Matheson et al (2006) reported that depression in women was predicted by residential instability and deprivation, after controlling for individual factors. Their measure of instability has parallels with the NeighFrag measure used here, using a mixture of household and building composition, residential mobility and youth (5-15 years); and so makes for a useful comparison to NeighFrag and the more common collective efficacy measures.

The authors argued that neighbourhood stressors such as deprivation and residential mobility may have a larger contribution to women's mental health than men's (Matheson et al., 2006). Because women may be more involved in neighbourhood life they may be more exposed to the stressors and therefore show more sensitivity in their mental health outcomes. If strong social bonds are considered protective for women's mental health (Propper et al., 2005) then living in a neighbourhood where it is more difficult to establish and maintain such bonds will act as a stressor for women, but not necessarily for men.

The association of NeighFrag and mental health was examined by strata of labour force status. The variable was used as a proxy for membership of alternative social groups (and therefore alternative sources of integration and regulation). There was general support for a difference in the association of NeighFrag and mental health by labour force status in the stratified and pooled analyses. There was little evidence of an association between NeighFrag and mental health for women who were not in the labour force. The estimates from the pooled analysis (but less so in the stratified analysis) suggested a small effect of NeighFrag for employed women. An examination of the crude data and the estimates from both types of analyses strongly suggested a large decrease in the reported mental health of unemployed women with increasing levels of NeighFrag.

The tests for effect modification by alternative social groups provided general support for the hypothesis that the neighbourhood setting could be a source of social support. In particular the labour force status stratification suggested that when women had little alternative social support from, in this example, a workplace, low levels of neighbourhood integration and regulation may be even more important in predicting mental health status.

For men, however, neighbourhood fragmentation remained unimportant in predicting their mental health status. While individual risk factors and neighbourhood deprivation was associated with mental health status, it would appear even for unemployed men, the risk of poorer mental health was not responsive to the type of social environment captured by the index.

Kavanagh et al (2006) also suggested that women may be more responsive to the neighbourhood environment because of the more intense way they participate in social networks. They also propose another factor; that women may spend more time locally because of their caring role in society (Kavanagh et al., 2006). The results from the effect modification by sex and labour force status obtained here suggest that it is the former mechanism that may explain the gendered NeighFrag/mental health association seen here. If the difference in association by gender had been a matter of women spending more time in the neighbourhood then an association between NeighFrag and mental health might have been expected for unemployed men. However the results obtained here indicate that the neighbourhood did not operate as a significant social group for men, or as a source of social integration and regulation.

The consistency of the findings here with the literature on women's mental health and the neighbourhood offer more support for a causal effect of neighbourhood fragmentation on mental health. The difference in the association by sub groups is plausible within the proposed theoretical mechanism. Indeed the fact that subgroups were selected for the effect modification analyses on theoretical grounds, rather than purely exploratory or statistical grounds, strengthens the case. However, further studies would be needed to better understand the mechanisms at play. For example, assessing whether NeighFrag modified the effect of unemployment on mental health (a "stress buffering" mechanism) would provide more information on the potential for neighbourhoods to act as a social resource.

There was no indication in the mental health analyses of a "u" shaped relationship between NeighFrag and mental health. In other words neighbourhoods with low levels of fragmentation did not appear to be a source of excessive integration or regulation, thereby posing an adverse risk for mental wellbeing. While this does not support my theoretical hypothesis it was perhaps not surprising. As discussed in the Chapter Three the skewed nature of the NeighFrag distribution indicated that the index appears to have captured a lack of something, rather than a full range of whatever was measured. That is to say, the index was better at measuring highly fragmented neighbourhoods than their converse, highly cohesive neighbourhoods. (In the same way, deprivation indices are less likely to capture affluence as well as deprivation). Therefore it was less likely that the index would be able to adequately capture excessive (in Durkheim's terms) levels of integration and regulation at the other end of the fragmentation spectrum that would result in poorer reported mental health.

It appeared that the association between NeighFrag and self-reported health was specific to the SF36Mental Health scale. There was some evidence of an association for the General Health and Vitality scales, but the statistical evidence was weaker and less consistent than for the MH scale. It would seem that deprivation made a more consistent, statistically significant and stronger contribution to all non-mental health scales.

The specificity of the association is supportive of a causal effect. The observed relationship between mental health and NeighFrag has a theoretically plausible pathway via the social integration and regulation mechanism. The weaker associations suggested for the General Health and Vitality scales could also offer tentative support rather than contradiction for a causal relationship because of their closer relationship with mental health construct in the SF36 measure (Ware et al., 2000). On the other hand, the scale which measures bodily pain, which showed no relationship with NeighFrag, has no psychological or biologically plausible pathway suggested by the theory.

In contrast to NeighFrag there was no evidence of an association between Congdon(NZ) and mental health for men or women, perhaps providing evidence against my hypotheses. One possible interpretation of this finding is that the results for NeighFrag were due to chance. The similarities in the distribution of the indices by geography and demographics suggest that this may be the case. However there were important differences on correlations between other contextual measures (NZDep and SoCInd) and the two indices, indicating that there the indices were not identical.

Therefore an alternative explanation for the differences in findings may lie in the differences between the indices. In chapter three it was argued that the reason for differences in the correlations between the indices may be that NeighFrag was a more comprehensive measure, designed and constructed in New Zealand, and therefore better captured the construct in this setting. If this were so, the lack of an association for Congdon(NZ) would be due to it not adequately measuring the construct here, whereas NeighFrag did.

While this may well be the case, another interpretation suggested by the data here is that the two indices are capturing related, but nevertheless different neighbourhood compositions. If this were the case the difference in the associations could be informative for understanding the construct. The most obvious cause of the difference in association was that the Congdon index used a subset of the NeighFrag variables. Figure 8:1 illustrates the hypothetical relationship that was empirically tested, this time showing the domains and the variables used to operationalize them.

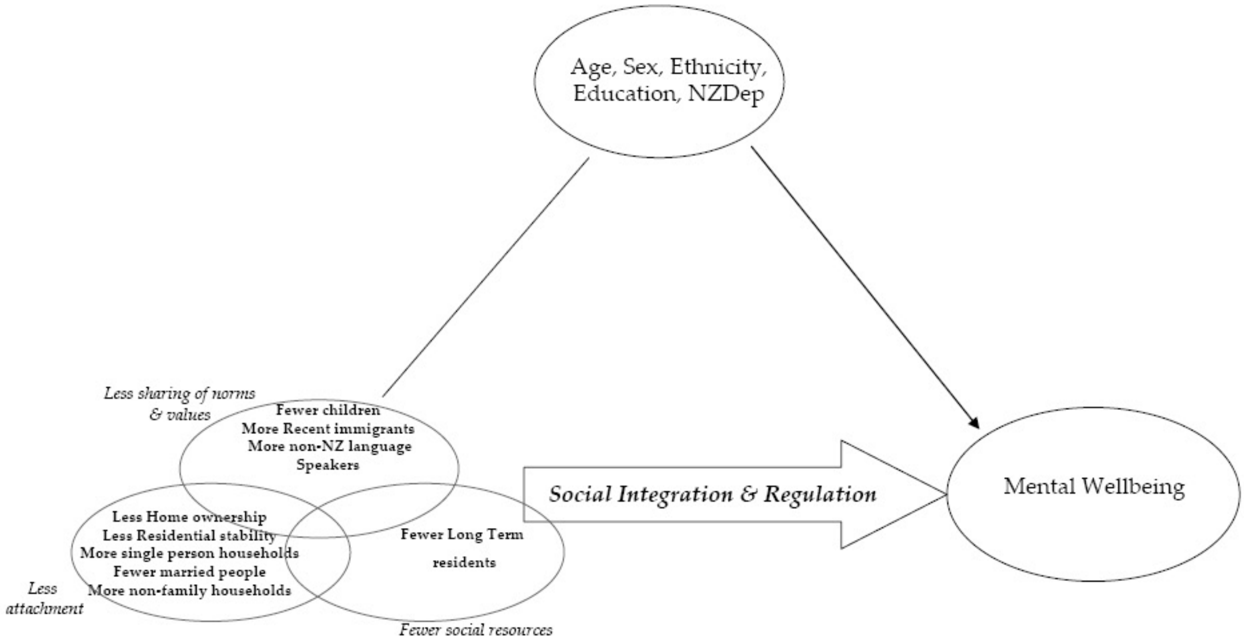

Figure 8:1 Proposed Relationship between NeighFrag and Mental Wellbeing

The next figure (Figure 8:2) uses font to emphasize how the two indices focus on different sets of variables. The variables used in the New Zealand version of the Congdon index (in italics) were clustered in a single domain, attachment. By comparison, the NeighFrag variables used the attachment variables, but also variables to do with children and immigration. While these variables were included for their theoretical contribution to neighbourhood life (which was supported by previous literature and factor analysis), it serves to remind us that on a simple level, the two indices draw on different weightings of neighbourhood compositions.

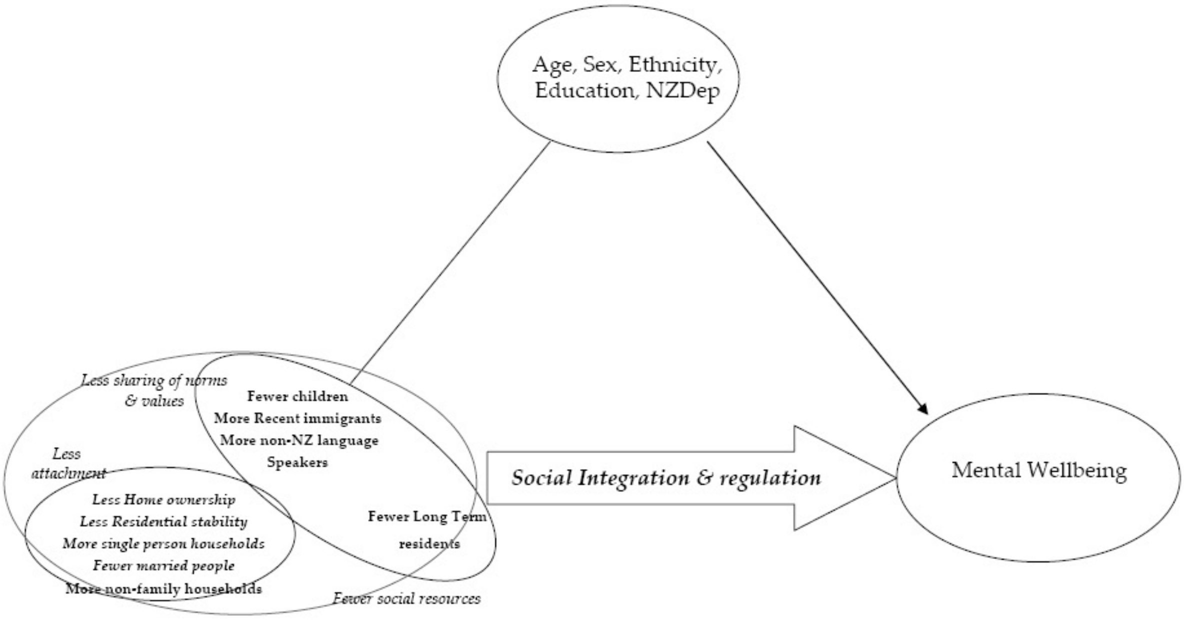

Figure 8:2 Comparing NeighFrag and Congdon(NZ)

The final illustration in Figure 8:3 goes on to suggest that the differences in the exposures may be part of the explanation for the differences in the findings. It could be argued that the empirical evidence has suggested the variables used to operationalize the sharing norms and values and social resource domains are primarily explaining the association between NeighFrag and mental health. In other words, the attachment type variables are not predictive of mental health in women whereas the family, immigration and long term residency factors are important.

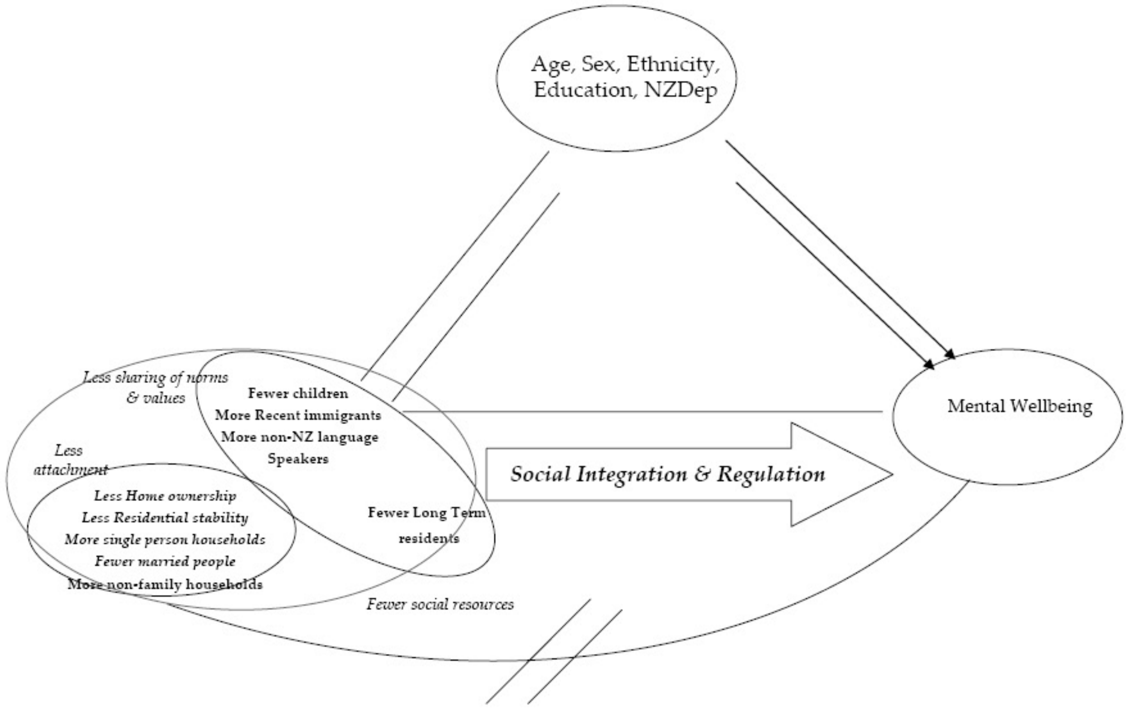

Figure 8:3 Different measures – Different Relationships?

Caution obviously needs to be applied when extrapolating out from the difference in associations. But the process of using the difference in findings to examine more closely what has been measured can also be informative for understanding the relationship between neighbourhood fragmentation and health. For example, it may be that what was being captured by the index was something about varying neighbourhood contexts for parenting and family life. If so, it would imply that parenting (or specifically mothering given the gendered findings) in NF10 was associated with poorer mental health than parenting in NF4&5.

This explanation does not necessarily contradict the integration and regulation hypothesis. Instead it suggests a more "specific" pathway (Diez Roux, 2002b, Riva et al., 2007), where a sector of society is more dependent on the neighbourhood context. Perhaps mothering in a neighbourhood with a lower proportion of other parents, higher transience, and less attachment to the local community provides mothers with less social support.

The alternative 'mothering' explanation raises further questions about what has been measured by the Index. Should a factor such as mothering be treated as a confounder between the index and mental health or should NeighFrag be regarded as a proxy for differences in mothering contexts? An alternative avenue is considering the extent to which NeighFrag has captured the underlying construct of neighbourhood fragmentation.

The limited ability of the index to capture all aspects of fragmentation was discussed in chapter three. An alternative explanation that needs to be considered is that the index is measuring something other than neighbourhood fragmentation. It is possible that NeighFrag has partially captured the compositional aspects of neighbourhood fragmentation, but also something else, such as mothering contexts. Such questions have important implications for what can be interpreted from the analyses. However, the process of working through the empirical results with an epidemiological understanding of confounding has proved a useful means of assessing the validity of the analyses and the hypotheses.

The relationship between NeighFrag and NZDep was further explored in the Health Survey analyses. There was certainly evidence of an independent association of both deprivation and fragmentation with mental health. As reported above the association of NeighFrag remained but was attenuated after the addition of NZDep into the regression model. The respective sizes of the estimates suggested that NeighFrag was the stronger contributor to mental health. Despite the correlation between the two exposures there was no suggestion that the model assumptions were violated.

