Estimating Selectivity-Corrected Productivity Effects of Profit Sharing

by

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Abstract

Using German firm panel data, we analyze the productivity effects of profit sharing. Since selectivity should be a severe problem in this context, we combine matching with a difference-in-differences method to rule out this potential bias. Our results suggest that, in fact, selectivity matters. Nevertheless, after accounting for it, we find that the introduction of profit sharing leads to a significantly higher productivity.

Keywords: Incentives, Profit Sharing, Productivity.

JEL Codes: M52, J33, C14

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"Incentives are the essence of economics."

(Lazear, 1987, 744)

"The ultimate objective of empirical work on incentives should be to find out why firms use the compensation policies they do, and to determine the impact of such policies on productivity or welfare. (…) there is a need to develop empirical tests where productivity measures are related to compensation policies, where the source of the variation in such policies has been accounted for (…).

(Prendergast, 1996, 21)

"(…) the cross-sectional data illustrate that firms that use profit sharing have higher productivity than those that do not. In the cross-section, this could simply reflect the possibility that firms with no profits rarely introduce such schemes, so higher profitability could have little to do with the effect of such schemes .(…)"

(Prendergast, 1999, 42)

1 Introduction

Economists have analysed the incentive effects of remuneration systems like fixed wages, piece rates, or bonus payments for many years. Of main interest in this discussion has always been the impact of variable, output-dependent wage components on the firm's performance. One variant of such an incentive scheme is profit sharing, which means that, in addition to the regular salary, employees get a variable payment which is directly linked to the profits of the company. Contrary to traditional bonuses like piece rates which are based on individual performance profit-sharing is a collective payment scheme which is applied to all or at least a large group of employees. It is most suitable where individual incentive schemes are not useful, e.g. when work is flexibly organized, or individual output is difficult to measure (e.g. Holmström, 1982, Prendergast, 1999).

The intention of profit sharing is to improve productivity by increasing the employee's incentives to decrease efforts because she directly benefits from a higher profit. However, there are also opposing effects like the incentive for free-riding which could cancel out the positive incentive effect of profit sharing. Therefore, from theory, it is a priori not clear whether profit sharing really affects the firm's performance.

As a consequence, numerous empirical studies have investigated the productivity effect of

profit sharing in the last 20 years and demonstrated strikingly similar results.[1] The Organization for Economic Co-operation and Development (1995, 160) states, *"(…) The consistency of the findings is remarkable. Profit sharing is associated with higher productivity levels in every case, regardless of methods, model specification and data used* (…) ". Only recently, some studies do not find any significant effect.[2]

Surprisingly, despite of the huge evidence for a positive effect of profit sharing, it is only used by a small proportion of companies. For example, in most European countries less than 15 % of all establishments offered such a remuneration system in 2001 (Poutsma, 2001).

A possible explanation for the limited application of profit sharing is that it requires strong preconditions to be successful, which in turn the majority of firms probably do not meet. However, it is also possible that profit sharing is not causal for the better performance but that it is due to selection. It is very likely that better managed and highly profitable firms are more in favour of introducing such a sharing system. In this case, the observed productivity advantages would be present *before* such an incentive system is introduced and a simple comparison of firms with and without profit sharing would be misleading. Despite of the importance, very few studies address this potential selection bias. One exception is FitzRoy and Kraft (1995) by using a Heckman selectivity model (Heckman, 1976). The authors report very strong selectivity effects. However, the estimator has been criticized for its strong distributional assumptions. An alternative, non-parametric method is matching. In recent years, it has become quite popular in economics[3], also for evaluations of firms, since it accounts for the selectivity problem, at least on observables.[4]

For our study, we have panel data of German firms that introduce profit sharing. The use of panel data enables us to combine the matching procedure with a difference-in-differences approach to control for selection on observable *and* unobservable factors which could both otherwise bias our results. We find that selectivity is in fact a problem. Firms with profit sharing are already more effective before the share system is introduced. Moreover, we show

[1] Studies analysing productivity effects of profit sharing are e.g. FitzRoy and Kraft (1987), Möller (2000), for Germany; Wadhwani and Wall (1990), Kruse (1992), Doucouliagos (1995), Cahuc and Dormont (1997), Blasi et al. (2008) with data from other countries.

