Essays on Wages and Minimum Wages in Frictional Labor Markets

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Introduction

Wages are one of the most fundamental elements of the labor market, acting as both a primary source of income for workers and an essential cost factor for firms. As the price of labor, wages influence workers' decisions to offer their working power to the market and firms' decisions to hire. Hence, wages play a crucial role in coordinating supply and demand. Based on classical market theory, excess labor supply leads to lower wages, while excess demand drives wages upward, allowing both sides of the market to converge. In this process, the flexibility of wages determines how well imbalances in supply and demand are mitigated by wage adjustments. In a perfectly competitive market, wages would adjust freely to ensure labor market equilibrium at all times.

This perfectly efficient solution, however, can only emerge in an idealized setting where no coordination or transaction barriers hinder the allocation of resources in the labor market. In reality, markets are rarely perfect. A variety of obstacles, known as frictions, impede markets from fully achieving their welfare-maximizing potential. Broadly speaking, frictions encompass all forms of imperfections that reduce the efficiency of the labor market. The entirety of market imperfections in an economy evokes the "natural rate of unemployment" (Friedman, 1968), which represents the structural component of unemployment. Ignoring these frictions, traditional models fail to deliver a realistic representation of actual labor market phenomena such as persistent unemployment and deep impacts of recessions (Hall, 1999). By explicitly modeling these frictions, modern economic theories offer a more nuanced understanding of the complex interplay between labor supply and demand and better explain a range of real-world phenomena.

For example, both workers and firms typically face incomplete information on the labor market. Workers are unaware of the wage offers from all potential employers, which undermines the "law of one price" and leads to wage dispersion across similar jobs (Mortensen, 2003). Furthermore, workers face limited mobility regarding their work location and cannot easily transition to a different occupation, hence, they are often tied to their local labor market. This immobility can result in a mismatch between unemployed workers and job vacancies, as the skills and qualifications of job seekers may not align with the requirements of available positions (Shimer, 2007).

Another fundamental characteristic of frictional labor markets regards the search process for jobs and workers. With incomplete information, workers must invest considerable time and resources to explore job opportunities. Similarly, firms face costs when advertising vacancies, screening applications, and evaluating candidates to improve their chances of finding a good match. These challenges make filling vacancies a time- and cost-intensive process. Such obstacles, which hinder matching of labor supply and demand, are referred to as search-and-matching frictions. These frictions are explicitly modeled in the seminal works of Diamond (1982), Mortensen (1982), and Pissarides (1985), which form the foundation of modern search-and-matching theory.

This theoretical framework provides a paradigm shift in understanding equilibrium states, moving beyond the traditional assumption of complete alignment between supply and demand. Instead, it identifies "equilibrium unemployment" and the simultaneous coexistence of vacancies as defining features of frictional labor markets (Pissarides, 2000).[1]

To understand how wages are determined in the context of a frictional labor market and grasp the relationship between wages and frictions, it seems worth taking a step

[1]The pivotal role of search-and-matching theory for the understanding of frictional labor markets is underscored by the awarding of the 2010 Nobel Prize in Economic Sciences to its pioneers, Peter Diamond, Dale Mortensen, and Christopher Pissarides.

back and first examine the basic determinants of job search and vacancy creation using the baseline search-and-matching framework (Mortensen and Pissarides, 1994).[2]

From the employer's perspective, unfilled job vacancies reduce production capacity. Firms create vacancies when they expect that hiring a worker will generate returns that exceed the costs of employment. The expected magnitude of these costs depends on two factors: wages and the expenses associated with the search and recruitment process. These, in turn, are influenced by the ratio of existing vacancies to job seekers, a measure referred to as labor market tightness.[3] In a slack labor market, characterized by a large pool of job seekers and relatively few vacancies, employers can expect a smooth recruitment process with access to a wide range of potential candidates. Conversely, for job seekers, labor market tightness has the opposite effect. In times of low unemployment and a high number of vacancies, job seekers have a plethora of potential employment opportunities, making the search for a better job more promising. However, for employers, recruitment becomes more challenging as numerous vacancies compete for a limited number of job seekers. Thus, labor market tightness represents a measure of worker scarcity, describing a hiring friction that affects job creation and other dynamics of the labor market.

When a job seeker and an open position match, they must agree upon a wage that both the worker is willing to accept and the employer is willing to pay. This wage must leave both parties better off compared to continuing their respective searches. Labor market tightness plays a central role in this process, as the availability of alternative vacancies influences the worker's outside options, while the pool of alternative candidates affects the firm's valuation of keeping a job vacant. This determines how search

[2]Wage formation in frictional labor markets has been formalized in the literature through various approaches, including wage bargaining (Mortensen and Pissarides, 1994), wage posting (Burdett and Mortensen, 1998), and efficiency wages (Yellen, 1984). While these approaches differ in their assumptions and implications, they share the central premise that wages are influenced by prevailing market conditions and labor market frictions, as workers and/or firms take these frictions into account when offering or negotiating wages.

[3]In the context of search-and-matching theory with on-the-job search, the pool of job seekers includes not only unemployed individuals but also employed workers seeking better job opportunities.

frictions are shared between employers and workers. High tightness implies greater difficulty for firms in finding workers, making them more willing to pay higher wages for a successful match. The same principle applies inversely for workers.[4] Thus, high labor market tightness can lead to rising wages.

However, the relationship between labor market frictions and wages is not unidirectional, as demonstrated by completing the mechanisms within the search-and-matching framework toward equilibrium. A higher wage, as described earlier, increases the costs of employment and thereby reduces vacancy creation. This, in turn, lowers labor market tightness, ceteris paribus.[5]

The interactions between frictions and wages ultimately bring the labor market to an equilibrium state, where the number of matches being created equals the number of matches ending, keeping the ratio of job vacancies to unemployed individuals constant. However, their levels may differ, which explains the existence of equilibrium unemployment and the coexistence of vacancies in a frictional labor market.[6]

Labor market institutions, such as minimum wage policies, can restrict the flexibility of wage adjustments, thereby influencing their interplay with labor market frictions. A binding minimum wage exogenously raises wages in the affected jobs, introducing downward wage rigidity above the equilibrium. From the employer's perspective, this results in higher employment costs, which can reduce labor demand and the creation of job vacancies. This, in turn, creates a dampening feedback effect on labor market tightness, as shown by Bossler and Popp (2024). Conversely, when market power allows employers to set wages below equilibrium, minimum wages can elevate wages closer to

[4]The so-called Wage Curve describes the positive relationship between labor market tightness and wages. The curve slopes upward, indicating higher wages in tighter labor markets. In addition to labor market tightness, wage determination depends on productivity, the worker's outside options, and their bargaining power.

[5]The so-called Job-Creation Curve reflects the firm's incentive to open vacancies based on labor market tightness. The curve slopes downward because firms post fewer vacancies as labor market tightness rises. In addition to wage levels, the job-creation condition depends on the firm's costs of posting vacancies, the probability of filling a vacancy, productivity, and the worker's bargaining power.

[6]The so-called Beveridge Curve illustrates the combinations of vacancies and unemployment at which the inflows into unemployment equal the outflows, ensuring that the labor market is in equilibrium.

competitive levels, increasing labor demand, reducing frictions, and improving labor market efficiency (Card and Krueger, 1994). Ultimately, the impact of a minimum wage crucially depends on its level and prevailing market conditions across multiple dimensions, such as employer market power, productivity, and the extent of search frictions, as shown by Blömer et al. (2024).

The interplay between wages and labor market frictions is inherently bidirectional and dynamic, with each influencing the other in manifold ways. Wage changes, whether driven by policy interventions or market forces, can simultaneously mitigate or amplify labor market frictions, depending on the specific characteristics and conditions of the labor market. Likewise, shifts in labor market frictions can feed back into wage dynamics, creating a complex cycle of influence. This bidirectional relationship has important implications for policy design and the evaluation of labor market interventions. For example, institutions like minimum wages can have both direct and indirect effects on labor market equilibria and matching efficiency. A nuanced understanding of these interdependencies is essential for minimizing unintended consequences and ensuring that policies are both effective in achieving their objectives and efficient in their implementation.

This thesis aims to shed light on the nexus between wages and labor market frictions from three different perspectives, with each chapter analyzing a distinct causal relationship. The first chapter proposes a machine learning-based approach to analyze long-run wage effects of the German minimum wage introduction in a frictional labor market where the group of affected workers develops dynamically over time. The second chapter analyzes how the minimum wage introduction affected vacancies, revealing frictional changes in the efficiency of matching processes. Finally, chapter three inverts the perspective, analyzing how hiring frictions imposed by high labor market tightness affect the development of wages.

Table 1: Overview of Dissertation Chapters

Dimensions / **Chapter**	**Chapter 1** Long-Run Minimum Wage Evaluation Using Machine-Learning-Based Treatment Bites	**Chapter 2** Fueling Frictions: Minimum Wage Effects on Job Vacancies	**Chapter 3** Scarce Workers, High Wages?
Co-Author(s)	Mario Bossler	–	Mario Bossler Martin Popp
Research Area	Wages and Frictions in the German Labor Market		
Analysis Period	2010–2020	2013–2019	2012–2022
Thematic Focus	Labor Market Institution		Labor Market Condition
Policy Context	Minimum Wage		Worker Shortage
Methodological Scope	Predictive Modelling Causal Analysis	Causal Analysis	Causal Analysis
Causal Relationship	Minimum Wage → Wages	Minimum Wage → Vacancies	Labor Market Tightness → Wages
Main Outcomes	Bite Gap, Bite Wage	Opening Vacancies Vacancy Duration Stock of Vacancies	Wage
Identifcation Strategy	Quasi-Experiment		
Empirical Method	L1-Regularization (LASSO) Difference-in-Differences	Difference-in-Differences	Multi-Dimensional Fixed Effects Instrumental Variables Estimation
Unit of Observation	Individuals	Occupations	Individuals
Primary Data	IEB	IEB Registered Vacancies	IEB Registered Vacancies Registered Job Seekers
Supplementary Data	German Social Accident Insurance	German Social Accident Insurance JVS	JVS

Notes: IEB = Integrated Employment Biographies; JVS = Job Vacancy Survey

Synopsis

The three chapters are thematically closely related but differ in various aspects, including the subject of analysis, the empirical strategy, and the precursory data transformation. Table 1 provides an overview of the key characteristics of the three studies, highlighting their similarities and differences. What unites all chapters is their focus on wages in the context of a frictional labor market and the goal of identifying causal effects in this context.

Of the three studies, one is single-authored, while the other two are collaborative works with co-authors. All three studies share an overarching research field, focusing on the German labor market from the early 2010s to around 2020. This period is particularly interesting, as it saw the introduction of a nationwide statutory minimum wage in Germany in 2015 – arguably the most debated labor market legislation since the Hartz reforms of 2003–2005 – affecting approximately 10 to 14 percent of the entire workforce (Caliendo et al., 2019). Additionally, these years were characterized by robust economic growth and the largest job growth since the 1950s, a phenomenon referred to as the "German Labor Market Miracle" (Burda and Seele, 2020). The Hartz reforms contributed to a significant reduction in structural unemployment, while demographic shifts further reduced the number of unemployed workers (Schneider and Rinne, 2019). Unemployment declined steadily after peaking at 13 percent in 2005, but as labor demand remained high, filling job vacancies became increasingly challenging. Since 2010, the number of vacancies per job seeker has tripled, reaching a peak in 2022 (see Chapter 3). Hence, the German labor market has been characterized by both substantial institutional changes and profound shifts in prevailing market conditions, making it a particularly compelling area of study.

Chapters 1 and 2 are grounded in the institutional context of the German minimum wage introduction in 2015. The first chapter examines long-term evaluation of the minimum wage within a dynamic, frictional labor market, accounting for wage dynamics unrelated to the minimum wage when estimating its effects. In contrast, Chapter 2 evaluates primarily the short-term effects of the minimum wage legislation, with a particular emphasis on vacancies as an indicator of search frictions. Chapter 3 shifts the focus away from the institutional context, instead examining the impact of changing market conditions. Specifically, the study investigates how the increasing labor shortage in Germany has affected wage growth.

Chapter 1 primarily focuses on a prediction model based on machine learning, whose predictions are then applied to a causal analysis of the wage effects from the Germany minimum wage introduction. The prediction model estimates individual minimum wage exposure based on observable characteristics, using a regression model for the intensive margin (bite gap) and a classification model for the extensive margin (bite).[7] This approach makes a methodological contribution to minimum wage research by enabling the annual prediction of minimum wage exposure. It allows for the flexible classification of individuals over time, distinguishing between those exposed or not exposed, or those more or less strongly affected by the minimum wage.

Existing ex-post analyses of the German minimum wage have primarily focused on the short-term effects of its introduction and the incremental increases in recent years on various outcomes (see Dütsch et al., 2024 for an overview). This focus can be partly attributed to the fact that, since its introduction in 2015 and the subsequent increases, only a limited amount of time has passed. However, it may also have methodological reasons. In common research designs, minimum wage exposure is typically determined based on pre-treatment wages, with groups of affected and non-affected (or less affected) units kept constant over time. This assumes that units affected at the time of the treatment remain equally affected later, an assumption that seems plausible in the short term. In the long run, particularly within the context of a frictional labor market, workers may experience wage dynamics even in the absence of a minimum wage. These

[7] The minimum wage was introduced at a level of €8.50. Both measures use wages to define an individual's exposure to the minimum wage. The "bite" divides individuals earning below €8.50 into the treatment group, while those earning at or above this threshold are classified into the control group. The "bite gap" provides a continuous measure of minimum wage exposure, representing the wage differential relative to the minimum wage.

dynamics are especially impactful for low-wage workers (Bossler and Schank, 2023; Naguib, 2022), who constitute the primary group of interest in minimum wage analyses. By dynamically predicting the bite, our approach can capture such wage dynamics.

Our approach primarily provides advantages for individual-level analyses. In analyses at an aggregated level, such as regions (Ahlfeldt et al., 2018; Bossler and Schank, 2023; Caliendo et al., 2018; Caliendo et al., 2022; Dolton et al., 2012; Dolton et al., 2015; Dustmann et al., 2022; Garloff, 2019; Schmitz, 2019; Stewart, 2002), occupations (Friedrich, 2020), or firms (Bossler, 2017; Bossler and Gerner, 2020; Machin et al., 2003; Riley and Bondibene, 2017), variations in the bite over time may be mitigated by individual upward and downward dynamics of workers within the aggregation levels. However, if wage dynamics independent of the minimum wage evolve differently across regions, occupations, or firms, bite variation over time could become a relevant issue at these levels as well.

We compare the wage effects of the minimum wage introduction in a causal analysis using our predicted bite with conventional effect estimates. The results show that our approach leads to lower and relatively constant wage effects over time. We argue that conventional estimates might overestimate the genuine effect of the minimum wage by not accounting for unrelated wage dynamics. These findings align with those of Derenoncourt et al. (2021), who demonstrate that the proximity of the period in which the minimum wage exposure is measured to the treatment period significantly influences the effect magnitudes.

Chapter 2 also aims to identify the causal effects of the minimum wage introduction in Germany, focusing on a channel whose relevance is rooted in the frictional nature of the labor market, namely persistent unsatisfied labor demand, i.e., vacancies. A minimum wage could also trigger adjustment responses from employers and employees in a frictionless market that lead to unsatisfied labor demand, but open positions could be filled immediately, and the labor market would return to equilibrium without vacancies. However, search and matching frictions hinder these adjustment processes, leading to persistent coexistence of unemployment and vacancies, making these to equilibrium variables (Pissarides, 2000). This channel is particularly relevant in light of previous findings on small effects of minimum wages on realized employment (e.g., Allegretto et al., 2011; Card and Krueger, 1994; Cengiz et al., 2019; Dube et al., 2010; Dube et al., 2016; Harasztosi and Lindner, 2019), as it reveals minimum wage effects that can emerge independently of effects on equilibrium employment.

The study examines the effects on the number of opening vacancies, which can result from changes in labor demand or the magnitude of replacement hires. The existing minimum wage literature shows that firms adapt to minimum wage policies by adjusting the margin of hires rather than layoffs (Bossler and Gerner, 2020; Gopalan et al., 2021; Jardim et al., 2018). Other studies provide evidence of increased worker reallocation between different types of firms (Dustmann et al., 2022) and a reduction in labor turnover (Dube et al., 2016). In summary, these findings suggest that minimum wages can have differing effects on hiring processes and, consequently, on the number of opening vacancies. This channel has been explored in the empirical minimum wage literature by Kudlyak et al. (2022) using state-specific minimum wage variation in the U.S., but has not yet been studied for national minimum wages. The results of Chapter 2 suggest that, with regard to all vacancies, the introduction of the minimum wage did not lead to a statistically significant change in vacancy openings, which could be explained by the contrasting effects on various aspects of the hiring process observed in the literature. Additional analyses of worker transitions show slightly fewer transitions between employers, particularly when these involve a change in occupation, which is consistent with previous findings of reduced labor turnover.

In addition to the effect on the total number of vacancy openings, Chapter 2 also examines, for the first time, the impact of the German minimum wage introduction on the duration of successfully filled vacancies and the share of canceled vacancies. Both outcome variables reveal the difficulty in filling open positions and how the introduction of the minimum wage has changed the extent of search and matching frictions.

This effect channel is supported by evidence of increased hiring standards (Butschek, 2022; Clemens et al., 2021), decreased occupational mobility (Liu, 2022), and intensified applicant screening (Gürtzgen et al., 2016). Consistent with these findings, my study shows that vacancy durations increased by 5–6 percent on average and that the share of canceled vacancies rose by 4–9 percent in the years after the minimum wage introduction. Additionally, complementary analyses using survey data suggest that firms became less willing to compromise on candidates. The results indicate that difficulty in filling vacancies increased, primarily due to adjustments in hiring processes, which ultimately heightened search and matching frictions in minimum wage occupations.

Aside from the frictional effects of the minimum wage, the increasing scarcity of workers in the 2010s implies increased hiring frictions for firms, even without minimum wage employees. From the workers' perspective, the increased labor market tightness strengthened their bargaining position and provided a wider range of employment options. Existing international and German literature on the effects of scarce labor includes several findings of reduced employment growth (Stevens, 2007; Beaudry et al., 2018; Bossler and Popp, 2024; Le Barbanchon et al., 2024). Furthermore, economic theory suggests that both in competitive labor markets and especially in frictional labor markets, positive wage effects may arise (e.g. Mortensen and Pissarides, 1994; Burdett and Mortensen, 1998). To explore this adjustment channel, which has been scarcely studied in the context of Germany, Chapter 3 examines the effects of high labor market tightness on wages.

Compared to Chapter 2, Chapter 3 shifts the perspective by using a specific form of labor market frictions as an explanatory variable to identify causal effects on equilibrium wages. Since labor market tightness is defined as the ratio of vacancies to job seekers, the explanatory variable is closely related the outcome variables in Chapter 2.

This reversed perspective raises the issues of reverse causality and endogeneity for two reasons. First, both external wage shocks (Chapter 2) and market-driven wage changes (Bassier et al., 2023) can affect the number of vacancies. Furthermore, workers adjust their job search behavior to wage changes, affecting the number of job seekers (Pissarides, 2009). Thus, both channels imply a feedback effect of wages on labor market tightness. Second, local shocks that simultaneously affect wages and labor market tightness would bias our.[8] To address these issues, we apply the Leave-One-Out instrumental variable strategy proposed by Azar et al. (2022), which instruments the labor market tightness of an occupational labor market region with the average log tightness across all other regions for the same occupation and year.

Consistent with recent studies in the U.S. (Autor et al., 2023) and Denmark (Hoeck, 2023), our findings show that labor market tightness has a statistically significant, yet economically moderate, positive effect on wages, explaining between 7.4 and 19.1 percent of real wage growth in Germany from 2012 to 2022. Additionally, we examine heterogeneous effects across several subgroups and find that newly hired workers, those in high-skill jobs, workers in Eastern Germany, and those in the service sector benefited particularly from wage growth due to labor market tightness. Furthermore, we demonstrate that wages in low-wage firms increased more substantially, contributing to a reduction in wage inequality in Germany, similar to the effects observed in the U.S. (Autor et al., 2023)

In all three studies, we apply empirical strategies designed for identifying causal effects in a quasi-experimental setting. Chapters 1 and 2 utilize a difference-in-differences design, while Chapter 3 employs a Mincer-type wage equation with multidimensional fixed effects and instrumental variable estimation.[9] In Chapter 1, the prediction model is based on the LASSO estimator, which selects relevant predictive variables in a data-driven manner based on their predictive power.[10] The regression models in Chapters 1

[8]This mainly concerns local productivity shocks affecting both wages and labor demand in specific labor markets, e.g. due to technological change making certain occupations obsolete.

[9]Technically, all three chapters rely on fixed effects estimation. The difference-in-differences design can be framed as a fixed effects model by including (1) treatment group fixed effects to control for time-invariant unobserved heterogeneity, (2) time fixed effects to account for common trends across all groups, and (3) an interaction term between the treatment indicator (bite) and the time indicator to estimate the treatment effect.

[10]The LASSO (Least Absolute Shrinkage and Selection Operator) method, proposed by Tibshirani (1996), performs variable selection to construct a sparse model that includes covariates with high

and 3 are fitted using individual-level data, while Chapter 2 uses aggregated data at the occupational level.

The empirical analyses in this dissertation primarily relied on large-scale administrative datasets from the Federal Employment Agency, which served as a crucial resource for all three studies. These datasets were carefully processed to address the specific research questions and methodologies of each study. Consequently, a significant portion of the work in this dissertation involved exploring, transforming, and processing extensive longitudinal datasets. In addition, all studies integrated data from auxiliary datasets to enrich the primary data and support complementary analyses.

The first primary data source in each study is the Integrated Employment Biographies (IEB) which collect administrative labor market data from social security records in Germany.[11] The dataset includes a variety of sociodemographic characteristics for full-time, part-time, and marginally employed workers, as well as their gross daily and monthly wages. The latter constitute the outcome variables of Chapters 1 and 3. However, Chapters 1 and 2 require hourly wages to measure minimum wage exposure, i.e., the bite. As the IEB does not contain information on hours worked, we supplement the IEB data with working hour records from the German Social Accident Insurance (Deutsche Gesetzliche Unfallversicherung, DGUV), which, for administrative reasons, are only available for the years 2010 to 2014. Chapter 2 measures minimum wage exposure based on new hire wages from the year before the minimum wage introduction and holds the bite constant for each occupation. This approach aligns with the conventional bite calculation and does not address the challenges highlighted in Chapter 1. However, since this analysis operates at the occupational level, individual wage dynamics unre-

predictive performance while excluding others. Regularization aims to improve predictive accuracy and interpretability.

