Aufsätze zur Selbstselektion und zum Messemanagement

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Abkürzungsverzeichnis

ACM	Association for Computing Machinery
ATE	Average treatment effect
ATT	Averate treatment effect of the treated
ATU	Averate treatment effect of the untreated
Aufl.	Auflage
AUMA	Ausstellungs- und Messe-Ausschuss der Deutschen Wirtschaft e.V.
biasaft	Standardized bias after matching
biasbef	Standardized bias before matching
bzw.	beziehungsweise
CIA	Conditional independence assumption
Coef.	Coefficient
Const.	Constant
CS	Common support
CVM	Covariate matching
DBW	Die Betriebswirtschaft (Zeitschrift)
d. h.	das heißt
DID	Difference-in-differences
DMTP	Differentiated mail transfer protocol
eds	Editors
e. g.	for example
E-mail	Electronic mail
Est.	Estimator
et al.	et alii (und andere)
etc.	et cetera
e. V.	eingetragener Verein
FAMAB	Verband Direkte Wirtschaftskommunikation e.V.
Hrsg.	Herausgeber
IFAU	Institute for Labour Market Policy Evaluation
IIA	Independence for irrelevant alternatives assumption
IS	Information systems

ISMA	International Securities Market Association
ISP	Internet service provider
ISR	Information Systems Research (Zeitschrift)
IT	Information technology
IZA	Forschungsinstitut zur Zukunft der Arbeit
Jg.	Jahrgang
KM	Kernel matching
LLM	Local linear matching
LPM	Local polynomial matching
MoB	Make-or-buy
NCDS	National child development study
NN	Nearest neighbour
No.	Number
Nr.	Nummer
NSW	National supported work demonstration
Obs.	Observations
OECD	Organisation for Economic Cooperation and Development
offsup	Off support
OLS	Ordinary least squares regression
pp	pages
PS	Propensity score
PSM	Propensity score matching
S.	Seite
SATT	Sample average treatment effect of the treated
SB	Standardized bias
s.e.	Standard error
SIAW	Schweizerisches Institut für Angewandte Wirtschaftsforschung
SPAM	Unsolicited bulk messages with commercial content
SUTVA	Stable unit treatment value assumption
u. a.	und andere
UI	Unemployment insurance
vgl.	vergleiche

Vol.	Volume
WiSt	Wirtschaftswissenschaftliches Studium (Zeitschrift)
www	World wide web
z. B.	zum Beispiel
ZEW	Zentrum Europäische Wirtschaftsforschung GmbH
ZfbF	Schmalenbachs Zeitschrift für betriebswirtschaftliche Forschung

Symbolverzeichnis

β	Coefficient of observable covariates X
$b(X)$	Balancing score
$CI+$	Upper bound confidence interval
$CI-$	Lower bound confidence interval
D	Treatment indicator
$E[(.)]$	Expectation operator
$\hat{f}\,(.)$	Nonparametric densitiy estimator
γ	Coefficient of unobservable covariates U
k	Number of continuous covariates
$K_M(i)$	Number of times individual i is used as a match
λ_0^1	Mean of $Y(1)$ for individuals outside common support
L	Number of options (treatments), *with* $l=1,\ldots,m,\ldots,L$
M	Number of matches
N	Set of individuals, with $i=1,\ldots,N$
p	Polynomial order
$P(.)$	Probability operator
$Pr(.)$	Probability operator
$P(X)$	Propensity Score
$\overline{P}$	Sample proportion of persons taking treatment
q	Treshold amount
$\sigma^2_{1(0)}$	Conditional outcome variance
$\hat{S}_{P(q)}$	Region of common support (given a treshold amount q)
$Sig+$	Upper bound significance level
$Sig-$	Lower bound significance level
t-hat+	Upper bound Hodges-Lehmann point estimate
t-hat-	Lower bound Hodges-Lehmann point estimate
τ_i	Individual treatment effect
τ_{ATE}	Average treatment effect
τ_{ATT}	Average treatment effect of the treated

τ_{SATT}	Sample average treatment effect of the treated
t	Posttreatment period
t'	Pretreatment period
U	Unobservable covariates
Var_{ATE}	Variance bound for ATE
Var_{ATT}	Variance bound for ATT
Var_{SATT}	Variance of sample average treatement effect
$Var(\hat{\tau}_{ATT})$	Variance approximation by Lechner
$V_{1[0]}(X)$	Variance of *X* in the treatment [control] group before matching
$V_{1M[0M]}(X)$	Variance of *X* in the treatment [control] group after matching
$w(.)$	Weighting function
X	Observable covariates
$\overline{X}_{1[1M]}$	Sample average of *X* in the treatment group before [after] matching
$\overline{X}_{0[0M]}$	Sample average of *X* in the control group before [after] matching
Y	Outcome variable
$Y(D)$	Potential outcome given *D*
Ω	Set of the population of interest

Überblick

Die vorliegende kumulative Dissertation untersucht zwei ausgewählte Problembereiche der Marketingforschung. Zum einen steht in den Abschnitten A und B die Selbstselektionsproblematik im Fokus. Werden in einem nicht-experimentellen Umfeld kausale Maßnahmeneffekte ermittelt, können Selbstselektionsverzerrungen auftreten, da eine Zuordnung zur Maßnahme nicht zufällig - wie etwa in einem experimentellen Umfeld[1] - erfolgt. In Abschnitt A wird eine Analysemethode im Detail erläutert, die dem Problem der Selbstselektion Rechnung trägt. In Abschnitt B wird ein Anwendungsbeispiel für diese Methode präsentiert. Zum anderen wird in Abschnitt C eine Make-or-Buy-Fragestellung qualitativ analysiert. Dabei wird der Frage nachgegangen, ob es für ein Unternehmen profitabler ist, Produkte oder Dienstleistungen selbst zu fertigen bzw. zu erbringen (Make) oder auf dem Markt zu beschaffen (Buy).

In den nachfolgenden Abschnitten A bis C werden die dieser Arbeit zugrundeliegenden drei Artikel präsentiert. Die nachstehende Tabelle fasst diese Artikel zusammen:

Abschnitt	**Autoren**	**Titel (Jahr)**	**Zeitschrift/Buch**	**Status**
A	Marco Caliendo Sabine Kopeinig	Some Practical Guidance for the Implementation of Propensity Score Matching (2008)	Journal of Economic Surveys	Publiziert
B	Marco Caliendo Michel Clement Dominik Papies Sabine Scheel-Kopeinig	The Cost Impact of Spam-Filters: Measuring the Effect of Information System Technologies in Organizations (2008)	Information Systems Research (ISR)	Wird in 3. Runde eingereicht.
C	Sabine Kopeinig Karen Gedenk	Make-or-Buy-Entscheidungen von Messegesellschaften (2005)	Kölner Kompendium der Messewirtschaft	Publiziert

Tabelle 1: Übersicht der Artikel

Die Artikel der Abschnitte A *Caliendo und Kopeinig (2008)* sowie C *Kopeinig und Gedenk (2005)* sind in der vorliegenden Form im *Journal of Economic Surveys* bzw. *Kölner Kompendium der Messewirtschaft* publiziert. Der Artikel aus Abschnitt B *Caliendo/Clement/Papies und Scheel-Kopeinig (2008)* ist als IZA Discussion Paper veröffentlicht. Dieser Artikel befindet sich im „editorial process“ und wird in dritter Runde bei der Zeitschrift *Information Systems Research (ISR)* eingereicht. Im folgenden Überblick erfolgt eine kurze Zusammen-

[1] Vgl. Harrison, G. W./List, J. A. (2004).

fassung der zentralen Erkenntnisse der oben genannten Artikel unter Darlegung der verfolgten Zielsetzung und der verwendeten Vorgehensweise.

Die Arbeiten in den Abschnitten A und B behandeln die Selbstselektionsproblematik. Dabei wird in *Caliendo und Kopeinig (2008)* eine Analysemethode - Propensity Score Matching (PSM) - im Detail vorgestellt. In *Caliendo/Clement/Papies und Scheel-Kopeinig (2008)* wird ein Anwendungsbeispiel für diese Methode präsentiert. In der empirischen Marketingforschung sollen häufig Erfolgswirkungen von Marketingmaßnahmen ermittelt werden. Nicht immer sind experimentelle Untersuchungsdesigns möglich, um kausale Effekte dieser Maßnahmen zu schätzen. Sollen beispielsweise Bonusprogramme oder Messebeteiligungen evaluiert werden, muss häufig auf nicht-experimentelle Daten zurückgegriffen werden. Das inhärente Problem der Selbstselektion soll an dem nachfolgenden Beispiel verdeutlicht werden: Wird der Effekt einer Messeteilnahme lediglich dadurch ermittelt, dass ein Zielgrößenvergleich (z. B. Auftragsvolumen) zwischen Messeausstellern und Nichtausstellern erfolgt, dann wird vernachlässigt, dass unter Umständen gerade erfolgreichere Unternehmen an der Messe teilnehmen und sich so also „selbst zur Maßnahme selektieren“. Dabei kann die Entscheidung an der Messe teilzunehmen sowohl von beobachtbaren (z. B. Exportvolumen, Mitarbeiterzahl der Unternehmen etc.) als auch von unbeobachtbaren Eigenschaften der Unternehmen abhängen. Die zentrale Idee des PSM-Ansatzes[2] ist es, aus der Gruppe der Nichtteilnehmer nur jene Untersuchungseinheiten für den Zielgrößenvergleich heranzuziehen, die den Teilnehmern bezüglich beobachtbarer Eigenschaften am ähnlichsten sind. Unterschiede in der Zielgröße zwischen den Teilnehmern an einer Maßnahme und der adjustierten Kontrollgruppe können dann als Maßnahmeneffekt interpretiert werden.[3]

Soll ein Maßnahmeneffekt mit Hilfe des Propensity Score Matchings evaluiert werden, ist der Anwender mit einer Vielzahl an Implementierungsschritten und Detailfragen konfrontiert. In methodischen Standardwerken[4] wird PSM noch nicht besprochen. Daher wird in *Caliendo*

[2] Propensity Score Matching geht zentral auf die Arbeiten von Rubin, D. (1974) sowie Rosenbaum, P./Rubin, D. (1983b, 1985) zurück.

[3] Zentrale Annahme des Matching-Ansatzes ist, dass Unterschiede zwischen Teilnehmern und Nichtteilnehmern lediglich auf beobachtbaren Eigenschaften beruhen. Diese Annahme ist u. a. als „selection on observables“ bekannt; vgl. Heckman, J./Robb, R. (1985).

[4] Z. B. Backhaus, K./Erichson, B./Plinke, W./Weiber, R. (2008) oder Greene, W. H. (2003).

und Kopeinig (2008) dem PSM-Anwender ein Leitfaden für die Umsetzung der Implementierungsschritte und der damit verbundenen Entscheidungen an die Hand gegeben werden.

Nach einer Darstellung des formalen Rahmens wird im Beitrag A gezeigt, wie PSM das Selbstselektionsproblem lösen kann und welche zentralen Annahmen dazu nötig sind.[5] Es werden fünf zentrale Implementierungsschritte aufgezeigt und mögliche Entscheidungsalternativen ausführlich diskutiert. Bei den Implementierungsschritten handelt es sich um die Schätzung des Propensity Scores (Schritt 1), die Auswahl des Matching-Algorithmus (Schritt 2), Overlap und Common Support (Schritt 3), Matching Qualität und Schätzung des Maßnahmeneffektes (Schritt 4) sowie um Sensitivitätsanalysen (Schritt 5). Abschließend werden noch praxisrelevante Sachverhalte - mit welchen PSM-Anwender konfrontiert sein können - dargestellt und Weiterentwicklungen des PSM-Ansatzes diskutiert.

Im Ergebnis liefert der vorliegende Beitrag eine bis dato einmalige, anwendungsorientierte, sequenzielle und gut verständliche Orientierungs- und Entscheidungshilfe bei der PSM-Implementierung. Außerdem erfolgt eine umfassende Bündelung und Auswertung der wissenschaftlichen Literatur zum Thema „Propensity Score Matching“.

Im Beitrag B *Caliendo/Clement/Papies und Scheel-Kopeinig (2008)* wird analysiert, ob die Installation eines Spam-Filters die Arbeitszeitverluste von Mitarbeitern, die u. a. dadurch entstehen, dass Spam-Mails überprüft werden müssen, reduzieren kann. Um diesen Maßnahmeneffekt mit nicht-experimentellen Daten unter Berücksichtigung möglicher Selbstselektionsverzerrungen zu messen, wird das Verfahren des Propensity Score Matchings angewandt.

Im Zusammenhang mit Spam-Mails entstehen Unternehmen sowohl zentrale Kosten auf IT-Ebene als auch individuelle Kosten auf Mitarbeiterebene. Sowohl eine Quantifizierung dieser Kosten als auch eine Analyse der Kostenwirkungen einer Schutzmaßnahme gegen Spam (Spam-Filter) erfolgte in der wissenschaftlichen Literatur bislang noch nicht. Der vorliegende Beitrag versucht diese Forschungslücke zu schließen.

[5] Vgl. dazu z. B. Heckman, J./Ichimura, H./Todd, P. (1997a).

Nach einer Zusammenfassung der bisherigen „Spam-Forschung“ und einer kurzen Beschreibung der Methode des Propensity Score Matchings wird die Datenerhebung und das Forschungsdesign beschrieben und deskriptive Ergebnisse präsentiert. Im Anschluss folgt die Matchinganalyse. Nach einer Darstellung der Matchingergebnisse werden auch Sensitivitätsanalysen hinsichtlich Effektheterogentität und unbeobachtbarer Heterogentität durchgeführt.

Der vorliegende Beitrag zeigt, dass Spam-Mails durchaus nennenswerte Kosten auf individueller Mitarbeiterebene aber vernachlässigbare Kosten auf IT-Ebene verursachen. Die Installation eines Spam Filters kann die Arbeitszeitverluste, die Mitarbeitern durch die Kontrolle und das Löschen von Spam Mails entstehen, um ca. 35 % reduzieren. Die Effektivität der Schutzmaßnahme hängt aber im Einzelnen maßgeblich von der individuellen Anzahl der erhaltenen Spam-Mails und vom Spam-Kenntnisstand des Mitarbeiters ab.

Kopeinig und Gedenk (2005) untersuchen im Messe-Kontext die Vorteilhaftigkeit von Make-or-Buy-(MoB)-Entscheidungen für Messe-Dienstleistungen, wie Gastronomie- oder Standbau-Services. Ziel des Beitrags ist es, Messeunternehmen eine Entscheidungshilfe bei der Auswahl von möglichen Make-or-Buy-Alternativen an die Hand zu geben. Nach einer Systematisierung relevanter MoB-Entscheidungen von Messegesellschaften werden mögliche MoB-Entscheidungsalternativen dargestellt. Dabei gibt es zwischen den beiden Extrema „Make“ und „Buy“ eine Vielzahl relevanter Organisationsformen, bei denen Messegesellschaften mit anderen Unternehmen kooperieren (Cooperate).[6] In der Praxis können über Messegesellschaften und Messe-Dienstleistungen hinweg die gewählten Organisationsformen erheblich variieren. Beispielsweise werden am Messestandort Frankfurt am Main Gastronomie-Services über ein Tochterunternehmen selbst erbracht. Andere Messegesellschaften favorisieren eine marktnahe Alternative und schließen Pachtverträge mit unabhängigen Messegastronomen ab.

Im Beitrag werden Einflussfaktoren auf die Vorteilhaftigkeit der Handlungsalternativen „Make“ vs. „Buy“ herausgearbeitet. Diese werden zum einen aus dem Transaktionskostenansatz[7] und zum anderen aus der konzeptionellen Literatur zum Messewesen abgeleitet. Im

[6] Vgl. Picot, A. (1991).
[7] Vgl. Coase, R. H. (1937), Williamson, O. E. (1975, 1985).

speziellen erfolgt eine qualitative Analyse der Vorteilhaftigkeit von „Make“-vs.“Buy“-Entscheidungen für zwei exemplarische Messe-Dienstleistungen.

Im Ergebnis bietet der vorliegende Beitrag speziell Messeunternehmen eine Entscheidungshilfe für den Entscheidungsprozess „Make-vs.-Buy“ für einzelne Messedienstleistungen. Die im Fokus der Arbeit stehende Vorgehensweise der Vorteilhaftigkeitsanalyse kann allgemein auf Make-or-Buy-Fragestellungen in anderen Branchen übertragen werden.

Abschnitt A:

Caliendo, Marco/Kopeinig, Sabine: Some Practical Guidance for the Implementation of Propensity Score Matching

Caliendo, Marco/Kopeinig, Sabine: Some Practical Guidance for the Implementation of Propensity Score Matching, Journal of Economic Surveys, 22. Jg. (1), 2008, S. 31 - 72.

SOME PRACTICAL GUIDANCE FOR THE IMPLEMENTATION OF PROPENSITY SCORE MATCHING

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Abstract. Propensity score matching (PSM) has become a popular approach to estimate causal treatment effects. It is widely applied when evaluating labour market policies, but empirical examples can be found in very diverse fields of study. Once the researcher has decided to use PSM, he is confronted with a lot of questions regarding its implementation. To begin with, a first decision has to be made concerning the estimation of the propensity score. Following that one has to decide which matching algorithm to choose and determine the region of common support. Subsequently, the matching quality has to be assessed and treatment effects and their standard errors have to be estimated. Furthermore, questions like 'what to do if there is choice-based sampling?' or 'when to measure effects?' can be important in empirical studies. Finally, one might also want to test the sensitivity of estimated treatment effects with respect to unobserved heterogeneity or failure of the common support condition. Each implementation step involves a lot of decisions and different approaches can be thought of. The aim of this paper is to discuss these implementation issues and give some guidance to researchers who want to use PSM for evaluation purposes.

Keywords. Propensity score matching; Treatment effects; Evaluation; Sensitivity analysis; Implementation

1. Introduction

Matching has become a popular approach to estimate causal treatment effects. It is widely applied when evaluating labour market policies (see e.g., Heckman *et al.*, 1997a; Dehejia and Wahba, 1999), but empirical examples can be found in very diverse fields of study. It applies for all situations where one has a treatment, a group of treated individuals and a group of untreated individuals. The nature of treatment may be very diverse. For example, Perkins *et al.* (2000) discuss the usage of matching in pharmacoepidemiologic research. Hitt and Frei (2002) analyse the effect of online banking on the profitability of customers. Davies and Kim (2003) compare

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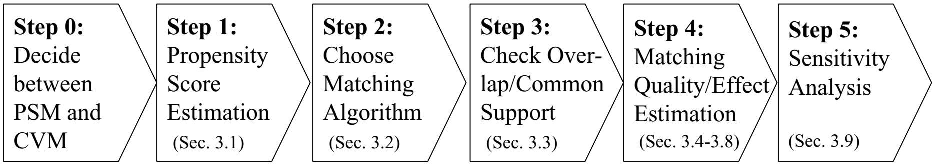
the effect on the percentage bid–ask spread of Canadian firms being interlisted on a US Exchange, whereas Brand and Halaby (2006) analyse the effect of elite college attendance on career outcomes. Ham *et al.* (2004) study the effect of a migration decision on the wage growth of young men and Bryson (2002) analyses the effect of union membership on wages of employees. Every microeconometric evaluation study has to overcome the fundamental evaluation problem and address the possible occurrence of selection bias. The first problem arises because we would like to know the difference between the participants' outcome with and without treatment. Clearly, we cannot observe both outcomes for the same individual at the same time. Taking the mean outcome of nonparticipants as an approximation is not advisable, since participants and nonparticipants usually differ even in the absence of treatment. This problem is known as selection bias and a good example is the case where high-skilled individuals have a higher probability of entering a training programme and also have a higher probability of finding a job. The matching approach is one possible solution to the selection problem. It originated from the statistical literature and shows a close link to the experimental context.[1] Its basic idea is to find in a large group of nonparticipants those individuals who are similar to the participants in all relevant pretreatment characteristics X. That being done, differences in outcomes of this well selected and thus adequate control group and of participants can be attributed to the programme. The underlying identifying assumption is known as unconfoundedness, selection on observables or conditional independence. It should be clear that matching is no 'magic bullet' that will solve the evaluation problem in any case. It should only be applied if the underlying identifying assumption can be credibly invoked based on the informational richness of the data and a detailed understanding of the institutional set-up by which selection into treatment takes place (see for example the discussion in Blundell *et al.*, 2005). For the rest of the paper we will assume that this assumption holds.

Since conditioning on all relevant covariates is limited in the case of a high dimensional vector X ('curse of dimensionality'), Rosenbaum and Rubin (1983b) suggest the use of so-called balancing scores $b(X)$, i.e. functions of the relevant observed covariates X such that the conditional distribution of X given $b(X)$ is independent of assignment into treatment. One possible balancing score is the propensity score, i.e. the probability of participating in a programme given observed characteristics X. Matching procedures based on this balancing score are known as propensity score matching (PSM) and will be the focus of this paper. Once the researcher has decided to use PSM, he is confronted with a lot of questions regarding its implementation. Figure 1 summarizes the necessary steps when implementing PSM.[2]

The aim of this paper is to discuss these issues and give some practical guidance to researchers who want to use PSM for evaluation purposes. The paper is organized as follows. In Section 2, we will describe the basic evaluation framework and possible treatment effects of interest. Furthermore we show how PSM solves the evaluation problem and highlight the implicit identifying assumptions. In Section 3, we will focus on implementation steps of PSM estimators. To begin with, a first decision has to be made concerning the estimation of the propensity score


Step 0: Decide between PSM and CVM
Step 1: Propensity Score Estimation (Sec. 3.1)
Step 2: Choose Matching Algorithm (Sec. 3.2)
Step 3: Check Over-lap/Common Support (Sec. 3.3)
Step 4: Matching Quality/Effect Estimation (Sec. 3.4-3.8)
Step 5: Sensitivity Analysis (Sec. 3.9)

CVM: Covariate Matching, PSM: Propensity Score Matching

Figure 1. PSM – Implementation Steps.

(see Section 3.1). One has not only to decide about the probability model to be used for estimation, but also about variables which should be included in this model. In Section 3.2, we briefly evaluate the (dis-)advantages of different matching algorithms. Following that we discuss how to check the overlap between treatment and comparison group and how to implement the common support requirement in Section 3.3. In Section 3.4 we will show how to assess the matching quality. Subsequently we present the problem of choice-based sampling and discuss the question 'when to measure programme effects?' in Sections 3.5 and 3.6. Estimating standard errors for treatment effects will be discussed in Section 3.7 before we show in 3.8 how PSM can be combined with other evaluation methods. The following Section 3.9 is concerned with sensitivity issues, where we first describe approaches that allow researchers to determine the sensitivity of estimated effects with respect to a failure of the underlying unconfoundedness assumption. After that we introduce an approach that incorporates information from those individuals who failed the common support restriction, to calculate bounds of the parameter of interest, if all individuals from the sample at hand would have been included. Section 3.10 will briefly discuss the issues of programme heterogeneity, dynamic selection problems, and the choice of an appropriate control group and includes also a brief review of the available software to implement matching. Finally, Section 4 reviews all steps and concludes.

2. Evaluation Framework and Matching Basics

Roy–Rubin Model

Inference about the impact of a treatment on the outcome of an individual involves speculation about how this individual would have performed had (s)he not received the treatment. The standard framework in evaluation analysis to formalize this problem is the potential outcome approach or Roy–Rubin model (Roy, 1951; Rubin, 1974). The main pillars of this model are individuals, treatment and potential outcomes. In the case of a binary treatment the treatment indicator D_i equals one if individual i receives treatment and zero otherwise. The potential outcomes are then defined as $Y_i(D_i)$ for each individual i, where $i = 1, \ldots, N$ and N denotes the total population. The treatment effect for an individual i can be written as

$$\tau_i = Y_i(1) - Y_i(0) \tag{1}$$

The fundamental evaluation problem arises because only one of the potential outcomes is observed for each individual i. The unobserved outcome is called the counterfactual outcome. Hence, estimating the individual treatment effect τ_i is not possible and one has to concentrate on (population) average treatment effects.[3]

Parameter of Interest and Selection Bias

Two parameters are most frequently estimated in the literature. The first one is the population average treatment effect (ATE), which is simply the difference of the expected outcomes after participation and nonparticipation:

$$\tau_{ATE} = E(\tau) = E[Y(1) - Y(0)] \tag{2}$$

This parameter answers the question: 'What is the expected effect on the outcome if individuals in the population were randomly assigned to treatment?' Heckman (1997) notes that this estimate might not be of relevance to policy makers because it includes the effect on persons for whom the programme was never intended. For example, if a programme is specifically targeted at individuals with low family income, there is little interest in the effect of such a programme for a millionaire. Therefore, the most prominent evaluation parameter is the so-called average treatment effect on the treated (ATT), which focuses explicitly on the effects on those for whom the programme is actually intended. It is given by

$$\tau_{ATT} = E(\tau|D = 1) = E[Y(1)|D = 1] - E[Y(0)|D = 1] \tag{3}$$

The expected value of ATT is defined as the difference between expected outcome values with and without treatment for those who actually participated in treatment. In the sense that this parameter focuses directly on actual treatment participants, it determines the realized gross gain from the programme and can be compared with its costs, helping to decide whether the programme is successful or not (Heckman *et al.*, 1999). The most interesting parameter to estimate depends on the specific evaluation context and the specific question asked. Heckman *et al.* (1999) discuss further parameters, like the proportion of participants who benefit from the programme or the distribution of gains at selected base state values. For most evaluation studies, however, the focus lies on ATT and therefore we will focus on this parameter, too.[4] As the counterfactual mean for those being treated – $E[Y(0)|D = 1]$ – is not observed, one has to choose a proper substitute for it in order to estimate ATT. Using the mean outcome of untreated individuals $E[Y(0)|D = 0]$ is in nonexperimental studies usually not a good idea, because it is most likely that components which determine the treatment decision also determine the outcome variable of interest. Thus, the outcomes of individuals from the treatment and comparison groups would differ even in the absence of treatment leading to a 'selection bias'. For ATT it can be noted as

$$E[Y(1)|D = 1] - E[Y(0)|D = 0] = \tau_{ATT} + E[Y(0)|D = 1] - E[Y(0)|D = 0] \tag{4}$$

The difference between the left-hand side of equation (4) and τ_{ATT} is the so-called 'selection bias'. The true parameter τ_{ATT} is only identified if

$$E[Y(0)|D = 1] - E[Y(0)|D = 0] = 0 \tag{5}$$

In social experiments where assignment to treatment is random this is ensured and the treatment effect is identified.[5] In nonexperimental studies one has to invoke some identifying assumptions to solve the selection problem stated in equation (4).

