The role of selection effects in organizational change:

Empirical evidence on worker participation and human resource management practices in Germany

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Contents

List of Tables

List of Figures

1 Introduction

1.1 Human resource management practices and employee participation – expected effects and incidence in Germany

Good workplace conditions and a cooperative relationship between management and workers are essential for the economic success of firms as well as employees. Personnel development, employee participation and involvement can improve communication between employer and workers and also help to optimize working processes and thus, be beneficial to all parties concerned, e.g. by increasing motivation, wages or job satisfaction of workers and firms' performance. During the last decades employment relationships have been strongly affected by changing labour market conditions. The acceleration of technological progress as well as structural and demographic change challenge both companies and employees. Especially in such times of rapid change in different fields it can be essential to establish stable and efficient industrial relations. There are several legal and voluntary arrangements and instruments which can influence working conditions. Besides traditional institutions like works councils and unions, also human resource management (HRM) practices play an important role in shaping the working environment and management-employee relationship.

Industrial relations have been subject to several developments during the last decades. A distinctive characteristic of industrial relations in Germany is a dual system of employee representation. It is based on unions which are solely responsible for collective bargaining and on works councils which represent interests of the employees on the establishment level. The existence of a works council is formally independent of unions. Nevertheless, works councils and unionization are positively correlated.[1] In Germany both union membership and collective bargaining coverage have remarkably decreased (Fitzenberger et al., 2011, Antonczyk et al., 2011). The incidence of works councils has also declined. Compared to a share of 17.2% of all German establishments with works councils which covered 56.4% of all employees in 2000 the percentage diminished to 13.4% and 50.6% in 2010, respectively (Datenkarten 2005, 2011, Hans-Böckler-Stiftung).

However, in Germany works councils are powerful institutions which are still very common especially in larger establishments. In contrast to several methods of voluntary employee participation, the rights of German works councils are legally defined by the

[1] Unions can submit a list with candidates for the election of a works council (Addison et al., 2001) and sometimes they even play an important role in initiating its implementation (Schlömer et al., 2007).

Works Constitution Act, which was introduced in 1952 and extended several times later on. Works councils can be elected in every establishment with five or more employees.

As workers' interests are represented, works councils should be beneficial to employees, but they are often said to affect employers adversely because they facilitate rent-seeking behaviour of employed insiders. Efficiency losses can occur as rights of co-determination could slow down decision-making and might delay necessary adjustments. The alternative opinion goes in line with the argumentation of Freeman and Medoff (1984) concerning the effects of unions. Works councils could improve communication between management and workers. Freeman and Lazear (1995) argue that information rights of works councils could also enhance efficiency and productivity in firms.

Structural changes can also necessitate the use of HRM practices (maybe as a complement to or even as a substitute for unions and works councils) to improve employer-employee relations and to adapt to changing market conditions. Intensifying in the 1990s, there has been a growing interest in how innovative human resource management can shape and improve working conditions. Godard and Delaney (2000) critically note that many researchers promoted a new paradigm focusing on "high performance" work and HRM practices which could replace more traditional labour institutions like unions or works councils.

In contrast to works councils which are initiated by the employees, the introduction of HRM practices is mainly promoted by the management and the personnel department. Nevertheless, e.g. in the case of training, it could also be the worker who suggests participation in courses. Workers could benefit from the implementation of such practices as they can enhance working conditions, facilitate professional development and improve employability, whereas firms could profit in form of higher productivity and increased performance. However, such organizational changes do not necessarily involve improvements for both sides.[2] High performance work practices (HPWPs) include a wide range of measures like team work, quality circles, job rotation, training or financial participation of employees, e.g. employee share ownership or profit sharing.

With regard to such group incentive payments, profit sharing is a rarely used practice in Germany. Bellmann and Möller (2011) find that, throughout the period from 2001 to

[2] HRM practices can increase productivity but this could also be at the expense of workers' job satisfaction if the new practices increase the pressure to perform or worsen the working atmosphere.

2009, only about 9% of German establishments offer profit sharing to their employees.[3] However, financial participation of workers is a regularly discussed topic in politics and sciences. The debate was renewed in 2005 when the former Federal President Horst Köhler suggested that employees in Germany should participate in the positive development of their companies (Bundespräsidialamt, 2005). Finally, in 2009 a new law promoting employee share ownership (the "Mitarbeiterkapitalbeteiligungsgesetz") came into force (Bundesgesetzblatt I 12, 2009). The political goal was to increase employee participation in the success and capital of the companies they work for, e.g. by tax advantages. However, all the new regulations only apply to share ownership schemes but not to profit sharing schemes.[4] From the employer's point of view, incentive payments like profit sharing should mainly boost employees' motivation and productivity because part of their wage depends on the overall performance of the firm and every worker should then behave profit maximizing. Besides productivity-related motives, Kruse (1996) describes several further reasons why firms could have an incentive to use group performance payment schemes. Profit sharing could also reduce labour turnover (e.g. because in bad economic situations establishments can more easily react by reducing wages instead of laying off workers). Reduced employee turnover increases the amortisation period of investments in human capital and thus, also the returns to training activities. Hence, besides a direct productivity effect caused by higher motivation of workers, there might also be an indirect effect via training.

Yet not only firms can benefit from investments in the qualification of workers. In the course of technological change, investments in human capital have become important for most employees to be successful in their job as they are regularly faced with new demands at the workplace, e.g. operation of new machines or application of new methods or computer programmes. Educational policy demands for lifelong learning point out its relevance in times of demographic challenges. The European Union, for example, emphasizes the need for continuous training in the strategic framework for European cooperation in education and training, where one of the strategic objectives is "Making

[3] Although profit sharing for employees is not a very common payment system in Germany and this is also true for many other countries, there are exceptions. In France for example, where profit sharing is mandatory for larger firms, according to the European Company Survey (ECS) data, 35% of all private companies with more than 10 employees apply profit sharing (see European Foundation for the Improvement of Living and Working Conditions, 2010).

[4] Although these new regulations should increase incentives for using employee share ownership, it remains to be seen whether they will lead to a rise in the application of such payment schemes. Bellmann and Möller (2011) show that until 2009 the share of establishments with employee stock ownership was even lower than the share of firms with profit sharing.

lifelong learning and mobility a reality" (Council of the European Union, 2009). Starting with Becker (1962) a lot of theoretical work on training investments and their effects has been carried out until today. It depends on the type of training, general or firm-specific, who invests in and profits from training and to what extent, the employee or the employer. Where employers profit from higher productivity of trained employees, workers should benefit from training investments in form of higher wages, enhanced employability and a reduced unemployment risk.

The growing relevance of lifelong learning also reflects in rising participation rates. In Germany, the share of people who participate in further vocational training has increased during the last decades and now is quite constant. The figures vary depending on the particular survey and the period under review. At the end of the 1970s the share of people who took part in training during a period of twelve months was about 10% and rose substantially to more than 25% in the mid-1990s (according to data from Berichtssystem Weiterbildung, see Kuwan and Thebis, 2005). The Continuing Vocational Training Survey even indicates shares of 32% in 1999 and 30% in 2005 (Behringer et al., 2007). According to the European Labour Force Survey, in 2010 almost 8% of all respondents attended training courses during the four weeks prior to the date of interview. However, compared to other European countries, especially the share of older employees participating in continuous training is quite low in Germany (Eurostat, Labour Force Survey).

To sum up, just like co-determination, the described HRM measures can have an impact on various firm level and individual economic outcomes, probably depending on job, individual and firms characteristics. Godard (2004) emphasizes that high performance work practices could more likely be beneficial to employers but also to employees in co-ordinated market economies like Germany where, among other things, workers have strong co-decision and representation rights. Frick (2002) also stresses the role of works councils in increasing employees' acceptance of changing working conditions. Thus, there could be a direct link between employee participation in form of works councils and the application and effectiveness of HRM measures.[5] With respect to co-determination at firm level, the focus of this dissertation is on determining the reasons for and

[5] Although, in this dissertation, no differentiation is made between the effects of the considered HRM measures with respect to the existence of co-determination, the relevance of works councils for the probability to implement such practices is taken into account if possible (see Chapters 3 and 4). For an overview over empirical evidence for the US and Germany considering a possible link between employee representation and high performance work practices and their effects on firm performance see Addison (2005).

consequences of introducing a works council for both firms and workers. The empirical analyses of the impact of profit sharing concentrate on two establishment level outcomes, firms' training intensity and productivity. Finally, the effects of training participation on workers' wage and perceived job security are examined.

A major difference of the three considered measures is their frequency of use. As described above, about half of all German employees are represented by a works council and a significant proportion of workers attend training courses. Only a small proportion of all companies, however, offer profit sharing schemes. Thus, if personnel actions induce positive effects, especially the introduction of profit sharing could lead to a competitive advantage for the respective companies. The relatively large proportion of employees who participate in training and have a works council in their firm, however, might indicate that both instruments have proven to be effective in achieving certain objectives.

1.2 A brief review of previous empirical literature and contributions of this dissertation

The three measures of employee participation and involvement this dissertation deals with are frequently analysed in the empirical literature. The effects of works councils have been of particular interest in Germany, where their co-determination rights provide them with notable power. Despite the fact that co-determination should have positive effects for employees, many establishments in which workers are entitled to elect a works council have none. Thus, it can be expected that selection effects may play an important role in this context and that works councils are introduced under certain circumstances.

An increasing empirical literature deals with the effects of innovative HRM measures (or high performance work practices) and their complementarities.[6] Godard (2004) argues that even the positive effects of such HPWPs for employers, which are often found in empirical research, are not unambiguous, much less are possible effects for employees and unions. The benefits of changing work organization depend on several characteristics of a firm and, among other things, selection may play an important role for the frequently found positive impact on firm's performance. With respect to financial employee participation, a large number of studies from different countries focus on different aspects

[6] Examples for such empirical studies which include several HRM practices are Huselid (1995), MacDuffie (1995), Ichniowski et al. (1997) and Wolf and Zwick (2003).

of financial group incentives like profit sharing. For continuing training the literature is even more extensive. This section gives a short overview over previous results and describes the contributions of this dissertation to different aspects of organizational change. As the incidence of the considered measures seems to depend on specific factors, all analyses in this dissertation have in common that selection effects are explicitly controlled for by the application of propensity score matching and difference-in-differences. These methods take into account observable or unobservable factors which could affect both the treatment status and the outcome variables of interest. Matching addresses the problem of selection on observables. The general advantages of matching over linear regression analysis are that the former is non-parametric and it imposes the common support assumption. In the absence of common support the model needs to be extrapolated over unobservable regions of the distribution of the right-hand variables. Although the common support assumption could also be imposed within a linear regression framework but researchers often ignore the common support problem (Blundell and Costa Dias, 2009). Smith and Todd (2005) point to the importance of using difference-in-differences in addition to matching. The difference-in-differences approach – similar to fixed-effects estimations – deals with time-invariant unobservable heterogeneity. It is usually used in the case of "natural experiments" where a group of the observed firms or people experience exogenous variation in the treatment variable of interest, e.g. caused by policy reforms. However, the method has also been used in settings with selection into treatment, like in the case of active labour market policies (Smith, 2000). In such evaluation studies, difference-in-differences is often combined with matching (Heckman et al., 1997, 1998). Both methods critically depend on assumptions about the selection process which will be discussed in the several chapters. The use of two different data sets, one on the establishment level and one on the individual level, allows to analyse effects both on the employers' and the employees' side.

1.2.1 Works councils

The extensive empirical literature on works councils deals with the effects of such institutions on economic outcomes like wages, productivity, profitability, employment or innovation activities. Addison et al. (2004b) and Jirjahn (2011) give comprehensive overviews over studies analysing the impact of works councils in Germany. Although

older studies draw a quite pessimistic picture of the effects of works councils, many more recent analyses find that they can positively influence a firm's performance. Jirjahn (2011) points out to the fact that the introduction of such a form of workers' representation might be endogenous. If this was not taken into account, effects could be under- or overestimated. Jirjahn (2009) uses the Hannover firm panel and finds that works councils are more likely to be adopted in establishments suffering from poor economic conditions. The results support the idea that employees demand works councils to protect their rents. Mohrenweiser et al. (2012) analyse trigger events for introducing a works council and come to the conclusion that works councils should protect workers against uncertainty.

Addison et al. (2010) show that firms with works councils pay higher wages. Pfeifer (2011) analyses effects on wages, productivity and firms' profits and provides evidence in favour of the productivity enhancing and rent-seeking functions of works councils. The results of Beckmann et al. (2010) also support the rent seeking hypothesis.

The study "**The causes and consequences of adopting a works council**" in Chapter 2[7] also addresses the question whether employees aim at claiming larger parts of the rent or at protecting their rents in bad economic situations. More precisely, on the establishment level, the determinants of introducing a works council are examined as well as effects on the wage level and on employee turnover. Moreover, on the individual level, besides the impact of a works council on workers' wages and overtime hours, especially effects on job security are analysed. Two different data sets are used, the German Socio-Economic Panel (SOEP) and the IAB Establishment Panel. This analysis contributes to the existing literature by finding that selection and firm heterogeneity play an important role in the context of works councils. Their election is initiated by the employees in establishments with low job security and comparatively high wages. The introduction of a works council does neither increase wages nor decrease fluctuation nor overtime work but reduces workers' concerns about losing their job. This is counter evidence for the rent-seeking hypothesis. Works councils seem to be an instrument to protect existing rents and secure workers' jobs.

[7] This chapter is based on a joint work with Kornelius Kraft.

1.2.2 Employee profit sharing

As described above, profit sharing is a quite infrequently used form of incentive payment in Germany. As the literature on the effects of such payment schemes mainly finds that firms benefit from its use or at least are not harmed, selection also seems to play an important role in this context. If managers are rational and aware of the potential positive effects of profit sharing, it could be expected that a majority of companies would apply it. An explanation for the fact that less than 10% of German establishments offer profit sharing to their employees could be that firms which apply this payment scheme have special characteristics and maybe also particular advantages with regard to its implementation and usefulness.

The most frequently analysed topic in the empirical literature on profit sharing is its impact on firm's productivity and performance. Many studies report positive effects (for Germany see e.g. FitzRoy and Kraft, 1987a, Möller, 2000)[8]. However, in some cases no positive impact is found. For example, Wolf and Zwick (2003) analyse German data and come to the conclusion that financial group incentives (profit sharing and employee share ownership) do not have an effect on productivity. Chapter 3 also deals with this topic. The study "**Just a question of selection? -The causal effect of profit sharing on a firm's performance**"[9] considers potential selection effects by combining propensity score matching with difference-and-differences. Using German panel data from the IAB Establishment Panel, production functions are estimated. The results show that especially productive firms introduce profit sharing. As the higher productivity before the implementation of profit sharing cannot completely be explained by observable factors, these firms may just be the better managed ones. However, although part of the higher observed productivity in profit sharing establishments is not caused by the use of this payment method, there is still a positive causal effect. The introduction of profit sharing increases productivity but not controlling for selection, especially on unobservable factors, can lead to an overestimation of its impact.

There are only few studies which focus on the impact of profit sharing on investments in human capital which in turn could also affect productivity. As described above, there are some theoretical arguments which point to a positive effect of profit sharing on training intensity. Previous studies on this topic are Azfar and Danninger (2001), Parent (2004),

[8] For international evidence see e.g. Wadhwani and Wall (1990), Kruse (1992), Kumbhakar and Dunbar (1993), Doucouliagos (1995), Cahuc and Dormont (1997), Fakhfakh and Perotin (2000), Blasi et al. (2010) or the Oxera-Treasury report (2007a, b).

[9] This chapter is based on a joint work with Kornelius Kraft.

Gielen (2011) and Green and Heywood (2011) who provide evidence in favour of such an effect. The study "**Profit sharing and training**" in Chapter 4[10] differs from these analyses by applying establishment level instead of individual level data. Moreover, it is possible to differentiate between firms which offer profit sharing to different shares of workers. Conditional difference-in-differences estimations reveal that the introduction of profit sharing only increases training intensity in firms which let the majority of all workers participate in profits.

1.2.3 Continuous training

Investments in human capital of workers should not only be beneficial to companies but also to participants. Innumerable empirical studies deal with the determinants and effects of continuing vocational training. For overviews over findings of previous studies see e.g. Bassanini et al. (2007) and Hansson (2008). One result of this training literature is that most investments in human capital take place at the beginning of working life. The probability to receive training decreases with age, but also the effects of training can depend on the age of participants. Most of the empirical studies analysing effects on the individual level focus on workers' wages. There is evidence for positive wage effects especially for younger employees (see e.g. Büchel and Pannenberg, 2004, Bassanini, 2006). Besides affecting wages, training can also enhance employability. However, regarding differences in the effects on job security and unemployment risk between older and younger employees, empirical evidence is mixed (see e.g. Büchel and Pannenberg, 2004, Bassanini, 2006, Picchio and van Ours, 2011). Beicht et al. (2006) and Zwick (2011) use German data sets and analyse training goals and the self-assessed effectiveness of training. They find that training purpose and perceived benefits can be very different for workers of different age. In the study "**The aims of lifelong learning: Age-related effects of training on wages and job security**" in Chapter 5 the effects of training on two different labour market outcomes for different age groups are explained by the fact that the purpose of courses is often different for older and younger employees. The results indicate that positive wage effects for younger employees and an increase in job security of older employees might be attributed to different training goals. Moreover, if the training history of participants is taken into account, there seem to be decreasing marginal returns to training participation with respect to job security.

[10] This chapter is based on a joint work with Kornelius Kraft.

2 The causes and consequences of adopting a works council

This chapter is based on the publication

Kraft, Kornelius and Julia Lang (2008): The Causes and Consequences of Adopting a Works Council, Jahrbücher für Nationalökonomie und Statistik, 228(5/6), 512-532.

2.1 Introduction

In Germany there is a lively discussion on the effects of works councils and this institution has also found some attention in other countries. The topics often dealt with are: consequences for productivity, profitability, turnover, investment, innovation and wages. We intend to enlarge the discussion in two respects. In the first place we investigate the reasons for introducing a works council. Subsequently we consider the effects of introducing a works council by use of a difference-in-differences framework. The application of this approach is motivated by the attempt to control for specific and fixed circumstances relevant for a company which may well affect the coefficient of the works council variable.

Two very different explanations for the adoption of a works council exist. On the one hand, works councils increase the bargaining power of the workforce and could therefore be used for rent sharing. Where this is the motivating factor, works councils will be elected if a company is prospering and if it is expected that the relevant business conditions will not deteriorate. However, the opposite is also possible. Most co-determination rights of works councils aim at protecting the employees against arbitrary decisions by the management. Every dismissal has to be justified and works councils have the possibility to oppose a dismissal if certain conditions are not met. If this is the main reason for introducing a works council, then it is likely to be at times when business prospects are relatively weak. We investigate whether the probability of a works council being introduced is higher in good or in poor economic conditions. [1]

Several studies have looked at the wage level in firms with and without works councils. It is usually found that firms which have works councils pay higher wages. We consider this question by application of a difference-in-differences model. This means we investigate the change in wages after the introduction of a works council. Moreover, we analyse effects on employee turnover, overtime work and perceived job security of workers.

We use two different data sources, and in one case it is possible to match the firms and to eliminate selectivity on observables.

The remainder of Chapter 2 is structured as follows: The next section provides an overview of the theoretical background and previous empirical research on the effects of works councils. Afterwards, Section 2.3 shortly describes our empirical strategy. Section

[1] Jirjahn (2009) also considers the reasons for the introduction of works councils by use of the Hannover Firm panel. The differences between his and our approach are the data sources, the determinants considered and the investigation of the consequences with the help of (conditional) difference-in-differences estimations.

2.4 and Section 2.5 present the data, estimation methods and results for the analysis with the IAB Establishment Panel and the SOEP, respectively. Section 2.6 concludes.

2.2 Background

2.2.1 Theoretical framework

The rights of works councils are defined by the Works Constitution Act, which was introduced in 1952 and extended in 1972, 1989 and 2001. According to this law works councils can be elected in establishments with five or more employees. However, works councils are by no means mandatory. In particular, smaller companies frequently have none. Their introduction depends on the initiative of the workforce. The workforce must meet to determine an electoral board which in turn carries out the election and ensures the introduction of the elected body.

The theoretical discussion about the effects of works councils has some relation to the discussion about unions but is also dissimilar in some respects. One can safely say (similarly to the discussion about the effects of unions) that the views on works councils are quite conflicting. The Works Constitution Act grants works councils explicit co-determination or veto rights, which provide them with quite a lot of bargaining power and go well beyond what is known in other countries. These rights rise to some extent with the size of the company, measured by the number of employees.

In a neoclassical view, labour market efficiency needs no institution that increases the bargaining power of the workforce. Therefore, some observers argue that this intervention into the labour market results from interest group activities during times when the Social Democrats were in power (either together with the liberals in the Seventies or in a coalition with the Green Party from 1998 until 2005). If this view explains the extension of works councils' co-determination rights, they may well act as a rent-seeking institution. Consequently works councils could aim at increasing wages, securing the employment of the current insiders and at improving working conditions in a way that workers' preferences are met. The results of empirical studies which found that companies with works councils pay higher wages are in accordance with this view.

The alternative opinion is much more optimistic about the effects of works councils. In line with the argumentation of Freeman and Medoff (1984) concerning the effects of unions, works councils are assumed to support a voice channel which improves

communication between management and employees. Communication (voice) might act as a substitute for exit in case of conflict. Then turnover rates should be lower in the presence of a works council. Low quit rates imply a more intense use of firm-specific skills, and thus productivity would rise. In this case works councils would be beneficial to companies and create rents.

FitzRoy and Kraft (1987b) argue that works councils are even better suited for introducing such a voice mechanism than unions, as communication between this institution and management is mandatory. Works councils are democratically elected representative bodies, which collect information on the preferences of workers. They allow workers to voice their discontent about workplace conditions as an alternative to quitting. Moreover, works councils can help to determine "optimal" working conditions in terms of allocation of working time, production speed or other issues of working conditions, thus solving a public good problem. The improved exchange of information and consultation as well as co-determination rights could increase efficiency in terms of productivity. It is most likely the legislator had something like a "voice" institution in mind when the Works Constitution Act became effective, but participation rights go far beyond this.

Works councils negotiate a number of issues and have a number of explicit co-determination or veto rights concerning topics such as new payment methods, determination of working time and the introduction of new technologies, in particular if they are designed to monitor the employees. On other issues works councils have to be informed or consulted before the decision is actually reached. Perhaps works councils are something like the archetypical voice-institution in the sense of Freeman and Medoff (1984) and many researchers in Germany have sympathies with this notion.

The typical counter-argument to this view is that apparently such institutions are not implemented by the capital owners themselves, although they are purportedly beneficial to the company as a whole. In a neoclassical world such an efficiency-enhancing institution would emerge without a need for intervention by a legislator. Freeman and Lazear (1995) offer a number of arguments as to why the market solution may be inefficient. They state that any participation rights are connected with a redistribution of resources, and that it is this redistribution process which impedes voluntary agreement. Even if the introduction of a works council would increase total rents, due to the bargaining power of the employees the share received by the capital owners might shrink by so much that their overall return would be lowered.

Formally the Works Constitution Act precludes negotiations between works councils and the management on wages, except on premiums and the introduction of new remuneration systems in general, but the veto rights of works councils on other issues provide them with bargaining power which could be used to bargain for higher wages. One way to reach this goal is by placing workers into higher wage groups. Wage groups in turn are determined on the industry level by negotiations between the unions and employer associations. Works councils and the management are responsible for grouping on the establishment level all jobs according to the industry-wide agreed skill structure.

In contrast to wage determination, works councils have an explicit co-determination right with respect to working time. Overtime has to be talked over with the works council and its introduction as well as its extension can be rejected by this body. Hence, here an effect would be more plausible than with respect to wages.

Moreover, works councils have quite substantial participation possibilities with respect to dismissals. Every dismissal has to be discussed with the works council and it can be opposed if it is considered to be unfair. The qualification "unfair" also includes the disregard of social factors (tenure, marital status, number of children).[2] Furthermore if a works council exists, in case of mass dismissals, redundancy payments are mandatory. Hence, dismissals become more expensive, can be delayed or even avoided at all if a works council exists. However, works councils need not necessarily behave in such a way. The works council is the "natural" institution to discuss with in case of any problem, and it could also be quite helpful if concession bargaining is needed. At the very least, communication with workers will be significantly more difficult in its absence, as no representative body exists and the management has no "natural" person to contact. Works councils could facilitate a (temporary) wage cut, if, in exchange for that cooperation, employment is secured for some time period.

Summarizing in line with the argumentation by Jirjahn (2009), if works councils act as an institution which protects employment security, they will be more frequently adopted if the company is in a crisis, experiences financial pressure and employment is in jeopardy. If in contrast, works councils aim at rent sharing, they are more frequently adopted if there is something to be distributed and if business conditions are rather good.

[2] However, the number of cases where a works council opposes a dismissal is not large.

2.2.2 Previous research

Starting with FitzRoy and Kraft (1985, 1987b) numerous studies have analysed the effects of works councils on several economic outcomes. The results are – as with the theoretical discussion – quite conflicting. The first studies by FitzRoy and Kraft (1985, 1987b, 1990) and Kraft (1986) express a rather negative view on the effects of works councils. According to their results works councils are associated with lower productivity, profitability and innovativeness. An effect on quits cannot be determined. These studies were based on a sample with a rather limited number of observations, as it was conducted before the much larger Hannover Firm Panel and IAB Establishment Panel data sources were available.

With respect to overtime work earlier studies report mixed results. Hübler and Meyer (1997) and Kölling (1997) show that overtime work is more frequently used in establishments with works councils, whereas Schank and Schnabel (2004) find no clear effects. However, in these studies only the existence of a works council was taken into account.

Hübler and Jirjahn (2003) use the Hannover Firm Panel to estimate productivity and wage effects of the existence of works councils and coverage by a collective bargaining agreement. They find higher productivity in firms which have a works council and are covered by a collective bargaining agreement and higher wages in firms with a works council and without a collective bargaining agreement.

Addison, Schnabel and Wagner produce numerous papers which report the results of studies on the effects of works councils. A survey on their own, as well as work by others, is presented in Addison et al. (2004b).[3] Addison et al. (2001) find that works councils are associated with lower employee turnover if all establishments are considered, but the effect is insignificant if the subsample of firms with 21–100 employees is used.[4] These somewhat inconclusive results are also found with respect to productivity. In contrast, works councils are associated with lower profitability and higher wages. The authors notice the problem of identifying causality because of the cross-sectional nature of the data set. Addison et al. (2010) find higher wages in establishments with works councils taking worker and firm heterogeneity into account. Pfeifer (2011) estimates

[3] See also Hübler (2003) and Jirjahn (2011) for overviews.

[4] Small firms mostly have no works council whereas almost all large establishments have one. That is why the authors use this subsample of firms with 21 to 100 employees, in which about half of the firms have a works council. Moreover, a works council's rights increase with firm size but are the same for establishments between 21 and 100 employees.

effects of works councils on wages, productivity and profits and finds differences with respect to the type of works council-management-relation. Frick (1996) reports a significant negative effect of works councils on dismissal and quit rates.

Especially older studies often neglect selectivity issues, not because they do not recognize them, but because the data necessary to control for such effects was usually unavailable. One exception is Addison et al. (2004a) who also use a conditional difference-in-differences approach to estimate the effect of introducing a works council on several outcome variables. They report no significant difference in several performance measures and quit rates between matched firms, but also point out that the small number of observations may impede a reliable empirical test. Nevertheless their methodology is innovative in the given context and absolutely appropriate.

Without such a clear-cut before-and-after comparison, it remains unclear whether works councils are introduced because the workforce has a long-term interest in the company, manifesting e.g., in low quit rates before a works council exists, or whether works councils really affect communication by introducing a voice channel. Similarly works councils may be adopted in firms with high productivity as they can influence the distribution of a relatively large "cake" and it is worth the effort and costs of creating this institution. This would explain the low profitability realized by firms with a works council. However, low profits also endanger the existence of the company in general, leading to incentives to introduce a safeguard against dismissals.

Jirjahn (2009) investigates the question as to whether works councils are introduced in times of economic problems or prosperity. Using the Hannover Firm Panel he finds that works councils are more likely to be adopted in establishments suffering from poor economic conditions and also when employment growth realized in the past has been rather low or negative. In addition works councils are more likely to be introduced if the company does not follow an expansive market strategy. Hence, his empirical evidence rejects the rent sharing hypothesis and supports the alternative view that works councils serve as safeguards in hard times.

Mohrenweiser et al. (2012) analyse trigger events which could lead to an introduction of a works council. They identify several relevant factors like firm acquisition, creation of a spin-off, a new owner and also a sector-wide economic downturn, and argue that works councils are introduced to protect workers against uncertainty.

Beckmann et al. (2010) test the rent seeking hypothesis versus the employment protection hypothesis using the IAB Establishment Panel. The two possible explanations are

analysed both theoretically and empirically. For the empirical analysis they estimate the effects of value added and employment growth on the probability to introduce a works council. In contrast to Jirjahn's (2009) and our results presented in this chapter, their results indicate that rent seeking is the major reason for establishing a works council.

2.3 Empirical strategy

We use two different data sources for our empirical analysis. On the one hand, the German Socio-Economic Panel (SOEP) is used, which collects information from thousands of households but provides hardly any information about the companies where the interviewed people work (if they are employed at all). The SOEP includes information concerning the assessment of employment prospects by the employees themselves. Given that it is the employees who ask for the adoption of a works council (or not), their opinion concerning existing working conditions is of major relevance.

