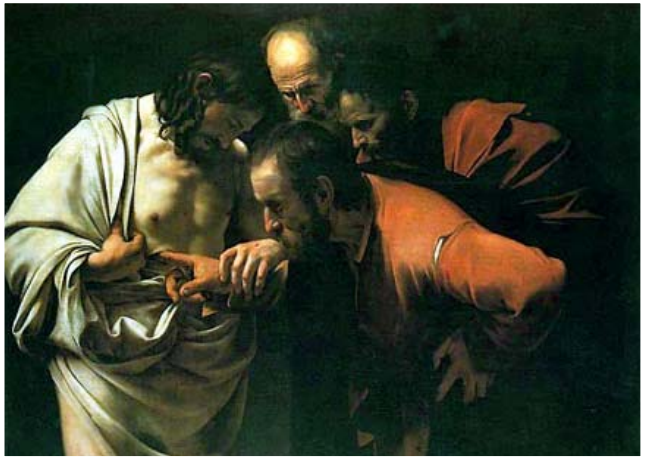
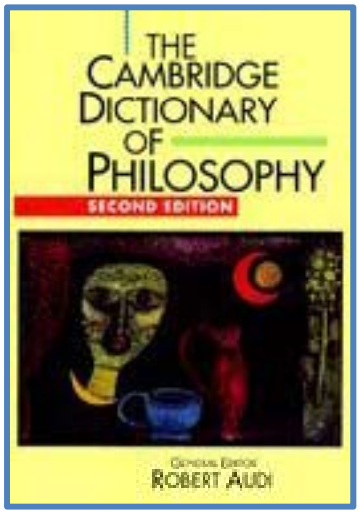


Istituto Superiore di Sanità

XXII Seminario Nazionale
La valutazione dell'uso e della sicurezza dei farmaci: esperienze in Italia
Roma, 9 dicembre 2013

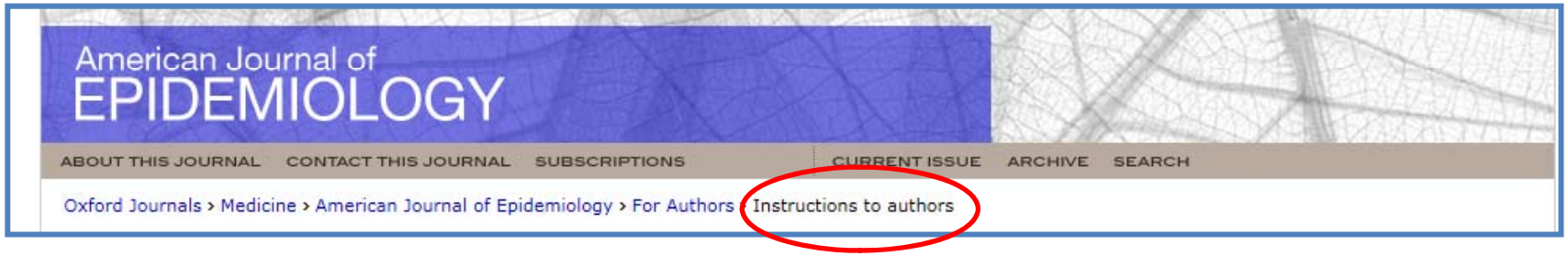
Il ragionamento causale in farmacoepidemiologia

Roberto Raschetti

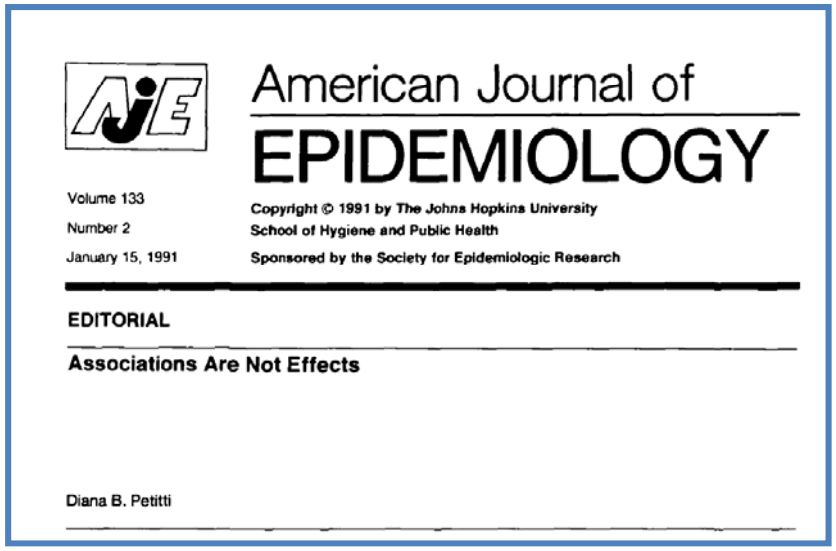


alla voce “causation”:

Il tentativo di "analizzare" la causalità sembra aver raggiunto un punto morto; le proposte a portata di mano sembrano così ampiamente divergenti che ci si chiede se siano tutte analisi di uno stesso concetto.
Ciascuna di esse sembra cogliere qualche aspetto importante della variegata nozione che esprimiamo attraverso il termine “causa” ma è dubbio che vi sia un concetto unitario di causalità che possa essere catturato in una illuminante analisi filosofica.



The Journal does not allow the use of the word "effect(s)" to denote "association(s)" in reports of single observational studies. Please refer to the editorial "Associations Are Not Effects" (Am J Epidemiol. 1991;133 (2):101–102).



