AN UNPUBLISHED QUANTITATIVE RESEARCH METHODS BOOK

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I have put together in this book a number of slightly-revised unpublished papers I wrote during the last several years. Some were submitted for possible publication and were rejected. Most were never submitted. They range in length from 2 pages to 91 pages, and in complexity from easy to fairly technical. The papers are included in an order in which I think the topics should be presented (design first, then instrumentation, then analysis). You might find some things repeated two or three times. That's because I wrote the papers at different times; the repetition was not intentional. There's something in here for everybody. Feel free to download anything you find to be of interest. Enjoy!

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CHAPTER 1: HOW MANY KINDS OF QUANTITATIVE RESEARCH STUDIES ARE THERE?

You wouldn't believe how many different ways authors of quantitative research methods books and articles "divide the pie" into various approaches to the advancement of scientific knowledge. In what follows I would like to present my own personal taxonomy, while at the same time pointing out some other ways of classifying research studies. I will also make a few comments regarding some ethical problems with certain types of research.

Experiments, surveys, and correlational studies

That's it (in my opinion). Three basic types, with a few sub-types.

1. Experiments

If causality is of concern, there is no better way to try to get at it than to carry out an experiment. But the experiment should be a "true" experiment (called a randomized clinical trial, or randomized controlled trial, in the health sciences), with random assignment to the various treatment conditions. Random assignment provides the best and simplest control of possibly confounding variables that could affect the dependent (outcome) variable instead of, or in addition to, the independent ("manipulated") variable of primary interest.

Experiments are often not generalizable, for two reasons: (1) they are usually carried out on "convenience" non-random samples; and (2) control is usually regarded as more important in experiments than generalizability, since causality is their ultimate goal. Generalizability can be obtained by replication.

Small but carefully designed experiments are within the resources of individual investigators. Large experiments involving a large number of sites require large research grants.

An experiment in which some people would be randomly assigned to smoke cigarettes and others would be randomly assigned to not smoke cigarettes is patently unethical. Fortunately, such a study has never been carried out (as far as I know).

2. Surveys

Control is almost never of interest in survey research. An entire population or a sample (hopefully random) of a population is contacted and the members of that population or sample are asked questions, usually via questionnaires, to which the researcher would like answers.

Surveys based upon probability samples (usually multi-stage) are the most generalizable of the three types. If the survey research is carried out on an entire well-defined population, better yet; but no generalizability beyond that particular population is warranted.

Surveys are rarely regarded as unethical, because potential respondents are free to refuse to participate wholly (e.g., by throwing away the questionnaire) or partially (by omitting some of the questions).

3. Correlational studies

Correlational studies come in various sizes and shapes. (N.B.: The word "correlational" applies to the type of research, not to the type of analysis, e.g., the use of correlation coefficients such as the Pearson product-moment measure. Correlation coefficients can be as important in experimental research as in non-experimental research for analyzing the data.) Some of the sub-types of correlational research are: (1) measurement studies in which the reliability and/or validity of measuring instruments are assessed; (2) predictive studies in which the relationship between one or more independent (predictor) variables and one or more dependent (criterion) variables are explored; and (3) theoretical studies that try to determine the "underlying" dimensions of a set of variables. This third sub-type includes factor analysis (both exploratory and confirmatory) and structural equation modeling (the analysis of covariance structures).

The generalizability of a correlational research study depends upon the method of sampling the units of analysis (usually individual people) and the properties of the measurements employed.

Correlational studies are likely to be more subject to ethical violations than either experiments or surveys, because they are often based upon existing records, the access to which might not have the participants' explicit consents. (But I don't think that a study of a set of anonymous heights and weights for a large sample of males and females would be regarded as unethical; do you?)

Combination studies

The terms "experiment", "survey", and "correlational study" are not mutually exclusive. For example, a study in which people are randomly assigned to different questionnaire formats could be considered to be both an experiment and a survey. But that might better come under the heading of "methodological research" (research on the tools of research) as opposed to "substantive research" (research designed to study matters such as the effect of teaching method on pupil achievement or the effect of drug dosage on pain relief).

Pilot studies

Experiments, surveys, or correlational studies are often preceded by feasibility studies whose purpose is to "get the bugs out" before the main studies are undertaken. Such studies are called "pilot studies", although some researchers use that term to refer to small studies for which larger studies are not even contemplated. Whether or not the substantive findings of a pilot study should be published is a matter of considerable controversy.

Two other taxonomies

Epidemiology

In epidemiology the principal distinction is made between experimental studies and "observational" studies. The basis of the distinction is that experimental studies involve the active manipulation (researcher intervention) of the independent variable(s) whereas observational studies do not. An observational epidemiological study usually does not involve any actual visualization of participants (as the word implies in ordinary parlance), whereas a study in psychology or the other social sciences occasionally does (see next section). There are many sub-types of epidemiological research, e.g., analytic(al) vs. descriptive, and cohort vs. case-control.

Psychology

In social science disciplines such as psychology, sociology, and education, the preferred taxonomies are similar to mine, but with correlational studies usually sub-divided into cross-sectional vs. longitudinal, and with the addition of quantitative case studies of individual people or groups of people (where observation in the visual sense of the word might be employed).

Laboratory animals

Much research in medicine and in psychology is carried out on infrahuman animals rather than human beings, for a variety of reasons; for example: (1) using mice, monkeys, dogs, etc. is generally regarded as less unethical than using people; (2) certain diseases such as cancer develop more rapidly in some animal species and the benefits of animal studies can be realized sooner; and (3) informed consent of the animal itself is not required (nor can be obtained). The necessity for animal research is highly controversial, however, with strong and passionate arguments on both sides of the controversy.

Interestingly, there have been several attempts to determine which animals are most appropriate for studying which diseases.

Efficacy vs. effectiveness

Although I personally never use the term "efficacy", in the health sciences the distinction is made between studies that are carried out in ideal environments and those carried out in more practical "real world" environments. The former are usually referred to as being concerned with efficacy and the latter with effectiveness.

Quantitative vs. qualitative research

"Quantitative" is a cover term for studies such as the kinds referred to above. "Qualitative" is also a cover term that encompasses ethnographic studies, phenomenological studies, and related kinds of research having similar philosophical bases to one another.

References

Rather than provide references to books, articles, etc. in the usual way, I would like to close this chapter with a brief annotated list of websites that contain discussions of various kinds of quantitative research studies.

1. Wikipedia

Although Wikipedia websites are sometimes held in disdain by academics, and as "works in progress" have associated comments requesting editing and the provision of additional references, some of them are very good indeed. One of my favorites is a website originating at the PJ Nyanjui Kenya Institute of Education. It has an introduction to research section that includes a discussion of various types of research, with an emphasis on educational research.

2. Medical Research With Animals

The title of the website is an apt description of its contents. Included are discussions regarding which animals are used for research concerning which diseases, who carries out such research, and why they do it. Nice.

3. Cancer Information and Support Network

The most interesting features on this website (to me, anyhow) are a diagram showing the various kinds of epidemiological studies and short descriptions of each kind.

4. Psychology.About.Com

Seven articles regarding various types of psychological studies are featured at this website. Those types are experiments, correlational studies, longitudinal research, cross-sectional research, surveys, and case studies; and an article about within-subjects experimental designs, where each participant serves as his(her) own control.

5. The Nutrition Source

This website is maintained by the T.H. Chan School of Public Health at Harvard University. One of its sections is entitled "Research Study Types" in public health, and it includes excellent descriptions of laboratory and animal studies, case-control studies, cohort studies, and randomized trials.

CHAPTER 2: SHOULD WE GIVE UP ON CAUSALITY?

Introduction

Researcher A randomly assigns forty members of a convenience sample of hospitalized patients to one of five different daily doses of aspirin (eight patients per dose), determines the length of hospital stay for each person, and carries out a test of the significance of the difference among the five mean stays. Researcher B has access to hospital records for a random sample of forty patients, determines the daily dose of aspirin given to, and the length of hospital stay for, each person, and calculates the correlation (Pearson product-moment) between dose of aspirin and length of stay. Researcher A's study has a stronger basis for causality ("internal validity"). Researcher B's study has a stronger basis for generalizability ("external validity"). Which of the two studies contributes more to the advancement of knowledge?

Oh; do you need to see the data before you answer the question? The raw data are the same for both studies. Here they are:

ID	Dose(in mg)	LOS(in days)	ID	Dose(in mg)	LOS(in days)
1	75	5	21	175	25
2	75	10	22	175	25
3	75	10	23	175	25
4	75	10	24	175	30
5	75	15	25	225	20
6	75	15	26	225	25
7	75	15	27	225	25
8	75	20	28	225	25
9	125	10	29	225	30
10	125	15	30	225	30
11	125	15	31	225	30
12	125	15	32	225	35
13	125	20	33	275	25
14	125	20	34	275	30
15	125	20	35	275	30
16	125	25	36	275	30
17	175	15	37	275	35
18	175	20	38	275	35
19	175	20	39	275	35
20	175	20	40	275	40

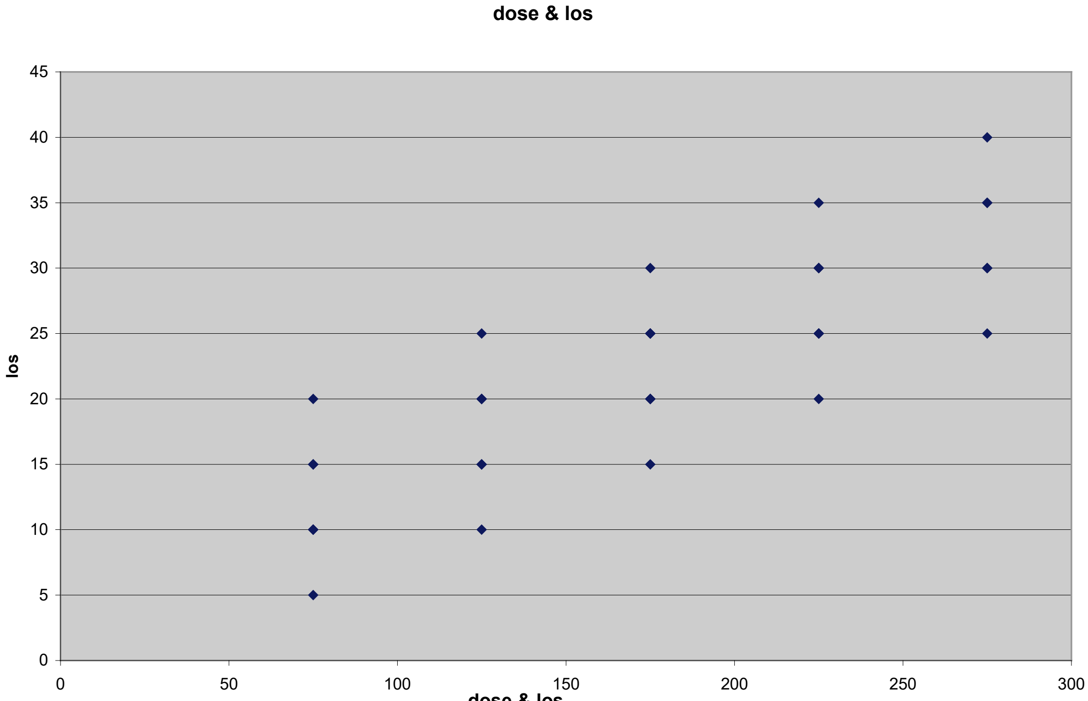
And here are the results for the two analyses (courtesy of Excel and Minitab). Don’t worry if you can’t follow all of the technical matters:

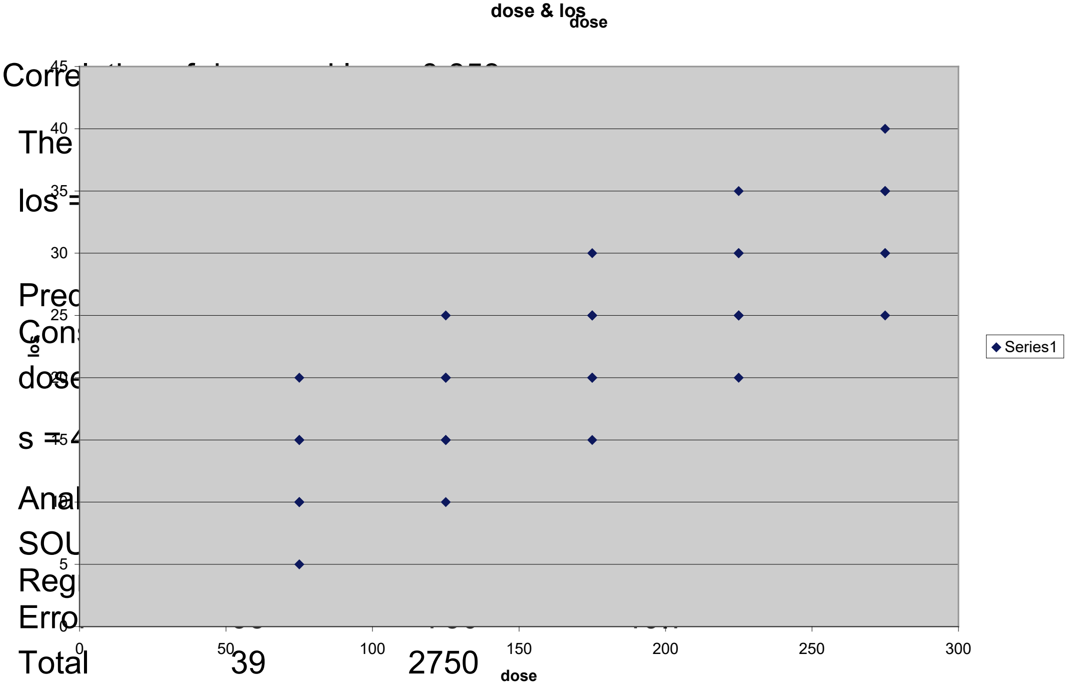
SUMMARY

Groups	Count	Sum	Mean	Variance
75 mg	8	100	12.5	21.43
125 mg	8	140	17.5	21.43
175 mg	8	180	22.5	21.43
225 mg	8	220	27.5	21.43
275 mg	8	260	32.5	21.43

ANOVA

Source of Variation	SS	df	MS	F
Between Groups	2000	4	500	23.33
Within Groups	750	35	21.43	
Total	2750	39		





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The results are virtually identical. (For those of you familiar with "the general linear model" that is not surprising.) There is only that tricky difference in the df's associated with the fact that dose is discrete in the ANOVA (its magnitude never even enters the analysis) and continuous in the correlation and regression analyses.

But what about the assumptions?

Here is the over-all frequency distribution for LOS:

```
     Midpoint    Count
           5         1  *
          10         4  ****
          15         7  *******
          20         8  ********
          25         8  ********
          30         7  *******
          35         4  ****
          40         1  *
```

Looks pretty normal to me.

And here is the LOS frequency distribution for each of the five treatment groups: (This is relevant for homogeneity of variance in the ANOVA and for homoscedasticity in the regression.)

```
 Histogram of los       treat = 75      N = 8
 Midpoint       Count
          5         1  *
         10         3  ***
         15         3  ***
         20         1  *
 Histogram of los       treat =125      N = 8
 Midpoint       Count
         10         1  *
         15         3  ***
         20         3  ***
         25         1  *
 Histogram of los       treat =175      N = 8
 Midpoint       Count
         15         1  *
         20         3  ***
         25         3  ***
         30         1  *
 Histogram of los       treat =225      N = 8
 Midpoint       Count
         20         1  *
         25         3  ***
         30         3  ***
         35         1  *
 Histogram of los       treat =275      N = 8
 Midpoint       Count
         25         1  *
         30         3  ***
         35         3  ***
         40         1  *
```

Those distributions are as normal as they can be for eight observations per treatment condition. (They're actually the binomial coefficients for n = 3.)

So what?

The "So what?" is that the statistical conclusion is essentially the same for the two studies; i.e., there is a strong linear association between dose and stay. The regression equation for Researcher B's study can be used to predict stay from dose quite well for the population from which his (her) sample was randomly drawn. You're only likely to be off by 5-10 days in length of stay, since the standard error of estimate, s, = 4.44. Why do we need the causal interpretation provided by Researcher A's study? Isn't the greater generalizability of Researcher B's study more important than whether or not the "effect" of dose on stay is causal for the non-random sample?

You're probably thinking "Yeah; big deal, for this one example of artificial data." Of course the data are artificial (for illustrative purposes). Real data are never that clean, but they could be.

Read on.

What do other people have to say about causation, correlation, and prediction?

The sources cited most often for distinctions among causation (I use the terms "causality" and "causation" interchangeably), correlation, and prediction are usually classics written by philosophers such as Mill (1884) and Popper (1959); textbook authors such as Pearl (2000); and journal articles such as Bradford Hill (1965) and Holland (1986). I would like to cite a few other lesser known people who have had something to say for or against the position I have just taken. I happily exclude those who say only that "correlation is not causation" and who let it go at that.

Schield (1995):

Milo Schield is very big on emphasizing the matter of causation in the teaching of statistics. Although he included in his conference presentation the mantra "correlation is not causality", he carefully points out that students might mistakenly think that correlation can never be causal. He goes on to argue for the need to make other important distinctions among causality, explanation, determination, prediction, and other terms that are often confused with one another. Nice piece.

Frakt (2009):

In an unusual twist, Austin Frakt argues that you can have causation without correlation. (The usual minimum three criteria for a claim that X causes Y are strong correlation, temporal precedence, and non-spuriousness.) He gives an example for which the true relationship between X and Y is mediated by a third variable W, where the correlation between X and Y is equal to zero.

White (2010):

John Myles White decries the endless repetiton of "correlation is not causation". He argues that most of our knowledge is correlational knowledge; causal knowledge is only necessary when we want to control things; causation is a slippery concept; and correlation and causation go hand-in-hand more often than some people think. His take-home message is that it's much better to know X and Y are related than it is to know nothing at all.

Anonymous (2012):

Anonymous starts out his (her) two-part article with this: "The ultimate goal of social science is causal explanation. The actual goal of most academic research is to discover significant relationships between variables." Ouch! But true? He (she) contends that we can detect a statistically significant effect of X on Y but still not know why and when Y occurs.

That looks like three (Schield, Frakt, and Anonymous) against two (White and me), so I lose? Perhaps. How about a compromise? In the spirit of White's distinction between correlational knowledge and causal knowledge, can we agree that we should concentrate our research efforts on two non-overlapping strategies: true experiments (randomized clinical trials) carried out on admittedly handy non-random samples, with replications wherever possible; and non-experimental correlational studies carried out on random samples, also with replications?

A closing note

What about the effect of smoking (firsthand, secondhand, thirdhand...whatever) on lung cancer? Would you believe that we might have to give up on causality there? There are problems regarding the difficulty of establishing a causal connection between the two even for firsthand smoking. You can look it up (in Spirtes, Glymour, & Scheines, 2000, pp.239-240). You might also want to read the commentary by Lyketsos and Chisolm (2009), the letter by Luchins (2009) regarding that commentary, and the reply by Lyketsos and Chisolm (2009) concerning why it is sometimes not reported that smoking was responsible for the death of a smoker who had lung cancer, whereas stress as a cause for suicide almost always is.

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CHAPTER 3: SHOULD WE GIVE UP ON EXPERIMENTS?

In the previous chapter I presented several arguments pro and con the giving up on causality. In this sequel I would like to extend the considerations to the broader matter of giving up on true experiments (randomized controlled trials) in general. I will touch on ten arguments for doing so. But first...

What is an experiment?

Although different researchers use the term in different ways (e.g., some equate "experimental" with "empirical" and some others equate an "experiment" with a "demonstration"), the most common definition of an experiment is a type of study in which the researcher "manipulates" the independent variable(s) in order to determine its(their) effect(s) on one or more dependent variables (often called "outcome" variables). That is, the researcher assigns the "units" (usually people) to the various categories of the independent variable(s). [The most common categories are "experimental" and "control".] This is the sense in which the term will be used throughout the present chapter.

What is a "true" experiment?

A true experiment is one in which the units are randomly assigned by the researcher to the categories of the independent variable(s). The most popular type of true experiment is a randomized clinical trial.

What are some of the arguments against experiments?

1. They are artificial.

Experiments are necessarily artificial. Human beings don't live their lives by being assigned (whether randomly or not) to one kind of "treatment" or another. They might choose to take this pill or that pill, for example, but they usually don't want somebody else to make the choice for them.

2. They have to be "blinded" (either single or double); i.e., the participants must not know which treatment they're getting and/or the experimenters must not know which treatment each participant is getting. If it's "or", the blinding is single; if it's "and", the blinding is double. Both types of blinding are very difficult to carry out.

3. Experimenters must be well-trained to carry out their duties in the implementation of the experiments. That is irrelevant when the subjects make their own choices of treatments (or choose no treatment at all).

4. The researcher needs to make the choice of a "per protocol" or an "intent(ion) to treat" analysis of the resulting data. The former "counts" each unit in the treatment it actually receives; the latter "counts" each unit in the treatment to which it initially has been assigned, no matter if it "ends up" in a different

treatment or in no treatment. I prefer the former; most members of the scientific community, especially biostatisticians and epidemiologists, prefer the latter.

5. The persons who end up in a treatment that turns out to be inferior might be denied the opportunity for better health and a better quality of life.

6. Researchers who conduct randomized clinical trials either must trust probability to achieve approximate equality at baseline or carry out some sorts of tests of pre-experimental.equivalence and act accordingly, by adjusting for the possible influence of confounding variables that might have led to a lack of comparability. The former approach is far better. That is precisely what a statistical significance test of the difference on the "posttest" variable(s) is for: Is the difference greater than the "chance" criterion indicates (usually a two-tailed alpha level)? To carry out baseline significance tests is just bad science. (See, for example, the first "commandment" in Knapp & Brown, 2014.)

7. Researchers should use a randomization (permutation) test for analyzing the data, especially if the study sample has not been randomly drawn. Most people don't; they prefer t-tests or ANOVAs, with all of their hard-to-satisfy assumptions.

8. Is the causality that is justified for true experiments really so important? Most research questions in scientific research are not concerned with experiments, much less causality (see, for example, White, 2010).

9. If there were no experiments we wouldn't have to distinguish between whether we're searching for "causes of effects" or "effects of causes". (That is a very difficult distinction to grasp, and one I don't think is terribly important, but if you care about it see Dawid, Faigman, & Fienberg, 2014, the comments regarding that article, and their response.)

10. In experiments the participants are often regarded at best as random representatives of their respective populations rather than as individual persons.

As is the case for good debaters, I would now like to present some counter-arguments to the above.

<u>In defense of experiments</u>

1. The artificiality can be at least partially reduced by having the experimenters explain how important it is that chance, not personal preference, be the basis for determining which people comprise the treatment groups. They should also inform the participants that whatever the results of the experiment are, the findings are most useful to society in general and not necessarily to the participants themselves.

2, There are some situations for which blinding is only partially necessary. For example, if the experiment is a counter-balanced design concerned with two different teaching methods, each person is given each treatment, albeit in randomized order, so every participant can (often must) know which treatment he(she) is getting on which occasion. The experimenters can (and almost always must) also know, in order to be able to teach the relevant method at the relevant time. [The main problem with a counter-balanced design is that a main effect could actually be a complicated treatment-by-time interaction.]

3. The training required for implementing an experiment is often no more extensive than that required for carrying out a survey or a correlational study.

4. Per protocol vs. intention-to-treat is a very controversial and methodologically complicated matter. Good "trialists" need only follow the recommendations of experts in their respective disciplines.

5. See the second part of the counter-argument to #1, above.

6. Researchers should just trust random assignment to provide approximate pre-experimental equivalence of the treatment groups. Period. For extremely small group sizes, e.g., two per treatment, the whole experiment should be treated just like a series of case studies in which a "story" is told about each participant and what the effect was of the treatment that he(she) got.

7. A t-test is often a good approximation to a randomization test, for evidence regarding causality but not for generalizability from sample to population, unless the design has incorporated both random sampling and random assignment.

8. In the previous chapter I cite several philosophers and statisticians who strongly believe that the determination of whether X caused Y, Y caused X, or both were caused by W is at the heart of science. Who am I to argue with them? I don't know the answer. I do know that I often take positions opposite to those of experts, whether my positions are grounded in expertise of my own or are merely contrarian.

9. If you are convinced that the determination of causality is essential, and furthermore that it is necessary to distinguish between those situations where the emphasis is placed on the causes of effects as opposed to the effects of causes, go for it, but be prepared to have to do a lot of hard work. (Maybe I'm just lazy.)

10. Researchers who conduct non-experiments are sometimes just as crass in their concern (lack of concern?) about individual participants. For example, does an investigator who collects survey data from available online people even know, much less care, who is who?

References:

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CHAPTER 4: RANDOM

Preface

What is the meaning of the word "random"? What is the difference between random sampling and random assignment? If and when a researcher finds that some data are missing in a particular study, under what circumstances can such data be regarded as "missing at random"? These are just a few of the many questions that are addressed in this chapter. I have divided it into 20 sections--one section for each of 20 questions--a feeble attempt at humor by appealing to an analogy with that once-popular game. But it is my sincere hope that when you get to the end of the chapter you will have a better understanding of this crucial term than you had at the beginning.

To give you some idea of the importance of the term, the widely-used search engine Google returns a list of over one billion web pages when given the prompt "random". Many of them are duplicates or near-duplicates, and some of them have nothing to do with the meaning of the term as treated in this chapter (for example, the web pages that are concerned with the rock group Random), but many of those pages do contain some very helpful information about the use of "random" in the scientific sense with which I am concerned.

I suggest that you pay particular attention to the connection between randomness and probability (see Section 2), especially the matter of which of those is defined in terms of the other. The literature is quite confusing in that respect.

There are very few symbols and no formulas, but there are LOTS of important concepts. A basic knowledge of statistics, measurement, and research design should be sufficient to follow the narrative (and even to catch me when I say something stupid).

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Section 1: What is the meaning of the word “random”?

According to Random House Webster's Dictionary [forgive me, but I just had to cite that source!], "random" is an adjective that means "occurring or done without definite aim, reason, or pattern". But that is a layperson's definition. A currently popular scientific definition of a random phenomenon, as given by the authors of a textbook used in many Advanced Placement statistics courses in high schools, is one for which individual outcomes cannot be specified but there is a mathematical distribution of outcomes when the number of repetitions is very large. However, Liu and Thompson (2002) claimed that interpretations of such a definition are often circular, and in his article "What is random?" Kac (1983) argued that most scientists never even bother to define "random"; they just take for granted that everyone knows what it means. He went on to say that the notion of something being "random" is actually very complicated.

[There is an article by May (1997) that is also entitled "What is random?", in which he refers to Kac's article. And there is a book entitled What is random?, by Beltrami (1999), which bears the subtitle Chance and order in mathematics and life. He (Beltrami) said that Kac's article "prompted the title of this book." (p. 146)]

My personal preference is that something is random if it is the result of a process in which chance is permitted to operate and plays a key role in the outcome.

Associated with the adjective "random" is the rather awkward noun "randomness". (At the amazon.com website a book-search of the word "randomness" results in approximately10,000 hits.) Wallis and Roberts (1962) included a chapter on randomness in their statistics book; it was defined as a property of a probabilistic process. In her book that bears the one-word title, Randomness, Bennett (1998) provided several definitions of randomness, but all were concerned with the notions of uncertainty and unpredictability. She also pointed out that even the experts have different views of it. In his book, The jungles of randomness, Peterson (1998) provided "a set of mathematical X rays that disclose the astonishing scope of randomness" (Preface, p. xii). The late 1990s seems to have been a productive time for books about randomness!

[TheThesaurasize website lists 71 synonyms for the word "randomness", but precious few of them have anything to do with chance.]

Perhaps equally as important as what the word "random" DOES mean is what the word DOES NOT mean (in science):

1. It does not mean "haphazard". Although it sounds like a contradiction in terms, something that is random is subject to the "laws" of probability. For example, if two people are to constitute a random sample, the probability that both of them will be selected is determined by mathematical formulas.

2. It does not mean "inconsequential". Although random outcomes often balance out, they can seriously attenuate certain matters such as the relationship between two variables--see, for example, Muchinsky (1996).

3. It does not mean "hopeless". There are many tried and true methods for understanding random phenomena and coping successfully with them.

4. Interestingly, and most importantly, it does not (necessarily) mean "representative". A small random sample, for example, may not reflect very well

the population from which it is drawn. As a matter of fact, it is not possible for a sample to be perfectly representative unless it consists of the entire population itself.

In the previous paragraphs I have used expressions such as "random phenomenon", "random sample", and "random outcomes". Those matters will be treated in greater detail in the remainder of this chapter, along with such terms as "random assignment", "random error", and many others (see Table of Contents). There are a few other statistical terms with the word “random” in them that are not treated in this book, but the online Pocket Dictionary of Statistics is very good for defining most if not all of them. (The first 34 entries under the letter "R" are all concerned with randomness.) There is also the term, “random access memory (RAM)”, which is an important concept associated with computers (I’m not very good at computers).

There are interesting answers to the question “How can there be such a concept as "random"? Surely everything has a structure if you look deeply enough. What does random actually mean? See the webpage http://www.fortunecity.com/emachines/e11/86/random.html that is associated with Ian Stewart’s (1989) book, Does God play dice?.

A good source for discussions of randomness of various kinds that is very funny, yet at the same time very instructive, is the book by Larry Gonick and Woollcott Smith entitled The cartoon guide to statistics (1993). I will refer to that book several times throughout this chapter.

Section 2: Which comes first, randomness or probability?

On page 161 of her book, Bennett (1998) discussed the attempts by vonMises (1957 and elsewhere) to define probability based upon the concept of randomness. In his theory of randomness, Kendall (1941) defined a “suite” (his term for an infinite sequence of a finite number of different symbols) as random if it satisfies certain probabilistic requirements. The first of these sources seems to imply that probability depends upon randomness; the second source seems to imply that randomness depends upon probability. What in the world is going on?

What is going on is a confusion as to which comes first--randomness or probability. Different authorities should be free to define randomness and probability in any way they choose (recall from Section 1 Kac's claim that most people don't define "random" at all), but it seems to me that you must take a stand one way or the other regarding which is the more basic concept and proceed from there. In the remainder of this chapter I would like to summarize a few of the various positions that have been taken concerning randomness and probability, tell you what mine is, and then ask you to decide for yourself who's "right" and who's "wrong".

Let's start with Bennett (1998). She claimed (page 9): "Probability is based on the concept of a random event...". I don't think that is always the case, if "a random event" is the tossing of a "fair" coin, the rolling of a perfectly balanced die, the drawing of a card from a well-shuffled deck, and the like. Probability applies to situations like those, to be sure--the probability of "heads" is 1/2; the probability of a "four" is one-sixth; the probability of an ace is 1/13; etc. But it also is applicable to other situations such as whether it will rain tomorrow or whether I'll win my tennis match, which may have nothing at all to do with random events. It would seem, therefore, that probability should either not be defined in terms of randomness or if it is it should be clear that only certain kinds of probabilities are so defined.

In David Moore's very popular statistics book, Statistics: Concepts and controversies (1979), Part III is entitled Drawing Conclusions From Data and consists of two chapters, one of which is “Probability: The Study of Randomness” (sound familiar?) and the other is “Formal Statistical Reasoning”. In the fifth edition (2001) Part III is entitled Chance and consists of four chapters: “Thinking About Chance”; “Probability Models”; “Simulation”; and “The House Edge: Expected Values”.

I agree with Kendall (1941) that randomness should be defined in terms of probability. I further attest that there are different kinds of randomness and each kind of randomness should be defined in terms of some kind of probability.

There are essentially three competing definitions of probability. The first definition, sometimes called the "a priori" or "deductive" definition, applies to symmetrical situations, and goes something like this:

The probability of a result A is equal to the ratio of the number of ways A can take place to the total number of equally likely results.

For example, we say that the probability of "heads" in a single toss of a "fair" coin is equal to 1/2, because there is one way that heads can take place, out of two equally likely results ("heads" and "tails").

There are two problems with that definition. The first problem has already been referred to: it only works for symmetrical situations. The second problem is that it is circular; probability is defined in terms of "equally likely" results, which is itself a probabilistic concept. It has at least one compensating feature, however. You don't have to actually toss the coin in order to talk about its probability of landing heads or tails!

The second definition, sometimes called the "relative frequency" or "empirical" definition, applies to any repeatable situation, and is:

The probability of a result A is equal to the limiting value of the ratio of the number of times A took place to the total number of results.

For example, in order to talk about the probability that a tossed thumbtack will land on its head, i.e., with its point up (note that the a priori definition wouldn't work here, since landing with its point up and landing on its side are not expected to be equally likely), you would toss the thumbtack a large number of times, count how many times it landed point up, count the total number of tosses, and divide the former by the latter. This definition also works for coins, whether fair or unfair; but there is a crucial change in the tense of the verb--from "can take place" to "took place".

There are also two problems with this definition, however: (1) What do we mean by "limiting value"? (How many times is a "large" number of times?); and (2) There is a rather strange time element entailed [bad pun]; you have to actually do something lots of times in order to get a fix on the probability of a particular result, which "from then on" (future tense?) gets associated with it.

The third definition is sometimes called the "subjective" or "personal" definition, and is the most controversial definition of the three (it is an integral part of the Bayesian approach to probability and statistics):

The probability of a result A is a number between 0 (impossibility) and 1 (certainty) that reflects a person's strength of conviction that A will take place.

For example, if you were interested in the probability that you would choose to order steak at a restaurant you had never been to before, you could assign to that eventuality a small number such as .01 if you don't like steak and/or can't afford it; or you could assign a large number such as .99, if you love steak and have lots of money.

The most serious objection to this third definition is that it is too subjective, with the probability of certain results having the potential to vary widely from one person to another. Some people argue vociferously that science in general, and mathematics in particular, should be objective. Proponents of the definition reply that complete objectivity is not possible in science or in anything else, and some point out that the selections of the .05 significance level or the 95% confidence interval in traditional statistical inference, for example, are decidedly subjective, albeit often reasonable, choices. Note also that the verb tense for this definition is "will take place".

What about randomness? None of the above definitions of probability explicitly refer to random events or random phenomena. Where do they fit in? To answer that question, let me now turn to a couple of non-coin examples from typical everyday research.

Example #1 (classical significance testing): Do boys do better in mathematics than girls do? In order to get some evidence regarding this question you need to give the same math test to a group of boys and a group of girls. You decide to take a random sample of 50 boys and a random sample of 50 girls from a very large public school district (say, New York or Los Angeles). When you go to sample the first person do you first think about the probability of a particular person being drawn and then worry about whether the draw is random; or do you first think about whether the draw is random and then worry about the probability of a particular person being drawn? I think it's the former; don't you? You want to define the probability that a particular person will be drawn and then you want to use some sort of randomizing device that will operationalize that probability.

Example #2 (survey research): What percentage of nurses smoke cigarettes? You decide to take a random sample of 1000 nurses from the population of all registered nurses who are members of the American Nurses Association (about two million people). You get access to a list of the R.N. license numbers of all two million of the nurses (the so-called "sampling frame"). You want each nurse in the population to have an equal chance of being drawn into your sample, so you might put each one of those numbers on a piece of paper, put the pieces of paper in a huge barrel, mix them up, and draw out 1000 of them. Although the purpose of the mixing is to accomplish randomness of selection, it is the equal likelihood that takes precedence.

For more on various meanings of probability and their applications I recommend that you read my favorite little probability book, Probabilities and life, by Emile Borel (1962); Shafer’s (1993) chapter in the Keren and Lewis handbook of methodological issues; and Salsburg's (2001) fascinating book, The lady tasting tea. I couldn’t find one mention of the word “random” or any of its derivatives in Borel’s book (but he does talk a lot about chance...see the following section). Shafer has a brief, but very nice, section on randomness (he talks about it with respect to numbers and an observer of those numbers). Salsburg doesn't say very much about randomness, but he talks a lot about probability and statistics. See if you can figure out whether they would regard probability as dependent upon randomness or randomness as dependent upon probability (or neither?).

For two interesting philosophical discussions of randomness, see Keene (1957) and Spencer Brown and Keene (1957). And for several cartoons that explain probability about as well as I’ve ever seen it (to paraphrase a popular saying, “A cartoon is worth a thousand words”), see Chapter 3 in Gonick and Smith (1993).

Section 3: Are randomness and chance the same thing?

As far as I'm concerned, the answer is "yes". As far as Google is concerned, the answer is apparently "no". It returns about a billion web pages for "chance" but only a little less than 24 million pages for "randomness" (about one billion for "random", as already cited in the Preface to this chapter).

Here are some examples. Consider first the tossing of a fair coin. The process of tossing such a coin usually possesses the property of randomness, because the result of any particular coin toss (heads or tails) usually cannot be pre-determined; it is a matter of chance, or “luck”-- Rescher's (2001) term for "randomness"--as to whether it will land face up (heads) or face down (tails) on any given toss. [I say "usually" because there is at least one person, statistician Persi Diaconis, who CAN pre-determine the results of the coins he tosses, by controlling the manner in which the toss is made--see McNamara (2003); Diaconis is an amateur magician. And see Ford (1983) regarding the randomness of a coin toss.]

The rolling of a fair die is a second example. It also possesses the characteristic of randomness for the same reason: The result of any particular roll (1, 2, 3, 4, 5, or 6) cannot be pre-determined. [As far as I know, Diaconis does not claim to be able to pre-determine the result of a die roll. But see Watson & Moritz (2003) regarding children’s judgments of the fairness of dice.]

A third example is the drawing of a card from a well-shuffled deck of cards. The card might be a black card or a red card; a spade, a heart, a diamond, or a club; an ace, 2, 3, 4, 5, 6, 7, 8, 9, 10, jack, queen, or king. Chance and chance alone (by virtue of the shuffling) determines what the actual result will be. [Card drawing by Diaconis is another exception here; he is VERY GOOD at pre-determining what card is drawn--see Mackenzie (2002) regarding Diaconis and see Gardner (1975a) regarding card shuffling in general.]

At the beginning of this section I said that chance and randomness are the same thing. But in Section 1 I claimed that probability and randomness are not the same thing. Aren't chance and probability the same thing, and by the transitive law shouldn't probability and randomness also be the same thing? My answer is "no" (chance and probability are not the same thing; Freedman, Pisani, & Purves, 1998, however, equate the two), but I'd better explain. I think of the results of events as "having" certain probabilities; I don't think of the results of events as "having" chance. For example, if the result in question is "fair coin lands heads", that result has an associated probability of .5; it doesn't have an associated chance of .5. "By chance" it may land heads, but "by chance" it may not. This may seem to you like a semantic distinction without a difference, but such a distinction between probability and chance is an important one (at least to me).

I recently came across a sentence that read "...[a particular statistic] has only a 5% chance of occurring by chance alone." I know what was meant by that: If the value for the population parameter that was stipulated in the hypothesis being tested was the true value, the probability was less than .05 that the given statistic would be as discrepant or more discrepant from the hypothesized value. But the "...chance...chance" wording blew my mind. The first usage (the 5% chance) is concerned with probability; the second usage (by chance alone) is concerned with randomness. I suggest that all of us avoid using expressions such as "there is a 5% chance" and concentrate on "there is a 5% probability" instead.

[It doesn't really fit here, but have you noticed that people often confuse "probability" and "odds"? When talking, for example, about the drawing of a spade in a single draw from a well-shuffled deck they say "the odds are 1 in 4". No, the probability is 1 in 4; the odds are 1 to 3 "in favor" and 3 to 1 "against".]

A fascinating example of the notion of randomness was provided by Peterson (1999), who discussed Divakar Viswanath's work regarding Fibonacci Numbers: a sequence of positive integers for which each integer is equal to the sum of the previous two integers (1, 1, 2, 3, 5, 8, 13, ...). He (Viswanath) wondered what would happen if you introduced an element of randomness in such a sequence (e.g., flipping a coin to decide whether to add or to subtract the previous two numbers). In so doing he discovered a new mathematical constant (1.13198824...). A constant resulting from randomness?! Neat, huh? (See Bennett, 1998, pp. 144-148, for a good discussion of Fibonacci Numbers.)

Beltrami (1999) discussed the equally fascinating example of "Janus-Faced" sequences of 0s and 1s (due to Bartlett, 1990) that are said to be random in one direction and deterministic in the other!

For an especially readable discussion of the role of chance in scientific arguments, see Chapter 2 of Abelson’s (1995) book. For an equally readable discussion of randomness and chance, see Levinson (1963), esp. pp. 184-186. Chance is also a favorite topic for courses, newsletters, and serious academic journals. See, for example, information regarding the quantitative literacy course developed at Dartmouth College (http://www.dartmouth.edu/~chance/), their Chance News, and the highly-regarded journal Chance. It's all wonderful stuff!

Section 4: Is randomness a characteristic of a process or a product?

In Section 1 I made reference to randomness as a characteristic of a process. That is actually a rather controversial matter, as Bennett (1998) explained in her chapter on "Randomness as Uncertainty" (see esp. pp. 165-172). Those (like me) who claim that something is random if it has been determined by a chance process appeal to some property of the object used in the process (e.g., the balance of a coin and a die) and/or the mixing mechanism for the process (e.g., the shuffling of a deck of cards). Others (like most missing-data experts and most measurement theorists--see Sections 11, 17, and 18) treat randomness as a characteristic of a product, and claim that it is the product that must be judged to be random or non-random. In the Preface to his book, Beltrami (1999, p. xiii) stated that there has been a general shift in the last several decades from randomness-as-process to randomness-as-product. [That may be true (alas) but I have at least two other people on my side. In the first of their two-volume handbook on data analysis, Keren & Lewis (1993) say: “Randomness is a property of the generating process rather than the outcome.” (p. 310)]

The consideration that separates the "product" advocates from the "process" advocates is whether or not you need any data in order to claim randomness. The product folks insist that data are necessary (but perhaps not sufficient), whereas the process folks insist that you can argue for or against randomness data-free, by appealing to one or more features of an alleged randomizing device.

A "middle-ground" approach is to argue that randomness pertains to the basic product of the alleged randomness-generating process. That is, it is those processes that must be judged to be random or non-random by virtue of the basic products that they generate. Once a process has been deemed to be random, any further data gathered as a result of the use of such a process need not be judged to be random or non-random.

Consider the following example: You would like to estimate the standard deviation of the heights of adult males. You have identified a target population of 1000 adult males, with associated ID numbers of 000 to 999, and you plan to draw a sample of size 50. You decide to use Minitab's random sampling routine (the process). You give it the proper commands and ask it to print the 50 sampled ID numbers. You indicate whether each of those numbers is less than or equal to 499 (and call those 0) or greater than 499 (and call those 1). You subject that string of 1s and 0s (the product) to one or more "tests of randomness" (see Section 7). Let's say that the string passes that (those) test(s), i.e., it is declared "random" and your sample is likewise declared "random". You then measure the heights of the 50 adult males, record them, calculate their standard deviation, and make whatever inference to the population of 1000 adult males is warranted. The heights of the 50 men in the sample need not be subject to any tests of randomness (Siegel & Castellan's [1988] argument to the contrary notwithstanding)--neither the randomness of their actual magnitudes nor the randomness of the order in which they were drawn--because the process has already been judged to be random. You may wind up with a lousy estimate of the standard deviation of the heights of all 1000 adult males in the sampled population, but that is a separate matter! (Do you follow that? Does it make sense?)

Section 5: What is a random-number generator?

A random-number generator is a device (usually a computer program of some sort) that creates sequences of single digits that are alleged to be "random" (in its product sense) and are useful in a variety of applications ranging from drawing a single sample from a population to establishing military codes that are very difficult to break. Some authors, e.g., Whitney (1984), insist that they be called "pseudo-random-number generators" because, they argue, if a sequence of digits can be generated it must have a deterministic, non-random force behind it.

Probability and randomness have often been defined in such a way that the result is circular (i.e., probability defined in terms of randomness and randomness defined in terms of probability) or is of infinite regress (A is defined in terms of B, which is defined in terms of C,...). The situation for random (and/or pseudo-random) number generators is analogous. How do we know that the numbers produced by a random-number generator are random? Can we test them for randomness? Perhaps (there are several such tests--see Section 7); but do we have to carry out a test of randomness for every sequence of numbers that is generated by an alleged random-number generator? (In my opinion, no, as discussed in the previous section.) Can we ever be sure that the numbers we're particularly interested in are random? (Alas, also no, in my opinion. We can never be "sure" of anything that has a stochastic, i.e., non-deterministic, component.)

It is important to understand that there is no such thing as "a" random number (Craw, 2003), even though there is currently a website that will give you "the random number of the day". Any number (1, 23, 617, whatever) can be one of the members of a set of numbers that are claimed to be random, with the randomness designation associated with the set and not with a particular member of that set. [Some numbers that are of special interest to mathematicians, for example “Omega” (see Gardner, 1979) have been given the designation of a “random” number or a “normal” number, but that designation has a particular and idiosyncratic meaning.]

There are lots of random-number generators in use today, both free-standing devices and routines built in to various statistical packages such as SAS, SPSS, and Minitab. The most common applications for random-number generators are in so-called "Monte Carlo" studies and in "bootstrap" inferences (Efron & Tibshirani, 1993). Monte Carlo studies are often undertaken in order to construct empirical sampling distributions for various statistics whose theoretical sampling distributions are too difficult to derive mathematically. (Well-defined population distributions having various properties are repeatedly sampled a very large number of times.) Bootstrap non-parametric methods are also used in the approximation of sampling distributions. A set of randomly sampled observations of size n is itself randomly sampled a very large number of times, with replacement both within sample and between samples, a particular statistic of interest is calculated for each sample, and a frequency distribution for that statistic is obtained. The actual value of the statistic for the given sample is compared to "all possible values" of the statistic for the given sample size, and an appropriate statistical inference is made (a test of a null hypothesis regarding, or a confidence interval for, a particular parameter of interest).

For an interesting discussion of the generation of random sequences of numbers I recommend the chapter by Pashley (1993). For further reading on random-number generators in general and for brief discussions of Monte Carlo and bootstrap methods, I recommend Chapter 8 in Bennett's (1998) book [entitled "Wanted: Random Numbers"); Chapter 9 in Peterson's (1998) book [he points out that many "seeds" for random-number generation lead to repeating cycles]; pp. 28-32, 243-245, and 289-291 of Salsburg's (2001) book; and the University of Utah's random-number-generators webpage. There is also the clever cartoon on page 65 of the Gonick and Smith (1993) book that shows a person pressing buttons to get random (actually pseudo-random) numbers.

Section 6: Where can you find tables of random numbers?

In his delightful essay, "Randomness as a resource", Hayes (2001) discussed the history and the ubiquity of random numbers and pseudo-random numbers, ranging from the results of 26,306 die rolls made by W.F.R. Weldon and his wife that were analyzed by Karl Pearson in 1900, to the 4.8 billion random bits made available to the public by Marsaglia (1995).

One of the most accessible and most frequently used tables is the RAND Corporation's table (1955, 2002), A million random digits with 100,000 normal deviates ["normal deviates"--how's that for an oxymoron?!]. That table has been subjected to all sorts of tests of randomness (see following section), and there have even been published Errata concerning it--provided by the statistician I.J. Good and available on the web. Brief excerpts taken from various portions of the table have been published in the backs of countless numbers of statistics textbooks. The numbers are alleged to be in random order when read in any direction (horizontally, vertically, diagonally, or whatever). Gardner (1975b) made some interesting observations concerning the RAND table, including his reference to a claim by Bork (1967) that such a table is strictly a twentieth century phenomenon: “A rational nineteenth-century man would have thought it the height of folly to produce a book containing only random numbers.” (p. 40 of Bork)

Other "classic" random-number tables are those generated by Tippett (1927), by Kendall and Babington-Smith (1938), and by Peatman and Schafer (1942). Kendall (1941) also propounded a theory of randomness, to which reference has already been made (see Section 2). In her book, Bennett (1998) pointed out that Tippett's alleged random numbers were soon (within ten years) found to be inadequate for many sampling applications (see Yule, 1938).

With the development of computers having ever-increasing speed and capacity, accompanied by the ever-increasing expertise of people to program them, it is far more common, however, for researchers to use random-number routines that are included in popular statistical packages such as Minitab. In even the older versions of Minitab (which I personally prefer to the newer ones), all you need do is give commands such as "Set 1:100 C1" and "Sample 10 C1 C2" and Shazam! you get a random sample of 10 numbers in Column 2 out of the 100 numbers from 1 to 100 in Column 1. You need to exercise extreme caution when using some computer-generated numbers that are alleged to be random, however. In a devastating but humorous article, Marsaglia (1968) showed that one popular (at the time) random-number generator, RANDU, was not very random at all.

You can also get random numbers on the internet. The website, random.org, for example, "offers true random numbers to anyone on the internet" [to quote from its website]. It also provides information regarding how those numbers are generated. And for a very rich source for the generation of random numbers and for all sorts of other statistical calculations I recommend the Interactive Stats section of the http://www.statpages.net website. It is REALLY nice.

Reference has already been made to the RAND book of single-digit random numbers, which also contains random decimal numbers that range from approximately -3.00 to +3.00 and have a normal distribution rather than a rectangular distribution. (See Box & Muller, 1958 and the Taygeta website concerning the generation of random normal deviates.)

Believe it or not, you can even get random names (first names--both male and female--and last names) on the internet. The website that provides them (for free) is www.kleimo.com. (They have over 21 million of them.) It's great fun. You can also choose an "obscurity factor" from 1 to 20 (the higher the number, the more obscure the name). I asked for five names that could be either male or female and had an obscurity factor of 20. I got as output the following names:

1. Minaya
2. Penelope Kornreich
3. Katy Pattillo
4. Jessie Bolten
5. Zelma Whitesides

You can't get much more obscure than that! Try it sometime. You'll like it.

How about random words? Yes, you can get those too--on the internet from Gammadyne software. The examples they give of words created by their Random Word Generator are: Asprari, Cropoli, Eclon, Enthyme, Flun, Lycand, Mofra, Nespresco, Nokamu, Shrunt, Strelm, and Vermack. They don't have any meanings (necessarily) and some of them are hard to pronounce, but think of all the possibilities! And you can get random "passphrases" that have been found to be useful in developing security codes. (See the Random Passphrase and Diceware Passphrase websites.)

And to properly use random words you need random sentences, don’t you? No problem; see Charles Kelly’s “Fun with Randomly-Generated Sentences” webpage, http://www.manythings.org/rs/.

Random poems? Try the plagiarist.com website. Random mathematical quotations? You can get them at the math.furma.edu website.

There is a website that will give you random facts (although I don't think they're generated by a random process) and another website that creates a random password (for free) that provides greater security protection than a password based upon lucky numbers, birthdates, or whatever.

Do you like baseball? I do (I'm what they call a "baseball nut"). There is a website called baseball-reference.com where you can call up a randomly-selected former major league baseball player and get all of his lifetime statistics.

If that isn't enough for you, you may want to try the random birthday generator that is included in John Pezzullo's marvelous collection of statistics stuff on the statpages.net website, and convince yourself that the probability of at least two out of n people having the same birthday is really quite high for relatively small n such as 25 or 30.

Finally, not to be outdone, another enterprising website (http://www.noentropy.net) with tongue in cheek provides NON-RANDOM, deterministic numbers. You can request from 1 to 10,000 of such numbers, but they're all 1.

Section 7: What are tests of randomness?

Most tests of randomness are tests of statistical significance applied to the product of a process (not the process itself) that is hypothesized to be random. The products are usually sequences of letters or numbers whose random or non-random pattern is of concern. Such tests are thought to be necessary by those who claim that randomness is strictly a property of a product; they are thought to be unnecessary and irrelevant by those who argue that randomness is strictly a property of a process.

The simplest of these is the so-called "runs test". (See Siegel & Castellan, 1988 and McKenzie et al., 1999.) Consider one sequence of the results of tossing a coin 30 times:

HHHHHHHHHHHHHHHTTTTTTTTTTTTTTT.

That sequence has two runs--an uninterrupted run of 15 Hs and an uninterrupted run of 15 Ts. Intuitively that seems like too few runs for a sequence to be judged to be random. It could have happened "by chance" with a fair coin, but most people would doubt it and would reject the hypothesis that the coin was a fair coin. The runs test would also reject that hypothesis.

Next consider another sequence of the results of tossing a different coin 30 times:

HTHTHTHTHTHTHTHTHTHTHTHTHTHTHT.

That sequence has 30 runs (of one symbol each). Intuitively that seems like <u>too many</u> runs for a random sequence. The runs test would also judge that coin to not be a fair coin.

Now consider the sequence HTHHTTHHHTTTHHHHTTTTHHHHHTTTTT. That has 10 runs--a run of one H, a run of one T, a run of two Hs, a run of two Ts, a run of three Hs, a run of three Ts, a run of four Hs, a run of four Ts, a run of five Hs, and a run of five Ts. That sequence "looks" more random, and the runs test would not reject the hypothesis that a fair coin produced it. But note the "non-random" regularity of that sequence.

Finally, consider the sequence TTHTHTHHHTHTTTHHTHHHTHTTTTHHTH. That has 18 runs (count 'em), looks much more random than any of the previous sequences, would be judged to be random by the runs test, and has no apparent regularity.

[Are you bothered by the fact that all four of the sequences were "too good" in the sense that each yielded exactly 15 heads and exactly 15 tails? Hmmm. Seems like there's two kinds of randomness or non-randomness going on here--one involving the relative numbers of heads and tails and the other involving the order in which they appear.]

For most tests of randomness all that can really be determined is a sort of "relative randomness", i.e., whether a sequence of digits is "more random than regular" or "more regular than random" (Griffiths & Tenenbaum, 2001; Pincus & Kalman, 1997; Pincus & Singer, 1996).

A particularly interesting approach to testing for randomness that does NOT involve a test of statistical significance is due to Chaitin (1966; 1975; 1990; 2001; 2002), who argued (drawing upon the previous work of Kolmogorov) that a sequence of digits is random if the shortest computer program for generating it is at least as long as the sequence itself. (See also Devlin, 2000.) By that rule the first example in this section (with Hs and Ts replaced by 1s and 0s, respectively) would be judged to be non-random, because a command "print 15 1s followed by 15 0s" is shorter than the sequence 111111111111111000000000000000. That particular sequence would also be judged to be non-random by the runs test.

The second example would also be judged to be non-random by Chaitin's definition (and by the runs test), because "print 15 pairs of alternating 1s and 0s" (or some such command) is shorter than 101010101010101010101010101010.

The third example is the most interesting. Unlike the runs test decision of "random", Chaitin's rule would claim "non-random" because the command "print patterns of pairs of alternating 1s and 0s, starting with one of each" (or, again, something to that effect--you can tell that I'm not a computer programmer!) can be made to be shorter than 101100111000111100001111100000.

The fourth example is easy. There is apparently no command other than "print 001010111010001101110100001101" that is shorter than 001010111010001101110100001101 itself.

I recommend that you "play around" with various sequences of 1s and 0s such as the above, "eyeball" each of them to make a judgment concerning which of the sequences you personally would regard as random and which you would not, and then subject each sequence to one or more of the tests of randomness and to Chaitin's rule, and see how the formal tests agree with your personal judgments.

Two of the most interesting sequences of digits are the mathematical constants e (the base for natural logarithms) and π (the ratio of the circumference of a circle to its diameter). The first of these, e, is equal to 2.71828... (it never cuts off) and the second of these, π, is equal to 3.14159...(it doesn't either). The question has been asked: Are the digits of π random? [It has also been asked about the digits of e.] Pathria (1961), for example, tested the first 10,000 digits of π for randomness; more recently, Bailey and Crandall (2001) subjected the first six billion digits of π (yes, it's known to at least that many places--see the article in the October, 1989 issue of Focus) to a test of randomness and found that each of the digits 0-9 appeared about six hundred million times. On the basis of that test they claimed that the six-billion-digit sequence for π is random. [By the way, the sequence of digits for π has also been put to music--see the MuSoft Builders website.]

There are a few other tests of randomness. For information regarding how to use them I suggest that you go to Chris Wetzel's website (just give Google the prompt "Wetzel randomness" and click on the first entry), or check out Marsaglia's (1995) series of such tests (called DIEHARD tests). And if you would like to study the results of coin tosses and are too lazy to actually toss the coins yourself, Ken White's Coin Flipping Page (on the web) will do it for you!

There is also an interesting literature in psychology regarding the extent to which people are accurate in their judgments about randomness. (See, for example, Bar-Hillel & Wagenaar [1991, 1993], Falk [1975, 1981], and Falk & Konold

[1997]. The first four of those references all bear the title “The perception of randomness”.)

Section 8: What is "random sampling" and why is it important?

(Simple) random sampling is a type of sampling from a population which is such that every sample of the same size has an equal chance of being selected. The word "simple" has been written in parentheses before "random sampling" because there are other types of random sampling (e.g., stratified random sampling--see Section 15), but whenever "random sampling" is not further modified it is assumed to be simple random sampling.

Consider a small population consisting of five persons A, B, C, D, and E. If you would like to draw a random sample of two persons from that population you must utilize a process such that the ten combinations A&B, A&C, A&D, A&E, B&C, B&D, B&E, C&D, C&E, and D&E are equally likely to be drawn. For example, you could write each of those ten combinations on a separate piece of paper, put those pieces of paper in a hat, stir them up, and draw out one piece of paper. The two letters that are written on that piece of paper would identify the two people who are to constitute your sample.

There are several reasons why random sampling is important:

(1) It is fair. If one of those combinations, say A&E [the TV network?] had a greater likelihood of being drawn than the others, any results based upon those two persons would be biased in their favor.

(2) It is objective. If you and I agree to draw a random sample of two persons from that population of five persons, your sample might be different from mine, but neither of us would be introducing any subjectivity into the process.

(3) Random sampling is an assumption that underlies just about every procedure for making statistical inferences from samples to populations. The formulas and tables that are commonly used for such inferences are not appropriate for non-random samples. As Bennett (1998) wrote: "The only way we can legitimately rate or compare the value of an observed sample statistic, such as a mean or a sum, within the hypothesized sampling distribution is to select a sample randomly." (p. 110)

Tables of random numbers (see Section 6) are the principal sources for carrying out random sampling. Rather than using letters, pieces of paper, and a hat for drawing that sample of two persons from a population of five persons, you could "code" them 1, 2, 3, 4, and 5 rather than A, B, C, D, and E; open up a table of single-digit random numbers to a "random" page (oh,oh! sounds circular already, doesn't it? but let's press on); close your eyes; drop your finger on a "random" spot on that page (that's worse?); and read off the digit that your finger landed on, along with the one to its immediate right (why the right? that's worst?). If both of those digits are different and/or are not 1, 2, 3, 4, or 5 you'll have to read a little further to the right (even "come around the bend" to the next row of digits?) until you encounter two different digits in the 1-5 range. Strange business, this randomness, isn't it?

I like to make the distinction between sampling persons and sampling already-existing measurements taken on persons. [Most people don't make this distinction, but you may have already figured out that I'm a loner when it comes to certain things.] Consider again this same simple example of persons A, B, C, D, and E, and an interest in measuring their heights. If we draw two persons from the five persons and then measure their heights in inches, should we be concerned about the randomness of the various identifying combinations A&B, A&C, etc., or should we be concerned about the randomness of their heights, say 67 & 72, 67 & 64, etc.? As you can gather from my remarks in Section 4, I would argue that we should only be concerned with the former, because the randomness--or non-randomness--occurs before the measurement. On the other hand, if their heights have already been measured, are stored in a file, and are sampled, some (for example, Siegel & Castellan, 1988) would be inclined to argue that it is the randomness of the heights (more specifically, the order in which the heights are drawn) that is of concern. I wouldn't, although I think it is important to make the distinction between sampling persons and sampling measurements. It is the process of selecting the persons that is to be judged to be random or non-random (in this case, how we choose the persons from the file), not the product of the process (in this case their heights).

There are those who do not actually draw samples at random but "regard" their samples as having been randomly drawn, for purposes of inferring from sample to population. Or they say something like: I'm using inferential statistics to generalize from the sample of subjects that I have to a population of subjects "like these". I think both of those positions are indefensible.

The statistical literature is replete with a number of excellent discussions of random sampling. But once again I especially recommend that you see some of the cartoons in Gonick and Smith (1993) that provide visual representations of the essential aspects of random sampling. The best of these, in my opinion, appear on pages 92 and 138.

Section 9: What is the difference between random sampling and random assignment?

One of the most bothersome (to me, anyhow) shortcomings in the methodological literature is the failure to properly distinguish between random sampling and random assignment. [Edgington (1995 and elsewhere) and Ludbrook & Dudley (1998) are notable exceptions.] The confusion is understandable since the two have much in common, but they have different purposes.

As indicated in the previous section, the purpose of random sampling is to provide the basis for making a statistical inference from a sample to the population from which the sample is drawn. We summarize the sample data by calculating some statistic (which we then know) for those data (a mean, a variance, a correlation coefficient, whatever) and infer something about the corresponding parameter (which we don't and may never know) for the sampled population. In terms of the jargon of the popular Campbell and Stanley (1966) book on experimental design, the matter is one of external validity (generalizability).

The purpose of random assignment (sometimes called "randomization") is quite different . First of all, it applies only to experimental research in which the independent variable will be "manipulated", i.e. some sort of "intervention" is to take place. We randomly assign subjects (participants in human research; animals in infra-human research; iron bars in metallurgical research; whatever) to "treatments" (the interventions) so that each subject has the same chance of being assigned to each treatment. Why? We want the subjects in the various treatment groups to be comparable at the beginning of the experiment, so that if they differ at the end of the experiment we can be reasonably assured that it is the treatments that "did it". That, again in the jargon of Campbell and Stanley, is a matter of internal validity (causality). For a particularly good discussion of causality see the article by Holland (1986), the accompanying comments, his rejoinder, and his later chapter (1993). There is also the paper by Freedman (2002). That paper gets a bit technical in spots, but he (Freedman, the senior author of the popular Freedman, Pisani, & Purves, 1998 statistics textbook) writes extremely well.

Ideally, we would like an experiment to possess both features (generalizability and causality), i.e., we would like to employ both random sampling and random assignment. For example, if we were to compare two different methods of teaching subtraction (take it from me that there are at least two methods), we would draw a random sample from the population of interest (say, all second graders in Los Angeles) and randomly assign half of them to be given Method A and the other half of them to be given Method B. If we were able to carry out such an experiment, we would be justified in using the traditional t-test of the significance of the difference between two independent sample means, provided that we were willing to make the usual assumptions of normality and homogeneity of variance. It is the random sampling that provides the justification.

Suppose, however, that we had random assignment but not random sampling, i.e., we had a non-random ("convenience") sample of the population to which we would like to generalize. In that case the appropriate analysis would not be the traditional t-test (which assumes random sampling), but a randomization test (not to be confused with a test of randomness)--sometimes called a permutation test (see Edgington, 1995 for the details)--which would provide the basis for the generalization not to the population itself (because of the lack of random sampling) but from the particular way that the participants in the sample happen to have been allocated to the treatments to all of the possible ways that those same people could have been allocated to the treatments. That would in turn provide a basis for claiming causality for those subjects, but any generalization to the full population would have to be a non-statistical one (based upon the researcher's judgment of the representativeness of the non-random sample).

For non-experimental research you might have random sampling but not random assignment (since there are no "treatments" to "assign" subjects to), in which case you would have the statistical basis for generalizability to the population, but an insufficient basis for assessing causality.

Finally, you might find yourself in a position of not having the luxury of either random sampling or random assignment. That doesn't necessarily mean that you should not carry out the study and report its results. But it does mean that you are restricted to the use of descriptive statistics only, with any sort of causal or generalizable interpretation being necessarily subjective and inadvisable. The best approach, in my opinion, to approximating causality in such studies is to use the technique called "propensity score analysis" (PSA)--see Rosenbaum and Rubin (1983). There is also the method advocated by Copas and Li (1997), whose long article bears the unfortunate title “Inference for non-random samples” (it is concerned solely with observational studies that have non-random assignment). And for some kinds of medical investigations with genetic aspects there is a recent approach that incorporates so-called “Mendelian randomization” (see Davey Smith & Ebrahim, 2003).

Associated with random assignment is the matter of "blocking". Rather than simply randomizing n subjects to two treatments A and B, with n/2 people assigned to each treatment, one might first create "blocks" of, say, four people each according to some variable, e.g., age, and randomly assign, within blocks, two people to Treatment A and two people to Treatment B. The four oldest persons would wind up two/two; the next four oldest persons also two/two; etc., with the total numbers in each of the treatments still n/2. The process could be refined even more by creating within each block what are sometimes called "random sandwiches", with the first oldest and the fourth oldest persons constituting the bread and the middle two persons constituting the meat. This is similar to the scheme used in a sport such as doubles in tennis, where the strongest player and the weakest player are pitted against the two players who are intermediate in strength.

[I can't resist pointing out that there are websites where you can purchase "real" random sandwiches. Just give the order and a sandwich consisting of a strange combination of ingredients will be delivered to your door!]

Although the objectives of random sampling and random assignment are different, you can use the same random-number tables for accomplishing both. One such source is the "Research Randomizer" website (www.randomizer.org). It will provide you, at no charge, a random sample of any n numbers out of any N numbers (where n is less than or equal to N) and/or a random assignment of n numbers into subsets of n_1, n_2, ..., n_k, where the sum of those subscripted n's is equal to n, and k is the number of "treatments".

For a fine article on the importance of randomization in experiments, see Boruch (2002). For an interesting exchange concerning random sampling vs. random assignment, see Shaver (1993) and Levin (1993). (See also Levin, 2002.) For a later "debate" on the necessity for randomization in clinical trials, see the article in Research in Nursing & Health by Sidani, Epstein, and Moritz (2003) and the commentary regarding that article by Ward, Scharf Donovan, and Serlin (2003).

Section 10: What is the randomized response method in survey research?

One of the most common problems in survey research is the refusal by many people to respond to sensitive questions that deal with emotionally charged matters, e.g., sexual behavior and religious beliefs. Even if they are promised anonymity ("nobody will ever be able to associate your response with your name") or confidentiality ("I can but I won't tell anyone else"), they will refuse to answer those questions when posed to them in a face-to-face interview and they will leave blank all such questions on a written questionnaire.

Several years ago Stanley Warner (1965) devised an ingenious procedure for trying to minimize non-response to sensitive questions, a method he called "randomized response". It goes something like this:

Let's say you were interested in estimating the percentage of college students who smoke marijuana (a sensitive matter that also has legal ramifications). Each respondent could be asked to use a table of random numbers to select a random number from 00 to 99 and if the number selected is, say, 70 or above the student would be asked to respond to a sensitive question such as "Do you smoke marijuana at least once a week?" If the number is 69 or below the student would be asked to respond to an unrelated question such as "Is the last digit of your student ID number odd?" [This example is described in considerable detail in the delightful article by Campbell & Joiner (1973) entitled "How to get the answer without being sure you've asked the question".] Nobody need know who answered which question (the responses consist of a simple "yes" or "no" for each student), but by making certain reasonable assumptions (that the percentage of students who have answered the sensitive question is 30 and the percentage of students who have an odd ID number is 50) and using a standard formula for conditional probabilities, the percentages of “yes” answers to the sensitive question can be calculated.

There are several variations of this technique that have been devised over the years (see, for example, Fox & Tracy, 1986) but all have the same objective of estimating the percentage of respondents who hold certain views on various sensitive issues or who engage in various sensitive practices.

Section 11: Under what circumstances can data be regarded as missing at random?

One of the most frustrating problems in data analysis is the absence of one or more pieces of data. Researchers usually go to great lengths in designing their studies, choosing the appropriate measuring instruments, drawing their samples, and collecting their data, only to discover that some of the observations are missing, due to a variety of reasons (an item on a questionnaire left blank, a clerk's neglect to enter an important piece of information, a data-recording instrument's failure, etc.). How do you cope with such a problem? The literature suggests that there are essentially two strategies--deletion or imputation. You can delete all or some of the non-missing data for the entities for which any data are missing; or you can try to impute (estimate) what the missing data "would have been". If you choose the imputation strategy, the literature further suggests that you need to determine whether or not the data are "missing at random". But when are data missing at random?

My personal opinion is "never", but I am apparently in the minority. One of the gurus of missing data, Donald B. Rubin (1976), defined three kinds of "missingness" (see also Little & Rubin, 2002):

1. Missing at random (MAR). Data are said to be MAR if the distribution of missingness does not depend upon the actual values of the missing data (i.e, what they would have been).

2. Missing completely at random (MCAR). Data are said to be MCAR if the distribution of missingness also does not depend upon the actual values of the other data that are not missing.

3. Missing not at random (MNAR). Data are said to be MNAR if the distribution of missingness does depend upon the actual values of the missing data.

The article by Schafer and Graham (2002) is particularly good for further clarifying those three kinds of missingness, along with examples of each. Rubin's definitions of random missingness are "product-oriented" rather than "process-oriented", i.e., one needs to make certain asumptions and/or analyze some actual evidence in order to determine whether or not, or the extent to which, data might be missing "at random". That view, although perhaps correct, is contrary to mine. It also appears (at least to me) to be contrary to most people's concept of a random phenomenon, where chance should play an essential role. People don't flip coins, roll dice, or draw cards in order to determine whether or not they will respond to a particular item on a questionnaire. Data entry clerks don't employ such devices in order to determine whether or not they will enter a participant's response in a data file. And recording instruments don't choose random times to break down. Do they?? And how can you analyze non-missing data to draw conclusions regarding the randomness or non-randomness of missing data?

In the spirit of Rubin's product-dependent concept of randomness, Schmitz and Franz (2001) even provide a method (the popular bootstrap technique) for testing whether or not the data from study dropouts are missing at random! Amazing.

The actual size of the total sample and the proportion of missingness need to be taken into consideration no matter what you decide about the "randomness of the missingness". If the sample is large and the proportion of missingness is small, it doesn't really matter what you do. But if, say, you have at least one missing observation for each member of your sample, and it is a different observation for each subject, you have a major, major problem. (In that case so-called "listwise deletion", where all of the data for a subject are deleted if the subject has any missing data, is not an option, since you would then have an n of 0.)

Section 12: What is a random variable?

"Random variable" is a term originating in mathematical statistics, and refers to a variable that can take on any value from some given probability distribution. For example, "outcome of a single toss of a fair coin" is a random variable (a so-called Bernoulli variable) that can take on the value H (1) or T(0) for any particular toss of such a coin with equal probability.

The most commonly discussed, but in my opinion the most over-rated, random variable is a variable that is distributed according to the normal, bell-shaped, Gaussian form. [I'll probably get into trouble for saying it's over-rated, but I (and Micceri, 1989) claim that most variables are NOT normally distributed--income, for example, is not normally distributed in any population---and the primary reason for the popularity of the normal distribution is that the mathematical statisticians know all about it!]

Traub (1994) based his development of classical reliability theory upon the concept of a random variable, as had Lord and Novick (1968) in their very popular textbook. I personally prefer Gulliksen's (1950) approach, which did not explicitly employ the concept of a random variable.

There is an interesting parallel between a random variable and the concept of an "event" in probability theory, as pointed out to me by my colleague and friend Ron Serlin (personal communication, March 10, 2003). Mathematical statisticians talk about a random variable Y taking on the value of y and an event E taking on the value of result e. For example, it is well-known that the probability of a "1" (= y) for a Bernoulli variable (= Y) is equal to P. Similarly, the probability of a "1" (= e) for a (fair or unfair) coin toss (= E) is equal to its P.

An interesting "take" on the concept of a random variable is that of Van Lehn (2002), who claims that a random variable is neither random nor a variable, but is best thought of as simply a function on a sample space.

For an entire collection of cartoons that illustrate the concept of a random variable better than any section of a traditional statistics textbook, see Chapter 4 in Gonick and Smith (1993).

Section 13: What is the difference between random effects and fixed effects in experimental research?

Random effects are effects in an experiment that can be generalized to a "population" of treatments from which a subset of treatments has been randomly sampled. For example, if in a particular study you randomly selected three drug dosages out of ten drug dosages to actually be tested against one another, and you found a statistically significant difference among the three, you would be justified in inferring that there was a difference among all ten. [But you could of course be wrong.]

Fixed effects, on the other hand, are effects that pertain only to the treatments that are specifically employed in the experiment itself. For example, if you were only interested in the effect of 100 milligrams of a particular drug vs. 200 milligrams of that drug, the research participants were randomly assigned to one or the other of those two doses, and you found a statistically significant difference between those two dosages, you would have no basis for generalizing the findings to other dosages such as 50 milligrams or 500 milligrams.

Some variables that are employed as covariates in experimental research are necessarily fixed, e.g., sex. There are just two sexes, so if and when sex is a variable in a research design generalizations to "other sexes" wouldn't make any sense. (It is of course possible for sex to be held constant, rather than to be a variable, if one were interested solely in an experimental effect for males only or for females only.)

Section 14: Can random sampling be either with replacement or without replacement?

Yes (but see Hoffman, 2002, who claims otherwise). If the first object is not replaced in the population before the second object is drawn, the second object admittedly has a different selection probability, but the process is nevertheless still random. The better approach to the problem, however, is to refer to the formal definition of random sampling (as given by Wallis & Roberts, 1962, for example) and think of the drawing of a random sample as a procedure for selecting a combination of n things from a population of N things, with each combination being equally likely to be the one actually drawn.

The matter of sampling with replacement or without replacement is rather fascinating. Virtually all of traditional statistical inference is based upon sampling with replacement, e.g., all of the well-known parametric significance tests for which a normal population distribution is known or assumed. But most procedures for actually drawing samples are based upon sampling without replacement. When using a table of random numbers (see Section 6), if an ID number is encountered that is the same as an ID number that has already been drawn, the researcher is told to ignore that number and go on to the next. The reason for that is to avoid having a person being selected more than once; if one or more persons were in the data two or more times, the sample observations would not be independent.

The saving grace in all of this is that most sample sizes are much smaller than most population sizes, so for all practical purposes it doesn't really matter whether the sampling is with or without replacement. It is extremely unlikely in such cases that a given ID number would be sampled more than once.

Section 15: What is stratified random sampling and how does it differ from stratified random assignment (blocking)?

Stratified random sampling is a two-step process. The population to be sampled is first divided into two or more parts called "strata" (singular: "stratum") and then a simple random sample is drawn from each stratum. For example, if you wanted to take a sample of 30 people from a population that consisted of 100 men and 200 women, and you wanted your sample to have the same proportion of men and women as the population had, you would draw a simple random sample of 10 men from the male stratum and a simple random sample of 20 women from the female stratum.

Stratified random assignment, better designated as blocking, pertains to the sample and not to the population, but is a similar process. That is, you first divide the sample (no matter how it has been drawn) into two or more strata and then you randomly assign to treatments within strata. For example, if you were carrying out an experiment to test the effect of an experimental treatment vs. a control treatment for a sample that consisted of 10 males and 20 females, and you wanted to be sure that you had the proper proportions of males and females in each of the treatments, you would randomly assign 5 of the males to the experimental treatment and the other 5 of the males to the control treatment, and you would randomly assign 10 of the females to the experimental treatment and the other 10 of the females to the control treatment. This would permit you to test the “main effect” of treatment, the “main effect” of sex, and the sex-by-treatment “interaction effect”.

Blocking also plays a key role in the statistical method for testing causality in non-experimental research called propensity score analysis (PSA)--see Section 9. What is entailed is the calculation of a propensity (to fall into one intact group vs. the other) score, the rank-ordering of those scores, the creation of a small, but not too small, number of blocks of those scores, and a comparison of the two groups within each of those blocks. (See Rosenbaum & Rubin, 1983 for more specific details.)

The best way to remember the difference between stratified random sampling and stratified random assignment (blocking) is to repeat the mantra "stratify the population; block the sample".

For a clever cartoon that illustrates stratified random sampling, see Gonick and Smith (1993), page 95.

Section 16: What is the difference between stratified random sampling and quota sampling?

The principal difference is the presence of a chance element in one (stratified random sampling) and the absence of a chance element in the other (quota sampling). For example, if you wanted to conduct a survey and you insisted that there be the same number of men and women, and of younger and older adults, in your survey, you could stratify the population to be sampled into, say, four strata (male, 18-40; female 18-40; male, over 40; female, over 40) and take a simple random sample of, say, 50 people from each of those strata. OR you could go out into the highways and byways and sample until you got 50 people (any 50 people...your "quota") for each of the four categories. Which approach do you think is better?

