Comment

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There is little that I can add to Professor Le Cam's survey and analysis of the literature on the central limit theorem, but a few remarks based on personal recollection may be of interest. As a student of Feller's, and later a colleague, I of course got some idea of his opinions on various matters, and heard various anecdotes of his experiences. Unfortunately, I never took advantage of the opportunity to ask questions that might have led to a coherent picture of how things were in the thirties, and I never kept notes on what I did hear. Thus the following remarks, based only on scattered recollections, are subject to all of the inaccuracies that affect undocumented memories after a lapse of over 10 years.

Feller certainly had (and expressed) great admiration for Lévy and the brilliant insights contained in his work, and I think that he would agree that much of Lévy's work was slower to receive recognition than it deserved. I do not, however, recall any mention of possible priority on necessary conditions for the central limit theorem.

It is my impression that Feller regarded his papers of 1935 and 1937 as a high point of his early career, and that he took special pleasure in them because they contradicted the opinion of authorities at the time that no sensible necessary conditions could be established. (Unfortunately, I recall no indication of who the "authorities" were.) This suggests that the question was in the air, so that it would not be surprising if Feller and Lévy were working on it independently. (I have a vaguer impression that Feller claimed some originality in introducing, or at least effectively dealing with, fully arbitrary scaling and location factors. For me, this tends to confirm Professor Le Cam's opinion that Feller had not seen Lévy's work of 1931 and 1934.) I personally do not doubt that both were convinced of their respective priority, and legitimately so in the sense that their work was done completely independently.

I have no idea whether Feller was familiar with Kolmogorov's measure-theoretic foundations of probability (Kolmogorov, 1933) in 1935. From what I remember his telling about that time, "random variables" and the like were not clearly defined entities that could be used in any rigorous discussion—a statement of probability theory had to be cast as a proposition in analysis before it could really be proved, and probabilistic notions had only motivational and heuristic value. It would be interesting to trace how, when, and where this attitude changed. Obviously Feller's papers of 1935 and 1937 were written in traditional style. He might have chosen that style for the sake of being more readily understood, even if his own ideas had already been changed by Kolmogorov (1933), but my guess (based only on a general impression, not on anything he ever explicitly said) is that while he might well have read Kolmogorov (1933) by 1935, it had not yet really changed his way of looking at things. Of course the central limit theorem (at least for independent variables) really is just a statement about convolution of distribution functions that can be viewed directly as a proposition in analysis, unlike, say, the strong law of large numbers, which can be much more naturally expressed in genuinely probabilistic language.

Professor Le Cam is entirely correct in describing my own method of proof of Lindeberg's theorem (referred to in the passage he cites from Feller (1971)) as differing from the original mostly by a change of terminology. As it happens, the idea of the proof for the simple case of identically distributed random variables came naturally from work I had done in my thesis on semigroups of operators, and it was only when I looked up Lindeberg (1922) to see whether the idea could be extended to obtain Lindeberg's more general result that I discovered the essential equivalence of the methods. The only proofs I had seen at that time involved characteristic functions, and the point of my paper was only to show that a little very elementary "soft" analysis could substitute for the (slightly) less elementary "hard" analysis involved in proving that convergence of characteristic functions implies convergence of distributions. It is hard now to see why Lindeberg's paper appeared difficult. Because the notion of linear operator was not yet automatically part of the common vocabulary, he needs several pages to establish basic facts that can now be dealt with in a paragraph, but the arguments are clear and straightforward. Perhaps it was the success of characteristic functions, rather than any real difficulty in the paper by Lindeberg (1922) that led to the latter almost dropping out of sight for so long.

There is one matter on which I disagree with Professor Le Cam. His remarks on Gauss are of course peripheral to the main subject of his paper, and evi-

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