ALMP
Retirement and Aging

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Study Materials and Reading List

- Slides of the lectures
- All materials provided on: http://home.cerge-ei.cz/pytlikova/LaborSpring16/

Readings:

Cahuc, Carcillo and Zylberberg, Labor Economics, Chapter 14, Active labor market policies.

Tito Boeri and Jan Van Ours (2008): *Economics of Imperfect Labor Markets*. Princeton University Press. Chapter 6: Retirement Programs, pp. 121-137.

OUTLINE

- Shortly about the exam

- ALMP - international perspective
- Evaluating labor market policies
- Retirement plans and aging

Exam

- Written
- Thursday 31.3.2016, 10.30-12
- Forms of questions

Two forms of questions: essay question

- **Question #2 [20 minutes]**:

There was a large increase in income inequality in the Central and Eastern European (CEE) and the Commonwealth of Independent States (CIS) countries during and after their economic transition. Was it mostly a positive or negative phenomenon? What could be the driving forces behind the observed rise in wage inequality in the CEE and CIS countries? Discuss.

Two forms of questions: False/True/Uncertain essay questions

Question #3 [15 minutes]: Answer the following questions as True, False or Uncertain and briefly explain your reasoning, use diagrams if needed:

- a) According to the Borjas (1987) selectivity theory, for workers who immigrate to the United States from a country with a less equal distribution of earnings the largest potential gain exists for unskilled workers. Thus there is a higher probability of positive selection of migrants from that country to the U.S.
- b) In general, women in low-paying jobs tend to earn a lower percentage of male earnings (for those males in low-paying jobs) than women in high paying jobs.
- c) In the model where employers discriminate against females, those employers that devalue the productivity of the females more than other employers will earn a lower profit.

ALMP – the purpose

- Active labor market policies aim at improving the situation, in terms of employment and earnings, of the unemployed and of disadvantaged population
  - The strict sense of "active labor market programs" (ALMPs) in the OECD and other international databases, includes only policy measures that are *targeted* at particular groups to help them find jobs
- Passive policies aim at increasing the well being of these groups without automatically pursuing any particular outcome
  - Passive measures are limited to cash benefits, including unemployment benefit, social assistance, and early retirement program

ALMP – the purpose

Public employment services

- Public employment services aim at reducing job search costs
- Public employment services promote matches between firms with vacant jobs and persons looking for it. These could take the form of public agencies or/and private organizations
  - US Employment and Training Administration
  - Pôle Emploi in France
  - Bundesagentur fur Arbeit in Germany
- Among their activities, it is *Job Search Assistance (JSA)* that falls into the category of active labor market policy
- They help in drafting unemployed people's resumes, in defining personalized search strategies and putting them in contact with potential employers

ALMP – the purpose

Labor market training

- Labor market training represents the bulk of active policy
  - The prevalent form of labor market training is *classroom training (CT)*
  - Duration of the program varies across countries and groups
- Apprenticeship represents a large part of training measures aimed specifically at the young in most countries
  - It includes classroom instruction and on-the-job training
- The goal of such *on-the-job training (OJT)* programs is to give employers an incentive (through subsidies) to give directly training to disadvantaged categories of workers

ALMP – the purpose

Subsidized employment

- Subsidized employment covers a wide range of measures
- Subsidies for employment in the private sector generally take the form of transfers to firms that hire members of particular groups
- *Public-service employment* as an active policy measure is the direct creation of jobs in the public sector and is addressed in principle to the young and to the long-term unemployed

ALMP – the purpose

- It is important to distinguish *temporary* public jobs created as part of an active labor market policy, from a general public-sector policy, which consists of creating *permanent* civil service jobs
- Another point is that the same individual may benefit from several of these measures at the same time, for public policy is often structured around programs with several stages (multi-faceted programs)

ALMP – public expenditure on LMP, 2002-2011

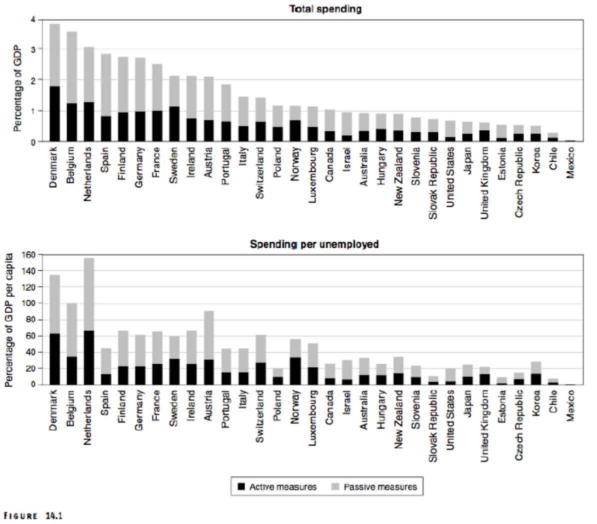


FIGURE 14.1
Spending on active and passive labor market programs in the OECD countries; average over the 2002–2011 period.

Note: In the bottom panel total spending in $PPP is divided by the number of unemployed, and the resulting spending per unemployed per person is then divided by the GDP per capita in $PPP.

Source: OECD Labor Market Programs database.

ALMP – public expenditure on ALMP, 2002-2011

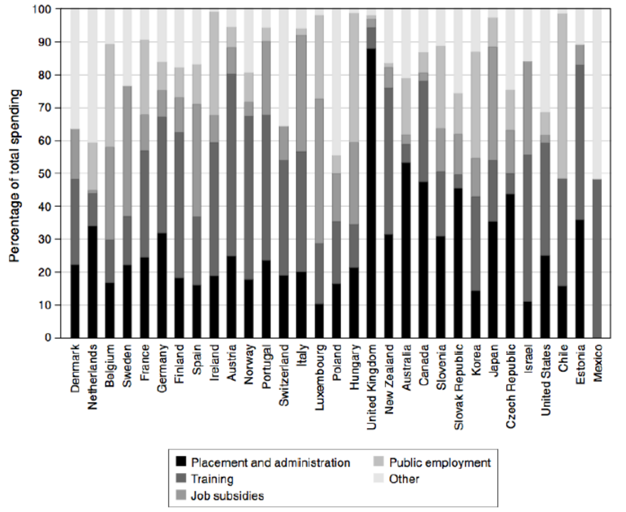


FIGURE 14.2
Breakdown of active spending by category in the OECD countries, average over the 2002–2011 period.

