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Risk factors for casualty in earthquakes: The application of epidemiologic principles to structural engineering **

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Abstract

Past investigations of the public health consequences of natural disasters, such as earthquakes, have generally suffered from a lack of comprehensive data due, in part, to the difficulty faced in mounting a significant data collection effort in the aftermath of such an event. This lack of meaningful data severely hampers the ability of casualty researchers to develop reliable and robust estimates of death and injury in future events, these numbers are critical in planning for mitigation and response activities. The basic data required as input to these models are risk factors for injury which can only effectively be estimated through a careful epidemiologic study of those injured (and not injured) in past events. This paper outlines the field of earthquake injury epidemiology and discusses the application of the collected data. An example of a recent case-control study (in progress) is given. Implications for the broader application of these techniques to structural engineering are suggested.

Key words: Epidemiologic methods; Physical injuries, Risk factors, Earthquakes; Buildings, Human behavior

1. Introduction

Over the past several decades, significant resources in research support and effort have been expended — in this country and elsewhere — on the problem of earthquake hazard mitigation. Most of this research effort has been directed toward questions of geophysical research and structural engineering. While this expenditure of effort has been entirely appropriate in terms of advancing scientific understanding of the underlying phenomena responsible for earthquake losses and the development of improved strategies for the mitigation of losses in future events,

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the tragic fact remains that earthquakes continue to exact a toll on human lives. The specific mechanisms of death and injury in earthquakes and the development of strategies to directly effect their reduction have not yet been the subject of extensive study.

Despite its apparent simplicity, obtaining reliable and accurate estimates of casualties associated with earthquakes has posed serious challenges. Such estimates have varied, in part, because there is no universally accepted method of classifying deaths and injuries as earthquake-related. Furthermore, documentation of injuries has generally taken a lower priority than rescue and treatment activities in the face of disaster. The Loma Prieta Earthquake was no exception. Initial press accounts put the total death toll in the hundreds [45, 46], an overestimate by a factor of 3 to 4, and even the scientific literature could not agree on a total count, offering a range of 60 to 67 deaths (e.g., [2, 9, 32, 50]). One year after the event, there was still no reliable information on morbidity (i.e., injury) associated with the earthquake [21]. This uncertainty still exists at the time of writing. The work described herein attempts to overcome these and other problems that have characterized previous research in this area.

While there have been clear advances in recent decades in most of the disciplines involved in earthquake casualty research, it is clear, with few notable exceptions (e.g., [16]), that epidemiological methods have only recently begun to be applied to this area. Recent efforts have confirmed the assertion that there is much to be learned from the detailed analytical study of earthquake casualty. As society endeavors to reduce the consequences of natural disasters, appropriately directed and focused efforts are required. In the earthquake injury field, these efforts can be identified most readily through comprehensive and detailed epidemiologic study of events as they occur.

This paper will discuss the application of epidemiologic principles to the study of earthquake-related injuries and the value of this approach for structural engineering.

2. Epidemiology and injury epidemiology

Epidemiology is the basic science underlying all public health prevention programs. A generally accepted definition of epidemiology is "the study of the distribution and determinates of health-related states or events in specified populations, and the application of this study to the control of health problems" [26]. Epidemiology measures risk factors — attributes or exposures that are associated with an increased probability of developing a specific health condition.

Historically, epidemiology concentrated on the causes and prevention of infectious diseases, such as cholera and measles. Over time, the focus of epidemiological investigations has expanded to include other medical conditions such as chronic diseases, genetic ailments, mental illness, environmental and occupational diseases, and physical injuries. While the basic epidemiologic principles described below apply to the study of all of these health outcomes, the proceeding illustrations come from the injury field.

