

MS&E 226: “Small” Data

Lecture 17: Additional topics in inference (v1)

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Warnings

Modeling assumptions: Regression

Remember that most of the inference we have discussed, e.g., for linear regression, requires certain assumptions to hold:

- ▶ The population model is linear.
- ▶ The errors are normally distributed.
- ▶ The errors are i.i.d., with mean zero.

These assumptions are easily violated; as discussed earlier in the course, many of these kinds of violations can be checked by examining the *residuals*, as well as by considering transformations of the variables.

Modeling assumptions: Hypothesis testing

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So failure to reject does not mean the null is true!

- ▶ Always be precise about your null hypothesis, and what sampling distribution it implies for your estimator.

E.g., in linear regression, to get the sampling distribution of $\hat{\beta}_j$ assuming $\beta_j = 0$, we *still* assume the linear normal model with i.i.d. errors.

Association vs. causation

Even in a correct regression model, significant relationships between covariate(s) and the outcome are evidence of *association* (i.e., correlation in the population model), not *causation*.

Suppose we fit a regression with p covariates X_1, \dots, X_j , and outcome Y .

A *causal* interpretation of $\hat{\beta}_j$ says that *if* we change X_j by 1 unit, *then* (holding other covariates constant) the outcome Y will change by $\hat{\beta}_j$ units.

Association vs. causation

For us to interpret $\hat{\beta}_j$ causally, we need to know that *changes in X_j in our data are (essentially) independent of all other variables* (this is sometimes called *exogeneity*).

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- ▶ *Collinearity*. If X_j is collinear (correlated with) other covariates in our data, then when X_j varies in our data, the reason for variation in Y might actually be associated variation in the correlated covariates.

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- ▶ *Collinearity*. If X_j is collinear (correlated with) other covariates in our data, then when X_j varies in our data, the reason for variation in Y might actually be associated variation in the correlated covariates.
- ▶ *Omitted variable bias*. If a covariate Z is left out of our model that is correlated with X_j and Y , then when X_j varies in our data, the reason for variation in Y might actually be the associated (unobserved) variation in Z .

Association vs. causation

How do we *enforce* exogeneity? Run an experiment where we (the experimenters) vary X_j exogenously at random, and observe the effect on Y .

This is why *randomized experiments* are the benchmark of causal inference.

More warnings

In the next two sections we look at two additional pitfalls:

- ▶ Multiple hypothesis testing
- ▶ Post-selection inference

Multiple hypothesis testing

An example: Multiple linear regression

Suppose that I have n rows of data with outcomes \mathbf{Y} and corresponding covariates \mathbf{X} . Suppose $p = 100$.

I run a linear regression with all the covariates and check statistical significance. I order the resulting covariates in descending order of p-value:

Covariate index	p-value
40	0.0070
58	0.018
93	0.034
69	0.040
57	0.042
10	0.047

You walk away excited: these six coefficients are all significant at the 95% level, and you now have a starting point for building your model.

An example: Multiple linear regression

In fact: *There is no relationship in this data between \mathbf{X} and \mathbf{Y} !*

I used synthetic data to generate this example, with:

- ▶ $Y_i \sim \mathcal{N}(0, 1)$ for each i , i.i.d.
- ▶ $X_{ij} \sim \mathcal{N}(0, 1)$ for each i, j , i.i.d.

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If we use a cutoff of 0.05 to determine whether a coefficient is “statistically significant”, then we are willing to accept a 5% rate of *false positives*: coefficients that look large due to random chance, despite the fact that there is really no underlying relationship.

This means with 100 covariates, we should expect 5 of the coefficients to be significant due to random chance alone — even if there is no effect there! (In our case we get slightly more than this.)

Multiple hypothesis testing

This is a systematic issue with statistical significance based on p-values from individual hypothesis tests:

If you use a cutoff of 0.05 (or 0.01, etc.), you should expect 5% (or 1%, etc.) of your discoveries (rejections) to be false positives.

This applies across all hypothesis tests you do: so for example, if you use a 5% cutoff every day at your job on every test you ever run, you will generate false positives in 5% of your hypothesis tests.

Multiple hypothesis testing

Is this a problem? Perhaps not: if you understand that false positives are generated in this way, you can be wary of overinterpreting significance with many hypothesis tests.

The problem is that interpretation of the results becomes much harder: which results are “trustworthy”, and which are “spurious”?

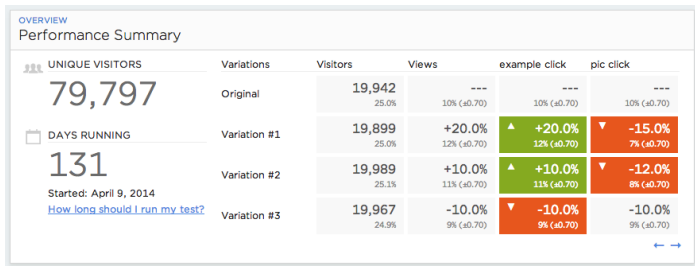
Multiple testing corrections

Multiple testing corrections provide a systematic approach to identifying “meaningful” effects when dealing with many simultaneous hypothesis tests.

