ExpEcon Methods:
Measurement Error & Attenuation Bias in OLS

ECON 8877

P.J. Healy

First version thanks to Changkuk Im

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Instrumental Variable (IV)

to deal with

Measurement Error (ME)

Gillen, Snowberg & Yariv (2019)

- **Measurement errors (ME)** in a lab
  - Participant's attention and focus
  - Rounding due to finite choice menus

- This paper illustrates the issue and proposes a mix of statistical tools (duplicate elicitations and IV approach) and design recommendations
  - Other ways to solve: improve elicitation techniques, multiple rounds, ...

Outline

- Gender gap in competition (**?**)

- Low correlation between different methods of measuring risk (**?**)

- Compound risk and ambiguity are separate phenomena (**???**)

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  - Linearly include controls
  - Principal component analysis
  - Instrumental variables

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- Compound risk and ambiguity are separate phenomena (**???**)

  $\implies$ very little difference btw the two attitudes

  - Obviously related instrumental variables (ORIV)

Measurement Error

Definition
The model $X = X^* + \nu_X$ with X^* and ν_X independent and $\mathrm{E}[\nu_X] = 0$ is known as **classical measurement error**.

Definition
We say that there is **endogeneity** in the linear model $Y = \beta X + \varepsilon$ if β is the parameter of interest and $\mathrm{E}[X\varepsilon] \neq 0$.

Measurement Error

Suppose that we are interested in to estimate the *relationship* between the two variables, Y^* and X^*.

But we can only observe variables measured with independent and identically distributed error,

$$Y = Y^* + \nu_Y \quad \text{and} \quad X = X^* + \nu_X$$

with $\mathrm{E}[\nu_k] = 0$, $\mathrm{Var}[\nu_k] = \sigma^2_{\nu_k}$, and $\mathrm{E}[\nu_Y \nu_X] = 0$.

Measurement Error

The ideal regression model would be

$$Y^* = \alpha^* + \beta^* X^* + \varepsilon^*.$$

Instead, we can only estimate

$$Y = \alpha + \beta X + \varepsilon$$

where α is a constant and ε is a mean-zero random noise.

In this case, we have an **endogeneity problem**.

Measurement Error

Why?

$$
\begin{aligned}
Y^* = \alpha^* + \beta^* X^* + \varepsilon^* &\implies Y - \nu_Y = \alpha^* + \beta^*(X - \nu_X) + \varepsilon^* \\
&\implies Y = \alpha^* + \beta^* X + \underbrace{(\varepsilon^* - \beta^* \nu_X + \nu_Y)}_{=\varepsilon}
\end{aligned}
$$

Hence

$$
\mathrm{E}[X\varepsilon] = \mathrm{E}[(X^* + \nu_X)(\varepsilon^* - \beta^* \nu_X + \nu_Y)] = -\beta^* \sigma^2_{\nu_X} \neq 0
$$

if $\beta^* \neq 0$ and $\sigma^2_{\nu_X} \neq 0$.

Measurement Error

Annotating finite-sample estimates with hats and population moments without hats, we have

$$\hat{\beta} = \frac{\widehat{\mathrm{Cov}}[Y, X]}{\widehat{\mathrm{Var}}[X]} = \frac{\widehat{\mathrm{Cov}}[\alpha^* + \beta^* X^* + \varepsilon^* + \nu_Y, X^* + \nu_X]}{\widehat{\mathrm{Var}}[X^* + \nu_X]}$$

and

$$\mathrm{E}[\hat{\beta}] = \mathrm{plim}_{n \to \infty} \hat{\beta} = \beta^* \underbrace{\left(\frac{\sigma^2_{X^*}}{\sigma^2_{X^*} + \sigma^2_{\nu_X}} \right)}_{<1} < \beta^*.$$

This is called **measurement error bias** or **attenuation bias**.

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Q. Against false positive... Is this a big problem?

