

Appendix C: Sensitivity Analyses of Univariate Results

1 Small area deprivation

The emphasis in this report was on individual-level socioeconomic factors. However, analyses of the association of NZDep91 (a small area measure of socioeconomic position) with mortality provided an invaluable tool for assessing the likely impact of:

- selection bias (the majority of the full cohort had a NZDep91 score)
- misclassification bias of the mortality outcome (NZDep91 scores were available for 90% of the eligible mortality records)
- and health selection effects. (No drift health selection was expected for small area deprivation. Thus, the analyses by NZDep91 set a comparative baseline for later analyses by income where drift health selection was possible.)

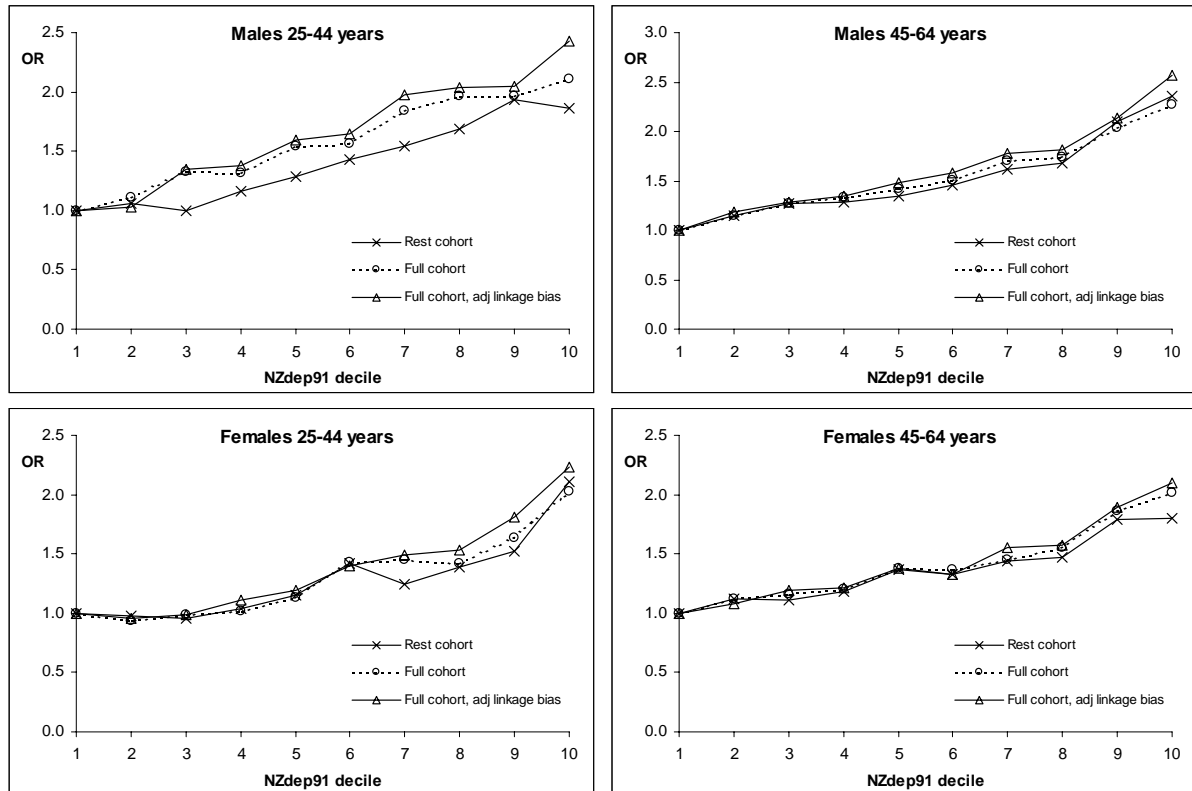
The rationale for these sensitivity analyses was outlined in Section 4.2.2 of Chapter 2: Methods.

1.1 Selection bias

The majority of analyses in this report were conducted upon the restricted cohort. The results from the NZCMS will be generalised to the total New Zealand population – essentially all those people completing the census. Therefore, all that was required to test for selection bias was to compare the association of socioeconomic position with mortality for the 25–64 year olds in the full cohort with that for the remaining 79.5% that remain in the restricted cohort. The reason for conducting these sensitivity analyses of selection bias was *not* primarily to derive the ‘true’ association of small area deprivation with mortality – that could be done more directly by simply reporting the results for the full census cohort. Rather, the reason was to estimate the magnitude of selection bias for other socioeconomic factors for which there were missing data, most notably equivalised household income.

Of the full census cohort aged 25–64 years (n=1,654,314), 99.0% had an assigned NZDep91 score (n=1,637,523). Of the restricted census cohort (n=1,315,932), 99.9% had an assigned NZDep91 score (n=1,314,852). Thus, the ‘full’ and ‘restricted’ cohort results for NZDep91 are highly representative of the true (and slightly larger) full and restricted cohorts.

Figure 27: Comparison of all-cause mortality gradients by NZDep91 deciles between: a) the restricted cohort, b) the full cohort (ie, adjusting for selection bias), and c) the full cohort adjusted for linkage bias



1.1.1 All-cause mortality

Three different all-cause mortality gradients by decile of NZDep91 are presented in Figure 27, for each of the four sex by age groups:

- the gradient among the restricted cohort (lines marked with crosses)
- the gradient among the full cohort (open circles)
- the gradient among the full cohort adjusted for linkage bias (open triangles).

The latter gradient will be considered in the following section on sensitivity analysis for linkage bias. The former two gradients allow an assessment of selection bias – the object of this section.

The restricted and full cohort line graphs for each of the four by sex age groups in Figure 27 demonstrated high concordance for 45–64 year old males, suggesting no substantial selection bias. For 45–64 year old females, the lines only notably diverged at decile 10, where the restricted cohort underestimated the full cohort odds ratio. A likely reason for the variation occurring only among the most deprived deciles is that 70.5% of the full cohort living in decile 10 remained in the restricted cohort, compared to 84.8% of those living in decile 1. Thus, there was a greater possibility of selection bias among the most deprived deciles. Among 25–44 year olds, the lines were unstable with a tendency for the restricted cohort to underestimate the full cohort gradient.

Table 54 attempts to quantify the percentage increase between the restricted and full cohort for the odds ratio comparisons of the least and most deprived deciles. The percentage increase was calculated by first determining the odds ratios for [decile 10 compared to (c.f.) decile 1], [decile 10 c.f. decile 2], [decile 9 c.f. decile 1], and [decile 9 c.f. decile 2], among the restricted and full cohort (and the full cohort adjusted for linkage bias to be described in the subsequent section). The reason for not just reporting the change in the decile 10 compared to decile 1 odds ratio was that it was unstable – particularly with decile 10 often shifting quite markedly. For each of these four odds ratios, in each of the four sex by age groups, the percentage increase in the *excess* odds ratio from the restricted to full cohort was calculated. (As a relative risk or odds ratio of 1.0 is a null finding, the actual relative effect size is given by the relative risk minus 1.0, the so-called ‘excess relative risk’ or, here, the ‘excess odds ratio’ (Rothman and Greenland 1998). For example, a change in the odds ratio from 2.0 to 2.1 corresponds to a 10% increase in the excess odds ratio.) Shown in the first row of Table 54 is the average of these four percentage increases in the excess odds ratio for each sex by age group. The results in the first row of Table 54 suggest that the gradient increased when moving from the restricted to the full cohort by 11% for 25–44 year old males and decreased by 7% for 45–64 year old males. Among females the gradient increased by 12% and 19% for 25–44 and 45–64 year olds. Put another way, these percentage increases correspond with 10%, -8%, 11%, and 16% *underestimates* of the ‘true’ mortality difference due to selection bias for analyses based on the restricted cohort, respectively (eg, for 45–64 year old females 16% = $100 \times (1 - [1/1.19])$).

Table 54: Percentage increase in the average excess odds ratio[†] for: a) adjusting for selection bias, b) adjusting for linkage bias, and c) adjusting for both selection and linkage bias

Cohort and adjustment comparison	Males		Females	
	25–44 years	45–64 years	25–44 years	45–64 years
a) Restricted cohort to full cohort (ie, adjusting for selection bias)	11%	-7%	12%	19%
b) Full cohort to full cohort adjusted for linkage bias (ie, adjusting for linkage bias)	29%	14%	19%	11%
c) Restricted to full cohort adjusted for linkage bias (ie, both selection and linkage bias)	45%	6%	34%	33%

[†] The percentage increase is the average increase for four excess odds ratios – decile 10 compared to decile 1; decile 10 compared to decile 2; decile 9 compared to decile 1; d decile 9 compared to decile 2. As such, it approximates the percentage increase for the quintile 5 compared to quintile 1 comparison.

Box 6: Summarising the effect of selection bias on all-cause mortality gradients by NZDep91

- There was little selection bias across the majority of NZDep91 deciles (Figure 27), for each of the four sex by age groups.
- There was some selection bias affecting the odds ratio comparisons of the most deprived to the least deprived deciles (particularly the decile 10 to decile 1 comparison). Thus, analyses on the restricted cohort *overestimated* the full cohort difference between the most and least deprived by 8% among 45–64 year old males and *underestimated* it by between 10% and 16% for the three remaining sex by age groups.

1.1.2 Cause-specific mortality

Table 55 presents further sensitivity analyses of possible selection bias by quintile of small area deprivation for four broad causes of death (cancer, cardiovascular disease, unintentional injury, and suicide), for 25–64 year olds combined. There was no substantial or consistent selection bias among males for cancer, cardiovascular disease and suicide deaths when considering all the quintiles of deprivation. For male unintentional injury deaths the odds ratio comparing quintile 5 to 1 was 1.67 for the restricted cohort, but only 1.35 for the full cohort – a percentage decrease of 48% in the excess odds ratio. Put another way, the restricted cohort overestimated the injury excess odds ratio by 92% due to selection bias (ie, $100 \times [1.67 - 1.35]/[1.35 - 1.0]$). However, the relative comparisons of the middle three deprivation deciles were not different between the full and restricted cohort. Thus, the apparent selection bias arose due to relative shifts in the mortality risk among the least and most deprived quintiles only. Breaking male unintentional injury into road traffic crash (RTC) and non-RTC deaths (results not presented) demonstrated that two changes between the full and restricted cohort were driving this selection bias:

- compared to the full cohort, the restricted cohort notably underestimated the RTC mortality risk among quintile 1
- compared to the full cohort, the restricted cohort notably overestimated the non-RTC mortality risk among quintile 5.

Among females, the cancer mortality gradient was underestimated for three out of the four non-reference quintile odds ratios in the restricted cohort relative to the full cohort (Table 55). However, the cancer association was not particularly strong in the first place, being an odds ratio of 1.43 and 1.28 for quintile 5 compared to quintile 1 among the full and restricted cohort, respectively. For cardiovascular disease, each of the restricted cohort odds ratios *overestimated* the full cohort odds ratio – but this was entirely due to a change in the relative position of the quintile 1 risk. Thus, there was little selection bias affecting the female cardiovascular disease gradient considered in its entirety. The results for female unintentional injury and suicide should be treated with caution due to smaller numbers, but suggest no substantive selection bias.

Table 55: Comparison of cause-specific odds ratios of mortality by small area deprivation for the restricted cohort versus the full census cohort, ages 25–64 years combined – a test of possible selection bias

	Cohort	Odds ratio (reference group quintile 1)					% change Quintile 5 OR†
		Quintile 1	Quintile 2	Quintile 3	Quintile 4	Quintile 5	
Males							
Cancer	Restricted	1.00	1.03	1.16	1.21	1.51	-7%
	Full	1.00	1.08	1.21	1.28	1.47	
CVD	Restricted	1.00	1.24	1.27	1.72	2.28	-6%
	Full	1.00	1.27	1.33	1.74	2.21	
Injury	Restricted	1.00	1.16	1.48	1.54	1.67	-48%
	Full	1.00	1.04	1.25	1.36	1.35	
Suicide	Restricted	1.00	1.41	1.01	1.71	2.04	-7%
	Full	1.00	1.45	1.20	1.80	1.97	
Females							
Cancer	Restricted	1.00	1.01	1.17	1.16	1.28	50%
	Full	1.00	1.05	1.17	1.24	1.43	
CVD	Restricted	1.00	1.44	1.65	1.86	2.69	-8%
	Full	1.00	1.34	1.58	1.80	2.55	
Injury	Restricted	1.00*	1.43*	1.69	1.19*	2.26	23%
	Full	1.00*	1.31*	1.71	1.38*	2.55	
Suicide	Restricted	1.00*	1.20*	1.48*	1.48*	2.07*	-6%
	Full	1.00*	1.18*	1.60*	1.31*	2.01*	

Note: Calculations are based on data random rounded to a multiple of three.

† The percentage change is that for the excess odds ratio, ie, the percentage change for [OR minus 1.0].

* Less than 30 deaths in the cell.

Box 7: Summarising the effect of selection bias on cause-specific mortality gradients by NZDep91

- There was notable selection bias for male unintentional injury deaths, such that analyses on the restricted cohort *overestimated* the NZDep91 comparison of the most to least deprived quintiles by 92%.
- There was some selection bias for female cancer deaths, such that analyses on the restricted cohort tended to *underestimate* the NZDep91 gradient by about a third – but as this cancer gradient was small in the first place, the selection bias was not particularly consequential.
- There was no evidence of substantial selection bias for the other broad causes of death by sex.

As with all-cause mortality, the implication of these sensitivity analyses for analyses of cause-specific mortality gradients by other socioeconomic factors (eg, household income) will be presented following sensitivity analyses of selection bias for education (Section 2.1 of this Appendix).

1.2 Linkage bias

1.2.1 All-cause mortality

As documented in Chapter 3 there was some linkage bias by NZDep91 and occupational class, such that inequalities in mortality by socioeconomic factors were *underestimated* due to linkage bias – particularly for the lowest compared to highest socioeconomic groups. Also plotted in Figure 27 are the line graphs for the all-cause mortality gradient among the full cohort adjusted for linkage bias (open triangles), using the linkage bias results from Chapter 3 (Table 20). The shift in this line graph compared to the full cohort line graph (open circles) represents the effect of linkage bias, and compared to the restricted cohort line (crosses) represents the net effect of both selection and linkage bias acting on the restricted cohort. Looking at the three line graphs Figure 27 for each of the four sex by age groups, first note that the *line graphs are generally similar*. However, if expressed in excess risk (or odds) ratio terms and for the extreme comparisons of the most deprived with the least deprived, then the linkage bias (and net effect of linkage and selection biases) becomes more than just trivial. The middle row of Table 54 presents the percentage increase from the full cohort to the full cohort adjusted for linkage bias for the excess odds ratios of deciles 9 and 10 compared to decile 1 and 2. There were 14% and 11% increases for 45–64 year old males and females, respectively, and 29% and 19% increases for 25–44 year old males and females. Considering the net impact of selection bias and linkage bias, the final row of Table 54 demonstrates that they largely offset each other for 45–64 year old males, but compounded each other for the three other sex by age groups. Regarding this latter compounding, the percentage increases was 45% for 25–44 year old males from the restricted cohort to the full cohort adjusted for linkage bias, and 34% and 33% for 25–44 and 45–64 year old females. Put another way, analyses on the restricted cohort *underestimated* the ‘true’ comparison of most and least deprived deciles due to the cumulative effect of linkage and selection biases by 31% for 25–44 year old males, and 26% and 25% for 25–44 and 45–64 year old females, respectively.

Box 8: Summarising the net impact of selection and linkage biases on all-cause mortality gradients by NZDep91

- The net effect of selection and linkage biases was relatively modest for the mid-decile comparisons, but was more notable comparing the least and most deprived deciles.
- Linkage bias and selection bias tended to off-set each other for males aged 45–64 years.
- Among females aged 25–44 and 45–64 years and males aged 25–44 years the selection and linkage biases compounded each other such that analyses on the restricted cohort *underestimated* the excess odds ratio comparisons of the most and least deprived deciles by about 25%.

1.2.2 Cause-specific mortality

As with the all-cause mortality gradients, there was linkage bias that affected the association of cause-specific mortality with small area deprivation. Using the log-linear regression results for linkage bias by small area deprivation in Table 21 the mortality odds ratios shown above in Table 28 and Table 29 for cancer, cardiovascular disease, injury and suicide were adjusted for linkage bias. Results are shown in Table 56. (Note that the results in Table 56 represent the effect of linkage bias only on the restricted cohort, not the net effect of selection and linkage biases.) There was a considerable percentage increase in the excess odds ratio for quintile 5 compared to quintile 1 for each cause of death among males. Or, put the other way, linkage bias causes an *underestimation* of the mortality gradient for all four causes of death among males. Among females, there was little linkage bias for cancer and cardiovascular disease, and there were too few deaths for a robust sensitivity analysis of injury and suicide deaths.

Table 56: Comparison of cause-specific age and ethnicity adjusted odds ratios of mortality by small area deprivation, with and without adjustment for linkage bias, for 25–64 year old males and females among the restricted cohort

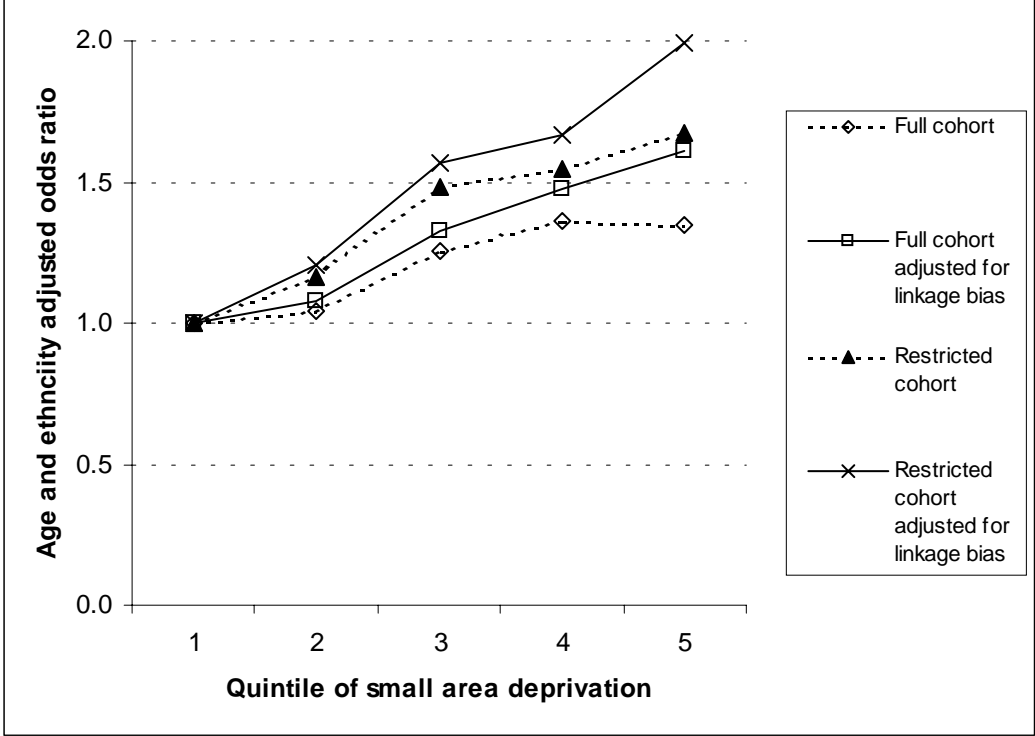
		NZDep91 quintile					% change Quintile 5 OR†
		1	2	3	4	5	
Males							
Cancer	Age/ethnicity adjusted	1.00	1.03	1.16	1.21	1.51	23%
	Plus linkage bias adjusted	1.00	1.03	1.21	1.22	1.62	
CVD	Age/ethnicity adjusted	1.00	1.24	1.27	1.72	2.28	14%
	Plus linkage bias adjusted	1.00	1.25	1.33	1.79	2.46	
Injury	Age/ethnicity adjusted	1.00	1.16	1.48	1.54	1.67	48%
	Plus linkage bias adjusted	1.00	1.20	1.56	1.67	1.99	
Suicide	Age/ethnicity adjusted	1.00	1.41	1.01	1.71	2.04	37%
	Plus linkage bias adjusted	1.00	1.43	1.37	1.88	2.43	
Females							
Cancer	Age/ethnicity adjusted	1.00	1.01	1.17	1.16	1.28	4%
	Plus linkage bias adjusted	1.00	1.02	1.18	1.19	1.30	
CVD	Age/ethnicity adjusted	1.00	1.44	1.65	1.86	2.69	11%
	Plus linkage bias adjusted	1.00	1.46	1.66	2.01	2.87	

Note: Age and ethnicity adjusted odds ratios are taken from Tables 21 and 22. The odds ratios further adjusted for linkage bias are calculated by dividing the age and ethnicity odds ratios by the risk ratio for the linkage bias by NZDep91 shown in Table 14.

