Causes and effects of worker mobility between firms: empirical studies for Germany

Dissertation

zur Erlangung des Grades

Doktor der Wirtschaftswissenschaften (Dr. rer. pol.)

der Wirtschaftswissenschaftlichen Fakultät
der Julius-Maximilians-Universität Würzburg

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Vorgelegt am 14. Juni 2017

Als Dissertation genehmigt

von der Wirtschaftswissenschaftlichen Fakultät

der Julius-Maximilians-Universität Würzburg

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Tag der mündlichen Prüfung:	13. September 2017

Diese Dissertation ist in englischer Sprache verfasst. Eine Zusammenfassung in deutscher Sprache gemäß § 7 (2) der geltenden Promotionsordnung vom 19. Oktober 1998 (KMBl II 1999 S. 53), zuletzt geändert durch Dritte Satzung vom 19. Dezember 2008, findet sich am Ende dieses Dokuments.

Acknowledgements

I thank Thomas Zwick, who supervised this dissertation, for his admirable commitment, constructive criticism, and constant encouragement during the last years. Some of the best ideas that went into this thesis are his rather than mine. As a co-author, he contributed enormously to the third chapter of this dissertation, a joint project from which I learned a lot. I also thank my second supervisor, Uwe Blien, for his comments on all three empirical studies in this thesis, and for his strong support of my work during the last years. Working at 'his' research department at IAB during most of my doctoral studies was an invaluable asset that I greatly appreciate.

I am no less indebted to Anette Haas, who has been my mentor and supporter even before I began working on this dissertation. It is no exaggeration to say that this thesis would not have come into being without her encouragement and highly constructive comments. Many thanks also to Katja Wolf, from whom I learned so much during our joint project (neither the learning nor the project are quite finished yet). Another great Thanks goes to Wolfgang Dauth, who never gets tired of giving excellent advice on tiny methodological problems as well as on entire papers.

Furthermore, I thank Oliver Falck, Andreas Mazat, and Tobias Lohse for the enjoyable work together, from which I still continue to benefit. Even though single-authored, the fourth chapter of this dissertation would never have come into being without our fruitful and inspiring cooperation.

I also thank Daniel F. Heuermann, Johannes Ludsteck, Jens Mohrenweiser, Frank Neffke, Michel Serafinelli, Joachim Wolff, and Nick Zubanov for very helpful comments on different parts of this thesis, and Klara Kaufmann for excellent research assistance.

Thanks also to Max Nathan and Henry Overman of the What Works Centre for Local Economic Growth for giving me the opportunity to visit and to contribute a tiny little bit to the great work they are doing.

I would also like to thank everybody at GradAB, the joint graduate program of IAB and FAU Erlangen-Nuremberg, for the financial support and the great team spirit I experienced throughout my doctoral studies.

I thank my parents, my grandparents, and my brother for their emotional support throughout the years and their honest acknowledgement of my work.

However, my biggest Thank You goes to Sarah for her love and patience.

"Everything in the world is purchased by labour."

David Hume

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1 Introduction

1.1 The focus of this dissertation

This doctoral thesis aims at contributing empirical insights on the causes and effects of worker mobility[1] between firms to the related literature. Worker mobility is an important mechanism of reallocation in the labor market, and it is subject to conditions and restrictions rather specific to this market. For instance, the mobility of workers is spatially bounded, implying that labor market competition between firms can be very limited. Workers' mobility decisions may be voluntary or forced, meaning that the causes, but also the effects of worker mobility can be very diverse (beneficial or detrimental) for the affected workers and firms. Furthermore, unlike other production factors, workers can be carriers and multipliers of knowledge, meaning that the reallocation function of worker mobility extends beyond reallocating labor, for instance, to reallocating intellectual resources. These specificities of worker mobility as a reallocation mechanism make it a highly interesting, yet challenging subject of empirical economic research.

This dissertation comprises three empirical studies, each presented in a separate chapter. Although the studies evolved independently of each other, they complement each other by addressing different aspects of worker flows between firms,[2] focusing on causes and effects of inflows, outflows, and employment growth as a result thereof. Furthermore, regional economic aspects feature prominently in two of the three studies, and all empirical analyses use micro data and micro-econometric methods. In chapter 2, co-authored by Katja Wolf, the firm-level productivity effects of worker inflows are studied with regard to the relative productivity levels of firms of origin and destination firms. Chapter 3, co-authored by Thomas Zwick,

[1] I refer to "worker mobility" rather than "labor mobility" in the title because this thesis is mostly concerned with worker flows between firms, rather than the mobility of labor in a broader sense (which would include, inter alia, commuting, international migration, and flows between employment and non-employment).

[2] More precisely, the empirical analyses are conducted at the establishment level. For the sake of brevity, I nevertheless refer to firms in the more theoretical and conceptual parts of the thesis.

studies the effects of regional labor market competition on firms' apprentice training and poaching activity. Chapter 4 analyzes how local broadband internet availability affects the employment growth of firms in Germany, with a particular focus on sectoral effect heterogeneity, and thus on the potential reallocation of workers between sectors (and thus, firms).

The three studies address important aspects within the broad field of research on worker mobility between firms, namely the heterogeneity of workers and firms, the limited and varying degree of competition for workers, and the importance of technological developments for labor demand and the resulting worker flows. Still, this list of topics is far from exhaustive. By focusing on three particular research questions, the main chapters (2-4) necessarily neglect other important aspects of worker mobility between firms. In the remainder of this introductory chapter, I would thus like to step back behind the specific research questions of chapters 2-4, and provide a brief overview of other important aspects of inter-firm worker flows on which there is substantial scientific evidence. This overview forms the background to the subsequent empirical studies, such that the thesis as a whole may be read as a non-exhaustive, yet comprehensive inquiry on worker mobility between firms which clearly points out its specific contributions and limitations.

1.2 The different margins of worker mobility

Perhaps the most obvious sources of worker flows, and therefore reallocation in the labor market, are the entry and exit of workers and firms into and out of the labor market. A key indicator in employment statistics, resulting from these flows as well as the stock of employment, is the employment growth rate. While this rate summarizes in one number the prosperity of any labor market, it does not nearly describe the amount of reallocation therein. Cahuc and Zylberberg (2004, p. 505) document considerable excess job reallocation, for the 1980s and 1990s, in most industrialized countries: National aggregates of job reallocation (job openings plus closures) are in the order of 20 percent of total employment, exceeding the net growth rate of occupied jobs by a factor of about ten. More recently, inter alia due to population aging, reallocation rates have been decreasing, but they remain at several multiples of net employment growth rates (see e.g. Hyatt and Spletzer (2013) for an analysis of US data for the period 1998-2010). Moreover, Cahuc and Zylberberg (2004, p. 508 sqq.) point out that reallocation within the labor market, rather than labor market entry and exit – on either the worker or the firm side – accounts for the bulk of total reallocation in the labor market.

For reasons explored in more detail in the next subsection, worker reallocation due to firm entry and exit is less important, compared to mobility between established firms, in continental Europe than in the UK and the US (Cahuc and Zylberberg, 2004, p. 505 sqq.; Bassanini, 2010). At the industry level, Bassanini (2010) finds that in most OECD countries, job creation and destruction (as well as hires and separations) are rather balanced (positively correlated), meaning that job-creating industries are also job-destroying industries. In contrast, in the US, the UK, and also Denmark, job creation and destruction are negatively correlated, suggesting that industries are either growing or shrinking; therefore, reallocation between industries is quantitatively more important in these countries. At the worker level, flows between employment and unemployment are more widespread among the workforce in the US than in Europe, where exits from employment to unemployment are rather concentrated among few workers. Moreover, in Europe the unemployed stay unemployed longer and face higher barriers to re-entry into employment (Cahuc and Zylberberg 2004, p. 511). Overall, in the (continental) European context, worker mobility between established (not opening and closing) firms, and directly from job to job (not passing through unemployment), is the main mechanism of reallocation in the labor market (Cahuc and Zylberberg, 2004, p. 505 sqq.). It is this important margin of worker mobility that this thesis focuses on.

Beyond the quantitative importance of worker mobility between firms, this margin of mobility is associated with other important measures of economic performance. For instance, the amount of worker mobility between firms is positively related to the business cycle (Cahuc and Zylberberg, p. 511 sq.). Looking into details, Bassanini et al. (2010) find for a sample of OECD countries that job reallocation (closely linked to worker reallocation) is positively related to aggregate productivity growth. Research at the micro level suggests that worker mobility between firms can be a channel of productivity-increasing knowledge spillovers (Almeida and Kogut, 1999; Song et al., 2003; Tambe and Hitt, 2014). An increasing number of micro-econometric studies therefore analyze patterns of worker mobility that may give rise to spillover effects (e.g. Stoyanov and Zubanov, 2012, 2014; Serafinelli, 2013). An important finding for Germany in the last decades is that highly paid workers increasingly sort into high-paying firms (Card et al., 2013), which may generate positive effects for some workers and firms, but also raise wage inequality.

To sum up this section, the findings reviewed above point to worker mobility between firms as one of the most important margins of reallocation in the labor market. For most of Europe, worker mobility between established (incumbent and continuing) firms constitutes the most important margin. However, there are cross-country differences in the relative importance of different margins. In the following subsection, I therefore review some relevant research on the importance of cross-national institutional differences for the amount and particular characteristics of worker mobility. Following the literature, the focus is on labor market regulation.

1.3 The importance of labor market regulation

It is trivial to note that labor markets are not perfect in the sense of neoclassical theory. For instance, an incremental rise in wages in one region compared to another, even with equal regional costs of living, will not lead all workers from the lower-paying region to move to the higher-paying region. Workers' spatial mobility is limited by various factors, most obviously the direct costs of moving. As Manning (2003) emphasizes, workers are also psychologically and socially attached to their jobs. These sources of market imperfection largely elude empirical analysis since they are hard or impossible to observe. In contrast, a well observable source of labor market imperfection, and therefore a partial explanation for the limited mobility of worker against the hypothetical case of a perfect market, is regulation.

With regard to its implications for worker mobility, an often studied aspect of labor market regulation is the "flexibility" of the labor market, usually referring to the ease with which workers can change jobs, respectively, be hired and fired. Flexibility in this sense, and employment protection laws (EPL) in particular, are thus among the best-studied regulatory aspects in labor economics, and one of the most salient regulatory issues in the context of worker mobility and reallocation. I therefore review some key findings from this literature in the following.

According to Bassanini (2010), both job and worker flows vary substantially across countries. They are particularly large in the US and UK, but also in Denmark, i.e. countries characterized by a high degree of labor market flexibility, suggesting that regulation is an important – and often underestimated – factor underlying national mobility patterns. For instance, Haltiwanger et al. (2014) challenge the conclusion of previous studies which attribute cross-country variation in job reallocation to firm-size and industry effects, and find that labor market regulation indeed seems to be an important factor. In particular, part of the firm-size effect works implicitly through regulation, as small firms are usually exempt from legal requirements such as employment protection.

There is also evidence that labor market regulation affects not just the quantity, but also the quality of moving workers. Gielen and Tatsiramos (2012) find that voluntary quit rates in particular are lower in countries with strict EPL. Moreover, the (negative) correlation between job satisfaction and quits is less pronounced in countries with strict EPL. This finding probably reflects the higher future employment risks associated with quitting in countries with strict EPL, which include lower job offer arrival rates (and hence a higher risk of mismatch in the next job), a higher risk of layoff in the next job during the probationary period, and ineligibility for unemployment benefits when quitting to unemployment.

At the same time, Gielen and Tatsiramos (2012) find that in countries with strict EPL, voluntary quits are associated with relatively high wage gains in the next job. These wage gains possibly compensate quitters for the above-mentioned risks, and they likely reflect a positive selection of workers who are sure to find an even better job match after quitting. Thus, relatively strict EPL may keep workers in Europe locked into their jobs and induce a positive selectivity of voluntary job quitters. In addition, unemployment benefits are much higher in Europe than in the US,[3] so laid-off workers in Europe face a lower pecuniary pressure to find a new job quickly. Furthermore, strict EPL induces employers to screen new hires carefully during the probationary period, which might keep less productive workers from quitting once they have completed this period in their current job.

The above-cited studies suggest that mobility patterns in strictly regulated labor markets are somewhat polarized, at least relative to more flexible labor markets: On the one hand, there is voluntary mobility directly from job to job (mobility out of opportunity); on the other hand, there is mobility out of necessity (following job losses), potentially involving longer periods of unemployment between subsequent jobs. Thus, labor market regulation has important consequences not only for the amount of worker reallocation, but also for the reasons underlying worker moves and possibly, the characteristics of moving workers.

Understanding how labor market regulation differs across countries, and how it affects worker flows, is thus important for interpreting empirical findings for a particular country, and may help explain differences between findings for different countries. Germany, which the empirical chapters in this thesis focus on, is characterized by relatively strict regulation (compared to, e.g., the US and the UK) and a rather low degree of worker mobility. The empirical findings of this thesis thus may not apply to other national labor markets. For instance, the findings in chapter 2 deviate strongly from those of a related study on Denmark. However, although no explicit cross-country comparisons are made, the regulatory context is discussed in detail whenever appropriate (for instance regarding the German apprenticeship system in chapter 3). Moreover, the potential selectivity of moving workers, according to the above-cited evidence a consequence of strict labor market regulation, is investigated in detail in chapter 2.

[3] According to the OECD (2009), in the US unemployment benefits are among the least generous among all OECD countries, at less than half of the net replacement rates in, e.g., Germany and France. Furthermore, unemployment benefits are paid for much longer periods in continental Europe than in the US.

1.4 The endogenous 'localness' of labor markets

Another important peculiarity of labor markets is that they are to a large degree local. Essentially, this 'localness' stems from the limited mobility of workers. The leading approaches to empirically delineate local labor markets therefore aim to minimize commuting across regions. For Germany, currently there exist two well-established definitions of labor market regions based on commuting patterns (Kosfeld and Werner, 2012 and BBSR, 2012). Such classifications are typically based on administrative regional units such as districts,[4] meaning that pre-defined administrative units are nested within labor market regions, which consequently (and conveniently) do not overlap.

However, by requiring congruence with administrative geographical units, the established definitions of labor market regions are in fact a compromise solution at the cost of precision. After all, most workers may not consider district (or county) borders as relevant barriers between their residence and potential workplaces, unless the administrative geographical structure is strongly reflected in transport infrastructure. Furthermore, requiring local labor markets not to overlap introduces discontinuities at the boundaries, assigning individuals on two sides of a border to different regions even though they essentially face the same local labor market (Manning and Petrongolo, 2011).

Due to these problems, and with the increasing availability of employment data at very low geographical scale (e.g. municipalities, postal codes, or land parcels defined by geo-coordinates), research on regional labor markets increasingly relies on tailor-made delineations of local labor markets, allowing for much greater precision as well as for overlap (see e.g. Mühlemann and Wolter, 2011). Longitudinal employment data furthermore allow for tracing individual mobility even across large distances, which is important as workplaces may also determine places of residence, rather than vice versa. Manning and Petrongolo (2011) model the 'localness' of the UK labor market, explicitly considering individuals' job search behavior. Estimates of the model suggest that the utility of a job offer decays strongly and exponentially

[4] In the case of Germany, Kreise (NUTS 3 regions; NUTS stands for *Nomenclature des unités territoriales statistiques*).

with distance from the place of residence, suggesting that local labor markets are geographically rather small for most workers – smaller than established concepts of labor market regions suggest.

The findings of Manning and Petrongolo (2011) point to another important feature of labor markets beyond being 'local', namely that local labor markets are the result of individuals' decisions. That is to say that the boundaries of local labor markets are endogenously determined. Regional economic theory points to agglomeration economies as a source of geographical clustering of industries and thus also labor markets (e.g. Duranton and Puga, 2004). In contrast, the concept of endogenous labor markets provides a theoretically fairly agnostic explanation for labor market clustering. As exemplified by Schmutte (2014) and Nimczik (2016), endogenous labor markets can be identified by means of network analysis, where workers' job-to-job transitions are exploited to obtain clusters across which there is little mobility, but which are characterized – and indeed constituted – by substantial mobility within. While lacking a comprehensive theoretical underpinning (which would have to consider also product and housing markets), the virtue of this approach lies in its generality. By 'letting the data speak' rather than imposing a specific structure of firms' and workers' location and mobility decisions, endogenous labor markets may coincide with, but do not require, industry clusters or agglomerations.

Using US data, Schmutte (2014) finds four largely self-contained labor market segments between which workers typically change jobs. Interestingly, these segments can only vaguely be described in terms of industry, occupation, or education levels. In contrast, the analysis finds an important role for sorting, that is, low- and high-wage workers increasingly clustering among themselves. This finding is in line with another strand of literature on the development of wage heterogeneity (see Card et al., 2013 for Germany). However, Schmutte (2014) does not explicitly consider a geographical dimension of clustering. In contrast, Nimczik (2016) analyzes data from Austria and finds that endogenous labor markets are indeed spatially concentrated. However, the geographical shape and size of the identified labor markets do not coincide with administrative boundaries, and they differ markedly between subgroups of the labor force. In particular, labor markets are less geographically concentrated, but more concentrated in terms of industry, for highly qualified as compared to less qualified workers. Furthermore, as one would expect with average educational attainment increasing, the geographical size of endogenous labor markets is found to increase over time (between 1975 and 2005).

The main lesson of this subsection is that labor markets are largely local, often in a geographical sense, and that this localization is neither static nor exogenously given. Instead, the 'localness' of labor markets is not only a cause, but also a consequence of how workers move between jobs. In the short run, one may take a given structure of local labor markets as given. For instance, in chapter 3 it is assumed (but also critically discussed) that the labor market for apprenticeship completers is clustered in the geographical and occupational dimensions. Although essentially static, this definition allows for some intertemporal variation in the size and shape of local labor markets: Unlike industry affiliations, the occupational profiles of firms within a region may change over time, and so may the delineation of local labor markets thus defined. Yet, research on local labor markets should routinely challenge the criteria by which it defines localness. Longitudinal individual-level and geo-coded data, including information on travel distance and time, as well as the concept of endogenous labor markets applied by means of network analysis, are promising tools for researchers taking this challenge.

1.5 Summary

This dissertation comprises three empirical studies on worker mobility between firms in Germany. The three research questions addressed imply a narrow focus on particular aspects of worker flows between firms in all three studies. This introduction provided important insights from the related literature that form the background to the following empirical studies. To summarize these insights, first, worker mobility between firms is an important reallocation mechanism, both in terms of quantity and regarding its importance for the allocation not just of labor, but also of knowledge and productivity in the labor market. Second, the overall amount of worker mobility, as well as the reasons underlying workers' mobility decisions and the characteristics of moving workers, are shaped by institutional settings, notably national labor market regulations such as employment protection. Third, labor markets are typically strongly local, particularly but not exclusively in a geographical sense, and studies referring to this ‘localness’ need to define and explain their concept of localness accurately.

Against this background, the following chapters (2-4) address specific causes and effects of worker mobility. Therein, the focus is on the firm level, accounting for the importance of labor reallocation at this margin, and consequently using micro data and micro-econometric methods. Chapters 2 and 3 in particular use linked employer-employee data to trace the causes and effects of individual worker moves at the firm level. Chapter 4 uses firm-level data on employment growth, a result of worker inflows and outflows, to investigate potential complementarities between broadband internet and labor. The chapters thus provide micro-foundations for important labor market phenomena also observed in the aggregate, notably the sorting of highly-paid workers into high-paying firms, the low apprentice training activity of firms in regions with strong labor market competition, and potential complementarities between information technology and skilled labor.

By focusing on Germany, the empirical analysis refer to an empirical case characterized by relatively strict labor market regulation, implying that the amount of worker mobility is limited and that mobile workers may be positively or negatively selected, for instance, in terms of individual productivity. Chapters 2 and 3 explicitly address the selectivity of workers who move into or out of a given firm, and discuss possible reasons underlying these worker moves. However, the empirical findings may not apply to other countries due to national regulatory specificities. Finally, regional economic conditions are considered in detail in chapters 3 and 4. Accounting for the above discussion, chapter 3 in particular critically discusses why the labor market under consideration is localized, and in which dimensions.

Thus, even though the aspects discussed above are not the focus of the following empirical chapters, they are not disregarded altogether, either. Taken together, the three following chapters aim at providing a comprehensive, if selective empirical discussion of causes and effects of worker mobility between firms.

2 The productivity effects of worker mobility between heterogeneous firms*

2.1 Introduction

Can firms get more productive by hiring particular workers? If so, who are these workers, and what makes them particularly valuable? These are the questions we address in this study. A growing literature has come to the tacit consensus that worker inflows to a firm increase productivity if they come from – in some sense – superior firms (notably, Stoyanov and Zubanov, 2012, 2014; Serafinelli, 2013; and Balsvik, 2011). Broadly speaking, superiority here is defined as higher productivity, but partly also by a higher wage level. The results from this literature are interpreted as evidence of spillover effects between heterogeneous firms, as workers moving from superior to inferior firms transfer knowledge acquired at a superior firm to the hiring firm. Thus, firms can get more productive by hiring workers from superior firms and thus exploiting their superior-firm experience.

However, this finding may not be obtained if workers moving from "better" to "worse" firms are not randomly selected. Indeed, as they move to a potentially less attractive employer, they could be negatively selected from their sending firms. In contrast, movers from inferior to superior firms could also be positively selected – only the best workers from inferior firms might be attractive to superior firms. As a novelty to the literature on productivity effects of worker inflows, thus, we control for such potential selectivity. To do so, we consider the relative wage position of moving workers within their sending establishments. Using this measure, we study whether the heterogeneity of sending and hiring establishments alone accounts for potential productivity effects of worker inflows, or whether workers' relative wage position within the sending establishments also plays a part.

* This chapter is co-authored by Katja Wolf and has been published as Stockinger and Wolf (2016).

In contrast to previous studies, our findings for Germany suggest that inflows from inferior firms increase hiring firms' productivity. At the same time, these inflows are positively selected, that is, they have held above-average wage positions at their sending firms. Once we control for this selectivity, the inflows' positive effect on hiring firms' productivity disappears. Descriptive findings indicate a simple rationale for the observed pattern: Upward-moving workers, who are individually highly productive, simply may not be able to receive an adequate wage with their initial (inferior) employer. Thus, their only possibility to correct the mismatch is moving to a superior firm. We cannot confirm the result of previous studies that inflows from superior firms positively affect hiring firms' productivity. We can, however, rationalize their neutral effect as stemming from a neutral sending-firm wage position, that is, from not being positively selected.

We thus contribute to the literature on firm-level productivity effects of worker inflows, and more broadly to the broad research area of labor mobility as a channel of spillover effects at the firm level. We tackle endogeneity and sensitivity issues by various econometric methods, as we cannot rely on quasi-experimental or otherwise randomized variation in our explanatory variables. To the best of our knowledge, our study is the first of its kind for Germany. Contrasting previous studies' findings for other countries, our results also point to the importance of labor market structures and institutions in shaping mobility processes, although it is beyond the scope of this study to address these directly. Finally, our study complements recent empirical research on the rise of (Western) German wage heterogeneity by demonstrating, at the micro level, a process of worker mobility between heterogeneous firms that may be at the root of increasing firm-level wage inequality.

This chapter proceeds as follows. The next section reviews theoretical considerations and previous empirical work on spillovers through worker mobility between firms. Section 2.3 presents the model framework we employ to detect worker inflows' effects on firm productivity. In Sections 2.4 and 2.5, the empirical model and descriptive statistics are presented. In Section 2.6 we discuss the econometric implementation of our model and estimation results. In Section 2.7, we draw conclusions.

2.2 Theoretical concepts and previous evidence

A starting point in the theoretical literature about worker mobility as a channel of firm-level productivity effects is the literature on knowledge spillovers, where it is widely acknowledged that workers can act as carriers of knowledge. The fact that not all knowledge can be codified (notably, in the form of patents), but that its exchange and implementation usually require personal interaction ("tacitness of knowledge"), has spurred a rich literature on localized knowledge spillovers, see e.g. Breschi and Lissoni (2001, 2009), Rosenthal and Strange (2004), Power and Lundmark (2004), and Abel et al. (2012). Given the tacitness of knowledge, the most concrete and arguably most effective channel of knowledge spillovers is the mobility of workers, who carry knowledge from one firm to another. According to the studies of Almeida and Kogut (1999) and Song et al. (2003), it is the clustering of skilled workers, combined with a high degree of mobility, that accounts for the localization of knowledge spillovers in the semiconductor industry in Silicon Valley. Thus, knowledge spillovers are a strongly localized phenomenon exactly because labor mobility is spatially concentrated.

Following the pioneer studies on Silicon Valley, a growing number of studies have considered worker mobility as a channel of knowledge spillovers, building on the idea that any (skilled) worker is a potential carrier of knowledge. A theoretical model including worker flows as the channel of spillovers has been developed by Dasgupta (2012), who seeks to explain knowledge diffusion processes through worker flows from multinational enterprises (MNEs) to host-country domestic firms. The basic proposition of this model and recent empirical studies is that there is potential for spillovers when workers move from "superior" firms, which should possess a great stock of knowledge and technological capacities, to "inferior" firms which benefit from the additional knowledge thus received. These empirical studies include Stoyanov and Zubanov (2012, 2014), Serafinelli (2013), and Maliranta et al. (2009), who also find that firms do not fully compensate incoming workers (knowledge carriers) for their productivity effects, implying that worker inflows indeed are a channel of positive externalities to firms.

Thus, previous studies emphasize the role of firm heterogeneity, arguing that the occurrence and extent of spillovers through worker mobility depend on the characteristics of sending firms. A specific branch of literature focuses on knowledge spillovers between multinational enterprises (MNEs) and domestic firms, with the distinction between multinational and domestic firms being a classical dividing line between heterogeneous firms (Melitz, 2003). The underlying assumption is that domestic firms receiving worker inflows from MNEs thus receive new knowledge on technology, workplace practices, or markets, since MNEs generally work at a higher scale and use more advanced technology than Non-MNEs (for a theoretical argument, see also Helpman et al. (2004)). One of the first studies in this area is Görg and Strobl (2005), who find that Ghanaian manufacturing firms whose executives have previously worked for MNEs achieve higher productivity levels than their domestic competitors. Balsvik (2011) finds evidence of spillovers from MNEs in the Norwegian manufacturing sector, as firms with high shares of workers with MNE experience achieve higher productivity levels. Similarly, Poole (2013) finds evidence of spillovers from worker flows between MNEs and domestic firms in Brazil, as identified by the wages of the receiving firms' incumbent workers.

The productivity gap between sending and receiving firms and its implications for knowledge spillovers have also been studied more generally (beyond the multinational-domestic context). Stoyanov and Zubanov (2012, 2014) find that labor productivity and total factor productivity in Danish manufacturing firms are positively associated with the inflow of workers from more productive manufacturing firms, and the relationship gets stronger as the productivity gap between sending and hiring firms widens. The effect is small but robust (hiring an average quantity of knowledge carriers with average quality, as compared to hiring none, corresponds to a productivity gain of 0.35 percent). Taking several means to reduce endogeneity bias, Stoyanov and Zubanov (2012, 2014) thus identify the upper bound of a potentially causal effect of hiring employees from more productive firms on hiring firms' productivity. However, the effect is statistically not significant for (otherwise equal) inflows from less productive firms.

Closely related to Stoyanov and Zubanov's (2012, 2014) productivity gap approach, Serafinelli (2013) studies the impact of worker inflows from high-paying firms (a proxy for highly productive firms) on receiving (non-high-paying) firms' productivity, finding a positive effect. This result, too, survives a number of measures against reverse causality bias, e.g. using local high-wage-firm downsizings as an instrument for the number of inflows from such firms. Analogous to Stoyanov and Zubanov's (2012, 2014) results, it is found that inflows from non-high-paying firms do not have a similar effect.

A number of related studies indicate qualitatively similar patterns – having hired workers with particularly valuable experience is typically positively associated with hiring firms' productivity, probably reflecting a positive externality to hiring firms. To mention just a selection, Møen (2005) finds that Norwegian manufacturers partly internalize knowledge spillovers from separating R&D workers by setting relatively steep tenure-earnings profiles for these workers. Kaiser et al. (2008) analyze Danish firms' innovation, finding that the inflow of R&D workers is strongly related to the number of a firm's patent applications. Maliranta et al. (2009) come to similar conclusions concerning hiring firms' Non-R&D activities, i.e. firms benefit from inflows' earlier R&D experience in terms of their Non-R&D productivity. In sum, these studies substantiate the claim that firms can benefit from other, structurally superior firms' productive and innovative activities by hiring workers previously employed there.

While the evidence on the positive effects of superior-firm inflows is growing, and the interpretation of these effects as knowledge spillovers is compelling, it is neither theoretically nor empirically straightforward to expect such an effect. A theoretical reason not to expect positive effects from such 'downward' inflows is that they might be negatively selected from their sending firms. In some cases, hiring firms might actively attract such workers, precisely because they expect them to bring new, advanced knowledge to the firm. However, that might require them to offer unusually high wages (compared to the firm's average wage level) to the worker, in order to outbid the sending firm. In general, inferior firms may not be able to set such wage incentives, and so employees at superior firms (let alone their better employees) might be better off staying with their current employer. In contrast, since moving from inferior to superior firms is likely to be beneficial to the moving worker's wage, superior firms should be able to select the best employees from inferior firms, inducing a positive selection of worker flows in the upward direction.

Empirical evidence pointing in this direction has been provided, e.g., by Martins (2008), who shows that worker flows from domestic (inferior) to foreign (superior) firms in Portugal typically have been the better-paid employees in their sending establishment. Accordingly, the argument continues, productivity spillovers may arise from inferior to superior firms, rather than in the opposite direction. In the context of Germany (and other countries), furthermore, related empirical findings also suggest that "upward" worker flows may boost destination firms' productivity. As documented by Card et al. (2013), wage inequality in Western Germany has increased substantially since the 1980s, one of the main reasons being an increasingly positive-assortative pattern of worker flows between firms, that is, high-wage workers increasingly sort into high-wage firms. Therefore, the average high-wage worker (who should be relatively productive) should be moving up, rather than down, in terms of the firm's wage level. This pattern suggests that upward worker flows (from inferior to superior firms) are positively selected, and movers in the opposite direction, possibly negatively selected. Therefore, any study on the productivity effect of inflows from superior firms (versus inflows from inferior firms) has to take into account their potential selectivity, a point given great emphasis in Stoyanov and Zubanov (2012, 2014) and Serafinelli (2013). Against this background, it seems uncertain whether the positive productivity effects from downward-mobile workers found in these studies prevail in Germany, and the answer is likely to depend on the precise nature of worker inflows' selection.

2.3 A model of worker inflows' productivity effects

The above-cited studies, Stoyanov and Zubanov (2012, 2014) and Serafinelli (2013) in particular, seek to explain firms' output and value added by the quality of worker inflows, in terms of whether their sending firm is superior or inferior to the hiring firm. In some sense, Stoyanov and Zubanov's (2012, 2014) "productivity gap" model generalizes previous approaches in the literature, by using firm productivity to define which sending firms are superior and which inferior, whereas earlier studies have focused on more specific categories of superiority, namely MNEs or R&D-conducting firms. Serafinelli's (2013) approach takes another rather general approach, but ranks sending and hiring firms by firm-fixed wage effects instead of productivity levels.

Stoyanov and Zubanov (2012, 2014) refer to inflows from more productive firms "spillover potentials" (SPs), since it is these workers who should possess superior knowledge from their firms of origin. For the sake of brevity, we will use the same term for inflows from superior establishments, while referring to all other *inflows* (those from inferior establishments) as Non-SPs. Serafinelli (2013) takes a slightly different approach, first dividing all firms into high-wage firms (HWFs) and Non-HFWs, according to their fixed wage effect. This effect is obtained from a regression of individual wages including person and firm fixed effects, as first proposed by Abowd, Kramarz, and Margolis (1999), henceforth AKM, and implemented by Abowd, Creecy, and Kramarz (2002). All firms in the top third of the firm fixed effect distribution are classified as HWFs, the remaining two thirds as Non-HWFs. In a second step, Non-HWFs are analyzed with respect to worker inflow effects on productivity. The estimation approach common to all of the just-cited studies is to regress output (or value added) on separate measures of inflows from superior and inferior firms, controlling for capital, labor, and controls for firms', incumbent workers', and inflows' characteristics.

We employ an estimation framework building on Stoyanov and Zubanov (2012, 2014) and Serafinelli (2013). As shown by Stoyanov and Zubanov (2014), a simple production function framework can be used to estimate the effect of worker inflows on hiring firms' productivity. Therein, labor is modeled as a heterogeneous input consisting of two groups: Inflows from superior firms (SPs) and all other workers (Non-SPs in our terminology plus incumbent workers), where SPs are supposed to be individually more productive due to their superior experience. Note that we may just as well hypothesize Non-SPs to be more productive than the rest; yet we follow Stoyanov and Zubanov's (2014) notation to simplify the exposition. We now briefly sketch their production function framework, starting from a hiring firm's production function in Cobb-Douglas form,

$$Y_{it} = A_{it} K_{it}^{\beta_K} L_{it}^{\beta_L},$$

where Y_{it} is the value added of firm *i* in year *t*. Labor in efficiency units is defined as

$$L_{it} = L_{it}^{rest} + \varphi H_{i,t-1}^{SP} = \left(L_{it}^{rest} + H_{i,t-1}^{SP}\right)(1 - s_{it} + s_{it}\varphi)$$
$$= \tilde{L}_{it}[1 + s_{it}(\varphi - 1)],$$

with L_{it} as effective labor input, $H_{i,t-1}^{SP}$ as the number of SPs who arrived at time *t-1* (hires from more productive firms), $\tilde{L}_{it}$ as the total number of workers ($L_{it}^{rest} + H_{i,t-1}^{SP}$), $s_{it} = \frac{H_{i,t-1}^{SP}}{\tilde{L}_{it}}$ as the share of SPs in total employment, and the productivity advantage of SPs over other workers as $\varphi > 1$. Inserting the expression for effective labor input into the production function yields

$$Y_{it} = A_{it} K_{it}^{\beta_K} \tilde{L}_{it}^{\beta_L} [1 + s_{it}(\varphi - 1)]^{\beta_L},$$

indicating that the labor productivity effect of hiring SPs is described by the factor

$$1 + s_{it}(\varphi - 1)$$

and their effect on total factor productivity is

$$[1 + s_{it}(\varphi - 1)]^{\beta_L}.$$

Since $s_{it}(\varphi - 1)$ is close to 0 for reasonable range of s_{it} and φ, one can use the approximation

$$\ln[1 + s_{it}(\varphi - 1)] \approx s_{it}(\varphi - 1)$$

to infer the production function in logs (indicated by lower-case letters):

$$y_{it} = a_{it} + \beta_k k_{it} + \beta_l \tilde{l}_{it} + \beta_l (\varphi - 1) s_{it}. \tag{2.1}$$

This equation states that firm productivity depends positively on the share of SPs within all of the firm's employees. We may simplify this expression to

$$y_{it} = a_{it} + \beta_k k_{it} + \beta_l \tilde{l}_{it} + \vartheta s_{it}, \qquad (2.2)$$

where ϑ replaces the combined effect of labor productivity and SPs' productivity advantage over the firm's other employees. According to this reduced-form model, thus, a firm's productivity depends positively on how many SPs it has hired in the previous period, expressed as a share within all of the firm's employees.

However, as pointed out by both by Stoyanov and Zubanov (2012, 2014) and Serafinelli (2013), inflows from superior firms might not be randomly selected from their sending firms. Considering that moving from a highly productive (or high-paying) firm to a less productive one might yield a negative outcome for the moving worker (a small or even negative wage change), workers moving in this direction could be negatively selected. The cited studies account for such a possible "lemons bias" by including accurate individual-level control variables on worker inflows. Thus, SPs and Non-SPs, or HWF and Non-HWF inflows, are made comparable in all individual-level aspects independent of firm-level characteristics, and differ only with respect to the relative productivity (or wage) level of their sending firms. Generally (that is, concerning both SPs and Non-SPs), our analysis focuses on skilled workers, who possess the potential to carry substantial productive knowledge. Also, we choose to focus on job moves without long interruptions (periods of non-employment between jobs), during which the skills and knowledge acquired in the sending firm may depreciate. In the following, we present our empirical implementation of the above-sketched model approach, devoting particular attention to our distinction of superior and inferior firms, and to the problem of worker inflows' potential selectivity.

2.4 Empirical implementation

2.4.1 Data

We construct a linked employer-employee data set based on German data provided by the Institute for Employment Research (IAB). Individual-level data are obtained from the Integrated Employment Biographies (IEB), establishment-level data from the Establishment History Panel (BHP) and the IAB Establishment Panel. The two former databases are 100 percent records of employment subject to social security contribution, while the IAB Establishment Panel is the largest establishment survey in Germany. The IEB contain precise information about individuals' labor market biographies. They are based on different administrative sources and contain daily information on every individual in Germany who is either in employment subject to social security, "minor" employment,[6] registered unemployed, or participating in measures of active labor market policy, excluding only civil servants and the self-employed. A detailed description of the IEB's construction is given in Vom Berge et al. (2013). The assignment of workers to establishments, as well as crucial variables such as begin and end dates of employment spells, are highly accurate and reliable as they are drawn from the official employment statistics of the Federal Employment Agency, which serves as the basis to compute contributions to social security. In our data, employers are not firms in a legal sense, but establishments, that is, spatially fixed production units which may be part of multi-establishment firms. While the lack of firm-level data (such as balance sheet information) does set limits to our analysis, we think that establishments are well suited for the analysis of worker inflows and productivity, as workers can be assigned unambiguously to establishments (unlike firms), allowing us to conduct a relatively fine-grained analysis of productivity effects, and spillover effects in particular.

We count an individual worker as an inflow in establishment i if he or she was employed in another plant j before and both employment spells are at least seven days long. Since we consider newly hired workers as knowledge carriers, we require them to satisfy several conditions. Most importantly, we disregard all inflows of unqualified workers, requiring inflows to have a tertiary education or at least hold a vocational degree, in line with previous studies. We exclude all inflows employed as apprentices, interns, or "minor" employees, either in the sending or hiring establishment. Moreover, only incoming workers between the ages of 15 and 65,

[6] Minor employment is defined as employment not subject to social security contribution, with the monthly wage not exceeding (currently) 450 Euros, see Section 8, Subsec. 1, No. 1, of the German Social Code IV (SGB IV).

the official retirement age, are included.[7] Furthermore, we choose to allow a maximum gap of half a year (182 days) between two consecutive employment spells. In case of a period of unemployment between two employment spells, it must not be longer than three months. By German standards, these transition periods should be generous enough to retain most if not all of the relevant worker transitions, but rule out overly long employment gaps during which workers' recently acquired experience (interpreted as human capital) may already begin to depreciate.

The key criterion for the identification of inflows from other establishments is a change in the establishment identification number (establishment ID). In this context three issues, which have plagued previous analyses of inter-firm worker flows using German employment data, have to be discussed. First, a worker could be employed by two employers at the same time. For each point in time (i.e. each day), we assign each worker to a single employer, using the highest daily wage as the criterion of assignment. Second, as Hethey and Schmieder (2010) point out, establishment IDs appear and disappear not only in case of plant creation and closure, but also in case of spin-offs, acquisitions, restructurings, and changes of owner. In our context, this means that we must not consider flows between establishment IDs to be real labor flows if all or a substantial fraction of incoming workers come from the same establishment ID, as this might reflect a spin-off, restructuring, acquisition, or change of owner. For each establishment and year, we detect and remove clustered outflows from an establishment ID that, according to Hethey and Schmieder (2010), are probably incidents of an owner change, acquisition, or similar events. Third, we must ensure that establishments between which we observe worker flows are not part of the same firm. We make use of a routine developed by Schäffler (2014) to estimate which establishments probably belong to the same firm, and disregard worker flows between such establishments. This procedure is based on establishments' names and legal form (for details, see Schäffler, 2014).[8] Thus, we ensure that the worker flows entering our analysis are not spurious in the

[7] In fact, the youngest inflow we observe is 19 years old, as it is hardly possible to obtain a vocational degree at a younger age.
[8] We thank Steffen Kaimer (IAB) for running this procedure, which requires the use of non-anonymized establishment data, on our behalf.

sense that they do not represent worker mobility between two economically independent (potentially competing) units of production.

Since the IEB contain no information on establishment-level variables like value added or capital, we draw these data from the IAB Establishment Panel, an unbalanced panel survey of German establishments, of which we use the waves 2003-2011 (see Fischer et al. (2009) for more information on the Establishment Panel). For details on the linking of employer and employee data, see Heining et al. (2013). In line with most of the previous literature, we only analyze productivity effects for (hiring) establishments in the manufacturing sector, which we define as the range of NACE[9] Rev. 1.1 (or, equivalently, ISIC[10] Rev. 3.1) divisions 15 through 41.[11] The interpretation of revenues (proxy for output) and intermediate inputs, and therefore value added, is more consistent when focusing on this sector.[12] To obtain the capital stock, we use the modified perpetual inventory method (PIM) by Müller (2008), deducing capital from net investment, which is surveyed in the Establishment Panel. The method uses investment data to infer the capital stock and industry-level depreciation rates for different categories of investment goods. We reckon that the method is adequate for the manufacturing sector, where the quality and depreciation of capital should be comparable within each of the different manufacturing industries. As emphasized by Ehrl (2013), whose procedure we also employ, the PIM must be further corrected for restructuring events such as insourcing, closure, sell-off, and spin-off of parts of the establishment.

