

What is already known on this topic?

Depression and cognitive impairment often occur together in old age

The temporal relation between depression and cognitive impairment is unclear

What this study adds?

Impairment of attention or memory in old age precedes the development of depressive symptoms

Presence of depressive symptoms, however, is not related to accelerated cognitive decline

aged 65 and older showing that depression is an early manifestation rather than a predictor of Alzheimer's disease.¹⁹ Thus, in elderly people the presence of depressive symptoms does not mean that they are at increased risk of cognitive decline.

Conclusions

We found that cognitive decline preceded depression in old age—specifically impairment of attention or memory preceded the development of depressive symptoms. Depression seems to be a concomitant symptom of cognitive impairment rather than an independent risk factor. Therefore, care givers should pay special attention to early detection and treatment of depressive symptoms in elderly people with cognitive impairment.

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Issues in the reporting of epidemiological studies: a survey of recent practice

Stuart J Pocock, Timothy J Collier, Kimberly J Dandreo, Bianca L de Stavola, Marlene B Goldman, Leslie A Kalish, Linda E Kasten, Valerie A McCormack

Abstract

Objectives To review current practice in the analysis and reporting of epidemiological research and to identify limitations.

Design Examination of articles published in January 2001 that investigated associations between risk factors/exposure variables and disease events/measures in individuals.

Setting Eligible English language journals including all major epidemiological journals, all major general medical journals, and the two leading journals in cardiovascular disease and cancer.

Main outcome measure Each article was evaluated with a standard proforma.

Results We found 73 articles in observational epidemiology; most were either cohort or case-control studies. Most studies looked at cancer and cardiovascular disease, even after we excluded specialty journals. Quantitative exposure variables predominated, which were mostly analysed as ordered categories but with little consistency or explanation regarding choice of categories. Sample selection, participant refusal, and data quality received insufficient attention in many articles. Statistical analyses commonly used odds ratios (38 articles) and

Department of Epidemiology and Population Health, London School of Hygiene and Tropical Medicine, London WC1E 7HT
Stuart J Pocock
professor
Timothy J Collier
lecturer
Bianca L de Stavola
reader
Valerie A McCormack
lecturer

continued over



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New England Research Institutes,
9 Galen St,
Watertown, MA
02472, USA

Kimberley J Dandreo
epidemiologist

Department of Obstetrics and Gynaecology, Harvard Medical School and Beth Israel Deaconess Medical Center, Boston, MA 02215, USA

Marlene B Goldman
associate professor

Clinical Research Program, Children's Hospital, Boston, MA 02115, USA

Leslie A Kalish
associate professor

Prometrika LLC, Cambridge, MA 02138, USA

Linda E Kasten
senior statistician

Correspondence to: S Pocock
stuart.pocock@lshtm.ac.uk

hazard/rate ratios (23), with some inconsistent use of terminology. Confidence intervals were reported in most studies (68), though use of P values was less common (38). Few articles explained their choice of confounding variables; many performed subgroup analyses claiming an effect modifier, though interaction tests were rare. Several investigated multiple associations between exposure and outcome, increasing the likelihood of false positive claims. There was evidence of publication bias.

Conclusions This survey raises concerns regarding inadequacies in the analysis and reporting of epidemiological publications in mainstream journals.

Introduction

Observational epidemiology generates a plethora of publications across numerous epidemiological and medical journals. Many texts tackle the quality of epidemiological studies, but few directly focus on epidemiological publications. We reviewed the quality and methodological acceptability of research epidemiology published in January 2001. We concentrated on analytical epidemiology—that is, studies that used observational data on people from the general population to quantify relations between exposures and disease.

Methods

We selected three types of English language journals: public, environmental, and occupational health journals with impact factor >2 , plus the *International Journal of Epidemiology* and the *Journal of Epidemiology and Community Health*; general and internal medicine journals with impact factor >2 ; and *Circulation* and the *Journal of the National Cancer Institute*, being the highest impact journals in cardiovascular disease and cancer. We then identified eligible articles published in January 2001. Eligible epidemiological articles analysed observational data in individuals to quantify associations between risk factors and disease outcomes. We excluded randomised controlled trials; observational studies of treatments; studies of people with disease which do not relate to general population risk; and studies that did not investigate individual associations.

For each article items were extracted with a standard proforma (see bmj.com).

Results

We identified 73 eligible epidemiological articles in 20 journals: 54 were in epidemiology journals. North American and European populations were studied in 39 and 28 articles respectively. Only six articles studied populations in developing countries.