† The percentage change is that for the excess odds ratio, ie, the percentage change for [OR minus 1.0].

A substantial selection bias was described for male unintentional injury deaths in the previous section such that the restricted cohort *overestimated* the gradient compared to the full cohort. In Table 56, however, a substantial linkage bias is shown such that the restricted cohort *underestimated* the gradient that would have been observed if there was no selection bias. Thus, these two biases off-set each other as shown in Figure 28. The lower line (open diamonds) represents the odds ratios among the full cohort, ie, unaffected by selection bias. The next line-up (square boxes) represents the full cohort odds ratios adjusted for linkage bias (ie, adjusting for both linkage bias and selection bias simultaneously). Note that this second line is similar to the restricted cohort line (solid triangles). Thus, the simple age and ethnicity adjusted odds ratios among the restricted cohort just so happen to be similar to the odds ratios that would be observed after adjusting for both selection and linkage biases. The top line (crosses) shows the restricted cohort odds ratios adjusted for linkage bias (but not for selection bias) – it overestimates the association of small area deprivation with unintentional injury mortality.

Figure 28: Net effect of adjusting for both selection bias and linkage bias for 25–64 year old male unintentional injury deaths by NZDep91 quintile



For other causes of death among males, the lack of any substantive and consistent selection bias described above meant that linkage bias was not off-set. Thus, the *net* effect of selection bias (Table 55) and linkage bias (Table 56 above) for male cancer, cardiovascular disease and suicide deaths will be broadly similar to that for linkage bias alone shown in Table 56 above.

Among female cardiovascular disease deaths the net effect of selection bias (Table 56) and linkage bias (Table 56 above) was negligible. However, in excess odds ratio terms, the cancer mortality gradient underestimate of about a third due to selection bias was not offset by any linkage bias – but the association of NZDep91 and cancer among females was modest to start with.

Box 9: Summarising the net impact of selection and linkage biases on cause-specific mortality gradients by NZDep91

For analyses on the restricted cohort for *males* aged 25–64 years:

- Linkage and selection biases off-set each other for *injury* deaths in the restricted cohort
- For other causes of death the lack of selection bias meant that the net impact of the two biases was simply approximated by the linkage bias, such that analyses on the restricted cohort *underestimated* the 'true' gradient by:
 - approximately 10% for *cancer* and *cardiovascular disease* deaths
 - approximately 25% for *suicide* deaths.

For analyses on the restricted cohort for *females* aged 25–64 years:

- The selection bias for female *cancer* deaths was neither compounded nor off-set by any linkage bias, such that the net effect was an approximately 30% *underestimate* of the 'true' gradient for analyses on the restricted cohort – but the cancer gradient was modest to start with.
- The net effect of linkage and selection biases for *cardiovascular disease* was negligible.
- The net effect for female *suicide* and *injury* deaths was unable to be robustly determined, but presumably the gradients by non-cancer and non-cardiovascular disease on average tended to be *underestimated* given the approximately 25% underestimate of the all-cause mortality described above in Box 9.

1.3 Health selection

1.3.1 Observed mortality risk over time

All-cause mortality, all labour force categories

Figure 29 below shows the observed mortality risk by six-month period over the three-year follow-up, by quintile of small area deprivation. For 45–64 year old males and females, the plots are roughly parallel. Among 25–44 year olds, the plots are unstable due to small numbers. These parallel lines are consistent with the theoretical expectation of no drift health selection for small area deprivation, and acts as a baseline for mortality plots by income presented later in this report.

Cancer and cardiovascular disease deaths among 45–64 year olds: all labour force categories, and excluding non-active labour force

It is likely that health selection, if acting, varies by cause of death. There were enough deaths by broad level of socioeconomic factor for cancer deaths among 45–64 year olds (both sexes) and cardiovascular disease among males 45–64 year olds to plot mortality risks by six-month period for all labour force categories *and* excluding the non-active labour force.

Figure 29: Mortality risk by six-month period following census night by quintile of small area deprivation, full cohort and all labour force categories

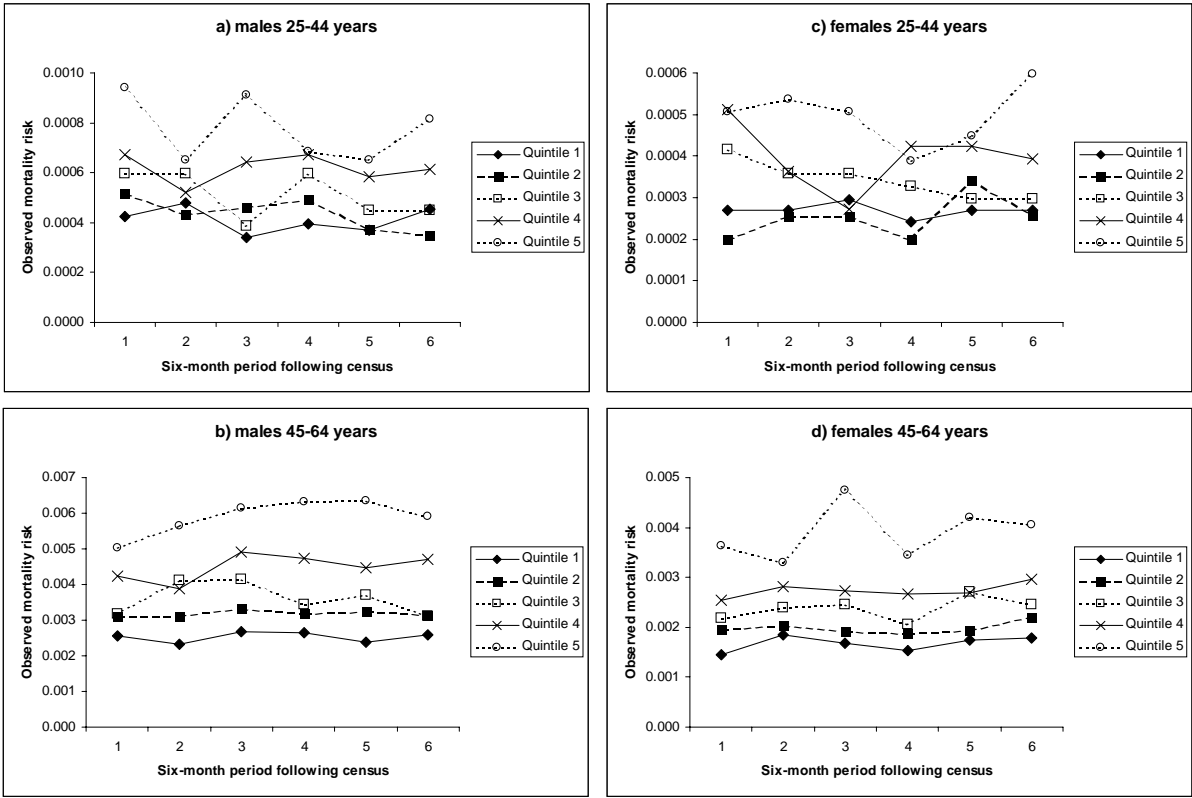
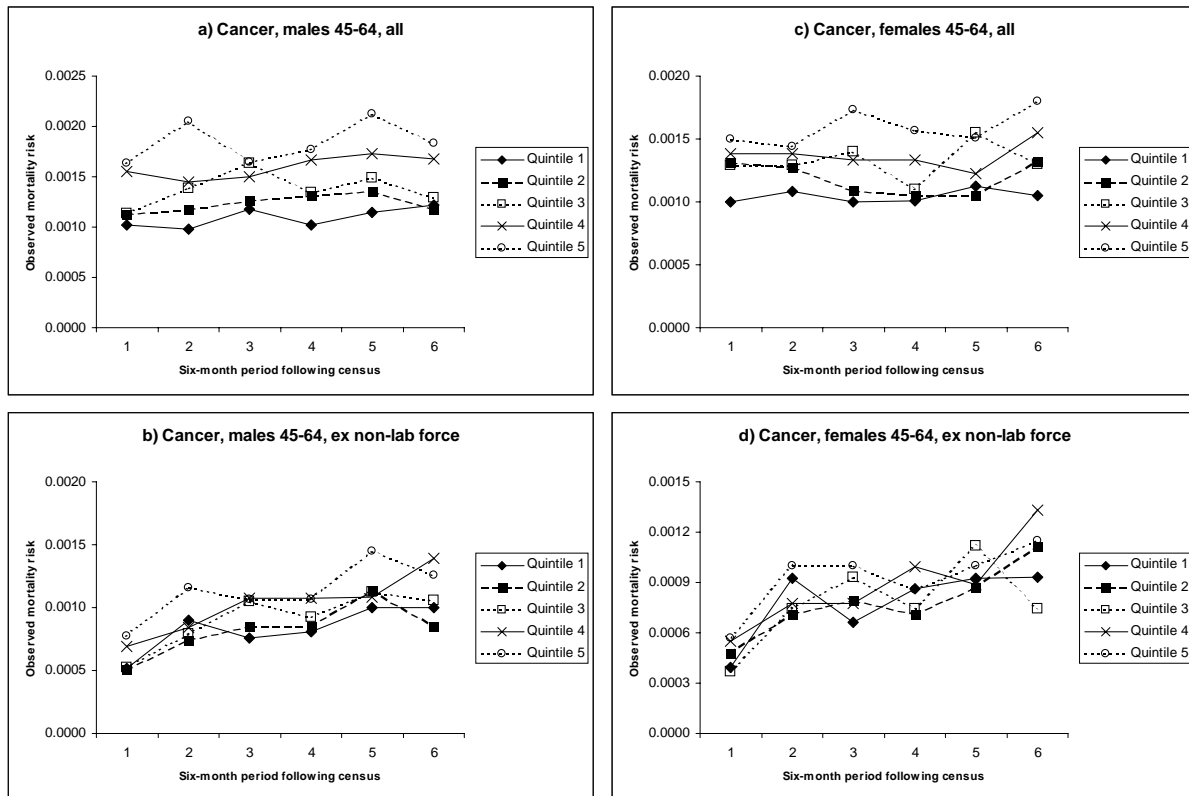


Figure 30 (below) shows the cancer mortality risks over time among 45–64 year olds, both for all labour force categories and excluding the non-active labour force (ie, including only the employed and unemployed). For both males and females the mortality risk lines are approximately horizontal and parallel for all labour force categories combined (ie, Figures a and c). Thus, there is no evidence of *drift* health selection, as would be expected given that one’s usual residence (and hence assigned decile of small area deprivation) is probably not substantively influenced by recent poor health. As with the all-cause mortality plots by NZDep91 in Figure 29, Figure 30a and Figure 30c serve as benchmarks for the later plots by income where drift health selection is theoretically plausible.

Figure 30: Mortality risk by six-month period following census night by small area deprivation, for 45–64 year old cancer deaths, for all labour force categories and excluding the non-active labour force



Excluding the non-active labour force, all mortality risk lines by quintile of NZDep91 are sloping upwards (Figure 30b and Figure 30d). Thus, people who die of cancer soon after census night are less likely to be in the active labour force on census night than those that die of cancer up to three years after census night. This is not surprising given that death from cancer is often preceded by a period of deteriorating health. If there was health selection out of the active labour force that was *differential* by level of deprivation (ie, if you had cancer, you were more likely to exit the active work force if you lived in a more deprived small area than if you lived in a less deprived area), then we would expect the mortality risk line for NZDep91 quintile 5 to be steeper than NZDep91 quintile 1. While there is imprecision about each point in Figure 30b and Figure 30d (eg, the 95% confidence intervals for each mortality risk ‘point’ were approximately plus or minus 0.0002 in Figure 30b), there was not convincing evidence that the slopes differed. (Note that this ‘test’ is only one test of differential health selection and may lack power. Similar plots by highest qualification and income presented latter in this report were suggestive of differential health selection among cancer deaths. Thus, a ‘best’ decision about differential health selection in the NZCMS requires a balanced consideration across further tests presented subsequently in this report.)

The pattern of mortality risk plots for 45–64 year old male cardiovascular disease deaths (all labour force categories) was similar to that for cancer. Cardiovascular deaths among 45–64 year olds in the active labour force were too few to allow a robust interpretation.

1.3.2 Excluding sickness beneficiaries

Included in the 1991 census data is whether each individual had been a recipient of a sickness benefit in the preceding 12 months. Excluding these people from the analyses would limit the cohort to a 'healthier' subgroup, reducing the likely effect of *drift* health selection. Assuming that the level of deprivation of where you live is not affected by (recent) poor health, excluding unhealthy people from the analysis should not alter the mortality gradient by NZDep91. However, the sickness benefit is means tested in New Zealand – among unhealthy people only those of lower incomes and socioeconomic position will actually receive a sickness benefit. Therefore, analyses excluding sickness beneficiaries will exclude a greater percentage of the unhealthy people in the deprived areas than the non-deprived areas, which theoretically should *reduce* the observed mortality gradient by stable socioeconomic factors such as NZDep91. In order to use the exclusion of sickness beneficiaries as a test of health selection effects for other socioeconomic factors such as income, we must first quantify the amount of this spurious reduction in the deprivation (and education) mortality gradient due to excluding sickness beneficiaries. For subsequent analyses of income, analyses excluding sickness beneficiaries will be suggestive of health selection effects only if the reduction in the income mortality gradient is substantially more than that observed for the NZDep91 and education mortality gradients.

Table 57 shows the age and ethnicity-adjusted odds ratios by small area deprivation for all cause mortality, for all members of the restricted cohort and following various exclusions. (The interpretation of analyses excluding pre-hospitalised deaths and the non-active labour force follows in points 1.3.3 and 1.4 respectively.) The percentage reduction for the quintile 5 compared to quintile 1 excess odds ratio after excluding sickness beneficiaries ranged between 9% and 18% for the four sex by age groups. The reductions in the mortality gradient for broad causes of death (Figures 31 and 32) were likewise modest only, although greater for male cancer and cardiovascular disease deaths compared to male injury and suicide deaths. The lack of reduction following the exclusion of sickness beneficiaries for injury deaths was probably due to only 4% of injury-decedents having been a sickness beneficiary in the preceding year compared to 10% of all deaths. Nine percent of suicide decedents had received a sickness benefit in the preceding year.

These analyses will be used as a baseline for the investigation of health selection bias affecting the household income mortality gradients.

Table 57: Odds ratios (95% CI) of all cause mortality for 25–64 year olds in the restricted cohort, by quintile of small area deprivation, for various exclusions testing for health selection

Exclusion criteria	Odds ratios (ref group Quintile 1)					% change to null of Quintile 5 OR†
	1	2	3	4	5	
Males						
<i>25–44 year olds</i>						
Nil	1.00	1.05 (0.85–1.30)	1.32 (1.07–1.62)	1.56 (1.28–1.91)	1.85 (1.50–2.27)	
Sickness beneficiaries	1.00	1.00 (0.80–1.25)	1.27 (1.02–1.57)	1.43 (1.16–1.77)	1.72 (1.39–2.14)	14%
Pre-hospitalised deaths	1.00	0.98 (0.77–1.25)	1.14 (0.90–1.45)	1.29 (1.02–1.63)	1.41 (1.11–1.80)	51%
Non-active labour force	1.00	1.06 (0.85–1.33)	1.32 (1.06–1.64)	1.45 (1.16–1.80)	1.73 (1.38–2.17)	14%
<i>45–64 year olds</i>						
Nil	1.00	1.20 (1.09–1.32)	1.31 (1.19–1.45)	1.54 (1.40–1.70)	2.07 (1.88–2.27)	
Sickness beneficiaries	1.00	1.19 (1.08–1.31)	1.29 (1.17–1.43)	1.49 (1.35–1.64)	1.90 (1.72–2.09)	16%
Pre-hospitalised deaths	1.00	1.20 (1.07–1.35)	1.25 (1.11–1.41)	1.47 (1.30–1.65)	1.81 (1.61–2.04)	24%
Non-active labour force	1.00	1.11 (0.98–1.25)	1.22 (1.07–1.39)	1.39 (1.22–1.58)	1.61 (1.40–1.84)	43%
Females						
<i>25–44 year olds</i>						
Nil	1.00	1.01 (0.78–1.31)	1.30 (1.01–1.66)	1.33 (1.03–1.71)	1.81 (1.41–2.32)	
Sickness beneficiaries	1.00	0.97 (0.74–1.27)	1.30 (1.01–1.68)	1.25 (0.96–1.63)	1.67 (1.29–2.16)	18%
Pre-hospitalised deaths	1.00	0.87 (0.60–1.27)	1.66 (1.20–2.31)	1.19 (0.83–1.72)	1.67 (1.17–2.38)	17%
Non-active labour force	1.00	1.08 (0.79–1.48)	1.44 (1.06–1.95)	1.39 (1.01–1.90)	1.55 (1.11–2.17)	32%
<i>45–64 year olds</i>						
Nil	1.00	1.08 (0.96–1.22)	1.27 (1.13–1.43)	1.37 (1.22–1.54)	1.70 (1.51–1.91)	
Sickness beneficiaries	1.00	1.08 (0.95–1.22)	1.27 (1.12–1.42)	1.34 (1.19–1.50)	1.63 (1.45–1.84)	9%
Pre-hospitalised deaths	1.00	1.01 (0.86–1.18)	1.34 (1.15–1.56)	1.28 (1.09–1.49)	1.67 (1.43–1.94)	4%
Non-active labour force	1.00	0.99 (0.82–1.20)	1.03 (0.85–1.25)	1.16 (0.95–1.42)	1.33 (1.07–1.64)	53%

Note: Injury and suicide deaths are not presented for females due to small numbers. For all remaining cells in the table there were at least 30 deaths.

† Percentage change is for the excess odds ratio for quintile 5 compared to quintile 1, compared to the same odds ratio with nil exclusions.