There are several classification schemes for epidemiologic studies. One such scheme divides studies into *experimental* and *observational*. In experimental studies, the investigators randomly assign study participants to a treatment or an exposure group to minimize variation in extraneous risk factors in comparison to the assigned exposures of interest. The goal is to make

the study groups as similar as possible except with respect to the exposures of interest, and to examine the effects of these different exposures on subsequent health outcomes. In observational studies, however, the study participants determine their own exposures and the investigators can only observe the effects of these self-selected exposures on health outcomes [40]. For ethical and logistical reasons, there are few opportunities for experimental studies in the area of injury prevention, and, thus, most injury studies are observational. Epidemiologic studies can also be characterized as *descriptive* or *analytical* (see below).

In epidemiology, elucidating a causal relationship between an exposure and health outcome (including injuries) typically follows a standard pattern of investigation [25]. The first evidence of an injury problem often comes from an anecdotal report or a clinical observation of one or several patients. These limited observations can be used to construct a case definition for the specific injury problem. The next step is to conduct descriptive studies by identifying a series of injured persons who meet the case definition in order to describe their characteristics on a population level. Central to this approach is the calculation of injury rates which allow meaningful comparisons between groups. The results of case-series studies can be used to alert both health professionals and high risk populations to the potential hazards involved. In some instances, such results also can be used to devise preventive strategies or actions However, descriptive studies are limited to characterizing the injured; they cannot provide estimates for individuals of the relative risk of being injured or not associated with the presence or absence of potential hazards. To examine and quantify relative risks, a comparison group of uninjured persons is needed. Thus, in the next step, analytical epidemiology studies — which include such comparison groups — are undertaken. Finally, if feasible and ethical, experimental studies, such as injury prevention or intervention trials, are conducted to examine the efficacy of interventions designed to reduce the occurrence or severity of injuries. Each progression represents a significant increase in study complexity and required resources.

The major types of epidemiologic studies are outlined in greater detail below. While the various study designs may differ with respect to specific approaches taken and measures of risk, they all attempt to evaluate the same phenomenon: the experience of a defined population in a given time period [33].

2.1. Ecological studies

In most epidemiologic studies, the unit of analysis is the individual. That is, health outcomes and risk factors are measured for each subject in the study. In contrast, *ecological studies* utilize summary measures for aggregates of individuals, defining ecological units. In ecological studies, summary measures of the frequency of disease or injury, and potentially adverse exposures are obtained for each ecological unit. An ecological unit is typically a geographic region or country. Analyses examine whether ecological units with higher average levels of exposure also have higher rates of disease or injury [25].

Ecological studies can be valuable in suggesting etiological hypothesis for health problems and guiding future research. However, because they rely on summary measures, they cannot demonstrate a causal relationship between exposure and disease or injury at the individual level. Such studies can lead to erroneous conclusions about the relationship between risk factors and the subsequent development of disease or injury in exposed individuals. The latter phenomenon is often referred to as the *ecological fallacy* [25].

2.2. Descriptive studies

Descriptive studies are conducted when little is known about the causes of an injury or disease. These studies describe the patterns of injury or disease in populations according to demographic characteristics, such as age, sex and level of education; geographic location; and time period. They can also convey how people got injured, including the mechanism of injury.

While they are useful in generating etiologic hypotheses, descriptive studies cannot definitively demonstrate that a risk factor causes a particular ailment. Once clues become available, analytical studies can be carried out to test hypotheses that suspected hazards cause a particular health condition. It is interesting to note that most engineering surveys after disasters are descriptive in nature.

One common type of descriptive study is a *case-series*. A case-series includes only those individuals that meet an *a priori* case definition of injury or disease. Those free of disease or injury are excluded. Two common sources of cases are the general population and hospitals.

Population-based case-series attempt to ascertain all cases or a representative sample of cases that occur in a defined general population in a given time period. If all cases are identified and ascertained, it is possible to compute injury or disease rates in the population. Rates can also be estimated from a sample of cases from the general population if the sampling fraction of all cases is known.

