

LETTERS TO THE EDITOR

Explanations for differences in health outcomes between neighbourhoods of varying socioeconomic level

EDITOR.—With much interest, I read the review of Pickett and Pearl regarding the effect of neighbourhood socioeconomic level on health outcomes.¹ They conclude that there is fairly consistent evidence for modest neighbourhood effects on health, because 23 of the 25 reviewed studies report a statistically significant association between at least one measure of social environment and a health outcome, after adjusting for individual level socioeconomic status.

I agree with the conclusion of the authors that most studies show only modest differences in health outcomes between neighbourhoods of varying socioeconomic level. However, I am far less sure than they are that this is a real neighbourhood effect. Incomplete adjustment for individual socioeconomic status may be a much more likely explanation for the modest differences as found. For instance, regarding mental health, differences between areas of varying socioeconomic level in Amsterdam, the Netherlands, become small and without statistical significance if individual socioeconomic status is adjusted for by several measures jointly. In contrast, adjustment for separate measures of individual socioeconomic status leaves modest differences between neighbourhoods of varying socioeconomic level. Some of these previously published results are shown in table 1.² Similar effects have been found for other measures of health like self reported health, health complaints and obesity, and to a lesser degree regarding smoking and long term functional limitations.³ In the same way, incomplete adjustment for individual socioeconomic status may explain the modest differences between areas as Pickett and Pearl found in their review.

Pickett and Pearl¹ propose another explanation for the findings as presented in the preceding section.^{2,3} They explain the mostly negative findings in these two publications by a lack of power, because of a limited number of areas (that is, 22). However, they seem to be unaware of a later reanalysis of the same data that contradicts their explanation.⁴ This reanalysis yields very similar (mostly small)

differences in health outcomes after adjustment for individual socioeconomic status for boroughs (n=22), neighbourhoods (n=92) and postcode sectors (n=76). At least for these data, a lack of power thus seems to be no valid explanation for mostly lacking area differences in health outcomes after rigorous adjustment for individual. I invite Pickett and Pearl to examine the impact of an incomplete adjustment for individual socioeconomic status in all studies that they included in their review: does this explanation hold, or do the results of studies on area differences in health outcomes vary because of other reasons?

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- 1 Pickett KE, Pearl M. Multilevel analyses of neighbourhood socioeconomic context and health outcomes: a critical review. *J Epidemiol Community Health* 2001;55:111-22.
- 2 Reijneveld SA, Schene AH. Higher prevalence of mental disorders in socioeconomically deprived urban areas in the Netherlands: community or personal disadvantage? *J Epidemiol Community Health* 1998;52:2-7.
- 3 Reijneveld SA. The impact of individual and area characteristics on urban socioeconomic differences in health and smoking. *Int J Epidemiol* 1998;27:33-40.
- 4 Reijneveld SA, Verheij RA, De Bakker DH. The impact of area deprivation on differences in health: does the choice of the geographical classification matter? *J Epidemiol Community Health* 2000;54:306-13.

Authors' reply

EDITOR.—We appreciate Dr Reijneveld's interest in our review of neighbourhood socioeconomic level and health outcomes.¹

Dr Reijneveld suggests that much of the "effect" attributed to neighbourhood level socioeconomic processes could be explained by a lack of control for individual level socioeconomic measures. We agree that neighbourhood studies are at risk for overestimating effects, and emphasised the importance of measuring individual level SES on page 116 of the article, and again in our discussion on pages 119 and 120. As we specifically mentioned, in general, adjusting for more measures of individual SES is associated with smaller effect sizes. However, as we also discussed, adjustment for individual level SES may in fact remove a true neighbourhood effect if individual level SES is affected by neighbourhood level socioeconomic circumstances. There is substantial evidence in the sociological literature showing that educational attainment is strongly influenced by neighbourhood level factors.² Despite this, our table's summary of published studies shows persistent neighbourhood level

associations after simultaneous statistical adjustment for multiple individual level indicators (for example, Shouls *et al* 1996, Jones and Duncan 1995, Robert 1998, Haan *et al* 1987). In addition, the evidence put forth in the above table regarding mental health does not make a strong case for multiple-indicator adjustment. The lack of statistical significance seems to be attributable to adjustment for income alone, rather than multiple adjustment (compare OR=1.21 (1.01, 1.46) adjusting only for income, to OR=1.18 (0.98, 1.42) adjusting for income, education and occupation). If area level socioeconomic factors act as proxies for individual level characteristics, it is probable that they are proxies for individual level income, which is lacking in many studies. Again, however, almost all of the reviewed studies that included individual level income information revealed some neighbourhood level associations (for example, Curry *et al* 1993, Robert 1998, Jones and Duncan 1995, Diez-Roux 1997, Waitzman and Smith 1998). We believe the statistical "effect" is real, although any interpretation is still unclear. While residual confounding is always a possible explanation, the body of evidence suggests that alternative explanations are also likely.

We are grateful to Dr Reijneveld for referring us to his reanalysis of neighbourhoods and health outcomes,³ which was published after the completion of our review. Indeed it seems that lack of statistical power in this case was not the explanation for the modest impact of area level deprivation on self reported health. An alternative explanation might be the relative homogeneity of socioeconomic status in the Netherlands. For example, among Dr Reijneveld's sample of 92 neighbourhoods, the least deprived neighbourhoods in terms of low income contained 35% low income residents, while the most deprived contained 52%. We suspect that the range between advantaged and deprived neighbourhoods would be far greater in many American cities and that neighbourhood "effects" might be much stronger at the extremes of the distribution.