Figure 31: Crude risk ratios of cause-specific mortality for 25–64 year old males in the restricted cohort, by quintile of small area deprivation, for various exclusions testing for possible health selection

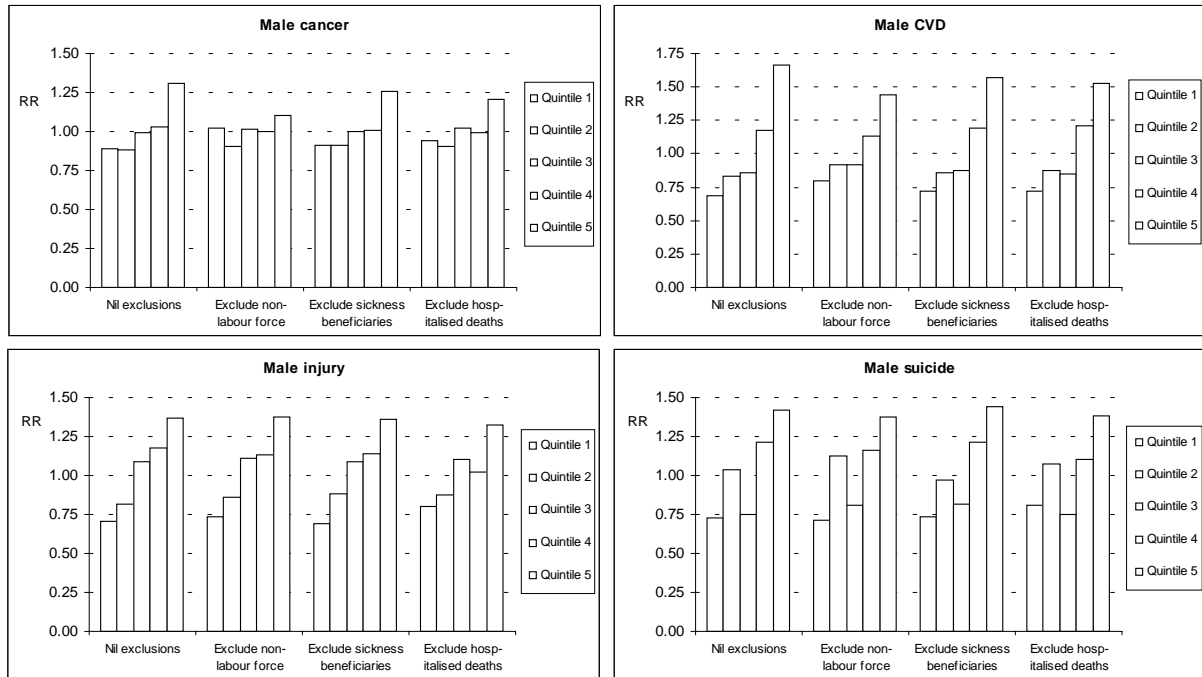
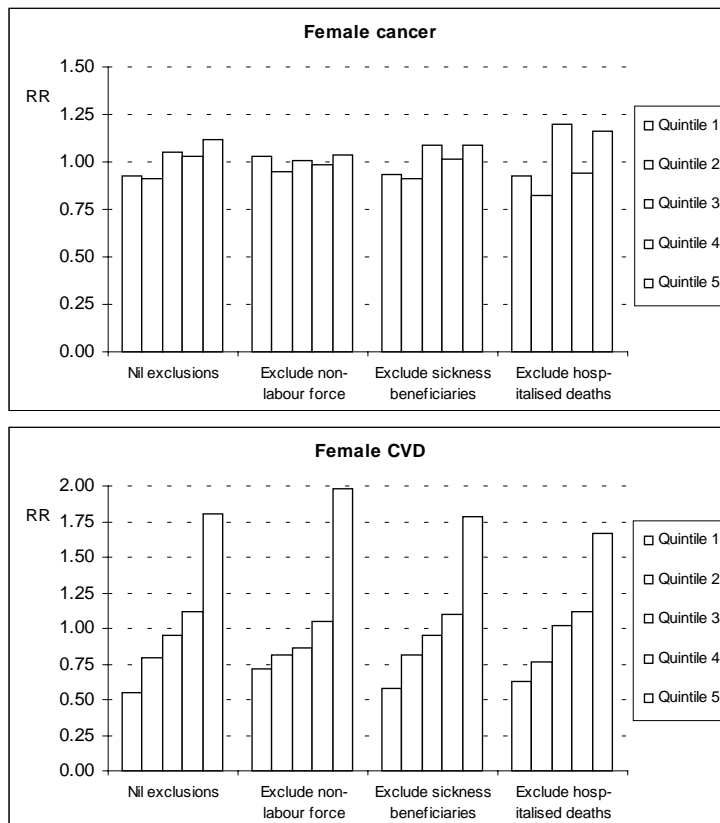


Figure 32: Risk ratios of cancer and cardiovascular mortality for 25–64 year old females in the restricted cohort, by quintile of small area deprivation, for various exclusions testing for possible health selection



1.3.3 Excluding decedents with a hospitalisation event between 1988 and census night

The baseline analyses for excluding pre-hospitalised deaths are also presented in Table 57 for all-cause mortality, and Figures 31 and 32 for broad causes of death. The percentage reductions for the quintile 5 compared to quintile 1 excess odds ratios for all-cause mortality were 51% and 24% for 25–44 and 45–64 year olds, respectively. The percentage reductions for female all-cause mortality were less – 17% and 4% respectively. However, the gradients after excluding pre-hospitalised deaths were somewhat unstable due to the large number of excluded deaths. By cause of death, the largest reduction in the deprivation mortality gradient following the exclusion of pre-hospitalised deaths tended to be for cancer deaths.

Assuming that deprivation mortality gradients are not susceptible to drift health selection, the reduction in these gradients following the exclusion of pre-hospitalised deaths was surprisingly high. For this to occur, the chance of being hospitalised prior to one's death (be it for the subsequent cause of death or an unrelated event) must be associated with deprivation independently of the association of deprivation with mortality. Whatever the reason, for exclusion of sickness beneficiaries to suggest drift health selection affecting the income–mortality gradient, the percentage reduction will have to be substantially greater than that found here for the baseline NZDep91 analysis.

1.4 Excluding the non-active labour force

Controlling socioeconomic mortality gradients for labour force status is difficult to interpret. Reasons include labour force status being a proxy for health status and variables other than health status, and being a proxy for both confounding and intermediary variables (see page 62 for previous discussion).

1.4.1 All-cause mortality

Table 57 shows the odds ratios of mortality by NZDep91 quintile for all labour force categories, and excluding the non-active labour force, using the quintile 5 to quintile 1 odds ratio, excluding the non-labour force results in a 14% and 43% reduction in the excess odds ratio for 25–44 and 45–64 year old males, respectively. Among females the comparable reductions are 32% and 53%, respectively. (Note that the 32% reduction for 25–44 year old females was only apparent for the quintile 5 to quintile 1 odds ratio – there was little change for the quintiles 2 to 4 odds ratios.) As discussed previously in this report, it has been argued in the international literature that a similar reduction for education mortality gradients following exclusion of the non-active labour force was a consequence of differential health selection. However, the mortality risk plots over time above (Section 1.3.1, page 209-210) *do not suggest* health selection out of the active labour force that is *differential* by level of deprivation for cancer deaths or cardiovascular disease deaths. It may be that the 'test' for differential health selection by plotting mortality risks over time lacks power – but it seems unlikely that the mortality risk plots over time would have failed to detect differential health selection that caused up to a 50% reduction in the NZDep91 gradient. Thus the reduction in the NZDep91 mortality gradient following the exclusion of the non-active labour force must also include components of one or more of:

- *Confounding by labour force status* by means other than short-term drift health selection. For example, personality characteristics and behaviours associated with being in the non-labour force may be associated with the relative deprivation of the neighbourhood you live in, and (independently of deprivation) associated with mortality risk.
- *Mediation by labour force status*, or factors it is a proxy for (most notably health status).
 - For example, where you live (and hence small area deprivation) may affect your employment opportunities, such that labour force status acts as an intermediary variable between deprivation and mortality risk (rather than being a confounder as in the above bullet point).
 - Regarding labour force status as a proxy for health status, apart from injury deaths poor health is usually an intermediary variable between socioeconomic factors and mortality. As poor health often results in movement into the non-active labour force, controlling for labour force status may ‘over-control’ the deprivation-mortality association. (Note that this mechanism is not health selection – poor health is assumed not to influence where you live in the short-term, and mortality risk plots (Section 1.3.1.2) did not suggest health selection out of active labour force that was differential by deprivation.)
- *Effect modification of the deprivation mortality gradient by labour force status* by mechanisms other than differential health selection, such that the gradient is weaker among the active labour force. Because of the strong association of labour force status with mortality risk, any decision about effect modification between labour force status and deprivation was difficult. Analyses (not shown) found that the *relative* deprivation gradient was weaker in the non-labour force than the active labour force, but that the *absolute* gradient was stronger. Thus, it was unclear whether there was truly interaction between labour force status and deprivation.

Putting aside the latter effect modification, one possible way to tease apart the relative contributions of confounding/mediation and differential health selection is to look at the effect of excluding the non-active labour force for specific causes of death – the subject of the next section.

1.4.2 Cause-specific mortality

Regarding the teasing apart of differential health selection from confounding or mediation, one might expect that deaths from cancer (and possibly cardiovascular disease) were vulnerable to differential health selection. Conversely, one might expect that deaths from injury were not associated with a period of poor health prior to death, and therefore not vulnerable to differential health selection. If we also make the assumptions that:

- 1 Any confounding/mediation of the association of deprivation with mortality by labour force status did not vary greatly by cause of death.
- 2 Any effect modification of the association of deprivation with mortality by mechanisms other than differential health selection did not vary greatly by cause of death.
- 3 Drift health selection does not affect the association of deprivation with mortality.

Then:

- If excluding the non-labour force resulted in *equivalent* reductions in the mortality gradients for each of cancer, cardiovascular disease, unintentional injury, and suicide deaths, this would provide evidence *against* a prominent role for differential health selection (and probably point to a major role of confounding/mediation as an explanation of reduced deprivation-mortality gradients among the active-labour force)
- If excluding the non-labour force resulted in *greater* reductions in the mortality gradients for cancer and cardiovascular disease deaths (compared to unintentional injury and suicide deaths), this would *support* a prominent role for differential health selection as a reason for the reduced gradient among the labour force.

(This test is subject to many assumptions. Perhaps the most important and questionable assumption is number 1 above – the robustness of this assumption will be returned to after presenting the ‘test’ results.)

Figure 31 (page 213) presents the mortality gradient by NZDep91 quintiles among 25–64 year old males for four broad causes of death: cancer, cardiovascular disease, unintentional injury, and suicide. Figure 32 shows the same graphs for females, but only for cancer and cardiovascular disease – there were too few unintentional injury and suicide deaths among females for a robust determination of the effect of the various exclusions. Both figures use crude data, with floating risk ratios. As the graphs do not use age and ethnicity adjusted odds ratios, the crude risk ratios may be somewhat confounded. Therefore, it is more important to look for patterns than exact differences.

For males in Figure 31, the mortality gradient is notably reduced following exclusion of the non-labour force for cancer and cardiovascular disease (percentage reductions in excess risk ratios comparing quintile 5 to 1 of 83% and 42%, respectively), but little changed for unintentional injury and suicide (percentage reductions of only 7% and 3%). While for females it was not possible to examine the change for unintentional injury and suicide deaths, the patterns in Figure 32 for cancer and cardiovascular disease are consistent with the pattern for males. The female cancer mortality gradient was essentially flattened (albeit small to start with), and the quintile 5 to quintile 1 excess risk ratio for cardiovascular disease reduced by 23%.

There are at least two important problems with this ‘test’. First, the size of the cancer mortality gradient by NZDep91 was small to start with, and a small amount of confounding by labour force status that nullified the cancer association may have only marginally reduced the stronger associations of NZDep91 with cardiovascular disease, injury and suicide. Thus, a constant amount of confounding/mediation by labour force across causes of disease may actually have produced the picture above. The interpretation all depends on whether a ‘constant’ amount of confounding causes an equivalent change in: the percentage excess odds ratio, or the absolute odds ratio.

Second, the assumption that any confounding/mediation of the deprivation-mortality association by labour force status is ‘constant’ by cause of death is questionable. For example, labour force status is strongly associated with tobacco smoking in New Zealand (Wilson 2000), and the cancer mortality gradient by deprivation was mainly due to lung cancer. Thus, the amount of confounding/mediation of the deprivation–cancer mortality association by labour force status (as a proxy for smoking in this instance) may have been greater for cancer than the other causes of disease.

Given all these reservations about this test for differential health selection, what can we conclude? At best, *these NZDep91 results (for males at least) suggest the possibility of differential health selection for cancer (and cardiovascular disease) – but no stronger conclusion can be made.* If there was differential health selection for cancer deaths (and cardiovascular disease death), then adjusting for labour force status in multivariate analyses may result in an underestimate of the independent association of small area deprivation (and by extrapolation other socioeconomic factors) with mortality. But it is not possible to specify with precision the effect of the relative contributions of differential health selection, effect modification and confounding/mediation by labour force status on the deprivation-mortality gradients. This issue will be pursued further in following sections.

2 Highest qualification

2.1 Selection bias

As with small area deprivation, most of the full cohort had a specified value for highest qualification. Similar analyses to test for a possible selection bias between the full and restricted cohort for all-cause mortality were conducted for highest qualification (see Figure 27 (page 201) for the comparable NZDep91 analysis). The analyses by highest qualification disclosed no substantial selection bias for all-cause mortality. The age and ethnicity-adjusted odds ratio for those with no qualification compared to those with a graduate/ postgraduate degree varied between the full and restricted cohort by less than 1% for each of 25–44 and 45–64 year old males, and by 3% and 7% for 25–44 and 45–64 year old females.

Table 58 below shows the results for cause-specific mortality. Perhaps, the only notable difference between the full cohort and the restricted cohort was for female cancer where the restricted cohort *overestimated* the gradient compared to the full cohort. Note that the comparable NZDep91 analysis (see Table 55, page 204) demonstrated the opposite – the restricted cohort *underestimated* the association relative to the full cohort. Also note that there was a tendency for the male injury odds ratios to be overestimated (ie, further from the null) in the restricted cohort due to selection bias, but by less than that suggested by the NZDep91 sensitivity analyses (Table 55). The results for female unintentional injury and suicide deaths must be treated with caution due to small numbers of deaths.

Table 58: Comparison of cause-specific odds ratios of mortality by highest qualification for the restricted cohort versus the full census cohort, 25–64 year olds combined – a test of possible selection bias

	Cohort	Age and ethnicity adjusted odds ratio				% change to Null tertiary OR†
		Tertiary	Trade	School	Nil	
<i>Males</i>						
Cancer	Restricted	0.74	0.79	0.84	1.00	-1%
	Full	0.74	0.78	0.81	1.00	
CVD	Restricted	0.57	0.80	0.70	1.00	-1%
	Full	0.56	0.79	0.69	1.00	
Injury	Restricted	0.51	0.75	0.79	1.00	8%
	Full	0.54	0.77	0.85	1.00	
Suicide	Restricted	0.56	0.89	1.06	1.00	-8%
	Full	0.52	0.93	1.05	1.00	
<i>Females</i>						
Cancer	Restricted	0.81	0.88	0.85	1.00	13%
	Full	0.84	0.87	0.85	1.00	
CVD	Restricted	0.49	0.54	0.74	1.00	-2%
	Full	0.48	0.52	0.75	1.00	
Injury	Restricted	0.68*	0.92*	0.83	1.00	-19%
	Full	0.62*	0.79*	0.76	1.00	
Suicide	Restricted	1.18*	0.87*	1.17*	1.00	80%
	Full	1.33	1.17*	1.34	1.00	

† The percentage change is that for the tertiary compared to nil excess odds ratio to the null, ie, the percentage change for the absolute value of [OR minus 1.0]. For example, the female cancer tertiary compared to nil OR changes from 0.81 to 0.84 – a change in the excess odds ratio of -0.19 to -0.16 (negative signs indicate preventative odds ratios). Thus the percentage reduction to the null is $0.03/0.19 \times 100 = 16\%$, or 13% if unrounded data are used.

* Less than 30 deaths in the cell.

Box 10: Summarising the effect of selection bias on mortality gradients by NZDep91 (Box 7) and highest qualification

All-cause mortality:

- There was no substantial evidence of selection bias affecting all-cause mortality gradients by highest qualification. This contrasts with the NZDep91 analyses that suggested a modest tendency for the restricted cohort to underestimate the gradient – except among 45–64 year old males where there was a slight overestimate.

Cause-specific mortality:

- The NZDep91 and highest qualification analyses were consistent in suggesting no notable selection bias affecting:
 - male cardiovascular disease, injury and suicide deaths
 - female cardiovascular deaths.
- Both NZDep91 and highest qualification analyses found that analyses on the restricted cohort overestimated the male injury gradient, particularly the NZDep91 analyses. (However, this selection bias would have probably been offset by linkage bias.)
- NZDep91 analyses suggested a selection bias that underestimated the female cancer gradient, but the highest qualification analyses were basically null (or even suggesting the reverse – an overestimation).
- Analyses for female injury and suicide deaths were not robust enough to allow interpretation.

Given the lack of a consistent pattern between the NZDep91 and education selection bias analyses (other than male injury), it seems difficult to reliably predict what the magnitude and direction of any selection bias might be for other socioeconomic factors such as income.

Figure 33: Mortality risk for each six-month period following census night by highest qualification, all labour force categories

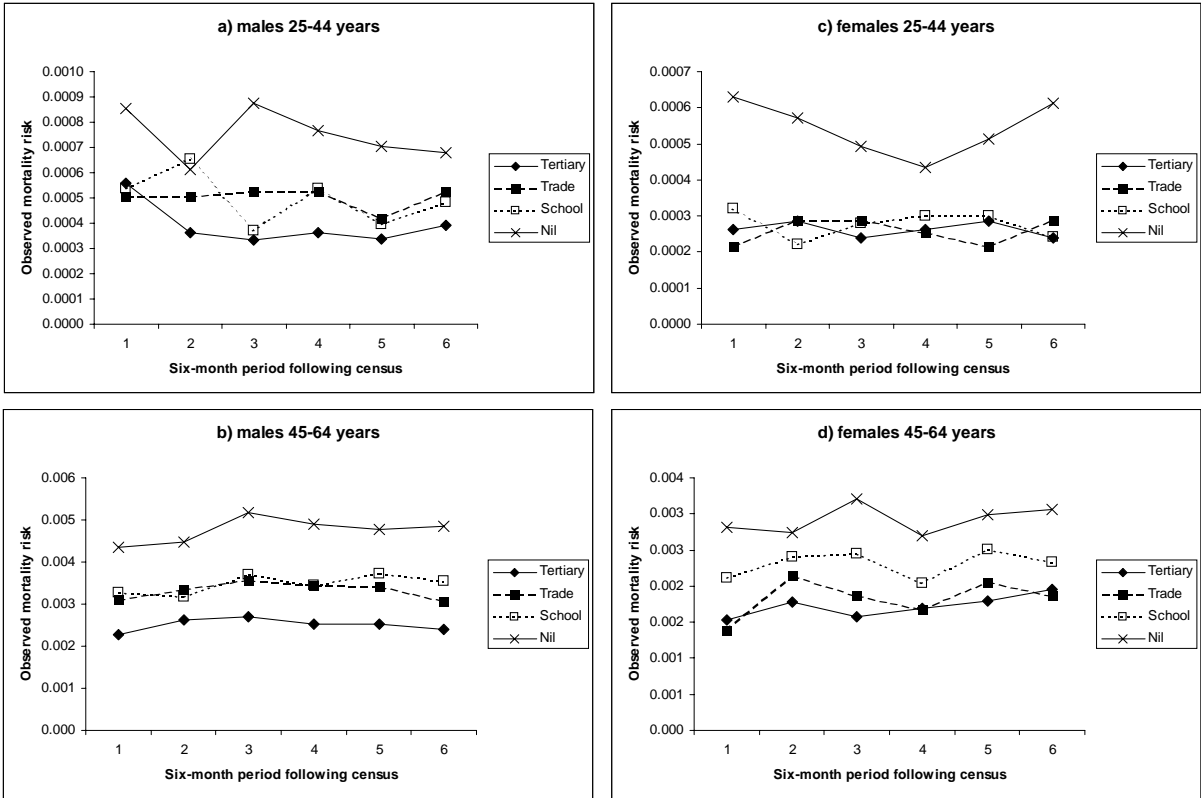
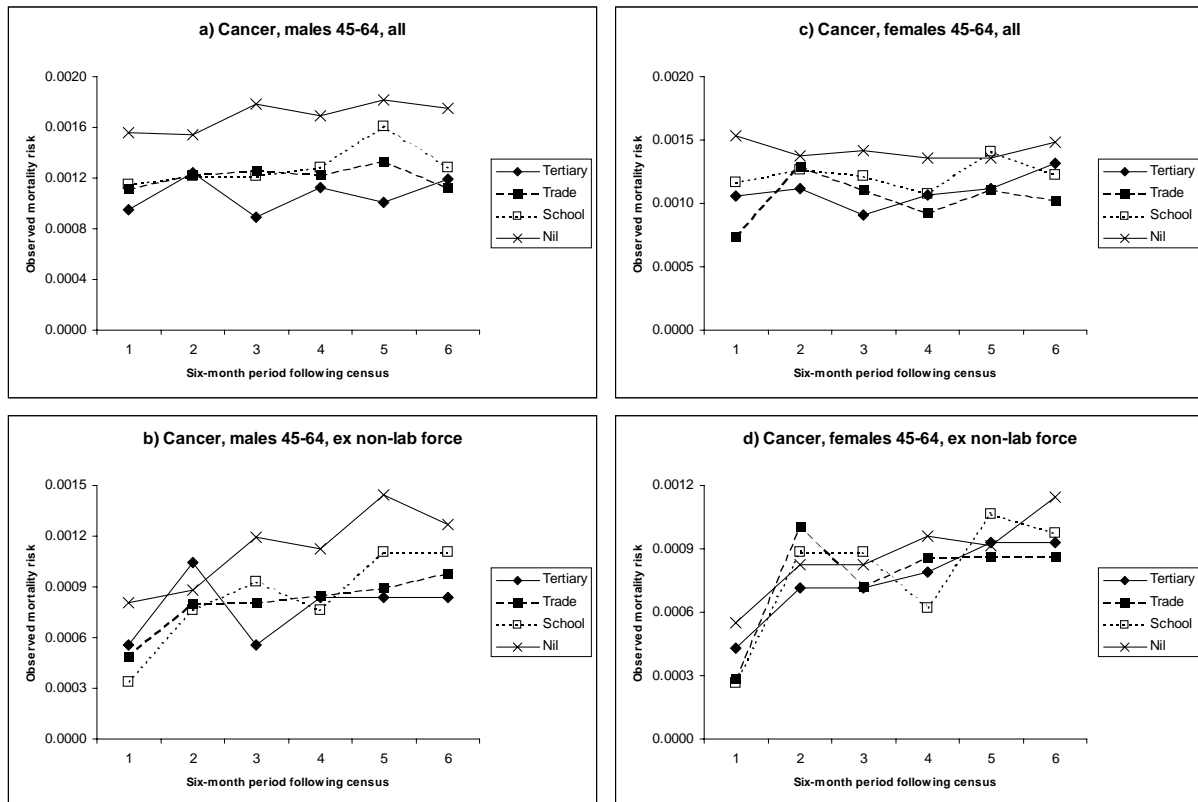