[11]The IEB data is derived from various social security datasets and also includes individual information for periods of job search or benefit receipt. For the analyses in this dissertation, I primarily use information on employment periods, which are contained in the employment history module ("Beschäftigten-Historik", BeH).

lated to the minimum wage play a less consequential role compared to individual-level analyses.

The second primary data source for Chapters 2 and 3 consists of information on job vacancies reported to the Federal Employment Agency. These data are available as aggregate-level stock datasets in the Agency's Data Warehouse. In Chapter 3, we combined this data with aggregated information on job seekers to calculate labor market tightness in occupational labor markets. For Chapter 2, the vacancy data were directly processed from the disaggregated, unprocessed population of all registered vacancy spells, allowing for precise and tailored calculations of vacancy flows, stocks, and durations.

The IAB Job Vacancy Survey (JVS), a representative establishment survey, plays an essential role in the analyses presented in Chapters 2 and 3. For the second chapter, the dataset offers valuable insights into the application and hiring behaviors of both employees and employers, contributing to complementary analyses. In Chapter 3, the survey data is used to estimate the total number of job vacancies by extrapolating from reported vacancies, using the survey's reported notification share for open positions.

Executive Summary of Essays

This section provides short summaries of all three self-contained essays. The first essay is co-authored by Mario Bossler (affiliated with IAB, TH Nuremberg, IZA, and LASER). The second essay is single-authored. The third essay is joint work with Mario Bossler and Martin Popp (affiliated with IAB, IZA, and LASER). Each article has been submitted to academic journals in economics and has been reviewed or is currently under review.

Long-Run Minimum Wage Evaluation Using Machine-Learning-Based Treatment Bites

Insights into the effects of minimum wages are of significant interest to both policy makers and researchers, particularly because economic theory does not provide definitive conclusions. Instead, outcomes depend heavily on the underlying market conditions. In standard research designs for ex post analyses, units affected by minimum wage legislation are compared to those that are either unaffected or less impacted to identify the causal effects of the policy.

When different minimum wages exist within a market (e.g., a country or industrial sector), this quasi-experimental variation can be leveraged for analysis. In contrast, for uniform national minimum wages, such as those introduced in Germany in 2015 or the UK in 1999, a measure of exposure is required to capture variation in treatment intensity – commonly referred to as the bite. This approach was first implemented by Card (1992) and has since been widely applied in numerous international and German studies.[12]

The bite is typically determined on the basis of observed wages shortly before the introduction of the minimum wage and is held constant for the analysis units over time. Although this time-constant pre-treatment bite may accurately reflect actual minimum wage exposure in the short term, it poses challenges for estimating long-term effects. In a frictionless market with equilibrium wages, the effects of a minimum wage would manifest immediately and completely after its introduction. However, search and matching frictions, along with delayed adjustment responses by firms and workers, can dynamically influence who is affected by the minimum wage over time. Unlike a one-time treatment, such as a training course, a minimum wage continuously affects the labor market after its introduction. In the long run, the composition of affected (treatment group) and unaffected or less affected (control group) units can shift due to wage dynamics unrelated to the minimum wage. Recent studies support this assumption, showing rising wages at the lower end of the wage distribution (Bossler and Schank, 2023) and upward mobility of low-wage workers along the wage distribution (Naguib, 2022) in Germany. Using the conventional time-constant pre-treatment bite captures only the effects on

[12] See, for example, Dolton et al., 2012; Dolton et al., 2015; Machin et al., 2003; Riley and Bondibene, 2017; Stewart, 2002; Stewart, 2004 for studies in the UK, and Dütsch et al., 2024 for a summary of German studies.

individuals who were initially affected but fails to account for the full population of individuals affected at later points in time.

In Chapter 1, we develop a machine learning model to predict minimum wage exposure at the individual level, creating a time-variant bite measure. This approach accounts for dynamic selection and captures effects for individuals entering the labor market after conventional bite measurement. Our method aims to provide an alternative for long-term evaluations, thereby contributing to the existing empirical literature on minimum wages.[13]

The analysis is based on the administrative data on individual employment histories (BeH).[14] The BeH is a comprehensive administrative dataset covering all employees in Germany, recorded from compulsory employer reports to the German social security system. In addition to daily wage information, the dataset includes a range of socio-demographic variables and details on employment relationships. To calculate the minimum wage exposure from hourly wages, we merge data on working hours from the German Social Accident Insurance. Due to legislative reasons, these data are only available for the years 2010–2014.

To predict the minimum wage exposure, we first identify variables in the BeH dataset with the greatest predictive power using various LASSO regression models. We use the selected variables in a logistic and a linear regression model to estimate two distinct measures. The first measure indicates whether an individual is affected by the minimum wage introduction (bite), while the second captures the treatment intensity in terms of the wage difference to be closed by the minimum wage (bite gap). We use data from 2010 to 2014 to train the parameters of our prediction model and to evaluate the prediction performance using out-of-sample forecasts. In the second step, we apply

[13] Until now, only one other study, Cengiz et al. (2022), has used machine learning methods to predict the groups of workers most severely affected by minimum wage increases. Unlike our study, however, they employ a data-driven selection process to define the target group and identify treatment effects using state-level variation in minimum wages.

[14] The BeH is the component of the Integrated Employment Biographies (IEB), which contains data on employment periods.

the models to predict the minimum wage exposure for the period after the minimum wage introduction (2015–2020). Finally, we use the predicted bite measures to estimate causal wage effects of the minimum wage introduction using difference-in-differences models. We then compare these wage effects, based on the predicted time-varying bite measures, with estimated effects from conventional time-constant pre-treatment bites.

Our results from the validation period indicate that the LASSO-based predictions of the bite gap produce lower prediction errors compared to the conventional time-constant pre-treatment measure from the second post-treatment year onward. This highlights that, particularly in the long run, wage dynamics independent of the minimum wage affects the composition of the treatment and control groups. In the difference-in-differences estimation of wage effects for the 2015–2020 period, using the predicted bite, we find statistically significant positive effects, in line with existing literature. However, compared to conventional estimates, these effects are smaller in magnitude and remain relatively constant over time. We conclude that minimum wage effects estimated using the time-constant pre-treatment bite measures may be upward biased for two reasons. First, conventional methods result in an increasingly selective treatment group over time, as individuals exiting the labor market drop out, leading to a positive selection of more stable employment relationships. In addition, these methods cannot include labor market entrants. Second, the literature documents wage growth at the lower end of the wage distribution even before the introduction of the minimum wage (Bossler and Schank, 2023), indicating that low-wage workers experience wage increases independently of the minimum wage policy. Hence, the treatment effects on the initially affected minimum wage workers may increase over time, while the genuine minimum wage effect, as suggested from our LASSO-predicted effect estimates, remains constant over time.

Our approach has some limitations, particularly concerning the selection of predictor variables. On the one hand, their predictive power may change over time, making them less effective after the minimum wage introduction compared to the validation period. On the other hand, it is challenging to identify variables that are not themselves endogenously affected by the minimum wage, requiring us to make assumptions about which variables can be considered exogenous and thus suitable for prediction.

Fueling Frictions: Minimum Wage Effects on Job Vacancies

Chapter 2 of this thesis aims to contribute to the empirical minimum wage literature by investigating the causal effects of Germany's statutory minimum wage introduction on vacancies, an adjustment margin beyond equilibrium employment that has received limited attention in existing research. Various international and German studies find employment effects close to zero or at least considerably smaller than what competitive labor market theory would imply.[15] However, minor effects on realized employment do not rule out adjustments in labor supply and demand that influence the matching process itself while not necessarily affecting its equilibrium outcomes. Recent empirical studies provide evidence for reduced hires (Bossler and Gerner, 2020; Gopalan et al., 2021; Jardim et al., 2018), stricter hiring standards (Butschek, 2022; Clemens et al., 2021), worker reallocation (Dustmann et al., 2022), reduction in labor turnover (Dube et al., 2016) and decreased occupational mobility (Liu, 2022). Such responses can not only increase labor market imbalances but also induce frictions, thereby affecting matching efficiency.

I employ difference-in-differences models to compare occupations with varying degrees of minimum wage exposure during the period from 2013 to 2019. This approach identifies the effects of the 2015 minimum wage introduction on several vacancy-related outcome variables. By analyzing vacancy flows (openings and closures), I examine whether the minimum wage introduction led to temporary or structural changes in unmet labor demand. In doing so, I distinguish between successfully filled and canceled vacancies to separate changes in labor demand (e.g., replacements or new hires) from

[15]Examples of minimal employment effects include international studies such as Allegretto et al. (2011), Card and Krueger (1994), Cengiz et al. (2019), Dube et al. (2010), Dube et al. (2016), and Harasztosi and Lindner (2019), and German studies such as Bossler and Gerner (2020), Caliendo et al. (2018), Dustmann et al. (2022), Garloff (2019), and Schmitz (2019).

effects on hiring difficulties. Even when recruitment efforts are not abandoned, hiring challenges can still impact successful placement processes. The duration of such processes is a critical factor in determining the cost- and time-intensity of matching workers and vacancies, thereby reflecting the degree of search frictions in the labor market. Using vacancy durations from completed, successful hiring processes, I analyze whether and how the minimum wage introduction affected the extent of frictions in recruitment. Finally, I examine the stock of vacancies, which results from the combined effects on vacancy openings and durations.

I utilize two different datasets of administrative data from the Federal Employment Agency (FEA) for my empirical analysis. First, I use a comprehensive dataset on the universe of vacancies reported to the FEA to precisely calculate vacancy flows, durations, and stocks. However, this data lacks wage information, making it hard to determine minimum wage exposure at the level of vacancies. To address this, I use IEB data combined with working-hour data from the German Social Accident Insurance to define minimum wage exposure from hiring wages. The IEB also provide the data for my supplementary analysis of worker flows. I aggregate the datasets by occupational groups and skill levels, resulting in 372 usable unique occupations.

The results indicate that the minimum wage introduction has not led to lower vacancy postings in affected occupations but increased the share of canceled vacancies by 4–9 percent on average. Furthermore, the duration of successfully filled vacancies increased by 5–6 percent, indicating a frictional effect on matching processes. The complementary analysis of worker transitions reveals slightly fewer transitions of employees between employers, especially if the transitions are associated with a change in occupation. The minimum wage has therefore increased the extent of search frictions, while simultaneously reducing labor turnover, which indicates higher matching efficiency.

This study uses unique data that enables the precise calculation of vacancy realted variables. However, data are limited to vacancies reported to the Federal Employment Agency which makes up about 41–49 percent of all vacancies during the analysis period

(Bossler et al., 2020). This limits the generalizability of the findings, as they may not fully capture total labor demand or reflect potential changes in firms' reporting behavior. Additionally, a structural break in occupational classifications constrained the pre-treatment period, making it difficult to fully disentangle anticipation effects from placebo effects in 2014.

Overall, the findings highlight how minimum wage policies can influence search frictions by analyzing the interplay between vacancy creation, duration, and worker transitions.

Scarce Workers, High Wages?

The minimum wage analyzed in the first two chapters represents an institution designed to increase wages at the lower end of the wage distribution, largely independent of market-driven processes. Outside of institutional frameworks, wages in competitive labor markets are determined by the equilibrium between labor supply and demand. In recent years, the ratio of job vacancies to unemployment – referred to as labor market tightness – has significantly increased in Germany, resulting in a growing number of vacancies relative to a shrinking pool of job seekers.

Chapter 3 examines how this heightened labor market tightness influences wages within the context of search frictions. Based on search-and-matching theory, an increase in labor market tightness is expected to exert upward pressure on wages (Mortensen and Pissarides, 1994). Moreover, the empirical literature highlights a negative relationship between labor market tightness and firms' labor demand (Bossler and Popp, 2024). However, there is limited empirical research specifically addressing the relationship between labor market tightness and wages.[16]

Using vacancy and unemployment data from the Federal Employment Agency's statistics, along with vacancy reporting rates from the IAB Job Vacancy Survey, we

[16] For analyses examining this relationship, see Autor et al. (2023), Brunow et al. (2022), and Hoeck (2023).

calculate labor market tightness as the ratio of vacancies to job seekers within local occupational labor markets. For the subsequent causal analysis, it is important to account for the potential endogeneity of this measure: wages can influence local labor market tightness, and local macroeconomic shocks may simultaneously affect both wages and labor market tightness. To address this issue and ensure exogeneity of the explanatory variable, we instumnet labor market tightness in a given region using the average tightness in the same occupation across all other regions (a Leave-One-Out instrument as proposed by Azar et al., 2022). We estimate the effect on wages using fixed-effects models by comparing labor markets with varying degrees of tightness over the period from 2012 to 2022. Additionally, we explore effect heterogeneities across various subgroups.

Our baseline IV regression with the full set of fixed effects and controls shows a significantly positive elasticity of 0.011, implying that an increase in labor market tightness by 100 log points raises the daily gross wages of regular full-time workers on average by 1.1 percent. We acknowledge that our baseline IV estimate is upward-biased in the presence of occupation specific productivity shocks at the national level, which our leave-one-out instrument does not protect against. Therefore, we use several proxies to control for these shocks with varying rigor. Under the most rigorous proxy (i.e., when conditioning on the number of vacancies in an occupation), the elasticity turns out lower but remains significantly positive with a value of 0.004, which plausibly forms a lower bound of the causal effect of log labor market tightness on log wages. Although the upper bound of 0.011 exceeds the lower bound by factor 2.6, both values imply positive but limited wage gains from the rising tightness. In light of our effect interval, the increase in tightness can explain between 7.4 and 19.1 percent of the rise in real wages in the German labor market between 2012 and 2022.

A large variety of robustness checks confirm our main effect at the upper-bound wage elasticity of 0.011. Further analyses of heterogeneous effects for subgroups show that the elasticities are comparatively higher for newly hired workers, specialists, experts, high-skilled workers, and workers in Eastern Germany and in the service sector.

Moreover, an additional analysis by wage deciles highlights that workers in the low-wage segment particularly benefit from rising labor market tightness. In light of this pattern, we show that the rising tightness has contributed to the observed decline in wage inequality over the last decade. Moreover, final analyses of firm-level wage-setting indicate that most of our effects stem from low-paying firms raising their overall wage level in response to higher tightness across the set of employed occupations.

Our findings imply that the limited impact of labor market tightness reflects a flat wage-setting curve, with our upper- and lower-bound log-log elasticities translating to linear coefficients between 0.013 and 0.032. These values may guide researchers in calibrating search-and-matching models. Furthermore, it is worth exploring whether the pay increases were sufficient to overcome firms' hiring frictions that usually arise in tight labor markets (Le Barbanchon et al., 2024). Building on the same data, Bossler and Popp (2024) find that, holding all other things equal, the doubling in tightness reduced firms' employment growth on average by 5 percent between 2012 and 2019. When abstracting that our time horizon is three years longer, our estimated wage increases between 0.6 and 1.5 percent (along with potential improvements in non-monetary job amenities) in response to higher tightness were seemingly not strong enough to maintain firms' employment growth. Finally, no signs of non-linearities in tightness can provide tentative indications that a further tightening of the German labor market due to intensifying demographic decline would lead to tangible but limited wage increases that further reduce wage inequality at the bottom end of the wage distribution.

Chapter 1

Long-Run Minimum Wage Evaluation Using Machine-Learning-Based Treatment Bites

Abstract*

The empirical evaluation of national minimum wages, as proposed by Card (1992), relies on a bite measure that captures treatment variation, typically defined using wage information from the last period before the intervention. Bite-dependent estimates face the problem of dynamic selection. That is, it only identifies effects on initially treated workers, but the treatment bite may change over time. We apply machine learning methods to predict the contemporaneous bite of the German minimum wage. This allows us to estimate the impact on contemporaneously treated workers instead of initially treated workers. In a validation period, LASSO-predicted bite measures show significant improvements over conventional time-constant measures. When estimating contemporaneous wage effects of the German minimum wage introduction, wage increases are positive but smaller than conventional estimates.

JEL Classification: J31, J38, C49, C21

Keywords: minimum wage, evaluation, dynamic selection, machine learning, LASSO

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1.1 Introduction

The empirical analysis of minimum wages in ex-post evaluations is highly relevant for policy-making, especially since the economic theory is not conclusive concerning the effects of a minimum wage. Empirical evaluations are particularly challenging in the case of uniform nationwide minimum wages, such as in the U.K. or Germany, since there is no quasi-experimental variation in the minimum wage level within countries. In these cases, evaluations typically rely on a bite measure that captures variation in the treatment intensity of the minimum wage. This approach was first applied by Card (1992) in an evaluation of minimum wages across federal states in the U.S. The bite captures the extent to which the minimum wage policy treats units of observations, and it is typically measured by the wage in the period before the exogenous policy intervention comes into force.[17] When comparing differentially treated units of observation (by utilizing their bite) in a difference-in-differences model, the estimation captures the treatment effect of the minimum wage policy.

The use of treatment bites in the empirical evaluation of minimum wages has become quite prominent since national minimum wages were introduced in the U.K. in 1999 and in Germany in 2015. For Germany, various studies summarized in Dütsch et al. (2024) use treatment bites applied to different levels of variation. Most evaluation studies compare differentially affected regions using a regional bite (Ahlfeldt et al., 2018; Bossler and Schank, 2023; Caliendo et al., 2018; Caliendo et al., 2022; Dustmann et al., 2022; Garloff, 2019; Schmitz, 2019). However, alternatives are presented by Friedrich (2020), who compares differently treated occupations; Bossler (2017) and Bossler and Gerner (2020), who compare affected and unaffected firms; and Dustmann et al. (2022), who compare affected and unaffected employees. Similarly, in the U.K., prominent evaluation studies identify the effects of the minimum wage by exploiting bite-dependent treatment variation at the individual level (Stewart, 2004), firm level (Machin et al., 2003; Riley and Bondibene, 2017), and regional level (Stewart, 2002; Dolton et al., 2012; Dolton et al., 2015).

[17] A simple worker-level binary bite is a dummy variable that captures whether or not an individual is paid a wage below the forthcoming minimum wage before it comes into force. Alternatively, the bite may capture the treatment intensity, referred to as the bite gap, representing the wage difference to be closed by the minimum wage.

In a frictionless market with an equilibrium wage, minimum wage legislation would elevate wages that fall short of the wage floor, with individuals with a larger bite gap receiving a relatively larger wage increase. However, in the presence of frictions, the labor market is dynamic, i.e., workers do not simply receive a homogeneous equilibrium wage but experience shocks that might temporarily make them minimum wage workers. Similarly, workers may receive wage gains irrespective of the minimum wage. Consequently, workers with a high minimum wage bite today might no longer be affected by the minimum wage a few years later.

Most bite-dependent evaluation studies focus on estimating short-run effects.[18] The lack of long-run evaluations is most likely due to the problem of dynamic selection, which comes into play when analyzing minimum wage effects over several years. Unlike the difference-in-differences-based evaluation of a one-time treatment, such as a training course, a minimum wage is introduced at one point in time but affects the labor market continuously. Hence, the group of treated individuals is not fixed and is likely to change. If treated individuals would have experienced positive wage growth even without the minimum wage introduction, they should no longer be assigned the same bite level. Conversely, some individuals may experience wage declines, making them affected by the minimum wage. Thus, using a time-constant pre-treatment bite, as is conventionally done, captures only the effect on initially treated individuals and fails to account for the full population of contemporaneously treated individuals.

In this study, we use machine learning to set up a prediction model based on observable characteristics to predict the bite of the German minimum wage introduction on the worker level. This allows us to obtain a more accurate and time-variant bite, capturing dynamic changes over time.[19] We use LASSO regression methods to select predictor variables based on

[18] Exceptions are presented in Dolton et al. (2012) and Dolton et al. (2015), who evaluate the U.K. minimum wage over a longer period, and in Caliendo et al. (2023), who present an evaluation of long-run employment effects of the German minimum wage. Both studies address the problem of dynamic changes of the bite by empirical updates of the bite measure, thereby relying on precise survey data on the contemporaneously affected workforce.

[19] We predict the bite of the German minimum wage, which was introduced at the level of €8.50 per working hour in 2015. Therefore, we disregard the increases in the minimum wage in 2017, 2019, and 2020. However, these increases did not exceed the overall wage growth (see Figure 1.F1 in the appendix). Hence, we do not expect any fundamental changes from these subsequent minimum wage increases.

their predictive power. We train and validate the bite prediction model for two distinctive bite measures (bite gap and incidence bite) in the period before the introduction of the minimum wage (2010-2014). First, we compare the predictions of our models to conventional time-constant pre-treatment (TCPT) bites based on a mean squared error criterion. Second, we use our models to predict the bite over the post-treatment period 2015-2020 and estimate the wage effects of the minimum wage introduction in Germany from the predicted bite measure. Finally, we compare the wage effects of the predicted bite with those of the TCPT bite. Bite prediction allows us to account for dynamic changes in the composition of the treatment and control groups over time. Furthermore, it enables us to include labor market entrants, thereby capturing the effects on workers who were not part of the labor force when the bite is measured in conventional analyses.

Our results show that the LASSO-based prediction of the bite can improve the conventional time-constant pre-treatment bite from the second post-treatment year onward. Hence, short-run minimum wage evaluations are not improved using LASSO-based bite prediction. In the longer run, however, dynamic selection between the treatment and control groups becomes more severe, and thus, the advantages of bite prediction come into play. Our estimation of the wage effects of the minimum wage introduction in Germany reveals significant wage increases when the new LASSO-based treatment bite is used. However, the effect size is considerably smaller than the one estimated with the conventional pre-treatment bite, suggesting an upward bias of wage effects in conventional estimations.

The remainder of this study is structured as follows. Section 1.2 describes the problem of dynamic selection in long-run bite-dependent minimum wage evaluations. Section 1.3 describes the LASSO estimation for the linear (regression) and logistic (classification) models, as well as the penalization scheme. This section also describes our data, the estimation strategy, and the variables we use to predict the bite. In Section 1.4, we present the results of the bite predictions for two distinctive measures (bite gap and incidence bite) and validate the predictions compared to our benchmark, the TCPT bite. In Section 1.5, we use the predicted bite to estimate the wage effects of the German minimum wage. Section 1.6 concludes the study.

