



Online article and related content  
current as of November 8, 2009.

## Physician Interpretations and Textbook Definitions of Blinding Terminology in Randomized Controlled Trials

P. J. Devereaux; Braden J. Manns; William A. Ghali; et al.

*JAMA*. 2001;285(15):2000-2003 (doi:10.1001/jama.285.15.2000)

<http://jama.ama-assn.org/cgi/content/full/285/15/2000>

Correction

[Contact me if this article is corrected.](#)

Citations

[This article has been cited 77 times.](#)  
[Contact me when this article is cited.](#)

Topic collections

Statistics and Research Methods; Randomized Controlled Trial  
[Contact me when new articles are published in these topic areas.](#)

Related Articles published in  
the same issue

The CONSORT Statement: Revised Recommendations for Improving the Quality of Reports of Parallel-Group Randomized Trials  
[David Moher et al. \*JAMA\*. 2001;285\(15\):1987.](#)

Use of the CONSORT Statement and Quality of Reports of Randomized Trials: A Comparative Before-and-After Evaluation  
[David Moher et al. \*JAMA\*. 2001;285\(15\):1992.](#)

Value of Flow Diagrams in Reports of Randomized Controlled Trials  
[Matthias Egger et al. \*JAMA\*. 2001;285\(15\):1996.](#)

CONSORT Revised—Improving the Reporting of Randomized Trials  
[Drummond Rennie. \*JAMA\*. 2001;285\(15\):2006.](#)

April 18, 2001  
[JAMA. 2001;285\(15\):2023.](#)

Subscribe

<http://jama.com/subscribe>

Permissions

[permissions@ama-assn.org](mailto:permissions@ama-assn.org)  
<http://pubs.ama-assn.org/misc/permissions.dtl>

Email Alerts

<http://jamaarchives.com/alerts>

Reprints/E-prints

[reprints@ama-assn.org](mailto:reprints@ama-assn.org)

# Physician Interpretations and Textbook Definitions of Blinding Terminology in Randomized Controlled Trials

P. J. Devereaux, MD

Braden J. Manns, MD

William A. Ghali, MD, MPH

Hude Quan, MD, PhD

Christina Lacchetti, MHSc

Victor M. Montori, MD

Mohit Bhandari, MD, MSc

Gordon H. Guyatt, MD, MSc

**T**O ASSESS THE VALIDITY OF A RANDOMIZED controlled trial (RCT), clinicians should note whether participants, health care providers, data collectors, and those assessing occurrence of target events of interest (judicial assessors of outcomes) are blind to participant allocation to treatment or control.<sup>1,2</sup> Some methodologists have suggested that allocation should also be concealed from data analysts and personnel writing the paper.<sup>3,4</sup> Authors of RCTs frequently use the terms “single,” “double,” and “triple” blind to communicate the blinding status of persons involved in the trials. We suspected that physicians and textbooks vary in their interpretations and definitions of these terms. Therefore, we surveyed physicians and systematically reviewed textbooks to test this hypothesis. A review of 200 RCTs provided an estimate of the clarity with which investigators identify who is blinded in studies they describe as single-, double-, and triple-blind.

See also pp 1987, 1992, 1996, and 2006.

**Context** When clinicians assess the validity of randomized controlled trials (RCTs), they commonly evaluate the blinding status of individuals in the RCT. The terminology authors often use to convey blinding status (single, double, and triple blinding) may be open to various interpretations.

**Objective** To determine physician interpretations and textbook definitions of RCT blinding terms.

**Design and Setting** Observational study undertaken at 3 Canadian university tertiary care centers between February and May 1999.

**Participants** Ninety-one internal medicine physicians who responded to a survey.

**Main Outcome Measures** Respondents identified which of the following groups they thought were blinded in single-, double-, and triple-blinded RCTs: participants, health care providers, data collectors, judicial assessors of outcomes, data analysts, and personnel who write the article. Definitions from 25 systematically identified textbooks published since 1990 providing definitions for single, double, or triple blinding.