Simulated Example

	Error as a percentage of Var[X] and Var[Y]					
	0%	10%	20%	30%	40%	50%
$\widehat{\text{Corr}}[X, Y]$	1.00	0.90***	0.80***	0.70***	0.60***	0.50***
	(0.00)	(0.02)	(0.04)	(0.05)	(0.06)	(0.08)
$\widehat{\text{Corr}}[\text{E}[X], \text{E}[Y]]$	1.00	0.95***	0.89***	0.82***	0.75***	0.66***
	(0.00)	(0.01)	(0.02)	(0.03)	(0.04)	(0.06)
ORIV $\widehat{\text{Corr}}[X, Y]$	1.00	1.00	1.00	1.00	1.00	1.00
	(0.00)	(0.01)	(0.02)	(0.04)	(0.06)	(0.10)

* Coefficients and standard errors are averages from 10,000 simulated regressions ($N = 100$).

$\Longrightarrow$ Even a bit of ME causes significant deviations from the true correlation of 1, i.e., $\text{Corr}[X^*, Y^*] = 1$.

Two Replicated Measures

Suppose that we elicit two replicated measures of X^*, i.e.,

$$X^a = X^* + \nu_X^a \quad \text{and} \quad X^b = X^* + \nu_X^b$$

with ν_X^a, ν_X^b i.i.d. random variables, and $\mathrm{E}[\nu_X^a \nu_X^b] = 0$.

Two-Stage Least Squares

Apply two-stage least squares (2SLS) to instrument X^a with X^b,

$$X^{a} = \pi_0 + \pi_1 X^{b} + \varepsilon_X \implies \hat{\pi}_1 = \frac{\widehat{\text{Cov}}[X^a, X^b]}{\widehat{\text{Var}}[X^b]} \approx \frac{\widehat{\text{Var}}[X^*]}{\widehat{\text{Var}}[X^b]}.$$

Then estimate $Y = \alpha + \beta(\hat{\pi}_0 + \hat{\pi}_1 X^b) + \varepsilon_Y$.

$$\hat{\beta} = \frac{\widehat{\text{Cov}}[\alpha^* + \beta^* X^* + \varepsilon^* + \nu_Y, \hat{\pi}_0 + \hat{\pi}_1 X^b]}{\widehat{\text{Var}}[\hat{\pi}_0 + \hat{\pi}_1 X^b]} \approx \frac{\beta^* \hat{\pi}_1 \widehat{\text{Var}}[X^*]}{(\hat{\pi}_1)^2 \widehat{\text{Var}}[X^b]} \to_p \beta^*.$$

Thus, $\hat{\beta}$ is a consistent estimate of β^*.

Instrumentation strategies

Q. Do we instrument X^a with X^b, or X^b with X^a?

- They may produce different results (see Table 5 in the paper).

A. The **obviously related IV (ORIV)** estimator consolidates the information from these different formulations.

ORIV

The ORIV regressions estimates a *stacked model* to consolidate the information from the two available instrumentation strategies:

$$\begin{bmatrix} Y \\ Y \end{bmatrix} = \begin{bmatrix} \alpha_1 \\ \alpha_2 \end{bmatrix} + \beta \begin{bmatrix} X^a \\ X^b \end{bmatrix} + \varepsilon,$$

instrumenting

$$\begin{bmatrix} X^a \\ X^b \end{bmatrix} \text{ with } W = \begin{bmatrix} X^b & 0_N \\ 0_N & X^a \end{bmatrix}$$

where N is the number of participants and 0_N is an $N \times 1$ zero matrix.

ORIV

This is equivalent to estimating a first stage for both instrumentation strategies, then estimating

$$\begin{bmatrix} Y \\ Y \end{bmatrix} = \begin{bmatrix} \alpha_1 \\ \alpha_2 \end{bmatrix} + \beta \begin{bmatrix} \hat{X}^{a} \\ \hat{X}^{b} \end{bmatrix} + \varepsilon.$$

The stacked regression will produce an estimated of β^* that is the *average* of the estimates from the two instrumentation approaches.

ORIV

Proposition 1
ORIV produces consistent estimates of β^*.

Proposition 2
The ORIV estimator satisfies asymptotic normality under standard conditions. The estimated standard errors, when clustered by participant, are consistent estimates of the asymptotic standard errors.