1.2 Dynamic Selection in Bite-Dependent Minimum Wage Evaluations

Why is it relevant to predict the bite of the minimum wage, and what is the rationale behind applying machine learning methods in that context? We need a good measure of the contemporaneous bite to estimate the effect of the minimum wage on contemporaneously affected individuals. If we use the simple TCPT bite, which is measured before a minimum wage introduction, then the treatment bite identifies an effect on individuals who were initially affected by the minimum wage introduction. In the long run, a time-fixed measure of the initial bite still identifies the treatment effect on individuals who were initially affected by the minimum wage introduction. At the same time, the policy-relevant question concerns the effect on contemporaneously affected individuals but not on individuals who may no longer be affected by the minimum wage.

The larger the individual mobility along the wage distribution, the larger the difference between contemporaneously and initially affected individuals. Wage dynamics that are not caused by the minimum wage leads to wage growth for some individuals, even in the absence of the minimum wage, while others might face wage decreases. In a recent study, Naguib (2022) empirically shows for Germany that such wage dynamics are present in the wage distribution and that low-wage earners can move up into higher deciles of the wage distribution, even without a minimum wage, i.e., before the minimum wage introduction. Such wage changes lead to dynamic selection in and out of the treatment group, which is not captured by the TCPT bite. In consequence, effect estimates based on the difference-in-differences approach may overestimate minimum wage effects if initially low-paid workers experience faster wage growth than initially high-paid workers.[20] In a recent study, Derenoncourt et al. (2021) investigate how mean reversion in wages influences their effect estimates and shows that the proximity of the period in which the minimum wage bite is measured to the treatment period severely influences the effect magnitudes.

If the contemporaneous minimum wage bite is of interest, then we want to measure a new bite at each point in time. In principle, one could characterize individuals who receive exactly the minimum wage as being affected. However, utilizing individuals who receive exactly the

[20] At the macro level, differential wage dynamics with respect to the initial wage height has already been proposed in Ashenfelter and Card (1982).

minimum wage to define the bite bears some specific problems, as it neglects the rounding of actual wages or jobs that pay a premium on top of the minimum wage. Moreover, this approach raises the question of how to deal with inaccurate measurement since the exact measurement of hourly wages is typically challenging with observed data (Bossler and Westermeier, 2020). Finally, it is unclear how to address noncompliance, i.e., workers who should receive the minimum wage but do not. These workers could either be included in the bite because they are paid below minimum wage or excluded from the bite because they are likely to remain below the minimum wage, even when it is raised to a higher level.

Instead of defining those workers who are paid below or exactly at the minimum wage, we propose the prediction of the contemporaneous bite using a prediction model. Such a model can be useful for minimum wage evaluations if it outperforms the simple TCPT bite. Until now, only one other study has used machine learning methods to predict groups of workers most severely affected by minimum wage increases, which is Cengiz et al. (2022). Their approach is novel because existing studies mainly concentrate on specific groups most severely affected by the minimum wage and, hence, most prone to potential adverse effects (e.g., teens). In contrast, Cengiz et al. (2022) relax this assumption and thereby allow for minimum wage effects on other groups of workers, which are identified through data-driven selection. However, in contrast to the analyses presented in our study, Cengiz et al. (2022) apply machine learning to define the target group of interest but identify its treatment effects through state-level variation in minimum wages. In contrast, we use the machine learning approach to predict the bite and identify a treatment effect from this predicted bite variation.

Only a few other studies use some heuristics to predict the bite of a minimum wage, mainly because no accurate information on hourly wages is available. For example, Riley and Bondibene (2017) use firm-level average labor costs of firms in the U.K. – cross-validated with a small sample of workers for which they observe accurate wage information – to predict whether firms are affected by the national minimum wage introduction. Ahlfeldt et al. (2018) use an imputation model to infer working hours information from a secondary data source. Combined with administrative data on monthly wages, this approach allows them to calculate hourly wages, thereby calculating the bite of the German 2015 minimum wage introduction. Möller and König (2008) predict the individual-level bite for workers in the German main construction sector because information on hourly wages is missing and use a probit model to infer the bite from observable characteristics in administrative employment data. In line with Möller and König (2008), we also use a prediction model for the bite of the minimum wage. However, our goal is to predict the contemporaneous bite in future periods, while Möller and König (2008) aim to estimate the bite at the time of the minimum wage introduction due to a lack of information on working hours.

1.3 Empirical Strategy

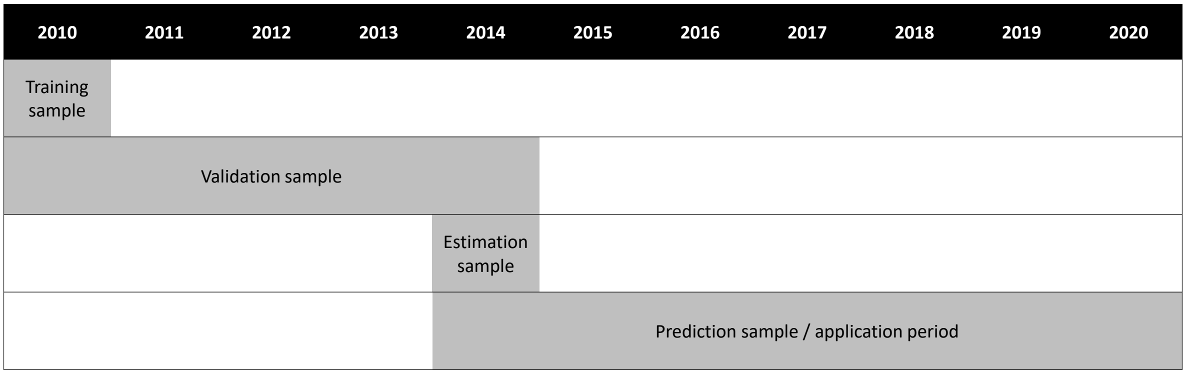
1.3.1 Estimation Plan

We aim to predict the contemporaneous bite of the German minimum wage introduction for the 2015-2020 period. We use this predicted bite in Section 1.5 to estimate the wage effects on contemporaneously affected workers. Hence, we refer to this period of interest as our *prediction period* or *application period*. Before predicting a bite in the period of interest, we validate our prediction model using an earlier validation period. For this validation, which is performed in Section 1.4, we use 2010 as the training period for our model (*training sample*) and the period 2011-2014 to evaluate our prediction method (*validation sample*). Moreover, 2011-2014 can be used for validation for two main reasons. (i) German administrative data used in our analysis only include information on hourly wages in the period 2010-2014 because it was mandatory for firms to report the working hours of their workers to the German Social Accident Insurance (Deutsche Gesetzliche Unfallversicherung, DGUV) during that time before such reporting was abolished for bureaucratic reasons. (ii) During 2011-2014, no national minimum wage was in place in Germany. Hence, this period provides a sample for which a bite can be predicted and evaluated against a contemporaneously calculated bite that is not itself endogenously affected by a minimum wage.

Figure 1.1 illustrates the analysis plan. First, we train a bite prediction model with data from 2010.[21] We validate this prediction in the pre-minimum wage period 2010-2014. After validating the model's performance in general, we train this model on data from the year just before the minimum wage was introduced to obtain predictions from the most recent

[21]In Appendix 1.A.2, we use 2011 as an alternative training year to check the robustness of our results.

Figure 1.1: Analysis Plan and Evaluation Schematic



year in which the bite is observed onward.[22] Based on this model, we predict the bite for the period 2015-2020, which is the period used to estimate the contemporaneous wage effects of the German minimum wage.

1.3.2 Implementation of the LASSO Estimator

We use a simple additive prediction model that can be formulated as follows:

$$bite_{i,t+r} = \beta_0 + x_{i,t}^{'}\beta_1 + \epsilon_{i,t+r} \tag{1.1}$$

The *bite* of individual i is predicted for some future period $t+r$. $bite_{i,t+r}$ is explained by predictors $x_{i,t}$, where β_1 is the coefficient vector of these explanatory variables in the prediction model. Since it is not *a priori* obvious which variables are good predictors of individuals' bite of the minimum wage, we select the explanatory variables in our model in a purely data-driven way based on their predictive power. To do so, we use regularized regression to select predictor variables and estimate the coefficients via OLS estimation after variable selection (post-LASSO-OLS).

The least absolute shrinkage and selection operator (LASSO) was first proposed by Tibshirani (1996) and belongs to a subclass of regularized regression methods. It works as a shrinkage estimator that consists of a linear least-squares regression model and a penalty

[22] In principle, it would be possible to conduct the prediction (to evaluate the minimum wage) based on a model trained on all years from 2010 to 2014. However, we want to apply the same procedure in the application and in the validation, where we only use a single training year.

term for the absolute sum of the coefficient values of all covariates (L1-regularization). In contrast to the ridge estimator (Hoerl and Kennard, 1970), which decreases the size of the coefficients to find an optimal solution to the minimization problem stated in Equation (1.2) (L2-regularization), the LASSO can set specific coefficients to zero, thereby excluding them from the model. By doing so, the LASSO aims to select a sparse solution that includes covariates with high predictive performance and excludes others. Hence, the LASSO can be used for model selection based on predictive power. The minimization problem of the LASSO estimator in a regression model setup with N observations and p explanatory variables can be formulated as follows:

$$\hat{\beta}^{lasso} = \underset{\beta_0,\beta}{\arg\min} \left\{ \sum_{i=1}^{N} (y_i - \beta_0 - \sum_{j=1}^{p} x_{ij}\beta_j)^2 + \lambda \sum_{j=1}^{p} |\beta_j| \right\} \tag{1.2}$$

While minimizing the first term in the equation corresponds to the OLS estimator, the second term imposes a penalty term for the total sum of coefficient values in the model. The absolute size of each coefficient determines its contribution to the penalty term of the equation.[23] Hence, the LASSO estimator aims to exclude those coefficients that add much noise but barely contribute to model fit. We use this model for continuous bite-gap prediction in Section 1.4.1.

For a classification problem, the model transforms to a logistic regression model (maximum likelihood) with the same penalization term attached as that used above. We apply this model to the classification problem in Section 1.4.2. To solve the model, we maximize the penalized log-likelihood function as follows:

$$\underset{\beta_0,\beta}{\arg\max} \left\{ \sum_{i=1}^{N} [y_i(\beta_0 + \sum_{j=1}^{p} x_{ij}\beta_j)) - \log(1 + e^{(\beta_0 + \sum_{j=1}^{p} x_{ij}\beta_j)})] - \lambda \sum_{j=1}^{p} |\beta_j| \right\} \tag{1.3}$$

The overall penalty level is determined by λ. If the penalty level is set to zero, the model is equal to a common OLS or logit estimator. We apply two different approaches for selecting the penalty parameter λ. First, we determine λ via 5-fold cross-validation and select the penalty level that minimizes the mean squared prediction error (MSE) over all folds, referred

[23]To ensure that each coefficient is treated equally concerning the scaling of the respective x-variables, all x-variables are mean standardized.

to as the CV-LASSO method.[24] Second, we apply the so-called rigorous LASSO (R-LASSO) proposed by Belloni et al. (2012). This approach provides theory-based algorithms for optimal penalization under heteroskedastic and non-Gaussian errors. Moreover, it does not solely focus on the predictive performance, but selects the penalty level ensuring consistent prediction and parameter estimation. The basic idea is to control the noise (variance in the estimator around the actual values) while keeping the bias of the estimator as low as possible. For a software implementation, we use the Stata package *LASSOPACK* developed by Ahrens et al. (2019).

Although random forests have been shown to outperform parametric estimation methods and other machine learning methods, especially in the context of propensity score estimation (Pirracchio et al., 2015; Cannas and Arpino, 2019), we opt to use the LASSO for a few reasons. First, in contrast to studies that use a data-generating process with only a few covariates, we discuss a setting with potentially very large sets of explanatory variables. In such cases, the LASSO can outperform random forest estimation, as shown in Goller et al. (2020) for the logistic estimation of propensity scores. Second, we observe a highly skewed distribution concerning the shares of individuals affected and unaffected by the minimum wage. Again, Goller et al. (2020) demonstrate that the LASSO outperforms random forests in propensity score estimation with unequally distributed shares of observations in treatment and control groups.

The model selection process is conducted in the training year and includes all observations in the data for that year (*training sample*). The selected model's prediction (generalization) performance is then tested on all observations in the *validation sample*. Using this procedure, we can ensure both internal validity in the training year and external validity in the years following the training year, for which we also assess the prediction accuracy.

To assess the performance of our estimator, we use the mean squared error (MSE) as a measure of predictive performance. As this measure describes the average squared deviation of predicted and observed values, it is an intuitive performance measure that ensures that

[24] For cross-validation, the training sample is split into 5 equal folds. In each of the cross-validation runs, one of the folds is used to test the out-of-sample prediction, while the remaining four folds are used to train the model. Thus, after 5 runs, every fold is once used as a validation sample. The lambda value that minimizes (maximizes) the objective function of the OLS estimator (Logit estimator) in Equation 1.2 (Equation 1.3) over all folds is selected.

those deviations from the actual bite are small. To establish better comparability of the MSE values between different models and to account for different levels of variance in the observed bite over the years, we take the root and normalize the root mean squared error (RMSE) by dividing the RMSE values by the observed standard deviation of the bite variable in the data for each year. This approach results in the normalized root mean squared error (NRMSE), which is our primary measure for prediction performance. This NRMSE is easy to interpret because it represents the ratio of the variation not explained by the prediction to the average (observed) variation in the respective variable.

In addition to assessing the predictive performance of our LASSO-selected models, we present additional insights about the nature of the prediction error. We conduct a bias-variance decomposition of the MSE into two parts: the squared bias and the variance. Note that this exercise yields a decomposition of the predicted bites relative to true bites, which we observe in the testing period.

$$\begin{aligned} MSE &= E[(bite_i^{true} - \widehat{bite_i^{LASSO}})^2] \\ &= Var(bite_i^{true} - \widehat{bite_i^{LASSO}}) + (E[bite_i^{true}] - E[\widehat{bite_i^{LASSO}}])^2 \\ &= (variance) + (bias)^2 \end{aligned} \tag{1.4}$$

Equation (1.4) shows that the MSE can be separated into the variance and the (squared) bias. The first part describes the unsystematic random error of the LASSO-predicted bite compared with the observed bite. The latter part shows to what extent the mean of the LASSO prediction differs from the true mean of the observed bite, which is the unexplained systematic error.

1.3.3 Data

Our main data source for the empirical analysis is a 2-percent sample of administrative employment histories ("Beschäftigten-Historik", BeH) of the Institute for Employment Research (IAB). The BeH is a rich administrative dataset comprising information on each employee in Germany, recorded from compulsory employer reports to the German social security system. Among other variables, the dataset contains information on the gross daily and monthly wages of full-time, part-time, and marginal employees. However, it does not include individuals' working hours, which are essential for the determination of hourly wages, which in turn determine the bite of the minimum wage. To calculate hourly wages, we merge data on working hours from the German Social Accident Insurance to the BeH observations of each individual. Due to legislative reasons, these data on working hours are only available for the years 2010-2014, as employers were required to report the working hours of their employees during this period. We therefore restrict our analysis period to the years 2010-2014. Moreover, the German minimum wage was introduced in 2015, so the minimum wage bite can only be determined in the years before the law came into force. Given the data on wages and working hours, we calculate an hourly wage for each employment spell in the dataset.

All estimations in this study are presented at the individual level. We differentiate between two bite variables for our estimations. The first captures whether an individual is affected by the minimum wage from a binary categorization (incidence bite). The incidence bite takes a value of 1 if we observe an hourly wage below the initial German minimum wage level of €8.50 per working hour and 0 otherwise. The second bite variable is the gap between an individual's wage and the minimum wage level for all individuals affected by the minimum wage. Hence, the bite gap is defined on the interval [0,8.50] with a value of 8.50 for a person who would earn a zero wage and a value of 0 for all unaffected individuals.[25] Both bite variables are calculated based on real wages, deflated by the consumer price index to account for inflation-driven wage changes and to make wages comparable over time. We restrict the data as follows. Individuals' ages range between 15 and 75 years. Wages are excluded below the first percentile of the wage distribution, as these show implausibly low monthly wages below €87 (first percentile of observed wages). We use real monthly wages and truncate high wages at the social security contribution limit in each year. We also truncate working hours at 60 per week, as this represents the legal limit of weekly working hours in Germany.

Table 1.1 presents summary statistics for both bite variables. It shows means, standard deviations, minima and maxima, as well as the number of observations in our sample for

[25]Since the first percentile of wages is dropped to exclude implausibly low wages, which is common when using administrative data, Table 1.1 shows that all observed bite gaps are strictly below €8.50.

Table 1.1: Descriptive Statistics of Bite Variables

		overall	2010	2011	2012	2013	2014
Incidence Bite	Mean	0.12	0.14	0.13	0.12	0.12	0.11
	Std.Dev	0.329	0.350	0.338	0.329	0.320	0.309
	Min	0.00	0.00	0.00	0.00	0.00	0.00
	Max	1.00	1.00	1.00	1.00	1.00	1.00
Bite Gap	Mean	0.30	0.36	0.31	0.28	0.27	0.25
	Std.Dev	1.057	1.159	1.086	1.029	1.018	0.986
	Min	0.00	0.00	0.00	0.00	0.00	0.00
	Max	8.17	8.15	8.12	8.09	8.13	8.17
Observations		2,530,266	492,695	500,132	507,613	512,494	517,332

NOTE: The table shows the distributional properties of both bite variables, as observed in the analysis sample. The values for the mean and standard deviation differ from those shown in Figures 1.3 and 1.6 because observations with missing values for explanatory variables are not considered in the model estimations. *Data source:* Beschäftigtenhistorik (BeH) of the Federal Employment Agency, 2010-2014, analysis sample.

each of the years. The full sample of all five years comprises approximately 2.53 million observations for which we can calculate an hourly wage.[26] Hence, the data cover approximately 500,000 observations each year. The numbers slightly increase over time, reflecting increasing employment numbers in Germany over the observation period. In contrast, we observe slightly decreasing bite levels over the years, with an overall average of 0.12 for the incidence bite and 0.30 for the bite gap. Hence, we characterize approximately 12 percent of the individuals in the sample as being affected by the minimum wage introduction, and the average bite-gap among all individuals is approximately €0.33.

1.3.4 Set of Explanatory Variables

The purpose of applying LASSO estimation in our case is to select the most powerful explanatory variables in terms of prediction performance for the minimum wage bite. In contrast to traditional regression models, the simple inclusion of all (available) explanatory variables does not lead to multicollinearity problems since the LASSO algorithm selects only the most powerful predictors and excludes explanatory variables that provide only a small contribution to model fit. Furthermore, LASSO variable selection prevents over-fitting and should, there-

[26]We have no information on either working hours or monthly wages for approximately 431,000 individuals, which equals a share of 14.5 percent of all observations.

fore, imply good generalization performance in out-of-sample forecasts. Nevertheless, there are some restrictions on the set of suitable variables, which mainly concern problems of endogeneity since predictor variables might themselves be affected by the minimum wage introduction. Hence, contemporaneously measured values of these variables cannot be used to predict the minimum wage bite.[27] Essentially, two types of variables seem to be suitable for our prediction problem:

a contemporaneously measured variables that are exogenous, as these are not themselves affected by the minimum wage,

b predetermined variables measured before the minimum wage introduction, where the predetermined information is exogenous, while the contemporaneous information may be endogenously affected by the minimum wage.

Table 1.2: Set of Explanatory Variables

	Group	Variables	Time
(1)	Socio-Demographics	Eastern Germany (2), gender (2), age-categories (12), foreign (2)	contemporaneous
(2)	Education	school education (5), professional education (4)	contemporaneous
(3)	Tenure	deciles of the distribution of tenure (current establishment affiliation) in days (10), deciles of the distribution of experience (total labor market participation) in days (10)	contemporaneous
(4)	Industry	industrial sector of the worker's firm (21)	contemporaneous
(5)	Wage	deciles of the distribution of log. monthly wages plus one missing category (11)	predetermined (2009)

Note: The numbers in parentheses are the numbers of categories for categorical variables. The total number of explanatory variables is 76 including wage variables or 65 excluding wage variables.

Table 1.2 shows an overview of the sets of explanatory variables in the analyses. In most specifications, we use the 65 contemporaneously measured variables shown in Table 1.2 but refrain from using wage variables. In Appendix 1.A, we run a robustness check in which we add wage variables as predictors. Wage variables may improve the predictive power but need to be

[27]This restriction may be relaxed when applying the approach by Chernozhukov et al. (2018), which allows the exogenous identification of endogenous (instrumented) predictor variables in the LASSO.

predetermined since wages are likely themselves affected by the minimum wage. Hence, we can only include predetermined wages due to endogeneity concerns. We expect the predetermined wage to exert decreasing predictive power over time for predicting the contemporaneous bite. This is because there may be additional (unexplained) wage variation over longer time spans. With long-term bite prediction in mind, it would be desirable for our model to minimize the usage of pre-treatment variables, as these may have decreasing predictive power in the long run. There is a second reason for the exclusion of predetermined variables, as they can only contain information for individuals who were in the sample in the respective year when the predetermined variable is measured. Thus, we do not observe the respective information for labor market entrants. In turn, if we were to consider only those individuals who were already in the sample in the year of measurement of such variables, it would result in a highly skewed sample. To circumvent this problem, we add a category for missing values in each predetermined variable and, thus, do not lose observations of entrants.

1.4 Validation of Bite Prediction

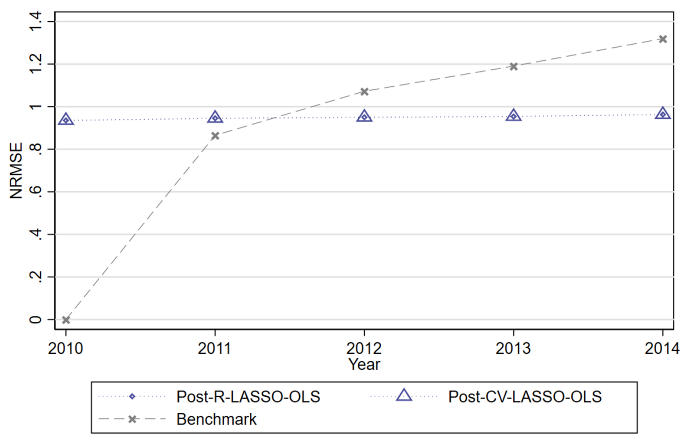
1.4.1 Bite-Gap Prediction

In the first step of the analysis, we estimate and validate the prediction model for the bite gap. The bite gap is a continuous measure of treatment intensity. It measures the gap between an individual's wage and the minimum wage and thus captures the intensive margin to which individuals are affected by the minimum wage. The bite gap is defined between zero for wages exactly at or above the minimum wage and 8.5 for (theoretically) zero wages. Using this bite measure, model Equation (1.1) boils down to a regression problem. Therefore, this model allows us to apply the linear (OLS-like) version of the LASSO estimator as described in Section 1.3.2. In the baseline specification of the model, we use the full set of plausibly exogenous explanatory variables and let the LASSO select those with the highest predictive power. Then, we predict the bite gap for each individual by estimating a post-LASSO-OLS using the selected variables.