[2] Wolf and Zwick (2003) data find no effect of profit sharing on productivity in German establishments. Analysing data from the UK, Bryson and Freeman (2008) only report a positive impact of profit sharing on productivity if it is combined with employee share ownership.

[3] Many applications of this method in the field of economics are policy evaluations, mainly the effects of active labour market programs, where the observed units are individuals, see e.g. Lechner and Wunsch (2009) and Lechner, Miquel and Wunsch (forthcoming).

[4] There are also studies where matching is applied to evaluate profit sharing, e.g. Kraft and Ugarkovic (2006).

that the selection bias is quite substantial. It is very likely that this result also applies to other human resource practices.

Nevertheless, after considering selection, the introduction of profit sharing still leads to a higher productivity.

The paper is structured as follows. Section 2 contains a short theoretical discussion of the impact of profit sharing on productivity. Section 3 addresses the problem of a possible selectivity bias and other indirect productivity effects of profit sharing. Section 4 describes the econometric methodology used for our estimations. In Sections 5 and 6 we present our data and the results of the empirical analysis before we conclude in Section 7.

2 Theoretical Considerations

The basic idea of an introduction of profit sharing is to avoid any conflict of interest between employer and employees by letting the workers directly participate in profits. This should lead to an increased effort and a higher willingness to cooperate, raising firm's overall efficiency (Kruse, 1992).

However, an argument against an impact of profit sharing on productivity is also straightforward. The productivity effect of any additional effort of an individual worker has to be shared with all the other workers. Therefore, before choosing to increase the effort, each employee weighs the disutility of working harder and the positive effect of getting a higher performance-related salary, where, however, the amount depends on the total number of employees. Unless the firm is very small, it is very likely that it is not advantageous for the individual to increase the effort. With N being the number of employees this is usually called the *1/N* or *free rider* problem. If this is the case, profit sharing does not improve productivity.

The counterargument to the *free rider* problem is peer group pressure. Workers usually know more about the effort of their co-workers than supervisors. With profit sharing, each employee cares about a high effort of his co-workers. Therefore, if employees can effectively monitor each other and punish shirking colleagues, free-riding can be prevented. In this case, costs for vertical supervision are reduced due to horizontal supervision by peers (FitzRoy and Kraft, 1987, Kandel and Lazear, 1992). Another argument against the 1/N problem is interdependent worker productivity. If effort levels are complementary, shirking will also decrease the productivity of co-workers and therefore the costs of shirking will rise. Thus, whether free riding really poses a problem in larger firms depends on the organization and the underlying

technology (Adams 2002, 2006, Heywood and Jirjahn, 2009).

Nevertheless, a prerequisite for any performance effect is that profit sharing is regarded as being fair, balancing both the interests of labour and capital. In contrast to individual piece rates, the overall profit is a clear performance indicator (as long as the balance sheets are not manipulated). Profit sharing systems are usually not altered in response to a higher than expected performance of the workforce, which is sometimes the case with piece rates.[5] Hence, profit sharing is a reliable and verifiable claim on total returns.

Altogether, from a theoretical point of view, it is not clear *a priori* whether productivity effects of profit sharing exist. An empirical test is therefore essential.

3 The Issue of Selectivity and Other Indirect Effects

If profit sharing has a positive impact on productivity, the behaviour of most companies is hard to explain, since only a minority of firms in Western countries make use of it. According to the statistics of the European Commission (Poutsma, 2001) the percentage of firms with a profit sharing system in the European Union is in general quite low (Table 1)[6]. Exceptions are France and the United Kingdom, where the legal framework and the tax system encourage the use of profit sharing.[7]

It is hard to believe that the majority of capital owners are persistently unaware of the possible productivity effects of such an incentive scheme. This behaviour would be, however, rational if profit sharing does not *cause* a productivity increase. The observed positive correlation might be just due to selectivity if e.g. better managed firms are just more likely to introduce profit-sharing. In this case, productivity is already higher before introduction. We address this problem by using a combined matching and difference-in-differences approach.