Errors in Outcome and Explanatory Variables

Suppose that $Y^a = Y^* + \nu_Y^a$, $Y^b = Y^* + \nu_Y^b$, with $\mathrm{E}[\nu_Y^a] = \mathrm{E}[\nu_Y^b] = 0$.

$$\begin{bmatrix} Y^a \\ Y^a \\ Y^b \\ Y^b \end{bmatrix} = \begin{bmatrix} \alpha_1 \\ \alpha_2 \\ \alpha_3 \\ \alpha_4 \end{bmatrix} + \beta \begin{bmatrix} X^a \\ X^b \\ X^a \\ X^b \end{bmatrix} + \varepsilon, \text{ with } W = \begin{bmatrix} X^b & 0_N & 0_N & 0_N \\ 0_N & X^a & 0_N & 0_N \\ 0_N & 0_N & X^b & 0_N \\ 0_N & 0_N & 0_N & X^a \end{bmatrix}.$$

* The existence of ME in Y does not change propositions 1 and 2, although estimated standard errors will increase.

Estimating Correlations

Note that

$$\hat{\beta} = \frac{\widehat{\mathrm{Cov}}[X, Y]}{\widehat{\mathrm{Var}}[X]} \implies \hat{\rho}_{XY} = \hat{\beta}\sqrt{\frac{\widehat{\mathrm{Var}}[X]}{\widehat{\mathrm{Var}}[Y]}}.$$

We cannot use $\mathrm{Var}[X] = \mathrm{Var}[X^*] + \mathrm{Var}[\nu_X]$.

Instead, use $\mathrm{Cov}[X^a, X^b] = \mathrm{Cov}[X^* + \nu^a{}_X, X^* + \nu^b{}_X] = \mathrm{Var}[X^*]$.
Thus,

$$\hat{\rho}^*_{XY} = \hat{\beta}^*\sqrt{\frac{\widehat{\mathrm{Cov}}[X^a, X^b]}{\widehat{\mathrm{Cov}}[Y^a, Y^b]}}.$$

Estimating Correlations

Proposition 3
$\hat{\rho}^*_{XY}$ is consistent with an asymptotically normal distribution, where standard errors can be derived using the delta method.

These standard errors can be consistently estimated using a bootstrap to construct confidence intervals.

Designing Experiments for ORIV

Assumption. MEs are *independent* across elicitations.

Q. How to design an experiment to achieve this?

A. The paper suggests:

1. Duplicated elicitations should use different numerical values.
2. When using an MPL, the response grid should be constructed so that implied values are not the same.
3. Duplicated items should be placed in different parts of the study.

Measures of Risk

- **Project**
  - Allocate 100 or 200 tokens btw a safe option and a project (e.g., returning 3 tokens w.p. 0.4 or nothing otherwise).
  - **?**
- **Qualitative**
  - Self-rate, on a scale of $0 - 10$, in terms of willingness to take risk.
  - **?**
- **Lottery menu**
  - Choose btw six 50/50 lotteries with different stakes.
  - **?**
- **MPLs**
  - E.g., 100 tokens w.p. $\frac{10}{20}$; 150 tokens w.p. $\frac{15}{30}$.

Overconfidence

Compound and Ambiguity

Correlation Results

	Raw Correlation			Corrected for ME		
	Project	Qualitative	Lottery	Project	Qualitative	Lottery
Qualitative	0.26***			0.40***		
	(0.029)			(0.043)		
Lottery	0.47***	0.25***		0.71***	0.40***	
	(0.029)	(0.032)		(0.046)	(0.052)	
Risk MPL	0.19***	0.13***	0.22***	0.30***	0.19***	0.38***
	(0.032)	(0.033)	(0.030)	(0.048)	(0.047)	(0.053)

1. The corrected correlations are substantially higher.
2. Some measures are noticeably more correlated.
    - Project is most correlated with others (e.g., Project & Lottery).
    - MPL & Qualitative are least correlated.

Misspecified Controls and ME

Y: Competition
D: Gender
X: Controls for risk aversion and overconfidence

Consider a regression model:

$$Y = \alpha D + X'\beta + \varepsilon.$$

Consider the model:

- $Y^* = X^*$;
- D and X^* are correlated;
  - Overconfidence & gender (**?**)
  - Risk aversion & gender (**??**)
- $X = X^* + \nu$.