Section 17: What is a random error?

A random error is any error that can be attributed to chance. The most common use of the term is in various theories of measurement where the emphasis is upon "random measurement error" due to the unreliability of measuring instruments.

The type of error that is associated with having a sample of a population rather than the entire population is usually called, logically enough, "sampling error", but the modifier "random" is taken to be understood. Random measurement error is often called "non-sampling error", but that term is confusing in educational and psychological research, where much of measurement error is actually attributable to the sampling of test items from populations of test items.

There is one other kind of error that is unique to educational and psychological testing. We could arrive at a total score on an achievement test, for example, by counting the number of wrong responses rather than the number of right responses. But the number of right responses and the number of wrong responses are both types of obtained scores, not error scores in the measurement-theoretical sense of that word (see next section), and in any event are not random.

Speaking of right responses and wrong responses, there is a vast literature in educational and psychology on the matter of chance success for certain kinds of tests (multiple-choice, true/false, and matching) in which answers are selected from various given alternatives, i.e., they are not supplied by the test-taker. Some measurement experts claim that scores on such tests should be corrected for chance success by subtracting some fraction of the wrong answers from the total number of right answers. The formulas for so doing assume that (a) some examinees do guess (whether you tell them to or not to) and (b) if and when they guess they guess randomly. Both (a) and (b) are questionable assumptions. I personally feel that the whole matter of correction for chance success has been highly over-rated. (For opposite points of view you might want to read Plumlee, 1952,1954; Mattson, 1965; Zimmerman & Williams, 1965; or the "correction-for-guessing" sections of most introductory textbooks in educational or psychological measurement.) However, I must admit that I once wrote a long article in which I provided formulas for estimating the reliability of a single test item where chance success is possible (Knapp, 1977).

There is an equally vast literature regarding the correction for chance agreement of various inter-rater and intra-rater reliability coeficients--see Cohen's (1960) discussion of his kappa coefficient and any other source that talks about that particular statistic. The formulas for doing that, like the correction for chance success formulas, assume that some raters provide some ratings randomly and some portion of the percent agreement between or within raters is chance agreement. I think that is also questionable and there is little or no need for Cohen's kappa. If there is ever any reason to believe that raters rate randomly all you need to do is raise the standard as to what constitutes "good" percent agreement.

For more on random measurement error, see any or all of the following: Cureton (1931), Cochran (1968), Grubbs (1973), Jaech (1985), Schmidt and Hunter

(1996), the "Standards" for educational and psychological tests (AERA, APA, & NCME (2014), and Dunn (2003).

Section 18: Does classical reliability theory necessarily assume random error?

The answer is an emphatic "no", as shown by Gulliksen (1950) in his excellent textbook on the theory of mental tests and as I have tried to demonstrate in my reliability book (Knapp, 2015). Most other sources, however, insist that the kinds of errors that are associated with reliability are random measurement errors. (They also claim that non-random measurement errors are “constant” errors and are associated with invalidity.)

Gulliksen (1950) explained how all of the theorems of classical reliability theory can be derived EITHER by first defining error score as random (mean equal to zero, zero correlation between error for one instrument and error for another instrument, etc.) and then defining true score as the difference between obtained score and error score; OR by first defining true score as the mean of an infinite number of obtained scores on parallel forms of the instrument and then defining error score as the difference between obtained score and true score.

Defining random measurement error first and then letting true score fall out as the difference between obtained score and error score is by far the more popular approach and, as you can see, its definition (alas) is "product-oriented" rather than "process-oriented" because it is based upon the assumption of, and/or evidence for, zero mean, zero correlations with other measurement errors, and the like.

Reference was made above to "an infinite number of obtained scores on parallel forms". The matter of parallel forms has been a controversial one ever since its introduction to measurement theory almost a century ago. It is a reasonable (albeit ethereal) notion in certain contexts such as spelling tests, but not in other contexts such as the measurement of length. For a spelling test it is at least possible to imagine a perhaps not infinite but very large number of parallel forms that could be constructed by randomly sampling words from an unabridged dictionary (without replacement within form but with replacement between forms). But for a yardstick or other instrument for measuring length it is almost impossible to define parallel forms, much less imagine an infinite number of them.

Parallel forms can be "randomly parallel" (as alluded to in the previous paragraph) or "rigorously parallel" (satisfying a variety of conditions). If you are interested in that distinction please see the textbooks written by Kelley (1927), by Gulliksen (1950), by Lord and Novick (1968), and by Nunnally and Bernstein (1994) and/or the articles written by Lord (1964) and by Novick (1966). Kelley was a strong advocate of the parallel form approach to reliability. Another measurement expert, Louis Guttman, questioned the entire concept of parallel forms. He favored the test-retest approach. Others, e.g., Lee Cronbach, contributed to the development of methods for determining "internal consistency" reliability (which Kelley refused to acknowledge as "reliability" at all; he referred to them as procedures for assessing the homogeneity of a number of variables--usually test items--that were alleged to measure the same construct.)

Section 19: What do you do if one or more persons who are randomly selected for a research study refuse to participate and/or refuse to be randomly assigned to a particular treatment?

In Section 11 I discussed the frustration associated with missing data and whether or not the absent data could be considered as being randomly missing. What is equally frustrating is to draw a random sample of people for a research study and have one or more of them refuse to participate at all or, in the case of an experiment, refuse to be assigned to a particular treatment. If that happens, what should you do?

You have several choices:

1. You can augment the cooperating segment of the sample with another random sample equal in size to that of the non-cooperating segment. For example, if you had selected a random sample of 50 subjects from a particular population and 10 of them refused to participate, you could select a random sample of an additional 10 subjects from that same population to replace the 10 refusals. This would restore your original sample size (unless some of the new 10 also refuse to participate!), but the final sample would not be nearly as "clean" as the originally drawn sample, as far as any inference to the population is concerned, since non-cooperators would not be represented in that sample.

2. You could go ahead and carry out the study on the basis of the sub-sample of cooperating respondents only, but that would constrain the generalizability to the sub-population of cooperators.

3. You could use a complicated analysis called "intent to treat" analysis (see, for example, Green, Benedetti, & Crowley, 2002), wherein the data for the "refuseniks" are counted in with the group to which they were assigned, no matter what they decided to do (or not do) subsequent to that assignment.

4. You could (but don't) try to estimate all of the data that those non-cooperating subjects would have provided had they been cooperators. There is a variety of techniques for estimating data when you have some non-missing data for people who do agree to participate in the study but do not provide full data (again see Section 11), but if people don't provide any data those techniques don't work.

If you are comparing "pretest" and "posttest" scores and there is some attrition between pretest and posttest, there is one thing you should not do, and that's to display in a table the summary descriptive statistical information (means, standard deviations, etc.) for the full sample at pretest time and for the reduced sample at posttest time. It's a classic case of "apples and oranges". If you feel the need for such a table you must display three sets of summary data: (1) pretest scores for the "dropouts"; (2) pretest scores for the "non-dropouts"; and (3) posttest scores for the non-dropouts. Summaries (2) and (3) are comparable; summary (1) provides a basis for determining whether the dropouts and the non-dropouts are sufficiently similar for the (2) vs. (3) comparison to be meaningful.

Section 20: What is a random walk?

A random walk is a series of steps that have a point of origin and are such that the direction each step takes is a matter of chance, i.e., it is a random process. The most widely-discussed application is to the drunk who starts at a bar and walks (stumbles, actually) in one direction or the other, with each direction having a known probability (the probabilities are usually assumed to be equal) of being followed. (See the cartoons on page 215 of Gonick & Smith, 1993 and on the Physics Random Walk website.) The typical research questions for such a situation are things like "What is the expected location after n steps?" or "What is the probability that the drunk is at a certain distance x, from the bar, after n steps?" (See, for example, the delightful paper by Monte, 2003 for an analysis of the case of x = 0, i.e., the drunk arrives right back at the bar). But there are many more serious applications of random walks to important problems in the physical and the social sciences. (See, for example, the online Training Handbook for Anthropometric Surveys.htm for a fascinating illustration of the use of a random walk for collecting household survey data.)

Is it too much of a stretch for me to claim that I have taken you on sort of a random walk through this chapter? If so, I hope your final destination has been an understanding of the concept of a random phenomenon and not back at "the bar" where you started out.

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CHAPTER 5: WHAT A PILOT STUDY IS…AND ISN'T

Introduction

Googling "pilot study" returns almost 10 million entries. One of the first things that come up are links to various definitions of a pilot study, some of which are quite similar to one another and some of which differ rather dramatically from one another.

The purpose of the present chapter is twofold: (1) to clarify some of those definitions; and (2) to further pursue specific concerns regarding pilot studies, such as the matter of sample size; the question of whether or not the results of pilot studies should be published; and the use of obtained effect sizes in pilot studies as hypothesized effect sizes in main studies. I would also like to call attention to a few examples of studies that are called pilot studies (some correctly, some incorrectly); and to recommend several sources that discuss what pilot studies are and what they are not.

Definitions

1. To some people a pilot study is the same as a feasibility study (sometimes referred to as a "vanguard study" [see Thabane, et al., 2010 regarding that term]); i.e., it is a study carried out prior to a main study, whose purpose is to "get the bugs out" beforehand. A few authors make a minor distinction between pilot study and feasibility study, with the former requiring slightly larger sample sizes and the latter focusing on only one or two aspects, e.g., whether or not participants in a survey will agree to answer certain questions that have to do with religious beliefs or sexual behavior.

2. Other people regard any small-sample study as a pilot study, whether or not it is carried out as a prelude to a larger study. For example, a study of the relationship between length and weight for a sample of ten newborns is not a pilot study, unless the purpose is to get some evidence for the quality of previously untried measuring instruments. (That is unlikely, since reliable and valid methods for measuring length and weight of newborns are readily available.) A defensible designation for such an investigation might be the term "small study" itself. "Exploratory study" or "descriptive study" have been suggested, but they require much larger samples.

3. Still others restrict the term to a preliminary miniature of a randomized clinical trial. Randomized clinical trials (true experiments) aren't the only kinds of studies that require piloting, however. See, for example, the phenomenological study of three White females and one Hispanic male by Deal (2010) that was called a pilot study, and appropriately so.

4. Perhaps the best approach to take for a pilot study is to specify its particular purpose. Is it to try out the design protocol? To see if subjects agree to be active participants? To help in the preparation of a training manual? Etc.

Sample size

What sample size should be used for a pilot study? Julious (2005) said 12 per group and provided some reasons for that claim. Hertzog (2008) wrote a long article devoted to the question. The approach she favored was the determination of the sample size that is tolerably satisfactory with respect to the width of a confidence interval around the statistic of principal interest. That is appropriate if the pilot sample is a random sample, and if the statistic of principal interest in the subsequent main study is the same as the one in the pilot study. It also avoids the problem of the premature postulation of a hypothesis before the design of the main study is finalized. The purpose of a pilot study is not to test a substantive hypothesis (see below), and sample size determination on the basis of a power analysis is not justified for such studies.

Hertzog (2008) also noted in passing some other approaches to the determination of sample size for a pilot study that have been suggested in the literature, e.g., "approximately 10 participants" (Nieswiadomy, 2002) and "10% of the final study size" (Lackey & Wingate, 1998).

Reporting the substantive results of a pilot study

Should the findings of a pilot study be published? Some researchers say "yes", especially if no serious deficiencies are discovered in the pilot. Others give a resounding "no". Consider an artificial example of a pilot study that might be carried out prior to a main study of the relationship between sex and political affiliation for nurses. There are 48 nurses in the sample, 36 of whom are females and 12 of whom are males. Of the 36 females, 24 are Democrats and 12 are Republicans. Of the 12 males, 3 are Democrats and 9 are Republicans. The data are displayed in Table 1.

Table 1: A contingency table for investigating the relationship between sex and political affiliation.

	Sex		
	Male	Female	Total
Political Affiliation			
Democrat	3 (25%)	24 (67%)	27
Republican	9 (75%)	12 (33%)	21
Total	12	36	48

The females were more likely to be Democrats than the males (66.67% vs. 25%, a difference of over 40%). Or, equivalently, the males were more likely to be Republicans (75% vs. 33.33%, which is the same difference of over 40%).

A sample of size 48 is "on the high side" for pilot studies, and if that sample were to have been randomly drawn from some well-defined population and/or known to be representative of such a population, an argument might be made for seeking publication of the finding that would be regarded as a fairly strong relationship between sex and political affiliation.

On the other hand, would a reader really care about the published result of a difference of over 40% between female and male nurses for that pilot sample? What matters is the magnitude of the difference in the main study.

Obtained effects in pilot studies and hypothesized effects in main studies

In the previous sections it was argued that substantive findings of pilot studies are not publishable and sample sizes for pilot studies should not be determined on the basis of power analysis. That brings up what is one of the most serious misunderstandings of the purpose of a pilot study, viz., the use of the obtained effects obtained in pilot studies as the hypothesized effects in the subsequent main studies.

Very simply put, hypothesized effects of clinically important interventions should come from theory, not from pilot studies (and usually not from anything else, including previous research on the same topic). If there is no theoretical justification for a particular effect (usually incorporated in a hypothesis alternative to the null), then the main study should not be undertaken. The following artificial, but not atypical, example should make this point clear.

Suppose that the effectiveness of a new drug is to be compared with the effectiveness of an old drug for reducing the pain associated with bed sores. The researcher believes that a pilot study is called for, because both of the drugs might have some side effects and because the self-report scale for measuring pain is previously untested. The pilot is undertaken for a sample of size 20 and it is found that the new drug is a fourth of a standard deviation better than the old drug. A fourth of a standard deviation difference is usually regarded as a “small” effect. For the main study (a randomized clinical trial) it is hypothesized that the effect will be the same, i.e., a fourth of a standard deviation. Cohen’s (1988) power and sample size tables are consulted, the optimum sample size is determined, a sample of that size is drawn, the main study is carried out, and the null hypothesis of no effect is either rejected or not rejected, depending upon whether the sample test statistic is statistically significant or not.

That is not an appropriate way to design a randomized clinical trial. It is difficult to imagine how a researcher could be comfortable with a hypothesized effect size arising from a small pilot study that used possibly deficient methods. Researchers admittedly find it difficult to postulate an effect size to be tested in a main study, since most theories don't explicitly claim that "the effect is large" or "the effect is small [but not null]", or whatever, so they often default to "medium". That too is inappropriate. It is much better to intellectualize the magnitude of a hypothesized effect that is clinically defensible than to use some arbitrary value.

Some real-world examples

In order to illustrate proper and improper uses of the term "pilot study" the following four examples have been selected from the nursing research literature of the past decade (2001 to 2010). The four studies might have other commendable features or other not-so-commendable features. The emphasis will be placed only on the extent to which each of the studies lays claim to being a pilot study. All have the words "pilot study" in their titles or subtitles.

1. Sole, Byers, Ludy, and Ostrow (2002), "Suctioning techniques and airways management practices: Pilot study and instrument evaluation".

This was a prototypical pilot study. The procedures that were planned to be used in a subsequent main study (STAMP, a large multisite investigation) were tried out, some problems were detected, and the necessary changes were recommended to be implemented.

2. Jacobson and Wood (2006), "Lessons learned from a very small pilot study".

This was also a pilot study, in the feasibility sense. Nine persons from three families were studied in order to determine if a proposed in-home intervention could be properly implemented.

3. Minardi and Blanchard (2004), "Older people with depression: pilot study".

This was not a pilot study. It was a "quasi-experimental, cross-sectional" study (Abstract) that investigated the prevalence of depression for a convenience sample of 24 participants. There was no indication that the study was carried out in order to determine if there were any problems with methodological matters, and there was no reference to a subsequent main study.

4. Tousman, Zeitz, and Taylor (2010), "A pilot study assessing the impact of a learner-centered adult asthma self-management program on psychological outcomes".

This was also not a pilot study. There was no discussion of a specific plan to carry out a main study, other than the following rather general sentence near the end of the article: "In the future, we plan to offer our program within a large health care system where we will have access to a larger pool of applicants to conduct a randomized controlled behavioral trial" (p. 83). The study itself was a single-group (no control group) pre-experiment (Campbell & Stanley's [1966] Design #2) in which change from pre-treatment to post-treatment of a convenience sample of 21 participants was investigated. The substantive results were of primary concern.

Recommended sources for further reading

There are many other sources that provide good discussions of the ins and outs of pilot studies. For designations of pilot studies in nursing research it would be well to start with the section in Polit and Beck (2011) and then read the editorials by Becker (2008) and by Conn (2010) and the article by Conn, Algase, Rawl, Zerwic, and Wymans (2010). Then go from there to Thabane, et al.'s (2010) tutorial, the section in Moher, et al. (2010) regarding the CONSORT treatment of pilot studies, and the articles by Kraemer, Mintz, Noda, Tinkleberg, and Yesavage (2006) and Leon, Davis, and Kraemer (2011). Kraemer and her colleagues make a very strong case for not using an obtained effect size from a pilot study as a hypothesized effect size for a main study. Kraemer also has a video clip on pilot studies, which is accessible at the 4Researchers.org website.

A journal entitled Pilot and Feasibility Studies has recently been published. Of particular relevance to the present chapter are the editorial for the inaugural issue by Lancaster (2015) and the article by Ashe, et al. (2015) in that same issue.

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CHAPTER 6: WOMB MATES

I've always been fascinated by twins ("womb mates"; I stole that term from a 2004 article in The Economist). As far as I know, I am not one (my mother and father never told me so, anyhow), but my name, Thomas, does mean "twin". I am particularly concerned about the frequency of twin births and about the non-independence of observations in studies in which some or all of the participants are twins. This chapter will address both matters.

Frequency

According to various sources on the internet (see for example, CDC, 2013; Fierro, 2014):

1. Approximately 3.31% of all births are twin births, either monozygotic ("identical") or dizygotic ("fraternal"). Monozygotic births are necessarily same-sex; dizygotic births can be either same-sex or opposite-sex.

2. The rates are considerably lower for Hispanic mothers (approximately 2.26%).

3. The rates are much higher for older mothers (approximately 11% for mothers over 50 years of age).

4. The rate for a monozygotic twin birth (approximately 1/2%) is less than that for a dizygotic twin birth.

An interesting twin dataset

I recently obtained access to a large dataset consisting of adult male radiologic technicians. 187 of them were twins, but not of one another (at least there was no indication of same). It was tempting to see if any of their characteristics differed "significantly" from adult male twins in general, but that was not justifiable because although those twins represented a subset of a 50% random sample of the adult male radiologic technicians, they were not a random sample of US twins. Nevertheless, here are a few findings for those 187 people:

1, The correlation (Pearson product-moment) between their heights and their weights was approximately .43 for 175 of the 187. (There were some missing data.) That's fairly typical. [You can tell that I like to investigate the relationship between height and weight.]

2, For a very small subset (n = 17) of those twins who had died during the course of the study, the correlation between height and weight was approximately .50, which again is fairly typical.

3. For that same small sample, the correlation between height and age at death was approximately -.14 (the taller ones had slightly shorter lives) and the correlation between weight and age at death was approximately -.42 (the heavier persons also had shorter lives). Neither finding is surprising. Big dogs have shorter life expectancies, on the average (see, for example, the pets.ca website); so do big people.

Another interesting set of twin data

In his book, Twins: Black and White, Osborne (1980) provided some data for the heights and weights of Black twin-pairs. In one of my previous articles (Knapp, 1984) I discussed some of the problems involved in the determination of the relationship between height and weight for twins. (I used a small sample of seven pairs of Osborne's 16-year-old Black female identical twins.) The problems ranged from plotting the data (how can you show who is the twin of whom?) to either non-independence of the observations if you treat "n" as 14 or the loss of important information if you sample one member of each pair for the analysis. 'tis a difficult situation to cope with methodologically. Here are the data. How would you proceed, dear reader (as Ann Landers used to say)?

Pair	Heights (X) in inches		Weights (Y) in pounds	
1 (Aa)	A: 68	a: 67	A: 148	a: 137
2 (Bb)	B: 65	b: 67	B: 124	b: 126
3 (Cc)	C: 63	c: 63	C: 118	c: 126
4 (Dd)	D: 66	d: 64	D: 131	d: 120
5 (Ee)	E: 66	e: 65	E: 123	e: 124
6 (Ff)	F: 62	f: 63	F: 119	f: 130
7(Gg)	G: 66	g: 66	G: 114	g: 104

Other good sources for research on twins and about twins in general

1. Kenny (2008). In his discussion of dyads and the analysis of dyadic data, David Kenny treats the case of twins as well as other dyads (supervisor-supervisee pairs, father-daughter pairs, etc.) The dyad should be the unit of

analysis (individual is "nested" within dyad); otherwise (and all too frequently) the observations are not independent and the analysis can produce very misleading results.

2. Kenny (2010). In this later discussion of the unit-of analysis problem, Kenny does not have a separate section on twins but he does have an example of children nested within classrooms and classrooms nested within schools, which is analogous to persons nested within twin-pairs and twin-pairs nested within families.

3. Rushton & Osborne (1995). In a follow-up article to Osborne's 1980 book, Rushton and Osborne used the same dataset for a sample of 236 twin-pairs (some male, some female; some Black, some White; some identical, some fraternal; all ranged in age from 12 to 18 years) to investigate the prediction of cranial capacity.

4. Segal (2011). In this piece Dr. Nancy Segal excoriates the author of a previous article for his misunderstandings of the results of twin research.

5. Twinsburg, Ohio. There is a Twins Festival held every August in this small town. Just google Twinsburg and you can get a lot of interesting information, pictures, etc. about twins and other multiples who attend those festivals

Note: The picture at the beginning of this paper is of the Bryan twins. To quote from the Wikipedia article about them:

"**The Bryan brothers** are identical twin brothers Robert Charles "Bob" Bryan and Michael Carl "Mike" Bryan, American professional doubles tennis players. They were born on April 29, 1978, with Mike being the elder by two minutes. The Bryans have won multiple Olympic medals, including the gold in 2012 and have won more professional games, matches, tournaments and Grand Slams than any other pairing. They have held the World No. 1 doubles ranking jointly for 380 weeks (as of September 8, 2014), which is longer than anyone else in doubles history."

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CHAPTER 7: VALIDITY? RELIABILITY? DIFFERENT TERMINOLOGY ALTOGETHER?

Several years ago I wrote an article entitled “Validity, reliability, and neither” (Knapp, 1985) in which I discussed some researchers’ identifications of investigations as validity studies or reliability studies but which were actually neither. In what follows I pursue the matter of confusion regarding the terms “validity” and “reliability” and suggest the possibility of alternative terms for referring to the characteristics of measuring instruments. I am not the first person to recommend this. As long ago as 1936, Goodenough suggested that the term “reliability” be done away with entirely. Concerns about both “reliability” and “validity” have been expressed by Stallings & Gillmore (1971), Feinstein (1985, 1987), Suen (1988), Brown (1989), and many others.

The problems

The principal problem, as expressed so succintly by Ennis (1999), is that the word “reliability” as used in ordinary parlance is what measurement experts subsume under “validity”. (See also Feldt & Brennan, 1989.) For example, if a custodian falls asleep on the job every night, most laypeople would say that he(she) is unreliable, i.e., a poor custodian; whereas psychometricians would say that he(she) is perfectly reliable, i.e., a consistently poor custodian.

But there’s more. Even within the measurement community there are all kinds of disagreements regarding the meaning of validity. For example, some contend that the consequences of misuses of a measuring instrument should be taken into account when evaluating its validity; others disagree. (Pro: Messick, 1995, and others; Anti: Lees-Haley, 1996, and others.) And there is the associated problem of the awful (in my opinion) terms “internal validity” and “external validity” that have little or nothing to do with the concept of validity in the measurement sense, since they apply to the characteristics of a study or its design and not to the properties of the instrument(s) used in the study. [“Internal validity” is synonymous with “causality” and “external validity” is synonymous with “generalizability.” ‘nuff said.]

The situation is even worse with respect to reliability. In addition to matters such as the (un?)reliable custodian, there are the competing definitions of the term “reliability” within the field of statistics in general (a sample statistic is reliable if it has a tight sampling distribution with respect to its counterpart population parameter) and within engineering (a piece of equipment is reliable if there is a small probability of its breaking down while in use). Some people have even talked about the reliability of a study. For example, an article I recently came across on the internet claimed that a study of the reliability (in the engineering sense) of various laptop computers was unreliable, and so was its report!

Some changes in, or retentions of, terminology and the reasons for same

There have been many thoughtful and some not so thoughtful recommendations regarding change in terminology. Here are a few of the thoughtful ones:

1. I've already mentioned Goodenough (1936). She was bothered by the fact that the test-retest reliability of examinations (same form or parallel forms) administered a day or two apart are almost always lower than the split-halves reliability of those forms when stepped up by the Spearman-Brown formula, despite the fact that both approaches are concerned with estimating the reliability of the instruments. She suggested that the use of the term "reliability" be relegated to "the limbo of outworn concepts" (p. 107) and that results of psychometric investigations be expressed in terms of whatever procedures were used in estimating the properties of the instruments in question.

2. Adams (1936). In that same year he tried to sort out the distinctions among the usages of the terms "validity", "reliability", and "objectivity" in the measurement literature of the time. [Objectivity is usually regarded as a special kind of reliability: "inter-rater reliability" if more than one person is making the judgments; "intra-rater reliability" for a single judge.] He found the situation to be chaotic and argued that validity, reliability, and objectivity are qualities of measuring instruments (which he called "scales"). He suggested that "accuracy" should be added as a term to refer to the quantitative aspects of test scores.

3. Thorndike (1951), Stanley (1971), Feldt and Brennan (1989), and Haertel (2006). They are the authors of the chapter on reliability in the various editions of the Educational Measurement compendium. Although they all commented upon various terminological problems, they were apparently content to keep the term "reliability" as is [judging from the retention of the single word "Reliability" in the chapter title in each of the four editions of the book].

4. Cureton (1951), Cronbach (1971), Messick (1989), and Kane (2006). They were the authors of the corresponding chapters on validity in Educational Measurement. They too were concerned about some of the terminological confusion regarding validity [and the chapter titles went from "Validity" to "Test Validation" back to "Validity" and thence to "Validation", in that chronological order], but the emphasis changed from various types of validity in the first two editions to an amalgam under the heading of Construct Validity in the last two.

5. Ennis (1999). I've already referred to his clear perception of the principal problem with the term "reliability". He suggested the replacement of "reliability" with "consistency". He was also concerned about the terms "true score" and "error of measurement". [More about those later.]

6. AERA, APA, and NCME Standards (2014). The titles of the two sections are "Validity" and "Errors of Measurement and Reliability/Precision", respectively.

Like the authors of the chapters in the various editions of Educational Measurement, the authors of the sections on validity express some concerns about confusions in terminology, but they appear to want to stick with “validity”, whereas the authors of the section on reliability prefer to expand the term “reliability”. [In the previous (1999) version of the Standards the title was “Reliability and Errors of Measurement”.]

My personal recommendations

1. I prefer “relevance” to “validity”, especially given my opposition to the terms “internal validity” and “external validity”. I realize that “relevance” is a word that is over-used in the English language, but what could be a better measuring instrument than one that is completely relevant to the purpose at hand? Examples: a road test for measuring the ability to drive a car; a stadiometer for measuring height; and a test of arithmetic items all of the form a + b = ___ for measuring the ability to add.

2. I’m mostly with Ennis (1999) regarding changing “reliability” to “consistency”, even though in my unpublished book on the reliability of measuring instruments (Knapp, 2015) I come down in favor of keeping it “reliability”. [Ennis had nothing to say one way or the other about changing “validity” to something else.]

3. I don’t like to lump techniques such as Cronbach’s alpha under either “reliability” or “consistency”. For those I prefer the term “homogeneity”, as did Kelley (1942); see Traub (1997). I suggest that time must pass (even if just a few minutes—see Horst, 1954) between the measure and the re-measure.

4, I also don’t like to subsume “objectivity” under “reliability” (either inter-rater or intra-rater). Keep it as “objectivity”.

5. Two terms I recommend for Goodenough’s limbo are “accuracy” and “precision”, at least as far as measurement is concerned. The former term is too ambiguous. [How can you ever determine whether or not something is accurate?] The latter term should be confined to the number of digits that are defensible to report when making a measurement.

True score and error of measurement

As I indicated above, Ennis (1999) doesn’t like the terms “true score” and “error of measurement”. Both terms are used in the context of reliability. The former refers to (1) the score that would be obtained if there were no unreliability; and (2) the average (arithmetic mean) of all of the possible obtained scores for an individual. The latter is the difference between an obtained score and the corresponding true score. What bothers Ennis is that the term “true score” would seem to indicate the score that was actually deserved in a perfectly valid test, whereas the term is associated only with reliability.

I don't mind keeping both "true score" and "error of measurement" under "consistency", as long as there is no implication that the measuring instrument is also necessarily "relevant". The instrument chosen to provide an operationalization of a particular attribute such as height or the ability to add or to drive a car might be a lousy one (that's primarily a judgment call), but it always needs to produce a tight distribution of errors for any given individual.

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CHAPTER 8: SEVEN: A COMMENTARY REGARDING CRONBACH'S COEFFICIENT ALPHA

A population of seven people took a seven-item test, for which each item is scored on a seven-point scale. Here are the raw data:

ID	item1	item2	item3	item4	item5	item6	item7	total
1	1	1	1	1	1	1	1	7
2	2	2	2	2	2	3	3	16
3	3	4	6	7	7	4	5	36
4	4	7	5	3	5	7	6	37
5	5	6	4	6	4	5	2	32
6	6	5	7	5	3	2	7	35
7	7	3	3	4	6	6	4	33

Here are the inter-item correlations and the correlations between each of the items and the total score:

	item1	item2	item3	item4	item5	item6	item7
item2	0.500						
item3	0.500	0.714					
item4	0.500	0.536	0.750				
item5	0.500	0.464	0.536	0.714			
item6	0.500	0.643	0.214	0.286	0.714		
item7	0.500	0.571	0.857	0.393	0.464	0.286	
total	0.739	0.818	0.845	0.772	0.812	0.673	0.752

The mean of each of the items is 4 and the standard deviation is 2 (with division by N, not N-1; these are data for a population of people as well as a population of items). The inter-item correlations range from .214 to .857 with a mean of .531. [The largest eigenvalue is 4.207. The next largest is 1.086.] The range of the item-to-total correlations is from .673 to .845. Cronbach's alpha is .888. Great test (at least as far as internal consistency is concerned)? Perhaps; but there is at least one problem. See if you can guess what that is before you read on.

While you're contemplating, let me call your attention to seven interesting sources that discuss Cronbach's alpha (see References for complete citations):

1. Cronbach's (1951) original article (naturally).
2. Knapp (1991).
3. Cortina (1993).
4. Cronbach (2004).
5. Tan (2009).
6. Sijtsma (2009).
7. Gadermann, Guhn, and Zumbo (2012).

OK. Now back to our data set. You might have already suspected that the data are artificial (all of the items having exactly the same means and standard deviations, and all of items 2-7 correlating .500 with item 1). You're right; they are; but that's not what I had in mind. You might also be concerned about the seven-point scales (ordinal rather than interval?). Since the data are artificial, those scales can be anything we want them to be. If they are Likert-type scales they are ordinal. But they could be something like "number of days per week" that something happened, in which case they are interval. In any event, that's also not what I had in mind. You might be bothered by the negative skewness of the total score distribution. I don't think that should matter. And you might not like the smallness (and the "seven-ness"? I like sevens…thus the title of this chapter) of the number of observations. Once the correlation matrix has been determined, the N is not of direct relevance. (The "software" doesn't know or care what N is at that point.) Had this been a sample data set, however, and had we been interested in the statistical inference from a sample Cronbach's alpha to the Cronbach's alpha in the population from which the sample has been drawn, the N would be of great importance.

What concerns me is the following:

The formula for Cronbach's alpha is kr_{avg} /[1 + (k-1)r_{avg}], where k is the number of items and r_{avg} is the average (mean) inter-item correlation, when all of the items have equal variances (which they do in this case) and is often a good approximation to Cronbach's alpha even when they don't. (More about this later.) Those r's are Pearson r's, which are measures of the direction and magnitude of the LINEAR relationship between variables. Are the relationships linear?

I have plotted the data for each of the items against the other items. There are 21 plots (the number of combinations of seven things taken two at a time). Here is the first one.

```
        -
item2   -                        *
        -
        -
     6.0+                              *
        -
        -                                  *
        -
        -
     4.0+                 *
        -
        -                                         *
        -
        -
     2.0+           *
        -
        -  *
        -
        -
        ----+---------+---------+---------+---------+---------+--item1
           1.2       2.4       3.6       4.8       6.0       7.2
```

I don’t know about you, but that plot looks non-linear, almost parabolic, to me, even though the linear Pearson r is .500. Is it because of the artificiality of the data, you might ask. I don’t think so. Here is a set of real data (item scores that I have excerpted from my daughter Katie’s thesis (Knapp, 2010)): [They are the responses by seven female chaplains in the Army Reserves to the first seven items of a 20-item test of empathy.]

ID	item1	item2	item3	item4	item5	item6	item7	total
1	5	7	6	6	6	6	6	42
2	1	7	7	5	7	7	7	41
3	6	7	6	6	6	6	6	43
4	7	7	7	6	7	7	6	47
5	2	6	6	6	7	6	5	38
6	1	1	3	4	5	6	5	25
7	2	5	3	6	7	6	6	35

Here are the inter-item correlations and the correlation of each item with the total score:

	item1	item2	item3	item4	item5	item6	item7
item2	0.566						
item3	0.492	0.826					
item4	0.616	0.779	0.405				
item5	0.060	0.656	0.458	0.615			
item6	0.156	0.397	0.625	-0.062	0.496		
item7	0.138	0.623	0.482	0.175	0.439	0.636	
total	0.744	0.954	0.855	0.746	0.590	0.506	0.566

Except for the -.062 these correlations look a lot like the correlations for the artificial data. The inter-item correlations range from that -.062 to .826, with a mean of .456. [The largest eigenvalue is 3.835 and the next-largest eigenvalue is 1.479] The item-to-total correlations range from .506 to .954. Cronbach's alpha is .854. Another great test?

But how about linearity? Here is the plot for item2 against item1 for the real data.

```
        -
  item2 -     *                            *      *      *
        -
        -
     6.0+           *
        -
        -         *
        -
        -
     4.0+
        -

        -
        -
        -
     2.0+
        -
        -  *
        -
        -
         ----+---------+---------+---------+---------+---------+--item1
            1.2       2.4       3.6       4.8       6.0       7.2
```

That’s a worse, non-linear plot than the plot for the artificial data, even though the linear Pearson r is a respectable .566.

Going back to the formula for Cronbach’s alpha that is expressed in terms of the inter-item correlations, it is not the most general formula. Nor is it the one that Cronbach generalized from the Kuder-Richardson Formula #20 (Kuder & Richardson, 1937) for dichotomously-scored items. The formula that always “works” is: $\alpha = [k/(k-1)]\{1-(\sum\sigma_i{}^2/\sigma^2)\}$, where k is the number of items, $\sigma_i{}^2$ is the variance of item i (for i=1,2,…,k) and σ^2 is the variance of the total scores. For the artificial data, that formula yields the same value for Cronbach’s alpha as before, i.e., .888, but for the real data it yields a value of .748, which is lower than the .854 previously obtained. That happens because the item variances are not equal, ranging from a low of .204 (for item #6) to a high of 5.387 (for item #1). The item variances for the artificial data were all equal to 4.

So what? Although the most general formula was derived in terms of inter-item covariances rather than inter-item correlations, there is still the (hidden?) assumption of linearity.

The moral to the story is the usual advice given to people who use Pearson r’s: ALWAYS PLOT THE DATA FIRST. If the inter-item plots don’t look linear, you might want to forgo Cronbach’s alpha in favor of some other measure, e.g., the ordinal reliability coefficient advocated by Gadermann, et al. (2012). There are tests of linearity for sample data, but this chapter is concerned solely with the internal consistency of a measuring instrument when data are available for an entire population of people and an entire population of items (however rare that situation might be).

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CHAPTER 9: ASSESSING THE VALIDITY AND RELIABILITY OF LIKERT SCALES AND VISUAL ANALOG SCALES

Introduction

Consider the following scales for measuring pain:

It hurts:	Strongly disagree	Disagree	Can't tell	Agree	Strongly agree
	(1)	(2)	(3)	(4)	(5)

How bad is the pain?: ____________________________________
no pain   excruciating

How much would you be willing to pay in order to alleviate the pain?______

The first two examples, or slight variations thereof, are used a lot in research on pain. The third is not. In what follows I would like to discuss how one might go about assessing (testing, determining) the validity and the reliability of measuring instruments of the first kind (a traditional Likert Scale [LS]) and measuring instruments of the second kind (a traditional Visual Analog Scale [VAS]) for measuring the presence or severity of pain and for measuring some other constructs. I will close the paper with a few brief remarks regarding the third example and how its validity and reliability might be assessed.

The sequence of steps

1. Although you might not agree, I think you should start out by addressing content validity (expert judgment, if you will) as you contemplate how you would like to measure pain (or attitude toward legalizing marijuana, or whatever the construct of interest might be). If a Likert-type scale seems to make sense to you, do the pain experts also think so? If they do, how many scale points should you have? Five, as in the above example, and as was the case for the original scale developed by Rensis Likert (1932)? Why an odd number such as five? In order to provide a "neutral", or "no opinion" choice? Might not too many respondents cop out by selecting that choice? Shouldn't you have an even number of scale points (how about just two?) so that respondents have to take a stand one way or the other?

The same sorts of considerations hold for the "more continuous" VAS, originally developed by Freyd (1923). (He called it a Graphic Rating Scale. Unlike Likert, his name was not attached to it by subsequent users. Sad.) How long should it be? (100 millimeters is conventional.) How should the endpoints read? Should there be intermediate descriptors underneath the scale between the two endpoints? Should it be presented to the respondents horizontally (as above) or vertically? Why might that matter?

2. After you are reasonably satisfied with your choice of scale type (LS or VAS) and its specific properties, you should carry out some sort of pilot study in which you gather evidence regarding feasibility (how willing and capable are subjects to respond?), "face" validity (does it appear to them to be measuring pain, attitude toward marijuana, or whatever?), and tentative reliability (administer it twice to the same sample of people, with a small amount of time in-between administrations, say 30 minutes or thereabouts). This step is crucial in order to "get the bugs out" of the instrument before its further use. But the actual results, e.g., whether the pilot subjects express high pain or low pain, favorable attitudes or unfavorable attitudes, etc., should be of little or no interest, and certainly do not warrant publication.

3. If and when any revisions are made on the basis of the pilot study, the next step is the most difficult. It entails getting hard data regarding the reliability and/or the validity of the LS or the VAS. For a random sample drawn from the same population from which a sample will be drawn in the main study, a formal test-retest assessment should be carried out (again with a short interval between test and retest), and if there exists an instrument that serves as a "gold standard" it should also be administered and the results compared with the scale that is under consideration.

Likert Scales

As far as the reliability of a LS is concerned, you might be interested in evidence for either or both of the scale's "relative reliability" and its "absolute reliability". The former is more conventional; just get the correlation between score at Time 1 and score at Time 2. Ah, but what particular correlation? The Pearson product-moment correlation coefficient? Probably not; it is appropriate only for interval-level scales. (The LS is an ordinal scale.) You could construct a cxc contingency table, where c is the number of categories (scale points) and see if most of the frequencies lie in the upper-right and lower-left portions of the table. That would require a large number of respondents if c is more than 3 or so, in order to "fill up" the c^2 cells; otherwise the table would look rather anemic. If further summary of the results is thought to be necessary, either Guttman's (1946) reliability coefficient or Goodman & Kruskal's (1979) gamma (sometimes called the index of order association) would be good choices for such a table, and would serve as the reliability coefficient (for that sample on that occasion). If the number of observations is fairly small and c is fairly large, you could calculate the Spearman rank correlation between score at Time 1 and score at Time 2, since you shouldn't have too many ties, which can often wreak havoc.

[Exercise for the reader: When using the Spearman rank correlation in determining the relationship between two ordinal variables X and Y, we get the difference between the rank on X and the rank on Y for each observation. For ordinal variables in general, subtraction is a "no-no". (You can't subtract a "strongly agree" from an "undecided", for example.) Shouldn't a rank-difference also be a "no-no"? I think it should, but people do it all the time, especially when they're concerned about whether or not a particular variable is continuous enough, linear enough, or normal enough in order for the Pearson r to be defensible.]

The matter of absolute reliability is easier to assess. Just calculate the % agreement between score at Time 1 and score at Time 2.

If there is a gold standard to which you would like to compare the scale under consideration, the (relative) correlation between scale and standard (a validity coefficient) needs to be calculated. The choice of type of validity coefficient, like the choice of type of reliability coefficient, is difficult. It all depends upon the scale type of the standard. If it is also ordinal, with d scale points, a cxd table would display the data nicely, and Goodman & Kruskal's gamma could serve as the validity coefficient (again, for that sample on that occasion). (N.B.: If a gold standard does exist, serious thought should be given to forgoing the new instrument entirely, unless the LS or VAS under consideration would be briefer or less expensive, but equally reliable and content valid.)

Visual Analog Scales

The process for the assessment of the reliability and validity of a VAS is essentially the same as that for a LS. As indicated above, the principal difference between the two is that a VAS is "more continuous" than a LS, but neither possesses a meaningful unit of measurement. For a VAS there is a surrogate unit of measurement (usually the millimeter), but it wouldn't make any sense to say that a particular patient has X millimeters of pain. (Would it?) For a LS you can't even say 1 what or 2 what,..., since there isn't a surrogate unit.

Having to treat a VAS as an ordinal scale is admittedly disappointing, particularly if it necessitates slicing up the scale into two or more (but not 101) pieces and losing some potentially important information. But let's face it. Most respondents will probably concentrate on the verbal descriptors along the bottom of the scale anyhow, so why not help them along? (If there are no descriptors except for the endpoints, you might consider collapsing the scale into those two categories.)

Statistical inference

For the sample selected for the LS or VAS reliability and validity study, should you carry out a significance test for the reliability coefficient and the validity coefficient? Certainly not a traditional test of the null hypothesis of a zero relationship. Whether or not a reliability or a validity coefficient is significantly greater than zero is not the point (they darn well better be). You might want to test a "null" hypothesis of a specific non-zero relationship (e.g., one that has been found for some relevant norm group), but the better analysis strategy would be to put a confidence interval around the sample reliability coefficient and the sample validity coefficient. (If you have a non-random sample it should be treated just like a population, i.e., descriptive statistics only.)

The article by Kraemer (1975) explains how to test a hypothesis about, and how to construct a confidence interval for, the Spearman rank correlation coefficient, rho. A similar article by Woods (2007; corrected in 2008) treats estimation for both Spearman's rho and Goodman & Kruskal's gamma. That would take care of Likert Scales nicely. If the raw data for Visual Analog Scales are converted into either ranks or ordered categories, inferences regarding their reliability and validity coefficients could be handled in the same manner.

Combining scores on Likert Scales and Visual Analog Scales

The preceding discussion was concerned with a single-item LS or VAS. Many researchers are interested in combining scores on two or more of such scales in order to get a "total score". (Some people argue that it is also important to distinguish between a Likert item and a Likert scale, with the latter consisting of a composite of two or more of the former. I disagree; a single Likert item is itself a scale; so is a single VAS.) The problems involved in assessing the validity and reliability of such scores are several magnitudes more difficult than for assessing the validity and reliability of a single LS or a single VAS.

Consider first the case of two Likert-type items, e.g., the following:

The use of marijuana for non-medicinal purposes is widespread.

Strongly Disagree	Disagree	Undecided	Agree	Strongly Agree
(1)	(2)	(3)	(4)	(5)

The use of marijuana for non-medicinal purposes should be legalized.

Strongly Disagree	Disagree	Undecided	Agree	Strongly Agree
(1)	(2)	(3)	(4)	(5)

All combinations of responses are possible and undoubtedly likely. A respondent could disagree, for example, that such use is widespread, but agree that it should be legalized. Another respondent might agree that such use is widespread, but disagree that is should be legalized. How to combine the responses to those two items in order to get a total score? See next paragraph. (Note: Some people, e.g., some "conservative" statisticians, would argue that scores on those two items should never be combined; they should always be analyzed as two separate items.)

The usual way the scores are combined is to merely add the score on Item 1 to the score on Item 2, and in the process of so doing to "reverse score", if and when necessary, so that "high" total scores are indicative of an over-all favorable attitude and "low" total scores are indicative of an over-all unfavorable attitude. The respondent who chose "2" (disagree) for Item 1 and "4" (agree) for Item 2 would get a total score of 4 (i.e., a "reversed" 2) + 4 (i.e., a "regular" 4) = 8, since he(she) appears to hold a generally favorable attitude toward marijuana use. But would you like to treat that respondent the same as a respondent who chose "5" for the first item and "3" for the second item? They both would get a total score of 8. See how complicated this is? Hold on; it gets even worse!

Suppose you now have total scores for all respondents. How do you summarize the data? The usual way is to start by making a frequency distribution of those total scores. That should be fairly straightforward. Scores can range from 2 to 10, whether or not there is any reverse-scoring (do you see why?), so an "ungrouped" frequency distribution should give you a pretty good idea of what's going on. But if you want to summarize the data even further, e.g., by getting measures of central tendency, variability, skewness, and kurtosis, you have some tough choices to make. For example, is it the mean, the median, or the mode that is the most appropriate measure of central tendency for such data? The mean is the most conventional, but should be reserved for interval scales and for scales that have an actual unit of measurement. (Individual Likert scales and combinations of Likert scales are neither: Ordinal in, ordinal out.) The median should therefore be fine, although with an even number of respondents that can get tricky (for example, would you really like to report a median of something like 6.5 for this marijuana example?).

Getting an indication of the variability of those total scores is unbelievably technically complicated. Both variance and standard deviation should be ruled out because of non-intervality. (If you insist on one or both of those, what do you use in the denominator of the formula... n or n-1?) How about the range (the actual range, not the possible range)? No, because of the same non-intervality property. All other measures of variability that involve subtraction are also ruled out. That leaves "eyeballing" the frequency distribution for variability, which is not a bad idea, come to think of it.

I won't even get into problems involved in assessing skewness and kurtosis, which should probably be restricted to interval-level variables in any event. (You can "eyeball" the frequency distribution for those characteristics just like you can for variability, which also isn't a bad idea.)

The disadvantages of combining scores on two VASs are the same as those for combining scores on two LSs. And for three or more items things don't get any better.

What some others have to say about the validity and the reliability of a LS or VAS

The foregoing (do you know the difference between "forgoing" and "foregoing"?) discussion consists largely of my own personal opinions. (You probably already have me pegged, correctly, as a "conservative" statistician.) Before I turn to my most controversial suggestion of replacing almost all Likert Scales and almost all Visual Analog Scales with interval scales, I would like to call your attention to authors who have written about how to assess the reliability and/or the validity of a LS or a VAS, or who have reported their reliabilities or validities in substantive investigations. Some of their views are similar to mine. Others are diametrically opposed.

1. Aitken (1969)

According to Google, this "old" article has been cited 1196 times! It's that good, and has a brief but excellent section on the reliability and validity of a VAS. (But it is very hard to get a hold of. Thank God for helpful librarians like Kathy McGowan and Shirley Ricker at the University of Rochester.)

2. Price, et al. (1983).

As the title of their article indicates, Price, et al. claim that in their study they have found the VAS to be not only valid for measuring pain but also a ratio-level variable. (I don't agree. But read the article and see what you think.)

3. Wewers and Lowe (1990)

This is a very nice summary of just about everything you might want to know concerning the VAS, written by two of my former colleagues at Ohio State (Mary Ellen Wewers and Nancy Lowe). There are fine sections on assessing the reliability and the validity of a VAS. They don't care much for the test-retest approach to the assessment of the reliability of a VAS, but I think that is really the only option. The parallel forms approach is not viable (what constitutes a parallel item to a given single-item VAS?) and things like Cronbach's alpha are no good because they require multiple items that are gathered together in a composite. It comes down to a matter of the amount of time between test and retest. It must be short enough so that the construct being measured hasn't changed, but it must be long enough so that the respondents don't merely "parrot back" at Time 2 whatever they indicated at Time 1; i.e., it must be a "Goldilocks" interval.

4. Von Korff, et al. (1993)

These authors developed what they call a "Quadruple Visual Analog Scale" for measuring pain. It consists of four items, each having "No pain " and "worst possible pain" as the two endpoints, with the numbers 0 through 10 equally spaced beneath each item. The respondents are asked to indicate the amount of pain (1) now, (2) typical, (3) best, and (4) worst; and then to add across the four items. Interesting, but wrong (in my opinion).

5. Bijur, Silver, and Gallagher (2001)

This article was a report of an actual test-retest (and re-retest...) reliability study of the VAS for measuring acute pain. Respondents were asked to record their pain levels in pairs one minute apart thirty times in a two-hour period. The authors found the VAS to be highly reliable. (Not surprising. If I were asked 60 times in two hours to indicate how much pain I had, I would pick a spot on the VAS and keep repeating it, just to get rid of the researchers!)

6. Owen and Froman (2005)

Although the main purpose of their article was to dissuade researchers from unnecessarily collapsing a continuous scale (especially age) into two or more discrete categories, the authors made some interesting comments regarding Likert Scales. Here are a couple of them:

"...equal appearing interval measurements (e.g., Likert-type scales...)" (p. 496)

"There is little improvement to be gained from trying to increase the response format from seven or nine options to, say, 100. Individual items usually lack adequate reliability, and widening the response format gives an appearance of greater precision, but in truth does not boost the item’s reliability... However, when individual items are aggregated to a total (sum or mean) scale score, the continuous score that results usually delivers far greater precision." (p. 499)

A Likert scale might be an "equal appearing interval measurement", but it's not interval-level. And I agree with the first part of the second quote (it sounds like a dig at Visual Analog Scales), but not with the second part. Adding across ordinal items does not result in a defensible continuous score. As the old adage goes, "you can't make a silk purse out of a sow's ear".

7. Davey, et al. (2007)

There is a misconception in the measurement literature that a single item is necessarily unreliable and invalid. Not so, as Davey, et al. found in their use of a one-item LS and a one-item VAS to measure anxiety. Both were found to be reliable and valid. (Nice study.)

8. Hawker, et al. (2011)

This article is a general review of pain scales in general. The first part of the article is devoted to the VAS (which the authors call "a continuous scale"; ouch!). They have this to say about its reliability and validity:

"Reliability. Test–retest reliability has been shown to be good, but higher among literate (r = 0.94, P< 0.001) than illiterate patients (r= 0.71, P < 0.001) before and after attending a rheumatology outpatient clinic [citation].

Validity. In the absence of a gold standard for pain, criterion validity cannot be evaluated. For construct validity, in patients with a variety of rheumatic diseases, the pain VAS has been shown to be highly correlated with a 5-point verbal descriptive scale ("nil", "mild", "moderate", "severe", and "very severe") and a numeric rating scale (with response options from "no pain" to "unbearable pain"), with correlations ranging from 0.71–0.78 and.0.62–0.91, respectively) [citation]. The correlation between vertical and horizontal orientations of the VAS is 0.99 [citation] " (page s241)

That's a lot of information packed into two short paragraphs. One study doesn't make for a thorough evaluation of the reliability of a VAS; and as I have indicated above, those significance tests aren't appropriate. The claim about the absence of a gold standard is probably warranted. But I find a correlation of .99 between a vertical VAS and a horizontal VAS hard to believe. (Same people at the same sitting? You can look up the reference if you care.)

9. Vautier (2011)

Although it starts out with some fine comments about basic considerations for the use of the VAS, Vautier's article is a very technical discussion of multiple Visual Analog Scales used for the determination of reliability and construct validity in the measurement of change. The references that are cited are excellent.

10. Franchignoni, Salaffi, and Tesio (2012)

This recent article is a very negative critique of the VAS. Example: "The VAS appears to be a very simple metric ruler, but in fact it's not a true linear ruler from either a pragmatic or a theoretical standpoint. " (page 798). (Right on!) In a couple of indirect references to validity, the authors go on to argue that most people can't discriminate among the 101 possible points for a VAS. They cite Miller's (1956) famous 7 + or - 2 rule), and they compare the VAS unfavorably with a 7-pont Likert scale.

<u>Are Likert Scales and Visual Analog Scales really different from one another?</u>

In the previous paragraph I referred to 101 points for a VAS and 7 points for an LS. The two approaches differ methodologically only in the number of points (choices, categories) from which a respondent makes a selection. There are Visual Analog Scales that aren't really visual, and there are Likert Scales that are very visual. An example of the former is the second scale at the beginning of this paper. The only thing "visual" about that is the 100-millimeter line. As examples of the latter, consider the pictorial Oucher (Beyer, et al., 2005) and the pictorial

Defense and Veterans Pain Rating Scale (Pain Management Task Force, 2010) which consist of photographs of faces of children (Beyer) or drawings of soldiers (Pain Management Task Force) expressing varying degrees of pain. The Oucher has six scale points (pictures) and the DVPRS has six pictures super-imposed upon 11 scale points, with the zero picture indicating "no pain", the next two pictures associated with mild pain, the fourth associated with moderate pain, and the last two associated with severe pain. Both instruments are actually amalgams of Likert-type scales and Visual Analog Scales.

I once had the pleasant experience of co-authoring an article about the Oucher with Judy Beyer. (Our article is cited in theirs.) The instrument now exists in parallel forms for each of four ethnic groups.

Back to the third item at the beginning of this paper

I am not an economist. I took only the introductory course in college, but I was fortunate to have held a bridging fellowship to the program in Public Policy at the University of Rochester when I was a faculty member there, and I find the way economists look at measurement and statistics problems to be fascinating. (Economics is actually not the study of supply and demand. It is the study of the optimization of utility, subject to budget constraints.)

What has all of that to do with Item #3? Plenty. If you are serious about measuring amount of pain, strength of an attitude, or any other such construct, try to do it in a financial context. The dollar is a great unit of measurement. And how would you assess the reliability and validity? Easy; use Pearson r for both. You might have to make a transformation if the scatter plot between test scores and retest scores, or between scores on the scale and scores on the gold standard, is non-linear, but that's a small price to pay for a higher level of measurement.

Afterthought

Oh, I forgot three other sources. If you're seriously interested in understanding levels of measurement you must start with the classic article by Stevens (1946). Next, you need to read Marcus-Roberts and Roberts (1987) regarding why traditional statistics are inappropriate for ordinal scales. Finally, turn to Agresti (2010). This fine book contains all you'll ever need to know about handling ordinal scales. Agresti says little or nothing about validity and reliability per se, but since most measures of those characteristics involve correlation coefficients of some sort, his suggestions for determining relationships between two ordinal variables should be followed.

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CHAPTER 10: RATING, RANKING, OR BOTH?

Suppose you wanted to make your own personal evaluations of three different flavors of ice cream: chocolate, vanilla, and strawberry. How would you go about doing that? Would you rate each of them on a scale, say from 1 to 9 (where 1 = awful and 9 = wonderful)? Or would you assign rank 1 to the flavor you like best, rank 2 to the next best, and rank 3 to the third? Or would you do both?

What follows is a discussion of the general problem of ratings vs. rankings, when you might use one rather than the other, and when you might want to use both.

Terminology and notation

Rating k things on a scale from 1 to w, where w is some convenient positive integer, is sometimes called "interactive" measurement. Ranking k things from 1 to k is often referred to as "ipsative" measurement. (See Cattell, 1944 or Knapp, 1966 for explanations of those terms.) The number of people doing the rating or the ranking can be denoted by n.

Advantages and disadvantages of each

Let's go back to the ice cream example, with k = 3, w = 9, and have n = 2 (A and B, where you are A?). You would like to compare A's evaluations with B's evaluations. Sound simple? Maybe; but here are some considerations to keep in mind:

1. Suppose A gives ratings of 1, 5, and 9 to chocolate, vanilla, and strawberry, respectively; and B gives ratings of 5, 5, and 5, again respectively. Do they agree? Yes and no. A's average (mean) rating is the same as B's, but A's ratings vary considerably more than B's. There is also the controversial matter of whether or not arithmetic means are even relevant for scales such as this 9-point Likert-type ordinal scale. (I have written two papers on the topic...Knapp,1990 and Knapp, 1993; but the article by Marcus-Roberts & Roberts, 1987, is by far the best, in my opinion.)

2. Suppose A gives chocolate rank 1, vanilla rank 2, and strawberry rank 3. Suppose that B does also. Do they agree? Again, yes and no. The three flavors are in exactly the same rank order, but A might like all of them a lot and was forced to discriminate among them; whereas B might not like any of them, but designated chocolate as the "least bad", with vanilla in the middle, and with strawberry the worst.

3. Reference was made above to the relevance of arithmetic means. If an analysis that is more complicated than merely comparing two means is contemplated, the situation can get quickly out of hand. For example, suppose that k = 31 (Baskin-Robbins' large number of flavors), w is still 9, but n is now 3

(you want to compare A's, B's, and C's evaluations). Having A, B, and C rate each of 31 things on a 9-point scale is doable, albeit tedious. Asking them to rank 31 things from 1 to 31 is an almost impossible task. (Where would they even start? How could they keep everything straight?) And comparing three evaluators is at least 1.5 times harder than comparing two.

Matters are even worse if sampling is involved. Suppose that you choose a random sample of 7 of the Baskin-Robbins 31 flavors and ask a random sample of 3 students out of a class of 50 students to do the rating or ranking, with the ultimate objective of generalizing to the population of flavors for the population of students. What descriptive statistics would you use to summarize the sample data? What inferential statistics would you use? Help!

A real example: Evaluating the presidents

Historians are always studying the accomplishments of the people who have served as presidents of the United States, starting with George Washington in 1789 and continuing up through whoever is presently in office. [At this writing, in 2016, Barack Obama is now serving his second four-year term.] It is also a popular pastime for non-historians to make similar evaluations.

Some prototypes of ratings and/or rankings of the various presidents by historical scholars are the works of the Schlesingers (1948, 1962, 1997), Lindgren (2000), Davis (2012), and Merry (2012). [The Wikipedia website cites and summarizes several others.] For the purpose of this example I have chosen the evaluations obtained by Lindgren for presidents from George Washington to Bill Clinton.

Table 1 contains all of the essential information in his study. [It is also his Table 1.] For this table, k (the number of presidents) is 39, w (the number of scale points for the ratings) is 5 (HIGHLY SUPERIOR=5, ABOVE AVERAGE=4, AVERAGE=3, BELOW AVERAGE=2, WELL BELOW AVERAGE=1), and n (the number of raters) is 1 (actually averaged across the ratings provided by 78 scholars; the ratings given by each of the scholars were not provided). The most interesting feature of the table is that it provides both ratings and rankings, with double ratings arising from the original scale and the subsequent tiers of "greatness". [Those presidents were first rated on the 5-point scale, then ranked from 1 to 39, then ascribed further ratings by the author on a 6-point scale of greatness (GREAT, NEAR GREAT, ABOVE AVERAGE, AVERAGE, BELOW AVERAGE, AND FAILURE. Three presidents, Washington, Lincoln, and Franklin Roosevelt are almost always said to be in the "GREAT" category.] Some presidents, e.g., William Henry Harrison and James Garfield, were not included in Lindgren's study because they served such a short time in office.

Table 1
Ranking of Presidents by Mean Score
Data Source: October 2000 Survey of Scholars in History, Politics, and Law
Co-Sponsors: Federalist Society & Wall Street Journal

	Mean	Median	Std. Dev.
Great			
1 George Washington	4.92	5	0.27
2 Abraham Lincoln	4.87	5	0.60
3 Franklin Roosevelt	4.67	5	0.75
Near Great			
4 Thomas Jefferson	4.25	4	0.71
5 Theodore Roosevelt	4.22	4	0.71
6 Andrew Jackson	3.99	4	0.79
7 Harry Truman	3.95	4	0.75
8 Ronald Reagan	3.81	4	1.08
9 Dwight Eisenhower	3.71	4	0.60
10 James Polk	3.70	4	0.80
11 Woodrow Wilson	3.68	4	1.09
Above Average			
12 Grover Cleveland	3.36	3	0.63
13 John Adams	3.36	3	0.80
14 William McKinley	3.33	3	0.62
15 James Madison	3.29	3	0.71
16 James Monroe	3.27	3	0.60
17 Lyndon Johnson	3.21	3.5	1.04
18 John Kennedy	3.17	3	0.73
Average			
19 William Taft	3.00	3	0.66
20 John Quincy Adams	2.93	3	0.76
21 George Bush	2.92	3	0.68
22 Rutherford Hayes	2.79	3	0.55
23 Martin Van Buren	2.77	3	0.61
24 William Clinton	2.77	3	1.11
25 Calvin Coolidge	2.71	3	0.97
26 Chester Arthur	2.71	3	0.56
Below Average			
27 Benjamin Harrison	2.62	3	0.54
28 Gerald Ford	2.59	3	0.61
29 Herbert Hoover	2.53	3	0.87
30 Jimmy Carter	2.47	2	0.75
31 Zachary Taylor	2.40	2	0.68

32 Ulysses Grant	2.28	2	0.89
33 Richard Nixon	2.22	2	1.07
34 John Tyler	2.03	2	0.72
35 Millard Fillmore	1.91	2	0.74
Failure			
36 Andrew Johnson	1.65	1	0.81
37T Franklin Pierce	1.58	1	0.68
37T Warren Harding	1.58	1	0.77
39 James Buchanan	1.33	1	0.62

One vs. both

From a purely practical perspective, ratings are usually easier to obtain and are often sufficient. The conversion to rankings is essentially automatic by putting the ratings in order. (See above regarding ranking large numbers of things "from scratch", without the benefit of prior ratings.) But there is always the bothersome matter of "ties". (Note the tie in Table 1 between Pierce and Harding for 37th place but, curiously, not between VanBuren and Clinton, or between Coolidge and Arthur.) Ties are equally problematic, however, when rankings are used.

Rankings are to be preferred when getting the correlation (not the difference) between two variables, e.g., A's rankings and B's rankings, whether the rankings are the only data or whether the rankings have been determined by ordering the ratings. That is because from a statistical standpoint the use of the Spearman rank correlation coefficient is almost always more defensible than the use of the Pearson product-moment correlation coefficient for ordinal data and for non-linear interval data.

It Is very unusual to see both ratings and rankings used for the same raw data, as was the case in the Lindgren study. It is rather nice, however, to have both "relative" (ranking) and "absolute" (rating) information for things being evaluated.

Other recommended reading

If you're interested in finding out more about rating vs. ranking, I suggest that in addition to the already-cited sources you read the article by Alwin and Krosnick (1985) and the measurement chapter in Richard Lowry's online statistics text.

A final remark

Although ratings are almost always made on an ordinal scale with no zero point, researchers should always try to see if it would be possible to use an interval scale or a ratio scale instead. For the ice cream example, rather than ask people to rate the flavors on a 9-point scale it might be better to ask how much they'd be willing to pay for a chocolate ice cream cone, a vanilla ice cream cone, and a strawberry ice cream cone. Economists often argue for the use of such "utils" when gathering consumer preference data. [Economics is usually called the study of supply and demand. "The study of the maximization of utility, subject to budget constraints" is more indicative of what it's all about.]

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CHAPTER 11: POLLS

"Poll" is a very strange word. It has several meanings. Before an election, e.g., for president of the United States, we conduct an opinion "poll" in which we ask people for whom they intend to vote. They then cast their ballots at a "polling" place, indicating for whom they actually did vote (that's what counts). Then after they emerge from the "polling" place we conduct an exit "poll" in which we ask them for whom they voted.

There are other less familiar definitions of "poll". One of them has nothing to do with elections or opinions: The 21st definition at Dictionary.com is "to cut short or cut off the hair, wool, etc., of (an animal); crop; clip; shear". And there is of course the distinction between "telephone poll" and its homonym "telephone pole"!

But the primary purpose of this chapter is not to explore the etymology of "poll". I would like to discuss the more interesting (to me, anyhow) matter of how the results of before-election opinion polling, votes at the polling places, and exit polling agree with one another.

Opinion polls

The most well-known opinion polls are those conducted by George Gallup and his colleagues. The most infamous poll (by Literary Digest) was conducted prior to the 1936 presidential election, in which Alfred Landon was projected to defeat Franklin Roosevelt, whereas Roosevelt won by a very wide margin. (A related goof was the headline in The Chicago Tribune the morning after the 1948 presidential election between Thomas E. Dewey and Harry S. Truman that proclaimed "DEWEY DEFEATS TRUMAN". Truman won, and he was pictured holding up a copy of that newspaper.)

Opinion polls should be, and sometimes but not always are, based upon a representative sample of the population to which the results are to be generalized. The best approach would be to draw what is called a stratified random sample whereby the population of interest, e.g., all registered voters in the U.S., is broken up into various "strata", e.g. by sex within state, with a simple random sample selected from each "stratum" and with the sample sizes proportional to the composition of the strata in the population. That is impossible, however, since there doesn't exist in any one place a "sampling frame" (list) of all registered voters. So for practical purposes the sampling is often "multi-stage cluster sampling" in which clusters, e.g., standard metropolitan statistical areas (SMSAs) are first sampled, with individuals subsequently sampled within each sampled cluster.

Some opinion polls use "quota sampling" rather than stratified random sampling. They are not the same thing. The former is a much weaker approach, since it lacks "randomness".

One of the most troublesome aspects of opinion polling is the matter of non-response, whether the sampling is random or not. It's one thing to sample a person; it's another thing to get him(her) to respond. The response rates for some of the most highly regarded opinion polls can be as low as 70 percent. The response rates for irreputable opinion polls are often as low as 15 or 20 percent.

One of the least troublesome aspects is sample size. The lay public find it hard to believe that a sample of, say, 2000 people, can possibly reflect the opinions of a population of 200,000,000 adults. There can always be sampling errors, but it is the size of the sample, not the size of the "bite" it takes out of the population, that is the principal determinant of its defensibility. In that respect, 2000 out of 200,000,000 ain't bad!

Actual voting at polling places

Every four years a sample of registered voters goes to a voting booth of some sort and casts votes for president of the United States. Unlike the best of opinion polls, however, the sample is always self-selected (nobody else determines who is in the sample and who is not). Furthermore, the votes cast are not associated with individual voters, and individual votes are never revealed (or at least are not supposed to be revealed, according to election laws).

[An aside: Political scientists cannot even study contingencies, e.g., of those who voted for Candidate A (vs. Candidate B) for president, what percentage voted for Candidate Y (vs. Candidate Z) for governor? If I were a political scientist I would find that to be very frustrating. But they have apparently accepted it and haven't done anything to challenge the voting laws.]

Exit polls

Immediately after an election, pollsters are eager to draw a sample of those who have just voted and to announce the findings before the actual votes are counted. (The latter can sometimes take days or even weeks.) As is the case for pre-election opinion polls, exit polls should be based upon a random sample of actual voters. But they often are not, and the response rates for such samples are even smaller than for pre-election opinion polls.

In addition to the over-all results, e.g., what percentage of exit poll respondents claimed to have voted for Candidate A, the results are often broken down by sex, age, and other demographic variables, in an attempt to determine how various groups voted the way they said they did.

Comparison of the results for opinion polls, actual votes, and exit polls

Under the circumstances, the best we can do for a presidential election is to compare, for the nation as a whole or for one or more subgroups, the percentage who said in a pre-election opinion poll that they were going to vote for Candidate A (the ultimate winner) with the percentage of people who actually voted for Candidate A and with the percentage of people who said in an exit poll that they had voted for Candidate A. But that is a very difficult statistical problem, primarily because the "bases" are usually very different. The opinion poll sample has been drawn (hopefully randomly) from a population of registered voters or likely voters; the actual voting sample has not been "drawn" at all, and the exit poll sample has been drawn (usually non-randomly) from a population of people who have just voted. As far as I know, nobody has ever carried out such a study, but some have come close. The remainder of this paper will be devoted to a few partially successful attempts.

Arnold Thomsen regarding Roosevelt and Landon before and after

In an article entitled "What Voters Think of Candidates Before and After Election" that appeared in The Public Opinion Quarterly in 1938, Thomsen wanted to see how people's opinions about Roosevelt and Landon differed before the 1936 election and after it had taken place. (Exit polls didn't exist then.) He collected data for a sample of 111 people (not randomly sampled) on three separate occasions (just before the election; the day after the election; and two weeks after the election). There was a lot of missing data, e.g., some people were willing to express their opinions about Roosevelt but not Landon, or vice versa. The results were very difficult to interpret, but at least he (Thomsen) tried.

Bev Harris regarding fraudulent touchscreen ballots

In a piece written on the AlterNet website a day after the 2004 presidential election, Thom Hartmann claimed that the exit polls showing John Kerry (the Democrat) defeating George W. Bush (the Republican) were right and the actual election tallies were wrong. He (Hartmann) referred to an analysis carried out by Bev Harris of blackboxvoting.org in which she claimed that the results for precincts in Florida that used paper ballots were more valid than touchscreen ballots and Kerry should have been declared the winner. Others disputed that claim. (As you might recall, the matter of who won Florida was adjudicated in the courts, with Bush declared the winner.) Both Hartmann and Harris argued that we should always use paper ballots as either the principal determinant or as back-up.

More on the 2004 presidential election

I downloaded from the internet the following excerpt by a blogger on November 6, 2004 (four days after the election): "Exit polling led most in the media to believe Kerry was headed to an easy victory. Exit polls were notoriously wrong in 2000 too -- that's why Florida was called incorrectly, too early.... Also, the exit polls were often just laughably inaccurate based on earlier normal polls of the states. Bush losing Pennsylvania 60-40 and New Hampshire 56-41? According to the exit polls, yes, but, um, sorry, no cookie for you. The race was neck and neck in both places as confirmed by a number of pre-election polls -- the exit poll is just wrong." Others claimed that the pre-election polls AND the exit polls were both right, but the actual tabulated results were fraudulent.

Analyses tabulated in Wikipedia

I copied the following excerpt from a Wikipedia entry entitled "Historical polling for U.S. Presidential elections"

United States presidential election, 2012

Month	2012	
	Barack Obama (D) %	Mitt Romney (R) %
April	45%	47%
	49%	43%
	46%	46%
May	44%	48%
	47%	46%
	45%	46%
June	47%	45%
	48%	43%
July	48%	44%
	47%	45%
	46%	46%
	46%	45%
August	47%	45%
	45%	47%
	47%	46%
September	49%	45%
	50%	43%
	50%	44%
October	50%	45%

	46%	49%
	48%	48%
	48%	47%
November	49%	46%
Actual result	51%	47%
Difference between actual result and final poll	+2%	+1%

That table shows for the presidential election in 2012 the over-all discrepancy between (an average of) pre-election opinion polls and the actual result as well as the trend for the months leading up to that election. In this case the findings were very close to one another.

<u>The whole story of Obama vs. Romney in 2012 as told in exit polls</u>

I couldn't resist copying into this paper the following entire piece from the New York Times website that I recently downloaded from the internet (I hope I don't get sued):

Sex

Mr. Obama maintained his 2008 support among women.

	All states	N.Y.	Mass.	Calif.	N.J.	Conn.	N.H.	Wis.	Iowa	Nev.	Pa.	Va.	Ohio	Fla.	Colo.	N.C.	Ariz.	Mo.	Ind.
Male	Romney: 52%	56	55	54	55	51	51	51	53	49	51	51	52	52	51	54	54	54	57
Female	Obama: 55%	68	65	64	62	63	58	57	59	57	56	54	55	53	51	51	50	53	52

Race & Ethnicity

The white vote went to Mr. Romney, mostly by wide margins. But Hispanics and Asians moved toward Mr. Obama, continuing their consolidation as Democrats.

	All states	N.Y.	Mass.	Calif.	N.J.	Conn.	N.H.	Wis.	Iowa	Nev.	Pa.	Va.	Ohio	Fla.	Colo.	N.C.	Ariz.	Mo.	Ind.
White	Romney: 59%	49	57	53	56	51	51	51	51	56	57	61	57	61	54	68	62	65	60
Black	Obam	94	92	96	96	93	N.	94	N.	92	93	9	96	95	N.	96	N.	94	89

	All states	N.Y.	Mass.	Calif.	N.J.	Conn.	N.H.	Wis.	Iowa	Nev.	Pa.	Va.	Ohio	Fla.	Colo.	N.C.	Ariz.	Mo.	Ind.
	a: 93%						A.		A.			3			A.		A.		
Hispanic	Obama: 71%	89	N.A.	72	N.A.	79	N.A.	66	N.A.	71	80	64	54	60	75	68	77	N.A.	N.A.
Asian	Obama: 73%	N.A.	N.A.	79	N.A.	N.A.	N.A.	N.A.	N.A.	50	N.A.	66	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.

Age

Young voters favored Mr. Obama, but less so than in 2008.

	All states	N.Y.	Mass.	Calif.	N.J.	Conn.	N.H.	Wis.	Iowa	Nev.	Pa.	Va.	Ohio	Fla.	Colo.	N.C.	Ariz.	Mo.	Ind.
18 - 29	Obama: 60%	72	73	71	63	66	62	60	56	68	63	61	63	66	N.A.	67	66	58	49
30 - 44	Obama: 52%	61	56	60	59	55	50	51	52	54	55	54	51	52	50	51	56	55	49
45 - 64	Romney: 51%	61	59	53	60	58	50	51	52	49	51	53	51	52	51	53	56	55	56
65 +	Romney: 56%	59	56	52	52	54	55	52	50	55	57	54	55	58	57	64	67	66	65

Education

	All states	N.Y.	Mass.	Calif.	N.J.	Conn.	N.H.	Wis.	Iowa	Nev.	Pa.	Va.	Ohio	Fla.	Colo.	N.C.	Ariz.	Mo.	Ind.
No college degree	Obama: 51%	66	62	60	55	62	50	51	52	56	57	50	53	52	54	52	52	54	53
Some college	Obama: 49%	65	60	59	56	60	50	51	51	50	51	51	51	50	55	54	53	55	52
College	Romney:	58	54	55	59	49	50	50	50	52	57	50	53	52	55	50	58	57	59

	All states	N.Y.	Mass.	Calif.	N.J.	Conn.	N.H.	Wis.	Iowa	Nev.	Pa.	Va.	Ohio	Fla.	Colo.	N.C.	Ariz.	Mo.	Ind.
	51%																		
Postgrad.	Obama: 55%	61	65	64	67	60	60	62	57	54	53	57	50	53	60	51	60	51	50

Income

Some of the president's firmest support came from low-income groups.

	All states	N.Y.	Mass.	Calif.	N.J.	Conn.	N.H.	Wis.	Iowa	Nev.	Pa.	Va.	Ohio	Fla.	Colo.	N.C.	Ariz.	Mo.	Ind.
Under $30,000	Obama: 63%	81	72	67	N.A.	73	61	67	68	68	74	61	63	61	60	58	53	60	60
$30,000 - $49,999	Obama: 57%	66	68	62	63	73	60	57	52	67	61	60	55	57	60	51	49	52	49
$50,000 or more	Romney: 53%	55	56	55	58	53	49	53	50	55	56	52	54	55	49	55	53	60	59
$100,000 or more	Romney: 54%	51	54	52	61	53	51	59	54	61	54	51	59	57	51	55	52	61	64

Size of Place

Cities shifted only slightly to Mr. Romney, and continue to be the centerpiece of the Obama majority. The suburbs broke back to the Republican side, while towns and rural areas solidified as Republican strongholds, more polarized from urban dwellers than before.

	All states	N.Y.	Mass.	Calif.	N.J.	Conn.	N.H.	Wis.	Iowa	Nev.	Pa.	Va.	Ohio	Fla.	Colo.	N.C.	Ariz.	Mo.	Ind.
Big cities	Obama: 69%	81	80	75	N.A.	N.A.	N.A.	83	N.A.	58	86	N.A.	60	67	N.A.	60	58	N.A.	66
Mid-sized cities	Obama: 58%	65	N.A.	60	N.A.	73	53	60	68	51	74	64	70	59	51	59	55	64	51
Small cities	Romney:	N.A.	N.A.	67	N.A.	49	59	73	52	65	60	75	57	61	N.A.	57	N.A.	58	N.A.