[9] Nomenclature Générale des Activités Economiques dans I'Union Européene.

[10] International Standard Industrial Classification.

[11] Within the period from which we draw data, the industry classification scheme has changed several times, notably, from the Classification of Industries 1993 (WZ93) to WZ03 in 2003 and from WZ03 to WZ08 in 2008. We deal with this problem by merging the industry code assigned by Eberle et al. (2011), who used intertemporal imputation of industry codes within establishments (establishments virtually never change industries) and a crosswalk between different classifications.

[12] A problem of the IAB Establishment Panel is that the entity referred to as the establishment may differ between the administrative records and the survey. To address this problem, we compare the total numbers of employees reported in the administrative register and the survey. We therefore drop establishment observations for which the reported numbers of regular employees (subject to social security, excluding marginal employees) deviate from each other by an implausibly large amount.

2.4.2 Identifying superior and inferior establishments

The key to deriving our estimation model is to identify worker inflows to each establishment in our sample and to determine for each inflow whether (s)he comes from a superior or inferior establishment. Unlike Stoyanov and Zubanov (2012, 2014), we do not have data on sending establishments' output, sales, or inputs – we only have these data (from the IAB Establishment Panel) for the sample of (potential) hiring establishments (some establishments, obviously, do not report any hires, but are still included in our analysis of productivity effects). Similar to Serafinelli (2013), thus, we consider ranking establishments using establishment fixed wage effects. We obtain these fixed effects from OLS wage regressions, separate for each of the relevant years, of all regular full-time employees (excluding apprentices and marginal employees) in any of the sending or hiring establishments at the reference date June 30. By performing the regression separately for each year, we identify the establishment fixed effect not from variation across time, but across workers. More explicitly, we estimate for each year:

$$\begin{aligned}\ln w_{pi} = \ & \beta_0 + \beta_1 male_p + \beta_2 age_p + \beta_3 age_p^2 + \\ & \textstyle\sum_{l=1}^{L} \beta_{4,l} occ\_stat_{lpi} + \sum_{m=1}^{M} \beta_{5,m} qual_{mp} + \sum_{n=1}^{N} \beta_{6,n} d\_occ2_{npi} + \\ & \theta_i + \epsilon_{pi},\end{aligned} \tag{2.3}$$

where $\ln w_{pi}$ is the log wage of worker p working at establishment i, occ_stat_{lpi} is a categorical variable indicating the occupational status of worker p in that particular job at plant i (e.g., blue-collar vs. white-collar, which can be related to different wage groups defined in collective agreements), $qual_{mp}$ is a categorical variable of worker p's qualification level, and d_occ2_{npi} is a two-digit occupation dummy. Wages, which are censored at the social security contribution limit (which concerns some 15 percent of employees), are imputed for censored observations adapting a modified version of the procedure proposed by Gartner (2005).[13] Importantly, the

[13] Additional to the covariates in (2.3), in the imputation we use region and industry fixed effects, the mean non-censored log wage in the establishment and year, and the share of censored worker observations in the establishment and year. Rather than including gender dummies, we run the imputation separately for four cells, dividing the population not only between women and men but also between Eastern and Western Germany.

results suggest that some 70 percent of unexplained wage variance is due to establishment fixed effects, indicating the importance of establishments for the determination of wages (see the estimation results for the first (2000) and last year (2010) in Appendix Table A.2.1). This finding is perfectly in line with empirical results for Denmark (for which Stoyanov and Zubanov conduct their analyses), despite marked structural differences between both countries' labor markets (see Christensen et al., 2005).

To be used as a criterion for ranking pairs of sending and hiring establishments, the estimated establishment fixed effects $\hat{\theta}$ are regressed on a set of industry dummies at the three-digit level, analogous to Stoyanov and Zubanov (2012, 2014) and Serafinelli (2013), yielding a corrected establishment fixed effect $\widehat{\theta'}$. This correction accounts for systematic productivity differences, e.g. due to industry-specific technologies, that we do not want to determine the ranking between pairs of establishments.

A simpler, readily available measure to rank sending and hiring establishments is their median wage. We merge the establishment median wage (computed only for full-time workers) from the BHP. We take its logarithm and, as above, clear it of 3-digit industry fixed effect, using the obtained measure as an alternative criterion to rank sending and hiring establishments. To assess both measures, which we want to reflect establishments' productivity, we compare it to direct measures of productivity, where available. We have information on value added and capital from the IAB Establishment Panel for all sample establishments (not including sending establishments, cf. above), so we assess the quality of $\hat{\theta}$ and the log median wage as measures of firm quality for these establishments. We compare both measures to TFP and log value added per worker as direct measures of establishment productivity, by which we would prefer to rank all sending and hiring establishments if we could. Table 2.1 presents these correlations. While far from a perfect fit, $\hat{\theta}$ is fairly correlated with labor productivity (log value added per worker). The correlation with TFP, obtained as the residual from a simple OLS regression of value added on capital and labor (all in logs), is rather low at about 0.28. This may be due to the measurement of capital, which we obtain using investment data and the perpetual inventory method, implying that also TFP is measured with some error. Comparing both alternative ranking criteria, the establishment median wage reflects productivity better than the establishment fixed effect, albeit by a small margin. We thus use both measures to rank sending and hiring establishments, to assess the robustness of our approach.

Table 2.1: Correlations between productivity and establishment ranking criteria

Correlations	TFP	log value added per worker	establishment fixed effect	log estab. median wage
TFP	1.000			
log value added per worker	0.889	1.000		
establishment fixed effect	0.277	0.528	1.000	
log estab. median wage	0.317	0.553	0.918	1.000

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

One might also consider using an establishment fixed wage effect from an AKM-style regression to rank sending and hiring establishments. Serafinelli (2013) uses such an effect to divide sending firms into high-wage and non-high-wage firms. An equivalent effect (the "CHK establishment effect") has already been computed for German establishments by Card, Heining, and Kline (2015), and it is available for a fraction of our sample. However, we still prefer $\hat{\theta}$ and the log median wage, as the CHK establishment effect is necessarily time-invariant across most of our observation period (it is constructed for several eight-year intervals), since it is derived from worker movements across establishments. Therefore, its correlations with direct productivity measures are substantially lower (at .13 for TFP and .33 for log value added per worker).

2.4.3 Sample

Our final estimation sample contains 1,791 manufacturing establishments (4,233 observations) and ranges over the years 2002 to 2007, where we have up to six observations per establishment. Grouping establishment observations by whether they have any inflows, any SP inflows, or any Non-SP inflows, yields the total numbers displayed in Table 2.2: Half of all establishment observations in our estimation sample have a positive number of worker inflows who satisfy all our criteria (qualified, full-time, etc.). Among these, less than 29 percent have at least one inflow from a superior (higher-paying) establishment, in line with the intuition that it may be hard for low-wage employers to attract such workers. In contrast, nearly three in four hiring establishment have at least one Non-SP inflow.

Table 2.2: Number of establishment observations by number of worker inflows

All sample establishments			Establishments with >0 inflows		
	Freq.	Percent		Freq.	Percent
>0 inflows	2,108	49.80	>0 Non-SPs	1,508	71.54
No inflows	2,125	50.20	>0 SPs	600	28.46
Total	4,233	100.00	Total	2,108	100.00

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

Adding up inflows and incumbent workers, the sample represents 884,595 workers; therein (due to data cleaning), 14,976 workers are counted as inflows, 43 percent of which (6,441) are classified as SPs. The sample excludes obvious outliers (at the establishment level) in terms of our central model variables, notably regarding the number of inflows. Furthermore, observations with missing values in any variable used for estimation are excluded. Establishments with less than five full-time equivalent employees are also excluded.

2.5 Descriptive analysis

2.5.1 Establishments

Table 2.3 summarizes establishment characteristics. It is worth noting that half our sample establishments are located in Eastern Germany, far above their share in the actual establishment population. This disproportion is due to the sampling design of the Establishment Panel, and we will account for it by running separate regressions for East and West. A potentially worrisome point in this context is that worker flows between East and West may be asymmetrically Westbound, due to the Western regions' higher productivity and wage levels. Yet this is not the case: Over 90 percent of flows change employers within the same part of the country, and East-to-West moves are no more frequent than moves in the reverse direction.

Table 2.3: Establishment characteristics

	log value added	log capital	log labor	Eastern Germany	median wage
Mean	15.099	14.913	4.104	0.499	81.473
SD	1.849	2.238	1.454	0.500	27.703
Min	9.483	7.346	1.609	0.000	15.165
Max	21.588	22.492	9.723	1.000	184.977
Means by subgroup:					
>0 inflows	16.147	16.075	4.968	0.406	91.542
No inflows	14.059	13.761	3.246	0.592	71.484
>0 Non-SPs	16.541	16.448	5.283	0.352	96.945
>0 SPs	15.155	15.136	4.177	0.540	77.963

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

The lower panel of Table 2.3 separates establishments by whether they have any hiring, zero hiring, hiring of SPs (spillover potentials, i.e. inflows from higher-paying establishments), or hiring of Non-SPs (inflows from lower-paying establishments). Clearly and unsurprisingly, establishments with a positive number of hires are larger and have higher value added and capital levels than non-hiring establishments. Among those which hire any workers, those hiring SPs are slightly smaller and have lower value added and capital levels than those hiring at least one Non-SP worker.[14] This was to be expected: By definition, hiring SPs means hiring from more productive establishments; thus, the larger and more productive an establishment, the less likely it is for a given worker inflow to be an SP.

Table 2.4 summarizes employment characteristics of our establishment sample, again with the focus on distinguishing hirers, non-hirers, and hirers of SPs, respectively Non-SPs.[15] Reassuringly, hiring establishments (irrespective of SP or Non-SP hiring) have substantially higher employment growth rates than non-hirers. Other characteristics follow the same ordinal pattern, notably the share of high-

[14] Descriptive statistics are based on the SP definition using the log establishment median wage, but almost unchanged if the establishment fixed wage effect is used instead (not reported).
[15] All statistics weighted by each establishment's full-time equivalent number of employees.

qualified workers (those with an academic degree) and the mean age of the employees (hiring firms employ younger workers).

Table 2.4: Employment-related establishment characteristics

	empl. growth rate	share high-qual.	share male	mean age	share inflows
Mean	0.016	0.104	0.800	41.704	0.017
SD	0.129	0.090	0.149	2.874	0.021
Min	-0.732	0.000	0.000	17.000	0.000
Max	2.269	0.899	1.000	59.000	0.323
Means by subgroup:					
>0 inflows	0.024	0.109	0.809	41.529	0.019
No inflows	0.007	0.068	0.737	43.063	0.000
>0 Non-SPs	0.024	0.110	0.814	41.410	0.020
>0 SPs	0.026	0.101	0.764	42.453	0.014

Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

2.5.2 Worker inflows and incumbent workers

In Table 2.5, we take a look at incumbent workers' and inflows' individual characteristics, also separating SPs and Non-SPs.[16] We find that inflows are more highly qualified than incumbents, yet they earn lower gross daily wages (at the hiring establishment, i.e. after the job move), presumably due to their lower age and tenure.[17] SPs have a much better skill structure than Non-SPs: The share of high-skilled SPs is roughly twice that of high-skilled Non-SPs. This finding is intuitive as, by definition, SPs have been employed at a relatively high-paying establishment early on (potentially all their previous working life). Such employers likely have higher formal qualification requirements, thus the better skill profile compared to Non-SPs. Non-SP inflows are also younger than SPs. Younger workers' job moves have been found to respond more strongly to wage incentives (cf. Hunt, 2006), which suggests that Non-SPs might be following wage incentives more than SPs.

[16] Incumbent workers here are restricted by the same criteria as inflows (only qualified full-time employees).
[17] All monetary variables are deflated to 2010 levels using the consumer price index.

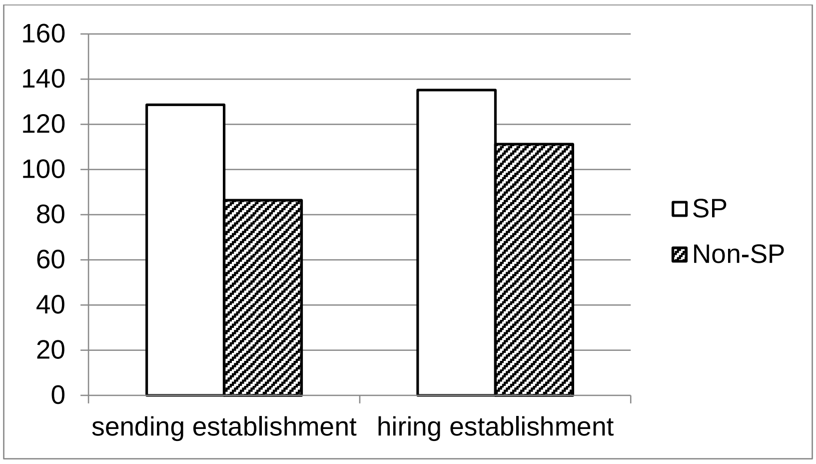
Table 2.5: Worker characteristics

	Mean age	Share male	Share high-qualified	Mean wage (hiring estab.)
Incumbent workers	41.674	0.844	0.138	123.175
All inflows	36.524	0.852	0.190	121.581
SPs	37.706	0.835	0.274	135.289
Non-SPs	35.631	0.865	0.127	111.316

Note: Share of SPs in all inflows = 0.43. Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

Looking at the wage profiles of SPs and Non-SPs (Figure 2.1), we find that SPs (white columns) have higher earnings levels both before and after the job move: Their mean sending-establishment daily wage (129 €) is well above Non-SPs' (shaded columns; 86 €), and is still some 20 percent higher at the hiring establishment, even though Non-SPs achieve tremendous wage gains (25 € on average) by their job move, almost four times as high as SPs' average wage change. Generally, thus, job movers appear to move out of opportunity rather than necessity, which suits our intention to focus on voluntary moves between jobs, rather than moves out of unemployment. It is not surprising that wage gains are larger for Non-SPs (movers to higher-paying establishments), but the magnitude of their gains appears striking, given that Non-SPs are less highly qualified than SPs.

Figure 2.1: Mean daily wage, SPs and Non-SPs



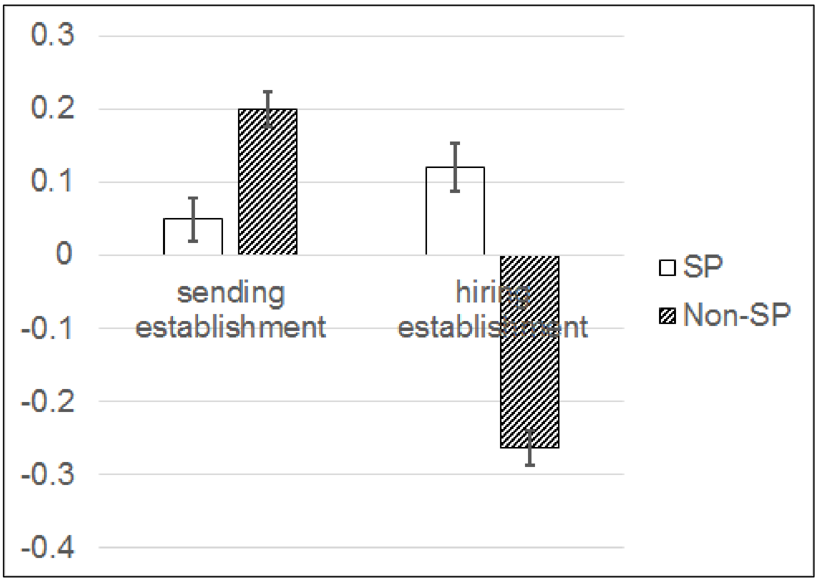
Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

It is not surprising that SPs generally earn higher wages, particularly at their sending establishments, as these are defined by paying relatively high wages. Comparing sending-establishment wages between SPs and Non-SPs is therefore trivial with respect to the between-establishment dimension. However, we have yet to consider the within-establishment dimension, to address the potential selectivity of within both groups of worker flows. We therefore compare the workers' rank (or relative quality) compared to their co-workers at the sending establishment. This metric, which we present in Figure 2.2, indicates whether the workers are positively or negatively selected from their sending establishment. We obtain the wage position of each moving worker, both for the sending (j) and hiring (i) establishment, from the wage regression used to obtain the establishment fixed effect in equation (2.3). We normalize the residual $\hat{\epsilon}_{pi}$, to make it comparable across establishments:

$$\hat{\epsilon}'_{pi} = \frac{\hat{\epsilon}_{pi} - \bar{\hat{\epsilon}}_{i}}{SD(\hat{\epsilon}_{i})} = \frac{\hat{\epsilon}_{pi}}{SD(\hat{\epsilon}_{i})}$$

(the mean residual of establishment i's workers, $\bar{\hat{\epsilon}}_{i}$, is equal to zero because the wage regression includes a constant). The parameter $\hat{\epsilon}'_{pi}$ indicates each worker's wage position relative to co-workers with the same age, gender, qualification, occupation, and occupation status. Thus, positive values of $\hat{\epsilon}'_{pi}$ indicate above-average earnings in a thus defined cell, while negative values indicate the opposite. We can therefore determine for each worker inflow whether the worker is positively or negatively positioned within his or her sending-establishment peer group. According to our estimates, Non-SPs are clearly positively selected among their peers in the sending establishment. This is not necessarily the case for SPs, who are only slightly positively selected from sending establishments, on average. Yet, Non-SPs do not move to a better relative wage position than SPs at their hiring establishments: Once arrived there, Non-SPs belong to the low-wage earners among their co-workers. In contrast, SPs generally move into positive wage positions. Again, we checked whether there are obvious imbalances between Eastern and Western German establishments, but found very little difference.

Figure 2.2: Mean wage position, SPs and Non-SPs



Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

Against this background, we do not have a clear expectation regarding our main research question – which worker inflows increase hiring establishments' productivity? On the one hand, SPs' generally higher wage levels and their experience at high-paying (and therefore, supposedly, highly productive) establishments suggests that SPs could be highly productive knowledge carriers, capable of increasing hiring establishments' productivity. On the other hand, Non-SPs are obviously a positive selection from their sending establishments, suggesting that Non-SPs could be even more likely than SPs to increase hiring establishments' productivity. In the following econometric analysis, thus, a central task is to control for inflows' individual productivity, in order to identify their productivity effect solely in terms of their origin (superior for SPs, inferior for Non-SPs) and to explore the reasons underlying this effect.

2.6 Econometric analysis

2.6.1 Specification

We implement the approach of Stoyanov and Zubanov (2014), that is, we estimate the productivity effects of hiring SPs and Non-SPs within a production function framework, where SPs and Non-SPs, together with the establishment's incumbent employees, can be thought of as heterogeneous factor inputs. Practically, the employment share of both inflow groups is added in the production function as derived above. Our estimation equation can be formulated as follows:

$$y_{it} = \beta_0 + \beta_k k_{it} + \beta_l l_{it} + \vartheta_1 share\_SP_{it} + \vartheta_2 share\_Non\_SP_{it} + controls\_SP_{it} + controls\_Non\_SP_{it} + ESTAB_{it} + EMPL_{it} + \varepsilon_{it},$$

where *y* is log value added and *k* and *l* are log capital and log labor,[18] respectively.[19] The core explanatory variables are $share\_SP_{it}$ and $share\_Non\_SP_{it}$, the labor shares of SPs and Non-SPs. Inflows are defined as all qualified full-time employees (satisfying a number of further criteria such as a plausible age range) who have arrived at some point between January 1st, *t-1* and January 1st, *t*, and are still present at January 1st, *t*. Their classification into SPs and Non-SPs is based on the sending and hiring establishment's median wage (or fixed wage effect) at June 30, *t-2*, since this is the last year they have potentially entirely spent at their former employer.

If it does not matter for hiring firms' productivity whether their skilled worker inflows originate from higher- or lower-paying (and therefore, approximately, more or less productive) establishments, we should obtain the same estimate for ϑ_1 and ϑ_2. If inflows do not matter for productivity at all we should obtain insignificant estimates for ϑ_1 as well as for ϑ_2. However, to ensure that we can interpret our estimates in this way, we have to ensure that SPs and Non-SPs do not differ in their individual productivity-relevant characteristics. We know from the descriptive analysis that they do differ in terms of qualification, age, wages, and wage positions, both in their sending and hiring establishments. Thus, we include several control variables for inflows (vectors $controls\_SP_{it}$ and $controls\_Non\_SP_{it}$): the share of high-qualified workers[20] among all (Non-)SPs and their respective mean age and mean of age squared. These controls are analogous to those used in Stoyanov and

[18] In measuring labor, we approximate full-time equivalents by applying the standard weights of .3 and .6, respectively, to workers with less than 18 hours per week, and those with 18 or more weekly work hours but less than full-time (we do not observe work hours more precisely).

[19] In most specifications, we include two lags of the dependent variable to account for autocorrelation.

[20] Holders of a university or university of applied sciences degree.

Zubanov (2012, 2014) and Serafinelli (2013). As argued above, an important characteristic of Non-SPs is their strongly positive selection from sending establishments. Since hiring Non-SPs may increase hiring establishments' productivity for precisely this reason, we optionally include the mean wage position of SPs and Non-SPs in their sending establishments.

The control variables vector $ESTAB_{it}$ includes categorical variables indicating whether the establishment is part of a larger enterprise, its legal form, the (self-reported) state of technical equipment, a dummy indicating young establishments (less than ten years old), and the share of exports in total revenues. Since these variables are almost entirely time-invariant, we drop them from all estimations based on within-establishment variance. $EMPL_{it}$ is the vector of employment structure controls, containing the share of high-qualified employees (holding a university or university of applied sciences degree), the mean age, and the share of males among all employees.

Concerning the estimation of establishment-level production functions, a fundamental problem is that input coefficients are estimated with bias in the Pooled OLS case since there can be omitted idiosyncratic productivity shocks and reverse causality, i.e. a direct influence of expected future productivity on inputs (for a very comprehensive and detailed discussion, see Eberhardt and Helmers, 2010). In our context, if we find a positive correlation between establishments' productivity and their hiring of certain workers, this might mean either that worker inflows increase productivity due to these workers' individual characteristics, or that highly productive establishments attract these workers because they anticipate their positive productivity path. The two main approaches to minimize this bias are, first, "structural" (control function) approaches trying to model unobserved idiosyncratic productivity determinants explicitly (Olley and Pakes, 1996; Levinsohn and Petrin, 2003; Ackerberg et al., 2006; Wooldridge, 2009), and second, dynamic panel data (DPD) approaches which use internal instruments in panel data sets (Arellano and Bond, 1991; Blundell and Bond, 1998, 2000).

For a detailed discussion of the pros and cons of both approaches, see Appendix section "Econometric issues of production function estimation". Both approaches have their advantages and disadvantages, and in our view, there is no ex ante reason to give one approach preference over the other. We will therefore employ both classes of estimators. One limitation we face either way, as already pointed out by Stoyanov and Zubanov (2012), is that we cannot control for unobserved hiring preferences regarding the origin of newly hired workers. This is because such preferences are not necessarily part of the unobserved idiosyncratic productivity shock that the "structural" estimators model explicitly. When using either of the DPD estimators, we cannot be sure that such preferences are time-invariant, such that we get rid of their biasing influence.

2.6.2 Results

As a baseline, we estimate the above empirical model using Pooled OLS, where we include two lags of the dependent variable (log value added) as this is found to remove residual autocorrelation. To begin with, we estimate a simplified model including the labor share of all inflows (SPs plus Non-SPs divided by labor) and the set of control variables defined in section 2.6.1. The first column of Table 2.6 indicates that productivity is not significantly related to hiring intensity as such (the share of inflows in total employment). In the second and third columns, we split inflows according to their classification as SPs or Non-SPs. Although we have found the log median wage to be more strongly related to establishment productivity, we also present results using the establishment fixed wage effect to define (Non-)SPs in the second (middle) column. The results indicate a positive association of Non-SP hiring with productivity, whereas the coefficient of SP hiring is near zero and insignificant.

Table 2.6: OLS estimates

Log value added	All inflows	(N)SPs defined by estab. FE	(N)SPs defined by estab. MW
L.Log value added	0.567***	0.566***	0.566***
	(38.405)	(38.364)	(38.320)
L2.Log value added	0.164***	0.164***	0.164***
	(11.889)	(11.866)	(11.899)
Log capital stock	0.037***	0.037***	0.037***
	(5.860)	(5.857)	(5.922)
Log labour	0.237***	0.237***	0.238***
	(16.302)	(16.030)	(16.103)
Share high-qual. inflows	-0.078**	-0.071**	-0.071**
	(-2.435)	(-2.237)	(-2.236)
Mean age inflows	0.015	0.002	0.003
	(1.319)	(0.854)	(1.170)
Mean age sq. inflows	-0.000	-0.000	-0.000
	(-1.391)	(-0.851)	(-1.114)
Labor share inflows	0.292		
	(1.033)		
Labor share SPs		-0.050	-0.157
		(-0.134)	(-0.401)
Labor share Non-SPs		0.764*	0.852**
		(1.730)	(2.006)
Observations	4233	4233	4233
Adjusted R^2	0.952	0.952	0.952

t statistics in parentheses. Standard errors clustered at establishment level. Year, 2-digit industry and labor market region (LMR) dummies included. ESTAB and EMPL control variables included. All regressions include a constant. * p<0.10, ** p<0.05, *** p<0.01. Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

Concerning the core production factors capital and labor, our estimates imply slightly increasing, but almost constant returns to scale (the sum of long-run capital and labor coefficients is 1.04). Finding that inflows' average qualification (measured by the share of high-qualified inflows) is negatively related to productivity may be surprising at first sight, yet manufacturing establishments may profit particularly from hiring workers with a vocational degree, who are specialized in industry-specific work tasks. In Germany, such workers have usually received their highest degree in the apprenticeship system, which defines them as mid-qualified (rather than high-qualified), notwithstanding their high productivity in the production process.

Table 2.7: OP and LP estimates

	OP	OP	LP	LP
(N)SPs defined by:	Estab. FE	Estab. MW	Estab. FE	Estab. MW
Dependent variable	Log revenues	Log revenues	Log value added	Log value added
Log intermediate inputs	0.609***	0.608***		
	(56.825)	(59.811)		
Log capital stock	0.059	0.541	0.117***	0.118***
	(0.008)	(0.079)	(2.901)	(2.804)
Log labour	0.324***	0.325***	0.724***	0.722***
	(23.866)	(23.321)	(27.432)	(26.074)
Share high-qual. inflows	-0.040*	-0.038*	-0.081	-0.080
	(-1.744)	(-1.677)	(-1.625)	(-1.625)
Mean age inflows	-0.000	0.001	0.004	0.004
	(-0.071)	(0.456)	(1.114)	(1.090)
Mean age sq. inflows	-0.000	-0.000	-0.000	-0.000
	(-0.275)	(-0.730)	(-1.425)	(-1.401)
Labor share SPs	0.244	0.135	-0.066	-0.556
	(0.920)	(0.534)	(-0.124)	(-1.001)
Labor share Non-SPs	0.622**	0.770***	1.017*	1.442***
	(2.404)	(2.620)	(1.753)	(2.622)
Observations	4227	4227	4233	4233

t statistics in parentheses. Standard errors obtained by bootstrap (1,000 replications). Trend (OP) resp. year dummies (LP), 2-digit industry and labor market region (LMR) dummies included. ESTAB and EMPL control variables included. LP regressions include a constant. *p<0.1, **p<0.05, ***p<0.01. Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

However, the OLS estimates are likely to be biased by determinants of productivity (deriving either from the amount of output or the efficiency of production in terms of factor use) observed by the establishment but not by the econometrician. Therefore, we estimate both of the latter OLS specifications using the estimators developed by Olley and Pakes (OP; 1996) and Levinsohn and Petrin (LP; 2003). The results are displayed in Table 2.7 and confirm the above finding: Having hired Non-SPs in the previous year is positively and significantly related to an establishment's productivity.

Another concern not yet addressed is that establishment heterogeneity, which is arguably rather persistent and to a large extent unobserved, may strongly co-determine productivity outcomes and hiring strategies. To address this additional concern of endogeneity regarding our central explanatory variables, we apply the System GMM estimator. This estimator accounts for unobserved time-invariant establishment heterogeneity by using within-establishment variation, and addresses reverse causality by instrumenting current differences of endogenous variables with past levels, and current levels with past differences.

Table 2.8: System-GMM estimates

Log value added	(N)SPs defined by FE	(N)SPs defined by MW	(N)SPs defined by FE	(N)SPs defined by MW
L.Log value added	0.406***	0.445	0.432	0.410***
	(5.616)	(1.376)	(1.012)	(5.455)
L2.Log value added	0.034	0.055	0.035	0.030
	(1.007)	(0.463)	(0.232)	(0.919)
Log capital stock	0.141**	0.170**	0.066	0.081
	(2.366)	(2.327)	(0.576)	(1.502)
Log labour	0.396***	0.275	0.486	0.448***
	(3.053)	(0.583)	(1.151)	(3.436)
Share high-qual. inflows	-0.035	-0.039	-0.072	-0.085*
	(-0.715)	(-0.642)	(-0.579)	(-1.673)
Mean age inflows	0.006	0.006	0.006	0.010**
	(1.408)	(0.874)	(0.908)	(2.293)
Mean age sq. inflows	-0.000*	-0.000	-0.000	-0.000**
	(-1.905)	(-1.079)	(-0.906)	(-2.156)
Labor share SPs	-0.831	-0.284	-1.237	-1.301
	(-0.399)	(-0.131)	(-0.386)	(-0.681)
Labor share Non-SPs	3.830	5.461**	1.333	0.348
	(1.321)	(2.159)	(0.454)	(0.152)
Mean wage pos. SPs			-0.082	0.016
			(-1.085)	(0.251)
Mean wage pos. Non-SPs			0.099	0.043
			(0.389)	(0.430)
Observations	4233	4233	4233	4233
Sargan p-value	0.26	0.58	0.518	0.204

t statistics in parentheses. Standard errors clustered at establishment level. Year, 2-digit industry and labor market region (LMR) dummies included. EMPL control variables included. All regressions include a constant. * p<0.10, ** p<0.05, *** p<0.01. Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

The results are presented in Table 2.8. Let us focus first one the more parsimonious specifications in the first and second column. Hiring Non-SPs is still found positively related to productivity, and it is significant at least for our (preferred) definition of (Non-)SPs by the sending establishment's median wage. For the definition based on establishment fixed wage effects, the Non-SP coefficient (3.830) is still not too far from statistical significance. Thus, even when controlling for reverse causality and unobserved time-invariant establishment characteristics, the share of

Non-SPs is positively associated with productivity. The long-run capital and labor coefficients still indicate near-constant returns to scale, with their sum close to one and therefore close to the results from the pooled specifications, suggesting our production function is appropriately specified.

Our preferred estimate of Non-SPs' productivity effect (5.461) would imply that the productivity gains of hiring Non-SPs are substantial: Hypothetically, the average sample establishment (which has a Non-SP labor share of about 0.9 percent) is roughly 4.9 percent more productive than an otherwise equal establishment that hires no Non-SPs. However, our result could be due to unobserved systematic differences between SP and Non-SP inflows, for which we have controlled so far only by including inflows' share of high-qualified, age, and age squared. Thus, we continue by addressing the insight of our descriptive analysis that Non-SPs constitute a positive selection from within their sending establishments, as assessed by their wage position relative to comparable co-workers (co-workers with the same age, qualification, occupation, etc.). We extend our specification to include inflows' mean sending-establishment wage position, separately for SPs and Non-SPs (columns three and four of Table 2.8). While the coefficients of both these variables are insignificant, Non-SPs' labor share coefficient drops sharply in magnitude and significance, using either the fixed-effect or the median-wage definition of (Non-)SPs. This finding suggests that the positive productivity outcome related to Non-SP hiring is due to these workers' positive selection from their sending establishments, so there is no statistically significant productivity effect of hiring workers from inferior establishments per se.

This is the main finding of our analysis: Hiring workers from inferior establishments is positively related to productivity because these workers are positively selected from their sending establishments. In contrast, hiring workers from superior establishments does not affect productivity; their superior-establishment experience is not valuable enough to affect hiring establishments' productivity through knowledge spillovers. These results can be further rationalized by our descriptive findings, which indicate that movers from inferior to superior establishments achieve tremendous wage increases. This is not least due to the fact that the bulk of unexplained wage variance between workers is due to establishment-level effects, as we have found in our auxiliary wage regression (Table A.2.1). That is, the best workers at lower-paying establishments, who are already being much better paid than their equally qualified co-workers, have little scope for further wage improvement when staying with their current employer. By moving to higher-paying establishments, thus, good workers are reallocated towards good firms, which not only increases their wages, but also hiring establishments' productivity.

To account for the disproportionate sampling of Eastern German establishments, we estimate the specifications derived above for the subsample of Western German establishments. The results, summarized in Table 2.9, corroborate the previous findings for the entire German sample: Non-SP hiring has a substantially positive and partly significant productivity coefficient, unless we control for inflows' selection from sending establishments. In the latter case, not only does the coefficient drop steeply, but we also find, in the specification where (Non-)SPs are defined by establishment fixed effects, Non-SPs' positive selectivity to be significantly related to productivity. The latter finding further substantiates our interpretation that hiring workers from inferior establishments increases productivity due to the positive selection of these workers. We report the estimates for Eastern German establishments in Table A.2.2 in the Appendix. We do not find any significant productivity effects associated with worker inflows into these establishments. The overall estimates (for Germany in total) would thus be larger and more precisely estimated, were it not for the disproportionate share of Eastern German establishments in our sample.

Table 2.9: System-GMM estimates, Western German establishments only

Log value added	(N)SPs defined by FE	(N)SPs defined by MW	(N)SPs defined by FE	(N)SPs defined by MW
L.Log value added	0.338***	0.368***	0.359***	0.350***
	(3.845)	(4.692)	(4.778)	(4.385)
L2.Log value added	0.011	0.017	-0.015	0.017
	(0.317)	(0.518)	(-0.425)	(0.481)
Log capital stock	0.104	0.104	0.036	0.081
	(1.124)	(1.246)	(0.483)	(1.194)
Log labour	0.506***	0.512***	0.611***	0.533***
	(2.893)	(3.335)	(4.130)	(4.478)
Share high-qual. inflows	-0.072	-0.079	-0.071	-0.089
	(-0.945)	(-1.438)	(-1.144)	(-1.341)
Mean age inflows	0.003	0.003	0.002	0.006
	(0.575)	(0.689)	(0.347)	(1.319)
Mean age sq. inflows	-0.000	-0.000	-0.000	-0.000
	(-0.705)	(-0.961)	(-0.555)	(-1.313)
Labor share SPs	1.216	0.638	1.278	-0.695
	(0.422)	(0.327)	(0.439)	(-0.370)
Labor share Non-SPs	2.091	5.041*	0.890	2.528
	(0.371)	(1.737)	(0.326)	(1.080)
Mean wage pos. SPs			-0.051	0.028
			(-0.615)	(0.393)
Mean wage pos. Non-SPs			0.139**	0.047
			(2.445)	(0.538)
Observations	2120	2120	2120	2120
Sargan p-value	0.523	0.800	0.865	0.438

t statistics in parentheses. Standard errors clustered at establishment level. Year dummies included. EMPL control variables included. All regressions include a constant. * p<0.10, ** p<0.05, *** p<0.01. Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

A final check we perform concerns external validity with respect to the business cycle. While the period of our estimates so far (2002-2007) contains both a stagnant phase in its earlier years and a period of strong growth later on, to this point, we have omitted the Great Recession of 2008/09. We have also run our regressions for a panel covering the period 2002-2010 (including both Eastern and Western German establishments), to see whether the changed hiring behavior during the recession affects the way inflows affect hiring establishments' productivity. In Germany and the manufacturing sector in particular, establishments reacted to the crisis by reducing work hours and hoarding labor, rather than by laying off large numbers of workers. The crisis also had a negative effect on hiring, and the few hires taken in during the crisis were probably different from 'normal-times' hires in non-random ways. OLS and System-GMM results, respectively, are presented in Tables A.2.3 and A.2.4 in the Appendix. While the overall pattern of results remains the same across all estimations, only OLS still yields a significant productivity coefficient of Non-SP hiring. The System-GMM estimator still yields a rather large coefficient which drops severely as inflows' selectivity is controlled for; however, the effect becomes insignificant. Possible reasons are that hiring numbers were too low during the recession to substantially affect production processes, or that working and machine-running hours were capped such that inflows could not make a substantial difference to the value added produced. In any case, our main findings are not contradicted, but only muted, by extending our model to times of economic downturn.

As a final note on the interpretation of our estimates, let us emphasize that our findings are not necessarily causal relationships. To obtain causal estimates, one would need a source of variation in SP/Non-SP inflows that is independent of hiring establishments' productivity. Such a source of variance for a large sample of hiring firms (and an even larger sample of sending firms) is hard to find. Possibly the best feasible approach has been taken by Serafinelli (2013), who uses the number of downsizings (substantial reductions in staff) of high-wage firms in the same region and industry as an instrumental variable for worker inflows from high-wage firms. Such downsizing events increase the potential supply of high-wage firm workers rather unexpectedly. We have constructed the same kind of instrument, dividing all establishments within each labor market region into a high-wage and a low-wage group, separated at the median of their median wage levels. Therein, the number of downsizing establishments are measured using several threshold values to define downsizing (the simplest one being a negative employment growth rate, others defining downsizing more narrowly). Several variants were considered in each case: First, both labor market regions and districts (NUTS 3 regions) were used as the relevant regional level. Second, instead of regions, regional industries (both at the labor market region and district levels) were considered. Unfortunately, it turned out none of the proposed instruments is strong enough in explaining our explanatory variables (the labor shares of SP and Non-SP inflows), so we have to rely on the above-presented estimates as approximations of potentially causal productivity effects.

2.7 Conclusions

We have investigated, at the establishment level, the productivity effects of hiring workers from superior and inferior establishments, as defined by establishments' relative wage level. In all estimations, we control for worker inflows' productivity-relevant characteristics, meaning that their productivity effects should stem only from their sending establishments' superiority or inferiority.

While previous studies find positive effects from hiring workers from superior firms, our estimates suggest that hiring workers from inferior (lower-paying) establishments increases hiring establishments' productivity. We also find that these workers are positively selected from their sending establishments, where they occupy relatively high wage positions. Indeed, this selectivity explains their positive productivity effect. For the subsample of Western German establishments, which are underrepresented in our total sample, we also find that these inflows' sending-establishment wage position is significantly positively related to hiring establishments' productivity. In contrast, hiring workers from higher-paying establishments does not seem to increase productivity, which is in line with the finding that they are not as positively selected from their sending establishments.

One reason for these contrary results might be the marked differences between national labor markets. As Jolivet et al. (2006) show, Germany and Denmark (where inflows from superior firms have been found to increase hiring firms' productivity) differ a lot with regard to the degree of job-to-job mobility as well as the reasons for mobility. In particular, mobility levels are higher, and workers more often move involuntarily, in Denmark.

To grasp the economic workings behind our findings, we have to consider the individual worker's perspective: Being a top earner at her initial employer, a worker at a lower-paying firm earns far less than the average equally qualified worker at a higher-paying firm. The only way to raise her wage to an adequate level, which then probably reflects her individual productivity, is moving to a higher-paying firm. Thus, our results reflect Card et al.'s (2013) finding of assortative worker mobility across heterogeneous firms: Good workers, as compared to their co-workers, move to good firms, as compared to the firm they leave. As we investigate mobility and production processes at the worker and establishment levels, our findings may be regarded as a micro-foundation for this aggregate mobility pattern and its implications.

To conclude, we would like to point out that a broader economic discussion of our results would have to address labor market frictions more thoroughly, not least because of the differing findings for different countries. The finding that worker mobility across firms can yield important wage gains indicates that some workers are initially badly matched with their employer. Overcoming this mismatch by moving to a "better" firm, highly productive workers reduce the amount of mismatch in the labor market. Explicitly incorporating the matching process in the empirical analysis, however, is beyond the scope of this study, as are the welfare effects associated with the identified mobility process. Further research might generate insight on these questions.

3 Apprentice poaching in regional labor markets*

3.1 Introduction

Poaching – hiring of employees against the will of the current employer – is an important threat to firm-sponsored general training. Both, training firms' risk of becoming a poaching victim, and firms' willingness and ability to commit poaching are likely to depend on the degree of competition in the labor market. Yet, direct empirical evidence on the link between competition and poaching is rather scarce. Our aim is to fill this gap in the literature, using an innovative empirical strategy to identify poaching and measuring competition at the level of regional occupational labor markets. Therein, we exploit the institutional peculiarities of the German apprenticeship system.

