# Education and the labour market: Empirical essays on enrolment decisions and the search for workers

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Dissertation

# Education and the labour market: Empirical essays on enrolment decisions and the search for workers

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# Foreword

> *"Self-control, openness, the ability to engage with others, to plan and to persist - these are the attributes that get people in the door and on the job, and lead to productive lives."*
>
> —James Heckman[1]

This quotation from a *New York Times* article by James Heckman summarises broadly but very illustratively important determinants of a life most people would define as "successful" or expressed in a more economic manner, as "productive". For such a productive life, a comprehensive and enriching education seems to be valued as similar important as commitment and dedication in a future working life since many of the characteristics Heckman highlights are learnt and shaped during formal education but also when interacting with others at work.

This dissertation is intended to investigate some aspects of these two fields, education and the labour market which basically every person will be in touch with sooner or later. The first two chapters deal with determinants and possible outcomes of mobility decisions whilst a person pursues her education. The final chapter 3 strives to answer questions, related to the search process at the labour market.

Chapter 1 and 2 do not only investigate a different "stage" of the career than chapter 3, the point of view is also slightly different. Whilst chapter 1 and 2 puts the individual, here the student, in focus, investigations chapter 3 focus on the behaviour of the employers side on the labour market.

Although seemingly intuitive, questions related to a well-founded education as central cornerstone for a successful career have not always attracted these levels of public interest as we observe nowadays. Scholars like Eric Hanushek or James Heckman dealt with this field of so called Education Economics already since the 1970s. However, the importance

[1]US-American Economist and laureate of the Nobel Memorial Prize in Economics in 2000 (Heckman, 2013).

of education gained firstly massive public attention when the results of the first OECD-comparison of student assessments were published, better known as PISA (Programme for International Student Assessment). Especially in Germany, the bad performance of local pupils in comparison to other OECD countries lead to a great discussions about the success and capability of the domestic educational system.

Whereas PISA lead to a remarkable increase in research related to the schooling success of pupils and possible determinants, up to now the tertiary educational system in Germany has not received this high level of attention, especially during the last few years. Although the introduction of tuition fees in Germany lead to an unique cut in a, up to this date, fee free university policy, research on universities in Germany seems to attract relatively less attention. Especially in comparison to a established body of research on the effects of tuition or personal determinants on study outcomes for Anglo American countries, results for Germany are relatively scarce.

My dissertation is intended to close this gap partially by focussing on two aspects of mobility for university enrolment in a German context. Chapter 1 puts the focus on the period of tuition fees in Germany and how these fees shaped mobility patterns of first year students. Chapter 2 deals with a more general question, namely whether mobility during the period of (academic) education explains subsequent mobility and monetary success.

Changing from this educational context to the labour market, I shed light on the search process when looking for new workers from an employer's perspective in chapter 3. This process is investigated by well-founded theoretical literature, whereas empirical results to verify theoretical outcomes are relatively rare in comparison.

The introduction of tuition fees in Germany, which is investigated in chapter 1 lead to a comprehensive debate, especially amongst students and policy-makers. Previous to the introduction of the fees, numerous comments on the pros and cons of such financial duties were published, like the following one in the newspaper *Die ZEIT* in July 2009:

> *"If there had been tuition fees, I would not have been able to study."*
>
> —FRANK-WALTER STEINMEIER[2]

This introduction of tuition fees in Germany in the aftermath of a decision of the Federal Constitutional Court in January 2005, declaring a ban of fees unconstitutional might

[2]Federal President of Germany and former Federal Minister of Foreign Affairs (Steinmeier, 2009).

mark the most significant change in financing of tertiary education in Germany within recent decades. Previously, studying in Germany was free of any fees in general. This might explain the massive public debates preceding the introduction of those fees. Whereas some opponents of those fees like Frank-Walter Steinmeier argued that tuition fees might hinder prospective students from lower income groups from pursuing an university degree, others argued that the barriers for children from low wage earners to go to university are deeply rooted in a relatively impermeable educational system (e.g. Spiewak (2009)). Advocates of a differentiated fee scheme which should charge only higher income groups argue in the sense of Karl Marx, who asserted already in 1875 in his critique of the "Gothaer Programm":

> *"If in some states [...] higher education institutions are also 'free', that only means in fact defraying the cost of education of the upper classes from the general tax receipts."*
>
> —KARL MARX[3]

These two quotes from a broad spectrum of arguments with respect to the pros and cons of tuition fees in Germany should give a small insight into the discussion which arose when some states decided to introduce tuition fees. However, not even 10 years after the first state had fees introduced, all states were fee free again (and are still fee free until now). In general, research has shown that there are significant negative effects of an increase in tuition fees on the enrolment behaviour of prospective students (see e.g. Neill (2009) or Wilkins et al. (2013)), especially for lower income groups (Coelli, 2009). Whereas these results stem from investigations of Anglo-American countries (USA, Canada or UK), who exhibit a significantly different fee scheme in terms of magnitude and heterogeneity between universities and states, the German case is different due to a very homogeneous level of fees (roughly €500 per term) and a state-wide introduction with no exemptions among public universities.

Therefore, the question of distributive effects in terms of access to university can hardly be answered by taking results from an international context. Although first empirical analyses (e.g. Baier and Helbig (2011)) do not find significant drops in enrolment rates of prospective students and explain these findings with fee-induced higher outcomes of a university degree, the discussion about the fairness of those fees went on until the abolishment in all related states.

[3]German economist and philosopher (Marx and Engels, 1970, p.2).

In chapter 1, I investigate a question, closely related to the findings of authors like Bruckmeier and Wigger (2014), Baier and Helbig (2011) or Mitze et al. (2015) finding small or negligible effects of the enrolment rates in reaction to an introduction of tuition fees. My focus lies on the question how tuition fees shaped migration behaviour of first year students in some states if they e.g. stem from a fee free state with a neighbouring fee charging state or vice versa. Since tuition fees were abolished relatively shortly after their introduction, I examine whether this short period of tuition fees (with introduction and abolishment shortly afterwards) has lead to a general effect in enrolment rates or migration patterns.

The question, whether the introduction of fees lead to changes in migration patterns is important in many respects. Firstly, Germans (and also German students) display a relatively low level of mobility: Whereas relocations in Germany happen in more than 80% within the same state, similar behaviour can be found regarding the willingness of prospective students to leave the home state in order to enrol in university. Taking into account that German students are less mobile in general, it is important to note that financial barriers to start to study increase if the home state charges fees. Since moving to another state and therefore having to rent an own flat/room etc. is the only possibility to avoid fees, the question whether students react spatially to an introduction of fees becomes a question of equal access to higher education. Only families of higher income groups might be capable to finance a university degree away from home.

Secondly, students in Germany are generally free to decide where to enrol. The level of tuition fees in Germany did not depend on the origin of the person enrolling, i.e. people coming from the same state did not receive fee discounts as some American universities offer for students from the same state. Therefore, the effects of fees on enrolment behaviour can help to answer the question whether fees have negative effects on enrolment rates (simply because they increase the costs to study) or whether they have positive effects by signalling a higher quality of eduction.

My results suggest that the generally low level of mobility of prospective students in Germany is not altered by the introduction of tuition fees in general: total flows to a state do not change significantly if this state introduces fees or abolishes them. However, the aspect of adjacency between two states when investigating these flows seems to be important. I find that the ratio of first year students (normalised by the respective age specific cohort) starting to study in a neighbouring state decreases significantly if this state introduces tuition fees whilst the home state does not.

Furthermore, I do not find any anticipatory effects in reaction to the abolishment of tuition fees: students do not seem to delay their enrolment decision to the date the fees are abolished in their home state again, if they have notice of this point in time.

Interestingly, aggregated results suggest a positive all over effect of the introduction and abolishment of tuition fees for the enrolment rates of those states. A state which experienced a period of tuition fees receives higher flows of incoming students than previous to the introduction of fees. This, in the first moment puzzling result, may be explained by the financing of universities in states with tuition fees. For example, universities in Bavaria generated €219 million of additional budget by tuition fees. To avoid an underfinancing of these universities after the abolishment of tuition fees, Bavarian government guaranteed a compensation of exactly this amount (Scherf, 2013). According to personal contact to the German *Hochschulrektorenkonferenz* (Conference of University Presidents), most universities in the other, fee abolishing states, complained successfully to ensure a public compensation as well.

Therefore, it could be argued that the period of tuition fees lead to a better financial endowment of universities in the respective states without a higher level of financial inclusion of students in comparison to states which never charged fees. By saying so, tuition fees lead to a better university financing without higher fees for students in the long run, which might be an explanation for the increased popularity of those states in recent years (after the abolishment of the fees).

Chapter 2 investigates a more general question related to the professional career, namely how mobility in the working life is shaped by mobility in younger years. This question is important from a policy perspective: If people who are mobile earn a higher lifetime income, the state should implement policies and programmes to foster mobility e.g. of high school or university students to increase the welfare of these persons but also the state itself due to higher tax earnings in the future (assuming the person stays in the country she grew up).

The so called ERASMUS-programme is an illustrative example of how a public policy can foster even transnational mobility. The ERASMUS-programme was founded in 1987 to simplify studying abroad (within the European Union) by increasing the collaboration between European universities and supporting the students with scholarships to overcome the extra costs of a stay abroad. The success of the programme is tremendous, and represents today the biggest scholarship-scheme worldwide for exchange terms with more than 4.4 million scholarships until 2017[4] (Schulze-von Laszewski, 2017).

[4]The year of the 30th anniversary of the founding of ERASMUS.

Researchers like Parey and Waldinger (2011) showed that the likelihood to work abroad is significantly higher for people who stayed abroad during university times (in this specific setting, by spending a term abroad with the ERASMUS-programme). Following these results, it can be argued that these people, exhibiting a higher level of mobility, are also more likely to enter higher levels of income as a well established literature on the so called "mobility premium" shows. Some people argue that the upsides of (international) mobility are even more diverse:

*"The benefits of studying abroad are almost endless."*
—MICHELLE OBAMA[5]

From a researcher's perspective, questions regarding the benefits and effects of migration are always connected to the issue of endogeneity: most people move for reasons and due to personal characteristics which can hardly be observed. Therefore, identifying really exogenous drivers for mobility is often problematic. We take a relatively small sample of graduates from Bavarian universities and track these people within Bavaria from school to their first professional position in the labour market.

We therefore narrow the broad focus from international mobility and its possible gains to one federal state of Germany and migration patterns of university graduates within. We do so because this allows us to use a dataset which tracks students down to the postal code of their high school, their university and their first job. Because of this, we are able to control for the state of Bavaria on a very fine level in comparison to related literature. However, we are restricted to the state of Bavaria only due to the dataset used.

We ensure that the sample of interest consists of graduates who are as similar as possible: in direct observable factors like family background and education but also in a more indirect, hardly observable dimension like influence of the neighbourhood and therefore environmental conditioning.

Our results indeed suggest that mobility at a relatively early stage in life (at enrolment to university) fosters later mobility, in our case when a person decides on whether to move when entering the labour market. We find significant higher movement rates for those people who did not chose the university closest located to their home but moved somewhere else.

[5] Former First Lady of the USA (Obama, 2014).

What we cannot find are higher earnings for those deciding to move more often. If we take all characteristics into consideration which are relevant for the decision to move (in our view), those who move do not have superior earnings when entering the labour market. However, the payouts of these mobility investments might occur at a later point in time (e.g. when the person has already spent a few years in her occupation), where relevant data is not available for our case.

Chapter 3 finally takes the opposite view at the labour market by investigating how an employer deals with problems during the recruitment process. The availability of data is a problem which occurs when identifying patterns during the recruitment process: in every dataset, to the best of our knowledge, firms are interviewed once during the recruitment process about its characteristics, e.g. which and how many channels used, how many people applied etc.. However, a panel structure is not delivered by the available data in order to track down which events occurred at what point in time to see e.g. if an additional search channel has lead to a significant increase in applications.

We overcome this issue by matching firms with different outcomes of the application process (firms which hired before the intended hiring date, around this date or afterwards) on observable characteristics. An unique feature of our investigation is the incorporation of the planned search duration (we know how long a firm planned to search) to control and match these firms also on unobservable dimensions which are relevant for the search process.

We are able to show manifold reactions of employers to problems during the recruitment process by employing this approach. Firstly, firms seem to need a sufficiently great pool of applicants considered to be suitable for a posted position in order to fill this position. We can show that, although all firms find suitable applicants, at least 4-5 should be found to fill a vacancy. If a firm has a smaller pool of suitable candidates, the likelihood that the filling date is delayed increases significantly.

Secondly, firms react towards this small pool size by increasing the number of search channels used and by being more willing to make concessions related to qualification and experience of the candidates.

Thirdly, we show that most of the applicants apply at an early point of the hiring process, meaning that a prolonged search duration does not necessarily increase the number of applications proportionally. Especially this point seems to be important from a policy perspective. In line with research on so called "phantom-vacancies" [6] our results suggest that firms should ensure that their vacancy posts are updated from time to time.

Otherwise, possible applicants seem to assume that a vacancy post, although still available (e.g. in an online platform like *linkedin*), might be filled if the date of publishing dates back a decent time.

To put chapter 3 in a global context, results suggest the importance of attracting suitable applicants at an early stage of application process. This issue seems to be prevalent especially in the context of the so called *"Fachkräftemangel"* (skilled labour shortage), subject to a massive public debate in Germany in recent years. If this trend of increasing shortage of suitable workers for certain jobs, although theoretically hard to explain, persists, more and more firms will face the problem of unfilled vacancies in the future.

By connecting this phenomenon to our results, firms could increase their likelihood of success by entering the search for a suitable worker with an increased number of search channels. Instead of activating further channels, once the previous ones did not deliver a suitable candidate, firms should consider that especially a "freshly" posted vacancy seems to attract applicants. Therefore, a higher number of search channels from the very beginning on might also lead to a higher number of suitable candidates (which also apply earlier). Although using more channels obviously increases the search costs, firms lacking suitable workers should incorporate the costs of an unfilled position, maybe even for a longer period, when deciding about the budget for the search for applicants.

Nowadays, firms which are not able to fill a vacancy seem to use incumbent employees and spread the extra work amongst them. Although a great part of those firms seem to increase payments to compensate for this additional work, it is questionable whether this strategy is sustainable in the long run. Again, arguing with the skilled labour shortage and an increase in the importance of a fair balance between work and leisure, especially pronounced amongst younger generations (known as "Generation Y"), it remains questionable whether workers might be willing to increase their workloads above the current level agreed on when entering the firm.

[6] See e.g. Albrecht et al. (2017)

# Chapter 1

# The effect of tuition fees on freshmen flows in Germany


**Abstract**

The introduction of tuition fees in Germany can be used as a quasi-experimental setting to study the manifold effects of such fees. I investigate migration patterns of students in reaction to the introduction and abolishment of those fees. By employing a Difference-in-Differences approach, I find small but significant effects of tuition fees on migration patterns if countries are neighbouring. Interestingly, an introduction and following abolishment of fees seem to have negative effects on the external enrolment of first year students. Posing the question whether students avoid paying fees by delaying enrolment decisions until tuition fees are abolished again does not lead to significant effects which could be clarified by a dataset with a higher sampling frequency, however.

## 1.1. Introduction

Studying at a German university or applied university ("Fachhochschule") has been without any tuition fees since the 1970s. This tuition-free regime in tertiary education stands in stark contrast to countries like the USA or UK, charging fees of several 1,000 dollars per term, at least partially. In Germany, some states ("*Bundesländer*") allowed their universities to charge tuition fees up to €500 per term in the short period between 2006 and 2014. This investigation is focussing on the effects these fees might have on migration patterns of prospective students.

Therefore, I do not solely investigate effects the *introduction* of tuition fees have on migration patterns of students but also the change in student flows in reaction to the *abolishment* of those fees. Since the period of tuition fees in Germany lasted, depending on the state, between 1 year (Hesse) and 8 years (Lower-Saxony), it is important to account for possible differences between the states to ensure that effects found are due to the tuition fee reform and not particular characteristics of the states.

My analysis enriches the existing literature in two different ways. Due to the fact that since 2014 all German states are fee free again, I am in the lucky position to investigate migration patterns in reaction to the introduction and the abolishment of tuition fees. This event of abolishment allows me to investigate effects of tuition fees in a second type of "reaction dimension": If a student has to decide on where to study in the period of existing tuition fees, she can avoid paying those fees only by migrating to a fee-free state. In contrast, students who receive their university entrance diploma in the transition period before the abolishment of fees, i.e. in the period *after* the notification about a fee abolishment but *before* the realisation of this abolishment, are offered a second way of studying without tuition fees, simply by delaying the enrolment to the date when studying is without fees again. The question whether students make use of this alternative avoidance behaviour will be investigated here as well.

My findings are, similar to previous investigations, relatively diverse: Whereas I cannot find strong evidence that students react to tuition fees by a stronger migration to other states, the abolishment of those fees in the home state seems to trigger students to opt more often for a (cost free) study course at home. However, the aspect of neighbourhood seems to be central for the question which state to choose to study. Whereas there are no effects of the flows from one to the other state if they are not neighbouring, flows from a tuition free state to a fee introducing state seem to be negatively influenced if these states share a common border. However, I cannot find evidence for an enrolment delaying decision in reaction to an approaching abolishment of tuition fees.

The structure of this investigation is as follows: section 1.2 gives a brief overview over related literature while section 1.3 explains the institutional setting, the introduction and abolishment is embedded in. The following section 1.4 describes the dataset employed for this analysis and presents the empirical approach. Additionally, section 1.5 is fully dedicated to the identification strategy and validation of important assumptions. Section 1.6 presents the results according to the set up of the used dataset (aggregated dataset in subsection 1.6.1 and a dyadic approach in subsection 1.6.2). Finally, section 1.7 summarises the main results.

## 1.2. Related literature

The versatile effects of tuition fees have been studied by a broad strand of literature. Especially for the United States of America and Canada, researchers have investigated the effects of the level of tuition fees on migration behaviour. The United States are especially interesting due to the facts that a) tuition fees vary highly between states and especially b) public universities charge higher fees from students from other states compared to "incumbent" students. As one of the first authors, Tuckman (1970) shows that there is a positive correlation between the outflow of students and the level of tuition fees charged in their home state. More recently, Baryla and Dotterweich (2001) find that the sensitivity of students towards tuition fees highly depends on the quality and recognition of the local student programs and economic conditions of the universities' location. In general, evidence regarding the elasticity of enrolment behaviour to the level of tuition fees is mixed. Whereas authors like Leslie and Brinkmann (1987) find significant negative effects of increased tuition fees on enrolment rates for Canada,[1] the research of e.g. Johnson and Rahman (2005) show only minor if not negligible effects of the tuition fees on enrolment behaviour. Coelli (2009) shows that a sharp increase in tuition fees in some Canadian provinces lead to lower enrolment rates especially amongst low-income groups. In their meta-analysis of 43 related studies, Havranek et al. (2017) show that enrolment rates seem to be negatively influenced by increasing tuition fees whereas male students and students at private universities seem to react more sensitive to such increases.

Literature on tuition fees in Europe mostly identifies small but insignificant effects of the level of tuition fees on enrolment behaviours. Canton and de Jong (2005) or Huijsman

[1] The authors find a drop of 3-6 percentage points per $1000 increase in tuition fees.

et al. (1986) focus on the relatively long post WWII period to identify determinants of enrolment for the case of the Netherlands finding only minor effects for the level of tuition fees. Since Germany, in contrast, introduced tuition fees in relatively recent years, research in this area concentrates on few empirical investigations. Most important, it should be kept in mind that the introduction of tuition fees in Germany is hardly comparable to other countries with tuition fees. This is due to the fact that the tuition fees are roughly the same for all public universities in all charging states in contrast to countries like the US where the level of tuition fees exhibits a much higher level of heterogeneity. Additionally, a quasi-experimental setting is given due to the fact that at the same time some states charge fees whilst others do not.

As one of the first authors for Germany, Hübner (2012) relates the introduction of tuition fees in some states in Germany to the transition rate from high school to university by employing a Difference-in-Differences approach. However, this study assumes a common treatment period for all fee introducing countries (although some states introduced fees one year ahead of the others) and controls for only very few variables which might influence transition rates as well. The study of Bruckmeier and Wigger (2014) closes this gap by allowing for different treatment periods and a set of new control variables leading to insignificant effects of fee introduction on the state specific transition rates.

Both investigations take the ratio between first year students and high school graduates in the respective year and state as dependent variable. By doing so, the results already include a) migration into/out of a state to begin studying and b) timing decisions (e.g. begin to study one year earlier or later). This post-migration and post-timing dependent variable does not allow to disentangle possible reactions as a) or b) of students in reaction to the introduction and also the later abolishment of tuition fees. Mitze et al. (2015) investigate the flow between states and universities depending on the tuition fee status (charging/non-charging) finding that male students show stronger tuition fee avoiding behaviour by studying in a tution free state against female and that this behaviour strongly depends on the distance between universities and the type of university[2]. They conclude that the reaction of students to avoid tuition fees decreases the further the distance to the next tuition free university is, which could be explained by the human capital theory of Becker (1994) arguing that the costs of avoiding tuition fees increase with distance to the next fee-free university.

[2] Mitze et al. (2015) distinguish between universities, applied universities ("Fachhochschulen") and colleges of art and music.

## 1.3. Institutional background

For a long period of time German public universities[3] were legally not allowed to charge tuition fees as regulated in the so called "Hochschulrahmengesetz". After 35 years of tuition free universities, the federal court decided in January 2005 that this ban of tuition fees represents an intervention into the legislative independence of the states in educational issues. German public universities did not charge fees for tuitions since 1970[4]. In reaction to this most authoritative case law, all western German states except for Bremen, Rhineland-Palatine and Saxony-Anhalt introduced tuition fees between 2006 and 2007. Table 1.1 summarises the introduction and abolishment dates of tuition fees for the states.

**Table 1.1:** Introduction and abolishment of tuition fees in German states

| State | Introduction fees | Fee abolishment |
|---|---|---|
| Baden-Wurttemberg | Spring 2007 | Spring 2012 |
| Bavaria | Spring 2007 | Fall 2013 |
| Hesse | Fall 2007 | Fall 2008 |
| Hamburg | Spring 2007 | Fall 2012 |
| Lower-Saxony | Fall 2006 | Fall 2014 |
| North Rhine-Westphalia | Fall 2006 | Fall 2011 |
| Saarland | Fall 2007 | Spring 2010 |
| Berlin | - | - |
| Brandenburg | - | - |
| Bremen | - | - |
| Mecklenburg-Western Pommerania | - | - |
| Rhineland-Palatinate | - | - |
| Saxony | - | - |
| Saxony-Anhalt | - | - |
| Schleswig-Holstein | - | - |
| Thuringia | - | - |

Before the introduction in 2006 and 2007, students were obliged to pay an administrative fee in order to study at a public university. This administrative fee accounted for 75 to 100 Euro and was meant to cover administrative costs of the university, at least partially.

In reaction to the supreme court's decision in January 2005, the conservative governed states Bavaria, Baden-Wurttemberg, Hamburg, Hessia, Lower-Saxony and North Rhine-Westphalia announced that they will start raising tuition fees up to €500 per term.[5] These fees were in general charged on top of administrative fees, such that the cost of studying

[3] In the following I will use university synonymously to describe all public tertiary educational institutions, including universities, applied universities, technical colleges and colleges of art and music.

[4] Previously, tuition fees were called "Hörgelder" and were repealed after student boycotts in 1970.

[5] All states except for Bavaria and North Rhine-Westphalia charged a fixed amount of €500 per term. In North Rhine-Wesphalia, 2 universities charged less than this amount (one university charged €350 per term, the other €275 per term). In Bavaria, the mean of tuition fees per term was €481 for universities and €417 for applied universities. Due to this relatively homogeneous level of fees, I assume that this treatment is similar between states.

increased exactly by the amount of fees, the respective university charged. Consequently, roughly half of all public German universities charged tuition fees at the "zenith" of the tuition fees in Germany.

Although states like Bavaria or Baden-Wurttemberg included many exemption clauses from paying tuition fees[6], no general financing scheme for prospective students with liquidity constraints was established. Therefore, the introduction of tuition fees in Germany represents a "basic institutional change from cost-free higher education to a tuition fee regime"(Mitze et al., 2015, p.391). In general, nearly all universities charged tuition fees up to the maximum amount of €500 per term, the level for applied universities was with approximately €400 only slightly lower.

Following massive protests especially from the student body, Hess abolished tuition fees around one year after their introduction with the beginning of fall term 2008. Saarland and North Rhine-Westphalia followed shortly afterwards. Ultimately, Lower-Saxony abolished tuition fees in fall 2014.

Some specialities of the German tertiary education system have to be kept in mind when analysing the effects of tuition fees and, especially, comparing them with findings in Anglo-American countries. Most importantly, there are no local admission restrictions for German students. In general, every student with a university entrance diploma ("Abitur") from whatever German state can apply for any university in Germany. However, there are local admission restrictions, implemented by the universities themselves ("Numerus Clausus"). As a result, universities can filter the load of applications by allowing only high school graduates with a diploma grade better than the Numerus Clausus for a certain field of study. Only medical degree programs[7] are managed via a nation-wide application procedure, handled by the *Stiftung für Hochschulzulassung* (known as ZVS), placing the applicants according to preferences and grades possibly all over Germany.

Public universities in Germany are mostly financed by the local and federal budget. On average, 75% of the financing stems from state governments. Specific research projects, clusters or schools (e.g."Exzellenzinitiative") are financed by federal budgets, adding up to approximately 15% of universities funding. In total, roughly 90% of public universities are financed by public funds, the remaining 10% stem from e.g. private sponsoring, contract research and students administrative or tuition fees (Hochschulrektorenkonferenz, 2019).

[6]In Baden-Wurttemberg, for example a family with more than two studying children had to pay tuition fees for a maximum of two children. In Bavaria, families with more two children had to pay no tuition fees for their children at all.

[7]I.e. pharmacy, human medicine, veterinary medicine and dentistry

## 1.4. Data

### 1.4.1 Treatment indicator, controls and variables of interest

I build a panel dataset of enrolment figures on the state level for the period between 2000 and 2015 to investigate the migration behaviour of students in reaction to the introduction and abolishment of tuition fees at public German universities . The data stem from the Federal Statistical Office and include information on the total number of first year students in a specific year and state[8] and a disaggregation of this number according to the state, the student received her university entrance diploma (Federal Statistical Office, 2019b). This data is merged with data on the total number of high school graduates (Federal Statistical Office, 2019a) and economic factors on the state level (GDP per capita and unemployment rate) (Statistical Office Baden-Wurttemberg (2018) and Federal Employment Agency (2019)). All data is available on the state level on a year-by-year basis and split by gender and type of tertiary education institution (universities and applied universities). I enrich this administrative data with information on the introduction and abolishment of tuition fees according to Table 1.1[9], information for each state on the date the reform of a shortened high school period ("G8") came into effect and information on the lag between graduating from high school and the beginning of studying on the state level (Federal Statistical Office, 2019b).

Since the main interest of this investigation are migration flows, I take the number of out of state migrating first year students as "base variable". Since this figure might vary especially with the respective age specific cohort, I normalise the out of state migration by this number. Therefore, the normalised out of state migration (NOM) for state $i$ in year $t$ can be formalised as

$$NOM_{it} = \frac{TOM_{it}}{POP_{it}} \tag{1.1}$$

where $TOM_{it}$ represents the total number of freshmen with a university entrance diploma from state $i$ starting to study in year $t$ in a state different from $i$ and $POP_{it}$ the size of the respective age specific cohort $i$ at time $t$[10]. This flow variable can be further split into student flows to states with tuition fees (fee out of state migration: $FOM_{it}$) and states without

[8]I decide to take the total number of first year students both from universities and applied universities to give a general overview over migration patterns in reaction to tuition fees in the tertiary education sector.

[9]I assume the date (term) when fees are introduced and abolished also as date of public disclosure. Therefore, I assume no anticipation effects due to a lag between public information on introduction/abolishment of tuition fees and realisation. Changing this points in time into the dates, information about fee introduction/abolishment became public do not alter my results significantly.

[10]The size of this age specific cohort as used by Federal Statistical Office (2019b) follows OECD (2018) standards, taking the share of people of age $x$ enrolled in tertiary education on the total population of age $x$, summed up for all ages.

tuition fees (no fee out of state migration: $NFOM_{it}$) both normalised again by the size of the respective age cohort in year $t$ and state $i$. By saying so, $NOM$ comprises of $FOM$ and $NFOM$ such that

$$NOM_{it} = FOM_{it} + NFOM_{it} \tag{1.2}$$

holds if at least one state charges tuition fees in time $t$. Before the introduction and after the abolishment of tuition fees in all states Equation 1.2 simplifies to $NOM_{it} = NFOM_{it}$.

Additionally the other dimension of interest are inflows into a state to investigate basically the reverse side. Again, I normalise all inflow values with the total number of high school graduates in the destination state at that point in time and separate this normalised total ratio of in-state migration (NIM) by the fee status (tuition fees introduced: yes/no) of the origin state. Similar to Equation 1.2 following relation holds for inflows:

$$NIM_{it} = FIM_{it} + NFIM_{it} \tag{1.3}$$

In order to investigate, whether the introduction and abolishment of tuition fees changed the rate at which young people enrol at universities I calculate the transition rate from school to university by modifying the nominator of Equation 1.1 and substituting $TOM_{it}$, the total number of students enrolling a different state than their home state by $TER_{it}$, which simply represents the total number of students who start to begin studying at time $t$ and received their university entrance diploma in state $i$, no matter in which state they enrol.

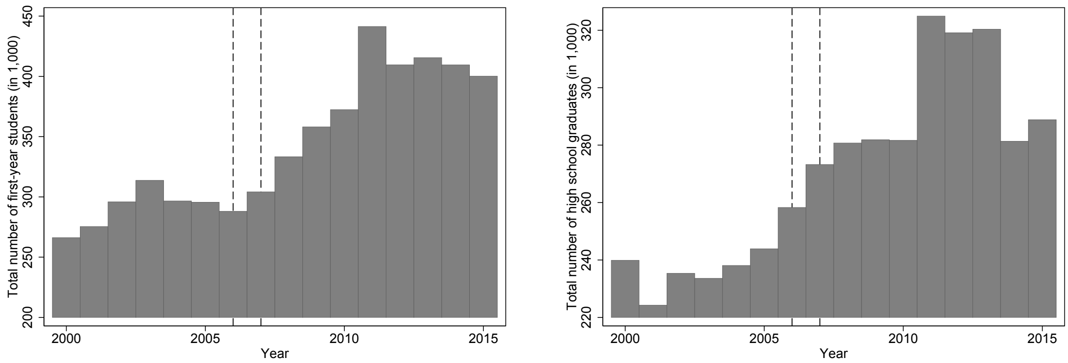
### 1.4.2 Descriptives

Figure 1.1 summarises the development of first year students and high school graduates in Germany for my period of interest. The two vertical dashed lines represent the years 2006 and 2007 when tuition fees were introduced. It is clearly visible that the number of first year students as well as the number of high school graduates increase during the years after the introduction of the tuition fees with no clear tendency beginning with the year 2010. The sharp rise in high school graduates in 2011 and 2012 with a significant drop afterwards can be explained by the introduction of a shortened high school period from 9 to 8 years ("G8"), leading to double graduation classes in the transition period[11].

It is not meaningful to form the direct ratio of the values of part a) and b) of Fig-

[11] The sharp drop in high school graduates in 2001 is due to the fact that Mecklenburg-Western Pommerania and Saxony-Anhalt introduced a 13th year of schooling in that year, meaning that in that respective year nearly nobody ($<100$) graduated from high school in these states.

ure 1.1 to form transition rates from high school to university especially due to the fact that not every person with the permission to enrol for a university degree does so directly after graduation from school. Indeed, investigations like those of Bruckmeier and Wigger (2014) show that less than half of those students starting a university class sometime does so directly after graduation. Therefore, we follow the approach of Federal Statistical Office (2019b) in forming transition rates by taking the percentage of people from a certain birth year who start to study and calculate the mean of this value for each birth cohort to account also for differences in schooling duration.



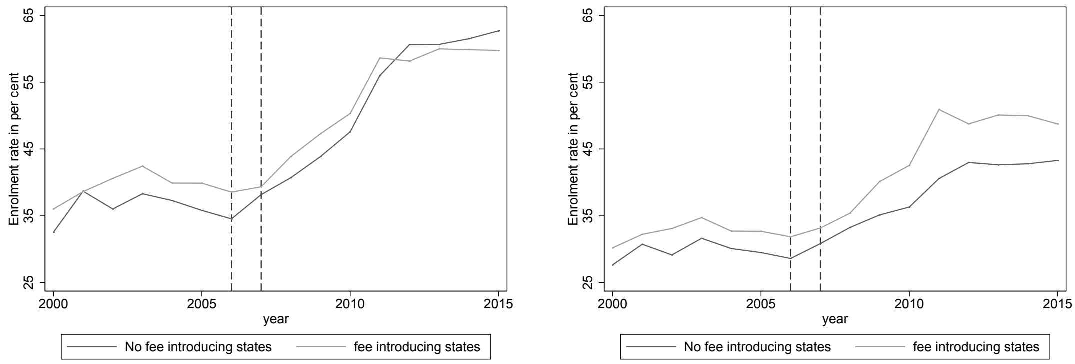

a) b)

**Figure 1.1:** Total number of a) first year students and b) high school graduates in Germany between 2000-2015.

*Notes:* Only German first year students and high school graduates considered. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a).

Figure 1.2 visualises this transition rate separated by tuition fee introducing and not introducing states. Whereas the left graph a) takes the transition rate based on the state the student starts to study, graph b) represents the means for the transition rate based on the state the student receives her university entrance diploma.

Both graphs are leading in the same direction, showing an increase in enrolment rates beginning in the period after the introduction of fees in 2006 and 2007. Whereas a) exhibits a slightly stronger increase in enrolment rates after 2006 and 2007 for the non introducing states against introducing states (the difference becomes smaller) there is a nearly parallel development in enrolment rates between these two groups in b). This seems to be reasonable since a) incorporates already possible migration patterns of students in reaction to the introduction of tuition fees. Since a) compares the number of freshmen with the size of the birth cohort according to the state the student starts to study, it includes already students who decided to start to study in a different state than the one they received their university entrance diploma from. A small catchup in the enrolment rates for no fee introducing states to fee introducing states might therefore be a first hint towards a student migration from fee to no fee introducing states to start to study.




a) b)

**Figure 1.2:** Enrolment rates based on a) location of university and b) high school

*Notes:* Calculations in a) are based on the state the student starts to study. Calculations in b) are based on the state the student receives her university entrance diploma. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a).