	All states	N.Y.	Mass.	Calif.	N.J.	Conn.	N.H.	Wis.	Iowa	Nev.	Pa.	Va.	Ohio	Fla.	Colo.	N.C.	Ariz.	Mo.	Ind.
	56%																		
Suburbs	Romney: 50%	52	57	56	58	55	50	52	49	54	55	50	51	51	50	54	60	52	56

Ideology

	All states	N.Y.	Mass.	Calif.	N.J.	Conn.	N.H.	Wis.	Iowa	Nev.	Pa.	Va.	Ohio	Fla.	Colo.	N.C.	Ariz.	Mo.	Ind.
Liberal	Obama: 86%	93	90	94	89	93	92	90	89	91	90	92	88	86	91	85	89	81	85
Moderate	Obama: 56%	63	55	60	60	56	56	61	63	58	54	56	57	53	51	57	56	53	55
Conservative	Romney: 82%	79	73	75	76	80	86	85	81	79	84	87	81	78	83	82	86	84	85

Married

	All states	N.Y.	Mass.	Calif.	N.J.	Conn.	N.H.	Wis.	Iowa	Nev.	Pa.	Va.	Ohio	Fla.	Colo.	N.C.	Ariz.	Mo.	Ind.
Married	Romney: 56%	53	55	51	55	54	50	54	52	54	55	55	56	56	53	60	63	62	62
Unmarried	Obama: 62%	74	67	67	65	65	58	61	63	64	64	61	62	61	60	61	62	56	55

Do You Think the Nation's Economy Is:

	All states	N.Y.	Mass.	Calif.	N.J.	Conn.	N.H.	Wis.	Iowa	Nev.	Pa.	Va.	Ohio	Fla.	Colo.	N.C.	Ariz.	Mo.	Ind.
Excellent or good	Obama: 90%	N.A.	N.A.	95	N.A.	93	89	93	92	95	91	92	93	91	92	91	N.A.	83	N.A.
Not so good/poor	Romney: 60%	50	N.A.	52	51	53	59	64	61	60	58	66	64	66	59	66	59	66	N.A.

Political Party

The independent vote was very close, but important states like New Hampshire tilted toward Mr. Obama.

	All states	N.Y.	Mass.	Calif.	N.J.	Conn.	N.H.	Wis.	Iowa	Nev.	Pa.	Va.	Ohio	Fla.	Colo.	N.C.	Ariz.	Mo.	Ind.
Democrat	Obama: 92%	95	93	94	95	94	96	95	95	96	91	94	93	90	96	91	89	90	91
Republican	Romney: 93%	88	93	91	88	91	94	95	93	93	93	94	94	92	94	96	92	96	93
Independent/Other	Romney: 50%	50	52	55	49	51	52	49	55	50	50	54	53	50	49	57	49	59	52

Are You Gay, Lesbian or Bisexual?

	All states	N.Y.	Mass.	Calif.	N.J.	Conn.	N.H.	Wis.	Iowa	Nev.	Pa.	Va.	Ohio	Fla.	Col o.	N.C.	Ariz.	Mo.	Ind.
Yes	Obama: 76%	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.
No	Tied: 49%	60	N.A.	60	57	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	53	N.A.	N.A.

Obama's Job Performance

	All states	N.Y.	Mass.	Calif.	N.J.	Conn.	N.H.	Wis.	Iowa	Nev.	Pa.	Va.	Ohio	Fla.	Colo.	N.C.	Ariz.	Mo.	Ind.
Approve	Obama: 89%	97	N.A.	89	86	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	92	N.A.	N.A.
Disapprove	Romney: 94%	N.A.	N.A.	91	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	N.A.	94	N.A.	N.A.

Exit Polls Methodology

The Election Day polls were based on questionnaires completed by voters as they left voting stations throughout the country on Tuesday, supplemented by telephone interviews with absentee and early voters. The polls were conducted by Edison Research of Somerville, N.J., for the National Election Pool, a consortium of ABC News, Associated Press, CBS News, CNN, Fox News and NBC News. The national results are based on voters in 350 randomly chosen precincts across the United States, and include absentee voters and early voters interviewed by telephone.

The state results are based on voters in 11 to 50 randomly selected precincts across each of 18 states analyzed by The Times. In certain states, some interviews were also conducted by telephone with absentee voters and early voters. In Colorado all interviews were by telephone and in Arizona the majority were. In theory, in 19 cases out of 20, the results from such polls should differ by no more than plus or minus 4 percentage points nationally, and 4 to 5 points in each state, from what would have been obtained by seeking to interview all voters who cast ballots in each of these elections.

Results based on smaller subgroups, like demographic groupings, have a larger potential sampling error. In addition to sampling error, the practical difficulties of conducting any survey of voter opinion on Election Day, such as the reluctance of some voters to take time to fill out the questionnaire, may introduce other sources of error into the poll.

The Times was assisted in its polling analysis by Ana Maria Arumi of Studio Arumi, Barry M. Feinberg of BMF Research & Consulting, Geoffrey D. Feinberg of Yale University, David R. Jones of Baruch College-CUNY, Michael R. Kagay of Princeton, N.J., Jeffrey W. Ladewig of the University of Connecticut, Helmut Norpoth of SUNY-Stony Brook, Annie L. Siegel of New York and Janet L. Streicher of Citibank.

A final note

If you'd like to read a critique of pre-election polls and don't mind some relatively heavy mathematics, I recommend that you read the article entitled "Lies, Damn Lies, and Pre-Election Polling" (2009), written by Walsh, Dolfin, and DiNardo, which is available for downloading from the internet free of charge.

CHAPTER 12: MINUS VS. DIVIDED BY

Introduction

You would like to compare two quantities A and B. Do you find the difference between the quantities or their ratio? If their difference, which gets subtracted from which? If their ratio, which quantity goes in the numerator and which goes in the denominator?

The research literature is somewhat silent regarding all of those questions. What follows is an attempt to at least partially rectify the situation by providing some considerations regarding when to focus on A-B, B-A, A/B, or B/A.

Examples

1. You are interested in the heights of John Doe (70 inches) and his son, Joe Doe (35 inches). Is it the positive difference 70 - 35 = 35, the negative difference 35 - 70 = -35, the ratio 70/35 = 2, or the ratio 35/70 = 1/2 = .5 that is of primary concern?

2. You are interested in the percentage of smokers in a particular population who got lung cancer (10%) and the percentage of non-smokers in that population who got lung cancer (2%). Is it the “attributable risk” 10% - 2% = 8%, the corresponding "attributable risk" 2% - 10% = -8%, the “relative risk” ("risk ratio") 10%/2% = 5, or the corresponding “relative risk” 2%/10% =1/5 =.2 that you should care about?

3. You are interested in the probability of drawing a spade from an ordinary deck of cards and the probability of not drawing a spade. Is it 13/52 - 39/52 = -26/52 = -1/2 = -.5, 39/52 - 13/52 = 26/52 = 1/2 = .5, (13/52)/(39/52) = 1/3, or (39/52)/(13/52) = 3 that is the best comparison between those two probabilities?

4. You are interested in the change from pretest to posttest of an experimental group that had a mean of 20 on the pretest and a mean of 30 on the posttest, as opposed to a control group that had a mean of 20 on the pretest and a mean of 10 on the posttest. Which numbers should you compare, and how should you compare them?

Considerations for those examples

1. The negative difference isn't very useful, other than as an indication of how much "catching up" Joe needs to do. As far as the other three alternatives are concerned, it all depends upon what you want to say after you make the comparison. Do you want to say something like "John is 35 inches taller than Joe"? "John is twice as tall as Joe"? "Joe is half as tall as John"?

2. Again, the negative attributable risk is not very useful. The positive attributable risk is most natural ("Is there a difference in the prevalence of lung cancer between smokers and non-smokers?"). The relative risk (or an approximation to the relative risk called an "odds ratio") is the overwhelming choice of epidemiologists. They also favor the reporting of relative risks that are greater than 1 ("Smokers are five times as likely to get lung cancer") rather than those that are less than 1 ("Non-smokers are one-fifth as likely to get lung cancer"). One difficulty with relative risks is that if the quantity that goes in the denominator is zero you have a serious problem, since you can't divide by zero. (A common but unsatisfactory solution to that problem is to call such a ratio "infinity".) Another difficulty with relative risks is that no distinction is made between a relative risk for small risks such as 2% and 1%, and for large risks such as 60% and 30%.

3. Both of the difference comparisons would be inappropriate, since it is a bit strange to subtract two things that are actually the complements of one another (the probability of something plus the probability of not-that-something is always equal to 1). So it comes down to whether you want to talk about the "odds in favor of" getting a spade ("1 to 3") or the "odds against" getting a spade ("3 to 1"). The latter is much more natural.

4. This very common comparison can get complicated. You probably don't want to calculate the pretest-to-posttest ratio or the posttest-to-pretest ratio for each of the two groups, for two reasons: (1) as indicated above, one or more of those averages might be equal to zero (because of how the "test" is scored); and (2) the scores often do not arise from a ratio scale. That leaves differences. But what differences? It would seem best to subtract the mean pretest score from the mean posttest score for each group (30 - 20 = 10 for the experimental group and 10 - 20 = -10 for the control group) and then to subtract those two differences from one another (10 -[-10] = 20, i.e., a "swing"'of 20 points), and that is what is usually done.

What some of the literature has to say

I mentioned above that the research literature is "somewhat silent" regarding the choice between differences and ratios. But there are a few very good sources regarding the advantages and disadvantages of each.

The earliest reference I could find is an article in Volume 1, Number 1 of the Peabody Journal of Education by Sherrod (1923). In that article he summarized a number of ratios that had just been developed, including the familiar mental age divided by chronological age, made some comments regarding differences, but did not provide any arguments concerning preferences for one vs. the other.

One of the best pieces (in my opinion) is an article that appeared recently on the American College of Physicians' website. The author pointed out that although differences and ratios of percentages are calculated from the same data, differences often "feel" smaller than quotients.

Another relevant source is the article that H.P. Tam and I wrote a few years ago (Knapp & Tam, 1997) concerning proportions, differences between proportions, and ratios of proportions. (A proportion is just like a percentage, with the decimal point moved two places to the left.)

There are also a few good substantive studies in which choices were made, and the investigators defended such choices. For example, Kruger and Nesse (2004) preferred the male-to-female mortality ratio to the difference between male and female mortality numbers. That ratio is methodologically similar to sex ratio at birth. It is reasonably well known that male births are more common than female births in just about all cultures. (In the United States the sex ratio at birth is about 1.05, i.e., there are approximately five percent more male births than female births, on the average.)

The Global Youth Tobacco Survey Collaborating Group (2003) also chose the male-to-female ratio for comparing the tobacco use of boys and girls in the 13-15 years of age range.

In an interesting "twist", Baron, Neiderhiser, and Gandy (1997) asked samples of Blacks and samples of Whites to estimate what the Black-to-White ratio was for deaths from various causes, and compared those estimates to the actual ratios as provided by the Centers for Disease Control (CDC).

Some general considerations

It all depends upon what the two quantities to be compared are.

1. Let's first consider situations such as that of Example #1 above, where we want to compare a single measurement on a variable with another single measurement on that variable. In that case, the reliability and validity with which the variable can be measured are crucial. You should compare the errors for the difference between two measurements with the errors for the ratio of two measurements. The relevant chapters in the college freshman physics laboratory manual (of all places) written by Simanek (2005) is especially good for a discussion of such errors. It turns out that the error associated with a difference A-B is the sum of the errors for A and B, whereas the error associated with a ratio A/B is the difference between the relative errors for A and for B. (The relative error for A is the error in A divided by A, and the relative error for B is the error for B divided by B.)

2. The most common comparison is for two percentages. If the two percentages are independent, i.e., they are not for the same observations or matched pairs of observations, the difference between the two is usually to be preferred; but if the

percentages are based upon huge numbers of observations in epidemiological investigations the ratio of the two is often the better choice, usually with the larger percentage in the numerator and the smaller percentage in the denominator.

If the percentages are not independent, e.g., the percentage of people who hold a particular attitude at Time 1 compared to the percentage of those same people who hold that attitude at Time 2, the difference (usually the Time 2 percentage minus the Time 1 percentage, i.e., the change, even if that is negative) is almost always to be preferred. Ratios of non-independent percentages are very difficult to handle statistically.

3. Quotients of probabilities are usually preferred to their differences.

4. On the other hand, comparisons of means that are not percentages (did you know that percentages are special kinds of means, with the only possible "scores" 0 and 100?) rarely involve quotients. As I pointed out in Example #4 above, there are several differences that might be of interest. For randomized experiments for which there is no pretest, subtracting the mean posttest score for the control group from the mean posttest score for the experimental group is most natural and most conventional. For pretest/posttest designs the "difference between the differences" or the difference between "adjusted" posttest means (via the analysis of covariance, for example) is the comparison of choice.

5. There are all sorts of change measures to be found in the literature, e.g., the difference between the mean score at Time 2 and the mean score at Time 1 divided by the mean score at Time 1 (which would provide an indication of the percent "improvement"). Many of those measures have sparked a considerable amount of controversy in the methodological literature, and the choice between expressing change as a difference or as a ratio is largely idiosyncratic.

The absolute value of differences

It is fairly common for people to concentrate on the absolute value of a difference, in addition to, or instead of, the "raw" difference. The absolute value of the difference between A and B, usually denoted as |A-B|, which is the same as |B-A|, is especially relevant when the discrepancy between the two is of interest, irrespective of which is greater.

Statistical inference

The foregoing discussion tacitly assumed that the data in hand are for a full population (even if the "N" is very small). If the data are for a random sample of a population, the preference between a difference statistic and a ratio statistic often depends upon the existence and/or complexity of the sampling distributions for such statistics. For example, the sampling distribution for a difference between two independent percentages is well known and straightforward (either the normal distribution or the chi-square distribution can be used) whereas the sampling distribution for the odds ratio is a real mess.

The essential matter to be taken into account is whether you get the same inferences for the difference and the ratio approaches. If the difference between two independent percentages is statistically significant at the .05 level, say, but their ratio is not, you have a real problem.

I carried out both analyses (with the help of Richard Lowry's nice VassarStats Statistical Computation website) for the following example taken from the StatPrimer website:

First % = 11/25 = 44.00; second % = 3/34 = 8.82; difference = 35.18; ratio = 4.99

The 95% confidence interval for the population difference is 10.36 to 56.91; the 95% confidence interval for the ratio is 1.55 to 16.05. 0 is not in the 95% confidence interval for the difference, so that difference is statistically significant at the .05 level. 1 is not in the 95% confidence interval for the ratio, so that is also statistically significant at the .05 level.

However, if some of those numbers are tweaked a bit I think it would be possible to have one significant and the other not, at the same alpha level. Try it.

A controversial example

It is very common during a presidential election campaign to hear on TV something like this: "In the most recent opinion poll, Smith is leading Jones by seven points." What is meant by a "point"? Is that information important? If so, can the difference be tested for statistical significance and/or can a confidence interval be constructed around it?

The answer to the first question is easy. A "point" is a percentage. For example, 46% of those polled might have favored Smith and 39% might have favored Jones, a difference of seven "points" or seven percent. Since those two numbers don't add to 100, there might be other candidates in the race, some of those polled had no preferences, or both. [I've never heard anybody refer to the ratio of the 46 to the 39. Have you?]

It is the second question that has sparked considerable controversy. Some people (like me) don't think the difference is important; what matters is the actual % support for each of the candidates. (Furthermore, the two percentages are not independent, since their sum plus the sum of the percentages for other candidates plus the percentage of people who expressed no preferences must add to 100.) Other people think it is very important, not only for opinion polls but also for things like the difference between the percentage of people in a sample who have blue eyes and the percentage of people in that same sample who have green eyes (see Simon, 2004), and other contexts.

Alas (for me), differences between percentages calculated on the same scale for the same sample can be tested for statistical significance, and confidence intervals for such differences can be determined. See Kish (1965) and Scott and Seber (1983).

Financial example: "The Rule of 72"

[My former colleague and good friend at Ohio State, Dick Shumway, referred me to a rule that his father, a banker, first brought to his attention.]

How many years does it take for your money to double if it is invested at an interest rate of r?

It obviously depends upon what r is, and whether the compounding is daily, weekly, monthly, annually, or continuously. I will consider here only the "compounded annually" case. The Rule of 72 postulates that a good approximation to the answer to the money-doubling question can be obtained by dividing the % interest rate into 72. For interest rates of 6% vs. 9%, for instance, the rule would claim that your money would double in 72/6 = 12 years and 72/9 = 8 years, respectively. But how good is that rule? The mathematics for the "exact" answer with which to compare the approximation as indicated by the Rule of 72 is a bit complicated, but consider the following table for various reasonable interest rates (both the exact answers and the approximations were obtained by using the calculator that is accessible at that marvelous website, www.moneychimp.com , which also provides the underlying mathematics):

r(%)	Exact	Approximation
3	23.45	24
4	17.67	18
5	14.21	14.40
6	11.90	12
7	10.24	10.29
8	9.01	9
9	8.04	8
10	7.27	7.20
11	6.64	6.55
12	6.12	6
...		
18	4.19	4

How good is the rule? In evaluating its "goodness" should we take the difference between exact and approximation (by subtracting which from which?) or should you divide one by the other (with which in the numerator and with which in the denominator?)? Those are both very difficult questions to answer, because the approximation is an over-estimate for interest rates of 3% to 7% (by decreasingly small discrepancies) and is an under-estimate for interest rates of 8% and above (by increasingly large discrepancies).

Do you see how difficult the choice of minus vs. divided by is?

Ordinal scales

It should go without saying, but I'll say it anyhow: For ordinal scales, e.g., the popular Likert-type scales, NEITHER a difference NOR a quotient is justified. Such scales don't have units that can be added, subtracted, multiplied, or divided.

Additional reading

If you would like to pursue other sources for discussions of the comparison of two numbers, there is the delightful article by Finney (2007). If you're interested in the sampling distributions of the difference and the ratio of two percentages, the epidemiological literature is your best bet, e.g., the Rothman and Greenland (1998) text. For an interesting discussion of differences vs. ratios in the context of learning disabilities, see Kavale (2003).

I mentioned reliability above (in conjunction with a comparison between two single measurements on the same scale). If you would like to see how that plays a role in the interpretation of various statistics, please visit my website (www.tomswebpage.net) and download any or all of my book, The reliability of measuring instruments (free of charge).

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CHAPTER 13: CHANGE

Introduction

Mary spelled correctly 3 words out of 6 on Monday and 5 words out of 6 on Wednesday. How should we measure the change in her performance?

Several years ago Cronbach and Furby (1970) argued that we shouldn't; i.e., we don't even need the concept of change. An extreme position? Of course, but read their article sometime and see what you think about it.

Why not just subtract the 3 from the 5 and get a change of two words? That's what most people would do. Or how about subtracting the percentage equivalents, 50% from 83.3%, and get a change of 33.3%? But...might it not be better to divide the 5 by the 3 and get 1.67, i.e., a change of 67%? [Something that starts out simple can get complicated very fast.]

Does the context matter? What went on between Monday and Wednesday? Was she part of a study in which some experimental treatment designed to improve spelling ability was administered? Or did she just get two days older?

Would it matter if the 3 were her attitude toward spelling on Monday and the 5 were her attitude toward spelling on Wednesday, both on a five-point Likert-type scale, where 1=hate, 2=dislike, 3=no opinion, 4=like, and 5=love?

Would it matter if it were only one word, e.g., antidisestablishmentarianism, and she spelled it incorrectly on Monday but spelled it correctly on Wednesday?

These problems regarding change are illustrative of what now follows.

A little history

Interest in the concept of change and its measurement dates back at least as long ago as Davies (1900). But it wasn't until much later, with the publication of the book edited by Harris (1963), that researchers in the social sciences started to debate the advantages and the disadvantages of various ways of measuring change. Thereafter hundreds of articles were written on the topic, including many of the sources cited in this chapter.

"Gain scores"

The above example of Mary's difference of two words is what educators and psychologists call a "gain score", with the Time 1 score subtracted from the Time 2 score. [If the difference is negative it's a loss, rather than a gain, but I've never heard the term "loss scores".] Such scores have been at the heart of one of the most heated controversies in the measurement literature. Why?

1. The two scores might not be on exactly the same scale. It is possible that her score of 3 out of 6 was on Form A of the spelling test and her score of 5 out of 6 was on Form B of the spelling test, with Form B consisting of different words, and the two forms were not perfectly comparable (equivalent, "parallel"). It might even have been desirable to use different forms on the two occasions, in order to reduce practice effect or mere "parroting back" at Time 2 of the spellings (correct or incorrect) at Time 1.

2. Mary herself and/or some other characteristics of the spelling test might have changed between Monday and Wednesday, especially if there were some sort of intervention between the two days. In order to get a "pure" measure of the change in her performance we need to assume that both of the testing conditions were the same. In a randomized experiment all bets regarding the direct relevance of classical test theory should be off if there is a pretest and a posttest to serve as indicators of a treatment effect, because the experimental treatment could affect the posttest mean AND the posttest variance AND the posttest reliability AND the correlation between pretest and posttest.

3. Gain scores are said by some measurement experts (e.g., O'Connor, 1972; Linn & Slinde, 1977; Humphreys, 1996) to be very unreliable, and by other measurement experts (e.g., Zimmerman & Williams, 1982; Williams & Zimmerman, 1996; Collins, 1996) to not be. Like the debate concerning the use of traditional interval-level statistics for ordinal scales, this controversy is unlikely ever to be resolved. I got myself embroiled in it many years ago (see Knapp, 1980; Williams & Zimmerman, 1984; Knapp, 1984). [I also got myself involved in the ordinal vs. interval controversy (Knapp, 1990, 1993).]

The problem is that if the instrument used to measure spelling ability (Were the words dictated? Was it a multiple-choice test of the discrimination between the correct spelling and one or more incorrect spellings?) is unreliable, Mary's "true score" on both Monday and Wednesday might have been 4 (she "deserved" a 4 both times), and the 3 and the 5 were both measurement errors attributable to "chance", and the difference of two words was not a true gain at all.

Some other attempts at measuring change

Given that gain scores might not be the best way to measure change, there have been numerous suggestions for improving things. In the Introduction (see above) I already mentioned the possibility of dividing the second score by the first score rather than subtracting the first score from the second score. This has never caught on, for some good reasons and some not-so-good reasons. The strongest arguments against dividing instead of subtracting are: (1) it only makes sense for ratio scales (a 5 for "love" divided by a 3 for "no opinion" is bizarre, for instance); and (2) if the score in the denominator is zero, the quotient is undefined. [If you are unfamiliar with the distinctions among nominal, ordinal, interval, and ratio scales, read the classic article by Stevens (1946).] The strongest argument in favor of the use of quotients rather than differences is that the measurement error could be smaller. See, for example, the manual by Bell(1999) regarding measurement uncertainty and how the uncertainty "propagates" via subtraction and division. It is available free of charge on the internet.

Other methodologists have advocated the use of "modified" change scores (raw change divided by possible change) or "residualized" change (the actual score at Time 2 minus the Time 2 score that is predicted from the Time 1 score in the regression of Time 2 score on Time 1 score). Both of these, and other variations on simple change, are beyond the scope of the present paper, but I have summarized some of their features in my reliability book (Knapp, 2015).

The measurement of change in the physical sciences vs. the social sciences

Some physical scientists wonder what the fuss is all about. If you're interested in John's weight of 250 pounds in January of one year and his weight of 200 pounds in January of the following year, for example, nothing other than subtracting the 250 from the 200 to get a loss of 50 pounds makes any sense, does it? Well, yes and no. You could still have the problem of scale difference (the scale in the doctor's office at Time 1 and the scale in John's home at Time 2?) and the problem of whether the raw change (the 50 pounds) is the best way to operationalize the change. Losing 50 pounds from 250 to 200 in a year is one thing, and might actually be beneficial. Losing 50 pounds from 150 to 100 in a year is something else, and might be disastrous. [I recently lost ten pounds from 150 to 140 and I was very concerned. (I am 5'11" tall.) I have since gained back five of those pounds, but am still not at my desired "fighting weight", so to speak.]

Measuring change using ordinal scales

I pointed out above that it wouldn't make sense to get the ratio of a second ordinal measure to a first ordinal measure in order to measure change from Time 1 to Time 2. It's equally wrong to take the difference, but people do it all the time. Wakita, Ueshima, & Noguchi (2012) even wrote a long article devoted to the matter of the influence of the number of scale categories on the psychological distances between the categories of a Likert-type scale. In their article concerned with the comparison of the arithmetic means of two groups using an ordinal scale, Marcus-Roberts and Roberts (1987) showed that Group I's mean could be higher than Group II's mean on the original version of an ordinal scale, but Group II's mean could be higher than Group I's mean on a perfectly defensible transformation of the scale points from the original version to another version. (They used as an example a grading scale of 1, 2, 3, 4, and 5 vs. a grading scale of 30, 40, 65, 75, and 100.) The matter of subtraction is meaningless for ordinal measurement.

Measuring change using dichotomies

Dichotomies such as male & female, yes & no, and right & wrong play a special role in science in general and statistics in particular. The numbers 1 and 0 are most often used to denote the two categories of a dichotomy. Variables treated that way are called "dummy" variables. For example, we might "code" male=1 and female =0 (not male); yes=1 and no=0 (not yes); and right=1 and wrong=0 (not right). As far as change is considered, the only permutations of 1 and 0 on two measuring occasions are (1,1), e.g., right both times; (1,0), e g., right at Time 1 and wrong at Time 2; (0,1), e.g., wrong at Time 1 and right at Time 2; and (0,0), e.g., wrong both times. The same permutations are also the only possibilities for a yes,no dichotomy. There are even fewer possibilities for the male, female variable, but sex change is well beyond the scope of this paper!

Covariance F vs. gain score t

For a pretest & posttest randomized experiment, Cronbach and Furby (1970) suggested the use of the analysis of covariance rather than a t test of the mean gain in the experimental group vs. the mean gain in the control group as one way of avoiding the concept of change. The research question becomes "What is the effect of the treatment on the posttest over and above what is predictable from the pretest?" as opposed to "What is the effect of the treatment on the change from pretest to posttest?" In our recent paper, Bill Schafer and I (Knapp & Schafer, 2009) actually provided a way to convert from one analysis to the other.

Measurement error

In the foregoing sections I have made occasional references to measurement error that might produce an obtained score that is different from the true score. Are measurement errors inevitable? If so, how are they best handled? In an interesting article (his presidential address to the National Council on Measurement in Education), Kane (2011) pointed out that in everyday situations such as sports results (e.g., a golfer shooting a 72 on one day and a 69 on the next day; a baseball team losing one day and winning the next day), we don't worry about measurement error. (Did the golfer deserve a 70 on both occasions? Did the baseball team possibly deserve to win the first time and lose the second time?). Perhaps we ought to.

What we should do

That brings me to share with you what I think we should do about measuring change:

1. Start by setting up two columns. Column A is headed Time 1 and Column B is headed Time 2. [Sounds like a Chinese menu.]

2. Enter the data of concern in the appropriate columns, with the maximum possible score (not the maximum obtained score) on both occasions at the top

and the rest of the scores listed in lockstep order beneath. For Mary's spelling test scores, the 3 would go in Column A and the 5 would go in Column B. For n people who attempted to spell antidisestablishmentarianism on two occasions, all of the 1's would be entered first, followed by all of the 0's, in the respective columns.

3. Draw lines connecting score in Column A with the corresponding score in Column B for each person. There would be only one (diagonal) line for Mary's 3 and her 5. For the n people trying to spell antidisestablishmentarianism, there would be n lines, some (perhaps all; perhaps none) horizontal, some (perhaps all; perhaps none) diagonal. If all of the lines are horizontal, there is no change for anyone. If all of the lines are diagonal and crossed, there is a lot of change going on. See Figure 1 for a hypothetical example of change from pretest to posttest for 18 people, almost all of whom changed from Time1 to Time 2 (only one of the lines is horizontal). I am grateful to Dave Kenny for permission to reprint that diagram, which is Figure 1.7 in the book co-authored by Campbell and Kenny (1999). [A similar figure, Figure 3-11 in Stanley (1964), antedated the figure in Campbell & Kenny. He (Stanley) was interested in the relative relationship between two variables, and not in change per se. He referred to parallel lines, whether horizontal or not, as indicative of perfect correlation.]

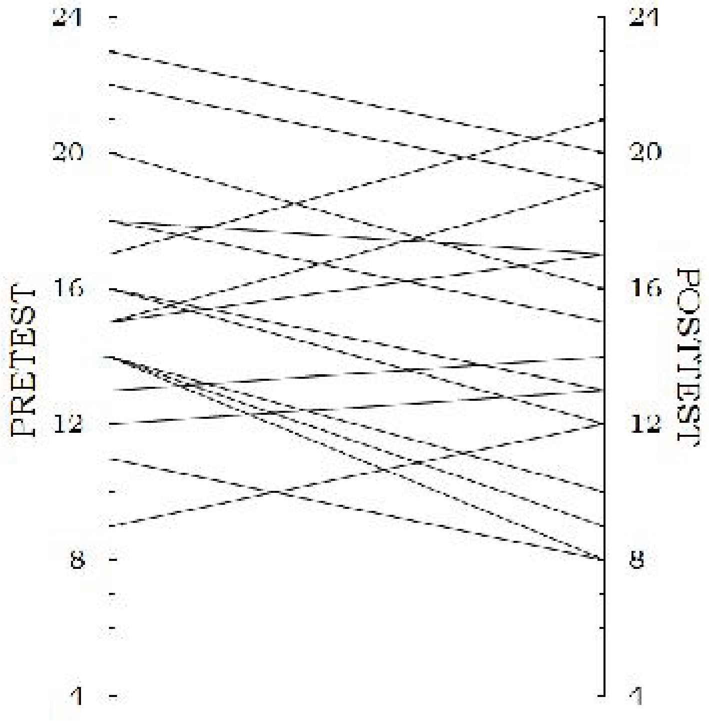

Figure 1: Some data for 18 hypothetical people.

Ties are always a problem (there are several ties in Figure 1, some at Time 1 and some at Time 2), especially when connecting a dichotomous observation (1 or 0) at Time 1 with a dichotomous observation at Time 2 and there are lots of ties. The best way to cope with this is to impose some sort of arbitrary (but not capricious) ordering of the tied observations, e.g., by I.D. number. In Figure 1, for instance, there is no particular reason for the two people tied at a score of 18 at Time 1 to have the line going to the score of 17 at Time 2 be above the line going to the score of 15 at Time 2. [It doesn't really matter in this case, because they both changed, one "losing" one point and the other "losing" two points.]

4. Either quit right there and interpret the results accordingly (Figure 1 is actually an excellent "descriptive statistic" for summarizing the change from pretest to posttest for those 18 people) or proceed to the next step.

5. Calculate an over-all measure of change. What measure? Aye, there's the rub. Intuitively it should be a function of the number of horizontal lines and the extent to which the lines cross. For ordinal and interval measurements the slant of the diagonal lines might also be of interest (with lines slanting upward indicative of "gain" and with lines slanting downward indicative of "loss"). But what function? Let me take a stab at it, using the data in Figure 1:

The percentage of horizontal lines (no change) in that figure is equal to 1 out of 18, or 5.6%. [Unless your eyes are better than mine, it's a bit hard to find the horizontal line for the 15th person, who "went" from 13 to 13, but there it is.] The percentage of upward slanting lines (gains), if I've counted correctly, is equal to 6 out of 18, or 33.3%. The percentage of downward slanting lines (losses) is equal to 11 out of 18, or 61.1%. A person who cares about over-all change for this dataset, and for most such datasets, is likely to be interested in one or more of those percentages. [I love percentages. See Chapter 15.]

Statistical inference from sample to population

Up to now I've said nothing about sampling (people, items, etc.). You have to have a defensible statistic before you can determine its sampling distribution and, in turn, talk about significance tests or confidence intervals. If the statistic is a percentage, its sampling distribution (binomial) is well known, as is its approximation (normal) for large samples and for sample percentages that are not close to either 0 or 100. The formulas for testing hypotheses about population percentages and for getting confidence intervals for population percentages are usually expressed in terms of proportions rather than percentages, but the conversion from percentage to proportion is easy (drop the % sign and move the decimal point two places to the left). Caution: concentrate on only one percentage. For the Campbell and Kenny data, for instance, don't test hypotheses for all of the 5.6%, the 33.3%, and the 61.1%, since that would be redundant (they are not independent; they add to 100).

If you wanted to go a little further, you could carry out McNemar's (1947) test of the statistical significance of dichotomous change, which involves setting up a 2x2 contingency table and concentrating on the frequencies in the "off-diagonal"(1,0) and (0,1) cells, where, for example, (1,0) indicates a change from yes to no, and (0,1) indicates a change from no to yes. But I wouldn't bother. Any significance test or any confidence interval assumes that the sample has been drawn at random, and you know how rare that is!

Some closing remarks, and a few more references

I'm with Cronbach and Furby. Forget about the various methods for measuring change that have been suggested by various people. But if you would like to find out more about what some experts say about the measurement of change, I recommend the article by Rogosa, Brandt, and Zimowski (1982), which reads very well [if you avoid some of the complicated mathematics]; and the book by Hedeker and Gibbons (2006). That book was cited in an interesting May 10, 2007 post on the Daily Kos website entitled "Statistics 101: Measuring change".

Most of the research on the measurement of change has been devoted to the determination of whether or not, or to what extent, change has taken place. There are a few researchers, however, who turn the problem around by claiming in certain situations that change HAS taken place and the problem is to determine if a particular measuring instrument is "sensitive", or "responsive", or has the capacity to detect such change. If you care about that (I don't), you might want to read the letter to the editor of Physical Therapy by Fritz (1999), the response to that letter, and/or some of the articles cited in the exchange.

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CHAPTER 14: SEPARATE VARIABLES VS. COMPOSITES

Introduction

I recently downloaded from the internet a table of Body Mass Index (BMI: weight in kilograms divided by the square of height in meters) as a function of height in inches and weight in pounds. I was struck by the fact that the same BMI can be obtained by a wide variety of corresponding heights and weights. For example, a BMI of 25 (which is just barely into the "overweight" category) is associated with measurements ranging from a height of 58 inches and a weight of 119 pounds to a height of 76 inches and a weight of 205 pounds. Although all of those combinations produce a BMI of 25, the "pictures" one gets of the persons who have those heights and weights are vastly different. Don't you lose a lot of valuable information by creating the composite?

I'm not the first person who has raised such concerns about BMIs. (See, for example, Dietz & Bellizi, 1999.) But I might be one of the few who are equally concerned about other composite measurements such as [cigarette]pack-years. Peto (2012a) made a strong case against the use of pack-years rather than packs per day and years of smoking as separate variables. There was also a critique of Peto (2012a) by Lubin & Caporaso (2012). followed by Peto's reply (2012b).

In what follows I would like to discuss some of the advantages and some of the disadvantages (both practical and technical) of research uses of separate variables vs. their composites.

Advantages of separate variables (disadvantages of composites)

The principal advantage of separate variables is the greater amount of information conveyed. As indicated above, the use of actual height and actual weight in a study of obesity, for example, better operationalizes body build than does the BMI composite.

A second advantage is that most people are more familiar with heights measured in inches (or in feet and inches) and weights measured in pounds than with the complicated expression for BMI. (Americans, especially non-scientist Americans, are also not familiar with heights in meters and weights in kilograms.)

A third advantage is that the frequency distributions for height separately and for weight separately tend to conform rather well to the traditional bell-shaped ("normal") form. The frequency distribution of BMI in some populations is decidedly non-normal. See Larson (2006) for an example.

There is the related matter of the sampling distributions of statistics (e.g., means, variances, correlation coefficients) for heights, weights, and BMI. Although all can be complicated, the sampling distributions for BMI-based statistics are much more so.

A final advantage of separate variables concerns measurement error. Formulas for the standard error of measurement for height in inches and the standard error of measurement for weight in pounds are straightforward and easy to interpret. For non-linear composites such as BMI, measurement error propagates in a very complex manner.

As illustrations of the propagation of measurement error, consider both body surface area (BSA) and body mass index (BMI). One fomula for body surface area (DuBois & DuBois, 1916) is the constant .20247 times height (in meters) raised to the .725 power times weight (in kilograms) raised to the .425 power. Body mass index (the Quetelet index), as indicated above, is equal to weight (in kilograms) divided by the square of height (in meters). Suppose you would like to get 95% confidence intervals for true body surface area and true body mass index for a hypothetical person, Mary Smith. You measure her height and get 60 inches; you measure her weight and get 120 pounds. Her obtained body surface area is 1.50 square meters and her obtained body mass index is 23.4 kilograms per square meter. Your height measuring instrument is said to have a standard error of measurement of 4 inches (that's awful) and your weight measuring instrument is said to have a standard error of measurement of 5 pounds (that's also awful); so the 95% confidence interval for Mary's true height is 60 ± 2(4) or from 52 inches to 68 inches, and the 95% confidence interval for Mary's true weight is 120 ± 2(5) or from 110 pounds to 130 pounds.

According to Taylor and Kuyatt (1994), if Y (the quantity you're interested in) is equal to any constant A times the product of X_1 raised to the power a and X_2 raised to the power b, then you can determine the "uncertainty" (using their term for standard error of measurement) associated with Y by the following formula:

$$(\text{Uncertainty of } Y) / |Y| = A\,[a^2(SE_{X1} / |X_1|)^2 + b^2(SE_{X2} / |X_2|)^2\,]^{.5}$$

where $|Y|$ is the absolute value of Y, SE_{X1} is the standard error of measurement for X_1 , $|X_1|$ is the absolute value of X_1 , SE_{X2} is the standard error of measurement for X_2 , and $|X_2|$ is the absolute value of X_2 , if both X_1 and X_2 are not equal to zero.

For body surface area, if height = X_1 and weight = X_2 , then A = .20247, a = .725, and b = .425. For body mass index, if again height = X_1 and weight = X_2 , then A = 1, a = 1, and b = -2. Substituting in the standard error (uncertainty) formula for Y and laying off two standard errors around the obtained BSA and the obtained BMI, we have

Body surface area: 1.50 ± 2 (.05) = 1.40 to 1.60

Body mass index: 23.5 ± 2 (3.3) = 16.9 to 30.1

Body surface area is often used as the basis for determining the appropriate dose of medication to be prescribed (BSA is multiplied by dose per square meter to get the desired dose), so you can see from this admittedly extreme example that reasonable limits for "the true required dose" can vary dramatically, with possible serious medical complications for a dose that might be either too large or too small.

Body mass index is often used for various recommended weight therapies, and since the lower limit of the 95% confidence interval for Mary's true BMI is in the "underweight" range and the upper limit is in the "obese" range, the extremely high standard errors of measurement for both height and weight had a very serious effect on BMI. (Thank goodness these are hypothetical data for very poor measuring instruments.)

Advantages of composites (disadvantages of separate variables)

The principal advantage of a composite is that it produces a single variable rather than having to deal with two or more variables. Again taking BMI as an example, if we wanted to use body build to predict morbidity or mortality, a simple regression analysis would suffice, where X = BMI and Y = some indicator such as age at onset of disease or age at death. For height and weight separately you would have two predictors X_1 = height and X_2 = weight, and a multiple regression analysis would be needed.

Another advantage is that composites like BMI and pack-years are so ingrained in the research literature that any suggestion of “de-compositing” them is likely to invoke concern by journal editors, reviewers, and readers of journals in which those composites have been mainstays.

A third advantage of composites, especially linear composites, is that they usually have greater validity and reliability than the individual components that comprise them. A simple example is a test of spelling ability. If a spelling test consists of just one item, it is likely to be less valid and less reliable than a total score based upon two or more items (Rudner, 2001). But see below for a counter-example.

A final advantage arises in the context of multiple dependent variables, highly correlated with one another, and with none designated as the “primary” outcome variable. Creating a composite of such variables rather than treating them separately can lead to higher power and obviates the necessity for any sort of Bonferroni-type correction. See Freemantle, Calvert, Wood, Eastaugh, and Griffin (2003) and Song, Lin, Ward, and Fine (2013).

Pack-years

I also recently downloaded from the internet a chart that defined pack-years (number of packs of cigarettes smoked per day times the number of years of smoking cigarettes) and gave the following example:

(70cigarettes/day ÷20 cigarettes/pack) X 10years = 35pack-years
(35cigarettes/day ÷20 cigarettes/pack) X 20years = 35pack-years
(20cigarettes/day ÷20 cigarettes/pack) X 35years = 35pack-years

That doesn't make sense (to me, anyhow). Those are three very different smoking histories, yet the "score" is the same for each.

Other composites

1. Socio-economic status (SES)

There is perhaps no better example than SES to illustrate some of the advantages and disadvantages alluded to above of the use of separate variables as opposed to various composites (they're usually called "indexes" or "indices"). For a thorough discussion of the operationalization of SES for the National Assessment of Educational Progress project, see Cowan, et al. (n.d.).

2. Achievement tests

What bothers me most is the concept of a "total score" on an achievement test, e.g., of spelling ability, whereby two people can get the same total score yet not answer correctly (and incorrectly) any of the same items. Consider the following data:

Person	Item 1	Item 2	Total score (= number right)
A	right (1)	wrong (0)	1
B	wrong (0)	right (1)	1

Does that bother you? Two diametrically opposite performances, same score?

The best solution to this problem is either to never determine a total score or to construct test items that form what's called a Guttman Scale (see Guttman, 1944, and Abdi, 2010). For a perfect Guttman Scale, if you know a person's total score you can determine which items the person answered correctly (or, for attitude scales, which items were "endorsed" in the positive direction). Everybody with the same total score must have responded in the same way to every item. Perfect Guttman Scales are very rare, but some measuring instruments, e.g, well-constructed tests of racial prejudice, come very close.

How much does it matter? An example

A few years ago, Freedman, et al. (2006) investigated the prediction of mortality from both obesity and cigarette-smoking history, using data from the U.S. Radiologic Technologists (USRT) Study. I was able to gain access to the raw data for a random sample of 200 of the males in that study. Here is what I found for age at death as the dependent variable:

Regression of deathage on height and weight: r-square = 2.1%

Regression of deathage on bmi: r-square = 0.1%

Regression of deathage on packs and years: r-square = 16.2%

Regression of deathage on pack-years: r-square = 6.4%

For these data, age at death is "more predictable" from height and weight separately than from bmi (but not by much; both r-squares are very small). And age at death is "more predictable" from packs and years separately than from pack-years (by almost 10%).

A final note

If you measure a person's height and a person's weight, or ask him(her) to self-report both, there is a handy-dandy device (an abdominal computed tomographic image) for determining his(her) BMI and BSA in one full swoop. See the article by Geraghty and Boone (2003.)

Acknowledgment

I would like to thank Suzy Milliard, Freedom of Information/Privacy Coordinator for giving me access to the data for the USRT study.

P.S.: This just in: There is a variable named egg-yolk years (see Spence, Jenkins, & Davignon, 2012; Lucan, 2013; Olver, Thomas, & Hamilton, 2013; and Spence, Jenkins, & Davignon, 2013). It is defined as the number of egg yolks consumed per week multiplied by the number of years in which such consumption took place. What will they think up next?

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CHAPTER 15: PERCENTAGES: THE MOST USEFUL STATISTICS EVER INVENTED

"Eighty percent of success is showing up."
- Woody Allen

“Baseball is ninety percent mental and the other half is physical.”
- Yogi Berra

"Genius is one percent inspiration and ninety-nine percent perspiration."
- Thomas Edison

Preface

I like to think of this chapter as a book within a book. It is essentially self-contained. If you learn nothing else about statistics other than percentages you will still have learned a lot.

You know what a percentage is. 2 out of 4 is 50%. 3 is 25% of 12. Etc. But do you know enough about percentages? Is a percentage the same thing as a fraction or a proportion? Should we take the difference between two percentages or their ratio? If their ratio, which percentage goes in the numerator and which goes in the denominator? Does it matter? What do we mean by something being statistically significant at the 5% level? What is a 95% confidence interval? Those questions, and much more, are what this chapter is all about.

In his fine article regarding nominal and ordinal bivariate statistics, Buchanan (1974) provided several criteria for a good statistic, and concluded: "The percentage is the most useful statistic ever invented…" (p. 629). I agree, and thus my choice for the title of this chapter. In the ten sections that follow, I hope to convince you of the defensibility of that claim.

The first section is on basic concepts (what a percentage is, how it differs from a fraction and a proportion, what sorts of percentage calculations are useful in statistics, etc.) If you're pretty sure you already understand such things, you might want to skip that section (but be prepared to return to it if you get stuck later on!).

In the second section I talk about the interpretation of percentages, differences between percentages, and ratios of percentages, including some common mis-interpretations and pitfalls in the use of percentages.

Section 3 is devoted to probability and its explanation in terms of percentages. I also include in that section a discussion of the concept of "odds" (both in favor of, and against, something). Probability and odds, though related, are not the same thing (but you wouldn't know that from reading much of the scientific and lay literature).

Section 4 is concerned with a percentage in a sample vis-à-vis the percentage in the population from which the sample has been drawn. In my opinion, that is the most elementary notion in inferential statistics, as well as the most important. Point estimation, interval estimation (confidence intervals), and hypothesis testing (significance testing) are all considered.

The following section goes one step further by discussing inferential statistical procedures for examining the difference between two percentages and the ratio of two percentages, with special attention to applications in epidemiology.

The next four sections are devoted to special topics involving percentages. Section 6 treats graphical procedures for displaying and interpreting percentages. It is followed by a section that deals with the use of percentages to determine the extent to which two frequency distributions overlap. Section 8 discusses the pros and cons of dichotomizing a continuous variable and using percentages with the resulting dichotomy. Applications to the reliability of measuring instruments (my second most favorite statistical concept--see Knapp, 2015) are explored in Section 9. The final section attempts to summarize things and tie up loose ends.

There is an extensive list of references, all of which are cited in the text proper. You might regard some of them as “old” (they actually range from 1919 to 2015). I like old references, especially those that are classics and/or are particularly apt for clarifying certain points. [And I’m old too.]

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Section 1: The basics

What is a percentage?

A percentage is a part of a whole. It can take on values between 0 (none of the whole) and 100 (all of the whole). The whole is called the base. The base must ALWAYS be reported whenever a percentage is determined.

Example: There are 20 students in a classroom, 12 of whom are males and 8 of whom are females. The percentage of males is 12 “out of” 20, or 60%. The percentage of females is 8 “out of” 20, or 40%. (20 is the base.)

To how many decimal places should a percentage be reported?

One place to the right of the decimal point is usually sufficient, and you should almost never report more than two. For example, 2 out of 3 is 66 2/3 %, which rounds to 66.67% or 66.7%. [To refresh your memory, you round down if the fractional part of a mixed number is less than 1/2 or if the next digit is 0, 1, 2, 3, or 4; you round up if the fractional part is greater than or equal to 1/2 or if the next digit is 5, 6, 7, 8, or 9.] Computer programs can report numbers to ten or more decimal places, but that doesn’t mean that you have to. I believe that people who report percentages to several decimal places are trying to impress the reader (consciously or unconsciously).

Lang and Secic (2006) provide the following rather rigid rule:

> “When the sample size is greater than 100, report percentages to no more than one decimal place. When sample size is less than 100, report percentages in whole numbers. When sample size is less than, say, 20, consider reporting the actual numbers rather than percentages.” (p. 5)

[Their rule is just as appropriate for full populations as it is for samples. And they don’t say it, perhaps because it is obvious, but if the size of the group is equal to 100, be it sample or population, the percentages are the same as the numerators themselves, with a % sign tacked on.]

How does a percentage differ from a fraction and a proportion?

Fractions and proportions are also parts of wholes, but both take on values between 0 (none of the whole) and 1 (all of the whole), rather than between 0 and 100. To convert from a fraction or a proportion to a percentage you multiply by 100 and add a % sign. To convert from a percentage to a proportion you delete the % sign and divide by 100. That can in turn be converted into a fraction. For example, 1/4 multiplied by 100 is 25%. .25 multiplied by 100 is also 25%. 25% divided by 100 is .25, which can be expressed as a fraction in a variety of ways, such as 25/100 or, in “lowest terms”, 1/4. (See the excellent On-

Line Math Learning Center website for examples of how to convert from any of these part/whole statistics into any of the others.) But, surprisingly (to me, anyhow), people tend to react differently to statements given in percentage terms vs. fractional terms, even when the statements are mathematically equivalent. (See the October 29, 2007 post by Roger Dooley on the Neuromarketing website. Fascinating.)

Most authors of statistics books, and most researchers, prefer to work with proportions. I prefer percentages [obviously, or I wouldn't have written this chapter!], as does Milo Schield, Professor of Business Administration and Director of the W. M. Keck Statistical Literacy Project at Augsburg College in Minneapolis, Minnesota. (See, for example, Schield, 2008). One of the reasons I don't like to talk about proportions is that they have another meaning in mathematics in general: "a is in the same proportion to b as c is to d". People studying statistics could easily be confused about those two different meanings of the term "proportion".

One well-known author (Gerd Gigerenzer) prefers fractions to both percentages and proportions. In his book (Gigerenzer, 2002) and in a subsequent article he co-authored with several colleagues (Gigerenzer, et al., 2007), he advocates an approach that he calls the method of "natural frequencies" for dealing with percentages. For example, instead of saying something like "10% of smokers get lung cancer", he would say "100 out of every 1000 smokers get lung cancer" [He actually uses breast cancer to illustrate his method]. Heynen (2009) agrees. But more about that in Section 3, in conjunction with positive diagnoses of diseases.

Is there any difference between a percentage and a percent?

The two terms are often used interchangeably (as I do in this book), but "percentage" is sometimes regarded as the more general term and "percent" as the more specific term. The AMA Manual of Style, the BioMedical Editor website, the Grammar Girl website, and Milo Schield have more to say regarding that distinction. The Grammar Girl (Mignon Fogarty) also explains whether percentage takes a singular or plural verb, whether to use words or numbers before the % sign, whether to have a leading 0 before a decimal number that can't be greater than 1, and all sorts of other interesting things.

Do percentages have to add to 100?

A resounding YES, if the percentages are all taken on the same base for the same variable, if only one "response" is permitted, and if there are no missing data. For a group of people consisting of both males and females, the % male plus the % female must be equal to 100, as indicated in the above example (60+40=100). If the variable consists of more than two categories (a two-categoried variable is called a dichotomy), the total might not add to 100 because of rounding. As a hypothetical example, consider what might happen if the variable is something like Religious Affiliation and you have percentages reported to the nearest tenth for a group of 153 people of 17 different religions. If those percentages add exactly to 100 I would be terribly surprised.

Several years ago, Mosteller, Youtz, and Zahn (1967) determined that the probability (see Section 3) of rounded percentages adding exactly to 100 is perfect for two categories, approximately 3/4 for three categories, approximately 2/3 for four categories, and approximately $\surd 6/c\pi$ for $c \geq 5$, where c is the number of categories and π is the well-known ratio of the circumference of a circle to its diameter (= approximately 3.14). Amazing!

[For an interesting follow-up article, see Diaconis & Freedman (1979). Warning: It has some pretty heavy mathematics!]

Here's a real-data example of the percentages of the various possible blood types for the U.S.:

O Positive	38.4%
A Positive	32.3%
B Positive	9.4%
O Negative	7.7%
A Negative	6.5%
AB Positive	3.2%
B Negative	1.7%
AB Negative	.7%

[Source: American Red Cross website]

Those add to 99.9%. The probability that they would add exactly to 100%, by the Mosteller, et al. formula, is approximately .52.

Can't a percentage be greater than 100?

I said above that percentages can only take on values between 0 and 100. There is nothing less than none of a whole, and there is nothing greater than all of a whole. But occasionally [too often, in my opinion, but Milo Schield disagrees with me] you will see a statistic such as "Her salary went up by 200%" or "John is 300% taller than Mary". Those examples refer to a comparison in terms of a percentage, not an actual percentage. I will have a great deal to say about such comparisons in the next section and in Section 5.

Why are percentages ubiquitous?

People in general, and researchers in particular, have always been interested in the % of things that are of a particular type, and they always will be. What % of voters voted for Barack Obama in the most recent presidential election? What %

of smokers get lung cancer? What % of the questions on a test do I have to answer correctly in order to pass?

An exceptionally readable source about opinion polling is the article in the Public Opinion Quarterly by Wilks (1940a), which was written just before the entrance of the U.S. into World War II, a time when opinions regarding that war were diverse and passionate. I highly recommend that article to those of you who want to know how opinion polls SHOULD work. S.S. Wilks was an exceptional statistician.

What is a rate?

A rate is a special kind of percentage, and is most often referred to in economics, demography, and epidemiology. An interest rate of 10%, for example, means that for every dollar there is a corresponding $1.10 that needs to be taken into consideration (whether it is to your advantage or to your disadvantage).

There is something called "The Rule of 72" regarding interest rates. If you want to determine how many years it would take for your money to double if it were invested at a particular interest rate, compounded annually, divide the interest rate into 72 and you'll have a close approximation. To take a somewhat optimistic example, if the rate is 18% it would take four years (72 divided by 18 is 4) to double your money. [You would actually have "only" 1.93877 times as much after four years, but that's close enough to 2 for government work! Those of you who already know something about compound interest might want to check that.]

Birth rates and death rates are of particular concern in the analysis of population growth or decline. In order to avoid small numbers, they are usually reported "per thousand" rather than "per hundred" (which is what a simple percent is). For example, if in the year 2010 there were six million births in the United States "out of" a population of 300 million, the ("crude") birth rate would be 6/300, or 2%, or 20 per thousand. If there were three million deaths in that same year, the (also "crude") death rate would be 3/300, or 1%, or 10 per thousand. [The actual numbers were fairly close to those I just cited as an example.]

One of the most interesting rates is the "response rate" for surveys. It is the percentage of people who agree to participate in a survey. For some surveys, especially those that deal with sensitive matters such as religious beliefs and sexual behavior, the response rate is discouragingly low (and often not even reported), so that the results must be taken with more than the usual grain of salt.

Some rates are phrased in even different terms, e.g., parts per 100,000 or parts per million (the latter often used to express the concentration of a particular pollutant).

What kinds of calculations can be made with percentages?

The most common kinds of calculations involve subtraction and division. If you have two percentages, e.g., the percentage of smokers who get lung cancer and the percentage of non-smokers who get lung cancer, you might want to subtract one from the other or you might want to divide one by the other. Which is it better to do? That matter has been debated for years. If 10% of smokers get lung cancer and 2% of non-smokers get lung cancer (the two percentages are actually lower than that for the U.S.), the difference is 8% and the ratio is 5-to-1 (or 1-to-5, if you invert that ratio). I will have much more to say about differences between percentages and ratios of percentages in subsequent chapters. (And see the brief, but excellent, discussion of differences vs. ratios of percentages at the American College of Physicians website.)

Percentages can also be added and multiplied, although such calculations are less common than the subtraction or division of percentages. I've already said that percentages must add to 100, whenever they're taken on the same base for the same variable. And sometimes we're interested in "the percentage of a percentage", in which case two percentages are multiplied. For example, if 10% of smokers get lung cancer and 60% of them (the smokers who get lung cancer) are men, the percentage of smokers who get cancer and are male is 60% of 10%, or 6%. (By subtraction, the other 4% are female.)

You also have to be careful about averaging percentages. If 10% of smokers get lung cancer and 2% of non-smokers get lung cancer, you can't just "split the difference" between those two numbers to get the % of people in general who get lung cancer by adding them together and dividing by two (to obtain 6%). The number of non-smokers far exceeds the number of smokers (at least in 2016), so the percentages have to be weighted before averaging. Without knowing how many smokers and non-smokers there are, all you know is that the average lung cancer % is somewhere between 2% and 10%, but closer to the 2%. [Do you follow that?]

What is inverse percentaging?

You're reading the report of a study in which there are some missing data (see the following section), with one of the percentages based upon an n of 153 and another based upon an n of 147. [153 is one of my favorite numbers. Do you know why? I'll tell you at the end of this chapter.] You are particularly interested in a variable for which the percentage is given as 69.8, but the author didn't explicitly provide the n for that percentage (much less the numerator that got divided by that n). Can you find out what n is, without writing to the author?

The answer is a qualified yes, if you're good at "inverse percentaging". There are two ways of going about it. The first is by brute force. You take out your trusty calculator and try several combinations of numerators with denominators of 153 and 147 and see which, if any, of them yield 69.8% (rounded to the nearest tenth of a percent). OR, you can use a book of tables, e.g., the book by Stone (1958), and see what kinds of percentages you get for what kinds of n's.

Stone's book provides percentages for all parts from 1 to n of n's from 1 to 399. You turn to the page for an n of 153 and find that 107 is 69.9% of 153. (That is the closest % to 69.8.) You then turn to the page for 147 and find that 102 is 69.4% of 147 and 103 is 70.1% of 147. What is your best guess for the n and for the numerator that you care about? Since the 69.9% for 107 out of 153 is very close to the reported 69.8% (perhaps the author rounded incorrectly or it was a typo?), since the 69.4% for 102 out of 147 is not nearly as close, and the 70.1% is also not as close (and is an unlikely typo), your best guess is 107 out of 153. But you of course could be wrong.

What about the unit of analysis and the independence of observations?

In my opinion, more methodological mistakes are made regarding the unit of analysis and the independence of observations than in any other aspect of a research study. The unit of analysis is the entity (person, classroom, school,…whatever) upon which any percentage is taken. The observations are the numbers that are used in the calculation, and they must be independent of one another.

If, for example, you are determining the percentage male within a group of 20 people, and there are 12 males and 8 females in the group (as above), the percentage of male persons is 12/20 or 60%. But that calculation assumes that each person is counted only once, there are no twins in the group, etc. If the 20 persons are in two different classrooms, with one classroom containing all 12 of the males and the other classroom containing all 8 of the females, then the percentage of male classrooms is 1/2 or 50%, provided the two classrooms are independent They could be dependent if, to take an admittedly extreme case, there were 8 male/female twin-pairs who were deliberately assigned to different classrooms, with 4 other males joining the 8 males in the male classroom. [Gets tricky, doesn't it?]

One of the first researchers to raise serious concerns about the appropriate unit of analysis and the possibility of non-independent observations was Robinson (1950) in his investigation of the relationship between race and literacy. He found (among other things) that for a set of data in the 1930 U.S. Census the correlation between a White/Black dichotomy and a Literate/Illiterate dichotomy was only .203 with individual person as the unit of analysis (n = 97,272) but was .946 with major geographical region as the unit of analysis (n = 9), the latter being the so-called "ecological" correlation between % Black and % Illiterate. His article created all sorts of reactions from disbelief to demands for re-analyses of data for which something other than the individual person was used as the unit of analysis. It (his article) was recently reprinted in the International Journal of

Epidemiology, along with several commentaries by Subramanian, et al. (2009a, 2009b), Oakes (2009), Firebaugh (2009), and Wakefield (2009). I have also written a piece about the same problem (Knapp, 1977a).

What is a percentile?

A percentile is a point on a scale below which some percentage of things fall. For example, "John scored at the 75th percentile on the SAT" means that 75% of the takers scored lower than he did and 25% scored higher. We don't even know, and often don't care, what his actual score was on the test. The only sense in which a percentile refers to a part of a whole is as a part of all of the people, not a part of all of the items on the test.

Section 2: Interpreting percentages

Since a percentage is simple to calculate (much simpler than, say, a standard deviation, the formula for which has 11 symbols!), you would think that it is also simple to interpret. Not so, as this section will now show.

Small base

It is fairly common to read a claim such as "66 2/3 % of doctors are sued for malpractice". The information that the claimant doesn't provide is that only three doctors were included in the report and two of them were sued. In the first chapter I pointed out that the base upon which a percentage is determined must be provided. There is (or should be) little interest in a study of just three persons, unless those three persons are very special indeed.

There is an interesting article by Buescher (2008) that discusses some of the problems with using rates that have small numbers in the numerator, even if the base itself is large. And in his commentary concerning an article in the journal JACC Cardiovascular Imaging, Camici (2009) advises caution in the use of any ratios that refer to percentages.

Missing data

The bane of every researcher's existence is the problem of missing data. You go to great lengths in designing a study, preparing the measuring instruments, etc., only to find out that some people, for whatever reason, don't have a measurement on every variable. This situation is very common for a survey in which questions are posed regarding religious beliefs and/or sexual behavior. Some people don't like to be asked such questions, and they refuse to answer them. What is the researcher to do? Entire books have been written about the problem of missing data (e.g., Little & Rubin, 2002). Consider what happens when there is a question in a survey such as "Do you believe in God?", the only two response categories are yes and no, and you get 30 yeses, 10 nos, and 10 "missing" responses in a sample of 50 people. Is the "%yes" 30 out of 50 (=60%) or 30 out of 40 (= 75%)? And Is the "%no" 10 out of 50 (=20%) or 10 out of 40 (=25%)? If it's out of 50, the percentages (60 and 20) don't add to 100. If it's out of 40, the base is 40, not the actual sample size of 50 (that's the better way to deal with the problem…"no response" becomes a third category).

Overlapping categories

Suppose you're interested in the percentages of people who have various diseases. For a particular population the percentage having AIDS plus the percentage having lung cancer plus the percentage having hypertension might very well add to more than 100 because some people might suffer from more than one of those diseases. I used this example in my little book entitled

Learning statistics through playing cards (Knapp, 1996, p. 24). The three categories (AIDS, lung cancer, and hypertension) could “overlap”. In the technical jargon of statistics, they are not “mutually exclusive”.

Percent change

Whenever there are missing data (see above) the base changes. But when you’re specifically interested in percent change the base also does not stay the same, and strange things can happen. Consider the example in Darrell Huff’s delightful book, How to lie with statistics (1954), of a man whose salary was $100 per week and who had to take a 50% pay cut to $50 per week because of difficult economic times. [(100-50)/100 = .50 or 50%.] Times suddenly improved and the person was subsequently given a 50% raise. Was his salary back to the original $100? No. The base has shifted from 100 to 50. $50 plus 50% of $50 is $75, not $100. [The illustrations by Irving Geis in Huff’s book are hilarious!] There are several other examples in the research literature and on the internet regarding the problem of % decrease followed by % increase, as well as % increase followed by % decrease, % decrease followed by another % decrease, and % increase followed by another % increase. (See, for example, the definition of a percentage at the wordIQ.com website; the Pitfalls of Percentages webpage at the Hypography website; the discussion of percentages at George Mason University’s STATS website; and the article by Chen and Rao, 2007.)

A recent instance of a problem in interpreting percent change is to be found in the research literature on the effects of smoking bans. Several authors (e.g., Lightwood & Glantz, 2009; Meyers, 2009) claim that smoking bans cause decreases in acute myocardial infarctions (AMI). They base their claims upon meta-analyses of a small number of studies that found a variety of changes in the percent of AMIs, e.g., Sargent, Shepard, and Glantz (2004), who investigated the numbers of AMIs in Helena, MT before a smoking ban, during the time the ban was in effect, and after the ban had been lifted. There are several problems with such claims, however:

1. Causation is very difficult to determine. There is a well-known dictum in research methodology that "correlation is not necessarily causation". As Sargent, et al. (2004) themselves acknowledged:

> "This is a “before and after” study that relies on historical controls (before and after the period that the law was in effect), not a randomised controlled trial. Because this study simply observed a change in the number of admissions for acute myocardial infarction, there is always the chance that the change we observed was due to some unobserved confounding variable or systematic bias." (p. 979)

2. Sargent, et al. found a grand total of 24 AMIs in the city of Helena during the six-month ban in the year 2002, as opposed to an average of 40 AMIs in other six-month periods just before and just after the ban. Those are very small numbers, even though the difference of 16 is "statistically significant". They also compared that difference of 16 AMIs to a difference of 5.6 AMIs between 18 and 12.4 before and after for a "not Helena" area (just outside of Helena). The difference between those two differences of 16 and 5.6 was also found to be small but "statistically significant". But having a "not Helena" sample is not the same as having a randomly comparable group in a controlled experiment in Helena itself.

3. But to the point of this section, the drop from 40 to 24 within Helena is a 40% change (16 "out of" 40); the "rebound" from 24 to 40 is a 66 2/3% change (16 "out of" the new base of 24). To their credit, Sargent et al. did not emphasize the latter, even though it is clear they believe it was the ban and its subsequent rescission that were the causes of the decrease followed by the increase.

[Note: The StateMaster.com website cites the Helena study as an example of a "natural experiment". I disagree. In my opinion, "natural experiment" is an oxymoron. There is nothing natural about an experiment, which is admittedly artificial (the researcher intervenes), but usually necessary for the determination of causation. Sargent, et al. did not intervene. They just collected existing data.]

I recently encountered several examples of the inappropriate calculation and/or interpretation of percent change in a table in a newpaper (that shall remain nameless) on % increase or decrease in real estate sales prices. The people who prepared the table used [implicitly] a formula for % change of the form (Time 2 price minus Time 1 price)/ Time 1 price. One of the comparisons involved a median price at Time 1 of $0 and a median price at Time 2 of $72,500 that was claimed to yield a 0% increase, since the calculation of ($72,500 - 0)/ 0 was said to be equal to 0. Not so. You can't divide by 0, so the percent increase was actually indeterminate.

Percent difference

Percent change is a special case of percent difference. (It’s change if it’s for the same things, usually people, across time.) Both percent difference (see the following section) and the difference between two percentages (see Section 5) come up all of the time [but they’re not the same thing, so be careful!].

The percent difference between two continuous quantities

Cole (2000) suggests that it is better to use logarithms when interpreting the percent difference between two continuous quantities. He gives the example of a comparison between the average height of British adult men (177.3 centimeters, which is approximately 69.8 inches, or slightly under 5’10”) and the average height of British adult women (163.6 centimeters, which is approximately 64.4 inches). The usual formula for finding the percent difference between two quantities x_1 and x_2 is $100(x_2 - x_1)/x_1$. But which do you call x_1 and which do you call x_2? Working the formula one way (with x_1 = the average height of the women and x_2 = the average height of the men), you find that the men are 8.4% taller than the women. Working the formula the other way (with x_1 = the average height of the men and x_2 = the average height of the women) you find that the women are 7.7% shorter than the men (the numerator is negative). Cole doesn't like that asymmetry. He suggests that the formula be changed to ($100\log_e x_2 - 100\log_e x_1$), where e = 2.1728… is the base of the natural logarithm system. If you like logarithms and you're comfortable working with them, you'll love Cole's article!

Comparisons between percentages that must add to 100

One annoying (to me, anyhow) tendency these days is to compare, by subtraction, the percentage of support for one candidate for political office with the percentage of support for another candidate when they are the only two candidates for that office. For example: "Smith is leading Jones by 80% to 20%, a difference of 60 points." Of course. If Smith has 80%, Jones must have 20% (unless there is a "no preference" option and/or there are any missing data), the difference must be 60%, and why use the word "points"?!

Milo Schield and I have one final disagreement, and it is in regard to this matter. He likes reporting the difference and using the word "points". In Section 5 I will begrudgingly return to a variation of the problem, in which the testing of the significance of the difference between two percentages is of interest, where the two percentages together with a third percentage add to 100.

Ratios vs. differences of percentages that don't have to add to 100

Consider an example (unlike the previous example) where it is reasonable to calculate the ratio of two percentages. Suppose one-half of one percent of males in a population of 100 million males have IQ scores of over 200 and two percent of females in a population of 100 females have IQ scores of over 200. (There are approximately 100 million adult males and approximately 100 million adult females in the United States.) Should we take the ratio of the 2% to the .5% (a ratio of 4 to 1) and claim that the females are four times as smart?

No. There are at least two problems with such a claim. First of all, having a number less than 1 in the denominator and a number greater than 1 in the numerator can produce an artificially large quotient. (If the denominator were 0, the ratio couldn't even be calculated, since you can't divide by 0.) Secondly, does it really matter how large such a ratio is, given that both numerator and denominator are small. Surely it is the difference between those two percentages that is important, not their ratio.

Although in general there are fewer problems in interpreting differences between percentages than there are in interpreting ratios of percentages, when subgroup comparisons are made in addition to an overall comparison, things can get very complicated. The classic case is something called Simpson's Paradox (Simpson, 1951) in which the differences between two overall percentages can be in the opposite direction from differences between their corresponding subgroup percentages. The well-known mathematician John Allen Paulos (2001) provided the following hypothetical (but based upon an actual lawsuit) example:

> "To keep things simple, let's suppose there were only two departments in the graduate school, economics and psychology. Making up numbers, let's further assume that 70 of 100 men (70 percent) who applied to the economics department were admitted and that 15 of 20 women (75 percent) were. Assume also that five out of 20 men (25 percent) who applied to the psychology department were admitted and 35 of 100 women (35 percent) were. Note that in each department a higher percentage of women was admitted.
>
> If we amalgamate the numbers, however, we see what prompted the lawsuit: 75 of the 120 male applicants (62.5 percent) were admitted to the graduate school as a whole whereas only 50 of the 120 female applicants (41.7 percent) were. "

How can that be? The "paradox" arises from the fact that there are unequal numbers of men and women contributing to the percentages (100 men and 20 women for economics; 20 men and 100 women for psychology). The percentages need to be weighted before they are combined into overall figures.

For additional discussions of Simpson's Paradox, see Knapp (1985), Baker and Kramer (2001), Malinas (2001), and Ameringer, Serlin, and Ward (2009).

Reporting of ranges in percentages across studies

In his editorial a few years ago, Cowell (1998) referred to an author's citing of the results of a successful surgical procedure as ranging from 43% to 100%, without mentioning that the 100% was for one successful procedure performed on one patient! He (Cowell) argued, as I have, that the base must always be given along with the percentage.

Other misunderstandings and errors in interpreting percentages

Milo Schield (2000) discussed a number of problems that people have when it comes to percentages in various contexts, especially rates. One example he cites is the claim made by some people that "if X% of A are B, then X% of B are A". No. If you don't believe Schield or me, try various numbers or draw a "Venn diagram" for two overlapping circles, A and B. He has written other interesting articles regarding statistical literacy (or lack of same), one of which (Schield, 2005) deals largely with misunderstandings of percentages. He also put on the internet a test of statistical literacy (Schield, 2002). You can get to it by googling "statistical literacy inventory" and clicking on the first entry. Here are three of the questions on that test (as cited in The Washington Post on February 6, 2009):

> 1. True or False. If a stock decreases 50 percent and then increases by 50 percent, it will be back to its original value.
>
> 2. True or False. If a stock drops from $300 to zero, that is a 300 percent decrease.
>
> 3. A company has a 30 percent market share in the Western US and a 10 percent market share in the Eastern US. What is the company's overall market share in the entire US?

Do you know the answers?

The interesting book, Mathsemantics, by Edward MacNeal (1994), includes a chapter on percentages in which the author discusses a number of errors that he discovered when a group of 196 applicants for positions with his consulting firm were tested. Some of the errors, and some of the reasons that people gave for having made them, are pretty bizarre. For example, when asked to express .814 as a percentage to the nearest whole percent, one person gave as the answer "1/8 %". One of my favorite examples in that chapter is to a person (fortunately nameless) who claimed that Richie Ashburn, a former baseball player with the Philadelphia Phillies, hit "three hundred and fifty percent" in one of his major league seasons. [I'm a baseball nut.] In case you're having trouble figuring out what's wrong with that, I'll help you out. First of all, as you now know (if you already didn't), percentages must be between 0 and 100. Secondly, baseball batting averages are "per thousand" rather than "per hundred". Ashburn actually hit safely about 35% of the time, which, in baseball jargon, is "three fifty", i.e., a proportion of .350.

Speaking of my love for baseball, and going back to Simpson's Paradox, in my article (Knapp, 1985) I provided real data that showed Player A had a higher batting average than Player B against both right-handed and left-handed pitching but had a lower overall batting average. I later discovered additional instances of batting averages that constituted evidence for a transitive case of Simpson's Paradox, i.e., one for which A > B > C against both right-handed and left-handed pitching, but for which A < B < C overall. (The symbol > means "greater than"; < means "less than".)

Section 3: Percentages and probability

What do we mean by the probability of something?

There are several approaches to the definition of probability. The first one that usually comes to mind is the so-called “a priori definition” that is a favorite of teachers of statistics who use coins, dice, and the like to explain probability. In the “a priori” approach the probability of something is the number of ways that something can take place divided by the total number of equally likely outcomes. For example, in a single toss of a coin, the probability of “heads” is the number of ways that can happen (1) divided by the total number of equally likely outcomes (2…”heads” or “tails”), which is 1/2, .50, or 50%, depending upon whether you want to use fractions, proportions, or percentages to express the probability. Similarly, the probability of a “4” in a single roll of a die, is 1 (there is only one side of a die that has four spots) divided by 6 (the total number of sides), which is equal to 1/6, .167, or 16.7% (to three “significant figures”).

But there are problems with that definition. In the first place, it only works for symmetric situations such as the tossing of fair coins and the rolling of unloaded dice. Secondly, it is actually circular, since it defines probability in terms of “equally likely”, which is itself a probabilistic concept. Such concerns have led to a different definition, the “long-run empirical definition”, in which the probability of something is the number of ways that something did happen (note the use of “did” rather than “can”) divided by the total number of things that happened. This definition works for things like thumbtack tosses (what is the probability of landing with its point up?) as well as for coins, dice, and many other probabilistic contexts. The price one pays, however, is the cost of actually carrying out the empirical demonstration of tossing a thumbtack (or tossing a coin or rolling a die…) a large number of times. And how large is large??

There is a third (and somewhat controversial) “subjective definition” of probability that is used in conjunction with Bayesian statistics (an approach to statistics associated with a famous equation derived by the clergyman/mathematician Rev. Thomas Bayes who lived in the 18th century). Probability is defined as a number between 0 and 1 (for fractions and proportions) or between 0 and 100 (for percentages) that is indicative of a person’s “strength of conviction” that something will take place (note the use of “will” rather than either “can” or “did”).

An example that illustrates various definitions of probability, especially the subjective definition, is the question of the meaning of “the probability of rain”. There recently appeared an article in The Journal of the American Meteorological Society, written by Joslyn, Nadav-Greenberg, and Nichols (2009), that was devoted entirely to that problem. (Weather predictions in terms of percentages and probabilities have been around for about a century---see, for example, Hallenbeck, 1920.)

I've already said that I favor the reporting of parts out of wholes in terms of percentages rather than in terms of fractions or proportions. I also favor the use of playing cards rather than coins or dice to explain probabilities. That should come as no surprise to you, since in the previous section I referred to my Learning statistics through playing cards book (Knapp, 1996), which, by the way, also concentrates primarily on percentages.

The probability of not-something

If P is the probability that something will take place, in percentage terms, then 100 - P is the probability that it will not take place. For example, if you draw one card from an ordinary deck of cards, the probability P that it's a spade is 13/52, or 1/4, or .25, or 25%. The probability that it's not a spade is 100 – 25 = 75%, which can also be written as 39/52, or 3/4, or .75.

Probabilities vs. odds

People are always confusing probabilities and odds. If P is the probability of something, in percentage terms, then the odds in favor of that something are P divided by (100 - P); and the odds against it are (100 - P) divided by P. The latter is usually of greater interest, especially for very small probabilities. For example, if you draw one card from an ordinary deck of cards, the probability P that it's a spade, from above, is 13/52, or 1/4, or .25, or 25%. The odds in favor of getting a spade are 25 divided by (100 – 25), or "1 in 3"; the odds against it are (100 – 25) divided by 25, or "3 to 1". [In his book, Probabilities and life , the French mathematician Emile Borel (1962) claims that we act as though events with very small probabilities never occur. He calls that "the single law of chance".]

There are actually two mistakes that are often made. The first is the belief that probabilities and odds are the same thing (so some people would say that the odds of getting a spade are 1/4, or 25%). The second is the belief that the odds against something are merely the reciprocal of its probability (so they would say that the odds against getting a spade are 4 to 1).

"Complex" probabilities

The examples just provided were for "simple" situations such as tossing a coin once, rolling a die once, or drawing one card from a deck of cards, for which you are interested in a simple outcome. Most applications of probability involve more complicated matters. If there are two "events", A and B, with which you are concerned, the probability that either of them will take place is the sum of their respective probabilities, if the events are mutually exclusive, and the probability that both of them will take place is the product of their respective probabilities, if the events are independent. Those are both mouthfuls, so let's take lots of examples (again using playing cards):

1. If you draw one card from a deck of cards, what is the probability that it is either a spade or a heart?

Since getting a spade and getting a heart are mutually exclusive (a card cannot be a spade and a heart), the probability of either a spade or a heart is the probability of a spade plus the probability of a heart, which is equal to 13/52 plus 13/52 = 26/52 = 1/2, or 50%. [It's generally easier to carry out the calculations using fractions, but to report the answer in terms of percentages.]