On the other hand, we use data from the IAB Establishment Panel, which collects information from thousands of German companies and includes forecasts by top management concerning business conditions and employment growth. Both data sources are complementary to each other and each one has advantages as well as disadvantages. The advantage of the SOEP is that decision to install a works council is determined by the assessment of the employees, not that of the top management. However, the IAB Establishment Panel collects a great deal of other relevant information describing the economic environment of the company in question, such as legal form, collective bargaining coverage, share of qualified, part-time as well as female workers and profitability.

Our approach is to use data on business conditions to explain the introduction of a works council. If weak economic prospects lead to the introduction of works councils, then this institution will be used to protect the workforce in such establishments which might also reflect in an increase in job security. If the contrary is true, works councils can be an instrument to increase bargaining power in order to acquire a larger part of the rent. In a second step the effects of an introduction of a works council are investigated. In a difference-in-differences estimation strategy we implement before-and-after comparisons, which intend to filter out time-invariant firm-specific effects. A necessary assumption for difference-in-differences to be valid is that the unobserved characteristics of observations

of the treatment and control group do not change in their unobserved characteristics over time (see e.g. Blundell and Costa Dias, 2009). Although this assumption cannot be tested, we will refer to it in some robustness tests in the following sections. With data from the IAB Establishment Panel we are also able to check for selectivity by applying a conditional difference-in-differences estimation, which takes into account selection on observable and unobservable factors.

2.4 The IAB Establishment Panel

2.4.1 Data and methods

The German IAB Establishment Panel of the Institute for Employment Research of the German Federal Employment Agency is a representative survey of German establishments employing at least one employee covered by social insurance. It started in 1993 with an annual survey of West German establishments and was extended to East Germany in 1998. Since 2001 more than 15,000 establishments have been observed every year. The IAB Establishment Panel provides detailed information on many labour market topics e.g. on employment and wages.

To construct our sample we identify all establishments without a works council in 2001 and without missing values for the considered variables. Of the remaining 2041 firms 154 had installed a works council by 2006[5]. The effect of the introduction of a works council on wages and employee turnover is investigated using a matching approach, which controls for observable factors that could be responsible for a firm's probability to establish a works council (Heckman et al., 1998). The outcome of a treated group is compared with the counterfactual outcome, which is substituted by an observation with similar observable factors, but without treatment. Y^1 is the outcome of a firm with (WC= 1) and Y^0 is the outcome of a firm without a works council (WC=0). Then the causal effect of the introduction of a works council is defined as[6]

$$E(Y^1-Y^0 \mid WC=1) = E(Y^1 \mid WC=1) - E(Y^0 \mid WC=1) \qquad (2.1)$$

[5] With 154 establishments introducing a works council in the relevant period the number of treaties is quite high in comparison with earlier studies (e.g. Addison et al, 2004a).
[6] Where E(.) denotes the expectations operator.

where $E(Y^0|WC=1)$ is not observable because it describes the outcome of an establishment with a works council in the case it would not have one. Therefore, a comparable counterpart for every treated firm must be found among the group of firms which actually have no works council. If the introduction is considered as a random assignment, the average outcome of firms without a works council $E(Y^0|WC=0)$ could be used as an estimator for $E(Y^0|WC=1)$. For these purposes Rubin (1977) introduced the conditional independence assumption (CIA). It says that the potential outcomes Y^0 and Y^1 are independent of the treatment status WC for firms with the same observable characteristics X:

$$(Y^0, Y^1) \perp WC \mid X. \tag{2.2}$$

We use propensity score matching proposed by Rosenbaum and Rubin (1983) to overcome the high dimensionality problem caused by the large number of exogenous variables to explain the introduction of a works council. The propensity score is a function of the X vector and describes the propensity of introducing a works council. There are several matching estimators which differ in relation to the weights of the observations of the control group. The estimated effect of introducing a works council for a treated firm i is

$$Y_i^1 - \sum_{j \in \{WC=0\}} w_{N_0}(i, j) Y_j^0 \,. \tag{2.3}$$

w_{N_0} (i, j) denotes the weighting function of all N_0 firms without a works council. The weight of a non-treated firm j is higher the more similar it is to the treated one and differs depending on the estimator used. We apply one-to-one nearest neighbour matching, which means that for every treated establishment only the most similar one of the control group is used as a match. This firm gets unity weight and all the other non-treated firms are weighted with zero. We use nearest neighbour matching with replacement.[7] Our matching approach includes the common support restriction, which is one important advantage of this method. It prevents extrapolation of the distribution of the right-hand variables over unobservable regions (Blundell and Costa Dias, 2009).

[7] 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 is a higher variance due to the smaller number of control units used (Dehejia and Wahba, 2002).

Clearly unobservable factors might also affect the probability of the introduction of a works council. To control for such unobservable factors the conditional difference-in-differences approach is appropriate. The difference-in-differences (DID) estimator compares the difference of the changes of outcomes of treated and non-treated firms:

$$E_i(Y^1_{t_1} - Y^1_{t_0}) - E_i(Y^0_{t_1} - Y^0_{t_0}). \quad . \quad (2.4)$$

t_0 denotes the period before and t_1 the period after the introduction of the treatment. Conditional difference-in-differences in addition considers selection on observables by use of the matched samples. [8] Hence, the second term of equation (2.4) is replaced by the change of the matched non-treated firm:

$$\sum_{j \in \{WC=0\}} w_{N_0}(i, j)\left(Y^0_{j,t_1} - Y^0_{j,t_0}\right). \quad (2.5)$$

The first step is the estimation of the propensity score. A dummy variable for the introduction of a works council between 2001 and 2006 serves as dependent variable.[9] Cameron and Trivedi (2005) propose the complementary log-log model when outcomes are rare. In contrast to other binary choice models its distribution is asymmetric around zero. Our sample consists of only 154 treated and 1887 non-treated observations and therefore this approach most likely leads to better results in our case. Based on these results the propensity score for every establishment with and without a works council is calculated.

The IAB Establishment Panel contains a large number of variables[10] which probably affect the decision to establish a works council (Addison et al., 2003). First it is expected that the probability of this kind of worker representation increases with the size of an establishment. Employees of small firms with flat hierarchies may consider a works council as unnecessary or even a hindrance. By contrast, in larger establishments

[8] Heckman et al. (1998) mention three different sources of possible selection bias, the neglect of common support, observed and unobserved heterogeneity. These factors are all taken into account by using conditional difference-in-differences.

[9] Our exogenous variables are from 2001, but the introduction necessarily takes place in a later period and hence the endogenous and most exogenous variables are not from the same year. However, note that one of the most important variables, namely that concerning expectations on employment covers a time period of five years. We also experimented with other time periods for the estimation of the propensity score and the effects of works councils. The use of shorter intervals of e.g. only three years, where the time period relevant for the explanatory variables is closer to the date when the works councils are introduced, leads to similar results for the determinants, but the number of treaties is lower.

[10] The mean values of the variables used can be found in Table 2.10 in the Appendix.

communication between the workforce and management is much easier if a representative body exists. Moreover, in larger firms workers tend to put more emphasis on unionization. Unionization and the existence of a works council are highly correlated. Furthermore, the composition of the workforce plays an important role. Workers with a long-term interest in the firm are expected to show more interest in adopting a works council and therefore we include the share of female employees and part-time employees in our estimations (which are expected to have lower average tenure). Blue-collar workers traditionally show more interest in worker representation institutions than white-collar workers and this suggests comparing the share of blue-collar workers in relation to total employment. Addison et al. (1997) suggest other forms of (financial) employee participation as alternatives to a works council. Hence, we add dummy variables for the existence of a profit sharing scheme or employee share ownership to our estimation. Addison et al. (1997) also mention the influence of headquarters on their branches concerning the decision on whether or not to introduce a works council. Moreover – as stated earlier – unions are expected to support the formation of a works council.

While the IAB Establishment Panel contains no information on the rate of unionization, it does tell us whether the firm is covered by a collective bargaining agreement. Hence, a dummy variable indicating collective bargaining coverage is included as well. In addition a dummy variable is used which indicates that establishments are located in Eastern Germany.

Other variables are the legal form (dummy for limited liability) and industry dummies representing specific economic conditions. As workers may either try to share or protect rents by establishing a works council, we include variables which express the profit situation of an establishment during the last year and expectations of the management with respect to the development of sales volume in the current year as well as expectations with respect to employment development both in the following year and in the next five years.[11] All these variables are measured on different Likert scales. However, we transform them into dummy variables, which equal one if the profit situation is good or very good, and if sales and employment are expected to increase, respectively.

[11] The introduction of a works council depends on the initiative of the employees. As mentioned before, we only have information on part of the management, which need not necessarily correspond to the expectations of the employees. However, if a firm is in crisis, this will probably be realized by all members of the firm.

2.4.2 Estimation results

Table 2.1 shows the estimation results of the complementary log-log regression on the impact of the suggested determinants of works councils. Most of the coefficients have the expected sign. The probability of adopting a works council increases with establishment size.

Table 2.1: Results of complementary log-log model - determinants of the introduction of works councils

Variable	Coefficient [Marginal effect]
Ln(number of employees)	0.744*** [0.043***]
Share of blue-collar workers	-1.096*** [-0.063***]
Share of part-time employees	-0.077 [-0.004]
Share of female employees	-0.407 [-0.023]
Branch plant (yes=1)	1.416*** [0.081***]
East German establishment (yes=1)	-0.335* [-0.019*]
Limited liability (GmbH, AG =1)	1.071*** [0.061***]
Collective bargaining (yes=1)	0.855*** [0.049***]
Profit sharing scheme (yes=1)	-0.341 [-0.020]
Employee share ownership (yes=1)	0.707* [0.040*]
Profit situation (t-1, good/very good=1)	-0.385** [-0.022**]
Expected sales current year (increasing=1)	0.156 [0.009]
Expected employment next year (increasing=1)	0.276 [0.016]
Expected employment in five years (1=sharply/somewhat higher)	0.024 [0.001]
Number of observations	2041

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Industry dummies included. Average marginal effects of the covariates in square brackets.
Source: IAB Establishment Panel, waves 2001, 2006, own calculations (controlled remote data access via FDZ).

Surprisingly the share of blue collar workers exerts a negative impact on the probability that a works council is introduced, whereas the coefficients of the share of female and part-time employees are not significant. Employees in branch plants with limited liability and collective bargaining as well as in West German firms introduce a works council more often. If establishments offer their employees a share ownership plan, the probability of adopting a works council is 4.0 percentage points higher; profit sharing in contrast has no influence. Firms reporting a good or very good profit situation in the last year have a 2.2 percentage points lower likelihood to establish a works council whereas the forecasts concerning sales and employment have no significant impact. This can be partial support for the hypothesis that works councils are rather introduced during bad times. Employees may expect a works council to help them to protect their future rents.[12]

To investigate the effects of adopting a works council on wages and turnover, we compare the average wages per employee and the share of dismissals and quits on the total number of employees of matched treaties and controls in 2006. With regard to the quality of matching we compare the difference between the means of the exogenous variables of the treated and untreated establishments before and after matching. After matching they no longer differ significantly. Furthermore, we compute the mean standardized bias among the exogenous variables. Matching leads to a bias reduction of 81% (see Table 2.11 in the Appendix).

The results of the effects on wages as well as on dismissals and quits are presented in Table 2.2.[13] There are highly significant differences between establishments with and without works councils concerning wages. At first sight works councils seem to increase wages. We also find a weakly significant lower rate of quits and dismissals in firms where a works council was introduced. These results would be in line with results reported by former studies (see Section 2.2.2).

In the next step we compare matching with the difference-in-differences estimator. When unobservable heterogeneity of the establishments is additionally taken into account, the positive effects on wages and negative effects on turnover disappear (see Table 2.2). If conditional difference-in-differences estimations are employed, treated and matched untreated establishments show similar wage growth and similar changes in dismissal and quit rates.

[12] This evidence is in accordance with the results of Jirjahn (2009).

[13] In order to check for the robustness of our results, in addition to nearest neighbour matching we also apply radius and kernel matching as well as local linear regression matching. All methods lead to similar results.

Table 2.2: Matching and conditional difference-in-differences - The effects of the existence and introduction of works councils on wages and turnover

	Introduction of works council	No works council	difference	t-value
Wages				
Matching	7.72	7.54	0.19	2.95***
Difference-in-Differences	0.11	0.13	-0.02	-0.41
Dismissals and Quits				
Matching	0.028	0.068	-0.041	-1.85*
Difference-in-Differences	-0.012	-0.007	-0.005	-0.34

***/**/* indicates statistical significance at the 1%, 5% and 10% level.
Source: IAB Establishment Panel, waves 2001, 2006, own calculations (controlled remote data access via FDZ).

Firms which introduced a works council during the period from 2001 to 2006 already experienced higher wages and lower turnover rates in 2001 and we do not find any effect of the introduction of a works council on wages and turnover. Works councils are introduced in establishments which pay above-average wages, but they in turn do not affect remuneration.

Jirjahn et al. (2011) show that learning plays an important role for the effectiveness of works councils. This can also be crucial for our analysis. If a large share of works councils are introduced only shortly before the date of the interview in 2006, we could underestimate introduction effects. Therefore, as a robustness check, we exclude firms where a works council was established after the year 2005 and only look at the effects of works councils in 2006 which were established at least one year ago. The results are very similar to those reported above.[14]

As mentioned above, the validity of difference-in-differences depends on the common trends assumption, which cannot be tested. However, we can at least compare pre-treatment trends in the outcome variables for those establishments we also observe before 2001. We use data from up to four years prior to 2001 and plot wage trends and turnover trends separately for treatment and control group observations (see Figures 2.1 and 2.2 in the Appendix). Although we find a peak in turnover rates in 1999 only for the control group, trends do not seem to be systematically different between both groups. Especially trends in wages are very similar and we also graphically find higher wages and lower turnover in treated firms before a works council is established.[15]

[14] The results are available from the authors upon request.
[15] In addition, we carry out several t-tests and find that both groups of firms neither significantly differ in wage growth nor in changes in turnover rates.

The negative effect of a good profit situation on the probability of introducing a works council and the missing wage effect point to the conclusion that rent-seeking does not appear to be the intent of the workforce when asking for the setting up of a works council.[16] Furthermore, it seems that especially those firms with stable employment relations (low number of dismissals and quits) install works councils, and not the other way round, as many suppose and claim. This result makes sense, as the formation of a worker representation institution is time consuming and only rewarding if workers have some long-term interest in the establishment in question.

2.5 The German Socio-Economic Panel

2.5.1 Data and methods

Having examined the effect of works councils on wages and turnover using data from the IAB Establishment Panel, in the next step the German Socio-Economic Panel (SOEP) is applied for additional estimations. The SOEP is a representative longitudinal survey of German households, which has been carried out annually since 1984. It includes numerous different topics and provides among other things information on household composition, housing conditions, health, employment, education, income and other living conditions (for further information on the data see Wagner et al., 2007).

Only for the years 2001 and 2006 the SOEP contains information on the existence of a works council in the company where the interviewed person worked. Our samples consist of full-time employees working in the same establishment throughout the relevant period.[17] Furthermore respondents provide information on topics they are concerned about, e.g. with respect to job security. Options for answers are: “very concerned”, “somewhat concerned” and “not concerned at all”. These three alternatives serve as categories for the endogenous variable “job security” in the first two estimations. Our strategy is a difference-in-differences estimation. We control for permanent unobserved differences between workers in firms without works councils and workers in firms which install a works council and analyse whether the introduction of a works council has an

[16] One might ask why firms pay high wages in times of economic crisis. It is likely that wages had been increased in better days, and now they are downwardly rigid.

[17] The data used in this study was extracted using the Add-On PanelWhiz for Stata®. PanelWhiz (http://www.PanelWhiz.eu) was written by Dr. John P. Haisken-DeNew (john@PanelWhiz.eu). See Haisken-DeNew and Hahn (2010) for details. The PanelWhiz generated do-file to retrieve the data used here is available from us upon request. Any data or computational errors in this study are our own.

impact on their assessment concerning job security. Finally, the effects of works councils on wages and overtime are estimated.

Several alternatives are tested. On the one hand, only employees are included who worked in an establishment without a works council in 2001. Using the waves of the years 2001 and 2006 this leads to a sample with 1618 observations of which 164 people report to be represented by a works council in 2006, but not in 2001. On the other hand, in an enlarged second sample, people are included who work in an establishment where a works council was introduced between 2001 and 2006, or are employed in an establishment which either has a works council in both periods or has no such institution in both periods. Excluded are just those observations where a works council has been abolished. This leads to a rise in the number of observations to 6144 cases. We account for the ordinal nature of the endogenous variable *Job security* using an ordered logistic regression for the first two estimations. A dummy variable for people is included if they report working in a company which did not have a works council in 2001, but has one in 2006 (variable *Treatment group dummy* in Tables 2.3-2.9). This variable has unit value in both years (2001 and 2006). In addition a time dummy, indicating observations from the year 2006, is created and this time dummy is interacted with the treatment group dummy. Hence, the interaction variable (variable *Introduction of works council = Treatment group dummy * year=2006* in Tables 2.3-2.9) characterizes the change of the dependent variable, assessment of job security, from 2001 to 2006 for people in those firms which have introduced a works council. Ai and Norton (2003) point out that the standard method to compute marginal effects based on the coefficients derived from nonlinear models is inappropriate in the case of interaction variables. However, Puhani (2012) argues that, although their remark on the derivative in non-linear models is correct, it does not represent the treatment effect in difference-in-differences models. Nevertheless, we additionally compute marginal effects in the way proposed by Ai and Norton (2003) which hardly differ from the marginal effects without considering the interaction.[18]

For the estimations whose results are reported in Tables 2.5 to 2.9, we additionally add a dummy for works council's existence which equals one both in 2001 and 2006 if a person worked in an establishment with this institution in both years. Some other control variables are also included in the estimation of job security.[19] We use age and age

[18] See Appendix 2.7.1 for the computation.

[19] The mean values of the variables used in the different estimations are reported in Table 2.12 in the Appendix 2.7.2.

squared, whereas tenure is measured using five-year-periods up to a reported tenure of 20 years. Moreover, better educated people are expected to find a new job more easily and to be dismissed less often. Taking account of qualification is realized by two dummy variables, one with unit value if an apprenticeship has been completed and another one with unit value if a person has gained a university degree. Furthermore we use dummy variables for the occupation of an employee and the industry as well as a dummy for East German firms. In addition firm size is considered. We use different size groups where establishments with 5 to 19 employees serve as control group.[20]

In order to determine the effect of works councils on wages we use the log of hourly wages in 2001 and 2006 as the dependent variable. Tenure, age, establishment size, education, as well as industry and occupation dummies, a time dummy, a dummy for East German firms and a dummy for female employees are used as control variables. Finally, our most important variables, namely dummies for existence and introduction of a works council, are included.

Finally, we analyse whether works councils use their explicit rights in order to reduce the number of overtime hours. A reduction of overtime hours is presumably requested even if the employees explicitly express their concerns with respect to job security. Exogenous variables are regular working time, age, tenure, gender, education, industry and occupation dummies, establishment size, a time trend and dummies for married women (without children) and mothers with at least one child younger than 16. We use a tobit model to estimate the effect of works councils on weekly overtime hours. Once employees with missing values in the additionally required variables are excluded, a total of 5047 observations are left for the estimations of hourly wages and overtime hours.

2.5.2 Estimation results

Table 2.3 shows the estimation results of workers' concerns with respect to job security for the first sample. In addition to the coefficients we present marginal effects for the interesting variables in Table 2.4. In general, concerns about job loss increase between 2001 and 2006. The negative coefficient of the *Treatment group dummy* indicates that employees in establishments where a works council is adopted are more afraid of losing their job. Table 2.4 shows that, before a works council is introduced, employees in such

[20] Besides the fact that works councils can only be elected in firms with at least 5 employees, dropping very small firms also has the effect that only people with protection against dismissal are included in our sample (which can be relevant for the estimations of job security effects).

establishments are 5.8 percentage points and 4.7 percentage points more likely to be very or somewhat concerned, respectively and have a lower probability to be not concerned about job loss at all of 10.5 percentage points.

Table 2.3: Ordered logistic regression results - The effect of the introduction of works councils on concerns with respect to job security

Variable	Coefficient
Tenure 5-10 years	0.224
Tenure 10-15 years	0.366**
Tenure 15-20 years	0.405*
Tenure > 20 years	0.498**
Age	-0.179***
Age^2	0.002***
Establishment size 20-99 employees	-0.008
Establishment size 100-199 employees	0.171
Establishment size 200-1999 employees	0.375**
Establishment size > 2000 employees	0.598***
East German establishment	-0.935***
University degree	0.679***
Apprenticeship	0.404*
Year=2006	-0.786***
Treatment group dummy	-0.502***
Introduction of works council	0.590**
Number of observations	1618
Pseudo R^2	0.09

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Industry and occupation dummies included, reference group for firm size: 5-19 employees; reference group for tenure groups: less than 5 years.
Source: SOEP waves 2001, 2006, own calculations.

Table 2.4: Average marginal effects on job security (introduction)

Variable	(1) Very concerned	(2) Somewhat concerned	(3) Not concerned at all
Treatment group dummy	0.058***	0.047***	-0.105***
Introduction of works council	-0.068**	-0.056**	0.123**

***/**/* indicates statistical significance at the 1%, 5% and 10% level.
Source: SOEP waves 2001, 2006, own calculations.

People seem to demand employee representation especially if they work in firms with uncertain employment perspectives. Hence, the question can be raised, whether a works council really has an effect on job security. The coefficient of the interaction variable *Introduction of works council* is positive and significant at the 5% level (see Table 2.3). When a works council is adopted between 2001 and 2006, the concerns about a possible job loss decrease during this period and the higher probability to worry about job security is cancelled out. The formation of a works council reduces the likelihood to be very or somewhat concerned by 6.8 percentage points and 5.6 percentage points, respectively, whereas the probability to have no worries about job security at all increases by 12.3 percentage points (see Table 2.4).[21] These results are in line with our first estimations based on the IAB Establishment Panel. Works councils are in particular established in firms in a bad economic situation and where employees more often fear a job loss. Thus, the introduction of a works council seems to help to protect rents but not to increase them. If a reduction in job security triggers the introduction of a works council, endogeneity would cause problems for our estimations. However, Figure 2.3 in the Appendix shows that there is not a sudden decrease in job security shortly before the introduction period, but employees of the treatment group report a somewhat lower job security throughout the observed pre-treatment period from 1996 to 2001.

With respect to the control variables, the results in Table 2.3 show that employees who have been working in the same firm for more than 10 years worry less about losing their job than people with less than five years tenure. Concerns are U-shaped in age. Obviously, older workers face a higher unemployment risk if they lose their current job. Completed vocational training as well as holding a university degree has a positive impact on the assessment of job security. Probably because of the higher unemployment rate in East Germany, employees working there are more often afraid of losing their job. People working in a firm with more than 200 employees worry less about job security than those working in very small firms with less than 20 employees.

The coefficients and marginal effects of the estimation including establishments with a works council throughout the whole period are presented in Tables 2.5 and 2.6.

[21] Although there is a lot of discussion about the objection of Ai and Norton (2003), we additionally estimate marginal effects of interaction terms according to the formula described in Appendix 2.7.1. Regarding e.g. the first category (very concerned about losing one's job) the marginal effect is negative (- 0.058) and also significant at the 5% level. In our case this marginal effect does not differ very much from the "usually calculated" average marginal effect of -0.068 and is even more similar to the marginal effect on the average of - 0.053.

Table 2.5: Ordered logistic regression results - The Effect of the introduction and existence of works councils on concerns with respect to job security

Variable	Coefficient
Tenure 5-10 years	0.001
Tenure 10-15 years	0.027
Tenure 15-20 years	0.002
Tenure > 20 years	0.069
Age	-0.209***
Age^2	-0.003***
Establishment size 20-99 employees	0.136
Establishment size 100-199 employees	0.157
Establishment size 200-1999 employees	0.182
Establishment size > 2000 employees	0.213*
East German establishment	-0.927***
University degree	0.391***
Apprenticeship	0.335***
Year=2006	-0.644***
Existence of works council in 2001 and 2006	-0.109
Treatment group dummy	-0.419**
Introduction of works council	0.520**
Number of observations	6144
Pseudo R^2	0.09

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Industry and occupation dummies included, reference group for firm size: 5-19 employees; reference group for tenure groups: less than 5 years.
Source: SOEP waves 2001, 2006, own calculations.

Table 2.6: Average marginal effects on job security (existence and introduction)

Variable	(1) Very concerned	(2) Somewhat concerned	(3) Not concerned at all
Existence of works council in 2001 and 2006	0.011	0.012	-0.023
Treatment group dummy	0.043**	0.046**	-0.089**
Introduction of works council	-0.053**	-0.057**	0.111**

***/**/* indicates statistical significance at the 1%, 5% and 10% level.
Source: SOEP waves 2001, 2006, own calculations.

The results are very similar to those obtained above. However, in this specification tenure has no longer a significant coefficient. Regarding establishment size only one of the dummy variables is significant. Employees working in an establishment where they are represented by a works council in both years are not significantly more or less worried about losing their job compared to workers in firms without a works council. This result goes in line with the effects for employees working in establishments where a works council is introduced, as the lower job security before its adoption is cancelled out by the introduction.

When a works council is introduced in firms with poor economic performance and where workers are afraid of losing their job, its objective should not be directed at increasing wages. We investigate the impact of works councils on hourly wages with the SOEP data next. The results are reported in Table 2.7. Most of the considered variables are significant and have the expected effects. The estimated coefficients for age indicate an inverted U-shaped relationship. Furthermore tenure, a university degree and a completed vocational training have positive effects on wages. Earnings are significantly lower for women and employees in East German establishments. With regard to establishment size, firms with more than 200 employees pay significantly higher wages than small firms with less than 20 workers. Hence, in accordance with existing empirical evidence we also find a size premium for wages. Employees working in establishments which had a works council in 2001 as well as in 2006, and also people working in firms, which introduced a works council, earn higher hourly wages. But the introduction of a works council itself does not lead to higher wages. To the contrary, the coefficient of the interaction term, which indicates the additional effect of an introduction, is even negative (although not significant). Companies which introduce a works council already paid more than 7% higher wages before the adoption has taken place. The introduction of a works council does not lead to an additional wage increase. [22]

[22] Since information on the existence of works councils is only available for 2001 and 2006 the variable "Introduction of a works council" and "Treatment group dummy" are highly correlated (r= 0.73). In view of the resulting high standard error of the interaction term, we additionally run separate estimations for the samples 2001 and 2006. Applying a Wald test on significant differences between the coefficients which takes account of the two variance-covariance matrices confirms our result from the difference-in-differences approach. The two coefficients (based on the two samples) of the treatment group dummy are not significantly different from each other.

Table 2.7: OLS results -The effect of the introduction and existence of works councils on ln(wages)

Variable	Coefficient
Tenure 5-10 years	0.028**
Tenure 10-15 years	0.062***
Tenure 15-20 years	0.080***
Tenure > 20 years	0.104***
Age	0.029***
Age^2*10^{-2}	-0.030***
Female	-0.200***
Establishment size 20-99 employees	0.014
Establishment size 100-199 employees	0.021
Establishment size 200-1999 employees	0.051***
Establishment size > 2000 employees	0.096***
East German establishment	-0.236***
University degree	0.173***
Apprenticeship	0.050***
Year=2006	0.088***
Existence of works council in 2001 and 2006	0.119***
Treatment group dummy	0.073***
Introduction of works council	-0.012
Number of observations	5047
Adjusted R^2	0.54

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Industry and occupation dummies included, reference group for firm size: 5-19 employees; reference group for tenure groups: less than 5 years.
Source: SOEP waves 2001, 2006, own calculations.

Finally, we investigate the influence of works councils on overtime hours. Our estimation results for the effect of a works council's existence and formation on overtime hours are presented in Table 2.8. Marginal effects for the variables of interest are reported in Table 2.9. In addition to a homoscedastic tobit model we estimate a heteroscedastic tobit model. If heteroscedasticity exists, the coefficients would be inconsistent in the first case.

Table 2.8: Tobit results - The effect of the introduction and existence of works councils on overtime hours

	Homoscedastic Tobit	Heteroscedastic Tobit
Regular working hours	-0.038	-0.052**
Tenure 5-10 years	-0.133	-0.011
Tenure 10-15 years	-0.090	-0.137
Tenure 15-20 years	-0.152	-0.267
Tenure > 20 years	-0.361	-0.337
Age	0.252***	0.280***
Age^2	-0.003***	-0.003***
Female (not married, no children)	-0.759***	-0.508***
Wife (no children under 16)	-1.481***	-1.383***
Mother (with children under 16)	-2.287***	-1.868***
Establishment size 20-99 employees	0.602**	0.559**
Establishment size 100-199 employees	0.671**	0.516*
Establishment size 200-1999 employees	0.574*	0.550*
Establishment size > 2000 employees	0.509	0.438
East German establishment	0.403**	0.216
University degree	2.052***	2.510***
Apprenticeship	0.852***	1.404***
Year=2006	-1.356***	-1.218 ***
Existence of works council in 2001 and 2006	-1.046***	-0.854***
Treatment group dummy	-1.327***	-1.173**
Introduction of works council	0.197	-0.005
Number of observations	5047	

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Industry and occupation dummies included, reference group for firm size: 5-19 employees; reference group for tenure groups: less than 5 years.
Source: SOEP waves 2001, 2006, own calculations.