We follow up on recent studies which find a negative relationship between regional employer competition and firms' training provision. Brunello and Gambarotto (2007) for example find a negative relationship between regional employment density, as well as industrial specialization, and employer-provided training in the UK. Similarly, Brunello and De Paola (2008) and Andini et al. (2013) detect a negative relationship between regional employment density and firm-sponsored training in Italy. Most closely related to our study, Mühlemann and Wolter (2011) find for Switzerland that the density of firms in the same region and industry is negatively related to firms' apprenticeship training activity. These studies mainly attribute lower training to poaching.

We aim at testing the hypothesis that poaching indeed is the main mechanism behind the negative correlation between regional employer competition and training. We use data on apprenticeship completers and their transition to skilled employment in Germany. To identify poaching, we apply an approach developed by

* This chapter is co-authored by Thomas Zwick and has been published as Stockinger and Zwick (2017a, 2017b).

Mohrenweiser, Zwick, and Backes-Gellner (2013). Their study, however, does not consider poaching in the context of regional competition.

Our three main contributions are the following. First, in line with the literature cited above, we find that establishments in highly competitive regions train fewer apprentices. Second, we find that establishments in highly competitive regions are significantly more likely to become victims of poaching. Similar to Mohrenweiser et al. (2013), however, we show that poaching incidence is small and not systematic. In addition, firms do not reduce their training activity in response to poaching. Therefore, poaching – as observed ex-post at the establishment level – cannot explain the negative impact of regional labor market competition on training activity.

As a consequence, our third contribution is to propose alternative explanations. We find that the availability and quality of apprenticeship completers who leave their training firm (including non-poached movers) are higher in more competitive as compared to less competitive regions. In addition, it is costlier to train apprentices in competitive regions. Besides the larger and better supply of apprenticeship completers, their hiring and wage costs are relatively low in agglomerations. These differences between labor market regions prevail for all apprenticeship completers; they have rarely been discussed in the literature before, but seem to have a much more pervasive impact than regional differences regarding the small group of poached apprenticeship completers.

This chapter proceeds as follows. In the next section, we introduce crucial theoretical concepts of poaching and present the institutional setting of apprenticeship training in Germany, which we exploit to apply these concepts. In section 3.3, we review previous empirical evidence on regional labor market competition, firms' training provision, and the sparse evidence on poaching. Section 3.4 presents our data base, sampling design, and the identification of poaching. Section 3.5 analyzes apprenticeship completers' wages with regard to their mobility and regional demand-side competition. Section 3.6 presents econometric specifications and estimation results of the correlation between regional labor market competition, apprenticeship training, and poaching. Section 3.7 discusses the implications of our empirical findings for the labor market more generally, and provides alternative explanations of the regional training patterns. Section 3.8 concludes.

3.2 Theoretical and institutional background

3.2.1 Apprentice training, imperfect competition, and poaching

We regard it as poaching when worker mobility between firms is a consequence of an active attraction by the hiring firm (“raider”) and against the will of the sending firm (“victim”), a concept underlying, e.g., the theoretical work of Combes and Duranton (2006). Poaching is obviously problematic with regard to workers who have recently received training sponsored by their employer. Our focus is therefore on workers in Germany who just completed an apprenticeship. For these workers, training firms have devoted time and money, usually incurring net costs during the training period (Soskice, 1994; Mohrenweiser and Zwick, 2009; Dionisius et al., 2009). These training investments only pay off in the longer run if the apprenticeship completer stays with the training firm for some time (Acemoglu and Pischke, 1998).

We consider as potential cases of poaching only job moves immediately after apprenticeship completion, or in other words, job moves that occur before the firm’s training investments can pay off. In addition, we want to make sure that the termination of the employment relationship directly after the apprenticeship training period is involuntary from the perspective of the training firm. Therefore, we concentrate on those apprenticeship completers with the highest productivity during apprenticeship training who in addition earn more at the new employer than their peers at the training firm (Mohrenweiser et al., 2013). Such an event is obviously undesirable to the training firm, and a rational training firm should want to avoid it. We seek to ensure that observed poaching events reflect free-riding on another firm’s training investment, by only considering mobility within occupations. Thereby, we exploit one of the central features of the German apprenticeship system, the transferability of occupation-specific training contents between firms. We briefly present crucial features of the German apprenticeship system before we turn to the possibility and observability of poaching.

The “dual” apprenticeship system is the main source of occupational qualification in Germany. As of 2014, half of the German working-age population held a vocational degree – usually acquired through an apprenticeship – as their highest qualification.[22] The German apprenticeship system makes poaching perfectly viable for a number of reasons. One reason why training firms can hardly prevent poaching is that they cannot force trained workers to stay for some time after training completion. It is therefore highly unlikely that non-compete covenants are made and enforced for apprenticeship completers.[23] Furthermore, there are no legal restrictions to make job offers to apprenticeship completers and to the wages offered to them. Apprenticeship completers also do not have to reimburse training costs if they switch the employer directly after training. It is also helpful for potential rival employers that all apprenticeships in one region and occupation end on the same day (the date of the final exam held in the chambers of industry and commerce or chambers of crafts). In addition, it is known to establishments which other establishments in the region train apprentices and in which occupation. Thus, a rival firm can easily outbid a training firm by offering a higher wage for a trained worker.

Another important aspect for the viability of poaching is that training contents are transferable and apprenticeship completers can signal their acquired skills to other firms. Apprenticeships are strongly regulated by the Vocational Training Act and the occupation-specific training curricula. The Vocational Training Act specifies the duration of training, necessary equipment and staff in charge of training, and other requirements for training firms. Training curricula are published, tailor-made for each occupation, and describe minimum standards which have to be met for a successful training completion. Apprenticeships are essentially employment contracts combining on-the-job training with actual productive work. One to two days per week are spent on theoretical contents in public vocational schools. These contents are necessarily general human capital. The chambers additionally monitor the quality of practical training provided in each training establishment in their region,

[22] According to the Federal Statistical Office (StBA, 2015).

[23] Starr (2015) reports differences in non-compete covenants between US states and occupations. He uses these differences to empirically show a positive correlation between the strength of non-compete enforcement and training intensity mainly in high education and high earnings occupations.

notably by intermediate exams taking place halfway through the apprenticeship. Therefore training contents can be characterised to a large extent as general human capital within the occupational domain (Acemoglu and Pischke, 1998). Apprenticeship completers in addition receive certificates from independent bodies that also grade the final examinations (vocational schools and chambers) that signal a minimum skill level (Acemoglu and Pischke, 2000). Certificates therefore signal the individual quality relative to those apprenticeship completers trained in the same occupation (Mohrenweiser, Wydra-Sommaggio, and Zwick, 2015). Considering all these institutional features, it is easily possible to identify apprenticeship completers in all training firms who are worth poaching, with a high degree of certainty about individual quality.

Poaching can be profitable for the raider since the training has increased the worker's productivity, but the raiding firm has not contributed to the costs of training – it free-rides on the training firm's investment. Furthermore, to the extent that a raider can observe a worker's productivity relative to other workers at the training firm, it also free-rides on the revelation of apprentices' individual productivity during the training period. The raider thus extracts a rent from the training firm and also weakens a potential competitor in the product market if it can attract a highly productive apprenticeship completer for a wage not higher than his or her productivity. Therefore, raiding can be a profit-maximizing strategy. Poaching however requires raiders to compete with training firms for their apprenticeship completers. Therefore, poaching is only successful if the raider is strong enough to counter the monopsony power of the training firm. This monopsony power mainly stems from three sources.

The first source of training firms' monopsony power is their information advantage regarding apprenticeship completers' quality (Chang and Wang, 1996, Acemoglu and Pischke, 1998). This information advantage implies that apprentices who are not retained by the training firm are an adverse selection (Greenwald, 1986). Empirical evidence of such adverse selection is provided by Mohrenweiser et al. (2015). They attribute the negative selection of moving apprenticeship completers to training establishments' information advantages on soft skills. They also argue however that learning about hard skills is symmetric for training employers and their potential rivals because these skills are visible from the graded final exams. Even though movers are negatively selected on average, however, a rival firm may offer a higher wage to attract particularly well-performing apprenticeship completers.

The second source of monopsony is the costliness of regional mobility. Mobility costs give local employers market power over workers, compared to more distant employers (Manning, 2011; Benson, 2013). Note that apprenticeship completers are immobile in comparison to higher educated and older employees. This emphasizes the importance of spatial monopsony (Harhoff and Kane, 1997). Thus, the level of competition for a firm's apprenticeship completers crucially depends on the number of other employment opportunities within a certain region.

The third source of monopsony power is losses incurred by a change of occupation. The main reason for this third source of monopsony power is that occupations are an important dimension of human capital specificity (Kambourov and Manovskii, 2009; Sullivan, 2010). Manning (2003) emphasizes the importance of occupational boundaries in generating monopsony power. In Germany, the labor market for skilled employees – and for apprenticeship completers in particular – is mainly defined along occupational demarcation lines (Deißinger, 2008). As a consequence, occupation changes are associated with worse wage outcomes than employer changes within an occupation (Göggel and Zwick, 2012; Fitzenberger et al., 2015). Failing to account for the importance of occupations thus leads to mismeasurement of competition for apprenticeship completers. We therefore only consider apprenticeship completers who do not switch their occupation and define regional competition on the basis of occupational labor markets as in Benson (2013).

3.2.2 Poaching as a regional externality

Poaching has received scholarly attention particularly in regional economic theory. Combes and Duranton (2006) for example develop a model to grasp the trade-off between the benefits of locating close to other firms (input sharing, labor pooling, knowledge spillovers) and its detriments (poaching and higher wages to prevent it).

The main assumptions and implications of their model can be summarized as follows: First, co-location of firms is a necessary condition for poaching, since worker mobility is spatially bounded. Second, firms co-locate nevertheless if the benefits of co-location outweigh its costs. Several theoretical papers thus discuss firm location in the face of a poaching threat. Rotemberg and Saloner (2000) suggest that immobile workers are more likely to invest in industry-specific human capital if there is competition between regional employers – a regional monopsony could exploit workers' mobility constraints by paying wages below their marginal product. As a consequence, firms which depend on the supply of industry-specific skills have to face a certain amount of competition. However, the model explicitly rules out firm-sponsored training. Matouschek and Robert-Nicoud (2005) and Almazan et al. (2007) propose models in which firms choose a location depending on the financing of their employees' training, with isolation being the preferable choice if the firms bear a high share of the training costs. These theoretical approaches all point towards a negative relationship between local competition and firm-sponsored training activity, such as apprentice training.

Yet, there is little empirical evidence on whether training firms actually choose isolated locations (thus forgoing positive agglomeration externalities) if they are vulnerable to trained-worker poaching. In our analysis, we also cannot control whether the fear of poaching influences the complex location decision of firms. However, as Mühlemann and Wolter (2011) argue, firms are not likely to choose their location based on their training activities because the average training establishment spends just about one percent of its skilled-worker wage bill on apprentice training. Apprentice training, while important, is not the core business of any firm, and should therefore be a minor factor in firms' location choices. If anything, training establishments may adjust their training activity downwards in anticipation of a competition-induced poaching threat. However, the available measures of competition (typically, the number of other employers in the same region and industry or occupation) do not vary much over time, so anticipation effects should play a minor role. Therefore, we take firms' location as given and regard it as unlikely that reverse causality is an important problem for our analysis.

Related evidence on the positive correlation between agglomeration and labor market competition is provided by Hirsch, Jahn, and Oberfichtner (2016) for German regions. Their study approaches employer competition via an analysis of the urban wage premium and its sources. They show that the urban wage premium is to a large extent due to tougher employer competition in dense labor markets. The results of Hirsch et al. (2016) have important implications for our analysis. In particular, they point to the importance of employer competition for individual labor market outcomes, notably wages: Firms in dense regions need to offer higher wages in order to attract workers, which may offset the greater ease with which they can find suitable workers. Firms may also need to use aggressive hiring strategies, such as poaching. These findings suggest that indeed, regional competition may increase the incidence of poaching. However, there is little empirical evidence to support this claim, with the exception of studies on the link between regional competition and firms' training provision. We review these studies in the next section.

3.3 Previous empirical evidence on regional labor market competition and poaching

The empirical analysis of poaching and firm-sponsored training is largely rooted in the literature on agglomeration effects that arise from economies of scale and spatial concentration of workers and firms (Marshall, 1890). This literature emphasizes worker mobility as an important channel of agglomeration externalities (labor pooling and knowledge spillovers). For a more recent theoretical discussion of these channels, see Duranton and Puga (2004). Empirical evidence on the positive effect of regional labor market density on labor turnover is provided by Andersson and Thulin (2013), who find this effect to be even stronger for more highly qualified workers. Similar results are obtained by Mühlemann, Ryan, and Wolter (2013) and Hirsch, Jahn, and Oberfichtner (2016), who also find that wages are higher in denser (and hence more competitive) regions, in order to limit the turnover of skilled employees. Moreover, there is ample evidence that worker mobility in dense regional labor markets is concentrated within industries and occupations (Bleakley and Lin, 2012; Andini et al., 2013), that is, dense regional labor markets allow for a better skill match between workers and firms (as predicted by theories of agglomeration advantages). Against this background, and because within-industry and within-occupation mobility allows for a better transfer of skills and knowledge acquired on the job, employers in dense regional labor markets are expected to be particularly reluctant to provide training to their workers. Empirical studies indeed find a negative relationship between regional competition and firm-sponsored training (Brunello and Gambarotto, 2007; Brunello and De Paola, 2008; Mühlemann and Wolter, 2011; Andini et al., 2013).

An early empirical study on Germany, closely related to ours, was conducted by Harhoff and Kane (1997). Motivated by the stark contrast between German-style apprentice training and the absence of such a training system in the US, Harhoff and Kane (1997) identify the relatively low levels of regional mobility in Germany as a factor which favors training. They argue that workers' limited spatial scope of job search gives training firms monopsony power over their trained workers. If worker mobility across regions is low, one can use regional variation in the number of potential employers to investigate the effect of regional employer competition on firms' training provision and recruiting behavior. Accordingly, Harhoff and Kane (1997) stress that regional firm and employment densities are significantly negatively related to firms' training participation and the share of apprentices trained.

The link between regional labor market competition and firm-sponsored training has been investigated also for other European countries and the US. Using data from the UK on employer-provided training, Brunello and Gambarotto (2007) study the relationship between individual workers' training participation and regional labor market competition.[24] Brunello and Gambarotto (2007) find a negative effect of regional employment density, measured at the NUTS 2 level (groups of counties), and industrial specialization on workers' participation in and the duration of firm-sponsored training. The negative effect of employment density on training as such is very robust, and its interaction with average regional firm size has a positive

[24] An important difference between the study by Brunello and Gambarotto (2007) and our study is that any kind of employer-provided training is considered, including continued vocational training. Compared to German-style apprentice training, therefore, their data may contain a large share of relatively firm-specific training, which should be less susceptible to poaching.

effect on training. This suggests that the threat of poaching more severely decreases firms' training provision in regions characterized by smaller firms, i.e. regions where competition can be expected to be stronger. Brunello and Gambarotto (2007) do not identify poaching directly, but they show that regional density has a positive impact on voluntary job mobility, even more so for workers who have recently received training. Since part of the overall amount of voluntary job mobility could be instances of poaching, Brunello and Gambarotto (2007) argue that trained workers are more likely to get poached by other firms as regional employment density increases.

In a related firm-level study, Brunello and De Paola (2008) derive from a search and matching model that regional employment density could be negatively correlated with employer-sponsored training. Using Italian data and measuring employment density at the NUTS 3 region level, they show that the (assumed) negative poaching externalities dominate potential positive (complementary) agglomeration effects on training. That is, it is assumed that agglomeration (employment density) is a source of both positive and negative externalities. Brunello and De Paola (2008) address endogeneity concerns by instrumenting regional employment density with historical lags (from the late 19th and early 20th century), suggesting that their findings represent a causal relationship. Similar to Brunello and Gambarotto (2007), the negative training effect of employment density is found to be driven by small firms and not for firms which are part of "industrial districts," supposedly because such districts are founded on co-operative relationships between employers, which poaching would undermine. Benson (2013) stresses that US hospitals subsidize nursing schools in their region. These schools provide general training for all trainees who remain active in the nursing occupation. His argument is that low regional mobility of nurses and few attractive occupational options outside nursing provides hospitals with sufficient monopsony power to invest in general training. An important result of the study is that the incentive for subsidies increases with the market share hospitals have in the occupational labor market for nurses.

The delineation of regions used to measure competition in the above-cited studies (administrative territorial units) may not capture competition very accurately, since commuting flows may reach across administrative borders. A functional definition of labor market regions based on commuting flows is thus better suited to capture competition faced by a firm in a given location (Kosfeld and Werner, 2012). Therefore, Mühlemann and Wolter (2011), who also analyze firms' training activity with regard to regional competition, apply a definition of regional labor markets based on travel time. Their study is a close reference to our analysis because it uses data on apprenticeship training in Switzerland. The Swiss apprenticeship system is very similar to Germany's, regarding the importance of on-the-job training, generality of contents, and standardization of final exams. Mühlemann and Wolter (2011) find that the regional density of firms in the same industry is negatively related to firms' training provision. This pattern is robust, inter alia, to the inclusion of region fixed effects. The negative effect of density is found to be driven primarily by firms abstaining from training altogether, rather than training fewer apprentices. Mühlemann and Wolter (2011) also show that the observed negative training effects apply only to firms which bear net costs of training. This means that regional competition is relevant only to training firms who would lose their training investment when being poached. This finding supports the interpretation that firms' lower training efforts reflect fear of poaching.

Finally, using Swiss data, Mühlemann, Ryan, and Wolter (2013) find that the number of establishments in a regional industry increases apprentices' (and skilled workers') pay relative to unskilled workers and their turnover rate relative to the national average of workers in the same occupation. The relative-wage effect of competition reflects firms' monopsony power over apprentices: When there are fewer local competitors, there is less need to provide a wage incentive to one's own apprentices and skilled workers to stay with the firm. The positive effect of local employer competition on skilled-worker and trainee turnover therefore might be due to a higher incidence of poaching in more competitive regions.

Overall, thus, related studies find a positive effect of regional competition on trained workers' wages and turnover rates, and a negative effect on firms' training provision. The authors usually attribute the latter effect to a higher poaching risk. Despite the substantial body of evidence on the regional determinants of training activity, direct evidence on regional competition and poaching is scarce, however. We can contribute to this literature in several ways. The above-cited studies take it as given that competition affects firms' training decisions through the poaching risk, but none of them identifies poaching empirically. Applying the identification strategy for poaching proposed by Mohrenweiser et al. (2013), we investigate the relationship between employer competition and poaching directly and analyze the consequences of poaching for training efforts. We use occupational labor markets as indicator of regional competition instead of regional industries. Occupational barriers are particularly important for apprenticeship completers because their training is almost perfectly transferable across firms if they stay in the occupation. We accordingly only include apprenticeship completers who do not change their occupation. Firms are usually assigned unambiguously to one industry, but especially large firms may demand labor across a broad range of occupations, some of which are likely relevant also to other industries.[25] Therefore, a purely industry-based identification of potential competitors neglects occupations that are less typical for one's own industry, and also neglects relevant competition from other industries. Besides the poaching analysis, we also look at differences between regional labor markets with respect to decisive drivers for apprenticeship training such as the availability and quality of employer movers and their costs in comparison to the costs of own training. In the next section, we present the data and empirical study design used in our empirical analysis.

3.4 Data

3.4.1 Data sources and sampling

The data bases we use are the Institute for Employment Research's (IAB) Employee History Panel (*Beschäftigtenhistorik*; BeH) and Establishment History Panel (BHP). The BeH and the BHP are generated from public employment records, administered by the Federal Employment Agency (BA), which serve as the basis for

[25] Some of the most frequent training occupations in our sample are used in many industries, for instance clerical workers (18% of apprenticeship completers), electricians (12%), and locksmiths (11%).

social security contributions. Besides the exact beginning and end dates of employment spells, these data include gross daily wages,[26] workers' level of qualification and occupation, their employers' industry and location at the NUTS 3 level (*Kreise* or districts), and a host of other variables. Since misreporting of data is subject to pecuniary sanctions, these data are highly reliable. Moreover, the BeH cover the universe of employment subject to social security in Germany, which excludes only civil servants and the self-employed. The BeH thus contain some 80 percent of all employees in Germany, and 100 percent of apprentices in the "dual" apprenticeship system, because they are employees subject to social security contribution at their training establishments.[27]

We sample establishments and apprentices as follows. We choose all establishments which were surveyed in the IAB Establishment Panel between 2003 and 2011.[28] This panel covers all industries except public authorities and not-for-profit establishments. We drop the public sector, non-profit establishments, as well as the agricultural sector, resulting in a sample of profit-oriented business establishments. Administrative employment data at the establishment level are taken from the BHP, a yearly panel resulting from an aggregation of the employment records of the BeH. These data contain all establishments in Germany with at least one employee subject to social security contribution, measured at June 30th of that year. At the individual level, we sample all employment spells from the BeH of all persons who were employed as apprentices at any of the sample establishments at some point in time between 1999 and 2010 (1.25 million persons). We follow these persons over time, focusing on their transition from apprenticeship to regular (skilled) employment. To sort out the apprenticeship completers for whom we can potentially observe poaching, we apply the following criteria to the sampled employment spells:

First, we consider only apprenticeships that took between 700 and 1,500 days. Shorter apprenticeship periods probably indicate drop-outs, longer durations may

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[26] Wages are censored at the top. However, the censoring threshold is the social security contribution limit, and therefore censoring does not affect apprentices' wages (which are usually less than half the wage of a qualified full-time worker).

[27] See Vom Berge et al. (2013) for further information on the BeH.

[28] The IAB Establishment Panel is a large establishment survey from which we can merge additional information not contained in the administrative data.

include exam repeaters because most apprenticeships take at most 3.5 years. We omit these individuals so as to obtain homogenous groups of apprentices and be able to assess their relative individual quality. We allow for some time of employment interruption, e.g. for sickness leave during the apprenticeship period. We also require that apprenticeships start between June and December, and end between January and August, since the hiring and final exam periods usually fall into these months, respectively.

Second, since wages are an important variable for our identification of poaching, we remove extreme wage outliers, defined as apprentices who earn more than twice or less than half the average wage in their two-digit occupation and apprenticeship completion year.

Third, since we want to identify poaching of successful apprenticeship completers,[29] we need to identify a transition from apprenticeship to regular employment. To be more specific, we only include apprentices who become employed in a full-time job as their first employment after completing apprenticeship training. We ensure that the job is not an internship or otherwise non-regular kind of employment.

Fourth, we construct so-called "cells" that contain all successful apprenticeship completers within one establishment, two-digit occupation, and completion year, and keep only cells with at least two apprentices. The reason for this sample restriction is that we need to compare individual apprentices to a peer who potentially moves to another firm to identify poaching.[30]

[29] We cannot identify apprenticeship completion directly, but our restrictions regarding apprenticeship duration and transition to regular employment allows us to identify completion very plausibly.

[30] We have to ensure that changes of the establishment identifier number reflect true changes of employer. This is potentially problematic with the data at hand because the establishment identifier does not always correspond to a single autonomous establishment, but may instead identify a plant, store, office, or other kind of branch belonging to the same company, and being located in the same district. We address these issues in several ways, as discussed in the subsection "Spurious worker mobility" in the Appendix.

Fifth, we drop cells in which all apprenticeship completers leave their training establishment because we need the comparison group of retained apprenticeship completers.

Sixth, we keep only apprentices whose transition to regular employment occurs within ten days after the observed end of their apprenticeship, and who stay within the same two-digit occupation. These restrictions ensure that training establishments are not able to get a return on training investments by employing the apprenticeship completer at a low wage for some more weeks or months after training completion.

Finally, there are some obvious outliers for the first wage as a fully qualified worker after apprenticeship training. We therefore drop apprenticeship completers for whom a gross daily wage below 10 or above 500 Euros is reported.

Applying all of these conditions results in a restricted sample composed of apprenticeship completers of rather large training establishments, which might not be representative of the population of apprenticeship completers and training establishments. To check whether our results may be driven by these sample restrictions, we construct a broader so-called "baseline" sample of apprenticeship completers, their training establishments, and the destination establishments of moving apprenticeship completers. This baseline sample is drawn analogously to the poaching sample with the exception that some of the strict conditions we have to impose to identify poaching are suspended (see section "Spurious worker mobility" and Table A.3.13 in the Appendix).

3.4.2 Identification of poaching

We proceed to identify poaching of apprenticeship completers. To be counted as poaching, a job move by an apprenticeship completer should satisfy two conditions: First, the move should be undesirable and unintended from the training firm's perspective. Second, the move should be accompanied by an active effort on the part of the hiring firm, that is, there should be an incentive set by the hiring firm to attract the apprenticeship completer. As argued by Mohrenweiser et al. (2013), it is plausible to assume that the most desirable apprenticeship completers receive the highest wages within their peer group (the "cell" defined by the same establishment, training occupation, and completion year) during their last apprenticeship spell. Therefore, we can interpret an apprenticeship completer's relative wage at the end of the apprenticeship as a signal of the training employer's intention to retain him or her as a skilled employee. Furthermore, we can use apprenticeship completers' skilled entry wages to infer which job moves were likely triggered by an attractive wage offer from an external hiring firm. We take these ideas to the data as follows. Within each cell, we compare wages between those who stay with the training establishment and those who move to another employer. If a mover's wage at the end of the apprenticeship is higher than that of the best-paid stayer in the same cell, the mover satisfies our first poaching condition (P1): The training firm would have liked to keep him or her, considering that it does keep one or more comparable apprentices who earned less at the end of the apprenticeship.[31]

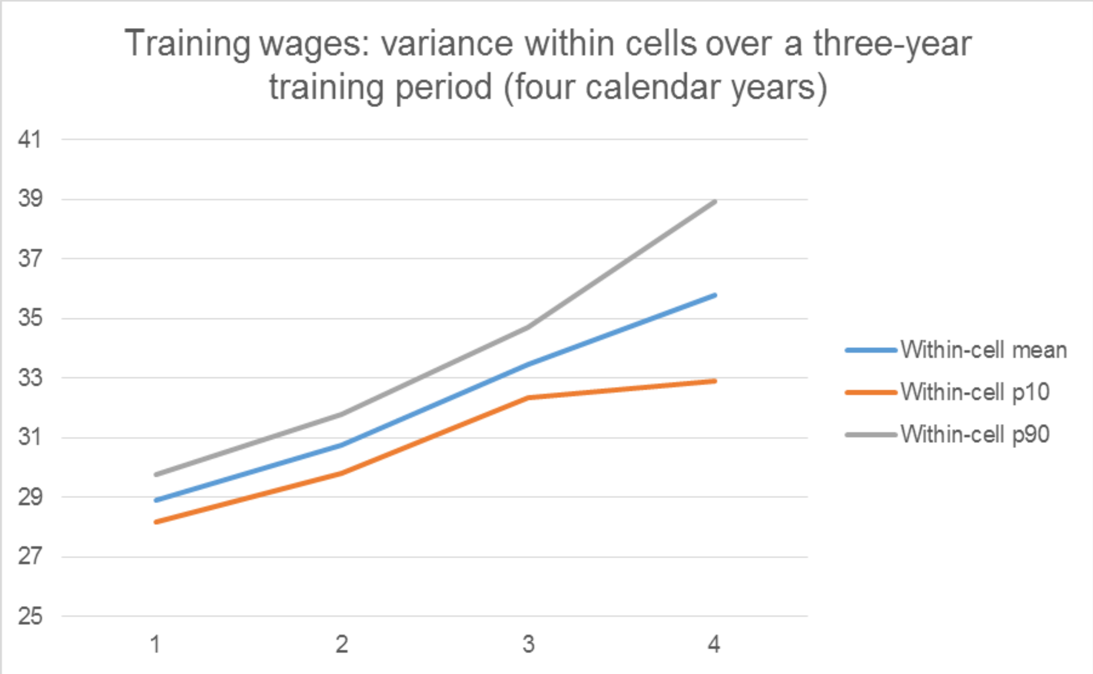
An important precondition for the validity of our first poaching condition is that wages vary across apprentices within a training establishment/completion year/training occupation cell, and that this variation is not spurious. In fact, there is substantial variance in training wages within cells, and it increases markedly towards the end of an apprenticeship, see Figure 3.1.[32] Employers therefore increasingly differentiate apprentice pay as the final exam approaches. We take the wage variance as evidence that employers signal desirability to their best apprentices by paying them relatively high wages and incentivizing them to stay after training completion. Conversely, it seems rational to pay lower wages to less desirable apprentices, in order to limit the sunk costs of training. Further evidence motivating the use of the wage position within the cell as an indicator of apprentices' desirability is presented in Mohrenweiser et al. (2013), Appendix A. In particular, wage

[31] Note that the first poaching condition does not rule out the possibility that the establishment has intended from the beginning to retain only a fraction of its apprentices. In this case, training establishments are most likely to get rid of the apprenticeship completers at the bottom of the within-cell wage distribution (see the descriptive analysis for evidence that movers' wage positions are significantly worse than stayers').

[32] A typical three-year training period lasts from September, year t, until June/July, year t+3.

positions in the final apprenticeship year have been found to correlate with external productivity indicators such as exam grades (Mohrenweiser et al., 2015).

Figure 3.1: Training wages and their variance within cells during apprenticeship training.



Data source: IAB Employment history (BEH) V09, Nuremberg 2012.

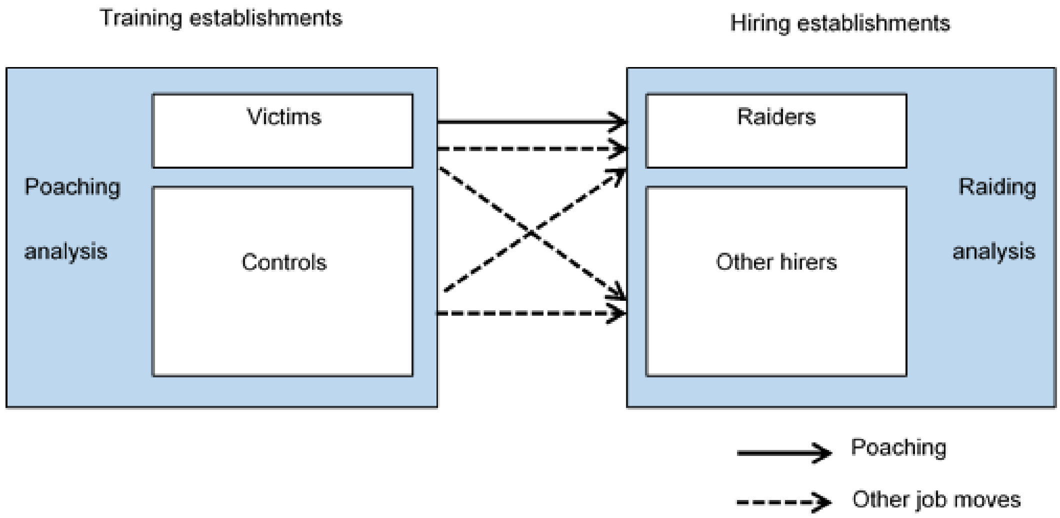
Our second condition for poaching (P2) is that, after having moved to another employer for his or her first skilled job, an apprenticeship completer receives a higher wage than any of his or her peers who stayed at the training establishment. We consider the highest wage of a stayer within a cell as the training establishment's revealed maximum willingness (or ability) to pay. If a mover receives a wage higher than this benchmark, we interpret this as a bidding competition between the training and hiring establishments which the latter has won.[33]

In combination, the two poaching conditions imply that an apprenticeship completer whom the training establishment would have liked to keep moves to another employer that offers a wage the training firm is unwilling or unable to counter. We refer to training establishments which lose at least one apprenticeship completer

[33] The hirer probably incurs a winner's curse, because the training establishment has an information advantage concerning the apprentice's productivity and might not be willing to retain the apprenticeship completer at the wage offered by the hirer.

due to poaching as "victims," the remaining training establishments that are not poaching victims are referred to as "controls." Analogously, with respect to the external hiring establishments, we refer to the destinations of poached apprenticeship completers as "raiders," and to all other destinations of moving apprenticeship completers from the victims and controls employers as "other hirers." Figure 3.2 provides an overview of the training and hiring establishment samples and the apprenticeship completer movements between establishments.[34] In the baseline sample, we distinguish training and hiring establishments, but not victims and controls, respectively raiders and other hirers.

Figure 3.2: Employee flows between training and hiring establishments



In both, the "poaching" and the baseline sample, we drop extreme outliers in terms of their apprentice share in total employment, i.e. observations above the 99th percentile. These observations might be parts of a firm that are devoted exclusively to apprentice training (training facilities). Furthermore, we delete the top percentile in terms of employment growth, because these firms might pursue exceptionally aggressive hiring strategies and bias the incidence of poaching upwards.

3.4.3 Samples and descriptive statistics

Table 3.1 summarizes the two samples used in the analysis. Our poaching sample comprises 21,416 training establishments with 134,602 apprenticeship completers.

[34] By definition, an establishment cannot be a poaching victim and a control establishment at the same time, but it can be a poaching victim in one year and a control establishment in another. The same rule applies to raiders and other hirers.

Some eight percent of apprenticeship completers leave their training establishment within ten days after training completion to start working for one of 5,811 external hiring establishments. The larger baseline sample necessarily yields a higher share of immediate movers, at 11.4 percent. Otherwise, the characteristics of apprenticeship completers do not differ substantially between the two samples, as reported in Figure A.3.1 in the Appendix. In the poaching sample, 0.6 percent of apprenticeship completers (eight percent of all movers) are poached. Both poaching conditions (P1 and P2) contribute to similar amounts to poaching, as shown by the percentage of movers fulfilling either of the two conditions; the measured poaching incidence is thus not driven primarily by one of the two conditions. The share of "raiders" in all external hiring establishments is about eleven percent, similar to the share of poached individuals of eight percent. Tables A.3.2 and A.3.3 in the Appendix provide summary statistics at the establishment level for the poaching and baseline samples, respectively.

Our central empirical question is whether apprenticeship completers' employer moves, including poaching, are related to regional labor market competition. Figure A.3.1 in the Appendix shows the geographical pattern of the mover share – the inverse of the retention rate – for the 141 German labor market regions.[35] The map suggests that overall, larger, more agglomerated labor market regions (e.g. the areas around Berlin, Munich, Stuttgart, and Frankfurt) tend to have higher shares of movers among all apprenticeship completers, but the relationship between agglomeration and turnover is not very pronounced. Figure A.3.2 displays the share of poached movers in all apprenticeship completers. This variable does not seem to correlate with agglomeration. However, agglomeration is not synonymous for competition, and so this merely descriptive look might not capture the potential correlation of poaching with regional competition. Furthermore, our poaching identification is based on relative earnings of apprenticeship completers and wage differences between those who stay with the training firm and those who move to another

[35] Note that our sample contains at least 12 apprenticeship completers and at least 5 training establishments for every labor market regions, and roughly 2,000 completers and 300 establishments on the region average. Thus, the sample can be considered fairly representative at the regional level.

employer. Previous studies indicate that besides employer mobility, also earnings of trained workers are affected by regional labor market competition (Mühlemann et al., 2013). We therefore proceed to a deeper analysis of apprenticeship completers' wages and their job mobility and their correlation with regional competition before we present our poaching analysis.

Table 3.1: Descriptive statistics.

Poaching sample	**N**	**n**	**Share of Total (N)**	**Share of Total (n)**
Apprenticeship completers (Total)	134,602	134,581		
- Stayers	124,475	124,458	0.925	0.925
- Movers	10,127	10,127	0.075	0.075
- Poached movers	855	855	0.006	0.006
- Movers satisfying P1	2,643	2,643	0.02	0.02
- Movers satisfying P2	2,363	2,363	0.018	0.018
Training establishments (Total)	21,416	4,639		
- Victims	559	409	0.026	0.088
- Controls	20,857	4,230	0.974	0.912
External hiring establishments (Total)	5,811	3,519		
- Raiders	623	516	0.107	0.147
- Other hirers	5,188	3,003	0.893	0.853
All establishments	27,039	7,926		
Baseline sample				
Apprenticeship completers (Total)	196,697	196,066		
- Stayers	174,282	173,814	0.886	0.887
- Movers	22,415	22,396	0.114	0.114
Training establishments	58,632	21,033		
External hiring establishments	14,723	10,624		
All establishments	72,402	30,084		

N is the number of observations and n is the number of unique individuals or establishments. Sum of n(stayers) and n(movers) may exceed n(completers) because of multiply observed completers who both stay and move. Sums of training and hiring establishments may exceed number of all establishments because of overlap. Data source: BEH V.09 and IAB Establishment History Panel (BHP) 7510 v1, Nuremberg 2012.

3.5 Regional wage analysis

Analogously to previous studies (Fitzenberger et al., 2015; Mohrenweiser et al., 2015), we expect to find that non-retained apprenticeship completers (movers) earn lower wages than those who are retained by their training establishment, reflecting an adverse selection of movers on average. Besides training wages,[36] we also consider apprenticeship completers' relative wages, that is, their deviation from the mean of the cell (training establishment, training occupation, and cohort). By referring to peers in the same cell, the relative wage is a direct measure of individual relative productivity or quality from the viewpoint of the training firm. We argue that the relative productivity of an apprenticeship completer is a good indicator of the attractiveness of the trained employee to be retained by the training firm.

We report results from regressions of these wage measures on a dummy for movers and occupation fixed effects in Table 3.2. We cluster standard errors at the level of regional occupational labor markets because the relevant labor markets for apprentices are bounded spatially and by occupations. Since we do not need the poaching variable in this step of the analysis, we report results using the broader baseline sample in the main text and results based on the poaching sample in the Appendix. The results suggest a significantly negative correlation between moving (not being retained) and both the absolute training wage and the within-cell wage position. For the absolute wage (columns 1-3), adding basic controls (establishment size, regional employment density) does not affect the estimate, which implies that movers' training wages are some two percent lower than stayers' on average. The relative wage (column 4) is already cleared of confounding factors at the establishment and other higher levels, and apprentices within cells are virtually identical with respect to age, education, and any other individual-level characteristics. This explains the extremely low explanatory power of the regression.[37]

[36] Unless otherwise specified, "training wages" refers to wages during the last spell of an apprenticeship, i.e. at training completion.

[37] The relative wage is not transformed into logs because its mean and median are obviously close to zero.

Table 3.2: Wage statistics of movers compared to stayers, baseline sample.

	(1) log training wage	(2) log training wage	(3) log training wage	(4) Training wage rel. to cell mean
Mover	-0.024***	-0.018***	-0.021***	-0.228***
	(-3.85)	(-2.98)	(-3.46)	(-3.14)
log full-time employment, estab.		0.051***	0.047***	
		(22.35)	(19.70)	
log empl. density, region			0.033***	
			(6.66)	
Constant	3.279***	3.123***	2.983***	7.53e-08
	(42.88)	(43.24)	(40.55)	(.)
Observations	196697	196500	196500	196697
Adjusted R^2	0.251	0.330	0.338	-0.000

t statistics in parentheses. All estimations include 2-digit occupation fixed effects. Standard errors clustered at region-occupation level (labor market regions, 2-digit occupations). * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

We obtain similar results for the narrower poaching sample, see Appendix Figure A.3.4. Our findings thus suggest that movers are adversely selected on average. These findings suggest that training establishments succeed in retaining their best apprenticeship completers. However, the findings may also indicate that training establishments' monopsony power is limited: They need to pay competitive wages to incentivize their more productive completers to stay, resulting in high average wages for stayers. Furthermore, movers may incur a wage penalty due to statistical discrimination, as they are negatively selected on average (Schönberg, 2007). In combination, both factors imply that stayers can demand higher wages than movers also after transition into a skilled job.[38]

The findings above imply that apprenticeship completers' wages reflect competition in the labor market, an important precondition for the validity of our identification of poaching. We can test this implication more explicitly by investigating the relationship between wages and regional competition. We measure regional competition as the log density (number per square kilometer) of establishments in the regional occupational labor market, that is, establishments within the same labor

[38] We do not report skilled-wage comparisons between stayers and movers because movers' destination establishments differ systematically from the training establishments (notably, the latter are much larger, due to requirements of the poaching identification). We found that controlling for establishment size and other available variables is insufficient to reduce the implied bias to an acceptable level.

market region with at least one employee in relevant occupations.[39] We leave aside the individual wage position, which is informative only with regard to heterogeneity within training cells (i.e. heterogeneity at the individual level). Instead, we consider the standard deviation (SD) of training wages at the cell level, as an indicator of wage differentiation within training establishments.[40] Additionally, we compute the difference between skilled wage (the wage earned directly after transition into skilled employment) and training wage. This difference can be interpreted as the quality-adjusted wage of an apprenticeship completer and therefore as a proxy of his or her effective wage costs. This assumes that training wages contain information on the value of the apprentice from several sources. First, large and prestigious training firms may pay more for all apprentices. Second, in occupations and during phases of the business cycle in which it is difficult to attract good apprentices, training firms might offer a bonus to the collective bargaining training wage, and finally we have seen above that training firms differentiate between apprentices in the same cohort and occupation according to their relative quality.