The computation of injury rates by mechanism permits the investigator to identify high risk groups in the population. The value of identifying high-risk groups is that it enables interventions to be targeted more efficiently to those at highest risk, and it also suggests those groups that need more intense study in order to identify causal factors. All injury studies should, if possible, be population-based.

Many case series obtain cases from sources whose population base is not known or easily definable. Hospitals are often considered to be such a source. Rates of injury in the population cannot be estimated from studies whose population base is not known. (However, rates can be computed for hospital-based case-series when all hospitals in an area are included, and all cases arising in that area are known to go to one of those hospitals for the study condition.) A limitation of hospital cases is that they may not be representative of all cases in the population with respect to disease severity and risk factors of interest. Many factors other than disease or injury severity, such as possession of health insurance or self-perception of the gravity of the illness, influence a person's decision to visit a hospital. If a sample of hospital cases is biased, conclusions based upon them may be erroneous.

Despite their limitations, case-series can be used to develop injury prevention policies. When hazards are obvious, there is little need to carry out more expensive, analytical epidemiologic studies. If a very expensive intervention strategy is needed, then larger and more thorough studies should be performed to determine if the intervention expense is "justifiable".

2.3. Analytical studies

Analytical studies are "designed to examine associations, commonly putative or hypothesized causal relationships" [26]. These studies not only are useful in identifying risk factors that are associated with the production of disease or injury, but they also permit an estimation of the

relative magnitude of such risk factors. There are three major types of analytic studies: cross-sectional, case-control, and cohort studies.

Cross-sectional studies

Cross-sectional studies measure the presence of disease or injury and potential risk factors of interest at one point in time or in a short interval in a defined population [26]. They include all cases, newly diagnosed and pre-existing, found in the population in a specified time period. Thus, they provide an estimate of prevalence, the proportion of the population that is diseased or injured at a particular time. Sometimes prevalence is referred to as the prevalence rate. However, prevalence is not technically a rate since it measures existing rather than new events per unit time.

The relative magnitude of risks associated with disease or injury in cross-sectional studies can be estimated by the prevalence rate ratio or the prevalence odds ratio. The former is the ratio of the prevalence of disease or injury among population members exposed to the potential risk factor to the prevalence among those not exposed [25]. The prevalence odds ratio is the ratio of the odds of existing illness or injury among the exposed to the odds of existing illness or injury among the non-exposed members of the population. The prevalence odds, in turn, is defined as the ratio of ill or injured to not ill or injured persons [40].

Cross-sectional studies are most useful in providing guidance on how to allocate health resources to address existing disease or injury in a community. Because these studies measure exposures and health outcomes for the same point in time, it is not possible to know whether the exposure precedes the health outcome or vice versa. Thus, this research design is not strictly capable of demonstrating a causal relationship between risk factors and disease. In some instances, current exposures may be meaningful surrogates for previous ones, and, thus, these studies can sometimes be used to examine disease or injury etiology [40].

Case-control studies

In a *case-control study*, "individuals with a particular condition or disease (the cases) are selected for comparison with a series of individuals in whom the condition or disease is absent (the controls). Cases and controls are compared with respect to existing or past attributes or exposures thought to be relevant to the development of the condition or disease under study" [44]. Case-control studies are also sometimes referred to as retrospective, case-comparison, case-referent or case-compeer studies [25].

Similar to case-series, a case-control study requires a specific and precise case definition. Additional criteria for entry into the study, such as living in a certain region, may also be required. Most of these studies enroll only incident or new cases that occur during a specific ascertainment period. However, sometimes prevalent cases comprise the case group. The sources of cases are typically the general population or hospitals.

The choice of an appropriate control group is critical for producing valid results. The guiding principles for selecting controls are that they should come from the same population as the cases, have the same potential for exposure as the cases, and be free of the study disease or injury when selected but be at risk of developing it. Sources of controls include hospitals, the community, neighbors, and relatives. Hospital controls are patients who are free of the study disease and visit the hospital for another pre-specified ailment. This ailment should neither be

caused by nor associated with the study disease nor caused by the same risk factors as the study disease.

