# Lecture 2. The (quasi-) experimental approach
# « As good as random… »

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Sciences Po

M2. Sociology Master

# The credibility revolution

- Compare to the counterfactual!
  - Treated group vs control group.
  - Measuring the treatment effect:

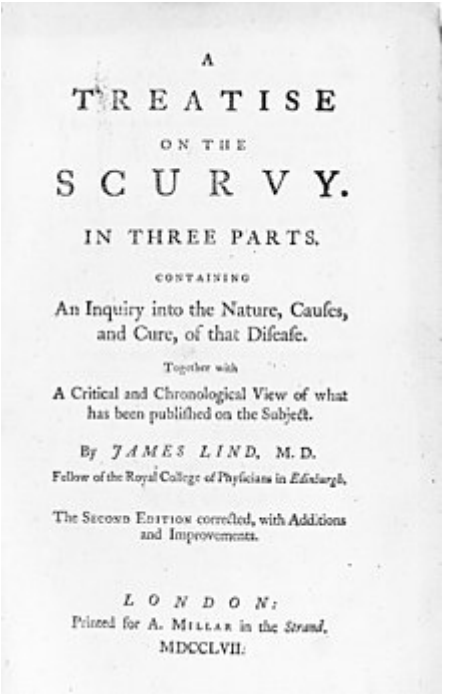
  => how much the treated group differs from the control group and how much this difference can only be related to the treatment.

- Four methods following the same idea
  - 1. Randomized controlled trials
  - 2. Natural experiments
  - 3. Differences in differences
  - 4. Regression discontinuity design

# 1. Randomized controlled trials

# James Lind and the scurvy (1747)

"On the 20th of May 1747, I selected twelve patients in the scurvy, on board the Salisbury at sea. **Their cases were as similar as I could have them.** They all in general had putrid gums, the spots and lassitude, with weakness of the knees. They lay together in one place, being a proper apartment for the sick in the fore-hold; and **had one diet common to all**, viz. water gruel sweetened with sugar in the morning; fresh mutton-broth often times for dinner; at other times light puddings, boiled biscuit with sugar, etc., and for supper, barley and raisins, rice and currants, sago and wine or the like. **Two were ordered each a quart of cyder a day. Two others took twenty-five drops of elixir vitriol three times a day . . . Two others took two spoonfuls of vinegar three times a day . . . Two of the worst patients were put on a course of sea-water . . . Two others had each two oranges and one lemon given them every day . . . The two remaining patients, took . . . an electary recommended by a hospital surgeon . . .** The consequence was, that the most sudden and visible **good effects were perceived from the use of oranges and lemons;** one of those who had taken them, being at the end of six days fit for duty . . . The other was the best recovered of any in his condition; and . . . was appointed to attend the rest of the sick. Next to the oranges, I thought the cyder had the best effects . . ."



A

TREATISE

ON THE

SCURVY.

IN THREE PARTS.

CONTAINING

An Inquiry into the Nature, Cauſes, and Cure, of that Diſeaſe.

Together with

A Critical and Chronological View of what has been publiſhed on the Subject.

By *JAMES LIND*, M. D.

Fellow of the Royal College of Phyſicians in *Edinburgh*.

The Second Edition corrected, with Additions and Improvements.

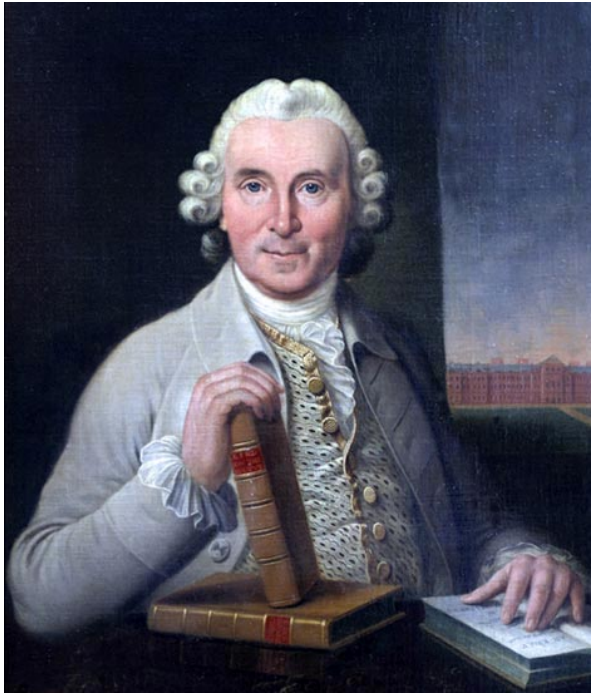
*LONDON:*

Printed for A. Millar in the *Strand*.

MDCCLVII.

# James Lind and the scurvy (1747)

- 12 scorbutic sailors. Divided in 6 group
  - Same diet + supplementation
    - cider
    - elixir vitriol
    - vinegar
    - sea water
    - electary recommended by surgeon
    - oranges and lemons ➔ rapid cure from scurvy
- First controlled trial (not totally randomized) … but not really taken seriously
  - Generalization of lemon in English Marine
  - … and the role of vitamin C identified much later (1930)



# Randomized controlled trials (RCT) experiments

- End of 19th century
  - Psychology: Peirce & Jastrow (1885) "On Small Differences in Sensation"
  - Education
- Randomized controlled trials experiment in medicine with a detailed protocol:
  - Marshall et al. (1948) "Streptomycin treatment of pulmonary tuberculosis: a medical research council investigation." *Br Med J*
  - Now the standard of scientific demonstration in medicine/pharmacology

# Ex. A pharmaceutical randomized experiment

- Canner, et al. 1986. "Fifteen year mortality in Coronary Drug Project patients: long-term benefit with niacin." *Journal of the American College of Cardiology*.
- Influence of different drugs for persons surviving Myocardial infarction
- Random assignment to different groups

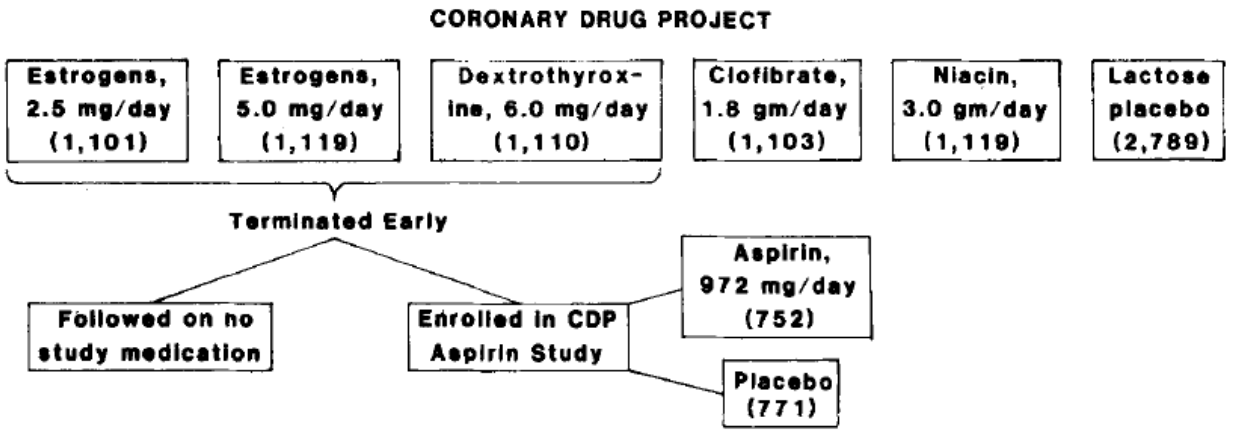



**Figure 1.** Treatment scheme in the Coronary Drug Project (CDP) and Coronary Drug Project Aspirin Study. Numbers of patients are given in parentheses.

# Results

**Table 2.** All-Cause Mortality (%) for a Mean Follow-up Period of 15 Years in the Estrogen, Clofibrate, Dextrothyroxine and Placebo Groups

| | Drug | | Placebo | | |
|---|---|---|---|---|---|
| Lipid-Lowering Drug | n | % | n | % | z Value |
| Low dose estrogen | 1,101 | 59.7 | 2,789 | 58.2 | 0.84 |
| High dose estrogen | 1,119 | 58.3 | 2,789 | 58.2 | 0.04 |
| Clofibrate | 1,103 | 57.8 | 2,789 | 58.2 | −0.25 |
| Dextrothyroxine | 1,110 | 57.0 | 2,789 | 58.2 | −0.67 |

**Table 3.** Mortality (%) by Cause for a Mean Follow-up Period of 15 Years in the Niacin and Placebo Groups

| Cause of Death | Niacin | Placebo | z Value |
|---|---|---|---|
| All causes | 52.0 | 58.2 | −3.52 |
| Coronary heart disease | 36.5 | 41.3 | −2.80 |
| Cerebrovascular causes | 1.4 | 1.6 | −0.34 |
| Other cardiovascular | 4.5 | 4.8 | −0.45 |
| Cancer | 4.0 | 4.4 | −0.59 |
| Other causes | 2.9 | 3.0 | −0.16 |
| Unknown or not coded | 2.7 | 3.0 | −0.56 |
| No. of patients | 1,119 | 2,789 | |

# Experiments in social sciences

- Durkheim: sociology as a non-experimental science ? … partially true but
  - "When the phenomena can be artificially produced at will by the observer, the method is that of experimentation proper. When, on the other hand, the production of facts is something beyond our power to command, and we can only bring them together as they have been spontaneously produced, the method used is one of indirect experimentation, or the comparative method." (Durkheim, 1894)
- Social psychology
- Behavioral experimental economics
- Public policy economics (Duflo and al. / Gurgand / etc.)
- Marketing

# Examples of randomized controlled trials experiments in sociology

- Salganik, Dodds & Watts. 2006 "Experimental study of inequality and unpredictability in an artificial cultural market" *Science*
- Downloading website of 48 unknown songs
  - 14 341 subjects: mostly teenagers
  - Platform 1: no indication of surfers' downloads (independence)
  - Platform 2: indication of surfers downloads.
  - Dispatched in 8 different worlds where downloading metrics evolve differently
    - experiment 1: displayed in a random order (16*3)
    - experiment 2: displayed in one single column following the decreasing order of downloads
  - Random assignment of surfers to the two platforms
- Goal of the experiment :
  - Role of social influence in the production of inequality through Winner take all mechanisms

# Results

More inequality when exposed to social influence

More unpredictability of success when exposed to social influence.

Stronger when the hierarchy is visible

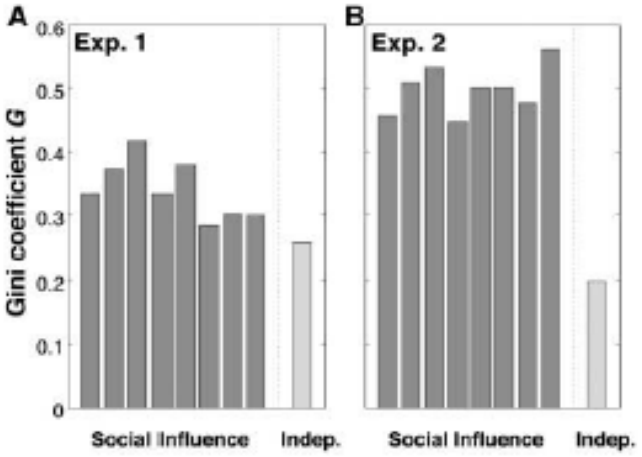



**Fig. 1.** Inequality of success for social influence (dark bars) and independent (light bars) worlds for (**A**) experiment 1 and (**B**) experiment 2. The success of a song is defined by $m_i$, its market share of downloads ($m_i = d_i / \sum_{k=1}^{S} d_k$, where $d_i$ is song $i$'s download count and $S$ is the number of songs). Success inequality is defined by the Gini coefficient $G = \sum_{i=1}^{S} \sum_{j=1}^{S} |m_i - m_j| / 2S \sum_{k=1}^{S} m_k$, which represents the average difference in market share for two songs normalized to fall between 0 (complete equality) and 1 (maximum inequality). Differences between independent and social influence conditions are significant ($P < 0.01$) (*18*).

