Marc Barbut The Historian

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Abstract

We examine the important role played by Marc Barbut in the study of the history of science. Marc Barbut was the main founder, and, over a period of thirty years, the main organizer of the Seminar on the history of Probability and Statistics at the EHESS. His personal work in history of science in the later part of his career is remarkably original. We will give a brief overview.

Résumé

Nous indiquons le rôle essentiel joué par Marc Barbut en histoire des sciences. Marc Barbut a été le principal fondateur et l'animateur pendant trente ans du Séminaire d'histoire du calcul des probabilités et de la statistique de l'EHESS. Ses travaux personnels en histoire des sciences, tardifs dans son œuvre, sont d'une remarquable originalité. Nous les rappelons rapidement.

It was a night in winter 1981, or in spring 1982, when the telephone rang at home, and for the first time, I heard, "Hello, this is Coumet." Coumet was always called Coumet on the phone, and Marc Barbut was Barbut. And so that's how it all started. In reality, Coumet and Barbut had already decided everything in advance. It was a question of organizing a seminar on the history of the probability calculus, in the widest sense, in the context of CAMS and of the Koyré Center, and I was asked to participate. How could I refuse Coumet and Barbut anything, men with whom I immediately got along? In more than 20 years of collaboration, I cannot remember a single instance of dissention among us. Rather, our "association" was constantly reinforced, deepened, and transformed into a warm friendship.

Coumet was a historian at heart, that sort of rare and sublime historian that you don't expect, a philosopher historian, but also a student of the long run, a bit in the vein of Guilbaud, with a sparkling, loving erudition, which says a lot about a long story. As for Barbut, I didn't know that he was interested in history; at least this wasn't immediately apparent. I knew, as did everyone, that he was a man of organization and of action and an activist mathematician, one of those who have the most talent for the diffusion and application of the science that he loved and in which he believed deeply. If he had decided to create a seminar on history, he surely had his reasons, which I didn't attempt to divine. It was only later that I discovered the limitlessness of

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Barbut's interest in history, in what it allows us to understand about mathematics, but also this demanding humanism that he practiced and sustained toward and against everything with rare loyalty and courage. And yet it was rather clear. Right away, one could understand that his vision of history greatly surpassed the framework of history that was practiced at the EHESS. One can read this between the lines in an assessment of his seminar that he drafted in 2006:

> We must emphasize that this seminar takes no position in the debate, largely false in our eyes, between internalist and externalist viewpoints in the history of the sciences, between history of scientific ideas and social history of the sciences. It is at the discretion of the lecturers, of their interests or their aims: certain among them give priority to the connection of ideas, others to the history of institutions where these ideas were developed, or to the histories of the men in whose heads they were born.

> We think, for our part, that these aspects are inseparable: there is no new idea that doesn't come in the wake of many other ideas; but all are born in brains that have encountered many other brains[2].

When the debates were beginning to degenerate into too-noisy "confrontations", Marc Barbut, with a talent and a firmness that no one contested, intervened so that the lecturer could finish his report in peace and so that the spirit of tolerance triumphed over the passions of the moment. He succeeded every time, an achievement continuously renewed. In this already, the seminar had something that was unique, at the heart of the learned Maison des Sciences de l'Homme. Marc Barbut had a surprising capacity to handle very heterogeneous groups of people, such as the audience at the seminar, to get them to work together on a common goal, and to make them achieve something. The abnormal longevity of the seminar is due primarily to his personal qualities. But this doesn't explain, for all that, the diversity of the subjects and the intellectual richness of the seminar that comes, without a doubt, from Marc Barbut's unique appetite for the history of probability and from his personal interest in "the history of quantitative methods and mathematics in the social sciences." We will try to see this more clearly, without really being sure of succeeding.

So at the beginning, there is mathematics. That's the starting point: the research and the teaching of mathematics practiced by Marc Barbut during more than sixty years, a long time — very long even on the historical time scale of the probability calculus, which began no doubt in the 17th century, but constantly had to begin again throughout its history, until the provisionally stable form it acquired in the 1950s, precisely when Marc Barbut was beginning his professional life. And what to say of the applications to the human sciences and of statistics in general, probabilistic or not, that was obliged many times over to battle against being totally eliminated, or else, when they had been eliminated, to be reborn from their ashes? Take the example of Bayesian statistics, which was extolled by the French school from Laplace to Borel and Darmois, the masters of Marc Barbut, on the same level, incidentally, as non-Bayesian statistics, and rejected without appeal by the dominant statistics (Fisher's and then Californian) until just recently, and which is barely emerging from a long purgatory. In these conditions, what perspective can one bring to a discipline so contradictorily appreciated? And above all, how to teach it? What does one teach? This was all the more in question because, beginning in the 1960s, a large pedagogical debate took root in the center of the mathematical community in France and abroad. How should mathematics be taught in general? It was an old debate, but one that took on a new form in those years. Must we continue to teach Euclidian geometry in the name of geometric intuition and a thousand-year-old tradition, or rather is it preferable to

