Controlled study of respiratory viruses and wheezing

P C Parkin, C Y Taylor, M Petric, S Schuh, M Goldbach, M Ipp

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Wheezing in early childhood is common, occurring in approximately 50% of children before the age of 6 years. Understanding the role of respiratory viruses in triggering acute wheezing in children has been compromised by the lack of comparison groups in previous studies.

The objective of this study was to investigate the association (using a control group) of two common viruses—influenza virus and respiratory syncytial virus (RSV)—with acute wheezing among children, aged 1–7 years, with a past history of wheezing.

METHODS

Children, aged 1–7 years, with two or more previous wheezing episodes, were enrolled from a paediatric community practice and an emergency department during two consecutive winters (1997/1998, 1998/1999) into this concurrent case-control study.

Cases had previous wheezing and current symptoms of an upper respiratory infection and acute wheezing (clinical score of at least 1). Controls had previous wheezing and current symptoms of an upper respiratory tract infection, without acute wheezing (clinical score of 0) at the time of enrolment or within the week following enrolment. Children were excluded if they had received immunisation for influenza in the year of enrolment. The study was approved by the Hospital for Sick Children Research Ethics Board, and informed parental consent was obtained.

Baseline characteristics were collected and wheezing severity was graded using a clinical score (minimum to maximum range: 0–10). Nasopharyngeal swab specimens were collected and examined for influenza viruses A and B and RSV by immunofluorescence microscopy (antibodies from Light Diagnostics, Temecula, CA) and cell culture (RMK cells: Viromed Diagnostics, Minneapolis, MN; and MDCK cells: American Type Culture Collection, Rockwood, MD).

The odds ratio and 95% confidence interval were determined for influenza virus and RSV in children with acute wheezing (cases) relative to children with upper respiratory symptoms alone (controls). Separate analyses were undertaken for community cases versus controls, and all cases (community and emergency cases) versus controls.

RESULTS

Table 1 shows baseline characteristics. Table 2 shows the association between acute wheezing and virus infection. For influenza virus, the odds ratio indicates that infection is not associated with acute wheezing. The adjusted odds ratio (all cases versus controls) for the risk of acute wheezing in those with influenza was 0.52 (95% confidence interval, 0.27 to 1.03). For RSV, the odds ratio indicates that infection is associated with a threefold increase in the risk of acute wheezing.

Children with influenza virus (n = 43) and RSV (n = 58) were compared. Children with influenza virus were older (median age 3.6 years v 2.4 years, p = 0.002), had a lower...
median clinical score (0 v 2, p < 0.001), were more likely to be recruited from the community practice (87% v 59%, p = 0.003), and were less likely to be wheezing acutely (47% v 81%, p = 0.0003), compared to children with RSV. There were no differences in sex, history of smoke exposure, family history of asthma, and history of atopy.

DISCUSSION

When cases and controls were analysed for the viral aetiology of their respiratory illness, cases were three times as likely to be infected with RSV, but almost half as likely to be infected with influenza virus compared with controls. This finding existed in both the community and emergency department setting.

An extensive body of literature, summarised by Pattemore and colleagues, has found that influenza virus and RSV are commonly identified in wheezing illnesses and asthma exacerbations occurring in childhood. The interpretation of these studies is compromised in that control groups were not included for comparison. The unique contribution of our study was the inclusion of a control group, to allow for an estimate of the strength of the association.

Our study may be limited by the case–control study design. Although a prospective cohort study would be appropriate, this would be an intensive and invasive process, requiring participants to undergo repeated nasopharyngeal swabs, both when symptomatic (wheezing) and asymptomatic (not wheezing). Therefore, we designed a case–control study, in which controls were chosen to be free of wheezing (outcome) but comparable to cases with respect to risk of exposure. Thus, controls were selected from those individuals seeking care for broadly defined symptoms of an upper respiratory infection. A strength of the design was the inclusion of both community and emergency department controls. Furthermore, determination of outcome (wheezing) and exposure (virus infection) was conducted concurrently.

