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STOCHASTIC RESERVE LOSSES: A REJOINDER

by Eleanor M. Birch & John M. Heineke*

Mr. Boorman's criticisms of our paper, published in the Spring 1967 issue of *The American Economist* deal with the following topics: 1) differentiation of the profit function; 2) the case where $r<i$; 3) marginal to total credit expansion ratio; and 4) choice of distribution. We shall comment on each of these in turn.

Regarding item (1), Mr. Boorman insists there are two ways to differentiate the profit function. He says that if $L=L(D)$, then of course the density function becomes $a[L(D)]$ and if $L=L(D)$ the density is $a(L)$ (*his* notation). This observation stems from Mr. Boorman's misunderstanding of the postulated density function. Orr and Mellon assumed $L:N(kD,l)$ which *implies* the density function, $a(l;D)$. D enters parametrically, by the assumption that $E(L)=kD$.

Much of Mr. Boorman's confusion stems from his confounding the random variable, L, and its realization, l, a notational misfortune he regrettably repeated from the original paper, although we pointed this out in our footnote 2. The term $\partial L/\partial D$ is not even defined, but let us assume he meant $\partial l/\partial D$, the term we discussed in our paper. He says our derivative should have included "at least three other terms." This, of course, is not true once we assume that $\partial l/\partial D=0$. We realized other terms would enter (those involving v, for example), if $\partial l/\partial D\neq 0$. We analyzed only one of these terms, then deleted it (and all other like terms) by assuming $\partial l/=0$. This brings up an interesting question. Does $\partial l/\partial D=0$? The realized value of L, i.e., l, shifts probabilistically with changes in D; that is, $E(L)$ changes as D changes. The probability that $L=l$ changes as D changes, but a change in D does not necessarily cause a change in l, which can, in any event range from $-\infty$ to $+\infty$. We discussed this point with mathematical statisticians and all agreed, after much thought, that $\partial l/\partial D=0$ was probably a justifiable assumption. Note, though, that Mr. Boorman did not address himself to this interesting question, but to the mechanics of taking the derivative. Incidentally, he should not be surprised that he obtained the same result we did since he made precisely the same assumption, viz $\partial l/\partial D=0$. The only difference is that we made ours explicit, while Mr. Boorman simply eliminated any dependence of l on D by the way he set up his original equation B. We are somewhat surprised to note, too, that he permitted v to depend on D after eliminating the dependence of l on D; since v is the critical value of l, it clearly can be no more dependent on D than l is.

Regarding item (2), we stated that "this formulation of the problem rescues Orr and Mellon from Miller's criticisms" and we still believe it. If the problem is formulated in such a way that loans must be cancelled, which, incidentally, seems to be the model Orr and Mellon had in mind, then it surely follows that $r>i$. If we adopt Mr. Boorman's view that the bank has a choice, then, of course his conclusions follow. But he can hardly criticize us for not having adopted his view.

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Mr. Boorman's comments on item 3 are among the most baffling. We were aware that we were assuming the loaned-up case as a starting point. Our critic seems to imply we have no right to assume anything, since Orr and Mellon failed to tell us what they had in mind. This is surely a curious view of scientific endeavor. Without belaboring this point, let us merely state that our results would still follow if we assume any other starting point.

Our statement that Orr and Mellon might have made a case for the normal distribution also bothered Mr. Boorman. He claims that "no such demonstration is forthcoming." This is not necessarily a defect; some of our readers may have even considered it a virtue! Nevertheless, Mr. Boorman is mistaken. Later in our paper, we say, "The function F(z) can be observed and the moments q_1 and q_2 estimated. An appropriate solution can be obtained by standardizing X(t) and appealing to the Central Limit Theorem." Most readers, we trust, recognized that this section of our paper was, indeed, an attempt to justify the use of the normal distribution. The quotation above also answers Mr. Boorman's last criticism, to wit: "To derive a workable formula, the distribution function explaining total reserve losses for each value of n — the number of check clearings — would have to be completely specified." This is not true. Once the function F(z) has been observed, the asymptotic distribution of X immediately follows. That is, $E[X(t)]=q_1t$ and $Var[X(t)]=q_2t$; therefore, $X(t)\text{-}q_1t/\ \ q_2t \rightarrow N(0,1)$ as $t \rightarrow \infty$. Apparently, Mr. Boorman's lack of familiarity with the properties of the compound Poisson variable, X, led him into error here.(1) He also has some misconceptions about our missing Table 1. It would be quite possible to have one; the difficulty lies not in complex mathematical problems, as he suggests, but rather in a lack of time and interest on our part. We could go to a bank and observe enough data to estimate the mean and variance of Z; a Table 1 would follow readily. The difference between our potential Table 1 and Orr's and Mellon's actual one is that theirs was based on hypothetical data, while ours would have to be based on actual observation of bank behavior. We do not regard this as a drawback.

Mr. Boorman also takes us to task for our remark about giving a more realistic weight to uncertainty. His heroic leap to save Orr and Mellon from ou rsharp tongues seems a bit forced since all we were dong here was agreeing with Orr's and Mellon's own estimate of the uncertainty in the system. In fact, in general, we find ourselves placed in an uncomfortable position by Mr. Boorman who persists in casting us in the role of prosecuting attorney vis-à-vis Orr and Mellon, while our true outlook is much closer to that of *amiscus curiae.*

1. For further explication, see H. Cramér, "On Some Questions Connected with Mathematical Risk," University of California, *Publications in Statistics,* 1954.