Results from bivariate regressions are presented in Table 3.3. Training wages and their standard deviation within cells (establishment, occupation, cohort) are higher in highly competitive regions. These findings are in line with the hypothesis that training wages are used by training firms as signals for their intention to retain apprentices (Mühlemann et al., 2013).[41] Furthermore, training establishments differentiate wages more strongly within training occupations if regional competition for apprenticeship completers in the occupation is stronger. A plausible interpretation of this finding is that firms respond to regional competition by incentivizing their

[39] More precisely, regional competition is measured for training establishments on the basis of all training occupations, and for hiring establishments, on the basis of all observed hiring occupations (we do not observe all external apprenticeship completer hirings for these establishments). Each occupation is weighted by its share in all trained (respectively observed hired) apprenticeship completers. Due to the definition of labor market regions, which are relatively homogenous in geographic size, it is virtually irrelevant whether we measure competition as the number or the density of regional competitors.

[40] Training wages (and their SD) are regressed on the competition faced by the training establishment; first-job wages and the wage difference between first job and training are regressed on the competition faced by the hiring establishment (which may be different from the training establishment).

[41] See Table 3 in Mühlemann, Ryan, and Wolter (2013).

best apprentices to stay, while paying relatively low wages to less desirable apprentices (whose wages become sunk costs in the case of non-retention). The wage difference between training and skilled work is, not surprisingly, also positively correlated with regional competition. Basic controls (establishment size and regional employment density) are presented in Tables A5 through A7 in the Appendix, leaving the results largely unchanged.[42] Using the smaller poaching sample, we obtain very similar results (results available on request).

Table 3.3. Wages and regional competition, baseline sample.

	(1) log training wage	(2) Within-cell SD of training wages	(3) log wage diff. job-training
log firm density reg.-occ.	0.042***	0.155***	0.040***
	(14.86)	(12.24)	(11.68)
Constant	3.394***	0.475***	3.653***
	(45.84)	(11.47)	(30.41)
Observations	196697	83510	195752
Adjusted R^2	0.275	0.055	0.142

t statistics in parentheses. The regression in column 2 contains one observation per training cell (establishment, occupation, year). All estimations include 2-digit occupation fixed effects. Standard errors clustered at region-occupation level (labor market regions, 2-digit occupations). * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

The results of our wage analysis are thus in line with previous research in that non-retained apprenticeship completers are negatively selected on average. Furthermore, we find that training establishments respond to regional competition by setting and differentiating wages strategically. Higher wages (and wage differentiation) during the last training period in regions with higher labor market competition lend additional credibility to our wage-based poaching identification. The patterns of wages found above can be both cause and consequence of apprenticeship completer mobility and poaching. We analyze the training activity of establishments and its possible causes, in the following sections.

[42] Both, regional employment and regional employment density are highly correlated with our competition indicator. At 0.41, the correlation coefficient is lower for the former, which we therefore prefer.

3.6 The impact of regional competition on training, retention, and poaching

The main empirical part of this paper comprises a set of analyses at the establishment level, the level at which decisions on apprentice training, retention, and poaching are made. First, we replicate the analyses of previous papers which found that regional competition has a negative effect on firms' training provision. We then analyze the effect of regional competition on the mobility of apprenticeship completers and on poaching. According to theoretical predictions and empirical findings in the literature (Blatter et al., 2016), we expect that higher labor market competition is associated with a lower retention rate after apprenticeship training and a higher poaching incidence. Furthermore, we analyze whether poaching victims respond to poaching by reducing their training activity. We address each of these questions in a separate subsection.

3.6.1 Establishments' training provision

First, we investigate training establishments' (victims' and controls') training activity with regard to regional labor market competition. We closely follow the specification of Mühlemann and Wolter (2011) and other related studies. Measuring an establishment's training activity by the (log) number of apprentices trained, we estimate the following specification for establishments' training provision:

$$\begin{aligned} &\ln(apprentices)_{it} \\ &= \beta_0 + \beta_1 comp_{\mathrm{ort}} + \beta_2 \ln L_{it} + \beta_3 share\_midqual_{it} \\ &+ \beta_4 \left(\frac{L_{it} - L_{it-1}}{L_{it-1}}\right) + \beta_5 \ln(med\_wage)_{it} + \beta_6 \ln L_{rt} \\ &+ \beta_7 \ln(empl\_dens)_{rt} + \mu_o + \delta_j + \vartheta_t + \theta_r + u_{it}. \end{aligned} \tag{3.1}$$

We thus regress the training activity of establishment *i* in year *t* on the degree of competition in its regional occupational labor market, $comp_{\mathrm{ort}}$, measured as the log density (number per square km) of establishments in the same regional occupational labor market.[43]

[43] Note that the index o may represent more than one training occupation per employer.

We include as control variables log labor (full-time employment)[44] and the share of medium-qualified workers, which is a good proxy of the share of employees who have completed an apprenticeship. Both variables are basic controls for the establishment's demand for apprentices. The main insight from Mohrenweiser et al. (2013) is that establishments' training and retention behavior is determined, inter alia, by temporary up- and downturns. It is therefore important to control for the establishment's employment growth rate. Further controls include the establishment's log median daily wage of full-time workers and the size (log labor) and density (log of employment per square kilometer) of the labor market region *r*. These controls should capture macro-regional effects on training activity that are not due to regional competition, but to other regional externalities (e.g. the positive and negative effects of agglomeration).

We also include fixed effects for training occupations (*o*), industries (*j*), years (*t*), and labor market regions (*r*).[45] Occupation fixed effects in particular are crucial to capture structural differences in supply and demand in the apprenticeship-completer labor market, since apprenticeship training contents are strongly occupation-specific. Ideally, standard errors should be clustered at the level of regional occupational labor markets, the level where competition (the key explanatory variable) varies. This is impossible, however, since there can be more than one training occupation per establishment. We therefore cluster standard errors at the level of regional two-digit industries. Throughout this section, we report results using the narrow poaching sample, since we can perform the core analyses on poaching (section 3.6.3) only for this sample. Analogous results for the baseline sample, where applicable, are reported as robustness checks in the Appendix.

The estimation results, presented in Table 3.4, reveal a significant negative effect of regional competition on establishments' training provision. All reported estimations include occupation fixed effects, which previous studies and our descriptive

[44] We only count full-time employees because the data do not contain working time for part-time employees. Note that apprentices are not full-time employees and hence not included in L.
[45] We identify occupations and industries at the two-digit level, respectively.

analysis have found to be crucial controls. Industry and year fixed effects are included in column 2. They add little to the overall explanatory power of the model. The same can be said of labor market region fixed effects, which are included in column 3.

Table 3.4: Impact of regional competition on apprenticeship training, poaching sample.

	(1) log apprentices	(2) log apprentices	(3) log apprentices
log estab. density, region-occupation	-.024**	-.019*	-.021**
	(-2.25)	(-1.873)	(-2.006)
log labor (full-time)	.612***	.633***	.637***
	(65.11)	(65.52)	(65.47)
Share mid-qual. employees	.208***	.165***	.168***
	(3.014)	(3.127)	(3.137)
Employment growth rate	.066	.070*	.066*
	(1.621)	(1.751)	(1.658)
log median daily wage	-.002	-.059	-.072*
	(-.106)	(-1.63)	(-1.692)
log employment LM region	.006	8.5e-05	.509***
	(.406)	(.007)	(2.729)
log empl. density LM region	-.014	-.016	.038
	(-.71)	(-.900)	(.363)
Constant	-.984***	-.0428	-6.604***
	(-5.49)	(-.184)	(-3.098)
Observations	21416	21416	21416
Adjusted R^2	.702	.723	.732

t statistics in parentheses. All estimations include 2-digit occupation fixed effects. Columns 2-3 includes 2-digit industry and year fixed effects. Column 3 includes labor market region fixed effects. Standard errors clustered at the region-industry level (labor market regions, 2-digit industries). * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

Our findings are qualitatively in line with previous studies. They can best be compared to Mühlemann and Wolter (2011), who use data from Switzerland, a country whose apprenticeship system is very similar to Germany's. Mühlemann and Wolter (2011) estimate the elasticity of apprentice employment with respect to the density of regional competition at around -0.2, about ten times our estimate. This large difference can be mainly explained by differences between their estimation sample and ours. In particular, 70 percent of their sample establishments do not employ a single apprentice; 99 percent have at most six apprentices. In contrast, our estimation sample contains 100 percent training establishments that are relatively large and have 34 apprentices on average (some establishments even have more than 1,000 apprentices).[46]

To analyze the impact of the specific sample we use on the regression results, we re-run the regressions with our larger baseline sample, see Table A.3.8 in the Appendix. This sample also contains relatively large training firms, but also includes more small training firms than the poaching sample. The baseline sample yields a highly statistically significant elasticity of about -.05. We therefore regard our results as broadly comparable to Mühlemann and Wolter's (2011).

3.6.2 Analysis of retention

In a second step, we investigate whether regional competition has a negative effect on the retention of apprenticeship completers by their training establishment. We use the same control variables as in equation (3.1), plus the log number of apprentices, and consider the number of movers directly after apprenticeship completion as the dependent variable.[47] We thus estimate:

$$\begin{gathered}\ln(movers)_{it} = \beta_0 + \beta_1 comp_{\mathrm{ort}} + \beta_2 \ln(apprentices)_{it} + \\ controls + \mu_o + \delta_j + \vartheta_t + \theta_r + u_{it}.\end{gathered} \tag{3.2}$$

The estimation results are displayed in Table 3.5, where the different specifications are analogous to the previous subsection. As expected, we find a positive elasticity for regional competition. Furthermore, the elasticity (in absolute values) is in the same order of magnitude as the negative effect of competition on training. Thus, the negative training effect of regional competition is roughly proportional to its negative retention effect. We run the same regression on the baseline sample (Appendix Table A.3.9), and obtain very similar results.[48]

[46] These numbers deviate from Table A2, which also includes the hiring establishments (raiders and other hirers).

[47] Since the number of movers is zero for a large number of observations, we actually use ln(movers + 1). This modification has a negligible effect on the results: The correlation between ln(movers) and ln(movers + 1) is 0.994.

[48] One might object that the number of movers (despite the log transformation corrected for zeros) is not an ideal dependent variable for an OLS regression due to the large number of zeros. Thus, we alternatively estimated a Probit model with the dependent variable being a dummy for having at least one mover among all apprenticeship completers (estimation results available on request). These estimations confirm that there is a strong and significant negative relationship between regional competition and retention.

Table 3.5: Impact of regional competition on retention of apprenticeship completers, poaching sample.

	(1) log movers	(2) log movers	(3) log movers
log estab. density, region-occupation	.036***	.031***	.031***
	(3.03)	(2.77)	(2.99)
log apprentices	.169***	.176***	.172***
	(11.4)	(11.2)	(11.1)
log labor (full-time)	-.058***	-.066***	-.067***
	(-6.16)	(-6.61)	(-6.38)
Share mid-qual. employees	-.017	-.021*	-.022*
	(-1.41)	(-1.68)	(-1.71)
Employment growth rate	-.183***	-.17***	-.174***
	(-6.45)	(-5.91)	(-6.06)
log median daily wage	-6.0e-03	.054**	.076***
	(-.428)	(2.25)	(2.71)
log employment LM region	.025**	.018*	.15
	(2.37)	(1.85)	(1.11)
log empl. density LM region	-.029*	-.028*	-.124*
	(-1.8)	(-1.82)	(-1.7)
Constant	-.216*	-.496***	-1.8
	(-1.84)	(-2.89)	(-1.14)
Observations	21416	21416	21416
Adjusted R^2	.177	.191	.214

t statistics in parentheses. All estimations include 2-digit occupation fixed effects. Columns 2-3 includes 2-digit industry and year fixed effects. Column 3 includes labor market region fixed effects. Standard errors clustered at the region-industry level (labor market regions, 2-digit industries). * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

3.6.3 Poaching analysis

Our ex-post identification of poaching yields, for each training establishment observation, a number of poaching incidents which can be used to estimate the relationship between an establishment's regional competition and poaching. The poaching variable is non-negative, integer-valued, and small but mostly zero (97 percent). Therefore, one might consider estimating a count data model. However, count data models are inappropriate if the dependent variable contains a large number of zeros. For the choice of an estimator, furthermore, it is crucial to decide whether the data-generating process can be seen as a two-stage decision, and whether the outcome at the second stage (the number of poached apprenticeship completers, given it is positive) is of interest independently of the first stage (number of poached apprenticeship completers positive versus zero). In the current case, there does not seem to be such a decision process, since of course establishments do not choose to get poached. Instead, the fact whether a training firm experiences poaching or not and the count of poached training completers both represent the same kind of event measured on different scales. Given these preconditions, we decide not to use a count data or hurdle (two-stage) model. Instead, a binary dependent variable indicating whether there is at least one poaching appears as a conservative choice for the dependent variable. We therefore choose to estimate a Probit model.

We follow the approach of Mohrenweiser et al. (2013) and estimate the probability that establishment *i* becomes a poaching victim in year *t* as follows:

$$\mathrm{P}(victim)_{it} = \beta_0 + \beta_1 \ln(apprentices)_{it} + \beta_2 comp_{\mathrm{ort}} + controls + \mu_o + \delta_j + \vartheta_t + \theta_r + u_{it}. \quad (3.3)$$

As in equation (3.2), we control for the log number of apprentices, which raises the probability of observing one or more incidents of poaching. The other control variables are as above.[49]

Table 3.6 provides the average marginal effects (reported as elasticities) from the Probit estimations.[50] A number of observations are dropped from all estimations because some of the included fixed effects perfectly predict the outcome. Across the three different specifications, we find that indeed, regional competition significantly increases training establishments' risk of having at least one apprenticeship completer poached by a competitor. This finding is in line with expectations arising from previous studies,[51] which attribute the negative effect of regional competition on training to an increased risk of poaching. Converted into absolute values, the estimated poaching elasticity of about 0.4 implies that a 100 percent increase in regional competition increases P(victim) by about one percentage point, or 38 percent of the sample mean (the share of poaching victims, 2.6 percent). Since the

[49] Since the dependent variable is only defined for establishments which fulfill our potential poaching conditions (see section 3.4.1), we can apply this specification only to the estimation sample of victims and controls (poaching sample) but not to the baseline sample.

[50] We obtain very similar results when estimating a linear probability model (results available on request).

[51] Brunello and Gambarotto (2007), Brunello and De Paola (2008), Mühlemann and Wolter (2011).

dependent variable represents a probability, we cannot directly compare this estimate to the effects found in the estimations for training (section 3.6.1) and retention (section 3.6.2), to assess the importance of poaching (as observed ex-post) for establishments' training decisions. Instead, we investigate the consequences of poaching at the level of individual establishments in the next subsection.

Table 3.6: Impact of regional competition on poaching, poaching sample.

	(1) Poaching victim	(2) Poaching victim	(3) Poaching victim
log estab. density, region-occupation	.426***	.396***	.433***
	(3.93)	(3.55)	(3.61)
log apprentices	.835***	.891***	.898***
	(6.42)	(6.83)	(6.91)
log labor (full-time)	-.558***	-.649***	-.667***
	(-5.64)	(-6.23)	(-6.18)
Share mid-qual. employees	-.222	-.261*	-.249*
	(-1.51)	(-1.81)	(-1.77)
Employment growth rate	-2.14***	-2.11***	-2.21***
	(-2.81)	(-2.77)	(-2.83)
log median daily wage	.041	.794**	.997***
	(.242)	(2.52)	(2.82)
log employment LM region	.203**	.165*	1.96
	(2.25)	(1.87)	(.754)
log empl. density LM region	-.448***	-.472***	-.435
	(-3.18)	(-3.3)	(-.243)
Observations	20758	20619	19174
Pseudo R^2	0.084	0.103	0.132
AIC	4815.3	4809.5	4802.1
BIC	5252.0	5642.5	6453.0

t statistics in parentheses. Average marginal effects (elasticities) after Probit. All estimations include 2-digit occupation fixed effects. Columns 2-3 includes 2-digit industry and year fixed effects. Column 3 includes labor market region fixed effects. Standard errors clustered at the region-industry level (labor market regions, 2-digit industries). * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

We would like to point out that we obtain the same results when measuring competition not for regional occupational labor markets, but regional two-digit industries analogously to the just-cited studies on the consequences of regional competition for training. The estimated elasticity for establishment density in the regional two-digit industry is around 0.39 and highly significant (results available on request). Overall, however, the occupation-based competition measure yields more robust estimates than the industry-based measure.

In the Appendix section “Raiding analysis”, we present the analogous raiding analysis that is a natural robustness check for the poaching regression. We confirm the positive effect of regional competition on raiding. This finding is not a surprise, however, considering that 70 percent of poached apprenticeship completers stay within their labor market region (see Table A.3.14), and that training and hiring establishments are likely to have similar occupational profiles. As a consequence, the level of regional competition is similar for training and hiring establishments.

3.6.4 Training response to poaching

The previous subsections have established that regional competition negatively affects establishments’ apprentice training and retention of apprenticeship completers, and that it has a positive effect on the incidence of poaching. However, it remains to be shown whether poaching victims react to poaching by training fewer apprentices. A reduction of training after poaching would suggest a causal link running from regional competition to poaching and further to lower apprentice training of the affected training establishments.

Given the results of Mohrenweiser et al. (2013), whose analysis of the training response we partly replicate in this section, we expect that poaching victims do not adjust their training activities downward in response to past poaching. Figures A3 through A5 in the Appendix display changes of relevant variables during the period from three years before until three years after a poaching event. Figure A.3.3 shows that the share of apprentices is somewhat (but not significantly) higher during the poaching year than in the years before and after. In contrast, the retention rate (Figure A.3.4) and employment (Figure A.3.5) drop significantly in the year of poaching.

These patterns suggest that poaching occurs during temporary downturns of the training employer, as found by Mohrenweiser et al. (2013). These downturns force training establishments to lay off workers and not to retain apprenticeship completers. The figures indicate that training establishments refrain from laying off apprentices in general, but they concentrate on laying off apprenticeship completers, instead. Firing apprentices is legally extremely hard once the probationary period (at most four months) is over.[52] In addition, given the low wages apprentices receive, firing them would not reduce labor costs much. Furthermore, apprentices' employment contracts expire on the day the final exam is passed. It is therefore relatively cheap and socially accepted to get rid of apprentices once they have completed their training. As a result, the apprentice share increases slightly in that year. However, poaching does not seem to change establishments' usual training behavior because the retention rate and the apprenticeship share quickly return to their normal levels, in tandem with the increase in employment, in the years after poaching.

We also confirm the multivariate results of Mohrenweiser et al. (2013) that poaching victims do not reduce their training activity after poaching (see Table 3.7). The dependent variable in these estimations is the share of apprentice hires in all employees (columns 1 and 2) respectively the log growth rate of the stock of apprentices (ln(apprentices in year t) minus ln(apprentices in year t-1); columns 3 and 4). The estimates again suggest that training effort does not change in the years following the poaching incident.

[52] The Federal Vocational Training Act (Berufsbildungsgesetz) contains a number of additional requirements, as compared to the Employment Protection Law (Kündigungsschutzgesetz), for laying off apprentices after the probationary period. In particular, apprentices may only be laid off for "important reasons" such as theft, severe misconduct in the workplace, etc.

Table 3.7: Reaction of poaching victims in terms of training activity, poaching sample.

	(1)	(2)	(3)	(4)
	Share apprentice hires		log growth apprentices	
L.victim	0.001	0.000	-0.001	-0.009
	(.844)	(.139)	(-.065)	(-.646)
L2.victim	-0.000	-0.000	-0.014	-0.009
	(-.288)	(-.379)	(-.854)	(-.667)
L3.victim	-0.000	0.000	-0.003	0.006
	(-.172)	(.273)	(-.197)	(.413)
log apprentices		0.020***		0.457***
		(43.69)		(40.23)
log labor (full-time)		-0.023***		-0.269***
		(-23.67)		(-11.5)
Share mid-qual. employees		-0.002***		-0.012*
		(-4.913)		(-1.691)
Employment growth rate		0.021***		0.679***
		(15.77)		(21.05)
log median daily wage		0.001		0.002
		(.586)		(.036)
log employment LM region		0.016**		-0.092
		(2.105)		(-.512)
log empl. density LM region		-0.009**		-0.082
		(-2.014)		(-.762)
Constant	0.026***	-0.065	-0.010***	1.606
	(247.2)	(-.760)	(-4.046)	(.786)
Observations	6644	6644	6642	6642
Adjusted R^2	-.284	.128	-.285	.11

t statistics in parentheses. Fixed effects (within-establishment) estimates. Columns 2 and 4 include year fixed effects. * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

3.7 Implications for the economics of apprentice training

In our empirical analysis, we confirm previous literature that regional labor market competition is negatively correlated with establishments' apprentice training activity, and positively with apprenticeship completers' job mobility and wages. We complement this finding with new evidence that there is a positive effect of regional competition on actual poaching. With less than three percent of apprenticeship completers being affected, poaching is however a rare and not systematic phenomenon. In addition, poaching does not appear to have any effect on the apprentice training strategy of the victims. Thus, it does not seem to be the actual poaching incidence which discourages firms in competitive regional labor markets from training apprentices. In this section, we add to the few papers that discuss alternative channels that explain the negative training effect of regional competition, in addition to the threat of poaching.

First, not only realized poaching might influence firms' training strategies, but also the perceived threat of poaching. Previous studies have only discussed this threat (or probability) of poaching, which is supposed to increase with regional labor market competition. Naturally, the mere threat of poaching cannot be observed in empirical data, which is why we use an ex-post definition of poaching. We might however assume that actual incidents of poaching influence the perception of training firms about the poaching threat. In this context, the market for apprenticeship completers can be regarded as a contestable market (for a survey of the relevant literature, see Brock, 1983). That is, employers can enter this market at no cost, whereas training establishments have incurred sunk costs for their training investment. The market for apprenticeship completers clearly is contestable in this sense. Overall, thus, the threat of poaching might have a stronger effect on training activity of firms in competitive regions than actual poaching.

The second important factor that differs between regions is the lower general retention rate of apprenticeship completers in more competitive regions, see section 3.6.2. That is, establishments in highly competitive regions might train fewer apprentices because they expect to retain a lower share of them, whether due to poaching or other kinds of outflows. A theoretical foundation for this relationship is provided, for example, by the model developed by Smits and Stromback (2001). This model suggests that the incentive of firms to invest in apprenticeship training is positively influenced, inter alia, by the retention rate of apprenticeship completers.[53]

Smits and Stromback's (2001) model furthermore emphasizes the importance of apprentices' productivity (and the degree to which it can be exploited through wage

[53] The profit function from apprenticeship training in Smits and Stromback (2001) is defined as $\Pi = -w1 - c(h) + (1-q)(h-w2)$, with Π profits, w1 apprentice earnings, c apprenticeship costs, q retention probability, h productivity of apprentices, and w2 skilled wage. A broader discussion of the profitability of apprenticeships, focusing on Germany and Switzerland, is provided by Mühlemann and Wolter (2014), who emphasize the role of national institutions and firm characteristics, rather than the regional environment.

compression) for training profitability. Regarding the importance of regional competition, however, what appears crucial for firms' training decisions is not apprentices' (and apprenticeship completers') productivity per se, but the productivity of apprentices who are retained compared to those who move elsewhere. We do not have data on apprentices' productivity. We can, however, use wage data to learn about the relative productivity of movers and stayers, assuming that training wages reflect productivity. Although movers are generally negatively selected (see section 3.5 and previous studies), this negative selection might be less pronounced in more competitive regional labor markets, where apprenticeship completers are more likely to find a better job match by moving to a competitor and where retention rates are lower. Previous empirical studies suggest that indeed, there is a positive relationship between regional competition and the individual productivity of moving workers. For instance, Andersson and Thulin (2013) find that regional density increases the mobility of higher qualified workers more than the mobility of lower qualification groups. We therefore investigate whether there are productivity differences between moving apprenticeship completers in more and less competitive regions.

Taking training wages as a productivity signal, we regress wages on a dummy for movers, the regional competition indicator, and an interaction term of the two (see Table 3.8, column 1).[54] We find that movers earn higher training wages in highly competitive regions than in less competitive regions, suggesting they are less negatively selected where regional competition is stronger. In column 2, we control for the size of the training establishment and the training region, as well as the average wage in the training region.[55] Establishment size in particular is an important driver of monopsony power and hence, wages (Manning, 2011). These additional controls leave our results almost unchanged.

[54] We use the baseline sample because the poaching variable is not needed here. We obtain qualitatively the same results using the poaching sample, see Table A10 in the Appendix.

[55] The average wage in the training region is defined as the average of establishment-level median wages.

Table 3.8. Training wage differences between retained and moving apprenticeship completers by regional competition, baseline sample.

	(1) log training wage	(2) log training wage	(3) log training wage
Mover	-0.023***	-0.018***	-0.014**
	(-4.07)	(-3.23)	(-2.11)
log firm density reg.-occ.	0.041***	0.011***	0.012***
	(14.99)	(4.25)	(4.37)
Mover*log firm density reg.-occ.	0.011***	0.009**	0.016***
	(2.61)	(2.25)	(3.80)
log full-time employment, training estab.		0.044***	0.044***
		(17.28)	(17.89)
log employment, training region		-0.014***	-0.014***
		(-3.06)	(-3.03)
log mean wage, training region		0.348***	0.346***
		(9.16)	(9.03)
Constant	3.392***	1.849***	1.855***
	(45.79)	(10.84)	(10.91)
Observations	196697	196500	190349
Adjusted R^2	0.276	0.350	0.349

t statistics in parentheses. All estimations include 2-digit occupation fixed effects. Standard errors clustered at region-occupation level (labor market region of training establishment, 2-digit occupations). Column 3 excludes interregional movers. * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

A major concern in the literature on wages and regional labor market competition is the fact that living costs, employer characteristics and labor market competition may be correlated and affect wages jointly (Boal and Ransom, 1997). Note, however, that this problem applies to agglomeration effects (which we control for by including region-level employment and wages) but not necessarily to regional competition. Nevertheless, to ensure that our results are not driven by such correlations, we exclude inter-regional movers in column 3, which leads to a minor sample restriction given that most employer changes are intra-regional. Again, this leaves the estimates mostly unchanged.

Table 3.9: Training wage position, differences between retained and moving apprenticeship completers by regional competition, baseline sample.

	(1) Training wage rel. to cell mean	(2) Training wage rel. to cell mean
Mover	-0.203***	-0.223***
	(-2.96)	(-2.76)
log firm density reg.-occ.	-0.003	0.003
	(-0.97)	(1.00)
Mover*log firm density reg.-occ.	0.047*	0.029
	(1.68)	(0.97)
Constant	-0.007	0.007
	(-0.97)	(0.99)
Observations	196697	190529
Adjusted R^2	-0.000	-0.000

t statistics in parentheses. All estimations include 2-digit occupation fixed effects. Standard errors clustered at region-occupation level (labor market region of training establishment, 2-digit occupations). Column 2 excludes interregional movers. * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

We also consider apprenticeship completers' training wages relative to the mean in their training cell in Table 3.9.[56] The results also indicate that movers are less negatively selected from among their peers if their training establishment is located in a highly competitive region. The effect is only marginally significant (column 1) and drops below conventional significance levels if inter-regional movers are excluded as a robustness check (column 2).[57] Still, the estimates point in the same direction as those for the absolute training wage and therefore do not invalidate our interpretation that movers in highly competitive regions are relatively favorably selected. Our findings therefore imply that a relatively large number and abler apprenticeship completers can be hired by external firms in more competitive regions, which is a disincentive to train own apprentices. Similar to previous studies (Mühlemann et al., 2013), our findings also suggest that the higher training costs implied by higher apprentice wages, which are largely sunk costs if apprentices move after training, deter employers in competitive regions from training.

[56] Analogous regression results for the poaching sample in Appendix Table A11.
[57] Due to the definition of the relative training wage (wage position within training the establishment), we do not include establishment- or region-level controls.

Additional to the relative quality and wage costs of apprentices, firms' training decisions are likely influenced by the wage costs of young skilled workers. An important topic discussed in the literature is the negative impact of fewness within regional (occupational) labor markets on wages of trained employees (Boal and Ransom, 1997; Manning, 2011; Mühlemann et al., 2013). Monopsony power is mainly measured within regions if employees' mobility costs are high, and within occupations if there are strong demarcation lines between occupations. Cases in point are remote mining towns, nurses, and teachers (Boal and Ransom, 1997; Benson, 2013). Regional and occupational barriers are well documented for the labor market of apprenticeship completers (Harhoff and Kane, 1997; Acemoglu and Pischke, 1998; Mühlemann et al., 2013). Many studies relate regional monopsony power to incentives to train and they concentrate on wage differences between unskilled and skilled employees (Stevens, 1994; Acemoglu and Pischke, 1998). Mühlemann et al. (2013) for example show that there is a positive effect of the number of regional employers on the wages of skilled employees and apprentices, an effect that is absent for unskilled employees. Analogously, wage differences between skilled and unskilled employees increase with the number of firms in a region. In contrast, the difference between skilled and apprentice wages is not affected.

From the perspective of firms that have to decide whether to train themselves instead of hiring apprenticeship completers (mainly) from their regional occupational labor market, however, the wage difference between externally hired and own apprenticeship completers should be more important than the wage difference between trained and untrained workers. We therefore compare the entry wages of movers and stayers, net of their previous training wages (also compare Table 3.3), in the context of regional competition. Subtracting the training wage from the skilled entry wage has the advantage that training wages control for productivity during training, thus yielding a wage indicator corrected for individual productivity.

Table 3.10: Wage increase between training and skilled job, differences between retained and moving apprenticeship completers by regional competition, baseline sample.

Dep. var.: log wage difference job-training	(1)	(2)	(3)	(4)
Mover	-0.087***	0.002	-0.015	0.005
	(-7.26)	(0.17)	(-1.43)	(0.35)
log firm density, reg.-occ.	0.042***	0.001	0.001	-0.000
	(12.36)	(0.34)	(0.28)	(-0.13)
Mover*log firm density, region-occupation	-0.022***	-0.022***	-0.021***	-0.029***
	(-3.98)	(-3.79)	(-3.67)	(-4.22)
log full-time employment, hiring estab.		0.069***	0.056***	0.070***
		(23.20)	(12.06)	(22.93)
log employment, hiring region		-0.014**	-0.009	-0.013**
		(-2.57)	(-1.02)	(-2.28)
log mean wage, hiring region		0.394***	0.475***	0.388***
		(9.12)	(4.59)	(8.94)
log full-time employment, training estab.			0.015***	
			(3.06)	
log employment, training region			-0.005	
			(-0.61)	
log avg. wage, training region			-0.085	
			(-0.87)	
Constant	3.658***	1.806***	1.826***	1.809***
	(30.47)	(8.07)	(8.05)	(8.01)
Observations	195752	195539	195495	189469
Adjusted R^2	0.145	0.208	0.208	0.210

t statistics in parentheses. All estimations include 2-digit occupation fixed effects. Standard errors clustered at region-occupation level (labor market region of hiring establishment, 2-digit occupations). Column 4 excludes interregional movers. * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

Regression results are reported in Table 3.10.[58] Again, the main regressor of interest is the interaction between the mover dummy and regional competition. We find a significant negative coefficient for this term, meaning that movers in highly competitive regions obtain lower wage increases between training and their first skilled job than movers in less competitive regions. This finding is robust across different specifications: In column 2, we include the size of the hiring establishment and its region, as well as the regional wage level. In column 3, we include the same variables of the training establishments, since the dependent variable is determined in both the training and hiring establishments. Finally, in column 4, we again restrict the sample to stayers and intra-regional movers, to rule out endogeneity bias from

[58] See Table A12 in the Appendix for results using the poaching sample.

inter-regional differences in costs of living and wages.[59] At first sight, our results appear to be at odds with our earlier finding that movers are a relatively good selection in competitive regions. Note, however, that the wage difference between training and skilled work already accounts for differences in individual quality. We therefore argue that movers' effective wage costs as skilled workers (taking into account their higher productivity as apprentices) are relatively low in competitive regions.

Our findings on wage differences between the last training period and the first wage as skilled employee also imply that higher regional labor market competition is associated with less opportunities for training firms to reduce skilled entry wages for their apprenticeship completers. Our findings finally reflect a recurring topic in regional economics – that agglomerations or "thick" regional labor markets allow for better firm-worker matching (Manning, 2011). Especially regarding apprenticeship completers, who are regionally not very mobile, the better supply of apprenticeship completers (from other firms) in dense regions may be a potent reason not to invest as much in own training.

This interpretation receives further support from the literature: Blatter et al. (2016) investigate the costs of hiring skilled workers in Switzerland. They find that training activity and the retention of own apprenticeship completers positively depend on external hiring costs. Our data do not contain direct hiring costs and therefore we cannot investigate their effect on training and hiring decisions.[60] We might argue, however, that the "pure hiring costs" beyond the wage offer necessary to attract skilled employees, such as advertising or the hiring procedure itself, are probably lower in dense regional labor markets, because it is not necessary to look for candidates working and living in another regional labor market. Overcoming mobility barriers in order to attract new employees from other labor market regions seems to

[59] Obviously, the training and hiring region are the same in this subsample. We therefore omit the training region controls in column 4.

[60] Another obstacle to replicate the Blatter et al. (2016) study is that we do not observe external hiring establishments' (raiders' and other hirers') own apprentice training in the same detail as for the sampled training establishments (victims and controls). A full analysis of the training behavior of raiding firms would require a larger and even more complex data base. Future work might pursue this kind of analysis.

be especially costly for apprenticeship completers in Germany, as indicated by their low level of inter-regional mobility. Wenzelmann et al. (2017) in addition find that recruitment costs in Germany decrease with the regional supply of apprenticeship completers. The last reason for lower hiring costs in agglomerations is the higher average quality of job candidates, meaning that less "lemons" have to be screened before finding a good match (Blatter et al., 2016).

Overall, we therefore conclude that the negative effect of regional competition on apprentice training works not only through the negative agglomeration effect of an increased poaching risk. It also works through the better availability of externally trained apprenticeship completers, a commonly positively perceived agglomeration effect. Other important aspects are a better selection, higher training costs, and relatively low labor and hiring costs of available apprenticeship completers. These additional reasons for lower training in more competitive regions have been discussed mainly in theoretical contributions so far. The few empirical contributions concentrated on one of the mechanisms analyzed here. Our paper therefore presents the first systematic empirical analysis of potential transmission channels between regional competition and training investments. It finds evidence for all the channels proposed in the literature, but reveals that poaching externalities are relatively unimportant.

3.8 Conclusions

We investigate whether poaching of apprenticeship completers in Germany is related to the regional labor market competition which training establishments face. We aim to contribute to a growing literature which suggests that regional labor market competition deters firms' training activity and claims that this is a consequence of firms' fear of having trained workers poached by competitors. Yet, none of these studies addresses the incidence of poaching directly. We apply an ex-post identification of poaching to address this gap in the empirical literature. Therein, we exploit the institutional design of the German apprenticeship system, which features training that is transferable between employers active in the same occupational labor market. In addition, individual trained workers' quality can be credibly shown by graded certificates.

Similar to previous studies, we find that the relationship between regional competition and German establishments' apprentice training efforts is significantly negative. Also in line with previous evidence, we find that regional competition decreases the retention of apprenticeship completers by their training firms. We finally find that poaching is positively associated with regional labor market competition. Endogeneity, in particular in the form of reverse causality, is unlikely to be a major problem in our estimations, as regional levels of competition are unlikely to be affected by the observed incidents of poaching in a labor market region. We also show that firms in competitive regions are more likely to "raid" apprenticeship completers (commit poaching). The last finding largely reflects the fact that more than 70 percent of apprenticeship completers stay in the same labor market region even when they change employers, which makes regions a suitable dimension for the analysis of competition in the first place.

However, we do not find poaching events to have any effect on victims' subsequent training behavior. We therefore seek to provide alternative explanations for the negative training effect of regional competition. We argue that certainly the (unmeasurable) threat of poaching might play a role. Yet more important might be regional differences that apply to all employer movers instead of differences that only concern the small group of poached apprenticeship completers. We find analogously to mainly theoretical papers that the retention rate is structurally lower in highly competitive labor market regions. Besides the higher availability of apprenticeship completers willing to move to another employer, this employee group is less adversely selected in more competitive regions. Moreover, more productive apprentices are more expensive, so the personnel costs of training firms are higher in competitive regions. Finally, hiring and (entry) wage costs of apprenticeship completers trained elsewhere are lower when we take into account their higher productivity. These differences all reduce the attractiveness of own training efforts in regions with strong labor market competition.

We therefore conclude that it is not actual poaching of apprenticeship completers that drives the lower training rates in highly competitive regional labor markets. Instead, the better availability and quality of apprenticeship completers who are willing to change their employer directly after training, and the lower hiring and wage costs of apprenticeship completers who move to another employer, seem to be more important.

4 The effect of broadband internet on establishments' employment growth: evidence from Germany*

4.1 Introduction

The expansion of broadband internet is one of the most important current developments regarding the technological infrastructure in industrialized countries. Across Europe and North America, federal and regional governments are investing large sums in the expansion of broadband, notably to rural areas. Germany, the empirical focus of this paper, is no exception. According to one estimate, the German federal government's goal to provide download rates of at least 50 Mbit/s to all households in Germany by 2018 requires investments of about € 20 billion, potentially even more.[62] One of the declared goals of broadband expansion is to promote business and employment. As yet, however, there is limited empirical evidence to inform policymakers of the economic effects of broadband expansion.

Existing evidence suggests that broadband expansion may close the "digital divide" between urban and rural areas and help prevent the depopulation of the latter, but may not stimulate employment (Briglauer et al., 2016). Moreover, there is relatively little empirical evidence on employment effects at the firm level, where employment decisions are made. This lack of micro-level evidence makes it hard to assess through which channels the potential employment effects of broadband come about; for instance, broadband might affect employment not only through changes in production technologies, but also through changes in product demand. Previous research suggests, however, that service-sector employment benefits more from broadband deployment than manufacturing employment (Bertschek et al., 2016; What Works Centre for Local Economic Growth, 2015). From a policy perspective,

* This chapter has been published as Stockinger (2017).

[62] See http://www.it-zoom.de/it-director/e/breitbandversorgung-in-deutschland-9341/, last accessed April 28, 2016. The federal state of Bavaria alone contributes € 1.5 bn to reach this goal within its territory (see http://www.schnelles-internet-in-bayern.de/foerderung/ueberblick.html, last accessed April 28, 2016).

sectoral effect heterogeneity is important also because industries are often spatially concentrated (for recent evidence on Germany, see Dauth et al., 2016). Since broadband expansion is primarily a regional policy objective, one should thus account for potential sectoral heterogeneity in broadband effects when deriving policy recommendations. I thus estimate the effects of local broadband availability on the employment growth of German firms,[63] thus focusing on potential firm-level complementarities between broadband and labor, and considering sectoral heterogeneity.

In order to identify the causal relationship between broadband availability and employment growth, I use exogenous differences in municipal broadband availability due to historically determined technical frictions. The analysis focuses on the years 2005-2009, when broadband was introduced in the rural parts of Western Germany and large parts of Eastern Germany. The results indicate that German establishments experienced very different employment growth effects in response to broadband expansion. For Western German establishments, I find a significant drop in employment growth rates for the manufacturing sector, but at the same time a significant increase in the service sector. Further estimations suggest the latter effect is driven by knowledge- and computer-intensive industries, which concentrate in the service sector. No significant results are found for Eastern Germany.

This chapter is structured as follows. The following section discusses the technological importance of broadband and its implications for the labor market, and summarizes the current state of empirical research on the labor market effects of broadband. Section 4.3 presents the data on local broadband infrastructure, and section 4.4, the identification strategy based on these data. Section 4.5 presents the estimation model and briefly describes the establishment-level employment data. Section 4.6 provides descriptive statistics. Section 4.7 presents the results. Section 4.8 discusses limitations of the analysis and potentials for further research. Section 4.9 concludes.

[63] In fact, the empirical analysis is based on establishments rather than firms. For better legibility, I refer to firms where the distinction is conceptually irrelevant.

4.2 Theoretical background and previous evidence

4.2.1 Broadband as a technology

Broadband internet has drastically reduced the cost of information and communication. A given amount of information can be delivered much faster via broadband than via basic internet service, and within a given amount of time, broadband can transfer higher-quality content than earlier technologies. In the terminology of Bresnahan and Trajtenberg (1995), broadband can be regarded as a "general purpose technology" (see also Harris, 1998 and Atkinson and McKay, 2007) which improves the conditions for innovation and productivity in a broad range of economic activities, due to complementarity with subsequent innovations in many industries. Furthermore, broadband may be regarded as an "enabling technology" which unfolds its full economic potential once it is widely available (Bresnahan and Trajtenberg, 1995). Thus, the productivity gains from ICT extend well beyond the ICT sector itself. Instead, ICT-using firms and households contribute to improvements in productivity and demand, and thus growth (OECD, 2008).