Table 2.9: Average marginal effects on overtime hours (existence and introduction)

Variable	Homoscedastic Tobit	Heteroscedastic Tobit
Existence of works council in 2001 and 2006	-0.671***	-0.557***
Treatment group dummy	-0.851***	-0.765**
Introduction of works council	0.126	-0.003

***/**/* indicates statistical significance at the 1%, 5% and 10% level.
Source: SOEP waves 2001, 2006, own calculations.

Except for a few variables both estimations produce similar results. Most of the considered determinants of overtime are significant. In the homoscedastic tobit model regular working hours have no impact on overtime, but employees in East German establishments report working more overtime. If heteroscedasticity is taken into account, the significance of the variables (but not the signs of the coefficients) is reversed. Regular working hours have a negative impact on overtime, whereas the coefficient of the dummy for firms in East Germany is insignificant. Tenure has no significant effect in either model but the coefficients for age point to an inverted U-shaped structure. Female employees work less overtime than male workers, especially if they are married or have younger children. Overtime hours increase with education level. For all considered size classes with at least 20 employees and 2000 at most, people work more overtime than in very small establishments with less than 20 employees. Generally, overtime hours decreased during the considered period. In contrast to wage determination, works councils enjoy explicit co-determination rights to limit overtime work. Both in establishments where a works council exists as well as where it is adopted, overtime hours are significantly lower, but the introduction is not responsible.[23] The marginal effects of the variable *Introduction of works council* are quite small and insignificant in both estimations. Hours have already been lower before a works council has been installed. Employees in firms where a works council is introduced already work more than 45 minutes less overtime a week.

As mentioned in Section 2.4.2, we could underestimate the impact of works councils if they were established only shortly before the interview date in 2006. As the SOEP only contains information on works councils for the years 2001 and 2006, we cannot identify employees working in firms with recently established works councils in the post-treatment year 2006. However, it is very unlikely that a disproportionately large share of introductions took place at the end of the observation period. The opposite seems to be true: many works councils were established during the first years after significant modifications of the Works Constitution Act in September 2001, which is at the beginning of our observation period.[24]

As a robustness test, just like in Section 2.4.2, we compare pre-treatment trends in the outcome variables for those people we also observe before 2001. As panel attrition is less

[23] A Wald test on equality of the coefficients of the treatment group dummy (based on separate estimations for 2001 and 2006) confirms this result. There is no significant difference between the two coefficients.

[24] See Mohrenweiser et al. (2010) for the number of newly established works councils between 2000 and 2007.

severe in the SOEP data, we even extend the pre-treatment observation period back to 1997. The separate trends in job security, wages and overtime for treatment and control group observations can be found in Figures 2.3 to 2.5 in the Appendix. Average job security follows a similar trend for both groups, which is also true for wages and overtime hours. Moreover, t-tests between treatment and control group show that both groups of employees are not significantly different with respect to wage growth, yearly changes in job security or growth of overtime hours in the pre-treatment period.

2.6 Conclusion

We investigate the determinants and effects of forming a works council in German firms using two data sets. At first, data from the IAB Establishment Panel and, second, information on workers derived from the German Socio-Economic Panel is applied. The expectations of the top management with respect to business conditions and employment change have no impact on the likelihood to introduce a works council. Concerns of workers with respect to job security are more severe in firms where a works council is established. These concerns are reduced if such a newly founded works council is in effect.

Works councils do not affect wages and labour turnover. These results are also in line with the Works Constitution Act, where negotiations on wages are not allowed. But overtime working also remains unaltered, although in this respect co-determination rights exist, which grant works councils quite explicit veto rights.

According to our results, works councils are not adopted in order to increase bargaining power and to acquire a larger part of the rents. Due to the fact that turnover is already low before works councils are installed, we conclude that a workforce with low quit rates and a long-term interest in a firm's existence and performance asks for a works council.

It seems that both the negative effects expected (by the critics) as well as the positive effects of works councils expected (by the supporters) are overrated. The estimated impact is largely due to the use of cross-sectional data, neglecting selectivity as well as group-specific effects.

Although the number of observations used for our study is not small, it would be useful to have access to information on many more firms in which works councils are adopted. This would render the test much more powerful. It also turns out that some firms abolish

their works councils. Hence there is an additional research question: What are the reasons for and the effects of abolishing a works council?

2.7 Appendix

2.7.1 Computation of marginal effects of interaction terms

Ai and Norton (2003) present formulas for the correct computation of interaction effects in probit and logit models. In such a situation the marginal effect is the cross-derivative of the expected value if continuous variables are used and discrete differences if the exogenous variables are dichotomous. Denoting the dependent variable by y and the two variables x_1 and x_2 that form the interaction variable by x_1x_2, the conditional mean of the endogenous variable y for the logit model is

$$E[y \mid x_1, x_2, X] = \frac{1}{1+e^{-(\beta_1 x_1+\beta_2 x_2+\beta_{12} x_{12}+X\beta)}} = F(u) \qquad (2.6)$$

with a vector of additional exogenous variables X. When both x_1 and x_2 are dummy variables the correct interaction effect of the variable x_1x_2 is now:

$$\frac{\Delta^2 F(u)}{\Delta x_1 \Delta x_2} = \frac{1}{1+e^{-(\beta_1+\beta_2+\beta_{12} x_1 x_2+X\beta)}} - \frac{1}{1+e^{-(\beta_1+X\beta)}} - \frac{1}{1+e^{-(\beta_2+X\beta)}} + \frac{1}{1+e^{-X\beta}}. \qquad (2.7)$$

The standard marginal effect ignores the second term on the right hand side of equation (2.7). We now apply this method to the ordered logit model. We extend equation (2.7) by including the threshold value α_i. Please note, that estimating an ordered logistic regression in Stata can differ from the procedure of other programs. Instead of setting the first threshold value α_1 to zero, Stata sets the constant to zero and estimates the threshold values to separate the different categories. Therefore α_1 is included in equation (2.8). In case of the first category (very concerned with respect to job security) the interaction effect is

$$\frac{\Delta^2 F(u)}{\Delta x_1 \Delta x_2} = \frac{1}{1+e^{-(\alpha_1-(\beta_1+\beta_2+\beta_{12} x_1 x_2+X\beta))}} - \frac{1}{1+e^{-(\alpha_1-(\beta_1+X\beta))}} - \frac{1}{1+e^{-(\alpha_1-(\beta_2+X\beta))}} + \frac{1}{1+e^{-(\alpha_1-X\beta)}} \qquad (2.8)$$

.

2.7.2 Tables and Figures

Table 2.10: Mean values – IAB Establishment Panel sample

Variable	Mean
Number of employees	23.26
Share of blue-collar workers	0.498
Share of part-time employees	0.169
Share of female employees	0.408
Branch plant (yes=1)	0.102
East German establishment (yes=1)	0.479
Limited liability (GmbH, AG =1)	0.539
Collective bargaining (yes=1)	0.458
Profit sharing scheme (yes=1)	0.117
Employee share ownership (yes=1)	0.029
Profit situation (t-1, good/very good=1)	0.355
Expected sales current year (increasing=1)	0.277
Expected employment next year (increasing=1)	0.165
Expected employment in five years (1=sharply/somewhat higher)	0.181
Introduction of a works council (yes=1)	0.076
Number of observations	2041

Source: IAB Establishment Panel, waves 2001, 2006, own calculations (controlled remote data access via FDZ).

Table 2.11: Matching quality: Difference of mean values/Mean standardized bias

Variable	Before Matching	After Matching
	Difference of mean values	
Ln(number of employees)	1.50***	0.03
Share of blue-collar workers	-0.09***	-0.04
Share of part-time employees	0.02	0.03
Share of female employees	0.06**	-0.00
Branch plant (yes=1)	0.31***	-0.06
East German establishment (yes=1)	-0.12***	-0.03
Limited liability (GmbH, AG =1)	0.21***	0.05
Collective bargaining (yes=1)	0.21***	-0.07
Profit sharing scheme (yes=1)	0.05*	0.01
Employee share ownership (yes=1)	0.04**	0.03
Profit situation (t-1, good/very good=1)	-0.03	-0.02
Expected sales current year (increasing=1)	0.14***	-0.04
Expected employment next year (increasing=1)	0.08**	-0.04
Expected employment in five years (1=sharply/somewhat higher)	0.07**	-0.01
Mean standardized error	26,57	4,99
Bias Reduction		0.81

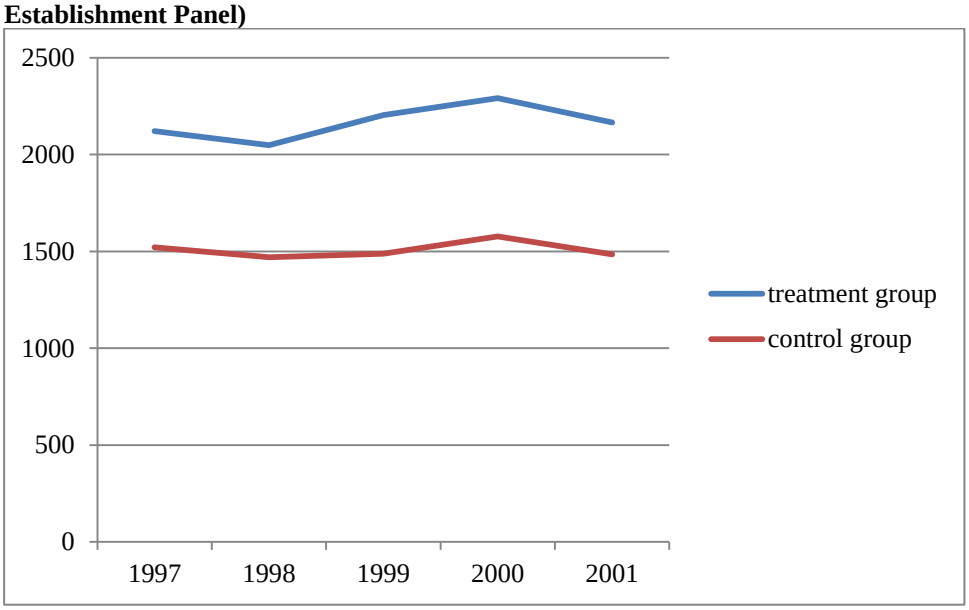
***/**/* indicates statistical significance at the 1%, 5% and 10% level. Source: IAB Establishment Panel, waves 2001, 2006, own calculations (controlled remote data access via FDZ).

Table 2.12: Mean values – SOEP sample

Variable	Mean
Tenure < 5 years	0.132
Tenure 5-10 years	0.259
Tenure 10-15 years	0.203
Tenure 15-20 years	0.143
Tenure > 20 years	0.263
Age	43.054
Female	0.316
East German establishment	0.244
Establishment size 5-20 employees	0.128
Establishment size 20-99 employees	0.193
Establishment size 100-199 employees	0.105
Establishment size 200-1999 employees	0.281
Establishment size≥ 2000 employees	0.293
University degree	0.244
Apprenticeship	0.683
Children under 16 in household	0.386
Married	0.692
Gross hourly wage (€)	15.844
Overtime hours	2.943
Regular working hours	38.430
Job security (1 = very concerned, ..,3=not concerned at all)	2.322
Existence of a works council	0.739
Number of observations	6144

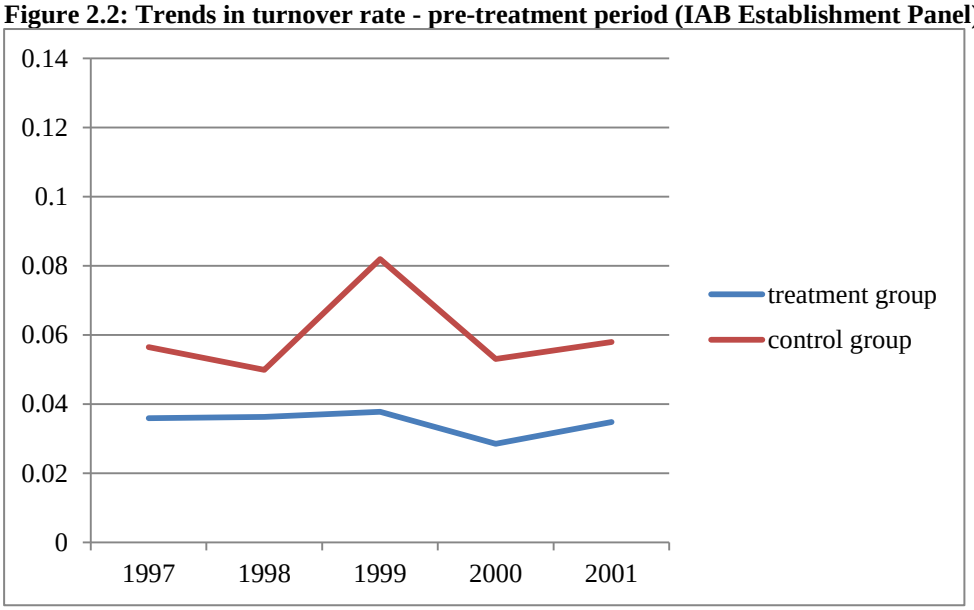
Source: SOEP waves 2001 and 2006, own calculations. Note: Number of observations differs for hourly wage, regular working hours and overtime hours (N=5047).

Figure 2.1: Trends in gross monthly wages (€) – pre-treatment period (IAB Establishment Panel)



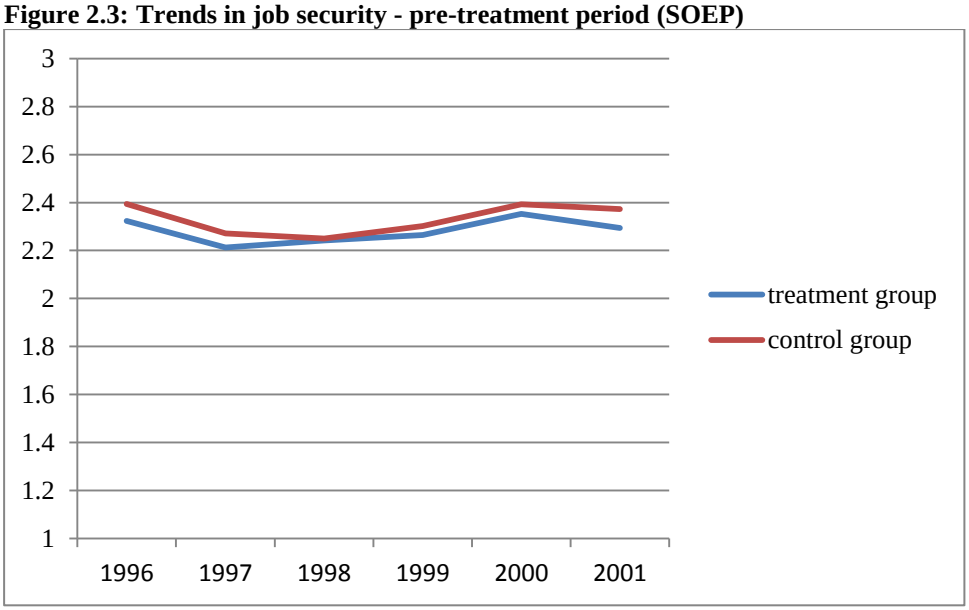
Source: IAB Establishment Panel, waves 1997-2001, own calculations (controlled remote data access via FDZ).

Figure 2.2: Trends in turnover rate - pre-treatment period (IAB Establishment Panel)



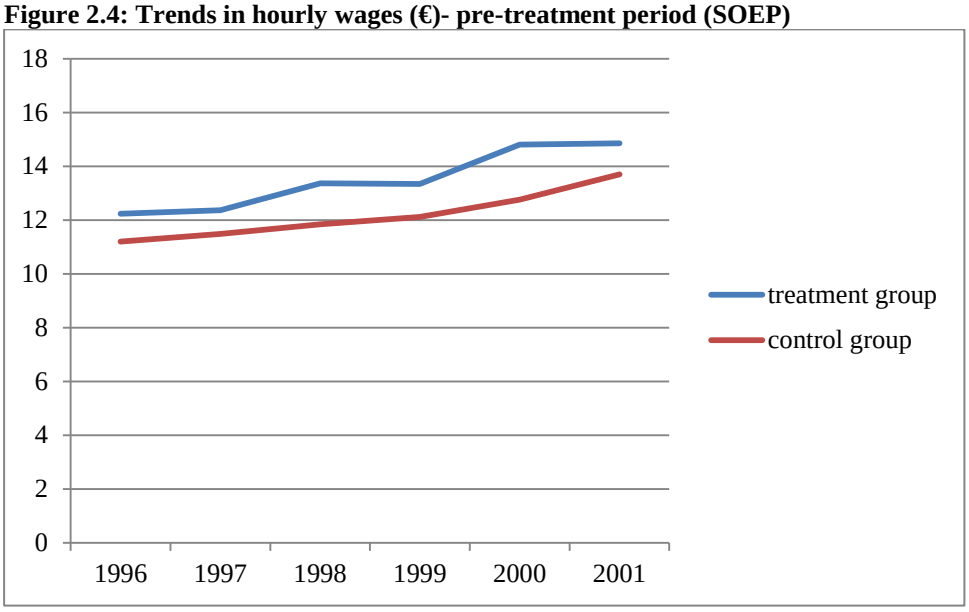
Source: IAB Establishment Panel, waves 1997-2001, own calculations (controlled remote data access via FDZ).

Figure 2.3: Trends in job security - pre-treatment period (SOEP)



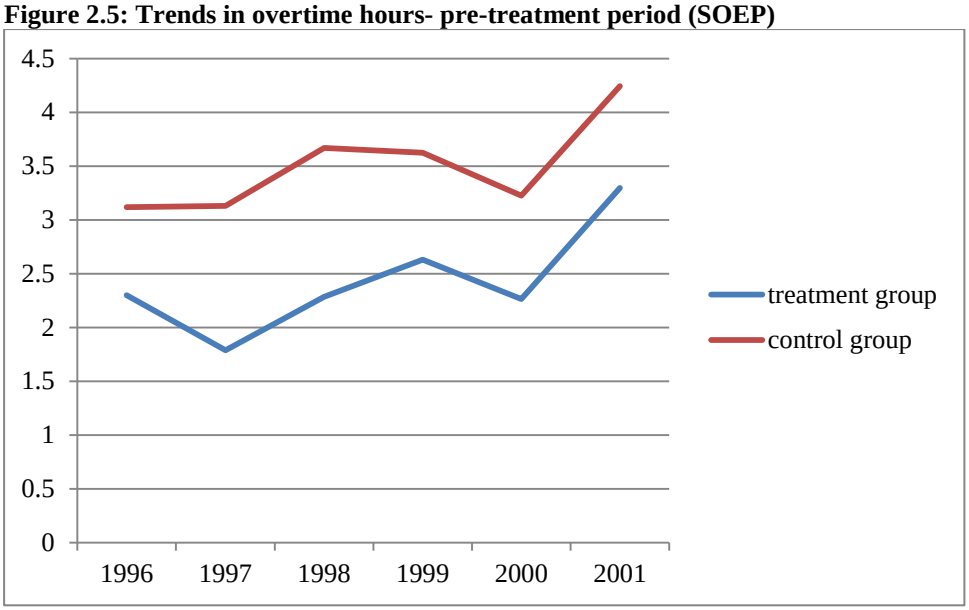
Source: SOEP waves 1996-2001, own calculations.

Figure 2.4: Trends in hourly wages (€)- pre-treatment period (SOEP)



Source: SOEP waves 1996-2001, own calculations.

Figure 2.5: Trends in overtime hours- pre-treatment period (SOEP)



Source: SOEP waves 1996-2001, own calculations.

3 Just a question of selection? - The causal effect of profit sharing on a firm's performance

This chapter is based on an unpublished manuscript of the same title, co-authored by Kornelius Kraft.

3.1 Introduction

Economists have analysed the incentive effects of remuneration systems such as fixed wages, piece rates, or bonus payments for many years. Of main interest in this discussion has been the impact of variable, output-dependent wage components on a 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 practical, e.g. when work is flexibly organized, or individual output is difficult to measure (e.g. Holmström, 1982, Prendergast, 1996).

The main purpose of profit sharing is to improve productivity by increasing the employees' incentives to enhance efforts because they directly benefit from a higher profit. However, opposing effects also exist, e.g. the incentive to free-ride which could cancel out the positive incentive effect of profit sharing. Therefore, in theory, it is a priori not clear whether profit sharing really affects the firm's performance.

As a consequence, numerous empirical studies have investigated productivity effects of profit sharing in the last 25 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 (…)*". Some more recent studies, however, have found no significant effect.[2]

Surprisingly, in spite of the considerable evidence in favour 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 the majority of firms probably do not meet.

[1] Studies analysing productivity effects of profit sharing are e.g. FitzRoy and Kraft (1987a), Möller (2000), for Germany; Wadhwani and Wall (1990), Kruse (1992), Kumbhakar and Dunbar (1993), Doucouliagos (1995), Cahuc and Dormont (1997), Fakhfakh and Perotin (2000), Blasi et al. (2010) or the Oxera-Treasury report (2007a, b) with data from other countries.

[2] Wolf and Zwick (2003) find no effect of group incentive payments on productivity in German establishments. Analysing data from the UK, Bryson and Freeman (2010) only report a positive impact of profit sharing on productivity if it is combined with employee share ownership.

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. FitzRoy and Kraft (1995) for example use a Heckman selection model (Heckman, 1976) and find very strong selection effects. However, the estimator has been criticized for its strong distributional assumptions. An alternative, non-parametric method is matching. In recent years, its popularity in economics has grown[3] - also for the evaluation of firms - since it accounts for the selectivity problem, at least on observables.[4]

To control for unobservable differences between firms with and without profit sharing, several authors who analyse the effects of group incentive payments use panel data and apply fixed-effects models. This procedure allows controlling for firm-specific unobservable differences between establishments when estimating productivity effects of profit sharing. Studies using fixed-effects estimators are e.g. Fakhfakh and Perotin (2000) who use French firm level data and find that profit sharing increases productivity. Kumbhakar and Dunbar (1993) analyse the productivity effects of employee share ownership plans (ESOP) and profit sharing plans in the US by applying both fixed-effects and random-effects models. Their results indicate that there are significant positive effects of both types of plans. Kruse (1992) also reports a higher productivity for profit sharing firms in the US. Jones and Kato (1995) find that ESOPs and bonus payments boost productivity in Japanese firms. There is also evidence for positive productivity effects of profit sharing for the UK (e.g. Wadhwani and Wall, 1990).

Although the fixed-effects models used in these studies can eliminate the effects caused by unobserved time-invariant differences between firms, we use another approach to control for these differences. As we are not only interested in the causal effect of an introduction of profit sharing but also in the question if these profit sharing firms were already more productive before, we apply difference-in-differences. If there are any differences in productivity caused by time-invariant unobservable factors this method can be used to quantify the productivity advantage.

We combine propensity score matching with a difference-in-differences approach to

[3] Many applications of this method in the field of economics are policy evaluations, mainly the effects of active labour market programmes, where the observed units are individuals, see e.g. Lechner and Wunsch (2009) and Lechner et al. (2011).

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

control for selection on observable and unobservable factors. Our results indicate that selectivity is indeed a problem. Firms with profit sharing are already more effective before the sharing system is introduced. Moreover, we show that the selection bias is quite substantial. Nevertheless, after considering selection, the introduction of profit sharing still leads to a higher productivity.

Chapter 3 is structured as follows. Section 3.2 contains a short theoretical discussion of the impact of profit sharing on productivity. Section 3.3 addresses the problem of a possible selection bias and other indirect productivity effects of profit sharing. Section 3.4 describes the econometric methodology used for our estimations. In Sections 3.5 and 3.6 we present our data and the results of the empirical analysis. Section 3.7 shows the results of additional specifications to test the robustness of our results before we conclude in Section 3.8.

3.2 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 the firm's overall efficiency (Kruse, 1992).

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

The counter-argument 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, 1987a, 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. This necessitates an empirical examination of this question.

3.3 The issue of selectivity and other indirect effects

Apart from a direct effort effect there may be other indirect effects of profit sharing which could have an impact on productivity in these firms. 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 attract the more productive ones as they tend to benefit more from it. It is quite realistic that risk-averse workers prefer a fixed wage to 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. As a consequence, firms with profit sharing are more productive because they employ a higher share of highly skilled or motivated 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).

Besides worker sorting, there is another reason why qualification and thus productivity

[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).

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 in firm-specific skills. Hence, in these firms, general and specific skills are probably above the average which is then responsible for the productivity advantage (for an empirical analysis of the effects of profit sharing on training see Chapter 4 in this dissertation).

If profit sharing has a positive (direct or indirect) impact on productivity, the behaviour of most companies is hard to explain, since only a minority of firms in European countries make use of it. According to the statistics of the European Company Survey (ECS) presented in Table 3.1, the percentage of firms with a profit sharing system in European countries is generally quite low (see European Foundation for the Improvement of Living and Working Conditions, 2010). One exception is France, where the legal framework encourages the use of profit sharing.[6]

Table 3.1: Incidence of profit sharing in selected countries (in firms with 10 employees and more)

Country	Profit sharing establishments (%)
Austria	8
Belgium	15
Denmark	14
France	35
Germany	14
Greece	4
Ireland	11
Italy	3
Netherlands	27
Spain	17
Sweden	24
UK	8

Source: European Company Survey 2009

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

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 rational, however, if profit sharing did not *cause* a productivity increase. The observed positive correlation might simply be due to selectivity if, for example, better managed firms are just more likely to introduce profit sharing. In this case, productivity should already be higher before introduction. 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 productivity effect, not the incentive effect or the described indirect effects of profit sharing.

3.4 Econometric method

3.4.1 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 on observables as well as for time-invariant 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 and 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 weighting in the comparison depending on the particular matching estimator that is applied (see Heckman et al., 1997). We use matching to obtain a sample consisting of similar observations with regard to the factors Z_{it} which determine the existence or introduction of a profit sharing scheme. Propensity score matching 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. The propensity score is the probability of observing a certain treatment as a function of Z_{it}. We estimate probit models to compute the propensity score for each firm in our sample. Afterwards treated and non-treated firms with similar propensity scores are matched. Different matching estimators are possible.[7] We use nearest neighbour matching with replacement where

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

only the most similar observation of the control group serves as a match.[8] Moreover, our matching approach also includes the common support restriction, which prevents extrapolation of the distribution of the right-hand variables over unobservable regions (Blundell and Costa Dias, 2009).

Regression-adjusted matching is proposed by Heckman et al. (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 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 selection 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 slightly modify the method. Heckman et al. (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 after that we control for other time-varying variables which have an impact on productivity in a separate regression.

3.4.2 Difference-in-differences

Using matching, we are able to control for selection on observables but not on unobservables. Therefore, we combine the regression-adjusted matching approach with difference-in-differences and, in the second step, we extend the simple production function regression and estimate the following equation:

$$Y_{it} = \beta X_{it} + \delta_1 TG_i + \delta_2 T_t + \delta_3 PS_{it} + \varepsilon_{it}. \qquad (3.1)$$

As mentioned above, Y_{it} is the outcome variable of interest and X_{it} is a vector of observable control variables. TG_i is the treatment group 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 time-invariant differences

[8] In addition to one-to-one nearest neighbour matching, we also tried matching with, for example, two or three neighbours (see Section 3.6).

between treatment and control group. T_t is a set of time dummies for all pre- and post-treatment years. The variable PS_{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

$$\hat{\delta}_3 = (\overline{Y}_{X_{it,}TG=1,t} - \overline{Y}_{X_{it',}TG=1,t'}) - (\overline{Y}_{X_{it,}TG=0,t} - \overline{Y}_{X_{it',}TG=0,t'}). \qquad (3.2)$$

$\overline{Y}$ stands for the sample average of the outcome variable Y for a particular group and year. 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. With this methodology we intend to identify the average treatment effect 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 observed in our sample. That means we exclude establishments which introduce profit sharing and only look at firms which always or never have profit sharing. In this case we use our variant of regression-adjusted matching but with this definition of treatment we cannot yet control for unobservable differences. 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.

The main advantage of our methodology is the possibility of comparing 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 establishments is already higher before they actually introduce the payment system. We consider both productivity levels – before and after the introduction - so that the "true" effect of profit sharing can be identified (by using *introduction of*

profit sharing as treatment variable). This effect can then be compared with the coefficient that we estimate when we define *existence of profit sharing* as treatment variable as we do in our first step.

Difference-in-differences controls for common time trends and permanent differences between the two groups considered. A crucial assumption for the validity of difference-in-differences 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 (see also discussion in Section 3.6), we view the combination of regression-adjusted matching and difference-in-differences as the best possible method currently available to deal with observable and time-invariant unobservable heterogeneity.

3.5 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. The panel provides detailed information on many labour market topics, where a core set of questions (e.g. on employment or turnover) is identical in every wave. However, there are additional questions which are posed irregularly, such as those on profit sharing. Data on this issue is available for the years 1998, 2000, 2001, 2005, 2007 and 2009. Our sample includes firms providing information on the relevant variables for at least two consecutive years between 1998 and 2008[9]. 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 uniform industry classifications and with information on profit sharing available for our analysis.

As mentioned earlier, we distinguish between firms which have profit sharing throughout the whole period we observe them between 1998 and 2007 (treatment *existence of profit sharing*) and firms which introduce profit sharing (treatment *introduction of profit sharing*).

[9] 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. We do not use information on profit sharing in 2009 as there were remarkable changes within the industry classifications.

