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ARMAND BOREL

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Armand Borel As I Knew Him. I do not remember exactly when I first met Armand Borel. It may have been in Paris in 1949 or in Zürich in 1950. After that we often met, and we soon became good friends. He liked to recall jokingly that he was the one from whom I learned that there existed five exceptional simple Lie groups, the study of which became for a while, shortly afterward, my special trade. According to accepted French usage between students and young scholars at the time, we always called each other by our family names, and I shall continue doing so here. A few years later, our friendship was extended to our wives, Gaby Borel and Marie–Jeanne Tits. Almost immediately, the four of us used the second person ("tu") to address each other, a quite unusual familiarity at the time between two French people of opposite sexes; there is no mystery, however: the Borels were Swiss and the Titses were still Belgian.

In the course of years, I came to know Borel very well and to appreciate his great qualities even more. All who have been in contact with him will agree, I believe, that among these qualities the dominant one was his exceptional earnestness, or maybe I should rather say conscientiousness. In German I would perhaps use the word "Gründlichkeit". This trait was always prominent when one worked with him; I shall give examples shortly when going over our joint work, but I had many other occasions to observe it: here are a few instances of diverse nature:

- when opinions were solicited for an appointment or an award, his reports were always highly appreciated for being the most informative and reliable, while being at the same time clear and concise;
- participants to meetings that were organized by him knew how seriously they were meant to work, but also how great was the reward; and, by the way, I suppose they had the same experience that I did, namely, that the proceedings were always published rapidly;
- in a completely different register (not totally disconnected from the previous one though), hiking was a serious matter for Borel; at the Oberwolfach meetings, he always belonged, of course, to the group of those who made the long walk (at high speed); I believe he felt that strong physical exertion was necessary to his intellectual activity;
- I believe that his knowledge of jazz and of a certain variety of Indian music happened at a professional level; I was unable to follow him there, but concerning classical music, my wife and I had with him very enriching discussions about the quality of performances we had heard together; he also introduced us to the art of his friend, the pianist Friedrich Gulda. The term "professional" used here could certainly be applied to most activities of Borel in any domain.

The tension that normally goes together with the extreme seriousness I have been talking about was probably responsible for a certain gruffness that some people

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resented but that was, in my experience, a purely superficial reflex. It was not rare that, to a question I had asked him, Borel first reacted as if I was offending him by asking something so elementary (or so stupid!). But immediately afterward came the answer to the question, perfectly clear and calm, exactly all I could hope for. A mutual friend of ours, who at first was very intimidated by Borel, had made the same observation. He told me that Borel on one occasion had asked him a question to which, for once, this friend knew the answer. To see what would happen, he then replied exactly as Borel would have done, and Borel accepted the scenario as the most natural thing in the world. But all this concerns relationships with equals and friends. The fact of the matter is that I never saw Borel react nastily to an expression of honest ignorance: he was always willing to explain things patiently to someone who did not know (I was often in that situation). On the other hand, he had very little tolerance for pretentious or arrogant attitudes.

It was not in Borel's habit to make inflated compliments; I already said that his professional evaluations were very much appreciated for that reason. But he was well capable of enthusiasm for beautiful achievements, in mathematics as well as in the arts, and he then expressed it with force.

His natural seriousness and his dedication to hard work should not overshadow the fact that he was—at least as I see him—of optimistic disposition, and that he liked to laugh (including about himself, as is illustrated by the quotation[1] of Bernard Shaw ending the introduction of [Œ 53]). In all our working sessions or during the many meals we had together, there were always long moments full of joy and good humor.

I have especially in mind our meetings in the late 1970s. Some of them occurred in Lausanne (or, more precisely, at La Conversion), where Marie-Jeanne and I enjoyed the wonderful hospitality of Gaby Borel. Alternatively, Armand could be visiting his mother in Paris and took advantage of this opportunity to come and see his friends, in particular my wife and me. The scheme was invariable: there suddenly came unexpectedly a ring of the phone; I answered and heard the unmistakable beautiful deep voice merely saying, "Allôô." This always unleashed mixed feelings: "What a nice surprise!" yet also, "How shall I manage to complete my (always urgent) program for the next few days?" Of course, the second problem got solved some way or other, and there remained only the pleasure of working and talking with him. The session of two or three days always ended with the ritual quotation: "When shall we three meet again?"

If I remember well, the last time we met was neither in Lausanne nor in Paris, but in Heidelberg after an editorial meeting. Shakespeare was not quoted that time, because, at the last moment, there arose a great confusion because Borel's hotel key had gotten lost. We separated in a hurry and we never met again.

Working with Armand Borel. It was great good fortune and a wonderful experience for me to collaborate with Armand Borel for many years: between 1965 and 1978. We wrote six papers together [Œ 66, 82, 92, 94, 97, 110]. Since their content is at least partially reviewed in the accompanying article by T. A. Springer, I shall restrict the technical part of my description to a few points that, in my opinion, deserve to be emphasized.

