Applied Microeconometrics I

Lecture 8: Difference-in-differences

Cristina Bratu

Aalto University

September 28, 2023

Lecture Slides

What did we do last time?

- Instrumental variables in a regression framework

$$\begin{aligned} Y_i &= \alpha + \rho S_i + u_i \\ u_i &\equiv \gamma A_i + \varepsilon_i \end{aligned}$$

  with $E[S_i \varepsilon_i] = 0$
- No RCT, can't observe A
- Come up with an instrument Z such that

$$Cov(Z_i, S_i) \neq 0 \text{ (instrument is relevant) and}$$
$$Cov(Z_i, u_i) = 0 \text{ (instrument is valid)}$$

What did we do last time?

- Write

$$\begin{aligned} Cov(Z_i, Y_i) &= Cov(Z_i, \alpha + \rho S_i + u_i) \\ &= \rho Cov(Z_i, S_i) + Cov(Z_i, u_i) \end{aligned}$$

- Since $Cov(Z_i, u_i) = 0$ if the instrument is valid, it follows that:

$$\rho = \frac{Cov(Z_i, Y_i)}{Cov(Z_i, S_i)} = \frac{\frac{Cov(Z_i, Y_i)}{Var(Z_i)}}{\frac{Cov(Z_i, S_i)}{Var(Z_i)}}$$

$\Longrightarrow$ ρ is the ratio between the coefficient on Z_i from a regression of Y_i on Z_i (reduced form) and the coefficient on Z_i from a regression of S_i on Z_i (first stage)

What did we do last time?

- Angrist and Krueger (1991) example: quarter of birth as an instrument for schooling
- Angrist and Evans (1998) example: Siblings sex mix instrument for family size
- Lundborg et al. (2016) example: IVF treatment as an instrument for having a child

Background

- It is not always possible to run a randomized controlled trial.
- When we can't run an experiment, we can try to learn about the impact of a treatment using an identification strategy based on selection on observables:
  - We can compare individuals exposed to the treatment with individuals that look like them in terms of observables, but who weren't exposed to the treatment.
  - Unfortunately, this evidence may be subject to selection biases and it is often difficult to interpret.
- Maybe we can instead look for an instrumental variable, but good instruments are difficult to find...
- What else can we do?

Example: the impact of death penalty

- Let us consider the case of a binary treatment
- For instance, we can consider the following question:
  - Does the death penalty reduce the homicide rate?
- We will follow the review of the literature by Donohue and Wolfers (2006) to analyze how different scholars have approached this question.

Example: the impact of death penalty

Before-and-after approach

- Some authors have analyzed how the homicide rate evolves before and after the abolition (or the introduction) of death penalty.
- For instance Dezhbakhsh and Shepherd (2004) use data from US states which have either introduced or abolished the death penalty between 1960 and 2000. They show that:
    - when the death penalty is abolished, the homicide rate tends to increase
    - when the death penalty is reinstated, the homicide rate tends to decrease

Table 1: Estimating How Changes in Death Penalty Laws Effect Murder: Selected Before and After Comparisons: 1960-2000

Dependent Variable: ***% Change in State Murder Rates Around Regime Changes***	Death Penalty Abolition			Death Penalty Reinstatement		
	1-Year Window (1)	2-Year Window (2)	3-Year Window (3)	1-Year Window (4)	2-Year Window (5)	3-Year Window (6)
	Panel B: Our Replication: Changes Around Death Penalty Shifts (Treatment)					
Mean Change	10.1%*** (2.9)	16.0%*** (2.3)	21.5%*** (2.6)	-6.3%* (3.4)	-7.0%** (2.9)	-3.8% (2.9)
Median Change	8.5%	13.8%	18.5%	-9.3%	-8.5%	-7.4%
Number of States Where Homicide Increased	35/46	39/46	41/46	12/41	15/39	14/39

- As Donohue and Wolfers (2005) point out, there are two possible interpretations for this empirical evidence:
  - Causal effect: the introduction (abolition) of death penalty decreases (increases) homicide rates
  - Spurious correlation: there are some confounding effects

Difference-in-differences

- How can we control for confounding effects?
- Difference-in-differences strategy: We can try to look for a control group which is similarly affected by these confounding effects.
- For instance, we can also examine the evolution of homicide rates during the same period in states that did not experience any policy change.
- Donohue and Wolfers 2005 show that this group exhibits very similar trends (Table 1, panel C).

