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Statistical Methods in Psychology Journals
Guidelines and Explanations

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Task Force on Statistical Inference

In the light of continuing debate over the applications of significance testing in psychology journals and following the publication of Cohen's (1994) article, the Board of Scientific Affairs (BSA) of the American Psychological Association (APA) convened a committee called the Task Force on Statistical Inference (TFSI) whose charge was "to elucidate some of the controversial issues surrounding applications of statistics including significance testing and its alternatives; alternative underlying models and data transformation; and newer methods made possible by powerful computers" (BSA, personal communication, February 28, 1996). Robert Rosenthal, Robert Abelson, and Jacob Cohen (cochairs) met initially and agreed on the desirability of having several types of specialists on the task force: statisticians, teachers of statistics, journal editors, authors of statistics books, computer experts, and wise elders. Nineindividuals were subsequently invited to join and all agreed. These were Leona Aiken, Mark Appelbaum, Gwyneth Boodoo, David A. Kenny, Helena Kraemer, Donald Rubin, Bruce Thompson, Howard Wainer, and Leland Wilkinson. In addition, Lee Cronbach, Paul Meehl, Frederick Mosteller and John Tukey served as Senior Advisors to the Task Force and commented on written materials.

The TFSI met twice in two years and corresponded throughout that period. After the first meeting, the task force circulated a preliminary report indicating its intention to examine issues beyond null hypothesis significance testing. The task force invited comments and used this feedback in the deliberations during its second meeting.

After the second meeting, the task force recommended several possibilities for further action, chief of which would be to revise the statistical sections of the *American Psychological Association Publication Manual* (APA, 1994). After extensive discussion, the BSA recommended that "before the TFSI undertook a revision of the *APA Publication Manual,* it might want to consider publishing an article in *American Psychologist,* as a way to initiate discussion in the field about changes in current practices of data analysis and reporting" (BSA, personal communication, November 17, 1997).

This report follows that request. The sections in italics are proposed guidelines that the TFSI recommends could be used for revising the APA publication manual or for developing other BSA supporting materials. Following each guideline are comments, explanations, or elaborations assembled by Leland Wilkinson for the task force and under its review. This report is concerned with the use of

statistical methods only and is not meant as an assessment of research methods in general. Psychology is a broad science. Methods appropriate in one area may be inappropriate in another.

The title and format of this report are adapted from a similar article by Bailar and Mosteller (1988) . That article should be consulted, because it overlaps somewhat with this one and discusses some issues relevant to research in psychology. Further detail can also be found in the publications on this topic by several committee members (Abelson, 1995 , 1997 ; Rosenthal, 1994 ; Thompson, 1996 ; Wainer, in press ; see also articles in Harlow, Mulaik, & Steiger, 1997).

Method

Design

Make clear at the outset what type of study you are doing. Do not cloak a study in one guise to try to give it the assumed reputation of another. For studies that have multiple goals, be sure to define and prioritize those goals.

There are many forms of empirical studies in psychology, including case reports, controlled experiments, quasi-experiments, statistical simulations, surveys, observational studies, and studies of studies (meta-analyses). Some are hypothesis generating: They explore data to form or sharpen hypotheses about a population for assessing future hypotheses. Some are hypothesis testing: They assess specific a priori hypotheses or estimate parameters by random sampling from that population. Some are meta-analytic: They assess specific a priori hypotheses or estimate parameters (or both) by synthesizing the results of available studies.

Some researchers have the impression or have been taught to believe that some of these forms yield information that is more valuable or credible than others (see Cronbach, 1975 , for a discussion). Occasionally proponents of some research methods disparage others. In fact, each form of research has its own strengths, weaknesses, and standards of practice.

Population

The interpretation of the results of any study depends on the characteristics of the population intended for analysis. Define the population (participants, stimuli, or studies) clearly. If control or comparison groups are part of the design, present how they are defined.

Psychology students sometimes think that a statistical population is the human race or, at least, college sophomores. They also have some difficulty distinguishing a class of objects versus a statistical population–that sometimes we make inferences about a population through statistical methods, and other times we make inferences about a class through logical or other nonstatistical methods. Populations may be sets of potential observations on people, adjectives, or even research articles. How a population is

defined in an article affects almost every conclusion in that article.

Sample

Describe the sampling procedures and emphasize any inclusion or exclusion criteria. If the sample is stratified (e.g., by site or gender) describe fully the method and rationale. Note the proposed sample size for each subgroup.

Interval estimates for clustered and stratified random samples differ from those for simple random samples. Statistical software is now becoming available for these purposes. If you are using a convenience sample (whose members are not selected at random), be sure to make that procedure clear to your readers. Using a convenience sample does not automatically disqualify a study from publication, but it harms your objectivity to try to conceal this by implying that you used a random sample. Sometimes the case for the representativeness of a convenience sample can be strengthened by explicit comparison of sample characteristics with those of a defined population across a wide range of variables.

Assignment Random assignment.

For research involving causal inferences, the assignment of units to levels of the causal variable is critical. Random assignment (not to be confused with random selection) allows for the strongest possible causal inferences free of extraneous assumptions. If random assignment is planned, provide enough information to show that the process for making the actual assignments is random.

There is a strong research tradition and many exemplars for random assignment in various fields of psychology. Even those who have elucidated quasi-experimental designs in psychological research (e.g., Cook & Campbell, 1979) have repeatedly emphasized the superiority of random assignment as a method for controlling bias and lurking variables. "Random" does not mean "haphazard." Randomization is a fragile condition, easily corrupted deliberately, as we see when a skilled magician flips a fair coin repeatedly to heads, or innocently, as we saw when the drum was not turned sufficiently to randomize the picks in the Vietnam draft lottery. As psychologists, we also know that human participants are incapable of producing a random process (digits, spatial arrangements, etc.) or of recognizing one. It is best not to trust the random behavior of a physical device unless you are an expert in these matters. It is safer to use the pseudorandom sequence from a well-designed computer generator or from published tables of random numbers. The added benefit of such a procedure is that you can supply a random number seed or starting number in a table that other researchers can use to check your methods later.

Nonrandom assignment.

