Performance Management, Broadband Technology, and Restructuring – Econometric Analyses using Linked-Employer-Employee Data

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Chapter 1

Introduction

At the latest since the seminal work of Coase (1937), scholars have thought about the question what constitutes a firm, how it is organized, and why it even exists. Prior to Coase, economists' predominant opinion was that the economic system was solely coordinated by the price mechanism. In such a world, there would be no organization and every individual would work independently. This is at odds with the observation that individuals do in fact organize into firms. To say it in the words of this great pioneer, "having regard to the fact that if production is regulated by price movements, production could be carried out without any organisation at all, well might we ask, why is there any organisation" (Coase, 1937, p. 388)? He argues that the primary reason why the allocation of resources via hierarchies or the entrepreneur is, in some instances, preferred over coordination via the market mechanism is that using this price mechanism involves significant costs that can be saved by relying on internal organization. This key insight kick-started various areas of economic and business research on the boundaries of and the coordination and hierarchies within firms, among others research in organizational and managerial economics, finance, human resources, accounting, and management. The different chapters of this thesis contribute to different streams of this literature but have important aspects in common: all studies analyze individual workers[1] and different worker groups within organizations and all use large, representative linked-employer-employee panel data sets and panel data methods, particularly fixed effects regressions. In the following, these unifying elements are outlined in more detail.

First, all chapters focus on workers and their roles in the organizations they work in. Chapter 2 examines the German case and analyzes if and how a performance management and evaluation process (PMEP) as operationalized by the joint presence of an appraisal interview (AI) and a written target agreement (TA) affects[2] employee engagement, which

[1]Note that the terms "workers" and "employees" are used synonymously throughout this thesis.

[2]Throughout this thesis, words such as "impact" or "effect", that imply a causal relationship between independent and dependent variables, are used. This is done for the purpose of readability. While doing our utmost to move as closely as possible to causality, we recognize that the methods we use do not always make causal interpretation possible.

is an employee attitude commonly used as a proxy for individual effort (Schaufeli and Bakker, 2004). We furthermore subdivide the PMEP into its two components to evaluate which one drives the overall effect or whether they both add value, and analyze behavioral channels, in particular procedural fairness and goal clarity, that might mediate a direct effect of a PMEP on employee engagement. Chapter 3 examines how the introduction of broadband internet in Brazil in the early 2000s influenced overall firm-level employment and within-firm employment structures. It also evaluates whether firms benefit from these changes. We subdivide workers into occupational and educational layers according to their job title and highest educational degree achieved and evaluate the impact of broadband internet on the relative prevalence and absolute number of workers in each layer. Furthermore, we examine the technology's impact on firms' overall wage bill and a potential mediation of a direct broadband internet effect on firm survival via these changes in employment structures. Chapter 4 focuses on Germany and analyzes the way in which two specific restructuring decisions influence labor structures of the restructuring establishment. I analyze the impacts of a relocation decision that moves parts of an establishment to other units within the same firm and of a separation measure that takes parts of the establishment outside the boundaries of the firm to be continued as a stand-alone firm. To this end, I again subdivide employees into educational and occupational layers in a similar fashion as in chapter 3 and analyze the impact of these restructuring measures on total employment, the relative prevalence and absolute number of the respective worker groups, and average wages paid. Hence, all chapters examine how individual workers and employment structures are influenced by firm-level measures or exogenous shocks affecting only a subset of firms.

Second, the empirical analyses in all chapters are based on large and representative linked-employer-employee panel data sets. We always employ questionnaire data on establishments, which are matched with information on employees working within these establishments. In chapter 2, this employee-level information is from an employee survey while in chapters 3 and 4, it is from administrative sources. Chapter 2 makes use of four waves from the time frame between 2012 and 2018 of the Linked Personnel Panel (LPP), a new German linked employer-employee panel data set that focuses on human resource management practices. These data are representative for all German private-sector establishments with more than 50 employees who pay mandatory social security contributions ("typical" employees). The establishment survey encompasses between 769 and 1,219 establishments per wave randomly drawn from the IAB Establishment Panel, which I also use in chapter 4 of this thesis. The establishment survey interviews managers on general firm characteristics and the HRM measures they use. On the employee-side, 6,500 to 7,500 employees per wave are randomly drawn from these establishments and interviewed on their attitudes towards their employer, their job, and their personal characteristics. In total, our least restrictive sample contains 16,506 employee-year observations. In chapter 3, we employ Brazilian data from 1996 to 2005, which are based on the Annual Social Information Report (RAIS), a mandatory questionnaire sent annually to the universe of Brazilian employers. These are required to annually provide an array of information on establishments and all employees working within them. For the purpose of our identification strategy, we match RAIS with data on all Brazilian main distribution frames (MDFs), which distribute telephone and internet signals, from the Brazilian telecommunications agency ANATEL. We enrich the matched sample with further data from administrative sources, restrict our sample to firms located in state capitals, and analyze firms with more than 10 employees. We follow firms over the above-mentioned time frame, such that our final sample contains 1,646,772 firm-year observations and detailed information on all formal Brazilian employers located in state capitals and employees working for them. Chapter 4 finally uses the Linked-Employer-Employee data of the German Institute for Employment Research (LIAB) in the time frame 2009 to 2017. This data set is based on the IAB Establishment Panel, a large and representative survey on establishments conducted annually since 1993. The LIAB combines this questionnaire data with administrative data sources on establishments from the Establishment History Panel (BHP) and all workers working within these establishments from the Integrated Employment Biographies (IEB) on June 30th of a given year. I focus on establishments with more than 50 typical workers, hence my main sample contains 19,271 firm-year observations. In sum, all chapters use large and representative linked-employer-employee data on establishments and workers working within them.

Third, the panel data described above permits the usage of panel data methods and, in particular, fixed effects regressions on the individual and firm level to take time-constant individual heterogeneity and potential omitted variable bias into account. In all three chapters, we combine these fixed effect regressions with further strategies to move as closely as possible to causal effects. In chapter 2, we employ different fixed effects specifications, in particular a specification using firm and time fixed effects, a specification using an interaction between firm and time fixed effects, and an individual (worker) fixed effects specification to evaluate the relationship between the presence of a performance management and evaluation process consisting of appraisal interviews and target agreements and employee engagement on the worker level. We furthermore exploit the data's rich information to control for an array of potentially time-varying characteristics on both the establishment and the individual worker level. In chapter 3, we combine the usage of firm and state-year fixed effects with a quasi-experimental research design and a difference-in-differences setting and use the exact geodistance between MDFs and single firms as a measure of connectivity to evaluate the causal impact of broadband internet on firms' occupational and educational employment structures. We take advantage of information on the geolocations of all Brazilian MDFs and firms and on the year of conversion of fixed-line telephone infrastructure for broadband internet provision for all state capitals. The conversion of this telephone infrastructure caused a natural experiment which allows us to employ an identification strategy comparable to e.g. Falck, Gold and Heblich (2014) and Hjort and Poulsen (2019). In particular, we exploit ADSL technology's limited signal range, dividing the universe of Brazilian firms located in the state capitals into firms treated and untreated with broadband internet. In chapter 4, I finally use firm fixed effects regressions together with a large set of contemporary and lagged controls to evaluate the influence of restructuring measures on the firm level on the employment structures of these firms. The baseline specification only combines contemporary controls with firms fixed effects to take into account both time-constant, firm-specific heterogeneity and characteristics that might vary over time. I then successively add lagged control variables in order to make firms more comparable before the restructuring measure is conducted and to evaluate robustness of results. All things considered, all chapters use fixed effects regressions combined with further econometric strategies to get as close to causality as possible. Even though they might differ in the degree in which causal interpretation is possible, we do our utmost to move as closely as possible to causality in all of them, given the possibilities the different data sets offer. In each paper, we furthermore conduct an array of robustness checks to verify the consistency of our results.

After having discussed the similarities between the papers, in what follows, I am going to give a short overview over the main research gap the different chapters exploit and their respective findings.

Chapter 2 aims at providing generalizable evidence on the way in which a PMEP influences employee engagement *on average* using large and representative linked-employer-employee data from a large advanced economy. This is important because prior literature in the fields of management accounting, organizational economics, and applied psychology has found mixed results with respect to the impact of target setting on employee-level effort and performance. While especially proponents of classical goal setting theory (e.g. Locke and Latham, 1991, 2002) emphasize on a positive impact of goals or targets on employee-level effort and performance, a more recent stream of literature has emerged that points out unintended side effects (Barsky, 2008; Ordóñez et al., 2009; Eyring and Narayanan, 2018; Holzacker et al., 2019), such that targets ultimately might do more harm than good. Furthermore, there is also more and more evidence of companies abandoning individual targets altogether. Prior literature has analyzed the impact of target setting on employee effort using cross-sectional data from one ore only a small number of firms (e.g. Sholihin et al., 2011; Sholihin and Pike, 2013), or via conducting lab or field experiments (e.g. Liu and Zhang, 2015; Li and Sandino, 2018). The prior type of study is prone to selection and omitted variable bias and results can hence not be interpreted causally, while the latter type solves these problems and exhibits a higher degree of internal validity. However, external validity and hence the possibility to generalize from the lab or single firm setting to real firms or firms in other business environments is a problem in all of these studies. Given recent doubts of the effectiveness of targets, evidence on the average effect of target setting from a large representative sample spanning multiple industries is important. To provide this evidence is the main research goal we aim to achieve in this paper. In addition to generalizability, the panel structure and size of the data further enables us to move closer to causality, as compared to prior studies using archival data. Our findings show a robust and significant effect of the presence of a PMEP consisting of an AI and a written TA on employee engagement. To evaluate potential effect heterogeneity, we run regressions separately for different firm sizes, industries, and employee groups and find little to no effect heterogeneity with respect to the direction and statistical significance of effects. The impact of the PMEP on engagement seems thus to be positive regardless of contextual factors. When subdividing the PMEP into its components, results show that both the AI and the TA contribute to the overall positive PMEP effect. We also analyze behavioral channels which might mediate this direct effect and show that it is partially mediated by procedural fairness and goal clarity. To conclude, chapter 2 evaluates the average effectiveness of target setting processes in a large and advanced economy and finds robust evidence that a PMEP, consisting of AIs and TAs, has a positive impact on employee-level engagement that is independent of contextual factors. These findings are relevant for practitioners who think about introducing or abandoning PMEPs or who already use AIs and think about additionally introducing TAs.

The research goal of chapter 3 is to provide causal evidence how broadband internet introduction in Brazil in the early 2000s has influenced firms' internal employment structures. This is important, since prior evidence shows broadband to result in skill biased technological change (SBTC) in advanced economies (Akerman, Gaarder and Mogstad, 2015) and a decrease in employment inequality in developing countries (Hjort and Poulsen, 2019) but there is only few evidence on broadband's labor market implications in an emerging market context. In addition, while prior literature has largely analyzed employment and employment structures in geographical areas, studies that specifically focus on firms and on the ways in which the technology influences overall employment, internal labor structures and hierarchies, and the extent to which firms benefit from these changes, are scarce. Since we find partly opposing effects as compared to Tian (2019), who examines the effect of a broadband amelioration program in Brazil between 2012 and 2014, we also show that even the effects of similar technologies are not homogeneous but might be heavily context-specific and e.g. depend on the technology's stage of maturity. In contrast to prior literature that has largely identified treatment with broadband internet via the geodistance between MDFs and municipality centroids (e.g. Falck, Gold and Heblich, 2014), our data allows us to use the exact geodistance between MDFs and firms to evaluate treatment, which is also a methodological step forward. We provide causal evidence that broadband internet access significantly reshapes within-firm employment structures and changes both the occupational and educational composition of firms.

In particular, we find polarization both in terms of the occupational and educational labor structures, such that layers at the very top and bottom of the occupational and educational pyramids expand in terms of shares, while intermediate layers decrease their employment shares. The arguably most interesting result is that the management layer is the only occupational layer that expands in terms of shares, a finding that diverges from previous worldwide evidence. We evaluate different theoretical explanations for our findings, among these SBTC and Autor, Levy and Murnane (2003)'s routinization hypothesis, and find that our results are most consistent with predictions of "management by exception" (Garicano, 2000; Bloom et al., 2014). In particular, results indicate that, in the initial stage, broadband was primarily used as a communications rather than an information technology. We further show that firms size decreases as a result of broadband internet by an average 7% which is driven by losses in employment in all occupational and educational layers. The rearrangements in the labor structure thus happen because some layers lose more than others. We furthermore show that firms internally promote high-skilled workers to management positions rather than hiring them from outside the firm, indicating that firm-specific human capital is important for dealing with the new technology effectively. We also examine whether firms benefit from these rearrangements and find that firms are able to decrease their overall wage bill and that rearrangements in the labor structure mediate a direct positive effect of broadband on firm survival. All things considered, chapter 3 shows that broadband internet has negative effects on overall employment and leads to significant rearrangements in the labor structure of firms and, in particular, an increase in the relative importance of managers. Firms benefit from these changes through a lower wage bill and a higher short-term probability to survive. Our findings are important for policy-makers, as they show negative employment effects of digitization, changes in the demand for different labor types, and an increase in firms' probability to survive in the market. When comparing results to Tian (2019), they also show that even the effects of similar technologies might depend on the specific context. When new technologies arise, policy makers hence need to keep in mind that effects of new technologies are not unambiguous, always depend on the specific context, and that technologies might lead to an increase in competitiveness but losses in employment on the firm level, especially of certain labor types.

Finally, chapter 4 evaluates the impact of restructuring measures that shrink the boundaries of the firm or establishment on the within-firm labor structure. There is an array of research on the reasons to restructure, stock market reactions to, and performance effects of restructuring (Eckbo and Thorburn, 2013). There is also adjacent literature on the impact of outsourcing (e.g. Dube and Kaplan, 2010; Goldschmidt and Schmieder, 2017) and private equity (PE) buyouts (e.g. Olsson and Tåg, 2017; Antoni, Maug and Obernberger, 2019) on within-firm employment structures and wages. The provision of the missing equivalent evidence of the impact of restructuring on within-firm employment structures is the main research goal of this paper. My findings show that employment in establishments conducting a relocation or separation restructuring measure decreases by a substantial 30.2%, ceteris paribus. Restructuring also induces significant changes in firms' labor structures and, specifically, a skill bias. While all educational, occupational, and task complexity groups lose in terms of the overall number of workers, some lose more than others, leading to significant shifts in the relative prevalence of different worker types. Results show that high-educated workers with a university degree, workers working in relatively high positions in the occupational hierarchy, and workers conducting rather complex tasks gain in terms of shares and thus become relatively more important within firms. Managers, whose share increases by more than 27%, experience the most pronounced positive effect. In contrast, particularly the shares of workers with occupational education, workers in relatively low positions in the occupational hierarchy, and workers conducting tasks characterized by a low complexity, decrease in terms of shares. Unlike prior literature on outsourcing and PE buyouts, I do not find significant wage effects. This skill bias towards higher educational, occupational, and task complexity layers is in line with SBTC (e.g. Katz and Autor, 1999) and suggests that, similar to PE buyouts (Agrawal and Tambe, 2016), restructuring measures might be a vehicle to adjust the labor force to new technological developments. The strong increase in the share of managers is in line with "management by exception" (Garicano, 2000) and suggests that in the post-restructuring phase, workers face more nonroutine problems or exceptions that need to be solved by managers. I propose that the documented rearrangements in the labor structure might be one channel via which firms realize the performance gains that previous literature documents. The implications of this study are important for policy makers and employment agencies, as they inform about which types of jobs are most endangered by restructuring decisions.

Chapter 2

Appraisal Interviews, Target Agreements, and Employee Engagement - New Evidence using Representative Data*

2.1 Introduction

Performance management systems constitute an important management practice in modern organizations (Otley, 1999; Franco-Santos, Lucianetti and Bourne, 2012). An essential ingredient of this framework is the performance management and evaluation process (PMEP), which comprises formal target setting between supervisors and subordinates and performance evaluation (Ferreira and Otley, 2009), typically operated via annual appraisal interviews (AI). These AIs can be conceptualized as a "conversation about performance" (Gordon and Stewart, 2009, p. 473) and might function by "providing feedback to employees, counseling and developing employees, and conveying and discussing compensation, job status, or disciplinary decisions" (Cederblom, 1982, p. 219). During these AIs, supervisors commonly make use of written target agreements (TA), where formalized targets are often set at the beginning of a fiscal year and reviewed in the subsequent year.

According to representative evidence from private establishments in Germany (data used in this paper), the PMEP is of high practical importance. In 2018, 85% of establishments reported using appraisal interviews while 80% reported employing written TAs. In addition, 64% of all employees working in these establishments reported being covered by an AI and 42% reported having both an AI and a written TA. Nevertheless, recently a lively debate about the outcomes and effectiveness of performance reviews and target setting has emerged in many organizations. For instance in 2015, Volkmar Denner, CEO of the German company Robert Bosch, publicly announced to abandon compensation plans

*This chapter is based on Kampkötter and Maier (2020).

based on the achievement of individual targets.[1] Companies such as Commerzbank, SAP, and Infineon have undergone similar changes.[2]

Target setting aims at increasing the organization's productivity by aligning employee incentives with organizational goals. The direct relationship between target setting and productivity is thus likely to be mediated through employee decision-making and effort (Bender et al., 2018). However, despite decades of research in management accounting, applied psychology, and organizational economics, prior literature has provided mixed evidence with respect to the impact of feedback and target setting characteristics on employee effort and employee performance[3] (e.g. Podsakoff and Farh, 1989; Locke and Latham, 1991; Fisher, Frederickson and Peffer, 2000; Christ, Sedatole and Towry, 2012) and on employee perceptions such as goal clarity and procedural fairness (e.g. Lau and Buckland, 2001; Marginson and Ogden, 2005; Derfuss, 2009; Hartmann and Slapničar, 2009; Voußem, Kramer and Schäffer, 2016).[4] Classical goal-setting theory (e.g. Locke and Latham, 1991, 2002) expects specific, challenging targets[5] to boost performance and the literature examining participation in target setting largely finds a positive impact on performance as well (e.g. Fisher, Frederickson and Peffer, 2000; Wentzel, 2002; Sholihin et al., 2011). Other studies, however, argue that challenging targets do not boost performance of all individuals (Eyring and Narayanan, 2018), that target setting is associated with costs often ignored by prior literature (Holzacker et al., 2019), and that these negative side effects often outweigh its benefits (Barsky, 2008; Ordóñez et al., 2009).

It is important to note that prior studies using archival data are typically based on cross-sectional data sets, often from single firms, with only a small number of observations. This makes causal and generalizable interpretation difficult. We address this gap by using a new, matched employer-employee data set, representative for all German private sector establishments with more than 50 employees and their respective workforce (Kampkötter et al., 2016). The first goal of this study is to provide generalizable and (as compared to previous studies using archival data) more causal evidence on the *average* effects of a PMEP, as conceptualized by the joint presence of an AI and written TA, on work engagement, our proxy for individual effort. This is possible due to two main characteristics of our data: First, the data set's representativeness and large number of observations enables us to make

[1]See http://www.manager-magazin.de/unternehmen/karriere/mitarbeitermotivation-schafft-die-boni-ab-a-1055113.html (November 25, 2021).

[2]See, for instance, https://www.reuters.com/article/us-sap-appraisals/europes-biggest-software-maker-sap-ditches-annual-reviews-idUSKCN10N0R0 (November 25, 2021) or https://www.handelsblatt.com/today/finance/variable-compensation-commerzbank-eliminates-most-individual-bonuses-in-revamp/23583676.html (November 25, 2021).

[3]In this study, we regard employee performance as a consequence of employee effort.

[4]The literature uses different proxies for effort such as goal commitment (Sholihin et al., 2011) or motivation (Locke and Latham, 2002). For the purpose of readability, we subsume these constructs under the term "effort".

[5]Note that goal setting literature uses the term goal instead of target. For the sake of readability, we use these two terms interchangeably.

generalizable statements and second, we are able to exploit the longitudinal dimension of our data and to use panel data methods. Second, we want to focus on effect heterogeneity by examining potential channels mediating the relationship between target setting and engagement, in particular procedural fairness and goal clarity. In line with Lind and Tyler (1988), we define procedural fairness as referring to the perception of fairness with respect to the process via which outcomes are determined. The evidence with respect to the effect of (participation in) target setting on procedural fairness is again heterogeneous. Some studies (e.g. Sholihin et al., 2011) find a positive impact of participation in budget setting and a sense of procedural fairness. Voußem, Kramer and Schäffer (2016), for instance, argue that target characteristics such as subjectivity of performance measures are important, while Ordóñez et al. (2009) argue that targets might even lead to a feeling of unfairness. We consider goal clarity as our second potential mediator. In line with Sholihin and Pike (2013), we define goal clarity as a clearer understanding of organizational members about their goals. In a meta-analysis, Derfuss (2009) finds a positive relationship between participation in budget setting and goal clarity. In a recent lab experiment, Anderson and Stritch (2015) in turn find a positive relationship between goal clarity and performance. Thus, the second goal of our analysis is to examine whether the direct relationship between our PMEP and work engagement is mediated by procedural fairness and goal clarity.

Due to the panel structure of our data set, we are able to include various fixed effects in our model to account for omitted variable bias problems. As information on employee effort is difficult to collect particularly in representative data sets, we employ the concept of work engagement (Kahn, 1990; Bakker, 2017), which is commonly applied in management and organizational psychology and has recently also been used in the management accounting literature (Li and Sandino, 2018). This effort proxy has been empirically validated in various countries (Schaufeli and Bakker, 2003) and its implementation in a representative linked employer-employee sample allows us to shed some light on the nexus between work effort and performance management practices. We are particularly interested in differentiating between the effects of a sole presence of an AI and the *additional* presence of a written TA in order to see if the effect of the PMEP on work engagement is driven entirely by the AI or if formalization via a written TA is of additional value for firms.

Our results show a positive and statistically significant effect of the presence of a PMEP on work engagement. This effect is robust across various specifications. We further find first evidence that both AIs and TAs positively affect engagement. While both the effects of AIs and TAs on work engagement are positive and statistically significant in all of our pooled OLS regressions, only the effect of AIs remain significant in our individual fixed effects specification. In this context, we explicitly discuss a problem commonly associated with the use of fixed effects in management practices research, namely that there is "not enough real time series variation (given measurement error) to identify any significant relationships" (Bender et al., 2018, p. 381). In our case, this means that only very few individuals switch from having no TA to having a TA without jointly switching from having no AI to having an AI. Hence, these HR measures seem to be applied quite simultaneously in firms. In our mediation analysis, we find that both procedural fairness and goal clarity *partially* mediate the direct effect of the PMEP on engagement.

We contribute to the literature in several ways. Our main contribution relates to the use of a representative, matched employer-employee data set aiming to provide more generalizable results on the use and effects of performance management. As outlined above, most studies examining the effect of target setting on individual effort and the channels mediating this relationship rely on lab or field experiments (e.g. Liu and Zhang, 2015; Li and Sandino, 2018; Holzacker et al., 2019), cross-sectional studies in single firms (e.g. Sholihin et al., 2011), or small non-random samples of firms (e.g. Sholihin and Pike, 2013). While experiments are the best way to establish internal validity and causality, they generally encounter problems related to external validity. Cross-sectional single firm case studies provide in-depth insights into the studied organization but are prone to issues regarding causality and generalizability. This has been emphasized by Derfuss (2009) who states that "many studies use small samples, and their conflicting findings might be due to statistical artifacts, such as sampling error" (p. 203). In the study most related to our paper, Sholihin et al. (2011) use data comprising 54 managers from a UK financial services institution and analyze both the direct effect of participative target setting on employee effort and the respective channels mediating this relationship. They find that procedural fairness and interpersonal trust fully mediate this direct relationship but call for future research using "larger samples from various organizations determined randomly" (p. 145) to further examine their propositions and findings. We are able to exploit four survey waves comprising between 771 and 1,219 establishments per wave and between 6,500 and 7,500 employees randomly drawn from these establishments. Thus, we complement prior literature by being able to test whether the relationships found also hold for larger, representative data sets. We run various empirical specifications to tackle endogeneity concerns and omitted variable bias usually associated with non-random treatment assignment.

Second, we add to the emerging literature on work engagement as a new proxy for employee effort at the workplace (Li and Sandino, 2018). Sholihin et al. (2011), for instance, analyze if participation in target setting affects goal commitment, which is defined as attachment to or determination to reach a goal (Locke and Latham, 1991) and willingness to put in effort to attain a goal (Renn et al., 1999) and argue that this concept is related to employee effort and ultimately employee performance. While we agree with this statement, we think that goal commitment as a concept might focus too much on the attainment of the goal and might thus be too narrow to measure effort in a more general sense, as effort directed on goal areas might also crowd out effort directed at non-goal areas (Ordóñez et al., 2009). We therefore employ the broader, commonly used work engagement scale (Kahn, 1990; Bakker, 2017).

Third, we also contribute to the literature examining participation in target setting. This is because, despite the fact that we do not know the exact contents of the AIs and TAs, this process is likely to be participative to some extent, as superior and subordinate meet in order to talk about the subordinate's targets. There are numerous studies examining these links using lab experiments, single firm studies and literature reviews (Locke and Latham, 2006; Derfuss, 2009). In a field experimental setting, Eyring and Narayanan (2018) provide evidence that challenging targets improve performance of above-median performers, but damp performers of below median performers. Finally, there is also evidence that the way that goals are set and communicated influences their effectiveness as well. Liu and Zhang (2015) find that performance is highest when the achievement of the target is revealed *ex post* (after the operation ends) rather than *ex ante* (before the operation starts) and when performance-contingent incentives are framed as a bonus rather than a penalty. Holzacker et al. (2019) emphasize the potential costs of relative target setting, and analyze costs and benefits using data from an industrial services company. Thus, while some studies emphasize a positive impact of targets on effort and performance, others argue that the way that targets are set is important and that under certain conditions, the impact on effort and performance might even be negative. We thus use our data to provide evidence how targets set via two common performance management practices influence work engagement *on average*. For firms thinking about introducing a PMEP, this may provide guidance about how this might influence employee effort.

Fourth, by differentiating between AIs and TAs, we also contribute to the literature analyzing whether formalization of targets is of additional value and provide evidence that it is indeed. Locke and Latham (1991) e.g. state that specific, or more formalized, targets induce higher levels of effort and performance as do "do-your-best" ones. In line with Hartmann and Slapničar (2009), we hypothesize that targets might possess high formality in case they are explicated by superiors in a quantitative and written fashion. By discussing the performance of the last year and key areas of improvement, targets for the next period are set in a relatively informal way during an AI and might then be formalized via a written TA. Therefore, we analyze if it is of additional value to move from a more informal way of setting targets to a more formal one. First, this might be important for firms that discuss implementing a PMEP and question whether an AI suffices or if a written TA adds additional value. Second, this information might also be interesting for firms that already use AIs and think about further formalizing the target setting process via a written TA.

Finally, we contribute to the debate about potential channels via which target setting affects effort and performance. In particular, we consider two potential mediators: procedural fairness and goal clarity. While most authors (e.g. Libby, 1999, 2001; Wentzel, 2002; Sholihin et al., 2011; Sholihin and Pike, 2013) find a positive link between (participation in) target setting and procedural fairness, Voußem, Kramer and Schäffer (2016) examine whether procedural and distributive fairness perceptions are influenced by the degree of subjectivity of performance measures and find an inverted U-shape of this relationship. Ordóñez et al. (2009) argue that targets or goals might even lead to a feeling of unfairness. This is because employees are heterogeneous with respect to their level of ability. The same targets might thus be to easy for some individuals and too hard for others but when tailoring goals to individuals, some might feel treated unfairly in case they have the feeling that rewards do not fairly match effort and performance. With regards to the impact of procedural fairness on effort and performance, there is no controversy in the literature such that most authors (e.g. Libby, 1999, 2001; Sholihin and Pike, 2009; Zapata-Phelan et al., 2009) find a positive effect. Derfuss (2009) finds a positive relationship between participation in budget setting and (among other dependent variables) goal clarity in a literature review, while Anderson and Stritch (2015) find a positive relationship between goal clarity and performance in an experimental study. Thus, we contribute to the literature by providing evidence on whether, on average, the positive effects of targets on procedural fairness found by most authors dominate the negative side effects emphasized by Ordóñez et al. (2009) and whether this translates into higher employee effort. Furthermore, we provide evidence whether goal clarity mediates the direct relationship between targets and work engagement.

The paper proceeds as follows: In Chapter 2.2, we review the literature and develop our hypotheses. In Chapter 2.3, we describe the data and our dependent and independent variables of interest. Chapter 2.4 presents our empirical results. Finally, Chapter 2.5 concludes.

2.2 Background and Hypotheses Development

2.2.1 The Impact of PMEP on Work Engagement

Prior literature has provided conflicting results with respect to the way in which targets influence employee effort and performance. Proponents of classical goal setting theory (e.g. Locke and Latham, 1991, 2002, 2006) argue that there is a "positive linear relationship" between the difficulty of the target and task performance, as long as different goals are not conflicting, the respective person is committed to the goal and possesses the necessary ability to attain it. They argue that challenging targets are more motivating because they induce a feeling of success when targets are met and because such targets help employees to grow in the workplace (Locke and Latham, 2006). Furthermore, attaining targets is often linked to bonus payments. According to principal-agent theory, this link between pay and performance aligns employee incentives with company goals and therefore induces effort (Jensen and Murphy, 1990). We further argue that the PMEP in our setting is rather participative, as the employee has at least the possibility to voice concerns during the AI. There is an array of literature on participation in target setting and the relationship to employee effort and performance. Sholihin et al. (2011), for instance, find a positive impact of participation in target setting on goal commitment, which they use as their proxy for employee effort. In a recent literature review, Derfuss (2009) finds a moderately strong relationship between participation in budgeting and employee behaviors beneficial to the organization.

However, there is also evidence suggesting that under certain circumstances, targets might lead to lower effort and performance. In a lab experiment, Seijts and Latham (2001) find that "do your best" outcome goals have a larger effect on performance than specific, difficult outcome goals, while they find the opposite for learning goals. This is the case as employees might focus too much on the attainment of the desired outcome than on learning, which is necessary to reach this outcome. Thus, even proponents of classical goal setting theory admit that specific, challenging targets might not have a positive impact on employee performance under every contingency. Li and Sandino (2018) provide evidence that challenging tasks might discourage below-median performers, while Ordóñez et al. (2009) highlight further potentially harmful side effects of target setting, among them crowding-out of intrinsic motivation by extrinsic motivation. Therefore, the impact of targets on employee effort is not unambiguous and the question about how targets affect employee effort *on average* in larger samples spanning many firms from different industries remains unanswered.

Since the bulk of literature on target setting and participation in target setting finds a positive impact of the presence of a PMEP on employee effort, we expect the average effect to be positive but emphasize that it is ultimately an empirical question. We thus formulate the following hypothesis:

Hypothesis 1: *There is a positive effect of the presence of a PMEP on work engagement.*

2.2.2 The Effect of Appraisal Interviews and Target Agreements on Work Engagement

Next, we analyze the question whether formalization of targets is of value by subdividing the overall PMEP into AIs and TAs. Locke and Latham (1991) state that specific, more formalized targets induce higher levels of effort as "do your best" targets. Hartmann and Slapničar (2009) further posit that formal targets are superior to informal ones as they provide higher feedback quality. They arguably specify the performance dimensions being evaluated and their link to rewards better, ultimately increasing goal orientation and motivation. We argue that the two components of the PMEP considered by us possess different degrees of formality. As described in more detail in Section 2.3, an employee can only have a written TA in case she also receives an AI. AIs are themselves not a completely informal way to set targets, as respondents in our data set are asked to only consider pre-scheduled AI meetings. Nevertheless, explicating these targets in a written form via a TA implies *additional* formality.

With respect to AIs, our study is closely related to Kampkötter (2017). By employing data on the employee level from the German Socio-Economic Panel (SOEP), he estimates the impact of performance appraisals on job satisfaction. Performance appraisals as a concept are closely related to AIs as both comprise a developmental and an evaluative function (e.g. Boswell and Boudreau, 2002). The developmental function aims at improving an employee's effectiveness by enhancing her skills, attitudes, and experiences (e.g. via identification of strengths, weaknesses, and training needs, or goal setting). Evaluation, in contrast, consists of comparing the employee's performance to a certain standard and is often linked to decisions such as pay increases, promotion, or termination decisions. Despite the similarities, we abstain from calling our measure a performance appraisal for several reasons. As pointed out by Aguinis, Gottfredson and Joo (2013) and recently by Bayo-Moriones, Galdon-Sanchez and de Morentin (2020), performance appraisals and performance management are two interrelated yet distinct concepts in the sense that performance management is more general. Likewise, we argue that the AI as part of our PMEP is more general than a performance appraisal as the focus is not only on past performance, but also future potential. Indeed, the specific question used by Kampkötter (2017) focuses more closely on the evaluative function, while our measure is more balanced between the evaluative and developmental functions. The author finds an overall positive effect of performance appraisals on job satisfaction, in particular when performance appraisals are linked to monetary outcomes. However, the question whether (performance) AIs are successful in increasing employee performance remains unexplored in this study. Furthermore, the data used only provides information on the employee but very crude information on the establishment the employee works in, an issue we are able to tackle using linked employer-employee data. Considering that performance appraisals may lead to higher job satisfaction, an employee attitude shown to lead to higher employee and organizational productivity (Krekel, Ward and De Neve, 2019), and that AIs are used to set targets, we expect a positive impact of AIs on employee engagement.

In contrast to the topic of performance appraisals, individual-level research specifically focusing on written TAs is scarce.[6] It is likely that TAs on the individual level influence performance on the establishment level indirectly via beneficial employee behavior, in particular via increased effort provision, but this has not been shown so far.

[6]Using German establishment-level data, Kampkötter, Marggraf and Zimmermann (2017) find that establishments using TAs achieve 5% higher sales, implying a positive impact of TAs on organizational performance.

We thus examine whether there is a positive effect of AIs, as our less formal way of target setting, on work engagement and whether a higher degree of formality as implied by written TAs provides *additional* value. We therefore formulate the two following hypotheses:

Hypothesis 2a: *There is a positive effect of the presence of an AI on work engagement.*
Hypothesis 2b: *There is an additional positive effect of the presence of a written TA on work engagement.*

2.2.3 Mediation Analysis

It is likely that our PMEP does not only affect work engagement directly but also indirectly via affecting other employee behaviors which then in turn affect this outcome. In particular, we consider procedural fairness and goal clarity as potential mediators.

With respect to procedural fairness, our study is most closely related to Sholihin et al. (2011) who use data comprising 54 managers from a UK financial services institution and analyze both the direct effect of participative target setting on employee effort and the respective channels mediating this direct relationship. They find that the direct effect is fully mediated by procedural fairness and interpersonal trust meaning that the direct effect becomes insignificant when these mediators are accounted for. We believe that, due to our methodological advantages, our study serves as a complement as we analyze firms in different industries and of different size.

There are various theories dedicated to understanding organizational justice.[7] Fairness Heuristics Theory (Lind and Tyler, 1988; Lind, 2001; van den Bos, Lind and Wilke, 2001), for instance, argues that in most work situations, individuals are at risk of being exploited. Due to this immanent risk of exploitation, they ask the question if the authority is to be trusted (Cropanzano et al., 2001). As it is impossible to accurately calculate trustworthiness for each relationship, individuals use heuristics to facilitate the decision. Procedures such as participation or voice signal in-group membership (van den Bos, Lind and Wilke, 2001), ultimately increasing procedural fairness perception. Leventhal (1980) posits that individuals fairness perceptions are influenced by six rules, in particular accuracy, bias suppression, consistency, correctability, ethicality of procedures, and representativeness. Ordóñez et al. (2009), in contrast, state that literature ignores potential negative effects of target setting on fairness perceptions. In particular, they argue that setting targets might in fact lead to a feeling of unfairness rather than fairness. This is because employees possess heterogeneous ability, making the same goal easily attainable for some individuals and too difficult to achieve for others. However, tailoring goals to individuals might in turn lead to a feeling of unfairness as some individuals might feel that rewards do not fairly match

[7] For an overview, see Cropanzano et al. (2001).

effort and performance. Despite the negative side effects emphasized by Ordóñez et al. (2009), we follow Sholihin and Pike (2013) in arguing that the PMEP considered by us both gives employees some degree of voice, in the sense that it is a rather participative way to set goals, and fulfills many of the six rules put forward by Leventhal (1980). Therefore, we expect a positive effect of the presence of our PMEP on employees' perception of procedural fairness.

There is an array of literature examining the link between procedural fairness and beneficial employee behaviors (e.g. Korsgaard, Schweiger and Sapienza, 1995), but most studies focus on the impact of procedural fairness on employee behaviors like group commitment (e.g. Colquitt, 2001) and not on individual engagement or effort per se. Exceptions specifically analyzing the impact of procedural fairness on goal commitment as a concept related to effort are Wentzel (2002) and Sholihin et al. (2011). Wentzel (2002) expects a positive relationship between procedural fairness and goal commitment for two reasons: First, attaining the goal should be in the self-interest of the employee in case procedures are fair (instrumental perspective) and second, compliance with the group policy, in this case the goal, should affirm group membership (relational perspective). Both Wentzel (2002) and Sholihin et al. (2011) find empirical support for this hypothesis. We follow the authors and expect a positive impact of procedural fairness on effort, here proxied by work engagement, and formulate the following hypothesis:

Hypothesis 3: *The direct effect of the presence of a PMEP on work engagement is mediated by procedural fairness.*

As compared to the literature on procedural fairness, studies that specifically examine goal clarity are relatively scarce in the management accounting literature, even though clarity about the organization's goals is essential for performance management. Sholihin and Pike (2013) e.g. state that "the existence of prespecified goals is likely to provide clearer understanding (goal clarity) for organizational members and indicate how they will be evaluated" (p. 32). Further, they argue that "goal specificity[8] and clarity informs employees of their responsibilities and performance targets" and that the "existence of specific goals will guide employees in deciding where they should direct their attention and effort" (p. 32). Since our PMEP consists of the joint presence of an AI and a written TA, we argue that goals set via this process are highly specific, inform employees about their responsibilities and performance targets and should thus increase goal clarity. With respect to the goal clarity-performance link, Anderson and Stritch (2015) expect that higher goal clarity leads to higher performance by referring to classical goal setting theory

[8]Note that Sholihin and Pike (2013), in line with Fang, Evans and Zou (2005), define goal specificity as "the extent to which the goals are clearly defined by a supervisor." We therefore consider goal specificity as being an antecedent to goal clarity.

(e.g. Dossett, Latham and Mitchell, 1979) and indeed find a positive impact in a lab experiment. However, in some circumstances, goal clarity might actually be detrimental to performance, as it might lead to tunnel vision (Seijts and Latham, 2001; Anderson and Stritch, 2015). Ordóñez et al. (2009) further argue that goals directed at goal areas might crowd out effort directed at non-goal areas without leading to a greater overall effort. We argue that engagement as our proxy for effort is rather general in the sense that it does not differentiate between effort directed at goal and non-goal areas, such that we can examine whether or not this is the case. We follow the bulk of the literature and expect both a positive relationship between the presence of our PMEP and goal clarity and between goal clarity and employee engagement and formulate the following hypothesis:

Hypothesis 4: *The direct effect of the presence of a PMEP on work engagement is mediated by goal clarity.*

2.3 Data

In order to examine the nexus between the use of AIs, TAs, and work engagement, we use the Linked Personnel Panel (LPP), a new, matched employer-employee data set. The LPP is representative for German private sector establishments with more than 50 employees subject to social insurance contributions (for a detailed description of the design of the data set and the sources of the applied constructs see Kampkötter et al. (2016)). Response rates are comparatively high, amounting to, for instance, 78% for the employer survey and 30% for the employee survey for the third wave. Overall, we find no significant selectivity effects on panel participation.[9] Surveyed establishments are randomly drawn from the IAB establishment panel, an annual survey of nearly 16,000 German establishments. In order to consider establishments of all sectors and size classes, the sample is drawn in a disproportionate stratified manner by establishment size, federal state and business sector.

We can make use of the four waves 2012, 2014, 2016/17 and 2018/19 of the LPP linked employer-employee data set. In detail, the employer survey covers between 769 and 1,219 establishments per wave. Establishment managers provide information on HRM practices and other firm characteristics. From these establishments, a random sample of employees working within the surveyed establishments (roughly between 6,500 and 7,500 individuals per wave) are interviewed at home via telephone (CATI) or web interface (CAWI) about job characteristics and perceptions, personal characteristics, attitudes towards their organization and behavioral variables. This feature of the data enables us to examine the link between the presence of AIs and TAs on the individual

[9] The data set is open to any researcher and is available via the Research Data Centre (FDZ) of the German Federal Employment Agency at the Institute for Employment Research (IAB). The DOI is: 10.5164/IAB.LPP1617.de.en.v1. For more details, see Haylock and Kampkötter (2019).

level and engagement, while simultaneously being able to control for organizational characteristics on the level of the establishment. Furthermore, the longitudinal structure of the data allows us to employ panel data methods, which enables us to move closer towards causality.

Our analysis is based on two items from the LPP employee survey. The item we use to measure the presence of an AI asks the interviewee the following question: *"Did you have an appraisal interview with your superior last year (e.g. on your professional growth or staff assessment)? Please consider only appraisal interviews for which an appointment was made."* This question is then used as a filter question for the item measuring the incidence of a formal TA, implying that, by construction, an employee can only be covered by a TA in case she is also covered by an AI. It is important to stress the formal character of AIs, since respondents should only consider meetings for which a formal appointment was made. The item measuring the presence of a TA (in conjunction with an AI) is based on the following question: *"Did your superior agree with you on the objectives fixed in writing during the appraisal interview?"*

We use these two questions to construct two alternate specifications for our main explanatory variables of interest, one we define as a full PMEP and one that differentiates between its components. We exploit this twofold strategy because we encounter a problem common to fixed effects analyses, namely that we do not have enough within-variation to separate the effect of the presence of the TA from the effect of the presence of the AI (Bender et al. (2018)). This problem stems from the fact that an employee can only have a TA in case she also has an AI. As a result, in a specification including both an AI and a TA dummy, the TA dummy has to be interpreted as an interaction term. Therefore, the control group for the TA variable are all individuals who report having an AI but no TA. In order to identify the effect of TAs using individual fixed effects, we therefore need a sufficient number of employees who report having an AI in both periods and either switch from having no TA to having one or switch from having a TA to having none. Among the individuals who are observed multiple times, most either do not switch at all within the four waves available to us, or they *jointly* switch in both the AI and the TA variable. Therefore, we cannot disentangle the effect of the AI from the one of the TA in our individual fixed effects specification in a meaningful way. For our first specification, we thus construct a dummy variable taking the value one if an employee is covered by both an AI and a TA, and zero otherwise. In case the dummy variable takes the value one, we define the employee as being subject to a full PMEP. The coefficient of this variable can be interpreted as the effect of the *joint* presence of AIs and TAs.[10] Our second specification differentiates between the presence of an AI and the presence of a TA by constructing two

[10]Note that this PMEP definition does not comprise variable pay, which is included as a control variable in the empirical specifications. However, PMEPs are often connected to variable pay. We therefore also experiment with specifications that include a measure of variable pay in the PMEP definition.

dummies: One that takes the value one in case an employee is covered by an AI and zero otherwise and one that takes the value one if an employee is covered by *both* an AI *and* a TA. Consequently, the TA dummy measures the *additional* impact of the written TA *in addition* to the effect of the AI.

As a proxy for individual effort, we apply the widely used, internationally validated nine-item work engagement scale UWES-9 by Schaufeli and Bakker (2004). Respondents were asked to indicate to which extent they agree with nine statements regarding their job such as the following on a five point Likert scale: *"At my work, I feel bursting with energy."*[11] The reported scores of every single item are then added up and divided by 9, such that the resulting *engagement score* represents an equally weighted average with values between 1 and 5. We further standardize this engagement score in order to make a quantitative interpretation in standard deviations possible.

Cronbach's Alpha of our engagement index is 0.915, suggesting a high degree of internal consistency of this construct. Our two potential mediators are conceptualized as follows. Goal clarity is based on two items from the organizational climate questionnaire by Patterson et al. (2005). Specifically, respondents are asked to state on a five point Likert scale to which extent they agree with the following statements: *"The superiors clearly communicate requirements and objectives"* and *"Everyone who works here is well aware of the long-term plans and direction of this company."* Again, the individual answers are added, the total score is divided by the number of items, and the resulting index is standardized. Procedural fairness is operated by one item from the justice scale by Kim and Leung (2007), which asks respondents to state on a five point Likert scale to which extent they agree with the following statement: *"The rules and procedures to make decisions are fair."* We again standardize this variable.

Furthermore, the data allows us to account for a rich set of control variables on the establishment and individual level. Establishment-level controls comprise industry (5 categories), region (north, east, south, west), a set of dummies capturing ownership structure, and a dummy capturing whether or not the establishment is independent. Individual-level controls include sex (0/1), age (8 dummies), supervisory position (0/1), full-time position (0/1), white-collar employee (0/1), monthly net income, type of employment contract (fixed term/permanent), permanent relationship (0/1), highest level of school and occupational or university education (7 dummies), household size, and survey method (CAWI/CATI). Establishment-level controls include industry (5 dummies), regional area (4 dummies), and establishment size (5 dummies). In all regressions, standard errors are clustered on the establishment level. Detailed descriptive statistics on our main dependent and independent variables are displayed in Tables A.1, A.2, and A.3 in Appendix A.2.

In 2019, the latest year comprised in our data, the mean (unstandardized) engagement index is 3.44, while the median is 3.56. Therefore, we observe a higher probability mass at

[11] A complete list of the items used can be found in Appendix A.1.

larger values of the engagement index, indicating that employees in our sample are, on average, rather engaged. The distribution is also rather stable over time (mean values range between 3.68 and 3.76 from 2012 to 2016). In total, our data comprises 16,506 employee-year observations that are non-missing with respect to the AI variable. 8,622 (52.24%) of these employee-year observations reported having an AI. Out of these, 5,875 (68.14%) also reported having a written TA.

2.4 Empirical Strategy and Results

2.4.1 The Direct Impact of a PMEP on Work Engagement

In order to investigate Hypothesis 1, i.e. whether there is a positive effect of the presence of a full PMEP on work engagement, we use OLS regressions employing various fixed effects in order to be able to make more causal statements. In addition, all of our specifications include a rich set of covariates on the establishment and individual level. Results are displayed in Table 2.1.

In column (1), we regress work engagement on PMEP by including all controls on the establishment and individual level as well as establishment size and year fixed effects. The positive PMEP coefficient is statistically and economically significant: the magnitude of the coefficient implies that the engagement score of employees covered by a PMEP is, on average, 0.203 standard deviations higher compared to employees without a PMEP. Therefore, column (1) provides first support to Hypothesis 1, indicating that target setting via a PMEP seems to have a positive impact on engagement. In column (2), we tackle the question whether the impact of our PMEP on work engagement is driven by the use of variable incentive pay since achieving pre-negotiated targets might be tied to a variable pay component. Thus, it might not be the PMEP *per se* that induces larger work engagement, but rather the link to variable pay. The simple correlation coefficient between the PMEP variable and the use of variable compensation is 0.24, suggesting that employees who report having a PMEP also tend to have a variable pay component. However, the size of this correlation is not large enough to suggest that the two variables capture the same effect. Results in column (2) are consistent with these descriptives: The coefficient of variable pay is positive and statistically significant, suggesting that employees who have a variable pay component show, on average, a higher work engagement. More important, the magnitude of the PMEP coefficient changes only marginally, suggesting that the relationship between the PMEP and work engagement is not just driven by incentive pay.

In columns (3) and (4), we additionally include establishment fixed effects to reduce the likelihood of omitted variable bias and to take a further step towards causality. In column (3), we separately include establishment and year fixed effects. Thereby, we are able to account for time-constant unobserved heterogeneity on the level of the establishment

Table 2.1: Direct Effect of PMEP on Work Engagement

	(1)	(2)	(3)	(4)	(5)
Variables	Engagement Index (std.)				
PMEP	0.203***	0.193***	0.245***	0.253***	0.0597**
	(0.0182)	(0.0184)	(0.0207)	(0.0210)	(0.0239)
Variable Pay		0.0548***	0.0706***	0.0636***	-0.0133
		(0.0196)	(0.0221)	(0.0227)	(0.0254)
Establishment Controls	yes	yes	yes	yes	yes
Employee Controls	yes	yes	yes	yes	yes
Year FE	yes	yes	yes		yes
Size FE	yes	yes			yes
Establishment FE			yes		
Establishment FE × Year FE				yes	
Individual FE					yes
Constant	-0.324***	-0.346***	-0.418***	-0.466***	0.223*
	(0.0629)	(0.0636)	(0.118)	(0.0433)	(0.124)
Observations	16,506	16,498	16,498	16,026	16,498
Number of Employees					12,057
R-squared (within)	0.076	0.077	0.189	0.225	0.028

The dependent variable Engagement Index is an index containing the weighted average of nine items and is standardized. All underlying items are measured on a 5-point Likert scale (between 1 and 5). PMEP is a dummy variable taking the value 1 if an individual has both an AI and a TA. Variable Pay is a dummy taking the value 1 if an individual has a variable pay component. Establishment-level controls include industry (5 dummies), regional area (4 dummies), and establishment size (5 dummies). Employee controls comprise female (0/1), age (8 dummies), supervisory position (0/1), white-collar employee (0/1), full-time position (0/1), monthly net income, type of employment contract (fixed term/permanent), permanent relationship (0/1), highest level of training qualification (7 dummies), household size, and survey method (CAWI/CATI). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

while simultaneously controlling for general market trends through time fixed effects. Results are robust as the coefficient of PMEP is positive and statistically significant. In fact, the magnitude of the effect even increases from 0.193 to 0.245, suggesting that the effect gets larger when taking the within-firm rather than the across-firm perspective (this difference is also statistically significant, p = 0.01). Here, we also account for unobserved establishment level heterogeneity such as time-constant performance management or leadership culture. In column (4), we include an interaction between establishment and year fixed effects. We thereby allow unobserved, establishment-specific characteristics to vary over time. Results remain virtually unchanged, both with respect to the magnitude and significance of the PMEP coefficient. Column (5) accounts for unobserved individual heterogeneity that both determines an employee's engagement level and coverage by a PMEP. Results of the individual fixed effects regression indicate that this is partly the case, as the size of the effect decreases from 0.253 to 0.0597. Hence, a within-person change in PMEP leads to an increase in work engagement of around 0.06 standard deviations. This indicates that the presence of a PMEP does indeed have a positive impact on work engagement. Between columns (4) and (5), we face a trade-off between representativeness and a higher degree of causality. As outlined above, among those employees who are observed multiple times, there are only few switchers. Hence, while the estimation in column (5) takes out individual time-constant heterogeneity, representativeness of results can be questioned. For this reason, we consider column (4) to be our most reliable estimation. As results of the fixed effects regression are qualitatively in line with our preferred specification, such that the coefficient is also positive and statistically significant, we conclude that our results provide overall support for Hypothesis 1.

