Bias From Using Occupational Smoking Prevalence to Adjust Occupational Incidence Cohort Lung Cancer Mortality Rates

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Objective: To describe how smoking correction factors based on comparing worker smoking prevalence with population smoking prevalence are biased if applied to an occupational incidence cohort. **Methods:** Relative rates of smoking for shorter-tenure workers derived from occupational cohort lung cancer studies were applied to incidence and prevalence population tenure distributions to calculate relative smoking estimates. **Results:** High smoking rates in short-tenure workers have little effect on prevalent worker rates (relative smoking estimates, 1.04 and 1.02) and much larger effect in occupational incidence populations (relative smoking estimates, 1.58 and 1.21), which have a much higher proportion of short tenure-workers. **Conclusions:** Smoking correction estimates derived from surveys of smoking habits in prevalent workers may introduce bias when applied to incidence workers because of very different proportions of short-tenure workers (length-time biased sampling).

Efforts were made to indirectly assess the influence of tobacco smoking in two lung cancer mortality studies^{1,2} of beryllium workers the great majority of whom were first employed in the period 1940 through 1969 (incidence cohort). Both studies used data on smoking in certain beryllium plants collected in a survey performed in 1968. The survey results weighted by representative risk ratios for smoking classes were compared with similarly weighted US smoking data collected in 1964 to 1965 and a correction factor calculated. The first study concluded that beryllium workers totaled more tobacco-related lung cancer risk than the norm for the United States and recommended dividing the beryllium worker lung cancer standard mortality ratios (SMRs) by 1.13 to correct for this. The second study concluded that beryllium worker tobacco-related lung cancer risk was very close to the US risk, so correction was not needed.

Bias might be introduced in these procedures by the difference in the employment tenure distribution in a cohort defined by being hired over a defined span of time (incidence cohort) and that of a cohort defined by being employed and surveyed at a point of time (prevalence cohort). Occupational prevalence cohorts will generally differ from incidence cohorts by having higher proportions of long-tenure subjects and lower proportions of shorter-tenure subjects, a length-time biased sampling effect. If the smoking rate in shorter-tenure subjects was higher than in long-tenure subjects, a correction factor derived for a prevalence cohort (low proportion short tenure subjects) would introduce bias if applied to an incidence cohort (high proportion shorter tenure subjects).

The goal of this study is to illustrate the differences in tenure distributions in occupational incidence and prevalence cohorts, to illustrate the association of smoking practices with employment tenure, and to show examples of the magnitude of the predicted bias.

METHODS

We defined an occupational incidence cohort as the set of all workers hired over a defined span of time. We used data from a beryllium worker cohort supplied to us by the National Institute for Occupational Safety and Health under a data use agreement. The occupational incidence cohort consisted of 5369 workers in three beryllium plants who were first hired and worked at least 2 days from January 1, 1940, through December 31, 1969.

We defined a prevalence subcohort as the subset of the incidence cohort that was employed on an arbitrary given date, April 1, 1968.

We sorted both the entire incidence cohort and the prevalence subcohort into “final tenure” categories. Final tenure was defined as the date of termination or December 31, 2005, if still employed, minus the date of hire. We also sorted the prevalence cohort by “prevalent tenure,” defined as April 1, 1968, minus the date of hire. The distributions by tenure category are shown in Table 1.

We were aware of studies^{3–7} of higher rates of mortality, and specifically of lung cancer mortality, in short-tenure workers, which commonly produce J- or U-shaped plots when lung cancer and all-cause standardized mortality ratios are plotted against employment tenure. An example of silica-exposed sand workers⁸ is shown in Fig. 1. The beginning of the “J/U” is produced by relatively high SMRs for lung cancer in workers with 1 year or less tenure, lower but still elevated SMRs in 1- to 5-year tenure workers, with lower SMRs close to 1.0 at 5- to 10-year tenure, and a subsequent rise in SMRs in longer-tenure workers.