We may have an erroneous conclusion that Y and D are correlated, even when controlling for X. e.g., simulation

Replication Results

	Chose to Compete ($N = 783$)		
	(1)	(2)	(3)
Male	0.19^{***}	0.11^{***}	0.048
Risk aversion: MPL #1		0.042^{***}	
Overplacement: CRT		0.026^{***}	
Risk aversion: project #2			0.067^{***}
Perceived performance: CRT			-0.042^{***}

* Guessed tournament rank, Tournament performance, Performance difference are controlled in (2) and (3).

- (1) and (2) replicate **?**.
- A different set of controls in (3) provides different result.
  - Statistical significance of controls is not a good indicator of whether a trait is fully controlled for.

Three Approaches to Solve

1. Include **multiple measures** for each of the possible controls X.
    - Cannot eliminate the effects of ME without a large enough number of controls. But how many?
2. Include **principal components** of the multiple controls.
3. **Instrument** each control with a duplicate.
    - Two controls are enough.

The paper suggests that the IV approach is preferable whenever feasible.

Regression Results

	Chose to Compete ($N = 783$)			
	(1)	(Sol 1)	(Sol 2)	(Sol 3)
Male	0.19^{***}	0.050	0.041	0.0063
6 risk aversion controls		$F = 4.9$		
12 overconfidence controls		$F = 1.8$		
Five Principal components			$F = 37$	
Instrumental variables				$\chi^2_7 = 24$

* (Sol 1) has 76 controls in total including Guessed tournament rank, Tournament performance, Performance difference.

$\Longrightarrow$ Estimated coefficients of gender variable is no more statistically significant.

Analysis of Variance (ANOVA)

Motivation

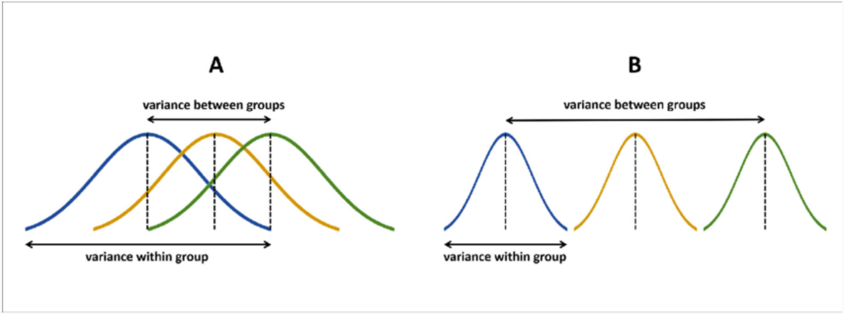

Figure 4 from **?**

Assumptions

Suppose that there are k groups of interest.

Assumptions (**?**):

1. The data must be measured either on interval or ratio scale.
2. The samples must be independent.
3. The dependent variable must be normally distributed.
4. The population from which the samples have been drawn must be normally distributed.
5. The variances of the population must be equal ($\sigma_1^2 = \cdots = \sigma_k^2 = \sigma^2$).
6. The errors are independent and normally distributed.

Hypothesis:

$$H_0 : \mu_1 = \cdots = \mu_k.$$

$$H_a : \text{At least one } \mu_i \text{ is different.}$$

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One-Way ANOVA

Consider a *between-subject* design ($1 \sim k$ treatments).
Each treatment j has n_j number of (i.i.d.) observations.
Let $n = \sum_{j=1}^{k} n_j$ be the total number of observations.

Define

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Define

- X_{ij}: i-th observation in treatment j
- $\bar{X}_j = \frac{\sum_{i=1}^{n_j} X_{ij}}{n_j}$: sample mean in treatment j ($j = 1, \ldots, k$).
- $\bar{X} = \frac{\sum_{j=1}^{k} \sum_{i=1}^{n_j} X_{ij}}{n}$: grand sample mean.

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$TSS = \sum_{j=1}^{k} \sum_{i=1}^{n_j} (X_{ij} - \bar{X})^2$: Total sum of squares.

$SS_b = \sum_{j=1}^{k} n_j(\bar{X}_j - \bar{X})^2$: Sum of squares between groups.

$SS_w = \sum_{j=1}^{k} \sum_{i=1}^{n_j} (X_{ij} - \bar{X}_j)^2$: Sum of squares within groups.