Note: Countries are ordered according to their level of total spending on active labor market programs (descending order).

Source: OECD Labor Market Programs database

ALMP – public expenditure on LMP, 1985-2011

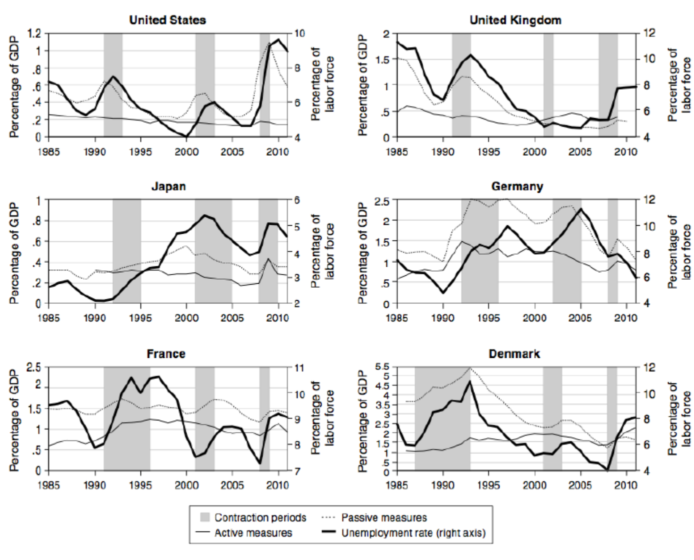


FIGURE 14.3
Spending on labor market programs and the economic cycle.

Source: OECD Labor Market Programs and Economic Outlook databases.

ALMP – public expenditure on LMP, 1985-2011, changes over business cycle

- Figure 14.3 shows that passive measures are strongly correlated with the rate of unemployment: they play as automatic stabilizers
- Benefiting from an active program most often depends on the decision of the case worker at the public employment service, and of availability of funds to finance the intervention
- The corresponding budgets are often discretionary and subject to the vote of parliaments
  - As a result, contrary to passive ones, total spending on active measure increases only very little with unemployment
  - Thus, the level of spending per unemployed person tends to decrease when unemployment rises

ALMP – correlation between unmpl and ALMP spending in 31 countries, 2002-2011

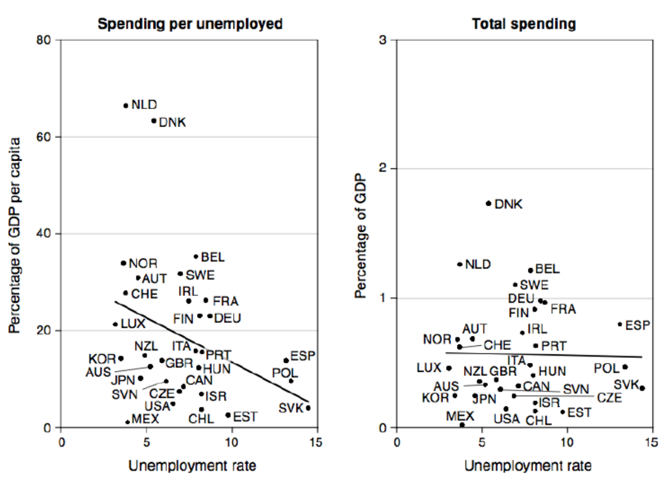


Figure 14.4
Spending on active labor market programs and unemployment, averages over the 2002–2011 period.

Note: In the left panel total spending in \$PPP is divided by the number of unemployed, and the resulting spending per unemployed per person is then divided by the GDP per capita in \$PPP.

Source: OECD Labor Market Programs and Labor Force Statistics databases.

ALMP – correlation between unmpl and ALMP spending in 31 countries, 2002-2011

- Figure 14.4 shows cross-country correlations between the unemployment rate and two measurements of spending on active measures
  - There is no correlation between total spending and unemployment
  - However, there is a negative correlation between spending per unemployed person and the unemployment rate
- This mere correlation does not mean that active measures reduce unemployment:
  - Unemployment might cause spending as much as spending might cause unemployment (*reverse causality*)
  - Spending and unemployment might both be the result of other factors, such as the institutions in the labor market and the quality of other public policies (*variable omission*)
  - Spending per unemployed person is mechanically lower when unemployment is high (*simultaneity*)

ALMP spending and institutions spending

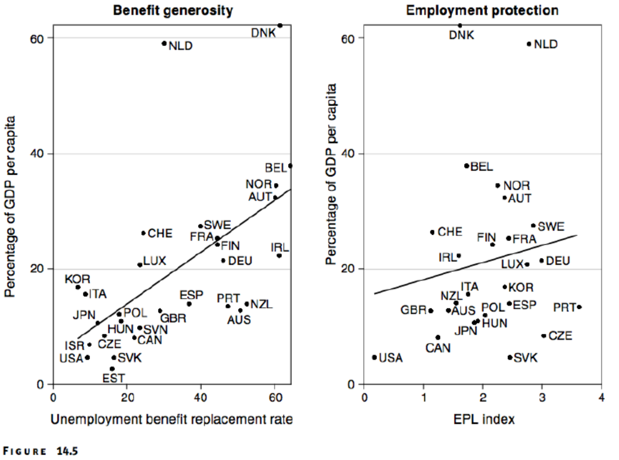


Figure 14.5
Spending on active programs per unemployed person and insurance systems in the labor market, averages over the period 2005–2010.

Notes: The replacement rate of unemployment benefits is the average ratio of net benefits to net income over a period of 5 years of unemployment based on the rules of benefits and tax systems. EPL index is the OECD index of the stringency of employment protection legislation (see chapter 13, section 2). Spending per unemployed person as a percentage of GDP per capita is defined as in figure 14.4.