This has been an active area of work in the last two decades in statistics, as the range of applications where many hypothesis tests are possible has increased.

Example: The Optimizely results page

Optimizely runs an “A/B testing” platform, a way to test different versions of a website’s design against each other. Here is a typical results page for an experiment:



The screenshot shows a dashboard titled "OVERVIEW Performance Summary". On the left, there are two summary cards: "UNIQUE VISITORS" with a value of 79,797 and "DAYS RUNNING" with a value of 131. Below the days running card, it says "Started: April 9, 2014" and has a link "How long should I run my test?". The main part of the dashboard is a table with columns: Variations, Visitors, Views, example click, and pic click. The table lists four variations: Original, Variation #1, Variation #2, and Variation #3. Each row shows the number of visitors, the percentage of total visitors, and the percentage change in views and clicks for two specific metrics. Green cells indicate a positive change, and red cells indicate a negative change. Small triangles and arrows are used to denote the direction of change.

UNIQUE VISITORS	Variations	Visitors	Views	example click	pic click
79,797	Original	19,942 25.0%	--- 10% (±0.70)	--- 10% (±0.70)	--- 10% (±0.70)
DAYS RUNNING 131 Started: April 9, 2014 How long should I run my test?	Variation #1	19,899 25.0%	+20.0% 12% (±0.70)	▲ +20.0% 12% (±0.70)	▼ -15.0% 7% (±0.70)
	Variation #2	19,989 25.1%	+10.0% 11% (±0.70)	▲ +10.0% 11% (±0.70)	▼ -12.0% 8% (±0.70)
	Variation #3	19,967 24.9%	-10.0% 9% (±0.70)	▼ -10.0% 9% (±0.70)	-10.0% 9% (±0.70)

Real dashboards can have many more simultaneous hypothesis tests present.

Example: Bonferroni correction

The simplest example of a multiple testing correction is the *Bonferroni* correction.

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The Bonferroni correction declares as significant (rejects the null) any hypothesis where the p-value is $\leq 0.05/p$, where p is the number of hypothesis tests being carried out.

In our example, $p = 100$, so only coefficients with p-values ≤ 0.0005 are declared significant — *none* in the example I showed!

Example: Bonferroni correction

Why does the Bonferroni correction work?

- ▶ For a collection of events A_1, \dots, A_p , we have the following bound:

$$\mathbb{P}(A_1 \text{ or } A_2 \text{ or } \dots A_p) \leq \sum_{j=1}^p \mathbb{P}(A_j).$$

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- ▶ So now let A_j be the event that the j 'th coefficient is declared significant, with a cutoff of $0.05/p$ on the p-value. Then:

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$$\mathbb{P}(A_j | \beta_j = 0) \leq \frac{0.05}{p}.$$

- ▶ Finally, suppose that all the β_j 's are in fact zero. Then the probability even one of the A_j 's true is $\leq p \times 0.05/p = 0.05$.

Multiple testing corrections

The Bonferroni correction works by essentially forcing your attention only on the smallest p-values (most significant results).

In practice, though, it can be too conservative, especially as the number of hypotheses (e.g., coefficients) increases.

Other methods have emerged to deal with this issue, to allow valid inference while being somewhat less conservative.

Post-selection inference

Selecting a model

Let's suppose we are building a model using linear regression.

We have $n = 100$ observations \mathbf{Y} , two covariates in the design matrix \mathbf{X} , and two models we consider:

- ▶ In the first model: $Y \sim 0 + X_1$.
- ▶ In the second model: $Y \sim 0 + X_1 + X_2$.

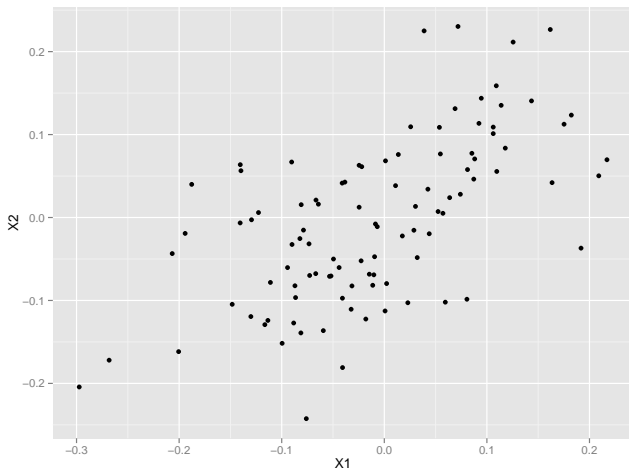
(Ignore the intercept for simplicity.)

Suppose that the design matrix \mathbf{X} is such that the two columns each have norm 1, and their sample correlation is $\approx 2/3$.

Also suppose $Y_i \sim 1.5X_{i2} + \varepsilon_i$, where $\varepsilon_i \sim \mathcal{N}(0, 1)$ (i.i.d.).