We compare the NRMSEs of our LASSO-based predictions with the TCPT bite gap for each year. This comparison allows for assessing whether we can outperform our benchmark

(TCPT bite gap) and how the value added from our prediction develops over time. We use the observations from 2010 for model training and those from the following four years for predictions. To be clear, the model does not include the data of the years 2011 to 2014 when it is trained such that the predictions in those years are considered out of sample. Figure 1.2 shows the NRMSE values for the years 2010 to 2014.

Figure 1.2: Prediction Performance of LASSO-Based Bite-Gap Predictions and the Benchmark



NOTE: Normalized root mean squared error (NRMSE) for post-LASSO-OLS estimations based on R-LASSO and CV-LASSO as well as TCPT bite as a benchmark. RMSE values are normalized by dividing them by the standard deviation of the bite variable in the data in each year. Training year: 2010, validation years: 2011-2014.

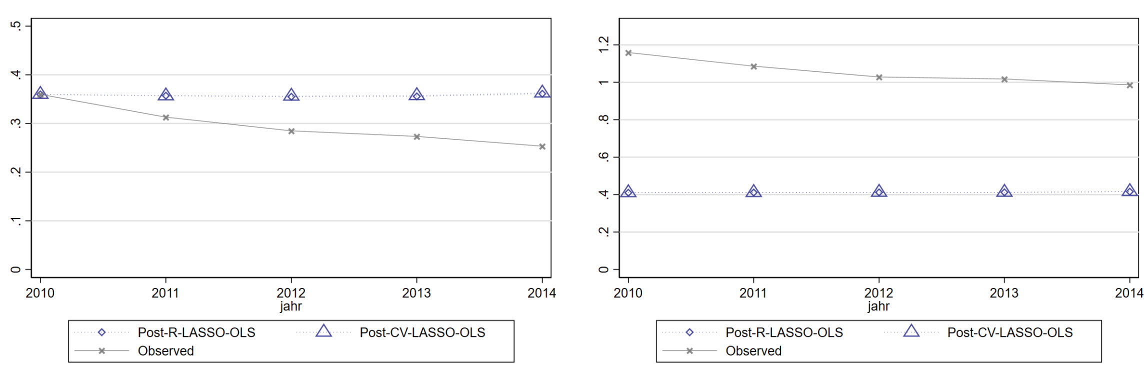
Comparing the two LASSO predictions with the TCPT bite gap as our benchmark reveals that the NRMSEs are approximately as large as the benchmark's NRMSE in the first year after the training period but the predictions outperform the benchmark from the second prediction year (2012) onward. The results indicate that using a time-constant pre-treatment bite gap is as good as our prediction for the year after the measurement of the TCPT bite gap but the prediction can outperform the benchmark already in the second year; i.e., the predictions can then provide a more accurate treatment measure compared with the conventional fixing of the pre-treatment bite. The error rates of the prediction increase slightly over the years but remain relatively constant, even in the out-of-sample predictions (2012-2014). Comparing the errors of 2010 with those of 2014, the increase in NRMSE corresponds to 3 percent for both models based on the R-LASSO and CV-LASSO penalization schemes. This finding indicates good generalization performance of our prediction model. Both penalization schemes (R-LASSO and CV-LASSO) perform similarly, although different variables are selected in each penalization scheme. The R-LASSO selects only 46 of the 66 predictor variables, while the CV-LASSO selects 56 variables from the full set of predictors.

Table 1.3: Error Decomposition for Post-LASSO-OLS

	Post-R-Lasso-OLS				Post-CV-Lasso-OLS			
	MSE	Bias	Sq. Bias	Var	MSE	Bias	Sq. Bias	Var
2010	0.8749	-0.0000	(0.0000)	0.8749	0.8747	-0.0000	(0.0000)	0.8747
2011	0.8939	0.0374	(0.0017)	0.8922	0.8937	0.0370	(0.0016)	0.8921
2012	0.9026	0.0662	(0.0046)	0.8980	0.9027	0.0673	(0.0048)	0.8979
2013	0.9095	0.0794	(0.0065)	0.9030	0.9097	0.0809	(0.0068)	0.9029
2014	0.9275	0.1108	(0.0119)	0.9155	0.9277	0.1122	(0.0123)	0.9155

Note: Mean squared error, (squared) bias and variance for post-LASSO-OLS estimations from R-LASSO and CV-LASSO models. The values are normalized by dividing them by the observed standard deviation in the data in each year to account for differing variances in the observed bite over the years.

Figure 1.3: Mean and Standard Deviation of Predicted and Observed Bite Gaps



(a) Mean

(b) Standard Deviation

Note: Panel (a) shows the observed and predicted mean values of the bite gap from post-R-LASSO and post-CV-LASSO linear models. Panel (b) shows the standard deviation of observed and predicted values of the bite gap from post-R-LASSO and post-CV-LASSO linear models. Training year: 2010, validation years: 2011-2014.

To obtain a more detailed picture of the errors in our prediction, we evaluate the bias-variance tradeoff. The decomposition results are presented in Table 1.3 for the two LASSO estimation methods. The MSE is almost entirely driven by the variance in the estimates, while the squared bias is close to zero. The bias slightly increases over the out-of-sample prediction years but remains relatively small. The variance in our estimator remains relatively constant throughout all years, which indicates that the unsystematic random part of the MSE remains similar in size. Thus, the relatively small MSE increases over the years show a good generalization performance of our LASSO-based prediction. Furthermore, it is notable how similar the R-LASSO and CV-LASSO perform despite each penalization scheme selecting a different number of predictor variables.

Figure 1.3 illustrates the additional properties of our prediction by comparing the means and standard deviations of the observed and predicted values of the bite gaps. Panel (a) shows the mean bite gap for the two LASSO models and the observed bite gap for each year. We observe a relatively constant value of the mean predicted bite gaps, while the mean of the observed bite gap decreases over time. The differences in means between observed and predicted bite gaps represent the bias of the estimator, as shown in Table 1.3. The graph illustrates that our predictions do not follow the downward trend of observed means over the years. Hence, while our model is able to predict the bite very accurately based on the MSE criterion, it shows some discrepancy in average developments over time. The R-LASSO and CV-LASSO perform equally in terms of mean prediction. Panel (b) shows the standard deviations of the observed and predicted bite gaps. Our LASSO-based predictions do not capture the same volatility as the observed bite gap in the data. We are only able to capture half of the observed standard deviation in our prediction models.

For robustness checks, which are presented in Appendix 1.A, we rerun our validation using predetermined wage variables as additional predictors (see Appendix 1.A.1) and an alternative training year (see Appendix 1.A.2). Adding predetermined wage information does not yield a meaningful improvement in prediction performance. Training the model on data from 2011 yields similar prediction errors; i.e., the prediction outperforms the benchmark in the second period after the training year.

1.4.2 Incidence Bite Prediction

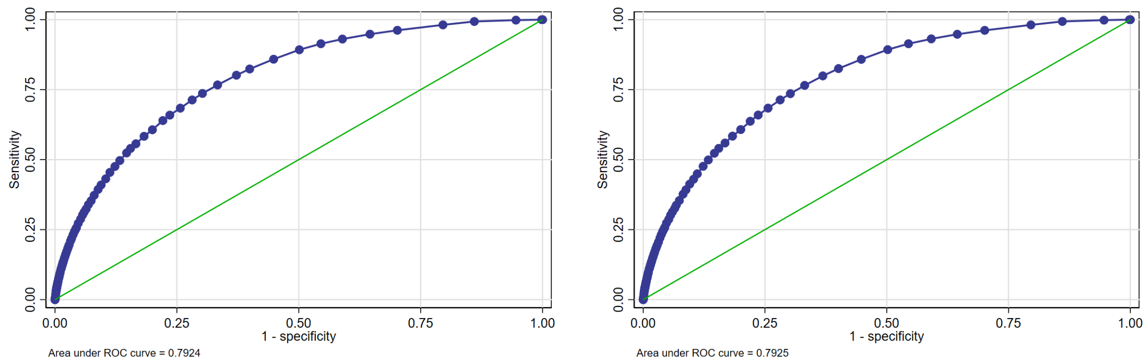
In this subsection, we continue to validate the LASSO-based prediction of the incidence bite, i.e., whether a worker is affected by the minimum wage. Other than the previous analyses in Section 1.4.1, which represented a linear regression problem (due to the continuous bite gap variable), the prediction of the incidence bite is a classification problem. We use a logit version of the LASSO estimator, which maximizes the penalized log-likelihood function, as shown in Equation (1.3), to select a sparse model that includes variables based on their predictive power, similar to the linear case described in Section 1.4.1. The estimation of the LASSO-logit yields a treatment probability for each individual in the data. However, we ideally want to classify individuals into either the treatment or control group.

Classification into treatment and control groups

The classification of individuals should ideally yield a large fraction of actually treated individuals to be classified as treated, which is assessed by the so-called true positive rate (TPR). Simultaneously, we want to keep the misclassification of individuals who are not treated but are falsely assigned to the treatment group (false positive rate (FPR)) as low as possible. The distribution of treated and untreated individuals is highly skewed in the data since only approximately 12 percent of observed individuals were initially affected by the minimum wage introduction (see Table 1.1). Given such an imbalanced estimation sample, we need to choose a cutoff rule that determines the probability threshold for each individual to be classified as treated or untreated. A simple cutoff at the predicted probability of 50 percent would lead to implausible predictions, with only a few individuals being assigned to the treatment group. Therefore, we choose the cutoff value that maximizes the area under the receiver operator characteristic curve (AUC). This curve shows the values for the TPR on the y-axis and the FPR on the x-axis at every potential cutoff value. Each cutoff value is represented by a dot on the curves in Figure 1.4. The cutoff value that maximizes the area under that curve corresponds to the point where the marginal increase in the FPR exceeds the marginal gain in the TPR.

Figure 1.5 shows the prediction performance of the LASSO-based logit estimation when applying the AUC-cutoff rule. As shown in Panel (a), the fraction of true positives dramatically decreases when using the simple TCPT bite from the training year. In contrast, the LASSO-based predictions show relatively high and consistent TPR values of approximately 75 percent over the prediction period from 2010 to 2014. Panel (b) reveals that approximately 69 percent of untreated individuals are correctly identified by the model. This value is lower than the

Figure 1.4: ROC Curves of LASSO Models



(a) R-LASSO

(b) CV-LASSO

NOTE: Receiver operator characteristic curves for the R-LASSO and CV-LASSO. Training year: 2010.

true negative rate of the TCPT bite. In combination, the mean error (Panel (c)) of LASSO-logit prediction lies at approximately 31 percent in the base year and slightly increases to approximately 32 percent in 2014. In contrast to the TCPT bite, the mean errors of the predictions do not increase much over time, which could make the model superior to the simple TCPT bite in the long run. However, we are not able to outperform our benchmark within the validation period (2010-2014).

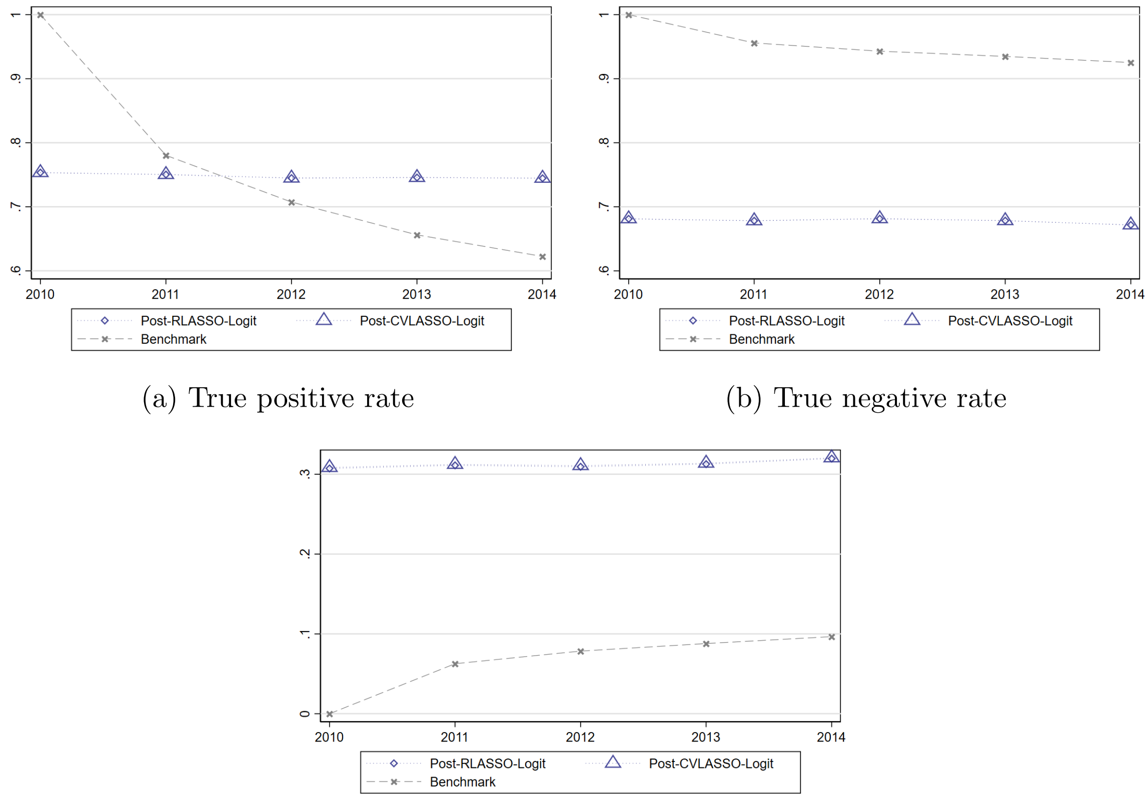
Table 1.4: Error Decomposition for Post-LASSO-Logit Using the $max(AUC)$ Cutoff Rule

	Post-R-Lasso-Logit				Post-CV-Lasso-Logit			
	MAE	Bias	Sq. Bias	Var	MAE	Bias	Sq. Bias	Var
2010	2.5041	1.9265	(0.4554)	2.0487	3.1609	2.7991	(0.9614)	2.1996
2011	2.7253	2.1485	(0.5270)	2.1982	3.4446	3.0807	(1.0836)	2.3610
2012	2.8604	2.2748	(0.5597)	2.3007	3.6394	3.2663	(1.1540)	2.4854
2013	3.0572	2.4799	(0.6290)	2.4282	3.8859	3.5177	(1.2657)	2.6202
2014	3.3534	2.7794	(0.7363)	2.6172	4.2489	3.8716	(1.4286)	2.8202

NOTE: Mean absolute error, (squared) bias and variance for post-LASSO-logit estimations from R-LASSO and CV-LASSO models. The values are normalized by dividing them by the observed standard deviation in the data in each year to account for differing variances in the observed bite over the years.

Again, we decompose the errors from our predictions into bias and variance to gain insights to the systematic and random parts of the mean absolute error (MAE). Since the MAE for a binary variable equals its mean squared error, this decomposition works equally

Figure 1.5: Prediction Performance of LASSO-Based Incidence Bite Predictions and the Benchmark Using the $max(AUC)$ Cutoff Rule.



(a) True positive rate

(b) True negative rate

(c) Mean error

NOTE: Error measures for post-LASSO-logit predictions and time-constant pre-treatment bites as a benchmark. The training sample consists of all observations in 2010. The validation sample comprises all observations in the years 2011-2014.

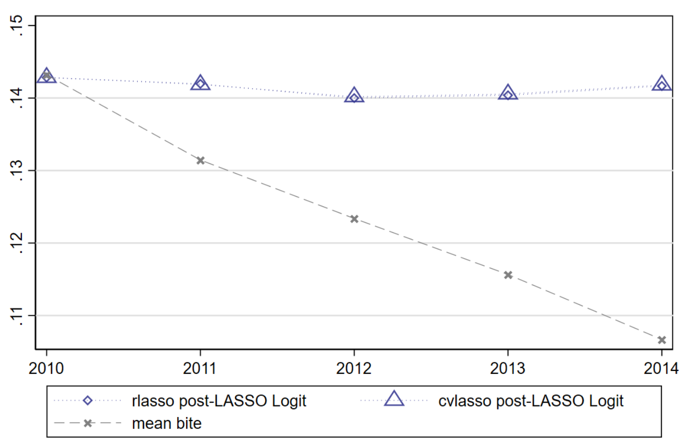
to the error decomposition in Section 1.4.1. The relatively high biases in Table 1.4 show that we systematically overestimate the bite with our LASSO-based model using the AUC-cutoff rule. This applies to both the R-LASSO and CV-LASSO. The values of the systematic error (bias) and the random error (variance) are approximately equal, and the MAE increases over the years. In Appendix 1.C, we show the results for two other cutoff selection rules. Different cutoff rules influence the tradeoff between true positives and true negatives but do not show an improvement concerning the mean error.

In Appendix 1.D, we present an alternative approach that restricts the population to individuals with a monthly wage below €1775. The advantage of this approach is that workers must only be classified if they are at reasonable risk of being in the treatment group. This avoids the classification of many workers that are undoubtedly true negatives. However, when we compare the respective LASSO-based predictions with the benchmark (TCPT bite), the resulting bites are again not superior in terms of mean error comparison.

Treatment Values from Predicted Probabilities

In light of the shortcomings of the classification of individuals based on predicted probabilities, we avoid classifying individuals into treatment and control groups. Instead, we directly use the predicted treatment probabilities from the LASSO-logit, which leads to an average treatment probability that should ideally match the average bite. Hence, this approach should result in a much smaller bias of the LASSO-based bite predictions.

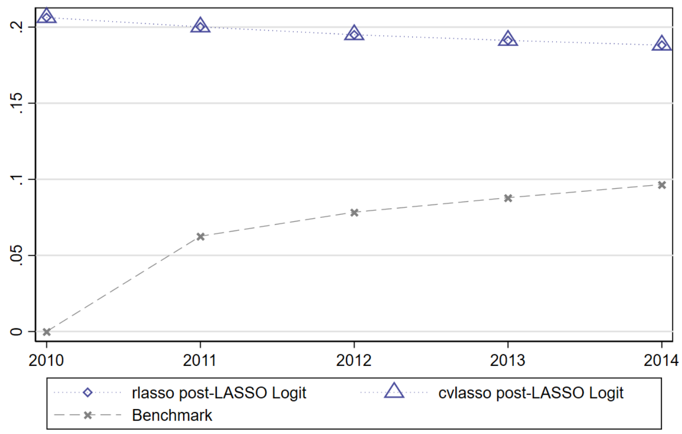
Figure 1.6: Predicted Probabilities from Post-LASSO-Logit and Mean Observed Bite Values



NOTE: Predicted probabilities from incidence bite predictions and observed mean values of the incidence bite. Training year: 2010, validation years: 2011-2014.

Figure 1.6 shows the average values of the predicted probabilities for each prediction year from a post-LASSO-logit estimation with variables selected by the R-LASSO and CV-LASSO. We compare these values with the mean of the observed TCPT bite. The result shows that our LASSO-based models are in the ballpark of the average bite, which is slightly above 0.1. However, the predicted probabilities cannot capture the decreasing trend in the mean bite over time, but the probabilities are estimated to be relatively constant in each prediction

Figure 1.7: Error Rates of Predicted Probabilities from Post-LASSO-Logit and the Benchmark



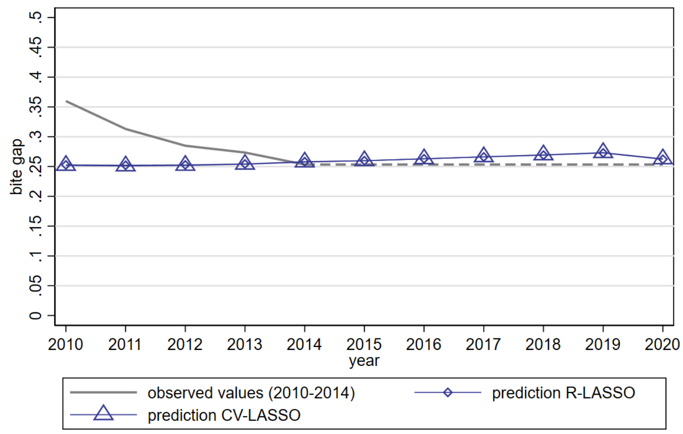
NOTE: Mean absolute error (MAE) values of predicted probabilities for incidence bite predictions. The values show the absolute difference between the predicted probability and the observed bite values. Training year: 2010, validation years: 2011-2014.

year. Moreover, the LASSO-based predictions are still not able to outperform the TCPT bite in terms of the mean absolute error, as illustrated in Figure 1.7.

1.5 Application: Wage Effects of the German Minimum Wage Introduction

While the previous sections solely focused on the validation of LASSO-based bite prediction in the years 2010 to 2014, we extend the time frame in this section. Based on our LASSO-selected prediction models, we forecast minimum wage bites for the post-treatment years after 2014. These post-treatment bites allow us to analyze the effect of the 2015 minimum wage introduction in Germany. First, we descriptively compare our bite predictions and forecasts with the benchmark, which is the observed bite of 2014 that is kept constant in the post-2014 period (TCPT bite). Second, we analyze the impact of the minimum wage on monthly wages in treatment effect regressions of log wages using the predicted bite. We thereby compare the results of conventional difference-in-differences specifications using TCPT bites with regressions that use the forecasted LASSO-based bites. The models are now trained based on the observations from 2014 since we do not desire a testing period. Instead, we want to train the model based on the latest information and forecast the bite from 2014 onward.

Figure 1.8: Average Bite-Gap Forecasts, Base Year 2014



NOTE: Mean values (trends) of the forecasts for the bite gap from R-LASSO and CV-LASSO-based predictions. The bite predictions are based on Equation 1.1. Training year for the predictions: 2014. The gray line shows the measured bite values from the BeH dataset, which are held constant for 2015-2020 with the value from 2014.