Table 1: Estimating How Changes in Death Penalty Laws Effect Murder: Selected Before and After Comparisons: 1960-2000

Dependent Variable: ***% Change in State Murder Rates Around Regime Changes***

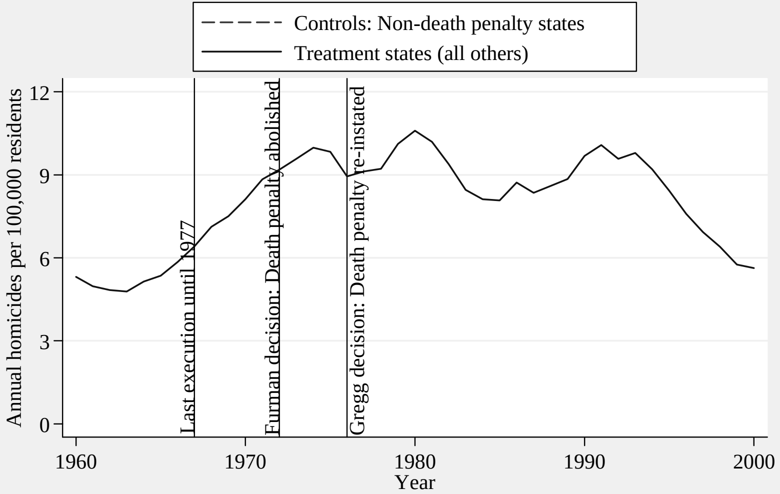
	Death Penalty Abolition			Death Penalty Reinstatement		
	1-Year Window (1)	2-Year Window (2)	3-Year Window (3)	1-Year Window (4)	2-Year Window (5)	3-Year Window (6)
	Panel B: Our Replication: Changes Around Death Penalty Shifts (Treatment)					
Mean Change	10.1%*** (2.9)	16.0%*** (2.3)	21.5%*** (2.6)	-6.3%* (3.4)	-7.0%** (2.9)	-3.8% (2.9)
Median Change	8.5%	13.8%	18.5%	-9.3%	-8.5%	-7.4%
Number of States Where Homicide Increased	35/46	39/46	41/46	12/41	15/39	14/39
	Panel C: Our Innovation: Changes in Comparison States (Control)					
Mean Change	8.7%*** (0.5)	16.0%*** (0.8)	20.6%*** (1.1)	-7.5%*** (1.5)	-6.6%*** (1.5)	-3.7%*** (1.3)
Median Change	8.5%	16.1%	20.9%	-11.5%	-9.8%	-5.2%
Number of States Where Homicide Increased	44/46	44/46	44/46	7/41	8/39	8/39

Table 1: Estimating How Changes in Death Penalty Laws Effect Murder: Selected Before and After Comparisons: 1960-2000

Dependent Variable: ***% Change in State Murder Rates Around Regime Changes***	Death Penalty Abolition			Death Penalty Reinstatement		
	1-Year Window (1)	2-Year Window (2)	3-Year Window (3)	1-Year Window (4)	2-Year Window (5)	3-Year Window (6)
	Panel D: Difference-in-Difference Estimates (Treatment-Control)					
Mean Change	1.4% (2.9)	-0.1% (2.4)	0.9% (2.8)	1.2% (3.7)	-0.5% (3.2)	-0.1% (3.2)
Median Change	<0.001% (2.7)	-2.3% (2.5)	-2.4% (3.6)	2.2% (3.5)	1.3% (4.5)	-2.2% (2.0)

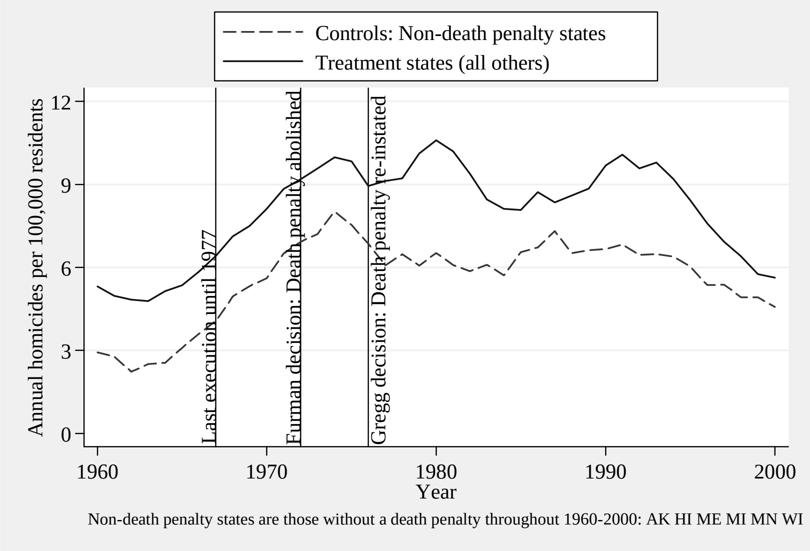
$\Longrightarrow$ If we compare the two groups, states that introduced or abolished the death penalty (Panel B) vs. states that did not make any changes (Panel C), we find no significant differences (panel D).

