



Infectious diseases and daycare and preschool education

Dear Editor,

A tendency towards the use of daycare and preschool education centers advances along with the process of urbanization which has occurred in Brazil,¹ following patterns already observed in countries with older industrialization.² The extensive review published in the last issue of *Jornal de Pediatria* proves to be of great importance,³ since the authors report the infectious diseases with an increased risk of transmission in a child daycare environment.

Albeit its extension and the large list of references cited by the authors, we would like to call attention to two aspects we consider relevant. Firstly, parasitic intestinal diseases are not mentioned among those diseases whose risk of transmission is increased in a collective daycare environment. We could not find adequate reason for this omission, since the authors mention dermatoses caused by ectoparasites, such as scabies and pediculosis.

In 2005,⁴ our group published a study demonstrating that 1- to 5-year-old children who attend daycare are 1.5 times more likely to have intestinal parasite infestations, when compared to controls of the same age, living in the same area. The occurrence of parasitic intestinal diseases equated to 63% in the daycare group versus 41.3% in the control group ($p < 0.01$).

Several reports available in the medical literature corroborate the statement that several regions and cities in Brazil have a high incidence of parasitic intestinal diseases.⁵ Additional data on this occurrence could not be provided in this letter due to the limited use of references, but we can state that this occurrence varied from 53.5% in Botucatu, state of São Paulo, to 23.4% in Estiva, state of Rio Grande do Sul. Therefore, we believe that it was remiss of the authors not to mention these diseases, considering an article published in a medium particularly aimed to reach a large number of pediatricians in Brazil and Latin America.

The second aspect is concerned with the article's extensive list of references (156), in which there are few citations of articles published in Brazilian journals (4) or of Brazilian authors (4 identified in articles in international journals and 3 dissertations). While the abstract suggests a systematic database search, the criteria for inclusion or exclusion of articles

are not clear. Thus, we could not reproduce the authors' steps, neither could we infer the criteria for selection of articles.

Jornal de Pediatria (JPED) and the Brazilian Journal of Mother and Child Health (Revista Brasileira de Saúde Materno Infantil, RBSMI), for instance, have no articles cited in the list of references. This omission is rather surprising, since the reviewed databases, MEDLINE and LILACS, contain JPED and LILACS contains RBSMI. A rapid search in SciELO, using the descriptor "creche" (*daycare*), returned two articles in JPED, and three in RBSMI. Complete search resulted in 136 articles. The search in MEDLINE returned fifteen articles, one of them in JPED.

According to a study published last year,⁶ a considerable increase was observed in the number of Brazilian scientific publications in English regarding child and adolescent health, accounting for 7,222 articles published and indexed in MEDLINE in the last 15 years (1990 – 2004). However, these figures represent only 1% of the English language articles in the databases. The increase of 213% during this period was considered excellent, although inferior to the one of 264% in publications regarding other age groups.

We are aware of the efforts JPED have been making to enhance quality and visibility of the journal, hence our belief this is a relevant aspect to be discussed. We consider that the citation of articles and authors published in JPED increases its worldwide visibility. This is the case to ask if regarding "infections and daycare", has *Jornal de Pediatria* been forgotten or has it forgotten the theme?

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Ricardo Queiroz Gurgel

Doutor. Professor adjunto, Departamento de Medicina, Universidade Federal de Sergipe (UFSE), São Cristóvão, SE, Brazil.

Jailson de Barros Correia

Doutor. Departamento de Doenças Infecciosas e Parasitárias, Universidade de Pernambuco (UPE), Recife, PE, Brazil. Diretor de Pesquisas, Instituto Materno-Infantil Prof. Fernando Figueira (IMIP), Recife, PE, Brazil.

Authors' reply

Dear Editor,

We read with interest the letter from Prof. Ricardo Queiroz Gurgel and Prof. Jailson de Barros Correia, in which they comment on our article published in *Jornal de Pediatria*. Preliminarily, we want to extend our appreciation to the editors for the commendable initiative and commitment towards promoting scientific publications and establishing an appropriate setting for debate, which is essential for the scientific community.

We want to thank Prof. Gurgel and Prof. Correia for their interest and pertinent comments on our article. Their comments give us the opportunity to broaden the discussion and also clarify some issues related to our paper.

With regard to the parasitic intestinal diseases, they were not included in our study because emphasis was given to diarrheal disease as a syndrome, since its clinical manifestation increases the risk of dissemination of the agent. Priority of notifiable diseases over other diseases has also contributed to our decision. On the other hand, our study was based on the diseases listed in Table 1 – Principal infectious diseases that have been described as occurring in daycare centers, and, although not explicitly indicated in the body of the text, to mention diarrheal diseases is implicitly to refer to parasitic intestinal diseases. Finally, if analysis of this class of infection proves necessary, its "omission" does not reduce the value of the publication or of the information provided for both national and foreign pediatricians.