Because case-control studies examine exposures that occur prior to the development of the health condition, they can be used to study cause and effect. In these studies, the relative risk of injury or disease of interest associated with an adverse exposure is estimated by the *relative odds* or *odds ratio*. The formal definition of the odds ratio is presented later.

Case-control studies are the most frequently conducted analytical studies in epidemiology. They are preferred when the studied health condition is rare, and exposure among those with the condition is common. They can explore the impact of multiple exposures or risks factors on disease. Finally, they have the advantage of generally requiring less time and fewer resources to execute than cohort studies, described below.

Cohort studies

Cohort studies start with a defined population (i.e., cohort) free of the disease or injury of interest. The cohort is then divided into those who are exposed and those not exposed to a potential risk factor, and followed forward in time to see who develops the health outcome in each exposure group. The incidence rate (or mortality rate) of the health outcome — the number of new cases (or deaths) arising from the population at risk in the follow-up period — can be calculated for each exposure group and compared to one another [25].

Various measures of disease or injury risk can be obtained from a cohort study. In addition to calculating incidence or mortality rates for each exposure group, one can compare the groups by taking the ratio or difference of their rates. The *rate ratio*, the ratio of incidence rates, provides an estimate of the magnitude of the association between a health outcome and a risk factor. Another measure of association is the *risk ratio*, also called the cumulative incidence ratio. The risk ratio is the probability of developing a condition in a specified time interval in the exposed group divided by the probability of developing the condition in the same interval among the non-exposed. The odds ratio, defined later, can also be estimated in cohort studies. The three types of ratios (rate, risk and odds) are related. They usually provide similar estimates of the *relative risk*, a term commonly applied to all of them [25].

Cohort studies can also be used to estimate the *attributable risk*, defined as the proportion of the disease or injury incidence that can be attributed to an exposure of interest. Attributable risk can be computed for the exposed persons only, or for the total population, including the exposed and non-exposed groups. The attributable risk associated with a potential hazard can also be estimated in case-control study.

Cohort studies are generally considered to provide stronger evidence for causality than case-control studies. This is because in cohort studies, exposure measures are made prior to the onset of disease, whereas in case-control studies, exposures are reported after disease or injury has already occurred and may be subject to errors in recall.

Cohort studies, however, are expensive in terms of time and money, often requiring long follow-up periods. Cohort studies are best carried out when the exposure is rare, and the medical condition is frequent among the exposed.

2.4. Experimental studies

A controlled experiment is the ideal research design, but in injury epidemiology, particularly in the study of disasters, there are seldom opportunities for such a study. There are some

instances, however, where behavioral characteristics, as factors which may contribute to injury, can be studied in a laboratory environment.

3. Study design issues

The validity of inferences that can be drawn from an epidemiological study depends critically on research design. The most sophisticated statistical analyses cannot rectify a poorly conceived or executed study. Perhaps one of the greatest contributions that epidemiology can make to the structural engineering community interested in human casualties is an appreciation of the means to detect and reduce systematic and random errors in studies of risk factors for injury and disease. Some of these issues are addressed briefly below.

3.1. Sample size and statistical power

Before embarking on an investigation, the epidemiologist must determine whether the proposed study will have adequate statistical power to detect a true effect of an exposure on a health outcome. If the study does not have sufficient power, it is not worth undertaking.

Statistical power is defined as the probability of rejecting the null hypothesis (e.g., there is no association between the health condition and risk factor), given that the null hypothesis is false. Power is related to sample size and generally the former increases with the latter.

Most types of injuries, especially severe ones, are relatively rare events. Therefore, care must be taken in planning an injury study to ensure that it will have a large enough sample size and statistical power to detect an association between potential hazards and injury, should one truly exist.

3.2. Confounding

Confounding is "the effect of an extraneous variable that wholly or partially accounts for the apparent effect of the study exposure, or that masks a true association" [44] To be a confounder, the extraneous variable must be both causally related to the injury or illness under investigation, and be associated with but not caused by the risk factor of interest.