AJE

American Journal of
EPIDEMIOLOGY

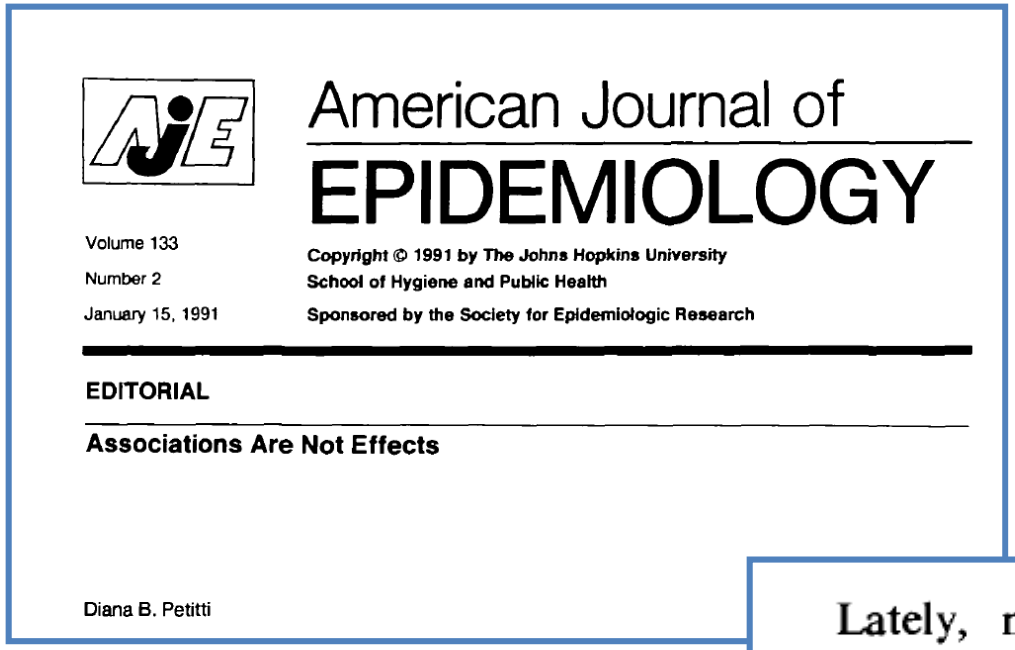
Volume 133
Number 2
January 15, 1991

Sponsored by the Society for Epidemiologic Research

EDITORIAL

Associations Are Not Effects

Diana B. Petitti



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EDITORIAL

Associations Are Not Effects

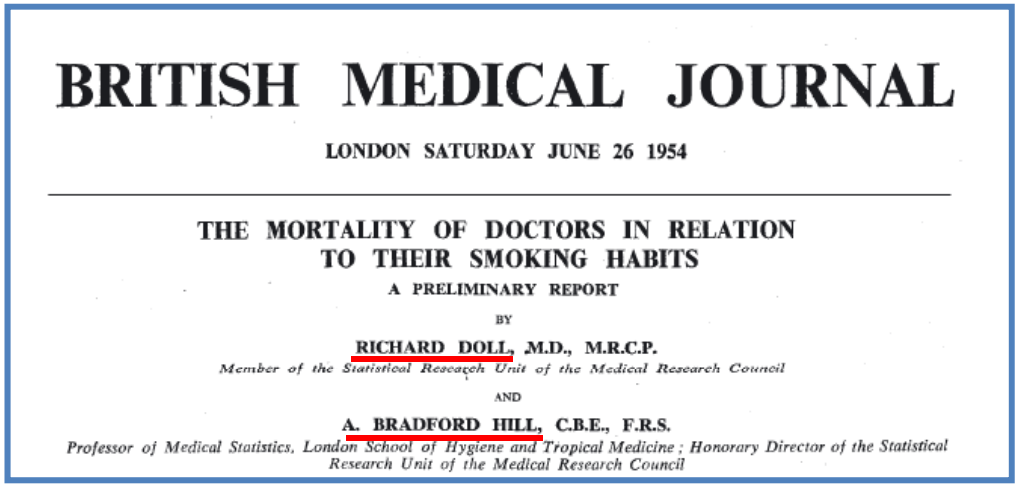
Diana B. Petitti

Una questione di linguaggio politicamente corretto?

Lately, much has been written about causal thinking in epidemiology (1–5). Publications delineating alternative criteria by which associations might be considered causal associations have become numerous (6–10). At the same time that epidemiologists expend increasing amounts of energy making explicit the process by which they reason and creating standards for deeming associations as causal, use of the word "effect" in published reports inadvertently elevates many statistical associations to the status of cause.

Associazione o relazione causale?

Il dibattito sul fumo negli anni ‘60



BRITISH MEDICAL JOURNAL

LONDON SATURDAY JUNE 26 1954

THE MORTALITY OF DOCTORS IN RELATION TO THEIR SMOKING HABITS

A PRELIMINARY REPORT

BY

RICHARD DOLL, M.D., M.R.C.P.

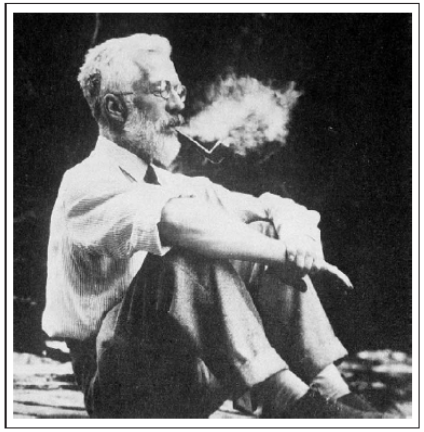
Member of the Statistical Research Unit of the Medical Research Council

AND

A. BRADFORD HILL, C.B.E., F.R.S.

Professor of Medical Statistics, London School of Hygiene and Tropical Medicine; Honorary Director of the Statistical Research Unit of the Medical Research Council

1954



For my part, I think it is more likely that a common cause supplies the explanation. Again, we do not know. I do not put forth any explanation as proved, but as requiring investigation. The obvious common cause to think of is the genotype. We are all different genotypes. I suppose in this nation there must be well over 150 million different genotypes. If one studies cancer in mice (and I suppose about half the mice of the world are kept to study cancer with), if one examines any of the many (and there are thousands) of inbred lines of mice (where we can get a hundred or two hundred individuals of the same genotype to study)—if you take, then, any two such lines of differing genotypes, they will, I believe, invariably be found to differ in the frequency, in the age incidence, and in the type of cancer which those mice suffer from. Consequently if there is any genotypic difference between the different smoking classes, we may expect differences in the type or frequency of cancer that they display.

CIGARETTES, CANCER, AND STATISTICS

Sir Ronald Fisher

SEVEN OR EIGHT years ago, those of us interested in such things in England heard of a rather remarkable piece of research carried out by Dr. Bradford Hill and his colleagues of the London School of Hygiene. We heard, indeed, that it was thought that he had made a remarkable discovery to the effect that smoking was an important cause of lung cancer. Dr. Bradford Hill was a well-known Fellow of the Royal Statistical Society, a member of Council, and a past president—a man of great modesty and transparent honesty. Most of us thought at that time, on hearing the nature of the evidence, which I hope to make clear a little later, that a good *prima facie* case had been made for further investigation. But time has passed, and although further investigation, in a sense, has taken place, it has consisted very largely of the repetition of observations of the same kind as those which Hill and his colleagues called attention to several years ago. I read a recent article to the effect that nineteen different investigations in different parts of the world had all concurred in confirming Dr. Hill's findings. I think they *had* concurred, but I think they were mere repetitions of evidence of the same kind, and it is necessary to try to examine whether that kind is sufficient for any scientific conclusion.