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[2] M. Barbut [2006], p. 136.

introduce the pupils very early to the natural logic of sets and functions, and to the robust algebra of vector spaces[3]? In the case of new mathematical teaching in economics and in the curriculum of social sciences — psychology or sociology, disciplines already greatly mathematized across the Atlantic and now opening up to quantitative methods in France — how is one supposed to educate "literary" French students in mathematics? For example, how does one plan to teach probability and statistics to an audience that has not taken a mathematical curriculum in high school and for which numbers, fractions in particular, are a distant and uncertain memory? One could continue for a long time with these types of questions.

Marc Barbut found himself confronted with all these problems all at once, not only theoretically, but practically. Which mathematics programs, which manuals at all levels up to research, should be proposed to students at schools of law and letters who, until then, had spent little time on quantitative methods? These were programs and manuals of which Barbut was in charge and that he was supposed to put in place within the required administrative period. Marc Barbut wasn't alone, no doubt, but one imagines the difficulties, the doubts, that he must have overcome. We know that with Guilbaud, Gréco, Magnier, Revuz and others, Marc Barbut participated in the Lichernowicz commission beginning in 1967, which should have helped him with these decisions. But we also know that this commission, subjected to contradictory constraints, didn't really achieve anything and was followed by a period of pedagogical trouble and tension from which we haven't escaped[4]. In the first courses Marc Barbut published, where

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[3] Not to speak of affine spaces, where Désargues would have been at a total loss. The algebraic definition of the "affine line" did a lot of harm, we know, to those serious about the reform of modern mathematics in secondary education, to the point where it finally made them reject all forms of geometry. To a lesser extent, the "tribes" of events on a finite set have somewhat tarnished the modern image of probability, which, very thankfully, has come back from it. The ridiculous does not kill every time.

We hardly want to denounce entirely the modern mathematics movement, as one sometimes denounces Bourbaki, often the movement's face and purveyor in France. The preceding mathematics was not that exciting. Notably, its teaching resolutely left many of the pupils on the sidelines. One among a myriad of examples is Julien Green, whose fine mind is known, telling of his studies at Janson de Sailly high school around 1910: "In class, anything related to science or even simple arithmetic gave me an unease that quickly turned into anxiety, for it was impossible for me to understand what it was about and why it was necessary to cover the blackboard in figures. Where was the reason for all this? On the other hand, history fascinated me" (J. Green, [1963], p. 97). "Where was the reason for all this?" That, one remembers, is young Stendhal's question to his mathematics teacher regarding algebraic rules that were so mysterious [1890]. How can one respond to anxious students who ask the reason for all that? "Modern mathematics", with its insistence upon deduction and the logic of discourse, could seem like a remedy to arithmetic anxiety. To truly understand this, you have to have lived the militant and communicative enthusiasm of the first masters of modern mathematics. For example (one among a myriad), remember the mission of Adolphe and Claudine Festraets, who, between 1969 and 1973, put the reform of mathematics in place in eastern Algeria. These Belgian disciples of Georges Papy, under Algerian contract, with a limitless devotion, wrote, reproduced, and circulated millions of pages of course material to all levels of secondary education, without any support of any kind aside from their love of mathematics and the love that they had for each other. Adolphe Festraets (1923-1992), physics professor at the Athénée Royal of Ixelles, had participated in clandestine courses at the Free University of Brussels during the war. He had made his own ULB's ideals of liberty: "Scientia vincere tenebras", against the shadows of the Algerian military security and bureaucracy, and those of the religious fundamentalism already on display. As he had to fight upon his return to Belgium against the linguistic shadows that were succeeding in dividing the Belgian association of mathematics professors (of which Festraets was the last unitary president), modern mathematics had to be practiced thereafter in two separate languages, according to the geographic location of the pupils. Without visible success, other than their admirable life, the Festraets fully lived the epic of "modern mathematics" in its unique and exemplary aspects.

[4] See for example Chevallard [1992] and d'Enfert *et al.* [2010, 2011].

As for the teaching of mathematics for the social sciences, the work of the Lichnerowicz commission was long preceded by specialized colloquiums and commissions, notably the colloquium organized by the Center of Social

algebra is clearly dominant, one can notice a bit of these debates, even though the viewpoint that is adopted comes only marginally from a modernist will; you sense especially the will to be clear and efficient[5]. A historical reflection on these themes was certainly needed. And so, from the first years of the seminar, topics relative to the history of teaching statistics and probability were considered, with a particular insistence on questions of "foundations", but this theme was never exclusive, and Marc Barbut, who participated actively in the debates, never wanted to make it a priority. That's not how to reach the bottom of things. Marc Barbut's historical seminar always had a larger philosophy and larger objectives. Let's look at that.