2. If two cards are drawn from a deck of cards, what is the probability that they are both spades?

This problem is a bit more difficult. We must first specify whether or not the first card is replaced in the deck before the second card is drawn. If the two "events", spade on first card and spade on second card, are to be independent (i.e., that the outcome of the second event does not depend upon the outcome of the first event) the first card must be replaced. If so, the desired probability is 1/4 for the first card times 1/4 for the second card, which is equal to 1/16 or 6.25%. If the first card is not replaced, the probability is 13/52 times 12/51 = 1/4 times 4/17 = 1/17 or 5.88%.

3. If two cards are drawn from a deck of cards, what is the probability that either of them is a spade?

This is indeed a complex problem. First of all, we need to know if the cards are to be drawn "with replacement" (the first card is returned to the deck before the second card is drawn) or "without replacement" (it isn't). Secondly, we need to specify whether "either" means "one but not both" or "one or both". Let us consider just one of the four combinations. (I'll leave the other three as exercises for the curious reader!)

If the drawing is with replacement and "either" means "one but not both", the possibilities that are favorable to getting a spade are "spade on first draw, no spade on second draw" and "no spade on first draw, spade on second draw". Those probabilities are, using the "or" rule in conjunction with the "and" rule, (1/4 times 3/4) plus (3/4 times 1/4), i.e., 3/16 + 3/16, or 6/16, or 3/8, or 37.5%.

4. In his other delightful book, How to take a chance, Darrell Huff (1959) discusses the probability of having two boys out of four children, if the probability of a boy and the probability of a girl are equally likely and independent of one another. Many people think the answer is 2/4 = 1/2 = .50 = 50%. Huff not only shows that the correct answer is 6/16 = 3/8 = .375 = 37.5%, but he (actually Irving Geis) illustrates each of the permutations. You can look it up (as Casey Stengel used to say). This is a conceptually different probability problem than the previous one. It just happens to have the same answer.

The birthday problem

There is a famous probability problem called “The Birthday Problem”, which asks: If n people are gathered at random in a room, what is the probability that at least two of them have the same birthday (same month and day, but not necessarily same year)? It turns out that for an n of 23 the probability is actually (and non-intuitively) greater than 50%, and for an n of 70 or so it is a virtual certainty! See, for example, the website www.physics.harvard.edu/academics/undergrad/probweek/sol46 and my favorite mathematics book, Introduction to finite mathematics (Kemeny, Snell, and Thompson, 1956). The best way to carry out the calculation is to determine the probability that NO TWO PEOPLE will have the same birthday (using the generalization of the “and” rule---see above), and subtract that from 100 (see the probability of not-something).

Risks

A risk is a special kind of percentage, and a special kind of probability, which is of particular interest in epidemiology. The risk of something, e.g., getting lung cancer, can be calculated as the number of people who get something divided by the total number of people who “could” get that something. (The risk of lung cancer, actually the “crude” risk of lung cancer, is actually rather low in the United States, despite all of the frightening articles about its prevalence and its admittedly tragic consequences.)

There is also an attributable risk (AR), the difference between the percentage of people in one group who get something and the percentage of people in another group who get that something. [N.B. "Attributable" doesn't necessarily mean causal.] In Section 1 I gave a hypothetical example of the percentage of smokers who get lung cancer minus the percentage of non-smokers who get lung cancer, a difference of 10% - 2% = 8%.

And then there is a relative risk (RR), the ratio of the percentage of people in one group who get something to the percentage of people in another group who get that something. Referring back again to smoking and lung cancer, my hypothetical example produced a ratio of 10%/ 2%, or “5 to 1”.

Risks need not only refer to undesirable outcomes. The risk of making a million dollars by investing a thousand dollars, for example, is a desirable outcome (at least for the “winner”).

The methodological literature is replete with discussions of the minimum value of relative risk that is worthy of serious consideration. The most common value is "2.00 or more", especially when applying relative risks to individual court cases. Those same sources often have corresponding discussions of a related concept, the probability of causality [causation], PC, which is defined as 1 - 1/RR. If the

RR threshold is 2.00, then the PC threshold is .50, or 50%. See Parascandola (1998); Robins (2004); Scheines (2008); and Swaen and vanAmelsvoort (2009) for various points of view regarding both of those thresholds.

Like probabilities in general, risks can be expressed in fractions, proportions, or percentages. Biehl and Halpern-Felsher (2001) recommend using percentages. (Yay!)

Sensitivity and specificity

In medical diagnostic testing there are two kinds of probability that are of interest:

1. The probability that the test will yield a "positive" result (a finding that the person being tested has the disease) if the person indeed has the disease. Such a probability is referred to as the sensitivity of the test.

2. The probability that the test will yield a "negative" result (a finding that the person being tested does not have the disease) if the person indeed does not have the disease. Such a probability is referred to as the specificity of the test.

We would like both of those probabilities to be 100% (a perfect test). Alas, that is not possible. No matter how much time and effort go into devising diagnostic tests there will always be "false positives" (people who don't have the disease but are said to have it) and "false negatives" (people who do have the disease but are said to not have it). [Worse yet, as you try to improve the test by cutting down on the number of false positives you increase the number of false negatives, and vice versa.] Its sensitivity is the probability of a "true positive"; its specificity is the probability of a "true negative".

There is something called Youden's Index (Youden, 1950), which combines sensitivity and specificity. Its formula can be written in a variety of ways, the simplest being J = sensitivity + specificity – 100. Theoretically it can range from a low of -100 (no sensitivity and no specificity) to a high of 100 (perfect test), but is typically around 80 (e.g., when both sensitivity and specificity are around 90%). [A more interesting re-formulation of Youden's Index can be written as J = (100-sensitivity) – (100-specificity), i.e., the difference between the true positive rate and the false positive rate.]

For example (an example given by Gigerenzer, 2002, and pursued further in Gigerenzer et al., 2007 with slightly changed numbers), a particular mammography screening test might have a sensitivity of 90% and a specificity of 91% (those are both high probabilities, but not 100%). Suppose that the probability of getting breast cancer is 1% (10 chances in 1000). For every group of 1000 women tested, 10 of whom have breast cancer and 990 of whom do not, 9 of those who have it will be correctly identified (since the test's sensitivity is 90%, and 90% of 10 is 9). For the 990 who do not have breast cancer, 901 will be correctly identified (since the test's specificity is 91%, and 91% of 990 is 901). Therefore there will be 9 true positives, 901 true negatives, 89 false positives, and 1 false negative.

Gigerenzer goes on to point out the surprising conclusion that for every positive finding only about 1 in 11 (9 out of the 98 "positives"), or approximately 9%, is correct. He argues that if a woman were to test positive she needn't be overly concerned, since the probability that she actually has breast cancer is only 9%, with the corresponding odds of "89 to 9" (almost 10 to 1) against it. A further implication is that it might not be cost-effective to use diagnostic tests with sensitivities and specificities as "low" as those.

In his delightful book entitled Innumeracy (note the similarity to the word "illiteracy"), Paulos (1988) provides a similar example (p. 66) that illustrates how small the probability typically is of having a disease, given a positive diagnosis.

For another (negative) commentary regarding cancer screening, see the JNCI editorial by Woloshin and Schwartz (2009).

Probabilistic words and their quantification in terms of percentages

The English language is loaded with words such as "always", "never", "sometimes", "seldom", etc. [Is "sometimes" more often than "seldom"; or is it the other way 'round?] There is a vast literature on the extent to which people ascribe various percentages of the time to such words. The key reference is an article that appeared in the journal Statistical Science , written by Mosteller and Youtz (1990; see also the several comments regarding that article in the same journal and the rejoinder by Mosteller and Youtz). They found, for example, that across 20 different studies the word "possible" received associated percentages throughout the entire scale (0% to 100%), with a median of 38.5%. (Some people didn't even ascribe 0% to "never" and 100% to "always". In an earlier article in the nursing research literature, Damrosch and Soeken (1983) reported a mean of 45.21% for "possible", a mean of 13.71% for "never" and a mean of 91.35% for "always".) Mosteller and Youtz quote former president Gerald R. Ford as having said that there was "a very real possibility" of a swine flu epidemic in 1976-77. In a previous article, Mosteller (1976) estimated the public meaning of "a very real possibility" to be approximately 29%, and Boffey (1976) had claimed the experts put the probability of a swine flu epidemic in 1976 -77 somewhat lower than that. Shades of concerns about swine flu in 2009!

There is a related matter in weather forecasting. Some meteorologists (e.g., Jeff Haby) have suggested that words be used instead of percentages. In a piece entitled "Using percentages in forecasts" on the weatherprediction.com website, he argues that probabilistic expressions such as "there is a 70% chance of a thunderstorm" should be replaced by verbal expressions such as "thunderstorms will be numerous". (See also the articles by Hallenbeck, 1920, and by Joslyn, et al., 2009, referred to above.) Believe it or not, there is an online program for doing so, put together by Burnham and Schield in 2005. You can get to it at the www.StatLit.org website.

There is also an interesting controversy in the philosophical literature regarding the use of probabilistic words in the analysis of syllogisms, rather than the more usual absolute words such as “All men are mortal; Socrates is a man; therefore, Socrates is mortal”. It started with an article in the Notre Dame Journal of Formal Logic by Peterson (1979), followed by an article by Thompson (1982), followed by an unpublished paper by Peterson and Carnes, followed by another article by Thompson (1986), and ending (I think) with a scathing article by Carnes and Peterson (1991). The controversy revolves around the use of words like “few”, “many”, and “most” in syllogisms.   An example given in Thompson’s second article (1986) is:

Almost 27% of M are not P.
Many more than 73% of M are S.
Therefore, some S are not P.

Is that a valid argument? (You decide.)

Chance success

I take an 80-item true-false test and I answer 40 of them correctly. Should I be happy about that? Not really. I could get around 40 (= 50%) without reading the questions, if the number of items for which “true” is the right answer is approximately equal to the number of items for which “false” is the right answer, no matter what sort of guessing strategy I might employ (all true, all false, every other one true, etc.)

The scoring directions for many objective tests (true-false, multiple-choice, etc.) often stipulate that every score on such tests be corrected for chance success. The formula is $R – W/(k-1)$, where R is the number of right answers, W is the number of wrong answers, and k is the number of choices. For the example just given, $R = 40$, $W = 40$, $k = 2$, so that my score would be $40 – 40/(2-1) = 40 – 40 = 0$, which is what I deserve!

For more on chance success and the correction for guessing, see Diamond and Evans (1973).

Percentages and probability in the courtroom

As you might expect, probability (in terms of either percentages or fractions) plays an important role in jury trials. One of the most notorious cases was that of the famous professional football player and movie star, O.J. Simpson, who was accused in 1994 of murdering his wife, Nicole, and her friend, Ronald Goldman.

There was a great deal of evidence regarding probabilities that was introduced in that trial, e.g., the probability that an individual chosen at random would wear a size 12 shoe AND have blood spots on the left side of his body. (Simpson wears size 12; the police found size 12 footprints nearby, with blood spots to the left of the footprints. Simpson claimed he cut his finger at home.) For more on this, see the article by Merz and Caulkins (1995); the commentary by John Allen Paulos (1995---yes, that Paulos), who called it a case of “statisticide”; and the letters by defense attorney Alan Dershowitz (1995, 1999). [Simpson was acquitted.]

Several years prior to the Simpson case (in 1964), a Mrs. Juanita Brooks was robbed in Los Angeles by a person whom witnesses identified as a white blonde female with a ponytail, who escaped in a yellow car driven by a black male with a mustache and a beard. Janet and Malcolm Collins, an inter-racial couple who fit those descriptions, were arrested and convicted of the crime, on the basis of estimates of the following probabilities for persons drawn at random:

P(yellow car) = 1/10 = 10%
P(male with mustache) = 1/4 = 25%
P(female with hair in ponytail) = 1/10 = 10%
P(female with blonde hair) = 1/3 = 33 1/3 %
P(black male with beard) = 1/10 = 10%
P(inter-racial couple in car) = 1/1000 = .1%

Product of those probabilities = 1/12,000,000 = .00000833%
[The convictions were overturned because there was no empirical evidence provided for those probabilities and their independence. Oy.]

There was another interesting case, Castenada v. Partida, involving the use of percentages in the courtroom, which was cited in an article by Gastwirth (2005). It concerned whether or not Mexican-Americans were discriminated against in the jury-selection process. (They constituted only 39% of the jurors, although they constituted 79.1% of the relevant population and 65% of the adults in that population who had some schooling.)

My favorite percentages and probability example

Let me end this chapter by citing my favorite example of misunderstanding of probabilities, also taken from Paulos (1988):

> “Later that evening we were watching the news, and the TV weather forecaster announced that there was a 50 percent chance of rain for Saturday and a 50 percent chance for Sunday, and concluded that there was therefore a 100 percent chance of rain that weekend.” (p. 3)

I think that says it all.

Section 4: Sample percentages vs. population percentages

Almost all research studies that are concerned with percentages are carried out on samples (hopefully random) taken from populations, not on entire populations. It follows that the percentage in the sample might not be the same as the percentage in the population from which the sample is drawn. For example, you might find that in a sample of 50 army recruits 20 of them, or 40%, are Catholics. What percentage of all army recruits is Catholic? 40%? Perhaps, if the sample "mirrors" the population. But it is very difficult for a sample to be perfectly representative of the population from which it is drawn, even if it is randomly drawn.

The matter of sampling error, wherein a sample statistic (such as a sample percentage) may not be equal to the corresponding population parameter (such as a population percentage) is the basic problem to which statistical inference is addressed. If the two are close, the inference from sample to population is strong; if they're not, it's weak. How do you make such inferences? Read on.

Point estimation

A "single-best" estimate of a population percentage is the sample percentage, if the sample has been drawn at random, because the sample percentage has been shown to have some nice statistical properties, the most important of which is that is "unbiased". "Unbiased" means that the average of the percentages for a large number of repeated samples of the same size is equal to the population percentage, and therefore it is a "long-run" property. It does NOT mean that you'll hit the population percentage on the button each time. But you're just as likely to be off "on the high side" as you are to be off "on the low side". How much are you likely to be off? That brings us to the concept of a standard error.

Standard error

A standard error of a statistic is a measure of how off you're likely to be when you use a sample statistic as an estimate of a population parameter. Mathematical statisticians have determined that the standard error of a sample percentage P is equal to the square root of the product of the population percentage and 100 minus the population percentage, divided by the sample size n, if the sample size is large. But you almost never know the population percentage (you're trying to estimate it!). Fortunately, the same mathematical statisticians have shown that the standard error of a sample percentage is APPROXIMATELY equal to the square root of the product of the sample percentage and 100 minus the sample percentage, divided by the sample size n; i.e.,

$\text{S.E.} \approx \surd\, P\,(100 - P)/\,n$ [the symbol ≈ means approximately equal to]

For example, if you have a sample percentage of 40 for a sample size of 50, the standard error is $\sqrt{40(60)/50}$, which is equal to 6.93 to two decimal places, but let's call it 7. So you would be likely off by about 7% (plus or minus) if you estimate the population percentage to be 40%.

Edgerton (1927) constructed a clever "abac" (mathematical nomogram) for reading off a standard error of a proportion (easily convertible to a percentage), given the sample proportion and the sample size. [Yes, that was 1927… 89 years ago!] It's very nice. There are several other nomograms that are useful in working with statistical inferences for percentages (see, for example, Rosenbaum, 1959). And you can even get a business-card-size chart of the standard errors for various sample sizes at the www.gallup-robinson.com website.

Interval estimation (confidence intervals)

Since it is a bit presumptuous to use just one number as an estimate of a population percentage, particularly if the sample size is small (and 50 is a small sample size for a survey), it is recommended that you provide two numbers within which you believe the population percentage to lie. If you are willing to make a few assumptions, such as sample percentages are normally distributed around population percentages, you should "lay off" two (it's actually 1.96, but call it two) standard errors to the right and left of the sample percentage to get a "95% confidence interval" for the population percentage, i.e., an interval that you are 95% confident will "capture" the unknown population percentage. (Survey researchers usually call two standard errors "the margin of error",) For our example, since the standard error is 7%, two standard errors are 14%, so 40% ± 14%, an interval extending from 26% to 54%, constitutes the 95% confidence interval for the population percentage. 40% is still your "single-best" estimate, but you're willing to entertain the possibility that the population percentage could be as low as 26% and as high as 54%. It could of course be less than 26% or greater than 54%, but you would be pretty confident that it is not.

Since two standard errors = $2 \sqrt{P (100 - P) / n}$ and $P(100 - P)$ is close to 2500 for values of P near 50, a reasonably good approximation to the margin of error is $100/ \sqrt{n}$.

I said above that the formula for the standard error of a percentage is a function of the population percentage, but since that is usually unknown (that's what you're trying to estimate) you use the sample percentage instead to get an approximate standard error. That's OK for large samples, and for sample and population percentages that are close to 50. A situation where it very much does matter whether you use the sample percentage or the population percentage in the formula for the standard error is in the safety of clinical trials for which the number of adverse events is very small. For example, suppose no adverse events occurred in a safety trial for a sample of 30 patients. The sample P = 0/30

= 0%. Use of the above formula for the standard error would produce a standard error of 0, i.e., no sampling error! Clearly something is wrong there. You can't use the sample percentage, and the population percentage is unknown, so what can you do? It turns out that you have to ask what is the worst that could happen, given no adverse events in the sample. The answer comes from "The rule of 3" (sort of like "The rule of 72" for interest rates; see Section 1). Mathematical statisticians have shown that the upper 95% confidence bound for zero events in a sample is approximately 3/n in terms of a proportion, or approximately 300/n in terms of a percentage. (See Jovanovic & Levy, 1997, and van Belle, 2002 regarding this intriguing result. The latter source contains all sorts of "rules of thumb", some of which are very nice, but some of the things that are called rules of thumb really aren't, and there are lots of typos.) The lower 95% confidence bound is, of course, 0. So for our example you could be 95% confident that the interval from 0% to 10% (300/30 = 10) "captures" the percentage of adverse events in the population from which the sample has been drawn.

The lower 95% confidence bound if all events in a sample are "adverse" is 100 - 300/n and the upper bound is 100. For our example you could be 95% confident that the interval from 90% to 100% "captures" the percentage of adverse events in the population from which the sample has been drawn.

There is nothing special about a 95% confidence interval, other than the fact that it is conventional. If you want to have greater confidence than 95% for a given sample size you have to have a wider interval. If you want to have a narrower confidence interval you can either settle for less confidence or take a larger sample size. [Do you follow that?] But the only way you can be 100% confident of your inference is to have an interval that goes from 0 to 100, i.e., the entire scale!

One reason why many researchers prefer to work with proportions rather than percentages is that when the statistic of interest is itself a percentage it is a bit awkward to talk about a 95% confidence interval for a %. But I don't mind doing that. Do you?

In Section1 I cited an article in the Public Opinion Quarterly by S.S. Wilks (1940a) regarding opinion polling. In a supplementary article in that same issue he provided a clear exposition of confidence intervals for single percentages and for differences between two percentages (see the following section for the latter matter). An article two years later by Mosteller and McCarthy (1942) in that journal shed further light on the estimation of population percentages. [I had the personal privilege of "TAing" for both Professor Mosteller and Professor McCarthy when I was doing my doctoral study at Harvard in the late 1950s. Frederick Mosteller, like S.S. Wilks, was an exceptional statistician.]

For a very comprehensive article concerning confidence intervals for proportions, see Newcombe (1998a). He actually compared SEVEN different methods for getting confidence intervals for proportions, all of which are equally appropriate for percentages.

Hypothesis testing (significance testing)

Another approach to statistical inference (and until recently far and away the most common approach) is the use of hypothesis testing. In this approach you start out by making a guess about a parameter, collect data for a sample, calculate the appropriate statistic, and then determine whether or not your guess was a good one. Sounds complicated, doesn't it? It is, so let's take an example.

Going back to the army recruits, suppose that before you carried out the survey you had a hunch that about 23% of the recruits would be Catholic. (You read somewhere that 23% of adults in the United States are Catholic, and you expect to find the same % for army recruits.) You therefore hypothesize that the population percentage is equal to 23. Having collected the data for a sample of 50 recruits you find that the percentage Catholic in the sample is 40. Is the 40 "close enough" to the 23 so that you would not feel comfortable in rejecting your hypothesis? Or are the two so discrepant that you can no longer stick with your hypothesis? How do you decide?

Given that "the margin of error" for a percentage is two standard errors and for your data two standard errors is approximately 14%, you can see that the difference of 17% between the hypothesized 23% and the obtained 40% is greater than the margin of error, so your best bet is to reject your hypothesis (it doesn't reconcile with the sample data). Does that mean that you have made the correct decision? Not necessarily. There is still some (admittedly small) chance that you could get 40% Catholics in a sample of 50 recruits when there are actually only 23% Catholics in the total population of army recruits.

We've actually cheated a little in the previous paragraph. Since the population percentage is hypothesized to be 23, the 23 should be used to calculate the standard error rather than the 40. But for most situations it shouldn't matter much whether you use the sample percentage or the hypothesized population percentage to get the standard error. [√40(60)/50 = 6.93 is fairly close to √23(77)/50 = 5.95, for example.]

The jargon of hypothesis testing

There are several technical terms associated with hypothesis testing, similar to those associated with diagnostic testing (see the previous section):

The hypothesis that is tested is often called a null hypothesis. (Some people think that a null hypothesis has to have zero as the hypothesized value for a parameter. They're just wrong.)

There is sometimes a second hypothesis that is pitted against the null hypothesis (but not for our example). It is called, naturally enough, an alternative hypothesis.

If the null hypothesis is true (you'll not know if it is or not) and you reject it, you are said to have made a Type I error.

If the null hypothesis is false (you'll not know that either) and you fail to reject it, you are said to have made a Type II error.

The probability of making a Type I error is called the level of significance and is given the Greek symbol α.
The probability of making a Type II error doesn't usually have a name, but it is given the Greek symbol β.

1 – β is called the power of the hypothesis test.

Back to our example

Null hypothesis: Population percentage = 23

If the null hypothesis is rejected, the sample finding is said to be "statistically significant". If the null hypothesis is not rejected, the sample finding is said to be "not statistically significant".

Suppose you reject that hypothesis, since the corresponding statistic was 40, but it (the null hypothesis) is actually true. Then you have made a Type I error (rejecting a true null hypothesis).

If you do not reject the null hypothesis and it's false (and "should have been rejected") then you would have made a Type II error (not rejecting a false null hypothesis).

The level of significance, α, should be chosen before the data are collected, since it is the "risk" that one is willing to run of making a Type I error. Sometimes it is not stated beforehand. If the null hypothesis is rejected, the researcher merely reports the probability of getting a sample result that is even more discrepant from the null hypothesis than the one actually obtained if the null hypothesis is true. That probability is called a p value, and is typically reported as p < .05 (i.e., 5%), p < .01 (i.e., 1%), or p < .001 (i.e., .1%) to indicate how unlikely the sample result would be if the null hypothesis is true.

β and/or power (= 1 – β) should also be stated beforehand, but they depend upon the alternative hypothesis, which is often not postulated. [In order to draw the "right" sample size to test a null hypothesis against an alternative hypothesis, the alternative hypothesis must be explicitly stated. Tables and formulas are available (see, for example, Cohen, 1988) for determining the "optimal" sample size for a desired power.]

The connection between interval estimation and hypothesis testing

You might have already figured out that you can do hypothesis testing for a percentage as a special case of interval estimation. It goes like this:

1. Get a confidence interval around the sample percentage.

2. If the hypothesized value for the population percentage is outside that interval, reject it; if it's inside the interval, don't reject it.

[Strictly speaking, you should use the sample percentage to get the standard error in interval estimation and you should use the hypothesized population percentage to get the standard error in hypothesis testing--see above--but let's not worry about that here.]

Neat, huh? Let's consider the army recruits example again. The sample percentage is 40. The 95% confidence interval goes from 26 to 54. The hypothesized value of 23 falls outside that interval. Therefore, reject it. (That doesn't mean it's necessarily false. Remember Type I error!)

It's all a matter of compatibility. The sample percentage of 40 is a piece of real empirical data. You know you got that. What you don't know, but you wish you did, is the population percentage. Percentages of 26 to 54 are compatible with the 40, as indicated by the 95% confidence you have that the interval from 26 to 54 "captures" the population percentage. 23 is just too far away from 40 to be defensible.

van Belle (2002) takes an idiosyncratic approach to interval estimation vs. hypothesis testing. He claims that you should use the hypothesis testing approach in order to determine an appropriate sample size before the study is carried out; but you should use interval estimation to report the results after the study has been carried out. I disagree. There are the same sorts of sources for the determination of sample size in the context of interval estimation as there are for the determination of sample size in the context of hypothesis testing. (See the reference to Walker & Lev, 1953 in the following paragraphs on sample size.) In my opinion, if you have a hypothesis to test (especially if you have both a null and an alternative hypothesis), you should use hypothesis-testing procedures for the determination of sample size. If you don't, go the interval estimation route all the way.

Caution: Using interval estimation to do hypothesis testing can be more complicated than doing hypothesis testing directly. I will provide an example of such a situation in Section 5 in conjunction with statistical inferences for relative risks.

Sample size

In all of the foregoing it was tacitly assumed that the size of the sample was fixed and the statistical inference was to be based upon the sample size that you were "stuck with". But suppose that you were interested in using a sample size that would be optimal for carrying out the inference from a sample percentage to the percentage in the population from which the sample had been drawn. There are rather straightforward procedures for so doing. All you need do is decide beforehand how much confidence you want to have when you get the inferred interval, how much error you can tolerate in making the inference, have a very rough approximation of what the population percentage might be, and use the appropriate formula, table, or internet routine for determining what size sample would satisfy those specifications.

Let's take an example. Suppose you were interested in getting a 95% confidence interval (95% is conventional), you don't want to be off by more than 5%, and you think the population percentage is around 50 (that's when the standard error is largest, so that's the most "conservative" estimate). The formula for the minimum optimal sample size is:

$n \approx 4z^2 P(100-P)/W^2$ [see, for example, Walker and Lev (1953, p. 70)]

where P is your best guess, W is the width of the confidence interval, and z is the number of standard errors you need to "lay off" to the right and to the left of the sample P (z comes from the normal, bell-shaped sampling distribution). Substituting the appropriate values in that formula (z is approximately equal to 2 for 95% confidence) you find that n is equal to $4(2)^2\ 50(100-50)/10^2 = 400$. If you draw a sample of less than 400 you will have less than 95% confidence when you get the sample P and construct the interval. If you want more confidence than 95% you'll need to lay off more standard errors and/or have a larger n (for three standard errors you'll need an n of about 900). If you want to stick with 95% confidence but you can tolerate more error (say 10% rather than 5%, so that W = 20), then you could get away with an n of about 100.

The Dimension Research, Inc. website actually does all of the calculations for you. Just google "dimension research calculator", click on the first entry that comes up, and click on "sample size for proportion" on the left-hand-side menu . Then select a confidence interval, enter your "best-guess" corresponding percentage P, and your tolerable ½ W, click the Calculate button, and Shazam! You've got n.

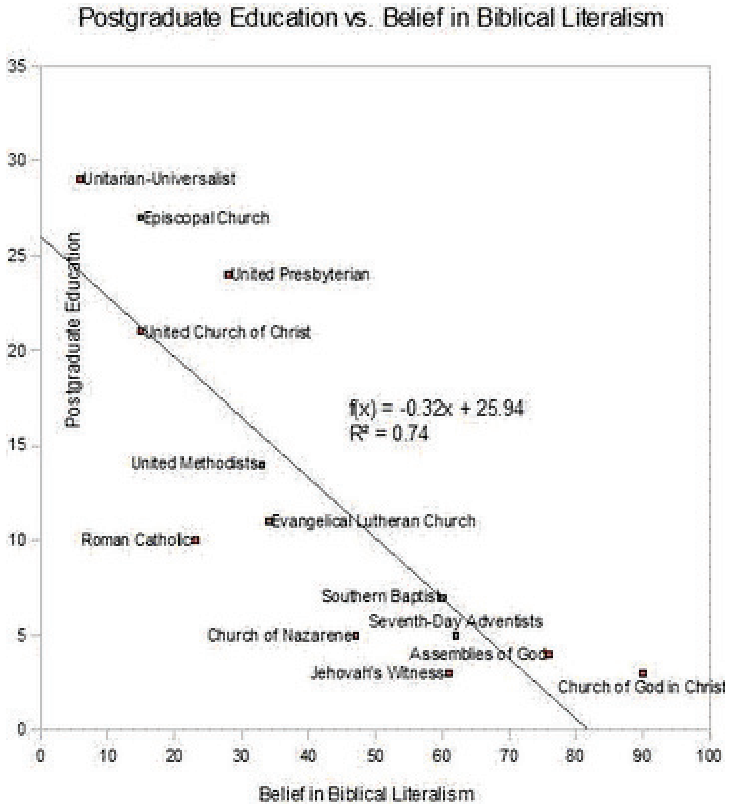
van Belle (2002) claims that you should have a sample size of at least 12 when you construct a confidence interval. He provides a diagram that indicates the width of an interval is very poor up to an n of 12 but starts to level off thereafter.

Percentage transformations

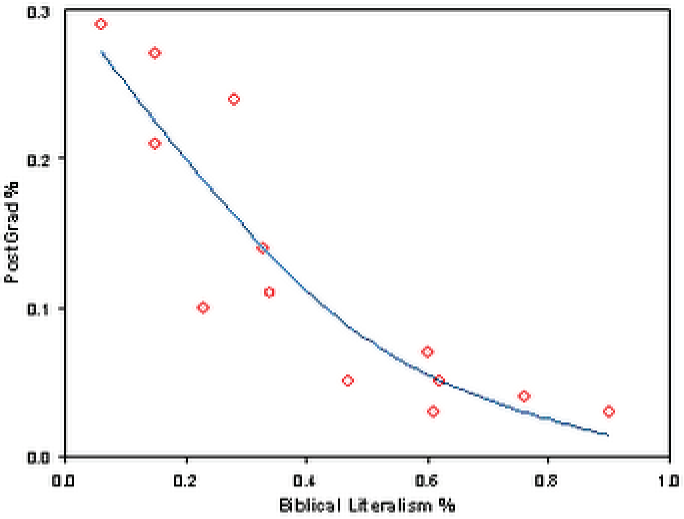
One of the problems when carrying out statistical inferences for percentages is the fact that percentages are necessarily “boxed in” between 0 and 100, and often have rather strange distributions across aggregates for which they have been computed. There can’t be less than 0% and there can’t be more than 100%, so if most of the observations are at the high end of the scale (large percentages) or at the low end of the scale (small percentages) it is almost impossible to satisfy the linearity and normal distribution assumptions that are required for many inferential tests.

Consider the following example taken from the Ecstathy website:

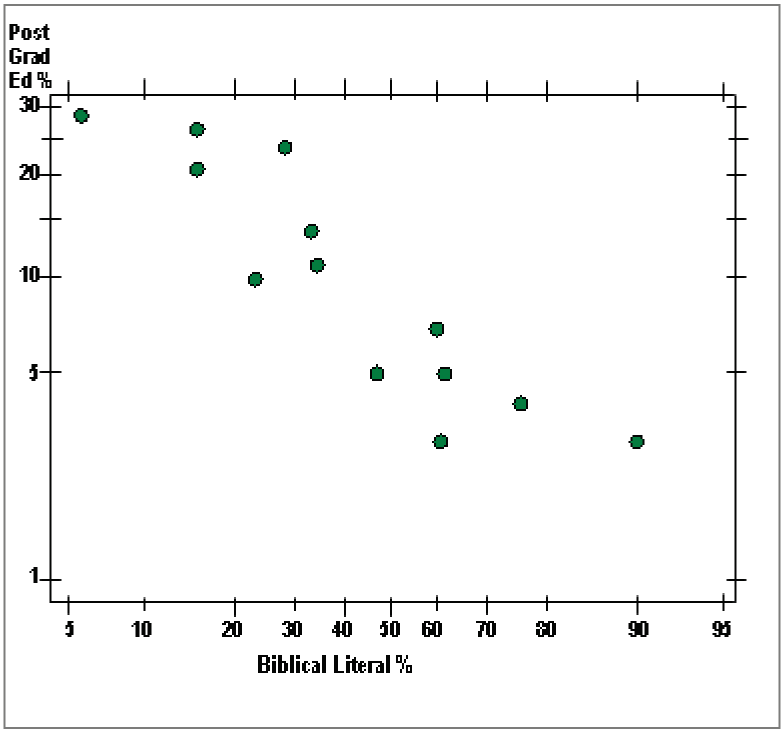
You have data regarding % Postgraduate Education and % Belief in Biblical Literalism for members of 13 religious denominations (Unitarian-Universalist, Episcopal Church, United Presbyterian, United Church of Christ, United Methodist, Evangelical Lutheran Church, Roman Catholic, Southern Baptist, Seventh Day Adventist, Church of Nazarene, Assemblies of God, Jehovah’s Witness, and Church of God in Christ), and you’re interested in the relationship between those two variables. You plot the data as depicted in the following scatter diagram (which includes the “best-fitting” line and the regression statistics:



Here is the plot without the names of the religions superimposed (and with proportions rather than percentages, but that doesn't matter):



You would like to use Pearson's product-moment correlation coefficient to summarize the relationship and to make an inference regarding the relationship in the population of religious denominations from which those 13 have been drawn (assume that the sample is a simple random sample, which it undoubtedly was not!). But you observe that the plot without the names is not linear (it is curvilinear) and the assumption of bivariate normality in the population is also not likely to be satisfied. What are you to do? The recommendation made by the bloggers at the website is to transform both sets of percentages into logits (which are special types of logarithmic transformations), plot the logits, and carry out the analysis in terms of the logits of the percentages rather than in terms of the percentages themselves. It works; here's the plot (this one looks pretty linear to me):



There are transformations of percentages other than logits that have been recommended in the methodological literature--see, for example, the articles by Zubin (1935), by Finney (1947; 1975), and by Osborne (2002). Zubin even provided a handy-dandy table for converting a percentage into something he called t or T (not the t of the well-known t test, and not the T of T scores). Nice.

The classic case of inferences regarding single percentages

You manufacture widgets to be sold to customers. You worry that some of the widgets might be defective, i.e., you are concerned about "quality control". What should you do? If the widgets are very small objects (such as thumbtacks) that are made by the thousands in an assembly-line process, the one thing you can't afford to do is inspect each and every one of them before shipping them out. But you can use a technique that's called acceptance sampling, whereby you take a random sample of, say, 120 out of 2000 of them, inspect all of the widgets in the sample, determine the percentage of defectives in the sample, and make a judgment regarding whether or not that percentage is “acceptable”.

For example, suppose you claim (hope?) that your customers won’t complain if there are 2% (= 40) or fewer defectives in the “lot” of 2000 widgets that they buy. You find there are 3 defectives (1.67%) in the sample. Should you automatically accept the lot (the population) from which the sample has been drawn? Not necessarily. There is some probability that the lot of 2000 has more than 2% defectives even though the sample has only 1.67%. This is the same problem that was discussed in a different context (see above) regarding the percentage of army recruits that is Catholic. Once again, you have three choices: (1) get a point estimate and use it (1.67%) as your single-best estimate; (2) establish a confidence interval around that estimate and see whether or not that interval “captures” the tolerable 2%; or (3) directly test the 2% as a null hypothesis.

There is an excellent summary of acceptance sampling available at myphliputil.pearsoncmg.com/student/bp_heizer...7/ct02.pdf. For the problem just considered, it turns out that the probability of acceptance is approximately .80 (i.e., an 80% probability). I used the same numbers that they do, their “widgets” are batteries, and they take into account the risk to the customer (consumer) as well as the risk to the manufacturer (producer).

A great website for inferences regarding percentages in general

The West Chester University website has an excellent collection of discussions of statistical topics. Although that website is intended primarily for students who are taking college courses online, any interested parties can download any of the various sections. Section 7_3 is concerned with the finite population correction that should be used for inferences regarding percentages for samples drawn from “small”, i.e., finite, populations. See also Krejcie & Morgan, 1970; Buonaccorsi, 1987; and Berry, Mielke, and Helmericks, 1988 for such inferences. (vanBelle (2002) argues that the correction can usually be ignored.) The website’s name is:
http://courses.wcupa.edu/rbove/Berenson/CD-ROM%20Topics/Section 7_3

Section 5: Statistical inferences for differences between percentages and ratios of percentages

In the previous section I talked about statistical inferences for a single percentage. Such inferences are fairly common for survey research but not for other kinds of research, e.g., experimental research in which two or more "treatments" are compared with one another. The inference of greatest interest in experimental research is for the difference between two statistics or the ratio of two statistics, e.g., the percentage of people in "the experimental group" who do (or get) something and the percentage of people in "the control group" who do (or get) something. The "something" is typically a desirable outcome such as "passed the course" or an undesirable outcome such as "died".

Differences between percentages

Just as for a single percentage, we have our choice of point estimation, interval estimation, or hypothesis testing. The relevant point estimate is the difference between the two sample percentages. Since they are percentages, their difference is a percentage. If one of the percentages is the % in an experimental group that "survived" (they got the pill, for example), and the other percentage is the % in a control group that "survived" (they didn't get the pill), then the difference between the two percentages gives you an estimate of the "absolute effect" of the experimental condition. If 40% of experimental subjects survive and 30% of control subjects survive, the estimate of the experimental effect is 10%.

But just as for a single percentage, it is better to report two numbers rather than one number for an estimate, i.e., the endpoints of a confidence interval around the difference between the two percentages. That necessitates the calculation of the standard error of the difference between two percentages, which is more complicated than for the standard error of a single percentage. The formula for two independent samples ("unmatched") and the formula for two dependent samples (matched by virtue of being the same people or matched pairs of people) are different.

The independent samples case is more common. The formula for the standard error of the difference between two independent percentages is:

$$S.E. \approx \surd\ P_1(100 - P_1)/n_1 + P_2(100 - P_2)/n_2$$

where the P's are the two percentages and the n's are the two sample sizes. It often helps to display the relevant data in a "2 by 2" table:

	Sample 1	Sample 2
“Success”	P_1	P_2
“Failure”	$100 – P_1$	$100 – P_2$

where “Success” and “Failure” are the two categories of the variable upon which the percentages are taken, and need not be pejorative.

Edgerton’s (1927) abac can be used to read off the standard error of the difference between two independent percentages, as well as the standard error of a single sample percentage. Later, Hart (1949) and Lawshe and Baker (1950) presented quick ways to test the significance of the difference between two independent percentages. Stuart (1963) provided a very nice set of tables of standard errors of percentages for differences between two independent samples for various sample sizes, which can also be used for the single-sample case. And Fleiss, Levin, and Paik (2003) provide all of the formulas you’ll ever need for inferences regarding the difference between percentages. They even have a set of tables (pp. 660-683) for determining the appropriate sample sizes for testing the significance of the difference between two percentages. (See also Hopkins & Chappell, 1994.)

The formula for dependent samples is a real mess, involving not only the sample percentages and the sample sizes but also the correlation between the two sets of data (since the percentages are for the same people or for matched people). However, McNemar (1947) provided a rather simple formula that is a reasonable approximation to the more complicated one:

$$S.E. \approx 100/n \sqrt{(b + c)}$$

where n (= n_1 = n_2 , since the people are paired with themselves or with their “partners”), b is the number of pairs for which the person in Sample 1 was a “success” and the partner in Sample 2 was a “failure”; and c is the number of pairs for which the person in Sample 1 was a “failure” and the partner in Sample 2 was a “success”.

Sample 1	Sample 2		
	“Success”	“Failure”	
“Success”	[a]	[b]	$P_1 = (a+b)/n$
“Failure”	[c]	[d]	
	$P_2 = (a+c)/n$		

a is the number of pairs for which both members were “successes”, and
d is the number of pairs for which both members were “failures”; but, rather surprisingly, neither a nor d contributes to the standard error.

If a researcher is concerned with change in a given sample from Time 1 to Time 2, that also calls for the dependent-samples formula.

An example to illustrate both the independent and the dependent cases

You are interested in the percentage of people who pass examinations in epidemiology. Suppose there are two independent samples of 50 students each (50 males and 50 females) drawn from the same population of graduate students, where both samples take an epidemiology examination. The number of males who pass the examination is 40 and the number of females who pass is 45.

Displaying the data as suggested above we have:

	Males	Females
Passed	40/50 = 80%	45/50 = 90%
Failed	10/50 = 20%	5/50 = 10%

The standard error of the difference between the two percentages is approximately $\sqrt{\ 80(20)/50 + 90(10)/50}$ = 7.07 (rounded to two places)

On the other hand, suppose that these students consist of 50 married couples who take the same course, have studied together (within pairs, not between pairs), and take the same epidemiology examination. Those samples would be dependent. If in 74% of the couples both husband and wife passed, in 6% of the couples wife passed but husband failed, in 16% of the couples husband passed but wife failed, and in 2 couples both spouses failed, we would have the following “2 by 2” table:

Husband	Wife		
	Passed	Failed	
Passed	37 [a]	3 [b]	40 (= 80%)
Failed	8 [c]	2 [d]	10
	45 (= 90%)		

S.E. $\approx$ 100/50$\sqrt{}$ (3 + 8) = 6.63

In both cases 80% of the males passed and 90% of the females passed, but the standard error is smaller for matched pairs since the data for husbands and wives are positively correlated and the sampling error is smaller. If the correlation between paired outcomes is not very high, say less than .50 (van Belle, 2002) the pairing of the data is not very sensitive. If the correlation should happen to be NEGATIVE, the sampling error could actually be WORSE for dependent samples than for independent samples!

Would you believe that there is also a procedure for estimating the standard error of the difference between two "partially independent, partially dependent" percentages? In the husbands and wives example, for instance, suppose there are some couples for which you have only husband data and there are some couples for which you have only wife data. Choi and Stablein (1982) and Thompson (1995) explain how to carry out statistical inferences for such situations.

Interval estimation for the difference between two independent percentages

As far as the interval estimation of the difference between two independent population percentages is concerned, we proceed just as we did for a single population percentage, viz., "laying off" two S.E.'s to the right and to the left of the difference between the two sample percentages in order to get a 95% confidence interval for the difference between the two corresponding population percentages.

The sample difference is 90% - 80 % = 10% for our example. The standard error for the independent case is 7.07%. Two standard errors would be 14.14%. The 95% confidence interval for the population difference would therefore extend from 10% - 14.14% to 10% + 14.14%, i.e., from -4.14% to 24.14%. You would be 95% confident that the interval would "capture" the difference between the two population percentages. [Note that the -4.14% is a difference, not an actual %.] Since Sample 2 is the wives sample and Sample 1 is the husbands sample, and we subtracted the husband % from the wife %, we are willing to believe that in the respective populations the difference could be anywhere between 4.14% "in favor of" the husbands and 24.14% "in favor of" the wives.

I refer you to an article by Wilks (1940b) for one of the very best discussions of confidence intervals for the difference between two independent percentages. And in the previous section I mentioned an article by Newcombe (1998a) in which he compared seven methods for determining a confidence interval for a single proportion. He followed that article with another article (Newcombe, 1998b) in which he compared ELEVEN methods for determining a confidence interval for the difference between two independent proportions!

Hypothesis testing for the difference between two independent percentages

In the previous section I pointed out that except for a couple of technical details, interval estimation subsumes hypothesis testing, i.e., the confidence interval consists of all of the hypothesized values of a parameter that are "not rejectable". For our example any hypothesis concerning a population difference of -4.14 through 24.14 would not be rejected (and would be regarded as "not statistically significant at the 5% level"). Any hypotheses concerning a population difference that is outside of that range would be rejected (and would be regarded as "statistically significant at the 5% level").

In a very interesting article concerning the statistical significance of the difference between two independent percentages (he uses proportions), the late and ever-controversial Alvan R. Feinstein (1990) proposed the use of a "unit fragility index" in conjunction with the significance test. This index provides an indication of the effect of a "switch" of an observation from one category of the dependent variable to the other category (his illustrative example had to do with a comparison of cephaloridine with ampicillin in a randomized clinical trial). That index is especially helpful in interpreting the results of a trial in which the sample is small. (See also the commentary by Walter, 1991 regarding Feinstein's index.)

Feinstein was well-known for his invention of methodological terminology. My favorite of his terms is "trohoc" [that's "cohort" spelled backwards] instead of "case-control study". He didn't like case-control studies, in which "cases" who have a disease are retrospectively compared with "controls" who don't, in an observational non-experiment.

There is an advantage of interval estimation over hypothesis testing that I've never seen discussed in the methodological literature. Researchers often find it difficult to hypothesize the actual magnitude of a difference that they claim to be true in the population (and is not "null"). The theory underlying their work is often not far enough advanced to suggest what the effect might be. They are nevertheless eager to know its approximate magnitude. Therefore, instead of pitting their research (alternative) hypothesis against a null hypothesis and using power analysis for determining the appropriate sample size for testing the effect, all they need to do is to specify the magnitude of a tolerable width of a confidence interval (for a margin of error of, say, 3%), use that as the basis for the determination of sample size (see the appropriate formula in Fleiss, et al., 2003), carry out the study, and report the confidence interval. Nice; straightforward; no need to provide granting agencies with weak theories; and no embarrassment that often accompanies hurriedly-postulated effects that far exceed those actually obtained.

Two examples of lots of differences between percentages

Peterson, et al. (2009) were interested in testing the effectiveness of a particular intervention designed to help teenagers to stop smoking. Using a rather elaborate design that had a tricky unit-of analysis problem (schools containing teenage smokers were randomly assigned to treatments). Their article is loaded with both confidence intervals for, and significance tests of, the difference between two percentages.

Sarna, et al. (2009) were also interested in stopping smoking, but for nurses rather than teenagers. Like Peterson, et al., their article contains several tables of confidence intervals and significance tests for the differences between percentages. But it is about a survey, not an experiment, in which nurses who quit smoking were compared to nurses who did not quit smoking, even though all of them registered at the Nurses QuitNet website for help in trying to do so.

If you're interested in smoking cessation, please read both of those articles and let me know (tknapp5@juno.com) what you think of them.

The difference between two percentages that have to add to 100

In Section 2 I said that I don't care much for the practice of taking differences between two percentages that have been calculated on the same base for the same variable, e.g., the difference in support for Candidate A and Candidate B for the same political office, when they are the only two candidates. I am even more opposed to making any statistical inferences for such differences.

In that same part of Section 2 I referred to a difference between two percentages that didn't have to add to 100, but their sum plus the sum of one or more other percentages did have to add to 100. Let us consider the difference in support for Candidate A and Candidate B when there is a third candidate, Candidate C, for the same political office. (Remember Ralph Nader?) The following example is taken from the guide that accompanies the statistical software StatPac (Walonick, 1996-2010):

In a random sample of 107 people, 38 said they planned to vote for Candidate A, 24 planned to vote for Candidate B, and 45 favored Candidate C for a particular public office. The researcher was interested only in the difference between the support for Candidate A (35.5%) and the support for Candidate B (22.4%). Is that difference statistically significant? Several statisticians have tackled that problem. (See, for example, Kish, 1965; Scott & Seger, 1983.) In order to test the significance of the difference between the 35.5% for Candidate A and the 22.4% for Candidate B, you need to use the z-ratio of the difference (35.5 - 22.4 = 13.1) to the standard error of that difference. The formula for the approximate standard error is the square root of the expression $[(P_1 + P_2) – (P_1 – P_2)^2]/n$, and the sampling distribution is normal. (Walonick uses t; he's wrong.) For this example $P_1 = 35.5$, $P_2 = 24.4$, and $n = 107$, yielding a standard error of .098 and a z of 1.82, which is not statistically significant at the conventional .05 level.

Ratios of percentages

Now for the "biggie" in epidemiological research. We've already discussed the difference between absolute risk, as represented by the difference between two percentages, and relative risk, as represented by the ratio of two percentages. Relative risk tends to be of greater importance in epidemiology, since the emphasis is on risks for large populations of people having one characteristic compared to risks for equally large populations of people having a contrasting characteristic.

The classic example is smokers vs. non-smokers and the relative risk of getting lung cancer. But let's take as a simpler example the relationship between maternal age and birthweight. Fleiss, et al. (2003) provide a set of hypothetical data for that problem. Here are the data:

	Maternal age	
Birthweight	≤ 20 years	> 20 years
≤ 2500 grams	10	15
> 2500 grams	40	135

The ratio of interest is the percentage of younger women whose baby is of low birthweight (10/50, or 20%) divided by the percentage of older women whose baby is of low birthweight (15/150, or 10%). The relative risk of low birthweight is therefore 20%/10%, or 2.00. If these data are for a random sample of 200 women, what is the 95% confidence interval for the relative risk in the population from which the sample has been drawn? Is the relative risk of 2.00 statistically significant at the 5% level? Although the first question is concerned with interval estimation and the second question is concerned with hypothesis testing, the two questions are essentially the same, as we have already seen several times.

I shall give only a brief outline of the procedures for answering those questions. The determination of an estimate of the standard error of the ratio of two percentages is a bit complicated, but here it is (Fleiss, et al., 2003, p. 132):

S.E. $\approx r \sqrt{} (n_{12} / n_{11}\, n_{1.} + n_{22} / n_{21}\, n_{2.})$, where

r is the relative risk

n_{11} is the number in the upper-left corner of the table (10 in the example)

n_{12} is the number in the upper-right corner (40)

n_{21} is the number in the lower-left corner (15)

n_{22} is the number in the lower-right corner (135)

$n_{1.}$ is the total for the first row (50)

$n_{2.}$ is the total for the second row (150)

Substituting those numbers in the formula for the standard error, we get

S.E. = .75 (to two decimal places)

Two standard errors would be approximately 1.50, so the 95% confidence interval for the population ratio would be from .50 to 3.50. Since that interval includes 1 (a relative risk of 1 is the same risk for both groups), the obtained sample ratio of 2.00 is not statistically significant at the 5% level.

Fleiss, et al. (2003) actually recommend that the above formula for estimating the standard error not be used to get a confidence interval for a ratio of two percentages. They suggest instead that the researcher use the "odds ratio" instead of the relative risk (the odds ratio for those data is 2.25), take the logarithm of the odds ratio, and report the confidence interval in terms of "log odds". [Here we go with logarithms again!] I don't think that is necessary, since everything is approximate anyhow. If those data were real, the principal finding is that younger mothers do not have too much greater risk for having babies of low birthweight than do older mothers. Fleiss et al. arrive at the same conclusion by using the logarithmic approach.

Another "hybrid" inferential problem

Earlier in this chapter I referred to procedures derived by Choi and Stablein (1982) and by Thompson (1995) for estimating the standard error of the difference between two percentages where the samples were partially independent and partially dependent, due to missing data. There is another interesting situation that comes up occasionally where you would like to test the difference between two independent percentage gains, i.e., where each gain is the difference between two dependent percentages. (A loss is treated as a negative gain.) Building upon the work of Marascuilo and Serlin (1979) [see also Levin & Serlin, 2000], Howell (2008) discussed a hypothetical example where a change from fall (42/70 = 60%) to spring (45/70 = 64.3%) for an intervention group is compared with change from fall (38/70 = 54.3%) to spring (39/70 = 55.7%) for a control group. The difference between the 4.3% gain for the intervention group and the 1.4% gain for the control group was not statistically significant, which is not surprising since the "swing" is only about 3%. Those of you who are familiar with the classic monograph on experimental design by Campbell and Stanley (1966) might recognize Howell's example as a special case of Campbell and Stanley's True Experimental Design #4, i.e., the Pretest/Posttest Control Group Design. (See also the article by Vickers, 2001 in which he discusses four different ways for analyzing the data for such a design.)

Sample size

In the previous section I talked about a handy-dandy internet calculator that determined the optimal sample size for a confidence interval for a single percentage. The situation for determining the optimal sample sizes for confidence intervals for the difference between two percentages or the ratio of two percentages (for either independent samples or for dependent samples) is much more complicated. (See Fleiss, et al., 2003, for all of the gory details. And the PASS2008 software is particularly good for carrying out all of the calculations for you [it is available for a 7-day free trial].)

Non-random samples and full populations

Suppose you have a non-random sample of boys and a non-random sample of girls from a particular school and you want to compare the percentage of boys in the boy sample who think that President Obama is doing a good job with the percentage of girls in the girl sample who think that President Obama is doing a good job. Would a confidence interval or a significance test of the difference between, or the ratio of, the two percentages be appropriate? Suppose you have percentages for the entire population of boys and the entire population of girls? Would a confidence interval or a significance test be appropriate there?

You can't imagine how controversial both of those matters are! The opinions range from "very conservative" to "very liberal". The very conservative people argue that statistical inferences are appropriate only for probability samples, of which "simple" random samples are the most common type (everybody has an equal and independent chance of being drawn into the sample) and not for either non-random samples or entire populations. Period. End of discussion. The very liberal people argue that they are appropriate for both non-random samples and for entire populations, since they provide an objective basis for determining whether or not, or to what extent, to get excited about a finding. The people in-between (which from a cursory glance at the scientific literature are the majority) argue that for a non-random sample it is appropriate to use statistical inferential procedures in order to generalize from the non-random sample to a hypothetical population of people "like these"; and/or it might be appropriate to use statistical inferential procedures for an entire population in order to generalize from a finding now to findings for that population at other times. As one of those "very conservative people" (we meet in a telephone booth every year), those last two arguments blow my mind. I don't care about hypothetical populations (do you?) and hardly anybody studies populations by randomly sampling them across time.

In his article, Desbiens (2007) did a review of the literature and found that many authors of research reports in medical education journals use statistical inferences for entire populations. He claims that they shouldn’t. I agree.

More than two percentages

The previous discussion was concerned with procedures for statistical inferences when comparing the difference between, or the ratio of, two percentages. It is natural to ask if these procedures generalize to three or more percentages. The answer is “sort of”.

If you’re interested in testing the significance of the difference AMONG several percentages (e.g., the percentage of Catholics who voted for Obama, the percentage of Protestants who voted for Obama, and the percentage of Jews who voted for Obama), there are comparable (and more complicated) formulas for so doing (see Fleiss, et al., 2003). Confidence intervals for the more-than-two case, however, are much more awkward to handle, primarily because there are three differences (A-B, A-C, B-C) to take into consideration. [There might also be those same three differences to take into consideration when carrying out the significance testing, if you care about pairwise differences as well as the overall difference. It’s just like the problem of an overall F test vs. post hoc comparisons in the analysis of variance, if that means anything to you!]

The situation for ratios is even worse. There is no appropriate statistic for handling A/B/C, for example, either via significance testing or confidence intervals.

Percentaging the wrong way

For the 2x2 table used to introduce the topic of the difference between two independent sample percentages (see above), the column designations were the two samples and the row designations were the matters of "success" and "failure". Could the rows be the sample designations and the columns be the "success" and "failure" designations? Yes, but you'd better be careful regarding how you do the percentaging!

Consider the following real-world example (Gondolf, 2012; Straus, 2014; Knapp, 2015) of the difference in male partners' reactions to assault by their female partners:

		Female Partner		
		No Assault	Assaulted	Total
Male Partner	No Assault	354	23	377
	Assaulted	85	101	186
Total		439	124	563 (=sample size)

Converting those frequencies into percentages, we have:

		Female Partner	
		No Assault	Assaulted
Male Partner	No Assault	81%	18%
	Assaulted	19%	82%

We could have set up the frequency table as:

		Male Partner		
		No Assault	Assaulted	Total
Female Partner	No Assault	354	85	439
	Assaulted	23	101	124
Total		377	186	563 (=sample size)

and then taken the percentages by rows to get

		Male Partner	
		No Assault	Assaulted
Female Partner	No Assault	81%	19%
	Assaulted	18%	82%

The interpretation would be the same no matter which way you did the percentaging: There is a difference of 82% vs. 18 % in the matter of male re-assault vs. no re-assault (or a ratio of 82 % to 18%). And that was what Straus (2014) concluded: a big effect.

Gondolf (2012), on the other hand, had done the percentaging not by rows, and not by columns. He divided each of the cell frequencies by the TOTAL SAMPLE SIZE, got the following table,

		Female Partner	
		No Assault	Assaulted
Male Partner	No Assault	63%	4%
	Assaulted	15%	18%

compared the 15% with the 18%, and claimed there was very little difference in re-assault by the males no matter whether they were assaulted or not by their female partners. He (Gondolf) was just plain wrong. Do you see how important it is to do the percentaging the right way? You must calculate the percentages across the categories of the independent variable (in this case, female assault) and compare the percentages across the categories of the dependent variable (in this case, male re-assault); i.e. the 82% to the 18%. Got it?

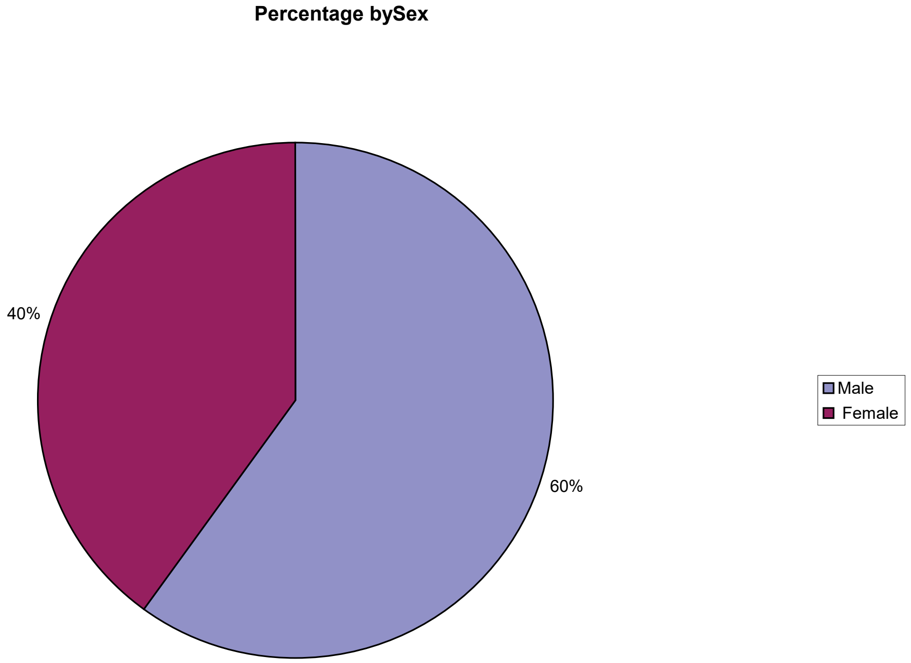
Section 6: Graphing percentages

I've never cared much for statistical graphics, except for scatter diagrams that facilitate the understanding of the form and the degree of the relationship between two variables. (See the scatter diagrams that I used in Section 4 to illustrate data transformations for percentages.) I also don't always agree with the often-stated claim that "a picture is worth a thousand words". (I like words.) But I realize that there are some people who prefer graphs to words and tables, even when it comes to percentages. I therefore decided to include in this chapter a brief section on how to display percentages properly when graphical techniques are used. You may want to adjust your "zoom" view for some of these graphs, in order to get a better idea of the information contained therein.

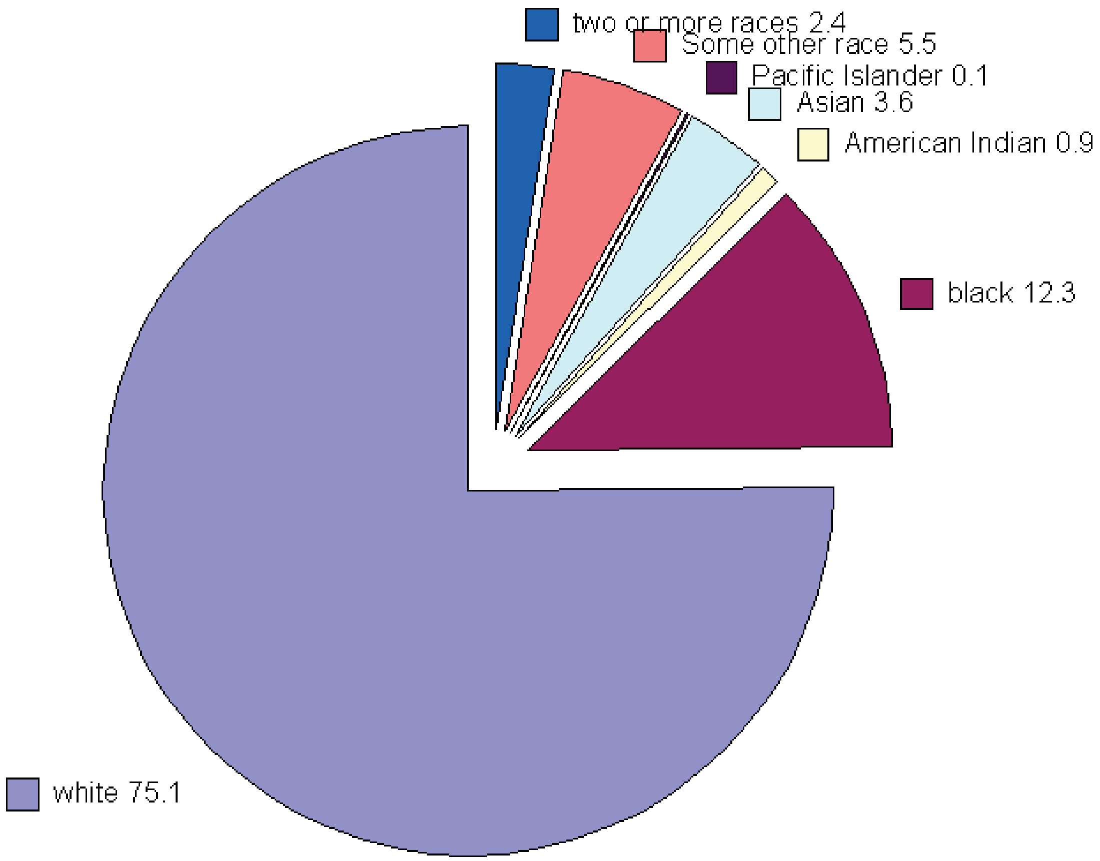
Pie charts

Far and away the most common way to show percentages is the use of pie charts, with or without colors. For example, if one of the findings of a survey is that 60% of cigarette smokers are males and 40% of cigarette smokers are females, that result could be displayed by using a "pie" (circle) divided into two "slices", a blue slice constituting 60% of the pie (216 of the 360 degrees in the circle) labeled MALES, and a red slice constituting the other 40% of the pie (the other 144 degrees) labeled FEMALES. There is absolutely nothing wrong with such charts, but I think they're unnecessary for summarizing two numbers (60 and 40)---actually only one number (60 or 40)---since the other follows automatically. If the variable has more than two categories, pie charts are somewhat more defensible for displaying percentages, but if the number of categories is too large it is difficult to see where one slice ends and another slice begins.

The software EXCEL that is part of Microsoft Office has the capability of constructing pie charts (as well as many other kinds of charts and graphs), and it is fairly easy to "copy and paste" pie charts into other documents. Here's one for the 60% male, 40% female example.



Here’s another, and more complicated, pie chart that illustrates one way to handle “small slices”. The data are for the year 2000.

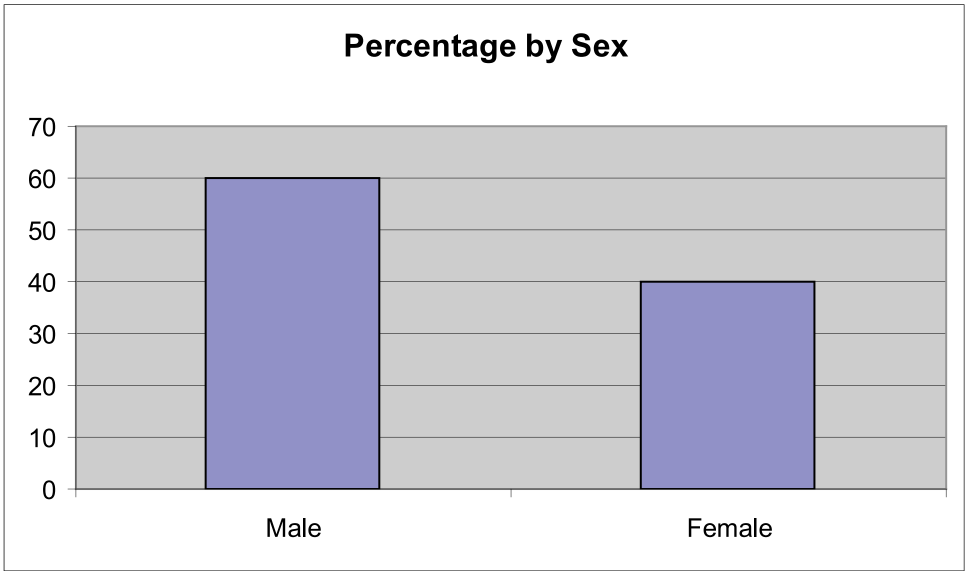


Percentages of the U.S. Population by Race, 2000 (data: U.S. Census Bureau).

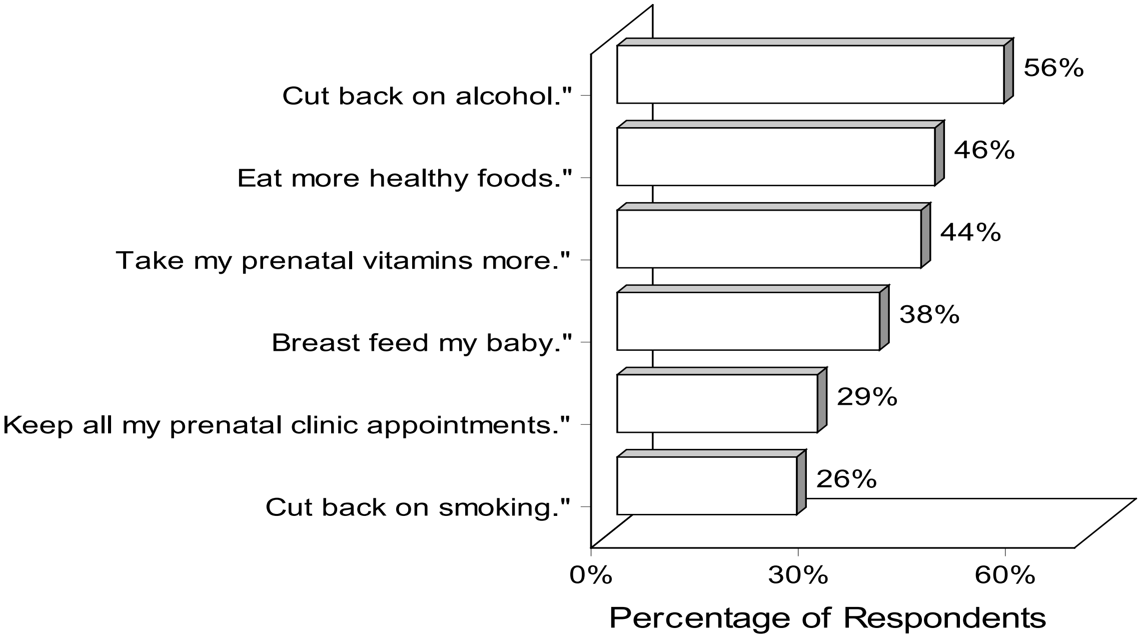
Some people are adamantly opposed to the use of pie charts for displaying percentages (van Belle, 2002, p. 160, for example, says "Never use a pie chart"), but Spence and Lewandowsky (1991) supported their use. They even provided data from experiments that showed that pie charts aren't nearly as bad as the critics claim.

Bar graphs

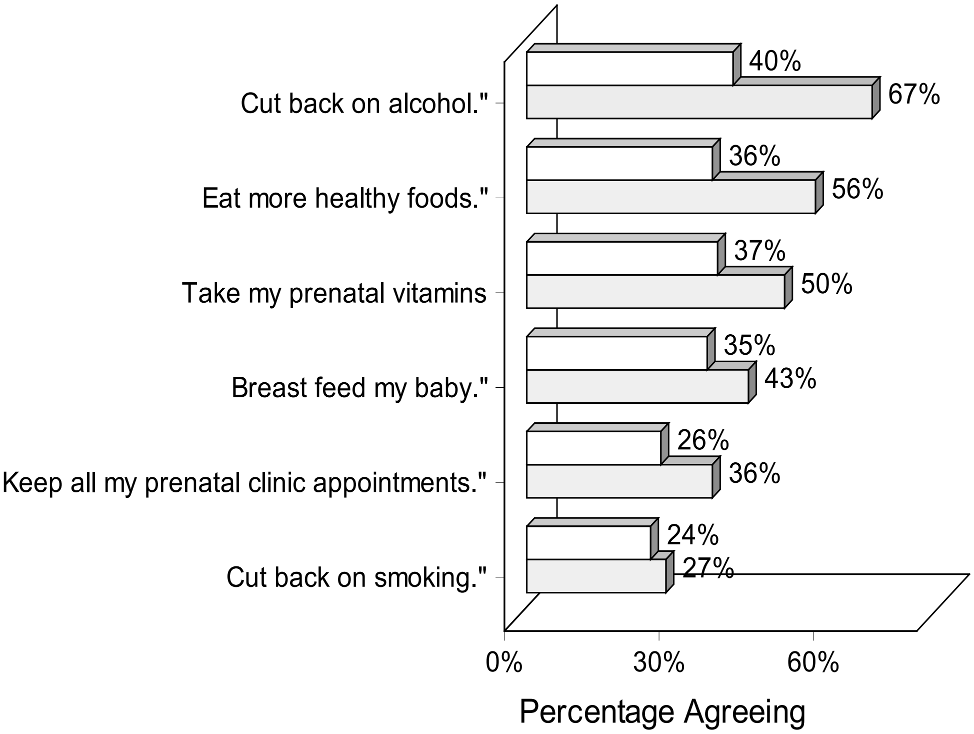
Bar graphs are probably the second most common way to display percentages. (But van Belle, 2002, doesn't like them either.) The categories of the variable are usually indicated on the horizontal (X) axis and the percentage scale usually constitutes the vertical (Y) axis, with bars above each of the categories on the X axis extending to a height corresponding to the relevant percentage on the Y axis. The categories need not be in any particular order on the X axis, if the variable is a nominal variable such as Religious Affiliation. But if the variable is an ordinal variable such as Socio-economic Status, the categories should be ordered from left to right on the X axis in increasing order of magnitude. Here's the 60%, 40% example as a bar graph:



Here's a bar graph for more than two categories. The data are percentages of responses by pregnant mothers to the question "Reading the [Preparing to Parent] newsletters helped convince me to...". Note that the bars are horizontal rather than vertical and the percentages do not add to 100 because more than one response is permitted.



Here’s a more complicated (but readable) bar graph for the “breakdown” of responses of two groups of pregnant women (those at risk for complications and those not at risk) in that same study:



For other examples of the use of bar graphs, see Keppel et al. (2008).

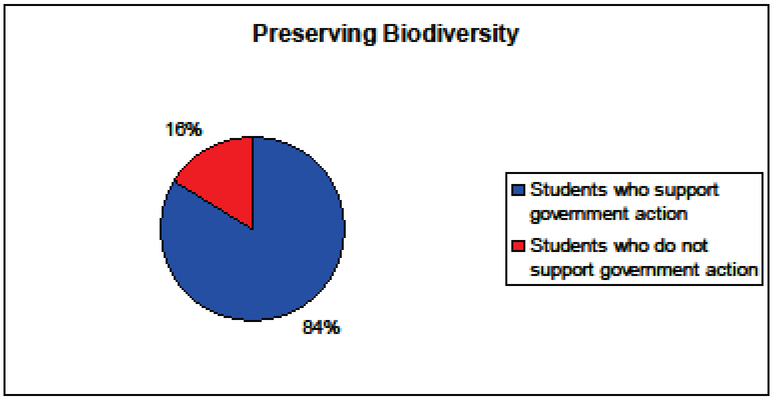
One of the least helpful percentage bar graphs I've ever seen can be downloaded from the StateMaster.com website. It is concerned with the percent of current smokers in each of 49 states, as of the year 2004. It lists those percents in decreasing order (from 27.5% for Kentucky to 10.4% for Utah; it lists Alaska, the District of Columbia, Puerto Rico, and the U.S. Virgin Islands, but not Hawaii). Each percent is rounded to one place to the right of the decimal point, and there is a bar of corresponding horizontal length right next to each of those percents. It is unhelpful because (a) the bars aren't really needed (the list of percents is sufficient); and (b) rounding the percents to one decimal place resulted in several ties unnecessarily (since the number of current smokers in each of the states and the population of each state are known or easily estimable, all of those ties could have been broken by carrying out the calculations to two decimal places rather than one).

A research example that used both a pie chart and a bar graph

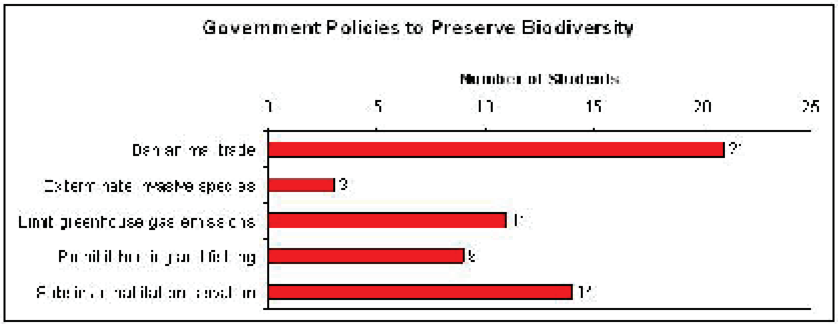
On its website, the Intel©Technology Initiative provides the following example of the use of a pie chart and a bar graph for displaying counts and percentages obtained in a survey regarding attitudes toward biodiversity. (Note that the bars in the bar graph are horizontal rather than vertical. It doesn’t really matter.)

Reporting Percentages

Charts can display survey data. The following chart shows that 21 out of 25 students, or 84 percent of students, support government action to preserve biodiversity. A pie chart is a good way to show percentages.



You could also report percentages for each of the five policies. The following bar graph shows how many students reported supporting each policy. You can use the numbers to compute percentages. A bar graph is a good way to report the number of items in selected categories.



For example, 44 percent (=11/25*100) of the students supported regulating greenhouse gas emissions. In comparison, the policy most frequently supported by students was tax breaks for electric and hybrid cars. Overall, 84 percent (=21/25*100) of the students reported that they support such tax breaks. The least frequently supported policy was building more nuclear power plants. Only 12 percent (3/21*100) of the students supported more nuclear power.

In their article, Spence and Lewandowsky (1991) reported results that indicated that bar graphs and pie charts were equally effective in displaying the key features of percentage data. They provided various bar graphs for displaying four percentages (A = 10%; B = 20%; C = 40%; D = 30%). Nice.

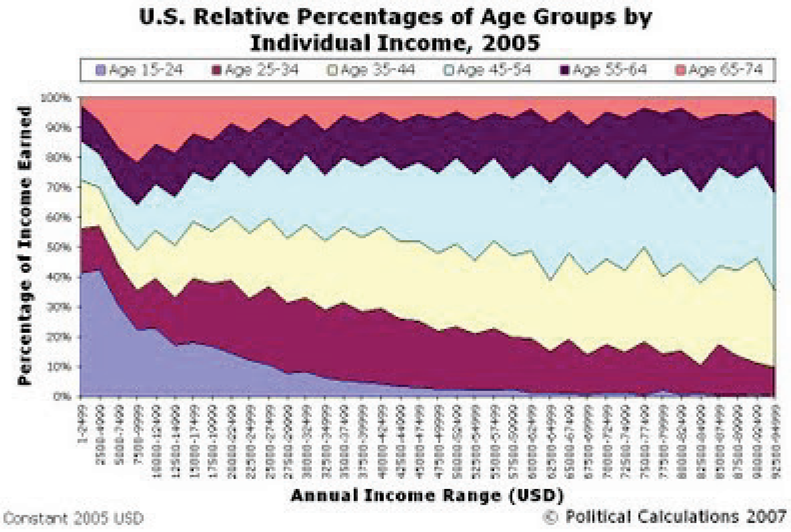
In Section 2 I referred to Milo Schield and the W.M. Keck Statistical Literacy Project at Augsburg College. In a presentation he gave to the Section on Statistical Education of the American Statistical Association, Schield (2006) gave a critique of pie and bar percentage graphs that appeared in the newspaper USA Today. I've seen many of those graphs; some are really bad.