Source: OECD Labor Market Programs and Employment Protection databases.

ALMP spending and institutions spending

Actually, some institutional features of the labor market are also positively correlated with spending on active measures

- As figure 14.5 shows, countries where the net replacement rate of unemployment benefit is high also feature high spending per unemployed person
- Spending per unemployed person is also positively correlated across countries with the strictness of employment protection
- This correlation would be even stronger if only job subsidies in the private sector were considered

ALMP – differences between countries

The United States

- The American active employment policy targets economically disadvantaged groups, and the beneficiaries are often defined with reference to a poverty threshold
- Programs, which aim to increase labor demand, are the exception in the United States. Most of the active policy measures are "supply-side" that aim to increase the human capital
- Another major item of active policy expenditure is job search assistance

ALMP – differences between countries

Sweden

- The "Swedish model" combines:
  - Gain for competitiveness in international trade with a wage policy indexed to productivity growth
  - An active employment policy favoring mobility of labor
- It gains a new objective of combating unemployment after the first oil shock (1973)
- Since the 1990s, active policy has privileged labor market training and subsidized employment, especially for young people and the long-term unemployed
- Since 2007, the unemployment benefit was cut back, benefit payments were made degressive and were capped so as to favor the return to work

ALMP – differences between countries

The United Kingdom

- The Thatcher government progressively abandoned all the measures supporting demand, in favor of "supply-side" policies
- Active employment policy mainly focuses on unskilled youth and on job searching
- Participation in active programs become compulsory after a few months of unemployment
  - If the unemployed person does not find a job during this phase, the program provides several options, including the possibility of offering a subsidy to potential employers, and enrollment in a full-time training course

Evaluating labor market policies

- Most of the recent empirical research is based on measuring the impact of policies on individual agents
- However, this impact is not observed directly, because the counterfactual situation is not observed by the researcher
- The evaluation of labor market policies is grounded in the notion of potential gain, which represents for a given indicator the difference in the presence and in the absence of the policy measure
- But, this approach may be flawed with selection bias and the difficulty to account for externalities

Evaluating labor market policies, challenges: selection bias and externalities

- Every labor market policy has a precise goal referred to as the individual's *response*
- To asses the efficiency of a policy, we have to compare the *same* wage that an individual will receive with and without the benefit of policy
- Hence, the main question is: how would a person or a firm who has benefited from a measure have responded if they had not benefited?
- This approach is based on the notion of "*potential* outcome"

Evaluating labor market policies, challenges: selection bias and externalities

- t_p denotes the period over which the "treatment" is applied
- y_{it}^1 represents the response of a treated agent
- y_{it}^0 the response of a non treated agent
- Results of these responses are *potential* and mutually exclusive:
  - We never observe *simultaneously* the realizations of these variables for the same individual
- We never observe the realizations the "counterfactual outcome". For a treated person i, the counterfactual outcomes correspond to realizations of y_{it}^0, whereas for an untreated agent j, they correspond to realizations of y_{jt}^1.

Evaluating labor market policies, challenges: selection bias and externalities

- The efficiency of a measure is generally assessed with the help of a *contrast variable*. Usually the ATT, the *average effect of the treatment on the treated* is adopted, defined by:

$$\mathbb{E}\,(\Delta \,|\, \delta = 1) = \mathbb{E}(y^1 \,|\, \delta = 1) - \mathbb{E}(y^0 \,|\, \delta = 1)$$

  - To assess the average gain, we have to make an *identifying assumption* to estimate the expected value of the counterfactual outcome
  - To do so, we must link unobserved responses to observed ones
  - The difficulty is to find a group of individuals, the control group, who have not undergone the treatment and are as nearly identical to the threated group as possible
- Policy measures can also be judged with the help of other contrast variables, lie the average treatment effect (ATE)

Evaluation based on controlled experiments

- The identification of a counterfactual outcome is often a difficult task
  - Therefore, controlled experiments are often regarded as the gold standard of policy evaluation
  - In that case here is no selection bias: those who are treated did not self-select but are chosen at random
- To assess the benefits of a given program, individuals are selected at random to be part of two groups - "control" and "treatment"
  - Randomization entails that observed and unobserved characteristics will be identical within each group
  - Thus, in principle, the difference in the outcome between the two groups stems only from the program

Evaluation based on controlled experiments, an example of randomization

Labor market programs can entail equilibrium effects (e.g. lower unemployment exit rates for the non-treated) that may lead to overestimate their impact.

The experiment of Crepon et al. (2013)

- In this experiment, private agencies provide intensive placement services to young graduates selected at random
- They were paid partially on delivery and conditionally on the individual finding a job with a contract of at least six months and staying employed for at least six months

Evaluation based on controlled experiments, an example of randomization

The identifying assumptions

- The first step consists in measuring the gain from the program
- The average treatment effects on the treated (ATT) is:

$$\mathbb{E}\left(y_A^1 - y_A^0 \,|\, \mathbf{X}, \delta = 1\right)$$

  - $\mathbf{X}$ is the set of observable characteristics
  - δ denotes a dummy which equals 1 if the individual is assigned to participate in program
  - y_A^0 and y_A^1 are the outcomes among participants when not-treated and treated respectively, after the program has taken place
- The problem is that y_A^0 is unobserved because the same participants cannot have been both treated and untreated. So, the strategy is to use the outcome observed for nonparticipants after the program was implemented
- However, this creates a problem of *causal inference*

Evaluation based on controlled experiments, an example of randomization

- Two identifying assumptions are required to observe the outcomes of nontreated individuals when they are not assigned
  - *Condition independence assumption (CIA)*: Nonparticipants are "comparable" to participants, i.e. no selection bias
  - *Stable unit treatment value assumption (SUTVA)*: Nonparticipants are not affected in any manner by the existence of the program i.e. no equilibrium effects
- The first assumption entails:

$$\mathbb{E}\left(y_A^0 \,|\, \mathbf{X}, \delta = 1\right) = \mathbb{E}\left(y_A^0 \,|\, \mathbf{X}, \delta = 0\right)$$