For some research questions, random assignment is not feasible. In such cases, we need to minimize effects of variables that affect the observed relationship between a causal variable and an outcome. Such variables are commonly called confounds or covariates. The researcher needs to attempt to determine the relevant covariates, measure them adequately, and adjust for their effects either by design or by

analysis. If the effects of covariates are adjusted by analysis, the strong assumptions that are made must be explicitly stated and, to the extent possible, tested and justified. Describe methods used to attenuate sources of bias, including plans for minimizing dropouts, noncompliance, and missing data.

Authors have used the term "control group" to describe, among other things, (a) a comparison group, (b) members of pairs matched or blocked on one or more nuisance variables, (c) a group not receiving a particular treatment, (d) a statistical sample whose values are adjusted post hoc by the use of one or more covariates, or (e) a group for which the experimenter acknowledges bias exists and perhaps hopes that this admission will allow the reader to make appropriate discounts or other mental adjustments. None of these is an instance of a fully adequate control group.

If we can neither implement randomization nor approach total control of variables that modify effects (outcomes), then we should use the term "control group" cautiously. In most of these cases, it would be better to forgo the term and use "contrast group" instead. In any case, we should describe exactly which confounding variables have been explicitly controlled and speculate about which unmeasured ones could lead to incorrect inferences. In the absence of randomization, we should do our best to investigate sensitivity to various untestable assumptions.

Measurement Variables.

Explicitly define the variables in the study, show how they are related to the goals of the study, and explain how they are measured. The units of measurement of all variables, causal and outcome, should fit the language you use in the introduction and discussion sections of your report.

A variable is a method for assigning to a set of observations a value from a set of possible outcomes. For example, a variable called "gender" might assign each of 50 observations to one of the values male or female. When we define a variable, we are declaring what we are prepared to represent as a valid observation and what we must consider as invalid. If we define the range of a particular variable (the set of possible outcomes) to be from 1 to 7 on a Likert scale, for example, then a value of 9 is not an outlier (an unusually extreme value). It is an illegal value. If we declare the range of a variable to be positive real numbers and the domain to be observations of reaction time (in milliseconds) to an administration of electric shock, then a value of 3,000 is not illegal; it is an outlier.

Naming a variable is almost as important as measuring it. We do well to select a name that reflects how a variable is measured. On this basis, the name "IQ test score" is preferable to "intelligence" and "retrospective self-report of childhood sexual abuse" is preferable to "childhood sexual abuse." Without such precision, ambiguity in defining variables can give a theory an unfortunate resistance to empirical falsification. Being precise does not make us operationalists. It simply means that we try to avoid excessive generalization.

Editors and reviewers should be suspicious when they notice authors changing definitions or names of variables, failing to make clear what would be contrary evidence, or using measures with no history and

thus no known properties. Researchers should be suspicious when code books and scoring systems are inscrutable or more voluminous than the research articles on which they are based. Everyone should worry when a system offers to code a specific observation in two or more ways for the same variable.

Instruments.

If a questionnaire is used to collect data, summarize the psychometric properties of its scores with specific regard to the way the instrument is used in a population. Psychometric properties include measures of validity, reliability, and any other qualities affecting conclusions. If a physical apparatus is used, provide enough information (brand, model, design specifications) to allow another experimenter to replicate your measurement process.

There are many methods for constructing instruments and psychometrically validating scores from such measures. Traditional true-score theory and item—response test theory provide appropriate frameworks for assessing reliability and internal validity. Signal detection theory and various coefficients of association can be used to assess external validity. Messick (1989) provides a comprehensive guide to validity.

It is important to remember that a test is not reliable or unreliable. Reliability is a property of the scores on a test for a particular population of examinees (Feldt & Brennan, 1989). Thus, authors should provide reliability coefficients of the scores for the data being analyzed even when the focus of their research is not psychometric. Interpreting the size of observed effects requires an assessment of the reliability of the scores.

Besides showing that an instrument is reliable, we need to show that it does not correlate strongly with other key constructs. It is just as important to establish that a measure does *not* measure what it should not measure as it is to show that it *does* measure what it should.

Researchers occasionally encounter a measurement problem that has no obvious solution. This happens when they decide to explore a new and rapidly growing research area that is based on a previous researcher's well-defined construct implemented with a poorly developed psychometric instrument. Innovators, in the excitement of their discovery, sometimes give insufficient attention to the quality of their instruments. Once a defective measure enters the literature, subsequent researchers are reluctant to change it. In these cases, editors and reviewers should pay special attention to the psychometric properties of the instruments used, and they might want to encourage revisions (even if not by the scale's author) to prevent the accumulation of results based on relatively invalid or unreliable measures.

Procedure.

Describe any anticipated sources of attrition due to noncompliance, dropout, death, or other factors. Indicate how such attrition may affect the generalizability of the results. Clearly describe the conditions under which measurements are taken (e.g., format, time, place, personnel who collected data). Describe

the specific methods used to deal with experimenter bias, especially if you collected the data yourself.

Despite the long-established findings of the effects of experimenter bias (Rosenthal, 1966), many published studies appear to ignore or discount these problems. For example, some authors or their assistants with knowledge of hypotheses or study goals screen participants (through personal interviews or telephone conversations) for inclusion in their studies. Some authors administer questionnaires. Some authors give instructions to participants. Some authors perform experimental manipulations. Some tally or code responses. Some rate videotapes.

An author's self-awareness, experience, or resolve does not eliminate experimenter bias. In short, there are no valid excuses, financial or otherwise, for avoiding an opportunity to double-blind. Researchers looking for guidance on this matter should consult the classic book of Webb, Campbell, Schwartz, and Sechrest (1966) and an exemplary dissertation (performed on a modest budget) by Baker (1969) .

Power and sample size.

Provide information on sample size and the process that led to sample size decisions. Document the effect sizes, sampling and measurement assumptions, as well as analytic procedures used in power calculations. Because power computations are most meaningful when done before data are collected and examined, it is important to show how effect-size estimates have been derived from previous research and theory in order to dispel suspicions that they might have been taken from data used in the study or, even worse, constructed to justify a particular sample size. Once the study is analyzed, confidence intervals replace calculated power in describing results.

Largely because of the work of Cohen (1969 , 1988), psychologists have become aware of the need to consider power in the design of their studies, before they collect data. The intellectual exercise required to do this stimulates authors to take seriously prior research and theory in their field, and it gives an opportunity, with incumbent risk, for a few to offer the challenge that there is no applicable research behind a given study. If exploration were not disguised in hypothetico-deductive language, then it might have the opportunity to influence subsequent research constructively.

