NOTES FOR SUMMER STATISTICS INSTITUTE COURSE

COMMON MISTAKES IN STATISTICS – SPOTTING THEM AND AVOIDING THEM

Day 4: Common Mistakes Based on Common Misunderstandings about Statistical Inference

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II. DATA SNOOPING

Remember Jelly Beans**:** http://xkcd.com/882/

Data snooping refers to statistical inference that the researcher decides to perform *after* looking at the data

- Also known as *post protocol analysis* or *post hoc analysis*
- Contrast with *pre-planned* inference ("*per protocol analysis*"), which the researcher plan has planned *before* looking at the data.

Data snooping can be done:

- professionally and ethically, or
- misleadingly and unethically, or
- misleadingly out of ignorance.

Misleading data snooping out of ignorance is a **common mistake** in using statistics.

The problems with data snooping are essentially the problems of multiple inference.

- So if you're likely to engage in data snooping infrence, plan to allocate some part of the overall Type I error rate to pre-planned inference and some part to data snooping.
  - For example, if you plan to have overall Type I error rate (FWER) 0.05, you might decide to use FWER 0.04 for pre-planned inference, and FWER 0.01 for data snooping.
- One way in which researchers unintentionally obtain misleading results by data snooping is in *failing to account for all of the data snooping they engage in*.
  - In particular, *in accounting for Type I error when data snooping, you need to count not just the actual hypothesis tests performed, but also all comparisons looked at when deciding which post hoc (i.e., not pre-planned) hypothesis tests to try.*
  - For lots of amusing examples, see Tyler Vigen's website http://tylervigen.com/.
    - The site allows you to choose two variables from a very large list and find their correlation.
    - I got tired counting at several hundred, but I would guess that he has listed over 1000 variables.
    - That makes around 1,000,000 pairs of variables.
    - If you did significance tests (at a .05 individual significance rate) for correlation for all those pairs, you would expect about 50,000 to be significant – so it shouldn't be surprising if many of these 50,000 pairs are indeed highly correlated.

A More Serious Example: A group of researchers plans to compare three dosages of a drug in a clinical trial.

- There's no pre-planned intent to compare effects broken down by sex, but the sex of the subjects is recorded.
- The researchers have decided to have an overall Type I error rate of 0.05, allowing 0.03 for the pre-planned inferences and 0.02 for any data snooping they might decide to do.
- The pre-planned comparisons show no statistically significant difference between the three dosages when the data are not broken down by sex.
- However, since the sex of the patients is known, the researchers decide to look at the outcomes broken down by combination of sex and dosage.
    - They notice that the results for women in the high-dosage group look much better than the results for the men in the low dosage group, and decide to perform a hypothesis test to check that out.
- *In accounting for Type I error, the researchers need to take the number of data-snooping inferences performed as 15, <u>not</u> one*.
    - The reason : *They've looked at fifteen comparisons* -- there are $3\times2 = 6$ dosage×sex combinations, and hence $(6\times5)/2 = 15$ pairs of dosage×sex combinations.
    - Thus the significance level for the post hoc test should not be 0.02, but (if the Bonferroni method is used) 0.02/15.

See the Appendix for more detailed suggestions on data snooping professionally and ethically.

III: P-HACKING, THE REPLICABILITY CRISIS, P-CURVING, AND "THE GARDEN OF FORKING PATHS"

P-hacking and the replicability crisis:

Simonsohn et al (2013) introduced the term ***p-hacking*** to refer to a common practice that involves data snooping and aspects of the file-drawer problem:

> *Performing many hypothesis tests in analyzing the data for a study, but when publishing the results of the study, omitting mention of those tests that were not statistically significant.*

- So in p-hacking, researchers don't relegate entire studies to "the file-drawer" -- just parts of studies.

P-hacking (like may other common mistakes discussed here) contributes to what has become known as the ***replicability crisis***:

> *The large number of published "findings" that have never been confirmed by a follow-up study*.

- Many such results might indeed be "irreproducible results."
- Ioannidis' paper, "Why Most Published Research Findings Are False," (Ioannidis 2005) brought widespread attention to the replicability crisis.
- Although there was initial skepticism and criticism of Ioannidis' claims, scientists have increasingly been recognizing the lack of replications, and the practices contributing to this, as a serious problem.
    - See, e.g., Pashler and Harris (2012)

There are many ways to p-hack. Some ways fall under the category of data snooping. These include:

- Collecting data until a statistically significant result is obtained.
    - Why is this a problem?