Another reason for a higher productivity of profit sharing firms can be worker sorting. Given that workers' productivity is heterogeneous, it is very likely that performance-related pay will

[5] Adjusting the terms of the piece rate scheme to favour the firm might result in "ratchet effects". The standard ratchet effect implies that workers may be unwilling to work hard today because they fear that the employer may infer that the workers' cost of effort is low and thus will offer a lower wage in future periods (see Lazear, 1986, Gibbons, 1987).

[6] Although these figures are somewhat older they did not change significantly during the last years (see e.g. Bellmann and Leber, 2007 for Germany).

[7] Poutsma (2001) extensively discusses the country differences concerning financial participation in various member states of the European Union.

attract the more productive ones as they tend to benefit more from it. [8] As a consequence, firms with profit-sharing are more productive because they employ a higher share of high-skilled workers. The higher share can also have a positive impact on less productive employees through mutual learning. In this case, profit sharing has an indirect effect on productivity because of positive worker sorting (Lazear, 1986). We control for this indirect effect by including the share of qualified workers as explanatory variable.

Table 1 – Incidence of profit sharing in selected countries

Country	**Profit sharing establishments (%)**
Denmark	10
France	57
Germany	13
Ireland	8
Italy	5
Netherlands	14
Portugal	7
Spain	8
Sweden	20
UK	40
USA***	16
Canada***	15

Source: Poutsma, 2001, ***OECD, 1995.

Besides worker sorting, there is another reason why qualification and thus, productivity could be higher in profit sharing firms. Empirical studies often report a lower turnover rate in firms that apply profit sharing (see e.g. Azfar and Danninger, 2001). Longer tenure of the employees, in turn, creates incentives to invest into firm-specific skills. Hence, in these firms, general and specific skills are probably above the average which is then responsible for the productivity advantage.

Thus, it is very likely that profit-sharing firms differ in many respects from other companies and that these differences are responsible for the estimated effect, not because of the incentive effect of profit sharing. A simple comparison of firms with and without profit sharing is

[8] It is quite realistic that workers are risk-averse and prefer a fixed wage to a variable, performance related pay. Then a firm with flexible pay will attract the less risk-averse workers. It is reasonable to assume that these employees are also more productive.

therefore probably misleading. However, the majority of previous empirical studies ignore these issues.

4 Econometric Method

Regression-Adjusted Matching

In order to estimate the effects of profit sharing on productivity, we use matching and difference-in-differences, i.e. we control for selection into treatment on observables as well as for unobserved heterogeneity. We first explain the matching methodology and then how it is combined with a difference-in-differences approach. The idea of the matching method is quite intuitive as it is based on the comparison of treated with non-treated observations, where the group of non-treated control observations is constructed to be as similar as possible to the group of treated units. If a non-treated unit is not similar to a specific treatment unit, it is either omitted or gets a low weight during the comparison depending on the particular matching estimator that is applied (see Heckman, Ichimura and Todd, 1997).

We use matching to obtain a sample consisting of similar observations with regard to the factors Z_{it} which determine the introduction of a profit sharing scheme. Propensity score matching as proposed by Rosenbaum and Rubin (1983) is applied to overcome the high dimensionality problem caused by the large number of exogenous variables determining the implementation of profit sharing.[9] The propensity score is the probability of observing a certain treatment as a function of Z_{it}. Therefore, we estimate Probit models to compute the propensity score for each firm in our sample. Now treated and non-treated firms with similar propensity scores are matched. Different matching estimators are possible. We use nearest neighbour matching with replacement[10] where only the most similar observation of the control group serves as a match.[11]

Regression-adjusted matching is proposed by Heckman, Ichimura and Todd (1997, 1998) and implies that, in addition to the variables Z_{it} that affect the treatment status, exogenous variables X_{it} which influence the outcome variable Y_{it} are considered. The vector X_{it} which

[9] See Caliendo and Kopeinig (2008) for an overview of propensity score matching estimators.

