion that only an abrupt re-interpretation and a concentration on purely incidental results could lead to Dedekind's assertion.

The following notions, introduced in the Definitions of Book V, are relevant:

a. Euclid tacitly expands the realm of magnitudes formerly thought comparable, that is, of magnitudes thought capable of having a relation or ratio to each other, from those which are commensurable and have to each other the ratio of a number to a number to include all magnitudes which are merely Eudoxan, namely those which can be made to exceed each other by multiplication (Def. 4).

b. The basis of the comparability of the ratios themselves is then laid by defining "same ratio" and thus proportionality, for these Eudoxan, i.e., general, magnitudes (Def. 5).

c. The definition of ratios other than same, i.e., of greater or less ratios, then completes the assignment of meaning to an ordering of ratios.

It is this last item which actually suggests a parallelism with Dedekind cuts. By Definition 4 we can find numbers n and m such that, given the incommensurable general magnitudes A and B, either $nA>mB$ or

1) Dedekind, Theory of Numbers, p. 40; see, for example, Heath, Elements II, p. 124; Weyl, Philosophy of Mathematics and Natural Science, p. 39.

$nA < mB$; Definition 7 then allows us to say that in the first case $m:n < A:B$ and in the second case $m:n > A:B$[2].

Now since A:B is a ratio of incommensurables, the case $m:n = A:B$ can never arise. Hence the ratio A:B seems to separate all the number ratios m:n into those less and those greater than itself - it seems to be a cut-producing irrational ratio. However, there is a difficulty about interpreting A:B so immediately as producing a Dedekind cut. This is the fact that the number ratios separated by A:B cannot really be made to correspond to the rational numbers. For the numbers used in Definition 7 are chosen one by one, as test multiples. Even if it were possible to interpret number ratios as fractions within a Euclidean context (which it is well known not to be), we would still not have the "number body" of rationals but only isolated members.[3] This collection of numbers would not be dense and would fail to have the characteristic required of cuts, namely that at least the lower segment should have no greatest or the upper segment no least member.

Furthermore the ratio of incommensurables A:B cannot in any way be understood as ordered among the number ratios i.e., as an irrational ratio among rational ratios. For while an axiom according to which "a magnitude which can be made both greater and less than an assigned magnitude can also be made equal, provided the magnitudes are homogeneous," was current in Euclid's time, there is no evidence at all that the converse was acceptable and that two magnitudes having such a relation may be concluded to be of like kind.[4] There is, therefore, no warrant for making an irrational number correspond to the ratio of incommensurables. In fact Book V is carefully framed in terms of general magnitudes[5] and the comparison of number ratios with ratios of general magnitudes is avoided until Book X, where incommensurables are, of course, excluded.

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2) For, taking the first case as an example, if m and n be the multiplying numbers, for $nA > mB$, $nm = mn$, which fulfills the definition of $A:B > m:n$ or $m:n < A:B$.

3) Mr. Kutler reminds me that this understanding of the multiples, while not disturbing for Def. 7 where only one pair of multiples meeting the given conditions is required, raises a great problem about the meaning of Def. 5. Everyone will remember the discussion in the Freshman year concerning the practical use of that definition, i.e., how any assertion can ever be made about "any equimultiples whatever."

from such proportions (Props. 5-7). Hence the number multiples used in Book V must be regarded as purely auxilliary testing numbers and should not be used in proportions at all.

It might be argued, however, that there is a way of reflecting the fact that all number ratios are either greater or less than A:B in a homogeneous medium which is obviously continuous - the straight line. Mr. Kutler showed how, having chosen a unit segment, one may, with the aid of Definition 5, find unique points on a line to represent A:B and every m:n, such that all the points standing for m:n less than A:B fall to the left and all those standing for m:n greater than A:B fall to the right of the point representing A:B. This is a perfectly orthodox Euclidean procedure, since Euclid himself names the segments which yield these points rational and irrational respectively (Bk. X, Def. 3). Furthermore one might argue that Euclid himself shows that as many rational points as you please lie between any chosen rational point and the given irrational point (Bk. X, Prop. 2). Is this, then, the legitimate way to make the transition?