Other graphical techniques for displaying percentages

Kastellec and Leoni (2007) provided several arguments and a great deal of evidence supporting the use of graphs to improve the presentation of findings in political science research. In their article they include real-data examples for which they have converted tables into graphs. Some of those examples dealt with percentages or proportions presented through the use of mosaic plots, dot plots, advanced dot plots, or violin plots (those are their actual names!). Rather than trying to explain those techniques here, I suggest that you read the Kastellec and Leoni article and see for yourself. (They're not just applicable to political science.) Their article also has an extensive set of references pro and con the use of graphs.

If you skipped Section 1 and you're having difficulty distinguishing among percentages, proportions, and fractions, I suggest that you take a look at the British website www.active-maths.co.uk/.../fracdec_index.html, which lays out nicely how each relates to the others.

And here's another example of the graphing of percentages (taken from the Political Calculations website):



That graph contains a lot of interesting information (e.g., that the percentage of people aged 65-74 who have very high incomes is almost as high as the percentage of people aged 25-34--read along the right-hand edge of the graph), but I personally find it to be too “busy”, and it looks like Jaws!

Section 7: Percentage overlap of two frequency distributions

One of the things that has concerned me most about statistical analysis over the years is the failure by some researchers to distinguish between random sampling and random assignment when analyzing data for the difference between two groups. Whether they are comparing a randomly sampled group of men with a randomly sampled group of women, or a randomly assigned sample of experimental subjects with a randomly assigned sample of control subjects (or, worse yet, two groups that have been neither randomly sampled nor randomly assigned), they invariably carry out a t-test of the statistical significance of the difference between the means for the two groups and/or construct a confidence interval for the corresponding "effect size".

I am of course not the first person to be bothered by this. The problem has been brought to the attention of readers of the methodological literature for many years. [See, for example, Levin's (1993) comments regarding Shaver (1993); Lunneborg (2000); Levin (2006); and Edgington & Onghena (2007).] As I mentioned in an earlier section of this chapter, some researchers "regard" their non-randomly-sampled subjects as having been drawn from hypothetical populations of subjects "like these". Some have never heard of randomization (permutation) tests for analyzing the data for the situation where you have random assignment but not random sampling. Others have various arguments for using the t-test (e.g., that the t-test is often a good approximation to the randomization test); and still others don't seem to care.

It occurred to me that there might be a way to create some sort of relatively simple "all-purpose" statistic that could be used to compare two independent groups no matter how they were sampled or assigned (or just stumbled upon). I have been drawn to two primary sources:

1. The age-old concept of a percentage.

2. Darlington's (1973) article in Psychological Bulletin on "ordinal dominance" (of one group over another). [The matter of ordinal dominance was treated by Bamber (1975) in greater mathematical detail and in conjunction with the notion of receiver operating characteristic (ROC) curves, which are currently popular in epidemiological research.]

My recommendation

Why not do as Darlington suggested and plot the data for Group 1 on the horizontal axis of a rectangular array, plot the data for Group 2 on the vertical axis, see how many times each of the observations in one of the groups (say Group 1) exceeds each of the observations in the other group, convert that to a percentage (he actually did everything in terms of proportions), and then do with that percentage whatever is warranted? (Report it and quit; test it against a hypothesized percentage; put a confidence interval around it; whatever).

Darlington's example [data taken from Siegel (1956)]

The data for Group 1: 0, 5, 8, 8, 14, 15, 17, 19, 25 (horizontal axis)
The data for Group 2: 3, 6, 10, 10, 11, 12, 13, 13, 16 (vertical axis)

The layout:

16							x	x	x
13					x	x	x	x	x
13					x	x	x	x	x
12					x	x	x	x	x
11					x	x	x	x	x
10					x	x	x	x	x
10					x	x	x	x	x
6			x	x	x	x	x	x	x
3		x	x	x	x	x	x	x	x
	0	5	8	8	14	15	17	19	25

The number of times that an observation in Group 1 exceeded an observation in Group 2 was 48 (count the x's). The percentage of times was 48/81, or .593, or 59.3%. Let's call that P_e for "percentage exceeding". [Darlington calculated that proportion (percentage) but didn't pursue it further. He recommended the construction of an ordinal dominance curve through the layout, which is a type of cumulative frequency distribution similar to the cumulative frequency distribution used as the basis for the Kolmogorov-Smirnov test.]

How does this differ from other suggestions?

Comparing two independent groups by considering the degree of overlapping of their respective distributions appears to have originated with the work of Truman Kelley (1919), the well-known expert in educational measurement and statistics at the time, who was interested in the percentage of one normal distribution that was above the median of a second normal distribution. [His paper on the topic was typographically botched by the Journal of Educational Psychology and was later (1920) reprinted in that journal in corrected form.] The notion of distributional overlap was subsequently picked up by Symonds (1930), who advocated the use of biserial r as an alternative to Kelley's measure, but he was taken to task by Tilton (1937) who argued for a different definition of percentage overlap that more clearly reflected the actual amount of overlap. [Kelley had also suggested a method for correcting percentage overlap for unreliability.]

Percentage overlap was subsequently further explored by Levy (1967), by Alf and Abrahams (1968), and by Elster and Dunnette (1971).

In their more recent discussions of percentage overlap, Huberty and his colleagues (Huberty & Holmes, 1983; Huberty & Lowman, 2000; Hess, Olejnik, & Huberty, 2001; Huberty, 2002) extended the concept to that of "hit rate corrected for chance" [a statistic similar to Cohen's (1960) kappa] in which discriminant analysis or logistic regression analysis is employed in determining the success of "postdicting" original group membership. (See also Preese, 1983; Campbell, 2005; and Natesan & Thompson, 2007.)

There is also the "binomial effect size display (BESD)" advocated by Rosenthal and Rubin (1982) and the "probability of superior outcome" approach due to Grissom (1994). BESD has been criticized because it involves the dichotomization of continuous variables (see the following section). Grissom's statistic is likely to be particularly attractive to experimenters and meta-analysts, and in his article he includes a table that provides the probabilistic superiority equivalent to Cohen's (1988) d for values of d between .00 and 3.99 by intervals of .01.

Most closely associated with the procedure proposed here (the use of P_e) is the work represented by a sequence of articles beginning with McGraw and Wong (1992) and extending through Cliff (1993), Vargha and Delaney (2000), Delaney and Vargha (2002), Feng and Cliff (2004), and Feng (2006). [Amazingly--to me, anyhow--the only citation to Darlington (1973) in any of those articles is by Delaney and Vargha in their 2002 article!] McGraw and Wong were concerned with a "common language effect size" for comparing one group with another for continuous, normally distributed variables, and they provided a technique for so doing. Cliff argued that many variables in the social sciences are not continuous, much less normal, and he advocated an ordinal measure d (for sample dominance; δ for population dominance). [This is not to be confused with Cohen's effect size d, which is appropriate for interval-scaled variables only.] He (Cliff) defined d as the difference between the probability that an observation in Group 1 exceeds an observation in Group 2 and the probability that an observation in Group 2 exceeds an observation in Group 1. In their two articles Vargha and Delaney sharpened the approach taken by McGraw and Wong, in the process of which they suggested a statistic, A, which is equal to my P_e if there are no ties between observations in Group 1 and observations in Group 2, but they didn't pursue it as a percentage that could be treated much like any other percentage. Feng and Cliff, and Feng, reinforced Cliff's earlier arguments for preferring δ and d, which range from -1 to +1. Vargha and Delaney's A ranges from 0 to 1 (as do all proportions) and is algebraically equal to (1 + d)/2, i.e., it is a simple linear transformation of Cliff's measure. The principal difference between Vargha and Delaney's A and Cliff's d, other than the range of values they can take on, is that A explicitly takes ties into account.

Dichotomous outcomes

The ordinal-dominance-based "percentage exceeding" measure also works for dichotomous dependent variables. For the latter all one needs to do is dummy-code (0,1) the outcome variable, string out the 0's followed by the 1's for Group 1 on the horizontal axis, string out the 0's followed by the 1's for Group 2 on the vertical axis, count how many times a 1 for Group 1 appears in the body of the layout with a 0 for Group 2, and divide that count by n_1 times n_2, where n_1 is the number of observations in Group 1 and n_2 is the number of obervations in Group 2. Here is a simple hypothetical example:

The data for Group 1: 0, 1, 1, 1
The data for Group 2: 0, 0, 1, 1, 1

The layout:

1				
1				
0		x	x	x
0		x	x	x
0		x	x	x
	0	1	1	1

There are 9 instances of a 1 for Group 1 paired with a 0 for Group 2, out of 4X5 = 20 total comparisons, yielding a "percentage exceeding" value of 9/20, or .45, or 45%.

Statistical inference

For the Siegel/Darlington example, if the two groups had been simply randomly sampled from their respective populations, the inference of principal concern might be the establishment of a confidence interval around the sample P_e . [You get tests of hypotheses "for free" with confidence intervals for percentages, as I pointed out in Section 4.] But there is a problem regarding the "n" for P_e. In that example the sample percentage, 59.3, was obtained with n_1 x n_2 = 9x9 = 81 in the denominator. 81 is not the sample size (the sum of the sample sizes for the two groups is only 9 + 9 = 18). This problem had been recognized many years ago in research on the probability that Y is less than X, where Y and X are vectors of length n and m, respectively. In articles beginning with Birnbaum and McCarty (1958) and extending through Owen, Craswell, and Hanson (1964), Ury (1972), and others, a procedure for making inferences from the sample probabilities to the corresponding population probabilities was derived.

The Owen, et al. and Ury articles are particularly helpful in that they include tables for constructing confidence intervals around a sample P_e . For the

Siegel/Darlington data, the confidence intervals are not very informative, since the 90% interval extends from 0 (complete overlap in the population) to 100 (no overlap), because of the small sample size.

If the two groups had been randomly assigned to experimental treatments, but had not been randomly sampled, a randomization test is called for, with a "percentage exceeding" calculated for each re-randomization, and a determination made of where the observed P_e falls among all of the possible P_e's that could have been obtained under the (null) hypothesis that each observation would be the same no matter to which group the associated object (usually a person) happened to be assigned.

For the small hypothetical example of 0's and 1's the same inferential choices are available, i.e., tests of hypotheses or confidence intervals for random sampling, and randomization tests for random assignment. [There are confidence intervals associated with randomization tests, but they are very complicated. See, for example, Garthwaite (1996).] If those data were for a true experiment based upon a non-random sample, there are "9 choose 4" (the number of combinations of 9 things taken 4 at a time) = 126 randomizations that yield P_e 's ranging from 0.00 (all four 0's in Group 1) to 80 (four 1's in Group 1 and only one 1 in Group 2). The 45 is not among the 10% least likely to have been obtained by chance, so there would not be a statistically significant treatment effect at the 10% level. (Again the sample size is very small.) The distribution is as follows:

P_e	frequency
.00	1
.05	22
.20	58
.45	40
.80	5
	126

To illustrate the use of an arguably defensible approach to inference for the overlap of two groups that have been neither randomly sampled nor randomly assigned, I turn now to a set of data originally gathered by Ruback and Juieng (1997). They were concerned with the problem of how much time drivers take to leave parking spaces after they return to their cars, especially if drivers of other cars are waiting to pull into those spaces. They had data for 100 instances when other cars were waiting and 100 instances when other cars were not waiting. On his statistical home page, Howell (2007) has excerpted from that data set 20 instances of "someone waiting" and 20 instances of “no one waiting”, in order to keep things manageable for the point he was trying to make about statistical inferences for two independent groups. Here are the data (in seconds):

Someone waiting (Group 1)
49.48 43.30 85.97 46.92 49.18 79.30 47.35 46.52 59.68 42.89
49.29 68.69 41.61 46.81 43.75 46.55 42.33 71.48 78.95 42.06

No one waiting (Group 2)
36.30 42.07 39.97 39.33 33.76 33.91 39.65 84.92 40.70 39.65
39.48 35.38 75.07 36.46 38.73 33.88 34.39 60.52 53.63 50.62

Here is the 20x20 dominance layout (I have rounded to the nearest tenth of a second in order to save room and not bothered to order each data set):

36.3	x	x	x	x	x	x	x	X	x	x	x	x	x	x	x
42.1	x	x	x	x	x	x	x	X	x	x	x	x		x	x
40.0	x	x	x	x	x	x	x	X	x	x	x	x	x	x	x
39.3	x	x	x	x	x	x	x	X	x	x	x	x	x	x	x
33.8	x	x	x	x	x	x	x	X	x	x	x	x	x	x	x
33.9	x	x	x	x	x	x	x	X	x	x	x	x	x	x	x
39.7	x	x	x	x	x	x	x	X	x	x	x	x	x	x	x
84.9			x			x									
40.7	x	x	x	x	x	x	x	X	x	x	x	x	x	x	x
39.7	x	x	x	x	x	x	x	X	x	x	x	x	x	x	x
39.5	x	x	x	x	x	x	x	X	x	x	x	x	x	x	x
35.4	x	x	x	x	x	x	x	X	x	x	x	x	x	x	x
75.1			x			x									
36.5	x	x	x	x	x	x	x	X	x	x	x	x	x	x	x
38.7	x	x	x	x	x	x	x	X	x	x	x	x	x	x	x
33.9	x	x	x	x	x	x	x	X	x	x	x	x	x	x	x
34.4	x	x	x	x	x	x	x	X	x	x	x	x	x	x	x
60.5			x			x						x			
53.6			x			x			x			x			
50.6			x			x			x			x			
	49.5	43.3	86.0	46.9	49.2	79.3	47.4	46.5	59.7	42.9	49.3	68.7	41.6	46.8	4

For these data P_e is equal to 318/400 = 79.5%. Referring to Table 1 in Ury (1972) a 90% confidence interval for π_e is found to extend from 79.5 – 36.0 to 79.5 + 36.0, i.e., from 43.5 to 100. A "null hypothesis" of a 50% proportion overlap in the population could not be rejected.

Howell actually carried out a randomization test for the time measures, assuming something like a natural experiment having taken place (without the random assignment, which would have been logistically difficult if not impossible to carry out). Based upon a random sample of 5000 of the 1.3785 x 10^{11} possible re-randomizations he found that there was a statistically significant difference at the 5% level (one-tailed test) between the two groups, with longer times taken when there was someone waiting. He was bothered by the effect that one or two outliers had on the results, however, and he discussed alternative analyses that might minimize their influence.

Disadvantages of the "percentage exceeding" approach

The foregoing discussion was concerned with the postulation of P_e as a possibly useful measure of the overlap of the frequency distributions for two independent groups. But every such measure has weaknesses. The principal disadvantage of P_e is that it ignores the actual magnitudes of the n_1 x n_2 pairwise differences, and any statistical inferences based upon it for continuous distributions are therefore likely to suffer from lower power and less precise confidence intervals. A second disadvantage is that there is presently no computer program available for calculating P_e . [I'm not very good at writing computer programs, but I think that somebody more familiar with Excel than I am would have no trouble dashing one off. The layouts used in the two examples in this paper were actually prepared in Excel and "pasted" into a Word document.] Another disadvantage is that it is not (at least not yet) generalizable to two dependent groups, more than two groups, or multiple dependent variables.

A final note

Throughout this section I have referred to the 10% significance level and the 90% confidence coefficient. The choice of significance level or confidence coefficient is entirely up to the researcher and should reflect his/her degree of willingness to be wrong when making sample-to-population inferences. I kinda like the 10% level and 90% confidence for a variety of reasons. First of all, I think you might want to give up a little on Type I error in order to pick up a little extra power (and give up a little precision) that way. Secondly, as illustrated above, more stringent confidence coefficients often lead to intervals that don't cut down very much on the entire scale space. And then there is my favorite reason that may have occurred to others. When checking my credit card monthly statement (usually by hand, since I like the mental exercise), if I get the units (cents) digit to agree I often assume that the totals will agree. If they agree, Visa's "null hypothesis" doesn't get rejected when perhaps it should be rejected. If they don't agree, if I reject Visa's total, and if it turns out that Visa is right, I have a 10% chance of having made a Type I error, and I waste time needlessly re-calculating. Does that make sense?

Section 8: Dichotomizing continuous variables: Good idea or bad idea?

A very bad idea, or at least so say Cohen (1983); Hunter and Schmidt (1990); MacCallum, Zhang, Preacher, and Rucker (2002); Streiner (2002); Owen and Froman (2005); Royston, Altman, and Sauerbrei (2006); Altman and Royston (2006); Taylor, West, and Aiken (2006); and others. [2006 was a good year for anti-dichotomization articles!] But it's done all the time. Is there no good defense for it? In what follows I'll try to point out some of its (admittedly few) advantages and its (unfortunately many) disadvantages.

Here are a few advantages:

Simplicity of description

When it comes to investigating the relationship between two variables X and Y, nothing is simpler than dichotomizing both variables at their medians and talking about what % were above the median on X and Y, what % were below the median on X and Y, what % were above on X but below on Y, and what % were below on X but above on Y. Having to plot the continuous data, trying to figure out whether or not the plot is "linear enough" to use Pearson r, worrying about outliers, etc., is a pain.

Simplicity of inference

Percent of agreement, i.e., the % for both above plus the % for both below, can be treated just like a simple percentage (see Section 4). The "single-best" point estimate of the population percentage of agreement is the sample percentage of agreement, the confidence interval for the population percentage is straight-forward, and so is the hypothesis test.

Applicability to "crazy" distributions

There are some frequency distributions of continuous or "near-continuous" variables that are so unusual that dichotomization is often used in order to make any sense out of the data. In the following sections I would like to consider two of them.

Number of cigarettes smoked per day

When people are asked whether or not they smoke cigarettes and, if so, approximately how many they smoke each day, the frequency distribution has a big spike at 0, lesser spikes at 20 (the one-pack-a-day people), 40 (two packs), and 60 (three packs), but also some small spikes at 10 (half pack), 30 (pack and a half), etc. Some people smoke (or say they smoke) just one cigarette per day, but hardly anyone reports 3, 7, 11 or other non-divisors of 20. In Table 1, below, I have provided a frequency distribution for 5209 participants in the well-known

Framingham Heart Study at Time 7 (which is Year 14--around 1960--of that study). I have also included some descriptive statistics for that distribution, in order to summarize its central tendency, variability, skewness, and kurtosis. (The distribution and all calculations based upon it were carried out in Excel, to far too many decimal places!)

Note some of the interesting features. The distribution exhibits the "crazy" pattern indicated in the previous paragraph, with several holes (particularly at the high end) and with "heapings" at observations ending in 0 and 5. It has a mode and a median of 0; a mean of about 9 1/2; standard deviation of about 13; skewness between 1 and 2; and kurtosis of approximately that same magnitude. [At first I thought that the 90 (4 1/2 packs per day!) was an error and should have been 9, but there was more than one such observation in the full data set.]

I am of course not the first person to study the frequency distribution of number of cigarettes smoked per day (see, for example, Klesges, Debon, & Ray, 1995 and the references they cite).

Table 1: Data for Year 14 of the Framingham Heart Study

# Cigs	Frequency		
0	2087		
1	73	Mean	9.477795
2	44		
3	65	Median	0
4	41	Mode	0
		Standard	
5	32	Dev.	13.18546
6	30	Variance	173.8564
7	26	Kurtosis	1.291503
8	19	Skewness	1.334515
9	15	Range	90
10	130	Minimum	0
11	15	Maximum	90
12	26	Sum	37134
13	6	Count	3918
14	6	Missing	1291
15	111		
16	9		
17	13		
18	19		
19	6		
20	581		
21	0		
22	10		

23	1
24	2
25	53
26	0
27	9
28	0
29	1
30	225
31	0
32	0
33	0
34	0
35	22
36	0
37	0
38	0
39	0
40	193
41	0
42	1
43	0
44	0
45	6
46	0
47	0
48	0
49	0
50	19
51	0
52	0
53	0
54	0
55	2
56	0
57	0
58	0
59	0
60	17
61	0
62	0
63	0
64	0
65	0
66	0
67	0
68	0

69	0
70	1
71	0
72	0
73	0
74	0
75	0
76	0
77	0
78	0
79	0
80	1
81	0
82	0
83	0
84	0
85	0
86	0
87	0
88	0
89	0
90	1

So what? That distribution fairly cries out to be dichotomized. But where to cut? The obvious place is between 0 and 1, so that all of the people who have "scores" of 0 can be called "non-smokers" and all of the people who have "scores" from 1 to 90 can be called "smokers". For the data in Table1 there were 2087 non-smokers out of 2727 non-missing-data persons, or 76.5%, which means there were 640 smokers, or 23.5%. [As usual, "missing" causes serious problems. I don't know why there were so many participants who didn't respond to the question. Can you speculate why?]

Klondike

Just about every computer that has Microsoft Windows as an operating system includes as part of a free software package the solitaire game of Klondike. It has a number of versions, but the one that is most interesting (to me) is the "turn-one, single pass through the pack" version. The object is to play as many cards as possible on the foundation piles of ace through king of each of the four suits. The possible "scores" (number of cards played to those piles) range from 0 to 52. Of considerable interest (again, to me, anyhow) is the frequency distribution of those scores. One would hope that the distribution could be derived mathematically, but since there is a deterministic aspect (skill) to the game (in addition to the stochastic aspect) and things can get very complicated very quickly, all such efforts to do so appear to have been unsuccessful. As the authors of a paper on solitaire (Yan, et al., 2005) put it: " It is one of the embarrassments of applied mathematics that we cannot determine the odds of winning the common game of solitaire." (p. 1554) Some probabilities have been mathematically derived for some versions of Klondike, e.g., the probability of being unable to play a single card in the "turn three, unlimited number of passes" version (see Latif, 2004).

I recently completed 1000 games of "turn-one, single-pass" Klondike [we retired professors have lots of time on our hands!], and the distribution of my scores is displayed in Table 2, below (summary descriptive statistics have been added, all from Excel). Note the long tail to the right with small frequencies between 19 and 31, a big hole between 31 and 52, and heaping on 52. (Once you're able to play approximately half of the deck on the foundation piles you can usually figure out a way to play the entire deck.) I won (got a score of 52) 36 times out of 1000 tries, for a success rate of 3.6%. [Note also the paucity of scores of 0. I got only two of them in 1000 tries. It's very unusual to not be able to play at least one card on the foundation piles. And it's positively re-inforcing each time you play a card there. B.F. Skinner would be pleased!]

Table 2: Results of 1000 games of Klondike

Score	Frequency
0	2
1	23
2	37
3	66
4	89
5	117
6	88
7	110
8	72
9	68
10	64
11	40
12	34
13	34
14	18
15	22
16	11
17	21
18	11
19	6
20	4
21	6
22	6
23	2

Mean	9.687
Median	7
Mode	5
Standard Dev.	9.495114
Variance	90.15719
Kurtosis	11.5888
Skewness	3.242691
Range	52
Minimum	0
Maximum	52
Sum	9687
Count	1000

24	1
25	3
26	3
27	0
28	2
29	2
30	1
31	1
32	0
33	0
34	0
35	0
36	0
37	0
38	0
39	0
40	0
41	0
42	0
43	0
44	0
45	0
46	0
47	0
48	0
49	0
50	0
51	0
52	36

Again, so what? This distribution also cries out to be dichotomized, but where? If all you care about is winning (being able to play all 52 cards on the foundation piles) the obvious place to cut is just below 52, call the winners (36 of them) 1's and the losers 0's, and talk about the percentage of winners (or, alternatively, the probability of winning), which is approximately 4%. Another reasonable possibility is to dichotomize at the median (of 7), with half of the resulting scores below that number and the other half above that number. Klondike is occasionally played competitively, so if you are able to play 7 or more cards you have approximately a 50% chance of beating your opponent.

[I just finished another 1000 games, with essentially the same results: 41 wins (4.1%), a mean of about 10; etc.]

Although he is generally opposed to dichotomizing, Streiner (2002) referred to situations where it might be OK, e.g., for highly skewed distributions such as the above or for non-linearly-related variables. [I love the title of his article!]

Now for a few of the disadvantages:

Loss of information

The first thing that's wrong with dichotomization is a loss of information. For the original variable, "number of cigarettes smoked per day", we have a pretty good idea of the extent to which various people smoke, despite its "crazy" distribution. For the dichotomy, all we know is whether or not they smoke.

Inappropriate pooling of people

For the "smoker vs.non-smoker" dichotomy there is no distinction made between someone who smokes one cigarette per day and someone who smokes four or more packs per day. Or, switching examples from smoking to age (above or below age 21, say), height (above or below 5'7"), or weight (above or below 130#), the problem could be even worse.

Decreased sensitivity or power

The principal objective of interval estimation is to construct a rather tight interval around the sample statistic so that the inference from statistic to corresponding parameter is strong. Confidence intervals for percentages derived from dichotomization are generally less precise than their counterparts for continuous variables. The situation for hypothesis testing is similar. If the null hypothesis is false you would like to have a high probability of rejecting it in favor of the alternative hypothesis, i.e., high power. The power for dichotomies is generally lower than the power for continuous variables. (But see Owen & Froman, 2005 for a counter-example.)

You will find discussions of additional disdvantages to dichotomization in the references cited at the beginning of this section.

So what's a researcher to do?

There is no substitute for common sense applied to the situation in hand.
A good rule to keep in mind is "when tempted to dichotomize, don't", UNLESS you have one or more "crazy" continuous distributions to contend with.

Section 9: Percentages and reliability

"Reliability and validity" are the "Rosencranz and Guildenstern" of scientific measurement. In Shakespeare's Hamlet people couldn't say one name without saying the other, and the two of them were always being confused with one another. Similarly, in discussing the properties of good measuring instruments, "reliability and validity" often come out as a single word; and some people confuse the two.

What is the difference between reliability and validity?

Simply put, reliability has to do with consistency; validity has to do with relevance. An instrument might yield consistent results from "measure" to "re-measure", yet not be measuring what you want it to measure. In this chapter I shall concentrate on reliability, in which I am deeply interested. Validity, though more important (what good is it to have a consistent instrument if it doesn't measure the right thing?), ultimately comes down to a matter of expert judgment, in my opinion, despite all of the various types of validity that you read about.

How do percentages get into the picture?

In the previous section I referred to a couple of advantages of dichotomies, viz., their simplicity for description and for inference. Consider the typical classroom spelling test for which 65% is "passing", i.e., in order to pass the test a student must be able to spell at least 65% of the words correctly. (We shall ignore for the moment why 65%, whether the words are dictated or whether the correct spelling is to be selected from among common misspellings, and the like. Those matters are more important for validity.)

Mary takes a test consisting of 200 words and she gets 63% right (126 out of the 200). You're concerned that those particular 200 words might contain too many "sticklers" and she really deserved to get 65% or more (at least 130 out of the 200; she only missed the "cutoff" by four words). Suppose that the 200 words on the test had been randomly drawn from an unabridged dictionary. You decide to randomly draw another set of words from that same dictionary and give Mary that "parallel form". This time she gets 61% right. You now tell her that she has failed the test, since she got less than 65% on both forms.

Types of reliability

The example just presented referred to parallel forms. That is one type of reliability. In order to investigate the reliability of a measuring instrument we construct two parallel forms of the instrument, administer both forms to a group of people, and determine the percentage of people who "pass" both forms plus the percentage of people who "fail" both forms: our old friend, percent agreement. Percent agreement is an indicator of how consistently the instrument divides people into “passers” and “failers”. But suppose that you have only one form of the test, not two. You can administer that form twice to the same people and again determine the % who pass both times plus the % who fail both times. This test/re-test approach is not quite as good as parallel forms, since the people might “parrot back” at Time 2 what they say at Time 1, therefore endowing the instrument with artificially high reliability.

Or suppose that you’re interested in the reliability of rating essays. You administer the essay test just once, but you ask the teacher to rate the students’ essays twice (so-called intra-rater reliability) or ask two different teachers to rate the students’ essays once each (inter-rater reliability). Percent agreement is again a good way to determine the extent to which the two sets of ratings agree. Robinson (1957) discussed the advantages and disadvantages of percent agreement vs. traditional Pearson correlations for measuring intra-rater or inter-rater reliability.

Got the idea?

Kappa

There is a strange (again, in my opinion) statistic called kappa (Cohen, 1960), which is percent agreement corrected for chance. Its formula is:

$\kappa = (P - P_c)/(100 - P_c)$

where P is actual percent agreement and P_c is the percent agreement that is expected “by chance”. So if two raters of essays have 80% agreement using a four-point rating scale, and if they were both susceptible to occasional random ratings (without reading the essay itself?), they could have (1/4)(1/4) = 1/16 = 6.25% agreement “by chance”. That would be P_c. Therefore, κ would be (80 – 6.25)/(100 – 6.25) = 78.67%.

There are two reasons why I think kappa is strange. First of all, I don’t think raters rate “by chance”. Secondly, even if they do, a researcher need only demand that the percent agreement be higher in order to compensate for same. [Hutchinson (1993) presented an argument for the use of tetrachoric correlation rather than kappa.] Landis and Koch (1977) claim that a kappa of 61% to 80% , for example, is indicative of “substantial” agreement. Why not up those numbers by 10% and define percent agreement of 71% to 90% as “substantial”? But kappa is VERY commonly used; see Fleiss et al. (2003) and some of the references that they cite.

One very interesting non-reliability use of kappa is in the detection of possible cheating on an examination (Sotaridona, 2006). Now there’s a context in which there is indeed liable to be a great deal of “chance” going on!

Criterion-referenced vs. norm-referenced measurement

The previous paragraphs described various ways for determining the reliability of an instrument where there is some sort of cutoff point above which there is “success” and below which there is “failure”. Such instruments are called criterion-referenced. On the other hand, instruments such as the SAT or the GRE do not have cutoff points; they are not “passed” or “failed”. Scores on those tests are interpreted relative to one another rather than relative to a cutoff point. They’re called norm-referenced. [Be careful not to confuse norms with standards. Norms are what are; standards are what should be.]

There are several other contributions in the criterion-referenced measurement literature regarding the use of percentages as indicators of the reliability of such instruments. For example, in building upon the work of Hambleton and Novick (1973), Subkoviak (1976), and others, Smith (2003) and, later, Walker (2005) advocated the use of the standard error of a percentage in the estimation of the reliability of a classroom test (a potentially different reliability for each student). The formula for the standard error becomes $\surd P(100-P)/k$, where P is the % of items answered correctly and k is the number of items (the “item sample size”, analogous to n, the traditional “people sample size”). For example, if John answered correctly 16 out of 20 items, his P is 80%, and his standard error is $\surd 80(100-80)/20$, which is about 9%. If Mary answered correctly 32 out of 40 items correctly (not necessarily items on the same test), her P is also 80% but her standard error is $\surd 80(100-80)/40$, which is about 6 1/3%. Therefore the evidence is more reliable for Mary than for John. The problem, however, is that the traditional formula for the standard error of a percentage assumes that the number of observations that contribute to the percentage (people, items,…, whatever) are independent of one another. That is much more defensible when people are sampled than when items are sampled.

Chase (1996) went one step further by discussing a method for estimating the reliability of a criterion-referenced test before it's ever administered!

Miscellany

There have been a number of other contributions in the literature regarding the uses of percentages in conjunction with the estimation of the reliability of a measuring instrument. Here are a few examples:

Barnette’s (2005) Excel program for computing confidence intervals for various reliability coefficients includes the case of percentages.

Feldt (1996) provides formulas for confidence intervals around a proportion of mastery.

Guttman (1946) discussed a method for determining a lower bound for the reliability of an instrument that produced qualitative (nominal or ordinal) data.

I (Knapp, 1977b) proposed a technique for determining the reliability of a single test item that has been dichotomously scored. Much later I (Knapp, 2015) put together a whole book on reliability, some of which was concerned with the use of percentages as indicators of the reliability of a measuring instrument.

Section 10: Wrap-up

In this section I have tried to explain why I think that percentages are "the most useful statistics ever invented". I hope you agree. But even if you don't, I hope you now know a lot more about percentages than you did when you started reading the section.

I also said I would tell you why 153 is one of my favorite numbers. It comes from the New Testament in a passage that refers to a miracle that Jesus performed when he made it possible for his apostles to catch a boatload of fish after they had caught nothing all day long. The evangelists claim that the catch consisted of 153 large fish. Who counted them? Was it exactly 153 fish?

I would like to close with a brief annotated bibliography of references that I did not get an opportunity to cite in the previous nine sections. Here it is (the full bibliographical information can be found in the References that follow the conclusion of this section):

Aiken, et al. (2003). This article in the Journal of the American Medical Association about the relationship between nurse educational level and patient mortality has tons of percentages in its various tables. (Hospital was the unit of analysis; n = 168 of them.) There were several letters to the editor of that journal in early 2004 regarding the article. I suggest that you read the article, the letters, and the rejoinder by Aiken et al., and make your own judgment. As they say on the Fox News Channel, "I report, you decide".

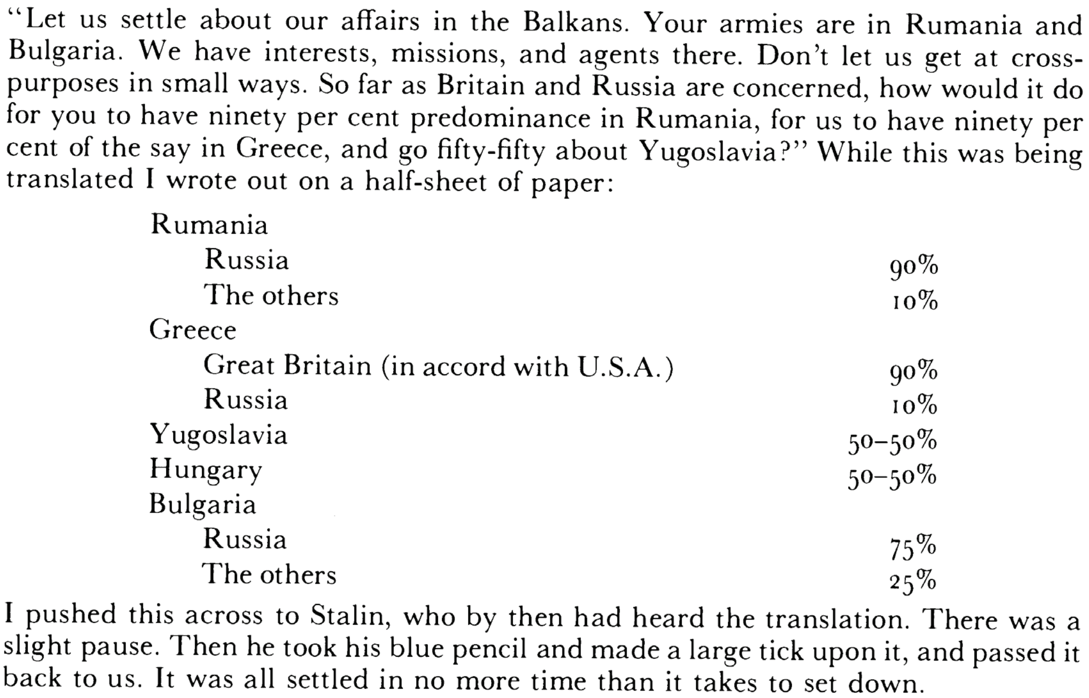
Azar (2004, 2007, 2008) has written several papers on "percentage thinking". Economists claim that many people behave irrationally when making shopping saving decisions by focusing on percentage saving rather than absolute saving. He cites the classic example (Thaler, 1980; Darke and Freedman, 1993) of a person who exerts more effort to save $5 on a $25 radio than on a $500 TV. It's the same $5. (See also Chen & Rao, 2007, for comparable examples.) Fascinating stuff.

Freedman, Pisani, & Purves (2007). This is far and away the best statistics textbook ever written (in my opinion), the illustrations are almost as hilarious as those in Darrell Huff's books, and there is some great stuff on percentages. (My favorite illustration is a cartoon in which a prospective voter says to a politician "I'm behind you 100 percent, plus or minus 3 percent or so" .) Check it out!

Gonick and Smith (1993). If you want to learn statistics on your own, and have a lot of laughs in the process, this book is for you. Through a combination of words, formulas, and cartoons (mostly cartoons, by Gonick) the authors summarize nicely most of the important concepts in statistics, both descriptive and inferential. My favorite cartoon in the book is the one on page 2 picturing a statistician dining with his date. He says to her: "I'm 95% confident that tonight's soup has probability between 73% and 77% of being really delicious!" They even discuss the probability of a disease given a positive diagnosis (pp. 46-50) and the estimation of confidence intervals for percentages--actually proportions (pp. 114-127) that we talked about in Section 3 and Section 5, respectively, in this chapter (but without the great illustrations that Gonick provides).

Paulos (2008). In this companion to his Innumeracy book (he really has a way with words!), Paulos claims that the arguments for the existence of God don't add up, and he closes the book with the tongue-in-cheek claim that "96.39 per cent" of us want to have a world that is closer to a heaven on earth than it is now. Amen.

Resis (1978). In what must be one of the most important applications of percentages known to mankind, Resis described a meeting in 1944 in which Winston Churchill suggested to Josef Stalin a way of dividing up European spheres of influence between Britain and Russia. On page 368 he cited Churchill's actual words, as follows:



"Let us settle about our affairs in the Balkans. Your armies are in Rumania and Bulgaria. We have interests, missions, and agents there. Don't let us get at cross-purposes in small ways. So far as Britain and Russia are concerned, how would it do for you to have ninety per cent predominance in Rumania, for us to have ninety per cent of the say in Greece, and go fifty-fifty about Yugoslavia?" While this was being translated I wrote out on a half-sheet of paper:

Rumania	
Russia	90%
The others	10%
Greece	
Great Britain (in accord with U.S.A.)	90%
Russia	10%
Yugoslavia	50–50%
Hungary	50–50%
Bulgaria	
Russia	75%
The others	25%

I pushed this across to Stalin, who by then had heard the translation. There was a slight pause. Then he took his blue pencil and made a large tick upon it, and passed it back to us. It was all settled in no more time than it takes to set down.

For some additional interesting information regarding this matter, just google "percentages agreement" [not to be confused with "percent agreement", which is a way of determining reliability]).

Robbins & Robbins (2003a and 2003b). This pair of articles represents one of the strangest, yet interesting, applications of percentages I have ever seen. The authors have collected data for estimating the percentage of people (both men and women) who have hair of various lengths! Read both articles. You'll like them.

Thibadeau (2000). It's hard to know whether Thibadeau is serious or not when he presents his arguments for doing away with all taxes and replacing all paper money and coins with electronic currency. But this is a delightful read (free, on the internet) and he has several interesting comments regarding percentages. My favorite one is in the section on sales taxes, where he says:

> " …sales tax is almost always a strange percentage like 6% or 7%. If something costs $1, we have to take the time to figure out whether the guy is giving the proper change on $1.07 for the five dollar bill. Most people don't check. " (p. 20)

Some great websites that I haven't previously mentioned:

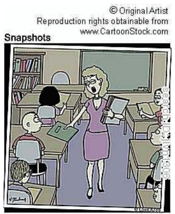
1. RobertNiles.com was developed by Robert Niles and is intended primarily for journalists who need to know more about mathematics and statistics. He has a particularly nice discussion of percentages.

2. Dr. Ray L. Winstead's website has a "Percentage metric time" clock that tells you at any time of any day what percentage of the day (to four decimal places!) has transpired. How about that?!

3. The website for the physics department at Bellevue College (its name is scidiv.bellevuecollege.edu/Physics/.../F-Uncert-Percent.html) calculates for you both the "absolute percentage certainty" and the "relative percentage certainty" of any obtained measurement. All you need do is input the measurement and its margin of error. Nice.

4. The Healthy People 2010 website has all sorts of percentages among its goals for the year 2010. For example, it claims that 65% of us are presently exposed to second-hand smoke [I think that is too high]; its goal is to reduce that to 45%.

5. The CartoonStock website has some great percentage cartoons. Here are two of the best (be sure to "zoom" in at 200%):



"Congratulations, Phillip. You've managed to score somehow lower than chance."

"NINETY-NINE PERCENT OF LAWYERS GIVE THE REST OF US A BAD NAME."

6. There is a downloadable file called Baker's Percentage (just google those words) that provides the ingredients for various recipes as percentages of the weight of the principal ingredient (usually flour). Unfortunately (in my opinion) all of the weights of the ingredients are initially given in grams rather than in ounces.

7. www.StatPages.org is John Pezzullo's marvelous website, which will refer you to sources for calculating just about any descriptive statistic you might be interested in, as well as carry out a variety of inferential procedures.

It's been fun for me. I hope it has been for you also.

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CHAPTER 16: THE UNIT JUSTIFIES THE MEAN

Introduction

How should we think about the mean? Let me count the ways:

1. It is the sum of the measurements divided by the number of measurements.

2. It is the amount that would be allotted to each observation if the measurements were re-distributed equally.

3. It is the fulcrum (the point at which the measurements would balance).

4. It is the point for which the sum of the deviations around it is equal to zero.

5. It is the point for which the sum of the squared deviations around it is a minimum.

6. It need not be one of the actual measurements.

7. It is not necessarily in or near the center of a frequency distribution.

8. It is easy to calculate (often easier than the median, even for computers).

9. It is the first moment around the origin.

10. It requires a unit of measurement; i.e., you have to be able to say the mean "what".

I would like to take as a point of departure the first and the last of these matters and proceed from there.

Definition

Everybody knows what a mean is. You've been calculating them all of your lives. What do you do? You add up all of the measurements and divide by the number of measurements. You probably called that "the average", but if you've taken a statistics course you discovered that there are different kinds of averages. There are even different kinds of means (arithmetic, geometric, harmonic), but it is only the arithmetic mean that will be of concern in this chapter, since it is so often referred to as "the mean".

The mean what

The mean always comes out in the same units that are used in the scale that produced the measurements in the first place. If the measurements are in inches, the mean is in inches; if the measurements are in pounds, the mean is in pounds; if the measurements are in dollars, the mean is in dollars; etc.

Therefore, the mean is "meaningful" for interval-level and ratio-level variables, but it is "meaningless" for ordinal variables, as Marcus-Roberts and Roberts (1987) so carefully pointed out. Consider the typical Likert-type scale for measuring attitudes. It usually consists of five categories: strongly disagree, disagree, no opinion, agree, and strongly agree (or similar verbal equivalents). Those five categories are most frequently assigned the numbers 1,2,3,4,and 5, respectively. But you can't say 1 what, 2 what, 3 what, 4 what, or 5 what.

The other eight "meanings of the mean" all flow from its definition and the requirement of a unit of measurement. Let me take them in turn.

Re-distribution

This property is what Watier, Lamontagne, and Chartier (2011) call (humorously but accurately) "The Socialist Conceptualization". The simplest context is financial. If the mean income of all of the employees of a particular company is equal to x dollars, x is the salary each would receive if the total amount of money paid out in salaries were distributed equally to the employees. (That is unlikely to ever happen.) A mean height of x inches is more difficult to conceptualize, because we rarely think about a total number of inches that could be re-distributed, but x would be the height of everybody in the group, be it sample or population, if they were all of the same height. A mean weight of x pounds is easier to think of than a mean height of x inches, since pounds accumulate faster than inches do (as anyone on a diet will attest).

Fulcrum (or center of gravity)

Watier, et al. (2011) call this property, naturally enough, "The Fulcrum Conceptualization". Think of a see-saw on a playground. (I used to call them teeter-totters.) If children of various weights were to sit on one side or the other of the see-saw board, the mean weight would be the weight where the see-saw would balance (the board would be parallel to the ground).

The sum of the positive and negative deviations is equal to zero

This is actually an alternative conceptualization to the previous one. If you subtract the mean weight from the weight of each child and add up those differences ("deviations") you get zero, again an indication of a balancing point.

The sum of the squared deviations is a minimum

This is a non-intuitive (to most of us) property of the mean, but it's correct. If you take any measurement in a set of measurements other than the mean and calculate the sum of the squared deviations from it, you always get a larger number. (Watier, et al., 2011, call this "The Least Squares Conceptualization".) Try it sometime, with a small set of numbers such as 1,2,3, and 4.

It doesn't have to be one of the actual measurements

This is obvious for the case of a seriously bimodal frequency distribution, where only two different measurements have been obtained, say a and b. If there is the same numbers of a's as b's then the mean is equal to (a+b)/2. But even if there is not the same number of a's as b's the mean is not equal to either of them.

It doesn't have to be near the center of the distribution

This property follows from the previous one, or vice versa. The mean is often called an indicator of "the central tendency" of a frequency distribution, but that is often a misnomer. The median, by definition, must be in the center, but the mean need only be greater than the smallest measurement and less than the largest measurement.

It is easy to calculate

Compare what it is that you need to do in order to get a mean with what you need to do in order to get a median. If you have very few measurements the amount of labor involved is approximately the same: Add (n-1 times) and divide (once); or sort and pick out. But if you have many measurements it is a pain in the neck to calculate a median, even for a computer (do they have necks?). Think about it. Suppose you had to write a computer program that would calculate a median. The measurements are stored somewhere and have to be compared with one another in order to put them in order of magnitude. And there's that annoying matter of an odd number vs. an even number of measurements.

To get a mean you accumulate everything and carry out one division. Nice.

The first moment

Karl Pearson, the famous British statistician, developed a very useful taxonomy of properties of a frequency distribution. They are as follows:

The first moment (around the origin). This is what you get when you add up all of the measurements and divide by the number of them. It is the (arithmetic) mean. The term "moment" comes from physics and has to do with a force around a certain point..

The first moment around the mean. This is what you get when you subtract the mean from each of the measurements, add up those "deviations", and divide by the number of them. It is always equal to zero, as explained above.

The second moment around the mean. This is what you get when you take those deviations, square them, add up the squared deviations, and divide by the number of them. It is called the variance, and it is an indicator of the "spread" of the measurements around their mean, in squared units. Its square root is the standard deviation, which is in the original units.

The third moment around the mean. This is what you get when you take the deviations, cube them (i.e., raise them to the third power), add them up, divide by the number of deviations, and divide that by the cube of the standard deviation. It provides an indicator of the degree of symmetry or asymmetry ("skewness") of a distribution.

The fourth moment around the mean. This is what you get when you take the deviations, raise them to the fourth power, add them up, divide by the number of them, and divide that by the fourth power of the standard deviation. It provides an indicator of the extent of the kurtosis ("peakedness") of a distribution.

What about nominal variables in general and dichotomies in particular?

I hope you are now convinced that the mean is OK for interval variables and ratio variables, but not OK for ordinal variables. In 1946 the psychologist S.S. Stevens claimed that there were four kinds of variables, not three. The fourth kind is nominal, i.e., a variable that is amenable to categorization but not very much else. Surely if the mean is inappropriate for ordinal variables it must be inappropriate for nominal variables? Well, yes and no.

Let's take the "yes" part first. If you are concerned with a variable such as blood type, there is no defensible unit of measurement like an inch, a pound, or a dollar. There are eight different blood types (A+, A-, B+, B-, AB+, AB-, O+, and O-). No matter how many of each you have, you can't determine the mean blood type. Likewise for a variable such as religious affiliation. There are lots of categories (Catholic, Protestant, Jewish, Islamic,...,None), but it wouldn't make any sense to assign the numbers 1,2,3,4,..., k to the various categories, calculate the mean, and report it as something like 2.97.

Now for the "no" part. For a dichotomous nominal variable such as sex (male, female) or treatment (experimental, control), it is perfectly appropriate (alas) to CALCULATE a mean, but you have to be careful about how you INTERPRET it. The key is the concept of a "dummy" variable. Consider, for example, the sex variable. You can call all of the males "1" (they are male) and all of the females "0" (they are not). Suppose you have a small study in which there are five males and ten females. The "mean sex" (sounds strange, doesn't it?) is equal to the sum of all of the measurements (5) divided by the number of measurements (15), or .333. That's not .333 "anythings", so there is still no unit of measurement, but the .333 can be interpreted as the PROPORTION of participants who are male (the 1's). It can be converted into a percentage by multiplying by 100 and affixing a % sign, but that wouldn't provide a unit of measurement either.

There is an old saying that "there is an exception to every rule". This is one of them.

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CHAPTER 17: THE MEDIAN SHOULD BE THE MESSAGE

Introduction

In one of his most poignant essays, the American paleontologist Stephen Jay Gould (1985) argued against a fixation on the median amount of life remaining (eight months) for people who suffer from mesothelioma, a metastasis from which he died 17 years later. He appealed to the frequency distribution of that variable, which is positively skewed, and hoped he would find himself far out in the right-hand tail (which he did). The title of his essay was "The median is not the message", a play upon the famous quote by Marshall McLuhan (1964) that "The medium is the message".

Gould's anti-median argument was based upon the distinction between a summary measure (the median) and the distribution to which it applies. In what follows I would like to present a pro-median argument, not because I favor the sole reliance on a single summary measure but because in my opinion it is the best we have. I shall also point out some of its weaknesses and necessary modifications, especially for ordinal variables for which there is no unit of measurement. Thus the title of this chapter.

The usual discussion in statistics textbooks

Almost every statistics textbook includes a section or an entire chapter on the advantages and disadvantages of various measures of "central tendency". The emphasis is most often placed upon the (arithmetic) mean, the median, and the mode, although attention is sometimes given to the geometric mean and the harmonic mean. The mean is usually preferred for continuous variables that are normally or near-normally distributed, largely because the mathematical statisticians know so much about the normal distribution and students in statistics courses have been calculating means all of their lives (having called them ”averages"). The median is a better indicator of "averageness" for variables that are highly skewed, e.g., income. [In his textbook, Pezzullo (2013) rightly contends that of the three the median is the only one that must be near the center of the distribution.] The mode is often denied any serious consideration, except for distributions that have two or more peaks. (Geometric means and/or harmonic means are of interest only in some of the physical sciences.)

The very best case for the median: Likert-type scales

In 1932 the American psychologist Rensis Likert (pronounced "lick-ert", not "like-ert") suggested the use of 5-point or 7-point scales for measuring attitudes, with the 5-point version being far and away the more popular... even today. The usual verbal labels are (1) Strongly disagree; (2) Disagree; (3) Undecided (neutral, no opinion); (4) Agree; and (5) Strongly agree. Each person is given a statement such as "Marijuana should be legalized" and is asked to provide the response that best represents his(her) opinion. There is a huge literature on Likert-type scales in which various aspects of their use are hotly debated, with the bases for controversy being matters such as "Why not an even number of scale points?"; "Are they interval scales or ordinal scales?"; and "What kinds of statistics are appropriate for analyzing data obtained for such scales?" It is to the last of these questions that I would now like to turn.

My personal opinion is that the median, and only the median, should be used to summarize the data for Likert-type scales, and there are some problems with that. Consider, for example, responses such as the following for six persons on a 5-point scale: 3, 3, 3, 4, 5, 5. What is the median of those numbers? Most authors of statistics textbooks would say 3.5 (the mean of the two middle numbers 3 and 4). I strongly disagree (please forgive the lame attempt at humor), for two very important reasons: calculating a mean for an ordinal scale is not appropriate (they have no unit of measurement); and 3.5 is not one of the scale points, so it doesn't make sense. Further embellishing on this second reason, I go even further by arguing that numbers should not be used for such scales; letters are both necessary and sufficient. (See the following chapter.) The response choices should be A (not 1), B (not 2), C (not 3), D (not 4), and E (not 5); i.e., the data are C, C, C, D, E, E. What is their median? They don't have one. But it's perfectly OK to claim that the median is undefined for that dataset. It doesn't have a mode either...or has two modes (a major mode of C and a minor mode of E). It also has no mean (even if appropriate, which it isn't, since you can't find the mean of a set of letters).

Academic grades and "Grade point averages (GPAs)"

Speaking of A, B, C, D, and E brings me to the matter of how academic grades are assigned and summarized. In most American high schools and colleges an A is given 4 points, a B is given 3 points, a C is given 2 points, a D is given 1 point, and an E (sometimes F rather than E) is given 0 points. Pluses and minuses are often awarded, with half a point usually added or subtracted. For example, a B- would be given 2.5 points, as would a C+, although some graders would assign a few more points to a B- than to a C+. And to summarize a student's achievement over the span of a quarter, a semester, a year, or an entire program of studies, such grades (the "points") are added together and divided by the number of courses taken, with or without first weighting each by the associated number of credit hours. That is a terrible system, as explained by Chansky (1964) several years ago. Here are some of its weaknesses:

a. Grade in course is an ordinal variable, much like a Likert-type scale. A grade point is a totally arbitrary entity. Unlike a dollar, a year, an inch, or a pound, a point is not an actual unit of measurement. You can't say "4 what", for example.

b. When pooling across individual grades it is inappropriate to get an average (arithmetic mean), for the same reason.

c. Even if it were defensible to do so (find the mean of the grades), the median of a person's grades (a letter, not a number) is much more reflective of his(her) typical achievement than the mean of such grades, irrespective of their distribution.

Statistical inferences for measures of central tendency

The arithmetic mean is also preferred as far as availability of methods for testing the statistical significance of a sample mean or putting a confidence interval around it are concerned. Its standard error ("sigma over the square root of n") is well-known and easily applied to practical problems such as the estimation of the mean height of a population of adult males, as long as the distribution of heights is normal or the sample size is large enough to invoke the Central Limit Theorem. Formulas for the standard error of the median are not readily available in most statistics textbooks. However, many years ago Walsh (1949) showed that there are methods for testing hypotheses about medians under certain reasonable conditions.

But there's more. Since for a normal distribution the mean, median, and mode are all equal to one another, if you know, or can assume, that the population distribution is normal, an inference for a population mean (based upon either a significance test or a confidence interval) automatically provides an inference for the population median and the population mode.

Some nonparametric tests for medians

The Sign Test

The sign test has a number of different applications. Here I shall consider the test of a hypothesis that a population median is equal to a particular value. As an example, consider the following artificial data on page 124 of the 1986 Minitab Reference Manual:

0,50,56,72,80,80,80,99,101,110,110,110,120,140,150,180,201,210,220,240,290, 309,320,325,400,500,507 (sample median = 144)

Minitab provides methods for testing a hypothesis about a population median and for putting a confidence interval around the sample median. For the above example, the null hypothesis that the population median = 115 (allegedly the current standard) against the alternative hypothesis that the population median is greater than 115 cannot be rejected at the .05 level (one-tailed test), despite the fact that 144 is considerably greater than 115. Minitab can also carry out two-tailed tests and approximate two-sided 95% confidence intervals. For those same data an interval from 110 to 210 would correspond to 93.9% confidence, and an interval from 101 to 220 would correspond to 97.6%. See pp. 124-125 of the 1986 manual for the details.

The Kolmogorov-Smirnov Test

The versatile but seldom used Kolmogorov-Smirnov (K-S) test for two independent samples might be an excellent choice for testing the significance of the difference between two sample medians, especially if the maximum difference between the two cumulative relative frequency distributions happens to fall at or near their medians. Consider the following example, taken from Goodman (1954), and see also Chapter 28 in this book:

Sample 1: 1, 2, 2, 2, 2, 4, 4, 4, 4, 5, 5, 5, 5, 5, 5 (n_1 = 15; median = 4)
Sample 2: 0, 0, 0, 0, 1, 1, 2, 2, 2, 2, 3, 3, 5, 5, 5 (n_2 = 15; median = 2)

The frequency distributions for Sample 1 are:

Value	Freq.	Rel. Freq.	Cum. Freq.	Cum. Rel. Freq.
0	0	0/15 = 0	0	0/15 = 0
1	1	1/15 = .067	1	1/15 = .067
2	4	4/15 = .267	5	5/15 = .333
3	0	0/15 = 0	5	5/15 = .333
4	4	4/15 = .267	9	9/15 = .600
5	6	6/15 = .400	15	15/15 = 1.000

The corresponding frequency distributions for Sample 2 are:

Value	Freq.	Rel. Freq.	Cum. Freq.	Cum. Rel. Freq.
0	4	4/15 = .267	4	4/15 = .267
1	2	2/15 = .133	6	6/15 = .400
2	4	4/15 = .267	10	10/15 = .667
3	2	2/15 = .133	12	12/15 = .800
4	0	0/15 = 0	12	12/15 = .800
5	3	3/15 = .200	15	15/15 = 1.000

The test statistic for the K-S test is the largest difference, D, between corresponding cumulative relative frequencies for the two samples. For this example the largest difference is for scale value 3, for which D = .800 - .333 = .467. How likely is such a difference to be attributable to chance? Using the appropriate formula and/or table and/or computerized routine, the corresponding p-value is .051 (two-tailed). If the pre-specified level of significance, α, is .05 and the alternative hypothesis is non-directional, the null hypothesis of no difference between the two population distributions cannot be rejected.

There are also procedures for constructing confidence intervals around D. See Sheskin (2011) for the details. And for more on the K-S test, see Chapter 28 of this book.

The Mann-Whitney Test

Buthmann (2008a) claimed that the Mann-Whitney (M-W) test, sometimes called the Wilcoxon test, is fine for testing the difference between medians. The observations are rank-ordered irrespective of group designation, and the difference between the mean rank for Sample 1 and the mean rank for Sample 2 is tested for statistical significance, which is alleged to constitute a test of the difference between the medians of the two samples. Hart (2001) and Campbell (2006) both contend that the matter is a bit complicated, because the shapes of the distributions also have to be taken into account.

Mood's Median Test

In a highly technical article, Mood (1954) discussed the relative asymptotic efficiency of several non-parametric tests for comparing two independent samples. Included among them was The Median Test, which he and his colleague had developed a few years before that (Brown & Mood, 1951). He showed that it generally had lower power than most other non-parametric approaches such as Mann-Whitney. Despite his acknowledgment of low power the test continued to be used for several years and was designated as Mood's Median Test. More recently Freidlin and Gastwirth (2000) argued that Mood's Median Test should no longer be used in statistical applications. [See also Buthmann (2008b).]

I prefer the K-S Test.

<u>One more (and last?) weakness of the median</u>

Suppose you have to write a computer program for calculating the mean and the median of a set of data. The mean is easier, because all it entails is the summation of n numbers and one division by n at the end. Summation goes very fast with computers and no other decisions need to be made. The median is more complicated. The computer program must first sort the data and then make several comparisons with the data [2n of them, according to Bent & John (1985)], to say nothing of resolving the dilemma of an odd number of numbers vs. an even number of numbers. As the data "come in", e,g., the 3,3,3,4,5,5 of the above example, some sort of algorithm must be created to produce "the median". [See the article by Tibshirani (2008) for a faster way of calculating the median.]

Fortunately, there already exist computer programs for calculating the median. Unfortunately, all of them [as far as I know] take the mean of the middle two numbers as the median for an even number of observations.

<u>The order of the mean, median, and mode in a positively skewed distribution</u>

The authors of some statistics textbooks claim that for a skewed-to-the-right distribution the mode is always less than the median, and the median is always less than the mean. [For a left-skewed distribution they are said to be always in the reverse order.] As von Hippel (2005) and Lesser (2005) explained, that is not true ["always" is too strong; "usually" is much better]. von Hippel gave an example of a positively skewed distribution for which there were so many observations at the median that there was not an equal number of observations on either side of it, resulting in the mean being less than the median for positive skew. Lesser gave a simpler example, for the binomial sampling distribution with p = .10 and n = 10, which is also positively skewed and for which the mean is also less than the median.

Reprise: How about between-group comparisons for Likert-type ordinal scales?

Can we use medians to compare a group of three people whose responses for a 5-point ordinal scale are ABC [median = B] with a group of three people whose responses are CDE [median = D], both descriptively and inferentially? Let's see how we might proceed.

Consider a relatively simple case for a small finite population for which the population size is five. The two sample medians are obviously not the same. The first median of B represents an over-all level of disagreement; the second median of D represents an over-all level of agreement. Should we subtract the two (D - B) to get C? No, that would be awful. Addition and subtraction are not defensible for ordinal scales, and even if they were, a resolution of C [undecided] wouldn't make any sense. If the two groups were random samples, putting a confidence interval around that difference would be even worse.

Testing the significance of the "difference" between the two medians, but not by subtracting, is tempting. How might we do that? If the two groups were random samples from their respective populations, we would like to test the hypothesis that they were drawn from populations that have the same median. We don't know what that median-in-common is [call it X, which would have to be A,B,C,D, or E], but we could try to determine the probability of getting, by chance, a median of B for one random sample and a median of D for another random sample, when the median in both populations is equal to X, where X = A or B or C or D or E.

Suppose X = A. How many ways could we get a median of B in a random sample of three observations? Here is a list of the possibilities:

ABB
ABC [what we actually got for the first sample]
ABD
ABE
BBB

BBC
BBD
BBE

If I've calculated properly, there are 35 different sample results for 3 observations on a 5-point scale, 8 of which produce a sample median of B. Knowing nothing about the median in the population, the probability of getting a sample median of B is therefore 8/35 or approximately .229. But if the population median is A then the probability of getting a sample median of B should be more likely because 4 of those 8 possibilities include one A.

The only way that A can be the population median for 5 observations is to have three A's among those 5, so that there is an A in the middle. There are 15 such combinations: AAAAA, AAAAB, AAAAC, AAAAD, AAAAE, AAABB, AAABC, AAABD, AAABE, AAACC, AAACD, AAACE, AAADD, AAADE, and AAAEE. When sampling from that population the probability of getting ABC is 1/15, or approximately .067 [when the observations in the population are AAABC]. The probability of getting CDE is zero. So it is highly unlikely [impossible?] that the two samples came from the same population with a median of A.

If X = B, there are 30 ways of getting a population median of B. The probability of getting ABC, with a sample median of B, is 8/30, or approximately .267. The probability of getting CDE is again zero.

If X = C, there are 32 ways in which the population median can be C. The probability of getting ABC, with a sample median of B, is 6/36, or approximately .167. The probability of getting CDE, with a sample median of D, is also 6/36. [That figures, since ABC and CDE are "equally close" to C.]

If X = D, there are 30 ways in which the population median can be D. The probability of getting ABC, with a sample median of B, is zero [not surprisingly because of the symmetry with a population median of B] and the probability of CDE, with a sample median of D, is .267.

If X = E, the probability of getting ABC, with a median of B, is zero, and the probability of CDE, with a median D, is 1/15 = approximately .067.

Putting all of this together, we have:

Population median	Pr. (ABC)	Pr. (CDE)
A	.067	0
B	.267	0
C	.167	.167
D	0	.167
E	0	.067

Are ABC and CDE significantly different? Certainly if the population median is A,B,D, or E. But not if it is C. What is the probability of each of those population medians? That's a Bayesian question that a frequentist like me doesn't know how to answer.

Does all of this make sense? If not, there's always the bootstrap and the jackknife. I prefer the latter [I might be the only one who does] because I don't like sampling with replacement.

If you're still not convinced that the median is to be preferred to the mean for ordinal scales, please read the article by Marcus-Roberts and Roberts (1987). They gave an example regarding the comparison of two groups for which the mean for Group 1 was higher than the mean for Group 2, yet for a defensible monotonic data transformation the mean for Group 1 was lower than the mean for Group 2. That doesn't happen with medians.

A final note

A recent article by Hellmann, Kris, and Rudin (2016) picks up where Gould left off, but concentrates on "milestones" (one year, two year, and five-year survival points) rather than on the frequency distribution of survival time. And for everything you've wanted to know about medians, and then some, see the entry for "Median" in Wikipedia. There is a discussion of a new [to me, anyhow] statistic called a medoid, which can be used for an even number of observations when you don't like to take the mean of the middle two numbers [which I don't like to do].

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CHAPTER 18: MEDIANS FOR ORDINAL SCALES SHOULD BE LETTERS, NOT NUMBERS

Introduction

Near the end of the previous chapter I cited an under-appreciated article by Marcus-Roberts and Roberts (1987) entitled "Meaningless statistics". On page 347 they gave an example of a five-point ordinal scale for which School 1 had a lower mean than School 2, but for a perfectly defensible monotonic transformation of that scale School 1 had the higher mean. The authors claimed that we shouldn't compare means that have been calculated for ordinal scales. I wholeheartedly agree. We should compare medians.

The matter of the appropriateness of means, standard deviations, and Pearson r's for ordinal scales has been debated for many years, starting with S.S. Stevens' (1946) proscription. I even got myself embroiled in the controversy, twice (Knapp, 1990, 1993).

What this chapter is not about

I am not concerned with the situation where the "ordinal scale" consists merely of the rank-ordering of observations, i.e., the data are ranks from 1 to n, where n is the number of things being ranked. I am concerned with ordinal ratings, not rankings. (Ratings and rankings aren't the same thing; see Chapter 10.)

The purpose of the present chapter

In this chapter I make an even stronger argument than Marcus-Roberts and Roberts made: If you have an ordinal scale, you should always report the median as one of the ordered categories, using a letter and not a number.

Two examples

1. You have a five-categoried grading scale with scale points A, B, C, D, and E (the traditional scale used in many schools). You have data for a particular student who took seven courses and obtained the following grades, from lowest to highest: D,C,C,B,B,B,A (there were no E's). The median grade is the fourth lowest (which is also the fourth highest), namely B. You don't need any numbers for the categories, do you?

2. You have a five-categoried Likert-type scale with scale points a (strongly disagree), b(disagree), c(undecided), d(agree) and e(strongly agree).

First dataset: You have data for a group of seven people who gave the responses a,b,b,b,c,d,e. The median is b (it's also the mode). No need for numbers.

Second dataset: You have data for a different group of seven people. Their responses were a,b,c,d,d,d,d (there were no e's). The median is d. Still no need for numbers.

Third dataset: You have data for a group of ten people who gave the following responses: a,a,b,b,b,c,c,c,d,d (still no e's). What is the median? I claim there is no median for this dataset; i.e., it is indeterminate.

Fourth dataset: You have data for a group of ten people who gave the following responses: a,a,a,a,a,e,e,e,e,e. There is no median for that dataset either.

Fifth dataset: You have the following data for a group of sixteen people who gave the following responses: a,b,b,b,b,c,c,c,c,c,c,d,d,d,d,e. That's a very pretty distribution (frequencies of 1, 4, 6, 4, and 1); it's as close to a normal distribution you can get for sixteen observations on that five-point scale (the frequencies are the binomial coeficients for n = 4). But normality is not necessary. The median is c (a letter, not a number).

What do most people do?

I haven't carried out an extensive survey, but I would venture to say that for those examples most people would assign numbers to the various categories, get the data, put the obtained numerical scores in order, and pick out the one in the middle. For the letter grades they would probably assign the number 4 to an A, the number 3 to a B, the number 2 to a C, the number 1 to a D, and the number 0 to an E. The data would then be 1,2,2,3,3,3,4 for the person and the median would be 3. They might even calculate a "grade-point average" (GPA) for that student by adding up all of those numbers and dividing by 7.

For the five datasets for the Likert-type scale they would do the same thing, letting strongly disagree = 1, disagree = 2, undecided = 3, agree = 4, and strongly agree = 5. The data for the third dataset would be 1,1,2,2,2,3,3,3,4,4, with a median of 2.5 (they would "split the difference" between the middle two numbers, a 2 and a 3, i.e., they would add the 2 and the 3 to get 5 and divide by 2 to get 2.5). The data for the fourth dataset would be 1,1,1,1,1,5,5,5,5,5, with a median of 3, again by adding the two middle numbers, 1 and 5, to get 6 and dividing by 2 to get 3.

What's wrong with that?

Lots of things. First of all, you don't need to convert the letters into numbers; the letters work just fine by themselves. Secondly, the numbers 1,2,3,4,and 5 for the letter grades and for the Likert-type scale points are completely arbitrary; any other set of five increasing numbers would work equally well. Finally, there is no justification for the splitting of the difference between the middle two numbers of the third dataset or the fourth dataset. You can't add numbers for such scales;

there is no unit of measurement and the response categories are not equally spaced. For instance, the "difference" between a 1 and a 2 is much smaller than the "difference" between a 2 and a 3. That is, the distinction between strongly disagree and disagree is minor (both are disagreements) compared to the distinction between disagree and undecided. Furthermore, the median of 2.5 for the third dataset doesn't make sense; it's not one of the possible scale values. The median of 3 for the fourth dataset is one of the scale values, but although that is necessary it is not sufficient (you can't add and divide by 2 to get it).

[I won't even begin to get into what's wrong with calculating grade-point averages. See Chansky (1964) if you care. His article contains a couple of minor errors, e.g., his insistence that scores on interval scales have to be normally distributed, but his arguments against the usual way to calculate a GPA are very sound.]

But, but,...

I know. People have been doing for years what Marcus-Roberts and Roberts, and I, and others, say they shouldn't.

How can we compare medians with means and modes without having any numbers for the scale points? Good question. For interval and ratio scales go right ahead, but not for ordinal scales; means for ordinal scales are a no-no (modes are OK).

How about computer packages such as Excel, Minitab, SPSS, and SAS? Can they spit out medians as letters rather than numbers? Excel won't calculate the median of a set of letters, but it will order them for you (using the Sort function on the Data menu), and it is a simple matter to read the sorted list and pick out the median. My understanding is the other packages can't do it (my friend Matt Hayat confirms that both SPSS and SAS insist on numbers). Not being a computer programmer I don't know why, but I'll bet that it would be no harder to sort letters (there are only 26 of them) than numbers (there are lots of them!) and perhaps even easier than however they do it to get medians now.

How can I defend my claim about the median for the third and fourth datasets for the Likert-type scale example? Having an even number of observations is admittedly one of the most difficult situations to cope with in getting a median. But we are able to handle the case of multiple modes (usually by saying there is no mode) so we ought to be able to handle the case of not being able to determine a median (by saying there is no median).

How about between-group comparisons?

All of the previous examples were for one person on one scale (the seven grades) or for one group of persons on the same scale (the various responses for the Likert-type scale). Can we use medians to compare the responses for the group of seven people whose responses were a,b,b,b,c,d,e (median = b) with the group of seven people whose responses were a,b,c,d,d,d,d (median = d), both descriptively and inferentially? That is the 64-dollar question (to borrow a phrase from an old radio program). But let's see how we might proceed.

The two medians are obviously not the same. The first median of b represents an over-all level of disagreement; the second median of d represents an over-all level of agreement. Should we subtract the two (d - b) to get c? No, that would be awful. Addition and subtraction are not defensible for ordinal scales, and even if they were, a resolution of c (undecided) wouldn't make any sense. If the two groups were random samples, putting a confidence interval around that difference would be even worse.

Testing the significance of the "difference" between the two medians, but not by subtracting, is tempting. How might we do that? If the two groups were random samples from their respective populations, we would like to test the hypothesis that they were drawn from populations that have the same median. We don't know what that median-in-common is (call it x, which would have to be a,b,c,d,or e), but we could try to determine the probability of getting, by chance, a median of b for one random sample and a median of d for another random sample, when the median in both populations is equal to x, for all x = a,b,c,d,and e. Sound doable? Perhaps, but I'm sure it would be hard. Let me give it a whirl. If and when I run out of expertise I'll quit and leave the rest as an "exercise for the reader" (you).

OK. Suppose x =a. How many ways could I get a median of b in a random sample of seven observations? Does a have to be one of the observations? Hmmm; let's start by assuming yes, there has to be at least one a. Here's a partial list of possibilities:

a,b,b,b,c,c,c
a,b,b,b,c,c,d
a,b,b,b,c,c,e
a,b,b,b,c,d,d
a,b,b,b,c,d,e (the data we actually got for the first sample)
a,b,b,b,c,e,e
a,a,b,b,c,c,c
a,a,b,b,c,c,d
a,a,b,b,c,c,e
a,a,b,b,c,d,d
...
I haven't run out of expertise yet, but I am running out of patience. Do you get the idea? But there's a real problem. How do we know that each of the possibilities are equally likely? It would intuitively seem (to me, anyhow) that a

sample of observations with two a's would be more likely than a sample of observations with only one a, if the population median is a, wouldn't it?

One more thing

I thought it might be instructive to include a discussion of a sampling distribution for medians (a topic not to be found in most statistics books). Consider the following population distribution of the seven spectrum colors for a hypothetical situation (colors of pencils for a "lot" in a pencil factory?)

Color	Frequency
Red (R)	1
Orange (O)	6
Yellow (Y)	15
Green (G)	20
Blue (B)	15
Indigo (I)	6
Violet (V)	1

That's a nice, almost perfectly normal, distribution (the frequencies are the binomial coefficients for n = 6). The median is G. [Did your science teacher ever tell you how to remember the names of the seven colors in the spectrum? Think of the name Roy G. Biv.]

Suppose we take 100 random samples of size five each from that population, sampling without replacement within sample and with replacement among samples. I did that; here's what Excel and I got for the empirical sampling distribution of the 100 medians: [Excel made me use numbers rather than letters for the medians, but that was OK; I transformed back to letters after I got the results.]

Median	Frequency
O	1
Y	25
G	51
B	22
I	1

You can see that there were more medians of G than anything else. That's reasonable because there are more Gs in the population than anything else. There was only one O and only one I, There couldn't be any Rs or Vs; do you know why?

Summary

In this chapter I have tried, hopefully at least partially successfully, to create an argument for never assigning numbers to the categories of an ordinal scale and to always report one of the actual categories as the median for such a scale.

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CHAPTER 19: INVESTIGATING THE RELATIONSHIP BETWEEN TWO VARIABLES

"What is the relationship between X and Y?", where X is one variable, e.g., height, and Y is another variable, e.g., weight, is one of the most common research questions in all of the sciences. But what do we mean by "the relationship between two variables"? Why do we investigate such relationships? How do we investigate them? How do we display the data? How do we summarize the data? And how do we interpret the results? In this chapter I discuss various approaches that have been taken, including some of the strengths and weaknesses of each.

The ubiquitous research question

"What is the relationship between X and Y?" is, and always has been, a question of paramount interest to virtually all researchers. X and Y might be different forms of a measuring instrument. X might be a demographic variable such as sex or age, and Y might be a socioeconomic variable such as education or income. X might be an experimentally manipulable variable such as drug dosage and Y might be an outcome variable such as survival. The list goes on and on. But why are researchers interested in that question? There are at least three principal reasons:

1. Substitution. If there is a strong relationship between X and Y, X might be substituted for Y, particularly if X is less expensive in terms of money, time, etc. The first example in the preceding paragraph is a good illustration of this reason; X might be a measurement of height taken with a tape measure and Y might be a measurement of height taken with a stadiometer.

2. Prediction. If there is a strong relationship between X and Y, X might be used to predict Y. An equation for predicting income (Y) from age (X) might be helpful in understanding the trajectory in personal income across the age span.

3. Causality. If there is a strong relationship between X and Y, and other variables are directly or statistically controlled, there might be a solid basis for claiming, for example, that an increase in drug dosage causes an increase in life expectancy.

What does it mean?

In a recent internet posting, Donald Macnaughton (2002) summarized the discussion that he had with Jan deLeeuw, Herman Rubin, and Robert Frick regarding seven definitions of the term "relationship between variables". The seven definitions differed in various technical respects. My personal preference is for their #6:

There is a relationship between the variables X and Y if, for at least one pair of values X' and X" of X, E(Y|X') ~= E(Y|X"), where E is the expected-value operator, the vertical line means "given", and ~= means "is not equal to". (It indicates that X varies, Y varies, and all X's are not associated with the same Y.)

Research design

In order to address research questions of the "What is the relationship between X and Y?" type, a study must be designed in a way that will be appropriate for providing the desired information. For relationship questions of a causal nature a double-blind true experimental design, with simple random sampling of a population and simple random assignment to treatment conditions, might be optimal. For questions concerned solely with prediction, a study based upon a stratified random sampling design is often employed. And if the objective is to investigate the extent to which X might be substituted for Y, X must be "parallel" to Y (a priori comparably valid, with measurements on the same scale so that degree of agreement as well as degree of association can be determined).