[10] Matching with replacement allows the observations of the control group to serve for different matches. It avoids the problem that treated and control units with different probabilities must be matched if there are only few similar comparison units. The disadvantage of matching with replacement is a higher variance due to the smaller number of control units used (Dehejia and Wahba, 2002).

[11] In addition to one-to-one nearest neighbour matching, we also tried matching with, for example, two or three neighbours, which leads to very similar results.

determines the outcome and the vector Z_{it} which determines treatment participation do not have to be mutually exclusive. The crucial point of this approach is to control for selectivity into a treatment on the one hand and to take into account time-varying variables influencing the outcome variable in a separate regression on the other hand. As we consider productivity, regression-adjusted matching is essential, as this allows us to take into account the development of the production factors as well as of other variables. However, we modify the method slightly. Heckman, Ichimura and Todd (1997, 1998) suggest, in a first step, to calculate the residuals from a regression of Y_{it} on exogenous variables X_{it}. Afterwards the average residuals for treated and matched control firms are compared to obtain the average treatment effect on the treated. We reverse the order of these two steps. First, we construct a matched sample and afterwards we control for other time-varying variables which have an impact on productivity in a separate regression.

Difference-in-Differences

Using matching, we are able to control for selectivity on observables but not on unobservables. Therefore, we combine the regression-adjusted matching approach with difference-in-differences and estimate the following equation:

$$(1) \qquad Y_{it} = \beta X_{it} + \delta_1 D_i + \delta_2 T_t + \delta_3 I_{it} + \varepsilon_{it},$$

where Y_{it} is the outcome variable of interest and X_{it} is a vector of observable control variables. D_i is the treatment variable defined as a dummy which equals one if a firm is treated at some point in time (it also equals one in pre-treatment periods) and zero if not. It controls for unobservable differences between treatment and control group. T_t is a set of time dummies for all pre- and post-treatment years. The variable I_{it} has unit value for treated establishments in post-treatment periods. Thus, we are interested in $\hat{\delta}_3$ which measures the average treatment effect. With *t'* representing periods before and *t* periods after treatment the difference-in-differences estimate $\hat{\delta}_3$ can be written as

$$(2) \qquad \hat{\delta}_3 = (\bar{Y}_{X_{it},D=1,t} - \bar{Y}_{X_{it},D=1,t'}) - (\bar{Y}_{X_{it},D=0,t} - \bar{Y}_{X_{it},D=0,t'}).$$

$\bar{Y}$ stands for the sample average of the outcome variable Y for a particular group and year. Since we also use matching we allow for group-specific as well as time specific effects.

To sum up, our procedure is as follows: After computing propensity scores by estimating Probit models, we construct a subsample consisting of all treated firms and matched controls. In a second step, we apply difference-in-differences to this sample when estimating Cobb-Douglas production functions controlling for variables X_{it} and unobserved (group-specific) heterogeneity. By this methodology we intend to identify the average treatment effects on the treated and to solve the selectivity problem.

Please note that we distinguish between two different treatments. First, we compare firms with and without profit sharing and therefore consider the existence of such a payment scheme as treatment. In this case, we define an observation as treated if a firm applies profit sharing in every year we observe it in our sample. That means we exclude establishments which introduce profit sharing and only look at firms which always or never have profit sharing. Looking at the existence of profit sharing we use our variant of regression-adjusted matching but with this definition of treatment we can not control for unobservable differences yet. Therefore, in a second step, we analyse the effects of an introduction of profit sharing. For this purpose we use an unbalanced panel with establishments never having profit sharing in the observation period and firms which introduce profit sharing at some point in time. Treated firms are now defined as those that introduce profit sharing during our observation period. Please note, according to the difference-in-differences methodology (and in contrast to the definition of treatment for profit sharing existence), observations are also identified as treated in those periods before they finally introduce profit sharing, not only in those after introduction.

The main advantage of our methodology is the possibility to compare the productivity effect of the introduction of profit-sharing with the efficiency level before the introduction. If better-managed firms are more likely to introduce profit-sharing, the productivity of these observations is already higher before they actually introduce the share system. We consider both productivity levels so that the “true” effect of profit-sharing can be identified (second definition of “treatment”). This effect can then be compared with the coefficient that we estimate when we define “existence of profit sharing” as treatment as we do in our first step. The latter effect is the one commonly reported and interpreted in previous studies.