- Deciding to exclude outliers on the basis of whether or not doing so will give a statistically significant result.
    - Why is this a problem?

- Trying out more than one measure of a quantity of interest, then selecting one that gives statistical significance when others do not.
    - Why is this a problem?

- First trying an analysis without breaking down into subgroups, then if results are not statistically significant, analyzing the data broken down into subgroups (e.g., gender), but reporting only the statistically significant results.
    - Why is this a problem?

- Trying various methods of "binning" (discussed below) until getting one that gives a statistically significant result.

Like data-snooping, *p-hacking is often done out of ignorance that it gives deceptive results*.

- There's also a gray area/slippery slope where researchers feel impelled to "make the most" of their data.
    - This can also lead to "spinning," which might also include describing results that are not statistically significant as "promising," or results that are questionably practically significant as "strong" rather than "modest."
- For a real example of p-hacking in cancer research, plus discussion of spinning and the file drawer problem, see Couzin-Frankel (2013)

Contrived example: The course description for this SSI course included the sentence,

> *"*In 2011, psychologists Simmons, Nelson and Simonsohn brought further attention to this topic by using methods common in their field to "show" that people were almost 1.5 years younger after listening to one piece of music than after listening to another."

Some of the things these authors did to produce this nonsensical conclusion:

- Lots of data snooping.
  - In particular, they gathered information on several covariates, but adjusted for only one (father's age), in the "report".
- Lack of transparency in reporting results.
  - In particular, not mentioning that they had gathered the information on other covariates and cherry-picked the that gave the result they wanted.
- The sample size was not set in advance.
  - There was no consideration of power in deciding on sample size.
  - Instead, the researchers checked every few observations and stopped when the results reached a preset significance level.
  - The sample size was too small to give reasonable power.
- There was no adjusting for multiple testing despite all the multiple inference involved in data snooping and in deciding when to stop sampling.

Caution*:* Although the Simmons et al paper did a good job of making the point that common but questionable practices can lead to absurd results, the author's recommendations for better practices fall short of what is needed. (See http://www.ma.utexas.edu/blogs/mks/2013/01/09/a-mixed-bag/ for more discussion.)

P-curving

Simonsohn et al (2013) have proposed a method, called p-curving, to help detect the presence of p-hacking.

- The purpose of p-curving is [to try] "to rule out selective reporting as a likely explanation for a set of statistically significant findings." (p. 5) – just as the purpose of significance testing is [to try] "to rule out chance as a likely explanation for an observed effect" (p. 5)

- A *p-curve* is "the distribution of statistically significant p-values for a set of independent findings" (p. 3)

- The utility of p-curves depends on results in mathematical statistics saying that a p-curve will have a different shape when the null hypothesis is false than when the null hypothesis is true, and that the shape will also depend on effect size and sample size.

    - The net result is that p-hacking will produce alterations in the shape of the p-curve.

- The authors have also produced an online app and user's guide at http://www.p-curve.com/

- The technique appears to have prompted a fair amount of discussion and self-questioning among psychologists.

    - You might want to do your own web search on the topic

The garden of forking paths

Gelman and Loken (2013 and 2014b) introduced the metaphor "Garden of Forking Paths" to refer to the many branching choices researchers can make when analyzing their data.

- They point out that there are many possible choices that researchers can make in analyzing data *that seem reasonable yet might be influenced by the data*.

- Thus different data sets analyzed for the same question might reasonably lead to different choices.

- Thus the metaphor "garden of forking paths" (from the short story by Jorge Luis Borges)

- These choices are *often not made deliberately to "game the system.*

- Thus terms such as "fishing" or "p-hacking," which suggest deliberate acts, are often falsely accusatory.

- Nonetheless, the fact that decisions are contingent on the data means that calculated p-values are not meaningful.

- At the same time, studying the data to find out patterns and make tentative analysis decisions can be of value in understanding the problems being studied.

Gelman and Loken also discuss possible (at least partial) solutions:

- Preregistration works for some fields.
- An exploratory study followed by pre-publication replication can work well in some situations (e.g., the Nosek et al study mentioned yesterday.)
- In areas where most data is observational, the authors recommend full study of the data despite the problem of multiplicities.
    - In some cases, multilevel modeling can help.
- Researchers need to distinguish carefully between exploratory and confirmatory data analysis and be aware of the value and limitations of each.
- More research is needed into how to handle the problem of multiple comparisons, particularly in light of the garden of forking paths.