First, a simple observation: If one looks at the list of Marc Barbut's publications, one observes that, until a relatively recent date, let's say 1995 for simplicity, the historical questions appear seldom and when they do, it's usually to illustrate a concept currently in his teaching program, without detailed historical analysis. What is more, in presenting the first number of the "bulletin" *Mathematics and Social Sciences*, Marc Barbut [1962(a)] emphasizes "pedagogy" and doesn't allude to history anywhere. One can read in multiple places this sort of historical reticence, common with mathematicians. History cannot be a goal in itself, nor even a suitable means. It is necessary to first teach the notions. Their history, when known, is second, subsidiary, and even somewhat blameworthy if one lingers over it too long and if one indulges in facile historical accounts that get away from what is essential. For example, in presenting essays written at the end of the 1960s, on the topic of Cantor's typology of total orders, Barbut makes a point of clarifying: "The present installment brings together two essay with pedagogical (not historical) aims, on total orders and their typology as it was developed in the last third of the 19th century, principally by the German mathematician Georg Cantor (1845-1918)" [2001]. Or again,

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Mathematics of the EPHE, 24th, 25th, and 26th May 1962, from which the journal *Mathematics and Social Sciences* emerged. See the account by Barbut [1962(b)], one of the principal organizers of the colloquium. There are numerous works on these topics, which we are not completely familiar with, and which we do not mention here so as not to be too long or appear too ignorant.

[5] With a particular insistence on first presenting the linear notions [1963], [1966], [1967], [1968]. For example, it is clarified ([1968], p. 1-2), that "the classic axioms of the probability calculus (Kolmogorov's axioms) are equivalent to giving a positive linear form on vectors of numerical risks". Following Pascal in this, Marc Barbut estimates that the linear notion of expectation is more intuitive and less abstract than that of probability, and that it provides a "means of initiation" that is more natural to probability. It's a point of view that he never stopped defending, seen in his beautiful article [2000]. For an alternative presentation of the same notions, in Barbut's journal, see Roy, for example [1963].

Barbut sometimes seemed to regret certain modernistic excesses [1984a], but could it have been otherwise? Remember that in the overheated intellectual atmosphere of the 1960s, a certain number of students and researchers, philosophers, psychologists, sociologists, anthropologists… wanted to reinvent themselves as mathematicians. The *Cours d'algèbre* by Roger Godement [1964], hastily revised, became, in spite of its author, a fashionable book that one had to have in one's library. Freud, Marx and Godement reigned for a moment over the republic of letters, quick to excite, a superficial reign no doubt, that could not affect the imperturbable course of things, but which one had to take into account to be credible. To get an idea of this fashion, one may consult e.g. Levi-Strauss [1954, 1964], who followed for a moment, without really catching on, the mathematics course that Guilbaud was giving to update his social science colleagues (so what to say about the reading of Godement?). Read also the nice study by Parlebas [2010], in which Marc Barbut of course appears.

On the other hand, Barbut very correctly emphasizes the "set-theoretic" and graphic (or geometric) presentation of combinatorics, which truly illuminates teaching of the topic and was not surpassed. See Barbut [1967] and Guilbaud [1963]. Let's draw the attention of the reader who would not yet know it to this last work, and notably its first part, which is splendid. Abundantly copied, it has never been equaled. The second part on the exponential calculus is no doubt less successful, but the subject, necessarily non-linear, could not be boiled down as brilliantly by Guilbaud's stunning pedagogy and breathtaking erudition. In this regard, Barbut is more realistic and undoubtedly better received in classes.

in the first study that he published comparing the notions of concentration of Gini and Lorenz on the one side and of Paul Lévy on the other, Barbut writes: "This study, which attempts to be purely empirical, exposits the methods by applying them to two sets of data." The emphasis is put on applications to the social sciences (the comparison of socio-economic status in the student population and in the working population) with a didactic goal, for an audience of professional sociologists, even though Barbut presents here a very interesting historical idea — a mathematical idea born in two different but neighboring forms, almost simultaneously and independently, in very applied mathematics and very pure mathematics — an entirely original idea, moreover, that had escaped the best historians in these disciplines[6]. Deep and overtly historical analyses would not appear under the pen of Marc Barbut until the end of the 1990s, when he no longer had any official teaching responsibilities. It is as if Marc Barbut's sense of duty, whose force and dependability is familiar to us, was preventing him from going beyond a simple allusion, incomprehensible to the largest number possible, from which he wouldn't take glory or vanity, as the goal was elsewhere and he had to dedicate himself totally to it.

