Effect of Energy Audits on the Adoption of Energy-Efficiency Measures by Small Companies

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German Energy Audit Program for SMEs

In place since 2008

- Subsidize energy audits in SMEs (<250 employees & since 2013: energy costs > 5000 € p.a.)
  - Initial audit: <=2 days, subsidize 80% of audit fee (max: 640 € per day)
  - Detailed audit: <= 10 days, subsidize 60% of audit fee (max 480 € per day)
- > 25.000 audits by certified auditors

Objective of program

- *Increase adoption* of energy efficiency measures by reducing lack of information

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Insights from academic litererature

Information as a barrier in SMEs

- ECEEE (2014), Fleiter et al. (2012), Schleich and Gruber (2008), Schleich (2009), Anderson and Newell (2004)

Effectiveness of audits

- *Households*: Murphy (2014), Frondel et al. (2013), Hirst and Goelz (1983)
  - Mixed evidence
  - Review by Hirst (1981, p. 624):“the lack of control groups in all but two of the evaluation efforts,....., seriously impairs the validity of the conclusions.”
- *SMEs*
  - This paper!

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Methodological aspects – How to estimate Avarage Effect of Treatment on the Treated (ATT)?

Key problem

- To properly evaluate program effects, we need to know „counterfactual“, i.e. what would the organization have done without the audit?
- By definition, counterfactual is unobservable

Possible Solutions

- Ask participants: Would you have implemented this measure without (subsidized) audit?
  - Problem: bias
- Random program assignment to control and treatment groups
  - Mean outcome of organizations not participating in the program would be the counterfactual
  - Problems: costs, identification of control group, participation of control-group companies
- Non-random treatment and control groups

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One solution: Matching Estimators for non-random treatment and control groups

- General idea: for each subject in the treatment group find (more or less) identical twins (i.e. same characteristics **X**) in the control group (= the matches) and compare outcomes, i.e. average the differences over all possible pairs of matched subjects; (consistent, ~ asymptotically normal)

- Intuitively, matching “mimics“ randomization!

- Use “inexact“ matching estimators , i.e. find matches such that conditional on X the treatment is as good as randomly assigned

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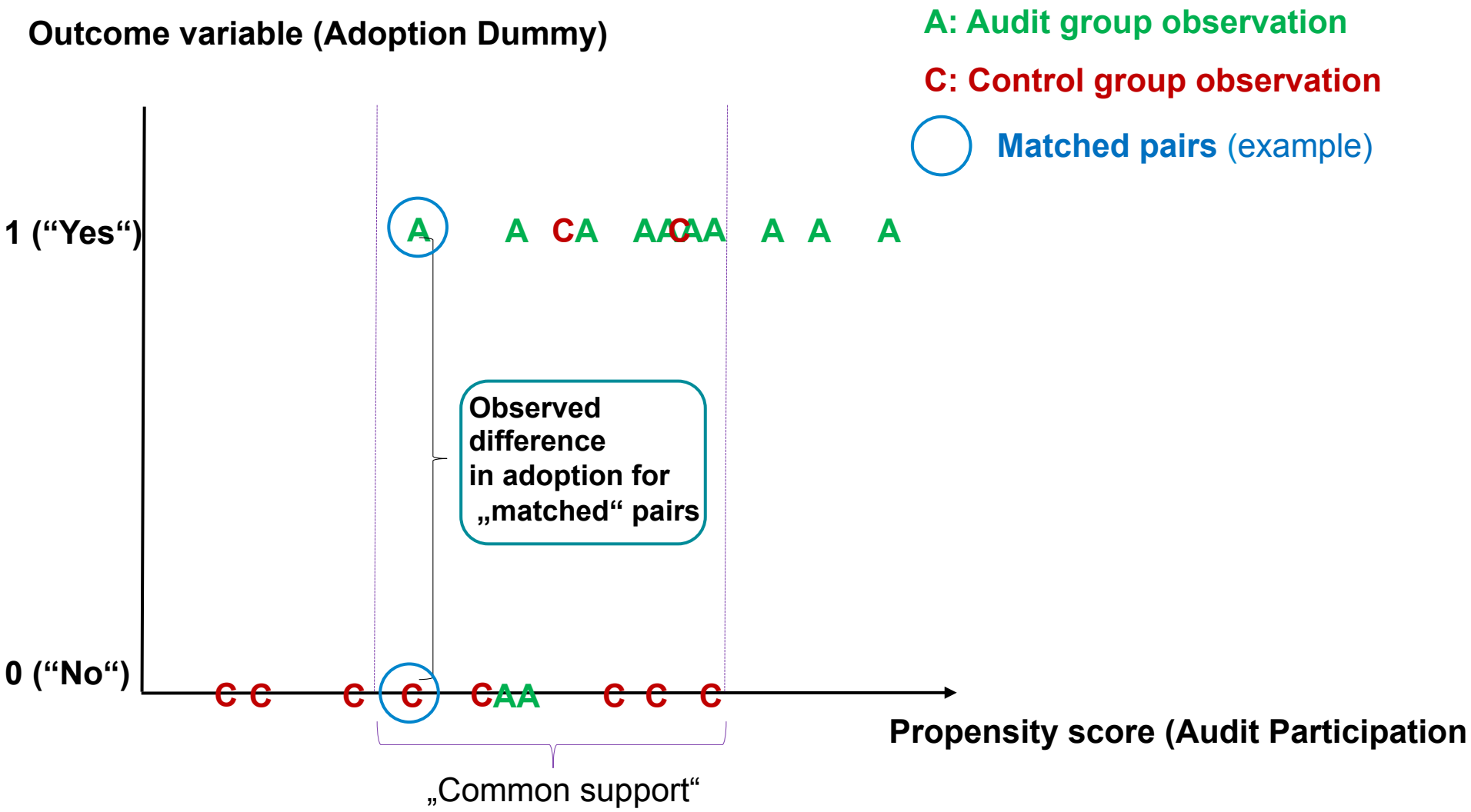
Propensity Score (PS) Matching Estimators (Rosenbaum and Rubin 1983)

