Who Discovered Analytic Sets?

a *n answer to this question, which I will call Question 1, requires the study of a fascinating segment of the history of mathematics, connected with the names of P. S. Aleksandrov (1896–1982), F. Hausdorff (1868–1942), N. N. Luzin (1883–1950), and M. Ya. Suslin (1894–1919). Analytic sets are also called*

A-sets or Suslin sets. I have chosen the term "analytic sets" because of its neutral character.

In 1915–16, Luzin was a young professor at Moscow University. Aleksandrov and Suslin were his students. Luzin was an excellent mathematician. Even more important was the inspiration that he conveyed to his students, starting this way the astonishing ascent of Moscow mathematics. In what follows, I shall use the original papers [Aleksandrov, 1916; Hausdorff, 1916; and Suslin, 1917].

In 1915, Aleksandrov, in Moscow, and Hausdorff, in Bonn, were separated by the front line of World War I. Independently, they proved the continuum hypothesis for Borel sets B in $\mathbb{R}^n$, which asserts that each B either is countable or has the power of the continuum. Both men used a representation, by means of closed or open sets, of all Borel sets B of a transfinite class $\mathcal{B}_\xi$, $\xi < \Omega$. Each developed his representation incompletely, only as far as it was useful for the proof. Their formulas or methods, which depended on ξ, went only in one direction, from B to closed (or open) sets A_n, $n = 1, 2, \ldots$. And they could not be inverted; that is, they were not defined for arbitrary A_n.

A new student of Luzin, Suslin joined the investigation in 1916. As Luzin described it (see [4]), a natural question for himself and his two students was to describe Aleksandrov's representation formally. Of course, it would have

been desirable to find a solution that would produce all Borel sets and nothing else, and therefore would be independent of transfinite numbers; I will call this Question 2.

Partial answers were given by Suslin [15]. He proposed to relabel a simple set sequence $\{A_n\}$ in a "crazy way" as a "Suslin tree":

(1) $$\{A_{\nu_0}\} := \{A_{n_1, \ldots, n_k}\}_{k, n_1, \ldots, n_k = 1}^{\infty}.$$

This is possible because the set of all natural numbers $n = 1, 2, \ldots$ and the set of all finite sequences $\nu_0 = (n_1, \ldots, n_k)$ of natural numbers are each countable. We write $\nu = (n_1, \ldots, n_k, \ldots)$ for infinite sequences, and $\nu_0 < \nu$ if ν_0 is a beginning of ν. Suslin defined the set operation

(2) $$B = \bigcup_{\nu} \{A_{n_1} \cap A_{n_1, n_2} \cap \cdots \cap A_{n_1, \ldots, n_k} \cap \cdots\}$$
$$= \bigcup_{\nu} \bigcap_{\nu_0 < \nu} A_{\nu_0},$$

calling it the A-operation. The union is extended over all (uncountably many) sequences ν. For closed A_{ν_0}, the operation (2) generates all Borel sets, but also many non-Borel sets. This was a partial answer to Question 2.

Sets produced by (2), the *analytic sets*, created a sensation in set theory. Even formula (2) was unusual, containing an uncountable union. Up to then we shunned unions of this type, for they could easily lead to undesirable non-measurable sets. Hausdorff called (2) an $s\delta$ operation. For him, σ, δ stood for countable unions and intersections, respectively; s, d stood for uncountable unions and intersections. Thus, (2) cannot be written as

$$(3) \qquad B' = \bigcup_{\nu_0} \{A_{n_1} \cap \cdots \cap A_{n_1, \ldots, n_k}\}$$

for this is a $\sigma\delta$, not an $s\delta$ operation.

The important development of 1927 was the second edition of Hausdorff's *Mengenlehre* [6] with a masterful presentation of the theory of Borel and analytic sets in metric spaces. He called these sets "Suslin sets." In the following period, general set theory became fashionable. In the West, books by Luzin, H. Hahn, K. Menger, and K. Kuratowski joined Hausdorff in his assignment of priorities. This fashion was also featured in a few Soviet publications. The authors of the historically important long memoir about set operations, Kantorovich and Livenson [8] could not be called unfriendly to Aleksandrov. But they claimed that "the first known (not elementary) analytic [set] operation is the *A*-operation of Suslin. With it he introduced a new and wider class of sets, viz., the *A*-sets." Aleksandrov's friend Andrei Kolmogorov gave a very balanced and fair testimony. In his review of set theory in the book, *Mathematics in the USSR for 15 years* [12] we read: "Suslin applied procedures of Aleksandrov's 1916 paper to discover a new class of sets of fundamental importance—the *A*-sets" (p. 38), and "the theory of *A*-sets has been fast developed by Suslin's methods" (p. 45). As we shall see later, Kolmogorov's formulation is a good description of the Suslin-Aleksandrov controversy, except that it disregards Hausdorff's contribution.