Evaluation based on controlled experiments, an example of randomization

The identifying assumptions

- The CIA implies that participation is unrelated to what individuals would earn in the absence of the program
  - Hence, observing the outcomes of the nontreated individuals determines exactly what would have happened to the treated individuals if they had not participated in the program
- The SUTVA rules out cases where the treatment of one individual affects another's outcome
  - However, there are many reasons to suspect the existence of such effects. If they do exist, the ATT estimator will be biased

Evaluation based on controlled experiments, an example of randomization

The Cross-Section Estimator

- The cross-section estimator of the average gain from the program is:

$$\tilde{\Delta}_{CS} = \overline{y}_A^T - \overline{y}_A^C$$

  - $\overline{y}_A^T$ and $\overline{y}_A^C$ are the average observed outcomes among individuals respectively assigned and not assigned to participate *after* the program has taken place
- There is no need for a time dimension to identify the average gain in the case of randomized (i.e. controlled) experiment, because it is not necessary to net out potential differences in outcome across groups absent the program

Evaluation based on controlled experiments, an example of randomization

A "naive" estimation (Crépon et al. (2013))

- In this estimation, we ignore that the treatment of an individual might affect another's outcome
- One can simply estimate the following equation using the OLS:

$$y_i = \alpha_1 + \beta_1 \delta_i + \mathbf{X}_i \gamma + \epsilon_i \quad (14.63)$$

  - δ_i takes values 1 if individual i is assigned to treatment group and 0 otherwise
  - y_i is a dummy taking a value of 1 if the person is under a fixed-term contract of six months or more (LTFC) or, alternatively, under any long-term job arrangement (fixed-term contract of more than six months or permanent contract, LT)

Evaluation based on controlled experiments, an example of randomization

But there is a difference between being assigned to participate and actually being treated:

- There is always some *non-take-up* who cannot be excluded from the sample of participants, because take-up is subject to selection bias
- Non-take-up limits the direct impact of program assignment on employment outcomes, and it also reduces the power of the identification strategy

Evaluation based on controlled experiments, an example of randomization, effect on long term fixed contracts (LTFC) and long term employment (LT)

- Table 2 shows the results
  - Participants assigned to treatment are only 0.7 percentage point more likely to have obtained a LTFC and 0.2 percentage point more likely to have a LT. But these estimates are not significant
  - If we consider only those who were not employed at the beginning of the study and for whom take-up was higher (about 43%): they were 1.7 percentage points more likely to have an LTFC and 1.5 percentage point more likely to have an LT if they were assigned to treatment than if they were not (column 4)

Evaluation based on controlled experiments, an example of randomization

Outcome	Variable	All participants	Not employed
LTFC	Assigned to treatment (β_1)	.007 (.005)	.017*** (.006)
	Control mean (α_1)	.20	.16
LT	Assigned to treatment (β_1)	.002 (.007)	.015 (.010)
	Control mean (α_1)	.47	.37
Obs.		21, 431	11, 806

Table 2: The impact of intensive job placement counseling on the employment outcomes of young educated workers, leaving out equilibrium effects.

Source: Crépon et al. (2013, Table III)

Note. The table reports ordinary least squares (OLS) regressions controlling for gender, education, past duration of unemployment and its square, cohort dummies, and 47 dummies for local area quintuplets. The dependent variables are employment outcomes when surveyed eight months after the random assignment: long-term fixed contracts (LTFC) are fixed-term contracts with a length of at least six months; long-term employment (LT) is either a long-term fixed contract or an indefinite-term contract. Column (2) restricts the sample to job seekers who did not report that they were employed at the time of randomization; column (3) restricts the sample to those who did. Standard errors in parentheses are robust to heteroskedasticity and clustered at the local area level. *** significant at the 1 percent level.

Evaluation based on controlled experiments, an example of randomization

- Equation (14.63) ignores the potential impact of the treatment on nonparticipants
- These externalities potentially bias the estimates
- Indeed, if nonparticipants are indirectly and negatively affected by the treatment of participants, then the observed difference between participants and nonparticipants reflects only in part the positive effect on employment, but also in part a negative effect on nonparticipants

Evaluation based on controlled experiments

- Randomization is not a solution since it cannot obviate economic and social interaction
- Another strategy is to make use of any heterogeneity in the size of treatment groups across areas
- Implanting this strategy requires the size of the treated group to be unrelated to the labor market outcome under study

Evaluation based on controlled experiments, an example of randomization

- To address this issue, Crépon et al. (2013) implemented a two-step randomization:
  1. In the first step, 234 local employment areas were assembled into 47 groups of 5 agencies. Each of 47 strata was randomly assigned to a group of a proportion p of job seekers to be treated (possible values are 0, 25, 50 or 100%)
  2. In the second step, the fraction p of all the eligible job seekers in the area was randomly selected to be assigned to treatment

Evaluation based on controlled experiments, an example of randomization

- We can compare individuals in these areas with those in other areas by estimating the following equation:

$$y_i = \alpha_2 + \beta_2 \delta_i \pi_i + \lambda_2 \pi_i + \mathbf{X}_i \gamma_2 + \varepsilon_i$$

  - π is a dummy for a person living in a local market with a positive fraction of individuals assigned to treatment (25, 50 or 100%)
  - $\mathbf{X}_i$ includes local area dummies on top of individual characteristics
  - λ_2 measures externalities
  - The sum $\beta_2 + \lambda_2$ is the effect of being assigned to treatment compared to being in an entirely unaffected labor market

Evaluation based on controlled experiments, an example of randomization

Outcome	Variable	Not employed	Not employed Men	Not employed Women
LTFC	Assigned to treatment (β_2)	.023***	.043***	.013
		(.008)	(.013)	(.010)
	In a program area (λ_2)	−.013	−.036***	−.001
		(.009)	(.013)	(.012)
	Net effect of program assignment ($\beta_2 + \lambda_2$)	.010	.007	.012
		(.008)	(.011)	(.011)
	Control mean (α_2)	.16	.131	.177
LT	Assigned to treatment (β_2)	.025**	.037**	.019
		(.012)	(.018)	(.014)
	In a program area (λ_2)	−.021*	−.043**	−.010
		(.013)	(.020)	(.018)
	Net effect of program assignment ($\beta_2 + \lambda_2$)	.003	−.006	.009
		(.011)	(.018)	(.016)
	Control mean (α_2)	.365	.372	.36
Obs.		11,806	4,387	7,419

Table 3: The impact of intensive job placement counseling on the employment outcomes of young educated workers, with equilibrium effects incorporated.