At the same time, graph b) of Figure 1.2 does not exhibit such a catchup after fee introduction since it does not account for migration pattern due to the calculation of transition rates based on the state the student received her university entrance diploma. However, the kink in 2011/2012 and the resulting greater difference in enrolment rates between fee and no fee introducing states could give a first clue about possible timing reactions of students in fee introducing states when fees are abolished. If a prospective student knows that fees are abolished in the foreseeable future, she might decide to wait another year to start university to avoid these fees. This kink coincides with the abolishment of tuition fees in North Rhine-Westphalia, the most dense state in terms of population but also first year students.

In general, it should be noted that Figure 1.2 nicely illustrates (at least partly) the validity of the common trend assumption which is important for the identification strategy via a Difference-in-Differences approach: no matter whether the enrolment rate is based on the location of the university or the high school, trends seem to move nearly parallel until the introduction of tuition fees.

## 1.5. Identification strategy

As argued in related literature (e.g. Dwenger et al. (2012) or Bruckmeier et al. (2013)), the introduction of tuition fees in Germany can be seen as a natural experiment, leaving some states as control group without introduction of those fees and treating other states with this shift from a basically fee-free education regime to a tuition fee based regime. In order to measure the effects on student migration patterns when fees are introduced and abolished it is important to ensure that there are no anticipatory effects before the treatment, i.e. that students do not react to the introduction of the tuition fees *before* the fees are actually introduced. Table 1.2 summarises the dates and therefore the differences in time local governments decided on the introduction of tuition fees and when they were actually implemented. Since for some states there are significantly big gaps between those two dates (e.g. for Baden-Wuerttemberg) it is important to ensure that there is no measurement error due to this anticipatory effects by setting the treatment indicator to the right point in time to minimise the risk of such effects. The same holds for the abolishment of tuition fees where the dates when the information about abolishment became public and the real date of abolishment are summarised in Table 1.3. I decided to take the date of "realisation" (the date from when on students had to (no more) pay tuition fees) as date for the treatment indicator due to two reasons:

**Table 1.2:** Dates of enacting and introduction of tuition fees

| Federal state | Date of enacting | Date of introduction |
|---|---|---|
| Baden-Wurttemberg | December 2005 | April 2007 |
| Bavaria | May 2006 | April 2007 |
| Hesse | October 2006 | October 2007 |
| Hamburg | December 2005 | April 2007 |
| Lower-Saxony | December 2005 | October 2006 |
| North Rhine-Westphalia | March 2006 | October 2006 |
| Saarland | July 2006 | October 2007 |

Firstly, it seems to be reasonable that paying tuition fees has an greater impact on prospective students than the sole information that they probably will have to pay tuition fees in a few years if they study in the respective state. Secondly, and this holds especially for the indicator of the tuition fee abolishment, the period between enactment and realisation is relatively short, meaning that students could have had only few months to react to this change before it came into force.[12,13]

I follow a classical Difference-in-Differences approach (DiD) as described by e.g. Donald and Lang (2007) in order to estimate the effects of the tuition fee introduction and abol-

[12] As noted in section 1.4 the data is only available on a yearly basis. Therefore, if enactment and realisation falls into the same year as e.g. the abolishment for Hesse or Bavaria, I am not able to disentangle these two dates.

[13] I also did the calculations with the different definition of the treatment indicator such that it turns into 1 whenever the law for tuition fee introduction or abolishment is enacted. However, this does not change the results significantly.

**Table 1.3:** Dates of enacting and abolishment of tuition fees

| Federal state | Date of enacting | Date of abolishment |
|---|---|---|
| Baden-Wurttemberg | December 2011 | April 2012 |
| Bavaria | April 2013 | October 2013 |
| Hesse | June 2008 | October 2008 |
| Hamburg | April 2011 | October 2012 |
| Lower-Saxony | December 2013 | October 2014 |
| North Rhine-Westphalia | February 2011 | October 2011 |
| Saarland | February 2010 | April 2010 |

ishment. Central to the question of causal inference is the outcome of a treated unit in the treated period if it had not been treated (counterfactual), in my case the migration pattern of students in states after the introduction of tuition fees if such fees had not been raised. The DiD approach argues that if these treated states exhibited similar trends in relevant variables as the other, non treated states (the control group) in the pre-treatment period, the outcome of the control group in the treatment period can be seen as possible counterfactual for the treatment group.

Before justifying this central common trend assumption in the subsequent subsection, I define the model to be estimated as follows:

$$Flow_{it} = \alpha + \beta_1 introduced_{it} + \beta_2 abolished_{it} + \beta_3 X_{it} + Z_i + T_t + \epsilon_{it} \tag{1.4}$$

$Flow_{it}$ represents the dependent flow variable (standardised inflow/outflow to a state). Our parameters of interest are $\beta_1$ and $\beta_2$ as they measure the effect of tuition fee introduction and abolishment on my dependent variable. $introduced_{it}$ is a binary treatment indicator turning one if state $i$ introduced tuition fees in year $t$. Importantly, this binary indicator stays one even after the state abolished the tuition fees again. A similar coding holds for $abolished_{it}$ if the respective state $t$ abolished the fees in year $t$. Consequently, this means that a state $i$ which once introduced tuition fees and abolished them in e.g. $t = 2$ has two "activated" treatment indicators ($introduced_{it}$ and $abolished_{it}$) from period $t = 2$ on. Therefore, $introduced_{it}$ measures the effect of an introduction of tuition fees on the dependent variable amongst all states whilst $abolished_{it}$ measures the mean effect of an introduction and succeeding abolishment of tuition fees on the dependent variable again for the entire group of German states.

$X_{it}$ represents my set of (1-period lagged) control variables (log of highschool graduates, unemployment rate, "G8-dummy" , GDP per capita - all for state $i$ at time $t$) while $Z_i$

implements state fixed effects and $T_t$ time fixed effects. The error term is represented by $\epsilon_{it}$.

As argued above the assumption of common trends before the treatment period for treatment and control group is central to the question whether we can interpret the coefficients $\beta_1$ and $\beta_2$ causally is. I rely on a graphical inspection of the variables of interest to test this assumption.

### 1.5.1 Common trend assumption

For the graphical inspection of the common trend assumption it is important to notice that there are two dates tuition fees were introduced. As depicted in Table 1.2, Lower-Saxony and North Rhine-Westphalia introduced tuition fees in 2006 whereas all other states (Baden-Wurttemberg, Bavaria, Hesse, Hamburg, Saarland) introduced tuition fees in 2007[14]. Since the following graphs are illustrated in an event-study like design, meaning that they are normalised to year 0 as the year of introduction (2006 or 2007), I always present two graphs: on the left-hand side always for the states introducing tuition fees in $t(0) = 2006$ and on the right-hand side with $t(0) = 2007$. In both cases the control group consists of those states which never introduced tuition fees.[15]

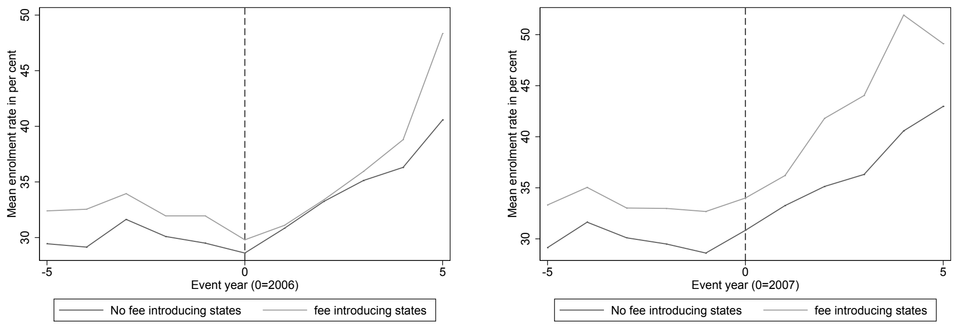
Figure 1.3 summarises important enrolment statistics and separates them by states which introduce and states which do not introduce tuition fees. As the graph nicely visualises, states of both regimes (with/without fees) have approximately parallel trends in their general enrolment rates a) and their external enrolment rates (number of freshmen who do not start to study in the state they received their university entrance diploma, normalised by the respective age cohort) for the time period before the introduction of tuition rates. Most interesting seems to be c) of Figure 1.3, visualising the mean net-migration rates, calculated as outflows minus inflows, normalised by the respective age cohort. A decrease in this rate can be interpreted as a higher "attractiveness" of the respective state to study in since this means that the number of those students flowing in becomes greater against the number of those students leaving the state to study somewhere else. Thus, the sharp drop for the no-fee-states (dark grey line) in both graphs of c) could hint towards a migration reaction of students to tuition fees by deciding more often to study in a no-fee-state. In general, graph c) of Figure 1.3 delivers further support for the common trend assumption previously to the introduction of tuition fees although there seems to be a relatively high level of heterogene-

[14]Since the data is on yearly basis, it is not possible to distinguish between different dates of introduction within one year.

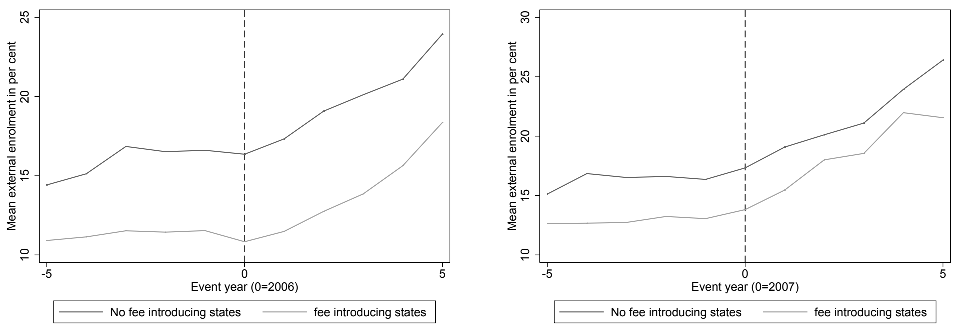
[15]Berlin, Brandenburg, Bremen, Mecklenburg-Western Pommerania, Rhineland-Palatinate, Saxony, Saxony-Anhalt, Schleswig-Holstein and Thuringia.

ity for states introducing tuition fees in 2007 after the year of introduction. The assumption of common trends as verified above is especially central for the investigation of the effects of the introduction of tuition fees since it validates that states seem to behave similar in the period previous to this introduction.

**a) Mean enrolment rates**




**b) Mean external enrolment rates**




**c) Mean net migration rates (out-inflows)**

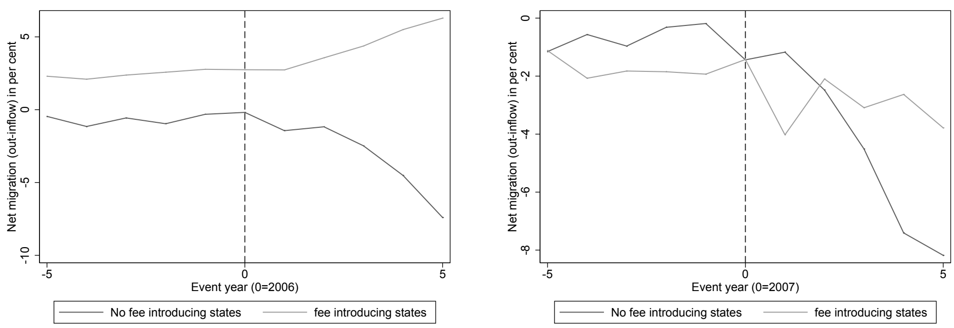



**Figure 1.3:** Mean enrolment rates normalised to states with & without the introduction of tuition fees

*Notes:* The left row is normalised to states, introducing tuition fees in 2006, the right row to states introducing tuition fees in 2007. Only German first year students and high school graduates considered. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a).

In contrast, the validity of the causal effect for the abolishment of these tuition fees highly depends on the question whether the pattern of migration flows depend on the duration of tuition fees, e.g. whether a state has a period of 1 or 5 years with tuition fees. I argue that the states seem to behave similar in this period, meaning that the coefficient on the variable $abolished_{it}$ in Equation 1.4 can be interpreted causally.

Figure A.1 - Figure A.3 in the Appendix are a graphical reproduction of a fully flexible time dummy regression for each fee introducing state separately. They show that the states seem to behave similar in all relevant flow variables within the period of tuition fees. However, two states seem to differ slightly, namely the "small states" Hamburg and Saarland. Therefore, results in section 1.6 are always further validated by a regression with a subset of states, leaving these two states out. The results of these analyses are available in Appendix A.2

## 1.6. Results

This section is split according to the set-up of the dataset used for the calculation of the empirical results. Whereas the results in subsection 1.6.1 are based on a "simple" panel dataset including $T = 16$ years (from 2000-2015) and $i = 16$ states[16], the underlying dataset for subsection 1.6.2 is build up in a dyadic manner, meaning that the unit of observation is not one state at one point in time but the combination of two states and the flows between these states at one point in time. More details of this setup are given at the beginning of subsection 1.6.2.

### 1.6.1 Results for a $n = 16$ approach

Starting with general enrolment rates as depicted in Table 1.4, we see that the introduction of tuition fees might even have positive effects for female students. These results somewhat contradict the findings of e.g. Hübner (2012) or Bruckmeier and Wigger (2014) finding negative or no results of an introduction of tuition fees on the transition rate from high school to university. However, it should be kept in mind that the dependent variable of Table 1.4 is the transition rate based on the state the student received her university entrance diploma. Therefore, my results take one step back in contrast to e.g. Hübner (2012) or Bruckmeier and Wigger (2014) who measure transition rates *post* migration while I measure transition

[16]The reason for the fact that the dataset of this part does not consist of $17 * 16 = 272$ observations but 236 observations is due to two facts: Firstly, the states of Mecklenburg-Western Pommerania and Saxony-Anhalt had to be taken out of the sample for $t = 2001$ due to a schooling reform (introduction of the 13st high school grade), leading to no representative results when calculating ratios. Secondly, to control for time specific effects one year had to be taken out of the sample to function as baseline year. This leads to the sample size $N = 236$ for the following results.

**Table 1.4:** Enrolment rates based on state of university entrance diploma

| | All | Female | Male |
|---|---|---|---|
| Tuition fees introduced | 0.0102* | 0.0137** | 0.0060 |
| | (0.0061) | (0.0067) | (0.0064) |
| Tuition fees abolished | 0.0091 | 0.0099 | 0.0081 |
| | (0.0057) | (0.0063) | (0.0060) |
| L.Dummy for introduction G8 | 0.0677*** | 0.0722*** | 0.0634*** |
| | (0.0058) | (0.0065) | (0.0061) |
| L.Local unemployment rate | −0.0039** | −0.0021 | −0.0055*** |
| | (0.0017) | (0.0015) | (0.0019) |
| L.Ln of high school graduates | 0.0529*** | 0.0526*** | 0.0499*** |
| | (0.0077) | (0.0086) | (0.0076) |
| L.Gdp per capita in federal state | −0.0000 | −0.0000 | −0.0000 |
| | (0.0000) | (0.0000) | (0.0000) |
| Observations | 236 | 236 | 236 |

*Notes:* L. stands for a 1-period lag of the respective variable. Yeardummies and state fixed effects not reported. Years: 2000-2015. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. An identical analysis for a subset of "big states" is conducted in Table A.1.

rates *pre* migration. Therefore, my results indicate that the introduction itself does not necessarily have negative effects of enrolment rates for prospective students receiving their high school diplomas in an introducing state. In fact, the enrolment rate in those states seem to have increased even after the introduction, especially for female prospective students. Their enrolment rate increases in fee introducing states after the introduction by on average 1.4 percent against no fee introducing states. The results stay robust against re-specifications of the sample as Table A.1 in the Appendix shows.

Coming to the more central question of this investigation, namely the movement patterns of prospective students in reaction to introduction and abolishment of tuition fees, Table 1.5 represents the results of regressing the external enrolment rate on the parameters as defined in Equation 1.4. Recalling that the external enrolment rate is measured as students with an university entrance diploma from state $i$ starting to study in every other state but $i$, normalised by the age specific cohort of $i$, shows that the external enrolment rate stays unchanged by the introduction of tuition fees. However, the following abolishment shows a significantly negative effect on the external enrolment, pronounced especially for male students. If a state abolishes tuition fees after the introduction, the rate of those students starting to study not in the "home" state decreases by 2 percent. This novel finding hints towards an asymmetric behaviour of students in reaction to tuition fees: whereas the ratio of those leaving their home state to study does not increase after the introduction of tuition fees, this figure decreases after the respective state decides to abolish tuition fees again.

These findings are further validated by the results of the net migration rate as depicted in Table 1.6. An increase in net migration means that the number of students leaving the state increases against the number of those entering the state, all normalised by the age

**Table 1.5:** Regression of external enrolment rate of freshmen

| | All | Female | Male |
|---|---|---|---|
| Tuition fees introduced | 0.0034 | 0.0002 | 0.0058 |
| | (0.0071) | (0.0078) | (0.0068) |
| Tuition fees abolished | $-0.0166^{**}$ | $-0.0146^{**}$ | $-0.0181^{***}$ |
| | (0.0066) | (0.0073) | (0.0064) |
| L.Dummy for introduction G8 | $0.0332^{***}$ | $0.0381^{***}$ | $0.0284^{***}$ |
| | (0.0068) | (0.0075) | (0.0065) |
| L.Local unemployment rate | $-0.0048^{**}$ | $-0.0020$ | $-0.0073^{***}$ |
| | (0.0020) | (0.0017) | (0.0021) |
| L.Ln of high school graduates | $0.0198^{**}$ | 0.0129 | $0.0226^{***}$ |
| | (0.0090) | (0.0100) | (0.0081) |
| L.Gdp per capita in federal state | $-0.0000$ | $-0.0000$ | $-0.0000$ |
| | (0.0000) | (0.0000) | (0.0000) |
| Observations | 236 | 236 | 236 |

*Notes:* L. stands for a 1-period lag of the respective variable. Yeardummies and state fixed effects not reported. Years: 2000-2015. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a). $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. An identical analysis for a subset of "big states" is conducted in Table A.2.

specific cohort of the respective state. Therefore, the negative coefficient on the tuition fee abolishment dummy in Table 1.6 is in line with the findings of Table 1.5 indicating that the abolishment of tuition fees negatively influences the net migration rate which can either be due to a higher inflow of students or a lower outflow of students as argued in Table 1.5. This abolishment lowers the net migration rate for male prospective students by roughly 2 percent which is in line with the findings of previous calculations and has a similar order of magnitude[17]. Analysing the mean outflow of prospective students to states which introduce tuition fees, the variable $FOM_{it}$ from subsection 1.4.1 is estimated in the upper part of Table 1.7 whilst the outflows to states which never introduced tuition fees ($NFOM_{it}$) are investigated in the lower part of Table 1.7.

The results of Table 1.7 further verify my previous findings, namely that although the introduction of tuition fees does not seem to have significant effects on the movement pattern as found as well by e.g. Bruckmeier and Wigger (2014), its abolishment in contrast seems to be relatively important for the migration patterns of students stemming from these states. Table 1.7 shows that for all three subgroups (all, female, male) the ratio of those students starting to study in a state with tuition fee decreases if the own home state abolishes tuition fees. This effect is relatively large for male prospective students where the ratio of those starting to study in another fee charging state decreases by roughly 2 percent against all other fee states if studying in the home state becomes costless again. At the same time, I cannot observe any effect of tuition fees on the outflow ratio to no fee introducing states as

[17]However, these results should be treated with caution as they stay not robust against regressing on only a sub sample of states as shown in Table A.4 in the Appendix. Therefore it might be the case that these results are mainly driven by the two small states Saarland and Hamburg, exhibiting slightly different migration patterns than the remaining 5 fee introducing states.

**Table 1.6:** Net migration rate (standardised out-inflows)

| | All | Female | Male |
|---|---|---|---|
| Tuition fees introduced | −0.0032 | −0.0010 | −0.0004 |
| | (0.0108) | (0.0127) | (0.0102) |
| Tuition fees abolished | −0.0145 | −0.0046 | −0.0224** |
| | (0.0100) | (0.0118) | (0.0094) |
| L.Dummy for introduction G8 | 0.0307*** | 0.0373*** | 0.0244** |
| | (0.0103) | (0.0122) | (0.0096) |
| L.Local unemployment rate | 0.0097*** | 0.0064** | 0.0099*** |
| | (0.0030) | (0.0028) | (0.0031) |
| L.Ln of high school graduates | 0.0755*** | 0.0838*** | 0.0807*** |
| | (0.0136) | (0.0162) | (0.0120) |
| L.Gdp per capita in federal state | −0.0000* | −0.0000** | −0.0000 |
| | (0.0000) | (0.0000) | (0.0000) |
| Observations | 236 | 236 | 236 |

*Notes:* L. stands for a 1-period lag of the respective variable. Yeardummies and state fixed effects not reported. Years: 2000-2015. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. An identical analysis for a subset of "big states" is conducted in Table A.4.

**Table 1.7:** Outflow to states, separated by regime (fee/no fee)

| | All | Female | Male |
|---|---|---|---|
| | | *to fee introducing states* | |
| Tuition fees introduced | −0.0021 | −0.0024 | −0.0011 |
| | (0.0031) | (0.0032) | (0.0033) |
| Tuition fees abolished | −0.0156*** | −0.0134*** | −0.0171*** |
| | (0.0029) | (0.0030) | (0.0031) |
| | | *to no fee introducing states* | |
| Tuition fees introduced | 0.0055 | 0.0026 | 0.0068 |
| | (0.0056) | (0.0063) | (0.0051) |
| Tuition fees abolished | −0.0010 | −0.0013 | −0.0010 |
| | (0.0052) | (0.0059) | (0.0048) |
| Observations | 236 | 236 | 236 |

*Notes:* Yeardummies and state fixed effects not reported. Controls for "G8", unemployment rates and gdp per capita for both states not reported. Years: 2000-2015. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. An identical analysis for a subset of "big states" is conducted in Table A.5 and Table A.6.

the lower part of Table 1.7 shows.

This finding of a negative effect of the outflow to other states after the abolishment (and introduction) of fees could be explained by the financial endowment of universities after the period of tuition fees. Whilst the universities within the fee charging states generated significant levels of income through this fees (e.g. in entire Bavaria €219 Million per year (Scherf, 2013)), most universities complained successfully against a cut of this income due to the abolishment of tuition fees. Therefore, the public hand stepped in to compensate for this loss according to private contact to the *Hochschulrektorenkonferenz* (Conference of University Presidents). Therefore, students could enjoy universities with better financial endowments without having to finance this endowment, at least partially via fees. This might explain the lower level of outflow to other states (and therefore higher attractiveness of home universities) after the period of tuition fees.

### 1.6.2 Results for a dyadic approach

In contrast to the previous section, the underlying dataset for analyses in this part is built in a dyadic manner, meaning that for each year I form every possible pairwise combination of states, leading to $16 * 16 = 256$ observations for each year. The advantage of this set-up is the possibility to measure flows between two states directly and not a sum of states (e.g. flows from all fee introducing states to all no fee introducing states). As Mitze et al. (2015, p.401) argue, the pairwise-approach is useful since "analysing the tuition fee effect for the overall regional migration balance, may lead to an underestimation of the true impact". The reason for this possible underestimation is due to the summation of flows as the numerical example in Figure 1.4 visualises.

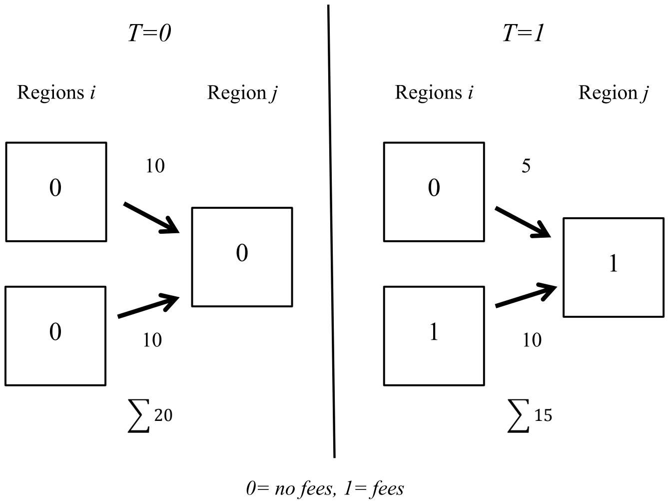



**Figure 1.4:** Numeric example for flows in dyadic approach following Mitze et al. (2015)

In a simple three country example, all drawn as boxes, the switch from region *j* from a no fee charging to a fee charging regime (from 0 to 1) would imply a reduction in flows from regions, subsumed as regions *i* of about $(20 - 15)/20 = 25\%$. However, since we cannot disentangle in the $n = 16$ approach from which states (with/without tuition fees) this reductions stem from, this approach might lose some precision. As the numerical example shows, the reduction stems entirely from the region in *i* which is still without tuition fees, the flow between the region in *i* introducing tuition fees and region *j* stays unchanged.

In addition to this advantage in being able to disentangle the flows by regimes (no-fee to fee; no-fee to no-fee, fee to fee and the other way round) I am now able to control for adjacencies of each state pair. Since investigations like those of Bruckmeier et al. (2013) or Mitze et al. (2015) argue that the mobility of students in Germany seems to be rather low, incor-

**Table 1.8:** Standardised flows from 2 to 1 if state 1 introduces fees and state 2 does not - neighbouring states

| | All | Female | Male |
|---|---|---|---|
| Tuition fees introduced in state 1 | $-0.0106^{***}$ | $-0.0161^{***}$ | $-0.0049^{*}$ |
| | (0.0031) | (0.0034) | (0.0028) |
| Tuition fees abolished in state 1 | $-0.0013$ | $-0.0029$ | 0.0012 |
| | (0.0026) | (0.0028) | (0.0024) |
| Observations | 450 | 450 | 450 |

*Notes:* Yeardummies and state fixed effects not reported. Controls for "G8", unemployment rates and gdp per capita for both states not reported. Years: 2000-2015. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a). $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. An identical analysis for a subset of "big states" is conducted in Table A.7.

porating the adjacency of two states should be plausible since moving into a neighbouring state to avoid tuition fees might be less costless than moving to another, not neighbouring state far away.

A pairwise investigation of each possible combination of states lead to an increase in the number of observations. Instead of having 16 observations per year, this number is increased to $16 * 15 = 240$ state pairs (observations) for each year of interest. The different number of observations for the following results are due to drawing different subgroups, e.g. investigating only the flows from pairs with one introducing and one not introducing state. Additional restrictions may occur if I restrict to neighbouring states only.

The most straightforward specification is presented in Table 1.8: The effect of the introduction (and abolishment) of tuition fees in state 1 on the flow from state 2, which never introduces tuition fees if these two states share a common border. The flows are again standardised by the age specific population of the home state (state 2). Control variables are the same as in subsection 1.6.1, now for both states of the respective state pair.

Interestingly, the coefficient on the introduction of tuition fees in Table 1.8 exhibits a negative sign which is in contrast to the results of the previous section, where the introduction of tuition fee did not seem to have any effect on the student flows. However, this might be especially due to the neighbourhood of the state pairs I restricted the analysis of Table 1.8 to. Additionally, this negative effect might have gone lost in the $n = 16$ approach because of the lower level of precision, where the flows between neighbouring and not neighbouring states might have washed out a clear statistical effect. This result is in line with the findings of authors like Bruckmeier et al. (2013) or Mitze et al. (2015) arguing that students in Germany seem to be willing to avoid tuition fees only if costs (and therefore distances from the home state/town) are relatively small.

Here is a numerical example to visualise the dimension of the change in flows due to tuition fees: Whereas in the year 2006 roughly 2 percent of the age specific cohort from

**Table 1.9:** Standardised flows from 2 to 1 if state 1 introduces fees and state 2 does not - not neighbouring states

| | All | Female | Male |
|---|---|---|---|
| Tuition fees introduced in state 1 | −0.0008 | −0.0010 | −0.0006 |
| | (0.0014) | (0.0014) | (0.0015) |
| Tuition fees abolished in state 1 | −0.0018 | −0.0019 | −0.0017 |
| | (0.0013) | (0.0013) | (0.0014) |
| Observations | 1710 | 1710 | 1710 |

*Notes:* Yeardummies and state fixed effects not reported. Controls for "G8", unemployment rates and gdp per capita for both states not reported. Years: 2000-2015. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. An identical analysis for a subset of "big states" is conducted in Table A.8.

Saxony started to study in the neighbouring state Bavaria, this number goes down to approximately 1.5 percent of the age specific cohort for 2008, the year when Bavaria introduced tuition fees. In total numbers, the number of first year students in Bavaria from Saxony went down from 907 in the year 2006 to 673 in 2008. The comparison between the total with the relative numbers shows the importance of normalising the number of students by the age specific cohort to control for trends in population growth

Interestingly, there does not seem to be an effect of the abolishment of tuition fees for state 1 on its inflows from neighbouring and not fee introducing states. This is somewhat puzzling since you would expect a more symmetric behaviour of prospective students on the abolishment of tuition fees: if the inflows to an introducing state went down after the introduction, a significant increase from these states after the abolishment would be intuitive.

In contrast to Table 1.8 investigating only neighbouring states, Table 1.9 is calculated based on state pairs with no common border. The insignificant coefficients on both dummies, the introduction and the abolishment of tuition fees is in line with previous research. If it becomes costly for a student to avoid tuition fees since she would have to move relatively far, the willingness to do so decreases significantly. At the same time, if e.g. the state of Hamburg abolishes tuition fees, this should be only of minor interest for a student who has to decide about studying in her home state relatively far away, e.g. in Saxony.

### 1.6.3 Geographical vs timed avoidance of tuition fees

So far, I only analysed direct flows between states in reaction to the introduction and abolishment of tuition fees. However, there could be other ways how prospective students avoid paying tuition fees. Whereas students can only avoid paying tuition fees after the introduction in 2006 and 2007 by emigrating to another, no fee charging, state, subsequent students have different possibilities to avoid fees when it becomes clear that they will be abolished. As Table 1.2 and Table 1.3 in section 1.5 show, there is a gap between the enacting of the introduction and abolishment of tuition fees and the realisation of those laws (i.e. the "true" introduction and abolishment of those fees). Students who plan to start studying sometime in the period between the enacting and the realisation of the abolishment of tuition fees could therefore simply wait to avoid paying tuition fees. This could possibly have the advantage of being able to study cost free *without* the need to leave the home state. Therefore, I argue that prospective students who receive their university entrance diploma relatively late in the tuition fee regime period in Germany have two possibilities to avoid paying fees. Either, if they wish to start studying immediately they could move to another state. Or, if they want to stay at home they could wait a few terms since they know the point in time when tuition fees will be abolished in the home state.

Federal Statistical Office (2019b) provides statistics for each state on transition ratios from high school to university, depending on the lag between graduation from high school and the enrolment at university. Therefore, I am able to check whether the rates of students who e.g. start to study one or two years after high school graduation increase if it is clear that tuition fees will be abolished in one or two years respectively. To give a first graphical impression, Figure 1.5 visualises the so called transition-index over time and the group of treated (fee introducing) and not treated states. Thereby, I define this transition-index according to the following formula:

$$Transition - index_{it} = \sum_{p=0}^{5} \frac{quota_{itp}}{\text{years since highschool graduation(p)} + 1} \tag{1.5}$$

where $quota_{itp}$ represents the following ratio: $\frac{\text{Freshmen starting } p \text{ years after graduating in } i \text{ at } t \text{ to study}}{\text{the sum of all high school graduates in } i \text{ at } t}$. The index in Equation 1.5 basically sums up the transition rates for a high school graduation year over the succeeding 5 years, each quota weighted by the inverse of the number of years since high school graduation.[18] Although the absolute values of the index cannot be interpreted instantaneously, the index is helpful to analyse how "fast" a high school graduation cohort passes over to university studies. The lower the index, the bigger is the sum of gaps between high school graduation and university enrolment.

[18] I add $+1$ to every weight to ensure that the transition rate directly after high school graduation, therefore in year 0 is not divided by 0

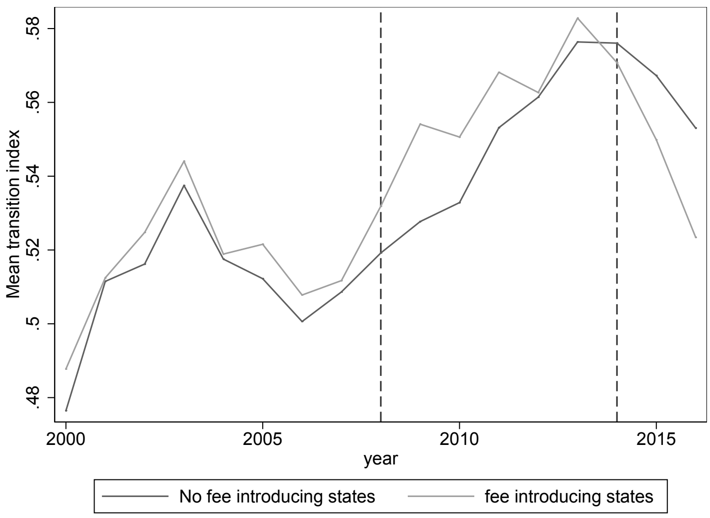



**Figure 1.5:** The transition rate of students over time (based on a self-calculated transition-index)

*Notes:* Source: Federal Statistical Office (2019b).

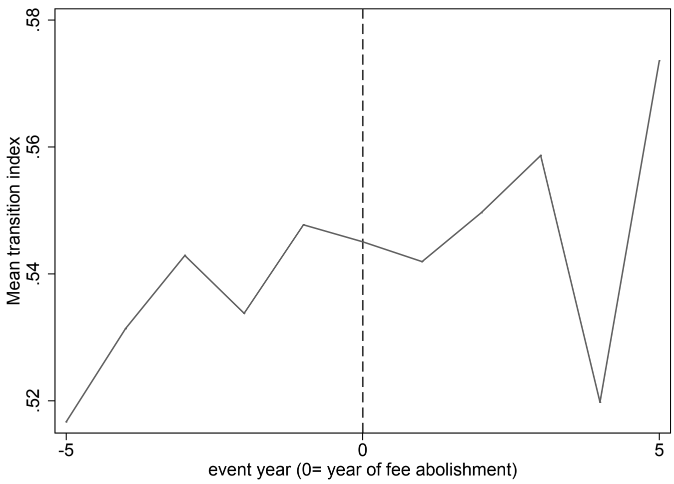



**Figure 1.6:** The transition rate of students over time - event study (based on a self-calculated transition-index)

*Notes:* Source: Federal Statistical Office (2019b).