But around him and from the beginning, everything was bathed in history. First, of course, with the presence of Guilbaud, the founding director of the Concordet Center, later CAMS, who was integrating elements of history into all his courses, writings and lectures, with erudition and a mind for history that many official historians envied. His analyses of Pascal or of Leibniz are magnificently intelligent and are still authoritative[7]. At Guilbaud's side, very present, were Father Costabel (1912-1989), his long-time friend, whose pre-eminent role in the history of the sciences for more than 40 years is known, and of course Coumet, at least after 1963. We are also familiar with the connections between Barbut and the historians of the 6th section of EPHE, later EHESS, Fernand Braudel or Jacques Le Goff in particular. What is more, despite its initial promises, the Bulletin *Mathematics and Social Sciences*, which Barbut directed, quickly opened up to the authentically historical studies of Guilbaud or Coumet, the logo of the review announcing already that it was putting itself under the historical protection of Desargues and of the convent of the Minimes of Paris[8]. It is obvious (and well known) that Barbut read and appreciated the historical works published in his journal or elsewhere. His decision to establish a historical seminar at the CAMS can be thus naturally explained, even if, at the beginning, he judged, by professional duty, lack of time or modesty, that he could not be enough of a historian to publish articles with an emphasis on history. In this he was wrong, so much so that the seminar could have equally been a way for Barbut to take on his appetite for history, to do it justice and to contribute somewhat to it.

But one doesn't get to the bottom of things in this way, nor to the richness of the themes Barbut addressed in his strictly historical works, belated as they were. In order to approach them, one can start by remembering that his father, Jean Barbut (1892-1983), was a student in the Ecole Polytechnique until 1913. Mobilized in 1914, he only went back to his studies at the Ecole

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[6] Barbut [1984(b)]. Also [1984(c)] which is even more laconic. Barbut writes simply in introduction: "The study of economic or social inequalities is at the same time one of the oldest and most recent purposes of the social sciences. Corado Gini's functions of concentration (and those of Paul Lévy) constitute an efficient tool in the analysis of these inequalities, and of their variations…"

One observes the same historical reserve in his articles of high popularization, e.g. [1959], where the emphasis is immediately put on "the mathematical aspects", even if the subject particularly lends itself to a historical analysis, Guilbaud [1961], Monjardet [2005].

[7] See for example Guilbaud [1954], [1961], [1962], [1964(b)] etc., Coumet [1964], [1965] [1966], [1970], [1972] etc.

[8] See Guilbaud [1964(a)] and Barbut [1999], [2007(b)].

Polytechnique in 1919. Thus he took the first course in probability taught by Paul Lévy, the notes of which he kept in his library. It is known that this course was very modern and inaugurated Lévy's work on probability [1922(a), (b), (c)], [1924], [1925], which would be pursued for a half century and would reach great heights [1934], [1937], [1948], [1970]. No copy of this course was known. To understand its value, one had to be a good enough historian of probability. Upon the death of his father, Marc Barbut inherited his library, but he was no doubt familiar with it before this. For a historian such as him, it was a gold mine. It contained for instance the second edition of the 1910 *Elements of the Theory of Probability* by Borel [1909], an unobtainable edition, and of course Levy's course from 1919, which Marc Barbut edited magnificently with Laurent Mazliak in the *JEHPS* [2008][9]. Each book from this library was a source of history. But even more, Jean Barbut seems to have kept up personal contact with Paul Lévy, to the extent that, when Marc Barbut — at the beginning of the 1950s, having finished his degree in Probability with an emphasis in Statistics at the Paris College of Sciences — requested a meeting with Paul Lévy upon the recommendation of his father, Lévy agreed whole-heartedly. It was an introduction to the major work of one of the greatest probability experts of the 20th century, an introduction with a human dimension, an influence found spread through Marc Barbut's historical works.

So Barbut met Lévy before even studying him, at a time when nobody was reading his works in France or even knew about him[10]. It was without surprise, therefore, that he learned the laws of "Pareto-Lévy" at the Guilbaud seminar in the 1950s, and, from then on, devoted himself to studying and to teaching them, finally making himself their historian when the time came. It was a matter of explaining the emergence of a new "universal law", the "Pareto law", on equal footing with the normal law, the first and since Quetelet seemingly the only universal law of statistics. To do this, he had to confront the works of Lévy, Fréchet, Pareto himself and others, and put them in perspective. This was the first thing to do. But he had to have also personally addressed and calculated multiple cases of applications of Pareto's laws, had to have interpreted, criticized, modified them, taught them dozens of times at all levels, in order to understand from

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[9] Remember that Marc Barbut and Laurent Mazliak in 2005 founded the *Electronic Journal of the History of Probability and of Statistics* and directed it together until 2011. The *JEHPS* is unanimously thought of as one of the most original and most interesting publications on the history of the sciences currently. We will not go into detail on this point, which has been very well described elsewhere.

[10] As one knows, no one in France in the 1930s really read Lévy, much less understood him, except for W. Doeblin who knew in advance what Lévy had written or would write, which helps a lot. Fréchet never completely understood Lévy and inversely; this is why their correspondence is interesting. See the magnificent edition that Marc Barbut, Bernard Locker and Laurent Mazliak did on this subject [2004].

