Master Advanced Module: Empirical Banking and Finance

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Summer term 2019

Monday 12:15h - 13:45h, Juridicum Hörsaal N
Monday 16:15h - 17:45h, Juridicum Hörsaal F

Lecture Notes

Overview for today's lecture

I. Introduction

- Organizational issues
- Topic and objectives of the course
- Some introductory remarks about empirical research
- Structure of the course
- Exam

Organizational issues

- Organization:
  - Monday, 12:15-13:45h, room Hörsaal N
  - Monday, 16:15-17:45h, room Hörsaal F
  - Level: Master in Economics (advanced module), first/ second year of Ph.D. (selected M.Sc. course)
  - Grading: written exam (100%)
- Contact details
  - Generally, we can talk after class.
  - For an appointment, please send me an e-mail with your request.
  - Office: Juridicum Altbau/ first floor (on the right)
  - e-Mail: schuewer@uni-bonn.de
- Course material
  - eCampus course: "Empirical Banking and Finance"
  - Password: ...
  - All lecture slides will be online today
- Prerequisites
  - A sound knowledge in econometrics at the level of the basic module "Econometrics" is required.

Introduction round

- Some info about me
- Who are you?
- Did you take any theoretical or empirical Banking/ Finance course before?
- What is your background in econometrics?
  - Basic module "Econometrics"?
  - Empirical projects, e.g., for your Bachelor thesis?
  - Other
- What do you expect from this course?

Topic of the Course

- This course deals with current **empirical research** in the area of banking and finance
- Students become familiar with the **econometric methods** used in this area of research and learn how to *critically assess* empirical research articles
- Lectures are complemented by **empirical tutorials** using the software **Stata**
- An integral part of the course is the *active participation* of students, including discussions and **paper presentations**

Objectives of the Course

- At the end of this course, students should be able to *read, understand, and critically assess empirical research articles* in the area of banking and finance
- A critical assessment of an empirical paper requires ...
  - ...some knowledge of *economic theory*
    $\Rightarrow$ e.g., "Banking & Securitization" by Dr. Eva Schliephake
  - ...good knowledge of *econometric methods*
  - ...an understanding of *potential pitfalls* in empirical analyses

Some introductory remarks about empirical research

Why Is Empirical Work Important?

- Economic **theory** tells us what the world *could* look like
- It can never tell us what the world *really* looks like
- To answer this question, we need real-world **data**
- Question: Are the observed data *consistent* with the proposed theory?
- Note: Even a highly plausible theory does not need to be true

Examples about theory & empirical evidence 1/2

Theory: Monetary incentives increase effort

- Experiment: School students who collected donations for a charity in a door-to-door fund-raising campaign were compensated for it (Gneezy and Rustichini, 2000a).
- Potential outcome: large comp. $>>$ small comp. $>>$ no comp
- Real outcome: large comp. $>>$ no comp. $>>$ small comp

Empirical evidence helps to better understand existing theories or to develop new theories (here: intrinsic vs. extrinsic motivation)

Examples about theory & empirical evidence 2/2

Theory: Fines prohibit misbehavior

- Experiment: A daycare was unhappy with late-coming parents, and a fine of $3 was introduced (Gneezy and Rustichini, 2000b).
- Potential outcome: Parents make more efforts to bring their kids in time and late-coming decreases.
- Real outcome: late-coming increases
- Potential explanation: $3 fine represents new information on how bad it is to be late.
- What happens if the fine is removed?
- Interestingly, parents continued to be late (because the message that being on time is not that important did not disappear).

* See Gneezy, Meier and Rey-Biel (JEP 2011) for an overview on interesting studies related to monetary incentives.

Exemplary Research Questions in Banking and Finance

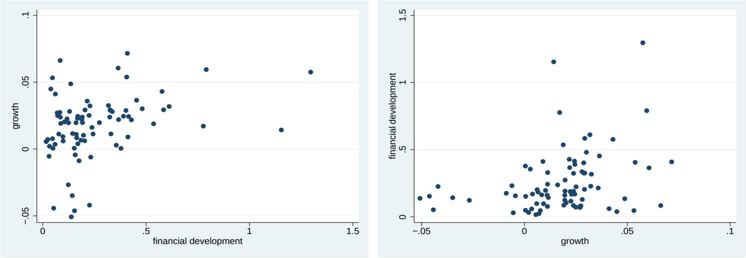
- Does financial development matter for economic growth?
- Does the banking system matter for economic growth (e.g., public ownership of banks, "Hausbank"lending)?
  - Does bank management compensation matter for bank risk-taking?
  - Does bank management compensation matter for financial stability?
    - What is the effect of management compensation via stock-options on bank risk-taking/ financial stability?
  - Does the design of the deposit insurance matter for bank risk-taking?
    - What is the effect of capital buffers on the effect of deposit insurance on bank risk-taking?
- ...

What are the respective empirical challenges?

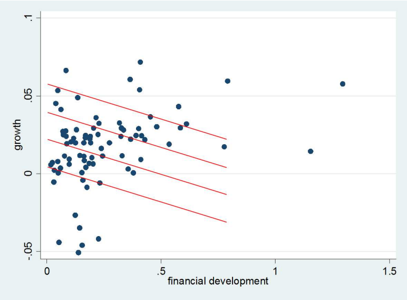
How do you interpret the following data?

Observations from several countries over several years.

- "growth" represents annual GDP growth
- "financial development" represents the annual ratio of private credit to GDP



How do you interpret the following data? (cont.)



Key questions (cont.)

- Do we observe *statistically and economically significant relations* between specific variables, e.g., financial development and economic growth?
- Do we observe *correlations* or *causality*?
  - Does the analysis include a plausible *counterfactual*?
  - What would be the *ideal experiment* – and how does it differ from the presented one?

Pitfalls in Empirical Research

- Empirical data are often *misused* (cf. the presentation of "facts" in talk shows) and are often subject to different interpretations
- Example: The number of children receiving public income assistance has increased – does this mean that children are better off or worse off?
- Hence, one should always be sceptical when empirical results are presented:
  - How good are the **data**?
  - Is the **methodology** appropriate?
  - Is the **interpretation** appropriate?
- Note: Almost all empirical researchers *overinterpret* their empirical results

Incentive Problems in Empirical Research

- *Insignificant* results can hardly be published
  $\Rightarrow$ Researchers search the data until they find something (and present the one out of 100 specifications where the variable of interest was significant) = **data mining**
- Unspectacular results cannot easily be published
  $\Rightarrow$ Researchers search for *surprising* results
- The details of the empirical analysis can hardly be checked
  $\Rightarrow$ Some researchers do not spend much time with *tedious data work*, even *data manipulation* can hardly be discovered

Please find arguments why erroneous data or incentive problems may be the bigger problem in empirical research.

Famous flaws in empirical work

- For the 1936 US presidential elections, a magazine had polled a sample of over 2 million people based upon telephone and car registrations. They predicted a landslide win for Landon with 57% vs. 43%.
  - Actually, Roosevelt won with 61% vs. 37%.
- Piketty (2013/2014), "Capital in the Twenty-First Century": Wealth inequality decreased between 1930 and 1975, but has increased again since then.
  - FT finds wrong data in the study.
- Reinhart-Rogoff (2010), "Growth in a Time of Debt": A country's annual growth declines by 2 percent when gross external debt reaches 60 percent of GDP.
  - Doctoral student finds "spreadsheet error" where the authors average 15 out of the 20 counties instead of 20 out of the 20 countries as well as some rather arbitrary sample and data decisions.
- A paper by Petersen (2009) finds that ...
  42% of papers published in top finance journals between 2001 and 2004 did not adjust the standard errors for possible dependences in the residuals (e.g., serial correlation).

Quality Control in Empirical Work

- Some journals now require the authors to *post their data sets* on the journal's webpage (but: these data sets may already be of poor quality)
- Students from all over the world try to *replicate* published papers
- But: A real check would also require that the data is *re-collected*, which is often not done
- There exist journals that specialize on the replication of empirical studies
- The best quality control is the *competition among researchers*, which hopefully helps to refute "wrong" results
- An empirical result is established only if several research groups have confirmed the result *independently* based on *different data sets*

Course overview 1/2

I Introduction

II Econometric foundations

Wooldridge Ch. 1-9

- Linear regressions (recap)
- Internal and external validity (recap)
- Data (recap)
- Interaction Terms

III Economic Foundations of Banking and Finance

- Theory of banking and financial intermediation

IV Cross-Sectional Regressions

- Goldsmith (1969), Financial Structure and Development
- King and Levine (1993), Finance and growth: Schumpeter might be right.

V Instrumental Variables Estimation

Wooldridge Ch. 15, 16.1-16.4

- La Porta et al. (1997), Legal Determinants of External Finance
- La Porta et al. (1998), Law and Finance
- Levine, Loayza and Beck (2000), Financial Intermediation and Growth: Causality and Causes

Course overview 2/2

VI Panel Methods/ Fixed-Effects Estimation

Wooldridge Ch. 13, 14

- De Haas and Van Horen (2012), Running for the Exit? International Bank Lending During a Financial Crisis

VII Differences-in-Differences Estimation

Wooldridge Ch. 13

- Jayaratne and Strahan (1969), The Finance-Growth Nexus: Evidence from Bank Branch Deregulation
- Lambert, Noth and Schüwer (2019), How do banks react to catastrophic events? Evidence from Hurricane Katrina

VIII The Method by Rajan/ Zingales (1998)

- Rajan and Zingales (1998), Financial Dependence and Growth

IX Further topics

Schedule

- A current version of the schedule is provided on eCampus.

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	12:15-12:45h, Hörsaal N				
	16:15-17:45h, Hörsaal F				
	LECTURES		TUTORIALS	STUDENT PRESENTATIONS	
.04.2019	Introduction				
	Econometric foundations II.1 Linear regressions (recap)	30-49			
.04.2019	Econometric foundations II.2-3 Internal and external validity/ Data (recap)	50-65, 66-77			
	Econometric foundations II.4 Interaction terms	79-98			
.04.2019	Econometric foundations II.4 Interaction terms (cont.)	example II		Intro: Rablen and Oswald (JHE 2008)	tbd
			T1: Introduction to Stata		
.04.2019	Easter Monday				
.04.2019	Economic foundations	100-128		Intro: Rubinstein (2006)	tbd
	Cross-sectional regressions	129-156			
.05.2019			T2: Functional form and interaction terms		
				Student presentations	tbd
.05.2019	Pentecost break (*Pfingsten*)				
	Pentecost break (*Pfingsten*)				
.05.2019	IV	157-190			
	IV (cont.)			IV: Acemoglu, Johnson, Robinson (2001)	tbd
.05.2019			T3: Instrumental variables estimation		
	IV (cont.)			IV: Peek and Rosengreen (2000)	tbd
				IV: La Porta et al. (2002)	tbd
.06.2019	Panel/ Fixed-Effects	191-226			
	Panel/ Fixed-Effects (cont.)			Panel/ FE: Beck, Behr and Güttler (2013)	tbd
				Panel/ FE: Haselmann, Schoenherr, Vig (2018	tbd
.06.2019			T4: Panel		
				Student presentations	tbd
.06.2019	Diff-in-Diff	227-257			
	Diff-in-Diff (cont.)			DiD: Puri, Rocholl and Steffen (2011)	tbd
.06.2019	Diff-in-Diff (cont.)			DiD: Cortes and Strahan (2017)	tbd
			T5: Diff-in-Diff	DiD: Berger and Roman (2017)	tbd
.07.2019	Method by Rajan and Zingales	258-279	Publication of Mock Exam on eCampus		
	Further topics: Clustering of standard errors			Student presentations	tbd
.07.2019	Discussion of Mock Exam				
	Discussion of Mock Exam (cont.) and Q&A				
	FIRST EXAM DATE				
	expected publication of exam results				
	exam review (*Klausureinsicht*)				
	SECOND EXAM DATE				
	expected publication of exam results				

Reading Material

- Detailed information on related literature is given in class
- Main textbook: *Introductory Econometrics – A Modern Approach* by Jeffrey M. **Wooldridge**, 4th edition, 2008/ 5th edition, 2012
- Main references for the *colloquium* are **journal articles** (which will be provided through eCampus)

Student presentations

- Students should present a paper (by themselves or with a team of 2-3 students $\approx$ 20-30min)
- You can send me your paper preference.
- Each student/ team is expected to present a *short summary* of the paper, to *interpret* the empirical results, and to *critically assess* the paper (regarding presentation, data, identification strategy etc.)
- Student presentations will start at the end of April

Why should you do this?

- It is good for the course, because it makes the course more interesting and entertaining for all of us.
- It is good for you, because this is the best way to get a better understanding of empirical research.

Key ingredients of a research paper (that are also important for your presentations)

- Motivation and research question
  - First empirical observations and policy relevance
  - Related literature/ research gap
  - Is it controversial?
- Research/ identification strategy
  - Challenges (reverse causality, ...)
  - How does the research strategy address these challenges?
- Data description
  - May include information on the institutional background
- Results
  - Statistical and (very important) economic significance
  - Is the research question answered in a satisfactory way?
- Discussion/ contribution of the paper

Student presentation research paper (1/3)

General Economics

- **Does the study of economics make students more selfish?** → Rubinstein (EJ 2006), A Sceptics's Comment on the Study of Economics, C1-C8
- **Does winning the Nobel price increase life expectancy?** → Rablen and Oswald (JHE 2008), Mortality and immortality: The Nobel Prize as an experiment into the effect of status upon longevity, 1462-1471
- **Do better institutions lead to higher income per capita?** → Acemoglu, Johnson and Robinson (AER 2001), The Colonial Origins of Comparative Development: An Empirical Investigation, 1369-1401

Banking/ Finance

- **Does government ownership of banks matter for economic development?** → La Porta, Lopez-De-Silanes and Shleifer (JF 2002), Government Ownership of Banks, 265-301
- **Did the dramatic fall in Japanese real estate markets in the 1990s affect the U.S.?** → Peek and Rosengreen (AER 2000), Collateral damage: Effects of the Japanese bank crisis on real activity in the United States, 30-45
- **Are Women Better Loan Officers?** → Beck, Behr and Guettler (RoF 2013), Gender and Banking: Are Women Better Loan Officers?, 1279-1321
- **What were the effects of unanticipated nuclear tests in Pakistan on bank lending?** → Khwajan and Mian (AER 2008), Tracing the impact of bank liquidity shocks: Evidence from an emerging market, 1413-1442

Student presentation research paper (2/3)

Banking/ Finance

- **How do banks allocate capital in the aftermath of natural disasters?** → Cortes and Strahan (JFE 2017), Tracing out capital flows: How financially integrated banks respond to natural disasters, 182-199
- **What is the effect of deposit insurance on bank risk-taking?** → Lambert, Noth and Schüwer (JFI 2017), How do insured deposits affect bank risk? Evidence from the 2008 Emergency Economic Stabilization Act, 81-102
- **What were the real effects of the US financial crisis?** Puri, Rocholl and Steffen (2011), Global retail lending in the aftermath of the US financial crisis: Distinguishing between supply and demand effects, JFE, 556-578
- **Does the economy benefit from bank bailouts?** Berger and Roman (2017), Did saving Wall Street really save main street? The real effects of TARP on local economic conditions, Journal of Financial and Quantitative Analysis, 1827-1867
- **Does membership in a social club (Lions Club/ Rotary Club/ etc.) affect credit allocation decisions of banks to firms inside the club?** Rainer Haselmann, Vikrant Vig and David Schoenherr (2018), Rent-Seeking in Elite Networks, Journal of Political Economy, 1638-1690.

Student presentation research paper (3/3)

Recent papers in the field of FinTech

- tbd

Exam

- The exam will cover a variety of topics
  - Economic background of discussed papers
  - Understanding of discussed econometric methods/ strategies
  - Interpretation of regression results from discussed papers
  - Interpretation of regression results from provided Stata code using various estimation strategies (diff-in-diff, interaction effects, IV, ...)
  - ...
- A mock exam will be provided in July.
- Everything is possible: 1,0 to fail.

- Open questions?

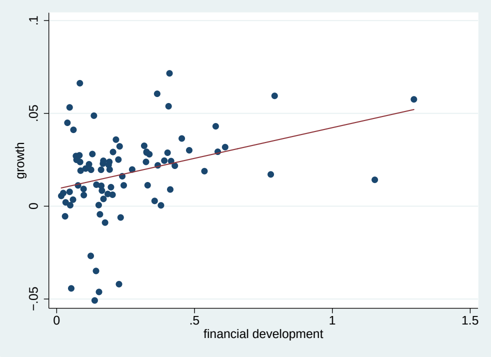
Course overview

Overview for today's and the following lectures

- Econometric foundations
  1. Linear regressions (recap)
  2. Internal and external validity (recap)
  3. Data (recap)
  4. Interaction terms

- Brief **recapitulation** of the material that you should largely already know from previous lectures. If you are *not* familiar with this material, you are highly recommended to read *Wooldridge, Chapters 1–9*.
- Topic "4. Interaction terms" may be new to most of you.

II.1. Linear Regressions



II.1.1. Multiple Linear Regression Model

- Model in the population:

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \cdots + \beta_k x_k + u$$

- k **independent variables** (**regressors**)
- β_0 = intercept
- $\beta_1, \beta_2, ...\beta_k$ are the **slope parameters** (**coefficients**) of the k regressors
- The (unobservable) **error term** u comprises all factors that have not explicitly been included in the model

Interpretation of the Slope Parameters

- $\beta_1 =$ *Expected effect* of a change in x_1 by one unit, when holding $x_2, ..., x_k$ constant
- We say that we **control** for $x_2, ..., x_k$ = **Ceteris-paribus condition**
- Mathematically, this is the *partial derivative* of the expected value of y with respect to x_1:
  $\beta_1 = \partial E(y|x_1, ...x_k)/\partial x_1$

II.1.2. Assumptions of the Multiple Linear Regression Model

Assumptions **MLR.1** until **MLR.4** (**MLR.5**) where MLR stands for multiple linear regression:

- **Assumption 1** (MLR.1): **Linear in parameters**
  - The model is **linear** in the *parameters*: $y = \beta_0 + \beta_1 x_1 + ... + \beta_k x_k + u$
  - Model does not have to be linear in the x-variables, which may enter non-linearly, e. g., $x^2, ln(x), ...$, interaction terms

Assumptions of the Multiple Linear Regression Model

- **Assumption 2** (MLR.2): **Random sampling**
  - The data are drawn as a random sample of size n
  - $(y_i, x_{1i}, ..., x_{ki})$ are **i.i.d.** (**identically and independently distributed**)

- **Assumption 3** (MLR.3): **No perfect multicollinearity**
  - There are no *exact linear relationships* between the explanatory variables (including the constant)
  - If, for example, one regressor is the multiple of another one, this condition is violated
  - Then, the OLS estimator cannot be calculated (division by zero)

Assumptions of the Multiple Linear Regression Model

- **Assumption 4** (MLR.4): **Zero conditional mean of the error term**

  $$E(u|x_1, x_2, ...x_k) = 0$$

  - For any combination of the x-variables, the error term has a *conditional expectation of zero*, i. e., in expectation, the data points lie on the true regression line:

    $$\begin{aligned} &E(y|x_1, x_2, ...x_k) \\ &= \beta_0 + \beta_1 x_1 + ... + \beta_k x_k + E(u|x_1, x_2, ...x_k) \\ &= \beta_0 + \beta_1 x_1 + ... + \beta_k x_k \end{aligned}$$

  - This is the **crucial OLS assumption**, which guarantees the **unbiasedness/consistency** of the OLS estimator

Assumptions of the Multiple Linear Regression Model

- Assumption 4 is violated if u (i. e., the omitted factors) is *correlated* with one or several of the regressors
- Examples:
  - **Omitted variables**
  - Misspecification of **functional form**
  - **Measurement error** in explanatory variables
  - **Simultaneity, reverse causality**

II.1.3. The OLS Estimator in the Multiple Linear Regression Model

- Minimize the *sum of squared deviations* of the data points from the regression line = **OLS estimation**:

$$\min_{\hat{\beta}_0, \hat{\beta}_1, \ldots, \hat{\beta}_k} \sum_{i=1}^{n} \left[y_i - \hat{\beta}_0 - \hat{\beta}_1 x_{1i} - \ldots - \hat{\beta}_k x_{ki} \right]^2$$

Notation

- $\beta_0, \beta_1, ...\beta_k$ = **Population parameters** (unknown, have to be estimated)
- $\hat{\beta}_0, \hat{\beta}_1, ...\hat{\beta}_k$ = **OLS estimators**
- **Predicted value / fitted value / fit** of y for given $x_{1i}, x_{2i}, ...x_{ki}$:
  $\hat{y}_i = \hat{\beta}_0 + \hat{\beta}_1 x_{1i} + ... + \hat{\beta}_k x_{ki}$
- **Residuals** = differences between y_i and the prediction of y_i:
  $\hat{u}_i = y_i - \hat{y}_i = y_i - \hat{\beta}_0 - \hat{\beta}_1 x_{1i} - ... - \hat{\beta}_k x_{ki}$

Derivation of OLS Estimator in Simple Regression Model

- Regression model:

$$\begin{aligned} y &= \beta_0 + \beta_1 x + u \\ \Rightarrow Cov(y,x) &= Cov(\beta_0 + \beta_1 x + u, x) \\ &= 0 + \beta_1 Var(x) + Cov(u,x) \\ &= \beta_1 Var(x) \quad \text{due to } \textbf{assumption 4} \text{ (MLR.4)} \end{aligned}$$

- This implies for the population:

$$\beta_1 = \frac{Cov(y,x)}{Var(x)}$$

- General estimation principle: **Analogy principle** (replace theoretical moments by sample moments)
- Result: **OLS estimator** in the simple model is the *empirical covariance of y and x divided by the empirical variance of x*
- Consistency of the OLS estimator hinges on **assumption 4**

OLS Estimator in Multiple Regression Model

- Model in the population:

$$y = \beta_0 + \beta_1 x_1 + \beta_2 x_2 + \cdots + \beta_k x_k + u$$

- Coefficient β_1 can be written in a very similar way:

$$\beta_1 = \frac{Cov(y, \tilde{x_1})}{Var(\tilde{x_1})},$$

  where $\tilde{x_1}$ is the residual of a regression of x_1 on all other regressors $x_2, ...x_k$ (other variables are partialed out)
  $\Rightarrow$ **Partitioned regression**

- This implies that we get the same expression as in the simple model with one regressor if x_1 is not correlated with any of the regressors (Why?)