Unconfoundedness and Common Support

One major strand of evaluation literature focuses on the estimation of treatment effects under the assumption that the treatment satisfies some form of exogeneity. Different versions of this assumption are referred to as unconfoundedness (Rosenbaum and Rubin, 1983b), selection on observables (Heckman and Robb, 1985) or conditional independence assumption (CIA) (Lechner, 1999). We will use these terms throughout the paper interchangeably. This assumption implies that systematic differences in outcomes between treated and comparison individuals with the same values for covariates are attributable to treatment. Imbens (2004) gives an extensive overview of estimating ATEs under unconfoundedness. The identifying assumption can be written as

Assumption 1. *Unconfoundedness:* $Y(0), Y(1) \amalg D \mid X$

where $\amalg$ denotes independence, i.e. given a set of observable covariates X which are not affected by treatment, potential outcomes are independent of treatment assignment. This implies that all variables that influence treatment assignment and potential outcomes simultaneously have to be observed by the researcher. Clearly, this is a strong assumption and has to be justified by the data quality at hand. For the rest of the paper we will assume that this condition holds. If the researcher believes that the available data are not rich enough to justify this assumption, he has to rely on different identification strategies which explicitly allow selection on unobservables, too. Prominent examples are difference-in-differences (DID) and instrumental variables estimators.[6] We will show in Section 3.8 how propensity score matching can be combined with some of these methods.

A further requirement besides independence is the common support or overlap condition. It rules out the phenomenon of perfect predictability of D given X.

Assumption 2. *Overlap:* $0 < P(D = 1|X) < 1$.

It ensures that persons with the same X values have a positive probability of being both participants and nonparticipants (Heckman *et al.*, 1999). Rosenbaum and Rubin (1983b) call Assumptions 1 and 2 together 'strong ignorability'. Under 'strong ignorability' ATE in (2) and ATT in (3) can be defined for all values of X. Heckman *et al.* (1998b) demonstrate that the ignorability or unconfoundedness conditions are overly strong. All that is needed for estimation of (2) and (3) is mean independence. However, Lechner (2002) argues that Assumption 1 has the virtue of identifying

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mean effects for all transformations of the outcome variables. The reason is that the weaker assumption of mean independence is intrinsically tied to functional form assumptions, making an identification of average effects on transformations of the original outcome impossible (Imbens, 2004). Furthermore, it will be difficult to argue why conditional mean independence should hold and Assumption 1 might still be violated in empirical studies.

If we are interested in estimating the ATT only, we can weaken the unconfoundedness assumption in a different direction. In that case one needs only to assume

Assumption 3. *Unconfoundedness for controls:* $Y(0) \amalg D \mid X$

and the weaker overlap assumption

Assumption 4. *Weak overlap:* $P(D = 1 \mid X) < 1$.

These assumptions are sufficient for identification of (3), because the moments of the distribution of $Y(1)$ for the treated are directly estimable.

Unconfoundedness given the Propensity Score

It should also be clear that conditioning on all relevant covariates is limited in the case of a high dimensional vector X. For instance if X contains s covariates which are all dichotomous, the number of possible matches will be 2^s. To deal with this dimensionality problem, Rosenbaum and Rubin (1983b) suggest using so-called balancing scores. They show that if potential outcomes are independent of treatment conditional on covariates X, they are also independent of treatment conditional on a balancing score $b(X)$. The propensity score $P(D = 1 \mid X) = P(X)$, i.e. the probability for an individual to participate in a treatment given his observed covariates X, is one possible balancing score. Hence, if Assumption 1 holds, all biases due to observable components can be removed by conditioning on the propensity score (Imbens, 2004).

Corollary 1. *Unconfoundedness given the propensity score:* $Y(0), Y(1) \amalg D \mid P(X)$.[7]

Estimation Strategy

Given that CIA holds and assuming additionally that there is overlap between both groups, the PSM estimator for ATT can be written in general as

$$\tau_{ATT}^{PSM} = E_{P(X)|D=1}\{E[Y(1)|D = 1, P(X)] - E[Y(0)|D = 0, P(X)]\} \qquad (6)$$

To put it in words, the PSM estimator is simply the mean difference in outcomes over the common support, appropriately weighted by the propensity score distribution of participants. Based on this brief outline of the matching estimator in the general evaluation framework, we are now going to discuss the implementation of PSM in detail.

3. Implementation of Propensity Score Matching

3.1 *Estimating the Propensity Score*

When estimating the propensity score, two choices have to be made. The first one concerns the model to be used for the estimation, and the second one the variables to be included in this model. We will start with the model choice before we discuss which variables to include in the model.

Model Choice – Binary Treatment

Little advice is available regarding which functional form to use (see for example the discussion in Smith, 1997). In principle any discrete choice model can be used. Preference for logit or probit models (compared to linear probability models) derives from the well-known shortcomings of the linear probability model, especially the unlikeliness of the functional form when the response variable is highly skewed and predictions that are outside the [0, 1] bounds of probabilities. However, when the purpose of a model is classification rather than estimation of structural coefficients, it is less clear that these criticisms apply (Smith, 1997). For the binary treatment case, where we estimate the probability of participation versus nonparticipation, logit and probit models usually yield similar results. Hence, the choice is not too critical, even though the logit distribution has more density mass in the bounds.

Model Choice – Multiple Treatments

However, when leaving the binary treatment case, the choice of the model becomes more important. The multiple treatment case (as discussed in Imbens (2000) and Lechner (2001a)) consists of more than two alternatives, for example when an individual is faced with the choice to participate in job-creation schemes, vocational training or wage subsidy programmes or to not participate at all (we will describe this approach in more detail in Section 3.10). For that case it is well known that the multinomial logit is based on stronger assumptions than the multinomial probit model, making the latter the preferable option.[8] However, since the multinomial probit is computationally more burdensome, a practical alternative is to estimate a series of binomial models as suggested by Lechner (2001a). Bryson *et al.* (2002) note that there are two shortcomings regarding this approach. First, as the number of options increases, the number of models to be estimated increases disproportionately (for L options we need $0.5(L(L - 1))$ models). Second, in each model only two options at a time are considered and consequently the choice is conditional on being in one of the two selected groups. On the other hand, Lechner (2001a) compares the performance of the multinomial probit approach and series estimation and finds little difference in their relative performance. He suggests that the latter approach may be more robust since a mis-specification in one of the series will not compromise all others as would be the case in the multinomial probit model.

Variable Choice:

More advice is available regarding the inclusion (or exclusion) of covariates in the propensity score model. The matching strategy builds on the CIA, requiring that the outcome variable(s) must be independent of treatment conditional on the propensity score. Hence, implementing matching requires choosing a set of variables X that credibly satisfy this condition. Heckman *et al.* (1997a) and Dehejia and Wahba (1999) show that omitting important variables can seriously increase bias in resulting estimates. Only variables that influence simultaneously the participation decision and the outcome variable should be included. Hence, economic theory, a sound knowledge of previous research and also information about the institutional settings should guide the researcher in building up the model (see e.g., Sianesi, 2004; Smith and Todd, 2005). It should also be clear that only variables that are unaffected by participation (or the anticipation of it) should be included in the model. To ensure this, variables should either be fixed over time or measured before participation. In the latter case, it must be guaranteed that the variable has not been influenced by the anticipation of participation. Heckman *et al.* (1999) also point out that the data for participants and nonparticipants should stem from the same sources (e.g. the same questionnaire). The better and more informative the data are, the easier it is to credibly justify the CIA and the matching procedure. However, it should also be clear that 'too good' data is not helpful either. If $P(X) = 0$ or $P(X) = 1$ for some values of X, then we cannot use matching conditional on those X values to estimate a treatment effect, because persons with such characteristics either always or never receive treatment. Hence, the common support condition as stated in Assumption 2 fails and matches cannot be performed. Some randomness is needed that guarantees that persons with identical characteristics can be observed in both states (Heckman *et al.*, 1998b).

In cases of uncertainty of the proper specification, sometimes the question may arise whether it is better to include too many rather than too few variables. Bryson *et al.* (2002) note that there are two reasons why over-parameterized models should be avoided. First, it may be the case that including extraneous variables in the participation model exacerbates the support problem. Second, although the inclusion of nonsignificant variables in the propensity score specification will not bias the propensity score estimates or make them inconsistent, it can increase their variance.

The results from Augurzky and Schmidt (2001) point in the same direction. They run a simulation study to investigate PSM when selection into treatment is remarkably strong, and treated and untreated individuals differ considerably in their observable characteristics. In their set-up, explanatory variables in the selection equation are partitioned into three sets. The first set (set 1) includes covariates which strongly influence the treatment decision but weakly influence the outcome variable. Furthermore, they include covariates which are relevant to the outcome but irrelevant to the treatment decision (set 2) and covariates which influence both (set 3). Including the full set of covariates in the propensity score specification (full model including all three sets of covariates) might cause problems in small samples in terms of higher variance, since either some treated have to be discarded from the

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analysis or control units have to be used more than once. They show that matching on an inconsistent estimate of the propensity score (i.e. partial model including only set 3 or both sets 1 and 3) produces better estimation results of the ATE.

On the other hand, Rubin and Thomas (1996) recommend against 'trimming' models in the name of parsimony. They argue that a variable should only be excluded from analysis if there is consensus that the variable is either unrelated to the outcome or not a proper covariate. If there are doubts about these two points, they explicitly advise to include the relevant variables in the propensity score estimation.

By these criteria, there are both reasons for and against including all of the reasonable covariates available. Basically, the points made so far imply that the choice of variables should be based on economic theory and previous empirical findings. But clearly, there are also some formal (statistical) tests which can be used. Heckman *et al.* (1998a), Heckman and Smith (1999) and Black and Smith (2004) discuss three strategies for the selection of variables to be used in estimating the propensity score.

Hit or Miss Method

The first one is the 'hit or miss' method or prediction rate metric, where variables are chosen to maximize the within-sample correct prediction rates. This method classifies an observation as '1' if the estimated propensity score is larger than the sample proportion of persons taking treatment, i.e. $\hat{P}(X) > \overline{P}$. If $\hat{P}(X) \leq \overline{P}$ observations are classified as '0'. This method maximizes the overall classification rate for the sample assuming that the costs for the misclassification are equal for the two groups (Heckman *et al.*, 1997a).[9] But clearly, it has to be kept in mind that the main purpose of the propensity score estimation is not to predict selection into treatment as well as possible but to balance all covariates (Augurzky and Schmidt, 2001).

Statistical Significance

The second approach relies on statistical significance and is very common in textbook econometrics. To do so, one starts with a parsimonious specification of the model, e.g. a constant, the age and some regional information, and then 'tests up' by iteratively adding variables to the specification. A new variable is kept if it is statistically significant at conventional levels. If combined with the 'hit or miss' method, variables are kept if they are statistically significant and increase the prediction rates by a substantial amount (Heckman *et al.*, 1998a).

Leave-One-Out Cross-Validation

Leave-one-out cross-validation can also be used to choose the set of variables to be included in the propensity score. Black and Smith (2004) implement their model selection procedure by starting with a 'minimal' model containing only two variables. They subsequently add blocks of additional variables and compare the resulting mean squared errors. As a note of caution, they stress that this amounts to choosing the propensity score model based on goodness-of-fit considerations, rather than based on theory and evidence about the set of variables related to the participation decision and the outcomes (Black and Smith, 2004). They also point out an interesting trade-off in finite samples between the plausibility of the CIA and the variance of the estimates. When using the full specification, bias arises from selecting a wide bandwidth in response to the weakness of the common support. In contrast to that, when matching on the minimal specification, common support is not a problem but the plausibility of the CIA is. This trade-off also affects the estimated standard errors, which are smaller for the minimal specification where the common support condition poses no problem.

Finally, checking the matching quality can also help to determine the propensity score specification and we will discuss this point later in Section 3.4.

Overweighting some Variables

Let us assume for the moment that we have found a satisfactory specification of the model. It may sometimes be felt that some variables play a specifically important role in determining participation and outcome (Bryson *et al.*, 2002). As an example, one can think of the influence of gender and region in determining the wage of individuals. Let us take as given for the moment that men earn more than women and the wage level is higher in region A compared to region B. If we add dummy variables for gender and region in the propensity score estimation, it is still possible that women in region B are matched with men in region A, since the gender and region dummies are only a subset of all available variables. There are basically two ways to put greater emphasis on specific variables. One can either find variables in the comparison group who are identical with respect to these variables, or carry out matching on subpopulations. The study from Lechner (2002) is a good example for the first approach. He evaluates the effects of active labour market policies in Switzerland and uses the propensity score as a 'partial' balancing score which is complemented by an exact matching on sex, duration of unemployment and native language. Heckman *et al.* (1997a, 1998a) use the second strategy and implement matching separately for four demographic groups. That implies that the complete matching procedure (estimating the propensity score, checking the common support, etc.) has to be implemented separately for each group. This is analogous to insisting on a perfect match, e.g. in terms of gender and region, and then carrying out propensity score matching. This procedure is especially recommendable if one expects the effects to be heterogeneous between certain groups.

Alternatives to the Propensity Score

Finally, it should also be noted that it is possible to match on a measure other than the propensity score, namely the underlying index of the score estimation. The advantage of this is that the index differentiates more between observations in the extremes of the distribution of the propensity score (Lechner, 2000). This is

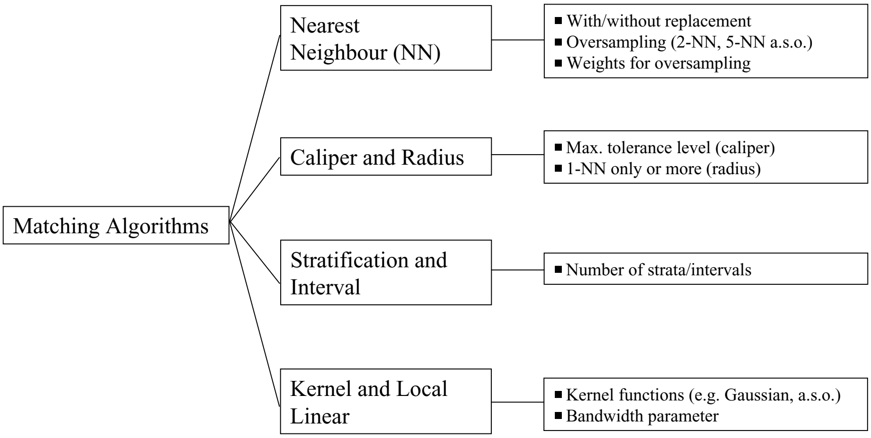

Matching Algorithms
Nearest Neighbour (NN)
■ With/without replacement
■ Oversampling (2-NN, 5-NN a.s.o.)
■ Weights for oversampling
Caliper and Radius
■ Max. tolerance level (caliper)
■ 1-NN only or more (radius)
Stratification and Interval
■ Number of strata/intervals
Kernel and Local Linear
■ Kernel functions (e.g. Gaussian, a.s.o.)
■ Bandwidth parameter

Figure 2. Different Matching Algorithms.

useful if there is some concentration of observations in the tails of the distribution. Additionally, in some recent papers the propensity score is estimated by duration models. This is of particular interest if the ‘timing of events’ plays a crucial role (see e.g. Brodaty *et al.*, 2001; Sianesi, 2004).

3.2 *Choosing a Matching Algorithm*

The PSM estimator in its general form was stated in equation (6). All matching estimators contrast the outcome of a treated individual with outcomes of comparison group members. PSM estimators differ not only in the way the neighbourhood for each treated individual is defined and the common support problem is handled, but also with respect to the weights assigned to these neighbours. Figure 2 depicts different PSM estimators and the inherent choices to be made when they are used. We will not discuss the technical details of each estimator here at depth but rather present the general ideas and the involved trade-offs with each algorithm.[10]

Nearest Neighbour Matching

The most straightforward matching estimator is nearest neighbour (NN) matching. The individual from the comparison group is chosen as a matching partner for a treated individual that is closest in terms of the propensity score. Several variants of NN matching are proposed, e.g. NN matching ‘with replacement’ and ‘without replacement’. In the former case, an untreated individual can be used more than once as a match, whereas in the latter case it is considered only once. Matching with replacement involves a trade-off between bias and variance. If we allow replacement, the average quality of matching will increase and the bias will decrease. This is of particular interest with data where the propensity score distribution is very different in the treatment and the control group. For example, if we have a lot of treated individuals with high propensity scores but only few comparison individuals with high propensity scores, we get bad matches as some of the high-score participants will get matched to low-score nonparticipants. This can be overcome by allowing replacement, which in turn reduces the number of distinct nonparticipants used to construct the counterfactual outcome and thereby increases the variance of the estimator (Smith and Todd, 2005). A problem which is related to NN matching without replacement is that estimates depend on the order in which observations get matched. Hence, when using this approach it should be ensured that ordering is randomly done.[11]

It is also suggested to use more than one NN (‘oversampling’). This form of matching involves a trade-off between variance and bias, too. It trades reduced variance, resulting from using more information to construct the counterfactual for each participant, with increased bias that results from on average poorer matches (see e.g. Smith, 1997). When using oversampling, one has to decide how many matching partners should be chosen for each treated individual and which weight (e.g. uniform or triangular weight) should be assigned to them.

Caliper and Radius Matching

NN matching faces the risk of bad matches if the closest neighbour is far away. This can be avoided by imposing a tolerance level on the maximum propensity score distance (caliper). Hence, caliper matching is one form of imposing a common support condition (we will come back to this point in Section 3.3). Bad matches are avoided and the matching quality rises. However, if fewer matches can be performed, the variance of the estimates increases.[12] Applying caliper matching means that an individual from the comparison group is chosen as a matching partner for a treated individual that lies within the caliper (‘propensity range’) and is closest in terms of propensity score. As Smith and Todd (2005) note, a possible drawback of caliper matching is that it is difficult to know *a priori* what choice for the tolerance level is reasonable.

Dehejia and Wahba (2002) suggest a variant of caliper matching which is called radius matching. The basic idea of this variant is to use not only the NN within each caliper but all of the comparison members within the caliper. A benefit of this approach is that it uses only as many comparison units as are available within the caliper and therefore allows for usage of extra (fewer) units when good matches are (not) available. Hence, it shares the attractive feature of oversampling mentioned above, but avoids the risk of bad matches.

Stratification and Interval Matching

The idea of stratification matching is to partition the common support of the propensity score into a set of intervals (strata) and to calculate the impact within each interval by taking the mean difference in outcomes between treated and control observations. This method is also known as interval matching, blocking and subclassification (Rosenbaum and Rubin, 1984). Clearly, one question to be answered is how many strata should be used in empirical analysis. Cochran (1968) shows that five subclasses are often enough to remove 95% of the bias associated with one single covariate. Since, as Imbens (2004) notes, all bias under unconfoundedness is associated with the propensity score, this suggests that under normality the use of five strata removes most of the bias associated with all covariates. One way to justify the choice of the number of strata is to check the balance of the propensity score (or the covariates) within each stratum (see e.g. Aakvik, 2001). Most of the algorithms can be described in the following way. First, check if within a stratum the propensity score is balanced. If not, strata are too large and need to be split. If, conditional on the propensity score being balanced, the covariates are unbalanced, the specification of the propensity score is not adequate and has to be respecified, e.g. through the addition of higher-order terms or interactions (see Dehejia and Wahba, 1999; Dehejia, 2005).

Kernel and Local Linear Matching

The matching algorithms discussed so far have in common that only a few observations from the comparison group are used to construct the counterfactual outcome of a treated individual. Kernel matching (KM) and local linear matching (LLM) are nonparametric matching estimators that use weighted averages of (nearly) all – depending on the choice of the kernel function – individuals in the control group to construct the counterfactual outcome. Thus, one major advantage of these approaches is the lower variance which is achieved because more information is used. A drawback of these methods is that possibly observations are used that are bad matches. Hence, the proper imposition of the common support condition is of major importance for KM and LLM. Heckman *et al.* (1998b) derive the asymptotic distribution of these estimators and Heckman *et al.* (1997a) present an application. As Smith and Todd (2005) note, KM can be seen as a weighted regression of the counterfactual outcome on an intercept with weights given by the kernel weights. Weights depend on the distance between each individual from the control group and the participant observation for which the counterfactual is estimated. It is worth noting that if weights from a symmetric, nonnegative, unimodal kernel are used, then the average places higher weight on persons close in terms of the propensity score of a treated individual and lower weight on more distant observations. The estimated intercept provides an estimate of the counterfactual mean. The difference between KM and LLM is that the latter includes in addition to the intercept a linear term in the propensity score of a treated individual. This is an advantage whenever comparison group observations are distributed asymmetrically around the treated observation, e.g. at boundary points, or when there are gaps in the propensity score distribution. When applying KM one has to choose the kernel function and the bandwidth parameter. The first point appears to be relatively unimportant in practice (DiNardo and Tobias, 2001). What is seen as more important (see e.g. Silverman, 1986; Pagan and Ullah, 1999) is the choice of the bandwidth parameter with the following

Table 1. Trade-offs in Terms of Bias and Efficiency.

Decision	Bias	Variance
Nearest neighbour matching:		
multiple neighbours/single neighbour	(+)/(−)	(−)/(+)
with caliper/without caliper	(−)/(+)	(+)/(−)
Use of control individuals:		
with replacement/without replacement	(−)/(+)	(+)/(−)
Choosing method:		
NN matching/Radius matching	(−)/(+)	(+)/(−)
KM or LLM/NN methods	(+)/(−)	(−)/(+)
Bandwidth choice with KM:		
small/large	(−)/(+)	(+)/(−)
Polynomial order with LPM:		
small/large	(+)/(−)	(−)/(+)

KM, kernel matching, LLM; local linear matching; LPM, local polynomial matching NN, nearest neighbour; increase; (+); decrease (−).

trade-off arising. High bandwidth values yield a smoother estimated density function, therefore leading to a better fit and a decreasing variance between the estimated and the true underlying density function. On the other hand, underlying features may be smoothed away by a large bandwidth leading to a biased estimate. The bandwidth choice is therefore a compromise between a small variance and an unbiased estimate of the true density function. It should be noted that LLM is a special case of local polynomial matching (LPM). LPM includes in addition to an intercept a term of polynomial order p in the propensity score, e.g. $p = 1$ for LLM, $p = 2$ for local quadratic matching or $p = 3$ for local cubic matching. Generally, the larger the polynomial order p is the smaller will be the asymptotic bias but the larger will be the asymptotic variance. To our knowledge Ham *et al.* (2004) is the only application of local cubic matching so far, and hence practical experiences with LPM estimators with $p \geq 2$ are rather limited.

Trade-offs in Terms of Bias and Efficiency

Having presented the different possibilities, the question remains of how one should select a specific matching algorithm. Clearly, asymptotically all PSM estimators should yield the same results, because with growing sample size they all become closer to comparing only exact matches (Smith, 2000). However, in small samples the choice of the matching algorithm can be important (Heckman *et al.*, 1997a), where usually a trade-off between bias and variance arises (see Table 1). So what advice can be given to researchers facing the problem of choosing a matching estimator? It should be clear that there is no ‘winner’ for all situations and that the choice of the estimator crucially depends on the situation at hand. The performance of different matching estimators varies case-by-case and depends largely on the data structure at hand (Zhao, 2000). To give an example, if there are only a few control observations, it makes no sense to match without replacement. On the other hand, if there are a lot of comparable untreated individuals it might be worth using more than one NN (either by oversampling or KM) to gain more precision in estimates. Pragmatically, it seems sensible to try a number of approaches. Should they give similar results, the choice may be unimportant. Should results differ, further investigation may be needed in order to reveal more about the source of the disparity (Bryson *et al.*, 2002).