First, we estimate separate probit models for each treatment to obtain the respective propensity scores. The literature on profit sharing has identified many determinants of profit sharing which must be considered (see e.g. OECD, 1995, Pendleton et al., 2003). [10] Looking at the mean values of these variables for firms with and without profit sharing, it is obvious that these two groups are very different in many respects (see Table 3.4 in the Appendix). We use four firm size dummies (20-49, 50-249, 250-499 and more than 499 employees, control group: 5 to 19 employees), a dummy for young (founded after 1990) and one for East German 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.

With respect to the *existence of profit sharing* we first drop all firms which introduce this remuneration system and identify 2123 observations of firms which always use profit sharing. Our matched sample consists of 3570 observations (existence sample).[11]

Concerning the second sample, we exclude establishments which always apply profit sharing. Matching is again done on the first year's observation.[12] We obtain 2795 treatment observations (including periods before the introduction, $TG_i=1$), of which 1052 observations are from periods when profit sharing has been introduced ($PS_{it}=1$). Our matched sample consists of 4611 observations.

Additionally 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 (3.1) is extended by a dummy variable with unit value when a firm always applies profit sharing. Then, for the matching procedure, 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 7619 observations. As we will see in the next section, the

[10] For a more detailed discussion on the determinants of profit sharing see also Section 4.4.

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

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

separate estimations for existence and introduction have a better matching quality than the joint estimation. Nevertheless, we will also present the results of the latter.

Some of the variables we use to estimate the probability of the respective treatment are also employed in our main regressions, the estimations of Cobb-Douglas production functions.[13] Output is measured as the logarithm of sales volume.[14] Unfortunately, there is no direct information on the capital stock available in the IAB establishment panel. However, information on total investments, capital-widening and replacement investments is included. A possible proxy variable for capital is the amount of replacement investments.[15] This variable has the advantage to be available for all industries. However it has the disadvantage that large variations in the investment expenditures can lead to implausible variations in the capital stock variable (Mueller, 2008). Nevertheless, we apply this specification as a robustness check of our results (see Chapter 3.7). For our main estimations, however, we follow the approach of Mueller (2008) who proposes a procedure to use the perpetual inventory method for short panels too. First, information on average economic lives of capital goods and on replacement investments is used to calculate the absolute value of the capital stock. As there are large variations in investment expenditures, the calculated values are averaged to smooth these variations. Afterwards, the traditional perpetual inventory method is applied, starting from the calculated average.

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. Additionally, we use dummies for investment in ICT, 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

[13] Alternatively, we could use the full set of explanatory variables both for matching and for the estimation of the production functions. However, we prefer using only selected variables based on theoretical hypotheses and previous empirical work. Nevertheless, including all variables in the matching procedure leads to very similar results, which are available from the authors upon request.

[14] An alternative for the dependent variable is value added. However, this is not our preferred measure because the question on material costs is asked as share of intermediate inputs in total sales and the answers do not seem to be very reliable. Furthermore, this question is characterized by quite a large number of missing values. Nevertheless, as a robustness check, we also present estimations with value added as dependent variable in Section 3.7.

[15] This variable is e.g. used by Zwick (2005) and Addison et al. (2006).

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.

3.6 Estimation results

As explained above, matching is applied to three different samples: the first one consists of firms which always or never have profit sharing, the second one includes firms which introduce profit sharing or never apply it and the third one comprises both cases. 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.[16] Therefore, for every year separate matching has to be applied. As the number of treated firms in 1998 is very small in our sample, we do not use these observations for a separate probit estimation but combine observations of establishments which enter the sample in 1998 and 2000.[17] As an example, the results of the three probit estimations for the years 1998/2000 are presented in Table 3.5 in the Appendix – the one for existence of profit sharing, for introduction of profit sharing, and for the combination of both treatments.[18] Our matching is successful as the differences in the mean values of treated and matched firms for the two separate samples for *existence* and *introduction of profit sharing* are strongly reduced. In all but one of the cases there is no longer any significant difference at all.[19] Matching also reduces the differences between treated and control establishments in the full sample, but in some cases significant differences still exist.[20] Thus, we prefer separate estimations for each treatment.

The estimation results of the Cobb-Douglas production functions are presented in Table 3.2. The results for *existence of profit sharing* where we only control for selectivity on

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

[17] Thus we need three different probit estimations for each sample: one for establishments first observed in 1998 or 2000, one for 2001 and one for 2005.

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

[19] 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 3.6. The mean values after matching for all the other years are available from the authors upon request.

[20] E.g. after matching for the "starting point" 1998/2000 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 3.6, column 3).

observables but not on unobservables are presented in column 1. They indicate that profit sharing appears to have quite a strong positive impact on sales. The highly significant coefficient of 0.148 implies that profit sharing firms have a productivity about 16.0% higher than firms which do not use this remuneration scheme.

Table 3.2: Estimation results - Production function

	(1) Existence of profit sharing	(2) Introduction of profit sharing	(3) Existence and introduction of profit sharing
$\ln(\text{Employment}_{t-1})$	0.824***	0.865***	0.840***
$\ln(\text{Capital}_{t-1})$	0.171***	0.147***	0.164***
Existence of profit sharing	0.148***		0.154***
Treatment group		0.067***	0.084***
Introduction of profit sharing		0.050**	0.047**
Collective bargaining	0.071***	0.060***	0.084***
Works council	0.142***	0.116***	0.134***
Share of qualified employees	0.493***	0.499***	0.483***
East German establishment	-0.151***	-0.167***	-0.152***
Share of part-time employees	-0.800***	-0.568***	-0.671***
Share of apprentices	-0.675***	-0.703***	-0.756***
Limited liability	0.103***	0.100***	0.099***
Export share	0.399***	0.550***	0.520***
Investment in ICT	0.055**	0.034*	0.023
Technical standard of equipment (1=very good,…,5=poor)	-0.047***	-0.014	-0.034***
Number of observations	3570	4611	7619

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

However, the second column of Table 3.2 shows that, after additionally controlling for time-invariant unobservable factors, more than half of the higher productivity observed for profit sharing firms has to be attributed to systematic unobservable differences between treated firms and controls (coefficient of 0.067 for *Treatment group*). 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 existent productivity advantage and boosts productivity by about 5%.

The two coefficients of the dummies *Treatment group* and *Introduction of profit sharing* do not exactly add up to the coefficient of the variable *Existence of profit sharing* in column 1, in fact their sum is somewhat smaller. Maybe this points to differences between short-term and long-term effects. Unfortunately, the duration of firms establish profit sharing in our sample is too short to estimate real long-term effects after the introduction.

Another explanation could be that firms which only realize a small or no positive impact of profit sharing abolish this payment method after a while. Thus, only those establishments which really benefit from profit sharing will maintain its application. To test this hypothesis, we estimate introduction effects of profit sharing for firms which again abolish it during the observation period. However, we find similar positive productivity effects of profit sharing for this group of establishments.

However, a Wald test on equality of the sum of coefficients of the introduction estimation (*Treatment group* + *Introduction of profit sharing*) and the coefficient of the variable *Existence of profit sharing* of the first estimation shows that they are not significantly different from each other.[21]

Our findings are confirmed by the results of the estimation where we include both treatments (column 3). The significant coefficient of 0.154 for *Existence of profit sharing* again indicates that firms which always apply profit sharing have a higher productivity than firms without profit sharing. And again firms which introduce profit sharing are already much more productive before the introduction has taken place (significant coefficient of 0.084).[22] The introduction of profit sharing still has a positive effect on productivity.[23]

As mentioned above, the validity of difference-in-differences depends on the common trends assumption, which cannot be tested. However, we can at least compare pre-treatment trends in productivity for establishments which introduce profit sharing later on and which do not. Therefore, we plot productivity trends separately for treatment group

[21]To test this cross-model hypothesis based on overlapping samples we use the STATA command "suest" (see Weesie, 1999).

[22]In column 3, the sum of the coefficients for *Existence of profit sharing*, *Treatment group* and the coefficient for *Introduction of profit sharing* slightly differ from the corresponding coefficients for the separate estimations in columns 1 and 2, but they are not significantly different.

[23] Again, the sum of the two coefficients for *Treatment Group* 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.

observations (as long as profit sharing is not applied yet) and control group observations (see Figure 3.1 in the Appendix). Although there seems to be a bit more variation in productivity for the treatment group, trends for treated and control firms do not seem to be different. Moreover, we also graphically find a higher productivity of treated firms before profit sharing is introduced.[24]

3.7 Robustness of the results

To test the sensitivity of our results we perform some further robustness checks. The results are presented in Table 3.3. First, instead of only one nearest neighbour we apply matching with two and three neighbours. The results are very similar to those obtained with one nearest neighbour and coefficients only slightly change.[25] Alternatively, we estimate the model with another proxy variable for capital. We do not use Mueller's (2008) procedure of the perpetual inventory method as it is not applicable to some industries and instead simply use replacement investments which also leads to similar results.[26] Finally, we use the logarithm of value added as dependent variable instead of the logarithm of sales volume.[27]

Comparing all different specifications, the estimated productivity effects of an introduction of profit sharing vary between 4.6% and 6.4% (coefficients between 0.045 and 0.062) and are the highest when replacement investments as proxy variable for capital are used. However, in this case also the sample is slightly different (as there are firms from some additional industries included). Again, establishments which introduce profit sharing are already more productive before its implementation. The productivity advantage is between 5.4% and 11.2% (coefficients between 0.053 and 0.106). The largest lead in productivity is also found if replacement investments are included in the estimations.

All in all, we find evidence for the existence of selectivity effects. It seem to be the generally better managed firms which are more likely to introduce profit sharing. Nevertheless, profit sharing also has a productivity increasing effect. However, it is much

[24] In addition we carry out several t-tests which indicate that both groups of firms do not significantly differ in average annual productivity growth.
[25] For the complete estimation results see Tables 3.7 and 3.8 in the Appendix.
[26] For the complete estimation results see Table 3.9 in the Appendix.
[27] For the complete estimation results see Table 3.10 in the Appendix.

smaller than the estimated impact obtained by applying the simpler method which neglects a before-and-after comparison. Considering unobserved heterogeneity is therefore essential for this kind of research.

Moreover, our results could 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. First, firms are faced with implementation and administration costs. Costs can even be higher for smaller firms with less personnel management expertise which may even need consultation of an external expert (Backes-Gellner et al., 2002). Moreover, if profit sharing is a bonus payment additionally to the fixed wage, wage costs also increase.

Table 3.3: Results of robustness checks

	(1) Existence of profit sharing	(2) Introduction of profit sharing	(3) Existence and introduction of profit sharing
Production function after matching with 2 nearest neighbours			
Existence of profit sharing	0.148***		0.147***
Treatment group		0.056***	0.077***
Introduction of profit sharing		0.052**	0.046**
Production function after matching with 3 nearest neighbours			
Existence of profit sharing	0.137***		0.146***
Treatment group		0.053***	0.079***
Introduction of profit sharing		0.045**	0.047**
Production function with replacement investments as proxy for capital			
Existence of profit sharing	0.162***		0.171***
Treatment group		0.106***	0.101***
Introduction of profit sharing		0.062**	0.057**
Production function with value added as dependent variable			
Profit sharing (Existence)	0.107***		0.127***
Treated establishment		0.064***	0.066***
Introduction of profit sharing		0.060**	0.053*

***/**/* denotes significance at the 1%/5%/10% level. Notes: Heteroscedasticity-consistent standard errors, control variables included (see Tables 3.7, 3.8, 3.9 and 3.10).
Source: IAB Establishment Panel, waves 1998-2008, own calculations (controlled remote data access via FDZ).

3.8 Conclusion

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 claim that it is *common knowledge* that profit sharing can increase productivity.

However, prior research has often neglected possible selectivity effects. Using data from the IAB Establishment Panel we apply propensity score matching and combine 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 selection especially on unobservable factors strongly overrates this effect.

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), methods of verifying 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 which have low fixed costs and/or special characteristics which facilitate the positive effect of profit sharing will introduce this incentive scheme. The others may instead rely 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. To quantify welfare effects of subsidizing profit sharing firms or of the obligation to apply a sharing system (like in France) has high relevance for public policy.

3.9 Appendix

Table 3.4: Mean values of firms with and without profit sharing

	Firms without profit sharing	Firms with profit sharing
Number of employees	92.653	221.405***
Export share	0.081	0.171***
Share of qualified employees	0.353	0.474***
Collective bargaining	0.525	0.590***
Works council	0.282	0.549***
Limited liability	0.654	0.846***
East German establishment	0.433	0.311***
Age (founded after 1990)	0.399	0.376**
Shift of responsibilities	0.155	0.268***
Teamwork	0.090	0.166***
Independent work groups	0.077	0.157***
ICT investment	0.585	0.818***
Technical standard of equipment (1=very good,…,5=poor)	2.163	2.061***
Share of part-time employees	0.129	0.104***
Share of apprentices	0.059	0.051***
Sales per employee (in 1000 €)	127.300	188.173***

***/**/*: 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 3.5: Results of probit estimations (1998/2000)

	(1) Existence of profit sharing	(2) Introduction of profit sharing	(3) Existence and introduction of profit sharing
Establishment size 20-49	0.331***	0.148*	0.225***
Establishment size 50-249	0.427***	0.327***	0.380***
Establishment size 250-499	0.567***	0.511***	0.547***
Establishment size 500+	0.877***	0.685***	0.773***
Export share	0.430**	0.112	0.256*
Share of qualified employees	0.568***	0.546***	0.568***
Collective bargaining	-0.019	-0.028	-0.023
Works council	0.309***	0.343***	0.338***
Limited liability	0.157**	0.187***	0.196***
Age (founded after 1990)	0.059	0.167**	0.129**
East German establishment	-0.142*	-0.110*	-0.125**
Shift of responsibilities	0.330***	0.127*	0.210***
Teamwork	0.021	0.122	0.093
Independent work groups	0.247**	0.095	0.159**
ICT investment	0.332***	0.232***	0.285***
Number of observations	2599	2915	3348
Pseudo R2	0.170	0.133	0.149

***/**/* denotes significance at the 1%/5%/10% level. Notes: Industry and year dummies included.
Source: IAB Establishment Panel, waves 1998, 2000, own calculations (controlled remote data access via FDZ).

Table 3.6: Matching quality: Differences in mean values after matching (1998/2000)

	(1) Existence of profit sharing	(2) Introduction of profit sharing	(3) Existence and introduction of profit sharing
Number of employees	26.77	-6.89	13.55
Export share	-0.010	-0.005	-0.003
Share of qualified employees	-0.000	0.001	-0.014
Collective bargaining	-0.023	-0.030	-0.006
Works council	-0.009	-0.023	-0.003
Limited liability	-0.030	0.012	0.019
Age (founded after 1990)	0.005	0.039	-0.017
East German establishment	-0.009	-0.002	-0.009
Shift of responsibilities	0.009	0.004	0.036**
Teamwork	-0.046	-0.004	-0.013
Independent work groups	0.002	0.012	0.003
Investment in ICT	-0.005	0.021	0.002

***/**/* denotes significance at the 1%/5%/10% level.
Source: IAB Establishment Panel, waves 1998, 2000, own calculations (controlled remote data access via FDZ).

Table 3.7: Production function after matching with 2 nearest neighbours

	(1) Existence of profit sharing	(2) Introduction of profit sharing	(3) Existence and introduction of profit sharing
ln(Employment$_{t-1}$)	0.827***	0.870***	0.844***
ln(Capital$_{t-1}$)	0.150***	0.144***	0.159***
Existence of profit sharing	0.148***		0.147***
Treatment group		0.056***	0.077***
Introduction of profit sharing		0.052**	0.046**
Collective bargaining	0.067***	0.060***	0.085***
Works council	0.131***	0.102***	0.132***
Share of qualified employees	0.476***	0.453***	0.476***
East German establishment	-0.144***	-0.167***	-0.142***
Share of part-time employees	-0.835***	-0.587***	-0.720***
Share of apprentices	-0.422***	-0.630***	-0.612***
Limited liability	0.084***	0.136***	0.096***
Export share	0.421***	0.542***	0.529***
Investment in ICT	0.059***	0.022	0.029**
Technical standard of equipment	-0.032***	-0.025**	-0.037***
Number of observations	4549	5861	9077

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

Table 3.8: Production function after matching with 3 nearest neighbours

	(1) Existence of profit sharing	(2) Introduction of profit sharing	(3) Existence and introduction of profit sharing
ln(Employment$_{t-1}$)	0.857***	0.868***	0.850***
ln(Capital$_{t-1}$)	0.149***	0.143***	0.156***
Existence of profit sharing	0.137***		0.146***
Treatment group		0.053***	0.079***
Introduction of profit sharing		0.045**	0.047**
Collective bargaining	0.054***	0.052***	0.084***
Works council	0.140***	0.114***	0.133***
Share of qualified employees	0.463***	0.449***	0.470***
East German establishment	-0.147***	-0.171***	-0.155***
Share of part-time employees	-0.846***	-0.626***	-0.698***
Share of apprentices	-0.458***	-0.682***	-0.563***
Limited liability	0.087***	0.147***	0.110***
Export share	0.437***	0.548***	0.514***
Investment in ICT	0.045***	0.026	0.030**
Technical standard of equipment	-0.036***	-0.028***	-0.035***
Number of observations	5246	6690	10004

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

Table 3.9: Production function with replacement investments as proxy for capital

	(1) Existence of profit sharing	(2) Introduction of profit sharing	(3) Existence and introduction of profit sharing
ln(Employment$_{t-1}$)	0.865***	0.870***	0.870***
ln(Capital$_{t-1}$)	0.102***	0.112***	0.109***
Existence of profit sharing	0.162***		0.171***
Treatment group		0.106***	0.101***
Introduction of profit sharing		0.062**	0.057**
Collective bargaining	0.066**	0.043*	0.052**
Works council	0.187***	0.166***	0.153***
Share of qualified employees	0.664***	0.519***	0.592***
East German establishment	-0.068***	-0.152***	-0.136***
Share of part-time employees	-1.013***	-0.792***	-0.889***
Share of apprentices	-0.885***	-0.881***	-0.964***
Limited liability	0.127***	0.097***	0.079**
Export share	0.549***	0.684***	0.523***
Investment in ICT	0.127***	0.097***	0.133***
Technical standard of equipment	-0.060***	-0.027*	-0.052***
Number of observations	2868	3487	5878

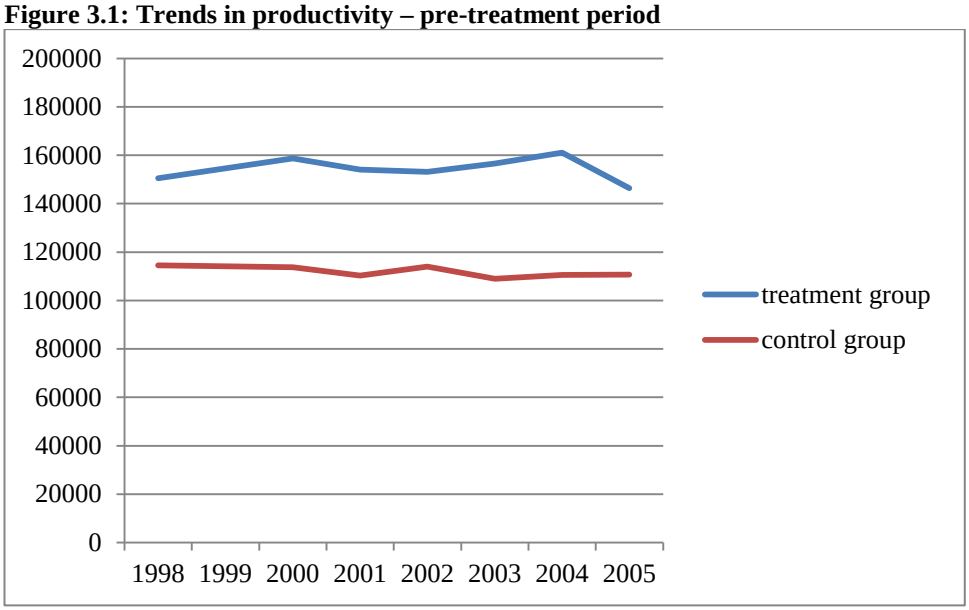
***/**/* denotes significance at the 1%/5%/10% level. Notes: Heteroscedasticity-consistent standard errors, industry and year dummies included.
Source: IAB Establishment Panel, waves 1998-2008, own calculations (controlled remote data access via FDZ).

Table 3.10: Production function with ln(value added) as dependent variable

	(1) Existence of profit sharing	(2) Introduction of profit sharing	(3) Existence and introduction of profit sharing
ln(Employment$_{t-1}$)	0.794***	0.873***	0.828***
ln(Capital$_{t-1}$)	0.157***	0.121***	0.144***
Existence of profit sharing	0.107***		0.127***
Treatment group		0.064***	0.066***
Introduction of profit sharing		0.060**	0.053*
Collective bargaining	0.100**	0.089***	0.091***
Works council	0.188***	0.075***	0.117***
Share of qualified employees	0.461***	0.436***	0.439***
East German establishment	-0.180***	-0.225***	-0.219***
Share of part-time employees	-0.842***	-0.703***	-0.807***
Share of apprentices	-0.694***	-0.897***	-1.030***
Limited liability	0.113***	0.060**	0.056**
Export share	0.330***	0.376***	0.384***
Investment in ICT	0.085***	0.059**	0.032
Technical standard of equipment	-0.021	-0.034**	-0.042***
Number of observations	3302	4251	7042

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

Figure 3.1: Trends in productivity – pre-treatment period



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

4 Profit sharing and training

This chapter is based on the publication

Kraft, Kornelius and Julia Lang (forthcoming): Profit sharing and training, Oxford Bulletin of Economics and Statistics.

4.1 Introduction

For obvious reasons incentives are of utmost interest in economics, and for many years economists have analysed the application and effects of incentive payments. One variant of performance-linked remuneration is profit sharing. This group incentive scheme implies that a part of the compensation of (almost) all employees is variable and depends on the overall profits of the firm. A large number of empirical studies report the effects of profit sharing on company performance, where productivity, profitability, wages and employment are frequently analysed. The reason most commonly suggested for the observed better performance is the incentive effect of a variable remuneration and, as profit sharing is a group incentive, peer group pressure to accord to somehow defined performance standards. Although this is probably the main reason for the frequently reported higher performance of organizations with a profit sharing system, there might also be indirect effects at work. The purpose of our study is to investigate the effect of profit sharing on training intensity as another possible cause of higher productivity.

Clearly, human capital is of prime importance not only for productivity but also for the profitability of a company. The literature on human capital[1] does not usually discuss the role of incentive payments like profit sharing. However, a collective incentive system may well affect investments in human capital. If a sharing system is employed, a return on investment costs is automatically guaranteed as a binding arrangement regulates the use of profits. This is not always the case in a traditional wage system as wages are largely determined by other factors such as seniority or favouritism of specific groups who have a particular bargaining power.

Profit sharing is an extreme form of group incentives, as the "group" consists of the whole workforce. Every member of the organization then has an interest in a high productivity of their co-workers and will encourage participation in training courses as any increase in productivity will benefit his or her own income. Furthermore, employees have a self-interest in helping colleagues during training on-the-job.

As emphasized by many authors, profit sharing is expected to align interests of employers and employees. One consequence may be lower employee turnover. Moreover, if profit

[1]Becker (1962, 1964) developed a framework for analysing investment in and returns to specific and general qualifications. Later theoretical contributions (Katz and Zidermann, 1990, Chang and Wang, 1996, Acemoglu and Pischke, 1999a, b, Stevens, 1994) considered market imperfections, such as asymmetric information and imperfect information, and modified Becker's conclusion on the financing of human capital investment. A survey of the theoretical literature is given by Leuven (2005). For an overview over results of empirical studies see e.g. Hansson (2008).

sharing is an add-on to and not a substitute of the going wage rate, total remuneration will increase. This also has a negative impact on quits. Clearly, longer expected tenure positively affects the incentives to invest in firm-specific human capital. Hence, the training decision may be one cause of the frequently observed better performance of companies with a sharing system.

Despite the given relevance, the relation between profit sharing and training is rarely analysed. The only studies of which we are aware are Azfar and Danninger (2001), Parent (2004), Gielen (2011) and Green and Heywood (2011). The authors all apply information from household panels, whereas we use company data for our empirical analysis. Azfar and Danninger (2001) and Gielen (2011) emphasize the negative effect of profit sharing on turnover, which in turn increases expected tenure and raises incentives for investing in firm-specific human capital. Parent (2004) regards profit sharing as a credible commitment of firms to reward the acquisition of firm-specific skills. Green and Heywood (2011) compare both channels and differentiate between a direct and indirect effect of profit sharing on training. They find a positive effect of profit sharing as it reduces hold-up problems and increases incentives to encourage training of colleagues (the direct effect) but no clear evidence for an indirect effect via reduced separations.

Our contributions to this topic are the following ones: First, our data allow us to disaggregate the profit sharing variable. We differentiate between firms where only a small, medium or large share of all workers benefit from profit sharing. This appears to be important because the percentage of workers of an establishment covered by profit sharing varies enormously.

It is well known that in the absence of tax incentives or mandatory requirement only a minority of all firms use profit sharing. Given the overwhelming empirical evidence on the beneficial effects of profit sharing this seems to be paradoxical. As it is very unlikely that the majority of all firms behave irrationally over a longer period, we suppose that selectivity effects are at work. Either only some firms have specific advantages from using such a specific payment scheme or firms which introduce it are in general the more efficient ones. In both cases simple cross-sectional comparisons would overestimate the average effect for all firms. Therefore, we take into account selection effects with respect to observable as well as unobservable factors by applying matching and conditional difference-in-differences. We find that profit sharing increases the training intensity, but only if the majority of all workers participate in this incentive payment.

This chapter is structured as follows. The next section contains a short theoretical

discussion of the impact of profit sharing on training. Section 4.3 describes the econometric methodology used for our estimations. In Sections 4.4 and 4.5 we present our data and the results of the empirical analysis before we conclude in Section 4.6.

4.2 Theoretical considerations

The relevance of human capital for the success of firms and the growth of economies is undisputed. Along with technical progress it determines productivity and the performance of companies. Reflecting its importance, human capital formation is an intensely investigated issue in economics; yet the causal relation running from profit sharing to training is largely ignored.[2]

Many empirical studies have shown that firms which use incentive systems such as profit sharing are more successful, measured in terms of productivity and profitability (see also Chapter 3).[3] The superior performance is usually explained by improved motivation of the workers, who increase their effort. However, profit sharing could also have an indirect impact on productivity via affecting qualification of the employees.

Profit sharing is a group-based incentive payment and an alternative to purely individual wage payments. This also alters incentives concerning training. Becker (1962, 1964) has shown that costs for investments into firm-specific human capital have to be shared between the firm and the employee in order to avoid moral hazard problems. Profit sharing provides a credible commitment on the part of the firm to share all additional returns. Hence the moral hazard problem is at least mitigated.

Recent contributions like Acemoglu and Pischke (1999a, b) demonstrate that a compressed wage structure implies incentives for the firm to invest in general employee training. There are several sources of wage compression. One reason is the existence of incomplete competition in the labour market, which implies that skilled workers have few alternative employers at their disposal and the firm has some monopsony power. Hence, even if qualifications are general, employees are unlikely to quit, although their wage is

[2] The literature on innovative human resource management policies deals with, among other things, both training and incentive payments. Many studies analyse, for example, how HRM practices affect productivity, and emphasize that complementarities among work practices can amplify a positive impact (see for example Huselid, 1995 or Ichniowski, Shaw and Prennushi, 1997). However, they do not consider any causal effect of incentive pay on human capital investments.

[3] Studies analysing such effects of profit sharing are e.g. FitzRoy and Kraft (1987a), 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.

below their productivity. In this case it also pays for the company to invest in general human capital. Another reason could be information asymmetries, first suggested by Katz and Ziderman (1990) who argue that asymmetric information might play an important role in the context of general training. If potential employers are looking for new employees, they will be less well informed about the skills a worker possesses than the current employer who trained this person. This reduces the advantages that a generally trained worker can gain by changing his or her job. If employees with general skills have no incentive to move to another firm, employers will (at least partly) pay for general training. But also if employers bear (part of) the cost for general training, employees must still be willing to participate in training courses. This will only be the case if workers can be sure that they will benefit from their training effort.[4]

The idea of a collective incentive system is to provide a clear basis for sharing returns, which are jointly created by capital and labour. Any increase in performance is divided between the factors of production. Profit sharing systems are not usually abandoned by the company and have simple rules concerning the division of profits. Thus, profit sharing provides a credible commitment on the part of the firm to share all additional returns. Parent (2004) motivates the necessity for a commitment by the very nature of specific skills. These skills are worthless outside of the particular firm and therefore no market value exists. Hence, there are no market forces at work which would lead to an optimal sharing rule. By introducing profit sharing, it is fixed that workers get a share of the profits and they can be confident that they will not be held up after investing time and money in the acquisition of specific skills.

As more recent theoretical approaches suggest that firms also have an incentive to invest in the general skills of their workers, just like in the case of specific training, it is not guaranteed that employees will get a fair share of the training benefits. Therefore, the positive effect of profit sharing as a binding commitment also applies with regard to general training.

Economists are sometimes surprised that profit sharing has any beneficial effects at all as the value of any productivity increase has to be shared with all other employees. However, obviously the worker in question has to bear alone the disutility from increased effort. This is called the 1/N (with N being the number of employees) or free rider problem. This problem is also present in the case of training investments. The return of

[4] In the empirical section we use a variable concerning training participation, but we have no information as to whether such training courses impart specific or general knowledge.

any further education has to be shared with all the others in the firm. Hence, the pay-outs resemble a prisoner's dilemma, where joint cooperative action would lead to an improvement on individual cheating, but the highest pay-out is received only if all others play cooperatively (invest in human capital) but I do not. However, in contrast to the classical prisoner's dilemma the behaviour of the other players is observed during the game and can be influenced by peer group pressure[5]. Furthermore, cooperation at work takes place every day and hence a repeated game would be much more appropriate to represent the situation than a one-shot prisoner's dilemma.