- Step 1: Run a logit (or a probit) model which regresses program participation on a set of relevant covariates.
  - Then calculate predicted values from logit (or probit) to generate propensity score $p(x_i)$ for all subjects in the treatment and control groups
- Step 2: Use the propensity scores to identify subjects in the control group which best match the subjects in the treatment group
- ATT: estimated by calculating the difference between the share of adoptions in the audit group companies and their matches in the control group


Outcome variable (Adoption Dummy)
A: Audit group observation
C: Control group observation
Matched pairs (example)
1 ("Yes")
0 ("No")
Observed difference in adoption for „matched" pairs
Propensity score (Audit Participation
„Common support"

Data sets for control group and treatment group

1. Treatment group = treatment group (KfW Evaluation, Mai et al (2014))
2. Control group : from survey of „similar“ companies (Survey in German Tertiary Sector (GHD Sektor, Schlomann et al. 2014)), also includes small industrial enterprises (< 20 employees)

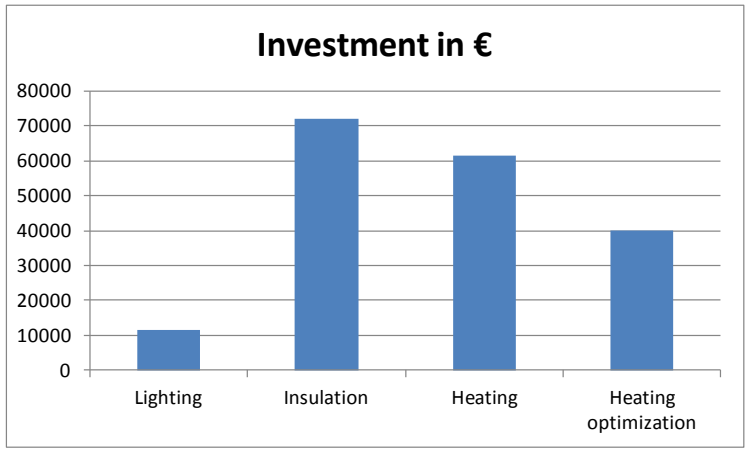
Identical questions on 4 generic measures. Adoption of the following measures since 2008

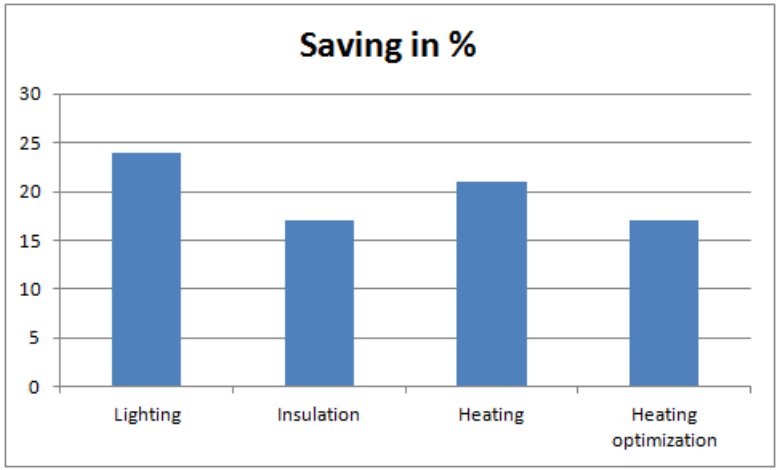
- lighting replacement (*lighting*)
- thermal insulation of the building (*insulation*)
- replacing the heating system (*heating*)
- optimization of the heating system (*heating optimization*)

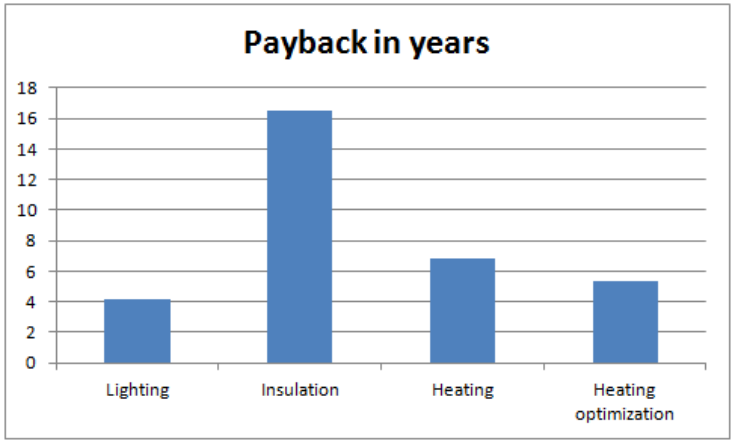
Randomize questions for control group (in pairs of 2)

Only keep observations for which companies had not decided to implement measures anyway