Figure 1.8 displays the prediction of the average bite compared to the TCPT bite.[28] We focus on the bite gap, as this method is promising in minimizing the prediction error and yields an average predicted bite gap that closely matches the population's mean of approximately 0.25. However, we note that the forecasted bite does not capture much variation over time.

To estimate the effects of the minimum wage, we estimate a standard difference-in-differences specification, which allows us to quantify the effect of the predicted bite. Moreover, we can compare this effect to conventional treatment effects that use the TCPT bite. We estimate the following specification with separate post-treatment effect interactions for the years 2015-2020, with the period 2010-2014 serving as a reference period:

$$ln(Wage)_{it} = \alpha + \sum_{k=2011}^{2020} Year_{k,t} * \gamma_k + Bite_{it} * \theta + \sum_{k=2015}^{2020} Year_{k,t} * Bite_{it} * \delta_k + Trend_t * Bite_{it} * \tau + \epsilon_{it} \tag{1.5}$$

The dependent variable of interest is the log monthly wage of each individual i at time t. α is the constant that captures the initial wage level of the control group, i.e., for individuals with $Bite_{it} = 0$ in the year 2010. γ_k captures common time effects for each year in the data,

[28] Appendix 1.E presents the predicted mean values along with the standard deviations, the minima and maxima of the bite gap and the incidence bite for 2010-2020.

where the time dummy $Year_{k,t} = 1$ if $k = t$ and zero otherwise. θ is the coefficient of the bite variable that captures wage differences by bite that are independent of the minimum wage. Note that the bite variable has subscripts i and t because the predicted bite varies over time. Coefficients δ_k on the interaction of the bite and year indicators capture the treatment effect interaction of interest for each post-treatment year in the data. τ controls for a bite-specific trend identified through variation before the minimum wage introduction. Finally, the specification includes an idiosyncratic error term, ϵ_{it}.

Table 1.5: Minimum Wage Effects on Wages – Bite Gap

	TCPT-bite	**predicted-bite**	
		R-LASSO	CV-LASSO
	(1)	(2)	(3)
	Log real wage	Log real wage	Log real wage
$bite * year_{2015}$	0.080***	0.041***	0.041***
	(0.002)	(0.005)	(0.005)
$bite * year_{2016}$	0.135***	0.037***	0.036***
	(0.003)	(0.006)	(0.006)
$bite * year_{2017}$	0.190***	0.050***	0.050***
	(0.003)	(0.007)	(0.007)
$bite * year_{2018}$	0.242***	0.048***	0.047***
	(0.004)	(0.008)	(0.008)
$bite * year_{2019}$	0.293***	0.049***	0.047***
	(0.004)	(0.009)	(0.009)
$bite * year_{2020}$	0.338***	0.034***	0.032***
	(0.004)	(0.010)	(0.010)
Observations	4,658,218	6,784,838	6,784,838

Note: The estimates are the treatment effects of the minimum wage on log wages from difference-in-differences estimations at the individual level. The independent variables are the predefined (Column 1) and predicted (Columns 2 and 3) bite gaps in each of the years from 2015 to 2020. The numbers of observations between the benchmark (TCPT bite gap) and predictions differ because the TCPT bite gap only captures pre-existing jobs, while the prediction also includes labor market entrants. Base year of the LASSO model: 2014. Robust standard errors are in parentheses. Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1.

Table 1.5 presents the wage effects of the bite gap. Column (1) shows the wage effect for the initially affected individuals from the TCPT bite. The wage effect increases over time: it is 0.08 log points in 2015 and grows to 0.34 log points in 2020. When using the predicted bites in Columns (2) and (3), we observe a wage effect of 0.04 log points in 2015 and a fairly constant wage effect ranging from 0.032 to 0.050 in the consecutive treatment years. The results suggest that the effect of the conventional estimation (TCPT bite) is overestimated compared to the effects of the predicted bites for contemporaneous employees in 2015-2020. Placebo regressions are presented in Appendix 1.G, where the effects of all bite-year interactions are close to zero in the years 2010-2013 (reference 2014).

The overestimation of the wage effects in 2015-2020 from the TCPT bite is due to substantial relative wage growth among the treatment group over time. This wage growth may be caused by two sources: (1) a genuine minimum wage effect and (2) wage growth at the lower end of the wage distribution, independent of the minimum wage. Theoretically, there is no obvious reason for an increasing minimum wage effect over time since the minimum wage remained fairly constant in real terms (see Appendix 1.F). In contrast, the literature documents rising wages at the bottom of the wage distribution, and this development was observed even before the minimum wage was introduced (Bossler and Schank, 2023). These findings suggest that initially low-paid individuals experienced wage growth that would also have emerged without the minimum wage introduction. Hence, the treatment effects on the initially affected minimum wage workers may increase over time, while the genuine minimum wage effect, as suggested from our LASSO-predicted effect estimates, remains constant over time.

Another substantial discrepancy between Column (1) and Columns (2) and (3) is that the latter columns include individuals who entered employment after the minimum wage was introduced, while the TCPT bite only identifies an effect for individuals who were initially affected and employed. The inclusion of entrants is also reflected in the higher number of observations in the respective estimations. In contrast, the exclusion of entrants yields an increasingly selective sample over time, and the results suggest that a restriction for such stable jobs leads to an increasingly positive selection in terms of wages, which could again explain the overestimation of wage effects from the TCPT bite.

Appendix 1.H presents treatment effect estimations on log wages in which the treatment variable is the binary incidence bite. While our LASSO-based prediction of the incidence bite has some shortcomings, as discussed in Section 1.4,[29] the estimates again show that

[29] The LASSO-based prediction of the incidence bite has a larger error than the TCPT bite and does not meet the mean incidence bite.

the effects of the predicted bites in Columns (2) and (3) are significantly smaller than the treatment effects of the TCPT bite.[30] These results confirm the conclusions from the bite gap, suggesting that the benchmark (TCPT bite) significantly overestimates the wage effects of the minimum wage.

1.6 Conclusion

We develop a novel approach to identify minimum wage effects for contemporaneously affected individuals in bite-dependent minimum wage evaluations in the medium to long run. To date, conventional bite-dependent minimum wage evaluations have identified effects on individuals who were initially affected and classified as treated. However, the initially treated workers may not define the policy-relevant treatment group as they might no longer be affected by the minimum wage after some years. Especially in the long run, initially affected individuals may no longer be the group that is influenced by the minimum wage. Instead, we would want to estimate an effect on contemporaneously affected individuals. This distinction is highly relevant, as the group of affected workers may change over time due to wage dynamics or job mobility that occurs independently of the minimum wage.

To define the group of contemporaneously affected employees, we predict the treatment bite of the German minimum wage introduction in the medium to long run. We use the LASSO estimator for the selection of explanatory variables based on their predictive power. The set of explanatory variables contains numerous variables on individual labor market biographies from German administrative employment records. We fit the LASSO-selected model to the data from 2010 to 2014, i.e., before the minimum wage introduction. In this period – absent a minimum wage – we can test the performance of the LASSO-based bite prediction. In this validation period, the LASSO-based bite gaps show a smaller average error than conventional time-constant pre-treatment bites after only two post-treatment years. The results indicate that conventional fixing of the pre-treatment minimum wage bite is appropriate to approximate the actual bite in the first year after the minimum wage introduction. In the long run, however, wage changes that occur independently of the minimum wage can lead to changes in

[30] The average predicted incidence bite, as applied in Table 1.H2, is approximately three times the observed mean incidence bite. Hence, the effect size, which is approximately one-third of the effect size in Columns (2) and (3) of Table 1.5, is in the same ballpark.

the composition of affected and unaffected individuals such that the error from pre-treatment bites exceeds that from machine-learning-based treatment bites. Our LASSO-based bite has two advantages. First, it addresses dynamic changes in the treatment group, and second, it allows us to define a bite for labor market entrants. In contrast, the conventional bite only includes initially employed individuals, leading to an increasingly selective treatment group over time.

We apply the LASSO-based bite to a difference-in-differences estimation of the wage effects of the German minimum wage introduction. The resulting significantly positive wage effects confirm the findings in the literature. However, the result suggests that the simple time-constant pre-treatment bite overestimates the wage effect from the second treatment year onward. This overestimation is in line with our argument that using a time-constant pre-treatment bite yields an increasingly selective treatment group over time.

While the presented approach allows us to estimate bite-dependent minimum wage effects in the medium to long run, we want to clarify that the presented approach also has some shortcomings. First, good predictor variables in the validation period (2010-2014) may not be good predictors after the introduction of a minimum wage. Second, it is challenging to find good bite predictors that are not themselves (endogenously) affected by the minimum wage. Hence, we have to make assumptions about the set of variables that we consider to be exogenous, especially if they are not predetermined.

Appendix - Chapter 1

1.A Robustness Checks

In this appendix, we present robustness checks for the predictions of the bite gap. First, we consider predetermined wage variables in the prediction and check whether they significantly improve our predictions. Second, we use 2011 as an alternative base year to check whether the results are sensitive to the training year of our model.

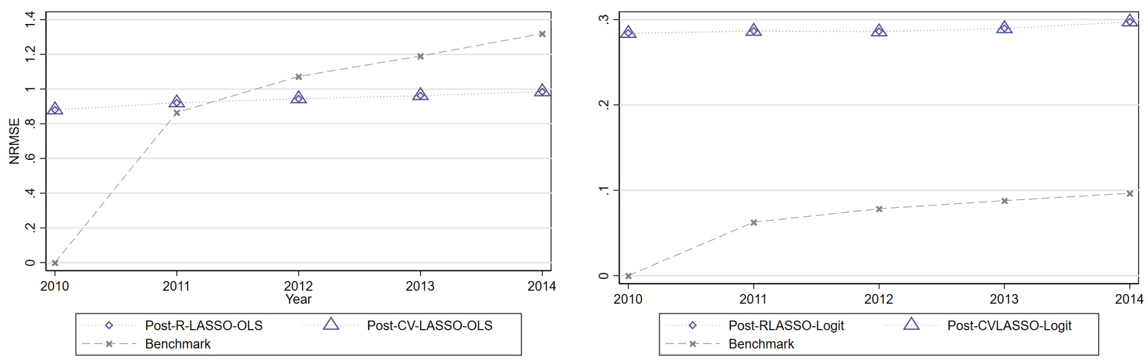
1.A.1 Considering Predetermined Explanatory Wage Variables

We assess LASSO-based bite prediction and consider predetermined wage information as explanatory variables. Since we only want to consider explanatory variables that are not endogenously affected by the minimum wage, we measure the wage information one year before the training year. Hence, the wage is certainly not affected by the minimum wage.

Figure 1.A1 presents the results from the LASSO-based predictions. The left Panel (a) shows the NRMSE for the bite gap, and the right Panel (b) shows the ME for the incidence bite using the AUC cutoff rule.

Compared to the main results, Panel (a) of Figure 1.A1 shows almost identical results regarding the NRMSE of the LASSO-predicted bite gap. Again, the LASSO-based bite gap outperforms the TCPT bite in the second year after the model is trained. However, its advantage over the prediction that does not include wage information is limited. Similarly, when we look at the incidence bite prediction in Panel (b), the mean absolute error of the LASSO-based

Figure 1.A1: Performance of LASSO-Based Predictions and the Benchmark, with Explanatory Wage Variables



(a) Bite Gap

(b) Incidence Bite

Note: Normalized root mean squared error (NRMSE)/mean absolute error (MAE) for estimations based on post-R-LASSO and post-CV-LASSO OLS/logit and time-constant pre-treatment bites. The cutoff optimization for the logit model is based on AUC maximization. Training year: 2010, validation years: 2011-2014.

predictions always exceeds the benchmark TCPT bite. Hence, predetermined wage variables do not substantially improve the predicted treatment bites.[31]

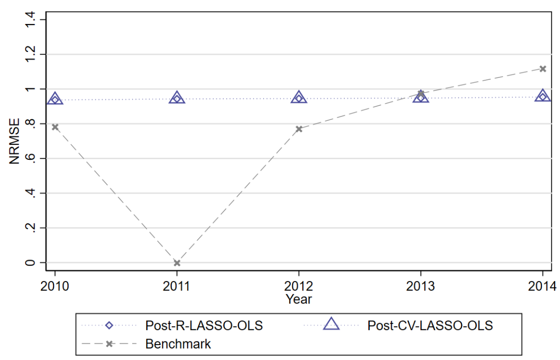
1.A.2 Using 2011 as an Alternative Base Year

Next, we change our initial specification with respect to the base year used for training the prediction model. We use 2011 as the base year instead of 2010 and again assess the prediction performance in the validation years 2010 and 2012-2014. This iteration of the base year serves as a robustness check to test whether similar patterns are observed when using a different base year. The test allows us to check whether the base year is specific in its wage structure or whether there are particualr changes between the base and test years that may affect prediction performance.[32] At the same time, changing the base year limits the number of subsequent validation years used to check long-run prediction performance.

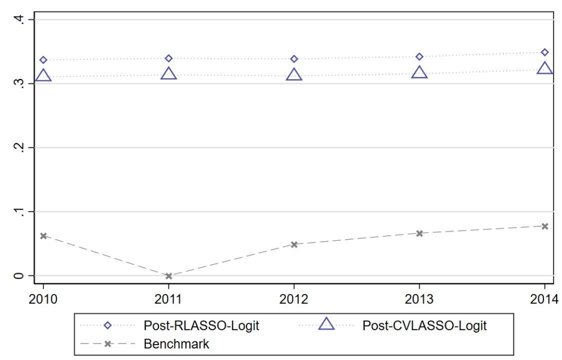
[31] Error decompositions are shown in Appendix 1.B.

[32] We are aware of structural breaks in some explanatory variables, including the occupational classifier and the part-time information in the administrative data between 2010 and 2011. Hence, the prediction performance in our test sample may be negatively affected by these changes (Fitzenberger and Seidlitz, 2020).

Figure 1.A2: Prediction Performance of LASSO-Based Predictions and the Benchmark, Base Year 2011



(a) Bite Gap



(b) Incidence Bite

Note: Normalized root mean squared error (NRMSE)/mean error for estimations based on post-R-LASSO and post-CV-LASSO OLS/logit and time-constant pre-treatment bites. Wage variables are excluded from the estimation sample. Training year: 2011, validation years: 2010, 2012-2014.

The results in Figure 1.A2 show similar patterns as before for the bite gap (Panel (a)) and the incidence bite (Panel (b)). The LASSO-based prediction improves relative to the benchmark over time. In the prediction of the bite gap, it yields almost the same performance as the benchmark one year after the base year. This result matches the observed pattern in our baseline specification. Again, the error rates are remarkably consistent over the years. From these results, we conclude that the choice of the base year has only a minor influence on the prediction performance.

Regarding the incidence bite, the results also do not change much compared to the baseline model. The mean error is greater than 0.3 for predictions based on both penalization schemes in the base year 2011. Again, the error rates are rather constant in all prediction years but slightly increase in the years after the base year. As before, changing the base year does not improve the prediction performance, and the prediction model does not beat the benchmark in our validation period.[33]

[33]The respective error decompositions are shown in Appendix 1.B.

1.B Error Decompositions for Robustness Checks

Table 1.B1: Error Decomposition from post-LASSO-OLS: Bite Gap, Including Wages, Base Year 2010

	Post-R-Lasso-OLS				Post-CV-Lasso-OLS			
	MSE	Bias	Sq. Bias	Var	MSE	Bias	Sq. Bias	Var
2010	0.7748	-0.0000	(0.0000)	0.7748	0.7746	-0.0000	(0.0000)	0.7746
2011	0.8481	0.0573	(0.0039)	0.8442	0.8479	0.0572	(0.0039)	0.8440
2012	0.8912	0.1019	(0.0110)	0.8802	0.8910	0.1019	(0.0110)	0.8801
2013	0.9255	0.1235	(0.0158)	0.9097	0.9254	0.1237	(0.0159)	0.9095
2014	0.9695	0.1662	(0.0268)	0.9426	0.9693	0.1664	(0.0269)	0.9424

Note: Mean squared error, (squared) bias and variance for post-LASSO-OLS estimations from the R-LASSO and CV-LASSO models. The values are normalized by dividing them by the observed standard deviation in the data in each year to account for differing variances in the observed bite over the years.

Table 1.B2: MAE Decomposition from Post-LASSO-Logit: Incidence Bite, Including Wages, Base Year 2010

	Post-R-Lasso-Logit				Post-CV-Lasso-Logit			
	MAE	Bias	Sq. Bias	Var	MAE	Bias	Sq. Bias	Var
2010	2.1663	1.5907	(0.3105)	1.8558	2.3138	1.8004	(0.3977)	1.9161
2011	2.3421	1.7668	(0.3564)	1.9857	2.5082	1.9936	(0.4538)	2.0545
2012	2.4693	1.8984	(0.3898)	2.0795	2.6421	2.1299	(0.4907)	2.1514
2013	2.6435	2.0829	(0.4437)	2.1998	2.8305	2.3275	(0.5541)	2.2764
2014	2.9114	2.3580	(0.5300)	2.3814	3.1207	2.6230	(0.6557)	2.4650

Note: Mean absolute error, (squared) bias and variance for post-LASSO-logit estimations from the R-LASSO and CV-LASSO models. The values are normalized by dividing them by the observed standard deviation in the data in each year to account for differing variances in the observed bite over the years.

Table 1.B3: Error Decomposition for Post-LASSO-OLS: Bite-Gap, Without Wages, Base Year 2011

	Post-R-Lasso-OLS				Post-CV-Lasso-OLS			
	MSE	Bias	Sq. Bias	Var	MSE	Bias	Sq. Bias	Var
2010	0.8787	-0.0329	(0.0015)	0.8773	0.8784	-0.0322	(0.0014)	0.8771
2011	0.8896	0.0000	(0.0000)	0.8896	0.8894	-0.0000	(0.0000)	0.8894
2012	0.8935	0.0249	(0.0007)	0.8929	0.8934	0.0266	(0.0007)	0.8927
2013	0.8984	0.0375	(0.0015)	0.8969	0.8985	0.0395	(0.0016)	0.8969
2014	0.9104	0.0654	(0.0042)	0.9063	0.9106	0.0671	(0.0044)	0.9062

NOTE: Mean squared error, (squared) bias and variance for post-LASSO-OLS estimations from the R-LASSO and CV-LASSO models. The values are normalized by dividing them by the observed standard deviation in the data in each year to account for differing variances in the observed bite over the years.

Table 1.B4: MAE Decomposition from Post-LASSO-Logit: Incidence Bite, Without Wages, Base Year 2011

	Post-R-Lasso-Logit				Post-CV-Lasso-Logit			
	MAE	Bias	Sq. Bias	Var	MAE	Bias	Sq. Bias	Var
2010	1.9444	1.7931	(0.3945)	1.5499	1.9431	1.7890	(0.3927)	1.5504
2011	2.2028	2.0600	(0.4845)	1.7183	2.2002	2.0550	(0.4822)	1.7181
2012	2.3520	2.2103	(0.5284)	1.8236	2.3529	2.2094	(0.5280)	1.8249
2013	2.5205	2.3814	(0.5801)	1.9404	2.5221	2.3814	(0.5801)	1.9420
2014	2.7832	2.6495	(0.6691)	2.1141	2.7868	2.6519	(0.6703)	2.1165

NOTE: Mean absolute error, (squared) bias and variance for post-LASSO-logit estimations from the R-LASSO and CV-LASSO models. The values are normalized by dividing them by the observed standard deviation in the data in each year to account for differing variances in the observed bite over the years.

1.C Alternative Cutoff Optimization for Incidence Bite Predictions

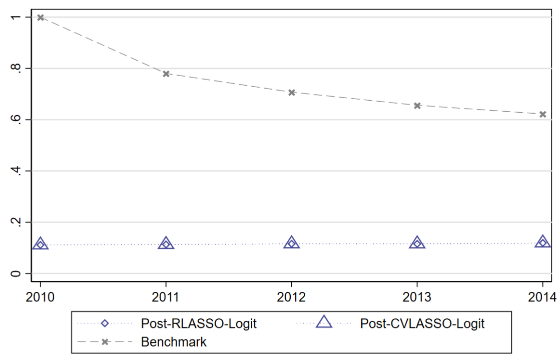
The classification of individuals should ideally yield a large fraction of truly treated individuals who are classified to be treated, the so-called true positive rate (TPR), and a large fraction of individuals who are actually untreated and are classified in the control group, the so-called true negative rate (TNR). To classify individuals as treated and untreated, we apply three different rules to choose a cutoff value along the probability distribution ($\hat{\Lambda}$). These cutoff rules determine the probability threshold for each individual to be classified as treated or untreated. The rules for setting the classification cutoff are as follows:

1. Minimization of the mean error (ME), which is the sum of the true positive and negative rates; i.e., $ME = TPR + TNR$.
2. Setting the error rate (ER) to unity, where the ER is the ratio of the TPR to the TNR; i.e., $ER = \frac{TPR}{TNR}$. The rationale here is to achieve a balance between both classification rates.

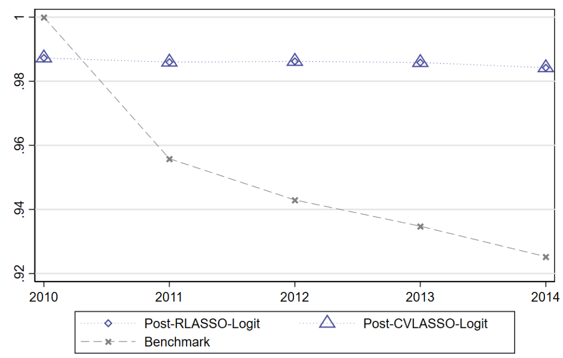
Figure 1.C1 shows the prediction performance when we use the minimization of ME as the cutoff rule. Looking at the true positive rate in Panel (a), the fraction of true positives substantially falls in our benchmark (TCPT bite). However, LASSO-logit prediction is even worse, showing a lower rate of true positives in all prediction years. In contrast, Panel (b) shows that the prediction of true negatives is fairly good, as the LASSO-logit outperforms the benchmark from $t + 1$ onward. In combination, the mean error of LASSO-logit prediction almost equals the TCPT bite in $t + 4$, as shown in Panel (c), and is likely to decrease below the error of the benchmark in the periods after that.

Figure 1.C2 shows the prediction performance when using $ER = 1$ to set the cutoff in the treatment classification. The results show lower TNR rates (Panel (b)), while TPR rates remain constant at approximately 0.72 (Panel (a)). In sum, however, the mean error rates of the LASSO predictions (Panel (c)) are at even higher levels than in the $ER = 1$ specification described above, and our predictions are not able to surpass those of the TCPT bite over the prediction period.