An example of confounding is the apparent association between yellow index finger (the risk factor of interest) and lung cancer (study disease). Yellow index finger does not actually cause lung cancer. However, smoking both causes lung cancer and is associated with the production of yellow index finger. Thus, smoking is a confounder; persons with yellow index finger will be more likely to smoke and, thus, be at increased risk of developing lung cancer [1].

Potential confounders should be identified and controlled in the design or analysis phase of the study, or both. There are several methods used to accomplish this. At the design phase, cases and controls (in case-control studies) and exposed or non-exposed persons (in cross-sectional or cohort studies) may be matched on the confounding variable. For example, suppose that gender was thought to be a confounder in a case-control study. If sixty percent of the cases were male, then controls would be selected so that sixty percent of the controls were also male. Because matching fixes the distribution of the confounder in a study, it is not possible to

examine the contribution of the confounder to the disease causation when this method is employed.

Adjusting for confounding of unmatched samples can be achieved by several statistical methods during the analysis. These methods generally involve stratification of the cases and controls, or exposed or non-exposed individuals, by different levels of the confounder. The measures of risk associated with the study exposure are then calculated for each stratum. If the risk estimates do not vary significantly across strata, then a weighted average is computed. These analysis techniques include the direct and indirect methods of adjustment for rates (both incidence and mortality), and the Mantel–Haenszel method for weighted odds ratios [31].

Other statistical methods exist for multivariate analysis, which allow the epidemiologist to estimate the magnitude of relative risk for the exposure(s) of interest while simultaneously controlling for the effects of other confounding variables and the interactions between variables [40]. The most commonly employed multivariate model for case-control studies is logistic regression. The texts by Rothman [40] and Schlesselman [44] provide an excellent introduction to multivariate analysis.

3.3. Interaction

Interaction or *effect modification* occurs when the risk of disease or injury in the presence of two (or more) factors differs from the risk expected to result from the combination of their individual effects. Another way of stating this is that an interaction is present when the magnitude of the association between a chosen risk factor and disease varies according to the level of one or more other risk factors [25]. An interaction may be positive or negative, that is, increase or decrease the magnitude of the association, respectively, over that which is expected. For example, cigarette smoking and exposure to asbestos are each known to be a risk factor for lung cancer in the absence of one another. However, smoking modifies the effect of asbestos on developing this disease. Among those exposed to asbestos, smokers are more likely to get lung cancer than non-smokers [18].

It is important to evaluate possible interactions among exposure variables. Stratifying by different levels of the potential effect modifier is one way in which to examine interaction. If the measures of association between the disease and the chosen (first) risk factor differ significantly across the strata, then an interaction is present. It is inappropriate to combine the risk estimates from different strata using a weighted average approach, when effect modification occurs.

3.4. Bias

Bias is "any systematic error in the design, conduct, or analysis of a study that results in a mistaken estimate of an exposure's effect on the risk of disease" [44]. It is often difficult or impossible to detect or quantify bias in an epidemiological study, should it occur. Therefore, epidemiologists expend considerable energy devising strategies to prevent or reduce potential biases in all phases of a study.

While dozens of potential biases have been described [41], most of them fall into three basic categories: *selection bias, information bias* and *misclassification*.

Selection bias occurs when the ascertainment of cases and controls in a case-control study, or exposed and non-exposed persons in a cross-sectional or cohort study is biased, resulting in erroneous estimate of the relative risk. If those ascertained differ systematically in exposure or disease characteristics from those not ascertained, bias can occur. There are various forms of selection bias.