Generic measures: characteristics







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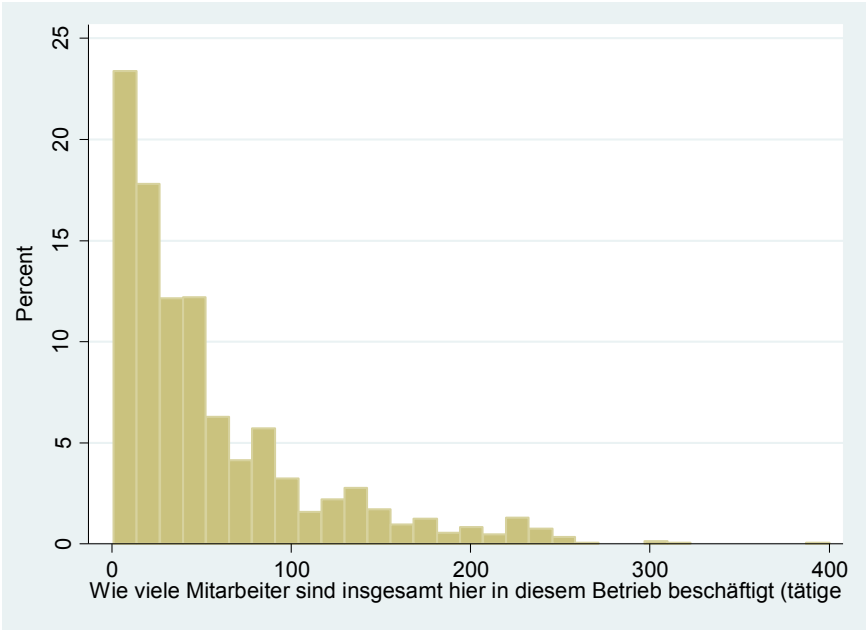
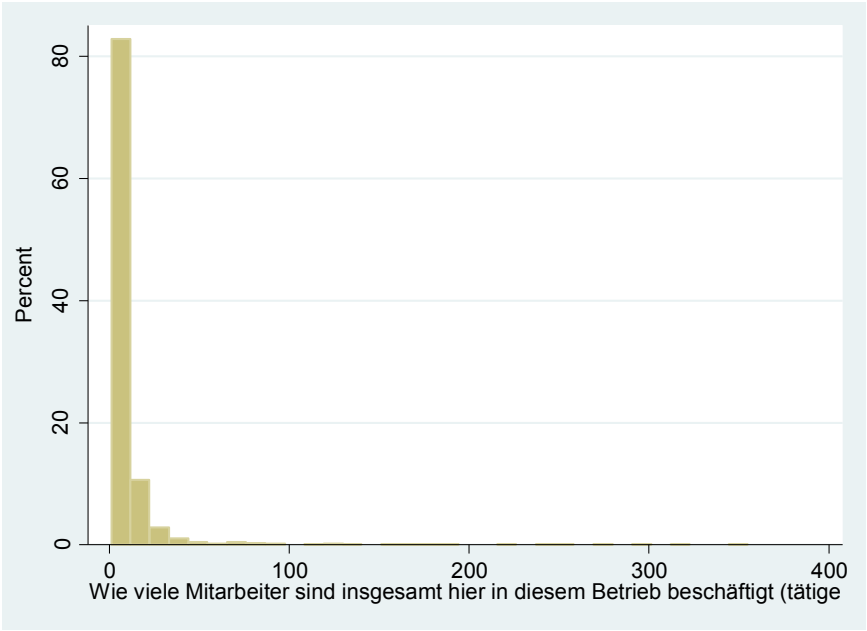
Number of employees in audit and original control group

Audit group (N= 1471)



Control group (N=2010)



- Limit analysis to companies with less than 50 employees

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Results of first step: Audit participation

Covariates	lighting		insulation		heating		heating optimization	
energy cost share	1.07	*	1.64	***	1.55	**	1.63	**
	(0.61)		(0.60)		(0.61)		(0.62)	
employees	1.43	***	1.45	***	1.46	***	1.54	***
	(0.09)		(0.09)		(0.09)		(0.10)	
emanager	-0.26		-0.12		-0.27		-0.26	
	(0.26)		(0.25)		(0.26)		(0.26)	
subsidiary	-0.91	***	-0.47	*	-0.62	**	-0.84	**
	(0.25)		(0.26)		(0.26)		(0.27)	
rented	-0.53	***	-0.53	***	-0.72	***	-0.56	***
	(0.15)		(0.15)		(0.16)		(0.15)	
hotels and restaurants	0.73	***	1.26	***	0.90	***	1.33	**
	(0.27)		(0.26)		(0.27)		(0.28)	
trade	0.24		0.54	***	0.45	**	0.62	***
	(0.22)		(0.21)		(0.22)		(0.22)	
services	0.75	***	1.23	***	0.94	***	1.21	**
	(0.22)		(0.22)		(0.23)		(0.23)	
metal	0.99	***	0.80	***	0.70	**	0.72	
	(0.34)		(0.30)		(0.32)		(0.31)	
foodstuffs	-0.10		0.14		0.12		0.04	
	(0.30)		(0.31)		(0.34)		(0.31)	
constant	-3.16	***	-3.56	***	-3.22	***	-3.53	***
	(0.28)		(0.28)		(0.29)		(0.30)	
LR(Chi2)	463.41	***	461.56	***	426.85	***	471.05	***
Pseudo R2	0.2864		0.2798		0.2813		0.3002	
N	1167		1190		1099		1139	