We made the assumption that the higher SMRs in less than 1 year and 1- to 4-year tenure workers were not due to exposures on the job but rather were due to cigarette smoking and were proportional to smoking rates. We then used the SMRs to estimate the relative smoking rates in these two groups. We then assumed that for tenure more than 5 years the rising mortality rates were related to increasing workplace exposure, for example, silica in the sand workers rather than to increased smoking. We therefore assigned a relative smoking rate of 1.0 to all workers with tenure over 5 years. We then used examples of these rates from two studies^{1,8} to create relative smoking estimates for the incidence cohort and the prevalent subcohort. We calculated the average relative smoking rate adjusted for the proportion of person-years in the final tenure categories 1 year or less, 1 to 4 years, and more than 5 years. The results of these calculations are shown in Table 2.

RESULTS

The percentages of person-years in the incidence cohort (Table 1 and Fig. 2) with final tenure under 1 and 5 years were 46.5% and 67.9%, respectively, compared with 1.6% and 7.7% in the prevalent subcohort. The percentages of person-years in the incidence cohort with final tenure over 30 and 40 years were 10.1% and 1.9%, respectively, compared with 36.9% and 7.2% in the prevalent subcohort.

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TABLE 1. Distribution of Employment Tenure in an Incidence Cohort (Hired From 1940 Through 1969) and the Prevalent Subcohort (Employed on April 1, 1968)*

Tenure Category, yrs	Incidence Cohort			Prevalent Subcohort		
	Final Tenure			Final Tenure		Prevalent Tenure
	Mean Tenure	n (%)	Person-yrs (%)	n (%)	Person-yrs (%)	n (%)
<1	0.30	2,532 (47.2)	94,677 (46.5)	26 (2.0)	868.27 (1.6)	131 (9.9)
1–4	2.42	1,167 (21.7)	43,626 (21.4)	93 (7.0)	3,290.41 (6.1)	255 (19.2)
5–9	7.08	402 (7.5)	14,188 (7.0)	117 (8.8)	3,993 (7.5)	446 (33.6)
10–14	12.33	242 (4.5)	8,464 (4.2)	170 (12.8)	6,232.76 (11.6)	266 (20.1)
15–19	17.60	219 (4.1)	7,799 (3.8)	179 (13.5)	6,613.43 (12.3)	110 (8.3)
20–24	22.13	200 (3.7)	7,858 (3.9)	178 (13.4)	7,113.11 (13.3)	42 (3.2)
25–29	27.92	166 (3.1)	6,578 (3.2)	142 (10.7)	5,705.23 (10.7)	76 (5.7)
30–34	32.47	182 (3.4)	8,106 (4.0)	165 (12.4)	7,473.06 (14.0)	0 (0.0)
35–39	37.43	182 (3.4)	8,539 (4.2)	179 (13.5)	8,427.19 (15.7)	0 (0.0)
>40	42.54	77 (1.4)	3,847 (1.9)	77 (5.8)	3,846.79 (7.2)	0 (0.0)
Total	7.14	5,369 (100)	203,682 (100)	1,326 (100)	53,563.30 (100)	1,326 (100)

*Prevalent cohort tenure is shown as both final tenure and prevalent tenure (ie, tenure as of April 1, 1968).

FIGURE 1. An example of the J- or U-shaped relationship of SMRs for lung cancer and all-cause mortality plotted against tenure for sand workers with silica exposure.⁸

Comparison of the April 1, 1968, prevalent tenure to the final tenure in the prevalence subcohort reveals striking differences. Of the 131 subjects in their first year of tenure on April 1, 1968, only 26 (20%) had a final tenure 1 year or less. The prevalence subcohort prevalent tenure distribution on April 1, 1968, differed substantially from the final tenure distributions of both the prevalence sub-cohort and the incidence cohort.

The assumed relative smoking rates for final tenure less than 1 year, 1 to 4 years, and more than 5 years, when weighted by the proportion of person-years in each category in the occupational incidence cohort versus the occupational prevalence cohort, gave different estimates for the total relative smoking rate in the two examples—for example, 1.58 in the sand workers and 1.21 in the beryllium workers (Table 2). The estimates using the prevalence subcohort person-year weights were much lower, 1.04 and 1.02, respectively.