The two vertical dashed lines in Figure 1.5 represent very generally the first date a state (Hesse in 2008) abolished tuition fee up to the last date a state changed back to the tuition fee free regime (Lower-Saxony in 2014). The graph shows no clear trend in a change in the transition speed for treated states in the period of tuition fee abolishment or previously.

In general, it should be noted that the sharp decline of the index in the end of the period of interest is due to the construction of the index: since for e.g. 2015, we can only observe 1 following year (2016), the index has to decrease against an index for an earlier year due to the fact that for e.g. 2010 I am able to observe all relevant subsequent 5 years to calculate the index according to Equation 1.5.

Generally, the transition index has a clear upward slope for both types of state regimes (with/without tuition fees), indicating that the speed high school graduates enrolled at universities has increased over recent years. Interestingly, there is a sharp drop around the years 2006/2007 coinciding with the 2 years where states introduced tuition fees. Afterwards, the transition speed in both regime states seem to increase significantly.

Figure 1.6 shows a transition index, normalised to the year, a state abolishes tuition fees, and as mean over all abolishing states . Similar to the previous graph, I cannot detect any significant change around the abolishment date which would hint towards a delay in enrolment rates to avoid tuition fees.

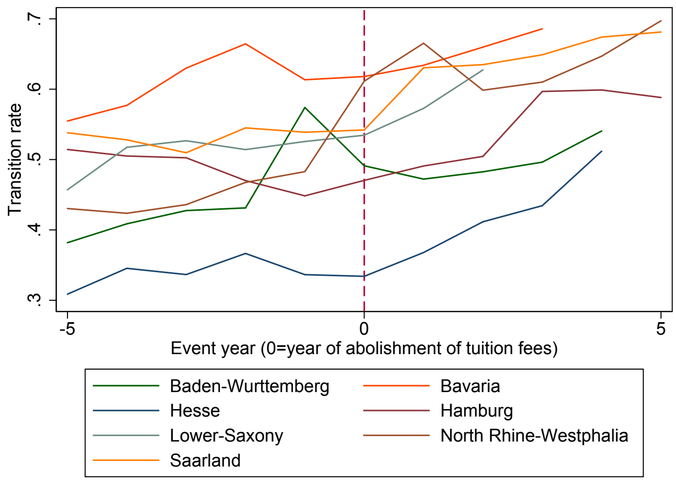



**Figure 1.7:** The transition rate of students normalised to the announcement of tuition fee abolishment

*Notes:* Source: Federal Statistical Office (2019b).

I use the difference between the date of enacting and the date of realisation of the law abolishing tuition fees to generate the respective transition ratio of interest to investigate in more detail whether students delayed their decision to start studying to avoid tuition fees. If e.g. a state announces before October in year $t$ that it will abolish tuition fees beginning in year $t + 1$, the transition rate 1 year after graduation should be significantly higher for the cohort graduating in $t$ if students delay their decision to study. Figure 1.7 depicts this relevant transition rate for all treated states normalised to the year when the information about the abolishment became public. If students avoid tuition fees by delaying the enrolment,

**Table 1.10:** Change in transition rate from high school to university after abolishment of tuition fees

| | All | Female | Male |
|---|---|---|---|
| Abolishment of tuition fees | $-0.0167$ | 0.0032 | $-0.0249$ |
| | (0.0178) | (0.0236) | (0.0285) |
| L.Dummy for introduction G8 | $-0.0075$ | 0.0003 | $-0.0152$ |
| | (0.0150) | (0.0197) | (0.0242) |
| L.Local unemployment rate | 0.0081 | 0.0508 | $0.0962^{**}$ |
| | (0.0315) | (0.0418) | (0.0387) |
| L.Ln of high school graduates | $-0.0609^{**}$ | $-0.0359$ | $-0.0864^{*}$ |
| | (0.0280) | (0.0365) | (0.0455) |
| L.Gdp per capita in federal state | $-0.0000$ | $-0.0000$ | 0.0000 |
| | (0.0000) | (0.0000) | (0.0000) |
| Observations | 58 | 58 | 58 |

*Notes:* L. stands for a 1-period lag of the respective variable. Yeardummies and state fixed effects not reported. Years: 2000-2015. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a). $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. An identical analysis for a subset of "big states" is conducted in Table A.9.

the enrolment rate should peak at the zero point since this would visualise the decision of students to delay the decision up to the point when studying in the respective state becomes costless.

The graph does not show any significant peak at the event date (the announcement about the tuition fee abolishment), meaning that based on this graph students do not time their enrolment decision to avoid tuition fees. Rather, the transition rates seem to be relatively unstable without the tendency of a clear trend over time. Therefore, an econometric specification is conducted to verify whether there are timing reactions after the information about the abolishment of tuition fees to avoid paying exactly these fees.

Regressing the relevant transition rate on a dummy indicating the abolishment of tuition fees in accordance with Equation 1.4 shows that there is no significant effect of the abolishment of tuition fees on the transition speed from high school to university. In fact, Table 1.10 indicates that there are no gender differences in the reaction to tuition fee abolishment in terms of delaying the enrolment decision to save fees.

The results in Figure 1.7 and Table 1.10, however, should be interpreted with caution due to two reasons. First, since data is available only on a yearly basis and as Table 1.3 shows, only one state (namely Hamburg) has a significant gap between announcement and realisation of the abolishing law, Hamburg is the only state I could really measure whether students delay the enrolment decision one year later to avoid fees (however, I could not find significant results for Hamburg either). Secondly, the transition rates are based on the state the student received her university entrance diploma. Therefore, I am not able to distinguish between students who start to study in the home state and in other states. By saying so, the results summarise within- and between-state transition, meaning that there could be some effect of the announcement of the tuition fee abolishment on the internal transition rate, I am not able to account for.

Although I cannot find any significant evidence that students avoid tuition fees by timing their enrolment decision, this result is important since it further supports the validity of my identification strategy for the detection of migration patterns in reaction to the introduction and abolishment of tuition fees. Such anticipatory effects can be problematic since it may dilute the explanatory power of the evaluation of quasi-experimental settings as the one investigated here.[19] Therefore, I take the insignificance of results regarding timing decisions as a further argument for the validity of my identification strategy as set up in Equation 1.4.

## 1.7. Conclusion

The question how tuition fees influence the decision of prospective students to enrol has been investigated comprehensively, especially for the UK, the USA or Canada with a long history of tuition fees for tertiary education. The German case is somewhat different due to the relatively low level of tuition fees and a short period of less than 10 years when 7 states introduced tuition fees but abolished them shortly afterwards. Furthermore, fees were introduced relatively equal in their amount (roughly €500 per term) amongst introducing states which makes a direct comparison to the very heterogeneous setting in the US and Canada harder.

The results of my investigation of the effects of these tuition fees on the migration patterns of German students sketch a relatively ambiguous picture in the first view: Although the total numbers of students migrating to or from a fee introducing state does not seem to change after the introduction of tuition fees, the effects do not seem to be symmetric for the case of the abolishment of those fees. If the home state abolishes tuition fees, the number of students emigrating to other states with tuition fees decreases significantly.

A more detailed picture was drawn by employing a pairwise dataset using the flows between each two state combination to find possible migration patterns. This refined approach puts the proximity of two states into focus, showing that there is a significant decrease in the flows from the home to the neighbouring state if this neighbouring state introduces tuition fees. This effect vanishes if the states do not share a common border. Additionally, the abolishment of tuition fees does not seem to have changed the flows between

[19] See e.g. Alpert (2016) for an illustrative example on the importance of considering anticipatory effects.

two states.

As a last part, the possibility of avoiding tuition fees by delaying the enrolment decision up to the point when studying is costless again (due to the abolishment of tuition fees) is investigated. However, I cannot find any evidence that students anticipated the abolishment of tuition fees and therefore delayed the enrolment decision. This result, however, should be seen carefully since a more detailed investigation by using monthly (or at least term wise) data should be able to reconcile possible anticipation reactions more comprehensively.

My results refine the findings of e.g. Bruckmeier and Wigger (2014), arguing that there are no changes in the transition effects from high school to university when introducing fees. In contrast to the authors, I control for between state migration, finding even positive effects of the introduction of tuition fees on the transition rate.

Furthermore, I validate the importance of controlling for proximity between two states when investigating the change in flows in reaction to tuition fees as Mitze et al. (2015) argues: Whereas the flows into a fee charging state from a fee free state decrease when the states share a common border, there are no significant effects if these states are no direct neighbours. A reverse effect, however, cannot be found after the abolishment of tuition fees. Additionally, I cannot find any evidence for the argument that prospective students may delay their enrolment decision to avoid paying tuition fees in their home state.

The question, why tuition fees seem to have such a small effect on migration patterns of German students should be answered by putting the level of fees in relation to the total costs of studying in Germany. According to a recent survey of Middendorff et al. (2016), students in Germany have on average a disposable monthly income of €918 (in the year 2016), adding up to €5,508 per term. The question whether to pay tuition fees of roughly €500 per term would increase the costs of studying by less than 10 %. Therefore, tuition fees in Germany do not seem to change costs of studying dramatically for at least a great part of the student body. Additionally, it should be kept in mind that many states implemented generous exemptions from paying those fees. As an example, more than 30 % of students in Bavaria were exempt from paying tuition fees (Bruckmeier and Wigger, 2014, p.20).

Therefore, the findings of only minor effects of tuition fees on migration patterns in Germany seem to be plausible and should be treated differently than the effects, tuition fees in countries like the USA can trigger where those fees often account for the greatest costs of succeeding a university degree.

## Appendix A

### A.1 Further validation of common trend assumption

Figure A.1 - Figure A.3 represent linear regressions of the main outcome variables of interest mean external enrolment rate, general enrolment rate and net migration rate on time dummies for each year and treated state. Therefore, each subgraph of Figure A.1 - Figure A.3 represents a regression of the respective treated state and all control states on treated-state-specific time dummies and a set of general time dummies, meaning that the estimates represent the most flexible estimation of the effects of tuition fee introduction and abolishment for treated states. The first vertical line represents the introduction of tuition fees, the second vertical line its abolishment for each state. As the graphs show, the states behave relatively similar with the exception of Saarland and Hamburg, exhibiting somewhat different patterns.

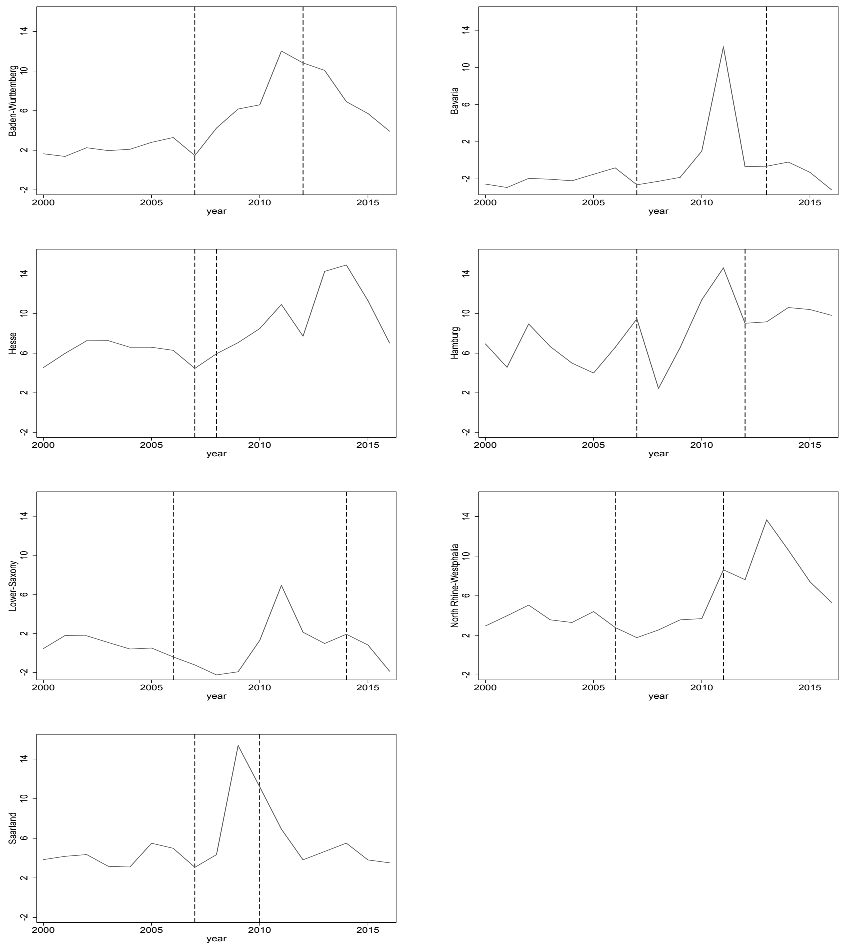



**Figure A.1:** Time dummy regression - Mean enrolment rate by fee introducing states

*Notes:* Each subgraph represents a regression of the respective treated state and all control states on a set of time dummies for the treated state and general time dummies. Only German first year students and high school graduates considered. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a).

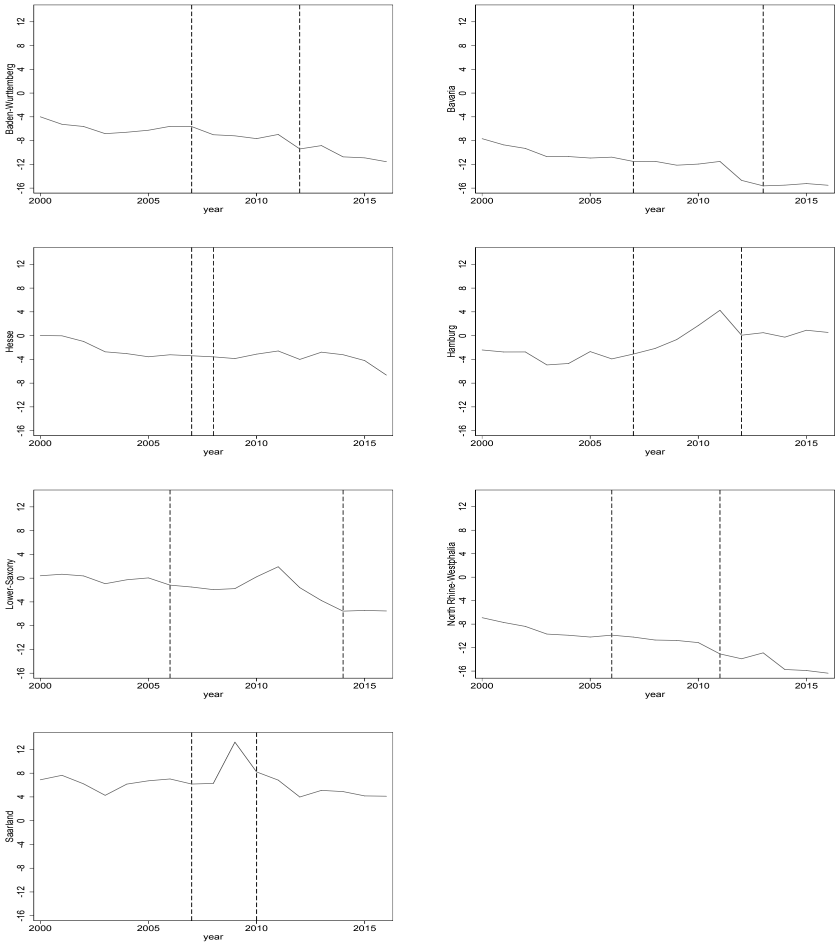



**Figure A.2:** Time dummy regression - mean external enrolment rates by fee introducing states

*Notes:* Each subgraph represents a regression of the respective treated state and all control states on a set of time dummies for the treated state and general time dummies. Only German first year students and high school graduates considered. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a).

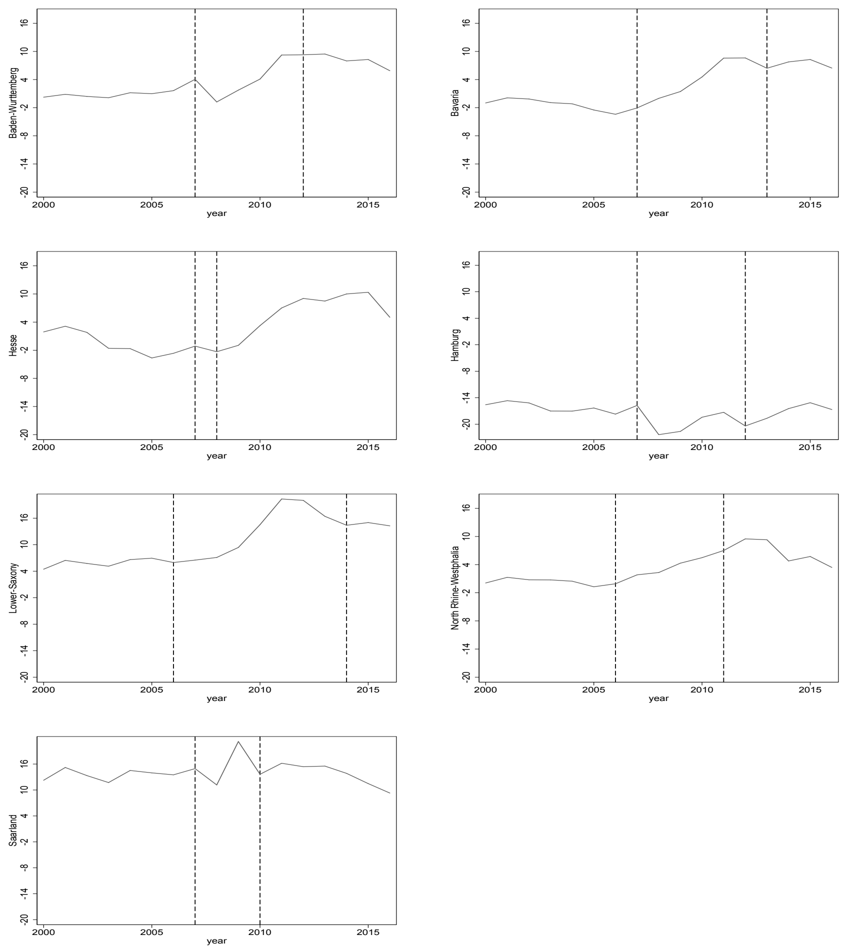



**Figure A.3:** Time dummy regression - Mean net migration by fee introducing states

*Notes:* Each subgraph represents a regression of the respective treated state and all control states on a set of time dummies for the treated state and general time dummies. Only German first year students and high school graduates considered. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a).

## A.2 Robustness checks

The tables in this section (Table A.1 - Table A.9) basically reconcile the estimations from section 1.6, however for a smaller sample leaving the two states Hamburg and Saarland out since these small states seem to exhibit slightly different patterns from the other "big" states.

**Table A.1:** Enrolment rates based on state of university entrance diploma - no small states

| | All | Female | Male |
|---|---|---|---|
| Tuition fees introduced | 0.0104 | 0.0128* | 0.0071 |
| | (0.0064) | (0.0068) | (0.0066) |
| Tuition fees abolished | 0.0110* | 0.0141** | 0.0074 |
| | (0.0063) | (0.0068) | (0.0065) |
| L.Dummy for introduction G8 | 0.0676*** | 0.0710*** | 0.0644*** |
| | (0.0063) | (0.0068) | (0.0064) |
| L.Local unemployment rate | −0.0043** | −0.0032** | −0.0051** |
| | (0.0017) | (0.0015) | (0.0020) |
| L.Ln of high school graduates | 0.0573*** | 0.0631*** | 0.0482*** |
| | (0.0081) | (0.0086) | (0.0078) |
| L.Gdp per capita in federal state | −0.0000 | −0.0000 | −0.0000 |
| | (0.0000) | (0.0000) | (0.0000) |
| Observations | 206 | 206 | 206 |

*Notes:* L. stands for a 1-period lag of the respective variable. Yeardummies and state fixed effects not reported. Years: 2000-2015. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Table A.2:** Regression of external enrolment rate of freshmen - no small states

| | All | Female | Male |
|---|---|---|---|
| Tuition fees introduced | −0.0049 | −0.0066 | −0.0043 |
| | (0.0074) | (0.0083) | (0.0068) |
| Tuition fees abolished | −0.0228*** | −0.0212** | −0.0242*** |
| | (0.0073) | (0.0082) | (0.0068) |
| L.Dummy for introduction G8 | 0.0325*** | 0.0374*** | 0.0274*** |
| | (0.0073) | (0.0082) | (0.0067) |
| L.Local unemployment rate | −0.0044** | −0.0027 | −0.0054*** |
| | (0.0020) | (0.0018) | (0.0021) |
| L.Ln of high school graduates | 0.0154 | 0.0144 | 0.0129 |
| | (0.0093) | (0.0104) | (0.0081) |
| L.Gdp per capita in federal state | −0.0000 | −0.0000 | −0.0000 |
| | (0.0000) | (0.0000) | (0.0000) |
| Observations | 206 | 206 | 206 |

*Notes:* L. stands for a 1-period lag of the respective variable. Yeardummies and state fixed effects not reported. Years: 2000-2015. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Table A.3:** Regression of the net migration rate - no small states

| | All | Female | Male |
|---|---|---|---|
| Tuition fees introduced | 0.0112 | 0.0180 | 0.0070 |
| | (0.0107) | (0.0119) | (0.0106) |
| Tuition fees abolished | −0.0055 | 0.0062 | −0.0163 |
| | (0.0106) | (0.0119) | (0.0105) |
| L.Dummy for introduction G8 | 0.0300*** | 0.0354*** | 0.0251** |
| | (0.0106) | (0.0119) | (0.0104) |
| L.Local unemployment rate | 0.0079*** | 0.0049* | 0.0098*** |
| | (0.0029) | (0.0026) | (0.0032) |
| L.Ln of high school graduates | 0.0940*** | 0.1059*** | 0.0919*** |
| | (0.0136) | (0.0151) | (0.0125) |
| L.Gdp per capita in federal state | −0.0000** | −0.0000** | −0.0000** |
| | (0.0000) | (0.0000) | (0.0000) |
| Observations | 206 | 206 | 206 |

*Notes:* L. stands for a 1-period lag of the respective variable. Yeardummies and state fixed effects not reported. Years: 2000-2015. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Table A.4:** Net migration rate (standardised out-inflows) - no small states

| | All | Female | Male |
|---|---|---|---|
| Tuition fees introduced | 0.0112 | 0.0180 | 0.0070 |
| | (0.0107) | (0.0119) | (0.0106) |
| Tuition fees abolished | −0.0055 | 0.0062 | −0.0163 |
| | (0.0106) | (0.0119) | (0.0105) |
| L.Dummy for introduction G8 | 0.0300*** | 0.0354*** | 0.0251** |
| | (0.0106) | (0.0119) | (0.0104) |
| L.Local unemployment rate | 0.0079*** | 0.0049* | 0.0098*** |
| | (0.0029) | (0.0026) | (0.0032) |
| L.Ln of high school graduates | 0.0940*** | 0.1059*** | 0.0919*** |
| | (0.0136) | (0.0151) | (0.0125) |
| L.Gdp per capita in federal state | −0.0000** | −0.0000** | −0.0000** |
| | (0.0000) | (0.0000) | (0.0000) |
| Observations | 206 | 206 | 206 |

*Notes:* L. stands for a 1-period lag of the respective variable. Yeardummies and state fixed effects not reported. Years: 2000-2015. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Table A.5:** Outflow to fee introducing states - no small states

| | All | Female | Male |
|---|---|---|---|
| Tuition fees introduced | −0.0052* | −0.0042 | −0.0053 |
| | (0.0031) | (0.0033) | (0.0033) |
| Tuition fees abolished | −0.0173*** | −0.0150*** | −0.0188*** |
| | (0.0031) | (0.0033) | (0.0032) |
| L.Dummy for introduction G8 | 0.0096*** | 0.0109*** | 0.0083*** |
| | (0.0031) | (0.0033) | (0.0032) |
| L.Local unemployment rate | 0.0045*** | 0.0040*** | 0.0042*** |
| | (0.0008) | (0.0007) | (0.0010) |
| L.Ln of high school graduates | 0.0027 | 0.0031 | 0.0051 |
| | (0.0039) | (0.0042) | (0.0038) |
| L.Gdp per capita in federal state | −0.0000 | −0.0000 | −0.0000 |
| | (0.0000) | (0.0000) | (0.0000) |
| Observations | 206 | 206 | 206 |

*Notes:* L. stands for a 1-period lag of the respective variable. Yeardummies and state fixed effects not reported. Years: 2000-2015. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Table A.6:** Outflow to no fee introducing states - no small states

| | All | Female | Male |
|---|---|---|---|
| Tuition fees introduced | 0.0002 | −0.0023 | 0.0009 |
| | (0.0060) | (0.0067) | (0.0055) |
| Tuition fees abolished | −0.0056 | −0.0062 | −0.0054 |
| | (0.0059) | (0.0066) | (0.0055) |
| L.Dummy for introduction G8 | 0.0229*** | 0.0265*** | 0.0190*** |
| | (0.0059) | (0.0067) | (0.0054) |
| L.Local unemployment rate | −0.0089*** | −0.0067*** | −0.0097*** |
| | (0.0016) | (0.0014) | (0.0017) |
| L.Ln of high school graduates | 0.0127* | 0.0113 | 0.0078 |
| | (0.0075) | (0.0084) | (0.0065) |
| L.Gdp per capita in federal state | −0.0000 | −0.0000 | −0.0000 |
| | (0.0000) | (0.0000) | (0.0000) |
| Observations | 206 | 206 | 206 |

*Notes:* L. stands for a 1-period lag of the respective variable. Yeardummies and state fixed effects not reported. Years: 2000-2015. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Table A.7:** Standardised flows if state 1 introduces fees and state 2 does not - neighbouring states - no small states

| | All | Female | Male |
|---|---|---|---|
| Tuition fees introduced in state 1 | −0.0116*** | −0.0175*** | −0.0053* |
| | (0.0033) | (0.0036) | (0.0030) |
| Tuition fees abolished in state 1 | −0.0009 | −0.0025 | 0.0018 |
| | (0.0028) | (0.0030) | (0.0026) |
| Observations | 420 | 420 | 420 |

*Notes:* Yeardummies and state fixed effects not reported. Controls for "G8", unemployment rates and gdp per capita for both states not reported. Years: 2000-2015. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Table A.8:** Standardised flows if state 1 introduces fees and state 2 does not - no neighbouring states - no small states

| | All | Female | Male |
|---|---|---|---|
| Tuition fees introduced in state 1 | −0.0003 | −0.0007 | 0.0001 |
| | (0.0017) | (0.0017) | (0.0018) |
| Tuition fees abolished in state 1 | −0.0009 | −0.0009 | −0.0009 |
| | (0.0016) | (0.0017) | (0.0017) |
| Observations | 1470 | 1470 | 1470 |

*Notes:* Yeardummies and state fixed effects not reported. Controls for "G8", unemployment rates and gdp per capita for both states not reported. Years: 2000-2015. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a). * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Table A.9:** Change in transition rate from high school to university after abolishment of tuition fees - no small states

| | All | Female | Male |
|---|---|---|---|
| Abolishment of tuition fees | $-0.0167$ | $0.0032$ | $-0.0249$ |
| | (0.0178) | (0.0236) | (0.0285) |
| L.Dummy for introduction G8 | $-0.0075$ | $0.0003$ | $-0.0152$ |
| | (0.0150) | (0.0197) | (0.0242) |
| L.Local unemployment rate | $0.0081$ | $0.0508$ | $0.0962^{**}$ |
| | (0.0315) | (0.0418) | (0.0387) |
| L.Ln of high school graduates | $-0.0609^{**}$ | $-0.0359$ | $-0.0864^{*}$ |
| | (0.0280) | (0.0365) | (0.0455) |
| L.Gdp per capita in federal state | $-0.0000$ | $-0.0000$ | $0.0000$ |
| | (0.0000) | (0.0000) | (0.0000) |
| Observations | 58 | 58 | 58 |

*Notes:* L. stands for a 1-period lag of the respective variable. Yeardummies and state fixed effects not reported. Years: 2000-2015. Source: Federal Statistical Office (2019b) and Federal Statistical Office (2019a). $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

# Chapter 2

# New region, new chances: Insights from a spatial analysis of a Bavarian graduate survey*

**Abstract**

The question how spatial mobility for jobs pays out monetarily is a prevalent issue in economic research. In this paper, we take one step back and raise the question what determines the mobility of people in their working life. Therefore, we take a representative sample of Bavarian university graduates and track their mobility patterns back to their high school times. The distance to the nearest university will be applied as an instrumental variable on the decision whether to move when going to the university or enrolling at an university nearby. Taking a subset of graduates who went to a suburban high school close to a city with university makes the distance exogenous, given a broad set of controls. We find that people who move to study exhibit a significantly higher likelihood to change location after graduation from university. These results stay robust against personal and socio-economic controls from the survey and add to the literature by arguing that job-mobility is rooted in mobility patterns when starting university education.

*This chapter is joint work with Valentin Lindlacher.

## 2.1. Introduction

The question whether spatial mobility in education pays out monetarily and determinants of this mobility has been investigated by a vast strand of literature, mostly focusing on the effects of being mobile internationally (e.g. (Parey and Waldinger, 2011) or Kratz and Netz (2018)).[1] The findings are manifold, a certain consensus, however, exists that mobility seems to pay out in working life in the long run in monetary terms. A central issue in investigating outcomes, monetary or in other dimensions, of mobility is the selection on unobserved, often hardly to define or measure dimensions like e.g. motivation. Whether an individual decides to move for a job, an apprenticeship or to study seems to be at least partly predetermined by her surrounding, her family background, the area she is living in and intrinsic motivation. Therefore, every research taking mobility as explanatory variable into account should handle this issue of selection carefully. In this investigation we focus exactly on this point by going one step back in contrast to the research question of most of the related papers: instead of asking if and in which dimensions mobility may pay out, we ask what might actually shape mobility for the job.

We analyse the effect of the distance to the nearest university when graduating from school on the decision to enrol at the local university[2] or somewhere else. In a second step, we use this instrument of exogenously given distance to university when graduating from school to model the likelihood that a person decides to move after university graduation. We use a survey containing very detailed information (on postal code level) about the mobility patterns of university graduates, beginning with the place where they got their university entrance diploma up to the location roughly 1.5 years after graduation of this person. Additionally, we have information on parental educational background and socio-economic factors of the investigated individuals. By saying so, we are able to form a group of university graduates which is similar in spatial patterns and socio-economic factors up to a level, not being investigated by the literature in detail so far. We argue that the decision of individuals in these "peer groups" to study at the university located the closest to the place of receiving the university entrance diploma or to leave "home" to study somewhere else increases the credibility of assuming this decision to be exogenous with respect to the variables mentioned in comparison to recent literature in this field significantly.

[1]In this investigation, we focus on mobility, defined geographically by moving from one place to another. Aspects of social mobility, i.e. whether and how people can reach a higher social status, job position or educational level than their parents is not part of this paper.

[2]"Local" is defined as university within the same labour market region. The concept of labour market regions is defined in greater detail in section 2.5.

We find that this instrumental variable approach delivers a significant increase in the likelihood to change location for the first job if a person (exogenously) changed location to study as well. This result stays robust against the inclusion of a vast set of controls. However, we show that this initial mobility does not necessarily pay out in later live by delivering higher wages. We check for the salary at the first job only, therefore a wage premium at a later point in the career is possible but cannot be investigated with our dataset.

In section 2.2 we present a selection of related literature, whereas section 2.3 describes the data we use. Section 2.4 deals with the institutional setting and how we identify the effects of early mobility. Following section 2.5 describes the empirical approach while section 2.6 presents the results. Finally, section 2.7 summarises the results.

## 2.2. Related literature

A vast part of the literature on mobility and its outcomes concentrates on the so called job-to-job mobility and raises the question whether a higher level of mobility for jobs results in greater earnings. Leary et al. (2014) use data from graduates in the UK and find superior earnings for workers with a higher level of mobility. Additionally, they detect a positive link between the decision where to study (leave the home region or not) and mobility in the subsequent career.

Spanning a more international context, Parey and Waldinger (2011) use the introduction and expansion of the European student exchange program ERASMUS as exogenous variable to determine whether staying abroad as a student increases the likelihood in working abroad at a later point in time. The authors find that a stay abroad increases the likelihood to work abroad later on by roughly 15%. A more heterogeneous picture is found by Di Cintio and Grassi (2013), investigating wage premiums for Italian university graduates if they a) chose to study not in their home region or b) migrate within the country for the first job. The authors find small losses for students who migrate for studying (a)) but significant gains for movers for the first job (b)) by employing a matching procedure.

In general, authors like Malamud and Wozniak (2010) find a higher level of mobility and higher willingness to move longer distances for college graduates than workers without a college degree by employing an instrumental variable approach. Similar results are found by Kodrzicki (2001) who evaluate the National Longitudinal Survey of Youth from 1979 to 1996. In contrast, Groen (2004) shows a positive relationship between the state a student decides to study and the state this respective student starts to work and therefore a home bias for the decision where to work.

Coming to the case of Germany and the question, how effects of mobility can be identified, research available is relatively little. The investigation of Krabel and Flöther (2011) uses a nation-wide survey amongst German graduates and finds a lower level of mobility for graduates in metropolitan areas and promising labour markets. At the same time, a higher level of mobility from school to university coincides with a higher mobility when starting the first job. The investigation models the employability of graduates by using a Heckman selection model and takes the results of this regression of employability on personal characteristics and explanatory variables for the determination of the probability of being mobile. Maier and Sprietsma (2016) use the variation in regional availability of university places as exogenous variation in migration decisions when students have to decide where to study. They find a strong dependency between this first level mobility (school to university) and the mobility when entering the labour market. Going one step further, a higher level of mobility does also coincide with a wage premium in the later working life, as the authors find.

In general, literature has identified a relatively clear nexus between the distance to a tertiarty education institution and the likelihood to enrol there. As one of the first authors, Kjellström and Regnér (1999) use a Swedish dataset to investigate the link between the distance between place of residence and the closest university on the one hand and enrolment rates at the other hand. They find a small but significant negative effect of distance on enrolment rates, controlling for a set of personal and parental characteristics. More recently, Frenette (2004) establishes this link for the Canadian Survey of Labour and Income Dynamics and finds a more pronounced effect for individuals from lower income families. These results are also found by Frenette (2006) who shows that the likelihood to enrol at an university decreases significantly if a person's residence does not lie within an acceptable "commuting distance" and that this effect is especially prevalent for people from the lower end of the income distribution again. For the case of Germany, Spiess and Wrohlich (2010) show a higher likelihood of university enrolment if the university is nearby when completing secondary education. They show that 5 years after high school graduation, 57% of those who live relatively far away from an university have enrolled while the share of people who enrolled at an university is 70% for those, living close to such an institution[3].