Source: Crépon et al. (2013, Table V). Note. The table reports ordinary least squares (OLS) regressions controlling for gender, education, past duration of unemployment and its square, cohort dummies, and 47 dummies for local area quintuplets. Column (2) restricts the sample to job seekers who did not report that they were employed at the time of randomization; column (3) restricts the sample to those who did.

Evaluation based on controlled experiments, an example of randomization

Table 3 shows the results for those not employed at the time of the assignment.

- Those assigned to treatment are 2.3 percentage points more likely to have an LTFC than those assigned to control status in the treatment labor markets. This gross effect of the program is of roughly the same order of magnitude as in table 14.2
- But the net effect of program assignment is not significant anymore. Column 4 and 5 show that both the gross effect of treatment and the externalities are stronger and more significant for men than for women. For men equilibrium effects almost fully offset the gross effect of treatment

Evaluation based on controlled experiments, an example of randomization

- These results indicate that a part of the program effects measured with the naive approach, in which equilibrium effects were left out of account, were due to an improvement in the search ability of some workers, which reduced the relative job search success of others
- All in all, this study shows that it is indeed important to account for equilibrium effects when evaluating labor market policies
- In principle, social experiments constitute the most convincing approach to evaluate the impact of labor market policies since selection bias is obviated
- But social experiments are difficult and costly to implement

Evaluation based on controlled experiments, an example of randomization

- Policies are often implemented under pressure and government often cannot wait for the results of experiments: a lot of programs are not designed ex-ante for being evaluated
- Besides, the fact that some individuals are randomly selected to participate in a pilot social program and others randomly excluded, can sometimes be difficult to accept
- In practice, a number of labor market programs adopt not just one type of intervention but an array of interventions to help workers find a job
- So a lot of evaluations of ALMPs are rather based on pre-existing programs that are often very specific

Evaluation based on observational data, an example of Diff in Diff

One example is the program of New Deal for Young People (NDYP) introduced in the United Kingdom in 1998

- The program comprised:
  - A first stage of four months called "Gateway", which included intensive counseling with interviews and some courses
  - A second stage, four options were offered: a voucher for subsidized employment in the private section, a period of paid training or full-time education, work in the voluntary sector or a job on the "Environmental Task Force"
- However, the selected areas as well as the participants were not chosen at random
- The researcher has to make these areas comparable to those where the pilot was not implemented, in order to build a control group

Evaluation based on observational data, an example of Diff in Diff

The difference-in-differences estimator

- The conditional independence assumption in the case of the difference-in-differences is often called the "common trend assumption", which means that the trends that may affect the results of participants and nonparticipants are identical, it reads:

$$\mathbb{E}\left(y_A^0 \,|\, \mathbf{X}, \delta = 1\right) = \mathbb{E}\left(y_B^0 \,|\, \mathbf{X}, \delta = 1\right) + m_t$$

- m_t is the spontaneous growth of the outcome variable
- This common trend assumption says that participants $(\delta = 1)$, if they had not been treated (0), would have had the same increase or decrease in outcome as the one observed for the nonparticipants (m_t)
- This also assumes that there are no externalities specifically influencing individuals in the control group

Evaluation based on observational data, an example of Diff in Diff

The difference-in-differences estimator

- In practice, this common trend assumption requires to check whether or not, the average outcome under consideration evolved in the same manner for participants and nonparticipants in periods before the measure was implemented
- It also requires to check that from the date of the program's commencement, no other factor could have generated any divergence within the average outcomes among participants and nonparticipants apart from the program itself
- *For the nonparticipants in the program*, the difference-in-differences (DD) estimator is:

$$\tilde{\Delta}_{DD} = \left(\overline{y}_A^T - \overline{y}_B^T\right) - \left(\overline{y}_A^C - \overline{y}_B^C\right)$$

Evaluation based on observational data, an example of Diff in Diff

The difference-in-differences estimator

- In principle, the DD estimator has the advantage of being insensitive to changes in the global state of the economy that affects the control group and the treatment group uniformly
- The common trend assumption may not be fulfilled if other policies or institutional changes influencing employment could impact the treatment and the control groups differently
- Ashenfelter's "dip" is another example, which may imply that the common trend assumption is not satisfied. He observed that the wages of participants in a training program had a tendency to fall off in the period just before they entered the program

Evaluation based on observational data, an example of Diff in Diff

Treatment effects and the search for externalities

- Going back to the New Deal for Young People, Blundell et al. (2004) use two eligible criteria, areas and age, to define the control and treatment groups
- In the first specification, they compare the treated to a control group of 19-24 years old people (same age as the treated) not living in the pilot areas. They identify the effect of the treatment on the treated, i.e. a *net* effect (the effect is positive)
- In the second one, Blundell et al. (2004) compare the treated to a control group made of 25-30 years old people (not eligible) of the same unemployment duration and living in the pilot areas. The estimated effect might be biased due to externalities. But the authors find a similar effect

Evaluation based on observational data, an example of Diff in Diff

The difference-in-differences estimator

- In the third specification, they compare the nontreated in a pilot area to young individuals of the same age not living in any of the pilot areas. This is a measure of externalities. They find no externalities
- In the fourth one, all young long-term unemployed (19-30 year-old) in a pilot area are compared to other young people not living in these areas. The positive effect was observed in the "whole" youth labor market. The estimated effect is smaller since part of the 19-30 group is not eligible
- These results are summarized in table 4 for young men
- Blundell et al. (2004) run the same regression on women but do not find any significant impact