There was little evidence from the intraclass correlations (ICC) of between-neighbourhood variation in the mental health outcomes. That is to say, it does not appear that residents in one neighbourhood are more alike in their mental health status than other study participants. Approximately 4% of the variation in the health outcome was explained by non-individual level factors. Other studies have also reported small amounts of variation of mental health at the area level. For example, Thomas et al (Thomas et al., 2007) found that there was a much larger variation observed at the household rather than post code level, even when individual factors were accounted for. But the small magnitude of the ICC's does not necessarily contradict support for importance of NeighFrag in predicting mental health outcomes across types of neighbourhoods (Merlo, 2003) (Blakely and Subramanian, 2006).

It should also be noted that there is little theoretical reason to suggest that mental health outcomes should be strongly clustered by neighbourhood, as opposed to by type of neighbourhood. As the outcome measure is a psychosocial response to the type of social environment, rather than evidence of a social practice or norm, the low ICC's do not necessarily contradict Durkheim's theory. ICC's measure the extent to which neighbourhood variation in the outcome can be explained by residents being more like each other than residents of other neighbourhoods. The argument that has been proposed here is that people residing in NF10 will have similar mental health outcomes, but not necessarily that within each neighbourhood residents will be more like their neighbours.

Physical activity (for example) may provide a better test of whether random effects measured by ICC's can capture Durkheim's concept that people's practices and norms will be affected by the practices and norms of their social group. For example, if we were interested purely in whether a characteristic such as the built environment affected walking behaviours, the main interest will be in the coefficients of the exposure on the outcome: it is the individual exposure to the type of neighbourhood that is affecting the outcome of individuals. Diez-Roux (Diez-Roux, 2000) referred to such a model as a contextual model that takes into account both group and individual level variables on the individual level outcome, arguing that multilevel analyses only need to be undertaken if there is evidence of within-neighbourhood residual correlation. If not, standard errors would not be biased and therefore there would be no statistical or theoretical reason for the addition of random effects into the model.

If, on the other hand, we thought that individuals' outcomes may be correlated within a neighbourhood, a mixed model that includes random effects would be appropriate (Diez-Roux, 2000). Following the above example, people in a neighbourhood might be influenced (or subject to "moral force") by other residents' practices because of how the built environment encouraged visible walking behaviours. If so, then we would expect to see a random effect, that is, people's walking practices would be more like their neighbours – partly in response to the built environment and because of the development of social norms around walking within that neighbourhood.

Returning to the case here, mental health status of residents could theoretically be correlated within neighbourhoods if local factors meant that the neighbourhood or residents influenced each other. For example, if 'misery begets misery' were true in a social setting then we would expect that a random effect would be present. A future test of this mechanism (which is more akin to the 'transmission' mechanism tested with smoking) could be to examine mental health by strata of NeighFrag: was there more evidence of misery begetting misery in the less fragmented neighbourhoods? On of the difficulties in testing such a mechanism is that data would be needed on the level of influence of non-neighbourhood factors, in order to understand how the various levels interact, and thereby interpret heterogeneity within the neighbourhood level. For the purposes of this thesis, however, the ICC's suggest that while there was a small within neighbourhood variation in outcomes, the primary neighbourhood 'effect' came from the type of neighbourhood.

8.3.1.3 Smoking Analyses: the 'Integration and Regulation' Mechanism -is NeighFrag a 'Social Deficit'?

The results from the smoking analyses proved to be unexpectedly interesting and informative, perhaps more from a theoretical than a purely epidemiological perspective. Smoking was used to investigate a variation of the 'integration and regulation' mechanism. Was NeighFrag acting as a general 'social deficit' and therefore neighbourhood stressor, in the same way that NZDep appears to act? If so, it was expected that NeighFrag and NZDep would demonstrate similar relationships with smoking.

The main effect smoking analyses offer varying levels of support for the hypothesis. An adverse association between NeighFrag and the risk of smoking was observed for 15-19 years olds, in support of the 'social deficit' hypothesis. However, a protective effect was observed for the older age group, where increased fragmentation was associated with a decreased risk of smoking for 20-24 year olds, in contradiction to the hypothesis.

It should be noted that the magnitude of the associations were small and the precision limited the reliability of the interpretations. When the estimates were assessed individually, the statistical evidence was weak. The interpretation of the NF1 estimates was particularly problematic given the co linear relationship between NeighFrag and NZDep. The strength of the association varied by sex across the two age groups with a weaker association observed for the 15-19 year old women and the 20-24 year old men. It is unlikely that neighbourhood fragmentation is an important risk factor in predicting youth smoking. Nevertheless the consistency of the opposing pattern was surprisingly clear across the sub groups, suggesting that the associations were not simply due to chance or bias.

The primary utility of the analyses for the purposes of my thesis lies in examining why the associations might have occurred, and what the difference in the associations in the age groups might tell us about neighbourhood fragmentation. Smoking can be regarded partly as a response to psychosocial stress (Stead et al., 2001) which could plausibly act as a response to a poorly integrated and regulated social environment. If smoking were taken as a marker for a stress response to an unbalanced neighbourhood environment the evidence presented here would suggest that highly fragmented neighbourhoods were stressful for 15-19 year olds, but that least fragmented neighbourhoods were stressful for 20-24 year olds.

One interpretation of the difference in the associations is that life course stage is a factor in determining what level of social integration and regulation is a 'healthy' balance for each age group. If so, it would appear that the less fragmented neighbourhood offered an excess of integration and regulation for 20-24 year olds, hence their increased smoking risk in these types of neighbourhoods. The less highly regulated neighbourhoods on the other hand offered a more balanced social environment for this age group. If this mechanism were plausible then the associations would offer support for the theory of a balanced level of social integration and regulation, rather than the alternative 'social deficit' theory.

The pattern observed in the 20-24 year olds contradicts the social control mechanism discussed earlier. Under this mechanism it would be expected that the more fragmented neighbourhoods would have less means of social control over resident's risky health practices, and therefore an increase in smoking might be expected (Rasmussen et al., 2005, Lundborg, 2005, Patterson et al., 2004). Yet a decrease was observed for the 20-24 year olds. So while social control might be a factor for the younger ages it doesn't explain the protective pattern.

It is more likely that the associations observed are because other confounding or casual factors have not been adequately accounted for. It is interesting to note that the strongest associations were observed for 20-24 year old women, with a 20% increase in the risk of smoking in NF1 compared to NF4,5, and an opposing 20% decrease if they live in NF10 compared to NF4,5. Graham et al (2006) have observed that the life course trajectories of women are established through early motherhood and socioeconomic hardship and that the disadvantaged trajectory is associated with smoking. The analyses controlled for labour force status but were not able to adequately distinguish between, and therefore control for, factors such as early parenting.

Such patterns suggest an alternative explanation based on migration processes into and out of neighbourhoods. The contrasting pattern in the association of NeighFrag and smoking may be explained by reasons for living in a neighbourhood at each age stage. There area likely to be a number of processes at play here. If, for example, young adults were more likely to move to highly fragmented neighbourhoods to continue their education then they are less likely to be smokers (as higher education is associated with lower risk of smoking (Borman et al., 1999)). Conversely, those who stay in the least fragmented areas may do so for reasons that are also known adverse risk factors, such as early parenthood (Graham and Der, 1999). In other words, the protective association seen in the 20-24 year olds may be largely due to non-smokers moving into highly fragmented neighbourhoods. Both underlying factors could well be explaining the different patterns and highlight the non-exchangeability of youth across the levels of NeighFrag.

8.3.2 Smoking Analysis: 'Transmission' Mechanism

It is well recognized that smoking is a social activity that can be strongly influenced by the social environment, particularly the practices of other smokers (Turner et al., 2006). The mechanism tested in the second set of smoking analyses was that the transmission of smoking practices within a neighbourhood would be modified by the level of neighbourhood fragmentation. It was hypothesized that individuals in a more fragmented neighbourhood would be less likely to be exposed to their neighbours. Therefore the effect of the smoking rates of their local neighbourhood on individual risk of smoking would be lower. Conversely, it was expected that in a less fragmented neighbourhood, the effect of local smoking rates on the individual risk of smoking was expected to be stronger, because the nature of neighbourhood life meant that individuals would be more exposed to local practices and norms.

A two step process was required to test the 'transmission hypothesis. Firstly, a main effect analysis was completed to examine the association of smoking practices of the adults living in a neighbourhood with the individual risk of smoking for youths living in that neighbourhood. As would be expected a strong association was observed between NAS (Neighbourhood Adult Smoking) and the individual risk of smoking in youths, even after controlling for known individual factors and NZDep.

While this association was not measured to test evidence of neighbourhoods acting as a social group, it does appear to support the case that individual behaviours are highly responsive to contextual factors (Wilcox, 2003).

The second step in the test of the 'transmission' hypothesis was to examine the effect of NAS on youth smoking by levels of NeighFrag. That is, did the effect of neighbourhood smoking vary by levels of fragmentation? In these analyses, NeighFrag was included in the model as an effect measure modifier of NAS. The answer was that yes, NeighFrag strongly modified the effect of NAS, but in contradiction to the hypothesis. The results indicated that smoking practices are more easily transmitted in the most highly fragmented neighbourhoods rather than less. The effect of NAS on the risk of individual youth smoking was significantly larger in NF10 than at lower levels of fragmentation, rather than smaller as predicted.

Why would the magnitude of the association between NAS and youth smoking be substantially larger in NF10 than in lower levels of NeighFrag? As discussed in chapter seven, it is unlikely that the difference in effect between NF10 and other levels of fragmentation would have been due to chance. The pattern of the effect stayed the same across the four subgroups. The direction and magnitude of the modification was supported by the statistical interaction results. It is therefore highly unlikely that the 'true' effect was as hypothesized (that is, opposite to that observed) or even just that there was no effect modification.

Given the strength of the finding, it is worth exploring alternative explanations for the results. If the hypothesized mechanism were true, one interpretation of the results suggest that there would need to be an alternative neighbourhood means of transmitting smoking risk in the youth population that was inversely related to NeighFrag. Youth would somehow need to be more exposed to, and influenced by the smoking behaviours of adult residents in highly fragmented neighbourhoods compared with less fragmented neighbourhoods.

One explanation is that youth may vary in their exposure to the adults smoking practices in their neighbourhood. It could be that youth in less fragmented neighbourhoods are less exposed to the neighbourhood adult smoking rates, and might be less influenced by them. Family and peer groups may be more important contexts than the neighbourhood.

Conversely, youth in NF10 were less likely to be living in family households, and would therefore be less influenced by family levels of smoking. However, for the hypothesis to hold true, they would also need to be more exposed and more influenced by the smoking practices of adults around them. Is it possible that youth living in NF10 were more exposed to the smoking practices of 25+ year olds than those living in other levels of NF?

An alternative explanation is that non-neighbourhood factors may be confounding the association. The social network analyses by Christakis and Fowler (Christakis and Fowler, 2008) suggest that peer groups may be more important than geography when looking at influence on smoking practices. However, network data was not available in the census dataset. On the other hand, household level factors could better explain the observed associations. In order to better assess the contribution of neighbourhood levels of smoking to individual smoking risk it would have been necessary to control for youth exposure to household levels of smoking. This was not done in this analysis as it would have further restricted the dataset, and very probably introduced selection bias. As importantly, the NAS/youth smoking risk was not the primary focus of the analysis but rather a means to an end. Therefore it was important that the NAS/youth smoking relationship be observed across all levels of NeighFrag with as least restricted dataset as possible. Nevertheless, future analyses that assessed the contribution of household smoking to the NAS/ smoking association would be helpful in more clearly understanding the relationship.

It may be that the relevant contextual exposure for youth smoking is not at the neighbourhood level. What might be important for youth is their exposure to household smoking rates or peer group practices rather than neighbourhood prevalence. However, it was not possible to differentiate between neighbourhood level exposure and other intermediate levels such as peers or households. But NAS could be seen as capturing non-individual level risk factors that happen to have been aggregated up to the neighbourhood level because that was how the data was available and therefore structured. It may be more useful (and accurate) to consider NAS as a proxy measure of household exposure. If this were the case then the next question is how might NeighFrag modify the household exposure?

Non-family households, such as flats, were more common in NF10 than in other levels of NeighFrag. Therefore NeighFrag could well have been acting as a proxy for the type of households youths live in. It is very probable that youth are more likely to end up living in households with smoking practices similar to their own. That is, endogenous processes could explain any association between household level smoking and youth risk of smoking. Therefore, because more youth in NF10 are more likely to have self-selected into a given household, their smoking risk will be more closely influenced by, or in fact aligned, with that of their current household. This may then become a recursive process as the smoking practices of housemates then reinforces the smoking practices of individuals.

A problem with the above scenario is that it appears to be reliant on youth living with 25+ year olds in flatting type households in order for their risk of smoking to be associated with NAS. Unless this was predominantly the case, it could be argued that they wouldn’t be exposed at all to the older populations smoking practices. In response, this would only be a challenge if the underlying drivers for smoking were substantially different between the two populations. As discussed above there may be little reason to suggest that smoking risk factors would be dissimilar between the youth and general population. Thus, because the smoking practices of 25+ year olds would be reflected in the youth population, the NAS of a neighbourhood would also act as a proxy for the smoking practices within households of that neighbourhood, no matter what the ages of the residents.

The effect of explaining the association at the household rather than neighbourhood level has been illustrated in Figure 8:7. Household smoking explains or contributes to NAS, the observed exposure. Household composition or type is a contributing factor in determining how youths may select into smoking or non-smoking households. In other words, the type of household affects the possibility of endogeneity between household smoking levels and individual smoking risk. Finally, because household type is related to NeighFrag, it raises the possibility that it is the degree of endogeneity that is modifying the NAS/smoking association, rather than neighbourhood fragmentation per se.

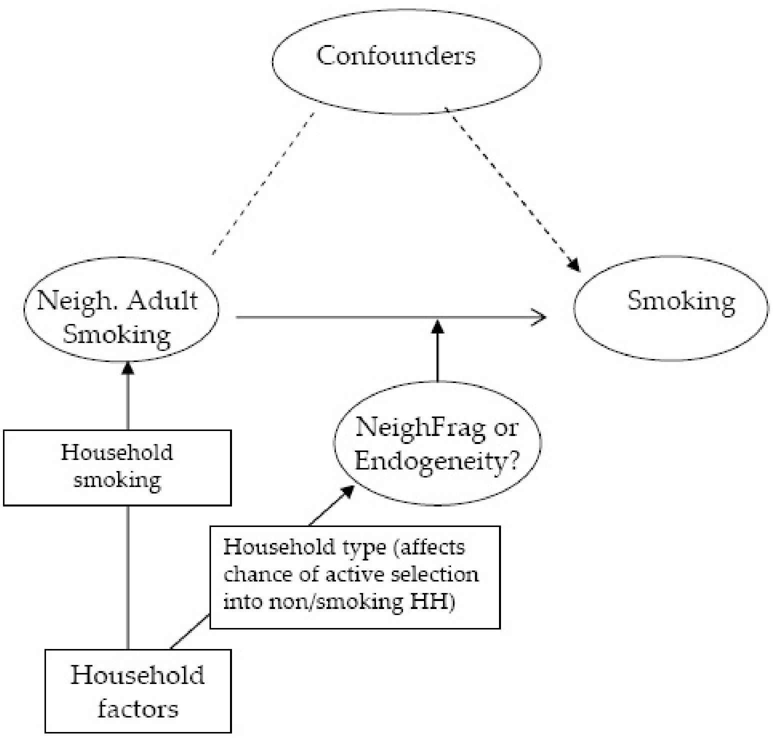

Figure 8:4 Endogenous Processes Between NeighFrag, Smoking and Youth

8.4 Is there Evidence of a Causal Neighbourhood Effect of NeighFrag on Health?

Once alternative explanations were considered there was less evidence for a causal effect of NeighFrag on smoking in New Zealand youth for either mechanism. The contrasting pattern of association between NeighFrag and smoking across the two age groups suggested the association may be due to other factors, such as reasons for living in a neighbourhood. In my view, the effect modification by NeighFrag was the result of causal factors at the household level that were being captured in the NeighFrag measure. Therefore it is unlikely that the observed within-level effect modification by NeighFrag was a 'true' causal effect.