To analyze whether average effects are driven by individuals working in specific firms or positions, we run the regressions separately for specific subsamples. Table A.4 shows effects separately for different size groups of firms, while Table A.5 shows results separately for different industries. In Table A.6, we split the sample by employee position, such that we run regressions separately for non-managers and managers (columns (1) and (2)) and for blue-collar and white-collar workers (columns (3) and (4)). Event though coefficients partly differ in size, there is little effect heterogeneity in the sense that all estimations report positive and statistically significant coefficients.[12] Hence, the presence of a PMEP seems to be associated with higher work engagement, regardless of the size of the firm or the industry it is active in and no matter if the covered individual is a manager or non-manager, a blue-collar or white-collar worker.

We run the following robustness checks for all estimations included in this paper to ensure the consistency of our results. Tables A.7 and A.8 depict results when clustering standard errors on the establishment-year and individual level, respectively, and show that our findings are robust to alternate clustering decisions. We also estimate lagged dependent variable specifications, which are depicted in Table A.9.[13] Our findings remain

[12]Note that for the sake of clarity, tables only show results of our preferred specification using the interaction between establishment and year fixed effects. Results of the other specifications are consistent, such that all but the individual fixed effects specifications report positive and statistically significant coefficients. The individual fixed effects estimates are either positive and marginally significant or not significant, which is not surprising given that the sample size and within-variation decreases when splitting the sample.

[13]Note that tables A.7, A.8, and A.9 show results of our preferred specification including the interaction between establishment and year fixed effects. However, results are robust to our other specifications. Column (1) in these tables refers to estimations in Section 2.4.1, column (2) to estimations in 2.4.2, and columns (3) to (6) to estimations in 2.4.3.

consistent to these alternate specifications.[14]

As outlined above, targets are often set in conjunction with a variable pay component and variable pay is also often regarded as being part of the PMEP. In Table A.10, we therefore interact the original PMEP with variable pay to account for the possibility that the PMEP might be more (or less) effective in combination with variable pay. Interestingly, this does not seem to be the case. The direct PMEP coefficient is positive and statistically significant in all specifications while the variable pay coefficient is positive and significant in all but the individual fixed effects specification. The interaction term is not significant in any specification, thus suggesting that the effect of the PMEP on work engagement does not depend on variable pay.[15] In tables A.11, A.12, and A.13, we experiment with alternate PMEP definitions that explicitly include variable pay. In Table A.11, we construct an index taking the value zero in case an individual reports neither having an AI, a TA nor variable pay, a value of one (two) if she is covered by one (two) and a value of three if she is covered by all three components. In Table A.12, we construct an alternate PMEP dummy variable that only takes the value 1 if an individual is covered by an AI and a TA and also receives a variable pay component. Since the control group in this estimation is heterogeneous and includes individuals that are covered by one or two but not all three measures, we only keep individuals in the control group that are covered by none of these measures in Table A.13. Results for the direct impact of the PMEP on engagement, depicted in column (1) of the respective tables, remain robust.[16] We conclude that the positive impact of the presence of a PMEP on work engagement we find is not driven by specific types of firms or workers and is very robust.

2.4.2 Separating the Impact of Appraisal Interviews and Target Agreements

In this section, we split the PMEP into its components. The aim is to analyze if the observed positive effect of the PMEP on employee engagement is driven by AIs alone or whether further formalization via written TAs in the performance management process provides

[14]To additionally tackle issues related to selection bias and reverse causality, we estimate the IV method proposed by Lewbel (2012) by using Stata's ivreg2h command developed by Baum and Schaffer (2012) as a further robustness check for our baseline specification in column (1). These issues might arise in case engaged individuals self-select into having a PMEP or in case they are chosen for having a PMEP based on their previous level of engagement. Our results remain qualitatively the same, such that the coefficient of PMEP is still positive and statistically significant.

[15]Conducting a mediation analysis would not make sense here, since the interaction term is already insignificant. Table A.10 hence includes the interaction term and does not show a specification without variable pay but is otherwise equivalent to Table 2.1.

[16]Note that these tables again depict our preferred specification. Results of the other specifications are consistent except that the individual fixed effect specification is not significant when using the two alternate PMEP dummy variables. This is not surprising because this definition further reduces within-variation, as an individual jointly has to switch between having no AI, TA, and variable pay to having all three components to be classified as a switcher. This issue will be discussed in more detail in section 2.4.2.

additional value.

Table 2.2: Direct Effects of AIs and TAs on Work Engagement

	(1)	(2)	(3)	(4)	(5)
Variables			Engagement Index (std.)		
Appraisal Interview (AI)	0.193***	0.124***	0.144***	0.150***	0.0959***
	(0.0193)	(0.0253)	(0.0268)	(0.0272)	(0.0301)
Target Agreement (TA)		0.111***	0.162***	0.169***	0.0104
		(0.0238)	(0.0265)	(0.0267)	(0.0268)
Variable Pay	0.0537***	0.0483**	0.0675***	0.0603***	-0.0152
	(0.0194)	(0.0196)	(0.0220)	(0.0227)	(0.0253)
Establishment Controls	yes	yes	yes	yes	yes
Employee Controls	yes	yes	yes	yes	yes
Year FE	yes	yes	yes		yes
Size FE	yes	yes			yes
Establishment FE			yes		
Establishment FE × Year FE				yes	
Individual FE					yes
Constant	-0.391***	-0.359***	-0.443***	-0.505***	0.205
	(0.0658)	(0.0636)	(0.119)	(0.0434)	(0.124)
Observations	16,528	16,498	16,498	16,026	16,498
Number of Employees					12,057
R-squared (within)	0.077	0.078	0.190	0.227	0.030

The dependent variable Engagement Index contains the weighted average of nine items and is standardized. All underlying items are measured on a 5-point Likert scale (between 1 and 5). AI is a dummy variable taking the value 1 if an individual has an AI, while TA is a dummy taking the value 1 if she additionally has a TA. Variable Pay is a dummy taking the value 1 if an individual has a variable pay component. Establishment-level controls include industry (5 dummies), regional area (4 dummies), and establishment size (5 dummies). Employee controls comprise female (0/1), age (8 dummies), supervisory position (0/1), white-collar employee (0/1), full-time position (0/1), monthly net income, type of employment contract (fixed term/permanent), permanent relationship (0/1), highest level of training qualification (7 dummies), household size, and survey method (CAWI/CATI). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

In column (1) of Table 2.2, we check whether there is a positive relationship between the presence of AIs and work engagement. Results show a positive and statistically significant coefficient of 0.193 standard deviations.[17] It is noteworthy that the coefficient

[17]Note that we directly control for variable pay in this specification. Results do not change when omitting

in this specification is relatively similar to the coefficient of the PMEP variable in column (2) of Table 2.1. Therefore, when not controlling for TAs, the AI variable seems to pick up the entire effect of the presence of a PMEP. This can have two reasons: Either the effect of a PMEP on work engagement is entirely driven by AIs such that formalization via written TAs does not increase engagement or there exists an omitted variable bias problem in column (1) of Table 2.2, such that part of the effect of AIs on engagement is actually due to the additional presence of a written TA. We test this conjecture in column (2) by including the TA dummy. The results show that the latter seems to be true. Both the AI and the TA variables show positive and statistically significant coefficients. The magnitude of the coefficient of the AI variable is reduced to 0.124, while the TA variable reports a coefficient of 0.111, indicating that approximately half of the effect of AIs as reported in column (1) can actually be attributed to the additional presence of a written TA.

Consistent with subsection 2.4.1, we also run two different specifications including establishment fixed effects. In column (3), we include establishment fixed effects and year fixed effects while we interact these two fixed effects in column (4). In analogy to Table 2.1, the results remain qualitatively the same. Results up to this point thus provide support for hypotheses 2a and 2b. There seems to be both a positive impact of AIs on employee effort as proxied by work engagement and an *additional* positive impact of formalization via a written TA.

In the last step, we again include individual fixed effects in column (5) to check robustness of results. As can be seen in Table 2.2, the effect of AIs on work engagement is still positive and statistically significant. However, the TA variable now turns statistically insignificant. One potential explanation refers to the joint presence of AIs and TAs in many firms, i.e. AIs and TAs are often introduced jointly for employees. If this is true, estimating the isolated effects of AIs and TAs in fixed effects specifications is almost impossible. The reasoning is the following: Fixed effects require a certain degree of variation within individuals, implying the need for a sufficient amount of *switchers*. In order to identify a significant effect for our TA variable, we would need a sufficient number of individuals who switch from having no TA in one period to having one in the next period or vice versa. At this point it is crucial to remember that by construction of the data set and also plausibly in firms, an individual can only have a TA if she also has an AI. A switch from zero to one in the TA variable can thus capture two different events: Either the employee obtains an AI and a TA *jointly* or the employee has already had an AI in the previous period and *in addition* obtains a TA in the actual period. In case AIs and TAs are introduced jointly, one cannot isolate the effects of an introduction of AIs and TAs. Descriptive results indeed suggest that, for instance, a large fraction of individuals who switch from having no AI to having one simultaneously switch from having no TA to having one. Therefore, the effect of the TA variable can only be identified if the data

variable pay from the regression equation.

contains enough individuals that report having an AI in two consecutive periods and switch from having no TA to having one or vice versa. Hence, it is highly likely that this lack of variation causes our fixed effects estimates for TAs to be insignificant.[18] At least, it nicely shows how challenging it can be to causally analyze isolated effects of simultaneously applied performance management practices using firm data. Of course, it might also be that time-constant individual heterogeneity such as ability has driven the results in the previous specifications and is now explicitly controlled for.

In sum, our analyses provide support for both hypotheses 2a and 2b. Results indicate a positive impact of AIs on work engagement. This effect is robust across all specifications. Results also provide evidence that formalization via written TAs further increases engagement, by showing a positive and statistically significant coefficient in all but the specifications including individual fixed effects. We point out that this is likely due to a lack in within-variation.

2.4.3 Mediation Analysis

In this section, we present our test of hypotheses 3 and 4, i.e. whether the direct effect of the PMEP on work engagement is mediated by procedural fairness and goal clarity.[19] To examine these two potential channels, we follow the mediation analysis approach put forward by Baron and Kenny (1986) and estimate three different equations. First, the potential mediator (goal clarity, procedural fairness) is regressed on the independent variable, here PMEP. In a second step, the dependent variable (engagement) is regressed on the potential mediator. In a third step, the dependent variable is regressed on both the mediator and the independent variable. Full mediation is achieved if the respective coefficients of interest are statistically significant in the first two regressions and if a previously significant relationship between the independent and the dependent variable in the first regression becomes insignificant when including the mediator in the third regression. A variable partially mediates the relationship between an independent and a dependent variable if it significantly decreases the direct path between the independent and the dependent variable rather than completely eliminating it. Very often statistical relationships, such as the relationship between the presence of PMEP and work engagement have multiple causes, such that full mediation is rather unlikely. Table 2.3 shows the results of our mediation analysis based on our preferred specification from column (4) of Table 2.1, which showed the positive direct effect of PMEP on work engagement. Columns (1) and (2) depict the first step of the actual mediation analysis, the regression of goal

[18] We conduct similar robustness checks as in Section 2.4.1. Specification with clustered standard errors on the establishment-time and individual level are depicted in column (2) of tables A.7 and A.8, respectively, while column (2) of Table A.9 depicts the lagged dependent variable specification. All results remain robust.

[19] Note that in this analysis, we do not differentiate between AIs and TAs. However, looking at the two performance management instruments separately, we find the same patterns as in the analysis presented above.

Table 2.3: Mediation Analysis

Variables	(1) Goal Clarity Index (std.)	(2) Procedural Fairness (std.)	(3) Engagement Index (std.)	(4) Engagement Index (std.)	(5) Engagement Index (std.)	(6) Engagement Index (std.)
PMEP	0.318***	0.215***		0.159***	0.200***	0.148***
	(0.0234)	(0.0246)		(0.0201)	(0.0201)	(0.0201)
Goal Clarity			0.223***	0.291***		0.215***
			(0.0107)	(0.0106)		(0.0108)
Procedural Fairness			0.197***		0.273***	0.195***
			(0.00966)		(0.00936)	(0.00967)
Variable Pay	0.190***	0.164***	-0.00280	0.00950	0.0201	-0.00774
	(0.0219)	(0.0203)	(0.0216)	(0.0220)	(0.0219)	(0.0215)
Establishment Controls	yes	yes	yes	yes	yes	yes
Employee Controls	yes	yes	yes	yes	yes	yes
Establishment FE × Year FE	yes	yes	yes	yes	yes	yes
Constant	-0.177***	-0.283***	-0.345***	-0.415***	-0.388***	-0.370***
	(0.0393)	(0.0386)	(0.0400)	(0.0409)	(0.0406)	(0.0397)
Observations	16,380	16,277	15,925	15,989	15,912	15,879
R-squared	0.279	0.239	0.310	0.286	0.284	0.313

The dependent variables are constructed as follows. Engagement Index contains the equally weighted average of nine items. Goal Clarity Index contains the equally weighted average of two items. Procedural Fairness contains one item. All items are measured on a 5-point Likert scale (between 1 and 5). The resulting variables are standardized. PMEP is a dummy variable taking the value 1 if an individual has both an AI and a TA. Variable Pay is a dummy taking the value 1 if an individual has a variable pay component. Establishment-level controls include industry (5 dummies), regional area (4 dummies), and establishment size (5 dummies). Employee controls comprise female (0/1), age (8 dummies), supervisory position (0/1), white-collar employee (0/1), full-time position (0/1), monthly net income, type of employment contract (fixed term/permanent), permanent relationship (0/1), highest level of training qualification (7 dummies), household size, and survey method (CAWI/CATI). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

clarity and procedural fairness on the PMEP. As can be seen from the coefficient of the PMEP variable in column (1), there seems to be a significantly positive association between the presence of a PMEP and goal clarity. The PMEP coefficient in column (2) also indicates a positive association with procedural fairness. Column (3) depicts the second step of the mediation analysis, namely the regression of the dependent variable, engagement index, on the potential mediators, goal clarity and procedural fairness.[20] Both the coefficients of the goal clarity index and the procedural fairness variables are positive and statistically significant, suggesting that both goal clarity and procedural fairness are positively associated with work engagement. Columns (4) to (6) depict the third step, the regression of the dependent variable, work engagement, on the potential mediators, goal clarity and procedural fairness, and PMEP as our main independent variable. In columns (4) and (5), we thereby depict the regression of work engagement on PMEP and both potential mediators separately, while in column (6), both potential mediators are included simultaneously. Results show that the size of the direct effect decreases from 0.253 to 0.159 (0.2) standard deviations, when goal clarity (procedural fairness) is accounted for, and to 0.15 standard deviations when both potential mediators are included.[21] Furthermore, while both goal clarity and procedural fairness seem to decrease the direct PMEP effect, mediation via goal clarity seems to be somewhat stronger.[22]

In sum, our main results are consistent with partial mediation and we find evidence for hypotheses 3 and 4. The PMEP seems to increase employees' feeling of procedural fairness by giving them some degree of voice. The increase in perceived procedural fairness then results in a higher level of work engagement or effort, respectively. Furthermore, we find that the PMEP increases goal clarity. This shows that performance management is a useful tool for firms as it helps to make the organizational goals more visible to the workforce. Again, this increase in goal clarity results in an increase in overall engagement. However, results indicate that procedural fairness and goal clarity do not fully, but only partially mediate the impact of the PMEP on work engagement, as the direct effect is still significant.

[20] In unreported further analyses, we also regress work engagement on the two potential mediators separately. Results are qualitatively robust, such that in both regressions, the coefficient of interest is positive and statistically significant.

[21] In Table A.14 in Appendix A.3, we show equivalent results using the individual fixed effect specification. This specification suggests a rather strong mediation because both the magnitude of the coefficient and the statistical significance declines. Columns (3) to (6) in tables A.7, A.8, and A.9 show results when using establishment-year and individual clusters and when including lagged dependent variables, respectively. Finally, columns (2) to (5) in tables A.11, A.12, and A.13 depict results when employing alternate PMEP definitions. Our findings remain robust in all alternate specifications.

[22] Note that these differences are also statistically significant ($p < 0.01$ for all three coefficients) in our specificantions using establishment and year fixed effects and in our specification using individual fixed effects. Due to restrictions in the number of variables that Stata allows in seemingly unrelated regression analyses, we were unfortunately not able to test, whether differences between columns are also significant for our main specification featuring an interaction between establishment and year fixed effects.

2.5 Conclusion

Classical goal setting theory (e.g. Locke and Latham (1991, 2002, 2006)) has long emphasized that there is a positive link between target setting mechanisms and employee effort. However, recent contributions (Barsky (2008); Ordóñez et al. (2009); Liu and Zhang (2015); Eyring and Narayanan (2018); Holzacker et al. (2019)) provide evidence that the way goals are set is important and that goal setting might also have negative side effects that can actually outweigh its potential benefits. Most contributions in the management accounting literature studying the impact of target setting on employee effort rely on experiments or cross-sectional single firm case studies. Generalizable evidence about how targets influence effort *on average* using large and representative data is missing. Indeed, Sholihin et al. (2011) call for such evidence based on "larger samples from various organizations determined randomly" (p. 145). We address this gap in the literature by investigating the impact of performance management, focusing on appraisal interviews and written target agreements, on work engagement, a proxy for individual effort. We do so by making use of four waves of the Linked Personnel Panel (LPP), a large and representative German matched employer-employee data set. As compared to prior studies, these data enable us to both make more causal statements by employing various fixed effects and to test the external validity using a representative data set.

Our results show a robust positive and statistically significant effect of the presence of a performance management and evaluation process (PMEP) on work engagement. When dividing the PMEP into its single components, we find a positive and statistically significant effect of AIs on work engagement. Furthermore, our results show a positive *additional* impact of TAs on work engagement. This effect is statistically significant in all but our individual fixed effects estimations. We explicitly discuss the challenge of analyzing isolated effects of performance management practices using firm data, namely the lag of within-variation: there are too few individuals in our data switching from having no TA to having one without jointly making the same switch in the AI variable. This makes it rather difficult to draw a causal statement about the additional effect of written target agreements on individual effort. In a next step, we present the results of a mediation analysis to learn more about the potential channels underlying our core results. We find that the direct effect of PMEP on work engagement is partially mediated by procedural fairness and goal clarity.

Of course, this study is not without weaknesses, the most important one being causality. We cannot be sure whether the effects we find are really causal, as there is no random assignment of employees into performance management practices. However, we employ various empirical specifications to move as closely to causality as possible, which confirm our baseline results. As described above, the main advantage of our data is that, due to the large sample size, the representativeness, and the panel structure of the data, we are able to make more generalizable statements across many different industries and firm sizes. As a consequence, however, the disadvantage is that we do not have information about the exact content of the AIs and the respective targets. In order to get a more complete picture of the effects of performance management on employee effort, we therefore regard our study as a good complement to prior single-firm econometric case studies and lab and field experiments.

Appendix A

A.1 Work Engagement - List of Items

Each of the following items should be answered on a five-point Likert scale from 1 (daily) to 5 (never):

1. At my work, I feel bursting with energy.
2. At my job, I feel strong and vigorous.
3. I am enthusiastic about my job.
4. My job inspires me.
5. When I get up in the morning, I feel like going to work.
6. I feel happy when I am working intensely.
7. I am proud of the work that I do.
8. I am immersed in my work.
9. I get carried away when I am working.

A.2 Descriptive Statistics

Table A.1: Descriptive Statistics - Main Dependent and Independent Variables

Variables	(1) N	(2) Mean	(3) Std. Dev.	(4) Min	(5) Max
Appraisal Interview	16,506	0.522	0.500	0	1
Target Agreement/PMEP	16,506	0.356	0.479	0	1
Engagement Index (standardized)	16,506	-0.00748	1.003	-3.206	1.588
Goal Clarity Index (standardized)	16,469	-0.00475	1.003	-2.792	4.563
Procedural Fairness (standardized)	16,392	-0.0142	1.011	-2.437	1.611
Engagement Index (non-standardized)	16,506	3.669	0.837	1	5
Engagement - Energy	16,506	3.388	1.006	1	5
Engagement - Strong	16,506	4.019	0.859	1	5
Engagement - Enthusiastic	16,506	3.755	1.027	1	5
Engagement - Inspiring	16,506	3.369	1.262	1	5
Engagement - Feel Like Working	16,506	3.499	1.133	1	5
Engagement - Happy	16,506	3.819	1.083	1	5
Engagement - Proud	16,506	4.088	1.007	1	5
Engagement - Immersed	16,506	3.623	1.155	1	5
Engagement - Carried Away	16,506	3.460	1.164	1	5
Procedural Fairness (non-standardized)	16,392	3.394	0.999	1	5
Goal Clarity - (non-standardized)	16,469	3.653	0.955	1	8
Goal Clarity - Long-Term Plans	16,475	3.558	1.196	1	8
Goal Clarity - Requirements & Objectives	16,500	3.747	1.048	1	8

Table A.2: Descriptive Statistics - Establishment-Level Control Variables

Variables	(1) N	(2) Mean	(3) Std. Dev.	(4) Min	(5) Max
Establishment-Level Controls:					
Establishment Independent	16,506	0.693	0.461	0	1
Industry - Manufacturing	16,506	0.299	0.458	0	1
Industry - Metal, Electronics, Automotive	16,506	0.402	0.490	0	1
Industry - Trade, Transportation, News	16,506	0.102	0.303	0	1
Industry - Business-Related Services	16,506	0.125	0.331	0	1
Industry - Information/Communication	16,506	0.0719	0.258	0	1
Region - North	16,506	0.192	0.394	0	1
Region - East	16,506	0.251	0.434	0	1
Region - South	16,506	0.276	0.447	0	1
Region - West	16,506	0.281	0.449	0	1
Size (Number of Employees) - Less Than 100	16,506	0.127	0.333	0	1
Size (Number Employees) - 100 to 249	16,506	0.231	0.422	0	1
Size (Number Employees) - 250 to 499	16,506	0.230	0.421	0	1
Size (Number Employees) - More Than 500	16,506	0.412	0.492	0	1
Principal Owner - Family/Founder	16,506	0.429	0.495	0	1
Principal Owner - Management /Entrepreneurship	16,506	0.151	0.358	0	1
Principal Owner - Financial Investor	16,506	0.0931	0.291	0	1
Principal Owner - Widely Held Stock Capital Market	16,506	0.110	0.312	0	1
Principal Owner - Government/ Public Sector	16,506	0.0233	0.151	0	1
Principal Owner - Other	16,506	0.194	0.395	0	1

Table A.3: Descriptive Statistics - Employee-Level Control Variables

VARIABLES	(1) N	(2) Mean	(3) Std. Dev.	(4) Min	(5) Max
Age Category - under 25 (0/1)	16,506	0.0385	0.192	0	1
Age Category - 25 to 39 (0/1)	16,506	0.233	0.423	0	1
Age Category - 40 to 54 (0/1)	16,506	0.521	0.500	0	1
Age Category - over 55 (0/1)	16,506	0.207	0.405	0	1
Bonuses/Extra Payments (0/1)	16,498	0.594	0.491	0	1
Education - None (0/1)	16,506	0.00418	0.0645	0	1
Education - Lower Secondary School (0/1)	16,506	0.220	0.414	0	1
Education - Intermediate Secondary School (0/1)	16,506	0.424	0.494	0	1
Education - University of Applied Sciences Entrance Qualification (0/1)	16,506	0.110	0.313	0	1
Education - General Higher Education Entrance Qualification (0/1)	16,506	0.235	0.424	0	1
Education - Other (0/1)	16,506	0.00685	0.0825	0	1
Female (0/1)	16,506	0.272	0.445	0	1
Fixed-Term Contract (0/1)	16,506	0.0451	0.208	0	1
Full Time/Part Time (0/1)	16,506	0.127	0.333	0	1
Net Income (in Euros)	16,506	2,418	1,842	1	74,221
Number Members Household	16,506	2.776	1.228	1	14
Serious Relationship (0/1)	16,506	0.841	0.366	0	1
Supervisor (0/1)	16,506	0.290	0.454	0	1
Training Qualification - None (0/1)	16,506	0.0210	0.143	0	1
Training Qualification - Apprenticeship (0/1)	16,506	0.462	0.499	0	1
Training Qualification - Vocational/ Business School (0/1)	16,506	0.0937	0.291	0	1
Training Qualification - Master Craftsman/ Technical College (0/1)	16,506	0.206	0.404	0	1
Training Qualification - University of Applied Sciences (0/1)	16,506	0.0992	0.299	0	1
Training Qualification - University (0/1)	16,506	0.114	0.318	0	1
Training Qualification - Other (0/1)	16,506	0.00436	0.0659	0	1
White Collar (0/1)	16,506	0.625	0.484	0	1

A.3 Additional Analyses

Table A.4: Effect Heterogeneity - Firm Size

	(1)	(2)	(3)	(4)
	<100	100-249	250-499	>499
Variables	Engagement Index (std.)			
PMEP	0.222***	0.304***	0.246***	0.244***
	(0.0837)	(0.0519)	(0.0458)	(0.0269)
Variable Pay	0.213***	0.0850*	0.0239	0.0330
	(0.0679)	(0.0491)	(0.0478)	(0.0345)
Establishment Controls	yes	yes	yes	yes
Employee Controls	yes	yes	yes	yes
Establishment FE × Year FE	yes	yes	yes	yes
Constant	-0.395**	-0.520***	-0.382***	-0.540***
	(0.153)	(0.0937)	(0.0871)	(0.0653)
Observations	1,812	3,656	3,771	6,787
R-squared	0.367	0.324	0.229	0.140

The dependent variable Engagement Index is an index containing the weighted average of nine items and is standardized. All underlying items are measured on a 5-point Likert scale (between 1 and 5). PMEP is a dummy variable taking the value 1 if an individual has both an AI and a TA. Variable Pay is a dummy taking the value 1 if an individual has a variable pay component. Establishment-level controls include industry (5 dummies), regional area (4 dummies), and establishment size (5 dummies). Employee controls comprise female (0/1), age (8 dummies), supervisory position (0/1), white-collar employee (0/1), full-time position (0/1), monthly net income, type of employment contract (fixed term/permanent), permanent relationship (0/1), highest level of training qualification (7 dummies), household size, and survey method (CAWI/CATI). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table A.5: Effect Heterogeneity - Industries

	(1) Processing	(2) Metal, Electrical Automotive	(3) Commerce, Traffic, & Communications	(4) Company-Rel. & Fin. Services	(5) ICT & Other Services
Variables	Engagement Index (std.)				
PMEP	0.270***	0.267***	0.285***	0.151***	0.261***
	(0.0443)	(0.0279)	(0.0892)	(0.0569)	(0.0637)
Variable Pay	0.0250	0.0282	0.172**	0.164**	0.100
	(0.0385)	(0.0352)	(0.0698)	(0.0733)	(0.0884)
Establishment Controls	yes	yes	yes	yes	yes
Employee Controls	yes	yes	yes	yes	yes
Establishment FE × Year FE	yes	yes	yes	yes	yes
Constant	-0.494***	-0.501***	-0.572***	-0.335**	-0.146
	(0.0772)	(0.0643)	(0.147)	(0.146)	(0.167)
Observations	4,790	6,549	1,591	1,971	1,125
R-squared	0.260	0.202	0.272	0.211	0.282

The dependent variable Engagement Index is an index containing the weighted average of nine items and is standardized. All underlying items are measured on a 5-point Likert scale (between 1 and 5). PMEP is a dummy variable taking the value 1 if an individual has both an AI and a TA. Variable Pay is a dummy taking the value 1 if an individual has a variable pay component. Establishment-level controls include industry (5 dummies), regional area (4 dummies), and establishment size (5 dummies). Employee controls comprise female (0/1), age (8 dummies), supervisory position (0/1), white-collar employee (0/1), full-time position (0/1), monthly net income, type of employment contract (fixed term/permanent), permanent relationship (0/1), highest level of training qualification (7 dummies), household size, and survey method (CAWI/CATI). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table A.6: Effect Heterogeneity - Worker Types

	(1) Non-Manager	(2) Manager	(3) Blue-Collar	(4) White-Collar
Variables	Engagement Index (std.)			
PMEP	0.269***	0.252***	0.344***	0.203***
	(0.0257)	(0.0403)	(0.0504)	(0.0281)
Variable Pay	0.0747**	0.0348	0.0255	0.0917***
	(0.0294)	(0.0406)	(0.0441)	(0.0281)
Establishment Controls	yes	yes	yes	yes
Employee Controls	yes	yes	yes	yes
Establishment FE × Year FE	yes	yes	yes	yes
Constant	-0.515***	-0.0784	-0.513***	-0.237***
	(0.0518)	(0.0923)	(0.0864)	(0.0606)
Observations	11,090	3,978	5,583	9,619
R-squared	0.240	0.328	0.303	0.247

The dependent variable Engagement Index is an index containing the weighted average of nine items and is standardized. All underlying items are measured on a 5-point Likert scale (between 1 and 5). PMEP is a dummy variable taking the value 1 if an individual has both an AI and a TA. Variable Pay is a dummy taking the value 1 if an individual has a variable pay component. Establishment-level controls include industry (5 dummies), regional area (4 dummies), and establishment size (5 dummies). Employee controls comprise female (0/1), age (8 dummies), supervisory position (0/1), white-collar employee (0/1), full-time position (0/1), monthly net income, type of employment contract (fixed term/permanent), permanent relationship (0/1), highest level of training qualification (7 dummies), household size, and survey method (CAWI/CATI). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table A.7: Clustering on the Establishment-Year Level

Variables	(1) Engagement Index (std.)	(2) Engagement Index (std.)	(3) Goal Clarity Index (std.)	(4) Procedural Fairness (std.)	(5) Engagement Index (std.)	(6) Engagement Index (std.)
PMEP	0.253***		0.318***	0.215***		0.148***
	(0.0195)		(0.0206)	(0.0225)		(0.0189)
Appraisal Interview (AI)		0.150***				
		(0.0272)				
Target Agreement (TA)		0.169***				
		(0.0260)				
Goal Clarity					0.223***	0.215***
					(0.00980)	(0.00985)
Procedural Fairness					0.197***	0.195***
					(0.00927)	(0.00928)
Variable Pay	0.0636***	0.0603***	0.190***	0.164***	-0.00280	-0.00774
	(0.0206)	(0.0205)	(0.0212)	(0.0201)	(0.0199)	(0.0199)
Establishment Controls	yes	yes	yes	yes	yes	yes
Employee Controls	yes	yes	yes	yes	yes	yes
Establishment FE $\times$ Year FE	yes	yes	yes	yes	yes	yes
Constant	-0.466***	-0.505***	-0.177***	-0.283***	-0.345***	-0.370***
	(0.0368)	(0.0368)	(0.0353)	(0.0373)	(0.0340)	(0.0342)
Observations	16,026	16,026	16,380	16,277	15,925	15,879
R-squared	0.225	0.227	0.279	0.239	0.310	0.313

The dependent variables are constructed as follows. Engagement Index contains the equally weighted average of nine items. Goal Clarity Index contains the equally weighted average of two items. Procedural Fairness contains one item. All items are measured on a 5-point Likert scale (between 1 and 5). The resulting variables are standardized. PMEP is a dummy variable taking the value 1 if an individual has both an AI and a TA. AI is a dummy variable taking the value 1 if an individual has an AI, while TA is a dummy taking the value 1 if she additionally has a TA. Variable Pay is a dummy taking the value 1 if an individual has a variable pay component. Establishment-level controls include industry (5 dummies), regional area (4 dummies), and establishment size (5 dummies). Employee controls comprise female (0/1), age (8 dummies), supervisory position (0/1), white-collar employee (0/1), full-time position (0/1), monthly net income, type of employment contract (fixed term/permanent), permanent relationship (0/1), highest level of training qualification (7 dummies), household size, and survey method (CAWI/CATI). Standard errors clustered on the establishment-year level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table A.8: Clustering on the Individual Level

Variables	(1) Engagement Index (std.)	(2) Engagement Index (std.)	(3) Goal Clarity Index (std.)	(4) Procedural Fairness (std.)	(5) Engagement Index (std.)	(6) Engagement Index (std.)
PMEP	0.253***		0.318***	0.215***		0.148***
	(0.0228)		(0.0208)	(0.0222)		(0.0216)
Appraisal Interview (AI)		0.150***				
		(0.0268)				
Target Agreement (TA)		0.169***				
		(0.0273)				
Goal Clarity					0.223***	0.215***
					(0.0104)	(0.0104)
Procedural Fairness					0.197***	0.195***
					(0.00994)	(0.00993)
Variable Pay	0.0636***	0.0603***	0.190***	0.164***	-0.00280	-0.00774
	(0.0219)	(0.0218)	(0.0206)	(0.0214)	(0.0207)	(0.0208)
Establishment Controls	yes	yes	yes	yes	yes	yes
Employee Controls	yes	yes	yes	yes	yes	yes
Establishment FE $\times$ Year FE	yes	yes	yes	yes	yes	yes
Constant	-0.466***	-0.505***	-0.177***	-0.283***	-0.345***	-0.370***
	(0.0463)	(0.0470)	(0.0412)	(0.0420)	(0.0426)	(0.0428)
Observations	16,026	16,026	16,380	16,277	15,925	15,879
R-squared	0.225	0.227	0.279	0.239	0.310	0.313

The dependent variables are constructed as follows. Engagement Index contains the equally weighted average of nine items. Goal Clarity Index contains the equally weighted average of two items. Procedural Fairness contains one item. All items are measured on a 5-point Likert scale (between 1 and 5). The resulting variables are standardized. PMEP is a dummy variable taking the value 1 if an individual has both an AI and a TA. AI is a dummy variable taking the value 1 if an individual has an AI, while TA is a dummy taking the value 1 if she additionally has a TA. Variable Pay is a dummy taking the value 1 if an individual has a variable pay component. Establishment-level controls include industry (5 dummies), regional area (4 dummies), and establishment size (5 dummies). Employee controls comprise female (0/1), age (8 dummies), supervisory position (0/1), white-collar employee (0/1), full-time position (0/1), monthly net income, type of employment contract (fixed term/permanent), permanent relationship (0/1), highest level of training qualification (7 dummies), household size, and survey method (CAWI/CATI). Standard errors clustered on the individual level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table A.9: Lagged Dependent Variable Specifications

Variables	(1) Engagement Index (std.)	(2) Engagement Index (std.)	(3) Goal Clarity Index (std.)	(4) Procedural Fairness (std.)	(5) Engagement Index (std.)	(6) Engagement Index (std.)
PMEP	0.0783***		0.201***	0.132***		0.0458*
	(0.0285)		(0.0354)	(0.0403)		(0.0278)
Appraisal Interview (AI)		0.114***				
		(0.0358)				
Target Agreement (TA)		0.0154				
		(0.0335)				
Goal Clarity					0.0917***	0.0885***
					(0.0161)	(0.0160)
Procedural Fairness					0.126***	0.126***
					(0.0152)	(0.0152)
Variable Pay	-0.00861	-0.0107	0.156***	0.0784**	-0.0350	-0.0349
	(0.0305)	(0.0305)	(0.0374)	(0.0338)	(0.0305)	(0.0304)
Lagged Dep. Var.	yes	yes	yes	yes	yes	yes
Establishment Controls	yes	yes	yes	yes	yes	yes
Employee Controls	yes	yes	yes	yes	yes	yes
Establishment FE $\times$ Year FE	yes	yes	yes	yes	yes	yes
Constant	-0.187***	-0.221***	-0.238***	-0.295***	-0.128**	-0.137**
	(0.0552)	(0.0557)	(0.0589)	(0.0652)	(0.0531)	(0.0539)
Observations	4,296	4,296	4,495	4,447	4,264	4,255
R-squared	0.631	0.632	0.492	0.398	0.654	0.654

The dependent variables are constructed as follows. Engagement Index contains the equally weighted average of nine items. Goal Clarity Index contains the equally weighted average of two items. Procedural Fairness contains one item. All items are measured on a 5-point Likert scale (between 1 and 5). The resulting variables are standardized. PMEP is a dummy variable taking the value 1 if an individual has both an AI and a TA. AI is a dummy variable taking the value 1 if an individual has an AI, while TA is a dummy taking the value 1 if she additionally has a TA. Variable Pay is a dummy taking the value 1 if an individual has a variable pay component. Establishment-level controls include industry (5 dummies), regional area (4 dummies), and establishment size (5 dummies). Employee controls comprise female (0/1), age (8 dummies), supervisory position (0/1), white-collar employee (0/1), full-time position (0/1), monthly net income, type of employment contract (fixed term/permanent), permanent relationship (0/1), highest level of training qualification (7 dummies), household size, and survey method (CAWI/CATI). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table A.10: PMEP-Variable Pay Interaction

	(1)	(2)	(3)	(4)
Variables	Engagement Index (std.)			
PMEP	0.201***	0.236***	0.240***	0.0712*
	(0.0356)	(0.0370)	(0.0380)	(0.0406)
Variable Pay	0.0583***	0.0669***	0.0575**	-0.00805
	(0.0225)	(0.0255)	(0.0262)	(0.0305)
PMEP ×	-0.0119	0.0120	0.0194	-0.0173
Variable Pay	(0.0428)	(0.0437)	(0.0440)	(0.0423)
Establishment Controls	yes	yes	yes	yes
Employee Controls	yes	yes	yes	yes
Year FE	yes	yes		yes
Size FE	yes			yes
Establishment FE		yes		
Establishment FE ×			yes	
Year FE				
Individual FE				yes
Constant	-0.348***	-0.416***	-0.463***	0.221*
	(0.0645)	(0.118)	(0.0442)	(0.125)
Observations	16,498	16,498	16,026	16,498
R-squared (within)	0.077	0.189	0.225	0.028

The dependent variable Engagement Index is an index containing the weighted average of nine items and is standardized. All underlying items are measured on a 5-point Likert scale (between 1 and 5). PMEP is a dummy variable taking the value 1 if an individual has both an AI and a TA. Variable Pay is a dummy taking the value 1 if an individual has a variable pay component. Establishment-level controls include industry (5 dummies), regional area (4 dummies), and establishment size (5 dummies). Employee controls comprise female (0/1), age (8 dummies), supervisory position (0/1), white-collar employee (0/1), full-time position (0/1), monthly net income, type of employment contract (fixed term/permanent), permanent relationship (0/1), highest level of training qualification (7 dummies), household size, and survey method (CAWI/CATI). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table A.11: PMEP Index

Variables	(1) Engagement Index (std.)	(2) Goal Clarity Index (std.)	(3) Procedural Fairness (std.)	(4) Engagement Index (std.)	(5) Engagement Index (std.)
PMEP	0.132***	0.197***	0.141***		0.0645***
	(0.00988)	(0.0103)	(0.0103)		(0.00944)
Goal Clarity				0.223***	0.214***
				(0.0107)	(0.0108)
Procedural Fairness				0.196***	0.194***
				(0.00966)	(0.00967)
Establishment Controls	yes	yes	yes	yes	yes
Employee Controls	yes	yes	yes	yes	yes
Establishment FE × Year FE	yes	yes	yes	yes	yes
Constant	-0.530***	-0.231***	-0.309***	-0.347***	-0.418***
	(0.0432)	(0.0396)	(0.0389)	(0.0392)	(0.0398)
Observations	16,026	16,380	16,277	15,934	15,879
R-squared	0.226	0.282	0.241	0.310	0.313

The dependent variables are constructed as follows. Engagement Index contains the equally weighted average of nine items. Goal Clarity Index contains the equally weighted average of two items. Procedural Fairness contains one item. All items are measured on a 5-point Likert scale (between 1 and 5). The resulting variables are standardized. PMEP Index in an index variable countimg the number of items (AIs, TAs, and Variable Pay) that an individual reports having and thus varies between 0 and 3. Establishment-level controls include industry (5 dummies), regional area (4 dummies), and establishment size (5 dummies). Employee controls comprise female (0/1), age (8 dummies), supervisory position (0/1), white-collar employee (0/1), full-time position (0/1), monthly net income, type of employment contract (fixed term/permanent), permanent relationship (0/1), highest level of training qualification (7 dummies), household size, and survey method (CAWI/CATI). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table A.12: PMEP Dummy Variable Comprising Variable Pay (Alternative I)

Variables	(1) Engagement Index (std.)	(2) Goal Clarity Index (std.)	(3) Procedural Fairness (std.)	(4) Engagement Index (std.)	(5) Engagement Index (std.)
PMEP (Alternative I)	0.230***	0.324***	0.224***		0.121***
	(0.0230)	(0.0215)	(0.0221)		(0.0219)
Goal Clarity				0.223***	0.217***
				(0.0107)	(0.0107)
Procedural Fairness				0.196***	0.195***
				(0.00966)	(0.00971)
Establishment Controls	yes	yes	yes	yes	yes
Employee Controls	yes	yes	yes	yes	yes
Establishment FE × Year FE	yes	yes	yes	yes	yes
Constant	-0.410***	-0.0497	-0.178***	-0.347***	-0.360***
	(0.0421)	(0.0380)	(0.0369)	(0.0392)	(0.0391)
Observations	16,026	16,380	16,277	15,934	15,879
R-squared	0.223	0.272	0.235	0.310	0.312

The dependent variables are constructed as follows. Engagement Index contains the equally weighted average of nine items. Goal Clarity Index contains the equally weighted average of two items. Procedural Fairness contains one item. All items are measured on a 5-point Likert scale (between 1 and 5). The resulting variables are standardized. PMEP is a dummy variable that takes the value 1 if an individual reports jointly having an AI, a TA, and variable pay and zero otherwise. The control group contains all individuals that report that they are not covered by one or more of these items. Establishment-level controls include industry (5 dummies), regional area (4 dummies), and establishment size (5 dummies). Employee controls comprise female (0/1), age (8 dummies), supervisory position (0/1), white-collar employee (0/1), full-time position (0/1), monthly net income, type of employment contract (fixed term/permanent), permanent relationship (0/1), highest level of training qualification (7 dummies), household size, and survey method (CAWI/CATI). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table A.13: PMEP Dummy Variable Comprising Variable Pay (Alternative II)

Variables	(1) Engagement Index (std.)	(2) Goal Clarity Index (std.)	(3) Procedural Fairness (std.)	(4) Engagement Index (std.)	(5) Engagement Index (std.)
PMEP (Alternative II)	0.356***	0.539***	0.422***		0.163***
	(0.0428)	(0.0412)	(0.0428)		(0.0413)
Goal Clarity				0.223***	0.211***
				(0.0107)	(0.0151)
Procedural Fairness				0.196***	0.207***
				(0.00966)	(0.0137)
Establishment Controls	yes	yes	yes	yes	yes
Employee Controls	yes	yes	yes	yes	yes
Establishment FE × Year FE	yes	yes	yes	yes	yes
Constant	-0.497***	-0.236***	-0.400***	-0.347***	-0.359***
	(0.0647)	(0.0581)	(0.0558)	(0.0392)	(0.0616)
Observations	7,873	8,023	7,974	15,934	7,793
R-squared	0.284	0.358	0.312	0.310	0.367

The dependent variables are constructed as follows. Engagement Index contains the equally weighted average of nine items. Goal Clarity Index contains the equally weighted average of two items. Procedural Fairness contains one item. All items are measured on a 5-point Likert scale (between 1 and 5). The resulting variables are standardized. PMEP is a dummy variable that takes the value 1 if an individual reports jointly having an AI, a TA, and variable pay and zero otherwise. Individuals that miss one but not all items are dropped from the sample such that the control group contains only individuals that are covered by neither one of these measures. Establishment-level controls include industry (5 dummies), regional area (4 dummies), and establishment size (5 dummies). Employee controls comprise female (0/1), age (8 dummies), supervisory position (0/1), white-collar employee (0/1), full-time position (0/1), monthly net income, type of employment contract (fixed term/permanent), permanent relationship (0/1), highest level of training qualification (7 dummies), household size, and survey method (CAWI/CATI). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table A.14: Mediation Analysis with Individual Fixed Effects

Variables	(1) Goal Clarity Index (std.)	(2) Procedural Fairness (std.)	(3) Engagement Index (std.)	(4) Engagement Index (std.)	(5) Engagement Index (std.)	(6) Engagement Index (std.)
PMEP	0.0834**	0.0657*		0.0493*	0.0537**	0.0461*
	(0.0354)	(0.0355)		(0.0257)	(0.0255)	(0.0236)
Goal Clarity Index			0.0859***	0.113***		0.0846***
			(0.0142)	(0.0115)		(0.0143)
Procedural Fairness			0.102***		0.120***	0.102***
			(0.0123)		(0.0103)	(0.0123)
Variable Pay	0.0553	0.0256	-0.0199	-0.0189	-0.0143	-0.0196
	(0.0347)	(0.0362)	(0.0245)	(0.0239)	(0.0237)	(0.0246)
Establishment Controls	yes	yes	yes	yes	yes	yes
Employee Controls	yes	yes	yes	yes	yes	yes
Year FE	yes	yes	yes	yes	yes	yes
Size FE	yes	yes	yes	yes	yes	yes
Individual FE	yes	yes	yes	yes	yes	yes
Constant	0.0389	-0.123	0.235*	0.215	0.240*	0.233*
	(0.168)	(0.173)	(0.127)	(0.137)	(0.137)	(0.121)
Number of Employees	12,251	12,188	11,992	12,040	11,990	11,974
Observations	16,839	16,740	16,395	16,461	16,384	16,351
R-squared (within)	0.019	0.011	0.069	0.048	0.059	0.070

The dependent variables are constructed as follows. Engagement Index contains the equally weighted average of nine items. Goal Clarity Index contains the equally weighted average of two items. Procedural Fairness contains one item. All items are measured on a 5-point Likert scale (between 1 and 5). The resulting variables are standardized. PMEP is a dummy variable taking the value 1 if an individual has both an AI and a TA. Variable Pay is a dummy taking the value 1 if an individual has a variable pay component. Establishment-level controls include industry (5 dummies), regional area (4 dummies), and establishment size (5 dummies). Employee controls comprise female (0/1), age (8 dummies), supervisory position (0/1), white-collar employee (0/1), full-time position (0/1), monthly net income, type of employment contract (fixed term/permanent), permanent relationship (0/1), highest level of training qualification (7 dummies), household size, and survey method (CAWI/CATI). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Chapter 3

Changing the Pyramids: The Impact of Broadband Internet on Firm Employment Structures*

3.1 Introduction

Over the past decades, rapid technological developments have generated complex changes in the demand for labor and the task composition of jobs (Autor, 2015). For advanced economies, empirical studies have found computerization and automation to result in labor market polarization and increasing inequality (Autor, Levy and Murnane, 2003; Goos and Manning, 2007; Acemoglu and Autor, 2011; Michaels, Natraj and Van Reenen, 2014; Acemoglu and Restrepo, 2018). The evidence on the impacts of broadband internet as a more recent information and communications technology (ICT) is mixed and it is found to result in skill biased technological change (SBTC) in developed economies (Akerman, Gaarder and Mogstad, 2015) and decreasing employment inequality in poor countries (Hjort and Poulsen, 2019). Bloom et al. (2014) explicitly point towards the possibility that different types of technologies might affect labor markets differently. This is of concern for policy makers, because if consequences for employment are technology-dependent, the direction of effects is much less clear. While the focus of prior research has largely been on overall employment and employment structures in geographical areas, there is little evidence on the impact of recent digital technologies on firm-level demand for different types of workers, particularly in an emerging economy context.

In this paper we provide novel, causal evidence on the way in which a new, digital technology affects the employment structure of firms.[1] We focus on broadband internet, which is arguably one of the most important new technologies introduced in the last

*This chapter is based on Barbosa et al. (2021).

[1] The data we analyze is on the establishment level. For the sake of readability, we use the terms "firm", "establishment", and "plant" interchangeably.

decades and which is recognized for its multifaceted effects.[2] Our context is broadband introduction in Brazilian cities at the beginning of the 2000s, at a time when it was still in its infancy. Studying this relationship in Brazil is particularly interesting due to the country's high level of job inequality and since it is one of the largest and most important middle-income countries worldwide. We combine data from administrative and public sources to obtain detailed employer-employee information, enabling us to identify occupational layers,[3] employee education levels, firm characteristics, and information on the infrastructure used to provide internet access.

Estimating the impacts of broadband internet is challenging as broadband infrastructure is not randomly distributed across space and firms have an incentive to self-select to connected locations. Similarly to European countries, Brazil's institutional background offers an interesting natural experiment to assess causality. We deal with endogeneity by exploiting both the gradual diffusion of broadband asymmetric digital subscriber line (ADSL) technology across state capitals and technological constraints creating exogenous variation in broadband access. In particular, the internet signal deteriorates in distance to the main distribution frame[4] (MDF) and ADSL hence is not feasible anymore if a certain threshold is exceeded. This allows us to compare firms within to firms outside of the technologically feasible perimeter at the extensive margin and estimate intention-to-treat effects.[5]

We first document that fast internet significantly reshapes both the occupational and educational pyramids.[6] Establishments react to broadband availability by increasing the share of managers and of operational positions at the very bottom of the firm hierarchy. The strongest effect is found at the top of the hierarchy. Relative to the baseline mean, our estimates imply an average increase in the share of management positions of 11%. These rearrangements happen at the expense of basically all other occupational layers. Furthermore, establishments rearrange educational layers in response to fast internet as

[2] To name a few, broadband internet has shown to affect educational outcomes (Faber, Sanchis-Guarner and Weinhardt, 2015; Sanchis-Guarner, Montalbán and Weinhardt, 2021), political outcomes (Falck, Gold and Heblich, 2014; Miner, 2015; Campante, Durante and Sobbrio, 2017), health (Billari, Giuntella and Stella, 2018), protests (Amorim, Lima and Sampaio, 2018), corruption (Andersen et al., 2011), crime (Bhuller et al., 2013; Diegmann, 2019), economic growth (Czernich et al., 2011), and trade (Kneller and Timmis, 2016; Barbero and Rodriguez-Crespo, 2018; Malgouyres, Mayer and Mazet-Sonilhac, 2019).