Displaying the data

For small samples the raw data can be listed in their entirety in three columns: one for some sort of identifier; one for the obtained values for X; and one for the corresponding obtained values for Y. If X and Y are both continuous variables, a scatterplot of Y against X should be used in addition to or instead of that three-column list. [An interesting alternative to the scatterplot is the "pair-link" diagram used by Stanley (1964) and by Campbell and Kenny (1999) to connect corresponding X and Y scores.] If X is a categorical independent variable, e.g., type of treatment to which randomly assigned in a true experiment, and Y is a continuous dependent variable, a scatterplot is also appropriate, with values of X on the horizontal axis and with values of Y on the vertical axis.

For large samples a list of the raw data would usually be unmanageable, and the scatterplot might be difficult to display with even the most sophisticated statistical software because of coincident or approximately coincident data points. (See, for example, Cleveland, 1995; Wilkinson, 2001.) If X and Y are both naturally continuous and the sample is large, some precision might have to be sacrificed by displaying the data according to intervals of X and Y in a two-way frequency contingency table (cross-tabulation). Such tables are also the method of choice for categorical variables for large samples.

How small is small and how large is large? That decision must be made by each individual researcher. If a list of the raw data gets to be too cumbersome, if the scatterplot gets too cluttered, or if cost considerations such as the amount of space that can be devoted to displaying the data come into play, the sample can be considered large.

Summarizing the data

For two continuous variables it is conventional to compute the means and standard deviations of X and Y, the Pearson product-moment correlation coefficient between X and Y, and the corresponding regression equation, if the objective is to determine the direction and the magnitude of the degree of linear relationship between the two variables. Other statistics such as the medians and the ranges of X and Y, the residuals (the differences between the actual values of Y and the values of Y on the regression line for the various values of X), and the like, might also be of interest. If curvilinear relationship is of equal or greater concern, the fitting of a quadratic or exponential function might be considered.

[Note: There are several ways to calculate Pearson's r, all of which are mathematically equivalent. Rodgers & Nicewander (1988) provided thirteen of them. There are actually more than thirteen. I derived a rather strange-looking one several years prior to that (Knapp, 1979) in an article on estimating covariances using the incidence sampling technique developed by Sirotnik & Wellington (1974).]

For categorical variables there is a wide variety of choices. If X and Y are both ordinal variables with a small number of categories (e.g., for Likert-type scales), Goodman and Kruskal's (1979) gamma is an appropriate statistic. If the data are already in the form of ranks or easily convertible into ranks, one or more rank-correlation coefficients, e.g., Spearman's rho or Kendall's tau, might be preferable for summarizing the direction and the strength of the relationship between the two variables.

If X and Y are both nominal variables, indexes such as the phi coefficient (which is mathematically equivalent to Pearson's r for dichotomous variables) or Goodman and Kruskal's (1979) lambda might be equally defensible alternatives.

For more on displaying data in contingency tables and for the summarization of such data, see Simon (1978) and Knapp (1999; 2015).

Interpreting the data

Determining whether or not a relationship is strong or weak, statistically significant or not, etc. is part art and part science. If the data are for a full population or for a "convenience" sample, no matter what size it may be, the interpretation should be restricted to an "eyeballing" of the scatterplot or contingency table, and the descriptive (summary) statistics . For a probability sample, e.g., a simple random random or a stratified random sample, statistical significance tests and/or confidence intervals are usually required for proper interpretation of the findings, as far as any inference from sample to population is concerned. But sample size must be seriously taken into account for those procedures or anomalous results could arise, such as a statistically significant relationship that is substantively inconsequential. (Careful attention to choice of sample size in the design phase of the study should alleviate most problems.)

<u>An example</u>

The following example has been analyzed and scrutinized by many researchers. It is due to Efron and his colleagues (see, for example, Diaconis & Efron, 1983). [LSAT = Law School Aptitude Test; GPA = Undergraduate Grade Point Average]

Law School	mean LSAT	mean GPA
1	576	3.39
2	635	3.30
3	558	2.81
4	578	3.03
5	666	3.44
6	580	3.07
7	555	3.00
8	661	3.43
9	651	3.36
10	605	3.13
11	653	3.12
12	575	2.74
13	545	2.76
14	572	2.88
15	594	2.96

```
          680 -
 LSAT         -                                                 2
              -                            *
              -                                           *
          640 -                                      *
              -
              -
              -                            *
          600 -
              -                  *
              -                         *
              -           *      *      *                   *
              -
          560 -                      *
              -        *
              -      *
              -
                 +----------+---------+---------+---------+---------+----GPA
               2.70      2.85      3.00      3.15      3.30      3.45
```

[The 2 indicates there are two data points (for law schools #5 and #8) that are very close to one another in the (X,Y) space. It doesn't clutter up the scatterplot very much, however. Note: Efron and his colleagues always plotted GPA against LSAT. I have chosen to plot LSAT against GPA. Although they were interested only in correlation and not regression, if you cared about predicting LSAT from GPA it would make more sense to have X = GPA and Y = LSAT, wouldn't it?]

Summary statistics

```
             N     MEAN    STDEV
 lsat       15      600.3     41.8
 gpa        15      3.095     0.244

 Correlation between lsat and gpa = 0.776

 The regression equation is
 lsat = 188 + 133 gpa  (standard error of estimate = 27.34)

 Unusual Observations
 Obs.     gpa      lsat       Fit      Stdev.Fit   Residual   St.Resid
   1       3.39    576.00    639.62     11.33     -63.62     -2.56R

 R denotes an obs. with a large st. resid.
```

Interpretation

The scatterplot looks linear and the correlation is rather high (it would be even higher without the outlier). Prediction of GPA from LSAT should be generally good, but could be off by about 50 points or so (approximately two standard errors of estimate).

If this sample of 15 law schools were to be "regarded" as a simple random sample of all law schools, a statistical inference might be warranted. The correlation coefficient of .776 for n = 15 is statistically significant at the .05 level, using Fisher's r-to-z transformation; and the 95% confidence interval for the population correlation extends from .437 to .922 on the r scale (see Knapp, Noblitt, & Viragoontavan, 2000), so we can be reasonably assured that in the population of law schools there is a non-zero linear relationship between LSAT and GPA.

Complications

Although that example appears to be simple and straightforward, it is actually rather complicated, as are many other two-variable examples. Here are some of the complications and some of the ways to cope with them:

1. Scaling. It could be argued that neither LSAT nor GPA are continuous, interval-level variables. The LSAT score on the 200-800 scale is usually determined by means of a non-linear normalized transformation of a raw score that might have been corrected for guessing, using the formula number of right answers minus some fraction of the number of wrong answers. GPA is a weighted heterogeneous amalgam of course grades and credit hours where an A is arbitrarily given 4 points, a B is given 3 points, etc. It might be advisable, therefore, to rank-order both variables and determine the rank correlation between the corresponding rankings. Spearman's rho for the ranks is .796 (a bit higher than the Pearson correlation between the scores).

2. Weighting. Each of the 15 law schools is given a weight of 1 in the data and in the scatterplot. It might be preferable to assign weights to the schools in order to reflect the number of observations that contribute to its average, thus giving greater weight to the larger schools. Korn and Graubard (1998) discuss some creative ways to display weighted observations in a scatterplot.

3. Unit-of analysis. The sample is a sample of schools, not students. The relationship between two variables such as LSAT and GPA that is usually of principal interest is the relationship that would hold for individual persons, not aggregates of persons, and even there one might have to choose whether to investigate the relationship within school or across schools. The unit-of-analysis problem has been studied for many years (see, for example, Robinson, 1950 and Knapp, 1977), and has been the subject of several books and articles, more recently under the heading "hierarchical linear modeling" rather than "unit of analysis" (see, for example, Raudenbush & Bryk, 2002 and Osborne, 2000).

4. Statistical assumptions. There is no indication that those15 schools were drawn at random from the population of all law schools, and even if they were, a finite population correction should be applied to the formulas for the standard errors used in hypothesis testing or interval estimation, since the population at the time (the data were gathered in 1973) consisted of only 82 schools, and 15 schools takes too much of a "bite" out of the 82.

Fisher's r-to-z transformation only "works" for a bivariate normal population distribution. Although the scatterplot for the 15 sampled schools looks approximately bivariate normal, that may not be the case in the population, so a conservative approach to the inference problem would involve a choice of one or more of the following approaches:

a. A test of statistical significance and/or an interval estimate for the rank correlation. Like the correlation of .776 between the scores, the rank correlation of .796 is also statistically significant at the .05 level, but the confidence interval for the population rank correlation is shifted to the right and is slightly tighter.

b. Application of the jackknife to the 15 bivariate observations. Knapp, et al. (2000) did that for the "leave one out" jackknife and estimated the 95% confidence interval to be from approximately .50 to approximately .99.

c. Application of the bootstrap to those observations. Knapp, et al. (2000) did that also [as many other researchers, including Diaconis & Efron, 1983 had done], and they found that the middle 95% of the bootstrapped correlations ranged from approximately .25 to approximately .99.

d. A Monte Carlo simulation study. Various population distributions could be sampled, the resulting estimates of the sampling error for samples of size 15 from those populations could be determined, and the corresponding significance tests and/or confidence intervals carried out. One population distribution that might be considered is the bivariate exponential.

5. Attenuation. The correlation coefficient of .776 is the correlation between obtained average LSAT score and obtained average GPA at those 15 schools. Should the relationship of interest be an estimate of the correlation between the corresponding true scores rather than the correlation between the obtained scores? It follows from classical measurement theory that the mean true score is equal to the mean obtained score, so this should not be a problem with the given data, but if the data were disaggregated to the individual level a correction for attenuation (unreliability) may be called for. (See, for example, Muchinsky, 1996 and Raju & Brand, 2003; the latter article provides a significance test for attenuation-corrected correlations.) It would be relatively straightforward for LSAT scores, since the developers of that test must have some evidence regarding the reliability of the instrument. But GPA is a different story. Has anyone ever investigated the reliability of GPA? What kind of reliability coefficient would be appropriate? Wouldn't it be necessary to know something about the reliability of the tests and the grades that "fed into" the GPA?

6. Restriction of range. The mere fact that the data are average scores presents a restriction-of-range problem, since average scores vary less from one another than individual test scores do. There is also undoubtedly an additional restriction because students who apply to law schools and get admitted have (or should have) higher LSAT scores than students in general. A correction for restriction of range to the correlation of .776 might be warranted (the end result of which should be an even higher correlation), and a significance test is also available for range-corrected correlations (Raju & Brand, 2003).

7. Association vs. agreement. Reference was made above to the matter of association and agreement for parallel forms of measuring instruments. X and Y could be perfectly correlated (for example, X = 1,2,3,4,5, and Y = 10,20,30,40,50, respectively) but not agree very well in any absolute sense. That is irrelevant for the law school example, since LSAT and GPA are not on the same scale, but for

many variables it is the matter of agreement that is of principal concern (see, for example, Robinson, 1957 and Engstrom, 1988).

8. Interclass vs. intraclass. If X and Y are on the same scale, Fisher's (1958) intraclass correlation coefficient may be more appropriate than Pearson's product-moment correlation coefficient (which Fisher called an interclass correlation). Again this is not relevant for the law school example, but for some applications, e.g., an investigation of the relationship between the heights of twin-pairs, Pearson's r would actually be indeterminate because we wouldn't know which height to put in which column for a given twin-pair.

9. Precision. How many significant digits or decimal places are warranted when relationship statistics such as Pearson r's are reported? Likewise for the p-values or confidence coefficients that are associated with statistical inferences regarding the coresponding population parameters. For the law school example I reported an r of .776, a p of (less than) .05, and a 95% confidence interval. Should I have been more precise and said that r = .7764 or less precise and said that r = .78? The p that "goes with" an r of .776 is actually closer to .01 than to .05. And would anybody care about a confidence coefficient of, say, 91.3?

10. Covariance vs. correlation. Previous reference was made to the tradition of calculating Pearson's r for two continuous variables whose linear relationship is of concern. In certain situations it might be preferable to calculate the scale-bound covariance between X and Y rather than, or in addition to, the scale-free correlation. In structural equation modeling it is the covariances, not the correlations, that get analyzed. And in hierarchical linear modeling the between-aggregate and within-aggregate covariances sum to the total covariance, but the between-aggregate and within-aggregate correlations do not (see Knapp, 1977).

Another example

The following table (from Agresti, 1990) summarizes responses of 91 married couples to a questionnaire item. This example has also been analyzed and scrutinized by many people. The item: Sex is fun for me and my partner (a) never or occasionally, (b) fairly often, (c) very often, (d) almost always.

	Wife's Rating				
Husband's Rating	Never fun	Fairly often	Very often	Almost always	Total
Never fun	7	7	2	3	19
Fairly often	2	8	3	7	20
Very often	1	5	4	9	19
Almost always	2	8	9	14	33
Total	12	28	18	33	91

What is the relationship between Wife's Rating (X) and Husband's Rating (Y)? There are many ways of analyzing these data in order to provide an answer to that question. In decreasing order of my preference, they are:

1. Percent agreement (strict). The number of agreements is equal to 7+8+4+14 = 33 (the sum of the frequencies in the principal diagonal) which, when divided by 91 and multiplied by 100, gives a percent agreement of 36.3%. If this sample of 91 couples were a simple random sample from some large population of couples, a confidence interval for the corresponding population percentage could be constructed. (Using the normal approximation to the binomial, the 95% confidence interval for percent agreement in the population would extend from 26.4% to 46.2%.) In any event, the relationship does not appear to be very strong.

2. Percent partial agreement (lenient). If "agreement" were to be defined as "not off by more than one category", the percent agreement is those 33 + the sums of the frequencies in the adjacent parallel diagonals, i.e., 7+3+9 = 19 and 2+5+9 = 16, for a total of 68 "agreements" out of 91 possibilities, or 74.7%.

3. Goodman and Kruskal's (1979) gamma. The two variables are both ordinal (percent agreement does not take advantage of that ordinality, but it is otherwise very simple and very attractive) and the number of categories is small (4), so by applying any one of the mathematically-equivalent formulas for gamma, we have gamma = .047.

4. Goodman and Kruskal's (1979) lambda. Not as good a choice as Goodman's gamma, because it does not reflect the ordinality of the two variables. For these data lambda = .159.

5. Somers' (1962) D. Somers' D is to be preferred to gamma if the two variables take on independent and dependent roles (for example, if we would like to predict husband's rating from wife's rating, or wife's rating from husband's rating). That does not appear to be the case here, but Somers' D for these data is .005.

6. Cohen's (1960) kappa. This is one of my least favorite statistics, since it incorporates a "correction" to percent agreement for chance agreements and I don't believe that people ever make chance ratings. But it is extremely popular in certain disciplines (e.g., psychology) and some people would argue that it would be appropriate for the wife/husband data, for which it is .129 (according to the graphpad.com website calculator and Michael Friendly's website). ["Weighted kappa", a statistic that reflects partial agreement, also "corrected for chance", is .237.] The sampling distribution of Cohen's kappa is a mess (see, for example, Fleiss, Cohen, & Everitt, 1969), but the graphpad.com calculator yielded a 95% confidence interval of -.006 to .264 for the population unweighted kappa.

7. Krippendorff's (1980) alpha . This statistic is alleged to be an improvement over Cohen's kappa, since it also "corrects" for chance agreements and is a function of both agreements and disagreements. For these data it is .130. (See the webpage entitled "Computing Krippendorff's Alpha-Reliability".) "Alpha" is a bad choice for the name of this statistic, since it can be easily confused with Cronbach's (1951) alpha.

8. Contingency coefficient. Not recommended; it also does not reflect ordinality and its range is not from the usually-desirable -1 to +1 or 0 to +1.

9. Rank correlation. Not recommended; there are too many "ties".

10. Pearson's r. Also not recommended; it would treat the two variables as interval-level, which they definitely are not. [I would guess, however, that over half of you would have done just that!]

A third (and final) example

One of the most common research contexts is a true experiment in which each subject is randomly assigned to an experimental group or to a control group and the two groups are compared on some continuous variable in order to determine whether or not, or the extent to which, the treatment as operationalized in the experimental condition has had an effect. Here X is a dichotomous (1, 0) nominal independent variable and Y is a continuous dependent variable. An example of such a context was provided by Dretzke (2001) in her book on the use of Excel for statistical analyses:

"A researcher wanted to find out if dreaming increased as a result of taking three milligrams of Melatonin before going to sleep each night. Nineteen people were randomly assigned to one of two treatment conditions: Melatonin (n = 10) and Placebo (n = 9). Each morning the number of dreams recalled were reported and tallied over a one-week period. " (p. 152) Here are the data:

Melatonin (X = 1)	Placebo (X = 0)
21	12
18	14
14	10
20	8
11	16
19	5
8	3
12	9
13	11
15	

1. Displaying the data. The listing of the 19 "scores" on the dependent variable in two columns (without identifiers), with 10 of the scores under the "Melatonin" column (experimental group) and 9 of the scores under the "Placebo" column (control group) seems at least necessary if not sufficient. It might be advisable to re-order the scores in each column from high to low or from low to high, however, and/or display the data graphically, as follows:

```
       -                                                  *
    20.0+                                                 *
       -                                                  *
Y      -                                                  *
       -
       -  *
    15.0+                                                 *
       -  *                                               *
       -                                                  *
       -  *                                               *
       -  *                                               *
    10.0+ *
       -  *
       -  *                                               *
       -
       -
     5.0+ *
       -
       -  *
          +---------+---------+---------+---------+---------+------X
        0.00      0.20      0.40      0.60      0.80      1.00
```

2. Descriptive statistics. Most data analysts would be interested in the mean and standard deviation of the dependent variable for each group, and the difference between the two means. (If there were an outlier or two, the medians and the ranges might be preferred instead of, or in addition to, the means and standard deviations.) And since the relationship between the two variables (X = type of treatment and Y = number of dreams recalled) is of primary concern, the point-biserial correlation between X and Y (another special case of Pearson's r) should also be calculated. For the given data those summary statistics are:

Melatonin: mean = 15.1; standard deviation = 4.3 (median = 14.5; range = 13)

Placebo: mean = 9.8; standard deviation = 4.1 (median = 10.0; range = 13)

Difference between the means = 15.1 - 9.8 = 5.3

Correlation between X (type of treatment) and Y (number of dreams recalled) = .56 (with the Melatonin group coded 1 and the Placebo group coded 0)

3. Inferential statistics. Almost everyone would carry out a two independent samples one-tailed t test. That would be inadvisable, however, for a number of reasons. First of all, although the subjects were randomly assigned to treatments there is no indication that they were randomly sampled. [See the opposing views of Levin, 1993 and Shaver, 1993 regarding this distinction. Random sampling, not random assignment, is one of the assumptions underlying the t test.] Secondly, the t test assumes that in the populations from which the observations were drawn the distributions are normal and homoscedastic (equal spread). Since there is apparently only one population that has been sampled (and that not randomly sampled) and its distribution is of unknown shape, that is another strike against the t test. (The sample observations actually look like they've been sampled from rectangular, i.e., uniform, distributions and the two samples have very similar variability, but that doesn't really matter; it's what's going on in the population that counts.)

The appropriate inferential analysis is a randomization test (sometimes called a permutation test)--see, for example, Edgington (1995)--where the way the scores (number of dreams recalled) happened to fall into the two groups subsequent to the particular randomization employed is compared to all of the possible ways that they could have fallen, under the null hypothesis that the treatments are equally effective and the Melatonin group would always consist of 10 people and the Placebo group would always consist of 9 people. [If the null hypothesis were perfectly true, each person would recall the same number of dreams no matter which treatment he/she were assigned to.] The number of possible ways is equal to the number of combinations of 19 things taken 10 at a time (for the number of different allocations to the experimental group; the other 9 would automatically comprise the control group), which is equal to 92378, a very large number indeed. As an example, one of the possible ways would result in the same data as above but with the 21 and the 3 "switched". For that case the Melatonin mean would be 13.3 and the Placebo mean would be 11.8, with a corresponding point biserial correlation of .16 between type of treatment and number of dreams recalled.

I asked John Pezzullo [be sure to visit his website some time, particularly the Interactive Stats section] to run the one-tailed randomization test for me. He very graciously did so and obtained a p-value of .008 (the difference between the two means is statistically significant at the .01 level) and so it looks like the Melatonin was effective (if it's good to be able to recall more dreams than fewer!).

Difficult cases

The previous discussion makes no mention of situations where, say, X is a multi-categoried ordinal variable and Y is a ratio-level variable. My advice: Try if at all posssible to avoid such situations, but if you are unable to do so consult your favorite statistician.

The bottom line(s)

If you are seriously interested in investigating the relationship between two variables, you should attend to the following matters, in the order in which they are listed:

1. Phrase the research question in as clear and concise a manner as possible. Example: "What is the relationship between height and weight?" reads better than "What is the relationship between how tall you are and how much you weigh?"

2. Always start with design, then instrumentation, then analysis. For the height/weight research question, some sort of correlational design is called for, with valid and reliable measurement of both variables, and employing one or more of the statistical analyses discussed above. A stratified random sampling design (stratifying on sex, because sex is a moderator of the relationship between height and weight), using an electronic stadiometer to measure height and an electronic balance beam scale to measure weight, and carrying out conventional linear regression analyses within sex would appear to be optimal.

3. Interpret the results accordingly.

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CHAPTER 20: SPECIFY, HYPOTHESIZE, ASSUME, OBTAIN, TEST, OR PROVE?

I am constantly amazed that many researchers don't understand the differences among the six verbs "specify", "hypothesize", "assume", "obtain", "test", and "prove".

An example

Consider the following example: You're interested in the relationship between height and weight, and you would like to carry out a study of that relationship for a simple random sample of some large population.

What do you SPECIFY? If you plan to use traditional statistical inference (significance testing) you need to specify the magnitudes of tolerable probabilities of Type I (alpha) and Type II (beta) errors before you see the data. (For the latter you can specify the power you want rather than the tolerable probability of a Type II error, where power = 1 - beta.) If you plan to use interval estimation you need to specify how confident you want to be with the finding you'll get and the tolerable margin of error (width of the confidence interval), also before you see the data.

What do you HYPOTHESIZE? If you plan to use significance testing you need to hypothesize both a null value (or set of values) for a particular parameter and an alternative value (or set of values) for that parameter. If you plan to use interval estimation you need not, nay cannot, hypothesize any values beforehand.

What do you ASSUME? For significance testing you need to assume the independence of the observations and random sampling (which you have), and you might need to assume a normal distribution of the observations in the population from which the sample is to be drawn. You might also need to assume homogeneity of variance, homogeneity of regression, and/or other things. For interval estimation the assumptions are the same. For Bayesian inference you need to consult your local friendly statistician.

What do you OBTAIN? For both significance testing and interval estimation the first thing you obtain is the appropriate sample size necessary for your specifications, before you embark upon the study. Upon completion of the study you obtain the relevant descriptive statistics, p-values, actual confidence intervals, and the like.

What do you TEST? For significance testing you test the null hypothesis against the alternative hypothesis. For interval estimation there is nothing to test per se.

What do you PROVE? Nothing.

So what's the problem?

1. Some people say you calculate (obtain) power for a study. No, you specify the power you want (directly; or indirectly by specifying the tolerable probability of a Type II error, which is 1 minus power). There is such a thing as post hoc power in which power is calculated after the fact for the effect size actually obtained, but it is a worthless concept. See below for more about post hoc power.

2. Some people say you specify the sample size. No, unless you're stuck with a particular sample size. As indicated above, you determine (calculate, obtain) the appropriate sample size.

3. Some people say you assume the null hypothesis to be true until, or unless, rejected. No, you hypothesize it to be true (although you usually hope that it isn't!), along with an alternative hypothesis, which you usually hope to be true.

4. Some people say you hypothesize that the population distribution is normal. No, you assume that (sometimes).

5. Some people say you prove the null hypothesis to be true if you don't reject it. No, you calculate the probability of getting the statistic you got, or anything more discrepant from the null-hypothesized parameter, if the null hypothesis is true. If that conditional probability is greater than your pre-specified alpha level, you cannot reject the null-hypothesized parameter. But that doesn't mean you've proven it to be true.

Some of those same people say you prove the null hypothesis to be false if you reject it. No; if the conditional probability is less than your pre-specified alpha you reject the null-hypothesized parameter. But that doesn't mean you've proven it to be false

Back to the example

What should you do?

a. If you're going to use significance testing, you should first SPECIFY alpha and beta. The conventional specifications are .05 for alpha and .20 for beta (power of .80), but you should preferably base your choices on the consequences of making Type I and Type II errors. For example, suppose your null hypothesis will be that the population correlation is equal to zero and you subsequently reject that hypothesis but it's true. There should be no serious consequence of being wrong, other than your running around thinking that there is a non-zero relationship between height and weight when there isn't. In that case you should feel free to specify a value for alpha that is more liberal than the traditional .05 (perhaps .10, double that probability?). If the null hypothesis of zero is pitted against an alternative hypothesis of, say, .90 (a strong relationship) and you subsequently do not reject the null but it's false, you will have missed a golden opportunity to be able to accurately predict weight from height. Therefore, you should feel free to decrease beta to .05 (increase power to.95) or even less.

b. If you're going to use interval estimation, you should first SPECIFY the maximum margin of error you will be able to tolerate when you make your inference from sample to population, along with the associated specification of how confident you want to be in making that inference. The former might be something like .10 (you'd like to come that close to the population correlation). The latter is conventionally taken to be 95% but, like alpha and beta in significance testing, is always "researcher's choice".

c. Once those specifications have been made, the next step is to use one of the various formulas and tables that are available for determining (OBTAINING) the sample size that will satisfy the specifications. If you've intellectualized things properly, it will be a "Goldilocks sample" (not too large, not too small, but just right).

d. For significance testing you are now ready to HYPOTHESIZE: one value (or set of values) for a parameter for the null hypothesis, and a competing value (or set of values) for the alternative hypothesis. For a study of the relationship between two variables (in your case, height and weight), the null-hypothesized parameter is almost always zero, i.e., the conservative claim that there is no relationship. Things are much trickier for the alternative hypothesis. You might want to hypothesize a particular value other than zero, e.g., .60, if you believe that the relationship is positive and reasonably large. (You probably would not want to hypothesize something like .98 because you can't imagine the relationship to be that strong.) Or you might not want to stick your neck out that far, so you might merely hypothesize that the correlation in the population is positive. (That is the conventional alternative hypothesis for a relationship study, whether or not it is actually stated.) There are other possibilities for the alternative hypothesis, but those should do for the present.

e. For interval estimation you get off easy, because there are no values to hypothesize. You have made your specifications regarding tolerable margin of error and degree of confidence, but you are uninterested, unwilling, or unable to speculate what the direction or the magnitude of the relationship might be.

No matter whether you choose to use significance testing or interval estimation, if the Pearson product-moment correlation coefficient is to be the statistic of principal interest you will need to ASSUME that in the population there is a bivariate normal distribution. If you prefer to rank-order the heights and weights (and lose some information) and use Spearman's rank correlation, that assumption is not necessary.

f. You're now ready to draw (OBTAIN) your sample, collect (OBTAIN) the actual heights and weights, and calculate (OBTAIN) the sample correlation. If you've chosen the significance testing approach you can TEST the null hypothesis of no relationship against whatever alternative hypothesis you thought to be relevant, and see whether the p-value corresponding to the sample correlation is less than or greater than your pre-specified alpha. If it is less, the sample correlation is statistically significant; if it is greater, the sample correlation is not. If you've chosen the interval estimation approach, you can construct (OBTAIN) the confidence interval around the sample correlation and make the inference that you are X% confident that the interval "captures" the unknown population correlation.

g. You will not have PROVEN anything, but if you've chosen the significance testing route you will have made the correct inference or you will have made either a Type I error (by rejecting a true null) or a Type II error (by not rejecting a false null) but there's no way you would be subject to a Type I error and a Type II error (you can't both reject and not reject the null). Unfortunately, alas, you will never know for sure whether you're right or not, but "the odds" will usually be in your favor. Similarly, if you've chosen interval estimation your inference that the parameter has been captured or has not been captured can be either right or wrong and you won't know which. But once again "the odds" will be in your favor. That should be comforting.

What you should not do

The first thing you should not do is use both significance testing AND interval estimation. As you might already know, a confidence interval consists of all of the values of a parameter that are "unrejectable" with a significance test. There is an unfortunate tendency these days to report the actual p-value, e.g., .003, from a significance test ALONG WITH a confidence interval (usually 95%) around the obtained statistic.

The second thing you should not do is report the so-called post hoc (or retrospective or observed) power, along with or (worse yet) instead of the a priori (ordinary) power. Post hoc power adds no important information, but has unfortunately been incorporated into some computer packages, e.g., SPSS's Analysis of Variance routines. It is perfectly inversely related to p-value.

Both things drive me up a wall. Please don't do either of them. Thank you.

CHAPTER 21: THE INDEPENDENCE OF OBSERVATIONS

What this chapter is NOT about

It is not about observation as the term is used in psychology, e.g., when the behavior of children at play is observed through one-way windows in a laboratory setting. It is also not about observational research as that term is used in epidemiology, i.e., as a type of research different from true experimental research, in that no variables are manipulated by the researchers themselves. And it is not about independent variables or independent samples, except tangentially.

What this paper IS about

It is concerned with the term "observation" defined as a measurement taken on an entity (usually a person). It might be a univariate observation, e.g., my height (71); a bivariate observation, e.g., my height and my weight (71, 145); or a multivariate observation, e.g., my sex, age, height, and weight (M, 85, 71, 145). If I were a twin (I'm not, but the name Thomas does mean "twin" in Aramaic), I could be interested in analyzing a data set that includes the bivariate observation for me and the bivariate observation for my twin, which might be something like (70, 150).

What is meant by the term "independence of observations"

Two or more observations are said to be independent if knowing one of them provides no information regarding the others. Using the same example of my height and weight, and my twin's height and weight, if you knew mine, you knew I had a twin, but you didn't know his/her (we could be "identical" or "fraternal") height and weight, you would suspect (and rightly so) that those two observations would not be independent.

Why it is important

For virtually every statistical analysis, whether it be for an experiment or a non-experiment, for an entire population or for a sample drawn from a population, the observations must be independent in order for the analysis to be defensible. "Independence of observations" is an assumption that must be satisfied, even in situations where the usual parametric assumptions of normality, homogeneity of variance, homogeneity of regression, and the like might be relaxed.

So what is the problem?

The problem is that it is often difficult to determine whether the observations obtained in a particular study are or are not independent. In what follows I will try to explain the extent of the problem, with examples; provide at least one way to actually measure the degree of independence of a set of observations; and mention some ways of coping with non-independence.

Some examples

1. In an article I wrote over thirty years ago (Knapp, 1984), I gave the following example of a small hypothetical data set:

Name	Height	Weight
Sue	5'6"	125#
Ginny	5'3"	135#
Ginny	5'3"	135#
Sally	5'8"	150#

Those four observations are not independent, because Ginny is in the data twice. (That might have happened because of a clerical error; but you'd be surprised how often people are counted in data more than once. See below.) To calculate their mean height or their mean weight, or the correlation between their heights and their weights, with n = 4, would be inappropriate. The obvious solution would be to discard the duplicate observation for Ginny and use n = 3. All three of those observations would then be independent.

2. Later in that same article I provided some real data for seven pairs of 16-year-old, Black, female identical twins (you can tell I like heights, weights, and twins):

Pair	Heights (in inches)		Weights (in pounds)	
1 (Aa)	A:68	a:67	A:148	a:137
2 (Bb)	B:65	b:67	B:124	b:126
3 (Cc)	C:63	c:63	C:118	c:126
4 (Dd)	D:66	d:64	D:131	d:120
5 (Ee)	E:66	e:65	E:119	e:124
6 (Ff)	F:62	f:63	F:119	f:130
7(Gg)	G:66	g:66	G:114	g:104

Are those observations independent? Hmmm. Nobody is in the data more than once, but as forewarned above there is something bothersome here. You might want to calculate the mean height of these women, for example, but how would you do it? Add up all of the heights and divide by 14? No; that would ignore the fact that a is a twin of A, b is a twin of B, etc. How about averaging the heights within each pair and finding the average of those seven averages? No; that would throw away some interesting within-pair data. How about just finding the average height for the capital letter twins? No; that would REALLY be wasteful of data. And things are just as bad for the weights.

The plot thickens if you were to be interested in the relationship between height and weight for the seven twin-pairs. You could start out all right by plotting Twin A's weight against Twin A's height, i.e., Y=148, X=68. When it comes to Twin a you could plot 137 against 67, but how would you indicate the twinship? (In that same article I suggested using colored data points, a different color for each twin-pair.) Likewise for B and b, C and c, etc. That plot would soon get out of hand, however, even before any correlation between height and weight were to be calculated.

Bottom line: These fourteen observations are not independent of one another.

3. In a recent study, Russak, et al. (2010) compared the relative effectiveness of two different sunscreens (SPF 85 and SPF 50) for preventing sunburn. Each of the 56 participants in the study applied SPF85 to one side of the face and SPF50 to the other side (which side got which sunscreen was randomly determined). They presented the results in the following table:

Sun Protection Factor	Sunburned	Not Sunburned
85	1	55
50	8	48

Sainani (2010) included that table in her article as an example of non-independent observations, because it implied there were 56 participants who used SPF 85 and 56 other participants who used SPF50, whereas in reality 56 participants used both. She (Sainani) claimed that the following table displayed the data correctly:

	SPF-50 Side	
	Sunburned	Not Sunburned
SPF-85 Side		
Sunburned	1	0
Not sunburned	7	48

The observations in this second table are independent. The best way to spot non-independent observations in such tables is to calculate row totals, column totals, and the grand total. If the grand total is greater than the number of participants there is a non-independence problem.

What to do about possibly non-independent observations

The best thing to do is to try to avoid the problem entirely, e.g., by not doing twin research and by not having people serve as their own controls. But that might be too much of a cop-out. Twin research is important; and the advantages of having people serve as their own controls could outweigh the disadvantages (see Knapp, 1982 regarding the latter matter, where I actually come down on the side of not doing so).

One thing that should always be tried is to get a measure of the degree of independence. In a conference presentation many years ago, Glendening (1976) suggested a very creative approach, and I summarized it in Knapp (1984). For the case of k aggregates, with n observations per aggregate, a measure of independence, I, is found by taking an F-type ratio of the variance of the aggregate means to one-nth of the variance of the within-aggregate observations. If that ratio is equal to 1, the observations are perfectly independent. If that ratio is greater than 1, the observations are not independent. For a simple hypothetical example, consider a case of k = 2 and n = 3 where the observations for the two aggregates are (1,7,13) and (5,11, 17), respectively. The variance of the aggregate means is equal to 4; the within-aggregate variance is equal to 12 (all variances are calculated by dividing by the number of things, not one less than the number of things); one-third of the within-aggregate variance is also equal to 4; ergo I = 1 and the observations are independent. (They even look independent, don't they?) For another hypothetical example with the same dimensions, consider (1,2,3) and (4,5,6). For those observations I is equal to 81/8 or 10.125, indicating very non-independent obervations. (They look independent, but they're not. It all has to do with the similarity between the aggregate observations and observations you might expect to get when you draw random samples from the same population. See Walker, 1928, regarding this matter for correlations between averages vs. correlations between individual measurements.)

For the heights of the seven pairs of twins (each pair is an aggregate), with k = 7 and n = 2, I is equal to 13.60. For the weights, I is equal to 8.61. The height observations and the weight observations are therefore both non-independent, with the former "more non-independent" than the latter. (Some prior averaging is necessary, since the within-aggregate variances aren't exactly the same.) That is intuitively satisfying, since height is more hereditary than weight in general, and for twins in particular.

If a more sophisticated approach is desired, non-independence of observations can be handled by the use of intraclass correlations, hierarchical linear analysis, generalized estimating equations, or analysis of mixed effects (fixed and random). Those approaches go beyond the scope of the present chapter, but the interested reader is referred to the articles by Calhoun, et al. (2008) and by McCoach and Edelson (2010).

What the consequences are of treating dependent observations as independent

We've already seen for the sunscreen example that one of the consequences is an artificial inflation of the sample size (112 rather than 56). An associated consequence is an artificial increase in the degree of statistical significance and an artificial decrease in the width of a confidence interval (again see Sainani, 2010 re Russak, et al., 2010). A third consequence, for the correlation between two variables, is that the "total" correlation for which the data for two or more aggregates are "pooled" together is usually larger than the within-aggregate correlations. Consider, for instance, the correlation between height and weight for males and females combined into one aggregate. Since males are generally both taller than and heavier than females, the scatter plot for the pooled data is longer and tighter than the scatter plots for males and for females taken separately.

What some other methodological critics say about independence of observations

One thing that bothers me is that most authors of statistics textbooks have so very little to say about the independence of observations, other than listing it as one of the assumptions that must be satisfied in a statistical analysis. Bland and Altman (1994) are particularly hard on textbook authors regarding this. (In my opinion, Walker & Lev, 1953 is the only textbook with which I am familiar that says everything right, but even they devote only a couple of pages to the topic.)

Some critics have carried out extensive reviews of the research literature in their various fields and found that treating non-independent observations as though they were independent is very common. My favorite articles are by Sauerland, et al. (2003), by Bryant, et al. (2006), and by Calhoun, et al. (2008). Sauerland, et al. chastise some researchers for the way they handle (or fail to handle) fingers nested within hands nested in turn within patients who are undergoing hand surgery. Bryant, et al. are concerned about limbs nested within patients in orthopedic research. Calhoun, et al. discuss the problem of patient nested within practice in medical research in general.

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CHAPTER 22: SIGNIFICANCE TEST, CONFIDENCE INTERVAL, BOTH, OR NEITHER?

Introduction

It is reasonably well-known that you can usually get a significance test "for free" by constructing a confidence interval around an obtained statistic and seeing whether or not the corresponding hypothesized parameter is "captured" by the interval. If it isn't inside the 95% confidence interval, for example, reject it at the .05 significance level and conclude that the sample finding is statistically significant. If it is, don't reject it; the sample finding is not statistically significant at that level. So if you want a significance test you can either carry it out directly or get it indirectly via the corresponding confidence interval.

If you want a confidence interval you can carry it out directly (the usual way) or you can get it indirectly by carrying out significance tests for all of the possible "candidates" for the hypothesized parameter (not very practicable, since there is an infinite number of them!).

But should you ever carry out a hybrid combination of hypothesis testing and interval estimation, e.g., by reporting the 95% confidence interval and also reporting the actual p-value that "goes with" the obtained statistic, even if it is greater than or less than .05? Some people do that. Some journals (e.g., The Journal of Managed Care and Specialty Pharmacy) require it. But at least one journal (Basic and Applied Social Psychology) has banned both.

It is also reasonably well-known that if you don't have a random sample you really shouldn't make any statistical inferences. (Just get the descriptive statistic(s) and make any non-statistical inferences that may be warranted.) Exception: If you have random assignment but not random sampling for an experiment, randomization tests (permutation tests) are fine, but the inference is to all possible randomizations for the given sample, not to the population from which the sample was [non-randomly] drawn.

In what follows I will try to convey to you what some of the practices are in various disciplines, e.g., education, nursing, psychology, medicine, and epidemiology (the disciplines that I know best). I will also give you my personal opinion of such practices and in an appendix to this paper I will provide a brief test of the correctness of various wordings of statistical inferences.

The significance test controversy

Up until about 50 years ago or thereabouts, traditional significance tests were about the only statistical inferential methods that were used. There were occasional arguments among research methodologists concerning the approach of R.A. Fisher vs. that of Jerzy Neyman and Egon Pearson; see, for example, the interesting discussion between Berkson (1942, 1943) and Fisher (1943) concerning the linearity of a set of data, and Salzburg's (2001) fascinating account of Fisher's conflicts with Karl Pearson (Egon's better-known father) and with Neyman. [Huberty (1987) has referred to Fisher's approach as significance testing and Neyman & Pearson's approach as hypothesis testing.] There were also a few researchers (e.g., Meyer, 1964), who argued in favor of the use of Bayesian inference, but most articles published in the professional journals continued to emphasize traditional significance testing.

That all started to change when Morrison and Henkel (1970) compiled a book with the same title as that of this section. The individual chapters were written by various people who were concerned about the overuse and/or misuse of significance tests, especially in sociology, along with a few defenders of the status quo. Things really came to a head in the 80s with the publication of a set of articles in the journal Social Service Review (Cowger, 1984; Glisson, 1985; Rubin, 1985; Cowger, 1985); the often-cited article by Gardner and Altman (1986),and the chapter by Woolson and Kleinman (1989) regarding practices in medicine and epidemiology. Then in the late 90s there appeared the book, What if there were no significance tests?, edited by Harlow, Mulaik, and Steiger (1997), with an emphasis on psychology and education. The latter work, like the Morrison and Henkel book, consisted of chapters written by people with different points of view, most of whom argued that significance tests should be replaced by confidence intervals around the corresponding "effect sizes".

[Interesting aside: Berkson's 1942 article (but not Fisher's response or Berkson's rejoinder) was included in Morrison and Henkel's 1970 book and was also cited in Cohen's 1994 article--see below--that was reprinted in the Harlow, et al. 1997 book.]

In the last ten years or so, confidence intervals have begun to replace significance tests, but significance tests still have their defenders. In epidemiology and medicine, and to a lesser extent in nursing, there has been a recent tendency to emphasize interval estimation (usually 95% confidence intervals) while at the same time reporting a variety of p-values that correspond to the area(s) in the tail(s) of the relevant sampling distribution(s).

Confidence intervals: The alleged panacea

One of the arguments against significance tests has been that many users of them botch the wording when they report the results of their studies. For example, many methodologists have rightly objected to statements such as "the probability is less than .05 that the null hypothesis is true". [Cohen (1994) made the unfortunate mistake of claiming that some people say "the probability is less than .05 that the null hypothesis is false". I've never heard anyone say that.] The null hypothesis is either true or false. There is no probability associated with it, at least in the classical, non-Bayesian context. The probability applies to the likelihood of the sample finding, given that the null hypothesis is true; i.e., it is a conditional probability.

The claim is often made that the wording of confidence intervals is much more straightforward, and researchers are less likely to say the wrong things. Not so, say Cumming (2007), Cumming and Finch (2005), Moye (2006), Sober (n.d.), and others. For every user of significance tests who says "the probability is less than .05 that the null hypothesis is true" you can find some user of confidence intervals who says "the probability is .95 that my interval includes the parameter". Your particular interval doesn't have that .95 probability; the probability, if that word is even relevant for confidence intervals, applies to all such intervals created in the same way.

The one sense in which confidence intervals constitute a panacea is that you don't have to do any hypothesizing beforehand! Researchers often find it difficult to specify the magnitude of a parameter in which they are interested, whether the basis for that specification be theory, previous research, or whatever. With interval estimation all you need to do is specify the confidence you want to have and the margin of error that is tolerable (usually the width or half-width of the confidence interval), and the requisite sample size for "capturing" the parameter can be determined.

One size confidence interval, different p-values

There recently appeared two articles concerned with smoking cessation efforts, one in the medical literature (Peterson, et al., 2009) regarding teenagers who smoke, and one in the nursing literature (Sarna, et al., 2009) regarding nurses who smoke. Although the former was a randomized clinical trial and the latter was an observational study, both used the same statistical inferential approach of constructing 95% confidential intervals throughout, accompanied by actual p-values.

The principal finding of the Peterson study was "an intervention effect on 6-month prolonged smoking abstinence at 12 months after becoming intervention eligible (21.8% vs 17.7%, difference = 4.0%, 95% CI = – 0.2 to 8.1%, P =.06" (page 1383). [They called that "almost conclusive evidence" (same paragraph, same page).] Two supplementary findings were: "Among female and male smokers, respectively, the corresponding intervention effects were 5% (95% CI = 0.5 to 10%, P = .03) and 2.9% (95% CI = – 4.3 to 9.7%, P = .41)" (also same paragraph, same page).

One of the principal findings of the mutltiple logistic regression analysis reported in the Sarna study, comparing "any quit attempt" with "no quit attempt" (the dichotomous dependent variable) for smokers of 10-19 cigarettes per day vs. smokers of 20+ cigarettes per day (one of the independent variables) was an odds ratio of 2.43, 95% confidence interval 1.07 to 5.52, P = .03 (Table 4, page

253). Another finding from that analysis for another independent variable, baccalaureate vs. graduate degree, was an odds ratio of 1.54, 95% confidence interval 0.65 to 3.66, P = .33 (same table, same page).

What we have here in both articles is what I referred to earlier in this paper as a hybrid combination of constant confidence interval percentage and varying p-values. I personally don't like it. If authors are concerned solely with 95% confidence intervals I think they should be concerned solely with .05 p-values. In the Peterson study, for example, the 95% confidence interval for that difference of 4.0% in prolonged smoking abstinence [it should be 4.1%] didn't include an odds ratio of 1.00, so of course p is less than .05. Should the reader of the article care that p is actually .03? I don't think so, [And I don't think they should have used the phrase "almost conclusive evidence"!] The only justification I can see for reporting actual p-values in conjunction with 95% confidence intervals is the incorporation in a meta-analysis with p-values from other studies carried out on the same topic.

No significance tests or confidence intervals

Whether to use significance tests, confidence intervals, or both, pales in comparison to the more serious matter of the appropriateness of any inferential statistics. The standard gospel is easy to espouse: Use traditional inferential statistics if and only if you have a random sample from a well-defined population. So what's the problem?

First of all, there are researchers whom I call the "regarders", who don't have a random sample but who like to think of it as a sample from which a statistical inference can be made to a population of entities "like these". They refuse to quit after reporting the descriptive statistics, apparently because they find it difficult and/or unsatisfying to interpret the data without the benefits of inferential statistics.

Secondly, there are the "populations are samples, too" advocates, who insist on carrying out some sort of statistical inference when they actually have data for an entire population. The inference is allegedly from a population at one point in time to that same population at other points in time, even though the time point has not been selected at random. (See the article by Berk, Western, & Weiss, 1995 about this, along with the various reactions to that article in the same journal.)

A third “camp” uses significance tests to tell them, or help to tell them, how excited to get about a particular finding. It should be theoretical or clinical importance that should provide such excitement, not inferential statistics.

Then there are the "random is random" folks who use traditional t-tests or other general linear model techniques to carry out significance tests, rather than randomization (permutation) tests, when they have random assignment but do not have random sampling. Edgington and Onghena (2007) and others (e.g., Ludbrook & Dudley, 2000) have tried to get people to stop doing that, but to little avail. [See also the articles by Levin (1993) and by Shaver (1993), who come down on opposite sides of the matter.] A traditional t-test can occasionally be used as an approximation to a randomization test, if the researcher does not have easy access to the computer software that is necessary for carrying out a randomization test.

Shortly after Morrison and Henkel (1970) compiled their book, the famous statistician John W. Tukey (1977) wrote his treatise on Exploratory data analysis. In that book he claimed that descriptive statistics had been given short shrift and researchers should "massage" their data more carefully before, or instead of, carrying out statistical inferences. He provided several techniques for summarizing sample data, e.g., stem-and-leaf diagrams and q-q plots, that help to bring out certain features in the data that other descriptive statistics do not, and inferential procedures can not. I agree with Tukey's claim about descriptive statistics getting short shrift [but I'm not attracted to some of his statistical graphics]. I have even seen articles that provide an analysis of variance (ANOVA) summary table but not the sample means that produced it!

A final note

In this chapter I have alluded to some criticisms that I have made in previous sources (Knapp, 1970; 1998; 1999; 2002). I could go on and on regarding some controversies regarding other practices. For example, should we limit a study to "at most one" statistical inference? (I say "at most" because some studies don't even warrant one.) And why do some people test the statistical significance of baseline differences between experimental and control groups in a randomized experiment? Don't they know that all such differences are due to chance, by definition? Don't they trust probability to balance the groups? And don't they understand that the significance test takes care of chance differences? Or how about one-sided vs. two-sided significance tests and confidence intervals? (See Cohen's delightful 1965 piece about an argument between Doctor One and Doctor Two). But I wanted to keep this short and sweet. I know it's [reasonably] short. I hope you've found it to be sweet.

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Appendix: The wording of significance tests and confidence intervals

The situation (adapted from Cumming & Finch, 2005): You are interested in the verbal ability of a large population of school children, and you have administered a test of verbal ability to a sample of 36 children drawn randomly from the population. You are willing to assume that the test scores are normally distributed in that population. The sample mean is 62 and the sample standard deviation (with division by n-1) is 30. For those data the estimated standard error of the mean is 5. A two-sided t-test of the hypothesis that the population mean is 70 (the test of verbal ability has been normed on a different population having that mean) produces a two-tailed p-value of .12. Using a critical value of t = 2.03, the two-sided 95% confidence interval extends from 51.85 to 72.15.

On a scale from 1 to 3 (where 1= just plain wrong, 2= wrong but generally conveys the right idea, and 3 = correct), rate each of the following wordings for the significance test and for the confidence interval:

1. The probability is .12 that the sample mean is 62.

2. The probability is less than .12 that the sample mean is 62.

3. The population mean is 70.

4. The probability is .12 that the population mean is 70.

5. The probability is less than .12 that the population mean is 70.

6. The population mean is not 70.

7. The probability is less than .12 that the population mean is not 70.

8. If you were to test another random sample of 36 schoolchildren from that same population, their sample mean would be 62.

9. If you were to test another random sample of 36 schoolchildren from that same population, the probability is .12 that their sample mean would be less than 70.

10. If the population mean is 70, the probability is less than .12 that you would get a sample mean that differs from the population mean by 8 points or more.

11. You are 95% confident that the sample mean is between 51.85 and 72.15.

12. The population mean is greater than or equal to 51.85 and less than or equal to 72.15.

13. The probability is .95 that the population mean is between 51.85 and 72.15.

14. You are 95% confident that the population mean is between 51.85 and 72.15.

15. The probability is .95 that the interval from 51.85 to 72.15 includes the population mean.

16. You are 95% confident that the interval from 51.85 to 72.15 includes the population mean.

17. If you were to test another random sample of 36 schoolchildren from that same population, their sample mean would be between 51.85 and 72.15.

18. If you were to test another random sample of 36 schoolchildren from that same population, the probability is .95 that their sample mean would be between 51.85 and 72.15.

19. The 95% confidence interval includes all of those values of the population mean that would be rejected at the .05 level of significance.

20. 95% of intervals constructed in this same manner would include the population mean.

I won't give you the right answers (partly because there is room for some disagreement), but I'll give you the hint that these items would come fairly close to constituting a perfect Guttman Scale. If you don't know what that is, you can look it up!

CHAPTER 23: N (or n) vs. N - 1 (or n - 1) REVISITED

Prologue

Over 45 years ago I (Knapp,1970) wrote an article regarding when you should use N and when you should use N - 1 in the denominators of various formulas for the variance, the standard deviation, and the Pearson product-moment correlation coefficient. I ended my "pro N" article with this sentence: "Nobody ever gets an average by dividing by one less than the number of observations." (page 626).

There immediately followed three comments (Landrum, 1971; Games, 1971; Hubert, 1972) concerning the matter of N vs. N - 1. Things were relatively quiet for the next few years, but the controversy has erupted several times since, culminating in a recent clever piece by Speed (2012) who offered a cash prize [not yet awarded] to the person who could determine the very first time that a discussion was held on the topic.

The problem

Imagine that you are teaching an introductory ("non-calculus") course in statistics. [That shouldn't be too hard. Some of you who are reading this might be doing that or have done that.] You would like to provide your students with their first formulas for the variance and for the standard deviation. Do you put N, N -1, n, or n - 1 in the denominators? Why?

Some considerations

1. Will your first example (I hope you'll give them an example!) be a set of data (real or artificial) for a population (no matter what its size)? I hope so.
N is fine, and is really the only defensible choice of the four possibilities. You never subtract 1 from the number of observations in a population unless you want to calculate the standard error of some statistic using the finite population correction (fpc). And almost nobody uses n to denote the population size.

2. Will that first example be for a sample?

N would be OK, if you use N for sample size and use something like N_{pop} for population size. [Yes, I have seen N_{pop}.]

N -1 would be OK for the sample variance, if you always use N for sample size, you have a random sample, and you would like to get an unbiased estimate of the population variance; but it's not OK for the sample standard deviation. (The square root of an unbiased estimate of a parameter is not an unbiased estimate of the square root of the parameter. Do you follow that?)

n would be OK for both the sample variance and the sample standard deviation, and is my own personal preference.

n - 1 would be OK for the sample variance, if you always use n for sample size, you have a random sample, and you would like to get an unbiased estimate of the population variance; but it's not OK for the sample standard deviation (for the same reason indicated for N - 1).

3. What do most people do?

I haven't carried out an extensive survey, but my impression is that many authors of statistics textbooks and many people who have websites for the teaching of statistics use a sample for a first example, don't say whether or not the sample is a random sample, and use n - 1 in the denominator of the formula for the variance and in the denominator of the formula for the standard deviation.

The massive compendium (1886 pages) on statistical inference by Sheskin (2011) is an interesting exception. On pages 12-13 of his book he provides all of the possible definitional formulas for standard deviation and variance (for population or sample, N or n, N-1 or n-1, biased or unbiased estimator, σ or s). He makes one mistake, however. On page 12 he claims that the formula for the sample standard deviation with n-1 in the denominator yields an unbiased estimator of the population standard deviation. As indicated above, it does not. (He later corrects the error in a footnote on page 119 with the comment: “Strictly speaking, s~ [his notation] is not an unbiased estimate of σ, although it is usually employed as such.” That’s a bit tortured [how does he know that?], but I think you get the idea.)

Another commendable exception is Richard Lowry's VassarStats website. For his "Basic Sample Stats" routine he gives the user the choice of n or n-1. Nice.

4. Does it really matter?

From a practical standpoint, if the number of observations is very large, no. But from a conceptual standpoint, you bet it does, no matter what the size of N or n. In the remainder of this chapter I will try to explain why; identify the principal culprits; and recommend what we should all do about it.

Why it matters conceptually

A variance is a measure of the amount of spread around the arithmetic mean of a frequency distribution, albeit in the wrong units. My favorite example is a distribution of the number of eggs sold by a super market in a given month. No matter whether you have a population or a sample, or whether you use in the denominator the number of observations or one less than the number of observations, the answer comes out in "squared eggs". In order to get back to the original units (eggs) you must "unsquare" by taking the square root of the variance, which is equal to the standard deviation.

A variance is a special kind of mean. It is the mean of the squared differences (deviations) from the mean. A standard deviation is the square root of the mean of the squared differences from the mean, and is sometimes called "the root mean square".

You want to get an "average" measure of differences from the mean, so you want to choose something that is in fact an "average". You might even prefer finding the mean of the absolute values of the differences from the mean to finding the standard deviation. It's a much more intuitive approach than squaring the differences, finding their average, and then unsquaring at the end.

The culprits

In my opinion, there are two sets of culprits. The first set consists of some textbook authors and some people who have websites for the teaching of statistics who favor N - 1 (or n - 1) for various reasons (perhaps they want their students to get accustomed to n - 1 right away because they'll be using that in their calculations to get unbiased estimates of the population variance, e.g., in ANOVA) or they just don't think things through.

The second set consists of two subsets. Subset A comprises the people who write the software and the manuals for handheld calculators. I have an old TI-60 calculator that has two keys for calculating a standard deviation. One of the keys is labelled σ_n and the other is labelled σ_{n-1}. The guidebook calls the first "the population deviation"; it calls the second "the sample deviation" (page 5-6). It's nice that the user has the choice, but the notation is not appropriate [and the word “standard” before “deviation” should not be omitted]. Greek letters are almost always reserved for population parameters, and as indicated above you don't calculate a population standard deviation by having in the denominator one less than the number of observations.

Subset B comprises the people who write the software and the manuals for computer packages such as Excel, Minitab, SPSS, and SAS. All four of those use n - 1 as the default. [Good luck in trying to get the calculation using n.]

n + 1 [not the magazine]

Believe it or not, there are a few people who recommend using n + 1 in the denominator, because that produces the minimum mean squared error in estimating a population variance. See, for example, Hubert (1972), Yatracos (2005), and Biau and Yatracos (2012). It all depends upon what you want to maximize or minimize.

Degrees of freedom

Is it really necessary to get into degrees of freedom when first introducing the variance and the standard deviation? I don't think so. It's a strange concept (as Walker, 1940, pointed out many years ago) that students always have trouble with, no matter how you explain it. The number of unconstrained pieces of data? Something you need to know in order to use certain tables in the backs of statistics textbooks? Whatever.

Pearson r

For people who use n in the denominator for the sample variance and sample standard deviation, the transition to the Pearson product-moment correlation coefficient is easy. Although there are at least 13 different formulas for the Pearson r (Rodgers & Nicewander, 1988; I've added another one), the simplest to understand is $\sum z_x z_y /n$, where the z's are the standard scores for the two variables X and Y that are to be correlated. The people who favor n - 1 for the standard deviation, and use that standard deviation for the calculation of the z scores, need to follow through with n - 1 in the denominator of the formula for Pearson r. But that ruins "the average cross-product of standard scores" interpretation. If they don't follow through with n - 1, they're just plain wrong.

Proportions and the t sampling distribution

It is well known that a proportion is a special kind of arithmetic mean. It is also well known that if the population standard deviation is unknown the t sampling distribution for n – 1 degrees of freedom should be used rather than the normal sampling distribution when making statistical inferences regarding a population mean. But it turns out that the t sampling distribution should not be used for making statistical inferences regarding a population proportion. Why is that?

One of the reasons is simple to state: If you are testing a hypothesis about a population proportion you always “know” the population standard deviation, because the population standard deviation is equal to the square root of the product of the population proportion multiplied by 1 minus the population proportion. In this case, if you don’t want to use the binomial sampling distribution to test the hypothesis, and you’ll settle for a “large sample” approximation, you use the normal sampling distribution. All of this has nothing to do with t.

Problems start to arise when you want to get a confidence interval for the population proportion. You don’t know what the actual population proportion is (that’s why you’re trying to estimate it!), so you have to settle for the sample proportion when getting an interval estimate for the population proportion. What do you do? You calculate “p hat” (the sample proportion) plus or minus some number c of standard errors (where the standard error is equal to the standard deviation of the product of “p hat” and “1- p hat” divided by the sample size n).

How does t get into the act (for the interval estimation of p)? It doesn’t. Some people argue that you should use n -1 rather than n in the formula for the standard error and use the t sampling distribution for n -1 degrees of freedom in order to make the inference. Not so, argued Goodall (1995), who explained why (it involves the definition of t as a normal divided by the square root of a chi-square). Bottom line: For proportions there is no n-1 and no t. It’s n and normal.

[Incidentally, using the sample “p hat” to get a confidence interval for a population p creates another problem. If “p hat” is very small (no matter what p happens to be), and n is small, that confidence interval will usually be too tight. In the extreme, if “p hat” is equal to 0 (i.e., there are no “successes”) the standard error is also equal to 0, indicating no sampling error whatsoever, which doesn’t make sense. There is something called the “Rule of Three” that is used to get the upper bound of 3/n for a confidence interval for p when there are no “successes” in a sample of size n. See, for example, Jovanovic and Levy, 1997.]

A call to action

If you happen to be asked to serve as a reviewer of a manuscript for possible publication as an introductory statistics textbook, please insist that the authors provide a careful explanation for whatever they choose to use in the denominators for their formulas for the variance, the standard deviation, and the Pearson r, and how they handle inferences concerning proportions. If you have any influence over the people who write the software and the manuals for computer packages that calculate those expressions, please ask them to do the same. I have no such influence. I tried very hard a few years ago to get the people at SPSS to take out the useless concept of "observed power" from some of its ANOVA routines. They refused to do so.

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CHAPTER 24: STANDARD ERRORS

Introduction

What is an error? It is a difference between a "truth" and an "approximation".

What is a standard error? It is a standard deviation of a sampling distribution.

What is a sampling distribution? It is a frequency distribution of a statistic for an infinite number of samples of the same size drawn at random from the same population.

How many different kinds of standard errors are there? Aye, there's the rub. Read on.

The standard error of measurement

The standard error of measurement is the standard deviation of a distribution of a person's or an object's obtained measurements around its "true score" (what it "should have gotten"). The obtained measurements are those that were actually obtained or could have been obtained by applying a measuring instrument an infinite (or at least a very large) number of times. For example, if a person's true height (only God knows that) is 69 inches and we were to measure his(her) height a very large number of times, the obtained measurements might be something like the following: 68.25, 70.00, 69.50, 70.00, 68.75, 68.25. 69.00, 68.75, 69.75, 69.25,... The standard error of measurement provides an indication of the reliability (consistency) of the measuring instrument.

The formula for the standard error of measurement is $\sigma\surd\ 1 - \rho$, where σ is the standard deviation of the obtained measurements and ρ is the reliability of the measuring instrument.

The standard error of prediction (aka the standard error of estimate)

The standard error of prediction is the standard deviation of a frequency distribution of measurements on a variable Y around a value of Y that has been predicted from another variable X. If Y is a "gold standard" of some sort, then the standard error of prediction provides an indication of the instrument's criterion-related validity (relevance).

The formula for the standard error of prediction is $\sigma_y\surd\ 1 - \rho_{xy}^2$, where σ_y is the standard deviation for the Y variable and ρ_{xy} is the correlation between X and Y.

The standard error of the mean

The standard error of the mean is the standard deviation of a frequency distribution of sample means around a population mean for a very large number of samples all of the same size. The standard error of the mean provides an indication of the goodness of using a sample mean to estimate a population mean.

The formula for calculating the standard error of the mean is $\sigma/\sqrt{n}$, where σ is the standard deviation of the population and n is the sample size. Since we usually don't know the standard deviation of the population, we often use the sample standard deviation to estimate it.

The standard errors of other statistics

Every statistic has a sampling distribution. We can talk about the standard error of a proportion (a proportion is actually a special kind of mean), the standard error of a median, the standard error of the difference between two means, the standard error of a standard deviation (how's that for a tongue twister?), etc. But the above three kinds come up most often.

What can we do with them?

We can estimate, or test a hypothesis about, an individual person's "true score" on an achievement test, for example. If he(she) has an obtained score of 75 and the standard error of measurement is 5, and if we can assume that obtained scores are normally distributed around true scores, we can "lay off" two standard errors to the left and two standard errors to the right of the 75 and say that we are 95% confident that his(her) true score is "covered" by the interval from 65 to 85. We can also use that interval to test the hypothesis that his(her) true score is 90. Since 90 is not in that interval, it would be rejected at the .05 level.

The standard error of prediction works the same way. Lay it off a couple of times around the Y that is predicted from X, using the regression of Y on X to get the predicted Y, and carry out either interval estimation or hypothesis testing.

The standard error of the mean also works the same way. Lay it off a couple of times around the sample mean and make some inference regarding the mean of the population from which the sample has been randomly drawn.

So what is the problem?

The principal problem is that people are always confusing standard errors with "ordinary" standard deviations, and standard errors of one kind with standard errors of another kind. Here are examples of some of the confusions:

1. When reporting summary descriptive statistics for a sample, some people report the mean plus or minus the standard error of the mean rather than the mean plus or minus the standard deviation. Wrong. The standard error of a mean is not a descriptive statistic.

2. Some people think that the concept of a standard error refers only to the mean. Also wrong.

3. Some of those same people think a standard error is a statistic. No, it is a parameter, which admittedly is usually estimated by a statistic, but that doesn't make it a statistic.

4. The worst offenders of lumping standard errors under descriptive statistics are the authors of many textbooks and the developers of statistical "packages" for computers, such as Excel, Minitab, SPSS, and SAS. For all of those, and for some other packages, if you input a set of data and ask for basic descriptive statistics you get, among the appropriate statistics, the standard error of the mean.

5. [A variation of #1] If the sample mean plus or minus the sample standard deviation is specified in a research report, readers of the report are likely to confuse that with a confidence interval around the sample mean, since confidence intervals often take the form of a ± b, where a is the statistic and b is its standard error or some multiple of its standard error.

<u>So what should we do about this?</u>

We should ask authors, reviewers, and editors of manuscripts submitted for publication in scientific journals to be more careful about their uses of the term "standard error". We should also write to officials at Excel, Minitab, SPSS, SAS, and other organizations that have statistical routines for different kinds of standard errors, and ask them to get things right. While we're at it, it would be a good idea to ask them to default to n rather than n-1 when calculating a variance or a standard deviation.

CHAPTER 25: IN (PARTIAL) SUPPORT OF NULL HYPOTHESIS SIGNIFICANCE TESTING

Introduction

For the last several years it has been fashionable to deride the use of null hypothesis significance testing (NHST) in scientific research, especially the testing of "nil" hypotheses for randomized trials in which the hypothesis to be tested is that there is zero difference between the means of two experimental populations. The literature is full of claims such as these:

"The null hypothesis, taken literally (and that's the only way you can take it in formal hypothesis testing), is always false in the real world." (Cohen, 1990, p. 1308)

"It is foolish to ask 'Are the effects of A and B different'? They are always different...for some decimal place." (Tukey, 1991, p. 100)

"Given the many attacks on it, null hypothesis testing should be dead." (Rindskopf, 1997, p. 319)

"[NHST is] surely the most bone-headedly misguided procedure ever institutionalized in the rote training of science students." (Rozeboom, 1997, p. 335)

"Logically and conceptually, the use of statistical significance testing in the analysis of research data has been thoroughly discredited." (Schmidt & Hunter, 1997, p. 37)

[1997 was a good year for attacks against NHST. In the same year there appeared an entire book entitled "What if there were no significance tests?" (edited by Harlow, Mulaik, & Steiger, 1997). It consisted of several chapters, some of which were pro NHST and some of which were con (mostly con). That book followed upon an earlier book entitled "The significance test controversy" (edited by Morrison & Henkel, 1970)]

In what follows in this paper, which has a title similar to that of Hagen (1997) and is in the spirit of Abelson (1997a, 1997b), I would like to resurrect some of the arguments in favor of NHST, but starting from a different perspective, viz., that of legal and medical decision-making.

Two very important null hypotheses (and their alternatives)

The nulls:
1. The defendant is innocent.
2. The patient is well.

The alternatives:
1. The defendant is guilty.
2. The patient is ill.

Let us consider first "The defendant is innocent". Unlike most scientific null hypotheses that we would like to reject, this hypothesis is an example of a hypothesis that we would like to be able to "accept", or at least "not reject", if, of course, the defendant is in fact innocent. How do we proceed?

1. We (actually the prosecuting attorneys) gather evidence that bears upon the defendant's guilt or innocence.

2. The "sample size" (amount of evidence) ranges from the testimony of one or two witnesses to multi-year investigations, depending upon the seriousness of the crime that the defendant is alleged to have committed.

3. The evidence is tested in court, with the attorneys for the defense (often court-appointed) arguing for the truth of the null hypothesis and with the prosecuting attorneys arguing for the truth of the alternative hypothesis.

4. An inference (verdict) is rendered regarding the hypotheses. If the null hypothesis is rejected, the defendant becomes subject to some sort of penalty ranging from a fine or community service to life imprisonment or death. If the null hypothesis is not rejected, the defendant is set free.

5. No matter what inference is made, we acknowledge that a mistake could be made. We might have made a "Type I error" by rejecting a true null hypothesis, in which case an innocent person will have been punished unjustly. We would like to keep the probability of such a decision to be small. Or we might have made a "Type II error" by not rejecting a false null hypothesis, in which case a guilty person will have been set free and might commit the same crime again, or perhaps an even worse crime. We would like to keep the probabilities of either of those eventualities very small. Fortunately, we cannot commit both of those errors simultaneously, but the probabilities work at cross-purposes. As we try to decrease the probability of making a Type I error we increase the probability of making a Type II error, and vice versa. The only way to keep both probabilities small is to increase the sample size, i.e., to obtain more evidence.

[In his very balanced and very long summary of the NHST controversy, Nickerson (2000) uses this same analogy to what happens in a U.S. court of law.]

Now for "The patient is well". We all prefer to be well than to be ill, so we often seek out medical advice, usually from doctors and/or nurses, to help us in deciding which we are at any given time. How do we proceed?

1. We (ourselves, the doctors, and the nurses) gather evidence that bears upon our wellness or our illness.

2. The "sample size" (amount of evidence) ranges from seeing what happens when we say "aaahhh" to the carrying out of various procedures such as MRIs and biopsies.

3. The evidence is tested in the clinic, with everybody hoping that the null hypothesis is true and the alternative hypothesis is false..

4. An inference (decision) is made regarding the hypotheses. If the null hypothesis is rejected, we are told that we are ill and some treatment is recommended. If the null hypothesis is not rejected, we are relieved to hear that, and we are free to leave the clinic.

5. No matter what inference is made, we acknowledge that a mistake could be made. There might have been a "Type I error" in the rejection of a true null hypothesis, in which case we would be treated for an ailment or a disease that we don't have. We would like to keep the probability of such a decision to be small. Or there might have been a "Type II error" in the failure to reject a false null hypothesis, in which case we would go untreated for an ailment or a disease that we had. We would like to keep the probabilities of either of those eventualities very small. Fortunately, both of those errors cannot occur simultaneously, but the probabilities work at cross-purposes. As we try to decrease the probability of making a Type I error we increase the probability of making a Type II error, and vice versa. The only way to keep both probabilities small is to increase the sample size, i.e., to obtain more evidence (have more tests taken).

Those two situations are very similar. The principal differences are (1) the parties in the legal example are "rooting for" different outcomes, whereas the parties in the medical example are "rooting for" the same outcome; and (2) the consequences of errors in the legal example are usually more severe than the consequences of errors in the medical example. Once a defendant has been incarcerated for a crime (s)he didn't commit, it is very difficult to undo the damage done to that person's life. Once a defendant has been set free from a crime that (s)he did commit, (s)he remains a threat to society. But if we are not treated for our ailment or disease it is possible to seek such treatment at a later date. Likewise for being treated for an ailment or a disease that we don't have, the principal consequence of which is unnecessary worry and anxiety, unless the treatment is something radical that is worse than the ailment or the disease itself.

A null hypothesis that is always true

One of the arguments against NHST is the claim that the null hypothesis is always false. I would like to give an example of a null hypothesis that is always true:

The percentage of black cards in a new deck of cards is equal to 50.

What is "null" about that? It is null because it is directly testable. There is no zero in it, so it is not "nil", but it can be tested by taking a random sample of cards from the deck, determining what percentage of the sampled cards is black, and making an inference regarding the percentage of black cards in the entire deck (the population). The difference between this example and both the legal example and the medical example is that either no error is made (a true null hypothesis is not rejected) or a Type I error has been made (a true null hypothesis is rejected). There is no way to make a Type II error. (Do you see why?)

Now for null hypothesis significance testing in research.

First of all it is well to distinguish between a hypothesis imbedded in a theory and a hypothesis arising from a theory. For example, consider the theoretical non-null hypothesis that boys are better in mathematics than girls are. One operational null hypothesis arising from the theory is the hypothesis that the mean score on a particular mathematics achievement test for some population of boys is equal to the mean score on that same test for some population of girls. Is that hypothesis ever true? Some people would argue that those two means will always differ by some amount, however small, so the hypothesis is not worth testing. I respectfully disagree. To put it in a spiritual context, only God knows whether or not it is true. We mere mortals can only conjecture and test. And that brings me to the point that opponents of null-hypothesis-testing always make, viz., we should use interval estimation rather than hypothesis testing if we are seriously interested in the extent to which boys and girls differ on that achievement test.

I have nothing against interval estimation (the use of confidence intervals). As a matter of fact, I generally prefer them to hypothesis tests. But,

1. I have said elsewhere (Knapp, 2002): "If you have hypotheses to test (a null hypothesis you may or may not believe a priori and/or two hypotheses pitted against one another), use a significance test to test them. If you don’t, confidence intervals are fine." (p. 241)

2. Although interval estimation often subsumes hypothesis testing (if the otherwise hypothesized parameter is in the interval, you can't reject it; if it isn't, you can and must), there are certain situations where it either does not or it has additional problems that are not associated with the corresponding hypothesis test. For example, if you want to make an inference regarding a population percentage (or proportion), the hypothesis-testing procedure is straightforward,

with the standard error of the percentage being a simple function of the hypothesized percentage. On the other hand, in order to get a standard error to be employed in a confidence interval for a percentage you have to use the sample percentage, since you don't know the population percentage (you're trying to estimate it). When the number of "successes" in a sample is very small, the sample percentage can be a serious under-estimate of the population percentage. This is especially true if the number of "successes" in the sample is equal to zero, in which case the use of the sample percentage in the formula for the standard error would imply that there is no sampling error at all! The "Rule of three" (Jovanovic & Levy, 1997) is an attempt to cope with such an eventuality, but provides only a shaky estimate.

3. One of the arguments for preferring confidence intervals over hypothesis tests is that they go rather naturally with "effect sizes" that are in the original units of the dependent variable (i.e., are not standardized), but as Parker (1995) pointed out in his comments regarding Cohen's 1994 article, the actual difference between two means is often not very informative. (See his "number of chili peppers" example.)

4. Interval estimation is not the panacea that it is occasionally acclaimed to be. For every user of hypothesis testing who says "the probability is less than .05 that the null hypothesis is true" there is a user of interval estimation who says "I am .95 confident that the difference between the two population means varies between a and b". The p value is not an indication of the truth of the null hypothesis, and it is the difference between the sample means that varies, not the difference between the population means.

5. We could do both, i.e., use the techniques of NHST power analysis to draw a random sample of a size that is "optimal" to test our particular null hypothesis against our particular alternative hypothesis (for alpha = .05, for example) but use interval estimation with a confidence interval of the corresponding level (.95, for example) for reporting the results. There are sample-size procedures for interval estimation directly; however, they are generally more complicated and not as readily available as those for NHST. But one thing we should not do (although you wouldn't know it by perusing the recent research literature) is to report both the upper and lower limits of the confidence interval AND the actual magnitude of the p-value that is found for the hypothesis test. If we care about 1-α confidence we should only care about whether p is greater than or less than α.

A compromise

Jones & Tukey (2000) suggested that if we're interested in the difference between the means of two populations, A and B, we should investigate the difference between the corresponding sample means and then make one of the following inferences:

1. The mean of Population A minus the mean of Population B is greater than 0.

2. The mean of Population A minus the mean of Population B is less than 0.

3. The sign of the difference is yet to be determined.

Read their article. You'll like it.

Some recent references

You might have gotten the impression that the problem has gone away by now, given that the latest citation so far is to the year 2002. I assure you that it has not. The controversy regarding the use, abuse, misuse, etc. of NHST is just as hot in 2016 as it was in the heyday year of 1997. Here are a few examples:

1.. LeMire (2010) recommends a different framework as the context for NHST. He calls it NHSTAF (the A and the F are for Argument and Framework) and it is based upon the work of Toulmin (1958). It's different, but interesting in its defense of NHST.

2. Lambdin (2012) claims that psychologists know about the weaknesses of NHST but many of them go ahead and use it anyhow. He calls this psychology's "dirty little secret". He goes on to blast significance tests in general and p-values in particular (he lists 12 misconceptions about them). His article is very well written and has lots of good references

3. White (2012), in the first of several promised blogs about NHST, tries to pull together most of the arguments for and against NHST, and claims that it is important to distinguish between the problems faced by individual researchers and the problems faced by the community of researchers. That blog includes several interesting comments made by readers of the blog, along with White's replies to most of those comments.

4. Wood (2013) is an even better blog, accompanied by lots of good comments (with Wood's replies), several important references, and great pictures of R.A. Fisher, Jerzy Neyman, and Egon Pearson!

5. Cumming (2013) is a relentless advocate of interval estimation, with the use of confidence intervals around sample "effect sizes" and with a heavy reliance on meta-analysis. He calls his approach (presumptuously) "The New Statistics".

A final note

Some of the writers who have contributed to the NHST controversy remind me of the radical left and the radical right in American politics; i.e., people who are convinced they are correct and those "across the aisle" are not. A little humility, coupled with the sort of compromise suggested by Jones and Tukey (2000), could go a long way toward a solution of this vexing problem.