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Results of propensity score estimators - Audit effects (in percentage points)

Measure	Group	Propensity score estimators N	nn(1)	N	nn(4)	N	Kernel
lighting	audit	575	20.74 ***	582	18.61 ***	582	20.73 ***
	control	585		585		585	
insulation	audit	598	11.18 ***	601	9.86 ***	601	10.52 ***
	control	589		589		589	
heating	audit	530	5.94 *	572	8.00 **	572	9.54 ***
	control	509		509		509	
heating optimization	audit	564	26.95 ***	607	27.66 ***	607	28.81 ***
	control	517		517		517	

Note 1: *** indicates significance at p<0.01, ** indicates significance at p<0.05 and * indicates significance at p<0.1 in an individual two-tailed t-test.
Note 2: Sample sizes for nn(4) are the same as for nn(1).

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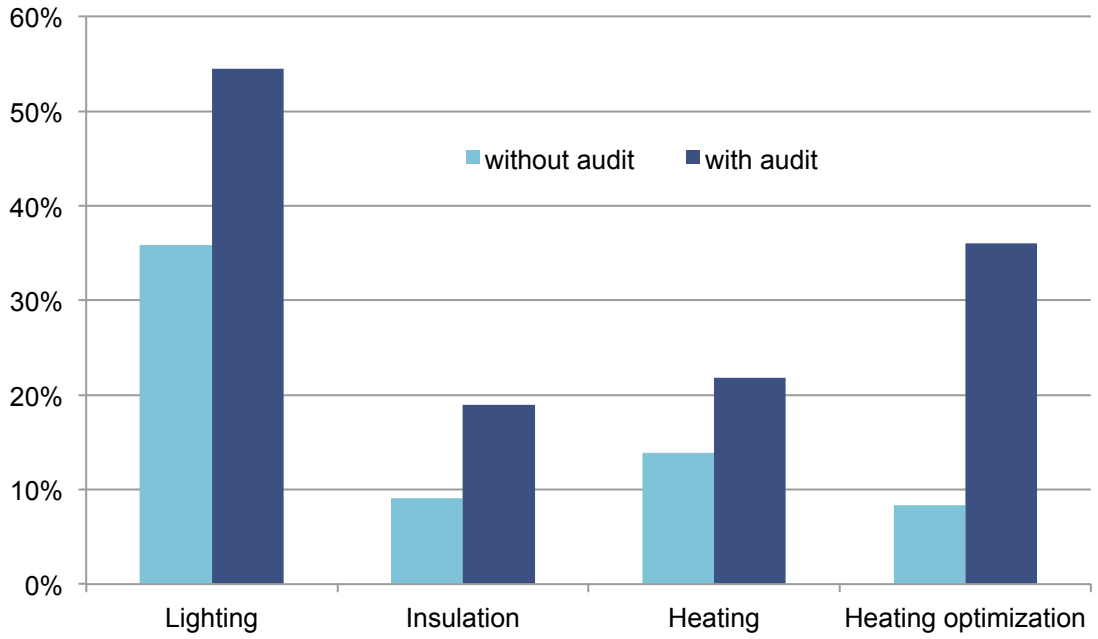
Adoption in control group and treatment group [based on nn(4)]



Data is for lighting

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Audit effects (in percentage points) – for **previously considered measures** only

Measure	Group	N	nn(1)	nn(4)	N	Kernel
lighting	audit control	391 585	17.18 ***	20.30 ***	392 585	19.39 ***
insulation	audit control	157 589	35.35 ***	27.77 ***	157 589	30.85 ***
heating	audit control	235 509	14.68 ***	16.74 ***	247 509	16.28 ***
heating optimization	audit control	177 517	24.58 ***	23.22 ***	193 517	24.42 ***

Note 1: *** indicates significance at p<0.01, ** indicates significance at p<0.05 and * indicates significance at p<0.1 in an individual two-tailed t-test.
Note 2: Sample sizes for nn(4) are the same as for nn(1).