In line with these predictions, Röller and Waverman (2001) find that telecommunication infrastructure seems to be causally related to economic growth in OECD countries, suggesting that better telecommunication infrastructure facilitates market transactions. Röller and Waverman (2001) also suggest that the growth effect of telecommunication infrastructure gets strongest as the infrastructure approaches universal coverage, enabling a large share of producers and consumers to use it. Similarly, Koutroumpis (2009) and Czernich et al. (2011) find that it takes a "critical mass" of broadband users for the technology for the full economic impact of broadband to unfold, in line with the "enabluing technology" hypothesis. In a survey of empirical evidence for the US, Holt and Jamison (2009) find that ICTs generally, and broadband more specifically, seem to have positive economic effects (notably, on output and productivity growth). In a cross-country comparison, Czernich et al. (2011) find that across OECD countries, broadband availability increases GDP growth.

As a broad technological advancement, thus, broadband seems to have far-reaching consequences for production, notably a positive effect on total factor productivity. In particular, broadband should increase the efficiency of economic activities which use information that can be "digitized" and thus shared via the internet. According to standard theory, this efficiency increase implies a reduction in the relative demand for kinds of labor that broadband substitutes for, and an increase in the relative demand for complementary labor (e.g. Cahuc and Zylberberg, p. 587 sqq.). Regarding total labor demand (and hence employment), the effect of technological advancements such as broadband introduction is theoretically ambiguous for two reason. First, broadband can be either a substitute for or a complement to labor, or both (in different sectors or groups of workers, respectively). This ambiguity can be addressed by analyzing different segments of the labor market (e.g. industries and qualification groups) separately. Second, the labor market effects of technological progress depend on the elasticity of demand in the product market (e.g. Blien and Ludewig, 2016). Therefore, for an assessment of the aggregate employment effects of broadband expansion, one has to account for its product-market implications. Accounting for this latter ambiguity, however, is beyond the scope of this paper. The aim is instead to identify firm-level employment effects.

At the micro level, the realms of broadband internet effects on the labor market can be broadly distinguished as follows. On the one hand, broadband may affect labor supply and matching. In particular, fast internet facilitates job search and increases the number and quality of job offers job seekers can receive, thus potentially affecting labor supply and worker-firm matching (see Mang, 2012 and Dettling, 2013). For instance, job seekers may find jobs faster, or in contrast, raise their expectations about job characteristics, due to an increased quantity and quality of search results. On the other hand, broadband may affect firms' labor demand. To make use of the local broadband infrastructure, firms may have to adjust their labor input, and different firms (and industries) may react differently to increased broadband availability. While this paper focuses on labor demand at the firm level, most studies on broadband and the labor market consider outcomes at the region level, since broadband availability varies mainly in the spatial dimension. In the following, I therefore review relevant studies at both, the region and micro levels.

4.2.2 Regional labor market effects of broadband

There are large differences in broadband coverage between core and peripheral areas (the "digital divide") that attract policymakers' attention, and a substantial number of empirical studies address the economic implications of these differences. Overall, these studies tend to find positive, but not necessarily large employment and wage effects. More specifically, broadband effects vary between types of regions, industries, and groups of workers (notably by skill level), according to one survey article (What Works Centre for Local Economic Growth, 2015). Another very comprehensive literature survey is provided by Bertschek et al. (2016), who also conclude that the overall labor market effects of broadband tend to be positive. More precisely, empirical studies tend to find unambiguously positive labor market effects of broadband adoption (that is, broadband use), and somewhat less robustly positive effects of mere broadband availability. Given the just cited surveys of empirical evidence, I narrow my own literature review to studies which focus on labor market outcomes, use data from developed countries, and critically discuss the credibility of the employed identification strategies.

Kandilov and Renkow (2010) find that a US broadband loan program targeted at rural areas had positive employment and wage effects. Also for the US, Kolko (2012) finds a positive local employment growth effect of broadband availability, which is stronger in IT-intensive industries and less densely populated areas. Using German data, Fabritz (2013) finds evidence suggestive of small positive employment effects of broadband availability at the municipality level, but not in the manufacturing sector. Thus, the effect is likely to stem from the service sector (which cannot be distinguished in the data). In contrast, Czernich (2014) studies the effect of broadband internet availability on unemployment at the municipality level in rural areas in Germany, not finding a significant effect.

Some studies investigate the effect of broadband on firm start-up, that is, employment growth at the extensive margin, as opposed to the intensive margin of employment growth at incumbent firms. Tranos and Mack (2016) investigate the relationship between broadband availability and presence of knowledge-intensive service establishments in the US. Using Granger causality tests, the paper finds evidence suggestive of a causal relationship between the presence of broadband providers in a county and of knowledge-intensive service establishments. In contrast, Atasoy (2013) addresses the intensive margin (existing firms), finding a positive relationship between employment and broadband availability, also for the US. In that study, the identification of effects relies on regression specifications including county and time fixed effects.

While studies on the effects of broadband availability dominate the literature (due to data availability), some studies at the regional level have also investigated the effects of broadband adoption, that is, the take-up of the technology by firms and households. Using propensity score matching, Whitacre et al. (2014a) find that broadband adoption has positive effects on employment and income in US rural areas. Applying a difference-in-differences approach, Whitacre et al. (2014b) add that high broadband adoption in rural areas is positively associated with the number of firms and employees, as well as with median household income. However, neither study finds robustly significant effects on labor market outcomes, and none of them claims to identify causal relationships. Yet, the studies demonstrate that, even if broadband availability cannot be shown to affect local labor markets, it might have effects on those who actually use it. In this spirit, further studies on adoption effects have been conducted at the firm and worker levels.

4.2.3 Firm- and worker-level effects of broadband

Similar to the region-level evidence, empirical studies at the firm level often consider various outcomes that might be affected by broadband (employment, employment growth, productivity, innovation). Even if employment effects are not addressed explicitly, however, these studies may hold lessons for the labor market implications of broadband. For instance, using a sample of firms in Germany, Bertschek et al. (2013) find that broadband use positively affects innovative activity, but not labor productivity. Canzian et al. (2015), who focus on rural areas in the Trento region (Italy), find a positive effect of a more recent broadband technology (ADSL2+) on firm productivity (sales, value added), but not employment. The paper exploits plausibly exogenous variation in broadband availability, caused by a stepwise rollout of the ADSL2+ technology.

Exploiting a similar infrastructure program in Norway which generated exogenous variation in broadband availability, Akerman et al. (2015) estimate the effect of broadband adoption in firms on wages and other person- and firm-level outcomes. Their findings suggest that broadband increases high-skilled workers' employment, productivity, and wages, but has negative effects on the low-skilled. Akerman et al. (2015) attribute their findings to the complementarity of internet use with abstract, non-routine work tasks, and its substitution for routine jobs, implying that broadband increases the productivity of workers who use ICT intensively. Ruling out alternative channels through which broadband might affect labor market outcomes, Akerman et al. (2015) thus demonstrate that production processes in firms are the main such channel. Moreover, since they find no effect on the output elasticity of capital, their findings suggest that broadband affects firms primarily through its effects on labor productivity.

Broadband adoption is observed only in few other firm-level studies. Colombo et al. (2013) estimate the impact of broadband adoption on the productivity of small and medium enterprises (SMEs) in Italy. While they find hardly any effect from basic broadband technologies, there tend to be positive productivity effects associated with more advanced technologies. De Stefano et al. (2014) exploit very detailed spatial data on firms located on both sides of a technological border in the Hull region (UK), one side served by a quasi-monopolist and the other by a small competitor who provided broadband five years earlier. The study finds no significant effect of broadband adoption on the sales, employment, labor productivity, or survival of firms. Haller and Lyons (2015) find that broadband adoption, while higher in highly productive firms, does not lead to further or accelerated productivity growth in the Irish manufacturing sector. Finally, Bertschek and Niebel (2016) find evidence of positive labor productivity effects of mobile internet use by German firms, supposedly working through increased flexibility in the organization of work.

Firm-level evidence thus points at limited and not always positive effects of broadband availability and adoption on labor market outcomes. An important difference emerges between the manufacturing and service sectors – the latter typically benefits more from broadband than the former. This pattern of results likely reflects the more intense use of ICT and, more specifically, digital technologies, in the service sector. A survey of 2,000 establishments in Germany found that the share of establishments that use modern digital technologies[64] is much larger in services sector than in manufacturing (Arntz et al., 2016). One should therefore expect relatively large effects of broadband on labor demand in services and ICT-intensive industries.

In the following, I aim to identify the effect of local broadband supply on employment growth in a sample of German establishments. I thereby focus on the demand-side channel of the labor market effects of broadband, since broadband availability is measured at production sites (or places of work), rather than at workers' places of residence (where broadband might affect labor supply, e.g. through job search behavior). Since broadband availability is measured at the municipality level, and because the sampled municipalities are relatively small, places of work and residence are relatively unlikely to coincide. Furthermore, I choose to investigate employment growth rather than employment levels as the outcome, because first, establishment are more likely able to adjust at this margin, and second, because the data I use are largely limited to employment-related indicators, but lack indicators on capital, investment, and output, which would be important covariates in an analysis of employment (level) outcomes. Before presenting the estimation model in detail, however, I provide details on the broadband data and the identification strategy used to extract exogenous variation in broadband availability.

[64] E.g. cloud computing, generating and utilizing big data, or (in the case of manufacturers) cyber-physical systems and "smart factories".

4.3 Broadband data

Nowadays, the term broadband is typically understood as an internet connection with data transfer rates of several Megabits per second (MB/s), currently up to 50 MB/s or even (rarely) 100 MB/s. This most recent state of technology is not the subject of this paper. Instead, I consider the first generation of broadband, which first became available in Germany by the year 2000. By far the most important broadband technology in Germany was and is DSL (digital subscriber line), which covers more than 90 percent of broadband subscriptions in Germany (Bundesnetzagentur, 2013). DSL allows downstream data transfer rates of at least 384 kilobits per second (kb/s). Prior technologies (dial-up, ISDN) allowed maximum download rates between 64 and 128 kb/s. Thus, while slow compared to today's broadband standard, DSL drastically improved the conditions for data- and communication-intensive economic activities, for instance organizational tasks such as file-sharing and video-conferences, marketing and sales via e-commerce, recruiting personnel through online portals, or other business activities.

Regarding its economic impact, an important aspect of the introduction of DSL is the fast speed at which the technology could be installed, because the necessary underlying infrastructure (the telephone network) was already in place, as explained in more detail in the following section. Furthermore, in contrast to some earlier technologies, DSL was offered from the very beginning as an "always-on" service for which users pay a flat-rate price, regardless of how intensely they use it. That is, besides increasing connection speed, DSL drastically reduced the marginal costs of internet service.

The main explanatory variable in this analysis is broadband availability, also referred to as broadband penetration. Data on broadband availability are obtained from the Broadband Atlas published by the Federal Ministry of Economics and Technology (BMWi, 2009). The data contain the share of households, at the municipality-year level, for which a DSL connection is available, that is, technically feasible but not necessarily adopted (used).

Figure 4.1: DSL availability in Germany, 2005-2009

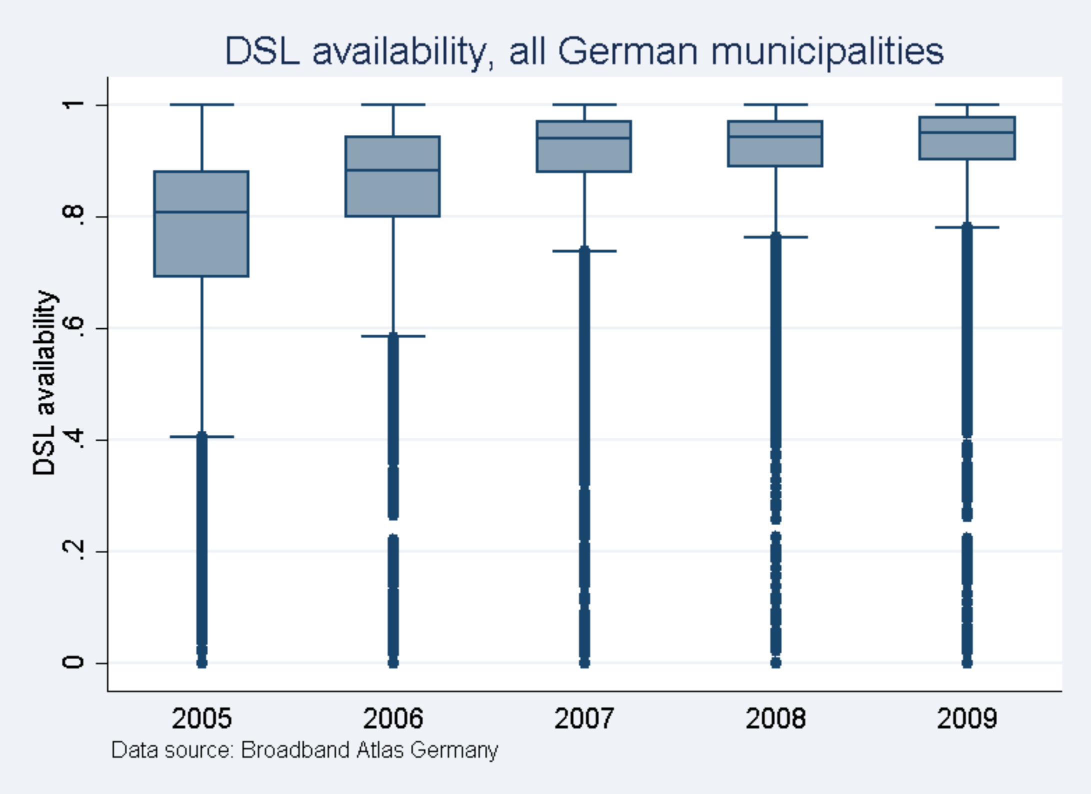
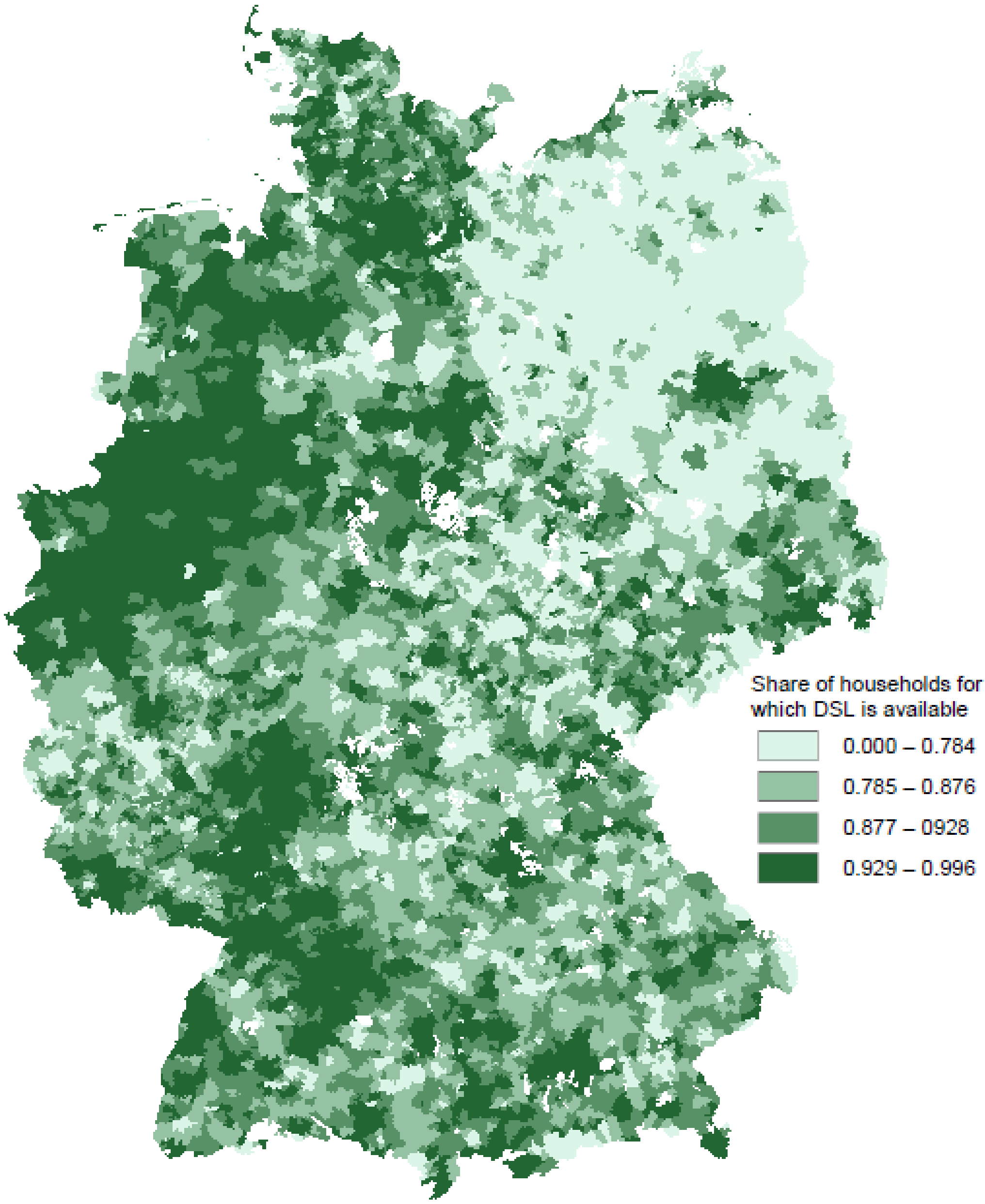


Figure 4.1 illustrates that DSL availability converged to a median of about 95 percent towards the end of the observation period (2005-2009), but there was substantial variation across and within municipalities at least in the earlier years. However, the convergence to full coverage across the country could also be crucial for the identification of economic effects since, as discussed above, ICT improvements typically reach full effectiveness only when a large mass of users is reached. Regarding the sample of relatively small firms used in this study (details below), this period of widespread broadband availability thus seems particularly interesting, compared to the early stages of DSL deployment which may have affected only large and technologically advanced firms.

Figure 4.2: Map of DSL availability in German municipalities



The map shows the mean of annual availability rates for the period 2005-2009. White spots are areas not belonging to any municipality (gemeindefreie Gebiete; e.g., forests, lakes, mountains, and military territories) or municipalities for which boundaries could not be harmonized over time. Data source: Broadband Atlas Germany.

Regarding the spatial variation of DSL availability, Figure 4.2 shows that availability rates in 2005-2009 were highest in densely populated regions such as Berlin, the Rhein-Ruhr area in West, the Rhein-Main (Frankfurt) region, and the major cities in the South (Munich, Stuttgart). In the following empirical analysis, however, these metropolitan areas are largely discarded (for Western Germany), as explained below. Outside the metropolitan areas, DSL availability varies visibly even between adjacent municipalities. That is, the rollout of DSL proceeded much faster in some municipalities than in others.

These sharp differences in DSL availability offer an opportunity to identify causal relationships between local broadband availability and labor market outcomes. The main empirical challenge involved is to find an exogenous source of variation in broadband availability between municipalities and intertemporal changes therein. Such a setting is unlikely to exist if broadband is provided by private firms, which have an obvious incentive to provide better access where firms and households are more likely to subscribe, which itself is more likely to occur where firms expect greater profits from using broadband. To tackle this challenge, I employ an IV approach developed and used by Czernich et al. (2011) and others,[65] which I briefly describe in the following.

4.4 Instrumental variables approach

Concerning Western Germany, the proposed identification strategy exploits technical properties of the public-switched telephone network (PSTN) built in the 1960s. During the observation period of this study, DSL was supplied using "fiber-to-the-node" (FTTN) technology, meaning that the more central part of the telephone network was already equipped with "fast" optical fiber connections, while the "last mile" between the most decentral nodes (main distribution frames or MDFs) was served via copper wires. Unlike telephone service, DSL service decays in quality as the distance between the MDF and the end user increases. The main supplier of DSL (Deutsche Telekom) defined the maximum acceptable amount of quality loss due to distance decay at a signal strength of 55 dB, which translates into a distance of approximately 4.2 km. Thus, beyond 4.2 km from the MDF, DSL service was not provided until, years later, at least part of the last-mile copper wires became replaced by fiber wires. That is, DSL did eventually become available even to users more than 4.2 km distant from their MDF, but only with a substantial time

[65] Czernich (2012; 2014), Fabritz (2013), and Falck et al. (2014).

lag, not least because telecommunication wires are installed subsurface in Germany.

When the PSTN and hence the MDFs were installed in the 1960s, the state-monopolist telecommunication provider (the German federal postal services) aimed to provide universal telephone service, even in remote areas. Therefore, the circa 8,000 MDFs were allocated relatively densely and evenly across the country (see Figure A.4.1 in the Appendix). The precise locations of MDFs were determined, notably, by the availability of lots or buildings were the MDF, which is the size of a small hut, could be placed.[66] The distance between an MDF and its users, in contrast, was irrelevant for the location decision. As argued extensively by Falck et al. (2014), the location of MDFs can thus be assumed to be exogenous with regard to local economic outcomes some 40 years later. Therefore, the ("fuzzy") distance threshold of 4.2 km can be used as an instrument for the local availability of DSL.

A caveat of the proposed IV approach is that it only applies to rural areas, for the following reasons. Reflecting the number of connected users (and thus the required capacity of telephone service), there are more MDFs in cities than in rural areas. Cities usually have at least one own MDF, and hardly any users in urban municipalities are more than 4.2 km distant from the MDF they are connected to. Even for smaller towns and villages which host at least one MDF within their boundaries, the distance between MDF and users rarely exceeds 4.2 km, since the average German municipality is the size of a circle with 3.2 km radius.[67] Furthermore, only for very few municipalities, the entire territory is more than 4.2 km distant from the MDF, emphasizing the quasi-randomness of MDF locations across the country.

Yet, a substantial number of rural municipalities do not host an MDF, meaning that the distance between any point in the municipality and the MDF is positive. Using

[66] The Data on MDF locations used in this study were originally provided by Deutsche Telekom (Germany's largest telecommunication provider) and further processed (aggregated to the municipality-year level and merged with other municipality-level data) by Falck et al. (2014).

[67] As of Dec. 31, 2015, Germany had 11,092 municipalities (https://de.statista.com/statistik/daten/studie/1254/umfrage/anzahl-der-gemeinden-in-deutschland-nach-gemeindegroessenklassen/, last accessed Feb. 10, 2017) and it has a land area of 357,386 square km (http://www.statistik-portal.de/Statistik-Portal/de_jb01_jahrtab1.asp, last accessed Feb. 10, 2017). Average municipality size is thus roughly 32 square km, equivalent to a circle with 3.2 km radius.

the municipality centroid as a proxy for the location of the municipality, one can thus measure the distance between the municipality and the MDF. Thus, the distance-based IV applies to Western German municipalities without an own MDF, resulting in a sample of rather rural municipalities. For a descriptive comparison of sample municipalities to all other municipalities, see Figure A.4.4.

As in Falck et al. (2014), thus, the IV is a dummy variable that classifies municipalities as being above or below the 4.2 km threshold as follows:

$$IV_m^{West} = \begin{cases} 1 \; if \; municipality \; centroid > 4200m \; from \; assigned \; MDF \\ 0 \; otherwise. \end{cases}$$

As argued by Falck et al. (2014), this dummy is a more credible IV than would be the linear distance between the municipality center and the MDF. In particular, the exact distance to the MDF might violate the exclusion restriction, i.e. it might have an effect on the outcome (employment growth) other than through DSL availability. This is because municipalities particularly far from the MDF are likely also very remote locations in other respects, such as access to transport infrastructure, that have a direct impact on the labor market.

The discussion so far focused only on the MDF the municipality was originally (in the 1960s) assigned to for telephone service. However, the assigned MDF is not necessarily the closest one. In fact, some municipalities above the 4.2 km threshold (with respect to the assigned MDF) are less than 4.2 kms distant from the nearest MDF, which has two important implications: First, this possibility is further evidence that the distance between MDF and users was indeed irrelevant for the original location of MDFs, respectively the assignment of municipalities to MDFs. Second, municipalities with an *assigned* MDF above the threshold but a *nearest* MDF below the threshold did, after all, obtain DSL by getting connected to the latter. I drop the (few) municipalities where this is the case from the estimation sample.[68] All three possible situations of municipalities without an own MDF are illustrated

[68] Alternatively, one could retain these municipalities and set the value of the IV to zero. I choose not to do so because an important robustness check (using the share of a municipality's land area above the distance threshold as an alternative IV) is only possible with respect to the assigned MDF (given the available data).

for a concrete example in Figure A.4.2 in the Appendix. Figure A.4.3 presents a map of all German municipalities without an own MDF, indicating whether they are above or below the distance threshold. The map shows that even adjacent (and therefore otherwise similar) municipalities differ with respect to their distance from the MDF. Being above or below the threshold thus is probably the most important reason for the sharp differences in DSL availability even between similarly located municipalities seen in Figure 4.2.

For Eastern Germany (the former German Democratic Republic), one cannot construct the same distance-based IV, but it is possible to exploit a historic "accident" that also exogenously caused some municipalities to receive DSL service later than others (see also Falck et al., 2014). When the Eastern German telephone network was modernized after the German Reunification in the early 1990s, the government and private providers anticipated a growing need for high data transfer rates. In a number of pilot projects and subsequently at a larger scale, a technology called OPAL (*Optische Anschlussleitung*) was therefore installed in 213 Eastern German catchment areas (i.e. areas served by one MDF). OPAL is a predecessor of modern fiber-wire technology, which at the time was regarded as capable of providing high-speed internet service. Unfortunately, the data transfer rates that were eventually required (for DSL) where strongly underestimated. Therefore, having the OPAL technology installed turned out to be a disadvantage for DSL rollout. In order to supply DSL, the local OPAL networks had to be either replaced altogether or substantially technically updated, both of which were time-consuming and costly enterprises. Being located in an OPAL area thus negatively affected DSL availability throughout the decade 2000-2009. An OPAL area dummy can therefore be used as an instrument for DSL availability in Eastern German municipalities:

$$IV_m^{East} = \begin{cases} 1 \; if \; OPAL \; area \\ 0 \; otherwise. \end{cases}$$

Although OPAL areas are relatively urban municipalities (or parts thereof), the value of the OPAL dummy can be reasonably regarded as quasi-random (see Figure A.4.1). To ensure the relevance of this IV for DSL availability, the estimation sample includes only municipalities whose geographic centroid is less than 4.2 km distant from the nearest MDF. In these municipalities, as in Western German municipalities below the distance threshold, DSL was easily available – except for OPAL areas. Restricting the Eastern sample to these municipalities results in a relatively urban sample (see Figure A.4.5 for a comparison of sampled and non-sampled municipalities).

4.5 Employment data and estimation model

The empirical specification I estimate can be written as a two-stage model, with establishment-level employment growth as the main outcome. Establishment-level data are obtained from the Establishment History Panel (BHP) of the Institute for Employment Research (IAB), an annual panel (at the reference date June 30) that contains employment aggregates and other characteristics of all establishments in Germany with at least one employee liable to social security.[69] The data are based on the administrative records of the German Federal Employment Agency, which contain all private and public sector employees liable to social security. I use a ten percent random sample of establishments observed in the BHP in the period 2005-2009. The sample is cleared of establishments observed for the first time in 2005 or later, since these are likely newly founded establishments which might have been attracted by broadband expansion, rather than experiencing broadband rollout as an exogenous technological shock. Further sample cleaning steps are documented in Appendix section "Sample restrictions".[70]

Regarding the estimation model, the main (second-stage) equation is:

$$\ln\left(\frac{L_{it+1}}{L_{it}}\right) = \beta_0 + \beta_1 \widehat{DSL}_{mt} + \boldsymbol{\beta_2 X_{it}} + \boldsymbol{\beta_3 X'_{mt}} + \theta_c + \vartheta_j + \mu_t + \varepsilon_{it}. \quad (4.1)$$

The dependent variable is the log growth rate of establishment *i*'s employment between years *t+1* and *t* (with *t* = 2005, …, 2009). L_{it} is the total number of employees at establishment *i* in year *t*. The explanatory variable of interest is the (instrumented) availability of broadband in municipality *m* and year *t* (DSL_{mt}), measured as the

[69] For a detailed description of the BHP, see Gruhl et al. (2012).

[70] The municipality-level sample restrictions implied by the identification strategy are independent of these establishment-level restrictions. That is, only a regional subset of the ten percent random sample of establishments enter the analysis.

share of households for which DSL is available.[71] Control variables at the establishment level (X_{it}) are the log number of full-time employees, the share of high-skilled employees, the log median daily wage for full-time workers, and three dummies for establishment age.[72] Unfortunately, the BHP does not contain data on capital, investment, output, or profitability.[73] Municipality-level controls (X'_{mt}) include the log number of full-time employees and its growth rate (between *t-1* and *t*), log employment density, the share of high-skilled employees, as well as the log mean wage of full-time workers. Municipality-level controls are computed using the entire population of establishments in the BHP (ca. 2 million establishments). The mean wage is computed as the municipality-year mean of establishment-level median wages. The municipality-level controls furthermore include the share of high-skilled employees. Finally, I include fixed effects for districts (*c*), three-digit industries (*j*), and years (*t*). Municipality fixed effects are not an option because the time-invariant instrument would drop out of the first-stage estimation equation.

At the first stage, broadband availability is instrumented using either of the IVs presented above, both of which vary between municipalities but not over time. Control variables, indices, and fixed effects are the same as in the second-stage equation. The first stage thus is:

$$DSL_{mt} = \alpha_0 + \alpha_1 IV_m + \alpha_2 \boldsymbol{X_{it}} + \alpha_3 \boldsymbol{X'_{mt}} + \pi_c + \rho_j + \sigma_t + u_{mt}. \qquad (4.2)$$

Since large establishments are rare but employ a major share of the national workforce, I weight all estimations by the establishment's number of full-time employees. Thus, the obtained estimates should be more meaningful regarding the overall employment growth effect of broadband. Furthermore, standard errors are clustered

[71] Municipality codes refer to territorial boundaries as of Dec. 31, 2008.

[72] High-skilled employees are defined by their occupation rather than education, because the latter variable has a large number of missing values. Occupations considered as high-skilled are: engineers, managers, professionals (e.g. lawyers, architects), semi-professionals (service-sector workers with an advanced qualification), and technicians (manufacturing-sector workers with an advanced qualification).

[73] The survey-based IAB Establishment Panel, which can be linked to the BHP, does contain some of this information. However, using only establishments surveyed in the Establishment Panel (ca. 16,000 observations/year) would result in a much too small estimation sample, considering that the IV strategy is applicable only to a subset of municipalities (in the case of Western Germany, these are rural municipalities which host only a small fraction of all establishments and employees).

at the municipality level, since this is the level at which the instruments vary. Finally, to account for the sectoral heterogeneity in broadband effects, I estimate the model not only for all sampled establishments, but also separately for different sectors.

Besides the rather obvious problem of potential endogeneity (the main motivation for the proposed IV approach), another rationale for employing IV regression is to address measurement error in the explanatory variable, see Angrist and Pischke (2009, p. 127 sqq.) and Hausman (2001). By using spurious variation in the explanatory variable, measurement error tends to bias OLS estimates towards zero. In the current context, the most obvious source of measurement error is that DSL availability is observed only at the municipality level, while employment growth is observed at the establishment level. Therefore, DSL availability is necessarily measured with error for some establishments. Regarding the Western German sample in particular, such mismeasurement is likely if the municipality is classified as being above the distance threshold, while the establishment location is in fact below the threshold, or vice versa. This particular source of error is further addressed in a robustness check. Another minor source of measurement error may be that DSL availability is measured with regard to households and not establishments. For the Western German sample, for which the precise distance to the MDF is crucial, DSL availability might thus differ between households and establishments, inasmuch as these are located differently across the municipality. The IV approach is intended to alleviate the estimation bias due to these measurement errors.

Regarding the interpretation of the estimates, under standard assumptions,[74] the IV estimator identifies the local average treatment effect (LATE) for "compliers," i.e. establishments in municipalities which have lower (higher) broadband availability if the technical friction used as an IV is (not) in place (Imbens and Angrist, 1994). Obviously, this consideration involves a counterfactual and therefore cannot be as-

[74] Validity of the exclusion restriction (the instrument affects employment growth only through DSL availability); relevance of the instrument; monotonicity of the instrument's effect on DSL availability (either positive or negative, including the possibility of a neutral effect for some units of analysis); see Angrist and Pischke, 2009, pp. 154 sq.).

sessed empirically. However, considering the massive negative associations between the IVs and broadband availability (documented below), this does not appear to be a major limitation. That is, municipalities had no choice but to comply, that is, to have lower (higher) local broadband availability if (not) being subject to one of the technical frictions. At the establishment level, at which the outcome is measured, a potential problem of non-compliance does arise: Establishments could obtain broadband service independently of the general local DSL rollout via private leased lines. However, at the time considered, these were affordable only to large firms.[75] Given the average size of our sample establishments (about ten full-time employees in both the Western and Eastern samples), thus, the identified LATE should be a fair approximation of the average treatment effect.

A related caveat to the interpretation of the estimated coefficient is that only broadband availability, but not broadband adoption by the establishments is observed. In potential outcomes terminology, this means the identified coefficient represents an intention-to-treat (ITT) effect. As discussed by Czernich (2014), the ITT effect is necessarily closer to zero than the effect of adopting and using broadband. Therefore, the estimated effect of DSL availability should understate the effect of DSL use on employment growth. At the same time, however, note that the observation period captures a relatively late stage of DSL rollout, when DSL was already an established and affordable technology. Firms that use the internet intensely therefore likely adopted DSL as soon as it became available to them. Thus, the difference between the ITT effect and the average treatment effect should be limited.

A final issue regarding the interpretation of the results is external validity, due to the geographic bias of the sample municipalities (relatively rural for Western Germany, relatively urban for Eastern Germany). The estimated effects may not extend to establishments in urban Western German or rural Eastern Germany municipalities.

[75] According to Fabritz (2013), 82 percent of German firms use the local DSL infrastructure.

4.6 Descriptive analysis

Pursuing the above-discussed identification strategy, I obtain two separate estimation samples for Western and Eastern Germany. Table 4.1 summarizes the number of municipalities, establishments, and observations for both samples and both possible values of the respective IV. For the Western German sample, broadband availability is exogenously determined by the MDF-distance threshold. The relevance of the instrument can thus be displayed as a relationship between a municipality's DSL availability and the distance to its assigned MDF.

Table 4.1: Sample sizes

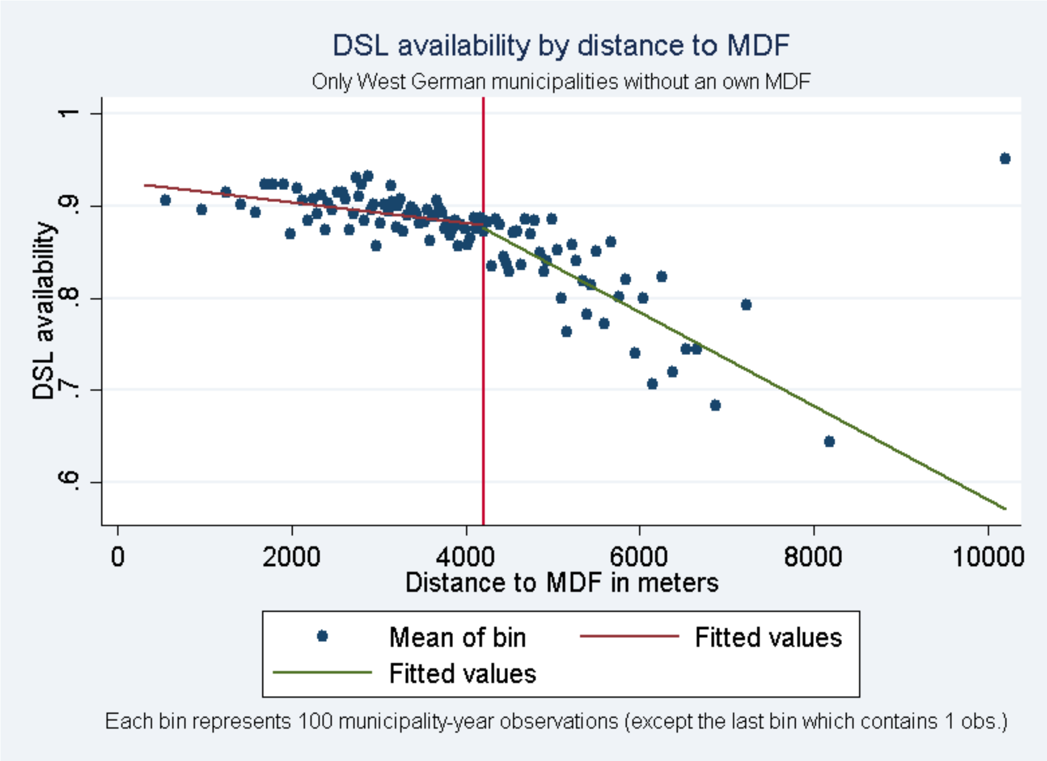
	Western Germany		Eastern Germany	
	IV=0	IV=1	IV=0	IV=1
Municipality observations	6,820	4,081	6,497	716
Municipalities	1,482	893	1,389	151
Establishment observations	21,708	11,765	55,766	14,989
Establishments	5,324	2,901	14,034	3,804

Data source: IAB Establishment History Panel (BHP) 7510 v1, Nuremberg 2012

Figure 4.3 plots DSL availability against the distance between municipality threshold and MDF; municipality-year observations are grouped into bins (each containing 100 observations) ordered by the same distance. As the graph shows, DSL availability decreases sharply at the 4.2 km threshold.[76] Figure 4.4 illustrates the relevance of the respective IV for both, the Western and Eastern German samples. In both cases, DSL availability is highly significantly correlated with the instrument – the technical frictions represented by the instruments indeed seem to inhibit DSL availability.

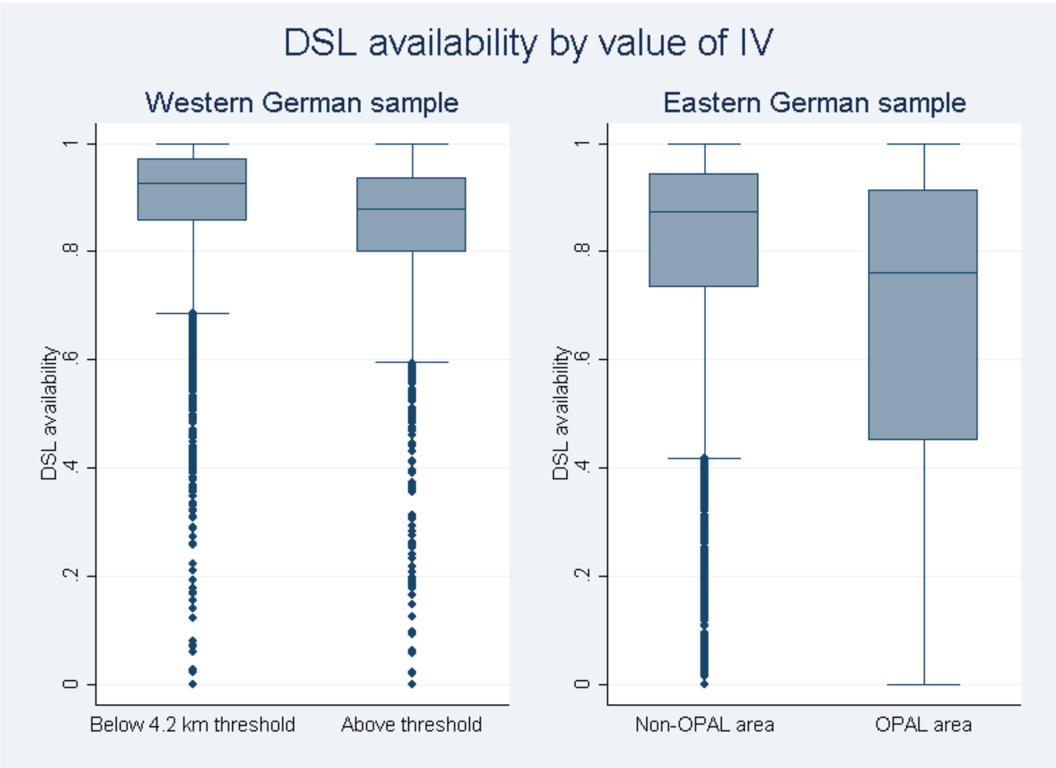
[76] The last bin, representing a municipality observation more than 10 km from the assigned MDF, can be considered an outlier.

Figure 4.3: DSL availability by distance between municipality centroid and MDF



Data source: Broadband Atlas Germany, Deutsche Telekom

Figure 4.4: DSL availability by value of the IV



Data source: Broadband Atlas Germany, Deutsche Telekom

The main issue concerning the validity of the identification strategy is whether the technical frictions on which the IVs are based can be regarded as exogenous, i.e. unrelated to the outcome of interest (except through their effect on DSL availability) and to unobserved determinants of the outcome. This question can be addressed to some extent by comparing group means of relevant covariates for either value of the binary IVs. Table 4.2 summarizes several municipality-level variables for both, the Western and Eastern German samples. The data refer to 1999, the year before DSL became available, so DSL use cannot possibly have affected these variables. The table reports raw group means at the municipality level and p-values from a t-test on equality of means cleared of district fixed effects (which are also used in the estimations).