In the 1950s, one can note another remarkable French exception, Benoît Mandelbrot (1924-2010), who was Lévy's pupil at the Ecole Polytechnique in 1945, and who, out of contrariness and without any encouragement from Lévy (nor any notice), quickly considered himself Lévy's "pupil". As with all imaginary parenthoods, this one had many consequences. In particular, it is at the source of Mandelbrot's first economic works. The latter, having begun by being interested in the Zipf law in textual statistics [1957], had learned at the Guilbaud seminar that this law was nothing but an avatar of Pareto's law of incomes. Mandelbrot knew Lévy's theory of stable laws [1923, 1925, 1934, 1937] (and he must have been one of the few in France at that time who knew this theory, if we exclude Fréchet, Darmois or Dugué, who don't seem to have seen the relationship with Pareto's laws). In any case, the closeness between the two theories was apparent to Mandelbrot right away. He undertook, with considerable energy, the study of the economic applications of the "Pareto-Lévy" laws (an eponym that Mandelbrot created), which benefitted from the theoretical strength of Lévy's stable laws and from the descriptive strength of Pareto's laws [1960]. One knows everything that Marc Barbut inherited from this double tradition, acquired at the same time, the end of the 1950s, and in the same place, the Guilbaud seminar at the EPHE-EHESS, as much for his teaching as for his research in statistics and history.

inside the difficulties, the traps, the *a priori* incomprehensible errors that made the greatest falter, starting with Pareto himself who "muddied about" a lot with "his" law, but also Fréchet and the others. So much so that the essay [1998(a)], already summarized in [1991], and developed in [2003], [2007(a)] and [2010], is truly a model of pedagogy and especially a masterpiece of conceptual history. To reach this level, it was not merely necessary for the author to know his subject, which is generally the case for conscientious historians; the author also and above all had to become his subject, dive into it, be a sort of man-history, more than a historian. He had to take part in the history he was retelling, as much as the scholars who had done it, without ever interposing himself between them and the course of things, without turning this into a celebration of himself, and this is infinitely rare. These works have been magnificently described, notably by Bernard Valade [2011]; it is useless to reiterate the point, but, without wanting to add anything to what has already been published, let's remember simply a seemingly minor detail that illustrates nevertheless Barbut's method of historical work and also his constant, renewed interest in these questions, despite everything and until the end.

As we have known for a long time, Pareto's laws do not fit income distributions in their totality very well. What is true for large incomes is not true for small ones, an objection Edgeworth had already formulated in 1896. So other laws were very quickly proposed, laws thought to fit the diversity of situations better; for example, Gibrat's log-normal law (which covertly reintroduced the universality of the normal law, Armatte [1995]). For his part, Fréchet [1939] had proposed fitting a Paretian law with negative index, which Marc Barbut [1998(b), 2007(a), p. 117] proposed calling a "contra-Paretian" law, to small incomes and a classical Paretian law to high incomes. To justify his proposal, Fréchet had fitting done at the Calculation Laboratory of the Paris Faculty of Sciences, which he had been directing at the IHP since 1941. The calculations that Villaret, the technician of the laboratory, performed proved disappointing, and Fréchet did not continue down this path — wrongly too, as this type of distribution fits the economic data very well, for example income taxes in France in 1975, which Marc Barbut considers in his beautiful Spanish article [2010]. In a letter dated February 2, 2010, Marc Barbut explains this point in the history of economics in the following way:

> Fréchet always wrote Pareto distributions in the form:
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> $P(x) = A/(x - x_0)\alpha$, (x_0 is the origin, $x > x_0$).
>
> When he was concerned with small incomes, he naturally wrote [the distribution function F, meaning 1-P(x)], $F(x) = B(x - x_0)^{\beta}$, $\beta > 0$.
>
> Then to state its distribution, he naturally wrote the second formula for $x_0 < x < \mu$, and the first for $x > \mu$.
>
> Out of habit, he wrote the same x_0, but in his mind, it probably wasn't the same.
>
> Then he gave the formulas to Villaret, so that the latter would perform the calculations to fit the empirical data. Villaret applied the formulas to the letter; this obviously didn't work.
>
> If Fréchet had performed the calculations himself (this wouldn't have been that difficult), he would have realized the error right away.
>
> This explanation seems highly likely to me. I have seen some other blunders, just as monumental, when people have their calculations done not by Villaret, but by the computer — that avatar of all the Villarets from here on.

To conclude this anecdote, let's add that Marc Barbut performed all his calculations himself, preferably "with a pocket calculator", "this sometimes prevents stupid mistakes," he wrote, even if it is "always long and tedious", ([2007(a)], p. 144).

But we haven't yet gotten to the bottom of things.