- We can also focus on the death penalty moratorium between 1972 and 1978.
- First, let us see how the number of homicides varies in states that have death penalty when:
    - death penalty was abolished
    - death penalty was reinstated

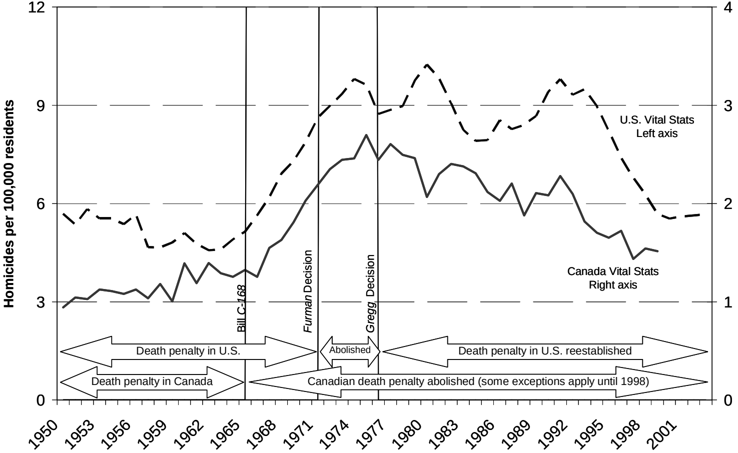


Non-death penalty states are those without a death penalty throughout 1960-2000: AK HI ME MI MN WI

Let us compare it to states that did not have death penalty



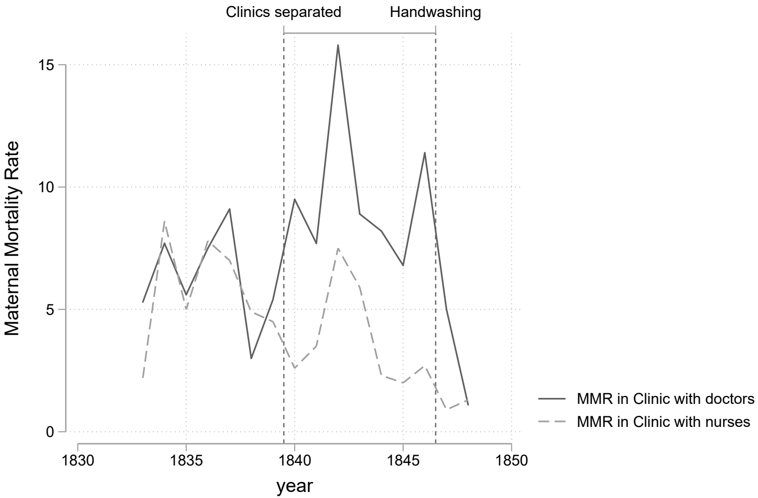
Or we can compare the evolution of homicide rates in the US and Canada



Example: Discovery of the importance of handwashing

- In 1840s the observers of Vienna's maternity hospital noted that death rates from postpartum infections were higher in one wing than the other
  - Wing 1 was attended by doctors and trainee doctors
  - Wing 2 was attended by midwives and trainee midwives
- Doctor Ignaz Semmelweis noted that the difference emerged in 1841 when the hospital moved to an "anatomical" training program involving cadavers
  - Only doctors received training with cadavers, not midwives
  - Hypothesis: Transference of "cadaveric particles" explains the difference in death rates
  - Intervention by Semmelweis: Handwashing with chlorine
- Policy implemented in May of 1847

Maternal mortality rates in Vienna's maternity hospital 1833-1848



Difference-in-differences (diff-in-diff, DiD)

- The above examples capture the main intuition behind the difference-in-differences analysis.
- We use the evolution of the outcome variable in the control group to construct a counterfactual of what would have happened in the treatment group in the absence of the treatment.
- The fundamental identifying assumption is that, in the absence of the treatment, both groups would follow parallel trends.
- Note that this empirical strategy allows for the existence of time-invariant differences between the two groups, but it assumes that there are no time-variant relevant differences.

