A PERSONAL PERSPECTIVE ON THE WORK OF THE "GANS" COMMITTEE

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1. As reported by Aumann and Furstenberg in DP 364 of the Center for the Study of Rationality (June 2004), the committee that investigated the Gans-Inbal results on the Bible Code was unable to replicate their results. Thus, the evidence gathered by the committee fails to confirm the existence of the putative code.

2. By their nature, significance tests of the kind performed by the committee can only show that a phenomenon exists (or is very likely to); they can never show that a phenomenon does *not* exist. Even when—like here—the test turns out negative, in principle it remains possible that a different design may yield a positive result.

3. Nevertheless, failure to confirm an experimental finding in a replication of a scientific experiment casts doubt on the validity of the original finding. When there is a question as to whether or not a phenomenon exists, a negative finding such as that of the committee must be taken as evidence against the existence of the phenomenon.

4. In this instance, doubts have been raised as to whether the committee's experimental design was carried out correctly; in particular, whether the data provided by the experts was in all cases correct (please see the section of this document authored by Doron Witztum). From some viewpoints, some of these doubts are unquestionably justified; in one case, an expert provided data some of which he himself subsequently acknowledged to be mistaken.

5. But, in spite of acknowledged mistakes in the data provided by the experts, the committee's work should not be entirely discounted. The input to the experiments consists not just of the data provided by the experts; the experts themselves, and even the process of selecting them, are part of the input. The question that the committee faced is whether the existence of a code could be verified scientifically by currently available methods. In this case, the methods included the exercise of judgment by the experts in deciding on the data to submit; the exercise of judgment in selecting the experts; and the exercise of judgment in selecting those who selected the experts (Professors Furstenberg and Lubotzky). All this was part of the experimental design. Perforce, the design took into account that some of the experts might make mistakes; this may be considered an inherent random element, like other random elements in the design (e.g., selection of a key for the random number generator, or indeed selection of random numbers once a key has been selected). Frankly, I don't see how the design could have been improved *a priori.*

6. As an observer—not a researcher! —I have been involved with the Codes research for close to twenty years, and have invested in it a tremendous amount of time and energy. Though the basic thesis of the research seems wildly improbable, for many years I thought that an ironclad case had been made for the codes; I did not see how "cheating" could have been possible. Then came the work of the "opponents" (see, for example, McKay, Bar-Natan, Bar-Hillel and Kalai, *Statistical Science* 14 (1999), 149-173). Though this work did not convince me that the data had been manipulated, it did convince me that it *could* have been; that manipulation was technically possible. The

arguments that ensued—including, on both sides, implicit or explicit accusations of manipulation—eventually became extremely complex, and I was unable to follow them sufficiently well to decide for myself who is right. Having become convinced that the only way to settle the matter to my satisfaction is to conduct an experiment designed and analyzed under my own supervision, I welcomed the suggestion of Eliyahu Rips to chair the committee referred to in Paragraph 1 above. Though fairly sure that the committee's work would convince almost no one who did not hold the corresponding opinion beforehand, I still thought it worthwhile to conduct the experiment just for the purpose of deciding the issue for myself. And, I decided that that would be the end of my own involvement in the Codes research.

7. During the years of the committee's work, I became convinced that the data is too complex and ambiguous, and its analysis involves too many judgment calls, to allow reaching meaningful scientific conclusions.

8. The matter of manipulation played a central role in the evaluation of the research, and also in the research itself: the committee's experiment was designed to avoid, at all costs, the remotest possibility of manipulation. I myself have gotten to know the people on both sides fairly well, and find the accusations of manipulation hard to believe; everyone involved seems sincere, and also to understand the issue of manipulation—which makes unintentional manipulation unlikely. Nevertheless, the basic thesis of the research is *a priori* even harder to believe than the possibility of manipulation. Moreover, matters of personal trust cannot be considered a legitimate part of a scientific analysis; results must be repeatable and objective, and their validation cannot depend on the analyst's personal assessment of the researchers' honesty.

9. An interesting feature of this research is that, as noted in Paragraph 6, almost everybody involved made up his mind early in the game—sometimes before seeing any evidence at all—and then was unwilling to consider changing it. The research has a high ideological content, and many people are unwilling to abandon ideologies, no matter what the evidence is. When I first presented the results of Witztum, Rips, and Rosenberg at the Center for the Study of Rationality at the Hebrew University, Professor Maya Bar-Hillel told me after the presentation, Bob, I won't believe this no matter *what* evidence you bring me. She now says—and no doubt believes—that this was not really meant literally; but I believe that it was, and indeed that it remains true today. Many others hold similar views.

10. Some readers may disagree with the view set forth in Paragraph 5 above, and hold that a factual error disqualifies the research. But even under this view, the committee's results certainly do not *support* the Codes. At best, they return us to where we were before the committee started its work—with a low *a priori* estimate of the probability that the Codes are real.

11. We come finally to the bottom line: *A priori,* the thesis of the Codes research seems wildly improbable. Though the original work of Witztum, Rips, and Rosenberg, and that of Gans, established a *prima facie* case for the existence of the codes, this case was undermined by the work of the "opponents" (see Point 6 above). Research conducted under my own supervision failed to confirm the existence of the codes—though it also did not establish their non-existence. So I must return to my *a priori* estimate, that the Codes phenomenon is improbable.