Computer programs that calculate power for various designs and distributions are now available. One can use them to conduct power analyses for a range of reasonable alpha values and effect sizes. Doing so reveals how power changes across this range and overcomes a tendency to regard a single power estimate as being absolutely definitive.

Many of us encounter power issues when applying for grants. Even when not asking for money, think about power. Statistical power does not corrupt.

Results

Complications

Before presenting results, report complications, protocol violations, and other unanticipated events in data collection. These include missing data, attrition, and nonresponse. Discuss analytic techniques devised to ameliorate these problems. Describe nonrepresentativeness statistically by reporting patterns and distributions of missing data and contaminations. Document how the actual analysis differs from the analysis planned before complications arose. The use of techniques to ensure that the reported results are not produced by anomalies in the data (e.g., outliers, points of high influence, nonrandom missing data, selection bias, attrition problems) should be a standard component of all analyses.

As soon as you have collected your data, before you compute *any* statistics, *look at your data.* Data screening is not data snooping. It is not an opportunity to discard data or change values to favor your hypotheses. However, if you assess hypotheses without examining your data, you risk publishing nonsense.

Computer malfunctions tend to be catastrophic: A system crashes; a file fails to import; data are lost. Less well-known are more subtle bugs that can be more catastrophic in the long run. For example, a single value in a file may be corrupted in reading or writing (often in the first or last record). This circumstance usually produces a major value error, the kind of singleton that can make large correlations change sign and small correlations become large.

Graphical inspection of data offers an excellent possibility for detecting serious compromises to data integrity. The reason is simple: Graphics broadcast; statistics narrowcast. Indeed, some international corporations that must defend themselves against rapidly evolving fraudulent schemes use real-time graphic displays as their first line of defense and statistical analyses as a distant second. The following example shows why.

Figure 1 shows a scatter-plot matrix (SPLOM) of three variables from a national survey of approximately 3,000 counseling clients (Chartrand, 1997). This display, consisting of pairwise scatter plots arranged in a matrix, is found in most modern statistical packages. The diagonal cells contain dot plots of each variable (with the dots stacked like a histogram) and scales used for each variable. The three variables shown are questionnaire measures of respondent's age (*AGE*), gender (*SEX*), and number of years together in current relationship (*TOGETHER*). The graphic in Figure 1 is not intended for final presentation of results; we use it instead to locate coding errors and other anomalies before we analyze our data. Figure 1 is a selected portion of a computer screen display that offers tools for zooming in and out, examining points, and linking to information in other graphical displays and data editors. SPLOM displays can be used to recognize unusual patterns in 20 or more variables simultaneously. We focus on these three only.

There are several anomalies in this graphic. The *AGE* histogram shows a spike at the right end, which corresponds to the value 99 in the data. This coded value most likely signifies a missing value, because it is unlikely that this many people in a sample of 3,000 would have an age of 99 or greater. Using

numerical values for missing value codes is a risky practice (Kahn & Udry, 1986).

The histogram for *SEX* shows an unremarkable division into two values. The histogram for *TOGETHER* is highly skewed, with a spike at the lower end presumably signifying no relationship. The most remarkable pattern is the triangular joint distribution of *TOGETHER* and *AGE*. Triangular joint distributions often (but not necessarily) signal an implication or a relation rather than a linear function with error. In this case, it makes sense that the span of a relationship should not exceed a person's age. Closer examination shows that something is wrong here, however. We find some respondents (in the upper left triangular area of the *TOGETHER—AGE* panel) claiming that they have been in a significant relationship longer than they have been alive! Had we computed statistics or fit models before examining the raw data, we would likely have missed these reporting errors. There is little reason to expect that *TOGETHER* would show any anomalous behavior with other variables, and even if *AGE* and *TOGETHER* appeared jointly in certain models, we may not have known anything was amiss, regardless of our care in examining residual or other diagnostic plots.

The main point of this example is that the type of "atheoretical" search for patterns that we are sometimes warned against in graduate school can save us from the humiliation of having to retract conclusions we might ultimately make on the basis of contaminated data. We are warned against fishing expeditions for understandable reasons, but blind application of models without screening our data is a far graver error.

Graphics cannot solve all our problems. Special issues arise in modeling when we have missing data. The two popular methods for dealing with missing data that are found in basic statistics packages–listwise and pairwise deletion of missing values–are among the worst methods available for practical applications. Little and Rubin (1987) have discussed these issues in more detail and offer alternative approaches.

Analysis Choosing a minimally sufficient analysis.

The enormous variety of modern quantitative methods leaves researchers with the nontrivial task of matching analysis and design to the research question. Although complex designs and state-of-the-art methods are sometimes necessary to address research questions effectively, simpler classical approaches often can provide elegant and sufficient answers to important questions. Do not choose an analytic method to impress your readers or to deflect criticism. If the assumptions and strength of a simpler method are reasonable for your data and research problem, use it. Occam's razor applies to methods as well as to theories.

We should follow the advice of Fisher (1935) :

Experimenters should remember that they and their colleagues usually know more about the kind of material they are dealing with than do the authors of text-books written without such personal experience, and that a more complex, or less intelligible, test is not likely to serve their purpose better, in any sense, than those of proved value in their own subject. (p. 49)

There is nothing wrong with using state-of-the-art methods, as long as you and your readers understand how they work and what they are doing. On the other hand, don't cling to obsolete methods (e.g., Newman—Keuls or Duncan post hoc tests) out of fear of learning the new. In any case, listen to Fisher. Begin with an idea. Then pick a method.

Computer programs.

There are many good computer programs for analyzing data. More important than choosing a specific statistical package is verifying your results, understanding what they mean, and knowing how they are computed. If you cannot verify your results by intelligent "guesstimates," you should check them against the output of another program. You will not be happy if a vendor reports a bug after your data are in print (not an infrequent event). Do not report statistics found on a printout without understanding how they are computed or what they mean. Do not report statistics to a greater precision than is supported by your data simply because they are printed that way by the program. Using the computer is an opportunity for you to control your analysis and design. If a computer program does not provide the analysis you need, use another program rather than let the computer shape your thinking.