Here it becomes necessary to have a look at the sequence of Dedekind's argument. Ostensibly he begins by analyzing the naturally given continuity of the straight line so that after finding its essence he may create a number system which shall possess the same property (p. 9). In fact, however, he begins not with the classical one-dimensional continuum but with a line already re-interpreted to be the analogue of a number system, namely with a line which is a collection of point individuals. Now for the classical understanding the notion of a collection of individuals and the notion of a continuum are mutually exclusive (Aristotle, <u>Physics</u> V, iii; VI, i). The continuity of the line is something underlying its points, a pre-mathematical quality the discussion of which has no place in the <u>Elements</u>[6)]. The way points lie on

4) See O. Becker, "Eudoxos-Studien III", <u>Quellen und Studien zur Geschichte der Mathematik</u>, etc., III, 1934, p. 244.

5) Dedekind, on the other hand, is quite ambiguous about the kind of magnitude which is the result of his construction, so that one may understand the reals to be the cuts themselves or the number producing them; the proof of the continuity of the real domain (p.20) bears either interpretation.

6) From the classical point to view the emphasis is far more on the fact that a line is everywhere divisible than on the fact that a point is hit whenever this is done (Heath, <u>Elements</u> I, p. 156).

the line could, therefore, never constitute the "essence" of continuity for Euclid, and the above construction would fail to have a meaning. But then, the whole enterprise of creating a continuous number system would have seemed self-contradictory to the ancients, for whom number is the discrete magnitude _par excellence_ (Bk. VII, Def. 2; Aristotle, _Metaphysics_ XII, x, 12).

Writing assignments indicated in recent archon reports

1 From the 2014 report

We all assigned calculus problems from among those in the manual, usually just the odd numbers, to be turned in by students. We would then go over in class any that presented difficulties.

Most of us assigned one paper (4–7 pages) each semester, for the most part allowing students to choose their topics. Jeff Smith assigned two "short-answer" papers for semester, for a total of 8 pages per semester. Louis Petrich gave students alternative due dates to better accommodate their other assignments. In the second semester, Steve Crockett encouraged them to write about a difficult proposition, a proposition the class hadn't done, or Query 31. In the first semester, I asked students to trace one question from Euclid through Descartes to Leibniz.

Some of us felt in retrospect that more focused paper topics and/or additional shorter assignments might have been valuable. Given the importance of Prop. VI, Cor. I and the confusions about it which emerged in my students' Newton papers, I might in future ask for a straightforward exposition of its proof.

2 From the 2015 report

Most of us assigned at least two papers in each semester. In the first semester I assigned one paper on Leibniz's calculus and another on Dedekind. In the second I assigned a paper on the difference between Leibniz's and Newton's approaches to the calculus and a paper on a topic of choice in the Principia. Some tutors used suggested questions written by me or by Louis Petrich. One of the most fruitful Newton topics several students took up in my class was the curious "solid" that Newton introduces in the first corollary of proposition 6 and that appears throughout the search for force laws.

3 From the 2016 report

3.1 First Semester

All tutors so far as I know assigned all the problem sets in the manual as written assignments. One tutor also had an oral for each student, on a topic of the student's choice, and otherwise required daily writing of a paragraph either explicating or questioning each reading. One tutor gave students four opportunities (one with Galileo, two with Leibniz, one with Dedekind; all with suggested questions) to write two papers, 4–6 pages in length. The remaining tutors assigned either one paper or two, of similar length. One tutor assigned a Dedekind paper on the question "Why does mathematics, a human invention, at least in Dedekind's conception of it, have properties?" Another tutor assigned a Leibniz paper on Snell's Law in "New Method" requiring an account of the mathematics and then a reflection on Leibniz's claims about final causality. Other tutors assigned their papers on Leibniz, giving students their choice of topic, while one tutor suggested questions such as: "What is dx?"; "What does Leibniz understand by a sum ($\int$)?"; "What is 'recondite geometry'?"; "How is Leibniz able to claim that to find a tangent 'is to draw a straight line joining two points on a curve'?"

3.2 Second Semester

All tutors so far as I know required demonstrations at the board from volunteers for at least some Newton propositions. (It is doubtful that one could make it through the schedule by requiring such demonstrations for all propositions.) And all the other writing assigned was presumably on *Principia*, the second semester text. The tutor who assigned an oral and daily writing first semester required a 10 page paper near the end of second semester, preceded by several written updates on progress. Another tutor continued his practice of giving students four opportunities to write two essays, each 5 pages in length. Other tutors also assigned two papers of similar length, with one tutor dividing the papers between the initial lemmas and propositions 1–17 in Book I. So far as I know, all of these papers were on topics of the student's choice.