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- $TSS = SS_b + SS_w$

One-Way ANOVA

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$SS_b = \sum_{j=1}^{k} n_j (\bar{X}_j - \bar{X})^2$: Sum of squares between groups.

$SS_w = \sum_{j=1}^{k} \sum_{i=1}^{n_j} (X_{ij} - \bar{X}_j)^2$: Sum of squares within groups.

- $MSS_b = \frac{SS_b}{k-1}$: Mean sum of squares for between groups.
- $MSS_w = \frac{SS_w}{n-k}$: Mean sum of squares for within groups.

Test statistic:

$$F = \frac{MSS_b}{MSS_w} \sim F_{k-1, n-k}.$$

One-Way ANOVA

Under the null, both MSS_b and MSS_w are unbiased estimator of σ^2.

If H_0 does not hold, then

- MSS_b is NOT an unbiased estimator of σ^2 *(biased upward)*.
- MSS_w is an unbiased estimator of σ^2.

⏩ Details

ANOVA Table

Sources of variation	SS	df	MSS	F-value
Between groups	SS_b	$k-1$	$MSS_b = \frac{SS_b}{k-1}$	$F = \frac{MSS_b}{MSS_w}$
Within groups	SS_w	$n-k$	$MSS_w = \frac{SS_w}{n-k}$	
Total	TSS	$n-1$		

Example

Switch points from three different MPLs (MPL 0, MPL 1, MPL 2):

```
. anova switch_average treatment

                          Number of obs =        119    R-squared     =  0.0827
                          Root MSE      =    1.16963    Adj R-squared =  0.0669

                   Source |  Partial SS         df         MS        F    Prob>F
              ------------+----------------------------------------------------
                    Model |   14.309397          2   7.1546983      5.23  0.0067
                          |
                treatment |   14.309397          2   7.1546983      5.23  0.0067
                          |
                 Residual |   158.69244        116   1.3680383
              ------------+----------------------------------------------------
                    Total |   173.00183        118   1.4661172
```

$\Longrightarrow$ Switch points from at least one MPL are different.

Beyond One-Way ANOVA

- **Two-Way ANOVA**: Compare more than one group
  - E.g., (stata) `anova switch_average treatment gender`

    ⏩ Screenshot

- **Analysis of covariance (ANCOVA)**: Controls for covariates

Regression with Dummy Variables

Regress Y on **dummy variables** can do the same thing!

For example,

$$Switch_i = \beta_0 + \delta_1 D_{i,1} + \delta_2 D_{i,2} + \varepsilon_i$$

where

$$D_{i,k} = \begin{cases} 0 \text{ if MPL } 0 \\ 1 \text{ if MPL } k \end{cases} .$$

Example

```
. reg switch_average i.treatment

      Source |       SS           df       MS      Number of obs   =       119
-------------+----------------------------------   F(2, 116)       =      5.23
       Model |  14.3093965         2  7.15469827   Prob > F        =    0.0067
    Residual |  158.692438       116  1.36803826   R-squared       =    0.0827
-------------+----------------------------------   Adj R-squared   =    0.0669
       Total |  173.001834       118  1.46611724   Root MSE        =    1.1696

------------------------------------------------------------------------------
switch_ave~e |      Coef.   Std. Err.      t    P>|t|     [95% Conf. Interval]
-------------+----------------------------------------------------------------
   treatment |
          1  |  -.7929842   .2537463    -3.13   0.002    -1.295561   -.2904076
          2  |  -.2035062    .269831    -0.75   0.452    -.7379405     .330928
             |
       _cons |   2.682317   .1804781    14.86   0.000     2.324858    3.039777
------------------------------------------------------------------------------
```

1. Switch points btwn MPL0 and MPL1 are significantly different.
2. Switch points btwn MPL0 and MPL2 are not sig. different.

Discussion

1. We can put more structures when regressing w/ dummy variables.
    - Control variables, cluster se, nonlinear regression, ...

2. When regressing w/ dummy variables, we need a *control* treatment.
    - E.g., MPL1 vs MPL2?
    - If we want to compare two groups, maybe do pairwise comparison with corrections?

ANOVA vs Regression w/ Dummy Variables

My takeaways are...

- To show the *overall* difference across multiple groups, use **ANOVA**.