IV: USING AN INAPPROPRIATE METHOD OF ANALYSIS

"Assumptions behind models are rarely articulated, let alone defended. The problem is exacerbated because journals tend to favor a mild degree of novelty in statistical procedures. Modeling, the search for significance, the preference for novelty, and the lack of interest in assumptions -- these norms are likely to generate a flood of nonreproducible results."

David Freedman, *Chance* 2008, v. 21 No 1, p. 60

Recall: Each frequentist inference technique (hypothesis test or confidence interval) involves *model assumptions.*

- Different techniques have different model assumptions.
- *The validity of the technique depends* (to varying extents) on whether or not the model assumptions are true for the context of the data being analyzed.
- Many techniques are *robust* to departures from at least some model assumptions.
    - This means that if the particular assumption is not too far from true, then the technique is still approximately valid.
    - Illustration: Rice Virtual Lab in Statistics Robustness Simulation

Thus, when using a statistical technique, it's important to ask:

- What are the model assumptions for that technique?
- Is the technique robust to some departures from the model assumptions?
- What reason is there to believe that the model assumptions (or something close enough, if the technique is robust) are true for the situation being studied?

Neglecting these questions is a* very common mistake *in using statistics.

- Sometimes researchers check only some of the assumptions, perhaps missing some of the most important ones.

Unfortunately, the model assumptions vary from technique to technique, so there are few if any general rules. One general rule of thumb, however is:

Techniques are least likely to be robust to departures from assumptions of independence.

- *Recall*: Assumptions of independence are often phrased in terms of "random sample" or "random assignment", so these are very important.
- One exception is that, for large enough populations, sampling *without* replacement is good enough, even though "independent" technically means sampling *with* replacement.
- Variance estimates depend strongly on the assumption of independence, so results can be very misleading when observations are not independent.

Note: Many techniques are most robust to violations of normality assumptions, at least if the sample size is large and the distribution is not strongly skewed or multimodal.

- This is because test statistics are often sums or linear combinations, which by "the" Central Limit Theorem are often approximately normally distributed. (See Appendix re Checking Model Assumptions)

General advice and cautions:

- You may need to look hard to find model assumptions and information about robustness!
  - For basic statistical techniques, DeVeaux, Velleman and Bock *Statistic, Data and Models* is quite good on model assumptions and robustness.
  - For other techniques, try searching for review articles in journals such as *Statistical Science*, *The American Statistician,* or *Journal of the American Statistical Society*.

- Sometimes simulations (*if* well done) can help. For example:
  - Simulations might help decide how plausible it is that your data come from a certain distribution.
  - Simulations can sometimes help get a feel for how robust a procedure is to departures from model assumptions.

- Do *not* automatically use default settings in software.

How do I know whether or not model assumptions are satisfied?

Unfortunately, there are no one-size-fits-all methods, but here are some rough guidelines:

1. When selecting samples or dividing into treatment groups, be very careful in randomizing *according to the requirements of the method of analysis to be used.*

   - Remember that “random” is not the same as “haphazard”!
   - Be careful to check the precise randomizing assumptions of the study design/method of analysis you plan to use.
     - For example, there are many types of ANOVA analyses, each with its own requirements for study design, including randomization.

2. Sometimes (*but not very often!*) model assumptions can be justified plausibly by well-established facts, mathematical theorems, or theory that’s well supported by sound empirical evidence.

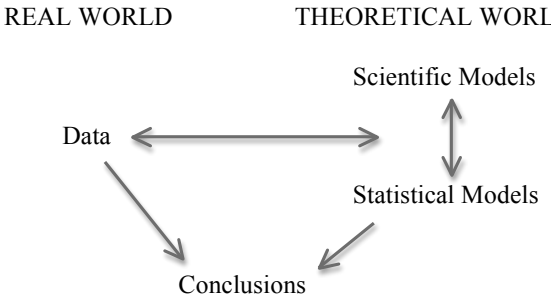
   - Here, "well established" means *well established by sound empirical evidence and/or sound mathematical reasoning*.
   - This is *not* the same as "well accepted," since sometimes things may be well accepted without sound evidence or reasoning.
   - More in Appendix

3. Sometimes a rough idea of whether or not model assumptions might fit can be obtained by plotting the data or residuals obtained from a tentative use of the model.