In 1957 or 1958, Barbut seems to still be hesitating on his orientation, which would not be definitive until after his appointment to the EPHE, in 1960, as study chief, then in 1962 as director of studies. He is still tempted by pure mathematical research. At Daniel Dugué and Georges Darmois's suggestion, he begins to study theoretically Paul Lévy's concentration functions, whose history is known. Around 1930, Lévy invested all his energy in Borel's "denumerable probabilities", which the Soviet school had developed in the 1920s. In particular, one wanted a definitive theory of series of independent random variables. Khintchine and Kolmogoroff demonstrated in 1925 that the probability of convergence of these series is always 0 or 1 and gave characterizations of these two situations, quickly improved by Kolmogoroff in 1928. So in 1931 Lévy set about giving a more natural interpretation (in his sense) of these results, in introducing what he calls the *dispersion* of a law of probability, and its inverse, the *concentration*. Traditionally, the dispersion of a law, its spread, or contrarily its concentration are measured by the variance, but the latter is only defined for the most regular laws, those for which we can talk about the mean of the squares of the deviations. This isn't always the case, far from it. In particular, Pareto's laws do not have variances, nor even generally means, the *medians* (a notion that dates back to Laplace at least, Barbut [1991]) having to be substituted for them. But the idea of dispersion or concentration is evidently general; it doesn't have anything to do *a priori* with the existence of moments. It expresses the greater or lesser spread of the probability. Lévy therefore proposes the following definition[11]:

> We will call function of concentration (or maximal concentration) of a random variable x the function:
>
> $$\alpha = f(l) = \text{Max } P\{x' < x < x''\} \qquad (x'' - x' = l)$$
>
> and function of dispersion, or simply dispersion, the inverse function…

The dispersion (like the variance) can only get larger with the addition of new variables. Lévy then shows that the Khintchine-Kolmogoroff conditions are explained "simply" with the help of the limiting function of dispersion.

For more than twenty years, Lévy's notion of concentration remained known to a select few, not to say totally unknown to the international mathematical community. It is in fact difficult to use for those who don't possess Lévy's vision. So much so that, in the new probability theory of the years 1930-1960, "analytical" methods long seemed very superior to "probabilistic" methods, Lévy's in particular. In any case, in 1958 few people had heard of them, apart from Lévy himself and maybe Dugué, who at the time was looking at Khintchine's and Lévy's arithmetic of probability laws, but by strictly analytical methods[12]. Dugué might have

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[11] Lévy [1931], chapter I, § 5. This notion is taken up again and studied in Lévy's great work [1937]. It was used by Doeblin in his first note [1936], signed with Lévy, and in most of his works of the end of the 1930s, especially [1939]. We do not know of other mathematical works about this notion before the end of the 1950s. See Hengartner, Theodorescu [1973].

[12] Dugué [1957] where it is never a question of concentration or dispersion, but only of characteristic functions.

discussed concentration functions with Lévy, with whom he had a good relationship, or with Darmois, who knew a lot of things. One could even imagine that Barbut was informed of the existence of Lévy's concentration functions by Lévy himself, even though we have no ounce of proof in this regard. Whatever the case may be, it seems that it was indeed Dugué who suggested to Marc Barbut that he examine the question of the inversion of concentration functions: a sufficiently regular function being given, determine all the laws that have it as their concentration function. The results Barbut obtained are of a beautiful elegance, and they have the distinction of having been presented at the Darmois seminar of the IHP in spring 1958 in the presence of Darmois, Fréchet, Lévy, and Kolmogorov, at the time a visiting professor in Paris. Here is a truly historical audience, which one imagines must have impressed the speaker but also Kolmogorov[13], who immediately took up his pen to add to the discussion from his point of view and to noticeably improve the Doeblin-Lévy concentration inequality, relaunching the study of Lévy's concentration and its applications, at least in Moscow. Parisian mathematics was then buzzing about topological vector spaces and homological algebra, and generally took the view that probability was empty of mathematical results (and that statistics was about estimating the height of the mainmast from the age of the captain).

Marc Barbut, who in his courses at the ISUP and elsewhere, was teaching Gini's curves and coefficients, could not fail to connect the two notions, coming from such different universes, the curves of Gini, the statistical economist who studied income inequalities, and the functions of Lévy, the pure mathematician who lived inside probabilistic inequalities, two ways to conceptualize the greater or lesser concentration of distributions, theoretical for the one, empirical for the other. Marc Barbut was indeed the only one able to make such a connection, so that he found himself to be at the same time one of the most intelligent theoreticians and the best informed historian about measures of statistical concentration and coefficients of inequalities. As

Marc Barbut seems to have gotten on well with Daniel Dugué. In 1958, when George Darmois left to retire, from the Parisian chair of probability calculus and mathematical physics, a rather lively competition occurred for Darmois' succession between Dugué, backed by Darmois who was his teacher, and Fortet, Fréchet's pupil. It was surely not easy to remain neutral in this matter. In the end Fortet was elected to Darmois's chair, Dugué having been named the director of the ISUP, not without consequences for the development of statistics and of probability in Paris in the 1960s, all the more so because these were not part of the Bourbakian choice, which then dominated without mercy the mathematics department at the Faculty of Sciences in Paris. On Fortet's remarkable work, see Brissaud [2002]. On Dugué, documents are rarer, but see the eulogy written by P. Deheuvels [1987].