There is no substitute for common sense. If you cannot use rules of thumb to detect whether the result of a computation makes sense to you, then you should ask yourself whether the procedure you are using is appropriate for your research. Graphics can help you to make some of these determinations; theory can help in other cases. But never assume that using a highly regarded program absolves you of the responsibility for judging whether your results are plausible. Finally, when documenting the use of a statistical procedure, refer to the statistical literature rather than a computer manual; when documenting the use of a program, refer to the computer manual rather than the statistical literature.

Assumptions.

You should take efforts to assure that the underlying assumptions required for the analysis are reasonable given the data. Examine residuals carefully. Do not use distributional tests and statistical indexes of shape (e.g., skewness, kurtosis) as a substitute for examining your residuals graphically.

Using a statistical test to diagnose problems in model fitting has several shortcomings. First, diagnostic significance tests based on summary statistics (such as tests for homogeneity of variance) are often impractically sensitive; our statistical tests of models are often more robust than our statistical tests of assumptions. Second, statistics such as skewness and kurtosis often fail to detect distributional irregularities in the residuals. Third, statistical tests depend on sample size, and as sample size increases, the tests often will reject innocuous assumptions. In general, there is no substitute for graphical analysis of assumptions.

Modern statistical packages offer graphical diagnostics for helping to determine whether a model appears to fit data appropriately. Most users are familiar with residual plots for linear regression modeling. Fewer

are aware that John Tukey's paradigmatic equation, *data = fit + residual,* applies to a more general class of models and has broad implications for graphical analysis of assumptions. Stem-and-leaf plots, box plots, histograms, dot plots, spread/level plots, probability plots, spectral plots, autocorrelation and cross-correlation plots, co-plots, and trellises (Chambers, Cleveland, Kleiner, & Tukey, 1983 ; Cleveland, 1995 ; Tukey, 1977) all serve at various times for displaying residuals, whether they arise from analysis of variance (ANOVA), nonlinear modeling, factor analysis, latent variable modeling, multidimensional scaling, hierarchical linear modeling, or other procedures.

Hypothesis tests.

It is hard to imagine a situation in which a dichotomous accept—reject decision is better than reporting an actual p value or, better still, a confidence interval. Never use the unfortunate expression "accept the null hypothesis." Always provide some effect-size estimate when reporting a p value. Cohen (1994) has written on this subject in this journal. All psychologists would benefit from reading his insightful article.

Effect sizes.

Always present effect sizes for primary outcomes. If the units of measurement are meaningful on a practical level (e.g., number of cigarettes smoked per day), then we usually prefer an unstandardized measure (regression coefficient or mean difference) to a standardized measure (r or d). It helps to add brief comments that place these effect sizes in a practical and theoretical context.

APA's (1994) publication manual included an important new "encouragement" (p. 18) to report effect sizes. Unfortunately, empirical studies of various journals indicate that the effect size of this encouragement has been negligible (Keselman et al., 1998 ; Kirk, 1996 ; Thompson & Snyder, 1998). We must stress again that reporting and interpreting effect sizes in the context of previously reported effects is essential to good research. It enables readers to evaluate the stability of results across samples, designs, and analyses. Reporting effect sizes also informs power analyses and meta-analyses needed in future research.

Fleiss (1994) , Kirk (1996) , Rosenthal (1994) , and Snyder and Lawson (1993) have summarized various measures of effect sizes used in psychological research. Consult these articles for information on computing them. For a simple, general purpose display of the practical meaning of an effect size, see Rosenthal and Rubin (1982) . Consult Rosenthal and Rubin (1994) for information on the use of "counternull intervals" for effect sizes, as alternatives to confidence intervals.

Interval estimates.

Interval estimates should be given for any effect sizes involving principal outcomes. Provide intervals for correlations and other coefficients of association or variation whenever possible.

Confidence intervals are usually available in statistical software; otherwise, confidence intervals for basic statistics can be computed from typical output. Comparing confidence intervals from a current study to intervals from previous, related studies helps focus attention on stability across studies (Schmidt, 1996). Collecting intervals across studies also helps in constructing plausible regions for population parameters. This practice should help prevent the common mistake of assuming a parameter is contained in a confidence interval.

Multiplicities.

Multiple outcomes require special handling. There are many ways to conduct reasonable inference when faced with multiplicity (e.g., Bonferroni correction of p values, multivariate test statistics, empirical Bayes methods). It is your responsibility to define and justify the methods used.

Statisticians speak of the curse of dimensionality. To paraphrase, multiplicities are the curse of the social sciences. In many areas of psychology, we cannot do research on important problems without encountering multiplicity. We often encounter many variables and many relationships.

One of the most prevalent strategies psychologists use to handle multiplicity is to follow an ANOVA with pairwise multiple-comparison tests. This approach is usually wrong for several reasons. First, pairwise methods such as Tukey's honestly significant difference procedure were designed to control a familywise error rate based on the sample size and number of comparisons. Preceding them with an omnibus *F* test in a stagewise testing procedure defeats this design, making it unnecessarily conservative. Second, researchers rarely need to compare all possible means to understand their results or assess their theory; by setting their sights large, they sacrifice their power to see small. Third, the lattice of all possible pairs is a straightjacket; forcing themselves to wear it often restricts researchers to uninteresting hypotheses and induces them to ignore more fruitful ones.

As an antidote to the temptation to explore all pairs, imagine yourself restricted to mentioning only pairwise comparisons in the introduction and discussion sections of your article. Higher order concepts such as trends, structures, or clusters of effects would be forbidden. Your theory would be restricted to first-order associations. This scenario brings to mind the illogic of the converse, popular practice of theorizing about higher order concepts in the introduction and discussion sections and then supporting that theorizing in the results section with atomistic pairwise comparisons. If a specific contrast interests you, examine it. If all interest you, ask yourself why. For a detailed treatment of the use of contrasts, see Rosenthal, Rosnow, and Rubin (in press) .

There is a variant of this preoccupation with all possible pairs that comes with the widespread practice of printing *p* values or asterisks next to every correlation in a correlation matrix. Methodologists frequently point out that these *p* values should be adjusted through Bonferroni or other corrections. One should ask instead why any reader would want this information. The possibilities are as follows:

- All the correlations are "significant." If so, this can be noted in a single footnote.