3.3 *Overlap and Common Support*

Our discussion in Section 2 has shown that ATT and ATE are only defined in the region of common support. Heckman *et al.* (1997a) point out that a violation of the common support condition is a major source of evaluation bias as conventionally measured. Comparing the incomparable must be avoided, i.e. only the subset of the comparison group that is comparable to the treatment group should be used in the analysis (Dehejia and Wahba, 1999). Hence, an important step is to check the overlap and the region of common support between treatment and comparison group. Several ways are suggested in the literature, where the most straightforward one is a visual analysis of the density distribution of the propensity score in both groups. Lechner (2001b) argues that given that the support problem can be spotted by inspecting the propensity score distribution, there is no need to implement a complicated estimator. However, some guidelines might help the researcher to determine the region of common support more precisely. We will present two methods, where the first one is essentially based on comparing the minima and maxima of the propensity score in both groups and the second one is based on estimating the density distribution in both groups. Implementing the common support condition ensures that any combination of characteristics observed in the treatment group can also be observed among the control group (Bryson *et al.*, 2002). For ATT it is sufficient to ensure the existence of potential matches in the control group, whereas for ATE it is additionally required that the combinations of characteristics in the comparison group may also be observed in the treatment group (Bryson *et al.*, 2002).

Minima and Maxima Comparison

The basic criterion of this approach is to delete all observations whose propensity score is smaller than the minimum and larger than the maximum in the opposite group. To give an example let us assume for a moment that the propensity score lies within the interval [0.07, 0.94] in the treatment group and within [0.04, 0.89] in the control group. Hence, with the ‘minima and maxima criterion’, the common support is given by [0.07, 0.89]. Observations which lie outside this region are discarded from analysis. Clearly a two-sided test is only necessary if the parameter of interest is ATE; for ATT it is sufficient to ensure that for each participant a close nonparticipant can be found. It should also be clear that the common support condition is in some ways more important for the implementation of KM than it is for the implementation of NN matching, because with KM all untreated observations are used to estimate the missing counterfactual outcome, whereas with NN matching only the closest neighbour is used. Hence, NN matching (with the additional imposition of a maximum allowed caliper) handles the common support problem pretty well. There are some problems associated with the 'minima and maxima comparison', e.g. if there are observations at the bounds which are discarded even though they are very close to the bounds. Another problem arises if there are areas within the common support interval where there is only limited overlap between both groups, e.g. if in the region [0.51, 0.55] only treated observations can be found. Additionally problems arise if the density in the tails of the distribution is very thin, for example when there is a substantial distance from the smallest maximum to the second smallest element. Therefore, Lechner (2002) suggests to check the sensitivity of the results when the minima and maxima are replaced by the tenth smallest and tenth largest observation.

Trimming to Determine the Common Support

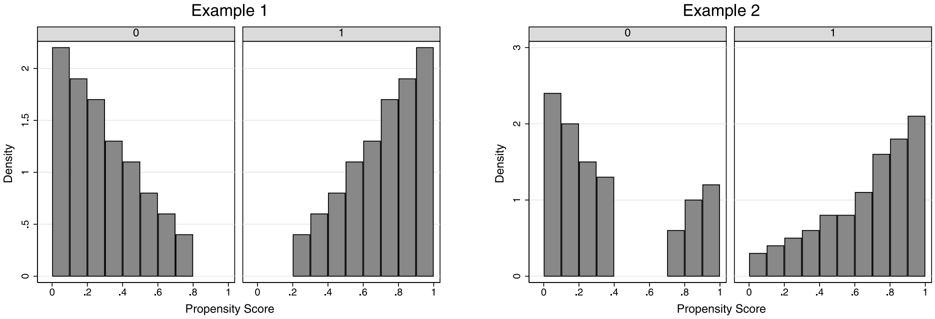
A different way to overcome these possible problems is described by Smith and Todd (2005).[13] They use a trimming procedure to determine the common support region and define the region of common support as those values of P that have positive density within both the $D = 1$ and $D = 0$ distributions, i.e.

$$\hat{S}_P = \{P : \hat{f}(P|D = 1) > 0 \text{ and } \hat{f}(P|D = 0) > 0\} \tag{7}$$

where $\hat{f}(P|D = 1) > 0$ and $\hat{f}(P|D = 0) > 0$ are nonparametric density estimators. Any P points for which the estimated density is exactly zero are excluded. Additionally – to ensure that the densities are strictly positive – they require that the densities exceed zero by a threshold amount q. So not only the P points for which the estimated density is exactly zero, but also an additional q percent of the remaining P points for which the estimated density is positive but very low are excluded:[14]

$$\hat{S}_{Pq} = \{Pq : \hat{f}(P|D = 1) > q \text{ and } \hat{f}(P|D = 0) > q\} \tag{8}$$

Figure 3 gives a hypothetical example and clarifies the differences between the two approaches. In the first example the propensity score distribution is highly skewed to the left (right) for participants (nonparticipants). Even though this is an extreme example, researchers are confronted with similar distributions in practice, too. With the 'minima and maxima comparison' we would exclude any observations lying outside the region of common support given by [0.2, 0.8]. Depending on the chosen trimming level q, we would maybe also exclude control observations in the interval [0.7, 0.8] and treated observations in the interval [0.2, 0.3] with the trimming approach since the densities are relatively low there. However, no large differences between the two approaches would emerge. In the second example we



The left side in each example refers to non-participants ($D=0$), the right side to participants ($D=1$).

Source: Hypothetical example

Figure 3. The Common Support Problem.

do not find any control individuals in the region [0.4, 0.7]. The 'minima and maxima comparison' fails in that situation, since minima and maxima in both groups are equal at 0.01 and 0.99. Hence, no observations would be excluded based on this criterion making the estimation of treatment effects in the region [0.4, 0.7] questionable. The trimming method on the other hand would explicitly exclude treated observations in that propensity score range and would therefore deliver more reliable results.[15] Hence, the choice of the method depends on the data situation at hand and before making any decisions a visual analysis is recommended.

Failure of the Common Support

Once one has defined the region of common support, individuals that fall outside this region have to be disregarded and for these individuals the treatment effect cannot be estimated. Bryson *et al.* (2002) note that when the proportion of lost individuals is small, this poses few problems. However, if the number is too large, there may be concerns whether the estimated effect on the remaining individuals can be viewed as representative. It may be instructive to inspect the characteristics of discarded individuals since those can provide important clues when interpreting the estimated treatment effects. Lechner (2001b) notes that both ignoring the support problem and estimating treatment effects only within the common support (subgroup effects) may be misleading. He develops an approach that can be used to derive bounds for the true treatment effect and we describe this approach in detail in Section 3.9.

3.4 *Assessing the Matching Quality*

Since we do not condition on all covariates but on the propensity score, it has to be checked if the matching procedure is able to balance the distribution of the relevant variables in both the control and treatment group. Several procedures to do so will be discussed in this section. These procedures can also, as already mentioned, help in determining which interactions and higher-order terms to include in the propensity score specification for a given set of covariates X. The basic idea of all approaches is to compare the situation before and after matching and check if there remain any differences after conditioning on the propensity score. If there are differences, matching on the score was not (completely) successful and remedial measures have to be done, e.g. by including interaction terms in the estimation of the propensity score. A helpful theorem in this context is suggested by Rosenbaum and Rubin (1983b) and states that

$$X \amalg D | P(D = 1|X) \tag{9}$$

This means that after conditioning on $P(D = 1|X)$, additional conditioning on X should not provide new information about the treatment decision. Hence, if after conditioning on the propensity score there is still dependence on X, this suggests either mis-specification in the model used to estimate $P(D = 1|X)$ (see Smith and Todd, 2005) or a fundamental lack of comparability between the two groups (Blundell *et al.*, 2005).[16]

Standardized Bias

One suitable indicator to assess the distance in marginal distributions of the X variables is the standardized bias (SB) suggested by Rosenbaum and Rubin (1985). For each covariate X it is defined as the difference of sample means in the treated and matched control subsamples as a percentage of the square root of the average of sample variances in both groups. The SB before matching is given by

$$\mathrm{SB}_{\mathrm{before}} = 100 \cdot \frac{\overline{X}_1 - \overline{X}_0}{\sqrt{0.5 \cdot (V_1(X) + V_0(X))}} \tag{10}$$

The SB after matching is given by

$$\mathrm{SB}_{\mathrm{after}} = 100 \cdot \frac{\overline{X}_{1M} - \overline{X}_{0M}}{\sqrt{0.5 \cdot (V_{1M}(X) + V_{0M}(X))}} \tag{11}$$

where X_1 (V_1) is the mean (variance) in the treatment group before matching and X_0 (V_0) the analogue for the control group. X_{1M} (V_{1M}) and X_{0M} (V_{0M}) are the corresponding values for the matched samples. This is a common approach used in many evaluation studies, e.g. by Lechner (1999), Sianesi (2004) and Caliendo *et al.* (2007). One possible problem with the SB approach is that one does not have a clear indication for the success of the matching procedure, even though in most empirical studies an SB below 3% or 5% after matching is seen as sufficient.

t-Test

A similar approach uses a two-sample *t*-test to check if there are significant differences in covariate means for both groups (Rosenbaum and Rubin, 1985). Before matching differences are expected, but after matching the covariates should be balanced in both groups and hence no significant differences should be found. The *t*-test might be preferred if the evaluator is concerned with the statistical significance of the results. The shortcoming here is that the bias reduction before and after matching is not clearly visible.

Joint Significance and Pseudo-R^2

Additionally, Sianesi (2004) suggests to reestimate the propensity score on the matched sample, i.e. only on participants and matched nonparticipants, and compare the pseudo-R^2s before and after matching. The pseudo-R^2 indicates how well the regressors X explain the participation probability. After matching there should be no systematic differences in the distribution of covariates between both groups and therefore the pseudo-R^2 should be fairly low. Furthermore, one can also perform a likelihood ratio test on the joint significance of all regressors in the probit or logit model. The test should not be rejected before, and should be rejected after, matching.

Stratification Test

Finally, Dehejia and Wahba (1999, 2002) divide observations into strata based on the estimated propensity score, such that no statistically significant difference between the mean of the estimated propensity score in both treatment and control group remain. Then they use *t*-tests within each strata to test if the distribution of X variables is the same between both groups (for the first and second moments). If there are remaining differences, they add higher-order and interaction terms in the propensity score specification, until such differences no longer emerge.

This makes clear that an assessment of matching quality can also be used to determine the propensity score specification. If the quality indicators are not satisfactory, one reason might be mis-specification of the propensity score model and hence it may be worth taking a step back, including for example interaction or higher-order terms in the score estimation and testing the quality once again. If after respecification the quality indicators are still not satisfactory, it may indicate a fundamental lack of comparability of the two groups being examined. Since this is a precondition for a successful application of the matching strategy, alternative evaluation approaches should be considered (see for example the discussion in Blundell *et al.*, 2005).

It should also be noted that different matching estimators balance the covariates to different degrees. Hence, for a given estimation of the propensity score, how the different matching methods balance the covariates can be used as a criterion to choose among them (leaving efficiency considerations aside).

3.5 *Choice-Based Sampling*

An additional problem arising in evaluation studies is that samples used are often choice-based (Smith and Todd, 2005). This is a situation where programme participants are oversampled relative to their frequency in the population of eligible persons. This type of sampling design is frequently chosen in evaluation studies to reduce the costs of data collection and to get a larger number of treated individuals (Heckman and Todd, 2004). We discuss this point briefly and suggest one correction mechanism introduced by Heckman and Todd (2004). First of all, note that under choice-based sampling weights are required to consistently estimate the probability of programme participation. Since population weights are not known in most choice-based datasets used in evaluation analysis the propensity score cannot be consistently estimated (Heckman and Todd, 2004). However, Heckman and Todd (2004) show that even with population weights unknown, matching methods can still be applied. This is the case because the odds ratio estimated using the incorrect weights (those that ignore the fact of choice-based samples) is a scalar multiple of the true odds ratio, which is itself a monotonic transformation of propensity scores. Hence, matching can be done on the (misweighted) estimate of the odds ratio (or of the log odds ratio). Clearly, with single NN matching it does not matter whether matching is performed on the odds ratio or the estimated propensity score (with wrong weights), since ranking of the observations is identical and therefore the same neighbours will be selected. However, for methods that take account of the absolute distance between observations, e.g. KM, it does matter (Smith and Todd, 2005).

3.6 *When to Compare and Locking-in Effects*

An important decision which has to be made in the empirical analysis is when to measure the effects. The major goal is to ensure that participants and nonparticipants are compared in the same economic environment and the same individual lifecycle position. For example, when evaluating labour market policies one possible problem which has to be taken into account is the occurrence of locking-in effects. The literature is dominated by two approaches, comparing the individuals either from the beginning of the programme or after the end of the programme. To give an example let us assume that a programme starts in January and ends in June. The latter of the two alternatives implies that the outcome of participants who reenter the labour market in July is compared with matched nonparticipants in July. There are two shortcomings to this approach. First, if the exits of participants are spread over a longer time period, it might be the case that very different economic situations are compared. Second, a further problem which arises with this approach is that it entails an endogeneity problem (Gerfin and Lechner, 2002), since the abortion of the programme may be caused by several factors which are usually not observed by the researcher.[17]

The above mentioned second approach is predominant in the recent evaluation literature (see e.g. Gerfin and Lechner, 2002; Sianesi, 2004) and measures the effects from the beginning of the programme. One major argument to do so concerns the policy relevance. In the above example the policy maker is faced with the decision to put an individual in January in a programme or not. He will be interested in the effect of his decision on the outcome of the participating individual in contrast with the situation if the individual would not have participated. Therefore comparing both outcomes from the beginning of the programme is a reasonable approach.

What should be kept in mind, however, is the possible occurrence of locking-in effects for the group of participants. Since they are involved in the programme, they do not have the same time to search for a new job as nonparticipants. The net effect of a programme consists of two opposite effects. First, the increased employment probability through the programme, and second, the reduced search intensity.[18] Since the two effects cannot be disentangled, we only observe the net effect and have to take this into account when interpreting the results. As to the fall in the search intensity, we should expect an initial negative effect from any kind of participation in a programme. However, a successful programme should overcompensate for this initial fall. So, if we are able to observe the outcome of the individuals for a reasonable time after the beginning/end of the programme, the occurrence of locking-in effects poses fewer problems but nevertheless has to be taken into account in the interpretation.

3.7 *Estimating the Variance of Treatment Effects*

Testing the statistical significance of treatment effects and computing their standard errors is not a straightforward thing to do. The problem is that the estimated variance of the treatment effect should also include the variance due to the estimation of the propensity score, the imputation of the common support and in the case of matching without replacement also the order in which the treated individuals are matched. These estimation steps add variation beyond the normal sampling variation (see the discussion in Heckman *et al.*, 1998b). For example, in the case of NN matching with one NN, treating the matched observations as given will understate the standard errors (Smith, 2000). Things get more complicated, since a much discussed topic in the recent evaluation literature centres around efficiency bounds of the different approaches and how to reach them. The aim of this section is to provide a brief overview of this ongoing discussion and more importantly to describe three approaches for the estimation of standard errors which are frequently used in the empirical literature.

Efficiency and Large Sample Properties of Matching Estimators

The asymptotic properties of matching and weighting estimators have been studied by for example Hahn (1998), Heckman *et al.* (1998b) and Abadie and Imbens (2006a). The results from Hahn (1998) are a good starting point for the efficiency discussion. He derives the semi-parametric efficiency bounds for ATE and ATT under various assumptions. He especially takes into account cases where the propensity score is known and where it has to be estimated.[19] Under the unconfoundedness assumption the asymptotic variance bounds for ATE and ATT are given by

$$\mathrm{Var}_{ATE} = E\left[\frac{\sigma_1^2(X)}{P(X)} + \frac{\sigma_0^2(X)}{1 - P(X)} + (E(Y(1)|X) - E(Y(0)|X) - \tau_{ATE})^2\right] \tag{12}$$

and

$$\mathrm{Var}_{ATT}^{PSunknown} = E\left[\frac{P(X)\sigma_1^2(X)}{E[P(X)]^2} + \frac{P(X)^2\sigma_0^2(X)}{E[P(X)]^2(1-P(X))} + \frac{(E(Y(1)|X) - E(Y(0)|X) - \tau_{ATT})^2 P(X)}{E[P(X)]^2}\right] \tag{13}$$

where $\sigma_D^2(X)$ are the conditional outcome variances for treated ($D = 1$) and untreated ($D = 0$) observations.

There is an ongoing discussion in the literature on how the efficiency bounds are achieved and if the propensity score should be used for estimation of ATT and ATE or not. In the above cited paper Hahn (1998) shows that when using nonparametric series regression, adjusting for all covariates can achieve the efficiency bound, whereas adjusting for the propensity score does not. Hirano *et al.* (2003) show that weighting with the inverse of a nonparametric estimate of the propensity score can achieve the efficiency bound, too. Angrist and Hahn (2004) use the results from Hahn (1998) as a starting point for their analysis and note that conventional asymptotic arguments would appear to offer no justification for anything other than full control for covariates in estimation of ATEs. However, they argue that conventional asymptotic results can be misleading and provide poor guidance for researchers who face a finite sample. They develop an alternative theory and propose a panel-style estimator which can provide finite-sample efficiency gains over covariate and propensity score matching.

Heckman *et al.* (1998b) analyse large sample properties of LPM estimators for the estimation of ATT. They show that these estimators are $\sqrt{n}$-consistent and asymptotically normally distributed. This holds true when matching with respect to X, the known propensity score or the estimated propensity score. They conclude that none of the approaches dominates the others *per se*. In the case of matching on the known propensity score, the asymptotic variance of Var_{ATT} is not necessarily smaller than that when matching on X.[20]

Abadie and Imbens (2006a) analyse the asymptotic efficiency of n nearest neighbour matching when n is fixed, i.e. when the number of neighbours does not grow with increasing sample size. They show that simple matching estimators include a conditional bias term of order $\mathrm{O}(N^{-1/k})$, where k is the number of continuous covariates. The bias does not disappear if k equals 2 and will dominate the large sample variance if k is at least 3. Hence, these estimators do not reach the variance bounds in (12) and (13) and are inefficient. They also describe a bias correction that removes the conditional bias asymptotically, making estimators $\sqrt{n}$-consistent. Additionally, they suggest a new estimator for the variance that does not require consistent nonparametric estimation of unknown functions (we will present that approach further below). Imbens (2004) highlights some caveats of these results. First, it is important to make clear that only continuous covariates should be counted in dimension k, since with discrete covariates the matching will be exact in large samples. Second, if only treated individuals are matched and the number of potential controls is much larger than the number of treated individuals, it can be justified to ignore the bias by appealing to an asymptotic sequence where the number of potential controls increases faster than the number of treated individuals.

Three Approaches for the Variance Estimation

There are a number of ways to estimate the variance of average treatment effects as displayed in equations (12) and (13). One is by 'brute force' (Imbens, 2004), i.e. by estimating the five components of the variance $\sigma_0^2(X)$, $\sigma_1^2(X)$, $E(Y(1)|X)$, $E(Y(0)|X)$ and $P(X)$ using kernel methods or series. Even though this is consistently possible and hence the asymptotic variance will be consistent, too, Imbens (2004) notes that this might be an additional computational burden. Hence, practical alternatives are called for and we are going to present three of them. Two of them, bootstrapping and the variance approximation by Lechner (2001a), are very common in the applied literature. Additionally, we are going to present a new method from Abadie and Imbens (2006a) that is based on the distinction between average treatment effects and sample average treatment effects.

Bootstrapping

One way to deal with this problem is to use bootstrapping as suggested for example by Lechner (2002). This method is a popular way to estimate standard errors in case analytical estimates are biased or unavailable.[21] Even though Imbens (2004) notes that there is little formal evidence to justify bootstrapping and Abadie and Imbens (2006b) even show that the standard bootstrap fails for the case of NN matching with replacement on a continuous covariate it is widely applied. An early example of use can be found in Heckman *et al.* (1997a) who report bootstrap standard errors for LLM estimators. Other application examples for bootstrapping are for example Black and Smith (2004) for NN and KM estimators or Sianesi (2004) in the context of caliper matching. Each bootstrap draw includes the reestimation of the results, including the first steps of the estimation (propensity score, common support, etc.). Repeating the bootstrapping R times leads to R bootstrap samples and R estimated average treatment effects. The distribution of these means approximates the sampling distribution (and thus the standard error) of the population mean. Clearly, one practical problem arises because bootstrapping is very time consuming and might therefore not be feasible in some cases.

Variance Approximation by Lechner:

An alternative is suggested by Lechner (2001a). For the estimated ATT obtained via NN matching the following formula applies:

$$\mathrm{Var}(\hat{\tau}_{ATT}) = \frac{1}{N_1}\mathrm{Var}(Y(1) \mid D = 1) + \frac{\Sigma_{j\in\{D=0\}}(w_j)^2}{(N_1)^2} \cdot \mathrm{Var}(Y(0) \mid D = 0) \tag{14}$$

where N_1 is the number of matched treated individuals and w_j is the number of times individual j from the control group has been used, i.e. this takes into account that matching is performed with replacement. If no unit is matched more than once, the formula coincides with the 'usual' variance formula. By using this formula to estimate the variance of the treatment effect at time *t*, we assume independent observations and fixed weights. Furthermore we assume homoscedasticity of the variances of the outcome variables within treatment and control group and that the outcome variances do not depend on the estimated propensity score. This approach can be justified by simulation results from Lechner (2002) who finds little difference between bootstrapped variances and the variances calculated according to equation (14).

Variance Estimators by Abadie and Imbens

To introduce this variance estimator, some additional notation is needed. Abadie and Imbens (2006a) explicitly distinguish average treatment effects given in Section 2 from sample average treatment effects. The latter estimators focus on the average treatment effects in the specific sample rather than in the population at large. Hence, the sample average treatment effect for the treated (SATT) is given by

$$\tau_{SATT} = \frac{1}{N_1} \sum_{i \in \{D=1\}} [Y_i(1) - Y_i(0)] \tag{15}$$

Abadie and Imbens (2006a) derived a matching variance estimator that does not require additional nonparametric estimation. The basic idea is that even though the asymptotic variance depends on the conditional variances $\sigma_1^2(X)$ and $\sigma_0^2(X)$, one actually need not estimate these variances consistently at all values of the covariates. Instead only the average of this variance over the distribution weighted by the inverse of $P(X)$ and $1 - P(X)$ is needed. The variance of SATT can then be estimated by

$$\text{Var}_{SATT} = \frac{1}{N_1} \sum_{i=1}^{N} \left(D_i - (1 - D_i) \cdot \frac{K_M(i)}{M} \right)^2 \hat{\sigma}_{D_i}^2(X_i) \tag{16}$$

where M is the number of matches and $K_M(i)$ is the number of times unit i is used as a match.

It should be noted that the estimation of the conditional variances requires estimation of conditional outcome variances $\sigma_D^2(X_i)$. Abadie and Imbens (2006a) offer two options. With the first option one assumes that the treatment effect is constant for all individuals i and that $\sigma_D^2(X_i)$ does not vary with X or D. This is the assumption of homoscedasticity, whereas heteroscedasticity is allowed in the second option, where it is explicitly allowed that $\sigma_D^2(X_i)$ differ in D and X.[22]

3.8 *Combined and Other Propensity Score Methods*

What we have discussed so far is the estimation of treatment effects under unconfoundedness with (propensity score) matching estimators. Imbens (2004) notes that one evaluation method alone is often sufficient to obtain consistent or even efficient estimates. However, combining evaluation methods is a straightforward way to improve their performance by eliminating remaining bias and/or improving precision. In this section we address three combined methods.

First, we introduce an estimator which combines matching with the DID approach. By doing so, a possible bias due to time-invariant unobservables is eliminated. Second, we present a regression-adjusted matching estimator that combines matching with regression. This can be useful because matching does not address the relation between covariates and outcome. Additionally, if covariates appear seriously imbalanced after propensity score matching (inexact or imperfect matching) a bias-correction procedure after matching may help to improve estimates. Third, we present how weighting on the propensity score can be used to obtain a balanced sample of treated and untreated individuals.[23]

Conditional DID or DID Matching Estimator

The matching estimator described so far assumes that after conditioning on a set of observable characteristics, (mean) outcomes are independent of programme participation. The conditional DID or DID matching estimator relaxes this assumption and allows for unobservable but temporally invariant differences in outcomes between participants and nonparticipants. This is done by comparing the conditional before–after outcome of participants with those of nonparticipants. DID matching was first suggested by Heckman *et al.* (1998a). It extends the conventional DID estimator by defining outcomes conditional on the propensity score and using semiparametric methods to construct the differences. Therefore it is superior to DID as it does not impose linear functional form restrictions in estimating the conditional expectation of the outcome variable and it reweights the observations according to the weighting function of the matching estimator (Smith and Todd, 2005). If the parameter of interest is ATT, the DID propensity score matching estimator is based on the following identifying assumption:

$$E[Y_t(0) - Y_{t'}(0)|P(X), D = 1] = E[Y_t(0) - Y_{t'}(0)|P(X), D = 0] \qquad (17)$$

where (t) is the post- and (t') is the pretreatment period. It also requires the common support condition to hold. If panel data on participants and nonparticipants are available, it can be easily implemented by constructing propensity score matching estimates in both periods and calculating the difference between them.[24] Smith and Todd (2005) find that such estimators are more robust than traditional cross-section matching estimators.