Table 4.2: Balance of municipality-level covariates

	West			East		
	IV=1	IV=0	p-value°	IV=1	IV=0	p-value°
Log FT empl. growth rate	0.043	0.041	0.729	0.019	0.003	0.026**
Log full-time employment	5.091	5.203	0.077*	6.565	6.175	0.006***
Log mean wage	4.370	4.384	0.448	4.112	4.093	0.307
Share high-skilled (occ.)	0.102	0.111	0.182	0.148	0.141	0.797
Share high-qual. (educ.)	0.027	0.031	0.584	0.066	0.061	0.351

*p<0.1, **p<0.05, ***p<0.01. °Net of district fixed effects. Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1.

In the Western German sample, municipalities above the distance threshold have lower employment levels than municipalities below the threshold. At first sight, this casts doubt on the exogeneity of the threshold instrument. However, there is a simple and plausible explanation for this observed difference that does not invalidate the underlying logic of the instrument: Municipalities above the threshold are expected to be even more remote than those below the threshold, simply because MDFs need some kind of physical infrastructure, and therefore must be near buildings or roads, which means they must be marginally closer also to workplaces. Hence, it is no surprise to find that municipalities more distant from their MDF have somewhat lower employment levels. In contrast, there are no significant differences in important employment structure variables such as the share of high-qualified workers and the wage level, showing that the two kinds of municipalities do not differ markedly in terms of labor market outcomes.

In the Eastern sample, OPAL areas are larger (in terms of employment) than other municipalities. Again, this finding is no surprise, because the few OPAL areas include some of Eastern Germany's largest cities, e.g. Dresden and Leipzig.[77] Furthermore, the technologically disadvantaged OPAL areas had disproportionately high (!) employment growth rates in 1999. Although I control for municipality-level employment growth in the regressions, this finding calls for a somewhat more cautious interpretation of the findings for Eastern Germany.

All things considered, both IV approaches fall short of a perfect randomization of municipalities. However, the imbalances are small, take on expected signs, and can be accounted for by controlling for the respective covariates. Note, furthermore, that all variables in Table 4.2 are measured at the municipality level, while the employment growth analysis is conducted at the establishment level. Conditional on the municipality-level and establishment-level controls, the found imbalances thus should not severely impair the identification of establishment-level effects.

4.7 Results

4.7.1 Main results

In the following, estimation results are represented mainly for four subsamples. The first sample split is between Western and Eastern Germany, due to the different identification strategies and the systematic geographic difference between both subsamples (rural for Western Germany, urban for Eastern Germany). The second split is between the manufacturing and service sectors, for which heterogeneous effects have been found in most previous studies.

To begin with, I run OLS regressions as a baseline against which the IV regressions can be interpreted. Table 4.3 reports estimates for the Western German manufacturing sector. The employment growth coefficient of DSL availability is negative but only marginally significant, unless year fixed effects are controlled for. The year dummies (omitted for brevity) plausibly indicate that employment growth rates were significantly lower between 2008 and 2010 than between 2005 and 2008. Most

[77] Note that Berlin is not contained in the Eastern German estimation sample (see section on sample restrictions in the Appendix).

other control variables also take on plausible signs. In particular, establishments with relatively high wages and high shares of high-skilled employees grow at faster rates. Furthermore, establishments in municipalities with relatively high employment density grow significantly slower (remember, however, that even these are rather rural municipalities, and that district fixed effects are controlled for). For the Western German service sector, the OLS estimates in Table 4.4 also indicate that DSL expansion had no effect on employment growth. Again, however, including year dummies changes the DSL coefficient substantially, and though insignificant at standard confidence levels, it indicates a positive relationship.

For Eastern German establishments, there is no clear indication of a significant effect of DSL on employment growth; moreover, the DSL coefficient does not differ noticeably between manufacturing (Table 4.5) and services (Table 4.6). Using the most comprehensive specification (column 6) and considering services in particular, I unexpectedly find negative employment growth coefficients for medium-aged (5-10 years) and even for young (<5 years) establishments, as opposed to older establishments. This finding might be due to reduced opportunities for business expansion in the course of the financial crisis of 2008-2009. However, model fit is rather poor for the Eastern sample. A potential reason for this pattern might be the more urban geographic structure of the Eastern sample, meaning that this sample contains a greater diversity of establishments and hence, a larger variation of employment growth, some of which may be explained by unobserved factors. Overall, thus, the estimated specification appears more appropriate and plausible for the Western German sample.

Table 4.3: OLS, West, manufacturing

Log employment growth rate	(1)	(2)	(3)	(4)	(5)	(6)
DSL availability	-0.026	-0.039*	-0.028	-0.000	0.002	0.004
	(-1.292)	(-1.928)	(-1.373)	(-0.002)	(0.125)	(0.177)
Log full-time empl. (estab.)		0.004*	0.005**	0.005**	0.001	-0.004
		(1.690)	(2.042)	(2.192)	(0.231)	(-1.597)
Log median wage (estab.)		0.008	0.008	0.008	0.022	0.051***
		(0.551)	(0.647)	(0.620)	(1.574)	(4.469)
Young estab.		-0.002	0.004	-0.003	-0.003	-0.010
		(-0.161)	(0.352)	(-0.294)	(-0.289)	(-0.997)
Mid-age estab.		-0.000	0.002	0.001	-0.003	-0.006
		(-0.016)	(0.215)	(0.158)	(-0.403)	(-0.946)
Share high-skilled (occ.)		0.040	0.060**	0.061**	0.056*	0.074***
		(1.384)	(2.024)	(2.025)	(1.845)	(2.681)
Log FT empl., municip.			0.003	0.003	0.003	0.004
			(0.989)	(0.911)	(0.773)	(1.165)
Log FT empl. dens., municip.			-0.009**	-0.008**	-0.008**	-0.011***
			(-2.407)	(-2.302)	(-2.186)	(-2.598)
Log mean wage (FT), municip.			0.016	0.003	-0.007	0.013
			(0.643)	(0.107)	(-0.356)	(0.637)
Log full-time empl. growth rate, municip.			0.036	0.031	0.009	-0.013
			(1.273)	(1.053)	(0.361)	(-0.540)
Share high-skilled (occ.), municip.			-0.067	-0.043	-0.036	0.017
			(-1.470)	(-0.937)	(-0.825)	(0.433)
Year FE				Yes	Yes	Yes
Industry FE (3-digit)					Yes	Yes
District FE						Yes
Observations	14823	14823	14823	14823	14823	14823
Adjusted R^2	0.000	0.006	0.008	0.017	0.043	0.065

t statistics in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Constant omitted from output. Standard errors clustered at the municipality level. Data source: Broadband Atlas Germany, BHP7510 v1.

Table 4.4: OLS, West, services

Log employment growth rate	(1)	(2)	(3)	(4)	(5)	(6)
DSL availability	-0.020	-0.021	0.009	0.043	0.048*	0.045
	(-0.630)	(-0.690)	(0.342)	(1.530)	(1.679)	(1.613)
Log full-time empl. (estab.)		-0.002	0.001	0.001	0.001	-0.001
		(-0.409)	(0.434)	(0.448)	(0.392)	(-0.595)
Log median wage (estab.)		0.018	0.012	0.014	0.012*	0.015**
		(1.426)	(1.487)	(1.645)	(1.656)	(2.569)
Young estab.		0.033***	0.035***	0.022**	0.026**	0.023*
		(4.165)	(4.271)	(2.416)	(2.377)	(1.932)
Mid-age estab.		0.015*	0.014**	0.014**	0.011*	0.007
		(1.956)	(1.987)	(2.031)	(1.799)	(1.344)
Share high-skilled (occ.)		0.011	0.017	0.017	0.014	0.014
		(0.758)	(1.278)	(1.225)	(1.069)	(1.115)
Log FT empl., municip.			0.000	0.000	-0.001	-0.004
			(0.038)	(0.067)	(-0.365)	(-1.018)
Log FT empl. dens., municip.			-0.017***	-0.016***	-0.012***	-0.008**
			(-3.175)	(-3.192)	(-3.046)	(-2.004)
Log mean wage (FT), municip.			0.083*	0.068	0.067*	0.097**
			(1.699)	(1.538)	(1.749)	(2.241)
Log full-time empl. growth rate, municip.			0.016	0.030	0.019	0.009
			(0.221)	(0.395)	(0.247)	(0.120)
Share high-skilled (occ.), municip.			-0.038	-0.021	-0.007	-0.014
			(-0.592)	(-0.344)	(-0.144)	(-0.357)
Year FE				Yes	Yes	Yes
Industry FE (3-digit)					Yes	Yes
District FE						Yes
Observations	18650	18650	18650	18650	18650	18650
Adjusted R^2	0.000	0.004	0.011	0.025	0.042	0.056

t statistics in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Constant omitted from output. Standard errors clustered at the municipality level. Data source: Broadband Atlas Germany, BHP7510 v1.

Table 4.5: OLS, East, manufacturing

Log employment growth rate	(1)	(2)	(3)	(4)	(5)	(6)
DSL availability	0.008	0.002	-0.001	0.013	0.012	0.021
	(0.576)	(0.143)	(-0.089)	(0.898)	(0.997)	(1.562)
Log full-time empl. (estab.)		0.005***	0.005***	0.005***	0.001	-0.000
		(3.050)	(2.760)	(2.592)	(0.361)	(-0.211)
Log median wage (estab.)		0.000	-0.003	-0.003	0.004	0.007
		(0.034)	(-0.388)	(-0.348)	(0.452)	(0.796)
Young estab.		0.014	0.014	-0.002	-0.003	-0.003
		(1.351)	(1.285)	(-0.162)	(-0.274)	(-0.257)
Mid-age estab.		0.002	0.002	-0.008	-0.009*	-0.010*
		(0.368)	(0.386)	(-1.469)	(-1.689)	(-1.825)
Share high-skilled (occ.)		0.027***	0.030***	0.031***	0.028*	0.023
		(2.817)	(3.281)	(3.332)	(1.853)	(1.524)
Log FT empl., municip.			-0.003	-0.002	0.002	-0.002
			(-1.079)	(-0.691)	(0.806)	(-0.517)
Log FT empl. dens., municip.			0.001	0.000	-0.003	-0.001
			(0.347)	(0.063)	(-0.954)	(-0.204)
Log mean wage (FT), municip.			0.032	0.023	-0.002	0.018
			(1.413)	(0.986)	(-0.112)	(0.789)
Log full-time empl. growth rate, municip.			0.056	0.070*	0.059*	0.049
			(1.626)	(1.871)	(1.667)	(1.371)
Share high-skilled (occ.), municip.			-0.007	-0.013	-0.008	0.009
			(-0.144)	(-0.267)	(-0.194)	(0.220)
Year FE				Yes	Yes	Yes
Industry FE (3-digit)					Yes	Yes
District FE						Yes
Observations	24138	24138	24138	24138	24138	24138
Adjusted R^2	0.000	0.005	0.005	0.012	0.026	0.032

t statistics in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Constant omitted from output. Standard errors clustered at the municipality level. Data source: Broadband Atlas Germany, BHP7510 v1.

Table 4.6: OLS, East, services

Log employment growth rate	(1)	(2)	(3)	(4)	(5)	(6)
DSL availability	-0.004	-0.006	-0.002	0.009	0.010	0.012
	(-0.527)	(-0.704)	(-0.271)	(1.104)	(1.250)	(1.437)
Log full-time empl. (estab.)		-0.000	0.000	0.001	0.001	0.001
		(-0.023)	(0.083)	(0.337)	(0.688)	(0.558)
Log median wage (estab.)		0.009*	0.009*	0.007	0.011	0.010
		(1.728)	(1.666)	(1.412)	(1.596)	(1.469)
Young estab.		-0.012	-0.012	-0.026***	-0.019**	-0.017**
		(-1.421)	(-1.442)	(-2.995)	(-2.264)	(-1.977)
Mid-age estab.		-0.001	-0.002	-0.009	-0.009*	-0.010*
		(-0.321)	(-0.372)	(-1.625)	(-1.696)	(-1.803)
Share high-skilled (occ.)		-0.001	-0.002	-0.001	-0.006	-0.006
		(-0.151)	(-0.176)	(-0.099)	(-0.357)	(-0.385)
Log FT empl., municip.			-0.001	0.000	0.002	0.003
			(-0.325)	(0.170)	(0.774)	(1.011)
Log FT empl. dens., municip.			-0.004	-0.004	-0.005*	-0.004
			(-1.318)	(-1.394)	(-1.669)	(-1.487)
Log mean wage (FT), municip.			0.033	0.013	0.006	0.003
			(1.435)	(0.564)	(0.296)	(0.140)
Log full-time empl. growth rate, municip.			-0.004	0.054*	0.047	0.030
			(-0.167)	(1.820)	(1.630)	(1.109)
Share high-skilled (occ.), municip.			0.028	0.027	0.040	0.006
			(0.639)	(0.605)	(1.068)	(0.182)
Year FE				Yes	Yes	Yes
Industry FE (3-digit)					Yes	Yes
District FE						Yes
Observations	48753	48753	48753	48753	48753	48753
Adjusted R^2	-0.000	0.000	0.001	0.009	0.015	0.019

t statistics in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Constant omitted from output. Standard errors clustered at the municipality level. Data source: Broadband Atlas Germany, BHP7510 v1.

The small and insignificant results obtained with OLS may indicate that there is no relationship between DSL availability and establishment-level employment growth. However, the OLS estimates may also be biased towards zero due to measurement error in the explanatory variable, as discussed above. Therefore, I turn to the IV estimates in the following. For the Western sample, I find a significantly negative (positive) employment growth effect of DSL for manufacturing (service) establishments, see Table 4.7 and Table 4.8. That is, the IV estimates broadly point in the same direction as the corresponding OLS estimates, but are larger (in absolute terms) and more precisely estimated, potentially due to measurement error in the DSL variable. The estimated coefficients imply that a 10 percentage point increase in DSL availability decreases the average employment growth rate of manufacturers by 2.4 percentage points, and increases that of service firms by 3.3 percentage points. The first-stage estimates indicate that the instrument is strong, with F statistics always above the threshold value of ten proposed by Stock et al., (2002). Furthermore, a modified version of the Durbin-Wu-Hausman test of regressor exogeneity[78] suggests that the DSL variable is indeed endogenous and hence, OLS estimates are biased.

For the Eastern sample, I find no significant effects of DSL availability on employment growth (see Table 4.9 and Table 4.10). Furthermore, the OPAL instrument is less strong, surpassing the conventional threshold of ten only in the more comprehensive specifications. Finally, the test of exogeneity yields insignificant results, suggesting that DSL availability can be considered as exogenous (given that the OPAL dummy is a valid IV). In any case, the findings indicate that there is no significant employment growth effect of DSL in Eastern German establishments. I therefore disregard the Eastern sample for the remainder of the analysis.

[78] Due to the clustering of the variance-covariance matrix, a robust score test by Wooldridge (1995) is performed by the software used for estimation (Stata).

Table 4.7: IV, West, manufacturing

Log empl. growth rate	(1)	(2)	(3)	(4)	(5)	(6)
Second stage						
DSL availability	-0.157*	-0.187**	-0.125	-0.134	-0.162*	-0.237**
	(-1.69)	(-2.20)	(-1.36)	(-1.49)	(-1.87)	(-2.30)
Estab. controls		Yes	Yes	Yes	Yes	Yes
Municip. controls			Yes	Yes	Yes	Yes
Year FE				Yes	Yes	Yes
Industry FE					Yes	Yes
District FE						Yes
Observations	14823	14823	14823	14823	14823	14823
Adjusted R^2	.[a]	.[a]	0.002	0.007	0.030	0.044
First stage						
Adj. R-sq.	0.070	0.090	0.115	0.263	0.342	0.456
Robust F stat.	27.587	31.751	19.221	21.021	42.273	54.294
Prob>F	0.000	0.000	0.000	0.000	0.000	0.000
Test of endog. p-value	0.113	0.052	0.254	0.082	0.044	0.012

t statistics in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Constant omitted from output. Standard errors clustered at the municipality level. [a]Not reported because model sum of squares is negative, a problem often arising in 2SLS estimation. Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1.

Table 4.8: IV, West, services

Log empl. growth rate	(1)	(2)	(3)	(4)	(5)	(6)
Second stage						
DSL availability	-0.028	-0.049	0.284**	0.259**	0.306***	0.325***
	(-0.27)	(-0.45)	(2.36)	(2.30)	(2.64)	(3.14)
Estab. controls		Yes	Yes	Yes	Yes	Yes
Municip. controls			Yes	Yes	Yes	Yes
Year FE				Yes	Yes	Yes
Industry FE					Yes	Yes
District FE						Yes
Observations	18650	18650	18650	18650	18650	18650
Adjusted R^2	0.000	0.004	.[a]	0.007	0.018	0.034
First stage						
Adj. R-sq.	0.060	0.068	0.122	0.210	0.246	0.404
Robust F stat.	32.732	33.754	14.930	16.638	14.869	28.646
Prob>F	0.000	0.000	0.000	0.000	0.000	0.000
Test of endog. p-value	0.931	0.775	0.011	0.034	0.009	0.002

t statistics in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Constant omitted from output. Standard errors clustered at the municipality level. [a]Not reported because model sum of squares is negative, a problem often arising in 2SLS estimation. Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1.

Table 4.9: IV, East, manufacturing

Log empl. growth rate	(1)	(2)	(3)	(4)	(5)	(6)
Second stage						
DSL availability	0.172	0.156	0.107	0.082	-0.019	-0.000
	(1.33)	(0.98)	(1.13)	(1.00)	(-0.20)	(-0.00)
Estab. controls		Yes	Yes	Yes	Yes	Yes
Municip. controls			Yes	Yes	Yes	Yes
Year FE				Yes	Yes	Yes
Industry FE					Yes	Yes
District FE						Yes
Observations	24138	24138	24138	24138	24138	24138
Adjusted R^2	.[a]	.[a]	.[a]	0.008	0.026	0.032
First stage						
Adj. R-sq.	0.009	0.070	0.165	0.273	0.323	0.405
Robust F stat.	4.342	3.256	10.310	11.699	12.415	22.792
Prob>F	0.037	0.071	0.001	0.001	0.000	0.000
Test of endog. p-value	0.121	0.238	0.230	0.379	0.753	0.777

t statistics in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Constant omitted from output. Standard errors clustered at the municipality level. [a]Not reported because model sum of squares is negative, a problem often arising in 2SLS estimation. Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1.

Table 4.10: IV, East, services

Log empl. growth rate	(1)	(2)	(3)	(4)	(5)	(6)
Second stage						
DSL availability	-0.161	-0.121	-0.034	-0.043	-0.057	-0.104
	(-0.19)	(-0.14)	(-0.30)	(-0.39)	(-0.50)	(-1.25)
Estab. controls		Yes	Yes	Yes	Yes	Yes
Municip. controls			Yes	Yes	Yes	Yes
Year FE				Yes	Yes	Yes
Industry FE					Yes	Yes
District FE						Yes
Observations	48753	48753	48753	48753	48753	48753
Adjusted R^2	.[a]	.[a]	-0.000	0.007	0.012	0.012
First stage						
Adj. R-sq.	0.000	0.027	0.139	0.261	0.275	0.386
Robust F stat.	0.104	0.083	4.711	5.271	4.645	14.427
Prob>F	0.748	0.773	0.030	0.022	0.031	0.000
Test of endog. p-value	0.811	0.873	0.772	0.622	0.535	0.101

t statistics in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Constant omitted from output. Standard errors clustered at the municipality level. [a]Not reported because model sum of squares is negative, a problem often arising in 2SLS estimation. Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1.

4.7.2 Knowledge- and computer-intensive industries

The results obtained so far raise the question why the employment growth effects of DSL differ between the manufacturing and service sectors. Since the proposed channel of these effects is the actual use of DSL in establishments, the main reason might be that service firms are more intense users of DSL. Akerman et al. (2015) explicitly test this channel of effects, failing to falsify it against a number of likely alternatives. Thus, the positive employment growth effect of DSL in the service sector might be driven by firms for which information is a key input and ICT is a key technology. To see whether this explanation is sound, I re-run the above IV regressions for two subsets of industries.

First, I consider knowledge-intensive industries as defined by Eurostat (2016).[79] Second, I consider a subsample of computer-intensive industries, splitting the sample at the median of an index defined by Falck et al. (2016). This index is based on the PIAAC survey of adult competencies conducted by the OECD, and it reflects the intensity of computer use by workers in the respective industry.[80] As indicated by Table 4.11, 87 percent of the establishment observations in the Western German sample classified as knowledge-intensive are active in the service sector. Overall, only 17.5 percent of the establishments are considered as knowledge-intensive. In contrast, just 55 percent of the establishments with above-median computer use intensity belong to the service sector.

[79] See Annex 7 “Knowledge Intensive Activities by NACE Rev.1.1” in Eurostat (2016).

[80] PIAAC stands for Programme for the International Assessment of Adult Competencies. The computer use index indicates the frequency with which employees conduct the following tasks at work: Creating or reading spreadsheets; using word-processing software; using programming language; engaging in computer-aided real-time discussions.

Table 4.11: Knowledge- and computer-use intensity by sector

frequency
row percentage
column percentage

	Knowledge intensity			Computer use intensity		
Sector	Non-KI	KI	Total	<median	≥ median	Total
Manufacturing	14,068	755	14,823	6,883	7,940	14,823
	94.91	**5.09**	**100.00**	**46.43**	**53.57**	**100.00**
	50.96	*12.87*	*44.28*	*43.75*	*44.76*	*44.28*
Services	13,537	5,113	18,650	8,850	9,800	18,650
	72.58	**27.42**	**100.00**	**47.45**	**52.55**	**100.00**
	49.04	*87.13*	*55.72*	*56.25*	*55.24*	*55.72*
Total	27,605	5,868	33,473	15,733	17,740	33,473
	82.47	**17.53**	**100.00**	**47.00**	**53.00**	**100.00**
	100.00	*100.00*	*100.00*	*100.00*	*100.00*	*100.00*

Data source: BHP7510 v1.

As one might expect, the employment growth effect of DSL availability is largest (and model fit is best) in the small subsample of knowledge-intensive industries (Table 4.12). Since these industries cluster in the service sector, the overall positive effect in this sector is likely to be driven by knowledge-intensive industries. Regarding computer use, the relatively broad subsample considered in Table 4.13 consists of 45 percent manufacturing establishment observations. Nevertheless, I also find a robustly positive employment growth effect for this subsample, similar in magnitude to the service-sector estimates. These results suggest that the sectoral effect heterogeneity (already found in numerous previous studies) is likely driven by differences in knowledge intensity and computer use. More importantly, this pattern of results supports the interpretation that broadband availability affects employment growth through broadband use, meaning also that the estimated intention-to-treat effects are a suitable proxy for the effect of broadband use on employment growth.

Table 4.12: IV second stage, West, knowledge-intensive industries

Log employment growth rate	(1)	(2)	(3)
DSL availability	0.381*	0.253	0.616***
	(1.86)	(1.39)	(3.12)
Establishment controls	Yes	Yes	Yes
Municipality controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE (3-digit)		Yes	Yes
District FE			Yes
Observations	5868	5868	5868
Adjusted R^2	0.079	0.114	0.135

t statistics in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Constant omitted from output. Standard errors clustered at the municipality level. Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1.

Table 4.13: IV second stage, West, computer-intensive industries

Log employment growth rate	(1)	(2)	(3)
DSL availability	0.237**	0.273**	0.273***
	(2.18)	(2.46)	(3.38)
Establishment controls	Yes	Yes	Yes
Municipality controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE (3-digit)		Yes	Yes
District FE			Yes
Observations	17740	17740	17740
Adjusted R^2	0.014	0.026	0.045

t statistics in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Constant omitted from output. Standard errors clustered at the municipality level. Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1.

4.7.3 Robustness checks

To assess the robustness of the findings obtained so far, I first test whether the estimates are sensitive to the measurement problem inherent to the distance-based IV approach. Every municipality had to be assigned a value of zero or one for the IV, depending on the distance between its centroid and the MDF. Regarding individual establishments, thus, this distance was necessarily measured with error. If a municipality's centroid is just above or just below the threshold, a substantial share of establishments may be assigned a wrong value for the IV. To tackle this issue, I consider two alternative approaches to measuring distance to the MDF, respectively, the degree to which a municipality is subject to technical obstacles. First, following Falck et al. (2014), I use a distance measure based on the municipality's population-weighted centroid, rather than its geographic centroid.[81] Thus, 13 percent of the Western German municipalities change status, mostly from being classified as above the threshold to being classified as below the threshold. This pattern is plausible because MDFs are located in buildings or other man-made structures, and therefore must be closer to the population-weighted than the geographic centroid. For the same reason, when using the population-weighted centroid, slightly more municipalities are found to be less than 4.2 km distant from their nearest (but not their assigned) MDF. As before, I drop these municipalities, resulting in a slightly smaller sample than in the previous estimations.

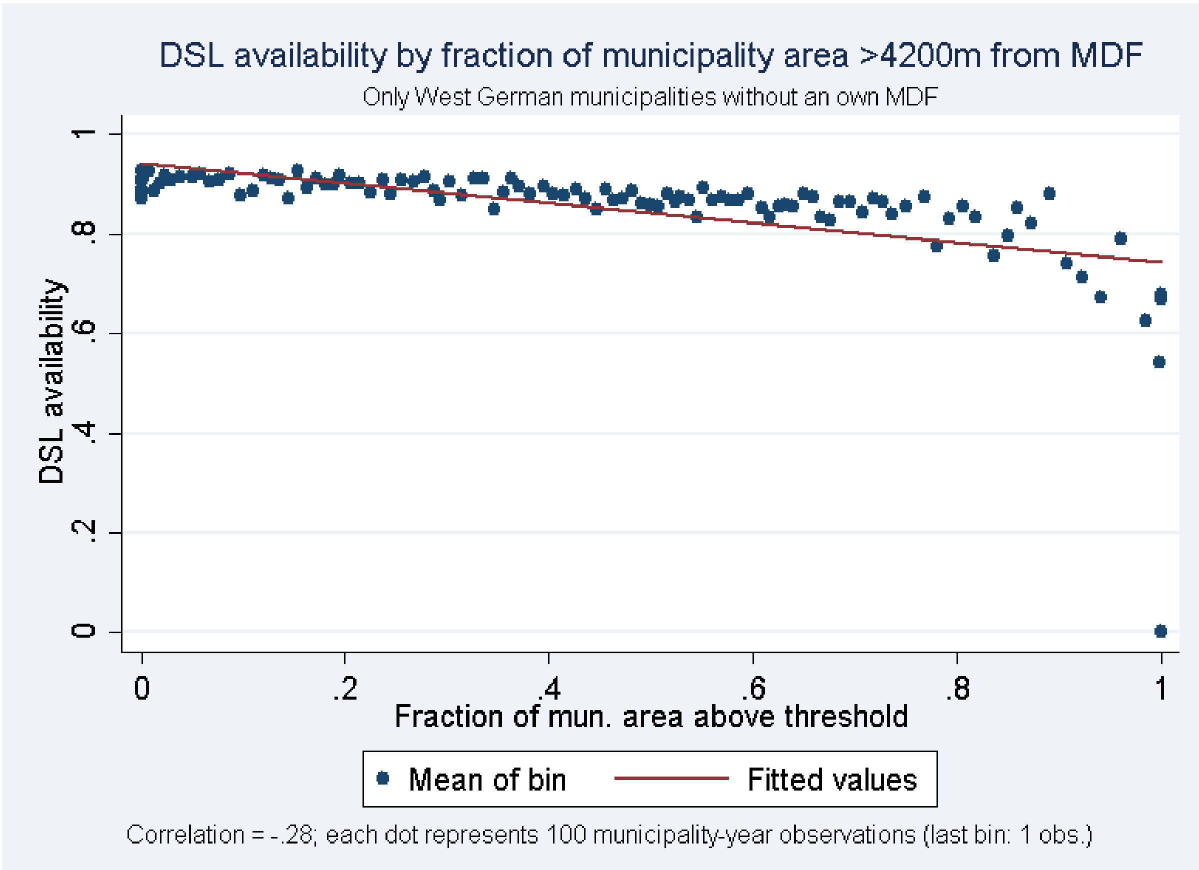
Second, I replace the threshold-dummy IV by the share of the municipality's land area that is more than 4.2 km distant from the assigned MDF, where distance is measured from the geographic municipality threshold.[82] The relationship between this variable and DSL availability is illustrated in Figure 4.5, which shows a significant negative relationship between municipalities' area share above the threshold and DSL availability. In particular, as the share of area above the threshold approaches 100 percent, DSL availability drops drastically. This is a plausible pattern: If almost the entire municipality is above the threshold, then so must be the households in the municipality, for whom DSL availability is measured. In contrast, if a substantial (but not extremely high) share of the municipality's area is above the threshold, this does not necessarily apply to the households, but possibly only to uninhabited territory. Furthermore, the graph reveals that extremely few municipalities are entirely above the distance threshold (taking on the value one on the horizontal axis). This finding reflects the dense and even distribution of MDFs across the country, and thus the exogenous location of MDFs. The share of area above the distance threshold thus should be a relevant instrument. Yet, there is no reason to prefer it over the main IV (the binary threshold dummy), since the land

[81] For details on the identification of population-weighted centroids, see Falck et al. (2014), who use the same data on DSL availability and MDF locations.

[82] On average, for municipalities above the threshold, 70 percent of the municipality area are actually more than 4.2 km distant from the assigned MDF. Similarly, for municipalities classified as below the threshold, 74 percent of the municipality area is actually below the threshold.

area too far from the MDF may not be representative of the area where most establishment are located. Nevertheless, if the 4.2 km threshold is a meaningful source of exogenous variation in DSL availability, then both the binary and the continuous IV should yield similar estimates.

Figure 4.5: DSL availability by fraction of municipality area >4200m from the MDF



Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1.

Table 4.14: Robustness check, West: Population-weighted centroid IV

	Manufacturing			Services		
Log employment growth rate	(1)	(2)	(3)	(1)	(2)	(3)
Second stage						
DSL availability	-0.023	-0.085	-0.143	0.113	0.165	0.194**
	(-0.26)	(-1.04)	(-1.50)	(0.90)	(1.46)	(2.18)
Establishment controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE (3-digit)		Yes	Yes		Yes	Yes
District FE			Yes			Yes
Observations	14029	14029	14029	17706	17706	17706
Adjusted R^2	0.017	0.041	0.060	0.024	0.038	0.052
First stage						
Adj. R-sq.	0.252	0.348	0.463	0.208	0.249	0.410
Robust F stat.	11.405	25.771	42.424	13.784	14.966	30.665
Prob>F	0.001	0.000	0.000	0.000	0.000	0.000
Test of endogeneity p-value	0.792	0.253	0.117	0.586	0.288	0.074

t statistics in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Constant omitted from output. Standard errors clustered at the municipality level. Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1.

Table 4.15: Robustness check, West: Share of area IV

	Manufacturing			Services		
Log employment growth rate	(1)	(2)	(3)	(1)	(2)	(3)
Second stage						
DSL availability	-0.026	-0.095	-0.053	0.032	0.106	0.225***
	(-0.28)	(-1.29)	(-0.74)	(0.29)	(1.11)	(2.87)
Establishment controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE (3-digit)		Yes	Yes		Yes	Yes
District FE			Yes			Yes
Observations	14823	14823	14823	18650	18650	18650
Adjusted R^2	0.016	0.038	0.064	0.025	0.041	0.047
First stage						
Adj. R-sq.	0.264	0.356	0.461	0.245	0.275	0.427
Robust F stat.	16.710	34.463	33.837	17.653	18.604	35.465
Prob>F	0.000	0.000	0.000	0.000	0.000	0.000
Test of endogeneity p-value	0.784	0.167	0.444	0.912	0.534	0.010

t statistics in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Constant omitted from output. Standard errors clustered at the municipality level. Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1.

By and large, the estimates from both robustness exercises are in line with the main results (see Table 4.14 and Table 4.15). Though not statistically significant at conventional levels for manufacturing establishments, the estimated effects of DSL availability take on the same signs as in the main IV regressions, and do not differ substantially in terms of magnitude, at least for the most restrictive specification (column 3). Therefore, the estimates based on the binary distance-threshold IV are not driven by the peculiar measurement of distance between 'the municipality' and the MDF.

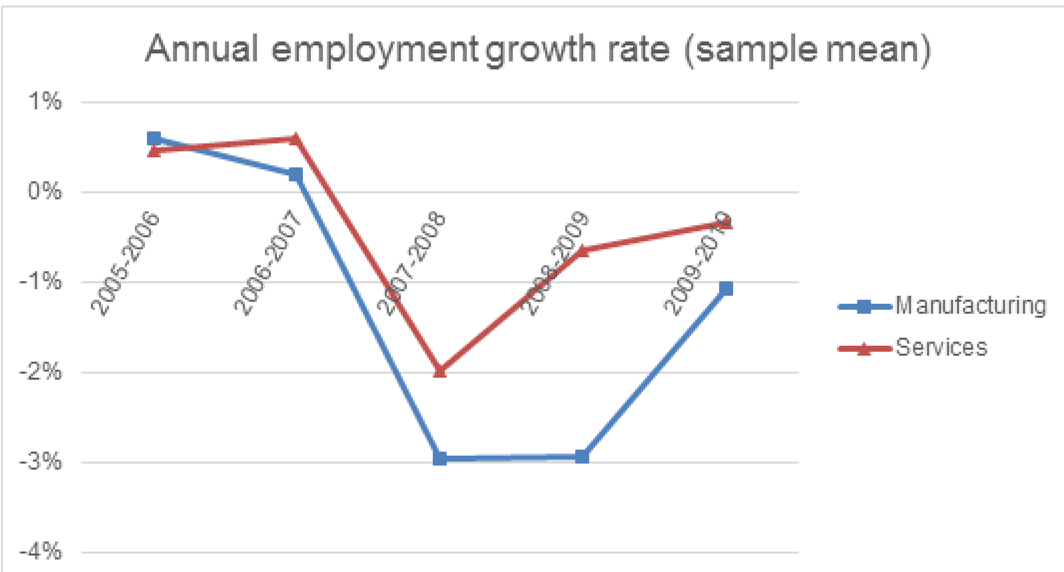
Another robustness check addresses the specific pattern of variation in the explanatory variable. The IV estimates are largely based on spatial variation in DSL availability (because the instruments are time-constant), while OLS uses more intertemporal variation in DSL availability. As argued above, IV is still likely to yield more accurate estimates since it alleviates problems of measurement error in DSL availability. One might object nevertheless that the IV estimates inflate the true effect of broadband on employment growth, since the IV model regresses annual employment growth rates on values of DSL availability which exhibit relatively little intertemporal variation. To address this concern, I estimate an alternative version of the main specification:

$$\ln\left(\frac{L_{it+5}}{L_{it}}\right) = \beta_0 + \beta_1 \widehat{DSL}_{mt} + \boldsymbol{\beta_2 X_{it}} + \boldsymbol{\beta_3 X'_{mt}} + \theta_c + \vartheta_j + \varepsilon_{it}, \qquad (4.3)$$

where t = 2005. That is, instead of using the year-to-year employment growth rate (and hence five annual slices of data), I construct the employment growth rate between the beginning and end of the observation period of employment (i.e. between 2005 and 2010). The regression therefore uses only one observation per establishment (and disregards establishments not observed both in 2005 and 2010). If the IV approach truly captures the causal relationship between DSL availability and employment growth, then the estimates from this regression should be qualitatively similar to the main results.

The results of this robustness exercise are displayed in Table 4.16, panel A. For the service sector, the estimates confirm the significant positive effect of broadband on employment growth. In terms of magnitude, the coefficient is considerably larger – as one would expect, since it measures the effect of DSL availability in 2005 on employment growth between 2005 and 2010. For the manufacturing sector, however, the effect is virtually zero. This finding is most likely due to the negative shock of the global financial crisis in 2008/2009, which hit the export-oriented German manufacturing sector particularly hard.

Figure 4.6: Annual employment growth rate by sector, Western Germany



Data source: BHP7510 v1

Figure 4.6 displays the mean annual employment growth rates for the Western German sample establishments of both sectors, showing that manufacturers suffered particularly sharp employment decreases and returned less quickly to normal growth rates than service firms. Thus, manufacturers experienced near-zero employment growth rates over the period 2005-2010 (on average, 0.8 percent, compared to 6.3 percent for services). Therefore, this five-year interval is a questionable time frame to assess employment growth effects for the manufacturing sector. I therefore estimate equation (4.3) also for the employment growth rate between 2005 and 2007, i.e. in "normal" times (Table 4.16, panel B).[83] The manufacturing-sector estimates remain insignificant,

[83] Note that this shorter observation window yields a slightly larger sample, since there is now a larger number of establishments observed at both the beginning and end of the observation period.

but clearly move towards a consistently negative effect for all specifications. Thus, the main IV results for manufacturers are broadly confirmed, but turn out less robust overall than the results for service establishments.

I report a number of further robustness checks in the Appendix. In Table A.4.3, I drop the top percent of establishments in terms of size (full-time employment). These establishment observations have considerable weight in the above regressions, which are weighted by full-time employment. However, the results are not sensitive to removing these establishments. Only the manufacturing-sector estimates turn out lower and only marginally significant. The service-sector estimates barely move in size and significance. Furthermore, in Table A.4.4 and Table A.4.5 I use full-time, respectively full-time-equivalent, employment rather than total employment to construct the employment growth rate. Again, the results do not change substantially for most specifications. In the service sector, only the full-time equivalent (but not the full-time) employment growth rate reacts significantly to DSL availability. This deviating finding is probably due to a higher share of part-time workers (and hence a larger discrepancy between full-time and full-time equivalent employment) in services, which however makes full-time employment a less interesting outcome for service establishments in the first place.

To summarize the results, it can be stated that DSL availability appears to have accelerated employment growth in Western German service establishments. This effect is probably driven by knowledge-intensive industries, which concentrate in the service sector. Similarly, computer-intensive industries (which comprise also a substantial share of manufacturing industries) grew faster in response to the expansion of first-generation broadband. The manufacturing sector at large, in contrast, tended to experience negative growth effects, although the evidence is somewhat less robust for this sector. For Eastern Germany, the analysis did not find any statistically significant employment growth effects due to increased DSL availability.

Table 4.16: Robustness check, West: long-run employment growth rates

	Manufacturing			Services		
A: 2005-2010	(1)	(2)	(3)	(1)	(2)	(3)
Second stage						
DSL availability	0.826	0.236	-0.505	1.620*	1.708*	0.923*
	(0.47)	(0.27)	(-0.72)	(1.66)	(1.91)	(1.84)
Establishment controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE (3-digit)		Yes	Yes		Yes	Yes
District FE			Yes			Yes
Observations	2520	2520	2520	2983	2983	2983
Adjusted R^2	0.119	0.693	0.758	.[a]	.[a]	0.138
B: 2005-2007	(1)	(2)	(3)	(1)	(2)	(3)
Second stage						
DSL availability	-0.703	-0.777	-0.534	0.938	0.926*	0.877**
	(-1.30)	(-1.46)	(-1.34)	(1.58)	(1.66)	(2.05)
Establishment controls	Yes	Yes	Yes	Yes	Yes	Yes
Municipality controls	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Industry FE (3-digit)		Yes	Yes		Yes	Yes
District FE			Yes			Yes
Observations	2775	2775	2775	3379	3379	3379
Adjusted R^2	.[a]	.[a]	0.171	.[a]	0.204	0.307

t statistics in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Constant omitted from output. Standard errors clustered at the municipality level. [a]Not reported because model sum of squares is negative, a problem often arising in 2SLS estimation. Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1.

4.8 Limitations and outlook

The main limitation of the results in this study concerns external validity: Do the effects found, in particular the findings for the sample of rural Western German municipalities, extend to urban Western Germany? This question is of interest because the bulk of establishments and employees are located in urban municipalities which are not included in the estimation sample, and because previous studies find different effects for rural and urban regions (where no consensus has been reached yet, see e.g. Fabritz, 2013 versus What Works Centre for Local Economic Growth, 2015). With the data at hand, I cannot address this question. The finding that knowledge- and computer-intensive industries are particularly (positively) affected by broadband expansion, however, seems likely to extend beyond the limited geographical scope of this study.

Furthermore, given the available data, I can only investigate the effects of broadband availability on labor demand at the margin of employment, but not work hours. In the short run, which this analysis focuses on, broadband may not increase (or decrease) labor demand at both margins, let alone at the same time. Instead, adjustments of hours might take place before adjustments in the number of employees. The employment growth effects found above thus might understate the total short-run labor demand effects of broadband expansion. In the longer run, in contrast, broadband expansion in the sense of increasing availability is likely to be less important economically, and less relevant to policymakers, than the increasing bandwidths (data transfer rates) of existing broadband connections (see Ahlfeldt et al., 2017, for an assessment of bandwidth in the context of property value). Further research thus should consider newer generations of broadband technology.