An unexpected finding was the trend of the association between influenza virus and wheezing, suggesting that infection might actually be associated with a reduced risk of wheezing (adjusted odds ratio 0.52, 95% confidence interval, 0.27 to 1.03). While this may have been a chance finding, it contrasts with previous studies, which have found a high influenza virus related morbidity in children with recurrent wheezing and asthma. New hypotheses have emerged regarding the role of viruses in promoting or preventing the development of persistent wheezing and asthma. Exposure to older children at home, children at day care, and repeated viral infections (other than lower respiratory tract infections) are thought to be protective.

Understanding the role of respiratory viruses in triggering acute wheezing, and in the long term development or prevention of recurrent wheezing, will be important when considering strategies such as immunisation and antiviral therapy. In this context, we conclude that in our study the role of influenza virus in triggering acute wheezing in young susceptible children less than 7 years of age was weak, while the role of RSV in these children was strong.

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Problems with scoring bruises

We write to draw attention to two problems with the recent study on a scoring system for bruising by Dunstan et al.1

Firstly, the authors did not publish confidence intervals for the likelihood ratios (LRs) derived from different score threshold values (table 3), thereby not allowing readers to judge whether the LRs are statistically—let alone clinically—significant. Secondly, the authors neglect the phenomenon of spectrum bias. This is a well described feature of many tests, whereby sensitivity and specificity (and hence derived LRs) of a test vary with disease severity or prevalence. Examples of spectrum bias have been described with several tests including exercise stress testing2 and UTI diagnosis.3

The study population had a prevalence of physical abuse of 40%, much higher than the general paediatric population. Since test performance—that is, LR—is not independent of the pre-test probability, the LRs generated by a study done on this population cannot necessarily be used in a population with a much lower prevalence of abuse, as the authors have done in table 4. Since spectrum bias tends to reduce test performance as the pre-test probability falls, the LR for any given score threshold would be smaller than that quoted when applied to a population with a lower prevalence of physical abuse.

As most settings would expect to have a lower prevalence of physical abuse than the study, this reduces the value of the proposed scoring system as a clinical tool.

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References
Procalcitonin as a prognostic marker in children with meningococcal septic shock

Carrol and coworkers confirm the findings from Karabocuoglu et al who reported that procalcitonin (PCT) was higher in children with severe meningococcaemia (fever, petechiae, purpuric rash, and haemodynamic instability) than in children with systemic meningococcal infection without shock (291.29 ± 167 vs 19.7 ± 23 ng/ml; p < 0.001).

Unfortunately, information is lacking in the report of Carrol et al., namely: a clear definition of severe MCD (defined in their paper as a Glasgow Meningococcal Septicaemia Prognostic Score ≥8) and median PCT values. Further, comparison in term of prediction of outcome between PCT level and generic or specific severity scoring systems. We report that admission PCT level is an accurate predictor of mortality in the subgroup of children with meningococcal septic shock (MSS). We prospectively investigated 35 children (median age: 16 months; Q1–Q3: 9–35) with MSS (defined as ecchymotic or necrotic purpura with shock, needing fluid expansion (median for the first 24 hrs: 90 ml/kg; Q1–Q3: 48–120) and catecholamine infusion) admitted to our PICU between July 1999 and May 2002. We estimated the accuracy in predicting death of PCT, C reactive protein (CRP: nephelometry) on admission, and the Pediatric Risk of Mortality (PRISM) score in 24 hrs of admission or at the time of death. Sensitivity, specificity, positive and negative predictive values, and percentage of well classified children were calculated at the following cut-off values: PCT >130 ng/ml (the best cutoff value of the PCT level was determined by χ² optimisation (Fisher’s test; p = 0.0004)), CRP <100 mg/l, PRISM value >20 and PRISM probability of death >50%.

Eleven of 35 children died (31%); predicted mortality with the PRISM score was 15.6 (standardised mortality ratio: 0.71; 95% confidence interval: 0.35–1.26). The median (Q1–Q3) PCT and CRP levels and PRISM value and probability of death were the following: survivors vs nonsurvivors) PCT 73 (15–210) vs 277 (208–606) ng/ml (p = 0.001); CRP 92 (44–160) vs 72 (41–109) mg/l (p = 0.025); PRISM value 17 (8–22) vs 33 (26–37) (p < 0.01); PRISM probability 19 (4–42) vs 88 (63–95%) % (p < 0.01). Performance characteristics and AUC SE of PCT, CRP, and PRISM score are given in the table and the figure.