Study designs and outcomes

There were 37 cohort studies, 25 case-control studies, 10 cross sectional studies, and one case-cohort study. Twenty eight articles, mostly case-control studies, investigated cancers, and 16, mostly cohort studies, investigated cardiovascular disease. Even after we excluded the specialist cancer and cardiology journals, only 31% of articles studied other diseases. Most disease outcomes were binary: non-fatal events or

Table 1 Types of exposure variable investigated in 73 epidemiological publications

| | No of articles |
|---|----------------|
| Nature of principal exposure variable: | |
| Lifestyle | 11 |
| Environmental | 11 |
| Pre-existing condition | 9 |
| Diet | 8 |
| Biochemical | 7 |
| Physiological | 6 |
| Socioeconomic | 6 |
| Fetal | 6 |
| Genetic | 5 |
| Other | 4 |
| Statistical type of exposure variable*: | |
| Continuous quantitative measure | 15 |
| Ordered categorisation of quantitative | 42 |
| Binary categorisation of quantitative | 9 |
| Binary (yes/no) indicator | 34 |
| Ordered categorical item | 11 |
| Unordered categorical item | 4 |

*Some authors analysed principal exposure variable in more than one way.

deaths, or both. Seven articles investigated quantitative disease markers, and four studied multiple category outcomes (for example, disease severity).

Cohort studies varied in size from 317 to 1.5 million participants (median 5072), the largest studies using official databases. Most exceeded 10 years' follow up, though in two studies it was one year. Eight cohort studies gave no information on drop outs. Twenty four cohort study articles gave no information on refusals to participate, perhaps relying on earlier publications. Follow up entailed monitoring for disease outcomes (30 studies), planned visits (13 studies), and questionnaires (five studies). The number of principal outcome events ranged from 44 to 28 795 (median 414). Five studies had fewer than 100 events.

Case-control studies mostly used some matching of controls to cases (21 of 25 studies). Five were nested within a cohort study. Fifteen studies had more than one control per case. Cases were mostly from hospitals or clinics (11 studies) or population surveillance (eight studies). Controls were commonly from the general population (13 studies) or hospitals or clinics (seven studies). The number of cases varied from under 100 to over 8000 (median 347).

Cross sectional studies—Half of these selected random samples from the general population. Study size varied from under 100 to over 10 000 participants (median 1500).

The case-cohort study investigated alcohol consumption and bladder cancer, with 594 cases and a random subcohort of 3170 participants from a large total cohort.¹

Exposure variables

Lifestyle and dietary factors received much attention (19 articles), most concerning cardiovascular outcomes (table 1). Environmental factors were covered in 11 articles.

Fifty articles studied quantitative exposure variables. They were commonly grouped into several ordered categories (42 articles), with the number of categories and cut points justified in 22 articles. Nine articles used one cut point—that is, two categories. Fifteen articles used linear associations to model effects of

continuous exposure: only two reported checking for linearity. Some articles presented results in two ways—for example, 11 articles analysed exposures both as continuous variables and ordered categories. Binary (yes/no) exposures were reported in 34 articles, while 15 articles reported exposures naturally in several categories, 11 with evident ordering.

Confounders

In 67 articles (92%) statistical analyses were adjusted for potential confounders. The extent of adjustment varied enormously: the median number of variables was seven, and two studies adjusted for over 20 confounders. How confounders were chosen (that is, pre-declared, selected post hoc, or statistical algorithm) was mostly unclear. Eleven articles used stepwise regression to select variables for final analysis. Twenty one case-control studies matched on other factor(s), commonly matching on two or three variables. Five articles contained no control for confounders.

Statistical estimates and inferences

Odds ratios were estimated in 38 articles, including all case-control studies (table 2). Rate ratios or hazard ratios were estimated in 22 cohort studies. Methods included proportional hazard models, Poisson models, and person years analyses. Six cohort studies reported odds ratios: two were really rate ratios, two used pooled logistic regression for time updated variables,² and two lacked event times.

All but five articles used confidence intervals; 35 articles contained no P values and only 15 gave P values for all primary results. Papers in the journal *Epidemiology* contained no P values, according to editorial policy at the time,^{3,4} which has since changed.⁵ Two articles presented neither P values nor confidence intervals, while 10 articles gave both throughout.

The figure shows the distribution of P values for the first reported result in each article's abstract. Twenty six articles had $0.01 < P < 0.05$ —that is, their first result achieved modest significance.