I am not a stranger to analytic sets. In the 1930s I enjoyed the geometric exposition of the theory by Luzin [11], preferring it to the dry formulas of Hausdorff's book. But K. Zeller and I had to use the Hausdorff version when we wanted to apply it to summability.

The *Riemann convergence set* $R(\mathcal{A}) := \{s\}$ of a series $\mathcal{A}$: $\sum_1^\infty a_n$ with real terms consists of all sums $s = \sum_{k=1}^\infty a_{n_k}$ of convergent rearrangements of $\mathcal{A}$. The familiar Riemann's theorem describes all possible $R(\mathcal{A})$: this set can be empty (for instance if $a_n \nrightarrow 0$); it can be any one-point set (if $\sum |a_n| < \infty$); and it can be the whole real line.

Now let C be a series summability method defined by a matrix. Replacing convergence of $\sum a_{n_k}$ by its *C*-summability in the above definition, we get the *Riemann C-set* $R(C, \mathcal{A})$ of C and $\mathcal{A}$. To find the sets $R(C, \mathcal{A})$ for a given C is extremely difficult. But we proved (Lorentz and Zeller, [10]) that the set of the $R(C, \mathcal{A})$ for all C and $\mathcal{A}$ coincides with that of all analytic sets of the line. This was probably the first time analytic sets were used to resolve a concrete problem of analysis.

From the early 1920s, Aleksandrov occasionally claimed the *A*-operation as his. We now have new sources of information about the priority questions; they are pointing in opposite directions. Aleksandrov's reminiscences [2] were published in *Uspekhi Mat. Nauk*, a journal that he edited until his death. A second source is the book [4] which contains complete stenographic reports of Luzin's 1936 trial at the Soviet Academy of Sciences. Believed lost or destroyed by the participants, a copy was found in the Academy's archives in 1993. The published volume contains enlightening commentaries by eminent Russian historians of mathematics, S. S. Demidov and others. Here I shall examine only a small, but central and illustrative sector of the trial, the Luzin-Aleksandrov controversy about analytic sets.

Luzin suffered political persecutions at two critical periods of his life. In 1930, after returning from a long and fruitful sojourn in Paris, he was attacked by E. Kol'man, a leading member of Moscow's Party Council and a professor at the Communist Academy (see Shields [14]). With horror, Luzin saw his older friend Egorov disappear into prison and die shortly afterwards. Kol'man denounced the activity of Egorov and his friends Luzin and P. A. Florinskiĭ as "fascist-tainted reactionary science inherited from the old Moscow mathematical school." To him Luzin's mathematics were idealistic, that is, opposing Marxism's materialistic philosophy. Luzin's position at the university became precarious when he refused to join the signers of a propaganda letter directed against the "enemies of the people." Luzin fled the university, finding a niche at the Academy of Sciences. In addition to real functions and set theory, he turned to applied mathematics, with only moderate success.

Analytic sets are not always Borel.

Luzin's trial in June 1936 was an integral part of Stalin's Great Terror of 1936–37. Directed against all independent thinkers—in the Party, in the intelligentsia, and in the population in general—it took a staggering number of victims. Davis [3, p. 1325] estimates that one million persons were sent to concentration camps or executed during its worst year. In most cases, the victims did not even understand the reason for their arrest.

To initiate the campaign against Luzin in 1936, his enemies laid a cunning trap, prompting him to praise mathematical work at one of the less-than-average high schools in Moscow—praise that was then used against him. Vilification at universities throughout the nation and in newspapers followed, with eight full-sized articles in the leading daily, *Pravda*, with titles like "About the so-called Academician Luzin" or "Enemy in Soviet Mask." Then followed the trial at the Academy, conducted in secret.

