Lakatos Revisited - Monsters, Exceptions and other Ugly Things

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In the first part of my talk I tried to reconstruct the situation of solid geometry around 1800. The second (shorter) part was dedicated to a critical examination of Lakatos' "reconstruction" of the history of Euler's formula.

Some elements of a history of solid geometry

Solid geometry is treated by Euclid in the books XI to XIII of his "Elements". Roughly speaking he discusses there three majors topics: the basics of the geometry of space, problems of volume, and the morphology of solids - in particular of polyhedra.
In the context of the basics of solid geometry at least two points are remarkable to my opinion. First there is the well-known question how to define the notion of "congruence" (Euclid calls it "equal and similar") for polyhedra: "Equal and similar solid figures are those contained by similar planes equal in multitude and in magnitude." (XI, def. 10, cf. Heath III, 261). In this definition Euclid follows a strategy which he uses often when he is treating solid problems: try to reduce them to plane (and therefore simpler) ones; cf. his proof below that there are no more than five regular solids (XIII 18 = Heath III, 507sq). Euclid's definition was critized in the 18th century by R. Simson as not being compatible with the idea of superposition – being considered as the basis of congruence (as it seems to be the case for plane congruence). Take let's say a triangular pyramid. Glue to its bottom another triangular pyramid with congruent base but smaller height on it. Than there are two possibilities: we can glue the pyramid going "outside" or we can glue it going "inside" – we can add it or we can subtract it. The two resulting solids are congruent in Euclid's sense but they can certainly not be superposed. What is worse in respect to further arguments used by Euclid: they certainly don't have same volume. One way to repair Euclid's definition is to restrict it to convex polyhedra – an idea discussed at some length by Legendre. But than another problem arises: Can convex congruent polyhedra in Euclid's sense always be superposed? This is the content of the famous "rigidy theorem" demonstrated with some gaps for the first by Cauchy in 1812. In general later mathematicians tended to defend Euclid by stating that he was only concerned with convex polyhedra (cf. Legendre 1813, 324sq) We will soon see that this is not completely sufficient inasfar we have to extend congruence to include symmetry. The problem of solid congruence is discussed by Heller 1964 and Hon/Goldstein 2005; it should be noted that there are subtleties like "Miller's solid" (also called pseudorhombicuboctahedron) [cf. Hartshorne 2000, 463 or Cromwell 1999, 89sq] that make the situation even worse as one may expect.
The second problem could be called the "implicit use of three-dimensionality". Let's look at XI, 3 (Heath III, 276): "If two planes cut another, their common section is a straight line." This is only true in 3-space so, from our modern point of view, there should be an extra hypothesis stating this. If you believe that there is only three-dimensional space you may skip it as superfluous. This is the case as late as in von Staudt's "Geometrie der Lage" (1847) and Baltzer's "Elemente der Mathematik. Band 2" (1867). Von Staudt's proof in Baltzer's reconstruction goes like that (cf. § 30): Let point A be in the intersection of the planes p and p'. Take in p two straight lines BC and DE passing through A such that the points B and D are on different sides in relation to p'. Join B and D by a straight line in p. This line must cut the plane p' in a point F. So F is a point in the intersection of p and p'. Therefore the straight line