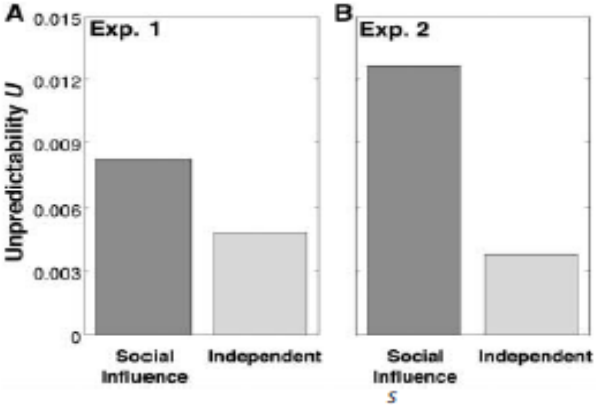



**Fig. 2.** Unpredictability of success for (**A**) experiment 1 and (**B**) experiment 2. In both experiments, success in the social influence condition was more unpredictable than in the independent condition. Moreover, the stronger social signal in experiment 2 leads to increased unpredictability. The measure of unpredictability $u_i$ for a single song $i$ is defined as the average difference in market share for that song between all pairs of realizations; i.e., $u_i = \sum_{j=1}^{W} \sum_{k=j+1}^{W} |m_{i,j} - m_{i,k}| / \binom{W}{2}$, where $m_{i,j}$ is song $i$'s market share in world $j$ and $W$ is the number of worlds. The overall unpredictability measure $U = \sum_{i=1}^{S} u_i / S$ is then the average of this measure over all $S$ songs. For the independent condition, we randomly split the single world into two subpopulations to obtain differences in market shares, and we then averaged the results over 1000 of these splits. All differences are significant ($P < 0.01$) (*18*).

*Experi*

# A testing based on social class (Rivera, Tilcsik, 2016)

**Figure 2.** Combinations of Résumé Items that Together Signal Social Class Background

| | Higher-class combination[a] | Lower-class combination[b] |
|---|---|---|
| Last name | Cabot | Clark[c] |
| Undergraduate athletic award | University athletic award[c] | University award for outstanding athletes on financial aid |
| Undergraduate extracurricular activity (2008-2011) | Peer mentor for first-year students[c] | Peer mentor for first-generation college students |
| Undergraduate extracurricular activity (2007-2011) | Sailing team | Track & field (relay team)[c] |
| Personal interests | Sailing, polo, classical music | Track & field,[c] pick-up-soccer, country music |

# Results

**Table 2.** Proportions of Applicants Receiving Interview Invitations by Gender and Social Class

| | Interview Invitations / Applications | % Invited to Interview |
|---|---|---|
| Higher-class man | 13/80 | 16.25 |
| Higher-class woman | 3/79 | 3.80 |
| Lower-class man | 1/78 | 1.28 |
| Lower-class woman | 5/79 | 6.33 |

# A partially randomized experiment

- Pager, Bonikowski, & Western. 2009. "Discrimination in a low-wage labor market: A field experiment." *ASR*
- Testing
  - 10 selected testers (4 whites, 4 blacks and 2 Latinos) among 300 candidates, grouped in groups of 3
  - Matched by age, education, physical appearance, and interactional skills
  - Identical fictitious resumes
  - Groups of 3 applying to same employment entry-level positions (319 announces). In half applications, the white says been released from prison after serving 18 months for a drug felony.

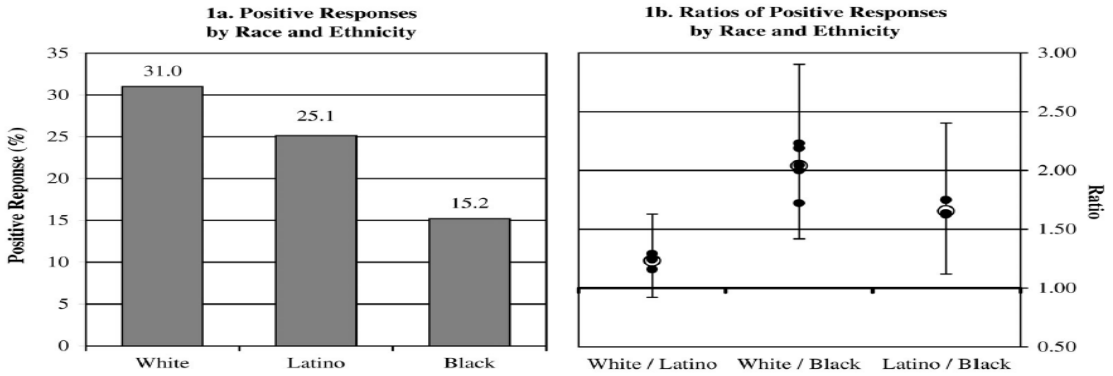



**Figure 1.** Positive Response Rates and Paired Comparisons by Race and Ethnicity

*Notes:* Positive responses refer to callbacks or job offers. Hollow circles in Figure 1b indicate point estimates of the ratio. Solid circles indicate ratios obtained by sequentially dropping testers from the analysis. We estimated 95 percent confidence intervals from a hierarchical logistic regression with employer and tester random effects. Number of employers = 171.

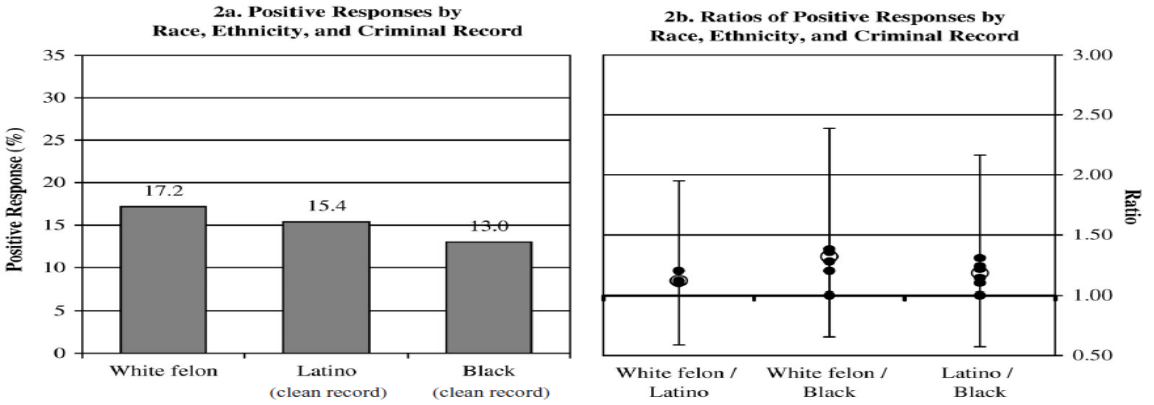



**Figure 2.** Positive Response Rates and Paired Comparisons by Race, Ethnicity, and Criminal Background

*Notes:* Positive responses refer to callbacks or job offers. Hollow circles in Figure 2b indicate point estimates of the ratio. Solid circles indicate ratios obtained by sequentially dropping testers from the analysis. We estimated 95 percent confidence intervals from a hierarchical logistic regression with employer and tester random effects. Number of employers = 169.

# A qualitative dimension

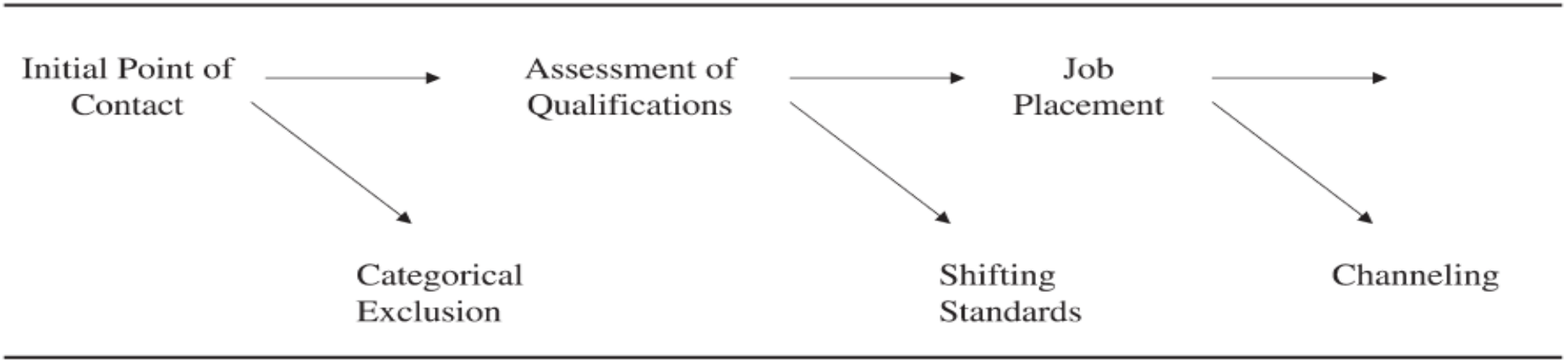



**Figure 3.** Discrimination at Three Decision Points

# Experiments in questionnaires: Vignette experiments

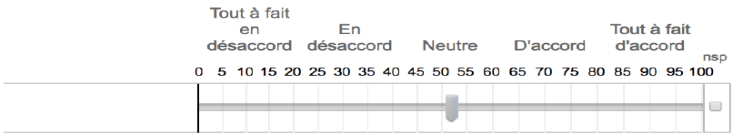
- Pablo Zamith. *The Making of Economists: A Transatlantic Investigation.*
- Introduction of randomized questions
  - Randomization of the framing (pre text)
  - Test of different effects :
    - Pear effects, teacher of effects,
    - "Proof of concept" rather than the estimation of the true effect
    - If effect non-significant => effect exists but the protocol if not powerful enough
    - If effect significant => qualitative proof of its existence but no estimation of its true magnitude

**Peer effects**

MOYENNES DE VOTRE CLASSE
(données provisoires)

DEGRÉ D'ACCORD
**23.1%**
**(tout à fait en désaccord)**

Il faudrait réguler les marchés financiers si l'on veut éviter une prochaine crise



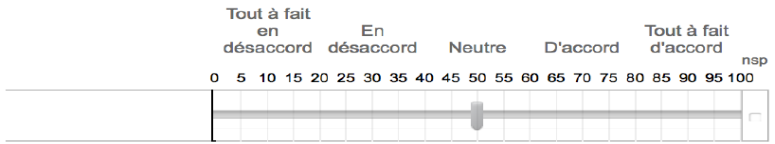

| ASSIGNMENT | N | AGREEMENT |
|---|---|---|
| Control | 157 | 57.5% (14.4) |
| Class average strongly agree | 140 | 60% (20.3) |
| Class average strongly disagree | 165 | 50.1%* (22.6) |
| Individual responses will be revealed to the class | 141 | 47.9%* (13.9) |

# Studies of master effect and formalization effect

« L'idée selon laquelle le salaire minimum détruit des emplois est toute simple: les travailleurs coûtent plus cher, donc on embauche moins »

**(Gilles Saint-Paul, économiste, Professeur à la Toulouse School of Economics)**

Aujourd'hui, pour réussir à créer de nouveaux emplois, il faut baisser le salaire minimum

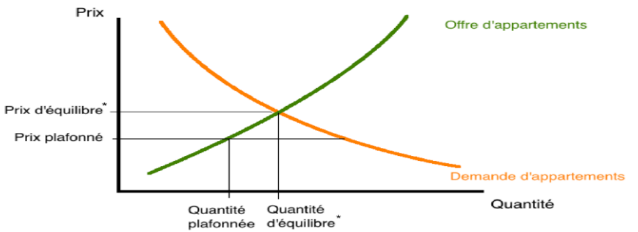



**Teacher effect**

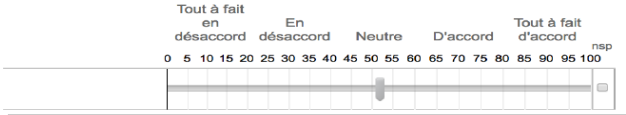
| ASSIGNMENT | N | AGREEMENT |
|---|---|---|
| Control | 121 | 49.5% (29.4) |
| Journalist | 117 | 47.2% (21.1) |
| Nobel Prize | 111 | 53.1%* (25.7) |
| Imaginary professor | 120 | 50.3% (34.1) |
| Professor from UT1 | 119 | 55.9%* (22.1) |

Dans votre cours de micro-économie vous avez vu le schéma suivant (inspiré du manuel de H.R. Varian) :

MARCHÉ DU LOGEMENT




Grâce à ce modèle, on peut conclure qu'un plafonnement des loyers aurait pour effet de limiter l'offre, provoquer une pénurie d'appartements et constitue donc une politique défavorable aux personnes qui cherchent un logement




**Formalization effect**

| ASSIGNMENT | N | AGREEMENT |
|---|---|---|
| Without model | 300 | 58.7% (25.6) |
| With model | 322 | 62.2%* (11.1) |

# Vignette Experiment (Mize, Manago, 2018)

"*Michael* is currently single but has had multiple happy relationships *with women* in the past. *Michael* has only dated *women* and one of his relationships with a *woman named Emily* lasted for over two years. The other night, *Michael* met *Matt* and felt attracted to him. At the end of the night, *Michael and Matt* went home together and had a casual sexual encounter."