On the other hand, there is some suggestion from the accumulated empirical evidence that NeighFrag could be an important contributor to mental health, in support of the hypothesised "social integration and regulation" mechanism. I have examined the evidence for and against the association providing evidence of a causal mechanism. The strength and dose response nature of the association, the specificity of the association, and the psychological plausibility of the association are all suggestive of a causal effect.

The final important factor when considering evidence for a causal effect is temporality (Bhopal, 2002). In other words, would exposure to NeighFrag precede a change in mental health status? Cross-sectional analyses are not able to reliably establish whether this may be the case and it remains one of the bigger challenges faced in the neighbourhoods and health field (Diez Roux, 2007, Kawachi and Subramanian, 2007). If mental health were a factor in determining where individuals resided then the NeighFrag/mental health associations would be a reflection of endogenous processes rather than a causal effect of NeighFrag. While there has been discussion in the literature of endogenous processes around mental illness and neighbourhood processes (Gleeson et al., 1998), and health and deprivation (Smith and Easterlow, 2005), there is little in the literature or in the proposed theory to suggest that mental *wellbeing* should be a factor in determining how people are 'sorted' into varying levels of fragmentation.

The associations do suggest that women living in more fragmented neighbourhoods report poorer mental health than those in less fragmented neighbourhoods, even after known confounders have been taken into account. The final question to be asked is; are there unmeasured factors that could more fully explain the association?

The selection of confounders was limited to those available in the data sets and the emphasis was on choosing recognised strongest factors, rather than seeking to perfectly control for every potential confounder. In reality there will be a number of unobserved confounding processes that may be contributing to the observed associations. Figure 8:5 illustrates the role played by some of these factors. It has been suggested in the discussion sections of the mental health and main effect smoking analyses that here that the associations could be explained by selection processes. That is, the association between NeighFrag and mental health may be primarily due to the endogenous processes such as reasons why individuals reside in different types of neighbourhoods.

Frank et al (2007) were able to demonstrate that people's preference for a type of neighbourhood was an important factor in understanding neighbourhood effects. There are likely to be many reasons why individuals reside in a given type of area that may need to be taken into account when understanding their mental health status. As one example, people may be required to be relatively mobile as they establish their careers. Therefore they might be more likely to live in more fragmented neighbourhoods where there are high rates of tenancy. If the early career stage were also associated with high levels of work stress as individuals are expected to prove themselves, work long hours and so on, then those living in more highly fragmented neighbourhoods will report poorer mental health.

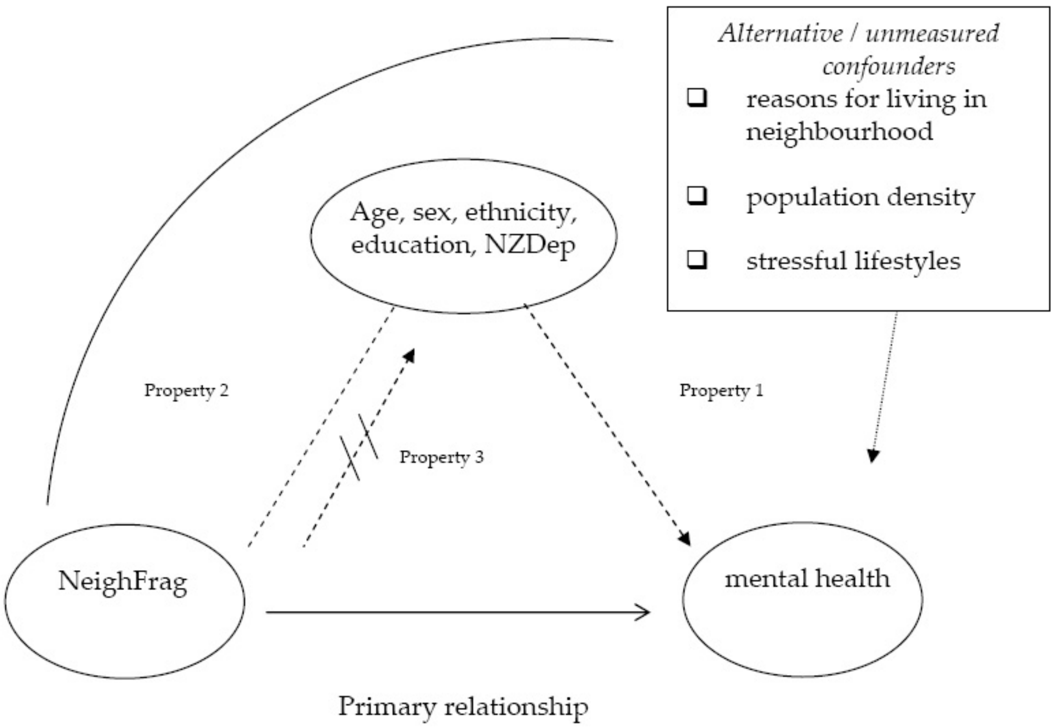

Figure 8.5 Alternative/unmeasured confounders to the NeighFrag/Mental Health Association

In either the smoking or mental health analyses it was not possible to look for better proxies for reasons for living in the type of neighbourhood, such as career stage. Accounting for unobservable factors such as these as confounders would require that some one living in NF1 would be essentially comparable or exchangeable with someone living in NF10. It is also possible that NeighFrag may be a proxy for reasons to live in the type of neighbourhood. If NeighFrag captured a particular 'lifecourse' or 'lifestyle' composition then the associations observed may be explained by the way these societal factors have clustered together by neighbourhood. Living in a highly fragmented neighbourhood may well be associated with poorer mental health either because of the nature of the social group, or because of the reasons for living there, or, most likely in my mind, a combination of the two.

Again, careful examination of the theory and data from the mental health analyses has turned the attention back to what is being measured by NeighFrag (and Congdon) and what is meant by neighbourhood level social fragmentation. The discussion above has highlighted the limits and possibilities posed when bringing theory and data together. The final chapter draws together the aims of the thesis with what has been able to be achieved.

Conclusion

Achieving a better understanding of the relationship between where people live and their health requires increasingly sophisticated research. A primary goal of this thesis has been to 'do' research in a way that not only recognizes the complicated nature of the relationship, but also allows some of that complexity to be seen empirically, and therefore better understood. In keeping with this agenda a body of evidence has been developed which explores a theoretical relationship between neighbourhood and health in New Zealand.

Following a series of analyses, the Index of Neighbourhood Social Fragmentation was found to be associated with variations in individual mental health and smoking in New Zealand, even after controlling for individual and neighbourhood confounders. It is likely that other factors, particularly at the individual level, better explain the associations, particularly for smoking. But this is not to say that the results were in some way 'spurious' or disappointing. Instead, I have argued that they have been most illuminating for the neighbourhood/health relationship. This is because the analyses presented in the thesis were designed to explore not just if, but how and why fragmentation might be related to health.

The relationship between neighbourhood-level social fragmentation and health that has been explored in the thesis has been used as a case study of the neighbourhoods and health field and social epidemiology. A means of operationalizing theory into data has been developed for the index creation and the subsequent analyses, which has furthered my understanding of the 'neighbourhood' and its potential relationship with health. The integration of theory and data throughout the thesis has allowed the empirical evidence to be illuminating, as seen in the main discussion in chapter eight.

The 'Neighbourhood':

Just what is meant by 'the neighbourhood' in contemporary New Zealand society has been an undercurrent to the thesis. The measurement of the neighbourhood-level social fragmentation construct was grounded in a theoretical model of neighbourhood collective social functioning. Two indices were created from administrative census data. One sought to operationalize the conceptual model as fully as possible (NeighFrag), the other developed for comparative purposes (Congdon(NZ)). Developing the indices highlighted the importance of understanding what is meant by the geographically bound neighbourhood. A conceptualisation of the geographical neighbourhood as a social group was established based on a reading of Durkheim's theories. The analytical process has provided some statistical support for this being the case, but it has also encouraged thinking about alternative explanations.

The social nature of the neighbourhood may not be an important contextual factor in the lives of most individuals. The thesis has demonstrated that framing the neighbourhood as a social group could be an important way of conceptualising how the local environment can play a role in people's lives. The three-domain concept of neighbourhood fragmentation has highlighted that social settings require certain factors to be present if they are to be effective in functioning as a social resource. For example, to return to the 'mothering' example discussed in Chapter Eight, it was proposed that mothers in highly fragmented neighbourhoods may have more difficulty in establishing supportive local peer groups (which may be a factor in their poorer mental health). If this were the case, the neighbourhood may not be the peer group per se, but could provide the setting for peer groups to develop. Having visible, easily accessible places to meet - "opportunity structures" (Baum and Palmer, 2002), for example, - could be one means of providing the neighbourhood structures that foster social groups for potentially vulnerable residents.

Rather than regarding neighbourhoods themselves as types of social groups, perhaps NeighFrag could be better understood as measuring the antecedents of interlinked social groups within the neighbourhood. This view would envisage the neighbourhood as a collection of social groups, rather like Balkundi's (2007) work-place social groups. The emphasis would then shift from the neighbourhood as a source of social support and networks to the neighbourhood as providing the 'substratum' (in Durkheim's terms) for important social networks. The characteristics of the neighbourhood become important not only for fostering the groups individually, but also for fostering the multiple ties between them. A highly fragmented neighbourhood would be one with fewer, and less interlinked ties between individuals and groups. This reading of neighbourhood fragmentation still utilises the 'social group' type mechanism from Durkheim, but at a different level. It suggests that neighbourhoods can provide the means for local social groups, but with the attention focused on local factors which support or hinder those component groups. The focus has therefore shifted from Durkheimian social groups to the antecedent conditions for them.

Theory and Data

The conceptual models provided an effective means of utilising valuable secondary data sources. The dangers of so called 'data dredging' were reduced by carefully designed analyses, but the theory also usefully promoted opportunistic analyses by grounding them in mechanisms. The carefully designed analyses have allowed some light to be shed on two specific mechanisms developed from Durkheim's theory of social groups: the effect of 'integration and regulation' on individual wellbeing, and 'transmission of social practices' within neighbourhoods. The theory has then also been vital in the interpretation of the analyses, not just for confirmation, but also when exploring alternative explanations.

The research undertaken here offers strong support for a 'specific' research agenda (Diez Roux, 2002b, Riva et al., 2007). 'Specific' associations were found, for example, NeighFrag was related to mental health in women, not men; the smoking/NeighFrag association was opposite across two age groups. Specific outcomes were tested, with sensitivity analyses finding that there was less evidence supporting a relationship between NeighFrag and self-reported physical health measures as hypothesised. Specific pathways (main effects and effect modification) were able to be successfully tested with different outcomes. Even within the limits of cross-sectional secondary datasets, an interesting and comprehensive array of pathways was able to be examined.

Analyses have helped to shed light on the multilevel relationship between individuals and their neighbourhood. Cross-level main effects and effect modification were investigated enabling inferences to be made about the relationship between a neighbourhood exposure and individual level outcomes and confounders, as well as modifiers at both levels. In other words, the methods were a response to the conceptual pathway, not simply a function of the available data (Diez-Roux, 1998, Frohlich et al., 2002).

The interpretation of the associations has demonstrated the value of working from a conceptual model. The model has provided a means of making sense of the data, the empirical findings, and also for considering the contradictions. It has fostered the development of innovative approaches, such as the effect modification of NAS by NeighFrag that would not have been considered unless one wanted to consider how social practices such as smoking might be transmitted. Similarly, the use of the labour force status variable as an effect modifier arose directly from the need to find a proxy for an alternative social group. The 'specificity' of the findings in particular demonstrated the value of the theory driven analyses compared with a more exploratory 'risk factor' approach in the interpretation of the results.

In this thesis, the purpose of analysing the direct relationship between NeighFrag and smoking was primarily exploratory. While a risk of such analyses is that any interpretation will necessarily be ad hoc, the results were useful because they could be interpreted in light of the theory proposed. Possibly the more important contribution came from the process of seeking alternative explanations. The process of interpreting the associations in chapter eight has highlighted other potential neighbourhood processes, such as migration.

Given the importance of smoking in driving health inequalities, the analyses have been of value for encouraging us to think carefully about increasing our understanding of lifecourse trajectories for smoking in young people. The migration into different types of neighbourhoods is unlikely to be caused by neighbourhood fragmentation, but examining how trajectories may be maintained by, for example, differential access to higher education (which is related to NeighFrag) has offered an important avenue of thought. It would seem that once adequate theoretical tools were developed the social theorist had a fine time mining away.

While the theory developed here has its source in the original works of Durkheim, the theories of Bourdieu have also been present in the assessment of the mechanisms. Bourdieu's works on social capital, which are increasingly being utilised in neighbourhoods research, provide a useful way of examining the ways in which local environment and residents jointly construct and consume social capital (Carpiano, 2008). But rather than just focusing on the production, quality, and consequences of the resource, his theoretical framework encourages us to ask about unequal access and utilisation of those social resources.

Such an approach could be a useful means of examining health inequalities by asking who does not benefit from neighbourhood social groups. It would have been interesting to have examined the degree of 'match' between a person and their neighbourhood to see if access to social support was dependent on individual characteristics. For example, in a less fragmented, predominantly family household type of neighbourhood, would a person living alone benefit from the higher levels of integration and regulation as much as someone living in a more typical household? Unfortunately the data was unavailable here, but it would certainly be an interesting avenue to pursue. Cross-level interactions such as these could be a fruitful means of exploring what neighbourhood means to people by observing the "non-typical residents"; in other words examining the counterfactual.

Taking a more Foucaldian approach Castel (1991) it could be argued that the creation of neighbourhood indices construct neighbourhoods as 'risky' places (Smith and Easterlow, 2005). Measures such as NZDep, NeighFrag and smoking prevalence (Barnett, 2000) could encourage researchers (and others such as policy makers and real estate agents) to use them to determine risk indices associated with residence in types of neighbourhoods, thereby (often unintentionally) stigmatising the people who live in them. While measures used this way can be useful for determining service funding levels, better service provision could also be made if there was a deeper understanding of what such measures can tell us about who lives where. It is known that many deprived people do not live in the most deprived places, and also that the people living in deprived places are not necessarily deprived (although to a lesser degree) (Salmond and Crampton, 2002). A danger of creating an index of social fragmentation is that the equivalent sociologistic fallacy will be committed. Similarly, the NeighFrag index should not be used to tell us about who is more or less integrated, either into the neighbourhood or other social settings (unless each component variable is seen as a marker of integration into societal institutions, which I would argue is problematic). NeighFrag can only be used to make inferences about the fragmentation of places.

The interaction between data and theory has allowed the empirical analyses to highlight important wider, societal-level processes (Moon and Barnett, 2003). While not explicitly examining them, the thesis has highlighted the need to better understand how neighbourhoods come to their current status: that is the "broader socio-historical context" (Moon and Barnett, 2003, p. 7) of the neighbourhood/health relationship. Part of understanding both the compositional characteristics of neighbourhoods, and of processes such as endogeneity, is the need to consider how people in New Zealand society are "sorted and sifted" into different types of neighbourhoods, including historically.

Future Research

The two mechanisms that have been tested here offer tantalising possibilities for future research. As has been suggested in chapter eight, neighbourhoods may well be places where health-related social practices are constructed and enacted, and therefore potentially transmitted. Examining how neighbourhood spaces can be social places for social interaction and practices would provide valuable insight into the potential of neighbourhoods to be sites of effective interventions of physical activity, for example.

The probable vulnerability of unemployed women in highly fragmented neighbourhoods has been highlighted in the analyses of mental health and NeighFrag. The mechanism proposed here has suggested that the mental health status of unemployed women was related to a combination of low levels of social support from either workplaces or neighbourhoods (Andren and Rosenqvist, 1987). A means of investigating how neighbourhoods may offer more or less opportunity for social support could be to compare cases of individuals who are vulnerable with those who are more resilient to their neighbourhood social environment. Who is most dependent on the neighbourhood to be a source of social integration and regulation? The mental health results and possibly the pattern of the NeighFrag/smoking association for 15-19 year olds suggest that New Zealand society is structured in such as way that those who are most dependent on the neighbourhood social group are also the most vulnerable in a changing, fluid world.