Our first and main joint paper, entitled "Groupes réductifs" [Œ 66], is also nicknamed "(le) Borel-Tits" by many of its users. Its main purpose is to set up the

[1] See footnote 7 of Tonny A. Springer's article

foundations of the relative theory of reductive groups; here "relative" refers to the fact that the ground field is not assumed to be algebraically closed. Major objects of that study include: tori, parabolic subgroups, the relative root system, the relative Weyl group, and the notion of split reductive group (a group having maximal tori that are *split*, that is, are direct products of multiplicative groups). The origin and circumstances of this collaboration are perhaps instructive: both Borel and I had already worked and published on the subject, which we approached with quite different backgrounds and aims; he was mainly influenced by Lie theory and algebraic geometry, and I by "synthetic" and projective geometry; he was primarily thinking in terms of tori and root systems, and I in terms of parabolic subgroups. However, our results were closely related and intertwined. In the winter of 1962–63, I was visiting the Institute for Advanced Study in Princeton, where he had been appointed permanent professor a few years before. We both knew of course that we had a wealth of common knowledge between us, but it took us quite a while to realize that the only sensible thing to do with it was to publish it jointly. The decision was made shortly before I left Princeton for Chicago in the spring of 1963, so that much of the work had to be done at later meetings, which we organized in Chicago and various other places, or by mail. I think that both of us learned much in the process: the resulting paper contained a lot more than what each of us knew before. Personally, besides great mathematics, I learned a considerable amount of writing technique. (I had a similar experience later on when I collaborated with Francois Bruhat; he and Borel, each one in his own way but both strongly influenced by the strict principles of Bourbaki's rigor, taught me how to write mathematics.)

Most of the results presented in "Groupes réductifs" have become widely known, and there is no need here to spend many words on them beyond the indications given in T. A. Springer's report. I wish however to make special mention of the main theorem of §7 of that paper, which went largely unnoticed, although Borel and I liked it and often referred to it in subsequent work: it states that any connected reductive k-group G contains a split reductive subgroup H, the root system of which is the system of "long" relative roots[2] of G.

One delicate point concerning the paper "Groupes réductifs" was the fact that its main results, while easily shown to hold over arbitrary perfect fields, could at first be extended to arbitrary fields only by using fairly deep scheme–theoretic techniques of Grothendieck. Borel disliked what he considered, I believe, a lack of proportion between means and aims, and was happy when he managed, in a joint paper [Œ 80] with T. A. Springer, to get rid of that disharmony (see the section "The Boulder Conference" in Springer's article on Borel).

When discussing the joint work by Borel and me, our paper [Œ 97] naturally comes second, considering both its length and the fact that its announcement [Œ 82] was our second publication chronologically. The paper [Œ 97] had its origin in a question that M. Goto asked us during the Boulder conference on algebraic groups in 1965. Roughly speaking, he asked: to what extent are the isomorphisms between the groups of rational points of two algebraic simple groups over two fields a combination of an isomorphism between the two fields and an algebraic ("morphic") isomorphism of the algebraic groups? We considered mainly the case of isotropic—that is, nonanisotropic—groups over infinite fields (although our paper also handles, but with different methods, the case of compact groups over nondiscrete local fields, where we slightly improve earlier results of É. Cartan and B. L. van der Waerden). The answer

[2] That is, when α and 2α are roots, α is to be dropped.

we gave to the problem is rather typical of what happened when one worked with Borel: no corner of the question was left in the dark. Instead of merely considering isomorphisms, we treated the case of homomorphisms with Zariski dense image, and the final result was characteristic free (to be sure, some special cases, especially in characteristic 2, were a bit of a headache, but under Borel's moral code nothing short of a complete solution could be satisfactory). Our main theorem included most earlier contributions to the subject and also answered open questions of R. Steinberg, among others. Talking about this paper gives an opportunity to emphasize how valuable Borel's encyclopaedic knowledge of the literature always was to ensure complete and accurate reference to earlier work and to avoid duplications.

Our collaboration on [Œ 92] was brought about by a fortuitous (and happy) circumstance. Here again, Borel's extensive knowledge of the literature played a role. Inspired by a recent paper of V. P. Platonov, he had written a note proving the analog for simple groups over algebraically closed fields of a classical result of V. V. Morozov: a maximal proper subalgebra of a semisimple complex Lie algebra either is semisimple or contains a maximal nilpotent subalgebra. Borel sent a copy of the manuscript of this note to a mutual friend of ours, who showed it to me. I observed that the argument of Borel's proof could be adapted to a considerably more general setting and provided results over arbitrary fields, for instance the following fact, which turned out to be of great importance in finite group theory: if G is a reductive k-group (k being any field) and U is a split[3] unipotent subgroup, then there exists a parabolic k-subgroup P of G whose unipotent radical contains U such that every k-automorphism of G preserving U also preserves P; then, in particular, P contains the normalizer of U. (Much earlier, I had conjectured this fact and given a case-by-case proof "in most cases", but the new approach, using Borel's argument, gave a uniform and much simpler proof.)

I mention *en passant* the complements [Œ 94] to "Groupes réductifs", containing results concerning, among other things, the closure of Bruhat cells in topological reductive groups and the fundamental group of real algebraic simple groups.

The last joint paper by Borel and me is a *Comptes Rendus* note [Œ 97] that in personal discussions we nicknamed "Nonreductive groups". Indeed, we had discovered that many of our results on reductive groups could, after suitable reformulation, be generalized to arbitrary connected algebraic groups. Examples are the conjugacy by elements rational over the ground field, of maximal split tori, of maximal split unipotent subgroups, and of minimal pseudoparabolic subgroups,[4] or the existence of a BN-pair (hence of a Bruhat decomposition). Complete proofs of those results never got published, but both Borel and I, independently, lectured about them (he at Yale University, and I at the Collège de France).

REFERENCES

[Œ] *Œuvres: Collected Papers*, Springer-Verlag, Berlin; vol. I, II, III, 1983; vol. IV, 2001.

[3]A unipotent group over a field k is said to be split if it has a composition series over k, all quotients of which are additive groups.

[4]Pseudoparabolic subgroups are a substitute for parabolic subgroups that one must use when dealing with nonreductive groups over nonperfect fields.