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CHAPTER 26: p-VALUES

I'm not a fan of p-values. They do have their place, e.g., in testing a null hypothesis such as "The proportion of successes is equal to .50" in some well-defined population, where "success" is "lived" (vs. "died"), "passed the final examination" (vs. "failed the final examination"), etc. But they do have problems, as indicated in what follows.

p and alpha

If I have the historical details right, the famous British statistician R.A. Fisher wasn't concerned about specifying alpha (the probability of making a Type I error) beforehand and determining whether or not p is less than alpha. That came later with Neyman and Pearson, along with the concept of power. Fisher didn't even intellectualize a hypothesis alternative to the null. What Fisher suggested (not dictated, as some people claim) was for the researcher to specify some probability level that was indicative of "chance" and to find out whether or not the obtained p-value was less than that. If so, the sample result was said to be (statistically) significant. He recommended .05 as a reasonable choice. It subsequently became enshrined in the research literature (alas).

p equal to or p less than

Suppose we carry out a study of the relationship between the length and the weight of newborn babies. Something (theory, practice, previous research, whatever) leads us to hypothesize that the relationship (Pearson product-moment correlation coefficient) is equal to .30 for some very large hospital in New York City. We draw a simple random sample of 200 single-birth newborns from that population, determine their lengths and weights (actually measuring them or getting the information from the hospital records), plot the data, calculate the Pearson r, and find that it is equal to .54. We carry out the significance test and discover that the probability of getting an r of .54 or more in a sample of 200 observations, given that the correlation in the population is .30, is .00002. [These same numbers, except for the .00002, appear in an online piece by Michael T. Brannick, albeit in a different artificial context.] What should we report?

p is equal to .00002?

p is less than .00002?

p is less than or equal to .00002?

p is approximately equal to .00002?

p is less than .0001? (SAS's default for very small p-values)

Something else?

Does it matter?

Two recent real examples

1. In a study published in the Annals of Epidemiology, Okosun, et al. (2010) reported several findings regarding the association between a continuous version of a scale for measuring the Metabolic Risk Syndrome (cMetS) and the usual version (MetS). Table 1 of their article contains p-values concerned with the differences among three ethnic groups of adolescents (Non-Hispanic Whites, Non-Hispanic Blacks, and Mexican-Americans) on a number of variables (age, height, weight, systolic blood pressure, diastolic blood pressure, etc.). Some of those p-values were indicated by p equal to something (e.g., p = 001 for time spent watching TV or videos for the females) and others were indicated by p less than something (e.g., p <.001 for that same variable for the males). Post hoc tests for pairs of ethnic groups were also carried out, with p < .05 as the only indicator of statistical significance.

2. Hartz, et al. (2014) were concerned with the difference in substance use between individuals with severe mental illnesses and individuals in the general population. In Table 3 of their article they report the following p-values for five different substance use variables, in addition to their corresponding 95% confidence intervals:

Variable	p-value
More than 4 alcoholic drinks per day	1.2 x 10 to the -188th power
At least 100 cigarettes in lifetime	< 1.0 x 10 to the -325th power
Daily smoking for more than a month	< 1.0 x 10 to the -325th power
Marijuana more than 21 times per year	2.6 x 10 to the -254th power
Recreational drugs more than 10 times	< 1.0 x 10 to the -325th power

[Note that the first and the fourth of those p-values are "equal to" and the other three are "less than", with the "less thans" all of the same magnitude.]

My take on the two examples

I see no reason for having some "equal to" p-values and some "less than" p-values for either example. For the cMetS example my preference is for confidence intervals only (around the differences for the pairs of samples).

The substance use example is more baffling. To their credit, the authors do provide confidence intervals around the obtained odds ratios for the various comparisons between the individuals with serious mental illnesses and individuals in the general population, but they should have quit there. The p-values do not add any useful additional information and are at least unusual (how often have you seen such tiny p-values?) if not wrong (I think they're wrong).

What is your take on p-values in general and these p-values in particular?

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CHAPTER 27: p, n, AND t: TEN THINGS YOU NEED TO KNOW

Introduction

You want to test a hypothesis or construct a confidence interval for a proportion, or for the difference between, or for the ratio of, two proportions. Should you use n or n-1 in the denominators for the formulas for the appropriate standard errors? Should you use the t sampling distribution or the normal sampling distribution? Answers to these and associated questions will be provided in what is to follow.

An example

In the guide that accompanies the StatPac Statistics Calculator, Walonick (1996-2010) gives an example of the proportions (he uses percentages, but that doesn't matter) of people who have expressed their plans to vote for Candidate A or Candidate B for a particular public office. The sample size was 107; the two proportions were .355 for Candidate A and .224 for Candidate B. (The other people in the sample planned to vote for other candidates.) How should various statistical inferences for this example be handled?

1. Single sample p and n

Let p be the sample proportion, e.g., the .355 for Candidate A, for a sample size n of 107. If you want to test a hypothesis about the corresponding population proportion π, you should use the binomial sampling distribution to do so. But since tables and computer routines for the binomial sampling distribution for (relatively) large sample sizes such as 107 are not readily available, most people choose to use approximations to the binomial. It is well known that for large samples p is normally distributed around π with standard error equal to the square root of π(1-π)/n, just as long as π is not too close to 0 or to 1. Some people use n-1 in the denominator rather than n, and the t sampling distribution rather than the normal sampling distribution. They're wrong. (See, for example, Goodall, 1995.)

The situation is similar for a confidence interval for π, but since π is unknown the sample proportion p must be used in its stead. Again, n and normal; there's no n-1 and no t.

2. The difference between two independent p's for their two n's

Let me change the example for the purpose of this section by considering the .355 for Candidate A in Survey #1 conducted by one organization vs. a proportion of .298 (I just made that up) for Candidate A in Survey #2 conducted by a different organization, so that those p's can be considered to be independent. For the usual null hypothesis of no difference between the two corresponding π's, the difference between p_1 and p_2 is approximately normally distributed around 0 with a standard error that is a function of the two p's and their respective n's. Again, no n-1's and no t. Likewise for getting a confidence interval for the difference between the two π's.

3. The difference between two non-independent p's and their common n

Once again modifying the original example, consider the p of .355 for Candidate A at Time 1 vs. a p of .298 for Candidate A at Time 2 for the same people. This is a case for the use of McNemar's test (McNemar,1947). The chi-square sampling distribution is most commonly employed for either testing a hypothesis about the difference between the corresponding π's or constructing a confidence interval for that difference, but there is an equivalent normal sampling distribution procedure. Both use n and there's no t.

4. The ratio of two independent p's

This doesn't usually come up in research in the social sciences, but it is very common in epidemiological research in the analysis of relative risks and odd ratios. As you might expect, things get very messy mainly because ratios almost always have more complicated sampling distributions than differences have. If you want to test the ratio of the p of .355 for Survey #1 to the p of .298 for Survey #2 (see above) against 1 or construct a confidence interval for the ratio of the two corresponding π's, see the compendium by Fleiss, Levin, and Paik (2003) for all of the gory details. You will find that there are no n-1's and no t's.

5. The difference between two p's for the same scale

I've saved the inference for the original Walonick example for last, because it is the most controversial. Let us consider the significance test only, since that was the inference in which Walonick was interested.

In order to test the significance of the difference between the .355 for Candidate A and the .224 for Candidate B, you need to use the ratio of the difference (.355-,224 = .131) to the standard error of that difference. The formula for the approximate standard error (see Kish, 1965; Scott & Seber, 1983; and Franklin, 2007) is the square root of the expression $[(p_1+ p_2) – (p_1 – p_2)^2]/n$, where n = 107 for this example. The relevant sampling distribution is normal, not t.
Why is this controversial? First of all, it doesn't make sense to some people (especially me). It's like testing the significance of the difference between the proportion of people who respond "strongly agree" and the proportion of people who respond "agree" to a question on an opinion poll. Or testing the significance of the difference between the proportion of people who are 5‘7" tall and the proportion of people who are 5'10" tall. The frequency distribution for the various scale points should be sufficient. Does anybody really care if the difference between the proportions for any two of them is statistically significant? And what significance level should be chosen? Are both Type I errors and Type II errors relevant?

Secondly, those two proportions in the Walonick example are bothersomely [is there such a word?] non-independent, especially if both are very close to .5. Apparently the multinomial sampling theory takes care of that, but I'm still skeptical.

Thirdly, if it makes sense to you to carry out the test, be sure to use the correct standard error (indicated above). Most people don't, according to Franklin. I'm afraid that Walonick used the wrong formula. He also used t. I can't get his ratio of 1.808 to come out no matter what I use for the standard error, whether for n or n-1, or for "pooled" p's or "unpooled" p's.

In the remainder of this chapter I would like to close with five additional comments (6 through 10) regarding p, n, and t.

6. It perhaps goes without saying, but I'd better say it anyhow: The p I'm using here is a sample proportion, not a "p-value" for significance testing. And the π is a population proportion, not the ratio of the circumference of a circle to its diameter.

7. I have a "thing" about the over-use of n-1 rather than n. The authors of many statistics textbooks first define the sample variance and the sample standard deviation with n-1 in the denominator, usually because they want their readers to get used to that when carrying out a t test or an ANOVA. But a variance should be an average (an arithmetic mean), and nobody gets an average by dividing by one less than the number of entities that contribute to it. And some of those same authors make the mistake of claiming that the standard deviation with n-1 in the denominator provides an unbiased estimate of the population standard deviation. That's true for the variance but not for the standard deviation. For more on this see my N vs. N-1 article (Knapp, 1970) and Chapter 23 of this book.

8. I also have a "thing" about people appealing to the use of the t sampling distibution rather than the normal sampling distribution for "small" samples. It is the absence of knowledge of the population variance, not the size of the sample, that warrants the use of t rather than normal.

9. I favor explicit inferences to finite populations rather than inferences for finite populations that use traditional infinite population procedures with a finite population "correction" involving n (the sample size) and N (the population size). I realize that my preference gets me into all sorts of difficult formulas, but I guess I'm willing to pay that price. All real populations that are of interest in scientific research are finite, no matter how large or how small.

10. I prefer percentages to proportions (see Chapter 15) and to talk about, for example, a 95 percent confidence interval for a population percent (I use "percentage" and "percent" interchangeably), but percentages are much more

understandable to students, particularly those who use the word “proportion” in contexts such as “a is in the same proportion to b as c is to d”.

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CHAPTER 28: THE ALL-PURPOSE KOLMOGOROV-SMIRNOV TEST FOR TWO INDEPENDENT SAMPLES

Introduction

You have data for a random sample of n_1 subjects from Population 1 and a random sample of n_2 subjects from Population 2. You'd like to test the significance of the difference between those two samples. What should you do? Carry out the traditional t test? Perhaps. But you probably should use the Kolmogorov-Smirnov test.

You have randomly assigned a random sample of n subjects to two treatment conditions (experimental and control), with n_1 subjects in the experimental group and n_2 subjects in the control group, where $n_1 + n_2 = n$, and you'd like to test the statistical significance of the effect on the principal outcome variable. What should you use there? The t test? No. Again, the better choice is the Kolmogorov-Smirnov test.

What is the Kolmogorov-Smirnov test? In what follows I will try to explain what it is, why it has not been used very often, and why it is an "all-purpose" significance test as well as an "all-purpose" procedure for constructing confidence intervals for the difference between two independent samples.

What it is

The Kolmogorov-Smirnov test, hereinafter referred to as the K-S test for two independent samples, was developed by Smirnov (1939), based upon previous work by Kolmogorov (1933). [See Hodges, 1957.] It compares the differences between two cumulative relative frequency distributions.

Consider the following example, taken from Goodman (1954):

Sample 1: 1, 2, 2, 2, 2, 4, 4, 4, 4, 5, 5, 5, 5, 5, 5 ($n_1 = 15$)

Sample 2: 0, 0, 0, 0, 1, 1, 2, 2, 2, 2, 3, 3, 5, 5, 5 ($n_2 = 15$)

The frequency distributions for Sample 1 are:

Value	Freq.	Rel. Freq.	Cum. Freq.	Cum. Rel. Freq.
0	0	0/15 = 0	0	0/15 = 0
1	1	1/15 = .067	1	1/15 = .067
2	4	4/15 = .267	5	5/15 = .333
3	0	0/15 = 0	5	5/15 = .333
4	4	4/15 = .267	9	9/15 = .600
5	6	6/15 = .400	15	15/15 = 1.000

The corresponding frequency distributions for Sample 2 are:

Value	Freq.	Rel. Freq.	Cum. Freq.	Cum. Rel. Freq.
0	4	4/15 = .267	4	4/15 = .267
1	2	2/15 = .133	6	6/15 = .400
2	4	4/15 = .267	10	10/15 = .667
3	2	2/15 = .133	12	12/15 = .800
4	0	0/15 = 0	12	12/15 = .800
5	3	3/15 = .200	15	15/15 = 1.000

The test statistic for the K-S test is the largest difference, D, between corresponding cumulative relative frequencies for the two samples. For this example the largest difference is for scale value 3, for which D = .800 - .333 = .467. How likely is such a difference to be attributable to chance? Using the appropriate formula and/or table and/or computerized routine (more about those later) the corresponding p-value is .051 (two-tailed). If the pre-specified level of significance, α, is .05 and the alternative hypothesis is non-directional the null hypothesis of no difference between the two population distributions cannot be rejected.

Nice. But what's wrong with using the t test? And why hasn't the K-S test been used more often? Let me take the first question first.

Why not t?

There are at least three things wrong with using the t test for such data:

1. The t test tests only the significance of the difference between the two sample means...nothing else. The K-S test is sensitive to differences throughout the entire scale.

2. The data might have come from a six-point Likert-type ordinal scale for which means are not appropriate; e.g., if the 0 is the leftmost scale value and might have nothing at all to do with "none". (As you undoubtedly know, there has been a never-ending controversy regarding treating ordinal scales as interval scales, to which I have contributed [Knapp, 1990, 1993], and to little or no avail. But see Marcus-Roberts & Roberts [1987] for the best resolution of the controversy that I've ever read.)

3. Even if means are appropriate the t test assumes that the two populations from which the samples have been drawn have normal distributions and equal variances. Those two assumptions are often very difficult to justify, robustness considerations to the contrary notwithstanding.

But isn't there a problem with loss of power by using this test? Not really. Wilcox (1997) has shown that the power of the K-S test can be quite high compared to that of various methods for testing differences between means.

Now for the second question.

Why has the K-S test for independent samples not been used more often?

At the beginning of his article Goodman (1954) says: "Recent results and tables on this topic have been prepared which contribute toward establishing the Kolmogorov-Smirnov statistic as a standard nonparametric tool of statistical analysis." (p. 160). That was in 1954. It's now more than sixty years later, and the K-S test is definitely not a "standard nonparametric tool", as Wilcox (1997) has documented. There are several reasons:

1. It's not even mentioned in some nonparametric statistics textbooks, chapters within general statistics textbooks, and methodological articles. Gibbons (1993), for example, treats the Sign, Wilcoxon (Mann-Whitney), Kruskal-Wallis, and Friedman tests, but there is nary a word about the K-S test.

2. Some people might be under the impression that the K-S test is strictly a goodness-of-fit test. There is indeed a K-S test of goodness-of-fit, but researchers seem to have been able to distinguish the chi-square test for independent samples from the chi-square test of goodness-of-fit, so if they can handle two chi-square tests they should be able to handle two K-S tests. (And the K-S test for two samples has even been extended to the case of three or more samples, just like the two-sample chi-square test has. See Conover [1980] and Schroer & Trenkler [1995] for details.)

3. There might be further concerns that it's too complicated and the necessary computer software is not readily available. Both concerns would be unfounded. It's simple to carry out, even by hand, as the above example in Goodman (1954) and comparable examples in Siegel and Castellan (1988) attest. Tables for testing the significance of D have been around for a long time (as have formulas for the case of two large samples) and there are at least two excellent internet sites where all the user need do is enter the data for the two samples and the software does the rest (see below).

K-S test confidence intervals

Many researchers prefer confidence intervals to significance tests, and they argue that you get significance tests "for free" when you establish confidence intervals around the test statistics. (If the null-hypothesized parameter is in the interval you can't reject it; if it isn't you can.) Sheskin's (2011) chapter on the Kolmogorov-Smirnov test for two independent samples (his Test 13) includes a procedure for constructing confidence intervals for D.

K-S test software

SAS includes a routine for both the two-sample K-S test and the goodness-of-fit K-S test. SPSS has only the latter, as does Minitab. Excel has neither, but there is a downloadable add-in that has both. There are two stand-alone routines that can carry out the two-sample K-S test. One of them (see the web address http://www.physics.csbsju.edu/stats/KS-test.n.plot_form.html) requires the entry of the raw data for each subject in each sample, with n_1 and n_2 each between 10 and 1024. That is the one I used to check Goodman's (1954) analysis (see above). The other (downloadable at Hossein Arsham's website via John Pezzullo's marvelous StatPages.org website) requires the entry of the frequency (actual, not relative) of each of the observations for each of the samples. I used that to run the two-sample K-S test for an interesting but admittedly artificial set of data that appeared in Table 1 of an article by Roberson, Shema, Mundfrom, and Holmes (1995). Here are the data:

Sample 1:

Value	Freq.	Rel. Freq	Cum. Freq.	Cum. Rel. Freq.
1	0	0/70 = 0	0	0/70 = 0
2	52	52/70 = .743	52	52/70 = .743
3	11	11/70 = .157	63	63/70 = .900
4	0	0/70 = 0	63	63/70 = .900
5	7	7/70 = .100	70	70/70 = 1.000

Sample 2:

Value	Freq.	Rel. Freq	Cum. Freq.	Cum. Rel. Freq.
1	37	37/70 = .529	37	37/70 = .529
2	10	10/70 = .143	47	47/70 = .671
3	0	0/70 = 0	47	47/70 = .671
4	0	0/70 = 0	47	47/70 = .671
5	23	23/70 = .329	70	70/70 = 1.000

This set of data is a natural for the K-S test but they do not discuss it among their suggested nonparametric alternatives to the t test. Although their article is concerned primarily with dependent samples, they introduce alternatives to t via this example involving independent samples. The two sample means are identical (2.457) but they argue, appropriately, that there are some very large differences at other scale points and the t test should not be used. (The p-value for t is 1.000.) They apply the Wilcoxon test and get a p-value of .003. Using the K-S test for the same data, John Pezzullo and I get D = .5285714 (for the scale value of 0) and the p-value is .000000006419. [Sorry to report so many decimal places, but that D is huge and that p is tiny.]

[Caution: For some reason the Arsham software requires at least six different categories (scale values) for the outcome variable used in the test. But John figured out that all you need to do is fill in frequencies of zero for any "ghost" category and everything will be fine. For the example just considered, the frequencies we entered for Sample 1 were 0, 52, 11, 0, 7, and 0 (that sixth 0 is for a non-existing sixth category) and for Sample 2 were 37, 10, 0, 0, 23, and 0 (likewise). Strange.]

In what sense is the K-S test "all-purpose"?

I've already shown that the K-S test for independent samples can be used in observational research for testing whether or not two samples have been randomly drawn from the same population distribution and in experimental research for testing a treatment effect when subjects have been randomly assigned to treatment conditions. Confidence interval procedures are also available. And since it "works" for ordinal scales as well as for interval scales (both of which can have cumulative relative frequency distributions), it can even be used for dichotomous dummy-coded (0, 1) outcome variables: All you need to determine is D (the difference for 0, since the difference for 1 has to be equal to zero) and either test it for statistical significance or put a confidence interval around it.

But surely the K-S test must have some drawbacks, or researchers wouldn't have neglected it for so long? There is one somewhat serious drawback: If the data for the underlying populations are discrete rather than continuous (which is always the case for dichotomies and for ordinal variables such as Likert-type scales), it has been shown (Noether, 1963) that the K-S test is slightly "conservative", i.e., the difference is "more significant" than the test has indicated. Otherwise, the only assumption that needs to be satisfied is the random assumption (random sampling of populations and/or random assignment to treatment conditions), but that assumption is common to all inferential procedures (and, alas, is violated a lot!).

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CHAPTER 29: TO POOL OR NOT TO POOL: THAT IS THE CONFUSION

Prologue

Isn't the English language strange? Consider the word "pool". I go swimming in a pool. I shoot pool at the local billiards parlor. I obtain the services of someone in the secretarial pool to type a manuscript for me. I participate in a pool to try to predict the winners of football games. I join a car pool to save on gasoline. You and I pool our resources.

And now here I am talking about whether or not to pool data?! With 26 letters in our alphabet I wouldn't think we'd need to use the word "pool" in so many different ways. (The Hawaiian alphabet has only 12 letters...the five vowels and seven consonants H,K,L,M,N,P,and W; they just string lots of the same letters together to make new words.)

What is the meaning of the term "pooling data"?

There are several contexts in which the term "pooling data" arises. Here are most of them:

1. Pooling variances

Let's start with the most familiar context for pooling data (at least to students in introductory courses in statistics), viz., the pooling of sample variances in a t test of the significance of the difference between two independent sample means. The null hypothesis to be tested is that the means of two populations are equal (the populations from which the respective samples have been randomly sampled). We almost never know what the population variances are (if we did we'd undoubtedly also know what the populations means are, and there would be no need to test the hypothesis), but we often assume that they are equal, so we need to have some way of estimating from the sample data the variance that the two populations have in common. I won't bore you with the formula (you can look it up in almost any statistics textbook), but it involves, not surprisingly, the two sample variances and the two sample sizes. You should also test the "poolability" of the sample variances before doing the pooling, by using Bartlett's test or Levene's test, but almost nobody does; neither test has much power.

[Note: There is another t test for which you don't assume the population variances to be equal, and there's no pooling. It's variously called the Welch-Satterthwaite test or the Behrens-Fisher test. It is the default t test in Minitab. If you want the pooled test you have to explicitly request it.]

2. Pooling within-group regression slopes

One of the assumptions for the appropriate use of the analysis of covariance(ANCOVA) for two independent samples is that the regression of Y (the dependent variable) on X (the covariate) is the same in the two populations that have been sampled. If a test of the significance of the difference between the two within-group slopes is "passed" (the null hypothesis of equality of slopes is not rejected), those sample slopes can be pooled together for the adjustment of the means on the dependent variable. If that test is "failed" (the null hypothesis of equality of slopes is rejected) the traditional ANCOVA is not appropriate and the Johnson-Neyman technique (Johnson & Neyman, 1936) must be used in its place.

3. Pooling raw data across two (or more) subgroups

This is the kind of pooling people often do without thinking through the ramifications. For example, suppose you were interested in the relationship between height and weight for adults, and you had a random sample of 50 males and a random sample of 50 females. Should you pool the data for the two sexes and calculate one correlation coefficient, or should you get two correlation coefficients (one for the males and one for the females)? Does it matter?

The answer to the first question is a resounding "no" to the pooling. The answer to the second question is a resounding "yes". Here's why. In almost every population of adults the males are both taller and heavier than the females, on the average. If you pool the data and create a scatter plot, it will be longer and skinnier than the scatterplots for the two sexes treated separately, thereby producing a spuriously high correlation between height and weight. Try it. You'll see what I mean. And read the section in David Howell's (2007) statistics textbook (page 265) regarding this problem. He provides an example of real data for a sample of 92 college students (57 males, 35 females) in which the correlation between height and weight is .60 for the males, .49 for the females, and .78 for the two sexes pooled together.

4. Pooling raw data across research sites

This is the kind of pooling that goes on all the time (often unnoticed) in randomized clinical trials. The typical researcher often runs into practical difficulties in obtaining a sufficient number of participants at a single site and "pads" the sample size by gathering data from two or more sites. In the analysis he(she) almost never tests the treatment-by-site interaction, which might "be there" and would constrain the generalizability of the findings.

5. Pooling data across time

There is a subtle version of this kind of pooling and a not-so-subtle version.

Researchers often want to combine data for various years or minutes or whatever, for each unit of analysis (a person, a school, a hospital, etc.), usually by averaging, in order to get a better indicator of a "typical" measurement. They(the researchers) usually explain why and how they do that, so that's the not-so- subtle version. The subtle version is less common but more dangerous. Here the mistake is occasionally made of treating the Time 2 data for the same people as though they were different people from the Time 1 people. The sample size accordingly looks to be larger than it is, and the "correlatedness" of the data at the two points in time is ignored, often to the detriment of a less sensitive analysis. (Compare, for example, data that should be treated using McNemar's test for correlated samples with data that are appropriately handled by the traditional chi-square test of the independence of two categorical variables.)

6. Pooling data across scale categories

This is commonly known as "collapsing" and is frequently done with Likert-type scales. Instead of distinguishing between those who say "strongly agree" from those who say "agree'", the data for those two scale points are combined into one over-all "agree" designation. Likewise for "strongly disagree" and "disagree". This can result in a loss of information, so it should be used as a last resort.

7. Pooling "scores" on different variables

There are two different ways that data can be pooled across variables. The first way is straightforward and easy. Suppose you were interested in the trend of average (mean) monthly temperatures for a particular year in a particular city. For some months you have temperatures in degrees Fahrenheit and for other months you have temperatures in degrees Celsius. (Why that might have happened is not relevant here.) No problem. You can convert the Celsius temperatures to Fahrenheit by the formula F = (9/5)C + 32; or you can convert the Fahrenheit temperatures to Celsius by using the formula C = (5/9) (F - 32).

The second way is complicated and not easy. Suppose you were interested in determining the relationship between mathematical aptitude and mathematical achievement for the students in your particular secondary school, but some of the students had taken the Smith Aptitude Test and other students had taken the Jones Aptitude Test. The problem is to estimate what score on the Smith test is equivalent to what score on the Jones test. This problem can be at least approximately solved if there is a normative group of students who have taken both the Smith test and the Jones test, you have access to such data, and you have for each test the percentile equivalent to each raw score on each test. For each student in your school who took Smith you use this "equipercentile method" to estimate what he(she) "might have gotten" on Jones. Assign to him(her) the Jones raw score equivalent to the percentile rank that such persons obtained on Smith. Got it? Whew!

8. Pooling data from the individual level to the group level

This is usually referred to as "data aggregation". Suppose you were interested in the relationship between secondary school teachers' numbers of years of experience and the mathematical achievement of their students. You can't use the individual student as the unit of analysis, because each student doesn't have a different teacher (except in certain tutoring or home-school situations). But you can, and should, pool the mathematical achievement scores across students in their respective classrooms in order to get the correlation between teacher years of experience and student mathematical achievement.

9. Pooling cross-sectional data to approximate panel data

Cross-sectional data are relatively easy to obtain. Panel (longitudinal) data are not. Why? The principal reason is that the latter requires that the same people are measured on each of the occasions of interest, and life is such that people often refuse to participate on every occasion or they are unable to participate on every occasion (some even die). And you might not even want to measure the same people time after time, because they might get bored with the task and just "parrot back" their responses, thereby artificially inflating the correlations between time points.

What has been suggested is to take a random sample of the population at Time 1, a different random sample at Time 2,...etc. and compare the findings across time. You lose the usual sensitivity provided by having repeated measurements on the same people, but you gain some practical advantages.

There is a more complicated approach called a cross-sectional-sequential design, whereby random samples are taken from two or more cohorts at various time points. Here is an example (see Table 1, below) taken from an article that Chris Kovach and I wrote several years ago (Kovach & Knapp, 1989, p. 26). You get data for five different ages (60, 62, 64, 66, and 68) for a three-year study(1988, 1990, 1992). Nice, huh?

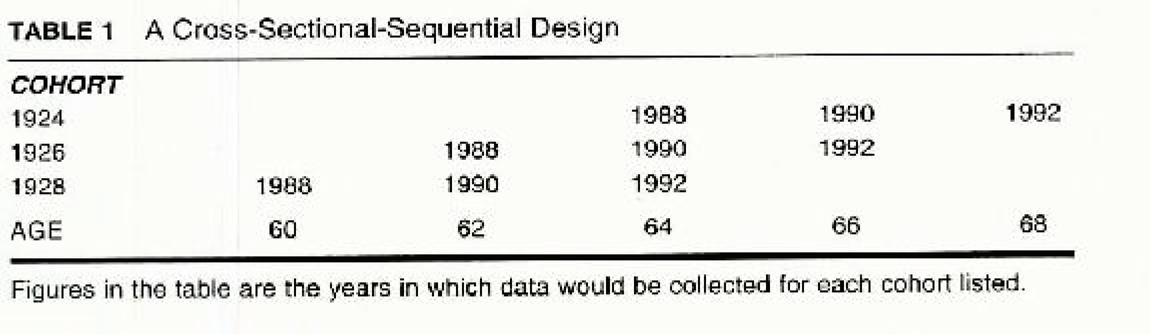

TABLE 1 A Cross-Sectional-Sequential Design

COHORT					
1924			1988	1990	1992
1926		1988	1990	1992	
1928	1988	1990	1992		
AGE	60	62	64	66	68

Figures in the table are the years in which data would be collected for each cohort listed.

10. Pooling findings across similar studies

This very popular approach is technically called "meta-analysis" (the term is due to Glass, 1976), but it should be called "meta-synthesis" (some people do use that term), because it involves the combining of results, not the breaking-down of results. I facetiously refer to it occasionally as "a statistical review of related literature", because it has come to replace almost all narrative reviews in certain disciplines. I avoid it like the plague; it's much too hard to cope with the problems involved. For example, what studies (published only? published and unpublished?) do you include? How do you determine their "poolability"? What statistical analysis(es) do you employ in combining the results?

Summary

So, should you pool or not? Or, putting it somewhat differently, when should you pool and when should you not? The answer depends upon the following considerations, in approximately decreasing order of importance:

1. The research question(s). Some things are obvious. For example, if you are concerned with the question "What is the relationship between height and weight for adult females?" you wouldn't want to toss in any height&weight data for adult males. But you might want to pool the data for Black adult females with the data for White adult females, or the data for older adult females with the data for younger adult females. It would be best to test the poolability before you do so, but if your sample is a simple random sample drawn from a well-defined population of adult females you might not know or care who's Black and who's White. On the other hand, you might have to pool if you don't have an adequate number of both Blacks and Whites to warrant a separate analysis for each.

2. Sample size. Reference was made in the previous paragraph to the situation where there is an inadequate number of observations in each of two (or more) subgroups, which would usually necessitate pooling (hopefully poolable entities).

3. Convenience, common sense, necessity

In order to carry out an independent samples t test when you assume equal population variances, you must pool. If you want to pool across subgroups, be careful; you probably don't want to do so, as the height and weight example (see above) illustrates. When collapsing Likert-type scale categories you might not have enough raw frequencies (like none?) for each scale point, which would prompt you to want to pool. For data aggregation you pool data at a lower level to produce data at a higher level. And for meta-analysis you must pool; that's what meta-analysis is all about.

A final caution

Just as "acceptance" of a null hypothesis does not mean it is necessarily true,"acceptance" in a poolability test does not mean that poolability is necessarily justified.

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CHAPTER 30: LEARNING STATISTICS THROUGH BASEBALL

Introduction

Twenty years ago I wrote a little book entitled Learning statistics through playing cards (Knapp, 1996), in which I tried to explain the fundamental concepts of statistics (both descriptive and inferential) by using an ordinary deck of playing cards for generating the numbers. In 2003 Jim Albert wrote Teaching statistics through baseball. What follows can be thought of as a possible sequel to both books, with its emphasis on descriptive statistics and the tabletop dice game "Big League Baseball".

The reason I am restricting most of this presentation to descriptive statistics is that there is no random sampling in baseball (more about this later), and random sampling is the principal justification for generalizing from a sample (a part) to a population (the whole). But it has been said, by the well-known statistician John Tukey and others, that there has been too much emphasis on inferential statistics anyhow. See his classic 1977 book, Exploratory data analysis (EDA) and/or any of the popular computer packages that have implemented EDA.

The price you might have to pay for reading this chapter is not in money (it's free) but in the time and effort necessary to understand the game of baseball. (Comedian Bob Newhart's satirical routine about the complications of baseball is hilarious!) In the next couple of sections I will provide the basics. If you think you need to know more, watch a few games at your local Little League field or a few Major League games on TV (especially if Los Angeles Dodgers broadcaster Vin Scully is the announcer).

How the game is played

As many of you already know, there are nine players on each team and the teams take turns batting and fielding. The nine players are:

1. The pitcher (who throws the ball that the batter tries to hit)
2. The catcher (who catches any ball the batter doesn't hit and some others)
3. The first baseman (who is "the guardian" of the base that the batter must first run to after hitting the ball)
4. The second baseman (the "guardian" of the next base)
5. The shortstop (who helps to guard second base, among other things)
6. The third baseman (the "guardian" of that base)
7. The left fielder (who stands about 100 feet behind third base and hopes to catch any balls that are hit nearby)
8. The center fielder (who is positioned similarly behind second base)
9. The right fielder (likewise, but behind first base).

The object of the game as far as the batters are concerned is to run counter-clockwise around the bases (from first to second to third, and back to "home plate" where the ball is batted and where the catcher is the "guardian"). The object of the game as far as the fielders are concerned is to prevent the batters from doing that.

Some specifics:

1. There are nine "innings" during which a game is played. An inning consists of each team having the opportunity to bat until there are three "outs", i.e., three unsuccessful opportunities for the runners to reach the bases before the ball (thrown by the fielders) gets there. If the runner reaches the base before the ball does, he is credited with a "hit".

2. Each batter can choose to swing the bat or to not swing the bat at the ball thrown by the pitcher. If the batter chooses to swing, he has three opportunities to try to bat the ball; failure on all three opportunities results in three "strikes" and an "out". If the batter chooses to not swing and the ball has been thrown where he should have been able to bat it, that also constitutes a strike. But if he chooses to not swing and the pitcher has not thrown the ball well, the result is called a "ball". He (the batter) is awarded first base if he refrains from swinging at four poor pitches. (Check out what Bob Newhart has to say about why there are three strikes and four balls!)

In reality there are often many combinations of swings and non-swings that result in successes or failures. For example, it is quite common for a batter to swing and miss at two good pitches, to not swing at two bad pitches, and to eventually swing, bat the ball, run toward first base, and get there either before or after the ball is caught and thrown to the first baseman.

3. The team that scores the more "runs" (encirclings of the bases by the batters) after the nine innings are played is the winner.

There are several other technical matters that I will discuss when necessary.

What does this have to do with statistics?

My favorite statistic is a percentage (see Chapter 15 of this book), and percentages abound in baseball. For example, one matter of great concern to a batter is the percentage of time that he bats a ball and arrives safely at first base (or even beyond) before the ball gets there. If a batter gets one successful hit every four times at bat, he is said to have a "batting average" of 1/4 or 25% or .250. (In baseball such averages are always carried out to three decimal places.) That's not very good, but is fairly typical. The average batting average of the regular players in the Major Leagues (there are two of them, the American League and the National League, with 15 teams in each league) has remained very close to .260 for many, many years. (See the late paleontologist Stephen Jay Gould's 1996 book, Full house, and his 2004 book, Triumph and tragedy in Mudville.)

Similarly, a matter of great concern to the pitcher is that same percentage of the time that a batter is successful against him. (One of the neat things about baseball is the fact that across all of the games played, "batting average of" for the batters must be equal to "batting average against" for the pitchers.) Batters who bat successfully much higher than .250 and pitchers who hold batters to averages much lower than .250 are usually the best players.

Other important percentages are those for the fielders. If they are successful in "throwing out" the runners 95 percent or more of the time (fielding averages of .950 or better) they are doing their jobs very well.

Some other statistical aspects of baseball

1. In a previous paragraph I pointed out that the average batting average has remained around .260. The average standard deviation (the most frequently used measure of variability...see below) has decreased steadily over the years. It's now approximately .030. (See Gould, 1996 and 2004, about that also.)

2. One of the most important concepts in statistics is the correlation between two variables such as height and weight, age and pulse rate, etc. Instead of, or in addition to, "batting average against", baseball people often look at a pitcher's "earned run average", which is calculated by multiplying by nine the number of earned runs given up and dividing by the number of innings pitched. (See Charles M. Schulz's 2004 book, Who's on first, Charlie Brown? , page 106, for a cute illustration of the concept.) Those two variables, "batting average against" and "earned run average", correlate very highly with one another, not surprisingly, since batters who don't bat very well against a pitcher are unlikely to score very many runs against him.

3. The matter of "weighting" certain data is very common in statistics and especially common in baseball. For example, if a player has a batting average of .250 against left-handed pitchers and a batting average of .350 against right-handed pitchers, it doesn't necessarily follow that his overall batting average is .300 (the simple average of the two averages), since he might not have batted against left-handed pitchers the same number of times as he did against right-handed pitchers. This is particularly important in trying to understand something called Simpson's Paradox (see below).

4. "Unit of analysis" is a very important concept in statistics. In baseball the unit of analysis is sometimes the individual player, sometimes the team, sometimes the league itself. Whenever measurements of various aspects of baseball are taken, they should be independent of one another. For example, if the team is

the unit of analysis and we find that there is a strong correlation between the number of runs scored and the number of hits made, the correlation between those same two variables might be higher and might be lower (it's usually lower) if the individual player is taken as the unit of analysis, and the number of "observations" (pieces of data) might not be independent in the latter case, since player is "nested" within team.

5. "Errors" arise in statistics (measurement errors, sampling errors, etc.) and, alas, are unfortunately also fairly common in baseball. For example, when a ball is batted to a fielder and he doesn't catch it, or he catches it and then throws wildly to the baseman, thereby permitting the batter to reach base, that fielder is charged with an error, which can sometimes be an important determinant of a win or a loss.

A "simulated" game

In his 2003 book, Jim Albert displays the following tables for simulating a game of baseball, pitch by pitch, using a set of three dice (one red die and two white dice). This approach, called [tabletop] Big League Baseball was marketed by Sycamore Games in the 1960s.

Result of rolling the red die in “Big League Baseball."

Red die	Pitch result
1, 6	Ball in play
2, 3	Ball
4, 5	Strike

Result of rolling the two white dice in “Big League Baseball."

		Second die					
		1	2	3	4	5	6
First die	1	Single	Out	Out	Out	Out	Error
	2	Out	Double	Single	Out	Single	Out
	3	Out	Single	Triple	Out	Out	Out
	4	Out	Out	Out	Out	Out	Out

	5	Out	Single	Out	Out	Out	Single
	6	Error	Out	Out	Out	Single	Home run

In the first Appendix to this chapter I have inserted an Excel file that lists the results of over 200 throws I made of those dice in order to generate the findings for a hypothetical game between two teams, call them Team A and Team B. [We retirees have all kinds of time on our hands to do such things, but I "cheated" a little by using RANDOM.ORG's virtual dice roller rather than actually throwing one red die and two white dice.] Those findings will be used throughout the rest of this chapter to illustrate percentages, correlations, and other statistical concepts that are frequently encountered in real-life research. "Big League Baseball" does not provide an ideal simulation, as Albert himself has acknowledged (e.g., it regards balls and strikes as equally likely, which they are not), but as I like to say, "it's close enough for government work".

You might want to print out the raw data in that Appendix in order to trace where all of the numbers in the next several sections come from.

Some technical matters regarding the simulated game:

1. I previously mentioned that balls and strikes are treated as equally likely, although they are not. Similarly, the probabilities associated with “white die 1” and “white die 2” do not quite agree with what actually happens in baseball, but once again they’re close enough.
2. You might have noticed that Batter A1 followed Batter A9 after each of the latter’s appearances. B1 likewise followed B9, etc. throughout the game. But it is quite often the case that players are replaced during a game for various reasons (injury, inept play, etc.). Once a player is replaced he is not permitted to re-enter the game (unlike in basketball and football).
3. There were a couple of occasions where a batter swung at a pitch when he already had three straight balls. That is unusual. Most batters would prefer to not swing at that fourth pitch, hoping it might also be a ball.
4. There was at least one occasion where a runner advanced one base when the following batter got a single. Sometimes runners can advance more than one base in such situations.
5. The various permutations in the second table do not allow for somewhat unusual events such as a batter actually getting struck by a pitch or a batter hitting into a “double play” in which both he and a runner already on base are out.
6. Most of the time a ball in play (a roll of a 1 or a 6 with the red die) resulted in an out, which is in fact usually the case. We don’t know, however, how those outs were made. For example, was the ball hit in the air and was caught by the left fielder? Was the ball hit on the ground to the second

baseman who then threw the ball to the first baseman for the out? Etc. As far as the score goes, it doesn't really matter. But it does matter to the players and to the "manager" (every team has a manager who decides who is chosen to play, who bats in what order, and the like).

7. We also don't know who made the errors. As far as the score goes, that doesn't really matter either. But it does matter to the individual players who made the errors, since it affects their fielding averages.
8. A word needs to be said about the determination of balls and strikes. In the game under consideration, if a strike was the result of a pitch thrown by the pitcher we don't know if the batter swung and missed (which would automatically be a strike) or if he "took" a pitch at which he should have swung. It is the "umpire" of the game (every game has at least one umpire) who determines whether or not a pitch was good enough for a batter to hit.
9. Although it didn't happen in this game, sometimes the teams are tied after nine innings have been played. If so, one or more innings must be played until one of the teams gets ahead and stays ahead. Again, unlike in basketball and football, there is no time limit in baseball.
10. If the team that bats second is ahead after 8 ½ innings there is no reason for them to bat in the last half of the 9th inning, since they have already won the game. That also didn't happen in this particular game, but it is very common.

Basic Descriptive Statistics

1. Frequency distributions

A frequency distribution is the most important concept in descriptive statistics. It provides a count of the number of times that each of several events took place. For example, in the simulated data in the Appendix we can determine the following frequency distribution of the number of hits made by the players on Team A in their game against Team B:

Number of hits	Frequency
0	3
1	2
2	4
3 or more	0

Do you see how I got those numbers? Players A5, A6, and A7 had no hits; Players A4 and A9 had one each (A4 had a double in the seventh inning; A9 had a single in the second inning); and Players A1, A2, A3, and A8 had two each (A1 had a double in the first inning and a home run in the third inning; A2 had a single in the third inning and a single in the seventh inning; A3 had a single in the seventh inning and a single in the ninth inning; and A8 had a single in the second inning and a single in the seventh inning). Check those by reading line-by-line for each of those players in the Appendix.

2. Measures of "central tendency"

The arithmetic mean (or, simply, the mean, i.e., the traditional "average"). There was a total of 10 hits made by the 9 players, yielding a mean of 10/9 = 1.111 hits.

The median. Putting the number of hits in rank order we have 0,0,0,1,1,2,2,2,and 2. The middle number in that set of nine numbers is 1, so the median is 1 hit.

The mode. The most frequently occurring number of hits is 2 (there are four of them), so the mode is 2 hits.

Others:

There is something called the geometric mean. It is calculated by finding the "nth" root of the product of the "n" events, where n is the total number of events (which are called "observations" in statistical lingo). There is also the harmonic mean…the reciprocal of the mean of the reciprocals of the n observations. Neither of those comes up very often, especially in baseball.

The mean is usually to be preferred when the actual magnitude of each observation is relevant and important, especially when the frequency distribution is symmetric (see below). The median is usually to be preferred when all of the actual magnitudes are less important and the frequency distribution is skewed. The mode is usually not reported because there is often more than one mode (in which case it can be said that there is no mode), but the case of two modes is of some interest. (See Darrell Huff's delightful 1954 book, How to lie with statistics, for some hilarious examples where the mean, the median, or the mode is to be preferred; and see my article about bimodality (Knapp, 2007).

3. Measures of variability

The range. The fewest number of hits is 0, and the greatest number of hits is 2, so the range is 2 - 0 = 2 hits.

The variance. This will be complicated, so hang on to your ballcaps. The variance is defined as the mean of the squared differences ("deviations") from the mean. [How's that for a difficult sentence to parse!] The three players who had no hits have a 0 - 1.111 = -1.111 difference from the mean. The square of -1.111 is 1.234 [trust me or work it out yourself]. Since there are three of those squared differences, their "contribution" to the variance is 3 x 1.234 = 3.702 squared hits. (More about "squared hits" in the next section.) The two players who had one hit each have a 1 - 1.111 = -.111 difference, which when squared is

.012, and when subsequently multiplied by 2 is .024 squared hits (their contribution to the variance). And the four players who had two hits each have a difference of 2 - 1.111 =.889, which when squared is .790 and when multiplied by 4 is 3.160 squared hits. Adding up all of those squared differences we have 3.702 + .024 + 3.160 = 6.886 squared hits. Dividing that by 9 (the number of players on Team A), we get a mean squared difference of .765 squared hits. That's the variance for these data. Whew!

The standard deviation. As you can see, the variance comes out in the wrong units (squared hits), so to get back to the original units we have to "unsquare" the .765, i.e., take its square root, which is .875 hits. That's the standard deviation. It provides an indication of the "typical" difference between a measurement and the mean of all of the measurements.

[Would you believe that some people divide the sum of the squared deviations from the mean by one less than the number of observations rather than the number of observations, when calculating the variance and the standard deviation? The reason for that is very complicated, alas, but need not concern us, since it has nothing to do with descriptive statistics.]

The mean [absolute] deviation. Rather than going through all of that squaring and unsquaring it is sometimes better to take the absolute value of each of the differences and find the mean of those. Doing so here, we would have 3x 1.111 + 2 x .111 + 4 x .889 = 3.333 + .222 + 3.556 = 7.111, divided by 9, which is .790 hits. This statistic doesn't come up very often, but it should.

4. Skewness and kurtosis statistics: There are a couple of other descriptive statistics that come up occasionally. One is an indicator of the extent to which a frequency distribution is symmetric (balanced), and is called a measure of the ”skewness” of the distribution. ("Outliers", i.e., unusual events, are a particularly bothersome source of skewness.) Another descriptive statistic is an indicator of the extent to which a frequency distribution has most of the events “piled up” at a particular place (usually around the middle of the distribution), and is called a measure of the “kurtosis” of the distribution. The procedures for calculating such measures are complicated (even more so than for the variance and the standard deviation). Suffice it to say that the above distribution is slightly skewed and not heavily concentrated in one place.

5. A measure of relationship: Pearson product-moment correlation coefficient

Suppose we were interested in the question: What is the relationship between the number of pitches thrown to a batter and the number of hits he gets? The data for Team A are the following:

Player	Number of pitches (X)	Number of hits (Y)
A1	9	2
A2	8	2
A3	16	2
A4	11	1
A5	7	0
A6	11	0
A7	16	0
A8	17	2
A9	7	1

The best way to describe and summarize such data is to construct a "scatter diagram", i.e., to plot Y against X. I "asked" my favorite statistical software, Minitab, to do this (I have an old but very nice version.) Here's what I got:

```
Y       -
        -       *   *                           *
        -
    1.80+
        -
        -
        -
        -
    1.20+
        -
        -   *               *
        -
        -
    0.60+
        -
        -
        -
        -
   -0.00+      *               *                   *   *
         --------+---------+---------+---------+---------+--------X
               8.0      10.0      12.0      14.0      16.0
```

[Each asterisk is a data point.]

I then asked Minitab to get the correlation between X and Y. It replied:

Correlation of X and Y = -0.223

Next, and last (at least for now), I asked Minitab to "regress" Y on X in order to get an equation for predicting Number of hits from Number of pitches. It replied with this (among other things):

```
The regression equation is
 Y = 1.47 - 0.0513 X

 s = 0.9671      R-sq = 5.0%      R-sq(adj) = 0.0%
```

Interpretation:

From the plot you can see that there is not a very strong relationship between the two variables X and Y. (The points are all over the place.) The correlation (specifically, the Pearson product-moment correlation coefficient) is actually negative (and small), indicating a slight inverse relationship, i.e., as X increased Y decreased and as X decreased Y increased. The "Pearson r" is a measure of the degree of linear relationship between two variables (how close do the points come to falling on a straight line?) and ranges between -1 and +1, with the negative values indicative of an inverse relationship and the positive values indicative of a direct relationship.

The R-sq of 5.0% is the square, in percentage terms, of the Pearson r. (.223 multiplied by itself is approximately .05.) It suggests that about 5% of the variance of Y is associated with X (but not necessarily causally...see next paragraph). The R-sq (adj) of 0% is an attempt to adjust the R-sq because you are trying to "fit" a line to only nine data points. (If there were just two data points you'd have to get a perfect fit, since two points determine a line.) For all intents and purposes, the fit in this case is bad.

Be particularly careful about interpreting any sort of relationship as causal. Even if the Pearson r had been 1.000 and the prediction of Y from X had been perfect, it would not necessarily follow that X caused Y, i.e., that having a certain number of pitches thrown to him would "make" a batter get a certain number of hits. The old adage that "correlation is not causation" is true.

There are lots of other measures of the relationship between two variables , e.g., Spearman’s rank correlation and Goodman & Kruskal’s gamma and lambda, but they too come up only occasionally.

Some descriptive statistics for Team B:

Frequency distribution of hits:

Number of hits	Frequency
0	5
1	4
2 or more	0

Players B1, B3, B4, B5, and B6 had no hits; B2 had a double in the first inning; B7 had a double in the seventh inning, B8 had a triple in the fourth inning, and B9 had a single in the third inning.

Their mean number of hits was 4/9 = .444.
The standard deviation was .497

So Team A had more hits (their mean was 1.111 and their standard deviation was .765) than Team B, which might be the reason why they won the game. But is the difference in the two means "statistically significant"? See the next section.

No inferential statistics for these data

My "liberal" colleagues would carry out a "t test" of the significance of the difference between the 1.111 for Team A and the .444 for Team B. I wouldn't. Here's why:

1. Although the data for the two teams constitute samples of their baseball prowess (assuming that they play more than one game against one another), there is nothing to indicate that the data we have come from random samples.

2. Random sampling is a requisite for inferential statistics in general, and for the t test in particular.

3. Because the example is a hypothetical one, the RANDOM.ORG program was used to generate the data, but it does not follow that every baseball game played by a team is a random sample of its typical performance. [Do you understand that?]

This is a very important matter. All we can say is that Team A had an average number of hits that was greater than the average number of hits obtained by Team B on this particular occasion.

Simpson's Paradox

It is well known in mathematical statistics that a percentage A can be greater than a percentage B for one category of a dichotomy [a dichotomy is a variable having just two categories], a percentage C can be greater than a percentage D for the other category of the dichotomy, yet the "pooled percentage" of A and C can be less than the "pooled percentage" of B and D. It all depends upon the numbers that contribute to the various percentages. In an article I wrote several years ago (Knapp, 1985) I gave an actual example of a batter X who had a higher batting average than another batter Y against left-handed pitching, had a higher batting average against right-handed pitching also, but had an overall lower batting average. [I wasn't the first person to point that out; many others have done so.] A few years later I sent a copy of that article to the Cooperstown

NY Baseball Hall of Fame, along with a transitive example where X >Y>Z against both left- and right-handed pitchers but X<Y<Z overall. The fact that such an eventuality can happen (it is admittedly not very frequent) presents an interesting dilemma for a manager who needs to decide whether to choose X or Y to be a player in any given game.

More on baseball and Pearson's r

I have chosen for illustrative purposes of further uses of the Pearson Correlation a set of data collected a few years ago consisting of the ages, heights, weights, and positions (pitcher, catcher, first base, etc.) of the players on each of the 30 Major League Baseball teams (14 in the American League and 16 in the National League; there are now, in 2016, 15 teams in each of the two leagues). This choice is not only based on (bad pun) my personal interest in the sport but also on people's general interest in height and weight. The total number of observations is 1034, although there is one missing weight (for a pitcher on the Cincinnati Reds). The data are available free of charge on the internet, but I have prepared Minitab, SPSS, and Excel versions that I will be happy to send to anyone who might be interested. (Some of you might have already gotten a copy of the data.) My personal preference is Minitab, and all of the analyses in what follows have been carried out by using that computer program (more specifically a 1986 version, with which I am perfectly satisfied).

Suppose you were interested in the question: "What is the relationship between height and weight?" for Major League Baseball Players. If you have handy the data I will be referring to throughout this paper, there are several prior questions that you need to ask yourself before you do any plotting, calculating, or inferring. Here are ten of them. (The questions are addressed to the reader, whether you are a teacher or a student.)

1. Do you care about all of the players (the entire population of 1034 players) or just some of them? If all of them, you can plot and you can calculate, but there is no statistical inference to be made. Whatever you might choose to calculate are parameters for populations, not statistics for samples.

2. If all of them, do you care about what teams they're on, what positions they play, or their ages; or are you only interested in the "over-all" relationship between height and weight, regardless of team membership, position, or age?

3. If you do care about any of those matters, when it comes to plotting the data, how are you going to indicate same? For example, since there are 30 teams, will you use some sorts of symbols to indicate which of the data points correspond with which of the teams? How can you do that without cluttering things up? [Even if you don't care, trying to plot 1034 points (or 1033 points...remember that one weight is missing) so you can see what is going on is no easy task. Sure,

Minitab or SPSS or Excel will do it for you (Minitab did it for me), but the picture might not be very pretty. More about that later.]

4. If just some of the players, which ones? The questions already raised are equally applicable to "sub-populations", where a sub-population consists of one of the 30 teams. (The number of observations for each of the teams varies from a low of 28 to a high of 38, with most around 35.) Sub-populations can and should be treated just like entire populations.

5. If you care about two of the sub-populations, say Team 10 and Team 27, how are you going to handle the "nesting" problem? (Player is nested within team.) Are you interested in the relationship between height and weight with the data for the two teams "pooled" together or in that relationship within each of the teams? This turns out to be one of the most important considerations in correlational research and is also the one that is often botched in the research literature. The "pooled" correlation can be vastly different from the correlation within each team. [Think about what would happen if you plot weight against height for a group of people that consists of half males and half females.] Therefore, even if you don't care about the within-team correlation, you should plot the data for each team separately and calculate the within-team correlations separately in order to determine whether or not the two sets of data are "poolable".

6. If you're interested in the entire population but rather than study it in full you want to take a sample from the population (the usual case) and generalize from sample to population (the usual objective), how do you do the sampling? Randomly? (Ideally.). What size sample, and why? With or without replacement? If with replacement, what will you do if you sample the same person more than once (unlikely, but possible)?

7. Suppose you choose to draw a simple random sample of size 30 without replacement. How will you do that? Use a table of random numbers found in the back of a statistics textbook? (The players are numbered from 1 to 1034, with the American League players listed first.) Use the random-sampling routine that is included in your favorite statistical "package" ? (All three of Minitab, SPSS, and Excel have them, and they are of varying degrees of user-friendliness.) Or use one of those that are available on the internet, free of charge? (There is a nice one at the random.org website.)

8. Do you expect the random sample of size 30 to consist of one player from each of the 30 teams? If so, you're dreaming. (It's possible but extremely unlikely.) Under what circumstances, if any, would you feel unsatisfied with the sample you draw and decide to draw a different one? (Please don't do that.)

9. Suppose you choose to draw a "stratified" random sample rather than a simple one. What variable (team, position, age) would you stratify on? Why? If age (a continuous variable carried out to two decimal places!), how would you do

the stratification? How do you propose to "put the pieces (the sub-samples) back together again" for analysis purposes?

10. Whether you stratify or not, will you be comfortable with the routines that you'll use to plot the data, calculate the Pearson r, and carry out the inference from sample to population? Do you favor significance tests or confidence intervals? (Do you know how the two are related?)

Plotting

Let's consider, in turn, three of the examples alluded to above:

1. Entire population
2. Two sub-populations (Team 10 and Team 27)
3. Simple random sample from entire population

I asked Minitab to plot weight (Y) against height (X) for all 1034 players. Here it is: [N* = 1 indicates that one point is missing; the symbols in the heart of the plot indicate how many points are in the various regions of the X,Y space...the * indicates a single point and the + indicates more than 9]:

```
        -
     300+
        -                                *
weight  -                          *    *
        -                      *
        -              *          2   *         *     *
     250+               *    *   2  4  4   5  * *   *    *
        -              *  2   4  6  +   6  4   2  3
        -         * *   3  6   +  +   +   8  7  *     * *
        -       * *   5  +  +   +  +   +  +   5  *  *
        -    *  *  3   6  +  +   +  +  +   8  6   4
     200+       * *   6   +  +  +   +  +  +   +   3 *
        -       2  +   +  +  +   +  +   +   5  * 3
        - *  *  8   +  +   +  +   +  7  *
        - *  *  4  8   8   +  6   8  2  4  *
        -    2  2   4  3   3  4   2  *
     150+       *       *  2
        -
        -
         --------+---------+---------+---------+---------+-----height
               69.0      72.0      75.0      78.0      81.0

      N* = 1
```

Notice the very-close-to-elliptical shape of the plot. Does that suggest to you that the relationship is linear but not terribly strong? [It does to me.] That's nice, because Pearson r is a measure of the direction and the magnitude of linear relationship between two variables. The famous British statistician Karl Pearson invented it over 100 years ago. It can take on any values between -1 and +1, where -1 is indicative of a perfect inverse (negative) linear relationship and +1 is indicative of a perfect direct (positive) linear relationship. Notice also how difficult it would be to label each of the data points according to team membership. The plot is already jam-packed with indicators of how many points there are in the various regions.

Take a guess as to the value of the corresponding Pearson r. We'll come back to that in the "Calculating" section (below).

I then asked Minitab to make three weight-vs.-height plots for me, one for Team 10, one for Team 27, and one for the two teams pooled together. Here they are:

First, Team 10 (number of players = 33):

```
weight  -
        -                                            *
        -
     225+
        -                       2     *
        -             *     *               *
        -
        -   *               *    *    *
     200+                *     5          *               *
        -
        -   *    *               2     2
        -                  *               *
        -                  *          2
     175+                *
        -             *
        -
        -                  *
        -
     150+
         ------+---------+---------+---------+---------+------+height
            70.5      72.0      73.5      75.0      76.5      78.0
```

Second, Team 27 (number of players = 36):

```
weight  -
        -                    *                     *
        -
     245+                    *
        -                 *
        -                 *  *
        -                 *
        -                 2
     210+                    2   *   *
        -        2  *         *   *
        -        *  *
        -           2   *     *
        -           2   3
     175+              *
        - *   *  *         *
        - *
        -
        -    *
     140+
         --+---------+---------+---------+---------+---------+-height
         70.0      72.5      75.0      77.5      80.0      82.5
```

Third, both teams pooled together (number of players = 69):

```
weight  -
        -                    *                     *
        -
     245+                    *
        -                 *         *
        -                 *  *
        -                 *
        -        *  *   2   3  *
     210+                    2   *   *
        - *      3   7   *   2  *   *  *
        -        *  *
        - *   *      2   3  2   2
        -           4   3   2
     175+           *  *
        - *   *  2         *
        - *         *
        -
        -    *
     140+
         --+---------+---------+---------+---------+---------+-height
         70.0      72.5      75.0      77.5      80.0      82.5
```

There are several things to notice regarding those three plots. [I'll bet you've "eye-balled" some of the features already.] The first thing that caught my eye was the difference in the plots for the two teams taken separately. The Team 10 plot, although reasonably linear, is rather "fat". The Team 27 plot is very strange-looking, does not appear to be linear, and has that extreme "outlier" with height over 82.5 inches and weight over 245 pounds. (He's actually 83 inches tall and weighs 260 pounds...a very big guy.) The third plot, for the pooled data, looks more linear but is dominated by that outlier.

Once again, try to guess what the three correlations will be, before carrying out the actual calculations (see the following section). What do you think should be done with the outlier? Delete it? Why or why not? Are the data for the two teams poolable, and having combined them is OK? Why or why not?

Lastly, I asked Minitab to draw a simple random sample of 30 players from the entire population of 1034 players, and to plot their weights against their heights. [The Minitab command is simply "sample 30 C1 C2", where C1 is the column containing a list of all 1034 ID numbers and C2 is where I wanted to put the ID numbers of the 30 sampled players. How would you (did you) do it?]

Here's the plot:

```
        -                     *
 weight -
        -                          *
        -
     250+
        -
        -                          *             *
        -                *         *    *
        -                *              *
     225+                        *
        -                *    *              *
        -       *    *    *         *
        -            *
        -       2         *
     200+     *    *    *         *
        -                *
        -                     *                  *
        -
        -       2
         --------+---------+---------+---------+---------+-----height
               72.0      73.5      75.0      76.5      78.0
```

What do you think about that plot? Is it "linear enough"? (See the section on testing linearity and normality, below.) Guess what the Pearson r is. If you used your software to draw a simple random sample of the same size, does your plot look like mine?

Calculating

I asked Minitab to calculate all five Pearson r's for the above examples (one for the entire population; three for teams 10 and 27; and one for the random sample). Here are the results:

Entire population: r = .532. I'd call that a moderate, positive relationship.

Team 10: r = .280. Low, positive relationship?

Team 27: r = .723 (including the outlier). Strong, positive relationship, but partially attributable to the outlier. The correlation is .667 without the outlier.

Two teams combined: r = .587 (including the outlier). Moderate, positive, even with the outlier. The correlation is .510 without the outlier. Pooling was questionable, but turned out to be not as bad as I thought it would be.
Random sample: r = .439. Low-to-moderate, positive relationship. It's an under-estimate of the correlation for the entire population. It could just as easily have been an over-estimate. That's what chance is all about

Inferring

As I indicated earlier in this chapter, for the entire-population example and for the two-team sub-population example, there is no statistical inference to be made. The correlation is what it is. But for the random-sample example you might want to carry out one or more statistical inferences from the sample data that you know and have, to the population data that you (in real life) would not know and wish you had. Let's see what the various possibilities are.

First, point estimation. If someone put a gun to your head and demanded that you give one number that is your best guess for the correlation between height and weight in the population of 1034 baseball players, what would you say? After plotting my random sample data and calculating the corresponding Pearson r for that sample, I'd say .439, i.e., the sample correlation itself. I would not be very comfortable in so doing, however, for three reasons: (1) my sample of 30 is pretty small; (2) I probably should make a "finite population correction", because the population is not of infinite size and the sample takes a bite (albeit small) out of that population; and (3) I happen to know (and you probably don't) from the mathematical statistics literature that the sample correlation is not necessarily "the best" single estimate of the population correlation. [It all has to do with unbiased estimation, maximum likelihood estimation, Bayesian inference, and other such esoteric matters, so let's not worry about it, since we can see that point estimation is risky business.]

Second, interval estimation. Rather than provide one number that is our best single guess, how about a range of numbers that might "capture" the population correlation? [We could always proclaim that the population correlation is between -1 and +1, but that's the entire range of numbers that any Pearson r can take on, so that would not be very informative.] It turns out that interval estimation, via so-called confidence intervals, is the usually preferred approach to statistical inference. Here's how it works.

You must first decide how confident you would like to be when you make your inference. This is always "researcher's choice", but is conventionally taken to be 95% or 99%, with the former percentage the more common. (The only way you can be 100% confident is to estimate that the correlation is between -1 and +1, but as already indicated that doesn't narrow things down at all.)

Next you must determine the amount of sampling error that is associated with a Pearson r when drawing a simple random sample of a certain size from a population. There are all sorts of formulas for the sampling error, but if you can assume that there is a normal bivariate (elliptical) distribution of X and Y in the population from which the sample has been drawn [more about this later], and you want your computer to do the work for you, all you need to do is tell your software program what your sample correlation and your sample size are and it will give you the confidence interval automatically. My favorite source is Richard Lowry's VassarStats website. I gave his interval estimation routine 95% confidence, my .439 correlation, and my "n" of 30, and it returned the numbers .094 and .690. I can therefore be approximately 95% confident that the interval from .094 to .690 captures the population correlation. Since I have the full population data (but wouldn't in real life) I see that my interval does capture the population correlation (of .532). But it might not have. All I now know is that in the long run 95% of intervals constructed in this way will do so and 5% will not.

Third, there is hypothesis testing, which (alas) is the most common type of statistical inference. On the basis of theory and/or previous research I could hypothesize (guess) that the correlation between height and weight in the population is some number, call it ρ (the Greek rho), which is the population counterpart to the sample Roman letter r. Suppose I claim that there is absolutely no (zero) linear relationship between the heights and the weights of Major League baseball players, because I theorize that there should be just as many short and heavy players, and tall and light players, as there are short and light and tall and heavy ones. I would therefore like to test the hypothesis that ρ is equal to zero (the so-called "null" hypothesis). Or, suppose that Smith and Jones had carried out a study of the heights and the weights of adults in general and found that the Pearson correlation between height and weight was .650. I might claim that the relationship should be the same for baseball players as it is for adults in general, so I would like to test the hypothesis that ρ is equal to .650. Let's see how both tests would work:

Hypothesis 1: ρ = 0, given that r = .439 for n =30

The test depends upon the probability that I would get a sample r of .439 or more when the population ρ = 0. If the probability is high, I can't reject Hypothesis 1; if it's low (say, .05 or thereabouts...the usual conventional choice), I can. Just as in the interval estimation approach (see above) I need to know what the sampling error is in order to determine that probability. Once again there are all sorts of formulas and associated tables for calculating the sampling error and, subsequently, the desired probability, but fortunately we can rely on Richard Lowry and others who have done the necessary work for us. I gave Lowry's software my r and my n, and a probability (p) of .014 was returned. Since that probability is less than .05 I can reject the hypothesis that ρ = 0. (I might be wrong, however. If so, I'm said to have made what the statisticians call a Type I Error: rejecting a true hypothesis.)

[<u>Important aside</u>: There is an interesting connection between interval estimation and hypothesis testing that is especially relevant for Pearson r's. If you determine the 95% confidence interval for ρ and that interval does not include 0, then you are justified in rejecting the hypothesis that ρ = 0. For the example just completed, the 95% confidence interval around .439 was found to go from .094 to .690, and that interval does not include 0, so once again I can reject Hypothesis 1, indirectly. (I can also reject any other values that are not in that interval.) The .439 is said to be "significant at the 5% level" (5% is the complement of 95% and I got a "p-value" less than .05.]

Hypothesis 2: ρ =..650, given that r = .439 for n =30

The logic here is similar. If the probability is high of getting a sample r of .439 (or anything more discrepant) when the population ρ = .650, I can't reject Hypothesis 2; if the probability is low, I can. But the mathematics gets a little heavier here, so I will appeal to the preceding "Important aside" section and claim that I cannot reject Hypothesis 2 because .650 is within my 95% confidence interval. (I might be wrong again, for the opposite reason, and would be said to have made a Type II Error: Not rejecting a false hypothesis.)

Since I know that ρ is .532...but wouldn't know that in real life (I can't stress that too often), I "should have" rejected both Hypothesis 1 and Hypothesis 2.

<u>Rank correlations and non-parametric inference</u>

In the Inferring section (see above) I said that certain things (e.g., the use of Richard Lowry's routine for determining confidence intervals) follow if you can assume that the bivariate distribution of X and Y in the population is normal.

What if you know it isn't or you are unwilling to assume that it is? In that case you can rank-order the X's, rank-order the corresponding Y's, and get the correlation between the ranks rather than the actual measures. There are several kinds of rank correlations, but the most common one is the Spearman rank correlation, call it r_S (pronounced "r-sub-S"), where the S stands for Charles Spearman, the British psychologist who derived it. It turns out that Spearman's r_S is identical to Pearson's r for the ranked data.

Consider as an example the height (X) and weight (Y) data for Team 27. Here are the actual heights, the actual weights, the ranks for the heights, and the ranks for the weights (if two or more people have the same height or the same weight, they are assigned the mean of the ranks for which they are tied):

ID	X	Y	Xrank	Yrank
1	73	196	12.0	17.0
2	73	180	12.0	9.0
3	76	230	31.0	31.5
4	75	224	24.0	30.0
5	70	160	1.5	2.0
6	73	178	12.0	7.0
7	72	205	6.5	22.5
8	73	185	12.0	11.5
9	75	210	24.0	26.0
10	74	180	17.5	9.0
11	73	190	12.0	15.0
12	73	200	12.0	19.5
13	76	257	31.0	35.0
14	73	190	12.0	15.0
15	75	220	24.0	29.0
16	70	165	1.5	3.0
17	77	205	34.5	22.5
18	72	200	6.5	19.5
19	77	208	34.5	24.0
20	74	185	17.5	11.5
21	75	215	24.0	28.0
22	75	170	24.0	5.0
23	75	235	24.0	33.0
24	75	210	24.0	26.0
25	72	170	6.5	5.0
26	74	180	17.5	9.0
27	71	170	3.5	5.0
28	76	190	31.0	15.0
29	71	150	3.5	1.0
30	75	230	24.0	31.5
31	76	203	31.0	21.0

32	83	260	36.0	36.0	[the "outlier"]
33	75	246	24.0	34.0	
34	74	186	17.5	13.0	
35	76	210	31.0	26.0	
36	72	198	6.5	18.0	

As indicated above, the Pearson r for the actual heights and weights is .723. The Pearson r for the ranked heights and weights (the Spearman r_S) is .707. [Thank you, Minitab, for doing the ranking and the correlating for me.]

Had this been a random sample (which it is not...it is a sub-population) you might have wanted to make a statistical inference from the sample to the population from which the sample had been randomly drawn. The procedures for so doing are similar to the procedures for ordinary Pearson r and are referred to as "non-parametric". The word "non-parametric" derives from the root word "parameter" that always refers to a population. If you cannot assume that there is a normal bivariate distribution of X and Y in the population whose principal parameter is the population correlation, "non-parametric" inference is called for.

Testing for linearity and normality

If you're really compulsive and can't judge the linearity and normality of the relationship between X and Y by visual inspection of the X,Y plot, you might want to carry out formal tests for both. Such tests are available in the literature, but it would take us too far afield (bad pun) to go into them. And if your data "fails" either or both of the tests, there are data transformations that make the relationship "more linear" or "more normal".

Regression

Closely related to the Pearson correlation between two variables is the regression of one of the variables on the other, for predictive purposes. Some people can't say "correlation" without saying "regression". [Some of those same people can't say "reliability" without saying "validity".] In this section I want to point out the connection between correlation and regression, using as an example the data for my simple random sample of 30 players drawn from the entire population of 1034 players.

Suppose that you were interested not only in estimating the direction and the degree of linear relationship between their heights (X) and their weights (Y), but were also interested in using their data to predict weight from height for other players. You would start out just like we already have, namely plotting the data, calculating the Pearson r, and making a statistical inference, in the form of an estimation or a hypothesis test, from the sample r to the population ρ. But then the focus switches to the determination of the line that "best fits" the sample plot.

This line is called the Y-on-X regression line, with Y as the dependent variable (the predictand) and X as the independent variable (the predictor). [There is also the X-on-Y regression line, but we're not interested in predicting height from weight.] The reason for the word "regression" will be explained soon.

I gave Minitab the command "regr c4 1 c3", asking for a regression analysis with the data for the dependent variable (Y) in column 4 and with the corresponding data for the one independent variable (X) in column 3. Here is what it (Minitab) gave me:

```
The regression equation is
 weight = - 111 + 4.39 height

 Predictor     Coef      Stdev    t-ratio
 Constant     -111.2     126.5     -0.88
 height        4.393     1.698      2.59

 s = 19.61     R-sq = 19.3%    R-sq(adj) = 16.4%

 Analysis of Variance

 SOURCE       DF            SS          MS
 Regression    1        2576.6      2576.6
 Error        28       10772.1       384.7
 Total        29       13348.7

 Unusual Observations
 Obs.  height   weight     Fit  Stdev.Fit  Residual   St.Resid
   6    79.0   190.00   235.87      8.44    -45.87     -2.59R
  27    75.0   270.00   218.30      3.68     51.70      2.68R
```

Let's take that output one piece at a time.

The first and most important finding is the equation of the best-fitting regression line. It is of the form Y' = a + bX, where a is the Y intercept and b is the slope. [You might remember that from high school algebra. Y' is the "fitted Y", not the actual Y.] If you want to predict a player's weight from his height, just plug in his height in inches and you'll get his predicted weight in pounds. For example, consider a player who is 6 feet 2 inches (= 74 inches) tall. His predicted weight is -111 + 4.39 (74) = 214 pounds. Do you know that will be his exact weight? Of course not; it is merely approximately equal to the average of the weights for the six players in the sample who are approximately 74 inches tall. (See the plot, above.) But it is a heck of a lot better than not knowing his height.

The next section of the output provides some clarifying information (e.g., that the intercept is actually -111.2 and the slope is 4.393) and the first collection of inferential statistics. The intercept is not statistically significantly different from zero (trust me that a t-ratio of -0.88 produces a p-value greater than .05), but the slope is (big t, small p; trust me again). The intermediate column (Stdev) is the so-called "standard error" for the intercept and the slope, respectively. When combined with the coefficient itself, it produces the t-ratio, which in turn produces the [unindicated, but less than .05] p-value. Got that?

The third section provides a different kind of standard error (see below)...the s of 19.61; the squared correlation in percentage form (check that .193 is the square of .439); and the "adjusted" squared correlation of 16.4%. The squared correlation needs to be adjusted because you are trying to fit a regression line for two variables and only 30 points. [Think what would happen if you had just two points. Two points determine a straight line, so the line would fit perfectly but unsatisfactorily.] The square root of the adjusted squared correlation, which is equal to .405, might very well provide "the best" point estimate of the population correlation (see above).

The "Analysis of variance" section re-inforces (actually duplicates) the information in the previous two sections and would take us too far afield (same bad pun) with unnecessary jargon, so let's ignore that for the time being. [Notice, however, that if you divide the MS for Regression by the MS for Error, you get the square of the t-ratio for the slope. That is not accidental. What is accidental is the similarity of the correlation of .439 to the slope of 4.393. The slope is equal to the correlation multiplied by the ratio of the standard deviation of Y to the standard deviation of X (neither is indicated, but trust me!), which just happens to be about 10.]

The last section is very interesting. Minitab has identified two points that are pretty far off the best-fitting regression line, one above the line (Obs. 27) and one below the line (Obs. 6), if you think they should be deleted. [I don't.]

Going back to predicting weight from height for a player who is 74 inches tall: The predicted weight was 214 pounds, but that prediction is not perfect. How good is it? If you take the 214 pounds and lay off the standard error (the second one) of 19.61 a couple of times on the high side and a couple of times on the low side you get a 95% confidence interval (yes, a different one) that ranges from 214 - 2(19.61) to 214 + 2(19.61), i.e., from 175 pounds to 253 pounds. That doesn't narrow things down very much (the range of weights in the entire population is from 150 pounds to 290 pounds), but the prediction has been based on a very small sample.

Why the term "regression"? The prediction just carried out illustrates the reason. That player's height of 74 inches is below the mean for all 30 players (you can see that from the plot). His predicted weight of 214 pounds is also below the mean weight (you can see that also), but it is closer to the mean weight than his height is to the mean height (how's that for a mouthful?), so his predicted weight is "regressed" toward the mean weight. The matter of "regression to the mean" comes up a lot in research in general. For example, in using a pre-experimental design involving a single group of people (no control group) measured twice, once before and once after the intervention, if that group's performance on the pretest is very low compared to the mean of a larger group of which it is apart, its performance on the posttest will usually be closer to the posttest mean than it was to the pretest mean. [It essentially has nowhere to go but up, and that is likely to be mis-interpreted as a treatment effect, whereas it is almost entirely attributable to the shape of the plot, which is a function of the less-than-perfect correlation between pretest and posttest. Think about it!]

Sample size

For my random-sample example I chose to take a sample of 30 players. Why 30? Why indeed? What size sample should I have I taken? Believe it or not, sample size is arguably the most important consideration in all of inferential statistics (but you wouldn't know it from actual practice, where many researchers decide on sample sizes willy-nilly and often not random sample sizes at that.)

Briefly put, the appropriate sample size depends upon how far wrong you're willing to be when you make a statistical inference from a sample to the population from which the sample has been drawn. If you can afford to be wrong by a lot, a small sample will suffice. If you insist on never being wrong you must sample the entire population. The problem becomes one of determining what size sample is tolerably small but not so small that you might not learn very much from it, yet not so large that it might approximate the size of the entire population. [Think of the appropriate sample size as a "Goldilocks" sample size.] So, what should you do?

It depends upon whether you want to use interval estimation or hypothesis testing. For interval estimation there are formulas and tables available for determining the appropriate sample size for a given tolerable width for the confidence interval. For hypothesis testing there are similar formulas and tables for determining the appropriate sample size for tolerable probabilities of making Type I Errors and Type II Errors. You can look them up, as Casey Stengel used to say. [If you don't know who Casey Stengel is, you can look that up also!]