In our study, PCT on admission was as accurate as the PRISM value and PRISM probability of death calculated within 24 hrs of admission or at the time of death, and more accurate than the CRP level in classifying survivors and nonsurvivors of MSS. These results accord with those of Hatherill et al who observed, in 37 children with MSS, that admission PCT level (values not indicated) was higher in nonsurvivors (11%) than in survivors (p = 0.04) and related to the severity of organ failure (p = 0.02); however, in the whole group of children with septic shock whatever the causative organism, admission PCT functioned worse than the PRISM score (AUC 0.73 (0.59–0.88) vs 0.83 (0.71–0.93); statistical comparison not performed).

The PRISM score is accepted in PICUs worldwide and has been reported to accurately predict outcome of meningococcal disease.

Table 1 Performance characteristics of PCT, CRP, and PRISM score in 35 children with MSS

<table>
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<th>Severity index (%)</th>
<th>PCT</th>
<th>CRP</th>
<th>PRISM score</th>
<th>PRISM probability</th>
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<td>100</td>
<td>91</td>
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<tr>
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</table>

References


Incidence of severe and fatal reactions to foods

Although the article by Macdonough et al regarding the incidence of severe and fatal reactions to food would be seem to be reassuring, we would like to express some concerns and raise some questions about the data presented. The first question is whether the ascertainment of cases is really as complete as the authors suggest. We acknowledge that the UK medical system may allow better reporting and access to mortality data than that of the US. However, the records acquired as described seem to represent the same underreporting issues as those in the US. Is it really unlikely that the BPSU misses a significant number of cases? Based upon a well characterised population in Olmstead county Minnesota and extrapolating the data to a US population of 280 million, it may be estimated that there are 200 deaths from anaphylaxis reactions to food each year.

A paper published in 2001, described methodology in which a National Registry had been established and was well publicised to US allergists. Very few reports were made by allergists and none by other physicians. No cases were initially reported by physicians who conduct research in food allergy. Nearly all the cases were ascertained from the press. These news articles appeared in local newspapers and were not reported in media with a large regional or national circulation. In an earlier effort to account for all cases of food anaphylaxis, only in Colorado, a significantly
The higher number of cases were reported from rural regions as compared to metropolitan areas strongly suggesting either misdiagnosis or inaccurate recording of cases in the emergency department log of busy hospitals.

A second concern is the reporting of cases only up to age 19. In the paper mentioned above, of 32 fatalities 10 occurred in youngsters up to age 15. An additional 10 occurred in adolescents aged 16 to 19. Why did MacDougall et al not include all adolescents? A third question must always be raised when fatal food anaphylaxis is studied. Is it not possible that cases of fatal asthma were actually initiated by unidentified allergic reactions to food? All authors in this field are likely to agree that the ultimate cause of death may be irreversible airway obstruction, and all would agree that poorly controlled asthma increases the risk of fatal anaphylactic reactions to food, but we would suggest that the trigger responsible for individual asthma fatalities is not always determined. What about fatalities that never reach the emergency department and are misclassified on death certificates as asthma fatalities? Individuals that die at home and are classified as asthma deaths are unlikely to be further investigated in either the US or the UK.

Fourthly, the authors’ definition of severity seems incomplete. Individuals with severe food reactions who self administer epinephrine often do not go to hospital, are less likely to have conditions that require hospitalisation or cause death, and often they do not report these reactions to their physicians unless specifically queried. Some survive the reaction without treatment, become convinced that they need not avoid a specific food, and never tell their physician. We could argue about the possible progression of these episodes to near fatal or fatal reactions, but the point we believe is that they are frequently under reported. The fifth concern includes the safe administration of epinephrine. We disagree about the risk to children of the administration of a single dose of epinephrine as opposed to withholding that dose. We have no disagreement about aggressive treatment of asthma concurrently, and in fact we think that point should be emphasised. However families reading this commentary may become more fearful, than they currently are, about administering epinephrine. We know that epinephrine is not always life saving even when administered in a timely fashion, however withholding it surely must increase the risk of mortality. Over dosage certainly may occur, but it seems more likely that an overdose would be administered by medically trained personnel than by parents. The over prescription of epinephrine is a debatable issue, however it seems a small price to pay, with a low risk, in order to save even one young life.