Another interesting question raised by the results of this study is whether broadband expansion triggered a reallocation of employment from manufacturing towards services and knowledge- and computer-intensive industries. The data and methodology used in this study are a suitable base for investigating this question, but they would need to be complemented by individual-level or linked employer-employee data. Using such data, one could also investigate the skill heterogeneity of broadband effects in Germany – that is, whether broadband yields gains for the high-skilled and losses for the low-skilled (as found by Akerman et al., 2015). Furthermore, one might consider studying the heterogeneity of broadband effects with respect to age groups. Recent empirical studies point to an age bias in ICT proficiency, relevant for labor market outcomes, with young workers ("digital natives") being more apt than older workers (Falck et al., 2016), and to an increased reallocation of young workers into ICT-intensive sectors (Autor and Dorn, 2009). Further research should explore these dimensions of effect heterogeneity.

4.9 Conclusions

This chapter investigates the effect of local broadband internet availability on the employment growth of German business establishments. To obtain credible causal estimates, technical frictions which impeded the rollout of broadband in rural Western Germany and urban Eastern Germany are used to construct instrumental variables for broadband availability. In line with previous studies, the empirical findings provide evidence of a positive employment growth effect for service-sector establishments; in contrast, I find negative employment growth effects for manufacturers. Seeking to explain this divergence of effects, I find positive employment growth effects for knowledge- and computer-intensive industries, which tend to be concentrated in the service sector. This pattern of results likely reflects a higher importance of knowledge as a factor of production, and computers as a core technology, in the service sector.

For Eastern Germany, I find no significant employment growth effects of broadband expansion in either sector. This finding could be due to the different geographic structure of the Eastern German sample (which is much more urban than the Western German sample, by the design of the IV approach), but also to the differences in industrial structure and technological development between both parts of the country. The internal validity of the findings is supported by a number of robustness checks, including the use of alternative instruments. However, one can only speculate about external validity, that is, whether the significant employment growth effects found for rural Western Germany apply to urban Western Germany, too.

The findings raise questions to be addressed by further research, for instance whether the opposing employment growth effects for manufacturing and services reflect a reallocation of workers between the two sectors. This question calls for additional analyses at the individual level, which could also address individual-level effect heterogeneity, in particular, between skill and age groups. Related empirical studies suggest that the employment and wage gains from broadband expansion fall disproportionately to the high-skilled, and potentially also to younger workers. In this context, the German experience might be of particular interest to policymakers, given the importance of medium-skilled workers trained in the apprenticeship system, and the ageing German population.

5 Summary and conclusion

5.1 Empirical findings

This dissertation investigates selected causes and effects of worker mobility between firms in three empirical studies for Germany. Chapter 2 investigates the productivity effects of worker inflows to manufacturing establishments, distinguishing inflows by their previous employers' wage level, as a proxy for productivity. The chapter is motivated by several empirical studies which find that worker inflows from more productive or higher-paying firms increase hiring firms' productivity. The analyses in chapter 2 are based on a unique linked employer-employee data set. The findings indicate that inflows from higher-paying establishments do not increase hiring establishments' productivity, but inflows from lower-paying establishments do. Further analyses suggest that this effect is due to a positive selectivity of such inflows from their sending establishments. These findings can be interpreted as evidence of a reallocation process by which the best employees of lower-paying establishments become hired by higher-paying establishments. This process reflects the assortative pattern of worker mobility in Germany documented by Card et al. (2013) for the past decades. The chapter thus contributes to the literature by linking establishment-level productivity analysis to the assortative pattern of inter-firm worker mobility, thereby providing a micro-foundation for the latter.

Chapter 3 focuses on a positive selection of workers moving between firms from another, more specific perspective. The analysis focuses on the importance of regional labor market competition for establishments' apprentice training and poaching of apprenticeship completers. Previous studies have found that firms provide less training if they are located in regions with strong labor market competition. This finding is usually interpreted as evidence of a higher risk of poaching in these regions. Yet, there is no direct evidence that regional competition is positively correlated with poaching. Building on a recently established approach to ex-post identify poaching of apprenticeship completers, this chapter is the first to directly investigate the correlation between regional labor market competition and poaching. Using German administrative data, it is found that competition indeed increases training establishments' probability of becoming poaching victims. However, poaching victims do not change their apprenticeship training activity in reaction to poaching. Instead, the findings indicate that the lower training activity in competitive regions can be attributed to lower retention rates, as well as a less adverse selection and lower labor and hiring costs of apprenticeship completers hired from rivals.

Chapter 4 investigates the effects of local broadband internet availability on establishment-level employment growth. The analysis uses data for Germany in the years 2005-2009, when broadband was introduced in rural regions of Western Germany and in large parts of Eastern Germany. Technical frictions in broadband rollout are exploited to obtain exogenous variation in local broadband availability. The results suggest that broadband expansion had a positive effect on employment growth in the Western German service sector and a negative effect in Western German manufacturing, suggesting that broadband expansion has accelerated the reallocation of workers from manufacturing to services. Furthermore, this pattern of results is driven by pronounced positive effects in knowledge- and computer-intensive industries, suggesting that it is the actual use of broadband in the production process that leads to complementary hiring, respectively a slow-down of employment growth, in the respective sectors. For Eastern Germany, no significant employment growth effects are found.

5.2 Synopsis

To conclude this thesis, I would like to recall the issues raised in the introduction and to reflect the results of the three empirical chapters against this background. This wrap-up is intended to help assess the contribution of the three empirical chapters to the literature on worker mobility between firms, which is much broader than the focus of the three previous chapters would suggest.

First, it has been shown at the outset that worker mobility between firms represents one of the most important margins of reallocation in the labor market, both in terms of quantity and regarding its implications, for instance, for aggregate productivity and growth. It is this margin of worker flows that all three empirical studies focus on. Using longitudinal linked employer-employee data and applying micro-econometric methods, the three studies illustrate the importance of inter-firm worker flows for outcomes such as firm productivity, as well as the factors underlying these flows, such as firm and worker heterogeneity, regional labor market competition, and technological conditions (exemplified by broadband infrastructure).

Second, in the introduction it was emphasized that labor market regulation shapes worker mobility. In Germany, which the empirical studies focus on, a relatively strict labor market regulation accounts for a relatively low level of worker mobility between firms, and a somewhat selective or even polarized structure of worker mobility (cf. Gielen and Tatsiramos, 2012). This polarization could be characterized as follows: On the one hand, there are sought-after, highly productivity workers exploiting better job opportunities; on the other hand, displaced workers with potentially poor further employment prospects (cf. ibid.). Focusing on the former group in particular, the three empirical studies illustrate the importance of selective worker mobility from different angles.

In Chapter 2, it is found that workers moving from lower-paying to higher-paying firms are positively selected in terms of their individual wage level. Such worker flows are furthermore found to increase productivity at the destination firm. At the same time, these flows contribute to the increase in wage inequality in Germany during the observation period. One way of interpreting these results is that, in a country characterized by rather strict labor market regulation, workers who voluntarily quit their job are positively selected, as emphasized by Gielen and Tatsiramos (2012). The most productive workers, moreover, might have to switch to higher-paying firms in order to increase their wages beyond a certain point. Reversely, high-paying and highly productive firms may need to hire the ‘best’ workers from competitors in order to further increase their productivity and stay ahead of competitors. The latter aspect is addressed more explicitly in chapter 3, where poaching of apprenticeship completers is analyzed as a selective hiring strategy targeting particularly productive workers.

Chapter 3 exploits the strict regulation of apprentice training in Germany, which makes poaching of apprenticeship completers feasible and, given plausible assumptions, observable. In line with theoretical expectations and related literature, it is found that poaching is a result of (inter alia) regional labor market competition. The chapter thus provides novel empirical insights on how competition in the labor market shapes processes of worker mobility. Furthermore, chapter 3 emphasizes the importance of regional disparities, including not only competition but also agglomeration economies (such as labor pooling), for the amount and characteristics of worker mobility between firms. At least regarding apprenticeship completers, regional competition between employers and agglomeration are found not only to increase worker mobility, but also to improve the average quality of moving workers (as identified by wages). Chapter 3 thus highlights that, besides national regulation, regional labor market structure is an important determinant of the quantity and quality of workers moving between firms.

Chapter 4 does not explicitly address the quality and potential selectivity of moving workers. However, the findings point to a possible reallocation of workers, triggered by broadband internet expansion, from most manufacturing industries towards services and knowledge-intensive industries. In line with the idea that workers do not select randomly into the latter group of industries, related studies find that highly qualified and young workers benefit most from advancements in information technology. In order to reap these benefits, these groups have been found to reallocate into more knowledge-intensive industries, unlike less qualified and older workers (e.g. Autor and Dorn, 2009). The results of chapter 4 thus possibly reflect a positive selection of workers moving out of declining and into prospering industries. Against the background of worker mobility being rare and selective in strictly regulated labor markets, as well as usually concentrated within industries, the chapter therefore highlights technological progress as another important force shaping the structure of inter-firm worker mobility.

A third important insight presupposed but not called into question in the empirical studies is that labor markets are to a large degree local, particularly but not exclusively in a geographical sense. This 'localness' primarily stems from the limited mobility of workers, not just in a spatial sense but also regarding non-spatial barriers, such as occupational qualification requirements.

Despite not explicitly conditioning on a particular dimension of 'localness,' chapter 2 reproduces an important finding that recent research has shed light on, namely that the localized structure of labor markets can be regarded as endogenously determined by worker flows. In line with the literature on endogenous labor markets (notably Schmutte, 2014), chapter 2 finds that highly-paid workers sort into high-paying firms. These findings point to productivity and other (endogenous) economic performance indicators, as opposed to regions, industries, and occupations (largely exogenous categories), as relevant dimensions of labor market 'localness'. In this sense, the findings from chapter 2 are in line with recent contributions emphasizing the endogeneity of local labor markets.

Chapter 3 highlights the varying degree of competition between employers across regional occupational labor markets. Descriptive findings support the notion that the labor market for apprenticeship completers is localized in the dimensions geography and occupation. The chapter therefore accounts for localness as a multi-dimensional concept. As the literature on endogenous local labor markets suggests, the particular concept of labor market localness invoked in empirical analyses should be adequately motivated. In chapter 3, it is argued extensively that geographical and occupational boundaries delineate labor markets for apprenticeship completers. The latter dimension has been largely neglected in previous studies. The chapter thus contributes to the literature in that it provides a well-motivated delineation of local labor markets and finds that the varying degree of competition across these labor markets accounts for differences in inter-firm worker mobility within them.

Finally, in chapter 4, broadband internet is analyzed as a locally supplied technology. Local broadband infrastructure can be regarded as an exogenous determinant of labor market outcomes, given an exogenous source of variation, at least in the short run. Importantly, this perspective considers broadband infrastructure as a technological condition that policy (a largely exogenous factor) can influence. Thus, notwithstanding the ‘endogenous localness’ of labor markets, the chapter highlights that policy can affect labor markets not only through national regulation, but also at the local level, and that relevant policy interventions include not only labor market regulation, but a broader set of policies.

It should be mentioned that despite the complementary literature review in the introduction and the empirical contributions of chapters 2-4, the list of topics related to inter-firm worker mobility discussed in this thesis remains incomplete. For instance, worker-firm matching is not addressed in detail, except implicitly in the analysis of worker-firm sorting in chapter 2. Furthermore, to understand the causes and effects of worker mobility between firms comprehensively, research on this topic needs to look also outside the labor market and labor market policy. This thesis has focused merely on a selection of such causes and effects, as well as on processes within the labor market (with the exception of broadband internet expansion as an underlying technological change). Ongoing and future research, notably in the fields of regional and urban economics, should improve our understanding of worker mobility between firms by appreciating the relevance of related markets and policy areas, including product markets as well as housing, firm location, and infrastructure in a broad sense.

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Appendix to chapter 2

Table A.2.1: Log wage regressions (for the year 2000)

	Coefficient	Standard error
male	0.1809	0.0002
low-skilled	-0.1764	0.0004
mid-skilled	-0.1299	0.0003
high-skilled	(omitted)	
age	0.0324	0.0001
age squared	-0.0003	0.0000
N (individuals)	7,378,477	
n (establishments)	70,873	
R-sq.	0.4433	
rho (fraction of residual variance due to establishment fixed effects)	0.6746	

Years 2001 sqq. omitted for brevity (very similar results). Sample: all qualified full-time workers at sending and hiring establishments, as of June 30th. All regressions include establishment, 2-digit occupation, and occupation status fixed effects. Data Source: Establishment History Panel; own calculations.

Table A.2.2: System-GMM regressions, Eastern German establishments only

	(N)SPs defined by FE	(N)SPs defined by MW	(N)SPs defined by FE	(N)SPs defined by MW
L.Log value added	0.411***	0.428***	0.367***	0.410***
L2.Log value added	0.075	0.080*	0.033	0.055
Log capital stock	0.082	0.082	0.064	0.042
Log labour	0.472***	0.423***	0.557***	0.524***
Share high-qual. inflows	-0.057	-0.079	-0.036	-0.050
Mean age inflows	0.009	0.010*	0.009	0.010*
Mean age sq. inflows	-0.000*	-0.000**	-0.000*	-0.000
Labor share SPs	-0.887	-1.085	-0.632	-1.115
Labor share Non-SPs	-1.353	-3.261	-1.902	-2.811
Mean wage pos. SPs			-0.097	0.003
Mean wage pos. Non-SPs			-0.020	-0.008
Observations	2113	2113	2113	2113
Sargan p-value	0.822	0.887	0.8	0.956

Dependent variable is log value added. Standard errors clustered at establishment level. Year dummies included. EMPL and inflow control variables included. All regressions include a constant. *p<0.1, **p<0.05, ***p<0.01. Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

Table A.2.3: OLS estimates, years 2002-2010

	All inflows	(N)SPs defined by FE	(N)SPs defined by MW
L.Log value added	0.556***	0.555***	0.555***
L2.Log value added	0.192***	0.191***	0.192***
Log capital stock	0.032***	0.031***	0.032***
Log labour	0.225***	0.228***	0.226***
Share high-qual. inflows	-0.051**	-0.046*	-0.048**
Mean age inflows	0.009	0.002	0.001
Mean age sq. inflows	-0.000	-0.000	-0.000
Labor share inflows	0.318		
Labor share SPs		0.071	0.057
Labor share Non-SPs		0.791**	0.604**
Observations	7278	7278	7278
R-squared	0.953	0.953	0.953

Dependent variable is log value added. Standard errors clustered at establishment level. Year, 2-digit industry and labor market region (LMR) dummies included. ESTAB and EMPL control variables included. All regressions include a constant. *p<0.1, **p<0.05, ***p<0.01. Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

Table A.2.4: System-GMM estimates, years 2002-2010

	(N)SPs defined by FE	(N)SPs defined by MW	(N)SPs defined by FE	(N)SPs defined by MW
L.Log value added	0.318***	0.317***	0.332***	0.325***
L2.Log value added	0.019	0.011	0.021	0.019
Log capital stock	0.145***	0.157***	0.115***	0.111***
Log labour	0.530***	0.538***	0.566***	0.566***
Share high-qual. inflows	-0.013	-0.018	-0.006	-0.000
Mean age inflows	0.001	-0.000	-0.000	-0.002
Mean age sq. inflows	-0.000	0.000	0.000	0.000
Labor share SPs	-0.724	0.181	0.359	0.778
Labor share Non-SPs	2.125	1.688	1.496	1.295
Mean wage pos. SPs			-0.032	-0.008
Mean wage pos. Non-SPs			0.029	0.061
Observations	7278	7278	7278	7278
Sargan p-value	0.386	0.324	0.238	0.423

Dependent variable is log value added. Standard errors clustered at establishment level. Year dummies included. EMPL control variables included. All regressions include a constant. *p<0.1, **p<0.05, ***p<0.01. Data Source: Integrated Employment Biographies, Establishment History Panel, IAB Establishment Panel and Employment Statics; own calculations.

Econometric issues of production function estimation

In a very comprehensive paper, Eberhardt and Helmers (2010) (hf. EH) review the most important problems encountered by econometricians using "fat" panel data (large N, short T) to estimate firm-level production functions. We refer to their paper for its comprehensiveness and emphasis on the imperfections of the data typically used (availability and quality of output and capital data, need for proxies, etc.).

EH argue that unobserved total factor productivity (TFP) is composed of firms' mean efficiency, period-specific effects, firm-specific effects, and an idiosyncratic component, and since the latter is observed by the firm but not the econometrician, there can be unobserved factors influencing firms' input choices, implying that failing to control for these factors renders OLS and fixed-effects estimates inconsistent. More explicitly, the main problem arises from the possibility of the firm to observe its idiosyncratic TFP shock *before* choosing its levels of capital and labor; the idiosyncratic effect thus is an omitted variable that needs to be controlled for. Otherwise, it is being transmitted to the observed inputs (capital and labor), i.e. the production factors' coefficients take up the idiosyncratic effect and are thus biased upward. In contrast, a downward bias can result from imprecise measurement of inputs (attenuation bias). The idiosyncratic TFP shock represents, above all, simultaneity or reverse causality, i.e. the simultaneous or reversed determination of factor inputs with respect to the realized output.

EH discuss three approaches to combat these endogeneity biases. The first approach, instrumenting factor inputs using factor prices, can be ignored in the case of our study. Instead, we focus on the problem of endogeneity (reverse causality) bias arising from establishments' anticipation of their productivity level and their according choice of inputs. The two main approaches to minimize this bias are, first, control function approaches trying to model the idiosyncratic TFP shock explicitly, and second, dynamic panel data (DPD) approaches making use of internal instruments in panel data sets. The first class of estimators has been developed by Olley and Pakes (1996) (OP), Levinsohn and Petrin (2003) (LP), Ackerberg et al. (2006) (ACF), and Wooldridge (2009) (WOP); the second class is rooted in the work of Arellano and Bond (1991) (AB) and Blundell and Bond (1998, 2000) (BB).

To construct the control function for the idiosyncratic TFP shock observed by the firm but not the researcher, OP, LP, ACF, and WOP need to assume that this shock is the only unobservable entering the investment (respectively, intermediate inputs) function. This "scalar unobservable assumption" (EH) cannot be tested. More specifically, to identify the labor coefficient, which should be more important, given our core explanatory variables, than identifying the capital coefficient, the structural estimators assume a discrete sequence of establishments' decisions about the particular factor inputs. Again, this assumption cannot be tested empirically (EH, p. 24). At best, the assumption could be plausible in some particular production processes (industries), but we do not expect it to hold across the entire manufacturing sector (let alone other sectors).

Using the longitudinal dimension of panel data, the DPD estimators control for time-invariant unobserved establishment heterogeneity. This eliminates omitted variable bias. However, the bias due to unobserved productivity shocks would be removed only if these were time-constant. The DPD estimators indicated above, by using internal IVs, take an additional step to combat this endogeneity bias. Furthermore, unlike the "structural" estimators (OP, LP, etc.), the DPD estimators allow one to test all crucial assumptions made about the data-generating process (DGP). It could thus be argued that, overall, the DPD estimators are a more conservative choice than any of the "structural" (control function) estimators. On the other hand, due to using only within-establishment variation in a fat panel, one may fail to identify effects with any precision using these estimators. Aiming to maximize the robustness of our findings, we employ both classes of estimators.

Appendix to chapter 3

Table A.3.1: Summary statistics for apprenticeship completers.

A: Poaching sample	**Mean**	**SD**	**Min**	**Max**	**N**
Stayer	0.925	0.264	0.000	1.000	134602
Interregional mover	0.212	0.408	0.000	1.000	10127
Poached mover	0.084	0.278	0.000	1.000	10127
Interregional poaching	0.299	0.458	0.000	1.000	855
Age	21.479	2.010	17.000	52.000	134602
Female	0.338	0.473	0.000	1.000	134602
Duration of apprenticeship	1068.275	159.715	700.000	1492.000	134602
Training wage	32.923	9.030	8.915	82.053	134602
First-job wage	80.274	17.706	10.406	490.424	134602
Wage difference job - training	47.350	17.279	-32.538	461.585	134602
Training wage rel. to cell mean	-0.000	3.856	-41.135	43.072	134602
B: Baseline sample					
Stayer	0.886	0.318	0.000	1.000	196697
Interregional mover	0.275	0.447	0.000	1.000	22415
Age	21.535	2.109	17.000	53.000	196697
Female	0.340	0.474	0.000	1.000	196697
Duration of apprenticeship	1068.327	158.556	700.000	1492.000	196697
Training wage	34.126	10.241	8.823	96.593	196697
First-job wage	82.799	21.425	10.011	482.386	196697
Wage difference job - training	48.673	19.161	-40.956	447.772	196697
Training wage rel. to cell mean	0.000	3.648	-48.575	48.071	196697

Data source: BEH V.09.

Table A.3.2: Summary statistics for establishments (poaching sample).

Establishments (poaching sample)	**Mean**	**SD**	**Min**	**Max**	**N**
Apprentices	27.966	64.855	0.000	1944.000	27039
Apprenticeship completers	6.285	9.736	1.000	356.000	21416
Movers	0.473	2.338	0.000	73.000	21416
Poachings	0.040	0.383	0.000	22.000	21416
Ext. appr. completer hires	1.754	2.986	1.000	65.000	5623
Raidings	0.145	0.615	0.000	18.000	5623
log estab. density, region-occupation	-0.823	1.641	-7.375	3.315	27039
log estab. density, region-industry	-2.445	1.620	-8.417	1.959	27039
Employees	629.158	1889.555	3.000	53391.000	27039
Full-time employment	523.681	1675.053	1.000	49438.000	27039
Share mid-qual. employees	0.758	0.152	0.000	1.000	27039
Employment growth rate	0.026	0.135	-0.317	1.313	27039
Median full-time daily wage	103.747	26.891	18.079	187.133	27039
East Germany	0.157	0.364	0.000	1.000	27039
log employment LM region	12.676	0.913	9.875	14.201	27039
log empl. density LM region	4.697	0.779	2.540	6.679	27039
Share apprentice hires	0.027	0.021	0.000	0.226	21416

Apprenticeship completers, movers and poached employees only defined for training establishments. External apprenticeship completer hires and raided employees only defined for external hiring establishments. Data source: BEH V.09 and BHP 7510 v1.

Table A.3.3: Summary statistics for establishments (baseline sample).

Establishments (baseline sample)	Mean	SD	Min	Max	N
Apprentices	15.668	45.178	0.000	1944.000	72402
Apprenticeship completers	3.373	7.011	1.000	367.000	58632
Movers	0.401	2.299	0.000	171.000	58632
Ext. appr. completer hires	1.534	3.454	1.000	192.000	13770
log estab. density, region-occupation	-0.737	1.572	-7.375	3.315	72402
log estab. density, region-industry	-1.964	1.708	-8.625	2.129	72402
Employees	336.062	1274.026	1.000	53391.000	72402
Full-time employment	275.059	1121.906	0.000	49438.000	72402
Share mid-qual. employ-ees	0.748	0.187	0.000	1.000	72402
Employment growth rate	0.038	0.265	-1.000	6.000	72402
Median full-time daily wage	84.696	27.805	1.430	245.400	72136
East Germany	0.201	0.400	0.000	1.000	72402
log employment LM re-gion	12.647	0.934	9.875	14.201	72402
log empl. density LM re-gion	4.646	0.788	2.540	6.679	72402

Apprenticeship completers and movers only defined for training establishments. External apprenticeship completer hires only defined for external hiring establishments. Data source: BEH V.09 and BHP 7510 v1.

Table A.3.4: Training wage differences between apprenticeship completers who move and stay with their training employers, poaching sample.

	(1) log training wage	(2) log training wage	(3) log training wage	(4) Training wage rel. to cell mean
Mover	-0.00630	-0.00819	-0.0104	-0.256**
	(-0.62)	(-0.84)	(-1.05)	(-2.24)
log full-time em-ployment, estab.		0.0401***	0.0359***	
		(11.10)	(9.13)	
log empl. density, region			0.0242***	
			(4.04)	
Constant	3.377***	3.115***	3.017***	0.00511
	(85.76)	(57.05)	(53.11)	(1.08)
Observations	134602	134602	134602	134602
Adjusted R^2	0.204	0.247	0.252	-0.000

t statistics in parentheses. All estimations include 2-digit occupation fixed effects. Standard errors clustered at region-occupation level (labor market regions, 2-digit occupations). * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

Table A.3.5: Training wages by regional labor competition, baseline sample.

	(1) log training wage	(2) log training wage	(3) log training wage
log firm density reg.-occ.	0.0422***	0.0238***	0.0208***
	(14.86)	(8.47)	(8.05)
log full-time employment		0.0467***	0.0464***
		(19.76)	(19.17)
log employment, region			0.00682
			(1.44)
Constant	3.394***	3.200***	3.105***
	(45.84)	(44.50)	(33.20)
Observations	196697	196500	196500
Adjusted R^2	0.275	0.337	0.338

t statistics in parentheses. Reduction of observation number in column 2 due to establishments with zero full-time employees. All estimations include 2-digit occupation fixed effects. Standard errors clustered at region-occupation level (labor market regions, 2-digit occupations). * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

Table A.3.6: Standard deviations of training wages in cell by regional labor competition, baseline sample.

	(1) Within-cell SD of training wages	(2) Within-cell SD of training wages	(3) Within-cell SD of training wages
log firm density reg.-occ.	0.155***	0.0558***	0.0211
	(12.24)	(5.01)	(1.60)
log full-time employment		0.329***	0.329***
		(27.12)	(27.35)
log employment, region			0.0834***
			(4.34)
Constant	0.475***	-0.793***	-1.960***
	(11.47)	(-6.97)	(-6.23)
Observations	83510	83314	83314
Adjusted R^2	0.055	0.099	0.100

t statistics in parentheses. Reduction of observation number in column 2 due to establishments with zero full-time employees. All estimations include 2-digit occupation fixed effects. Standard errors clustered at region-occupation level (labor market regions, 2-digit occupations). * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

Table A.3.7: Differences between first skilled wages and training wages by labor market competition, baseline sample.

	(1) log wage diff. job-training	(2) log wage diff. job-training	(3) log wage diff. job-training
log firm density reg.-occ.	0.0398***	0.0128***	0.00806***
	(11.68)	(4.09)	(2.72)
log full-time employment		0.0715***	0.0711***
		(23.26)	(22.90)
log employment, region			0.0115**
			(2.01)
Constant	3.653***	3.359***	3.198***
	(30.41)	(27.97)	(22.49)
Observations	195752	195539	195539
Adjusted R^2	0.142	0.201	0.202

t statistics in parentheses. Reduction of observation number in column 2 due to establishments with zero full-time employees. All estimations include 2-digit occupation fixed effects. Standard errors clustered at region-occupation level (labor market regions, 2-digit occupations). * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

Table A.3.8: Impact of regional competition on apprenticeship training, baseline sample.

	(1) log apprentices	(2) log apprentices	(3) log apprentices
log estab. density, region-occupation	-.0433***	-.0496***	-.0455***
	(-5.838)	(-7.043)	(-6.502)
Log labor (full-time)	.5487***	.5703***	.5709***
	(82.59)	(80.88)	(79.74)
Share mid-qual. employees	.1398***	.1219***	.1241***
	(6.29)	(6.199)	(6.161)
Employment growth rate	.0898***	.0874***	.0864***
	(4.391)	(4.257)	(4.224)
log median daily wage	-5.4e-04	.0149	.0035
	(-.0238)	(.6468)	(.1368)
log employment LM region	-.007	-.0121	.374***
	(-.616)	(-1.283)	(2.669)
log empl. density LM region	.0189	.0192	.0024
	(1.322)	(1.471)	(.0302)
Constant	-.7948***	-.0101	-4.638***
	(-5.705)	(-.0478)	(-2.988)
Observations	58436	58436	58436
Adjusted R^2	.695	.71	.714

t statistics in parentheses. All estimations include 2-digit occupation fixed effects. Columns 2-3 includes 2-digit industry and year fixed effects. Column 3 includes labor market region fixed effects. Standard errors clustered at the region-industry level (labor market regions, 2-digit industries). * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

Table A.3.9: Impact of regional competition on retention, baseline sample.

	(1) log movers	(2) log movers	(3) log movers
log estab. density, region-occupation	.0238***	.0189***	.0198***
	(3.784)	(3.222)	(3.4)
log apprentices	.1161***	.1139***	.1142***
	(18.04)	(17.42)	(17.69)
Log labor (full-time)	-.0421***	-.0393***	-.0406***
	(-10.98)	(-10.65)	(-10.87)
Share mid-qual. employees	.0013	-6.4e-04	-.0026
	(.359)	(-.1702)	(-.6758)
Employment growth rate	-.1821***	-.1846***	-.1804***
	(-15.02)	(-15.57)	(-15.22)
log median daily wage	.0424***	.0436***	.061***
	(3.516)	(3.959)	(5.018)
log employment LM region	.0261***	.0218***	.138*
	(4.368)	(4.09)	(1.72)
log empl. density LM region	-.0308***	-.0266***	-.0812
	(-3.647)	(-3.373)	(-1.548)
Constant	-.3745***	-.1362	-1.422
	(-5.04)	(-1.248)	(-1.587)
Observations	58436	58436	58436
Adjusted R^2	.167	.179	.186

t statistics in parentheses. All estimations include 2-digit occupation fixed effects. Columns 2-3 includes 2-digit industry and year fixed effects. Column 3 includes labor market region fixed effects. Standard errors clustered at the region-industry level (labor market regions, 2-digit industries). * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

Table A.3.10: Training wage differences between retained and moving apprenticeship completers by regional competition, poaching sample.

	(1) log training wage	(2) log training wage	(3) log training wage
Mover	0.00146	-0.00304	-0.00203
	(0.17)	(-0.37)	(-0.21)
log firm density reg.-occ.	0.0287***	0.00252	0.00280
	(9.21)	(0.80)	(0.90)
Mover*log firm density reg.-occ.	0.0154**	0.0154**	0.0218***
	(2.19)	(2.38)	(3.10)
log full-time employment, training estab.		0.0302***	0.0301***
		(6.83)	(7.00)
log employment, training region		-0.0219***	-0.0217***
		(-3.68)	(-3.72)
log avg. wage, training region		0.474***	0.473***
		(10.46)	(10.50)
Constant	3.370***	1.393***	1.393***
	(96.86)	(6.96)	(7.06)
Observations	134602	134482	132341
Adjusted R^2	0.220	0.279	0.278

t statistics in parentheses. All estimations include 2-digit occupation fixed effects. Standard errors clustered at region-occupation level (labor market region of training establishment, 2-digit occupations). Column 3 excludes interregional movers. * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

Table A.3.11: Training wage position, differences between retained and moving apprenticeship completers by regional competition, poaching sample.

	(1) Training wage rel. to cell mean	(2) Training wage rel. to cell mean
Mover	-0.215*	-0.239*
	(-1.95)	(-1.92)
log firm density reg.-occ.	-0.00168	0.00393
	(-0.65)	(1.40)
Mover*log firm density reg.-occ.	0.0577	0.0405
	(1.27)	(0.88)
Constant	0.00339	0.00296
	(0.92)	(0.85)
Observations	134602	132460
Adjusted R^2	-0.000	-0.000

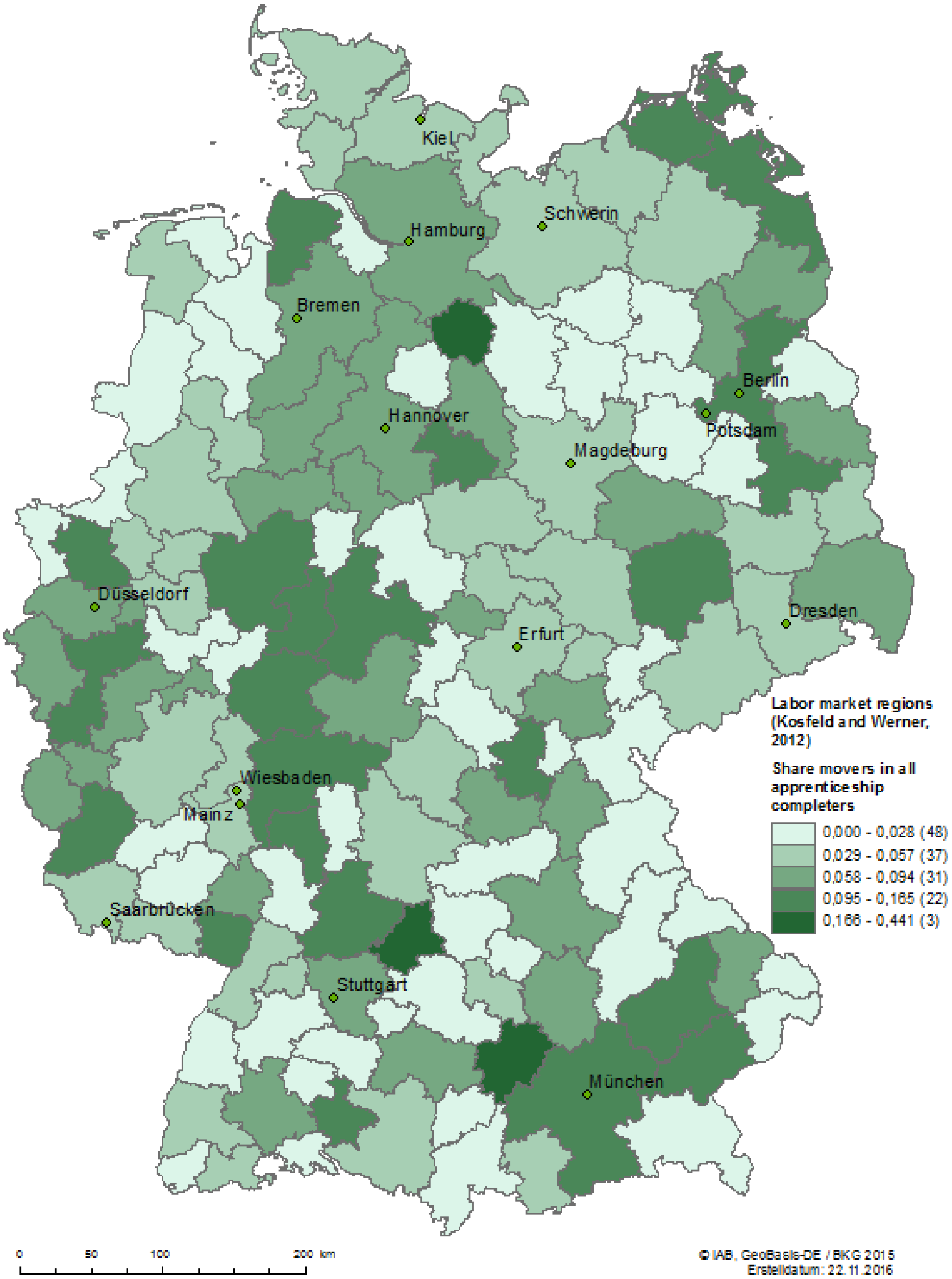
t statistics in parentheses. All estimations include 2-digit occupation fixed effects. Standard errors clustered at region-occupation level (labor market region of training establishment, 2-digit occupations). Column 2 excludes interregional movers. * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

Table A.3.12: Wage increase between training and skilled job, differences between retained and moving apprenticeship completers by regional competition, poaching sample.

Dep. var.: log wage diff. job-training	(1)	(2)	(3)	(4)
Mover	-0.0590**	0.0226	0.0159	0.0190
	(-2.57)	(0.96)	(0.88)	(0.79)
log firm density, reg.-occ.	0.0298***	-0.0084*	-0.0084*	-0.0092**
	(7.31)	(-1.96)	(-1.93)	(-2.19)
Mover*log firm density, reg.-occ.	-0.0224**	-0.029***	-0.030***	-0.038***
	(-2.53)	(-3.26)	(-3.22)	(-3.57)
log full-time employment, hiring estab.		0.0574***	0.0534***	0.0577***
		(11.76)	(6.34)	(11.59)
log employment, hiring region		-0.024***	0.0016	-0.023***
		(-3.26)	(0.08)	(-3.13)
log avg. wage, hiring region		0.542***	0.477**	0.540***
		(9.29)	(2.20)	(9.26)
log full-time employment, training estab.			0.00452	
			(0.46)	
log employment, training region			-0.0260	
			(-1.32)	
log avg. wage, training region			0.0640	
			(0.31)	
Constant	3.734***	1.305***	1.313***	1.304***
	(31.97)	(4.81)	(4.81)	(4.81)
Observations	134102	133976	133975	131863
Adjusted R^2	0.132	0.179	0.179	0.180

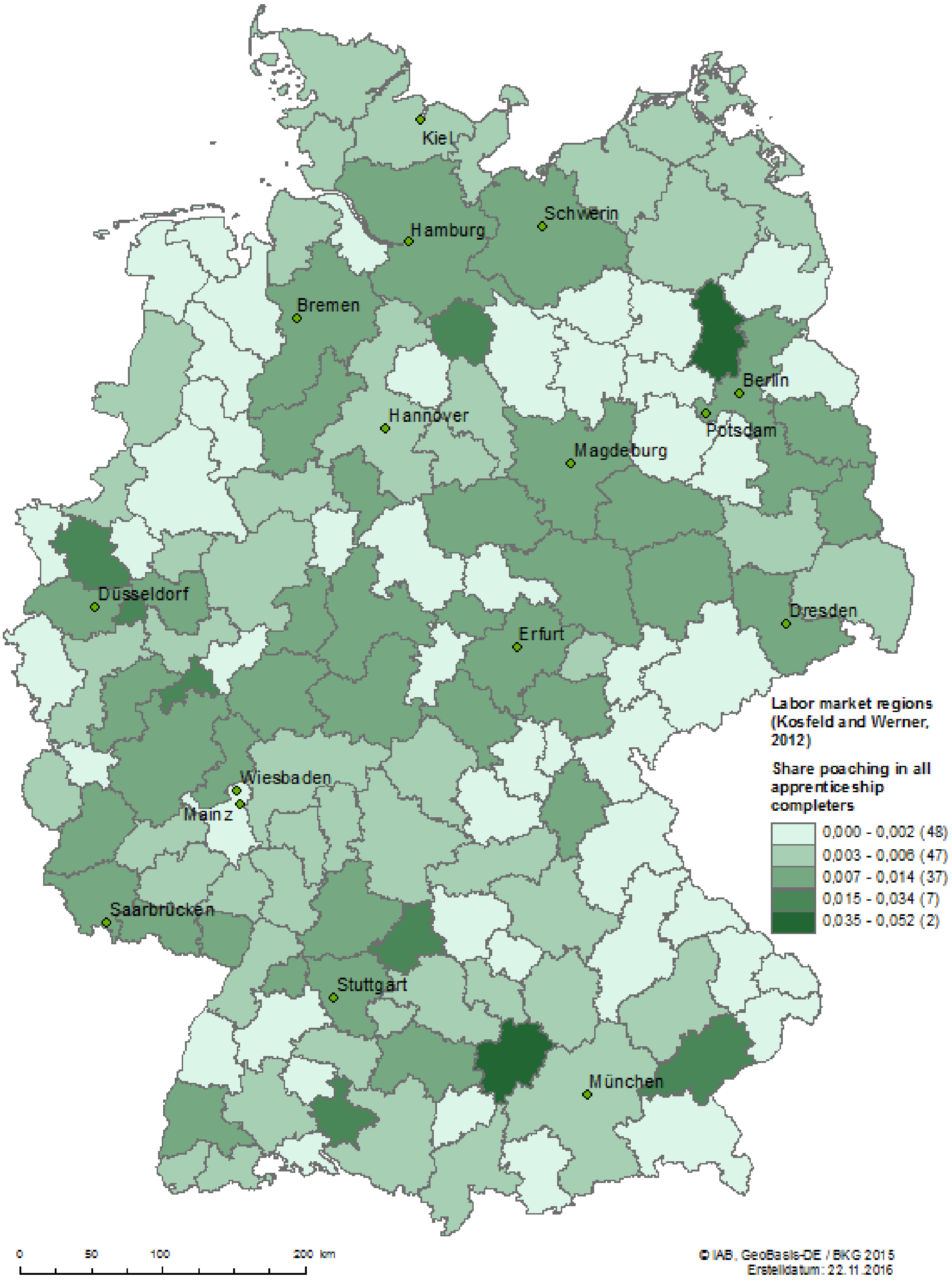
t statistics in parentheses. All estimations include 2-digit occupation fixed effects. Standard errors clustered at region-occupation level (labor market region of hiring establishment, 2-digit occupations). Column 4 excludes interregional movers. * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

Figure A.3.1: Share of movers in all apprenticeship completers in the estimation sample (1999-2010).