- 4 treatments:
  - (1) man with a heterosexual dating history but recent same-sex encounter,
  - (2) man with a gay dating history but recent different-sex encounter,
  - (3) woman with a heterosexual dating history but recent same-sex encounter
  - (4) woman with a gay dating history but recent different-sex encounter
- Question on attribution of sexual orientation
  - how likely they thought the target character was heterosexual, bisexual, or gay/lesbian (from 0 to 100)

# Results

- Survey
- Nationally representative sample
- 2000 participants

**Table 1**. Means for Sexual Orientation Percent Guesses Based on Vignette Characteristics (Top Panel) and Fractional Response Logit Regression Results (Bottom Panel); Study 1 ($N = 1{,}965$)

| | Percent Certainty That Target Character Is Listed Sexual Orientation | | |
|---|---|---|---|
| | Heterosexual | Bisexual | Gay/Lesbian |
| Woman (Heterosexual Past) | 51 | 41 | 15 |
| Man (Heterosexual Past) | 31 | 51 | 24 |
| Woman (Lesbian Past) | 11 | 54 | 40 |
| Man (Gay Past) | 9 | 51 | 47 |
| | **Fractional Response Logit Results** | | |
| | Heterosexual | Bisexual | Gay/Lesbian |
| Woman vs. Man (Heterosexual Past) | .205*** (.022) | −.101*** (.023) | −.078*** (.017) |
| Woman vs. Man (Lesbian/Gay Past) | .029* (.014) | .027 (.023) | −.068** (.023) |

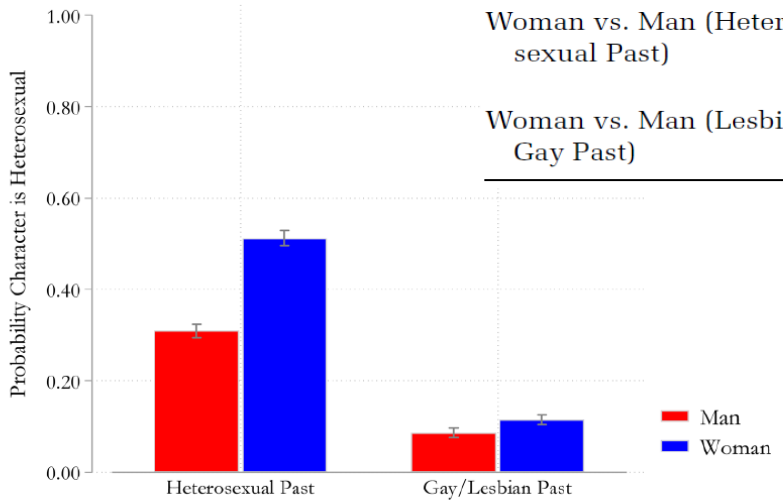



**Figure 2.** Probability Rating That Vignette Character Is Heterosexual, Study 1

# Why randomized experiment ?

- Randomized assignment ensures that all individual characteristics (both observed and moreover unobserved) will be equiprobably assigned to the treated group or the control group
  - Several techniques: simple randomized assignment or stratified randomized assignment
- Estimation is not biased by a confounding variable (unobserved heterogeneity)
- Great simplification of statistical work
  - Magnitude of the effect: difference (or ratio) in means
  - Significance of the effect: test of difference of means or of proportions
- Randomized experiment versus random sample
  - Random sample: establish representative statistics of a population => external validity
  - Randomized experiment : randomized assignment within a sample => internal validity.

# Experiment and its blinds

- Simple blind
  - the subjects do not know in which group they are (treated or placebo)
- Double blind
  - the subjects and the persons who are giving the treatment do not know in which group the subjects are
- Triple blind
  - the patients, the persons who are giving the treatment and the statisticians don't know in which group the patients are
- FDA: Preregistration of statistical models

# Randomized experiment in social sciences is rarely a blind experiment with placebo

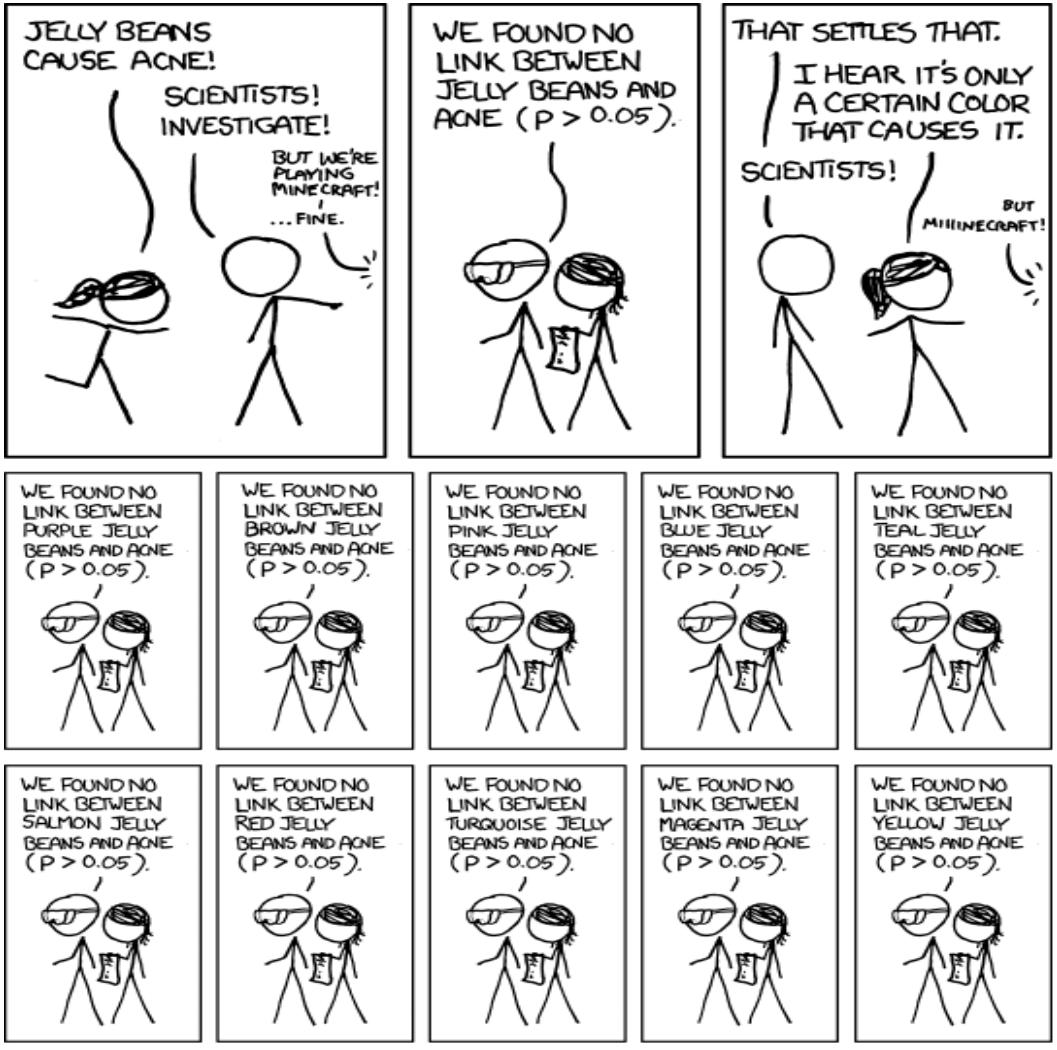
- Placebo, which should have the shape, the taste, etc. of the treatment don't always exist.
- Often two groups: one that is the object of a treatment and the other one that gets nothing.
- To avoid bias rather analyze the *intention to treat* rather than the *treatment on treated*
  - Why?
    - Attrition phenomenon between randomized assignment and complete treatment.
    - Attrition is not random and may be due to social characteristics*treatment

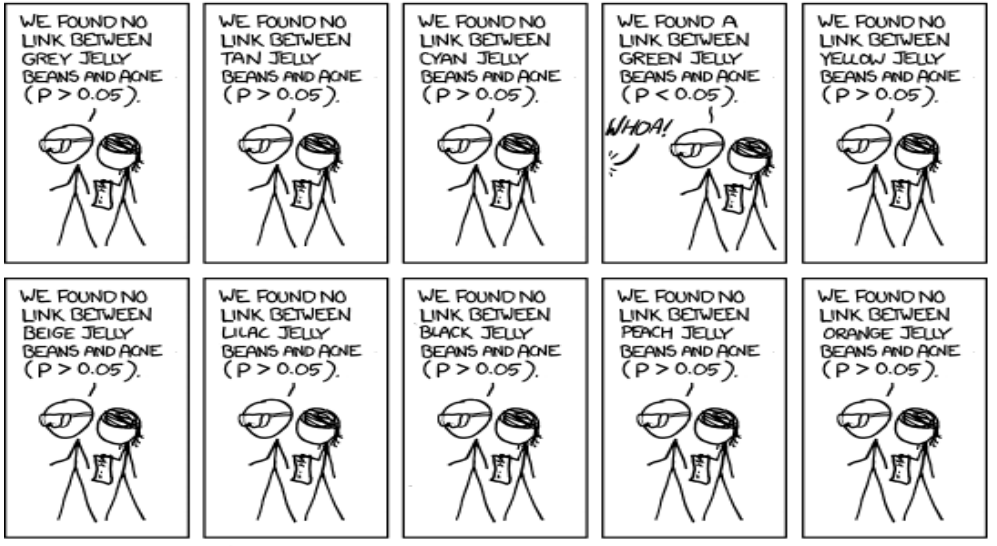
# Identifying and interpreting: Are we really testing what we say we test?