When investigating the effects of mobility it is important to control for family characteristics (e.g. education of the parents but also own ability) to ensure that findings are

[3]Far away is defined as more than 12.5 kilometres to the closest university while closely located are those, having an university within a radius of 6 kilometres to their residence.

causal. However, as the growing literature on urban economics has shown, the location where a person grows up might be central to her further career and development. Mion and Naticchioni (2009) show for the case of Italy that skills seem to be sorted spatially which is similar to the descriptive findings of Combes et al. (2012), showing the difference in skill and wage distributions between differently dense areas in France. Even more basically, Bosquet and Overman (2019) show a positive raw elasticity of roughly 4% between the size of the city, an individual is born and her later earnings.

## 2.3. Data

To investigate determinants of mobility we use the Bavarian Graduates Panel (Bayerisches Absolventen Panel - BAP)[4], a survey amongst graduates from Bavarian universities and applied universities. The BAP is conducted by the Bavarian State Institute for University Research and Development (Bayerisches Staatsinstitut für Hochschulforschung und Hochschulplanung - IHF). The survey focusses on the transition from university to the labour market and aims to cover all Bavarian universities and applied universities with possibly all fields of study, if a field had at least 10 graduates in the respective survey year. The survey is conducted approximately every 2-3 years with the first cohort interviewed in 2003-2004.

In the survey, graduates are asked about their course of study, their first position at the labour market, socio-economic indicators and when and where they received their university entrance diploma. A distinct feature of the BAP with respect to other graduate surveys is the possibility to track persons spatially very finely (up to post codes) since graduates indicate the post code of the school, they graduated from, the university where spatial information can be generated easily and the post code of their first position at the labour market[5]. Graduates are interviewed up to three times after graduation. Whereas the first wave takes place roughly 1.5 years after graduation with a focus on the transition from university to labour market, the second wave (approximately 5 years after graduation) and third wave (approximately 10 years after graduation) are more interested in employment history and on the job training.

For that reason, we only use the first wave of the BAP and concentrate on the two graduation cohorts of 2005/2006 and 2009/2010. We concentrate our investigation on these two

[4]More information can be found at http://www.bap.ihf.bayern.de.

[5]Graduates do not directly indicate the post code of their employers office but the post code of their private address after beginning to work. Although this might not perfectly correlate with the employers address this represents the best possible proxy for first job location of graduates available.

cohorts mainly due to the fact that the questionnaire of the BAP varies relatively strongly between cohorts and the biggest overlap of variables which are important for our investigation exists in these two cohorts. The latest cohort (2013/2014) for instance has no such detailed information on the high school location. In total, 22,296 graduates participated in the first interview of these two relevant cohorts.[6]

As the survey took place at Bavarian universities, we have no information on graduates who went to high school in Bavaria but did not go to a Bavarian university or did not study at all. Hence, we can only analyse the mobility pattern of graduates who limited their mobility to the state they went to high school and conditioned on graduating. The fact that we only have university graduates might be less critical as university graduates are more mobile compared to people without a university degree. We therefore argue that the results for university graduates can be seen as upper bound results with respect to the entire population.

The fact that we only investigate movement within Bavaria does not seem to be problematic for our identification neither. Firstly, German students do not seem to be very mobile between states. Statistics from the Federal Statistical Office (2019b) show that roughly 60% of all freshmen in Bavaria also stem from Bavaria[7] and that only 20% of all Bavarian high school graduates who decide to study leave Bavaria for enrolment. Secondly, this seems to hold true for the general population: According to Deutsche Post Adress (2018), more than 85% of all relocations in Germany happen within the same state (Bundesland).

The survey does not include questions about moving out. Therefore, we have to define movements based on the location of high school, chosen university and first job. Also, we do not know the location of the home (town) but only the high school the graduates went to. However, this should not differ a lot regarding the distance to the home, and therefore, the distance to the nearest university.

## 2.4. Institutional background and identification strategy

The main aspect of this investigation deals with the question which effect mobility in the early stage of the career, i.e. from high school to university, has on later mobility and monetary payouts. In order to provide the necessary background information, the following part should briefly clarify the institutional background in Germany regarding admission to

[6] In 2005/2006 6,819 graduates participated which equals a respondent rate of 38.9%. For the cohort 2009/10 the respondent rate was 37.5% with 15,477 interviewed graduates.

[7] This percentage corresponds to the year 2014, the values for other years differ only slightly.

universities and applied universities. In general, students in Germany are not obliged to regional boundaries when applying for a degree course. Except for medical degree programs[8] where application procedures are centralised, students have to apply at universities and applied universities directly for their preferred field. Since we leave out medical degrees and focus on diploma ("Diplom"), bachelor and master degrees, every student within our sample basically had the possibility to apply to any university and applied university[9] as long as she received a university entrance diploma.

In total, Bavaria has 12 universities[10] while we leave out the University of the Armed Forces Munich and University of Neuendettelsau since our sample does not include graduates from these universities. Additionally, 19 universities of applied sciences are located in the federal state of Bavaria[11] where we have to leave out the EVNH Nürnberg and the Catholic Foundation University Of Applied Sciences Munich due to similar data restrictions as above. Except for the University of Eichstätt-Ingolstadt and the Catholic Foundation University Of Applied Sciences Munich, all universities and applied universities mentioned are public universities while the two exceptions are under ecclesiastical sponsorship.[12]

Coming to the identification strategy, the central aspect is to ensure an exogenous variation in the mobility of high school graduates in their decision, which university to choose and, by saying so, whether to move or not to move for studying. Going back to the human capital theory of Becker (1994) high school graduates should decide to go to university if the expected payouts outrun the expected cost of studying. This, very basic, argumentation should also hold for the decision whether to leave home to study or not as e.g. Mitze et al. (2015) argue. We think of costs as commuting which depends on the distance to the university and the costs of leaving a social group (family and friends) and find contact to a new group for instance. The cost of commuting are of course affected by the decision to move. At the same time, moving may come with benefits like making new friends or being able to choose from a wider field of study courses. To sum up, commuting costs can

[8] i.e. pharmacy, human medicine, veterinary medicine and dentistry

[9] Due to the setup of the BAP we can only investigate students which decided to study at a tertiary education institution within the federal state of Bavaria.

[10] i.e. University of Augsburg, University of Bamberg, University of Bayreuth, University of Erlangen-Nürnberg, LMU Munich, TU Munich, University of Passau, University of Regensburg, University of Würzburg, University of Eichstätt-Ingolstadt, University of the Armed Forces Munich, University of Neuendettelsau.

[11] These are: Amberg-Weiden, Ansbach, Aschaffenburg, Augsburg, Coburg, Deggendorf, Hof, Ingolstadt, Kempten, Landshut, Munich, Neu-Ulm, Nürnberg, Regensburg, Rosenheim, Weihenstephan-Triesdorf, Würzburg-Schweinfurt, EVNH Nürnberg, Catholic Foundation University Of Applied Sciences Munich

[12] Although under ecclesiastical sponsorship, the University of Eichstätt-Ingolstadt and the Catholic Foundation University Of Applied Sciences Munich are similar to the public universities since they do not charge tuition fees and are open to public.

be minimised by moving whilst the social costs for setting up a new personal environment rise. Obviously, social costs can be seen as benefits, depending on personal preferences.

In general, these two important determinants (expected costs and benefits of leaving home to study) seem to be highly influenced by parental and general socio-economic as well as peripheral characteristics. As one of the first authors Greenwood (1975) argues that migration (in general, not related to students) is highly influenced by socio-economic characteristics and the environment, a person is living in. Mchugh and Morgan (1984) show that the economic conditions of the destination area, i.e. the region the university or college is located in, seem to be important for students when deciding which educational institution to go to. This is confirmed by the more recent work of Agasisti and Dal Bianco (2007) for students in Italy.

As argued above, central to the decision whether to move or not seems to be the environment of the prospective student. Whereas factors like parental background, the number of children or gender can be controlled for, the unobserved dimension of this environment might highly influence the decision whether to leave home to study. If we do not take care of this unobserved dimension and use the "movement-variable" when starting to study as explanatory variable for further calculations, results might be highly biased. This holds especially true if further investigations relate to movement patterns. Results here might be biased since they do not necessarily reflect the interrelation of interest (e.g. how mobility for the first job influences wages) but these results might stem from the underlying differences in individuals due to their different environment they grew up when they were younger. A priori it may be hard to argue whether the bias is up-, or downwards sloping.

We argue that students, stemming from the same suburban region and controlled for observable socio-economic factors should be similar in the sense that the decision whether to take the university which is located in the neighbourhood of the school the student graduated from or to leave this area to study somewhere else can be seen as an exogenous decision. However, this assumption is relatively strong since it does not incorporate the argumentation that the decision whether to leave or stay for studying might be correlated with i.e. intrinsic motivation and might be endogenous therefore.

In order to take this point into account, we employ an instrumental approach by instrumenting the decision whether to leave or to stay via the distance to the most proximate university the student has after graduation. We argue that the place the prospective student receives her university entrance diploma is exogenous to the location of the closest university, i.e. the student does not and cannot influence the location of her high school in such way that she is closer located to her favoured university.

At the same time, characteristics of the parents might influence where they settle, either close to a city with a university or not. If these parental characteristics are again linked to personal characteristics of their descendants, endogeneity might still arise between personal characteristics and the place of residence when graduating from high school. However, we argue that we are able to control for this indirect link by taking care of parental characteristics which might shape the decision, where to settle.

We do not compare the city centre with the suburbs and those in turn not with the country side. We exclude high schools located in the countryside as these pupils have to move anyway if they go to university. We also exclude high schools within the central urban area, as cities are more heterogeneous than the suburbs and distance within the city, especially for smaller cities, is less important.

In summary, our identification succeeds the following argumentation: prospective students growing up in a common region and controlled for their family background are similar in unobservable and observable dimensions, except for their intrinsic motivation in leaving home to study. To overcome this endogeneity, we take the distance to the university nearest by since a closely located university should decrease the likelihood to move. Therefore, we argue that the (exogenously given) distance between high school and university is a valid instrument for the decision to move to study when we e.g. want to investigate the effect of this first movement after high school on the decision to move when entering the labour market after university graduation.

## 2.5. Empirical approach

As argued in section 2.4, we take a group of students which is similar in terms of the location they graduated from high school, control for family characteristics, and use the distance from high school to closest located university as instrumental variable for the decision to go to this university or chose another university in another labour market region (LMR, *Arbeitsmarktregion*). The concept of labour market regions was developed by the Federal Ministry for Economic Affairs and Energy. LMRs coincide usually with 2 or 3 districts (*Kreise*) and are defined as regions where workers might commute within, but not between.[13] We use this instrument of distance between high school and closest university to

[13] More specific, LMRs are defined as regions where at least 65% of all wage earners with residence in this region also work in this region and that at least 65% of all paid jobs are filled with domestic workers (stemming from this region). Additionally, commuting times within a LMR should not exceed 45 minutes one way. For more information see www.bbsr.bund.de.

investigate how the movement decision between high school and university influences the likelihood to move to another city after graduating from university.

Figure 2.1 illustrates how we decided to form our group of interest to have a set of students, as similar as possible in unobservable and observable dimensions. We calculate distances as road distances by using the Stata tool *osrmtime* by Huber and Rust (2016) to account for the geography and streets which might reflect commuting more realistically and draw lines around the city centre according to these distances.[14]

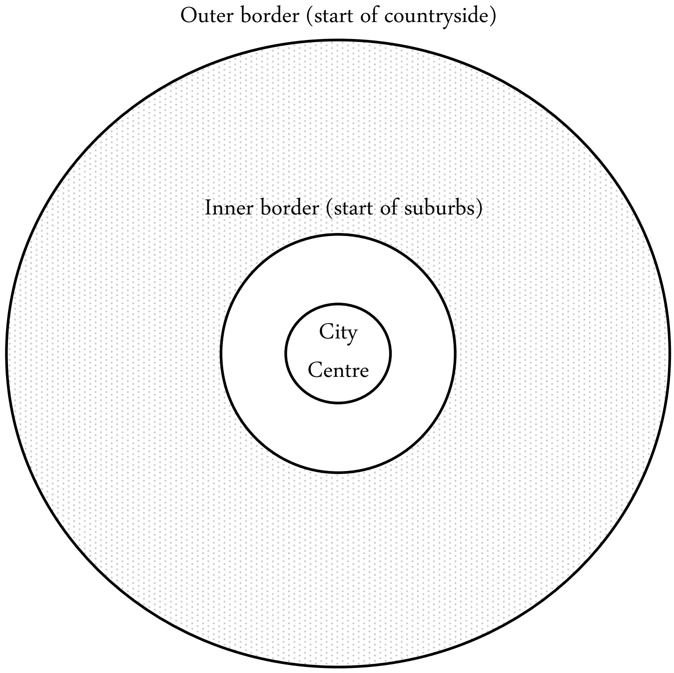



**Figure 2.1:** Schematic selection of group of interest (in grey dotted area)

The outer border of our group of interest in defined as a maximum of 30 (for Munich) or 15 ( for all other cities) kilometres street distance to the next bigger city centre. We argue that roughly at this line, geographical structures change from a suburban environment to a more countryside characterised infrastructure.

Coming back to Figure 2.1 we define the inner and the outer border separately for Munich and all other relevant cities with a university or an applied university. Munich represents the centre of Bavaria in many dimensions (economical, cultural, educational) with a relatively widespread network of public transportation. For Munich, the average travel

[14]The tool calculates distances between longitudinal and latitudinal specified places by using open source street maps. We used a street map of Germany provided by Geofabrik (http://www.geofabrik.de/).

distance between the city centre and a terminal stop of a suburban train (*S-Bahn*) is 39 kilometres while this distance is 11 kilometres when taking the metro (*U-Bahn*) instead of the suburban train.[15]

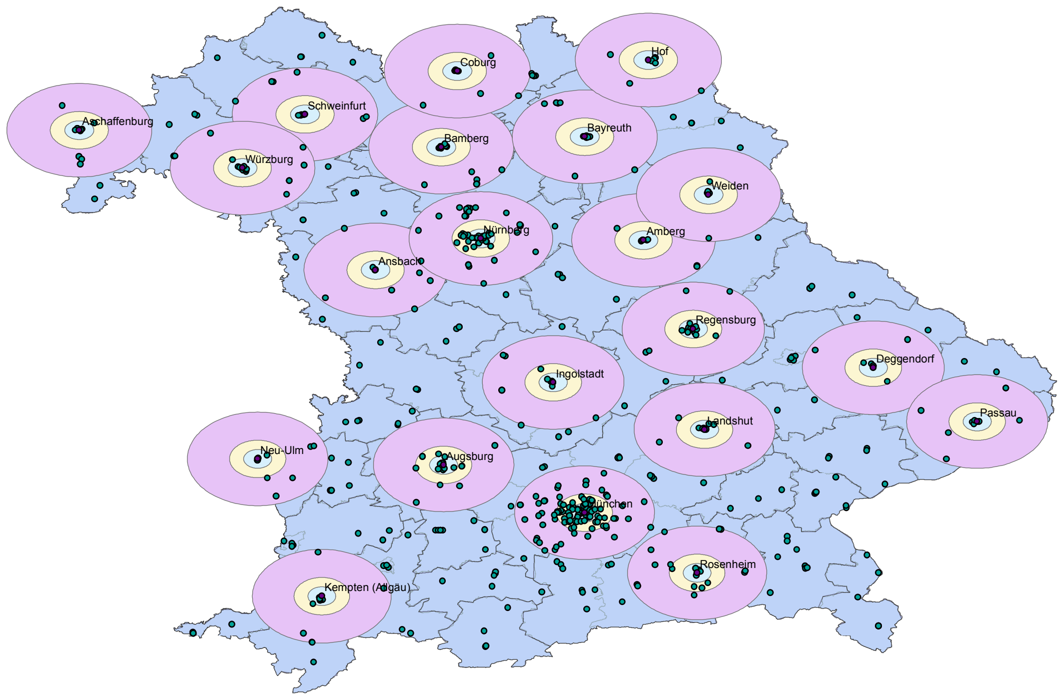
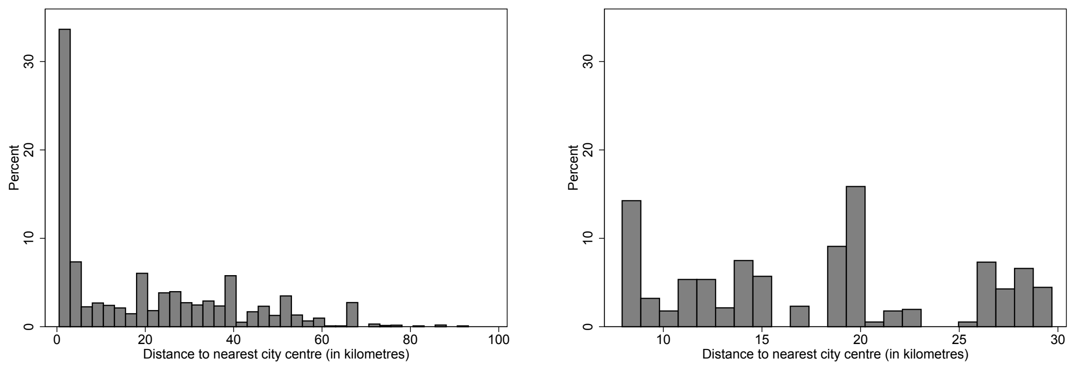



**Figure 2.2:** Distribution of schools and city centres in Bavaria

By having these street distances, we define the outer border of Figure 2.1 for Munich at 30 kilometres travel distance, the inner circle at 15 kilometres. We do so to ensure a mostly homogeneous group of people, especially in terms of availability of public transportation since this might be a main driver for the decision where to study. By taking the inner circle at 15 kilometres, we ensure that these people are "far enough" spatially located from the terminal stops of the metro, which shapes to a certain degree the border of the city. At the same time, taking 30 kilometres as outer border ensures that all people within this ring between 15-30 kilometres away from the city centre are similarly close to a stop of the suburban train and, by saying so, have equally good public transportation connections to the centre of Munich. For the other relevant cities, we take half of the distances as for Munich, therefore the inner circle is at 7.5 kilometres from the city centre, the outer circle is drawn

[15]The distances are measured as simple arithmetic mean of the sum of distances between Munich's city centre, defined as Marienplatz (location of the town hall) and the terminal stop, measured by using Google Maps.

at 15 kilometres. Both values seem to be reasonable for the expansiveness of the respective public transportation network.




a) b)

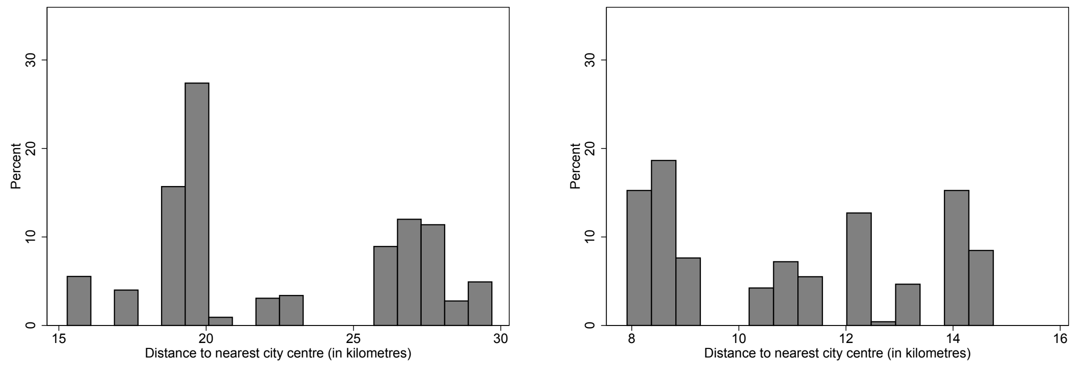
**Figure 2.3:** Fraction of people with relevant distance between high school and relevant city centre for a) full sample and b) relevant subsample.

*Notes:* Distances are relevant road distances. Source: Bavarian Graduates Panel, cohort 2005/06 and 2009/10.

Figure 2.2 illustrates the distribution of high schools (green dots) in Bavaria and circles around the city centres of 5 kilometres (inner circle) and 25 kilometres (outer circle). We see that there are only very few overlaps of schools, belonging basically to the catchment areas of two or more cities. Therefore, most prospective students can decide between only one university in the closer neighbourhood with the next closely located university being significantly far away. Furthermore, the graph nicely illustrates that schools, except for those in the direct city centre, are spread relatively even within Bavaria without having big overlaps between circles around the city centre.

Figure 2.3 gives a histogram of the pecentage of graduates with respect to the distance between the high school they received their university entrance diploma from and the city centre of the next relevant city with a university. The distances are measured as road distances. What this graph should illustrate is the proper selection of our subsample, i.e. that the left-hand side of the graph shows a high level of heterogeneity especially with a great fraction of people who live relatively close to (or even within) a city with a university. If we restrict the sample to the persons within the "doughnut" as drawn in Figure 2.1, the distribution amongst people with respect to this relevant distance becomes higher even as the right-hand side b) of Figure 2.3 shows. We argue that the restriction to this subsample ensures that effects found are not driven by few observations with a relatively short distance between high school and university as depicted in a) of Figure 2.3.

We split up the histogram b) in Figure 2.3 between students closely located to Munich and all other relevant cities. Percentages can be found in Figure 2.4 with left-hand side a) presenting the percentages for Munich and right-hand side presenting the results for all other cities of our sample. The picture of those two histograms is somewhat similar, however it should be noted that the dimension of the x-axis (the distance to the closest city centre) is larger for histogram a) of Figure 2.4 than for b) of the same figure, accounting for the mentioned bigger expansiveness of Munich in contrast to the other cities. Therefore, Figure 2.4 supports our argumentation to set different distances to city centres for Munich and all other cities when setting up our sample of interest.




a) b)

**Figure 2.4:** Fraction of people with relevant distance between high school and relevant city centre for relevant subsample and a) Munich only b) all other cities.

*Notes:* Distances are relevant road distances. Source: Bavarian Graduates Panel, cohort 2005/06 and 2009/10.

### 2.5.1 Estimation

As explained above, we take a subset of individuals based on the distance of their high school where they received their university entrance diploma, to the closest city centre with an university or applied university. We want to know how the decision of those seemingly similar individuals to go not to this closest tertiary education institution influences her further mobility behaviour. Therefore, we implement a two-stage-least-squares instrumental approach (2SLS) to instrument the (possibly biased) variable of moving for the university by road distance to the closest university. We argue that for the decision to move the commuting distance to university is important. Therefore, we take the distance to the university as an instrument. As mentioned before, the survey does not include questions about movements. Because of this, we define moving as a change in the LMR. According to the definition of LMRs, commuting times should be acceptable within these areas but not between. We argue that this should hold true, no matter whether a person commutes to her job or her university. Because of this, we define a person with a high school in a different LMR than the university she enrolled in as "moved for university". Analogously, if the indicated first residence after graduating from university lies in another LMR than the university, we define the graduate as having moved after graduating and therefore being mobile after graduating. Formally, the first stage of our approach regresses the variable "move for university" ($X$) on our set of explanatory variables (including the distance to the most proximate university), summarised as $Z$

$$X_i = \alpha_0 + \alpha_1 Z_i + e_i \tag{2.1}$$

, where $Z$ consists of variables, all exogenous with respect to $X$. The predicted values of $X_i$, $\hat{X}_i$, calculated by $\hat{X}_i = \hat{\alpha_0} + \hat{\alpha_1} Z_i$ are then used in the second stage to calculate the effect of our instrumented variable (move for university) on our variable of interest (e.g. mobility when entering the labour market), represented by $Y$:

$$Y_i = \beta_0 + \beta_1 \hat{X}_i + \epsilon_i \tag{2.2}$$

Equation 2.2 is the central equation we are estimating to model the link between movement at an early stage of the career on later mobility pattern. Thereby, $\hat{X}_i$ consists of the estimated values of following variables: distance to the next university/applied university (in road kilometres - instrument for movement for university) and following control variables: socio economic characteristics of the prospective student (age, marital status, children, gender), characteristics of the parents (education and professional position), educational characteristics (grade of university entrance diploma, grade of university degree, dummy for internship, dummy for exchange term, dummy for working experience), controls for the region of residence and dummies for the type of university (university/applied university).

### 2.5.2 Restriction of the dataset

Our dataset for the cohorts 2005/06 and 2009/10 consist in total of roughly 10,000 interviewed individuals. To ensure a mostly causal identification, we have to restrict the sample in several dimensions. 2,362 bachelor graduates have do be dropped since they are interviewed whilst still succeeding another degree (mostly a masters degree) and therefore are still students. The sample decreases further by implementing restrictions regarding the high school location: First, 3,338 graduates are dropped as they went to a high school which is located in an LMR where no university (of applied sciences) is located. These students would by definition always be accounted as movers, even for their nearest university. Therefore, we do not incorporate them in our analysis. More observations get lost when considering graduates which went to a high school within a city. By removing graduates with a distance to the city centre closer than the calculated radius of the city, 2,903 observations are removed. People, moving back to their "home" LMR after finishing university somewhere else are coded as not moved since we are interested in the effects a migration for university has on later mobility and not the likelihood to go to the home area again.

Finally, we take an additional distance to the city border and remove graduates too far away from the city centre (outside of the outer boarder of Figure 2.1). This removes further 2,791 graduates. Hence, the final sample of interest shrinks to roughly 500 observations.[16]

## 2.6. Results

As a first result and to get an impression of the relationship between movement for university and the subsequent first job, Table 2.1 is a basic regression (without any controls) of the mobility for university (as a reminder, mobility is defined as enrolling at another university than the closest one) on the later mobility for the first job. The results are presented for the entire sample, once with an instrumented "move for university" variable with the distance to the closest university and once without an instrumentation.

We see that there seems to be a high level of, at least, correlation between these two variables[17]. However, results of Table 2.1 should not be emphasised too much due to the fact that we neither include any control variables here nor ensure homogeneity within the group of investigated individuals. Table 2.1 is separated by the choice of the sample.

Whereas specification 1 and 2 take all individuals graduated for the calculation, specification 3 and 4 concentrate on a subsample consisting of only the individuals within the inner border of Figure 2.1. The table clearly shows that there seems to be a link between mobility on the different stages of the career (school $\rightarrow$ university and university $\rightarrow$ first job).

However, as we argued in section 2.5, it is important to ensure that these mobility patterns are not driven by unobserved heterogeneity, either directly through e.g. family

[16]Some of the graduates have several characteristics which lead to a drop, e.g. still succeeding a university degree and stemming from a LMR without a university nearby. Therefore, adding up the mentioned values does not lead to the sample size of 500 individuals.

[17]Regressing movement for university on the distance to the closest university delivers significant results. We argue that the instrument is relevant therefore.

**Table 2.1:** Likelihood to move for first job based on migration for university - full sample

| | (1)<br>Probit-IV - full sample | (2)<br>Probit-full sample | (3)<br>Probit-IV - within circle | (4)<br>Probit- within circle |
|---|---|---|---|---|
| Moved for university | $0.3085^{***}$<br>(0.0699) | $0.1866^{***}$<br>(0.0338) | $0.6148^{***}$<br>(0.1221) | $0.3118^{***}$<br>(0.0569) |
| Observations | 6255 | 6255 | 3352 | 3352 |

*Notes:* Source: Bavarian Graduates Panel, Cohort 2005/06 and 2009/10. Standard errors in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

background or indirectly, for example by a specific neighbourhood which stimulates mobility more than another neighbourhood. Therefore, we enrich and modify the underlying strategy of Table 2.1 in two dimensions: Firstly, we restrict our dataset to only those persons within the two outer rings of Figure 2.1 to ensure homogeneity in the mentioned indirect dimension. Secondly, we control for a vast set of socioeconomic factors like the education and professional position of the parents, the grades in school and university of the respective individual, family status etc.[18]

Table 2.2 presents the results similar to the ones of Table 2.1, enriched by these two dimensions mentioned above. As previously, measuring the effect of a change in the LMR for enrolling at a university on later enrolment seems to be underestimated by a simple binary indicator as the probit specifications (model (2) and (4) in Table 2.1 and model (2) in Table 2.2) exhibit a remarkably smaller coefficient than the specification with the distance to the closest university as instrument (model (1) and (3) in Table 2.1 and model (1) in Table 2.2). This seems to be reasonable, due to the fact that the instrument (distance between high school and closest university in road kilometres) measures the costs of going to the closest university or somewhere else in a much finer way than a simple binary variable, indicating whether the student moved for enrolling at a university or not.

The specification (1) in Table 2.2 can be seen as our baseline specification, controlling for all measurable factors of heterogeneity and coming closest to an exogenous change in location for university studies in our setting. The coefficient indicates that if a person moves for her tertiary education, the likelihood to move afterwards is higher than for a student who decides to stay in the home region to study.

Importantly, it should be noted that both specifications (1) and (2) have a binary variable (Moved for job:yes/no) as outcome variable and are therefore estimated with probit or an IV-enriched probit approach. Therefore, interpretations of the coefficient are cumbersome if not impossible due to this non-linear specification of the model.[19] A possibility to

[18] All controls are defined in the notes of the respective tables and in section 2.5.

[19] see e.g. Liao (1994) for a detailed description of issues arising from interpreting non-linear models.

**Table 2.2:** Likelihood to move for first job based on migration for university - reduced sample

| | (1)<br>Probit-IV -reduced sample | (2)<br>Probit - reduced sample | (3)<br>OLS-IV - reduced sample |
|---|---|---|---|
| Moved for university | 4.2769* | -0.0460 | 1.0020* |
| | (2.2764) | (0.1640) | (0.5342) |
| Grade University | 0.0860* | 0.0891** | 0.0165* |
| | (0.0491) | (0.0364) | (0.0099) |
| Intern Experience | -0.2941 | 0.2164 | -0.0618 |
| | (0.3864) | (0.1777) | (0.0888) |
| Grade High School | 0.0474 | 0.1794 | 0.0177 |
| | (0.2268) | (0.1384) | (0.0526) |
| German | -0.2742 | -0.2115 | -0.0524 |
| | (1.0867) | (0.6698) | (0.2611) |
| Female | -0.6871 | 0.0316 | -0.1600 |
| | (0.4439) | (0.1509) | (0.1020) |
| Experience abroad | -0.1114 | 0.2588* | -0.0184 |
| | (0.2997) | (0.1482) | (0.0669) |
| Job Mother | 0.0203 | -0.0627 | 0.0003 |
| | (0.1524) | (0.0943) | (0.0350) |
| Job Father | 0.0637 | 0.1056 | 0.0197 |
| | (0.1305) | (0.0811) | (0.0307) |
| Education Mother | 0.4929* | 0.1795 | 0.1136* |
| | (0.2904) | (0.1563) | (0.0671) |
| Education Father | -0.1451 | -0.0433 | -0.0396 |
| | (0.1577) | (0.0938) | (0.0379) |
| Observations | 515 | 515 | 528 |

*Notes:* Controls for district of high school graduation, cohort interviewed, terms studied, type of university entrance diploma, marital status, children, age and type of university (university or applied university) not reported. Source: Bavarian Graduates Panel, Cohort 2005/06 and 2009/10. Standard errors in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

overcome this issue is the calculation of margins and their interpretation at means. However, this might be not meaningful in this context due to the IV specification and the level of variance (e.g. in terms of grade at high school) to interpret the effect of a person at mean,[20] a change in location for university would have on her later decision whether to move for a job.

Because of this, we interpret the results only in their significance and sign, not in their magnitude. For that, a third column is included which estimates an IV OLS specification. We are aware of possible problems of a linear interpretation of a binary outcome variable and argue that this specification is meant only to get a sense of the magnitude of the arising effect. We find from column (3) of Table 2.2 that the likelihood to move for the first job doubles roughly if a person previously had to move for enrolling at a university.

What should be noted in the general interpretation of our results is an issue, occurring for students who decide to study at the university in their neighbourhood. Unfortunately, our dataset delivers only postal code information for the university, a student enrols and

[20]This means we would interpret the effect of a synthetic observation, having mean values for all relevant variables.

the high school she graduates from. We coded a movement for a student if she chooses a university which is not the closest one to the place she graduated from school, based on this postal code information. We can therefore not distinguish between students who study at their home university and decide to do not leave the parental home and students who move out, although they decided to enrol at the most proximate university. Both types of students (home stayers and leavers, both enrolled at closest university) are dropped from our sample.

Since the costs for those students are lower since they can choose their closest university and move out, thereby keeping the environment at least partially constant, we argue that an inclusion of those students should lower the effects we find. Therefore, results as in Table 2.1 or Table 2.2 should be treated as upper bound results.

### 2.6.1 The case of Munich

Bavaria exhibits some specialities in its structure in comparison to other states. Especially the strong centralisation towards Munich, as "heart" of the state in many dimensions, separates Bavaria from most other German states with a more evenly spread structure in terms of economy, culture or population density in general. Apart from Berlin, Munich is the only German city with more than one public university (Technical University Munich (TUM) and Ludwig Maximilian University Munich (LMU)) and a set of applied universities. Table B.1 in the Appendix gives an overview of the universities, individuals of our relevant subsample were enrolled at.[21]

As the table shows, students from both universities located in Munich (TUM and LMU) account for roughly 37% of the sample, adding the HaW Munich increases this number to nearly 50%. The table basically mirrors the distribution of students in our subsample, based on the region where they received their university entrance diploma. Here, roughly 58% of the students graduated from a high school in the LMR Munich. Table B.2 in the Appendix shows the distribution according to LMR of high school graduation.[22] For the reason that roughly half of our sample consists of individuals who studied in Munich and therefore presumably the highest level of heterogeneity for this city and the aspect that Munich represents the centre of Bavaria, we decide to separate our analysis to students from Munich and

[21] Differences in the number of observations in Table B.1 and regression results like in Table 2.2 may occur if we have very few observations for some high school/university region combinations such that fixed effects (control dummies) in one of this areas lead to perfect collinearity. Furthermore, not all control variables are available for all students of the subsample presented in Table B.1.

[22] If we account for a graduation from a high school in the LMR of Munich and check the distribution of those graduates among Bavaria, we see that approximately 70% of those, graduating from school in Munich (and decide to study) go to an university in Munich. Table B.3 in the Appendix shows this distribution.