With regard to the second aspect addressed by Prof. Gurgel and Prof. Correia, we would like to point out that we did not promote a systematic review, since its conceptual and

methodological presuppositions are outside the scope of the review we have proposed here. As stated in the abstract, we focused our review on "studies that have compared the risk of infectious diseases for children cared for in and out of home, related risk to the type of out-of-home care and assessed the effectiveness of preventive measures." Following the delimitation of the methodology, we promoted a careful and large scale search for material to form the basis of our central hypothesis. Priority was assigned to the citation and the use of review articles in order to enhance the solidity of our arguments. Have we failed to cite or refer to a study? Possibly so. Due to the sheer volume of publications, we might have unintentionally failed in this aspect. However, we are confident that seminal studies regarding the theme were mentioned, which is evidenced by the overall content of the letter from Prof. Gurgel and Prof. Correia and the comments addressed in their review.

With relation to the search in SciELO database, 40 of the 136 articles refer to swine breeding or other animals, 42 refer to themes regarding education and development, 25 to nutritional or metabolic diseases and diet, 6 to themes regarding oral health, 2 report on hearing disorders, 2 on morbidity and mortality in daycare centers (one of them was mistakenly not mentioned in our review, being cross-referenced by the original paper in reference 92),¹ 1 reports on assistance profile, 3 on other themes, and finally, 15 report on infectious diseases. Out of these last articles, two are related with references 32 and 74,^{2,3} one is included in the review article in reference 6 (*Journal of Public Health*),⁴ 8 describe outbreaks or constitute prevalence studies without the comparison groups focused on in our study, 1 describes vaccination recommendations, 2 are the articles already cited on parasitic intestinal diseases, and finally, one last article, in *Jornal de Pediatria*, on recurrent acute otitis media, pointing out daycare as a frequent risk factor,⁵ which we failed to include in the references, although relevant to the theme of our study.

Considerately, we do not intend to foster debate on the means or conception concerning the worldwide visibility of our national journals. This is a rather complex subject and any consideration on this matter requires a setting suitable for further argumentation. We agree with both authors on "the efforts editors have made to enhance quality and visibility of this journal", attitude certainly shared with editors of other Brazilian journals. We appreciate the thorough work of the editors of *Jornal de Pediatria* on assuring its place as a major journal in the field of Pediatrics. We would like to highlight the importance of this work as a central element in reaching both the academic community and the professionals, thus enhancing the worldwide visibility of this journal.

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Maria Nesti

Mestre. Médica pediatra, Serviço de Epidemiologia Hospitalar, Núcleo de Informação em Saúde, Hospital das Clínicas, Faculdade de Medicina, Universidade de São Paulo (USP), São Paulo, SP, Brazil.

Moises Goldbaum

Doutor. Professor, Departamento de Medicina Preventiva, Faculdade de Medicina, Universidade de São Paulo (USP), São Paulo, SP, Brazil. Pesquisador 2, Conselho Nacional de Desenvolvimento Científico e Tecnológico (CNPq).

Efficacy of new microprocessed phototherapy system with five high intensity light emitting diodes (Super LED)

Dear Editor,

We have read with interest the article by Martins et al.¹ in which two types of phototherapy using different light sources were evaluated. We believe that this is the first published Brazilian study testing equipment in which LEDs were used that employ LEDs. We really like the detailed description of the radiation monitor, information on its wavelength range and on the spectrum of the light sources used. We consider that mention of these data and standardization of the measurements are essential for understanding and evaluating results in phototherapy. We would like to address some comments we consider pertinent to the discussion.

In the results, the authors refer to the mean irradiance expressing values in $\mu\text{W}/\text{cm}^2/\text{nm}$. We believe that they probably refer to the mean of irradiance measurements at the point of peak intensity. Spectral irradiance is quantified² as $\mu\text{W}/\text{cm}^2/\text{nm}$ whereas irradiance values are expressed in Watts/m^2 . Mean irradiance may more suitably designate the weighted assessment of irradiance at the surface area exposed to light. If weighted irradiance were measured at the area illuminated, a considerable difference would be observed in the irradiance emitted by the two phototherapy devices,

since the center to periphery decrease in spectral irradiance of the halogen phototherapy unit is significantly greater than that of the device equipped with LEDs.³⁻⁵

Another aspect regarding the results refers to the authors' report on the patients on Super LED phototherapy, who relapsed with elevated total bilirubin when withdrawn from treatment and needed to go back on phototherapy. To avoid controversy over efficacy of this type of treatment, the authors could have elaborated this in their discussion. This phototherapy system actually has a greater capacity of rapidly reduce bilirubinemia, which caused the withdrawal levels (not informed on the study) to occur before the patient had had a better glucuronidation capacity so he could have been able to suppress new increases..

Finally, we believe that the wider emission spectrum of the halogen lamp, unlike the explanation provided on the study, did not interfere in the results of the phototherapy. Of the light spectrum of 380 nm to 600 nm, only the range between 400 and 500 nm will be absorbed by bilirubin, thus determining its conversion into an isomer and a product of photooxidation. The decrease in values is actually attributed to the difference in the intensity rather than in the quality of irradiance.

We appreciate the authors' article and we do hope that we have contributed toward a better understanding of the data reported.

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Fernando Perazzini Facchini

Professor colaborador voluntário, Departamento de Pediatria, Universidade Estadual de Campinas (UNICAMP), Campinas, SP, Brazil.