Difference-in-differences (diff-in-diff, DiD)

- With only two states (A and B) and two periods ($t = 1, 2$) diff-in-diff strategy can be illustrated in a very simple way
- Suppose that:
  - **At period** 1: Both states have the death penalty
  - **At period** 2: State A abolishes death penalty while state B keeps it
- Now assume that homicide rate $Y_{i,t}$ in state $i = A, B$ at period $t = 1, 2$ is determined by:

$$Y_{i,t} = \alpha_i + \lambda_t + \rho D_{i,t} + \epsilon_{i,t}$$

  where
  - $D_{i,t} = 1$ if death penalty is abolished and zero otherwise
  - α_i is a state-specific variable that is constant over time
  - λ_t is a time shock that is common to both states
  - $E(\epsilon_{i,t}) = 0$
- Our goal is to find out the causal effect ρ of abolishing the death penalty

Difference-in-differences (dif-in-dif)

$$Y_{i,t} = \alpha_i + \lambda_t + \rho D_{i,t} + \epsilon_{i,t}$$

- This setting allows for several comparisons in which there is variation in the use of death penalty
- Suppose we compared states A and B in period $t = 2$? (cross-state comparison)
  - $E(Y_{A,2}) - E(Y_{B,2}) = (\alpha_A - \alpha_B) + \rho$
  - the effect is confounded by the unobserved time-fixed difference in homicide rates in the two states
- Suppose we compared state A between periods $t = 1$ and $t = 2$? (before-after comparison, as in the paper by Dezhbakhsh and Shepherd (2004) discussed earlier)
  - $E(Y_{A,2}) - E(Y_{A,1}) = (\lambda_2 - \lambda_1) + \rho$
  - the effect is confounded by changes in time in homicide rates that would have taken place even in the absence of the abolition

	State $i = A$	State $i = B$	Difference
Period $t = 1$	$\alpha_A + \lambda_1$	$\alpha_B + \lambda_1$	$(\alpha_A - \alpha_B)$
Period $t = 2$	$\alpha_A + \lambda_2 + \rho$	$\alpha_B + \lambda_2$	$(\alpha_A - \alpha_B) + \rho$
Difference	$(\lambda_2 - \lambda_1) + \rho$	$(\lambda_2 - \lambda_1)$	ρ

Suppose we compared changes in states A and B between periods $t = 1$ and $t = 2$? (difference-in-differences)

$$\rho = [E(Y_{A,2}) - E(Y_{B,2})] - [E(Y_{A,1}) - E(Y_{B,1})] =$$
$$(\alpha_A - \alpha_B) + \rho - (\alpha_A - \alpha_B)$$

- Comparing changes instead of levels adjusts for the state-specific differences that are fixed in time.

$$\rho = [E(Y_{A,2}) - E(Y_{A,1})] - [E(Y_{B,2}) - E(Y_{B,1})] =$$
$$(\lambda_2 - \lambda_1) + \rho - (\lambda_2 - \lambda_1)$$

- Comparing changes instead of levels adjusts for changes that would have taken place over time even in the absence of treatment.

Main threats to the validity of DiD estimates

1. Would the treated group have evolved in the same way as the control group in the absence of tretment?
2. Why did the treatment group adopt the policy, and not the control group?
   - There could have been other shocks that led the treatment group to adopt the policy; if these shocks also affected the outcome, the causal effect we are estimating is biased.
3. Policies are usually implemented in bundles $\rightarrow$ the outcome variable may be affected by these other policies.
   - We need to be precise about the interpretation: the effect we are estimating may be the effect of the treatment bundle, as opposed to the effect of the one policy we are interested in.
4. The treatment should not affect the control group (there should be no general equilibrium effects).
5. The composition of the treatment and control groups should not change as a result of treatment.
   - If composition changes, we would confound the effect of the treatment with the changes in the composition.

