

From: [REDACTED]  
Sent: Friday, 30 January 2009 12:00 AM  
To: MCS  
Subject: comments on draft report

Dear Sir or Madam,

I am writing with respect to the draft report entitled "A SCIENTIFIC REVIEW OF MULTIPLE CHEMICAL SENSITIVITY: IDENTIFYING KEY RESEARCH NEEDS".

I have a Masters in Statistics and I have a few concerns about this draft from a statistical viewpoint.

One section I find concerning is section 3.1.7 on the subject of psychiatric disorders as a possible cause of MCS.

First of all I find the following sentence in this section troubling:

"The importance of interactions between biological, psychological and social factors in the aetiology of psychological disorders has also been noted (Barlow, 1993) and the use of neuropsychological testing in MCS has been reviewed (Bolla, 2000). "

My problem with the first part of this sentence is that the research does not appear to be directly in relation to MCS.

The second part of this sentence is fairly meaningless as it merely notes that there has been neuropsychological testing in the case of MCS, not what the conclusions were. If there are no important conclusions from this research this part of the sentence should be removed. It is important not to quote research papers to give a false sense of evidence to support a theory unless some meaningful conclusion can be drawn from them. This whole sentence provides only the weakest appearance of evidence to the theory of psychological disorders and should be removed or redrafted.

Also troubling is the next paragraph:

"Some researchers also suggest that MCS is an iatrogenic disorder where those providing treatment may inadvertently provide support to their symptoms and concepts of illness (Black, 1995; Labrage & McCaffrey, 2000; Sparks 2000a). "

Some statistics would be useful here so that we can tell what the likelihood of this happening may be however I believe it is well known that if anything MCS is typically met with poor understanding and patients experience a lack of support.

The next sentence in this section is also problematic:

"The prevalence of psychiatric morbidity in MCS has been studied. Black (2000) reported that depending on the assessment procedure used, the prevalence of psychiatric disorders in MCS subjects is 42-100% ".

Why are these statistics so vague? If they are so vague are they meaningful at all? What problem did the researcher encounter that made them unable to determine if the prevalence was in fact 42% or 100%? Is the determination of mental illness reliable? Obviously the implications are very different if the true prevalence is 42% rather than 100%. If the true prevalence is merely 42% then this tends to suggest that psychiatric disorder is not the cause of MCS. There is a similar vagueness in other statistics in this paragraph. A statistician desperately needs to look over this section and examine the methodology in the papers concerned. Comments in your report need to be adjusted to reflect the weakness in evidence - it is important not to present weak evidence as strong evidence. Is there any evidence that the psychiatric disorders were pre-existing in the patient's history? Your report mentions anxiety (21%) and depressive disorders (19%). Is this not what you would expect an illness such as MCS to cause? Comments in your report need to be adjusted to reflect the ambiguity in the evidence.

I find this pattern of vagueness continues throughout this section and heightened anxiety is remarked upon again and again. Have these patients been compared to another group of patients with a stressful illness which is poorly understood and lacking in proper diagnosis and treatment methods? All in all, I find this section to be of low scientific merit and in desperate need of redrafting. It is irresponsible and offensive to suggest that a large group of people complaining of feeling physically ill have problems of a merely psychiatric nature when there is no strong evidence to suggest this is the case. Comments in this section need to be changed to reflect the weakness in the evidence.

My final concern relates to the prevalence section of this report. It is important that the population sampled from for prevalence calculations be representative of the population as a whole, if it is not this could lead to bias. For instance, it is well known that many patients with MCS avoid public buildings. This means that calculations based on taking a random sample of people in a public building would be lower than the true prevalence. In particular I draw your attention to the following paragraph on page 34 of your report:

“Bell and colleagues reported that 15-22% of a sample of college students reported feeling moderately or severely ill after exposure to at least three of five common substances (ie. pesticides, paint, perfume, car exhaust and new carpet) (Bell et al. 1993a, b). Subsequently, the same investigators found that 28% of college students considered themselves to be "especially sensitive to certain chemicals", but the results were dependent on the type of query. Only 9.7% reported illnesses related to chemicals and only 0.2% of college students reported physician-diagnosed MCS (Bell et al. 1996).”

The main problem with this paragraph is that many MCS sufferers would be unable to attend college and hence the true prevalence statistics would be much higher than the ones quoted here. It is also worth noting that college students do not constitute the age group with the highest expected prevalence rate and that this is not the prevalence rate of the population as a whole.

It is very important that a statistician check the sampling methods used in the studies these prevalence calculations are taken from otherwise these statistics may be drastically biased and virtually worthless. Clearly this has not been done properly as the above excerpt shows. I

would also very much like to be able to check the methodology of the two South Australian surveys were commissioned by the State Health Department (September 2002 and June 2004) that have been used to determine the prevalence of MCS in Australia. By what method were these subjects randomly selected? Were there non-respondents? I have not been able to find this study in the references section of this report. Would you please advise me as to where it is available?

Also on the subject of prevalence data, I believe it would be of great interest to see some data on how long sufferers of MCS have had their illness. Also, is the rate of new cases of MCS steady or increasing? Perhaps this could be the subject of future research if no such data is presently available.

I look forward to your reply,

Regards,

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