DISCUSSION

These data show striking discrepancies between the tenure distributions of an occupational incidence cohort population compared with a prevalence subcohort. Furthermore, the distribution of prevalent tenure in the prevalence subcohort did not predict final tenure, other than the obvious fact that final tenure for any individual worker cannot be less than the prevalent tenure. Even with tenure known at the time of the 1968 smoking prevalence survey, it would shed little light on the final tenure of the surveyed workers.

Classifying occupational cohorts as either incidence, prevalence, or a combination raises the question, what is the US population at a point in time? A cross-sectional survey of the US population defines a prevalence cohort that can be thought of as an incidence population (birth and immigration) selected by mortality and emigration. In the United States, emigration overall is small and mortality modest until age 60 years and more. The occupational prevalence subcohort is an occupational incidence cohort created entirely by immigration (hire) and then selected by mortality and very heavily by high rates of emigration (termination). Thus, an occupational prevalence subcohort is different in construction from both the US prevalence population and the occupational incidence cohort, which in turn are different from each other.

Comparing the prevalent smoking rates in the surveyed prevalence subcohort weighted by estimates of lung cancer to similarly weighted prevalent smoking rates of the US population can be used to calculate a correction factor, but this correction factor cannot be applied to the incidence cohort. The incidence subcohort is dissimilar from both the prevalence subcohort and the prevalent US population with respect to many characteristics, in particular in the proportion of short-tenure workers compared with the prevalence subcohort. This difference alone combined with higher rates of smoking in short-tenure workers would make the correction factor biased on the low side. Overall, the comparison of the prevalence subcohort to the US population and applying it to the incidence cohort are akin to comparing an apple to an orange and applying the difference to a banana.

We have made two very large assumptions that short-tenure workers have higher rates of smoking than longer-tenure workers and that this higher rate of smoking in short-tenure workers can be estimated from higher rates of lung cancer. Shorter-tenure workers would have higher rates of smoking than longer-tenure workers hired at the same time only if smokers left employment early at higher rates than nonsmokers. Two articles^{9,10} suggest they do. A study of US Air Force recruits⁹ found that the rate of discharge in the first year was 19.4% in smokers and 11.8% in nonsmokers. From an initial smoking prevalence of 28.5, it can be calculated that the smoking rate in those who left the first year was 39.6 and of those that remained, 26.7, a ratio of 1.48. A study of an incidence cohort of US Navy women recruits¹⁰ found that daily smokers had first-year attrition of 13.8% compared with 7.3% for nonsmokers. Smokers were also less

TABLE 2. Calculation of Relative Smoking Rates for Incidence Cohorts and Prevalence Subcohorts With Assumed Smoking Relative Rates Proportional to SMRs for Less Than 1 and 1- to 5-Year Tenure Workers*

	Final Tenure Category	Sand Workers ⁸ (Steenland)	Reading Plant Beryllium Workers ¹
Relative smoking rates	<1	2.09	1.38
	1–5	1.33	1.15
	>5	1.00	1.00
Incidence cohort Person-years proportion	<1	0.46	0.46
	1–5	0.21	0.21
	>5	0.32	0.32
Overall relative smoking rate		1.58	1.21
Prevalent subcohort person-years proportion	<1	0.02	0.02
	1–5	0.06	0.06
	>5	0.92	0.92
Overall relative smoking rate		1.04	1.02

* A relative smoking rate of 1.0 was assigned to workers with more than 5-year tenure.

FIGURE 2. Comparison of the percentage of subjects in employment tenure categories on the basis of Table 1. The first two are final tenure for the incidence cohort and the prevalent subcohort and the third is the prevalent tenure (tenure as of April 1, 1968) of the prevalent subcohort.

likely to complete their term (45.5% vs 62.8%), and less likely to reenlist (21% vs 33%). Thus, higher attrition of smokers extends in this study beyond the first year of tenure into the 1- to 5-year tenure range. These two studies support the assumption in this article that short-tenure workers have higher rates of smoking than long-tenure workers. Also to be noted is the lack of smoking rate information on persons who terminate employment after tenure longer than from 1 to a few years. Our assumption of no variation in smoking rates in those leaving employment with tenure from 5 to 40 years or more serves the purposes of our illustrations and is often assumed in occupational epidemiology, but is without foundation.