[3] Following Caliendo, Monte and Rossi-Hansberg (2015), we define a layer as a group of employees with largely similar knowledge and who perform similar tasks.

[4] Main distribution frames are the termination points for copper telephone wires and responsible for connecting users to the operator's main lines via a local exchange. They are part of a complex internet infrastructure with backbones at its core and backhauls as intermediate links to smaller cables at the outskirts of the networks.

[5] The setup is closely related to the one by Falck, Gold and Heblich (2014), but instead of measuring the distance between MDFs and municipality centroids, our data permits exploiting the exact geodistance between firm and MDF locations.

[6] We follow Garicano (2000) in calling the occupational firm structure a pyramid and extend this denomination to educational groups. This term implies that firms are structured in hierarchical layers and that each layer is smaller than the one below.

follows: they increase the shares of both high- and low-educated employees while the share of medium-educated employees declines. The effect is strongest at the top of the pyramid, such that the share of high-educated workers increases by 7%. Our dynamic treatment effects are robust and show that these structural changes are persistent over time.

Even though Brazil is a large emerging economy with a very different labor force as compared to developed countries, some of the responses to technological change we find are relatively similar (Levy and Murnane, 1992; Autor, Katz and Kearney, 2006; Spitz-Oener, 2006; Goos and Manning, 2007; Goos, Manning and Salomons, 2009, 2014; Autor and Dorn, 2013). This pattern sharply contrasts with previous evidence from emerging economies, which has found skill-bias favoring high-educated workers or an absence of labor market polarization for Latin American and Asian countries (Goldberg and Pavcnik, 2007; Amiti and Cameron, 2012; Messina, Pica and Oviedo, 2016; Almeida, Fernandes and Viollaz, 2017). However, we find that polarization is concentrated at the very extremes of the occupational pyramid, a finding that diverges from previous worldwide evidence. For instance, the shares of many white-collar positions and service and seller occupations decline. In addition, we find the share of machine operators and assemblers, which prior literature documents to be easily substituted by automation technology, to rise. Our results underscore that the impacts of different technologies on employment structures are not homogeneous but heavily technology- and context-dependent.

After providing an exhaustive set of robustness tests to attest our results, we dig deeper to understand the reasons behind the observed changes in employment patterns. Literature has largely drawn on SBTC and routinization hypotheses (proposed by Autor, Levy and Murnane (2003), henceforth ALM). While SBTC predicts a rise in relative demand for skilled jobs and workers, the ALM hypothesis posits that demand for jobs comprising a large fraction of non-programmable nonroutine tasks should rise in detriment to those with more easily programmable routine tasks. We employ two different analyses to check consistency of our results with these theories. First, we investigate demand for each worker type in terms of the number of workers. Second, we examine heterogeneity of results across industries.

SBTC is not sufficient to explain our results, since high-speed internet introduction substitutes medium-educated workers in a stronger fashion as compared to low-educated ones. This substitution appears to be stronger in larger firms and in industries where this type of worker is more abundant. Furthermore, high-skilled jobs other than managers do not benefit from this technological change in any industry. While some of our results seem to be in line with the ALM hypothesis, others are not. In particular, occupations with a highly nonroutine task content are not benefited in every industry. Furthermore, firms in historically routine-intensive sectors should experience stronger declines in these type of jobs, which we do not find. We thus conclude that neither SBTC nor ALM is able to fully explain the observed structural changes. These exercises also reveal that broadband internet leads to job losses in all layers (which are weaker for managers and high-educated workers), thereby leading to a decrease in overall firm size. For the average establishment, our estimates imply that internet substitutes for 1.9 jobs, corresponding to a decrease in firm size of 7%. This indicates that broadband has the potential to substitute for labor working in a variety of jobs and conducting heterogeneous tasks. For a very different environment and technological phenomenon, we find an analogous job loss to the one caused by robots in US labor markets (Acemoglu and Restrepo, 2020).

As the relative increase in the management layer is by far our most consistent result, we put a larger focus on the top hierarchical layer. Bloom et al. (2014) show that, in case new technology reduces within-firm communication costs, letting production workers deal with the most common problems while delegating nonroutine issues to managers becomes more attractive. In this spirit, we subdivide the management layer into top and middle management and investigate which positions exactly drive the expansion of the management layer. Results show that only middle management increases its employment share independent of alleged differences in coordination needs across firms. This is consistent with "management by exception" predictions, as middle managers are known to be problem solvers, to provide guidance, and to execute organizational plans (Delmestri and Walgenbach, 2005). These findings suggest that, at least in the initial stage, broadband internet has primarily been used as a *communications* rather than an *information* technology, as this type of technology can cause centralization in decision making, lower worker-autonomy, and hence a more vertical organization.

Motivated by the unique patterns we document, we analyze whether or not firms benefit from the technological change and resulting reorganization. First, we estimate that internet connectivity leads establishments to hire significantly less and strategically promote high-skilled workers to management positions. Despite these promotions, the overall wage bill declines, such that the internal reorganizations jointly with the overall decrease in firm size causes significant labor cost savings. Next, we provide evidence on the ways in which broadband affects firm survival, our proxy for firm performance, by examining firms that entered the market shortly before broadband introduction. Results indicate a decline in short-run establishment mortality (especially for less experienced firms), which is partially to fully mediated by the structural changes we observe. Taken together, we provide suggestive evidence that firms reorganize their labor structure in an efficiency-seeking way and are able to realize short-run performance gains.

Our paper relates to various streams of literature. Besides the aforementioned studies, this work is linked to the empirical literature on the impact of technological change on labor market outcomes (Almeida, Corseuil and Poole, 2017; Dettling, 2017; Poliquin, 2020; Gürtzgen et al., 2021; Bhuller, Kostol and Vigtel, 2021; Pérez, Fernández-Macías and Winter-Ebmer, 2021), firm organization (Bresnahan, Brynjolfsson and Hitt, 2002; Acemoglu et al., 2007; Caliendo and Rossi-Hansberg, 2012; Caliendo et al., 2020), and productivity and firm performance (Bartel, Ichniowski and Shaw, 2007; Commander, Harrison and Menezes-Filho, 2011; Colombo, Croce and Grilli, 2013; DeStefano, Kneller and Timmis, 2018; Cariolle, Goff and Santoni, 2019; Kriebel and Debener, 2019). We also relate to the growing literature on division of labor. We complement Tian (2019)'s work by showing that the effect of broadband technology on management is not unambiguous.[7]

We directly contribute to the literature on knowledge-based hierarchies (Rosen, 1982; Garicano, 2000; Bloom et al., 2014) and organizational change (Caroli and Reenen, 2001; Crespi, Criscuolo and Haskel, 2007) by highlighting the ways in which occupational layers are reorganized as a consequence of new technologies, ultimately resulting in a lower firm mortality. To our best knowledge, our study is the first to provide evidence that changes in employment structures are a channel linking technology to firm survival.

We add to the literature in important ways. By employing a quasi-experimental research design which makes causal interpretation plausible, we provide new insights on the way in which availability of new technology influences within-firm reallocation of jobs and labor. We offer an alternate explanation on the way in which technology changes employment patterns, which highlights firms' need for reorganization. Finally, our results suggest that the impacts of both different and related technologies are not always homogeneous but context-specific. We hence alert for potential unintended side effects of upcoming technological advances on employment.

The rest of the paper unfolds as follows. Section 3.2 provides background information on broadband internet emergence in Brazil and the general economic situation at the time. Section 3.3 discusses the data in details and describes our empirical strategy. This section also provides some descriptive trends in employment structures during our period of analysis. Section 3.4 presents our main results together with several robustness tests, mechanisms underlying structural changes, and the analysis of labor movements, payroll, and firm survival. We conclude in Section 3.5.

3.2 Institutional Background

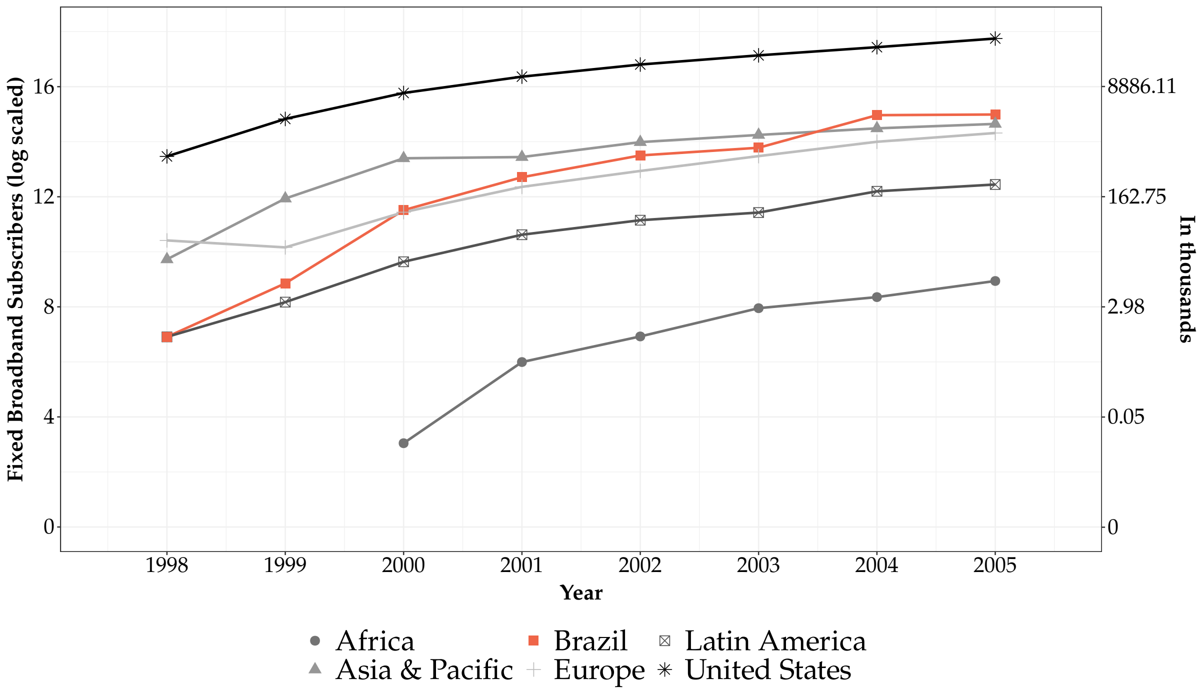
3.2.1 Diffusion of Broadband Technology in Brazil

The beginnings of internet in Brazil date back to the late eighties, when the federal government started investing into network layers to exclusively serve selected universities

[7]This paper also employs Brazilian data and shows that broadband causes a decline in the share of managers. We provide two reasons for which our results deviate. First, while the author explores the improvement of broadband infrastructure between 2012 and 2014, at a time when the technology and its potential were well known, we explore broadband introduction in the early 2000s, when the technology was still new and adoption involved higher cost of knowledge acquisition. Second, we specifically focus on larger cities.

and research institutions. In the aftermath of the privatization of the state-owned operator Telebras in 1998, which previously held the monopoly on the entire Brazilian fixed-line telecommunications network, faster ADSL connections were made available.[8] From the early broadband era on, Brazil has ranked among the top fifteen nations in terms of fixed broadband internet subscription numbers and among the most dynamic countries in the ICT Development Index (Tamayo, 2003). Figure 3.1 depicts the development in subscription numbers of residential and business users during the period of analysis and shows rapid growth, as compared to other world regions.

Figure 3.1: Evolution of Broadband Internet Usage across Continents



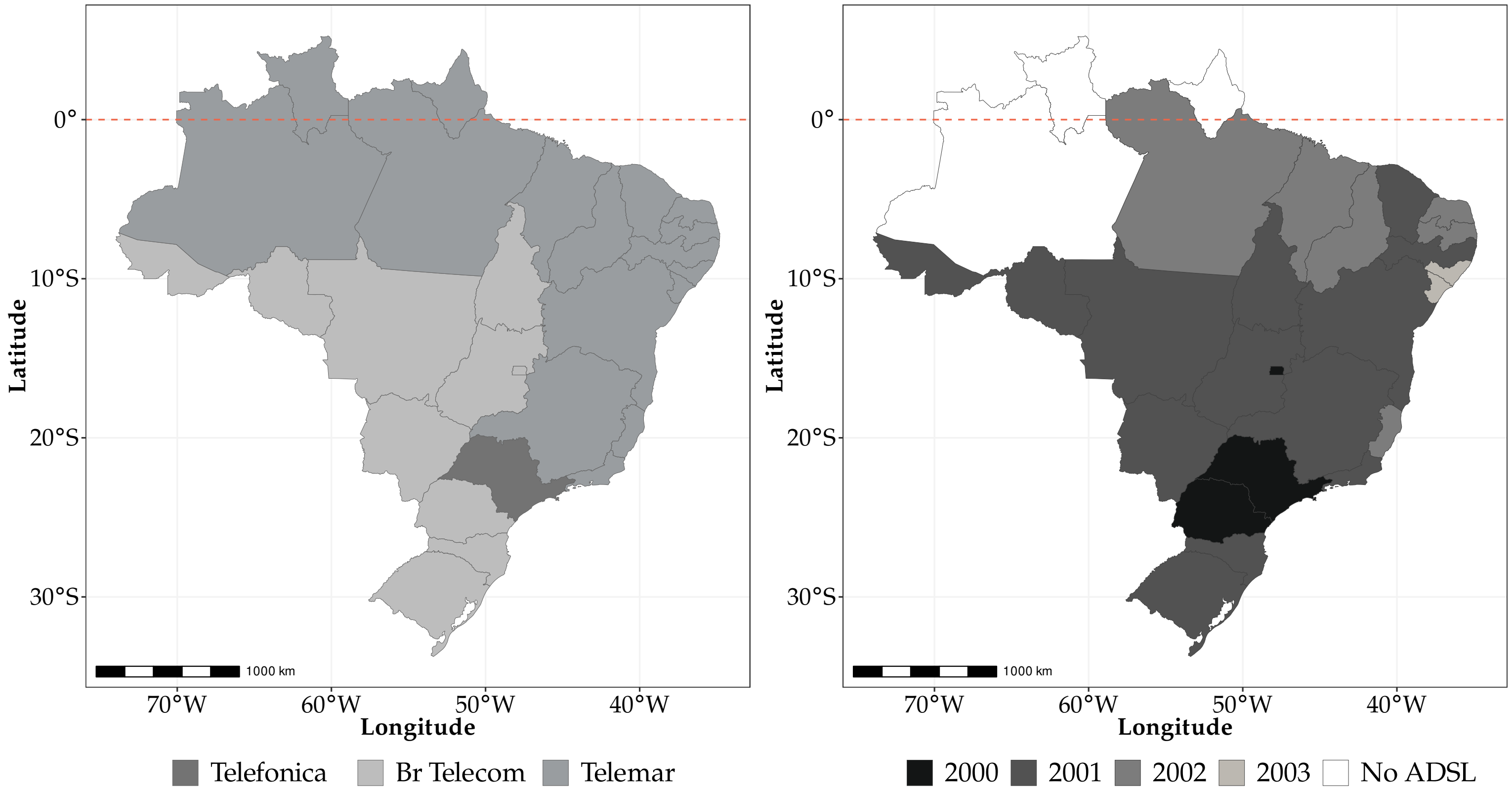
Notes: The graph shows adoption of broadband internet featuring connection speeds exceeding 256 kbps by both commercial and residential users over time. The gray lines depict the evolution across continents while the red line depicts broadband adoption in Brazil. *Source:* Graph based on Enterprise Surveys Data from the World Bank.

As part of the privatization event, Brazil was divided geographically into three licensing areas for fixed-line communications, each one administered by one major telecommunications company operating as concessionaire: Brasil Telecom, Telefonica, and Telemar.[9] The left panel in Figure 3.2 graphically depicts the allocation of states to con-

[8]In the late nineties, some experiments with Integrated Services Digital Network (ISDN) technology were made (Knight, Feferman and Foditsch, 2016). As compared to dial-up connections, exhibiting a data rate of 56 kbps, ISDN doubled connection speed. Other technologies, such as cable modems, 3G, radio, and satellites gained greater importance only from the mid-2000s on (ANATEL, 2008). In later years, especially in the 2010s, investments were made to ameliorate and expand the backbone infrastructure.

[9]Embratel, which held the monopoly on international and long distance calls, was another big player.

Figure 3.2: Licensing Areas and Broadband Roll-Out across States



Notes: The left panel shows licensing areas of the three largest Brazilian telecommunication companies. Telefonica's licensing area consists of the state of São Paulo. Brasil Telecom covers all states in the South and Center Brazilian regions, and three states in the North region. Telemar's licensing area comprises the Northeastern states almost entirely, and the remaining states in the Southeastern and North regions. The right graph shows broadband internet roll-out across capitals of the highlighted states between 2000 and 2003. The blank areas represent Northern states without copper wires or backbone infrastructure to support broadband technology. *Source*: Graph based on data from ANATEL.

cessionaires and provides an idea of the vast concession areas covered by these de facto monopolies on fixed-line communications. The pre-existing telephone infrastructure these companies inherited from Telebras included the old MDFs and copper cable networks (Jensen, 2011; ANATEL, 2013). These structures gave them a competitive advantage, as they could be re-purposed for the provision of fast ADSL internet connections at low cost. Jointly with Internet Service Providers (ISPs),[10] the concessionaires developed products to serve private and business users. Depending on the licensing region, connection speed thereby ranged from 256 Kbps to 2 Mbps. Usage by businesses gradually increased as the telecommunications companies converted their telephone infrastructure and promoted their portfolio of internet services (Telemar, 2002; Commander, Harrison and Menezes-Filho, 2011). In absolute terms, prices were high at the beginning but low relative to other Latin American countries. Broadband diffusion efforts were targeted at the largest urban areas first, usually state capitals and other main cities.[11] The right panel of Figure 3.2 shows the progressive roll-out of the technology across states.The three concessionaires were able to maintain their dominant position in the ADSL market for years. In 2006, each of the three companies still had a market share of more than 93% in their respective licensing area.[12]

A well-known feature of ADSL technology is that copper wires allow for fast data transmission over just a limited range. The data signal gradually weakens the farther away the user is from the MDF. Above distances of 4 to 4.2 km, ADSL technology is not feasible anymore (Falck, Gold and Heblich, 2014; Knight, Feferman and Foditsch, 2016).[13] Since the Brazilian infrastructure was, as compared to other countries (Campante, Durante and Sobbrio, 2017; Bauernschuster, Falck and Woessmann, 2014), subject to additional obstacles,[14] telecommunications companies opted to restrict ADSL provision in order to maintain service quality. In particular, access was usually restricted to 2.5 km in the early broadband period.[15] The crucial factors allowing establishments to access

However, this company entered the broadband market only in a few cities in the South and Southeastern regions and not before 2006, when it partnered with a cable TV operator to provide internet connections via cable modem technology.

[10]By regulation, the specialized ISPs directly served the end user and obtained the right to do so from the concessionaire.

[11]Internet was less pervasive in areas with lower population density, since costs of infrastructure expansion were significantly higher (Jensen, 2011).

[12]Information on broadband deployment in Brazil can be found in management and administrative reports provided by the aforementioned telecommunications companies (Telefonica, 2001; Brasil Telecom, 2001; Telemar, 2002).

[13]This maximum distance varies across countries. In the UK, for instance, broadband internet connections are possible up to a maximum distance of just 2 km (DeStefano, Kneller and Timmis, 2018).

[14]Differently from most other countries using ADSL infrastructure, copper wires in Brazil are above surface. This solution is relatively cheap, but infrastructure deteriorates much faster as a result of, for instance, climate, topography, and oxygen induced corrosion, especially in coastal regions. Thus, the combination of the listed obstacles with the poor quality of the preexisting wire network and the large demand in metropolitan areas for broadband services causes the physical signal range to deteriorate, even for connections up to 2Mbps (Jayant, 2018).

broadband internet were thus the distance to the closest MDF coupled with the timing of ADSL introduction in the state of location. These features form the foundation of our identification strategy.

3.2.2 Economic Background

Between 1985 and 1999, Brazil experienced a long period of hyperinflation and economic stagnation. The measures taken to stabilize the economy combined monetary and fiscal policies and comprised a new floating currency, new labor regulations, liberalization in many sectors, and privatization of formerly state-owned enterprises. Brazil's economy, however, just started to show signs of recovery in response to these policies in the early 2000s. Inevitably, these changes in the economic environment had important implications for firms' ability to thrive in the market. In 2003, firms aged up to two (four) years e.g. had a survival rate of 50% (40%) (Angelelli, Moudry and Llisterri, 2006).

As compared to its counterpart in developed economies, the Brazilian labor force was characterized by relatively low average education levels during the period of analysis. In particular, most individuals did not achieve a high school degree. During the economic transition, the labor market experienced important structural changes and new trends in worker composition and job skills emerged. The share of employees with higher educational attainment, for instance, increased by roughly 50% between 1995 and 2005.[16] Due to unequal regional access to education and a higher quality of the local labor force, the Southern and Southeastern regions spearheaded this growth (IBGE, 2006). Despite this increase in the number and fraction of high-educated workers, average educational attainment is still low as compared to both developed countries and other Latin American countries such as Argentina or Chile (Fernández and Messina, 2018).

3.3 Data and Empirical Framework

3.3.1 Data Sources and Variables

We employ data from several sources. Information on establishments comes from the Annual Social Information Report (RAIS). This administrative data is based on an annual survey sent by the Brazilian Ministry of Labor and Employment to all formal Brazilian employers. Completion and submission of the questionnaire is mandatory. It contains detailed information on the universe of all formally employed workers in Brazil (around 60 million per year) and the establishments they work in, including the full establishment

[15]See https://www.folhadelondrina.com.br/geral/linha-speedy-novo-servico-da-telefonica-so-atendeu-a-3_500-pedidos-265464.html (November 25, 2021).

[16]Information obtained from the Brazilian National Household Sample Survey (PNAD).

address, which we observe from 2002 on,[17] and other characteristics, such as industry and legal form. We enrich these data with information on the establishments' year of market entry from the Brazilian Fiscal Authority data base and on exporting status using data from the Brazilian Secretariat of Foreign Trade (SECEX).

We use RAIS to construct all establishment-level variables and also take advantage of the data set's panel structure. Our main outcome variables relate to occupational skill and employee education levels. We construct occupational groups by first translating the Brazilian Classification of Occupations (CBO) into the International Standard Classification of Occupations 1988 (ISCO-88). In accordance with ISCO-88, we then group jobs into hierarchical layers based on job titles and divide them into "managers", "professionals", "technicians and associate professionals", "clerks", "sellers and service workers", "craft and related trades workers", "machine operators and assemblers", and "elementary occupations".[18,19] Details on the way in which occupations are grouped into hierarchical layers are depicted in Table B.1 in Appendix B. We follow the literature in grouping employees into three educational brackets based on educational achievement. We define an individual as being high-educated if the highest degree attained is a college degree (bachelor, master, or PhD), as medium-educated if it is a high school degree, and as low-educated if a high school degree has not been attained. Additionally, we also analyze firm size,[20] hiring and promotion behavior, payroll, and firm survival.

We furthermore employ data from the Brazilian National Telecommunications Agency (ANATEL), which comprises precise information on the coordinates (latitude and longitude) of all Brazilian MDFs.[21] We use this information in conjunction with firm addresses to calculate geodistances between firms and MDFs. Even though we lack information on actual broadband adoption, the georeferenced data allows us to determine connectivity of firms.

3.3.2 Sample Definition and Descriptive Statistics

We restrict our analysis to establishments located in the metropolitan areas of state capitals since, according to official reports published by the telecommunications companies, ADSL

[17]For most of the sample, we therefore have to assume that firms remain at one location during our observation period. We conduct a falsification test using two states treated in 2003 to check the role of endogenous location decisions. Results, which are discussed in more detail in Appendix B, remain qualitatively similar, indicating this is not an issue.

[18]For the sake of clarity, we use "technicians" instead of "technicians and associate professionals", "machine operators" instead of "machine operators and assemblers", and sometimes aggregate the the lowest four occupational layers into a group which we call "other operational."

[19]Figure B.2 in Appendix B shows the wage structure of these occupational layers and confirms the inverted pyramid shape found by previous literature (Tãg, 2013).

[20]We winsorize this variable by its 99 percentile value to deal with large outliers. Results are robust to other winsorization decisions and are available upon request.

[21]The data can be retrieved from ANATEL's website using the following link: `https://www.gov.br/anatel/pt-br/dados` (November 25, 2021).

technology was made available in the biggest cities of their respective licensing areas first.[22] Figure B.3 in Appendix B depicts the spacial distribution of all MDFs in Brazil and those located within metropolitan areas (black dots). As we know the year of broadband roll-out for each state, we can precisely identify the treatment year for each capital.[23] A downside is that we have to define one treatment year (after which MDFs are able to provide fast internet connection) for all MDFs located in a given capital, as we do not have precise information on the date of conversion of single MDFs to Digital Subscriber Line Access Multiplexers (DSLAMs).[24] We are aware that this might introduce bias, but in the worst case, this should bias our estimates downwards.

Our sample comprises firms in the time span between 1996 and 2005. We start in 1996 since this year is close to the year of Telebras privatization and since we want to keep our analysis free of the disturbances caused by the hyperinflation prior to and currency reform in 1994. Furthermore, we regard the time between this year and the treatment years as being sufficient to check for anticipation effects. We end in 2005 to keep our analysis free from the influences of other technologies that became available in later years. To link firms to MDFs, we convert firm addresses to geolocations. This geocoding process generates heterogeneous levels of match accuracy.[25] We only keep matches in the best accuracy groups, as the identification of the impact of interest requires fine-grained information on firm locations. The firms remaining in our sample represent roughly 90% of all matched plants. We then calculate geodistances between each firm and the closest MDF, as it is most likely that firms are connected to the closest source. We exclude the following types of firms from our sample: microentrepreneurs with less than 10 employees, government firms, religious institutions, firms from the primary sector, and firms that we observe migrating to a different location. In alternate specifications, we use a more balanced sample comprising only plants active during the entire time span.

Table B.2 in Appendix B shows some general characteristics of the sampled firms. The main sample contains 224,564 matched establishments. The year of firm entry is 1992 on average, indicating that the sample largely comprises firms that have entered the market long before broadband arrival. Firms are mainly active in the commerce and service sectors (84%), with slightly higher averages in cities located in Telemar's licensing area.[26] Manufacturing firms are more prevalent in São Paulo, which is the most

[22]Since such reports do not exist for every state, we hand-collect the missing information from renowned news websites.

[23]Note that we hence neither have information on ADSL diffusion in other cities nor precise dates of arrival in each capital.

[24]To our best knowledge, there is no available data on the date of MDF conversion. Since the state capitals' fixed-line telephone infrastructures are very old (ANATEL, 2013), we can be certain that they have already been installed at the time of broadband arrival and that location decisions are hence independent of broadband technology.

[25]This is because the address in the RAIS database is not always exact such that, for instance, the building number is missing in some cases.

[26]See Table B.3 in Appendix B for the detailed categorization of industries.

Table 3.1: Summary Statistics

	All		Pre-broadband		Post-Broadband	
	mean	s.d.	mean	s.d.	mean	s.d.
	(1)	(2)	(3)	(4)	(5)	(6)
Firm size	27.09	52.97	27.14	53.29	27.06	52.77
Occupations						
Managers	0.0492	0.1138	0.0366	0.0909	0.0571	0.1254
Professionals	0.0441	0.1493	0.0499	0.1556	0.0405	0.1451
Technicians	0.0841	0.1709	0.0876	0.1751	0.0818	0.1681
Clerks	0.2011	0.2405	0.2074	0.2445	0.1973	0.2378
Sellers and service wkrs	0.1946	0.2938	0.1979	0.2982	0.1925	0.2910
Craft and rel. trades wkrs	0.1428	0.2680	0.1422	0.2713	0.1431	0.2659
Machine operators	0.1004	0.2120	0.0978	0.2115	0.1020	0.2123
Elementary occup.	0.1546	0.2570	0.1643	0.2642	0.1485	0.2522
Education						
High-educated	0.0735	0.1680	0.0610	0.1468	0.0813	0.1795
Medium-educated	0.3131	0.3019	0.2661	0.2889	0.3426	0.3061
Low-educated	0.5872	0.3576	0.6583	0.3433	0.5428	0.3592
N. of observations	1,646,772		633,774		1,012,998	

Notes. This table shows means and standard deviations for firm size and shares of occupational and educational groups. Statistics in columns (1)-(2) are calculated using all observations for each state-year, while columns (3)-(4) (columns (5)-(6)) show statistics before (after) broadband arrival. Sample restrictions are described in Section 3.3.2.

important Brazilian industrial hub and the unique state served by Telefonica. When using the balanced sample, we lose approximately 60% of establishments, but this subsample exhibits rather similar unconditional means than the full set. The only exception is a higher firm age, which is not surprising given that entry is not allowed for.

Table 3.1 reports means and standard deviations of the outcome variables for the main sample separately for the pre-broadband and broadband eras. The arguably most interesting descriptive evidence refers to changes within the distributions of occupational and educational levels. We observe a growth in the share of management positions and a simultaneous decrease in the shares of other occupations. We further observe some degree of educational upgrading, as the share of low-educated workers decreases, while the share of high- and medium-educated workers expands. Figure B.4 in Appendix B shows that high-educated workers largely work in high occupational layers, while medium-educated (low-educated) workers are concentrated in clerical, technician and other operational jobs.

3.3.3 Identification Strategy

Broadband technology is not randomly distributed across space, as both supply and demand side factors influence location decisions. Telecommunications companies have incentives to allocate fast connections in a profit maximizing fashion. On the demand side, broadband adoption might be correlated with industry specifics and other unobserved firm-level factors that simultaneously affect firm outcomes. To circumvent these potential endogeneities, we explore a quasi-experiment derived from a technological feature restricting fast internet availability. In particular, our identification strategy exploits both limitations in the range of broadband signals and temporal variation in technology availability to make within-comparisons between treatment and control group firms.

A reasonable argument against using the actual distance between firms and MDFs as a source of variation is omitted variable bias, since location decisions are endogenous and taken by telecommunications companies and firms, respectively. Therefore, treatment and control groups are instead defined based on the extensive margin of ADSL connectivity. The identification hypothesis is that, conditional on firm fixed effects and common shocks on the state-time level, firms located within and outside of a certain perimeter around the MDF follow similar trends prior to broadband diffusion. Consequently, any post-period changes in their outcomes are exclusively explained by the technological event. Based on these considerations, our first empirical specification takes the following differences-in-differences (DD) form:

$$y_{it} = \beta Close_i \times Post_{st} + \delta_i + \eta_{st} + \epsilon_{it} \tag{3.1}$$

, with y_{it} being the outcome of a given firm i at time t. $Close_i$ is a dummy taking the value one if firm i is located within the treatment perimeter of a given MDF, while $Post_{st}$ is a state-time binary variable equal to one for all years t in which broadband technology is available in state s and zero otherwise. δ_i are firm fixed effects and control for time-constant unobserved plant-level heterogeneity. η_{st} are state-year fixed effects and take into account all state-specific trends that affect firms homogeneously. These fixed effects are also important to control for unobserved state-level shocks that may affect the outcomes of interest simultaneously to the arrival of broadband technology. ϵ_{it} is an idiosyncratic error term. The treatment effect, measured by the parameter β, is thus identified by comparing the within-variation in the outcome of interest of firms located in state s within the treatment perimeter to the within-variation of firms located in the same state outside of the perimeter at the time of ADSL arrival.

Recent methodological advances in econometric literature show that when roll-out of an intervention is heterogeneous, the single coefficient from a DD specification is potentially biased (see Borusyak and Jaravel (2017); Goodman-Bacon (2021); Callaway and Sant'Anna (2020); de Chaisemartin and D'Haultfœuille (2020); Imai and Kim (2021); Sun and Abraham (2020); Athey and Imbens (2021)). This bias may arise as a consequence of inaccurate weighting when comparing treatment and control units over time. Furthermore, summarizing dynamic treatment effects into unique coefficients can hide an interesting dynamic evolution of the studied shock. To deal with these issues, we additionally perform an event-study and estimate the following fixed effects regression equation:

$$y_{it} = \sum_{k=-j}^{J} \beta_k Close_i \times D_k + \delta_i + \eta_{st} + u_{it} \tag{3.2}$$

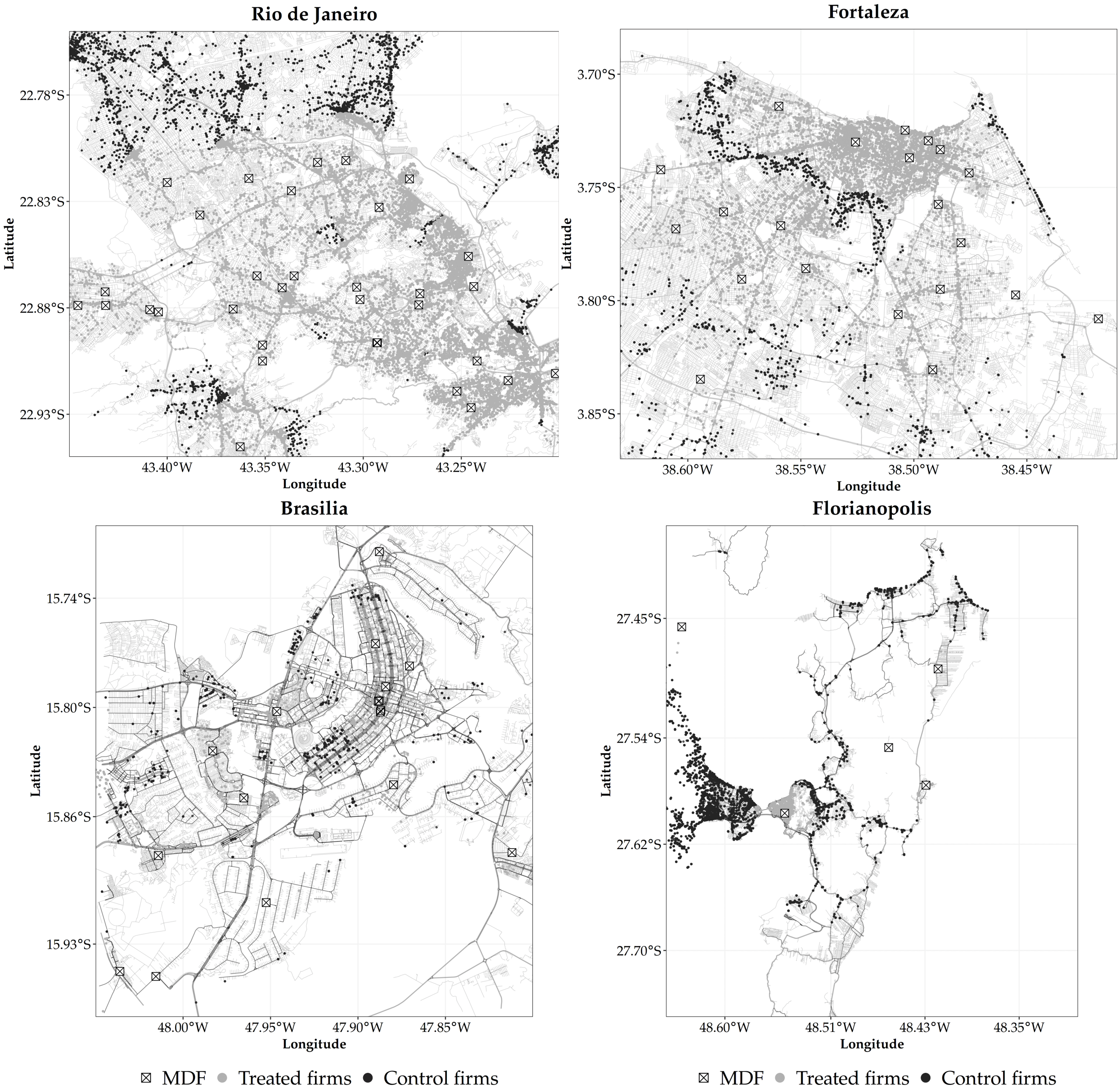
, with D_k being time-specific dummies measuring the distance in years relative to the event. When interacting these dummies with $Close_i$, the coefficients β_k capture anticipatory effects for periods with $k < 0$ and dynamic treatment effects of broadband availability for $k \geq 0$. The remaining variables are the same as the ones included in Equation 3.1. Even though this configuration does not test the underlying common trend assumption directly, it nevertheless provides evidence that outcomes of interest follow common paths prior to fast internet arrival, such that $\beta_{k<0} = 0$. To reduce collinearity issues between the interaction term $Close_i \times D_k$ and the state-time fixed effects η_{st}, we impose the restriction that $\beta_{k\leq-5} = \beta_{-5}$ and thus show all event-study results in a time window of five years around treatment. To account for heteroskedasticity and potential serial correlation within firms, standard errors are robust and clustered at the plant-level in all specifications.

Treated firms are defined as being located within 2.5 km to the closest MDF, while the control group contains all plants outside the treatment perimeter but within a distance of 10 km. Figure 3.3 illustrates firm dispersion around MDFs in four state capitals. Light gray dots represent treated firms, while black dots depict untreated ones. These graphs give an idea of the variation between treated and untreated firms within cities. Even though the majority of firms in our sample are treated (74%), we have sufficient variation in the control group to perform our analyses. To show that results are not dependent on the definition of both perimeters, we perform robustness checks using alternate treatment and control radii. As firms are not equally distributed among treatment and control groups, we additionally estimate DD coefficients using a weighting scheme.

We conduct various other robustness checks to verify the consistency of our results. We show that our findings are not sensitive to using alternate standard error clustering methods. We also provide evidence that establishments within and outside the treatment perimeter are comparable in terms of observable characteristics, regardless which perimeter is used to define treatment status. Additionally, we use flexible specifications with different fixed effects and linear trends, and show that results are robust to excluding MDFs with a high overlap of treatment perimeters and with a very high fraction of treated firms. We further implement falsification tests to investigate the likelihood of firm migration and randomize treatment such that spurious years are defined as treatment years.

Before going through our main analysis, we graphically inspect some key descriptive patterns. Figure 3.4 depicts shares of occupational and educational levels for treated and control firms during the transitional period in a pyramidal format. For simplicity, we

Figure 3.3: Distribution of Main Distribution Frames and Firms across Space

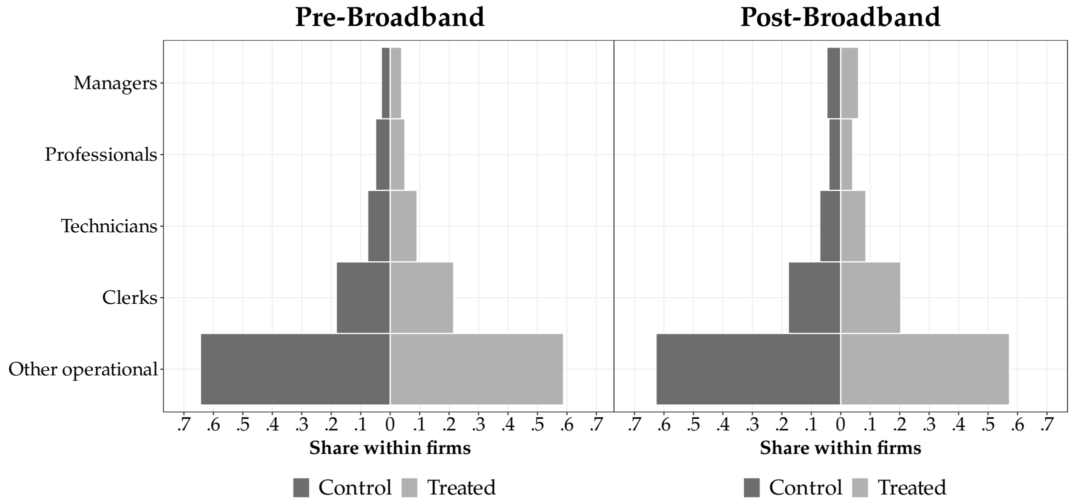


Notes: This figure shows MDF dispersion in four large Brazilian state capitals: Rio de Janeiro, Fortaleza, Brasília, and Florianópolis. Treated establishments (light grey dots) are located within a 2.5 km perimeter around a given MDF while control group establishments (black dots) are located at a distance between 2.5 to 10 km from the closest MDF. *Source*: Graph based on data from ANATEL and RAIS.

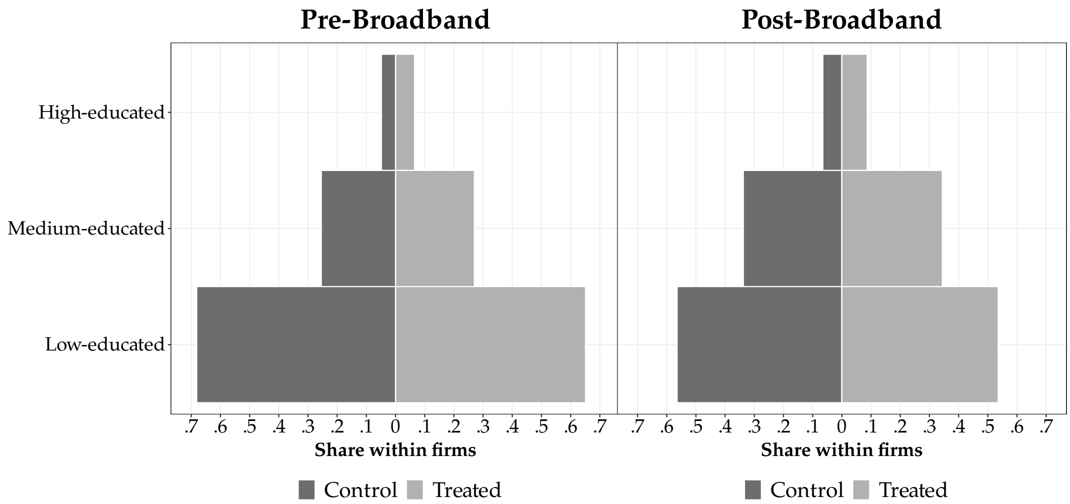
pool observations and divide them into pre- and post-broadband observations by taking the specific treatment year for each Brazilian state into account. Treated firms appear to employ more high-skilled labor and their organizational structure features more top and middle positions even before the technological event. One striking feature that the top panel depicts is an increase in the share of management positions, while the shares of all other occupational groups decrease or remain the same. In the bottom panel, the figure shows an overall trend towards educational upgrading. These graphs provide first

Figure 3.4: Within-Firm Employment Shares

(a) Occupations



(b) Education



Notes: This figure shows pyramid-shaped plots for shares of occupational (top panel) and educational groups (bottom panel). It shows averages both for treated (light-gray bars) and control firms (dark-gray bars) separately for the pre- (left pyramids) and post-broadband periods (right pyramids).

descriptive evidence that firm structure does, in fact, change after broadband internet introduction and that treatment and control groups might be affected differently.

3.4 Results

In this section, we show the effect of broadband introduction on different occupational and educational groups by performing both static and dynamic analyses. We check robustness of our main results before evaluating the applicability of different explanations. We end by analyzing hires, promotions, and total payroll to investigate efficiency gains, and examine whether rearrangements in employment structures drive a direct effect of broadband on firm survival.

3.4.1 The Impact on Firm Structure

Table 3.2 differentiates between occupational and educational outcomes and provides

Table 3.2: Effect of Broadband on Employment Shares

	Main sample		Balanced sample	
	coef. / SE	baseline	coef. / SE	baseline
Outcomes	(1)	(2)	(3)	(4)
Occupations				
Managers	0.0040***	[0.0366]	0.0043***	[0.0347]
	(0.0005)		(0.0006)	
Professionals	-0.0021***	[0.0499]	-0.0021***	[0.0524]
	(0.0005)		(0.0006)	
Technicians	-0.0009	[0.0876]	0.0010	[0.0872]
	(0.0007)		(0.0009)	
Clerks	-0.0039***	[0.2074]	-0.0018	[0.2104]
	(0.0010)		(0.0012)	
Sellers and service wkrs	-0.0071***	[0.1979]	-0.0074***	[0.1986]
	(0.0009)		(0.0011)	
Craft and rel. trades wkrs	-0.0029***	[0.1422]	-0.0030***	[0.1359]
	(0.0009)		(0.0011)	
Machine operators	0.0033***	[0.0978]	0.0025**	[0.1003]
	(0.0008)		(0.0011)	
Elementary occup.	0.0045***	[0.1643]	0.0043***	[0.1727]
	(0.0010)		(0.0013)	
Education				
High-educated	0.0042***	[0.0610]	0.0045***	[0.0612]
	(0.0006)		(0.0008)	
Medium-educated	-0.0120***	[0.2661]	-0.0116***	[0.2544]
	(0.0012)		(0.0015)	
Low-educated	0.0040***	[0.6583]	0.0054***	[0.6772]
	(0.0013)		(0.0015)	
N. of observations	1,646,772		852,463	
State × year FE	Yes		Yes	
Firm FE	Yes		Yes	

Notes. This table shows treatment effects of broadband internet on the shares of occupational and educational groups within firms as displayed in Equation 3.1. Each cell reports the coefficient of a separate regression. The treatment variable is the interaction between $Close_i$, a dummy that equals 1 for firms located within 2.5 km to the closest MDF, and $Post_{st}$, which is a dummy that equals 1 after broadband arrival in state s. Columns (1) and (3) show estimated coefficients and standard errors obtained from the main and balanced panels, respectively. Columns (2) and (4) display unconditional means for the pre-broadband observation period (baseline means) in brackets. Sample restrictions are described in Section 3.3.2. Heteroskedasticity robust standard errors clustered at the firm level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

estimates of Equation 3.1. In column (1), we show intention-to-treat effects using the main sample. Results reveal that access to broadband causes a statistically and economically significant within-firm increase in the share of management positions of 0.4 percentage points, which translates into a 11% increase relative to the baseline mean. Employment shares of the two lowest occupational levels are positively affected by broadband internet introduction as well. In particular, the fraction of machine operators increases by 0.33 percentage points (3.4%), while the share of elementary occupations increases by 0.45 percentage points (2.7%). In contrast, the relative prevalence of many white and blue collar jobs shrinks. Estimates show that the shares of employees working in professional occupations, as clerks, as sellers, in service occupations, or as craftsmen experience significant decreases, while technicians are not affected significantly. The expansion in the top hierarchical layer is mirrored by an increase in the fraction of high-educated employees of 0.42 percentage points (7%). Results further show a decrease in the share of medium-educated and an increase in the share of low-educated employees of 1.2 (4.5%) and 0.4 (0.6%) percentage points, respectively.

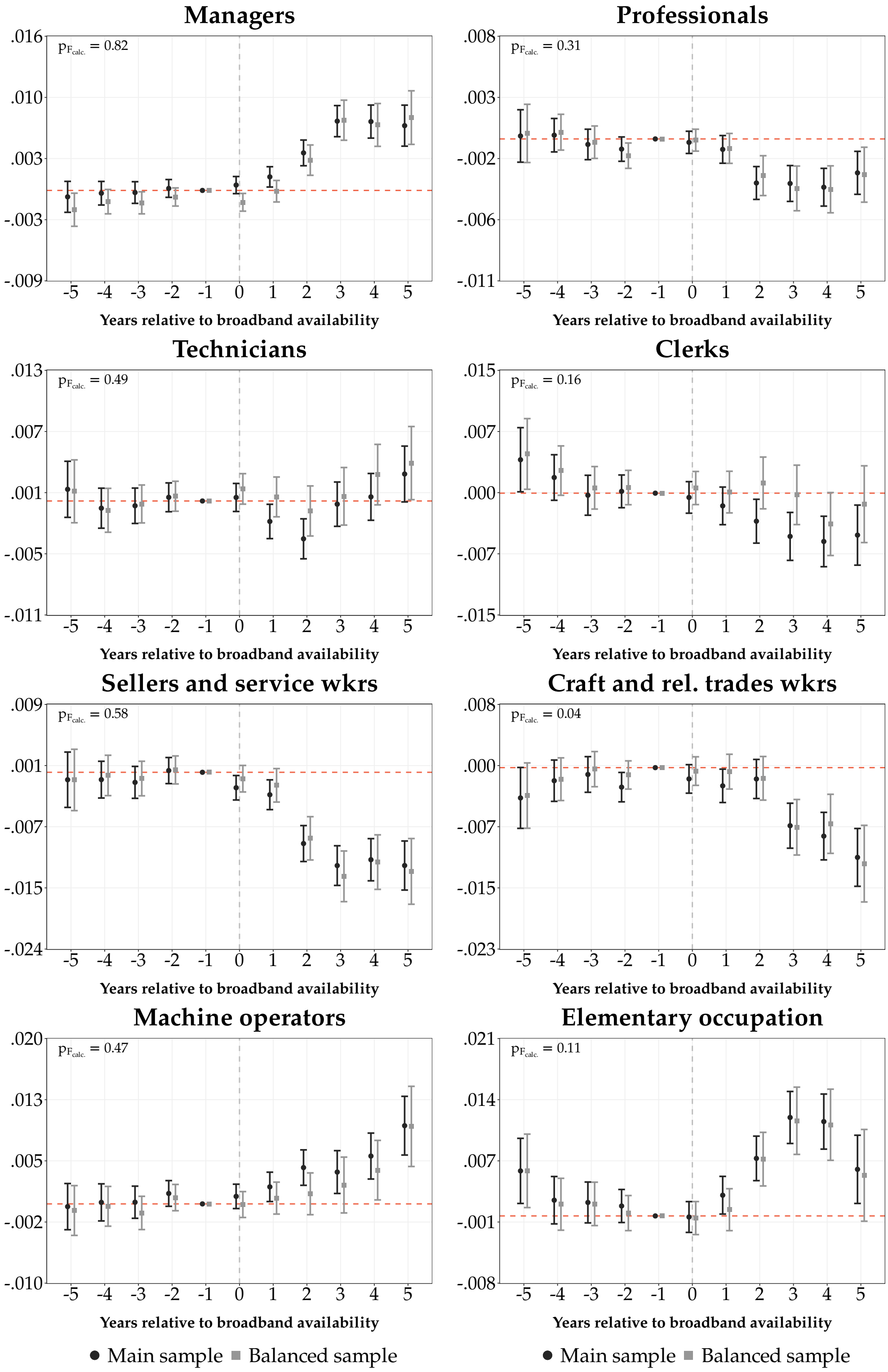
In column (2), we run similar estimations using the balanced sample. In case main results were driven by sample selection, we would expect coefficients to be sensitive to only considering firms active during the entire time span. However, point estimates and their precision remain virtually unchanged. Table B.4 in Appendix B shows that our main results are robust to several different ways of calculating standard errors. In Section B.1 in Appendix B, we show suggestive evidence that potential migration of firms to different locations does not drive our results.