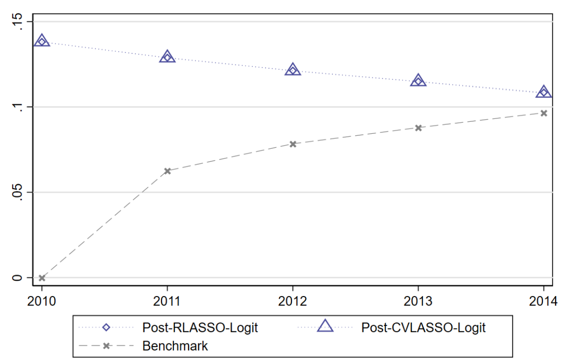
Figure 1.C1: Prediction Performance of LASSO-Based Incidence Bite Predictions and the Benchmark Using the $min(ME)$ Cutoff Rule



(a) True positive rate



(b) True negative rate



(c) Mean error

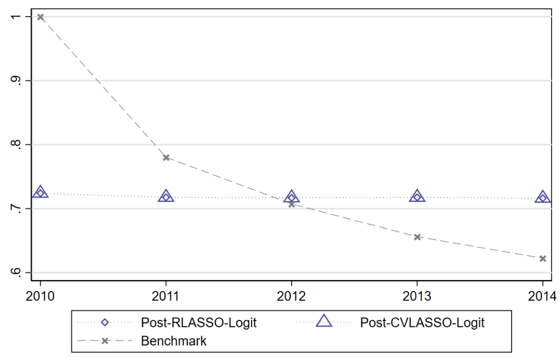
NOTE: Error measures for post-LASSO-logit predictions and time-constant pre-treatment bites. The training sample consists of all observations in 2010. The validation sample comprises all observations in the years 2011-2014.

Table 1.C1: Error Decomposition for Post-LASSO-Logit Using the $min(ME)$ Cutoff Rule

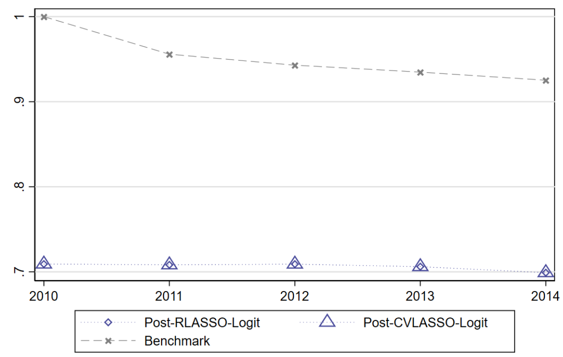
	Post-R-Lasso-Logit				Post-CV-Lasso-Logit			
	MAE	Bias	Sq. Bias	Var	MAE	Bias	Sq. Bias	Var
2010	1.1263	-0.9030	(0.1001)	1.0263	1.1526	-1.1284	(0.1562)	0.9964
2011	1.1322	-0.8690	(0.0862)	1.0460	1.1392	-1.1100	(0.1407)	0.9985
2012	1.1242	-0.8516	(0.0784)	1.0457	1.1283	-1.0995	(0.1308)	0.9975
2013	1.1294	-0.8278	(0.0701)	1.0593	1.1209	-1.0885	(0.1212)	0.9997
2014	1.1482	-0.7822	(0.0583)	1.0899	1.1104	-1.0719	(0.1095)	1.0009

NOTE: Mean absolute error, (squared) bias and variance for post-LASSO-logit estimations from the R-LASSO and CV-LASSO models. The values are normalized by dividing them by the observed standard deviation in the data in each year to account for differing variances in the observed bite over the years.

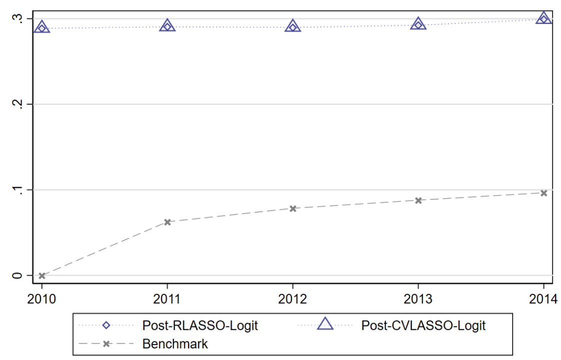
Figure 1.C2: Prediction Performance of LASSO-Based Incidence Bite Predictions and the Benchmark Using the $ER = 1$ Cutoff Rule



(a) True positive rate



(b) True negative rate



(c) Mean error

NOTE: Error measures for post-LASSO-logit predictions and time-constant pre-treatment bites. The training sample consists of all observations in 2010. The validation sample comprises all observations in the years 2011-2014.

Table 1.C2: Error Decomposition for Post-LASSO-Logit Using the $ER = 1$ Cutoff Rule

	Post-R-Lasso-Logit				Post-CV-Lasso-Logit			
	MAE	Bias	Sq. Bias	Var	MAE	Bias	Sq. Bias	Var
2010	2.3516	1.7065	(0.3573)	1.9942	3.1609	2.7991	(0.9614)	2.1996
2011	2.5439	1.8934	(0.4093)	2.1346	3.4446	3.0807	(1.0836)	2.3610
2012	2.6768	2.0304	(0.4459)	2.2309	3.6394	3.2663	(1.1540)	2.4854
2013	2.8578	2.2180	(0.5032)	2.3547	3.8859	3.5177	(1.2657)	2.6202
2014	3.1393	2.5034	(0.5973)	2.5420	4.2489	3.8716	(1.4286)	2.8202

NOTE: Mean absolute error, (squared) bias and variance for post-LASSO-logit estimations from the R-LASSO and CV-LASSO models. The values are normalized by dividing them by the observed standard deviation in the data in each year to account for differing variances in the observed bite over the years.

1.D Restriction to Monthly Wages Below €1,775 for Incidence Bite Prediction

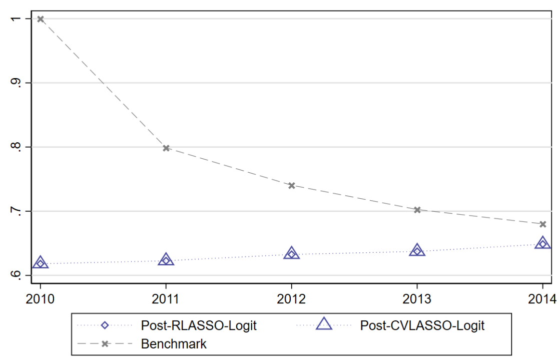
The initial minimum wage level in Germany was €8.50 per working hour. The maximum number of legal working hours worked permanently is 48 per week in Germany. Hence, all individuals with monthly wages above €1,775 should not be affected by the minimum wage since their hourly wage is above €8.50, even if they are working a maximum of 48 hours per week, where on average, one month has 4.35 weeks (8.50 * 48 * 4.35=1774.8). Excluding wages above that threshold from our estimation sample for the incidence bite prediction could improve performance mainly for two reasons. First, this should lead to a much less skewed distribution between treated and untreated individuals, which decreases the severe impact of the cutoff rule choice on the prediction outcome (see Appendix 1.C). Second, the scope of the prediction slightly changes, as we do not try to predict minimum wage bites for the full population but only for those individuals who could be affected by the minimum wage in general, based on their observed monthly wage.[34]

The results for this specification are presented in Figure 1.D1 and show various differences compared to the main specification using the full sample. At first glance, the performance appears to be worse overall because all three measures deteriorate. We see a lower TPR at approximately 0.62 (Panel (a)), a lower TNR at approximately 0.62 (Panel (b)), and a higher ME at approximately 0.38 (Panel (c)) compared to the main specification (see Section 1.5). However, the low mean error when using the full sample is mainly caused by the high (absolute) number of correctly classified negatives who are high-wage earners. Compared to the development of our benchmark (TCPT bite), the performance of the predictions increases over time. The TPR and TNR of the benchmark decrease quite quickly, which shows that strong dynamics are present in this subsample and that individuals tend to change treatment statuses more often. This finding underlines the importance of reclassifying the minimum wage bite, especially for long-run analyses.

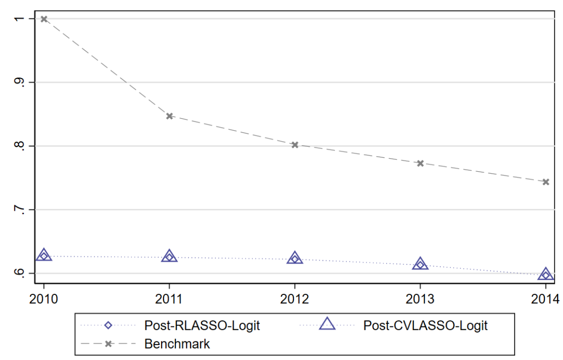
[34]From a theoretical perspective, the sample restriction assumes that the minimum wage does not cause large spillovers, which would lift directly affected individuals above the monthly wage threshold of €1,775.

The MAE decomposition in Table 1.D1 reveals a few more details of our predictions from the restricted sample. It is noticeable that the MAE is mainly caused by the variance in the error term. The bias of our predictions is much lower than that for the estimations using the full sample. We observe only slight increases in the MAE over the years, which again shows a good generalization performance of our predictions.

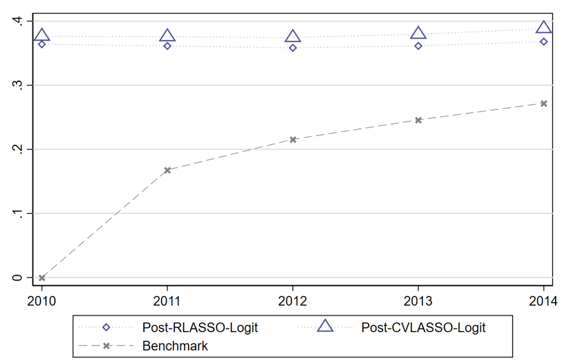
Figure 1.D1: Prediction Performance of LASSO-Based Incidence Bite Predictions and the Benchmark for Monthly Wages below €1,775 Using the $max(AUC)$ Cutoff Rule.



(a) True positive rate



(b) True negative rate



(c) Mean error

Note: Error measures for post-LASSO-logit predictions and time-constant pre-treatment bites. The training sample consists of all observations in 2010. The validation sample comprises all observations in the years 2011-2014. The sample is restricted to monthly wages below €1,775. The $max(AUC)$ cutoff rule is used.

Table 1.D1: Error Decomposition for Post-LASSO-Logit Using AUC Cutoff Rule for Monthly Wages below €1,775

	Post-R-Lasso-Logit				Post-CV-Lasso-Logit			
	MAE	Bias	Sq. Bias	Var	MAE	Bias	Sq. Bias	Var
2010	1.6197	0.4089	(0.0389)	1.5808	1.6172	0.4075	(0.0386)	1.5786
2011	1.6807	0.5347	(0.0642)	1.6166	1.6743	0.5311	(0.0633)	1.6110
2012	1.7206	0.6341	(0.0878)	1.6328	1.7145	0.6307	(0.0869)	1.6276
2013	1.7876	0.7364	(0.1153)	1.6723	1.7857	0.7400	(0.1164)	1.6693
2014	1.8981	0.9091	(0.1692)	1.7289	1.8964	0.9105	(0.1698)	1.7267

NOTE: Mean absolute error, (squared) bias and variance for post-LASSO-logit estimations from the R-LASSO and CV-LASSO models. The values are normalized by dividing them by the observed standard deviation in the data in each year to account for differing variances in the observed bite over the years.

1.E Descriptive Statistics of Predicted Bites

Table 1.E1: Predicted Bite Gaps

Bite-Gap		2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020
R-LASSO	Mean	0.25	0.25	0.25	0.25	0.26	0.26	0.26	0.27	0.27	0.27	0.26
	Std.Dev	0.31	0.31	0.31	0.31	0.32	0.32	0.32	0.33	0.33	0.33	0.31
	Min	-0.24	-0.24	-0.22	-0.22	-0.22	-0.22	-0.22	-0.20	-0.20	-0.22	-0.22
	Max	2.25	2.25	2.25	2.25	2.25	2.25	2.25	2.25	2.25	2.25	2.25
CV-LASSO	Mean	0.25	0.25	0.25	0.25	0.26	0.26	0.26	0.27	0.27	0.27	0.26
	Std.Dev	0.31	0.31	0.31	0.31	0.32	0.32	0.32	0.33	0.33	0.33	0.31
	Min	-0.27	-0.27	-0.25	-0.25	-0.25	-0.27	-0.25	-0.25	-0.25	-0.25	-0.25
	Max	2.26	2.26	2.26	2.26	2.26	2.26	2.26	2.26	2.26	2.26	2.26
Observations		572,509	585,715	592,856	600,576	609,196	617,094	627,076	637,117	648,855	654,319	639,525

NOTE: The table shows the descriptive statistics of post-LASSO-OLS-based predictions for the bite gap excluding wages as explanatory variables. The training year for the model is 2014.

Table 1.E2: Predicted Incidence Bites

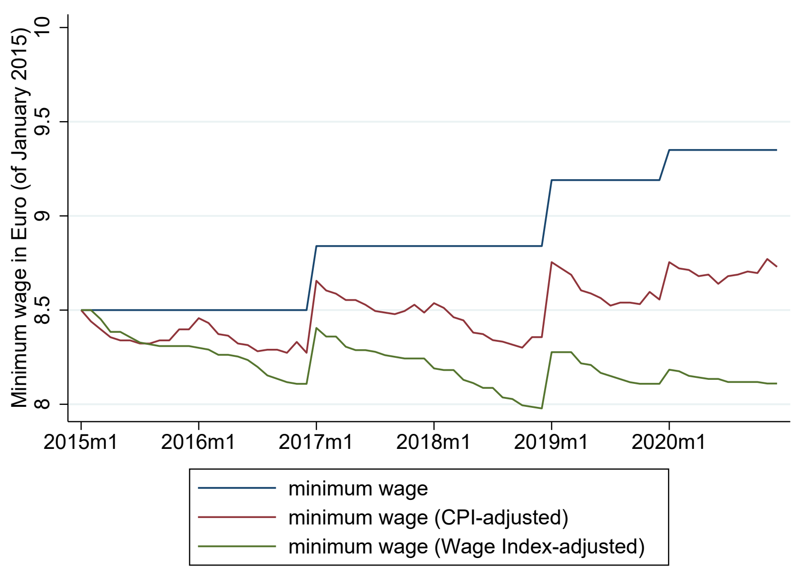
Incidence Bite		2010	2011	2012	2013	2014	2015	2016	2017	2018	2019	2020
R-LASSO	Mean	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11
	Std.Dev	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.12	0.12	0.12	0.11
	Min	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	Max	0.93	0.93	0.93	0.91	0.93	0.93	0.93	0.91	0.91	0.92	0.91
CV-LASSO	Mean	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11
	Std.Dev	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.11	0.12	0.12	0.11
	Min	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
	Max	0.93	0.93	0.93	0.92	0.93	0.93	0.93	0.91	0.92	0.92	0.92
Observations		572,569	585,771	592,917	600,642	609,260	617,169	627,155	637,196	648,922	654,396	639,612

NOTE: The table shows the descriptive statistics of post-LASSO-logit-based predictions for the incidence bite excluding wages as explanatory variables. The values are derived from predicted probabilities of the logit model. The training year for the model is 2014.

1.F Minimum Wage Development

In this appendix, we show the nominal development of the minimum wage along with the real levels of the minimum wage. Figure 1.F1 illustrates the development of the German national minimum wage, which was introduced at a nominal hourly wage level of €8.50 and then increased stepwise to €9.35 by 2020. If we deflate the nominal minimum wage by the consumer price index (CPI) of the German Statistical Office, as depicted by the red line, the resulting real minimum wage level only increased very slightly. In a final exercise, we deflate the minimum wage by the Wage Index of the German Statistical Office, which aims to reflect the development of the average wages of all regular jobs in the economy. When we adjust the minimum wage for this development of average wages, the resulting green line shows a slightly falling trend, which implies that average wages increased more than the minimum wage, although the difference is small.

Figure 1.F1: Price- and Wage-Adjusted Minimum Wage Development



Note: Development of the nominal minimum wage, as proposed by the German Minimum Wage Commission (blue line). CPI-deflated minimum wage (red line), where the CPI is from the German Statistical Office. Wage Index-deflated minimum wage (green line), where the Wage Index depicts the development of gross hourly wages and is calculated by the German Statistical Office. All curves start at €8.50 in January 2015.

Wage Index deflation is interesting, as it reflects the relative increase in the minimum wage (compared to the average wage). This comparison mimics the intuition of the difference-in-differences approach, which in turn compares the minimum wage worker to all other workers. While this comparison is not exactly the same since minimum-wage-induced wage increases may themselves influence the average wage, it sheds some light on how the genuine relative wage effect of the minimum wage may evolve over time. From the development of the Wage-Index-deflated green line, we would not expect the wage effect of the minimum wage to increase over time when comparing the affected workers with the counterfactual wage development of all other workers, such as in a difference-in-differences setting.

1.G Placebo Tests for Minimum Wage Effect Estimations Based on the Bite Gap

Table 1.G1: Minimum Wage Effects on Wages – Bite Gap

	TCPT-bite	**predicted-bite**	
		R-LASSO	CV-LASSO
	(1)	(2)	(3)
	Log real wage	Log real wage	Log real wage
$bite * year_{2010}$	-0.001	-0.008	-0.008
	(0.003)	(0.005)	(0.005)
$bite * year_{2011}$	0.001	0.005	0.005
	(0.002)	(0.005)	(0.005)
$bite * year_{2012}$	-0.001	-0.006	-0.005
	(0.002)	(0.005)	(0.005)
$bite * year_{2013}$	-0.002	-0.011**	-0.011**
	(0.002)	(0.005)	(0.005)
$bite * year_{2015}$	0.079***	0.037***	0.037***
	(0.002)	(0.005)	(0.005)
$bite * year_{2016}$	0.134***	0.033***	0.033***
	(0.002)	(0.005)	(0.005)
$bite * year_{2017}$	0.190***	0.047***	0.046***
	(0.002)	(0.005)	(0.005)
$bite * year_{2018}$	0.242***	0.045***	0.044***
	(0.002)	(0.005)	(0.005)
$bite * year_{2019}$	0.292***	0.045***	0.044***
	(0.002)	(0.005)	(0.005)
$bite * year_{2020}$	0.337***	0.030***	0.029***
	(0.002)	(0.005)	(0.005)
Observations	4,658,218	6,784,838	6,784,838

NOTE: The estimates are the treatment effects of the minimum wage on log wages from difference-in-differences estimations at the individual level. The independent variables are the predefined (Column 1) and the predicted (Columns 2 and 3) bite gaps in each of the pre-treatment years 2010 to 2013 and post-treatment years 2015 to 2020. 2014 is the base period. The coefficients for the pre-treatment years are placebo tests. The numbers of observations between the benchmark (TCPT bite gap) and predictions differ because the TCPT bite gap only captures pre-existing jobs, while the prediction also includes labor market entrants. Base year of the LASSO model: 2014. Robust standard errors are in parentheses. Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1.

1.H Minimum Wage Effects on Wages Based on the Incidence Bite

Table 1.H1: Minimum Wage Effects on Wages – Incidence Bite – Based on Predicted Probabilities

	TCPT-bite	**predicted-bite**	
		R-LASSO	CV-LASSO
	(1)	(2)	(3)
	Log real wage	Log real wage	Log real wage
2015 - 2014	0.291***	0.125***	0.127***
	(0.006)	(0.015)	(0.015)
2016 - 2014	0.474***	0.144***	0.146***
	(0.007)	(0.018)	(0.018)
2017 - 2014	0.656***	0.188***	0.185***
	(0.008)	(0.020)	(0.020)
2018 - 2014	0.820***	0.226***	0.224***
	(0.009)	(0.023)	(0.023)
2019 - 2014	0.986***	0.230***	0.230***
	(0.010)	(0.026)	(0.026)
2020 - 2014	1.137***	0.259***	0.255***
	(0.012)	(0.029)	(0.029)
Observations	4,658,218	6,785,609	6,785,609

NOTE: The estimates are the treatment effects of the minimum wage on log wages from difference-in-differences estimations. The independent variables are the predefined incidence bites and predicted probabilities of the incidence bites in each of the years 2015 to 2020. The numbers of observations between the benchmark (TCPT bite) and predictions differ because the TCPT bite only captures pre-existing jobs, while the prediction also includes labor market entrants. Base year of the LASSO model: 2014. Robust standard errors are in parentheses. Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1.

Table 1.H2: Minimum Wage Effects on Wages – Incidence Bite – Based on the AUC Cutoff-Rule

	TCPT-bite	**predicted-bite**	
		R-LASSO	CV-LASSO
	(1)	(2)	(3)
	Log real wage	Log real wage	Log real wage
2015 - 2014	0.291***	0.019***	0.018***
	(0.006)	(0.003)	(0.003)
2016 - 2014	0.474***	0.019***	0.017***
	(0.007)	(0.004)	(0.004)
2017 - 2014	0.656***	0.022***	0.019***
	(0.008)	(0.004)	(0.004)
2018 - 2014	0.820***	0.023***	0.020***
	(0.009)	(0.005)	(0.005)
2019 - 2014	0.986***	0.021***	0.017***
	(0.010)	(0.006)	(0.006)
2020 - 2014	1.137***	0.062***	0.058***
	(0.012)	(0.006)	(0.006)
Observations	4,658,218	6,785,609	6,785,609

NOTE: The estimates are the treatment effects of the minimum wage on log wages from difference-in-differences estimations. The independent variables are the predefined incidence bites and predicted incidence bites based on the AUC cutoff rule in each of the years 2015 to 2020. The numbers of observations between the benchmark (TCPT bite) and predictions differ because the TCPT bite only captures pre-existing jobs, while the prediction also includes labor market entrants. Base year of the LASSO model: 2014. Robust standard errors are in parentheses. Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1.