With a collective incentive payment every member of the group has a strong interest in a good performance by their co-workers. Hence, monitoring by a foreman is substituted or complemented by supervision on the same hierarchical level. This kind of supervision is probably very efficient and at the same time cost minimizing.

With regard to profit sharing, shared pay-outs imply that every member of the organization benefits from higher productivity of other members. It now pays to support on-the-job training of colleagues, since profit sharing creates a monetary reward for pushing co-workers to train more intensively. Perhaps even more important is that in such a situation it is in the interest of every employee to help others at the workplace. Hence, training-on-the-job will be intensified, become more efficient and most likely be much cheaper than if it is done by supervisors.

Additionally, profit sharing may have several effects on employee turnover. If paid on top of the going wage rate, profit sharing increases total remuneration.[6] As quits are partly determined by monetary factors, turnover will decline.[7] Work relations may also be affected as the intention of profit sharing is an alignment of interests of capital owners and workers. These factors in turn increase expected tenure and lead to a higher investment in specific human capital (see e.g. Azfar and Danninger, 2001). With a higher level of specific human capital the firm also reduces dismissals, as it wants to retain its investment.[8]

Clearly, as an alternative to joint incentives, a working and flexible individual wage system could offer efficient rewards for any investments. However, not every firm has such an efficient wage system. Wages are frequently determined by bargaining with a

[5] For the effects of peer group pressure, see Kandel and Lazear (1992).
[6] Kraft and Ugarkovic (2007) show that profit sharing is not a substitute for the usual wage rate.
[7] Even if profit sharing is a substitute for the fixed wage and not an add-on, it can help to stabilize employment over the business cycle by rendering remuneration more flexible.
[8] If the firm finances general training as well, the incentives to keep the employees are enhanced.

union and then other factors besides efficiency, such as seniority or power of specific groups within the negotiating union, affect remuneration. Furthermore, efficient rewards require an exact identification of the productivity effects of training, which is in many cases rather difficult as only the aggregate output is observed. Performance evaluation is frequently subjective, leaving room for evaluators' preferences, biases, discriminations and favouritism (Prendergast and Topel, 1993). This is all absent with binding arrangements as it is usually done in the case of profit sharing.

4.3 Econometric methodology

In Western industrialized countries only a minority of all firms make use of the instrument of profit sharing. According to the statistics of the European Company Survey (ECS) the percentage of firms with a profit sharing system in European countries is generally quite low (see European Foundation for the Improvement of Living and Working Conditions, 2010). In Germany only 14% of all firms with 10 employees and more used profit sharing in 2009 which is exactly the average of all countries in this survey.

Probably the majority of capital owners do behave rationally by not using profit sharing. Hence, there may be other reasons which inhibit apparently useful variable remuneration schemes. It is most likely that some firms have specific advantages in implementing them. There may be special conditions with respect to, for example, the work content (simple or demanding) or verification possibilities of the individual performance due to work organization (team versus individual tasks), that are responsible for the working of profit sharing. That means that firms with specific characteristics may use profit sharing, while others have no interest in such a kind of incentive and in contrast rely on other motivational instruments such as efficiency wages, tournaments or piece rates.

Hence, to analyse the effect of profit sharing on training, a mere comparison of firms with and without such a remuneration system is not appropriate. Different econometric approaches are suggested to account for factors that could affect a firm's probability to introduce profit sharing. One popular method to overcome the problem of selection in terms of observable factors is matching. The idea of this method is to compare the outcome of a treated group with the outcome of a similar group without treatment, which substitutes the counterfactual outcome (the outcome of a firm with treatment if it was not

treated). Additionally, in order to remove the influence of unobserved time-invariant characteristics, we use a conditional difference-in-differences estimation, which combines matching with a before-after comparison.

A closer look at the proportion of the workforce which is covered by the sharing system reveals that in many cases only a small proportion of all employees participate in profit sharing. That could mean that incentive payments are only used for the top management.[9] A variable and profit-related remuneration for the top management is also an incentive scheme, but it is very different from letting all employees of an establishment participate. Hence, it appears to be crucial to identify the share of workers who actually benefit from profit sharing.

We divide our treatment group of profit sharing firms into three different groups. The most important treatment in our analysis is profit sharing for the majority of the workers (PS=3), whereas PS=1 represents establishments, where only a small percentage of the employees is covered by a profit related payment. All establishments with a coverage between the two corresponding threshold values fall in category PS=2 and firms without profit sharing are represented by PS=0.

The matching framework for binary treatments is extended to multiple treatments by Imbens (2000) and Lechner (2001, 2002a, b).[10] Following Lechner's procedure there are M+1 different treatments with outcomes $Y^0,Y^1,\ldots,Y^M$. For each treatment m only Y^m can be observed and all other outcomes are counterfactuals. In the case of multiple treatments each group can be used as treatment as well as comparison group. The average treatment effect of treatment m relative to treatment k is

$$E(Y^m-Y^k|PS=m) = E(Y^m|PS=m) - E(Y^k|PS=m)\,. \qquad (4.1)$$

$E(Y^k|PS=m)$ stands for the counterfactual outcome. By definition, the counterfactual outcome is not observable since it describes the hypothetical outcome of not using profit sharing (or having a different level PS=k of profit sharing) in a company which in fact

[9] Unfortunately, the data we use provides no information on which type of employees (e.g. with respect to their occupational position) benefit from profit sharing in a specific firm.

[10] Matching for multiple or continuous treatments has mainly been applied before to the evaluation of active labour market policy programs. Lechner et al. (2007), for example, use this method to distinguish between different training programs for the unemployed and their effects on the outcomes of interest. The multiple treatment approach is less often applied to other topics. One exception is Görg et al. (2008) who investigate the effects of government support on exporting activity by distinguishing between small, medium and large subsidies. They also combine multiple treatment matching with difference-in-differences.

applies a specific level of profit sharing (PS=m). Therefore, a comparable counterpart for every treated firm must be found among the group of firms without or with another treatment. If the treatment status was considered as randomly assigned, the average outcome of firms with treatment k $E(Y^k|PS=k)$ could serve as an estimator for $E(Y^k|PS=m)$. However, as mentioned above, we expect selection playing an important role in the context of profit sharing. Without random assignment the problem in equation (4.1) could be solved if the characteristics which promote the establishment's decision to apply profit sharing for a certain proportion of the workforce can be determined. For these purposes Rubin (1977) introduced the conditional independence assumption (CIA) for binary treatments. Lechner (2001) shows that the CIA can be redefined for multiple treatments. The CIA states that the potential outcomes Y^k and Y^m are independent of the treatment status PS for firms with the same observable characteristics X:

$$Y^m, Y^k \perp PS \mid X . \tag{4.2}$$

Clearly, with an increasing number of variables X it gets very difficult to find an adequate match with the same characteristics. To overcome this problem, Rosenbaum and Rubin (1983) proposed propensity score matching. The propensity score is a function of the vector X and describes the probability of introducing a certain level of profit sharing. Now firms with similar propensity scores are matched. Different matching estimators are possible. We use nearest neighbour matching with replacement[11] and compare firms without profit sharing with those with different profit sharing intensities in 2009.

However, unobservable factors might also influence the impact of profit sharing. To additionally control for these factors the conditional difference-in-differences approach is appropriate which combines matching and a difference-in-differences comparison. We now focus on firms which implement profit sharing between 2007 and 2009 and again estimate treatment effects for multiple treatments. Using the difference-in-differences estimator, the average treatment effect of treatment m relative to treatment k is the difference between the change in the outcome of firms with treatment m and those with treatment k during the considered period (where t_0 indicates the pre-treatment 2007 period and t_1 the post-treatment period 2009):

[11] More precisely, we use nearest neighbour matching with two neighbours as this improves the matching quality compared to the case where we only use one nearest neighbour. In addition to nearest neighbour matching we also tried other matching methods, e.g. kernel matching, which leads to similar results (see Section 4.5.3).

$$E(Y^{m}_{t_1}-Y^{m}_{t_0}) - E(Y^{k}_{t_1}-Y^{k}_{t_0}) . \quad (4.3)$$

To control for observable as well as unobservable heterogeneity, the conditional difference-in-differences approach compares the difference of the changes of outcomes of treated and matched firms without treatment or with another treatment. Therefore, the second part of equation (4.3) is replaced by the difference in the outcome of matched establishments.

Difference-in-differences controls for common time trends and permanent differences between the two groups considered. A crucial assumption for 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). Although this assumption cannot be tested, as a robustness check, we compare changes in the dependent variable between treatment and control group observations in the pre-treatment period in Section 4.5.3.

To sum up, our approach is the following: We distinguish between different levels of profit sharing intensity and analyse the effects of these different levels. First we consider the *existence* of a certain profit sharing intensity in 2009. We compare the outcomes of the three different profit sharing levels (m=1, 2, 3) with the outcome of firms without profit sharing (k=0). Afterwards we analyse the effects of an *introduction* of profit sharing between 2007 and 2009. Therefore, we use a second sample and can additionally control for unobservable differences between the treatment groups and the control group by combining matching with difference-in-differences (conditional difference-in-differences).

In order to compute the propensity scores we specify multinomial logit models. Although the endogenous variable is based on the percentage of employees with profit sharing, we do not treat it as an ordered but as an unordered variable model. In our sample the majority of establishments with profit sharing offer it either to almost all or to a very small share of employees. We prefer the multinomial over the ordered logit model, because there appear to be different motives at work if an establishment introduces a profit sharing scheme for all workers or applies it only to a minority.

As an alternative to multinomial logit or probit models Lechner (2001, 2002a, b) suggests the estimation of binary choice models for each subsample of firms with treatments m and k for all different combinations of treatments. As a robustness test, we additionally

follow this approach, both for existence and introduction of different profit sharing levels and combine each treatment PS=1 or PS=2 or PS=3 with treatment PS=0.

4.4 Data and descriptive statistics

For our estimations we use the German IAB Establishment Panel of the Institute for Employment Research of the German Federal Employment Agency. The panel includes annual information about many general topics like turnover, the number of employees and wages. However, there are also questions which are only posed irregularly. Data on the number of participants in training are available for ten waves of the survey.[12] Training intensity is computed by dividing the number of people taking part in training in the first six months of the year by the total number of employees. With regard to firm or industry level data, the share of trained employees is a frequently used measure (see e.g. Dearden et al., 2006, Zwick, 2006). In contrast to studies applying individual level data, we do not have any information on the amount of training like training duration or the number of training events.[13] Data on profit sharing are provided for 1998, 2000, 2001, 2005, 2007 and 2009. Along with the question of whether the establishment offers profit sharing at all, in 2000, 2001, 2005 and 2009 the question is also asked which proportion of employees benefits from it. We use this information to construct our treatment variables for the multiple treatment approach.

We consider two different samples for our estimations. Regarding the *existence* of profit sharing as treatment variable we use data for 2009 and construct our first sample. It consists of 4742 non-treated and 1075 profit sharing firms, with 1067 of them also reporting the share of employees involved in profit sharing.[14] For the 1067 firms with information on profit sharing intensity, Figure 4.1 presents the distribution of the proportion of employees whose remuneration partly depends on a firm's performance. It is obvious that the mere existence of profit sharing does not imply that it is applied to the whole workforce. On the one hand in more than 40% of all profit sharing establishments in 2009 all employees of the firm participate. On the other hand, in one third of all profit

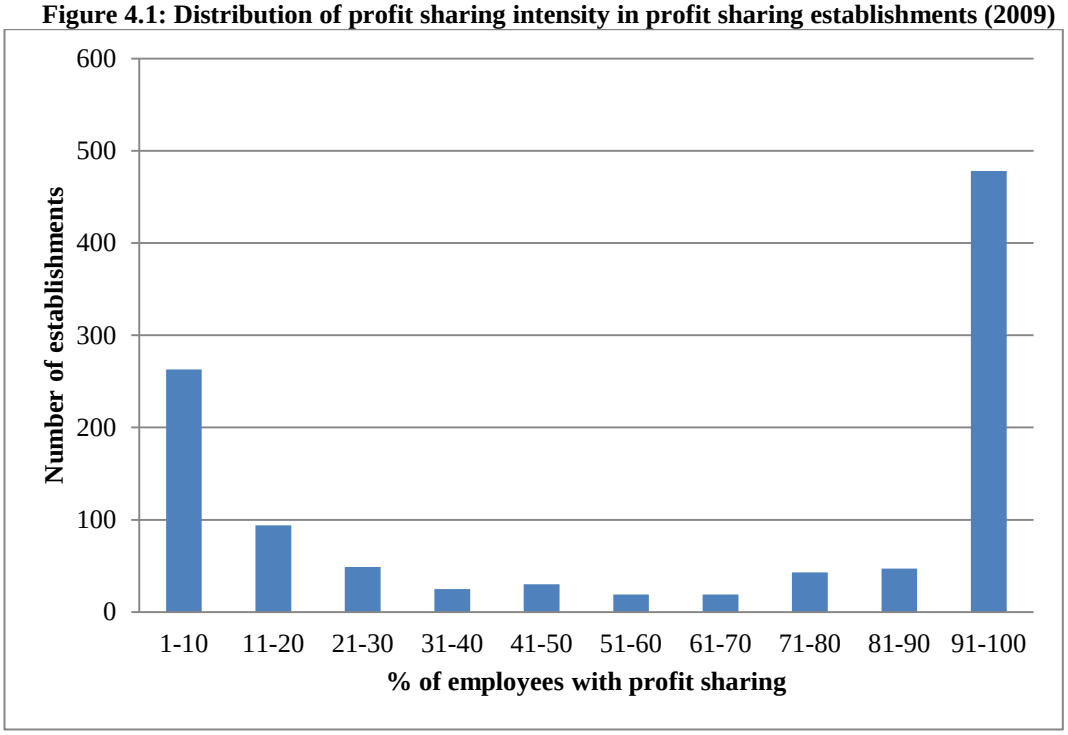
[12] More precisely, training information is available for the waves 1993, 1997, 1999, 2001, 2003, 2005, 2007, 2008, 2009 and 2010. Most of these waves also include questions about the form and financing of training.

[13] Gielen (2011) additionally uses the number of training events. Azfar and Danninger (2001) take into account both training incidence and training duration in weeks.

[14] We only consider establishments with at least 5 employees for our analysis. Furthermore we exclude non-profit firms and establishments from the forestry, agriculture and fisheries sector.

sharing firms, less than 20% of the workforce is covered by profit sharing. These figures indicate that responding firms and researchers may interpret the question differently. The literature on profit sharing usually assumes that all or at least most of the employees of an establishment receive, besides a fixed wage, a variable profit-related part, whereas in practice almost half of the profit sharing firms in our sample only let a minority of their workforce participate. In such cases it is quite likely that profit sharing applies only to the top management. The impact of such a variable remuneration system for the top management is also an interesting and much discussed issue,[15] but it has nothing to do with the common notion of profit sharing. Hence, we focus on establishments with a high ratio of employees participating in profit sharing.

Figure 4.1: Distribution of profit sharing intensity in profit sharing establishments (2009)



Source: IAB Establishment Panel, wave 2009, own calculations (controlled remote data access via FDZ).

For this purpose we divide establishments with profit sharing and distinguish between three treatments. In our first sample, where we consider the *existence* of a profit sharing scheme in 2009, treatment PS=1 is defined as profit sharing for less than 20% of all employees. The second treatment PS=2 represents profit sharing for at least 20% up to 99% of the workforce and PS=3 for all workers.

[15] Representative references include Jensen and Murphy (1990) and Hall and Liebmann (1998).

For our second sample, where we look at the *introduction* of profit sharing between 2007 and 2009, we identify all establishments which do not have a profit sharing system in 2007. The treatment observations are defined as those firms which introduced profit sharing between 2007 and 2009, whereas the control group consists of all firms without profit sharing throughout this period. We obtain a sample of 333 establishments which introduced profit sharing (four of these do not report profit sharing intensity) and 3670 firms without such a remuneration scheme. One third of the establishments introducing profit sharing offer it to at most 10% and one third to more than 95% of their workers. Hence, we define treatment PS=1 as profit sharing for less than 10% of all employees, the second treatment PS=2 as profit sharing for at least 10% up to 95% of the workforce and PS=3 as profit sharing for 96% or more.[16]

Compared to the first sample, where we look at the existence of profit sharing schemes, profit sharing intensity is lower in the second sample. One explanation for this observation could be that profit sharing is first introduced in some departments of a company or for a specific share of employees and later extended to others. Alternatively, if profit sharing is a substitute for a part of the former fixed wage and even if all workers have the option to participate in a profit sharing scheme immediately, only a proportion of the employees will accept the offer directly after its introduction (and without any experience with the operation of the new payment system). When we consider firms in the IAB Establishment Panel which introduced profit sharing, the data shows that in the first year observed after introduction, the share of covered employees is on average 47.3% but increases to more than 65% after a few years.

In our first sample only about 18.5% of all establishments use a profit sharing system in 2009 and in the second sample about 8.3% of those without profit sharing in 2007 introduce it by 2009. Not even half of those can be seen as profit sharing firms in the "classical" sense as they offer it to only a small proportion of the workforce. Therefore, different selectivity effects could be present. Hence, we match firms with different coverage levels with those without profit sharing with regard to the determinants of this financial incentive scheme. In the literature a number of variables are discussed which may affect the application of a sharing system for the workforce (see e.g. OECD, 1995,

[16] We also tried different classifications for our samples (e.g. dividing the data into just two groups), which leads to similar results. Nevertheless, based on the bimodal distribution of profit sharing intensity it is reasonable to construct two different treatment groups which include the firms around the two peaks of the distribution and to put the remaining observations into a third group. Firms with intermediate profit sharing intensity appear to be different from the firms with low or high intensity (e.g. these firms are much smaller) and therefore we think we should not put them in the same group with one of the other two treatments.

Pendleton et al., 2003). The following results for the mean values relate to the second sample which considers the introduction of profit sharing.[17] Table 4.1 presents the mean values of all relevant variables for establishments which introduce or never use profit sharing between 2007 and 2009 as well as the mean values for the three different levels of multiple treatments at the beginning of the treatment period in 2007 (only the difference of the training intensity is measured as the difference between 2007 and 2009).

A comparison of the mean values of the change in the share of trained people between 2007 and 2009 confirms our basic hypothesis. In firms which introduced profit sharing between 2007 and 2009, the share of employees taking part in training increased from the first half of 2007 to the first half of 2009 by 7.0 percentage points which is significantly higher than the respective value of 1.5 percentage points in firms without a profit sharing system. However, a significant increase in the training intensity can only be found if a large proportion of employees participate in profit sharing. Compared to establishments without such a payment scheme, firms with profit sharing for the majority of workers (PS=3) realize a higher difference in training intensity of 10 percentage points.

Turning to the variables which may determine the use of profit sharing, we concentrate on comparing firms without a sharing system (PS=0) with those establishments which let at least 96% of their employees participate (PS=3). The size of a firm could have both positive and negative effects on the probability to introduce profit sharing. Larger firms may bear the costs of the introduction of a new remuneration system more easily. Furthermore, the more employees, the more difficult it becomes to monitor them and problems of asymmetric information become more severe. If a profit sharing scheme is applied, aims of the employer should become aligned with those of the workers, which may cause them to monitor each other. On the other hand the free rider problem is more severe for larger firms. Table 4.1 shows that firms with a high percentage of profit sharing and those without differ significantly in size. Treated firms (PS=3) have 152 whereas the non-treated ones only have 61 employees on average. For our estimations size is considered by five different size categories (dummy variables for firms with 5-19, 20-49, 50-249, 250-499 and at least 500 employees).

[17] The mean values for firms with and without profit sharing and the different profit sharing intensities for the first sample (existence of profit sharing in 2009) can be found in Table 4.8 in the Appendix.

Table 4.1: Mean values of profit sharing and non-profit sharing firms in 2007

Variable	Firms without profit sharing (PS=0)	All firms with profit sharing	Firms with profit sharing for less than 10% of employees (PS=1)	Firms with profit sharing for 10%-95% of employees (PS=2)	Firms with profit sharing for 96%-100% of employees (PS=3)
Difference of training intensity between 2009 and 2007	0.015	0.070***	0.052	0.040	0.115***
Number of employees	60.597	126.664***	180.671***	76.591	152.082***
Shift responsibilities (dummy)	0.118	0.201***	0.183*	0.182**	0.236***
Teamwork (dummy)	0.063	0.135***	0.146***	0,124***	0.136***
Independent work groups (dummy)	0.045	0.108***	0.146***	0,086**	0.100***
Share qualified employees (percent)	0.057	0.107***	0.072	0.126***	0.112***
Collective bargaining (dummy)	0.434	0.423	0.561**	0.409	0.336**
Works council (dummy)	0.185	0.348***	0.488***	0.263**	0.364***
Export share	0.059	0.127***	0.133***	0.110***	0.141***
ICT investment (dummy)	0.442	0.646***	0.695***	0.606***	0.655***
Limited liability (dummy=1 if AG, GmbH)	0.599	0.820***	0.927***	0.737***	0.836***
Age (dummy=1 if founded after 1990)	0.518	0.484	0.463	0.438*	0.536
East German establishment (dummy)	0.429	0.393	0.341	0.394	0.436
N	3670	333	82	137	110

***/**/* indicates statistical significance at the 1%, 5% and 10% level; refers to a t-test comparing the mean values of non-profit sharing firms with the mean values of profit sharing firms and firms with different profit sharing intensities, respectively.
Source: IAB Establishment Panel, waves 2007, 2009, own calculations (controlled remote data access via FDZ).

As mentioned above, human resources literature suggests that monetary incentives come along with other HRM practices. All these measures are expected to be more common in establishments whose work organization is very complex and where individual output is difficult to observe. Hence, we include dummies for different HRM measures (the shift of responsibilities, teamwork and independent work groups) in our estimations.[18] Firms

[18] Employers are asked if these HRM practices were introduced during the last three years.

which employ profit sharing systems are also expected to be characterized by a large share of qualified employees and by a high investment in information and communication technology. Table 4.1 supports these suggestions. All variables mentioned above are significantly different for non-treated and treated firms if treatment PS=3 is considered. These profit sharing firms have a considerably higher share of qualified workers and are more likely to invest in information and communication technology (ICT). In addition, they more often implement forms of non-monetary forms of worker participation. Zwick (2004) shows that different forms of employee participation could have mutually reinforcing effects. As profit sharing is easier to implement in a consultative environment where the level of trust between employees and management is high, we use the existence of a works council to control for a cooperative climate.[19] The existence of a works council could also help to introduce a profit sharing system as then management could easily talk with a representative body about the exact design of the system. By contrast, the impact of unions on profit sharing is not clear. Workers whose earnings partly depend on the firm's performance are supposed to be more strongly affiliated with their employer and less with workers' representation organizations like unions. The observation that unions have been rather hostile towards profit sharing systems supports this presumption. However, if profit sharing is paid in addition to the base wage, as is documented in a number of empirical studies (see e.g. Wadhwani and Wall, 1990, Bhargava and Jenkinson, 1995, Kraft and Ugarkovic, 2007), there is at least no financial reason for unions to oppose this kind of remuneration system. We use a dummy variable which equals one if a firm is covered by a collective bargaining agreement. In our sample not only the share of firms with a works council is significantly higher among those with profit sharing for most of the workers, but also the share of firms with a collective agreement.

Introducing a flexible remuneration system may be especially valuable for firms whose profits are risky and show a high variance because they could transfer parts of the risk to the employees. Hence, we include the export share of firms which is significantly higher for profit sharing establishments. It is suggested that the frequency of profit sharing is higher in young and growing companies (dummy variable *Age*, which equals one if an establishment is founded after 1990) which, however, is not supported by our simple comparison of means. Moreover, Möller (2000) finds that firms located in East Germany

[19] One could also argue that the existence of a works council stands for the contrary situation, where the workforce is in need of a representative body, as industrial relations are controversial.

are less likely to introduce profit sharing, which cannot be confirmed from our analysis if we concentrate on profit sharing for almost all employees (PS=3).[20] Finally, we expect profit sharing more likely to be introduced in companies with limited liability, i.e. joint stock companies (AG) and non-public limited liability companies (GmbH), which is true for firms in our sample.

Looking at establishments reporting to offer profit sharing to a small percentage of employees (PS=1), these firms do not have more qualified workers than the reference group of firms without sharing (but the other profit sharing firms have). The comparison of the mean values of the considered determinants points to the relevance of selection effects, and therefore we think a method to take account of it is needed.

4.5 Estimation results

4.5.1 Existence of profit sharing

At first, we present the estimations for the matching approach, where we look at the existence of different profit sharing levels in 2009 and do not control for unobservable differences yet. Table 4.2 reports the coefficients and marginal effects (in square brackets) of the multinomial logit estimation we need to calculate the propensity scores.

We find that most variables have the expected effects but the impact of several variables depends on the coverage level. Larger firms are more likely to use profit sharing, but the size effect appears to be more pronounced for establishments with a low or medium coverage level. The higher the export share of a firm, the more often it applies profit sharing to a medium proportion of employees or to all workers (PS=2 or PS=3), whereas firms with a low coverage (PS=1) are less often located in East Germany and are more likely to apply teamwork or independent work groups. Works councils seem to play an important role especially for profit sharing firms where all employees participate in the profit sharing system (PS=3).

We now use the results of the multinomial logit estimations to calculate the propensity scores needed for the matching procedure. In order to check if our nearest neighbour matching was successful, a comparison of the mean values of the exogenous variables after matching is required. After matching the differences in the means of the relevant

[20] However, if we, similar to Möller (2000), consider the existence of profit sharing and do not differentiate between different levels of profit sharing intensity, we also find that West German establishments more often apply profit sharing (see Table 4.8 in the Appendix).

variables between firms without profit sharing and those with different profit sharing intensities are strongly reduced and there are no longer any significant differences (see Table 4.9 in the Appendix).

Table 4.2: Estimation results of multinomial logistic regression (existence of profit sharing)

Variables	PS=1	PS=2	PS=3
Establishment size 20-49[1]	1.143*** [0.066***]	0.492*** [0.016**]	0.268 [0.004]
Establishment size 50-249	2.080*** [0.017***]	0.428** [0.009]	0.464*** [0.013]
Establishment size 250-499	2.527*** [0.113***]	0.927*** [0.032**]	0.340 [-0.001]
Establishment size 500+	2.631*** [0.113***]	1.064*** [0.033**]	1.066*** [0.042***]
Export share	-0.000 [-0.007]	0.754** [0.032**]	0.657** [0.036**]
Share of qualified employees	1.057** [0.036*]	0.977** [0.031*]	1.574*** [0.085***]
Collective bargaining	0.004 [0.002]	-0.466*** [-0.022***]	0.017 [0.004]
Works council	0.109 [-0.001]	0.306* [0.009]	0.967*** [0.041***]
Limited liability	0.934*** [0.037***]	0.379** [0.008]	0.897*** [0.048***]
Age	0.197 [0.009]	-0.134 [-0.008]	0.192 [0.012]
East German establishment	-0.305** [-0.015**]	-0.088 [-0.004]	0.116 [0.010]
Shift of responsibilities	0.313** [0.010]	0.478*** [0.019**]	0.401*** [0.020**]
Teamwork	0.369** [0.017**]	0.183 [0.008]	-0.106 [-0.010]
Independent work groups	0.353* [0.015*]	0.168 [0.006]	0.124 [0.004]
ICT investment	0.390*** [0.013**]	0.386*** [0.012*]	0.677*** [0.037***]
Number of observations	5809		
Pseudo R2	0.169		

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Industry dummies are included in the analysis but are not reported.[1]Reference group: firms with 5 to 19 employees. Average marginal effects of the covariates in square brackets.
Source: IAB Establishment Panel, wave 2009, own calculations (controlled remote data access via FDZ).

As a robustness check we additionally specify probit models for the subsamples of firms with each treatment PS=1, PS=2 or PS=3 and firms without profit sharing PS=0 (and drop all firms with other treatments). The results of the probit estimations are reported in Table 4.10 in the Appendix. Table 4.3 shows the results of matching for the multiple treatments.

Table 4.3: Estimation results matching 2009[21]

Treatment group	Matched firms without profit sharing	Difference in training intensity between treatment group and matched control group
Multiple treatments (after multinomial logit)		
PS=1 (<20% of employees) N=312 Training intensity: **0.253**	PS=0 (no profit sharing) N=4742 Training intensity: **0.232**	**0.022**
PS=2 (20% - 99% of employees) N=312 Training intensity: **0.377**	PS=0 (no profit sharing) N=4742 Training intensity: **0.278**	**0.099*****
PS=3 (100% of employees) N=443 Training intensity: **0.370**	PS=0 (no profit sharing) N=4742 Training intensity: **0.276**	**0.094*****
Multiple treatments (after binary probits)		
PS=1 (<20% of employees) N=312 Training intensity: **0.253**	PS=0 (no profit sharing) N=4697 Training intensity: **0.242**	**0.011**
PS=2 (20%-99% of employees) N=312 Training intensity: **0.377**	PS=0 (no profit sharing) N=4731 Training intensity: **0.284**	**0.092*****
PS=3 (100%% of employees) N=443 Training intensity: **0.370**	PS=0 (no profit sharing) N=4742 Training intensity: **0.273**	**0.097*****

***/**/* indicates statistical significance at the 1%, 5% and 10% level.
Source: IAB Establishment Panel, wave 2009, own calculations (controlled remote data access via FDZ).