- To put more *structures* (control variables, cluster se, nonlinear regression), regress with **dummy variables** with a proper specification.
    - E.g., we often do t-test to show the overall difference between two groups, and then run regressions to have further analysis.

The End

Appendix

- **Overestimation and overplacement**
  - How many they think they answered correctly.
  - Where they think they are in the performance distribution of all participants
- **Overprecision**
  - How confident they are of their guess (six-point qualitative scale).
- **Perception of academic performance**
  - Where in the grade distribution of their entering cohort they believe they would fall over the next year.

⏩ Caltech Cohort Study: Risk measures

Appendix

- **Compound MPL**
  - Same as MPL except that the number of balls is uniformly drawn.
- **Ambiguous MPL**
  - Same as MPL except that the composition of the urn was chosen by the dean of undergraduate students of Caltech.

⏩ Caltech Cohort Study: Risk measures

Appendix: Simulated Example

Y: Participation in dangerous sports
D: Gambling
X: Risk attitude (experimentally measured)

Consider a model:

- $Y^* = X^*$;
    - $X^* \sim N[0, 1]$ and $Y = Y^* + \zeta$ where $\zeta \sim N[0, 1]$.
- $D = 0.5 \cdot X^* + \eta \quad$ where $\eta \sim N[0, 0.9]$;
- $X = X^* + \nu \quad$ where $\nu \sim N[0, \sigma_\nu^2]$.

Consider a regression model:

$$Y = \alpha D + \beta X + \varepsilon$$

Model

Appendix: Simulated Example

Tabelle 1: Simulated Regressions

	ME as a Percent of Var[X], i.e., $\frac{\sigma_\nu^2}{\sigma_\nu^2+\sigma_{X*}^2}$					
	0%	10%	20%	30%	40%	50%
$\hat{\alpha}$	.00	.06	.11	.16	.21*	.26***
	(.11)	(.11)	(.12)	(.12)	(.12)	(.12)
$\hat{\beta}$	1.00***	.87***	.75***	.64***	.54***	.44***
	(.12)	(.11)	(.11)	(0.10)	(.10)	(.09)

True model: $\alpha = 0$ and $\beta = 1$.

Model

Appendix: F-statistic for One-Way ANOVA

Consider $X_{ij} = \mu_j + \varepsilon_{ij}$, $\varepsilon_{ij} \sim N(0, \sigma^2)$ and independent.
We can rewrite it as

$$X_{ij} = \mu + \alpha_j + \varepsilon_{ij}$$

where $\mu = \frac{1}{k}\sum_{j=1}^{k} \mu_j$, and α_j is called sample effect.
Under $n_j = m$, we have

$$\mathrm{E}[SS_b] = (k-1)\sigma^2 + m\sum_{j=1}^{k} \alpha_j^2 \quad \text{and} \quad \mathrm{E}[SS_w] = k(m-1)\sigma^2.$$

Hence, MSS_b is an unbiased estimator of σ^2 only if the null ($\alpha_1 = \cdots = \alpha_k = 0$) is true, while MSS_w is an unbiased estimator regardless of the null.
In particular, MSS_b gets larger as α_j increases.

Since $MSS_b/\sigma^2 \sim \chi^2_{k-1}$, $MSS_w/\sigma^2 \sim \chi^2_{n-k}$, and they are independent (by Cochran theorem), $F = \frac{MSS_b}{MSS_w} \sim F_{k-1,n-k}$. Slide: One-Way ANOVA

Appendix: Two-Way ANOVA

```
. anova switch_average treatment gender

                         Number of obs =        115    R-squared     =  0.0734
                         Root MSE      =    1.09278    Adj R-squared =  0.0484

                  Source | Partial SS         df         MS        F    Prob>F
              -----------+----------------------------------------------------
                   Model |  10.503141          3    3.501047      2.93  0.0367
                         |
               treatment |   10.37179          2   5.1858948      4.34  0.0153
                  gender |  .00133147          1   .00133147      0.00  0.9734
                         |
                Residual |  132.55193        111   1.1941615
              -----------+----------------------------------------------------
                   Total |  143.05507        114    1.254869
```

▶▶ Slide: Beyond One-Way ANOVA

Literatur