4 From the 2017 report

Most tutors did some mix of shorter papers focused on the details of an individual proposition or other technical issue and longer papers reflecting on the arc of some sequence or the larger issues at stake in an author's project, generally amounting to no less than the equivalent of two 4-5 page papers per semester.

5 From the 2021 report

First, what I assigned each semester: Fall—an initial short paper on what Leibniz means by the letter "d," followed by a longer paper, due end of term, on a topic and reading of the student's choice. (Most picked Leibniz; a few wrote on Dedekind; none on Galileo.) Spring—a longer paper on Newton, due end of term. (Most wrote on Book III.)

Next, emailed reports from three tutors about their assignments:

> In my tutorial, the written work for the first semester consisted of homework assignments (the problems in the manual) and one major paper on Leibniz. The written work for the second semester consisted of a very short paper on Newton's lemmas and a major paper on some one or more of Propositions 1–17 of Book I.

> On writing assignments: I assigned a paper each semester.
>
> First semester I assigned a paper on "New Method". Students were to come up with their own question, though I supplied some prompts. I also encouraged them to see whether they could draw any connections between their Leibniz seminar readings and the "technical" approaches of "New Method". I think this worked pretty well; I really did stress to them that I didn't just want "philosophy" papers, but they should dive into the details of the math. First semester I also had students regularly hand in homework—some of the problems in the manual.
>
> Second semester I assigned a paper on the Lemmas. I encouraged students to craft a question based on their reading of the Scholium to the Lemmas, and then explore it by discussing one or two Lemmas in detail. One advantage this had is it meant specific students became "experts" on certain Lemmas, and they were helpful once we got into the propositions.

> As for writing assignments, I'll just report that I assigned one 5 page paper per semester: a Leibniz paper towards the end of our time with him (the timing of which worked well, I thought), and a Newton paper after we finished the Lemmas (this worked well in itself, but I felt something of a lack for not having a paper on the remainder of the *Principia*, although to be honest that was strategically intended).

Note for page 33

This note tries to clarify some things Leibniz says in the paragraph on page 33 of the Junior Math manual, where he criticizes "previously published methods."

Let us take Figure 9 on that page as posing a tangent problem for a curve whose equation includes irrationals.

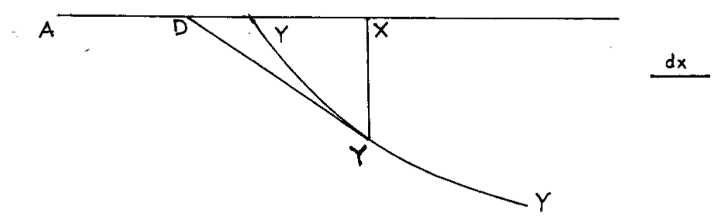

Figure 9: our figure, not Leibniz's

For example, suppose the curve's equation is $y = \sqrt{x}$. For any x, this equation will give us a value for y that determines one point of the tangent (at point Y on the curve). The tangent's other point D can be found if we can determine the length of DX—say, by an equation for DX in terms of x.

Leibniz's approach to the problem is straightforward, given how he defines dy at the beginning of "A New Method." The ratio of dy to any arbitrary line dx, he says, will be the same as the ratio of XY to DX. The way he puts it in this paragraph is that dy is "a fourth proportional for DX." That is,

$$DX : XY :: dx : dy. \tag{1}$$

The problem at this point is all but solved, since we know all the terms in the proportion save DX. $XY = y = \sqrt{x}$; dx is of arbitrary length; and Leibniz gives us a rule to determine dy. Using either the root rule or the power rule extended:

$$dy = d(\sqrt{x}) = \frac{1}{2\sqrt{x}}\,dx.$$

Leibniz's proportion in (1) thus becomes

$$DX : \sqrt{x} :: dx : \frac{1}{2\sqrt{x}}\,dx. \tag{2}$$

The rest is a matter of algebra. We equate extremes and means to get the equation

$$DX \cdot \frac{1}{2\sqrt{x}}\,dx = \sqrt{x}\,dx$$

and solve for DX:

$$DX = \frac{\sqrt{x}\,dx}{\frac{1}{2\sqrt{x}}\,dx} = 2x. \tag{3}$$

Could we find equation (3), however, without Leibniz's calculus? That is, could we find (3) without dy? One approach is suggested earlier in the manual, under Figure 17 (page 44).