The need for such scrutiny was brought home to me very forcibly about a year ago in an annotation published by the British Medical Association's Journal, leading up to the almost shrill conclusion that it was necessary that every device

Centennial Review, 2: 151-166, (1958).

1958

Published by Oxford University Press on behalf of the International Epidemiological Association
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International Journal of Epidemiology 2009;**38**:1175–1191
doi:10.1093/ije/dyp289

REPRINTS AND REFLECTIONS

Smoking and lung cancer: recent evidence and a discussion of some questions*

Jerome Cornfield,[1] William Haenszel,[2] E. Cuyler Hammond,[3] Abraham M. Lilienfeld,[4] Michael B. Shimkin[5] and Ernst L. Wynder[6]

Summary This report reviews some of the more recent epidemiologic and experimental findings on the relationship of tobacco smoking to lung cancer, and discusses some criticisms directed against the conclusion that tobacco smoking, especially cigarettes, has a causal role in the increase in broncho-genic carcinoma. The magnitude of the excess lung-cancer risk among cigarette smokers is so great that the results can not be interpreted as arising from an indirect association of cigarette smoking with some other agent or characteristic, since this hypothetical agent would have to be at least as strongly associated with lung cancer as cigarette use; no such agent has been found or suggested. The consistency of all the epidemiologic and experimental evidence also supports the conclusion of a causal relationship with cigarette smoking, while there are serious inconsistencies in reconciling the evidence with other hypotheses which have been advanced. Unquestionably there are areas where more research is necessary, and, of course, no single cause accounts for all lung cancer. The information already available, however, is sufficient for planning and activating public health measures. – *J. Nat. Cancer Inst.* **22**:173–203, 1959.

1959

Published by Oxford University Press on behalf of the International Epidemiological Association
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International Journal of Epidemiology 2009;38:1175–1191
doi:10.1093/ije/dyp289

REPRINTS AND REFLECTIONS

Smoking and lung cancer: recent evidence and a discussion of some questions*

Jerome Cornfield,[1] William Haenszel,[2] E. Cuyler Hammond,[3] Abraham M. Lilienfeld,[4] Michael B. Shimkin[5] and Ernst L. Wynder[6]

L'articolo contiene la prima *sensitivity analysis* in uno studio osservazionale, sostituendo l'affermazione qualitativa "*association does not imply causation*" con una affermazione quantitativa circa l'ampiezza che dovrebbe avere una distorsione per poter spiegare l'associazione osservata tra trattamento e risposta.

Paul R Rosenbaum (2004)

Both the absolute and the relative measures serve a purpose. The relative measure is helpful in 1) appraising the possible noncausal nature of an agent having an apparent effect; 2) appraising the importance of an agent with respect to other possible agents inducing the same effect; and 3) properly reflecting the effects of disease misclassification or further refinement of classification. The absolute measure would be important in appraising the public health significance of an effect known to be causal.

The first justification for use of the relative measure can be stated more precisely, as follows:

> If an agent, A, with no causal effect upon the risk of a disease, nevertheless, because of a positive correlation with some other causal agent, B, shows an apparent risk, r, for those exposed to A, relative to those not so exposed, then the prevalence of B, among those exposed to A, relative to the prevalence among those not so exposed, must be greater than r.

Thus, if cigarette smokers have 9 times the risk of nonsmokers for developing lung cancer, and this is not because cigarette smoke in a causal agent, but only because cigarette smokers produce hormone X, then the proportion of hormone-X-producers among cigarette smokers must be at least 9 times greater than that of nonsmokers. If the relative prevalence of hormone-X-producers is considerably less than ninefold, then hormone X cannot account for the magnitude of the apparent effect (Appendix A).

Sotto la presidenza di J.F. Kennedy nel giugno del 1962 viene nominata una commissione di 10 esperti coordinata dal Surgeon General Luther L. Terry.

La commissione si riunì diverse volte esaminando (con l'ausilio di 150 consulenti) più di 7000 articoli scientifici. Alla fine dei lavori predispose un rapporto di quasi 400 pagine intitolato *Smoking and Health.*

Il rapporto fu presentato l'11 gennaio 1964 (scegliendo un sabato sia per minimizzare gli effetti sulla Borsa sia per massimizzare la copertura da parte dei giornali domenicali).

«Cigarette smoking is a health hazard of sufficient importance in the United States to warrant appropriate remedial action».

SMOKING *and* HEALTH

REPORT OF THE ADVISORY COMMITTEE
TO THE SURGEON GENERAL
OF THE PUBLIC HEALTH SERVICE

U.S DEPARTMENT OF HEALTH, EDUCATION, AND WELFARE
Public Health Service

1964

Statistical methods cannot establish proof of a causal relationship in an association. The causal significance of an association is a matter of judgment which goes beyond any statement of statistical probability. To judge or evaluate the causal significance of the association between the attribute or agent and the disease, or effect upon health, a number of criteria must be utilized, no one of which is an all-sufficient basis for judgment. These criteria include:

a) The consistency of the association
b) The strength of the association
c) The specificity of the association
d) The temporal relationship of the association
e) The coherence of the association

I “viewpoints” di A.B.Hill

Discorso presidenziale alla
Section of Occupational Medicine of the Royal Society of Medicine

1: **Strength of Association**. The stronger the relationship between the independent variable and the dependent variable, the less likely it is that the relationship is due to an extraneous variable.

2: **Consistency**. Multiple observations, of an association, with different people under different circumstances and with different measurement instruments increase the credibility of a finding.

3: **Specificity** in the causes. In the ideal situation, the effect has only one cause. In other words, showing that an outcome is best predicted by one primary factor adds credibility to a causal claim.

4: **Temporality**. It is logically necessary for a cause to precede an effect in time.

5: **Biological gradient**. Dose Response relationship. There should be a direct relationship between the risk factor (i.e., the independent variable) and people’s status on the disease variable (i.e., the dependent variable).

6: **Plausibility**. It is easier to accept an association as causal when there is a rational and theoretical basis for such a conclusion.

7: **Coherence**. A cause-and-effect interpretation for an association is clearest when it does not conflict with what is known about the variables under study and when there are no plausible competing theories or rival hypotheses. In other words, the association must be coherent with other knowledge.

8: **Experimental Evidence**. Any related research that is based on experiments will make a causal inference more plausible.

9: **Analogy**. Sometimes a commonly accepted phenomenon in one area can be applied to another area.