[13] Barbut [1958/2002], and Kolmogoroff [1958(a), (b)].

In Paris, Kolmogorov had announced that he would offer a course entitled: "Spectral theory of dynamic systems and stochastic processes", which was supposed to be a sort of synthesis between his exceptional works on dynamic systems [1954] and the spectral theory of second-order processes developed in Paris by Blanc-Lapierre and Fortet [1953]. This course was never published. See Sinaï [1989] for Kolmogorov's works of that period.

Kolmogorov's first lecture took place at the IHP, Darboux amphitheater, Friday, April 25, 1958, at 3:30 p.m., in front of a sparse audience, among which was Marc Barbut. His notes have been preserved. They are very well taken. Kolmogorov evidently presents his entire course. But notes from the second lecture are not available. In truth, we do not know how many lectures were given. In fact, Kolmogorov, who was to have stayed in Paris until the end of summer 1958, left quickly for Moscow for an unknown reason. Perhaps the KGB had been informed about one of the many plots of May 1958 and feared that the most beautiful jewel of Soviet mathematics would be taken hostage by colonels little concerned with ergodic theory. Who knows?

Here was a lost opportunity, undoubtedly for the Parisian mathematicians, but also for Marc Barbut, who may have looked back on it nostalgically. This is an imagined story, as uncertain as can be, but of clear import. Kolmogorov's greatest Parisian regret seems to have been that he did not find any mathematician to go mountain climbing with him. On the other hand, he greatly enjoyed his visit to Rodin's house in Meudon. Kolmogorov and Aleksandrov's love for Rodin is otherwise known: Kolmogorov [2000], p. 156. A photograph of the "bronze age" was in Alexandrov's room in Komarovka, the house he shared with Kolmogorov.

in the case of the Pareto-Lévy laws, history and theoretical reflection again shed light on one another to the larger benefit of students and researchers in Social Sciences, to whom Marc Barbut would finally address his last book, marvelous in all aspects, *The measure of inequalities. Ambiguities and paradoxes* [2007(a)].

But we haven't yet gotten to the bottom of things.

It is clear, in fact, that, for Barbut, the seminar on history of probability was not limited to the sciences he practiced and knew, but was supposed to encompass other sciences, and of course the community – human society – studied by the social sciences. In reality, as we quickly see, it goes well above and beyond this. Marc Barbut, very early on, engaged himself in politics, in trade unionism and in many movements. In particular, he was a long-time activist of a trade union, the SGEN, which was always, like him, passionate about pedagogy. Marc Barbut participated, for a period, in its governing counsels and particularly dedicated himself to defining the union's position on consecutive French governments' policies in Algeria. The SGEN, led by Paul Vignaux, one of Guilbaud's close friends, was relatively close to the forefront on these topics. It was a delicate matter, for many union members belonged to Algerian institutions[14]. The SGEN under Vignaux often had positions close to those of the UNEF, the student union that openly fought against forcing conscripted soldiers, notably deferred conscripts, to fight in Algeria, and against the widespread torture, proof of which was becoming more and more evident and which would be even clearer with the Audin affair (1957-1958). We have not been able to find the union documents signed by Barbut during that time[15], but it is certain that he was outraged by the repression and the torture in Algeria and that he fought to denounce them as much as he could and more. Let's take just one random example. As we have said, from about 1956 to 1959, Barbut attended the seminar on probability led by Darmois at the IHP, a seminar that was still mixed – probability and theoretical and applied statistics alternating according to the choices of the lecturers and Darmois's eclectic tastes[16]. Barbut preserved the seminar's announcements; they usually contained a rather lengthy summary of the planned lectures, often with a blank back that allowed extra note-taking and whatnot. These summaries-announcements are included in archives from Marc Barbut that we have been able to consult. It is obvious that Barbut, like most seminar attendees, listened only half-heartedly to the often esoteric arguments of the lecturers and did something else at the same time, the nature of which we can sometimes see by looking at the blank backs of the announcements. Let's then take the presentation of January 8, 1959. The lecturer was Pierre Thionet, and he talked about "recent developments in the theory of polling."[17] Marc Barbut mainly occupied himself, it seems, with preparing the February 4 meeting of SGEN's administrative council and had begun a list of people committed,

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[14] The literature on this subject does not seem abundant; in any case we are not familiar with it. See nonetheless Singer [1993]. On the other hand, literature on the French intellectuals' fight against torture in Algeria is extensive and well known. For what touches most particularly on mathematicians, see Schwartz [1997] and the article by L. Schwartz in the collective work Hartog *et al.* [2007]. The title of this work, incidentally, was rather appropriate for Marc Barbut, mathematician in the city, historian in the city, man in the city.