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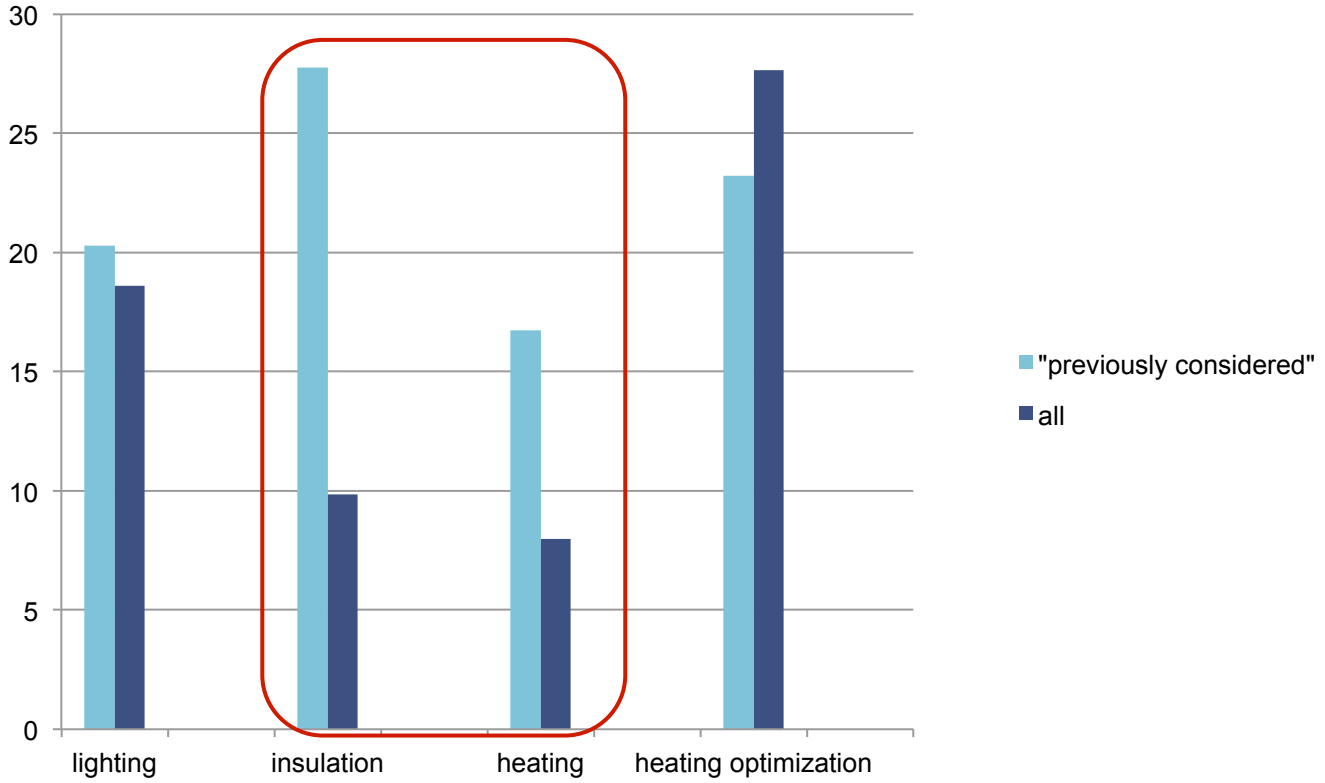
Audit effects for all measures compared to previously considered measures



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Summary on program effectiveness

Audits are effective, but effectiveness differs by measures

Absolute ATT effects

- lighting (20 percentage points) , heating optimization (28 percentage points), thermal insulation (11 percentage points) & exchange heating system (6-15 percentage points)
- highest for the lower-cost measures

Relative ATT effects

- Highest for heating optimization – measure with highest lack of information?

For pre-considered higher-cost measures audit effectiveness is higher than for not-pre-considered measures

- Audits help overcome intra-organizational barriers – audit provides third party „certification“ of economic performance of measures? Any other ideas?

Caveat

- Non-random program participation (self selection)
  - Problem: selection bias if observable or unobservable characteristics, which affect program (here: audit) participation also affect outcome (here: adoption)

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Thank you!

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Differences in covariates between groups before and after matching (L)

Table A2: Means of covariates between audit and control group before and after nn(1) matching (for *lighting*)

Covariates		Unmatched / Matched	Mean: Audit group	Mean: Control group	Difference between groups
energy cost share	share	U	0.091	0.111	***
		M	0.092	0.100	
employees (log)	numbers	U	2.692	1.488	***
		M	2.678	2.664	
emanager	0/1 dummy	U	0.115	0.065	***
		M	0.117	0.153	*
subsidiary	0/1 dummy	U	0.076	0.118	**
		M	0.077	0.090	
rented	0/1 dummy	U	0.423	0.583	***
		M	0.428	0.369	*
hotels and restaurants	0/1 dummy	U	0.115	0.094	
		M	0.113	0.129	
trade	0/1 dummy	U	0.222	0.275	**
		M	0.224	0.230	
services	0/1 dummy	U	0.253	0.292	
		M	0.252	0.285	
metal	0/1 dummy	U	0.125	0.031	***
		M	0.122	0.085	**
foodstuffs	0/1 dummy	U	0.067	0.074	
		M	0.068	0.043	*
other production	0/1 dummy	U	0.218	0.234	
		M	0.2209	0.2278	

Note 1: *** indicates significance at p<0.01, ** indicates significance at p<0.05 and * indicates significance at p<0.1 in an individual two-tailed t-test.

BACKUP SLIDES

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Propensity score matching estimator
(Rosenbaum and Rubin 1983)

Other matching estimators

- Multiple nearest neighbors (here: 4)
- Kernel estimator (uses all companies in control group but attaches lower weights to more distant companies)