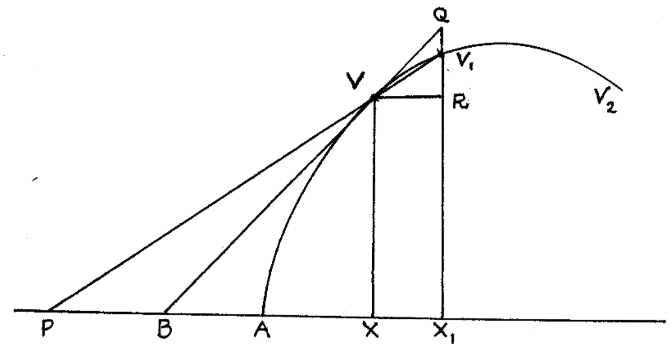

Figure 17

Using what is shown here with the letters from Figure 9, we could extend DX to DX_1, with a corresponding shift from point Y to point Y_1 on the curve $y = \sqrt{x}$. If we make the extension small enough (let it be e, using Fermat's letter for it), then we would approximate proportion (2) in terms like so:

$$DX : \sqrt{x} :: DX + e : \sqrt{x+e}. \tag{4}$$

This proportion already shows one thing Leibniz mentions about "previously published methods": it makes no use of dy by relying on the straight line DX (in this case for the quantity e). We can also already see something of how not using dy "confuses everything," according to Leibniz. For the way Leibniz defines dy allows the proportion in (2) to be exact; yet it can never be exact once we rely on DX to recast the proportion in (4). Since $\sqrt{x}$ and $\sqrt{x+e}$ are values for ordinates under a curve, the ratio of DX to $\sqrt{x}$ will never be the same as that of $DX + e$ to $\sqrt{x+e}$, no matter how small e is.

If we forge ahead, we can find other ways everything gets confused for Leibniz in such an approach. First, we equate extremes and means in (4) to get the equation:

$$DX \cdot \sqrt{x+e} = \sqrt{x} \cdot DX + e.$$

But having dispensed with dy, we no longer have a "root rule" to deal with $\sqrt{x}$ or $\sqrt{x+e}$. Instead, as Leibniz points out, it seems "we must eliminate fractions and irrationals"—in this case by squaring both sides of the equation to rid it of any root signs, like so:

$$(DX)^2 \cdot x + e = x \cdot (DX + e)^2.$$

But this move forces us to multiply out, like so:

$$(DX)^2 x + (DX)^2 e = (DX)^2 x + 2DXex + e^2 x.$$

We can do one step of simplification immediately, cancelling $(DX)^2x$ to get

$$(DX)^2e = 2DXex + e^2x.$$

But now we confront the problem of what to do with e. It looks like we could just divide it through, to get

$$(DX)^2 = 2DXx + ex. \tag{5}$$

But this leaves us with an e. The only way to get rid of it, it seems, is to treat it as *so* small it is like nothing, even though we treated it like something to divide it through in (5). Still, this questionable move will get us to (3). For in that case, where $e = 0$ as it were, $ex = 0$ too, so we can drop that term from (5) like so,

$$(DX)^2 = 2DXx.$$

And we can reduce this to

$$DX = 2x.$$

The treatment here of e might be considered a final confusion for Leibniz. The closest term to it in his proportions (1) and (2) is dx. Yet not only is dx defined for those proportions as arbitrary and finite; it also appears twice in (3) only to cancel out, unproblematically. (Perhaps this clarifies what footnote 7 on page 33 means when it says that "Leibniz has a general method that can always find a finite equation that does not involve dx.")