[15] They certainly exist, but it would take a long search that we were not able to undertake. Let's call attention nonetheless to the document that Barbut wrote after the Parisian demonstration of October 17, 1961, savagely repressed by the police [1961].

[16] We know that this diversity, permitted only by Darmois's open spirit and goodwill, would not last. After Darmois's death at the beginning of 1960, Dugué's statistics on one hand and Fortet's probability on the other would live separate lives. Marc Barbut, who liked and respected Darmois, no longer attended the historical Borel-Fréchet-Darmois-Fortet seminar. Which, we know, did nothing to hurt him.

[17] On the important works of Thionet in polling theory, see Armatte [2003].

like him, to the denunciation of torture in Algeria. Nothing indicates the reasons for this list — a colloquium, a resolution, a "manifesto", or a demonstration of some sort, we do not know and it matters little. On this list can be found many pre-eminent university professors whose names reappeared often at that time: Laurent Schwartz, Evry Schatzman, Jean-Pierre Vigier, Paul Fraisse, Andre Châtelet, Jean-Paul Mathieu, Alfred Kastler, Lucie Prenant, Jean Bruhat, André Hauriou, Pierre Dieterlen, Jean-Jacques Mayoux, Jacques Madaule, Madeleine Rebérioux, Jean-Claude Pecker, Pierre Deyon, and others, unimpeachable personalities known by everyone; and then, at the end of the list, names known by few or not at all. One of them attracts attention: Mlle Pouteau. Who was Miss Pouteau, lost in the midst of so many activist giants? Surely Micheline Pouteau (1931-2012), lycée professor of English at Neuilly. Clearly neither her age nor her notoriety was sufficient to include her on the list. Rather, she was an activist in the SGEN or a similar group, invested particularly in the fight against torture and for Algerian independence. One among many perhaps, but she wasn't lacking in guts or determination, as she was a secret member of the Janson network and was sentenced to ten years in prison in September 1960 following the group's trial. After having escaped from the Roquette prison in 1961, she succeeded in reaching Italy, where she stayed until 1967, when she was granted amnesty. She retook her place in National Education, without talking about her involvement in Algeria ever again until her retirement[18]. In order for her name to be a part of the list at Thionet's seminar, Marc Barbut must have known her well enough and shared some of her convictions. This helps us better understand the level of involvement of Barbut, the only mathematician to sign the first list of the Manifesto of the 121, without ever boasting of it, even though he was risking lifetime banishment from the ranks of National Education[19]. Surely we cannot understand the seminar on the history of probability without having this element present in our memory.

But we haven't yet gotten to the bottom of things.

Yet it is simple. Nothing would have existed without Marc Barbut, without his tireless devotion to the service of all and the idea that he made of himself from his duties and from his duty as a man, without his particular genius, his enthusiasm, his fraternity, his generosity, his love of mathematics and of life, whose history sometimes offers a moving and ephemeral image[20].

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[18] See her account in Evans [2008].

[19] Laurent Schwartz, absent from Paris, only signed the second list. Therefore, he was not a signatory of the Manifesto of the 121, intended to protest especially the verdicts of the trial of the Janson network. Sometimes the mathematician Paul Lévy is mentioned as being on the list. There is indeed a Paul Lévy on the list, but this is a very common last name, and nothing indicates that it is the mathematician. In fact, everything is against it. Paul Lévy was an intransigent patriot, even if he had been very poorly treated by the French government during the Occupation (Locker [2001]). It is hard to imagine him signing a call for the absolution of desertion and treason. But one never knows, and we will leave the reader to make his or her own judgment on this point of external history.

[20] Which is the image of which? Indeed, since its origins, the theory of probability has borrowed its vocabulary and its intuitions from the theory of history, at least when it comes to game theory, randomness (the randomness of history in particular), probability, causes, events, dependencies, past, present and future, duration, time series, trajectories, histories, etc. Symmetrically, the philosophy of history, when it became an independent discipline around the middle of the 18th century, eagerly took statistics as a model. Both of them, each on its own scale and in its own world – do they not try to "discover a regular course" at the heart "of the game of the freedom of human will", in a way that "what in individual subjects strikes us by its entangled and irregular form, can nevertheless be known in the entirety of the species…" (Kant, [1990] p. 69)? Is this not the enterprise of which Laplace [1986] and Cournot [Oeuvres], masters of the Barbut seminar, made themselves the prodigious heralds? These multiple and reciprocal correspondences could explain or justify, marginally, seen from the outside, the existence and the success of the seminar, without grasping the heart of the matter that we have attempted to reach here, knowing that we will not ever do so.

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