It has been more than 10 years since social epidemiologists focused their attention on estimating the independent effect of neighbourhood contexts on health outcomes. My read of the literature is that the flurry of research, and the corresponding taxpayer investment, is tapering, with fewer and fewer neighbourhood-effect studies being funded or published. After a decade’s worth of ambiguous results, this is probably a good thing. As I wrote almost 10 years ago, without experimental manipulation it is very difficult to determine the extent of a neighbourhood’s influence on health outcomes. Among other issues, selection and structural confounding undermine the identification of neighbourhood effects in all non-experimental designs. Further, the potential for policy change as a result of neighbourhood effects research seems increasingly remote, at least in the USA. The new paper by Sariaslan and colleagues (hereinafter ‘the Authors’) only reinforces my perspective.

The paper merits attention because the Authors create and exploit an astonishingly rich data resource that seems available nowhere else, certainly not in the USA, where distrust of government monitoring appears to be growing. For purposes here, the key characteristics of the data are its population scope, the composite deprivation index, the longitudinal information on siblings and the observed residential mobility. The Authors’ careful analysis shows that the independent effect of neighbourhood contexts on violence and substance misuse is negligible. Among other strengths, I am most impressed with the Authors’ efforts to consider alternative explanations for their findings. Social epidemiology needs more of this kind of work.

But, as science tends to advance by criticism, I shall offer some. First, I am not convinced by the Authors’ specification of their deprivation index. Do divorce and immigrant status really imply deprivation? Further, if the index was created from the very same data used to assess its relationship to the study’s outcome variables, any association between the index and outcomes capitalizes on chance, rendering P-values too small. It is always best to create an index (i.e., latent variable) in a different data set than the one in which its associations with other variables are evaluated; split-half and independent sample techniques are recommended. That said, it is hard to imagine alternative specifications or procedures would alter the study’s conclusions. I also might quibble with the Authors’ use of the term ‘quasi-experiment’. I reserve this term for designs with an exogenous intervention/treatment that is not randomized by a researcher or by Mother Nature. But again, this has no impact on the findings.

Ultimately, my primary concern rests with the Authors’ (in)ability to identify desired causal effects. As the word implies, identification is the process of teasing out empirically defensible causal effects from competing explanations even as sample sizes approach infinity. Identification means that one and only one explanation or model explains the data, that there are no competing explanations for the very same results. Table 2 in the Authors’ paper reveals the expected relationships: as neighbourhood deprivation increases, so too does the prevalence of undesirable outcomes. This is straightforward descriptive epidemiology and tells an important story: the probability of a subject committing a violent crime is 0.008 in an advantaged neighbourhood and 0.055 in a deprived neighbourhood. Although negligible in absolute terms, there is a nearly 7-fold increase in violent crime from the lowest to the highest decile of deprivation.

The Authors next ask how much variation is potentially attributable to neighbourhoods themselves. The answer comes in their Table 3, which presents intra-class correlation coefficients (ICCs). Results show that 12% and 4% of violence and substance misuse, respectively, are potentially attributable to neighbourhoods. Although negligible in absolute terms, there is a nearly 7-fold increase in violent crime from the lowest to the highest decile of deprivation.
model [see Rodriguez and Elo 2003 for presentation of ICC for binary outcomes’]

\[ y_{ij} = \mu + \mu_i + \epsilon_{ij}. \]

Let \( i \) index neighbourhoods and \( j \) the persons nested within them. Within the neighbourhood effects literature, \( \mu \) is a constant and represents the grand mean of the outcome variable, \( y_{ij} \). Neighbourhood effects are represented by \( \mu_i \), which are assumed to be distributed \( N \sim (0, \sigma^{2}_\mu) \). The person-level residuals are \( \epsilon_{ij} \) and assumed to be distributed \( N \sim (0, \sigma^{2}_e) \). The ICC is

\[ \rho = \frac{\sigma^{2}_\mu}{\sigma^{2}_\mu + \sigma^{2}_e}, \]

and said to represent the degree of clustering in \( y_{ij} \) that is potentially attributable to neighbourhoods. Few question this. But if \( \sigma^{2}_\mu \) is not independent of \( \sigma^{2}_e \), then \( \rho \) does not estimate the desired proportion, which could be larger or smaller, depending on the covariance and the causal forces driving it.