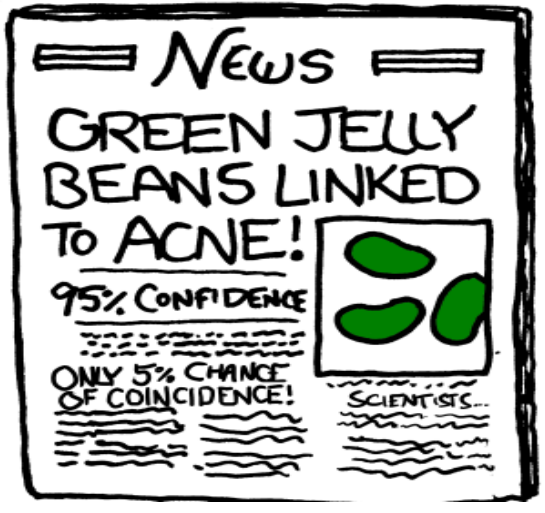
- Translating a theoretical mechanism to experimental manipulation
  - Tilcsik. 2011. "Pride and prejudice" *AJS*.
    - Theoretical hypothesis: Discrimination against homosexual males
    - Experimental manipulation: Contrast in reply to applications between
      - treasurer of Gay and lesbian alliance
      - treasurer of Progressive and socialist alliance
  - Bertrand & Mullainathan. 2004. "Are Emily and Greg More Employable than Lakisha and Jamal? … "*AER*.
    - Imaginary linked to Emily and Greg (white) vs Lakisha and Jamal (black)
    - Information on race OR on race and class?

# Technical limits

- Number of observations (limit linked to any approach based on statistical testing)
  - Significance depends on size…
- Heterogeneity
  - Experimental protocol estimates the mean effect
  - Effect can be stronger for some subgroups (males versus females, young versus elder)
  - Possibility of heterogeneity analysis
    - Subgroup analysis
  - Risk of data mining
    - P-hacking => we almost always find subgroups for which differences are significant (cf. Caricature next slide)
    - FDA: compulsory preregistration of the tested subgroups
- Spillover effects
  - Subjects are not the only persons impacted. Externalities (kin, neighbors, networks)→ effects in return.
- Debatable generalization
  - Internal validity of treated vs control difference within a given sample.
    - Sample often made of volunteers and non representative (biased)
    - Voluntary inquirers and scientists
    - Estimated parameters → not that for the whole population
  - Even with a representative sample… Experiment valid in partial equilibrium. Effect not necessarily the same in general equilibrium


JELLY BEANS CAUSE ACNE!
SCIENTISTS! INVESTIGATE!
BUT WE'RE PLAYING MINECRAFT!
... FINE.
WE FOUND NO LINK BETWEEN JELLY BEANS AND ACNE (P > 0.05).
THAT SETTLES THAT.
I HEAR IT'S ONLY A CERTAIN COLOR THAT CAUSES IT.
SCIENTISTS!
BUT MINECRAFT!
WE FOUND NO LINK BETWEEN PURPLE JELLY BEANS AND ACNE (P > 0.05).
WE FOUND NO LINK BETWEEN BROWN JELLY BEANS AND ACNE (P > 0.05).
WE FOUND NO LINK BETWEEN PINK JELLY BEANS AND ACNE (P > 0.05).
WE FOUND NO LINK BETWEEN BLUE JELLY BEANS AND ACNE (P > 0.05).
WE FOUND NO LINK BETWEEN TEAL JELLY BEANS AND ACNE (P > 0.05).
WE FOUND NO LINK BETWEEN SALMON JELLY BEANS AND ACNE (P > 0.05).
WE FOUND NO LINK BETWEEN RED JELLY BEANS AND ACNE (P > 0.05).
WE FOUND NO LINK BETWEEN TURQUOISE JELLY BEANS AND ACNE (P > 0.05).
WE FOUND NO LINK BETWEEN MAGENTA JELLY BEANS AND ACNE (P > 0.05).
WE FOUND NO LINK BETWEEN YELLOW JELLY BEANS AND ACNE (P > 0.05).


WE FOUND NO LINK BETWEEN GREY JELLY BEANS AND ACNE (P > 0.05).
WE FOUND NO LINK BETWEEN TAN JELLY BEANS AND ACNE (P > 0.05).
WE FOUND NO LINK BETWEEN CYAN JELLY BEANS AND ACNE (P > 0.05).
WE FOUND A LINK BETWEEN GREEN JELLY BEANS AND ACNE (P < 0.05).
WHOA!
WE FOUND NO LINK BETWEEN YELLOW JELLY BEANS AND ACNE (P > 0.05).
WE FOUND NO LINK BETWEEN BEIGE JELLY BEANS AND ACNE (P > 0.05).
WE FOUND NO LINK BETWEEN LILAC JELLY BEANS AND ACNE (P > 0.05).
WE FOUND NO LINK BETWEEN BLACK JELLY BEANS AND ACNE (P > 0.05).
WE FOUND NO LINK BETWEEN PEACH JELLY BEANS AND ACNE (P > 0.05).
WE FOUND NO LINK BETWEEN ORANGE JELLY BEANS AND ACNE (P > 0.05).

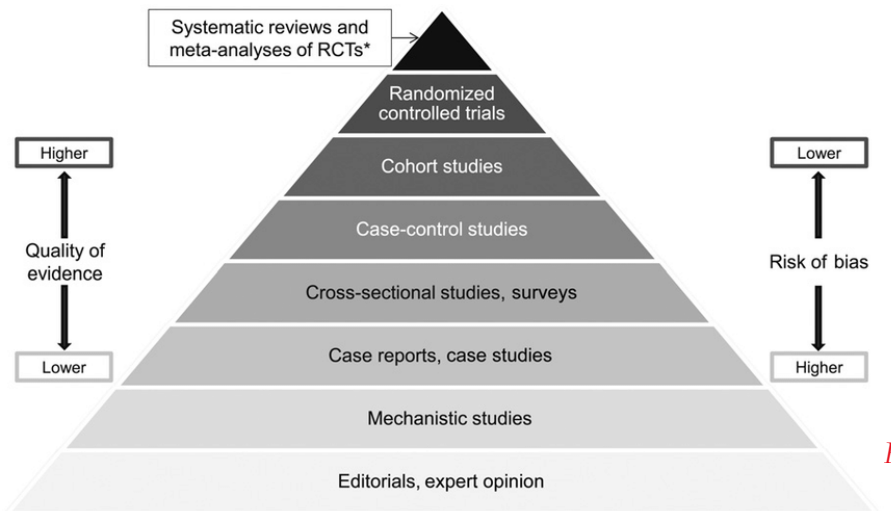

NEWS
GREEN JELLY BEANS LINKED TO ACNE!
95% CONFIDENCE
ONLY 5% CHANCE OF COINCIDENCE!
SCIENTISTS...

# Conceptual limits

- Experiment ≠ true life
  - Experiment conditions can modify Experiment results
    - Hawthorne effect
    - Subjects try to please the inquirers
    - Seen as a game
  - The experimental framing overlooks embeddedness of social life
    - Layers of interpretation
    - Sensibility to the framing and the wording of the experiment
- Many social objects, and notably the most important can not be the object of experiments
- Causal mechanism underlying the treatment's efficacy is not necessarily clear
  - Cf. Lind. Why lemon?
- Ethical problems
  - Manipulation of subjects became an issue
    - Cf. experiment à la Milgram
  - Equity problems between subjects
    - When outcome very different between treated and control (cf. AIDS experimental treatment)

# The golden standard of science and its critique

- Deaton, & Cartwright. 2018. "Understanding and misunderstanding randomized controlled trials." *Social Science & Medicine*
- Deaton. 2010. "Instruments, randomization, and learning about development." *JEL*




- Unscrewing RCT's hegemony
- One method among others
- Interesting
- But with limits : heterogeneity and generalization
- Not above
- On top of the
- pyramid of proof?

# 2. Natural experiments

# Natural experiments

- "Natural" situation which resembles an experimental situation
  - randomized or quasi-randomized assignment of a population between treated group and group of control
  - Not constructed for experimental means.
- Random draws as a social assignment device
  - Juries (tribunal)
  - Roommates in dormitories
- Randomized games and lotteries
- Randomized or quasi-randomized phenomenon
  - Sex ratio at birth / Month of birth
- Academic recruitment (Godechot, 2016)

# Academic natural experiments

- EHESS:
  - Godechot. 2016. "The chance of influence…" *Social Networks*
  - 1961-2005 : part of EHESS's electoral commission is randomly drawn. 2209 applications, 146 exams, social sciences only.
- Spain:
  - Zinovyeva, Bagues. 2015. "The Role of Connections in Academic Promotions", *American Economic Journal*.
  - 2002-2006: Assistant and full professors are nationally selected by a 7-member recruitment committee randomly drawn in the discipline. 30 000 applications, all disciplines, 967 exams

# Experimental framework for testing the true causal effect of a given contact

- Experimental framework
  - Treatment: the contact is randomly drawn in the electoral commission
  - Control: the contact, although eligible, is not drawn in the electoral commission
  - Treatment's causal effect: treatment effect – control effect
- If the draw is really random, it is orthogonal (independent of) individual characteristics.
  - No unobserved heterogeneity. No reverse causality. No need to multiply control variables.
- However, the mechanisms through which contacts have an effect can be debated:
  - conscious favoritism, intellectual bias, shared common interests, reduced costs of evaluation, etc.

# First descriptive results

**Table 7. Vote share and proposition of candidates by the electoral commission depending on the supervisor's membership of the electoral commission**

| **Candidatures whose PhD advisor is** | Mean (s.d.) | N | % | N | Mean (s.d.) | N | % | N | Mean (s.d.) | N | % | N |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| **Randomly drawn member of EC** | **28.1 % (0.334)** | **62** | **34 %** | **62** | **31.3 % (0.345)** | **54** | **37 %** | **54** | **30 % (0.329)** | **48** | **38 %** | **54** |
| **Member of EHESS outside electoral commission** | **22.2 % (0.263)** | **360** | **20 %** | **360** | **21.7 % (0.261)** | **371** | **20 %** | **373** | **22 % (0.266)** | **377** | **20 %** | **379** |
| Member of EC as a member of scientific council | 30.4 % (0.307) | 13 | 31 % | 13 | 30.4 % (0.307) | 13 | 31 % | 13 | 30.4 % (0.307) | 13 | 31 % | 13 |
| Member of EC as member of the bureau | 31.2 % (0.323) | 18 | 22 % | 18 | 31.2 % (0.323) | 18 | 22 % | 18 | 31.2 % (0.323) | 18 | 22 % | 18 |
| Outside EHESS | 17.5 % (0.268) | 1741 | 16 % | 1756 | 17.5 % (0.268) | 1741 | 16 % | 1756 | 17.5 % (0.268) | 1741 | 16 % | 1756 |
| All candidatures | 18.7 % (0.27) | 2194 | 17 % | 2209 | 18.7 % (0.27) | 2194 | 17 % | 2209 | 18.7 % (0.27) | 2194 | 17 % | 2209 |
| Definition of the membership the electoral commission | Drawn as titular or substitute if possible, presence otherwise | | | | Drawn as titular (if possible, presence otherwise) | | | | Presence (if possible, composition otherwise) | | | |

# A simple model

- $P(Success) = a.Drawn + b\ Exofficio + c.Undrawn + exam\_j + u$
- Causal effect of the contact is estimated by Treatment-control : $a-c$
- Reformulation:
- $P(Success) = a'.Drawn + b'.Exofficio + c.Ehess + exam\_j + u$
  - With $Ehess = Drawn + Exofficio + Undrawn$
  - and $a' = (a-c)$
- LPM: linear probability models. OLS with robust cluster standard errors (logistic regressions available)

# 13 to 20 percentage points of being put forward

| Applications whose PhD advisor is : | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| Randomly drawn member of the EC | 0.137** | 0.129* | 0.187*** | 0.220** | 0.215** | 0.139 |
| | (0.062) | (0.066) | (0.068) | (0.085) | (0.091) | (0.104) |
| Ex-officio member of the EC | 0.056 | 0.019 | 0.050 | -0.002 | 0.029 | 0.137 |
| | (0.076) | (0.072) | (0.081) | (0.107) | (0.089) | (0.189) |
| Member of EHESS | 0.040 | 0.051* | 0.021 | 0.014 | 0.015 | 0.035 |
| | (0.029) | (0.027) | (0.030) | (0.035) | (0.036) | (0.055) |
| Competitive exam fixed effects | No | Yes | Yes | Yes | Yes | Yes |
| Field | All competitive exams | All competitive exams | All experimental exams | All experimental exams with composition | Assist. Pr. experimental exams | Professor experimental exams |
| Number of applications [n1 ; n2] | 2209 [357; 62] | 2209 [357; 62] | 991 [184; 55] | 749 [143 ; 42] | 563 [131; 33] | 428 [53; 22] |