Figure B.5 in Appendix B illustrates the evolution of unconditional averages for each outcome in the five years around treatment and helps us to descriptively inspect if our identifying assumptions are likely to hold. Graphs depict that different occupational and educational groups follow different trends. However, within these groups, pre-treatment trends seem to be rather similar for treatment and control firms. There also seems to be a response to treatment.

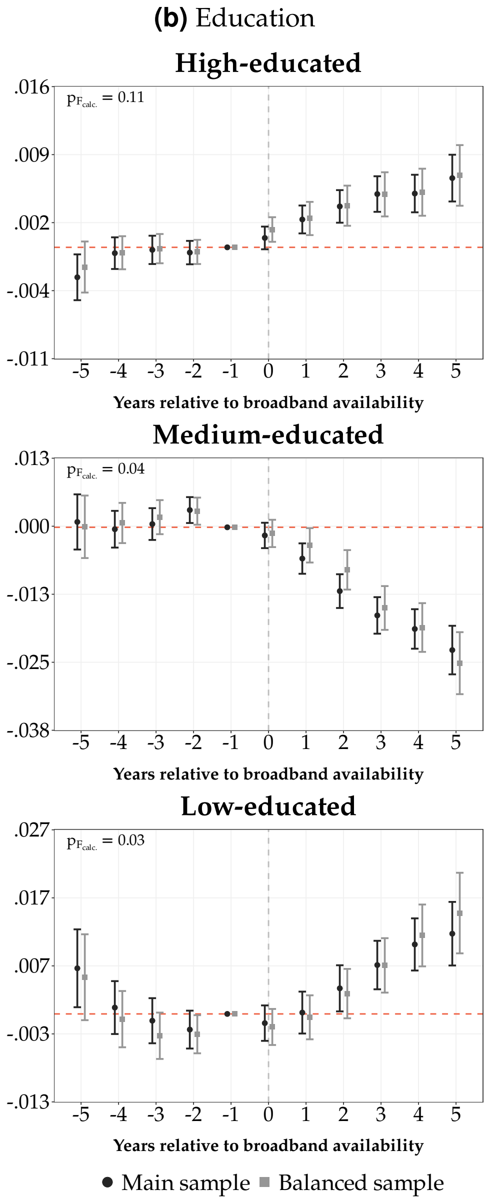
In Figure 3.5, we present a formal test using our event-study specification in Equation 3.2. The graphs show coefficients of a fully-dynamic specification and 95% confidence intervals for the main (black dots) and balanced (gray dots) samples. Following the recommendation of Borusyak and Jaravel (2017), we also execute F-tests for joint significance of pre-treatment coefficients for all our outcomes. Overall, p-values are very high, with the exception of the ones for craft and related trades workers, medium- and low-educated workers. However, these low p-values are driven by events that happen relatively long (i.e. more than four years) before treatment, or by close events that influence coefficients in the opposite way as compared to the treatment effects. All other pre-treatment coefficients are virtually equal to zero. We hence conclude that non-linear trends are not an issue, thus strengthening our argument for exogeneity of events.

Figure 3.5: Effect of Broadband on Employment Shares - Development over Time

(a) Occupations



(continuing)



Notes: This figure shows coefficients from event-study estimations as depicted in Equation 3.2. Black dots show estimated coefficients using our main sample, while light-gray points show estimates using the balanced sample. Vertical lines represent 95% confidence intervals. $p_{F_{calc}}$ is a p-value stemming from an F-test for joint significance of all pre-treatment dummies.

For occupational outcomes, the sizes of treatment effects progressively increase in the first few years after treatment and flatten after approximately three years. We observe a rather strong increase in the share of managers, which starts directly in the first year after broadband introduction. When subdividing employees by education, results reveal that establishments react by directly increasing the share of high-educated employees, while the decrease (increase) in the share of medium-educated (low-educated) employees appears with a time lag of one (two) years. In contrast to the graphs depicting occupational outcomes, we do not observe a flattening of curves for educational outcomes, such that the share of high-educated and low-educated (medium-educated) employees seems to grow (decline) constantly. We obtain very similar coefficients when running the regressions using the balanced sample. Figure B.6 in Appendix B shows that our results remain effectively unchanged when weighting the effects by sample size or implementing a semi-dynamic specification.

The graphs show that broadband availability causes persistent effects on employment structures. Even though we lack data to measure broadband adoption directly, the progressive evolution of rearrangements is suggestive evidence that effects are in fact driven by gradual adoption of the new technology. It is also in line with the descriptive evolution in broadband subscriptions depicted in Figure 3.1 and with information from the World Bank (2003) confirming intensive usage of internet technologies such as websites or emails by Brazilian firms during our period of analysis. In sum, the average treatment effects displayed in Table 3.2 summarize the dynamic treatment effects presented in Figure 3.5.

3.4.2 Robustness Tests

Before analyzing channels associated with the structural changes we document, we perform several robustness checks to ascertain the strength of our identification strategy.

We start by defining alternative treatment and control radii. In the early broadband era, concessionaires mainly provided fast internet connections to potential users located at distances shorter than the technologically feasible threshold. Due to technological advances however, considering expansion to firms located farther away from the MDF became reasonable in later years. We account for these potential sources of bias in two ways: we first follow the literature and consider 4.2 km as the maximum technologically feasible treatment threshold. Second, we evaluate sensitivity of results with respect to the definition of the control perimeter, by reducing it up to a minimum of 5.2 km, while keeping our main treatment definition unchanged.[27]

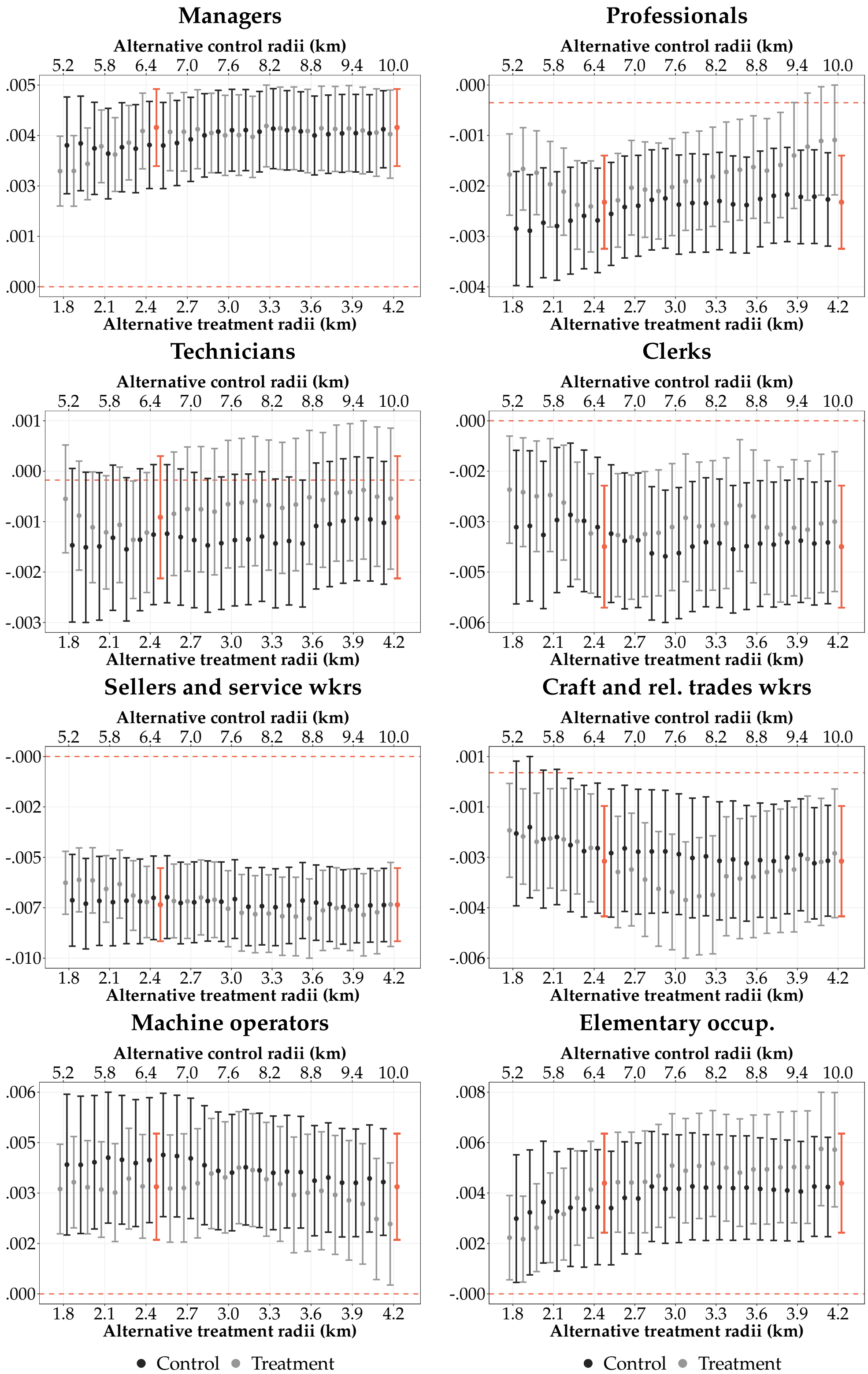
Figure 3.6 presents estimated coefficients for multiple treatment (gray dots) and control (black dots) radii. As expected, the impacts are generally weaker for treatment radii smaller than 2.5 km. When narrowing the control group towards the cutoff of internet connectivity, the point estimates barely change or decline smoothly, which is expected given that we now compare more similar establishments. We note that the higher (lower) the treatment (control) radius, the lower the number of firms in the control group.[28] Nevertheless, estimates are very precise. Additionally, other MDFs in neighboring municipalities might

[27]The rationale for this exercise is that control group firms tend to be located in neighborhoods or cities around capital centers. A smaller control perimeter hence ensures greater comparability of treatment and control units in terms of location conditions and incentives to locate at a given location.

[28]Specifically, when considering the highest (lowest) treatment radius while keeping the control radius constant, 84.2% (64.7%) of the firms in the sample are in the treatment group. When keeping the treatment radius constant while shrinking the control radius to the lowest possible one, 83.3% of firms are defined as treated.

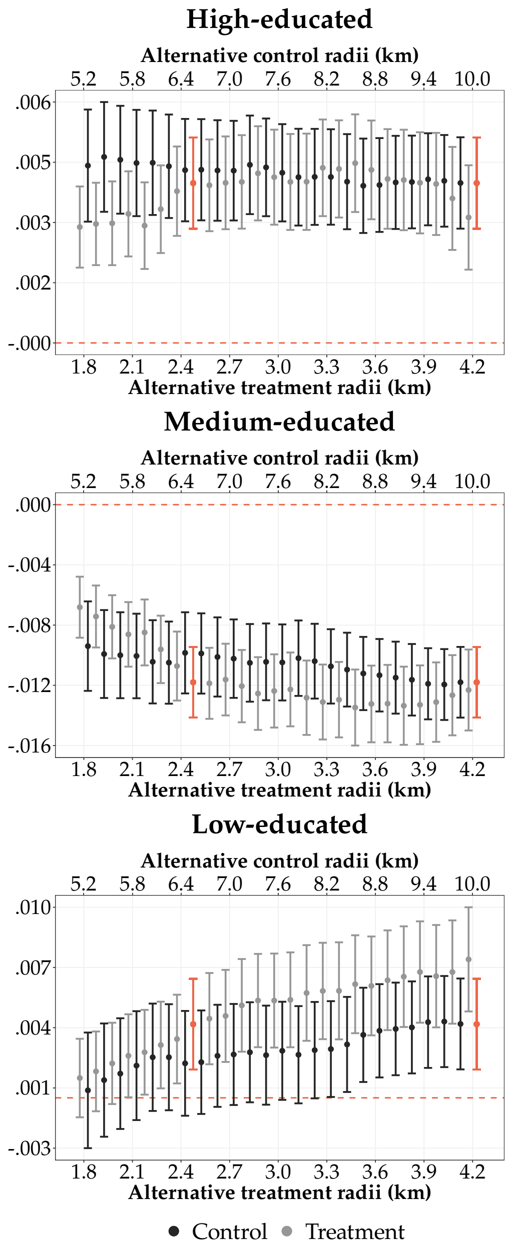
Figure 3.6: Effect of Broadband using Alternate Treatment and Control Radii

(a) Occupations



(continuing)

(b) Education



Notes: This figure shows coefficients for estimations of Equation 3.1 with varying definitions of treatment and control radii. Each coefficient stems from a single regression. Light-gray dots show coefficients when the treatment perimeter definition is varied from a minimum of 1.8 km to a maximum of 4.2 km. Black dots show estimated coefficients when the control radius varies between 5.2 km and 10 km. Red dots highlight estimates using the default treatment and control definitions. Vertical lines represent 95% confidence intervals.

have provided fast internet to control group firms as well. One may be concerned that firms located outside the treatment perimeter of the MDFs in our sample could also be treated at some point in time. The mean distance between control group firms and out-of-sample MDFs in neighboring municipalities is 8.2 km. Thus, treatment perimeters of these MDFs, for the most part, do not comprise locations of firms in our sample and the resulting measurement error with respect to treatment status is likely irrelevant.

Next, Figure B.7 in Appendix B investigates comparability of treatment and control group firms with respect to observable characteristics to rule out the possibility that establishments located farther from the city center systematically differ in terms of industry and age. Each of the plotted coefficients stems from a single regression, with the respective industry, year of firm entry, or legal form as the dependent variable and different treatment radii as the main independent variables, controlling for specific state-year and MDF fixed effects. The economically small coefficients, combined with their overall large confidence intervals and flat shape across several definitions of treatment radii confirm that treatment and control firms operate in similar sectors. The last graph indicates that treated firms are older on average than those in the control group. Figure B.8 in Appendix B shows the distribution of the years of entry for both groups and displays a higher peak for treated firms in the nineteen-sixtees. Otherwise, the histograms are much alike. Even though coefficients are statistically significant, we regard these differences as irrelevant, since we obtain essentially the same results when using only preexisting firms (see Table 3.2), when dropping firms active before 1970, and by splitting the overall sample by median firm age (see Table B.5 in Appendix B).

The 681 geocoded MDFs are often located very closely to each other, as depicted in Figure 3.3, and tend to be concentrated in locations with greater economic activity. Hence, one may wonder to which extent our results are driven by MDF clusters in the cities' more developed quarters. We perform several robustness checks by excluding firms connected to particular MDFs. In column (1) of Table B.6, we drop all MDFs (and the respective connected firms) from the sample which are only surrounded by treated firms. This procedure eliminates almost half of the MDFs in our sample but keeps approximately two thirds of total establishment-year observations. In particular, treated and control firms linked to MDFs located on the cities' outskirts tend to be retained. In columns (2)-(7), we remove all establishments connected to MDFs exhibiting geodistances to the next MDF smaller than 1 to 2 km. In Table B.7, we exclude all establishments within .8 to 2.2 km from the closest MDF. Coefficients are not sensitive to any of these exercises, neither in terms of magnitude nor precision. We conclude that our results are not driven by firms located in more central or densely populated areas.

In Table 3.3, we challenge our main specification by changing and adding controls to test the sensitivity of our estimates. In column (1), we include licensing area-specific linear trends to allow for different dynamics between licensing areas but common to all states within an area and year. Regressions in column (2) control for interactions between year of entry and time dummies to account for the possibility that potential shocks might affect cohorts differently. Next, we control for industry-time specific shocks by inserting a sector-year interaction in column (3). We thereby address the concern that, despite the fact that treated and control firms are active in similar industries, firms in some sectors and at some points in time might have had differing incentives regarding technological

Table 3.3: Effect of Broadband on Employment Shares Including Different Controls and Linear Trends

Outcomes	(1)	(2)	(3)	(4)
Occupations				
Managers	0.0040***	0.0038***	0.0036***	0.0031***
	(0.0005)	(0.0005)	(0.0005)	(0.0007)
Professionals	-0.0021***	-0.0018***	-0.0021***	-0.0022***
	(0.0005)	(0.0005)	(0.0005)	(0.0007)
Technicians	-0.0009	-0.0009	-0.0005	-0.0010
	(0.0007)	(0.0007)	(0.0007)	(0.0010)
Clerks	-0.0039***	-0.0041***	-0.0029***	-0.0026**
	(0.0010)	(0.0010)	(0.0010)	(0.0013)
Sellers and service wkrs	-0.0071***	-0.0074***	-0.0056***	-0.0041***
	(0.0009)	(0.0009)	(0.0009)	(0.0012)
Craft and rel. trades wkrs	-0.0029***	-0.0028***	-0.0036***	0.0001
	(0.0009)	(0.0009)	(0.0009)	(0.0013)
Machine operators	0.0033***	0.0036***	0.0008	0.0015
	(0.0008)	(0.0008)	(0.0008)	(0.0011)
Elementary occup.	0.0045***	0.0041***	0.0055***	0.0022*
	(0.0010)	(0.0010)	(0.0010)	(0.0014)
Education				
High-educated	0.0042***	0.0040***	0.0022***	0.0028***
	(0.0006)	(0.0006)	(0.0006)	(0.0008)
Medium-educated	-0.0120***	-0.0114***	-0.0091***	-0.0082***
	(0.0012)	(0.0012)	(0.0012)	(0.0016)
Low-educated	0.0040***	0.0032**	0.0032***	0.0028*
	(0.0013)	(0.0013)	(0.0013)	(0.0017)
N. of observations	1,646,772	1,640,043	1,642,693	1,646,673
State × year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
Linear licensing area trends	Yes	No	No	No
Year of entry × year	No	Yes	No	No
Industry × year FE	No	No	Yes	No
MDF × year FE	No	No	No	Yes

Notes: This table shows estimated treatment effects of broadband internet including different sets of controls. Each cell reports the coefficient of a separate regression. The treatment variable is the interaction between $Close_i$, a dummy that equals 1 for firms located within 2.5 km to the closest MDF, and $Post_{st}$, which is a dummy that equals 1 after broadband arrival in state s. The control group comprises firms outside the treatment perimeter but within a distance of 10 km. Column (1) includes a linear licensing area time trend, column (2) controls for an interaction between the year of market entry and year fixed effects, column (3) adds industry-year fixed effects, and column (4) controls for an MDF-year interaction. Sample restrictions are described in Section 3.3.2. Heteroskedasticity robust standard errors clustered at the firm level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

investments. In the last column, we include an interaction between MDF and year fixed effects to control for the possibility of heterogeneous improvements in MDF quality. Since estimates remain qualitatively and quantitatively similar, we conclude that our findings are not sensitive to the inclusion of trends and group-specific fixed effects.

In order to provide supplementary evidence that the effects we find are not obtained purely by chance, we conduct a nonparametric permutation test. It consists of randomly "shuffling" years of broadband technology arrival for each location, ceteris paribus. The generated samples enable us to draw from the null distribution we would expect if there was no statistical relationship between treatment events and dependent variables. Figure B.9 displays histograms of estimates (horizontal axis) obtained from 500 permutations for each outcome. The real estimates (red vertical lines) are located far away from the probability mass for all outcomes for which we found a significant change in the previous section and the p-values we obtain from tests are small.

Hence, the robustness checks strongly suggest estimated impacts of broadband introduction on employment structures to be both reliable and causal.

3.4.3 Explaining Structural Change

3.4.3.1 SBTC and Routinization Hypotheses

In order to understand the impact of technological changes on labor market outcomes, the traditional literature resorts to two main explanations: SBTC and routinization. SBTC[29] posits that technology complements skilled workers while substituting for unskilled ones and predicts an increase in demand for skilled labor. Hence, this theory suggests a clear-cut positive relationship between education and employment shares. The ALM hypothesis divides jobs by the prevalence of routine relative to nonroutine tasks. Routine task are repetitive, easily programmable and can thus be easily substituted by technology while nonroutine tasks are much harder to substitute, as underlying rules are not sufficiently well-understood to be programmed. Goos and Manning (2007) and Spitz-Oener (2006) argue that nonroutine analytical and interactive tasks are complementary to technology. These are usually performed by managers and other skilled professionals at the top of the occupational distribution and by high-educated employees, who have a comparative advantage in performing them. Easily substitutable routine manual and routine cognitive task are largely performed by workers in the middle of the occupational distribution and with medium education levels. Workers at the bottom of the distributions largely perform nonroutine manual tasks which are not affected by technology directly but due to economy-wide effects, employment of these groups is expected to be positively affected by technology. Following these considerations, routinization theory predicts labor market polarization.

At first glance, our findings seem to be in line with ALM theory, as we find polarization both for the occupational and educational pyramids. However, our main results show that only the share of management positions increases, while the shares of professionals, associate professionals, and technicians decline or are not affected. This finding is at odds

[29]See e.g. Card and DiNardo (2002) for a discussion.

Table 3.4: Effect of Broadband on Size of Employment Groups

		Smaller firms		Larger firms	
	Size	Share	Size	Share	Size
Outcomes	(1)	(2)	(3)	(4)	(5)
Firm size	-0.0719***				
	(0.0048)				
Occupations					
Managers	-0.0085***	0.0033***	-0.0013	0.0048***	-0.0161***
	(0.0025)	(0.0008)	(0.0027)	(0.0006)	(0.0044)
Professionals	-0.0170***	-0.0014*	-0.0097***	-0.0028***	-0.0244***
	(0.0025)	(0.0007)	(0.0024)	(0.0006)	(0.0044)
Technicians	-0.0268***	-0.0008	-0.0147***	-0.0008	-0.0397***
	(0.0035)	(0.0011)	(0.0036)	(0.0009)	(0.0060)
Clerks	-0.0593***	-0.0037**	-0.0476***	-0.0043***	-0.0723***
	(0.0039)	(0.0015)	(0.0046)	(0.0012)	(0.0064)
Sellers and service wkrs	-0.0452***	-0.0084***	-0.0436***	-0.0057***	-0.0461***
	(0.0036)	(0.0014)	(0.0043)	(0.0011)	(0.0059)
Craft and rel. trades wkrs	-0.0431***	-0.0035**	-0.0556***	-0.0023**	-0.0301***
	(0.0038)	(0.0014)	(0.0045)	(0.0012)	(0.0063)
Machine operators	-0.0303***	0.0018	-0.0500***	0.0050***	-0.0094
	(0.0037)	(0.0013)	(0.0041)	(0.0011)	(0.0061)
Elementary occup.	-0.0245***	0.0054***	-0.0217***	0.0034**	-0.0280***
	(0.0043)	(0.0015)	(0.0047)	(0.0013)	(0.0072)
Education					
High-educated	-0.0082***	0.0025***	0.0015	0.0061***	-0.0182***
	(0.0030)	(0.0008)	(0.0032)	(0.0009)	(0.0051)
Medium-educated	-0.1029***	-0.0079***	-0.0785***	-0.0165***	-0.1284***
	(0.0048)	(0.0018)	(0.0058)	(0.0016)	(0.0078)
Low-educated	-0.0737***	0.0002	-0.0920***	0.0081***	-0.0547***
	(0.0049)	(0.0019)	(0.0062)	(0.0016)	(0.0072)
N. of observations	1,646,772	965,980	965,980	674,166	674,166
State × year FE	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes

Notes. This table shows treatment effects of broadband internet on the logarithm of the overall number of employees and the number of employees in a certain occupational or educational group. Each cell reports the coefficient of a separate regression. The treatment variable is the interaction between $Close_i$, a dummy that equals 1 for firms located within 2.5 km to the closest MDF, and $Post_{st}$, which is a dummy that equals 1 after broadband arrival in state s. The control group comprises firms outside the treatment perimeter but within a distance of 10 km. Column (1) shows estimates using the entire sample. Columns (2)-(5) show estimated coefficients for small and large firms, separated by a median split based on employment in the pre-broadband period. Sample restrictions are described in Section 3.3.2. Heteroskedasticity robust standard errors clustered at the firm level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

with routinization theory, as this type of workers largely perform nonroutine analytical tasks, which are expected to be complemented by technology. The negative and significant coefficient of "sellers and service workers" is also not in line with ALM theory, since this category largely contains retail salesmen and other service occupations that require nonroutine interactive face-to-face interaction.

We then investigate in Table 3.4 how broadband affects employment in terms of the number of workers to evaluate the suitability of the two above-mentioned theories to explain our results and to better understand how shares of different employee types change. In column (1), we show that treated firms are 7% smaller in terms of overall employment as compared to control firms. The negative net effect on firm size is driven by decreases in the number of employees in all occupational and educational groups, suggesting a strong substitution effect of the new technology. The positive impact of broadband availability on the share of low-educated employees in Table 3.2 can be explained by the fact that the negative employment effect is more pronounced for medium-educated workers. In columns (2) to (5), we perform a median split with respect to pre-treatment firm size. Results indicate that the share of management positions and high-educated workers increases due to decreases in all other layers in both larger and smaller firms. However, while the size of these two groups declines in larger firms, it does not change significantly in smaller firms. Thus, managers and high-educated workers are the only groups who are not hurt in all types of firms.

ALM argue that, as the price of computer capital declines, initially routine-intensive industries will make relatively more investments into computer capital, hence leading to a higher increase in the demand for skilled labor, which holds a comparative advantage in nonroutine tasks. If routinization were to explain the observed rearrangements, we would expect the patterns to be driven by certain technology-intensive industries in which routine tasks can be more easily replaced or which might reap the benefits of broadband access more easily. In other words, industries intensive in routine tasks prior to broadband arrival should experience larger increases in demand of nonroutine jobs and high-skill labor and larger decreases in routine jobs.

Table 3.5 splits the overall sample by sectors and shows regression estimates separately for firms active in manufacturing, commerce, and services. Baseline means reveal that the pre-broadband occupational distributions of workers are remarkably different across sectors, reflecting a heterogeneous pre-treatment demand for labor. Responses with respect to the shares of occupations featuring a relatively high nonroutine task content are also heterogeneous across sectors. Results reveal that professionals in manufacturing and commerce experience non-negative (albeit insignificant) effects, and that the negative and statistically significant overall coefficient is entirely driven by the service sector, which has traditionally been more intensive in this kind of profession. Seller occupations also do not expand in any industry in response to the technological shock. Moreover, craft workers largely decline in the commerce sector, but are historically more abundant in manufacturing. These single DD coefficients summarize the coefficients of our semi-dynamic model well (see Figure B.10 in Appendix B). Taken together, these patterns hardly fit the ALM predictions. Additionally, tables 3.5 and B.8 in Appendix B reveal a large

Table 3.5: Effect of Broadband on Employment Shares by Industry

	Manufacturing		Commerce		Services	
	coef. / SE	baseline	coef. / SE	baseline	coef. / SE	baseline
Outcomes	(1)	(2)	(3)	(4)	(5)	(6)
Occupations						
Managers	0.0033***	[0.0226]	0.0025***	[0.0421]	0.0050***	[0.0398]
	(0.0008)		(0.0009)		(0.0008)	
Professionals	-0.0006	[0.0188]	0.0008	[0.0149]	-0.0054***	[0.0962]
	(0.0006)		(0.0006)		(0.0011)	
Technicians	-0.0003	[0.0671]	-0.0035***	[0.0692]	0.0017	[0.1143]
	(0.0012)		(0.0011)		(0.0014)	
Clerks	-0.0022	[0.1214]	-0.0047***	[0.2207]	-0.0013	[0.2442]
	(0.0016)		(0.0017)		(0.0016)	
Sellers and service wkrs	-0.0010	[0.0507]	-0.0108***	[0.3187]	-0.0043***	[0.1796]
	(0.0011)		(0.0020)		(0.0013)	
Craft and rel. trades wkrs	-0.0106***	[0.3814]	-0.0051***	[0.1064]	-0.0010	[0.0394]
	(0.0026)		(0.0014)		(0.0011)	
Machine operators	0.0055**	[0.2122]	-0.0002	[0.0667]	0.0008	[0.0601]
	(0.0024)		(0.0012)		(0.0010)	
Elementary occup.	0.0000	[0.1043]	0.0138***	[0.1467]	0.0014	[0.2122]
	(0.0020)		(0.0018)		(0.0016)	
Education						
High-educated	0.0029***	[0.0389]	0.0022***	[0.0307]	0.0026**	[0.0984]
	(0.0009)		(0.0007)		(0.0013)	
Medium-educated	-0.0132***	[0.1584]	-0.0091***	[0.2879]	-0.0096***	[0.3078]
	(0.0021)		(0.0022)		(0.0020)	
Low-educated	0.0072***	[0.7836]	0.0005	[0.6683]	0.0045**	[0.5809]
	(0.0024)		(0.0023)		(0.0019)	
N. of observations	377,567		582,137		682,989	
State × year FE	Yes		Yes		Yes	
Firm FE	Yes		Yes		Yes	

Notes. This table shows treatment effects of broadband internet on the shares of occupational groups separately for different industries including manufacturing, commerce, and services. Industries are defined according to the 2-digit Brazilian CNAE code as outlined in Table B.3. Each cell shows the coefficient of a separate regression. The treatment variable is the interaction between $Close_i$, a dummy that equals 1 for firms located within 2.5 km to the closest MDF, and $Post_{st}$, which is a dummy that equals 1 after broadband arrival in state s. The control group comprises firms outside the treatment perimeter but within a distance of 10 km. Columns (2), (4), and (6) display unconditional means for the pre-broadband observation period (baseline means) in brackets. Sample restrictions are described in Section 3.3.2. Heteroskedasticity robust standard errors clustered at the firm level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

degree of heterogeneity across sectors regarding workers with different educational levels.

In addition, there is a clear pattern towards a relative expansion in the management layer regardless of industry, with magnitudes ranging from 6% to 15%. High-educated workers also experience positive effects. Interestingly, effects are stronger in industries exhibiting rather narrow pre-treatment management layers, hinting towards a higher need to expand the management layer in these industries as a result of broadband internet introduction.

As outlined above, the net employment effect of the technology is negative for all types of workers. However, broadband seems to complement some worker types, such that the substitution effect is much less pronounced. While some patterns seem to be in line with traditional theories, the fact that managers are the only high-skilled occupational group who consistently expand in terms of employment shares strengthens our view that, in contrast to results found for computerization, ALM and SBTC cannot fully explain changes caused by recent digitization in Brazil. To understand this result, we therefore exploit other explanations that specifically emphasize on the management layer.

3.4.3.2 Theories in Organization

In order to investigate this higher relative demand for managers, we draw on the theory of knowledge-based hierarchies (Bloom et al., 2014). The impact of new ICT on (de-)centralization and span of control, defined as the number of workers reporting to a manager, depends on whether it is primarily used as an information or as a communications technology.[30] By decreasing the cost of information acquisition, information technology makes knowledge acquisition easier, thereby leading to a higher worker autonomy (decentralization) and increasing span of control. In contrast, communications technology leads to more centralization, as communication between managers and workers becomes easier. The effect on managerial span of control is ambiguous, as more questions are asked but each question takes less time to answer. In case the new technology leads to more nonroutine problems, delegation of these problems to managers becomes more important, span of control decreases and the management layer expands. Since we analyze the technology at the time of introduction, we expect that initially, the increase in the number of questions dominates, making a decrease in managerial span of control or an expansion of the management layer the rational response.

In Table 3.6, we provide evidence supporting this rationale and examine the alternate explanation that broadband might affect centralization via facilitating coordination (Hart and Moore, 2005), by splitting the management layer into top and middle management.[31] Results in column (1) show that the overall increase in the fraction of managers is entirely driven by middle management, while the impact on the share of top managers is negative. This is in line with the theory, as top managers are largely occupied with strategic questions. In contrast, workers seem to "push up" nonroutine decisions to mid-level managers, who

[30] The authors posit that agents have to take decisions which often require costly knowledge to solve occurring problems. Thus, the organization needs to decide upon the fraction of problems that each worker has to solve by herself and the respective knowledge that she has to acquire. For all remaining problems, workers ask their managers, who specialize in nonroutine problem solving ("exceptions"), whereby communications costs are incurred. There is thus a tradeoff between information acquisition and communication costs. Based on relative costs, the firm chooses the optimal managerial span of control.

[31] In particular, we identify top management by searching the management layer for the word "diretor" (director) and middle management by searching for the word "gerente" (manager).

Table 3.6: Effect of Broadband on Leadership Levels

		Plants		Sectors		Type of firms		Exporting status	
	All	Single	Multi	Single	Multi	Ltd.	Corp.	Non-exp.	Exp.
Outcomes	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Top managers	-0.0003**	-0.0006**	-0.0002	-0.0003*	-0.0004*	-0.0001	-0.0013***	-0.0004***	0.0001
	(0.0001)	(0.0003)	(0.0001)	(0.0002)	(0.0002)	(0.0001)	(0.0004)	(0.0001)	(0.0007)
Baseline mean	[0.0027]	[0.0020]	[0.0043]	[0.0021]	[0.0034]	[0.0018]	[0.0063]	[0.0025]	[0.0063]
Mid-level managers	0.0034***	0.0048***	0.0021***	0.0039***	0.0027***	0.0023***	0.0065***	0.0032***	0.0059***
	(0.0005)	(0.0010)	(0.0005)	(0.0006)	(0.0007)	(0.0005)	(0.0011)	(0.0005)	(0.0017)
Baseline mean	[0.0332]	[0.0229]	[0.0566]	[0.0374]	[0.0286]	[0.0297]	[0.0475]	[0.0329]	[0.0393]
N. of observations	1,646,772	499,356	1,147,416	930,145	716,627	1,326,851	319,921	1,569,826	76,937
State × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table shows treatment effects of broadband internet on the shares of different management levels separately for different types of firms. Each cell shows the coefficient of a separate regression. The treatment variable is the interaction between $Close_i$, a dummy that equals 1 for firms located within 2.5 km to the closest MDF, and $Post_{st}$, which is a dummy that equals 1 after broadband arrival in state s. The control group comprises firms outside the treatment perimeter but within a distance of 10 km. Column (1) includes the main sample, as described in Section 3.3.2. Columns (2) and (3) split the sample by single- and multi-plant firms. Columns (4) and (5) split the sample by firms operating in single or multi industries. Columns (6) and (7) split the sample by legal form (limited vs. corporations). Columns (8) and (9) split the sample by non-exporters vs. exporters, based on the pre-treatment exporting status. Heteroskedasticity robust standard errors clustered at the firm level in parentheses. Unconditional means for the pre-broadband observation period (baseline means) in brackets. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

act as problem solvers. These findings are consistent with predictions on the effects of technologies that improve communication within organizations.

Furthermore, if results were driven by better coordination, effects should be stronger when the need for coordination is relatively more important. We thus follow Bloom et al. (2014) in splitting the overall sample into establishments that are part of a multi-plant firm vs. single-plant firms[32] and multi-industry vs. single-industry firms. We provide additional analyses by also splitting firms by legal form and pre-treatment exporting status. Results are rather mixed, such that effect sizes are similar when splitting single- and multi-plant firms and single- and multi-industry firms, while they are much stronger for corporations and exporters. Furthermore, coefficients for middle managers are always positive and significant, suggesting that expanding the middle management layer is important for all types of organizations. The semi-dynamic estimates in Table B.9 in Appendix B confirm the documented patterns.

We thus conclude that the impacts we find are not consistent with broadband solely fostering coordination. While some aspects are in line with this theory, the observed reorganization of jobs in response to broadband availability are best explained by management by exception. We argue that broadband introduces nonroutine problems and firms respond by expanding the share of managers.

[32]We are able to figure out if establishments are connected to a headquarter or if they are single firms because the unique establishment identifier consists of two parts and contains both a headquarter and a plant identifier.

3.4.4 Are These Changes Beneficial to Firms?

3.4.4.1 Efficiency

We now investigate to which extent rearrangements caused by broadband are a way of firms to deal with technological change efficiently. We begin by examining *how* firms readjust their labor force. More recent theories on human capital (Gibbons and Waldman, 2004, 2006; Lazear, 2009; Gathmann and Schönberg, 2010) discuss that certain firm-specific skills may lead firms to invest in individuals in order to re-utilize their knowledge and abilities in other positions. Thus, the more firms and workers idiosyncratically invest into these firm-specific skills, the lower the likelihood of turnover. In case broadband introduces large amounts of exceptions requiring firm-specific knowledge and if high-skilled workers have a comparative advantage in solving these, it would be more efficient for the firm to strategically promote these types of workers to management positions instead of hiring them.[33]

Table 3.7 presents the results of this analysis. The number of hires is measured as the (log) number of employees hired in year t. Since we know the date of admission and position for each employee, we can track career progress within firms. Promotion outcomes hence reflect upgrades from lower to higher occupational layers for individuals who were already working in the establishment in the previous year. As is apparent from column (1), the introduction of broadband significantly reduces the number of hired individuals in all but the very bottom occupational layer. The analysis on promotion to the management layer in columns (2) to (4) shows the following patterns: as compared to control firms, treated firms promote fewer employees in lower occupational and educational layers. In addition, larger (smaller) firms promote more technicians (high-educated workers). This is consistent with the notion that both a certain level of education and firm-specific human capital are important to deal with the new technology effectively and that firms reorganize to take advantage of their workers' expertise.[34]

In the next step, we examine firm wage bill. Since efficiency is also a measure of outputs relative to inputs, firms can become more efficient by either increasing output or decreasing inputs. We do not observe accounting figures for more direct measures of inputs and outputs, wherefore we focus on the impacts on total payroll to check whether broadband internet leads to changes in overall labor costs. Our main findings indicate there are two opposing effects that might influence payroll: on the one hand, the management layer

[33] In the Brazilian case, labor regulations allow private sector firms to hire and dismiss employees without cause regardless of the type of job contract and occupation. Yet, there are many direct and indirect costs associated with hiring and separation decisions, which employers have to take into account. In addition to direct wages, hiring encompasses payment of contractual benefits and training costs. Separations involve payment of severance indemnities, which substantially increase in employee tenure.

[34] We do not discard that, in line with findings by Hjort and Poulsen (2019), firms might use on-the-job training to help their workers catch up with broadband technology and that these programs might also be related to promotion decisions.

Table 3.7: Effect of Broadband on Hires and Promotions

		Promoted to managers		
	Hired	All	Smaller firms	Larger firms
	(1)	(2)	(3)	(4)
Occupations				
Managers	-0.0066***			
	(0.0015)			
Professionals	-0.0079***	-0.0003	0.0000	-0.0010
	(0.0017)	(0.0005)	(0.0000)	(0.0010)
Technicians	-0.0105***	0.0016**	0.0010	0.0030*
	(0.0024)	(0.0008)	(0.0010)	(0.0010)
Clerks	-0.0357***	0.0000	0.0000	0.0000
	(0.0031)	(0.0009)	(0.0010)	(0.0020)
Sellers and service wkrs	-0.0318***	-0.0018***	0.0000	-0.0040***
	(0.0029)	(0.0007)	(0.0010)	(0.0010)
Other operational	-0.0011	-0.0045***	-0.0020***	-0.0070***
	(0.0043)	(0.0009)	(0.0010)	(0.0020)
Education				
High-educated	-0.0065***	0.0001	0.0010**	-0.0010
	(0.0020)	(0.0009)	(0.0010)	(0.0020)
Medium-educated	-0.0769***	-0.0018	0.0000	-0.0030
	(0.0040)	(0.0011)	(0.0010)	(0.0020)
Low-educated	-0.0098**	-0.0025***	-0.0020**	-0.0030*
	(0.0045)	(0.0009)	(0.0010)	(0.0020)
N. of observations	1,646,772	1,642,092	963,670	671,811
State × year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes

Notes: This table shows treatment effects of broadband internet on the logarithm of the number of new hires and promotions. Each cell reports the coefficient of a separate regression. The treatment variable is the interaction between $Close_i$, a dummy that equals 1 for firms located within 2.5 km to the closest MDF, and $Post_{st}$, which is a dummy that equals 1 after broadband arrival in state s. The control group comprises firms outside the treatment perimeter but within a distance of 10 km. Column (1) shows the effect on hires and columns (2)-(4) track employees from each occupational and educational layers as they are promoted to management positions. Sample restrictions are described in Section 3.3.2. Heteroskedasticity robust standard errors clustered at the firm level in in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

expands and some individuals are promoted to higher occupational layers, implying potential wage bill increases. On the other hand, the overall number of employees declines as a result of broadband internet, implying a negative effect on payroll.

Table 3.8 reports results of our wage analysis. Column (1) displays average coefficients, while columns (2) to (4) show coefficients separately for different industries. Results show negative and highly significant coefficients and effect sizes are almost identical across industries, both in terms of total payroll and average wages. These changes are not driven by differences in the number of hours worked. Hence, our results indicate that the negative effect that stems from the overall lower number of workers dominates and firms are additionally able to pay a lower average wage to remaining workers, such

Table 3.8: Effect of Broadband on Wages and Hours Worked

Outcomes	All (1)	Manufacturing (2)	Commerce (3)	Services (4)
Total payroll	-0.1455***	-0.1537***	-0.1795***	-0.1226***
	(0.0110)	(0.0227)	(0.0184)	(0.0172)
Average wages	-0.0646***	-0.0586***	-0.0834***	-0.0531***
	(0.0073)	(0.0148)	(0.0125)	(0.0113)
Hours worked	-0.0129	-0.0019	-0.0251	-0.0015
	(0.0149)	(0.0174)	(0.0161)	(0.0333)
N. of observations	1,602,825	365,840	566,943	666,401
State × year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes

Notes. This table shows treatment effects of broadband internet on firms' overall payroll. Each cell shows the coefficient of a separate regression. The treatment variable is the interaction between $Close_i$, a dummy that equals 1 for firms located within 2.5 km to the closest MDF, and $Post_{st}$, which is a dummy that equals 1 after broadband arrival in state s. The control group comprises firms outside the treatment perimeter but within a distance of 10 km. Column (1) shows the average effect while columns (2) - (4) subdivide the sample by industry into manufacturing, commerce, and services. Industries are defined according to the 2-digit Brazilian CNAE code as outlined in Table B.3. Sample restrictions are described in Section 3.3.2. Heteroskedasticity robust standard errors clustered at the firm level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

that broadband leads to a reduction in overall wage costs. This suggests that broadband internet leads to changes in the employment structure of firms that make significant cost savings in terms of wage costs possible. Despite the fact that our payroll measure only captures one aspect, it nevertheless provides a first signal, in conjunction with hiring and promotion behavior, that firms likely become more efficient and competitive due to broadband access and the resulting reorganization.

3.4.4.2 Firm Survival

Bartel, Ichniowski and Shaw (2007) show that firms adopting new technologies experience productivity increases. In addition, research has found a positive association between managerial practices, firm-level productivity and firm survival (Bloom and Van Reenen, 2007; Bloom et al., 2012). If promotions to management overcome the "Peter Principle"[35] and lead to a better worker performance, the changes in employment structures we document should result in higher firm-level productivity and thus better odds to survive. In other words, if rearrangements in fact result in efficiency gains, we should observe mediation of a direct negative effect of broadband on firm mortality. We now test this presumption.

The approach of our survival analysis is similar to Araujo, Mion and Ornelas (2016),

[35]The Peter Principle hypothesis affirms that organizations may take inefficient promotion decisions when promoting employees to managerial positions, thus resulting in a managerial mismatch (Benson, Li and Shue, 2019). This is because firms tend to promote the best worker as a compensation for her job performance instead of promoting the best candidate in terms of leadership quality.

Table 3.9: Effect of Broadband on Firm Mortality

	Dependent variable: firm mortality							
	Year of broadband availability							
	All period		t=2000		t=2001		t=2002	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
$Close_i \times Post_{st} \times Cohort_{t-1}$	-0.0079**	-0.0052**	-0.0034	-0.0023	-0.0123**	-0.0072*	-0.0212*	-0.0161*
	(0.0032)	(0.0025)	(0.0043)	(0.0035)	(0.0052)	(0.0039)	(0.0125)	(0.0092)
$Close_i \times Post_{st} \times Cohort_{t-2}$	-0.0036	-0.0025	-0.0071*	-0.0044	-0.0039	-0.0018	0.0024	-0.0008
	(0.0031)	(0.0023)	(0.0042)	(0.0032)	(0.0049)	(0.0037)	(0.0127)	(0.0083)
$Close_i \times Post_{st} \times Cohort_{t-3}$	-0.0061**	-0.0021	-0.0051	-0.0067**	-0.0123**	-0.0012	0.0196	0.0194**
	(0.0031)	(0.0023)	(0.0040)	(0.0030)	(0.0050)	(0.0037)	(0.0133)	(0.0092)
N. of observations	260,751	260,751	120,624	120,624	120,425	120,425	19,702	19,702
State × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Employment shares	No	Yes	No	Yes	No	Yes	No	Yes

Notes. This table shows treatment effects of broadband internet on firm mortality. For each treatment year between 2000 and 2002, firms included in the estimations are conditioned to be active for "$2002 - t + 1$" years. The treatment variable is the interaction between $Close_i$, a dummy that equals 1 for firms located within 2.5 km to the closest MDF, and $Post_{st}$, which is a dummy that equals 1 after broadband arrival in state s. The treatment variable is interacted with the cohort of market entry. Even columns control for employment shares. Columns (1) and (2) pool all treatment years (2000, 2001, and 2002). Columns (3) to (8) split the sample for each treatment year. Heteroskedasticity robust standard errors clustered at the firm level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

except that we focus on mortality instead of survival.[36] Specifically, to compare treated and control units, we use cohorts of firms entering the market shortly ($t-1$, $t-2$, or $t-3$) before each treatment year t. Additionally, we evaluate mortality probabilities conditional on firms being active "x" years after the event.[37] For instance, in places where broadband was made available in 2000 (2001), our sample comprises only firms active until at least three (two) years after the shock.

The odd columns in Table 3.9 show DD estimates for the direct effect, separately for each subsample receiving treatment in the same year. Regressions pool cohorts and include cohorts active one, two, and three years before the treatment event. Firms in different cohorts of entry seem to benefit from broadband availability as their mortality significantly drops.[38] Even columns depict results when controlling for occupational and educational shares to check potential mediation of this direct effect by rearrangements in

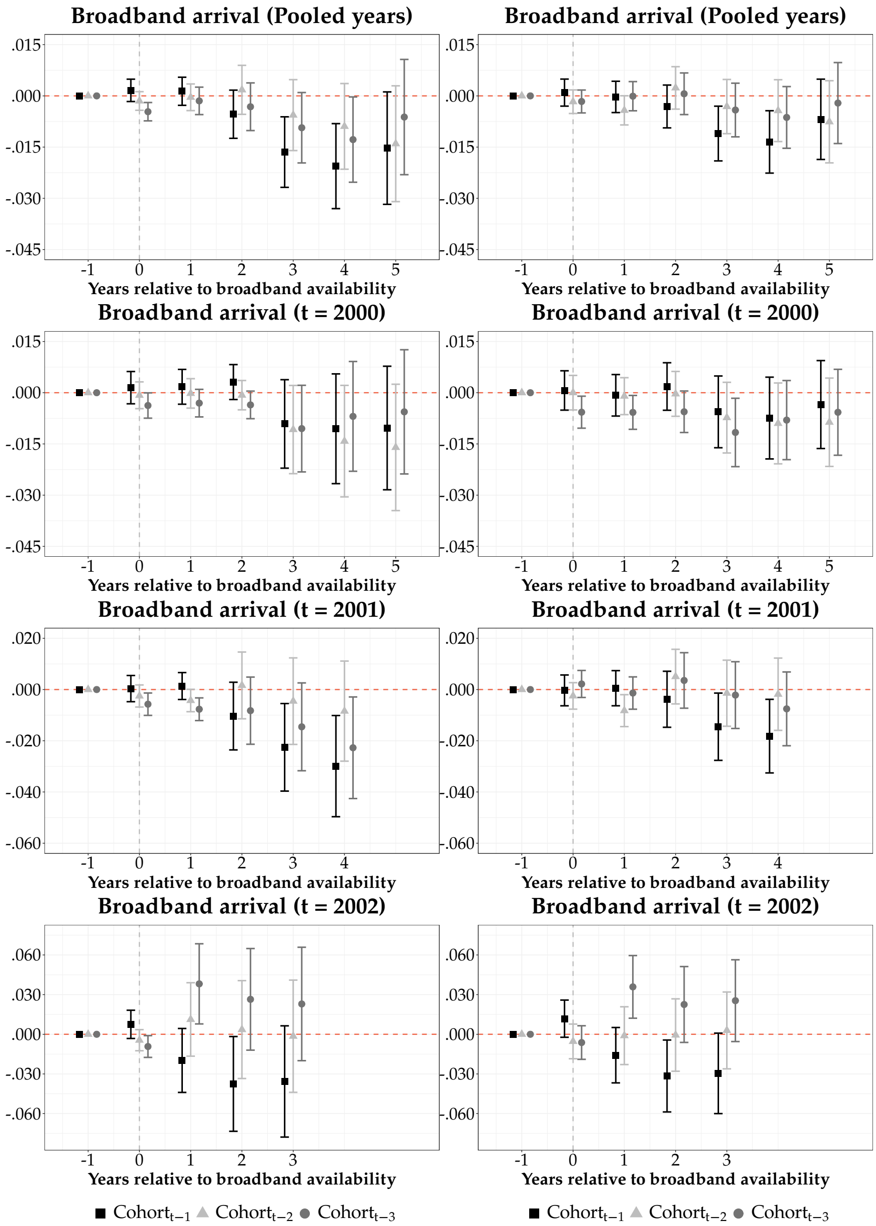
[36] To construct the mortality outcome, we follow the authors' methodology by identifying the first year in which a firm does not appear in the data and replace the missing observations with "ones" from this year onwards.

[37] The literature on international trade (Blum, Claro and Horstmann, 2013; Cadot et al., 2013; Albornoz, Fanelli and Hallak, 2016; Araujo, Mion and Ornelas, 2016) usually analyzes survival probabilities conditional on both market entry and being active for a certain period of time. Since we have information on firm addresses from 2002 on, we use heterogeneity in the time that firms are still active in the market to perform our survival analysis, separately for distinct cohorts entering the market at different times.

[38] Note that Figure B.11 shows that mortality progresses over time, regardless of the cohort.

Figure 3.7: Effect of Broadband on Firm Mortality - Development over Time

(a) Without controls **(b)** With controls



Notes: This figure shows coefficients from event-study estimations as depicted in Equation 3.2 for pooled years and separately for each treatment year. The dependent variable is binary and takes the value 1 if a firm leaves the market. Regressions include interactions between $Close_i \times Post_{st}$ and cohort of entry. The left (right) panel shows the dynamic effects on mortality without (with) controlling for the occupational and educational shares. Vertical lines represent 95% confidence intervals.

the employment structure. For all cohorts experiencing declines in mortality, estimated coefficients drop in magnitude and, in many cases, become insignificant, suggesting partial to full mediation of the direct impact of broadband on mortality by the changes in employment structures.