Usual checks

1. The two groups evolved similarly in the past (although note that this is neither a sufficient nor a necessary condition for the validity of the empirical strategy!)
2. The timing of the adoption of the policy was as good as random (no anticipation).
3. No other policies were adopted at the same time.
4. Verify that there is no reason to believe that the control group was affected.

Fixed effects vs. Difference-in-differences

- Difference-in-differences is an application of the familiar individual fixed-effects model with panel data:

$$Y_{it} = \alpha_i + \lambda_t + \rho D_{it} + X_{it}^{'}\beta + \epsilon_{i,t}$$

  where t denotes time (or something else, we return to this later) and i individuals
- α_i varies across i but not across t whereas λ_t varies across t but not across i
- The key to the identification of ρ is that we have repeated observations on i over t

Fixed effects vs. Differences-in-differences

- Then we can "eliminate" α_i and identify ρ either by converting the data into deviations from i-specific means:
  $$Y_{it} - \bar{Y_i} = \lambda_t - \bar{\lambda} + \rho(D_{it} - \bar{D_i}) + (X_{it} - \bar{X_i})'\beta + (\epsilon_{it} - \bar{\epsilon_i})$$
- or by differencing over t
  $Y_{it} - Y_{it-1} = \lambda_t - \lambda_{t-1} + \rho(D_{it} - D_{it-1}) + (X_{it} - X_{it-1})'\beta + (\epsilon_{it} - \epsilon_{it-1})$
- These transformations will provide more or less the same results.

Fixed effects vs. Difference-in-differences

- Difference-in-differences is an application of the fixed effects model where:
  - i often refers to more aggregate groups
  - Units in the treatment group start being exposed to the treatment at time t (i.e.: a new law is implemented in a certain region, but not in the control regions)
- The difference-in-differences framework helps us to think much more carefully about identification issues.

Famous example: Card and Krueger (1994)

Effect of Minimum wages on employment

- Theory:
  - In a competitive model the result of increasing the minimum wage is to reduce employment.
  - However, in a monopsonistic model an increase in minimum wages can actually increase employment.
- On April 1, 1992, New Jersey raised the state minimum wage from $4.25 to $5.05, whereas in the bordering state of Pennsylvania the minimum wage stayed at $4.25 throughout this period.
- Card and Krueger (1994) evaluated the effect of this change on the employment of low-wage workers.
- They conducted a survey to some 400 fast-food restaurants from the two states just before the NJ reform, and a second survey to the same outlets 7-8 months after.

Treatment and Control Locations

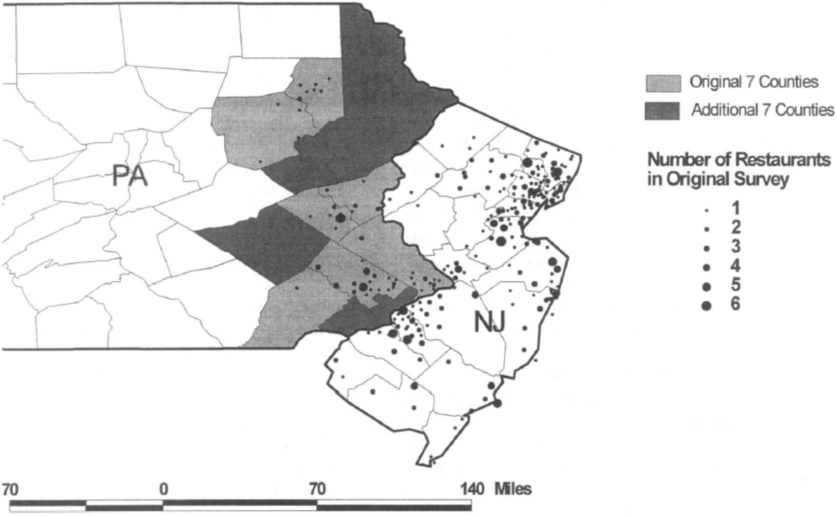


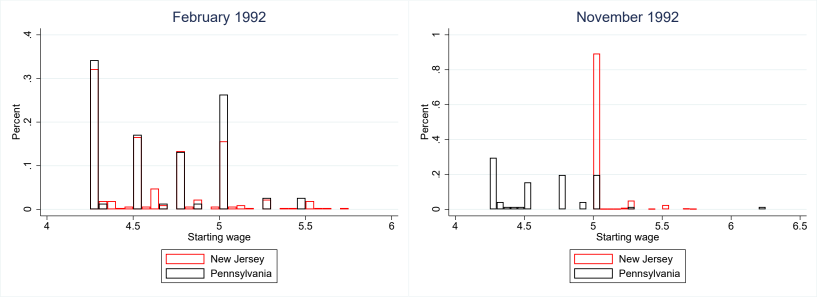
FIGURE 1. AREAS OF NEW JERSEY AND PENNSYLVANIA COVERED BY ORIGINAL SURVEY AND BLS DATA

Famous example: Card and Krueger (1994)

Why fast-food restaurants?

