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Plato: Parmenides 149a7-c3. A Proof by Complete Induction?

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1. Introduction

It is a generally accepted opinion that the first instance of *conscious use* of Complete Induction (henceforth CI) as a proof method is contained in the *Traité du triangle arithmétique* by Blaise PASCAL (1623–1662):

> Quoique cette proposition ait une infinité de cas, j'en donnerai une démonstration bien courte, en supposant 2 lemmes.
>
> Le 1, qui est évident de soi-même, que cette proportion se rencontre dans la seconde base; car il est bien visible, que ϕ est à σ comme 1 à 1.
>
> Le 2, que si cette proportion se trouve dans une base quelconque, elle se trouvera nécessairement dans la base suivante.
>
> D'où il se voit qu'elle est nécessairement dans toutes les bases: car elle est dans la seconde base par le premier lemme; donc par le second elle est dans la troisième base, donc dans la quatrième, et à l'infini.[1]

PASCAL then proceeds to prove lemma 2, i.e. the inductive step.

A very different question is to establish whether, before PASCAL, convincing examples of *use* of CI as a proof technique are attested, disconnected from the perception of the fact that it happened to be a particular instance of a general demonstrative scheme. Scholars have found proofs fitting in the scheme of CI in almost every geographical ambit of pre-Pascalian mathematics.[2] Some of these proposals, especially those concerning the ancient mathematical corpus, are untenable, even surprising in view of the

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[1] PASCAL 1954, p. 103. The passage is contained in the *Conséquence Douzième*. The treatise was composed towards the end of 1654 and published posthumously, together with earlier drafts and related treatises, in 1665, with the title *Traité du Triangle arithmétique, avec quelques autres petits traités sur la même matière*. We can find in it four proofs in the scheme of CI, with almost identical wording and pattern of proof: see also pp. 113–114, 122 and 150–151 in PASCAL 1954.

[2] See RASHED 1972–73 for Arab mathematics (X–XII century), RABINOVITCH 1970 for the works of LEVI BEN GERSON (1288–1344). A pivotal point in the scholarly debate on this subject is the ascription of proofs by CI to Francesco MAUROLICO (1494–1575): see VACCA 1909 for the first proposal, and the subsequent discussions in BUSSEY 1917 and, in a deeper way, in FREUDENTHAL 1953.

serious misunderstandings they contain.[3] Recently, the polemical tone of the debate has attained a local maximum: in FOWLER 1994 *versus* UNGURU 1994 preconceptions of historiographical method have strongly influenced the evaluation of the relevant instance of CI. In this sense, both FOWLER's and UNGURU's analyses, though interesting in several respects, seem to me to have missed their target. Moreover, a careful survey of the literature shows that every single scholar sets up his own reading of the principle of CI, and on this basis he is able to affirm or to deny that specific proofs constitute well formed examples of it.

My aim is to increase the confusion on this subject. In fact, I suggest to regard a PLATONIC passage, i.e. *Parmenides* 149a7-c3, as a full-fledged example of proof by CI. A comparison with an extremely similar text by ARISTOTLE (*An. pr.* 42b6-16) will allow me to put forward a conjecture about the sources of the Platonic text.

The paper is organized as follows: in Sect. 2 I discuss the meaning and structure of a proof by CI, extracting from its "ritual structure" the characteristics I regard as essential. In Sect. 3 the Platonic passage is presented and analysed. On the basis of the discussion of Sect. 2, I propose to regard it as the only extant example of proof by CI in the ancient mathematical corpus.[4] In Sect. 4 I briefly discuss other alleged examples of CI in ancient mathematical and philosophical works.

2. Principles of complete induction and proofs by complete induction

In order to clarify the meaning of the title of this section, compare the following two deductive patterns ($P(.)$ is a property of natural numbers):

a. $$\frac{\begin{array}{l} P(1) \\ \forall n \in N,\, P(n) \rightarrow\ P(n+1) \end{array}}{\forall n \in N,\, P(n)}$$

b. If some collection of natural numbers satisfies: *i.* the unit belongs to the collection, and *ii.* the successor of any number in the collection also belongs to the collection, then this collection will comprise all of the natural numbers.[5]

We could be tempted to regard them as equivalent formulations of CI, but the equivalence involves assumptions or problems like:

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[3] See for instance the lapse by STAMATIS in EUCLIDES, vol. I, p. XXXV. I find unacceptable, if only because lacking in a supporting discussion, statements like the following: "Cependant on peut trouver quelques démonstrations par récurrence ou induction complète. On ne retrouvera jamais le leitmotiv moderne, [. . .] et ceux qui ne voient l'induction complète qu'accompagnée de sa rengaine auront le droit de dire qu'on ne la trouve pas dans les Eléments. Pour nous, nous la voyons dans les prop. 3, 27 et 36, VII, 2, 4 et 13, VIII, 8 et 9, IX. [. . .] Dans tous les cas le raisonnement est mené jusqu'au point où sa répétition mécanique assurera sa généralité." (ITARD 1961, pp. 73–74).

[4] I prefer not to use the term "ancient Greek mathematics". I intend to refer to a very concrete, tangible object, not to a conceptual framework or to a specific way of doing mathematics. I agree with O. Neugebauer that "a concept as "Greek mathematics" [. . .] seems to me more misleading than helpful." (NEUGEBAUER 1969, p. 190.)

[5] This is the fifth Peano axiom as given in FOWLER 1994, p. 263.

1. Every property defines a set, and the converse.
2. The concept of "collection of all natural numbers" does not give rise to problems and can be freely used instead of quantification over every single natural number.
3. The difference between formalized and informal statements is not a substantial issue. Moreover, it is crucial to remark that neither a. nor b. represent a formalization of the *Principle* of CI (henceforth PCI): to this purpose it is necessary to premise a statement specifying that the property *P*(.), or the collection we are dealing with, is generic.

If employed in connection with the ancient mathematical corpus, any of these definitions of CI is misleading: a consideration of the above very partial list of "modern" problems they carry with themselves will suffice. It is thus necessary to refer to a "definition" formulated in a language which is the most informal possible, in particular with respect to the problem of expressing the different levels of generality. To this end it is useful to start with the claim by STAMATIS: "inductio completa primum ab Aristotele commemorata est: τὸ καθόλου δὲ ὑπάρχει τότε, ὅταν ἐπὶ τοῦ τυχόντος καὶ πρώτου δεικνύηται (Anal. Poster. 73b32)."[6] This is a serious and surprising mistake. In fact, the statement is transparent, even considering the extreme density of the Aristotelian style; we could translate it "[a predicate] applies universally whenever it is shown [to apply] in a generic case and in a primary way", where "primary" means that the relation between subject and predicate must be regarded as immediate with respect to the properties which define the essence of the subject.[7] In a proof formalized in a modern way, the generality alluded to by the definition in *An. post.* 73b32 is secured by the presence of universal quantifiers acting upon the free variables; in an informal context, generality is guaranteed by recognizing that a proof which does not use some specific property of a mathematical object holds also for every object of the same *genus* not sharing that particular property.[8] Explicit *awareness* of this fact,[9] and its massive *use* in technical contexts, are a fundamental achievement of Greek thought.

In the particular case of CI, its character of generality follows from the remark that, both in the inductive step and in the conclusion, *n* acts as a "free variable". Only in this sense CI instantiates the Aristotelian prescription (which then assumes the *status* of statement belonging to what is nowadays termed "proof theory")[10]: very much as every general proof does, CI proceeds by instantiation and subsequent generalization over the "free variables" (which, in other contexts, may even be geometrical figures, as in the examples ARISTOTLE provides during his proof). But it is completely erroneous to maintain the converse, i.e. that *An. post.* 73b32 formalizes PCI.

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[6] EUCLIDES, vol. I, p. XXXV.

[7] The example provided in 73b33-39 to clarify the meaning of this statement is completely unambiguous.

[8] Problems could arise at the moment of specifying the most general *genus* of objects the proof may be applied to (see also the discussion in Sect. 4).