Figure 34: Cancer mortality risk for each six-month period following census night by highest qualification for 45–64 year olds, all labour force categories and excluding the non-active labour force



2.2 Health selection

2.2.1 Observed mortality risk over time

All-cause mortality, all labour force categories

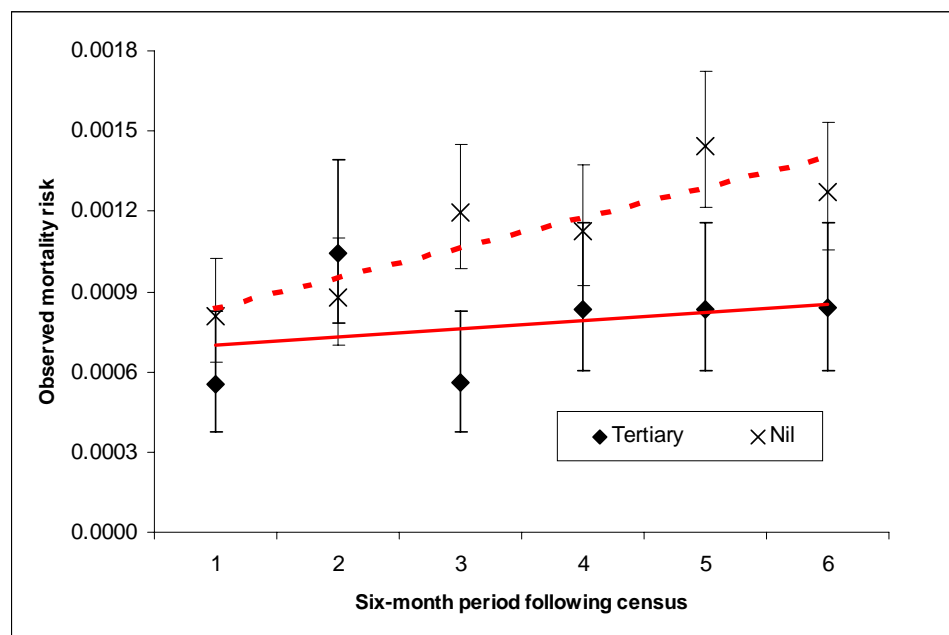
Figure 33 above shows the mortality risk for each six-month period of the three-year follow-up. For later income and labour force analyses using this same method, one would expect different trends in mortality risk by time if there were significant drift health selection effects. However, for education we expect no difference in trends between education categories, because for the vast majority of people aged 25–64 poor health is unlikely to have affected their highest qualification. Figure 33 tests this assumption. While the lines are somewhat erratic for 25–44 year olds due to few deaths, the mostly parallel plots are consistent with no drift health selection by education, and thus also consistent with those shown for small area deprivation (Figure 29, page 209).

Cancer and cardiovascular disease deaths among 45–64 year olds: all labour force categories, and excluding non-active labour force

Figure 34 (above) plots the cancer mortality risks over time for the active labour force only, for 45–64 year olds. There was some evidence of a steeper cancer mortality plot over time for those with nil education compared to those with tertiary education among males (Figure 34b). This difference is demonstrated more clearly in Figure 35, where the mortality risk plots are plotted only for these two educational groups and includes trend-lines (calculated in Microsoft Excel). If the first six-months of deaths were excluded (as they were in the cohort analyses), then the pattern of diverging slopes shown in Figure 35 becomes even more prominent. Thus, there was some suggestion of differential health selection by educational status for cancer mortality among 45–64 year old males. On the other hand:

- the cancer mortality risk plots over time were also rising for the ‘school’ and ‘trade’ groups among 45–64 year old males in Figure 34b
- there was no suggestion of differing cancer mortality risk slopes among 45–64 year old females (Figure 34d)
- there was no suggestion of differing cardiovascular mortality risk slopes by education among either males or females aged 45–64 years after excluding the non-active labour (results not shown)
- previously presented mortality risk plots by NZDep91 excluding the non-active labour force status failed to suggest differential health selection for either males and females aged 45–64 years, and for either cancer (Figures 30b, d) or cardiovascular disease.

Figure 35: Cancer mortality risk for each six-month period following census night for 45–64 year old males with tertiary or nil qualifications, excluding the non-active labour force



Note: Error bars are 95% confidence intervals about each mortality risk, calculated according to Rothman and Greenland (1998; pp.240–1).

In light of these bullet points, the apparently flat cancer mortality risk plot for the tertiary qualification group among males aged 45–64 years may just have been an aberrant result. *Thus, it is not possible to point confidently to strong evidence of differential health selection by stable socioeconomic factors (ie, NZDep91 and highest qualification) in the NZCMS.*

(Mortality risk plots by occupational class will be considered later in this appendix, and do suggest some differential health selection – but only in the first six or 12 months.)

2.2.2 Exclusion of sickness beneficiaries

The exclusion of decedents who were sickness beneficiaries caused the tertiary qualification compared to nil qualification excess odds ratio for all-cause mortality to decrease by up to 11% across the four sex by age groups (Table 59 below). The similar percentage reductions for the NZDep91 quintile 5 compared to quintile 1 odds ratio were somewhat greater at 9% to 18% (Table 57, page 212). If *drift* health selection was notably biasing the all-cause mortality gradients by household income in the NZCMS, then excluding sickness beneficiaries should produce a substantially greater reduction in the all-cause mortality gradients for income (presented later in report).

2.2.3 Exclusion of decedents with a hospitalisation event between 1988 and census night

The exclusion of decedents hospitalised between 1988 and census night reduced to the null the tertiary qualification compared to nil qualification excess odds ratio for all-cause mortality by 4% to 21%, across the four sex by age groups (Table 59). The similar percentage reductions for the NZDep91 quintile 5 compared to quintile 1 odds ratio ranged from 4% to 51% (Table 57). As with the sickness beneficiary exclusion above, if *drift* health selection was notably biasing the all-cause mortality gradients by household income in the NZCMS then the later sensitivity analyses for income excluding pre-hospitalised deaths should produce a substantially greater reduction in the all-cause mortality gradients.

Table 59: Odds ratios (95% CI) of all-cause mortality for 25–64 year olds in the restricted cohort, by highest qualification, for various exclusions testing for health selection

Exclusion criteria	Odds ratios (ref group = nil qualifications)				% change to null of tertiary OR †
	Tertiary	Trade	School	Nil	
Males					
<i>25–44 year olds</i>					
Nil	0.57 (0.47–0.69)	0.74 (0.63–0.87)	0.73 (0.61–0.87)	1.00	
Sickness beneficiaries	0.61 (0.50–0.75)	0.78 (0.66–0.92)	0.75 (0.62–0.91)	1.00	10%
Pre-hospitalised deaths	0.66 (0.53–0.82)	0.81 (0.67–0.99)	0.86 (0.70–1.07)	1.00	21%
Non-labour force	0.63 (0.51–0.77)	0.78 (0.65–0.93)	0.76 (0.62–0.93)	1.00	14%
<hr/>					
<i>45–64 year olds</i>					
Nil	0.65 (0.59–0.72)	0.80 (0.74–0.85)	0.80 (0.73–0.87)	1.00	
Sickness beneficiaries	0.69 (0.62–0.76)	0.81 (0.75–0.87)	0.82 (0.75–0.90)	1.00	10%
Pre-hospitalised deaths	0.70 (0.62–0.78)	0.80 (0.73–0.87)	0.79 (0.71–0.88)	1.00	13%
Non-labour force	0.73 (0.64–0.83)	0.83 (0.75–0.92)	0.87 (0.77–0.98)	1.00	22%
<hr/>					
Females					
<i>25–44 year olds</i>					
Nil	0.56 (0.45–0.69)	0.63 (0.49–0.80)	0.61 (0.50–0.75)	1.00	
Sickness beneficiaries	0.61 (0.49–0.76)	0.65 (0.51–0.84)	0.65 (0.53–0.80)	1.00	11%
Pre-hospitalised deaths	0.58 (0.43–0.78)	0.56 (0.39–0.80)	0.60 (0.45–0.80)	1.00	4%
Non-labour force	0.67 (0.52–0.86)	0.62 (0.45–0.85)	0.61 (0.46–0.80)	1.00	25%
<hr/>					
<i>45–64 year olds</i>					
Nil	0.70 (0.63–0.78)	0.79 (0.69–0.90)	0.86 (0.79–0.95)	1.00	
Sickness beneficiaries	0.70 (0.62–0.78)	0.78 (0.68–0.89)	0.86 (0.78–0.94)	1.00	-1%
Pre-hospitalised deaths	0.76 (0.66–0.88)	0.84 (0.70–1.00)	0.96 (0.85–1.08)	1.00	20%
Non-labour force	0.82 (0.69–0.97)	0.90 (0.73–1.11)	1.02 (0.86–1.21)	1.00	40%

Note: Injury and suicide deaths are not presented for females due to small numbers. For all remaining cells in the table there were at least 30 deaths.

† Percentage change is for the excess odds ratio for quintile 5 compared to quintile 1, compared to the same odds ratio with nil exclusions.

2.3 Excluding the non-labour force

2.3.1 All-cause mortality

The reductions in the gradient of all-cause mortality by highest qualification following restriction to the active labour force only (Table 59 above) tended to be less than that for small area deprivation (Table 57, page 212) – but were broadly consistent. The percentage reductions to the null for the tertiary compared to nil qualification odds ratio (and the equivalent reduction for the quintile 5 versus quintile 1 deprivation odds ratio in parentheses taken from Table 57) were 14% (14%) and 22% (43%) for 25–44 and 45–64 year old males respectively, and 25% (32%) and 40% (53%) for 25–44 and 45–64 year old females respectively.

2.3.2 Cause-specific mortality

Regarding cause-specific mortality, the interpretation of the crude risk ratios following exclusions of the non-labour force were problematic due to the strong confounding by age of the association of education with mortality. However, the male results were broadly consistent with those shown in Figure 31 (page 213) for small area deprivation – the reduction in the gradient for cancer and cardiovascular disease deaths following exclusion of the non-labour force was greater than that for suicide deaths, and the gradient actually increased for injury deaths.

3 Household tenure

Only the restricted cohort results are presented for all-cause mortality by housing tenure – a proxy for asset wealth.

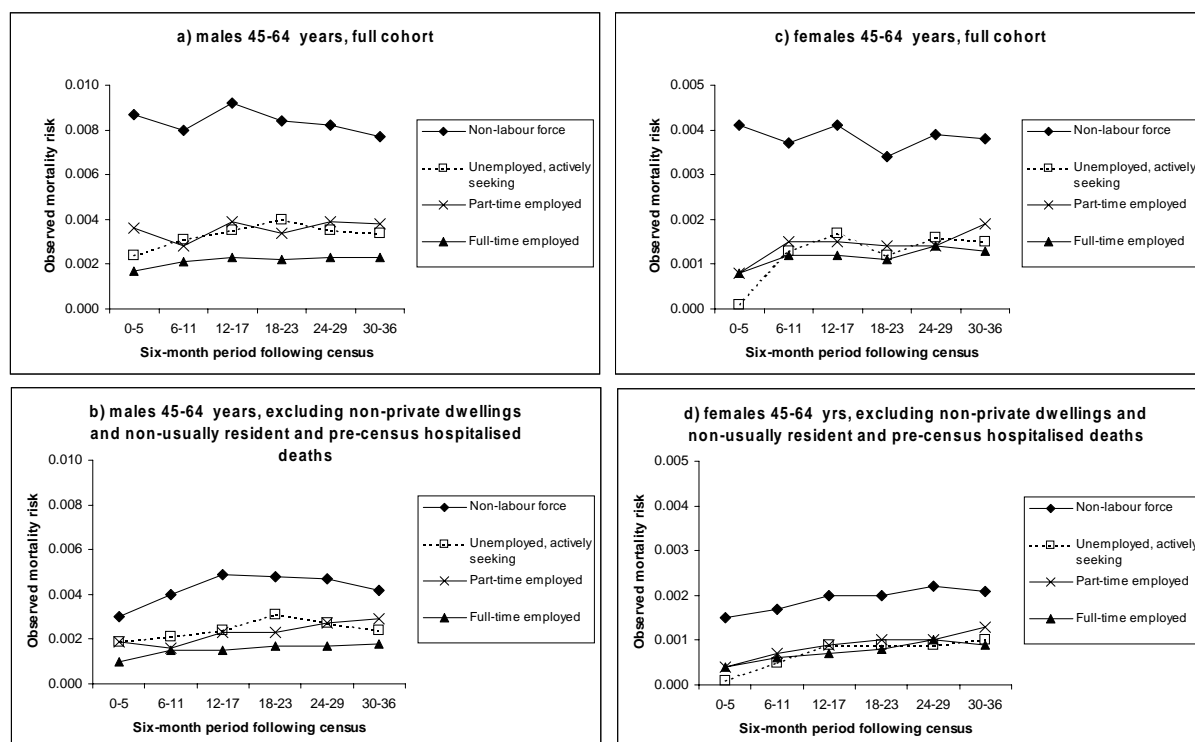
4 Labour force status

There was no suggestion of selection bias for labour force status analyses on the restricted cohort, and a direct assessment of linkage bias was not possible. Thus, the focus for sensitivity analyses was on health selection.

4.1 Health selection

Perhaps the most informative way to assess the impact of health selection on labour force status is to view plots of mortality risk over time by labour force status. Selected plots are presented in Figure 36 (below) for males and females aged 45–64 years. In Figure 36, the lines for the ‘seeking or available’ category are not shown due to small numbers, and hence unstable plots.

Figure 36: Mortality risk for each six-month period following census night by labour force status for 45–64 year old males and females



Several points are demonstrated or suggested by the plots in Figure 36:

- The mortality risk tended to decrease over time among the 'Retired, homemaker, permanently sick, students, etc' (the majority of the non-labour force) in the full cohort for both males and females aged 45–64 years. This trend was the same among the restricted cohort (not shown), and more pronounced among 25–44 year olds. Regarding the 25–44 year olds, the mortality risk halved from the first to the last six-month period of follow-up among both males and females in the non-labour force aged 25–44 years – a steep reduction in mortality. This diminishing mortality over time is what would be expected with short-term health selection (ie, people who are sick and expected to die soon moving out of the labour force), and was also demonstrated in the OPCS Longitudinal Study (Fox and Goldblatt 1982; Fox et al 1985).
- There was no similar falling mortality risk over time among the unemployed 45–64 year olds in the full cohort. Rather there was a tendency for the mortality risk to increase over time in a similar way to the full-time employed, ie, a healthy worker effect. An inspection of 95% confidence intervals (not shown) for the unemployed mortality risks plotted in Figure 36 suggested that the apparent healthy worker effect was unlikely to be due to chance. (Among the unemployed 25–44 year olds, there were too few deaths to draw any conclusions.) This apparent 'healthy worker effect' among the unemployed strongly argues against the elevated mortality risk among the unemployed being due to health selection. Rather, poor health presumably moves people out of employment to the non-labour force, not to the unemployed. This conjecture is also consistent with the relatively rigorous definition of unemployment in the 1991 census, where the person is required to be both actively seeking work and available for work.

- As in the above bullet point, it is also possible to argue that the part-time employed demonstrate a healthy worker effect among the full cohort, arguing against the elevated mortality of the part-time employed being due to health selection.
- The lower two plots in Figure 36 add further weight to the above conclusions. Excluding sickness beneficiaries and those decedents who were hospitalised most notably reduces the mortality risk among the 'retired permanently sick, students, etc', particularly early in the follow-up. This reduction is supportive of a large health selection effect among the non-labour force. Further, applying the same exclusions to 25–44 year olds dramatically reduced the excess risk of mortality among the non-labour force.
- Among 45–64 year old males the *relatively* larger mortality risk among the unemployed (and part-time employed) compared to the full-time employed is not diminished by excluding people with a previous hospitalisation (plot b). This provides further support to the conclusion based on the full cohort plots that the elevated mortality risk among the unemployed (and part-time employed) was not due to health selection.

4.2 Conclusion

The elevated mortality among the non-labour force is, in large part at least, due to health selection.

Both the unemployed and the part-time employed had elevated mortality risks compared to the full-time employed. While this elevation may be the result of confounding (and will be assessed later in the multivariate analyses), the analyses here strongly suggest that it was *not* due to health selection. Further, there was no apparent selection bias, and linkage bias would probably have caused (if anything) an underestimate of the increased mortality among the non-referent labour force groups.

Finally, the particularly notable healthy worker effect between the first and second six-month periods of follow-up supports the decision in the NZCMS to discard linked deaths for the first six months as a means of mitigating against health selection effects in the cohort analyses.

5 Occupational class

Occupational class analyses presented a challenge due to only current occupation being available on the census. The unlinked occupational class analyses by Pearce and colleagues, however, provide a useful point of comparison (Davis et al 1999a; Kawachi et al 1991; Marshall et al 1993; Pearce and Bethwaite 1997; Pearce et al 1983a; Pearce et al 1983b; Pearce et al 1984; Pearce et al 1985; Pearce and Howard 1986; Pearce et al 1991; Pearce et al 1993).

The main cohort results for occupational class were presented in Chapter 6. The objectives of this section were to:

- to assess the likely impact of health selection on the association of occupational class with mortality observed in the NZCMS
- compare and contrast the results from unlinked analyses (ie, as done by Pearce and colleagues) and results using the NZCMS, with particular attention to identifying sources of numerator–denominator bias that may be affecting unlinked analyses.

Regarding the latter comparability with the unlinked analyses of Pearce and colleagues, this report also:

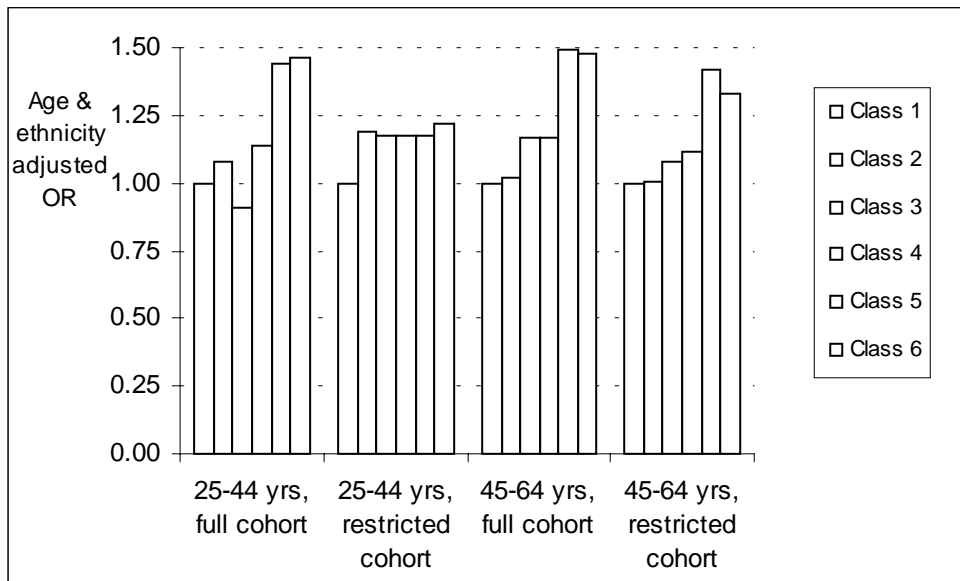
- presents results adjusted for age only, in addition to those adjusted for age and ethnicity, as the unlinked analyses of Pearce and colleagues standardised for age only
- examines how the occupational class assignment among decedents varies between census and mortality data. This variation is best framed as an analysis of numerator–denominator bias between the unlinked and linked analyses, but may also include components of health selection
- conducts analyses using the occupational class based on the death registration form as the exposure (rather than that derived from the census record) among linked deaths, thus imitating an unlinked analysis
- conducts analyses using the occupational class 1 and 2 divisions proposed by Davis et al (Davis et al 1997) to allow direct comparability with the submitted unlinked 1995–97 analyses by Pearce and Sporle (personal communication, 2000).