Our estimation results reveal that establishments provide only significantly more training if profit sharing affects at least 20% of their employees. Firms with a medium or high coverage level of profit sharing have a higher training intensity of more than 9 percentage points compared to matched firms without profit sharing. We also estimated average treatment effects for binary treatments, where we only consider the existence or introduction of profit sharing, regardless of the percentage of workers covered. The average treatment effects after matching for these binary treatments can be found in Table 4.11 in the Appendix. We find a positive effect on all treated establishments of 7.2 percentage points but, as can be seen in Table 4.3, this effect is driven by those firms which let at least 20% of the workers participate in the profits.

[21]In some cases we lose some observations due to the common support restriction. Another reason for differing numbers of observations is that in some cases one of the industry dummies causes perfectly predicted failure.

4.5.2 Introduction of profit sharing

After the matching analysis we now present the results for the treatment *introduction of profit sharing* where we additionally control for unobservable differences. Therefore, we again require a multinomial logit model to estimate the propensity scores. We divide the firms which introduce profit sharing as explained in Section 4.3.

Table 4.4: Estimation results of multinomial logistic regression (introduction of profit sharing)

Variables	PS=1	PS=2	PS=3
Establishment size 20-49[1]	2.632*** [0.049***]	0.479** [0.013*]	0.012 [-0.003]
Establishment size 50-249	3.049*** [0.057***]	0.198 [0.003]	0.077 [-0.001]
Establishment size 250-499	3.972*** [0.075***]	0.155 [0.002]	-0.504 [-0.017]
Establishment size 500+	3.712*** [0.069***]	0.461 [0.010]	0.424 [0.007]
Export share	0.109 [0.001]	0.908** [0.028*]	0.687 [0.016]
Share of qualified employees	-0.139 [-0.005]	1.594*** [0.049***]	1.186* [0.028*]
Collective bargaining	-0.010 [0.001]	-0.154 [-0.004]	-0.617** [-0.016**]
Works council	0.274 [0.004]	0.293 [0.008]	0.666** [0.017**]
Limited liability	0.888* [0.016*]	0.346 [0.001]	0.743** [0.018**]
Age	0.149 [0.003]	-0.455** [-0.015**]	0.081 [0.003]
East German establishment	-0.179 [-0.003]	-0.089 [0.003]	-0.089 [-0.002]
Shift of responsibilities	-0.092 [-0.002]	0.190 [0.006]	0.407 [0.010]
Teamwork	0.300 [0.005]	0.351 [0.005]	0.244 [0.006]
Independent work groups	0.519 [0.009]	0.250 [0.007]	0.342 [0.008]
ICT investment	0.325 [0.005]	0.335 [0.010]	0.480 [0.012]
Number of observations	3999		
Pseudo R2	0.140		

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Industry dummies are included in the analysis but are not reported. [1]Reference group: firms with 5 to 19 employees. Average marginal effects of the covariates in square brackets.
Source: IAB Establishment Panel, waves 2007, 2009, own calculations (controlled remote data access via FDZ).

The estimation results for the introduction of profit sharing (marginal effects in square brackets) are presented in Table 4.4. They indicate that, in particular, representation of the employees' interests by a works council and the share of qualified employees increase the probability to introduce profit sharing for the majority of workers (PS=3), whereas coverage by a collective bargaining agreement has a negative impact. Furthermore, the marginal effects of the variable indicating the export share and the dummy for a firm's age are only significant in the case of medium coverage. In contrast, firm size appears to be relevant concerning the decision to introduce profit sharing if only a minority of employees is covered (probably the senior management). This last result is quite plausible, as with increasing firm size control problems and asymmetric information gain relevance. Our matching procedure works as no significant differences exist any longer in the mean values between treated and matched control firms.[22]

In addition to a multinomial logit model, in order to calculate the propensity scores, we estimate binary probit models for subsamples again. The results are presented in Table 4.13 in the Appendix.

Table 4.5 shows the results of conditional difference-in-differences. It turns out that, just like in the case of existence of profit sharing, the effects of profit sharing depend on the share of workers covered. When we take into account unobservable heterogeneity but only consider a binary treatment, the results indicate that firms which introduce profit sharing have a significantly higher training intensity compared to firms without profit sharing (see Table 4.11 in the Appendix). However, distinguishing between different coverage levels in Table 4.5, we find that this overall effect is only caused by firms with high profit sharing intensity. Profit sharing for a small share of employees (maybe only for managers) has no effect on training. Only firms which introduce profit sharing for the majority of the workforce show a significant rise in the training intensity. Depending on whether a multinomial logit model or binary probit estimations are used to obtain the propensity scores, the positive training effect is 8.2 and 11.6 percentage points respectively.

[22] The differences in the means between treated and matched firms are reported in Table 4.12 in the Appendix.

Table 4.5: Estimation results conditional difference-in differences 2007-2009[23]

Treatment group	Matched firms without profit sharing	Difference in training difference between treatment group and matched control group
Multiple treatments (after multinomial logit)		
PS=1 (<10% of employees) N=82 Training difference: **0.052**	PS=0 (no profit sharing) N=3670 Training difference: **0.006**	**0.046**
PS=2 (10% - 95% of employees) N=135 Training difference: **0.036**	PS=0 (no profit sharing) N=3670 Training difference: **0.003**	**0.033**
PS=3 (>95% of employees) N=109 Training difference: **0.116**	PS=0 (no profit sharing) N=3670 Training difference: **0.033**	**0.082****
Multiple treatments (after binary probits)		
PS=1 (<20% of employees) N=82 Training difference: **0.052**	PS=0 (no profit sharing) N=3431 Training difference: **0.006**	**0.046**
PS=1 (20%-99% of employees) N=137 Training difference: **0.040**	PS=0 (no profit sharing) N=3330 Training difference: **0.030**	**0.011**
PS=3 (100%% of employees) N=110 Training difference: **0.115**	PS=0 (no profit sharing) N=3625 Training difference: **-0.001**	**0.116*****

***/**/* indicates statistical significance at the 1%, 5% and 10% level.
Source: IAB Establishment Panel, waves 2007, 2009, own calculations (controlled remote data access via FDZ).

4.5.3 Robustness checks

As mentioned above, the common trend assumption is crucial for the validity of our difference-in-differences results. Thus, as a robustness test - for those firms we also observe before 2007 - we compare changes in training intensities between treatment and control firms during the pre-treatment period (2003 to 2007). As data on training is not available for every year, we focus on the change in the share of trained employees between 2003 and 2005 and between 2005 and 2007. In both cases the difference in the change in training intensities between treatment group and control group firms is very small and insignificant (the difference in the change between 2003 and 2005 is -0.012, the difference in the change between 2005 and 2007 is -0.001). This means that pre-treatment trends in training do not differ.

To test the robustness of our results with respect to the choice of the matching procedure, we tried different numbers of nearest neighbours and kernel matching. Tables 4.6 and 4.7 show that the treatment effects are very similar to the ones obtained before. The effects

[23] Just like for the treatment "existence of profit sharing", with "introduction of profit sharing" as treatment variable we sometimes lose observations due to the common support restriction and because in some cases one of the industry dummies causes perfectly predicted failure in the probit estimations.

for existence of profit sharing are only highly significant for treatments PS=2 and PS=3, which means that at least a medium share of workers are covered by profit sharing.

Table 4.6: Matching and conditional difference-in-differences with 3 nearest neighbours

	Matching 2009	**Difference-in-differences 2007-2009**
Treatment group/control group	Difference in training intensity between treatment group and matched control group	Difference in training difference between treatment group and matched control group
Multiple treatments (after multinomial logit)		
PS=1/PS=0	**0.028**	**0.031**
PS=2/PS=0	**0.106*** **	**0.028**
PS=3/PS=0	**0.096*** **	**0.091** **
Multiple treatments (after binary probits)		
PS=1/PS=0	**0.022**	**0.062**
PS=2/PS=0	**0.092*** **	**0.017**
PS=3/PS=0	**0.092*** **	**0.120*** **

***/**/* indicates statistical significance at the 1%, 5% and 10% level.
Source: IAB Establishment Panel, waves 2007, 2009, own calculations (controlled remote data access via FDZ).

Table 4.7: Matching and conditional difference-in-differences with kernel matching

	Matching 2009	**Difference-in-differences 2007-2009**
Treatment group/control group	Difference in training intensity between treatment group and matched control group	Difference in training difference between treatment group and matched control group
Multiple treatments (after multinomial logit)		
PS=1/PS=0	**0.036***	**0.042**
PS=2/PS=0	**0.108*** **	**0.017**
PS=3/PS=0	**0.101*** **	**0.103** **
Multiple treatments (after binary probits)		
PS=1/PS=0	**0.027**	**0.044**
PS=2/PS=0	**0.105*** **	**0.021**
PS=3/PS=0	**0.096*** **	**0.101** **

***/**/* indicates statistical significance at the 1%, 5% and 10% level.
Source: IAB Establishment Panel, waves 2007, 2009, own calculations (controlled remote data access via FDZ).

The results again indicate a training effect of more than 9 percentage points. Only in one case (kernel matching after a multinomial logit estimation to calculate propensity scores)

also profit sharing for the minority of employees (PS=1) has a positive impact on training intensity. However, with 3.6 percentage points the effect is much smaller and only significant on the 10% level. With regard to conditional difference-in-differences, the results confirm the previous finding that only profit sharing for the majority of employees leads to a significant increase in training intensity. The effects vary between 9.1 and 12.0 percentage points which again are similar to the effects of 8.2 and 11.6 percentage points in Table 4.5.

4.6 Conclusion

We investigate the impact of profit sharing intensity on training. In order to reduce possible selectivity effects a matching approach is applied which compares treated units with similar untreated units. Unobserved heterogeneity is removed by a conditional difference-in-differences approach.

Using a sample of German firms we find that the introduction of profit sharing significantly increases the share of employees who get trained. Hence, according to our results, collective performance pay works not only due to increased effort by the employees, but also because of higher participation in training courses. In our view this is an important contribution towards explaining the frequently observed higher productivity of companies with a profit sharing system. Thus, profit sharing has a direct and an indirect impact. Such direct and indirect effects may also be present if other incentive schemes are applied. However, compared with collective payments they may work differently. For example tournaments will not presumably encourage cooperation at the workplace and are therefore more often observed if agents work in isolation. The comparison of the effectiveness and usefulness of incentive systems in different situations and surroundings appears to be a promising research area. Specific advantages in the use of a particular system and selectivity will probably not only be an issue with respect to profit sharing.

Incentives affect the performance of an organization in many different ways. Aside of human capital, other factors determining the performance of an organization may be affected, such as innovativeness or investment. In our view future research should focus on the many indirect ways in which incentives work.

Most previous work of which we are aware takes profit sharing as a zero/one variable when applying company data, where all workers or none are covered. A crucial point of our approach is that we can observe and take into account the proportion of workers who are actually covered by a profit sharing scheme. To our knowledge there is no study taking into account the specific distribution of profit sharing intensity over establishments which has turned out to be very important for our results. The positive effects on training intensity are only present if the majority of the workforce participates in this group incentive payment. It would be interesting to test whether this effect is also present in other samples.

4.7 Appendix

Table 4.8: Mean values of profit sharing and non-profit sharing firms in 2009

Variable	Firms without profit sharing	All firms with profit sharing	Firms with profit sharing for less than 20% of employees	Firms with profit sharing for 20%-99% of employees	Firms with profit sharing for 100% of employees
Training intensity	0.219	0.337***	0.253**	0.377***	0.370***
Number of employees	70.835	185.017***	176.333***	168.965***	204.406***
Shift responsibilities (dummy)	0.136	0.248***	0.266***	0.244***	0.237***
Teamwork (dummy)	0.073	0.136***	0.173***	0,131***	0.113***
Independent work groups (dummy)	0.053	0.113***	0.144***	0,099***	0.099***
Share of qualified employees (percent)	0.064	0.138***	0.103***	0.138***	0.161***
Collective bargaining (dummy)	0.456	0.515***	0.574***	0.433	0.530***
Works council (dummy)	0.216	0.470***	0.462***	0.375***	0.542***
Export share	0.063	0.142***	0.136***	0.106***	0.171***
ICT investment (dummy)	0.388	0.643***	0.628***	0.612***	0.677***
Limited liability (dummy=1 if AG, GmbH)	0.616	0.824***	0.914***	0.708***	0.842***
Age (dummy=1 if founded after 1990)	0.521	0.487**	0.484	0.442***	0.519
East German establishment (dummy)	0.420	0.364***	0.327***	0.330***	0.418
N	4742	1075	312	312	443

***/**/* indicates statistical significance at the 1%, 5% and 10% level; refers to a t-test comparing the mean values of non-profit sharing firms with the mean values of profit sharing firms and firms with different profit sharing intensities, respectively.
Source: IAB Establishment Panel, wave 2009, own calculations (controlled remote data access via FDZ).

Table 4.9: Differences of mean values for multiple treatments after matching (Existence of profit sharing)

Variables	PS=1/PS=0	PS=2/PS=0	PS=3/PS=0
Establishment size 20-49	0.003	-0.013	0.026
Establishment size 50-249	0.000	-0.016	-0.026
Establishment size 250-499	-0.001	0.016	0.015
Establishment size 500+	0.001	0.014	0.001
Export share	0.021	-0.004	0.001
Shift responsibilities	-0.011	-0.013	0.017
Teamwork	-0.017	-0.018	0.020
Independent work groups	-0.006	0.021	0.023
Share of qualified employees	0.016	0.022	0.009
Collective bargaining	-0.032	0.037	0.003
Works council	0.026	0.026	-0.029
ICT investment	-0.016	0.019	-0.024
Limited liability	-0.008	-0.014	-0.014
Age	0.006	0.008	-0.027
East German establishment	0.037	0.001	-0.042

***/**/* indicates statistical significance at the 1%, 5% and 10% level.
Source: IAB Establishment Panel, wave 2009, own calculations (controlled remote data access via FDZ).

Table 4.10: Results of probit estimations (Existence of profit sharing)

Variables	PS=1/PS=0	PS=2/PS=0	PS=3/PS=0
Establishment size 20-49[1]	0.678*** [0.068***]	0.253*** [0.026***]	0.102 [0.013]
Establishment size 50-249	0.979*** [0.099***]	0.236** [0.024**]	0.186** [0.023**]
Establishment size 250-499	1.246*** [0.125***]	0.462*** [0.048***]	0.108 [0.014]
Establishment size 500+	1.378*** [0.139***]	0.447** [0.046**]	0.564*** [0.071***]
Export share	0.046 [0.005]	0.400** [0.041**]	0.308** [0.039**]
Share of qualified employees	0.571** [0.058**]	0.510** [0.052**]	0.859*** [0.108***]
Collective bargaining	-0.005 [-0.001]	-0.225*** [-0.023***]	0.018 [0.002]
Works council	0.071 [0.007]	0.122 [0.013]	0.379*** [0.048***]
Limited liability	0.444*** [0.045***]	0.173** [0.018**]	0.416*** [0.052***]
Age	0.103 [0.010]	-0.067 [-0.007]	0.106 [0.013]
East German establishment	-0.148* [-0.015*]	-0.051 [-0.005]	0.056 [0.007]
Shift of responsibilities	0.169** [0.017**]	0.231*** [0.024***]	0.226*** [0.028***]
Teamwork	0.166* [0.017*]	0.104 [0.011]	-0.092 [-0.012]
Independent work groups	0.204* [0.021*]	0.061 [0.006]	0.074 [0.009]
ICT investment	0.183*** [0.018***]	0.191*** [0.020***]	0.361*** [0.045***]
Number of observations	5009	5043	5185
Pseudo R2	0.195	0.165	0.205

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Note: Industry dummies are included in the analysis but are not reported. [1]Reference group: firms with 5 to 19 employees. Average marginal effects of the covariates in square brackets.
Source: IAB Establishment Panel, wave 2009, own calculations (controlled remote data access via FDZ).

Table 4.11: Estimation results matching and conditional difference-in-differences for binary treatments

Matching		
Treatment group (all firms with profit sharing)	Matched firms without profit sharing	Difference in training intensity between treatment group and matched control group
N=1072 Training intensity**: 0.337**	N=4742 Training intensity: **0.265**	**0.072*****
Conditional difference-in-differences		
Treatment group (all firms with profit sharing)	Matched firms without profit sharing	Difference in training difference between treatment group and matched control group
N=333 Training difference**: 0.070**	N=3659 Training difference: **0.004**	**0.066*****

***/**/* indicates statistical significance at the 1%, 5% and 10% level.
Source: IAB Establishment Panel, waves 2007-2009, own calculations (controlled remote data access via FDZ).

Table 4.12: Differences of mean values for multiple treatments after matching (Introduction of profit sharing)

Variables	**PS=1/PS=0**	**PS=2/PS=0**	**PS=3/PS=0**
Establishment size 20-49	0.018	-0.044	-0.041
Establishment size 50-249	0.018	0.063	0.032
Establishment size 250-499	-0.024	0.011	-0.018
Establishment size 500+	-0.012	-0.004	-0.005
Export share	0.016	0.003	-0.015
Shift responsibilities	0.049	0.011	-0.028
Teamwork	0.018	-0.033	0.000
Independent work groups	0.030	0.007	0.013
Share of qualified employees	0.011	-0.002	0.011
Collective bargaining	0.030	-0.007	0.041
Works council	0.006	0.037	-0.018
ICT investment	0.012	-0.019	0.037
Limited liability	-0.024	0.033	0.018
Age	-0.061	0.000	0.050
East German establishment	-0.006	-0.070	-0.009

***/**/* indicates statistical significance at the 1%, 5% and 10% level.
Source: IAB Establishment Panel, waves 2007-2009, own calculations (controlled remote data access via FDZ).

Table 4.13: Results of probit estimations (Introduction of profit sharing)

Variables	PS=1/PS=0	PS=2/PS=0	PS=3/PS=0
Establishment size 20-49[1]	1.015*** [0.046***]	0.200* [0.016*]	-0.011 [-0.001]
Establishment size 50-249	1.198*** [0.056***]	0.058 [0.005]	0.038 [0.002]
Establishment size 250-499	1.975*** [0.076***]	0.022 [0.001]	-0.218 [-0.013]
Establishment size 500+	1.489*** [0.068***]	0.176 [0.014]	0.140 [0.008]
Export share	0.051 [0.002]	0.460** [0.036**]	0.312 [0.019]
Share of qualified employees	-0.064 [-0.003]	0.783*** [0.061***]	0.560* [0.036*]
Collective bargaining	-0.036 [-0.002]	-0.088 [-0.007]	-0.281*** [-0.017***]
Works council	0.150 [0.007]	0.161 [0.013]	0.310** [0.019**]
Limited liability	0.377** [0.017**]	0.150 [0.012]	0.309** [0.019**]
Age	0.082 [0.004]	-0.205** [-0.016**]	0.040 [0.002]
East German establishment	-0.093 [-0.004]	0.028 [0.002]	-0.027 [-0.002]
Shift of responsibilities	-0.061 [-0.003]	0.110 [0.009]	0.182 [0.011]
Teamwork	0.159 [0.007]	0.168 [0.013]	0.165 [0.010]
Independent work groups	0.258 [0.012]	0.101 [0.008]	0.149 [0.009]
ICT investment	0.123 [0.006]	0.171* [0.013*]	0.193* [0.012*]
Number of observations	3513	3467	3735
Pseudo R2	0.206	0.091	0.123

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Note: Industry dummies are included in the analysis but are not reported. [1]Reference group: firms with 5 to 19 employees. Average marginal effects of the covariates in square brackets.
Source: IAB Establishment Panel, waves 2007-2009, own calculations (controlled remote data access via FDZ).

5 The aims of lifelong learning: Age-related effects of training on wages and job security

This chapter is based on the SOEPpaper No. 478 of the same title.

5.1 Introduction

In the course of rapid technical progress lifelong investments in human capital have become indispensable for the majority of employees to be successful in their job. Starting with Becker (1962) not only a lot of theoretical work has been carried out until today. There is also an extensive empirical literature dealing with training investments and their effects.[1] Despite many educational policy demands for lifelong learning, most continuing training takes place at the beginning of working life. Many empirical studies show that the incidence of training activities decreases with age (e.g. Booth, 1991, Bassanini et al., 2007). With regard to Becker's human capital theory this finding seems to be plausible because the older an employee the shorter is the amortisation period of training investments. An argument in favour of training especially for older workers is that their initial training was a long time ago and their skills could have become obsolete over time. Regarding the effects of training, empirical analyses find that they often differ by age. Most studies analyse wage effects of training and they provide evidence for increasing wages especially for younger workers (e.g. Bassanini, 2006; Pfeifer et al., 2008 for German employees). However, workers cannot only benefit from training in form of rising wages but training can also increase their employability and decrease the risk of becoming unemployed.

In this study I use data from the German Socio-Economic Panel (SOEP) for the years 2000, 2004 and 2008 and first also estimate training effects for younger and older employees. The results indicate that training has very different impacts on the considered labour market outcomes of workers of different ages. Applying difference-in-differences I find that older workers realize no rise in wages but their perceived employment security increases. Younger workers, however, mainly benefit from training as it induces a higher wage growth. Furthermore, the results indicate that some of the effects depend on whether workers have repeatedly participated in training or if training takes place occasionally. The findings suggest that there might be a decrease in marginal returns from successive training especially in the case of perceived job security of older workers. Another explanation for the results could be that for some employees with specific jobs or positions regular training is indispensable and just something like a routine job activity or might even be obligatory. Thus, it could be possible that for this group of employees

[1] For an overview over possible effects of training see e.g. Bassanini et al. (2007) and Hansson (2008).

positive training effects are weaker or non-existent (as colleagues with the same job or in a similar position have a similar training experience).

It is very likely that younger and older employees pursue different objectives when participating in training courses. To analyse the different labour market effects of training for older and younger workers in more detail, I focus on the goals of training courses. I identify wage and job security effects of training courses with different purposes. My results show that the two most important aims respondents assign to courses have very different impacts on the two considered labour market outcomes. If workers are trained to adjust to new demands in their current job (the most often stated aim), this can reflect in higher perceived job security after training. In contrast, as could be expected, further qualification for professional advancement (the second most important purpose) increases wages as well as training to get introduced to a new job. Different aims could explain different outcomes between several groups of trained people. The positive impact of training on job security of older employees could be attributed to the fact that they mainly attend training courses which aim at adjusting skills to new requirements of their job. For this group of employees training can help to fill a productivity gap in their current job. In contrast, compared to older workers, younger employees more often participate in courses aiming at qualifying them for professional advancement or in induction training courses, which could explain their increasing wages. In contrast to job security effects for older workers, positive wage effects for young employees do not depend on the pre-sample training history.

This chapter is structured as follows: The next section provides an overview of the theoretical considerations as well as the empirical results regarding incentives for continuous training and its effects on labour market outcomes. Afterwards, Section 5.3 describes the data and the econometric approach used to estimate training effects, followed by the empirical analysis in Section 5.4. Finally, Section 5.5 concludes.

5.2 Background

5.2.1 Training goals in the course of working life

The theoretical discussion about investments in human capital mainly focuses on the question who invests in and benefits from training, employees or employers[2]. Studies about the determinants of training participation show that individual, job, firm and institutional characteristics are important. With regard to individual factors the probability to attend training courses can depend on the amortisation period for training investments. As it is much shorter the older a worker is, human capital theory indicates that training incidence decreases with age which is also found empirically (for example Booth, 1991, Ok and Tergeist, 2003, Büchel and Pannenberg, 2004, Bassanini et al., 2007).

There are several reasons why employers and employees consider investing and taking part in training, respectively. The purpose of investments in human capital may differ depending on the phase of working life. Training courses which aim at the introduction to a new job typically take place at the beginning of the career directly after initial training (Lois, 2007) but might also be necessary if a worker changes his or her job or place of work during working life.

Motivation for training might also change over time. This does not necessarily mean that motivation for training decreases with age, but that older and younger workers may have different motives for training participation (Zwick, 2011). Ebner et al. (2006) find that goal orientation strongly changes during the lifecycle. Young adults aim at growth, whereas older people focus on maintenance and loss prevention. With regard to training activities, this can reflect in different training purposes. Younger workers may choose courses which boost their career and increase their wage. Older workers might pursue very different aims. As their initial training took place a long time ago, many skills might have become obsolete over time which worsens labour market prospects for this group of employees. Thus, for older workers continuous training can be important to adjust their skills and knowledge to new job requirements caused e.g. by technological change. A severe loss of human capital of older workers or a decreasing demand for certain

[2] Becker (1962) established the idea that investments in human capital could be differentiated according to their specialisation and, depending on it, there can be different effects on workers' wages and differences in the division of training costs between employers and employees. Contrary to his theoretical predictions many empirical studies find that training is usually at least partly paid for by the firm, although most training activities are identified as investment in general human capital. More recent theoretical approaches can explain these observations, e.g. Katz and Ziderman (1990), Acemoglu and Pischke (1999a, b).

occupations can even involve retraining for a new job although empirical studies indicate that job mobility decreases with age (see e.g. Zimmermann, 1998).

Wages of older workers might exceed their productivity due to seniority-based payment systems and stagnating or even decreasing productivity above a certain age, which negatively affects their employability (Skirbekk, 2003). With regard to empirical evidence, some studies find a wage-productivity gap for older workers (e.g. Dostie, 2011, Cataldi et al., 2011) whereas others report no evidence for older employees' productivity lagging behind wages (e.g. van Ours and Stoeldraijer, 2010). Skirbekk (2003) argues that older workers' experience has a positive impact on their productivity but only up to a given duration. Afterwards, job performance may decline because cognitive and physical abilities decrease with age. Whether this is the case, however, will strongly depend on the occupation of an employee. If there really is a discrepancy between wage and productivity of older employees, it may be necessary to increase productivity by adjusting knowledge and skills to current demands in the job. Training participation of older workers would then be a measure to close a potential wage-productivity gap and improve employability.

If purposes of training courses are very different for older and younger workers, also the effects on different labour market outcomes should differ. Young employees at the beginning of their working life aim at professional advancement which can reflect in higher wages, whereas older employees try to keep their skills up-to-date to prevent (job) losses. If older workers participate in training courses because productivity even lags behind their wages, they will certainly not be rewarded by a wage increase. Therefore, training of older employees might not reveal in higher wages but in an increase in employability and job security.

Participation in courses that aim at introducing participants to a new job can reflect in higher wages if it is due to a career move, but starting a new job (regardless of whether getting training or not) can also be associated with a decrease in job security as normally there is a probationary period in a new job. Furthermore, workers might not know what to expect from a new job (with respect to job requirements, colleagues, superiors) when starting it. Clearly, in such a case training does not necessarily have a causal impact on job security but just occurs in a situation of high uncertainty.

5.2.2 Empirical evidence on training effects

The extensive empirical literature on wage effects mainly confirms that workers benefit from training as it involves an increase in wages.[3] Only very few studies report no positive impact of training.[4] With regard to financing of courses Booth and Bryan (2005) find a positive wage effect of employer-financed training for British employees whereas participation in self-financed courses has no effect.[5] Pfeifer et al. (2008) obtain similar results for Germany whereas Gerfin (2004) reports somewhat lower effects for employer-sponsored training in Switzerland compared to all work-related training but the difference is not significant.

Besides effects on wages, some studies also analyse the impact of training on employability and job security or career opportunities. Dieckhoff (2007) shows that, in Germany, continuous training decreases the probability to get unemployed and increases the probability to find a new job for unemployed people which is not the case in Denmark and the UK. Furthermore, for Danish and German workers, trained ones are more likely to change into higher level occupations. Analysing European data, Ok and Tergeist (2003) come to the conclusion that, besides rising wages, continuing training significantly increases the chances to find a new job after having been laid off, but find no significant effect on the probability to be laid off for employed people.

Several studies report that different groups of employees – e.g. workers of different age - benefit from training in different ways. Melero (2004), using data from the British Household Panel Survey (BHPS), shows that training is important for careers of female workers. The correlation of training and promotion chances for women decreases with age, whereas there is no clear pattern for male employees. Wage increases caused by promotion and training are lower for older workers.

[3] See e.g. Lynch (1992), Loewenstein and Spletzer (1998), Parent (1999), Frazis and Loewenstein (2005) for the US; Booth (1991), Blundell et al. (1996), Budria and Pereira (2004), Gerfin (2004), Büchel and Pannenberg (2004), Muehler et al. (2007) for European countries.

[4] For example Goux and Maurin (2000) analyse wages of trained French workers. They find that, after controlling for selection into training, there is no significant wage effect anymore. They suppose that especially employees with high ability get trained. Pischke (2001) also finds positive but insignificant effects of training on wages of German employees.

[5] This contradicts Becker's theory, where only specific training is partly paid for by the employer, but in return the employer also profits from training investments by an increase in productivity which should exceed the increase in employees' wages. If workers finance their training without any financial help (which according to Becker's theory would be the case for general training) wage growth could be expected to be larger than in the case of employer-sponsored training. Although more recent theoretical approaches can explain the fact that employers pay for general training (see e.g. Acemoglu and Pischke, 1999a, b), it is not obvious why wage effects should be even higher in this case.

Bassanini (2006) analyses European data and comes to the conclusion that employees disadvantaged on the labour market - older and low-skilled workers - profit from training taken with previous employers by increased perceived job security, whereas workers with already good career prospects on the labour market (young and highly qualified employees) benefit in form of wage growth. Training taken with the current employer induces wage increases and higher job security for all workers, regardless of age, gender or educational attainment.