- Effect is more important when restricting to exams with competition between applicants whose contact is randomly drawn and applicants whose contact is eligible but not drawn
- Effect more important for assistant professor level exams

# +6 to +9% share of votes

| Applications whose PhD advisor is : | 1 | 2 | 3 | 4 | 5 | 6 |
|---|---|---|---|---|---|---|
| Randomly drawn member of the EC | 0.059 | 0.053 | 0.090** | 0.098* | 0.113* | 0.064 |
| | (0.039) | (0.039) | (0.040) | (0.050) | (0.057) | (0.051) |
| Ex-officio member of the EC | 0.088 | 0.05 | 0.077 | 0.094 | 0.017 | 0.293** |
| | (0.054) | (0.049) | (0.06) | (0.085) | (0.06) | (0.108) |
| Member of the EHESS | 0.046** | 0.053*** | 0.036* | 0.041* | 0.043* | 0.022 |
| | (0.019) | (0.016) | (0.020) | (0.023) | (0.024) | (0.037) |
| Competitive exam fixed effects | No | Yes | Yes | Yes | Yes | Yes |
| Field | All competitive exams | All competitive exams | All experimental exams | All experimental exams with composition | Assist. Pr. Experimental exams | Professor experimental exams |
| Number of applications | 2194 | 2194 | 991 | 749 | 563 | 428 |
| [n1 ; n2] | [357; 62] | [357; 62] | [184; 55] | [143 ; 42] | [131; 33] | [53; 22] |

- Effect is smaller and more significant that for the put forward. Interpretation: non-linearity.
- Still significant when restricting to truly "experimental" exams.

# Randomized experiment is here not double blind

- Double blind: neither the patient nor the experimenter know if the patient takes treatment or placebo.
- Ehess
  - Commission knows who is applying
    - Possible influence on the probability of presence on D-day.
    - Members can push their contacts to apply (or not to apply)
  - Applicants may know who is member of the commission
    - Strategic application (or withdrawal)
- "Experimental conditions" may modify the results of the experiment.

# Do the randomized experiment modify the behavior of the commission?

- Probably to be present on d-day for a drawn member
- Yes, it does modify members' behavior=> More present when contact apply
- Solution: intention to treat rather than treatment on treated.

| Variables | 1<br>(Logit) | 2<br>(OLS) |
|---|---|---|
| Drawn substitute member | -1.405 ***<br>(0.146) | -0.306 ***<br>(0.031) |
| Ex-officio member | 1.06 ***<br>(0.154) | 0.146 ***<br>(0.021) |
| At least one former advised PhD applies | 0.918 **<br>(0.404) | 0.133 ***<br>(0.048) |
| Drawn substitute member * At least one former advised PhD applies | 0.386<br>(0.884) | 0.053<br>(0.207) |
| Ex-officio member * At least one former advised PhD applies | 1.172 **<br>(0.459) | 0.093 ***<br>(0.023) |
| Competitive exam fixed effects | Yes | Yes |
| Field | All competitive exams with composition and presence | All competitive exams with composition and presence |
| N | 2820 | 2820 |

# Do the experiment modify the applicants' behavior?

- Probability for a potential applicant to apply
- No, It does not modify applicants' behavior

| Variables | 1 (Logit) | 2 (OLS) |
|---|---|---|
| Number of years since PhD | 0.156 ** | 0.0007 |
| | (0.073) | (0.0006) |
| Squared number of years since PhD | -0.022 *** | -0.0001** |
| | (0.006) | (0.00004) |
| Advisor drawn member of the EC | 0.184 | 0.003 |
| | (0.183) | (0.003) |
| Advisor ex-officio member of the EC | -0.012 | -0.00008 |
| | (0.227) | (0.002) |
| Advisor member of the EHESS | 0.403 *** | 0.004** |
| | (0.101) | (0.0009) |
| Competitive exam fixed effects | Yes | Yes |
| Field | Assist. pr. exams | Assist. pr. exams |
| Number of potential applications | 41 530 | 41 530 |

# Is the random draw really random?

- Col 1 & 2. Probability of being put forward by the electoral commission
- Col 3 & 4. Probability of having one's PhD supervisor randomly drawn in the electoral commission
- The random draw is really random:
  Characteristics influencing the outcome (ENS) are not correlated with the random assignment to electoral commission

| | Proposed by EC | | Advisor drawn in EC | |
|---|---|---|---|---|
| Variables | 1 (Logit) | 2 (OLS) | 3 (Logit) | 4 (OLS) |
| Woman | -0.268 | -0.028 * | -0.119 | -0.006 |
| | (0.164) | (0.016) | (0.491) | (0.085) |
| Born outside France | -0.434 ** | -0.042 ** | -0.152 | -0.013 |
| | (0.201) | (0.019) | (0.702) | (0.109) |
| *École Normale Supérieure* alumni | 0.574 *** | 0.093 *** | 0.829 | 0.152 |
| | (0.222) | (0.032) | (0.925) | (0.158) |
| *Agrégation* (High school professor exam) | 0.491 ** | 0.052 * | -0.244 | -0.025 |
| | (0.206) | (0.026) | (0.968) | (0.159) |
| Already member of the EHESS | 0.93 *** | 0.124 *** | -0.631 | -0.149 |
| | (0.172) | (0.022) | (0.7) | (0.141) |
| Age | -0.045 *** | -0.005 *** | -0.0003 | 0.0002 |
| | (0.013) | (0.001) | (0.045) | (0.009) |
| Anthropology | 0.21 | 0.021 | 0.463 | 0.074 |
| | (0.222) | (0.025) | (0.962) | (0.137) |
| History | 0.291 * | 0.031 | 0.522 | 0.089 |
| | (0.165) | (0.019) | (0.749) | (0.117) |
| Sociology | 0.011 | -0.009 | 0.401 | 0.066 |
| | (0.221) | (0.023) | (0.916) | (0.134) |
| Economics | 0.095 | 0.008 | 0.663 | 0.081 |
| | (0.272) | (0.031) | (1.311) | (0.24) |
| Number of previous trials | 0.487 *** | 0.058 *** | -0.525 | -0.086 |
| | (0.173) | (0.018) | (0.622) | (0.107) |
| Square number of previous trials | -0.034 | -0.004 | 0.105 | 0.02 |
| | (0.026) | (0.003) | (0.109) | (0.018) |
| Number of publications | 0.018 *** | 0.003 *** | 0.015 | 0.002 |
| | (0.004) | (0.001) | (0.033) | (0.004) |
| Competitive exam fixed effects | Yes | Yes | Yes | Yes |
| Field | All competitive exams | All competitive exams | All competitive exams. Applications with advisor at EHESS drawn or undrawn | All competitive exams. Applications with advisor at EHESS drawn or undrawn |
| N | 2171 | 2171 | 418 | 418 |

Comparison

## Table A1: The role of connections, by type of connection

| | 1 | 2 | 3 | 4 | 5 | 6 | 7 | 8 | 9 | 10 | 11 | 12 |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | Means | | | The effect of connections on candidates' success | | | Pre-exam quality of promoted candidates | | | Post-exam quality of promoted candidates | | |
| | All | FP | AP | All | FP | AP | All | FP | AP | All | FP | AP |
| **Strong connection:** | | | | | | | | | | | | |
| - PhD advisor | 3 | 3 | 3 | 0.141*** | 0.098*** | 0.173*** | -0.186*** | -0.190 | -0.156** | -0.074 | 0.015 | -0.102 |
| | | | | (0.014) | (0.020) | (0.019) | (0.065) | (0.128) | (0.074) | (0.073) | (0.153) | (0.080) |
| - Co-author | 8 | 10 | 6 | 0.065*** | 0.077*** | 0.051*** | -0.005 | -0.036 | -0.015 | -0.100* | -0.009 | -0.206*** |
| | | | | (0.009) | (0.011) | (0.013) | (0.051) | (0.069) | (0.075) | (0.052) | (0.073) | (0.073) |
| **Institutional connection:** | | | | | | | | | | | | |
| - Same university | 26 | 28 | 25 | 0.040*** | 0.038*** | 0.041*** | -0.065** | -0.070 | -0.069* | -0.090*** | -0.062 | -0.115** |
| | | | | (0.004) | (0.006) | (0.006) | (0.030) | (0.048) | (0.038) | (0.033) | (0.049) | (0.044) |
| **Weak tie:** | | | | | | | | | | | | |
| - PhD thesis committee member | 7 | 9 | 5 | 0.029*** | 0.021** | 0.042*** | 0.002 | -0.039 | 0.032 | 0.130** | 0.100 | 0.148* |
| | | | | (0.008) | (0.010) | (0.013) | (0.059) | (0.086) | (0.081) | (0.060) | (0.085) | (0.084) |
| - Link by invitation | 4 | 8 | 0.5 | 0.043*** | 0.045*** | 0.020 | 0.015 | -0.057 | 0.427 | 0.002 | -0.062 | 0.564** |
| | | | | (0.012) | (0.013) | (0.046) | (0.076) | (0.071) | (0.324) | (0.070) | (0.068) | (0.281) |
| - Same PhD thesis committee | 10 | 21 | 2 | 0.009 | 0.006 | 0.046* | 0.049 | 0.010 | 0.326* | 0.069 | 0.025 | 0.440*** |
| | | | | (0.007) | (0.007) | (0.025) | (0.054) | (0.055) | (0.172) | (0.050) | (0.053) | (0.146) |
| **Indirect tie:** | | | | | | | | | | | | |
| - Same PhD advisor | 0.3 | 0.3 | 0.2 | 0.048 | 0.089 | 0.023 | -0.338 | -1.291*** | 0.286 | -0.456* | -0.673 | -0.331 |
| | | | | (0.046) | (0.086) | (0.053) | (0.334) | (0.346) | (0.479) | (0.271) | (0.426) | (0.393) |
| - Same co-author | 14 | 12 | 15 | -0.002 | 0.005 | -0.006 | -0.007 | -0.096 | 0.035 | -0.068 | -0.143 | -0.032 |
| | | | | (0.006) | (0.009) | (0.007) | (0.052) | (0.088) | (0.065) | (0.052) | (0.103) | (0.060) |
| - Same PhD thesis committee member | 8 | 8 | 9 | 0.001 | 0.005 | -0.002 | 0.084 | 0.040 | 0.088 | 0.086 | 0.210** | 0.032 |
| | | | | (0.007) | (0.011) | (0.009) | (0.054) | (0.101) | (0.063) | (0.056) | (0.104) | (0.065) |
| Constant | | | | 0.113*** | 0.106*** | 0.118*** | 0.419*** | 0.525*** | 0.347*** | 0.457*** | 0.484*** | 0.436*** |
| | | | | (0.002) | (0.003) | (0.003) | (0.018) | (0.027) | (0.023) | (0.017) | (0.026) | (0.022) |
| Adjusted R-squared | | | | 0.012 | 0.012 | 0.013 | 0.002 | 0.004 | 0.004 | 0.004 | 0.003 | 0.012 |
| Number of observations | | | | 31750 | 13612 | 18138 | 3573 | 1446 | 2127 | 3573 | 1446 | 2127 |