Having said all that against our approach, there is one thing to recommend it. It is easier to know what we mean by $\sqrt{x+e}$ than what we might mean by $\frac{1}{2\sqrt{x}}\,dx$. Both have their origin in $\sqrt{x}$, but the rule Leibniz gives us to get $\frac{1}{2\sqrt{x}}\,dx$ obscures its relation to $\sqrt{x}$. We might look at the problem this way. For a given x, $\sqrt{x}$ will give us a point on the curve, and $\sqrt{x+e}$ will give us another point on the curve, at a distance e away from the first, which we can make as small as we like. What then does $\frac{1}{2\sqrt{x}}\,dx$ give us? Certainly not, in any straightfoward sense, another point on the curve. (If we graph $\frac{1}{2\sqrt{x}}$, we get a completely different, and different-looking, curve.) The usual interpretation—Leibniz alludes to it later in the paragraph in question (p. 34)—throws things into greater confusion, since it admits not simply infinitesimals into the picture, but infinitesimals in finite ratios of different sizes. At point $(1,1)$ on the curve, for example, we are invited to take $dy = \frac{1}{2}dx$ as referring to a "characteristic triangle" at that point, with infinitely small legs dx and dy, yet where dy is half the length of dx. Does this make dy twice as small? Infinitely infinitely small? Or what? Nonetheless, in this strange ratio—so we are assured—the hypotenuse of the triangle will connect infinitely close points $(1,1)$ and $(1+dx, 1+dy)$ on the curve, and form the tangent when extended—as if a tangent could touch the curve at two points rather than one (which is indeed what Leibniz says on page 34). Against all this, the point $(1+e, \sqrt{1+e})$ on the curve, as close to $(1,1)$ as we please given e, is a model of clarity, despite making the procedure for finding the tangent more confusing.

This last point suggests a final question: what explains how clear the procedure becomes with Leibniz's more baffling dy?

(1) ON A METHOD FOR THE EVALUATION OF MAXIMA AND MINIMA[1]

The whole theory of evaluation of maxima and minima presupposes two unknown quantities and the following rule:

Let a be any unknown of the problem (which is in one, two, or three dimensions, depending on the formulation of the problem). Let us indicate the maximum or minimum by a in terms which could be of any degree. We shall now replace the original unknown a by $a + e$ and we shall express thus the maximum or minimum quantity in terms of a and e involving any degree. We shall adequate [*adégaler*], to use Diophantus' term,[2] the two expressions of the maximum or minimum quantity and we shall take out their common terms. Now it turns out that both sides will contain terms in e or its powers. We shall divide all terms by e, or by a higher power of e, so that e will be completely removed from at least one of the terms. We suppress then all the terms in which e or one of its powers will still appear, and we shall equate the others; or, if one of the expressions vanishes, we shall equate, which is the same thing, the positive and negative terms. The solution of this last equation will yield the value of a, which will lead to the maximum or minimum, by using again the original expression.

Here is an example:

To divide the segment AC [Fig. 1] *at E so that AE × EC may be a maximum.*

Fig. 1


We write $AC = b$; let a be one of the segments, so that the other will be $b - a$, and the product, the maximum of which is to be found, will be $ba - a^2$. Let now $a + e$ be the first segment of b; the second will be $b - a - e$, and the product of the segments, $ba - a^2 + be - 2ae - e^2$; this must be adequated with the preceding: $ba - a^2$. Suppressing common terms: $be \sim 2ae + e^2$. Suppressing e: $b = 2a$.[3] To solve the problem we must consequently take the half of b.

We can hardly expect a more general method.

ON THE TANGENTS OF CURVES

We use the preceding method in order to find the tangent at a given point of a curve.

Let us consider, for example, the parabola BDN [Fig. 2] with vertex D and of diameter DC; let B be a point on it at which the line BE is to be drawn tangent to the parabola and intersecting the diameter at E.

[1] This paper was sent by Fermat to Father Marin Mersenne, who forwarded it to Descartes. Descartes received it in January 1638. It became the subject of a polemic discussion between him and Fermat (*Oeuvres*, I, 133). On Mersenne, see Selection I.6, note 1.

[2] See Selection IV.7, note 5.

[3] Our notation is modern. For instance, where we have written (following the French translation in *Oeuvres*, III,122) $be \sim 2ae + e^2$, Fermat wrote: B in E adaequabitur A in E bis + Eq (Eq standing for E quadratum). The symbol $\sim$ is used for "adequates."

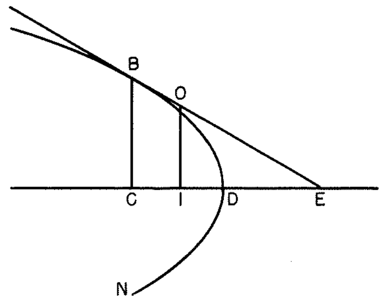

Fig. 2

We choose on the segment BE a point O at which we draw the ordinate OI; also we construct the ordinate BC of the point B. We have then: $CD/DI > BC^2/OI^2$, since the point O is exterior to the parabola. But $BC^2/OI^2 = CE^2/IE^2$, in view of the similarity of triangles. Hence $CD/DI > CE^2/IE^2$.