- None of the correlations are "significant." Again, this can be noted once. We need to be reminded that this situation does not rule out the possibility that combinations or subsets of the correlations may be "significant." The definition of the null hypothesis for the global test may not include other potential null hypotheses that might be rejected if they were tested.

- A subset of the correlations is "significant." If so, our purpose in appending asterisks would seem to be to mark this subset. Using "significance" tests in this way is really a highlighting technique to facilitate pattern recognition. If this is your goal in presenting results, then it is better served by calling attention to the pattern (perhaps by sorting the rows and columns of the correlation matrix) and assessing it directly. This would force you, as well, to provide a plausible explanation.

There is a close relative of all possible pairs called "all possible combinations." We see this occasionally in the publishing of higher way factorial ANOVAs that include all possible main effects and interactions. One should not imagine that placing asterisks next to conventionally significant effects in a five-way ANOVA, for example, skirts the multiplicity problem. A typical five-way fully factorial design applied to a reasonably large sample of random data has about an 80% chance of producing at least one significant effect by conventional *F* tests at the .05 critical level (Hurlburt & Spiegel, 1976).

Underlying the widespread use of all-possible-pairs methodology is the legitimate fear among editors and reviewers that some researchers would indulge in fishing expeditions without the restraint of simultaneous test procedures. We should indeed fear the well-intentioned, indiscriminate search for structure more than the deliberate falsification of results, if only for the prevalence of wishful thinking over nefariousness. There are Bonferroni and recent related methods (e.g., Benjamini & Hochberg, 1995) for controlling this problem statistically. Nevertheless, there is an alternative institutional restraint. Reviewers should require writers to articulate their expectations well enough to reduce the likelihood of post hoc rationalizations. Fishing expeditions are often recognizable by the promiscuity of their explanations. They mix ideas from scattered sources, rely heavily on common sense, and cite fragments rather than trends.

If, on the other hand, a researcher fools us with an intriguing result caught while indiscriminately fishing, we might want to fear this possibility less than we do now. The enforcing of rules to prevent chance results in our journals may at times distract us from noticing the more harmful possibility of publishing bogus theories and methods (ill-defined variables, lack of parsimony, experimenter bias, logical errors, artifacts) that are buttressed by evidently impeccable statistics. There are enough good ideas behind fortuitous results to make us wary of restricting them. This is especially true in those areas of psychology where lives and major budgets are not at stake. Let replications promote reputations.

Causality.

Inferring causality from nonrandomized designs is a risky enterprise. Researchers using nonrandomized designs have an extra obligation to explain the logic behind covariates included in their designs and to

alert the reader to plausible rival hypotheses that might explain their results. Even in randomized experiments, attributing causal effects to any one aspect of the treatment condition requires support from additional experimentation.

It is sometimes thought that correlation does not prove causation but "causal modeling" does. Despite the admonitions of experts in this field, researchers sometimes use goodness-of-fit indices to hunt through thickets of competing models and settle on a plausible substantive explanation only in retrospect. McDonald (1997) , in an analysis of a historical data set, showed the dangers of this practiceand the importance of substantive theory. Scheines, Spirites, Glymour, Meek, and Richardson (1998 ; discussions following) offer similar cautions from a theoretical standpoint.

A generally accepted framework for formulating questions concerning the estimation of causal effects in social and biomedical science involves the use of "potential outcomes," with one outcome for each treatment condition. Although the perspective has old roots, including use by Fisher and Neyman in the context of completely randomized experiments analyzed by randomization-based inference (Rubin, 1990b), it is typically referred to as "Rubin's causal model" or RCM (Holland, 1986). For extensions to observational studies and other forms of inference, see Rubin (1974 , 1977 , 1978) . This approach is now relatively standard, even for settings with instrumental variables and multistage models or simultaneous equations.

The crucial idea is to set up the causal inference problem as one of missing data, as defined in Rubin's (1976) article, where the missing data are the values of the potential outcomes under the treatment *not* received and the observed data include the values of the potential outcomes under the received treatments. Causal effects are defined on a unit level as the comparison of the potential outcomes under the different treatments, only one of which can ever be observed (we cannot go back in time to expose the unit to a different treatment). The essence of the RCM is to formulate causal questions in this way and to use formal statistical methods to draw probabilistic causal inferences, whether based on Fisherian randomization-based (permutation) distributions, Neymanian repeated-sampling randomization-based distributions, frequentist superpopulation sampling distributions, or Bayesian posterior distributions (Rubin, 1990a).

If a problem of causal inference cannot be formulated in this manner (as the comparison of potential outcomes under different treatment assignments), it is not a problem of inference for causal effects, and the use of "causal" should be avoided. To see the confusion that can be created by ignoring this requirement, see the classic Lord's paradox and its resolution by the use of the RCM in Holland and Rubin's (1983) chapter.

The critical assumptions needed for causal inference are essentially always beyond testing from the data at hand because they involve the missing data. Thus, especially when formulating causal questions from nonrandomized data, the underlying assumptions needed to justify any causal conclusions should be carefully and explicitly argued, not in terms of technical properties like "uncorrelated error terms," but in

terms of real world properties, such as how the units received the different treatments.

The use of complicated causal-modeling software rarely yields any results that have any interpretation as causal effects. If such software is used to produce anything beyond an exploratory description of a data set, the bases for such extended conclusions must be carefully presented and not just asserted on the basis of imprecise labeling conventions of the software.

Tables and figures.

Although tables are commonly used to show exact values, well-drawn figures need not sacrifice precision. Figures attract the reader's eye and help convey global results. Because individuals have different preferences for processing complex information, it often helps to provide both tables and figures. This works best when figures are kept small enough to allow space for both formats. Avoid complex figures when simpler ones will do. In all figures, include graphical representations of interval estimates whenever possible.

Bailar and Mosteller (1988) offer helpful information on improving tables in publications. Many of their recommendations (e.g., sorting rows and columns by marginal averages, rounding to a few significant digits, avoiding decimals when possible) are based on the clearly written tutorials of Ehrenberg (1975 , 1981).