**Table 2.3:** Likelihood to move for first job based on migration for university - Munich only

| | (1) Probit-IV - reduced sample | (2) Probit-reduced sample | (3) OLS-IV - reduced sample |
|---|---|---|---|
| Moved for university | 4.2988** | 0.4444* | 0.7354** |
| | (2.1537) | (0.2328) | (0.3431) |
| Grade University | 0.1080 | 0.0889 | 0.0120 |
| | (0.0812) | (0.0671) | (0.0107) |
| Intern Experience | -0.6644 | -0.0157 | -0.1123 |
| | (0.5032) | (0.2502) | (0.0831) |
| Grade High School | 0.0136 | 0.1134 | -0.0051 |
| | (0.2911) | (0.2042) | (0.0496) |
| German | 0.0000 | 0.0000 | 0.1197 |
| | (.) | (.) | (0.2189) |
| Female | -0.5728 | -0.0216 | -0.0882 |
| | (0.4314) | (0.2167) | (0.0685) |
| Experience abroad | 0.0224 | 0.2106 | 0.0191 |
| | (0.3147) | (0.2124) | (0.0523) |
| Job Mother | 0.2164 | 0.0285 | 0.0398 |
| | (0.2099) | (0.1287) | (0.0360) |
| Job Father | 0.0939 | -0.0405 | 0.0113 |
| | (0.1895) | (0.1226) | (0.0328) |
| Education Mother | 0.3516 | 0.1902 | 0.0708 |
| | (0.2893) | (0.1958) | (0.0496) |
| Education Father | -0.2446 | -0.1303 | -0.0446 |
| | (0.2072) | (0.1408) | (0.0355) |
| Observations | 288 | 288 | 306 |

*Notes:* Controls for district of high school graduation, cohort interviewed, terms studied, type of university entrance diploma, marital status, children, age and type of university (university or applied university) not reported. Source: Bavarian Graduates Panel, Cohort 2005/06 and 2009/10. Standard errors in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

the rest of Bavaria.

Table 2.3 regresses the likelihood to move for the first job on the movement for university on a subsample of students who graduated from a high school within the LMR of Munich. The remaining students who did not graduate from a Munich high school form the subsample of the regression in Table B.11 in the Appendix. We see that the effect movement for university studies has on later mobility for the first job is especially pronounced for high school graduates from Munich. As previously, the true effect seems to be underestimated by a simple probit regression, since the coefficient on the interrelation is significantly higher for the IV-estimation in column (1) in Table 2.3. At the same time, this effect found does not seem to be valid for high school graduates from other LMRs as Table B.11 in the Appendix does not verify any significant interrelation between mobility from school to university and university to job market, the simple probit regression in column (2) in Table B.11 even gives a negative and significant coefficient.

We do not want to overemphasis the results of Table 2.3 or Table B.11 due to the fact that the Bavarian Graduates Panel consists largely of persons stemming from Munich and surrounding areas. Therefore, we have a higher level of heterogeneity, especially for these regions, which we utilise in this subsection. Therefore, the findings that the positive effects of a movement for university has on general mobility is especially pronounced for persons from Munich should be validated with further analyses and especially by using a dataset, containing higher levels of heterogeneity also for other LMRs but Munich.

### 2.6.2 Does higher mobility pay out monetarily?

Whereas we modelled determinants of later job mobility in the previous sections, this subsection is meant to check whether mobility pays out monetarily, i.e. whether people who decide to move for the first job earn more than the respective home stayers. The Bavarian Graduate Panel includes information on the yearly wage for the first job. We use this information to investigate whether mobile people earn more in this first position, based on the approach we used to model mobility.

Literature in this field delivers relatively homogeneous results regarding the question, whether spatial mobility leads to superior earnings. For a sample of German graduates, Maier and Sprietsma (2016) find significantly higher earnings for mobile graduates, using the number of students in the respective labour market region as instrument for mobility. Leary et al. (2014) show a similar relationship for the case of UK graduates. Theoretically, Raphael and Riker (1999) and more recently Lkhagvasuren (2014) show that higher levels of mobility should induce higher wages.

We showed in section 2.6 the importance of accounting for mobility when enrolling at university when investigating determinants of mobility for the first job. Now, we use this approach to check whether mobility for the first job also pays out while accounting for the relevant levels of unobserved heterogeneity: As before, we restrict our sample according to the location of the school, the individual received her university entrance diploma. Secondly, we control for personal and parental characterstics by including a set of control variables as e.g. in Table 2.3.

Thirdly, we now instrument for the mobility for the first job with mobility for university and this variable, again instrumented by distance to the closest university. The relevant identifying assumption, next to the previous argumentation that mobility for university is exogenous when taking the correct subsample and controlling for socioeconomic factors, is that there is a direct, positive and linear relationship between mobility after school and after university. We basically follow the empirical approach proposed by Maier and Sprietsma (2016) who regress the effects of mobility for the first job on the respective earnings. However, mobility for the first job is modelled by accounting for mobility for university education. We do the same, but instrument for mobility for university education with the

**Table 2.4:** Determinants of log of yearly income based on previous mobility

| | (1)<br>IV - reduced sample | (2)<br>OLS-reduced sample |
|---|---|---|
| Moved for first job | 0.1395 | 0.0402 |
| | (0.2169) | (0.0337) |
| Grade University | 0.0123 | 0.0119 |
| | (0.0132) | (0.0138) |
| Intern Experience | -0.0083 | -0.0049 |
| | (0.0355) | (0.0363) |
| Grade High School | -0.0273 | -0.0200 |
| | (0.0309) | (0.0279) |
| German | -0.1176 | -0.1181 |
| | (0.1239) | (0.1297) |
| Female | -0.1935*** | -0.1940*** |
| | (0.0290) | (0.0304) |
| Experience abroad | 0.0117 | 0.0188 |
| | (0.0325) | (0.0301) |
| Job Mother | -0.0125 | -0.0149 |
| | (0.0196) | (0.0197) |
| Job Father | 0.0361** | 0.0394** |
| | (0.0175) | (0.0168) |
| Education Mother | 0.0027 | 0.0074 |
| | (0.0296) | (0.0291) |
| Education Father | 0.0300 | 0.0280 |
| | (0.0192) | (0.0196) |
| Observations | 422 | 422 |

*Notes:* Controls for district of high school graduation, cohort interviewed, terms studied, type of university entrance diploma, marital status, children, age and type of university (university or applied university) not reported. Source: Bavarian Graduates Panel, Cohort 2005/06 and 2009/10. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log of yearly income, which include the sum of monthly payments plus possible bonuses.

established distance between high school and closest university.

The results for this approach are mapped in Table 2.4 showing that mobile people do not earn higher (logged) wages than their not moving counterparts, if we use the subsample as in Table 2.2 and instrument for the first job mobility as mentioned before. The dependent variable is the log of the yearly income, including bonuses. This result stays robust against a respecification of the estimation approach: In Table B.5 in the Appendix, we estimate the log of the yearly wage in the first job on mobility for this first job, accounted for selection on the mobility for university by using a Heckman selection model as proposed firstly by Heckman (1979). The respecification does not alter results significantly, indicating that there is no significant effect of the mobility for the first job on the respective earnings, as well.[23]

[23]Instead of using yearly wages as dependent variable like e.g. in Table 2.4 we also do this investigation using hourly wages. The advantage of using hourly wages might be a better comparability since wages are normalised by hours worked. We calculate hourly wages by dividing monthly salaries with the realised number of monthly hours worked. Results for these calculations, using the IV approach and the Heckman selection model and controling for the case of Munich can be found in the Appendix from Table B.7 to Table B.10. Results stay basically unchanged with a small but significant effect for movement for the first job when employing the Heckman model.

However, as already argued in subsection 2.6.1 it should be kept in mind that the sample size is relatively small, leading to a smaller level of heterogeneity than it could be expected with a bigger dataset.

We conduct the analysis of the wage premium based on an IV-strategy as in Table 2.4 or a Heckman selection model as in Table B.5 also for a subsample of persons graduating from school in the LMR Munich to check, whether we observe different effects for this region. Results are presented in Table B.4 and Table B.6 in the Appendix. The Heckman selection model delivers a slightly significant effect, mobility for the first job has on the wage. This supports the previous results, setting the effects for Munich apart from other cities in Bavaria.

## 2.7. Conclusion

In this paper, we add to the existing literature on determinants of spatial mobility of university graduates in two dimensions. Firstly, we show that the likelihood to move when entering the labour market for the first time is highly determined by previous mobility, i.e. the decision whether to study in the home area or somewhere else. We set up an identification strategy which is, in our view, able to control for direct and indirect channels which influence the decision to move for university studies and therefore models this decision as exogenous as possible.

Secondly, we show that this positive interrelation of mobility decisions in the career is especially pronounced for individuals, graduating from a school in the LMR of Munich. Furthermore, we show that there does not seem to be a premium when moving for the first job if our identification approach is employed.

Our findings are novel to existing literature in several dimensions: Firstly, the results that a higher level of mobility in the job might be rooted in mobility for earlier university studies points toward a nexus in a growing field of research in urban economics, showing the importance of the place of birth for later earnings and the literature on wage premium for higher levels of mobility.

Secondly, we find that by controlling for early mobility patterns, this premium seems to be not existent any longer which is somewhat contradicting to the well established literature on mobility premiums. However, migration even with small distances is often driven mainly by endogenous and hardly to measure factors like mobility. Our approach tries to control for these factors more comprehensively than many other related studies. Therefore, we consider the mobility premium found by other authors be driven, at least partially, by these factors.

However, two limitations arise in our analysis. Firstly, the number of relevant observations shrinks dramatically when we implement the identification strategy to account for unobserved heterogeneity. Here, a greater sample might help to validate the results found on a broader database. Secondly, the dataset does not allow for distinguishing between people who move when enrolling at university within the same LMR and people who stay at their parents' home when starting university studies. Although our dataset allows to track people down to the level of postal codes of their schools and universities, which is very precise in contrast to other, related investigations, this drawback only allows to distinguish between relocations between LMRs. Therefore, we argue that our results should be treated as upper bound results since costs for people, moving within a LMR when enrolling as students should be lower as for people who change the region.

# Appendix B

## B.1 Composition of sample

**Table B.1:** Composition of sample based on university

| Name of University | No. obs. | Freq. | Cum. |
|---|---|---|---|
| HaW Kempten | 1 | 0.18 | 0.18 |
| HaW Weihenstephan | 1 | 0.18 | 0.36 |
| Uni Würzburg | 1 | 0.18 | 0.53 |
| HaW Ansbach | 2 | 0.36 | 0.89 |
| HaW Hof | 3 | 0.53 | 1.43 |
| HaW Ingolstadt | 3 | 0.53 | 1.96 |
| HaW Deggendorf | 4 | 0.71 | 2.67 |
| Uni Bamberg | 4 | 0.71 | 3.39 |
| HaW Amberg-Weiden | 6 | 1.07 | 4.46 |
| HaW Aschaffenburg | 6 | 1.07 | 5.53 |
| Uni Eichstätt-Ingolstadt | 7 | 1.25 | 6.77 |
| HaW Rosenheim | 9 | 1.60 | 8.38 |
| HaW Regensburg | 11 | 1.96 | 10.34 |
| Uni Bayreuth | 11 | 1.96 | 12.30 |
| HaW Landshut | 13 | 2.32 | 14.62 |
| Uni Augsburg | 16 | 2.85 | 17.47 |
| HaW Nürnberg | 26 | 4.63 | 22.10 |
| Uni Regensburg | 26 | 4.63 | 26.74 |
| HaW Würzburg-Schweinfurt | 29 | 5.17 | 31.91 |
| HaW Augsburg | 31 | 5.53 | 37.43 |
| Uni Passau | 36 | 6.42 | 43.85 |
| Uni Erlangen-Nürnberg | 44 | 7.84 | 51.69 |
| HaW München | 64 | 11.41 | 63.10 |
| TU München | 77 | 13.73 | 76.83 |
| LMU München | 130 | 23.17 | 100.00 |
| Total | 561 | 100.00 | |

*Notes:* HaW = *Hochschule für angewandete Wissenschaften* (University of Applied Sciences). Source: Bavarian Graduates Panel, Cohort 2005/06 and 2009/10.

**Table B.2:** Composition of sample based on LMR of university entrance diploma

| Name of LMR | No. obs. | Freq. | Cum. |
|---|---|---|---|
| Ansbach | 1 | 0.18 | 0.18 |
| Würzburg | 1 | 0.18 | 0.36 |
| Deggendorf | 6 | 1.07 | 1.43 |
| Aschaffenburg | 10 | 1.78 | 3.21 |
| Weiden | 10 | 1.78 | 4.99 |
| Landshut | 11 | 1.96 | 6.95 |
| Amberg | 12 | 2.14 | 9.09 |
| Passau | 14 | 2.50 | 11.59 |
| Rosenheim | 25 | 4.46 | 16.04 |
| Regensburg | 30 | 5.35 | 21.39 |
| Augsburg | 50 | 8.91 | 30.30 |
| Nürnberg | 66 | 11.76 | 42.07 |
| München | 325 | 57.93 | 100.00 |
| Total | 561 | 100.00 | |

*Notes:* Source: Bavarian Graduates Panel, Cohort 2005/06 and 2009/10.

**Table B.3:** Distribution of universities if students graduated from high school in LMR Munich

| Name of LMR | No. obs. | Freq. | Cum. |
|---|---|---|---|
| HaW Amberg-Weiden | 1 | 0.31 | 0.31 |
| HaW Deggendorf | 1 | 0.31 | 0.62 |
| HaW Kempten | 1 | 0.31 | 0.92 |
| Uni Bamberg | 1 | 0.31 | 1.23 |
| Uni Würzburg | 1 | 0.31 | 1.54 |
| HaW Ingolstadt | 2 | 0.62 | 2.15 |
| Uni Eichstätt-Ingolstadt | 4 | 1.23 | 3.38 |
| Uni Augsburg | 5 | 1.54 | 4.92 |
| Uni Bayreuth | 5 | 1.54 | 6.46 |
| HaW Rosenheim | 6 | 1.85 | 8.31 |
| Uni Regensburg | 6 | 1.85 | 10.15 |
| Uni Erlangen-Nürnberg | 7 | 2.15 | 12.31 |
| HaW Landshut | 8 | 2.46 | 14.77 |
| HaW Augsburg | 9 | 2.77 | 17.54 |
| Uni Passau | 18 | 5.54 | 23.08 |
| HaW Würzburg-Schweinfurt | 23 | 7.08 | 30.15 |
| HaW München | 56 | 17.23 | 47.38 |
| TU München | 58 | 17.85 | 65.23 |
| LMU München | 113 | 34.77 | 100.00 |
| Total | 325 | 100.00 | |

*Notes:* HaW = *Hochschule für angewandete Wissenschaften* (University of Applied Sciences). Source: Bavarian Graduates Panel, Cohort 2005/06 and 2009/10.

## B.2 Robustness checks

**Table B.4:** Determinants of log of yearly income based on previous mobility - only LMR Munich

| | (1)<br>OLS-IV - reduced sample | (2)<br>Probit-reduced sample |
|---|---|---|
| Moved for first job | 0.1297 | 0.0940* |
| | (0.2755) | (0.0552) |
| Grade University | 0.0411 | 0.0424* |
| | (0.0254) | (0.0250) |
| Intern Experience | -0.0522 | -0.0518 |
| | (0.0489) | (0.0523) |
| Grade High School | -0.0246 | -0.0227 |
| | (0.0413) | (0.0414) |
| German | -0.1702 | -0.1616 |
| | (0.1641) | (0.1613) |
| Female | -0.1473*** | -0.1470*** |
| | (0.0411) | (0.0439) |
| Experience abroad | -0.0237 | -0.0218 |
| | (0.0426) | (0.0428) |
| Job Mother | -0.0065 | -0.0067 |
| | (0.0250) | (0.0267) |
| Job Father | 0.0336 | 0.0330 |
| | (0.0235) | (0.0247) |
| Education Mother | 0.0134 | 0.0145 |
| | (0.0365) | (0.0381) |
| Education Father | 0.0020 | 0.0011 |
| | (0.0271) | (0.0280) |
| Observations | 241 | 241 |

*Notes:* Controls for district of high school graduation, cohort interviewed, terms studied, type of university entrance diploma, marital status, children, age and type of university (university or applied university) not reported. Source: Bavarian Graduates Panel, Cohort 2005/06 and 2009/10. Standard errors in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. The dependent variable is the log of yearly income, which include the sum of monthly payments plus possible bonuses.

**Table B.5:** Determinants of log of yearly income based on previous mobility by using a Heckman selection model

| | (1)<br>Heckman selection model |
|---|---|
| Moved for first job | 0.0381 |
| | (0.0266) |
| Grade University | 0.0175 |
| | (0.0135) |
| Intern Experience | -0.0222 |
| | (0.0319) |
| Grade High School | -0.0119 |
| | (0.0226) |
| German | -0.0164 |
| | (0.1106) |
| Female | -0.1752*** |
| | (0.0256) |
| Experience abroad | 0.0247 |
| | (0.0251) |
| Job Mother | -0.0055 |
| | (0.0158) |
| Job Father | 0.0337** |
| | (0.0138) |
| Education Mother | -0.0065 |
| | (0.0243) |
| Education Father | 0.0265 |
| | (0.0165) |
| Observations | 538 |

*Notes:* Controls for district of high school graduation, cohort interviewed, terms studied, type of university entrance diploma, marital status, children, age and type of university (university or applied university) not reported. Source: Bavarian Graduates Panel, Cohort 2005/06 and 2009/10. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$. The dependent variable is the log of yearly income, which include the sum of monthly payments plus possible bonuses.

**Table B.6:** Determinants of log of yearly income based on previous mobility by using a Heckman selection model - only LMR Munich

| | (1) Heckman selection model |
|---|---|
| Moved for first job | 0.0940* |
| | (0.0515) |
| Grade University | 0.0424* |
| | (0.0234) |
| Intern Experience | -0.0518 |
| | (0.0488) |
| Grade High School | -0.0227 |
| | (0.0386) |
| German | -0.1616 |
| | (0.1505) |
| Female | -0.1470*** |
| | (0.0410) |
| Experience abroad | -0.0217 |
| | (0.0399) |
| Job Mother | -0.0067 |
| | (0.0249) |
| Job Father | 0.0330 |
| | (0.0230) |
| Education Mother | 0.0144 |
| | (0.0356) |
| Education Father | 0.0011 |
| | (0.0262) |
| Observations | 311 |

*Notes:* Controls for district of high school graduation, cohort interviewed, terms studied, type of university entrance diploma, marital status, children, age and type of university (university or applied university) not reported. Source: Bavarian Graduates Panel, Cohort 2005/06 and 2009/10. Standard errors in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$. The dependent variable is the log of yearly income, which include the sum of monthly payments plus possible bonuses.

## B.3 The monetary effects on hourly wages of mobility for university and job

**Table B.7:** Determinants of log of hourly wage based on previous mobility - full subsample

| | (1)<br>IV - reduced sample | (2)<br>OLS-reduced sample |
|---|---|---|
| Moved for first job | 0.1227 | 0.0316 |
| | (0.2334) | (0.0376) |
| Grade University | 0.0255 | 0.0245 |
| | (0.0175) | (0.0182) |
| Intern Experience | -0.0178 | -0.0147 |
| | (0.0392) | (0.0404) |
| Grade High School | 0.0048 | 0.0132 |
| | (0.0372) | (0.0321) |
| German | -0.0541 | -0.0542 |
| | (0.1368) | (0.1439) |
| Female | -0.1674*** | -0.1665*** |
| | (0.0327) | (0.0343) |
| Experience abroad | -0.0168 | -0.0106 |
| | (0.0357) | (0.0336) |
| Job Mother | -0.0327 | -0.0347 |
| | (0.0216) | (0.0221) |
| Job Father | 0.0422** | 0.0455** |
| | (0.0197) | (0.0187) |
| Education Mother | 0.0231 | 0.0278 |
| | (0.0333) | (0.0326) |
| Education Father | 0.0341 | 0.0320 |
| | (0.0214) | (0.0219) |
| Observations | 415 | 415 |

*Notes:* Controls for district of high school graduation, cohort interviewed, terms studied, type of university entrance diploma, marital status, children, age and type of university (university or applied university) not reported. Source: Bavarian Graduates Panel, Cohort 2005/06 and 2009/10. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Table B.8:** Determinants of log of hourly wage based on previous mobility - only LMR Munich

| | (1)<br>2SLS - reduced sample | (2)<br>Probit-reduced sample |
|---|---|---|
| Moved for first job | 0.0782 | 0.0958 |
| | (0.2934) | (0.0604) |
| Grade University | 0.0535* | 0.0528* |
| | (0.0275) | (0.0274) |
| Intern Experience | -0.0647 | -0.0650 |
| | (0.0535) | (0.0572) |
| Grade High School | 0.0143 | 0.0132 |
| | (0.0462) | (0.0458) |
| German | -0.0803 | -0.0847 |
| | (0.1797) | (0.1763) |
| Female | -0.1103** | -0.1105** |
| | (0.0452) | (0.0483) |
| Experience abroad | -0.0415 | -0.0425 |
| | (0.0470) | (0.0472) |
| Job Mother | -0.0389 | -0.0388 |
| | (0.0276) | (0.0295) |
| Job Father | 0.0345 | 0.0348 |
| | (0.0257) | (0.0270) |
| Education Mother | 0.0262 | 0.0256 |
| | (0.0399) | (0.0417) |
| Education Father | 0.0117 | 0.0121 |
| | (0.0295) | (0.0307) |
| Observations | 238 | 238 |

*Notes:* Controls for district of high school graduation, cohort interviewed, terms studied, type of university entrance diploma, marital status, children, age and type of university (university or applied university) not reported. Source: Bavarian Graduates Panel, Cohort 2005/06 and 2009/10. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Table B.9:** Determinants of log of hourly wage based on previous mobility by using a Heckman selection model - full subsample

| | (1)<br>Heckman selection model |
|---|---|
| Moved for first job | 0.0505* |
| | (0.0296) |
| Grade University | 0.0303 |
| | (0.0191) |
| Intern Experience | -0.0221 |
| | (0.0343) |
| Grade High School | 0.0246 |
| | (0.0263) |
| German | 0.0677 |
| | (0.1302) |
| Female | -0.1574*** |
| | (0.0287) |
| Experience abroad | 0.0068 |
| | (0.0280) |
| Job Mother | -0.0237 |
| | (0.0186) |
| Job Father | 0.0414*** |
| | (0.0151) |
| Education Mother | 0.0199 |
| | (0.0275) |
| Education Father | 0.0285 |
| | (0.0182) |
| Observations | 538 |

*Notes:* Controls for district of high school graduation, cohort interviewed, terms studied, type of university entrance diploma, marital status, children, age and type of university (university or applied university) not reported. Source: Bavarian Graduates Panel, Cohort 2005/06 and 2009/10. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

**Table B.10:** Determinants of log of hourly wage based on previous mobility by using a Heckman selection model - only LMR Munich

| | (1) Heckman selection model |
|---|---|
| Moved for first job | 0.0959* |
| | (0.0563) |
| Grade University | 0.0528** |
| | (0.0255) |
| Intern Experience | -0.0649 |
| | (0.0534) |
| Grade High School | 0.0132 |
| | (0.0427) |
| German | -0.0847 |
| | (0.1645) |
| Female | -0.1104** |
| | (0.0451) |
| Experience abroad | -0.0425 |
| | (0.0440) |
| Job Mother | -0.0388 |
| | (0.0275) |
| Job Father | 0.0348 |
| | (0.0252) |
| Education Mother | 0.0256 |
| | (0.0389) |
| Education Father | 0.0121 |
| | (0.0286) |
| Observations | 311 |

*Notes:* Controls for district of high school graduation, cohort interviewed, terms studied, type of university entrance diploma, marital status, children, age and type of university (university or applied university) not reported. Source: Bavarian Graduates Panel, Cohort 2005/06 and 2009/10. Standard errors in parentheses. $^{*}$ $p < 0.1$, $^{**}$ $p < 0.05$, $^{***}$ $p < 0.01$.

## B.4 Additional material

**Table B.11:** Likelihood to move for first job based on migration for university - without Munich

| | (1) Probit-IV - reduced sample | (2) Probit-reduced sample | (3) OLS-IV - reduced sample |
|---|---|---|---|
| Moved for university | 0.0449 | -0.7949*** | 0.2921 |
| | (4.5387) | (0.2840) | (1.2801) |
| Grade University | 0.1278** | 0.1312** | 0.0252** |
| | (0.0572) | (0.0537) | (0.0127) |
| Intern Experience | 0.5227* | 0.5278* | 0.1349 |
| | (0.2956) | (0.2874) | (0.0859) |
| Grade High School | 0.1975 | 0.1649 | 0.0893 |
| | (0.2894) | (0.2243) | (0.0813) |
| German | 0.0000 | 0.0000 | -1.0203** |
| | (.) | (.) | (0.4925) |
| Female | -0.0512 | 0.1201 | -0.0702 |
| | (0.9581) | (0.2485) | (0.2561) |
| Experience abroad | 0.1401 | 0.2518 | -0.0023 |
| | (0.6487) | (0.2359) | (0.1657) |
| Job Mother | -0.2002 | -0.1887 | -0.0492 |
| | (0.1759) | (0.1609) | (0.0527) |
| Job Father | 0.2487 | 0.2960** | 0.0556 |
| | (0.2874) | (0.1305) | (0.0832) |
| Education Mother | 0.5546 | 0.4187 | 0.2009 |
| | (0.8010) | (0.3162) | (0.2201) |
| Education Father | -0.0934 | -0.1011 | -0.0296 |
| | (0.1573) | (0.1478) | (0.0468) |
| Observations | 214 | 214 | 222 |

*Notes:* Controls for district of high school graduation, cohort interviewed, terms studied, type of university entrance diploma, marital status, children, age and type of university (university or applied university) not reported. Source: Bavarian Graduates Panel, Cohort 2005/06 and 2009/10. Standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

# Chapter 3

# Dynamics and endogeneity of firms' recruitment behaviour*


### Abstract

We use detailed German data to carefully document how the vacancy-filling hazard evolves from the recruiting firms' perspectives. We further show how firms adjust their search behaviour when they are unable to fill their vacancy within the planned search duration. We find that the vacancy-filling hazard is increasing during the planned search period and that it decreases thereafter. Most applicants arrive early in the recruitment process. Firms' willingness to pay higher wages or to hire less qualified, inexperienced or unemployed applicants increases when they are unable to hire before the intended starting date. Models of random search, directed search, and stock-flow matching differ substantially in the way they assume that job seekers and firms behave during the recruitment process in these respects and we conjecture that our findings are most readily explained by stock-flow matching models - if these were amended by time-consuming screening technologies.


*This chapter is joint work with Christian Holzner. This chapter is published, see Ehrenfried and Holzner (2019) for details.

## 3.1. Introduction

Search and matching models have been used extensively throughout in labour economics to explain worker and firm behaviour, to model macroeconomic trends, and to evaluate labour market policies. The various types of models - random search, directed search, stock-flow matching - used in the literature differ substantially in the way they assume that job seekers and firms behave during the recruitment process.[1] While many studies investigate the search behaviour of workers and gives us some guidance to assess which type of model fits workers' behaviour best,[2] relatively little empirical work has been done in order to understand the recruitment process of firms and to get an idea which theories are best able to explain it.

In this paper we identify new patterns about the recruitment behaviour of firms using the entropy balancing technique to account for observed heterogeneity and the information on the planned search duration of vacancies to account for unobserved heterogeneity. Using the German Job Vacancy Survey, we find that the vacancy-filling hazard is increasing during the planned search duration and decreases thereafter. Most applicants arrive early in the recruitment process and the willingness to pay higher wages or to hire less qualified or experienced applicants increases for firms that have been unable to hire until the intended starting date.

In a seminal paper, van Ours and Ridder (1992) find for the Netherlands that in the first two weeks after the announcement of a vacancy the majority of applicants apply. Then the arrival rate of applicants drops. The evidence on the shape of the hazard is mixed. Coles and Smith (1998), Coles and Petrongolo (2008), Kuo and Smith (2009), or Andrews et al. (2013) provide support for stock-flow matching by showing that the hazard rate is highest in the first two weeks after registering the vacancy with the UK Job Centre and drops

[1]In the classical random sequential search model workers and firms meet at random and only one potential partner at a time (see Pissarides (2000), and Cahuc et al. (2014) for an overview). In directed search models firms post wages and workers direct their applications to those jobs offering the highest utility (see Wright et al. (2019) for an overview). In stock-flow matching models the inflow of vacancies (job seekers) matches with the stock of job seekers (vacancies) and if a vacancy (job seeker) was unsuccessful initially it (she) becomes part of the stock (see Coles and Muthoo (1998), Coles and Smith (1998), Ebrahimy and Shimer (2010), and Carrillo-Tudela and Hawkins (2016)).

[2]The early literature concentrated on the use and return of different search channels (see Holzer (1988), and Blau and Robins (1990) for the US, Osberg (1993) for Canada, and Gregg and Wadsworth (1996) for the UK). More recent evidence on the time spent on searching for a job is provided by Krueger and Mueller (2012) and on the use of different search channels by Kuhn and Skuterud (2004) and Kuhn and Mansour (2014). Kuo and Smith (2009), Andrews et al. (2013), and Kettemann et al. (2017) show that the longer a worker is unemployed the higher the probability that she matches with a newly posted vacancy. This rejects random search in favour of the stock-flow-matching. Kettemann et al. (2017) also find supportive evidence for directed search by showing that workers direct their search towards more productive firms.

sharply thereafter. Using data on vacancies registered with the Austrian Public Employment Service, Kettemann et al. (2017) find – similar to what we find – that if the hazard is centred around the intended starting date, it is first increasing up to the intended starting date and decreasing thereafter.

In the second part, we analyse how firms that have been unlucky during the search process respond by adjusting their search intensity, their qualification and/or experience requirements, and the wages they pay. This allows us to say more about which theory is best able to predict the observed patterns.

We use the entropy balancing technique developed by Hainmueller (2012) to construct synthetic control groups to identify the effect of being unlucky in the search process on the recruitment strategy of firms. The use of information on the planned search duration of a vacancy to control for unobserved differences in firm and vacancy characteristics is equally important. By comparing vacancies with similar planned search durations we are able to control for all unobserved characteristics that firms expect to influence their search duration. We therefore assume that after controlling for the planned search duration and other observable firm and vacancy characteristics, the need to search longer than the planned search duration is due to random bad luck shocks in the search process.

Using this strategy, we split our sample into four groups. The group *early* includes all vacancies that successfully finished their search process at least four weeks prior to the intended starting date, the group *in time* includes all vacancies which successfully finished their search process around the intended starting date, the group *delayed* includes all vacancies that successfully finished their search process after the first week of the intended starting date, and the final group *failed* consists of vacancies which could not be filled at all.

We find that firms which are able to hire an applicant some time ahead of the intended starting date are contacted by a significantly higher fraction of suitable applicants and report to have faced significantly fewer recruitment problems due to an insufficient number of suitable applicants or due to high wage demands by applicants than firms which hire *in time*, *delayed*, or *fail* to hire. We can also show that those firms that hire *in time* activate additional search channels compared to firms which hire relatively *early*. Firms which hire *delayed* or *failed* to hire activate even more search channels.[3] The use of the public employment agency and the use of newspapers and online ads experience the highest increase. Furthermore, firms which are only able to hire *delayed* make significantly more concessions in terms of the

[3]Russo et al. (2000) find that the use of more search channels is associated with a longer vacancy duration. Our results suggest that this is due to reverse causality, i.e., that firms activate additional search channels after having failed to hire a worker within the planned search duration.

required qualification and experience and the willingness to hire previously unemployed workers than the firms which hire *in time* and even more compared to firms which hire some time before the intended starting date. Firms with a *delayed* recruitment also increase their willingness to bargain over pay and pay more often more than initially intended compared to firms which hire *early* or *in time*. This also holds true if we account for match-quality by dropping firms that make concessions in terms of qualification and experience required.[4]

The decline in the arrival rate of applicants over time and the decrease in the vacancy-filling hazard after the intended starting date can be explained by the basic stock-flow matching model.[5] Newly posted vacancies initially receive many applications from the stock of job seekers. If a vacancy cannot be filled by matching with an applicant from the stock of job seekers, it has to wait until new workers start to search. Since the inflow of new job seekers is small compared to the stock of job seekers, the number of new applicants and the vacancy-filling hazard drops after a vacancy was unable to match initially. This decrease in the matching probability reduces firms' outside option and is therefore also able to explain why firms – after the intended starting date – are more willing to bargain over pay, pay higher wages, or to make concessions by accepting workers with a lower qualification or experience than initially posted. If the basic stock-flow matching model is combined with a time-consuming screening technology, it seems likely that it can also explain why the vacancy-filling hazard is increasing up to the intended starting date. If one introduces a time-consuming screening technology, firms will screen sequentially and use a reservation productivity to decide on whether or not to hire. Given the number of applicants a newly opened vacancy receives from the stock of unemployed job seekers, the firm will decrease the reservation productivity as the number of remaining unscreened applicants decreases. The likelihood that one of the screened applicants passes the reservation productivity threshold then increases as time passes.

Other theories are also able to explain certain parts of the observed pattern. The use of planned search durations and the increase in the vacancy-filling hazard during this period can be explained by advanced notice or the intention of firms to gather applicants because they want to induce competition among applicants. Burdett and Cunningham (1994) and

[4]Faberman and Menzio (2018) find for the US that higher wages are positively correlated with vacancy duration. They build a directed search model, which - under certain parameter constellations - is able to explain their findings, if they assume that some degree of worker and job heterogeneity is unobserved in their empirical analysis. In light of the stock-flow matching model and our results their findings can also be explained by reverse causality, i.e. that firms pay higher wages because they have been unsuccessful in finding a worker initially.

[5]See Coles and Muthoo (1998), Coles and Smith (1998), Ebrahimy and Shimer (2010), and Carrillo-Tudela and Hawkins (2016)

Burdett and Cunningham (1998) explain the increasing vacancy-filling hazard by showing that firms lower their reservation productivity as the time of advance notice draws down. Models with multiple applications like in Albrecht et al. (2006) or Gautier and Holzner (2018) embedded into a dynamic model with a Poisson arrival rate of applications – as generally assumed in dynamic search models – can explain the increasing vacancy-filling hazard before the intended starting date since the Poisson arrival rate implies that the number of firms which have gathered sufficiently many applicants increases over time. A Poisson arrival rate of applicants is, however, unable to explain that most applicants arrive early and that the arrival rate of applications and subsequently the vacancy-filling rate decreases after the intended starting date. Phantom vacancies combined with a dynamic directed search model can explain this pattern as shown by Albrecht et al. (2017). The idea here is that applicants anticipate that the likelihood of an application being considered by a firm decreases the longer the vacancy has been posted. Applicants therefore apply more often to newly posted vacancies than to older ones. However, this theory is unable to explain why firms should be more willing to make concessions in terms of qualification or experience or by paying higher wages if they are not successful initially. Instead of making concessions, it would be optimal to signal to the market that their vacancies are no phantoms by posting the vacancies again. The remainder of this paper is organized as follows. Section 3.2 describes the dataset we use, i.e., the German Job Vacancy Survey. Section 3.3 presents the estimated hazard of finding a worker in different specifications and explains how we form the four groups of vacancies depending on when the search process was successful or failed. Our identification strategy is laid out in section 3.4. Section 3.5 contains our main results and describes the dynamics and the adjustments made by firms during the recruitment process. In section 3.6 we discuss which theory is most suitable to explain our empirical findings. Section 3.7 concludes our findings.