Homoscedasticity and Heteroscedasticity

- **Assumption 5** (MLR.5): **Homoscedasticity:**
  $Var(u|x_1, ...x_k) = \sigma^2$
  - The error term has a *constant variance*, conditional on the x-variables

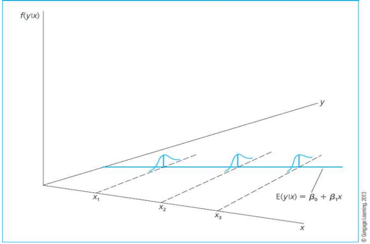


FIGURE 2.8 The simple regression model under homoskedasticity.

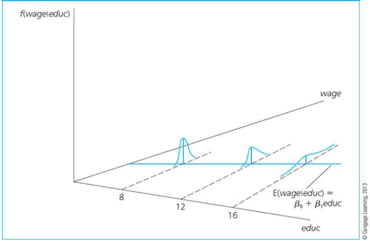


FIGURE 2.9 Var(*wage*|*educ*) increasing with *educ*.

Source: Wooldridge (2008)

The OLS Estimator Under Heteroscedasticity

- **Heteroscedasticity:** $Var(u|x_1, ...x_k) \neq \sigma^2$, i. e., the variance *differs* across different observations
- Under heteroscedasticity, the OLS estimator remains **unbiased**, **consistent**, and **asymptotically normal**
- However, it is **not efficient** (which means that it has a relatively large variance)
- In empirical practice, this loss in efficiency is typically accepted
- However, one has to use **heteroscedasticity-consistent** or **robust standard errors**
- "Normal" standard errors yield misleading results

Properties of the OLS Estimator

- Under the four assumptions MLR.1 – MLR.4, the OLS estimator is ...
  - **Unbiased**
  - **Consistent**
  - **Asymptotically normally distributed**
- **Gauss-Markov-Theorem:** Under the assumptions MLR.1 – MLR.5, the OLS estimator is **BLUE** (*best linear unbiased estimator*), i. e., among all linear unbiased estimators, it is the one with the lowest variance (we say: the OLS estimator is **relatively efficient**)
- Note: "Linear" here means that y_i is a linear function of the variables specified in the model
- If the error term is additionally **normally distributed**, the OLS estimator is the *best unbiased estimator*, i. e., it is also better than non-linear estimators

II.1.5. Hypothesis Tests in the Multiple Linear Regression Model

- Several types of tests:
  1. Tests referring to **one** regression coefficient (**t-test**), e. g., $H_0 : \beta_1 = 0$, $H_1 : \beta_1 \neq 0$ (*individual significance*)
  2. Tests with **several sub-hypotheses** referring to **several** regression coefficients (**F-test**), e. g., $H_0 : \beta_1 = 0$ and $\beta_2 = 0$, $H_1 : \beta_j \neq 0$ for j=1 and/ or j=2 (*joint significance*)
  3. Tests referring to a **linear combination** of several regression coefficients (**t-test**), e. g., $H_0 : \beta_1 = \beta_2$

  2. and 3. can also be combined in an F-test

II.1.6. Goodness of Fit Measures

- Question: How good can the data be explained by the estimation?
- 3 measures:
  1. **Standard error of regression** (SER)
  2. **Coefficient of determination**: R^2
  3. **Adjusted** $R^2 = \bar{R}^2$

Standard Error of Regression

$=$ Estimator of the *standard deviation of the error term* u_i

- Measures the dispersion of the data y_i around the regression line
- Standard Error of Regression (SER) is measured in units of y_i
- Definition:

$$SER = \hat{\sigma}, where \ \hat{\sigma}^2 = \frac{1}{n-k-1}\sum_{i=1}^{n}\hat{u}_i^2 = \frac{SSR}{n-k-1}$$

- *degrees of freedom* $n-k-1 =$ number of observations (n) - number of estimated parameters $(k+1)$

Coefficient of Determination: R^2

R^2 = Share of the *explained variation* (SSE) in the *total variation* (SST) of y_i

- total sum of squares (SST) = total variation
- explained sum of squares (SSE) = explained variation
- residual sum of squares (SSR) = unexplained variation

$$R^2 = \frac{SSE}{SST} = 1 - \frac{SSR}{SST}$$

- Note that SSR is *reduced* (R^2 is *raised*) whenever an additional explanatory variable is added unless the coefficient is exactly zero (unlikely in practice)
- If one wants to compare the R^2 of two regressions with a different number of regressors, one should use the adjusted R^2

Adjusted R^2

- The **adjusted** R^2 takes the *automatic increase* in R^2 into account by adjusting the *degrees of freedom*:

$$\bar{R}^2 = 1 - \frac{n-1}{n-k-1}\frac{SSR}{SST}$$

II.2. Internal and External Validity

- Reading material: *Stock/Watson (2007), Chapter 9*
- The goal of most empirical studies is the estimation of a *causal effect*, e. g., the effect of the development of the financial system on the economic growth of a country
- How can we judge whether an empirical study yields reliable results? What can go wrong?
- Two criteria:
  1. **Internal validity:** Are the results valid for the considered population?
  2. **External validity:** Are the results generalizable to other populations?

Internal Validity

- Potential threats to **internal validity**:
  1. *Estimators* are biased / inconsistent, i. e., they do not on average deliver the correct value
  2. *Standard errors* (= estimated standard deviations) of the estimators are biased / inconsistent
     $\Rightarrow$ Wrong inference in hypothesis testing

External Validity

- Potential threats to **external validity**:
  - Results cannot be generalized to other countries, times, institutional setups etc.
  - Problem also arises in experimental contexts, e. g., mice vs. men
- External validity can be checked by repeating the same study with *different data*

Threats to Internal Validity: Inconsistent Estimators

- 5 major reasons for *biased / inconsistent OLS estimators*:
  (1) **Omitted variables**
  (2) **Misspecification of functional form**
  (3) **Measurement error in the independent variables (errors-in-variables problem)**
  (4) **Sample selection**
  (5) **Simultaneity, reverse causality**
- In all these cases, the *regressors are correlated with the error term*
  $\Rightarrow$ Violation of the central OLS assumption **MLR.4**

(1) Omitted Variables

- An **omitted variable** leads to biased / inconsistent estimators if the omitted variable
  1. is a *determinant* of y (i. e., it is part of the error term) and
  2. it is *correlated* with at least one of the regressors
- Otherwise the included variable may in reality measure the effect of the omitted variable
- Solutions:
  - Inclusion of all relevant determinants
  - Inclusion of **proxy variables**
  - **Instrumental variables estimation**
  - **Panel methods** (esp. *fixed effects estimation*)

(2) Misspecification of Functional Form

- Can also be interpreted as a problem of **omitted variables**
- Example: Linear instead of quadratic specification of the regression function
  - Quadratic term is an omitted variable that is correlated with an included regressor (the linear term)
- Problem can often be solved easily by specifying flexible functional forms allowing for *non-linearities* (polynomials, logs etc.)

(3) Error-in-Variables Problem

- **Measurement error** in one or more of the *independent variables* leads to a correlation of the regressors with the error term
- Reasons for measurement error:
  - Wrong measurement
  - Wrong answer in surveys (consciously or unconsciously)
  - Typos in the data
  - Use of proxy variables

“Classical” Measurement Error

- Under **classical measurement error**, the measured variable differs from the true variable by a *random error*
- This implies that the effect of the regressor tends to be *underestimated* (in absolute value) $\Rightarrow$ **Attenuation bias**
- The larger the variance of the random error of the measurement error, the larger is the underestimation
- Hence, we are unlikely to find strong results in noisy data

Solutions to Measurement Error

- Most importantly, one should try to find good data with little noise
- Econometric solutions:
  1. **Instrumental variables estimation** (e. g., using a second measurement as an instrument)
  2. Correction of an estimator if the form of the measurement error is known

(4) Sample Selection

- A problem of sample selection arises if the selection into the sample depends on the value of the *dependent variable* y
- Examples: Study of the profitability of firms over a long time span (the least profitable firms drop out), survey of food quality at the University cafeteria, teaching evaluation in elective courses
- Problem does not arise in a truly random sample
- Solution: **Heckman correction**

(5) Simultaneity

- Also called **reverse causality** or **endogeneity**
- We assume that causality runs from x to y
- If causality runs in *both* directions, x will be correlated with the error term leading to biased and inconsistent estimators
- Very frequent problem in empirical analyses
- Standard example: Estimation of a demand (or supply) function $\Rightarrow$ Quantities and prices are determined *simultaneously*
- In almost any empirical study, there are arguments why y may also affect x (and not just vice versa)
- Most important solution: **Instrumental variables estimation**

Threats to Internal Validity: Inconsistent Standard Errors

- So far: Inconsistency of the estimator
- Now: Inconsistency of *standard errors*
- Even if the estimators are unbiased / consistent, standard errors may be estimated inconsistently
- Consequence: Statistical inference from hypothesis testing is invalid
- 2 major reasons for inconsistent standard errors:
  (1) **Heteroscedasticity**
  (2) **Autocorrelation**

Heteroscedasticity

- Heteroscedasticity is present in almost any data set
- Problem can easily be "solved" by using **robust** (*heteroscedasticity-consistent*) standard errors
- Note, however, that estimators are *not efficient* under heteroscedasticity, i. e., they do not have the smallest variance

Autocorrelation (Serial Correlation)

- Error terms are correlated across different observations
  - Random sample excludes autocorrelation
  - Typical phenomenon in *time series data*
- Heteroscedasticity-consistent standard errors are inconsistent in this context
- However, there also exist *autocorrelation-consistent standard errors*

Internal and External Validity of Forecasts

- If a regression model is used for **forecasting**, it is *not* important that the model estimates a causal effect
- Any variable that helps to improve the forecast is useful even if it is not causally related to the variable to be explained
- For forecasts, it is important that
  - the estimation has *high explanatory power*,
  - the estimation is *precise*,
  - the results of the estimation can be generalized to other populations
    $\Rightarrow$ **External validity** is particularly important

Conclusion

- In order to judge the reliability of an empirical study, we have to analyze **internal** and **external validity**
- In the following, we will mostly deal with the threats to internal validity and with the solutions to such problems

II.3. Data

- Data are the *crucial ingredient* of any empirical analysis
- If the data are poor, the most complicated econometric method cannot produce useful results
- Data work requires ...
  1. great care
  2. institutional knowledge
  3. a lot of time
- In the preparation of the data set, there exist the largest *incentive problems* because this process can hardly be controlled from outside
  - Quality of data preparation depends crucially on the researcher's *work ethics*
  - **Transparency** is key! (Show your data, such as descriptive statistics etc.)

Types of Data Sets

- Data sets differ across various dimensions:
  - *Macroeconomic* data (typically at the country level) vs. *microeconomic* data (e. g.,households, firms)
  - *Cross-sectional data* (many observational units at one point in time), *time series data* (observation of variables over time) or combinations (*repeated cross-sections* or *panel data*)
  - Data from the *real world* vs. *experimental* data (*laboratory experiments* or *field experiments*)
  - Official data vs. other published data vs. survey data

Cross-Sectional Data

- Subjects $i = 1, ..., N$ (at one point in time)
- Assumption: Observations were sampled randomly, i. e., they are **identically and independently** distributed (**i. i. d.**)
- Assumption is harmless in cross-sectional data sets
- Examples of cross-sectional data sets:
  - Balance sheet data of all German banks in 2011
  - Stock prices of all listed European companies on 31 December 2011
  - Macroeconomic data of OECD countries in 2012

Time Series Data

- Time series data allow for the identification of *dynamic relationships*
- Units of observation = points in time: $t = 1, ..., T$
- Most important difference to cross-sectional data: Time series data are *ordered* in a natural way (order is random in cross-sections)
- Random variables that are ordered according to time are also called a **stochastic process**
- A time series data set is just **one** realization of this stochastic process
- Population = all possible time series that could have occurred

Time Series Data

- Note: The **independence** assumption is typically violated in time series data (many time series data are *persistent*)
- The observations are often not distributed **identically** either if the structure of the economy has changed (**structural breaks**)
- Conclusion: Time series data require completely different econometric methods than cross-sectional data (we do not deal with these methods here)
- Examples of time series data sets:
  - Evolution of German GDP between 1945 and 2011
  - Evolution of the DAX in the year 2011

Combination of Cross-Sections and Time Series

- Many data sets have a cross-sectional <u>and</u> a time series dimension
  1. Subjects: $i = 1, ..., N$
  2. Time periods: $t = 1, ..., T$
- 2 types:
  1. **Repeated cross-sections**: From some population, random samples are drawn at different points in time (different subjects!)
  2. Panel data: The **same** subjects are observed over several time periods
- Crucial distinction: In repeated cross-sections, random samples are drawn in every period, in panel data sets, a random sample is drawn only in the very beginning

Repeated Cross-Sections

- Example of a repeated cross-section: Every month 1000 firms are selected randomly and are asked about their business expectations
- Example from the official statistics: Microcensus
- Important statistical property of the sample: Observations are **independently** distributed, even over different time periods
- But: Possibly they are **not identically** distributed
- Example: Distribution of business expectations may change after a crisis

Panel Data

- Panel data help us to identify causal effects even when the objects of observation are subject to (time-invariant) *unobserved heterogeneity*
- With panel data, we can identify effects that cannot be identified in the cross-section (age vs. cohort effects)
- Examples of panel data set:
  - Balance sheet data of all German banks between 1990 and 2011
  - Daily stock prices of all listed European companies in the year 2011
  - Macroeconomic data of OECD countries from 1970 until 2011
- In this case, the observations are generally **neither identically distributed**, **nor independently distributed** over time (e. g. time-invariant firm or country characteristics)

Panel Data

- Important problem with panel data: **Panel attrition**
  - Composition of the panel does not remain constant over time (number of subjects tends to be reduced in spite of re-sampling)
  - Example: Bankruptcy of firms, merger of firms
  - Moreover, not all data points are available for all firms
  - In both cases, we have an **unbalanced panel**
- Important question: Is the data selection *random* or *systematic*?
  - Example of systematic sample selection: **Survivorship bias**

When Should We Use Which Data?

- Basic rule: It is better to have more data
  - *Greater precision* of the estimation
  - With more data, one can estimate *more complex models* with many parameters
- But: In order to be able to exploit the size of the data set, we have to be willing to assume that the same model is valid in the entire data set
  - Example: When using time series data, we cannot go back in time as far as possible because we may not be willing to assume that the world has remained the same (**structural breaks**)

When Should We Use Which Data?

- For different types of questions, we need different types of data
  - Example: In order to find out what determines the financing structure of firms, we need microeconomic (firm-level) data
  - But: If we try to answer a macroeconomic question, microeconomic data may not help to increase precision
- Most real-world data sets are not ideal (rather the opposite)
- Often it helps to imagine the *ideal data set* for the given research question to understand the limits of the available data

Conclusion

- Data are the *crucial element* of any empirical analysis
- More data is better if we are willing to assume a certain stability of the underlying model
- Different types of questions require different types of data
- Certain data structures help to identify certain effects
- Different data types have different statistical properties that have to be taken into account in the analysis

Course overview

II.4 Interaction terms

Outline for today

- Why interaction terms?
- Specification of interaction models
- Example I
  - Interaction of continuous and dummy variable
  - Interaction of two continuous variables
  - Interaction of two continuous variables and a dummy variables
- Example II
  - Interaction of two dummy variables

References: Brambor, Clark and Golder (2006), Understanding Interaction Models: Improving Empirical Analyses, Political Analysis 14, 63-82.

* The following slides are based on slides from Felix Noth.

Why interaction terms?

- Interaction models are useful when researchers want to test conditional hypotheses
  - Does the effect of education income differ for men and women?
  - Does the effect of class size on testscore differ for different shares of non-native students?
- A basic interaction model looks like

$$y = \beta_0 + \beta_1 x + \beta_2 z + \beta_3 xz + \epsilon.$$

- With interaction terms, we cannot extract the effect (and its significance) of x or z directly from their coefficients β_1 or β_2.
- The effect (and its significance) of x on y now also depends on z, and the effect of z on y also depends on x.

The marginal effect in interaction models

- In a standard model like $y = \beta_0 + \beta_1 x + \epsilon$, the marginal effect of x on y is

$$\frac{\partial y}{\partial x} = \beta_1.$$

- In the presence of an interaction term as in $y = \beta_0 + \beta_1 x + \beta_2 z + \beta_3 xz + \epsilon$, it is

$$\frac{\partial y}{\partial x} = \beta_1 + \beta_3 z.$$

- That means, that the effect of x on y is not constant over the value range of z.

Do not interpret constitutive terms as unconditional marginal effects

- In a model like $y = \beta_0 + \beta_1 x + \beta_2 z + \beta_3 xz + \epsilon$ coefficients of constitutive terms do not represent unconditional (average) effects
- β_1 only gives the average marginal effect if $z = 0$.
  - This might not be the effect we are interest in.
  - It might be economically meaningless, e.g., for age of zero.
- Generally, it is not valid to say – given a positive and significant β_1 – that an increase in x leads to an increase in y

Specification of interaction models

- When we fit interaction models, we must include all constitutive terms, i.e., all variables that are part of any interaction should enter the regression with their single terms.
- Example:

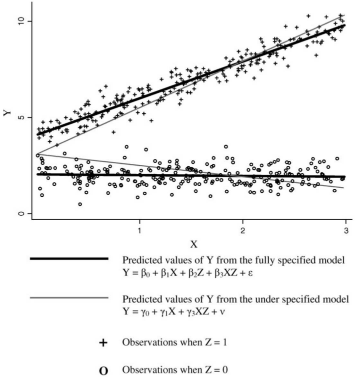
$$y = \beta_0 + \beta_1 x + \beta_2 z + \beta_3 xz + \epsilon$$

  is fully specified.
- If you leave out any combination, this may lead to biased (inconsistent) estimates, i.e.,

$$y = \beta_0 + \beta_1 x + \beta_2 xz + \epsilon$$

  is under specified.

Include all constitutive terms



- We run $y = \beta_0 + \beta_1 x + \beta_2 z + \beta_3 xz + \epsilon$
- The bias occurs as soon as $\beta_2 \neq 0$, that means that both lines have different constants
  - Leaving out z from the regression forces the two lines to meet on the y axis
- Then, if z is left out from the regression and is either correlated with x or xz, $\beta_0, \beta_1, \beta_3$ will be biased

SEs in interaction models

- Including all constitutive terms causes multicollinearity, which increase SEs and makes significant coefficients less likely
  - High multicollinearity means that there is not enough information in the data to estimate coefficients accurately and this is then reflected in high (but true) SEs
- These standard errors are never in any sense “too” large - they are always the “correct” standard errors.

Example I (this and the following slides)

*The General Social Survey**

- This survey provides a variety of social and demographic trends for the U.S. between 1972 and 2010 at a 2 year interval
- This version here (corrected for missing data) has 23,713 observations for around 1,000-2,000 individuals per observation year between 1974 and 2010

	Mean	SD
Income	21,947.91	26,704.38
Log(income)	9.52	1.09
Education	13.37	2.91
Age	40.41	13.32
Female	0.49	0.50
Health	3.17	0.74

Health is measured as poor (1), fair (2), good (3) or excellent (4).

* http://www.norc.uchicago.edu/GSS+Website/

Baseline regression

$$Log(income)_{it} = \beta_0 + \beta_1 Educ_{it} + \beta_2 Age_{it} + \beta_3 Female_{it} + \beta_4 H2_{it} + \beta_5 H3_{it} + \beta_6 H4_{it} + \epsilon_{it}$$

- Interpretation?
  - Note: dummies are interpreted in relation to the category that is left out.