- Unfortunately, these methods are typically better at telling you when the model assumption does *not* fit than when it does.
- Some examples, guidelines, and cautions are in the Appendix.
- But always remember "The Big Picture":

Robert Kass' Big Picture of Statistical Inference

In Kass (2011, p. 6, Figure 1), Robert Kass has proposed the following diagram to depict the "big picture" in using statistics:



Points this picture is intended to show include:

- Both statistical and scientific models are abstractions, living in the "theoretical" world, as distinguished from the "real" world where data lie.
- Conclusions straddle these two worlds: *conclusions about the real world typically are indirect, via the scientific models.*
- "When we use a statistical model to make a statistical inference we implicitly assert that the variation exhibited by data is captured reasonably well by the statistical model, so that the theoretical world corresponds reasonably well to the real world." (p. 5)
- Thus "careful consideration of the connection between models and data is a core component of … the art of statistical practice…" (p. 6)

For a recent accessible discussion of problems with model assumptions in a topic of current wide interest (value-added models in education), see Wainer (2011).

V. METHODS FOR CHECKING MODEL ASSUMPTIONS

(See Appendix)

VI. SOME SPECIFIC SITUATIONS WHERE MISTAKES INVOLVING MODEL ASSUMPTIONS ARE COMMON

A. Comparing groups in studies with drop-outs (Intent-to-treat analysis)

B. Using a two-sample test comparing means when cases are paired (and generalizations)

C. Not distinguishing between fixed and random factors

D. Analyzing data without regard to how they were collected

E. Pseudoreplication

F. Mistakes in regression

For more discussion of some inappropriate methods of analysis, see:

- References in the Appendix
- Harris et al (2009)
- The Common Mistakes in Using Statistics website at http://www.ma.utexas.edu/users/mks/statmistakes/TOC.html

A. Intent to Treat Analysis: Comparing groups when there are Dropouts

The Problem: In many forms of comparison of two treatments involving human subjects (or animals or plants), there are subjects who do not complete the treatment.

- They may die, move away, encounter life circumstances that take priority, or just decide for whatever reason to drop out of the study or not do all that they are asked.
- It's tempting to just analyze the data for those completing the protocol, essentially ignoring the dropouts. ***This is usually a serious mistake***, for two reasons:

1. In a good study, subjects should be randomized to treatment.
    - Analyzing the data for only those who complete the protocol *destroys the randomization, so that model assumptions are not satisfied*.
    - To preserve the randomization, outcomes for *all* subjects *assigned* to each group (whether or not they stick with the treatment) need to be compared. This is called **intent-to-treat** (or intention-to-treat, or ITT) analysis.

2. Intent-to-treat analysis is usually more informative for consumers of the research.

- For example, in studying two drug treatments, dropouts for reasons not related to the treatment can be expected to be, on average, roughly the same for both groups.
- But if one drug has serious side-effects that prompt patients to discontinue use, that would show up in the drop-out rate, and be important information in deciding which drug to use or recommend.

Reason 1 (and sometimes also reason 2) also applies when treatments are applied to animals, plants, or even objects.

Unfortunately, when subjects drop out of an experiment, data collection for them is incomplete.

- Thus, analysis often requires figuring out how best to deal with missing data.

For more information on intent-to-treat analysis, see Freedman (2005, pp. 5, 15), Freedman (2006), van Belle (2008, pp. 156 – 157), and Moher et al (2010)

B. Using a Two-Sample Test Comparing Means when Cases Are Paired (and similar problems)

One of the model assumptions of the two-sample t-tests for means is that the observations *between groups*, as well as within groups, are independent.

- Thus if samples are chosen so that there is some natural pairing, then the members of pairs are not independent, so the two-sample t-test is *not* appropriate.

Example 1: A random sample of heterosexual married couples is chosen. Each spouse of each pair takes a survey on marital happiness. The intent is to compare husbands' and wives' scores.

- The two-sample t-test would compare the *average* of the husband's scores with the *average* of the wives' scores.
- However, it's *not* reasonable to assume that the samples of husbands and wives are independent -- some factors influencing a particular husband's score are likely to influence his wife's score, and vice versa.
- Thus the independence assumption *between* groups for a two-sample t-test is violated.
- In this example, we can instead consider the individual differences in scores for each couple: (husband's score) - (wife's score). If the questions of interest can be expressed in terms of these differences, then we can consider using the one-sample t-test (or perhaps a non-parametric test if the model assumptions of that test are not met).

Example 2: A test is given to each subject before and after a certain treatment. (For example, a blood test before and after receiving a medical treatment; or a subject matter test before and after a lesson on that subject)

- This poses the same problem as Example 1: The "before" test results and the "after" test results for each subject are *not independent*, because they come from the same subject.
- The solution is the same: analyze the *difference in scores*.
- Example 2 is a special case of what is called *repeated measures*: some measurement is taken more than once on the same unit.
    - Because repeated measures on the same unit are not independent, the analysis of such data needs a method that takes this lack of independence into account.
    - There are various ways to do this; just which one is best depends on the particular situation.