Interpreting Causality in the Health Sciences
F.Russo, J. Williamson
International Studies in the Philosophy of Science
July 5, 2007

Meeting January 14 1965

President’s Address

The Environment and Disease: Association or Causation?

by Sir Austin Bradford Hill CBE DSC FRCP(hon) FRS
(*Professor Emeritus of Medical Statistics, University of London*)

Amongst the objects of this newly-founded Section of Occupational Medicine are firstly ‘to provide a means, not readily afforded elsewhere, whereby physicians and surgeons with a special knowledge of the relationship between sickness and injury and conditions of work may discuss their problems, not only with each other, but also with colleagues in other fields, by holding joint meetings with other Sections of the Society’; and, secondly, ‘to make available information about the physical, chemical and psychological hazards of occupation, and in particular about those that are rare or not easily recognized’.

At this first meeting of the Section and before, with however laudable intentions, we set about instructing our colleagues in other fields, it will be proper to consider a problem fundamental to our own. How in the first place do we detect these relationships between sickness, injury and conditions of work? How do we determine what are physical, chemical and psychological hazards of occupation, and in particular those that are rare and not easily recognized?

There are, of course, instances in which we can reasonably answer these questions from the general body of medical knowledge. A particular, and perhaps extreme, physical environment cannot fail to be harmful; a particular chemical is known to be toxic to man and therefore suspect on the factory floor. Sometimes, alternatively, we may be able to consider what *might* a particular environment do to man, and then see whether such consequences are indeed to be found. But more often than not we have no such guidance, no such means of proceeding; more often than not we are dependent upon our observation and enumeration of defined events for which we then seek antecedents. In other words we see that the event B is associated with the environmental feature A, that, to take a specific example, some form of respiratory illness is associated with a dust in the environment. In what circumstances can we pass from this observed *association* to a verdict of *causation*? Upon what basis should we proceed to do so?

I have no wish, nor the skill, to embark upon a philosophical discussion of the meaning of ‘causation’. The ‘cause’ of illness may be immediate and direct, it may be remote and indirect underlying the observed association. But with the aims of occupational, and almost synonymously preventive, medicine in mind the decisive question is whether the frequency of the undesirable event B will be influenced by a change in the environmental feature A. *How* such a change exerts that influence may call for a great deal of research. However, before deducing ‘causation’ and taking action we shall not invariably have to sit around awaiting the results of that research. The whole chain may have to be unravelled or a few links may suffice. It will depend upon circumstances.

Disregarding then any such problem in semantics we have this situation. Our observations reveal an association between two variables, perfectly clear-cut and beyond what we would care to attribute to the play of chance. What aspects of that association should we especially consider before deciding that the most likely interpretation of it is causation?

(1) *Strength*. First upon my list I would put the strength of the association. To take a very old example, by comparing the occupations of patients with scrotal cancer with the occupations of patients presenting with other diseases, Percival Pott could reach a correct conclusion because of the *enormous* increase of scrotal cancer in the chimney sweeps. ‘Even as late as the second decade of the twentieth century’, writes Richard Doll (1964), ‘the mortality of chimney sweeps from scrotal cancer was some 200 times that of workers who were not specially exposed to tar or mineral oils and in the eighteenth century the relative difference is likely to have been much greater.’

To take a more modern and more general example upon which I have now reflected for over fifteen years, prospective inquiries into smoking have shown that the death rate from cancer of the lung in cigarette smokers is nine to ten times the rate in non-smokers and the rate in heavy cigarette smokers is twenty to thirty times