Non-response bias is an important type of selection bias. It occurs when potential study subjects refuse to participate in the investigation. Non-participants almost always differ from those who choose to cooperate with respect to exposure or disease attributes, or both [25]. The best way to ameliorate the effects of non-participation is to prevent it from happening in the first place. A variety of techniques have been developed for this purpose including training staff to approach potential study candidates in a professional manner; explaining the benefits and risks of the study to those contacted; informing potential subjects they can refuse to answer any particular question or forgo any particular exams (e.g., drug screens on urine samples); and providing information through personal mailings, the media or respected community leaders to confirm the legitimacy of the study. If a high rate of refusal prevails despite the researcher's best efforts, then it is important to try to assess the degree of difference between participants and non-responders This can be done by examining the differences between the two groups on any variables available to the investigator (such as gender or city of residence), or surveying non-responders on their reasons for declining to participate, or both.

Information bias can result from inaccurate reporting or recording of exposures or health outcome data. Examples of this bias include errors made during the abstraction of medical records, and inaccurate information provided by interviews conducted with proxies for study subjects not capable of doing the interview themselves. An important source of concern in case-control studies is recall bias. The latter bias can occur if cases — who may be more likely to search for explanations for their ailments — recall their exposures more accurately than controls. One way to try to reduce recall bias is to query respondents about their exposures prior to soliciting information on their health status.

Misclassification occurs when the disease or exposure status assigned to an individual is in error. Misclassification may be non-differential (random) or differential (non-random). Non-differential misclassification occurs when the error in the designation of exposure (or disease) is independent of the disease (or exposure) status. Differential misclassification is present when the assignment to an exposure (disease) category is not independent of the disease (exposure) status. The effect of non-differential misclassification is to bias the measure of association to the null, i.e., towards a no-effect level. Differential misclassification will bias the measure of association either towards or away from the null, depending on how the error is distributed across disease and exposure categories [40]. With differential misclassification, the apparent strength of the association between the risk factor and disease may be inflated or deflated; the direction of the association (indicating that exposure is harmful or protective) may be reversed.

It is possible to correct estimates of the relative risk if the magnitude of the misclassification errors is known. However, this information is rarely available to the epidemiologist.

3.5. Funding

The level of the funding available to conduct the research will certainly influence the type of study undertaken (e.g., whether it will be descriptive or analytical) and its scope.

3.6. Ethical issues

Ethical considerations include taking steps to protect the confidentiality of information on study subjects, providing potential subjects with the opportunity to grant informed consent to participate, protecting participants' rights to privacy, and considering the balance of risks and benefits of the research to the study participants. Investigators should be sensitive to the potential emotional stress that may be induced by asking ill or injured persons to recall the circumstances related to their health status.

3.7. Injury severity measures

The simplest way in which to categorize injury is to indicate its presence or absence. However, a great degree of important information is lost with such a dichotomous classification system. Over the past 15 years, a number of scales have been developed to measure injury severity [8, 28]. These scales allow the epidemiologist to describe the impact of single and multiple injuries to an individual. Injury severity scales may be based on anatomical criteria, physiological and biochemical parameters, or pre-existing variables such as age and sex known to affect injury severity, or both. To be most useful, an injury severity scale should produce valid and reproducible scores which are independent of any medical interventions received by the patient, and should be easy to use with commonly available data.

The Abbreviated Injury Scale (AIS) 1990 Revision satisfies the above criteria [3]. The AIS severity score is an anatomically-based system that categorizes injuries by six body regions using a six point ordinal scale ranging from minor (AIS = 1) to currently untreatable or unsurvivable (AIS = 6) injuries. The Injury Severity Score (ISS), a measure of the combined effect of individual injuries to a person, can be computed from the AIS severity scores [7]. The advantages of using the ISS include that it correlates well with the probability of death [17]; it is the most widely used injury severity scale, facilitating comparisons across studies [29]; and, it has been shown to have high inter- and intra-rater reliability for both penetrating and blunt injuries, even among persons without medical training [29]. This technique is being used in the case-control study of the Loma Prieta earthquake described below.

