Correspondence

Seroprevalence of Syphilis and HIV in intravenous drug users

Sir,

I read with interest the paper titled “Seroprevalence of Syphilis and the HIV in intravenous drug users” and I submit respectfully the following comments: The authors mentioned in the Result Section, and I quote “there were 934 admissions to the Al-Amal Hospital for detoxification of drug use during the period selected. Of the 934 patients”. Furthermore, there is no mention in the methodology section or anywhere else, that these patients were indeed, intravenous drug abusers in contrast to other drug abusers such as cannabis, alcohol, marijuana, etc. Therefore, based on the authors’ above statement, it is impossible that all of these 934 admissions to their hospital during the 6 months period were 100% intravenous drug abusers, in addition that all 934 admissions were new patients and none were re-admitted during the same period, as all admissions were considered new patients, which is highly unlikely, particularly in this setting.

Hence, it is not surprising to find that the authors results are different and discrepant from other studies in the West. Furthermore, the title appears to be misleading as not all admissions studied were intravenous drug abusers, and finally, it would have been more interesting if the authors had Hepatitis B and C markers performed on their patients, as both are sexually and parenterally transmitted.

In summary, I believe this paper’s figures are unlikely to represent true facts about intravenous drug users in Saudi Arabia.

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Sir,

Zimmo and Njoh have provided interesting data on “Seroprevalence of Syphilis and HIV in intravenous drug users” in the Kingdom. However, their interpretation of the serological tests for syphilis (STS) deserves some comments. They found that 23 (2.46%) of 934 patients were reactive to the VDRL (Venereal Diseases Research Laboratory) test, and only 10 (1.07%) were positive by the TPHA (Treponemal Pallidum Hemagglutination Assay) test and presumed these patients to be syphilisics. In a country such as Saudi Arabia where both bejel and syphilis co-exist, it is erroneous to conclude that these patients have had syphilis, without proper confirmatory test or clinical evidence of syphilitic lesions.

Bejel or endemic syphilis is caused by Treponemal pallidum subsp. Endemicum and it is a non-venereal treponematosis that has its onset in childhood and is transmitted from child to child by close skin contact. Pockets of bejel have been reported in rural areas of the northwest and Asir region of the Kingdom. The etiological agent of bejel is morphologically and serologically indistinguishable from Treponemal pallidum subsp. Pallidum, the cause of venereal syphilis. In any community the social consequences of mistaking non-venereal bejel for venereal syphilis can be catastrophic especially in females. In the developed countries, where nonvenereal treponematoses (Bejel, Yaws, Pinta) do not exist, it is usually assumed that patients with positive STS have acquired venereal syphilis, but this is not so in countries where both nonvenereal treponematoses such as bejel and syphilis co-exist. It is for this reason that we called the attention of physicians in the Kingdom in our publication on “Bejel in Pregnancy” to the problem of interpretation of STS in patients in the Kingdom. In the study by Zimmo and Njoh, the patients with positive VDRL test should have had the sera titrated. In Bejel the titre is usually equal to or less than 1 in 8, while it is much higher in active venereal syphilis.

A confirmatory treponemal test capable of detecting treponemal IgM antibodies should have been performed such as FTA IgM (Welcome Reagents Ltd, UK) or EIA IgM (Cantia Syphilis M, Mercia Diagnostics, Surrey, UK). These tests are capable of detecting treponemal IgM class antibodies. If the confirmatory tests are negative, then the positive VDRL and TPHA tests were due to old bejel, indicating residual treponemal IgG antibodies from an old treponemal exposure. It is to be remembered that while VDRL detects both IgM and IgG antibodies, TPHA detects mainly IgG antibodies and should not be used as a confirmatory test for syphilis as in this instance. Treponemal antigens tests such as TPHA and FTA Abs once reactive remain so for life.

We would like to speculate that the reason for low seroprevalence of STS in this study, compared with that in developed countries, was because most of the patients had bejel in childhood rather than venereal syphilis in adult life. We have not had the opportunity to review the data quoted by the authors emanating from the Ministry of Health that the prevalence on syphilis in Jeddah is 2% i.e. 2,000 per 100,000 of the population. If this were so then
syphilis should be a serious public health concern in Jeddah, (more prevalent than tuberculosis) and hence considerably large numbers of congenital syphilitic babies should be seen in the Pediatric units in Jeddah, which is clearly not the case. We hope this data also does not suffer from incorrect interpretation of the STS. In our hospital, we confirm all positive VDRL and TPHA tests (in both patients and blood donors) by the EIA IgM test and we have only seen two positive cases in three years and these were in patients with clinical evidence of syphilis and definite history of exposure.