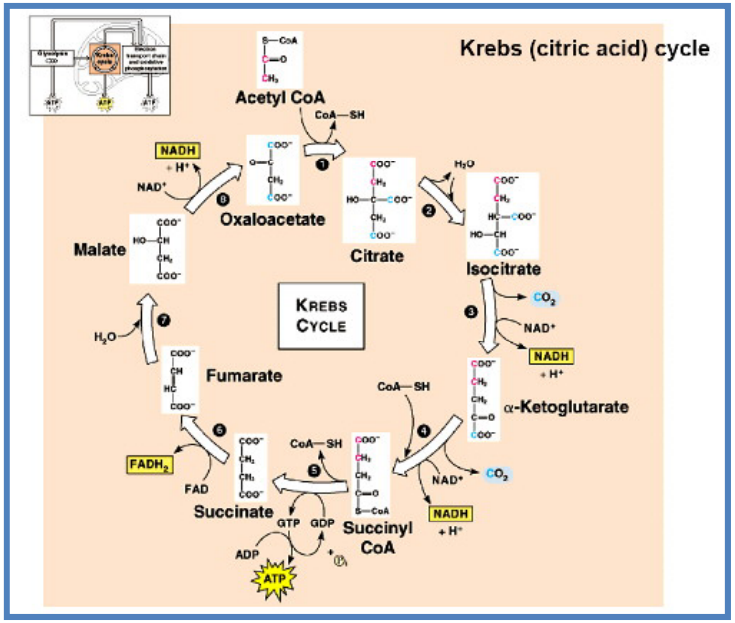
1965

Alcuni approcci al concetto di causalità in campo biomedico

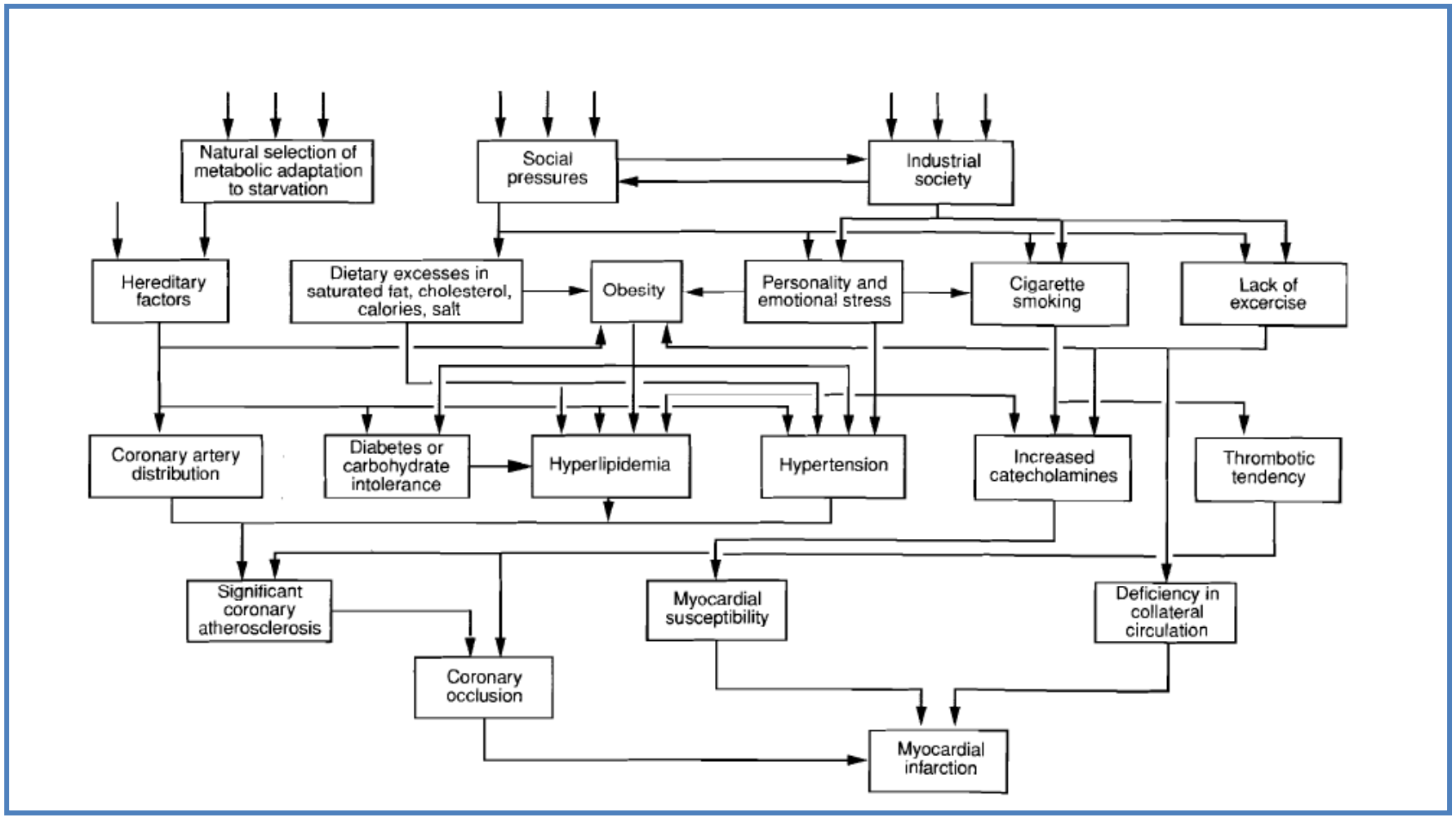
meccanicistiche: le connessioni causali possono essere comprese in termini di processi fisici (reazioni chimiche, alterazioni a livello cellulare,ecc)

"In it (the typical English book of physics) there are nothing but strings that move around pulleys, which roll around drums, which go through pearl beads, which carry weights, and tubes which pump water while others swell and contract . . . we thought we were entering the tranquil and neatly ordered abode of reason, but we find ourselves in a factory" (P Duhem, 1914)

"Duhem was writing about English physics, but the impression he would have of contemporary international molecular biology would surely be similar" (KF Schaffner, 1993)



Causal web



Causal-web model for myocardial infarction. (Source: Friedman, G. D. (1974). Primer of Epidemiology. *New York: McGraw-Hill;*

Sufficient-Component Causes

Causation and Causal Inference in Epidemiology

Kenneth J. Rothman, DrPH, Sander Greenland, MA, MS, DrPH, C Stat

American Journal of Public Health | Supplement 1, 2005, Vol 95, No. S1

A sufficient cause guarantees that its effect will occur; when the cause is present, the effect *must occur.*

A sufficient-component cause is made up of a number of components, no one of which is sufficient on its own but which taken together make up a sufficient cause.

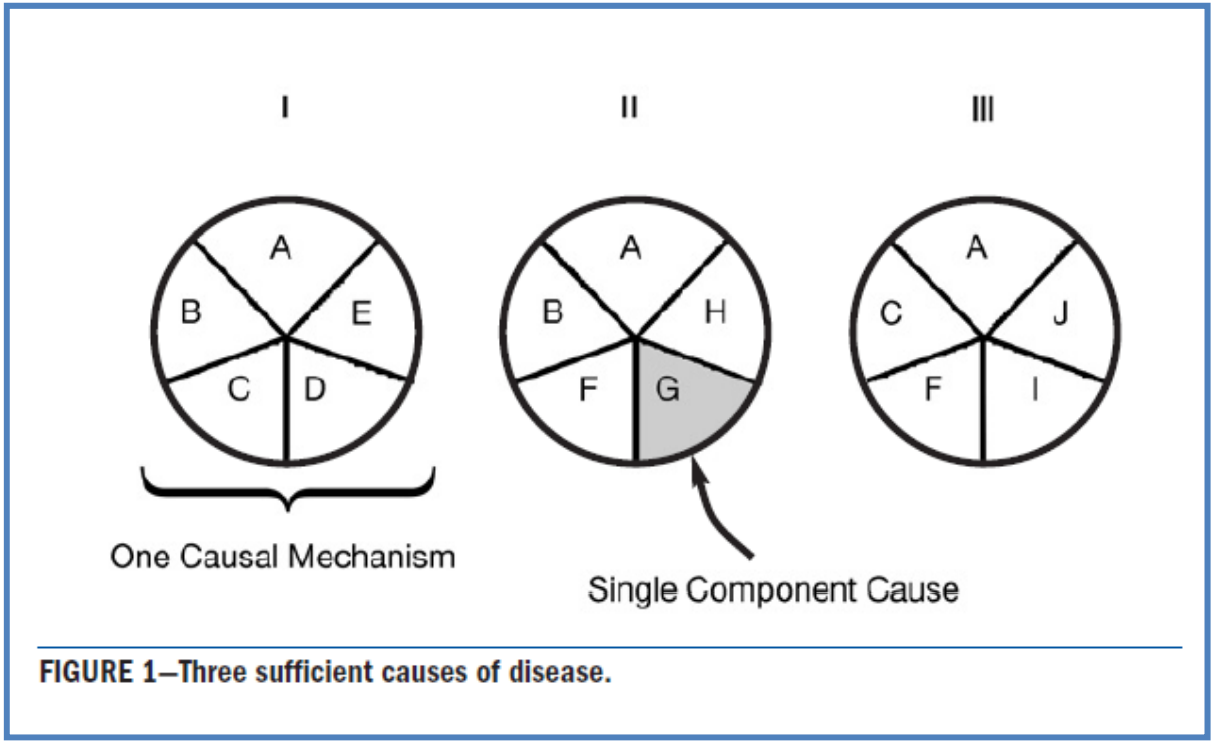


FIGURE 1—Three sufficient causes of disease.