joining these two points is in the intersection. In comparaison with Euclid's proof it is revealing that von Staudt explicitly plays with the order in 3-space, order being an important theme of the geometrical investigations of the 19th century.
The problems of volume are attacked by Euclid in two ways: first by equidecomposability/equicomplementarity, second by exhaustion. I mention only one proposition (XI, 28 = Heath III, 330sq) in this context: "If a parallelepipedal solid be cut by a plane through the diagonals of the opposite planes, the solid will be bisected by the planes." Note that this implicitly presupposes that the diagonals of opposite planes are in a plane – a remark made by Clavius. The proposition is demonstrated by Euclid by showing that the polygons (triangles and parallelograms) which form the two prisms in which the parallelepiped is decomposed are pairwise congruent. Therefore the prisms are congruent so they have same volume. This is defective because the two prisms can't be superposed since they are in general symmetric in relation to a point. So one can not infer that they have same volume. This is more are less the first place where the "riddle of symmetry" shows up. (In principle it is already there in XI, 26 but in a more implicit form).
I now go directly to morphology. The central question of this is: what kinds of polyhedra (we restrict ourselves to them) are there? Clearly this should be prepared by defining exactly the relations between the polyhedra that are in play here. Relations were another big theme of the 19th century, Euclid remains quite silent on it. His central result and the culminating point of his "Elements" is the theorem (XIII, 18a = Heath III, 506sq): "No other figure, besides the said five figures, can be constructed which is contained by equilateral and equiangular figures equal to one another." The proof has two components: First we show that only five solutions (today called "Platonic solids") are possible, second we demonstrate that these five possibilities really exist by constructing the solids. The first proof is based on a reduction to dimension two (XI, 21 = Heath III, 309sq): "Any solid angle is contained by plane angles less than four right angles." Actually this is only proved by Euclid in the case of three plane angles forming a solid vertex, but this argument can easily generalised to more plane angles. This is important for the octahedron, the dodecahedron and the icosahedron. This point was discussed by Poinsot (1809) in connection with his stellar polyhedra.
The existence of the solids is proved by explicit constructions. Euclid's constructions in 3-space require a supplementary postulate not explicitly formulated by him: "Given three points not on a line. Then it is always possible to construct a plane through them." This is the only difference to plane constructions.
Substantial progresses beyond Euclid were made by Kepler who rediscovered and enumerated systematically the Archimedean solids as well as two stellar solids. Kepler was also interested in filling the plane and the space by regular figures; cf. his "Harmonice mundi" (1617). His methods were similar to Euclid's in particular he used also the dimension reduction. An important step was taken later by Euler. In connection with his temptations in the morphology of polyhedra he discovered his famous formula (1750) which stipulated further investigations in particular in the 19th century. With Euler began the combinatorial theory of polyhedra; his demonstration was one using solid method (reduction of the number of vertices by cutting off pyramids). Euler gave a complete enumeration (from the combinatorial point of view) of the polyhedra up to 10 faces (cf. Euler 1758).
With Legendre's "Eléments de géométrie" (1794) a new chapter in the history of solid geometry began. I can not give here a complete discussion of his contributions nor of their relations to precursors in particular to than onegoing research in crystallography (Hauy). So I will restrict myself to some remarks on the main important features:

1. Legendre pays much attention to the different types of relations between geometrical figures – so he coined the term “equivalent” to denote “having same area” and “equal” for congruent (cf. Note 1 “Sur quelques noms et definitions”).
2. Legendre integrates spherical geometry in his systems – it serves as the “missing link” between plane and solid geometry.
3. Legendre treats explicitly the case of symmetric polyhedra; for polyhedra he distinguishes in sum four relations: congruence, symmetry, equivalence and similarity.

Legendre shared the critics given by R. Simson; for him too superposition is at the root of congruence. In plane geometry the difference between congruence and symmetry (Legendre considers only symmetry only to a plane not to a point) is not important because symmetric plane figures can always be superimposed by a rotation about the axis in the space. This is no more true in space: symmetric polyhedra can not be superimposed by a rotation – this would require a four-dimensional space, an idea that Legendre doesn’t invoque at all. (There is an interesting passage (note to § 140) in Möbius’ “Barycentrischer Calcul” in which he states explicitly that this solution is not possible because there is no four-dimensional space.) Legendre goes on and tries to identify the true reason of symmetry. He finds it in the different arrangement of the faces of a polyhedron around a vertex: if there are the plane angles a, b and c around a vertex, then there are two different arrangements namely a, b, c and a, c, b giving rise to two symmetric polyhedral vertices. In general, symmetric polyhedra can be refered to a plane - their vertices being equally distant from it on different sides. It is interesting that Legendre gives an argument showing that symmetric polyhedra have equal volume. Legendre thus made the first attempt to grasp on a conceptual level the difference between two symmetrical polyhedra – remember that Kant used this difference to prove that the space is an intuitive and not a conceptual entity. The state of the art of classifying polyhedra around 1830 is nicely exposed in the article “Vieleckiger Körper” written bei Grunert for Klügel’s “Mathematisches Wörterbuch” (Grunert 1831).