## 3.2. The data

For our analysis, we use the German Job Vacancy Survey collected by the Institute for Employment Research.[6] The survey has been conducted annually since 1989. The quality of the data and the depth of questions has increased over the years. We use the years 2005 to 2014 since most of the information we need for our analysis is not available before. This

[6]The data used in this article is made available to us by the Research Data Centre of the German Federal Employment Agency at the Institute for Employment Research (IAB), Nuremberg. For details see Kettner et al. (2011) or Brenzel et al. (2016).

period covers good and bad labour market periods. The German Job Vacancy Survey is a repeated cross section. The representative samples are drawn from German establishments which employ at least one person subject to social security contribution. For confidentiality reasons the data does not contain establishment identifiers.

The yearly cross-sectional survey delivers detailed information on the interviewed establishment like the number of employees, the industry, the number of vacancies, the number of hires and quits in the previous year, and the region the establishment is located. The firm's current economic condition is measured by binary indicators for "low sales", "financial constraints" and "skilled labour shortage".

In the second part of the survey, the firm is interviewed in detail on the last case of a successfully filled vacancy and (if applicable) on the last case of a aborted recruitment attempt in the survey year.[7] The questionnaire for vacancies which failed to hire a worker is limited and the number of observations is relatively low (5.6% of our total sample).[8] We include information on failed vacancies if the respective information is available.

The survey also contains detailed information on the characteristics of a vacancy like occupation, qualification and experience required for the job, whether the position is full- or part-time, permanent or temporary and if temporary, whether it is a seasonal job or replaces another worker temporarily. Moreover, the firm is asked when it started to search for applicants, when it stopped searching (signed an agreement with the later hired worker), when the intended starting date was, and when employment actually started. This unique feature of the German Job Vacancy Survey allows us to estimate the baseline hazard over the recruitment process and to compare the recruitment strategies of vacancies which found a suitable applicant some time ahead of the intended starting date with vacancies which found a suitable applicant in time or some time after the intended starting date, and with vacancies which failed to hire.

Firms are also asked to provide information on the search and hiring channels used. Binary variables to indicate which channels were used are available for the following channels: Advertisement in print media, on company's website, on online job platforms, on social media platforms (Facebook, Twitter etc.), public employment agency (online and offline services), speculative applications to the firm, private employment agencies, internal postings, interns, apprentices, or referrals by employees (network). In our empirical analysis we group these channels into five main groups as shown in Table 3.1.

[7] The vacancy could have opened already in the year before the survey year.

[8] Since the Institute for Employment Research (IAB) does not provided weights, we are unable to calculate the representative number of failed vacancies.

**Table 3.1:** Grouping of search channels

| **Name of main group** | **Assigned search channels** |
|---|---|
| Classic | - Advertisement in print media |
| | - Advertisement on company's website |
| | - Advertisement on online job platforms |
| | - Advertisement on social media platforms |
| Internal | - Internal postings |
| | - Interns |
| | - Apprentices |
| Network | - Referrals by employees |
| Speculative appl. | - Speculative applications |
| PEA | - Public employment agency (PEA) |

All advertisements of the company – on- and offline – are grouped together into the group *classic*. We group on- and offline advertisements because of the high overlap in content and the similarity in how they address applicants. If a company nowadays decides to publish an advertisement in a newspaper, the same job advertisement will typically be displayed on the newspaper's website and on the company's website as well. The group *internal* refers to all search channels which address all groups of employees who are already known to the firm such as all regular employees (addressed by internal postings), and interns or apprentices. The group *network* refers to firms that asked their employees to approach potential applicants in their personal network. *Speculative applications* are applications received by firms without having posted a respective vacancy. Firms which register their vacancies with the *public employment agency (PEA)* are counted as using the PEA search channel. The channel *private employment agency* is dropped since this search channel is only used by 7.28% of firms and is hence of minor importance in the German context.

Apart from the information on the timing of the search process and the search channels used, the survey provides information on problems which can arise during the search process. Firms can indicate via binary variables whether they had problems in finding enough suitable applicants, and whether they had problems in hiring workers because pay claims of applicants were too high. The survey also includes questions on how many persons applied overall and how many are regarded as suitable for the job.

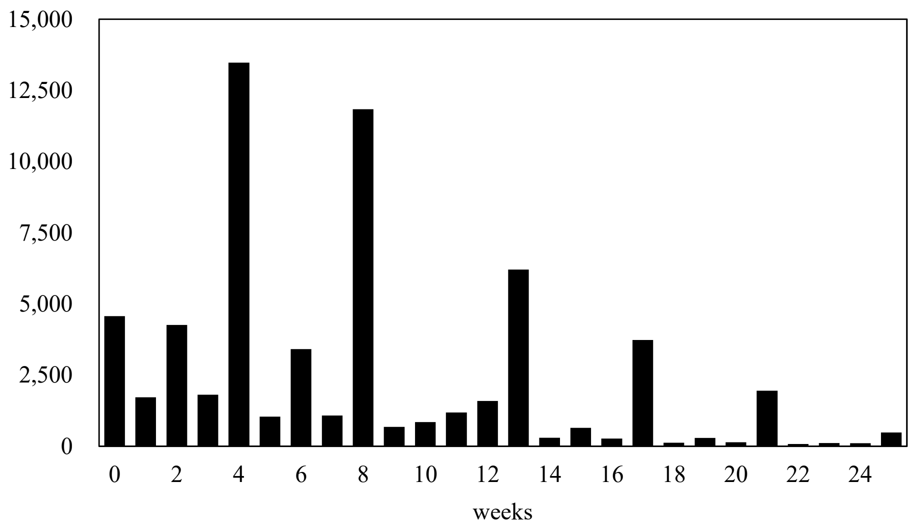
Additionally, the survey contains several questions which can be used to evaluate how firms reacted if they were unable to find a suitable applicant within the intended search period. The first set of questions concerns the firm's willingness/need to adjust the wage. We know whether the firm bargained over pay, the wage it paid to the hired worker, and whether it paid more than initially intended. The second set of questions concerns the firm's willingness/need to make concessions in terms of qualification and experience required. The respective information is provided by firms if they answer the question whether the hired worker's qualification and experience level was below what was initially expected. Finally, we can use the information on the number and types of search channels used to evaluate whether firms which were unable to hire a suitable worker in time increased their search effort.

Not all variables are available for the years 2005 to 2014. The years on which the respective analysis is based are shown below the respective tables and figures.

## 3.3. Planned search duration and the vacancy-filling hazard

### 3.3.1 Planned search duration

Figure 3.1 shows the frequency of vacancies with different planned search durations. The planned search duration is measured by the difference between the intended employment starting date and the starting date of the search process. In our sample 52.1% of firms start their recruitment process at least eight weeks before the intended starting date and 18.7% of firms start their recruitment process less than four weeks ahead. The planned search duration is longer for vacancies offering permanent jobs, full-time jobs, and jobs which require a high qualification and experience level, and shorter for temporary jobs, especially seasonal jobs. The respective OLS-estimates are shown in Table C.1 in the Appendix.




Data: German Job Vacancy Survey 2005-2014

**Figure 3.1:** Frequency of vacancies according to planned search duration

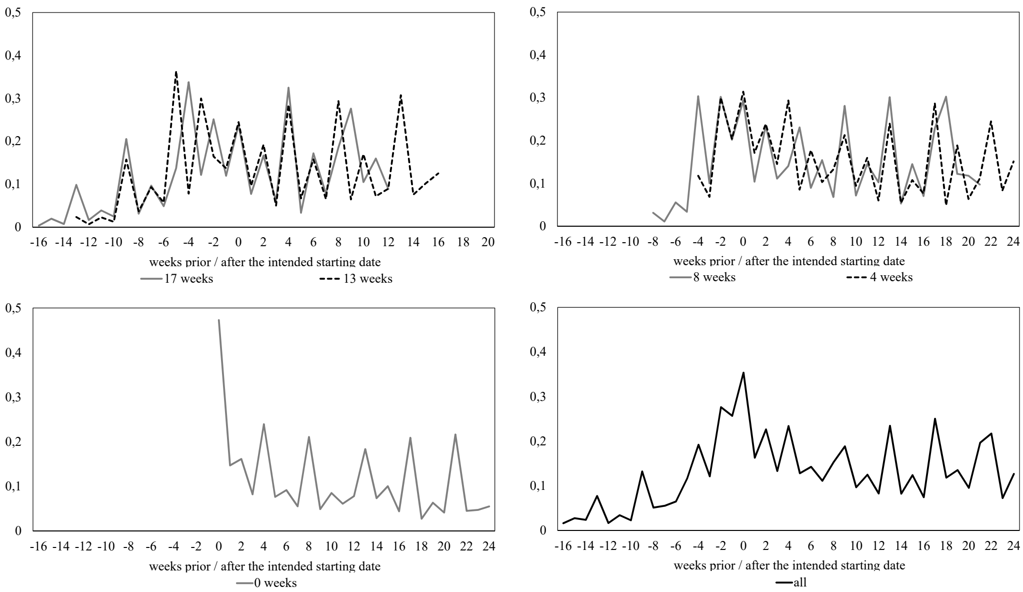
### 3.3.2 Computed vacancy-filling hazard rates

So far, the literature has paid very little attention to how the planned search duration shapes the hazard of filling a vacancy. Figure 3.2 shows the weekly recruiting hazard rates of vacancies with 17, 13, 8, 4, and 0 weeks of planned search duration. The hazard rates are calculated as the number of vacancies which stopped searching (signed an agreement with an applicant) in a given week divided by the number of vacancies at risk (i.e., still searching) at the beginning of the week. In order to emphasise the role of the planned search duration we centred the hazard rates around the intended starting date. The hazard rates of vacancies with 17, 13, and 8 weeks of planned search duration are highest around the 5th to 2nd week in advance of the intended starting date. The hazard rates for vacancies with 4 weeks planned search durations have their highest hazard rates in the two weeks prior to the intended starting date and the hazard rate of vacancies with no planned search duration is highest in the first week after the intended starting date (which coincides with the first week after the search process started). The black solid line shows the hazard rate of all vacancies. The respective hazard is again calculated by dividing the number of vacancies hiring in the respective week by the number of vacancies at risk at the beginning of the respective week. Note that the number of firms at risk in a specific week consist of firms which searched unsuccessfully in the previous week and new firms which started to search in the respective week. The respective number of firms at risk are shown in Table C.2 in the Appendix. The overall hazard of filling a vacancy is first increasing until the intended starting date. It stays high for several weeks around the intended starting date and declines thereafter.

### 3.3.3 Estimated vacancy-filling hazard rates

Next, we estimate the effect of searching a certain number of weeks before or after the intended starting date on the vacancy-filling hazard rate. To do so, we use week-indicators in such a way that the indicator is one if the vacancy is searching in the 16th, 15th, and so on week before the intended starting date, and 1st, 2nd, 3rd and so on week after the intended starting date and zero otherwise.

In Figure 3.3a) we estimate an exponential hazard model. The exponential hazard model assumes a constant underlying baseline hazard that we normalize to zero by omitting the constant in the regression and by demeaning the rest of the variables (except the week indicators). In the estimation we control for a host of vacancy characteristics as well as for year-, industry-, and region-fixed effects. The respective estimates are shown in Table C.3 in



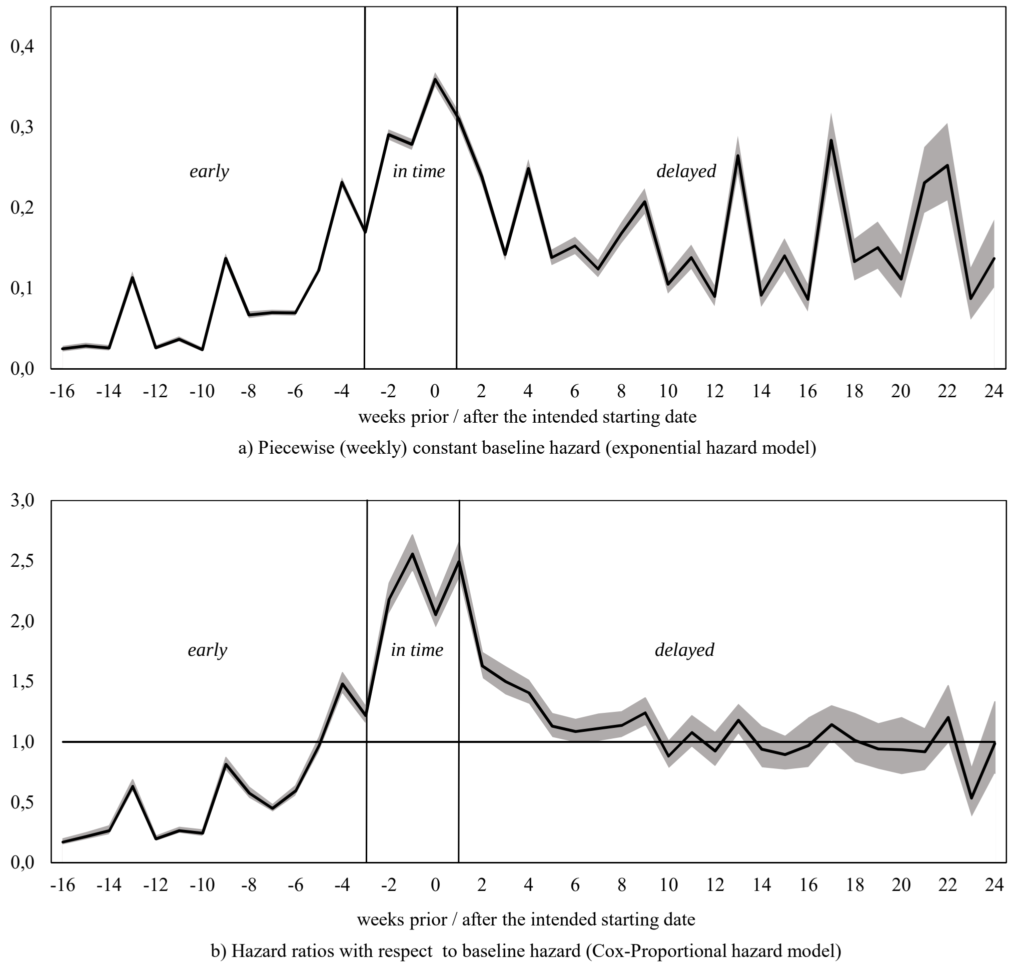

Data: German Job Vacancy Survey 2005-2014

**Figure 3.2:** Vacancy-filling hazard for different planned search durations

the Appendix. The week-indicators estimate the weekly constant hazard rate of filling the vacancy in the "x"-th week before/after the intended starting date, where week 0 is defined as the week before the intended starting date. The estimated weekly vacancy-filling hazard rates start from very low values around 0.02 to 0.06 which indicate that between 2% to 6% of those firms at risk hire somebody within the respective week. The hazard increases until the intended starting date up to values beyond 0.3 indicating that around one third of firms searching in the respective week are able to hire an applicant. Shortly after the intended starting date the hazard decreases again.

In Figure 3.3 b) we present the estimates of the hazard ratios for our week-indicators based on a Cox-Proportional Hazard model. The Cox-Proportional Hazard model allows for a flexible baseline hazard. The underlying baseline hazard varies for each week after a firm started searching for a worker. Our definition of the week indicators with respect to the intended starting date ensures that the underlying baseline hazard and our week-indicators are not collinear. The flexible baseline hazard in the Cox-Proportional Hazard model absorbs some of the variation of the vacancy-filling hazard and explains why the hazard ratios shown in Figure 3.3b) fluctuate less than the hazard rates shown in Figure 3.3a) which are estimated with an exponential hazard model assuming a constant baseline hazard.

The hazard-ratios in Figure 3.3b) start from very low values around 0.2. This implies for example that a vacancy which is searching 16 weeks prior to the intended starting date




The grey shaded areas represent the 95% confidence interval. Data: German Job Vacancy Survey 2005-2014

**Figure 3.3:** Hazard estimates centred around the intended starting date

has a baseline hazard 5-times smaller than the baseline hazard of other vacancies which searched equally long. The hazard-ratio increases during the planned search duration to values up to 2.5 around the intended starting date. This implies that vacancies around the intended starting date have a baseline hazard 2.5-times above the baseline hazard of other vacancies which searched equally long. After the first week of the intended starting date the hazard ratios decrease again to values around 1, which implies that the baseline hazard is comparable to the baseline hazard of other vacancies which searched equally long. The pattern of the estimated hazard ratios remains almost the same if we treat the exit of unsuccessful vacancies as a competing risk and estimate the Cox-Proportional Hazard accordingly (see Table C.3 in the Appendix for the respective results).

We are not the first to show that the vacancy-filling hazard is increasing up to the intended starting date and decreasing thereafter. Kettemann et al. (2017) show a similar picture for vacancies registered with the Austrian Public Employment Service. Other studies like Coles and Smith (1998), Gregg and Petrongolo (2005), and Coles and Petrongolo (2008) show that the hazard rate of vacancies registered with UK Job Centres is highest in the first month after the start of the search process. Kuo and Smith (2009) and Andrews et al. (2013) use weekly data from UK Job Centres and show that the hazard rate of vacancies is highest in the first two weeks.

When we instead calculated the weekly vacancy-filling hazard using the standard piecewise-constant (weekly) estimation approach,[9] we would obtain a fluctuating hazard with no clear pattern (see Figure C.1a) in the Appendix). If we follow our approach, we always find a hazard rate which is increasing up to the intended starting date and decreases thereafter. The only exception occurs if we take firms with no planned search duration (see Figure C.1 a) in the Appendix). In this case, our approach and the standard approach coincide and give the same estimates. The differences in the shape of the vacancy-filling hazard rates between the standard and our approach can therefore be explained by the fact that our observations are centred around the intended starting date.

### 3.3.4 Definition of groups *early*, *in time*, and *delayed*

Since the main objective of the paper is to investigate how firms' recruitment behaviour changes before, around, and after the intended starting date, we split our sample into four groups. We label the first group *early*. This group includes all vacancies which successfully finished their search process at least 4 weeks prior to the intended starting date. The second group is labelled *in time*. It includes all vacancies that successfully finished their search process at most four weeks prior to the intended starting date and no later than one week after the intended starting date (weeks -3, -2, -1, 0, and 1). The third group, which we label *delayed*, includes all successful vacancies which need longer than one week after the intended starting date to recruit an applicant. The fourth group includes all vacancies which failed to hire a worker and cancelled the recruitment process. This fourth group is labelled *failed*. 37.6% of all vacancies in our sample (with all covariates available) belong to the group *early* hires, 37.0% to the group *in time* hires, 18.1% to the group *delayed* hires, and 7.3% to the group *failed* to hire. The results shown below are not sensitive to slight changes in the thresholds defining the different groups.

[9] The standard approach to estimate a piecewise-constant (weekly) baseline hazard (see e.g. Wooldridge, 2010, ch.20) uses week-indicators, which are equal to one if the vacancy is searching in the respective week and zero otherwise. The respective week-indicators are defined as searching in 1st, 2nd, 3rd, 4th, ... week after search started.

## 3.4. Identification of firms' recruitment behaviour

The aim of the paper is to evaluate how firms' recruitment behaviour changes if they are unlucky during the recruitment process. We regard those firms that hire *early* as being lucky and those which hire *in time* or *delayed*, or those which *failed* to hire as being hit by bad luck. Ideally, we would have liked to have data on the points in time when firms which are not successful in the early recruitment phase adjusted their strategy, e.g. the points in time when they start to use certain search channels or the point in time when they start to make concessions. Unfortunately, in our dataset - and this holds (to the best of our knowledge) for all vacancy datasets currently available - only information on whether or not e.g. a search channel has been used or a concession has been made is available, but not the point in time when this event took place. We therefore have to compare the recruitment behaviour of vacancies which hired *in time* or *delayed*, or *failed* to hire, with the recruitment behaviour of vacancies which hired *early*. The only exception is the use of the PEA search channel for the years 2013 and 2014. We use this information to validate our identification approach on the use of the PEA search channel in section 3.5.

Adverse selection of firms makes it difficult to identify how firms reacted if they had been unlucky during the recruitment process. For example, one would expect that more attractive firms are more likely to find a worker early. If attractive firms use a different recruitment strategy or adjust their recruitment behaviour differently in case they are not successful initially, then the differences in recruitment behaviour between *early* hiring firms and firms hiring *in time* or *delayed* are partly driven by the (un)observable characteristics which determine the attractiveness of a firm. In order to be able to interpret our results as changes in recruitment behaviour due to bad luck we need to rule out that the observed differences in recruitment behaviour are driven by selection.

In order to identify the effect of being unlucky in the recruitment process on firms' recruitment behaviour, we use the entropy balancing re-weighting technique developed by Hainmueller (2012). Entropy balancing, like e.g. propensity score matching, takes into account the selection on observables by producing weights which are subsequently used to reweight the comparison (control) observations in an OLS regression. In the entropy balancing step, we match on a host of vacancy characteristics, like the required qualification and experience level, whether the job is permanent, temporary or seasonal, requires weekend work, and whether it is full- or part-time. On the firm level, we include the number of employees (log), and the binary indicator variables "low sales", "financial constraints", and “skilled labour shortage" to control for the economic condition of the firm. The entropy balancing is done year by year. This ensures that vacancies are always matched within the same year. This accounts for possible differences in labour market conditions over time.

We are also able to account for selection along unobservable characteristics by matching on planned search duration. By conditioning on planned search duration, we control for all factors firms take into account when forming expectations on their likely search duration. Take for example the case where an unobservable characteristic - like bad reputation - is positively correlated with the search duration of a vacancy. As long as a firm takes this fact into account by increasing its planned search duration accordingly, we are able to control for this effect. Only if the firm is unable to adjust its planned search duration accordingly - maybe because a worker left without advanced notice and has to be replaced immediately, or if the firm already has a suitable candidate for the vacancy when starting to search, we are unable to account for the underlying unobserved characteristic. If in such a case the unobserved characteristic is correlated with some outcome variables, then the respective estimates are biased. To avoid such cases, we restrict our sample to vacancies with a planned search duration of more than four weeks. For our identification, we therefore assume that firms with a planned search duration of more than four weeks take expected recruitment difficulties into account when deciding on their planned search duration.

The main advantage of entropy balancing compared to standard matching and weighting techniques like propensity score weighting or nearest neighbour matching is its higher effectiveness in reducing covariate imbalance. This is accomplished by, generally speaking, re-weighting the observations of the different comparison groups such that predefined moments (in our case mean and variance) are similar to the ones of the reference group. While many weights potentially fulfil such requirements, entropy balancing chooses those which deviate as little as possible from uniform weights. Thereby, entropy balancing is advantageous since it overcomes the cumbersome rechecking in propensity score methods where “researchers ‘manually’ iterate between propensity score modelling, matching, and balance checking until they attain a satisfactory balancing solution” (Hainmueller, 2012, p.25). Unlike propensity score matching methods, entropy balancing improves the covariate balance of all conditioning variables[10] and is fully non-parametric.[11] Entropy balancing fits the covariates very well as shown in Table C.4 in the Appendix. For robustness checks, however,

[10]Using propensity score matching and similar methods can lead to a better balance between some covariates at the cost of a worse balance between other conditioning variables. See e.g. King and Nielsen (2016).

[11]We implement entropy balancing by using the program “ebalance” (Hainmueller and Xu, 2013) in Stata 14.2.

we also employ in subsection C.2 in the Appendix weights obtained from a combination of propensity score and nearest neighbour matching, the so-called radius matching approach proposed by Huber et al. (2015), Lechner et al. (2011), and Lechner and Wunsch (2009).

Entropy balancing assigns all observations in the reference group a weight equal to one. The observations in the comparison groups are assigned the respective entropy balancing weights. In order to ensure a common support of planned search duration, we choose to define the vacancy group *in time* as the reference (treatment) group. The vacancy groups *early*, *delayed*, and *failed* are the comparison (control) groups. Besides the conditional independence assumption, an important condition for propensity score methods has to be valid, the assumption of common support, meaning that for any conditioning variable there exist observations for both treatment and control group. Whereas for propensity score methods this condition is reached by simply selecting only those observations in the treatment group which have a propensity score not higher than the maximum propensity score of the observations in the control group (Caliendo and Kopeinig, 2008), it is unclear how to ensure this condition for entropy balancing. By choosing the group *in time* as the reference group, we ensure common support on planned search duration. For the radius matching approach used as a robustness check in subsection C.2 in the Appendix, we enforce the common support assumption. When comparing the number of observations from radius matching with the number of observations from entropy balancing, we find that the number of observations in entropy balancing is only 3.2% higher than in radius matching, i.e., 3.2% of our observations are not on the common support.

Calculating the entropy weights for all covariates including the planned search duration to match unobservable characteristics is the first step. The second step is the estimation of the treatment effect, implemented by an OLS regression based on the re-weighted sample. Given our formal definition of the group *in time* as the reference (treatment) group and the groups *early*, *delayed*, and *failed* as comparison (control) groups, we estimate the average treatment effect on the untreated, which can be obtained from,

$$\widehat{\beta} = \left(X'WX\right)^{-1} X'y, \tag{3.1}$$

where $y$ is the vector of outcomes, $W$ a diagonal matrix with 1 in the diagonal cells for vacancies in the *in time* group and entropy balancing weights in the diagonal cells for the comparison (control) groups. $X$ is a $n \times (k+3)$ matrix. $n$ equals the overall number of observations. $k+3$ equals the total number of control variables, where the first column is a vector of 1$s$, the second column a vector indicating the comparison group *early* and the third

column a vector indicating the comparison group *delayed*. The $k$ additional columns in $X$ contain all remaining control variables. Since for the vacancies in the group *failed* the information on the planned search duration is only available for certain years and since some covariates are not available at all, our sample would be relatively small if we restricted it to observations with information on all three comparison groups. Therefore, we estimate the behaviour of *failed* vacancies separately (where $X$ is a $n \times (k+2)$ matrix).
The OLS regressions allow us to account for additional explanatory variables. We also control for occupation-fixed effects (according to 3 digit ISCO-classification) and for time-varying effects on the regional level (180 labour market regions) by including interaction terms for years and regions. In regressions where we investigate outcomes related to the hired person (e.g. wages paid), we include gender and experience (and experience squared) of the hired worker. We also include all covariates used in the first entropy balancing step. This allows us to investigate whether balancing is effective by testing whether the estimated treatment effects change if we exclude the covariates from the first step. The coefficients are not statistically significantly different if we omit the covariates used for entropy balancing. If all factors which jointly determine the outcome $y$ and when a vacancy hires (fails to hire) $D \in \{early, in\_time, delayed, failed\}$ are observable (either directly or indirectly via the planned search duration) and controlled for by the weights from entropy balancing, $EB(X)$, we can interpret our results causally. This is properly formalized in the conditional independence assumption,

$$E\left[y|EB(X), D = m\right] = E\left[y|EB(X), D = in\_time\right], \tag{3.2}$$

where $m \in \{early, delayed, failed\}$. We assume that this assumption holds.

### 3.5. Dynamics of the recruitment process

We describe our results by chronologically “following” firms through their search process. First, we show which problems might occur whilst firms look for suitable applicants. Then we investigate how many (suitable) candidates apply and how firms adjust their search behaviour. Finally, we show how firms change their wage policy and make concessions in order to increase the likelihood of filling the vacancy.

In this section we present results based on entropy balancing and consider only those vacancies which had more than or 4 weeks of planned search duration. In Appendix C.2 we show that the results are robust to including also vacancies with a shorter planned search duration or if we use instead of entropy balancing radius matching.

### 3.5.1 Problems during the recruitment process

Firms which were not able to hire up to the intended starting date have been unlucky in the search process. If firms are unlucky, we would expect that they report having problems in finding suitable applicants. We can see that this is indeed the case as shown in Table 3.2, which shows the respective mean for the reference group *in time* and the estimated difference for the three comparison groups (*early*, *delayed* and *failed*). While the fraction of firms reporting problems in finding enough suitable applicants is around 12.4% for the *in time* group, this value increases by 17.7 percentage points for the *delayed* group. For firms that *failed* to hire the increase was much larger: 41.0 percentage points. This means that around 53.4% of *failed* firms report this problem. The opposite holds for the firms which are able to fill their vacancy *early*. Here only 6.7% report this problem.

**Table 3.2:** Problems in the recruitment process

| | Group *In Time* | Difference between *In Time* and | | |
|---|---|---|---|---|
| | (Mean) | *Early* | *Delayed* | *Failed* |
| | (1) | (2) | (3) | (4) |
| Not enough suitable applicants | 0.1239 | -0.0572*** | 0.1768*** | 0.4102*** |
| (Standard Error) | | (0.0043) | (0.0061) | (0.0124) |
| Nr. of Obs. | 15,870 | 15,480 | 7,920 | 4,005 |
| Pay claim of applicants too high | 0.0522 | -0.0236*** | 0.0804*** | 0.2542*** |
| (Standard Error) | | (0.0025) | (0.0040) | (0.0109) |
| Nr. of Obs. | 20,579 | 20,256 | 9,878 | 4,005 |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with entropy balancing weights and a restricted sample, including only vacancies with at least 28 days of planned search duration. Standard errors are reported in the parentheses. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$. Data for "Not enough suitable applicants": German Job Vacancy Survey 2009-2014. Data for "Pay claim of applicants too high": German Job Vacancy Survey 2010-2014.

A similar picture arises for the variable which indicates whether firms had problems in the recruitment process due to higher pay claims of their applicants. For the *in time* group, only 5.2% report recruitment problems due to higher wage demands. For vacancies hiring *early*, the fraction is only 2.9%. At the same time 13.3% of those firms hiring *delayed* report recruitment problems because applicants demand higher wages. This problem is again most prominent for *failed* vacancies, where around 30.6% report too high pay claims by applicants.

We interpret the increase in problems reported during the recruitment process as evidence that random matching frictions are driving longer search durations. In other words, the increase in problems reported during the recruitment process supports our identification strategy that longer search durations are driven by random bad luck shocks and not by unobserved heterogeneity.

### 3.5.2 Adjustments during the recruitment process

Table 3.3 shows the average number of applicants, the average number of suitable applicants, and the fraction of suitable applicants for the *in time* group and the estimated difference for the three comparison groups (*early*, *delayed* and *failed*). Our estimates suggest that a vacancy which hires *early* receives on average 13.9 applicants. This number increases to 16.2 applicants on average for vacancies in the *in time* group. The increase in the number of applicants for those vacancies which hire *delayed* is only minor (only 0.9 applicants more). This evidence is in line with the results reported by van Ours and Ridder (1992) who show that the majority of applicants arrive within the first two weeks after the announcement of the vacancy. Then the arrival rate of applicants drops almost to zero. Our results also suggest that *failed* vacancies are unlucky and receive 3.8 fewer applicants than the *in time* group.

**Table 3.3:** Applicants and suitable applicants

| | Group *In Time* | Difference between *In Time* and | | |
|---|---|---|---|---|
| | (Mean) | *Early* | *Delayed* | *Failed* |
| | (1) | (2) | (3) | (4) |
| Number of applicants | 16.1968 | -2.3076*** | 0.8971** | -3.8181*** |
| (Standard Error) | | (0.5321) | (0.4107) | (0.8043) |
| Nr. of Obs. | 18,375 | 17,466 | 9,095 | 1,732 |
| Number of suitable applicants | 4.6472 | -0.6383*** | -0.2827** | N.A. |
| (Standard Error) | | (0.1131) | (0.1300) | |
| Nr. of Obs. | 18,237 | 17,353 | 8,971 | |
| Fraction of suitable applicants | 0.5035 | 0.0745*** | -0.0795*** | N.A. |
| (Standard Error) | | (0.0048) | (0.0044) | |
| Nr. of Obs. | 17,942 | 16,963 | 8,879 | |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with entropy balancing weights and a restricted sample, including only vacancies with at least 28 days of planned search duration. Standard errors are reported in the parentheses. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$. Data: German Job Vacancy Survey 2005-2014.

The average number of suitable applicants increases from 4.0 suitable applicants for vacancies which hire *early* to 4.6 suitable applicants for vacancies which hire *in time* and decreases to 4.4 suitable applicants for vacancies which hire *delayed*. This suggests that firms need 4 to 5 suitable applicants to successfully hire somebody. *Early* vacancies are lucky because 57.8% of their 13.9 applicants are suitable, which allows them to get the necessary number of suitable applicants early. Vacancies hiring *in time* (*delayed*) are less lucky since only 50.4% (42.4%) of their applicants are suitable, which forces them to wait longer for the necessary number of suitable applicants to arrive.[12] For *failed* vacancies we have no

[12] The average fraction of suitable applicants is substantially higher than the ratio of the average number of suitable applicants to the average number of applicants. This difference results from the fact that firms with a large number of applicants receive relatively few suitable applicants while firms with a small number of applicants receive a relatively high share of suitable applicants.

information on the number of suitable applicants.

The first adjustment measure used by "unlucky" firms is to increase the search effort by activating more search channels. Vacancies which hire *early* use on average 1.67 search channels and vacancies which hire *in time* 1.84 search channels. If a firm is not able to hire *in time*, it activates additional search channels. *Delayed* vacancies use 0.34 search channels more, failed vacancies 0.54.