We would like to counsel that results of STS in Saudi patients should be interpreted with great caution as incorrect interpretation may have serious social consequences and appropriate confirmatory tests should be performed in every instance of positive STS.

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Reply from authors

We would like to thank Dr. Altraif for his valid comments. The 934 patients mentioned in the article were admitted and tested between the 1st April 1996 and the 31st October 1996. Re-admission of patients within 6 months is uncommon in most cases for various reasons. It is our error that all the 934 patients were classified as intravenous drug users. In this hospital it has been found that about 70-80% of all admissions are intravenous drug users. This includes those who use the IV route only or along with other routes of drug administration. The rest of the patients use other routes such as oral inhalation.

We would also like to thank Dr. Osoba and Mr. Al-Shareef for their valid comments about the interpretation of serological tests for syphilis in Saudi patients. We agree that bejel has been reported in rural areas of the Northwest and Asir region particularly in the Bedouins who had been born and bred in the desert. So far, to our knowledge there is no similar reports documenting the prevalence of bejel in the major cities like Riyadh and Jeddah. None of the specific treponemal serological tests are capable of differentiating between syphilis, yaws, pinta or bejel, so caution should be used in interpreting the results of the serological tests for syphilis in our area. A strongly positive VDRL test or strongly positive FTA-ABS IgM test would make syphilis the likely diagnosis. A negative FTA-ABS IgM test does not help differentiate between old yaws and latent syphilis. The clinical presentation and the history of sexual contact becomes an important issue in the diagnosis of syphilis versus bejel. In our serologically positive patients, two patients had VDRL titre 1 = 16 & 1-32 while the rest were 1 = 8. Three patients had another sexually transmitted disease. Six patients admitted having multiple sexual partners. All these patient are Heroin-IV users and are engaged in high risk sexual practice. Nine of them are from Jeddah, while one was from Makkah. The prevalence of syphilis in some areas of the Kingdom was 2% in Jeddah, 4-7% in Tabook and 6.6% in Riyadh Abbas, in a five year study of antenatal samples in Jeddah at King Abdulaziz University Hospital observed an increase from 0.24% in 1979 to 1.4% in 1983 Aman, reported an overall increase from 2.1% in 1981 to 4.3% in 1983 among our-patient clinic attendants of the same hospital.

Again we agree with Dr. Osoba that bejel is endemic in some parts of Saudi Arabia and great caution should be taken in interpreting the serological tests for syphilis in Saudi Arabia. Further Kingdom wise studies should be carried out now to establish the prevalence of syphilis and bejel.

So, the fact that we found the low prevalence of 1.0% in heroin IV users who are engaged in high risk sexual practice cannot be due to bejel, because if bejel was prevalent we should have a much higher number of cases.

We think if bejel prevalence is high in the cities, we should have a much more positive screening serological test. Pace found the rate of seropositivity FTA-ABS in the male nomads 19% and in semi nomads 14.5%.

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Reply from author

I read with great interest the comment of Dr. Samani and I appreciate his valuable criticism. I fully agree and support the necessity for a double blind placebo controlled trial to prove the efficacy of a given drug or drugs, etc.

However, the regimen tested in our study have been subjected to intensive investigations and its efficacy have been demonstrated in several controlled trials in Western population. Therefore, the need for a placebo control group was not of a major importance.

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Efficacy of metronidazole-Based triple therapy on eradicating H pylori positive peptic ulcers in mainly Saudi patients

SIR,

The article of Saleh M. Al-Amri et al,1 was very interesting, but it is an open labeled trial and can not prove the benefits of the antibiotic regimen. For proving the efficacy of a drug regimen, we must design a double blind placebo controlled trial. Without a control group, who receive placebo instead of antibiotics, we can not say: “This is the effect of antibiotics”, but this kind of trial is a good beginning for controlled trials. Now I have a question from researchers: “Why aren’t we going to correct our researches’ methodology so that every one from all around the world can use it?” I think Dr. Saleh et al could consider a matched control group that receive anti-acid plus placebo for the same time, couldn’t they? But I appreciate the good work of this group especially in other parts of the article that have no errors.

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References