Similar Problem: *Hierarchical (multilevel) situations may violate model assumptions of independence*

Example: Researchers are studying how well scores on a standardized eighth grade math exam predict performance on an Algebra I end-of-course exam for ninth-grade students.

- They have data from an entire school district.
- They propose to analyze it by simple linear regression.
- However, standard regression methods of inference assume that observations are uncorrelated, whereas observations from students in the same school can be expected to be correlated.
- Instead, the researchers need to use a multilevel (also called hierarchical) model that takes into account that observations from the same school may be correlated.

C. Inappropriately Designating an Effect as Fixed, Variable, or Random

In Analysis of Variance and Multilevel Modeling, there are two types of factors: *fixed effect* and *random effect*.

- *Fixed effect factors and random effect factors are analyzed differently*, *so it's important to classify a factor correctly*.
- *Confusing the matter further, different definitions of "fixed" and "random" effects are used by different people.*

Correct classification of a factor as fixed or random depends on

- the context of the problem,
- the questions of interest, and
- how the data are gathered, and
- the method of analysis

1. Fixed and random effects ***for Analysis of Variance***:

***Fixed effect factor* in Analysis of Variance**: Data has been gathered from *all the levels of the factor that are of interest*.

Example: The purpose of an experiment is to compare the effects of three specific dosages of a drug on the response.

- "Dosage" is the factor.
- The three specific dosages in the experiment are the levels.
- There is no intent to say anything about other dosages.
- Therefore this is a fixed factor.
- The analysis will estimate the effect of each of the three dosages.

***Random effect factor* for Analysis of Variance**:

- The factor has *many possible levels*.
- *All* possible levels are of interest.
- Only a *random sample of levels* is included in the data.
- The analysis will estimate the *variability* of effects of the factor as levels vary, but not effects of specific levels.

Example: A large manufacturer is interested in studying the effect of machine operator on the quality of the final product. The researcher selects a random sample of operators from the large number of operators at the manufacturer's factories and collects data on just these operators.

- The factor is "operator."
- Each operator is a level of the factor.
- Since interest is not just in the operators for whom data is gathered, this is a random factor.
- The analysis will *not* estimate the effect of each of the operators in the sample, but *will instead estimate the variability attributable to the factor "operator"*.

(See Appendix for more discussion)

The appropriate statistical analysis depends on whether the factor is treated as fixed or as random. That is, fixed and random effects require different models

- Consequently, inferences may be incorrect if the factor is classified inappropriately.
- Mistakes in classification are most likely to occur when more than one factor is considered in the study.

Example: Two surgical procedures are being compared.

- Patients are randomized to treatment.
- Five different surgical teams are used.
- To prevent possible confounding of treatment and surgical team, each team is trained in both procedures, and each team performs equal numbers of surgery of each of the two types.
- Since the purpose of the experiment is to compare the *procedures*, the intent is to generalize to other surgical teams.
- Thus *surgical team* should be considered as a *random factor,* not a fixed factor.

Comments:

- This example can help understand why inferences might be different for the two classifications of the factor: Asserting that there is a difference in the results of the two procedures *regardless of the surgical team* is a stronger statement than saying that there is a difference in the results of the two procedures *just for the teams in the experiment*.
- Technically, the levels of the random factor (in this case, the five surgical teams) used in the experiment should be a random sample of all possible levels.
  - In practice, this is usually impossible, so the random factor analysis is usually used if there is reason to believe that the teams used in the experiment could reasonably be a random sample of all surgical teams who might perform the procedures.
  - However, this assumption needs careful thought to avoid possible bias.
  - For example, the conclusion would be sounder if it were limited to surgical teams that were trained in both procedures in the same manner and to the same extent, and who had the same surgical experiences, as the five teams actually studied.

2. Fixed and random effects ***for Multilevel (Hierarchical) Modeling:***

In this context, definitions vary, but one common one is that a ***fixed effect*** is one that is the same for all units within the same grouping, whereas a ***random effect*** is one that is allowed to vary between units of the same grouping.

Simple example: Suppose we're using a linear model for the heights of a group of children.

- Since some children are inherently taller than others, it may be appropriate to allow different intercepts for different children.
- This would give a model

$$h_{ij} = \alpha_j + \beta A_i + \varepsilon_{ij},$$

where h_{ij} is the height of child j at age A_i.