Alternatively, attention could be turned back to the neighbourhood by comparing vulnerable and resilient neighbourhood populations. For example, communities with similarly high NeighFrag levels may have populations with unexpectedly poorer or better mental health. Investigating, for example, other fragmenting characteristics, such as well supported public places ("opportunity structures" (Baum and Palmer, 2002)) could be a way of understanding how neighbourhoods can support their residents, as a group, and the more vulnerable among them.

Taking such an approach would also encourage examination of supra-neighbourhood processes (Cummins and Macintyre, 2006). The provision and quality of facilities within each neighbourhood is dependent on a wider context, and the relationships between neighbourhoods (Riva et al., 2007). For example, local government and health authorities provide playgrounds, libraries, and health services within neighbourhoods. The quantity, quality and utilisation of resources, on the other hand, may be dependent on complex interactions between the individual, household, neighbourhood, and wider contextual factors. An interesting approach would be to investigate how the ability of a neighbourhood to act as a collective was related to the provision of health-related resources in a neighbourhood. For example, are some communities less able to work together to gain what they consider positive resources, such as adequate pathways, or conversely to resist negative resources, such as liquor or gambling outlets?

As well as examining how places within neighbourhoods can support better mental health, the relationship between neighbourhood fragmentation and mental illness warrants further investigation. As discussed earlier, it may be that some neighbourhood environments make it easier for individuals to fall between the cracks of health services and local social networks (Drukker et al., 2004). Gleeson and colleagues (Gleeson et al., 1998) have also demonstrated how services are not evenly spread across areas, suggesting that access to care may be more problematic for individuals living away from service clusters. And, of course, going back to the original investigations into social fragmentation and health: are some areas better able to regulate mental illness and therefore offer support against 'suicidogentic' currents for those who are less able to resist their pathological "passions"?

In my mind, a very interesting means of investigating how and why neighbourhood fragmentation would be related to health would be to take Balkundi's (Balkundi et al., 2007) approach. Observing the web of ties that connect individuals and groups across the neighbourhood (rather than workplace) would demonstrate the construction of, and access to social resources, as well as the "moral forces" that are constructed and experienced by people. But as important as observing the networks is the investigation of their antecedents: who lives in the neighbourhood and why; how it came to be; what factors in the neighbourhood and its wider context support or hinder individuals' social ties, and so on. Observing health practices within such networks, for example, would create a most illuminating picture.

Tools such as NeighFrag will never be able to achieve the richness of purpose designed surveys, direct observation and qualitative methods. While it would be wonderful to actually measure (for example) the social networks of each neighbourhood it is understandably not considered practical. What NeighFrag can provide is a useful means of capturing a snapshot of the compositional social characteristics of a neighbourhood. The constraints of measures such as NeighFrag are numerous, but 'thin but broad' measures such as these do provide tools for investigating the dynamic relationship between where we live and our health.

Concluding Remarks

Is there evidence that the neighbourhood acts as a social group in Durkheimian terms? The mental health findings have provided empirical support for the theory. Is there a 'causal effect' in epidemiological terms of the Index of Neighbourhood Social Fragmentation on mental health? It has been recognised that the analyses presented here cannot completely answer that question from an epidemiological understanding of 'effect'. The analyses were cross-sectional and were not able to capture other important factors such as reasons for living in a neighbourhood. I have also been less able to explore the interaction of neighbourhood fragmentation with other contextual factors in this thesis. It is therefore highly likely that examination of other neighbourhood factors and those beyond the neighbourhood will form an important part of any explanation for the empirical results seen here. But the theoretical and empirical results presented in this thesis give us more confidence that that types of neighbourhood settings may well be important contextual predictors of mental health for women because of the social resources provided in neighbourhoods.

This thesis has developed a more subtle understanding of the complex relationship between neighbourhoods and health and a more sophisticated means of investigating it. The epidemiological question of causal effect has provided a useful and robust lens through which to view both the theory and data. Knowing what needs to be seen, however, has come from the theory. Together, social epidemiology and theory have provided a means of shedding a bit more light into the black box.

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Appendix 1

Fitting complex models using Health Survey data

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Contents

Recommendations

There is good reason not to use the final weight supplied by the Ministry of Health for the purposes of model estimation. Instead, the proposed solution uses 20 dummy variables to approximate the survey selection process and thus make a suitable adjustment to the standard errors of the estimates – under the usual implicit survey assumptions that non-responders are like responders, and that there is no differential non-response. Inference is then from the sample to the target population, as is common. As these implicit assumptions are unlikely to be true, some caution needs to be exercised in interpreting the statistics (as always).

The proposed approach is to include key likely-confounders / exposures-of-interest, plus three more variables indicating survey design, all as dummy [class/nominal] variables in unweighted analyses. These variables (all described in more detail on page 3) are:

confounders/exposures of interest		
Age	use the 4 life-style age groups	3 dummy variables
Sex		1 dummy variable
Ethnicity	this is also a *design variable*	3 dummy variables
design variables		
Stratum		6 dummy variables
PSU8	this is a grouped PSU-size variable	7 dummy variables
Adults in household	Q277, missing categories removed	4 dummy variables

The proposed approach can be used for **linear logistic models**, in single or two-level frameworks, with or without random intercepts, using SAS (proc GLIMMIX), and also for **general linear models** (continuous outcome variable), using, for example, proc GLM. These can also be run in MLwiN but with reservations about (a) the adequacy of the convergence possible, and (b) some small, but non-negligible, differences in the estimated parameter values which may indicate inadequate algorithms. MLwiN users should probably check their proposed final models in a SAS run. In *all* cases the estimated standard errors are likely to be a little smaller than they should be, but this may be of little practical importance. **Single-level linear logistic models** can also be run using SAS proc SURVEYLOGISTIC, and **single-level general linear models** using SAS proc SURVEYREG. In these SAS cases, if the design-variables above are used as well as cluster and strata statements, the standard errors should be close to accurate.

If any other *canonical*[1] **generalised linear models with random intercepts** are desired – such as models with Poisson errors and a log link – they can be done using GLIMMIX or MLwiN. If random slopes as well as random intercepts are desired, only MLwiN can cope (but with reservations as above). However, if *hybrid* **models** are desired – such as using a log link but Binominal errors, to estimate relative risks rather than odds ratios – only GLIMMIX can cope.

For **general linear models**, using either MLwiN or SAS proc SURVEYREG – and, if desired, for **single-level linear logistic models** using SAS proc SURVEYLOGISTIC – a model using the MoH-supplied final weights, which incorporate post-survey adjustments, can be run *without* using the three *design-only* variables, thus saving 17 parameters (but watch out for multi-collinearity, especially if ethnicity is included). Inference then goes from the sample-adjusted-for-non-response to the target-population (with the ususal implicit assumptions). In these cases the standard errors will be too small and a jackknife process (see *Appendix 1*) should be used to empirically estimate appropriate standard errors for the otherwise-'correct' parameter estimates.

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[1] *Canonical models*: e.g. Normal errors and identity link; Binomial errors and logit link; Poisson errors and log link. *Hybrid models* use a non-canonical-model link for a given error distribution.

The experimental model

Models of smoking and social fragmentation have been investigated in depth. The variables are:

Smoking — Q141, binary, recoded 0, 1 (4 missing values), dependent variable

Social fragmentation — using sf01_fdec, the full soc frag deciles based on 2001 CAUs (category 1 is the reference category) , main 'exposure' variable

controlling for

(i) *survey-design variables* (to try to avoid weighting)

- (a) Stratum_no (selection variable, 1-7; is NOT equivalent to ethnicity)
- (b) (i) PSU_no (the clustering variable, if possible, with 1178 values); otherwise simpler ways to codify the psu clusters, such as
  - (ii) 'psu-size', the number of observations in a PSU (47 categories)*, or
  - (iii) 'psu8', psu-size grouped into 1-9, 10-19, etc. (8 categories)*
- (c) number of adults in the household (1 to 5)

* the SAS code is given in *Appendix 1*, page 17

(ii) *usual confounding variables*

- (a) Age – Lifecycle_agegrps has four groups: 15-24, 25-44, 45-64, 65+; and there is also Age (in years) and Agegrp10yr (in 10-year groups)
- (b) Sex (M, F)
- (c) Ethnicity (using ETH_MPAO) – also a design variable
- (d) Nzdep01_quintiles (1-5; least to most deprived)

Specific confounding variables for this analysis are not included here as they are unnecessary for an understanding of the technical issues of interest. Such variables may be exposure to indoor tobacco smoke at work, Q152 (recoded 0, 1; 17 missing values), and Language, Q267, (recoded 0, 1; no missing values).

The problems

A large number of real models were explored

(a) to identify the effect of using a weight statement in various packages

(b) to establish if any package can do a multi-level model in a generalised/general linear model framework with the sampling design of the MoH survey

(c) to identify a work-around in the event that no package can actually do all that we want

(d) to establish the statistical adequacy of the work-around, exploring multi-collinearity issues, practicalities such as convergence issues and numbers of parameters, and model-building issues such as the possible need for interaction terms.

The sampling issues

We want to cope with a sampling design

- that has *clustering* in the selected Primary Sampling Units (labelled PSU though, in fact, they are meshblocks), which means that our *effective* sample is smaller than the *actual* one (people in clusters are more like each other than people from different clusters) – and standard errors should therefore be larger than those for a random sample

- that was *stratified* (PSUs were sampled from a roughly ethnically stratified sampling frame [on the basis of a variously-defined 'high' proportion of specific ethnic groups]

- in which *households* were selected with different probabilities, including two for which a census was taken (Chatham and Pitt Islands)

- in which *individuals* were selected from households (of varying sizes), one per household

- and for which the only weight information we have is a *final weight* incorporating (a) selection probabilities of PSU, (b) selection probabilities of house-within-PSU, (c) selection probability of one person within the house, and (d) some post-survey adjustments. The purpose of (d) is to allow good estimates of NZ-population parameters and is essential for estimating population totals.

In a framework of *generalised linear models* [linear models generalised to cope with non-Normal errors], we want to consider multi-level models

(a) that have random intercepts for psus (level 2 in MLwiN parlance)
(b) that may have random slopes, too, but I suspect this level of complication may not be possible in either the social fragmentation or CRAINZ analyses (too many parameters)
(c) so that we can contrast area-level effects with individual-level effects.

The framework for a *general linear model* [Normal errors] is considered here as a special case of estimating a generalised linear model.

The technical issues concerning weighting

The major technical problems concerning weighting are:

(a) Can weighting be incorporated?

(b) Is such weighting accurate?

Single level models

Some canonical generalised linear models can be accomplished using SAS (only models with a logit link and Binomial errors in proc SURVEY LOGISTIC), and in MLwiN (Binomial errors or Poisson errors, each with their canonical links – logit and log, respectively – but not hybrid models such as log links and binomial errors).

SAS will cope correctly with a single-level model of stratified, clustered, and *selection probability-weighted* samples via proc SURVEY LOGISTIC, a procedure based on the survey design package SUDAAN. The estimates and their estimated standard errors are both OK.

MLwiN will cope with selection probability-weighted samples (and clustering in two-level models) but the results (in this logistic framework) are dubious. The weights are of particular concern, as shown in the comparable models 1 and 2 in table 1, where we can safely pretend for this purpose of comparison that the weights are 'pure' selection weights at a single level. While both models may have issues of near-multi-collinearity (discussed more fully later), they ought to produce similar, if inappropriate, estimates. They do not.

Models 3 and 4 introduce control for clustering. Again the models differ substantively. The two SAS models do not differ by much and this is almost-certainly due to the fact that the cluster information per age/sex/ethnic group will be the same information as that contained in the *selection* probabilities. The additional information that is, in fact, in the final weight has apparently not introduced any near-multi-collinearity issues because the standard errors are not unduly inflated in model 3 when compared to model 1.

Table 1: Comparison of single-level logistic models of smoking and social fragmentation in SAS and MLwiN, with weights

					Sex (F)		age 15-24		age 25-44		45-64	
	software	**strata**	**cluster**	**weights**	est	se	est	se	est	se	est	se
1	SAS	no	no	yes	0.086	0.065	1.200	0.132	1.297	0.112	0.954	0.116
2	MLwiN	no	no	yes	0.062	0.045	1.314	0.090	1.374	0.075	0.946	0.078
3	SAS	no	stmt	yes	-0.086	0.066	1.200	0.134	1.297	0.115	0.954	0.113
4	MLwiN	no	yes*	yes	0.059	0.052	1.124	0.115	1.305	0.100	0.944	0.104

* this has level 2 identified as psu in order to account for clustering in the 1178 psus

Regardless of the above comparisons, from theoretical considerations neither package could cope correctly with these complex samples when the weights comprise both selection probabilities *and post-survey adjustments for non-response*. The reasoning for this is that the post-survey weighting process introduces a level of empirical uncertainty beyond the theoretical sampling errors (so that standard errors should increase a bit more than that due solely to selection weights).

Even SURVEY LOGISTIC and other SURVEY procedures in SAS cannot cope with this added level of uncertainty in their calculation of standard errors, although the estimates themselves should be OK. This is because the standard errors for the sampling issues are based on theory, but the post-sampling adjustments are purely empirical and the theory does not extend to them. Therefore, if we wish to draw inferences directly from the sample (allowing for stratification, clustering, and *selection* probabilities), then the SAS procedures are fine. In the Health Survey analyses, we could do this if we had the (intermediate) selection probabilities, and not just the final weights. *But if we wish to draw inferences from the sample (adjusted for non-response) to the full population (thus using the final weight, the only one we have) then empirical procedures to produce appropriate standard errors are necessary. This is true whichever package or procedure is used.* The (group) jackknife procedures are considered the best to estimate the variances of the (most likely, correct) parameter estimates. This is discussed in more detail in *Appendix 1*.

Multi-level models

In SAS, proc SURVEY LOGISTIC cannot add a random intercept. To run such a two-level model you have to use proc GLIMMIX (which calls proc MIXED repeatedly). GLIMMIX can cope with many flavours of canonical and hybrid generalised linear models, but does not explicitly incorporate clustering effects (effectively reducing the sample size), unlike the specifically designed CLUSTER statement in SURVEY LOGISTIC. Nor, in fact, does MIXED cope with selection probabilities, because there the weight statement is a variance adjustment, not a selection probability. It appears that there is no algebraic connection between the inverse variance weights and the selection probability weights either in terms of the algorithms needed to estimate weighted regressions, or between the weights themselves. Thus we could not adapt proc MIXED to approximately suit our purpose by using clustering and final weights.

In MLwiN, we can easily run a two-level model and add a random intercept. We can add other multi-level refinements as well, if we wish. But the use of weighting is problem, as shown earlier, regardless of whether the weights are appropriate or not. Furthermore, MLwiN is set up to use explicit sampling weights at *each* level, although a rather messy pseudo-decomposition of final weights has been suggested (see the MLwiN Help file). However, like SAS, it does not cope with the non-sampling adjustments incorporated in the final weight.

Post-survey adjustments

Post-survey weighting assumes, implicitly, that the information we have in the sample responders is basically the same as that among the non-responders. [This is also assumed when we draw inferences from the sample responders to our target population without post-stratification adjustments, which has been common practice.]

If the response rate is more-or-less constant among the demographic-characteristic groups that we use to re-create the target population – e.g age/sex/ethnicity, which would probably give 56 sub-groups [7x2x4] – then the selection weights would be equal to the final, adjusted, weights. However, in the Health Survey it is almost certain that the non-response varies by demographic sub-group. *One issue to be addressed in any analytic strategy, therefore, is whether there are any inherent dangers in using demographic sub-sample information to estimate the larger demographic sub-group.* For example, if there is just one respondent in one of the sub-groups, then that information is assumed to represent all the wider group, which is unlikely; and, furthermore, there is no variation postulated in that group at all. In the event that inferences from such a sub-group are deemed unwise then the strategy should address whether the analysis proceeds without that sub-group and the write-up annotated accordingly. But because we do not have access to the selection weights we cannot judge the relative unevenness of response rates across demographic sub-groups, and thus judge whether we have some exceptionally-weighted (selection plus high non-response adjustment) responders. Including such responders in any analysis should increase standard errors accordingly, but will not unless we have adequate jackknife samples from which to estimate standard errors empirically. Including such responders could also lead to a false sense of adequacy.

