

against disease transmission. Strategies should be sustained, regional (large as practicable); acknowledging, and guided by, Aboriginal kinship networks; and recommend observed drug treatment (which was negotiated for the final treatment in our study). A non-uniform approach could include more frequent treatment in hyperendemic communities, probably leading to less net use of antibiotic treatment.

Reports from the Kimberley Population Health Unit show a very mixed picture, with wide year-to-year fluctuations in prevalence in many communities. While hyperendemic communities remain in a region, the prevalence of trachoma may increase unnoticed in communities no longer screened because their prevalence has dropped below 5%. If not looked for, it is unlikely to be noticed. Further analysis, such as the graph provided by Johnson and Mak, is to be applauded in the context of a thorough analysis. When this happens, it will greatly strengthen the case for active trachoma control in other regions.

For trachoma prevention, and for many other reasons, we believe environmental health interventions are critical. These remain difficult to evaluate given the high mobility of people in Aboriginal communities. Reliable, long-term, regional environmental health and mobility data are needed as part of this broad issue. □

## Gestational diabetes in Victoria in 1996: incidence, risk factors and outcomes

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**TO THE EDITOR:** Stone and colleagues must be complimented for their study of gestational diabetes mellitus (GDM) published recently in the Journal.<sup>1</sup> Although the study was comprehensive, in that it linked two pertinent records for 99.3% of the women delivering in Victoria in 1996, their analysis was necessarily limited to the parameters recorded in those two data sets.

Stone et al do not refer to relevant recent work from Toronto, Ontario,<sup>2</sup> and Sunshine, Victoria,<sup>3</sup> which has already determined the maternal risk factors for GDM: increasing age, racial origin, family history of diabetes mellitus, and pre-pregnancy body mass index. Data on the latter two of these four factors were specifically noted as not available to Stone's group, but could have been mentioned as *established* risk factors to consider in patient management.

The findings of Stone et al regarding age are similar to those previously reported. However, the relative risk for GDM in the Sunshine cohort increased from 25 years of age (odds ratio, 1.9; 95% CI, 1.3–2.7).<sup>3</sup> Recasting the all-Victorian data<sup>1</sup> with “<25 years” as the reference datum would most likely produce a similarly significant result to that found in the Sunshine study.

Again, the Victorian data on racial origin are but the Sunshine data writ large. Stone and colleagues had to choose a reference datum for this parameter, but have not avoided a problem that bedevils all such work in the “New World”: there is as yet no definitive Australian reference datum available for racial origin. For example, the designation “Australian” may be used, but parents who are themselves second or third generation Australian-born may have, say, pure Maltese ancestry, giving them a high risk of GDM and thus distorting the reference datum. I have argued this case in more detail elsewhere,<sup>4</sup> and took measures to circumvent the problem in the Sunshine study.<sup>3</sup>

Among their findings, Stone et al confirm that there is a significantly increased incidence of macrosomia in GDM-affected infants. I agree. Their work is unquestionably the definitive statement of Victoria's GDM-related macrosomia status (as at 1996), but it lacks one vital ingredient: it defines macrosomia, but does not cite the source of the data used as the study's benchmark. To enable comparisons with their work in future years, could we please have that benchmark referenced so that others may use it too?

1. Stone CA, McLachlan KA, Halliday JL, et al. Gestational diabetes in Victoria in 1996: incidence, risk factors and outcomes. *Med J Aust* 2002; 177: 486-491.

2. Naylor CD, Sermer M, Chen E, Farine D. Selective screening for gestational diabetes mellitus. Toronto Trihospital Gestational Diabetes Project Investigators. *N Engl J Med* 1997; 337: 1594-1596.
3. Davey RX, Hamblin PS. Selective versus universal screening for gestational diabetes mellitus: an evaluation of predictive risk factors. *Med J Aust* 2001; 174: 118-121.
4. Davey R. Of gestational diabetes, finesse, and an antipodean snark. *Diabetes Care* 1999; 22: 873-874. □

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**IN REPLY:** We thank Davey for his comments, which provide us with the opportunity to highlight the benefits and limitations of reports using population-based data. The value of population-based data is that the reported incidence, risk factors and outcomes reflect current practice in the whole of Victoria and are not subject to bias introduced by local referral patterns or clinical practice. Our study<sup>1</sup> shows that, in addition to established risk factors for gestational diabetes mellitus (GDM), the reported incidence varies according to hospital size and geographic location, demonstrating the type of bias that can occur. In addition, the large number of subjects in our study (over 60 000) enables more accurate analysis of subgroups.

A limitation, already highlighted in our discussion, is that we are restricted to the parameters available within the data sources used. Davey and Hamblin's article<sup>2</sup> demonstrates the difficulty of obtaining individual patient data on body mass index, racial grouping, and family history of diabetes. Even working at the hospital level, they had to extrapolate from population-level data to derive an estimate of these risk factors among the control subjects.<sup>2</sup> Given that their study population is a subgroup of ours,<sup>1</sup> it is no surprise that the two studies showed similar results.

An important implication for providers of health services is that, with increases in the age at which mothers give birth and in the number of births to

Asian-born mothers,<sup>3</sup> we predict that the prevalence of GDM in Victoria will rise.

Davey correctly points out that our article does not refer to a relevant 1997 study by the Toronto group.<sup>4</sup> However, we do actually refer to a later publication by the same group.<sup>5</sup>

The problem of ethnicity and migration arises in studies of conditions that are not only polygenic but also a result of complex interactions between a person's genes and his or her environment.