Table 1.H3: Minimum Wage Effects on Wages – Incidence Bite – Only Wages Below €1,775, Based on Predicted Probabilities

	TCPT-bite	**predicted-bite**	
		R-LASSO	CV-LASSO
	(1)	(2)	(3)
	Log real wage	Log real wage	Log real wage
2015 - 2014	0.265***	0.160***	0.160***
	(0.006)	(0.017)	(0.017)
2016 - 2014	0.407***	0.095***	0.103***
	(0.007)	(0.020)	(0.020)
2017 - 2014	0.530***	0.083***	0.098***
	(0.008)	(0.023)	(0.023)
2018 - 2014	0.632***	0.058**	0.081***
	(0.010)	(0.026)	(0.026)
2019 - 2014	0.741***	0.039	0.066**
	(0.011)	(0.030)	(0.030)
2020 - 2014	0.832***	0.129***	0.156***
	(0.012)	(0.034)	(0.033)
Observations	1,255,586	2,361,978	2,361,978

NOTE: The estimates are the treatment effects of the minimum wage on log wages from difference-in-differences estimations. The independent variables are the predefined incidence bites and predicted probabilities of the incidence bites in each of the years 2015 to 2020. The numbers of observations between the benchmark (TCPT bite) and predictions differ because the TCPT bite only captures pre-existing jobs, while the prediction also includes labor market entrants. Base year of the LASSO model: 2014. Robust standard errors are in parentheses. Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1.

Table 1.H4: Minimum Wage Effects on Wages – Incidence Bite – Only Wages Below €1,775, Based on the AUC Cutoff-Rule

	TCPT-bite	**predicted-bite**	
		R-LASSO	CV-LASSO
	(1)	(2)	(3)
	Log real wage	Log real wage	Log real wage
2015 - 2014	0.265***	0.038***	0.038***
	(0.006)	(0.005)	(0.005)
2016 - 2014	0.407***	0.022***	0.025***
	(0.007)	(0.006)	(0.006)
2017 - 2014	0.530***	0.022***	0.027***
	(0.008)	(0.007)	(0.006)
2018 - 2014	0.632***	0.015**	0.022***
	(0.010)	(0.007)	(0.007)
2019 - 2014	0.741***	0.009	0.018**
	(0.011)	(0.008)	(0.008)
2020 - 2014	0.832***	0.033***	0.046***
	(0.012)	(0.009)	(0.009)
Observations	1,255,586	2,361,978	2,361,978

Note: The estimates are the treatment effects of the minimum wage on log wages from difference-in-differences estimations. The independent variables are the predefined incidence bites and predicted incidence bites based on the AUC cutoff rule in each of the years 2015 to 2020. The numbers of observations between the benchmark (TCPT bite) and predictions differ because the TCPT bite only captures pre-existing jobs, while the prediction also includes labor market entrants. Base year of the LASSO model: 2014. Robust standard errors are in parentheses. Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1.

Chapter 2

Fueling Frictions: Minimum Wage Effects on Job Vacancies

Abstract[*]

This study analyzes the impact of Germany's 2015 statutory minimum wage introduction on unmatched labor demand and search frictions. Comprehensive administrative data on the universe of registered vacancies enables the precise calculation of the flows, durations, and stocks of vacancies. I employ difference-in-differences models to estimate effects on the occupational level, measuring minimum wage exposure based on hiring wages. The findings reveal a substantial increase in search frictions, as evidenced by longer search durations and more aborted searches. Analyses of worker transitions indicate that these effects are driven primarily by changes in firms' screening intensity rather than workers' application behavior.

JEL Classification: J31, J38, J63

Keywords: minimum wage, job vacancies, labor demand, search frictions

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2.1 Introduction

The impact of minimum wage policies on labor markets has long been a central topic in economic research and political debates, with a primary focus on equilibrium outcomes such as employment levels and the wage distribution. Various empirical minimum wage studies have identified employment effects that are near zero or at least much lower than the theory on competitive labor markets implies (e.g., Allegretto et al., 2011; Card and Krueger, 1994; Cengiz et al., 2019; Dube et al., 2010; Dube et al., 2016; Harasztosi and Lindner, 2019). However, small effects on realized employment do not rule out that a binding minimum wage leads to adjustment reactions in both labor supply and demand that affect the matching process itself but not necessarily its equilibrium outcomes.

Recent empirical studies provide evidence that firms adapt to minimum wage policies using the margin of hires rather than layoffs (Bossler and Gerner, 2020; Gopalan et al., 2021; Jardim et al., 2018) or tightening their hiring standards (Butschek, 2022; Clemens et al., 2021).[35] Other studies have revealed evidence of worker reallocation (Dustmann et al., 2022), a reduction in labor turnover (Dube et al., 2016) and decreased occupational mobility (Liu, 2022). Such responses can not only increase labor market imbalances but also induce frictions, thereby affecting matching efficiency.

This paper leverages unique vacancy data to analyze the effects of Germany's 2015 statutory minimum wage introduction on unrealized labor demand and the amount of friction present in matching processes. A novel dataset on the universe of vacancies registered at the Federal Employment Agency allows me to accurately measure flows of vacancy openings and closings, stocks of pending vacancies, and vacancy durations for occupational groups in Germany. Although closely related, these variables may reveal different impact channels of the minimum wage on labor market matching beyond the realized part of labor demand.

I analyze changes in vacancy flows to uncover (temporary) imbalances in labor supply and demand. Short-term hikes in vacancy openings may indicate transitory workforce restructuring efforts, whereas permanent shifts in the number of vacancy openings can point to

[35] Throughout this study, I use the terms 'firm' and 'establishment' interchangeably to refer to local units of a company in my data.

structural changes in unfilled labor demand, such as labor substitution or shifts in turnover rates. The time needed to fill an open position, on the other hand, influences how costly and time-consuming the matching process is. Hence, the duration of vacancies reflects the extent of frictions in the labor market. Shifts in labor demand and supply, whether temporary or structural, can affect these frictions. Nevertheless, even if the number of vacancies and job seekers remains unchanged, frictions may vary solely due to adjustments in workers' search behavior or firms' hiring criteria, thereby affecting the efficiency of job matching.

On the basis of search-and-matching theory (Mortensen and Pissarides, 1994), the minimum wage effects on vacancies are twofold. First, a direct effect on vacancy postings arises from changes in firms' hiring decisions. Depending on the underlying labor market model, firms may either reduce or expand their production due to the minimum wage. In a labor market characterized by perfect competition, a binding minimum wage inevitably leads to a reduction in production in affected firms and negative employment effects (Stigler, 1946). In a monopsonistic labor market, on the other hand, the profit-maximizing output level can increase due to higher wage costs induced by the minimum wage (Manning, 2003). An expansion of production can then lead to the creation of new jobs, thus increasing the number of vacancy openings.

Second, indirect effects stem from the impact of the minimum wage on labor supply and matching efficiency. From the perspective of labor supply, a minimum wage can reduce the incentive for affected workers to search for other, better-paid positions, leading to a decrease in labor turnover. This mechanism can decrease the number of job vacancies, as fewer replacement hires become necessary. At the same time, firms may face greater difficulties in filling vacant positions because the pool of applicants decreases, possibly increasing the duration of vacancies. A minimum wage policy may encourage more unemployed individuals to re-enter the labor market if the minimum wage lies above their reservation wage. Consequently, more workers could apply for available positions, thereby reducing the duration of job vacancies.

From the perspective of labor demand, a binding minimum wage increases labor costs for affected firms. When hiring new employees, firms may therefore be inclined to ensure that they select the most productive candidate. This approach can lead to more intensive applicant screening and stricter hiring standards (Butschek, 2022; Clemens et al., 2021; Gürtzgen et al., 2016), thereby prolonging not only the duration of hiring processes but also the overall duration of job vacancies. Increasing vacancy durations can, in turn, mechanically increase the stock of pending vacancies.

To identify the effects of the German minimum wage introduction, I compare vacancy flows, durations, and stocks for 372 occupational groups between 2013 and 2019 via difference-in-differences models. For each occupation, I use administrative data on individual wages to measure minimum wage exposure based on the share of new hires below the minimum wage before its introduction. I exploit variation in minimum wage exposure at the occupational level, as the vacancy data do not provide information on (offered) wages, which prevents the determination of minimum wage exposure at the level of vacancies.

In the empirical analysis, I first confirm that the German minimum wage is binding at the occupational level by estimating its effect on monthly wages. Furthermore, the introduction of the wage floor did not substantially reduce total employment at the occupational level, in accordance with regional and firm-level studies (e.g. Ahlfeldt et al., 2018; Bossler and Gerner, 2020; Dustmann et al., 2022).

In my vacancy analysis, I first estimate the impact of the minimum wage introduction on new vacancy openings. The results show no substantial change in the vacancy creation of affected occupations. However, distinguishing between filled and canceled vacancies reveals that the share of canceled vacancies rose by 4–9 percent for occupations with an average level of exposure to the minimum wage, indicating increased frictions in employers' search for workers. The subsequent analysis of vacancy durations confirms the hypothesis of increased frictions, as the durations of newly opened vacancies in 2016 and 2017 increased by approximately 5–6 percent in occupations with average minimum wage exposure compared to 2013. Additional analyses at the quarterly level show a lengthening of new vacancies from as early as the third quarter of 2014, indicating anticipation effects after the legislative decision for the minimum wage introduction. In the final part of the vacancy analyses, I estimate effects on the job vacancy rate, which represents the share of unsatisfied labor demand in total labor demand. The analysis summarizes the effects of vacancy openings and durations, both of which affect the vacancy stocks. In the year of minimum wage introduction, the estimated average treatment effect on the job vacancy rate is approximately 4 percent.

To investigate the mechanisms underlying the frictional effects on vacancies, I subsequently estimate minimum wage effects on worker transitions between employers, both within and across occupations. The results indicate lower employee turnover, as workers change jobs less frequently and more often stay in their firm within the same occupation. In addition to job-to-job transitions, the introduction of minimum wage also reduced turnover rates between unemployment and employment.

This study contributes to the understanding of minimum wage effects in several ways. First, it adds to the literature on minimum wages by highlighting vacancies as an alternative adjustment channel apart from realized employment effects. Vacancies can indicate minimum wage-related adjustment efforts even if they are not reflected in observable changes in total employment. In the context of vacancies in a frictional labor market, my study builds on previous findings of stricter hiring standards (Butschek, 2022; Clemens et al., 2021) and more intensive applicant screening due to the presence of a minimum wage (Gürtzgen et al., 2016).

Second, the scientific literature on the effects of minimum wage on vacancies is scarce, particularly for nationwide minimum wages, such as in Germany. For the U.S., the study of Kudlyak et al. (2022) exploits state-specific variation in minimum wages between 2005 and 2018. The authors estimate a negative effect of minimum wage increases on the stock and the creation of vacancies. However, to my knowledge, no empirical study has yet analyzed the effects of a statutory national minimum wage on the creation and duration of vacancies.

Third, by additionally analyzing worker transitions, the frictional changes attributed to the matching process are related to observable matching outcomes. The findings of the current study could therefore complement the findings of other minimum wage studies, which reveal lower turnover (Bachmann et al., 2012; Brochu and Green, 2013; Dube et al., 2016; Portugal and Cardoso, 2006) and lower occupational mobility (Liu, 2022) due to minimum wages.

Fourth, the link between vacancies and transitions allows for conclusions about whether the effects on matching efficiency are predominantly supply-driven or demand-driven. Embedded in the search-and-matching framework, the results could thus contribute to a better understanding of the matching function, which is often described as a ”black box”.

The remainder of the paper is structured as follows: Section 2.2 describes theoretical adjustment margins of minimum wages depending on the underlying market form and relates them to empirical findings from the international and German minimum wage literature. Section 2.3 outlines the institutional background of the minimum wage introduction in Germany. Section 2.4 describes the administrative data used on job vacancies and individual employment histories. Section 2.5 displays the specifications of the empirical model. Section 2.6 presents estimation results of the effects of the minimum wage introduction, first on wages and employment and then on various characteristics of vacancies. Section 2.7 explores the mechanisms underlying the vacancy results by analyzing worker transitions and survey information on firms' hiring processes. Section 2.8 concludes the paper.

2.2 Literature Review

This section briefly describes the impact channels of a minimum wage in the competitive model, under monopsony power, and in the search-and-matching model framework; it also integrates results from the empirical literature to show how firms' and workers' adjustment mechanisms can affect vacancies. In competitive markets, a binding minimum wage reduces optimal labor input, decreases employment, and crowds out jobs with productivity below the minimum wage (Brown, 1999). Empirical studies show mixed effects; some find moderate negative impacts on employment (e.g., Clemens and Wither, 2019; Currie and Fallick, 1996; Neumark and Wascher, 1992), whereas others find effects close to zero (e.g., Allegretto et al., 2011; Card and Krueger, 1994; Cengiz et al., 2019; Dube et al., 2010; Dube et al., 2016; Harasztosi and Lindner, 2019). Focusing on the German minimum wage, a number of studies find minor employment effects at the firm and regional levels (e.g., Bossler and Gerner, 2020; Caliendo et al., 2018; Dustmann et al., 2022; Garloff, 2019; Schmitz, 2019). However, studies show evidence for a reduction in marginal employment (Holtemöller and Pohle, 2020; Schmitz, 2019), mainly in East Germany (Friedrich, 2020), and some conversion into regular employment (Caliendo et al., 2018; Garloff, 2019).[36]

[36]Regular employment is defined as jobs that yield earnings above 450 euros per month. This amount previously served as the threshold for "mini-jobs", which are not subject to social security contributions in Germany. This threshold was applied prior to September 30, 2022.

Limited negative employment effects might reflect firms' labor market power (Manning, 2003). In monopsonistic markets with limited competitiveness, firms can set wages below worker productivity, allowing moderate minimum wages to increase employment toward competitive levels. By quantifying monopsony through labor market concentration, empirical minimum wage research provides evidence for positive employment effects in highly concentrated labor markets (Azar et al., 2023; Popp, 2021).

Search-and-matching theory, which is based on the canonical model of Mortensen and Pissarides (1994), provides a more comprehensive view of labor market dynamics by explicitly modeling the coexistence of unemployment and job vacancies, worker flows between labor market states, and wage determination.[37] Furthermore, job creation and destruction can be endogenized. In this framework, the matching function links unemployed (and employed) job seekers to firms with vacancies. The model includes frictions such as search and vacancy costs, making job matching costly and time-consuming. The frictional nature of the labor market also enables this framework to explicitly model adjustment costs, which workers and firms incorporate in their labor supply and demand decisions. This model is, therefore, suitable for examining not only the outcomes of minimum wage-related adjustments but also the process of adjustments.[38]

From a labor demand perspective, search-and-matching theory suggests that a binding minimum wage decreases vacancy postings due to the reduced profitability of affected jobs and thus lowers expected returns from posting a vacancy.[39] Coincidently, firms may try to compensate for higher wage costs by hiring more productive workers, which might induce

[37]Major early contributions to this theoretical framework, which is also known as the DMP model, include Diamond (1982), Pissarides (1985), and Mortensen (1986) For a historical overview of the DMP model, see Pissarides (2011).

[38]While the basic model assumes homogeneous workers and jobs, various extensions introduce heterogeneity with respect to workers' skills and preferences or job qualities, and enable on-the-job searching and job-to-job movements. Various minimum wage studies have used the search-and-matching framework to identify different channels for minimum wage-related adjustments (e.g., Acemoglu, 2001; Berg and Ridder, 1998; Bontemps et al., 1999; Burdett and Mortensen, 1998; Eckstein and Kenneth, 1990; Flinn, 2006; Blömer et al., 2024)). For a comprehensive derivation of the implementation of a minimum wage in the DMP context, see Flinn (2011).

[39]The value of posting a vacancy from the firm's perspective depends on the vacancy cost (costs of posting a vacancy, screening applicants, and hiring a new worker), the arrival rate of a match for a vacancy (as a function of labor market tightness), and the expected returns from a match. The latter decreases with higher wages; hence, vacancy creation decreases with higher wages.

intensified applicant screening and stricter hiring standards. Based on establishment survey information, Gürtzgen et al. (2016) find descriptive evidence for stricter hiring criteria after the German minimum wage policy was introduced. Similarly, using worker fixed effects as a proxy for worker productivity, Butschek (2022) provides evidence for significantly increased hiring standards through more selective hiring and intensified applicant screening after the reform came into force.[40] Furthermore, he finds no evidence that productivity-driven wage increases are caused by the self-selection of workers into treated establishments. If that were the case, then affected establishments would possibly face more suitable matching opportunities, which could decrease vacancy durations. However, if the effect on hiring quality is demand driven, then one would, ceteris paribus, expect longer vacancy durations. Clemens et al. (2021) provide evidence for stricter hiring standards and skill-upgrading in the U.S. Using Burning Glass vacancy data, the authors show that federal minimum wage increases lead to a greater number of job advertisements in low-wage occupations, which require a high school diploma both within and across firms. Taken together, minimum wage-related adjustments on the demand side tend to lower vacancy creations but may prolong vacancy durations.

Opposing effects on labor supply can counteract demand-related effects. In search-bargaining models, wages are determined via Nash bargaining, which is based on workers' values of having a job or being unemployed, and firms' values of filled positions and vacancies. The Nash bargaining solution, along with an exogenous bargaining power parameter, determines how the surplus is split, making wage setting endogenous. A minimum wage overrides the bargaining solution if the bargaining wage falls below the minimum wage, giving affected workers a larger surplus share and increasing their "effective" bargaining power (Flinn, 2006). On the basis of this mechanism, minimum wage effects on the supply side depend heavily on the endogenous search effort responses of workers (Acemoglu, 2001; Flinn, 2006; Pissarides, 2000). A minimum wage can increase job-seekers' search effort by increasing expected returns to employment relative to unemployment.[41] It may also encourage nonseeking unemployed individuals to apply if the minimum wage exceeds their reservation wage (Meer and West,

[40] Worker productivity is estimated from a two-way fixed-effects wage regression, as introduced by Abowd et al. (1999) (AKM worker fixed effects).

[41] Empirical evidence on this channel is rather limited. A recent working paper by Piqueras (2023) provides empirical evidence for the U.S., estimating an increase in search effort of 6.1 percent due to a minimum wage increase of 12 percent.

2016).[42] By increasing the pool of searching workers (and their search intensity), a minimum wage can make it easier to fill vacancies despite increased hiring standards and improve matching quality. This might reduce the duration of vacancies, which in turn reduces firms' costs of filling a vacancy and might encourage them to open more vacancies.

Independent of this feedback on vacancy creation, Brown et al. (2014) show that the positive effect on workers' job acceptance rates can outweigh the negative effect on firms' job offer rates at sufficiently low minimum wages. Stemming from the labor supply channel, a minimum wage can reduce vacancy durations (and stocks) in certain circumstances.[43] Empirical studies confirm a negative correlation between entry wages and vacancy durations. Using data on vacancies reported to the Austrian Public Employment Service, Mueller et al. (2023) find that higher starting wages are associated with shorter vacancy durations. However, the authors emphasize that the estimated elasticity is economically small (-0.07– -0.21) and that other recruiting efforts, such as hiring standards, may be more important drivers of matching efficiency. Bassier et al. (2023) use U.K. job advertisement data to estimate elasticities ranging from -3 to -5 for vacancy durations with respect to wages. The authors argue that the comparatively large estimates arise from their identification strategy, which addresses potential bias from unobserved job, worker, and firm characteristics; the likely correlation of firm-level wages with the wages of competitor firms; and the possible endogeneity of wage adjustments to perceived hiring difficulties.

Minimum wages can also affect labor market dynamics. In the search-and-matching framework with endogenous job destruction, a minimum wage eliminates jobs below a certain productivity level, thereby increasing transitions into unemployment. With two-sided heterogeneity, churning flows may rise as the optimal combination of firm and worker characteristics changes. This mechanism is examined by Drechsel-Grau (2023) via a structural search-and-

[42] Using German administrative data, Blömer et al. (2024) estimate an equilibrium job search model and show that, owing to this mechanism, unemployment is a nonmonotonic function of different minimum wage levels.

[43] The canonical search-and-matching model with wage bargaining cannot account for the empirical findings of a negative correlation between wages and vacancy durations as higher wages are associated with tighter labor markets, which in turn, lead to a longer duration of vacancies. Instead, the findings are consistent with wage-posting theories of directed search (Moen, 1997), where higher wage offers attract more applicants, and with random search (Burdett and Mortensen, 1998), where higher wage offers increase the acceptance probability of workers.

matching model with endogenous job search effort and vacancy posting, multiple employment levels, a progressive tax-transfer system, and worker and firm heterogeneity. He argues that worker reallocation occurs over time when the minimum wage cuts deep into the wage distribution. Analyzing the German labor market, Dustmann et al. (2022) find evidence for sizable reallocation of workers from small, low-paying firms to larger, higher-paying firms due to the minimum wage introduction. Clemens et al. (2021) provide similar evidence for the U.S. labor market, revealing that upgraded skill requirements led firms to substitute low-skilled workers with moderately higher-skilled workers following state-level minimum wage increases between 2011 and 2016. These results illustrate scenarios in which minimum wage-related labor market adjustment processes occur without manifesting in equilibrium employment effects.

Apart from reallocation, job-to-job transitions can decrease due to a minimum wage, as such a policy compresses the wage distribution, therby reducing the expected gains from on-the-job searching (Berg and Ridder, 1998). Bachmann et al. (2012) confirms this mechanism empirically, showing reduced job-to-job transitions after the introduction of a sectoral minimum wage in the German construction sector. Other studies show evidence for dampening minimum wage effects on hirings and separations in Canada (Brochu and Green, 2013) and Portugal (Portugal and Cardoso, 2006). In the U.S., Dube et al. (2016) find a significant negative effect on employment flows after minimum wage increases, although the impact on overall employment is near zero. A recent study by Liu (2022) provides evidence for reduced occupational mobility due to minimum wage increases in the U.S., highlighting two channels in the search-and-matching framework. On the labor supply side, a minimum wage compresses wages, which reduces the gain from switching jobs and thus lowers the mobility of workers. On the labor demand side, a decrease in vacancy posting can lead to lower transition possibilities. According to Dube et al. (2007) and Dube et al. (2016), these decreases in transitions can be interpreted as a sign of improved match quality.

The partially opposing effects of a minimum wage on labor demand and labor supply show that adjustment reactions do not necessarily manifest themselves in equilibrium employment outcomes. Even without substantial employment effects, these adjustment reactions can influence not only the extent of frictions (e.g., through screening intensity) but also the efficiency of matching in the labor market.

