to determining the nature and extent of sensitization. Even then, the outcome would be based on the 'balance of probabilities', as the only direct way of measuring cause and effect in individuals is to perform an allergen specific challenge. The attempts made in our study to qualify the nature of work related respiratory symptoms were confounded by a lack of compliance by the workers. We would suggest that our studies, and the studies of Smith et al., have all determined point prevalence rates of symptoms and sensitization. The assumption by Smith et al. appears to be that workers reporting irritant symptoms, but with sensitization to bakery allergens, are not important. We have taken an alternative view, because we do not know the respiratory sequelae for these individuals, i.e. do they remain within this symptom group or are they an 'at risk' population who are more likely to develop serious respiratory problems related to their working environment if exposure continues. Neither our study nor the studies reported by Smith et al. were prospective in design to enable this question to be answered.

Perhaps we could also correct a couple of other points. The author's comment about the implications of our data is perplexing. We found immunological sensitization and workers reporting respiratory symptoms at dust levels below 5 mg/m$^3$; in no way can this be taken as evidence for ascribing a no adverse effect level for flour dust. Furthermore, with regard to exposure limits, there is no Occupational Exposure Standard (OES) for total inhalable dust. The figure of 10 mg/m$^3$ for total inhalable dusts defines a 'substance hazardous to health' where there is no indication of the need of a lower value (where, for example, the substance is classified as toxic or harmful). This means that the Control of Substances Hazardous to Health Regulations 1999 apply, including the requirement to do a risk assessment, if workers are exposed above this level. Thus the 10 mg/m$^3$ is a trigger value rather than a legal limit.

Dear Sir,

I read with interest the recent paper by Smith and Patton which described the findings of a population of workers in milling, baking and other flour-using industries over five years. I wonder if the authors were able to obtain evidence to examine the effectiveness of the surveillance programme. In particular, do they have data from another group of similarly exposed workers not under surveillance which could be used to compare outcomes with their study group that was under surveillance?

Second, the authors indicated that workers with a history of asthma over the previous five years were not taken into jobs where there was any possible dust exposure. Do they have data to examine the effectiveness of this aspect of the surveillance programme, that is, to compare their findings with workers with a history of asthma who were allowed to work in dust-exposed areas?

Gary Liss
Medical Consultant,
Ontario Ministry of Labour

REFERENCE


AUTHOR'S REPLY

Dr Liss has raised an interesting point about the effectiveness of health surveillance for respiratory conditions. Hard evidence to support a clear benefit to employees from health surveillance is quite difficult to obtain and we do not have such data. Nevertheless, there are two small pointers which suggest there may be some such value. In our study, we compared outcomes of those whose symptoms started prior to the commencement of health surveillance and those whose symptoms started afterwards. In essence these were subgroups without and with surveillance, respectively, at the time when they became symptomatic. Although the numbers were quite small, the new cases of recent onset identified by health surveillance did appear to fare better than the ones who had been symptomatic before the surveillance programme began. Having said this, several other factors may have been responsible for the difference, such as a progressive reduction in dust exposures over a number of years.

There is also some support for a positive effect on outcome from comparison with other studies. The four other studies of outcome of occupational asthma in the UK reported a much more dismal prognosis. The first three studies were based on referrals to specialist occupational lung disease clinics and the fourth from cases reported by either chest physicians or occupational physicians. It is therefore likely that the cases came from a mixed bag of situations regarding the presence or
absence of previous health surveillance. Unfortunately the studies are not directly comparable with our work because they have looked just at asthma cases, whereas we included allergic rhinitis. Their cases also had a wide variety of causative agents, whereas ours were largely due to either flour or fungal amylase. Furthermore, by virtue of the fact that the cases came either from specialist referral or reporting, some selection of the cases probably arose such that they represented the more severe end of the disease spectrum. Taking all these factors into account, it would be fair to say that we can only hypothesize that the better outcome in our study was the result of identifying individuals at an earlier stage.

The second point raised by Dr Liss is whether there is any data to support the effectiveness of excluding known asthmatics from working in potentially dusty areas. Again we do not have good scientific data in support of the policy. The reason why the restriction was first instituted was anecdotal experience of individuals with pre-existing asthma faring badly in dusty jobs and often needing to be moved because of their worsening symptoms. However, there is also quite a reasonable theoretical basis for the policy, taking into account the concept that asthma is dependent upon airway hyperreactivity. Under circumstances of increased airway reactivity, as in pre-existing asthmatics, it might be expected that significant dust exposure would produce bronchoconstriction and hence a worsening of the symptoms.

In summary, I would have to say that the notion of benefit, both from respiratory health surveillance programmes and a policy of exclusion of asthmatics from dusty jobs, has a fairly shaky scientific basis and any value has really to be taken somewhat on trust.

Trevor Smith
Group Medical Adviser

REFERENCES


REFINING THE COMPUTATION OF TOPIC-BASED IMPACT FACTORS — SOME SUGGESTIONS

Dear Sir,

The proposal of 'an alternative to journal-based impact factors' by Takahashi et al. to compute the average impact of articles on a particular topic is an interesting approach. It has the potential to solve the problem of estimating the impact factors (IF) for articles in relatively small fields. Although there have been many articles proposing modified journal IFs, I cannot recall any that translated the data into article-based topic-based IFs.

In contrast to the journal IF, the proposed topic-based IF attempts to group articles rather than journals using medical subject headings (MeSH) in MEDLINE. It therefore relies on the assumption of objectivity in the application of MeSH, but this unfortunately is not always true. The MEDLINE indexing system involves human indexing, which has both advantages and disadvantages. The latter includes inconsistency between indexers in applying MeSH and changing terminology.

As an alternative, the Science Citation Index (SCI) database can be used to create topical databases as Melino and I did recently in an analysis of apoptosis literature. Co-citation clustering would also bring in articles that would not be found in a MEDLINE search. While it is always best to obtain actual citation counts for each article from an SCI search, I believe their method will produce useful results, even though the universe of articles retrieved may not include all the articles that might be relevant to the topic chosen. I would like to see specific examples of their proposed method based on real data as a development of the hypothetical example in their letter. Their method will produce an expected citation impact based on averaging citation counts for the entire database, but only actual citation data for each paper can demonstrate the variations from the most cited to the least cited papers.

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AUTHOR’S REPLY

We are delighted to read Dr. Garfield's response to our proposal on impact factors (IF), especially as Dr. Garfield is the founder of the concept and later developed its use in evaluating journals and their papers. Our proposal was an attempt to improve the IF system so that researchers in different subspecialties, each with unique publication customs, can be provided with their own reference standards.

A major criticism against the current IF system is focused around the issue of cross-subspecialty comparisons. Although the problem with objectivity in MEDLINE keywords (MeSH) should be recognized, MeSH is widely accepted for retrieving articles from the literature. Hence the formation of article groups based on MeSH would serve as a useful and practical unit for calculating the 'topic-based' IF. For individual papers, the