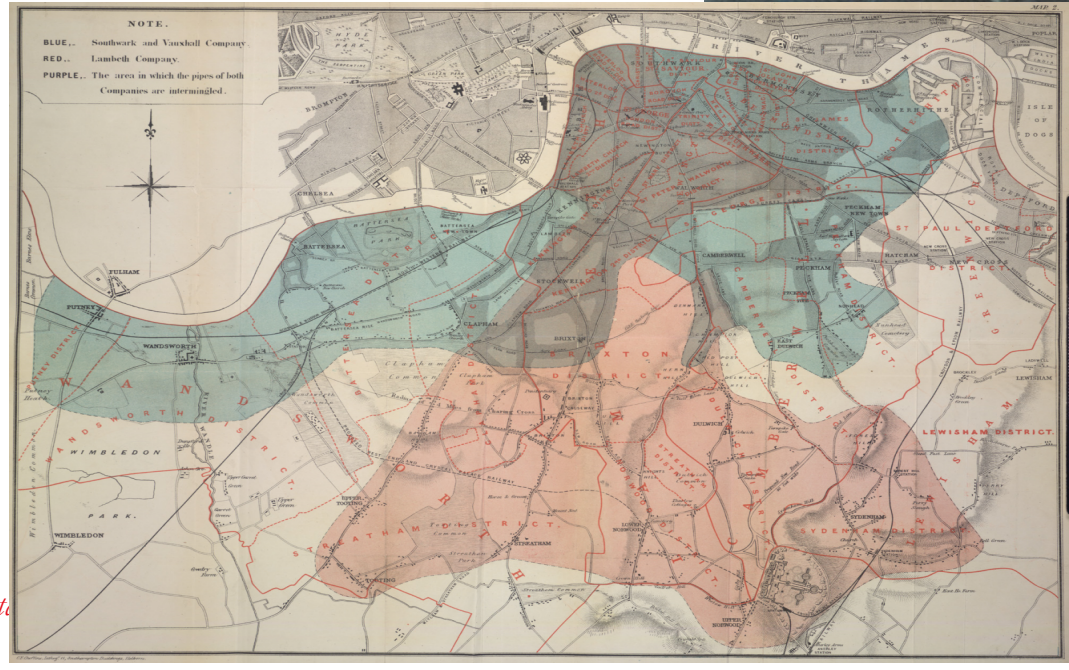
*Notes*: Columns 1-3 provide information on the means of the corresponding variables. Columns 4-12 report OLS estimates, standard errors clustered by exam are reported in parentheses. Columns 4-6 provide information from an analysis similar to the one reported in Table 5. Columns 7-12 replicate the analysis in Table 8. * – p-value<0.10, ** – p-value<0.05, *** – p-value<0.01.

Zinovyeva & Bagues 2015

# 3. Differences-in-Differences (or *Diff-in-Diff*)

# John Snow (1855) and the cholera

- Snow. 1855. *On the Mode of Communication of Cholera (2nd edittion)*
- Water distributed in London by different private companies
- South of London, two big companies:
  - Lambeth Company (pink) : water coming from Ditton on Thames, 22 miles upstream)
  - Southwark and Vauxhall Company (blue) (water coming from the Thames in the center of London)
- Sometimes two different water companies in the same street




*Experiment*

# Cholera epidemics of 1853/54

- Rate of death for 10 000 persons
  - Lambeth 10
  - Southwark and Vauxhall 150
- It could be due to water or to other factors
- Snow compares with the rate of death during previous cholera epidemics (1849)
  - Lambeth 150
  - Southwark and Vauxhall 125
- In 1852, Lambeth Company moves its water origin from Hungerford Bridge (center of London) to Ditton

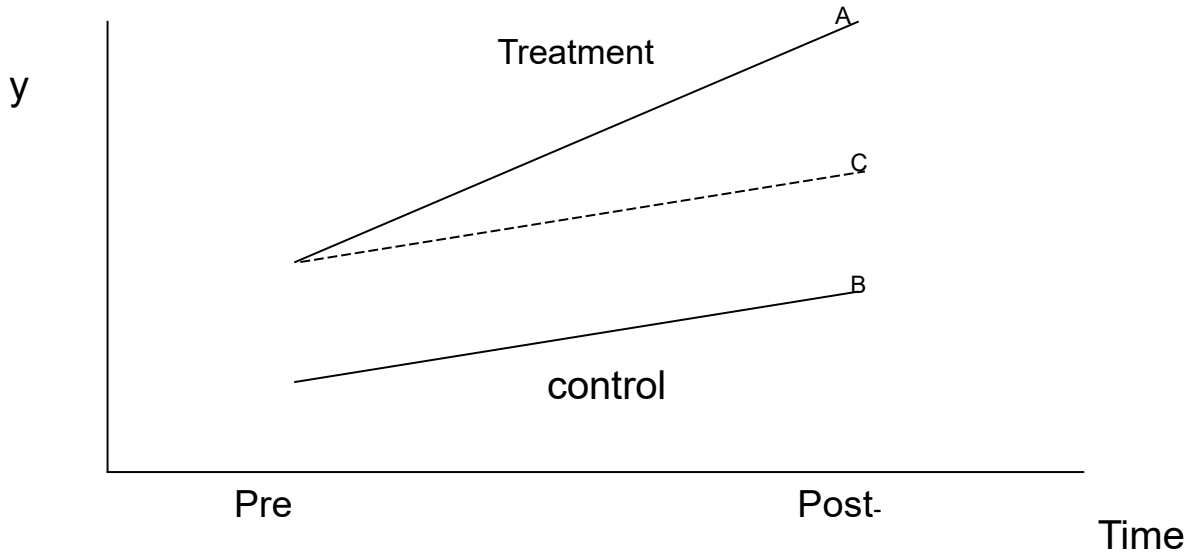
# Estimated effect of clean water

| | 1849 | 1853/54 | Difference |
|---|---|---|---|
| Vauxhall and Southwark | 125 | 150 | 25 |
| Lambeth | 150 | 10 | -140 |
| Difference | -25 | 140 | 165 |

# Base idea of differences-in-differences

- Keeping from RCT the opposition between treated & control groups
- We are not sure that the only difference between treated and control groups lies in the treatment
- => A weaker hypothesis: the difference between treated and control groups is time invariant.
  - Difference pre-treatment is the difference due to « unobservable » factors
  - Difference post-treatment is the difference due to « unobservable » factors + causal effect
  - Difference-in-differences is the causal effect

# A graphical presentation



# Some notations

- Simple difference

$$Treated - control$$

$$T_1 - C_1$$

- The difference-in-difference is the following estimation:

$$Diff\text{-}in\text{-}Diff = (Treated_{post} - Treated_{ante}) - (control_{post} - control_{ante})$$

$$Diff\text{-}in\text{-}Diff = (T_1 - T_0) - (C_1 - C_0)$$

- Classical notations :

$T_1 = \mu_{11}$ ; $T_0 = \mu_{10}$ ; $C_1 = \mu_{01}$ ; $C_0 = \mu_{00}$

$Diff\text{-}in\text{-}Diff = (\mu_{11} - \mu_{01}) - (\mu_{10} - \mu_{00})$

# Econometric estimations

- With a panel
  - We measure outcomes for the same individuals, before and after
  - We estimate evolution in outcomes

  $\Delta y_i = \beta_0 + \beta_1 * TG + \varepsilon_i$ where $TG$ is the treated group
  - $\beta_1$ is the diff-in-diff estimator
- Without panel
  - Individuals before and after are not the same

  $y_{it} = \beta_0 + \beta_1 * GT + \beta_2 * t + \beta_3 * t * TG + \varepsilon_{it}$
  - $\beta_3$ is the diff-in-diff estimator

# Strength and limits

- Strong hypotheses
  - Difference between treated and control groups would have remained constant in the absence of any treatment
  - Or the diff-in-diff is uniquely due to the "Treatment" and does not owe anything to any other changes in the treated group between period 1 and 2
- If we have more than two periods, one can do a graphical/statistical verification of the invariance of unobserved differences (before treatment or during treatment).

# Example (Light, Massoglia, Dinsmore, 2019)

- Effect of crises (Sept 11th) on foreigners' punishment
  - –No national effect
  - –A local effect

TABLE 1
SEPTEMBER 11 AND THE PUNISHMENT OF NON-U.S. CITIZENS IN U.S. FEDERAL COURTS

| | % INCARCERATED | | SENTENCE LENGTH (Logged Months) | |
|---|---|---|---|---|
| DISTRICT COURT | Pre-9/11 | Post-9/11 | Pre-9/11 | Post-9/11 |
| New York/Washington, D.C.: | | | | |
| Non-U.S. citizen | 85 | 92 | 3.17 | 3.08 |
| U.S. citizen | 75 | 74 | 2.71 | 2.49 |
| DD | 8 | | .12 | |
| Rest of U.S. District Courts: | | | | |
| Non-U.S. citizen | 95 | 96 | 3.27 | 3.22 |
| U.S. citizen | 83 | 84 | 3.07 | 3.07 |
| DD | 0 | | −.05 | |
| DDD | 8 | | .17 | |

NOTE.—Pre-9/11 corresponds to 12 months before September 11, 2001. Post-9/11 includes 12 months after September 11, 2001. DD = difference in differences; DDD = difference in difference in differences

# 4. Regression discontinuity design

"Ces rites marquent une séparation, une frontière sacrée, comme celle qui, dans les concours, sépare le dernier reçu du premier collé, quart de point magique qui crée une différence pour toute la vie. Les grands concours sont les rites magiques par lesquels nos sociétés instituent leurs héritiers légitimes."

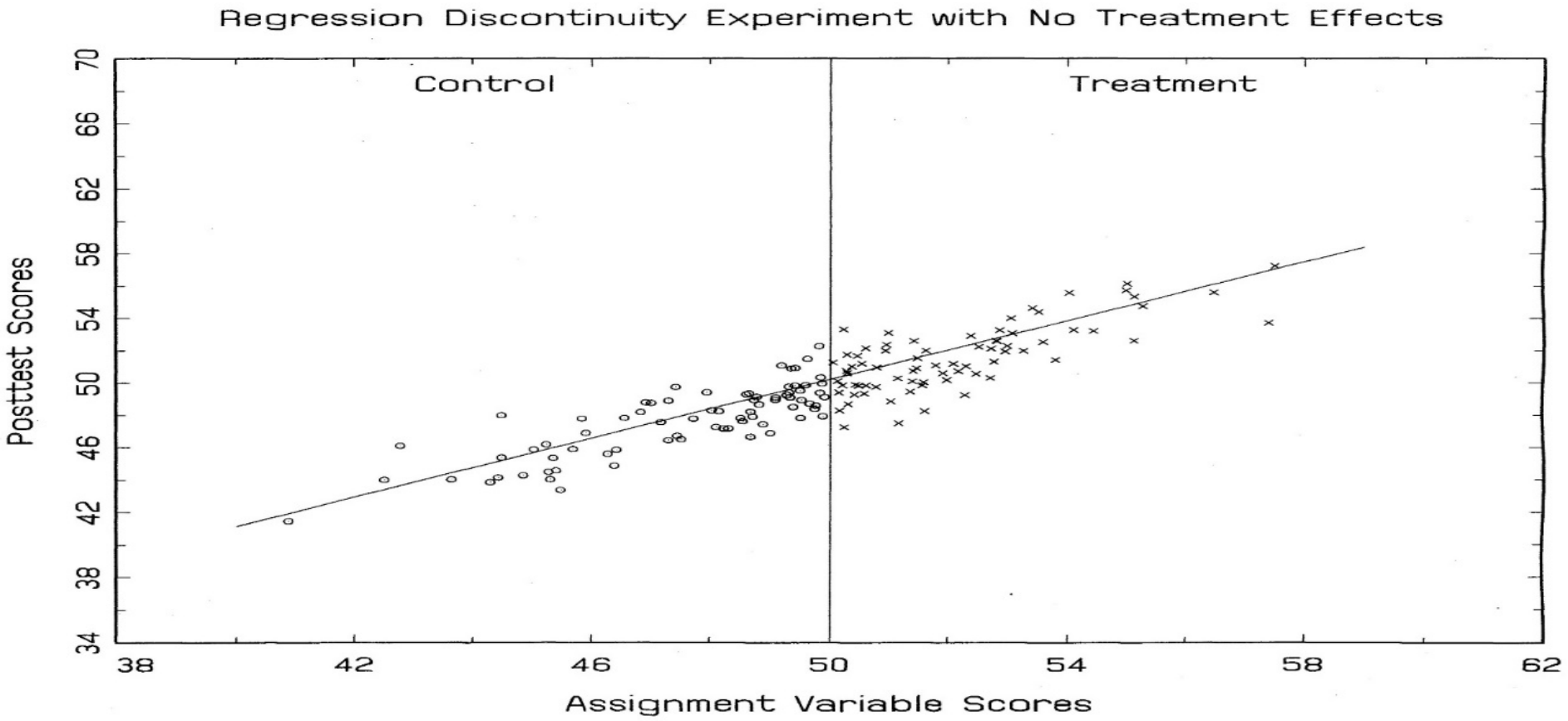
P. Bourdieu (Entretien avec Eribon)