A common deficiency of graphics in psychological publications is their lack of essential information. In most cases, this information is the shape or distribution of the data. Whether from a negative motivation to conceal irregularities or from a positive belief that less is more, omitting shape information from graphics often hinders scientific evaluation. Chambers et al. (1983) and Cleveland (1995) offer specific ways to address these problems. The examples in Figure 2 do this using two of the most frequent graphical forms in psychology publications.

Figure 2 shows plots based on data from 80 graduate students in a Midwestern university psychology department, collected from 1969 through 1978. The variables are scores on the psychology advanced test of the Graduate Record Examination (GRE), the undergraduate grade point average (GPA), and whether a student completed a doctoral degree in the department (PhD). Figure 2A shows a format appearing frequently in psychology journal articles: two regression lines, one for each group of students. This graphic conveys nothing more than four numbers: the slopes and intercepts of the regression lines. Because the scales have no physical meaning, seeing the slopes of lines (as opposed to reading the numbers) adds nothing to our understanding of the relationship.

Figure 2B shows a scatter plot of the same data with a locally weighted scatter plot smoother for each PhD group (Cleveland & Devlin, 1988). This robust curvilinear regression smoother (called LOESS) is available in modern statistics packages. Now we can see some curvature in the relationships. (When a model that includes a linear and quadratic term for GPA is computed, the apparent interaction involving

the PhD and no PhD groups depicted in Figure 2A disappears.) The graphic in Figure 2B tells us many things. We note the unusual student with a GPA of less than 4.0 and a psychology GRE score of 800; we note the less surprising student with a similar GPA but a low GRE score (both of whom failed to earn doctoral degrees); we note the several students who had among the lowest GRE scores but earned doctorates, and so on. We might imagine these kinds of cases in Figure 2A (as we should in any data set containing error), but their location and distribution in Figure 2B tells us something about this specific data set.

Figure 3A shows another popular format for displaying data in psychology journals. It is based on the data set used for Figure 2 . Authors frequently use this format to display the results of *t* tests or ANOVAs. For factorial ANOVAs, this format gives authors an opportunity to represent interactions by using a legend with separate symbols for each line. In more laboratory-oriented psychology journals (e.g., animal behavior, neuroscience), authors sometimes add error bars to the dots representing the means.

Figure 3B adds to the line graphic a dot plot representing the data and 95% confidence intervals on the means of the two groups (using the *t* distribution). The graphic reveals a left skewness of GRE scores in the PhD group. Although this skewness may not be severe enough to affect our statistical conclusions, it is nevertheless noteworthy. It may be due to ceiling effects (although note the 800 score in the no PhD group) or to some other factor. At the least, the reader has a right to be able to evaluate this kind of information.

There are other ways to include data or distributions in graphics, including box plots and stem-and-leaf plots (Tukey, 1977) and kernel density estimates (Scott, 1992 ; Silverman, 1986). Many of these procedures are found in modern statistical packages. It is time for authors to take advantage of them and for editors and reviewers to urge authors to do so.

Discussion

Interpretation

When you interpret effects, think of credibility, generalizability, and robustness. Are the effects credible, given the results of previous studies and theory? Do the features of the design and analysis (e.g., sample quality, similarity of the design to designs of previous studies, similarity of the effects to those in previous studies) suggest the results are generalizable? Are the design and analytic methods robust enough to support strong conclusions?

Novice researchers err either by overgeneralizing their results or, equally unfortunately, by overparticularizing. Explicitly compare the effects detected in your inquiry with the effect sizes reported in related previous studies. Do not be afraid to extend your interpretations to a general class or population if you have reasons to assume that your results apply. This general class may consist of populations you

have studied at your site, other populations at other sites, or even more general populations. Providing these reasons in your discussion will help you stimulate future research for yourself and others.

Conclusions

Speculation may be appropriate, but use it sparingly and explicitly. Note the shortcomings of your study. Remember, however, that acknowledging limitations is for the purpose of qualifying results and avoiding pitfalls in future research. Confession should not have the goal of disarming criticism. Recommendations for future research should be thoughtful and grounded in present and previous findings. Gratuitous suggestions ("further research needs to be done ...") waste space. Do not interpret a single study's results as having importance independent of the effects reported elsewhere in the relevant literature. The thinking presented in a single study may turn the movement of the literature, but the results in a single study are important primarily as one contribution to a mosaic of study effects.

Some had hoped that this task force would vote to recommend an outright ban on the use of significance tests in psychology journals. Although this might eliminate some abuses, the committee thought that there were enough counterexamples (e.g., Abelson, 1997) to justify forbearance. Furthermore, the committee believed that the problems raised in its charge went beyond the simple question of whether to ban significance tests.

The task force hopes instead that this report will induce editors, reviewers, and authors to recognize practices that institutionalize the thoughtless application of statistical methods. Distinguishing statistical significance from theoretical significance (Kirk, 1996) will help the entire research community publish more substantial results. Encouraging good design and logic will help improve the quality of conclusions. And promoting modern statistical graphics will improve the assessment of assumptions and the display of results.

More than 50 years ago, Hotelling, Bartky, Deming, Friedman, and Hoel (1948) wrote, "Unfortunately, too many people like to do their statistical work as they say their prayers–merely substitute in a formula found in a highly respected book written a long time ago" (p. 103). Good theories and intelligent interpretation advance a discipline more than rigid methodological orthodoxy. If editors keep in mind Fisher's (1935) words quoted in the *Analysis* section, then there is less danger of methodology substituting for thought. Statistical methods should guide and discipline our thinking but should not determine it.

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Figure 1. Scatter-Plot Matrix

Note. M = male; F = female.

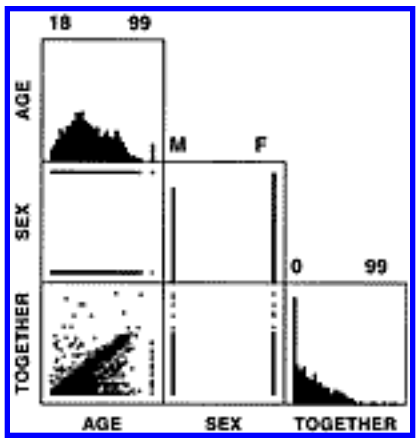


Figure 2. Graphics for Regression

Note. GRE = Graduate Record Examination; GPA = grade point average; PhD and No PhD = completed and did not complete the doctoral degree; Y = yes; N = no.