The phenomenon of symmetry is also important in spherical geometry. Here it was discussed at the example of a spherical triangle and its diametral counterpart which could not be superposed; thus they are not congruent by SSS. It seems that this was first formulated by Segner (around 1760). Once again the important point is here the different orientation that is the different sequence of the sides of the triangle. Perhaps the most important feature in the context of spherical geometry in Legendre’s book is its use in the well known proof of Euler’s formula. Project the polyhedron on a sphere and calculate the areas of the resulting spherical polygons. Implicit in Legendre’s proof is the question not raised by its author himself: What kind of polyhedra can be projected like that? Or: Is it true that the projected polyhedron always covers the sphere exactly once? This problem was discussed by Poinsot in his work on stellar polyhedra, which contains examples where the projected polyhedron covers the sphere more than once. Poinsot deduced from that the idea to introduce a new term into Euler’s formula – thus opening the long discussion of “generalising Euler’s formula” (cf. Lakatos 1976). Note that Legendre’s idea of projecting the polyhedron requires that the latter is considered as a surface and not as filled up solid. This was an important step in the conceptualisation of the idea of a polyhedron.

In sum one can say that Legendre constructed the conceptual frame in which solid geometry could be discussed during the whole 19th century. This may explain partly the impressive success of this book.

I want to finish this short overview by mentioning Bravais’ article on symmetric polyhedra of 1849. As it is well known Bravais played an important role in the development of crystallography (cf. Scholz 1989) by introducing lattices and determinating their symmetry-

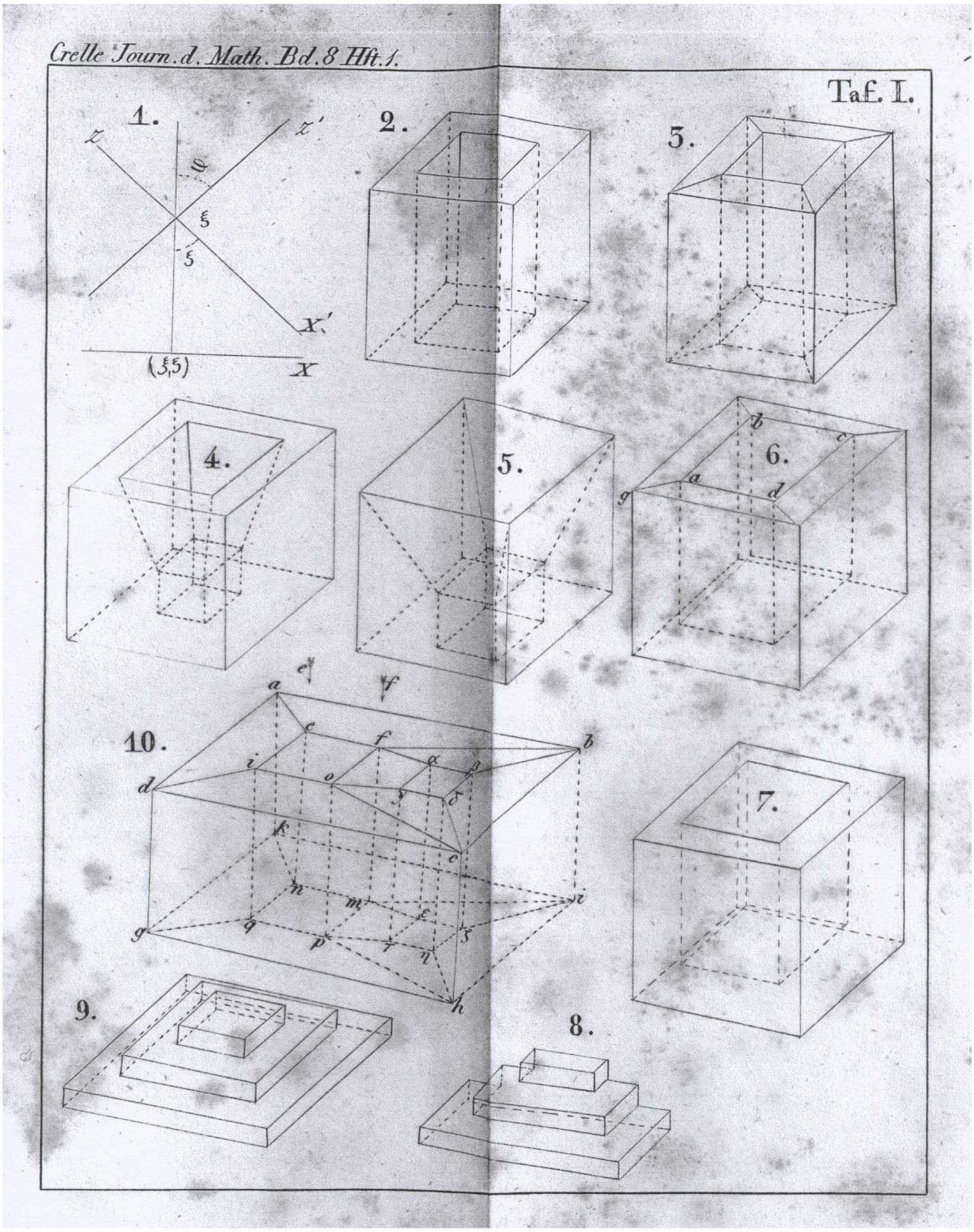
classes. This was preceded by an extensive study on the symmetry of polyhedra in which Bravais distinguished among others central and plane symmetry - called by him symmetry and inversion (he missed rotational reflection). Bravais' work is remarkable inasfar he managed implicitly motion in space without even having this notion! For exemple he proved that two central symmetries always differs by a translation, or, otherwise said, that the combination of two different reflections in a point is a translation. He arrived at this in considering the vertices of polyhedra and their images (Scholz calls this "the operational point of view", cf. Scholz 1989). In sum one may say that Bravais arrived at a rather complete understanding of the different types of symmetry – including rotational symmetry – and their mutual interdependencies, a result which could be easily reinterpreted in the realm of group-theory.
In literature the phenomenon of symetry was introduced by a mathematician – that is by Lewis Carroll writing "Through the Looking Glass" (1872).