Robert Templeton at MSD has provided a quick macro to produce jackknife estimates (see page 18); and the MoH final weight replicates could be used. So the process can be automated to a large extent (in SAS), but it does mean that any model has to be run 101 times! Note that this process could only be safely adopted for single-level models using SAS proc SURVEY LOGISTIC or proc SURVEY REG, that is, models where weights are treated appropriately.

It would therefore be sensible to try to create a satisfactory work-around that adequately approximates the sampling issues without the use of any weight statement. The suggestion (from various sources) is that linear terms be added to our models that, as near as possible, mirror the sampling design process. This would be tantamount to approximating the selection weight and thus the package-produced theory-based standard errors would be approximately correct. (But note that inferences would then be made from the sample to the target population with the implied assumptions that the non-responders and responders are alike and that there is no differential non-response.)

Conclusions

(a) Final weighting cannot be incorporated correctly by either package.
(b) If a weight statement is used with final weights, it would always underestimate the standard errors.
(c) If we had the actual selection weights instead of the final weights we could get correct parameter estimates and their estimated standard errors from SAS for single level models.
(d) If a SAS proc SURVEY LOGISTIC weighted single-level model is used (without the design variables as well) we should take the almost-final weighted model and run a jackknife procedure to estimate the standard errors of the parameter estimates. These can be set up via a SAS macro.
(e) For all other models we should build our models using sampling-design variables among the explanatory terms to approximate selection probability information.

Selection weight approximations

In order to identify how well simple linear combinations of available sample information can be used to represent the final weight (given that we don't know the selection weight – bother!), ordinary regressions with final weight as the dependent variable are shown in table 2. All the independent variables are entered as a set of dummy variables. Two approximate ways of adjusting for clustering are used, each variable using dummy variables for its categories. The variables are psu47 and psu8, where each classifies an observation by the size of its particular (geographic) cluster, actual in the case of psu47 and grouped in the case of psu8. The reasoning behind their use is that the size of a cluster may reflect the degree to which its component observations are more alike than a randomly selected group of the same size.

Model 8 includes all the sampling information, as described in the MoH document, as linear terms, except for the *actual* probabilities of selection, since that would require knowledge of denominator populations (for strata, PSUs), as far as can be ascertained. The model explains 79 percent of the overall variation in the final weight. The remaining 21 percent of the variation is due to variations in PSU size (and thus minor variations in selection probabilities of individuals), some other small variations in probabilities of selection due to the over-sampling process, and the adjustments made for non-response. The latter may have been considerable as the overall non-response was 28 percent and, inevitably, it would have varied, at least somewhat, by age/sex/ethnic group.

The various models in table 2 aim to indicate the most practical approximation to the selection weights. Interpretation is complicated by the fact that we do not know the selection weights, only the final (adjusted) ones!

Table 2: Multiple regression models for Finalwgt

Model	Straum	Ethnicity	Interaction	Sex	Age group	Adults	PSU	NZdep	R^2
	Dependent variables (confounders/design/ post-survey adjustments; all as dummies)*								
	design	*design*	*?design*	*post-survey*	*post-survey*	*design*	*design*	*post-survey*	
		confounder		*confounder*	*confounder*			*? confounder*	
1	yes	yes		yes	yes - 4				0.56
2	yes	yes		yes	yes - 4		yes - 47		0.60
3	yes	yes	str*PSU-47	yes	yes - 4		yes - 47		0.62
4	yes	yes	str*eth	yes	yes - 4		yes - 47		0.62
5	yes	yes	PSU-47*eth	yes	yes - 4		yes - 47		0.63
6	yes	yes		yes	yes - 4			yes - 5	0.57
7	yes	yes		yes	yes - 4		yes - 47	yes - 5	0.62
8	yes	yes		yes	yes - 7	yes - 5	yes - 1178	yes - 10	**0.79**
9	yes	yes		yes	yes - 7	yes - 5	yes - 47	yes - 10	0.72
10	yes	yes		yes	yes - 4	yes - 5	yes - 47	yes - 10	0.72
11	yes	yes		yes	yes - 4	yes - 5	yes - 47	yes - 5	0.71
12 ?use	yes	yes		yes	yes - 4	yes - 5	yes - 8	yes - 5	0.70
13 use	**yes**	**yes**		**yes**	**yes - 4**	**yes - 5**	**yes - 8**		**0.68**
14	yes	yes		yes	yes - 7	yes - 5	yes - 47	yes - 5	0.72
15	yes	yes		yes	yes - 7	yes - 5	yes - 47		0.70
16	yes	yes		yes	yes - 4		yes - 1178		0.71
17	yes	yes		yes	yes - 4		yes - 47		0.60
18	yes	yes		yes	yes - 7		yes - 47		0.61
19	yes	yes		yes	yes - 7		yes - 47	yes - 5	0.63
20	yes	yes		yes	yes - 7		yes - 47	yes - 10	0.63
21	yes	yes		yes	yes - 4		yes - 47	yes - 10	0.62

* The numbers attached to each variable indicate the number of categories.

Note: Models 8 and 16 each took over 45 minutes to run! The rest took a few seconds. And these were ordinary regressions – no iterations required. The models are not practical propositions!

If we compare the pairs of models in table 2, we can draw the following conclusions:

(i) changing from NZdep deciles to NZdep quintiles makes little difference to the proportion of variance explained (R^2) (models 10, 11 and 19, 20)

(ii) changing from lifestyle age-groups (4) to 10-year age-groups (7) makes little difference (models 9, 10 and 20, 21)

(iii) changing from (impractical) 1178 psus to 47 psu-sizes [number of responders in the psu]* makes a difference of 0.07 to 0.11 in the proportion of variance explained (models 8, 9 and 16, 17) *defined in *Appendix 1* in the SAS code for PSU8

(iv) changing from 47 psu-size variables to 8 grouped-psu-size variables makes little difference (models 11, 12) (and saves 39 dummy variables)

(v) introducing the household size variable does make a difference: 0.09 in the proportion of variance explained (models 14, 19; 15, 18)

(vi) introducing some interactions makes a small difference of 0.02 to 0.03 (model 2 vs models 3, 4, 5)

(vii) not using a psu design variable makes a difference of 0.04 - 0.15 (models 1, 2 and 1, 16)

These observations suggest that **model 13 (or, possibly, model 12) should be the preferred modelling approach.** They account for a sizeable (and acceptable) proportion of the final weight (which we know includes elements of post-survey adjustment) and does not need vast numbers of dummy variables. [Note that introducing NZDep(5) helps a little, but at the cost of four more dummy variables, so could be left out if there are convergence problems and no other modelling reason to include them.]

The approximation might be improved by the addition of interaction terms, but at the cost of ever-increasing numbers of parameters to estimate, so interaction terms could be introduced into those models where there is enough power. However, I suggest that they be ignored, at least until a near-final model is run. At that point interactions could be introduced but I suspect the added uncertainty involved in introducing many more dummy variables is likely to be undesirable, and multi-collinearity might also become a problem.

Single level models

Table 3 explores the relative merits of using the 46 dummies of PSU-size or the 7 dummies for its eight groups in a generalised model. Note that none of these models includes the number of adults in the household, thus removing possible interpretation problems that might occur from the extra dummy variables needed. These experimental models were all accomplished in SAS.

Table 3: Selected single-level logistic models of smoking and social fragmentation controlling for age, sex, ethnicity and selected design indicators*

					Sex (F)		age 15-24		age 25-44		45-64	
model	**strata**	**cluster**	**adults**	**weight**	est	se	est	se	est	se	est	se
SAS Proc SurveyLogistic												
1	fixed	psu47	fixed	no	0.058	0.045	1.423	0.096	1.476	0.078	1.013	0.080
2	fixed	psu8	fixed	no	0.051	0.045	1.430	0.095	1.483	0.077	1.019	0.080
3	stmt	stmt	no	yes	-0.086	0.066	1.200	0.135	1.297	0.115	0.954	0.114
4	stmt	no	no	yes	-0.086	0.065	1.200	0.132	1.297	0.112	0.954	0.116
5	fixed	stmt	no	no	0.063	0.046	1.315	0.092	1.378	0.078	0.934	0.081
6	fixed	psu8	no	no	0.062	0.044	1.307	0.090	1.379	0.075	0.935	0.078
7	fixed	psu47	no	no	0.070	0.045	1.304	0.090	1.374	0.076	0.932	0.079

* indicators were either a set of dummy variables ('fixed') or a SAS statement ('stmt');
psu<x> had *x-1* dummy variables while strata had 7 and adults had 4.

Reassuringly, the standard errors in models 1 and 2 vary by a maximum of 0.001, while the parameter estimates vary a little more, by a maximum of 0.007 – but this is to be expected given the extra 39 dummy variables in model 1.

Thus the suggested strategy is to use 20 survey-design dummy variables to represent the variations in selection probabilities – 6 for strata, 7 for PSU grouped-size, 4 for the number of adults in the household from which the respondent was selected, and 3 for ethnicity (to allow for the complications of over-sampling).

Model 3 would be correct if the final weight was in fact just the overall selection weight. Because it has the added adjustment for non-response, etc, which has been treated only as a further selection weight, the properly-increased standard errors in model 3 are still somewhat too *small* – they do not account for the empirical uncertainty introduced by the non-response adjustments.

There is a niggling concern about the correct adjustment for clustering. Standard errors are increased in the presence of substantial clustering, because the relative 'alikeness' *within* clusters reduces the effective sample size (the square root of which is in the denominator of usual standard error estimators). The effect of the proper treatment of clustering (by a cluster statement, model 3) can be judged by comparing it to model 4. It appears that the added effect of clustering (beyond that introduced, essentially, by the weight) is minimal. The effect of the proper adjustment for clustering can also be judged from the three unweighted models 5, 6 and 7 . The effect is minimal, the maximum discrepancy in standard errors being 0.003. Again, the slightly larger discrepancies between the parameter estimates in the models – 0.011 or less – is most likely fully due to the 7 (or 46) extra dummy terms being estimated in model 6 (or 7).

Two-level models

There is no further issue as far as using linear terms for the design-variables are concerned.

Conclusions

(a) We can use 20 survey-design dummy variables to represent the variations in selection probabilities – 6 for strata, 7 for PSU grouped-size, 4 for the number of adults in the household from which the respondent was selected, and 3 for ethnicity (to allow for the complications of over-sampling).

(b) The additional effect of clustering within PSUs is quite minor when demographic confounders are included in the model. Fixed effect methods of accounting for this clustering (using psu8) are acceptable.

Multi-collinearity

Early explorations suggested the possibility of near multi-collinearity between the ethnicity and stratum dummy variables, even though all ethnicities are represented in each stratum. (The near-collinearity probably occurs with the three much-smaller strata.) This led to an examination of the final weight by ethnicity. Mean final weights are shown below for the 12519 respondents with complete data for the smoking and Social Fragmentation analyses.

group	n	mean	sd	min	max
Asian	1172	134.4	154.3	1.73	750.00
Maori	4120	69.4	132.8	1.53	750.00
Pacific	908	127.5	141.7	1.20	750.00
Other	6329	327.3	209.7	2.62	750.00

Clearly, final weight is related to ethnicity, as you would expect from the sample design. Therefore, to put both in an analysis may introduce some element of multi-collinearity, as would be indicated if there were increases in the standard errors of all, or many, of the estimates. Two models that examine this are shown in table 4.

Table 4: Selected single level survey-logistic models of smoking and social fragmentation in SAS

					Sex (F)		age 15-24		age 25-44		45-64	
model	ethnicity	strata	cluster	weight	est	se	est	se	est	se	est	se
1	no	stmt	stmt	yes	-0.073	0.063	1.375	0.127	1.407	0.111	1.005	0.111
2	yes	stmt	stmt	yes	-0.086	0.066	1.200	0.135	1.297	0.115	0.954	0.113

Neither model includes the number-of-adults dummy variables.

Since the standard errors in the two models vary by 0.007 or less, there is no evidence that near multi-collinearity with ethnicity is a problem. But is there any other evidence of near multi-collinearity? Consider the models shown in Table 5.

Table 5: Further selected single level survey-logistic models of smoking and social fragmentation in SAS

				Sex (F)		age 15-24		age 25-44		45-64	
model	strata	cluster	weight	est	se	est	se	est	se	est	se
1	fixed	stmt	no	0.063	0.046	1.315	0.092	1.378	0.078	0.934	0.081
2	fixed	psu8	no	0.062	0.044	1.307	0.090	1.379	0.075	0.935	0.078
3	stmt	stmt	yes	-0.086	0.066	1.200	0.135	1.297	0.115	0.954	0.114
4	no	stmt	yes	-0.086	0.066	1.200	0.134	1.297	0.115	0.954	0.113

No model includes the number-of-adults dummy variables, but all include ethnicity.

The SAS model 3 is a theoretically appropriate model, accounting properly for the clustering and the weighting, as long as you pretend that the final weight is a pure selection weight – and for the purpose of comparison, it does not matter whether the final weight is technically correct or not. Clearly the standard errors in models 3, and 4 (provided to show the lack of impact of strata), are larger than the others in the table. (The negative estimate for sex is of no great concern, because the term is far from significantly different from zero.) The cluster statement does not appear to be the driver for increasing the standard errors since the standard errors are not increased in model 1. The driver for the increased standard errors seems to be the weight, a conclusion that can also be drawn from the models shown in table 3. But could an alternative explanation for the increased standard errors in models 3 and 4 be some near-multi-collinearity?

If we examine the standard errors for the (non-significant) social fragmentation dummy variables (not shown) we find that the range of their standard errors in a SAS model with both strata and cluster statements, as well as the three confounder groups (age/sex/ethnicity), but ignoring weighting, is 0.136 - 0.164. When a weight statement is added, the range becomes 0.249 - 0.277. The standard errors have increased 60 - 80 percent. *Clearly, multi-collinearity IS an issue in the presence of weights, at least in this demonstration model relating smoking to social fragmentation.*

A note on collinearity diagnostics

Beware of the SAS collinearity diagnostics. If dummy variables are present, SAS will indicate multi-collinearity but this may only be collinearity between the *reference groups* (whether or not an intercept is included). The collinearity diagnostics only work for *interval-level* explanatory variables (such as age in years, or BMI), not for nominal ones (such as the 0-1 dummy variables).

To look for multi-collinearity, you should examine the standard errors and check for 'large' increases in standard errors as more variables are added to a model, step-wise fashion. Experience will indicate what, if any, variables may cause a problem. In a large data set like the Health Survey, increases of 50% or more might indicate some near-multi-collinearity that needs to be examined and removed if possible (by removing explanatory variables). Collinearity is more likely with dummy variables representing lots of small groups (so particularly likely if interaction terms are added). The problem is only of importance to inferences among the exposure variable of interest, but is a problem to good-model building if it occurs in *any* variable. Of particular concern might be the stratum dummy variables. The stratum sizes are **64**, 307, 834, **38**, 2695, **68**, and 8323 records respectively, so strata 1, 4, and 6 should be watched, especially if there are lots of parameters in the model.

Conclusion

Multi-collinearity could be an issue in some models, particularly for dummy variables representing small groups. Standard errors in stepwise models should be monitored for sudden increases which would indicate such a problem and suggest either (i) an inappropriate model, and/or (ii) unreliable parameter estimates and/or confidence intervals (or hypothesis tests). Thus models might start with only the exposure of interest, then add the usual age/sex/ethnicity likely-confounder variables, and then add the stratum, psu8, and number of adults dummies. In the event that there appears to be some multi-collinearity, remove any non-significant exposure variables. You could then try removing confounders one at a time to see if their effect is essentially negligible. The survey-design variables need to remain, so the next step, if one is needed, that might help would be to analyse the data in separate subgroups. On the other hand you might just have to accept the uncertainty (high standard errors), in the interests of epidemiological soundness (adding confounders) and statistical soundness (keeping design variables) .

Software solutions

The software

Two packages are readily available to us: SAS and MLwiN.

SAS is well-established and can deal with a variety of general and generalised linear models (including logistic), both single-level and two-levels, through proc GLIMMIX, as well as survey logistic models through proc SURVEY LOGISTIC. GLIMMIX can incorporate random intercepts through a 'random' statement, but it cannot deal with selection weights. SURVEY LOGISTIC can cope with (pure) selection weights, but cannot add a random intercept term.