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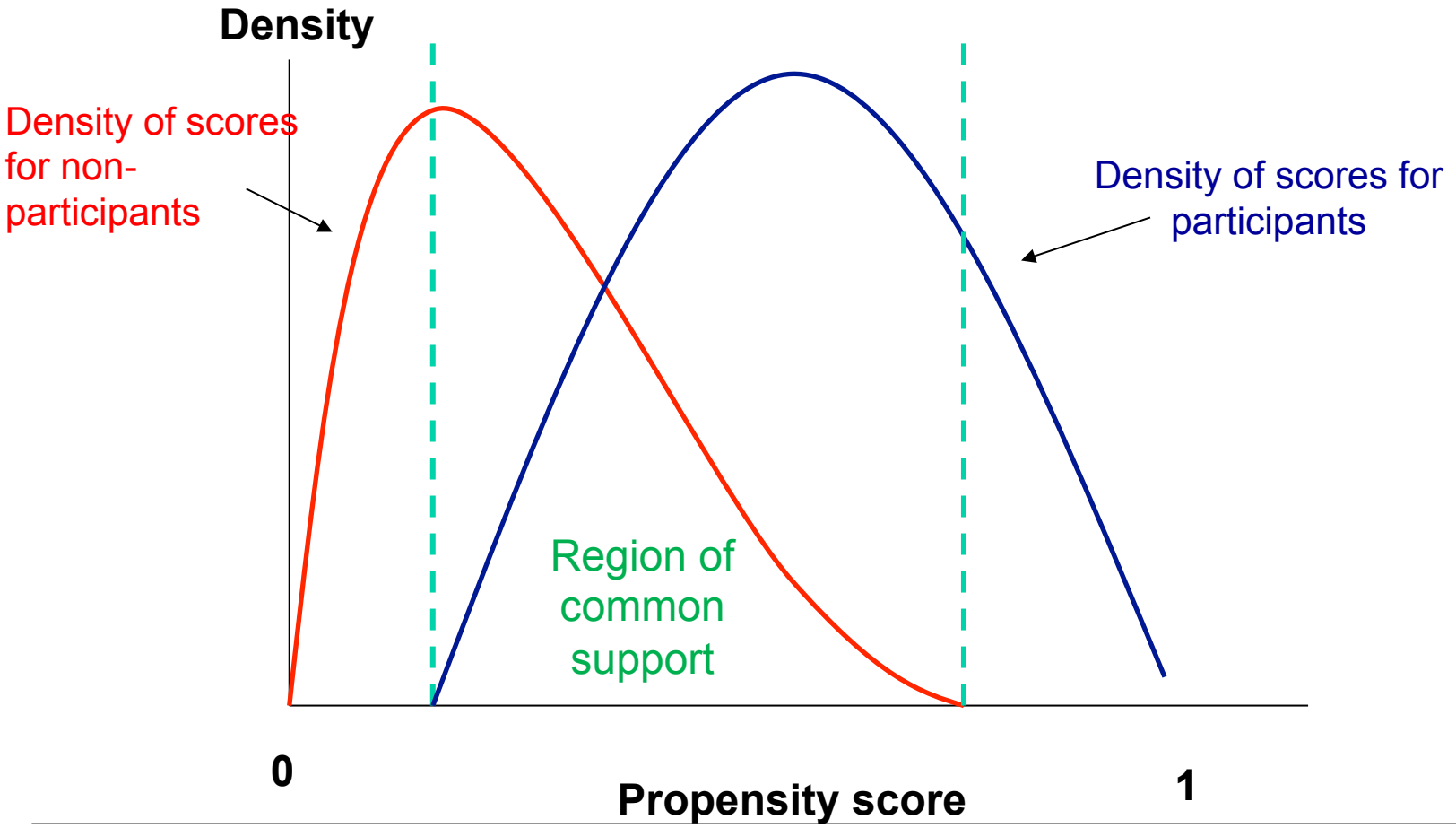
Common support



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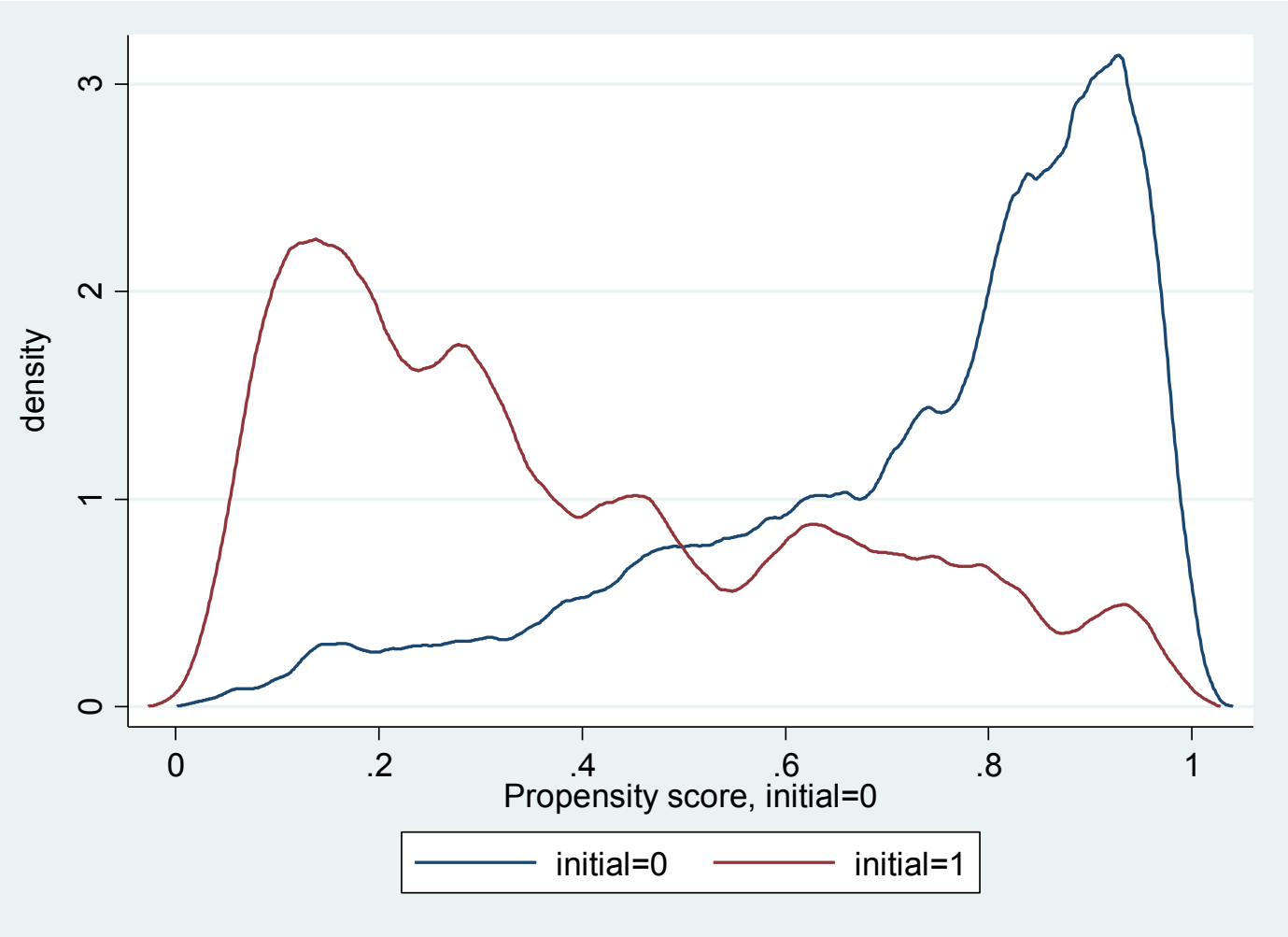