Dependent variable: Log(income)	
Education	0.1103***
	(0.0022)
Age	0.0162***
	(0.0005)
Female	-0.6197***
	(0.0126)
Health dummies:	
H2(Fair)	0.2291***
	(0.0466)
H3(Good)	0.4001***
	(0.0447)
H4(Excellent)	0.4817***
	(0.0452)
Constant	7.2993***
	(0.0562)
Obs.	23604
F-value	1069.63
Adj. R2	0.2136

Interaction of continuous and dummy variable

You may be interested in the marginal effect of education on income *conditional on the sex of the individual*:

$$
\begin{aligned}
Log(income)_{it} = \beta_0 + \beta_1 Education_{it} + \beta_2 Age_{it} + \beta_3 Female_{it} \\
+ \beta_4 (Education_{it} \times Female_{it}) \\
+ \beta_5 H2_{it} + \beta_6 H3_{it} + \beta_7 H4_{it} + \epsilon_{it}.
\end{aligned}
$$

- The conditional marginal effect of education is

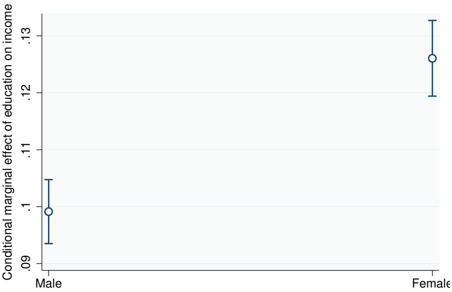
$$\frac{\partial Log(income)}{\partial Education} = \beta_1 + \beta_4 Female$$

Interaction of continuous and dummy variable (cont'd)

Regression

Dependent variable: Log(income)	
Education	0.0991***
	(0.0029)
Age	0.0163***
	(0.0005)
Female	-0.9798***
	(0.0600)
Education × Female	0.0269***
	(0.0044)
H2(Fair)	0.2292***
	(0.0465)
H3(Good)	0.3992***
	(0.0447)
H4(Excellent)	0.4807***
	(0.0452)
Constant	7.4471***
	(0.0611)
Obs.	23604
F-value	923.64
Adj. R2	0.2148

Graphical presentation



- The effect of education on income is significantly larger for women!

Interaction of continuous and dummy variable (cont'd)

You may be interested in the marginal effect of education on income *conditional on the health of the individual*

$$\begin{aligned} Log(income)_{it} = & \beta_0 + \beta_1 Education_{it} + \beta_2 Age_{it} + \beta_3 Female_{it} \\ & + \beta_4 H2_{it} + \beta_5 H3_{it} + \beta_6 H4_{it} + \\ & + \beta_7 Education_{it} \times H2_{it} \\ & + \beta_8 Education_{it} \times H3_{it} \\ & + \beta_9 Education_{it} \times H4_{it} + \epsilon_{it}. \end{aligned}$$

- E.g., the conditional marginal effect of education for $H2 = 1$ ("fair" health) is

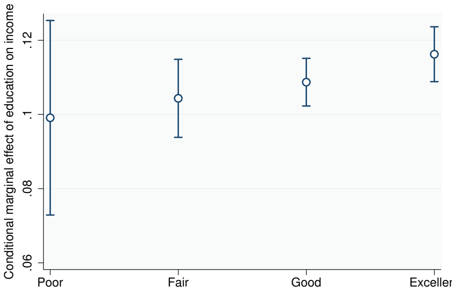
$$\frac{\partial Log(income)}{\partial Education} = \beta_1 + \beta_7$$

Interaction of continuous and dummy variable (cont'd)

Regression

Dependent variable: Log(income)	
Education	0.0991***
	(0.0134)
Age	0.0162***
	(0.0005)
Female	-0.6192***
	(0.0126)
H2(Fair)	0.1710
	(0.1754)
H3(Good)	0.2904*
	(0.1681)
H4(Excellent)	0.2675
	(0.1710)
Education $\times$ H2(Fair)	0.0052
	(0.0144)
Education $\times$ H3(Good)	0.0096
	(0.0138)
Education $\times$ H4(Excellent)	0.0171
	(0.0139)
Constant	7.4322***
	(0.1645)
Obs.	23604
F-value	713.65
Adj. R2	0.2137

Graphical presentation



- The effect of education on income is always significantly positive, but does not differ significantly between health status.

Interaction of two continuous variables

You may be interested in the marginal effect of education on income *conditional on the age of the individual*

$$\begin{aligned} Log(income)_{it} = {} & \beta_0 + \beta_1 Education_{it} + \beta_2 Age_{it} + \beta_3 Female_{it} \\ & + \beta_4 (Education_{it} \times Age_{it}) \\ & + \beta_5 H2_{it} + \beta_6 H3_{it} + \beta_7 H4_{it} + \epsilon_{it}. \end{aligned}$$

- The conditional marginal effect is

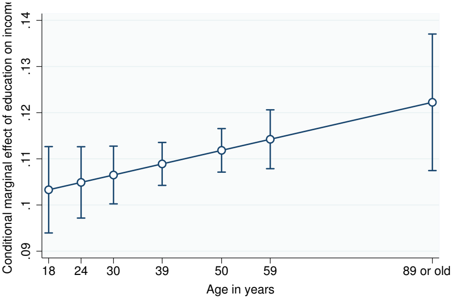
$$\frac{\partial Log(income)}{\partial Education} = \beta_1 + \beta_4 Age$$

Interaction of two continuous variables (cont'd)

Regression

Dependent variable: Log(income)	
Education	0.0985***
	(0.0075)
Age	0.0128***
	(0.0021)
Female	-0.6195***
	(0.0126)
Education $\times$ Age	0.0003*
	(0.0002)
H2(Fair)	0.2261***
	(0.0466)
H3(Good)	0.3964***
	(0.0448)
H4(Excellent)	0.4787***
	(0.0453)
Constant	7.4557***
	(0.1099)
Obs.	23604
F-value	917.29
Adj. R2	0.2137

Graphical presentation



- The effect of education on income is significantly different between very young and very old individuals!

Interaction of two continuous variables (cont'd)

You may be interested in the marginal effect of education on income *conditional on the level of education*

$$
\begin{aligned}
Log(income)_{it} = {} & \beta_0 + \beta_1 Education_{it} + \beta_2 Education_{it}^2 \\
& + \beta_3 Age_{it} + \beta_4 Female_{it} \\
& + \beta_5 H2_{it} + \beta_6 H3_{it} + \beta_7 H4_{it} + \epsilon_{it}.
\end{aligned}
$$

- The conditional marginal effect is

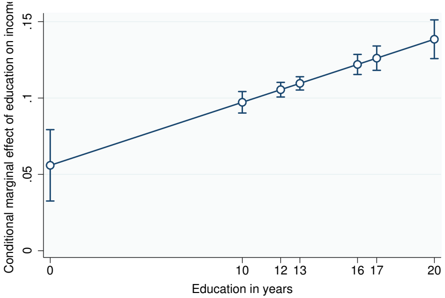
$$\frac{\partial Log(income)}{\partial Education} = \beta_1 + \beta_2 \; 2Education$$

Interaction of two continuous variables (cont'd)

Regression

Dependent variable: Log(income)	
Education	0.0559***
	(0.0119)
Education × Education	0.0021***
	(0.0004)
Age	0.0159***
	(0.0005)
Female	-0.6151***
	(0.0126)
H2(Fair)	0.2327***
	(0.0465)
H3(Good)	0.4082***
	(0.0447)
H4(Excellent)	0.4874***
	(0.0452)
Constant	7.6458***
	(0.0934)
Obs.	23604
F-value	920.71
Adj. R2	0.2143

Graphical presentation



- The effect of education on income is significantly higher for higher levels of education!

Continuous × continuous × dummy interaction

You may be interested in the marginal effect of education on income *conditional on the level of education and the sex of the individual*

$$\begin{aligned} Log(income)_{it} = & \beta_0 + \beta_1 Education_{it} + \beta_2 Education_{it}^2 \\ & + \beta_3 Age_{it} + \beta_4 Female_{it} \\ & + \beta_5 (Education_{it} \times Female_{it}) \\ & + \beta_6 (Education_{it}^2 \times Female_{it}) \\ & + \beta_7 H2_{it} + \beta_8 H3_{it} + \beta_9 H4_{it} + \epsilon_{it}. \end{aligned}$$

- The conditional marginal effect is

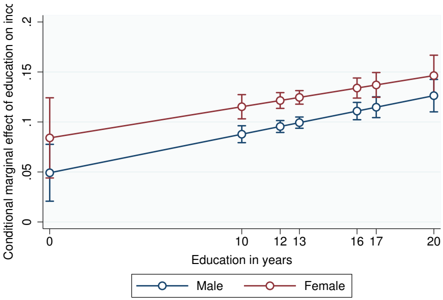
$$\frac{\partial Log(income)}{\partial Education} = \beta_1 + \beta_2 \; 2Education + \beta_5 Female + \beta_6 \; 2(Education \times Female)$$

Continuous × continuous × dummy interaction (cont'd)

Regression

Dependent variable: Log(income)	
Education	0.0492***
	(0.0145)
Education × Education	0.0019***
	(0.0005)
Age	0.0160***
	(0.0005)
Female	-1.0131***
	(0.1659)
Education × Female	0.0349
	(0.0248)
Education × Education × Female	-0.0004
	(0.0009)
H2(Fair)	0.2319***
	(0.0465)
H3(Good)	0.4058***
	(0.0447)
H4(Excellent)	0.4853***
	(0.0452)
Constant	7.7597***
	(0.1077)
Obs.	23604
F-value	720.65
Adj. R2	0.2153

Graphical presentation



- The effect of education on income is significantly higher for higher levels of education and differ significantly between men and women for 10-16 years of education!

Example II

Slides including questions and answers for Example II will be provided in class.

Take away and outlook

- Interaction models provide useful possibilities
- But, you must be careful in implementing and interpreting interaction terms
  - Always include all terms
  - Do not interpret single coefficients as unconditional effect
  - Always calculate standard errors for the interactions

Course overview

III Economic Foundations of Banking and Finance

Outline

1. Empirical facts about the development of banks and financial markets
   - Pagano et al (2014), Is Europe Overbanked?
2. The role of banks and financial markets
   - Levine (2004), Finance and Growth: Theory and Evidence
   - Merton and Bodie (1995), A Conceptual Framework for Analyzing the Financial Environment
3. Does finance benefit society?
   - AFA presidential address 2015 by Luigi Zingales

1. Empirical facts

- Since the mid 1990s, the banking sector and financial markets around the world have undergone radical transformations.
- The banking sector has ballooned in asset size, the sector has greatly increased the supply of private credit and expanded into new lines of business.
- Financial markets have seen major innovations and the trading volume of some products has increased dramatically (e.g., credit derivatives)
- Finally, the financial crisis of 2007-2009 has questioned the benefits of financial innovation and leads to a less optimistic and more distinguished view about finance.

The source for all figures in this section is Pagano et al. (2014).

Famous quotes about finance ...

> *As long as the music is playing, you've got to get up and dance.*

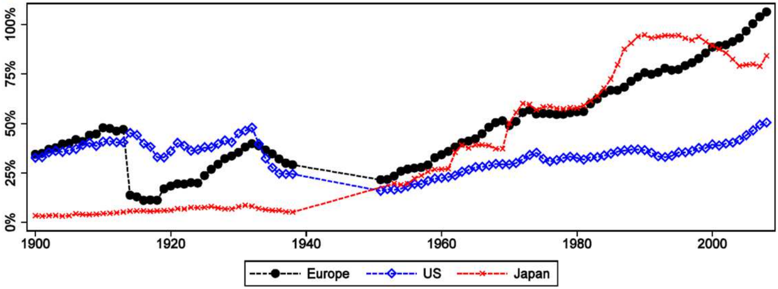
Chuck Prince, CEO of Citigroup, about the bank's continued commitment to leveraged buy-out deals, despite fears of reduced liquidity because of the occurring sub-prime meltdown in July 2007.

> *The only useful thing banks have invented in the last 20 years is the ATM.*

Paul Volcker, former chairman of the Federal Reserve, in a speech in 2009.

Growth of banking system size over the last 30 years

Figure 1: Bank loans to GDP in US, Japan, and Europe

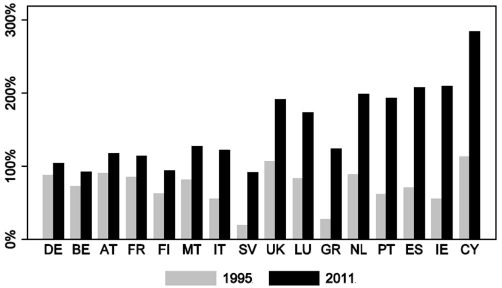


Source: Schularick and Taylor (2012). Notes: Bank loans refers to resident banks' loans to the domestic private sector (households and non-financial corporations). The data therefore exclude foreign (and foreign currency) loans; and loans to the financial and public sectors. Europe represents an average (weighted by GDP) of DK, DE, ES, FR, IT, NL, SE and the UK.

- In western Europe and the US, the ratio of domestic bank loans (to the private sector) to GDP was relatively stable around 40% until about 1980
- Since then, the ratio has trended upwards

Bank credit-to-GDP has increased everywhere in Europe - with significant differences across countries

Figure 2: Bank loans to GDP (%) in selected EU countries

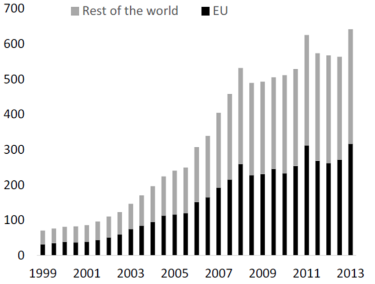


Source: World Bank Financial Development and Structure Dataset. Notes: Bank loans includes private credit by deposit money banks. This series includes intra-financial sector loans, unlike that of Schularick and Taylor (2012), and therefore levels are somewhat higher.

- The extent of the increase of the ratio varies across countries
- 4 countries experienced only a modest increase, 9 saw a substantial increase, whereas in 5 countries which received EU assistance the increase was most substantial

Development of derivatives markets

Figure 3: Derivatives by region (notional; US$tn)

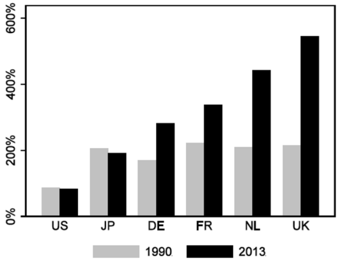


Source: BIS. Notes: The chart shows notional amounts outstanding of the main OTC derivatives (rates, FX, equities). Here, a derivative is considered "European" if it is referenced on an EU currency or entity.

- The derivative market has experienced a terrific growth from to 2000 to the beginning of the financial crisis (2007/ 2008)

Growth of banks' balance sheets in Europe vs. US and Japan

Figure 5: Resident banks' assets as % of domestic GDP

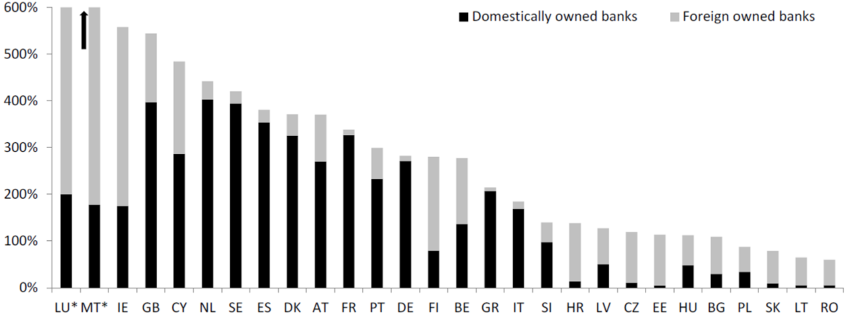


Source: Helgi Library; OECD; national central banks. The US observation in 2013 would increase by 32 percentage points including Fannie and Freddie and by a further 30 percentage points under IFRS-equivalent accounting.

- Since the 1990s EU bank's balance sheets have grown rapidly
- The total assets of the EU banking sector (including foreign-owned entities) amounted to 334 % in 2013

The size of domestic and foreign banks across Europe as of 2013

Figure 6: Total consolidated assets of domestic and foreign owned banks / GDP (%) in 2013 H1

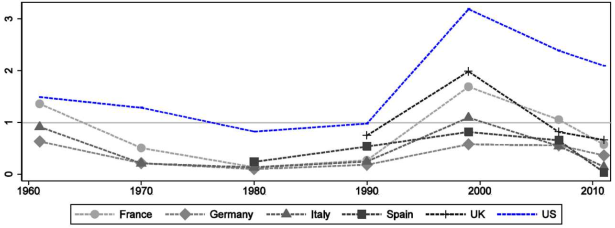


Sources: ECB consolidated banking data and the IMF World Economic Outlook. Notes: 2013 H1 refers to the first half of 2013. For presentational purposes the y-axis was truncated at 600%. The values for Luxembourg and Malta are 1719% and 798% respectively.

- In several countries the ratio exceeded 400% in 2013
- In contrast, using the broadest numbers, in the US the ratio would amount to only 145%

Comparison of financial structure in selected countries

Figure 9: Financial structure (measured as the ratio of stock market capitalisation to bank credit to the private sector)

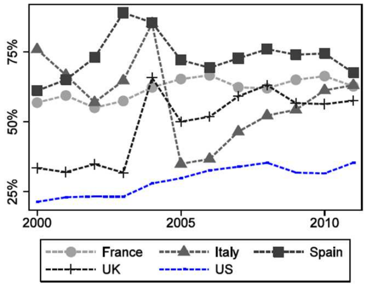


Sources: Rajan and Zingales (2003); Schularick and Taylor (2012); and the World Bank. Notes: Financial structure is measured as the ratio of stock market capitalisation to bank credit (to the private sector). Special thanks to Luigi Zingales for sharing data.

- European countries and the US became more market-based through the 1990s and more bank-based (measured by the ratio of stock market capitalization to bank credit to the private sector)
- The shift towards the market was more profound in the US during the 1990s, the shift to banks was stronger in Europe in the 2000s: As a result Europe and the US differ more strongly in financial structure now than in the 1990s

Bank concentration in selected countries

Figure 10: Top three bank assets as % of total bank assets by country

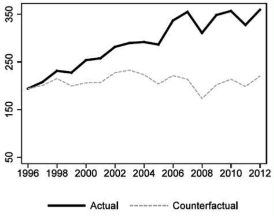


Source: Bankscope; World Bank Financial Development and Structure Dataset.

- The European banking system has become more concentrated
- Since 2000, the proportion of a national banking system's total assets held by its three largest banks has increased in all major EU countries (except Italy)

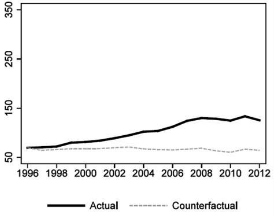
Thought experiment: If the largest 20 banks' assets had grown in line with nominal GDP since 1996 ...

Figure 12: EU: Actual and "counterfactual" total banking system assets / GDP (%)



Source: Bloomberg; own calculations. Notes: "Actual" plots actual observations on the ratio of total banking-system assets to GDP. "Counterfactual" is the same, except that the assets of the largest 20 EU banks are assumed to grow in line with nominal GDP from 1996. The largest 20 EU banks are BNPP, BBVA, Santander, Barclays, Commerzbank, Danske, Deutsche, Dexia, HSBC, ING, Intesa, KBC, LBG, Natixis, RBS, SEB, Societé Génerale, Standard Chartered, Svenska Handelsbanken and UniCredit. The denominator is the sum of the nominal GDPs of the nine EU countries home to at least one top 20 bank (i.e. BE, DK, DE, ES, FR, IT, NL, SE and the UK).

Figure 13: US: Actual and "counterfactual" total banking system assets / GDP (%)

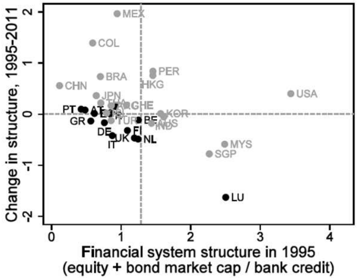


Source: Bloomberg; own calculations. Notes: "Actual" plots actual observations on the ratio of total banking-system assets (estimated using IFRS accounting standards) to GDP. "Counterfactual" plots these observations *as if* the assets of the top 20 US banks had grown in line with nominal US GDP from 1996. The top 20 US banks are BB&T, Bank of America, Bank of New York Mellon, Capital One, Citigroup, Fifth Third, Goldman Sachs, JPMorgan Chase, KeyCorp, M&T, Morgan Stanley, Northern Trust, PNC, Regions, SLM, State Street, SunTrust, US Bancorp, Washington Mutual and Wells Fargo.