Effect modifiers

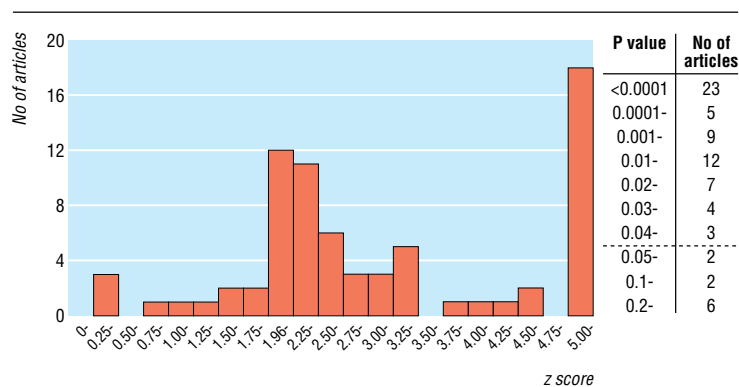
Forty three articles contained subgroup analyses, 34 of which claimed possible effect modification. Only eight articles gave statistical tests for interaction. In one paper the result of the interaction test was not significant but the authors still claimed a synergistic interaction.

Multiple analyses

One problem is that some articles investigated many associations between exposure and outcome, which are often hard to interpret as few authors predefine all study hypotheses and many selectively report "interesting" associations. Ten articles each reported over 100 estimates of the effect of exposure and outcome (the highest was 264). Some articles selectively emphasised the most significant associations, inflating the risk of false positive results through multiple hypothesis testing.

Refusals

Thirty four studies (47%) gave no information on the number of participants who refused to participate.



Distribution of P values for first primary result in each article and corresponding absolute values of standardised normal deviates z (two sided $P=0.05$, 0.01, 0.001, and 0.0001 correspond to $z=1.96$, 2.58, 3.29, and 3.89, respectively)

Discussion

Our survey into the current state of epidemiological publications in high impact journals raises concerns regarding aspects of study design, analysis, and reporting that could lead to misleading results in some publications. Our focus on high impact epidemiological and general medical journals has by design under-represented epidemiology in the many specialist medical journals. Our sample articles may be better quality as journals that publish only occasional epidemiological articles may be less discriminating.⁶

Types of study

Research on cancer and cardiovascular disease dominates published epidemiology, and few articles were concerned with developing countries. Cohort studies were the most common design, especially for cardiovascular disease, while the case-control design was appropriately chosen for rarer outcomes—for example, cancers.

Exposure variables

Most exposures were quantitative, usually grouped into ordered categories rather than analysed as continuous variables. Methodologists have emphasised the importance of appropriate selection of categories and presentational methods⁷⁻¹⁰ but few articles gave reasons for the choice of categories and analyses, raising suspicions that alternative groupings might have also been explored. Furthermore, articles generally did not discuss the quality of the data. Ad hoc categorisations and measurement errors might explain many inconsistencies in published results.¹¹

Measures of association and inferences

Overall, authors presented appropriate estimates of their associations. Case-control studies used odds ratios, and most cohort studies used some form of rate

Table 2 Principal statistical estimates for associations between exposure and outcome in 73 epidemiological publications according to study design

| Type of result | Cohort | Case-control | Cross sectional | Case-cohort |
|------------------------|--------|--------------|-----------------|-------------|
| Odds ratio | 6 | 25 | 7 | — |
| Hazard/rate ratio | 22 | — | — | 1 |
| Risk ratio | 2 | — | 1 | — |
| Regression coefficient | 2 | — | — | — |
| Other | 5 | — | 2 | — |

ratio. Hazard ratios from proportional hazards models appear more often than rate ratios from Poisson models, which are appropriate only when rates stay constant over time.¹² Nomenclature and methods did not always match—for example, we had to check the results and methods sections carefully to identify what authors actually meant by “relative risk” or “risk ratio.”

Confidence intervals were usually presented as appropriate expressions of statistical uncertainty, but in some papers text and tables were made unwieldy by their excessive use. Hypothesis testing appeared in about half of articles, indicating rehabilitation of P values in observational studies.^{5 13 14} None the less, conclusions should not rely on arbitrary cut offs such as $P < 0.05$.

The distribution of P values in the figure has a peak around $0.01 < P < 0.05$ suggesting that publication bias affects epidemiology, as such significant findings are presumably more publishable.¹⁵ Compared with randomised trials, authors of epidemiological studies have more options on what to publish so publication bias is more complex and of potentially greater concern.

Adjustment for confounders

Most authors adjusted for potential confounders, though the extent varied greatly. Though techniques for such adjustment are established, their implementation seems inconsistent. Few explained how and why they chose variables for adjustment. A few were overenthusiastic and included too many variables in small studies. Some used stepwise regression to reduce the set of adjustment variables, a practice not without problems.^{16 17} Such procedures do not consider whether a variable's inclusion in the model affects the estimated effect of the exposure—that is, whether the variable is a confounder.

Some reported both unadjusted analyses and analyses adjusted for covariates, which appropriately informs readers of the role confounders had.