Monsters, examples, and other ugly things

If one studies the history of the live sciences one may roughly state that the 19th century was the century of monsters. Certainly this statement is one-sided and exaggerated but it contains a true kernel. Before – let's say – 1750 "monsters" like Siamese twins (the famousest pair is perhaps Chang and Eng) or deformed people (remember the "Elephant man") were considered to have no scientific interest at all. They were outside the order of nature thus yielding no information on it. In the 19th century they became the object of an intense scientific interest, the Geoffroy Saint-Hilaire even proposed a new branch of science - the so-called teratology (from "tera" that is miracle) – designated to study them. The idea behind this was that the monsters show in particular clear way the functioning of nature – they are results of "to much" or "to little"., that is, they illustrate the ontogenesis by showing the consequences of disturbing it. In the line of thought Diderot one even tried to produce monsters in an artificial way (without great success) – in literature monsters like Frankenstein (1817) came up. Nota bene: Frankenstein is a scientific monster not mystic one like the old monsters of mythology (the hydra for example).
It is an interesting fact – at least in my opinion – that in the 19th century monsters appeared also in mathematics without calling them like that. There were two disciplines in which they played a prominent role during the 19th century: the theory of polyhedra and real analysis (both studied by Lakatos) – two disciplines in which there are many objects of a great individuality (cf. Poinsot).
The notions of "monster" and "teratology" were first used in the context of mathematics by Poincaré around 1890 who wanted to caracterize the than new way of doing analysis. He stated: "Logic sometimes makes monsters." (Poincaré 1998-99, 109) And "It is the beginner who has to be familiarised with the teratological museum." Prominent objects in this museum were continuous nowhere differentiable functions à la Weierstrass. The term "monster" became prominent with Lakatos who used to describe the tendency to exclude unpleasant examples as "pathological" (the so-called "monster-barring"). A more recent example are the "monster-groups", that is very very big groups.
Let us now turn to polyhedra again. Euler himself gave some examples (by drawings) of strange polyhedra fiúlfilling his formula. The first examples of non-Eulerian polyhedra appeared in the work of Poinsot (1809) and Lhuilier (1813)/Hessel (1832). Both Lhuilier and Hessel stated that their ideas on non-Eulerean polyhedra were stimulated by certain crystals:

“The exception I just mentioned should be presented by nature itself.” (Lhuilier 1813, 290) And Hessel cited the example of an opaque cube-shaped crystal inside a lucid cube-shaped one: it is evident that one has v – e + s = 4 in this case.

What is important here for my general point of view: These exceptions are not arbitrarily constructed by the free mathematical mind to falsify a statement - these exceptions possess a fundamentum in re. Neither Lhuilier nor Hessel spoke of a counter-example or even of a monster but simply of “exceptions” being “numerous” (Lhuilier) such that Euler’s formula “is not true in general but only for those polyhedra without re-entring solid angles.” Therefore mathematicians like Legendre – who had no assumptions on the polyhedra under consideration – “deduced general conclusions whereas one should them subordinate under a special point of view under which the treated subject is considered.” (Lhuilier 1813, 274).