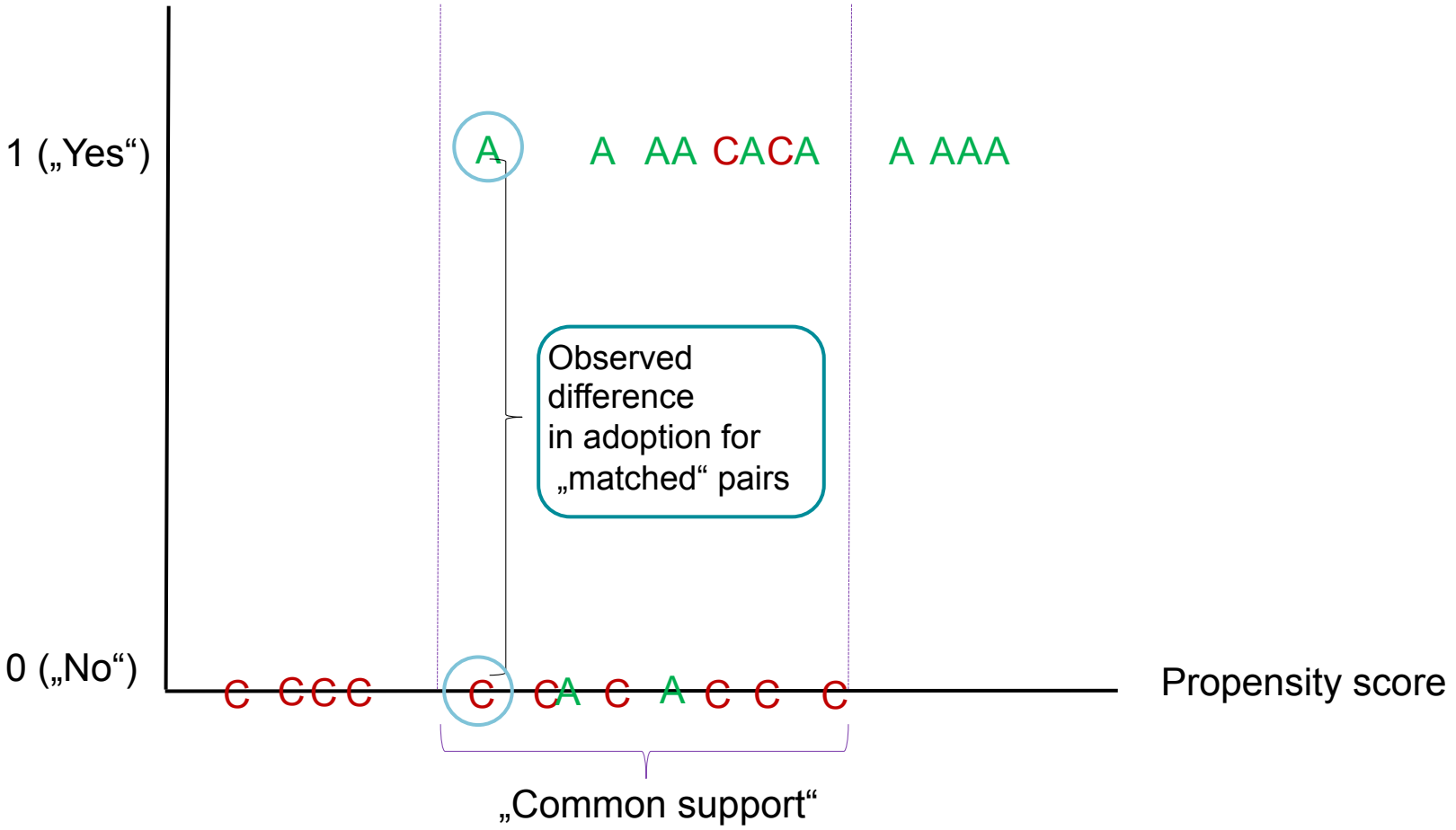
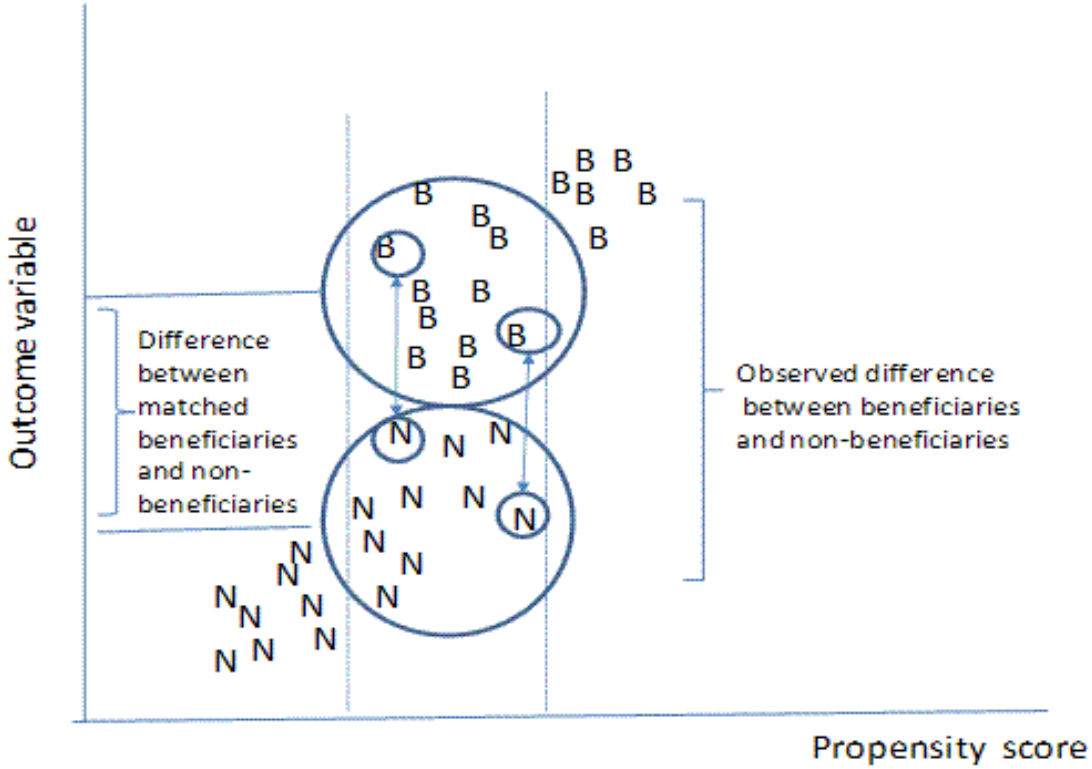