- The growth of the size of the EU banking system (relative to GDP) since 1996 is entirely attributable to the growth of the largest 20 banks
- In the US, the same phenomenon is observable, however the magnitude of the effect is much lower

Financial structures 1995 vs. 2011

Figure 26: Changing financial structures

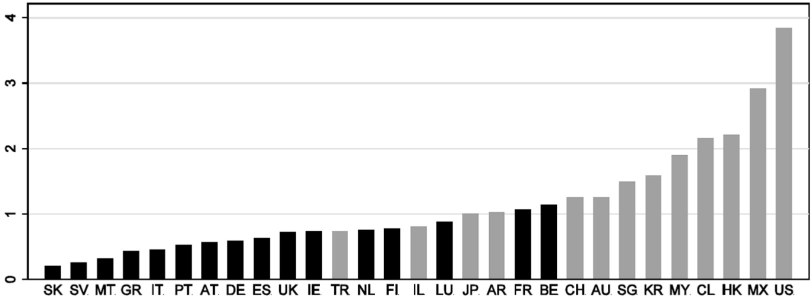


Source: World Bank Financial Development and Structure Dataset.
Notes: Financial structure is measured as the ratio of stock and bond market capitalisation to bank credit (to the private sector). Bank loans includes private credit by deposit money banks.

- Most non-EU countries have become more market-based since 1995
- By contrast, most EU countries have become even more so in the same period

Financial structure across the world

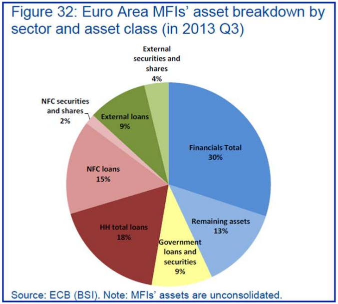
Figure 27: Financial structure in 2011: Europe compared with the rest of the world



Source: World Bank Financial Development and Structure Dataset. Financial structure is measured as the ratio of stock and bond market capitalisation to bank credit (to the private sector). Bank loans includes private credit by deposit money banks.

- Most EU countries were more bank-based than the rest of the world in 2011

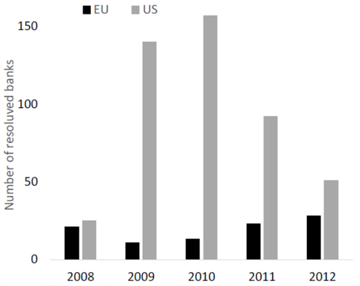
Breakdown of Euro Area monetary financial institutions' (MFIs') assets



- EU banks hold Euro 42tn of assets overall
- More than a quarter of the euro area MFIs' assets are claims on other financial firms in the euro area
- Credits to households and firms in the EU amount to 31% of assets

Bank resolution in the US vs. the EU

Figure 37: Bank resolution in the US and EU



Source: FDIC and Open Economics. US data count the number of banks which failed and for which the FDIC was appointed receiver. EU data are from Open Economics, and count the total number of banks which failed (in a broad sense). EU data therefore include distressed mergers and part nationalisations; US data do not.

- The number of failed banks in the EU was relatively small compared to the US
- Does this mean that EU banks had less financial problems?
  - Maybe, but national supervisors in the EU have been more reluctant to shut down distressed banks than the supervisor in the US.
  - Such “regulatory forbearance” may create so-called zombie banks and cause further problems.

2. The role of banks and financial markets?

Economists disagree sharply about the role of the financial sector in economic growth.

Finance is not even discussed in a collection of essays by the "pioneers of development economics" (Meier and Seers, 1984), including three Nobel Prize winners, and Nobel Laureate Robert Lucas (1988, p.6) dismisses finance as an "over-stressed" determinant of economic growth. Joan Robinson (1952, p. 86) famously argued that "where enterprize leads finance follows." From this perspective, finance does not cause growth; finance responds to changing demands from the "real sector."

At the other extreme, Nobel Laureate Merton Miller (1988, p.14) argues that, "[the idea] that financial markets contribute to economic growth is a proposition too obvious for serious discussion." Drawing a more restrained conclusion, Bagehot (1873), Schumpeter (1912), Gurley and Shaw (1955), Goldsmith (1969), and McKinnon (1973) reject the idea that the finance-growth nexus can be safely ignored without substantially limiting our understanding of economic growth.

Levine (2004)

Why should we care?

- The *advancement of economic growth* is of major importance, especially for underdeveloped economies
- If the development of the financial system were a determinant of economic growth, one could increase economic growth by improving the financial infrastructure
  $\Rightarrow$ Then the reform of financial systems should be one of the priorities in international development policy
- In fact, we have seen a shift towards a *stronger focus on financial sector reforms* since the 1990s (e. g., International Monetary Fund, World Bank)
- This shift was caused at least partly by the increasing empirical evidence pointing towards a *causal effect of financial development on economic growth*

Some theory

- The development of financial systems may affect economic growth in two ways:
  1. By raising **capital accumulation** (savings rates)
  2. By improving the **resource allocation** and thereby fostering **productivity growth**
- Literature on growth accounting suggests that physical capital accumulation accounts only for a small share of long-run economic growth
- Therefore, the second point has received most of the attention

Theoretical channels

- How financial development may affect economic growth and, in particular, improve the allocation of resources:
  1. Mobilization and pooling of savings
  2. Information production (ex ante)
  3. Monitoring, corporate governance (after provision of finance)
  4. Risk management
- All arguments rely on the idea that financial institutions mitigate *information problems* and/or reduce *transaction costs*
- In complete markets without any frictions (such as asymmetric information and transactions costs), there would be no particular role for financial institutions (**Modigliani/Miller theorem**)

(1) Mobilization and Pooling of Savings

- Financial intermediaries *mobilize* deposits by reducing transaction costs (e. g., by reducing the number of financial contracts) and by dealing better with problems of asymmetric information (see, e. g., Diamond, Review of Economic Studies, 1984)
- Investment projects often require large amounts, depositors would like to offer only small amounts $\Rightarrow$ Financial intermediaries *pool* small deposits and grant large loans
- This affects both the capital accumulation and the quality of resource allocation (because otherwise some projects would be limited to an inefficient scale)

(2) Information Production (Ex Ante)

- Capital can only flow towards its most efficient use if there is *information* about the "quality" of different investment projects
- Individual savers often do not have the ability and incentive to collect information on the quality of investment projects
- With better information production, capital will be invested more efficiently, which raises the productivity of investments

(3) Monitoring and Corporate Governance

- **Monitoring** and **corporate governance** may improve the allocation of capital within firms
- Individual savers are not able and willing to monitor firms
- Delegating monitoring to a financial intermediary may reduce monitoring costs (But who monitors the monitor? See Diamond, Review of Economic Studies, 1984)
- Managers have incentives to pursue projects that provide the largest benefits for themselves, and not necessarily for the firm
- Functioning markets may foster corporate governance: Link manager compensation to stock prices, threat of take-overs

(4) Risk Management

- Financial development may improve cross-sectional **risk diversification** (across different assets), **intertemporal risk-sharing**, and reduce **liquidity risk**
- Seminal paper on **liquidity insurance** through financial intermediaries: Diamond/Dybvig (Journal of Political Economy, 1983)
- Risk diversification and risk sharing may affect economic growth: Investment in risky projects (and hence innovation) is more likely if such risks can be diversified or shared with other agents

Alternative frameworks for analyzing financial systems

Several alternative frameworks for analyzing financial systems have been suggested by the literature.

For example, Merton and Bodie (1995) suggest to focus on the following 6 functions to analyze financial systems:

1. Clearing and Settling Payments
2. Pooling resources and subdividing shares
3. Transferring resources across time and space
4. Managing risk
5. Providing information
6. Dealing with incentive problems

These functions are relevant for several levels of analysis:

- Level of the system
- Level of an institution
- Level of an activity
- Level of a product

3. Does finance benefit society?

The following is based on the presidential address by Luigi Zingales from the University of Chicago (JF 2015) to the American Finance Association annual meeting in 2015.

- The idea that finance benefits society seems obvious to most academic economist.
- However, surveys show a different picture for the society at large
  - 57% of readers of the Economist disagree with the statement that "financial innovation boosts economic growth".
  - 48% of Americans think that finance hurts the U.S. economy, while only 34% think that it benefits it (December 2014).

Zingales' suggestions for the economic profession

- Economists "should acknowledge that our view of the benefits of finance is inflated"
  - An industry does not pay $138 billion in fines in two years if nothing is wrong.
- Theoretical reasons and empirical evidence suggest that a component of finance has been pure rent seeking. Economic research should focus more on this aspect.
  - Economists should be the "watchdogs of the financial industry, not its lapdogs."
- Economists should also review how they teach finance
  - The study of Economics and Finance is associated with higher selfishness of students.
  - Self-selection does not seem to be the only explanation.
  - Experimental evidence suggests that the study of economics encourages students to lean towards profit-maximisation (e.g., Rubinstein 2006).

Post-Crisis Literature: Too Much Finance?

- In light of the recent financial crisis, the consensus about a constant beneficial relation between financial development and economic growth has started to crumble
  - Not all financial activities may be beneficial for economic growth (especially non-intermediation activities)
  - Non-linear relationship between financial development and economic growth
  - Diversion of human capital from more productive activities

Post-Crisis Literature: Too Much Finance?

- Arcand, Berkes, and Ugo Panizza (2012) find a non-linear effect of financial depth on output growth
  $\rightarrow$ The marginal effect becomes negative above a credit-to-GDP ratio of 80 - 100 %
- Langfield and Pagano (Economic Policy, 2015) find that bank-biased financial structures are associated with higher systemic risk and lower economic growth
  - This negative effect of a bank-based financial structure seems to be particularly strong during housing market crises

$\Rightarrow$ Can the differential results partly be explained by *changing ideologies?*

Course overview

IV Cross-Sectional Regressions

Outline

1. How to measure financial development
   - Beck (2015), Finance and Growth - beware the measurement
2. Early empirical evidence
   - Goldsmith (1969), Financial structure and development
   - King and Levine (1993), Finance and growth: Schumpeter might be right
3. Banks vs. markets
   - Levine (2002), Bank-based or market-based financial systems: Which is better?
4. Bank-level Evidence

1. How to Measure Financial Development?

- *Start from theory!*
- Indications from theory:
  - Access to finance through financial intermediaries, markets, and even firms (trade credit)
  - Quality of screening and monitoring
  - Access to finance for innovating firms
  - Corporate governance of firms (including banks)
  - Diversification and risk-sharing opportunities

How to Measure Financial Development?

- Common measures of **financial development**:
  - Assets of financial intermediaries / GDP
  - Loans to private firms / GDP
  - Stock market capitalization / GDP
  - Trading volume / stock market capitalization (turnover ratio, liquidity of the stock market)
  - Institutional aspects: Regulation, government ownership of banks,...
  - ...
- All these measures capture very different aspects of the financial system
- Moreover, all of these variables are subject to **measurement error**

Beware the measurement

Beck (VoxEU 2015) stresses that the *ratio of private credit to GDP*, which is often used in the literature, is a crude proxy measure for financial development.

- It measures quantity, not quality
- It only focusses on the regulated financial institutions
- It does not capture the maturity structure
- It does not capture the ease with which enterprises and households can access credit
- It is not clear that a higher ratio is associated with a more efficient and developed financial markets
  - Increases in private credit to GDP might reflect credit bubbles

Nevertheless

> *Private credit-to-GDP is the worst indicator to measure financial development, except for all others that are available.*

2 Early Empirical Evidence: Goldsmith (1969)

- First systematic analysis of the relationship between financial development and economic growth: *Goldsmith (1969)*
  - 35 countries
  - Time period: 1860–1963 (very long!)
  - Measure of financial development: Assets of financial intermediaries / gross national product
  - Main result: Strong *positive correlation* between financial development and gross national product

Goldsmith (1969) - Weaknesses

- Question: Is there a **causal** relationship between financial development and economic growth?
  - Can economic growth be raised by improving financial institutions, or is the development of the financial system simply a response to economic development = **reverse causality**?
- Answering this question is crucial if one wants to derive any policy implications

Goldsmith (1969) - Weaknesses

- Correlation analysis is *not* well-suited to identify causal effects
  $\Rightarrow$ **Lack of identification strategy**
- Other weaknesses:
  1. Small set of countries (but long time period)
  2. No analysis of sources of economic growth (capital accumulation vs. productivity growth)
  3. No control variables
     $\Rightarrow$ **Omitted variable problem**

King and Levine (1993) [KL]

- Important early paper: *King and Levine (Quarterly Journal of Economics, 1993)*
  - 77 countries
  - Time period: 1960–1989 (much shorter than Goldsmith)
  - Alternative *dependent variables* for economic growth
  - Alternative *explanatory variables* for financial development
  - Inclusion of *control variables*

KL - Dependent Variables

- King and Levine also analyze the *sources* of economic growth (productivity growth vs. capital accumulation)
- Dependent variables:
  - GYP: Average real per-capita GDP growth
  - GK: Average real per-capita growth of capital stock
  - INV: Average annual investment / GDP
  - EFF: "Efficiency" = growth that cannot be attributed to capital growth, Solow residual $= GYP - 0.3 \cdot GK$

KL - Explanatory Variables

- Measures of **financial development**:
  - LLY: Liquid liabilities (M3) / GDP
  - BANK: Deposit bank domestic credit / total domestic credit including central bank
  - PRIVATE: Credit to the private sector / total domestic credit (both excluding interbank credit)
  - PRIVY: Credit to non-financial private sector / GDP
- All indicators are *averaged* over 1960–1989

KL - Control Variables

Control variables:

- Log of initial income
- Log of initial secondary school enrollment rate
- Government expenditure / GDP
- Inflation rate
- (Exports + imports)/GDP

KL - Estimation

- Empirical model:

$$Y_i = \alpha + \beta F_i + \gamma X_i + \varepsilon_i, \quad i = 1, ...77$$

- Y_i = *average* economic growth in country i (measure of steady-state growth)
- F_i = *average* financial development of country i
- X_i = set of control variables

KL - Results

TABLE VII
GROWTH AND CONTEMPORANEOUS FINANCIAL INDICATORS CROSS-COUNTRY: 1960–1989

Dependent Variable	*LLY*	*BANK*	*PRIVATE*	*PRIVY*
GYP	0.024***	0.032***	0.034***	0.032***
	(0.009)	(0.010)	(0.010)	(0.010)
	[0.007]	[0.005]	[0.002]	[0.002]
R^2:	0.50	0.50	0.52	0.52
GK	0.022***	0.022**	0.020**	0.025***
	(0.006)	(0.008)	(0.008)	(0.007)
	[0.001]	[0.012]	[0.011]	[0.001]
R^2:	0.65	0.62	0.62	0.64
INV	0.097***	0.133***	0.115***	0.102***
	(0.029)	(0.038)	(0.036)	(0.034)
	[0.001]	[0.001]	[0.002]	[0.004]
R^2:	0.46	0.46	0.45	0.44
EFF	0.018**	0.026**	0.027***	0.025***
	(0.008)	(0.010)	(0.009)	(0.009)
	[0.026]	[0.010]	[0.003]	[0.006]
R^2:	0.42	0.43	0.45	0.44

(standard errors in parentheses) [*P*-values in brackets] Observations = 77

KL - Results

- Financial development has a statistically significant, positive impact on economic growth, capital accumulation, investment, "efficiency" (at least at 5 %-level)
- **Economic significance**: How large are these coefficients economically? (This is important for the interpretation!)
- Example: Coefficient of PRIVY in regression of GYP $= 0.032$
  - How is this interpreted?
  - $\Rightarrow$ Increasing PRIVY by 1 ($=$ 100 percentage points) increases GDP growth by 3.2 percentage points
  - $\Rightarrow$ Is this a large effect?
    (you have to know the units of measurement, look at descriptive statistics in Table I!)

KL - Results/ Descriptive Statistics

TABLE I

THE AVERAGE LEVEL OF FINANCIAL DEVELOPMENT AND THE CONTEMPORANEOUS GROWTH RATE OF REAL PER CAPITA GDP: 1960–1989

	Very fast	Fast	Slow	Very slow	Correlation with growth	(*P*-value)
LLY	0.60	0.38	0.29	0.22	0.55	(0.001)
BANK	0.81	0.73	0.71	0.60	0.44	(0.001)
PRIVATE	0.70	0.56	0.61	0.51	0.37	(0.001)
PRIVY	0.35	0.27	0.20	0.13	0.50	(0.001)
GYP	0.045	0.026	0.014	−0.005		

Very fast: $GYP > 0.03$, Fast: $GYP > 0.02$ and < 0.03, Slow: $GYP > 0.005$ and < 0.02, Very slow: $GYP < 0.005$.

LLY = Ratio of liquid liabilities to GDP, *BANK* = Deposit money bank domestic credit divided by deposit money bank plus central bank domestic credit, *PRIVATE* = Ratio of claims on the nonfinancial private sector to total domestic credit, *PRIVY* = Ratio of claims on the nonfinancial private sector to GDP, *GROWTH* = Average annual real per capita growth, 1960–1989.

Observations: Approximately twenty in each of the four categories.

KL - Results/ Interpretation

- Example: Raising PRIVY from the mean of the slowest growing quartile of countries (0.13) to the mean of the fastest growing quartile of countries (0.35) raises annual real GDP growth by $0.22 \cdot 3.2 = 0.704$ percentage points
  $\Rightarrow$ This is a lot! (23 % accumulated over 30 years)
- Compare with actual growth difference between these country groups = 5 percentage points, PRIVY explains about 14 percent of this growth differential

KL - How Credible Are These Results?

- Poor *measurement* of financial development (strong focus on banks, see Levine and Zervos, American Economic Review, 1998, for an analysis of the effect of stock market development on economic growth)
- Financial system is **not exogenous**, but it may depend on economic growth (**reverse causality/ simultaneity**)
- In addition, there will still be many **omitted variables**
  - Formally: $E(\varepsilon|F, X) \neq 0$
- Hence: Causal effect is *not identified*

KL - Using Initial Values of Financial Development

- Robustness check in King/Levine (1993): Use of **initial values** (1960) of financial development
  - Idea: Financial development may not be exogenous, but *initial* financial development is **predetermined**
  - Assumption: Future growth does not affect initial financial development
- But: Financial development may depend on *expected* economic growth ⇒ For a given past growth performance, banks are more likely enter countries with better growth prospects

KL - Robustness

- Results appear to be **robust** to the use of initial values of financial development
- Coefficient of financial development is of the same order of magnitude
- Although this is still not entirely convincing, it strengthens the case for a causal effect of financial development on economic growth

Critique

- Cross-sectional regressions suffer from a number of shortcomings:
    1. Weak link between estimation and **economic theory** (i. e., the precise microeconomic mechanism that drives the effect of financial development on economic growth)
    2. Poor **measurement** of financial development (strong focus on banks)
    3. Poor **identification** of causal effects

3. Banks vs. Markets: Levine (2002)

- Cross-country regression as before (48 countries, 1980–1995)
- Empirical model:

$$Y_i = \alpha + \beta F_i + \gamma S_i + \delta X_i + \varepsilon_i$$

- Y_i = *average* real per-capita GDP growth in country i
- F_i = *average* financial development of country i
- S_i = *average* financial structure of country i (higher value denotes a more market-based system)
- X_i = Control variables
- Note: γ measures the effect of financial structure *conditional* on general financial development

Levine (2002) Hypotheses

- Hypothesis 1 (**Bank-based view**): A bank-based system is more beneficial for economic growth $\rightarrow \beta > 0, \gamma < 0$.
- Hypothesis 2 (**Market-based view**): A market-based system is more beneficial for economic growth $\rightarrow \beta > 0, \gamma > 0$
- Hypothesis 3 (**Financial services view**): What matters is the general development of the financial system; financial structure is irrelevant $\rightarrow \beta > 0, \gamma = 0$
- Hypothesis 4: Banks are particularly important when a country is at a low stage of *economic development*: Add an interaction term $+\kappa S_i Y_i \rightarrow \gamma < 0, \kappa > 0$
- Hypothesis 5: Banks are particularly important when a country has a weak *legal system*: Add an interaction term $+\lambda S_i L_i \rightarrow \gamma < 0, \lambda > 0$

Levine (2002) Results I

- Structure variables are *never statistically significant*, interaction terms are also insignificant
- Results on financial development are similar as before
- This contradicts the bank-based **and** the market-based view, but is consistent with the **financial services view**
  - Problem: It is not random which country has which type of financial system $\rightarrow$ Financial structure is **endogenous**
  - However, results from IV and panel estimations are very similar
  - Interpretation: Economic growth depends on *general financial development*, but not so much on the institutional features of the financial system

Levine (2002) Results II

- Results from Levine (2002) complement Levine/Zervos (American Economic Review, 1998): When stock market development **and** banking development are included jointly in a growth regression, *both* are statistically and economically significant
- This suggests that banking **and** market development are beneficial for economic growth and that banks and markets perform *different*, and potentially *complementary*, functions

