Response to Reviewers’ comments

We have found the reviewers' comments very useful and constructive and have responded to them as follows:

Richard O’Connor

1. The potential for bias in the administration of questionnaires in the workplace and home is noted as a limitation. Page 17

2. These variables were dichotomised on the advice of a statistician because although the data were normally distributed, there was "clumping" of data at certain points eg in salaries around the 100s and it was felt that the use of continuous variables was not suitable in this case. Because of the large sample size the power was though to be more than adequate.

3. The point that 49% of all respondents smoke more than 10/day has been clarified. Page 9

4. "the relatively low prevalence ...." This sentence has been clarified Page 9.

5. We have estimated an average cost per cigarette smoked in the three groups. Page 12

6. The fact that exploratory questions were only asked of moderate and heavy smokers because it was felt that light smoking would not impact very much on other household expenditures, but we have is mentioned as a limitation. Page 17.

7. The name of the Treaty has been changed. Page 18

8. The limitations have been moved to the end of the discussion. Page 17

Jonathan Samet

1. The sampling and selection processes for both urban and rural sites are now described in much greater detail. Page 5-6

2. The unadjusted odds ratios do come from univariate analysis (Pearson's Chi-Squared as stated on Page 8). The adjusted model includes only age, residence and the variable and not all the variables. This is now clarified on page 8. We take the point about the simplicity of P values but the odds ratios, we believe adds important information about the magnitude (strength) of the relationships.

3. What this survey adds is its focus on lower socio-economic status groups and its comprehensive approach. We have not seen any other studies which include data on prevalence, cessation, knowledge and expenditure. We are not attempting to contribute to scientific evidence for tobacco control measures,
but we believe that different population groups might benefit from different combinations of approaches and that given limited resources for tobacco control selecting the most effective interventions is crucial. We found, for example, that knowledge about personal health risk was high, but awareness of dangers of passive smoking is low. The latter should therefore be a focus for health education interventions. In addition knowledge of the existence of nicotine replacement is non-existent, when this has been shown to be very effective elsewhere. Additional comments about the tobacco control measures in Zhejiang are added on pages 16, 17 and 18.

4. (Minor essential revision 1) I have made statements about decline more cautious. However, the fact that our prevalence figures are from the traditionally highest prevalence group and they are still lower that the sentinel surveillance figures does suggest that a reduction has occurred.

Xinguang Chen

1. The study was carried-out to compare a range of health-related issues in migrant workers, poor urban workers and rural workers, all unskilled and low income. The reason for comparing the groups was because previous studies such as Chen et al 2004 present data from one of these groups (migrants) without comparing with other groups making it difficult to interpret and to decide which groups (if any) need to be targeted for tobacco control.

2. We believe this study is very justified. One of the major issues in tobacco control today is that those in lower socio-economic groups are most likely to smoke and least likely to quit, as stated in the first paragraph of the paper. Therefore to explore smoking behaviours in this group has great policy relevance. We chose to study the three distinct poor groups in Chinese society: rural workers, unskilled urban and migrant workers. We have added the word "unskilled" to the background to help clarify this. (page 5). It is already mentioned in the methods.

For Zhejiang Province these are all low income groups. The urban group while earning considerably more than the rural is low income by urban standards. The cost of living in a wealthy city like Hangzhou is very much higher than in poor Western Zhejiang. The problems of using income as a measure of economic status across urban and rural, when purchasing power varies hugely is mentioned as a limitation.

We are not clear what the point is about a review of published studies on smoking among rural migrants. We have cited the Chen paper which is not a review and we could find no other peer-reviewed papers which specifically address this question. The statement of the reviewer that the findings "seemingly imply that smoking among unskilled workers in urban setting might be an important public health problem" is strange since it is the rural workers who smoked the most.

Of course poverty alleviation may in the long term be one measure which will lead to reduction in smoking but there is much that can be done in the short term and we are NOT implying that poverty alleviation is the answer. It is simply a fact in most countries that the poor smoke more.

3. The point here is that some tobacco control measures are relevant across all groups eg increasing awareness of passive smoking and improving access to cessation support. But we also recommend that rural areas which have hitherto been much ignored in tobacco control measures receive some attention in this regard. The focus of the paper is not unskilled workers in urban settings but is about all three groups equally.

4. The sampling and data collection have now been described in more detail. Pages 5-7. We are not saying, as is stated in the limitations, that this study is representative for other parts of China. In sampling an inner city and a suburban district and two poor counties the results are at least indicative of the situation in Hangzhou and Western Zhejiang. It is a truism of research that sampling is may lead to bias (not error, as the reviewer states). The fact that some urban-based respondents completed the questionnaire at workplaces and rural respondents at home is noted as a limitation. The types of workunit are shown in Table 1 under occupation. We have added a footnote about the different types of factory which were included.

5. In the analysis section current and heavy smoking are clearly defined. (Page 7) Current means at least one cigarette per day, as stated, so the answer to the question is that one cigarette per month does NOT mean current smoking. Definitions of quitting and attempted quitting are now added. (Page 7) But we do note on P10 the problem with the use of the term quitting because 52% of the quitters had not classified themselves as regular smokers.
As noted on Page 7 the major questions used were from the 1996 national survey. The questionnaire was developed in Chinese and so there were no translation issues.

6. We have re-worked the tables at the reviewer's suggestion so that there are now five tables with occupational and age variables for all male smokers presented separately. We agree that this mode of presentation makes much more sense.

7. We have been through the paper and have added some citations where we think necessary eg to the background on Page 5 and about hukou. Page 6

8. More description of the study site has been added. Page 5. "provincial" has been added (Page 5) and women to female (Page 7). Hukou is explained page 6 and a reference has been added. This answers better the question about the difference between the urban population and migrants. We think the unadjusted odd ratios are also of interest and think they should remain. For example the finding that education is not significant even before adjustment is worth reporting. It is true that the large sample size will make more relationships significant. But this gives greater credibility and validity to the large number of adjusted odds ratios which are not significant.

The point we were making about the sample being from low income groups was to explain why the prevalence of smoking was higher in our study than in the two most recent studies in Hangzhou, which included all income groups. It was not to make a point about the relationship between SES and smoking which is of course an inverse one.

We agree that the prevalence of smoking might be lower in migrants in formal occupations, but the latter account for over 95% of the migrant workforce in Hangzhou, so these workers are representative of the overwhelming majority. However, this is an important point and has now been added as a limitation.

Joy de Beyer

1. We agree that it would be good to put cessation in the title and so have changed "heavy smoking" to "cessation" because to leave both in makes the title too cumbersome.

2. Yes, reference 29 got missed! It has now been added together with the year in the text for which we have a figure. We have searched with no success in international and Chinese literature to find a more recent figure but have failed.

3. The paper has been re-proofed to pick-up all typos.

4. The phrase has been changed (in the abstract).

5. We totally agree about the complexities around price elasticity and the effects on the poor. We have expanded the latter point to clarify that the poor may be more affected because evidence from elsewhere suggests they are less likely to quit (P 16). We did not mean because of the absolute increases in the cost of cheap and more expensive cigarettes which would come from higher percentage taxation.