Due to the substantial problems with occupational class data for females, male data dominates the sensitivity analyses.

5.1 Selection bias for restricted cohort results

Figure 37 presents the occupational class mortality gradients for those in the full cohort with an occupational class. There will be occasion to include occupational class in multivariate analyses with other socioeconomic factors such as education, income, and car access. Thus, it is useful to know the amount of selection bias incurred when assessing the gradient among the restricted cohort. Figure 37 (below) shows the occupational class mortality gradient for males aged 25–44 and 45–64 years, for both the full and restricted cohort. There was a selection bias, such that the gradients were underestimated in the restricted cohort – particularly among 25–44 year olds. The magnitude of selection bias was the same for age-only adjusted odds ratios.

Figure 37: Age and ethnicity adjusted odds ratios of all-cause mortality by NZSEI occupational class among males – a test of selection bias between the full and restricted cohorts



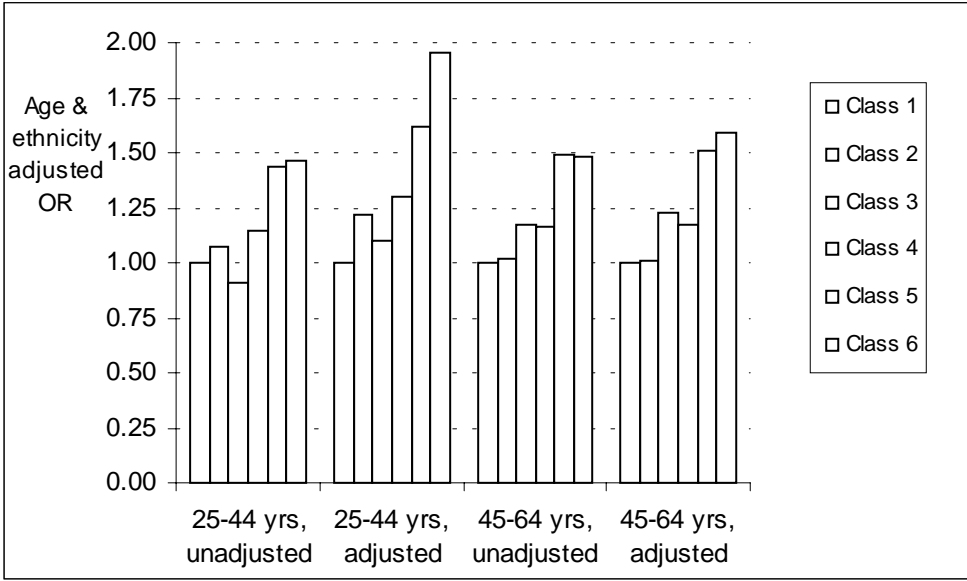
These results for selection bias also add to the accumulating evidence on selection bias in this report. For all-mortality, there was no selection bias of note by highest education for either 25–44 or 45–64 year old males (Section 2.1 of this appendix), but there was selection bias by NZDep91 such that the restricted cohort modestly underestimated the gradient among 25–44 year olds and modestly overestimated it among 45–64 year olds. Adding the occupational class analyses to this accumulating evidence, there is now evidence of selection bias causing:

- an underestimate of the 25–44 year old male all-cause mortality gradient for analyses on the restricted cohort for two out of three socioeconomic factors tested
- an underestimate (occupational class), no effect (education), and an overestimate (NZDep91) of the 45–64 year old male all-cause mortality gradient for analyses on the restricted cohort.

5.2 Linkage bias

Results present in Chapter 3 demonstrated linkage bias by occupational class among males – particularly among 25–44 year olds, and for occupational class 6. Using the risk ratios reported in Table 23, the occupational class mortality gradients were adjusted for linkage bias. The gradients increased, particularly for 25–44 year olds males such that after adjustment for linkage bias there was a near doubling of the odds of death between occupational class 6 and class 1 (Figure 38 below).

Figure 38: Age and ethnicity adjusted odds ratios of all-cause mortality by NZSEI occupational class among males in the full cohort– the effect of adjusting for linkage bias



5.3 Numerator–denominator bias

Unlinked occupational class analyses, such as those by Pearce and colleagues, may be prone to numerator–denominator bias whereby the occupation is collected differently between mortality and census data. In New Zealand there are likely to be differences between census and mortality data due to the occupation on the census being *current* and that on the death registration form being *usual*. The objective of this section is to assess these likely numerator–denominator biases.

Tables 60 and 61 below present a summarised cross-classification of occupational class for the mortality and census data, for male and female decedents respectively. The most informative results in Tables 60 and 61 are the final columns that present the ratio of the number of census to mortality records for each class. For males with any assigned occupational class the overall ratio was 0.56, ie, there were 44% fewer male decedents with an occupational class on census data than on mortality data. The fact that the ratios are relatively constant by occupational class (with the exception of occupational class 4 to be discussed subsequently) means that there should be at most only moderate numerator–denominator biases affecting unlinked occupational class analyses. However, the comparability of these ratios by occupational class does not rule out systematic biases affecting both the unlinked and linked analyses in the same direction. For example, differential health selection out of the labour force may cause an underestimate of lower occupational class deaths in census data and a tendency to ‘promote the dead’ would also tend to cause an underestimate of the lower occupational class deaths on mortality data.

Table 60: Cross-classification of mortality by census occupational class for 5844 male 25–64 year old deaths during the second and third year of follow-up

Occupational class	Census data	Number (%) by census data that:			Mortality data	Number (%) by mortality data that:			Census total to mortality total ratio
		Had same class on mortality data	Had different class on mortality data	Had no occupation on mortality data		Had same class on census data	Had different class on census data	Had no occupation on census data	
Class 1	240	165 (69%)	69 (29%)	6 (3%)	387	168 (43%)	75 (19%)	147 (38%)	0.62
Class 2	300	150 (50%)	126 (42%)	24 (8%)	462	150 (32%)	126 (27%)	189 (41%)	0.65
Class 3	498	279 (56%)	189 (38%)	33 (7%)	861	276 (32%)	216 (25%)	369 (43%)	0.58
Class 4	531	333 (63%)	162 (31%)	36 (7%)	1182	333 (28%)	177 (15%)	672 (57%)	0.45
Class 5	627	369 (59%)	180 (29%)	72 (11%)	1137	372 (33%)	159 (14%)	609 (54%)	0.55
Class 6	252	93 (37%)	111 (44%)	51 (20%)	393	93 (24%)	60 (15%)	243 (62%)	0.64
Farmers	306	261 (85%)	33 (11%)	12 (4%)	507	261 (51%)	57 (11%)	189 (37%)	0.60
Subtotal with occupation	2754	1650 (60%)	867 (31%)	234 (8%)	4932	1650 (33%)	867 (18%)	2415 (49%)	0.56
No occupation	3090	NA	NA	678 (22%)	912	NA	NA	678 (74%)	3.39

Regarding occupational class 4 for males, the census to mortality ratio of 0.45 is substantially lower than the other ratios. This discrepancy suggests that there was a tendency to overestimate the number of deaths in occupational class 4 on mortality data compared to census data.

For female deaths (Table 61), the census to mortality ratio increased with lower occupational class. *Thus, unlinked analyses using only female mortality data will find a shallower (or even inverse) occupational class mortality gradient compared to linked census–mortality data.* This discrepancy between the census and mortality data is not due to differential health selection – that should cause the ratios to decrease, not increase, with lower class. A possible explanation is that females of lower occupational class tend to have frequent part-time or casual work that is detected cross-sectionally by the census, but tends not to be entered as an ‘usual occupation’ on the death registration form. It is interesting to note that slightly more female deaths had an occupational class according to census data (1158) compared to mortality data (1016) – for male deaths many more had a mortality record occupation (4932) than a census record occupation (2754).

Table 61: Cross-classification of mortality by census occupational class for 3798 female 25–64 year old deaths during the second and third year of follow-up

Occupational class	Census data	Number (%) by census data that:			Mortality data	Number (%) by mortality data that:			Census total to mortality total ratio
		had same class on mortality data	had different class on mortality data	had no occupation on mortality data		had same class on census data	had different class on census data	had no occupation on census data	
Class 1	63	27 (43%)	24 (38%)	12 (19%)	54	24 (44%)	12 (22%)	15 (28%)	1.17
Class 2	141	72 (51%)	27 (19%)	39 (28%)	211	72 (34%)	57 (27%)	81 (38%)	0.67
Class 3	177	57 (32%)	48 (27%)	72 (41%)	190	60 (32%)	72 (38%)	54 (28%)	0.93
Class 4	264	96 (36%)	45 (17%)	123 (47%)	227	96 (42%)	33 (15%)	96 (42%)	1.16
Class 5	219	63 (29%)	48 (22%)	111 (51%)	162	63 (39%)	27 (17%)	69 (43%)	1.35
Class 6	216	54 (25%)	27 (13%)	132 (61%)	134	51 (38%)	12 (9%)	63 (47%)	1.61
Farmers	78	15 (19%)	9 (12%)	57 (73%)	40	15 (38%)	6 (15%)	18 (45%)	1.95
Subtotal with occupation	1158	387 (33%)	225 (19%)	549 (47%)	1016	384 (38%)	225 (22%)	402 (40%)	1.14
No occupation	2640	NA	NA	2232 (85%)	2782	NA	NA	2235 (80%)	0.95

Another useful piece of information that can be gleaned from Tables 60 and 61 above was that for both males and females there was much greater variation between census and mortality data for the assignment of a lower occupational class. For example, a greater percentage of male decedents assigned to occupational class 6 on the mortality data had no occupation on census data (62%, second to last column of Table 60) than did male decedents assigned to occupational class 1 on mortality data (38%). Likewise, for assignment of male census occupational class a similar trend was evident for having no occupation on mortality data (20% and 3% for occupational class 6 and 1, respectively). Further, the same trends were evident for females (Table 61). The reason for these trends was that of the decedents with no occupation on census data (mortality data) but with an occupation on mortality data (census data), the latter occupation was more likely to be for a lower occupational class (data not shown). These trends suggest that *both* census and mortality data may be underestimating the number of lower occupational class deaths more so than higher occupational class deaths. Whether this means that both unlinked (eg, those by Pearce and colleagues) and linked analyses (eg, those in the NZCMS) are likely to underestimate the occupational class mortality gradient, however, is uncertain. It is likely that entry and exit (or cycling) into and out of employment among the lower occupational classes may be more common than among the higher occupational classes. Thus, there may be a similar tendency among the census denominator to underestimate the number of people in the lower occupational classes, causing no overall bias in the occupational mortality gradients.

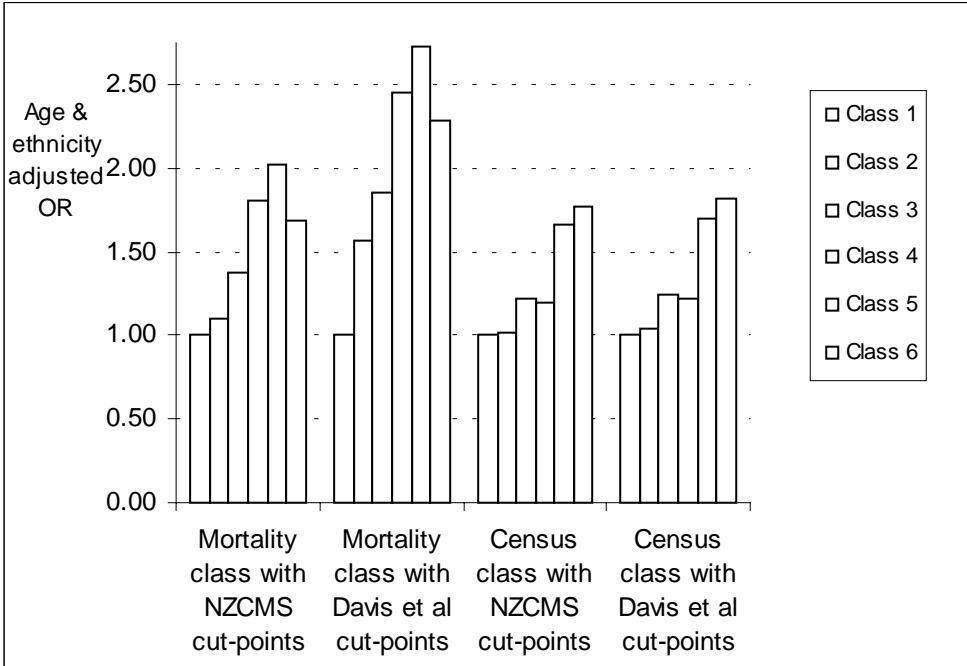
One possible numerator–denominator bias suggested in the literature for unlinked analyses is a tendency to ‘promote the dead’ whereby a decedent’s occupational class is categorised as higher than their actual class. It was possible to test this directly in the NZCMS. Shown in Tables 60 and 61 are the numbers of decedents that changed occupational class between census and mortality data. Of the 774 male decedents with an assigned occupational class of 1 to 6 on mortality data, and who were assigned to another class (ie, 1 to 6) on census data, 357 were assigned a higher occupational class on census data and 417 were assigned a lower occupational class on census data. Thus, there was a tendency for higher occupational class on the mortality data consistent with the ‘promoting the dead’ hypothesis, but the magnitude of the bias was small. The same numbers for females were 126 and 87, respectively, again consistent with a (modest) tendency to promote the dead.

There was a specific numerator–denominator bias apparent for the NZSEI occupational classes. The NZCMS uses a different cut-off between occupational classes 1 and 2 than that originally proposed by Davis et al (Davis et al 1997), for reasons described in Chapter 2 of this report. The occupational class mortality gradients presented below in Figure 39 demonstrate the effect of using the NZCMS cut-points versus the Davis et al cut-points, for 45–64 year old males. (Analyses for 25–44 year olds found the same conclusions as follow.) The first set of columns in Figure 39 present the occupational class mortality gradient using the NZCMS cut-points, where occupational class for the deaths was taken from the mortality data – thus this analysis imitates an unlinked analysis. The second set of columns uses the cut-point between occupational classes 1 and 2 proposed by Davis et al (1997), but still uses the mortality data occupational class for decedents.

There is a dramatic difference between the gradient in the first and second set of columns, due to the mortality risk among the Davis et al class 1 being substantially lower than that in the NZCMS class 1. Correspondingly, using the Davis et al cut-points, and occupational class 1 as the reference category, the gradient appears much steeper. If the mortality risk among the Davis et al class 1 was *truly* much less than the NZCMS class 1, then one would expect a similar change in the gradient using the census assigned occupational classes for the entire cohort-base. The third and fourth set of columns in Figure 39 present the gradient using the NZCMS and Davis et al cut-points and census assigned occupational class – there is virtually no difference in the gradient between the third and fourth set of columns.

Thus, this report concludes that there is a substantial numerator–denominator bias on the mortality data *within* the 70 to 90 range of NZSEI scores. It appears that the coding of occupation on mortality data is assigning many decedents to an occupation with a NZSEI score between 70 and 75, when in fact the census coding would have assigned them to an occupation with a NZSEI score between 75 and 90. Put another way, the mortality gradient depicted in the second set of columns of Figure 39 is affected by a numerator–denominator bias that causes a substantial underestimate of the mortality risk in occupational class 1. An initial inspection of the draft results for 1995–97 by Pearce, Davis and Sporle show a gradient for 1995-97 by NZSEI occupational class that is remarkably similar to that shown in the second set of columns of Figure 39 below. Thus, it seems likely that the apparent numerator denominator bias for mortality data compared to census data about the cut-point between NZSEI classes 1 and 2 also affects 1995-97 mortality data.

Figure 39: Odds ratios of all-cause mortality by NZSEI occupational class among 45–64 year old males, using NZCMS versus Davis et al cut-points, and mortality versus census data occupational class for decedents



Note: All analyses use the full cohort, age-only adjusted odds ratios – but additionally adjusted for linkage bias. Occupational class 1 is the references group.

Putting aside the apparent numerator–denominator bias affecting class 1 aside, there are two other useful findings demonstrated in Figure 39. First, and as suggested by the ratios in the final column of Table 60 (page 230), the NZSEI occupational class 4 mortality risk appears to be overestimated by mortality data. Second, using the census data, occupational class 6 has a higher mortality risk (after adjustment for linkage bias) than occupational class 5. Using mortality data, or an unlinked analysis, the mortality risk is apparently lower in class 6.

Summarising, there were apparent numerator–denominator biases:

- biasing the mortality risk for different NZSEI score cut-points between classes 1 and 2
- causing a relative overestimate of the mortality risk among class 4 using mortality data
- and causing a relative underestimate of the mortality risk for class 6 using mortality data; or conversely, a relative overestimate of the mortality risk for class 5 using mortality data.

Taking all these into account, however, it is important to note that there is broad agreement between the unlinked and linked analyses (eg, the first and third set of columns in Figure 39 are broadly comparable).

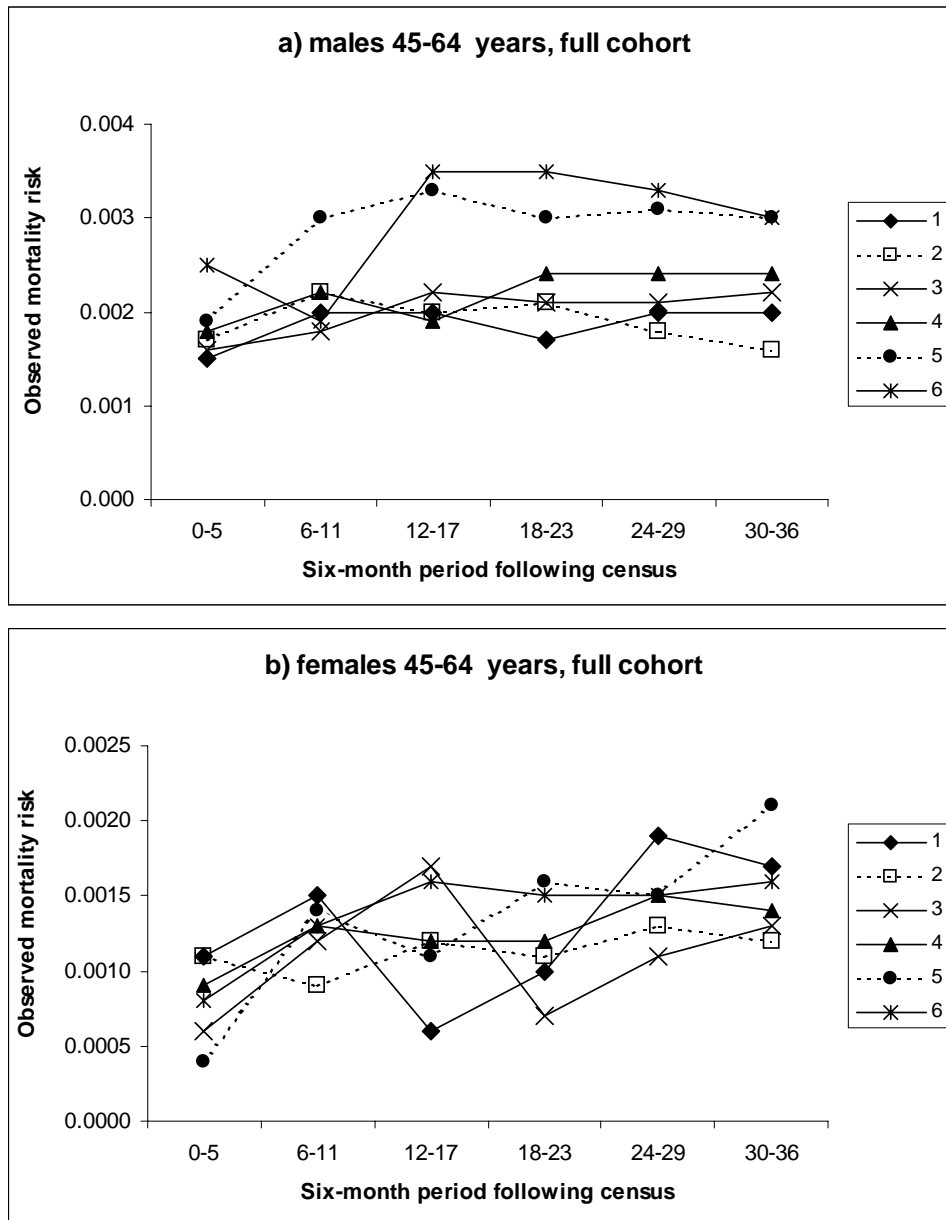
5.4 Health selection

Sensitivity analyses, excluding sickness beneficiaries and decedents with a hospitalisation between 1988 and census night, are not presented for occupational class mortality gradients. As the occupational class mortality gradients were already restricted to those in the labour force (a possible form of differential health selection itself), such sensitivity analyses were difficult to interpret and probably unreliable. Rather, plots of mortality risk over time have been relied on to investigate differential health selection.