- In this example, α is called a *random effect* and β is called a *fixed effect*.

Note:
i. In this context, *the α_j's are estimated, and we're not interested in levels other than the ones corresponding to the children in the study. This contrasts with the use of "random effect" in ANOVA*.

ii. Some people use the terminology *variable effect* or *varying effect* rather than *random effect* in this context. That helps avoid the confusion with the use of "random effect" in ANOVA.

iii. See http://andrewgelman.com/2005/01/25/why_i_dont_use/ for more detail on the various ways the terms "fixed" and "random" are used.

D. Analyzing Data without Regard to How They Were Collected

Using a two-sample t-test when observations are paired (see above) is one example of this. Here's another:

Example: [See Potcner and Kowalski (2004) for data and details.] An experiment was conducted to study the effect of two factors (pretreatment and stain) on the water resistance of wood.

- Two types of pretreatment and four types of stain were considered.
- For reasons of practicality and economy, the experiment was conducted with a *split-plot design* as follows:
  - Six entire boards were the *whole plots*.
  - One pretreatment was applied to each board, with the two pretreatments randomly assigned to the six boards (three boards per pretreatment).
  - Then each pre-treated board was cut into four smaller pieces of equal size (these were the *split-plots*).
  - The four pieces from each entire board were randomly assigned to the four stains.
  - The water resistance of each of the 24 smaller pieces was measured; this was the response variable.
- The following chart shows the p-values of the three significance tests involved if the correct split-plot analysis is used, and also if an incorrect analysis (assuming a crossed design, with the 6 treatment combinations randomly assigned to the 24 smaller pieces of wood, with 4 small pieces per treatment combination) is used.
- Note that the conclusions from the two analyses would be quite different!

p-values	**Correct (Split Plot) Analysis**	**Incorrect (Crossed Design) Analysis**
Interaction	0.231	0.782
Pretreatment	0.115	0.002
Stain	0.006	0.245

Additional lessons to learn from this example:

- If you're using data collected by someone else, *be sure to find out how it was collected; that might affect how you need to analyze it.*
- If you're making data available to others, *be sure to include a description of how the data was obtained*.

Some of the many considerations to take into account in deciding on an appropriate method of analysis include:

- The sampling or randomization method
- Whether or not there was blocking in an experimental design
- Whether factors are nested or crossed
- Whether factors are fixed or random
- Pseudoreplication (See below)

E. PSEUDOREPLICATION

The term *pseudoreplication* was coined by Hurlbert (1984,p. 187) to refer to

> "the use of inferential statistics to test for treatment effects with data from experiments where either treatments are not replicated (though samples may be) or replicates are not statistically independent."

His paper concerned ecological field experiments, but pseudoreplication can occur in other fields as well.

In this context, *replication* refers to having more than one experimental (or observational) unit with the same treatment. Each unit with the same treatment is called a *replicate*.

> *Note*: There are other uses of the word replication -- for example, repeating an entire experiment is also called replication; each repetition of the experiment is called a replicate. This meaning is related to the one given above: If each treatment in an experiment has the same number r of replicates (in the sense given above), then the experiment can be considered as r replicates (in the second sense) of an experiment where each treatment is applied to only one experimental unit.

Heffner et al (1996, p. 2558) distinguish a pseudoreplicate from a *true replicate*, which they characterize as

> "the smallest experimental unit to which a treatment is independently applied."

Most models for statistical inference require *true* replication.

- *True* replication permits the estimation of *variability within a treatment*.
- Without estimating variability within treatments, it is impossible to do statistical inference.

Illustration: Consider comparing two drugs by trying drug A on person 1 and drug B on person 2.

- Drugs typically have different effects in different people.
- So this simple experiment will give us *no* information about generalizing to people other than the two involved.
- But if we try each drug on several people, then we can obtain some information about the *variability* of each drug, and use statistical inference to gain some information on whether or not one drug might be more effective than the other on average.

True replicates are often confused with repeated measurements or with pseudoreplicates. The following illustrate some of the ways this can occur.

Examples:

1. Suppose a blood-pressure lowering drug is administered to a patient, and then the patient's blood pressure is measured twice.
   - This is a *repeated measurement*, not a replication.
   - It can give information about the *uncertainty in the measurement process*, but *not* about the *variability in the effect of the drug.*
   - On the other hand, if the drug were administered to two patients, and each patient's blood pressure was measured once, we can say *the treatment has been replicated*, and the replication may give some information about the variability in the effect of the drug.