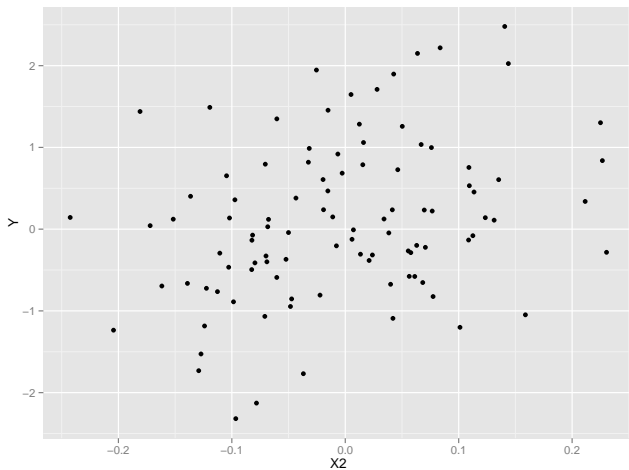
Selecting a model

Example plot of X2 vs. X1:



Selecting a model

Example plot of Y vs. X2:



Selecting a model

We follow this procedure:

1. We check for significance of $\hat{\beta}_2$ in the complete model. If it is significant, we keep it in the model; if it is not, we drop it.
2. We use the resulting fit to estimate $\hat{\beta}_1$, as well as its standard error \widehat{SE}_1 .
3. When done we check if $\hat{\beta}_1$ is statistically significant at the 5% level.

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We find that $\hat{\beta}_1$ is significant in 12.8% of the universes — even though by using a 5% threshold, we should have controlled our false positive rate at 5%!

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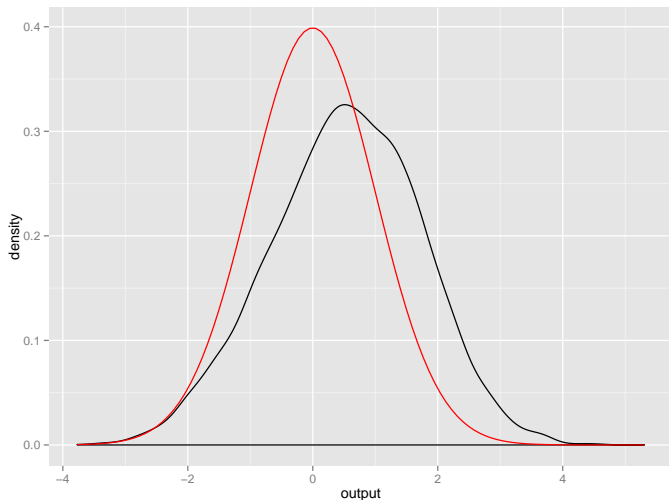
But the universes in which this happens are also *more likely* to be those universes where X_1 does a good job of explaining the variation in Y !

As a result, our selection process makes us more likely to *choose* models in which $\hat{\beta}_1$ is significant, even if the true β_1 is zero; but when we do our hypothesis test, we don't account for this fact.

(Formally, we are doing the hypothesis test assuming that under the null $\hat{\beta}_1$ is $\mathcal{N}(0, \widehat{SE}_1^2)$, when in fact under the null together with our selection process, $\hat{\beta}_1$ is actually more likely to be positive.)

Selecting a model: A picture

The black density plot is the sampling distribution of $\hat{\beta}_1/\widehat{SE}_1$ under our selection process (over 10,000 parallel universes), while the red curve is the $\mathcal{N}(0, 1)$ density:



Post-selection inference

This is an example of *post-selection inference*:

- ▶ You apply lots of procedures to your data to find a model you “like”.
- ▶ *Then* you report your regression table, with p-values, etc.
- ▶ But by doing so, you have *favorably biased* your selection of p-values!

(A less generous term for this type of exercise is “p-value hacking”.)

Post-selection inference

How can we deal with this?

- ▶ We can take into account our selection procedure when we determine the distribution of $\hat{\beta}_1$ under the null hypothesis, and use this “corrected” sampling distribution for significance testing.

Progress is being made on this approach, but it remains less common in most practical settings.

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- ▶ We can recognize that this type of biasing of our results is an issue, and perhaps take with a grain of salt the low p-values and statistical significance we observe in a final model.
- ▶ We can *validate* our findings on new data: Fit the same model on a completely new data set, and compute the p-values and significance levels there.

These are correctly interpretable, because now the model selection is not *based* on the data we are using to check significance.

Concluding thoughts

So now what?

Inference seems loaded with warnings: it is easy to read too much into a statistical analysis, and see “phantom” effects that are not real.

So: how are we meant to build meaningful inferences in practice?

Inference for linear regression

If we've built a meaningful linear regression model, then:

The relationships we declare to be present in our estimated model should be exactly those that are present in the population model.

(*Recall:* Here we are only talking about association, not causation. Additional steps need to be taken to make sure we can interpret a relationship causally.)

External validity

The best evidence that we have a meaningful population model is that it delivers meaningful insight even *outside* of the data on which it was built.

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What it means in practice is that inference does not stop at just one dataset and one modeling approach; often we should ask for corroborating evidence (from other studies, other datasets, other experiments, common sense, etc.) to strengthen the case for a particular relationship.

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