4. Bank-level Evidence I

Besides (macroeconomic) country-level studies, there exist a large number of (microeconomic) bank-level studies on the question whether banks provide services that financial markets cannot provide

- James (Journal of Financial Economics, 1987) finds *positive announcement effects* of new loan agreements or loan renewals (but not of security issues) in **event studies**
  - In contrast, issuing equity-like instruments leads to **negative** abnormal returns

Bank-level Evidence II

- Lummer and McConnell (Journal of Financial Economics, 1989) find abnormal returns only for *loan renewals*: Effects are positive if loan conditions change favorably and negative if conditions worsen; strong negative effects are found when loans are canceled
  - This is consistent with the idea that bank collect information during **bank relationships**; changes in the relationship can signal changes in borrowers' creditworthiness

Bank-level Evidence III

- Slovin, Sushka, and Polonchek (Journal of Financial Economics, 1993) find that a *bank failure* (in this case by Continental Illinois) leads to negative abnormal returns of firms that maintained a close relationship with that bank
  - Reason: Private information that was collected during the bank relationship cannot easily be transferred
  - Hence, the microeconomic evidence strongly suggests that **banks are special** and that their services cannot easily be replicated by financial markets

Course overview

V Instrumental Variables Estimation

Outline

1. Review of IV Estimation
   1.1 Motivation and Idea of IV Estimation
   1.2 IV Estimator with 1 regressor and 1 instrument
   1.3 Two-Stage Least Squares (TSLS)
   1.4 Tests in IV Estimation
   - Reading material: Wooldridge, Chapter 15
2. Applications from the finance and growth literature
   - La Porta, Lopez-de-Silanes, Shleifer (JPE 1998), Law and Finance
   - La Porta, Lopez-de-Silanes, Shleifer, Vishny (JF 1997), Legal Determinants of External Finance
   - Levine, Loayza, Beck (JME 2000), Financial intermediation and growth: Causality and causes
3. Student presentations
4. Tutorial

V.1.1 Motivation and Idea of IV Estimation

- **Instrumental variables estimation (IV estimation)** = method to obtain consistent estimators if (at least) one regressor is correlated with the error term (violation of the central OLS assumption **MLR.4**)
- It does not matter why x and u are correlated, IV is a general solution to this problem independent of its source
- Most important applications: **Omitted variables**, **simultaneity**, **measurement error** in an independent variable

Omitted Variables and IV Estimation

- The omission of a variable leads to **biased/inconsistent** OLS estimators if
  1. the omitted variable is a determinant of y and
  2. the omitted variable is correlated with at least one included x-variable
- Sometimes the problem can be *ignored* if one can argue that the coefficient of interest is biased in the direction of the null hypothesis and the null hypothesis can nevertheless be rejected (*"conservative" estimation*)
  - Often this is not a satisfactory solution

Solutions

1. Inclusion of the omitted variable or of a **proxy variable**
   - Often this is not possible
2. **Panel data methods** (fixed effects estimation, estimation in first differences): One can control for *time-invariant* unobserved variables by *differencing them out* (more on this later)
   - Panel data is often not available
   - This does not work if one is interested in the effect of a time-invariant variable
   - This solves the problem only partly if the omitted variable varies over time
3. **Instrumental variables estimation**

Idea of IV estimation

- If we cannot control for or difference out an omitted variable, it becomes part of the error term
- Consider the following regression model:

$$y = \beta_0 + \beta_1 x + u$$

- Problem: $Cov(x, u) \neq 0 \Rightarrow$ OLS estimator is **biased / inconsistent**
- Idea of IV estimation: Find a variable z that is correlated with x, but not with the error term u

Validity of Instrumental Variables

- In order to yield consistent estimators, instrumental variables have to satisfy 2 conditions:
  1. **Exogeneity** of the instrument: $E(u|z) = 0 \Rightarrow Cov(z, u) = 0$
  2. **Relevance** of the instrument: $Cov(z, x) \neq 0$
- Note: An already included regressor *cannot* serve as an instrument (**exclusion restriction**)
- Can an omitted variable serve as an instrument?
- Condition 1 cannot fully be tested, whereas condition 2 can easily be tested through a simple OLS regression of x on z and a t-test on the coefficient of z
  - Remember: OLS estimator of slope coefficient in simple regression = covariance of x and z divided by the variance of z

Standard Example: Returns to Education

- Goal: Estimation of returns to education by regressing log income on years of education
- Problem: *Ability* is unobservable and likely to be correlated with the educational decision
- Do the following variables satisfy the two conditions of a valid instrument (exogeneity, relevance) ?
  - Last four figures of the number of the identity card
  - IQ
  - Parental education
  - Number of children in the family

Further Examples

- Does putting criminals in jail reduce crime?
  - What are the concerns for a simple OLS estimation?
  - What are potential instruments?
- Does aggressive treatment of heart attacks prolong lives?
  - What are the concerns for a simple OLS estimation?
  - What are potential instruments?

See Stock/ Watson (2012), p. 490-495.

V.1.2 IV Estimator with 1 regressor and 1 instrument

Derivation of the IV Estimator (1 Regressor, 1 Instrument)

- Regression model:

$$
\begin{aligned}
& y = \beta_0 + \beta_1 x + u \\
& \qquad \Rightarrow Cov(y,z) = Cov(\beta_0 + \beta_1 x + u, z) \\
& \qquad\qquad\qquad\quad = 0 + \beta_1 Cov(x,z) + Cov(u,z) \\
& \qquad\qquad\qquad\quad = \beta_1 Cov(x,z) \quad \text{if instrument is } \textbf{exogenous}
\end{aligned}
$$

- This implies:

$$
\beta_1 = \frac{Cov(y,z)}{Cov(x,z)} \quad \text{if } Cov(x,z) \neq 0 \text{ (instrument is } \textbf{relevant}\text{)}
$$

- Again application of **analogy principle** (replace theoretical moments by sample moments)
- Result: **IV estimator** is the *empirical covariance of y and z divided by the empirical covariance of x and z*

Properties of the IV Estimator

- If the instruments are **valid**, the IV estimator is **consistent**
- But: It is **biased**, therefore we need a *large sample* for IV estimation
- Variance of IV estimator depends negatively on the correlation of x and z
  - If x and z are hardly correlated, one speaks of **weak instruments**
- If x and u are *uncorrelated*, one should use OLS, which is **more efficient** in this case

Weak Instruments

- Consequences of **weak instruments**:
  1. *High standard errors* of estimators
  2. *Bias/inconsistency* of estimators
- Note: If z and u are correlated (violation of **exogeneity**), the asymptotic bias of the IV estimator can be substantial if the correlation of x and z is small relative to the correlation of z and u
- The IV estimator has a *smaller bias* than the OLS estimator if $Corr(z,u)/Corr(z,x) < Corr(x,u)$
- General rule: One should only use **strong** instruments

Goodness of Fit

- R^2 is *not* a suitable measure to judge the quality of an IV estimation
  - Reason: Variance decomposition (SST = SSE + SSR) assumes that x and u are uncorrelated (which is not the case here)
  - Hence, R^2 does not have a natural interpretation and can even be negative
  - Moreover, the explanatory power of the regression is not very large by construction
    (other than in typical OLS regressions, a high R^2 is *not* important in IV estimations)

V.1.3 Two-Stage Least Squares (TSLS) estimator

- So far we assumed that the number of instruments is equal to the number of "endogenous" x-variables
- How do we deal with the situation where we have *more* valid instruments than endogenous variables?
- Idea: We choose the *"best"* linear combination of all valid instruments, i. e., the linear combination that is most strongly correlated with the endogenous variable
  $\Rightarrow$ **Fit** of a regression of the endogenous variable on all instruments (plus the included exogenous variables)

Estimation with TSLS

- *2 stages:*
  1. Regress the endogenous variable on the instruments and all included exogenous variables and derive the **fit** of this regression
     - Idea: The part of x that is correlated with z is by assumption uncorrelated with the error term u of the original regression
     - Reason: **Exogeneity** of z
     - Note: The larger the correlation of x and z, the more variation of x is left (test for relevance!) and the more precise the IV estimator will be
  2. Regress y on the fit from stage 1 and on the included exogenous variables
- The resulting TSLS estimators are **consistent** because they satisfy the central OLS assumption **MLR.4** by construction

Several Endogenous Variables

- TSLS can also be applied when there are *several endogenous variables*
- But: We need more instruments
- **Order condition**: We need at least as many excluded exogenous variables (instruments) as included endogenous variables
- Note: Here we have to estimate a separate first stage for *each* endogenous variable
- When there is exactly one endogenous variable and one instrument, the **TSLS estimator** coincides with the simple **IV estimator**

V.1.4 Tests in IV Estimation

- *3 important tests:*
  1. Test of **endogeneity**: Is IV estimation necessary? Is the error term really correlated with the x-variables?
  2. Test of **relevance**: Is the instrument sufficiently correlated with the endogenous RHS variable?
  3. Test of **overidentifying restrictions**: Are the employed instruments exogenous?
- Note: For tests 1 and 3 we need **valid instruments**!
- The Stata command `ivreg2` implements many different tests that are useful in the context of IV estimation

1. Test of Endogeneity

- *Two-stage test:*
  1. Regress the endogenous x-variable on all instruments and included exogenous variables and calculate the **residuals** of the regression
  2. Plug the residual from the first stage as additional regressor in the structural equation, estimate the regression with OLS, and test for **significance** of the coefficient of the residuals (t-test)
- Intuition: The residuals are the part of x that is potentially correlated with u (the remaining part is by assumption uncorrelated)
  $\Rightarrow$ Significant effect of the residuals suggests that there is a correlation of x and u

1. Test of Endogeneity

- If the coefficient of the residuals is significantly different from zero, the regressor is **endogenous** and IV estimation is to be preferred
- If the coefficient is *not* significant, one should use OLS, which is *more efficient*
- Note: The test works only under the assumption that the instruments are **valid** (i. e., exogenous and relevant)

2. Test of Relevance

- **Relevance** can easily be tested in the first stage of 2SLS regression through a simple **t-test** on the coefficient of z (or an **F-test** on all z in the case of several IVs)
- Null hypothesis: The coefficients of all IVs are equal to zero in the first stage (test of **joint significance**)
  - Remember: OLS estimator of slope coefficient in first stage $=$ covariance of x and z divided by the variance of z
- But: Standard critical values may be *misleading* and are not sufficient to rule out **weak instruments**

2. Test of Relevance

- *Rule of thumb*: With one endogenous RHS-variable, the F-statistic should be at least 10 (corresponding to a t-statistic of 3.16 in case of just one IV)
- Further details can be found in Staiger/Stock (Econometrica, 1997)
- A formal test of *weak identification* was developed by Stock and Yogo (2005)

3. Test of Overidentifying Restrictions

- Question: How can we test whether the employed instruments are **exogenous**?
- If there are *exactly as many* instruments as endogenous x-variables (**exact identification**), the exogeneity of instruments *cannot* be tested
- If there are *more* instruments than endogenous x-variables (**overidentification**), it can be tested whether the **overidentifying** instruments are exogenous (under the assumption that the instruments needed for identification are valid) $\Rightarrow$ **Test of overidentifying restrictions**
- Example of a simple test (not used in practice):
  - Estimate an IV model using just as many (valid) instruments as needed for exact identification
  - Consider the correlation of the residuals from IV estimation with the additional instruments

J-Test of Overidentifying Restrictions

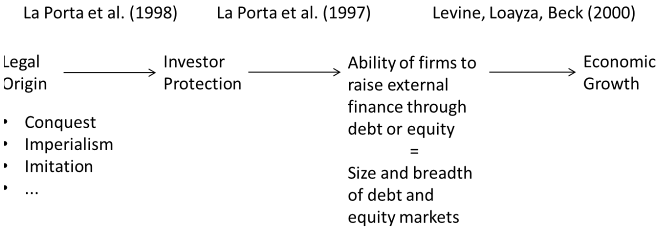
- Test procedure:
  1. Calculate the **residual** from **TSLS** estimation
  2. Regress the residual on all (included and excluded) exogenous variables and calculate the R^2 (a high R^2 points towards a correlation of the exogenous variables and the error term)
  3. Test statistic: Under H_0 (exogeneity of instruments), $n \cdot R^2$ is asymptotically χ^2-distributed (degrees of freedom = number of **overidentifying restrictions**)
- Under the null hypothesis, the residual (= estimator of the error term) is *not correlated* with any of the exogenous variables
- Note: In an exactly identified model, the R^2 from the second step is equal to zero by construction $\Rightarrow$ Test not possible

Conclusion

- IV estimation allows for **consistent** estimation of parameters even if there is a correlation of the regressors and the error term
- But: IV estimation yields reliable results only if the instruments are **valid**, i. e., if they are **relevant** and **exogenous**
- Otherwise, the results from **IV** estimation are not necessarily more reliable than those from **OLS** estimation
- In practice, it is very hard to find valid instruments
- Trade-off between **bias** and **efficiency**

V.2 Application: Finance and growth - Using Legal Origin As Instrumental Variable

- La Porta, R., F. Lopez-de-Silanes, A. Shleifer and R. W. Vishny (1998): "Law and Finance," *Journal of Political Economy*.
- La Porta, R., F. Lopez-de-Silanes, A. Shleifer and R. W. Vishny (1997): "Legal Determinants of External Finance," *Journal of Finance*.
- Levine, R., N. Loayza and T. Beck (2000): "Financial Intermediation and Growth: Causality and Causes," *Journal of Monetary Economics*.



Estimation

- The potential **endogeneity of financial development** can be tackled with an IV procedure
- A valid instrumental variable has to satisfy two conditions:
  1. **Relevance**: It has to be correlated with financial development.
  2. **Exogeneity**: It should affect economic growth only through its effect on financial development (and on the other covariates) and should not itself be a function of economic growth.

Legal Origin as Instrumental Variable

- La Porta, Lopez-de-Silanes, Shleifer, Vishny (LLSV, JF 1997, JPE 1998) distinguish four different legal systems: English, French, German, Scandinavian
- They show that legal origin is an important determinant of the financial infrastructure of a country
  $\Rightarrow$ **Relevance**!
- Example: Strong correlation between legal origin and investor protection, contract enforcement etc.
- Claim: Legal systems are **exogenous** because most countries adopted them involuntarily (through occupation or colonization)
- In any case, economic growth (in the 20th century) is unlikely to have driven the adoption of legal systems
- Moreover, legal origin is **predetermined**

Levine, Loayza, Beck (JME 2000)

- See also: Beck/Levine/Loayza (JFE 2000)
- Data:
  - 71 (63) countries
  - Time period: 1960-1995
- Measures of financial development (again strong focus on banks!)
  - LIQUID LIABILITIES: Cf. LLY above
  - COMMERCIAL-CENTRAL BANK: Cf. BANK
  - PRIVATE CREDIT: Cf. PRIVY
- Broad set of control variables
- Alternative instruments: Religion, distance from equator

Table 2
Legal origin and financial intermediary development, 1960–1995[a]

	Financial intermediary development					
	Liquid liabilities		Commercial-central bank		Private credit	
C	3.829	0.958	4.506	3.063	4.027	– 0.674
	(*0.000*)	(*0.081*)	(*0.000*)	(*0.000*)	(*0.000*)	(*0.386*)
ENGLISH	– 0.134	0.249	– 0.170	0.022	– 0.717	– 0.090
	(*0.325*)	(*0.038*)	(*0.002*)	(*0.716*)	(*0.002*)	(*0.646*)
FRENCH	– 0.434	– 0.052	– 0.270	– 0.078	– 0.894	– 0.268
	(*0.001*)	(*0.703*)	(*0.000*)	(*0.152*)	(*0.000*)	(*0.190*)
GERMAN	0.477	0.683	0.048	0.152	0.401	0.738
	(*0.016*)	(*0.000*)	(*0.100*)	(*0.010*)	(*0.076*)	(*0.002*)
INCOME		0.330		0.166		0.541
		(*0.000*)		(*0.000*)		(*0.000*)
Obs.	71	71	71	71	71	71
Prob(F-test)	*0.001*	*0.000*	*0.040*	*0.000*	*0.000*	*0.000*
R-square	0.23	0.44	0.12	0.30	0.26	0.55

Table 3

Financial intermediation and growth: cross-section regressions, 1960–1995. Dependent variable: Real per capita GDP growth, 1960–1995. Instrumental variables: legal origin dummy variables[a]

Explanatory variable	Coefficient	Standard error	*t*-statistic	*P*-value	Number of observations	J-statistic	Hansen-test OIR
Regression Set #1: simple conditioning information set							
PRIVATE CREDIT	2.515	0.814	3.090	0.003	71	0.00189	0.13
COMMERCIAL-CENTRAL BANK	10.861	3.086	3.520	0.001	71	0.01626	1.15
LIQUID LIABILITIES	1.723	0.844	2.041	0.045	71	0.03491	2.48
Regression Set #2: policy conditioning information set							
PRIVATE CREDIT	3.222	1.245	2.589	0.012	63	0.00799	0.50
COMMERCIAL-CENTRAL BANK	9.641	4.039	2.387	0.021	63	0.0373	2.35
LIQUID LIABILITIES	2.173	0.908	2.394	0.020	63	0.037999	2.39
Regression Set #3: full conditioning information set							
PRIVATE CREDIT	3.356	1.150	2.918	0.005	63	0.02239	1.41
COMMERCIAL-CENTRAL BANK	11.289	3.258	3.465	0.001	63	0.00325	0.20
LIQUID LIABILITIES	2.788	0.903	3.089	0.003	63	0.03901	2.46

[a]Critical values for Hansen-Test Over Identifying Restrictions (2 d.f.): 10% 4.61; 5% = 5.99.

Are the Instruments Valid?

- IVs are motivated on the basis of *economic theory* - nice!
- IVs seem to be **relevant**
  - But: First-stage regression does not seem to include all relevant control variables
- Null hypothesis of **exogeneity** of instruments cannot be rejected in J-test/Hansen-test of **over-identifying restrictions**
- Hence, the tests tend to support the validity of instruments
- But: Do you really think that legal origin is exogenous?

Results

- Financial development has again a **statistically** and **economically significant** positive effect on economic growth
- If we were willing to accept that legal origin is a valid IV, this effect would have a causal interpretation
- However, the exogeneity of legal origin has to be questioned $\Rightarrow$ Legal origin affects economic growth through many channels, not only through financial development
- Critique of King/Levine (1993) is also largely relevant for this paper

V.3 Student presentation

- Acemoglu, Johnson and Robinson (AER 2001), The Colonial Origins of Comparative Development: An Empirical Investigation

  [Acemoglu et al. (2001)] exploit differences in European mortality rates [around the year 1800] to estimate the effect of institutions on economic performance.

V.4 Tutorial

- See *eCampus* for data and documents.