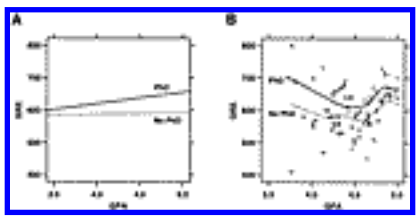
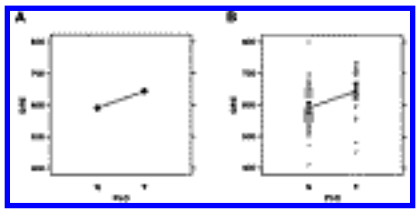
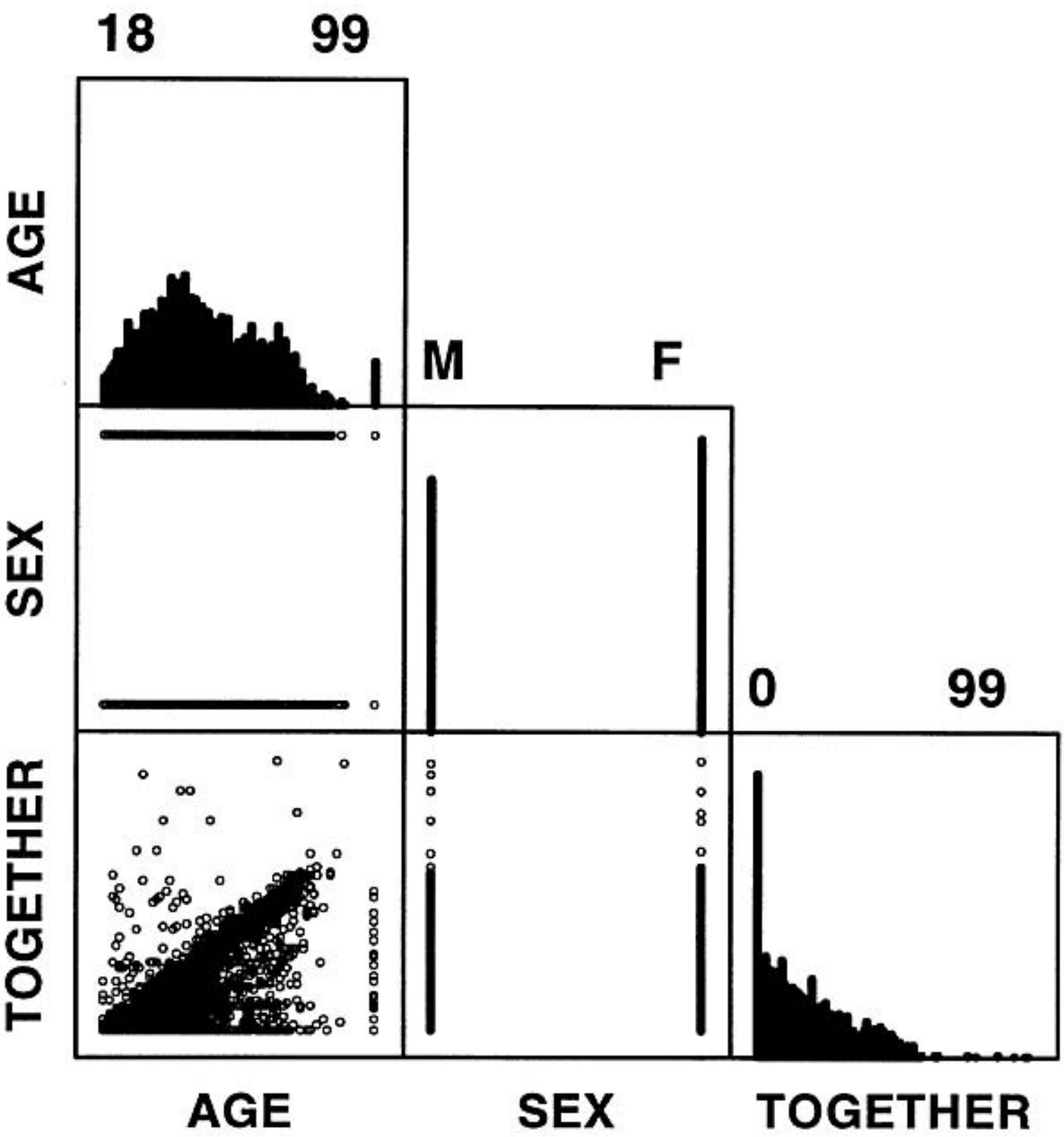


Figure 3. Graphics for Groups

Note. GRE = Graduate Record Examination; N = no; Y = yes.