As compared to our main analysis above, the samples in the survival analysis are smaller and one may be concerned about composition effects potentially driving our results. In Table B.10 in Appendix B, we show that results for occupational and educational shares remain valid when estimating effects using the survival analysis subsamples. The semi-dynamic effects depicted in Figure 3.7 provide additional support for our finding that changes in firms' employment structures mediate a direct negative effect of broadband internet on firm mortality. Our analysis hence suggests that broadband internet and the resulting adjustments in employment structures are an important channel that help firms survive in the market.

3.5 Conclusion

Promoting new technologies is of great interest to policy makers due to high expectations with respect to potential increases in aggregate productivity. However, there is only very limited knowledge on the way firms respond to technology diffusion in terms of changing their labor structure. In this paper, we use Brazilian data to analyze how broadband internet affects employment structures of firms located in large urban areas. We estimate treatment effects by exploiting the conversion of telephone infrastructure for ADSL provision and the technology's limited signal range for a quasi-experimental research design with plausible exogenous variation in broadband availability.

Results show within-firm changes in the employment pyramids which transform the organization as a whole. We find polarization with respect to both educational and occupational groups, a phenomenon largely documented for advanced economies. Yet, in our context, jobs polarize in an exceptional way and patterns cannot entirely be explained by broadband inducing a skill bias or changes in the task-content of jobs. Our findings suggest that the introduction of the new technology and the need to adjust to it cause a large fraction of nonroutine problems, thereby leading to an expansion in the share of managers. The overall effect on employment is negative and job losses largely affect medium- and low-educated workers.

Our findings have important repercussions on debates on public policies aiming to promote technologies. First, they warn of digitization bringing about disruptive consequences for labor markets in large developing economies as well. Second, they highlight that not just different, but similar technologies in varying contexts might also generate distinct impacts (Tian, 2019), depending on the stage of maturity, the degree of user familiarity, and the purpose of use. Our study shows a loss in jobs that does not hurt all worker types equally, but labor structure rearrangements also seem to make organizations more efficient and to increase their short-run chances to survive in the market. Ultimately, this might enable them to provide stable employment for remaining employees. When promoting technology, policy makers should therefore be aware of its features and carefully weight potential costs against benefits.

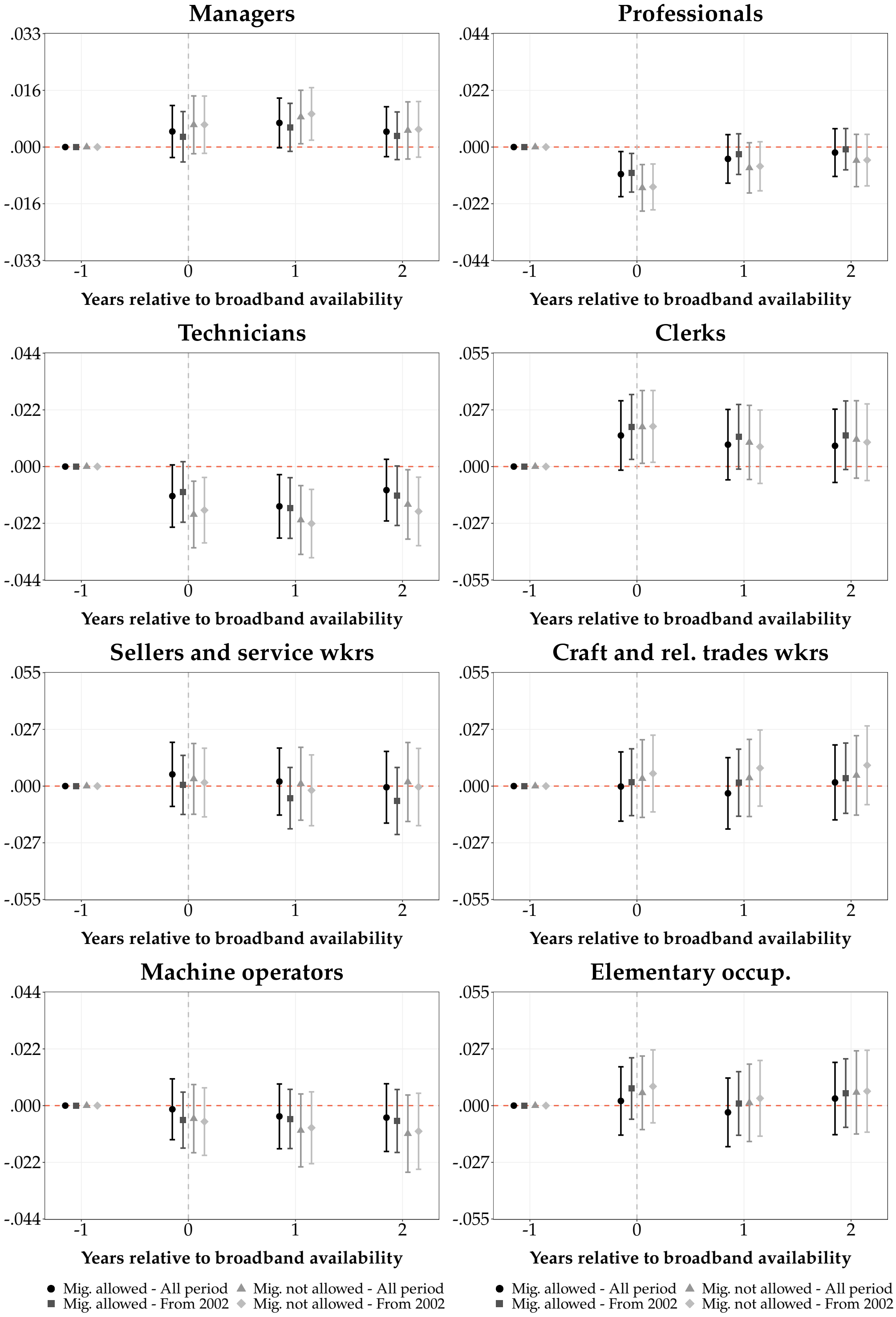
Appendix B

B.1 Establishments' Location Decisions

The RAIS data has a limitation with respect to information on establishments addresses. Since we do not observe firm locations before 2002, our assignment of firms to treatment and control groups might potentially be subject to a certain degree of measurement error. Fortunately for us, two Northeastern states, Alagoas and Sergipe, did not obtain ADSL internet before 2003, such that we observe firm locations both before and after treatment. To exclude the possibility that our results are influenced by firm migration, we perform a falsification check by solely using observations from these two states. We observe around 3.6% of all establishments switching their treatment status in these two states, which is a little higher than the overall mean in the period between 2003 and 2005 for the whole sample (2.9%). In Figure B.1, we present results of an event-study analysis for our occupational and educational outcomes. In the left panel, we allow establishments to migrate and consequently change their treatment status by using a fuzzy DD design. We compare results to our main estimations in which we drop firms we observe migrating and changing their treatment status. In addition, we also run all regressions by only including the year before treatment (2002) as the pre-treatment period and by including all pre-treatment periods (from 1996 on) available to us. Results are virtually identical. Even though these two states are not fully representative for the universe of Brazilian states, the robustness of results strengthens our main approach. We conclude that firm migration is not a threat to the reliability of our findings.

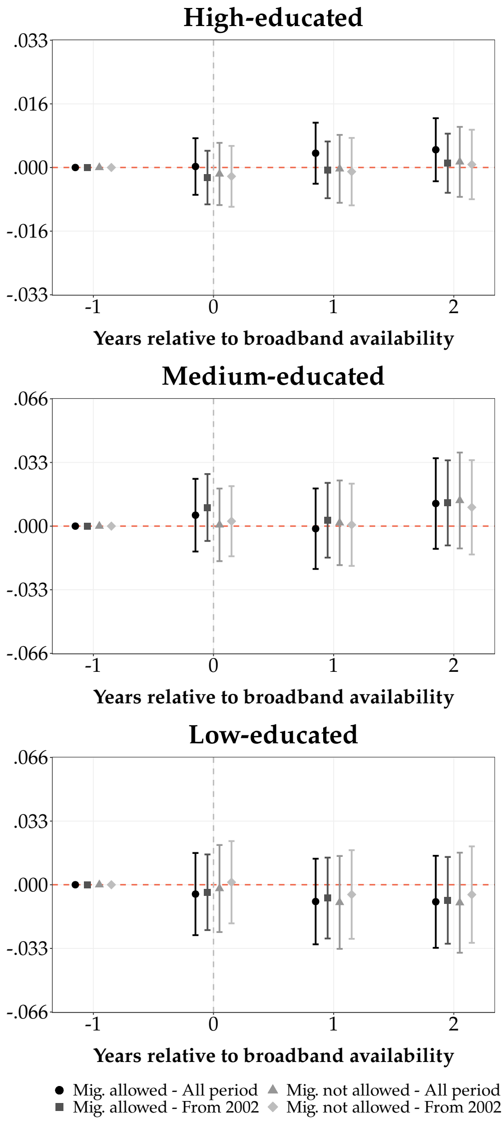
Figure B.1: Effect of Broadband on Employment Shares - Evaluating the Role of Migration

(a) Occupations



(continuing)

(b) Education



Notes: This figure shows coefficients from the event-study estimation, as depicted in Equation 3.2, for the states of Alagoas and Sergipe. The graphs show fuzzy DD estimates (allowing for firm migration) and staggered estimates (not allowing for firm migration). They also depict separate coefficients for estimations using the full sample and for a restricted sample that only includes observations from 2002 on. Vertical lines represent 95% confidence intervals.

B.2 Tables

Table B.1: Occupational Categorization

Occupational level	Description
Managers	C-level managers; Mid-level (administrative, commercial, financial, executive, store, sales, bank, production, personnel, marketing, recruitment, training, research and development, financial product, credit, public relations, postal and telecommunications operations, planning, advertising, operational processing, portfolio management, transportation, retail, and wholesale, restaurant, hotel, and bar) managers; Radio and television producers
Professionals	Educational professionals (basic and higher education); Educational coordinators; Pharmacists; Systems analysts; Engineers; Lawyers; Physicians; Accountants; Economists; Administrators; Writers; Journalists; Editors; Human resources analysts; Advertising professionals; Professional training instructors; Market analysts; Reporters; Public relations agents; Surgeons; Dentists; Nutritionists; Nursing professionals (with college degree); Legal consultants; Educational supervisors
Technicians	Administrative assistants and agents; Supervisors (administrative, accounting and finance, clerical, warehouse, accounts payable, sales and purchasing); Technical sales agents; Sales and purchasing promoters; Technical salesmen and buyers; Technicians (electronic, laboratory, control, production and operations, production planning, accounting, insurance, electronic maintenance, nursing, telecommunication and telephony); Occupational safety quality inspectors; Legal service assistants; Travel agents; Commercial representatives; Speakers; Advertising agents; Preschool teachers; Computer programmers; PC operators; X-ray operators; Designers; Securities brokers
Clerks	Assistants (office, personal, insurance, import and export service, library, sales); Cashiers; Administrative workers; Supply and storage workers; (Bank) tellers; Clerks (warehouse, materials, bank, accounting, production, invoicing, postal services, payroll); (Executive) secretaries; Collectors; Typists; Messengers; Credit analysts; Automatic data processing machine operators; Ticket sellers; Counter attendants, Ticket agents; Operators (copier machines, reception, storage and handling of material, foreign currency exchange, telephone); Archivist; Stenographers; Typists; Road transport road transport service chiefs (passengers and cargo); Quality and product inspection operators; Receptionists; Flight supervisors
Sellers and service workers	Assistants (nursing, pharmacy, clinical analysis, laboratory); Babysitters; Bartenders; Ticket clerks; Firemen; Hairdresser; Chambermaids/-lains; Barmaids; Stewards (cabin, collective transport, of passenger cars); Caterers; Cooks; Beauticians; Gas station attendants; Waiters; Housekeepers; Security guards; Gardeners; Housekeepers; Manicurists; Grocers; Dispatchers; Sellers of wholesale and retail trade; Lifeguards

(continuing)

Occupational level	Description
Craft and rel. trades workers	Butchers, Mechanics (motor vehicles, motorcycles, heating, ventilation, refrigeration); Assemblers (concrete, metal structures); Carpenters; Confectioners; Tailors; Electricians; Plumbers; Toolmakers; Tinsmiths; Printers; Loggers; Masseurs; Bakers; Bricklayers; Painters; Metal polishers; Locksmiths; Construction workers; Welders; Turners; Building maintenance workers
Machine operators	Driver assistants; Tire repairmen; Sewers; Packers; Metal smelters; Printers; Launderers; Plastic molders; Machinery assemblers; Truckers; Drivers; Operators (furnace, excavator, forklift, construction and mining machinery, stationary machines, machine tools, metal pressing); Paint manufacturing workers
Elementary occup.	Doormen; Assembly workers; Lift operators; Chambermaids/-lains; Cargo boys; Drivers (animal-drawn vehicles, pedal-powered vehicles); Railroad maintenance workers; Janitors; Grave-diggers; Packers; Domestic servants; Shoe shiners; Dockworkers; Cleaners; Street cleaners; Garbage men; Prospectors; Caretakers; Street vendors; Dish and vehicle cleaners; Dry cleaners; Window cleaners; Telemarketing sellers; Ironers; Toy makers; Freight movers and unloaders; General service workers; Security guards; Building janitors

Notes. This tables shows the classification of occupations into occupational layers. The Brazilian Classification of Occupations (CBO) contained in the RAIS data base is converted into the 1988 International Standard Classification of Occupations (ISCO88) before occupations are grouped into layers. Details on occupational layers are described in section 3.3.

Table B.2: Firm Characteristics

	Main sample				Balanced sample			
	All	Telefonica	Br Telecom	Telemar	All	Telefonica	Br Telecom	Telemar
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Year of entry	1992	1991	1993	1992	1984	1984	1986	1985
	(9.9574)	(10.5193)	(8.9267)	(9.7906)	(9.2596)	(9.5538)	(8.2922)	(9.2406)
Industry								
Manufacturing	0.1600	0.2209	0.1416	0.1187	0.1865	0.2623	0.1560	0.1307
	(0.3666)	(0.4148)	(0.3486)	(0.3234)	(0.3895)	(0.4399)	(0.3628)	(0.3371)
Commerce	0.3589	0.3404	0.3809	0.3654	0.3344	0.3149	0.3549	0.3443
	(0.4797)	(0.4738)	(0.4856)	(0.4815)	(0.4718)	(0.4645)	(0.4785)	(0.4751)
Services	0.4811	0.4387	0.4775	0.5160	0.4791	0.4228	0.4891	0.5250
	(0.4996)	(0.4962)	(0.4995)	(0.4997)	(0.4996)	(0.4940)	(0.4999)	(0.4994)
Legal form								
Limited	0.8093	0.8206	0.8106	0.7998	0.8059	0.8189	0.8026	0.7956
	(0.3929)	(0.3837)	(0.3918)	(0.4001)	(0.3955)	(0.3851)	(0.3981)	(0.4033)
Corporations	0.1907	0.1794	0.1894	0.2002	0.1941	0.1811	0.1974	0.2044
	(0.3929)	(0.3837)	(0.3918)	(0.4001)	(0.3955)	(0.3851)	(0.3981)	(0.4033)
N. of firms	224,564	82,623	38,039	103,902	85,376	33,866	13,243	38,267

Notes. This table shows summary statistics about establishments characteristics for the main and balanced samples. It depicts the average year of entry, industry shares and the legal form shares for the overall samples and separately for licensing areas. Details on sample selection are described in Section 3.3.2.

Table B.3: Industry Categorization

Industry	Code	Description
Manufacturing	15-19, 20-37, 40-41, 45	Production of food and drinks; Tobacco industry; Textile industry (production of textiles and clothes); Leather processing and production of leather products; Water, gas, and electricity; Construction; Basic metallurgy; Production of metal products excluding machinery and equipment; Production of machinery and equipment; Production of office machinery and computer equipment; Production of electrical machinery and electric materials; Production of electronic material and communication equipment; Production of medical and precision machinery; Production of precision and optical instruments, automation machinery and clocks ; Automotive industry; Production of other transportation equipment; Wood processing; Paper and cellulose production; Editing, printing, and reproduction of recordings; Production of coke, oil refining, production of nuclear fuels, and alcohol; Chemical industry, Rubber and plastic industry; Production of non-metallic minerals (glass, cement, etc.); Production of furniture and diverse industries; Recycling
Commerce	50-52	Trade and repair of automobiles and motorcycles and fuel trade; Retail trade and repair of personal and domestic objects
Services	55, 60-67, 70-74, 80, 85, 92	Mail and telecommunications; Informatics and related services; Research and development; Transportation-related activities and travel agencies; Business-to-business services; Cinematographic and audiovisual works, news agencies; Real estate; Hotels and food-related services; Financial intermediation; Insurance and pensions; Auxiliary activities related to insurance and pensions; Education; Health and social services

Notes. This table shows the grouping of industries into "manufacturing", "commerce", and "services" based on the 2-digit Classification of Economic Activities (CNAE) contained in the RAIS data base.

Table B.4: Effect of Broadband on Employment Shares (p-Values) using Alternate Standard Errors

	Clustered by			
	Firm	Radius × state	MDF	Conley's method
Outcomes	(1)	(2)	(3)	(4)
Occupations				
Managers	0.0000	0.0000	0.0000	0.0000
Professionals	0.0000	0.0272	0.0037	0.0000
Technicians	0.2505	0.3763	0.3630	0.0271
Clerks	0.0000	0.0062	0.0039	0.0000
Sellers and service wkrs	0.0000	0.0000	0.0000	0.0000
Craft and rel. trades wkrs	0.0014	0.1208	0.0423	0.0000
Machine operators	0.0001	0.0060	0.0076	0.0000
Elementary occup.	0.0000	0.0004	0.0053	0.0000
Education				
High-educated	0.0000	0.0000	0.0000	0.0000
Medium-educated	0.0000	0.0000	0.0000	0.0000
Low-educated	0.0010	0.0482	0.0965	0.0000
N. of observations	1,646,772	1,646,772	1,646,772	1,646,772
State × year FE	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes
N. of clusters	224,564	46	678	-
Bootstrap	Yes	No	No	No

Notes: This table shows p-values for alternate ways of calculating standard errors for the main outcomes. Dependent variables are the shares of each occupational and educational groups within establishments. The treatment variable $Close_i$ is a dummy that equals 1 for establishments located within 2.5 km to the closest MDF, interacted with a dummy $Post_{st}$ that equals 1 after broadband arrival in state s. The control group comprises establishments outside the treatment perimeter but within a distance of 10 km. Column (1) depicts p-values when clustering by firm, while column (2) shows results when clustering on the treatment status-state level (i.e. for each state there are two categories identifying whether or not a company is in located within the treatment perimeter). In column (3), standard errors are clustered on the MDF level and in column (4), we use Conley (1999)'s method for clustering standard errors.

Table B.5: Effect of Broadband on Employment Shares Excluding Older Firms and by Firm Age

	Excluding Older Firms		Firm Age			
	Main sample	Balanced sample	Older		Younger	
	coef. / SE	coef. / SE	coef. / SE	baseline	coef. / SE	baseline
Outcomes	(1)	(2)	(3)	(4)	(5)	(6)
Occupations						
Managers	0.0039***	0.0040***	0.0037***	[0.0350]	0.0047***	[0.0396]
	(0.0005)	(0.0007)	(0.0006)		(0.0008)	
Professionals	-0.0022***	-0.0023***	-0.0021***	[0.0539]	-0.0019**	[0.0431]
	(0.0005)	(0.0007)	(0.0006)		(0.0008)	
Technicians	-0.0009	0.0009	-0.0001	[0.0871]	-0.0020*	[0.0886]
	(0.0008)	(0.0010)	(0.0009)		(0.0012)	
Clerks	-0.0041***	-0.0020	-0.0025**	[0.2060]	-0.0063***	[0.2085]
	(0.0010)	(0.0013)	(0.0012)		(0.0017)	
Sellers and service wkrs	-0.0071***	-0.0075***	-0.0076***	[0.1888]	-0.0061***	[0.2152]
	(0.0009)	(0.0012)	(0.0011)		(0.0015)	
Craft and rel. trades wkrs	-0.0028***	-0.0027**	-0.0035***	[0.1455]	-0.0022	[0.1357]
	(0.0010)	(0.0012)	(0.0012)		(0.0015)	
Machine operators	0.0036***	0.0031***	0.0037***	[0.1042]	0.0027*	[0.0871]
	(0.0009)	(0.0011)	(0.0011)		(0.0014)	
Elementary occup.	0.0043***	0.0042***	0.0047***	[0.1671]	0.0038**	[0.1588]
	(0.0011)	(0.0014)	(0.0013)		(0.0017)	
Education						
High-educated	0.0042***	0.0041***	0.0046***	[0.0629]	0.0036***	[0.0580]
	(0.0006)	(0.0008)	(0.0008)		(0.0010)	
Medium-educated	-0.0113***	-0.0105***	-0.0133***	[0.2509]	-0.0103***	[0.2930]
	(0.0013)	(0.0016)	(0.0015)		(0.0021)	
Low-educated	0.0031**	0.0044***	0.0058***	[0.6745]	0.0011	[0.6294]
	(0.0013)	(0.0016)	(0.0016)		(0.0021)	
N. of observations	1,538,533	762,193	857,243		782,406	
State × year FE	Yes	Yes	Yes		Yes	
Firm FE	Yes	Yes	Yes		Yes	

Notes. This table shows treatment effects of broadband internet on occupational and educational layers when excluding older establishments and separately for older and younger establishments. Each cell reports the coefficient of a separate regression. The treatment variable $Close_i$ is a dummy that equals 1 for establishments located within 2.5 km to the closest MDF, interacted with a dummy $Post_{st}$ that equals 1 after broadband arrival in state s. The control group comprises establishments outside the treatment perimeter but within a distance of 10 km. In Columns (1)-(2), establishments active before 1970 are excluded from the sample. Columns (3) and (5) report estimated coefficients separately for old and new establishments, which are divided by a median split based on the year of entry. Columns (4) and (6) display unconditional means for the pre-broadband observation period (baseline means) in brackets. Heteroskedasticity robust standard errors clustered at the establishment level in parentheses. Significance levels: *** p<0.01, **p<0.05, * p<0.1.

Table B.6: Effect of Broadband on Employment Shares Excluding MDFs

	Excluding MDFs						
	100% treat	< 1.0km	< 1.2km	< 1.4km	< 1.6km	< 1.8km	< 2.0km
Outcomes	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Occupations							
Managers	0.0034***	0.0035***	0.0036***	0.0036***	0.0034***	0.0032***	0.0032***
	(0.0005)	(0.0005)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Professionals	-0.0015***	-0.0020***	-0.0021***	-0.0021***	-0.0022***	-0.0025***	-0.0021***
	(0.0005)	(0.0005)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Technicians	-0.0006	-0.0012	-0.0011	-0.0011	-0.0009	-0.0006	0.0001
	(0.0008)	(0.0008)	(0.0008)	(0.0008)	(0.0009)	(0.0009)	(0.0010)
Clerks	-0.0040***	-0.0033***	-0.0032***	-0.0036***	-0.0028**	-0.0022*	-0.0020
	(0.0010)	(0.0011)	(0.0011)	(0.0011)	(0.0012)	(0.0012)	(0.0013)
Sellers and service wkrs	-0.0061***	-0.0067***	-0.0066***	-0.0059***	-0.0059***	-0.0058***	-0.0059***
	(0.0009)	(0.0010)	(0.0010)	(0.0010)	(0.0011)	(0.0011)	(0.0012)
Craft and rel. trades wkrs	-0.0020**	-0.0020*	-0.0023**	-0.0023**	-0.0015	-0.0012	-0.0017
	(0.0010)	(0.0011)	(0.0011)	(0.0011)	(0.0011)	(0.0012)	(0.0012)
Machine operators	0.0026***	0.0038***	0.0039***	0.0040***	0.0036***	0.0033***	0.0032***
	(0.0009)	(0.0010)	(0.0010)	(0.0010)	(0.0011)	(0.0011)	(0.0012)
Elementary occup.	0.0042***	0.0033***	0.0033***	0.0028**	0.0026**	0.0025*	0.0025*
	(0.0011)	(0.0012)	(0.0012)	(0.0012)	(0.0012)	(0.0013)	(0.0014)
Education							
High-educated	0.0032***	0.0042***	0.0042***	0.0039***	0.0031***	0.0031***	0.0032***
	(0.0006)	(0.0007)	(0.0007)	(0.0007)	(0.0007)	(0.0008)	(0.0008)
Medium-educated	-0.0111***	-0.0122***	-0.0123***	-0.0121***	-0.0114***	-0.0121***	-0.0124***
	(0.0013)	(0.0014)	(0.0014)	(0.0014)	(0.0015)	(0.0016)	(0.0016)
Low-educated	0.0049***	0.0047***	0.0048***	0.0048***	0.0055***	0.0068***	0.0070***
	(0.0013)	(0.0014)	(0.0014)	(0.0015)	(0.0015)	(0.0016)	(0.0017)
N. of observations	1,141,543	1,234,021	1,146,946	1,067,350	909,068	774,878	697,157
N. of MDFs	335	647	636	624	600	556	534
State × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows the estimated coefficients when excluding MDFs and their connected firms in areas with a high MDF concentration. Each cell reports the coefficient of a separate regression. The treatment variable $Close_i$ is a dummy that equals 1 for establishments located within 2.5 km to the closest MDF, interacted with a dummy $Post_{st}$ that equals 1 after broadband arrival in state s. The control group comprises establishments outside the treatment perimeter but within a distance of 10 km. This main sample consists of 670 MDFs located across state capitals where backbone infrastructure is available. In column (1), regressions exclude all MDFs surrounded by only treated establishments. In Columns (2)-(7), regressions exclude all MDFs (and respective establishments) exhibiting an MDF-to-MDF distance smaller than 1 to 2 km. Heteroskedasticity robust standard errors clustered at the establishment level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table B.7: Effect of Broadband on Employment Shares Excluding Firms

	Keeping firms (km)							
	[0.8,10]	[1,10]	[1.2,10]	[1.4,10]	[1.6,10]	[1.8,10]	[2,10]	[2.2,10]
Outcomes	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Occupations								
Managers	0.0031***	0.0030***	0.0025***	0.0024***	0.0031***	0.0035***	0.0036***	0.0038***
	(0.0005)	(0.0006)	(0.0006)	(0.0006)	(0.0007)	(0.0008)	(0.0009)	(0.0012)
Professionals	-0.0011**	-0.0008	-0.0008	-0.0006	-0.0007	-0.0009	-0.0016*	-0.0012
	(0.0005)	(0.0006)	(0.0006)	(0.0006)	(0.0007)	(0.0008)	(0.0009)	(0.0011)
Technicians	-0.0010	-0.0012	-0.0009	-0.0008	-0.0011	-0.0009	0.0004	0.0010
	(0.0008)	(0.0008)	(0.0009)	(0.0009)	(0.0010)	(0.0011)	(0.0013)	(0.0017)
Clerks	-0.0030***	-0.0031***	-0.0030***	-0.0031**	-0.0033**	-0.0040***	-0.0039**	-0.0058***
	(0.0010)	(0.0011)	(0.0011)	(0.0012)	(0.0013)	(0.0015)	(0.0017)	(0.0022)
Sellers and service wkrs	-0.0057***	-0.0053***	-0.0048***	-0.0042***	-0.0035***	-0.0037***	-0.0049***	-0.0057***
	(0.0010)	(0.0010)	(0.0010)	(0.0011)	(0.0012)	(0.0013)	(0.0015)	(0.0019)
Craft and rel. trades wkrs	-0.0031***	-0.0029***	-0.0028**	-0.0028**	-0.0033**	-0.0040***	-0.0039**	-0.0055***
	(0.0010)	(0.0010)	(0.0011)	(0.0012)	(0.0013)	(0.0014)	(0.0016)	(0.0021)
Machine operators	0.0028***	0.0024**	0.0026***	0.0021*	0.0010	0.0008	0.0005	0.0016
	(0.0009)	(0.0010)	(0.0010)	(0.0011)	(0.0012)	(0.0013)	(0.0015)	(0.0019)
Elementary occup.	0.0044***	0.0041***	0.0039***	0.0039***	0.0043***	0.0050***	0.0056***	0.0060***
	(0.0011)	(0.0012)	(0.0012)	(0.0013)	(0.0014)	(0.0016)	(0.0018)	(0.0022)
Education								
High-educated	0.0033***	0.0032***	0.0028***	0.0029***	0.0031***	0.0035***	0.0038***	0.0054***
	(0.0007)	(0.0007)	(0.0007)	(0.0008)	(0.0009)	(0.0010)	(0.0011)	(0.0014)
Medium-educated	-0.0114***	-0.0116***	-0.0110***	-0.0124***	-0.0130***	-0.0134***	-0.0138***	-0.0179***
	(0.0013)	(0.0014)	(0.0014)	(0.0015)	(0.0017)	(0.0018)	(0.0021)	(0.0027)
Low-educated	0.0053***	0.0054***	0.0057***	0.0075***	0.0078***	0.0068***	0.0073***	0.0083***
	(0.0014)	(0.0014)	(0.0015)	(0.0016)	(0.0017)	(0.0019)	(0.0022)	(0.0028)
N. of observations	1,126,217	985,729	849,179	734,653	644,612	575,692	520,116	472,169
N. of MDFs	639	612	581	547	501	468	412	374
State × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: This table shows the estimated coefficients by restricting for highly concentrated MDFs areas. The treatment variable $Close_i$ is a dummy that equals 1 for establishments located within 2.5 km to the closest MDF, interacted with a dummy $Post_{st}$ that equals 1 after broadband arrival in state s. The control group comprises establishments outside the treatment perimeter but within a distance of 10 km. In column (1), regressions exclude all MDFs with only treated establishments. In Columns (2)-(7), regressions exclude connected establishments with a distance of less than 0.8 to 2.2 km from the nearest MDF. Heteroskedasticity robust standard errors clustered at the establishment level are in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table B.8: Effect of Broadband on Size of Employment Groups by Industry

Outcomes	Manufacturing (1)	Commerce (2)	Services (3)
Firm size	-0.0855***	-0.0850***	-0.0607***
	(0.0104)	(0.0074)	(0.0076)
Occupations			
Managers	-0.0161***	-0.0114***	-0.0053
	(0.0048)	(0.0044)	(0.0042)
Professionals	-0.0125***	-0.0047**	-0.0360***
	(0.0044)	(0.0023)	(0.0053)
Technicians	-0.0334***	-0.0282***	-0.0272***
	(0.0067)	(0.0050)	(0.0063)
Clerks	-0.0606***	-0.0634***	-0.0512***
	(0.0074)	(0.0067)	(0.0066)
Sellers and service wkrs	-0.0141**	-0.0764***	-0.0301***
	(0.0056)	(0.0070)	(0.0059)
Craft and rel. trades wkrs	-0.1045***	-0.0456***	-0.0178***
	(0.0106)	(0.0052)	(0.0051)
Machine operators	-0.0494***	-0.0324***	-0.0274***
	(0.0098)	(0.0051)	(0.0050)
Elementary occup.	-0.0528***	0.0095	-0.0279***
	(0.0091)	(0.0067)	(0.0068)
Education			
High-educated	-0.0294***	-0.0004	-0.0172***
	(0.0054)	(0.0035)	(0.0059)
Medium-educated	-0.1217***	-0.0854***	-0.0938***
	(0.0095)	(0.0077)	(0.0081)
Low-educated	-0.0727***	-0.0828***	-0.0637***
	(0.0107)	(0.0077)	(0.0078)
N. of observations	377,567	582,137	682,989
State × year FE	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes

Notes: This table shows treatment effects of broadband internet on the size of occupational groups separately for firms active in manufacturing, commerce, and services. Industries are defined according to the 2-digit Brazilian CNAE code as outlined in Table B.3. Each cell reports the coefficient of a separate regression. The treatment variable $Close_i$ is a dummy that equals 1 for establishments located within 2.5 km to the closest MDF, interacted with a dummy $Post_{st}$ that equals 1 after broadband arrival in state s. The control group comprises establishments outside the treatment perimeter but within a distance of 10 km. Heteroskedasticity robust standard errors clustered at the establishment level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table B.9: Semi-Dynamic Effects of Broadband on Leadership Levels

		Plants		Sectors		Legal form		Exporting status	
	All	Single	Multi	Single	Multi	Ltd.	Corp.	Non-exp.	Exp.
Outcomes	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
					Top managers				
$Close_i \times Post_{st+0}$	0.000	0.0001	-0.0002	-0.0001	0.0002	0.0002	-0.0005	0.0001	-0.0002
	(0.0001)	(0.0001)	(0.0003)	(0.0002)	(0.0002)	(0.0001)	(0.0005)	(0.0001)	(0.0008)
$Close_i \times Post_{st+1}$	-0.0001	-0.0001	-0.0002	0.000	-0.0002	0.0001	-0.0010*	-0.0001	-0.0001
	(0.0001)	(0.0002)	(0.0003)	(0.0002)	(0.0002)	(0.0001)	(0.0005)	(0.0001)	(0.0009)
$Close_i \times Post_{st+2}$	-0.0003	0.000	-0.0008**	-0.0001	-0.0005	0.000	-0.0013**	-0.0003*	0.0009
	(0.0002)	(0.0002)	(0.0004)	(0.0002)	(0.0003)	(0.0002)	(0.0005)	(0.0002)	(0.0009)
$Close_i \times Post_{st+3}$	-0.0007***	-0.0006**	-0.0010***	-0.0006***	-0.0009***	-0.0004*	-0.0021***	-0.0008***	-0.0003
	(0.0002)	(0.0002)	(0.0004)	(0.0002)	(0.0003)	(0.0002)	(0.0006)	(0.0002)	(0.0010)
$Close_i \times Post_{st+4}$	-0.0005***	-0.0004*	-0.0006	-0.0005**	-0.0005	-0.0002	-0.0015***	-0.0006***	0.0001
	(0.0002)	(0.0002)	(0.0004)	(0.0002)	(0.0003)	(0.0002)	(0.0006)	(0.0002)	(0.0008)
$Close_i \times Post_{st+5}$	-0.0007***	-0.0004	-0.0015***	-0.0008***	-0.0006	-0.0003	-0.0025***	-0.0009***	0.0003
	(0.0002)	(0.0003)	(0.0005)	(0.0003)	(0.0004)	(0.0002)	(0.0007)	(0.0002)	(0.0008)
					Middle managers				
$Close_i \times Post_{st+0}$	0.0005	-0.0003	0.0020**	0.0012**	-0.0004	-0.0001	0.0024**	0.0004	0.0028
	(0.0004)	(0.0005)	(0.0009)	(0.0006)	(0.0006)	(0.0005)	(0.0010)	(0.0004)	(0.0018)
$Close_i \times Post_{st+1}$	0.0013***	0.0004	0.0030***	0.0021***	0.0003	0.0006	0.0036***	0.0012**	0.0033*
	(0.0005)	(0.0005)	(0.0011)	(0.0007)	(0.0007)	(0.0005)	(0.0011)	(0.0005)	(0.0018)
$Close_i \times Post_{st+2}$	0.0032***	0.0019***	0.0046***	0.0041***	0.0019**	0.0017***	0.0080***	0.0033***	0.0016
	(0.0006)	(0.0006)	(0.0013)	(0.0008)	(0.0009)	(0.0006)	(0.0015)	(0.0006)	(0.0023)
$Close_i \times Post_{st+3}$	0.0063***	0.0048***	0.0076***	0.0064***	0.0063***	0.0050***	0.0098***	0.0061***	0.0099***
	(0.0007)	(0.0008)	(0.0015)	(0.0010)	(0.0011)	(0.0008)	(0.0018)	(0.0008)	(0.0027)
$Close_i \times Post_{st+4}$	0.0060***	0.0041***	0.0077***	0.0064***	0.0054***	0.0043***	0.0104***	0.0056***	0.0111***
	(0.0008)	(0.0009)	(0.0017)	(0.0011)	(0.0012)	(0.0008)	(0.0020)	(0.0008)	(0.0027)
$Close_i \times Post_{st+5}$	0.0060***	0.0045***	0.0066***	0.0062***	0.0055***	0.0050***	0.0066**	0.0056***	0.0090***
	(0.0010)	(0.0011)	(0.0022)	(0.0013)	(0.0015)	(0.0010)	(0.0027)	(0.0010)	(0.0029)
N. of observations	1,646,772	1,147,416	499,356	930,145	716,627	1,326,851	319,921	1,569,826	76,937
State × year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table shows semi-dynamic treatment effects of broadband internet on the shares of different management levels separately for different types of firms. Each cell shows the coefficient of a separate regression. The treatment variable $Close_i$ is a dummy that equals 1 for establishments located within 2.5 km to the closest MDF, interacted with a dummy $Post_{st}$ that equals 1 after broadband arrival in state s. The control group comprises establishments outside the treatment perimeter but within a distance of 10 km. Column (1) includes the main sample. Columns (2) and (3) split the sample by single- and multi-plant establishments. Columns (4) and (5) split the sample into firms operating in single and multiple industries. Columns (6) and (7) split the sample by legal form (limited vs. corporations). Columns (8) and (9) split the sample into non-exporters and exporters, based on their pre-treatment exporting status. Heteroskedasticity robust standard errors clustered at the establishment level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

Table B.10: Effect of Broadband on Employment Shares using the Survival Analysis Sample

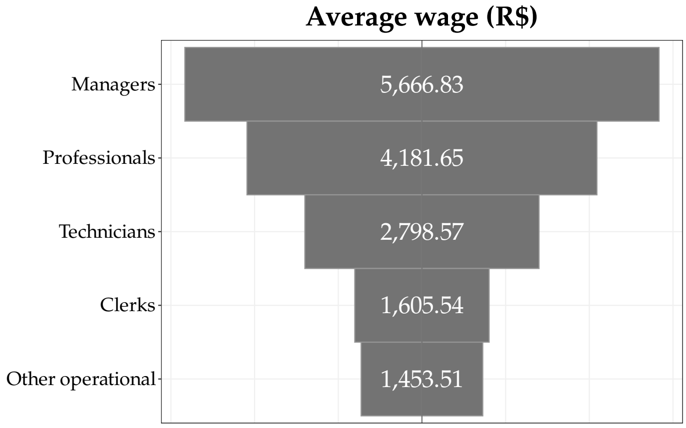
	Occupations					Education		
	Managers	Professionals	Technicians	Clerks	Other operational	High-educated	Medium-educated	Low-educated
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Pooled years of broadband availability								
$Close_i \times Post_{st} \times Cohort_{t-1}$	0.0068***	-0.0052***	-0.0046*	-0.0045	0.0029	-0.0021	-0.0131***	0.0117**
	(0.0020)	(0.0019)	(0.0027)	(0.0037)	(0.0051)	(0.0024)	(0.0048)	(0.0050)
$Close_i \times Post_{st} \times Cohort_{t-2}$	0.0060***	-0.0034**	-0.0021	-0.0061*	-0.0032	0.0027	-0.0140***	0.0057
	(0.0016)	(0.0016)	(0.0023)	(0.0032)	(0.0046)	(0.0020)	(0.0042)	(0.0045)
$Close_i \times Post_{st} \times Cohort_{t-2}$	0.0077***	-0.0013	-0.0024	-0.0091***	0.0056	0.0095***	-0.0121***	0.0064
	(0.0016)	(0.0015)	(0.0023)	(0.0032)	(0.0046)	(0.0020)	(0.0041)	(0.0044)
N. of observations	260,751	260,751	260,751	260,751	260,751	260,751	260,751	260,751
Panel B: Year of broadband availability "t=2000"								
$Close_i \times Post_{st} \times Cohort_{t-1}$	0.0110***	-0.0026	-0.0086**	-0.0022	-0.0092	0.0024	-0.0096	-0.0046
	(0.0031)	(0.0025)	(0.0040)	(0.0054)	(0.0074)	(0.0037)	(0.0070)	(0.0074)
$Close_i \times Post_{st} \times Cohort_{t-2}$	0.0082***	-0.0026	0.0021	-0.0017	-0.0113*	0.0062*	-0.0018	-0.0059
	(0.0026)	(0.0023)	(0.0036)	(0.0049)	(0.0068)	(0.0033)	(0.0062)	(0.0067)
$Close_i \times Post_{st} \times Cohort_{t-2}$	0.0109***	-0.0013	-0.0030	-0.0053	-0.0063	0.0131***	-0.0154***	0.0016
	(0.0025)	(0.0021)	(0.0034)	(0.0045)	(0.0064)	(0.0031)	(0.0058)	(0.0063)
N. of observations	120,624	120,624	120,624	120,624	120,624	120,624	120,624	120,624
Panel C: Year of broadband availability "t=2001"								
$Close_i \times Post_{st} \times Cohort_{t-1}$	0.0038	-0.0062**	-0.0014	-0.0056	0.0112	-0.0071**	-0.0116	0.0225***
	(0.0028)	(0.0030)	(0.0039)	(0.0056)	(0.0079)	(0.0035)	(0.0073)	(0.0077)
$Close_i \times Post_{st} \times Cohort_{t-2}$	0.0045*	-0.0024	-0.0056*	-0.0098**	0.0059	-0.0012	-0.0168***	0.0137**
	(0.0024)	(0.0025)	(0.0033)	(0.0047)	(0.0070)	(0.0028)	(0.0063)	(0.0068)
$Close_i \times Post_{st} \times Cohort_{t-3}$	0.0056**	-0.0007	-0.0018	-0.0106**	0.0173**	0.0054*	-0.0026	0.0102
	(0.0023)	(0.0025)	(0.0033)	(0.0049)	(0.0071)	(0.0028)	(0.0064)	(0.0069)
N. of observations	120,425	120,425	120,425	120,425	120,425	120,425	120,425	120,425
Panel D: Year of broadband availability "t=2002"								
$Close_i \times Post_{st} \times Cohort_{t-1}$	-0.0001	-0.0116	-0.0023	-0.0115	0.0390**	-0.0029	-0.0252	0.0448***
	(0.0061)	(0.0084)	(0.0094)	(0.0135)	(0.0178)	(0.0089)	(0.0177)	(0.0161)
$Close_i \times Post_{st} \times Cohort_{t-2}$	0.0019	-0.0138**	0.0003	-0.0047	-0.0026	0.0015	-0.0399**	0.0226
	(0.0044)	(0.0058)	(0.0075)	(0.0110)	(0.0158)	(0.0056)	(0.0158)	(0.0145)
$Close_i \times Post_{st} \times Cohort_{t-3}$	-0.0002	-0.0016	-0.0046	-0.0255**	0.0151	0.0075	-0.0385***	0.0141
	(0.0041)	(0.0056)	(0.0080)	(0.0110)	(0.0172)	(0.0054)	(0.0144)	(0.0149)
N. of observations	19,702	19,702	19,702	19,702	19,702	19,702	19,702	19,702
State $\times$ year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Firm FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes. This table shows treatment effects of broadband internet on occupational and educational shares using the survival analysis samples. Each column within each panel reports coefficients from the same regression. Regressions include interactions between $Close_i \times Post_{st}$ and cohort of entry. For each treatment year, firms are conditioned to be active for "$2002 - t + 1$" years. The treatment variable $Close_i$ is a dummy that equals 1 for establishments located within 2.5 km to the closest MDF, interacted with a dummy $Post_{st}$ that equals 1 after broadband arrival in state s. The control group comprises establishments outside the treatment perimeter but within a distance of 10 km. Heteroskedasticity robust standard errors clustered at the establishment level in parentheses. Significance levels: *** p<0.01, ** p<0.05, * p<0.1.

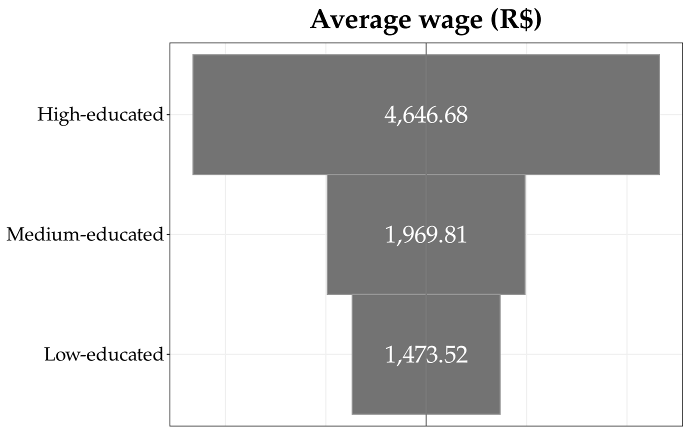
B.3 Figures

Figure B.2: Average Wages by Occupational and Educational Groups

(a) Occupations

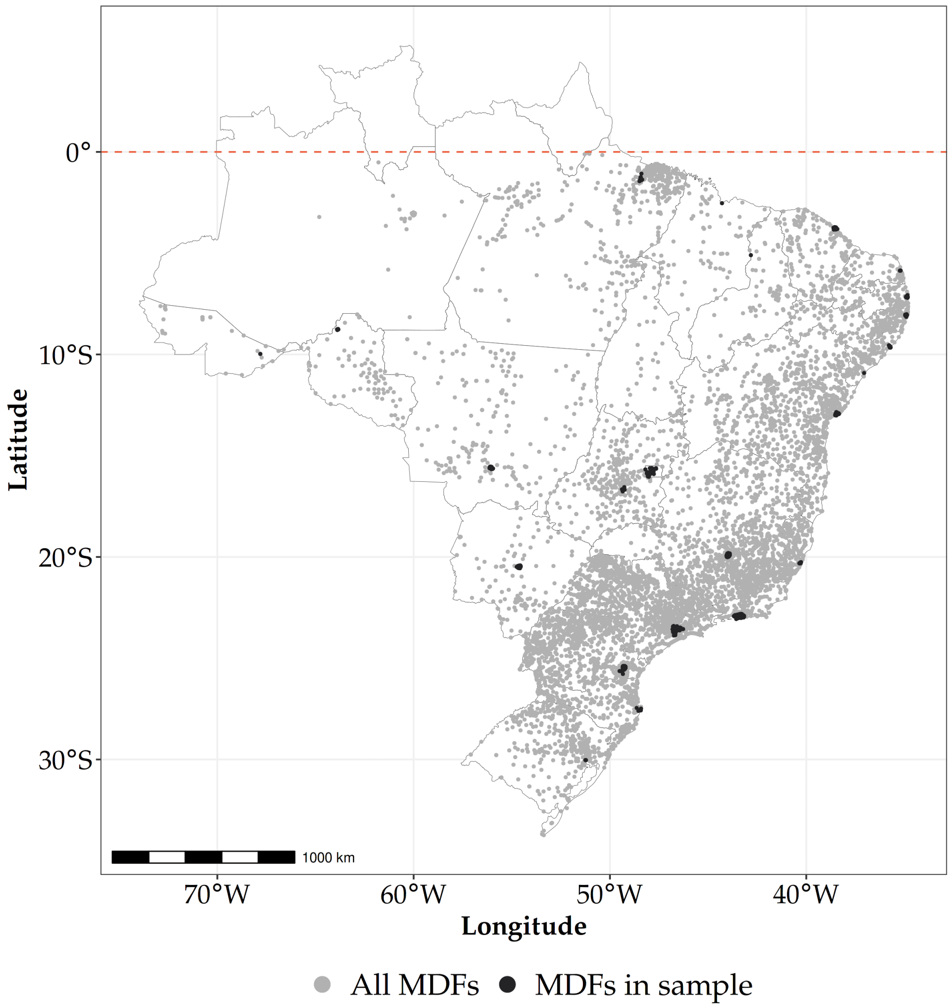


(b) Education



Notes: This figure shows average annual wages of occupational (top panel) and educational (bottom panel) layers in an inverse-pyramid format. Average wages of each worker group are measured in Brazilian reals (R$). Wages are deflated using the 2018 consumer price index (CPI).