Course overview

VI Panel methods and fixed effects estimation

Outline

1. Introductory remarks
2. Estimation with repeated cross-sections
   - Test for structural break
3. Simple estimations with panel data
   - Pooled OLS
   - Estimation in first differences
4. Fixed effects estimation
   - Within-transformation and between-transformation
   - Dummy variable regression
   - Interpretation of estimated fixed effects
5. Fixed effects estimation vs. estimation in first differences
6. Concluding remarks

VI.1 Introductory remarks

- Reading material: *Wooldridge, Chapters 13 and14*
- The key for identifying causal effects is to control for omitted factors
- If these are *observed*, we can enter them as **control variables**
- If they are *unobserved*, we can try to fix the problem with **IV estimation**
- If we do not have valid instruments, we have to look for alternative ways of identification
- Here: We exploit the fact that the same (or similar) subjects are *observed repeatedly* over time

Data issues

- Estimation methods differ depending on whether we have **repeated cross-sections** or **panel data**
- In the following, we assume that the number of subjects in the cross-section is large, whereas the time dimension is relatively small (large N, small T)
- This is important for the derivation of asymptotic distributions (the asymptotical analysis runs through N, not T)
- Under this condition, we can ignore problems typical for time series analysis (non-stationarity, spurious regression, unit roots)

VI.2. Estimation With Repeated Cross-Sections

- Advantage of repeated cross-sections relative to simple cross-section = *larger sample*:
  - More precise estimation
  - But: This works only if the true regression model does not change over time (*no* **structural break**)
- Simplest estimation method: Multiple linear regression on the basis of *pooled* data (**Pooled OLS**)
- Each observation is treated in the same way, the time structure does not enter explicitly

Changes of the Regression Function Over Time

- What can we do if the regression function is suspected to have *changed* over time?
- A change in the *level* of the dependent variable can be captured by including **time dummies**
  - Note: One dummy variable has to be dropped to avoid **perfect multicollinearity** (typically the first time dummy is dropped = reference period)
- A change in the *slopes* can be modelled through **interaction terms**
- Changes in the model parameters over time can be tested by using t-tests or F-tests
  $\Rightarrow$ **Test for structural break**

Test For Structural Break

- Generate a dummy variable d^{after} that is equal to zero before the conjectured structural break, and equal to one afterwards
- Interact all variables with the dummy variable d^{after}:

$$\begin{aligned} y = & \beta_0 + \beta_1 \cdot x_1 + \cdots + \beta_k \cdot x_k \\ & + \delta_0 \cdot d^{after} + \delta_1 \cdot x_1 \cdot d^{after} + \cdots + \delta_k \cdot x_k \cdot d^{after} + u \end{aligned}$$

- This implies that we estimate *different* equations for *different* time periods
- Splitting the sample and running separate regressions on the subsamples would lead to *identical* regression coefficients

Test For Structural Break

- **Null hypothesis** of the test for structural break:
  $\delta_0 = 0, \delta_1 = 0, \ldots, \delta_k = 0$
  $\Rightarrow$ Test of **joint significance**
- If we can *reject* the null hypothesis, implying joint significance of the coefficients, there is a **structural break**
- Then, we should estimate *different* regression functions for *different* time periods
- An alternative test is to test only for the joint significance of the interaction terms (and not the time dummy)

VI.3 Simple Estimations with Panel Data

- Now: We observe the *same* individuals at several points in time
- Simplest case: N individuals, 2 time periods: $t = 1, 2$
- Problem: "Individuals" (households, firms, countries etc.) differ with regard to many characteristics that are partly *unobserved*
- If we do not control for these factor, there may be an **omitted variable bias**
- Main advantage of panel data: We can relatively easily control for **time-invariant unobserved characteristics**

Regression model

- Regression model for subject i at time t:

$$y_{it} = \beta_0 + \delta_0 \cdot d2_t + \beta_1 \cdot x_{it} + \nu_{it},$$
$$\text{where} \quad \nu_{it} = a_i + u_{it}$$

- Index it denotes individual i at time t (Note: Such indices do not make sense in repeated cross-sections!)
- $d2_t$ = time dummy for second period
  - $\Rightarrow$ Model allows for different intercepts for the two time periods (difference $= \delta_0$)
- Error term ν_{it} consists of two components:
  1. a_i = *time-invariant* **fixed effect**
     $\Rightarrow$ Captures all observed and unobserved time-invariant factors that determine y
  2. u_{it} = idiosyncratic error term
     (= error term without any time-invariant factors)

Pooled OLS

- Simplest estimation method: **Pooled OLS**
- Estimator yields consistent results only if assumption **MLR.4** is satisfied: $E(\nu_{it}|x_{it}) = 0$
- Hence, a_i and u_{it} must not be correlated with x_{it}
- Is this a plausible assumption?

Pooled OLS

- Assumption is not implausible for the idiosyncratic error term u_{it}
- However, the assumption is *questionable* for a_i: It is very likely that the explanatory variables are correlated with the time-constant characteristics
- Example: Estimation of returns to education
  - Dependent variable: Wage of person i
  - a_i is the (unobserved) ability of a person
  - Explanatory variables (e. g., education) are typically correlated with a_i
  - Consequence: Pooled OLS leads to **biased** and **inconsistent** estimators (a_i = **omitted variable**)

Estimation in First Differences

- Simple solution to solve this problem:
  Estimation in **first differences**:

$$
\begin{aligned}
y_{i1} &= \beta_0 + \beta_1 \cdot x_{i1} + a_i + u_{i1} \\
y_{i2} &= \beta_0 + \delta_0 + \beta_1 \cdot x_{i2} + a_i + u_{i2}
\end{aligned}
$$

- Model in first differences:

$$
\begin{aligned}
y_{i2} - y_{i1} &= \delta_0 + \beta_1 \cdot (x_{i2} - x_{i1}) + (u_{i2} - u_{i1}) \\
\Delta y_i &= \delta_0 + \beta_1 \cdot \Delta x_i + \Delta u_i
\end{aligned}
$$

- By first-differencing, the time-invariant effect a_i *disappears*!

Estimation in First Differences

- The model in first differences can be estimated by **OLS**
- Condition for consistency: $E(\Delta u_i | \Delta x_i) = 0$
- Hence, Δu_i and Δx_i must not be correlated
  $\Rightarrow$ u_{it} must be correlated neither with x_{i1} nor with x_{i2}, i. e., the *neighboring* observations (**strict exogeneity**, to be distinguished from *contemporaneous* exogeneity)

Estimation in First Differences

- Typical violation of strict exogeneity: Inclusion of **lagged endogenous variables**
  - y_{i1} (lagged endogenous variable in period 2) is correlated with u_{i1} (error in period 1)
- Note: The parameter β_1 can only be estimated if x_{it} varies over time for at least one individual
- Precision of the estimation depends on the variation of x_{it} over time
- Hence: Effects of **time-invariant** variables *cannot* be estimated with this method!

Estimation in First Differences

- Shortcomings of estimation in first differences:
  - Reduction in the *number of observations*: First observation is lost, all subjects i are lost for which we have only one data point, problem is exacerbated in an **unbalanced panel** (loss of data after each missing value)
  - Reduction in the *variation* of the explanatory variables: x varies typically much more than Δx
  - Consequence of both problems: **High standard errors**
  - Moreover, differencing exacerbates potential problems from **measurement error**, which may lead to **attenuation bias**
- Question: Does it make sense to estimate returns to education through estimation in first differences?

VI.4 Fixed Effects Estimation

- Reading material: *Wooldridge, Chapter 14.1*
- We saw in the last subsection that the fixed effect a_i disappears by differencing
- The same principle underlies **fixed effects estimation**
- Instead of differencing, one uses a **within-transformation** of the data: Calculate the *deviations of all variables from their individual-specific mean over time*
- Again the fixed effect disappears such that the transformed model can by estimated by **Pooled OLS** after the transformation

Regression Model

- Consider the following regression model:

$$\begin{aligned} y_{it} &= \beta_0 + \beta_1 \, x_{it} + \nu_{it}, \quad t = 1, 2, \dots T, \quad i = 1, \dots N \\ &= \beta_0 + \beta_1 \, x_{it} + a_i + u_{it} \end{aligned}$$

  - Inclusion of an intercept in deviation from Wooldridge
  - Assumption: a_i has a mean of 0
  - Extension to several x-variables is straightforward

Within-Transformation

- Averaging the previous equation over time:

$$\bar{y}_i = \beta_0 + \beta_1 \, \bar{x}_i + a_i + \bar{u}_i, \quad \text{where}$$

$$\bar{y}_i = \frac{\sum_{t=1}^{T} y_{it}}{T}, \quad \text{analogously for } \bar{x}_i \text{ and } \bar{u}_i$$

- **Within-transformation**:

$$y_{it} - \bar{y}_i = \beta_1 \, (x_{it} - \bar{x}_i) + (u_{it} - \bar{u}_i), \quad t = 1, 2, ... T, \quad i = 1, 2, ... N$$

- The fixed effect a_i disappears due to the within-transformation ($y_{it} - \bar{y}_i$ is the time-demeaned data on y)

Fixed Effects Estimator (Within-Estimator)

- The transformed model can now be estimated by **Pooled OLS** $\Rightarrow$ **Fixed effects estimator** or **Within-estimator**
  - Why "within"?
  - For the estimation, we do not use the differences in the levels of y across individuals, we only use the *deviations from individual-specific means*
- Note: For consistent estimation we do *not* have to make any assumptions regarding the correlation of a_i and x_{it}
- Typically, one also includes time dummies in fixed effects estimation

Between-Estimator

- In contrast to the fixed effects estimator, an estimator that considers *only* the differences in the levels across individuals is called the **between-estimator**
- The between-estimator is derived from an OLS-Regression of the *means* (see formula above) where a_i is treated as part of the error term
  - Note: Here we only use N observations (instead of $N \cdot T$), i. e. this is a cross-sectional regression
  - The estimator is consistent only if a_i is *not* correlated with x_{it} in *all* time periods
  - But then there exist better estimators
    $\Rightarrow$ Between-estimator is irrelevant in practice

Properties of the Fixed Effects Estimator

- Condition for the consistency of the FE estimator:
  $E(u_{it} - \bar{u}_i | x_{it} - \bar{x}_i) = 0$
  $\Rightarrow$ Idiosyncratic error term u_{it} has to be uncorrelated with the x_{it} in *all* time periods (**strict exogeneity**, assumption stronger than above)
  - A correlation of a_i and x_{it} is unproblematic

- Note: The estimation results are typically presented in *levels*, not in deviations from individual-specific means
  - Reason: Interpretation is easier (coefficients are identical)
  - Regression output typically contains a constant (not present in within-estimation)

Properties of the Fixed Effects Estimator

- The number of **degrees of freedom** of FE estimation is $N \cdot T - k - N$ (and not $N \cdot T - k$)
  - Intuition: When estimating N individual-specific means, we "lose" N degrees of freedom
  - The correction for the degrees of freedom is important, e. g., for the estimation of the standard error of regression (SER) and hence for the standard errors of the estimator
- Question: How to calculate the R^2 of the estimation?
  - R^2 is typically calculated on the basis of the *transformed* model (R^2 of the within-model)
  - The fixed effects already "explain" a large part of the variation in y_{it}, but this is not very informative

Properties of the Fixed Effects Estimator

- Again the effects of **time-invariant** variables (e. g., sex, cohort) *cannot* be estimated
  - Time-invariant variables can be interacted with other variables that vary over time, e. g., with the time dummies
  - Hence, we can estimate *changes* in the effects of time-invariant variables over time even if the effect itself cannot be estimated

Properties of the Fixed Effects Estimator

- Additional problem when we include **time dummies** (for all periods but the first):
  - Now we can no longer identify the effect of variables that increase (or decrease) for every subject by the *same amount* in a given year
    - Example: Macroeconomic variables, age
  - A similar problem arises if we include a **linear time trend** instead of time dummies (we cannot identify the effect of variables that increase or decrease in *all years* and for *all subjects* by the same amount)
    - Example: Age

Dummy Variable Regression

- Alternative view of fixed effects estimation: a_i are parameters to be estimated (one parameter for each individual i)
- This implies that there is a separate intercept for each individual ($\beta_0 + a_i$)
- Estimation: Add a dummy variable for each individual $\Rightarrow$ **Dummy variable regression**
- The number of parameters to be estimated is very large ($N + k$)
  - For estimation, we need at least two points in time

Properties of the Dummy Variable Regression

- The estimation yields *exactly the same* estimator $\hat{\beta}$ and standard errors as fixed effects estimation
  - Correct degrees-of-freedom calculations (this is also true for the Stata command `xtreg,fe`)
  - FE estimation can be seen as the second stage of a **partitioned regression**; first stage: regress y_{it} on N dummy variables and calculate the residuals (used as dependent variables in the second stage)
- But: Dummy variable regression typically yields a very high R^2
  - Reason: It does not abstract from the explanatory power of the dummy variables

Interpretation of Estimated Fixed Effects

- Dummy variable regression directly yields the estimators $\hat{a}_i$, $i = 1, \ldots N$
- The coefficients can also easily be obtained from the FE results:

$$\hat{a}_i = \bar{y}_i - \hat{\beta}_0 - \hat{\beta}_1 \bar{x}_i, \quad i = 1, \ldots N$$

  - A positive $\hat{a}_i$ indicates that an individual has a high y (independently of the x-variables)
  - ... negative $\hat{a}_i$ ... low y...

- In most software packages, the constant from fixed effects estimation is equal to the *average* intercept of all individuals ($\hat{\beta}_0$)

Interpretation of Estimated Fixed Effects

- In most cases, we are interested in the β-parameters and not so much in a_i
- Problem in the interpretation of $\hat{a}_i$:
  - Estimators $\hat{a}_i$ are **not consistent**
  - Reason: Asymptotical analysis runs through N rather than T in panel methods
  - An increase in N raises the number of parameters to be estimated, but it does not improve the estimation of $\hat{a}_i$
  - Only an increase in T would improve the estimation of a_i
  - In most panel data sets, T is relatively small

VI.5 Fixed Effects Estimation vs. Estimation in First Differences

- For $T = 2$, both estimation methods (FE and first differences) lead to *identical* results
- For $T \geq 3$, the estimators are *not* identical
- But: Both estimators are **unbiased** and **consistent** (for $N \to \infty$) under the assumption of **strict exogeneity** (and the other usual assumptions)

Fixed Effects Estimation vs. Estimation in First Differences

- Rules of thumb:
  - If u_{it} is *uncorrelated* over time, the FE estimator is *more efficient* (i. e., it has a smaller variance)
  - If u_{it} follow a **random walk** (i. e., if $u_{it} = u_{it-1} + \nu_{it}$ where ν_{it} is **white noise**) = *strong autocorrelation*, an estimation in first differences is preferable
  - In all other cases the decision is difficult
- One may want to run both estimations to see whether the results differ
- In practice, most researchers prefer fixed effects estimation

Other Problems of the Fixed Effects Estimator

- Fixed effects estimator with *large T*:
  - If T is large, one should be careful in using the FE estimator (especially if N is not very large)
  - Potentially one runs into time series problems (e. g., *spurious regression* due to **non-stationarity**, **unit roots**)
  - In such cases, estimation in first differences is preferable
- **Measurement error** and the FE estimator:
  - Similar to the estimation in first differences, the measurement error problem is exacerbated by the within-transformation (**attenuation bias**)
  - In this case, the FE estimator is preferred because it generally has a smaller bias as long as $T \geq 3$

Fixed Effects Estimation in an “Unbalanced Panel”

- In most panel data sets, we do not observe all individuals at all points in time
- Fixed effects estimation can also be employed in an **unbalanced panel**
  - Note that the individual-specific means have to be calculated on the basis of the correct number of observations T_i
  - The number of total observations is now $T_1 + T_2 + ... + T_N$ (instead of $N \cdot T$)
  - Individuals with just one observation drop out of the analysis

Fixed Effects Estimation in an "Unbalanced Panel"

- Crucial question: Are the missing data distributed randomly, or is there a problem of **sample selection**?
- Note: A correlation of "sample attrition" with time-invariant factors (the fixed effects) is unproblematic in FE estimation

VI.6 Concluding remarks

- Panel methods can also be used when there is no time dimension
- Example: Data with monozygotic twins
  - Unobserved effect = family effect
  - "time" = first twin, second twin
- Analogous application of panel methods
- Methods can be used for any kind of **clusters**

Summary

- Central advantage of panel data: One can control for **time-invariant unobserved heterogeneity** and thereby avoid omitted-variable bias
  - Fixed effects a_i disappear through *differencing* (estimation in first differences) or *within-transformation* (fixed effects estimation)
- Note: If the unobserved heterogeneity varies over time, the bias cannot be removed completely by these methods!
- Fixed effects estimation is one of the most popular estimation methods in empirical research

Course overview

I Introduction

II Econometric foundations

III Economic Foundations of Banking and Finance

IV Cross-Sectional Regressions

V Instrumental Variables Estimation

VI Panel Methods/ Fixed-Effects Estimation

VII Differences-in-Differences Estimation

VIII The Method by Rajan/ Zingales (1998)

IX Further topics

VII Difference-in-Difference Estimation

Outline

1. Introductory remarks
2. Regression model
3. Extensions
4. Application
   - Jayaratne and Strahan (1996): Growth Effects of Banking Deregulation
   - (Schüwer, Lambert and Noth (2019): How do banks react to catastrophic events? Evidence from Hurricane Katrina)
5. Tutorial

Reading material: Wooldridge, Chapter 13.2.

VII.1 Introduction

- Question: What is the effect of a certain event or policy change?
  - What is the effect of higher minimum wages on employment?
  - What is the (hypothetical) effect of bank nationalization on GDP?
  - ...
- A key challenge for such analyses is to consider the counterfactual
  - *What would have happened in the absence of the considered event or policy change?*

Natural Experiments or Quasi-Experiments

- Differences-in-differences procedures are often used to evaluate the effects of *reforms* or other *policy changes* when such changes affect only a *(randomly chosen) subpopulation* $\Rightarrow$ **Natural experiment or quasi-experiment** (= experiment that was not designed by the researcher but by "nature")
- One considers an *exogenous* policy change that affects a group of observations (the **treatment group**), but not the remaining observations (the **control group**)
- Central identifying assumption: The treatment is assigned *randomly* (given the control variables), it must not depend on the dependent variable or its unobserved determinants
- Consequence: In the absence of treatment, both groups of observations would have *behaved similarly over time* (given the control variables)

Challenges

- Example: Evaluation of the effect of a labor market reform that was carried out in some states, but not in others, on unemployment rates
- What should be discussed and explored in the context of such an analysis?
  - Was the reform a reaction of policymakers to labor market developments?
  - If the reform was carried out because unemployment rates were expected to rise in these states in the absence of reform (while the same is not true for other states), this is not a good experiment
- Important reference on natural experiments: Meyer (Journal of Business and Economic Statistics, 1995)

Basic Idea

- In order to evaluate the effect of a policy change, we compare states where the policy change was carried out (**treatment states**) and other states where it was not (**control states**)
- For each group of states, we calculate the difference of the variable of interest *before* and *after* the year of the policy change
- The *difference in the two differences* is attributed to the policy change
- The control states are meant to mimic the behavior of the treatment states in the absence of treatment

Unobserved effects

- Similar to fixed effects estimation, differences-in-differences estimation exploits **repeated observations** and **differencing** procedures in order to control for unobserved effects
- But: Typically one controls for **time-variant** unobserved effects at the *group level* (e. g., state or country)
  - One may also controls for **time-invariant** unobserved heterogeneity at the *subject level*
  - If we do not control for time-invariant unobserved heterogeneity at the subject level, we do not necessarily need panel data, in which the *same* subjects are observed over time; **repeated cross-sections** would be sufficient

VII.2 Regression Model

- Consider a model with 2 periods: $t = 1, 2$
- $d2 =$ dummy variable that turns 1 in the second period
- $dT =$ dummy variable that is one for all members of the treatment group
- Regression model:

$$y = \beta_0 + \delta_0 \cdot d2 + \beta_1 \cdot dT + \delta_1 \cdot d2 \cdot dT + u$$

Interpretation of Regression Parameters

- Regression model:

$$y = \beta_0 + \delta_0 \cdot d2 + \beta_1 \cdot dT + \delta_1 \cdot d2 \cdot dT + u$$

- Interpretation of regression parameters:
  - $\beta_0 =$ expected value of y in period 1 for the control group
  - $\beta_0 + \delta_0 =$ expected value of y in period 2 for the control group
  - $\delta_0 =$ expected change in y from period 1 to period 2 for the control group

Interpretation of Regression Parameters

- Regression model:

$$y = \beta_0 + \delta_0 \cdot d2 + \beta_1 \cdot dT + \delta_1 \cdot d2 \cdot dT + u$$

- Interpretation of regression parameters (ctd.):
  - $\beta_0 + \beta_1 =$ expected value of y in period 1 for the treatment group
  - $\beta_0 + \delta_0 + \beta_1 + \delta_1 =$ expected value of y in period 2 for the treatment group
  - $\delta_0 + \delta_1 =$ expected change in y from period 1 to period 2 for the treatment group
  - $\delta_1 =$ *additional* change in y from period 1 to period 2 for the treatment group (**average treatment effect** = central parameter of interest!)