Picchio and van Ours (2011) report a positive impact of training participation in the previous year on the probability of not becoming unemployed for both younger and older workers in the Netherlands. Büchel and Pannenberg (2004) analyse SOEP data and find, among other things, positive wage effects especially for the group of younger workers, at least in West Germany, but in their study this is also the age group for which training reduces the risk to become unemployed. Pfeifer et al. (2008) also use SOEP and BHPS data. For Germany, they find that employer-financed training significantly increases wages, where the effects are higher for younger and male employees. Furthermore, training is also beneficial to low-skilled and older workers– also especially participation in employer-financed courses – as it reduces their unemployment risk. British workers also realize a wage increase by participating in employer-financed training. This positive effect is stronger for older employees and women.

Besides training incidence many studies also take into account the amount of training, measured e.g. by the duration of training or the number of courses. For example Büchel and Pannenberg (2004) or Booth and Bryan (2007) find that wages increase with the number of training courses. However, Franzis and Loewenstein (2005) report that the wage return to an extra hour of training diminishes rapidly with the amount of training received. In contrast, Arulampalam and Booth (2001) show that not the number of training courses but only training incidence matters for wage growth of young men. As I find quite clear effects of training incidence in this study and the results on training intensity are somewhat ambiguous, I only mention other measures of training (number of courses and duration) in connection with some robustness checks (see Chapter 5.4.4). Nevertheless, depreciation of human capital can necessitate multiple or repeated training events.

5.2.3 Empirical evidence on training goals

Different effects for specific groups of workers can be explained by the fact that employees (and employers) consider training necessary due to different reasons. Budria and Pereira (2007) take into account the purpose of training courses of Portuguese workers. They differentiate between courses which are aimed at updating or improving skills that employees need for their current job and courses which aim at developing skills for future employment. They find higher wage effects for training which improves skills needed for the current job.

Booth and Bryan (2007) analyse British data and also differentiate between the aims of training. They divide courses into four different training types, "induction", "current skills", "future skills" and "general skills", which are not mutually exclusive. In their analysis of wage effects however, they mainly focus on "current skills training" and find that this type of training increases wages in current as well as future jobs.

Beicht et al. (2006) and Zwick (2011) use German data sets and compare the relevance of different goals and the self-assessed effectiveness of or benefits from training. Beicht et al. (2006) find that with regard to socio-demographic aspects, older employees consider most of the different goals of training less important than younger workers. This is especially the case for aims which indicate an enhancement of career opportunities. With regard to the returns of further training, the assessed benefits of training participation mostly decrease with age. Compared to young workers, the oldest workers (aged 55-64) report lower perceived gains for almost all categories (except for social and professional contacts), even for job security. Zwick's (2011) results mainly confirm these findings. His analysis also shows that employees of the oldest age group report less importance of different training goals, they even attribute less importance to higher job security. Concerning the benefits of training, younger workers find their training activities more effective with regard to most training goals, except for higher earnings and job security. Zwick's (2011) study focuses on explaining why training for older employees is less effective. He comes to the conclusion that this is caused by a wrong allocation of training contents and training forms.

I estimate the effects of training participation on two labour market outcomes, wages and job security. Besides estimating effects for different age groups, I try to explain different effects by different training purposes. As courses with different aims may even have opposing effects on some labour market outcomes, it can be crucial to distinguish between different purposes. The precise classification of training goals is described in the

following section. In contrast to the latter two studies, I do not have variables based on direct assessment of the benefits of courses. Instead, I use perceived job security and monthly wages of workers as dependent variables. At least for wages this method has the advantage that the measure of the effects of training is not subjective and it is possible to exactly quantify wage increases. Furthermore, the data of Beicht et al. (2006) and Zwick (2011) do not allow to control for unobserved differences between employees. Moreover, I distinguish between repeated and occasional training events and find that the effects of training participation can differ depending on pre-sample training participation.

5.3 Data and econometric approach

For the empirical analysis I use the waves 2000, 2004 and 2008 of the German Socio-Economic Panel (SOEP)[6] but focus on the period from 2004 to 2008 later on in the estimations (for further information on the data see Wagner et al., 2007).

The sample used for the analysis of training effects is restricted to full-time employees.[7] As I want to analyse effects on perceived job security and most workers in Germany enjoy protection against dismissal I exclude workers who are not covered.[8] In the surveys 2000, 2004 and 2008 (and also in 1989 and 1993), respondents younger than 65 are asked about further training activities during the last three years. Besides informal training (reading scientific or professional publications or attending congresses) people are also asked if they received any formal training and how many professional training courses they have participated in. Furthermore, for the three most recent courses, the SOEP provides information on the duration, timing and the host organization the course was held on. Moreover, there are questions about e.g. financial support, participation certificates or the aims of training. For the year 2008 I use this information on the purpose of the last three training courses an employee participated in. Respondents can choose

[6] The data used for this study was extracted using the Add-On PanelWhiz for Stata®. PanelWhiz (http://www.PanelWhiz.eu) was written by Dr. John P. Haisken-DeNew (john@PanelWhiz.eu). See Haisken-DeNew and Hahn (2010) for details. The PanelWhiz generated do-file to retrieve the data used here is available from the author upon request. Any data or computational errors in this study are my own.

[7] I exclude implausible answers and outliers by dropping full-time employed people with less than 30 working hours and a monthly gross income of less than 600 Euros. The final sample consists of workers aged 20-64 (civil servants and apprentices excluded).

[8] As workers who start a new job after 1 January 2004 only enjoy protection against dismissal in firms with more than 10 workers (before this date the threshold was more than 5 employees) I exclude the two smallest firm size categories which are firms with less than 5 workers and firms with 5 to 19 workers.

between five different categories which are “retraining for a different profession or job”, “introduction to a new job”, “qualification for professional advancement”, “adjusting to new demands in my current job” and “other” (where multiple answers are possible). I generate dummy variables for each category which each equal one if a person states to have at least participated in one course (out of a maximum of three courses) with this certain purpose. Besides the possibility that people can choose multiple aims of one course,[9] different courses could have had different goals. Therefore, the different dummies for training purpose are not mutually exclusive. The sample for the base analysis consists of 2075 people who are observed in 2000, 2004 and 2008. Out of these employees 769 say that they have participated in training in the last three years up to the time they were interviewed in 2008. Information on training participation prior to 2004 and 2000 is only used to identify the training history of a person.

The upper part of Table 5.1 shows the mean values for the variables used in this analysis both for training participants and non-participants before the training period of interest (2004-2008) in the year 2004. There seems to be persistence in training participation as 34.1% of all training participants between 2004 and 2008 also repeatedly took part in training between 1996 and 2004. Maybe this fact could be explained by different needs for keeping up to date in different occupations or industries, as, for example, not every job is affected by technological change in a similar way. If regular participation in training is even just something normal for specific employees and is necessary to practise a certain profession at all,[10] effects on wages and job security can differ from those for workers for which training is less common. However, the observed persistence of training events could also be caused by different (unobservable) individual factors like ability and motivation, which have an impact on workers’ training decisions. Thus, for the empirical analysis, I differentiate between workers who also regularly participated in training in the period before 2004 (between 2000 and 2004 as well as between 1996 and 2000) and those who did not.

[9] About 86% of all training participants only report one purpose of one course.

[10] For example, for practicing physicians in Germany periodic continuing medical training is virtually obligatory since otherwise they risk a loss of certification or financial penalties (see § 95d SGB V for panel doctors, § 137 SGB V for medical specialists in hospitals).

Table 5.1: Differences in mean values of pre-training characteristics between training participants and non-participants

	Non-participants (between 2004 and 2008)	Training participants (between 2004 and 2008)
Pre-training characteristics in 2004		
Repeated training participation between 1996 and 2000 and between 2000 and 2004	0.080	0.341***
Female (dummy)	0.285	0.317
Years of education	12.134	13.407***
Age(years)	42.637	40.745***
Tenure (years)	12.315	11.787
Firm size: 20-99 workers (dummy)	0.251	0.176***
Firm size: 100-199 workers (dummy)	0.142	0.108**
Firm size: 200-1999 workers (dummy)	0.337	0.332
Firm size: 2000 workers and more (dummy	0.270	0.385***
Workplace in East Germany (dummy)	0.240	0.246
Working hours	38.560	38.722
Job change between 2004 and 2008 (dummy)	0.081	0.100
Gross monthly wage (€)	2867.54	3428.68***
Job security (1 = very concerned, ..,3=not concerned at all)	2.064	2.122*
Differences in outcome variables between 2008 and 2004		
Difference gross monthly wage (€)	210.741	362.637***
Difference job security	0.165	0.226*
Number of observed employees	1306	769

***/**/* indicates statistical significance of a t-test between the mean values of the variables of workers with and without training between 2004 and 2008 at the 1%, 5% and 10% level.
Source: SOEP waves 2000, 2004 and 2008, own calculations.

Regarding observable characteristics, Table 5.1 shows that in this sample employees participating in training are on average younger and better educated than those without training and work in larger firms. Job security is measured on a 3-point Likert scale. Respondents are asked whether they are concerned about their job security. There are three possible answers: people can be “very concerned” (1), “somewhat concerned” (2) or “not concerned at all” (3). Employees who participate in training already have higher

wages and a weakly significant higher perceived job security than those in the control group, even before the courses take place. The lower part of Table 5.1 shows the change in the outcome variables between 2004 and 2008, the period in which workers potentially attend training courses. After the period of three years in which participants received training, they achieve a significantly higher wage increase and a larger rise in self-reported job security compared to workers who did not participate in any courses.

For the empirical analysis I focus on the waves 2004 and 2008. I apply difference-in-differences to evaluate the impact of training on wages and perceived job security, which allows considering unobservable time-invariant group-specific characteristics.[11] The general estimation equation which will be modified later on according to the specific questions of the subsections is

$$Y_{it} = \delta_1 TG_i + \delta_2 Training08_{it} + \beta X_{it} + \varepsilon_{it}. \qquad (5.1)$$

Y_{it} is the dependent variable (wages and job security, respectively); X_{it} consists of a set of covariates (including a time dummy for the post-treatment year 2008). TG_i is a dummy variable which indicates belonging to the treatment group, i.e. participating in training between 2004 and 2008 and equals one for participants in both years. *Training08* only equals one for trained workers after the training has taken place, i.e. in 2008. Thus δ_2 is the coefficient of interest as it measures the treatment effect.

As the dependent variables are log monthly gross wages and the ordinal variable subjective job security (measured on a 3-point Likert scale), OLS and an ordered logit models are applied, respectively.

Table 5.2 presents the incidence and purpose of training courses. To compare workers of different ages, I divide all employees into three age groups according to their age in 2008. The first group consists of the youngest workers up to an age of 39 years. Workers between 40 and 49 are in the second group and all workers who are at least 50 years old are combined in the third age group. The first column shows that more than one third of all employees participated in training between 2004 and 2008. The share of training participants in two successive periods before 2004 is 17.7%. Out of these 71.4% continue

[11] A crucial assumption for the validity of difference-in-differences is that both groups of people, training participants and non-participants, do not seriously change in their unobserved characteristics over time (see e.g. Blundell and Costa Dias, 2009). It is not clear if this assumption holds in this context. If it did not, the treatment effect of training could be wrongly estimated.

their training activities between 2004 and 2008 (not reported in Table 5.2). 29.9% of all employees never participated in any training courses between 1996 and 2008.

Table 5.2: Training incidence and purpose of courses

	All employees	**Age ≤ 39** (p-value of t-test of equality of coefficients for groups age≤39 & age=40-49)	**Age 40-49** (…for groups age=40-49 & age≥50)	**Age≥50** (…for groups age≥50 & age≤39)
Training participation between 2004 and 2008	0.367	0.424 (0.410)	0.401 (0.000)	0.299 (0.000)
Repeated training participation between 1996 and 2000 & between 2000 and 2004	0.177	0.166 (0.074)	0.206 (0.004)	0.150 (0.460)
No training participation between 1996 and 2008	0.299	0.246 (0.181)	0.280 (0.002)	0.354 (0.000)
Number of observed employees	2075	488	855	732
Purpose of training courses (last three years before survey 2008, trained people only)				
Retraining for different position or job	0.007	0.005 (0.878)	0.006 (0.651)	0.009 (0.597)
Introduction to a new job	0.046	0.078 (0.068)	0.041 (0.251)	0.023 (0.009)
Qualification for professional advancement	0.289	0.420 (0.000)	0.268 (0.052)	0.196 (0.000)
Adjusting to new demands in current job	0.785	0.710 (0.102)	0.772 (0.002)	0.877 (0.000)
Other	0.153	0.169 (0.653)	0.155 (0.569)	0.137 (0.358)
Number of observed employees	769	207	343	219

Notes: p-values of t-tests of equality between coefficients of different age groups in brackets.
Source: SOEP waves 2000, 2004 and 2008, own calculations.

The last three columns in Table 5.2 report the incidence of training activities for different age groups. 42.4% of all employees up to 39 and 40.1% of all employees between 40 and 49 participated in at least one course between 2004 and 2008, whereas the share of workers older than 49 who report to have taken part in training is only 29.9%, which is

significantly different from both the shares of training participants in the other two age groups.

For the subsample of training participants, the second part of Table 5.2 shows the aims of training activities. The purpose of courses most often stated by employees is adjusting to new requirements in their current job. With regard to all trained people, 78.5% participated at least in one course with this purpose. The second most important reason for workers to get trained (for 28.9% of all training participants) is qualifying for professional advancement. The third most frequent category is the residual category "other" (15.3%), followed by "introduction to a new job" (4.6%) and "retraining" (0.7%).[12]

Taking into account differences by age groups, for all workers of different age the most frequently stated aim is adapting to new demands of their current job, followed by career reasons. Nevertheless, there are differences between older and younger employees. Workers younger than 40 significantly more often get trained to qualify for professional advancement compared to workers age 40 and more and they also more frequently attend courses which aim at introducing them to a new job (although the difference is only weakly significant compared to workers 50 and older), whereas older ones more frequently need training to adjust to new requirements of their job. The differences between the share of workers who get trained to adjust their knowledge and skills to new demands in the current job increases with age and is significantly different between the group of workers older than 50 and the other two age groups. This is just what could be expected, as younger employees at the beginning or in the middle of their professional life probably try to improve their career prospects. The problem that obtained skills get redundant is especially one of older employees whose initial training was a long time ago. Therefore, this group of workers could be more in need for training to adjust to new qualification requirements which also reflects in the data.

[12] Although I would expect retraining to be a rare event, the percentage of courses with this purpose is very low. The small share of people who are retrained could be explained by the fact that the sample only includes people who were permanently employed during the observation period. Retraining typically aims at unemployed people and is often financed by the Federal Employment Agency. As there are only five retrained employees, I exclude this category from the empirical analysis of courses with different aims in the following section.

5.4 Empirical analysis

5.4.1 Differences between regular and infrequent training

Before differentiating between different age groups or different aims of training courses, I estimate wage and job security effects of training for all trained employees. However, I distinguish between groups of employees with regard to the possibility that they also could have participated in training before the period I consider for the estimations. First, there are workers who did neither participate in training courses in the pre-sample period between 1996 and 2004 nor in the following (treatment) period until 2008. This group of workers is used as the reference group in the following estimations.

The second group consists of people who did not report regular training participation between 1996 and 2004[13] but participated in courses between 2004 and 2008 (infrequently trained employees). Employees in the third group took part in training between 1996 and 2000, between 2000 and 2004 and also between 2004 and 2008 (frequently trained employees). The last group participated in training before 2004 but not after this year (only pre-sample participation). For the second and third group, I estimate difference-in-differences whereas the fourth group is only considered by a dummy variable. This approach allows analysing potential increasing or decreasing marginal benefits of repeated training events by comparing training after a period with and without regular training.

Please note that I only use the years 2004 and 2008 for the estimations. I am mainly interested in the effects of current participation between 2004 and 2008 in 2008 after training has taken place, which is only feasible for the second and third group.

The treatment group dummy *TG* in equation 5.1 is split up in a treatment group dummy *TGinfreq* for workers with infrequent participation and a treatment group dummy *TGfreq* for people with frequent training activities, which each equal one in both 2004 and 2008 for employees belonging to the respective group. The variables which indicate treatment groups (*TGinfreq* and *TGfreq*) control for time-invariant unobservable differences between (infrequently or frequently) trained and untrained people which also could have an impact on the outcome variables. The treatment itself is training participation between 2004 and 2008 - *Training08(infreq)* for infrequently trained workers and

[13] Note that people belonging to this group of irregular trained workers could have participated in training between 1996 and 2000 or between 2000 and 2004. As a robustness check I also exclude people reporting one former training period, which leads to similar results (see Section 5.4.4).

Training08(freq)) for frequently trained ones - which only equal one in the post-training period 2008 for the respective group of people.

The dummy variable *TGpre* indicates pre-sample training participants (people who took part in training before 2004 but not between 2004 and 2008) and equals one in both years (2004 and 2008) if a worker belongs to this group. Thus, for this group of employees I cannot estimate causal training effects as the coefficient can just reflect unobservable heterogeneity. The equation which is estimated is

$$Y_{it} = \delta_1 TGpre_{it} + \delta_2 TGfreq_i + \delta_3 Training08(freq)_{it} + \delta_4 TGinfreq_i + \delta_5 Training08(infreq)_{it} + \beta X_{it} + \varepsilon_{it}. \quad (5.2)$$

The set of control variables X_{it} includes the explanatory variables which are listed in Table 5.1, but age is considered by three age group dummies according to the differentiation in Table 5.2. δ_3 and δ_5 measure the treatment effects of training for participants with and without repeated pre-sample training, respectively.

The results of wage effects including the different groups of trained workers are reported in Table 5.3. The control variables have the expected effects. Women earn less than men, employees in East Germany earn less than similar workers in West Germany and wages increase with age, tenure, years of education, hours worked and firm size.

The dummy *TGpre* denoting workers with pre-sample training participation only (i.e. participation before 2004), indicates that people belonging to that group have higher wages of 2.8% in the period 2004 to 2008 compared to workers who never participated in any course in the pre-sample period and in the observation period. However, for this group it is not possible to differentiate between a selection effect and a treatment effect. Besides a real treatment effect (which was realized before 2004 but could still at least partly persist), the coefficient could just reflect that especially high-ability workers or highly motivated ones with already higher wages are more likely to receive training. For the remaining two groups of trained employees difference-in-differences allows to distinguish between both possibilities or at least to quantify the treatment effect. The two treatment group variables *TGfreq* and *TGinfreq* show that people who participate in training between 2004 and 2008 already earn more regardless of training participation in this period.

For the group of workers who already received training repeatedly before 2004 this higher wage of 6.9% could both be explained by prior training events and by

unobservable differences. The higher wage - even before training - of 5.0% of workers who infrequently participate in training should mainly reflect positive selection on unobservable factors affecting both training participation and wages. As this coefficient is lower than (although not significantly different from) the coefficient of *TGfreq*, it can be supposed that *TGfreq* includes an effect from selection on time-invariant unobservable factors as well as a positive effect of training events prior to the observation period.

Table 5.3: OLS Results – effects on wages for different groups of trained workers

Only pre-sample participation (before 2004)	
TGpre	0.028*
Participation in all periods (1996-2000, 2000-2004 and 2004-2008)	
TGfreq	0.069***
Training 08 (freq)	0.014
Participation between 2004 and 2008 (no regular participation before 2004)	
TGinfreq	0050***
Training 08 (infreq)	0.031**
Age 40-49	0.105***
Age ≥50	0.091***
Female	-0.184***
Years of education	0.045***
Tenure	0.007***
Firm size: 100-199 workers	0.041**
Firm size: 200-1999 workers	0.082***
Firm size: 2000 workers and more	0.114***
Workplace in East Germany	-0.258***
Working hours	0.009***
Job change	0.020
Number of observations	4150
R^2	0.567

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Occupation and industry dummies as well as a year dummy included but not reported. Reference group for firm size: 20-99 employees; reference group for age groups: 39 years old and younger. Standard errors are robust and clustered.
Source: SOEP waves 2000, 2004 and 2008, own calculations.

With regard to the treatment variables there are differences depending on whether participants regularly attended courses before. The coefficient of the variable

Training08(freq) of 0.014 shows that, for the group of frequent participants, there is no significant wage effect of the latest training events between 2004 and 2008. In contrast, for employees who are infrequently trained, the coefficient of *Training08(infreq)* indicates a significant positive wage effect of training participation. The wage increase caused by training is on average 3.1% between 2004 and 2008. As there is no significant wage effect for frequently trained people this could point to decreasing marginal returns to training. As mentioned before, another explanation could be that in some positions or jobs regular training is very common or just essential and therefore wage effects could be lower. However, as the treatment group effect for regularly trained workers is somewhat higher than (although not significantly different from) the treatment group effect for infrequently trained workers, decreasing marginal benefits of training seem to be even more plausible.

Table 5.4 presents the coefficients (column (1)) and marginal effects (columns (2)-(4)) of the ordered logit estimation with perceived job security as dependent variable.[14] Compared to the estimation results of the wage equation, less control variables have a significant impact. Higher education has a weakly significant positive effect on job security. One additional year of tenure increases the probability to be not concerned about job security by 0.3 percentage points and decreases the probability to be very or somewhat concerned by 0.2 and 0.1 percentage points respectively. Compared to similar West German workers, workers in East Germany have a higher probability to be very or somewhat concerned of 11.5 and 3.8 percentage points. Moreover, firm size positively affects job security.

With regard to the training variables not all have a significant effect. People who only participated in training before the observation period (*TGpre*), have a higher job security. The marginal effects indicate that these workers are more likely to be not concerned at all about potential job loss by 3.7 percentage points compared to employees not taking part in training at all between 1996 and 2008. Surprisingly, and in contrast to the wage estimation, there is neither a significant treatment group (*TGfreq*) nor a treatment effect (*Training08(freq)*) for the group of workers who permanently participated in training both

[14] Ai and Norton (2003) argue that the standard method to compute marginal effects based on the coefficients of nonlinear models is inappropriate for interaction variables. The training variable in this analysis is a treatment variable which is the interaction between a treatment group dummy and a time dummy. That means the objection of Ai and Norton (2003) would be relevant for the estimation of treatment effects in the case of the ordered logit model. However, Puhani (2012) argues that this is not problematic in this case as the derivative of an interaction variable in non-linear models does not represent the treatment effect in difference-in-differences models. Thus, I will directly interpret coefficients and marginal effects in this section.

in the observation period and in the pre-sample period. However, the treatment group dummy *TGfreq* is almost significant at the 10% level (p-value of 0.102).

Table 5.4: Ordered logistic regression results – effects on job security for different groups of trained workers

		Marginal effects		
	(1) Coefficients	(2) Very concerned	(3) Somewhat concerned	(4) Not concerned at all
Only pre-sample participation (before 2004)				
TGpre	0.175*	-0.023*	-0.014*	0.037*
Participation in all periods (1996-2000, 2000-2004 and 2004-2008)				
TGfreq	0.218	-0.028*	-0.019	0.047
Training 08 (freq)	-0.056	0.008	0.004	-0.012
Participation between 2004 and 2008 (no regular participation before 2004)				
TGinfreq	-0.107	0.015	0.008	-0.022
Training 08 (infreq)	0.369***	-0.045***	-0.035***	0.080***
Age 40-49	-0.020	0.003	0.002	-0.004
Age ≥50	0.088	-0.012	-0.007	0.019
Female	-0.051	0.007	0.004	-0.011
Years of education	0.034*	-0.005*	-0.003*	0.007*
Tenure	0.015***	-0.002***	-0.001***	0.003***
Firm size: 100-199 workers	0.137	-0.018	-0.011	0.029
Firm size: 200-1999 workers	0.199**	-0.026**	-0.016*	0.042**
Firm size: 2000 workers and more	0.216**	-0.029**	-0.018*	0.046**
Workplace in East Germany	-0.763***	0.115***	0.038***	-0.152***
Job change	0.062	-0.008	-0.005	0.013
Number of observations	4150			
Pseudo R^2	0.053			

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Occupation and industry dummies as well as a year dummy included but not reported. Reference group for firm size: 20-99 employees; reference group for age groups: 35 years old and younger. Standard errors are robust and clustered.
Source: SOEP waves 2000, 2004 and 2008, own calculations.

Finally, for the group of workers without frequent pre-sample training participation there is a strong treatment effect. Concerning job security before training, these people do not differ from those without any training during the observation period. The coefficient of *TGinfreq* is even negative. Participation in training significantly increases their perceived

job security. The marginal effects for *Training08(infreq)* show that training reduces the probability to be very or somewhat concerned about losing one's job by 4.5 and 3.5 percentage points, respectively and therefore increases the probability to be not concerned at all by 8.0 percentage points. With regard to the fact that the average probability of being very concerned is 17.3%, training reduces it by more than one fourth which is a remarkable decrease.

Just like in the case of wages, it is not obvious why training should only increase job security if it follows a period without frequent training participation. Again, maybe this could be explained by the possibility that these employees have jobs where training is necessary and just a normal regular event or there also exist decreasing marginal benefits of training with respect to perceived job security. As the treatment group effect for workers with successive training periods (*TGfreq*) is positive and almost significant and the treatment group effect for infrequently trained workers (*TGinfreq*) is negative, it is possible that training increases job security with decreasing marginal returns. A Wald-test on equality of the sum of the coefficients of *TGfreq* and *Training08(TGfreq)* and of the sum of the coefficients of *TGinfreq* and *Training08(TGinfreq)* indicates that there is no significant difference in the perceived job security of repeatedly and occasionally trained employees after training participation between 2004 and 2008. The same is true for employees with pre-sample training participation only (*TGpre*).

To analyse training effects in more detail, I concentrate on comparing both treatment groups with the group of non-participants and will not further consider people with pre-sample training only (*TGpre*=1). The following analysis tries to find out if training effects are different for workers of different age and for different training aims.

5.4.2 Differences between age groups

In order to analyse wage and job security effects for workers of different age groups I construct two subsamples. The first one includes (permanent) non-participants and people without successive pre-sample training but training in the observation period, the second one obtains all (permanent) non-participants and workers who participated regularly in the pre-sample periods 1996-2000 and 2000-2004 as well as between 2004 and 2008.

The treatment group variables *TGfreq* and *TGinfreq* are each split up into three different variables according to the three different age groups. The same is done for the two

treatment dummies (*Training08(freq)* and *Training08(infreq)*). The equation which is estimated separately for both subsamples is

$$\begin{aligned} Y_{it} = & \delta_1 TG(Age \leq 39)_i + \delta_2 Training08(Age \leq 39)_{it} + \\ & + \delta_3 TG(Age=40\text{-}49)_i + \delta_4 Training08(Age=40\text{-}49)_{it} + \\ & + \delta_5 TG(Age \geq 50)_i + \delta_6 Training08(Age \geq 50)_{it} + \beta X_{it} + \varepsilon_{it}. \end{aligned} \tag{5.3}$$

Table 5.5 presents the results for the two wage estimations.[15] The treatment group dummies *TG(Age40-49)* and *TG(Age≥50)* indicate that workers aged 40 and older who participate in training courses are those who already earn more. This is true for both samples (1) and (2). With regard to continuously trained workers, young employees also earn more before the last training event (see *TG(Age≤39)* in sample (1)). In contrast, for young workers up to 39 without successive periods of pre-sample training the selection effect indicated by *TG(Age≤39)* in sample (2) is even negative, although not significant. However, in both samples training strongly affects younger workers' wages. In the case of three successive training periods the wage effect of training participation in the observation period is 7.6% and is almost as high as the effect for infrequently trained young workers (7.8%). Employees older than 40 do not benefit from training in form of a wage increase. Although there is no significant wage effect for the group of workers between 40 and 49, compared to the oldest group the treatment coefficients are higher. The positive effect of training on wages decreases with age. One reason for that might be that older and younger workers participate in training for different purposes, which will be analysed in the following section.

Although the coefficients of the treatment group and treatment dummies for both estimations (1) for workers with and (2) for workers without repeated pre-sample training slightly differ in size, Wald tests on equality of the coefficients of both estimations show that they are not significantly different from each other. Thus, for wage effects the results by age groups shed a different light on the results from Table 5.3. For younger employees permanence or continuance of training in terms of successive training periods does not have a different impact on wages compared to nonrecurring or infrequent training periods. Training participation increases wages for younger workers regardless of whether they participate regularly or infrequently. At least for young employees a

[15] The estimations include the same control variables as reported in Table 5.3.

decrease of marginal returns to training suggested by the results without differentiating between age groups (Table 5.3) cannot be confirmed.

Table 5.5: OLS Results – effects on wages for different groups of trained workers

(1) Participation in all periods (1996-2000, 2000-2004 and 2004-2008)	
TG(Age≤39)	0.072*
Training08(Age≤39)	0.076***
TG(Age40-49)	0.119***
Training08(Age40-49)	0.017
TG(Age≥50)	0.126***
Training08(Age≥50)	-0.028
Number of observations	1760
R^2	0.581
(2) Participation between 2004 and 2008 (no regular participation before 2004)	
TG(Age≤39)	-0.010
Training08(Age≤39)	0.078***
TG(Age40-49)	0.113***
Training08(Age40-49)	0.021
TG(Age≥50)	0.110***
Training08(Age≥50)	0.016
Number of observations	1996
R^2	0.547

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Included control variables: Age group dummies, female dummy, years of education, tenure, firm size dummies, East Germany dummy, working hours, job change, occupation and industry dummies as well as a year dummy. Standard errors are robust and clustered.
Source: SOEP waves 2000, 2004 and 2008, own calculations.