Finally, we are very concerned that families will interpret this paper to mean that death from food allergy is very unlikely, and therefore they may relax their vigilance. If families of younger children become less concerned when their children become adolescents it may be difficult to institute a good prevention program. This is in direct opposition to the goal of education programs in the US (The Food Allergy and Anaphylaxis Network, www.foodallergy.org) and UK (The Anaphylaxis Campaign) aimed at making individuals with food allergy and the general population more aware of the problem and the potential for mortality. It is truly unfortunate that we cannot accurately identify all of the individuals who die during allergic reactions to food and use this information to do a better job of preventing these tragedies. We must continue our campaigns of education of medical professionals and the public, and we must be certain that emergency treatment is available when and where it is needed.

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References


Authors’ reply
We thank Bock et al for their interest in our article. We respect their views on the interpretation of the data but it is of course for each reader to come to their own opinion on these. We would like to respond to their comments on the accuracy and validity of our data.

Did our paper under ascertain deaths? Bock et al base their concerns on our methods of case ascertaining and on comparison with another study. We cannot be certain about this but as the text indicated we used many sources and spoke to many experts in the field. We agree we did not search local newspapers but this would have been almost impossible as few were on CD-ROM in the 1990s. As mentioned, we did search national newspapers and all cases we came across were already known through one of our other sources. Finally, since publication, no one has told us of a case we appear to have missed. We specifically studied children up to 15 years because this is the group we were interested in. Many recommendations on risks to children are based on inferences from data covering all ages and we wanted to bring a proper paediatric perspective. Indeed the interpretation Bock et al give to the paper they cite is grossly misleading. They suggest extrapolation to a US population would lead to 200 deaths from food each year; yet the paper, in which there is only one death (occurring during exercise), covers all ages and reactions to all allergens, not just food.

The issue of whether asthma deaths may have been precipitated by food allergy is an important question which we addressed “If a child’s symptoms are only asthmatic and no allergen is suspected, then there is no means for attributing such reactions to food or for knowing if a causal link exists”. Furthermore, such deaths will never have been reported in surveys of food allergy in other countries or in other age groups. No group has been able to address this question satisfactorily and it is a key area for further research.

We are not sure we agree that children, who have self administered epinephrine, often do not go to hospital. However we do not know the proportion and said as much, excluding this group from our definition of severity.

Finally we agree that education of professionals and the public should continue based on the best data available. This include those parents whose children are truly at high risk as well as those many parents that think any immediate hypersensitivity reaction to food means their child is at high risk of an allergic death; when in reality the risk, in the absence of asthma, seems very small. Differ ent parents will come to different views about how to proceed faced by a severe but very small risk, just as we all do in many aspects of our lives.

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Physiologic management of DKA

