

# **ExpEcon Methods: Measurement Error & Attenuation Bias in OLS**

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ECON 8877

P.J. Healy

First version thanks to Changkuk Im

Updated 2026-01-28

# Instrumental Variable (IV)

to deal with

## Measurement Error (ME)

But aren't IVs for applied folks???

- **Measurement errors (ME)** in the lab
  - Participant's attention and focus
  - Rounding due to finite choice menus
  - Measures of risk/ambiguity aversion
- 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, ...

- 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
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- Compound risk and ambiguity are separate phenomena (???)
  - ⇒ very little difference btw the two attitudes
    - Obviously related instrumental variables (ORIV)

# Measurement Error

## Definition

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

## Definition

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



# 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  $E[\nu_k] = 0$ ,  $\text{Var}[\nu_k] = \sigma_{\nu_k}^2$ , and  $E[\nu_Y \nu_X] = 0$ .

# Measurement Error

The ideal regression model would be

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

Instead, we can only estimate

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

where  $\alpha$  is a constant and  $\varepsilon$  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

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

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

# Measurement Error

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

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

and

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

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

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

⇒ 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  $\nu_X^a, \nu_X^b$  i.i.d. random variables, and  $E[\nu_X^a \nu_X^b] = 0$ .

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

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

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



# 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.

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

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

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

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  $\beta^*$  that is the *average* of the estimates from the two instrumentation approaches.

**Proposition 1**

ORIV produces consistent estimates of  $\beta^*$ .

**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  $E[\nu_Y^a] = E[\nu_Y^b] = 0$ .

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

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

# Estimating Correlations

Note that

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

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

Instead, use  $\text{Cov}[X^a, X^b] = \text{Cov}[X^* + \nu_X^a, X^* + \nu_X^b] = \text{Var}[X^*]$ .

Thus,

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

# 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*** (0.029)			0.40*** (0.043)		
Lottery	0.47*** (0.029)	0.25*** (0.032)		0.71*** (0.046)	0.40*** (0.052)	
Risk MPL	0.19*** (0.032)	0.13*** (0.033)	0.22*** (0.030)	0.30*** (0.048)	0.19*** (0.047)	0.38*** (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***

\* Gussed 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.

⇒ Estimated coefficients of gender variable is no more statistically significant.

# Analysis of Variance (ANOVA)

# Motivation

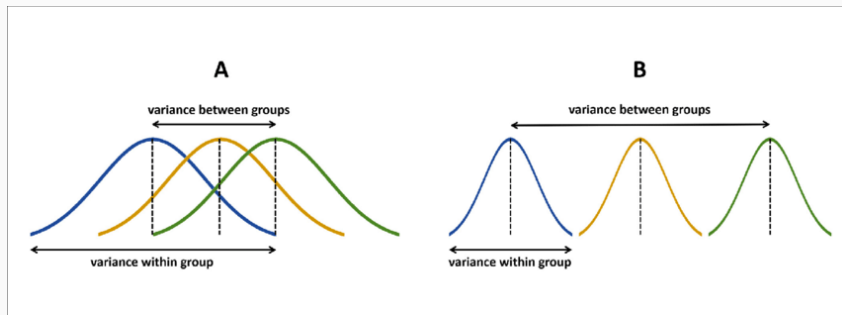


Figure 4 from ?



# 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 ( $\sigma_1^2 = \dots = \sigma_k^2 = \sigma^2$ ).
6. The errors are independent and normally distributed.

## Hypothesis:

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

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

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$H_a$  : At least one  $\mu_i$  is different.

# 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.

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$X_{ij}$ :  $i$ -th observation in treatment  $j$

$\bar{X}_j = \frac{\sum_{i=1}^{n_j} X_{ij}}{n_j}$ : sample mean in treatment  $j$  ( $j = 1, \dots, k$ ).

$\bar{X} = \frac{\sum_{j=1}^k \sum_{i=1}^{n_j} X_{ij}}{n}$ : 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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$SS_w = \sum_{j=1}^k \sum_{i=1}^{n_j} (X_{ij} - \bar{X}_j)^2$ : 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  $\sigma^2$ .

If  $H_0$  does not hold, then

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

►► 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		

⇒ 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,

$$\text{Switch}_i = \beta_0 + \delta_1 D_{i,1} + \delta_2 D_{i,2} + \varepsilon_i$$

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
Model	14.3093965	2	7.15469827	F(2, 116)	=	5.23
Residual	158.692438	116	1.36803826	Prob > F	=	0.0067
				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 MPLO and MPL1 are significantly different.
2. Switch points btwn MPLO 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



- **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

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

Consider a regression model:

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

# Appendix: Simulated Example

**Table 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 (.11)	.06 (.11)	.11 (.12)	.16 (.12)	.21* (.12)	.26*** (.12)
$\hat{\beta}$	1.00*** (.12)	.87*** (.11)	.75*** (.11)	.64*** (0.10)	.54*** (.10)	.44*** (.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  $\alpha_j$  is called sample effect.

Under  $n_j = m$ , we have

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

Hence,  $MSS_b$  is an unbiased estimator of  $\sigma^2$  only if the null ( $\alpha_1 = \dots = \alpha_k = 0$ ) is true, while  $MSS_w$  is an unbiased estimator regardless of the null.

In particular,  $MSS_b$  gets larger as  $\alpha_j$  increases.

Since  $MSS_b/\sigma^2 \sim \chi_{k-1}^2$ ,  $MSS_w/\sigma^2 \sim \chi_{n-k}^2$ , and they are independent (by Cochran theorem),  $F = \frac{MSS_b}{MSS_w} \sim F_{k-1, n-k}$ .

## 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	<b>10.503141</b>	<b>3</b>	<b>3.501047</b>	<b>2.93</b>	<b>0.0367</b>
treatment	<b>10.37179</b>	<b>2</b>	<b>5.1858948</b>	<b>4.34</b>	<b>0.0153</b>
gender	<b>.00133147</b>	<b>1</b>	<b>.00133147</b>	<b>0.00</b>	<b>0.9734</b>
Residual	<b>132.55193</b>	<b>111</b>	<b>1.1941615</b>		
Total	<b>143.05507</b>	<b>114</b>	<b>1.254869</b>		