Regression-Adjusted and Bias-Corrected Matching Estimators

The regression-adjusted matching estimator (developed by Heckman *et al.*, 1997a, 1998a) combines LLM on the propensity score with regression adjustment on covariates. By utilizing information on the functional form of outcome equations and by incorporating exclusion restrictions across outcome and participation equation, it extends classical matching methods. Heckman *et al.* (1998b) present a proof of consistency and asymptotic normality of this estimator. Navarro-Lozano (2002) provides a nice example for an application by evaluating a popular training programme in Mexico.

In cases where (substantial) differences in covariates between matched pairs remain after matching, additional regression adjustments may be helpful to reduce such differences. If matching is not exact, there will be some discrepancies that lead to a potential bias. The basic idea of the bias-correction estimators is to use the difference in the covariates to reduce the bias of the matching estimator. Rubin (1973, 1979) first proposed several matched sample regression adjustments in the context of Mahalanobis metric matching and they have been more recently discussed by Abadie and Imbens (2006a) and Imbens (2004).

Weighting on the Propensity Score

Imbens (2004) notes that propensity scores can also be used as weights to obtain a balanced sample of treated and untreated individuals.[25] Such estimators can be written as the difference between a weighted average of the outcomes for the treated and untreated individuals, where units are weighted by the reciprocal of the probability of receiving treatment.[26] An unattractive feature of such estimators is that the weights do not necessarily add up to one. One approach to improve the propensity score weighting estimator is to normalize the weights to unity. If the propensity score is known, the estimator can directly by implemented. But, even in randomized settings where the propensity score is known, Hirano *et al.* (2003) show that it could be advantageous in terms of efficiency considerations to use the estimated rather than the ‘true’ propensity score. However, as Zhao (2004) notes, the way propensity scores are estimated is crucial when implementing weighting estimators and mis-specification of the propensity score may lead to substantial bias.[27]

3.9 *Sensitivity Analysis*

Checking the sensitivity of the estimated results becomes an increasingly important topic in the applied evaluation literature. We will address two possible topics for a sensitivity analysis in this section. First, we are going to discuss approaches that allow the researcher to assess the sensitivity of the results with respect to deviations from the identifying assumption. Second, we show how to incorporate information from those individuals who failed the common support restriction to calculate bounds of the parameter of interest (if all individuals from the sample at hand would have been included).

Deviations from Unconfoundedness or Unobserved Heterogeneity

We have outlined in Section 2 that the estimation of treatment effects with matching estimators is based on the unconfoundedness or selection on observables assumption. However, if there are unobserved variables which affect assignment into treatment and the outcome variable simultaneously, a ‘hidden bias’ might arise (Rosenbaum, 2002). It should be clear that matching estimators are not robust against this 'hidden bias'. Researchers become increasingly aware that it is important to test the robustness of results to departures from the identifying assumption. Since it is not possible to estimate the magnitude of selection bias with nonexperimental data, the problem can be addressed by sensitivity analysis. Even though the idea for such analyses reaches far back in the literature only a few applied studies take them into account. However, it seems that this topic has come back into the mind of applied researchers and will become more important in the next few years. The aim of this section is to give a brief overview of some of the suggested methods.[28]

One of the earliest examples for sensitivity analysis in the evaluation context can be found in Rosenbaum and Rubin (1983a). They propose to assess the sensitivity of ATE with respect to assumptions about an unobserved binary covariate that is associated both with the treatment and the response. The basic idea is that treatment is not unconfounded given the set of observable characteristics X but would be unconfounded given X and an unobservable covariate U. Based on different sets of assumptions about the distribution of U and its association with D and the outcomes $Y(0)$ and $Y(1)$ it is then possible to check the sensitivity of the results with respect to variations in these assumptions.

Imbens (2003) builds on this approach but does not formulate the sensitivity in terms of coefficients on the unobserved covariate and rather presents the sensitivity results in terms of partial R^2s. This eases the interpretation and additionally allows a comparison of the partial R^2s of the unobserved covariates to those for the observed covariates in order to facilitate judgements regarding the plausibility of values necessary to substantially change results obtained under exogeneity. Both approaches use a parametric model as the basis for estimating ATEs. Parametrization is not needed, however, in the following two approaches.

The first approach was proposed by Rosenbaum (2002) and has been recently applied in Aakvik (2001), DiPrete and Gangl (2004) and Caliendo *et al.* (2007). The basic question to be answered here is whether inference about treatment effects may be altered by unobserved factors. In other words, one wants to determine how strongly an unmeasured variable must influence the selection process in order to undermine the implications of matching analysis. To do so it is assumed that the participation probability π_i is not only determined by observable factors (x_i) but also by an unobservable component (u_i): $\pi_i = \Pr(D_i = 1 \mid x_i) = F(\beta x_i + \gamma u_i)$. γ is the effect of u_i on the participation decision. Clearly, if the study is free of hidden bias, γ will be zero and the participation probability will solely be determined by x_i. However, if there is hidden bias, two individuals with the same observed covariates x have differing chances of receiving treatment. Varying the value of γ allows the researcher to assess the sensitivity of the results with respect to 'hidden bias'. Based on that, bounds for significance levels and confidence intervals can be derived. (For details see Rosenbaum (2002) and Aakvik (2001). Becker and Caliendo (2007) provide an implementation in Stata).

A different approach was recently proposed by Ichino *et al.* (2006). It additionally allows assessment of the sensitivity of point estimates and specifically the sensitivity of ATT matching estimates. They derive point estimates of the ATT under different possible scenarios of deviation from unconfoundedness. To do so they impose values of the parameters that characterize the distribution of U. Given these parameters, the value of the confounding factor for each treated and control subject is predicted and the ATT is reestimated now including the influence of the simulated U in the set of matching variables. By changing the assumptions about the distribution of U, they can assess the robustness of the ATT with respect to different hypotheses on the nature of the confounding factor. Their approach also allows one to verify whether there exists a set of plausible assumptions on U under which the estimated ATT would be driven to zero by the inclusion of U in the matching set. By modelling the nature of U based on already existing variables in the data, it is possible to assess the robustness of the estimates with respect to deviations from unconfoundedness that would occur if observed factors were omitted from the matching set.

A somewhat different strategy is to focus on estimating the causal effect of a treatment that is known to have a zero effect, e.g. by relying on the presence of multiple control groups (see the discussion in Imbens (2004) for details). If one has a group of eligible and ineligible nonparticipants, the ‘treatment effect’ which is known to be zero can be estimated using only the two control groups (where the ‘treatment’ indicator then has to be a dummy for belonging in one of the two groups). Any nonzero effect implies that at least one of the control groups is invalid. However, as Imbens (2004) points out, not rejecting the test does not imply that the unconfoundedness assumption is valid, but makes it more plausible that it holds. A good example of such a comparison can be found in Heckman *et al.* (1997a).

Overall, it should be noted that none of the tests can directly justify the unconfoundedness assumption. However, they provide some scope for making the estimates more credible if the results are not sensitive to different assumptions about unobservables factors. Clearly, if the results turn out to be very sensitive the researcher might have to think about the validity of his/her identifying assumption and consider alternative strategies. In any case, these tests should be applied more frequently.

Failure of Common Support

In Section 3.3 we have presented possible approaches to implement the common support restriction. Those individuals that fall outside the region of common support have to be disregarded. But, deleting such observations yields an estimate that is only consistent for the subpopulation within the common support. However, information from those outside the common support could be useful and informative especially if treatment effects are heterogeneous.

Lechner (2001b) describes an approach to check the robustness of estimated treatment effects due to failure of common support. He incorporates information from those individuals who failed the common support restriction to calculate nonparametric bounds of the parameter of interest, if all individuals from the sample at hand would have been included. To introduce his approach some additional notation is needed. Define the population of interest with Ω which is some subset from the space defined by treatment status ($D = 1$ or $D = 0$) and a set of covariates

X. Ω^{ATT} is defined by $\{(D = 1) \times X\}$ and W^{ATT} is a binary variable which equals one if an observation belongs to Ω^{ATT}. Identification of the effect is desired for $\tau_{ATT}(\Omega^{ATT})$. Due to missing common support the effect can only be estimated for $\tau_{ATT}(\Omega^{ATT*})$. This is the effect ignoring individuals from the treatment group without a comparable match. Observations within common support are denoted by the binary variable W^{ATT*} equal to one. The subset for whom such effect is not identified is $\widetilde{\Omega}^{ATT}$.

Let $\Pr(W^{ATT*} = 1|W^{ATT} = 1)$ denote the share of participants within common support relative to the total number of participants and λ_0^1 be the mean of $Y(1)$ for individuals from the treatment group outside common support. Assume that the share of participants within common support relative to the total number of participants as well as ATT for those within the common support and λ_0^1 are identified. Additionally, assume that the potential outcome $Y(0)$ is bounded: $\Pr(\underline{Y} \leq Y(0) \leq \overline{Y}|W^{ATT*} = 0, W^{ATT} = 1) = 1$.[29] Given these assumptions, the bounds for ATT $\tau_{ATT}(\Omega^{ATT}) \in [\underline{\tau}_{ATT}(\Omega^{ATT}), \overline{\tau}_{ATT}(\Omega^{ATT})]$ can be written as

$$\begin{aligned}\underline{\tau}_{ATT}(\Omega^{ATT}) = \tau_{ATT}(\Omega^{ATT*})\Pr(W^{ATT*} = 1|W^{ATT} = 1)\\ + \left(\lambda_0^1 - \overline{Y}\right)[1 - \Pr(W^{ATT*} = 1|W^{ATT} = 1)]\end{aligned} \tag{18}$$

$$\begin{aligned}\overline{\tau}_{ATT}(\Omega^{ATT}) = \tau_{ATT}(\Omega^{ATT*})\Pr(W^{ATT*} = 1|W^{ATT} = 1)\\ + \left(\lambda_0^1 - \underline{Y}\right)[1 - \Pr(W^{ATT*} = 1|W^{ATT} = 1)]\end{aligned} \tag{19}$$

Lechner (2001b) states that either ignoring the common support problem or estimating ATT only for the subpopulation within the common support can both be misleading. He recommends to routinely compute bounds analysis in order to assess the sensitivity of estimated treatment effects with respect to the common support problem and its impact on the inference drawn from subgroup estimates.

3.10 *More Practical Issues and Recent Developments*

Before we conclude the paper in the next section, we will point out some additional topics which might be of relevance in applied research. What we have discussed so far is basically a static and binary evaluation framework where an individual can participate in one programme (or not). However, in most realistic evaluation settings this framework might not be appropriate, e.g. when evaluating the effects of labour market policies. First of all, researchers are usually not confronted with only one, but a different set of programmes (programme heterogeneity). Second, an unemployed can successively enter into different programmes as long as (s)he is unemployed. Finally, choosing the right control group and the problem of random programme starts is a recently much discussed topic in the evaluation literature, too. These issues as well as a short listing of available software tools to implement matching are discussed in this section.

Programme Heterogeneity

The standard evaluation framework as presented in Section 2 considers only two possible states for each individual, i.e. participation and nonparticipation. To account for programme heterogeneity, this approach has been extended by Imbens (2000) and Lechner (2001a) to the multiple treatment framework which considers the case of $L + 1$ mutually different and exclusive treatments. For every individual only one component of the $L + 1$ different outcomes $\{Y(0), Y(1), \ldots, Y(L)\}$ can be observed, leaving L as counterfactuals. Participation in treatment l is indicated by $D \in \{0, 1, \ldots, L\}$. The interest lies in the causal effect of one treatment relative to another treatment on an outcome variable. Even though Lechner (2001a) defines several parameters of interest, we will focus once again on the ATT. In the multiple treatment notation, that effect is defined as a pairwise comparison of the effects of the treatments m and l for an individual randomly drawn from the group of participants in m only:

$$\tau_{ATT}^{ml} = E[Y(m) - Y(l) \mid D = m] = E[Y(m) \mid D = m] - E[Y(l) \mid D = m] \quad (20)$$

As discussed in Section 2, the causal treatment effect in the presented framework is not identified. To overcome the counterfactual situation, the unconfoundedness assumption has to be adapted to the multiple treatment framework:

$$Y(0), Y(1), \ldots, Y(L) \amalg D \mid X \quad (21)$$

This assumption can be weakened when one is interested in pairwise programme comparisons only. If we further assume that those receiving treatment m have a counterpart in the comparison group, i.e. if there is common support, the counterfactual mean can be constructed as $E[Y(l) \mid D = m, X]$. Lechner (2001a) also shows that the generalization of the balancing property holds for the case of multiple treatments as well. To estimate τ_{ATT}^{ml} matching can be done by using the conditional choice probability of treatment m given either treatment m or l and covariates X as a balancing score:

$$P(D = m \mid X, D \in \{m, l\}) = \frac{P(D = m \mid X)}{P(D = m \mid X) + P(D = l \mid X)} \quad (22)$$

If the conditional choice probability is modelled directly, no information from subsamples other than those containing participants in m and l is needed and one is basically back in the binary treatment framework. Since the choice probabilities will not be known *a priori*, they have to be replaced by an estimate, e.g. a probit model. If all values of m and l are of interest, the whole sample is needed for identification. In that case either the binary conditional probabilities can be estimated or a structural approach can be used where a complete choice problem is formulated in one model and estimated on the full sample, e.g. with a multinomial probit model. We have discussed the (dis-)advantages of the multinomial modelling in comparison to discrete estimation of binomial models already in Section 3.1.

Sequential Matching Estimators

Extending the standard evaluation framework for the case where individuals can participate in subsequent treatments has been recently proposed by Lechner and Miquel (2005).[30] These 'programme careers' cannot be addressed properly in the basic framework. Problems occur because the assignment into a subsequent programme is not independent of the assignment into previous programmes. Additionally, outcomes in subsequent periods will be influenced by previous participation decisions. Hence, a dynamic selection problem arises. Most empirical work about dynamic selection problems ignores intermediate outcomes and treats the sequence participation as being determined from the start. Mainly, problems are circumvented by either estimating the effect of the first programme only (see e.g. Gerfin and Lechner, 2002) or applying the static framework subsequently (see e.g. Bergemann *et al.*, 2001). The sequential matching framework is a powerful tool and is applicable for situations where individuals can participate more than once in a programme and where it is possible to identify treatment sequences. It allows intermediate outcomes to play a role in the participation decision for sequential participation and thus allows estimation in a dynamic context. To our knowledge Lechner (2004) is the only application so far and hence practical experiences with sequential matching estimators are rather limited.

Choosing the Right Control Group – Random Programme Starts

Another important topic in applied evaluation research is to choose an appropriate control group. In the 'usual' evaluation set-up for matching estimators, we have a group of participants and a group of nonparticipants. Both groups are usually observed from a certain starting point t to an end point T. The researcher does not have any information outside this limited time interval. Controls are defined as those individuals who did not participate in any programme in $[t, T]$, whereas participants are those individuals who took part in a programme for a certain interval τ in $[t, T]$.

In a series of papers, Sianesi (2001, 2004) casts doubt if this standard approach is appropriate. She suggests a solution which is based on a redefinition of the control group. Instead of defining controls as those who never participate, she defines controls as those who did not participate until a certain time period. Hence, the corresponding parameter of interest in this setting is then defined as the effect of joining a programme now in contrast to waiting longer. Fredriksson and Johansson (2004) formalize her approach and argue that the standard way of defining a control group might lead to biased results, because the unconfoundedness assumption might be violated. The reason for this is that in the standard approach the treatment indicator itself is defined conditional on future outcomes. In fact, in the context of labour market policies it can be argued that an unemployed individual will join a programme at some time, provided his unemployment spell is long enough (Sianesi, 2004). Hence, if the reason for nonparticipation is that the individual has found a job before a participation in the programme was offered or considered, it leads to negatively biased effects.

Available Software to Implement Matching

The bulk of software tools to implement matching and estimate treatment effects is growing and allows researchers to choose the appropriate tool for their purposes. The most commonly used platform for these tools is Stata and we will present the three most distributed ones here. Becker and Ichino (2002) provide a programme for PSM estimators (*pscore, attnd, attnw, attr, atts, attk*) which includes estimation routines for NN, kernel, radius, and stratification matching. To obtain standard errors the user can choose between bootstrapping and the variance approximation proposed by Lechner (2001a). Additionally the authors offer balancing tests (blocking, stratification) as discussed in Section 3.4.

Leuven and Sianesi (2003) provide the programme *psmatch2* for implementing different kinds of matching estimators including covariate and propensity score matching. It includes NN and caliper matching (with and without replacement), KM, radius matching, LLM and Mahalanobis metric (covariate) matching. Furthermore, this programme includes routines for common support graphing (*psgraph*) and covariate imbalance testing (*pstest*). Standard errors are obtained using bootstrapping methods.

Finally, Abadie *et al.* (2004) offer the programme *nnmatch* for implementing covariate matching, where the user can choose between several different distance metrics. Variance approximations as proposed by Abadie and Imbens (2006a) are implemented to obtain standard errors of treatment effects.

4. Conclusion

The aim of this paper was to give some guidance for the implementation of propensity score matching. Basically five implementation steps have to be considered when using PSM (as depicted in Figure 1). The discussion has made clear that a researcher faces a lot of decisions during implementation and that it is not always an easy task to give recommendations for a certain approach. Table 2 summarizes the main findings of this paper and also highlights sections where information for each implementation step can be found.

The first step of implementation is the estimation of the propensity score. We have shown that the choice of the underlying model is relatively unproblematic in the binary case whereas for the multiple treatment case one should either use a multinomial probit model or a series of binary probits (logits). After having decided about which model to be used, the next question concerns the variables to be included in the model. We have argued that the decision should be based on economic theory, a sound knowledge of previous research and also information about the institutional settings. We have also presented several statistical strategies which may help to determine the choice. If it is felt that some variables play a specifically important role in determining participation and outcomes, one can use an 'overweighting' strategy, for example by carrying out matching on subpopulations.

The second implementation step is the choice among different matching algorithms. We have argued that there is no algorithm which dominates in all data

Table 2. Implementation of Propensity Score Matching.

Step	Decisions, questions and solutions	Section
1. Estimation of propensity score		
Model choice	◇ Unproblematic in the binary treatment case (logit/probit)	3.1
	◇ In the multiple treatment case multinomial probit or series of binomial models should be preferred	3.1
Variable choice	◇ Variables should not be influenced by participation (or anticipation) and must satisfy CIA	3.1
→ Economic issues	Choose variables by economic theory and previous empirical evidence	3.1
→ Statistical issues	'Hit or miss' method, stepwise augmentation, leave-one-out cross-validation	3.1
→ Key variables	'Overweighting' by matching on subpopulations or insisting on perfect match	3.1
2. Choice among alternative matching algorithms		
Matching algorithms	◇ The choice (e.g. NN matching with or without replacement, caliper or kernel matching) depends on the sample size, the available number of treated/control observations and the distribution of the estimated propensity score → Trade-offs between bias and efficiency!	3.2
3. Check overlap and common support		
Common support	◇ Treatment effects can be estimated only over the CS region!	3.3
→ Tests	Visual analysis of propensity score distributions	3.3
→ Implementation	'Minima and maxima comparison' or 'trimming' method Alternative: Caliper matching	3.3
4.1 Assessing the matching quality		
Balancing property	◇ Is the matching procedure able to balance the distribution of relevant covariates?	3.4
	◇ If matching was not successful go back to step 1 and include higher-order terms, interaction variables or different covariates	↩ Step 1

Table 2. *Continued*

Step	Decisions, questions and solutions	Section
	◇ After that, if matching is still not successful it may indicate a fundamental lack of comparability between treatment and control group	
	→ Consider alternative evaluation approaches	
→ Tests	Standardized bias, *t*-test, stratification test, joint significance and pseudo-R^2	3.4
4.2 Calculation of treatment effects		
Choice-based sample	◇ Sample is choice-based? Match on the odds ratio instead of the propensity score	3.5
When to compare	◇ Compare from the beginning of the programme to avoid endogeneity problems	3.6
	→ Pay attention to the possible occurrence of locking-in effects	3.6
Standard errors	◇ Calculate standard errors by bootstrapping or variance approximation	3.7
Combined methods	◇ Think about combining PSM with other evaluation methods to possibly eliminate remaining bias and/or improve precision	3.8
5. Sensitivity analysis		
Hidden bias	◇ Test the sensitivity of estimated treatment effects with respect to unobserved covariates	3.9
	→ If results are very sensitive reconsider identifying assumption and consider alternative estimators	
Common support	◇ Test the sensitivity of estimated treatment effects with respect to the common support problem	3.9
	→ Calculate Lechner bounds. If results are very sensitive reconsider variable choice	↩ Step 1

CS, common support; NN, nearest neighbour; CIA, conditional independence assumption.

situations and that the choice involves a trade-off between bias and efficiency. The performance of different matching algorithms varies case-by-case and depends largely on the data sample. If results among different algorithms differ substantially, further investigations may be needed to reveal the source of disparity.

The discussion has also emphasized that treatment effects can only be estimated in the region of common support. To identify this region we recommend to start with a visual analysis of the propensity score distributions in the treatment and comparison group. Based on that, different strategies can be applied to implement the common support condition, e.g. by 'minima and maxima comparison' or 'trimming', where the latter approach has some advantages when observations are close to the 'minima and maxima' bounds and if the density in the tails of the distribution is very thin.

Since we do not condition on all covariates but on the propensity score we have to check in the next step if the matching procedure is able to balance the distribution of these covariates in the treatment and comparison group. We have presented several procedures to do so, including SB, *t*-test, stratification test, joint significance and pseudo-R^2. If the quality indicators are not satisfactory, one should go back to step 1 of the implementation procedure and include higher-order or interaction terms of the existing covariates or choose different covariates (if available). If, after that, the matching quality is still not acceptable, this may indicate a lack of comparability of the two groups being examined. Since this is a precondition for a successful application of the matching estimator, one has to consider alternative evaluation approaches.

However, if the matching quality is satisfactory one can move on to estimate the treatment effects. The estimation of standard errors is a much discussed topic in the recent evaluation literature. We have briefly discussed (some) efficiency and large sample properties of matching estimators and highlighted that the discussion in this direction is not final yet. Keeping that in mind, we have introduced three approaches for the estimation of variances of treatment effects which are used, i.e. bootstrapping methods, the variance approximation proposed in Lechner (2001a) and the variance estimators proposed by Abadie and Imbens (2006a). Another important decision is 'when to measure the effects?' where we argue that it is preferable to measure the effects from the beginning of the treatment. Clearly, what has to be kept in mind for the interpretation is the possible occurrence of locking-in effects.

Finally, a last step of matching analysis is to test the sensitivity of results with respect to deviations from the identifying assumption, e.g. when there are unobserved variables which affect assignment into treatment and the outcome variable leading to a 'hidden bias'. We have pointed out that matching estimators are not robust against this bias and that researchers become increasingly aware that it is important to test the sensitivity of their results. If the results are sensitive and if the researcher has doubts about the validity of the unconfoundedness assumption he should either consider using alternative identifying assumptions or combine PSM with other evaluation approaches.

We have introduced some possible combinations in Section 3.8 where we presented the DID matching estimator, which eliminates a possible bias due to time-invariant unobservables, as well as regression-adjusted and bias-corrected matching estimators. All approaches aim to improve the performance of the estimates by eliminating remaining bias and/or improving precision. Last, in Section 3.10 we discussed some additional topics which might be of relevance in applied research, e.g. programme heterogeneity, sequential matching estimators and the choice of the right control group.

To conclude, we have discussed several issues surrounding the implementation of PSM. We hope to give some guidance for researchers who believe that their data are strong enough to credibly justify the unconfoundedness assumption and who want to use PSM.

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Notes

1. See e.g. Rubin (1974), Rosenbaum and Rubin (1983, 1985a) or Lechner (1998).
2. The decision whether to apply PSM or covariate matching (CVM) as well as to include the propensity score as an additional covariate into Mahalanobis metric matching will not be discussed in this paper. With CVM distance measures like the Mahalanobis distance are used to calculate similarity of two individuals in terms of covariate values and the matching is done on these distances. The interested reader is referred to Imbens (2004) or Abadie and Imbens (2006a) who develop covariate and bias-adjusted matching estimators and Zhao (2004) who discusses the basic differences between PSM and CVM.
3. Note that the stable unit treatment value assumption (SUTVA) has to be made (see Rubin (1980) or Holland (1986) for a further discussion of this concept). It requires in particular that an individual's potential outcomes depend on his own participation only and not on the treatment status of other individuals in the population. Peer-effects and general equilibrium effects are ruled out by this assumption (Sianesi, 2004).
4. For distributions of programme impacts, the interested reader is referred to Heckman *et al.* (1997b). Another parameter one might think of is the average treatment effect on the untreated (ATU): $\tau_{ATU} = E(\tau \mid D = 0) = E[Y(1) \mid D = 0] - E[Y(0) \mid D = 0]$. The treatment effect for those individuals who actually did not participate in the programme is typically an interesting measure for decisions about extending some treatment to a group that was formerly excluded from treatment.
5. See Smith (2000) for a discussion about advantages and disadvantages of social experiments.
6. See Heckman and Robb (1985), Heckman *et al.* (1999), Blundell and Costa Dias (2002) or Caliendo and Hujer (2006) for a broader overview of evaluation strategies including situations where selection is also based on unobservable characteristics.
7. Once again, to identify ATT it is sufficient to assume $Y(0) \amalg D \mid P(X)$.
8. Especially the 'independence from irrelevant alternatives' assumption (IIA) is critical. It basically states that the odds ratio between two alternatives is independent of other alternatives. This assumption is convenient for estimation but not appealing from an economic or behavioural point of view (for details see e.g. Greene, 2003).