5.4.1 Observed mortality risk over time

Plots of mortality risk over time by current occupational class allow the investigation of two forms of health selection that may act over the short term: *differential* health selection out of the labour force, and short-term *drift* health selection up and down occupational classes. If present, these two forms of health selection will give opposite patterns of mortality risks plotted over time – a divergence for differential health selection, and a convergence for short-term health selection down occupational classes. Strictly speaking, plots of mortality risk over time by occupational class allow comment only on the net effect of these two forms of health selection. However, previous research suggests that drift health selection for occupational classes is negligible (Fox et al 1985; Power et al 1996; van de Mheen et al 1999). Accordingly, the patterns in mortality risk over time in terms of differential health selection have been interpreted.

Figure 40: Mortality risk for each six-month period following census night by NZSEI occupational class for 45–64 year old males and females



The observed mortality risks by time period after census night are shown in Figure 40 above for males and females aged 45–64 in the full cohort. Unfortunately, the point estimates are too unstable for females (due to small numbers) to allow interpretation. However, for males there was little difference between the mortality risks in the first six months, consistent with differential health selection that would be more likely to force lower occupational class people with poor health out of the work force than higher occupational classes. Over time, the mortality risks diverge to the expected higher mortality for occupational classes 5 and 6. Of note, the occupational class 6 mortality risk did not rise till the second year of follow-up. While this delayed rise may just be random variation, it may also be a longer apparent differential health selection among the lowest occupational class. (As such, it was one of the contributing reasons to discarding all census respondents dying in the first year for the cohort analyses of occupational class.) Thus, there was evidence of differential health selection affecting the occupational class mortality gradient – but not beyond one year of follow-up, and

particularly in the first six-months of follow-up. It is unclear whether the mortality risks would have diverged further over a period of follow-up longer than three years, although there is some suggestion of stability in the last three six-month periods of follow-up.

If the pattern in Figure 40 is assumed to reflect differential health selection, then what are the implications? First, as occupational class cohort analyses discarded deaths in the first year, and assuming that further divergence would not occur with a longer follow-up, then the cohort analyses of occupational class mortality gradients in this report might be relatively free of differential health selection. However, based on the international literature and the possibility that the plots for all-cause mortality may hide some further divergence by cause of death beyond three years of follow-up, it is more prudent to conclude that some differential health selection may be affecting the occupational class analyses in the NZCMS – but probably not greatly. Second, the pattern does add some support to the suggestion of differential health selection for cancer deaths among 45–64 year old males by highest qualification (Figure 35). But, the support it offers is of limited practical importance as the differential health selection by occupational class appeared to be mostly in the first six months, and all cohort analyses in the NZCMS discard deaths in the first six-months.

5.4.2 Adjustment of the *current* occupational class mortality gradient to approximate the *usual* occupational class mortality gradient

One method proposed by Kunst and colleagues to adjust occupational mortality gradients for differential health selection was to assess the change in the educational mortality gradient between that for all people and that among just the labour force, and assume that this same difference would apply to occupational class mortality gradients (Kunst et al 1998b) (see discussion in Appendix A, page 169). However, as discussed in Appendix A (and elsewhere in this report), such a method may adjust not only for differential health selection, but also for confounding/mediation of the association of *usual/last* occupational class with mortality by *current* labour force status. This possibility is supported by the pattern in Figure 40 above of notable differential health selection in the first year, but little evidence thereafter – although a three-year follow-up limits this conclusion. Nevertheless, using the association of small area deprivation and education with mortality including and excluding the non-labour force presented previously in this report (crude data), it was possible to adjust the current occupational class mortality gradients to approximate the (unobserved) usual occupational class mortality gradients.

The adjustment was conducted as follows:

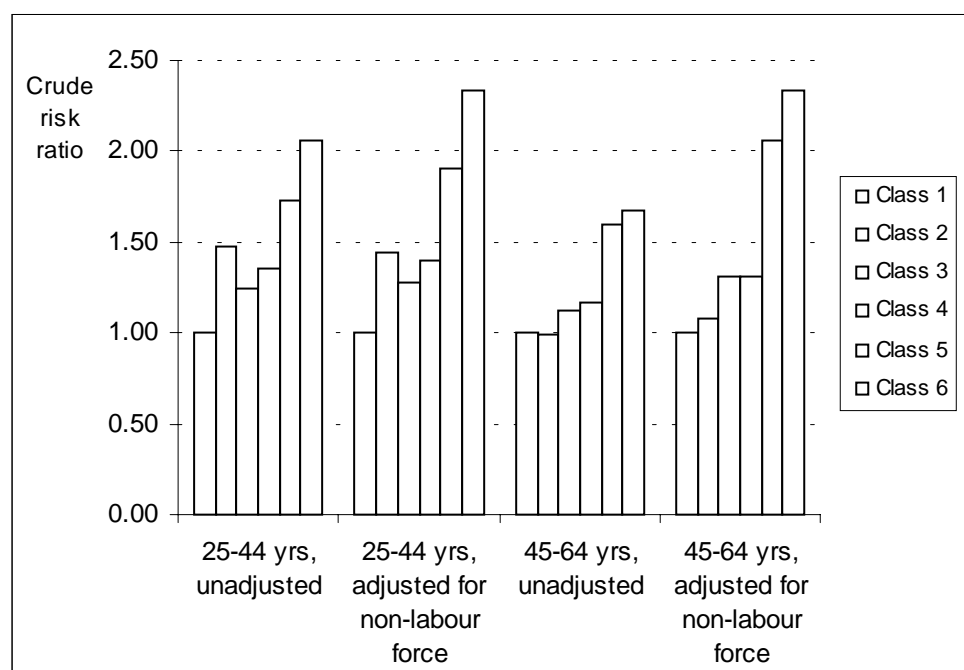
- Approximating the percentage distribution of the population by occupational class, the following small area deprivation and education categories were assumed to be equivalent to occupational classes 1 to 6:
 - [NZDep91 decile 1] and [graduate, postgraduate] for occupational class 1
 - [deciles 2 and 3] and [undergraduate, technical, teaching] for class 2
 - [deciles 4 and 5] and [trade certificate, other tertiary] for class 3
 - [deciles 6 and 7] and [10-12 years of school] for class 4
 - [deciles 8 and 9] and [nil qualification] for class 5
 - [decile 10] and [nil qualification] for class 6.

- The average change in the mortality risk ratio for the deprivation and highest qualification gradient after excluding the non-labour force was determined, where decile 1 and 'graduate, postgraduate' highest qualification were the reference categories.
- Doing so, the ratios to be multiplied into the *current* occupational class odds ratios for 25–44 year old males were 1.00, 0.98, 1.02, 1.03, 1.10, and 1.13 and for 45–64 year old males were 1.00, 1.09, 1.18, 1.13, 1.29, and 1.40, for occupational classes 1 to 6 respectively.

Thus, for example, the mortality risk ratio among males aged 45–64 years in occupational class 6 compared to occupational class 1 (where current occupation only is available) should be multiplied by 1.40 to estimate the risk ratio that would have been observed if usual occupation was available.

Figure 41 below presents the crude risk ratios of mortality by occupational class for males aged 25–44 and 45–64 years in the first and third set of columns. The second and fourth set of columns present the occupational class mortality gradient adjusted for exclusion of the non-labour force, for 25–44 and 45–64 year olds respectively, using the above estimated adjustment ratios. The gradient changes modestly for 25–44 year olds, but increases notably for 45–64 year olds.

Figure 41: Crude risk ratios of all-cause mortality by NZSEI occupational class among 25–44 and 45–64 year old males, before and after adjustment for labour force status



Note: The 'unadjusted' crude risk ratios were adjusted for linkage bias – but not labour force participation.

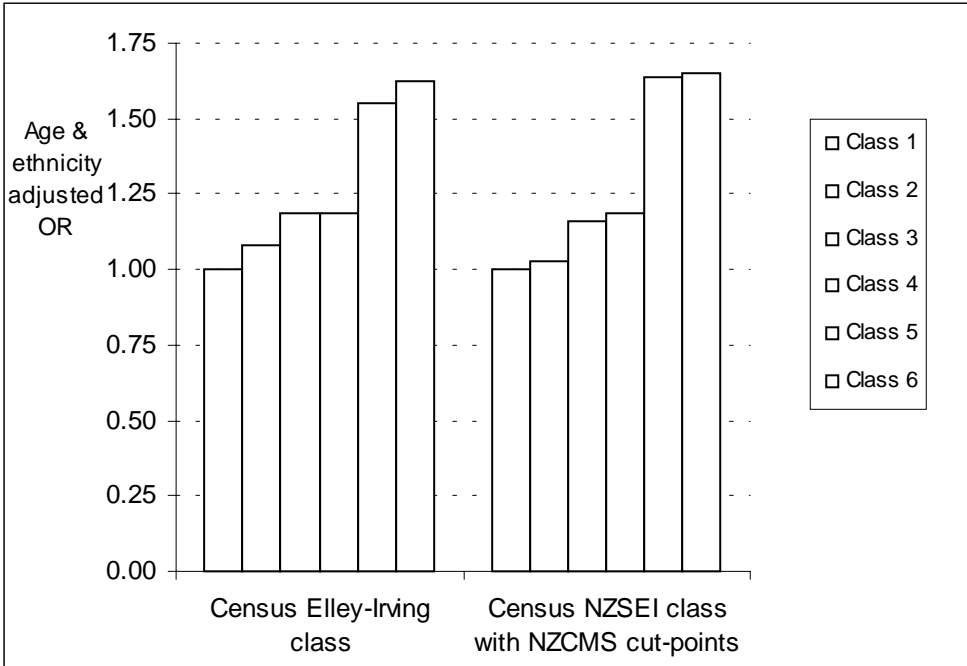
The method used above to adjust for non-labour force status is approximate only, and uses crude data only. The underlying assumption was that the gradient changes by deprivation and education that occur when the non-labour force were excluded was the same as that for occupational class – a questionable assumption. However, it does at least suggest that:

- the use of *current* occupation probably results in a substantial underestimate of the *usual* occupational class mortality gradient for 45–64 year old males, but less so for 25–44 year old males
- adjusting for the exclusion of the non-labour force results in a similar gradient for 25–44 and 45–64 year olds.

Kunst and colleagues have proposed another method to adjust occupational mortality gradients based on *current* occupation to approximate those for *usual* occupation (Kunst 1997; Kunst et al 1996; Kunst et al 1998c). This method draws on survey data external to the census-cohort study that give estimates of the proportion of each occupational class (based on *usual* occupation) that were in the labour force at the time of the census. Such a sensitivity analysis may be conducted in New Zealand using Household Labour Force Survey, but was beyond the scope of this report.

5.5 Comparison of NZSEI and Elley-Irving occupational mortality gradients

Figure 42: Odds ratios of all-cause mortality by NZSEI and Elley-Irving occupational class among 45–64 year old males



Note: The odds ratios are age-adjusted only, and not adjusted for linkage bias (linkage bias was only estimated for NZSEI classes.) Analyses are on the full census cohort, and use the census occupation codes.

The published analyses of occupational class mortality gradients in New Zealand by Pearce and colleagues have used Elley-Irving occupational classes. This classification system requires NZSCO68 codes. Fortunately, both NZSCO68 and NZSCO90 codes were available for 1991 census data, allowing a direct comparison of the NZSEI and Elley-Irving occupational class mortality gradients. Figure 42 presents the gradient for the two different occupational class schemes – the gradients are almost identical.

This comparability between the NZSEI and Elley-Irving gradients means that Pearce and colleagues should be able to extend their times series analysis of occupational class mortality gradients despite having to switch from Elley-Irving to NZSEI classes. However, the above comparability was for the census occupation codes, not the death registration form codes. Unfortunately, comparability on the latter could not be assessed due to the absence of both NZSCO68 and NZSCO90 codes for the 1991–94 time-period. Nevertheless, the two occupational class systems should also be comparable for death registration data and the resultant unlinked analyses.

5.6 Conclusion

The analyses of the association of occupational class with mortality in the NZCMS were limited by the availability of current occupation only. However, it was reasonable to conclude that:

- there was an occupational class mortality gradient in the expected direction for both males and females aged 25–64 years, although only weakly for females
- compared to a male occupational mortality gradients by *usual* occupation, the gradients observed in the NZCMS by *current* occupation were probably an underestimate – particularly for 45–64 year old males
- it was unclear whether the shallower gradient for *current* occupational class compared to *usual* occupational class was due to health selection or confounding/mediation by the range of variables that labour force status may be a proxy for. The plots of mortality risk over time for current occupational class suggested that differential health selection had largely worn-off after the first year of follow-up
- the occupational class mortality gradient by *current* occupation observed in the NZCMS was steeper among 25–44 year old males compared to 45–64 year old males. However, if *usual* occupation data were available there would probably have been little difference in the gradient between the two age-groups.

The above analyses by occupational class also provide important comparative information for unlinked analyses such as those by Pearce and colleagues:

- The linked and unlinked analyses were broadly comparable for *males*.
- Unlinked analyses for females would grossly underestimate the female occupational class mortality gradient due to numerator–denominator biases.
- Despite the broad agreement of the male gradients, there appeared to be several specific numerator–denominator biases for male death registration form occupational class compared to census occupational class:
 - using the cut-off between occupational class 1 and 2 recommended by Davis et al (1997) resulted in a notable underestimate of the class 1 mortality risk according to death registration form data – a cut-off NZSEI score of 70 between occupational classes 1 and 2 is recommended for unlinked analyses

- occupational class 4 deaths appeared to be overestimated by death registration form data relative to census data
- compared to census data, occupational class 6 deaths appeared to be underestimated relative to occupational class 5 deaths by death registration form data. Adjusting for this numerator–denominator bias would probably make the class 6 mortality risk higher (rather than lower) than the class 5 mortality risk in unlinked analyses.
- The occupational class mortality gradients appeared similar for the Elley Irving and NZSEI scales.

6 Equivalised household income

6.1 Likely impact of selection bias and linkage bias

It was not possible to directly estimate selection and linkage bias in the NZCMS for equivalised household income. Therefore, only ‘likely estimates’ are possible.

6.1.1 All-cause mortality

Selection bias for analyses on the restricted cohort has been measured previously in this report for analyses by small area deprivation, highest qualification, and occupational class (Section 1.1, 2.1 and 5.1 of Appendix C). Linkage bias has been measured for small area deprivation and occupational class (Sections 3.3.1 and 3.3.2 of Chapter 3, respectively). On the basis of these sensitivity analyses, it seems plausible to expect that the net impact of selection and linkage biases was an underestimation of the odds ratio comparisons for the lowest compared to the highest equivalised household income groups of approximately:

- 20% for 25–44 year old males and 25–44 and 45–64 year old females
- and 10% for males aged 45–64 years.

This underestimation would be mainly a consequence of linkage bias. Comparisons of middle-income households with high-income households would probably not be underestimated by as much – *the biases mainly operated at the extremes*. Accordingly, the univariate all-cause mortality odds ratios comparing the lowest (<\$10,000) and second lowest (\$10–\$14,999) equivalised household income groups compared with the highest household income group (≥\$70,000) for all New Zealanders might have been in the range of:

- 2.15 to 2.55 for males aged 25–44 and 45–64 years (compared with 1.92 to 2.37 observed, calculated from Table 48)
- 1.75 to 2.05 for females aged 25–44 and 45–64 years (compared with 1.55 to 1.85 observed, calculated from Table 48).

6.1.2 Cause-specific mortality

While the sum of linkage and selection bias affecting cause-specific income mortality gradients must equal that expected above for all-cause mortality, it was not possible to estimate reliably the contribution by each cause of death. However, based on the conclusions for the net effect of both biases by NZDep91 (Box 9, page 208), selection biases for education and NZDep91 (Box 10, page 219), and (unstable) estimates of

linkage bias by occupational class for males (Table 24, page 70), it might be reasonable to conclude that:

- there was probably a modest underestimate of the *male cancer* and *cardiovascular disease* gradients
- there was probably little or no net bias affecting the *male injury* gradient
- there was probably an underestimation of the *male suicide* mortality gradient by household income, such that the odds ratio comparing <\$20,000 to >\$50,000 was greater than 2.5 (observed was 2.27)
- there was probably a sizeable (say 30%) underestimation of the *female cancer* gradient, but it was relatively modest to start with (Table 41, page 95)
- there was probably little or no net bias affecting the *female cardiovascular disease* gradient
- the direction and magnitude of any net bias of the *female injury* and *suicide* gradients was uncertain.

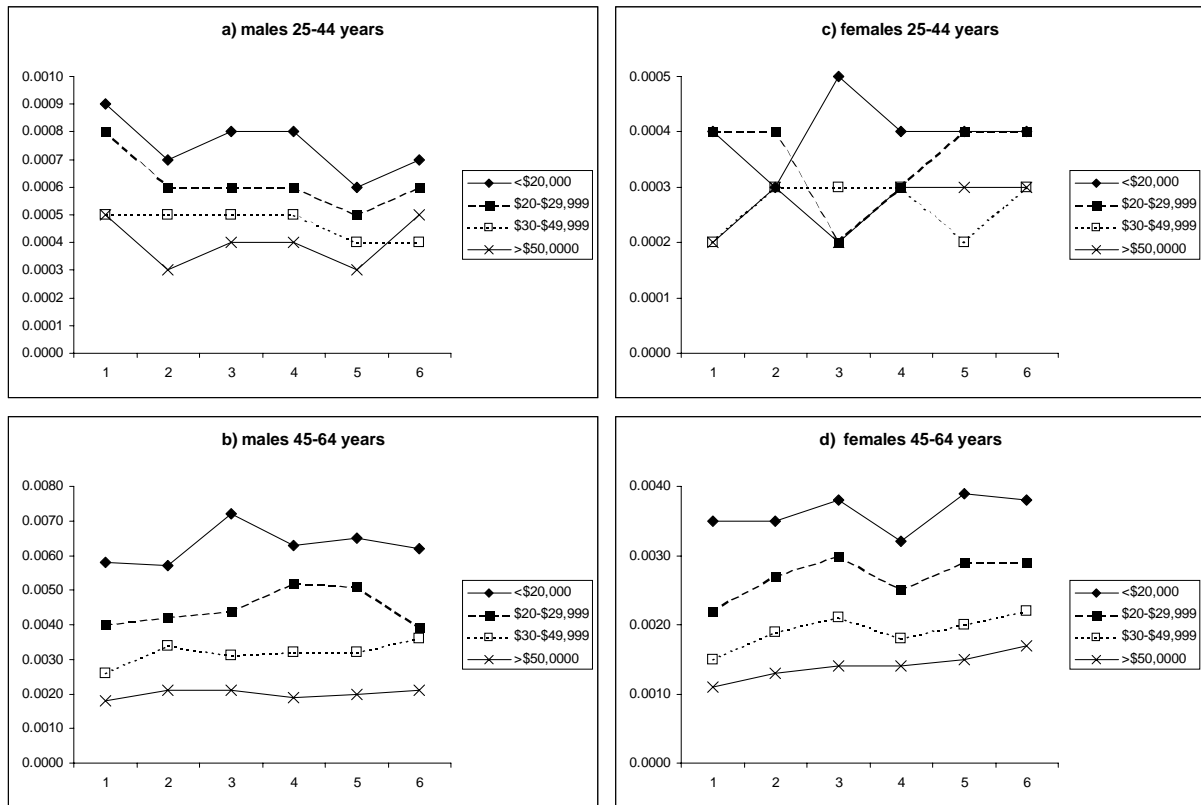
6.2 Health selection

Disentangling the possible impact of health selection on income gradients proved challenging. Most notably, plots of the mortality risk over time suggested no short-term health selection, but the association of income with mortality excluding the non-labour force greatly reduced the income gradient. The results presented in the following sections attempt to present a logical and stepwise series of analyses investigating possible health selection.