Difference-in-differences controls for common time trends and permanent differences between the two groups considered. A crucial assumption of difference-in-differences to be valid is that firms with and without profit sharing do not seriously change in their unobserved

characteristics over time (see e.g. Blundell and Costa Dias, 2009). Even though we do not know whether this assumption holds, we view the combination of regression-adjusted matching and difference-in-differences as the best possible method currently available to deal with observable and unobservable heterogeneity.

5 Data and Specifications

For our estimations, we use the German IAB Establishment Panel of the Institute for Employment Research of the German Federal Employment Agency. It is a survey of German companies employing at least one employee covered by social insurance. Data has been collected annually since 1993. The IAB Establishment Panel was extended to East Germany in 1996. Since 2001, more than 15,000 establishments have been observed every year. The panel provides detailed information on many labour market topics. A core set of topics (e.g. employment, turnover) is identical in every wave. However, there are additional questions which are posed irregularly like those on profit sharing. Data on this issue is available for the years 1998, 2000, 2001, 2005, and 2007. Our sample includes firms providing information on the relevant variables for at least two consecutive years between 1998 and 2008[12]. In order to maximize sample size, especially the number of firms that introduce a profit sharing scheme, we consider all firms that introduce profit sharing at any time during the observation period, not only in one specific year. After constructing all the required variables we only use the five years with information on profit sharing available for our analysis.

As mentioned earlier, we distinguish between firms which have profit sharing during all periods considered (existence of profit sharing) and firms which introduce profit sharing (introduction of profit sharing). A comparison between the effects of existence and introduction of profit sharing will turn out to be highly significant concerning identification of the true effect of profit sharing.

First, we estimate separate probit models for each treatment to obtain the respective propensity scores. The literature on profit sharing has identified a lot of determinants of profit sharing that has to be considered (see e.g. OECD, 1995, Pendleton et al., 2003). We use four firm size dummies (20-49, 50-249, 250-499 and more than 499 employees, control group: less than 20 employees), a dummy for young (founded after 1990) and one for East German

[12] As some questions always relate to the previous year (e.g. sales) we need the wave of 2008 to construct some variables for the year 2007.

establishments. Furthermore, we control for the legal form (limited liability), existence of a works council as well as collective bargaining agreements, and the share of exports of a firm. We also include year and industry dummies. Additional variables are a dummy for investment in information and communication technology (ICT), and three dummies controlling for other HRM practices, namely independent work groups, teamwork, and shift of responsibilities.

In a next step, nearest neighbour matching is separately applied on both samples. We match firms during the first year in which we observe them in our sample[13]. With respect to existence of profit sharing we first drop all firms which introduce this remuneration system and we identify 955 observations of firms which always use profit sharing. Our matched sample consists of 1676 observations (existence sample)[14].

Concerning the second sample, we exclude establishments which always apply profit sharing. Matching is again done on the first year's observation[15]. We obtain 1464 treatment observations (including periods before the introduction, $D_i=1$), of which 563 observations are from periods when profit sharing has been introduced ($I_{it}=1$). Our matched sample consists of 2533 observations.

As a robustness check we also use the full sample - firms without profit sharing, firms that introduce it and establishments which always have profit sharing. This implies that the difference-in-differences equation (1) is extended by a dummy variable with unit value when a firm always applies profit sharing. Then treatment is defined as use of profit sharing, independent of whether it is introduced or exists during all periods that we observe. We again apply nearest neighbour matching with replacement. Our matched sample consists of 4196 observations. As we will see in the next section, the separate estimations for existence and introduction have a much better matching quality as the joint estimation. Nevertheless, we will also present the results of the latter to show robustness.

Some of the variables we use to estimate the probability of the respective treatment are also employed in our main regressions, the estimation of Cobb-Douglas production functions. Output is measured as the logarithm of sales volume.[16] Unfortunately, there is no information

[13] As firms must be observed for at least two periods, 2005 is the last year an establishment can enter our panel.