9. See e.g. Breiman *et al.* (1984) for a theoretical discussion and Heckman *et al.* (1998a) or Smith and Todd (2005) for applications.
10. See Smith and Todd (2005) or Imbens (2004) for more technical details.
11. This shortcoming is circumvented by an optimal full matching estimator which works backwards and rearranges already matched treated individuals if some specific treated individual turns out to be a better (closer) match for an untreated previously matched individual (see Gu and Rosenbaum (1993) or Augurzky and Kluve (2007) for detailed descriptions).
12. It should be noted that the increase in the variance is due to the imposition of the common support and hence variance comparisons between matching estimators with and without caliper are not obvious.
13. The trimming method was first suggested by Heckman *et al.* (1997a, 1998a).
14. For details on how to estimate the cut-off trimming level see Smith and Todd (2005). Galdo (2004) notes that the determination of the smoothing parameter is critical here. If the distribution is skewed to the right for participants and skewed to the left for nonparticipants, assuming a normal distribution may be very misleading.
15. In a most recent paper Crump *et al.* (2005) point out that both methods presented here are somewhat informal in the sense that they rely on arbitrary choices regarding thresholds for discarding observations. They develop formal methods for addressing lack of support and especially provide new estimators based on a redefinition of the estimand.
16. Smith and Todd (2005) note that this theorem holds for any X, including those that do not satisfy the CIA required to justify matching. As such, the theorem is not informative about which set of variables to include in X.
17. It may be the case for example that a participant receives a job offer and refuses to participate because he thinks the programme is not enhancing his employment prospects or because lack of motivation. As long as the reasons for abortion are not identified, an endogeneity problem arises.
18. These ideas data back to Becker (1964) who makes the point that human capital investments are composed of an investment period, in which one incurs the opportunity cost of not working, and a payoff period, in which ones employment and/or wage are higher than they would have been without the investment.
19. Hahn (1998) shows that the propensity score does not play a role for the estimation of ATE, but knowledge of the propensity score matters for the estimation of ATT.
20. Whereas matching on X involves k-dimensional nonparametric regression function estimation (where $k = 1, \ldots, K$ are the number of covariates), matching on $P(X)$ only involves one-dimensional nonparametric regression function estimation. Thus from the perspective of bias, matching on $P(X)$ is preferable, since it allows $\sqrt{n}$-consistent estimation of τ_{ATT} for a wider class of models (Heckman *et al.*, 1998b).
21. See Brownstone and Valletta (2001) for a discussion of bootstrapping methods.
22. See Abadie and Imbens (2006a) and Abadie *et al.* (2004) for details about the derivation of the relevant formulas and some easy implementable examples.
23. Due to space constraints we cannot address all possible combinations. For a combination of propensity score methods with an instrumental variable approach the interested reader is referred to Abadie (2003), and how to combine DID with weighting on the propensity score has been recently proposed by Abadie (2005).
24. Smith and Todd (2005) present a variant of this estimator when repeated cross-section data are used instead of panel data. With repeated cross-section data the

identity of future participants and nonparticipants may not be known in t', Blundell and Costa Dias (2000) suggest a solution for that case.

25. See e.g. Imbens (2004) or Wooldridge (2004), Section 18.3.2, for a formal description of weighting on propensity score estimators.
26. See Imbens (2004) for a formal proof that this weighting estimator removes the bias due to different distributions of the covariates between treated and untreated individuals.
27. In the recent methodological literature several estimators have been proposed that combine weighting on propensity score estimators with other methods. Due to space limitations we cannot address these topics. The interested reader is referred to for example Hirano and Imbens (2002) who apply a combined weighting on propensity score and regression adjustment estimator in their analysis or Abadie (2005) who combines DID and weighting estimators.
28. See Ichino *et al.* (2006) or Imbens (2004) for a more detailed discussion of these topics.
29. For example, if the outcome variable of interest is a dummy variable, $Y(0)$ is bounded in [0, 1].
30. See Lechner and Miquel (2005) and Lechner (2004) for a sequential (three-periods, two-treatments) matching framework.

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ABSTRACT

The Cost Impact of Spam Filters: Measuring the Effect of Information System Technologies in Organizations*

More than 70% of global e-mail traffic consists of unsolicited and commercial direct marketing, also known as spam. Dealing with spam incurs high costs for organizations, prompting efforts to try to reduce spam-related costs by installing spam filters. Using modern econometric methods to reduce the selection bias of installing a spam filter, we deploy a unique data setting implemented at a German university to measure the costs associated with spam and the costs savings of spam filters. The applied methodological framework can easily be transferred to estimate the effect of other IS technologies (e.g., SAP) implemented in organizations. Our findings indicate that central IT costs are of little relevance since the majority of spam costs stem from employees who spend working time identifying and deleting spam. The working time losses caused by spam are approximately 1,200 minutes per employee per year; these costs could be reduced by roughly 35% through the installation of a spam filter mechanism. The individual efficiency of a spam filter installation depends on the amount of spam that is received and on the level of knowledge about spam.

JEL Classification: M12, M15

Keywords: spam, spam filter, treatment effects, propensity score matching

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Introduction

Spam[1] is defined as the use of electronic communication channels to send unsolicited bulk messages with commercial content indiscriminately to recipients (OECD 2005). However, whether an e-mail is perceived as spam depends on the preferences of the user; some believe that the opportunity offered is a good deal and will purchase the promoted product. Thus, spamming continues to be a profitable business. Even with low purchasing probabilities, spamming can be economically viable because the variable operating costs of using online communication channels are close to zero. Furthermore, already low market entry costs continue to decline because of strong price competition among e-mail list providers, which offer millions of validated e-mail addresses for prices less than US$50 (Sipior, Ward, and Bonner 2004; Hann et al. 2006).

Spammers rely on various business models, either selling their (validated) e-mail address lists to other spammers, or directly promoting their own or third-party products. Mostly, they advertise third-party products using a revenue share model. The profit depends on the product, timing of the campaign, opening rate, and purchase probability, which is influenced by the quality of the spam mail. Potentially high profits and low market entry barriers continuously attract spammers, even though legal actions against spam have been initiated by legislation (Zhang 2005). Since October 2003, more than 50% of the total global mail traffic has been classified as spam, and in January 2008, the quota was greater than 75% (Messagelabs 2008).

Spamming is accompanied by negative externalities, with the largest share of the costs associated with sending unsolicited bulk messages being borne by e-mail service providers and those

[1] The association between unsolicited bulk e-mail and the word "spam" derives from a comedy sketch by the British comedy group Monty Python, in which the name of Hormel Foods' meat product "spam" gets mentioned about a hundred times within just a few minutes.

who receive spam (Melville et al. 2006; Goodman, Cormack, and Heckerman 2007). As a result, large e-mail providers must handle billions of “abusive mails” per day (MAAWG 2007). Organizations complain about the costs of spam, which reduces their employees’ productivity by forcing employees to allocate their limited attention resources to the messages (Falkinger 2007). Thus, the spamming phenomenon affects individuals (in organizations) as well as economies on a global scale; in response, technical, market, and legal actions seek to reduce the costs of spam (OECD 2005; Joseph and Thevaranjan 2008).

Despite the strong interest in reducing the costs of spam, we find only limited academic research that addresses the effectiveness of anti-spam actions on costs (Joseph and Thevaranjan 2008; Pavlov, Melville, and Plice 2008). A variety of management-related studies conducted by consulting companies (e.g., Vircom 2004) measure a few indices on a corporate level and then generalize these costs or break them down to an individual level. However, precise measures of individual or corporate costs (e.g., from data centers) that assess the magnitude of the problem have not been subject to academic consideration. Most spam-related research focuses instead on mechanisms to reduce spam or its impact. This type of literature can be grouped in four streams: First, theoretical papers by economists analyze and model market mechanisms to overcome the externalities of spam by increasing the costs for the sender (e.g., Kraut et al. 2005). Second, significant research focuses on legal issues to increase the risk (and cost) for spammers (e.g., Zhang 2005). Third, another stream of literature addresses user perceptions of spam (e.g., Morimoto and Chang 2006). Fourth, literature found in the field of information systems and computer science contributes ways to enhance filtering technologies by identifying, marking, and filtering unsolicited e-mails (e.g., Cormack and Lynam 2007). The effect of market mechanisms and legal actions are long term and typically beyond the control of IS managers, whereas most individuals and organizations use filter technologies to reduce the amount of spam in inboxes. However, despite the widespread use of spam filters, no study—to our knowledge—empirically addresses the question whether or not spam filters or other countermeasures really reduce costs.

This dearth of research is especially surprising for two reasons: First, with regard to the corporate level, company investments in information technology involve high risk and unclear outcomes (Dewan, Shi, and Gurbaxani 2007; Dewan and Ren 2007). Second, although spam filters are designed to reduce the spam burden and thus spam-related costs, this desired effect does not necessarily occur; the net benefit of the adoption and usage of spam filters depends on the costs of installation and the sum of time losses due to updating and training the filter, as well as for checking the filter results regarding potential misclassifications (false positives/negatives). Thus, although spam filters help users identify spam, they also lead to new costs due to two reasons: (1) filter technology may not be sufficient enough to justify the recurring costs after installation (e.g., updates and training of the filter), and (2) substantial misclassifications of relevant e-mails occur because filters are not sufficiently trained by individuals. Therefore, the individual net cost effect remains an open issue, and it remains unclear whether spam filters do indeed reduce costs.

We address this research gap and pursue two major research aims. First, we measure the central and individual costs of spam in an organization to evaluate the magnitude of the spam problem. Second, we take a first step towards evaluating the efficiency of countermeasures by focusing on spam filters. Without a controlled experimental setting, evaluating the cost effects of spam filters in cross-sectional samples requires rigorous control for the presence of a selection bias, which could arise because respondents using a spam filter might have different characteristics than those who do not. For example: In our data we find that users of spam filters have *higher* spam costs than those who do not use a filter. This raises the question of causality: Do users install filters because of high spam costs, or do filters cause rather than reduce costs as noted above? Ignoring this selection effect would lead to biased results pertaining to the effect of spam filters with a typical cross-sectional sample. Therefore, we reduce this selection bias by drawing on a propensity score-matching procedure (Imbens 2004; Smith and Todd 2005).

To measure the effect of spam filters on the individual costs of spam, we collected data from a German university and differentiate between the individual costs of the employees and the costs to the university's central data center. Our data set comprises information regarding 1,000 employees.

Our research contributes to IS literature in two dimensions: First, the nature of our data set (which includes individual and organizational costs) enables us to provide an indication of the costs of spam on both individual and corporate levels, as well as to show the impact of spam filters on individual costs. Second, because of the likely presence of a sample selection bias in many IS research settings focusing on the impact of implemented IT interventions in organizations (e.g., the introduction of SAP), we demonstrate the application of propensity score matching, and provide guidelines and implications for its extension to other research questions. Thus, our methodological framework can be applied to many other settings that focus on estimating the success of IS interventions when experiments are not feasible.

The remainder of this article is organized as follows: In the next section, we present a brief overview of related research. We then introduce the method utilized to quantify the cost-saving effects of spam filters by correcting for selection bias. Section 4 summarizes the structure of the field experiment we conducted to estimate the costs of spam, and presents some descriptive statistics. We discuss our estimation results in section 5 and conclude with implications for theory and practice.

Related Research

Contrary to the public presence of the spam phenomenon, academic research has devoted minimal attention to researching spam-related costs. Previous publications include reports by the OECD (2005) and the European Union (2004). The latter categorizes costs incurred by spam into direct and indirect costs, distinguishing between five different cost components. *Direct costs* include (1) losses of working time and productivity caused by the need to delete spam and install and train filters, and (2) central costs that accrue in data centers and IT departments as a result of the installation of countermeasures. Furthermore, (3) direct costs may arise if Internet service providers (ISP) must adapt their capacities to respond to increased spam. *Indirect costs* refer to the effect of spam on e-mail usage: (4) e-mails can be erroneously identified as spam (false positives) or (5) might contain viruses or other potentially harmful features (European Union 2004). Several of these cost components are directly caused by the decision to adopt a spam filter: the installation and training of filter mechanisms, the central costs, and the control of the spam filter especially for false positives. Only if these costs do not exceed the cost savings achieved by the filter will a spam filter mechanism have the desired effect.

Despite these numerous consequences, academic research invests little effort into quantifying spam-related costs, especially with regard to possible cost-saving (or cost-causing) effects associated with the widespread use of spam filters. Existing research primarily addresses ways to reduce the amount of spam and can be grouped into four streams. The first major stream addresses a key characteristic of electronic communication, namely, the low marginal costs of e-mail distribution. This characteristic represents the primary reason for the existence of spam, because in the offline world, the sender-pays system prevents advertisers from engaging in heavy spamming. In the online world however, the sender does not bear significant costs for excessive mailings; instead, those costs are externalized to ISPs and recipients, which characterizes e-mailing as a digital commons (Melville et al. 2006). Economists attempt to reduce the potential for externalizing the costs associated with e-mailing by confronting spammers with greater e-mailing costs. This would make spam less attractive, thus changing the economics of e-mailing. For example, one approach involves e-mail postage (Kraut et al. 2005), and another proposes a bonded sender program that requires senders to deposit a certain amount of money if not listed on a white list. If the recipient declares the e-mail to be spam, the deposit gets retained (Joseph and Thevaranjan 2008). Despite their theoretical efficiency, these and other comparable approaches could not yet be implemented. The considerable coordination effort associated with these measures makes it doubtful whether this gap will change in the near future.

The second cluster of literature deals with legal measures against spam. However, these measures have the inherent limitation that, in many cases, spammers set the pace for technical developments (Melville et al. 2006). Furthermore, legal measures are sustainable only if they are coordinated on a global basis. Since such coordination is rare, it cannot be expected that legal countermeasures will have a sustainable impact on costs (Zhang 2005).

A strong basis in the third literature stream suggests the implementation of technical measures against spam, whether implemented centrally (e.g., IT department or ISP) or decentralized (on each user's computer). Research focuses on technical issues such as blocking IP numbers, filtering e-mails, or authentication mechanisms (e.g., Sahami et al. 1998; Park and Deshpande 2006; Duan, Dong, and Gopalan 2007; Cormack and Lynam 2007).

A nascent, fourth stream of literature explores user perceptions of the growing spam burden by measuring the attitude or inconvenience costs of spam (Yoo, Shin, and Kwak 2006; Morimoto and Chang 2006). However, none of these publications addresses the effect of technical measures on the development of spam-related costs. Our research relates to the last two groups of publications, in that we focus on both the costs and user perceptions of filter mechanisms.

Method

Our research goal is to shed light on the unresolved issue of whether the installation of a spam filter reduces working time losses experienced by employees. A suitable framework to address such a question is the potential outcome approach also known as the Roy-Rubin model (Roy 1951; Rubin 1974). To measure the *individual causal effect* of a spam filter we seek to compare working times – with and without a spam filter – for the same user at the same time. Let the potential outcomes – here working time losses in minutes – be defined as $Y_i(D_i)$ for each individual i and let D_i denote the treatment indicator – here: installation of a spam filter. The *individual causal effect* is simply the difference between both potential outcomes, hence: $Y_i(1)-Y_i(0)$.[2] Unfortunately, one of these potential outcomes is unobservable or counterfactual, so instead, we consider comparing the mean working time losses of employees before and after they install the spam filter. However, relying solely on this approach would also be problematic, because employees could change their behavior, e.g., by making an initial online purchase or subscribing to a newsletter. Furthermore, external circumstances might change as well, such as an overall increase in Internet usage due to new and faster connections. Therefore, solely comparing a situation today (installed spam filter) with a situation in the past (no spam filter) could be very misleading as a result of unobserved effects over time.

Another approach is to compare the mean working times of those employees who have installed a spam filter (i.e., the "treatment group" hereafter) and those who have not ("control

[2] See e.g. Heckman, LaLonde, Smith (1999) or Imbens (2004) for a detailed discussion of the evaluation framework.

group"). In an experimental setting, i.e., where the installation of spam filters is randomly assigned, this would be a feasible strategy (Harrison and List 2004). However, such an experimental setting is not an option in the case of spam filters or comparable technological investments for most companies. Further, our empirical data (see Tables 3-5 in the following Section) indicates that users with a spam filter differ in more aspects then just the decision to install a filter, such that simply comparing mean working times of the treatment and the control group would yield a biased estimate of the *average treatment effect of the treated (ATT)* denoted by τ_{ATT}:

$$E[Y(1) \mid D=1] - E[Y(0) \mid D=1] = \tau_{ATT} + E[Y(0) \mid D=1] - E[Y(0) \mid D=0]\,. \qquad (1)$$

The difference between the left-hand expression and the τ_{ATT} can be called the *"self-selection bias"*. It is reasonable to assume that users with high spam-related costs have a higher propensity to install spam filters. For example, those who use their e-mail frequently probably differ from those who use it irregularly. To make meaningful comparisons and estimate the causal effects of the spam filter, we must find a proper substitute for the unobservable component $E[Y(0) \mid D=0]$.

To address the self-selection problem, we assume that potential working time losses are independent of the installation decision, given a set of relevant, observable variables *X* (*Conditional Independence Assumption).*[3] By referring to *relevant* variables, we mean that they simultaneously influence the decision to install a spam filter and the outcome variable. So selection must be solely due to observable variables, which is generally a strong assumption. Even though we are confident that our data covers the crucial *X* variables to justify this assumption, we subsequently test the sensitivity of our results to this assumption. With a large set of variables *X* and a small sample

[3] See Imbens (2004) for an overview of different nonparametric estimation approaches to average treatment effects under exogeneity.

of users (as is the case in our data), it is difficult to find users from the control group who have exactly the same variable values as each user in the treatment group. Therefore, we follow Rosenbaum and Rubin (1983) who have shown that it is sufficient to use the propensity score *P(X)* – here: the probability of installing a spam filter – instead of the whole set of observed characteristics *X* in order to balance the distribution of covariates between both groups.

The basic idea behind propensity score matching (PSM) is to approximate the counterfactual working times of individuals in the treatment group by finding similar users in terms of their propensity score values in the control group. Formally, the PSM estimator can be written as:

$$\tau_{ATT}^{PSM} = E_{P(X)|D=1}[Y(1) \mid D=1, P(X)] - E[Y(0) \mid D=0, P(X)]\,. \tag{2}$$

It is simply the mean difference in outcomes over the common support, appropriately weighted by the propensity score of the treatment group individuals. Restricting the comparison to individuals who fall inside the region of common support ensures that only comparable individuals, whose propensity score values overlap, are used to estimate the treatment effect. To ensure that users from the control group have a positive probability of belonging to the treatment group, we further assume that *P(D=1|X) < 1 (Overlap Assumption).*

Based on these two assumptions we can use PSM to estimate the average treatment effect of the treated (Heckman et al. 1998). The most straightforward matching estimator is nearest neighbor (NN) matching. For each user from the treatment group, we choose one user from the control group who is closest in terms of the propensity score. In addition, NN matching can be done with or without replacement. In the former case, a user from the control group can appear more than once as a matching partner; in the latter case a user is considered only once. Whereas NN matching algorithms use only a few observations from the control group to construct the counterfactual outcome for each treated individual, Kernel matching (KM) is a nonparametric matching estimator that uses weighted averages of (nearly) all individuals from the control group. Because KM estimators use more of the available information to estimate causal effects, lower variances are achieved. However, this method also employs users from the control group, though they are potentially poor matches reflecting the trade-off between bias and efficiency. It should be clear that the imposition of the common support condition is very important for KM. Different criteria for imposing common support are available[4]; we use the "MinMax" criterion, according to which users from the treatment group whose PS is higher (smaller) than the maximum (minimum) PS in the control group are dropped from the analysis. This highlights the great advantage of PSM compared with ordinary least squares regression (OLS). With PSM and the common support condition, the treatment effects are estimated only within the common support, so individuals for individuals without comparable matches are excluded. The estimated treatment effect must then be interpreted over the region of common support.

Below we briefly describe the data that was gathered to determine the cost-saving effects of the individual decision to install a spam filter.

Research Design and Data

We conduct our research at a German university with approximately 8,000 employees (including the university's hospitals), whose size resembles that of a medium-sized company.[5] A unique advantage of this setting results from the integrated structure of the institution, which combines most sources for spam-related costs into one organization: the university serves as an e-mail provider, operates its own data center, and employs a sufficiently high number of computer users. Our data collection can thus be restricted to two points of measurement within the universi-

[4] See Smith and Todd (2005) and Lechner (2002) for an overview.

[5] Universities previously have served as research settings for spam research (e.g., Melville et al. 2006).

ty: (1) We measure all central costs incurred by the university data center, where all computer-related tasks are centralized, and (2) we contact 5,000 university employees (excluding the university's hospitals) to measure the individual costs using an online questionnaire that also contains a set of covariates.

Central Costs

We collect information about the expenses incurred by the provider through interviews with IT experts from the university data center. The data center had reacted to its increasing spam burden by setting up an infrastructure based on free open source software (SpamAssassin) that checks all incoming e-mails for spam properties before labeling them in accordance with their spam probability and forwarding them to recipients. On average, the data center processes 170,000 e-mails per day from outside the university; in 2005 and 2006, the spam quota was about 90%.

This infrastructure creates expenses in four categories. Prior to the installation, organizational and administrative tasks had to be performed to obtain clearance from all relevant organizational units. The expenses for hard- and software were accompanied by labor costs for installing the system and training of the staff. In addition, the initiative against spam generates recurring costs, because the data center regularly provides support to the university's e-mail users. Furthermore, all anti-spam measures must be controlled for efficiency and are subject to constant further development. These activities together create expenses on a regular basis. For the purpose of comparability, we aggregate these regular costs for a period of one year. Therefore, the sum of all costs (Table 1) equals Euro 15,120 for the first year after and including installation.

>> Insert Table 1 about here <<

Individual Measures

Our second measure pertains to the individual costs to the employees of the university, whom we contacted by e-mail in the winter season 2004/2005 to invite them to fill out an online questionnaire. We contacted 5,000 employees and received exactly 1,000 responses (20%). We note that 52.7% of the 1,000 respondents already had adopted a spam filter.

Adoption research (e.g., theory of reasoned action, technology acceptance model) suggests that the perceived utility of an innovation drives adoption behavior—in this case, spam filter installation. We adopt this view and assume that the propensity to install a spam filter is strongly driven by the amount of spam an individual receives. We use this measure as a proxy for the perceived utility of a countermeasure. Furthermore, we measure demographic variables and the individual's own spam-prevention mechanisms. We control for psychographic variables related to the user's reactance to spam and measure the level of distribution of his or her e-mail address to others. We rely on these variables to analyze each individual's decision to install a spam filter.

Costs. On the basis of interviews we conducted prior to launching the questionnaire, we separate recurring from nonrecurring costs. *Recurring* costs refer to the daily time involved in deleting spam and controlling spam filter results for false positives. For the 1,000 respondents, these activities take up an average of 4.87 minutes per day. The *nonrecurring* costs include inquiries about the spam filter and installation by the respondent or an assistant. We aggregate all time expenditures into an index that covers the costs for one year, assuming that the average employee in Germany works approximately 250 days. Table 2 reports the respective time expenditures, indicating an average time loss of more than 1,200 minutes per year. Direct monetary costs on the individual level are not included, since the installation of the university spam filter did not require any payments by the respondents.

Table 2 also shows significantly greater time losses (1,597 minutes per year) for the 527 participants that installed a spam filter (D = 1) compared with the loss of 858 minutes for the 473 participants without one (D = 0). This finding suggests that either a spam filter causes working time losses, or that users with high costs have a stronger inclination to install a filter. The latter explanation would suggest a sample selection bias. Thus, to measure the potential cost savings of spam filters for the 1,000 users, we must reduce the selection bias using appropriate methods.

>> Insert Table 2 about here <<

Individual Factors. We assume that the most important driver of the adoption decision is the amount of spam that each person is confronted with. We further assume heterogeneity exists in individuals' approaches for dealing with spam. We therefore include variables that capture actions related to spam prevention, such as whether the e-mail address gets used strategically to prevent spam. Furthermore, we ask how respondents identify an e-mail as spam (Table 3). The descriptive analysis shows significant differences between users with and without spam filters in several dimensions. For example, significant differences in age and gender emerge between the two groups; we also identify strong differences in the quantity of spam mails received, such that those with a spam filter receive more than three times as much spam as those without. Spam filter users have a higher propensity to use alternative e-mail addresses and request to be removed from mailing lists more often. Finally, spam filter users rely more on inspecting the subject line before they open e-mails.