Otherwise said: The existence of "exceptions" doesn't refute the general statement; exceptions are the motivation to think about additional hypotheses such that the too general statement is restricted to those cases in which it is true. The attitude towards exceptions at that period (and a fortiori before it) was more liberal, the dichotomy ("not for all" is equivalent to "there is one for that it is not") was not yet fully developed. If there are some exceptions but many good cases that's not too bad: "Other distinguished mathematicians (Legendre, Cauchy, Gergonne and Steiner) gave proofs for the general validity of this theorem. But it suffers exceptions." (Hessel 1832, 13). That is exactly the attitude of an historian of nature who discovers something unusual like the black sheep or the white raven. We can find it also in real analysis – cf. for example the famous article by Abel (1829) in which he discusses Cauchy's theorem stating that any infinite sum of continuous functions ist once again a continuous function. Like Lhuilier and Hessel Abel has found "exceptions".

We are here at the starting point of the intriguing history of Euler's formula and of topology – "reconstructed" by Lakatos. From a mathematical point of view, the key issue in it is the idea of simply connectedness. The polyhedron and all its faces should be simply connected. For these polyhedron Euler's formula is correct. A statement like that was first formulated by von Staudt in his "Geometrie der Lage" (1849, § 49). From a methodological point of view it's central issue – following Lakatos – is the application of the method of "proofs and refutations" that is the testing of general theorems on particular objects leading to refined statements – roughly speaking the application of Popperean falsificationism to mathematics.

Since I have here not the space to go deeper into the details I will only stress some points concerning Lakatos' position:

1. "Proofs and refutations" is not the method of mathematics but a method (among others) of it. It typically applies to disciplines with a high content of objects of great individuality.
2. "Proofs and refutations" presupposes an understanding of general assertations which is caracterized by an extensional, that is set theoretic, point of view. This was only elaborated during the 19th century, so Lakatos' analysis doesn't apply to elder mathematics at all.
3. "Proofs and refutations" is closely related to the idea that mathematics is a free construction of the human mind. The difference between "natural" or "genuine" mathematics and "artificial" mathematics is no more appreciated. This may be called following H. Mehrtens the "modern" understanding of mathematics.
4. The elder mathematics wasn't concerned with general statements true for all elements of a certain class but with statements true in some paradigmatic cases. (N.B. this is also true for the beginners in mathematics today.)

There is an obvious question: why these changes took place in the 19th century? We know that questions like this are difficult to answer and there is few hope to find a mono-causal explanation. A hint to a plausible answer is given by our point 3 above – mathematics as a free construction of the human mind. This idea was developed in the realm of the Humboldtean German universities of the 19th century and it is remarkable how many "german trained" (like Abel who was not a native German) mathematicians enters in our story. This is especially true for real analysis. A paradigmatic formulation of this idea was given by Dirichlet's famous dictum "for the glory of the human mind". I can't enter here into deeper details, but I want to indicate that there is a very interesting article by D. Bloor elaborating some ideas indicated here and many others too. Bloor's central thesis is: The method of proofs

and refutations is characteristic for a change in the structure of the mathematical community. It is related to the than new needs in communicating and producing mathematics.
Bloor's two-dimensional model is taken from anthropology (M. Douglas). This surprising fact is explained by the following parallel: "They deal with the way men respond to things which do not fit into the boxes and boundaries of accepted ways of thinking; they are about anomalies to publicly-accepted schemes of classifications." (Bloor 1978, 245).
Different social structures cause different reactions: for instance if mathematicians work in stable independent institutions there is the possibility to accept the co-existence of theorems and exceptions. In A they keep to the theorem, in B they study the exceptions. A principal difficulty of Bloor's model is obvious: the idea that there is an unique and general truth which cannot be relativized is crucial th mathematics. The truth in Paris must be the same as in Toulouse! So there is certainly further work to do on this point.
Whereas – following Bloor once again – mathematicians in an individualistic, pluralistic, competitive and pragmatic situation – shortly: in Popper's "open society" – tend to the method of proof and refutation. In such a situation pressure is exerted towards innovation and novelty and by this the transactions across the boundaries of existing classifications are encouraged, discontinuity is more desired than regularity and mistakes are tolerated, risks taken.
With these very short and rough remarks I stop here. I think that Bloor's idea is worth of further and deeper considerations. Leave it or love it!

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