**Results** Physician respondents identified 10, 17, and 15 unique interpretations of single, double, and triple blinding, respectively, and textbooks provided 5, 9, and 7 different definitions of each. The frequencies of the most common physician interpretation and textbook definition were 75% (95% confidence interval [CI], 65%-83%) and 74% (95% CI, 52%-90%) for single blinding, 38% (95% CI, 28%-49%) and 43% (95% CI, 24%-63%) for double blinding, and 18% (95% CI, 10%-28%) and 14% (95% CI, 0%-58%) for triple blinding, respectively.

**Conclusions** Our study suggests that both physicians and textbooks vary greatly in their interpretations and definitions of single, double, and triple blinding. Explicit statements about the blinding status of specific groups involved in RCTs should replace the current ambiguous terminology.

JAMA. 2001;285:2000-2003

www.jama.com

## METHODS

### Target Population and Data Collection

We surveyed attending physicians between February and May 1999 within departments of medicine at 3 Canadian university tertiary care centers: Dalhousie University, McMaster University, and University of Calgary Foothills Hospital. Respondents completed a survey that defined the 6 groups who are potential candidates for blinding in an

**Author Affiliations:** Department of Medicine, Dalhousie University, Halifax, Nova Scotia (Dr Devereaux); Departments of Clinical Epidemiology and Biostatistics (Ms Lacchetti and Dr Guyatt), Surgery (Dr Bhandari), and Medicine (Dr Guyatt), McMaster University, Hamilton, Ontario; Departments of Medicine (Drs Manns and Ghali) and Community Health Sciences (Drs Manns, Ghali, and Quan), University of Calgary, Calgary, Alberta; and Department of Medicine, Mayo Clinic and Foundation, Rochester, Minn (Dr Montori). Dr Devereaux is now with the Department of Medicine, McMaster University.

**Corresponding Author and Reprints:** P. J. Devereaux, MD, Department of Medicine, McMaster University, 1200 Main St W, Room 2C12, Hamilton, Ontario, Canada L8N 3Z5 (e-mail: philipj@mcmaster.ca).

RCT: participants, health care providers, data collectors, data analysts, judicial assessors of the outcomes (those ultimately deciding whether a participant meets the criteria for a study's outcome), and personnel writing the paper (those writing either of 2 drafts of a paper prior to the breaking of the randomization code, with draft 1 written assuming that group A is the treatment group and draft 2 assuming that group B is the treatment group). We randomized the order in which the 6 groups were listed. Respondents offered their opinions concerning which group(s) is blinded in single-, double-, and triple-blinded trials. Physicians could include as many groups as they thought appropriate for each blinding term but they were only allowed to provide 1 definition per term.

Using the terms *clinical epidemiology*, *randomized controlled trial*, and *evidence-based medicine*, we systematically identified textbooks published since 1990 with definitions for single, double, and/or triple blinding at the university libraries of McMaster and Dalhousie Universities. Two of us (P.J.D., C.L.) independently evaluated the textbooks, recorded all definitions of the blinding terminology, and resolved disagreements by consensus. The chance-correct agreements, assessed through means of a  $\kappa$  statistic, were near perfect ( $\kappa \geq 0.9$ ).

Starting with June 2000, we systematically identified the 40 most recent RCTs published in the *Annals of Internal Medicine*, *BMJ*, *JAMA*, *The Lancet*, and *The New England Journal of Medicine*. Two of us (V.M.M., M.B.) independently evaluated the RCTs for reporting of single, double, and triple blinding and the blinding status of the 6 groups identified above. Duplicate evaluation of the first 60 RCTs established near-perfect agreement for all variables assessed ( $\kappa \geq 0.8$ ).

### Data Analysis

We determined the proportion of physician respondents who chose each interpretation, the frequency of the various textbook definitions, and the

associated 95% confidence intervals (CIs). We also performed the Fisher exact test to assess heterogeneity in physician interpretation of single, double, and triple blinding across the 3 sites.<sup>5</sup> To avoid cells with sparse data, we collapsed all but the 2 most frequently chosen options into a single category when conducting these tests. We determined the proportion of RCTs reporting the terms single, double, and triple blinding; within these studies we then determined the frequency of blinding among the groups identified above.