[9] See for instance PROCLUS 1873, 207.4-25. A recent analysis of the "shaping of generality in Greek mathematics" is carried through in NETZ 1999, chap. 6. I do not agree with some of his conclusions, and I am currently working on this subject.

[10] For ARISTOTLE as "proof theorist", see for instance SMITH 1984, SMITH 1986, or SMILEY 1994 and references therein.

On this basis, I think the following sequence could correctly describe the series of steps which are necessary to achieve a proof by CI:

A. To set the basis of induction.
B. To enunciate the inductive step and to explicitly recognize its generality ($\approx$ universal quantification $\approx$ *An. post.* 73b32).[11]
C. To *prove* the inductive step and to explicitly recognize the generality of the proof ($\approx$ universal quantification $\approx$ *An. post.* 73b32).
D. To recognize the generality of the conclusion ($\approx$ universal quantification $\approx$ *An. post.* 73b32).

Step A is often trivial; the real proof is developed in B and C. D is matter of "formal awareness", restricted to the context of one *specific* proof in the scheme of CI; it may be expressed in an informal way: but it is crucial that its statement be explicit and clearly verbalized.[12] The sequence A–D is not, *a priori*, beyond the *possibilities* of the ancient mathematical corpus. But, as we have seen, an additional move is needed to recognize it as a valid demonstrative scheme (i.e. to establish a sort of PCI): actually, a further application of *An. post.* 73b32 is enough, where the generalization is made on that *specific* property, whose applicability to every natural number had been proved in A–D ($\approx$ universal quantification on the "variable" $P(.)$ in version a. of CI). This step (of a metamathematical character) is never explicitly made in the ancient mathematical corpus.[13]

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[11] In the following, I shall denote in this way the implicit or explicit ascription of generality to a statement, whenever it results from a (possible) application of an argument of the kind of *An. post.* 73b32. I do not claim the usage is always explicit (it is almost never explicit). It is a convenient shorthand, and there is no anachronism to invoke it in dealing with PLATO.

[12] In the case of informal arguments, things go even worse: indeed, "essendo l'obiettivo quello di riconoscere una regola logica in sequenze discorsive che non hanno mai la scansione netta dei passaggi delle dimostrazioni matematiche, è inevitabile ricorrere a ricostruzioni e precisazioni di significati che sono atti interpretativi, e quindi in certa misura soggettivi" (see p. 177 in BELLISSIMA-PAGLI 1996). The history of the logical rule called *Consequentia Mirabilis* ($(\neg A \rightarrow A) \rightarrow A$; it appears in *Elem*. IX.12, 36) reconstructed in that book has several points of contact with that of CI.

[13] Someone could (misleadingly) venture that the situation was not so clear even to PASCAL: indeed, in the passage we have quoted, he speaks of "cette proposition" and clearly does not "quantify" on it – i.e. he does not establish CI as a *principle*; moreover, a little earlier (PASCAL 1954, p. 101; *Conséquence Huitième*) he proves a proposition using Incomplete Induction ("Et ainsi à l'infini"); the same thing happens in Lemme IV, which immediately precedes Proposition I (p. 113). Finally, it is very relevant that PASCAL "proves" the validity of his demonstrative scheme by CI appealing to an argument by *Incomplete Induction*: see the last paragraph of our quotation. Equally relevant is the fact that he never "proves" his "second lemme" – the inductive step – for a generic n; rather he deals with a particular case (for instance, from $n = 4$ to $n = 5$ in the proofs at pp. 103 and 113–114), of which he soon afterwards declares the generality: "On le montrera de même dans tout le reste, puisque cette preuve n'est fondée que sur ce que cette proportion se trouve dans la base précédente, et que chaque cellule est égale à sa précédente, plus à sa superieure, ce qui est vrai partout." – which is a paradigm instance of application of *An. post.* 73b32. Hence, PASCAL does not recognize independence or priority – even intuitive – to the argument by CI, regarding it as a shorthand and a convenient form of formalization of the argument by Incomplete

Very strong objections against the mere *possibility* that "ancient Greek mathematics" could develop single proofs by CI (*even in absence* of an explicit recognition of its validity as a demonstrative scheme) have been raised in UNGURU 1991.

The first argument, constituting in my opinion the core (pp. 280–284) of his paper, is summarized by the author as follows: "without a number that can serve as an independent variable, it is impossible to formulate a true proof by mathematical induction, in which the claim requiring proof is a function of the natural numbers" (p. 284). This happens because of the "wide, and historically unbridgeable, gap between the Greek and the modern conceptualization of number" (p. 281). The latter is "indeterminate, general, abstract" whilst the former "is always a 'number of"' (p. 284). I think that, as it stands, UNGURU's argument must be rejected:

– whenever it is supported only by mathematical sources, it is based on an undue application of inductive reasoning: from the fact that a well determined set is empty (the sources analysed by UNGURU in which CI is extant) it is inferred that every set containing it is empty. As a consequence,
– the author presents a series of meta-historical, meta-theorical, meta-textual, meta-contextual arguments, providing an aprioristic direction to the analysis of every possible textual evidence.[14] These supporting arguments are based on a handful of quotations from primary sources,[15] whereas a detailed exegesis is devoted to the relevant passages of KLEIN 1968, with further references to WITTGENSTEIN and W. C. BOOTH. This way, an easy historiographic hypothesis results, which the empirical basis provided by the ancient mathematical corpus will never be able to falsify. The latter seems to be the main flaw of the "*quasi*-intentional approach" advocated by UNGURU (p. 289).
– Even accepting UNGURU's thesis as well founded, the obstruction arising from the fact that "the claim requiring proof is a function of the natural numbers" still allows for a way out: this happens when the "function" (i.e. the property $P(.)$) is very simple, for example if it maps every number into itself, that is $P(n) = n$; or it is a "slight variation" of the latter mapping, $P^*(n) = n - 1$ for instance; or it is a combination of both, such as $P^{**}(n) = P(n) - P^*(n) = 1$. The arithmetical and meta-arithmetical level are not easy to disentangle in this case: in order to use a "function" which maps every integer n into itself it is not necessary to conceive the numbers in an abstract

Induction: compare the statements at p. 122 "Quoique cette proposition ait une infinité de cas, je la démontrerai néanmoins *en peu de mots* par le moyen de deux lemmes" and at p. 150 "Quamvis infiniti sint hujus propositionis casus, [. . .], *breviter* tamen demonstrabo, positis duobus assumptis." (italics mine). These considerations are worthy of further elaboration, but here this hint will suffice. See also the following note 21 and the well-balanced remarks made in RASHED 1972–73, section IV.

[14] It is not a coincidence that textual considerations, which I regard as crucial in the present case, are almost absent from UNGURU's paper.

[15] Most (provided it is meaningful to make use of this term with such a poor sample) of them are philosophical sources: ARISTOTLE: *Meth.* 1020a13 and 1057a2-5; ALEXANDER OF APHRODISIAS: *In Meth.* 86, 5–6 (Hayduck). Luckily also EUCLIDES: *Elem.* VII def. 2 is included.

way, nor to have any concept of "function" at all.[16] In other words, UNGURU fails to distinguish between PCI and CI, and applies arguments to the latter which should properly have been applied to the former.

The second objection (p. 278) is well summarized by the statement "Its [of a CI] conclusions [. . .] are valid for all natural numbers. Needless to emphasize that the concept of 'all natural numbers' is foreign to Greek mathematics." The last claim is correct, but its relevance to our problem is not clear: an easy way out is to make use of a wording which does not refer to the set of all natural numbers, and to state that the property under analysis is true *for every single* integer:[17] a possibility that ancient Greek language easily allowed for, see for instance *Elem.* IX.8.[18]

On the other hand, in FOWLER 1994, which is an explicit confutation of UNGURU 1991, reconstructions in the scheme of CI – using techniques which are well rooted in the ancient mathematical corpus – are presented (pp. 254–257), both of a proposition which is not attested[19] and of the well known relationship between side numbers s_n and diagonal numbers d_n: $(d_n)^2 - 2(s_n)^2 = \pm 1$.