Interpretation of Regression Parameters

- Regression model:

$$y = \beta_0 + \delta_0 \cdot d2 + \beta_1 \cdot dT + \delta_1 \cdot d2 \cdot dT + u$$

- Alternative interpretation:
  - $\beta_1 =$ expected difference in y in period 1 between the control group and the treatment group
  - $\beta_1 + \delta_1 =$ expected difference in y in period 2 between the control group and the treatment group
  - $\delta_1 =$ expected *change* in the difference in y between the control group and the treatment group from period 1 to period 2
- Model can be estimated easily by calculating the difference in the differences of the respective **sample analogs** (averages)

Estimators

- Regression model:

$$y = \beta_0 + \delta_0 \cdot d2 + \beta_1 \cdot dT + \delta_1 \cdot d2 \cdot dT + u$$

- C = control group, T = treatment group
- $\hat{\beta}_0 = \bar{y}_{1,C}$
- $\hat{\delta}_0 = \bar{y}_{2,C} - \bar{y}_{1,C}$
- $\hat{\beta}_1 = \bar{y}_{1,T} - \bar{y}_{1,C}$
- $\hat{\delta}_1 = (\hat{\delta}_0 + \hat{\delta}_1) - \hat{\delta}_0 = (\bar{y}_{2,T} - \bar{y}_{1,T}) - (\bar{y}_{2,C} - \bar{y}_{1,C})$
  $= (\hat{\beta}_1 + \hat{\delta}_1) - \hat{\beta}_1 = (\bar{y}_{2,T} - \bar{y}_{2,C}) - (\bar{y}_{1,T} - \bar{y}_{1,C})$

  $\rightarrow$ **Differences-in-Differences Estimator**

Differences-in-Differences Estimator

- Why not take *simple differences*?
  - Simple difference over time (after – before) assumes that nothing else has happened between period 1 and 2
  - Simple difference between treatment and control group in period 2 assumes that the two were identical before the reform
- Idea: All changes between period 1 and 2 that are only observed in the treatment group (but not in the control group) are attributed to the treatment
- Identification would hence be impossible in time series data or cross-sectional data
- But: We do not need panel data, repeated cross-sections would be sufficient
  - Additional advantage of panel data: One can control for unobserved heterogeneity at the *individual* level
  - This is only relevant for reforms carried out at the individual level

Identification

- Key **identifying assumption**:
  - In the absence of treatment, the treatment group would have evolved *in the same way* as the control group
- Treatment and control groups may differ, but all the important differences are by assumption captured by the **fixed effect** (coefficient of treatment dummy)
- One should check this *graphically* by comparing the evolution of the variable of interest in the times before and after the reform across the control and the treatment group
- Note: Composition of the two groups must not change due to the policy change (e. g., migration)

VII.3 Extensions

- Regression model can easily be extended in various directions:
  (a) Varying treatment intensities
  (b) Heterogeneous effects
  (c) Further control variables at the micro or group level
  (d) Group-specific trends

(a) Varying Treatment Intensities

- Consider a model with several reforms
- The model above assumes that all reforms are equally strong
  - This is often not the case
- Reforms may be modelled by a *continuous variable* instead of a 0/1 variable
- This allows us to take the *size* of the reform (**treatment intensity**) into account

(b) Heterogeneous Effects

- Consider a model with several reforms
- Model above assumes that all reforms shift the mean by the same amount
  - This is often not realistic
  - Strength of the effect of a reform may depend on group characteristics
- Reform indicator may be *interacted with some group characteristic* that determines how strongly a group is affected by a given reform

(c) Further Control Variables

- One can include additional (time-varying!) *control variables* at the group level
- This may give the identifying assumption more credibility
- Further controls at the micro level are normally not useful to reduce omitted variable bias at the group level, but they may increase efficiency
- Be careful when using micro data on the left hand side and group-level data on the right hand side $\Rightarrow$ This requires an adjustment of standard errors for **within-group correlation** of error terms (**clustered standard errors**)

(d) Group-Specific Trends

- Consider a model with more than two time periods
- Estimated treatment effect may merely capture a **group-specific trend**
- One could therefore run the following regression model:

$$y = \beta_0 + \gamma_0 \cdot t + \delta_0 \cdot d2 + \beta_1 \cdot dT + \gamma_1 \cdot t \cdot dT + \delta_1 \cdot d2 \cdot dT + u$$

- Model allows for group-specific intercepts and group-specific trends
- If the treatment effect is still significant, this strengthens the finding
- If the treatment effect becomes insignificant, this puts the identification strategy into question
- Note: In order to identify group-specific trends and treatment effects, you need a reasonable number of time periods

Test of Identification Strategy: Granger-Type Causality

- Idea: Past reforms may affect the current value of y, but future reforms should not
- Include lags (to measure **post-treatment effects**) and leads (to measure **anticipatory effects**) of the reform
- The finding of significant anticipatory effects would cast doubt on the identification strategy (violation of **random assignment**)

Further Issues

- The two dimensions of the differences-in-differences model do not have to be "groups" and "time," the framework is much more general
- Underlying idea is always that one wants to compare a treatment and a control group
- Setup can also be extended to three or more dimensions
- Differences-in-differences estimation is very popular among empirical researchers

VII.4 Application 1: Growth Effects of Banking Deregulation

- *Jayaratne, Jith, and Philip E. Strahan (1996): "The Finance-Growth Nexus: Evidence from Bank Branch Deregulation," *Quarterly Journal of Economics*, 111, 639–670
- Evaluation of the effect of bank branching deregulation in the United States on state-level growth in a differences-in-differences framework
- Advantage: As with panel methods, we can control for time-invariant unobserved heterogeneity (at the state level)
- But: One has to check carefully whether the **common trend assumption** of control and treatment groups is justified

Data on Branching Restrictions

- Important feature of the data: Deregulation in different states took place at different points in time
- *Timing* of deregulation: End of deregulation process
  - Timing of reforms is always a major issue in natural experiments
- 13 states deregulate before the beginning of the sample period (1972)
- 3 states have not yet deregulated at the end of the sample period (1992)
- The analysis is carried out at the state level (it would also be conceivable to conduct a similar analysis at the bank level)

Empirical Model

- Regression model:

$$Y_{t,i}/Y_{t-1,i} = \alpha_t + \beta_i + \gamma \cdot D_{t,i} + \epsilon_{t,i},$$

  where $i = 1, ...50$ denotes the states and $t = 72, ...92$ denotes the years
- $Y_{t,i}$ is real per capita income (output) in year t and state i
- Hence, the dependent variable is the growth factor ($= 1 +$ growth rate)
- $D_{t,i}$ is a dummy variable that turns 1 in the year of the branching reform

Empirical Model

- The time dummies α_t capture the *common evolution over time*
- The fixed effects β_i capture time-invariant (observed and unobserved) factors at the state level (**state fixed effect**)
- γ is the **causal effect** of interest
- Question: How many coefficients have to be estimated in this model?

Empirical Model

- Model is extended by allowing for *regional business cycles*: Introduce $\alpha_{t,j}$ instead of α_t, where $j \in \{Northeast, South, Midwest, West\}$
- Note that this implies the loss of a large number of degrees of freedom, since we have to estimate $21 \cdot 4$ time dummies instead of 21
- Would it be possible to interact the time dummies with the state dummies?

TABLE II
GROWTH REGRESSIONS: BASIC MODELS

Growth based on personal income:	Estimated percentage point change in growth	Adjusted R^2 (number of observations)
1. Basic model, OLS	0.94*	49%
	(0.26)	(1015)
2. Basic model, WLS	1.19*	70%
	(0.24)	(1015)
3. Regional effects, OLS	0.51*	62%
	(0.23)	(974)
4. Regional effects, WLS	0.59*	78%
	(0.18)	(974)
Growth based on gross state product:		
5. Basic model, OLS	1.03*	43%
	(0.36)	(668)
6. Basic model, WLS	1.08*	65%
	(0.30)	(668)
7. Regional effects, OLS	0.69*	54%
	(0.33)	(641)
8. Regional effects, WLS	0.84*	72%
	(0.24)	(641)

Results

- Coefficient of interest is statistically significant at the 5% level in all specifications
- Effect is economically large: Annual economic growth increases by 0.5 to 1.2 percentage points following bank branching deregulation
- Regional business cycle appears to capture a large part of the effect, but the effect is remains relatively large

Identification

- Crucial question: Is this a **valid natural experiment**?
- Assumption: It is random which states carried out reforms and when they did it
  - Is this reasonable? (The answer would generally be no...)
  - States may have deregulated because they were doing poorly, output went up afterwards not necessarily because of the reforms, but maybe because of the turn of the business cycle
  - States may have deregulated in anticipation of future good growth prospects
- Question cannot be answered conclusively because we cannot control for state-specific business cycles

Critique

- Early and influential paper using the differences-in-differences methodology in the banking context
- Random assignment of reforms is always questionable, but the authors try hard to convince the reader that there is no problem in their case
  $\Rightarrow$ Results appear to be quite robust
- Timing of reforms is a serious issue
- Overall, the paper quite credibly establishes the positive growth effect of branching deregulation

VII.4 Application 2: How do insured deposits affect bank stability? – Lambert, Noth and Schüwer (JFI 2017)

- This paper tests whether an increase in insured deposits causes banks to become more risky.
- Based on the U.S. Emergency Economic Stabilization Act in October 2008, which increased the deposit insurance coverage from \$100,000 to \$250,000 per depositor and bank, the analysis uses a difference-in-difference estimation technique conditional on propensity score matching.
- Details are provided on separate slides.

Course overview

VIII The Method by Rajan/ Zingales (1998)

Outline

1. Identification strategy
2. Data and empirical model
3. Results
4. Variation of the method by Rajan/Zingales
5. Concluding remarks

Reading material: Raghuram G. Rajan and Luigi Zingales (1998): "Financial Dependence and Growth", *American Economic Review*, 88(3), 559-586.

See also Ross Levine (2005), "Finance and Growth: Theory and Evidence", in: Aghian and Durlauf, *Handbook of Economic Growth*, Chapter 12, in particular 3.3.1 Industry level analyses.

VIII.1 Identification Strategy

Paper presents a new methodology of how to identify the causal effect of financial development on economic growth using *disaggregated data* (industry level)

- Identification idea builds directly on one particular *theoretical mechanism* through which financial development may affect economic growth
- Financial markets and institutions mitigate problems of **asymmetric information** and thereby lower the costs of external financing (relative to internal financing) and improve the access to finance
  $\Rightarrow$ *Mitigation of financing constraints*
  $\Rightarrow$ Higher growth of existing firms and establishment of new firms
  $\Rightarrow$ Higher growth at the macroeconomic level

Identification Strategy

- Industries that (for exogenous/technological reasons) depend strongly on **external financing** - in contrast to internal financing, such as retained earnings - should benefit most from financial development
- **Testable hypothesis:** The *growth difference* between industries that depend strongly on external financing and those that do not depend much on external financing should be largest in countries where the financial system is highly developed

Identification Strategy

- Analysis is based on *industry-level data* for a broad set of countries at one point in time
- Data set is a cross-section, but it has a **panel** structure (but no time dimension)
- Dependence of an industry on external financing ("**external dependence**") is measured as the *actual* external dependence of this industry in the United States

VIII.2 Data and empirical model

- 36 industries
- 41 countries
- Economic growth of an *industry* is calculated over the period 1980–1990
- Measures of **financial development**:
  1. *Total capitalization ratio* = (domestic credit + stock market capitalization)/GDP, averaged over 1980–1990
  2. *Accounting standards* (index), 1990
  3. *Domestic credit* / GDP, averaged over 1980–1990

Empirical Model

$$Y_{j,i} = \alpha + \beta X_{j,i} + \gamma \cdot ExternalDependence_j \cdot F_i + \nu_j + \mu_i + \epsilon_{j,i}$$

- $Y_{j,i}$ = average annual real growth rate of *value added* in industry j of country i over 1980–1990
- $X_{j,i}$ = control variables that vary in both dimensions
  - Here: *Industry share* = initial (1980) share of industry j's value added in total value added in manufacturing in country i
  - Idea: Mature industries grow more slowly
- F_i = average *financial development* in country i over 1980–1990 (accounting standards: 1990)

Empirical Model

$$Y_{j,i} = \alpha + \beta X_{j,i} + \gamma \cdot \mathit{ExternalDependence}_j \cdot F_i + \nu_j + \mu_i + \epsilon_{j,i}$$

- ν_j = **fixed industry effect** (constant across countries)
  - Captures all factors that affect a particular industry in different countries in the same way
- μ_i = **fixed country effect** (constant across industries)
  - Captures all factors that affect all industries of a particular country in the same way
- $\mathit{ExternalDependence}_j$ = Share of external financing in total capital expenditures in a particular US industry j (both averaged over the 1980s)

Empirical Model

$$Y_{j,i} = \alpha + \beta\, X_{j,i} + \gamma \cdot \mathit{ExternalDependence}_j \cdot F_i + \nu_j + \mu_i + \epsilon_{j,i}$$

- Coefficient of main interest $= \gamma$
- It measure the effect of external dependence on the *effect* of financial development on economic growth
- If externally dependent industries grow relatively faster in countries with highly developed financial systems, γ would be expected to be *positive*

Empirical Model

- Data structure allows to control for **unobserved country and industry characteristics** by including fixed effects
- Question: How large is the effect of financial development on economic growth?
  - This effect *cannot be identified* within this model!
  - **Level effect** of financial development on economic growth is absorbed by the country fixed effect

Industry statistics

Table 1—Pattern of External Financing and Investment Across Industries in the United States During the 1980's

		All companies		Mature companies		Young companies	
ISIC code	Industrial sectors	External dependence	Capital expenditures	External dependence	Capital expenditures	External dependence	Capital expenditures
314	Tobacco	−0.45	0.23	−0.38	0.24	—	—
361	Pottery	−0.15	0.20	0.16	0.41	−0.41	0.13
323	Leather	−0.14	0.21	−1.33	0.27	−1.53	0.16
3211	Spinning	−0.09	0.16	−0.04	0.19	—	—
324	Footwear	−0.08	0.25	−0.57	0.23	0.65	0.26
372	Nonferrous metal	0.01	0.22	0.07	0.21	0.46	0.24
322	Apparel	0.03	0.31	−0.02	0.27	0.27	0.37
353	Petroleum refineries	0.04	0.22	−0.02	0.22	0.85	0.28
369	Nonmetal products	0.06	0.21	0.15	0.22	−0.03	0.26
313	Beverages	0.08	0.26	−0.15	0.28	0.63	0.26
371	Iron and steel	0.09	0.18	0.09	0.16	0.26	0.19
311	Food products	0.14	0.26	−0.05	0.25	0.66	0.33

Continued ...

Country statistics

TABLE 2—FINANCIAL DEVELOPMENT ACROSS COUNTRIES

Country	Accounting standards	Total capitalization over GDP	Domestic credit to private sector over GDP	Per capita income (dollars)
Bangladesh	—	0.20	0.07	121
Kenya	—	0.28	0.20	417
Morocco	—	0.41	0.16	807
Sri Lanka	—	0.44	0.21	252
Pakistan	—	0.53	0.25	290
Costa Rica	—	0.53	0.26	2,155
Zimbabwe	—	1.01	0.30	441
Jordan	—	1.16	0.54	1,109
Egypt	24	0.74	0.21	563
Portugal	36	0.82	0.52	2,301
Peru	38	0.28	0.11	842
Venezuela	40	0.34	0.30	3,975
Colombia	50	0.21	0.14	1,150
Turkey	51	0.35	0.14	1,081
Chile	52	0.74	0.36	2,531
Brazil	54	0.33	0.23	1,650
Austria	54	1.00	0.77	9,554

Continued ...

VIII.3 Results

TABLE 4—INDUSTRY GROWTH AND VARIOUS MEASURES OF DEVELOPMENT

Variable	Financial development measured as: Total capitalization	Bank debt	Accounting standards	Accounting standards in 1983	Accounting standards and capitalization	Instrumental variables
Industry's share of total value added in manufacturing in 1980	−0.912 (0.246)	−0.899 (0.245)	−0.643 (0.204)	−0.587 (0.223)	−0.443 (0.135)	−0.648 (0.203)
Interaction (external dependence × total capitalization)	0.069 (0.023)	—	—	—	0.012 (0.014)	—
Interaction (external dependence × domestic credit to private sector)	—	0.118 (0.037)	—	—	—	—
Interaction (external dependence × accounting standards)	—	—	0.155 (0.034)	—	0.133 (0.034)	0.165 (0.044)
Interaction (external dependence × accounting standards 1983)	—	—	—	0.099 (0.036)	—	—
R^2	0.290	0.290	0.346	0.239	0.419	0.346
Number of observations	1217	1217	1067	855	1042	1067
Differential in real growth rate	1.3	1.1	0.9	0.4	1.3	1.0

Results

- Industry share enters *negatively* and is highly significant (standard errors in parentheses)
- Interaction term is *positive* and highly significant (independent of which measure is used for financial development)
- How can we judge the **economic significance** of the coefficients?
  $\Rightarrow$ Authors calculate the "**differential in real growth rate**" (last line in the table)

Differential in Real Growth Rate

- See column (1) using total capitalization as measure of financial development
- Consider the industries at the *75th and 25th percentiles* of **external dependence**: Machinery (high dependence), Beverages (low dependence)
- Consider the countries at the *75th and 25th percentiles* of **financial development**: Italy (high financial development), Philippines (low financial development)
- Question: How much faster does Machinery grow than Beverages in Italy compared to the Philippines?

Differential in Real Growth Rate

- Consider **interaction term**:
  $\hat{\gamma} \cdot \mathit{ExternalDependence} \cdot \mathit{TotalCapitalization}$
- *Italy:*
  - Machinery: $0.069 \cdot 0.45 \cdot 0.98 = 3.04\%$
  - Beverages: $0.069 \cdot 0.08 \cdot 0.98 = 0.54\%$
  - Growth differential: 2.50%, i. e., in Italy, Machinery grows by 2.50 percentage points faster than Beverages (holding constant the industry share and ignoring industry fixed effects at the moment)
- *Philippines:*
  - Machinery: $0.069 \cdot 0.45 \cdot 0.46 = 1.43\%$
  - Beverages: $0.069 \cdot 0.08 \cdot 0.46 = 0.25\%$
  - Growth differential: 1.17%, i. e., in the Philippines, Machinery grows by 1.17 percentage points faster than Beverages (holding constant the industry share and ignoring industry fixed effects)

Differential in Real Growth Rate

- **Differential in real growth rate** $= 1.33\%$, i. e., the growth difference between Machinery (externally dependent) and Beverages (not externally dependent) is by 1.33 percentage points larger in Italy (the financially developed country) than in the Philippines (the financially underdeveloped country)
- Given an average annual growth rate of 3.4 percent (see descriptive statistics), this is a *very large* number

Critique

- Novel and powerful procedure to identify the causal effect of financial development on economic growth
- Procedure controls for many unobserved country and industry characteristics by including **fixed effects**
- Identification procedure is closely linked to *economic theory*
- Analysis is based on disaggregated data, which increases the **degrees of freedom** (1,217 instead of 41 observations)
- Results appear to be very *robust* and *quantitatively important*

Critique

- But: **Level effect** of financial development on growth *cannot be identified*, only the differential effect can be identified
- Identification relies on some *strong assumptions* (e. g., no frictions in US capital markets)
- Nevertheless, this analysis yields much more convincing results than the other procedures presented earlier in this course
- Procedure is nowadays widely used in many different contexts

VIII.4 Variation of the mothod by Rajan/Zingales

- Dell'Ariccia, Giovanni, Enrica Detragiache, and Raghuram Rajan (2008): "The real effect of banking crises," *Journal of Financial Intermediation*, 17, 89–112
- Paper attempts to identify the effect of banking crises on real activity
- Hypothesis: Sectors that depend more on external financing should perform relatively worse in a banking crisis
- Methodological difference to Rajan/Zingales: Paper also exploits **time series variation**
- Data: Panel data from 28 sectors in 41 countries over the period 1980-2000

Variation of Rajan/Zingales Methodology

- Empirical model:

$$y_{ijt} = \sum_{ij} \alpha_{ij} d_{ij} + \sum_{it} \beta_{it} d_{it} + \sum_{jt} \gamma_{jt} d_{jt}$$

$$+\delta \cdot FINDEP_j \cdot BANK\_CRISIS_{it} + \psi SHARE_{ijt-1} + \epsilon_{ijt}$$

- Paper includes **all double interactions** of dummy variables, e. g., sector j in country i (captures country-specific characteristics of a sector, 41*28), sector j in period t (captures global sectoral trends, 28*20), country i in period t (captures macroeconomic factors in a given country, such as national business cycle, 41*20)
  $\Rightarrow$ This helps to control for many things!
- Number of observations: 16,227 (2,526 are absorbed by dummy variables)

VIII.5 Concluding remarks

- Increasing number of **firm-level studies** in recent years
  $\Rightarrow$ further disaggregation
- Main result is undisputed: Financial development seems to have a **causal effect on economic growth**, no matter how this effect is estimated/identified

Course overview

IX Further topics: bank risk

Outline

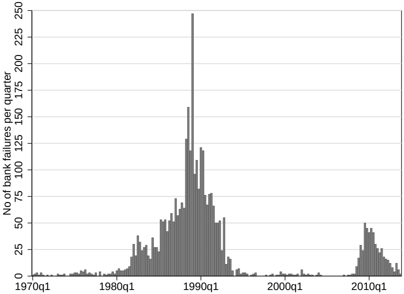
1. Discussion of different risk measures
2. Application: Causes of bank failures (Cole/ White 2011)
3. Application: Bank governance, regulation and risk taking (Laeven/Levine 2009)

* Thanks to Felix Noth (University of Magdeburg) for providing several lecture slides for this section.

IX.1 Discussion of different risk measures

- It is important to measure and to understand the risk of financial institutions
- Why?
  - It helps policymakers to ensure financial stability (banking supervision, setting the right incentives, ...)
  - Risk plays an important role for other key measures in banking: competition, provision of loans, ...
- The available risk proxies all have their vices and virtues.