Evaluation based on observational data, an example of Diff in Diff

Treatment group	Control group	Number of Observations	Effect on Employment Probability (β)
19-24-year-olds living in treatment areas	19-24-year-olds living in all control areas	3,716	.110** (.039)
19-24-year-olds living in treatment areas	25–30-year-olds living in treatment areas	1,096	.104* (.055)
25–30-year-olds living in treatment areas	25–30-year-olds living in matched control areas	983	.055 (.058)
19–30-year-olds living in treatment areas	19-30-year-olds living in all control areas	6.896	.066** (.029)

Table 4: The impact on employment of intensive job placement assistance for young and long-term unemployed men at the tenth month after starting an unemployment spell.
Source: Blundell et al. (2004, Table 1).
Note. The table reports ordinary least squares (OLS) regressions controlling for marital status, sought occupation, region, age, and labor market history (number of unemployment spells). The dependent variable is whether an individual has left unemployment between the sixth and the eighth months of an unemployment spell, among individuals having completed a six-month spell of unemployment which began over a predefined time interval. Standard errors in parentheses. ** significant at the 5 percent level, * at the 10 percent level.

Evaluation based on observational data, matching

The common trend assumption may not be satisfied if the labor market policy is heterogeneous with respect to some observable characteristics.

The technique of matching

- Another approach, named *matching*, consists of extracting from the sample a control and a treated group of individuals *similar* on the basis of *observable* characteristics
- Researchers, thus, try to set a single match for each person in the treated group. For an individual in the treated group, the researcher picks out the corresponding individual who is in closest proximity to i and assigns $h(i)$ to the control group based on observable characteristics that are potentially important for the outcome at stake

Evaluation based on observational data, matching

The technique of the propensity score

- Another technique, called *propensity score matching*, uses the propensity score, $p(\mathbf{X}) = \mathbb{P}(\delta = 1|\mathbf{X})$, which is the probability for an individual to be a participant conditional on his observed characteristics
- The propensity score can be estimated in a first stage with a logit or a probit model. To do so, we regress the dummy of treatment on the vector of covariates that determines selection into the treatment: $\delta_i = \alpha + \mathbf{X}_i\boldsymbol{\beta} + \varepsilon_i$, where ε_i is an error term

Evaluation based on observational data, matching

The limits of the matching techniques

- Under the conditional independence assumption, matching two groups allows to employ a simple cross-section estimator to assess the impact of the policy measure. This estimation is only possible if no unobservable characteristics influence the result
- The matching method does have one major limitation: it is necessary to have an overlap between the two groups in terms of characteristics. This method can only measure the effect of the treatment within the common support region

Evaluation based on observational data, matching

Regressions or matching?

- Matching defines cells of observations in which individuals have homogeneous characteristics: a specific treatment and control are weighted to produce an overall average treatment effect
- This matching approach is more flexible than the regression one since it allows variations of the effect of the policy across cells
- But regressions that include covariates can also be viewed as a species of matching with specific weights
- In sum, in practice, the regression, the matching and propensity score matching approaches should not produce major empirical divergences, especially with large samples

Evaluation based on observational data, duration analyses

Very detailed information on the duration of unemployment allows to use the "timing of events" in order to identify the causal impact of interventions.
Treatment and control groups may be defined time after time because not everybody enters the program at the same time.

Overview of the effects – results from meta-analysis

- The evaluation of postprogram effects has been a thriving field of research. Hence, it will be worth presenting meta-analyses which provide an overview of the results
- Meta-analysis methods assemble results from many studies in order to identify the impact of one type of program
- The method comes down to identifying a common measure of effect size and then building a weighted average across studies
- The most difficult task for meta-analyses is to define a standardized measure of program impact because the measurement of outcomes varies across studies

Overview of the effects – results from meta-analysis

- Despite the difficulty to build a standardized measure, Card et al. (2010) and Kluve (2010) managed to identify the the sign and significance of the program impact at three points in a large number of cases:
  1. A short-term impact at approximately one year after completion of the program
  2. A medium-term impact roughly 2 years after program completion
  3. A long-term impact roughly 3 years after program completion
- Then, they run an ordered probit model to fit the sign/significance of estimated program impact on a set of study characteristics
- The ordered probit is a generalization of the probit model to the case of more than two outcomes of the dependent variable

Overview of the effects – results from meta-analysis

	Overall Sample	Austria, Germany & Switzerland	Nordic Countries	Anglo Countries
Basic Methodology (%)				
Cross Sectional with Comparison Group	3.0	0.0	5.7	0.0
Longitudinal with Comparison Group	51.3	80.6	30.2	75.0
Duration Model with Comparison Group	36.2	19.4	43.4	0.0
Experimental Design	9.1	0.0	18.9	25.0
Covariate Adjustment Method (%)				
Matching	50.8	73.1	30.2	45.0
Regression	42.7	26.9	52.8	40.0

Table 5: Evaluation Methods Used in Program Effects Evaluations - 1995-2007.
Notes: percentage of estimates on a total of 199 estimates of treatment effect drawn from 99 studies.
Source: Card, Kluve and Weber (2010, table4 p. F461).

Overview of the effects – results from meta-analysis

	Short-term treatment effect		Medium-term treatment effect	
Type of program (omitted: Mixed and other)	Marginal effect	t-stat	Marginal effect	t-stat
Public job creation	.31	(0.67)	−.46	(−0.62)
Private sector subsidy	.14	(0.33)	.79	(0.86)
Placement	.72	(1.63)	1.16	(1.36)
Training	.22	(0.57)	1.14	(1.68)
Obs.	181		92	

Table 6: The effectiveness of labor market programs in Europe - 1983-2007.
Notes: Models are ordered profits. The dependent variable is a categorical variable indicating whether the estimate of the program effect is negative (-1), insignificant (0), or positive (+1). Controls include the square root of the size of the sample used, the duration of the program, the type of measurement in the study (e.g. time in registered unemployment, other type of duration, post program earning), the age and gender of participants when they are pooled, a dummy for experimental designs, dummies for the type of participants (registered unemployed, long-term unemployed, or any other disadvantaged group) and country group dummies (one for English speaking countries, one for Nordic Countries, one for Austria, Germany and Switzerland and one for the other countries represented in the sample). T- stats of the marginal effects are reported in adjacent columns based on standard errors (clustered by study).
Source: Card, Kluve and Weber (2010, tables 7 and 8, p. F468-F469).