Luzin had ample reasons to believe that he was fighting for his life. Indeed, the KGB had prepared compromising materials about him. His friend Florinskiĭ, mathematician, engineer, and orthodox priest, arrested in February 1933 together with a friend, was broken by the KGB. They con-

fessed to belonging to the KGB-invented "Party for the Rebirth of Russia," with a future "government" including Luzin as foreign minister and another mathematician, the academician Chaplygin, as prime minister (V. Shentalinsky, [13], pp. 111–115). This material, with potentially deadly consequences for Luzin, was never used.

Famous mathematicians formed the interrogating commission at the Academy's trial. Of these, Lyusternik, Shnirelman, and Gel'fond already belonged to the "initiating group" responsible for Egorov's downfall. They were joined by Sobol'ev. Luzin's former students were represented by Aleksandrov, Kolmogorov, and Khinchin. This revealed a split among Luzin's students: Lavrentiev and P. S. Novikov were present, but did not say a word against Luzin, a sign of civil courage, while Menshov and Nina Bari (one of the best Soviet female mathematicians) were missing altogether. Actually, Kolmogorov said very little. Among the full members of the Academy one saw the "red professor" O. Yu. Schmidt, later famous for his Arctic expeditions, the completely mute I. M. Vinogradov, and S. N. Bernstein, the only faithful and persistent Luzin defender.

Aleksandrov, who replaced Egorov as the president of the Moscow Mathematical Society, a post he was to hold for 32 years, was the natural leader of the anti-Luzin group and the most aggressive and sarcastic interrogator. Present at most sessions of the trial, Luzin had no legal counsel.

Luzin had a complex, sensitive, and highly excitable nature. His lectures were excellent, full of ideas, hypotheses, suggestions for investigation. He charmed people at the first meeting. Inspiring adoration by many of his students, he reserved his own for his French teachers Borel and Lebesgue. Sometimes he would attribute to them his own discoveries.

Aleksandrov was quite different. Having enjoyed a rich cultural upbringing, he was at home with literature, especially German, and theater. As rumor will have it, after his disappointment in Moscow in 1917–18, he seriously considered a theatrical career in the Western provinces, and he gave up the idea only because of the possibility of political problems under the Bolsheviks. Extremely ambitious, he befriended two of the best Soviet mathematicians, Uryson (who died prematurely in 1924) and Kolmogorov. With Uryson, he published joint papers and founded the Moscow topological school. He was a good lecturer, a witty raconteur, but his stature as mathematician was definitely below Luzin's. A strange antipathy, even hate, separated him from his teacher.

At the trial, Luzin stood accused of having plagiarized from his students, in particular, of having "borrowed" from Suslin the notion of analytic sets. Aleksandrov was deeply involved. Forty years later he declared: "For me the ques-

tion of priority in this case [of the A-operation and analytic sets] was never indifferent, concerning my first and (probably therefore) my dearest result" (Aleksandrov, [2], p. 235).

Terminology rarely plays an essential role in priority discussions. This case was an exception. As described in his autobiography, Aleksandrov [2] visited Hausdorff in Bonn in 1924. In his description we read: "To Hausdorff's question on how the new sets should be called, I firmly replied, Suslin sets, because he was the first mathematician proving that they are really new [and not just Borel] sets." By not suggesting that the defining operation is also Suslin's, Aleksandrov indirectly reserved for himself the credit for the discovery of the A-operation. In his book *Mengenlehre* (6), Hausdorff followed this advice only partly, calling both the sets and the operation (2) Suslin's. At the time of Luzin's trial in 1936, Aleksandrov, translating Hausdorff's book, completely changed Hausdorff's Suslin-terminology to A-terminology. This led to heated controversy between Aleksandrov and Luzin at the trial.

Even more interesting than the terminology are Aleksandrov's following statements. In his reminiscences [2, p. 235], he said categorically that "Suslin suggested the name 'operation A' for the new set operation I had constructed, and the name 'A-sets' for the sets which result from its application to closed sets. He stressed that he was suggesting this terminology in my honor." We compare this with Aleksandrov's words spoken at the 1936 trial ([4], p. 90): "He [Suslin] never told me that he called them A-sets in my honor. It was Luzin who formulated the term while lecturing at Moscow University. Incautiously, he underlined this." As a faculty member at Leningrad University in the 1930s, I heard two versions of what motivated Suslin to call his sets A-sets: (1) to honor Aleksandrov and (2) to parallel the common use of B-set for Borel set.