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- 1 Pickett KE, Pearl M. Multi-level analyses of neighborhood socioeconomic context and health outcomes: a critical review. *J Epidemiol Community Health* 2001;55:111-22.
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Table 1 Odds ratios (and 95% confidence intervals) comparing the prevalence of apoor mental health (that is, an increased score on the General Health Questionnaire) for tertiles of Amsterdam boroughs, grouped by deprivation; crude, adjusted for age and gender, and additionally adjusted for individual socioeconomic status

| | Crude | Adjusted for age/gender* | Age, gender + occupation‡ | + income† | + education§ | + occupation, income, education |
|------------------|-------------------|--------------------------|---------------------------|-------------------|-------------------|---------------------------------|
| Household income | | | | | | |
| least deprived | 1 | 1 | 1 | 1 | 1 | 1 |
| intermediate | 1.35 (1.12, 1.62) | 1.32 (1.10, 1.59) | 1.27 (1.06, 1.52) | 1.21 (1.01, 1.46) | 1.32 (1.11, 1.58) | 1.18 (0.98, 1.42) |
| most deprived | 1.38 (1.16, 1.64) | 1.37 (1.15, 1.62) | 1.24 (1.05, 1.48) | 1.13 (0.95, 1.35) | 1.32 (1.12, 1.56) | 1.09 (0.91, 1.30) |

The least deprived tertile of boroughs is used as the reference category in all models; see original publication for details.² *Adjusted for age, gender and their interactions. †Income in five levels, adapted for the number of people in the household which depend on it (1 or more). ‡Present occupation in five levels; if no job: unemployed and looking for work, student, long term disabled, housekeeping, and retired. §Highest degree earned in four levels: primary school, lower secondary school, higher secondary school, post-secondary education.

Sex ratio at birth, latitude, hormones, and temperature

EDITOR,—Grech *et al*¹ reported that sex ratio (proportion male) at birth declines highly significantly with increase in geographical latitude in Europe. I offered evidence that this may be attributable to variation of maternal hormone levels with latitude.² Grech,³ while acknowledging this possibility, proposes that the effect with latitude may be secondary to an effect with temperature. Accordingly he writes: “an interesting study would be the analysis of seasonal variations of sex ratios at birth, for individual countries. A latitude effect would thus be excluded, and any variations in the birth sex ratio would be more likely to be caused by a temperature variation effect”. If Grech were correct, conceptions in cold seasons should produce an excess of female births. I have reviewed 17 studies of seasonality of sex ratio at birth.⁴ The results of the smaller studies were inconclusive, but the largest studies found that in the USA, over half a century, sex ratios at birth tended to be high in early summer, and low in autumn and winter. This was so for black and white births, for urban and rural births and for stillbirths. The variation was very slight—that is, from a peak of 0.5140 in June, to a trough of 0.5116 in February. Thus there is a tendency for a low sex ratio at birth to be associated with spring and summer conceptions (rather than winter ones). In short, the available evidence is against Grech’s suggestion. However, the variation of sex ratio with season and latitude (and a number of other variables, for example, maternal age, paternal age, birth order, race, social class, smoking) is so slight as probably to preclude useful search

for the causes. In contrast, large sex ratio variations are associated with various forms of placental pathology. Highly substantial and significant male excesses are associated with abruptio placenta, placenta praevia, fatty liver of pregnancy, and toxemia: highly significant female excesses are found with placenta accreta and extrauterine pregnancy.⁵ I suggest that workers should pursue this variation because it is clear that some of these disorders are not caused by the (sex of the) fetus. I have hypothesised instead that they are caused by maternal hormone profiles that predate the disorder and are responsible both for the disorder and the sex of the fetus. The point could be tested, among others, by examining the extent to which hormone concentrations control fallopian tube motility.

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- 1 Grech V, Vassallo-Agius P, Savona-Ventura C. Declining male births with increasing geographical latitude in Europe. *J Epidemiol Community Health* 2000;54:244–6.
- 2 James WH. Sex ratio at birth and latitude. [Letter]. *J Epidemiol Community Health* 2001;55:216.
- 3 Grech V. Author’s reply. *J Epidemiol Community Health* 2001;55:216.
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Author’s reply: More on sex ratios at birth

EDITOR,—The proposed theory—that is, that maternal hormone profiles are responsible for reproductive disorders that may produce an

excess of one gender over another—is very interesting.¹

However, the difference in gender ratio by latitude noted in our study, although small, was highly significant ($p < 0.001$) because of the large number of live births included (over 16 million) over the relatively short period studied (1990–95).² While a large literature review by James in 1987 failed to find any strong association between ambient temperatures and M/F,³ a more recent study by Lerchl in 1999, which included approximately 50 million live births, showed that in Germany, over the period 1946–1995, significantly more male children were born in April to June. Naturally this peak was conceived in July to August, the warmest part of the year.⁴

How could maternal hormone profiles and the environment be tied together? What might influence maternal hormone levels in different latitudes, and hence different countries? Would such hormonal differences be innate, perhaps because of interracial differences, or might external influences, such as diet or sunlight be responsible?

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- 1 James WH. Sex ratios of offspring and the causes of placental pathology. *Hum Reprod* 1995;10:1403–6.
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