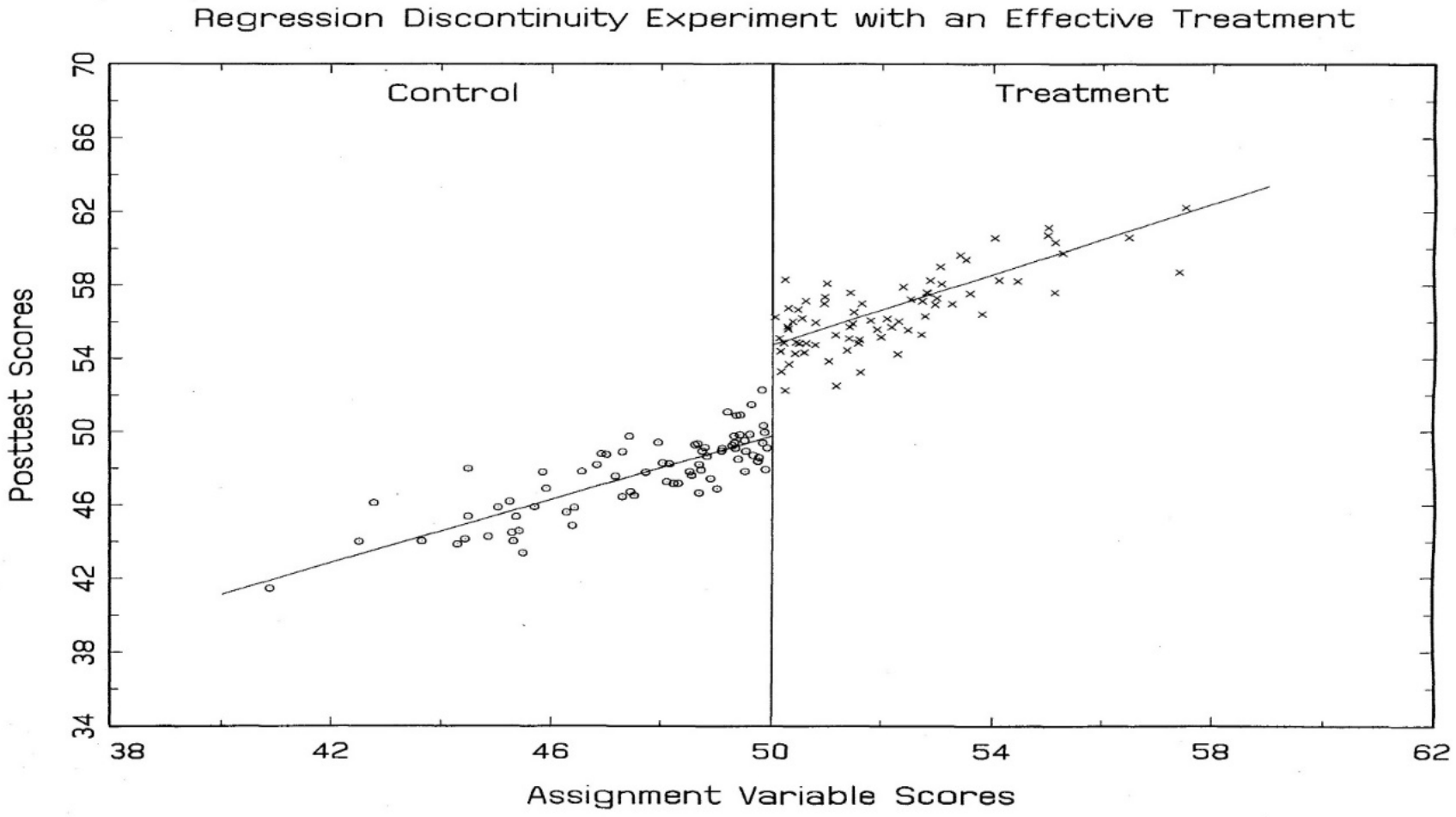
# Regression discontinuity design

- Invented in psychology: Thistlewaite & Campbell. 1960. "Regression-Discontinuity Analysis: An alternative to the ex post facto experiment". *Journal of Educational Psychology*
- The assignment to receive or not a treatment depends on a threshold on a measurable variable (continuous)
- For instance, drivers arrested with more than a certain degree of alcohol in the blood have the obligation to follow a treatment. Groups below the threshold serve as control group for comparison.
- The treatment effect is measured around the discontinuity between treated group and control group (we don't do the simple difference between the two groups).

# Application conditions

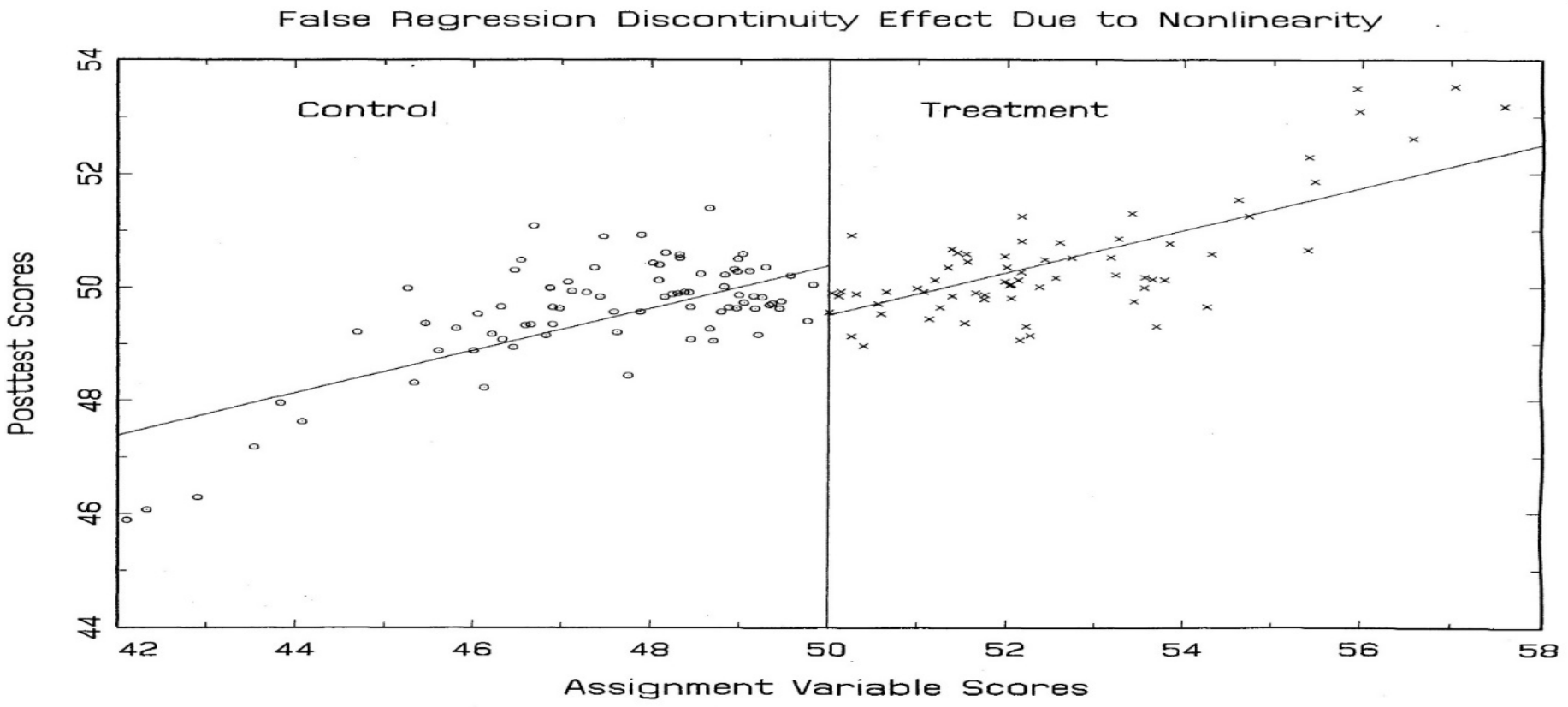
- Exogenous threshold, non manipulable, which activate some actions
  - Absolute majority => effect of the election
  - Rank of the last hired in a school exam => school effect
  - Threshold of activation of a social or fiscal measure => effect of a social policy
  - Etc.

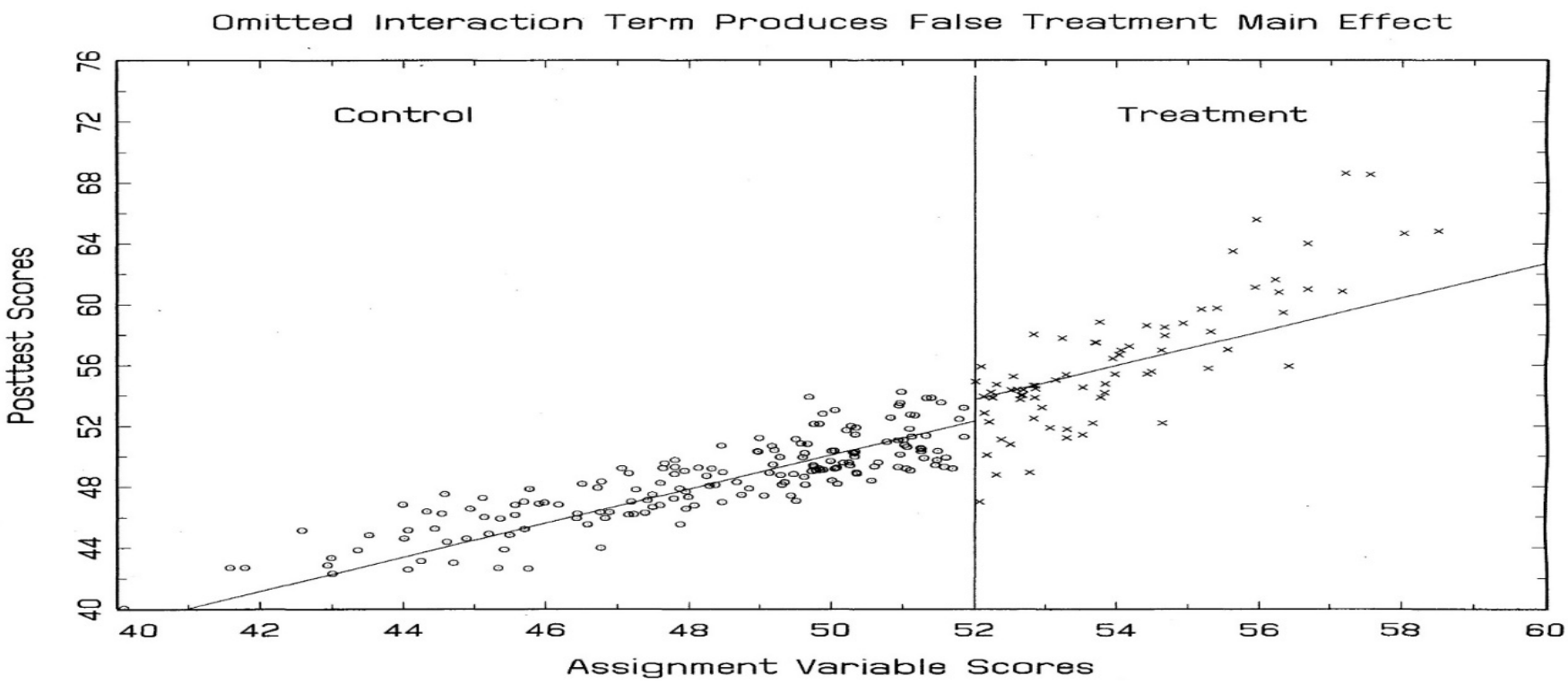

Regression Discontinuity Experiment with No Treatment Effects
Control
Treatment
Posttest Scores
70
66
62
58
54
50
46
42
38
34
38
42
46
50
54
58
62
Assignment Variable Scores


