Reviewer's report

Title: Mortality and morbidity among people living close to incinerators: a cohort study based on dispersion modeling for exposure assessment

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Reviewer: Pascal Empereur-Bissonnet

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Major Compulsory Revisions

Why did the authors define a priori the study area as a 3.5 km radius circle around the 2 incinerators, despite the atmospheric dispersion model predicts larger exposures? See figures 2a-c where the plumes (from incinerators’ stacks and traffic axes) widely go out of the study area.

“Heavy metals” are never defined. What does exactly include this term? It is a very sensitive point because municipal and hospital waste incinerators do not release the same metals (in nature and in quantity/flow), and characteristics of the emitted chemicals have to be input in the dispersion model for calculation.

What precisely is the exposure indicator? Is it an atmospheric concentration? Is it the “total air concentration” (gaseous and particle concentration)? Is it a mean concentration over one year, or over the whole period of exposure? Did this indicator take into account the accumulation of persistent pollutants in the environment such as metals, dioxins, PAHs, etc.

Why authors did not take into account the potential confounder that is « rural or urban » status? In this study, where « most of the area … is used for agriculture », it should be essential because 1) we know that in rural areas self-production is very common (of vegetables, poultry, eggs, milk, meat,… which are pathways of human exposure); 2) several organic and non-organic agents emitted from waste incinerators are transferred into the food chain (metals, dioxins, PAHs…) and are persistent in the environment.

Authors present this work as a « cohort study » but, except the health status (and residential history), they did not collect data at an individual level, especially concerning very important confounders such as tobacco smoke, alcohol consumption, occupational exposure, type and origin of food, socioeconomic status, residential history and NO2 (road and industry) exposure. So, actually, this study has a hybrid design somewhere between a cohort study and (close to) an ecological study.

Despite authors said they took into account the exposure to road traffic and other local industrial sources of air pollution (expressed by the NO2 air concentration), the results shown do not include this strong confounder, even this factor seems to be very influent: see results on table 3 where the risks of respiratory and chronic pulmonary diseases trends to decrease with incinerator exposure that is inversely correlated with NO2 exposure. What are this results taking unto
account the NO2 exposure?

Previous epidemiological studies showed that exposure to incinerators is not a strong factor of health risk of cancers or others diseases (Relative Risk < 2). Is this study, including less than 500,000 person-years, able to show any health effect in the exposed population? We would appreciate a discussion on this key point.

Finally, authors made an error of interpretation when they try to explain the discrepancy between the observed effect on mortality and their no observed excess of incidence (page 14). The two studied waste incinerator plants (municipal and hospital) were still in function, so people exposure to their release continues and the health effect of this persistent stress goes on, permanently expressed by the occurrence of new cases of acute or chronic diseases. So, there is no reason to observe, currently, an excess of mortality not associated with an excess of incidence.

Minor Essential Revisions

In the “Methods” section, authors referred to a previous study aiming to validate predictions from the ADMS dispersion model, but the protocol of validation is not very clear.

Since atmospheric dispersion model outputs are very sensitive to the input parameters related to the studied pollutant(s), it should be useful to cite and describe what were the “heavy metals” characteristics used in this study.

Symbol of statistical significance (stars) are missing on tables 3 and 4.

Authors are too conclusive in two sections. First, when they explain that all-cancer mortality in women was "mainly due to a gradient of increasing risk for stomach, colon, liver, breast…” (page 13), despite they did not have a look on all cancer types. Second, when they wondered that the excess of pleural cancer is "probably due to an occupational exposure to asbestos in the small industrial area” (page 17), but not gave arguments in favor of this assumption.

Discretionary Revisions

Nothing to declare.

**Level of interest:** An article of limited interest

**Quality of written English:** Needs some language corrections before being published

**Statistical review:** Yes, but I do not feel adequately qualified to assess the statistics.

**Declaration of competing interests:**

I declare that I have no competing interests.