MLwiN is more recent and the generalised linear model components are fairly new (and probably less-well tested). It copes explicitly with level-2 clustering, but its treatment of weights is problematic, and especially problematic if the selection probabilities are not explicit at *each* level. A complicated method allows you to de-compose a final weight into level-based pseudo-weights but the results are also problematic.

Practical solutions

As described earlier, we can use design-variable information as linear terms (dummies) in our model instead of using a weight: stratum, a grouped-size indicator for the psus (psu8), the number of adults in the selected household, and ethnic group.

We should also include the usual potential confounder variables of age-group, sex, and ethnicity. These will have the added benefit of allowing, in part, for some of the post-survey adjustments, perhaps along with the quintiles of NZDep if that seems warranted in particular models.

We can build our models by including these variables. Our usual interpretations then hold – we draw inferences about the target population from the sample making the usual assumptions that differential non-response bias is negligible, and non-responders are not too dissimilar from responders.

In single level models, SAS proc SURVEY LOGISTIC and MLwiN will give similar results. This is shown for two sets of models in table 6 – models 1 and 2, and 5 and 6 – where the slight differences in estimated standard errors (0.004 or less) are most likely due to the easier convergence criteria used in MLwiN – 10^{-3}, as opposed to the default 10^{-8} in SAS.

Table 6: Comparison of (essentially) single level unweighted models of smoking and social fragmentation in SAS, proc SURVEY LOGISTIC, and MLwiN

					Sex (F)		age 15-24		age 25-44		45-64	
	software	strata	cluster	adults	est	se	est	se	est	se	est	se
1	SAS	fixed	psu8	fixed	0.051	0.045	1.430	0.095	1.483	0.077	1.019	0.080
2	MLwiN	fixed	psu8	fixed	0.051	0.044	1.430	0.094	1.484	0.076	1.020	0.079
3	SAS	fixed	psu8 *	fixed	0.051	0.046	1.430	0.101	1.484	0.080	1.020	0.083
4	MLwiN	fixed	psu8 * †	fixed	0.051	0.044	1.430	0.094	1.484	0.076	1.020	0.079
5	SAS	fixed	stmt	no	0.063	0.046	1.315	0.092	1.378	0.078	0.934	0.081
6	MLwiN	fixed	yes † ‡	no	0.063	0.044	1.315	0.089	1.378	0.074	0.934	0.077

* Model 3 has an *additional* 'cluster' (and 'strata') statement. Model 4 has level 2 identified as 'psu' [*not* psu8] *in addition* to psu8. The additions account for any clustering in the 1178 psus not already accounted for by the fixed PSU8 dummies.

† Models 4 and 6 could be classed as two-level models but they have no parameter refinements at level 2, so are essentially comparable to the SAS models.

‡ Model 6 has level 2 identified as 'psu', to compare with the SAS model.

Models 3 and 4 provide reassurance that the use of the fixed design effects is coping quite well with the clustering within the 1178 PSUs. Clustering is of concern, of course, because the usual standard errors need to be adjusted upwards in the presence of any clustering effect. Both models include the software-appropriate ways to account for clustering *as well as* the fixed-effect design effects which are all that we will be able to use in SAS proc GLIMMIX runs. The software differences in models 3 and 4 are slightly larger than in the other pairs in this table, although (as one would hope) the parameter estimates agree, at least to 0.001. The SAS standard error estimates are now up to 0.007 larger than the MLwiN ones, suggesting that the specific cluster statement in SAS is allowing for an element of clustering not fully accounted for by the limited number of PSU8 effects.

This is encouraging, for two reasons. First, it is highly unlikely that there is no clustering effect at all, so we would expect the introduction of a cluster statement to have some noticeable effect given that only seven dummy variables of PSU8 are approximating the full clustering effect. Second, **the additional amount of clustering is small, and unlikely to make a difference to conclusions drawn from any model which does not, or can not, include a specific cluster statement as well as the fixed design-effects.**

It is, however, of concern that the MLwiN estimates in model 4 are unchanged from model 2. This may be due to differences in philosophy between the single-level approach of Survey Logistic, and the multi-level approach of MLwiN which aims, primarily, to *disentangle residuals* at each level. Which is right? I suspect that SAS is more accurate, because it is increasing the standard errors in a way I would expect, unlike MLwiN. Note that the clustering effect may be larger, or smaller, in models involving other outcome and exposure variables. This could be checked with two quick SAS proc Survey Logistic runs for any final combination of variables in a given analysis (regardless of the actual method of producing the final model, say, a proc Glimmix run). One run could be similar to that shown in Appendix 1; the other would be the same run minus the 'cluster' and 'strata' statements. The observed difference in standard errors between the two outputs for the exposure variables of interest is the likely level of unaccounted-for clustering in the proc Glimmix run (unweighted, of course) with fixed design-variable effects. Hopefully, this minimal extra work could give rise to a one-line reassurance in the Methods section of any resulting paper.

In all other types of generalised linear models, SAS proc GLIMMIX can be used to model random intercepts. As long as no weight statement is used, SAS and MLwiN should give similar – but not near-identical – results, judging by the two models in table 7. The differences are not due to the chosen MLwiN estimation method: the IGLS and RGLS estimates for model 2 differ by at most 0.001 (not shown). Thus, again, part of the difference, at least, is likely to be due to the weaker convergence criteria used in MLwiN. Of note is the fact that the MLwiN process would not converge to even 10^{-4}, at least in well over 100 iterations. There were also some models which would not converge to anything beyond 10^{-2}, which leads to a concern about the differences between the algorithms used in SAS and those used in MLwiN.

Table 7: Comparison of two-level unweighted models of smoking and social fragmentation in SAS, proc GLIMMIX, and MLwiN

					Sex (F)		age 15-24		age 25-44		45-64	
	software	**strata**	**cluster**	**adults**	est	se	est	se	est	se	est	se
1	SAS*	fixed	psu8	no	0.057	0.044	1.281	0.086	1.351	0.070	0.911	0.073
2	MLwiN*	fixed	psu8	no	0.057	0.044	1.302	0.090	1.372	0.075	0.933	0.078

* a random statement for psu was used in GLIMMIX, and a random intercept in MLwiN

Conclusions

(a) Design information can be incorporated as dummy variables in both SAS and MLwiN.

(b) Two-level logistic models with random intercepts can be fitted in SAS proc GLIMMIX and in MLwiN but the MLwiN model should probably be treated as approximate.

(c) More complicated hybrid generalised linear models can only be fitted in SAS.

(d) More complex multi-level canonical* models can only be fitted in MLwiN but, given (b) above, the results should be treated with caution. *defined in the footnote on page 2

Fitting general linear models

The same general conclusions about weights apply as those for the generalised linear models discussed in the foregoing:

(a) Use of the final weights is problematic due to the non-sampling component

(b) Use of a weight statement is almost-certainly problematic in any procedure except SAS proc SURVEY REG.

(c) Dummy variables can be used to approximately mimic the survey design selection probabilities in unweighted SAS proc GLIMMIX and MLwiN models, with some reservations about the accuracy of all the MLwiN results, and the SAS standard errors.

(d) A jackknife procedure using the MoH replicate final weights could be used to appropriately estimate standard errors in single-level weighted SAS proc SURVEY REG models that do not contain the purely design-mimicking dummy variables.

Notes about the supplied weights

The final weight

The Ministry document 'Methodology' indicates that the selection of strata and PSUs within them aimed to 'ensure equal probability of selection for every dwelling within the target population'. Selection was then one person per dwelling, so depended on household size (adults) – information in variable Q277. While each stratum indicated a high proportion of a certain ethnicity, selection within a stratum was not restricted to one ethnicity. However, some ethnic groups were over-sampled, so ethnicity is a key design variable (as well as one of interest). Probability of PSU selection varied by strata, so stratum is an important design component.

This suggests that a combination of stratum, PSU, ethnicity, and household size *largely* identified the *sampling strategy* variables. In turn, this suggests that the final weight is composed of stratum, PSU, ethnicity, and household size information, plus extra information on non-sampling errors such as non-response/non-contact, since the final weight was determined to re-produce the population structure of NZ in 2001.

The extra adjustment to produce the final weights does not involve sampling design, so the 'extra' element of error in the weighted parameter estimates is not governed by sampling theory. If we can approximate the sampling (selection) weights by some means other than using a weight, the standard errors of our estimates will allow for the sampling errors introduced by the survey design. To allow also for non-sampling errors (bias), we would have to use an empirical method (such as a group jackknife) which could be rather time consuming for the type of models we want to fit.

Use of weights

In epidemiological-type surveys we normally assume that relationships between variables are similar among the responders and the non-responders as long as key variables are distributed *similarly* in these two groups. Further, we do not usually worry about differential non-response. If we analyse data within the age/sex/whatever groups that might have differential non-response there is, in fact, no problem. If differential non-response is considered at all, the argument is that there are much larger 'errors' in our data than the small adjustments needed to allow for differential non-response. But this argument is inadequate when the purpose at hand is to estimate population totals, which may have fiscal or other important implications. In the powerful modern computing environment, another argument says we should use these more-correct estimation methods whenever feasible.

The key issue for surveys where the main purpose is to investigate *relationships* is whether there is an appreciable *differential* non-response in key groups, because this could affect the relative importance of those groups in determining the relationships (e.g. calculating a correlation coefficient). One way to approach this is to analyse the survey separately within these groups, but this would be cumbersome and we would lose large amounts of statistical power. A preferable approach would be to include the group information in the model. The linear terms may not cope fully with the differential response, although interaction terms might improve matters. The problem with introducing interaction terms, of course, is the plethora of extra variables needed, especially if nominal variables are involved. However, we could build models with linear terms, and check the final model for any change caused by introducing interaction terms as long as we have enough power to do so. The best approach, however, is to use the survey weights and a jackknife method to estimate standard errors. *This pre-supposes that a procedure is available that correctly utilises the weights in the first place.* As we have seen, this is only applicable in special circumstances for the sorts of models we want to explore.

We have shown that we can not use weights safely in either MLwiN (see table 1), or in SAS proc GLIMMIX (see *Multi-level models*, page 6). The standard errors are wrong in both cases, and the parameter estimates are suspect in MLwiN. We can use weights in SAS proc SURVEY LOGISTIC, or proc SURVEY REG, for single-level models, as long as there is no major near-multi-collinearity. Thus, only in the SAS 'SURVEY' procedures could we use the jackknife procedure and the MoH-supplied replicated weights.

Use of the replicated weights

The replicated weights [Finalwgt_rep1, etc.] were created by the delete-a-group form of the empirical jackknife procedure.

Using the replicated weights, 100 replicated analyses could be used to estimate the standard errors of the parameter estimates, where the final standard deviation of an estimate is the standard deviation of the 100 estimates of the parameter. In this way we attempt to control adequately for the various forms of sampling and non-sampling error in our sample data.

Suggestion: It might be instructive to run one such model just to see how much difference it actually makes over and above the suggested use of 20 dummy variables to describe the main features of the survey design. I suspect it makes little difference (because the inferences from sample-to-population using the usual theory-based standard errors, and sample-adjusted-for-non-response-to-population using empirical [jackknife] standard errors, assume the same things, namely non-responders are like responders, and there is no differential non-response). If so, it would provide both reassurance and an opportunity for a smug statement in a methods section!

Appendix 1: SAS

NOTE:
Please contact June if you have any queries or difficulties in using/adapting these codes.

Sample code for creating the variable **PSU8** *and running the procedures* **SurveyLogistic** *and* **Glimmix.**

```
*----CREATE PSU8----;

libname s 'E:\Test'; *----change as required----;
x       'cd E:\Test';

*----create cluster size [psu_total] from 1178 unique PSU numbers----;
*----NOTE: a numeric version of the character variable psu_no (psu) had
     been made in an earlier data step using psu = inputn(PSU_no,8.);

proc  sort   data=s.nzhs2003_curf_nz_v2c;          by psu_no;
proc  means  data=s.nzhs2003_curf_nz_v2c noprint; by psu_no;
      output out=b n=psu_total;    *----psu_total has 47 categories----;
      var    psu;
run;


*----create new file and add psu8----;

data  s.newcurf;
merge s.nzhs2003_curf_nz_v2c b; by psu_no;
psu8  = int(psu_total/10)+1;
run;


*----check results----;

proc freq data=s.newcurf;
     tables psu_no*psu_total*psu8 / list;
run;
```

[Sample lines from the cross-tabulation list are shown below.]

PSU_no	psu_total	psu8	Frequency	Percent	Cumulative Frequency	Cumulative Percent
1	12	2	12	0.10	12	0.10
2	9	1	9	0.07	21	0.17
3	11	2	11	0.09	32	0.26
4	9	1	9	0.07	41	0.33
5	22	3	22	0.18	63	0.50
6	27	3	27	0.22	90	0.72
7	13	2	13	0.10	103	0.82
8	18	2	18	0.14	121	0.97
9	2	1	2	0.02	123	0.98
10	8	1	8	0.06	131	1.05
.....						
1171	7	1	7	0.06	12450	99.37
1172	12	2	12	0.10	12462	99.47
1173	8	1	8	0.06	12470	99.53
1174	12	2	12	0.10	12482	99.62
1175	12	2	12	0.10	12494	99.72
1176	12	2	12	0.10	12506	99.82
1177	10	2	10	0.08	12516	99.90
1178	13	2	13	0.10	12529	100.00

```
*----EXAMPLES, both set up for convergence to 0.00001, although SAS seems
     to have little difficulty in converging to the default 1E-8----;

*----Single-level model; SURVEYLOGISTIC----;

proc printto new file='TestRuns.out'; run; *---file for selected output---;
ods select none;

ods select type3 parameterestimates;
title1 'Single-level logistic model using proc SurveyLogistic';
title2 'Fixed effects for ethnicity, stratum, psu8, and adults';
proc  surveylogistic data=s.newcurf;
      class    sf01_fdec(ref='1') Lifecycle_agegrps Sex Eth_MPAO stratum_no
               psu8 hhadults / param = ref;
   strata  stratum_no;
   cluster psu;
      model    smoke (event='1') = sf01_fdec Lifecycle_agegrps Sex Eth_MPAO
               stratum_no psu8 hhadults / GCONV=1E-5;
run;
proc printto; run;


*----Two-level model: GLIMMIX----;

title1 'Two-level logistic model using proc Glimmix';
title2 'Fixed effects for ethnicity, stratum, psu8, and adults';
title3 'Random intercept at level-2 (psu)';

proc   glimmix data=s.minicurf2 pconv=1E5;
class  sf01_fdec Lifecycle_agegrps Sex Ethnic stratum_no psu8 hhadults;
model  smoke (event='1') = sf01_fdec Lifecycle_agegrps Sex Ethnic
       stratum_no psu8 hhadults /dist=binary solution;
random int/subject=psu;    *----random int[ercept] at level 2(psu)----;
run;
```

Sample macro for creating **jackknife estimates**.

The following SAS macro has been adapted by June Atkinson from one written by Robert Templeton. June intends to extend the macro to deal with the Survey Logistic procedure, when required.

```
*----Repeatedly run the survey procedure for each jackknife and put
     the results together in one data set----;


%MACRO JACKS (NUM_JACKS=100,DATASET=jacks);

*----NUM_JACKS is the number of jackknife weights you want to use.
     Default=100. DATASET is the data set the analyses are to be
     done on. Default=jacks----;

%DO I=1 %TO &NUM_JACKS;

ODS  OUTPUT STATISTICS=MEAN&I (RENAME=(MEAN=MEAN&I));

PROC SURVEYMEANS DATA=&dataset;
     STRATA       STRATUM_NO;
     CLUSTER      PSU;
     WEIGHT       FINALWGT_REP&I ;
     VAR          <.....>;
RUN;
%END;
```

[macro continued overleaf]

```
DATA ALLMEANS;
  MERGE %DO I=1 %TO &num_jacks ; MEAN&I  %END; ;
  STD_ERR=SQRT((&num_jacks /(&num_jacks-1))* CSS(OF MEAN1-MEAN&num_jacks));
  OVERALL_MEAN=MEAN(OF MEAN1-MEAN&num_jacks);
  LOWER=OVERALL_MEAN- 1.96 *STD_ERR;
  UPPER=OVERALL_MEAN+ 1.96 *STD_ERR;
RUN;

PROC PRINT DATA=ALLMEANS;
  VAR OVERALL_MEAN STD_ERR LOWER UPPER;
RUN;
```

SAS procedures: capabilities

Proc SURVEYLOGISTIC

(i) Logistic models (Binomial errors and a logit link) in a survey setting. Logistic models are a member of the class of generalised linear models. The dependent (outcome) variable is usually binary, such as smoker (yes/no). Independent variables (exposures, confounders, exposure-modifiers, etc.) can be nominal (as one or more dummy yes/no variables), ordinal (ditto, which thus ignores the ordering), or interval (such as BMI or age).