Regression Discontinuity Experiment with an Effective Treatment
Control
Treatment
Posttest Scores
70
66
62
58
54
50
46
42
38
34
38
42
46
50
54
58
62
Assignment Variable Scores

# Advantages and disadvantages

- Advantages
  - When well done, regression discontinuity design enable an unbiased estimation of the treatment effect.
- Limits
  - Statistical power is lower than for RCT with the same number of observations. Statistical power is critical.
  - Effects are unbiased only if the functional form between assignment variable and the outcome variable is well modeled:
    - Non-linear relations
    - Eventual interactions


False Regression Discontinuity Effect Due to Nonlinearity
Control
Treatment
Posttest Scores
Assignment Variable Scores
44
46
48
50
52
54
42
44
46
48
50
52
54
56
58


Omitted Interaction Term Produces False Treatment Main Effect
Control
Treatment
Posttest Scores
Assignment Variable Scores
40
42
44
46
48
50
52
54
56
58
60
40
44
48
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56
60
64
68
72
76

# Econometric estimations

**Beware of centering the variable x on the threshold !** ***(not necessary for model 1) but necessary for all other models***

***x'=x-threshold***

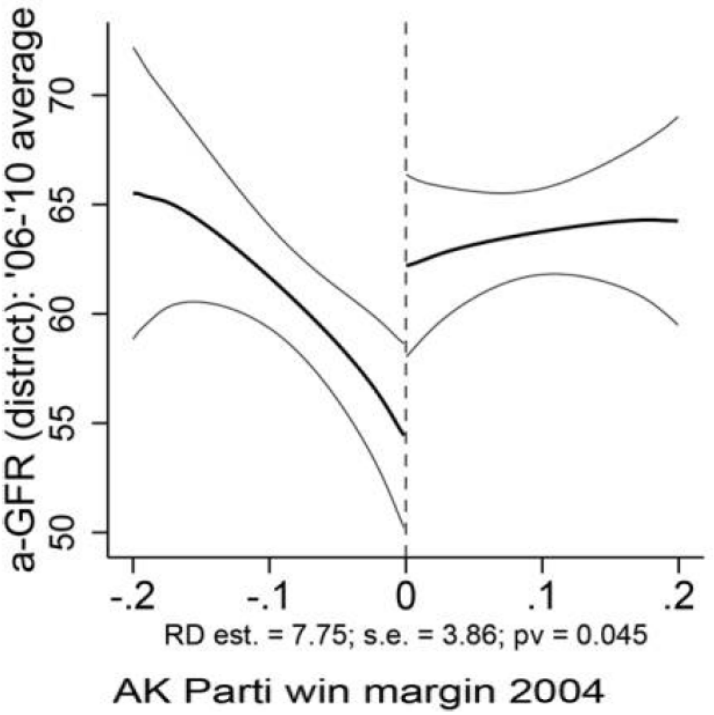
- Simple linear model (first estimations)
  - $y_i = \beta_0 + \beta_1 * x' + \beta_2 * (x' > 0) + \varepsilon_i$ *[alternatively* $y_i = \beta_0 + \beta_1 * x + \beta_2 * (x > threshold) + \varepsilon_i$*]*
  - The causal effect is measured by $\beta_2$
  - Limit: it supposes that the functional form is the same on both side of the threshold.
- Linear model with a change in slope
  - $y_i = \beta_0 + \beta_1 * x + ' \beta_2 * (x' > 0) + \beta_3 * x' * (x' > 0) + \varepsilon_i$
  - The causal effect is measured by $\beta_2$
- The non-linear model with a change in the shape
  - $y_i = \beta_0 + \beta_1 * x' + \beta_2 * x'^2 + \beta_3 * (x' > 0) + \beta_4 * x' * (x' > 0) + \beta_5 * x'^2 * (x' > 0) + \varepsilon_i$
  - The causal effect is measured by $\beta_3$

# Common practices

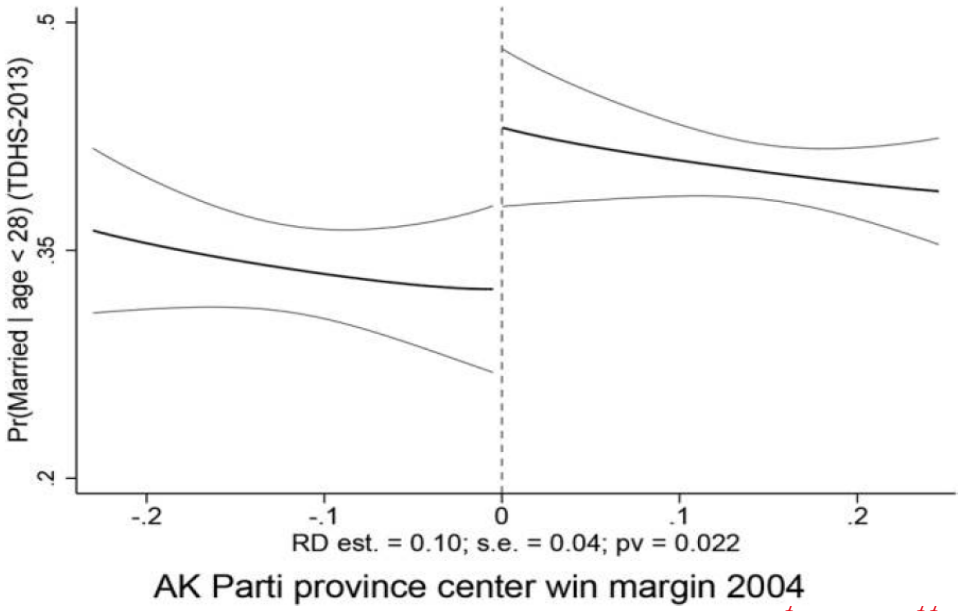
- Graphic investigation of the relation plays an important role
  - Proof
  - Model validation
  - In some cases (notably for dichotomous outcome variables) we calculate the average of observations for groups of k observations (k=10)
- Estimation of a non-linear shape on the whole distribution (quadratic or more)
- Estimation of a more sophisticated non-linear form, LOESS, etc.
- Estimation of a linear form on the distribution around the threshold (+20% below / over )

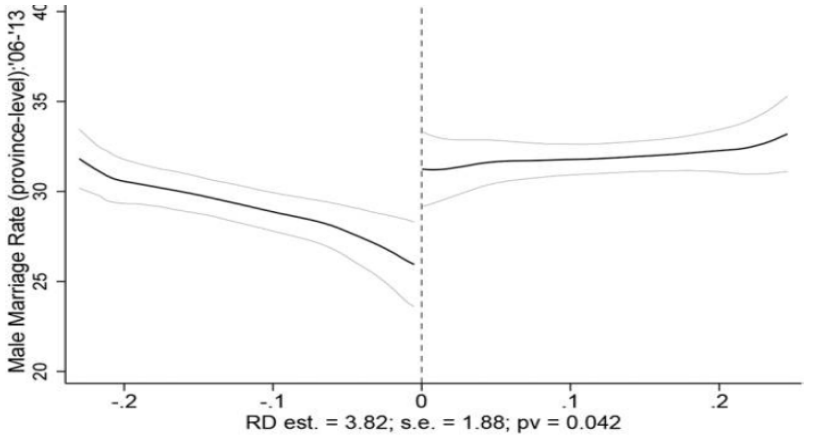
# Impact of political Islam (Aksoy, Billari, 2018)

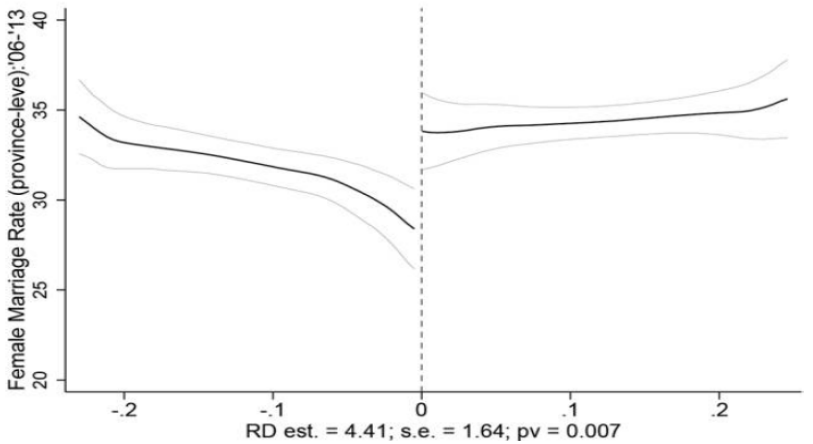
- AKP has populist policies : pro-family, pro-fertility for religious reasons
- At the local level, AKP municipalities are very involved in social policies
- RDD: Regression discontinuity design
  - 916 districts in Turkey
  - Local elections 2004
  - Assignment variable: AKP party win margin over challenger party
  - Threshold Score>0 (victory)
  - Impact on the general fertility rate
    - +7.75‰ period 2006-10



- Impact on marriage
  - +3.8%* for males, +4.4%* for females
  - +10%* for age<28

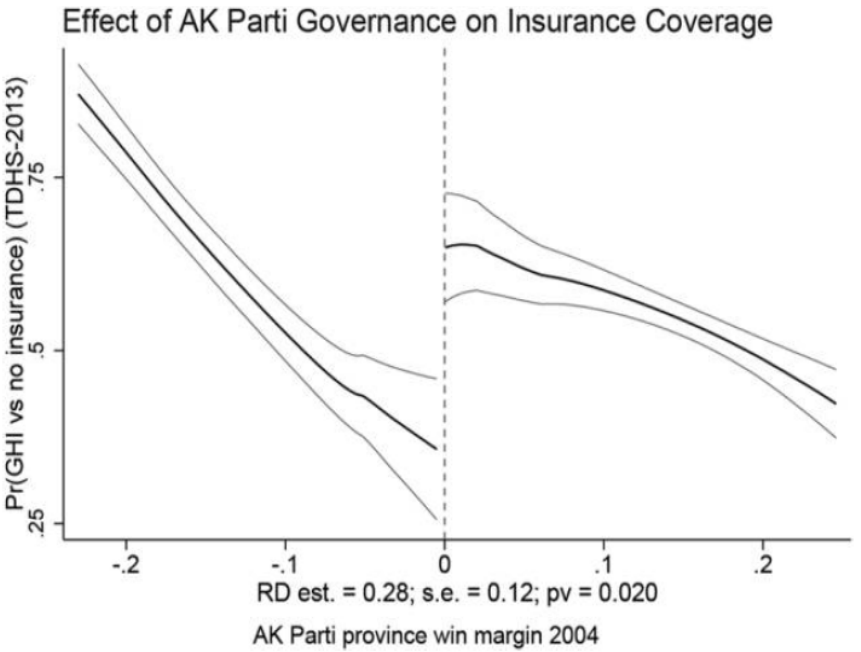


# Mediation mechanisms: social welfare

- AKP Leaders increase social welfare
  - +0.28%* social welfare at the threshold
- Alternatives explanations investigated (also with RDD)
  - Religiosity does not increase
  - No more migration from or towards these regions
  - Unemployment does not vary
  - Ideal number of child is the same



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