FOWLER supports his reconstructions by observing (pp. 261–264) that: *i*. in the *Elements* use is made of the Principle of Descent (PD) (*Elem.* VII.31) and of the well-ordering principle (*Elem.* VII.31 ΑΛΛΩΣ)[20]; *ii*. "these two principles are now generally, though not universally, perceived as being equivalent to each other and to mathematical induction." Remarking that "before the nineteenth century, we find a more 'natural' approach to the natural numbers", he concludes: "It is in this sense that I believe that the Greeks did use principles equivalent to mathematical induction, and that they could have used mathematical induction itself had the situation presented itself. Moreover, such a

[16] The arguments we can find in MUELLER 1981 appear better balanced, although in many respects close to those advocated by UNGURU. Referring *only* to the *Elements*, he says that "[. . .] numbers are not characterized as generated from units in a serial order. They are simply finite aggregates of units." (paraphrase of *Elem.* VII def. 2: ἀριθμὸς δὲ τὸ ἐκ μονάδων συγκείμενον πλῆθος) "Of course, there are important relations between these aggregates, but the relation of successor is not one which plays an important role in the *Elements*. Thus it can be said that the integers themselves are not conceived in the structural way conducive to the use of induction, but that there is inductive reasoning about collections or sequences of positive integers. This difference might be compared to the difference in formal theories between the use of induction in the object language and its use in the metalanguage" (p. 69). In this way, MUELLER denies that EUCLIDES could consciously use the *principle* of CI, but does not assert that could be impossible for him to present proofs in the scheme of CI. In fact, not even this latter case does occur in the *Elements* (see Sect. 4), and MUELLER rightly points out that "where Euclid proceeds in this quasi-inductive way, the implicit induction is on the number of terms involved in a construction or assertion and not on the integers themselves" (p. 69). As we shall see, where PLATO proceeds in a *fully* inductive way, the *explicit* induction will be precisely of this kind.

[17] The same difference exists between version a. and b. of PCI.

[18] οἱ πέντε διαλείποντες πάντες [*scil.* ἀριθμοὶ] in EUCLIDES II, pp. 194.17, 195.20.

[19] "If as many numbers as we please beginning from a unit be set out, their sum will be a triangular number" (p. 254).

[20] This alternative proof is deemed interpolated by HEIBERG, since it appears only in the Theonine manuscripts: "ante prop. λα´ add. Theon" (EUCLIDES II, p. 237).

situation did present itself, in the properties of figured numbers and side-and-diagonal numbers [. . .] I believe it very probable that some earlier, now lost, treatment of some of these topics, in particular the side-and-diagonal numbers, did in fact prove the described properties – [. . .] – and that these proofs could very probably have been by some form of mathematical induction."

I think that FOWLER's argument must be rejected:

– an equivalence between PCI and PD may be established only after the validity of both of them as patterns of proof is accepted, and after an outline is provided of a proof (even informal) of their equivalence. To require this, even before the 19th century, is not a "historical solecism" but a necessary condition for the argument's consistency. If this does not occur, so much the worse for the argument.[21]
– Conjecturing is often necessary but the risk of losing contact with the extant textual data is always present. As we have said, FOWLER sets up (pp. 254–255) the reconstruction, in an inductive frame, of a proposition not existing in the ancient mathematical corpus but "modelled [. . .] on Heath's translation of *Elementa* IX.36". Concluding his analysis, he confesses that "these kinds of arguments are not explicitly attested in Greek mathematics, but the later commentators [. . .] give abundant though unsophisticated testimony to an earlier 'pebble (*psephoi*) arithmetic' using these ingredients." It is quite the contrary: the evidence that proofs of the kind he develops in his reconstruction are *not* attested is a strong argument *against* the presence of CI as a widespread proof technique.

[21] I think instead that the counter-objection in UNGURU 1994, p. 271 cannot be regarded as valid. He maintains that PD and PCI "are not conceptually the same procedure", since PD "deals with a finite number of cases", while PCI "covers an infinity of cases". In fact, even a proof by CI explicitly deals with only a finite number of cases (indeed only one, in the proof of the inductive step). Precisely the awareness of the quasi-general character of this proof introduces the infinitary component in CI. But the same happens in PD, since this latter is a valid step in a deductive chain exactly because of the character of generality granted to it by its being true for *every* decreasing sequence of integers. In both kinds of proof one passes from instantiation to the general case precisely via application of *An. post*. 73b32. Rather, I believe that PCI and PD cannot be regarded as "equivalent" (I consider it obvious that they cannot be historically equivalent) for a different series of reasons: they are not so 1) from the point of view of intuitive evidence – which, when one has to "choose" a proof technique instead of another, has a decisive weight; 2) since they involve very different logico-mathematical concepts. We cannot presume that different concepts can be grasped with equal ease (for instance the concept of decreasing sequence in PD or the notion of property defined on an infinite collection of objects in PCI; or, as MUELLER (p. 78) rightly points out, "unlike the principle of induction, the least number principle or the denial of infinitely descending chains does not seem to depend upon a genuinely structural conception of the positive integers. For these principles can be understood solely in terms of the Euclidean conception of numbers as finite concatenations of units"); 3) since every distinct technique employed to prove the same theorem gives it a different meaning, (if nothing else) because it changes the net of propositions needed to achieve the proof. Moreover, 4) to speak of equivalence is meaningful only after PCI and PD have been recognised as such, and after logical techniques have been developed to give sense to this equivalence. Even in this case, an equivalence proof can be higly nontrivial (even worse if we want it formalized: one should take a look at §40 in KLEENE 1952).

Summarizing, the conception of number we may extract from the ancient mathematical corpus seems to rule out the possibility that a *principle* of CI could be used.[22] Indeed, the latter is never attested, not even in a rudimentary stage.[23] Proofs in the scheme of CI, working on the number of terms involved in a statement and not on the integers as such – even proofs such that the steps from A to D above outlined are verbalized and formalized in a way which is coherent with what we *think* to be the ontology underlying "ancient Greek mathematics" – seem to be well within the range of possibilities.[24] Surprisingly, the only full-fledged example, though very simple, does not appear in a mathematical work, but in PLATO's *Parmenides*.

3. Analysis of Plato, *Parmenides*, 149a7-c3

The Platonic passage – bracketed numerals are inserted for reference – is the following:

> (1) Δύο ἄρα δεῖ τὸ ὀλίγιστον εἶναι, εἰ μέλλει ἅψις εἶναι. – Δεῖ. – (2) Ἐὰν δὲ τοῖν δυοῖν ὅροιν τρίτον προσγένηται ἑξῆς, αὐτὰ μὲν τρία ἔσται, αἱ δὲ ἅψεις δύο. – Ναί. – (3) Καὶ οὕτω δὴ ἀεὶ ἑνὸς προσγιγνομένου μία καὶ ἅψις προσγίγνεται, (4) καί συμβαίνει τὰς ἅψεις τοῦ πλήθους τῶν ἀριθμῶν μιᾷ ἐλάττους εἶναι. (5) ᾧ γὰρ τὰ πρῶτα δύο ἐπλεονέκτησεν τῶν ἅψεων εἰς τὸ πλείω εἶναι τὸν ἀριθμὸν ἢ τὰς ἅψεις, (6) τῷ ἴσῳ τούτῳ καὶ ὁ ἔπειτα ἀριθμὸς πᾶς πασῶν τῶν ἅψεων πλεονεκτεῖ· (7) ἤδη γὰρ τὸ λοιπὸν ἅμα ἕν τε τῷ ἀριθμῷ προσγίγνεται καὶ μία ἅψις ταῖς ἅψεις. – Ὀρθῶς. – (8) Ὅσα ἄρα ἐστὶν τὰ ὄντα τὸν ἀριθμὸν, ἀεὶ μιᾷ αἱ ἅψεις ἐλάττους εἰσὶν αὐτῶν. – Ἀληθῆ.[25]
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> (1) Then they must be two, at least, if there is to be contact. – They must. – (2) And if to the two terms a third be added in immediate succession, they will be three, while the contacts [will be] two. – Yes. – (3) And thus, one [term] being continually added, one contact also is added, (4) and it follows that the contacts are one less than the number of terms. (6) For the whole successive number [of terms] exceeds the number of all the contacts as much (5) as the first two exceeded the contacts, for being greater in number than the contacts: (7) for afterwards, when an additional term is added, also one contact to the contacts [is added]. – Right. – (8) Then whatever the number of terms, the contacts are always one less. – True.[26]

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[22] If applied to the *principle* of CI, some of UNGURU's arguments seem to be well-founded. But he intended to apply them to the very different question of the existence of proofs in the scheme of CI, and I think that the passage by PLATO I shall presently discuss confutes them.