4. Earthquake injury epidemiology

Earthquake injury epidemiology can be defined as the study of the distribution of death and injury in earthquakes and the causes of fatal or nonfatal injury. The causal mechanisms are difficult to elucidate precisely, as are the appropriate variables and indicators describing them. It is necessary to consider building construction types, and their performance during earthquakes, the influence of nonstructural components of buildings and building contents, occupancy and occupant behavior, emergency and rescue response, and medical treatment provided. These areas have not traditionally been the responsibility of any single field, but require the interaction of several disciplines.

A rigorous epidemiological approach to the study of earthquakes is based on the study of diseases or injury in whole populations, rather than a limited number of individual patients and

their treatment [26, 27]. As such, it seeks to determine risk factors or predict disease outcomes that can then be used to develop sound principles for injury prevention in future events.

Unfortunately, detailed epidemiologic studies of injuries in past events have not, in general, occurred. Thus, it is not clear exactly where prevention and treatment efforts and finances should be focused. If it is found, for example, that most severe injuries or deaths are resulting from inappropriate responses on the part of the victims, then education should be targeted as a priority item. If the interaction between building contents and occupants is causing high rates of injury, then more action needs to be taken to ensure proper anchorage of these contents. If certain structural or non-structural building components are found to be particularly dangerous, then their design for new buildings can be altered to improve safety. If a large number of people are dying because they are not being extricated quickly enough from collapsed buildings, then extrication techniques or rescue equipment need improvement. If a large number of people are dying after successful extrication from severely damaged or collapsed structures, then it is necessary to improve emergency treatment procedures.

5. The need for comprehensive casualty data

5.1. Applications of epidemiologic data on earthquake casualty

The results of epidemiologic studies of injuries in disasters — in this case, earthquakes — can be used to effect casualty reduction by improving both mitigation and preparedness and response activities.

An overall objective of collecting data after earthquakes is to scientifically measure and describe the health effects of earthquakes and the factors that contribute to these effects, with the goals of

(1) assessing the needs of earthquake-affected populations;
(2) matching resources efficiently to needs;
(3) preventing further adverse health effects;
(4) evaluating earthquake relief effectiveness; and
(5) making future earthquake contingency plans.

Data collection after earthquakes and analysis of these data can also be linked to an emergency decision-making process. In addition, earthquake casualty researchers have an important role to play in providing informed advice about the probable health effects which may arise in a future earthquake, in establishing priorities for action, and in emphasizing the need for accurate information as the basis for relief decisions.

Epidemiology has made major contributions to the planning of more effective relief efforts after disasters. For example, the assessment of health needs and disease surveillance after earthquakes have occurred. Studies of attendance at clinics following the 1976 earthquake in Guatemala, for example, showed that by the time international medical disaster assistance arrived, new trauma admissions had fallen off dramatically [14, 43]. This finding has led to the realization that efforts should be made to increase the local capacity to respond to disasters rather than rely on outside assistance. Well-designed epidemiologic studies have also shown that contrary to popular belief, major outbreaks of food or water-borne diseases rarely follow natural disasters [47].

One of the important uses for casualty data which deserves special attention is casualty estimation modeling: This represents the framework into which the outcomes from individual studies may fit. Loss estimation models are frequently used by planners and public officials in preparedness activities. "Loss" may refer to property and economic losses, or to casualties. One of the important potential uses for the data collected through earthquake injury epidemiologic studies is as a database with which casualty estimation models may be developed or refined.

There are some serious doubts as to the validity of existing casualty estimation models — both pre-event and post-event. While they often have been instrumental in motivating preparedness activities, their reliability and, thus, usefulness for detailed planning is generally limited. Most are based on engineering models with little input from medicine or epidemiology. It is common for post-event damage estimates and related casualty estimates to be highly suspect.

