The clinical thinking is not based on intuition, it is closer to an equation based on the presentation of the clinical features and on the knowledge of epidemiological and etiological data, properly demonstrated in scientific studies.

Knowledge of the viral etiological profile of the respiratory infections in childhood is a major contribution to the pediatrician, especially regarding young children, since viral bronchiolitis is the first cause of hospitalization of infants in developed countries, and its impact can be remarkably higher under unfavorable socioeconomic conditions. However, the etiology of viral respiratory infections depends on research into methods that present higher sensitivity and specificity. These methods are expensive, therefore great research groups are formed so that, in clinical practice, the physician can make use, in most of the cases, of their clinical knowledge and thinking.[2]

The study published in Jornal de Pediatria aims, as stated in its title, at conducting a surveillance of the occurrence of viral infections in children younger than 5 years hospitalized for lower respiratory tract problems. This objective bears the knowledge of the frequency of the different viruses in this population, and their distribution and seasonal profile throughout the year. This study does not include children with upper respiratory tract infections alone, such as rhinosinusitis. The respiratory viruses occurred mainly in their first year of life, bronchiolitis by respiratory syncytial virus (RSV) being the most frequent infection. In this study, we highlight the importance of the human metapneumovirus (HMPV), being the second most frequent virus. The difference in the seasonal trends between these two main agents is remarkable, since the RSV, as previously published by the authors, presents annual season during the fall and winter months. The HMPV spreads more homogeneously throughout the year, what contributes towards the occurrence of high rates of infants´ hospitalization during the months following the RSV season.[3]

Viral respiratory infections occur mostly in previously healthy children and present with a characteristic clinical course. Thus, it is possible for the pediatrician to diagnose even without an etiological confirmation, since they are aware of the importance, the distribution of the main agents, and the characteristics of the infection. That is what happens, as a general rule, in the acute viral bronchiolitis. This way, it is possible to, confidently, avoid expensive and unnecessary treatments that may be responsible for undesirable side effects, such as antibiotic therapy. It does not apply to patients with risk factors involved, who present with more severe infection, as well as to previously healthy patients with more severe clinical features, in which, regardless of identification of respiratory virus, bacterial infection can not be excluded as the only cause or in association with viral infection. Fortunately, these are rare cases in populations with characteristics similar to the ones in the study carried out by the authors.

Surveillance of viral respiratory infections grows more and more important, since the technological race for the development of new therapeutics, especially prophylaxis, intensifies year after year. The etiological profile is dynamic and needs monitoring. We hope that we have brought our contribution to, in a near future, write to Jornal de Pediatria on new vaccines or antiviral drugs that can reduce the impact of RSV and HMPV in infants, such as the current example of the rotavirus in controlling the impact of acute diarrhea.

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Statistical and epidemiological methods in prevalence studies: odds ratio vs. prevalence ratio

Dear Editor,

When reading the article by Rodrigues et al.,[1] "The association between cardiorespiratory fitness and cardiovascular risk in adolescents," it was possible to find some errors regarding the application of statistical and epidemiological methods, as well as their respective interpretations; however, in our opinion, this does not invalidate the findings, but it can depreciate the scientific method of that study, which is not desirable for the authors, readers or even Jornal de Pediatria, which is such an important journal.

That article is a result of a cross-sectional study, i.e., a prevalence study, situation in which association estimates are preferentially calculated by prevalence ratio (PR) or, less adequately, by odds ratio (OR) and their respective 95% confidence intervals (95%CI). That is so because, in this type of study, it is not possible to determine incidence. Analyzing the results, one can see that OR and relative risk (RR) were used as association estimators. Both are inadequate, since it is known that OR overestimates strength of association,[2,3] and

RR cannot be estimated, since it is the possibility of calculating prevalence, and not incidence.[4]

Another important aspect verified was that the 95%CI of estimators used was not published. In our opinion, their visualization is of great help in a proper analysis of results, because it allows estimating adequacy of sample size and verifying statistical significance of the association, besides being an extra aspect when searching for causal inference.

We hope to have contributed with our observations and suggest that, in studies of that nature, preference is given to estimation of strength of association by PR, always showing its 95%CI. Therefore, applicability and interpretation of statistical tools used in epidemiological studies are adequate to their purposes.

We stress the importance of that article and reinforce that reported inadequacies do not diminish the merit, nor invalidate results; presentation forms only need to be corrected.

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Authors' reply

Dear Editor,

We read and appreciated the contributions sent by Professor Altacílio Nunes concerning the article "The association between cardiorespiratory fitness and cardiovascular risk in adolescents"[1] about application of statistical methods, an area in which we have much to learn.

The authors would like to clarify that, in that type of study, odds ratio (OR), although numerically higher, as shown in Tables 3 and 4,[1] follows relative risk (RR) and is a good estimate for it.[2] Such method (RR) has been recommended as first choice to determine exposure risk to a certain disease, and perhaps the most adequate to the objectives of that study.

However, it is inadequate in cross-sectional research studies, such as that being discussed here. Option for not using prevalence ratio (PR), although it can be used, is due to the fact that, in cross-sectional studies,[3] OR allows identification of possible associations in which PR may lead to false conclusions.[2]

The authors acknowledge not drawing attention to the fact that, although RR has lower values for investigated associations, due to methodological inadequacy, those that should be considered are OR values, an estimated approximation of RR. The authors do not acknowledge inadequacy of using OR in their research study.

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Data with unexpected values should be checked

Dear Editor,

I would like to stress the quality of the article "Nutritional assessment of iron status and anemia in children under 5 years old at public daycare centers," by Vieira et al.[1] (Jornal de Pediatria, Vol. 83, No. 4, 2007), and also ask the authors if there might be a typing error in the values of free erythrocyte protoporphyrin (FEP) in Table 3: should not it be, for instance, 67 (61-74), 55 (53-57) and 50 (48-52)? Even if they are geometric means (antilogarithm of arithmetic mean of original value logarithms), I cannot understand how transformed values were 6.7 (6.1-7.4), etc., as shown in Table 3 (compare with the value of 69.6% of children who had FEP levels higher than 40 µmol/mol heme, Table 1). Another possibility that I considered was that those values are actually arithmetic mean of original value natural logarithms (base e), but that is not included in the methodology, which makes this hypothesis less likely.

I would also like to confirm the values of serum ferritin, shown in Table 3, since the magnitude of such values is possible, although equally "weird," since they seem to be a little low in relation to the information in Table 1, which shows that 30.8% of the children had values lower than 12 ng/mL.

Thank you for being so kind to forward this request to the authors.