**Table 3.4:** Search channels

| | Group *In Time* | Difference between *In Time* and | | |
|---|---|---|---|---|
| | (Mean) | *Early* | *Delayed* | *Failed* |
| | (1) | (2) | (3) | (4) |
| Number of search channels activated | 1.8411 | -0.1740*** | 0.3403*** | 0.5441*** |
| (Standard Error) | | (0.0121) | (0.0136) | (0.0587) |
| Nr. of Obs. | 20,579 | 20,256 | 9,878 | 786 |
| Use of search channel: Classic | 0.5291 | -0.0891*** | 0.1064*** | -0.1352*** |
| (Standard Error) | | (0.0061) | (0.0063) | (0.0097) |
| Nr. of Obs. | 20,579 | 20,256 | 9,878 | 4,005 |
| Use of search channel: Internal | 0.2416 | -0.0001 | 0.0553*** | 0.0352*** |
| (Standard Error) | | (0.0055) | (0.0058) | (0.0106) |
| Nr. of Obs. | 20,579 | 20,256 | 9,878 | 4,005 |
| Use of search channel: Speculative | 0.2660 | -0.0212*** | 0.0374*** | 0.0110 |
| (Standard Error) | | (0.0057) | (0.0061) | (0.0268) |
| Nr. of Obs. | 20,579 | 20,256 | 9,878 | 786 |
| Use of search channel: Network | 0.4047 | 0.0602*** | 0.0202*** | 0.0473* |
| (Standard Error) | | (0.0065) | (0.0066) | (0.0283) |
| Nr. of Obs. | 20,579 | 20,256 | 9,878 | 786 |
| Use of search channel: PEA | 0.4000 | -0.1238*** | 0.1210*** | 0.2166*** |
| (Standard Error) | | (0.0061) | (0.0067) | (0.0147) |
| Nr. of Obs. | 20,579 | 20,256 | 9,878 | 2,360 |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with entropy balancing weights and a restricted sample, including only vacancies with at least 28 days of planned search duration. Standard errors are reported in the parentheses. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$. Data: German Job Vacancy Survey 2005-2014.

Firms most commonly use print or online media or internet platforms to advertise their vacancies. These channels, summarised as classic search channel, are used by 44.0% of vacancies hiring *early* and increases by 8.9 and 10.6 percentage points for vacancies hiring *in time* or *delayed*. The internal search channel i.e., the practice to fill the position with another employee or to hire an intern or apprentice seems to be used relatively rarely by vacancies (24.2% of those hiring *early* or *in time*). Both, *delayed* and *failed* vacancies, use this channel significantly more often (increase of 5.5 and 3.5 percentage points, respectively), which suggests that the internal search channel is only used if hiring via other search channels fails. The fraction of firms receiving (and using) speculative applications increases slightly from 24.5% for *early* hiring firms to 26.6% for firms hiring *in time* to 30.3% for *delayed* hiring firms. Since job seekers – not vacancies – decide on whether or not to send a speculative application, this increase is most likely due to the increased search duration. Contacts initiated by own employees (network search channel) are most frequently observed in the group of vacancies hiring *early* (46.5%). The use of the network then decreases for vacancies hiring *in time* (40.5%) and increases again for vacancies which hire *delayed* (42.5%) or *failed* to hire (45.2%). The search channel with the highest increase in usage is the intermediation service provided by the public employment agency (*early* 27.6%, *in time*, *delayed* 40.0%, and *failed* 61.7%).

**Table 3.5:** Fraction of vacancies with active PEA search channel

| Activating PEA search channel ... | Vacancies hiring ... (groups) *early* (1) | *in time* (2) | *delayed* (3) | weighted average (4) |
|---|---|---|---|---|
| | | lower bound | | |
| *early* | 0.231 | 0.237 | 0.249 | 0.238 |
| *in time* | | 0.317 | 0.412 | 0.354 |
| *delayed* | | | 0.452 | 0.452 |
| | | upper bound | | |
| *early* | 0.345 | 0.310 | 0.314 | 0.323 |
| *in time* | | 0.415 | 0.521 | 0.459 |
| *delayed* | | | 0.572 | 0.572 |

Data: German Job Vacancy Survey 2013-2014.

For the years 2013 and 2014, we have additional information on the date when the vacancy was registered with the PEA for the public employment agency search channel. This allows us to calculate when the PEA search channel was actually activated. For the groups *in time* and *delayed*, we observe how they adjusted the use of the PEA search channel in the *in time* period if they were not able to hire an applicant in the *early* period. We provide a lower and an upper bound. The lower bound is calculated based on the sample vacancies, which provided the information on the timing of PEA activation. The upper bound is calculated based on the assumption that those vacancies which reported to have used the PEA but did not report the timing of PEA activation activated the PEA in the same pattern as those in the same group (*early*, *in time*, and *delayed*).

The lower bound calculation suggests an increase of 8.0 percentage points for the group hiring *in time* and 16.5 percentage points for the group hiring *delayed*, the upper bound calculation suggest and increase of 10.5 and 20.7 percentage points for the groups *in time* and *delayed*, respectively. The last column (4) presents the weighted averages over the groups *in time* and *delayed*. The weighted averages suggest an increase in PEA activation of 11.6 and 13.6 percentage points for the lower and upper bound respectively. Our estimated increase using entropy balancing equals 12.1 percentage points and lies in between the lower and upper bound. The reaction to not hiring *in time* can only be observed for the *delayed* hiring group. The respective increases in the activation of the PEA search channel are calculated by considering the difference in the weighted averages. They are 9.8 and 11.3 percentage points for the lower and upper bound, respectively. This is slightly lower than the 12.1 increase estimated based on entropy balancing.

The increase in search effort can explain the increase in the number of applicants, which we observe for vacancies in the groups *in time* and *delayed* compared to vacancies in group *early* (see Table 3.3). That additional information on open vacancies can increase the number of applicants is also shown by Skandalis (2018), who shows that media news spreading the information that an expanding plant needs to hire many workers leads to a 60% increase in job applications in the following month.

The numbers presented in Table 3.3 and Table 3.4 suggest that the increase in the average number of search channels of 0.17 from the group *early* to the group *in time* and of 0.34 from the group *in time* to the group *delayed* is leading to an increase in the number of applications by 2.3 and 0.9, respectively. This suggests decreasing returns. It is not surprising that these additional search channels have a lower return than the ones chosen initially, since it is rational to choose first the search channels that are most efficient and activate the less efficient ones later. This suggests that the classic search channels and the PEA are not the most efficient search channels to start with. This is in line with evidence from the workers' side. Holzer (1988), and Blau and Robins (1990) for the US, Osberg (1993) for Canada, and Gregg and Wadsworth (1996) for the UK show that the productivity of the classic search channels and the PEA (in generating offers and acceptances) is lower than the productivity of networks and speculative applications.

### 3.5.3 Willingness to make concessions

Increasing the number of search channels to receive more (suitable) applications is one way to increase the chances to hire somebody. Another way to increase the probability of hiring is to adjust the terms of employment by either increasing the wage or by decreasing the required level of qualification. We refer to these as concessions. The German Job Vacancy Survey contains a set of questions regarding wage bargaining, wage payments, and qualification, experience, and previous labour market status of the hired worker, which allow us to evaluate whether a firm was willing to make such concessions.

While vacancies in the group *early* hire only few workers with an experience level below the initially required level, this fraction is significantly higher (9.6% and 16.0%) for firms which hired *in time* or *delayed* respectively, as shown in Table 3.6. A similar pattern emerges for the qualification level required. The respective fractions of firms which hired a worker with a qualification level below the required level is 5.0% for *early* hires, 7.6% for

**Table 3.6:** Concessions related to worker characteristics

| | Group *In Time* (Mean) (1) | Difference between *In Time* and *Early* (2) | *Delayed* (3) | *Failed* (4) |
|---|---|---|---|---|
| Experience lower than required | 0.0957 | -0.0322*** | 0.0640*** | N.A. |
| (Standard Error) | | (0.0036) | (0.0048) | |
| Nr. of Obs. | 19,615 | 19,410 | 9,236 | |
| Qualification lower than required | 0.0759 | -0.0264*** | 0.0561*** | N.A. |
| (Standard Error) | | (0.0033) | (0.0044) | |
| Nr. of Obs. | 19,400 | 19,270 | 8,988 | |
| Hired previously unemployed | 0.3571 | -0.0773*** | 0.0081 | N.A. |
| (Standard Error) | | (0.0059) | (0.0063) | |
| Nr. of Obs. | 20,579 | 20,256 | 9,878 | |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with entropy balancing weights and a restricted sample, including only vacancies with at least 28 days of planned search duration. Standard errors are reported in the parentheses. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$. Data: German Job Vacancy Survey 2005-2014.

*in time* hires, and 13.2% for *delayed* hires. Unemployed individuals are often thought of as being less productive than employed individuals with the same observable characteristics. If we take unemployment as a signal for lower productivity, then hiring an unemployed worker can be regarded as a concession. The fraction of hired workers who have previously been unemployed increases from 28.0% for vacancies which hire *early* to 35.7% and 36.5% for vacancies which hire *in time* or *delayed* respectively.

The results in Table 3.6 show that firms are less willing to hire somebody with lower qualification or experience or a previously unemployed worker if they are lucky and have enough suitable applicants already before the intended starting date. Once this date is approaching or even exceeded, firms are more willing to hire a less qualified or experienced worker or somebody who is unemployed.

Another way to make concessions is to adjust wages. The pattern which we observe for hourly wages can be explained with a *match-quality effect* and a *bargaining effect*. The *match-quality effect* predicts that wages paid by vacancies hiring workers with high qualification and experience are higher than wages paid by vacancies hiring workers with low qualification and experience. The *bargaining effect* predicts that wages paid by firms hiring after the intended starting date should be higher since the bargaining power decreases as the hazard of hiring a worker decreases after the intended starting date.

The first row in Table 3.7 on wage bargaining supports the hypothesis of the *bargaining effect*. Firms which hire *early* or *in time* bargain over wages in 32.9% and 31.8% of all cases. For vacancies which hire *delayed*, the fraction which report that they bargained over pay increases significantly by 5.7 percentage points. A similar pattern is shown in the second row which presents the fraction of firms which report that they paid more than initially intended. It shows that the fraction of firms which reported to have paid higher wages than initially expected increases from around 8.1% and 9.2% for vacancies which hire *early* or *in time* to 13.7% for vacancies which hire *delayed*.

**Table 3.7:** Concessions related to wages

| | Group *In Time* (Mean) | Difference between *In Time* and | | |
|---|---|---|---|---|
| | | *Early* | *Delayed* | *Failed* |
| | (1) | (2) | (3) | (4) |
| Wage bargaining | 0.3177 | -0.0110 | 0.0570** | N.A. |
| (Standard Error) | | (0.0109) | (0.0110) | |
| Nr. of Obs. | 6,040 | 5,964 | 3,127 | |
| Paid more than intended | 0.0861 | -0.0059 | 0.0508*** | N.A. |
| (Standard Error) | | (0.0036) | (0.0042) | |
| Nr. of Obs. | 20,370 | 20,023 | 9,777 | |
| Hourly wage (Euro) | 13.1879 | 0.3552*** | 0.4060*** | N.A. |
| (Standard Error) | | (0.1377) | (0.1298) | |
| Nr. of Obs. | 2,578 | 2,496 | 1,508 | |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with entropy balancing weights and a restricted sample, including only vacancies with at least 28 days of planned search duration. Standard errors are reported in the parentheses. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$. Data for "Wage bargaining took place": German Job Vacancy Survey 2011-2013. Data for "Average hourly wage": German Job Vacancy Survey 2014. Data for "Paid more than intended": German Job Vacancy Survey 2005-2014.

The pattern for average hourly wages shown in Table 3.7 can be explained as follows: The group of vacancies which hire *early* makes less often concessions and thus hires workers with on average higher qualification and experience and less often unemployed workers. This results in a significantly higher wage (13.54 Euros) compared to the group of vacancies which hire *in time* (13.19 Euros). Firms that hire *delayed* pay again significantly higher wages (13.59 Euros). Given that these firms make concessions as shown in Table 3.6, the higher wage can be explained by the *bargaining effect* dominating the *match-quality effect*.

Table 3.8 controls for the *match-quality effect* by conditioning on vacancies which made no concessions in terms of qualification and experience. To account for the wage-difference due to unemployment, the results are presented separately for vacancies which hired a previously unemployed worker and for vacancies which hired a previously employed worker. If a previously employed worker is hired, bargaining over wages and payment above the initially intended level increases only if the firm is not able to hire until the intended starting date. This can be explained by the *bargaining effect*. Wages paid by firms hiring *early* are still somewhat higher than wage paid by firms hiring *in time*, but the effect is not statistically significant. The wage pattern for previously unemployed workers is the same.

For previously unemployed workers, firms are generally less willing to bargain or increase payment above the initially intended level. This can be explained by the generally worse outside option that unemployed have compared to employed workers or by selection,

**Table 3.8:** Concessions related to wages - controlled for qualification and experience

| | Group *In Time* | Difference between *In Time* and | | |
|---|---|---|---|---|
| | (Mean) | *Early* | *Delayed* | *Failed* |
| | (1) | (2) | (3) | (4) |
| | previously employed workers | | | |
| Wage bargaining | 0.3243 | -0.0020 | 0.0748*** | N.A. |
| (Standard Error) | | (0.0135) | (0.0148) | |
| Nr. of Obs. | 3,623 | 4,458 | 1,714 | |
| Paid more than intended | 0.0810 | 0.00137 | 0.0519*** | N.A. |
| (Standard Error) | | (0.0047) | (0.0058) | |
| Nr. of Obs. | 11,089 | 13,651 | 5,014 | |
| Hourly wage (Euro) | 13.93 | 0.1741 | 0.3872** | N.A. |
| (Standard Error) | | (0.1964) | (0.1897) | |
| Nr. of Obs. | 1,558 | 1,810 | 826 | |
| | previously unemployed workers | | | |
| Wage bargaining | 0.2671 | -0.0466** | 0.0587** | N.A. |
| (Standard Error) | | (0.0249) | (0.0251) | |
| Nr. of Obs. | 1,460 | 792 | 606 | |
| Paid more than intended | 0.0542 | -0.0155*** | 0.0322*** | N.A. |
| (Standard Error) | | (0.0060) | (0.0075) | |
| Nr. of Obs. | 5,742 | 3,530 | 2,103 | |
| Hourly wage (Euro) | 11.85 | 0.3485 | 0.7894*** | N.A. |
| (Standard Error) | | (0.2712) | (0.2438) | |
| Nr. of Obs. | 536 | 356 | 264 | |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with entropy balancing weights and a restricted sample, including only vacancies with at least 28 days of planned search duration. Standard errors are reported in the parentheses. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$. Data for "Wage bargaining took place": German Job Vacancy Survey 2011-2013. Data for "Average hourly wage": German Job Vacancy Survey 2014. Data for "Paid more than intended": German Job Vacancy Survey 2005-2014.

i.e. that unemployed workers are worse in bargaining or related skills. Most interestingly however, there is a different trend in wage bargaining over the recruitment process if we compare previously employed and previously unemployed hires. The fact that bargaining over wages and payment above the initially intended level is less often observed by vacancies hiring a previously unemployed worker *early* compared to vacancies which hire a previously unemployed *in time* can be explained if we assume that unemployed workers prefer an earlier employment start over higher wages. If this is the case, then we should not only see lower levels of wage bargaining in the comparison between the vacancy groups *early* and *in time* as shown in Table 3.8, we should also see that firms which hire *early* are more willing to bargain about when employment starts with previously unemployed than with previously employed workers. We have no information on whether or not firms and workers bargained about the date when employment should start. However, we observe when employment actually started and can therefore investigate this hypothesis by analysing how the probability to hire prior to the intended starting date depends on the previous employment status of the hired worker.

In Table 3.9 we present OLS estimates of the effect that the previous employment sta-

**Table 3.9:** Employment start prior or after intended starting date and employment status

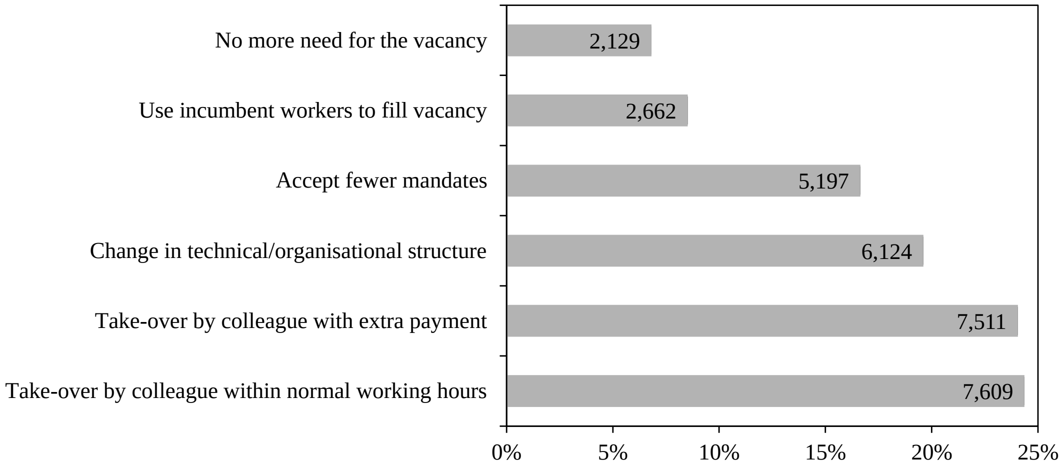
| | Vacancies hiring *early* | |
|---|---|---|
| | all vacancies (1) | at least 3 months between hiring and intended starting date (2) |
| previously unemployed workers | 0.0653*** | 0.0860** |
| | (0.0060) | (0.0337) |
| Nr. of Obs. | 18,779 | 2,109 |
| $R^2$ | 0.0895 | 0.4080 |

Estimated with OLS. Standard errors are reported in the parentheses. Covariates on firm and vacancy level are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$. Data: German Job Vacancy Survey 2005-2014.

tus has on the probability to hire prior to the intended starting date (the indicator variable is 1 if employment started prior to the intended starting date and 0 otherwise). The sample consists only of vacancies which hire *early*. We control for the same set of firm- and vacancy characteristics as in Tables 3.7 to 3.8. Column (1) in Table 3.9 shows that the probability to hire prior to the intended starting date is 6.5 percentage points higher if the hired worker was previously unemployed. To rule out that this result is due to the legal notice period of 3 months for employed workers in Germany, we restrict the sample in column (2) to *early* hiring firms which hired (signed the employment contract) at least 3 months ahead of the intended starting date. The result is robust. This supports our hypothesis that previously unemployed worker prefer earlier employment over higher wages and thus bargain less over wages as the estimates in Table 3.8 show.

### 3.5.4 Reaction if firms fail to hire

The German Job Vacancy Survey includes some information on how *failed* firms react if they fail to hire. The survey asks how firms handle the tasks, which should originally be done by the newly employed worker. Figure 3.4 summarises the replies to this question. Multiple answers are allowed. Most firms spread the tasks among incumbent workers either with or without an extra compensation (24.1% and 24.4% respectively) or delegate the work entirely to one incumbent worker (8.5%). Other firms try to overcome the labour shortage by increasing technical or organisational efficiency (19.6%). 16.6% of firms accept fewer orders and reduce output, because they do not have the labour force needed. Only few firms (6.8%) indicate that they do not need the vacancy any longer due to changes in the product market situation.




No. of Obs.: 17,084. Data: German Job Vacancy Survey 2005-2014

**Figure 3.4:** Reaction of firms towards failure in filling their vacancy

## 3.6. Theoretical explanations

In the following section we discuss in how far different theories are able to explain the recruitment pattern, which we observe.

### 3.6.1 Summary: Empirical findings

Our results show that more than 80% of firms plan to need more than four weeks to recruit a worker. This suggests that most firms do not intend to hire immediately. They rather screen and gather applications until they have enough suitable applicants so that the chances of hiring one of them is sufficiently high. In line with the evidence by van Ours and Ridder (1992), we also find that the majority of applicants arrive early. Close to and especially after the intended starting date, the number of additionally arriving applicants is low.

We also show that the vacancy-filling hazard rate increases during the planned search duration, that most hiring takes place around the initially intended starting date, and that after the intended starting date the vacancy-filling hazard falls. At the same time the fraction of suitable applicants decreases.

If firms have not received enough applications from suitable candidates, they activate additional search channels to increase the number of applications. The additionally activated search channels have a lower return than the initially activated ones. Firms which are unable to hire until the intended starting date are more willing to hire workers with a qualification or experience below the initially required level or a previously unemployed workers. In addition, they become more and more willing to bargain over wages and pay wages above the initially intended level. The shape of the vacancy-filling hazard suggests that the higher willingness to bargain over wages increases as the likelihood to hire somebody decreases.

### 3.6.2 Planned search duration and the increasing hazard until the intended starting date

Burdett and Cunningham (1994) and Burdett and Cunningham (1998) suggest that firms want to wait with hiring because of advance notice of workers who want to leave the firm and need to be replaced in a few weeks/months time. Another reason for observing planned search durations is that firms decide to gather a sufficiently high number of suitable applicants before starting to negotiate over pay. The reason is that firms are able to pay lower wages if the number of applicants exceeds the number of firms competing for them as Albrecht et al. (2006) or Gautier and Holzner (2018) show.

In a random search model with advance notice Burdett and Cunningham (1994) and Burdett and Cunningham (1998) show – in the spirit of van den Berg (1990) – that the longer the advance notice the pickier firms are and that firms lower their reservation productivity as the intended starting date approaches. This explains the increasing hazard rate as the intended starting date approaches.

If applicants arrive at a Poisson rate – as dynamic search models generally assume – then gathering applicants can also explain the increasing vacancy-filling hazard during the planned search duration. The Poisson arrival rate of applicants predicts over time an increase in the number of firms which have gathered sufficiently many applicants to let them compete for the job. Thus, an increasing number of firms will hire as time passes.

### 3.6.3 Number of (suitable) applicants and decreasing hazard after the intended starting date

Traditional search models which assume that workers randomly search have difficulties in explaining the decline in applicants over time and the decrease in the vacancy-filling hazard after the intended starting date. Instead, stock-flow matching models as proposed by Coles and Muthoo (1998) and Coles and Smith (1998) and more recently by Ebrahimy and Shimer (2010) and Carrillo-Tudela and Hawkins (2016) are build to explain this pattern. Newly posted vacancies initially receive many applications from the stock of job seekers. If a vacancy cannot be filled by matching with an applicant from the stock of job seekers, it has to wait until new workers start to search. Since the inflow of new job seekers is small compared to the stock of job seekers, the number of new applicants and the vacancy-filling hazard drops after a vacancy was unable to match with somebody from the stock of job seekers. Another explanation for the decreasing number of applicants and the decreasing vacancy-filling hazard over time is provided by research on phantom vacancies by Cheron and Decreuse (2016) and Albrecht et al. (2017). Especially the latter paper, which combines directed search with phantom vacancies, is able to explain this pattern. Phantom vacancies are vacancies which are still advertised although they are no longer available either because somebody has already been hired or because the screening process is already at such an advanced stage that it is unlikely that the firm will consider late arriving applications. Albrecht et al. (2017) show that workers anticipate this and are more likely to apply to newly posted vacancies than to older vacancies. Thus, the number of arriving applicants and the vacancy-filling hazard decrease over time.

### 3.6.4 Reservation productivity and wages

The increased willingness to make concessions after the intended starting date can be explained with stock-flow matching. The decline in potential matching partners after a firm failed to hire somebody from the stock of job seekers decreases its outside option. Thus, firms that remain unlucky and cannot find a suitable worker until the intended starting date are willing to make concessions by accepting workers with a lower qualification or experience or by increasing their willingness to bargain over pay and to pay higher wages than initially intended.

The increased willingness to make concessions or pay higher wages is difficult to explain with other models. Take for example the directed search model with phantom vacancies as in Albrecht et al. (2017). If firms fail to find a suitable worker until the intended starting date, it would be optimal for them to post this information and signal to job seekers that they are no phantoms. They would then receive equally many applications as new vacancies and there would be no need to make concessions.

### 3.6.5 Missing element I: Screening applicants

One reason for not hiring immediately is the need to screen applicants. The models we are aware of are not explicitly modelling the time needed to screen applicants. Directed search models or stock-flow matching models, in which sorting and screening of workers is assumed to take place instantaneously,[13] could be amended to account for a planned search duration. And heterogeneity in planned search durations could be explained with heterogeneity in the time needed to screen applicants.

A directed search model with screening might be able to explain the increasing vacancy-filling hazard. If directed search is embedded in a dynamic model, where workers randomly (at a Poisson rate) receive notice about an available vacancy, workers will apply at a Poisson rate. If one further assumes economies of scale in screening, firms will wait until they have enough applications collected. The increasing hazard during the planned search duration can then again be explained by the Poisson arrival rate, which predicts that the number of firms which have gathered sufficiently many applicants increases over time.

If one introduces a screening period into a stock-flow matching model, then one might be able to explain the increase in the vacancy-filling hazard during the planned search duration. If firms follow a sequential screen policy, because screening is time-consuming and time is costly, then it seems plausible that firms will use a reservation productivity rule in order to decide when to stop screening. Given the number of applicants a firm received (from the stock of job seekers) and the expected low inflow of new job seekers, the reservation productivity should be decreasing over time as less and less unscreened applicants are left. Thus, as time progresses, it becomes more and more likely that one of the screened applicants passes the reservation productivity threshold and hiring takes place.

### 3.6.6 Missing element II: Increasing the workload if firms do not hire

Vacancies which fail to hire report having received not enough applications from suitable candidates although they searched more intensely. They also report that applicants were not willing to accept their low wage offers. These firms were apparently not willing to meet the higher wage demands to secure a hire. The fact that most of them spread the tasks of the vacant position among incumbent workers (with or without extra pay) suggests that this is the cheaper option for them. We are not aware of any theory which considers this inside option when deciding on which wage to offer.

[13] See Guerrieri et al. (2010), Shao (2014), Chang (2018), Guerrieri and Shimer (2014), Chen et al. (2016), Williams (2016), Davoodalhosseini (2019), Holzner and Watanabe (2018) for directed search models with screening and Coles and Muthoo (1998), Coles and Smith (1998), Ebrahimy and Shimer (2010), and Carrillo-Tudela and Hawkins (2016) for stock-flow matching models with screening.

## 3.7. Conclusion

In this paper we identify new patterns about the recruitment behaviour of firms. We use entropy balancing on observable vacancy characteristics to generate synthetic control groups for firms which have been unlucky in the search process. In addition, we are able to control for unobserved vacancy characteristics by controlling for planned search durations. Using this method, we show that the vacancy-filling hazard is increasing during the planned search period and decreases thereafter, that most applicants arrive early in the recruitment process, and that the willingness to pay higher wages or to hire less qualified or experienced applicants increases for firms which have been unlucky and unable to hire until the intended starting date.

We compare our findings with the predictions of different theories and argue that stock-flow matching models – if suitably amended by a time-consuming screening technology – are able to explain the whole recruitment pattern which we observe. Other theories are only partly able to explain our findings. For example, a dynamic directed search model with phantom vacancies and screening is able to explain the initially increasing and later decreasing vacancy-filling hazard, it is, however, unable to explain why firms should be willing to make concessions once they fail to hire initially.

# Appendix C

## C.1 Planned duration and hazard rates

**Table C.1:** Determinants of planned search duration (OLS-regression)

| Dependent Variable: | Log of planned search duration |
|---|---|
| Low qualification required | -0.2794*** |
| | (0.0207) |
| High qualification required | 0.1331*** |
| | (0.0159) |
| Experience required | 0.0481*** |
| | (0.0101) |
| Permanent position | 0.0628*** |
| | (0.0115) |
| Full-time employment | 0.0419*** |
| | (0.0128) |
| Seasonal employment | -0.0735*** |
| | (0.0247) |
| Temporary employment | -0.2207*** |
| | (0.0168) |
| Log of firm size (employees) | -0.0086** |
| | (0.0037) |
| Financial constraints | -0.0240 |
| | (0.0202) |
| Low sales | -0.0309** |
| | (0.0147) |
| Skilled labour shortage | -0.0785*** |
| | (0.0190) |
| Nr. of Obs. | 52,336 |
| $R^2$ | 0.1392 |

Results represent OLS-results with robust standard errors in parentheses. Control variables for year, industry, region, month of search start and day within month of search start are not reported. Data: German Job Vacancy Survey 2005-2014. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$.

**Table C.2:** Hazard rates for different planned search duration

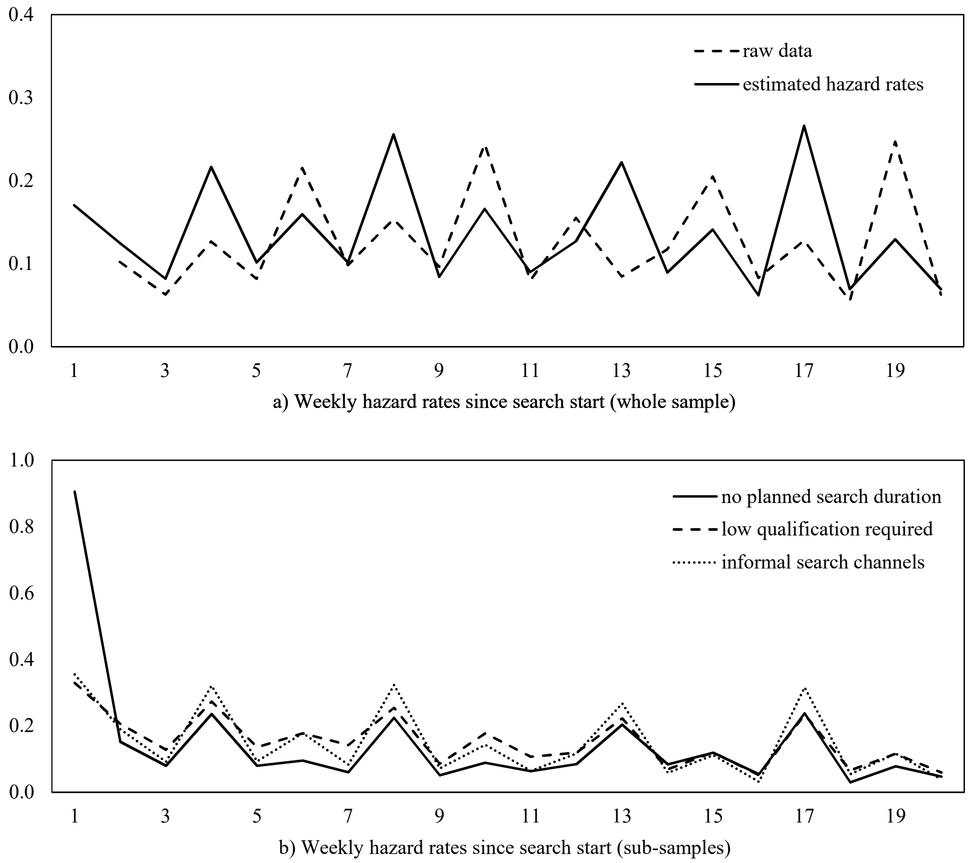
| Weeks relative to the intended starting date | Weeks of planned search duration 0 (1) | 4 (2) | 8 (3) | 13 (4) | 17 (5) | all (6) | all nr. at risk (7) |
|---|---|---|---|---|---|---|---|
| -18 | | | | | | 0.3196 | 7,620 |
| -17 | | | | | 0.0177 | 0.0469 | 8,918 |
| -16 | | | | | 0.0033 | 0.0161 | 8,779 |
| -15 | | | | | 0.0194 | 0.0273 | 9,282 |
| -14 | | | | | 0.0070 | 0.0237 | 9,329 |
| -13 | | | | 0.0232 | 0.0983 | 0.0771 | 15,314 |
| -12 | | | | 0.0063 | 0.0162 | 0.0165 | 15,727 |
| -11 | | | | 0.0224 | 0.0386 | 0.0341 | 16,651 |
| -10 | | | | 0.0122 | 0.0244 | 0.0226 | 16,932 |
| -9 | | | | 0.1571 | 0.2050 | 0.1323 | 17,230 |
| -8 | | | 0.0318 | 0.0365 | 0.0310 | 0.0511 | 26,791 |
| -7 | | | 0.0115 | 0.0929 | 0.0964 | 0.0553 | 26,501 |
| -6 | | | 0.0560 | 0.0572 | 0.0485 | 0.0649 | 28,452 |
| -5 | | | 0.0341 | 0.3636 | 0.1377 | 0.1169 | 27,656 |
| -4 | | 0.1181 | 0.3037 | 0.0778 | 0.3377 | 0.1921 | 37,888 |
| -3 | | 0.0690 | 0.1008 | 0.2995 | 0.1214 | 0.1212 | 32,424 |
| -2 | | 0.3006 | 0.3021 | 0.1644 | 0.2510 | 0.2763 | 32,760 |
| -1 | | 0.2040 | 0.2020 | 0.1369 | 0.1194 | 0.2567 | 25,446 |
| 0 | 0.4729 | 0.3142 | 0.2937 | 0.2446 | 0.2388 | 0.3537 | 23,480 |
| 1 | 0.1472 | 0.1714 | 0.1042 | 0.0961 | 0.0770 | 0.1630 | 15,175 |
| 2 | 0.1615 | 0.2389 | 0.2357 | 0.1919 | 0.1667 | 0.2266 | 12,701 |
| 3 | 0.0824 | 0.1456 | 0.1114 | 0.0499 | 0.0605 | 0.1331 | 9,823 |
| 4 | 0.2396 | 0.2935 | 0.1408 | 0.2850 | 0.3249 | 0.2340 | 8,516 |
| 5 | 0.0765 | 0.0858 | 0.2308 | 0.0668 | 0.0332 | 0.1282 | 6,523 |
| 6 | 0.0918 | 0.1776 | 0.0899 | 0.1575 | 0.1717 | 0.1426 | 5,687 |
| 7 | 0.0555 | 0.1034 | 0.1547 | 0.0652 | 0.0777 | 0.1112 | 4,876 |
| 8 | 0.2109 | 0.1328 | 0.0686 | 0.2939 | 0.1854 | 0.1530 | 4,334 |
| 9 | 0.0492 | 0.2128 | 0.2810 | 0.0644 | 0.2759 | 0.1885 | 3,671 |
| 10 | 0.0853 | 0.0946 | 0.0721 | 0.1697 | 0.1048 | 0.0967 | 2,979 |
| 11 | 0.0612 | 0.1597 | 0.1452 | 0.0718 | 0.1596 | 0.1249 | 2,691 |
| 12 | 0.0782 | 0.0604 | 0.1029 | 0.0893 | 0.0886 | 0.0828 | 2,355 |
| 13 | 0.1837 | 0.2401 | 0.3013 | 0.3072 | 0.3056 | 0.2347 | 2,160 |
| 14 | 0.0736 | 0.0547 | 0.0534 | 0.0755 | 0.1600 | 0.0823 | 1,653 |
| 15 | 0.1005 | 0.1079 | 0.1452 | 0.1020 | 0.2857 | 0.1239 | 1,517 |
| 16 | 0.0442 | 0.0767 | 0.0708 | 0.1250 | 0.1000 | 0.0745 | 1,329 |
| 17 | 0.2092 | 0.2875 | 0.2284 | 0.4026 | 0.9259 | 0.2504 | 1,230 |
| 18 | 0.0275 | 0.0493 | 0.3026 | 0.1087 | 1.0000 | 0.1182 | 922 |
| 19 | 0.0636 | 0.1887 | 0.1226 | 0.2195 | | 0.1353 | 813 |
| 20 | 0.0415 | 0.0640 | 0.1183 | 0.1563 | | 0.0953 | 703 |
| 21 | 0.2165 | 0.1118 | 0.0976 | 0.9259 | | 0.1965 | 636 |
| 22 | 0.0452 | 0.2448 | 0.4459 | 1.0000 | | 0.2172 | 511 |
| 23 | 0.0474 | 0.0833 | 0.0976 | | | 0.0725 | 400 |
| 24 | 0.0552 | 0.1515 | 0.2432 | | | 0.1267 | 371 |

Data: German Job Vacancy Survey 2005-2014.