>> Insert Table 3 about here <<

Reactance. Spam not only reduces productivity in the workplace, but can also be perceived as intrusive. Prior research associates unsolicited mails with a perceived loss of control that can lead to the psychological effect of reactance (Morimoto and Chang 2006). If spam is perceived to be intrusive, thus leading to a sense of loss of control, the recipient might be inclined to take actions to restore his or her original state, which in this case would be a spam-free environment. Therefore, we control for behavioral changes caused by reactance; we also measure individual spam sensitivity to control for possible influences and account for the perceived degree of loss of control (Table 4). This measure therefore includes properties that, according to the individual, constitute the bothersome factors of spam.

The descriptive results indicate that particularly users with spam filters feel that they have wasted significantly more time with, and lost confidence in, the e-mail medium, which then increases reactance because they feel bothered by spam. We also find that filter users are significantly more sensitive to what they perceive as spam (e.g., fun mail, ads from business partners, large attachments). Furthermore, we detect significant differences in the perceptions of spam properties, such that users without spam filters note significantly higher fears about the potential hazards of spam.

>> Insert Table 4 about here <<

Distribution. The last group of variables controls for the usage habits associated with the e-mail address. We capture the degree to which the e-mail address has been distributed to known or unknown contacts, with the assumption that a "generous" distribution of the e-mail address leads to a considerably higher spam load, which in turn increases the probability of spam filter installation (Table 5). The descriptive measures indicate no differences in the distribution of the e-mail address to known contacts, but we find significantly higher distribution levels to unknown contacts by those who have installed spam filters.

>> Insert Table 5 about here <<

Cost Analysis

Group Characteristics before Matching

The average working time loss for users with spam filters is 1,597 minutes; for those without it is 858 minutes. A simple mean comparison suggests that a spam filter increases working time losses by 739 minutes. However, the groups differ in their observed characteristics, as previously stated. Users who have installed a spam filter appear to be significantly older and more likely to be men. They receive more e-mails (solicited and unsolicited) and exhibit a higher information level about spam (see Table 3). As Table 4 reveals, they also have a greater perception that the amount of spam mails, wasted time to control spam mails, and mail handling time have risen in general. Furthermore, they are less inclined to believe that spam mails can damage their personal computers. Finally, these users publish their e-mail addresses on Web sites, in online directories, or in online forums more frequently (see Table 5). The large differences in observed characteristics indicate that a simple mean comparison of working time losses between both groups cannot yield an unbiased estimate that answers the question of how much a spam filter can reduce working time losses.[6]

Propensity Score Estimation[7]

The first step of PSM is to estimate the propensity score, which we do using a binary logit model.[8] Our dependent variable equals 1 for users that installed a spam filter and 0 otherwise.

[6] Although it might seem to be a natural solution to control for other factors contributing to the installation decision by running a regression analysis to determine the cost effects of spam filters, this will not solve the issue: standard regression analysis does not implement a common support condition, so users with diverse characteristics are compared, and estimates are extrapolated even to regions in which common support (and number of observations) is low. Because of this disadvantage, we implement propensity score matching.

[7] See Caliendo and Kopeinig (2008) for practical guidance on how to implement propensity score matching.

Table 6 presents the results of the propensity score estimation, including the previously derived explanatory variables.[9] We drop those respondents for whom we lack information about the key information spam properties from the analysis, reducing the number of observations to 520 users who have installed a spam filter and 440 users have not.

>> Insert Table 6 about here <<

With this logit specification, we achieve a hit rate of 73.1%.[10] However, the aim of the propensity score estimation is not to maximize the hit rate but rather to balance the covariates between both groups, which we subsequently test by calculating standardized biases. Age, gender[11], and the number of e-mails and spam mails received all significantly affect the decision to install a spam filter. Furthermore, factors that we relate to reactance, such as the increase in time expenditures for handling e-mails, the perceived increase of received spam mails, or the perception that spam mails are harmful to personal computers, increase the probability of installing a spam filter. The level of information about spam also positively affects installation decision.

The propensity score distribution obtained from the logit estimation is depicted in Figure 1, which indicates that the PS distribution differs considerably between the treatment and the control group. Hence, NN matching algorithms without replacement would create poor matches due to the high-score users from the treatment group, which likely get matched to low-score users from the control group. The PS interval of treated (untreated) users is [0.033 – 0.999] ([0.048 –

[8] In the case of a binary treatment, the estimation with either a logit or a probit model should yield similar results. We also estimate the propensity score using a probit model and obtain similar values.

[9] We also test for multicollinearity. All variables have variance inflation factor values < 10.

[10] Hit rates are computed as follows: If the estimated propensity score is greater than the sample proportion of users that have installed a spam filter (i.e. $\hat{P}(X) > \overline{P}$), observations are classified as *1*. If $\hat{P}(X) \leq \overline{P}$, observations are classified as *0*.

[11] Note that the sign for gender is now negative (as opposed to the bivariate analysis, Table 3), indicating that male users have a lower propensity to install a filter if a multivariate analysis is deployed that controls for relevant factors (Table 6).

0.931]). Hence, the common support (based on the MinMax criterion) lies between 0.048 and 0.931; consequently, 74 treated users (treated off support) had to be dropped from our analysis.

>> Insert Figure 1 about here <<

Matching Results

We present three different matching estimators in our analysis: single NN matching (matching estimator A hereafter), single NN matching with common support condition (matching estimator B), and KM with common support condition (matching estimator C). For the KM estimator we use an Epanechnikov kernel function with a bandwidth parameter, according to Silverman (1986), of 0.06. [12] In Table 7, we present our matching results. All estimated effects are negative, which indicates that the installation of a spam filter lowers average working time losses, regardless of the algorithm chosen. However, the absolute effects differ between the matching algorithms.

Matching estimator A (NN matching) does not impose the common support condition, resulting in an effect of -814.48 minutes. That is, the effect of installing a spam filter reduces working time losses by roughly 800 minutes. However, this result must be treated with caution for two reasons: First, no individuals are dropped from the analysis, so that even treated individuals that cannot be properly compared with untreated users are used to measure the effect, and some control individuals appear repeatedly; for example, one member of the control group gets used 72 times. Second, any interpretation of these results should be preceded by an evaluation of matching quality. To determine whether the matching procedure balances the distribution of covariates

[12] We have tested several different bandwidths and distributions that all yield similar results. Detailed estimation results for all matching algorithms are available on request from the authors.

between both groups, Rosenbaum and Rubin (1985) propose using standardized biases (SB). Standardized biases before and after matching are defined as follows:

$$Biasbef = \frac{(\overline{X}_1 - \overline{X}_0)}{\sqrt{0.5 \cdot (V_1(X) + V_0(X))}}; \; Biasaft = \frac{(\overline{X}_{1M} - \overline{X}_{0M})}{\sqrt{0.5 \cdot (V_{1M}(X) + V_{0M}(X))}}, \tag{4}$$

where $\overline{X}_{1[0]}(V_{1[0]}(X))$ is the mean (variance) in the treatment (control) group before matching, and $\overline{X}_{1[0]M}(V_{1[0]M}(X))$ the corresponding values after matching. The SB after matching for estimator A is highest at 15.64%. Even though this level represents a reduction compared with the situation before matching (20.12%), it is clearly not sufficient. In general, it is suggested that standardized differences should be below 5% (Sianesi 2004; Caliendo and Kopeinig 2008). Therefore, matching estimator A is not satisfactory, and we turn to the next two approaches.

For estimators B and C, we impose common support conditions and drop 74 users from the treatment group (*offsup*). These estimators balance the covariate differences between the groups, and, through the matching procedure, more than 60% of the covariate differences are removed. In addition, Sianesi (2004) suggests re-estimating propensity scores on the matched sample, and comparing the pseudo-R^2 values before and after matching. The pseudo-R^2 after matching should be lower, because systematic differences in the distribution of the covariates between groups should have been removed by matching. In our analysis, we achieve pseudo-R^2 values of 0.247 before and 0.062 (estimator B) and 0.024 (estimator C) after matching.

>> Insert Table 7 about here <<

Table 7 shows that the use of matching estimator B does not balance the covariate distribution satisfactorily (bias aft > 5%), whereas using estimator C does. Consequently, we rely on estimator C as the appropriate measure, and find that the causal effect of a spam filter installation on working time losses equals approximately -439.52 minutes and is significant.[13] Therefore, in our sample, the installation of a spam filter is beneficial and decreases working time losses by more than 400 minutes per year. Although savings of approximately seven hours per year might not sound too impressive, it becomes more so if viewed within the organizational context. Consider, for example, an average wage of 30 Euro per hour, and assume that 1,000 employees work for a company; the seven hours saved accumulate to a considerable sum that clearly exceeds the central costs associated with installing a spam filter mechanism.

Effect Heterogeneity

As we noted above, we observe considerable heterogeneity with regard to variables that characterize the usage intensity of e-mail communication, and we find that the decision to install a filter is strongly influenced by the intensity of e-mail communication. This notion implies that a spam filter might not be a necessary and efficient option for all users. Hence, we conducted group-specific matching procedures in order to uncover underlying factors that account for heterogeneity in the magnitude of the treatment effect. The group-specific results in Table 8 show that the desired cost saving effects of a spam filter installation do not occur in any case, rather that the size and the direction of the effect depends on user characteristics.

First, the number of spam mails received by an individual plays a central role in the cost effect of a spam filter. We see that a spam filter only saves costs for those users who are bothered by a large spam burden. For users who receive less than 10 spam mails per day, the cost effect of a spam filter is even positive. This implies that a spam filter does not save, but rather induces costs for users who only receive few spam mails; in these cases a manual identification and eli-

[13] To draw inferences about the significance of the effect, we report bootstrapped standard errors (*s.e.*) with 200 replications. Heckman et al. (1998) show that bootstrapping is valid for drawing inferences for kernel matching methods.

mination will probably be more efficient. A likely reason is that the costs associated with the installation, training, and control of the filter exceed the beneficial effect of saving time through classification of e-mails.

>> Insert Table 8 about here <<

Second, since the efficiency of spam filter training or manual handling of spam mails is likely to depend on the individual's know-how, we include the level of proficiency in dealing with spam in the group-specific analysis. If the cost effects of a spam filter are analyzed conditionally on how well informed the user is, we observe that significant cost-saving effects only occur when the user is not well informed concerning spam. If a user does not have a profound knowledge about spam, a manual inspection appears to be less efficient than an automatic classification; in this case, the filter uses information that can only be readily substituted by visual inspection as is the case of experienced users. For these well-informed users, the cost-saving effects are present but fail to be significant, indicating that an experienced user might as well rely on his or her proficiency to manually classify e-mails.

Sensitivity to Unobserved Heterogeneity

The validity of our estimates depends on the conditional independence assumption. For this assumption to be fulfilled, we must observe all variables that simultaneously influence the propensity to install a spam filter, and the outcome variable. Because of this very strong assumption, we validate whether unobserved heterogeneity might alter our results by applying the bounding approach proposed by Rosenbaum (2002). The basic idea of this approach is to determine how strong an unobserved variable must be to influence the decision to install a spam filter and to change our matching results. Becker and Caliendo (2007), and DiPrete and Gangl (2004) provide guidance for implementing this bounding approach in the case of a discrete or metric outcome variable. As a starting point, we assume that the propensity score is influenced not only by observed variables *X*, but also by unobserved variables *U*, such that $P(D = 1 | \beta X + \gamma U)$. If the selection is based solely on observable variables *X*, the study is free of hidden bias, γ will be 0, and the installation probability will be determined solely by *X*. However, if a hidden bias exists, two individuals with the same observed covariates *X* will have differing chances of installing a spam filter.

>> Insert Table 9 about here <<

By varying the influence of γ, we can examine the sensitivity of our results to two different scenarios. First, we consider a situation in which we underestimate (t-hat-) the true treatment effect; second, we address a situation in which we overestimate (t-hat+) the true treatment effect. For both scenarios, we re-estimate the test statistics (see Table 9) and check the significance of the coefficients. Given the negative estimated treatment effect, the bounds that emerge under the assumption that we have overestimated the true treatment effect are of less importance. The effect is significant at γ = 1 and becomes even more significant for increasing values of γ if we have overestimated the true treatment effect. However, the bounds under the assumption that we have underestimated the treatment effect reveal that even high levels of γ would not alter the significance of the results. To be more specific, at a value of γ = 1.8, the result remains significant at the 5% level; at γ = 1.9, it would be still significant at the 10% level. Only at a γ-value of 2.0 do the results become insignificant. However, γ = 2 implies that the unobserved component in $P(D = 1 | \beta X + \gamma U)$ would have to be as strong as the observed component. Given the informative data at hand, this is rather unlikely; therefore, we can state that only very high levels of unobserved heterogeneity would alter our results.

Conclusion and Limitations

Our analysis shows that the existence of spam confronts organizations with significant expenses, primarily in the form of working time losses. Every year, employees waste an average of 1,200 minutes—or two working days—dealing with spam.

When an organization decides to react by setting up a spam filter mechanism, it incurs further expenses, and the cost-saving effects have been unclear thus far. We clarify this situation by showing that spam filters can reduce individual spam-related costs. The effect is strong; cost savings accumulate to 439 minutes per person per year, and our findings are significant and insensitive to unobserved heterogeneity. The magnitude of the different cost components also suggests that the primary concern in organizations should be the effectiveness of filter mechanisms on the individual level rather than central costs caused by spam. Due to the fact that cost-saving effects only occur for those users with an excessive spam load, those with little knowledge about spam, or those lacking adequate countermeasures, companies should primarily address these users in order to reduce costs through spam filters. For these users the installation of a spam filter will lead to the desired effect. If a user is well informed or is not strongly affected by spam, a company should not encourage the implementation of technical countermeasures. In this case, manual inspections appear to be more efficient than filter mechanisms that tend to increase overall spam costs.

We derive our conclusions by applying an econometric matching approach that controls for selection bias. It is unlikely that the selection bias is unique to our sample; rather, a selection bias probably poses a problem for a multitude of other research questions in IS. Within organizations, this effect might arise in evaluations of the effectiveness of optional IT innovations, for which an experimental setting is not available. Consider, for example, a mobile e-mail solution offered to employees. To evaluate its effectiveness, the company cannot use a simple cross-sectional approach because the estimation cannot distinguish whether the outcome measure (e.g., efficiency) causes or is affected by adoption. A similar case might be made for adoptions of antivirus software, optional SAP modules, and hardware (e.g., Blackberry).

Comparisons between several organizations encounter a similar problem. Consider the introduction of a new accounting software system. An efficiency evaluation cannot occur without correcting for a sample selection bias because cross-sectional estimation procedures cannot distinguish whether efficient companies tend to be early adopters of new software, or whether the adoption of new software enhances their efficiency. Thus, when experimental settings are infeasible, we recommend the application of a quasi-experimental setting that draws on matching procedures and thus provides a viable and efficient way to correct for sample selection bias.

Finally, we note some limitations to our study. First, our research focuses on a single German university. Studying different organizations (companies) with different organizational settings and in different countries would certainly yield deeper insights into this important matter. Second, the data we use is gathered through self-reported measures, which is common practice in research and provides generally accepted validity. However, a comparison with observed measures, perhaps in an experimental setting, might enhance generalizability. Third, though we demonstrate the positive effects of spam filters on the individual and organizational levels, we cannot extend our findings to general welfare implications because it remains unclear whether spam filters represent the most efficient way to deal with spam in the long term. Some theoretical evidence suggests that the widespread use of filters might even increase the overall amount of spam (Melville et al. 2006). This question therefore should be addressed by research in order to derive long-term recommendations about spam policy.

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Tables and Figures

Table 1: Central costs

Nonrecurring tasks	Time Expenditure	Costs
Administrative tasks (e. g., coordination of privacy concerns, legal and organizational clearance).	58 hours	Euro 1,740
Acquisition costs for hard- and software	-	Euro 3,000
Setup of infrastructure (e. g., installation, training)	78 hours	Euro 2,340
Recurring costs		
Maintenance and further development, support, training	268 hours	Euro 8,040
Total costs for provider	404 hours	Euro 15,120

Table 2: Spam-induced working time losses

	Symbol	Total	D=1 spam filter	D=0 no filter
Number of observations		1000	527	473
Time expenditure per year Index = $(w_1 \cdot 250)+w_2+w_3+w_4+w_5+w_6+w_7$	*Spamtime*	1247.22 (1686.33)	1,596.90*** (1,813.70)	857.63 (1,436.78)
(w_1) Time expenditure for daily spam treatment[a]	*Daily*	4.87 (6.70)		
(w_2) Time expenditure for finding university spam filter[a]	*Research1*	17.03 (20.11)		
(w_3) Time expenditure for university filter installation[a]	*Installation1*	15.68 (18.87)		
(w_4) Time expenditure for filter installation by assistant[a]	*Installation2*	11.73 (11.63)		
(w_5) Time expenditure for finding alternative filter[a]	*Research2*	53.34 (54.83)		
(w_6) Time expenditure for installation of alternative filter[a]	*Installation3*	34.40 (72.23)		
(w_7) Time expenditure for installation of alternative filter by assistant[a]	*Installation4*	23.37 (21.24)		

$^{***/**/*}$ Statistical difference (two-sided t-test) from D = 0 at the 1%, 5%, and 10% levels, respectively.

[a] Variable measured in minutes.

Notes: Numbers in brackets are standard deviations.

Table 3: Individual factors

	Symbol	Total	D=1 spam filter	D=0 no filter
Number of observations		1000	527	473
Age	*Age*	40.80 (11.22)	42.98^{***} (11.27)	38.37 (10.66)
Gender (2=male)	*Gender*	1.57 (0.50	1.61^{***} (0.53)	1.47 (0.52)
Spam quantity Spam = $(w_1 \cdot w_2) / 100$	*Quantity*	16.880 (28.397)	25.62^{***} (32.66)	7.14 (18.37)
(w_1) Number of e-mail per day	*Email*	23.378 (31.615)	8.14^{***} (13.18)	4.66 (4.45)
(w_2) Spam share (in %)	*Spamshare*	44.846 (36.038)	57.18^{***} (34.52)	31.33 (32.59)
Spam prevention				
(w_1) Avoided to publish e-mail address on website[a]	*Website*	2.942 (1.692)	2.92 (1.70)	2.96 (1.68)
(w_2) Avoided transfer of e-mail address[a]	*Transfer*	2.539 (1.458)	2.57 (1.49)	2.51 (1.42)
(w_3) Alternative e-mail address was used[a]	*Alternative*	2.723 (1.833)	2.83^{**} (1.85)	2.60 (1.81)
(w_4) Uncommon e-mail address was used[a]	*Uncommon*	1.257 (0.796)	1.28 (0.84)	1.23 (0.75)
(w_5) Requested removal from e-mail-lists[a]	*Removal*	1.821 (1.437)	1.90^{*} (1.47)	1.73 (1.40)
Spam control				
(w_1) Inspection of sender[a]	*Sender*	4.733 (0.768)	4.75 (0.71)	4.71 (0.83)
(w_2) Inspection of subject[a]	*Subject*	4.721 (0.783)	4.77^{**} (0.69)	4.67 (0.87)
(w_3) Inspection by opening e-mail[a]	*Open*	1.670 (0.961)	1.64 (0.90)	1.71 (1.02)
Level of information on spam[a]	*Infolevel*	2.699 (1.121)	3.07^{***} (1.12)	2.28 (0.97)

$^{*** / ** / *}$ Statistical difference (two-sided t-test) from D = 0 at the 1%, 5%, and 10% levels, respectively.

[a] Variable measured on a 5-point Likert-type scale (1 = disagree – 5 = agree).

Notes: Numbers in brackets are standard deviations.

Table 4: Reactance

	Symbol	Total	D=1 spam filter	D=0 no filter
Number of observations		1000	527	473
Behavioral changes				
(w_1) Usage of e-mail was reduced[a]	*Reduction*	1.276 (0.775)	1.32* (0.86)	1.23 (0.67)
(w_2) Time expenditures for e-mails have been increased[a]	*Timeincrease*	2.807 (1.499)	3.20*** (1.48)	2.37 (1.39)
(w_3) Confidence in e-mail was reduced[a]	*Confidence*	2.294 (1.282)	2.42*** (1.30)	2.16 (1.24)
Spam sensitivity				
(w_1) Advertising e-mail from unknown sender[a]	*Unknown*	4.809 (0.617)	4.83 (0.59)	4.78 (0.64)
(w_2) Unsolicited e-mail by political or other organization[a]	*Advertise1*	4.568 (0.870)	4.58 (0.86)	4.56 (0.89)
(w_3) Unsolicited e-mail by non-commercial organization[a]	*Advertise2*	4.410 (1.010)	4.45 (0.94)	4.36 (1.08)
(w_4) Fun e-mails[a]	*Fun*	2.594 (1.455)	2.70*** (1.49)	2.48 (1.40)
(w_5) Advertising e-mail from business partners[a]	*Advertise3*	2.561 (1.346)	2.66*** (1.37)	2.45 (1.31)
(w_6) E-mail with large attachment[a]	*Attachment*	2.141 (1.334)	2.23* (1.39)	2.04 (1.26)
(w_7) Solicited commercial e-mail[a]	*Solicited*	1.711 (1.061)	1.70 (1.08)	1.72 (1.04)
Spam properties				
(w_1) Spam e-mail is unsolicited[a]	*Unsolicited*	4.520 (0.925)	4.57*** (0.87)	4.46 (0.99)
(w_2) Spam surge cannot be stopped[a]	*Nonstop*	4.209 (1.122)	4.25 (1.08)	4.16 (1.16)
(w_3) Spam e-mail is potentially harmful for own computer[a]	*Damage*	4.185 (1.254)	4.00*** (1.37)	4.40 (1.06)
(w_4) Perceived amount of spam received[a]	*PercAmount*	3.835 (1.353)	4.14*** (1.24)	3.48 (1.40)
(w_5) Perceived magnitude of time expenses for spam[a]	*PercTime*	3.399 (1.385)	3.60*** (1.35)	3.16 1.39

$^{***/**/*}$ Statistical difference (two-sided t-test) from D = 0 at the 1%, 5%, and 10% levels, respectively.

[a] Variable measured on a 5-point Likert-type scale (1 = disagree – 5 = agree).

Notes: Numbers in brackets are standard deviations.

Table 5: Distribution of e-mail addresses

	Symbol	Total	D=1 spam filter	D=0 no filter
Number of observation		1000	527	473
Distribution of e-mail address to known contacts				
(w_1) E-mail address was distributed to colleagues / business partners[b]	*Colleagues*	0.916 (0.278)	0.92 (0.28)	0.92 (0.28)
(w_2) E-mail address was distributed to friends / acquaintances[b]	*Friends*	0.544 (0.498)	0.53 (0.50)	0.56 (0.50)
Distribution of e-mail address to unknown contacts				
(w_1) E-mail address was published on websites[b]	*Publwebsites*	0.721 (0.449)	0.79*** (0.41)	0.65 (0.48)
(w_2) E-mail address was published in online directories[b]	*Directories*	0.673 (0.469)	0.73*** (0.44)	0.61 (0.49)
(w_3) E-mail address was published in online forums[b]	*Forum*	0.069 (0.254)	0.10*** (0.30)	0.04 (0.19)
(w_4) E-mail address was used when signing up for newsletters or webpages[b]	*Newsletter*	0.317 (0.466)	0.33 (0.47)	0.30 (0.46)

*** / ** / * Statistical difference (two-sided t-test) from D = 0 at the 1%, 5%, and 10% levels, respectively.

[b] Variable measured as binary variable (0/1).

Notes: Numbers in brackets are standard deviations.

Table 6: Estimation results of the Logit model

	Independent Variables	*Coef.*	*s.e.*	*P>|z|*
	Age	.049***	.0089	0.000
	Gender	-.334**	.1667	0.045
Individual factors	*Quantity*	.016***	.0044	0.000
	E-mail	.040***	.0156	0.010
	Website	.070	.0597	0.244
	Transfer	.023	.0651	0.719
	Alternative	.006	.0529	0.912
	Uncommon	.009	.1021	0.932
	Removal	.024	.0565	0.668
	Sender	.001	.1425	0.992
	Subject	.189	.1412	0.182
	Open	-.135	.0839	0.107
	Infolevel	.794***	.0915	0.000
Reactance	*Reduction*	.083	.1126	0.462
	Timeincrease	.115*	.0712	0.105
	Confidence	-.008	.0715	0.912
	Unknown	.031	.1433	0.827
	Advertising1	.090	.0653	0.168
	Fun	.003	.0701	0.966
	Attachment	.015	.0767	0.843
	Advertise2	.207*	.1348	0.125
	Advertise3	-.236	.1526	0.122
	Solicited	.003	.0805	0.965
	PercAmount	.165*	.0851	0.053
	PercTime	-.037	.0814	0.651
	Unsolicited	.145	.0987	0.142
	Damage	-.172**	.0732	0.019
	Nonstop	-.053	.0850	0.531
Distribution	*Friends*	-.082	.1688	0.628
	Colleagues	-.165	.2941	0.575
	Publwebsites	.241	.2017	0.233
	Directories	.236*	.1721	.0170
	Forum	.583	.3720	0.117
	Newsletter	.027	.1796	0.883
	Const.	-5.96***	1.224	0.000

Notes: Number of observations: 960; Pseudo-R^2=0.247.
$^{***/**/*}$Statistical significance at the 1%, 5%, and 10% levels, respectively.