How might the independence assumption be violated? One way is if the social forces that make some places advantaged and others disadvantaged also produce corresponding variation in the person-level residuals. Although such data are not hard to simulate, I have not seen this question addressed empirically. Yet if we believe that neighbourhood context affects people and people affect neighbourhood context, then we ought to accept that the independence assumption is at least suspect, especially in observational designs. The upshot: ICC statistics may not tell us very much about neighbourhood effects.

Interclass correlation coefficient issues aside, the real challenge to estimating neighbourhood effects is (multilevel) confounding. Everyone seems to agree that persons in the 1st decile of deprivation are not exchangeable with those in the 10th decile; Table 2 would suffice if we believed they were. The essential questions, then, are: What should we condition on? And, how should we specify our model?

Given current practice, the Authors’ specification is defensible. Table 4 presents crude and adjusted effect estimates from mixed regression models. Unadjusted models show neighbourhood deprivation is related to outcomes. Adjusted models show the crude effects are confounded; no effects are discernable after adjusting for families. But built into the adjusted models is the assumption that families and schools are not affected by neighbourhoods. Like the independence assumption for the ICC, this assumption is key for the proper interpretation of effect estimates. If we view schools and families as intermediaries between neighbourhoods and health outcomes, which they are, then we must not condition on them. But if we do not condition on them we suffer confounded effects. This dilemma—to condition or not—is a symptom of the core identification problem for neighbourhood effects in observational designs.

It seems that one way through the identification thicket is to examine the outcomes of exchangeable persons who, for reasons beyond their control, experienced different neighbourhood environments. Much to their credit, the Authors aim to do just this when they examine the effects of changing neighbourhoods on siblings within the same family. The key assumptions are that (i) siblings are exchangeable and that (ii) the environments they are exposed to are sufficiently different to yield meaningful effects. The exchangeability assumption is not unreasonable, but issues such as birth order, child’s sex, innate personality and parental resources for a given child at a given time are worthy of further investigation. The second assumption is more challenging. For this to hold, a family/household would need to change (several) deciles of neighbourhood deprivation in a way that is unrelated to the care of their children. Without some exogenous shock (e.g. injury, death, lottery, etc.), it is hard to imagine how this could come about. The exogenous shock requirement is admittedly difficult to meet. This is why I think even within-family longitudinal neighbourhood effect studies are not much more valuable than cross-sectional ones.

In conclusion, Sariaslan and colleagues offer a well-crafted paper that exploits an exceptionally rich data set. Unfortunately, I do not think it is good enough to overcome the fundamental identification problems associated with the estimation of neighbourhood effects from observational data. It could be that neighbourhoods do not affect study outcomes. Or, it could be that neighbourhoods cause the observed 7-fold increase in risk. We cannot tell because we cannot identify the desired effects in observational designs, no matter how astonishing and plentiful the data. Accounting for families, as recommended by Sariaslan and colleagues, is almost sure to attenuate any estimated neighbourhood effects. But I think this is inappropriate, since neighbourhoods affect families.

As far as I can tell, the only way to identify a neighbourhood effect in an observational design is to mimic the desired experiment by sampling homogeneous persons residing in heterogeneous neighbourhood environments. I called this a matched-sampling design and offered an example of its implementation.\(^{9,13}\) The key is that all observed persons must literally be able to move to all observed neighbourhoods.\(^{2}\) Still, the best solutions I see are natural and researcher-designed experiments to answer our questions. Unfortunately, as noted by the Authors, results from the American Moving to Opportunity Study and several large community randomized trials are neither promising nor clear.\(^{9,13}\)

Thus, I differ from Sariaslan in the reasons why it is time for social epidemiologists to turn their attention away from neighbourhood effects. Whereas I remain convinced that neighbourhoods matter, I do not think we can identify the parameters we seek.
Authors appear to think neighbourhoods do not matter as much as families. So be it. In the end, I agree with them that research into the health impacts of families is a superb direction in which to move.

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References