The second assumption that relative rates of lung cancer could be used to suggest relative rates of smoking in short-tenure workers seems on the face to be plausible. It would not be foolish to be concerned that the 1.48 higher rate of smoking in recruits leaving the Air Force in the first year might predict a similarly higher future rate of lung cancer. Neither would it be foolish to conclude that if these subjects did have a higher rate of lung cancer it could be attributable to their higher rate of smoking. Higher disease rates and higher mortality in short-tenure workers have been attributed to either “life style” or “other job exposures.” Smoking and alcohol are among the primary lifestyle factors relatable to excess poor health outcomes in short-tenure workers and are themselves

correlated. Ill-health preceding employment⁷ including accidents, violence, and cardiovascular disease is also predictors of early termination and associated with alcohol and smoking. An analysis of mortality patterns⁵ found that short-term compared with long-term workers had excess deaths because of diseases of the circulatory system, arteriosclerotic heart disease, emphysema, diseases of the digestive system, cirrhosis of the liver, motor vehicle accidents, suicide, and cancers of the stomach, colon, lung, prostate, and brain, of which several are associated with cigarette smoking and alcohol excess.

The other hypothesis of why short-tenure workers have higher rates of lung cancer and other diseases is that short-tenure workers have exposures to carcinogens and other harmful substances in jobs before and/or after their short tenure. Persons who leave employment after short tenure in one industry certainly have a greater opportunity to take or to have already taken another job with occupational lung carcinogen exposure. A very small study⁴ that classified prior employment history as either having or not having risk of lung cancer found no difference in the lung cancer SMR of less than 1-year tenure workers subclassified by their prior work history. The proportion of less than 1-year tenure workers with lung cancer with a prior work history associated with lung cancer risk (3/6) was lower than that of longer-tenure workers with lung cancer (6/6).

Subdivision of less than 1-year workers typically shows decreasing mortality as tenure rises from less than 1 to 12 months,⁶ which is likely not attributable to different availability for other employment. To produce a U-shaped mortality pattern with a nadir at 5- to 10-year tenure in studies of putative lung carcinogens, it has to be true that 5 to 10 years of exposure to the putative carcinogen environment conveys less lung cancer risk than the same 5 to 10 years employed elsewhere, on average. This seems unlikely. The hypothesis that the higher rate of lung cancer in very short-tenure workers is due to carcinogen exposure in other jobs deserves further study, but at present has little objective support.

Smokers tend to be underemployed, and unemployed persons smoke more than employed persons.¹¹ The higher rate of lung cancer in short-tenure workers may be related not only to their smoking at the time of employment but also to higher rates of smoking while unemployed before and after a short period of employment. An Italian population cross-sectional study¹¹ of employment and smoking suggested that smoking was positively associated with social stressors and negatively with personal coping and social support resources in a two-way interaction. This is consistent with the wide range of negative performance indicators in military recruits who are smokers.¹⁰ The picture of the pool of persons available for employment is that it

contains a fraction of individuals with characteristics that inhibit success in military and industrial employment and these characteristics are associated with smoking. When hired into jobs, persons with these characteristics (including smoking) are more likely to exit employment after short tenures and return to unemployed status, perpetuating both the higher rate of smoking in the unemployed and in the short-tenure ex-worker.

CONCLUSIONS

The practice of comparing the prevalence of smoking in occupational prevalence subcohorts of workers with US rates and then applying a correction factor to an occupational incidence cohort can be biased due to the underrepresentation of short-tenure workers in prevalence cohorts and the higher rate of smoking in short-tenure workers. Correction factors on the basis of prevalence subcohorts likely substantially underestimate the smoking rates of incidence cohorts, which have much higher proportions of short-tenure workers.

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