Table 7: Matching results

Est.	*Effect*	*s.e.*	*t-value*	*offsup*	*biasbef*	*biasaft*	*R^2after*
A	-814.48	379.34	-2.15	0	20.12	15.64	0.119
B	-468.37	275.04	-1.70	74	20.12	8.30	0.062
C	-439.52	225.06	-1.95	74	20.12	4.90	0.024

offsup: Number of users outside common support region.
biasbef: Mean standardized bias (over all variables used in PS-specification) before matching.
bias aft: Mean standardized bias after matching.
Est.: A(B): Nearest Neighbor Matching without (with) common support condition,
C: Kernel matching (Epanechnikow kernel function, bandwidth parameter: 0.06) with common support condition.
s.e. Standard errors based on 200 bootstrap replications.

Table 8: Group analysis: matching results (group-specific scores)

Obs.	*Effect*	*s.e.*	*t-value*	*offsup*	*biasbef*	*biasaft*	*R^2after*
Number of spam mails <=10							
n(D=1): 232 n(D=0): 357	156.96	86.14	1.82	14	14.84	4.35	0.019
Number of spam mails >10							
n(D=1): 288 n(D=0): 83	-688.73	397.40	-1.73	62	16.25	9.75	0.069
Level of information on spam <3							
n(D=1): 152 n(D=0): 271	-528.92	301.67	-1.75	3	17.32	5.9	0.028
Level of information on spam >=3							
n(D=1): 368 n(D=0): 169	-160.92	296.65	-0.54	71	20.20	7.35	0.044

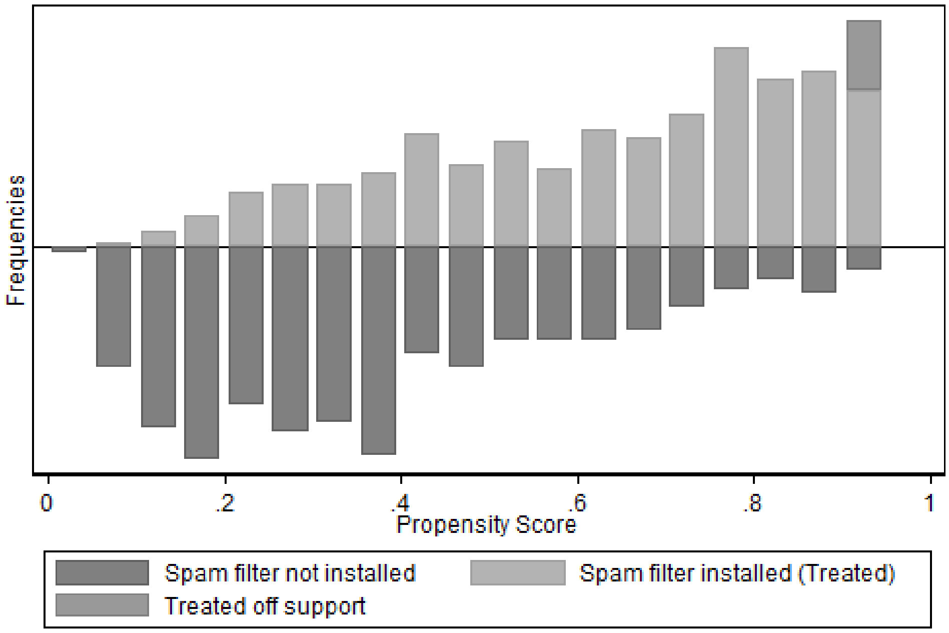
offsup: Number of users outside common support region.
biasbef: Mean standardized bias (over all variables used in PS-specification) before matching.
bias aft: Mean standardized bias after matching.
Estimator C: Kernel matching (Epanechnikow kernel function, bandwidth parameter: 0.06) with common support condition.
s.e. Standard errors based on 200 bootstrap replications.

Table 9: Sensitivity analysis, unobserved heterogeneity

Gamma	*t-hat+* *(Sig+)*	*t-hat-* *(Sig-)*	*CI+*	*CI-*
1	-484.037 (4.0e-13)	-484.037 (4.0e-13)	-663.714	-317.766
1.1	-561.568 (4.4e-16)	-401.175 (1.5e-10)	-737.963	-258.395
1.2	-636.654 (0)	-338.821 (1.7e-08)	-790.884	-211.95
1.3	-700.058 (0)	-288.26 (7.3e-07)	-833.384	-173.577
1.4	-754.04 (0)	-245.048 (.000015)	-872.107	-140.304
1.5	-793.397 (0)	-209.412 (.000176)	-908.88	-105.836
1.6	-827.199 (0)	-178.921 (.001264)	-948.688	-71.8773
1.7	-858.578 (0)	-152.968 (.006165)	-990.629	-37.0085
1.8	-887.141 (0)	-126.06 (.02185)	-1023.3	-3.449
1.9	-915.892 (0)	-99.5326 (.059483)	-1053.24	27.0441
2	-946.357 (0)	-73.8106 (.130113)	-1083.04	54.8713

Gamma: log odds of differential assignment due to unobserved factors
Sig+: upper bound significance level
Sig-: lower bound significance level
t-hat+: upper bound Hodges-Lehmann point estimate
t-hat-: lower bound Hodges-Lehmann point estimate
CI+: upper bound confidence interval (a= .95)
CI-: lower bound confidence interval (a= .95)

Figure 1: Distribution of the Propensity Score. Common Support



Note: This figure shows the distribution of the propensity score for individuals who installed a spam filter (upper half) and those who did not (lower half). According to the MinMax-criterion 74 users from the first group must be exluded from the analysis (Treated off support), because their propensity score values lie outside the region of common support.

Abschnitt C:

Kopeinig, Sabine/Gedenk, Karen: Make-or-Buy-Entscheidungen von Messegesellschaften

Kopeinig, Sabine/Gedenk, Karen: Make-or-Buy-Entscheidungen von Messegesellschaften, in: Delfmann, Werner/Köhler, Richard/Müller-Hagedorn, Lothar (Hrsg.): Kölner Kompendium der Messewirtschaft - Das Management von Messegesellschaften, 1. Aufl., Köln: Kölner Wissenschaftsverlag, 2005, S. 227 - 249.

11 MAKE-OR-BUY-ENTSCHEIDUNGEN VON MESSEGESELLSCHAFTEN

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1 Problemstellung

Unter einer Make-or-Buy-Entscheidung wird die Entscheidung zwischen dem Fremdbezug oder der Eigenfertigung von Leistungen verstanden. Die „Make“-Entscheidung entspricht dabei der Eigenfertigung bzw. der vertikalen Integration, während man unter „Buy“ den Bezug am Markt bzw. ein Outsourcing versteht. Zwischen diesen beiden Extremen gibt es eine Vielzahl relevanter Kooperationsformen als Hybride.[1]

Messegesellschaften sehen sich an vielen Stellen mit Make-or-Buy-Entscheidungen konfrontiert, z.B. bei der Datenverarbeitung und beim Auslandsvertrieb. Besonders relevant ist die Frage nach „Make or Buy“ derzeit in vielen Messegesellschaften bei den Services, welche sie Ausstellern und Messebesuchern anbieten. Dazu zählen beispielsweise Gastronomie-, Standbau-, Logistik- und Reise-Services. Deutsche Messegesellschaften konkurrieren heute nicht nur untereinander, sondern stehen auch mit nationalen privaten Messeveranstaltern im Wettbewerb.[2] Darüber hinaus verschärft sich der internationale Wettbewerb um den Messeplatz Deutschland. Des Weiteren müssen deutsche Messegesellschaften gestiegene Kundenwünsche sowohl seitens der Messeaussteller als auch der –besucher erfüllen. Um sich in diesem Umfeld abheben zu können, setzen deutsche Messegesellschaften verstärkt darauf, ihren Kunden ein „Full-Service-Paket“ zu schnüren. Das Ziel dabei ist es insbesondere, den Ausstellern sämtliche Dienstleistungen im Rahmen der Messebeteiligung aus einer Hand anzubieten („one face to the customer“). Hier ergibt sich z.T. ein erhebliches Erlös- bzw. Gewinnpotenzial.[3] Die entsprechenden Leistungen müssen aber nicht notwendigerweise selbst erstellt werden. Für Messegesellschaften ist es daher wichtig, sich mit Make-or-Buy-Entscheidungen für ergänzende Services auseinander zu setzen.

Make-or-Buy-Fragestellungen sind Gegenstand zahlreicher wissenschaftlicher und nicht-wissenschaftlicher Publikationen.[4] Dabei werden insbesondere Beschaffungsprozesse produzierender Unternehmen betrachtet, aber auch andere

[1] Vgl. Picot (1991), S. 339; Williamson (1991), S. 269.

[2] Vgl. Witt (2003), S. 506.

[3] Vgl. AUMA-Bericht (2001/2002), S. 5; Die Welt (2005), S. 13.

[4] Vgl. z.B. Walker / Weber (1984) oder Liebermann (1991). Einen ausführlichen Literaturüberblick über empirische Studien gibt Klein (2004).

betriebliche Teilfunktionen wie die Datenverarbeitung oder der Vertrieb. Beispielsweise kann die Frage, ob der Vertrieb über einen unternehmenseigenen Außendienst oder über rechtlich selbständige Personen bzw. Unternehmen durchgeführt werden soll, als eben solche Entscheidung aufgefasst werden.[5] ZYGOJANNIS diskutiert diese Fragestellung im Beitrag: „Gestaltung des Auslandsvertriebs“. Im Fokus der Literatur zu „Make or Buy“ steht die Identifikation von Einflussfaktoren, welche eine „Make“- oder aber eine „Buy“-Entscheidung vorteilhafter erscheinen lassen. Solche Einflussfaktoren lassen sich zunächst aus dem Transaktionskostenansatz ableiten. Diese auf COASE zurückgehende und v.a. durch WILLIAMSON weiterentwickelte Theorie identifiziert zentrale Einflussfaktoren wie Spezifität und Komplexität der zu erbringenden Leistung, anhand derer eine Überprüfung der Vorteilhaftigkeit verschiedener Entscheidungsalternativen möglich ist.[6] Darüber hinaus zeigt die konzeptionelle Literatur zu den jeweils betrachteten Entscheidungen weitere Einflussfaktoren auf.

Make-or-Buy-Entscheidungen im Messewesen sind in der Literatur bislang nicht thematisiert worden. Die Übertragung von Erkenntnissen der bisherigen Literatur zu Make-or-Buy-Entscheidungen ist nicht ohne weiteres möglich. Zum einen sind die Einflussfaktoren des Transaktionskostenansatzes sehr abstrakt und bedürfen einer Konkretisierung und Anwendung auf den Messe-Kontext. Zum anderen ist die konzeptionelle Literatur zum Messewesen bislang nicht systematisch unter dem Gesichtspunkt von Make-or-Buy-Entscheidungen ausgewertet worden. So ergeben sich bei Messen Besonderheiten im Vergleich zu anderen Branchen z.B. aus der Existenz von Mehrfachzielen der Anteilseigner (Gewinn und Umwegrendite).

Ziel unseres Beitrags ist es daher, einen Überblick über Make-or-Buy-Entscheidungen von Messegesellschaften zu geben. Wir wollen relevante Entscheidungen und Entscheidungsalternativen identifizieren und Einflussfaktoren auf die Vorteilhaftigkeit der Handlungsalternativen diskutieren. Der Beitrag beginnt in Abschnitt 2 mit einer Systematisierung von Make-or-Buy-Entscheidungen von Messegesellschaften. Abschnitt 3 zeigt auf, welche Entscheidungsalternativen dabei zur Verfügung stehen. Beispiele machen deutlich, dass Messegesellschaften durchaus zu unterschiedlichen Entscheidungen gelangen, was noch einmal die Bedeutung der Fragestellung unterstreicht. In Abschnitt 4 präsentieren wir Einflussfaktoren auf die Vorteilhaftigkeit von „Make“ vs. „Buy“, die wir zum einen aus dem Transaktionskostenansatz ableiten (Abschnitt 4.1) und zum anderen aus der konzeptionellen Literatur zum Messe-

[5] Vgl. z.B. Anderson (1985) oder Krafft / Albers / Lal (2004).

[6] Vgl. Coase (1937); Williamson (1975); Williamson (1985).

wesen (Abschnitt 4.2). Diese Einflussfaktoren werden anschließend in Abschnitt 5 eingesetzt, um exemplarisch die Vorteilhaftigkeit von Eigenfertigung vs. Fremdbezug bei Gastronomie- und Standbau-Services zu prüfen. Der Beitrag schließt mit einer Zusammenfassung in Abschnitt 6.

2 Relevante Make-or-Buy-Fragestellungen

Im Folgenden betrachten wir Messegesellschaften als Veranstalter von Messen mit eigenem Gelände und überwiegend eigenem Messeprogramm. In Deutschland befinden sich diese Messegesellschaften zumeist im Besitz von Kommunen und/oder Bundesländern. Damit erfolgt eine Abgrenzung von reinen Betriebsgesellschaften, die hier nicht betrachtet werden sollen.[7]

Die Analyse von Make-or-Buy-Entscheidungen ist grundsätzlich für viele Funktionsbereiche und Aktivitäten von Messegesellschaften relevant. Abbildung 13 gibt einen Überblick über diese Aktivitäten in Form einer Wertschöpfungskette.

So denken Messegesellschaften zum einen darüber nach, sekundäre Aktivitäten wie die Datenverarbeitung oder die Lohn- und Gehaltsverrechnung aus dem Unternehmen auszulagern. Diese Entscheidungen zu sekundären Aktivitäten werden hier nicht weiter betrachtet, da sie nicht messespezifisch sind.

Zum anderen können primäre Aktivitäten der Gegenstand von Make-or-Buy-Überlegungen sein. Die zentrale, primäre Aktivität einer Messegesellschaft besteht in der räumlichen und zeitpunktbezogenen Zusammenführung von Besuchern und Ausstellern, typischerweise von (potenziellen) Käufern und Verkäufern, in einem Markt.[8] Das Kerngeschäft der Messegesellschaft ist dabei die Präsentation von Branchen, Firmen und Produkten.[9] Hier stellt sich die Make-or-Buy-Fragestellung kaum, da Aktivitäten des Kerngeschäfts von hoher strategischer Bedeutung sind und in aller Regel im Unternehmen bleiben.[10]

[7] Vgl. Groth (1992), S. 161.

[8] Vgl. Stoeck (1999), S. 29f.

[9] Vgl. Goschmann (2003), S. 46.

[10] Vgl. Quinn (1999), S. 43.

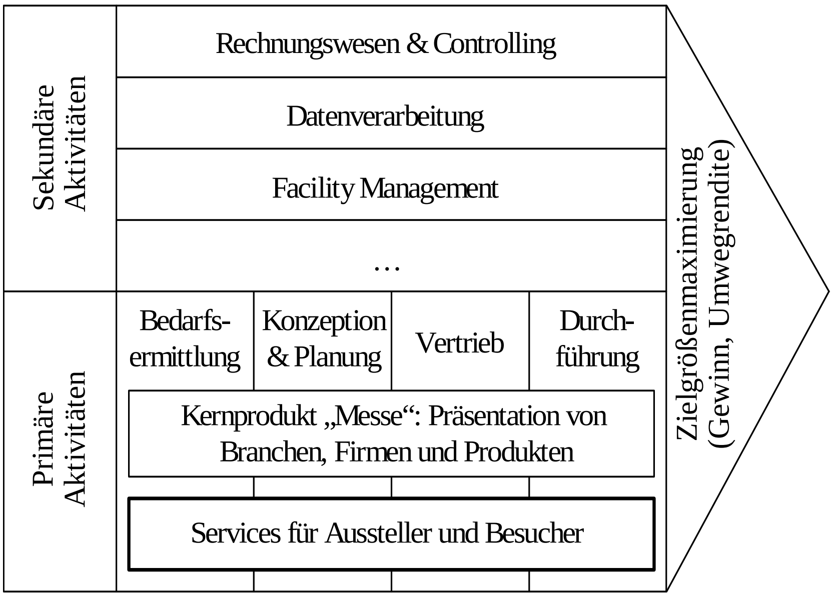

Sekundäre Aktivitäten
Rechnungswesen & Controlling
Datenverarbeitung
Facility Management
…
Primäre Aktivitäten
Bedarfs-ermittlung
Konzeption & Planung
Vertrieb
Durch-führung
Kernprodukt „Messe“: Präsentation von Branchen, Firmen und Produkten
Services für Aussteller und Besucher
Zielgrößenmaximierung (Gewinn, Umwegrendite)

Abbildung 13: Wertschöpfungskette einer Messeveranstaltung

Hoch relevant sind Make-or-Buy-Entscheidungen dagegen bei den Dienstleistungen, welche Messegesellschaften zusätzlich zum Kerngeschäft für Besucher und Aussteller anbieten. Tabelle 4 gibt einen Überblick über derartige Services.

Vor der Messe	Während der Messe	Nach der Messe
▪ Standbau-Services	▪ Technische Services	▪ Standbau-Services
▪ Marketing-Services	▪ Personal-Services	▪ Logistik-Services
▪ Logistik-Services	▪ Gastronomie-Services	▪ After-Sales-Services
▪ Reise-Services	▪ Hotel-Services	▪ Reise-Services
▪ …	▪ …	…

Tabelle 4: Services für Messeaussteller und –besucher

Im Folgenden werden wir uns auf Make-or-Buy-Entscheidungen für diese Services konzentrieren, und dabei insbesondere Gastronomie- und Standbau-Services näher betrachten. Eine Analyse gerade dieser beiden Service-Bereiche erscheint aus zwei Gründen interessant. Zum einen variiert bei ihnen die gewählte Organisationsform in der Praxis über Messegesellschaften hinweg erheblich, wie im folgenden Abschnitt gezeigt wird. Zum anderen wird so eine Dienstleistung betrachtet, die sich primär an die Aussteller richtet (Standbau), und eine weitere, deren Zielgruppe vor allem Besucher sind (Gastronomie).

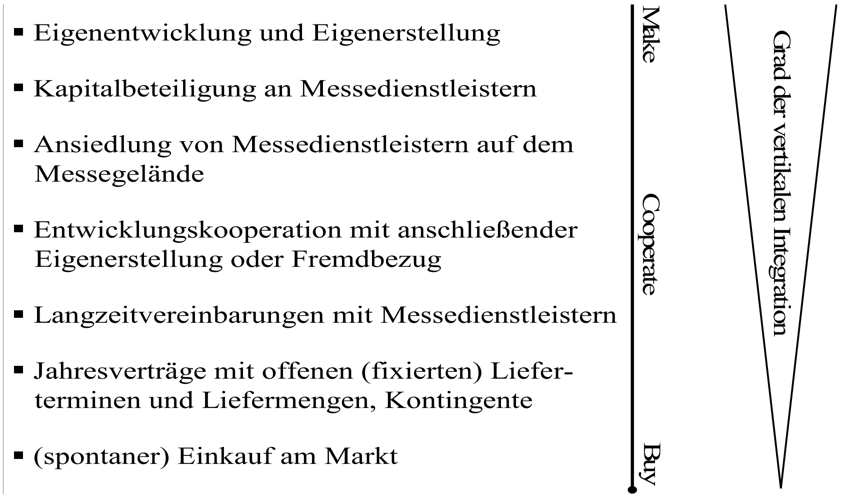
Gastronomie-Services stellen aufgrund hoher und unterschiedlicher Erwartungen der Messekunden einen bedeutenden und gleichzeitig problematischen Servicebereich dar.[11] Messeaussteller und –besucher erwarten eine umfassende, qualitativ hochwertige und preisgünstige gastronomische Versorgung auf dem Messegelände. Diese umfasst neben dem Betrieb unterschiedlicher Restaurants, Bistros und Snack-Points auch die Standbelieferung sowie das Veranstaltungscatering, z.B. bei Pressekonferenzen oder Empfängen.

Zu den Standbau-Services zählen die Planung, Konzeption und Umsetzung sowie Montage und Demontage des Messestandes. Ausgaben für den Messestand stellen einen großen Kostenblock für Aussteller und somit ein erhebliches Umsatzpotenzial für Messegesellschaften dar. Messestandbauer arbeiten im Auftrag der Messegesellschaften und/oder der Messeaussteller. Ersteres ist insbesondere dann der Fall, wenn Messegesellschaften Mietnormstände anbieten.

3 Make-or-Buy-Entscheidungsalternativen

Make-or-Buy-Entscheidungen sollen hier nicht als Entweder-Oder-Entscheidungen begriffen werden. Vielmehr gibt es zwischen den Alternativen Eigenerstellung („Make“) und Einkauf am Markt („Buy“) eine Vielzahl relevanter Mischformen, bei denen Messegesellschaften mit anderen Unternehmen kooperieren („Cooperate“). In Abbildung 14 sind mögliche Entscheidungsalternativen dargestellt. Der Grad der vertikalen Integration nimmt dabei von oben nach unten ab.

[11] Vgl. Tauberger / Wartenberg (1992), S. 240.



- Eigenentwicklung und Eigenerstellung
- Kapitalbeteiligung an Messedienstleistern
- Ansiedlung von Messedienstleistern auf dem Messegelände
- Entwicklungskooperation mit anschließender Eigenerstellung oder Fremdbezug
- Langzeitvereinbarungen mit Messedienstleistern
- Jahresverträge mit offenen (fixierten) Lieferterminen und Liefermengen, Kontingente
- (spontaner) Einkauf am Markt

Abbildung 14: Make-or-Buy-Entscheidungsalternativen[12]

Messegesellschaften wählen durchaus unterschiedliche Grade der vertikalen Integration bei den verschiedenen Dienstleistungen. Beispielhaft sei hier die Koelnmesse Service GmbH angeführt, die im Bereich der ergänzenden Dienstleistungen nahezu das komplette Spektrum möglicher Entscheidungsalternativen abdeckt. So bietet die Koelnmesse Service GmbH technische Services aus eigener Hand an. Im Bereich der Standbau-Services unterhält sie eine Kooperationsvereinbarung mit dem Partnerunternehmen Uniplan. Darüber hinaus bestehen langfristige Vertragsbeziehungen mit Dienstleistern, z.B. Pachtverträge mit Messegastronomen. Dabei wird zumeist mittelständischen, ortsansässigen Unternehmen das Recht eingeräumt, gastronomische Anlagen auf dem Messegelände (in Messehallen) zu nutzen und gewinnbringend zu betreiben. Einige Dienstleistungen, wie z.B. Personal-Services (Standpersonal, Standbewachung) oder Hotel-Services (Partnerhotels) werden lediglich zwischen Messeausstellern und Drittunternehmen vermittelt.

Andere deutsche Messegesellschaften haben im Bereich ergänzender Dienstleistungen andere Strukturen vorgezogen. Beispielsweise werden Gastronomie-Services von der Messe Frankfurt ausschließlich über ein eigenes Tochterunternehmen angeboten (Accente Gastronomie Service GmbH). Auch die Leipziger Messe verfügt über ein Tochterunternehmen im Gastronomiebereich. Ob auch messefremde Unternehmen durch Messeaussteller etwa für das Standcatering

[12] In Anlehnung an Picot (1991), S. 340.

beauftragt werden dürfen, hängt von der Unternehmenspolitik der einzelnen Messegesellschaften ab. Auf dem Messegelände Frankfurt dürfen Messeaussteller Fremdcaterer nur nach Absprache mit Accente (gegen eine Ausfallentschädigung) beauftragen. Auf den Messegeländen Hannover, München, Leipzig u.a. ist die Standbewirtung dagegen offen und kann auch von messefremden Unternehmen betreut werden.[13]

Im Servicebereich Standbau betreibt die Leipziger Messe eine vollständige Integration in der Form, dass Standbau-Services über ein Tochterunternehmen mit unternehmenseigenem Personal angeboten werden. Eine hybride Organisationsform hat hier die Messe München gewählt, die zu 85% an der Messeplanungs- und Messebaufirma Meplan beteiligt ist. Ein weiteres Beispiel für eine vertikale Kooperation findet sich bei der Nürnberg Messe GmbH. Sie bietet zusammen mit drei Messebauunternehmen über das Internetportal www.standkonfigurator.de elf unterschiedliche Standbaumodelle an, welche gemeinsam entwickelt wurden.

Tabelle 5 zeigt diese und andere Praxisbeispiele noch einmal im Überblick.

[13] Vgl. m+a report, (2003), S. 48f.