Effect modifiers

Subgroup analyses were common, and half of the articles claimed some effect modification. In clinical trials^{18 19} and epidemiology²⁰ overinterpretation of subgroup analyses presents three problems: increased risk of false claims of effect modification when several subgroup analyses are explored; insufficient use of statistical tests of interaction, which more directly assess the evidence for an effect modifier, compared with misleading uses of subgroup P values or confidence intervals; and the need to exercise restraint, viewing subgroup findings as exploratory and hypothesis generating rather than definitive.

Multiplicity

Some studies explore many associations without considering the consequent increased risk of false positive findings.^{20 21} Such “data dredging”¹¹ biases publications towards exaggerated claims. Investigators often focus on the most significant associations. This is accentuated in cohort studies with multiple publications, where what gets published can be highly selective. Particularly in small studies, apparently strong associations may be spurious and not supported by subsequent studies.

Study size

Few studies gave any power calculation to justify their size. One proposal is that cohort studies, specifically in

What is already known on this topic

Papers in observational epidemiology vary greatly in quality, content, and style

There are no generally accepted reporting guidelines for epidemiological studies

What this study adds

This survey shows some common failings in the analysis and reporting of epidemiological publications

Particular problems are that some studies are too small and make exaggerated claims; P values and confidence intervals are not always used appropriately; adjustment for confounders is often poorly explained; subgroup analyses and multiple associations are both overinterpreted; the choice of groupings and analyses for quantitative exposures is highly variable; and publication bias exists

There is a serious risk that some epidemiological publications reach misleading conclusions

coronary heart disease, require over 400 events to achieve sufficiently precise estimation.²² This is around the median number of events in our cohort studies, suggesting that many are underpowered, unless the associations with risk are pronounced. For instance, a cohort study relating bone mass to risk of colon cancer had only 44 incident cases.²³

Our case-control studies had a median of 347 cases, suggesting that many could detect only large effects.

Sample selection

A study's representativeness depends on the source of participants and the proportion participating.²⁴ Information on refusals and drop outs is often lacking. Authors should document the process of sample selection and the participation rate.

Conclusions and key findings

We have identified issues of concern surrounding the design, analysis, and report of epidemiological research. We feel primary responsibility for improvement rests with authors, though journals and peer reviewers need to be vigilant to enhance the quality of articles.

The following limitations merit particular attention:

- The participant selection process—for example, information on exclusions and refusals—often lacks details
- The quality of data collected, and any problems therein, are often insufficiently described
- Some studies are too small and may be prone to exaggerated claims, while few give power calculations to justify their size
- Quantitative exposure variables are commonly grouped into ordered categories, but few state the rationale for choice of grouping and analyses
- The terminology for estimates of association—for example, the term “relative risk”—is used inconsistently

- Confidence intervals are appropriately in widespread use but were presented excessively in some articles
- P values are used more sparingly, but there is a tendency to overinterpret arbitrary cut offs such as $P < 0.05$
- The selection of and adjustment for potential confounders needs greater clarity, consistency, and explanation
- Subgroup analyses to identify effect modifiers mostly lack appropriate methods—for example, interaction tests—and are often overinterpreted
- Studies exploring many associations tend not to consider the increased risk of false positive findings
- The epidemiological literature seems prone to publication bias
- There are insufficient epidemiological publications in diseases other than cancer and cardiovascular diseases and in developing countries
- Overall, there is a serious risk that some epidemiological publications reach misleading conclusions.

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Mortality rates and self reported health: database analysis by English local authority area

Robert G E Kyffin, Michael J Goldacre, Mike Gill

Mortality rates are commonly used as summary measures of current health status when comparing different populations. Their use in this way is often criticised, however, because mortality rates, though readily available and objective, are such an extreme measure of ill health. Surveys of self reported health, as an alternative approach to quantifying the health of a population, tend to be regarded as flawed because of their subjectivity. The UK census in 2001 included two measures of self reported health. We compared their values for each local authority area with the mortality rates for each area to find out whether mortality and self reported health are correlated.

Methods and results

For each local authority area in England, we took the age standardised mortality rates for the major causes of death pooled for 1999 and 2001 from the *Compendium of Clinical and Health Indicators*.¹

For the same areas, we calculated age standardised rates of self reported health status using data from the 2001 census,² using the European population as the standard. For the census question on general health everyone was asked whether, over the past 12 months, their "health had on the whole been good, fairly good, or not good." For the census question on limiting long term illness everyone was asked whether they had "any long term illness, health problem, or disability (including those due to old age)" which limited their daily activities or the work they could do. We plotted the rate for people in each local authority area who answered that their health had been "not good," and for those who answered "yes" to the question about long term

South East Regional Public Health Group, Government Office for the South East, Bridge House, Guildford, Surrey GU1 4GA

Robert G E Kyffin
public health intelligence officer
Mike Gill
regional director of public health

continued over

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Table A on bmj.com gives the correlation variables; the full results and details of excluded local authorities are on bmj.com