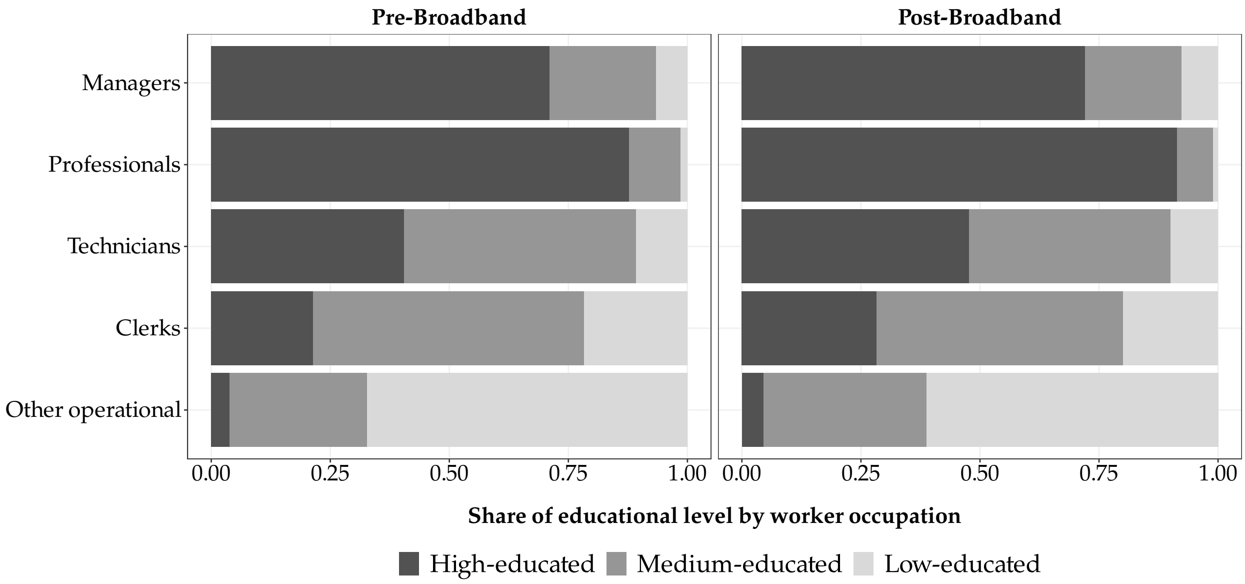
Figure B.3: Spatial Distribution of MDFs in Brazil



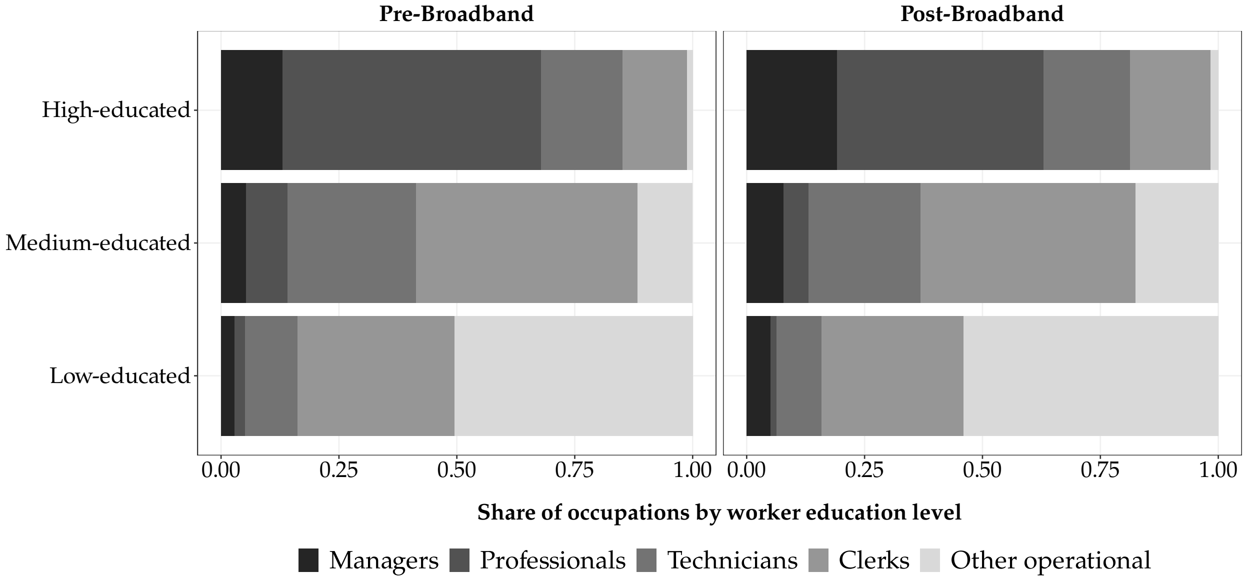
Notes: This map shows the spatial distribution of MDFs in Brazil. Light-gray dots depict all MDFs while black dots represent the MDFs in Brazilian state capitals included in our sample. Grey lines depict state borders. *Source*: Graph based on data from ANATEL.

Figure B.4: Relationship between Occupational and Educational Groups

(a) Occupations



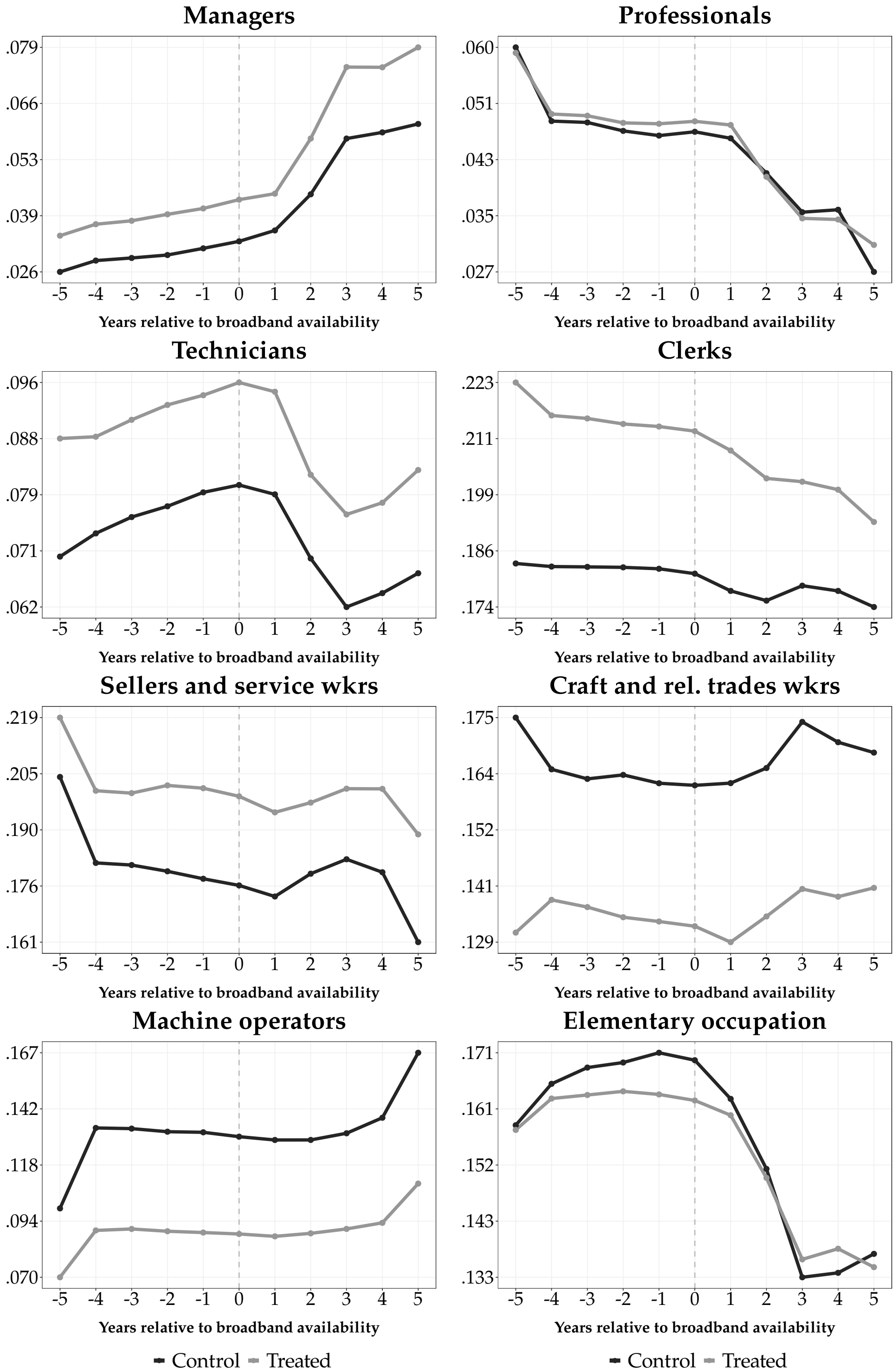
(b) Education



Notes: This figure shows the way in which occupational and educational layers are related. The top panel plots educational shares of workers in the different occupational layers in the pre- and post-broadband period. The bottom panel splits the sample by educational achievement and depicts occupational shares in the different educational groups, separately for the pre- and post-broadband periods. Sample restrictions are described in Section 3.3.2.

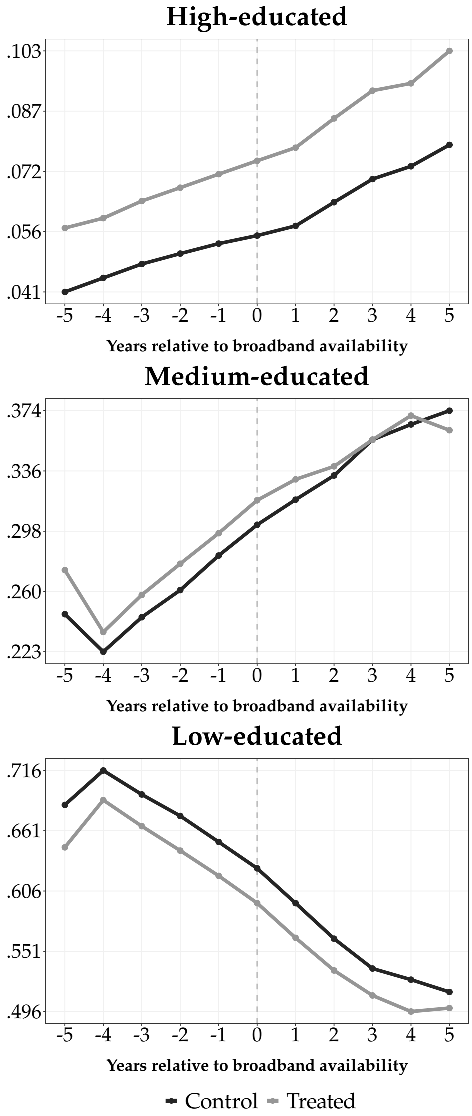
Figure B.5: Unconditional Average Trends in Employment Shares

(a) Occupations



(continuing)

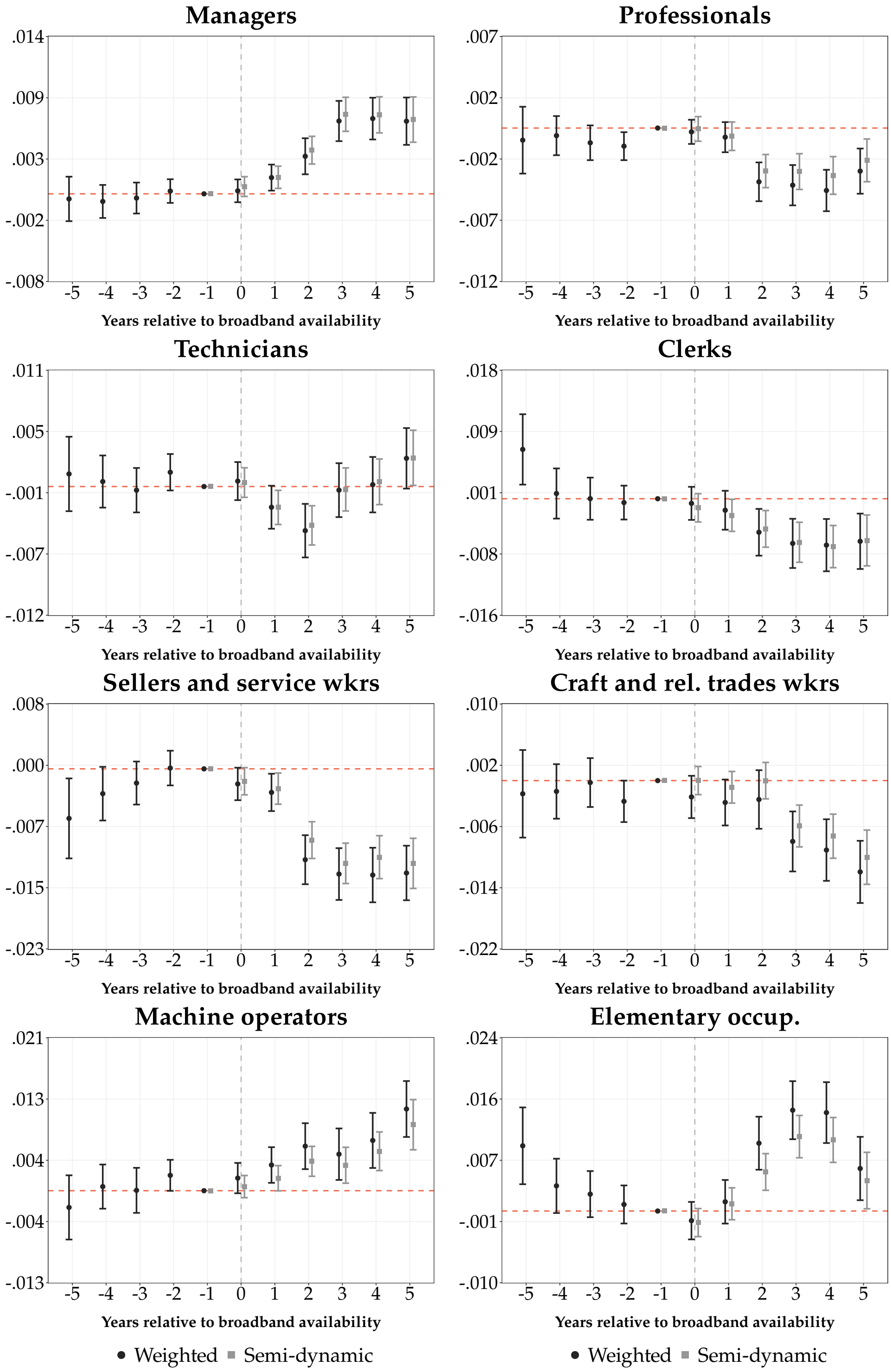
(b) Education



Notes: This figure shows unconditional means of occupational and educational shares before and after broadband availability. In each panel, the light-gray line represents treated firms while the black line represent control firms. Sample restrictions are described in Section 3.3.2.

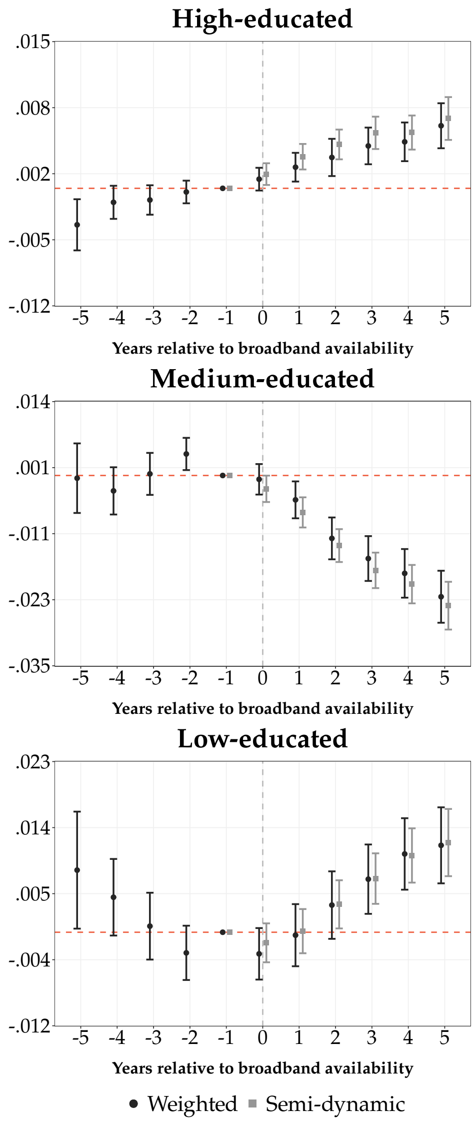
Figure B.6: Effect of Broadband on Employment Shares - Weighted and Semi-Dynamic Effects

(a) Occupations



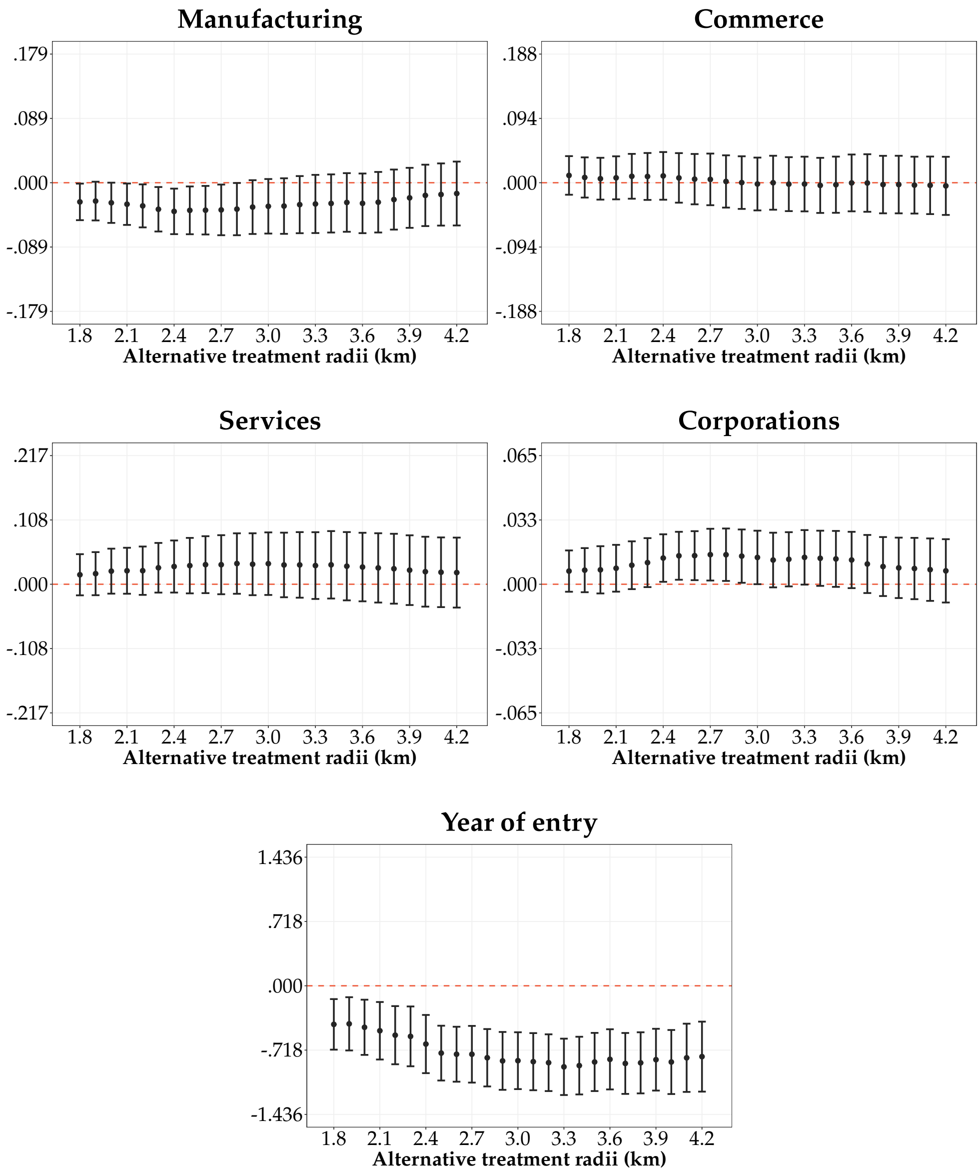
(continuing)

(b) Education



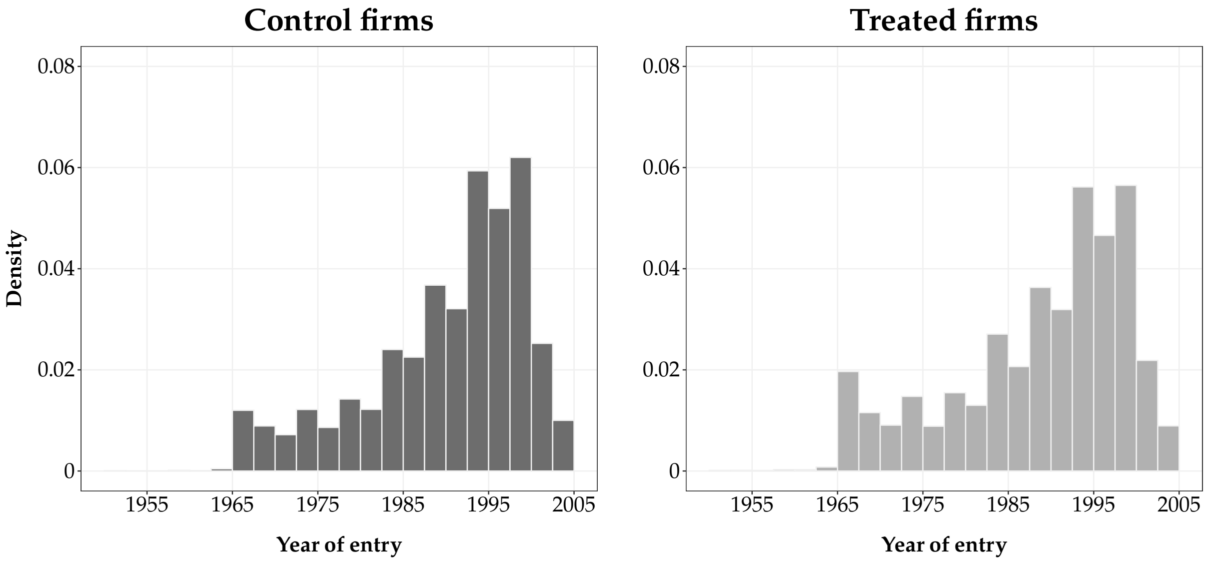
Notes: This figure shows coefficients of event-study estimations as depicted in Equation 3.2. Black dots refer to estimated coefficients when weighting effects by sample size, while light-gray points refer to semi-dynamic estimates. Vertical lines represent 95% confidence intervals. Sample restrictions are described in Section 3.3.2.

Figure B.7: Balance of Treatment and Control Establishments in Terms of Observable Characteristics



Notes. This figure shows the correlation between observable firm characteristics and being located inside different treatment perimeters. Each coefficient stems from a different regression. The dependent variables are dummies for industry (as outlined in table B.3), the year of firm entry, and dummy for legal form. The independent variable of interest is a dummy variable taking the value 1 if a firm is located within different distances ranging from 1.8 to 4.2 km from the closest MDF. The control group comprises firms outside the treatment perimeter but within a distance of 10km. All specifications include specific state × year and MDF fixed effects. Vertical lines represent 95% confidence intervals. Sample restrictions are described in Section 3.3.2.

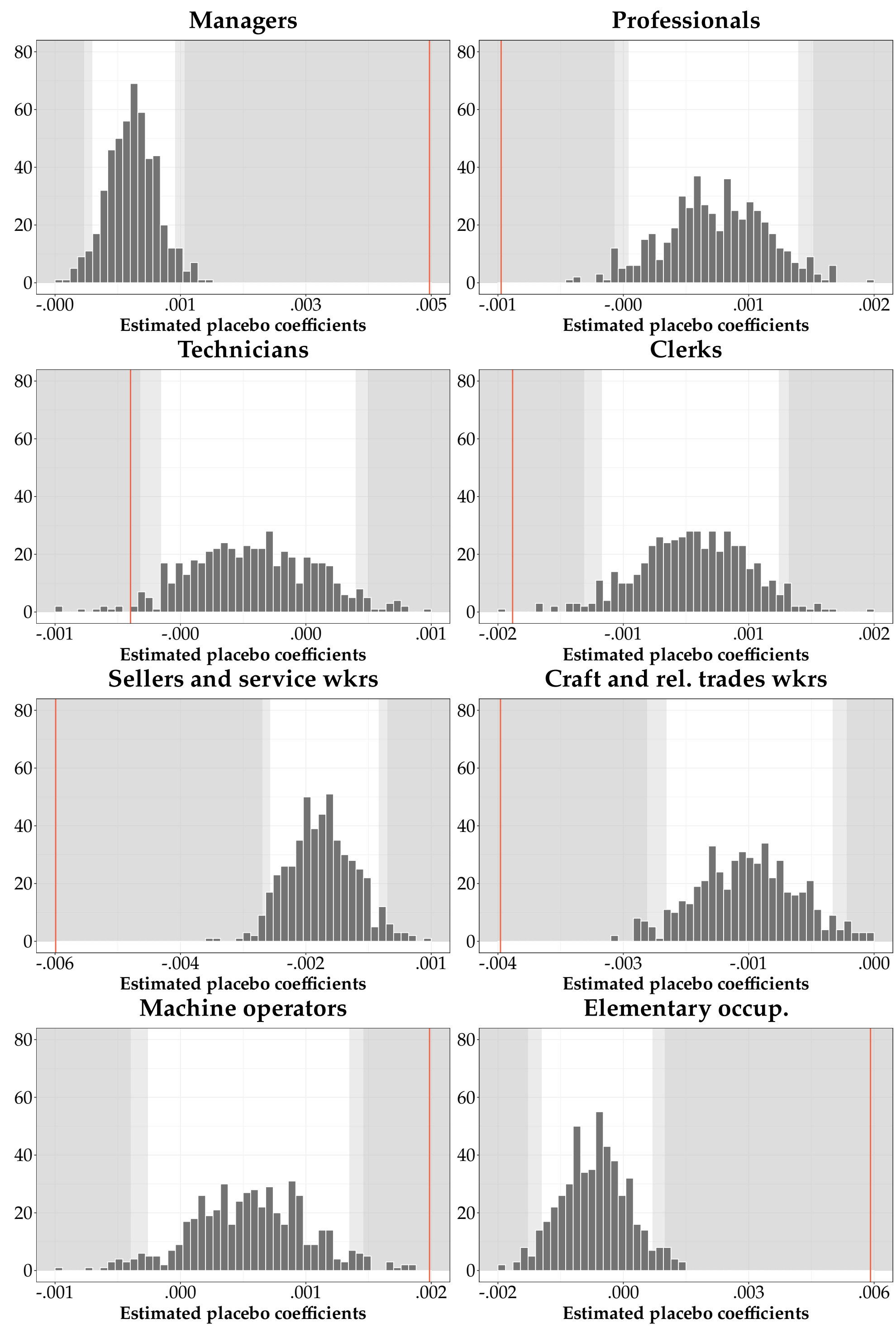
Figure B.8: Distribution of Year of Firm Entry



Notes. This figure shows histograms for the year of firm entry separately for control firms (left-hand side) and treatment firms (right-hand side). The horizontal axis is trimmed to depict years between 1950 and 2005.

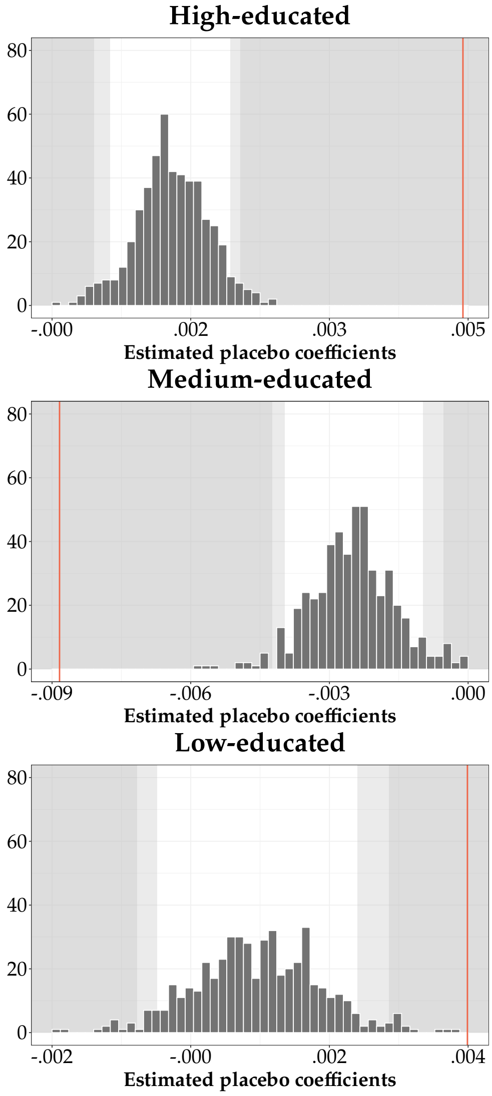
Figure B.9: Permutation Tests

(a) Occupations



(continuing)

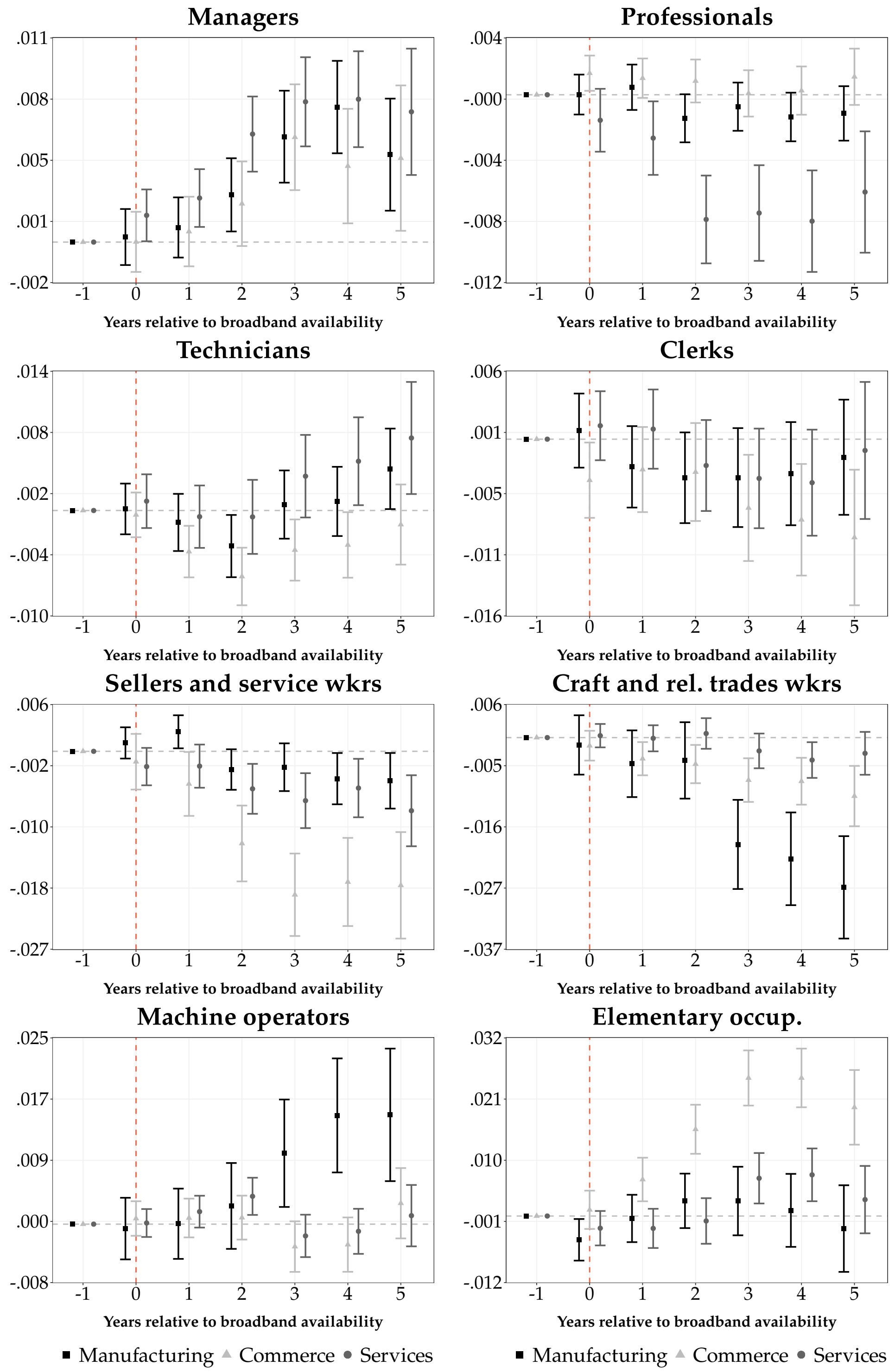
(b) Education



Notes: This figure shows distributions of estimated placebo coefficients obtained from 500 permutation tests randomly shuffling the year of broadband availability to test the null hypothesis that $\beta = 0$. True estimates are represented by the vertical red line. The lighter and darker gray areas highlight the 10 and 5% rejection areas, respectively. Sample restrictions are described in Section 3.3.2.

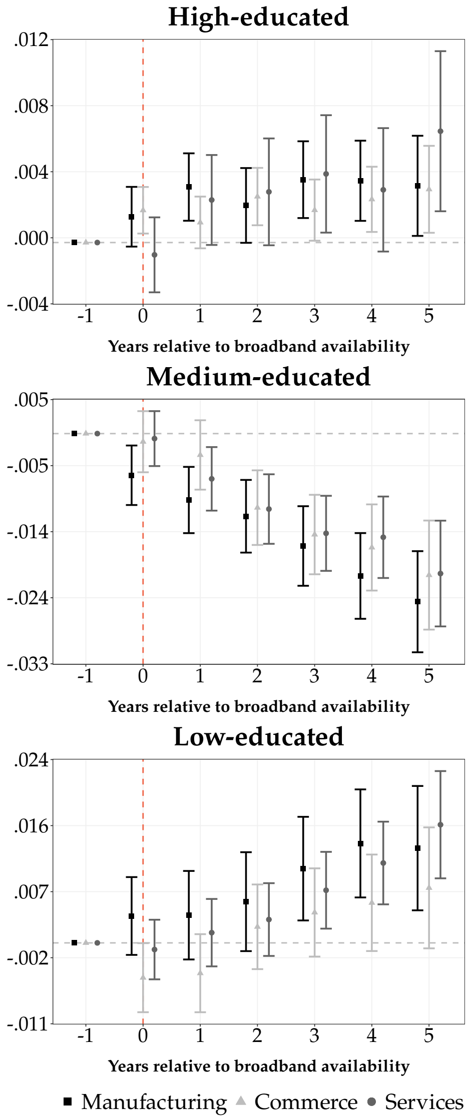
Figure B.10: Effect of Broadband on Employment Shares across Industries - Semi-Dynamic Effects

(a) Occupations



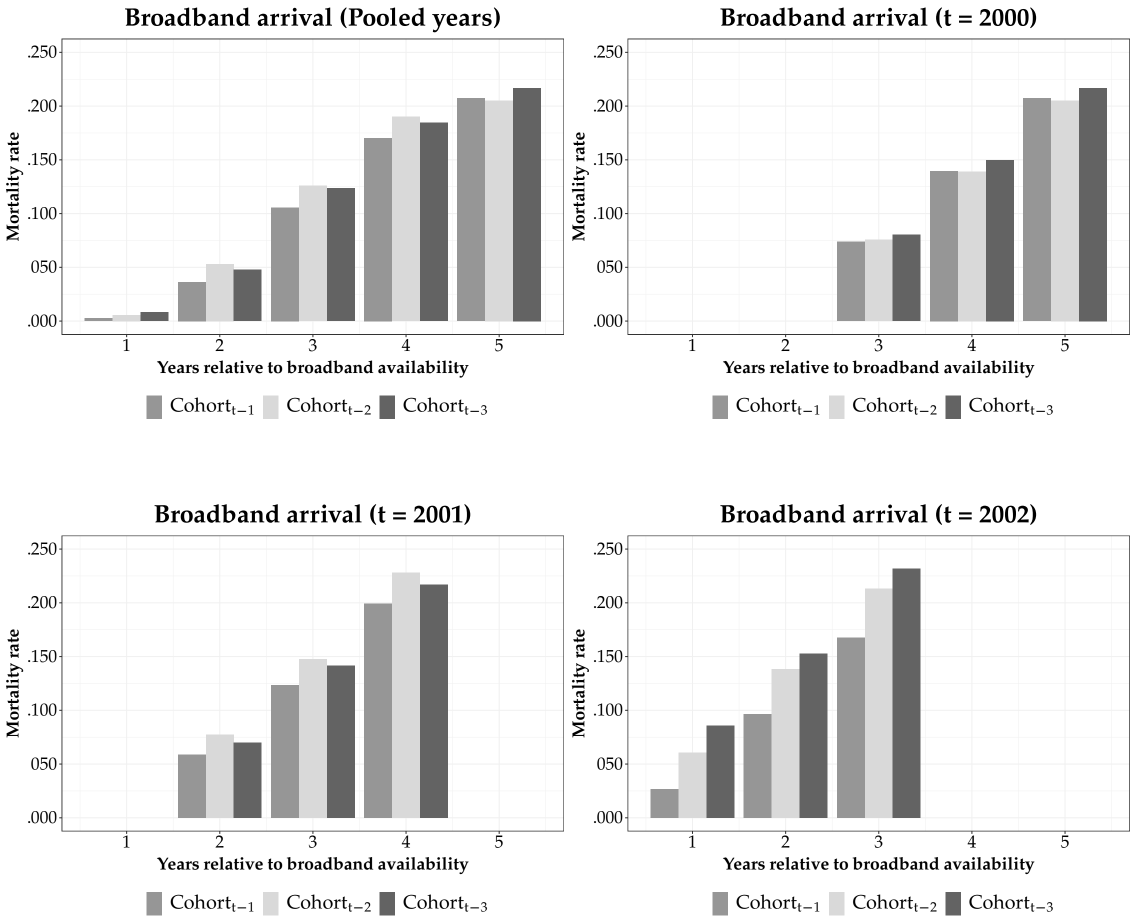
(continuing)

(b) Education



Notes: This figure shows coefficients from a semi-dynamic specification based on Equation 3.2. Squares represent coefficients for manufacturing firms, triangles represent coefficients for commerce firms, and points represent coefficients for service firms. Vertical lines represent 95% confidence intervals. Sample restrictions are described in Section 3.3.2.

Figure B.11: Establishments Mortality Rate by Cohort of Entry



Notes: This figure shows mortality rates for the survival analysis sample. The top left graph shows mortality rates by pooling firms treated in 2000, 2001, and 2002, while the remaining graphs show mortality rates separately for each treatment year.

Chapter 4

Restructuring and Employment Structures – Evidence from German Linked-Employer-Employee Data*

4.1 Introduction

In an increasingly globalized economy, firms often need to adapt in order to remain competitive. In addition to measures that increase the boundaries of the firm, such as mergers and acquisitions, this adaption often involves rather painful restructuring decisions,[1] whereby firms are internally reorganized (Brickley and Van Drunen, 1990; Aksin and Masini, 2008) or parts of the business are divested, spun off, or carved out (Bergh, Johnson and Dewitt, 2008; Eckbo and Thorburn, 2013).[2] One well-known example from the German chemical industry showing that such restructuring events can be very beneficial for both parties is the spinoff of Lanxess AG by Bayer AG in 2005. After an internal restructuring of Lanxess's predecessor company Bayer Polymers AG, which was

*This chapter is based upon an unpublished manuscript by Maier (2021).

[1]In line with Rupp (2012), I define restructuring measures as measures which change or realign the establishment, the firm, or the corporation in parts or entirely (p. 11).

[2]The differentiation between the terms divestures, spinoffs, carveouts and other terms is not consistent in the literature and terms are often used interchangeably. Eckbo and Thorburn (2013) define divestures, spinoffs, and carveouts as follows: A divesture is "the sale of a portion of a firm's assets to a third party ... in a private transaction" (p. 7). In a spinoff, "a public company distributes its equity ownership in a subsidiary to its shareholders" (p. 15), while an equity carveout "is an initial public offering (IPO) of a fraction of the stock in a subsidiary" (p. 25). They additionally define the term "splitoff" as being similar to a spinoff, except that in a splitoff, "shareholders are offered to exchange parent company stock for subsidiary stock" (p. 24). Other authors consider internal outsourcing (Powell and Yawson, 2012) or divestures (e.g. Jain, Kini and Shenoy, 2011; Brauer, Mammen and Luger, 2017) as umbrella terms for the other types of restructuring decisions. In addition, the legal texts discussed in Section 4.2 introduce the term hive-down (see Section 4.2 for a definition). My data, however, does not permit me to differentiate between these restructuring measures. In what follows, I hence use the terms interchangeably and only differentiate between them when this differentiation is important.

split up into the new units Bayer MaterialScience AG and Lanxess,[3] this business unit was entirely spun off.[4] Lanxess largely inherited low-margin and low-growth chemicals and polymer products, giving it a rather difficult start.[5] In the years to come however, Lanxess itself underwent a series of restructuring measures and carved out business units to raise the funds to strategically acquire other companies, which made the company very successful, such that it even temporarily entered the DAX30 in 2012.[6] Such restructuring measures continue to have great economic importance. More recently, Volkswagen AG internally restructured its trucking business to create the new unit Traton[7] comprising Man and Scania, which it subsequently spun off in 2019.[8] After a similar internal restructuring in which Daimler AG first restructured into a holding company containing a car, a truck, and a financial services division, it announced to split the car from the truck business to create two stand-alone companies in 2021.[9]

In economics and business research, the determinants of firm boundaries and the ways in which firms are organized have been key interests ever since the seminal paper by Coase (1937). While expansion of firm boundaries via vertical integration is relatively well-studied, there is fewer research on restructuring events that move parts of the business outside the boundary of the firm, i.e. vertical disintegration (Jain, Kini and Shenoy, 2011). There are various strategies how firms or units within firms can shrink their boundaries, among these divestures, carveouts, spinoffs, or internal restructuring decisions, such as internal outsourcing or the development of shared service centers (Aksin and Masini, 2008). The incentives and reasons for value creation of such restructuring events are manifold and briefly discussed hereafter.[10]

As restructured functions are usually not part of the firm's core competencies, divesting them might allow firms to increase focus, thereby leading to a more efficient management of core assets, eliminating negative synergies between business units, and limiting costly cross-subsidization and inefficient allocation of resources to poorly performing units (e.g. John and Ofek, 1995; Daley, Mehrotra and Sivakumar, 1997; Gertner, Powers and Scharfstein, 2002; Dittmar and Shivdasani, 2003; Ahn and Denis, 2004). Divesting a unit and selling it might also be beneficial if there is a better fit between the divested unit and the buyer, as compared to the seller, or if the buyer exhibits higher productivity (John and

[3]See https://www.wallstreet-online.de/nachricht/9798642-lanxess-bayers-erfolgreich-ster-spinoff/all (November 25, 2021).

[4]See https://www.handelsblatt.com/unternehmen/industrie/tauschverhaeltnis-fuer-spinoff-bekannt-gegeben-lanxess-startet-mit-milliarden-hypothek/2412598.html (November 25, 2021).

[5]See https://www.ft.com/content/261f6a6a-ff75-11d8-be93-00000e2511c8 (November 25, 2021).

[6]See https://www.ft.com/content/aff023c0-00ce-11e2-9dfc-00144feabdc0 (November 25, 2021).

[7]See https://www.ft.com/content/443ddd84-45a5-11e9-b168-96a37d002cd3 (November 25, 2021).

[8]See https://www.ft.com/content/2e63f626-9913-11e9-9573-ee5cbb98ed36(November 25, 2021)

[9]See https://www.ft.com/content/64910c9a-75c4-4e7b-9ca6-2d35c009498d (November 25, 2021).

[10]For more details and potential further reasons, see Eckbo and Thorburn (2013), which is a literature review on the topic. In the following argumentation on reasons to and results of restructuring, I use a small fraction of the literature discussed in this exhaustive review and update it with more recent literature.

Ofek, 1995; Maksimovic and Phillips, 2001). Restructuring measures can also mitigate agency problems, such as internal power struggles, causing allocation of resources to and rent-seeking of poorly performing units (e.g. Rajan, Servaes and Zingales, 2000; Scharfstein and Stein, 2000) and information asymmetry between managers and investors (Best, Best and Agapos, 1998; Krishnaswami and Subramaniam, 1999; Gilson et al., 2001; Bergh, Johnson and Dewitt, 2008). In addition, they might lead to increased managerial discipline (Chemmanur and Yan, 2004; Chemmanur, Krishnan and Nandy, 2014). Selling assets might also be financially beneficial for both the parent firm and the subsidiary. In particular, selling parts of the firm might both alleviate financial constraints of the parent company and serve as a mechanism for financing more efficient investment projects (Lang, Poulsen and Stulz, 1995; Allen and McConnell, 1998; Vijh, 2002; Dittmar and Shivdasani, 2003) and lower the cost of capital for financing high-growth subsidiaries (Schipper and Smith, 1983).[11]

Instead of divesting parts of the business, many firms also change their internal structure to generally strengthen the firm (Powell and Yawson, 2012), to cope with market pressures, and to reduce duplication of business functions, thereby increasing efficiency and reducing costs (Brickley and Van Drunen, 1990). In particular, services are often centralized in a shared services center, a process also known as internal outsourcing. This might help "companies save costs, increase available time for value-added activities in line positions, improve measurement capability, and achieve better service quality due to a more focused management attention" (Aksin and Masini, 2008, p. 240). Costs can furthermore be saved via the standardization of processes and economies of scale (McIvor, McCracken and McHugh, 2011). From the perspective of the single establishment, these measures again result in a decrease of establishment boundaries. The reasons to conduct restructuring measures that decrease firm or establishment boundaries are thus relatively well-studied. Other than their impact on performance, which will be discussed in more detail below, research on their effects is comparatively scarce.

My data allows me to explore three particular restructuring measures: an internal restructuring measure that relocates parts of the establishment to other units within the same firm, a separation measure (such as a spinoff, carveout, or divesture) that takes parts of the establishment outside the boundaries of the firm to be continued as an independent business, and a layoff measure, during which parts of the establishment are completely closed down. As Powell and Yawson (2012) provide compelling evidence that layoffs are quite different from other restructuring measures both in terms of determinants and

[11] According to Eckbo and Thorburn (2013), further reasons for value creation include wealth transfers from bond- to shareholders and so-called clientele effects, meaning that the spinoff increases investors' possibilities for diversification. In addition, spinning off parts of the firm might increase the takeover possibility, which allows shareholders to sell their shares at a premium. Carveouts, in particular, might also create value because they facilitate future restructuring decisions. There are potential further reasons discussed in the literature, which, due to the restrictions in the scope of this review, are not discussed in further detail.

results, I focus on relocation and separation measures in my main analysis but always contrast results with a specification in which the "closed down" alternative is included in the definition of the main independent variable of interest. I also focus on larger firms with more than 50 workers who are required to pay mandatory social security contributions (henceforth "typical" workers) at the time of entering the data set but always compare results to a less restrictive specification in which I also include smaller firms exhibiting more than 10 typical workers. I analyze the impact of these restructuring measures on the educational and occupational labor structure of employees working in these establishments, which is largely unexplored by the literature so far. Due to the novelty of the topic, this paper is largely explorative in nature, such that I do not develop hypotheses on the direction of effects but rather rationalize them based on adjacent literature. I make use of the Linked-Employer-Employee-Data (LIAB) of the Institute for Employment Research (IAB), which is a large German linked-employer-employee panel data set combining detailed questionnaire data on establishments from the IAB Establishment Panel[12] with information on employees working in these establishments from administrative sources.[13] These rich annual data are available for all years from 1993 on, sample establishments multiple times, and thus make panel analysis possible. In this paper, I use the data between 2009 and 2017 and employ fixed effects regressions and an array of establishment-level controls to account for omitted variable bias. In addition, by using different sets of lagged controls, I am able to make establishments more comparable in the period before the restructuring measure is conducted.

I find significant rearrangements in the labor structure following restructuring events. I first show that these measures result in sizeable decreases in establishment-level employment. In fact, estimations suggest that employment declines by more than 30% following a restructuring event. When subdividing workers by education, results show that employment shares of educational groups change significantly. While the share of the most common educational group in Germany, workers with occupational education, decreases, the employment share of the highest educational layer, workers with university degree, increases by almost 12%. Results of estimations analyzing the overall number of workers in different educational groups reveal that these changes in shares occur because employment of some educational groups declines more than employment of others. In

[12]This data is on the establishment level. The IAB defines an establishment as a "a regionally and economically separate unit, in which employees liable to social security work" (Fischer et al., 2008). It thus comprises both single-establishment firms and establishments which are part of a multi-establishment firm. For the sake of readability, the terms "firm", "establishment", and "plant" are used interchangeably in this paper.

[13]In particular, this study uses the cross-sectional model of the Linked-Employer-Employee Data (LIAB) 1993 - 2017 (version 1) made available by the IAB (Schmidtlein et al., 2019, DOI: 10.5164/IAB.LIABQM29317.de.en.v1). Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the IAB and subsequently remote data access. For data documentation, see Alda and Herrlinger (2005), Fischer et al. (2009), and Schmidtlein, Seth and Umkehrer (2019). Section 4.4.1 discusses the data in more detail.

particular, estimates suggest that while the number of workers with university degree declines by a comparatively low 24%, the respective number of workers with occupational education decreases by more than 32%. Hence, results suggest a significant degree of educational upgrading of workers remaining in the organization. When subdividing the labor force into occupational layers, I find that only the management layer gains in terms of shares, intermediate layers do not experience statistically significant changes, and low occupational layers lose. More specifically, estimates suggest an expansion of the management layer of 27.5%, which is also economically significant. Again, these rearrangements are driven by some occupational groups losing more than others in terms of size. While employment of managers declines by just 15.4%, employment of the lowest occupational group, "other operational", declines by 41.6%. These results again suggest a skill bias towards the top of the occupational distribution. In supplemental analyses, I furthermore analyze the task complexity of jobs.[14] This analysis again reveals a tendency towards firms retaining employees conducting relatively complex tasks. I conduct further analyses to verify the robustness of my results and to provide further exploratory insights. Taken together, the documented skill bias hints towards firms strategically using restructuring to increase their average labor quality. This suggests that, equivalently to private equity (PE) buyouts, restructuring might serve as a vehicle to catalyze organizational change (Davis et al., 2014) and, in particular, carry out technological modernization (Antoni, Maug and Obernberger, 2019) resulting in a pattern of skill-biased technological change (SBTC).[15] Given this increase in average labor quality, one would expect a positive effect on average wages. Coefficients are positive, yet statistically insignificant. In unreported analyses, I dig deeper, analyze average wages of the different occupational and educational groups, and do not find any consistently significant coefficients. Hence, results suggest that there are no large impacts on average wages of workers remaining in the firm and that the positive but insignificant coefficients of average wages stem from the fact that average labor quality increases.

This paper contributes to several streams of literature. First and foremost, I contribute to the literature on the ways in which organizational change and measures that shrink the boundaries of the firm affect labor markets. Previous literature evaluating such effects has focused on outsourcing and on private equity (PE) buyouts. According to my best knowledge, similar evidence for restructuring decisions is missing. The outsourcing literature has thereby largely analyzed wage effects for the outsourced workers. Abraham and Taylor (1996) focus on the U.S. and janitorial services, evaluate different motivations

[14]Note that the task complexity measure is based on the last digit of the German Classification of Occupations 2010, which I also use to classify the occupational layers. For a more detailed description, see Section 4.4.

[15]Skill-biased technological change (see e.g. Card and DiNardo, 2002, for a discussion) posits that technology complements skilled and substitutes for unskilled labor, wherefore the relative demand for more skilled labor groups should increase to the detriment of less skilled ones.

for outsourcing and, among other results, find that wage cost savings seem to be a primary motivation to contract out these non-core services. Berlinski (2008) also makes use of U.S. data to evaluate wage differentials between janitors and security guards employed by a contractor as compared to the same types of workers that are employed in-house and finds that, conditional on the industry workers are assigned to, the prior group earns 15% to 17% less. Dube and Kaplan (2010) employ a U.S. data set to estimate whether guards and janitors outsourced to service contractors experience a differential in terms of wages and benefits and find that this is the case indeed. Furthermore, they find that outsourced workers unionize at lower rates and that outsourcing occurs mainly in high-rent industries, consistent with the view that firms outsource in order to exclude non-core workers from firm- or industry-specific rents. By focusing on cleaning, security, logistics, and food services in Germany, Goldschmidt and Schmieder (2017) analyze domestic, on-site outsourcing to business service providers and find a negative effect of moving jobs outside the firm boundaries on the wages of outsourced workers of approximately 10-15%. In an unpublished yet very related contribution, Brändle (2015) also uses the LIAB data to analyze how firms that combine international outsourcing (offshoring) with a mass layoff differ from non-offshoring firms in terms of employment structure before the measure and how they change as a result of it. He focuses on the task content of jobs and finds that offshoring firms are characterized by a higher share of non-routine interactive jobs before the measure, lay off workers that largely perform routine cognitive and routine manual tasks and are afterwards characterized by a higher share of non-routine interactive and non-routine cognitive tasks.