Before considering the sensitivity analyses, useful information that may assist the disentangling of health selection includes:

- the income exposure in the NZCMS was equivalised *household* income. Therefore, one would expect the effect of *drift* health selection consequent on one person's ill-health to be mitigated by the income of other members of the household. This would be particularly so for females, where, for 1991–94 and 45–64 year olds at least, the main income earner would often be a male partner.
- the household income was that for the 12 months prior to census night. This 'exposure ascertainment' period, plus the exclusion of deaths in the first six months, should mitigate against short-duration health selection effects.

Figure 43: Mortality risk for each six-month period following census night by four-levels of household equivalised income, 25–64 year olds

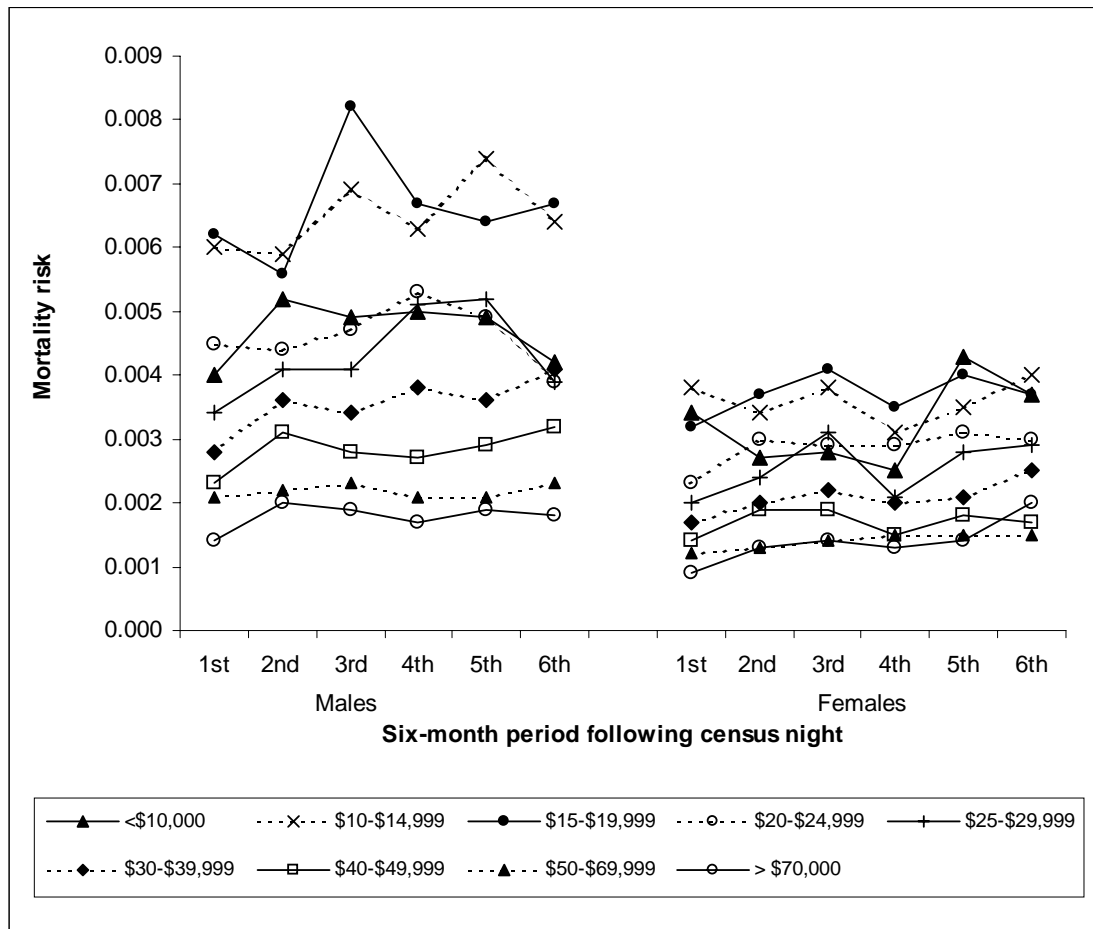


6.2.1 Observed mortality risk over time

All-cause mortality, all labour force categories

Figure 43 shows the plots of mortality risk by time period for each level of equivalised household income, for males and females. Unfortunately, the point estimates for females aged 25–44 are too unstable (due to small numbers) to allow a meaningful interpretation. If *drift* health selection over the short-term was occurring, then one would expect the lines in Figure 43 to converge over time. That is, the low-income people would have high mortality initially due to poor health, and this would fall over time as the unhealthy died or improved in health. Conversely, the mortality risk among the high-income people would be low initially and increase over time as people became unwell, (dropped their income), and died.

Figure 44: Mortality risk for each six-month period following census night by household equivalised income for 45–64 year old males and females



Although not compelling overall, there is some evidence of drift health selection in Figure 43. First, it might be argued that the overall pattern is of the mortality risk lines converging over time among 25–44 year old males (Figure 43a). However, small numbers and SNZ privacy requirements make this conclusion tenuous. For example, 95% CIs are about ± 0.00015 (ie, 15-30% of the observed mortality risk at each point in time). Second, for the middle two income categories among 45–64 year old males (Figure 43b) there was convergence during the last six-month period. Figure 44 demonstrates that this was also the case for finer strata of equivalised household income. However, both Figures 43b and 44 demonstrate that apart from this last six-month period the overall pattern among 45–64 year old males is one of approximately parallel mortality risk lines. Third, the mortality risk among 45–64 year old females with a high income ($\geq \$50,000$) rose approximately 50% over time (Figure 43d), from a risk of 0.0011 (95% CI 0.0009 to 0.0014) in the first six-month period to 0.0017 (95% CI 0.0014 to 0.0020) in the last six-month period. Among low-income people ($< \$20,000$) the rise was only 10%, from 0.0035 (95% CI 0.0030 to 0.0039) in the first six-month period to 0.0038 (95% CI 0.0033 to 0.0042) in the last six-month period. Thus there was some evidence of convergence in mortality risk by equivalised household income over time among 45–64 year old females, consistent with some drift health selection.

As well as interpreting the income mortality risk plots in isolation, they should also be interpreted relative to the 'base-line' plots by NZDep91 (Figure 29, page 209) and highest qualification (Figure 33, page 219). Such a comparison suggests that the possible convergence of plots by income among 25–44 year olds was not seen in the baseline analyses. The pattern of plots for 45–64 year old males was not notably different by socioeconomic factors. Among females aged 45–64, there was a rising mortality risk over time among those with tertiary education, warning against over-interpretation of the rising mortality risk among high income females in Figure 43d.

Two additional problems with the mortality risk plots limit their interpretation with regard to drift health selection. First, it may be that over a longer period of follow-up the mortality risk plots would have more convincingly demonstrated drift health selection. Second, drift health selection will only apply to the component of the income–mortality association that is not due to confounding by other factors. The relative height of the lines in the above plots over time is confounded by factors such as age and education, and it is likely that these factors will exaggerate the distance between the low-income and high-income plots. Thus, for drift health selection to explain the unconfounded association of income with mortality in a short-duration study such as the NZCMS, does not require that the line-plots fully converge. Rather, they would only have to 'converge' to a point where the remaining differences in the height of the lines were due to confounding of the association of income with mortality by factors other than health status. Therefore, the plots for 25–45 year old males and 45–64 year old females (Figures 43a, d) might be more suggestive of drift health selection than they first appear.

Cancer and cardiovascular disease deaths among 45–64 year olds: all labour force categories, and excluding non-active labour force

Death from cancer is usually preceded by a period of poor health. Therefore, if drift health selection was truly biasing the income–mortality gradients, we would expect to a health selection pattern more clearly among cancer deaths. Figures 45a and 45c below show the cancer mortality risk plots over time for 45–64 year old males and females in all labour force categories. As with all-cause mortality, there was not compelling evidence of drift health selection among males, but some suggestion of drift health selection among females. Certainly, the pattern of slopes was not notably different from those for all-cause mortality, which would have been expected if there was drift health selection. Furthermore, other than some suggestion of convergence for 45–64 year old females, the pattern of *slopes* for the cancer mortality plots was not notably different from that for the baseline socioeconomic factors where drift health selection should not theoretically be operating – small area deprivation (Figure 30) and education (Figure 34).

As with the cancer plots, the cardiovascular mortality risk plots for 45–64 year old males were also not particularly suggestive of drift health selection (not shown).

Figure 45: Cancer mortality risk for each six-month period following census night by equivalised household income for 45–64 year olds, all labour force categories and excluding the non-active labour force

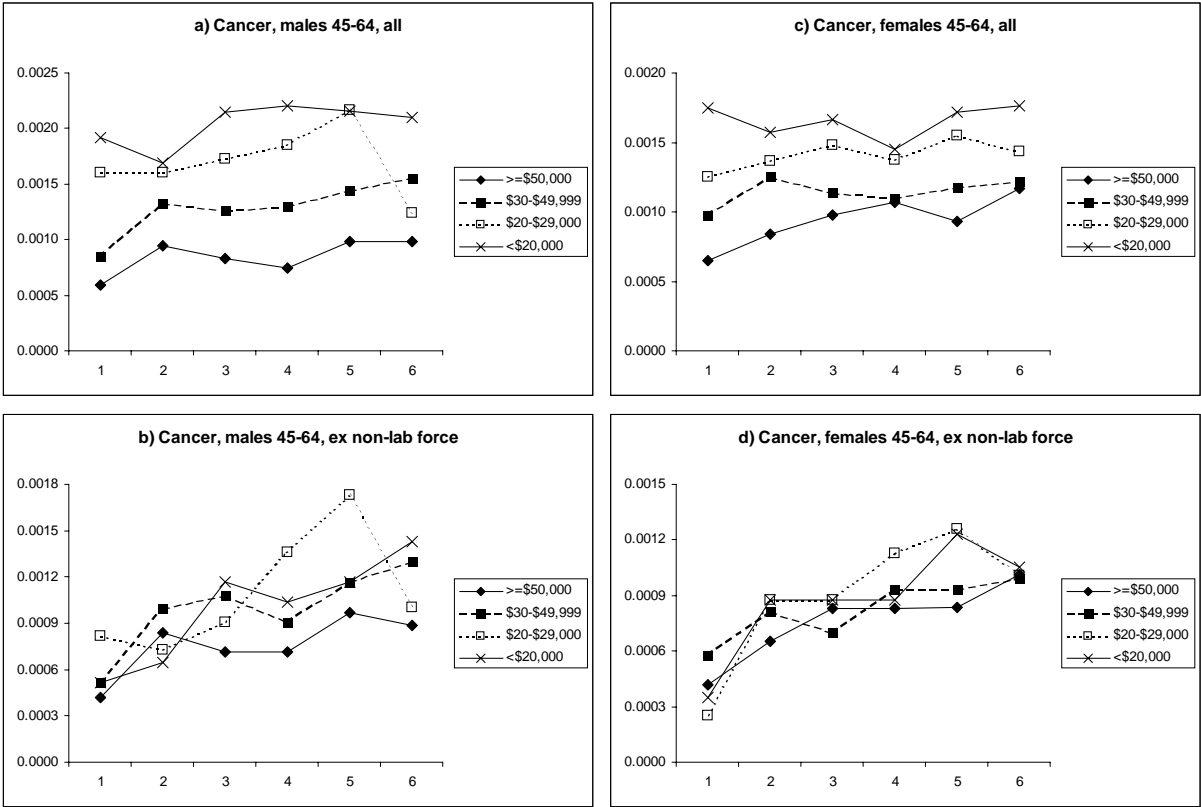
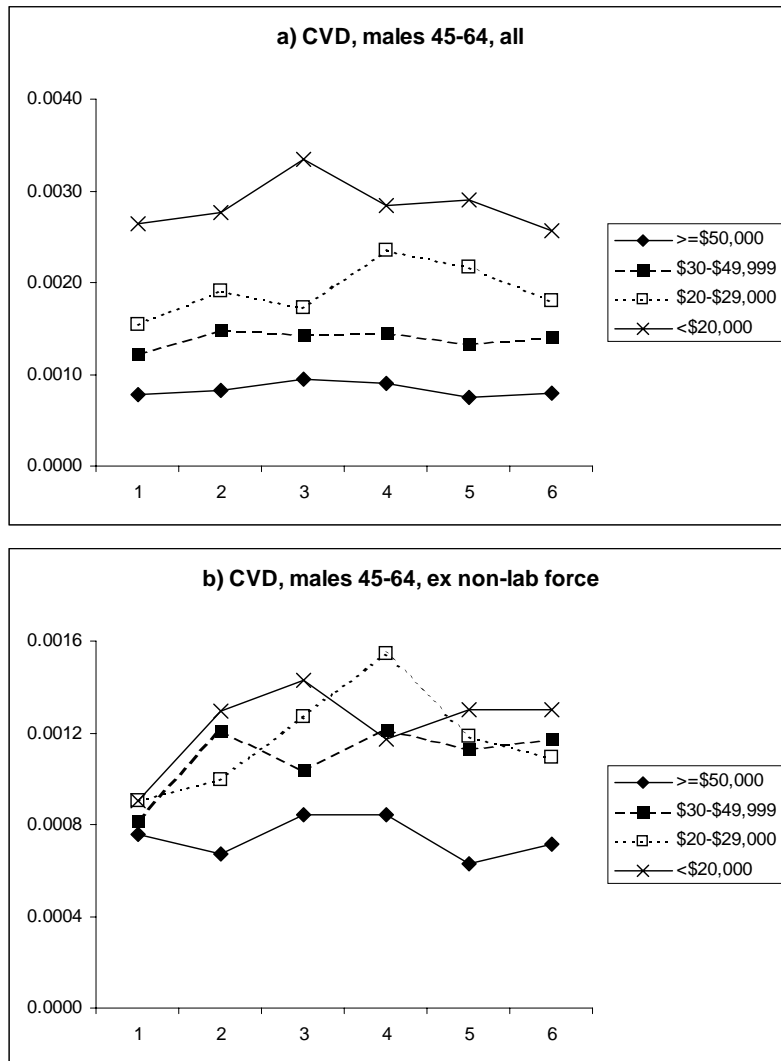
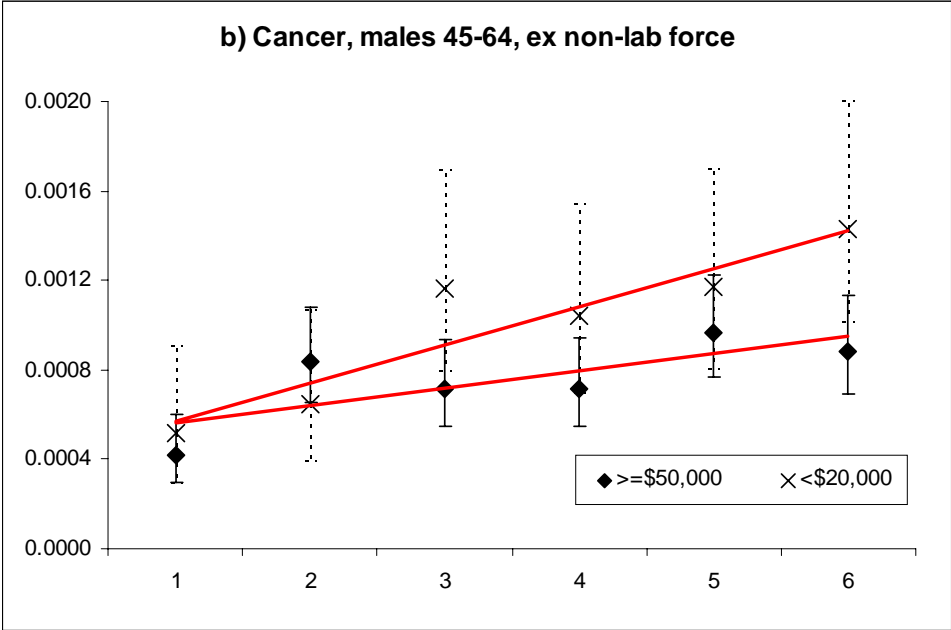


Figure 46: Cardiovascular disease mortality risk for each six-month period following census night by equivalised household income for 45–64 year old males, all labour force categories and excluding the non-active labour force



Mortality risk plots, excluding the non-active labour force for 45–64 year old male and female cancer deaths, are shown in Figures 45b and 45d above, respectively, and for 45–64 year old male cardiovascular disease deaths in Figure 46b. Among 45–64 year old male cancer deaths, there was some evidence of steeper slopes for the low-income strata compared to the high-income strata. Figure 47 presents the plots for just those with an equivalised household income of greater than or equal to \$50,000, and those less than \$20,000, to more clearly demonstrate the differing slopes. (If deaths in the first six months were deleted from Figure 47 (consistent with the cohort analyses) the difference in slopes became more convincing.) There was little evidence of differing slopes for 45–64 year old female cancer deaths and for 45–64 year old male cardiovascular disease deaths. Thus differential health selection by income was only supported for 45–64 year old male cancer deaths. Previous plots in this report also suggested differential health selection for 45–64 year old male cancer deaths for education (Figures 34b and 35), but not for deprivation (Figure 30b).

Figure 47: Cancer mortality risk for each six-month period following census night for 45–64 year old males with high and low equivalised household income, excluding the non-active labour force



6.2.2 Excluding sickness beneficiaries

The association of equivalised household income with all-cause mortality before and after, excluding sickness beneficiaries, is shown in Table 62. The percentage reduction to the null for the excess odds ratio after excluding sickness beneficiaries, comparing people with an equivalised household income over \$50,000 to less than \$20,000, ranged between 11% to 32% for the four sex by age groups. The percentage reductions among 45–64 year olds were similar to those for small area deprivation (Table 57) and highest qualification (Table 59). However, the percentage reductions among 25–44 year old males (32%) and females (27%) were greater than those for small area deprivation (14% and 18%) and highest qualification (10% and 11%). Thus, these results are suggestive of some *drift* health selection affecting the 25–44 year old income–mortality gradients. An alternative explanation is that equivalised household income is more highly correlated with receipt of a sickness benefit among 25–44 year olds than either small area deprivation or highest qualification.