[23] But the statement of the principle of CI must obviously have been preceded by more or less explicit applications of CI, as is natural in the *historical* development of a mathematical object or of a proof technique.

[24] I completely agree with MUELLER's remarks quoted in note 16.

[25] Greek text from PLATO, *Parmenides* (Burnet).

[26] I have reworked the translation in PLATO 1926, p. 275.

Remarks

a. The text is embedded in the discussion of the consequences of the second hypothesis PARMENIDES envisages in his inquiry about the nature of the "one": "if the one exists" (142b3).[27] In particular, it is the main argument supporting the conclusion: "the one touches and does not touch itself and the others" (149d6).

b. The reasoning has the following structure (the congruences with the scheme A–D outlined in Sect. 2 are conspicuous):

A. The basis of induction is established in (1). In (5) this very fact is explicitly recognized (τὰ πρῶτα δύο); moreover, ᾧ γὰρ plays the role of pointing out that (1) or (2) are crucial steps in the proof. As will become clear in (4), the property to be regarded as relevant is the following: (number of terms) − (number of contacts) = 1.

B. The validity of the inductive step is shown as evident in two particular cases ($n = 2$ and $n = 3$) in (1) and (2) and *stated* in general in (5)–(6), where ὁ ἔπειτα... πᾶς πασῶν has the function to make generality explicit, even if, by *variatio* with respect to (3) (see below), the attributive form substitutes the adverbial form. The transition from (1)–(2) to (5)–(6) is very subtle: it constitutes the logical center of the argument and it is verbalized in a slightly compressed form: it is worth a detailed discussion.

ὁ ἔπειτα ἀριθμὸς πᾶς presents two difficulties: the role of ἔπειτα and of πᾶς. The former is conveniently rendered as "successive", but it may refer both to a generic successive integer, and to a "successive" in the sense of a repetition of the scheme employed in (1)–(2)[28] and synthesized in (5), to which (6) is explicitly set in parallel: in the same way in which $n = 3$ is obtained by adding in succession (προσγένηται ἑξῆς) a third term to $n = 2$, ὁ ἔπειτα ἀριθμὸς is what one obtains as *successive* from the *preceding* case by adding a single unit. Such an interpretation is strengthened by the explicit correlation τὰ πρῶτα... ὁ ἔπειτα, which links both the results and the proofs, and which qualifies the number under consideration as a *second* term in a logico-temporal sequence whose paradigm instance is displayed in (2). The way πᾶς is employed provides further clues. Indeed, its use in conjunction with the article ὁ, its postponement and the position, correlated to the subsequent πασῶν, give strong support to the choice of translation "the whole successive number" (i.e. the whole aggregate of units which constitutes it). In this way, (6) refers to the transition to "the whole successive number" as far as it is obtained, as a whole, adding a further unit, in the way explained in (2) as a particular case. To assume, instead, the meaning "every" for πᾶς would make conclusion (8), already contained in (6) as a particular instance, completely useless. A further element of generalization is present in (5)–(6): it is not declared that in the "first two"

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[27] ἓν εἰ ἔστιν (142b3). It is not relevant to my purposes to discuss the question whether the above translation provides the correct meaning of the Platonic expression.

[28] In fact, the reference to "the first two" in (5) carries out a twofold function: first because both cases verify the requested property, but also in the way in which this fact is proved to occur: the case $n = 2$ is *asserted* to be true, while $n = 3$ is *proved* following the scheme of (2). And in this scheme one passes from an integer to the next. I find unlikely that "the first two" can refer to the first two "terms", i.e. to the state of affairs described in (1): it would be difficult, for instance, to explain the presence in (5) of ἅψεων in the plural, since only one contact is present.

cases the difference must be equal to the unit; the property of invariance of the difference is proved whichever the initial difference may be: in fact, the proof that follows does not employ the fact that the initial difference is a well determined number (a further instance of "application" of *An. post.* 73b32). This remark gives weight to my contention that "the first two" in (5) is a reference to the logical and argumentative connection established in (1)–(2), rather than to a pleonastic duplication of the basis of the induction.

C. The proof of the inductive step and the statement of its generality are provided in (3), where ἀεὶ combines the idea of extratemporal validity with that of indefinitely repeated operation; and in (7). The latter is explicitly connected with (5) and (6), *qua* their proof, by the use of γὰρ, whereas ἤδη... τὸ λοιπὸν is employed in a sense which abstracts from the primary (temporal) meaning of the words to assume a logical function:[29] τὸ λοιπὸν refers to a series of demonstrative steps as (2).

D. The generality of the conclusion is expressed in (8), which enlarges the field of validity of (4) and slightly paraphrases it. In (8), the role of quantifier is played by the completely unambiguous phrase, Ὅσα... ἀεὶ.... The particle ἄρα establishes that (8) is a logical consequence of what has been said before. We see then that several phrases can work as "quantifiers"; among them, the more explicit ones employ the words ἀεὶ and πᾶς, which will hold this technical meaning even in more formalized contexts.

I propose a concise "interlinear", formalized in a modern way, of the Platonic passage. As should be clear, this rephrasing tendentiously separates the variable labelling the property from the objects the latter applies to; as it stands, it strains the reading of the Greek text, in which an induction on the number of terms in contact is employed, not an induction on the natural numbers. Nevertheless, I think it worth giving since it conveniently synthesizes the logical structure of the proof.

0. One needs to show that $P^*(n) = P(n) - 1$, where $P(n)$ = number of n terms in contact, and $P^*(n)$ = number of contacts of n consecutive terms (these symbols are shorthands of the corresponding wording).

A. (1) If $n = 2$, $P(2) = 2$ and $P^*(2) = 1$.

B. (2) If $n = 2+1$ ('Eὰν δὲ τοῖν δυοῖν ὅροιν τρίτον προσγένηται ἑξῆς), $P(2+1) = 3$ (αὐτὰ μὲν τρία ἔσται) and $P^*(2+1) = 2$ (αἱ δὲ ἅψεις δύο). (5) Following the same scheme as in (2) (*the correlatives* τὰ πρῶτα δύο... ὁ ἔπειτα and ᾧ... τῷ ἴσῳ τούτῳ), [in the more general case] in which $P(2) - P^*(2) = m$ (ᾧ) and hence $P(2+1) - P^*(2+1) = m$, (6) [one obtains] also (καὶ) $P(n+1)$ (ὁ ἔπειτα ἀριθμὸς πᾶς) $- P^*(n+1)$ = (πασῶν τῶν ἅψεων πλεονεκτεῖ) m (τῷ ἴσῳ τούτῳ).

C. (3/7) Indeed (Καὶ οὕτω δὴ/γὰρ), for every n (ἀεὶ + *participle*/ἤδη... τὸ λοιπὸν), $P(n+1) = P(n)+1$ (ἑνὸς προσγιγνομένου/ἕν... τῷ ἀριθμῷ προσγίγνεται) if and only if (*genitive absolute with temporal shade*... καὶ/ἅμα... τε... καὶ) $P^*(n+1) = P^*(n) + 1$ (μία... ἅψις προσγίγνεται/μία ἅψις ταῖς ἅψεις).

D. (4/8) It follows (καὶ συμβαίνει/ἄρα), for every n (*in explicit form it is absent, but notice the use of the determinate article in the subsequent subordinate*/ Ὅσα ...