Multiple regression

I haven't said very much about age. The correlation between height and weight for the entire population is .532. Could that correlation be improved if we took into account the players' ages? (It really can't be worse even if age doesn't correlate very highly with weight; its actual correlation with weight for these data is only .158, and its correlation with height is -.074...a negative, though even smaller, correlation.) I went back to the full data and gave Minitab the command "regr c4 2 c3 c5", where the weights are in Column 4, the heights are in Column 3, and the ages are in Column 5. Here is what it returned (in part):

```
The regression equation is
 weight = - 193 + 0.965 age + 4.97 height

 1033 cases used 1 cases contain missing values

 Predictor       Coef        Stdev      t-ratio
 Constant    -192.66        17.89      -10.77
 age            .9647       .1249        7.72
 height       4.9746        .2341       21.25

 s = 17.30      R-sq = 32.2%     R-sq(adj) = 32.1%
```

We can ignore everything but the regression equation (which, for those of you who are mathematically inclined, is the equation of a plane, not a line), the s, and the R-sq, because we have full-population data. Taking the square root of the R-sq of .322 we get an R of .567, which is higher than the r of .532 that we got for height alone, but not much higher. [It turns out that R is the Pearson r correlation between Y and Y'. Nice, huh?] We can also use the new regression equation to predict weight from height and age, with a slightly smaller standard error of 17.30, but let's not. I think you get the idea.

The reason it's called multiple regression is because there is more than one independent variable.

Summary

That's about it (for now). I've tried to point out some important statistical concepts that can be squeezed out of the baseball data. Please let me know (my e-mail address is tknapp5@juno.com) if you think of others. And by all means (another bad pun) please feel free to ask me any questions about this "module" and/or tell me about all of the things I said that are wrong.

Oh, one more thing: It occurred to me that I never told you how to calculate a Pearson r. In this modern technological age most of us just feed the data into our friendly computer program and ask it to do the calculations. But it's possible that you could find yourself on a desert island some time without your computer and have a desire to calculate a Pearson r. The second appendix that follows should help. (And you might learn a few other things in the process.)

Appendix 1: The raw data for the game of Team A vs. Team B

	Pitch			
	red	white#1	white#2	Result
First inning				
Team A				
A1	1	2	2	Double; A1 reaches second base
A2	6	4	2	A2 is out; A1 still on second
A3	4			Strike 1
	4			Strike 2
	3			Ball 1
	2			Ball 2
	6	4	6	A3 is out; A1 still on second
A4	3			Ball 1
	1	1	4	A4 is out; end of first half of first inning
Team B				
B1	1	3	5	B1 is out.
B2	6	2	2	Double; B2 reaches second base
B3	3			Ball one
	3			Ball two
	2			Ball three
	1	5	5	B3 is out; B2 still on second
B4	4			Strike one
	2			Ball one
	5			Strike two
	1	6	1	B4 reaches first base on error; B2 advances to third base
B5	2			Ball one
	6	1	2	B5 is out; end of first inning; no score
Second inning				
Team A				
A5	3			Ball one
	2			Ball two
	3			Ball three
	3			Ball four; A5 awarded first base (That's called a "walk".)
A6	2			Ball one
	2			Ball two
	1	2	1	A6 is out; A5 is still on first

A7	5			Strike one
	5			Strike two
	6	3	1	A7 is out; A5 is still on first
A8	5			Strike one
	5			Strike two
	3			Ball one
	6	6	5	Single; A8 reaches first base; A5 advances to second base
A9	1	5	2	Single; A9 reaches first base; A8 advances to second, A5 to third
A1	5			Strike one
	4			Strike two
	1	6	3	A1 is out; end of first half of second inning; no score

Team B

B6	3			Ball one
	6	2	6	B6 is out
B7	2			Ball one
	3			Ball two
	3			Ball three
	6	4	1	B7 is out
B8	1	1	4	B8 is out; end of second inning; still no score

Third inning

Team A

A2	1	6	4	A2 is out
A3	3			Ball one
	3			Ball two
	5			Strike one
	2			Ball three
	4			Strike 2
	3			Ball four; A3"walks"
A4	4			Strike 1
	4			Strike two
	1	5	5	A4 is out; runner A3 is still on first base
A5	1	3	1	A5 is out; end of first half of third inning; still no score

Team B

B9	1	5	6	Single; B9 reaches first base
B1	4			Strike 1

	6	3	4	B1 is out; B9 is still on first
B2	2			Ball one
	6	4	3	B2 is out; B9 is still on first
B3	3			Ball one
	5			Strike one
	3			Ball two
	3			Ball three
	6	6	3	B3 is out; end of third inning; still no score

Fourth inning

Team A

A6	5			Strike one
	2			Ball one
	2			Ball two
	6	3	5	A6 is out
A7	2			Ball one
	3			Ball two
	4			Strike one
	3			Ball three
	2			Ball four; A7 "walks"
A8	4			Strike one
	1	4	1	A8 is out; A7 is still on first
A9	4			Strike one
	5			Strike two
	6	5	5	A9 is out; end of first half of fourth inning; still no score

Team B

B4	4			Strike one
	3			Ball one
	1	5	3	B4 is out
B5	5			Strike one
	5			Strike two
	5			Strike three; B5 is out
B6	3			Ball one
	2			Ball two
	2			Ball three
	3			Ball four; B6 "walks"
B7	5			Strike one
	2			Ball one
	6	6	1	B7 reaches first base on error; B6 advances to second base
B8	4			Strike one
	2			Ball one

	3			Ball two
	6	3	3	Triple; B8 reaches third base; B6 and B7 score; Team B leads 2-0
B9	4			Strike one
	4			Strike two
	5			Strike three; B9 is out; end of fourth inning; Team B leads 2-0

Fifth inning

Team A

A9	3			Ball one
	4			Strike one
	1	4	4	A9 is out
A1	1	6	6	Home run; Team B now leads 2-1
A2	6	5	6	Single; A2 reaches first base
A3	2			Ball one
	4			Strike one
	6	3	1	A3 is out; A2 is still on first
A4	1	2	6	A4 is out; end of first half of fifth inning; Team B still leads 2-1

Team B

B1	4			Strike one
	1	1	2	B1 is out
B2	1	4	1	B2 is out
B3	3			Ball one
	5			Strike one
	6	2	1	B3 is out; end of fifth inning; Team B still leads 2-1

Sixth inning

Team A

A5	1	1	3	A5 is out
A6	5			Strike one
	6	2	1	A6 is out
A7	3			Ball one
	2			Ball two
	4			Strike one
	2			Ball three
	1	1	2	A7 is out; end of first half of sixth inning; Team B still leads 2-1

Team B

B4	1	1	5	B4 is out
B5	4			Strike one
	2			Ball one
	1	4	5	B5 is out
B6	6	3	5	B6 is out; end of sixth inning; Team B still leads 2-1

Seventh inning

Team A

A8	3			Ball one
	4			Strike one
	2			Ball two
	6	2	3	Single; A8 reaches first base
A9	1	1	2	A9 is out; A8 is still on first
A1	4			Strike one
	4			Strike two
	1	6	2	A1 is out; A8 is still on first
A2	1	5	2	Single; A2 reaches first base; A8 advances to second base
A3	1	5	6	Single; A3 reaches first base; A2 advances to second base, A8 to third base
A4	5			Strike one
	3			Ball one
	1	2	2	Double; A4 reaches second base; A3 advances to third base; A8 and A2 score; Team A now leads 3-2
A5	1	3	6	A5 is out; end of first half of seventh inning; Team A still leads 3-2

Team B

B7	6	2	2	Double; B7 reaches second base
B8	6	4	2	B8 is out; B7 remains on second
B9	1	1	2	B9 is out; B7 remains on second
B1	4			Strike one
	5			Strike two
	3			Ball one
	4			Strike three; B1 is out; end of seventh inning; Team A still leads 3-2

Eighth inning

Team A

A6	4			Strike one
	1	4	4	A6 is out
A7	4			Strike one
	2			Ball one
	6	5	5	A7 is out
A8	2			Ball one
	4			Strike one
	4			Strike two
	6	4	6	Batter is out; end of first half of eighth inning; Team A still leads 3-2
Team B				
B2	6	6	1	Error; B2 reaches first base
B3	5			Strike one
	1	1	5	B3 is out; B2 remains on first
B4	6	6	3	B4 is out; B2 remains on first
B5	1	4	1	B5 is out; end of eighth inning; Team A still leads 3-2
Ninth inning				
Team A				
A9	5			Strike one
	1	1	6	Error; A9 reaches first base
A1	1	5	3	A1 is out; A9 remains on first
A2	5			Strike one
	5			Strike two
	3			Ball one
	4			Strike three; A2 is out; A9 remains on first
A3	1	3	2	Single; A3 reaches first base; A9 advances to second base
A4	4			Strike one
	6	4	5	A4 is out; end of first half of ninth inning; Team A still leads 3-2
Team B				
B6	4			Strike one
	3			Ball one
	3			Ball two
	5			Strike two
	5			Strike three; B6 is out
B7	3			Ball one
	2			Ball two
	4			Strike one

	4			Strike two
	2			Ball three
	5			Strike three; B7 is out
B8	6	3	4	B8 is out; end of game; Team A wins 3-2

Appendix 2: [My thanks to Joe Rodgers for the great article he co-authored with Alan Nicewander, entitled "Thirteen ways to look at the correlation coefficient", and published in The American Statistician, 1988, volume 42, number 1, pages 59-66. I have included some of those ways in this appendix.]

Here are several mathematically equivalent formulas for the Pearson r (actually ρ, since these formulas are for population data):

1. $$\rho = \frac{\sum z_X z_Y}{n}$$

This is the best way to "think about" Pearson r. It is the average (mean) product of standardized variable X and standardized variable Y. A standardized variable z , e.g., z_X , is equal to the raw variable minus the mean of the variable, divided by the standard deviation of the variable, i.e., $z_X = (X - \mu_X)/\sigma_X$. This formula for r also reflects the product-moment feature (the product is of the z's; a moment is a mean). Since X and Y are usually not on the same scale, what we care about is the <u>relative</u> relationship between X and Y, not the absolute relationship. It is not a very computationally efficient way of calculating r, however, since it involves all of those intermediate calculations that can lead to round-off errors.

2. $\rho = 1 - 1/2$ [variance of $(z_Y - z_X)$]

This a variation of the previous formula, involving the difference between "scores" on the standardized variables rather than their product. If there are small differences, the variance of those differences is small and the r is close to +1. If the differences are large (with many even being of opposite sign), the variance is large and the r is close to -1.

3. $$\rho = \frac{N\sum XY - (\sum X)(\sum Y)}{\sqrt{[N\sum X^2 - (\sum X)^2]\,[N\sum Y^2 - (\sum Y)^2]}}$$

(N.B.: the square root is taken of the product of the bracketed terms in the denominator)

This formula looks much more complicated (and it is, in a way), but involves only the number of observations, the actual X and Y data, and their squares. In "the good old days" before computers, I remember well entering the X's in the left end of the keyboard of a Monroe or Marchant calculator, entering the Y's in the right end, pushing a couple of buttons, and getting $\sum X$, $\sum Y$, $\sum X^2$, $\sum Y^2$, and $2\sum XY$ in the output register all in one fell swoop! [That was quite an accomplishment then.]

4. ρ = cosine (θ), where θ is the angle between a vector for the X variable and a vector for the Y variable in the n-dimensional "person space" rather than the two-dimensional "variable space". [If you don't know anything about trigonometry or multi-dimensional space, that will mean absolutely nothing to you.]

5. If you really want a mathematical challenge, try using the formula in the following excerpt from an article I wrote about 25 years ago (in the Journal of Educational Statistics, 1979, volume 4, number 1, pages 41-58). You'll probably have to read a lot of that article in order to figure out what a gsm is, what all of those crazy symbols are, etc.; but as I said, it's a challenge!

rather than the more familiar correlation. It is true that the <u>population</u> correlation coefficient can be expressed in terms of gsm's, as follows:

$$\begin{aligned}\rho = {} & N(N-1)\left(\begin{bmatrix}11\\00\end{bmatrix} - \begin{bmatrix}10\\01\end{bmatrix}\right)\Bigg\{N(N-1)^2\begin{bmatrix}22\\00\end{bmatrix} + 2N(N-1)\begin{bmatrix}11\\11\end{bmatrix} \\ & + N(N-1)^3\begin{bmatrix}20\\02\end{bmatrix} - 2N(N-1)^2(N-2)\begin{bmatrix}20\\01\\01\end{bmatrix} + 4N(N-1)(N-2)\begin{bmatrix}11\\10\\01\end{bmatrix} \\ & + N(N-1)(N-2)(N-3)\begin{bmatrix}10\\10\\01\\01\end{bmatrix} - 4N(N-1)^2\begin{bmatrix}21\\01\end{bmatrix}\Bigg\}^{-1/2}\end{aligned} \qquad (10)$$

But ρ is a nonlinear function of the gsm's, so the corresponding function of δX is not unbiasedness-preserving. It therefore doesn't help to use the incidence sampling and gsm approach to statistical inference when the sample gsm's are unbiased estimates of the population gsm's but the statistic of interest (in this case the <u>sample</u> correlation coefficient) is not an unbiased estimate of the relevant parameter.

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CHAPTER 31: LEARNING STATISTICS THROUGH FINITE POPULATIONS AND SAMPLING WITHOUT REPLACEMENT

Introduction

Just about every statistics course and just about every statistics textbook concentrates on infinite populations and sampling with replacement, with particular emphasis on the normal distribution. This chapter is concerned solely with finite populations and sampling without replacement, with only a passing reference to the normal distribution.

Are populations infinite or finite? Real world populations are all finite, no matter how small or how large. How do we draw samples? With replacement or without replacement? Real world samples are all drawn without replacement. It would be silly to draw some observations once and some more than once. Ergo, let's talk about finite populations and sampling from them without replacement.

Two examples

1. On his website, statistician Robert W. Hayden gives the artificial example of a population of six observations from which all possible samples of size three are to be drawn. He claims that many basic statistical concepts can be learned from discussing this example. I agree.

Consider one of the cases Hayden talks about: A population consisting of the observations 3,6,6,9,12, and 15.

a. It has a frequency distribution. Here it is

Observation	frequency
3	1
6	2
9	1
12	1
15	1

b. It has a mean of (3+6+6+9+12+15)/6 = 51/6 = 8.50

c. It has a median of 7.50 (if we "split the difference" between the middle two values, which is OK if the scale is interval or ratio...but see Chapter 17).

d. It has a mode of 6 (there are more 6's than anything else).

e. It has a range of 15-3 = 12.

f. It has a variance of $[(3-8.5)^2 + 2(6-8.5)^2 + (9-8.5)^2 + (12-8.5)^2 + (15-8.5)^2]/6$ = 97.50/6 = 16.25. Curiously, Hayden divides the sum of the squared differences from the mean by 5...one less than the number of observations, rather than the number of observations. Many people divide the sum of the squared differences of the observations in a sample from the sample mean by one less than the number of observations (for a couple of complicated reasons), but Hayden is the only one I know of who calculates a population variance the way he does.

g. It has a standard deviation of $\sqrt{16.25}$ = 4.03

It has some other interesting summary measures, but those should suffice for now.

As indicated above, Hayden considers taking all possible samples of size three from a population of six observations, without replacing an observation once it is drawn. For the 3,6,6,9,12,15 population they are:

3,6,6 (both 6's; they are for different things: people, rats, hospitals...whatever)
3,6,9 (for one of the 6's)
3,6,9 (for the other 6)
3,6,12 (for one of the 6's)
3,6,12,(for the other 6)
3,6,15 (for one of the 6's)
3,6,15 (for the other 6)
3,9,12
3,9,15
3,12,15
6,6,9 (both 6's)
6,6,12 (both 6's)
6,6,15 (both 6's)
6,9,12 (one of the 6's)
6,9,12 (the other 6)
6,9,15 (one of the 6's)
6,9,15 (the other 6)
6,12,15 (one of the 6's)
6,12,15 (the other 6)
9,12 15

Therefore there are 20 such samples.

Suppose you would like to estimate the mean of that population by using one of those samples. The population mean (see above) is 8.50.

The first sample (3,6,6) would produce a sample mean of 5 (an under-estimate). The second and third samples (3,6,9) would produce a sample mean of 6 (also an under-estimate).

The fourth and fifth samples (3,6,12) would produce a sample mean of 7 (still an under-estimate).
The sixth and seventh samples (3,6,15) would produce a sample mean of 8 (an over-estimate).
The eighth sample (3,9,12) would also produce a sample mean of 8 (an over-estimate).
The ninth sample (3,9,15) would produce a sample mean of 9 (another over-estimate).
The tenth sample (3,12,15) would produce a sample mean of 10 (still another over-estimate).
The eleventh sample (6,6,9) would produce a sample mean of 7 (an under-estimate).
The twelfth sample (6,6,12) would produce a sample mean of 8 (an over-estimate).
The thirteenth sample (6,6,15) would produce a sample mean of 9 (an over-estimate) .
The fourteenth and fifteenth samples (6,9.12) would produce a sample mean of 9 (an over-estimate).
The sixteenth and seventeenth samples (6,9,15) would produce a sample mean of 10 (an over-estimate).
The eighteenth and nineteenth samples (6,12,15) would produce a sample mean of 11 (an over-estimate).
The twentieth sample mean (9,12,15) would produce a sample mean of 12 (an over-estimate).

The possible sample means are 5,6,6,7,7,7,8,8,8,8,9,9,9,9,10,10,10,11,11, and 12. The frequency distribution of those means is the sampling distribution for samples of size three taken from the 3,6,6,9,12,15 population. Here it is:

Sample mean	Frequency
5	1
6	2
7	3
8	4
9	4
10	3
11	2
12	1

Ten of them are under-estimates, by various amounts; ten of them are over-estimates, also by various amounts. But the mean of those means (do you follow that?) is 8.50 (the population mean). Nice, huh? But the problem is that in real life if you have just one of those samples (the usual case) you could be lucky and come close to the population mean or you could be 'way off. That's what sampling is all about.

I could say a great deal more about this example but I'm eager to move on to another example.

2. One of the most interesting (to me, anyhow) populations is the 50 states of the United Sates. [I have several examples of the use of this population in my Learning statistics through playing cards book (Knapp, 1996).]

Here are some data for that population:

state	admrank	nhabrank	arearank
DE	1	46	49
PA	2	6	32
NJ	3	9	46
GA	4	10	21
CT	5	30	48
MA	6	13	45
MD	7	19	42
SC	8	26	40
NH	9	42	44
VA	10	12	37
NY	11	3	30
NC	12	11	29
RI	13	44	50
VT	14	49	43
KY	15	25	36
TN	16	16	34
OH	17	7	35
LA	18	22	33
IN	19	14	38
MS	20	32	31
IL	21	5	24
AL	22	23	28
ME	23	41	39
MO	24	17	18
AR	25	34	27
MI	26	8	22
FL	27	4	26
TX	28	2	2
IA	29	31	23
WI	30	18	25
CA	31	1	3
MN	32	21	14
OR	33	29	10
KS	34	33	13
WV	35	38	41

NV	36	36	7
NE	37	39	15
CO	38	24	8
ND	39	48	17
SD	40	47	16
MT	41	45	4
WA	42	15	20
ID	43	40	11
WY	44	50	9
UT	45	35	12
OK	46	28	19
NM	47	37	5
AZ	48	20	6
AK	49	49	1
HI	50	43	47

where:

1. state is the two-letter abbreviation for each of the 50 states
2. admrank is the rank-order of their admission to the union (Delaware was first, Pennsylvania was second,...,Hawaii was fiftieth).
3. nhabrank is the rank-order of number of inhabitants, according to the 2000 census (California was first, Texas was second,...,Wyoming was fiftieth).
4. arearank is the rank-order of land area (Alaska is first, Texas is second,..., Rhode Island is fiftieth).

Of considerable interest (at least to me) is the relationship between pairs of those variables (admission to the union and number of inhabitants; admission to the union and land area; and number of inhabitants and land area). I (and I hope you) do not care about the means, variances, or standard deviations of those variables. (Hint: If you do care about such things for this example, you will find that they're the same for all three variables.)

The relationships (something called Spearman's rank correlations) for the entire population are as follows (the correlation can go from -1 through 0 to +1, where the negative correlations are indicative of inverse relationships and the positive correlations are indicative of direct relationships):

+.394 for admrank and nhabrank
-.720 for admrank and arearank
-.013 for nhabrank and arearank

The relationship between the rank-order of admission to the union and the rank-order of number of inhabitants is direct but modest; the relationship between the rank-order of admission to the union and the rank-order of land area is inverse and rather strong; and the relationship between the rank-order of number of inhabitants and the rank-order of land area is essentially zero. Those all make sense, if you think about it and if you call upon your knowledge of American history!

But what happens if you take samples from this population? I won't go through all possible samples of all possible sizes, but let's see what happens if you take, say, ten samples of ten observations each. And let's choose those samples randomly.

I got on the internet and used something called the Research Randomizer. The numbers of the states that I drew for each of those sets of samples were as follows (sampling within sample was without replacement, but sampling between samples was with replacement; otherwise I would run out of states to sample after taking five samples!):

Set 1	Set 2	Set 3	Set 4	Set 5	Set 6	Set 7	Set 8	Set 9	Set 10
1	6	2	1	1	2	2	2	10	12
5	11	7	7	12	3	6	12	20	15
9	21	11	8	19	10	11	17	22	23
17	23	13	10	20	13	14	21	28	24
21	26	23	13	36	17	26	27	29	35
23	27	29	18	39	20	28	34	31	36
25	34	31	19	43	27	30	38	32	37
29	36	34	27	46	35	44	43	42	39
33	39	40	40	48	41	46	48	43	45
48	40	48	46	49	45	50	50	47	47

For the first set (DE,CT,NH,OH,IL,ME,AR,IA,OR,and AZ) the sample data are (the numbers in parentheses are the ranks for the ranks; you need those because all ranks must go from 1 to the number of things being ranked, in this case 10):

state	admrank	nhabrank	arearank
DE	1(1)	46(10)	49(10)
CT	5(2)	30(5)	48(9)
NH	9(3)	42(9)	44(8)
OH	17(4)	7(2)	35(7)
IL	21(5)	5(1)	24(4)
ME	23(6)	41(8)	39(6)
AR	25(7)	34(7)	27(5)
IA	29(8)	31(6)	23(3)
OR	33(9)	29(4)	10(2)
AZ	48(10)	20(3)	6(1)

The rank correlations are (using the ranks of the ranks):

-.382 for admrank and nhabrank

-.939 for admrank and arearank

+.612 for nhabrank and arearank

The -.382 is a poor estimate of the corresponding population rank correlation (and is actually of opposite sign). The -.939 isn't too bad (both are negative and high). The +.612 is terrible (the population rank correlation is approximately zero).

If you have easy access to a statistics package that includes the calculation of Spearman's rank correlation, why don't you take a crack at Set 2 and see what you get.

Other samples of other sizes would produce other rank correlations; and don't be surprised if the sample correlations are quite different from their population counterparts. Small samples take small "bites" out of a population of size 50.

But where are the formulas?

In the typical introductory course in statistics you are bombarded by all sorts of complicated formulas. Case in point: The formula for the population standard deviation is $\sqrt{} \sum (X - \mu)^2/N$; and the formula for the standard error of the mean is $\sigma/\sqrt{}n$ (for sampling with replacement). I could present similar formulas for finite populations (and for sampling without replacement), but I won't. They are similar to those (some are actually more complicated) and all involve the so-called "finite population correction" (fpc), about which I'll have more to say in the next section.

The difference between two percentages

I would like to close this paper with a discussion of one of the most common problems in statistics in general, viz., the estimation of, or the testing of a hypothesis about, the difference between two percentages. Some examples are: What is the difference between the percentage of men who are Chief Executive Officers (CEOs) and the percentage of women who are Chief Executive Officers? What is the difference in the percentage of smokers who get lung cancer and the percentage of non-smokers who get lung cancer? What is the difference in the recovery percentage of patients who are given a particular drug and the recovery percentage of patients who are given a placebo?

Krishnamoorthy and Thomson (2002) give the artificial but realistic quality control example of the percentages of non-acceptable cans produced by two canning

machines. (They actually do everything in terms of proportions, but I prefer percentages. They are easily convertible from one to the other: multiply a proportion by 100; divide a percentage by 100.) "Non-acceptable" is defined as containing less than 95% of the purported weight on the can. Each machine produces 250 cans. One machine is expected to have an approximate 6% non-acceptable rate and the other machine is expected to have an approximate 2% non-acceptable rate. A sample of is to be drawn from each machine. The authors provide tables for determining the appropriate sample size for each machine, depending upon the tolerance for Type I errors (rejecting a true hypothesis) and Type II errors (not rejecting a false hypothesis). For their specifications the appropriate sample size was 136 cans from each machine for what they called "the Z test", which was one of three tests discussed in their article and for which the normal sampling distribution is relevant. (The mathematics in their article is not for the faint of heart, so read around it.)
Their formulas involved the finite population correction, which subtracts one from both the population size and the sample size, as well as subtracts the sample size from the population size. That strikes me as backwards. Shouldn't we always start with finite populations and sampling without replacement, and then make corrections for (actually extensions to) infinite populations and sampling with replacement?

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CHAPTER 32: BIAS

The word "bias" means different things to different people. The Free Online Dictionary lists seven meanings for bias as a noun, one as an adjective, and two as a transitive verb. Porta's (2008) epidemiology dictionary has 15 entries for the term. Bethel Powers and I (2011) provided several meanings that bias has in quantitative nursing research and in qualitative nursing research. Many of the meanings, e.g., the use of the term in sewing (a diagonal cut on a piece of cloth), have nothing to do with scientific research. In what follows I shall concentrate on several kinds of bias that can arise in research design, instrumentation, and data analysis.

Biased estimator

The easiest context of the term to deal with is the inferential statistical matter of a biased estimator. A sample statistic is said to be a biased estimator of a population parameter if the arithmetic mean of its sampling distribution is not equal to that parameter. Under the typical assumptions of inferential statistics, the sample variance with the sample size n in the denominator is a biased estimator of the population variance. (The statistic with n-1, rather than n, in the denominator is an unbiased estimator.) But that is not to say that division by n is necessarily bad. Division by n is the way we usually get a mean; and division by n also produces a maximum likelihood estimator of the population variance. (The statistic itself is an estimator; the number so calculated is an estimate.)

Measurement bias

Measurement bias occurs when a measuring instrument is found to produce "scores" that differ substantially from subgroup to subgroup. For example, a particular instrument for measuring blood pressure that yields systematically higher systolic readings for women than it does for men is said to be biased, if it is known that the two sexes have equal systolic pressures.

But it is in the social sciences, not the physical or biological sciences, in which measurement bias is most often found. The principal context is the measurement of intelligence. Test A might be said to be biased against women; Test B might be said to be biased against Blacks; etc. There was an interesting "debate" on the internet (Whiting & Ford, n.d.) during which it was claimed that the factor structure underlying an intelligence test should even be the same for all subgroups of people to whom it is administered; otherwise the test is biased.

Publication bias

This is the biggie. The literature is replete with articles whose authors claim (sometimes with empirical evidence, sometimes without) that there is a bias on the part of reviewers and editors to prefer to include in their journals manuscripts for studies in which there is at least one statistically significant finding. A statistical significance test can result in a correct decision to reject a false null hypothesis, a Type I error (the rejection of a true null hypothesis), a correct decision to not reject a true null hypothesis, or a Type II error (the non-rejection of a false null hypothesis). If studies in which a null hypothesis is not rejected are seldom published, an excess of Type I errors over Type II errors is expected.

Biased sample

This is not the same thing as a biased estimator based upon a sample. It's actually much worse. A sample is said to be biased if it is not representative of the population to which inferences are desired to be made. An obvious example is a sample of college students in an introductory psychology course, if inferences are to be made to adults in general. (But you might be surprised to know how often such samples are used for that purpose.)

Some people argue (again sometimes with empirical evidence, sometimes without) that a small sample can never be representative of a large population. They would reject out of hand the typical sample used in a Gallup poll that is used to get a snapshot of the opinions of the American people at a given point in time. Such polls are often based upon samples of a few thousand people, out of approximately 200 million adults, are usually randomly drawn, but often have response rates less than 50 percent.

The term "representative" is itself controversial: Representative with respect to what? It is often not even used by statisticians, who talk only about probability samples (simple random samples or more complex random samples) vs. non-probability samples ("convenience" samples of various kinds). Random samples are representative enough for their work, since chance is a great equalizer and a protector against many biases.

Experimenter bias

In a true experiment (randomized clinical trial) that is not double-blinded, those carrying out the experiment might, consciously or unconsciously, give greater attention to those who receive the experimental treatment; or perhaps the other way 'round (greater attention to the controls). The "treatment effect" could thus be some combination of an actual effect and an attention effect.

Assignment bias

In a quasi-experiment where the assignment to treatment is not random, a researcher might favor certain groups or certain individuals over others. For example, subjects who are sicker might be chosen to receive the experimental treatment, in the hope that they would get better and their lives would be prolonged. Or subjects who are healthier might get preference, in the hope that the experimental treatment might work better for them.

Rater bias

This type of bias can come up in a variety of contexts, ranging from educational situations in which teachers grade their students on performance and/or attitude, to laboratory settings in which researchers rate the interactions between mothers and their children. It is often claimed that teachers favor girls. It is similarly claimed that some researchers find certain mother/child combinations to be "cuter" than others, and thus tend to give them higher ratings.

Contamination bias

If you think about it, all experiments should be run with each person in an isolation booth (just like in the old days of TV quiz shows), so that no person in the control group gets exposed to any feature of the experimental group, and vice versa. In the real world that is usually not feasible, but to the extent that control subjects are free to mingle with experimental subjects, contamination is possible and even likely. This is one of the reasons that the unit of analysis is often shifted from the individual person to the school, the hospital, or whatever.

Non-response bias

Non-response bias plagues almost all of survey research (some people sampled at random might refuse to be part of the sample, for example) but can also be a problem in other types of research (e.g., a randomized clinical trial in which they are told that by chance they might or might not get a particular experimental treatment).

Although it is not usually called non-response bias, there is the associated problem of missing data. Some people might agree to participate in a study but for a variety of reasons they don't provide all of the data that they are asked to provide. It might be background data (e.g., refusal to divulge their ages); it might be data for an important variable in the study itself (e.g., refusal to have blood drawn); or it might be something as simple as the omission of one or more items on an achievement test.

A very interesting dilemma can occur in a randomized clinical trial when some subjects who are randomly assigned to an experimental treatment participate to a limited extent, are switched to another treatment, or drop out of the study altogether. By virtue of the random assignment the treatment groups are comparable at the beginning of the study, but if there is any limited participation, treatment switching, or dropout, that might no longer be the case. When it comes to an analysis of the data, there are two schools of thought. One is the Intent(ion)-to-treat (ITT) approach, in which every subject's data are associated with the treatment to which he(she) was assigned, regardless of how little or how much of that treatment he(she) actually received. The other is the per-protocol (PP) approach, in which the data for all subjects are associated with the treatment they got, which is not necessarily the treatment to which they were assigned. Whichever approach is chosen there will be a missing-data problem. For ITT what are missing are the data that would be otherwise associated with the opposite treatment. For PP some subjects might have to be eliminated entirely from the analysis. See Gross and Cobb (2004) for a good discussion of the biases associated with both of those approaches.

Digit preference bias

People seem to like numbers that end in zero or five (I'm not sure why that is). When asked how old they are, some people who are age 29 or age 32 might say "30", for example. (Demographers call this phenomenon "age heaping".) The result is that age is occasionally not measured as accurately as it could be if such bias didn't exist. (Asking for date of birth would avoid that problem, but might create another one. Some people don't know their dates of birth or don't think about them as often as they think about their ages in years.)

So what should we do about bias?

Reference has already been made to using n-1 rather than n to get an unbiased estimate of a population variance. Well-written manuscripts for well-designed studies should be accepted for publication whether or not statistically significant results are obtained. (I remember once reading about a recommendation that researchers submit only the design aspects of a study for initial publication consideration. If the manuscript is accepted the results are subsequently provided. Nice.) Better training of experimenters, raters, and the like should help in the prevention or minimization of such biases. But for most of the other biases the solutions are more difficult. It could be argued that "bias", in the sense of discrimination of some sort, is inherent in human nature. A woman discriminates against Peter if she chooses to marry Paul (if both are suitors). Some editors and some reviewers don't like certain authors' writing styles (some don't like mine, for example). You can't force people to participate in a study, so non-response will always be a problem. However, there is an ingenious approach called the randomized response technique that has been designed to improve non-response to sensitive questions. See, for example, Campbell & Joiner (1973). Also nice.

Additional reading

For a brief description of each of 32 different kinds of bias that might arise in medical research, see the section entitled "Varieties of bias to guard against" in Indrayan's (2012) textbook that is available via the medicalbiostatistics.com website.

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CHAPTER 33: NUMBERS OF THINGS

Introduction

This chapter is about the number of quantities that are appropriate for different aspects of quantitative research, especially in education and in nursing (the two fields I know best). The topics range from "What size sample should you have?" to "How many reviewers should evaluate a given manuscript?" Each section (there are 20 of them) starts out with a question, an answer is provided, and then one or more reasons are given in defense of that answer. Most of those answers and reasons are based upon the best that there is in the methodological research literature; I have provided two references for each section. You might find some of them to be old, but old is not necessarily bad and is often very good. [I've also sneaked in some of my own personal opinions about the appropriate "n" for various situations.]

The first 15 sections have to do with fairly common statistical matters involving n. The last five sections are concerned with authorship matters (one of my favorite topics).

I could have used N rather than n. Methodologists generally prefer to use N for the number of things in an entire population and n for the number of things in a sample drawn from a population. For the purpose of this chapter I prefer n throughout.

Section I

Question: What size sample should you have?

Answer: It depends

Why?

There are many matters to take into consideration. For example,

1. how many observations you can afford to take with the resources you have.
2. whether you only want to summarize the data in hand or whether you want to make an inference from a sample to a population from which the sample was drawn.
3. how far off you can afford to be IF you want to make a sample-to-population inference.
4. the degree of homogeneity in the population from which the sample was drawn.
5. the reliability and validity of your measuring instrument(s).

Let's take those matters one at a time.

1. Suppose you wanted to determine the relationship between height and weight for the population of third graders in your classroom. [Yes, that is a perfectly defensible population.] Even struggling school districts and financially-strapped school principals can probably afford to purchase a stadiometer/scale combination that is found in just about every doctor's office. (There might already be one in the school nurse's office.) If you have a class of, say, 20 students, and you have a couple of hours to devote to the project, you can have each pupil stand on the scale, lower the rod at the top of the scale onto his(her) head, read off his(her) height and weight, and write them down on a piece of paper. Do that for each pupil, make a scatter diagram for the data, and calculate the Pearson product-moment correlation coefficient, using one of the many formulas for it (see Chapter 19 of this book) or have some sort of computer software do the plotting and the calculating for you. There. End of project. Not very costly. Also of limited interest to anyone other than yourself, those pupils, and perhaps their parents, but that's a separate matter from the determination of the sample size, which in this case is the size of the population itself.

[An aside: Do you think you should make two separate scatter diagrams: one for the boys (suppose there are 10 of them) and one for the girls (suppose there are 10 of them also)? Or would one that includes the data for both boys and girls suffice? Think about that.]

But if you wanted to determine that relationship for the entire school, or the entire school district, or the entire state, etc., unless you had a huge grant of some sort the cost of carrying out the study would undoubtedly be beyond your financial capability, so you would likely resort to sampling (see next paragraph).

2. Rather than using all of the pupils in your third-grade classroom you might want to take a random sample of your elementary school, determine the relationship between height and weight for that sample, and infer that the relationship for the entire school should be about the same, plus or minus some amount. The size of that sample would depend upon how accurate you would like that inference to be (see next paragraph).

3. The accuracy of a sample-to-population inference depends upon the so-called "margin of error", which in turn depends upon the sample size. If you want to be very accurate, you must draw a very large sample. If you don't mind being far off, a very small sample size would suffice, even as few as three people. If you choose only one pupil you can't calculate a Pearson r. If you choose two pupils the correlation must be +1, -1, or indeterminate. [Do you know why?] If you were interested in estimating some other quantity, say the mean height in a population, you might even be able to get away with n = 1 (see next paragraph).

4. If you happen to know that everyone in the population is exactly alike with respect to a particular characteristic, e.g., age in years at time of entering kindergarten, and that is a characteristic in which you are interested, it would not

only be possible to estimate its mean with an n of 1 but it would be wasteful of resources to take any larger sample size than 1. (There is a whole sub-specialty in psychological research called single case analysis where n is always equal to 1, but various analyses are carried out within person.)

5. The more reliable the measuring instrument, the smaller the margin of error, all other things being equal. And the more valid the measuring instrument, the more you are convinced that you're measuring the right quantity.

For a discussion of the justification for sample size in general, see Kelley, Maxwell, and Rausch (2003). For a discussion of "sample size" (number of occasions) for single case analysis, see Kratochwill and Levin (2010).

Section 2

Question: How many observations in a nested design are comparable to how many observations in a non-nested design?

Answer: This many: $mk/[1 + \rho(m-1)]$, where m is the number of observations within cluster, k is the number of clusters, and ρ is the within-cluster correlation.

Why?

It all has to do with the independence (or lack of same) of the observations. In a nested design, e.g., one in which participants are "run" in clusters, the observations for participants within each cluster are likely to be more similar to one another than the observations for participants in different clusters. Using the above formula for a simple case of two clusters with seven observations within each cluster and a within-cluster correlation (a measure of the amount of dependence among the observations) of .50, we get 9.33 (call it 9). That is the "effective sample size" for the nested design, compared to a larger sample size of 2(7)= 14 if the observations for all of the participants had been independent in a non-nested design. If you would carry out a traditional significance test or construct a traditional confidence interval for those data using the n of 14, you would be under-estimating the amount of sampling error and therefore be more likely to make a Type I error or have a confidence interval that is too tight.

For discussions of the general problem and for further examples, see Killip, Mahfoud, and Pearce (2004) and Knapp (2007a).

Section 3

Question: How many treatment groups should you have in a true experiment (randomized controlled trial)?

Answer: Usually just two.

Why?

The focus of most true experiments is on the difference in the effectiveness of two treatments: one experimental, one control. Some people argue that you should have at least three groups: one experimental group, another experimental group, and a placebo group (no treatment at all); or that you should have a factorial design for which the effects of two or more variables could be tested in a single study (see following paragraph). Having three treatment conditions rather than two can get complicated in several respects. First of all, three treatments are at least 1.5 times harder to manage than two treatments. Secondly, the analysis is both more difficult and rather controversial. [Do you have to conduct an over-all comparison and then one or more two-group comparisons? If so, which groups do you compare against which other groups? How do you interpret the results? Do you have to correct for multiple comparisons?] Third, what should the placebo group get(do) while the two experimental groups are receiving their respective treatments?

The article by Green, Liu, and O'Sullivan (2002) nicely summarizes the problems entailed in using factorial designs, but see Freidlin, Korn, Gray, et al. (2008) for a counter-argument to theirs.

Section 4

Question: How many points should a Likert-type attitude scale have?

Answer: It doesn't really matter.

Why?

Several researchers have studied this problem. The consensus of their findings (see, for example, Matell & Jacoby, 1971) is that neither the reliability nor the validity of a measuring instrument is affected very much if you have two (the minimum), five (the usual; also the number that Rensis Likert used in his original 1932 article in the Archives of Psychology), or whatever number of categories for the scale. That is actually counter-intuitive, because you would expect the greater the number of categories, the more sensitive the scale. The more important consideration is the verbal equivalent associated with each of the points. For example, is "sometimes" more often or less often than "occasionally"?

It also doesn't seem to matter whether the number of scale points is odd or even. Some investigators like to use an odd number of scale points so that a respondent can make a neutral choice in the middle of the scale. Others object to that, insisting that each respondent should make a choice on either the agree side or the disagree side, and not be permitted to "cop out".

It is the analysis of the data for Likert-type scales that is the most controversial. Some people (like me) claim that you should not use means, standard deviations, and Pearson r's for such scales. Others see nothing wrong in so doing. The latter position is clearly the predominant one in both the education and the nursing literature. But see Marcus-Roberts and Roberts (1987) for a convincing argument that the predominant position is wrong.

Section 5

Question: How many items should you have on a cognitive test?

Answer: As many as practically feasible.

Why?

The reliability of a test is positively related to the number of items (the greater the number of items, the higher the reliability, all else being equal, especially the validity of the instrument). And if your test is multiple-choice, that same claim also holds for the number of options per item (the greater the number of options, the higher the reliability). Ebel (1969, 1972) explains both of these matters nicely. Common sense, however, dictates that you can't have huge numbers of items or choices, because that would lead to fatigue, boredom, cheating, or resistance on the part of the test-takers.

The standard error of measurement, which is a scale-bound indicator of the reliability of a measuring instrument for an individual person, can actually be approximated by using the formula .43 times the square root of k, where k is the number of items on the test. (You can look it up, as Casey Stengel used to say.) It is then possible to establish a confidence interval around his(her) obtained score and get some approximate idea of what his(her) "true score" is. The "true score" is what the person would have gotten if the test were perfectly reliable, i.e., what he(she) "deserved" to get.

Section 6

Question: How many equivalent forms should you have for a measuring instrument?

Answer: At least two (for "paper-and-pencil" tests).

Why?

If you want to determine the reliability of a measuring instrument, it is best to have more than one form so you can see if the instrument is consistent in the scores that it produces. You could administer a single form on two separate occasions and compare the results, but participants might "parrot back" all or many of the same responses on the two occasions, leading to an over-estimate of its "real" reliability. Most researchers don't administer two forms once each OR the same form twice. They just administer one form once and determine the item-to-item internal consistency (usually by Cronbach's alpha), but that is more an indicator of an instrument's homogeneity or the coherence of its items than its reliability (see Kelley, 1942).

The well-known Scholastic Aptitude Test (SAT) has more than two equivalent forms, mainly because the test has been developed to sample a large amount of content and because the developers are so concerned about cheating. The various forms are subject to severe security precautions.

If the instrument is a physical measuring instrument such as a stadiometer (for measuring height) there is no need for two or more equivalent forms (the stadiometer can't "parrot back" a height). But you must have two or more forms, by definition, if you're carrying out a method-comparison study. (See Bland & Altman, 2010 for several examples of that kind of study, which is "sort of" like reliability but the instruments are not equivalent or parallel, in the usual sense of those terms.)

Section 7

Question: How many judges should you have for an inter-rater reliability study?

Answer: One or more

Why?

It depends upon whether you're interested in within-judge agreement or between-judges agreement. If the former (which is actually referred to as intra-rater reliability) you might only care about the consistency of ratings given by a particular judge, in which case he(she) would have to rate the same individuals on at least two occasions. It is also necessary to specify whether it is the determination of the degree of absolute agreement which is of concern or whether the determination of the degree of relative agreement is sufficient. (If there are four persons to be rated and the judge gives them ratings of 1,3,5, and 7 at Time 1 and gives them ratings of 2,4,6, and 8, respectively, at Time 2, the absolute agreement is zero but the relative agreement is perfect.)

If between-judges agreement is of primary concern, the seriousness of the rating situation should determine whether there are only two judges or more than two. It is usually more "fair" to determine the consensus of ratings made by several judges rather than just the average ratings for two judges, but it is a heck of a lot more work!

Two sources for interesting discussions of the number of judges and how to determine their agreement are Stemler (2004) and LeBreton and Senter (2008). LeBreton and Senter use a 20-questions format (not unlike the format used in this chapter).

Section 8

Question: How many factors are interpretable in a factor analysis?

Answer: As many as there are eigenvalues greater than or equal to one for the correlation matrix.

Why?

If you have v variables, the "worst" (most heterogeneous solution) that can happen is that all of the eigenvalues of the correlation matrix are equal to one; i.e., each variable is its own factor. In the opposite extreme, the "best" (most homogeneous solution) that can happen is that there is just one big eigenvalue equal to n and therefore indicative of a unidimensional construct. (See Kaiser, 1960 for his "little jiffy" approach.) Although he has some concerns about the eigenvalues-greater-than-one "rule", Cliff (1988) provides a nice summary of its justification.

[If you don't know what an eigenvalue (sometimes called a latent root or a characteristic root) is, you can find out by reading the two sources cited in the preceding paragraph or by looking it up in a multivariate statistics textbook.]

The answer holds for both exploratory factor analysis and confirmatory factor analysis. [I've never understood the need for confirmatory factor analysis. Why bother to hypothesize how many factors there are and then find out how smart you are? Why not just carry out an exploratory factor analysis and find out how many there are?]

Section 9

Question: What is the minimum number of observations that will produce a unique mode for a variable?

Answer: Three

Why?

Suppose you have one observation, a. If so, you don't have a variable. Suppose you have two observations, a and b. If they're both the same, you again don't have a variable. If they're different, neither can be the mode. Now suppose you have three observations. If they're all the same, no variable. If they're all different, no mode. If two of them, say a and b, are the same but the third, c, is different from either of them, then a = b= the mode.

The mode doesn't come up very often, but it should. A manufacturer of men's overalls can only produce a small number of different sizes of overalls, and if optimization of profits is of primary concern (what else?) the modal size of men in general is what is important to know, so that more overalls of that size are produced than any other. A more scientific example arises in the case of something like blood type. Lacking specific information of the blood types of people in a given community, the American Red Cross would probably want to keep a greater supply of the modal blood type (which is O positive) than any other. Note that for blood type neither the mean nor the median would be relevant (or able to be computed), because the measurement of blood type employs a nominal scale.

There is an excellent Australian source on the internet called Statistics S1.1: Working with data, which is accessible free of charge and which is the finest source I've ever seen for understanding what a mode is and how it differs from a median and a mean. (It is intended for school children and uses British currency and British spelling, but I hope that doesn't bother you.) And if you ever get interested in a set of data that has two modes (bimodality), I wrote a very long article about that (Knapp, 2007b).

Section 10

Question: How many categories should you have in a two-way contingency table ("cross-tab")?

Answer: However many it takes (but be careful).

Why?

Two-way contingency tables are used to investigate the relationship between two nominal variables, such as sex and political affiliation, race and blood type, and the like. For the sex-by-political affiliation example, sex requires two categories (male and female) and political affiliation requires at least two (Democrat and Republican) and as many as six or more (if you include Independent, Libertarian, Green, None, and others). For race you might have the usual four (Caucasian, African-American, Hispanic, Asian-American) or even more, if you need to add Native-American and others. Blood type requires eight (A positive, A negative, B positive, B negative, AB positive, AB negative, O positive, and O negative). But if you have a relatively small sample you might have to "collapse" a, say, 4x8 table, into a smaller table, and depending upon how you do the collapsing you can get quite different answers for the relationship between the row variable and the column variable. Collapsing of categories is all too common in quantitative research, and should only be used when absolutely necessary. (See Cohen, 1983, and Owen & Froman, 2005.)

Section 11

Question: How many ways are there to calculate a Pearson product-moment correlation coefficient?

Answer: At least 14

Why?

In a classic article several years ago, Joe Rodgers and Alan Nicewander (1988) showed there were 13 mathematically equivalent ways. I subsequently wrote to Joe and told him about a 14th way (Knapp, 1979). There might be even more.

It of course doesn't really matter which formula is used, as long as it's one of the 14 and the calculations are carried out properly, by computer program, by hand, or whatever. Far more important is whether a Pearson r is appropriate in the first place. It is strictly an indicator of the direction and degree of LINEAR relationship between two variables. Researchers should always plot the data before carrying out the calculation. If the plot "looks" linear or if the data "pass" a test of linearity, fine. If not, a data transformation is called for.

Section 12

Question: How many significance tests should be carried out for baseline data in a true experiment (randomized controlled trial)?

Answer: None.

Why?

If the participants have been randomly assigned to treatment conditions, there is no need for testing baseline differences. (See Senn, 1994, and Assmann, Pocock, Enos, & Kasten, 2000.) The significance test or the confidence interval takes into account any differences that might have occurred by chance. And there are at least two additional problems: (1) how do you determine what variables on which such tests should be performed?; and (2) what do you do if you find a statistically significant difference for a particular variable? [Use it as a covariate? That's bad science. Covariates should be chosen based upon theoretical expectations and before seeing any data.]

Section 13

Question: How many independent (predictor) variables should you have in a multiple regression analysis?

Answer: Since it's "multiple" you need at least two, but don't have too many.

Why?

Some sources suggest a "rule of thumb" of a 10:1 ratio of number of participants to number of independent variables, but it all depends upon how good a "fit" you require. If you choose to base the determination of the number of independent variables upon a power analysis, you could use Cohen's (1992) table "in reverse"; i.e., you specify the level of significance, the desired power, and your sample size; and then read off the number of independent variables that would give you the power you want.

Knapp and Campbell-Heider (1989) provided a summary of the various guidelines that have been promulgated for the number of participants vs. the number of variables for a variety of multivariate analyses, including multiple regression analysis.

Section 14

Question: How many degrees of freedom are there for a given statistic and its sampling distribution?

Answer: It's usually something minus one.

Why?

The concept of number of degrees of freedom (df) is probably the most mysterious concept in all of statistics. Its definitions range all the way from "the number of unconstrained observations" to "something you need to know in order to use a table in the back of a statistics book". Let's take a couple of examples:

1. For a sample mean, if you know what all of the observations are except for one, and you also know what the mean is, then that one observation must be such that the sum of it and the others is equal to n times the mean, where n is the total number of observations. Therefore, the number of degrees of freedom associated with a sample mean is n-1. And if you use a table of the t sampling distribution to construct a confidence interval for the unknown population mean you find the value of t for n-1 degrees of freedom that you should lay off on the high side and on the low side of the sample mean.

2. For a 2x2 contingency table ("cross-tab"), if you want to test the significance of the difference between two independent proportions or percentages, if you know one of the cell frequencies, and you know the two row (r) totals and the two

column (c) totals, the other three cell frequencies are not free to vary, so there is only one degree of freedom associated with that table, calculated by multiplying (r-1) by (c-1), which in this case = (2-1)x(2-1) = 1 x 1 =1. You then calculate the value of chi-square by using the traditional formula and refer that value to a table of the chi-square sampling distribution for df=1 to find out if your result is or is not statistically significant with respect to your chosen alpha level.

Statistical consultants occasionally tell clients that "you lost one degree of freedom for this" or "you lost two degrees of freedom for that", which usually conveys nothing to the clients, since the clients don't know they have any to start with!

For all you ever need to know (and then some) about degrees of freedom, see the article by Helen Walker (1940...yes, 1940), but see also Ron Dotsch's Degrees of Freedom Tutorial (accessible free of charge on the internet) if you are particularly interested in the concept as it applies to the analysis of variance and the associated F tests.

Section 15

Question: How many statistical inferences should you make for a given study?

Answer: No more than one.

Why?

If you have data for an entire population (no matter what its size) or if you have data for a "convenience", non-random sample, no statistical inferences are warranted. If you do have data for a random sample and you want to make an inference from the sample to the population, one hypothesis test or one confidence interval (but not both, please) is fine. If you have a complicated study involving several groups and/or several variables, and if you carry out more than one statistical inference, it is usually incumbent upon you to make some sort of statistical "correction" (e.g., Bonferroni) or your error probabilities are greater than you assume a priori. Best to avoid the problem entirely by concentrating on a single parameter and its sample estimate.

If you are familiar with the research literature in whatever your particular discipline might be, you might find some of the claims in the previous paragraph to be unreasonable. Just about every research report is loaded with more than one p-value and/or more than one confidence interval, isn't it, no matter whether for a population, a convenience sample, or a random sample? Yes, that's true, but it doesn't make it right.

For opposing views on the matter of adjusting the alpha level when making more than one significance test, see the article by O'Keefe (2003) and the response by

Hewes (2003). Neither of them argues for having no more than one statistical inference per study, but I do.

Section 16

Question: How many pages should a manuscript to be submitted for publication have?

Answer: Far fewer than a dissertation has.

Why?

Every journal has its own guidelines regarding the maximum number of pages (sometimes the number of double-spaced "typewritten" pages; sometimes the number of journal-sized pages: sometimes the number of "bytes"), but, interestingly enough, rarely any guidelines regarding the minimum number. Doctoral dissertations, which are often the source of manuscripts, are notoriously redundant, and the researcher often finds it very difficult to "pare down" a long dissertation to a shorter document of typical journal length. (See Pollard, 2005.)

The length of the manuscript should be proportional to the complexity of the study upon which the manuscript is based. I think five double-spaced pages (approximately 2-3 journal-sized pages) would be an absolute minimum and 50 or so (including tables and figures) would be a maximum. Anything longer than 50 pages should probably be broken up into two or more manuscripts.

Pollard doesn't provide any specific recommendations for the number of pages, but the American Meteorological Society (2012) does. Here's what their authors' guide says:

"Expedited Contributions: Length of no more than 2500 words (approximately 9 double-spaced pages), including the manuscript body through appendices and acknowledgments, but not the abstract, figure caption list, or references. No length waivers can be requested for this submission type. Expedited Contributions may not contain more than a total of six tables and figures.

All other manuscript types: Length of 7500 words or less not counting the abstract, figure caption list or references. If a submission exceeds the word limit the author must upload a cover letter with the manuscript presenting a justification for the length of the manuscript to request the Chief Editor’s approval of the overage."

You can't get more explicit than that!

Section 17

Question: How many references should a manuscript have?

Answer: At least one (and if only one, not to another paper by the same author).

Why?

It is the rare manuscript that stands all by itself. Some sort of review of related literature is usually necessary as a foundation upon which the manuscript in hand is built. The number of references should be roughly proportional to the length of the manuscript. Some of the references are often to "classic" books and/or journal articles, but most are to sources that are more specifically relevant for the study being reported. Some references are often to unpublished documents that are accessible on the internet.

For other recommendations regarding the number of references, see Dimitroulis (2011) and the New England Journal of Medicine's (NEJM) Author Center.

Section 18

Question: How many manuscripts should be generated from a given study?

Answer: No more than two

Why?

I'm tempted to say "one study, one manuscript" (just like "one person, one vote"), but it could be the case that the substantive findings might be of interest to one audience and the methods used might be of interest to another audience. Or the theoretical implications might be of interest to one audience and the practical implications might be of interest to a different audience.

Aaronson (1994) was concerned that authors try to generate too many manuscripts from a single study. Blancett, Flanagin, and Young (1995) agreed. In an era of "publish or perish" it is understandable that researchers should make every effort to draw their work to the attention of their peers, but it should be the quality, not the quantity, of the work that is the deciding factor.

Section 19

Question: How many authors should a manuscript have?

Answer: No more than three.

Why?

The first author should be the principal investigator for the study itself. If the study is huge, involving lots of people and lots of data, a co-investigator (second author) might be necessary to carry out the study successfully and to assist in the writing up of the results. If the analysis of the data is technically complicated, a third person (a statistician or statistical consultant) might need to be a third author so that things are said "the right way". All other people who participated in the study in any important way should be acknowledged in the manuscript but not listed as authors. I once read an article in the Journal of the National Cancer Institute that had 160 authors. [I counted them.] In his article with the delightful title, "How many neurosurgeons does it take to write a research article", King (2000) said he had found more than that.

King actually started a trend. In subsequent years there were other articles with the titles "How many _____ does it take to write a research article?", all of which (including King's) were serious reviews of the trajectories over the last several years in the numbers of authors of articles in their respective speciality journals, but all capitalizing on the old joke "How many______does it take to change a light bulb?"

The article by Erlen, Siminoff, Sereika, and Sutton (1997) has a good discussion of the problem of multiple authorship, including considerations regarding the order in which the authors should be listed.

[This just in: A couple of other articles in JNCI with over 200 authors each!]

<u>Section 20</u>

Question: How many reviewers should evaluate a given manuscript?

Answer: At least three.

<u>Why</u>?

The typical alternative decisions that are made by reviewers go something like this: Accept as is; Accept if certain revisions are made; Reject unless certain revisions are made; and Reject. Since the middle two categories could be combined, for simplicity let's consider a three-point scale: (1) Accept essentially as is (perhaps there are a few minor typos); (2) Revisions are necessary (reasonably major revisions other than mechanical errors such as a few references out of alphabetical order); and (3) Reject (unsalvageable). If there are three reviewers, here are all of the possible combinations and the decision that I think should be made (by the editor):

1,1,1->1; 1,1,2->1; 1,1,3->2; 1,2,2->2; 1,2,3->2; 1,3,3->2; 2,2,2->2; 2,2,3->2; 2,3,3->3; 3,3,3->3.

Do you agree? 2 is the modal decision (6 out of the 10), and my understanding is that is what usually happens in practice (very few manuscripts are accepted forthwith and very few are rejected outright). Three reviewers should be sufficient. If there are two and their respective recommendations are 1 and 3 (the worst case), the editor should "break the tie" and give it a 2. If there is just one reviewer, that's too much power for one individual to have. If there are more than three, all the better for reconciling differences of opinion, but the extra work involved might not be worth it.

The March, 1991 issue of Behavioral and Brain Sciences has lots of good stuff about the number of reviewers and related matters. Kaplan, Lacetera, and Kaplan (2008) actually base the required number of reviewers on a fancy statistical formula! I'll stick with three.

Alternate section (if one of the previous five is no good)

Question: What is the maximum number of journals you should try before you give up hope for getting a manuscript published?

Answer: Three.

Why?

If you are successful in getting your manuscript published by the first journal to which you submit it (with or without any revisions), count your blessings. If you strike out at the first journal, perhaps because your manuscript is not deemed to be relevant for that journal's readership or because the journal has a very high rejection rate, you certainly should try a second one. But if you get rejected again, try a third, and also get rejected there, you should "get the message" and concentrate your publication efforts on a different topic.

One thing you should never do is submit the same manuscript to two different journals simultaneously. It is both unethical and wasteful of the time of busy reviewers. I do know of one person who submitted two manuscripts, call them A and B, to two different journals, call them X and Y, respectively, at approximately the same time. Both manuscripts were rejected. Without making any revisions he submitted Manuscript A to Journal Y and Manuscript B to Journal X. Both were accepted. Manuscript review is very subjective, so that sort of thing, though amusing, is not terribly surprising. For all its warts, however, nothing seems to work better than peer review.

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CHAPTER 34: THREE

I have always been fascinated by both words and numbers. (I don't like graphs, except for frequency distributions, scatter diagrams, and interrupted time-series designs). The word "TWO" and the number "2" come up a lot in statistics (the difference between two means, the correlation between 2 variables, etc.). I thought I'd see if it would be possible to write a chapter paper about "THREE" and "3". [I wrote one about "SEVEN" and "7"---regarding Cronbach's Alpha (see Chapter 14), not the alcoholic drink.] What follows is my attempt to do so. I have tried to concentrate on ten situations where "threeness" is of interest.

1. Many years ago I wrote a paper regarding the sampling distribution of the mode for samples of size three from a Bernoulli (two-point) population. Students are always confusing sampling distributions with population distributions and sample distributions, so I chose this particular simple statistic to illustrate the concept. [Nobody wanted to publish that paper.] Here is the result for Pr(0) = p_0 and Pr(1) = p_1:

Possible Data	Mode	Relative Frequency
0,0,0	0	$p_0{}^3$
0,0,1	0	$p_0{}^2\ p_1$
0,1,0	0	"
1,0,0	0	"
1,1,0	1	$p_1{}^2\ p_0$
1,0,1	1	"
0,1,1	1	"
1,1,1	1	$p_1{}^3$

Therefore, the sampling distribution is:

Mode	Relative Frequency
0	$p_0{}^3 + 3p_0{}^2\ p_1$
1	$p_1{}^3 + 3p_1{}^2\ p_0$

For example, if p_0 = .7 and p_1 = .3:

Mode	Relative Frequency
0	.343 + 3 (.147) = .343 + .441 = .784
1	.027 + 3 (.063) = .027 + .189 = .216

2. I wrote another paper (that somebody did want to publish) in which I gave an example of seven observations on three variables for which the correlation matrix for the data was the identity matrix of order three. Here are the data:

Observation	X_1	X_2	X_3
A	1	2	6
B	2	3	1
C	3	5	5
D	4	7	2
E	5	6	7
F	6	4	3
G	7	1	4

That might be a nice example to illustrate what can happen in a regression analysis or a factor analysis where everything correlates zero with everything else.

3. My friend and fellow statistician Matt Hayat reminded me that there are three kinds of t tests for means: one for a single sample, one for two independent samples, and one for two dependent (correlated) samples.

4. There is something called "The rule of three" for the situation where there have been no observed events in a sample of n binomial trials and the researcher would like to estimate the rate of occurrence in the population from which the sample has been drawn. Using the traditional formula for a 95% confidence interval for a proportion won't work, because the sample proportion p_s is equal to 0, 1-p_s is equal to 1, and their product is equal to 0, implying that there is no sampling error!. The rule of three says that you should use [0, 3/n] as the 95% confidence interval, where n is the sample size.

5. Advocates of a "three-point assay" argue for having observations on X (the independent, predictor variable in a regression analysis) at the lowest, middle, and highest value, with one-third of them at each of those three points.

6. Some epidemiologists like to report a "three-number summary" of their data, especially for diagnostic testing: sensitivity, specificity, and prevalence.

7. And then there is the standardized third moment about the mean (the cubed mean of the deviation scores divided by the cube of the standard deviation), which is Karl Pearson's measure of the skewness of a frequency distribution, and is sometimes symbolized by $\sqrt{b_1}$. [Its square, b_1, is generally more useful in mathematical statistics.] Pearson's measure of the kurtosis of a frequency distribution is b_2, the standardized fourth moment about the mean (the mean of the fourth powers of the deviation scores divided by the standard deviation raised to the fourth power), which for the normal distribution just happens to be equal to 3.

8. If you have a sample Pearson product-moment correlation coefficient r, and you want to estimate the population Pearson product-moment correlation

coefficient ρ, the procedure involves the Fisher r-to-z transformation, putting a confidence interval around the z with a standard error of 1/√ (n-3), and then transforming the endpoints of the interval back to the r scale by using the inverse z-to-r transformation. Chalk up another 3.

9. Years ago when I was studying plane geometry in high school, the common way to test our knowledge of that subject was to present us with a series of k declarative statements and ask us to indicate for each statement whether it was always true, sometimes true, or never true. [Each of those test items actually constituted a three-point Likert-type scale.]

10. Although it is traditional in a randomized controlled trial (a true experiment) to test the effect of one experimental treatment against one control treatment, it is sometimes more fruitful to test the relative effects of three treatments (one experimental treatment, one control treatment, and no treatment at all). For example, when testing a "new" method for teaching reading to first-graders against an "old" method for teaching reading to first-graders, it might be nice to randomly assign one-third of the pupils to "new", one-third to "old", and one-third to "none". ". It's possible that the pupils in the third group who don't actually get taught how to read might do as well as those who do. Isn't it?

CHAPTER 35: ALPHABETA SOUP

Introduction

Twenty years ago I wrote a little statistics book (Knapp, 1996) in which there were no formulas and only two symbols (X and Y). It seemed to me at the time (and still does) that the concepts in descriptive statistics and inferential statistics are difficult enough without an extra layer of symbols and formulas to exacerbate the problem of learning statistics. I have seen so many of the same symbols used for entirely different concepts that I decided I would like to try to point out some of the confusions and make a recommendation regarding what we should do about them. I have entitled this paper "Alpha beta soup" to indicate the "soup" many people find themselves in when trying to cope with multiple uses of the Greek letters alpha and beta, and with similar multiple uses of other Greek letters and their Roman counterparts.

A little history

I'm not the only person who has been bothered by multiple uses of the same symbols in statistical notation. In 1965, Halperin, Hartley, and Hoel tried to rescue the situation by proposing a standard set of symbols to be used for various statistical concepts. Among their recommendations was alpha for the probability associated with certain sampling distributions and beta for the partial regression coefficients in population regression equations. A few years later, Sanders and Pugh (1972) listed several of the recommendations made by Halperin, et al., and pointed out that authors of some statistics books used some of those symbols for very different purposes.

Alpha

As many of you already know, in addition to the use of alpha as a probability (of a Type I error in hypothesis testing, i.e.,"the level of significance"), the word alpha and the symbol α are encountered in the following contexts:

1. The Y-intercept in a population regression analysis for Y on X. (The three H's recommended beta with a zero subscript for that; see below.)

2. Cronbach's (1951) coefficient alpha, which is the very popular indicator of the degree of internal consistency reliability of a measuring instrument.

3. Some non-statistical contexts such as "the alpha male".

Beta

The situation regarding beta is even worse. In addition to the use of betas as the (unstandardized) partial regression coefficients, the word beta and the symbol β are encountered in the following contexts:

1. The probability of making a Type II error in hypothesis testing.

2. The standardized partial regression coefficients, especially in the social science research literature, in which they're called "beta weights". (This is one of the most perplexing and annoying contexts.)

3. A generic name for a family of statistical distributions.

4. Some non-statistical contexts such as "the beta version" of a statistical computer program or the "beta blockers" drugs.

Other Greek letters

1. The confusion between upper-case sigma (∑) and lower-case sigma (σ). The former is used to indicate summation (adding up) and the latter is used as a symbol for the population standard deviation. The upper-case sigma is also used to denote the population variance-covariance matrix in multivariate analysis.

2. The failure of many textbook writers and applied researchers to use the Greek nu (ν) for the number of degrees of freedom associated with certain sampling distributions, despite the fact that almost all mathematical statisticians use it for that. [Maybe it looks too much like a v?]

3. The use of rho (ρ) for the population Pearson product-moment correlation coefficient and for Spearman's rank correlation, either in the population or in a sample (almost never stipulated).

4. It is very common to see π used for a population proportion, thereby causing all sorts of confusion with the constant $\pi = 3.14159...$ The upper-case pi (Π) is used in statistics and in mathematics in general to indicate the product of several numbers (just as the upper-case sigma is used to indicate the sum of several numbers).

5. The Greek letter gamma (γ) is used to denote a certain family of statistical distributions and was used by Goodman and Kruskal (1979) as the symbol for their measure of the relationship between two ordinal variables. There is also a possible confusion with the non-statistical but important scientific concept of "gamma rays".

6. The Greek letter lambda (λ) is used as the symbol for the (only) parameter of a Poisson distribution, was used by Goodman and Kruskal as the symbol for their measure of the relationship between two nominal variables, and was adopted by Wilks for his multivariate statistic (the so-called "Wilks' lambda"), which has an F sampling distribution.

7. The Greek delta (δ) was Cohen's (1988 and elsewhere) choice for the hypothesized population "effect size", which is the difference between two population means divided by their common standard deviation. The principal problems with the Greek delta are that it is used to indicate "a very small amount" in calculus, and its capitalized version (Δ) is often used to denote change. (Cohen's delta usually has nothing to do with change, because its main use is for randomized trials where the experimental and control groups are measured concurrently at the end of an experiment.) Some non-statistical contexts in which the word delta appears are: Delta airlines; delta force; and the geographic concept of a delta

8. Cohen (1960) had earlier chosen the Greek letter kappa (κ) to denote his measure of inter-rater reliability that has been corrected for chance agreement. This letter should be one of the few that don't cause problems. [The only confusion I can think of is in the non-statistical context where the kappa is preceded by phi and beta.]

9. Speaking of phi (pronounced "fee" by some people and "fy" by others), it is used to denote a measure of the relationship between two dichotomous nominal variables (the so-called "phi coefficient") in statistics. But it is used to denote "the golden ratio" of 1.618 and as one of many symbols in mathematics in general to denote angles.

10. Lastly (you hope) there is epsilon (ε), which is used to denote "error" (most often sampling error) in statistics, but, like delta, is used to indicate "a very small amount" in calculus.

<u>Some of their Roman counterparts</u>

H,H, and H (1965) and Sanders and Pugh (1972) agreed that population parameters should be denoted by Greek letters and sample statistics should be denoted by Roman letters. They also supported upper-case Roman letters for parameters, if Greek letters were not used, and lower-case Roman letters for statistics. There are still occasional violators of those suggestions, however. Here are two of them:

1. The use of s for the sample standard deviation is very common, but there are two s's, one whose formula has the sample size n in the denominator and the other whose formula has one less than the sample size in the denominator, so they have to be differentiated from one another notationally. I have wriiten

extensively about the problem of n vs. n-1 (Knapp, 1970; and Chapter 23 of this book.)

2. Most people prefer α for the population intercept and a for the sample intercept, respectively, rather than β_0 and b_0 .

The use of bars and hats

Here things start to get tricky. The almost universal convention for symbolizing a sample mean for a variable X is to use an x with a horizontal "bar" (overscore?) above it. Some people don't like that, perhaps because it might take two lines of type rather than one. But I found a blog on the internet that explains how to do it without inserting an extra line. Here's the sample mean "x bar" on the same line: $\bar{x}$. Nice, huh?

As far as hats (technically called carets or circumflexes) are concerned, the "rule" in mathematical statistics is easy to state but hard to enforce: When referring to a sample statistic as an estimate of a population parameter, use a lower-case Greek letter with a hat over it. For example, a sample estimate of a population mean would be "mu hat" ($\hat{\mu}$). [I also learned how to do that from a blog.]

What should we do about statistical notation?

As a resident of Hawaii [tough life] I am tempted to suggest using an entirely different alphabet, such as the Hawaiian alphabet that has all five of the Roman vowels (a,e,i,o,u) and only seven of the Roman consonants (h,k,l,m,n,p,w), but that might make matters worse since you'd have to string so many symbols together. (The Hawaiians have been doing that for many years. Consider, for example, the name of the state fish: Humuhumunukunukuapuaa.)

How about this: Don't use any Greek letters. (See Elena C. Papanastasiou's 2003 very informative yet humorous article about Greek letters. As her name suggests, she is Greek.) And don't use capital letters for parameters and small letters for statistics. Just use lower-case Roman letters WITHOUT "hats" for parameters and lower-case Roman letters WITH "hats" for statistics. Does that make sense?

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CHAPTER 36: VERBAL 2x2 TABLES

Introduction

Two-by-two tables (also referred to as 2x2 tables) for displaying frequencies and percentages were treated in Chapter 15. 2x2 tables are also very useful devices for displaying verbal information.

Notation and jargon

Every 2x2 table is of the form $\begin{matrix} a & b \\ c & d \end{matrix}$, where a,b,c,and d are pieces of information.

They might be individual words or word phrases. The places where the information lies are called "cells". a and b constitute the first row (horizontal dimension) of the table, c and d constitute the second row; a and c constitute the first column (vertical dimension), b and d the second column; a and d form the "major (principal) diagonal", and b and c form the "minor diagonal".

Surrounding the basic 2x2 format there is often auxiliary information. Such things are called "marginals"

Some examples

My favorite example of a 2x2 table that conveys important verbal information is the table that is found in just about every statistics textbook in one form or another. Here is one version (H_o is the "null" hypothesis, i.e., the hypothesis that is directly tested):

		TRUTH	
		H_o is true	H_o is false
DECISION	Reject H_o	Type I error	No error
	Do not reject H_o	No error	Type II error

In that table, a = Type I error, b = No error, c = No error, and d = Type II error. The other words, albeit essential, are headings and marginals to the table itself. [More about this table later.]

What makes these tables useful is that many concepts involve distinctions between "sub-concepts" for which 2x2 tables are ideal in laying out those distinctions. Scientific theories are particularly concerned with exemplars of such distinctions and with procedures for testing them.

Here is an example of a 2x2 table (plus marginals), downloaded from an internet article entitled 'How to evaluate a 2 by 2 table", that is used as a basis for defining and determining the sensitivity and the specificity of a diagnostic testing procedure, where a = TP denotes "true positive", b = FP denotes "false positive", c = FN denotes "false negative", and d = TN denotes "true negative" [This table is actually a re-working of the table given above for summarizing the difference between a Type I error and a Type II error. Can you figure out which cells in this table correspond to which cells in the previous table?]

	Disease present	Disease absent	
Test positive	TP	FP	Total positive
Test negative	FN	TN	Total negative
	Total with disease	Total without disease	Grand total

And here is an example of a verbal 2x2 table for understanding the difference between random sampling and random assignment. It is adapted from Display 1.5 on page 9 of the textbook The Statistical Sleuth, 3rd edition, written by F.L. Ramsey and D.W. Schafer (Brooks/Cole Publishing Co., 2013).

	ASSIGNMENT	
	Random	Non-random
SAMPLING		
Random	Causal inference OK Inference to population OK	Causal inference NG Inference to population OK
Non-random	Causal inference OK Inference to population NG	Causal inference NG Inference to population NG

OK = warranted; NG = not warranted

Back to the H_0 table

There are symbols and associated jargon associated with such a table. The probabilities of making some of the errors are denoted by various symbols: the Greek α (alpha) for the probability of a Type I error; and the Greek β (beta) for the probability of a Type II error. But perhaps the most important concept is that of "power". It is the probability of NOT making a Type II error, i.e., the probability of correctly rejecting a false null hypothesis, which is usually "the name of the game".

Epilogue

When I was in the army many years ago right after the end of the Korean War, I had a fellow soldier friend who claimed to be a "polytheistic atheist". He claimed that were lots of gods and he didn't believe in any of them. But he worried that he might be wrong. His dilemma can be summarized by the following 2x2 table:

		TRUTH	
		There is at least one god	There are no gods
BELIEF	God(s)	No error	Error
	No god(s)	Error	No error

I think that says it all.

CHAPTER 37: STATISTICS WITHOUT THE NORMAL DISTRIBUTION: A FABLE

Once upon a time a statistician suggested that we would be better off if DeMoivre, Gauss, at al. never invented the "normal", "bell-shaped" distribution. He made the following outrageous claims:

1. Nothing in the real world is normally distributed (see, for example, the article entitled "The unicorn, the normal curve, and other improbable creatures", written by Theodore Micceri in Psychological Bulletin, 1989, 105 (1), 156-166.) And in the theoretical statistical world there are actually very few things that need to be normally distributed, the most important of which are the residuals in regression analysis (see Petr Keil's online post of February 18, 2013). Advocates of normal distributions reluctantly agree that real-world distributions are not normal but they claim that the normal distribution is necessary for many "model-based" statistical inferences. The word "model" does not need to be used when discussing statistics.

2. Normal distributions have nothing to do with the word "normal" as synonymous with "typical" or as used as a value judgment in ordinary human parlance. That word should be saved for clinical situations such as "your blood pressure is normal (i.e., OK) for your age".

3. Many non-parametric statistics, e.g. the Mann-Whitney test, have power that is only slightly less than their parametric counterparts if the underlying population distribution(s) is(are) normal, and often have greater power when the underlying population distribution(s) is(are) not. It is better to have fewer assumptions rather than more, unless the extra assumptions "buy" you more than they cost in terms of technical difficulties. The assumption of underlying normality is often not warranted and if violated when warranted can lead to serious errors in inference.

4. The time spent on teaching "the empirical rule" (68, 95, 99.7) could be spent on better explanations of the always-confusing but crucial concept of a sampling distribution (there are lots of non-normal ones). Knowing that if you go one standard deviation to the left and to the right of the mean of a normal distribution you capture approximately 68% of the observations, if you go two you capture 95%, and if you go three you capture about 99.7% is no big deal.

5. You could forget about "the central limit theorem", which is one of the principal justifications for incorporating the normal distribution in the statistical armamentarium, but is also one of the most over-used justifications and often mis-interpreted. It isn't necessary to appeal to the central limit theorem for an approximation to the sampling distribution of a particular statistic, e.g., the difference between two independent sample means, when the sampling distribution of the same or a slightly different statistic, e.g., the difference

between two independent sample medians, can be generated with modern computer techniques such as the jackknife and the bootstrap.

6. Without the normal distribution, and its associated t sampling distribution, people might finally begin to use the more defensible randomization tests when analyzing the data for experiments. t is only good for approximating what you would get if you used a randomization test for such situations, and then only for causality and not generalizability, since experiments are almost never carried out on random samples.

7. Descriptive statistics would be more appropriately emphasized when dealing with non-random samples from non-normal populations, which is the case for most research studies. It is much more important to know what the obtained "effect size" was than to know that it is, or is not, statistically significant, or even what its "confidence limits" are.

8. Teachers wouldn't be able to assign ("curve") grades based upon a normal distribution when the scores on their tests are not even close to being normally distributed. (See the online piece by Prof. S.A. Miller of Hamilton College. The distribution of the scores in his example is fairly close to normal, but the distribution of the corresponding grades is not. Interesting. It's usually the other way 'round.)

9. There would be no such thing as "the normal approximation" to this or that distribution (e.g., the binomial sampling distribution) for which present-day computers can provide direct ("exact") solutions.

10. The use of rank-correlations rather than distribution-bound Pearson r's would gain in prominence. Correlation coefficients are indicators of the relative relationship between two variables, and nothing is better than ranks to reflect relative agreement.

That statistician's arguments were relegated to mythological status and he was quietly confined to a home for the audacious, where he lived unhappily ever after.