Problema: determinismo

Inferenze statistiche: usate per mostrare che una causa produce una differenza negli effetti (modelli probabilistici, controfattuali, ecc)

Statistics and Causal Inference

PAUL W. HOLLAND*

Journal of the American Statistical Association
December 1986, Vol. 81, No. 396, Theory and Methods

The reaction of many statisticians when confronted with the possibility that their profession might contribute to a discussion of causation is immediately to deny that there is any such possibility. "That correlation is not causation is perhaps the first thing that must be said" (Barnard 1982, Some authors focus on the ultimate meaningfulness of the notion of causation. Others are concerned with deducing the causes of a given effect. Still others are interested in understanding the details of causal mechanisms. The emphasis here will be on *measuring the effects of causes* because this seems to be a place where statistics, which is concerned with measurement, has contributions to make. It is my opinion that an emphasis on the effects of causes rather than on the causes of effects is, in itself, an important consequence of bringing statistical reasoning to bear on the analysis of causation and directly opposes more traditional analyses of causation.

Teoria probabilistica della causalità

Patrick Suppes (1970)

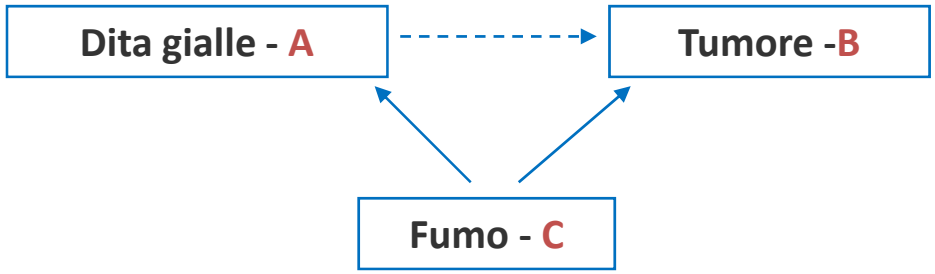
Roughly speaking, the modification of Hume's analysis I propose is to say that one event is the cause of another if the appearance of the first event is followed with a high probability by the appearance of the second, and there is no third event that we can use to factor out the probability relationship between the first and second events.

Dati due istanti di tempo: $t_1 < t_2$ **e** $P(C_{t1}) > 0$

Se: $P(B_{t2} \mid C_{t1}) > P(B_{t2} \mid \text{non-}C_{t1})$

C è una causa *"prima facie"* di B

Cause Spurie



Se **A** e **B** sono ambedue causate da un terzo fattore **C** potrebbe anche essere che:

$$P(B \mid A) > P(B \mid \text{non-}A)$$

Anche se **A** non causa **B**

Il modello di Neyman-Rubin

esiti potenziali

controfattualità

Le radici del modello risalgono agli anni 1920-1930 grazie ai contributi di Ronald A. **Fisher** e di Jerzy **Neyman** nell'ambito degli studi sperimentali.

Il modello è stato poi espanso da Donald **Rubin** negli anni '70 per ricomprendere il caso degli studi osservazionali.

The Design of Experiments

By

Sir Ronald A. Fisher, Sc.D., F.R.S.

Honorary Research Fellow, Division of Mathematical Statistics, C.S.I.R.O., University of Adelaide; Foreign Associate, United States National Academy of Sciences, and Foreign Honorary Member, American Academy of Arts and Sciences; Foreign Member of the Swedish Royal Academy of Sciences, and the Royal Danish Academy of Sciences and Letters; Member of the Pontifical Academy; Member of the German Academy of Sciences (Leopoldina); formerly Galton Professor, University of London, and Arthur Balfour Professor of Genetics, University of C

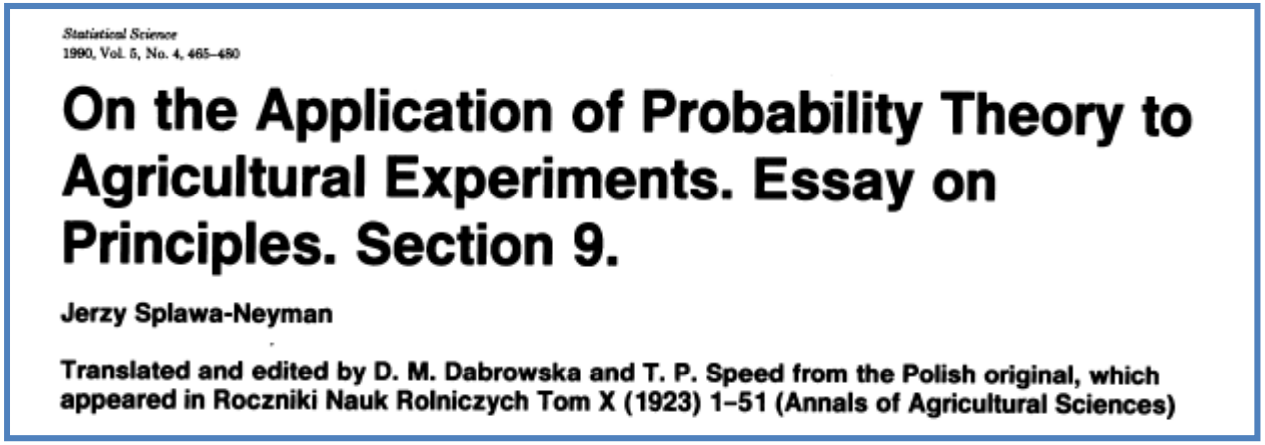
HAFNER PUBLISHING
New York

Fisher descrive la logica degli studi sperimentali

1935

CONTENTS

Neyman descrive gli effetti causati dai trattamenti come confronto di esiti potenziali (potential outcomes) sotto trattamenti alternativi.



Statistical Science
1990, Vol. 5, No. 4, 465–480

On the Application of Probability Theory to Agricultural Experiments. Essay on Principles. Section 9.

Jerzy Splawa-Neyman

Translated and edited by D. M. Dabrowska and T. P. Speed from the Polish original, which appeared in Roczniki Nauk Rolniczych Tom X (1923) 1–51 (Annals of Agricultural Sciences)

1923

Potential Outcomes (esiti potenziali): I valori assunti da una misura di interesse dopo l'applicazione del trattamento e la non applicazione del trattamento alla medesima unità

Journal of Educational Psychology
1974, Vol. 66, No. 5, 688-701

ESTIMATING CAUSAL EFFECTS OF TREATMENTS IN RANDOMIZED AND NONRANDOMIZED STUDIES[1]

DONALD B. RUBIN[2]

Educational Testing Service, Princeton, New Jersey

A discussion of matching, randomization, random sampling, and other methods of controlling extraneous variation is presented. The objective is to specify the benefits of randomization in estimating causal effects of treatments. The basic conclusion is that randomization should be employed whenever possible but that the use of carefully controlled nonrandomized data to estimate causal effects is a reasonable and necessary procedure in many cases.