[29] We could say "a tempore transfertur ἤδη ad causarum vel ratiocinandi seriem et consequentiam" (BONITZ 1870 *sub voce* ἤδη).

ἐστὶν τὰ ὄντα τὸν ἀριθμὸν, ἀεὶ), $P^*(n) = P(n) - 1$ (τὰς ἅψεις τοῦ πλήθους τῶν ἀριθμῶν μιᾷ ἐλάττους εἶναι/μιᾷ αἱ ἅψεις ἐλάττους εἰσὶν αὐτῶν).

c. It is clear that this proof does not invoke a particular instance regarded as generic (with a tacit reference to something like *An. post.* 73b32; most quasi-inductive proofs contained in the *Elements* are framed in this way – see below), or after which phrases like "and so on" are called on. Rather, there are two proofs nested the one in the other: (1)–(4) set out a proof by CI of (4) (we have to include also this latter since in this case the συμπέρασμα plays the role of an absent πρότασις); (5)–(7) explicitly state (5), (6) and prove (7) the inductive step in full generality, and this requires in any case a separated argument – this fact is made apparent by the use of γὰρ, which introduces in (5) the sub-proof. Moreover, notice that in (1)–(4) the inductive step is verbalized (3) by reporting a sketch of its *proof* (generalized through the use of ἀεὶ).

d. It is crucial, on the basis of our discussion in Sect. 2, that the "numerical variable" labelling the property coincides with the concrete object (τὸ πλῆθος) representing the extension of the concept identified by the property itself, so that the induction is on the number of terms in contact. This sort of merging is reflected in the terminological ambiguity connected with the transition from the grammatical subjects ὄντα, ὅροι (or a gender neuter with implied subject) to ἀριθμοὶ (in 6–7) and then back to ὄντα.[30] Notice also the passage from τοῦ πλήθους τῶν ἀριθμῶν[31] to ἀριθμοὶ. We are faced with an oscillation of meaning from "item or term in a series" to "amount, sum", which makes the presence of πλῆθος unnecessary.[32] Relevant in this sense is the use of a different gender (ἕν *versus* μία) to denote the "one" which is to be added to the terms or to the contacts.

e. The whole argument *does not* imply the conclusion PLATO draws from it in 149c4: "But if only the one exists, and not the dyad, there can be no contact." The latter is an immediate consequence of (1), but cannot be deduced from (8), which holds for every *successive* number. Yet, the passage employs a lexicon, a syntaxis and a logical linking of the arguments revealing a good degree of formalization. This suggests that PLATO does not really master the proof he is elaborating, and that the latter is a (reworked) *excerptum* from some other source. The following remarks point to the same end:

i. The proof is very simple and is expounded in a pedagogical way (the interlocutor is a boy): the conclusion is drawn in (4) and repeated almost *verbatim* in (8). The only

[30] Remark here the extraordinary writing technique: the transition occurs in (5), where the role of τὸν ἀριθμὸν is ambiguous, since it can be construed both as an accusative of relation – and in this case the subject should be still τὰ πρῶπα δύο – , and as the subject of the subordinate τὸ πλείω εἶναι – very much as in (6)–(7).

[31] I follow the *lectio* of the best manuscripts. HEINDORF's emendation τὸν ἀριθμὸν does not change the substance of the argument.

[32] See LSJ *sub voce* ἀριθμὸς and recall also *Elem.* VII def. 2.

difficulties are of a logical order, and this is natural with a kind of proof probably regarded as unusual.[33] Recall, for the sake of comparison, that the property of ratios established in *Parmenides* 154d1-7, very important for a reconstruction of pre-Euclidean proportion theory,[34] is stated without proof.

ii. The wording has several peculiarities: the form συμβαίνει + infinitive is employed in the technical meaning of denoting a logical consequence. It is not an *unicum* in the Platonic *corpus*,[35] but in *Parmenides* there are no further occurrences of this construction, a surprising fact if one envisages the character of the dialogue.[36] The form ἀεὶ + participle in 149b2 is well attested in the technical literature as standard wording for an indefinitely repeated operation. It is important to notice that this wording is present also in other relics of pre-Euclidean formalized mathematics.[37] The impersonal use, and without any moral hue, of πλεονεκτέω has no analogue in the Platonic dialogues, nor is it attested in early technical works.[38] I regard it as unlikely that this verb could have been contained in the source; maybe it has been introduced by PLATO as a *variatio*.[39] Also the use of προσγίγνομαι in this meaning is not attested as common in the *technical* literature, standardized on προστίθημι. I suggest that in this case we do not have a reworking by PLATO, rather a testimony of a terminological field not yet well settled (see further comments in the following point f).[40]

iii. A fraction of the text (5)–(7) has a more abstract character: we can observe a transition, in (6)–(7), to a coherent use of ἀριθμὸς in order to denote the "objects" in contact; moreover, (5)–(7) prove the truth of statement (4) as a particular case (ᾧ... ἐπλεονέκτησεν... τῷ ἴσῳ τούτῳ... πλεονεκτεῖ) and do not make use of the fact that the difference is equal to one. This suggests the existence of a source in which the argument had been discussed at a greater degree of generality.

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[33] This fact explains its length, and the careful setting out of the relevant steps.

[34] See FOWLER 1987, pp. 42–44, 64, 320.

[35] See for instance *Phaedo* 74a2-4.

[36] The verb συμβαίνω occurs at several places in *Parmenides*. However, its presence is restricted to the short summaries Parmenides provides in order to pinpoint the development of the discussion (see *Parmenides* 136a–c, 137b4, 142b–c, 160b5, 163c1; in 143d4 the verb assumes the meaning "to occur"), and in them only the participle τὰ συμβαίνοντα, or an (indirect) interrogative phrase (such as τὶ χρὴ συμβαίνειν) are employed. The relevant fact is that *only* in this passage a relation of consequence is expressed through συμβαίνει + infinitive; elsewhere, the particles οὐκοῦν, ἄρα or the form ἀνάγκη + infinitive are used.

[37] See Appendix IV (pp. 235–244) in KNORR 1978, in the context of a detailed comparison of the several forms of wording of the bisection principle. KNORR's analysis suggests that the following texts can be ascribed to pre-Euclidean formulations: those in *Elementa* XII, most of those extant in the Archimedean *corpus* and the Lemma to *Sphaerica* III. 9 of THEODOSIUS. It is more difficult to decide whether a direct quotation from ANTIPHON is present in the relevant passage in SIMPLICIUS, *In Phys*. 55 (Diels).

[38] For use in later authors, see e.g. IAMBLICHUS, *Theologoumena arithmeticae*, 83.14 (De Falco).

[39] An indication in this direction is the presence in (5) of the accusative τὸν ἀριθμὸν, whose function is to clarify in which sense the terms "exceed" the contacts.

[40] See ARISTOTELES, *Phys*. 245a13 and 260a32 for a very similar usage of προσγίγνομαι in a non-mathematical context.

f. The term ὅρος in 149a8 is attested in the best manuscripts[41] and provides us with a clue to set out a sensible conjecture about the sources of the Platonic passage. In fact, let us read the following Aristotelian extract, in which bracketed numerals have been inserted in order to make a comparison with the corresponding propositions in the text of *Parmenides* easier:[42]