1. A large source of employment for low-wage workers.
2. They comply with minimum wage regulations (especially franchised restaurants).
3. Fairly homogeneous job, so good measures of employment and wages can be obtained.
4. Not difficult to collect data.
   - Response rates 91% in NJ and 73% in Pennsylvania (less in Penn., because the interviewer was less persistent).

Distribution of wage rates, before and after



Famous example: Card and Krueger (1994)

- Treatment group: Fast-food restaurants in New Jersey ($i = NJ$)
- Control group: Fast-food restaurants in Pennsylvania ($i = PA$)
- Denote the period before April 1992 with $t = 0$ and period after April 1992 with $t = 1$
- Let D_{it} be a dummy that takes the value 1 for the states that increase the minimum wage (NJ) and 0 otherwise (PA)
- At period $t = 0$, $D_{NJ,0} = D_{PA,0} = 0$
- At period $t = 1$, $D_{NJ,1} = 1$ and $D_{PA,1} = 0$

Famous example: Card and Krueger (1994)

- Write employment in state i at period t as:

$$L_{it} = \alpha_i + \lambda_t + \rho D_{it} + \epsilon_{it}$$

- Now:

$$\begin{aligned} E(L_{PA,0}) &= \alpha_{PA} + \lambda_0 \\ E(L_{NJ,0}) &= \alpha_{NJ} + \lambda_0 \\ E(L_{PA,1}) &= \alpha_{PA} + \lambda_1 \\ E(L_{NJ,1}) &= \alpha_{NJ} + \lambda_1 + \rho \end{aligned}$$

- The difference-in-differences estimator of ρ is:

$$\rho = [E(L_{NJ,1}) - E(L_{NJ,0})] - [E(L_{PA,1}) - E(L_{PA,0})]$$

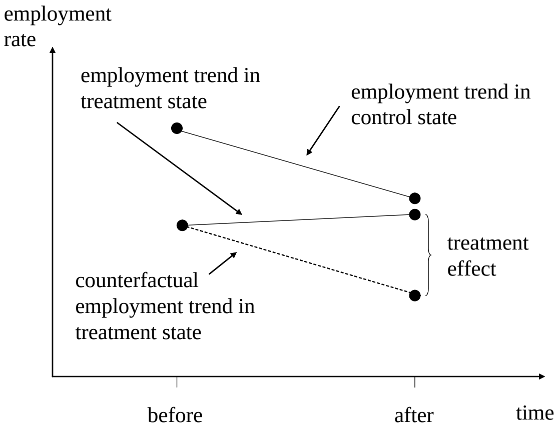


Figure 5.2.1: Causal effects in the differences-in-differences model

Table 5.2.1: Average employment per store before and after the New Jersey minimum wage increase

Variable		PA (i)	NJ (ii)	Difference, NJ-PA (iii)
1.	FTE employment before,	23.33	20.44	-2.89
	all available observations	(1.35)	(0.51)	(1.44)
2.	FTE employment after,	21.17	21.03	-0.14
	all available observations	(0.94)	(0.52)	(1.07)
3.	Change in mean FTE	-2.16	0.59	2.76
	employment	(1.25)	(0.54)	(1.36)

Notes: Adapted from Card and Krueger (1994), Table 3. The table reports average full-time equivalent (FTE) employment at restaurants in Pennsylvania and New Jersey before and after a minimum wage increase in New Jersey. The sample consists of all stores with data on employment. Employment at six closed stores is set to zero. Employment at four temporarily closed stores is treated as missing. Standard errors are reported in parentheses

I. Semmelweis

Suppose cadaverous particles adhering to hands caused the same disease among maternity patients that cadaverous particles adhering to the knife caused in Kolletschka. Then if those particles are destroyed chemically, so that in examinations patients are touched by fingers but not by cadaverous particles, the disease must be reduced. Semmelweis, I. quoted in Kadar (2019)