Outcome variable (Adoption Dummy)
1 („Yes“)
0 („No“)
Observed difference in adoption for „matched“ pairs
Propensity score
„Common support“

Figure 1. A graphical representation of matching on the propensity score



Means of covariates between audit and control group (restricted to < 50 employees!) (Lighting)

Covariates		Mean		Difference
		Audit group	Control group	between groups
energy cost share	share	0.091	0.111	***
employees (log)	numbers	2.692	1.488	***
emanager	0/1 dummy	0.115	0.065	***
subsidiary	0/1 dummy	0.076	0.118	**
rented	0/1 dummy	0.423	0.583	***
hotels and restau-rants	0/1 dummy	0.115	0.094	
trade	0/1 dummy	0.222	0.275	**
services	0/1 dummy	0.253	0.292	
metal	0/1 dummy	0.125	0.031	***
foodstuffs	0/1 dummy	0.067	0.074	
other production	0/1 dummy	0.218	0.234	

Data is for lighting

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Evaluation based on potential outcome (= counterfactual) framework [Roy (1951)& Rubin (1974)]

We are interested in the average treatment effect of treatment on the treated (ATT), i.e. the effect of the program on those participating in the program:

$$\tau_{ATT} = E(\tau / D = 1) = E[Y(1) / D = 1] - E[Y(0) / D = 1]$$

The counterfactual mean for those participating in the programm, i.e. $E[Y(0) / D = 1]$ cannot be observed.

If participation in the program was random: $E[Y(0) / D = 1] = E[Y(0) / D = 0]$
We could just use the expected outcome of those not participating in the program as the counterfactual, i.e. ATT is said to be „identified“

Program/policy Evaluation

Key assumptions:

- Conditional independence assumption (CIA) (= ~ unconfoundedness, selection on „observables“):

  Conditional on the set of relevant covariates - treatment assignment is independent of the outcomes. Thus, the covariates are nonresponsive to the audit participation. The CIA implies that program participation only depends on observable subject characteristics and that all covariates which affect both the program participation and the outcome must be observed.

Program/policy Evaluation

Common support (=overlap) assumption: subjects with the same covariates have a positive probability of participating in the program and also of not participating in the audit program. In other words, each subject has a positive probability of being in the control group and being in the treatment group. (To calculate ATT, it is sufficient that potential matches exist in the control group.)

Also need the following assumptions:

Stable unit-treatment value: the treatment effect for each subject *i* in (1) is not affected by the participation of other subjects in the program (no interference).

No variation in treatment : treatments are assumed to be comparable across subjects.