Let y(E) be the value of Y measured[5] at t_2 on the unit, given that the unit received the experimental Treatment E initiated at t_1;

Let y(C) be the value of Y measured at t_2 on the unit given that the unit received the control Treatment C initiated at t_1;

Then y(E) − y(C) is the causal effect of the E versus C treatment on Y for that trial, that is, for that particular unit and the times t_1, t_2.

Rubin descrive il suo modello

1974

La struttura del modello: {Ui , T , Y, W}

Unità (U_i) **:** una persona, cosa, luogo sul quale opererà un trattamento ad un particolare istante

Trattamento (T) **:** un intervento, l'effetto del quale (attraverso una specifica misura sulle unità) il ricercatore vuole valutare in relazione al non-intervento (controllo)

Esiti potenziali (Y) : I valori di una misura di interesse dopo l'applicazione del trattamento **Y(1)** e la non applicazione del trattamento **Y(0)** alla medesima unità

Assignment Mechanism (W) : processo attraverso il quale si decide quali unità ricevono il trattamento e quali ricevono il controllo

Causal Effect (effetto causale) Per ciascuna unità $\mathbf{U_i}$ il confronto dell'esito potenziale in due situazioni: con il trattamento e senza il trattamento (controllo).

Y(1) – Y(0)

Stable unit treatment value assumption (SUTVA)

a) non vi è interferenza tra le unità
b) vi è un solo tipo di trattamento ed un solo tipo di controllo.

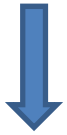
Lo stato di trattamento di una qualsiasi unità non influenza gli esiti potenziali delle altre unità.

In questa condizione l'insieme completo degli esiti potenziali in una popolazione di N unità può essere rappresentato in una tabella con due colonne ed N righe.

Unità	Trattamento Y(1)	Controllo Y(0)
1	$Y_1(1)$	$Y_1(0)$
.	.	.
.	.	.
J	$Y_j(1)$	$Y_j(0)$
.	.	.
N	$Y_N(1)$	$Y_N(0)$

Quale meccanismo di assegnazione del trattamento alle unità?

Quando l'assegnazione è **Random** la probabilità di ricevere il trattamento (*propensity score*) è uguale per ogni unità e non dipende dal valore di qualche esito potenziale non osservato.



Ignorable Assignment Mechanism

$$(Y1, Y0) \perp T$$

Statistics and Causal Inference

PAUL W. HOLLAND*

Journal of the American Statistical Association
December 1986, Vol. 81, No. 396, Theory and Methods

Fundamental Problem of Causal Inference. It is impossible to *observe* the value of $Y_t(u)$ and $Y_c(u)$ on the same unit and, therefore, it is impossible to *observe* the effect of t on u.

Unità	Trattamento Y(1)	Controllo Y(0)
1	$Y_1(1)$	?
2	?	$Y_2(0)$
3	?	$Y_3(0)$
4	$Y_4(1)$	?
5	?	$Y_5(0)$
6	$Y_6(1)$	?

Effetto causale medio = $\overline{Y(1)} - \overline{Y(0)}$

Lo sviluppo di uno studio randomizzato

Una dimostrazione per assurdo attraverso l'Ipotesi Nulla (H_0 : assenza di effetto).

sotto H_0 sarà, per ogni unità: **$Y(1) = Y(0) = Y_{obs}$**

Le unità sono assegnate random al trattamento e al controllo, generando una delle $\binom{N}{N/2}$ possibili assegnazioni (tutte equiprobabili)

Si osservano le risposte al trattamento e si misura l'effetto causale *osservato*

Unità	Trattamento Y(1)	Controllo Y(0)
1	?	55.0
2	?	72.0
3	?	72.7
4	70.0	?
5	66.0	?
6	78.9	?

Y (1) **-*Y (0)*** **=5.1**

Si derivano gli esiti *potenziali* non osservati usando l'ipotesi nulla e gli esiti osservati

Unità	Trattamento Y(1)	Controllo Y(0)
1	55.0	55.0
2	72.0	72.0
3	72.7	72.7
4	70.0	70.0
5	66.0	66.0
6	78.9	78.9

La sequenza di randomizzazione ottenuta è una delle 20 possibilmente osservabili per 6 unità

Si misura l'effetto del trattamento per ciascuna delle possibili assegnazioni random

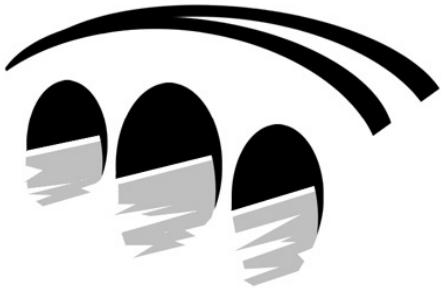
Sequenze di Randominizzazione	Y(1) - Y(0)	
111000	-5,1	
110100	-6,9	
110010	-9,5	
110001	-0,9	
101100	-6,4	
101010	-9,1	
101001	-0,5	
100110	-10,9	
100101	-2,3	
100011	-4,9	
011100	4,9	
011010	2,3	
011001	10,9	←
010110	0,5	
010101	9,1	←
010011	6,4	←
001110	0,9	
001101	9,5	←
001011	6,9	←
000111	**5,1**	← Effetto osservato

Si determina quanto sia estremo il valore dell'effetto osservato (livello di significatività,o p-value)

Studi randomizzati imperfetti (protocolli non seguiti esattamente):