(ii) Allows for clustering via the 'cluster' statement, and stratification (not so crucial) via the 'strata' statement.

(iii) Single-level models only.

(iv) The weight statement allows for *selection* probabilities (but is inadequate for weights that are also adjusted for differential non-response, such as the MoH final weight).

(v) Can be used with Jackknife replicate-weights for selection probabilities adjusted for differential non-response.

(vi) Can also provide suitable models using the design-variable-fixed-effects, no-weights approach.

Proc GLIMMIX

(i) Single-level and simple two-level generalised linear mixed (random and fixed effects) models which can use a variety of links and error distributions. Identity link and Normal errors produces linear multiple regression models. Logit link and Binomial errors produces logistic models. The sample should be simple random, that is, not have a complex survey design. The form of the dependent variable should be consistent with the link and the error-distribution. Thus both general linear models and linear logistic models can be fitted, as well as several others. Permitted types of variables are described in Proc Surveylogistic (i) and Proc Surveyreg (i).

(ii) Can not allow for any non-simple-random survey characteristics (clustering, strata).

(iii) Two-level models can have random intercepts by using the 'random' statement.

(iv) The weight statement assumes the weights to be proportional to the reciprocals of the error variances, or similar, so is not suitable for selection-probability weights.

(v) Since the weight statement is not geared to selection probabilities, in whole or in part, empirical Jackknife estimates of standard errors can not be utilised.

(vi) Can provide suitable models using the design-variable-fixed-effects, no-weights approach.

Proc GLM

(i) Linear multiple-regression models for simple random samples (See Surveyreg (i) for descriptions of variables).
(ii) Can not allow for any non-simple-random survey characteristics (clustering, strata).
(iii) Single-level models only.
(iv) The weight statement assumes the weights to be proportional to the reciprocals of the error variances, or similar, so is not suitable for selection-probability weights.
(v) Since the weight statement is not geared to selection probabilities, in whole or in part, empirical Jackknife estimates of standard errors can not be utilised.
(vi) Can provide models using the design-variable-fixed-effects, no-weights approach.

Proc SURVEYREG

(i) General linear multiple-regression models (Normal errors) in a survey setting. The dependent (outcome) variable is continuous [interval] (e.g. BMI) but not necessarily Normal or even completely symmetric. The residual term, however, is Normal, so any non-Normality in the dependent variable should be accounted for by the explanatory variables. Hence the exploration of the distribution of residuals in good analytic practice.
(ii) Allows for clustering via the 'cluster' statement, and stratification (not so crucial) via the 'strata' statement.
(iii) Single-level models only.
(iv) The weight statement allows for *selection* probabilities (but is inadequate for weights that re also adjusted for differential non-response).
(v) Can be used with Jackknife replicate-weights for selection probabilities adjusted for differential non-response
(vi) Can also provide models using the design-variable-fixed-effects, no-weights approach.

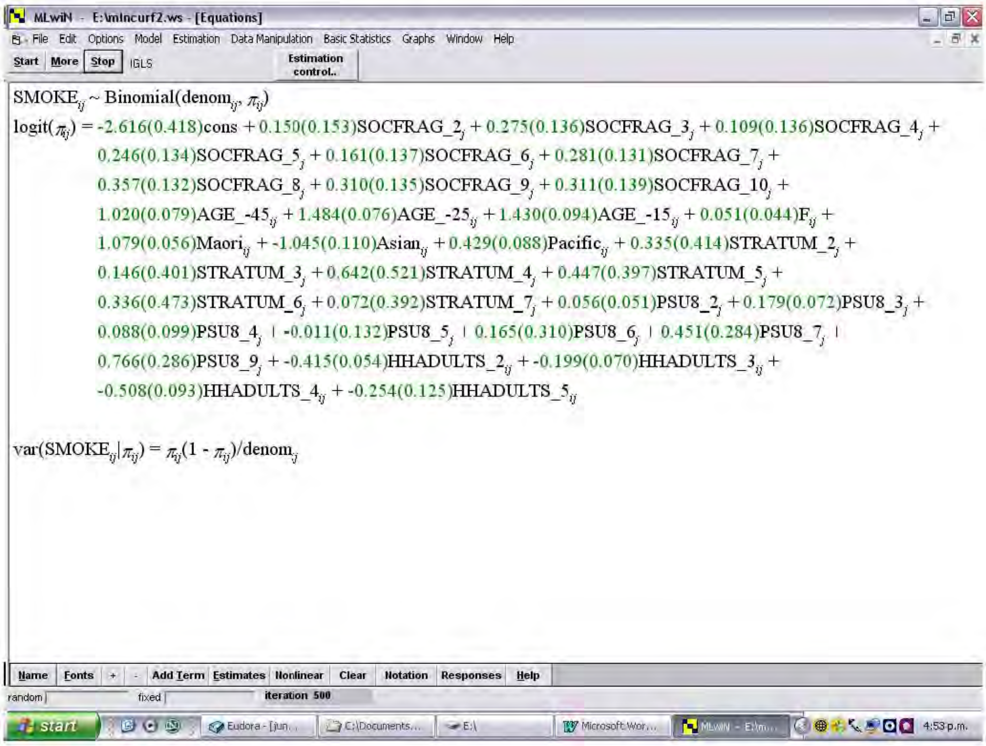
Appendix 2: MLwiN

A screen capture of an MLwiN run is shown on the next page. It has two levels: psu is level-2 (1178 values); and a generated ID of 1 to 12529 is level-1. The model does NOT have a random intercept (see model 4, table 6) thus allowing for clustering at the PSU-level in a single-level model framework.

Estimation control was set to 0.001 (note that the default is 0.01). Occasionally, a model did not converge.

Note: the category variable 'PSU_9$_i$' is not a mistake; it is automatically produced – clearly the MLwiN code does not allow a variable name ending in a number – PSU8 – to have a category member ending in the same number.

The simple, essentially single-level, model on the next page has level-2 defined (subscript j) but does not have a random intercept at level 2.



MLwiN: capabilities

The following description uses the same framework as the descriptions of the capabilities of the various SAS procedures in *Appendix 1*.

(i) General linear multiple-regression models (Normal errors) as well as canonical generalised linear models (e.g. Binomial errors with a logit link; Poisson errors with a log link).

(ii) Allows for clustering via the level-2 identification in the dependent variable definition but, apparently, not adequately.

(iii) Single-level models, two-level models, and more, with allowances for random intercepts and random coefficients. Most flexible modelling across two or more levels.

(iv) The weight adjustments do not appear to be adequate. Furthermore, if used, component weights are required *at each level*, so any overall weight would have to be empirically de-composed into pseudo-weights at each level. The method of decomposition (see MLwiN help file) should be considered as developmental.

(v) Since the weight adjustments are not adequate, empirical Jackknife estimates of standard errors could not be considered reliable.

(vi) Can provide *probably approximate* models using the design-variable-fixed-effects, no-weights approach.

Appendix 2

Table Appendix 2:1 Unadjusted risk of smoking; Women 15-19

	n with smoking data	*n smokers*	*% who smoke*	*Unadjusted odds Ratio*
Ethnicity				
Mäori	16,971	6,855	40.4%	**3.10**
Pacific	7,272	1,215	16.7%	**0.91**
nonMäori,nonPacific	66,918	11,859	17.7%	**1**
Education				
no qualifications	25,143	7,305	29.1%	**1.71**
Qualifications	66,021	12,627	19.1%	**1**
Labour Force Status				
unemployed	12,129	4,059	33.5%	**1.85**
non-labour	35,379	6,645	18.8%	**0.85**
employed	43,653	9,225	21.1%	**1**
NeighFrag				
decile 1	2,382	432	18.1%	**0.61**
deciles 2,3	8,682	1,908	22.0%	**0.85**
deciles 4,5 (ref)	10,725	2,853	26.6%	**1**
deciles 6,7	15,564	4,575	29.4%	**1.16**
deciles 8,9	21,378	6,633	31.0%	**1.25**
decile 10	12,504	3,531	28.2%	**1.08**
Neighbourhood Deprivation				
quintile 1 (least deprived)	18,366	2,376	12.9%	**1**
quintile 2	15,768	2,637	16.7%	**1.33**
quintile 3	15,825	3,426	21.6%	**1.87**
quintile 4	20,073	5,076	25.3%	**2.26**
quintile 5 (most deprived)	21,126	6,414	30.4%	**2.93**
Total	91,164	19,932	21.9%	

Table Appendix 2:2 Unadjusted risk of smoking; Men 15-19 yrs

	n with smoking data	*n smokers*	*% who smoke*	*Unadjusted odds Ratio*
Ethnicity				
Mäori	15663	4659	29.7%	2.21
Pacific	6882	1260	18.3%	1.10
nonMäori,nonPacific	66939	11238	16.8%	1
Education				
no qualifications	29649	7950	26.8%	1.95
Qualifications	59841	9207	15.4%	1
Labour Force Status				
unemployed	10542	3273	31.0%	1.73
non-labour	35055	4779	13.6%	.62
employed	43896	9108	20.7%	1
NeighFrag				
decile 1	3102	456	14.7%	0.77
deciles 2,3	11193	1980	17.7%	0.95
deciles 4,5 (ref)	14109	2535	18.0%	1
deciles 6,7	20583	4035	19.6%	1.09
deciles 8,9	27108	5592	20.6%	1.18
decile 10	13389	2559	19.1%	1.09
Neighbourhood Deprivation				
quintile 1 (least deprived)	18984	2475	13.0%	1
quintile 2	16296	2607	16.0%	1.27
quintile 3	16320	3168	19.4%	1.60
quintile 4	18609	4071	21.9%	1.87
quintile 5 (most deprived)	19284	4833	25.1%	2.20
Total	89490	17157	19.2%	

Table Appendix 2:3 Unadjusted risk of smoking; Women 20-24 yrs

	n with smoking data	n smokers	% who smoke	Unadjusted odds Ratio
Ethnicity				
Mäori	16,881	8,796	52.1%	2.92
Pacific	7,020	2,169	30.9%	1.30
nonMäori, nonPacific	77,628	20,763	26.7%	1
Education				
no qualifications	15,216	8,754	57.5%	3.75
qualifications	86,307	22,971	26.6%	1
Labour Force Status				
unemployed	9,192	3,909	42.5%	1.90
non-labour	25,149	9,039	35.9%	1.40
employed	67,176	18,774	27.9%	1
NeighFrag				
decile 1	2,088	645	30.9%	0.94
deciles 2,3	9,069	2,961	32.6%	1.00
deciles 4,5 (ref)	12,726	4,104	32.2%	1
deciles 6,7	20,523	6,942	33.8%	1.06
deciles 8,9	33,195	10,974	33.1%	1.02
decile 10	23,919	6,102	25.5%	0.71
Neighbourhood Deprivation				
quintile 1 (least deprived)	17,442	3,735	21.4%	1
quintile 2	16,506	4,347	26.3%	1.33
quintile 3	18,822	5,934	31.5%	1.70
quintile 4	23,925	8,127	34.0%	1.89
quintile 5 (most deprived)	24,825	9,582	38.6%	2.33
Total	101,523	31,725	31.2%	

Table Appendix 2:4 Unadjusted risk of smoking; Men 20-24 yrs

	n with smoking data	*n smokers*	*% who smoke*	*Unadjusted odds Ratio*
Ethnicity				
Mäori	14,889	6060	40.7%	**1.86**
Pacific	6,375	2,127	33.4%	**1.39**
nonMäori,nonPacific	73,221	19,338	26.4%	**1**
Education				
no qualifications	16,632	8,730	52.5%	**3.44**
Qualifications	77,853	18,795	24.1%	**1**
Labour Force Status				
unemployed	9,054	3,633	40.1%	**1.68**
non-labour	16,251	4,080	25.1%	**0.85**
employed	69,183	19,818	28.6%	**1**
NeighFrag				
decile 1	2,211	603	27.3%	**0.88**
deciles 2,3	9,147	2,631	28.8%	**0.93**
deciles 4,5 (ref)	12,375	3,711	30.0%	**1**
deciles 6,7	19,287	5,955	30.9%	**1.05**
deciles 8,9	29,691	9,099	30.6%	**1.02**
decile 10	21,777	5,535	25.4%	**0.79**
Neighbourhood Deprivation				
quintile 1 (least deprived)	17,379	3,729	21.5%	**1**
quintile 2	15,849	4,044	25.5%	**1.26**
quintile 3	17,376	5,130	29.5%	**1.56**
quintile 4	22,089	6,972	31.6%	**1.70**
quintile 5 (most deprived)	21,795	7,656	35.1%	**1.93**
total	94,485	27,528	29.1%	

Appendix 3

Table Appendix 3:1
Progressively adjusted Odds ratios of NeighFrag and youth smoking; 15-19 year old women

Exposure Variables	*Model 1*	*Model 2*	*Model 3*
	NF	***NF & individual***	***NF & individual & NZDep***
Neighbourhood Fragmentation			
NF Dec 1 (least fragmented)	0.61 (0.50-0.73)	0.68 (0.58-0.80)	0.85 (0.74-0.99)
NF Dec 2,3	0.83 (0.73-0.94)	0.85 (0.76-0.95)	0.96 (0.87-1.05)
NF Dec 4,5 (Ref)	1	1	1
NF Dec 6,7	1.11 (0.99-1.24)	1.05 (0.96-1.16)	0.97 (0.89-1.05)
NF Dec 8,9	1.21 (1.08-1.35)	1.16 (1.05-1.26)	0.97 (0.90-1.05)
NF Dec 10 (most fragmented)	1.24 (1.09-1.41)	1.33 (1.19-1.48)	1.06 (0.96-1.16)
Ethnicity			
Mäori		2.54 (2.44-2.65)	2.40 (2.31-2.50)
Pacific People		0.75 (0.69-0.80)	0.69 (0.64-0.74)
NonMäori NonPacific		1	1
Education Qualifications			
No qualifications		0.82 (0.78-0.87)	0.82 (0.77-0.86)
School qualifications		0.46 (0.44-0.49)	0.47 (0.44-0.49)
Post-School qualifications		1	1
Labour Force Status			
Unemployed		1.50 (1.44-1.58)	1.48 (1.42-1.55)
Non-labour		0.75 (0.72-0.78)	0.74 (0.71-0.77)
Employed		1	1
Neighbourhood Deprivation			
Most deprived			2.22 (2.04-2.42)
Quintile 4			1.89 (1.74-2.06)
Quintile 3			1.57 (1.45-1.71)
Quintile 2			1.26 (1.16-1.37)
Least deprived			
Random Variance (s.e. of variance)			
Individual level	0.970 (0.0046)	0.970 (0.0046)	0.970 (0.0046)
Neighbourhood level	0.240 (0.0147)	0.130 (0.0098)	0.074 (0.0067)

Table Appendix 3:2
Progressively adjusted Odds ratios of NeighFrag and youth smoking; 15-19 year old men