### RESULTS

All 23 Dalhousie University cardiologists, 46 of the 52 McMaster University general internists, and 22 of the 24 University of Calgary Foothills Hospital general internists completed the survey (response rate, 92%). Of the 91 respondents, 83 provided an interpretation of triple blinding.

Of the 25 textbooks<sup>2,3,6-28</sup> that fulfilled our entry criteria, 17, 25, and 5 provided a definition for single, double, and triple blinding, respectively. A number of textbooks provided 2 definitions for single ( $n=6$ ), double ( $n=3$ ), and triple ( $n=2$ ) blinding.

The TABLE demonstrates that 75% (95% CI, 65%-83%) of physicians believed and 74% (95% CI, 52%-90%) of textbooks reported that study subjects were unaware of allocation in single-blind studies. The remaining 25% of physicians chose 9 alternative interpretations; 26% of textbooks provided 4 alternative definitions. Physicians chose 17 different interpretations of double blinding and textbooks provided 9 different definitions, the most frequent of which, blinding of both patients and their caregivers, was chosen and defined by 38% (95% CI, 28%-49%) and 43% (95% CI, 24%-63%) of physicians and textbooks, respectively. Physicians identified 15 different interpretations and textbooks provided 7 unique definitions for triple blinding. Only 18% (95% CI, 10%-28%) of the physicians and 14% (95% CI, 0%-58%) of the textbooks selected or defined the most frequent category of triple blinding.

The analyses for heterogeneity in physician interpretation of blinding terminology demonstrated similar variation in interpretation across sites, with  $P=.96$  for single blinding,  $P=.91$  for double blinding, and  $P=.13$  for triple blinding.

Of the 200 RCTs evaluated, 5 reported being single-blind and 83 double-blind. In the 5 studies reported as single-blind, 1 study made no mention of which group was blinded, 2 studies identified 1 group as blinded, 1 study identified 2 groups as blinded, and 1 study identified 3 groups as blinded. In the 83 studies reported as double-blind, 41 made no mention of which groups were blinded, 29 studies identified 1 group as blinded, 11 studies identified 2 groups as blinded, 1 study identified 3 groups as blinded, and 1 study identified 4 groups as blinded.

### COMMENT

While methodologists advocate assessing the blinding status of individuals involved in an RCT when determining trial validity,<sup>1-3,13,16,19,24,29</sup> clinicians depend on authors and editors to present blinding information in a transparent form. Authors often rely on the terms "single," "double," and "triple" blind to convey the blinding status of individuals involved in their study. Indeed, these terms, particularly double blind, have become almost a matter of convention.<sup>30</sup>

Our study demonstrates important variation among physicians in their interpretation of who is unaware of allocation when authors use the terms "single," "double," and "triple" blind. This variability suggests that the current terminology hinders readers who are trying to accurately assess trial validity. The variation in textbook definitions demonstrates that there is no consensus among authorities as to what these terms mean. Our survey of 200 recent RCTs in high-impact journals demonstrated that, in 50% of trials, authors who use the term "double-blind" failed to make any statement about who was blinded, and specified only 1 group—likely omitting information about 1 or more other groups who were blinded—in another 35%.

Given the variability we found, the current blinding terminology is failing to achieve the basic objectives of clear research communication. The likelihood that knowledge of patient allocation introduces bias makes this problem serious. The placebo effect can inflate the size of the treatment effect in

studies when participants are not blinded.<sup>31,32</sup> When unblinded, clinicians may differentially administer powerful treatments other than those under study, influence a patient's compliance with study medication or willingness to continue in the study, and affect patient reporting of symptoms.<sup>1,33</sup>