The literature on PE buyouts focuses on impacts on the labor structure of target firms more directly and also provides theoretical explanations for observed changes.[16] Davis et al. (2014) use U.S. data combining the firm with the establishment perspective to analyze how PE buyouts affect employment, wages, and total factor productivity of target firms and single establishments within these firms. They find that target firms engage in significantly more job creation and destruction, such that they decrease employment in existing establishments but conduct more greenfield job creation, divestures, and acquisitions, resulting in only moderate net employment losses of less than 1%. Wages in preexisting establishments decrease but total factor productivity of target firms as a whole increases. Overall, findings show that PE buyouts catalyze the process of creative destruction and benefit target firms via the reallocation of resources and a higher productivity. Based on the observation that PE acquisitions often serve as a vehicle for major organizational change and, in particular, large investments into IT, Agrawal and

[16]Note that the literature on PE buyouts is very large. Since the focus of the paper at hand is on restructuring, a thorough discussion would go beyond its scope. Hence, I restrict myself to reviewing the papers also named as very related by Antoni, Maug and Obernberger (2019) regarding the impact of PE buyouts on employment and wages. I do, however, recognize that there is further important literature on the topic.

Tambe (2016) examine whether they create positive spillovers for employees working in target firms. The authors argue that particularly workers in jobs heavily affected by information technology might benefit from resulting IT-investments via the acquisition of new skills and improved labor market outcomes. By analyzing U.S. data, the authors find this to be the case indeed, such that workers in acquired firms experience longer employment and shorter unemployment spells, higher long-term wages, and a general increase in employability. Olsson and Tåg (2017) examine the Swedish case and find that PE buyouts do not have aggregate employment effects but that low-productivity workers performing routine tasks experience higher unemployment incidences as compared to their peers. They also find that target firms exhibiting a relatively low productivity increase intangible assets after the buyout and experience productivity increases. The authors explain their findings by PE buyouts alleviating agency problems that prevent managers of target firms (before the buyout) to adequately respond via job polarization[17] to general market trends such as automation or offshoring. The adjustments after the buyout then result in a higher firm productivity. Antoni, Maug and Obernberger (2019) finally employ German data to analyze the ways in which PE buyouts affect overall employment and individual employees. They find that target firms both replace individual workers and reduce overall employment and that workers suffer significant losses in terms of earnings. They also evaluate the applicability of different theories, in particular organizational stream lining, predicting a decrease in administrative staff and managers, technological modernization (i.e. SBTC and job polarization), and transfer of wealth from (older) target firm employees to new owners. As they find higher employment and wage losses for white collar workers but not for managers, no large changes in the composition of the workforce, and no particularly adverse effects for older workers, they conclude that neither one of these theories is fully able to explain results.

I contribute to this line of literature by showing that relocation and separation restructuring measures have huge negative impacts on overall establishment-level employment and are accompanied by significant changes in the labor structure. My results are thus consistent with the notion that, similarly to evidence on PE buyouts by Davis et al. (2014), restructuring measures serve as a tool to catalyze organizational change. Since my findings show educational upgrading and a shift towards higher layers in the occupational hierarchy and more complex tasks, they are generally in line with SBTC predictions, thus suggesting that firms strategically use restructuring measures to adjust their labor force to technological developments. In contrast to prior literature analyzing both the wages of workers leaving the firm during an outsourcing measure and workers remaining in

[17]Job polarization is based on the observation that both high- and low-wage employees have increased their employment shares to the detriment of medium-wage employees, who have experienced a decline in employment shares. Autor, Levy and Murnane (2003) explain this empirical observation by medium-skilled employees conducting a high fraction of easily programmable routine tasks, which can be substituted by technology.

the firm after a PE buyout, I do not find any wage effects for remaining workers after a restructuring measure.

Second, I also add to the small literature that specifically focuses on the ways in which restructuring measures affect managers. In particular, Seward and Walsh (1996) focus on how restructuring measures influence internal corporate control mechanisms of the spun off entity and show that this entity is typically run by an inside CEO from the original combined firm. They also show that, as a result of restructuring, these CEOs can now receive a performance-related, market-based compensation package and that they are primarily controlled by outside directors. Wruck and Wruck (2002) examine the ways in which spinoffs restructure top management. They show that spinoffs do not just move a division outside the boundaries of the firm but significantly restructure top management. Among a multitude of other findings, they find that the top management layer of the spinoff firm typically consists of top managers (both from the original firm and other firms) and former division managers of the original firm, thus combining division managers' firm-specific knowledge with top managers' general human capital regarding corporate governance. They also show that the market reaction to the spinoff announcement strongly depends on the composition of the spinoff firm's management team, such that the positive reaction is strongest when the new top management team combines a former division head with a top manager "jumping ship" from the parent to the spinoff firm. Feldman (2016) analyzes whether spinoffs lead to a better incentive alignment between management compensation and stock market performance and indeed finds this to be the case for spinoff managers but not for parent firm managers.

I contribute to this line of literature by showing that relocation and separation measures increase the share of managers in a both statistically and economically significant fashion, thus making managers relatively more important within the firm. This is in line with the theory on knowledge-based hierarchies, also called "management by exception" (e.g. Garicano, 2000). This theory states that, while workers specialize in solving routine problems, they ask managers when they face nonroutine problems. Hence, the more nonroutine problems they are confronted with, the more important management tasks become and the more managers (relative to the overall workforce) a firm needs to employ. My results thus suggest that relocation and separation restructuring measures lead to more nonroutine problems, at least in the initial post-restructuring phase, wherefore a higher share of managers is needed to solve them.

Finally, I add to the literature on the impacts of restructuring measures on firm performance. Even though Brauer (2006) mentions "mixed results" of prior literature with respect to the divesture-performance relationship and outlines that effects are context-specific, the direction prior literature finds is predominantly positive (Lee and Madhavan, 2010).[18] The existing work thereby focuses both on the parent company, the divested,

[18]Note that literature on layoffs finds negative stock market reactions (Elayan et al., 1998; Chen et al., 2001;

and both units. In particular, Schipper and Smith (1983), Klein, Rosenfeld and Beranek (1991), John and Ofek (1995), Dittmar and Shivdasani (2003), Veld and Veld-Merkoulova (2008), Jain, Kini and Shenoy (2011), Sun and Shu (2011) and Prezas and Simonyan (2015) all document positive abnormal stock price reactions to divesture, spinoff or carveout announcements for the parent company. Klein, Rosenfeld and Beranek (1991) additionally show positive effects on subsidiary share prices if the parent company announces a selloff of these shares. John and Ofek (1995) furthermore examine different accounting performance measures and find that the divesting firm's remaining assets are more profitable in the post-divesture period. Dittmar and Shivdasani (2003) and Gertner, Powers and Scharfstein (2002) find a more efficient investment behavior of both the units remaining with the parent firm and the spun-off entities, respectively. Dittmar and Shivdasani (2003) and Burch and Nanda (2003) additionally observe a decrease in the diversification discount.[19] Maksimovic and Phillips (2001) and Chemmanur, Krishnan and Nandy (2014) analyze the impact of asset sales and spinoffs on total factor productivity. While the prior work finds an increase in total factor productivity for the transferred units, the latter finds that spinoffs are associated with improvements in overall total factor productivity of the parent company and the transferred unit. These are driven by cost savings rather than increases in sales and by plants remaining with the parent company rather than spun-off units. Similarly to the study at hand, Brickley and Van Drunen (1990) analyze both internal and external restructuring measures (i.e. changing the number of divisions or subsidiaries) and find a positive stock price reaction as well. Interestingly, they also document a short-term decline in earnings performance in the years following the restructuring measure, which they attribute to an increase in short-term expenses. Powell and Yawson (2012) analyze the impact of internal restructuring measures on firm survival and find a positive impact of divestures on firm survival. In a very recent contribution, Girod and Whittington (2017) differentiate between restructuring and reconfiguration and find positive (negative) performance effects for restructuring (reconfiguring) firms.[20] Bergh and Lim (2008) and Brauer, Mammen and Luger (2017) analyze spinoffs and selloffs and only selloffs, respectively, and find that the positive impact of these measures on firm accounting performance is moderated by (the specific type of) firms' previous spinoff and selloff experience. In a study very related to the paper at hand, Li (2013) looks at takeovers and finds that the acquiring firm increases the productivity of the target firm via a more

Hillier et al., 2007) and a decrease in survival probability (Powell and Yawson, 2012). As outlined above, this shows that pure layoffs are rather different from other (internal) restructuring measures, wherefore I do not analyze layoffs in my main specification.

[19]The concept of the diversification discount is based on empirical observations that show that "diversified firms trade at a discount relative to nondiversified firms in their industries" (Campa and Kedia, 2002, p. 1731).

[20]The authors define "restructuring" as changes "in fundamental principles of organizational design", while reconfiguration is defined as "unit changes within existing organizational principles" (Girod and Whittington, 2017, p. 1121). Restructuring hence changes the organization in a more fundamental way as compared to reconfiguring.

efficient usage of both capital and labor.

I contribute to this line of research by showing that restructuring measures significantly reshape the within-firm employment structure and particularly induce a skill bias towards labor with rather high education levels working in high positions and conducting complex tasks. Consistent with the SBTC argument outlined above, I propose that firms use restructuring measures to adjust their labor force to new technology, thereby increasing firm productivity. Furthermore, there is considerable evidence that more qualified labor is also more productive, which then translates into a higher firm productivity (e.g. Fleisher et al., 2011; Kampelmann and Rycx, 2012; Benos and Karagiannis, 2016). The higher average labor quality in the post-restructuring period alone hence could be a channel via which restructuring firms achieve performance gains.

The paper proceeds as follows: Section 4.2 provides information on the institutional background governing restructuring measures in Germany. Section 4.3 outlines the empirical strategy, while section 4.4 discusses data used, data manipulations applied, and some key descriptive statistics on the final sample. In Section 4.5, results of the empirical exercise are discussed. Section 4.6 concludes.

4.2 Institutional Background

This chapter summarizes the German institutional background and especially the laws and institutions that govern restructuring decisions.[21] In particular, the focus is on the implications that these restructuring measures have on employees. In general, companies in Germany are allowed to choose their organizational structure and legal form and are also allowed to change it. Legally, these changes are governed by the German Works Constitution Act (*Betriebsverfassungsgesetz*) and Transformation Act (*Umwandlungsgesetz*). It is important to know that, according to section 1 of the Works Constitution Act, in all establishments with more than five employees who are eligible to vote, the employer cannot prevent the election of a works council. Depending on the type of restructuring measure and the implications it has on employees, this works council has considerable voice. The independent variable of interest that I analyze is based on the following IAB Establishment Panel question: *"Were parts of this establishment closed down or relocated with other company units between 1 July 2016 and 30 June 2017,[22] or separated and continued as independent businesses?"* Respondents are thereby allowed to cross one or multiple items.

[21] Note that, within the scope of this chapter, only the most important laws and their implications will be named, as a more thorough analysis of the institutional and legal background is not the objective of this paper.

[22] Note that this formulation is from the English translation of the 2017 IAB Establishment Panel questionnaire. The translations (but not the original questions in German) change over time. The questionnaires can be downloaded from the FDZ homepage under https://fdz.iab.de/en/FDZ_Establishment_Data/IAB_Establishment_Panel.aspx (November 25, 2021).

As outlined above, I focus on the relocation and separation measures. These can legally be classified as establishment alterations (*Betriebsänderungen*) or as transfers of businesses (*Betriebsübergänge*) or a combination of the two.

Relocation, in the sense of the questionnaire item, is an internal restructuring measure (this could be, for example, the setup of a shared service center), constitutes a fundamental change in establishment organization and is hence an establishment alteration.[23] In general, an establishment alteration is any "alteration of organization, structure, area of activity, operation, production, location etc. (own translation based on Dimartino, 2019, p. 54)," which "may entail substantial prejudice to the staff or a large sector thereof" (see Works Constitution Act[24], section 111). In particular, section 111 specifically mentions transfers "of the whole or important departments of the establishment" as establishment alterations. These alterations result in the following legal obligations: in case the establishment has more than 20 workers eligible to vote, management has to inform the works council about the planned measures in a timely and thorough fashion and discuss them with the works council. In line with section 112 of the Works Constitutions Act, management and works council can agree on a reconciliation of interests (which is optional) but have to agree on a social compensation plan which should help alleviating potential economic disadvantages that arise as a result of the restructuring measure (Seel, 2010; Dimartino, 2019).[25]

The separation measure, in contrast, constitutes a transfer of business, which might or might not occur in conjunction with an establishment alteration. During a pure transfer of business, the establishment or parts of the establishment are transferred from an original to a new owner via a legal transaction under retention of identity (Dimartino, 2019).[26] More specifically, the analyzed separation measure implies a division of the firm or establishment for the purpose of transferring assets, which is defined and regulated by sections 123 to 173 of the German Transformation Act.[27] In particular, due to the rather unspecific formulation in the questionnaire, the separation measure can both comprise a spinoff (*Abspaltung*), as defined by section 123, subsection (2), or a hive-down (*Ausgliederung*), as defined by section 123, subsection (2).[28] Section 324 regulates the rights and obligations

[23]See Rupp (2012), p. 37 for an illustrative example of such an establishment alteration.

[24]The English version of the Works Constitution Act that is used for these quotes can be downloaded from the homepage of the Federal Office of Justice of the Ministry for Justice and Consumer Protection under `https://www.gesetze-im-internet.de/englisch_betrvg/index.html` (November 25, 2021).

[25]In case management and works council do not reach an agreement on a social compensation plan, such a compensation plan can be enforced by the reconciliation board of the German employment agency (see Works Constitution Act, section 112, subsections 2 to 5).

[26]Economic identity is defined as the "organizational entirety comprising people and/or objects for the purpose of a permanent exercise of an economic activity (own translation based on Dimartino, 2019, p. 49)." Whether or not identity is retained needs to be evaluated on a case-by-case basis (Dimartino, 2019).

[27]I use the English translations of the Transformation Act, which can be downladed from the website of the Federal Office of Justice of the Ministry for Justice and Consumer Protection under `https://www.gesetze-im-internet.de/englisch_umwg/index.html` (November 25, 2021) for all formulations that apply to this law.

[28]The difference between a spinoff and a hive-down is that during a spinoff, the shareholders of the old owner obtain shares of the new owner (whether it already exists or is newly founded), while during a

that apply to such business transfers and states that "Section 613a subsection (1) and subsections (4) through (6) of the Civil Code (BGB)[29] shall not be prejudiced by the effects of a merger, a division into several enterprises, or an asset transfer having been entered in the register." Hence, in case economic identity is maintained, the rights and obligations that apply to such a transfer of business are regulated by Section 613a of the German Civil Code (Diepholz and von Horn, 2008). This section states in general that the new owner succeeds in all rights and duties resulting from existing employment relationships. Section 613, subsection (4) specifically states that "termination of the employment relationship of an employee by the previous employer or by the new owner due to transfer of a business or a part of a business is ineffective", wherefore layoffs due to transfers of businesses are not allowed. In case conditions are fulfilled, management does not have to involve the works council (Dimartino, 2019). As mentioned above, this applies only to cases in which ownership of establishments or parts of establishments is transferred without altering the establishment. In case the transfer of business is associated with alterations such as layoffs however, the above mentioned laws governing alterations apply as well (Rupp, 2012), which means that the works council has to be included in the decision-making process in a similar fashion as in the relocation measure.

The next section outlines the empirical strategy that I use to estimate the impact of the discussed restructuring measures on the within-firm employment structures of establishments undergoing restructuring.

4.3 Identification Strategy

In order to estimate the relationship between corporate restructuring, within-firm employment and wages, I estimate the following two-way fixed effects regression in the spirit of a differences-in-differences (DD):

$$y_{it} = \beta \times Res_{it} + \delta_i + \eta_t + Controls_{it} + \phi_{it-1} + \epsilon_{it} \tag{4.1}$$

, with y_{it} being the respective dependent variable of interest for firm i at time t and Res_{it} being the restructuring proxy and thus the key independent variable. In particular, Res_{it} is a dummy taking the value one in year t and all future periods if establishment i indicates that it either relocated or separated parts of the establishment.[30] The variable remains one in all periods after the measure has been conducted because I expect it to

hive-down, the old owner itself obtains shares of the new owner (Teichmann, 1993).

[29] The English translation of the BGB is, again, made available by the Federal Office of Justice and can be downloaded under `https://www.gesetze-im-internet.de/englisch_bgb/` (November 25, 2021).

[30] More detailed information on the construction of this variable are to be found Chapter 4.4.2.

not only have an effect in the first, but also in future periods.[31] All specifications include establishment fixed effects δ_i, which capture all time-constant firm characteristics and time fixed effects η_t, capturing year-specific shocks that affect all firms equally. $Controls_{it}$ is a vector of control variables comprising state (15 categories[32]) and industry dummies (213 categories[33]), which account for the possibility that establishments might change the state they are located or industry they are active in. It further includes five dummies for legal form, four dummies for the establishment's degree of independence, a dummy for the presence of a works council, and three dummies for the establishment's type of wage agreement. To make firms more comparable *ex ante*, i.e. before an restructuring measure is undertaken, and to check robustness, ϕ_{it-1} comprises varying sets of lagged controls, in particular four dummies for establishment size, three dummies for the annual result, and four dummies for the degree of competitive market pressure. The next section discusses the data used and provides key descriptive statistics.

4.4 Data

4.4.1 Data Sources

I employ the cross-sectional model of the linked employer-employee data (LIAB) made available by the Institute for Employment Research (IAB) of the German Federal Labor Office (BA).[34] This data links the establishment-level questionnaire data of the IAB Establishment Panel to administrative data on establishments from the Establishment History Panel (BHP) and employees working in these establishments from the Integrated Employment Biographies (IEB).

The IAB Establishment Panel is a large, representative survey conducted annually since 1993 and contains establishment-level questionnaire data on employment policies. Nowadays, in each wave approximately 16,000 establishments employing workers who are required to pay compulsory social security contributions are surveyed. Due to a high response rate coupled with scrutiny in the process of data monitoring and error correction, the data achieves a very high quality (Fischer et al., 2009). The data set should both make panel analysis possible and reflect dynamics of firm entries and exits, for which

[31]Note that the independent variable is reminiscent of a DiD setting. However, since a key assumption underlying DiD is exogenous treatment, while firms in my setup likely self-select into restructuring, I abstain from calling it a DiD.

[32]The Federal Republic of Germany actually has 16 states. The data, however, does not differentiate between the neighboring states Saarland and Rhineland Palatinate and pools them into one category.

[33]This number refers to the number of distinct industries in column (1) of Table 4.1. The overall sample contains 257 distinct industries.

[34]Unless other sources are specifically quoted, information in this chapter is derived from two main sources, which describe the data in detail: Information on the LIAB, particularly on the administrative data, comes from Schmidtlein, Seth and Umkehrer (2019). Information on the IAB Establishment Panel is taken from Fischer et al. (2008).

reason the bulk of surveyed establishments is re-surveyed in the following years but new establishments also enter the data. Thus, the sample contains four different subsamples: establishments from the prior wave that are re-surveyed, establishments that could not be reached in the last wave and are re-surveyed in the current wave, and new establishments that either did not have an establishment identifier in the prior year[35] or that are added to counteract panel mortality and to keep sample size constant. The sample of participating establishments is drawn from the universe of all German establishments with more than one typical worker. It is stratified by establishment size, industry, and state, wherefore large establishments, establishments from smaller states, and establishments active in Eastern Germany's manufacturing industry are oversampled. The questionnaire both contains questions that are asked every year and questions on special subjects that change every year. Due to the panel data methods used, the paper at hand is limited to the first category. The interviews are primarily conducted orally with the interviewer being present at the surveyed establishment, but due to funding limitations, in some waves and states, the surveys were also sent via mail and completed in a written form. The response rates of the different subsamples are rather heterogeneous and are highest for re-surveyed establishments. In 2017, the last wave employed in this study, the overall response rate was e.g. 59.9%, while the response rate among re-surveyed establishments (83%) was much higher than for establishments that could not be reached in the prior wave (28.8%) and establishments that enter the data for the first time (25.6%) (Bechmann et al., 2019).

The personal data on employees is from the Integrated Employment Biographies, which combine information from several data sources to reproduce employees' entire employment biographies. They contain information on the universe of workers registered as typically employed, as marginally employed, as unemployed, or as taking part in an unemployment measure within the observation period. As it is common in the literature, I focus my analysis on typically employed workers. The employee data are available in yearly slices and contain person identifiers and information on the employee and the job she is working in for all employees working in a given establishment on the 30th of June. These data can furthermore be combined with data from the Establishment History Panel, containing detailed data on establishments such as the establishment location on the county level, the establishment age, or the industry the establishment is active in on the five digit level.

The IAB Establishment Panel serves as a basis for the matched LIAB data. In a first step, all surveyed establishments in a given year are selected from the IAB Establishment Panel. In a second step, all employees working in these establishments on June 30th of the respective year for at least one day are selected from the IEB and the respective biographies are made available. Furthermore, administrative information from the Employment

[35] Note that such establishments are not necessarily new establishments that entered the market in the prior year but that there are several reasons why an establishment obtains a new establishment identifier.

History Panel is added. Hence, the LIAB contains all establishments that were surveyed between 1993 and 2017. In addition, it contains information on all employees working in these establishments on June 30th of the survey year. The next section describes the manipulations of the data set I conducted in detail and provides descriptives on the final sample and the key dependent and independent variables of interest.

4.4.2 Sample Definition and Descriptive Statistics

Before matching the different data sets, I conduct the following manipulations. I drop all years before 2009, because some important control variables are not available in prior years[36] and to keep the analysis free from influences of the 2008 financial crisis.[37] From the IAB Establishment Panel data, I drop all establishments active in agriculture and fishery, all public and non-profit organizations and all organizations with the government as the majority owner.

The administrative worker data is on the individual level and available in yearly slices. Since the focus is on typical employment, I drop all workers with other types of employment contracts. The data furthermore contains rather fine-grained information on the highest educational degree attained. I aggregate this information to obtain four different educational levels: without occupational education, with occupational education, college degree, and university degree. Furthermore, the employer is required to classify the occupation every worker works in according to the 5-digit German Classification of Occupations 2010 (KldB 2010). The KldB 2010 is constructed such that conversion to the international ISCO-08 standard is easy. The first four digits group jobs according to field-specific competencies that are necessary for a certain job. The fifth digit provides information on the task complexity of occupations and differentiates between helper, professional, complex specialist, and complex expert occupations. On its homepage,[38] the German employment agency provides a conversion key, which I use to convert the KldB 2010 to the internationally used ISCO-08 standard.[39] In line with this standard, I then group employees into ten groups: 1. Managers, 2. Professionals, 3. Technicians and associate professionals, 4. Clerical support workers, 5. Services and sales workers, 6. Skilled agricultural, forestry, and fishery workers, 7. Craft and related trades workers, 8. Plant and machine operators and assemblers, 9. Elementary occupations, and 10.

[36]Note that I lag some control variables in order to make establishments more comparable in the period before the restructuring measure was conducted. The data for these variables thus comprises observations from 2008.

[37]After a recession from the second quarter of 2008 to the first quarter of 2009, German GDP started growing again from the second quarter of 2009 on. See https://www.destatis.de/EN/Press/2021/05/PE21_244_81.html;jsessionid=C2088C26F08EB5C0F89DD0301B70014A.live711 (November 25, 2021).

[38]See https://statistik.arbeitsagentur.de/DE/Navigation/Grundlagen/Klassifikationen/Klassifikation-der-Berufe/KldB2010/Arbeitshilfen/Umsteigeschluessel/Umsteigeschluessel-Nav.html (November 25, 2021).

[39]For a thorough description of the ISCO-08 standard, see ILO (2012).

Armed forces occupations.[40] As the focus of this paper is on private, non-primary sector establishments, I drop groups 6 and 10 and all other workers whose job titles suggest them working in fishery or agriculture. I then analyze all other occupational groups as hierarchical layers. In supplementary analyses, I also analyze the fifth digit separately to obtain some additional insights on the way in which restructuring measures influence task complexity within establishments. Since there was a change in standards from the German Classification of Occupations 1988 (KldB 1988) to the much more detailed KldB2010, reports prior to the 30th of November 2011 were conducted according to the old standard and had to be re-classified ex post, potentially leading to some degree of measurement error (Schmidtlein et al., 2019). In the analysis of occupational layers and task complexity, I therefore always contrast results of the original specification with results of a specification using only observations from 2012 on to evaluate the role of measurement error. I then collapse the data on the establishment level and keep the overall sum of workers and workers in a certain occupational, educational (and also task complexity) group and the mean establishment wage.[41] Hereafter, I merge the yearly slices together, before merging the resulting panel data via establishment identifier and year to the IAB Establishment Panel and the BHP. I keep only successful matches.

These procedures result in an unbalanced panel comprising 93,878 observations that are non-missing with respect to the key independent variable of interest, corresponding to 27,120 single firms in the observation period 2009 to 2017. In my main specification, I trim by firm size and only keep establishments that report having more than 50 typical employees upon entering the data set, since the focus of this paper is more on larger firms and these are also more likely to conduct such restructuring measures. However, since the data shows that there is also a significant fraction of smaller firms that state they have restructured (the median firm size is at the time of restructuring is 54 employees), I also compare results to a specification in which I only exclude firms with less than 10 typical workers. In the least restrictive main fixed effects regression in column (1) of Table 4.1, the number of observations decreases to 19,271 observations corresponding to 4,059 single firms.[42]

[40] I follow Barbosa et al. (2021) in shortening the names of groups 3, 4, and 8 to "technicians", "clerks", and "machine operators" in my tables.

[41] Since this information is from the social security accounts, data on wages are only available up to the social security threshold and are thus censored (Bender, Haas and Klose, 2000; Schmidtlein et al., 2019). In case any impacts of restructuring on average wages were driven by the upper tail of the wage distribution, this would not be reflected in my estimations of average wages, leading to a certain degree of bias. I abstain from imputing the missing wage information, as suggested by Gartner (2005), as the wage analysis is not the main focus of the paper at hand. Furthermore, in unreported results, I also experimented with wages of different occupational, educational, and task complexity groups. As results were entirely insignificant, it seems like restructuring did not cause any large changes in the within-firm wage structure. I do, however, not discard the possibility that there might have been some unobserved changes at the upper end of the wage distribution.

[42] I use the specification in column (1) of Table 4.1 to calculate all descriptive statistics referring to the 2009-2017 sample. To calculate additional descriptive statistics for the 2012-2017 sample, I run an equivalent

Using this final data set, I construct my key variable of interest and the dependent variables I analyze. The key variable of interest measuring restructuring is based on an IAB Establishment Panel question surveyed every year since 1994 asking respondents whether parts of the establishment were closed down (*"geschlossen"*), relocated to different units within the company (*"ausgegliedert"*), or separated (*"ausgegründet"*), i.e. continued as an independent business.[43] As already stated above, there is evidence that pure shutdowns are different from other types of restructuring measures. Thus, I do not include the "closed down" alternative in the definition of my main variable of interest, and instead define it as taking the value 1 in the current and all future periods in case an establishment indicates that it either relocated parts of the establishment to other business units within the firm or separated them.[44] I do, however, define an alternate restructuring measure including the "closed down" alternative to see if results do in fact diverge. As outlined in Section 4.2, the "relocated" item thereby measures an establishment alteration, while the item "separated" measures a transfer of business, which might be combined with an establishment alteration.

Out of the 19,271 observations included in column (1) of Table 4.1, 1,993 indicate that they have conducted a restructuring measure in the current or a prior period, which corresponds to 10.34% of all observations in the sample and 509 single firms. On average, firms are observed 6 years in the data and firms conducting a restructuring measure drop out of the sample 2 years after restructuring. Due to the unbalanced nature of the data, firms can enter the data and directly report a restructuring measure. The fixed effects setup in this paper identifies the relationship between restructuring and within-firm changes in the employment structures via the within-variation, i.e. via firms that report a restructuring measure only after being present in the data for at least one period. The restructuring effect is hence identified via 260 switchers that switch from not having conducted a restructuring measure to having conducted one.

Table C.1 contains detailed information on sampled establishments.[45] As can be seen in the table, the relative majority of firms in the sample are limited (85.3%), independent (54%) and have a works council (67.3%) but no wage agreement (47.9%). In the prior year, firms were mostly medium-sized (66%), generated a positive annual result (79.8%) and

regression using this sample.

[43]In 2010, the questionnaire additionally differentiates between relocated or separated within Germany and abroad. Since this is the only time within the sample period that this information is available, while it is not clear for all other years, I add the domestic cases to the cases abroad to obtain a measure equivalent to the one available for the other years.

[44]Note that respondents can cross multiple items. In case firms simultaneously cross "closed down" and "relocated" or "separated", I define them as having relocated or separated, respectively. All observations of firms stating at any point in time that they closed down without simultaneously stating that they also relocated or separated are dropped from the sample in order to obtain a cleaner control group.

[45]These and the descriptive statistics in Table C.2 refer to the sample employed in column (1) of Table 4.1. Information on industries (241 categories in the final sample) and states (15 categories) is not included in this table for the sake of clarity.

experienced strong competitive pressure (52.9%). Table C.2 provides information on the analyzed dependent variables of interest on the employee level. As outlined above, the measurement of occupations and task complexity groups might be subject to a certain degree of measurement error, wherefore Panel B also provides these variables for the sample including only observations after 2012. The table shows that most workers have an occupational education (78.5%) and work with professional tasks (63% in Panel A and 58.8% in Panel B). Most workers furthermore work in lower to intermediate positions, while only a minority works in professional (8.22% in Panel A and 8.05% in Panel B) or management positions (3.5% in Panel A and 4.37% in Panel B). The average daily wage is 86.28 in 2009 inflation-adjusted euros. There are some differences between the two panels, suggesting a certain degree of measurement error in Panel A. For example, the share of management positions is 24.9% higher in Panel B than in Panel A. The respective results using the main sample with observations from 2009 to 2017 hence need to be interpreted with caution.

4.5 Results

This chapter presents empirical results. Section 4.5.1 discusses the effect of restructuring on overall employment, while Section 4.5.2 examines the way in which restructuring affects the within-firm distribution of jobs by focusing on education, occupation, and task complexity. Section 4.5.4 investigates whether and how wages change as a result of the restructuring measure.

4.5.1 Impact of Restructuring on Overall Employment

Table 4.1 shows the restructuring measures' aggregate employment effects. Column (1) includes only control variables, firm fixed effects and year fixed effects. In columns (2) to (4), I successively add lagged control variables to ensure robustness of results and to make establishments more comparable in the year before the restructuring measure was conducted. In column (2), I add lagged establishment size, in column (3), I additionally include a lagged self-evaluated performance indicator, and in column (4), I further add a lagged indicator evaluating the degree of competitive pressure the establishment is subject to.[46] Estimates show a robust negative effect of the restructuring measure on establishment-level employment. Especially between columns (2) and (4), the size of the coefficient barely changes, thus indicating that there is little omitted variable bias in column (2). The coefficient in column (4) implies that the effect of the restructuring measure on overall employment is substantial, as establishment size decreases by 30.2%, ceteris paribus. These results are robust to alternate clustering decisions. Panel A of

[46] Note that all further tables contain similar specifications.

Table 4.1: Effect of Restructuring on Employment

	(1)	(2)	(3)	(4)
Variables		Log(Employment)		
Restructuring	-0.351***	-0.266***	-0.263***	-0.264***
	(0.0529)	(0.0426)	(0.0432)	(0.0433)
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes
Observations	19,271	15,545	14,683	14,664

Note: This regression shows the impact of restructuring on the natural logarithm of employment per establishment. The data in this regression is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees in typical employment relationships, i.e. who are required to pay social security contributions. The dependent variable is the natural logarithm of the number of employees per establishment. Restructuring is a dummy variable taking the value 1 in the current and all later years if an establishment has relocated or separated parts of the establishment in the current or any prior year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.3 depicts estimations when standard errors are clustered on the sector level, while Panel A of Table C.4 shows results when standard errors are clustered on the sector-year level. In Table C.5, I am less restrictive and only trim the sample to firms with more than 10 typical employees at the time of entry. While statistical significance remains unchanged, the size of the coefficient in Panel A implies an even more substantial decrease in employment of 34.6%. In Table C.6, I include the "closed down" alternative in the definition of the independent variable of interest. As can be seen in Panel A of the table, inclusion of the "closed down" alternative decreases the size of the estimated coefficient, such that relocation and separation measures seem to decrease firm size more as compared to a close down measure. In particular, the estimate in column (4) implies that restructuring measures decrease total establishment level employment by 25%. While these results establish a statistically significant and economically substantial negative effect of restructuring on overall establishment-level employment, the next section looks at rearrangements in the labor structure following the restructuring measures.

4.5.2 Impact of Restructuring on Employee Shares

This chapter discusses my main results, in particular the ways in which restructuring measures change within-firm employment structures. In particular, I analyze if these measures affect every type of employee equally or whether they lead to changes in the educational and occupational structure of labor. In the latter case, this would be evidence that firms strategically use restructuring to change their labor structures or that the restructuring measures themselves make certain types of workers more important. Table 4.2 depicts main results for educational shares.

Table 4.2: Effect of Restructuring on Educational Shares

	(1)	(2)	(3)	(4)
Variables	Independent Variable: Restructuring			
University degree	0.0162***	0.0147**	0.0137**	0.0137**
	(0.00572)	(0.00641)	(0.00645)	(0.00647)
College degree	0.000920	-0.000549	0.000482	0.000499
	(0.00404)	(0.00411)	(0.00405)	(0.00406)
Occupational education	-0.0164**	-0.0142*	-0.0132*	-0.0133*
	(0.00736)	(0.00777)	(0.00786)	(0.00788)
W/o occupational Education	-0.000725	-0.0000102	-0.000975	-0.000959
	(0.00272)	(0.00314)	(0.00321)	(0.00322)
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes
Observations	19,257	15,531	14,669	14,650

Note: This regression shows the impact of restructuring on educational shares. The data in this regression is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees working in a typical employment relationship, i.e. who are required to pay social security contributions. The dependent variables are the shares of employees in a certain educational group. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were relocated or separated during the respective or any prior year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

As shown in the table, there seems to be a pronounced rearrangement towards the top of the educational pyramid. While coefficients for all other groups are negative or non-significant, the only group that gains in shares is employees holding a university degree. Again, coefficients are remarkably stable and barely change between columns (2) and (4), thus suggesting a rather low amount of omitted variable bias. In particular, the coefficient in column (4) implies that the analyzed restructuring measures increase the share of employees holding a university degree by 1.37 percentage points which, evaluated at the sample mean as depicted by Table C.2, translates into an economically significant increase of 11.9%. This expansion at the top of the educational distribution mainly happens at the expense of the most common educational group, namely workers with occupational education. The statistical and economic significance of this effect is, however, low. In particular, the coefficient in column (4) suggests that the restructuring measure decreases the share of workers with occupational education by 1.33 percentage points or 1.7%. Table C.7 show results when standard errors are clustered on the industry and industry-year level, respectively. Results are, again, robust.[47] Table C.8 shows results when being less restrictive and restricting the sample to firms with more than 10 workers at the time of entry. Results are robust, such that the only educational group that experiences a gain in shares are employees holding a university degree. Panel A of Table C.14 again compares these results to a specification including the "closed down" alternative in the definition of the independent variable of interest. This inclusion increases statistical power and thus statistical significance but does not greatly alter the size of effects.

Table 4.3 shows results when splitting workers by occupation. Panel A thereby depicts results of specifications equivalent to the ones in Table 4.2, i.e. that include all observations from 2009 on. As outlined above however, these specifications might be subject to a certain degree of measurement error. For this reason, Panel B depicts results of an equivalent specification comprising only observations from 2012 on. For simplicity and in line with Barbosa et al. (2021), the table just shows results for the first four layers and subsumes all lower layers into the group "other operational." The negative and highly significant coefficients of "other operational" in both panels suggest that the restructuring measure primarily hurts lower occupational layers in terms of employment shares. In particular, the coefficient in column (4) of Panel A implies a decrease in the share of these positions of 4 percentage points or 7.1%, while the coefficient in column (4) of Panel B implies a decrease of 3.5 percentage points or 6.1%.[48] Table C.10 in Appendix C splits this aggregated group into its components and shows that coefficients are consistently negative (albeit partly not statistically significant) for all subgroups. All higher occupational layers exhibit positive

[47]Note that, while clustering on the industry level reduces and clustering on the industry-year level increases statistical significance, the main result, namely that the relative prevalence of workers holding a university increases, remains constant.

[48]Note that in this and all further tables discussing occupational and task complexity layers, coefficients are evaluated at their associated means. Hence, coefficients in Panel A are evaluated at the means in Panel A of Table C.2, while coefficients in Panel B are evaluated at the respective means in Panel B of Table C.2.

Table 4.3: Effect of Restructuring on Occupational Shares

	(1)	(2)	(3)	(4)
Variables	Independent Variable: Restructuring			
Panel A: 2009–2017				
Managers	0.0183***	0.0153***	0.0156***	0.0156***
	(0.00531)	(0.00534)	(0.00543)	(0.00203)
Professionals	0.00276	0.00224	0.00128	0.00122
	(0.00700)	(0.00795)	(0.00746)	(0.00267)
Technicians	0.00789	0.0104	0.0133	0.0135***
	(0.00825)	(0.00947)	(0.00919)	(0.00356)
Clerks	0.00535	0.0101	0.0104	0.0101**
	(0.00854)	(0.00875)	(0.00897)	(0.00459)
Other Operational	-0.0343***	-0.0380***	-0.0406***	-0.0404***
	(0.0104)	(0.0105)	(0.0108)	(0.00508)
Observations	19,257	15,531	14,669	14,650
Panel B: 2012–2017				
Managers	0.0123**	0.0120**	0.0120**	0.0120***
	(0.00479)	(0.00533)	(0.00548)	(0.00241)
Professionals	0.00296	0.00419	0.00475	0.00457**
	(0.00446)	(0.00503)	(0.00497)	(0.00195)
Technicians	0.00485	0.00350	0.00296	0.00316
	(0.00449)	(0.00522)	(0.00539)	(0.00262)
Clerks	0.0140**	0.0149**	0.0159**	0.0154***
	(0.00586)	(0.00696)	(0.00722)	(0.00241)
Other Operational	-0.0341***	-0.0345***	-0.0356***	-0.0351***
	(0.0109)	(0.0122)	(0.0124)	(0.00402)
Observations	12,739	10,312	9,709	9,697
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size$_{t-1}$		yes	yes	yes
Performance$_{t-1}$			yes	yes
Competition$_{t-1}$				yes

Note: This regression shows the impact of restructuring on occupational shares. The data in this regression is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees working in a typical employment relationship, i.e. who are required to pay social security contributions. Panel A includes observations from 2009 on, while Panel B comprises observations after 2012. The dependent variables are the shares of employees in a certain occupational group. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were relocated or separated during the respective year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size$_{t-1}$ measures the establishment's lagged size (4 dummies), Performance$_{t-1}$ measures the annual result (3 dummies), and Competition$_{t-1}$ measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

(but largely non-significant) coefficients.[49] Interestingly, the intermediate layer "clerks" significantly increases in terms of shares in Panel B of Table 4.3, suggesting an increased importance of this higher occupational layer at the expense of all lower layers. Most strikingly however, the management layer exhibits a consistently positive and statistically significant coefficient in both panels. In particular, the coefficient in column (4) of Panel A implies an enormous, statistically and economically significant increase in the fraction of managers of 1.56 percentage points or, evaluated at the sample mean, 44.6%. In column (4) of Panel B, this effect decreases to 1.2 percentage points which, evaluated at its respective sample mean, translates into a smaller, albeit both still statistically and economically significant effect of 27.5%. This large difference in the size of coefficients again suggests a rather high degree of measurement error in the measurement of management positions before 2012.

I again conduct robustness checks to evaluate the validity of my findings and to provide additional insights. Tables C.11 and C.12 show results when clustering standard errors on the industry and industry-year level. Results remain robust to these alternate clustering decisions.[50] Table C.13 depicts a less restrictive specifications using a sample limited to firms entering the data with at least 10 typical employees. Results in Panel A remain robust, such that coefficients are consistently positive and statistically significant for managers, negative and significant for other operational positions, and positive but insignificant for all intermediate layers. In Panel B however, the effect size for managers decreases to .5 percentage points and the coefficients are not statistically significant in all columns anymore. Hence, the increase in the share of managers seems to occur predominantly in larger firms. Table C.14 again shows results when including the “closed down” alternative in the definition of the independent variable of interest. This inclusion does not alter statistical significance of results but changes the size of coefficients. In particular, the coefficient for management positions decreases to 11.2 percentage points in Panel A and 7.7 percentage points in Panel B, while the coefficient for other operational positions decreases to 2.87 percentage points in Panel A or 2.47 percentage points in Panel B. This suggests that the overall effect is driven by relocation and separation measures and that shut down measures do not alter the within-firm employment structure in an equally strong fashion.

To gain some additional exploratory insights on the changes in worker types, I finally split workers according to the complexity of the tasks they perform in their jobs, which,

[49]Note that, due to the low number of switchers, these non-significant results could partly be a result of low statistical power, such that only the most pronounced effects reach statistical significance. Despite of this potential problem, results nicely show a shift in the relative prevalence of lower to higher layers in the occupational hierarchy.

[50]Again, results are less significant when clustering on the industry level and more significant when clustering on the industry-year level. However, main results, i.e. the increase in the share of managers and decrease in the share of workers working in “other operational” positions, remain significant in all specifications.

as outlined above, corresponds to the 5th digit of the KldB 2010. Results are depicted by Table C.15 and again show that the occupational classification in Panel A is likely subject to a significant degree of measurement error. While the lowest task complexity group loses in terms of shares in both panels, results for higher task complexity groups change between the panels. In particular, while the second highest task complexity group, "complex specialist tasks", significantly gains in Panel A, this increase is not statistically significant anymore in Panel B. In contrast, the positive coefficient for the highest task complexity group, "complex expert tasks", is insignificant in Panel A, but strongly significant in Panel B. Despite the measurement error problems in Panel A, both panels again show a significant skill bias of the restructuring measure, such that jobs with a higher degree of task complexity appear to be retained more.

Summed up, results in this section suggest that the restructuring measure hurts employees with a lower level of education working with less complex tasks in lower occupations in the occupational hierarchy the most. Employees in higher layers of the occupational hierarchy, employees with high levels of education, and employees who perform more complex tasks seem to be hurt less by the restructuring measure.

There is thus a significant skill bias, which suggests that firms strategically use restructuring to lay off low-skilled types of workers and retain high-skilled types to a larger extent. This pattern is in line with skill-biased technological change, which argues that new technology leads to a higher relative demand for high-educated or skilled labor to the detriment of less educated or less skilled worker types (Katz and Autor, 1999). As outlined above, recent literature (Agrawal and Tambe, 2016; Antoni, Maug and Obernberger, 2019) has argued that PE buyouts might be a way to execute technological change and to overcome resistance to skill-biased technological change. My findings hence suggest that firms use restructuring measures in a similar fashion and adjust the skill level of their workforce to be able to cope with new technological developments. This better fit between workforce and technology together with the well-known face that more educated employees are also more productive (Fleisher et al., 2011; Kampelmann and Rycx, 2012; Benos and Karagiannis, 2016) might be channels via which restructuring firms realize performance gains. The large increase in the share of managers after the restructuring event is particularly striking. According to the theory on "management by exception" (e.g. Garicano, 2000), workers specialize in routine problems and ask managers whenever they face nonroutine problems. The more exceptions workers face, the higher the demand for managers, who specialize in solving nonroutine problems. In line with this theory, this suggests restructuring to lead to an increase in the number of nonroutine exceptions that need to be solved by managers.[51]

[51]Note that the average firm only remains in the data for two years after restructuring. Hence, it is possible that these problems largely occur in the short run after restructuring and that the number of nonroutine problems and thus the share of managers decreases again in the long run.

4.5.3 Impact of Restructuring on Size of Layers

While the prior section established that there are significant changes in the within-firm employment structures following restructuring measures, this section examines *how* the shares change by looking at the impact of restructuring on the absolute number of employees in occupational and educational groups. It thereby tries to answer the question if the observed changes in the within-firm employment structure occur because some groups lose more than others, or if there are also groups that benefit from the measure in terms of an increase in employment. Table 4.4 shows results for educational groups.

Table 4.4: Effect of Restructuring on Size of Educational Layers

Variables	(1)	(2)	(3)	(4)
	Independent Variable: Restructuring			
University Degree	-0.258***	-0.221***	-0.215***	-0.215***
	(0.0437)	(0.0437)	(0.0448)	(0.0448)
College Degree	-0.202***	-0.198***	-0.187***	-0.186***
	(0.0446)	(0.0495)	(0.0494)	(0.0494)
Occupational Education	-0.371***	-0.286***	-0.282***	-0.282***
	(0.0561)	(0.0450)	(0.0453)	(0.0454)
W/o Occupational Education	-0.199***	-0.151***	-0.159***	-0.159***
	(0.0432)	(0.0463)	(0.0468)	(0.0468)
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes
Observations	19,271	15,545	14,683	14,664

Note: This regression shows the impact of restructuring on the natural logarithm of the number of employees working in a certain educational group. The data in this regression is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees working in a typical employment relationship, i.e. who are required to pay social security contributions. The dependent variables are the log number of employees in a certain educational group. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were relocated or separated during the respective year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Since estimated coefficients are all negative and highly statistically significant, results show that none of the educational groups actually gains in numbers. The rearrangement in shares depicted in Table 4.2 thus occur because some layers seem to lose more than others. In particular, coefficients in column (4) of Table 4.4 suggest that while employment of the most common occupational group, employees with occupational education, decreases by 32.6%, employment of employees with university education decreases by only 24%.[52] As depicted by Panel B in Table C.3 and Table C.4, results again remain robust to clustering standard errors on the industry and industry-year level. Table C.5 shows results when only trimming the sample to firms with more than 10 employees at the time of entry. Results remain similar, such that all types of educational groups lose in terms of size and that rearrangement in shares are caused by the fact that the most common group, namely workers with occupational education, lose disproportionately more. Table C.6 shows results when including the "closed down" alternative in the definition of the restructuring variable. Results remain qualitatively similar.

Table 4.5 shows results for the number of workers in occupational layers. Interestingly, in Panel A, it seems like the number of managers does not decrease following the restructuring measure. However, Panel B reveals that this finding is likely due to measurement error, as the management layer significantly decreases in terms of size. It again shows that the rearrangements in the labor structure are caused by the lowest layer, "other operational", losing much more in terms of the number of employees as all other layers, thus explaining the consistently positive coefficients of all other layers in Table 4.3.[53] Coefficients in column (4) of Panel B of Table 4.5 in fact suggest that, while the number of managers (professionals) e.g. decreases by just 15.4% (13%), which translates in a loss of just 1.6 management positions (5.5 professional positions) in the average firm, the number of workers working in other operational positions declines by a staggering 41.6%, which is equivalent to more than 93 positions.[54] As depicted by Tables C.17 and C.18, results are again robust to alternate clustering decisions. Table C.19 depicts results of my less restrictive specification including firms with more than 10 employees upon entering the data set. Results are robust, such that all occupational layers decrease in size but the job loss of other operational positions is much higher as of all higher occupational layers. In Table C.20, I again include the "closed down" alternative in the definition of

[52]Note that via the same rationale, one would also expect an increase in the share of employees having a college degree and employees without occupational education. Both employee types exhibit insignificant coefficients in Table 4.2. The reason for this unexpected result might be that the overall share of these employees is relatively low in general (just 2.94% for employees with college degree and 7% for employees without occupational education), such that the differences in the percentage decreases depicted here are not sufficient to significantly alter the overall shares of these employees. For the average firm, the coefficient would translate in decrease of employment of workers with college degree (without occupational education) of only 2.3 (3.7) workers.

[53]Not all coefficients of these higher layers are, however, statistically significant. As outlined above, this might be due to the fact that there are only 260 switchers in the data such that this might also be a statistical power problem.

[54]Table C.16 again depicts results for the lower layers comprised in the "other operational" category and confirms the result that losses are much higher for these layers, especially for lower subgroups.

Table 4.5: Effect of Restructuring Size of Occupational Layers

Variables	(1)	(2)	(3)	(4)
	Independent Variable: Restructuring			
Panel A: 2009–2017				
Managers	-0.0447	-0.0110	-0.0150	-0.0133
	(0.0482)	(0.0547)	(0.0555)	(0.0307)
Professionals	-0.234***	-0.227***	-0.226***	-0.227***
	(0.0550)	(0.0600)	(0.0599)	(0.0269)
Technicians	-0.274***	-0.230***	-0.216***	-0.215***
	(0.0590)	(0.0599)	(0.0601)	(0.0283)
Clerks	-0.280***	-0.220***	-0.217***	-0.218***
	(0.0510)	(0.0501)	(0.0506)	(0.0255)
Other Operational	-0.449***	-0.383***	-0.386***	-0.385***
	(0.0694)	(0.0625)	(0.0638)	(0.0278)
Observations	19,271	15,545	14,683	14,664
Panel B: 2012–2017				
Managers	-0.156***	-0.152***	-0.144***	-0.143***
	(0.0440)	(0.0462)	(0.0460)	(0.0209)
Professionals	-0.157***	-0.133**	-0.121**	-0.122***
	(0.0507)	(0.0522)	(0.0535)	(0.0240)
Technicians	-0.234***	-0.197***	-0.199***	-0.198***
	(0.0652)	(0.0661)	(0.0683)	(0.0237)
Clerks	-0.228***	-0.184***	-0.178***	-0.179***
	(0.0567)	(0.0536)	(0.0549)	(0.0231)
Other Operational	-0.410***	-0.348***	-0.351***	-0.348***
	(0.0859)	(0.0820)	(0.0837)	(0.0270)
Observations	12,751	10,324	9,721	9,709
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes

Note: This regression shows the impact of restructuring on the natural logarithm of the number of employees working in a certain occupational group. The data in this regression is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees working in a typical employment relationship, i.e. who are required to pay social security contributions. Panel A includes observations from 2009 on, while Panel B comprises observations after 2012. The dependent variables are the shares of employees in a certain occupational group. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were relocated or separated during the respective year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

the independent variable of interest. Results show negative and statistically significant coefficients for all occupational layers but effect sizes are again lower for all types of employees, confirming the observation above that the "closed down" measure seems to have a weaker impact on within-firm employment structures, as compared to relocation or separation measures. Finally, in Table C.21, I also provide suggestive evidence on the way in which restructuring influences the number of workers within firms working with different levels of task complexity. It is again striking how different results in the two panels are and how strong the measurement error in Panel A hence is. While Panel A e.g. reports the strongest declines for helper tasks, Panel B reports the largest negative coefficients for professional tasks. Results in Panel B again confirm the found skill bias and explains results in Table C.15: while all task complexity groups decline in terms of the number of workers, the scope of this decline is different for different worker groups. In particular, column (4) suggests that while employment of workers conducting professional tasks declines by 32%, employment of workers conducting complex expert tasks decreases by just 16.4%.