> [...] τὸ μὲν πλῆθος τῶν ὅρων ὡσαύτως ἑνὶ ὑπερέξει τὰς προτάσεις (ἢ γὰρ ἔξωθεν ἢ εἰς τὸ μέσον τεθήσεται ὁ παρεμπίπτων ὅρος· ἀμφοτέρως δὲ (4′) συμβαίνει ἑνὶ ἐλάττω εἶναι τὰ διαστήματα τῶν ὅρων, αἱ δὲ προτάσεις ἴσαι τοῖς διαστήμασιν)· οὐ μέντοι αἰεὶ αἱ μὲν ἄρτιαι ἔσονται οἱ δὲ περιττοί, ἀλλ᾽ ἐναλλάξ, ὅταν μὲν αἱ προτάσεις ἄρτιαι, περιττοὶ οἱ ὅροι, ὅταν δ᾽ οἱ ὅροι ἄρτιοι, περιτταὶ αἱ προτάσεις· (7′) ἅμα γὰρ τῷ ὅρῳ μία προστίθεται πρότασις, ἂν ὁποθενοῦν προστεθῇ ὁ ὅρος, ὥστ᾽ (5′) ἐπεὶ αἱ μὲν ἄρτιαι οἱ δὲ περιττοὶ ἦσαν, (6′) ἀνάγκη παραλλάττειν (7″) τῆς αὐτῆς προσθέσεως γινομένης. [43]
>
> ... the number of the terms will exceed that of the premisses, as before, by one (for each further term which is introduced will be placed either externally or intermediately; but in either case (4′) it follows that the intervals are one fewer than the terms, and there are as many premisses as intervals); the former will not, however, always be even and the latter odd, but alternately when the premisses [are] even the terms [will be] odd, and when the terms [are] even the premisses [will be] odd ((7′) for wherever a term is added one premiss is added as well together with the term), so that (5′) since the premisses were even and the terms odd, (6′) they must change accordingly (7″) when the same addition is made to both.[44]

i. ARISTOTLE is briefly discussing the relationship between number of terms, premisses and conclusions in the case of a complex syllogism. The logical structure of the argument is incomplete, very compressed in the crucial points and excessively expanded on trivial questions. The feeling is strong that ARISTOTLE is reworking and mixing two arguments, leaving unexpressed the most important parts of them. The simplest among the arguments is reduced to the remark that "when the premisses [are] even the terms [will be] odd, and when the terms [are] even the premisses [will be] odd". A proof, very short, is provided in (7′)–(7″); in the steps which are made explicit the proof mimics the one contained in the Platonic text (see below). The second argument hinted at by ARISTOTLE refers to the more refined combinatoric analysis[45] needed to explicitly determine the relationship between number of terms, premisses and conclusions in a syllogism which contains *n* terms. ARISTOTLE says: "thus there will be many more conclusions

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[41] See the critical apparatus in PLATO, *ad loc*. The *lemma* has been variously emended, if only because its gender does not fit for the neuter of the subject τρίτον. This *variatio* can be easily justified on stylistic grounds, but see the subsequent analysis.

[42] The connection between the two passages had already been pointed out in EINARSON 1936 (see note 56 on pp. 163–164).

[43] ARISTOTELES, *An. pr*. 42b6-16.

[44] I have slightly reworked the translation in ARISTOTLE 1938, p. 331.

[45] There is no explicit trace of it in the Platonic text. But recall that the *milieu* of combinatoric analysis has always been an ideal one for proofs in the scheme of CI, from LEVI BEN GERSON to PASCAL.

than either terms or premisses".[46] But the sketchy setting up of the problem, which is developed in the text just quoted and in a few subsequent lines, shows an awareness of some subtle aspects[47] which is not counterbalanced by the triviality of the conclusion.[48]

ii. There are clear points of consonance with the Platonic passage. The demonstrative pattern, though less cogent, is similar: compare the final sentences (7′) and (7″) with the corresponding ones in *Parmenides*. The imperfect tense verb ἦσαν which denotes the first pair of even-odd quantities has a parallel in the unique instance of secondary sequence verb ἐπλεονέκτησεν which is present in the Platonic passage. Syntactical and lexical choices in (4′)/(4) and in (7′)/(7) are strictly related: notice the presence of συμβαίνω and the interchange of subject between ὅροι and διαστήματα, which implies the alternance ἐλάττω εἶναι/ὑπερέχειν, parallel to ἐλάττους εἶναι/πλεονεκτεῖν. Moreover, in addition to the already commented on term ὅρος, the correspondence between τὸ πλῆθος τῶν ὅρων and τοῦ πλήθους τῶν ἀριθμῶν is remarkable. (7′) and (7) are almost the same sentence, as written by PLATO and by ARISTOTLE.

iii. The dissonances are interesting as well. It is clear that (7′)–(7″) constitute in no way a correct proof by CI. But we should recall that in the Aristotelian *corpus* almost *no* proof is *formalized* in a convenient way, and could not have been, given the character of the extant writings. On the other hand, ARISTOTLE extensively employs arguments which shorten and rework more refined mathematical proofs. The importance of the above text, if read in strict parallel with the Platonic one, lies exactly in revealing the existence of mathematical argumentative patterns otherwise destined to be submerged. In this perspective, it is relevant to remark the change of subject διαστήματα/προτάσεις, displayed by the clause αἱ δὲ προτάσεις ἴσαι τοῖς διαστήμασιν. It is not easy to understand this fact without the assumption that ARISTOTLE is in fact making reference to a text or to a representation of the syllogisms as intervals and their terms: a text or representation which should have been conveniently formalized, as the density of technical terms (see below) shows, and whose content PLATO slightly reworks, in view of his philosophical-stylistic aims. In my opinion a derivation of this kind is emphasized by the transition from one term of the contraposition διάστημα/ ἅψις to the other. This transition exploits the ambiguity of the first word: it is an "interval" separating the very terms it links. Hence, it is not surprising that in ARISTOTLE we can find a more standardized technical lexicon,[49] whereas PLATO preserves almost unchanged the logical

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[46] ARISTOTELES, *An. pr.* 42b25.

[47] Notice for instance the careful separation – just to declare the separation immaterial – of the cases in which the intervening term is added externally or in an intermediate position. This Aristotelian strategy is not new: see, in a different context, *Phys.* VI.2, 233b2-11 (compare the discussion in KNORR 1982, note 19 at p. 120).

[48] This fact had already been recognized by WAITZ: "Hoc quidem certum est, sed miror, quod in numerum conclusionum non amplius inquisivit. [. . .] Sit numerus propositionum = n, numerus terminorum = $n + 1$, erit numerus conclusionum $n(n - 1)/2$". (WAITZ, *Organon*, I, *ad* 42b25, p. 441). Now, this proposition can find a convenient formalization just in the way proposed in FOWLER 1994 (see note 19 above). I think this could be a good (albeit very tenuous) textual link missing in FOWLER's reconstruction.

[49] The presence of the standard verbs ὑπερέχω and προστίθημι, instead of the highly unusual πλεονεκτέω and προσγίγνομαι, should be enough. In my opinion, this lexical nor-

structure of the argument. Among the conceptual points ARISTOTLE alludes to (relationship even-odd and possible relevance of the position of the interpolated term), PLATO does not deal with the second point, which could have been relevant for him (the first is not relevant). Perhaps such an accessory discussion could have slowed down rhythm and cogency of PARMENIDES' reasoning.

g. Collecting the remarks made on both texts, the individuation of the environment in which to place a possible *Ur-text*, as well as the contents of such an "archetype", is almost forced: a Pythagorean source is very likely. As a further point, I only remark the coherent and systematic use, in the passage from the *Analytica*, of terms like διάστημα, ὅρος,[50] παρεμπίπτω,[51] προστίθημι, and the introduction, resulting again from ARISTOTLE, in a context dealing with the properties of odd and even. A thorough discussion of such a question would take us outside the scope of the present paper, for instance to completely rethink the connection between Aristotelian syllogistic and Pythagorean arithmetic, as well as to reconsider the alleged narrowness of interest and absence of nontrivial results elaborated by "ancient Greek mathematics" in the field of combinatoric analysis.[52]

4. Complete induction and the 'sorites'

A certain number of propositions contained in ancient mathematical works have been alleged as instances of proof by CI. I list here some of them:

Elem. VII. 3, 14, 17, 35; VIII. 13; IX. 8, 9, 20 (STAMATIS)[53].

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malization reflects a closer agreement with the original text, whose range of terminological possibilities easily included all the relevant terms (see next remark), rather than a process of refinement of the technical lexicon which had occurred in the – comparatively – few years which separate these works of PLATO and ARISTOTLE.