Exposure Variables	***Model 1***	***Model 2***	***Model 3***
	NF	***NF & Individual***	***NF & Individual & NZDep***
Neighbourhood Fragmentation			
NF Dec 1 (least fragmented)	0.74 (0.62-0.87)	0.79 (0.69-0.92)	0.92 (0.80-1.06)
NF Dec 2,3	0.98 (0.87-1.10)	1.00 (0.91-1.10)	1.08 (0.99-1.18)
NF Dec 4,5 (Ref)	1	1	1
NF Dec 6,7	1.12 (1.01-1.24)	1.10 (1.01-1.19)	1.04 (0.96-1.12)
NF Dec 8,9	1.21 (1.09-1.33)	1.18 (1.09-1.28)	1.07 (0.99-1.15)
NF Dec 10 (most fragmented)	1.24 (1.11-1.39)	1.42 (1.29-1.56)	1.22 (1.12-1.34)
Ethnicity			
Mäori		1.66 (1.59-1.73)	1.58 (1.51-1.65)
Pacific People		0.90 (0.83-0.96)	0.84 (0.78-0.90)
NonMäori NonPacific		1	1
Education Qualifications			
No qualifications		0.93 (0.88-0.99)	0.93 (0.87-0.98)
School qualifications		0.40 (0.38-0.43)	0.41 (0.39-0.43)
Post-School qualifications		1	1
Labour Force Status			
Unemployed		1.45 (1.38-1.53)	1.44 (1.37-1.51)
Non-labour		0.55 (0.52-0.57)	0.55 (0.53-0.57)
Employed		1	1
Neighbourhood Deprivation			
Most deprived			1.71 (1.57-1.85)
Quintile 4			1.57 (1.45-1.70)
Quintile 3			1.41 (1.30-1.52)
Quintile 2			1.20 (1.11-1.30)
Least deprived			1
Random Variance (s.e. of variance)			
Individual level	0.970 (0.0046)	0.980 (0.0047)	0.980 (0.0047)
Neighbourhood level	0.160 (0.0116)	0.084 (0.0078)	0.059 (0.0064)

Table Appendix 3:3
Progressively adjusted Odds ratios of NeighFrag and youth smoking; 20-24 year old women

Exposure Variables	*Model 1*	*Model 2*	*Model 3*
	NF	*NF & individual*	*NF & individual & NZDep*
Neighbourhood Fragmentation			
NF Dec 1 (least fragmented)	0.88 (0.74-1.04)	1.01 (0.87-1.17)	1.21 (1.06-1.39)
NF Dec 2,3	1.01 (0.90-1.14)	1.05 (0.95-1.15)	1.15 (1.05-1.25)
NF Dec 4,5 (Ref)	1	1	1
NF Dec 6,7	1.07 (0.96-1.19)	1.04 (0.95-1.13)	0.97 (0.90-1.04)
NF Dec 8,9	1.08 (0.97-1.19)	1.03 (0.95-1.11)	0.90 (0.84-0.97)
NF Dec 10 (most fragmented)	0.79 (0.70-0.89)	0.88 (0.80-0.97)	0.74 (0.68-0.80)
Ethnicity			
Mäori		2.20 (2.12-2.29)	2.13 (2.05-2.21)
Pacific People		0.92 (0.87-0.98)	0.88 (0.83-0.94)
NonMäori NonPacific		1	1
Education Qualifications			
No qualifications		3.44 (3.30-3.60)	3.35 (3.21-3.50)
School qualifications		1.39 (1.35-1.44)	1.38 (1.34-1.42)
Post-School qualifications		1	1
Labour Force Status			
Unemployed		1.43 (1.36-1.50)	1.41 (1.34-1.48)
Non-labour		1.05 (1.01-1.08)	1.03 (1.00-1.07)
Employed		1	1
Neighbourhood Deprivation			1.93 (1.79-2.08)
Most deprived			
Quintile 4			1.78 (1.65-1.92)
Quintile 3			1.54 (1.43-1.65)
Quintile 2			1.27 (1.18-1.37)
Least deprived			1
Random Variance (s.e. of variance)			
Individual level	0.980 (0.0044)	0.980 (0.0044)	0.980 (0.0044)
Neighbourhood level	0.220 (0.0129)	0.100 (0.0077)	0.066 (0.0055)

Table Appendix 3:4
Progressively adjusted Odds ratios of NeighFrag and youth smoking; 20-24 year old men

Exposure Variables	***Model 1***	***Model 2***	***Model 3***
	NF	***NF & individual***	***NF & individual & NZDep***
Neighbourhood Fragmentation			
NF Dec 1 (least fragmented)	0.86 (0.74-1.00)	0.92 (0.81-1.05)	1.04 (0.92-1.19)
NF Dec 2,3	0.95 (0.85-1.05)	0.96 (0.88-1.04)	1.02 (0.95-1.11)
NF Dec 4,5 (Ref)	1	1	1
NF Dec 6,7	1.04 (0.94-1.14)	1.01 (0.94-1.09)	0.96 (0.90-1.03)
NF Dec 8,9	1.07 (0.98-1.17)	1.04 (0.97-1.12)	0.95 (0.89-1.01)
NF Dec 10 (most fragmented)	0.86 (0.78-0.96)	0.97 (0.89-1.05)	0.84 (0.78-0.91)
Ethnicity			
Mäori		1.42 (1.37-1.48)	1.38 (1.32-1.43)
Pacific People		1.06 (1.00-1.12)	1.01 (0.95-1.08)
NonMäori NonPacific		1	1
Education Qualifications			
No qualifications		3.46 (3.32-3.61)	3.36 (3.22-3.50)
School qualifications		1.27 (1.23-1.32)	1.27 (1.22-1.31)
Post-School qualifications		1	1
Labour Force Status			
Unemployed		1.36 (1.30-1.43)	1.34 (1.28-1.41)
Non-labour		0.81 (0.78- 0.85)	0.81 (0.78-0.84)
Employed		1	1
Neighbourhood Deprivation			
Most deprived			1.63 (1.52-1.75)
Quintile 4			1.52 (1.42-1.63)
Quintile 3			1.36 (1.27-1.45)
Quintile 2			1.19 (1.11-1.27)
Least deprived			1
Random Variance (s.e. of variance)			
Individual level	0.980 (0.0046)	0.980 (0.0046)	0.980 (0.0046)
Neighbourhood level	0.140 (0.0096)	0.066 (0.0058)	0.045 (0.0045)

Appendix 4

Table Appendix 4:1
Neighbourhood Adult Smoking Stratified by NeighFrag: Odds Ratios of Smoking; 15-19 yrs Women

	all		*Stratified by NF*	
		NF2,3	***NF8,9***	***NF10***
Exposure Variables	***Model 1***	***Model 2a***	***Model 2b***	***Model 2c***
Ethnicity				
Mäori	2.35 (2.26-2.45)	2.41 (2.12-2.74)	2.46 (2.30-2.64)	2.37 (2.14-2.62)
Pacific People	0.67 (0.63-0.72)	1.00 (0.72-1.38)	0.63 (0.57-0.70)	0.69 (0.58-0.81)
NonMäori NonPacific	1	1	1	1
Education				
No qualifications	0.81 (0.77-0.85)	0.74 (0.62-0.88)	0.84 (0.76-0.92)	0.94 (0.82-1.09)
School qualifications	0.47 (0.45-0.49)	0.46 (0.39-0.54)	0.47 (0.43-0.52)	0.53 (0.47-0.60)
Post-School qualifications	1	1	1	1
Labour Force				
Employed	1	1	1	1
Unemployed	1.49 (1.42-1.56)	1.50 (1.29-1.74)	1.44 (1.32-1.56)	1.24 (1.11-1.39)
Non-labour	0.75 (0.72-0.77)	0.70 (0.62-0.79)	0.74 (0.69-0.79)	0.66 (0.60-0.72)
NZDep (Quintiles)				
Least deprived	1	1	1	1
Quintile 2	1.16 (1.07-1.26)	1.08 (0.92-1.26)	1.07 (0.90-1.27)	1.06 (0.73-1.55)
Quintile 3	1.28 (1.17-1.39)	1.10 (0.89-1.37)	1.12 (0.94-1.35)	1.21 (0.84-1.74)
Quintile 4	1.41 (1.28-1.55)	1.34 (1.00-1.79)	1.17 (0.96-1.43)	1.16 (0.82-1.64)
Most deprived	1.45 (1.29-1.62)	1.34 (0.91-1.96)	1.18 (0.93-1.50)	1.10 (0.75-1.60)
Neighbourhood Adult Smoking (NAS)				
comparing most to least NAS	2.05 (1.79-2.34)	2.06 (1.42-3.00)	2.21 (1.70-2.87)	3.42 (2.36-4.97)
Random Variance (s.e. of variance)				
Individual level	0.980 (0.0046)	0.980 (0.0137)	0.980 (0.0084)	0.980 (0.0110)
Neighbourhood level	0.057 (0.0059)	0.022 (0.0154)	0.047 (0.0097)	0.110 (0.0241)

Adjusted for individual education, ethnicity, labour force status, neighbourhood deprivation. 95% confidence intervals.

Table Appendix 4:2
Neighbourhood Adult Smoking Stratified by NeighFrag: Odds Ratios of Smoking; 15-19 yrs Men

	all	*stratified by NF*		
		NF2,3	***NF8,9***	***NF10***
Exposure Variables	***Model 1***	***Model 2a***	***Model 2b***	***Model 2c***
Ethnicity				
Mäori	1.55 (1.48-1.62)	1.64 (1.44-1.87)	1.71 (1.58-1.84)	1.39 (1.23-1.58)
Pacific People	0.83 (0.78-0.90)	1.19 (0.87-1.62)	0.82 (0.74-0.91)	0.84 (0.71-0.99)
NonMäori NonPacific	1	1	1	1
Education				
No qualifications	0.92 (0.86-0.97)	0.92 (0.77-1.09)	0.88 (0.80-0.98)	1.07 (0.91-1.26)
School qualifications	0.41 (0.39-0.44)	0.41 (0.35-0.49)	0.38 (0.34-0.42)	0.51 (0.44-0.59)
Post-School qualifications	1	1	1	1
Labour Force				
Employed	1	1	1	1
Unemployed	1.45 (1.38-1.52)	1.48 (1.27-1.72)	1.38 (1.27-1.51)	1.31 (1.15-1.49)
Non-labour	0.55 (0.53-0.58)	0.53 (0.47-0.60)	0.54 (0.50-0.58)	0.54 (0.49-0.60)
NZDep (Quintiles)				
Least deprived	1	1	1	1
Quintile 2	1.15 (1.06-1.24)	1.20 (1.00-1.45)	0.91 (0.76-1.09)	1.08 (0.77-1.52)
Quintile 3	1.26 (1.15-1.37)	1.27 (0.98-1.64)	1.01 (0.83-1.22)	1.34 (0.97-1.85)
Quintile 4	1.35 (1.23-1.49)	1.25 (0.88-1.78)	0.98 (0.79-1.21)	1.32 (0.97-1.79)
Most deprived	1.38 (1.23-1.54)	1.10 (0.68-1.76)	0.87 (0.68-1.11)	1.42 (1.02-1.97)
Neighbourhood Adult Smoking (NAS)				
comparing most to least NAS	1.50 (1.31-1.72)	1.82 (1.17-2.83)	1.92 (1.45-2.55)	2.36 (1.70-3.28)
Random Variance (s.e. of variance)				
Individual level	0.980 (0.0047)	0.980 (0.0133)	0.980 (0.0085)	0.980 (0.0121)
Neighbourhood level	0.054 (0.0062)	0.080 (0.0221)	0.056 (0.0110)	0.066 (0.0187)

Adjusted for individual education, ethnicity, labour force status, neighbourhood deprivation. 95% confidence intervals.

Table Appendix 4:3
Neighbourhood Adult Smoking Stratified by NeighFrag: Odds Ratios of Smoking; 20-24 yrs Women

	all		*Stratified by NF*	
		NF2,3	***NF8,9***	***NF10***
Exposure Variables	***Model 1***	***Model 2a***	***Model 2b***	***Model 2c***
Ethnicity				
Mäori	2.11 (2.04-2.20)	1.97 (1.72-2.24)	2.24 (2.10-2.39)	1.94 (1.78-2.12)
Pacific People	0.85 (0.81-0.91)	0.87 (0.64-1.18)	0.81 (0.75-0.89)	0.96 (0.85-1.09)
NonMäori NonPacific	1	1	1	1
Education				
No qualifications	3.36 (3.22-3.51)	3.72 (3.23-4.29)	3.06 (2.84-3.29)	3.61 (3.23-4.04)
School qualifications	1.38 (1.34-1.43)	1.45 (1.31-1.61)	1.35 (1.28-1.43)	1.42 (1.33-1.51)
Post-School qualifications	1	1	1	1
Labour Force				
Employed	1	1	1	1
Unemployed	1.41 (1.34-1.48)	1.41 (1.19-1.68)	1.40 (1.28-1.52)	1.09 (0.98-1.20)
Non-labour	1.03 (1.00-1.07)	1.19 (1.06-1.34)	1.07 (1.01-1.13)	0.75 (0.69-0.81)
NZDep (Quintiles)				
Least deprived	1	1	1	1
Quintile 2	1.06 (0.99-1.14)	1.24 (1.07-1.43)	1.13 (0.97-1.30)	0.97 (0.76-1.23)
Quintile 3	1.06 (0.98-1.14)	1.12 (0.93-1.36)	1.38 (1.18-1.62)	0.95 (0.76-1.19)
Quintile 4	1.02 (0.94-1.11)	1.12 (0.86-1.46)	1.37 (1.15-1.63)	0.92 (0.74-1.14)
Most deprived	0.89 (0.80-0.98)	0.82 (0.57-1.17)	1.28 (1.04-1.57)	0.80 (0.63-1.02)
Neighbourhood Adult Smoking (NAS)				
comparing most to least NAS	2.69 (2.39-3.03)	2.31 (1.66-3.23)	2.05 (1.63-2.59)	3.48 (2.72-4.47)
Random Variance (s.e. of variance)				
Individual level	0.980 (0.0044)	0.990 (0.0149)	0.990 (0.0077)	0.980 (0.0091)
Neighbourhood level	0.049 (0.0048)	0.020 (0.0120)	0.044 (0.0078)	0.043 (0.0099)

Adjusted for individual education, ethnicity, labour force status, neighbourhood deprivation. 95% confidence intervals.

Table Appendix 4:4
Neighbourhood Adult Smoking stratified by NeighFrag: Odds Ratios of smoking; 20-24 yrs Men

	All	Stratified by NF		
		NF2,3	***NF8,9***	***NF10***
Exposure Variables	***Model 1***	***Model 2a***	***Model 2b***	***Model 2c***
Ethnicity				
Mäori	1.37 (1.31-1.42)	1.38 (1.21-1.58)	1.39 (1.30-1.49)	1.32 (1.21-1.45)
Pacific People	0.99 (0.93-1.06)	1.05 (0.79-1.40)	1.01 (0.92-1.10)	0.98 (0.86-1.12)
NonMäori NonPacific	1	1	1	1
Education				
No qualifications	3.35 (3.21-3.50)	3.39 (2.97-3.86)	3.25 (3.02-3.50)	3.75 (3.37-4.18)
School qualifications	1.27 (1.22-1.31)	1.34 (1.19-1.49)	1.23 (1.16-1.31)	1.36 (1.27-1.46)
Post-School qualifications	1	1	1	1
Labour Force				
Employed	1	1	1	1
Unemployed	1.35 (1.28-1.41)	1.38 (1.17-1.63)	1.33 (1.22-1.44)	1.38 (1.25-1.52)
Non-labour	0.81 (0.78-0.84)	0.79 (0.67-0.92)	0.85 (0.79-0.92)	0.74 (0.69-0.81)
NZDep (Quintiles)				
Least deprived	1	1	1	1
Quintile 2	1.07 (1.00-1.15)	1.16 (1.00-1.34)	1.01 (0.89-1.16)	1.02 (0.79-1.32)
Quintile 3	1.09 (1.02-1.17)	1.18 (0.96-1.44)	1.15 (0.99-1.33)	1.03 (0.81-1.30)
Quintile 4	1.10 (1.02-1.20)	1.25 (0.95-1.66)	1.17 (1.00-1.37)	1.00 (0.80-1.25)
Most deprived	1.04 (0.95-1.15)	1.29 (0.89-1.88)	1.12 (0.92-1.35)	0.85 (0.66-1.09)
Neighbourhood Adult Smoking (NAS)				
comparing most to least NAS	1.85 (1.65-2.06)	1.28 (0.91-1.82)	1.64 (1.32-2.03)	2.75 (2.10-3.59)
Random variance (s.e. of variance)				
Individual level	0.980 (0.0046)	0.990 (0.0148)	0.990 (0.0082)	0.990 (0.0095)
Neighbourhood level	0.035 (0.0041)	0.024 (0.0136)	0.026 (0.0066)	0.052 (0.0119)

Adjusted for individual education, ethnicity, labour force status, neighbourhood deprivation. 95% confidence intervals.