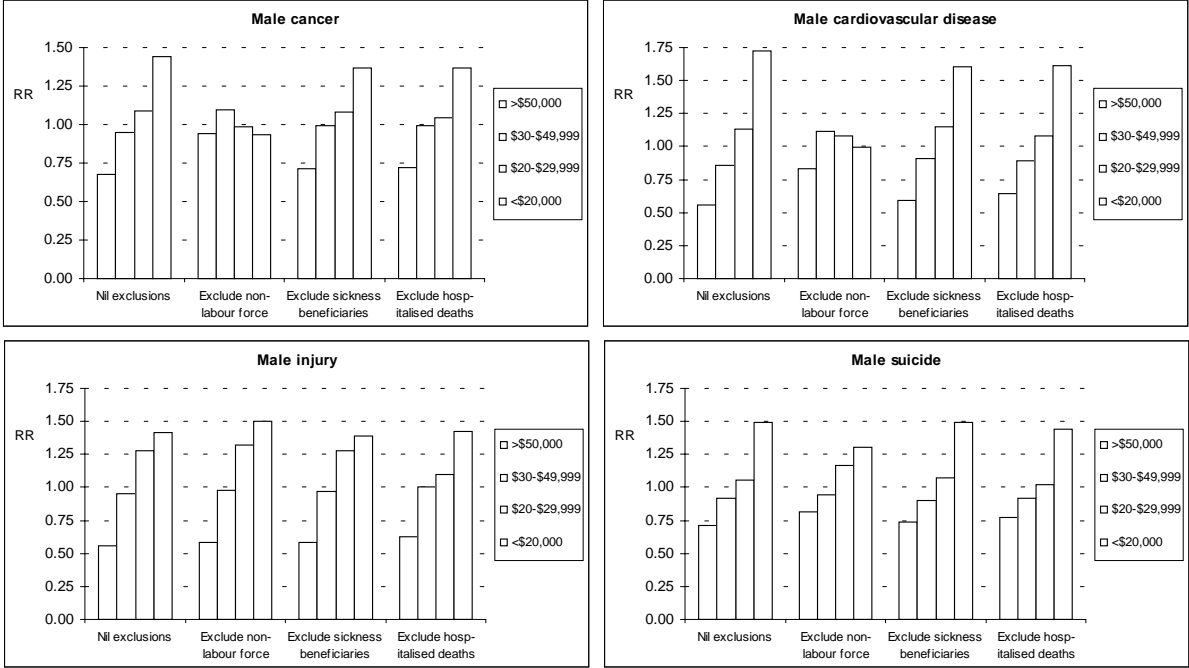
Table 62: Odds ratios of all-cause mortality for 25–64 year olds in the restricted cohort, by household income, for various exclusions testing for health selection

Exclusion criteria	Odds ratios (ref group = <\$20,000)				% change to null of ≥\$50 OR †
	≥\$50,000	\$30–\$49,999	\$20–\$29,999	<\$20,000	
Males					
<i>25–44 year olds</i>					
Nil	0.65 (0.55–0.78)	0.81 (0.68–0.96)	0.94 (0.79–1.10)	1.00	
Sickness beneficiaries	0.76 (0.63–0.92)	0.94 (0.78–1.13)	1.06 (0.89–1.27)	1.00	32%
Pre-hospitalised deaths	0.74 (0.60–0.91)	0.95 (0.77–1.16)	0.92 (0.75–1.12)	1.00	24%
Non-labour force	0.81 (0.66–0.98)	0.95 (0.78–1.15)	1.05 (0.87–1.27)	1.00	44%
<hr/>					
<i>45–64 year olds</i>					
Nil	0.54 (0.49–0.59)	0.77 (0.70–0.83)	0.89 (0.83–0.96)	1.00	
Sickness beneficiaries	0.61 (0.56–0.67)	0.86 (0.79–0.94)	0.92 (0.85–1.00)	1.00	17%
Pre-hospitalised deaths	0.61 (0.55–0.68)	0.81 (0.73–0.90)	0.88 (0.80–0.97)	1.00	16%
Non-labour force	0.75 (0.67–0.84)	1.05 (0.93–1.17)	1.01 (0.90–1.14)	1.00	47%
<hr/>					
Females					
<i>25–44 year olds</i>					
Nil	0.70 (0.56–0.86)	0.71 (0.57–0.88)	0.91 (0.74–1.12)	1.00	
Sickness beneficiaries	0.78 (0.62–0.97)	0.78 (0.62–0.98)	0.90 (0.72–1.12)	1.00	27%
Pre-hospitalised deaths	1.00 (0.75–1.33)	0.94 (0.69–1.27)	0.84 (0.61–1.15)	1.00	100%
Non-labour force	0.89 (0.69–1.16)	0.83 (0.63–1.10)	0.96 (0.72–1.28)	1.00	65%
<hr/>					
<i>45–64 year olds</i>					
Nil	0.66 (0.59–0.74)	0.77 (0.69–0.86)	0.95 (0.87–1.04)	1.00	
Sickness beneficiaries	0.70 (0.63–0.78)	0.80 (0.72–0.90)	0.97 (0.89–1.07)	1.00	11%
Pre-hospitalised deaths	0.76 (0.66–0.87)	0.79 (0.68–0.91)	0.89 (0.78–1.00)	1.00	29%
Non-labour force	0.83 (0.70–0.99)	0.88 (0.73–1.06)	0.97 (0.80–1.18)	1.00	51%

Note: Injury and suicide deaths are not presented for females due to small numbers. For all remaining cells in the table there were at least 30 deaths.

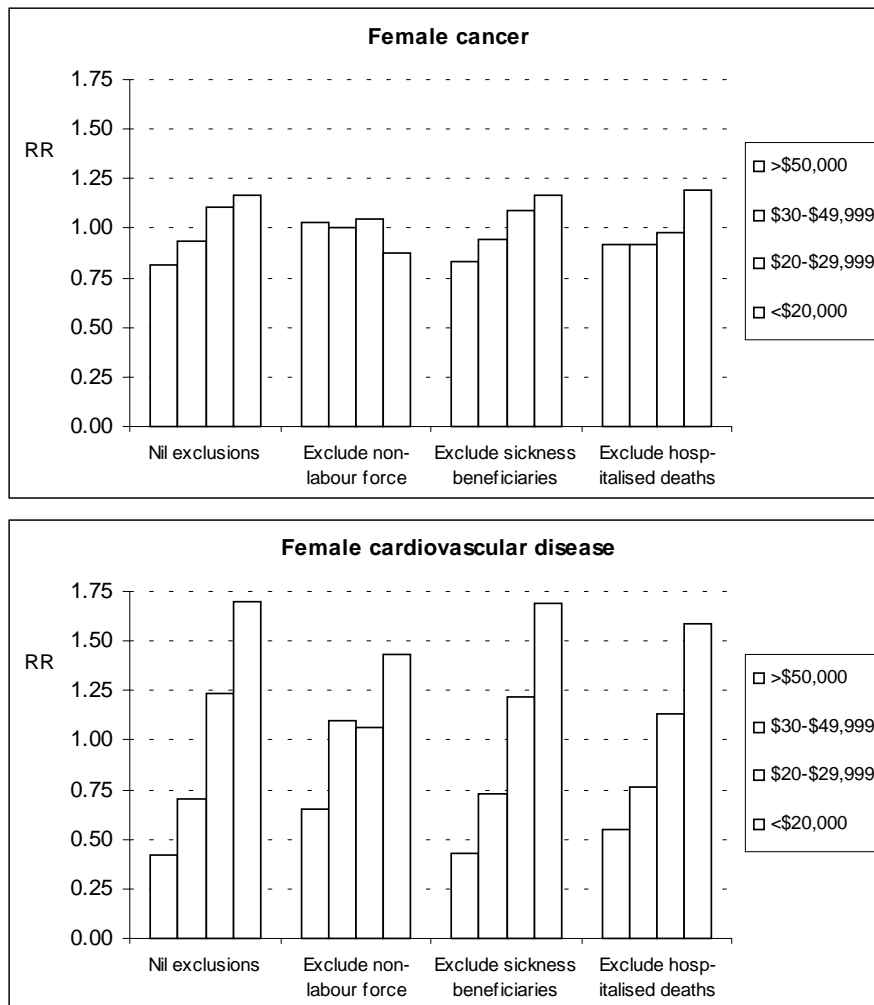
† Percentage change is for the excess odds ratio for quintile 5 compared to quintile 1, compared to the same odds ratio with nil exclusions.

Figure 48: Risk ratios of cause-specific mortality for 25–64 year old males in the restricted cohort, by household income, for various exclusions testing for possible health selection



By cause of death for 25–64 year olds combined (Figures 48 and 49), there was little evidence of any substantive diminution of the income–mortality gradient following the exclusion of sickness beneficiaries for any of the examined causes of death – except perhaps for male cancer and cardiovascular disease. However, this was the same pattern as that for small area deprivation (Figure 31), failing to strongly suggest drift health selection as a bias affecting income mortality gradients.

Figure 49: Risk ratios of cancer and cardiovascular disease mortality for 25–64 year old females in the restricted cohort, by household income, for various exclusions testing for possible health selection



6.2.3 Excluding decedents with a hospitalisation event between 1988 and census night

The association of equivalised household income with all-cause mortality before and after excluding deaths hospitalised between 1988 and census night is also shown in Table 62. The percentage reduction to the null for the excess odds ratio, comparing people with an equivalised household income over \$50,000 to less than \$20,000, ranged between 16% to 100% for the four sex by age groups. Only the reduction to the null for the 25–44 year old females (100%) was substantially greater than the comparable reductions for small area deprivation (17%; Table 57) and highest qualification (4%; Table 59). However, the number of deaths among 25–44 year olds ($n=651$), particularly after exclusion of pre-hospitalised deaths ($n=324$), was the smallest of all four sex by age groups. Consequently, the 95% confidence intervals about the odds ratios for 25–44 year old females after exclusion of pre-hospitalised deaths are wide (Table 62). Thus, as with the exclusion of sickness beneficiaries, there was not convincing evidence of *drift* health selection affecting the all-cause mortality gradients by income for the sensitivity analyses excluding pre-hospitalised deaths.

By cause of death for 25–64 year olds combined (Figures 48 and 49 above), the pattern for the exclusion of pre-hospitalised deaths was very similar to that for the exclusion of sickness beneficiaries described in the previous section. It was also very similar to that for pre-hospitalised deaths in the NZDep91 analyses (Figure 31, page 213).

6.3 Excluding the non-active labour force

Income is highly dependent on labour force status – to have a high household income, at least one person in the household needs a high-paying job. This ‘necessary’ relationship between labour force status and NZDep91 or education does not exist. However, if income were truly associated with mortality, then we would still expect to see a strong association among the active labour force where there is large variation in incomes. Put in epidemiological terms there is a strong association between labour force status and income, but it is not an exact concordance. Given the strong association of labour force status with mortality risk, then the income–mortality association should be more prone to confounding by labour force status than either the education–mortality or deprivation–mortality gradients. If present, this confounding will be disclosed when restricting the analyses to the active labour force – presumably for all specific causes of death associated with income.

On the other hand, income is theoretically the socioeconomic factor in the NZCMS most likely to be affected by drift health selection. As labour force status is a proxy for, among other things, health status, large reductions in the income–mortality association when restricting analyses to the active labour force would also be suggestive of health selection – particularly if larger reductions were evident for causes of death preceded by poor health.

6.3.1 All-cause mortality

For each sex by age group, the income mortality gradient was approximately halved following exclusion of the non-active labour force. The reductions to the null for the $\geq \$50,000$ compared to $< \$20,000$ odds ratio for all-cause mortality following exclusion of the non-labour force (Table 62 above) were:

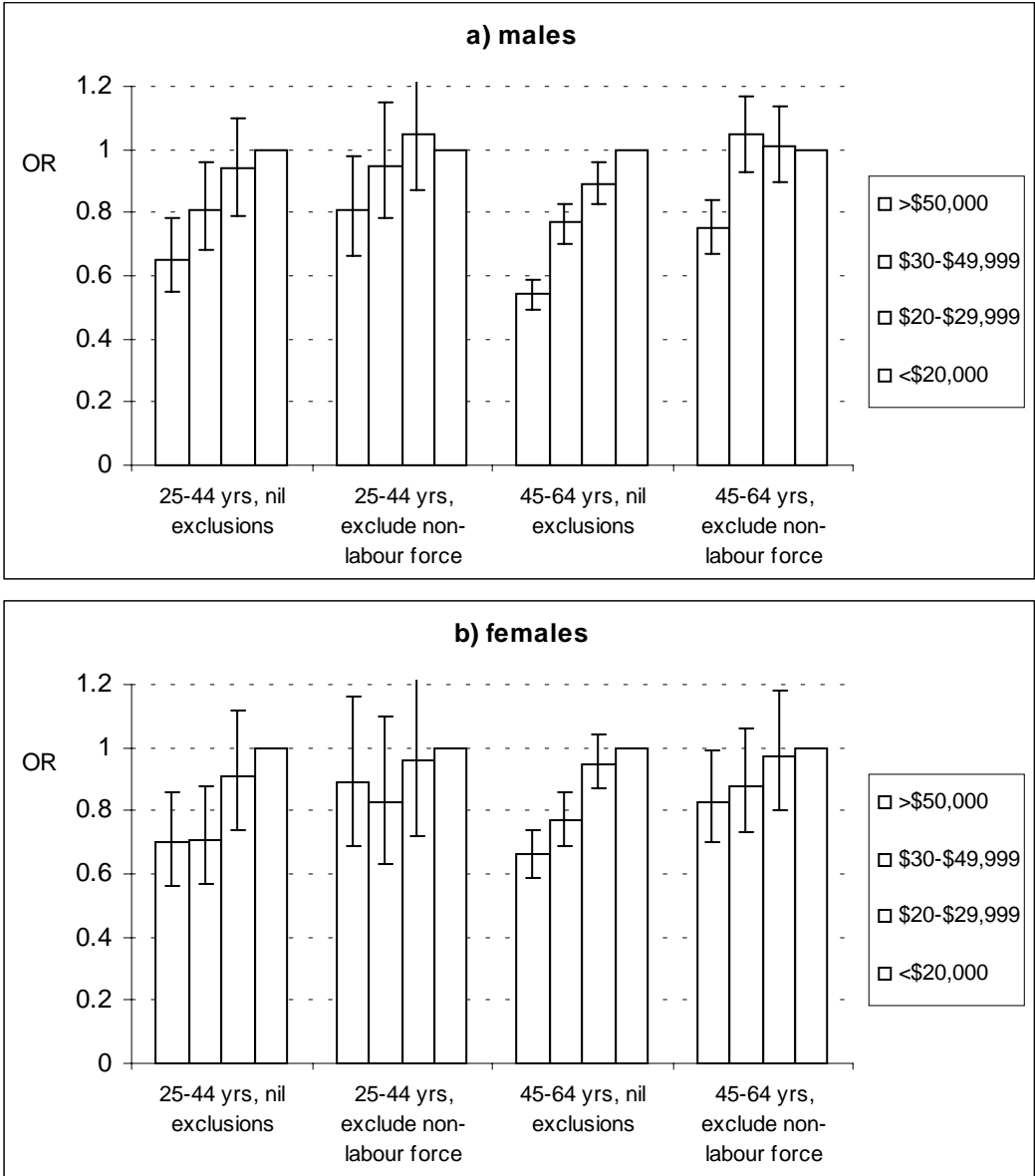
- 44% among 25–44 year old males, compared to 14% for the quintile 5 compared to quintile 1 deprivation (Table 57) and 14% for the tertiary qualification compared to nil qualification (Table 59) reductions to the null following exclusion of the non-labour force, respectively
- 47% among 45–64 year old males, compared to 43% and 22% for deprivation and highest qualification
- 65% among 25–44 year old females, compared to 32% and 25% for deprivation and highest qualification
- 51% among 45–64 year old females, compared to 53% and 40% for deprivation and highest qualification.

Figure 50 (below) plots the income mortality gradient for each sex by age group, before and after excluding the non-labour force, using the odds ratios and 95% confidence intervals in Table 62, page 247. For males, the figure highlights that after excluding the non-labour force only the highest income category ($\geq \$50,000$ equivalised household income) had a notable and statistically significantly lower mortality risk than the poorest household income category ($< \$20,000$). Moreover, there was essentially little difference between the three income categories up to $\$50,000$. For females, a suggestion of a gradient remains after excluding the non-labour force, but the 95% confidence intervals nearly all included 1.0. For both males and females, the patterns shown in Figure 50 may be affected by linkage (and selection) bias, such that a small gradient might have remained after excluding the non-labour force if there had been no linkage bias. However, on balance, linkage bias would have been unlikely to alter the substantive interpretation that excluding the non-labour force dramatically reduces the income mortality gradient – more so than for NZDep91 and highest qualification.

6.3.2 Cause-specific mortality

The effect of excluding the non-labour force on the income mortality gradient varied markedly by cause of death. The crude floating risk ratios for 25–64 year olds combined for all labour force categories and excluding the non-active labour force are shown in Figure 48, page 248 for males and Figure 49, page 249 for females. As with the comparable figure using crude risk ratios for various exclusions for small area deprivation (Figure 31), there is some confounding by age and ethnicity. For example, a stronger association of income and cancer is depicted by the crude risk ratios in Figure 48 for males than in the age and ethnicity-adjusted odds ratios in Table 40. While this confounding means that the gradients shown in Figure 48 and Figure 49 are not exactly correct, it does not invalidate a comparison of the relative changes in the gradient for the various exclusions.

Figure 50: Odds ratios of all-cause mortality for 25–44 and 45–64 year old males and females by equivalised household income for the restricted cohort with no exclusions and excluding the non-labour force



Note: Error bars are 95% confidence intervals.

The main finding in Figure 48 for males was that the exclusion of the non-labour force dramatically reduced the gradient for cancer and cardiovascular disease, moderately reduced the gradient for suicide deaths, but did not alter the unintentional injury gradient. This pattern is consistent with that observed for the small area deprivation gradients after excluding the non-labour force (Figure 31), although the reductions of the cancer and cardiovascular disease gradients for income were greater than for deprivation.

6.4 Conclusion

There was a strong univariate association of equivalised household income with all-cause mortality, and most specific causes of mortality. These associations were probably not greatly affected by selection bias, and somewhat underestimated due to linkage bias (see Section 6.1).

The two major difficulties for the income analyses were determining: (a) whether health selection affected the income–mortality gradients, and (b) what was causing the income–mortality gradients to decrease dramatically following the exclusion of the non-active labour force.

Theoretically, there were two possible types of health selection: *drift* health selection and *differential* health selection. Further, we would only expect health selection to operate for causes of death where a period of poor health is common before death (eg, cancer and cardiovascular disease). Finally, the health selection is framed as being a bias over the short-term (ie, a couple of years). The mortality risk plots over time in this chapter found some evidence of *differential* health selection for the association of income with cancer among males, but not for the association of income with other causes of death. Regarding *drift* health selection, there was some occasional evidence from the mortality risk plots and exclusions of sickness beneficiaries and pre-hospitalised deaths, but it was patchy, inconsistent, and not usually notably different from the ‘baseline’ NZDep91 and education analyses. Thus, the tests of health selection in this chapter, and compared to baseline analyses in previous chapters, did not strongly suggest health selection *over the short term*.

How can this conclusion that the income–mortality association was little affected by health selection be reconciled with the larger reductions in the income–mortality gradient following exclusion of the non-active labour force than for NZDep91 and highest qualification? In short, not easily.

There are five possible ways that excluding the non-active labour force might decrease the income–mortality association:

- 1 *Drift health selection, whereby labour force status is a proxy for health status.* While the above sensitivity analyses suggested little drift health selection by income in the short-term, it may be that:
 - the ‘tests’ for short-term drift health selection used were too crude
 - drift health selection was acting over a longer period than the three years observable in the NZCMS
 - while short-term drift health selection of the income–mortality association was modest, it was enough in combination with the reasons listed below to drive the income–mortality association further to the null than the associations of other socioeconomic factors with mortality.
- 2 *Differential health selection, whereby labour force status is a proxy for health status.* This may also have been one reason, but certainly not the major reason as discussed above. Also, differential health selection, if present, should apply to all socioeconomic factors – not just income.

- 3 *Confounding by labour force status* (by means other than short-term drift health selection). Such confounding was undoubtedly occurring, and for good reason, given the strong correlation between labour force status and income. But if it was the major reason for the reduction of the income-gradient, more notable reductions for the injury (and suicide) gradients – not just the cancer and cardiovascular disease gradients – would have been expected.
- 4 *As a proxy for health status – an intermediary variable between income and mortality*. This mechanism is different from drift health selection, where health status actually influences the socioeconomic factor of interest (ie, reverse causation). Rather, this possible mechanism is common to all socioeconomic factors, and involves over-controlling the association of a socioeconomic factor with mortality by the inclusion of a proxy for an intermediary variable (ie, health status). It is not clear why this mechanism would reduce the income gradient more than, say, the education gradient.
- 5 *Effect modification of the income–mortality gradient by labour force status* by mechanisms other than differential health selection, such that the gradient is weaker among the active labour force. It is not clear why this mechanism should be more important for income than other socioeconomic factors.

On balance, and putting aside the fifth reason, it seems likely that a *combination* of the first four reasons explains why the income gradient diminishes dramatically when excluding the non-active labour force. There was a similar pattern of varying reduction in the cause-specific mortality gradients by NZDep91 (Section 1.4) – although not as marked as that for income. Thus, a moderate variation in the mix of the first four reasons between NZDep91 and income may have been enough to make the difference. It is interesting to speculate that a modest amount of drift health selection for income, either over a period longer than three years or simply not reliably detected by the sensitivity analyses in this report, may be enough to make the difference between the NZDep91 and income analyses. However, it is impossible to be more precise in drawing conclusions.