[50] The importance of these two terms in the field of the musical theory inspired by Pythagorean ideas and of the researches on the properties of ratios between integers is evident. It is enough to read fragment B2 DK of ARCHYTAS, or the *Sectio canonis*. A classical discussion of the terminology can be found in SZABÒ 1978, pp. 103–119. For the peculiar lexicon introduced by ARISTOTLE in his theory of syllogism see for instance EINARSON 1936 or SMITH 1978, but the whole subject is worth a more detailed analysis, which I plan to undertake elsewhere. The insertion of the *Sectio canonis* in the Pythagorean tradition is a well-established fact. See for instance BARKER 1981, or FOWLER 1987, pp. 143–153.

[51] ἐμπίπτω is the technical term used when referring to the *interpolation* of a mean proportional. It is employed, for instance, in this sense in *Elem*. VIII. 8–10, 19–21, 24–27. It is present also in prop. 3 – an earlier version of which is ascribed to ARCHYTAS by BOETHIUS – of *Sectio canonis*; and ARISTOTLE makes use of it *with this meaning* in *An. post*. 84b12. For a full discussion of BOETHIUS' "translation" of ARCHYTAS' result and proof see KNORR 1975, pp. 212–225.

[52] But see STANLEY 1997 for a first, strong indication in an exactly opposite direction. This paper analyses PLUTARCH, *Table-Talk* VIII. 9, 732f. Contrary views are expressed in BIGGS 1979, while in ROME 1930 is said that an attentive "depouillement des 'mathematici graeci minores"' is needed.

[53] See note 3.

Elem. VII. 3, 27, 36; VIII. 2, 4, 13; IX. 8, 9 (ITARD)[54].
PROCLUS, *In Platonis Rem Publicam*, II. 27.1–29.4 (Kroll)[55]. (FREUDENTHAL)[56].

I do not want to embark on a detailed analysis of these texts: FREUDENTHAL 1953 and UNGURU 1991 will suffice to this end. The answer is always the same: they *are not* well formed examples of proofs by CI, not even in the very "relaxed" meaning of this expression advocated in FOWLER 1994.[57] Typically, the proofs are framed in a quasi-general way, with instantiation of a few cases and subsequent (implicit) appeal to the generality of the scheme of proof.[58] In several instances, notably some among those connected with the property of side and diagonal numbers, the proof itself is altogether lacking.

I shall focus my attention, instead, on a different issue. I take as a *datum*, on the basis of the analysis carried on in the preceding sections, that proofs in the scheme of CI had actually been developed in a pre-Euclidean arithmetical context, and that their demonstrative structure could have represented (as is attested by the care PLATO employs in stressing the relevant steps) a sort of logical challenge. I assume as a further *datum* that proofs by CI are absent from later mathematical works, and that the absence of CI, even in contexts in which we should regard as "natural" its application, is a strong indication that the generality of this proof technique had not been understood. I leave to others the task of *explaining* this fact.[59] Rather, I shall now briefly discuss a piece of evidence supporting my contention that proofs by CI actually existed but that their potentialities were misunderstood. DIOGENES LAERTIUS and SEXTUS EMPIRICUS report the following forms of the paradox known as the "Sorites":[60]

> It cannot be that if two is few, three is not so likewise, nor that if two or three are few, four is not so; and so on up to ten. But two is few, therefore so also is ten.[61]

> [T]he Sage [. . .] will assent to "Fifty-one is few"; for there is nothing between this representation and that of "Fifty is few". But as "Fifty is few" was the apprehensive repre-

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[54] See note 3.

[55] Or THEON OF SMYRNA, *Expositio Rerum ad Legendum Platonem Utilium*, 42.10–45.8 (Hiller), or IAMBLICHUS, *In Nicomachi Arithmeticam Introductionem Liber*, 91.3–93.6 (Pistelli-Klein). These passages discuss the construction of successive side and diagonal numbers.

[56] See note 2.

[57] But fragments like ARCHYTAS' A24 DK and ZENO's B1 DK could be worth a more detailed discussion.

[58] This step of the proof is usually verbalized through the repetition of the relevant part of the πρότασις, preceded by the adverb ὁμοίως. A paradigm instance of this way of proceeding is represented by *Elem*. IX. 8. PROCLUS concludes his quasi-general proof with "καὶ ἀεὶ οὕτως" (II. 29.4).

[59] I only remark that textual tradition has been strongly selective in this respect, having *almost* completely erased information on what we could call "techniques of combinatoric analysis", even developed in a very simple form (but see note 52 for textual support to the fact that techniques of this kind should have existed).

[60] I am indebted to Lucio Russo for illuminating discussions on this point. For a thorough analysis of the 'Sorites' in the Hellenistic philosophical tradition see BARNES 1982 and BURNYEAT 1982.

[61] English translation from DIOGENES LAERTIUS, II, p. 191 (VII. 82).

> sentation placed last in order, “Fifty-one is few” is the first non-apprehensive one. The Good Man, therefore, will assent to the non-apprehensive representation “Fifty-one is few”. And if he will assent to this as being in no wise different from “Fifty is few”, he will assent also to the non-apprehensive “Ten thousand is few”; for every non-apprehensive representation is equal to every other non-apprehensive representation.[62]

Remarks

1. In the above examples the numerical variable labelling the property coincides with the “objects” the property *directly* applies to. This fact links the object language with the metatheorical level in the same way as we have observed in PLATO. Notice also that in this case the numbers involved are never “numbers of”.

2. The property under consideration (very often a variation on $P(n) \approx$ “n is few”) is what we could call nowadays a “vague concept”.[63] Clearly, CI does not work for vague concepts. The difficulties connected with such a kind of predicates, and an inability to clearly circumscribe the most general *genus* of predicates for which CI does work could have resulted in (explicitly or implicitly) deeming this type of reasoning unreliable in the context of a proof. The coherent use of quasi-general proofs in books VII-IX of the *Elements* is, very likely, an editorial choice which entailed a standardization of later works. Moreover, the paradoxes themselves could have been built, and can be construed as, an explicit critique to inductive proofs: they display, exploiting the weak point of the argument by unduly stretching the set of allowed predicates, the typical features of an argument *ad hominem*.[64]

3. PLATO sets up a correct proof by CI but is wrong in its application. He somehow reverses the ascending direction of the argument, referring the property just proved to an integer which cannot be reached by the induction. Some versions of the “Sorites” make appeal to a heap (the Greek name has this origin), from which a unit is subtracted in succession.[65] Here the vague concept is that of “heap”, and the inductive chain is

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[62] English translation (slightly modified) from SEXTUS EMPIRICUS, p. 225 (*Adv. Math*. VII. 418–419). Other *loci* in SEXTUS are *Adv. Math*. IX. 182–190, *Hyp. Pyrrh*. II. 253, III. 80. See also CICERO, *Acad*. II. 93, GALENUS *On Medical Experience* 17.1–17.3 (extant only in Arabic; cited in LONG-SEDLEY 1990, vol. 1, pp. 222–223). For a complete list see BARNES 1982.

[63] The “Sorites” itself often has a new name: it is called Wang’s paradox, the only difference being that it is now stated with an *explicit* use of CI, and so the conclusion “n is few” follows for *every* integer. In my opinion, this is an instance of the modern tendency to attach a content to formal manipulations: Wang’s paradox clearly displays an undue application of CI. The “Sorites”, insofar as it does not make use of CI, only allows for a comparison of a pair of well determinate integers. For a discussion of Wang’s paradox, see for instance DUMMETT 1978. See also *The Monist*, **LXXXI** (1998), dedicated to *Vagueness*.

[64] Recall that EUBULIDES of Miletus is credited to have put forward arguments in dialectical fashion, among which the “Sorites” is included. EUBULIDES was contemporary of ARISTOTLE and pupil of EUCLIDES, the founder of the Megarian school (see DIOGENES LAERTIUS, I, p. 236 (II. 108–109)).

[65] See HORATIUS, *Epistulae* II. 1.34 ff., GALENUS *On Medical Experience* 20.3.

descending. Clearly, a descending inductive chain is nothing more than a shorthand, standing for a *finite* number of identical steps. Failure of distinguishing between ascending and descending inductions amounts to a weakening of the logical independence of CI, and reduces it to a notational device, eventually superseded by the method of quasi-general proofs.