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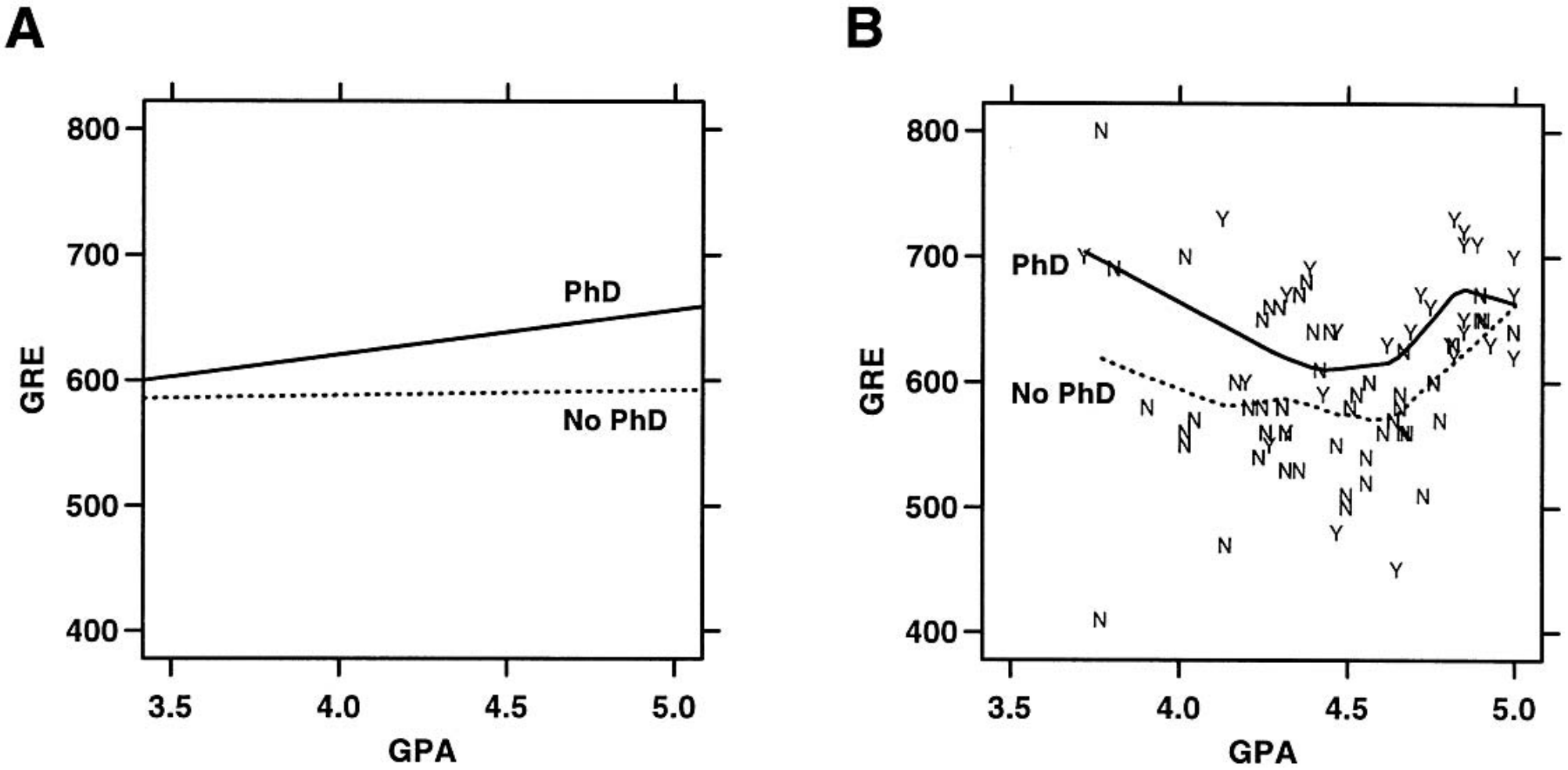
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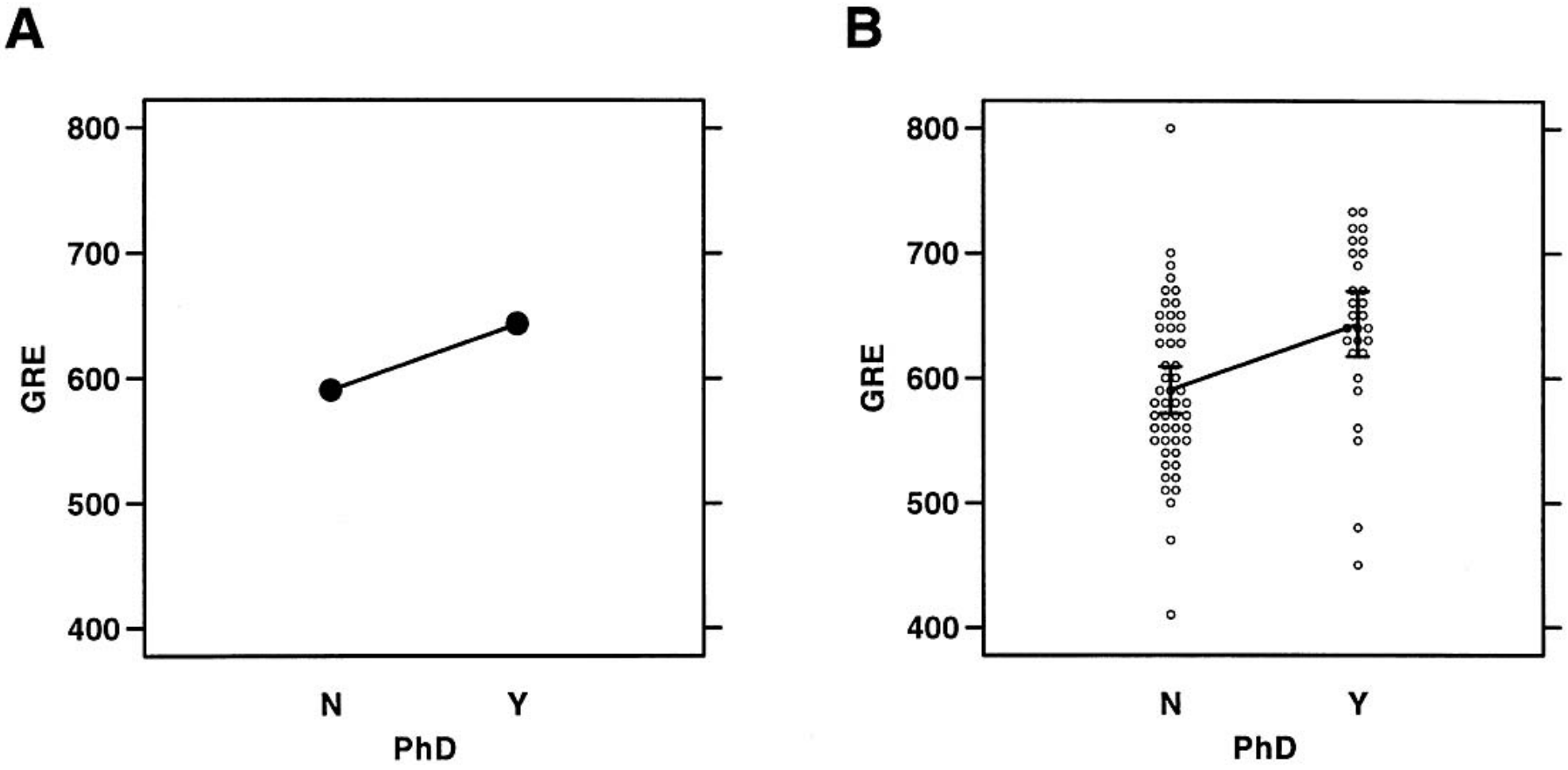
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