2. A researcher is studying the effect on plant growth of different concentrations of CO_2 in the air.
   - He needs to grow the plants in a growth chamber so that the CO_2 concentration can be set.
   - He has access to only two growth chambers, but each one will hold five plants.
   - However, since the five plants in each chamber share whatever conditions are in that chamber besides the CO_2 concentration (and in fact may also influence each other), the individual plants do *not* constitute independent replicates – they're pseudoreplicates.
   - The growth chambers are the experimental units: the treatments (CO_2 concentrations) are applied to the growth chambers, not to the plants independently.

3. Two fifth-grade math curricula are being studied.
   - Two schools have agreed to participate in the study.
   - One is randomly assigned to use curriculum A, the other to use curriculum B.
   - At the end of the school year, the fifth-grade students in each school are tested and the results are used to do a statistical analysis comparing the two curricula.
   - There is *no true replication* in this study; *the students are pseudo-replicates*.
   - The schools are the experimental units; they, not the students, are randomly assigned to treatment.
   - Within each school, the test results (and the learning) of the students in the experiment are not independent; they're influenced by the teacher and by other school-specific factors (e.g., previous teachers and learning, socioeconomic background of the school, etc.).

Consequences of doing statistical inference using pseudoreplicates rather than true replicates

*Variability will probably be under**estimated***. This will result in:

- Confidence intervals that are too small.
- An inflated probability of a Type I error (falsely rejecting a true null hypothesis).

Comments

- Note that in Example 2, there's no way to distinguish between effect of treatment and effect of growth chamber; thus the two factors (treatment and growth chamber) are *confounded*. Similarly, in Example 3, treatment and school are confounded.
- Example 3 may also be seen as applying the two treatments to two different *populations* (students in one school and students in the other school)
- Observational studies are particularly prone to pseudoreplication.
- Regression can sometimes partially account for lack of replication, provided data are close enough to each other.
    - The rough idea is that the responses for nearby values of the explanatory variables can give some estimate of the variability.
    - However, having replicates is better.

(See Appendix for suggestions on dealing with pseudoreplication.)

F. MISTAKES IN REGRESSION

There are many common mistakes involved in regression!

Only one will be discussed here; some others will be listed at the end of these notes, with a web reference to more discussion.

Overfitting

> *With four parameters I can fit an elephant and with five I can make him wiggle his trunk*.
> John von Neumann

If we have n distinct x values and corresponding y values for each, it is possible to find a curve going exactly through all n resulting points (x, y); this can be done by setting up a system of equations and solving simultaneously.

- But this is *not* what regression methods typically are designed to do.
- Most regression methods (e.g., least squares) estimate *conditional means* of the response variable given the explanatory variables.
- They're *not* expected to go through all the data points.

For example, with one explanatory variable X (e.g., height) and response variable Y (e.g., weight), if we fix a value x of X, we have a *conditional distribution of Y given X = x* (e.g., the conditional distribution of weight for people with height x).

- This conditional distribution has an expected value (population mean), which we will denote E(Y|X = x) (e.g., the mean weight of people with height x).
- This is the *conditional mean of Y given X = x*. It depends on x -- in other words, E(Y|X = x) is a mathematical function of x.

In least squares regression (and most other kinds of regression), *one of the model assumptions is that the conditional mean function has a specified form*.

- Then we use the data to find a function of x *that approximates the conditional mean function E(Y|X = x)*.
- This is different from, and subtler (and harder) than, finding a curve that goes through all the data points.

Example: To illustrate, I've used simulated data:

- Five points were sampled from a joint distribution where the conditional mean E(Y|X = x) is known to be x^2, and where each conditional distribution Y|(X = x) is normal with standard deviation 1.
- I used least squares regression to estimate the conditional means by a quadratic curve $y = a + bx + cx^2$. That is, I used least squares regression, with

  $$E(Y|X=x) = \alpha + \beta x + \gamma x^2$$

  as one of the model assumptions, to obtain estimates a, b, and c of α, β, and γ (respectively), based on the data.
  - There are other ways of expressing this model assumption, for example,