Non compliance
Missing data

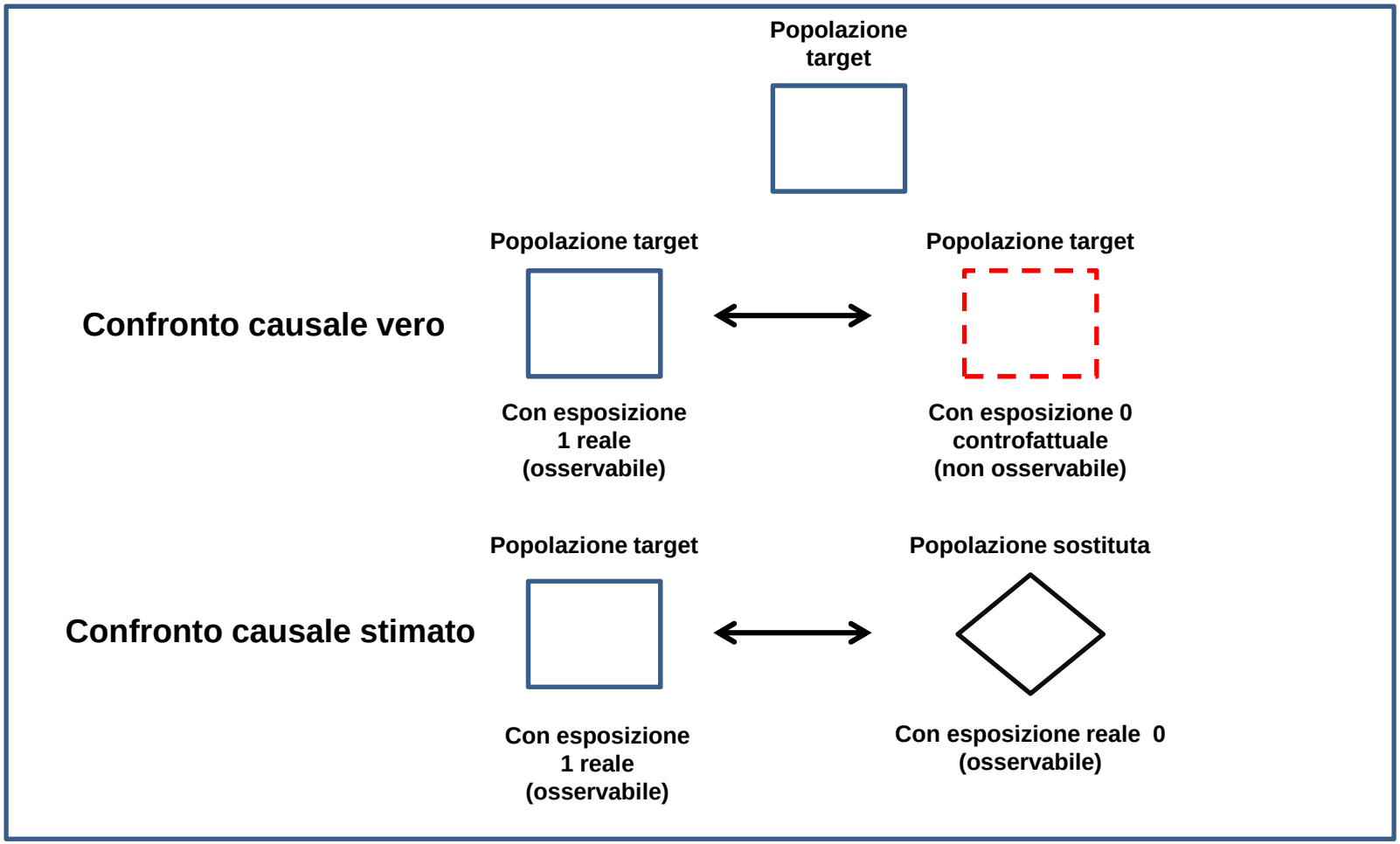
Un ponte verso gli **studi osservazionali**



Observational study: An attempt to draw inferences about the causal effect of an active treatment versus a control treatment based on data in which the investigator did not decide which units would receive treatment and which would receive control, but rather observed the assignments that the units received, according to an unknown assignment mechanism.

Rubin

Studi osservazionali



Il meccanismo di assegnazione del trattamento non è più completamente ignorabile. E' possibile fare però inferenze causali consistenti nella misura in cui la probabilità di trattamento è indipendente dagli esiti potenziali *dato l'insieme delle covariate osservate*

$$(Y1, Y0) \perp (T|X)$$

Interpretare la causalità in Sanità

Epidemiologic Perspectives & Innovations

BioMed Central

Published: 29 July 2005

Epidemiologic Perspectives & Innovations 2005, **2**:8

Analytic Perspective

Open Access

Causal thinking and causal language in epidemiology: it's in the details

Robert Lipton*[1] and Terje Ødegaard[2]

Abstract

Although epidemiology is necessarily involved with elucidating causal processes, we argue that there is little practical need, having described an epidemiological result, to then explicitly label it as causal (or not). Doing so is a convention which obscures the valuable core work of epidemiology as an important constituent of public health practice. We discuss another approach which emphasizes the public health "use value" of research findings in regard to prediction and intervention independent from explicit metaphysical causal claims. Examples are drawn from smoking and lung cancer, with particular focus on the original 1964 Surgeon General's report on smoking and the new version released in 2004. The intent is to help the epidemiologist focus on the pertinent implications of research, which, from a public health point of view, in large part entails the ability to predict and to intervene. Further discussion will center on the importance of differentiating between technical/practical uses of causal language, as might be used in structural equations or marginal structural modeling, and more foundational notions of cause. We show that statistical/epidemiological results, such as "smoking two packs a day increases risk of lung cancer by 10 times" are in themselves a kind of causal argument that are not in need of additional support from relatively ambiguous language such as "smoking causes lung cancer." We will show that the confusion stemming from the use of this latter statement is more than mere semantics. Our goal is to allow researchers to feel more confident in the power of their research to tell a convincing story without resorting to metaphysical/unsupportable notions of cause.

Epidemiologic Perspectives & Innovations

BioMed Central

Published: 06 June 2006

Epidemiologic Perspectives & Innovations 2006, **3**:7

Commentary

Open Access

Causal thinking and causal language in epidemiology: a cause by any other name is still a cause: *response to Lipton and Ødegaard*

Clarence C Tam*

"I can tell you that smoking two packs a day for N years increases your risk of lung cancer by 10 times".

"I can tell you with 95 percent certainty that smoking two packs a day for N years increases your risk of lung cancer by between A and B times".

"I can tell you with 95 percent certainty that smoking two packs a day for N years increases your risk of lung cancer by between A and B times, assuming that there is no systematic error in my observations"

"I can tell you with 95 percent certainty that smoking two packs a day for N years increases your risk of lung cancer by between A and B times, and I have tried to correct for biases C, D and E using prior distributions S, T and U, which I believe (though I cannot be certain) are rational and exhaustive"

In generale si potrebbe affermare che, nella ricerca in campo biomedico, si perseguono due obiettivi:

cognitivo (explanation) : **identificare fattori causali e i meccanismi di una patologia**
refers to the classical epistemological question of how causes are discovered and which is the most effective model of explanation Vineis P Causality in epidemiology Soz.- Präventivmed. 48 (2003) 80–87

di intervento (inference) : **orientare le politiche sanitarie**
refers to the "burden of proof" which is needed to consider an agent as a cause of disease Vineis P Causality in epidemiology Soz.- Präventivmed. 48 (2003) 80–87

La questione centrale rimane quando e attraverso quali criteri riteniamo che la conoscenza acquisita sia sufficiente per giustificare una azione, questione questa che coinvolge le politiche, i valori sociali ed economici e che dovrebbe coinvolgere sempre più gli epidemiologi.