To the Editor,

I would like to make some comments about the article by Hamilton Domingos et al entitled "Correlação eletro-ecocardiográfica no diagnóstico da hipertrofia ventricular esquerda", published in *Arquivos Brasileiros de Cardiologia* (1998; 71: 31-5). Although during the last decades, the many existing electrocardiographic criteria for diagnosis of left ventricular hypertrophy (LVH) have undergone innumerable comparative evaluations, there is no consensus as to the most accurate so far. Extensive studies with careful correlation between electrocardiogram (ECG) and echocardiographic or autopsy analysis have led to conclusions that do not coincide. This can be attributed to several variables that influence the accuracy of certain electrocardiographic criteria of LVH, particularly those that analyze only QRS voltage. Among them we can cite physical biotype, body fat, breast presence and size, age, hypertrophy pattern, etc [1]. The subject is really important, controversial and deserves additional clarifying studies. The referred article, however, does not contribute to answering this question, due to its many methodological and interpretational errors. First of all, I do not believe any light can be shed upon this complex subject with such a small population sample size (n=30). In addition, besides being small, the study sample is very heterogeneous in its analyzed characteristics (age, hypertension levels, and race). Relevant characteristics, such as the presence of conducting disorders, pulmonary emphysema, and obesity were not considered. If the sample had been large enough, such factors could probably never have been considered, because a supposed natural homogenization of these characteristics might have occurred. It should be remembered, however, that even in larger studies, part of the discrepancies in the results can be attributed to factors not considered in biased samples. Of particular importance in the study considered is the presence of only one individual older than 30 years. It is known that in this age group there is a greater occurrence of false positive results with voltage criteria, such as the Sokolow-Lyon [1]. An even greater weakness is the attempt to define specificity and positive predictive value (PPV) for several criteria based on the analysis of a population sample where only five individuals did not have LVH at the echocardiogram, and only one of them was a male. It is a true methodological flaw, especially when analyzing such a complex and controversial matter, under the influence of innumerable variables. To calculate specificity and PPV, it would be ideal to analyze a non-hypertensive control group without LVH. It is obvious that the authors could work with a restricted population sample, with restrictions of the objectives of the work, for example, to analyze the accuracy of these criteria when applied to a population of male hypertensive elders. It seems to me, however, that that was not the intention of the authors. In addition, in the discussion, they approach several other questions related to LVH that were not the objective of the study and that could not be inferred from it, including the analysis of subgroups, which is a practice dangerous enough in large studies, but much more so in such a small one. Inappropriate statistical conclusions were taken, such as the suggestion that there would be no difference in sensitivity between the criteria of Sokolow-Lyon and Gubner-Ungerleider (40% vs 28%) only because of this restricted sample p=0.15. Finally, when in the discussion the authors consider questions that were not objectives of their research, they end up with basic conceptual errors such as the statement that in concentric LVH there is the addition of myocytes in a parallel manner and in dilatation there is addition of myocytes in a series. It is known that in hypertrophy there is no addition of myocytes but of myofibrils inside myocytes. Even though the term "addition" has not been explicitly used, it is inferred in the text.

In the electrocardiographic analysis of LVH, we should use the criteria already established in the studies that have applied adequate methodology, particularly the Romhilt-Estes and Cornell voltage score [2]. A selection of criteria based on the Bayes' theorem should also be used, founded on the previous knowledge of the diagnostic characteristics of each criterion in each population, as well as the pre-test probability of the presence of LVH in the individual supposed to be evaluated.

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To the Editor,

In response to Dr. Fernando Ganzarolli de Oliveira's criticisms of the article "Correlação eletro-ecocardiográfica no diagnóstico da hipertrofia ventricular esquerda" (Arq Bras Cardiol 1998; 71: 31-5), of our authorship, we would like to clarify that the goal of our work with such a reduced population sample size (n=30) has never been to compare to the large studies on the already established electrocardiographic criteria. Instead, we meant to discuss such a diagnostic approach, as well as to demonstrate the low sensitivity of the electrocardiogram (ECG) for the detection of left ventricular hypertrophy (LVH).

In regard to the basic conceptual error cited by our colleague about the parallel or in series myocyte growth, we were unfortunat in our statement, when we used the term "myocytes" in the place of "myofibrils". This mistake, however, does not invalidate the question raised in our study about the electrocardiographic sensitivity in concentric or eccentric LVH. Although this question has not been one of our research goals, we understand that research studies do not only aim at clarifying definitive data. They also serve to bring into discussion controversial topics, aiming to stimulate the development of new studies.

Therefore, we consider the criticism on the sample size valid in regard to the comparison of the four criteria, but it does not invalidate the low sensitivity obtained either for each isolated criterion or the four simultaneously considered. In this regard, there is agreement in the data obtained by other authors such as Casale et al [1] and Romhilt et al [2].

In regard to the myocyte versus myofibril error, we consider our colleague's interpretation pertinent but loaded with destructive intention and character, because there was no modification in the context or meaning of the study's questioning.

In conclusion, we would like to stress that even the already established criteria (cited by Dr. Oliveira) present unmatching values in relation to sensitivity and specificity in the detection of LVH, according to data of the large studies[1-3].

Therefore, even using Bayes' theorem or knowing the diagnostic characteristics of each population, the low sensitivity of ECG in the diagnosis of LVH seems evident and, consequently, the use of the echocardiogram as an additional propaedeutic method is mandatory.

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