[14] As we use nearest neighbour matching with replacement, firms without profit sharing are used as a match for more than one treated establishment.

[15] Note, in the case of the "introduction sample" matching takes place with respect to an observation which at this point in time actually has not yet introduced profit sharing.

[16] An alternative for the dependent variable is value added. We do not use this measure because the question on material costs is asked as share of intermediate inputs in total sales and the answers are not very reliable. Furthermore, this question is characterized by quite a large number of missing values. Addison et al. (2006) discuss the problems connected with this variable in more detail.

on the capital stock available in the IAB establishment panel. We follow the approach used for example by Zwick (2005) and Addison et al. (2006) working with the same data, and use replacement investments as proxy for capital.[17] Labour input is measured as the logarithm of the number of employees. To avoid endogeneity, we use lagged variables for both capital and employment. We control for the composition of labour by adding several variables. We include the share of part-time employees, of apprentices and of qualified employees. Furthermore, we control for differences in the (self-stated) technical standard of equipment (1=very good,…, 5=poor) of a firm. We also create a dummy for investment in ICT. Additionally, we use dummies for the legal form (limited liability), for firms covered by collective bargaining, for a works council active in an establishment, and for East German establishments. Finally, we add year dummies and the share of sales exported. Since we include numerous covariates and since we are mainly interested in the effects of the existence and the introduction of profit sharing, we only present the estimated coefficients for the other explanatory variables and do not discuss them.

6 Estimation Results

As explained above, matching is applied to three different samples: 1) existence of profit sharing, 2) introduction of profit sharing and 3) joint estimation of existence and introduction. The approach is complicated by the fact that in our sample not every observation has the same "starting point". In order to maximize the number of observations, firms may enter our sample in 1998, 2000, 2001, or 2005. Therefore, for every year separate matching has to be applied. As an example, the results of the three probit estimations for the year 2000 are presented in Table A2 in the Appendix – the one for existence,[18] for introduction of profit sharing, and for the combination of both treatments.[19] Our matching is successful as there are no longer any significant differences in the mean values of treated and matched firms for the two separate

[17] The share of expansion investment and the amount of total investment are asked in the IAB Establishment panel. Using these variables enables us to construct the total amount of replacement investments (see e.g. Zwick 2005).

[18] For the treatment "existence of profit sharing" the number of treated firms in 1998 is too small to use these observations for a separate probit estimation. Therefore, for existence of profit sharing, we combine observations of establishments which enter the sample in 1998 and 2000.

[19] The results of the probit estimations for all the other years are available from the authors upon request.

samples for "existence" and "introduction of profit sharing".[20] Matching also reduces the differences between treated and control establishments in the full sample, but in some cases significant differences still exist[21]. Thus, we prefer the separate estimations for each treatment.

The estimation results of the Cobb-Douglas production function are presented in Table 2. The results for "existence of profit sharing" where we only control for selectivity on observables but not on unobservables are presented in column 1.

Table 2 - Production Function

	Existence of profit sharing (1)	Introduction of profit sharing (2)	Existence and introduction of profit sharing (3)
ln(Employment$_{t-1}$)	0.871***	0.901***	0.877***
ln(Capital$_{t-1}$)	0.101***	0.106***	0.113***
Profit sharing (Existence)	0.249***		0.228***
Treated establishment		0.111***	0.105***
Introduction of profit sharing		0.098***	0.093***
Collective bargaining	0.102**	0.020	0.081***
Works council	0.131**	0.109**	0.134***
Share of qualified employees	0.542***	0.473***	0.570***
East German establishment	-0.053	-0.144***	-0.140***
Share of part-time employees	-1.021***	-0.588***	-0.795***
Share of apprentices	-1.241***	-0.827***	-1.036***
Limited liability	0.067	0.117***	0.093***
Export share	0.430***	0.452***	0.487***
Investment in ICT	0.084	0.087***	0.101***
Technical standard of equipment (1=very good,…,5=poor)	-0.091***	-0.063***	-0.067***
Number of observations	1676	2533	4196

Heteroscedasticity-consistent standard errors, Industry and year dummies included, ***/**/* denotes significance at the 1%/5%/10% level.
Source: IAB Establishment Panel, waves 1998-2008, own calculations (controlled remote data access via FDZ).