Overview of the effects – results from meta-analysis

Type of program (omitted: training)	Negative treatment effect: Marginal effect at sample mean	t-stat	Positive treatment effect: Marginal effect at sample mean	t-stat
Public job creation	.17	(1.99)	−.25	(−2.25)
Private sector subsidy	−.15	(−4.00)	.31	(3.34)
Placement and sanctions	−.20	(−3.69)	.44	(4.29)
Young workers programs	.16	(2.19)	−.24	(−2.39)
Obs.	137			

Table 7: The effectiveness of labor market programs in Europe - 1983-2007.

Notes: The dependent variable is a categorical variable indicating whether the estimate of the program effect is negative (-1), insignificant (0), or positive (+1). Table entries document the marginal effects (evaluated at the sample mean) from the corresponding ordered probit regression for the negative and positive outcomes respectively, i.e. the difference in the predicted probability for achieving a negative (positive) treatment effect which arises from changing an indicator among the explanatory factors from 0 to 1. Controls include: the type of research design (experimental, etc.) and timing of study, labor market institutions, macroeconomic context (unemployment, GDP growth, ALMP spending) and country dummies. T-stats of the marginal effects are reported in adjacent columns. The underlying standard errors adjust for clustering by study.
Source:Kluve (2010, table 4, p. 911).

Overview of the effects – results from meta-analysis

Table 5 shows that controlled experiments are still relatively rare in the literature.

- Table 6 and 7 show the results of the meta-analyzes:
  - Job creations in the public sector are more often ineffective than other interventions and even appear detrimental, with negative treatment effects
  - Job search assistance has a favorable impact in the short run. Kluve (2012) finds that evaluations of placement assistance and private sector subsidies come up with higher effects than do training programs
  - Long-run impacts are generally more often positive and strong than short-run impact
  - The context of programs matters less than their nature when it comes to explaining their effectiveness
  - Evaluations that measure outcomes based on time spent in registered employment show more positive short-term results than evaluations based on employment or earnings outcomes

Overview of the effects – results from meta-analysis

Summary

- Among labor market policies, a distinction is made between:
  - *Active policy* measures, which aim to improve the functioning of the labor market, and
  - *Passive policy* measures, which seek instead to improve the living conditions of workers
- Public agencies occupy an important place in the array of institutions that manage job offers in many countries. From the social optimum, placement agencies are only justified if they guarantee a better matching of unemployed persons than the "natural" process would, and if operating them does not incur excessively high fixed costs

Summary

- Employment subsidies in the form of reduced labor costs for the employer generate upward pressure on the negotiated wages
- When unemployment benefits are perfectly indexed to wages, the employee captures the *whole* subsidy, and at equilibrium subsidies have no effect on employment. Conversely, when unemployment benefits are imperfectly indexed or wages are rigid, employment subsidies reduce the unemployment rate
- All labor market policies may exert externalities on non-participants and thus lead to equilibrium effects that may diminish or enhance the total effect of the measures on employment and wages.
- Subsidies offered to some firms to hire disadvantaged workers or create certain types of jobs may put pressure on wages, which in turn can weigh down job creation among firms who cannot benefit from such subsidies but still compete on the same market

Summary

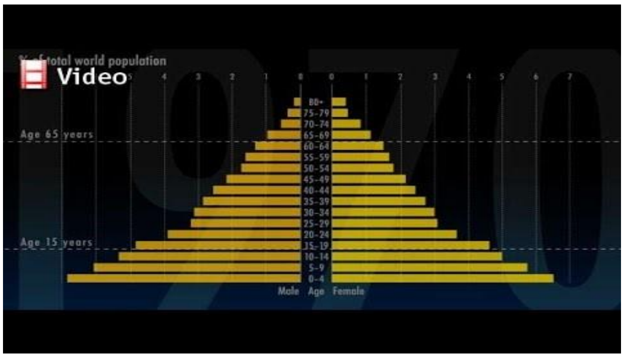
- To evaluate the impact of employment policies, we must compare the performances of the individuals who benefit from measures with those of individuals who do not.
- This kind of assessment poses problems, since the characteristics of the individuals who do benefit from employment policies creates a potential selection bias
- It is possible to deal with this problem by assessing the performance of policies for groups of individuals possessing identical characteristics (the matching method). The existence of unobserved characteristics nevertheless constitutes an unavoidable limitation on this type of approach
- Social experiments, which consist of choosing the beneficiaries of employment policies at random within the guidelines and comparing their performances with those of non-beneficiaries, make it possible to deal with this problem
- Empirical assessments suggest that non-targeted employment subsidies, or the creation of public-sector jobs, are costly measures that should only find marginal applications

Summary

- The appraisal of active employment policies yields mixed results. Studies carried out in the United States conclude that only adult, economically disadvantaged women appear to derive any real benefit, for an acceptable cost, from measures to promote training. Overall, job search assistance is the least costly of the active policies, and probably one of the most effective
- Some programs of job subsidies in the private sector may also enhance the chance of employment. Evaluations of temporary public employment creation programs lead to insignificant or even negative effects on the chances of holding a job in the regular labor market at the end of the program
- There is also evidence that the threat of having to enter mandatory programs, with the presence of sanctions against half-hearted job searches, increase unemployment exits
- Finally, all empirical research dedicated to assessing employment policies generally neglects their macroeconomic effects.