Lastly, the source of information on macrosomia was 1996 population data. We have since produced a percentile chart of weight (g) for gestational age (weeks) based on 15 years of Victorian data.<sup>6</sup>

1. Stone CA, McLachlan KA, Halliday JL, et al. Gestational diabetes in Victoria in 1996: incidence, risk factors and outcomes. *Med J Aust* 2002; 177: 486-491.
2. Davey RX, Hamblin PS. Selective versus universal screening for gestational diabetes mellitus: an evaluation of predictive risk factors. *Med J Aust* 2001; 174: 118-121.
3. Riley M, Halliday J. Births in Victoria 1999-2000. Melbourne: Perinatal Data Collection Unit, Victorian Government Department of Human Services, 2001.
4. Naylor CD, Sermer M, Chen E, Farine D. Selective screening for gestational diabetes mellitus. Toronto Trihospital Gestational Diabetes Project Investigators. *N Engl J Med* 1997; 337: 1594-1596.
5. Sermer M, Naylor CD, Farine D, et al. The Toronto Tri-Hospital Gestational Diabetes Project. A preliminary review. *Diabetes Care* 1998; 21 Suppl 2: B33-B42.
6. Halliday J, Ellis I, Stone C. WUDWAW: "Who usually delivers whom and where". Report on models of antenatal care. Melbourne: Perinatal Data Collection Unit, Department of Human Services, 1999. □

## Injecting drug use in Australia: needle/syringe programs prove their worth, but hepatitis C still on the increase

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**TO THE EDITOR:** Law and Batey<sup>1</sup> rely on a flawed study for their conclusion that needle/syringe programs (NSPs) have saved lives and money.

The study in question<sup>2</sup> compared the incidence of HIV and hepatitis C virus (HCV) infections in cities round the world and concluded that cities with NSPs had achieved reductions in HIV incidence that were not seen in cities without NSPs. However, interestingly, no similar reduction in HCV incidence was reported.

Detailed reading of the study shows that a third of the cities (22/67) without

NSPs were in Thailand — a country in which, unfortunately, there are many other reasons why HIV incidence is increasing rapidly. Given the large proportion of Thai cities included in the study, it is plausible that the rapid rise in HIV incidence in these cities biased the overall results of the study, leading to an erroneous conclusion that NSPs themselves were associated with a reduction in HIV incidence in cities worldwide.

Further reading of the study shows that HCV incidence was not measured in any studies in Thailand. So, the reported lack of effect of NSPs on HCV incidence depends on comparisons between cities with and without NSPs from other parts of the world, perhaps less affected by some of the problems in Thailand.

In conclusion, if Thai cities had been excluded from the study, it seems likely that no change in the incidence of either HCV or HIV might have been found in association with NSPs. The original study needs urgent re-analysis to see if this is in fact the case.

1. Law MG, Batey RG. Injecting drug use in Australia: needle/syringe programs prove their worth, but hepatitis C still on the increase. *Med J Aust* 2003; 178: 197-198.
2. Commonwealth Department of Health and Ageing. Return on investment in needle and syringe programs in Australia. Canberra: Department of Health and Ageing, 2002. Available at: <http://www.health.gov.au/pubhlth/publicat/hac.htm> (accessed Jun 2003). □

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**IN REPLY:** Copeman's criticism of the report *Return on investment in needle and syringe programs in Australia*<sup>1</sup> is essentially that the comparison of the effectiveness of needle/syringe programs (NSPs) is confounded by other factors. This point, and its implications for the results, was extensively discussed in that report. Copeman suggests that the estimated reduction in HIV due to NSPs might largely be attributable to the inclusion of data from many cities in Thailand that do not have NSPs. This criticism is not supported by the data. A sensitivity analysis including only cities from developed countries was performed at the time of the report (see Methods, Section 3.1.2, page 13<sup>1</sup>), but was not included among the

report's results because of space constraints and because it didn't alter the main findings. The analysis of cities in developed countries showed an overall mean reduction in the annual rate of change in HIV seroprevalence of -30.0%, compared with -24.7% based on all cities, albeit with lower statistical significance ( $P=0.105$  v  $P=0.057$ ), reflecting the loss in power through exclusion of cities.

Copeman's assertion that the report indicated that NSPs had no effect on rates of hepatitis C virus (HCV) infection is incorrect. The report estimated that, following the introduction of NSPs, HCV prevalence among injecting drug users declined by 2% per annum, compared with no introduction of NSPs ( $P<0.001$ ).

The report is freely available on the Internet,<sup>1</sup> and we encourage readers to look at it for themselves.

1. Commonwealth Department of Health and Ageing. Return on investment in needle and syringe programs in Australia. Canberra: Department of Health and Ageing, 2002. Available at: <http://www.health.gov.au/pubhlth/publicat/hac.htm> (accessed Jun 2003). □

## Statistical methods in clinical trials

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**TO THE EDITOR:** GebSKI and Keech describe with clarity and accuracy the important basic concepts of statistical analysis for physicians.<sup>1</sup> I would like to draw attention to two issues.

Firstly, the authors refer to common measurement scales that are used in medicine. It is crucial to understand the limits of a measurement to begin to appreciate results from any study. They describe the continuous scale and offer blood pressure and temperature measurements as examples. This scale refers to data determined such that the distance between any two points is known and measurable. Siegel used the term "ratio scale" if there was a true zero point to the measurement.<sup>2</sup> This contrasts to an ordinal categorical scale, in which the intervals are not constant. The scale referred to can be transformed, and is anchored with respect to