One of the important features of such a model is the potential for identifying how the variables interact and how independent risk factors modulate the expected outcomes. Sensitivity of the model to small changes in the variables should be addressed. Spatial models would greatly assist in resource allocation planning both before and after an event. The development of probabilistic models is seen as an important need in this area. Not only should expected numbers of casualties be given, but also the variances of the estimates and the associated statistical moments for the various variables and indicators in the model. In this way, estimates can be improved (i.e., the variance of the estimate reduced) as more data become available. Pre-event predictions can be modified quickly after the event by performing reconnaissance activities.

6. Past research and findings in casualty data collection

6.1 General

A critical review of the scientific literature on the causes of earthquake-related deaths and injuries leads to the following general conclusions. First, there is a paucity of epidemiological investigations of earthquakes, despite their lethality. This dearth has arisen from inadequate funding and, until recently, from a relative lack of interest in the subject area by researchers [49]. Second, almost all of the published epidemiological studies on earthquake-related injuries are descriptive rather than analytical, precluding the ability to establish and quantify the magnitude of the relationship between significant risk factors and injuries. An extensive literature review revealed only six analytical studies of earthquake-related injuries that have been conducted to date (see Table 1).

Third, documentation of deaths and, in particular, non-lethal injuries is often incomplete in the aftermath of disaster, particularly in less developed countries. Fourth, injuries are often vaguely and inconsistently defined in the previous epidemiological studies. For example, the definition of injuries may include conditions other than physical trauma [42], as well as any affliction treated after the disaster, whether or not it was earthquake-related [14]. Investigators often employ different schemes to classify injury severity levels [11]. Fifth, most previous

Table 1
Building and structure-related risk factors for injury: analytical studies

Risk factor	Odds ratio (95% CI) [a]	Reference
Inside building vs. not inside	12 20 (3.62–63.69)	[5]
For those inside a building		
Construction materials		
Adobe brick vs non-adobe houses (mostly wood frame)	26.8 [b,c]	[16]
Concrete/mixed materials vs. wood	3 4 (1.1–13 5)	[10]
Age of building		
⩾ 8 yrs old vs ⩽ 8 yrs old	3.21 [b]	[16]
Number of floor levels		
⩾ 5 floors vs. ⩽ 5 floors	3.45 (1.76–6.74)	[5]
⩾ 7 floors vs ⩽ 7 floors	34.7 (8.1–306.9)	[10]
Location on floor		
Located on floors 2–4 vs. 1	2.60 (1.42–4 75)	[5]
Located on floors ⩾ 5 vs. 1	4.02 (1 08–14 9)	[5]
Located in mid-level vs bottom or top	2.3 (1.3–4.2)	[10]
Stayed indoors vs. ran outdoors	4.82 (2 34–10.0)	[5]
Entrapment		
Being trapped vs not trapped (mortality)	107.24 [b,c]	[12]
	67.3 [b] (49 7–91.3)	[37]
Being trapped vs. not trapped (morbidity)	5.17 [b,c]	[12]
	11.4 [b] (10.2–12.7)	[37]
Duration of entrapment: ⩾ 1 hr vs. ⩽ 1 hr	2.79 (1.52–5.13)	[37]

[a] CI = Confidence interval.
[b] Rate ratio, not odds ratio.
[c] Calculated from information provided in the publication.

epidemiological studies of earthquake populations have been conducted solely by health researchers, even though the topic calls for an interdisciplinary approach which draws on structural engineering, geology, architecture, epidemiology and emergency medicine.

6.2. *Risk factors for physical injury*

The majority of studies on the risk factors for earthquake-associated injuries actually appear in the earthquake engineering literature. These studies have generally been executed along strict disciplinary lines without input from health professionals. Thus, despite their quantitative approach, these engineering studies do not employ standard methods or meet minimal criteria generally required by epidemiologists to accurately and reliably assess risks. For example, some surveys of earthquake victims are not based on random, probability based samples of a clearly defined study population [6, 15 (San Fernando earthquake)]; others do not report sufficient information to evaluate the sampling methodology [34, 35, 38, 39]; still others sampled highly select groups [36], making it difficult to generalize the results to the rest of the population.