The strongest example Luzin's accusers could cite for his alleged plagiarism, a charge that eventually could not stand up at the trial, was the following. Suslin's expressions of deep gratitude to his teacher Luzin in the introduction of his paper [15] were interpreted as signs of Luzin's plagiarism, implying that they must have been written under pressure by him. Vehemently denying this, Luzin insisted that Suslin wrote the introduction alone. To this and other similar arguments that could be neither proven nor disproven, Aleksandrov offered Luzin mocking advice: "As a sign of our past friendship, allow me, your former student who will be grateful to you all his life, to give you in this difficult moment a really sincere [piece of] advice. You would do much better to give up hotly defending your rightfulness in cases when [defense] is impossible and to find the necessary courage and humility to accept the accusations against you."

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It is very fortunate for our inquiry that cooperation between Luzin and Aleksandrov during 1915–16 was also discussed at the trial. According to the record ([4], p. 89, p. 159), Aleksandrov expressed profound thanks to his teacher for the proposed subject for investigation, but minimized his contribution. Luzin was bound by the unwritten rule that demanded from a doctoral supervisor (which he in essence was) that the teacher never divulge his part in the joint work. At the trial Luzin implied that he had never done this before and was doing it only under the pressure of accusations. We can believe his testimony because he would have been foolish to insult Aleksandrov and many of his assembled students by misrepresentation.

This is what Luzin ([4], pp. 160–161) said to Aleksandrov in my free translation:

During 1915 you always came to my dacha with pages of incorrect attempts which I revised. In spite of my concerns, by means of tables of sets, a proof emerged for the Borel class $\mathcal{B}_4$. I asked you to do this for the general case. After joint work, a transfinite proof appeared. The reduction to one table [of sets] was entirely mine. (This probably meant the second table of Aleksandrov [1].) Afterwards, do you know what problem arose? How can the representation table of a Borel set be reconstructed? This was completely my problem [Luzin's problem was one of the formulations of our Question 2]. We both worked on it. But then you asked to be excused because of the difficulty of the problem. I still possess a postcard where you wrote this. Exactly at this point, at this second table, the work of us three [Aleksandrov, Suslin, Luzin] intermingled. This allowed me to say in my lectures that it remained for you to make a small step, and the discovery [of the operation A] would be yours. But neither you nor I made this step.

"I do not deny this," replied Aleksandrov.

Aleksandrov's admission proves that he was not the discoverer of operation A. Suslin was, and he gave a partial answer to Question 2: Applied to trees (1) of closed sets, this operation produces all Borel sets, but also non-Borel sets. Seven years later, in 1923, Luzin and Sierpiński gave a complete answer to Question 2. Operation A produces all Borel sets and these only if it is restricted to trees for which all terms in the union (2) are disjoint.

How did the cooperation of Luzin and Suslin develop afterwards? Luzin did not say. We can assume that he suggested his student answer Question 2. The title of Suslin's paper (which many find inappropriate), "On a definition of B-measurable sets without transfinite numbers," clearly indicates such a suggestion. But it is useless to guess about the extent of their cooperation.

When and why did this deep animosity between Luzin and Aleksandrov develop? Aleksandrov indicated that it began in 1923, when Luzin, chief editor of the *Mat. Sbornik*, invited contributions by his friend Uryson to the journal,

but not by Aleksandrov. (At that time Luzin was more powerful than Aleksandrov; in 1936 the relation was reversed.)

More likely, the aversion started as early as 1916, when Luzin accepted Aleksandrov's resignation from the triumvirate too easily, and helped Suslin to prepare his paper. Working alone on the general continuum hypothesis, Aleksandrov suffered a failure, and left for the Ukraine, returning to Moscow and mathematics a full two years later.