Unblinded data collectors can distort trial results based on intensity of examination, the likelihood of repeating a test for an unexpected finding, the recording of outcomes, or differential encouragement during performance testing.<sup>2,3,34</sup> Unblinded judicial assessors can bias the interpretation of marginal findings.<sup>1,2</sup> Unblinded data analysts and authors can introduce bias through decisions on patient withdrawals, selection of outcomes to analyze or report, choice of time points demonstrating maximum or minimum effects, and a myriad of other decisions in the analysis or reporting process.<sup>3,4,35,36</sup>

The impact of the blinding status of each of these groups remains uncertain. The only studies assessing the influence of blinding status on trial outcomes have focused on the reporting of double blinding. This research has demonstrated conflicting findings: 2 studies have suggested that RCTs without mention of double blinding are more likely to favor the experimental group than RCTs with mention of double blinding,<sup>37,38</sup> whereas 1 study failed to confirm this association.<sup>30</sup> Variability in who was actually blinded in reports of double-blind trials may account for these discrepant findings.

Our study is limited in that we surveyed physicians within the departments of medicine at only 3 academic institutions in Canada. However, given that McMaster University has made special efforts to educate its internists in critical appraisal of the medical literature, one would expect the highest likelihood of consistent interpretation in this institution. The results are remarkably similar across the 3 sites of our survey and are consistent with the varied definitions provided by textbooks, suggesting that our findings may be widely generalizable.

Our study has demonstrated enormous ambiguity in the conventional ways of describing blinding. Our results suggest that authors and journal editors should abandon the terms single, double, and triple blind, and substitute descriptions stating which of the relevant groups were unaware of alloca-

**Table.** Physician Interpretations and Textbook Definitions of Single, Double, and Triple Blinding

Interpretations and Definitions	% (No./Total)	
	Physicians	Textbook Definitions
<b>Single blinding</b>		
Participants	75 (68/91)	74 (17/23)
Participants and health care providers	9 (8/91)	0
Health care providers	4 (4/91)	9 (2/23)
Investigators*	NA†	9 (2/23)
Judicial assessors‡	3 (3/91)	4 (1/23)
Data collectors	2 (2/91)	4 (1/23)
Other groups	7 (6/91)	0
<b>Double blinding</b>		
Participants and health care providers	38 (35/91)	43 (12/28)
Participants and investigators*	NA†	21 (6/28)
Participants and judicial assessors‡	5 (5/91)	14 (4/28)
Participants, health care providers, data collectors, and judicial assessors‡	13 (12/91)	0
Participants, health care providers, data collectors, judicial assessors,‡ and data analysts	10 (9/91)	0
Participants, health care providers, and data collectors	7 (6/91)	0
Participants, health care providers, data collectors, and data analysts	7 (6/91)	0
Participants and data collectors	5 (5/91)	4 (1/28)
Judicial assessors‡ and assignment to treatment or control	NA†	4 (1/28)
Participants, health care providers, and judicial assessors‡	1 (1/91)	4 (1/28)
Participants, health care providers, investigators,* and health care personnel§	NA†	4 (1/28)
Other groups	13 (12/91)	7 (2/28)
<b>Triple blinding</b>		
Participants, health care providers, data collectors, judicial assessors,‡ and data analysts	18 (15/83)	0
Participants, health care providers, data collectors, judicial assessors,‡ data analysts, and authors	18 (15/83)	0
Participants, health care providers, and data collectors	16 (13/83)	0
Participants, health care providers, and data analysts	10 (8/83)	14 (1/7)
Participants, health care providers, and judicial assessors‡	8 (7/83)	14 (1/7)
Participants, data collectors, and data analysts	1 (1/83)	14 (1/7)
Participants, data analysts, and investigators*	NA†	14 (1/7)
Participants, judicial assessors,‡ and data analysts	0	14 (1/7)
Participants, data collectors, and judicial assessors‡	7 (6/83)	0
Participants, investigators,* and monitors/sponsors*	NA‡	14 (1/7)
Participants, health care providers, data collectors, and judicial assessors‡	7 (6/83)	0
Other groups	14 (12/83)	14 (1/7)

\*Not defined further.

†Includes a term that was not an option available for physicians; NA indicates not applicable.