[20] The differences of the mean values of treated and matched control firms for the particular year for which we also present the probit estimations, can be found in Table A3. The mean values after matching for all the other years are available from the authors upon request.

[21] E.g. after matching for the year 2000 there still is a significant difference between treated and control establishments in terms of the frequency of use of one of the HRM practices (see Table A3, column 3).

We see that profit sharing appears to have quite a strong positive impact on sales. The highly significant coefficient of 0.249 implies that profit sharing firms have a higher productivity of about 28.3% compared to firms which do not use this remuneration scheme.
However, the second column of Table 2 shows that, after controlling additionally for unobservable factors, more than 50 % of the higher productivity observed for profit sharing firms has to be attributed to systematic differences between treated firms and controls. Those firms which introduce profit sharing are already more successful than the control firms before the new remuneration method has actually been installed. Hence, in particular the highly productive firms introduce profit sharing. Nevertheless, the actual introduction of profit sharing has an additional effect on top of the already existing productivity advantage and boosts productivity by about 10%.

The two coefficients of the treatment group dummy and the introduction dummy do not exactly add up to the coefficient of the variable "existence of profit sharing" in column 1, but their sum is somewhat smaller. Nevertheless, a Wald test on equality of the sum of coefficients of the introduction estimation ("treated establishment" + "introduction of profit sharing") and the coefficient of the variable "profit sharing existence" of the first estimation shows that they are not significantly different from each other.[22]
Our findings are confirmed by the results of the estimation where we include both treatments (column 3). The significant coefficient of 0.228 in the case of "existence of profit sharing" again indicates that firms which always apply profit sharing have a higher productivity than firms without profit sharing, and firms which introduce profit sharing are already much more productive before the introduction has taken place (significant coefficient of 0.105).[23] Moreover, the introduction of profit sharing still has a positive effect on productivity. Again, the sum of the two coefficients for "treated establishment" and "introduction of profit sharing" is smaller than the coefficient of "existence of profit sharing" but a Wald test demonstrates that the difference is not significant. All in all, we find evidence for the existence of selectivity effects. The generally better managed firms are more likely to introduce profit sharing. Nevertheless, profit sharing also has a productivity increasing effect

[22]To test this cross-model hypothesis based on overlapping samples we use the STATA command "suest" (see Weesie, 1999).
[23]In column 3, the coefficients for "existence of profit sharing", "treated establishment" and "introduction of profit sharing" are all slightly smaller than the corresponding coefficients for the separate estimations in column 1 and 2, but they are not significantly different.

which is, however, much smaller compared to the simpler method which neglects a before-and-after comparison. Considering selectivity effects is therefore essential for this kind of research.

Moreover, our results can explain why so many firms do not introduce profit sharing. The positive productivity effect might be too small if the implementation of such a remuneration system is quite costly.

7 Conclusions

The possible productivity effects of human resource practices have been discussed quite intensively in recent years. This encompasses organizational aspects as well as monetary incentives such as profit sharing. Looking at former studies investigating this topic, one could virtually speak of *common knowledge* that profit sharing increases productivity.

However, prior research has mostly neglected possible selectivity effects. In most cases, cross-sectional data has been used which makes it difficult to identify causal effects. Using the IAB Establishment Panel we applied the matching method and combined it with difference-in-differences. With this approach we are able to take these problems into account and to identify the average treatment effect on the treated.

Our empirical results point to a significant productivity effect of profit sharing even after controlling for possible selectivity effects. Therefore, our research confirms previous studies that profit sharing affects productivity in a positive way. However, we also show that not addressing selectivity especially on unobservables strongly overrates this effect. It is very likely that this problem is also relevant for other human resource practices which should be tested in the near future.