Summed up, results of this chapter suggest that restructuring measures hurt all types of workers but effect sizes differ between worker groups. In particular, there seems to be a significant skill bias, such that worker groups with lower education, at lower levels of the occupational hierarchy, and workers performing tasks with a lower level of complexity seem to be hurt more by restructuring measures. These differences in the size of effects explain the rearrangements in the labor structures established in Section 4.5.2 and show that firms use restructuring measures strategically to adjust their labor forces and to increase their average labor quality. On a methodological level, this chapter also provides an illustrative example for the importance of taking into account the change in the occupational classification from KldB 1988 to KldB 2010 when working with the LIAB data, as mentioned by Schmidtlein et al. (2019), since using data on occupations before 2012 would lead to wrong conclusions.

4.5.4 Impact of Restructuring on Wages

Since according to prior literature, firms restructure to obtain more focus on core competencies and get rid of non-core areas, it is possible that the remaining (core) employees could extract higher rents from the company. In order to check this possibility, I also run regressions using average inflation-adjusted wages in 2009 euros as the dependent variable.[55]

Table 4.6 shows empirical results. As can be seen, coefficients are positive but close to zero and not significant in any estimation. Thus, it seems like restructuring does not

[55]Wages are manually inflation deflated using the Harmonised Indices of Consumer Prices (HICPs) available on the Eurostat homepage under https://ec.europa.eu/eurostat/databrowser/view/tec00118/default/table?lang=en (November 25, 2021).

Table 4.6: Effect of Restructuring on Average Wages

	(1)	(2)	(3)	(4)
Variables	Average Inflation-Adjusted Wages			
Restructuring	0.00568	0.00328	0.00443	0.00445
	(0.00638)	(0.00703)	(0.00723)	(0.00725)
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes
Observations	19,257	15,531	14,669	14,650

Note: This regression shows the impact of restructuring on the natural logarithm of inflation-adjusted wages. The data in this regression is trimmed, such that only establishment with more than 50 employees when entering the data set are included. Regressions include all employees working in a typical employment relationship, i.e. who are required to pay social security contributions. The dependent variable is the natural logarithm of the average inflation-adjusted wages, deflated to the base year 2009. The independent variable Restructuring is a dummy variable taking the value 1 if parts of the establishment were relocated or separated during the respective year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

have any impacts on average wages paid in companies. This is rather surprising, given that average labor quality increases and one would thus expect an increase in wages. In unreported results, I thus dig deeper and also run regressions for average wages of the different occupational, educational, and task complexity groups. Coefficients are almost exclusively insignificant. Hence, it seems like restructuring does not influence the within-firm wage distribution. The positive, albeit statistically insignificant, coefficients can be explained by the increase in average labor quality. One reason for this non-significance could be that unions in Germany are rather strong and often bargain over wages collectively, such that wages are rather rigid in comparison to other countries. Furthermore, even though I try to control for any pre-restructuring differences using indicators of lagged size, competitive pressure, and performance, I do not observe the reasons for which firms conduct these restructuring measures. If it is out of necessity due to economic problems in the pre-restructuring period, unions, and also individuals, might be cautious in the wage bargaining process to not endanger further jobs. As outlined in Section 4.4, establishments remain in the sample for just two years following the restructuring measure. Hence, any impacts on wages might not have materialized yet during the period of observation.

4.6 Conclusion

To maintain an edge over their competitors, firms frequently need to reinvent themselves. These adjustments often involve restructuring measures such as internal reorganization (e.g. Brickley and Van Drunen, 1990; Aksin and Masini, 2008), moving units outside the boundaries of the firm via divestures, spinoffs, or carveouts (e.g. Eckbo and Thorburn, 2013), or combinations of these measures. While prior literature has largely found positive effects of restructuring on firm value and performance (e.g. Chemmanur and Yan, 2004; Jain, Kini and Shenoy, 2011), there is, according to my best knowledge, no literature on the ways in which they affect firm-level employment structures. I address this gap and investigate how two particular restructuring measures, a relocation measure that moves parts of the establishment to different locations within the same firm and a separation measure that takes parts of the establishment outside the boundaries of the firm, affect within-firm employment structures. I thereby focus on both workers' education levels and the occupations they work in. I make use of the 2009-2017 waves of the LIAB data, which is a large, representative linked-employer-employee data set from Germany and take advantage of its longitudal structure. In particular, I employ firm fixed effects regressions in combination with an array of contemporary and lagged time-varying characteristics to make firms more comparable in the period before the restructuring measure was conducted, to limit the prevalence of omitted variable bias, and to approach causality.

Findings show that restructuring measures are accompanied by significant readjustments in the labor structure. Results show an overall decrease in establishment-level employment of more than 30%. All worker types lose in terms of employment but there is a significant skill bias regarding the size of effects. In particular, the most common German educational worker type, workers with occupational education, and workers working in lower positions in the occupational hierarchy and with helper tasks, seem to suffer the most in terms of employment. This results in a decrease of their shares, wherefore the relative importance of these worker groups within the firm declines. Conversely, the number of workers with university degree, in higher occupational layers, and workers working with more complex tasks decreases less, wherefore the shares of these worker types and hence their relative importance within the firm increases. Most notably, managers at the peak of the occupational hierarchy experience the most pronounced positive effect in terms of shares, which increases by more than 27%, hence suggesting that the restructuring measures I analyze make management tasks more important. In contrast to prior literature that documents negative wage effects for workers leaving the firm, I do not find any wage effects for remaining workers. Overall, findings are broadly consistent with the notion that, in line with evidence that exists for PE buyouts (Agrawal and Tambe, 2016), firms use restructuring as a means to strategically adjust their labor force and to increase the quality of their labor force, arguably to implement skill-biased technological change. In line with the literature on "management by exception" (Garicano, 2000), these measures seem create a large amount of exceptions in the post restructuring phase that need to be solved by managers. Ultimately, these changes in the labor structure might a channel that helps firms realize performance gains. For policy-makers and, in particular, employment agencies and unions, these results might be important to better understand which types of jobs are endangered the most when restructuring measures are conducted and how organizations change as a result.

Of course, this analysis is not without weaknesses. As restructuring is not randomly assigned but firms endogenously choose to self-select into conducting such a measure, the most important one is selection into treatment. Indeed, Çolak and Whited (2007) find that firms that spinoff or divest are different from firms that do not. Even though I control for as many differences as possible by using a large set of controls, both in the current and the previous period, and firm fixed effects, this analysis is likely subject to a certain amount of selection bias. In case a valid instrument for restructuring can be found, future research could use an instrumental variable approach, thereby alleviating any remaining concerns regarding self-selection.

Appendix C

C.1 Descriptive Statistics

Table C.1: Descriptive Statistics - Control Variables

Variables	N (1)	mean (2)	s.d. (3)	min (4)	max (5)
Contemporary Controls:					
Legal Form - Individual (0/1)	19,271	0.00944	0.0967	0	1
Legal Form - Partnership (0/1)	19,271	0.0255	0.158	0	1
Legal Form - Limited (0/1)	19,271	0.853	0.354	0	1
Legal Form - Corporation (0/1)	19,271	0.0800	0.271	0	1
Legal Form - Other (0/1)	19,271	0.0318	0.175	0	1
Establishment - Independent (0/1)	19,271	0.540	0.498	0	1
Establishment - Headquarter (0/1)	19,271	0.165	0.372	0	1
Establishment - Subsidiary (0/1)	19,271	0.285	0.451	0	1
Establishment - Intermediate Entity (0/1)	19,271	0.00965	0.0978	0	1
Works Council (0/1)	19,271	0.673	0.469	0	1
Wage Agreement - None (0/1)	19,271	0.479	0.500	0	1
Wage Agreement - Industry (0/1)	19,271	0.134	0.340	0	1
Wage Agreement - Company (0/1)	19,271	0.387	0.487	0	1

(continuing)

Variables	N (1)	mean (2)	s.d. (3)	min (4)	max (5)
Lagged Controls:					
Micro Enterprise (0/1)	16,314	0.00374	0.0610	0	1
Small Enterprise (0/1)	16,314	0.0164	0.127	0	1
Medium-Sized Enterprise (0/1)	16,314	0.660	0.474	0	1
Large Enterprise (0/1)	16,314	0.320	0.466	0	1
Annual Result - Negative (0/1)	15,467	0.102	0.303	0	1
Annual Result - Even (0/1)	15,467	0.0994	0.299	0	1
Annual Result - Positive (0/1)	15,467	0.798	0.401	0	1
Competitive Pressure - None (0/1)	16,268	0.0291	0.168	0	1
Competitive Pressure - Weak (0/1)	16,268	0.0749	0.263	0	1
Competitive Pressure - Medium (0/1)	16,268	0.367	0.482	0	1
Competitive Pressure - Strong (0/1)	16,268	0.529	0.499	0	1

Table C.2: Descriptive Statistics - Key Independent and Dependent Variables

Variables	(1) Mean	(2) Std. Dev.	(3) Min	(4) Max
Panel A: 2009–2017				
Employment - Total	348.6	1,650	0	58,069
Inflation-Adjusted Daily Wage	86.28	30.28	1.656	190.8
Education				
Share University Degree	0.115	0.143	0	1
Share College Degree	0.0294	0.0380	0	1
Share Occupational Education	0.785	0.157	0	1
Share W/O Occupational Education	0.0708	0.0963	0	1
N. University Degree	52.82	422.3	0	18,187
N. College Degree	10.57	66.55	0	3,493
N. Occupational Education	258.1	1,152	0	37,606
N. W/O Occupational Education	23.26	84.50	0	4,149
Occupations				
Share Managers	0.0350	0.0533	0	1
Share Professionals	0.0822	0.137	0	1
Share Technicians	0.154	0.150	0	1
Share Clerks	0.163	0.194	0	1
Share Other Operational	0.5741	0.2805	0	1
Share Sellers & Service Wkrs.	0.0780	0.185	0	1
Share Craft and Rel. Trades Wkrs.	0.215	0.237	0	1
Share Machine Operators	0.194	0.250	0	1
Share Elementary Wkrs.	0.0789	0.161	0	1
N. Managers	10.55	75.96	0	3,593
N. Professionals	42.58	393.7	0	16,922
N. Technicians	60.50	347.7	0	15,270
N. Clerks	49.81	227.6	0	15,016
N. Other Operational	174.48	749.38	0	26689
N. Sellers & Service Wkrs.	16.84	101.5	0	11,069
N. Craft and Rel. Trades Wkrs.	73.52	486.0	0	19,455
N. Machine Operators	59.97	245.1	0	8,279
N. Elementary Wkrs.	24.15	154.5	0	6,523
Task Complexity				
Share Complex Expert Tasks	0.0971	0.126	0	1
Share Complex Specialist Tasks	0.133	0.128	0	1

(continuing)

Variables	(1) Mean	(2) Std. Dev.	(3) Min	(4) Max
Share Professional Tasks	0.630	0.250	0	1
Share Helper Tasks	0.140	0.216	0	1
N. Complex Expert Tasks	45.51	417.8	0	18,008
N. Complex Specialist Tasks	50.38	273.8	0	10,511
N. Professional Tasks	201.9	899.6	0	28,799
N. Helper Tasks	17.62	123.1	0	6,523
N. of observations		19,258 – 19,271		
Panel B: 2012–2017				
Occupations				
Share Managers	0.0437	0.0580	0	1
Share Professionals	0.0805	0.137	0	1
Share Technicians	0.158	0.151	0	1
Share Clerks	0.141	0.172	0	1
Share Other Operational	0.585	0.279	0	1
Share Sellers & Service Wkrs.	0.0783	0.187	0	1
Share Craft and Rel. Trades Wkrs.	0.199	0.224	0	1
Share Machine Operators	0.202	0.253	0	1
Share Elementary Wkrs.	0.0969	0.175	0	1
N. Managers	13.40	87.53	0	3,593
N. Professionals	43.45	430.5	0	16,922
N. Technicians	63.80	382.3	0	15,270
N. Clerks	44.41	202.5	0	5,976
N. Other Operational	179.97	779.66	0	26689
N. Sellers & Service Wkrs.	16.69	66.41	0	1,607
N. Craft and Rel. Trades Wkrs.	69.58	492.9	0	19,455
N. Machine Operators	63.34	248.9	0	6,023
N. Elementary Wkrs.	30.36	179.8	0	6,523
Task Complexity				
Share Complex Expert Tasks	0.0992	0.124	0	1
Share Complex Specialist Tasks	0.142	0.133	0	1
Share Professional Tasks	0.588	0.246	0	1
Share Helper Tasks	0.171	0.231	0	1
N. Complex Expert Tasks	47.19	460.4	0	18,008
N. Complex Specialist Tasks	54.38	292.7	0	10,511
N. Professional Tasks	192.9	916.4	0	28,799
N. Helper Tasks	50.51	226.8	0	6,523
N. of observations		12,741–12,751		

C.2 Additional Analyses

Table C.3: Effects of Restructuring on Employment and Size of Educational Groups – Industry Clusters

	(1)	(2)	(3)	(4)
Variables	Indep. Variable: Restructuring			
Panel A: Total Employment				
Employment	-0.351***	-0.266***	-0.263***	-0.264***
	(0.0670)	(0.0521)	(0.0527)	(0.0528)
Observations	19,271	15,545	14,683	14,664
Panel B: Education				
University Degree	-0.258***	-0.221***	-0.215***	-0.215***
	(0.0547)	(0.0509)	(0.0514)	(0.0515)
College Degree	-0.202***	-0.198***	-0.187***	-0.186***
	(0.0638)	(0.0656)	(0.0635)	(0.0636)
Occupational Education	-0.371***	-0.286***	-0.282***	-0.282***
	(0.0698)	(0.0536)	(0.0538)	(0.0540)
W/o Occupational Education	-0.199***	-0.151***	-0.159***	-0.159***
	(0.0458)	(0.0487)	(0.0489)	(0.0487)
Observations	19,271	15,545	14,683	14,664
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
$Size_{t-1}$		yes	yes	yes
$Performance_{t-1}$			yes	yes
$Competition_{t-1}$				yes

Note: This regression shows the impact of restructuring on employment and employment of educational groups. The data is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees in typical employment relationships, i.e. who are required to pay social security contributions. The dependent variables are the natural logarithms of the total number of employees and employees in a certain educational group per establishment. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were relocated or separated during the respective or any prior year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). $Size_{t-1}$ measures the establishment's lagged size (4 dummies), $Performance_{t-1}$ measures the annual result (3 dummies), and $Competition_{t-1}$ measures the degree of competitive pressure (4 dummies). Standard errors clustered on the industry level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.4: Effects of Restructuring on Employment and Size of Educational Groups – Industry-Year Clusters

	(1)	(2)	(3)	(4)
Variables	Indep. Variable: Restructuring			
Panel A: Total Employment				
Employment	-0.351***	-0.266***	-0.263***	-0.264***
	(0.0347)	(0.0331)	(0.0345)	(0.0346)
Observations	19,271	15,545	14,683	14,664
Panel B: Education				
University Degree	-0.258***	-0.221***	-0.215***	-0.215***
	(0.0547)	(0.0509)	(0.0514)	(0.0515)
College Degree	-0.202***	-0.198***	-0.187***	-0.186***
	(0.0638)	(0.0656)	(0.0635)	(0.0636)
Occupational Education	-0.371***	-0.286***	-0.282***	-0.282***
	(0.0698)	(0.0536)	(0.0538)	(0.0540)
W/o Occupational Education	-0.199***	-0.151***	-0.159***	-0.159***
	(0.0458)	(0.0487)	(0.0489)	(0.0487)
Observations	19,271	15,545	14,683	14,664
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
$Size_{t-1}$		yes	yes	yes
$Performance_{t-1}$			yes	yes
$Competition_{t-1}$				yes

Note: This regression shows the impact of restructuring on employment and employment of educational groups. The data is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees in typical employment relationships, i.e. who are required to pay social security contributions. The dependent variables are the natural logarithms of the total number of employees and employees in a certain educational group per establishment. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were relocated or separated during the respective or any prior year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). $Size_{t-1}$ measures the establishment's lagged size (4 dummies), $Performance_{t-1}$ measures the annual result (3 dummies), and $Competition_{t-1}$ measures the degree of competitive pressure (4 dummies). Standard errors clustered on the industry-year level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.5: Effects of Restructuring on Employment and Size of Educational Groups Including Small Firms

	(1)	(2)	(3)	(4)
Variables	Indep. Variable: Restructuring			
Panel A: Total Employment				
Employment	-0.343***	-0.298***	-0.298***	-0.297***
	(0.0406)	(0.0383)	(0.0392)	(0.0393)
Panel B: Education				
University Degree	-0.158***	-0.148***	-0.143***	-0.141***
	(0.0329)	(0.0350)	(0.0361)	(0.0360)
College Degree	-0.214***	-0.219***	-0.212***	-0.211***
	(0.0336)	(0.0383)	(0.0387)	(0.0387)
Occupational Education	-0.350***	-0.303***	-0.303***	-0.302***
	(0.0418)	(0.0393)	(0.0402)	(0.0403)
W/o Occupational Education	-0.200***	-0.160***	-0.162***	-0.161***
	(0.0341)	(0.0366)	(0.0369)	(0.0370)
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes
N	42,982	34,950	33,201	33,166

Note: This regression shows the impact of restructuring on employment and employment of educational groups. The data is trimmed, such that only establishment with more than 10 typical employees when entering the data set are included. Regressions include all employees in typical employment relationships, i.e. who are required to pay social security contributions. The dependent variables are the natural logarithms of the total number of employees and employees in a certain educational group per establishment. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were relocated or separated during the respective or any prior year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.6: Effects of Alternate Restructuring on Employment and Size of Educational Groups

Variables	(1)	(2)	(3)	(4)
	Indep. Variable: Restructuring (Alternate)			
Panel A: Total Employment				
Employment	-0.296***	-0.225***	-0.223***	-0.223***
	(0.0375)	(0.0307)	(0.0313)	(0.0314)
Observations	20,697	16,801	15,876	15,856
Panel B: Education				
University Degree	-0.201***	-0.165***	-0.164***	-0.163***
	(0.0321)	(0.0323)	(0.0329)	(0.0329)
College Degree	-0.131***	-0.122***	-0.111***	-0.110***
	(0.0329)	(0.0358)	(0.0360)	(0.0360)
Occupational Education	-0.315***	-0.246***	-0.241***	-0.241***
	(0.0395)	(0.0320)	(0.0324)	(0.0325)
W/o Occupational Education	-0.209***	-0.169***	-0.178***	-0.178***
	(0.0341)	(0.0371)	(0.0380)	(0.0380)
Observations	20,697	16,801	15,876	15,856
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes

Note: This regression shows the impact of restructuring on employment and employment of educational groups. The data is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees in typical employment relationships, i.e. who are required to pay social security contributions. The dependent variables are the natural logarithms of the total number of employees and employees in a certain educational group per establishment. Each coefficient stems from a single regression of these dependent variables on Restructuring (Alternate), which is a dummy variable taking the value 1 if parts of the establishment were closed down, relocated or separated during the respective year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.7: Effect of Restructuring on Educational Shares - Alternate Clustering Decisions

	(1)	(2)	(3)	(4)
Variables	Indep. Variable: Restructuring			
Panel A: Industry Clusters				
University Degree	0.0162**	0.0147**	0.0137*	0.0137*
	(0.00670)	(0.00736)	(0.00736)	(0.00737)
College Degree	0.000920	-0.000549	0.000482	0.000499
	(0.00467)	(0.00472)	(0.00459)	(0.00461)
Occupational Education	-0.0164**	-0.0142*	-0.0132	-0.0133
	(0.00816)	(0.00829)	(0.00832)	(0.00834)
W/o Occupational Education	-0.000725	-0.0000102	-0.000975	-0.000959
	(0.00321)	(0.00354)	(0.00356)	(0.00356)
Panel B: Industry-Year Clusters				
University Degree	0.0162***	0.0147***	0.0137***	0.0137***
	(0.00385)	(0.00442)	(0.00440)	(0.00441)
College Degree	0.000920	-0.000549	0.000482	0.000499
	(0.00225)	(0.00259)	(0.00257)	(0.00258)
Occupational Education	-0.0164***	-0.0142***	-0.0132***	-0.0133***
	(0.00414)	(0.00441)	(0.00452)	(0.00453)
W/o Occupational Education	-0.000725	-0.0000102	-0.000975	-0.000959
	(0.00189)	(0.00211)	(0.00214)	(0.00215)
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size$_{t-1}$		yes	yes	yes
Performance$_{t-1}$			yes	yes
Competition$_{t-1}$				yes
Observations	19,257	15,531	14,669	14,650

Note: This regression shows the impact of restructuring on educational shares. The data is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees working in a typical employment relationship, i.e. who are required to pay social security contributions. The dependent variables are the shares of employees in a certain educational group. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were relocated or separated during the respective or any prior year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size$_{t-1}$ measures the establishment's lagged size (4 dummies), Performance$_{t-1}$ measures the annual result (3 dummies), and Competition$_{t-1}$ measures the degree of competitive pressure (4 dummies). Standard errors clustered on the industry (Panel A) and industry-year level (Panel B) in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.8: Effect of Restructuring on Educational Shares Including Small Firms

	(1)	(2)	(3)	(4)
Variables	Indep. Variable: Restructuring			
University Degree	0.0191***	0.0165***	0.0161***	0.0164***
	(0.00509)	(0.00565)	(0.00580)	(0.00580)
College Degree	-0.00151	-0.00211	-0.00132	-0.00132
	(0.00348)	(0.00370)	(0.00377)	(0.00377)
Occupational Education	-0.0128*	-0.0114	-0.0114	-0.0117
	(0.00667)	(0.00745)	(0.00768)	(0.00769)
W/o Occupational Education	-0.00472	-0.00303	-0.00341	-0.00340
	(0.00301)	(0.00330)	(0.00338)	(0.00339)
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size$_{t-1}$		yes	yes	yes
Performance$_{t-1}$			yes	yes
Competition$_{t-1}$				yes
Observations	42,941	34,911	33,169	33,134

Note: This regression shows the impact of restructuring on educational shares. The data is trimmed, such that only establishment with more than 10 typical employees when entering the data set are included. Regressions include all employees working in a typical employment relationship, i.e. who are required to pay social security contributions. The dependent variables are the shares of employees in a certain educational group. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were relocated or separated during the respective or any prior year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size$_{t-1}$ measures the establishment's lagged size (4 dummies), Performance$_{t-1}$ measures the annual result (3 dummies), and Competition$_{t-1}$ measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.9: Effects of Alternate Restructuring on Educational Shares

	(1)	(2)	(3)	(4)
Variables	Indep. Variable: Restructuring (Alternate)			
University Degree	0.0144***	0.0139***	0.0127***	0.0127***
	(0.00418)	(0.00465)	(0.00463)	(0.00464)
College Degree	0.00197	0.000857	0.00206	0.00209
	(0.00276)	(0.00278)	(0.00275)	(0.00275)
Occupational Education	-0.0142***	-0.0130**	-0.0121**	-0.0121**
	(0.00512)	(0.00544)	(0.00552)	(0.00553)
W/o Occupational Education	-0.00214	-0.00179	-0.00261	-0.00262
	(0.00206)	(0.00245)	(0.00253)	(0.00254)
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes
Observations	20,683	16,787	15,862	15,842

Note: This regression shows the impact of restructuring on educational shares. The data is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees in typical employment relationships, i.e. who are required to pay social security contributions. The dependent variables are the shares of employees in a certain educational group. Each coefficient stems from a single regression of these dependent variables on Restructuring (Alternate), which is a dummy variable taking the value 1 if parts of the establishment were closed down, relocated or separated during the respective year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.10: Effect of Restructuring on Occupational Shares – Lower Layers

	(1)	(2)	(3)	(4)
Variables	Indep. Variable: Restructuring			
Panel A: 2009–2017				
Sellers and service wkrs.	-0.00464	-0.00797*	-0.00763*	-0.00769***
	(0.00417)	(0.00412)	(0.00421)	(0.00275)
Craft and rel. trades wkrs.	-0.000987	0.00136	-0.00127	-0.000988
	(0.00781)	(0.00818)	(0.00831)	(0.00492)
Machine operators	-0.00860	-0.00781	-0.00752	-0.00757
	(0.00724)	(0.00737)	(0.00756)	(0.00525)
Elementary occup.	-0.0201***	-0.0236***	-0.0242***	-0.0242***
	(0.00646)	(0.00615)	(0.00628)	(0.00477)
Observations	19,257	15,531	14,669	14,650
Panel B: 2012–2017				
Sellers and service wkrs.	-0.00452	-0.00221	-0.00211	-0.00233
	(0.00364)	(0.00288)	(0.00254)	(0.00193)
Craft and rel. trades wkrs.	-0.00347	-0.00412	-0.00446	-0.00372
	(0.00689)	(0.00709)	(0.00732)	(0.00327)
Machine operators	-0.00988	-0.0155**	-0.0165**	-0.0165***
	(0.00601)	(0.00750)	(0.00770)	(0.00304)
Elementary occup.	-0.0163**	-0.0127	-0.0125	-0.0125***
	(0.00796)	(0.00831)	(0.00845)	(0.00279)
Observations	12,739	10,312	9,709	9,697
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes

Note: This regression shows the impact of restructuring on occupational shares. The data is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees working in a typical employment relationship, i.e. who are required to pay social security contributions. Panel A includes observations from 2009 on, while Panel B comprises observations after 2012. The dependent variables are the shares of employees in a certain occupational group. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were relocated or separated during the respective year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.11: Effect of Restructuring on Occupational Shares – Industry Clusters

	(1)	(2)	(3)	(4)
Variables	Indep. Variable: Restructuring			
Panel A: 2009–2017				
Managers	0.0183***	0.0153***	0.0156***	0.0156***
	(0.00533)	(0.00530)	(0.00540)	(0.00540)
Professionals	0.00276	0.00224	0.00128	0.00122
	(0.00732)	(0.00847)	(0.00799)	(0.00802)
Technicians	0.00789	0.0104	0.0133	0.0135
	(0.0101)	(0.0118)	(0.0114)	(0.0114)
Clerks	0.00535	0.0101	0.0104	0.0101
	(0.00873)	(0.00921)	(0.00935)	(0.00937)
Other Operational	-0.0343***	-0.0380***	-0.0406***	-0.0404***
	(0.0117)	(0.0124)	(0.0126)	(0.0127)
Observations	19,257	15,531	14,669	14,650
Panel B: 2012–2017				
Managers	0.0123**	0.0120**	0.0120*	0.0120*
	(0.00530)	(0.00602)	(0.00620)	(0.00627)
Professionals	0.00296	0.00419	0.00475	0.00457
	(0.00495)	(0.00561)	(0.00556)	(0.00559)
Technicians	0.00485	0.00350	0.00296	0.00316
	(0.00494)	(0.00558)	(0.00578)	(0.00578)
Clerks	0.0140**	0.0149*	0.0159*	0.0154*
	(0.00674)	(0.00849)	(0.00865)	(0.00860)
Other Operational	-0.0341***	-0.0345***	-0.0356***	-0.0351***
	(0.0113)	(0.0130)	(0.0131)	(0.0130)
Observations	12,739	10,312	9,709	9,697
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes

Note: This regression shows the impact of restructuring on occupational shares. The data is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees working in a typical employment relationship, i.e. who are required to pay social security contributions. Panel A includes observations from 2009 on, while Panel B comprises observations after 2012. The dependent variables are the shares of employees in a certain occupational group. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were relocated or separated during the respective year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the industry level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.12: Effect of Restructuring on Occupational Shares – Industry-Year Clusters

	(1)	(2)	(3)	(4)
Variables	Indep. Variable: Restructuring			
Panel A: 2009–2017				
Managers	0.0183***	0.0153***	0.0156***	0.0156***
	(0.00359)	(0.00378)	(0.00392)	(0.00394)
Professionals	0.00276	0.00224	0.00128	0.00122
	(0.00407)	(0.00468)	(0.00463)	(0.00465)
Technicians	0.00789	0.0104	0.0133**	0.0135**
	(0.00562)	(0.00666)	(0.00666)	(0.00668)
Clerks	0.00535	0.0101	0.0104*	0.0101
	(0.00579)	(0.00625)	(0.00631)	(0.00633)
Other Operational	-0.0343***	-0.0380***	-0.0406***	-0.0404***
	(0.00691)	(0.00777)	(0.00797)	(0.00799)
Observations	19,257	15,531	14,669	14,650
Panel B: 2012–2017				
Managers	0.0123***	0.0120***	0.0120**	0.0120**
	(0.00397)	(0.00461)	(0.00478)	(0.00482)
Professionals	0.00296	0.00419	0.00475	0.00457
	(0.00316)	(0.00355)	(0.00355)	(0.00358)
Technicians	0.00485	0.00350	0.00296	0.00316
	(0.00348)	(0.00395)	(0.00407)	(0.00405)
Clerks	0.0140***	0.0149***	0.0159***	0.0154***
	(0.00471)	(0.00568)	(0.00586)	(0.00584)
Other Operational	-0.0341***	-0.0345***	-0.0356***	-0.0351***
	(0.00794)	(0.00914)	(0.00933)	(0.00932)
Observations	12,739	10,312	9,709	9,697
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes

Note: This regression shows the impact of restructuring on occupational shares. The data is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees working in a typical employment relationship, i.e. who are required to pay social security contributions. Panel A includes observations from 2009 on, while Panel B comprises observations after 2012. The dependent variables are the shares of employees in a certain occupational group. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were relocated or separated during the respective year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the industry-year level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.13: Effect of Restructuring on Occupational Shares Including Small Firms

	(1)	(2)	(3)	(4)
Variables	Indep. Variable: Restructuring			
Panel A: 2009–2017				
Managers	0.0145***	0.0139***	0.0143***	0.0143***
	(0.00460)	(0.00502)	(0.00518)	(0.00179)
Professionals	0.00527	0.00282	0.00254	0.00271
	(0.00530)	(0.00616)	(0.00595)	(0.00276)
Technicians	0.00564	0.00908	0.0124	0.0125***
	(0.00694)	(0.00806)	(0.00786)	(0.00362)
Clerks	0.000699	0.00496	0.00467	0.00441
	(0.00725)	(0.00783)	(0.00748)	(0.00413)
Other Operational	-0.0261***	-0.0307***	-0.0339***	-0.0340***
	(0.00840)	(0.00895)	(0.00913)	(0.00452)
Observations	42,939	34,908	33,167	33,132
Panel B: 2012–2017				
Managers	0.00600*	0.00531	0.00521	0.00524***
	(0.00346)	(0.00398)	(0.00410)	(0.00184)
Professionals	0.00407	0.00456	0.00512	0.00541***
	(0.00344)	(0.00407)	(0.00418)	(0.00188)
Technicians	0.00781	0.00879	0.00863	0.00880***
	(0.00537)	(0.00609)	(0.00630)	(0.00286)
Clerks	0.00852	0.00707	0.00744	0.00700***
	(0.00533)	(0.00615)	(0.00629)	(0.00265)
Other Operational	-0.0264***	-0.0257**	-0.0264**	-0.0265***
	(0.00862)	(0.0102)	(0.0105)	(0.00364)
Observations	28,913	23,719	22,483	22,459
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes

Note: This regression shows the impact of restructuring on occupational shares. The data is trimmed, such that only establishment with more than 10 typical employees when entering the data set are included. Regressions include all employees working in a typical employment relationship, i.e. who are required to pay social security contributions. Panel A includes observations from 2009 on, while Panel B comprises observations after 2012. The dependent variables are the shares of employees in a certain occupational group. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were relocated or separated during the respective year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.14: Effects of Alternate Restructuring on Occupational Shares

	(1)	(2)	(3)	(4)
Variables	Indep. Variable: Restructuring (Alternate)			
Panel A: 2009 – 2017				
Managers	0.0128***	0.0110***	0.0112***	0.0112***
	(0.00359)	(0.00363)	(0.00372)	(0.00164)
Professionals	0.00533	0.00599	0.00517	0.00512**
	(0.00481)	(0.00543)	(0.00518)	(0.00214)
Technicians	0.00245	0.00268	0.00428	0.00442
	(0.00568)	(0.00654)	(0.00646)	(0.00291)
Clerks	0.00346	0.00789	0.00818	0.00801**
	(0.00621)	(0.00643)	(0.00661)	(0.00384)
Other Operational	-0.0240***	-0.0276***	-0.0288***	-0.0287***
	(0.00754)	(0.00766)	(0.00788)	(0.00423)
Observations	20,683	16,787	15,862	15,842
Panel B: 2012 – 2017				
Managers	0.00801**	0.00788**	0.00785**	0.00767***
	(0.00318)	(0.00354)	(0.00367)	(0.00187)
Professionals	0.00481	0.00663*	0.00627*	0.00617***
	(0.00308)	(0.00351)	(0.00340)	(0.00152)
Technicians	0.00275	0.00148	0.00128	0.00140
	(0.00317)	(0.00370)	(0.00378)	(0.00218)
Clerks	0.00907**	0.00930**	0.00980**	0.00947***
	(0.00396)	(0.00466)	(0.00485)	(0.00209)
Other Operational	-0.0246***	-0.0253***	-0.0252***	-0.0247***
	(0.00727)	(0.00807)	(0.00823)	(0.00329)
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size$_{t-1}$		yes	yes	yes
Performance$_{t-1}$			yes	yes
Competition$_{t-1}$				yes
Observations	13,628	11,100	10,467	10,454

Note: This regression shows the impact of restructuring on occupational shares. The data is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees in typical employment relationships, i.e. who are required to pay social security contributions. The dependent variables are the shares of employees in a certain occupational group. Each coefficient stems from a single regression of these dependent variables on Restructuring (Alternate), which is a dummy variable taking the value 1 if parts of the establishment were closed down, relocated or separated during the respective year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size$_{t-1}$ measures the establishment's lagged size (4 dummies), Performance$_{t-1}$ measures the annual result (3 dummies), and Competition$_{t-1}$ measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.15: Effect of Restructuring on Task Complexity Shares

	(1)	(2)	(3)	(4)
Variables	Indep. Variable: Restructuring			
Panel A: 2009–2017				
Complex Expert Tasks	0.0134*	0.00816	0.00903	0.00899
	(0.00742)	(0.00800)	(0.00797)	(0.00799)
Complex Specialist Tasks	0.0159*	0.0168*	0.0182**	0.0185**
	(0.00815)	(0.00873)	(0.00896)	(0.00898)
Professional Tasks	-0.00195	0.00505	0.00252	0.00250
	(0.0106)	(0.0106)	(0.0109)	(0.0109)
Helper Tasks	-0.0274***	-0.0300***	-0.0298***	-0.0300***
	(0.00837)	(0.00767)	(0.00783)	(0.00786)
Observations	19,257	15,531	14,669	14,650
Panel B: 2012–2017				
Complex Expert Tasks	0.0118**	0.0111**	0.0113**	0.0111**
	(0.00493)	(0.00544)	(0.00545)	(0.00549)
Complex Specialist Tasks	0.00354	0.00238	0.00177	0.00201
	(0.00439)	(0.00472)	(0.00492)	(0.00494)
Professional Tasks	0.00512	0.00368	0.00391	0.00390
	(0.00810)	(0.00892)	(0.00916)	(0.00924)
Helper Tasks	-0.0205**	-0.0172**	-0.0170*	-0.0170*
	(0.00827)	(0.00867)	(0.00880)	(0.00886)
Observations	12,739	10,312	9,709	9,697
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes

Note: This regression shows the impact of restructuring on task complexity shares. The data is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees working in a typical employment relationship, i.e. who are required to pay social security contributions. The dependent variables are the shares of employees in a certain task complexity group. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were relocated or separated during the respective or any prior year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.16: Effect of Restructuring on Size of Occupational Layers – Lower Layers

	(1)	(2)	(3)	(4)
Variables	Indep. Variable: Restructuring			
Panel A: 2009–2017				
Sellers and service wkrs.	-0.178***	-0.178***	-0.154***	-0.154***
	(0.0496)	(0.0537)	(0.0534)	(0.0302)
Craft and rel. trades wkrs.	-0.352***	-0.294***	-0.310***	-0.308***
	(0.0632)	(0.0623)	(0.0636)	(0.0282)
Machine operators	-0.307***	-0.270***	-0.271***	-0.270***
	(0.0605)	(0.0654)	(0.0665)	(0.0393)
Elementary occup.	-0.319***	-0.324***	-0.334***	-0.332***
	(0.0750)	(0.0812)	(0.0830)	(0.0499)
Observations	19,271	15,545	14,683	14,664
Panel B: 2012–2017				
Sellers and service wkrs.	-0.192***	-0.165***	-0.153***	-0.153***
	(0.0462)	(0.0459)	(0.0441)	(0.0239)
Craft and rel. trades wkrs.	-0.302***	-0.278***	-0.278***	-0.273***
	(0.0756)	(0.0701)	(0.0720)	(0.0257)
Machine operators	-0.302***	-0.311***	-0.314***	-0.311***
	(0.0577)	(0.0665)	(0.0682)	(0.0294)
Elementary occup.	-0.295***	-0.263**	-0.261**	-0.259***
	(0.0883)	(0.104)	(0.107)	(0.0331)
Observations	12,751	10,324	9,721	9,709
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes

Note: This regression shows the impact of restructuring on the natural logarithm of the number of employees working in a certain occupational group. The data is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees working in a typical employment relationship, i.e. who are required to pay social security contributions. Panel A includes observations from 2009 on, while Panel B comprises observations after 2012. The dependent variables are the shares of employees in a certain occupational group. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were closed down, relocated, or separated during the respective year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.17: Effects of Restructuring on Size of Occupational Layers – Industry Clusters

Variables	(1)	(2)	(3)	(4)
	Indep. Variable: Restructuring			
Panel A: 2009 – 2017				
Managers	-0.0447	-0.0110	-0.0150	-0.0133
	(0.0550)	(0.0598)	(0.0594)	(0.0596)
Professionals	-0.234***	-0.227***	-0.226***	-0.227***
	(0.0639)	(0.0678)	(0.0672)	(0.0674)
Technicians	-0.274***	-0.230***	-0.216***	-0.215***
	(0.0776)	(0.0747)	(0.0749)	(0.0749)
Clerks	-0.280***	-0.220***	-0.217***	-0.218***
	(0.0540)	(0.0547)	(0.0545)	(0.0547)
Other Operational	-0.449***	-0.383***	-0.386***	-0.385***
	(0.0802)	(0.0716)	(0.0728)	(0.0728)
Observations	19,271	15,545	14,683	14,664
Panel B: 2012 – 2017				
Managers	-0.156***	-0.152***	-0.144***	-0.143***
	(0.0506)	(0.0498)	(0.0502)	(0.0506)
Professionals	-0.157**	-0.133**	-0.121*	-0.122*
	(0.0608)	(0.0606)	(0.0624)	(0.0630)
Technicians	-0.234***	-0.197**	-0.199**	-0.198**
	(0.0863)	(0.0831)	(0.0843)	(0.0849)
Clerks	-0.228***	-0.184***	-0.178***	-0.179***
	(0.0688)	(0.0655)	(0.0667)	(0.0674)
Other Operational	-0.410***	-0.348***	-0.351***	-0.348***
	(0.104)	(0.0977)	(0.0994)	(0.1000)
Observations	12,751	10,324	9,721	9,709
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
$Size_{t-1}$		yes	yes	yes
$Performance_{t-1}$			yes	yes
$Competition_{t-1}$				yes

Note: This regression shows the impact of restructuring on the natural logarithm of the number of employees working in a certain occupational group. The data is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees in typical employment relationships, i.e. who are required to pay social security contributions. The dependent variables are the natural logarithms of employees in a certain occupational group per establishment. Restructuring is a dummy variable taking the value 1 in the current and all later years if an establishment has relocated or separated parts of the establishment. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). $Size_{t-1}$ measures the establishment's lagged size (4 dummies), $Performance_{t-1}$ measures the annual result (3 dummies), and $Competition_{t-1}$ measures the degree of competitive pressure (4 dummies). Standard errors clustered on the industry level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.18: Effects of Restructuring on Size of Occupational Layers – Industry-Year Clusters

Variables	(1)	(2)	(3)	(4)
	Indep. Variable: Restructuring			
Panel A: 2009 – 2017				
Managers	-0.0447	-0.0110	-0.0150	-0.0133
	(0.0379)	(0.0416)	(0.0407)	(0.0408)
Professionals	-0.234***	-0.227***	-0.226***	-0.227***
	(0.0382)	(0.0410)	(0.0412)	(0.0414)
Technicians	-0.274***	-0.230***	-0.216***	-0.215***
	(0.0418)	(0.0457)	(0.0464)	(0.0466)
Clerks	-0.280***	-0.220***	-0.217***	-0.218***
	(0.0334)	(0.0365)	(0.0370)	(0.0371)
Other Operational	-0.449***	-0.383***	-0.386***	-0.385***
	(0.0450)	(0.0467)	(0.0481)	(0.0481)
Observations	19,271	15,545	14,683	14,664
Panel B: 2012 – 2017				
Managers	-0.156***	-0.152***	-0.144***	-0.143***
	(0.0322)	(0.0325)	(0.0329)	(0.0330)
Professionals	-0.157***	-0.133***	-0.121***	-0.122***
	(0.0426)	(0.0440)	(0.0456)	(0.0459)
Technicians	-0.234***	-0.197***	-0.199***	-0.198***
	(0.0513)	(0.0544)	(0.0549)	(0.0552)
Clerks	-0.228***	-0.184***	-0.178***	-0.179***
	(0.0445)	(0.0456)	(0.0466)	(0.0470)
Other Operational	-0.410***	-0.348***	-0.351***	-0.348***
	(0.0642)	(0.0646)	(0.0659)	(0.0663)
Observations	12,751	10,324	9,721	9,709
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes

Note: This regression shows the impact of restructuring on the natural logarithm of the number of employees working in a certain occupational group. The data is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees in typical employment relationships, i.e. who are required to pay social security contributions. The dependent variables are the natural logarithms of the total number of employees and employees in certain occupational group per establishment. Restructuring is a dummy variable taking the value 1 in the current and all later years if an establishment has relocated or separated parts of the establishment. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the industry-year level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.19: Effect of Restructuring on Size of Occupational Layers Including Small Firms

	(1)	(2)	(3)	(4)
Variables	Indep. Variable: Restructuring			
Panel A: 2009–2017				
Managers	0.0145***	0.0139***	0.0143***	0.0143***
	(0.00460)	(0.00502)	(0.00518)	(0.00179)
Professionals	0.00527	0.00282	0.00254	0.00271
	(0.00530)	(0.00616)	(0.00595)	(0.00276)
Technicians	0.00564	0.00908	0.0124	0.0125***
	(0.00694)	(0.00806)	(0.00786)	(0.00362)
Clerks	0.000699	0.00496	0.00467	0.00441
	(0.00725)	(0.00783)	(0.00748)	(0.00413)
Other Operational	-0.0261***	-0.0307***	-0.0339***	-0.0340***
	(0.00840)	(0.00895)	(0.00913)	(0.00452)
Observations	42,939	34,908	33,167	33,132
Panel B: 2012–2017				
Managers	-0.121***	-0.125***	-0.127***	-0.125***
	(0.0332)	(0.0363)	(0.0363)	(0.0158)
Professionals	-0.107***	-0.0985**	-0.0912**	-0.0886***
	(0.0364)	(0.0392)	(0.0403)	(0.0164)
Technicians	-0.182***	-0.159***	-0.163***	-0.161***
	(0.0475)	(0.0511)	(0.0530)	(0.0183)
Clerks	-0.208***	-0.189***	-0.184***	-0.185***
	(0.0420)	(0.0414)	(0.0425)	(0.0176)
Other Operational	-0.354***	-0.311***	-0.314***	-0.312***
	(0.0588)	(0.0602)	(0.0622)	(0.0184)
Observations	28,939	23,745	22,503	22,479
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
$Size_{t-1}$		yes	yes	yes
$Performance_{t-1}$			yes	yes
$Competition_{t-1}$				yes

Note: This regression shows the impact of restructuring on the natural logarithm of the number of employees working in a certain occupational group. The data is trimmed, such that only establishment with more than 10 typical employees when entering the data set are included. Regressions include all employees working in a typical employment relationship, i.e. who are required to pay social security contributions. Panel A includes observations from 2009 on, while Panel B comprises observations after 2012. The dependent variables are the natural logarithms of the total number of employees and employees in certain occupational group per establishment. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were closed down, relocated, or separated during the respective year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). $Size_{t-1}$ measures the establishment's lagged size (4 dummies), $Performance_{t-1}$ measures the annual result (3 dummies), and $Competition_{t-1}$ measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.20: Effects of Alternate Restructuring on Size of Occupational Layers

	(1)	(2)	(3)	(4)
Variables	Indep. Variable: Restructuring (Alternate)			
Panel A: 2009 – 2017				
Managers	-0.0608	-0.0238	-0.0312	-0.0303
	(0.0370)	(0.0417)	(0.0422)	(0.0252)
Professionals	-0.158***	-0.134***	-0.138***	-0.138***
	(0.0392)	(0.0429)	(0.0427)	(0.0218)
Technicians	-0.246***	-0.213***	-0.204***	-0.204***
	(0.0431)	(0.0449)	(0.0457)	(0.0234)
Clerks	-0.237***	-0.183***	-0.177***	-0.177***
	(0.0373)	(0.0366)	(0.0373)	(0.0210)
Other Operational	-0.360***	-0.311***	-0.311***	-0.310***
	(0.0509)	(0.0470)	(0.0482)	(0.0232)
Observations	20,697	16,801	15,876	15,856
Panel B: 2012 – 2017				
Managers	-0.135***	-0.132***	-0.123***	-0.123***
	(0.0313)	(0.0340)	(0.0343)	(0.0168)
Professionals	-0.122***	-0.0987***	-0.0880**	-0.0882***
	(0.0344)	(0.0359)	(0.0367)	(0.0188)
Technicians	-0.206***	-0.179***	-0.176***	-0.175***
	(0.0433)	(0.0437)	(0.0452)	(0.0190)
Clerks	-0.203***	-0.174***	-0.169***	-0.170***
	(0.0390)	(0.0386)	(0.0398)	(0.0186)
Other Operational	-0.326***	-0.284***	-0.279***	-0.276***
	(0.0570)	(0.0552)	(0.0564)	(0.0215)
Observations	13,640	11,112	10,479	10,466
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes

Note: This regression shows the impact of restructuring on the natural logarithm of the number of employees working in a certain occupational group. The data is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees in typical employment relationships, i.e. who are required to pay social security contributions. The dependent variables are the natural logarithms of the number of employees in a certain occupational group per establishment. Each coefficient stems from a single regression of these dependent variables on Restructuring (Alternate), which is a dummy variable taking the value 1 if parts of the establishment were closed down, relocated or separated during the respective year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

Table C.21: Effect of Restructuring on Size of Task Complexity Layers

	(1)	(2)	(3)	(4)
Variables	Indep. Variable: Restructuring			
Panel A: 2009–2017				
Complex expert tasks	-0.259***	-0.249***	-0.239***	-0.239***
	(0.0551)	(0.0600)	(0.0611)	(0.0612)
Complex specialist tasks	-0.241***	-0.210***	-0.197***	-0.195***
	(0.0618)	(0.0606)	(0.0613)	(0.0614)
Professional tasks	-0.365***	-0.281***	-0.284***	-0.284***
	(0.0545)	(0.0471)	(0.0476)	(0.0477)
Helper tasks	-0.455***	-0.417***	-0.422***	-0.422***
	(0.0781)	(0.0840)	(0.0855)	(0.0857)
Observations	19,271	15,545	14,683	14,664
Panel B: 2012–2017				
Complex expert tasks	-0.180***	-0.157***	-0.153***	-0.152***
	(0.0506)	(0.0518)	(0.0528)	(0.0531)
Complex specialist tasks	-0.255***	-0.210***	-0.211***	-0.210***
	(0.0643)	(0.0641)	(0.0663)	(0.0667)
Professional tasks	-0.326***	-0.280***	-0.277***	-0.278***
	(0.0708)	(0.0635)	(0.0651)	(0.0657)
Helper tasks	-0.315***	-0.262***	-0.261***	-0.258***
	(0.0806)	(0.0897)	(0.0916)	(0.0921)
Observations	12,751	10,324	9,721	9,709
Controls	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Year FE	yes	yes	yes	yes
Size_{t-1}		yes	yes	yes
Performance_{t-1}			yes	yes
Competition_{t-1}				yes

Note: This regression shows the impact of restructuring on the natural logarithm of the number of employees working in a certain task complexity group. The data is trimmed, such that only establishment with more than 50 typical employees when entering the data set are included. Regressions include all employees working in a typical employment relationship, i.e. who are required to pay social security contributions. The dependent variables are the log number of employees in a certain task complexity group. Each coefficient stems from a single regression of these dependent variables on Restructuring, which is a dummy variable taking the value 1 if parts of the establishment were relocated or separated during the respective year. Controls comprise industry (213 dummies), state (15 dummies), legal form (5 dummies), degree of establishment independence (4 dummies), type of wage agreement (3 dummies), and work council (0/1). Size_{t-1} measures the establishment's lagged size (4 dummies), Performance_{t-1} measures the annual result (3 dummies), and Competition_{t-1} measures the degree of competitive pressure (4 dummies). Standard errors clustered on the establishment level in parentheses. The symbols *,**, and *** represent significance levels of 10%, 5%, and 1%, respectively.

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