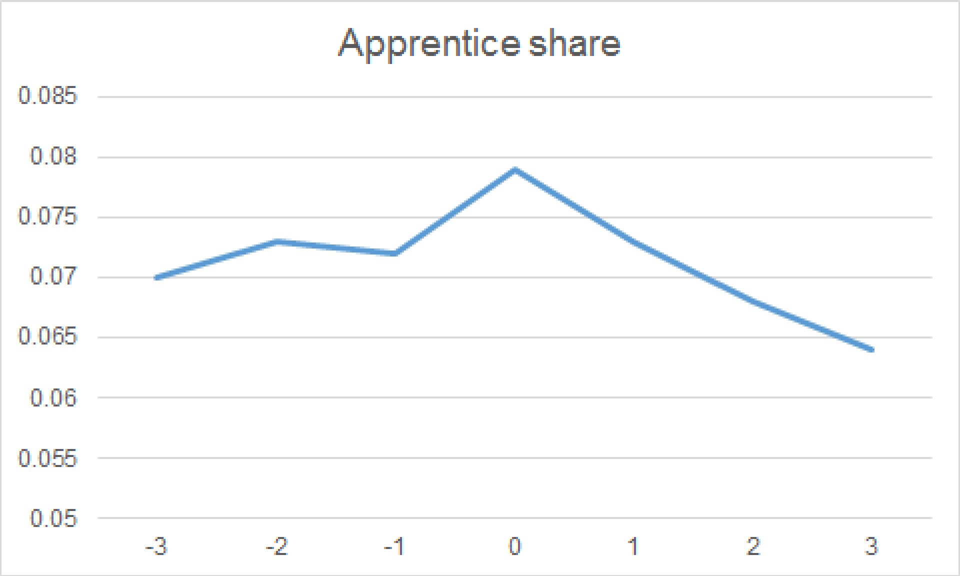
Data source: BEH V.09.

Figure A.3.2: Share of poachings in all apprenticeship completers in the estimation sample (1999-2010).



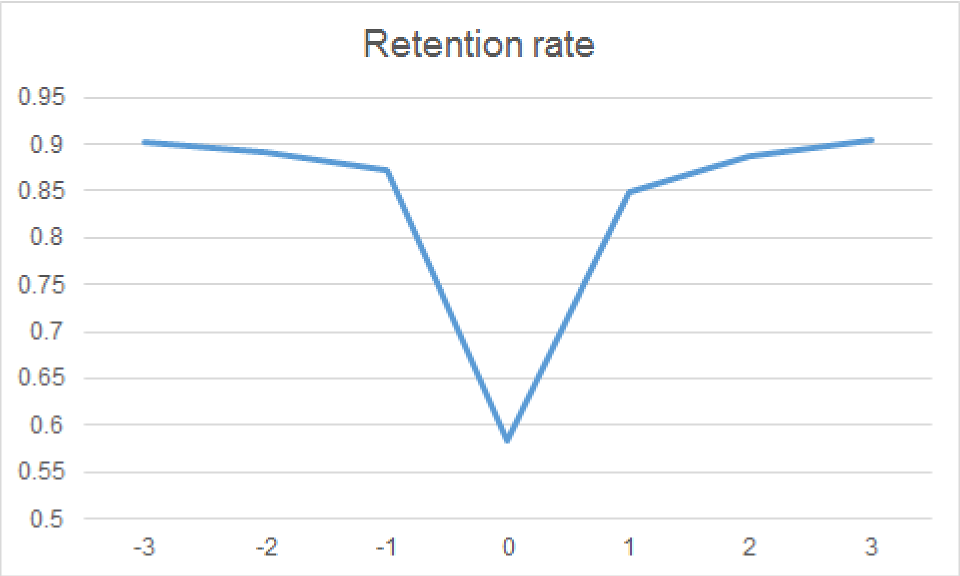
Data source: BEH V.09.

Figure A.3.3: Apprentice share by year for one-time poaching victims



0 is the year of poaching. Means for an unbalanced panel of 317 poaching victims (N = 1,280). Data source: BEH V.09 and BHP 7510 v1.

Figure A.3.4: Retention rate by year for one-time poaching victims



0 is the year of poaching. Means for an unbalanced panel of 317 poaching victims (N = 1,280). Data source: BEH V.09 and BHP 7510 v1.

Figure A.3.5: Log employment by year for one-time poaching victims

0 is the year of poaching. Means for an unbalanced panel of 317 poaching victims (N = 1,280). Data source: BEH V.09 and BHP 7510 v1.

Spurious worker mobility

To rule out spurious job moves of apprenticeship completers (mainly establishment changes within a firm), we use a procedure developed by Schäffler (2014). This procedure identifies which establishments most likely belong to the same firm and excludes worker flows between such establishments because they should not be subject to "normal" employer competition.[84] The assignment of establishment IDs in the IAB data also implies that the entry or exit of IDs need not reflect true openings or closures of establishments, but may also indicate changes of owner, acquisitions, spin-offs, restructurings, or other events in which worker transitions between establishment IDs are probably due to decisions taken at the firm or establishment level, rather than the worker level. Therefore, also such worker transitions do not reflect true worker mobility between competing employers. We use a file produced by Hethey and Schmieder (2010) which contains, for all establishment IDs and the years of the first and last appearance of that ID, the likely cause of its (dis-)appearance. We exclude moves between establishment IDs that are likely due

[84] The method proposed by Schäffler (2014) requires the use of de-anonymized data: Establishments' firm affiliation is derived from their names and addresses. We thank Steffen Kaimer (IAB) for carrying out this procedure for us and providing us with an anonymous file containing an estimated firm ID for every establishment ID and year.

to spin-offs, closures or acquisitions of the training establishment, or other establishment ID changes in completers' employment records that most likely do not reflect real worker mobility. In a further data cleaning step, we also drop remaining clusters of apprenticeship completers' establishment ID changes that appear to be too large to be considered as individual mobility decisions by the workers.

Table A.3.13: Overview of sample construction steps in poaching and baseline sample.

		Poaching sample	Baseline sample
Individual and cell level (apprenticeship completers)			
1a	Apprenticeship duration 700-1,500 days	Yes	Yes
1b	Begin and end dates of apprenticeship in a plausible calendar month (regular apprenticeship year)	Yes	Yes
2	Deletion of wage outliers (less than 50 or more than 200 percent of mean wage in the same training occupation and year)	Yes	Yes
3	All completers must transition into full-time employment	Yes	Yes
4	At least two apprenticeship completers in establishment/occupation/year cell	Yes	No
5a	Deletion of training establishments with zero stayers	Yes	No
5b	Deletion of spurious interfirm mobility (rule out within-firm establishment changes)	Yes	Yes
6a	All completers must transition into full-time employment within 10 days	Yes	(Yes: 30 instead of 10 days)
6b	All completers must transition into full-time employment within the same 2-digit occupation	Yes	Yes
7	Drop if first-job wage < 10€ or > 500€	Yes	Yes
Establishment level (training establishments)			
I	Deletion of outlier establishment observations in terms of apprentice share in total employment, i.e. observations above the 99^{th} percentile	Yes	Yes
II	Deletion of top percentile of establishment observations in terms of employment growth	Yes	Yes
III	Only services and manufacturing	Yes	Yes
Establishment level (hiring establishments)			
I	Deletion of outlier establishment observations in terms of apprentice share in total employment, i.e. observations above the 99^{th} percentile	Yes	Yes
II	Deletion of top percentile of establishment observations in terms of employment growth	Yes	Yes
III	Only services and manufacturing	Yes	Yes

Raiding analysis

To exploit our identification of poaching further, we also consider the effects of regional labor market competition from the perspective of the hiring establishments. The estimation sample now consists of raiders and other hirers, all of which hire at least one external apprenticeship completer (see Figure 2). Inevitably, this reduces the estimation sample size considerably. We can estimate a specification analogous to equation (3.3) but with the dependent variable being the probability of "raiding" at least one apprenticeship completer from another establishment, and controlling for the number of externally hired apprenticeship completers, rather than the number of own apprentices:

$$\begin{aligned} \mathrm{P}(raider)_{it} = \ & \beta_0 + \beta_1 \ln(ext\_appr\_hires)_{it} + \beta_2 comp_{\mathrm{ort}} \\ & + controls + \mu_o + \delta_j + \vartheta_t + \theta_r + u_{it}. \end{aligned} \tag{3.4}$$

Note that there is one important difference to the above estimations, rooted in the change of perspective from training to external hiring establishments. From a training establishments' perspective, all its apprenticeship completers are potential poaching targets (ignoring for the moment the details of our poaching definition). From the perspective of external hirers, all apprenticeship completers within geographical reach (say, within the same labor market region) and in relevant occupations are potential raiding targets. We observe the potential total number of poaching victims only for the training establishments. For the external hirers, we observe only the actually hired external apprenticeship completers.[85] It is plausible to assume that the actually hired apprenticeship completers constitute a positive selection from all those the external establishment could have hired. As a consequence (and confirming this assumption), the share of raidings in all *observed* potential raidings (external apprenticeship completer hires) is relatively high, at 8.4 percent. For comparison, the training establishments' share of poachings in all potential poachings (apprenticeship completers) is only 0.67 percent. Hence, we expect the estimated effect of competition on raiding to be much larger than the effect on being a poaching victim.

[85] Furthermore, we only observe the subset of hires from the observed training establishments (victims and controls). See section 4.2 and Figure 4.2 in particular.

This said, estimation results are presented in Table C1. The estimates are indeed much larger than those from the poaching estimation. In absolute values, a 100 percent increase in competition increases the raiding probability by about ten percentage points, about ten times the estimate of the poaching effect, a factor roughly proportionate to the extent to which the share of raidings in all potential raidings is overstated, as just discussed. Therefore, we find that the effect of regional competition on poaching and raiding (which are, after all, the same thing viewed from different perspectives) is closer to the poaching estimate (plus one percentage point for a 100 percent increase in competition).

Table A.3.14: Impact of regional competition on raiding, external hiring establishments from the poaching sample.

	(1) Raider	(2) Raider	(3) Raider
log estab. density, reg.-occ.	.9756***	1.059***	1.135***
	(6.168)	(6.75)	(6.675)
log ext. appr. completer hires	.4297***	.489***	.5551***
	(4.44)	(4.892)	(5.774)
Log labor (full-time)	.0616*	.0207	.0336
	(1.765)	(.5557)	(.868)
Share mid-qual. employees	-.0964	.0616	.0986
	(-.5477)	(.3402)	(.5009)
Employment growth rate	.0161	.2755	.2573
	(.0699)	(1.134)	(1.025)
log median daily wage	.3257**	1.081***	1.232***
	(2.471)	(5.732)	(5.929)
log employment LM region	.2077**	.2262***	.2711
	(2.491)	(2.624)	(.0836)
log empl. density LM region	-1.147***	-1.245***	-1.334
	(-6.209)	(-6.592)	(-.6038)
Observations	5747	5723	5490
Pseudo R^2	0.097	0.124	0.160
AIC	3614.0	3594.6	3615.8
BIC	3893.6	4200.0	4904.8

t statistics in parentheses. Average marginal effects (elasticities) after Probit. All estimations include 2-digit occupation fixed effects. Columns 2-3 includes 2-digit industry and year fixed effects. Column 3 includes labor market region fixed effects. Standard errors clustered at the region-industry level (labor market regions, 2-digit industries). * p<0.10, ** p<0.05, *** p<0.01. Data source: BEH V.09 and BHP 7510 v1.

Appendix to chapter 4

Table A.4.1: Summary statistics, Western sample

Establishment level	**count**	**mean**	**sd**	**min**	**max**
Log empl. growth rate	33473	-0.007	0.263	-1.099	0.827
Full-time employment	33473	9.840	36.765	1.000	1377.000
Log full-time empl. (estab.)	33473	1.247	1.179	0.000	7.228
Log median wage (estab.)	33473	4.157	0.499	2.306	5.246
Young estab.	33473	0.080	0.271	0.000	1.000
Mid-age estab.	33473	0.378	0.485	0.000	1.000
Share high-skilled (occ.)	33473	0.075	0.191	0.000	1.000
Dummy manufacturing	33473	0.443	0.497	0.000	1.000
Dummy services	33473	0.557	0.497	0.000	1.000
Municipality level					
DSL availability	10901	0.865	0.166	0.000	1.000
Distance to assigned MDF >4200m	10901	0.374	0.484	0.000	1.000
Distance to assigned MDF >4200m, pop.-w. centroid	10356	0.283	0.450	0.000	1.000
Fraction of municipality area >4200m from assigned MDF	10901	0.428	0.299	0.000	1.000
Log FT empl., municip.	10901	5.140	1.248	0.693	10.113
Log FT empl. dens., municip.	10901	2.527	1.224	-1.816	7.617
Log mean wage (FT), municip.	10901	4.339	0.172	2.913	5.137
Log full-time empl. growth rate, municip.	10901	0.004	0.144	-3.091	1.497
Share high-skilled (occ.), municip.	10901	0.118	0.077	0.000	0.649
Own MDF (yes/no)	10901	0.000	0.000	0.000	0.000
Distance to assigned MDF	10901	3875.177	1430.371	304.452	10200.920
Distance to assigned MDF, pop.-w. centroid	10901	3577.541	1557.988	25.692	10093.380
Distance to nearest MDF	10901	3744.732	1316.459	86.527	9988.840
Distance to nearest MDF, pop.-w. centroid	10901	3343.899	1362.057	25.692	9338.061

Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1

Table A.4.2: Summary statistics, Eastern sample

Establishment level	**count**	**mean**	**sd**	**min**	**max**
Log empl. growth rate	72891	-0.014	0.272	-1.099	0.827
Full-time employment	72891	10.855	41.330	1.000	3172.000
Log full-time empl. (estab.)	72891	1.336	1.211	0.000	8.062
Log median wage (estab.)	72891	3.912	0.468	2.306	5.208
Young estab.	72891	0.102	0.303	0.000	1.000
Mid-age estab.	72891	0.613	0.487	0.000	1.000
Share high-skilled (occ.)	72891	0.113	0.244	0.000	1.000
Dummy manufacturing	72891	0.331	0.471	0.000	1.000
Dummy services	72891	0.669	0.471	0.000	1.000
Municipality level	**count**	**mean**	**sd**	**min**	**max**
DSL availability	7774	0.755	0.282	0.000	1.000
OPAL municipalitiy	7774	0.105	0.306	0.000	1.000
OPAL area, pop.-w. centroid	7529	0.105	0.306	0.000	1.000
Fraction of municipality area >4200m from assigned MDF	7774	0.340	0.221	0.000	1.000
Log FT empl., municip.	7774	5.974	1.593	0.693	11.963
Log FT empl. dens., municip.	7774	2.644	1.322	-1.688	6.182
Log mean wage (FT), municip.	7774	4.078	0.152	3.142	4.861
Log full-time empl. growth rate, municip.	7774	-0.009	0.128	-1.521	2.570
Share high-skilled (occ.), municip.	7774	0.147	0.076	0.000	0.620
Own MDF (yes/no)	7774	0.668	0.471	0.000	1.000
Distance to assigned MDF	7774	2297.083	1494.680	75.316	12144.847
Distance to assigned MDF, pop.-w. centroid	7774	1978.502	1856.296	26.243	14080.074
Distance to nearest MDF	7774	2084.063	1133.629	47.045	4199.971
Distance to nearest MDF, pop.-w. centroid	7774	1604.050	1342.296	26.243	5983.694

Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1

Sample restrictions

The samples of establishments I use are based on a ten percent random sample from all establishments observed in the BHP between 2005 and 2009. I restrict this sample as follows. First, I exclude establishments which appear in the BHP for the first time in 2005 or later, since these may have been attracted to their particular location exactly because of broadband availability. To do so, I use the BHP's information on the first appearance of each establishment ID. These are the establishment ID entry variable created by Hethey and Schmieder (2010) and the first appearance date of the ID.

Furthermore, I limit the sample to establishment observations with at least one full-time employee, since only for these, the establishment's median gross daily wage, an important control variable, is observed (the IAB data do not contain precise working hours or hourly wages). I also check for implausible values and outliers in important establishment characteristics, dropping some rare establishment observations with a reported median daily wage for full-time workers below 10 Euros (this concerns less than one percent of establishment observations). Such implausibly low values can arise if a substantial fraction of an establishment's workers (typically in very small establishments) hold a position but are not actively working, as is the case during sickness leave (after six weeks), maternity leave, or sabbaticals.

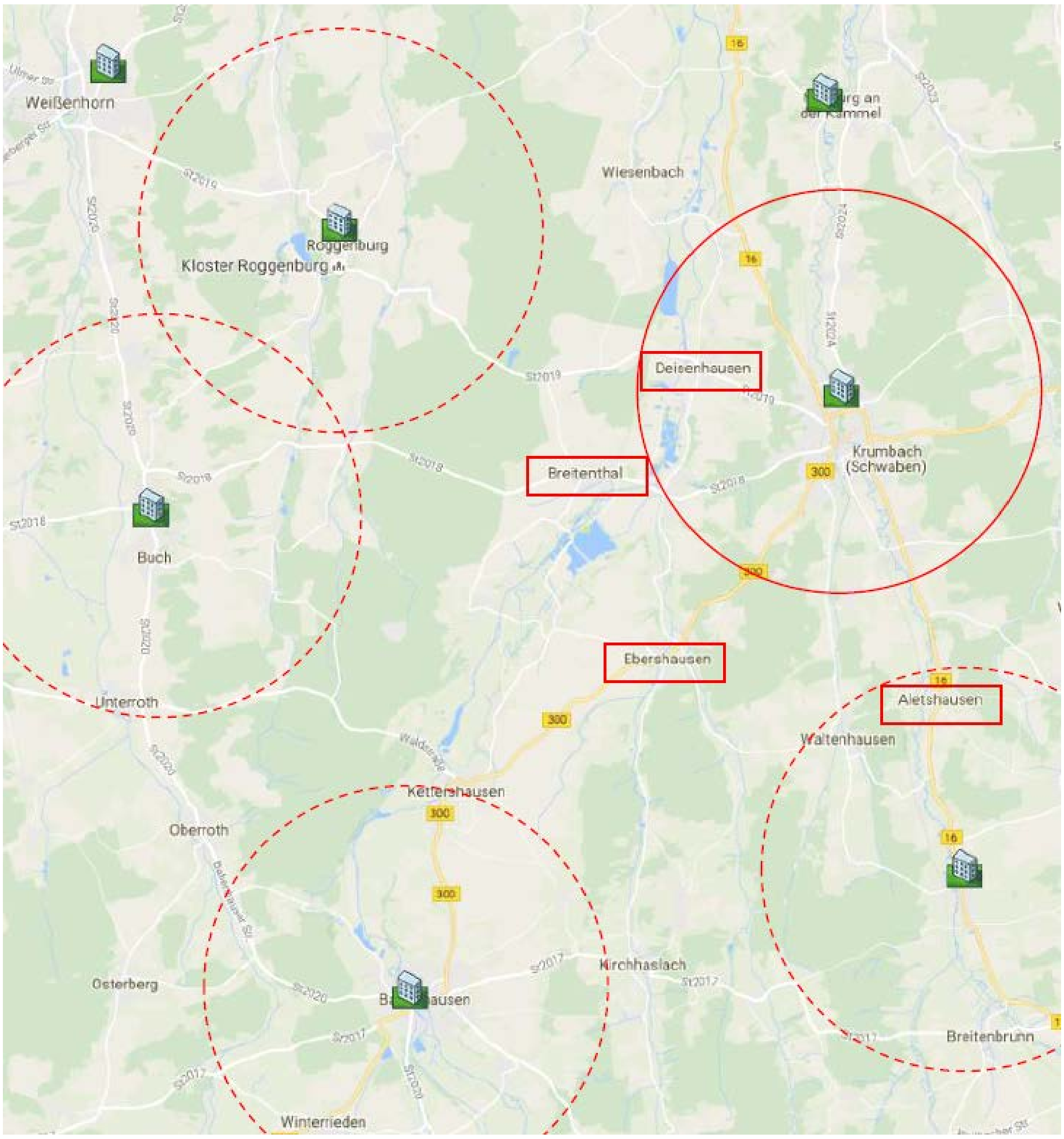
Finally, I drop all observations below the first and above the 99th percentile in terms of employment growth, as there are a small number of establishment observations with implausibly small (negative) or large growth rates.

At the municipality level, I drop the federal state of Berlin because the DSL data do not report separate values of DSL availability for the formerly separate Eastern and Western parts of the city, although these have historically different telecommunication infrastructures and thus probably also systematically different DSL availability rates.

Figure A.4.1: MDF locations (Western Germany) and OPAL areas (Eastern Germany)

Source: Falck et al. (2014), online appendix.

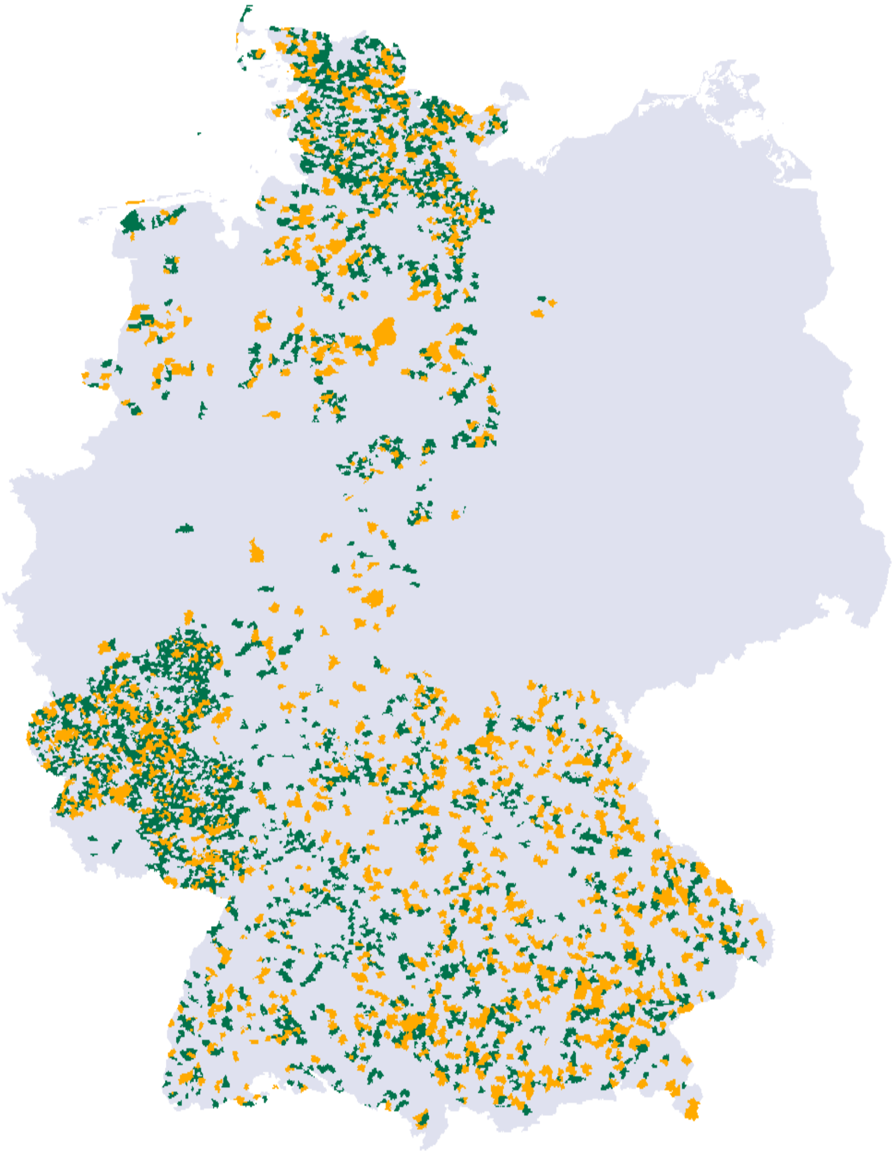
Figure A.4.2: Example of MDF catchment areas and distance thresholds used to construct the Western German IV.



The map shows four municipalities (highlighted in rectangles) in a rural area in Bavaria and five MDFs. These four municipalities are assigned to the MDF in Krumbach, whose catchment area (with 4.2 km radius) is illustrated approximately by the solid circle. The Northernmost highlighted municipality (Deisenhausen) is within 4.2 km of its assigned MDF. For this municipalitiy, the IV would have the value zero, as there is no technical impediment. The most Southeastern highlighted municipality (Aletshausen) is more than 4.2 km from the assigned MDF, but within 4.2 km from its nearest MDF (to the South), whose catchment area is represented by a dashed circle. For this municipality, the IV therefore is set to missing, and the municipality is excluded from the estimation sample. The two Westernmost municipalities (Breitenthal and Ebershausen) are more than 4.2 km both from their assigned MDF and from any other MDF. For these municipalities, the IV has the value one, indicating a technical impediment to obtaining DSL service.

Source: http://meinkontes.de/hvt/, accessed November 23, 2016. Map data © 2016 GeoBasis-DE/BKG (© 2009), Google.

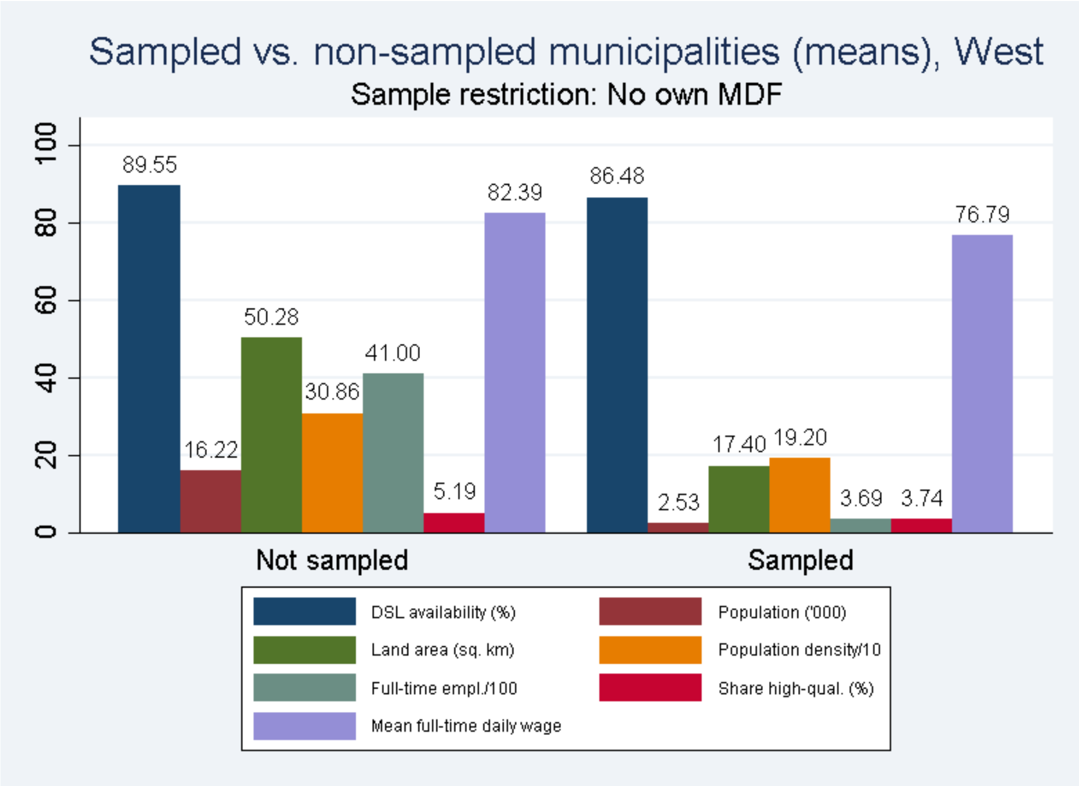
Figure A.4.3: : Municipalities above vs. below the 4.2 km threshold (distance to assigned MDF).



The map shows Western German municipalities without an own MDF. Municipalities colored in green are less than 4.2 km from their assigned MDF. Municipalities colored in yellow are more than 4.2 km from their assigned MDF. Distance between each municipality and its assigned MDF is measured using the municipality's geographic centroid. The strikingly high number of sample municipalities in Rhineland-Palatinate in the Southwest and their low number in North-Rhine Westphalia in the West is due to extreme differences in municipality size. North-Rhine Westphalian municipalities are on average ten times as large as those in Rhineland-Palatinate (86 vs. 8.6 square km). Thus, there are a very large (small) number of municipalities without an own MDF in Rhineland-Palatinate (North-Rhine Westphalia).

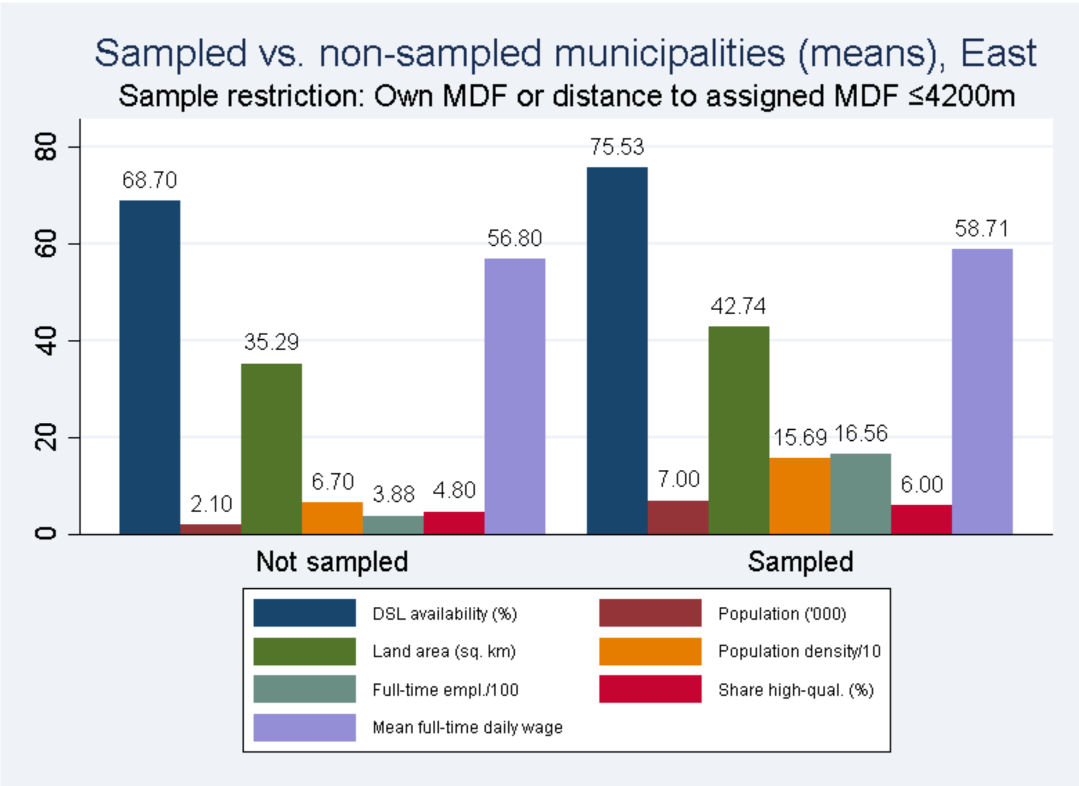
Data source: Deutsche Telekom

Figure A.4.4: Sampled vs. non-sampled municipalities, Western Germany



Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1

Figure A.4.5: Sampled vs. non-sampled municipalities, Eastern Germany



Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1

Table A.4.3: Robustness check, West: Excluding 1% largest establishments

	(1)	(2)	(3)
A: Manufacturing			
DSL availability	-0.078	-0.119	-0.203*
	(-0.87)	(-1.30)	(-1.86)
Observations	14565	14565	14565
Adjusted R^2	0.010	0.031	0.037
B: Services			
DSL availability	0.307***	0.308***	0.313***
	(2.75)	(2.71)	(3.14)
Observations	18571	18571	18571
Adjusted R^2	.[a]	.[a]	0.005
Specification details for A and B			
Establishment controls	Yes	Yes	Yes
Municipality controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE (3-digit)		Yes	Yes
District FE			Yes

t statistics in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Constant omitted from output. Standard errors clustered at the municipality level. [a]Not reported because model sum of squares is negative, a common problem arising in 2SLS estimation. Note: The extremely low R-squared value found in Panel B, column 3 is due to the 2SLS estimation procedure. An OLS estimation using "manually" constructed fitted values of DSL availability found an adjusted R-squared of 0.035. Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1.

Table A.4.4: Robustness check, West: Full-time employment growth rate

	(1)	(2)	(3)
A: Manufacturing			
DSL availability	-0.110	-0.156	-0.261**
	(-1.17)	(-1.55)	(-2.18)
Observations	14823	14823	14823
Adjusted R^2	0.017	0.035	0.042
B: Services			
DSL availability	0.170	0.207	0.155
	(1.18)	(1.46)	(1.13)
Observations	18650	18650	18650
Adjusted R^2	0.024	0.036	0.051
Specification details for A and B			
Establishment controls	Yes	Yes	Yes
Municipality controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE (3-digit)		Yes	Yes
District FE			Yes

t statistics in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Constant omitted from output. Standard errors clustered at the municipality level. Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1.

Table A.4.5: Robustness check, West: Full-time equivalent employment growth rate

	(1)	(2)	(3)
A: Manufacturing			
DSL availability	-0.115	-0.143	-0.237**
	(-1.27)	(-1.58)	(-2.20)
Observations	14823	14823	14823
Adjusted R^2	0.012	0.035	0.046
B: Services			
DSL availability	0.220*	0.262**	0.246**
	(1.88)	(2.16)	(2.28)
Observations	18650	18650	18650
Adjusted R^2	0.012	0.025	0.044
Specification details for A and B			
Establishment controls	Yes	Yes	Yes
Municipality controls	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
Industry FE (3-digit)		Yes	Yes
District FE			Yes

t statistics in parentheses. * p<0.10, ** p<0.05, *** p<0.01. Constant omitted from output. Standard errors clustered at the municipality level. Data source: Broadband Atlas Germany, Deutsche Telekom, BHP7510 v1.

Zusammenfassung

Diese Dissertation untersucht ausgewählte Aspekte der Mobilität von Beschäftigten zwischen Betrieben. Einleitend (Kapitel 1) wird diese Form der Arbeitsmobilität auf Basis einer knappen Literaturübersicht näher charakterisiert. So wird die Bedeutung von Beschäftigtenmobilität zwischen bestehenden Betrieben als Reallokationsmechanismus am Arbeitsmarkt hervorgehoben, verglichen etwa mit der Reallokation, die durch Gründung und Schließung von Betrieben zustande kommt. Zudem wird aufgezeigt, welche Bedeutung institutionelle Rahmenbedingungen, insbesondere die Regulierung des Arbeitsmarkts, für die Mobilität von Beschäftigten zwischen Betrieben haben. Im Falle Deutschlands, das eine eher strikte Regulierung aufweist, führt der landesspezifische institutionelle Kontext demnach zu einem relativ geringen Maß an Beschäftigtenmobilität sowie teilweise (im Falle freiwilliger Betriebswechsel) zu einer Positivselektion von Betriebswechslern. Schließlich wird hervorgehoben, dass Arbeitsmärkte in hohem Maße lokal sind, wobei sich die Lokalität nicht zwingend und nicht allein auf die geographische Dimension beschränkt. Die Beschäftigtenmobilität zwischen Betrieben ist daher ein stark lokales Phänomen und potentiell konstitutiv für lokale Arbeitsmärkte.

Der Hauptteil der Dissertation (Kapitel 2-4) besteht aus drei weitgehend unabhängig voneinander entstanden empirischen Studien. Alle drei Studien nutzen administrative Beschäftigungsdaten für Deutschland sowie mikroökonometrische Methoden. Im zweiten Kapitel, welches in Co-Autorenschaft mit Katja Wolf entstand, werden die betrieblichen Produktivitätswirkungen von Beschäftigtenzugängen je nach Produktivitätsniveau des Herkunftsbetriebs untersucht. Frühere empirische Studien zeigen, dass Zugänge von Beschäftigten aus produktiveren Betrieben die Produktivität der einstellenden Betriebe steigern. Wir untersuchen diesen Zusammenhang für Deutschland anhand eines eigens generierten Linked Employer-Employee Datensatzes. Dabei ordnen wir Herkunfts- und Zielbetriebe von Betriebswechslern anhand ihres Medianlohns. Unsere Ergebnisse zeigen, dass Beschäftigtenzugänge aus höher entlohnenden Betrieben keine Wirkung auf die Produktivität der Zielbetriebe haben. Zugänge aus geringer entlohnenden Betrieben

hingegen haben unseren Ergebnissen zufolge einen positiven Produktivitätseffekt. Weitere Analysen ergeben, dass dieser Effekt in einer Positivauswahl dieser Beschäftigten aus ihren Herkunftsbetrieben begründet liegt. Ein Teil der produktivsten Beschäftigten von Betrieben mit niedrigerem Lohnniveau wechselt also zu Betrieben mit höherem Lohnniveau. Dieser Prozess spiegelt ein bereits bekanntes Muster der Beschäftigtenmobilität in Deutschland wider, wonach sich hochbezahlte Beschäftigte zunehmend in hoch entlohnende Betriebe sortieren. Unsere Ergebnisse können daher als Mikro-Fundierung für dieses gesamtwirtschaftliche Muster dienen.

Im dritten Kapitel, basierend auf einer Studie in Kooperation mit Thomas Zwick, wird der Zusammenhang regionalen Arbeitsmarktwettbewerbs mit dem Ausbildungsverhalten und dem Abwerben von Ausbildungsabsolventen von Betrieben untersucht. Frühere Studien haben gezeigt, dass Betriebe in Regionen mit starkem Arbeitsmarktwettbewerb weniger ausbilden. Dies wird üblicherweise als Beleg dafür interpretiert, dass in diesen Regionen ein erhöhtes Risiko bestehe, Ausbildungsabsolventen abgeworben zu bekommen. Allerdings gibt es keine direkten empirischen Belege für diesen Zusammenhang. Auf Basis eines neuartigen Ansatzes, das Abwerben von Ausbildungsabsolventen ex post zu identifizieren, untersucht diese Studie erstmals direkt den Zusammenhang zwischen regionalem Arbeitsmarktwettbewerb und Abwerbungen. Hierfür nutzen wir administrative Daten für Deutschland. Unsere Ergebnisse zeigen, dass regionaler Wettbewerb tatsächlich mit einer höheren Wahrscheinlichkeit zusammenhängt, dass Ausbildungsbetriebe Opfer von Abwerbungen werden. Allerdings ändern die betroffenen Betriebe nicht ihr Ausbildungsverhalten als Reaktion auf Abwerbungen. Stattdessen zeigen unsere Ergebnisse, dass die niedrigere Ausbildungsaktivität in Regionen mit starkem Arbeitsmarktwettbewerb eher mit einer generell geringeren Übernahmewahrscheinlichkeit von Ausbildungsabsolventen zusammenhängt. Zudem sind in solchen Regionen die Betriebswechsler unter den Ausbildungsabsolventen relativ positiv selektiert; gleichzeitig verursachen sie relativ geringe Kosten für die einstellenden Betriebe.

Das vierte Kapitel untersucht die Wirkungen der lokalen Verfügbarkeit von Breitband-Internet auf das Beschäftigungswachstum in Betrieben. Es werden Daten aus

Deutschland für den Zeitraum 2005-2009 genutzt, als Breitband-Internet in den ländlichen Regionen Westdeutschlands und weiten Teilen Ostdeutschlands eingeführt wurde. Es werden verschiedene technische Hürden des Breitbandausbaus genutzt, um exogene Varianz in der lokalen Verfügbarkeit von Breitband-Internet zu erhalten und so den Effekt von Breitband-Internet auf betriebliches Beschäftigungswachstum von den Effekten unbeobachteter Einflussgrößen und einer möglichen inversen Kausalbeziehung zu isolieren. Die Ergebnisse legen nahe, dass der Breitband-Ausbau einen positiven Effekt auf das Beschäftigungswachstum in westdeutschen Dienstleistungsbetrieben hatte, im westdeutschen Industriesektor hingegen einen negativen Effekt. Dieses Ergebnismuster geht einher mit deutlichen positiven Effekten in wissens- und computerintensiven Branchen. Dies legt nahe, dass die genannten Effekte auf die tatsächliche Nutzung von Breitband-Internet im Produktionsprozess zurückgehen und dass Betriebe in den jeweiligen Sektoren komplementär zu Breitband Beschäftigung aufbauen, beziehungsweise den Beschäftigungsaufbau verlangsamen. Für Ostdeutschland werden keine signifikanten Effekte auf betriebliches Beschäftigungswachstum gefunden.

Abschließend (Kapitel 5) werden die empirischen Ergebnisse der Kapitel 2-4 vor dem Hintergrund der Erkenntnisse aus Kapitel 1 reflektiert. Dabei bleibt zunächst festzuhalten, dass alle drei empirischen Kapitel Beschäftigtenmobilität auf Betriebsebene analysieren und damit die quantitativ bedeutsamste Form der Reallokation auf dem Arbeitsmarkt, die zudem weitreichende ökonomische Funktionen hat (beispielsweise Wissenstransfer). Zweitens bestätigen die drei empirischen Kapitel den Befund, dass Beschäftigtenmobilität in Deutschland unter anderem einer Positivselektion von Beschäftigten zuzurechnen ist, die insbesondere in den Kapiteln 2 und 3 aus verschiedenen Perspektiven näher untersucht wird. Drittens und letztens tragen alle drei empirischen Studien der eingangs betonten Lokalität von Arbeitsmärkten auf verschiedene Weise Rechnung.