Rejoinder

I. J. Good

I am pleased that the discussants have filled some important gaps in my account of Poisson's work and its influence. But their contributions hardly overlap so probably further gaps remain.

Diaconis and Engel have provided a most useful succinct survey and bibliography of a topic of sufficient philosophical and mathematical interest to deserve a name, say the Magnification of Indiscernible Differences, where microscopic changes in initial conditions lead to macroscopic differences. ("Amplification" would be as good a term as "magnification" but I prefer the acronym MID to AID. To show my appreciation to Diaconis and his coworkers, another acronym will be suggested by the last sentence of this paragraph.) Poincaré (1912, pp. 4–7) mentioned such situations and presented them as his first kind of fortuitous phenomena, while assuming, however, that the laws of nature are deterministic. In such situations the results appear to humans to be random whether or not the laws of nature are deterministic. The outcome is exceedingly sensitive to the initial conditions, but, up to a point, the final probability distribution is *insensitive* to the initial ones. It sounds somewhat paradoxical. Such situations occur in physics, in history, in economics, in wars, in the first picosecond after God pressed the Big Bang Button, in games of chance, and even in games of skill. In games of pure chance they occur all the time but in games of high skill less frequently. For example, in tennis a negligible difference of impulsive force when the ball is struck, a difference much smaller than the standard error of the force intended by a champion, can decide who *becomes* the champion. Deviations, imperceptible, amplified, can often nullify incredible successes.

I referred briefly, with appropriate citations, to Poisson's work on the effect of changing the number of jurymen votes required for a conviction, and to the extensive development of the topic by Gelfand and Solomon. It is good that Solomon has now provided more details, for this was one of Poisson's main practical applications of the theory of probability.

I am relieved that Heyde's comments were not more critical, for his distinguished joint book with Seneta on Bienaymé is a mine of information concerning statistics in the 19th century. The human interest is I hope not entirely absent from my article but it is dealt with very succinctly, especially in a few words in the third paragraph. It would not have been polite to emphasize Poisson's faults at the Bicentennial conference, especially as some Poissons were present.

Singpurwalla has also added very interesting historical information. I agree with his comment that subjective probability is required to measure logical probability. An intelligent machine would have to depend on its own probabilities and our best chance of proving that we are not intelligent machines is to prove that we are not intelligent.

Singpurwalla asked me to state once and for all and in simple English whether I am (i) a card-carrying Bayesian, or (ii) a noncard-carrying solipsist, or (iii) a Doogian. Well, if I were a solipsist I wouldn't believe in the existence of cards, and I do, so I'm not a solipsist. I know there are some solipsistic indications in the foundations of quantum mechanics (Wigner, 1962; Wheeler, 1978), but they are controversial. I shall return to the topic of quantum mechanics below.

Regarding (i), there are so many varieties of Bayesianism that I cannot give a simple yes or no answer. I am some kind of Bayesian, and the kind was described in my note (Good, 1971) where 6^6 varieties were mentioned, and the number is doubled if we allow for "dynamic probability." I prefer to say that I believe in a Bayes/non-Bayes compromise, and that puts me in category (iii). I do not, for example, regard tail-area probabilities as absolute nonsense, but rather regard them as having a rough Bayesian (? Doogian) justification whenever they have any justification at all (compare Good, 1950, p. 94), and this applies to any other "non-Bayesian" technique. An informal Bayesian approach makes it clear (i) that the conventional P value of 5% is not worth writing home about although any P value can be worth *publishing*, and (ii) that any specific P value P_0 *supports* even a precise null hypothesis H if the sample size N is large enough. (Given P_0, $\exists\ N$ such that $\Pr(H \mid P_0) > \Pr(H)$.) "Standardizing" the P value to sample size 100 (Good, 1982) is one of several examples of a Bayes/non-Bayes compromise.

In the fifties of this century I would have described myself as a Bayesian because most other statisticians were on my "right". Now there are many statisticians on my left, and they have dragged the most frequent meanings of "Bayesian" with them. My own position has remained unchanged, but it has become potentially misleading to call myself a Bayesian without qualifications. In simple English, I am a Bayesian and I am not a Bayesian. The law of the excluded middle applies only when words are unambiguous.

Singpurwalla mentioned some work on the reconciliation of probability judgments. Two further