Moreover, the increase in productivity might not be high enough to cover fixed costs associated with the introduction of a new remuneration system. This could explain why only a minority of firms use this incentive scheme. These fixed costs probably differ between firms as well as the potential productivity improvement. Work content (simple or demanding), verification possibilities of the individual performance, i.e. work organization (team versus individual tasks), turnover, cultural differences, industrial relations, firm size and many other factors may affect the efficiency of profit sharing. If this is the case only those firms introduce this incentive scheme that have low fixed costs and/or their characteristics favour the positive

effect of profit sharing. The others rely instead on other motivational instruments like tournaments, promotions, or on dismissals as a penalty in the case of insufficient performance.

The identification of the population average treatment effect (instead of the average treatment effect on the treated) is a task for future research. Quantification of the welfare effects of subsidization of profit sharing firms or the obligation to apply a sharing system (like in France) clearly has very high relevance for public policy.

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Appendix

Table A1 - Mean values of firms with and without profit sharing

Variables	Firms without profit sharing	Firms with profit sharing
Number of employees	101	205***
Export share	0.08	0.18***
Share of qualified employees	0.37	0.53***
Collective bargaining	0.55	0.57*
Works council	0.33	0.57***
Limited liability	0.65	0.85***
East German establishment	0.42	0.33***
Age (founded after 1990)	0.40	0.40
Shift of responsibilities	0.17	0.27***
Teamwork	0.11	0.16***
Independent work groups	0.09	0.15***
ICT investment	0.78	0.91***
Technical standard of equipment (1=very good,…,5=poor)	2.12	2.04***
Share of part-time employees	0.14	0.11***
Share of apprentices	0.06	0.05**
Sales per employee (in 1000 €)	136.00	209.69***

***/**/*: Mean values are significantly different at the 1%/5%/10% level.
Source: IAB Establishment Panel, waves 1998-2008, own calculations (controlled remote data access via FDZ).

Table A2 - Results of Probit Estimations

	Existence of Profit Sharing (1998 & 2000)	Introduction of Profit Sharing (2000)	Existence and introduction of profit sharing (2000)
Establishment size 20-49	0.349**	0.151	0.203**
Establishment size 50-249	0.467***	0.298**	0.338***
Establishment size 250-499	0.673***	0.432**	0.514***
Establishment size 500+	0.835***	0.731***	0.708***
Export share	0.732***	0.168	0.366**
Share of qualified employees	0.689***	0.605***	0.723***
Collective bargaining	-0.070	-0.302***	-0.215***
Works council	0.479***	0.398***	0.421***
Limited liability	-0.022	0.193**	0.164**
Age (founded after 1990)	0.262**	0.128	0.169**
East German establishment	-0.150	-0.159*	-0.163**
Shift of responsibilities	0.303***	0.132	0.201***
Teamwork	0.129	0.038	0.099
Independent work groups	-0.170	0.047	-0.001
ICT investment	0.392**	0.276**	0.346***
Number of observations	1531	1697	1990
Pseudo R2	0.180	0.132	0.146

Industry and year dummies included, ***/**/* denotes significance at the 1%/5%/10% level.
Source: IAB Establishment Panel, waves 1998-2008, own calculations (controlled remote data access via FDZ).

Table A3 – Matching Quality: Differences in mean values after matching

	Existence of Profit Sharing (1998 & 2000)	Introduction of Profit Sharing (2000)	Existence and introduction of profit sharing (2000)
Establishment size 20-49	0.031	-0.007	0.008
Establishment size 50-249	-0.026	-0.032	0.004
Establishment size 250-499	-0.046	-0.003	-0.025
Establishment size 500+	0.036	-0.010	0.008
Export share	0.002	-0.001	-0.011
Share of qualified employees	0.005	-0.013	0.003
Collective bargaining	-0.010	0.003	-0.025
Works council	-0.026	0.027	-0.019
Limited liability	0.026	-0.015	-0.010
Age (founded after 1990)	-0.077	0.007	0.031
East German establishment	-0.046	0.007	-0.005
Shift of responsibilities	0.005	-0.042	0.051**
Teamwork	0.000	-0.010	0.008
Independent work groups	0.005	-0.037	0.011
Ict investment	-0.010	0.003	0.016

Source: IAB Establishment Panel, waves 1998-2008, own calculations (controlled remote data access via FDZ).