Retirement plans and aging

Demographic changes



https://www.youtube.com/watch?v=QwfH1gYkXTw
http://www.economist.com/blogs/graphicdetail/2014/11/daily-chart-10
https://www.ined.fr/en/everything_about_population/videos/animation-population-pyramid/

Retirement programs

- Large-scale retirement a rather recent phenomenon: until beginning of the twentieth century, not many workers- retired, they worked as long as they could, and if they stopped working, the retirement involved often a few years of dependence on children.
- Today – extended period of financed independence and leisure
- Forced retirement – mandatory retirement age, early retirement programs (offers that cannot be refused)
- Defined benefit (DB)-defined contribution (DC) programs
- Comparison of retirement and early retirement age and incentives to retire across countries and time;

Table 8.6: Standard age and earliest age of entitlement to public old-age pensions; Pension replacement rate

	Retirement ages							
	Earliest Males		Standard Males		Standard Females		Pension replacement rate	
	1969	2003	1969	2003	1969	2003	Males	Females
Australia	65	55	65	65	60	62.5	56.4	56.4
Austria	65	65	65	65	60	60	90.9	90.9
Belgium	60	60	65	65	60	63	63.0	63.0
Canada	66	60	66	65	66	65	57.4	57.4
Czech Republic	–	58.5	–	61.5	–	59.5	64.4	64.4
Denmark	67	65	67	65	67	65	86.7	86.7
Finland	65	62	65	65	65	65	68.8	68.8
France	60	60	65	60	65	60	63.1	63.1
Germany	65	63	65	65	65	65	58.0	58.0
Greece	60	60	60	65	55	65	110.1	110.1
Hungary	–	62	–	62	–	62	102.2	102.2
Iceland	67	65	67	67	–	67	84.2	84.2
Ireland	70	65	67	66	70	66	38.5	38.5
Italy	55	57	60	65	55	65	77.9	63.4
Japan	60	60	65	65	65	65	39.2	39.2
Korea	–	55	–	60	–	60	71.8	71.8
Luxembourg	62	60	65	65	62	65	96.2	96.2
Mexico	–	65	–	65	–	65	38.3	31.7
Netherlands	65	60	65	65	65	65	96.8	96.8
New Zealand	60	65	65	65	65	65	41.7	41.7
Norway	70	67	70	67	70	67	69.3	69.3
Poland	–	65	–	65	–	60	74.9	55.2
Portugal	65	55	65	65	65	65	69.2	69.2
Slovak Republic	–	60	–	60	–	57	72.9	72.9
Spain	65	60	65	65	55	65	84.5	84.5
Sweden	63	61	67	65	67	65	64.0	65.0
Switzerland	65	63	65	65	62	63	64.3	64.3
Turkey	60	60	65	60	55	55	104.0	104.0
United Kingdom	65	65	65	65	60	60	41.1	41.1
United States	62	62	65	65	65	65	52.4	52.4

B&vO –Tab 6.1 pg 123

Retirement programs

- Age and Employment
  - Important role of early retirement and retirement programs and their reforms
  - Table 8.7
- Age and Productivity
  - Most employer (and also employees) believe in a rule of thumb that average labor productivity declines after an age between 40-50;
  - However, a large heterogeneity across workers, occupations/jobs
  - Research – mixed evidence, B&vanO pgs chapter 6.

Table 8.7: Average age of transition to inactivity

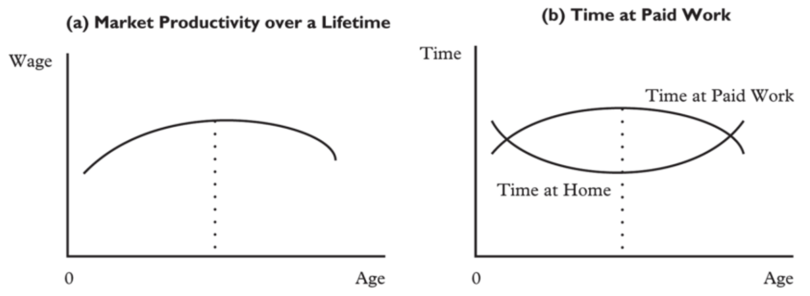
	Males			Females		
	1967	2002	Change	1967	2002	Change
Australia	67.3	63.2	−4.1	67.7	60.6	−7.1
Austria		59.6	–	–	–	–
Belgium	64.2	58.5	−5.7	63.5	56.8	−6.7
Canada	66.0	63.1	−2.9	61.4	61.4	−5.0
Denmark	–	65.3	–	–	62.1	–
Finland	69.3	60.8	−8.5	62.2	59.8	−2.4
France	67.3	59.3	−8.0	66.8	59.4	−7.4
Germany	–	60.9	–	–	60.2	–
Greece	68.1	62.4	−5.7	64.5	60.9	−3.6
Hungary	70.5	57.8	−12.7	69.5	56.0	−13.5
Iceland	–	69.5	–	–	67.8	–
Ireland	–	65.1	–	–	66.1	–
Italy	64.3	61.2	−3.1	59.6	60.5	0.9
Japan	72.2	69.6	−2.6	68.9	65.7	−3.2
Korea	67.1	68.0	0.9	65.5	66.7	1.2
Luxembourg	66.1	59.8	−6.3	66.1	59.8	−6.3
Netherlands	–	61.0	–	–	59.1	–
New Zealand	–	64.2	–	–	62.2	–
Norway	68.0	63.6	−4.4	69.2	62.3	−6.9
Poland	73.4	60.9	−12.5	72.4	58.8	−13.6
Portugal	–	65.8	–	–	63.5	–
Spain	–	61.6	–	–	61.3	–
Sweden	69.3	63.5	−5.8	67.0	62.0	−5.0
Switzerland	72.5	66.6	−5.9	73.0	63.2	−9.8
Turkey	77.3	62.5	−14.8	63.1	61.8	−1.3
United Kingdom	–	63.1	–	–	61.2	–
United States	69.9	65.0	−4.9	68.6	62.9	−5.7

B&vO –Tab 6.2 pg 126

Retirement programs

- Lifetime incomes are higher the longer workers put off retirement
- An increases in pension benefits reduces the price of retirement, increasing the demand for leisure, encouraging the worker to retire earlier
- If pension benefits are constant, wage increases have a substitution and income effect, so lifetime income may not be altered
- Option value – continue to work if expected present value of immediate retirement
- Option value of work:
  - Positive: continue to work
  - Negative: retire now

Life-cycle allocation of time



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EXAM 31.3.2016, 10.30-12.00

Active labour market policies; Unemployment benefits

Retirement and aging; Early retirement plans