Now the point B is given, consequently the ordinate BC, consequently the point C, hence also CD. Let $CD = d$ be this given quantity. Put $CE = a$ and $CI = e$; we obtain

$$\frac{d}{d-e} > \frac{a^2}{a^2 + e^2 - 2ae}.$$ [4]

Removing the fractions:

$$da^2 + de^2 - 2dae > da^2 - a^2e.$$

Let us then adequate, following the preceding method; by taking out the common terms we find:

$$de^2 - 2dae \sim -a^2e,$$

or, which is the same,

$$de^2 + a^2e \sim 2dae.$$

Let us divide all terms by e:

$$de + a^2 \sim 2da.$$

On taking out de, there remains $a^2 = 2da$, consequently $a = 2d$.

Thus we have proved that CE is the double of CD—which is the result.

This method never fails and could be extended to a number of beautiful problems; with its aid, we have found the centers of gravity of figures bounded by straight lines or curves, as well as those of solids, and a number of other results which we may treat elsewhere if we have time to do so.

I have previously discussed at length with M. de Roberval[5] the quadrature of areas bounded by curves and straight lines as well as the ratio that the solids which they generate have to the cones of the same base and the same height.

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[4] Fermat wrote: D ad $D - E$ habebit majorem proportionem quam $Aq.$ ad $Aq. + Eq. - A$ in E bis (D will have to $D - E$ a larger ratio than A^2 to $A^2 + E^2 - 2AE$).

[5] See the letters from Fermat to Roberval, written in 1636 (*Oeuvres*, III, 292–294, 296–297).

Now follows the second illustration of Fermat's "e-method," where Fermat's e = Newton's o = Leibniz' dx.[6]

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(2) CENTER OF GRAVITY OF PARABOLOID OF REVOLUTION, USING THE SAME METHOD[7]

Let $CBAV$ (Fig. 3) be a paraboloid of revolution, having for its axis IA and for its base a circle of diameter CIV. Let us find its center of gravity by using the same method which we applied for maxima and minima and for the tangents of curves; let us illustrate, with new examples and with new and brilliant applications of this method, how wrong those are who believe that it may fail.

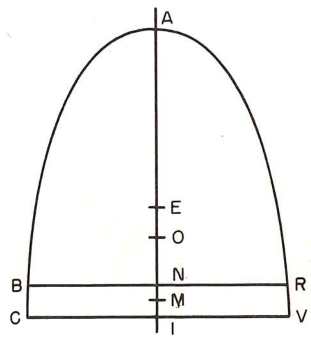

Fig. 3

In order to carry out this analysis, we write $IA = b$. Let O be the center of gravity, and a the unknown length of the segment AO; we intersect the axis IA by any plane BN and put $IN = e$, so that $NA = b - e$.

It is clear that in this figure and in similar ones (parabolas and paraboloids) the centers of gravity of segments cut off by parallels to the base divide the axis in a constant proportion (indeed, the argument of Archimedes can be extended by similar reasoning from the case of a parabola to all parabolas and paraboloids of revolution[8]). Then the center of gravity of the segment of which NA is the axis and BN the radius of the base will divide AN at a point E such that $NA/AE = IA/AO$, or, in formula, $b/a = (b - e)/AE$.

[6] The gist of this method is that we change the variable x in $f(x)$ to $x + e$, e small. Since $f(x)$ is stationary near a maximum or minimum (Kepler's remark), $f(x + e) - f(x)$ goes to zero faster than e does. Hence, if we divide by e, we obtain an expression that yields the required values for x if we let e be zero. The legitimacy of this procedure remained, as we shall see, a subject of sharp controversy for many years. Now we see in it a first approach to the modern formula: $f'(x) = \lim\limits_{e \to 0} \dfrac{f(x + e) - f(x)}{e}$, introduced by Cauchy (1820–21).

[7] This paper seems to have been sent in a letter to Mersenne written in April 1638, for transmission to Roberval. Mersenne reported its contents to Descartes. Fermat used the term "parabolic conoid" for what we call "paraboloid of revolution."

[8] "All parabolas" means "parabolas of higher order," $y = kx^n$, $n > 2$. The reference is to Archimedes' *On floating bodies*, II, Prop. 2 and following; see T. L. Heath, *The works of Archimedes* (Cambridge University Press, Cambridge, England, 1897; reprint, Dover, New York), 264ff.

The portion of the axis will then be $AE = (ba - ae)/b$ and the interval between the two centers of gravity, $OE = ae/b$.