Not only wage effects could differ by age but also the impact of training on job security. The results for different age groups as well as for both frequent and occasional training are reported in Table 5.6.[16] The upper part (1) for frequently trained people shows that, regardless of age group, a third period with training participation has no effect on perceived job security. For young workers the coefficient is even negative, although not significant. Moreover, marginal effects show that repeated training for young workers significantly increases the propensity to be somewhat concerned about potential job loss by 1.2 percentage points. In contrast, training of occasionally trained employees seems to

[16] The estimations include the same control variables reported as in Table 5.4.

have strong positive effects especially for older workers (*Training08(Age≥50)* in the lower part (2) of Table 5.6). Their probability to be very or somewhat concerned decreases by 7.6 and 5.2 percentage points, respectively and their probability to be not concerned at all increases by 12.8 percentage points.

Table 5.6: Ordered logistic regression results – effects on job security for different age groups

		Marginal effects		
	(1) Coefficients	(2) Very concerned	(3) Somewhat concerned	(4) Not concerned at all
(1) Participation in all periods (1996-2000, 2000-2004 and 2004-2008)				
TG(Age≤39)	0.365	-0.048	-0.029	0.076
Training08(Age≤39)	-0.323	0.050	0.012***	-0.062
TG(Age40-49)	0.303	-0.041	-0.022	0.063
Training08(Age40-49)	0.042	-0.006	-0.003	0.008
TG(Age≥50)	0.187	-0.026	-0.013	0.039
Training08(Age≥50)	0.208	-0.028	-0.015	0.043
Number of observations	1760			
Pseudo R^2	0.058			
(2) Participation between 2004 and 2008 (no regular participation before 2004)				
TG(Age≤39)	-0.101	0.015	0.005	-0.020
Training08(Age≤39)	0.348	-0.047*	-0.025	0.072
TG(Age40-49)	0.095	-0.014	-0.005	0.019
Training08(Age40-49)	0.344*	-0.047*	-0.025	0.072*
TG(Age≥50)	-0.262	0.041	0.009*	-0.050
Training08(Age≥50)	0.598***	-0.076***	-0.052**	0.128***
Number of observations	1996			
Pseudo R^2	0.057			

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Included control variables: Age group dummies, female dummy, years of education, tenure, firm size dummies, East Germany dummy, job change dummy, occupation and industry dummies as well as a year dummy. Standard errors are robust and clustered.
Source: SOEP waves 2000, 2004 and 2008, own calculations.

However, it seems that older workers without repeated pre-sample training who received training between 2004 and 2008 were worried more before training took place compared to untrained older workers, although only the marginal effect for being somewhat

concerned is weakly significant (see *TG(Age≥50)* for infrequently trained workers in sample (2)). Although the positive training effect on job security for infrequently trained employees is stronger for the oldest age group, there also seem to be weak effects for middle-aged workers. With respect to the youngest age group only one marginal effect is weakly significant. However, the size of the marginal effects is similar for both the young and the middle age group.

Again, comparing repeatedly with occasionally trained employees, the coefficients of the different treatment group dummies for the first group are higher (although not significantly different). That could indicate that frequently trained employees already reduced their worries about job loss by former training events and that there exist decreasing returns with respect to job security. The fact that young workers seem to be even a bit more worried if they get permanent training is surprising. However, note that after the last period of training between 2004 and 2008, overall, they do not feel more unsecure than workers without or with occasional training.[17]

To sum up, the effects of training on wages and on perceived job security differ by age. Younger workers benefit from attending training courses as their wage increases whereas older workers' job security rises. Especially with regard to job security there seem to be decreasing marginal returns to training, as participation has no significant impact any longer for people who already regularly attended training courses in the pre-treatment period. For young workers I do not find evidence for decreasing marginal wage returns to training. Regardless of whether they participated in training before, they always experience a wage increase by an additional period with training.

The results obtained in this section confirm the findings of Pfeifer et al. (2008) who report a wage effect of training especially for younger German workers and an effect on unemployment risk especially for older ones, as well as the findings of Bassanini (2006) whose results also indicate stronger wage effects for younger and job security effects especially for older employees. In contrast, Büchel and Pannenberg (2004) come to the conclusion that it is the group of younger workers who benefits from training with regard to both wages and unemployment risk. However, none of these studies differentiates between infrequent and permanent training participation or takes into account the goals of training.

[17] This can be shown by mutual Wald tests, comparing the sum of the treatment group coefficient and the treatment coefficient for the two groups of workers with permanent or occasional training with workers without training.

5.4.3 Differences between training goals

As there are remarkable differences with respect to training effects between workers of different age, it can be expected that younger and older employees also have different objects when participating in training. Beicht et al. (2006) and Zwick (2011) show that the assessment of both training objectives and effectiveness differs for workers of different age. Thus, in this section, wage and job security effects of courses with different purposes are analysed. As I found no job security effects for frequently trained employees in the previous estimations, I concentrate on comparing infrequently trained workers with non-participants. The treatment group dummy *TGinfreq* and the treatment dummy *Training08(infreq)* are each divided into four different dummy variables according to four different training goals[18]. As there are only three infrequently trained employees stating retraining as aim of a training course I cannot include this category in the estimations.[19] The estimation equation is similar to equation (5.3) but with separate treatment group and treatment dummies according to training goals instead of age groups. Table 5.7 shows the results of the wage estimation.

Table 5.7: OLS Results – effects of different training goals on wages

Participation between 2004 and 2008 (no regular participation before 2004)	
TG(Introduction)	-0.129**
Training08(Introduction)	0.112***
TG(Professional advancement)	0.062*
Training08(Professional advancement)	0.050*
TG(Adjustment to new demands)	0.072***
Training08(Adjustment to new demands)	0.009
TG(Other)	0.048
Training08(Other)	0.005
Number of observations	1994
R^2	0.546

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Included control variables: Age group dummies, female dummy, years of education, tenure, firm size dummies, East Germany dummy, working hours, job change, occupation and industry dummies as well as a year dummy. Standard errors are robust and clustered.
Source: SOEP waves 2000, 2004 and 2008, own calculations.

[18] Note that these different aims are not mutually exclusive.
[19] As only one worker exclusively reported retraining, I drop the corresponding two observations for the following estimations.

First, workers who get trained to qualify for professional advancement or to adjust to new demands in the current job already have significantly higher wages before training participation. The treatment group effect for workers who participate in courses with the aim "introduction to a new job" is negative and significant. Those workers earn on average 12.9% less than employees who did not receive training. Note that this is probably due to the fact that trained people who start a new job are not compared to job starters without training but to all non-participants. After a period with training attendance between 2004 and 2008 this wage gap is closed as the coefficient of *Training08(Introduction to new job)* shows a significant wage increase of 11.2%. Besides, only courses which aim at professional advancement have a positive impact on wages. The wage increase for infrequently trained workers who participated in such a course is 5%. These two aims are significantly more often stated by the youngest age group (see Table 5.2), which is the only age group with positive wage effects (see Table 5.5).

Moreover, Table 5.2 indicated that older employees significantly more often report that the training courses they attended aimed at adjusting to new requirements in their job. Table 5.8 shows how different training goals affect perceived job security of trained workers. Only training with the purpose to adapt to new demands in the current job has a significant positive impact. Participation in such a course reduces the probability to be very concerned and somewhat concerned by 5.6 and 3.0 percentage points, respectively and thus increases the probability to be not concerned at all by 8.6 percentage points.

Workers who receive training when they start a new job seem to be more worried before they participate in training, whereas training participation somewhat reduces these worries. Besides the fact that both effects are not significant, as mentioned above, the lower perceived job security may just reflect that job starters (with training) are compared to all non-participants (who mostly will not just have started a new job).

To sum up, differentiating between the goals of training could provide one possible explanation for different training effects of workers of different age.[20]

[20] Unfortunately, it is not possible to use separate dummy variables for different goals for each age group as in some cases there would be not enough observations to run any estimations.

Table 5.8: Ordered logistic regression results – effects of different training goals on job security

		Marginal effects		
	(1) Coefficients	(2) Very concerned	(3) Somewhat concerned	(4) Not concerned at all
Participation between 2004 and 2008 (no regular participation before 2004)				
TG(Introduction to new job)	-0.651	0.112	0.004	-0.116
Training08(Introduction to new job)	0.227	-0.032	-0.015	0.047
TG(Professional advancement)	-0.050	0.007	0.003	-0.010
Training08(Professional advancement)	0.172	-0.024	-0.011	0.035
TG(Adjustment to new demands)	-0.007	0.001	0.000	-0.001
Training08(Adjustment to new demands)	0.409***	-0.056***	-0.030**	0.086***
TG(Other)	-0.045	0.007	0.007	-0.009
Training08(Other)	0.133	-0.019	-0.019	0.027
Number of observations	1994			
Pseudo R^2	0.057			

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Included control variables: Age group dummies, female dummy, years of education, tenure, firm size dummies, East Germany dummy, job change dummy, occupation and industry dummies as well as a year dummy. Standard errors are robust and clustered.
Source: SOEP waves 2000, 2004 and 2008, own calculations.

5.4.4 Robustness checks

Finally, to check the robustness of the results, especially with respect to the training variable definition, some additional estimation results are presented. First of all, the treatment group of infrequently trained workers (*TGinfreq*) is replaced by the subgroup of workers who did not participate in training at all in the pre-sample period 1996-2004 but between 2004 and 2008.[21] The positive effects of training participation on wages and job security after eight years without any training are even stronger than in the case of the former definition of infrequent training.[22] The estimated average increase in wages of 3.9% (see Table 5.9, part A in the Appendix) is even higher than the estimated effect of 3.1% for infrequently trained workers with the original definition. This is also true for job security effects. The coefficient of irregular training participation is 0.596 (see Table 5.10, part A in the Appendix). Marginal effects indicate that training decreases the

[21] So far, people with training either between 1996 and 2000 or between 2000 and 2004 were also included in the treatment group of infrequently trained employees.
[22] The estimations are run with a subsample of workers without any training and with employees falling under the modified definition of being infrequently trained.

probability to be very or somewhat concerned about job security by 7.9 and 4.7 percentage points, respectively (compared to 4.5 and 3.5 percentage points with the original definition, see Table 5.4). This goes in line with the previous results, which show that there may be a decrease in marginal returns to training.

As some studies found stronger labour market effects of employer-sponsored training compared to self-financed training (e.g. Pfeifer et al., 2008), I also run estimations for employer-financed training only. This leads to a loss of 100 observations as 50 workers participated in training courses without any financial support from the employer. The results are very similar to those obtained by including all training courses, regardless of financing. The coefficients only slightly differ in size (see Table 5.9, part B and Table 5.10, part B in the Appendix). For example, for infrequent training participation employer-financed training increases wages by 2.9% (compared to 3.1% if self-financed training is not excluded, see Table 5.3). With regard to job security, irregular training reduces the likelihood to be very or somewhat concerned by 4.8 and 3.8 percentage points, respectively (compared to 4.5 and 3.5 percentage points, see Table 5.4).

Moreover, besides the dummy variable for training incidence, I additionally include the volume of received training (measured in hours) and the number of attended training courses, respectively. In contrast to the results of Büchel and Pannenberg (2004) who found positive effects of training incidence, volume and number of courses at least on wages, I do not figure out such a clear impact of hours spent for training courses or of the number of training courses. With regard to training volume, the variable is insignificant and for occasional training even negative in the case of job security, whereas training incidence has the same effects as in the estimations without including training duration (see Table 5.12, part A).[23] The overall training effect (of training incidence and duration), however, stays positive and significant for infrequently trained workers up to a maximum duration of about 335 hours. The job security effect varies between 0.381 for one hour and 0.287 for 335 hours of training. For longer durations it gets insignificant.

Regarding wage effects of training, duration has a significant negative impact for both groups of training participants. In contrast, the coefficient for training incidence still is positive and significant in the case of occasional training participation (see Table 5.11, part A). The overall wage effect is significant for employees with at most 154 hours of training, which is true for the majority of infrequent participants (less than 8% of training

[23] The number of observations drops to 3954 because of missing values in the duration variable.

participants report a total duration of more than 154 hours between 2004 and 2008). The wage increase varies between 4.2% and 2.5%. For frequently trained workers the overall effect is insignificant.

Finally, instead of training duration, the number of training courses is included.[24] Note that the distribution of training courses between 2004 and 2008 is obviously different for infrequently and frequently trained workers, although this is not automatically implied by my definition of the two groups (see Figures 5.1 and 5.2 in the Appendix). Employees who already regularly participated in training also attend more courses in the following period with training than employees who occasionally participated in courses before. That means that for the group of repeatedly trained people training not only takes place more regularly but also more often. This supports the idea that for employees with certain jobs or positions, training could be something like a essential routine job activity and does not necessarily result in higher wages or job security.

With regard to wage effects, all training variables – the training incidence dummies as well as the variables measuring the number of courses – are positive but insignificant both for infrequently and regularly trained workers (see Table 5.11, part B). However, the overall effect of training incidence and the number of training courses gets significant for a certain number of courses in the case of infrequent participation. For three to six courses the estimated wage effects are significant and lie between 3.0% and 4.1%. This means that more than 50% of all occasional training participants in my sample realize a significant wage increase compared to non-participants (see Figure 5.2 in the Appendix for the distribution of the number of courses).

The effects of training on job security somewhat change compared to the specification without including the number of courses. For infrequently trained workers training incidence significantly increases job security and the number of courses is positive but insignificant (see Table 5.12, part B in the Appendix). The overall training effect for this group of participants increases with the number of courses but gets insignificant for employees participating in more than 17 courses. However, only three infrequently trained workers report more than 17 courses between 2004 and 2008 (see Figure 5.2 in the Appendix). In contrast, the coefficient of the dummy for training incidence is negative and significant in the case of regularly trained workers and the number of courses has a significant positive effect on job security (see Table 5.12, part B in the Appendix). Again,

[24] The number of observations drops to 4136 because of missing values in the variable measuring the number of courses.

the overall effect of training shows that only for certain numbers of courses training has a significant impact on job security. For repeatedly trained people who only participate in one course the effect is negative and significant on the 10% level, but less than 9% of all participants only attended one course (compared to more than 26% of infrequently trained workers, see Figures 5.1 and 5.2 in the Appendix). In contrast, for regularly trained employees with more than 11 courses the effect on job security is positive and significant. This means that for this sample about 6.5% of the group of employees with regular training are positively affected. All in all, taking into account different measures of training mainly confirms the previous results obtained in this analysis as it is mainly the group of infrequent participants who benefit from training.

5.5 Conclusion

In this chapter I analyse the impact of training participation on workers' wages and perceived job security. I use SOEP data and apply difference-in-differences to control for time-invariant unobservable heterogeneity between the groups of training participants and non-participants. The results of the empirical analysis indicate that there are also differences with regard to the continuity of training participation. At least for job security there seem to be decreasing marginal returns to training.

Moreover, there are considerable differences between older and younger workers. Especially young employees benefit from training in form of a wage increase whereas older employees experience a significant rise in job security. For older workers such a positive effect is only realized after a period without regular training participation. People who already frequently participated in training before the observation period report no significant rise in their perceived job security after the last training event. In contrast, the positive effect of one additional training period on wages of young employees is still high after repeated pre-sample training.

Different effects can be explained by different aims of training courses older and younger workers participate in. Although for all workers, regardless of age, the most frequent purpose of training is adjusting skills to new demands in the current job, the share of courses with this aim significantly increases with age. This can be due to the fact that older workers' skills they acquired in their initial training might have become obsolete over time. Perhaps their productivity is even below their wage which necessitates

continuing training. Qualification for professional advancement and introduction to a new job are important reasons to participate in training especially for workers at the beginning of their career.

Therefore, I additionally estimate wage and job security effects of courses with different purposes. I come to the conclusion that training to adapt to new requirements of the current job has a positive impact on job security but not on wages, whereas training in order to qualify for career advancement or when starting a new job only affects wages. Thus, differences in training effects for people of different ages might be explained by different training purposes. All in all, taking into account the objectives of training participants but also their training history seems to be important for the evaluation of lifelong training.

5.6 Appendix

Table 5.9: OLS Results – wage effects of irregular training (different definition) and of employer-financed training

A) Participation between 2004 and 2008 (no participation between 1996 and2004)	
TGinfreq	0.068***
Training 08 (infreq)	0.039*
Number of observations	1596
R^2	0.556
B) (Partly) employer-financed training	
(1) Participation in all periods (1996-2000, 2000-2004 and 2004-2008)	
TGfreq	0.068***
Training 08 (freq)	0.012
(2) Participation between 2004 and 2008 (no regular participation before 2004)	
TGinfreq	0.077***
Training 08 (infreq)	0.029**
Number of observations	3842
R^2	0.571

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Included control variables: Age group dummies, female dummy, years of education, tenure, firm size dummies, East Germany dummy, working hours, job change dummy, occupation and industry dummies as well as a year dummy. Part B: additionally TGpre included. Standard errors are robust and clustered.
Source: SOEP waves 2000, 2004 and 2008, own calculations.

Table 5.10: Ordered logistic regression results – wage effects of irregular training (different definition) and of employer-financed training

A) Participation between 2004 and 2008 (no participation between 1996 and2004)	
TGinfreq	0.011
Training 08 (infreq)	0.596***
Number of observations	1596
Pseudo R^2	0.060
B) (Partly) employer-financed training	
(1) Participation in all periods (1996-2000, 2000-2004 and 2004-2008)	
TGfreq	0.262*
Training 08 (freq)	-0.085
(2) Participation between 2004 and 2008 (no regular participation before 2004)	
TGinfreq	-0.173
Training 08 (infreq)	0.391**
Number of observations	3842
Pseudo R^2	0.052

***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Included control variables: Age group dummies, female dummy, years of education, tenure, firm size dummies, East Germany dummy, job change dummy, occupation and industry dummies as well as a year dummy. Part B: additionally TGpre included. Standard errors are robust and clustered.
Source: SOEP waves 2000, 2004 and 2008, own calculations.

Table 5.11: OLS results – wage effects of training duration and number of training courses

A) Training duration (hours*10^{-3})	
(1) Participation in all periods (1996-2000, 2000-2004 and 2004-2008)	
TGfreq	0.080***
Training 08 (freq)	0.025
Training duration 08 (TGfreq)	-0.070*
(2) Participation between 2004 and 2008 (no regular participation before 2004)	
TGinfreq	0.060***
Training 08 (infreq)	0.042***
Training duration 08 (TGinfreq)	-0.113***
Number of observations	3954
R^2	0.570
B) Number of training courses	
(1) Participation in all periods (1996-2000, 2000-2004 and 2004-2008)	
TGfreq	0.072***
Training 08 (freq)	0.001
Number of courses 08 (TGfreq)	0.003
(2) Participation between 2004 and 2008 (no regular participation before 2004)	
TGinfreq	0051***
Training 08 (infreq)	0.018
Number of courses 08 (TGinfreq)	0.004
Number of observations	4136
R^2	0.568

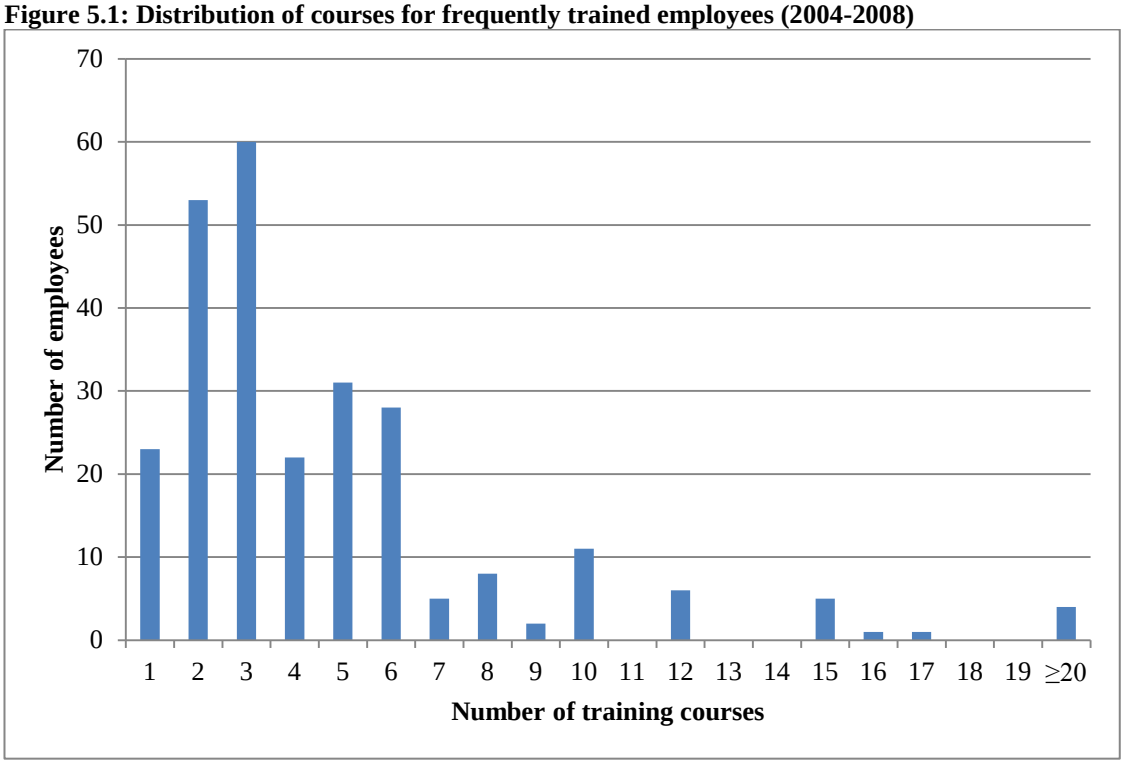
***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Included control variables: Age group dummies, female dummy, years of education, tenure, firm size dummies, East Germany dummy, working hours, job change dummy, TGpre, occupation and industry dummies as well as a year dummy. Standard errors are robust and clustered.
Source: SOEP waves 2000, 2004 and 2008, own calculations.

Table 5.12: Ordered logistic regression results – job security effects of training duration and number of training courses

A) Training duration (hours*10^{-3})	
(1) Participation in all periods (1996-2000, 2000-2004 and 2004-2008)	
TGfreq	0.278**
Training 08 (freq)	-0.109
Training duration 08 (TGfreq)	0.113
(2) Participation between 2004 and 2008 (no regular participation before 2004)	
TGinfreq	-0.089
Training 08 (infreq)	0.382***
Training duration 08 (TGinfreq)	-0.282
Number of observations	3954
Pseudo R^2	0.053
B) Number of training courses	
(1) Participation in all periods (1996-2000, 2000-2004 and 2004-2008)	
TGfreq	0.223*
Training 08 (freq)	-0.412*
Number of courses 08 (TGfreq)	0.076**
(2) Participation between 2004 and 2008 (no regular participation before 2004)	
TGinfreq	-0.105
Training 08 (infreq)	0.277*
Number of courses 08 (TGinfreq)	0.028
Number of observations	4136
Pseudo R^2	0.054

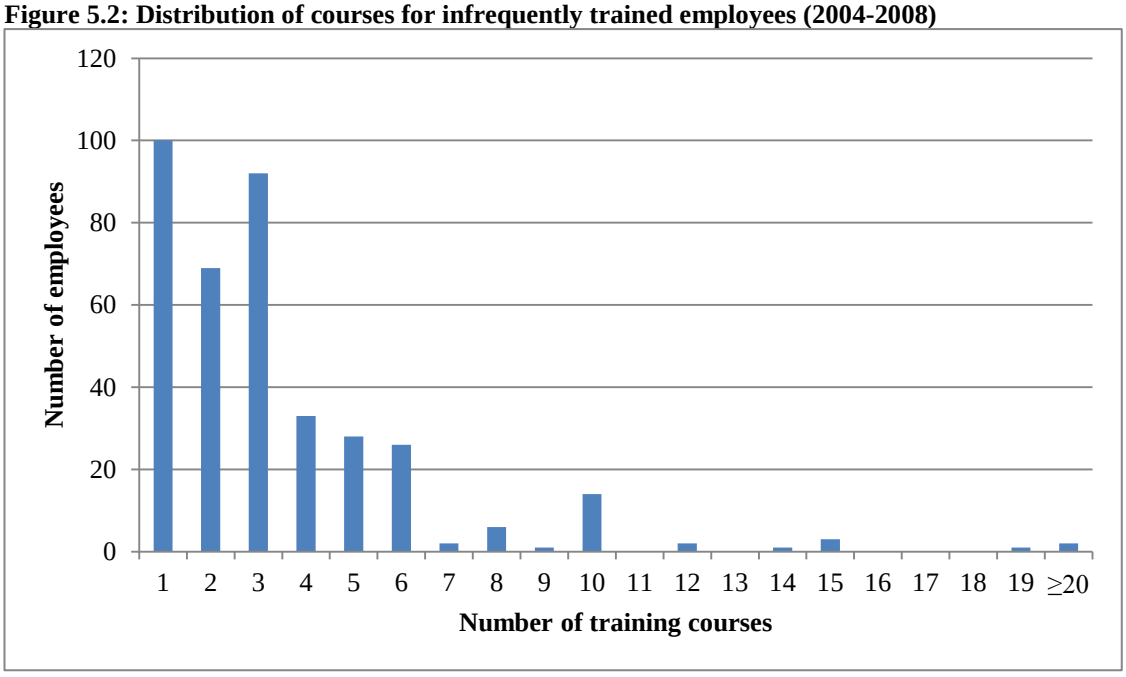
***/**/* indicates statistical significance at the 1%, 5% and 10% level. Notes: Included control variables: Age group dummies, female dummy, years of education, tenure, firm size dummies, East Germany dummy, job change dummy, TGpre, occupation and industry dummies as well as a year dummy. Standard errors are robust and clustered.
Source: SOEP waves 2000, 2004 and 2008, own calculations.

Figure 5.1: Distribution of courses for frequently trained employees (2004-2008)



Number of observations: 260
Source: SOEP waves 2000, 2004 and 2008, own calculations.

Figure 5.2: Distribution of courses for infrequently trained employees (2004-2008)



Number of observations: 380
Source: SOEP waves 2000, 2004 and 2008, own calculations.

6 Final remarks

This dissertation provides some further insights in how employee participation and involvement can affect both employees and employers, focusing on works councils and two HRM measures. In this context it is important to consider the conditions under which works councils or specific work practices are implemented. To take into account a possible selection bias, two evaluation methods are applied and combined. Matching allows considering selection on observable characteristics, whereas difference-in-differences eliminates selection bias due to unobservable time-invariant differences between firms or people which are affected by a measure and those who are not.

Chapter 2 shows that works councils are introduced in firms where employees are afraid of losing their job. Works councils help to reduce these worries. Their effects on other individual and firm level outcomes like wages, fluctuation or overtime work are insignificant. However, wages are already higher and overtime hours are lower in firms before a works council is introduced. This supports the idea that rent protection is the major motive of workers for electing a works council.

Chapters 3 and 4 deal with the effects of profit sharing on productivity and training intensity in German establishments. The results indicate that selection again plays and important role. It is especially the highly productive firms with specific observable and unobservable characteristics that decide to implement profit sharing. Nevertheless, profit sharing additionally increases productivity. It also has a positive impact on training intensity (which might, besides increasing worker effort, be an additional channel for productivity gains), but the share of workers covered by profit sharing is crucial for the effects. Only firms where the majority of employees benefits from profit sharing, realize such an increase in the share of trained workers.

Chapter 5 analyses the impact of training participation on workers' wages and job security. The analysis shows that there are significant differences between younger and older participants with respect to training goals and effects. Younger workers benefit from higher wages and older workers from increasing job security. Moreover, the training history of a person also plays a role for the impact of any further training activities.

In summary, the four studies show that both works councils as a traditional institution of industrial relations and profit sharing and training as two measures of modern human resource management can positively influence economic outcomes of employees as well as firms. However, controlling for selection and unobservable heterogeneity is important for causal empirical analyses, as the introduction of such measures seems to crucially depend on the characteristics of individuals and firms. These specific characteristics can

also be decisive for the effectiveness of organizational change. The results of Chapters 2, 3 and 4 show that neglecting unobservable differences between treatment and control groups leads to an overestimation of the positive effects of works councils on wages and negative effects on employee turnover as well as to an overestimation of the positive impact of profit sharing on training and productivity.

Of course the studies in this dissertation also have limitations. As shown in Chapter 4, the share of employees participating in profits is crucial for the existence of positive effects on training intensity. This will probably also be true for its impact on productivity. However, due to data limitations, it was not possible to include information on profit sharing intensity in the analysis of productivity effects in Chapter 3.

As already mentioned in the introduction, there is some evidence that works councils could have an impact on the probability to implement HRM practices (which is considered in Chapters 3 and 4) but also on their effectiveness, which is not explicitly analysed in this thesis.

Another aspect which must be noted is that the estimated effects are average treatment effects on the treated and it is not easy to answer the question if non-treated people or establishments would be affected by the considered treatment in a similar way.

Moreover, there are also firms where specific HRM practices or works councils are abolished and employees who break off training. The reasons for and consequences of this behaviour have hardly been analysed so far, which offers scope for further research.

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Eidesstattliche Versicherung

Hiermit versichere ich, dass ich diese Dissertation selbstständig verfasst habe. Bei der Erstellung der Arbeit habe ich mich ausschließlich der angegebenen Hilfsmittel bedient. Die Dissertation ist nicht bereits Gegenstand eines erfolgreich abgeschlossenen Promotions- oder sonstigen Prüfungsverfahrens gewesen.

Dortmund, 14. November 2012