‡Judicial assessors of outcomes.

§Laboratory technician, pharmacist, or others.

tion. This change in reporting would be consistent with the Consolidated Standards of Reporting Trials (CONSORT) statement, which calls for reporting of the blinding status of the specific groups involved in RCTs.<sup>39</sup> As long as journal reports of RCTs include the term "double blind," clinicians will risk inaccurate inferences

about the validity of the studies on which they base their clinical practice.

**Author Contributions:** *Study concept and design:* Devereaux, Manns, Ghali, Guyatt.  
*Acquisition of data:* Devereaux, Manns, Ghali, Guyatt, Lacchetti, Montori, Bhandari.  
*Analysis and interpretation of data:* Devereaux, Manns, Ghali, Guyatt, Quan, Montori, Bhandari.  
*Drafting of the manuscript:* Devereaux.  
*Critical revision of the manuscript for important intellectual content:* Devereaux, Manns, Ghali,

Guyatt, Quan, Lacchetti, Montori, Bhandari.  
*Statistical expertise:* Manns, Ghali, Guyatt, Quan.  
*Administrative, technical, or material support:* Manns, Ghali, Guyatt, Lacchetti.

*Study supervision:* Ghali, Guyatt.

**Funding/Support:** Dr Devereaux is supported by a Heart and Stroke Foundation of Canada/Canadian Institutes of Health Research Award; Dr Manns is supported by a Kidney Foundation of Canada/Alberta Heritage Foundation for Medical Research Fellowship Award; Dr Ghali is supported by an Alberta Heritage Foundation for Medical Research Population Health Investigator Award.