Let M be the center of gravity of the remaining part $CBRV$; it must necessarily fall between the points N, I, inside the figure, in view of Archimedes' postulate 9 in *On the equilibrium of planes*, since $CBRV$ is a figure completely concave in the same direction.[9]

But

$$\frac{\text{Part } CBRV}{\text{Part } BAR} = \frac{OE}{OM},$$

since O is the center of gravity of the whole figure CAV and E and M are those of the parts.

Now in the paraboloid of Archimedes,

$$\frac{\text{Part } CAV}{\text{Part } BAR} = \frac{IA^2}{NA^2} = \frac{b^2}{b^2 + e^2 - 2be};$$

hence by dividing,

$$\frac{\text{Part } CBRV}{\text{Part } BAR} = \frac{2be - e^2}{b^2 + e^2 - 2be}.$$

But we have proved that

$$\frac{\text{Part } CBRV}{\text{Part } BAR} = \frac{OE}{OM}.$$

Then in formulas,

$$\frac{2be - e^2}{b^2 + e^2 - 2be} = \frac{OE\ (= ae/b)}{OM};$$

hence

$$OM = \frac{b^2ae + ae^3 - 2bae^2}{2b^2a - be^2}.$$

From what has been established we see that the point M falls between points N and I; thus $OM < OI$; now, in formula, $OI = b - a$. The question is then prepared from our method, and we may write

$$b - a \sim \frac{b^2ae + ae^3 - 2bae^2}{2b^2e - be^2}.$$

Multiplying both sides by the denominator and dividing by e:

$$2b^3 - 2b^2a - b^2e + bae \sim b^2a + ae^2 - 2bae.$$

[9] This is postulate 7 in the modern Heiberg edition, and is translated in Heath, p. 190, as follows: "In any figure whose perimeter is concave in (one and) the same direction the center of gravity must be within the figure." (On the term "concave in the same direction," see Heath, p. 2.)

Since there are no common terms, let us take out those in which e occurs and let us equate the others:

$$2b^3 - 2b^2a = b^2a, \quad \text{hence} \quad 3a = 2b.$$

Consequently

$$\frac{IA}{AO} = \frac{3}{2}, \quad \text{and} \quad \frac{AO}{OI} = \frac{2}{1},$$

and this was to be proved.[10]

The same method applies to the centers of gravity of all the parabolas ad infinitum as well as those of paraboloids of revolution. I do not have time to indicate, for example, how to look for the center of gravity in our paraboloid obtained by revolution about the ordinate;[11] it will be sufficient to say that, in this conoid, the center of gravity divides the axis into two segments in the ratio 11/5.

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9 TORRICELLI. VOLUME OF AN INFINITE SOLID

Evangelista Torricelli (1608–1647) succeeded Galilei at Florence as mathematician to the grand duke of Tuscany. He was well acquainted with the works of Archimedes, Galilei, and Cavalieri, and corresponded with Mersenne, Roberval, and other mathematicians. He computed many areas, volumes, and tangents, discussed the cycloid, performed what we now see as partial integration, and had an idea of the inverse character of tangent and area problems. He was aware of the logical difficulties in the method of indivisibles (see Selection IV.6). Torricelli is best known as a physicist (we speak of the "vacuum of Torricelli" in the mercury barometer), but his *Opere* (ed. G. Loria and G. Vassura, 3 vols.; Montanari, Faenza, 1919) show his ingenuity also in mathematics. From the *Opere* his manuscript "De infinitis spiralibus" (*c.* 1646) has been republished (with improved text) with an Italian translation by E. Carruccio (Domus Galilaeana, Pisa, 1955). Our selection is from *De solido hyperbolico acuto* (*c.* 1643), not published until 1919 in the *Opere,* vol. I, part 1, pp. 191–221. Here we see how he integrated, by a purely geometric method, an integral with an infinite range of integration, but yet finite, something quite remarkable in those days. The method used is that of indivisibles, in this case formed by circles in parallel planes.

ON THE ACUTE HYPERBOLIC SOLID

Consider a hyperbola of which the asymptotes AB, AC enclose a right angle [Fig. 1]. If we rotate this figure about the axis AB, we create what we shall call

[10] These relations were known to Archimedes (see note 8). But Fermat solved this problem on centers of gravity, hence a problem in the integral calculus, with what we might call an application of the principle of virtual variations.

[11] Here ACI of Fig. 3 is rotated about CI.