Inward and Chambers provide a provocative description and discussion of the continuing confusion regarding the issues surrounding rehydration and treatment of the pediatric patient with diabetic ketoacidosis (DKA). They review some of the key issues that link fluid therapy to complications from brain swelling, and question the appropriateness of using a volume of fluid calculated by “maintenance plus deficit”, calling for a second revolution in the management of DKA. In the accompanying commentary, Edge makes several statements concerning fluid therapy in DKA, including that “DKA is associated with severe fluid losses”, “any guidelines for fluid and electrolyte management must be simple to calculate”, that administration of base is a risk factor for renal complications, and that despite published guidelines and “changes in protocols”, there is no evidence that the “incidence of cerebral oedema has changed over the past 20 years”. It is our opinion that the problem in the rehydration of the pediatric patient with DKA...
The degree of dehydration ranges from negligible (<1%) to extreme (>20%). Severe ketoacidemia, however, does cause vasoconstriction which may be manifested peripherally by cool, mottled skin, and Kussmaul breathing which leads to very dry oropharyngeal mucosa. The striking appearance of a parched mouth and the presence of cool, even mottled skin without a critical assessment of vital signs and examination of distal (foot) pulses often results in an erroneous impression of shock and “severe dehydration.” A method for estimation of the volume of deficit was described in 1990 and we continue to use this approach. Successful intravascular repletion requires not only gradual deficit replacement (evenly over 48 hours) but an accurate estimation of the volume of deficit along with correction of the clinical and biochemical response. If the deficit is assumed to be 10–15% but is actually only 3%, that patient will receive excess water independent of the more gradual timeframe and independent of the clinical and biochemical response. Guidelines that have proposed “safe” limits to fluid volumes administered such as 4 litres/m²/day or 50 kg body weight/4 hours violate the concept of the individualised assessment of the degree of deficit. Hypovolemic shock will not be corrected by the administration of large volumes of fluid to children who already have a volume deficit. If the deficit is actually only 3%, they require as little as a typical maintenance dose. The volume of deficit along with correction of the clinical and biochemical response should be better defined. Rapid administration or “pushes” of hypertonic sodium bicarbonate should not be given. On the other hand, there is no evidence that administration of physiologic concentrations of base in the rehydration solution are either harmful or undesirable. In our experience, this practice mitigates the development of hyperchloremic acidosis during treatment.

Physiologic management was first described in its complete form when reference was made to the correction of acidemia and osmolarity. Inward CD, Fiordalisi I, Finberg L. Safe management of diabetic ketoacidemia. J Pediatr 1994;124:879–91.


References

The Position Statement on Injection Technique
The Position Statement on Injection Technique (March 2002, Royal College of Paediatrics and Child Health) discusses needle size and length for childhood immunisation. It concludes that there would seem to be insufficient evidence to advise any recommendation to change current practice in the use of small needles. As the authors of a research study that aimed to provide some evidence base for immunisation practice we would like to respond to this.

Our study of 119 babies aged 4 months receiving their third dose of DPT/Hib vaccine found that significantly less redness and swelling occurred when infants were immunised using the longer 23 gauge 25mm (blue hub) needle rather than when the shorter 25 gauge 19mm (orange hub) needle was used. The magnitude of the reductions was substantial. The position statement is correct to note that in our study the difference in tenderness did not reach statistical significance. However we believe our study still

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justifies a recommendation for the use of the longer needle for immunisation in 4 month old infants.

We believe the non-significant difference in tenderness with the different needles must be interpreted with caution, and should not be taken as a rationale for ignoring the significant benefits in terms of reduced redness and swelling. Tenderness was in fact reduced by the same relative amount as redness, but as tenderness occurred less frequently, the results were not formally statistically significant. We have used Bayesian analyses (using an “uninformative” prior distribution) to formally compute the chance that there is a clinically significant reduction (of at least 25% as specified in the protocol) in tenderness between the long and short needles. At six hours the probability of a clinically significant decrease in tenderness with the longer needle is 73%, whereas the chance of a clinically significant increase is only 2%. The evidence is therefore clearly in the direction of the longer needle causing less harm.

We recognise the need for further evidence on which to base immunisation practice at each of the infant immunisation ages. To this end, we are now conducting a randomised controlled trial involving over 600 infants aimed at providing a definitive answer. In the meantime, we reiterate our recommendation to practitioners to use the longer needle for immunising 4 month old infants.

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Reference

CORRECTION

The paper by Parkin et al in the September issue of Archives (Arch Dis Child 2002;87:221–2) was missing acknowledgements. The following paragraph should have been included:

Rita Arseneault, Audrey Bell-Peter, Diana Cohen, Pauline Matthews, Suzanne Stewart, and Olwen Tennis participated in patient enrollment and data collection. Derek Stephens assisted in statistical consultation. Rose Cheung and Carol Collins did the immunofluorescence microscopy and virus isolation tests. Dr Raymond Tellier oversaw virus testing for part of the time while he was on service.

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