Another variant of the history of the Aleksandrov-Luzin relationship is even grimmer. In Leningrad many mathematicians believed that Aleksandrov was homosexual, a criminal offense in tsarist Russia, as well as in Soviet Russia, although rarely prosecuted. Perhaps Luzin had offended his sensibilities in this connection. A note accusing Luzin appeared in a public statement by Kolmogorov at Moscow University in 1936. He reminded the audience of Luzin's great service to mathematics "before his moral and political disintegration." This was echoed by Aleksandrov [2], when the author told that he "found his teacher in the highest sphere of human values, a sphere that he later abandoned." Aleksandrov quoted Goethe that "each guilt finds its revenge in life."
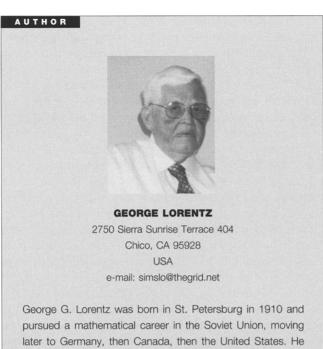
But I must discuss also the fourth participant on this scene. Hausdorff's role in the discovery of analytic sets was never properly described in the Soviet literature. The main difference between the two 1916 proofs was between the transparent Boolean set operations of Hausdorff and the "tables of sets" of Aleksandrov, inherited from a 1905 paper by Lebesgue. Furthermore, Hausdorff started with open sets in his construction, while Aleksandrov employed their complements—closed sets. This difference is not that important so far as Borel sets $\mathcal{B}$ are concerned. For analytic sets $\mathcal{A}$, the matter is different. It is known that the complement $C(A)$ of $A \in \mathcal{A}$ is also analytic only if A is a Borel set; in other words, that $\mathcal{A} \cap C(\mathcal{A}) = \mathcal{B}$. There is no real symmetry between the classes $\mathcal{A}$ and $C(\mathcal{A})$, however. Analytic sets coincide with the continuous images of Borel sets (Luzin); on $\mathbb{R}^1$, they coincide with the Riemann summability sets (see above).

Therefore we compute the dual of (2), obtained by taking complements. For the complement of A we get

$$(4) \qquad B =: C(A) = C\left(\bigcup_\nu \bigcap_{\nu_0} A_{\nu_0}\right) = \bigcap_\nu C\left(\bigcap_{\nu_0} A_{\nu_0}\right)$$
$$= \bigcap_\nu \bigcup_{\nu_0} B_{\nu_0}$$

where the $B_{\nu_0} =: C(B_{\nu_0}A_{\nu_0})$ form a Suslin tree. Formula (4) yields, with open B_{ν_0}, all sets B that are complements to analytic sets, and only these. Hausdorff [5] does not have (4), but Suslin trees are there, as are unions like $\bigcup_{\nu_0} B_{\nu_0}$ ([5], p. 436), absent from Aleksandrov's paper. It is easier to guess (4) from Hausdorff's paper than to guess (2) from Aleksandrov's. However, to get analytic sets, a complement must be taken.

Russian literature after 1990 about Suslin includes a

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good biography (Igoshin, [7]) and an article (Tikhomirov, [16]), "The discovery of A-sets." The conclusions of both authors, reached without benefit of the extensive new source [4], resemble those of Kolmogorov [12]. The proofs sketched in Tikhomirov's article are based on three essentially different definitions of analytic sets, and on the existence of universal analytic sets. In the collection *Kolmogorov in Perspective* ([9], p.4) A. N. Shiryaev refers to the new sets simply as "A-sets (analytic sets, introduced by Aleksandrov)."

We see that Aleksandrov came very close to what he had accused Luzin of, that is, to borrow from Suslin the definition of operation A. Suslin found it with some encouragement from Luzin. Hausdorff's attitude was commendable. Devoted to the readership of his books and ignoring petty concerns, in his *Mengenlehre* [1927] he gave

us a remarkable, impartial, and just exposition of the new theory.

The results of the Academy-based trial deserve a separate analysis in the English-language literature. It ended mildly for Luzin. Why was his life spared, why was he not expelled from the Academy? According to the editors of the *Delo* [4] he was saved by the highest Party echelons, perhaps even by Stalin himself. They insisted that accusations against Luzin should be formulated in academic rather than political terms. Accordingly, Aleksandrov stated a couple of times that Luzin's behavior displayed no anti-Soviet attitudes. The outcome of the trial suggested that mathematics was a cherished science of the Party. The Golden Years of Soviet mathematics, particularly in Moscow, had begun.

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