Entscheidungsalternative	**Services**		
	Gastronomie	*Standbau*	*Weitere*
Eigenerstellung (Make)	*Frankfurt:* Tochterunternehmen Accente Gastronomie Service GmbH *Leipzig:* Tochterunternehmen Leipziger Messe Gastronomie GmbH	*Leipzig:* Tochterunternehmen FAIRNET – Gesellschaft für Messe-, Ausstellungs- und Veranstaltungsservice mbH	*Technische Services, Köln:* Angebot von Elektroinstallationen aus eigener Hand
Kapitalbeteiligung an Messedienstleistern		*München:* 85 % Kapitalbeteiligung an der Messebaufirma Meplan GmbH	
Ansiedlung von Messedienstleistern			*Logistik, Stuttgart:* Speditionsunternehmen Schenker unterhält auf dem Messegelände ganzjährig ein Büro
Entwicklungskooperation		*Nürnberg*: Kooperation mit drei Messebauern	
Langzeitvereinbarungen mit Messedienstleistern	*Köln:* Pachtvertrag mit einem Messegastronom *München*: Pachtverträge mit zwei Messegastronomen	*Köln:* Kooperation mit Uniplan	
Kontingentvereinbarungen			*Hotel, Köln:* Kontingentvereinbarungen mit Partnerhotels im Raum Köln

Tabelle 5: Praxisbeispiele – Entscheidungsalternativen

Es kann also festgehalten werden, dass Make-or-Buy-Entscheidungen nicht nur über Services variieren, sondern auch bei der gleichen Dienstleistung über Messegesellschaften hinweg. Dies macht es umso interessanter, im Folgenden Einflüsse auf die Vorteilhaftigkeit der verschiedenen Entscheidungsalternativen zu diskutieren.

4 Einflussfaktoren auf die Make-or-Buy-Entscheidung

4.1 Einflussfaktoren aus dem Transaktionskostenansatz

Der Transaktionskostenansatz geht auf COASE zurück und ist vor allem von WILLIAMSON entscheidend weiterentwickelt worden.[14] Der Fokus der Theorie liegt auf den Austauschbeziehungen bzw. Transaktionen zwischen Unternehmen.[15] Unter einer Transaktion wird dabei die Übertragung von Verfügungsrechten an Gütern und Dienstleistungen zwischen Wirtschaftssubjekten verstanden.[16] WILLIAMSON hält fest, dass jedes Problem, das direkt oder indirekt als Vertragsproblem zu formulieren ist, mit Hilfe der Transaktionskostentheorie untersucht werden kann.[17]

Transaktionen können entweder innerhalb der Unternehmensgrenzen bzw. in Hierarchien („Make“) oder aber am Markt („Buy“) durchgeführt werden. Unternehmen sollten dabei diejenige Transaktionsform wählen, welche die niedrigeren Transaktionskosten aufweist.[18] Unter Transaktionskosten werden in diesem Zusammenhang die Kosten der Anbahnung, Vereinbarung, Abwicklung, Kontrolle und Anpassung der mit den einzelnen Phasen einer Transaktion verbundenen Aktivitäten verstanden.[19] Höhe und Struktur der Transaktionskosten für die einzelnen Handlungsalternativen hängen von den Eigenschaften der jeweiligen Leistung (Transaktionsdimensionen) ab.[20] WILLIAMSONS mikroanalytischer Ana-

[14] Vgl. Coase (1937); Williamson (1975); Williamson (1985).

[15] Vgl. Rindfleisch / Heide (1997), S. 30.

[16] Vgl. Williamson (1990), S. 20.

[17] Vgl. Williamson (1990).

[18] Vgl. Bogaschewsky (1995), S. 164.

[19] Vgl. Picot (1991), S. 344; Picot (1982), S. 270.

[20] Vgl. Picot (1991), S. 344.

lyserahmen beruht im Wesentlichen auf dem Wechselspiel zwischen Annahmen über das menschliche Verhalten und diesen Transaktionsdimensionen. Mit den zentralen Verhaltensannahmen der begrenzten Rationalität und des Opportunismus wird die neoklassische Annahme des „homo oeconomicus“ aufgegeben.[21] Darüber hinaus trifft WILLIAMSON die Annahme der Risikoneutralität. Zu den Transaktionsdimensionen zählen Spezifität, Unsicherheit/Komplexität und Häufigkeit.[22] Abbildung 15 zeigt die geschilderten Zusammenhänge noch einmal im Überblick.

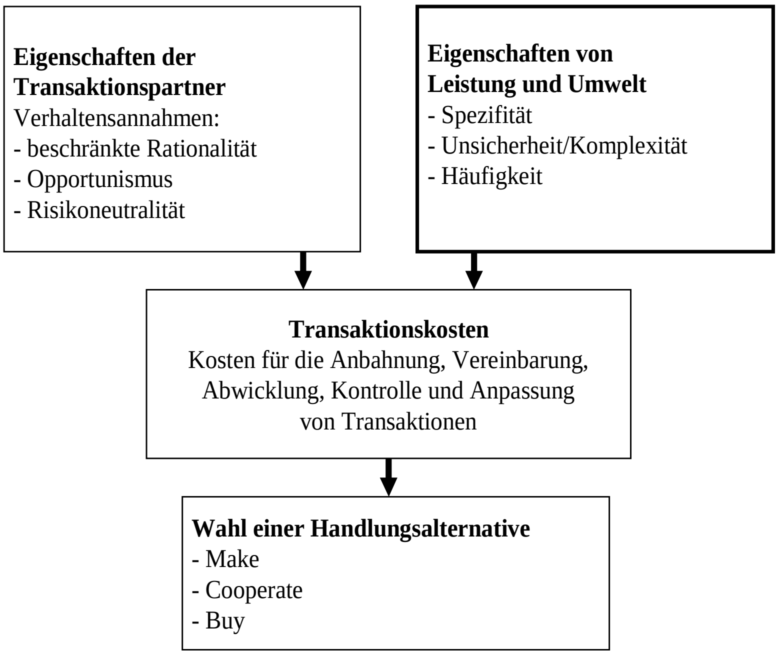

Eigenschaften der Transaktionspartner
Verhaltensannahmen:
- beschränkte Rationalität
- Opportunismus
- Risikoneutralität
Eigenschaften von Leistung und Umwelt
- Spezifität
- Unsicherheit/Komplexität
- Häufigkeit
Transaktionskosten
Kosten für die Anbahnung, Vereinbarung, Abwicklung, Kontrolle und Anpassung von Transaktionen
Wahl einer Handlungsalternative
- Make
- Cooperate
- Buy

Abbildung 15: Grundgedanke des Transaktionskostenansatzes

Die Eigenschaften der Leistung bzw. der Umwelt stellen wesentliche Einflüsse auf die Vorteilhaftigkeit der Make-or-Buy-Handlungsalternativen dar. Unter Spezifität versteht man die exklusive Gestaltung und Widmung von Ressour-

[21] Vgl. Williamson (1990), S. 34.

[22] Vgl. Williamson (1990), S. 59ff.; Williamson (1991), S. 281.

cen.[23] Wenn zur Erstellung einer Leistung bestimmte Ressourcen notwendig sind, die nur unter großen Verlusten anders beschafft werden könnten, spricht man von hoher Spezifität. Beispielhafte Ressourcen sind Werkzeuge oder Maschinen. Aber auch Know-how, Personalqualifikation oder Qualitätseigenschaften können spezifisch sein. Im Einzelnen unterscheidet WILLIAMSON (1991) zwischen sechs Formen der Spezifität, welche in Tabelle 6 wiedergegeben sind.[24]

Form	**Investitionen der Transaktionspartner …**
Standort-Spezifität	in ortsgebundene Einrichtungen
Sachkapital-Spezifität	in spezifische Maschinen oder Technologien
Humankapital-Spezifität	in spezifische Mitarbeiterqualifikation
Marken-Spezifität	in den Markennamen
Widmungs-Spezifität	in nichtspezifische Anlagen, die aber nur für die geplante Transaktion erfolgen und bei deren Wegfall Überkapazitäten entstehen
Temporäre Spezifität	müssen in einem engen, zeitlichen Rahmen getätigt werden (z.B. Montagearbeiten, Just-in-time-Produktion)

Tabelle 6: Formen der Spezifität

Eine Kernaussage des Transaktionskostenansatzes ist, dass hohe Spezifität zum Versagen von Marktmechanismen führt.[25] Bei einer „Buy“-Lösung würden sich beide Marktpartner in eine starke wechselseitige Abhängigkeit begeben. Dies kann nur bei gegenseitigem Vertrauen erfolgreich sein. Liegt dagegen opportunistisches Verhalten vor, muss jeder Vertragspartner befürchten, dass sein Gegenüber den Vertrag auflöst und spezifische Investitionen verloren gehen. Eine hohe Spezifität von Transaktionen spricht daher für eine hierarchische, d.h. für eine „Make“-Lösung.

Die zweite Transaktionsdimension ist Unsicherheit/Komplexität. Unsicherheit bezieht sich auf qualitative, quantitative, terminliche oder technische Änderun-

[23] Vgl. Picot (1991), S. 347.

[24] Vgl. Williamson (1991), S. 281.

[25] Vgl. Picot / Dietl (1990), S. 180.

gen der im Fokus der Untersuchung stehenden Leistung bzw. der Unternehmensumwelt. Im Falle von Komplexität sind Leistung und Umwelt zwar sicher, aufgrund begrenzter Rationalität aber nicht vollständig überschaubar.[26] Hohe Unsicherheit bzw. Komplexität sprechen dafür, Transaktionen nicht rein über Märkte durchzuführen, sondern stärker vertraglich abzusichern. Dies gilt insbesondere bei hoher Spezifität, da in diesem Fall nicht einfach am Markt neue Tauschbeziehungen eingegangen werden können.[27]

Eine weitere Transaktionsdimension ist die Häufigkeit der Durchführung. Wird eine Leistung häufig und regelmäßig benötigt, so dürfte aufgrund von Skalen- und Lerneffekten eine Eigenerstellung besonders vorteilhaft sein. Dies gilt insbesondere bei hoher Spezifität. Eine hoch spezifische Leistung wird im Extremfall von einem Zulieferer nur für einen Abnehmer erbracht. Damit hat der Zulieferer in Bezug auf diese hochspezifische Transaktion keine Skalenvorteile gegenüber dem Abnehmer, falls dieser die nachgefragte Leistung selbst erstellt.[28]

Tabelle 7 fasst die Hypothesen des Transaktionskostenansatzes zu Einflüssen auf die Make-or-Buy-Entscheidung noch einmal zusammen. Man erkennt, dass eine hierarchische im Vergleich zu einer Marktlösung umso vorteilhafter ist, je höher Spezifität, Unsicherheit/Komplexität und Häufigkeit sind.

Einflussfaktor	**Vorteilhaftigkeit von „Make“ vs. „Buy“**
Spezifität	+
Unsicherheit/Komplexität	+
Häufigkeit	+

Tabelle 7: Hypothesen zu Einflussfaktoren des Transaktionskostenansatzes

[26] Vgl. Picot/Dietl (1990), S. 179.

[27] Vgl. Klein (2004), S. 14.

[28] Vgl. Bogaschewsky (1995), S. 168 f.

4.2 Weitere Einflussfaktoren

Neben den transaktionskostenbezogenen Faktoren sind auch ziel-, ressourcen- und marktbezogene Einflussfaktoren zu berücksichtigen, die sich aus der konzeptionellen Marketing- und Messeliteratur ableiten lassen. Tabelle 8 gibt einen Überblick über relevante Faktoren und die entsprechenden Hypothesen.

Einflussfaktor	**Vorteilhaftigkeit von „Make“ vs. „Buy“**
Zielbezogene Einflussfaktoren	
Bedeutung der Leistung	+
Ziele Umwegrendite	– / +
Ressourcenbezogene Einflussfaktoren	
Know-how-Bedarf	–
Kapitalbedarf	–
Technologie-Bedarf	–
Marktbezogene Einflussfaktoren	
Bedeutung von Kundennähe	+
Qualitätsanforderungen	+
+ positiver Einfluss – negativer Einfluss	

Tabelle 8: Weitere Einflussfaktoren

Zu den zielbezogenen Einflussfaktoren zählt zunächst die Bedeutung der Leistung für das Unternehmen.[29] Wie bereits in Abschnitt 2 diskutiert, werden Kernaktivitäten von besonderer strategischer Relevanz sicherlich im Unternehmen durchgeführt werden. Aber auch für andere Transaktionen gilt, dass die Organisationsform umso stärker zu einem „Make“ tendieren sollte, je größer die Bedeutung der Leistung für das Unternehmen ist.

Des Weiteren sind die besonderen Besitzverhältnisse von Messegesellschaften zu beachten, die dazu führen können, dass Anteilseigner mehrere Ziele verfolgen. So streben Kommunen bzw. Länder als typische Mehrheitseigner von Messe-

[29] Vgl. Picot (1991), S. 346.

gesellschaften in Deutschland in der Regel nicht nur einen hohen Gewinn der Messen an, sondern verfolgen auch gesellschafts- und regionalpolitische Ziele.[30] Vielfach wird hier die Umwegrendite als Erfolgsgröße herangezogen, welche widerspiegelt, welchen Einfluss Messen auf das wirtschaftliche Umfeld haben, z.B. im Hotel- und Gastronomiebereich. Die Koelnmesse beispielsweise beziffert die Umwegrendite für 2003 mit 4,90 Euro. Dies bedeutet, dass jeder durch die Koelnmesse erzielter Euro Umsatz den im Raum Köln ansässigen Unternehmen Umsätze in Höhe von 4,90 Euro bescherte. Öffentliche Institutionen als Anteilseigner haben somit ein Interesse daran, dass Transaktionen von in der Region ansässigen Unternehmen durchgeführt werden. Dies könnte Entscheidungen zur vertikalen Integration erschweren, wenn die Befürchtung besteht, der regionalen Wirtschaft Geschäft zu entziehen. In erster Linie sprechen die Ziele der Anteilseigner jedoch für eine vertikale Integration, wenn anderenfalls außerhalb der Region ansässige Unternehmen die Leistung erstellen würden.

In Zusammenhang mit ressourcenbezogenen Einflussfaktoren muss eine Messegesellschaft prüfen, ob die Dienstleistung besondere Anforderungen in Bezug auf das erforderliche Know-how sowie die notwendige Technologie und Kapitalausstattung stellt. Eine Integration von Services, die sich im Hinblick auf die Struktur, die Technologie und das Management wesentlich vom Kerngeschäft unterscheiden, ist oftmals mit prohibitiven Transaktionskosten verbunden.[31]

Schließlich muss die Messegesellschaft auch marktbezogene Einflussfaktoren berücksichtigen.[32] Durch die Eigenerstellung von Dienstleistungen agiert die Messegesellschaft sehr marktnah und erhält quasi aus erster Hand Kundeninformationen, z.B. Kenntnisse über die Präferenzen von Messeausstellern und –besuchern. Diese Informationen kann die Messegesellschaft im Wettbewerb für sich nutzen. Eine hohe Bedeutung von Marktnähe und den entsprechenden Kundeninformationen spricht daher für die vertikale Integration. Auch hohe Qualitätsanforderungen sprechen tendenziell dafür, Leistungen selbst zu erstellen, da hier eine stärkere Kontrolle gewährleistet werden kann.

[30] Vgl. Groth (1992), S. 162.

[31] Vgl. Picot (1991), S. 347f.

[32] Vgl. Homburg / Krohmer (2003), S. 711ff. Die Autoren fassen diese Einflussfaktoren als „Effektivitätsüberlegungen“ zusammen.

5 Untersuchung ausgewählter Make-or-Buy-Fragestellungen

Nachdem mögliche Make-or-Buy-Fragestellungen im Bereich der Services für Messeaussteller und –besucher identifiziert, Entscheidungsalternativen aufgezeigt und Einflussfaktoren benannt wurden, soll nun exemplarisch für Gastronomie- und Standbau-Services eine Make-or-Buy-Analyse durchgeführt werden. Dazu werden die in Abschnitt 4 diskutierten Einflussfaktoren auf die Vorteilhaftigkeit von „Make“- vs. „Buy“-Entscheidungen herangezogen.

5.1 Gastronomie-Services

Bei den Gastronomie-Services ist zunächst die Spezifität als sehr hoch einzustufen. Hier sind hohe Investitionen in standort- und sachkapitalspezifische Einrichtungen erforderlich. Insbesondere müssen Ausstattungsinvestitionen, z.B. für Großküchen- oder Restaurantausstattungen, getätigt werden, die bei Abbruch der Transaktion nicht anderweitig eingesetzt werden können. Unsicherheit und Komplexität sind für Gastronomie-Services eher als gering einzustufen. Die Nachfrage ist vergleichsweise einfach zu prognostizieren, und größere Marktveränderungen sind wenig wahrscheinlich. Schließlich kann die Häufigkeit der Transaktionen als groß bezeichnet werden, da Gastronomie-Services auf jeder Messe angeboten werden.

Bei den zielbezogenen Einflussfaktoren ist die Bedeutung von Gastronomie-Services für Messegesellschaften tendenziell gering bis mittel. Zwar kann eine schlechte Dienstleistungsqualität zu erheblicher Verärgerung bei Messeausstellern und –besuchern führen, und ein qualitativ hochwertiges Angebot kann ein gewisses Differenzierungspotential gegenüber den Wettbewerbern bieten.[33] Zentral für die Entscheidung von Ausstellern und Besuchern, an einer Messe teilzunehmen, dürften jedoch eher andere Faktoren sein. Vorgaben von Anteilseignern in Zusammenhang mit dem Ziel der Umwegrendite sind bei Gastronomie-Services nicht zu erwarten, da davon auszugehen ist, dass sowohl bei einer markt- als auch einer hierarchienahen Lösung die Wertschöpfung in der Messeregion verbleibt.

[33] Vgl. Suhling (2003), S. 1123.

Der Kapitalbedarf für gastronomische Einrichtungen ist hoch. Die erforderlichen Investitionen übersteigen in der Messegastronomie u.a. aufgrund von erhöhten Sicherheitsvorschriften und besonderen technischen Anforderungen durch Spitzenauslastungen zum Teil erheblich die sonst üblichen Gastronomieinvestitionen. Die Verfügbarkeit von Know-how und Technologien stellen dagegen keine wesentlichen Ressourcenbarrieren dar. Schließlich sprechen Kundennähe und bessere Möglichkeiten der Qualitätsüberwachung generell für eine Eigenerstellung von Dienstleistungen, zumindest jedoch für eine Kooperationslösung. Beide Aspekte dürften für Gastronomie-Services allerdings nur von mittlerer Bedeutung sein.

Tabelle 9 fasst die obigen Ausführungen zusammen und zeigt, welche Empfehlungen sich daraus ergeben. Man erkennt, dass einige Einflussfaktoren für eine hierarchienahe Lösung sprechen, während andere eine marktnahe Lösung vorteilhafter erscheinen lassen. Dies passt zu der Feststellung aus Abschnitt 3, dass deutsche Messegesellschaften unterschiedliche Organisationsformen wählen. Offenbar gewichten sie die einzelnen Argumente verschieden.

Einflussfaktor	**Ausprägung**	**Empfehlung**
	Einflussfaktoren aus der Transaktionskostenanalyse	
Spezifität	hoch	hierarchienahe Lösung
Unsicherheit/Komplexität	gering	marktnahe Lösung
Häufigkeit	hoch	hierarchienahe Lösung
	Zielbezogene Einflussfaktoren	
Bedeutung der Leistung	gering – mittel	marktnahe Lösung
Ziel Umwegrendite	bei allen Alternativen gleichermaßen erfüllt	–
	Ressourcenbezogene Einflussfaktoren	
Know-how-Bedarf	gering	hierarchienahe Lösung
Kapitalbedarf	hoch	marktnahe Lösung
Technologie-Bedarf	gering - mittel	hierarchienahe Lösung
	Marktbezogene Einflussfaktoren	
Bedeutung von Kundennähe	mittel	Kooperationslösung
Qualitätsanforderungen	mittel	Kooperationslösung

Tabelle 9: Gastronomie-Services

5.2 Standbau-Services

Die Spezifität von Standbau-Services ist als eher gering einzustufen. Standort- und sachkapitalspezifische Investitionen fallen nur in geringem Maße an, da die meisten Einrichtungen und Maschinen häufig auch bei Beziehungsabbruch weiterhin zum Einsatz kommen können. Auch die Unsicherheit in Bezug auf Leistungen und Umwelt ist eher gering. Schließlich ist die Häufigkeit der Transaktionen hoch. Die daraus folgenden Kostendegressions-Effekte sind dann besonders groß, wenn Standbau-Services auch für andere Messeplätze angeboten werden können.

Ähnlich wie bei den Gastronomie-Services sind auch Standbau-Services von geringer bis mittlerer strategischer Bedeutung für Messegesellschaften. Ein gelungener Standbau ist zwar wichtig für Messeaussteller. Aufgrund der Existenz zahlreicher Standbau-Unternehmen mit qualitativ hochwertigem Angebot ist das Differenzierungspotenzial für Anbieter allerdings begrenzt. Die Zielsetzung Umwegrendite könnte bei Standbau-Services tendenziell dazu führen, dass eine hierarchienahe Lösung bevorzugt wird. Ist bei einer Markt-Lösung doch zu befürchten, dass Standbau-Aufträge in erheblichem Umfang an nicht ortsansässige Unternehmen vergeben werden.

Bezüglich der Ressourcen geht eine Eigenerstellung der Standbauaktivitäten mit der Bereitstellung von erheblicher maschineller Ausrüstung und personellem Know-how einher. Auch wenn die erforderlichen Technologien bereit stehen, ist der Kapitalbedarf auf jeden Fall groß.

Schließlich kann die Bedeutung der Kundennähe in Bezug auf den Standbau als mittel eingestuft werden. Die Qualität von Messeständen muss aus Ausstellersicht sehr gut sein. Der Messestand ist der Kulminationspunkt eines ausstellenden Unternehmens und repräsentiert für die Tage der Veranstaltung den Unternehmenssitz.[34] Um die Qualität sicherzustellen und zu kontrollieren, könnte man eine hierarchienahe Lösung empfehlen. Allerdings sind in Deutschland Messebauunternehmen im FAMAB[35] organisiert. Um in diesem aufgenommen zu werden, müssen hohe Qualitätskriterien erfüllt und eingehalten werden. Im Bereich

[34] Vgl. Holtmann (2003), S. 69.

[35] FAMAB: Fachverband Konzeption und Dienstleistung Design ▪ Exhibition ▪ Event e. V.

der Standbau-Services kann unter Qualitätsgesichtspunkten also auch eine marktnahe Lösung angestrebt werden.

Tabelle 10 fasst die obigen Ausführungen zusammen und zeigt, welche Empfehlungen sich daraus ergeben. Auch hier wird deutlich, dass Einflussfaktoren unterschiedliche Organisationsformen besonders vorteilhaft erscheinen lassen. Die in der Realität zu beobachtenden Unterschiede in der Wahl einer „Make“- vs. „Buy“-Lösung deuten wiederum darauf hin, dass Messegesellschaften die einzelnen Argumente unterschiedlich stark gewichten.

Einflussfaktor	**Ausprägung**	**Empfehlung**
Einflussfaktoren aus der Transaktionskostenanalyse		
Spezifität	gering	marktnahe Lösung
Unsicherheit/Komplexität	gering	marktnahe Lösung
Häufigkeit	hoch	hierarchienahe Lösung
Zielbezogene Einflussfaktoren		
Bedeutung der Leistung	gering – mittel	marktnahe Lösung
Ziel Umwegrendite	bei Hierarchie besser erfüllt	hierarchienahe Lösung
Ressourcenbezogene Einflussfaktoren		
Know-how-Bedarf	hoch	marktnahe Lösung
Kapitalbedarf	hoch	marktnahe Lösung
Technologie-Bedarf	mittel	Kooperationslösung
Marktbezogene Einflussfaktoren		
Bedeutung von Kundennähe	mittel	Kooperationslösung
Qualitätsanforderungen	hoch/hohe Qualität von Fremdanbietern	–

Tabelle 10: Standbau-Services

6 Zusammenfassung

Make-or-Buy-Entscheidungen sind für Messegesellschaften besonders relevant in Zusammenhang mit den Services, die sie für Aussteller und Besucher anbieten. Messegesellschaften müssen prüfen, ob und in welcher Form diese speziellen Dienstleistungen selbst erbracht oder über externe Unternehmen angeboten werden sollten. Bei einer Eigenerstellung ergeben sich z.T. erhebliche Umsatzpotenziale, aber auch Risiken. Inwieweit eine solche vertikale Integration allerdings tatsächlich erfolgreich ist, hängt von zahlreichen Einflussgrößen ab.

In diesem Beitrag zeigen wir zunächst auf, welche Make-or-Buy-Entscheidungen Messegesellschaften zu treffen haben und welche Handlungsalternativen ihnen dabei zur Verfügung stehen. Anschließend leiten wir aus dem Transaktionskostenansatz und der konzeptionellen Literatur zum Messewesen Einflussgrößen auf die Vorteilhaftigkeit von „Make“ vs. „Buy“ ab.

Bei näherer Betrachtung von Gastronomie- und Standbau-Services zeigt sich, dass diese Einflussfaktoren keine eindeutige Empfehlung zulassen, sondern dass einige Argumente für eine vertikale Integration sprechen und andere für eine Marktlösung. Ob Messegesellschaften sich für eine „Make“- oder eine „Buy“-Lösung entscheiden, hängt also von der Gewichtung der Argumente ab. Diese ist bei den deutschen Messegesellschaften derzeit offenbar unterschiedlich, so dass in der Praxis unterschiedliche Organisationsformen zu beobachten sind. Für Messegesellschaften, die ihre Make-or-Buy-Entscheidung für einzelne Unternehmensaktivitäten neu überdenken wollen, bietet unser Beitrag eine Entscheidungshilfe.

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