The preceding remarks strongly suggest that the "Sorites" paradoxes originate from a (tendentiously) wrong use of a mathematical demonstrative technique, which disappeared early from the available supply of proof schemes: CI, in the form we have seen at work in the *Parmenides*, is a good candidate for it.

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Sources and works cited

ARCHIMEDES, *Opera mathematica*. Ed. I. L. Heiberg. 2 ed. 3 voll. Leipzig, B. G. Teubner, 1910–15. (Reprinted 1972)

ARISTOTELES, *Analytica priora et posteriora*. Recensuit brevique adnotatione critica instruxit W. D. Ross, praefatione et appendice auxit L. Minio-Paluello. Oxford, Oxford University Press, 1964.

ARISTOTELES, *Physica*. Recensuit brevique adnotatione critica instruxit W. D. Ross. Oxford, Oxford University Press, 1950.

ARISTOTLE 1938, *Prior Analytics*. Edited and translated by H. Tredennick. Cambridge (Mass.), Harvard University Press.

BARKER A. 1981, Methods and aims in the Euclidean Sectio Canonis. *Journal of Hellenic Studies*, **101**, pp. 1–16.

BARNES J. 1982, Medicine, Experience and Logic. In *Science and Speculation*. Edited by J. Barnes, J. Brunschwig, M. F. Burnyeat, M. Schofield. Cambridge, Cambridge University Press, pp. 24–68.

BELLISSIMA F. - PAGLI P. 1996, *Consequentia mirabilis*. Firenze, Leo S. Olschki.

BIGGS N. L. 1979, The roots of combinatorics. *Historia Mathematica*, **6**, pp. 109–136.

BONITZ H. 1870, *Index Aristotelicus*. Ed. H. Bonitz. Vol. V in *Aristotelis Opera*. Ed. I. Bekker. Berlin (Academia litterarum regia borussica), G. Reimer. (Reprinted: Berlin, De Gruyter, 1961)

BURNYEAT M. F. 1982, Gods and heaps. In *Language and Logos*. Edited by M. Schofield, M. C. Nussbaum. Cambridge, Cambridge University Press, pp. 315–338.

BUSSEY W. H. 1917, The Origin of Mathematical Induction. *American Mathematical Monthly*, **24**, pp. 199–207.

CAJORI F. 1918, Origin of the name Mathematical Induction. *American Mathematical Monthly*, **25**, pp. 197–201.

DIOGENES LAERTIUS, *Lives of Eminent Philosophers*. With an English translation by R. D. Hicks. Cambridge (Mass.), Harvard University Press, 1931.

DUMMETT M. 1978, *Wang's paradox*. In *Truth and Other Enigmas*. London, Duckworth, pp. 248–268.

EINARSON B. 1936, On Certain Mathematical Terms in Aristotle's Logic. *The American Journal of Philology*, **LVII**, pp. 33–54; 151–172.

EUCLIDES, *Elementa*. Post I. L. Heiberg edidit E. S. Stamatis. 5 voll. Leipzig-Stuttgart, B. G. Teubner, 1969–1977.

FOWLER D. 1987, *The Mathematics of Plato's Academy*. Oxford, Clarendon Press.

FOWLER D. 1994, Could the Greeks Have Used Mathematical Induction? Did They Use It? *Physis*, **XXXI**, pp. 253–265.

FREUDENTHAL H. 1953, Zur Geschichte der vollständigen Induktion. *Archives Internationales d'Histoire des Sciences*, **6**, pp. 17–37.

ITARD J. 1961, *Les livres arithmétiques d'Euclide*. Paris, Hermann.

KLEIN J. 1968, *Greek Mathematical Thought and the Origin of Algebra*. Cambridge (Mass.), MIT Press.

KLEENE S. C. 1952, *Introduction to Metamathematics*. Amsterdam, North-Holland; New York, Van Nostrand.

KNORR W. R. 1975, *The Evolution of the Euclidean Elements*. Dordrecht-Boston, D. Reidel.

KNORR W. R. 1978, Archimedes and the Pre-Euclidean Proportion Theory. *Archives Internationales d'Histoire des Sciences*, **28**, pp. 183–244.

KNORR W. R. 1982, Infinity and Continuity: The Interaction of Mathematics and Philosophy in Antiquity. In *Infinity and Continuity in Ancient and Medieval Thought*. Edited by N. Kretzmann. Ithaca, Cornell University Press, pp. 112–145.

LONG A. A. - SEDLEY D. N. 1990, *The Hellenistic Philosophers*. Cambridge, Cambridge University Press.

LSJ H. G. LIDDELL, R. SCOTT, H. S. JONES, *A Greek-English Lexicon*. Oxford, Clarendon Press 1983.

MUELLER I. 1981, *Philosophy of Mathematics and Deductive Structure in Euclid's Elements*. Cambridge (Mass.), MIT Press.

NETZ R. 1999, *The Shaping of Deduction in Greek Mathematics*. Cambridge, Cambridge University Press.

NEUGEBAUER O. 1969, *The Exact Sciences in Antiquity*. New York, Dover.

PASCAL B. 1954, *Œuvres Complètes*. Édition établie et annotée par J. Chevalier. Paris, Gallimard.

PLATO 1906, *Parmenides*, in *Platonis Opera*, vol. II, recognovit brevique adnotatione critica instruxit J. Burnet. Oxford, Oxford University Press.

PLATO 1926, *Cratylus, Parmenides, Greater Hippias, Lesser Hippias*. With an English translation by H. N. Fowler. Cambridge (Mass.), Harvard University Press.

PROCLUS 1873, *In Primum Euclidis Elementorum Librum Commentarii*. Ex recognitione G. Friedlein. Leipzig, B. G. Teubner. (Reprinted: Hildesheim, Georg Olms Verlag 1992)

RABINOVITCH N. L. 1970, Rabbi Levi Ben Gerson and the Origins of Mathematical Induction. *Archive for History of Exact Sciences*, **6**, pp. 237–248.

RASHED R. 1972–73, L'induction mathématique: al-Karaji, as-Samaw'al. *Archive for History of Exact Sciences*, **9**, pp. 1–21.

ROME A. 1930, Procédés anciens de calcul des combinaisons. *Annales de la Société scientifique de Bruxelles*, **A 50**, pp. 97–104.

SEXTUS EMPIRICUS, *Against the Logicians*. With an English translation by R. G. Bury. Cambridge (Mass.), Harvard University Press, 1935.

SMILEY T. 1994, Aristotle's Completeness Proof. *Ancient Philosophy*, **14**, pp. 25–38.

SMITH R. 1978, The Mathematical Origins of Aristotle's syllogistic. *Archive for History of Exact Sciences*, **19**, pp. 201–209.

SMITH R. 1984, Aristotle as Proof Theorist. *Philosophia Naturalis*, **27**, pp. 590–597.

SMITH R. 1986, Immediate Propositions and Aristotle's Proof Theory. *Ancient Philosophy*, **6**, pp. 47–68.

STANLEY R. P. 1997, Hipparchus, Plutarch, Schröder, and Hough. *American Mathematical Monthly*, **104**, pp. 344–350.

SZABÒ A. 1978, *The Beginnings of Greek Mathematics*. Dordrecht-Boston, D. Reidel.
UNGURU S. 1991, Greek Mathematics and Mathematical Induction. *Physis*, **XXVIII**, pp. 273–289.
UNGURU S. 1994, Fowling after Induction. *Physis*, **XXXI**, pp. 267–272.
VACCA G. 1909, Maurolycus, the first discoverer of the principle of mathematical induction. *Bulletin of the American Mathematical Society*, **XVI**, pp. 70–73.
WAITZ TH., *Organon graece*, novis codicum auxiliis adiutus recognovit, scholiis ineditis et commentario instruxit Th. Waitz, 2 vols., Leipzig 1844–46. (Reprinted: Aalen 1965)

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