## REFERENCES

- Guyatt GH, Sackett DL, Cook DJ, for the Evidence-Based Medicine Working Group. Users' guides to the medical literature, II: how to use an article about therapy and prevention, A: are the results of the study valid? *JAMA*. 1993;270:2598-2601.
- Greenhalgh T. *How to Read a Paper: The Basics of Evidence-Based Medicine*. London, England: BMJ Publishing Group; 1997:39, 62-63.
- Jadad A. *Randomised Controlled Trials*. London, England: BMJ Publishing Group; 1998:20-36.
- Gøtzsche PC. Blinding during data analysis and writing of manuscripts. *Control Clin Trials*. 1996;17:285-290.
- Mehta CR, Patel NR. A network algorithm for performing Fisher's exact test in RxC contingency tables. *J Am Stat Assoc*. 1983;78:427-434.
- Christie D, Gordon I, Heller R. *Epidemiology: An Introductory Text for Medical and Other Health Science Students*. Sydney, Australia: University of New South Wales Press Ltd; 1997:91.
- Dixon RA, Munro JF, Silcocks PB. *The Evidence-Based Medicine Workbook*. Oxford, England: Butterworth-Heinemann; 1997:5.
- Dunn G, Everitt B. *Clinical Biostatistics: An Introduction to Evidence-Based Medicine*. New York, NY: Halstead Press; 1995:120-121.
- Elwood M. *Critical Appraisal of Epidemiological Studies and Clinical Trials*. New York, NY: Oxford University Press; 1998:8, 9, 70-71.
- Fletcher RH, Fletcher SW, Wagner EH. *Clinical Epidemiology: The Essentials*. Philadelphia, Pa: Williams & Wilkins; 1996:148-149.
- Friedland DJ. *Evidence-Based Medicine: A Framework for Clinical Practice*. Stanford, Conn: Appleton & Lange; 1998:163.
- Friedman GD. *Primer of Epidemiology*. New York, NY: McGraw-Hill Inc; 1994:163.
- Gray JAM. *Evidence-Based Healthcare: How to Make Health Policy and Management Decisions*. New York, NY: Churchill Livingstone Inc; 1997:78-86.
- Greenberg RS, Daniels SR, Flanders WD, Eley JW, Boring JR. *Medical Epidemiology*. Norwalk, Conn: Appleton & Lange; 1996:93-94.
- Hassard TH. *Understanding Biostatistics*. St Louis, Mo: Mosby-Year Book Inc; 1991:156-157.
- Ingelfinger JA, Mosteller F, Thibodeau LA, Ware JH. *Biostatistics in Clinical Medicine*. 3rd ed. New York, NY: McGraw-Hill Inc; 1994:265-266.
- Jones R, Kinmonth AL. *Critical Reading for Primary Care*. New York, NY: Oxford University Press; 1995:221.
- Knapp RG, Miller MC. *Clinical Epidemiology and Biostatistics*. Baltimore, Md: Williams & Wilkins; 1992:136.
- McKibbin A, Eady A, Marks S. *PDQ Evidence-Based Principles and Practice*. Hamilton, Ontario: BC Decker Inc; 1999:33-60.
- Norell SE. *Workbook of Epidemiology*. New York, NY: Oxford University Press; 1995:151.
- Riegelman RK, Hirsh RP. *Studying a Study and Testing a Test*. New York, NY: Little Brown & Co; 1996:8, 97.
- Riffenburgh RH. *Statistics in Medicine*. San Diego, Calif: Academic Press; 1999:18.
- Rosser WW, Shafir MS. *Evidence-Based Family Medicine*. Hamilton, Ontario: BC Decker Inc; 1998:15-16.
- Sackett DL, Richardson WS, Rosenberg W, Haynes RB. *Evidence-Based Medicine: How to Practice and Teach EBM*. New York, NY: Churchill Livingstone; 1997:91-96.
- Spilker B. *Guide to Clinical Trials*. New York, NY: Raven Press; 1991:15-17.
- Valanis B. *Epidemiology in Health Care*. Stanford, Conn: Appleton & Lange; 1999:361-362.
- Wassertheil-Smoller S. *Biostatistics and Epidemiology: A Primer for Health Professionals*. 2nd ed. New York, NY: Springer-Verlag; 1995:130.
- Whisnant JP. *Stroke: Populations, Cohorts and Clinical Trials*. Oxford, England: Butterworth-Heinemann; 1993:26-27.
- Crombie IK. *The Pocket Guide to Critical Appraisal*. London, England: BMJ Publishing Group; 1996:43-49.
- Moher D, Pham B, Jones A, et al. Does quality of reports of randomised trials affect estimates of intervention efficacy reported in meta-analyses? *Lancet*. 1998;352:609-613.
- Benson H, McCallie DP. Angina pectoris and the placebo effect. *N Engl J Med*. 1979;300:1424-1429.
- Bowling A. *Research Methods in Health*. Philadelphia, Pa: Open University Press; 1998:197-198.
- Karlowski TR, Chalmers TC, Frenkel LD, Kapikian AZ, Lewis TL, Lynch JM. Ascorbic acid for the common cold: a prophylactic therapeutic trial. *JAMA*. 1975;231:1038-1042.
- Guyatt GH, Pugsley SO, Sullivan MJ, et al. Effect of encouragement on walking test performance. *Thorax*. 1984;39:818-822.
- May GS, DeMets DL, Friedman LM, Furberg C, Passamani E. The randomized clinical trial: bias in analysis. *Circulation*. 1981;64:669-673.
- Gøtzsche PC. Methodology and overt and hidden bias in reports of 196 double-blinded trials of nonsteroidal, anti-inflammatory drugs in rheumatoid arthritis [published correction appears in *Control Clin Trials*. 1989; 50:356]. *Control Clin Trials*. 1989;10:31-56.
- Colditz GA, Miller JN, Mosteller F. How study design affects outcomes in comparisons of therapy, I: medical. *Stat Med*. 1989;8:441-454.
- Schulz KF, Chalmers I, Hayes RJ, Altman DG. Empirical evidence of bias: dimensions of methodological quality associated with estimates of treatment effects in controlled trials. *JAMA*. 1995;273:408-412.
- Begg C, Cho M, Eastwood S, et al. Improving the quality of reporting of randomized controlled trials: the CONSORT statement. *JAMA*. 1996;276:637-639.