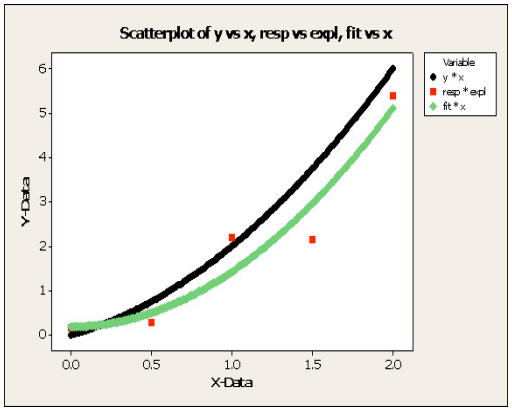
    $$y = \alpha + \beta x + \gamma x^2 + \varepsilon,$$

    or

    $$y_i = \alpha + \beta x_i + \gamma x_i^2 + \varepsilon_i$$

The graph below shows:

- The five data points in *red* (one at the left is mostly hidden by the green curve)
- The curve $y = x^2$ of true conditional means (*black*)
- The graph of the calculated regression equation (in *green*).



Note that:

- The points sampled from the distribution do *not* lie on the curve of means (black).
- The green curve is not exactly the same as the black curve, but is close.
- In this example, the sampled points were mostly below the curve of means.
- Since the regression curve (green) was calculated using just the five sampled points (red), the red points are more evenly distributed above and below it (green curve) than they are in relation to the real curve of means (black).

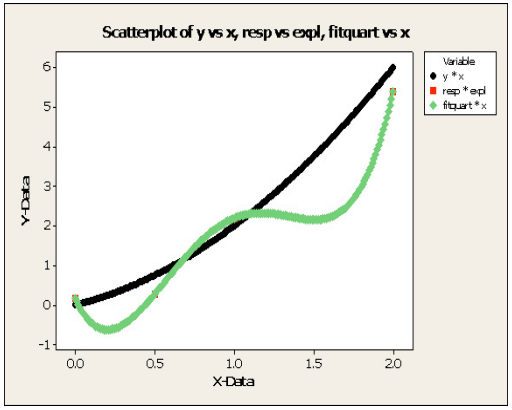
Note: In a real world example, we would *not* know the conditional mean function (black curve) -- and in most problems, would not even know in advance whether it is linear, quadratic, or something else.

- Thus, *part of the problem of finding an appropriate regression curve is figuring out what kind of function it should be*.

Continuing with this example, if we (naively) try to get a "good fit" by trying a quartic (fourth degree) regression curve -- that is, using a model assumption of the form

$$E(Y|X=x) = \alpha + \beta_1 x + \beta_2 x^2 + \beta_3 x^3 + \beta_4 x^4,$$

we get the following picture:



You can barely see any of the red points in this picture.

- That's because they're all on the calculated regression curve (green).
- We've found a regression curve that fits all the data!
- But it's *not* a good regression curve -- because what we're really trying to estimate by regression is the *black curve* (curve of conditional means).
- We've done a rotten job of that; we've made the mistake of *over-fitting*. We've fit an elephant, so to speak.

If we had instead tried to fit a cubic (third degree) regression curve -- that is, using a model assumption of the form

$$E(Y|X=x) = \alpha + \beta_1 x + \beta_2 x^2 + \beta_3 x^3,$$

we'd get something more wiggly than the quadratic fit and less wiggly than the quartic fit.

- However, it would still be over-fitting, since (by construction) the correct model assumption for these data would be a quadratic mean function.

See the Appendix for suggestions on trying to avoid overfitting.

Other Common Mistakes in Using Regression

For further discussion of these mistakes, see links from **http://www.ma.utexas.edu/users/mks/statmistakes/regression.html**

- Using Confidence Intervals when Prediction Intervals Are Needed.
- Over-interpreting High R^2
- Mistakes in Interpretation of Coefficients
  - Interpreting a coefficient as a rate of change in Y instead of as a rate of change in the conditional mean of Y.
  - Not taking confidence intervals for coefficients (i.e., uncertainty of estimation of coefficients) into account
  - Interpreting a coefficient that's not statistically significant
  - Interpreting coefficients in multiple regression with the same language used for a slope in simple linear regression.
  - Neglecting the issue of multiple inference when dealing with more than one coefficient in the same data set.
- Mistakes in Selecting Terms
- Assuming linearity is preserved when variables are dropped. (*See also Appendix.)*
- Problems involving stepwise model selection procedures.

See also *http://www.jerrydallal.com/LHSP/important.htm* for another common mistake in using regression.

If you have further questions, feel free to:

Consult my website Common Mistakes in Using Statistics (table of contents at http://www.ma.utexas.edu/users/mks/statmistakes/TOC.html)

Email me at mks@math.utexas.edu (or through this class's Canvas site)

Leave a comment on my blog, Musings on Using and Misusing Statistics, http://www.ma.utexas.edu/blogs/mks/