Bank failures



Notes: Data on U.S. banks provided by the FDIC

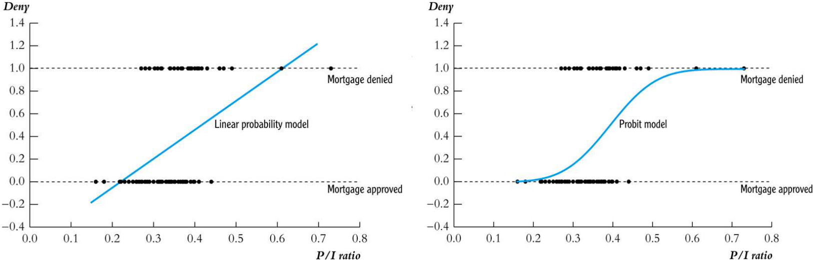
- Bank failure is the ultimate proxy for bank risk, but not always useful for practical purposes
- Nevertheless, used as dependent variable in regressions enables us to explore determinants of bank failurs and to calculate default probabilities

Bank failures and regressions

- If one wants to assess the predictive power of some variable like, e.g., the equity ratio for bank failures, it seems natural to use the information that some banks fail (failure=1) or survive (fail=0) as dependent variable
- You may think of standard OLS regression like $failure_{it} = \beta_0 + \sum_{i=1}^{k} \beta_i x_{kit} + \epsilon_{it}$ with failures either 0 or 1

Problems with linear OLS and 0/1 dependent variables

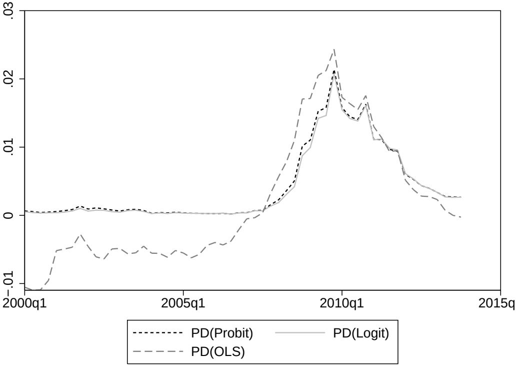
- An example for mortgage loan acceptations/denials (binary: $y \in (0,1)$) depending on the price/income ratio



Notes: Graphs are from Stock and Watson 2005

- OLS potentially predicts y-values greater than 1 or lower than 0
- Typical larger prediction errors in the middle of the x-distribution
- Probit/logit models employ certain assumptions about the distribution of the data (probit: cumulative normal distribution) and thereby achieve predictions within the 0/1 limit and fit the middle part much better

Probabilities of default over time



- The figure represents predicted default probabilities from the logit, probit and OLS regression
- Negative PDs for OLS prediction
- Similar pattern before/after financial crisis

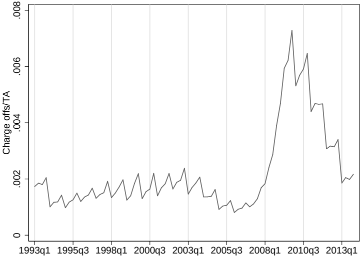
Balance sheet and P&L proxies 1/2

The literature uses several proxies for bank risk that can be derived from balance sheet and p&l data

- Non-performing loans (non accruals): loans on which the borrower is not making interest payments or repaying any principal in 90 days or less
- Charge-offs: After 120 days, loans are written off and this is accounted in the p&l
  - FDIC: *Total loans and leases charged-off (removed from balance sheet because of uncollectibility), less amounts recovered on loans and leases previously charged-off.*
- Loan loss allowance: a reserve put aside to absorb potential losses – booked as a contra-asset in the balance sheet
  - FDIC: *Each bank must maintain an allowance (reserve) for loan and lease losses that is adequate to absorb estimated credit losses associated with its loan and lease portfolio (which also includes off-balance-sheet credit instruments).*
- Loan loss provision: money a bank sets aside to cover potential losses on loans. The provision for loan losses is the current period expense for loan losses established in the current period
  - FDIC: *The amount needed to make the allowance for loan and lease losses adequate to absorb expected loan and lease losses (based upon management's evaluation of the bank's current loan and lease portfolio).*

Balance sheet and P&L proxies 2/3

(h) Non-performing loans



(i) Charge-offs

Balance sheet and P&L proxies 3/3

Critique

- Balance sheet and p&l data provide *ex post* measures for risk that retrospectively show how risk has evolved (or at a specific point in time)
- Also, accounting measures can be manipulated
- In periods of crisis accounting rules might change for specific institutions for some time

Z-scores 1/3

The Z-score is defined as:

$$Z = \frac{(RoA + Equity/Assets)}{\sigma_{RoA}}$$

- RoA is return on assets, Equity/Assets is bank's total equity over total assets and σ_{RoA} is the standard deviation of RoA (volatility)
- The measure is based on the assumption that a bank is insolvent when the equity is not sufficient to cover losses
- If $-RoA > Equity/Assets$ the banks is assumed to be insolvent
- By standardizing with bank's volatility, the Z-score indicates the number of standard deviation to insolvency (distance to default)
- If the Z-score is higher, a bank is more stable

Z-scores 2/3

- Due to the typical skewed distribution of Z-scores, usually the natural logarithm of Z-score is used

Z-scores 3/3

- The Z-score is very often used because data is easy available and the meaning is quite intuitive
- A shortcoming in empirical applications is usually that only few points in time are availability
- If only annual data (and only for some three to four years) is available, the calculation for the standard deviation becomes seriously problematic – would you trust a volatility that builds on three data points?
- If you have ten years you may calculate a constant volatility – but this is only second best
- First best: we would like to have a volatility that changes over time – a rolling window version
- Another problem is that the Z-score does not allow for an assessment of liquidity risk or problems that arise from the asset structure

Other proxies

- If one deals with listed banks, market measures can be used
  - Ratings
  - Volatility of daily returns
  - Credit default swaps, CDS-spreads
- In most countries with important banking markets, only a fraction of banks are listed
- Therefore, we must deal with data for non-listed banks

IX.2 Application: Cole and White (2011)

Déjà Vu All Over Again: The Causes of U.S. Commercial Bank Failures This Time Around

- Study investigates whether financial characteristics from the pre crisis period can explain U.S. bank failures in 2009
- Are those factors the same as in previous banking crises?
- Regression model:

$$Failure_{i,2009} = \boldsymbol{X}_{2009-t}\boldsymbol{\beta} + \epsilon$$

- Failures is a 0/1 variable indicating whether a bank failed in 2009 or not
- Alternative: banks are regarded as in distress if $Equiy + Reserves - 0.5 \times NPA < 0$

Explanatory variables

Table 1 Variable acronyms and explanations. All variables (except LNSIZE) are expressed as a decimal fraction of total assets

TE	Total Equity
LLR	Loan Loss Reserves
ROA	Return on Assets (Net Income)
NPA	Non-performing Assets=sum of (PD3089, PD90+, NonAccrual, OREO):
PD3089	Loans Past Due 30–89 Days but Still Accruing Interest
PD90+	Loans Past Due 90+ Days but Still Accruing Interest
NonAccrual	Nonaccrual Loans
OREO	Other Real Estate Owned
SEC	Securities Held for Investment plus Securities Available for Sale
BD	Brokered Deposits
LNSIZE	Log of Bank Total Assets
CASHDUE	Cash & Due
GOODWILL	Intangible Assets: Goodwill
RER14	Real Estate Residential Single-Family (1–4) Family Mortgages
REMUL	Real Estate Multifamily Mortgages
RECON	Real Estate Construction & Development Loans
RECOM	Real Estate Nonfarm Nonresidential Mortgages
CI	Commercial & Industrial Loans
CONS	Consumer Loans

Descriptives and t-tests for 2008

	All		Survivors		Failures			
Variable	Mean	S.E.	Mean	S.E.	Mean	S.E.	Difference	t-Difference
TE	0.123	0.001	0.124	0.001	0.076	0.002	0.048	22.67 ***
LLR	0.010	0.000	0.009	0.000	0.020	0.001	−0.011	−12.71 ***
ROA	0.004	0.000	0.005	0.000	−0.026	0.002	0.031	14.98 ***
NPA	0.030	0.000	0.026	0.000	0.126	0.005	−0.099	−20.41 ***
SEC	0.200	0.002	0.204	0.002	0.106	0.005	0.097	18.41 ***
BD	0.048	0.001	0.043	0.001	0.172	0.010	−0.129	−13.44 ***
LNSIZE	11.925	0.016	11.899	0.017	12.593	0.074	−0.694	−9.14 ***
CASHDUE	0.062	0.001	0.062	0.001	0.045	0.003	0.018	5.74 ***
GOODWILL	0.005	0.000	0.006	0.000	0.003	0.001	0.003	3.84 ***
RER14	0.142	0.001	0.143	0.001	0.104	0.005	0.039	6.93 ***
REMUL	0.015	0.000	0.014	0.000	0.029	0.003	−0.015	−5.43 ***
RECON	0.076	0.001	0.070	0.001	0.232	0.008	−0.162	−21.09 ***
RECOM	0.166	0.001	0.164	0.001	0.226	0.007	−0.062	−9.28 ***
CI	0.100	0.001	0.100	0.001	0.092	0.004	0.008	1.77 *
CONS	0.045	0.001	0.046	0.001	0.016	0.001	0.030	18.75 ***
Obs	7,146		6,883		263			

*, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively

Descriptives and t-tests for 2007

	All Banks		Survivors		Failures			
Variable	Mean	S.E.	Mean	S.E.	Mean	S.E.	Difference	t-Difference
TE	0.132	0.001	0.133	0.001	0.105	0.003	0.028	9.68 ***
LLR	0.0086	0.000	0.0085	0.000	0.0116	0.000	−0.003	−8.12 ***
ROA	0.0097	0.000	0.0099	0.000	0.0043	0.001	0.006	6.49 ***
NPA	0.019	0.000	0.018	0.000	0.047	0.003	−0.029	−11.18 ***
SEC	0.204	0.002	0.207	0.002	0.112	0.005	0.095	16.41 ***
BD	0.034	0.001	0.030	0.001	0.127	0.008	−0.097	−11.54 ***
LNSIZE	11.848	0.016	11.823	0.016	12.533	0.075	−0.710	−9.23 ***
CASHDUE	0.048	0.001	0.049	0.001	0.027	0.002	0.021	10.81 ***
GOODWILL	0.006	0.000	0.006	0.000	0.006	0.001	0.000	−0.13
RER14	0.136	0.001	0.138	0.001	0.093	0.005	0.045	8.47 ***
REMUL	0.013	0.000	0.012	0.000	0.027	0.003	−0.015	−5.57 ***
RECON	0.085	0.001	0.077	0.001	0.280	0.010	−0.203	−20.86 ***
RECOM	0.154	0.001	0.152	0.001	0.217	0.007	−0.065	−9.76 ***
CI	0.102	0.001	0.102	0.001	0.097	0.005	0.005	1.08
CONS	0.048	0.001	0.049	0.001	0.018	0.001	0.031	19.18 ***
Obs.	7,355		7,092		263			

*, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively

Logistic regression results for FDIC closed banks (Table 10)

Variable	2008 Marginal Effect	2008 t-stat	2007 Marginal Effect	2007 t-stat	2006 Marginal Effect	2006 t-stat	2005 Marginal Effect	2005 t-stat	2004 Marginal Effect	2004 t-stat
TE	−0.62	−9.54 ***	−0.16	−2.94 ***	−0.01	−0.55	−0.04	−1.43	0.03	1.70 *
LLR	0.03	0.22	−0.45	−1.30	−1.03	−2.26 **	−1.03	−2.17 **	−1.34	−2.80 ***
ROA	−0.12	−3.62 ***	−0.07	−0.75	−0.18	−2.44 **	−0.40	−3.18 ***	−0.09	−1.42
NPA	0.17	7.21 ***	0.27	5.83 ***	0.35	5.14 ***	0.20	1.91 *	0.26	3.00 ***
SEC	−0.04	−2.59 ***	0.01	0.38	0.00	−0.28	−0.01	−0.47	−0.02	−1.17
BD	0.01	1.37	0.02	1.28	0.01	0.71	0.00	0.56	0.02	1.57
LNSIZE	0.00	0.01	0.00	0.98	0.00	2.15 **	0.00	1.49	0.00	1.27
CASHDUE	−0.05	−2.25 **	−0.20	−2.31 **	−0.26	−2.85 ***	−0.21	−2.99 ***	−0.20	−2.87 ***
GOODWILL	0.60	6.63 ***	0.19	2.36 **	0.01	0.11	−0.03	−0.39	−0.09	−1.04
RER14	−0.06	−4.19 ***	−0.05	−2.14 **	−0.04	−1.95 *	−0.03	−1.41	−0.03	−1.82 *
REMUL	0.03	1.59	0.07	2.68 ***	0.06	2.16 **	0.08	3.14 ***	0.10	4.19 ***
RECON	0.04	2.96 ***	0.08	5.07 ***	0.09	6.33 ***	0.10	7.09 ***	0.10	7.73 ***
RECOM	−0.01	−0.74	0.01	0.90	0.02	1.12	0.02	1.13	0.01	0.77
CI	−0.04	−2.19 **	−0.01	−0.64	−0.02	−0.88	−0.02	−0.87	−0.01	−0.46
CONS	−0.03	−0.71	−0.19	−2.35 **	−0.26	−3.14 ***	−0.19	−2.67 ***	−0.06	−1.43
Pseudo-R-square	0.690		0.321		0.255		0.227		0.205	
Failures	117		117		114		111		106	
Survivors	6,883		7,092		7,138		7,276		7,396	
Obs.	7,000		7,209		7,252		7,387		7,502	

*, **, and *** indicate statistical significance at the 0.10, 0.05, and 0.01 levels, respectively

Conclusion

- These results are important for at least two reasons:
  1. they offer strong support for the CAMELS approach to judging the safety and soundness of commercial banks
  2. they indicate that most banks in the current crisis are failing in ways that are quite recognizable to anyone who has studied the hundreds of bank failures that occurred during the 1984-1992 period

IX.3 Application: Bank governance, regulation and risk taking – Laeven and Levine (JFE, 2009)

- Paper investigates whether a bank's corporate governance has an impact on its risk taking and the interplay with regulation
- Data on 10 largest banks (as year-end 2001) operating in countries for which governance indicators are available
- 279 banks from 48 countries
- Risk proxy is the Z-score
- Other control variable are for example deposit market share of a bank, total assets, GDP per country

Main variables

Corporate governance

- A bank has a large owner if the shareholder has direct and indirect voting rights that sum to 10% or more. If no shareholder holds 10% of the voting rights, the bank is classified as widely held
- CF is the cash flow rights of the largest shareholder of the bank
- Large owner on management board takes a value of one if a large shareholder has a seat on the management board of the company, and zero otherwise
- Managerial ownership equals the total cash flow rights of senior management

Regulation

- DI is a dummy variable that takes a value of one if the country has deposit insurance, and zero otherwise
- Capital Requirements equals the minimum capital requirement in the country
- Capital stringency is an index of regulatory oversight of bank capital. It measures the regulatory approach to assessing and verifying the degree of capital at risk in a bank
- Restrict is an index of regulatory restrictions on the activities of banks

Data

Variable	Number of banks	Mean	Standard deviation	Minimum	Maximum
Bank level					
z-Score	270	2.88	0.96	−1.56	5.14
$\sigma(ROA)$	270	0.01	0.03	0.00	0.46
Equity volatility	203	0.45	0.36	0.03	4.50
Earnings volatility	246	0.83	1.38	0.03	12.17
CF	270	0.24	0.25	0.00	1.00
Large owner on mgt board	270	0.31	0.46	0.00	1.00
Managerial ownership	266	0.06	0.14	0.00	0.68
Revenue growth	251	0.02	0.24	−0.86	1.87
Market share	234	0.14	0.22	0.00	1.84
NYSE	270	0.13	0.33	0.00	1.00
Size	251	16.20	2.13	10.94	20.77
Loan loss provision ratio	243	0.23	0.33	−2.56	2.64
Liquidity ratio	240	0.04	0.05	0.00	0.50
Country level					
Per capita income	48	8.79	1.49	5.54	10.70
Rights	48	2.98	1.31	0.00	5.00
Capital requirements	48	8.69	1.23	8.00	12.00
Capital stringency	41	3.12	1.25	0.00	5.00
Restrict	41	9.02	2.40	5.00	14.00
DI	47	0.79	0.41	0.00	1.00
Enforce	47	7.13	2.15	3.55	9.99
M&A	44	23.90	18.65	0.00	65.63
GDP volatility	47	0.03	0.02	0.00	0.12

Notes: Table is from Laeven and Levine (2009)

Results I

Variable	*z-Score* (1)	Fixed effects (2)	*Per capita income* (3)	Instrumental variables (4)	*Equity volatility* (5)	*Earnings volatility* (6)	*z-Score* (02–04) (7)	Country level (8)	Bank level (9)	Board and management (10)
Revenue growth	0.075	0.261	0.232	0.434	−0.065	1.446	0.593	0.165	−0.174	−0.125
	(0.512)	(0.348)	(0.507)	(0.430)	(0.114)	(1.125)	(0.476)	(0.356)	(0.289)	(0.287)
CF	−1.406***	−0.504*	−1.180***	−3.484***	0.222**	1.801**	−0.706*	−0.989***	−0.850**	−0.913**
	(0.415)	(0.293)	(0.379)	(1.088)	(0.095)	(0.870)	(0.357)	(0.349)	(0.408)	(0.399)
Per capita income			0.161***	0.059	−0.058**	−0.250***	0.087	0.201***	0.404*	0.413*
			(0.051)	(0.075)	(0.023)	(0.069)	(0.064)	(0.055)	(0.222)	(0.206)
Rights								0.067	0.115*	0.091
								(0.060)	(0.064)	(0.062)
Capital requirements								0.171**	0.202**	0.185*
								(0.068)	(0.090)	(0.101)
Capital stringency								0.053	0.043	0.050
								(0.069)	(0.077)	(0.078)
Restrict								−0.118***	−0.085**	−0.094**
								(0.034)	(0.036)	(0.040)
DI								−0.630***	−0.543**	−0.568***
								(0.216)	(0.211)	(0.204)
Enforce									−0.042	−0.046
									(0.126)	(0.121)
Concentration									−0.409	−0.370
									(0.538)	(0.557)
M&A									0.000	0.000
									(0.006)	(0.006)
Size									−0.104*	−0.098*
									(0.054)	(0.050)
Loan loss provision									−0.083	−0.036
									(0.158)	(0.210)
Liquidity									−0.097	0.724
									(1.071)	(1.217)
Large owner on mgt board										0.003
										(0.237)
Managerial ownership										0.363
										(0.703)

Notes: Table is from Laeven and Levine (2009)

Results I – summary

The existence of a large owner with substantial cash flow rights is associated with greater risk

- These results are consistent with the view that owners tend to advocate for more bank risk taking than managers and debt holders
- Large owners with substantial cash flow rights have greater incentives and power to increase bank risk taking than small shareholder
- Results are robust against the inclusion of control variable
- Instrumental variable regression support results
  - Instrument for CF-right is the average CF rights of other banks in the country (rational: innovations in the risk of one bank does not influence the cash flow rights of other banks)

Results II

Variable	(1)	(2)	(3)	(4)	(5)	$\sigma(ROA)$ (6)
Revenue growth	0.183	0.261	0.311	0.156	0.363	0.011**
	(0.374)	(0.292)	(0.293)	(0.339)	(0.277)	(0.005)
CF	0.555	0.807	1.009	0.468	5.247**	−0.074
	(2.658)	(0.516)	(0.863)	(0.287)	(2.277)	(0.064)
Per capita income	0.204***	0.193***	0.185***	0.192***	0.176***	−0.003**
	(0.058)	(0.053)	(0.057)	(0.051)	(0.053)	(0.001)
Rights	0.071	0.047	0.066	0.052	0.044	0.001
	(0.063)	(0.059)	(0.061)	(0.061)	(0.061)	(0.002)
Capital requirements	0.221*	0.154**	0.152**	0.151**	0.183*	0.002
	(0.117)	(0.065)	(0.066)	(0.069)	(0.101)	(0.003)
Capital stringency	0.052	0.219**	0.045	0.057	0.154**	−0.005*
	(0.070)	(0.085)	(0.065)	(0.064)	(0.071)	(0.003)
Restrict	−0.118***	−0.123***	−0.060	−0.123***	−0.078*	−0.001
	(0.034)	(0.038)	(0.038)	(0.033)	(0.039)	(0.002)
DI	−0.630***	−0.704**	−0.613***	−0.297	−0.315	−0.000
	(0.218)	(0.261)	(0.209)	(0.195)	(0.194)	(0.006)
CF ∗ capital requirements	−0.185				−0.224	−0.020*
	(0.316)				(0.263)	(0.011)
CF ∗ capital stringency		−0.607***			−0.383**	0.022*
		(0.186)			(0.168)	(0.012)
CF ∗ restrict			−0.218**		−0.187**	0.017**
			(0.091)		(0.079)	(0.007)
CF ∗ DI				−1.688***	−1.764***	0.063**
				(0.453)	(0.383)	(0.030)
Number of countries	40	40	40	40	40	40
Number of observations	219	219	219	219	219	219
R^2	0.34	0.38	0.36	0.36	0.40	0.45

Notes: Table is from Laeven and Levine (2009)

Results II – summary

- The stabilizing effects of an intensification of capital stringency regulations diminish when the bank has a large owner with the incentives and power to increase bank risk
- Capital requirements do not have significant nonlinear effects that depend on ownership structure
- An increase in activity restrictions is not associated with a significant change in a bank's risk if the bank is widely held
- Explicit deposit insurance has a risk enhancing effect when banks have a large owner