**Table C.3:** Hazard-Ratios of Filling a Vacancy

| Weeks relative to the intended starting date | Exponential Hazard-Model Hazard-Rates (1) | S.E. (2) | Cox-Proportion Hazard-Model Hazard-Ratio (3) | S.E. (4) | Competing Risk Model Hazard-Ratio (5) | S.E. (6) |
|---|---|---|---|---|---|---|
| -18 or more | 0.0282 | (0.0008) | 0.2093 | (0.0082) | 0.1654 | (0.0062) |
| -17 | 0.0669 | (0.0038) | 0.4329 | (0.0260) | 0.3687 | (0.0216) |
| -16 | 0.0247 | (0.0017) | 0.1698 | (0.0128) | 0.1364 | (0.0103) |
| -15 | 0.0284 | (0.0019) | 0.2154 | (0.0152) | 0.1761 | (0.0124) |
| -14 | 0.0257 | (0.0018) | 0.2644 | (0.0188) | 0.2294 | (0.0162) |
| -13 | 0.1135 | (0.0037) | 0.6323 | (0.0263) | 0.5368 | (0.0219) |
| -12 | 0.0262 | (0.0014) | 0.1957 | (0.0116) | 0.1603 | (0.0094) |
| -11 | 0.0367 | (0.0016) | 0.2653 | (0.0133) | 0.2161 | (0.0108) |
| -10 | 0.0237 | (0.0013) | 0.2425 | (0.0141) | 0.2079 | (0.0120) |
| -9 | 0.1372 | (0.0030) | 0.8139 | (0.0283) | 0.6941 | (0.0236) |
| -8 | 0.0669 | (0.0022) | 0.5756 | (0.0241) | 0.4911 | (0.0203) |
| -7 | 0.0697 | (0.0017) | 0.4483 | (0.0164) | 0.3667 | (0.0135) |
| -6 | 0.0693 | (0.0017) | 0.5957 | (0.0220) | 0.5052 | (0.0185) |
| -5 | 0.1221 | (0.0022) | 0.9717 | (0.0319) | 0.8147 | (0.0263) |
| -4 | 0.2318 | (0.0031) | 1.4820 | (0.0457) | 1.2302 | (0.0374) |
| -3 | 0.1698 | (0.0024) | 1.2179 | (0.0380) | 0.9903 | (0.0309) |
| -2 | 0.2911 | (0.0032) | 2.1810 | (0.0655) | 1.7837 | (0.0534) |
| -1 | 0.2788 | (0.0035) | 2.5579 | (0.0781) | 2.0416 | (0.0628) |
| 0 | 0.3598 | (0.0045) | 2.0535 | (0.0620) | 1.4553 | (0.0451) |
| 1 | 0.3111 | (0.0047) | 2.4931 | (0.0787) | 1.6870 | (0.0571) |
| 2 | 0.2384 | (0.0045) | 1.6286 | (0.0549) | 1.2189 | (0.0420) |
| 3 | 0.1419 | (0.0040) | 1.4993 | (0.0595) | 1.1897 | (0.0478) |
| 4 | 0.2490 | (0.0057) | 1.4062 | (0.0503) | 0.9384 | (0.0351) |
| 5 | 0.1380 | (0.0049) | 1.1300 | (0.0507) | 0.8260 | (0.0381) |
| 6 | 0.1529 | (0.0055) | 1.0857 | (0.0492) | 0.8964 | (0.0410) |
| 7 | 0.1239 | (0.0054) | 1.1113 | (0.0578) | 0.9368 | (0.0497) |
| 8 | 0.1682 | (0.0067) | 1.1374 | (0.0549) | 0.8054 | (0.0413) |
| 9 | 0.2076 | (0.0081) | 1.2413 | (0.0594) | 0.8321 | (0.0427) |
| 10 | 0.1050 | (0.0064) | 0.8826 | (0.0584) | 0.8366 | (0.0569) |
| 11 | 0.1383 | (0.0077) | 1.0782 | (0.0674) | 1.1162 | (0.0714) |
| 12 | 0.0898 | (0.0066) | 0.9253 | (0.0723) | 0.9493 | (0.0764) |
| 13 | 0.2649 | (0.0119) | 1.1788 | (0.0628) | 0.9754 | (0.0554) |
| 14 | 0.0913 | (0.0080) | 0.9401 | (0.0864) | 1.1557 | (0.1101) |
| 15 | 0.1403 | (0.0104) | 0.8954 | (0.0714) | 1.2167 | (0.0984) |
| 16 | 0.0864 | (0.0087) | 0.9697 | (0.1036) | 1.4527 | (0.1592) |
| 17 | 0.2840 | (0.0165) | 1.1432 | (0.0739) | 1.2510 | (0.0866) |
| 18 | 0.1330 | (0.0131) | 1.0114 | (0.1031) | 1.5258 | (0.1625) |
| 19 | 0.1506 | (0.0148) | 0.9416 | (0.0958) | 1.6744 | (0.1723) |
| 20 | 0.1113 | (0.0137) | 0.9344 | (0.1198) | 2.0282 | (0.2665) |
| 21 | 0.2310 | (0.0208) | 0.9185 | (0.0879) | 1.3928 | (0.1421) |
| 22 | 0.2528 | (0.0243) | 1.2021 | (0.1220) | 2.3795 | (0.2558) |
| 23 | 0.0874 | (0.0162) | 0.5344 | (0.1021) | 1.2468 | (0.2420) |
| 24 | 0.1370 | (0.0211) | 0.9871 | (0.1508) | 2.7707 | (0.4253) |
| 25 | 0.1885 | (0.0264) | 1.0691 | (0.1579) | 2.1814 | (0.3388) |
| Low qualification required | 1.1184 | (0.0148) | 1.1081 | (0.0141) | 1.1074 | (0.0147) |
| High qualification required | 0.8348 | (0.0089) | 0.8285 | (0.0086) | 0.8675 | (0.0085) |
| Full-time employment | 0.9335 | (0.0101) | 0.9838 | (0.0027) | 0.9865 | (0.0027) |
| Financial constraints | 0.9722 | (0.0158) | 0.9751 | (0.0155) | 0.9377 | (0.0147) |
| Low sales | 1.0173 | (0.0119) | 1.0144 | (0.0116) | 0.9884 | (0.0113) |
| Skilled labour shortage | 0.6484 | (0.0089) | 0.6447 | (0.0087) | 0.5450 | (0.0074) |

Data: German Job Vacancy Survey 2005-2014.




Data: German Job Vacancy Survey 2005-2014

**Figure C.1:** Hazard rates since search start

### C.2 Robustness checks

This section investigates whether the results are robust to including also vacancies with a shorter planned search duration or if we use instead of entropy balancing radius matching.

*Sample including vacancies with shorter planned search duration*

In the previous section, we restricted the sample to vacancies with more than 28 days of planned search duration to account for unobservable characteristics of a vacancy. E.g. a firm indicating a shorter planned search period might be exposed to a certain limitation (e.g. a short term order which has to be fulfilled) and simply needs a new employee as fast as possible. Therefore, such firms might differ in their way of searching for a worker or in their willingness to accept less qualified candidates in contrast to other firms which do not suffer such restrictions.

Table C.5 in the Appendix displays the results for problems during the recruitment process for the unrestricted sample with entropy balancing weights. As in our benchmark specification in section 3.5 *early* hires report significantly fewer problems because of not having enough suitable applicants or higher wage demands by applicant in contrast to the *in time* group and even more so in contrast to the *delayed* and *failed* groups. The results are also similar for the number of (suitable) applicants and the fraction of suitable applicants as shown in Table C.6 in the Appendix, i.e., the number of applicants increases from the groups *early* to the groups *in time* and *delayed* and decreases again for the group *failed*. The fraction of suitable applicants among all applicants decreases from the highest value for the *early* group down to the lowest value for the *delayed* group (suitable applicants are not available for *failed* vacancies). The picture remains also unchanged for the increase in search intensity, which we observed in the restricted sample (see Table C.7 in the Appendix). Also the pattern of how firms adjust the single search channels remains the same. Table C.8 in the Appendix shows that vacancies, which are not able to hire *early*, are like in the benchmark case more and more willing to hire applicants, who are unemployed or have an experience or a qualification lower than initially required. Also wage bargaining, payment above the initially intended level, and the hourly wage of all hired workers follows a similar pattern as in the benchmark case (see Table C.9 in the Appendix). The difference in hourly wages between the groups *early* and *in-time* is no longer statistically significant, but the sign is the same.[14] The results for the sub-sample of matches, in which no concessions were made, are also generally in line with the benchmark case. That the results are robust to including vacancies with a planned search duration shorter than 28 days is comforting and shows that the documented dynamics of the recruitment pattern holds in general.

**Table C.4:** Raw and weighted covariate means with entropy balancing weights

| Variable | Means *In Time* | Means *Early* Raw | Means *Early* EB | Means *Delayed* Raw | Means *Delayed* EB | Means[1] *In Time* | Means *Failed* Raw | Means *Failed* EB |
|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Planned search duration | 60.633 | 112.454 | 60.640<br>(0.014) | 67.474 | 60.651<br>(0.031) | 62.094 | 125.563 | 62.010<br>(0.007) |
| Low qualification required | 0.114 | 0.064 | 0.114<br>(-0.001) | 0.086 | 0.114<br>(0.008) | 0.119 | 0.091 | 0.119<br>(-0.015) |
| High qualification required | 0.189 | 0.256 | 0.189<br>(0.001) | 0.265 | 0.189<br>(0.005) | 0.182 | 0.238 | 0.182<br>(-0.011) |
| Experience required | 0.444 | 0.468 | 0.444<br>(0.000) | 0.493 | 0.444<br>(0.011) | N.A. | N.A. | N.A. |
| Permanent position | 0.444 | 0.510 | 0.444<br>(0.000) | 0.529 | 0.444<br>(0.016) | N.A. | N.A. | N.A. |
| Full time position | 0.782 | 0.789 | 0.782<br>(0.001) | 0.852 | 0.782<br>(0.026) | N.A. | N.A. | N.A. |
| Seasonal work | 0.068 | 0.041 | 0.068<br>(-0.001) | 0.042 | 0.068<br>(-0.007) | N.A. | N.A. | N.A. |
| Temporary employment | 0.171 | 0.128 | 0.171<br>(-0.001) | 0.101 | 0.171<br>(-0.011) | N.A. | N.A. | N.A. |
| Log of firm size (employees) | 3.848 | 3.813 | 3.848<br>(0.000) | 3.922 | 3.848<br>(0.020) | 3.837 | 3.710 | 3.837<br>(0.008) |
| Financial distress | 0.071 | 0.063 | 0.071<br>(0.000) | 0.070 | 0.071<br>(-0.001) | 0.059 | 0.080 | 0.059<br>(-0.007) |
| Low sales | 0.153 | 0.130 | 0.153<br>(0.000) | 0.168 | 0.153<br>(0.000) | 0.140 | 0.177 | 0.140<br>(-0.014) |
| Skilled labour shortage | 0.085 | 0.061 | 0.085<br>(0.000) | 0.193 | 0.085<br>(0.066) | 0.086 | 0.331 | 0.086<br>(-0.008) |
| No. of Obs. | 20,579 | 20,256 | | 9,878 | | 13,817 | 4,005 | |

t-statistics are reported in parentheses below respective values and refer to a two sided t-test against the unweighted mean of the *In Time* Group. Thereby, the t-statistics in row (3) and (5) refer to a t-test against the means of row (1), while the t-statistics in row (8) refer to the means in row (6). Raw means are unweighted means while the rows, marked with "EB" represent weighted means by an entropy balancing procedure. Data: German Job Vacancy Survey 2005-2014.

[1] The difference between Means *In Time* in this row and the respective means from row (1) is due to a lack of data for failed vacancies for several years in contrast to vacancies, which were filled. This also explains the difference in the number of observations for row (1) and row (6).

[14] For hourly wages of previously unemployed workers hired *early*, we are unable to present results, because the entropy balancing weights obtained for this small sample (wages are only available in 2014) are due to the lack of common support (the planned search duration in the *early* group has to be longer than 28 days by definition, while the planned search duration in the *in time* group includes a majority of vacancies with less then 28 days of planned search duration) are equal to zero.

**Table C.5:** Problems in the recruitment process - OLS with Entropy Balancing (full sample)

| | Group *In Time* (Mean) (1) | Difference between *In Time* and *Early* (2) | *Delayed* (3) | *Failed* (4) |
|---|---|---|---|---|
| Not enough suitable applicants | 0.1126 | -0.0553*** | 0.1689*** | 0.4287*** |
| (Standard Error) | | (0.0048) | (0.0050) | (0.0109) |
| Nr. of Obs. | 21,646 | 15,534 | 11,645 | 4,928 |
| Pay claim of applicants too high | 0.0475 | -0.0230*** | 0.0753*** | 0.2503*** |
| (Standard Error) | | (0.0026) | (0.0033) | (0.0096) |
| Nr. of Obs. | 28,524 | 20,326 | 14,589 | 4,928 |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with entropy balancing weights. Standard errors are reported in the parentheses. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$. Data for "Not enough suitable applicants": German Job Vacancy Survey 2009-2014. Data for "Pay claim of applicants too high": German Job Vacancy Survey 2010-2014.

**Table C.6:** Applicants and suitable applicants - OLS with Entropy Balancing (full sample)

| | Group *In Time* (Mean) (1) | Difference between *In Time* and *Early* (2) | *Delayed* (3) | *Failed* (4) |
|---|---|---|---|---|
| Number of applicants | 14.4071 | -2.8221*** | 1.7537*** | -2.2941*** |
| (Standard Error) | | (0.5520) | (0.3331) | (0.7168) |
| Nr. of Obs. | 24,911 | 17,516 | 13,270 | 2,104 |
| Number of suitable applicants | 4.3431 | -0.8496*** | -0.1587 | N.A. |
| (Standard Error) | | (0.1145) | (0.1060) | () |
| Nr. of Obs. | 24,801 | 17,403 | 13,097 | |
| Fraction of suitable applicants | 0.5343 | 0.0795*** | -0.0922*** | N.A. |
| (Standard Error) | | (0.0061) | (0.0038) | () |
| Nr. of Obs. | 24,253 | 17,012 | 12,919 | |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with entropy balancing weights. Standard errors are reported in the parentheses. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$. Data: German Job Vacancy Survey 2005-2014.

**Table C.7:** Search channels - OLS with Entropy Balancing (full sample)

| | Group *In Time* (Mean) (1) | Difference between *In Time* and *Early* (2) | *Delayed* (3) | *Failed* (4) |
|---|---|---|---|---|
| Number of search channels activated | 1.7683 | -0.1836*** | 0.3405*** | 0.5547*** |
| (Standard Error) | | (0.0139) | (0.0113) | (0.0522) |
| Nr. of Obs. | 28,524 | 20,326 | 14,589 | 920 |
| Use of search channel: Classic | 0.4691 | -0.0921*** | 0.1169*** | -0.1013*** |
| (Standard Error) | | (0.0073) | (0.0053) | (0.0081) |
| Nr. of Obs. | 28,524 | 20,326 | 14,589 | 4,928 |
| Use of search channel: Internal | 0.2163 | 0.0036 | 0.0543*** | 0.0445*** |
| (Standard Error | | (0.0065) | (0.0047) | (0.0089) |
| Nr. of Obs. | 28,524 | 20,326 | 14,589 | 4,928 |
| Use of search channel: Speculative | 0.2664 | -0.0319*** | 0.0332*** | 0.0116 |
| (Standard Error) | | (0.0068) | (0.0051) | (0.0236) |
| Nr. of Obs. | 28,524 | 20,326 | 14,589 | 920 |
| Use of search channel: Network | 0.4354 | 0.0661*** | 0.0003 | 0.0291 |
| (Standard Error) | | (0.0081) | (0.0056) | (0.0263) |
| Nr. of Obs. | 28,524 | 20,326 | 14,589 | 920 |
| Use of search channel: PEA | 0.3811 | -0.1292*** | 0.1358*** | 0.2293*** |
| (Standard Error) | | (0.0072) | (0.0056) | (0.0133) |
| Nr. of Obs. | 28,524 | 20,326 | 14,589 | 2,860 |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with entropy balancing weights. Standard errors are reported in the parentheses. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$. Data: German Job Vacancy Survey 2005-2014.

**Table C.8:** Concessions related to worker characteristics - OLS with Entropy Balancing (full sample)

| | Group *In Time* (Mean) (1) | Difference between *In Time* and *Early* (2) | *Delayed* (3) | *Failed* (4) |
|---|---|---|---|---|
| Experience lower than required | 0.0905 | -0.0277*** | 0.0634*** | N.A. |
| (Standard Error) | | (0.0044) | (0.0039) | |
| Nr. of Obs. | 27,197 | 19,480 | 13,627 | |
| Qualification lower than required | 0.0722 | -0.0194*** | 0.0565*** | N.A. |
| (Standard Error) | | (0.0041) | (0.0037) | |
| Nr. of Obs. | 26,932 | 19,339 | 13,312 | |
| Hired previously unemployed | 0.3984 | -0.0899*** | 0.0037 | N.A. |
| (Standard Error) | | (0.0075) | (0.0054) | |
| Nr. of Obs. | 28,524 | 20,326 | 14,589 | |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with entropy balancing weights. Standard errors are reported in the parentheses. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$. Data: German Job Vacancy Survey 2005-2014.

**Table C.9:** Concessions related to wages - OLS with Entropy Balancing (full sample)

| | Group *In Time* (Mean) | Difference between *In Time* and *Early* | *Delayed* | *Failed* |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Wage bargaining | 0.3020 | -0.0127 | 0.0614*** | N.A. |
| (Standard Error) | | (0.0124) | (0.0094) | |
| Nr. of Obs. | 7,854 | 5,978 | 4,406 | |
| Paid more than intended | 0.0802 | -0.0072 | 0.0449*** | N.A. |
| (Standard Error) | | (0.0042) | (0.0034) | |
| Nr. of Obs. | 28,227 | 20,092 | 14,444 | |
| Hourly wage (Euro) | 13.0253 | 0.1416 | 0.2223** | N.A. |
| (Standard Error) | | (0.1280) | (0.1008) | |
| Nr. of Obs. | 3,842 | 2,514 | 2,330 | |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with entropy balancing weights. Standard errors are reported in the parentheses. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$. Data for "Wage bargaining took place": German Job Vacancy Survey 2011-2013. Data for "Average hourly wage": German Job Vacancy Survey 2014. Data for "Paid more than intended": German Job Vacancy Survey 2005-2014.

**Table C.10:** Concessions related to wages - controlled for qualification and experience - OLS with Entropy Balancing (full sample)

| | Group *In Time* (Mean) | Difference between *In Time* and *Early* | *Delayed* | *Failed* |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| | | previously employed workers | | |
| Wage bargaining | 0.3142 | -0.0014 | 0.0762*** | N.A. |
| (Standard Error) | | (0.0147) | (0.0130) | |
| Nr. of Obs. | 4,535 | 4,467 | 2,333 | |
| Paid more than intended | 0.0767 | 0.0029 | 0.0516*** | N.A. |
| (Standard Error) | | (0.0054) | (0.0050) | |
| Nr. of Obs. | 14,441 | 13,680 | 7,106 | |
| Hourly wage (Euro) | 13.86 | -0.0802 | 0.1791 | N.A. |
| (Standard Error) | | (0.1696) | (0.1440) | |
| Nr. of Obs. | 2,260 | 1,814 | 1,250 | |
| | | previously unemployed workers | | |
| Wage bargaining | 0.243 | -0.0427* | 0.0672*** | N.A. |
| (Standard Error) | | (0.0249) | (0.0196) | |
| Nr. of Obs. | 2,139 | 796 | 963 | |
| Paid more than intended | 0.0610 | -0.0112 | 0.0224*** | N.A. |
| (Standard Error) | | (0.0073) | (0.0056) | |
| Nr. of Obs. | 11,466 | 2,855 | 4,807 | |
| Hourly wage (Euro) | 13.22 | N.A. | 0.1672 | N.A. |
| (Standard Error) | | | (0.1732) | |
| Nr. of Obs. | 3,176 | | 1,710 | |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with entropy balancing weights. Standard errors are reported in the parentheses. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$. Data for "Wage bargaining took place": German Job Vacancy Survey 2011-2013. Data for "Average hourly wage": German Job Vacancy Survey 2014. Data for "Paid more than intended": German Job Vacancy Survey 2005-2014.

*Radius matching with the baseline sample*

As an alternative to the entropy balancing approach, we use a radius matching approach to match the *in time* group with the *early*, *delayed* and *failed* hires. The literature on propensity score matching suggests to use radius matching proposed by Huber et al. (2015), Lechner et al. (2011), and Lechner and Wunsch (2009), because it allows for a relatively high precision especially in settings when many good comparison observations are available. The approach is a combination of propensity score matching and nearest neighbour matching. After calculating the propensity score of belonging to the groups *early*, *delayed* or *failed* hires respectively, a radius is formed around each observation in group *in time*. Every observation in this radius is then used to form a synthetic control observation in the group *early*, *delayed* or *failed* hires respectively, where all control observations are weighted by inverse probability weighting, based on their distance towards the observations in the *in time* group. This approach is applied onto the restricted sample with vacancies, which have more than 28 days of planned search duration to control also unobservable dimensions.[15]

The radius matching approach is unlike the entropy balancing approach not always able to match the mean of the covariates. The reported t-statistics in Table C.11 in the Appendix suggest that the means of the planned search duration, the low and high qualification requirement, full-time job, temporary job, firm size, low sales, financial constraints, and skilled labour shortage between the *in time* group and at least for one of the comparison groups *early*, *delayed* or *failed* are statistically significantly different. In the respective OLS-regressions we control again for the set of covariates used in the matching. Not surprisingly, we find that in some cases the coefficients with and without the control of the matching covariates differ significantly. However, the differences are small in size.

Despite the insufficient balancing of covariates due to radius matching, the results are qualitatively the same as with entropy balancing. This can be seen by comparing the respective Tables C.12 to C.17 in the Appendix with the respective Tables in section 3.5.

[15]We also used the standard propensity score and nearest neighbour matching estimator. The results are very similar.

**Table C.11:** Raw and weighted covariate means with radius matching weights

| Variable | Means *In Time* | Means *Early* | | Means *Delayed* | | Means[1] *In Time* | Means *Failed* | |
|---|---|---|---|---|---|---|---|---|
| | | Raw | RM | Raw | RM | | Raw | RM |
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) |
| Planned search duration (log) | 3.967 | 4.566 | 3.967 | 4.034 | 3.934 | 3.971 | 4.548 | 4.230 |
| | | | (0.035) | | (-5.196) | | | (27.244) |
| Low qualification required | 0.113 | 0.064 | 0.125 | 0.086 | 0.108 | 0.114 | 0.090 | 0.088 |
| | | | (3.746) | | (-1.236) | | | (-5.427) |
| High qualification required | 0.189 | 0.256 | 0.172 | 0.265 | 0.178 | 0.186 | 0.244 | 0.189 |
| | | | (-4.206) | | (-2.108) | | | (0.542) |
| Experience required | 0.445 | 0.468 | 0.441 | 0.493 | 0.451 | N.A. | N.A. | N.A. |
| | | | (-0.890) | | (1.028) | | | |
| Permanent position | 0.444 | 0.510 | 0.448 | 0.529 | 0.445 | N.A. | N.A. | N.A. |
| | | | (0.679) | | (0.186) | | | |
| Full time position | 0.782 | 0.789 | 0.785 | 0.852 | 0.798 | N.A. | N.A. | N.A. |
| | | | (0.620) | | (3.183) | | | |
| Seasonal work | 0.068 | 0.041 | 0.071 | 0.042 | 0.063 | N.A. | N.A. | N.A. |
| | | | (1.121) | | (-1.873) | | | |
| Temporary employment | 0.171 | 0.128 | 0.176 | 0.100 | 0.151 | N.A. | N.A. | N.A. |
| | | | (1.414) | | (-4.188) | | | |
| Log of firm size (employees) | 3.848 | 3.813 | 3.848 | 3.922 | 3.848 | 3.823 | 3.704 | 3.746 |
| | | | (-0.010) | | (-0.048) | | | (-3.344) |
| Financial distress | 0.071 | 0.063 | 0.065 | 0.070 | 0.068 | 0.070 | 0.091 | 0.057 |
| | | | (-2.382) | | (-1.008) | | | (-3.544) |
| Low sales | 0.153 | 0.130 | 0.148 | 0.168 | 0.145 | 0.153 | 0.191 | 0.141 |
| | | | (-1.300) | | (-1.644) | | | (-2.115) |
| Skilled labour shortage | 0.085 | 0.061 | 0.075 | 0.193 | 0.083 | 0.084 | 0.321 | 0.162 |
| | | | (-3.477) | | (-0.450) | | | (16.848) |
| *N* | 20,568 | 19,206 | | 9,522 | | 22,485 | 5,067 | |

t-statistics are reported in parentheses below respective values and refer to a two sided t-test against the unweighted mean of the *In Time* Group. Thereby, the t-statistics in row (3) and (5) refer to a t-test against the means of row (1), while the t-statistics in row (8) refer to the means in row (6). Raw means are unweighted means while the rows, marked with "RM" represent means calculated by using the means of a radius matching approach. Data: German Job Vacancy Survey 2005-2014.

[1] The difference between Means *In Time* in this row and the respective means from row (1) is due to a lack of data for failed vacancies for several years in contrast to vacancies, which were filled. However, fewer matching variables might also lead to a greater common support between treatment and control group. These two factors explain the difference in the number of observations for row (1) and row (6).

**Table C.12:** Problems in the recruitment process - Radius matching

| | Group *In Time* (Mean) | Difference between *In Time* and *Early* | *Delayed* | *Failed* |
|---|---|---|---|---|
| | (1) | (2) | (3) | (4) |
| Not enough suitable applicants | 0.1239 | -0.0563*** | 0.1740*** | 0.4296*** |
| (Standard Error) | | (0.0052) | (0.0071) | (0.0109) |
| Nr. of Obs. | 15,863 | 14,729 | 7,653 | 5,067 |
| Pay claim of applicants too high | 0.0522 | -0.0258*** | 0.0833*** | 0.2464*** |
| (Standard Error) | | (0.0028) | (0.0050) | (0.0087) |
| Nr. of Obs. | 20,568 | 19,206 | 9,522 | 5,067 |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with with weights, generated with the radius matching approach. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$.
Data for "Not enough suitable applicants": German Job Vacancy Survey 2009-2014. Data for "Pay claim of applicants too high": German Job Vacancy Survey 2010-2014.

**Table C.13:** Applicants and suitable applicants - Radius matching

| | Group *In Time* (Mean) (1) | Difference between *In Time* and *Early* (2) | *Delayed* (3) | *Failed* (4) |
|---|---|---|---|---|
| Number of applicants | 16.1985 | -2.9223*** | 1.5266** | -4.1774*** |
| (Standard Error) | | (0.5368) | (0.5393) | (0.7306) |
| Nr. of Obs. | 18,366 | 16,553 | 8,770 | 1,767 |
| Number of suitable applicants | 4.6477 | -0.8781*** | -0.0570 | N.A. |
| (Standard Error) | | (0.1290) | (0.1946) | () |
| Nr. of Obs. | 18,230 | 16,437 | 8,652 | |
| Fraction of suitable applicants | 0.5035 | 0.0819*** | -0.0812*** | N.A. |
| (Standard Error) | | (0.0059) | (0.0050) | () |
| Nr. of Obs. | 17,935 | 16,075 | 8,564 | |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with with weights, generated with the radius matching approach. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$.
Data: German Job Vacancy Survey 2005-2014.

**Table C.14:** Search channels - Radius matching

| | Group *In Time* (Mean) (1) | Difference to *Early* (2) | *Delayed* (3) | *Failed* (4) |
|---|---|---|---|---|
| Number of search channels activated | 1.8412 | -0.1984*** | 0.3458*** | 0.5438*** |
| (Standard Error) | | (0.0148) | (0.0156) | (0.0533) |
| Nr. of Obs. | 20,568 | 19,206 | 9,522 | 794 |
| Use of search channel: Classic | 0.5292 | -0.1033*** | 0.1133*** | -0.1538*** |
| (Standard Error) | | (0.0074) | (0.0072) | (0.0074) |
| Nr. of Obs. | 20,568 | 19,206 | 9,522 | 5,067 |
| Use of search channel: Internal | 0.2416 | -0.0039 | 0.0612*** | 0.0443*** |
| (Standard Error) | | (0.0067) | (0.0066) | (0.0092) |
| Nr. of Obs. | 20,568 | 19,206 | 9,522 | 4,082 |
| Use of search channel: Speculative | 0.2660 | -0.0282*** | 0.0377*** | 0.0180 |
| (Standard Error) | | (0.0069) | (0.0070) | (0.0243) |
| Nr. of Obs. | 20,568 | 19,206 | 9,522 | 794 |
| Use of search channel: Network | 0.4044 | 0.0738*** | 0.0135** | 0.0409 |
| (Standard Error) | | (0.0081) | (0.0076) | (0.0253) |
| Nr. of Obs. | 20,568 | 19,206 | 9,522 | 794 |
| Use of search channel: PEA | 0.3998 | -0.1367*** | 0.1201*** | 0.2040*** |
| (Standard Error) | | (0.0073) | (0.0077) | (0.0132) |
| Nr. of Obs. | 20,568 | 19,206 | 9,522 | 2,406 |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with with weights, generated with the radius matching approach. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$.
Data: German Job Vacancy Survey 2005-2014.

**Table C.15:** Concessions related to worker characteristics - Radius matching

| | Group *In Time* (Mean) (1) | Difference between *In Time* and *Early* (2) | *Delayed* (3) | *Failed* (4) |
|---|---|---|---|---|
| Experience lower than required | 0.0956 | -0.0312*** | 0.0566*** | N.A. |
| (Standard Error) | | (0.0044) | (0.0053) | |
| Nr. of Obs. | 19,604 | 18,411 | 8,907 | |
| Qualification lower than required | 0.0758 | -0.0262*** | 0.0526*** | N.A. |
| (Standard Error) | | (0.0041) | (0.0051) | |
| Nr. of Obs. | 19,389 | 18,278 | 8,674 | |
| Hired previously unemployed | 0.3570 | -0.0839*** | 0.0257*** | N.A. |
| (Standard Error) | | (0.0072) | (0.0073) | |
| Nr. of Obs. | 20,568 | 19,206 | 9,522 | |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with with weights, generated with the radius matching approach. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$.
Data: German Job Vacancy Survey 2005-2014.

**Table C.16:** Concessions related to wages - Radius matching

| | Group *In Time* (Mean) (1) | Difference between *In Time* and *Early* (2) | *Delayed* (3) | *Failed* (4) |
|---|---|---|---|---|
| Wage bargaining | 0.3177 | -0.0100 | 0.0648*** | N.A. |
| (Standard Error) | | (0.0129) | (0.0125) | |
| Nr. of Obs. | 6,037 | 5,710 | 3,025 | |
| Paid more than intended | 0.0860 | -0.0016 | 0.0508*** | N.A. |
| (Standard Error) | | (0.0045) | (0.0049) | |
| Nr. of Obs. | 20,359 | 18,990 | 9,421 | |
| Hourly wage (Euro) | 13.1913 | 0.4166** | 0.4865*** | N.A. |
| (Standard Error) | | (0.1406) | (0.1423) | |
| Nr. of Obs. | 2,576 | 2,328 | 1,462 | |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with with weights, generated with the radius matching approach. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$.
Data for "Wage bargaining took place": German Job Vacancy Survey 2011-2013. Data for "Average hourly wage": German Job Vacancy Survey 2014. Data for "Paid more than intended": German Job Vacancy Survey 2005-2014.

**Table C.17:** Concessions related to wages - controlled for qualification and experience - Radius matching

| | Group *In Time* (Mean) (1) | Difference between *In Time* and *Early* (2) | *Delayed* (3) | *Failed* (4) |
|---|---|---|---|---|
| | | previously employed workers | | |
| Wage bargaining | 0.3242 | 0.0199 | 0.0660*** | N.A. |
| (Standard Error) | | (0.0159) | (0.0167) | |
| Nr. of Obs. | 3,621 | 4,303 | 1,660 | |
| Paid more than intended | 0.0810 | 0.0007 | 0.0545*** | N.A. |
| (Standard Error) | | (0.0054) | (0.0068) | |
| Nr. of Obs. | 11,085 | 13,079 | 4,840 | |
| Hourly wage (Euro) | 13.93 | 0.4139** | 0.3188 | N.A. |
| (Standard Error) | | (0.2053) | (0.2092) | |
| Nr. of Obs. | 1,558 | 1,724 | 792 | |
| | | previously unemployed workers | | |
| Wage bargaining | 0.2674 | -0.0542** | 0.0553** | N.A. |
| (Standard Error) | | (0.0253) | (0.0271) | |
| Nr. of Obs. | 1,440 | 733 | 588 | |
| Paid more than intended | 0.0542 | -0.0081 | 0.0438*** | N.A. |
| (Standard Error) | | (0.0071) | (0.0088) | |
| Nr. of Obs. | 5,660 | 3,243 | 1,995 | |
| Hourly wage (Euro) | 11.88 | 0.3047 | 0.7651*** | N.A. |
| (Standard Error) | | (0.2488) | (0.2738) | |
| Nr. of Obs. | 530 | 300 | 232 | |

Differences are the coefficients of the respective indicator variables for belonging to the group *Early*, *Delayed* or *Failed* in an OLS regression with with weights, generated with the radius matching approach. Covariates of firm and vacancy characteristics are not reported. Interaction dummies between region and year are not reported. 3 digit occupational dummies (based on ISCO-88 classification) are not reported. * indicates $p < 0.1$,** indicates $p < 0.05$, *** indicates $p < 0.01$. Data for "Wage bargaining took place": German Job Vacancy Survey 2011-2013. Data for "Average hourly wage": German Job Vacancy Survey 2014. Data for "Paid more than intended": German Job Vacancy Survey 2005-2014.

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