While employment effects and changes in worker flows reflect the outcome of adjustments in the labor demand and supply decision of firms and individuals, looking at vacancies and their durations as unrealized labor demand allows to observe adjustment processes directly, even if they do not ultimately materialize in changes in total employment. To my knowledge, the first article to empirically evaluate the effect of a minimum wage on vacancy postings directly was Kudlyak et al. (2022). Using quarterly county-level vacancy data and state-level minimum wage variation between 2005 and 2018, the authors estimate a short-run elasticity of vacancy stocks to minimum wage increases of -0.24 in "at-risk" occupations relative to other occupations. The binary treatment measure identifies "at-risk" occupations as those in which at least 5 percent of workers earn at or below 110 percent of the minimum wage.

2.3 Institutional Background

In Germany, the introduction of a nationwide minimum wage marked a significant development in labor market regulation. Prior to its introduction, Germany relied primarily on collective bargaining agreements between employers' associations and trade unions to determine wage levels in various industries and regions. The existence of wage floors had long been ensured through extensive coverage of collective agreements, which effectively regulated wages (Bachmann et al., 2014). However, over the past 25 years, there has been a notable decrease in collective bargaining coverage, accompanied by a rise in wage inequality among low-wage workers (Dustmann et al., 2009).

Before the introduction of the nationwide minimum wage, sector-specific minimum wages had already been stipulated across a wide range of sectors. These sector-specific wage floors were not established by the government directly; rather, they originated from collective bargaining agreements made within each sector. The Federal Ministry of Labor and Social Affairs (BMAS) declared these collective bargaining agreements to be universally binding, ensuring their widespread application.[44]

[44]The Posting of Workers Directive of 1996, which is also known as the "Arbeitnehmer-Entsendegesetz", allowed the Federal Ministry of Labor and Social Affairs to extend industry-wide collective bargaining contracts negotiated between worker unions and employers' associations to cover all employees within the relevant industry, irrespective of whether the firm was affiliated with the employer association. The extension of the wage floor to all firms within a sector thus resulted in a sectoral minimum wage.

In the campaign leading up to the 2013 German federal election, the minimum wage was one of the most prominent and controversial topics. In the coalition agreement following the 2013 federal elections, the two coalition partners of the Social Democratic Party (SPD) and the Conservative Union (CDU/CSU) eventually agreed to introduce a nationwide statutory minimum wage of 8.50 euros per working hour. The necessity of such a policy was justified by decreasing collective bargaining coverage. Following its implementation on January 1, 2015, this minimum wage applied to the majority of workers, with only a few exceptions.[45] As a result, the reform affected approximately 10 to 14 percent of the entire workforce who were previously paid below the new wage floor (Caliendo et al., 2019).

The minimum wage has been increased several times since its introduction (see Appendix Figure 2.A1 for illustration). These gradual increases followed the recommendations of the Minimum Wage Commission from 2017 to 2022. On October 1, 2022, a substantial increase to 12 euros per hour was mandated by law. Subsequent gradual increases scheduled for 2024 and 2025 are intended to follow the recommendations of the Minimum Wage Commission. While substantial increases, especially those that occurred in 2022, indicate a need for further research in future studies, the focus of this paper is on the effects of the minimum wage introduction within the 2015–2019 period. This focus ensures that potential adverse effects from the COVID-19 crisis are excluded. The increases that occured in the years 2017 and 2019 can be considered separate treatments, although their effects may overlap with the estimated introduction effect of the minimum wage in the years following 2017. However, when adjusted for inflation, the increases amount to only 1 percent (2017) and 0.7 percent (2019), resulting in small effects expected from these minimum wage adjustments during the analysis period.

2.4 Data

I use administrative data from two different sources, both of which stem from the Federal Employment Agency (FEA), for my empirical analysis. First, I leverage a new dataset of all

[45] Exceptions were applied to individuals aged younger than 18 without vocational training, apprentices, interns and long-term unemployed persons for six months in their first job after unemployment. Temporary exemptions were applied in specific occupations to the meat industry, hairdressing, agriculture, forestry, gardening, the textile industry, dry cleaning, postal services and temporary agency work in East-Germany (for details, see Mindestlohnkommission, 2016, p. 155).

vacancies reported to the FEA to calculate vacancy inflows, outflows, durations, and stocks. However, the vacancy data lack offered wage information; hence, they do not reveal whether vacancies are affected by the minimum wage. To overcome this issue, I use data on individual employment histories (IEB) from the Institute for Employment Research (IAB) to measure the minimum wage exposure at the occupational level based on wage information for new hires. The IEB data also help to calculate worker flows between occupations, which serve to estimate minimum wage effects on labor market dynamics.

I aggregate both datasets at the level of occupational groups, additionally differentiating by skill requirement levels. Occupations follow the German classification of occupations (KldB 2010, see Bundesagentur für Arbeit, 2011), which uses a five-digit code to distinguish between 1,286 different occupations. The first four digits represent the area of expertise with increasing levels of detail, and the fifth digit indicates the skill requirement level in four categories (Matthes and Paulus, 2013).[46] Owing to the detailed classification, few observations exist for many 5-digit-level occupations. To address this, I aggregate occupations to the 3-digit level by expertise and the fifth digit by skill level, resulting in 431 distinct 3+5-digit occupations.[47] While the primary temporal aggregation is annual, some specifications use quarterly aggregation. Both datasets are merged by 3+5-digit occupations and the time identifier (year or quarter).

The time series of the IEB data shows a structural break concerning the classification of occupations between 2011 and 2012 due to the adoption of a new occupational classification scheme (KldB 2010). Owing to methodological changes, the new occupational codes cannot be transcoded to the old ones, thereby making meaningful comparisons at the occupational level before and after adoption impossible. The adoption of the new classification scheme caused inaccuracies in the occupation variable until the end of 2012. Therefore, my analysis sample starts in 2013. See Appendix 2.B for details on the reclassification of occupational codes and the resulting structural break.

[46]The four levels of skill requirements are as follows: 1 - Unskilled or semiskilled activities (Helper), 2 - Specialist activities (Professional), 3 - Complex specialist activities (Specialist), and 4 - Highly complex activities (Expert).

[47]This aggregation has been used in other studies, e.g., Friedrich, 2020.

Section 2.4.1 briefly describes the structure of the vacancy dataset and the key steps taken to identify the opening, duration, and closing of vacancies. I refer the reader to Appendix 2.C for details. Section 2.4.2 briefly describes the measure of minimum wage exposure. Appendix 2.D contains further details on the IEB dataset and the identification of worker flows used for complementary analyses in Section 2.7.

2.4.1 Preparation of Vacancy Data

The vacancy dataset includes approximately 13 million usable vacancy observations for jobs with placement orders reported to the FEA between 2013 and 2019.[48] It contains information on occupation, social security status, vacancy opening and closing dates, the intended start of employment, and whether a vacancy was filled or canceled.

This information helps to differentiate between successfully filled and canceled vacancies. Canceled vacancies occur when the job no longer exists or when the employer deliberately stops searching via the FEA placement system. A vacancy is filled when the employer informs the FEA of a successful hire, regardless of the search channel used (Bundesagentur für Arbeit, 2018).[49] Some data inaccuracies can occur in the distinction between canceled and filled vacancies. FEA quality reports indicate that positions filled through other channels may be mistakenly categorized as canceled if the employer does not inform the FEA of using another hiring channel (Bundesagentur für Arbeit, 2018).

I calculate the inflow (newly opened) and outflow (closed) of vacancies for each occupation on the basis of daily spell data. These flow variables are sum-aggregated both quarterly and annually. Hence, the quarterly (yearly) inflow of vacancies represents the sum of all newly opened vacancies within a quarter (year). The stock of vacancies includes all pending vacancies at the 15$^{\text{th}}$ day of each month, and the quarterly (yearly) values represent means over the months of a quarter (year).

[48] I use observations for jobs subject to social security contributions and marginal employment jobs but exclude vacancies for apprenticeship positions and other forms of employment.

[49] In contrast to data obtained from the Austrian vacancy register, which is used in the study by Mueller et al. (2023), the FEA data cannot be linked to the social security data of a person who is successfully hired via a placement order.

The evolution of the total vacancy stock exhibits a similar trend to that of the officially reported vacancy stocks by the FEA (see Appendix Figure 2.C1). The level is slightly below the official numbers, which can be attributed to a series of filters applied during the processing stage to plausibly distinguish individual vacancies from job offers that encompass multiple vacancies (see Appendix 2.C for details). The share of filled vacancies among all vacancies decreased from approximately 59 percent in 2013 to approximately 51 percent in 2019. Hence, almost one out of two open vacancies in 2019 was ultimately canceled.

Measuring the Duration of Vacancies

The key advantage of the vacancy dataset is its detailed timing information; the data include the dates when a vacancy was opened (t_{open}), closed (t_{close}), and the earliest intended employment start (t_{start}). These variables enable the calculation of different measures for vacancy durations. According to the Bureau of Labor Statistics (BLS) in the Job Openings and Labor Turnover Survey (JOLTS), a position is considered open if it meets three criteria, namely the existence of a specific position with work available (including full-time, part-time, permanent, and temporary jobs), a start date within 30 days, and active external recruitment efforts. I refer to this definition as the BLS vacancy duration (d_{BLS}).

The FEA defines vacancy duration as the period from the earliest intended employment start date to the deregistration (filling or canceling) of the vacancy (Statistik der Bundesagentur für Arbeit, 2020).[50] Thus, the FEA vacancy duration (d_{FEA}) is defined similarly to the BLS vacancy duration, while the 30-day threshold is reduced to zero. This stricter definition implies that a vacancy exists only if the position could be filled immediately, i.e., it is not occupied, even if the employer had already started searching for a new worker earlier.

As an alternative definition, I eliminate the criterion of an immediate job start and measure the duration of a vacancy as the time between the vacancy posting and closing (total vacancy duration, d_{total}). This definition does not rely on the earliest intended start of employment, which might be measured imprecisely since it can change over a vacancy's lifespan in the data.[51] The total vacancy duration accounts for the search efforts of employers that

[50] The establishment's representative at the FEA Employer Service is responsible for opening, managing, and closing vacancies with a placement order in the FEA placement system.

[51] I use the information on the intended start of employment at the time a vacancy is opened.

begin well in advance of the date when the job becomes available. On the downside, this definition is less strict regarding the requirements of active search and the availability of work for a position. Hence, it allows for persistent vacancies that might not reflect active search processes. The above measures are calculated as follows:[52]

$$d_{FEA} = t_{close} - t_{start}$$

$$d_{BLS} = t_{close} - max\{t_{open}, t_{start} - 30\}$$

$$d_{total} = t_{close} - t_{open}$$

Compared with official statistics on vacancy durations reported by the FEA for jobs that are subject to social insurance contributions, the FEA duration of vacancies, according to my data preparation, closely follows the officially reported numbers, with a correlation of 0.99, as shown in Appendix Figure 2.C2.[53]

2.4.2 Measure of Treatment Intensity

For the analysis of vacancies, the treatment measure would ideally be based on the distribution of posted wages for vacancies. However, as my vacancy dataset does not include information on offered wages, I measure exposure to the minimum wage on the basis of observed hiring wages in the IEB dataset. Since hires reflect the realized outcome of posted vacancies, the wages of posted vacancies and hires should be closely related. I first identify new employment relationships that represent genuine new hires not caused by short-term re-employment, taking up work after prolonged illness, maternal leave or parental leave in the IEB dataset (see Appendix 2.D for details). I then use the wages observed in the first spell of a new employment relationship to determine each occupation's minimum wage exposure, i.e. the bite. Although the IEB dataset provides daily wages, it lacks the hourly wages crucial for minimum wage exposure. However, owing to temporary reporting obligations, individual working hours information from the German Social Accident Insurance (Deutsche Gesetzliche Unfal-

[52] I consider only vacancies with positive durations and exclude erroneous cases where t_{close} is earlier than t_{start} or t_{open}.

[53] The discrepancies in the levels are attributable to the fact that the FEA excludes vacancies with a planned employment duration of not more than seven days in official statistics.

lversicherung, DGUV) is available for the years 2010–2014, and allows linking to individual employment spells in the IEB dataset for this period. I use this linkage to calculate hourly wages from daily wages for new hires in 2014. For each occupation, I calculate the share of new hires earning less than 8.50 euros per working hour in 2014. The bite of the minimum wage is held constant for each occupation over time.

2.4.3 Properties of the Final Analysis Sample

For the analysis of minimum wage effects, I exclude people under 18 years of age without training qualifications, apprentices, and interns in the IEB dataset, as these groups were not eligible for the German minimum wage.[54] In both the vacancy and employment data, I exclude 43 occupations that were temporarily exempted from the minimum wage.[55] The analysis sample contains both employees subject to social security contributions and marginally employed workers.[56] I consider only occupations with at least 100 annual job observations and at least 20 hiring wage values in 2014. In addition, I exclude occupations for which there is not at least one vacancy opening and closing observed in each quarter. This results in the exclusion of 59 out of 431 occupations, leaving 372 in the final sample. The excluded occupations represent only 0.24 percent of all employment relationships in the IEB dataset.

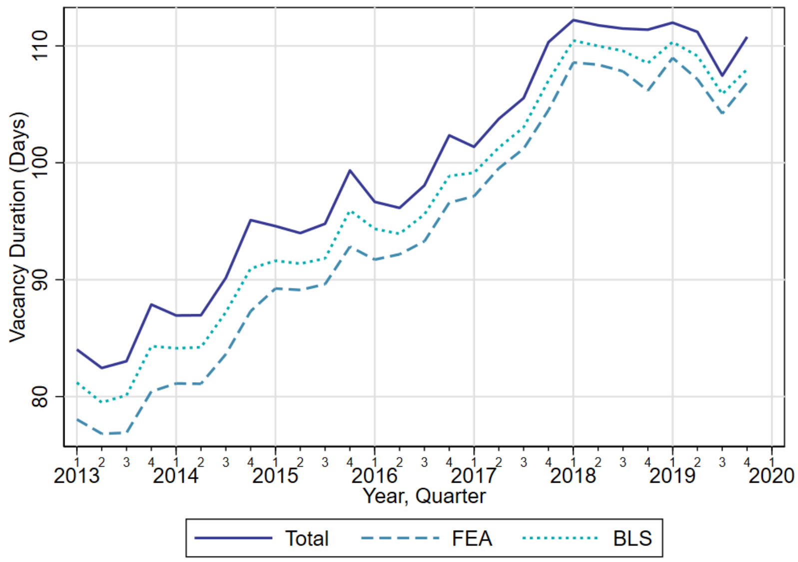
The variables of both datasets are aggregated at the level of occupation-time-cells. Flow variables such as vacancy openings and closures, as well as worker flows, are aggregated at both quarterly and yearly levels via simple addition. I measure time-varying stock variables (number of employment relationships, number of vacancies) for each quarter on the 15th of the middle month (February, May, August, November). The yearly values are calculated as the means over quarters of a year.

[54] Exceptions were also applied for long-term unemployed individuals for six months in their first job after unemployment. However, I do not exclude this group because I cannot identify long-term unemployment in my data as my analysis sample contains only employment history data (BeH). However, Umkehrer and Berge (2020) show that this exception was very rarely used.

[55] This concerns occupations in the meat industry, hairdressing, agriculture, forestry, gardening, the textile industry, dry cleaning, postal services and temporary agency work in East-Germany (for details, see Mindestlohnkommission, 2016, p. 155).

[56] Other types of employment are excluded. This concerns those in partial retirement, working students, working family members of employees in agriculture, irregularly employed individuals, pensioners, volunteers, and homeworkers.

Figure 2.1: Vacancy Durations for Filled Vacancy Inflows (Analysis Sample)



NOTE: The figure shows mean vacancy durations for filled vacancies at the time of vacancy opening. The durations are calculated based on the quarterly aggregated analysis sample for 372 occupations. The lines show the total vacancy durations (solid line), the FEA vacancy duration (dashed line), and the BLS vacancy duration (dotted line).

Figure 2.1 shows the quarterly mean duration of successfully filled vacancy inflows measured according to the three different definitions. The total vacancy duration clearly has higher values than the other two measures do since it includes persistent vacancies for jobs that have not yet become vacant. The durations of vacancies according to all three measures exhibit a noticeable seasonal pattern. The vacancies that were filled in the fourth quarter have, on average, a longer vacancy duration. While the official statistics of the FEA show a similar pattern (Appendix Figure 2.C2), they calculate the vacancy duration monthly on the basis of closing vacancies and consider only vacancies for jobs subject to social security contributions.[57]

As the definitions of the BLS and the FEA measures of vacancy durations are closely related, both curves move almost in parallel, apart from a level difference. Therefore, causal analysis focuses on the total vacancy duration and the FEA vacancy duration. The regression tables include effects on the BLS duration as robustness checks.

[57]See Appendix Figure 2.C3 for the vacancy durations on the basis of closing vacancies.

Table 2.1: Descriptive Statistics of the Hiring Bite

	Mean	Median	SD	Min	Max	N
Hires per Occupation	4,913	838	13,849	20	151,206	372
Hiring Bite per Occupation	0.22	0.16	0.16	0.01	0.71	372

NOTE: The first row shows descriptive statistics for the number of identified hiring observations used for the bite calculation per occupation. The second row shows descriptive statistics for the hiring bite per occupation calculated from the wage of hires in each occupation in the year 2014. SD = Standard Deviation, N = Number of Observations.

Table 2.1 shows descriptive statistics for the bite of the minimum wage in the analysis sample. The mean bite at the occupational level amounts to 22 percent.[58] The minimum wage bite shows considerable variation between occupations. In 26 of the 372 occupations, no more than 5 percent of the new hires were paid less than 8.50 euros per working hour in 2014 (see Appendix Figure 2.E1). The majority of occupations (251) paid wages below the minimum wage for between 5 and 30 percent of their newly hired workers in 2014. The bite distribution has a wide right tail, encompassing values between 30 and 71 percent.

2.5 Empirical Model

I estimate the effects of the minimum wage introduction via difference-in-differences models with a nonbinary treatment variable. The models are specified at the occupational level. Hence, I compare the outcomes of differentially affected occupations before and after the German minimum wage introduction. The treatment intensity (bite) is defined as a continuous variable denoting the share of hires at or below the initial minimum wage level of 8.50 euros in 2014, as described in Section 2.4.2. In contrast to the binary treatment classification based on the incidence of hiring wages below 8.50 euros, this measure not only captures whether any hiring wages within an occupation are below the minimum wage (extensive margin) but also accounts for differences in the intensive margin of the treatment. Accounting for

[58]My bite measure is at a higher level than the bite values of existing studies at the occupational level (e.g., Friedrich, 2020) because the bite is only calculated for new hires' wages (hiring bite) in my case and does not consider wage increases in existing employment relationships. The bite for incumbents in my analysis sample has a mean of 11 percent. Nevertheless, the value is above that of the bite in Friedrich (2020) (7 percent), which may be due to the different data collection methods (survey vs. administrative data). For recalls, the mean bite is 15 percent, which shows that hires are more exposed to the minimum wage than incumbent/recalled workers are.

treatment intensity seems particularly relevant for analyses at the occupational level since sector- or establishment-specific wage differences can lead to a dispersion of hiring wages within occupations.

In the baseline specification, I estimate the treatment effect on a yearly frequency. The estimation of year-specific treatment effects aims to maximize the statistical power of the estimator, as seasonal fluctuations are smoothed out. This specification can quantify a yearly average minimum wage effect independently of possible short-term or seasonal fluctuations. The model is specified according to the following equation:

$$y_{o,y} = \alpha_o * occ_o + \beta_i * year_i + \delta_{o,i} * bite_o * year_i + \varepsilon_{o,i} \tag{2.1}$$

The outcome variable in occupation o is observed in each year (i). Occupation fixed effects ($\alpha_o * occ_o$) capture time-constant heterogeneity between occupations. The year fixed effect ($\beta_i * year_i$) captures a common time trend. The effect of interest is identified by the interaction of the occupation-specific bite and the year. The coefficient of this interaction ($\delta_{o,i}$) captures variation in the outcome variable that depends on the bite and is explained neither by common variation over time nor by occupation-specific level differences. Thus, under certain conditions, such as common trends and no spillover effects, the coefficient should identify the causal effect of the minimum wage. I estimate cluster robust standard errors to account for the correlation of the variation in the outcome variable at the occupational level. The base period for the year fixed effect and the treatment interaction is the year 2013 in the vacancy analysis. When analyzing wage, employment, and worker-flow effects, the base period is 2014.

For the common trends assumption to hold, statistically significant estimated effects should not be present before the treatment, i.e., before the reform affects firms' hiring behavior. The minimum wage became binding on January 1, 2015. Nevertheless, anticipation effects may be relevant, particularly in search processes, when employers assume that the minimum wage will be introduced. Therefore, minimum wage-related effects may have already occurred in the last two quarters of 2014 when the law was discussed in parliament and passed by the German Federal Council in July 2014.

For a more granular understanding of the timing of the effects of the minimum wage introduction and potential seasonal patterns, I use the quarterly aggregated dataset and modify the baseline equation by adding quarter-fixed effects and interacting the bite with years and quarters simultaneously ($bite_o * year_i * quarter_j$), as shown in Equation (2.2):

$$
\begin{aligned}
y_{o,i,j} = \alpha_o * occ_o + \beta_i * year_i + \gamma_j * quarter_j + \\
\delta_{o,i,j} * bite_o * year_i * quarter_j + \varepsilon_{o,i,j}
\end{aligned}
\tag{2.2}
$$

Unlike in Equation (2.1), the treatment effect is now identified by the triple interaction of bite, year, and quarter. The base period for this specification is the first quarter of 2014.

If the quarterly variation in the outcome variable is correlated with the bite, then the baseline model might identify spurious minimum wage effects. High-bite occupations can, for example, be typical seasonal occupations where labor demand exhibits seasonal (quarterly) variation regardless of the minimum wage. To account for such bite-specific quarterly differences, I adjust the outcome variables for a quarter-bite-specific trend as an alternative model specification.

As a first step, I regress the outcome variable on the quarter-bite interaction via a fixed-effects model according to Equation (2.3):

$$
y_{o,j} = \alpha_o * occ_o + \theta_j * quarter_j * bite_o + \varepsilon_{o,j} \tag{2.3}
$$

The coefficient θ_j should identify a linear bite-specific trend for each quarter separately. In the second step, I adjust the outcome variable in each quarter by $\theta_j * bite_o$. The quarter-bite-specific trend is identified across all quarters from 2013 to 2019. Hence, the specification involves the assumption that the quarterly bite-specific fluctuations are not influenced by the minimum wage itself but remain constant over time. To identify the trend-adjusted minimum wage effect, I estimate the baseline model (Equation 2.2) using the adjusted outcome variable.

I weight all regressions by the average number of existing jobs within each occupation between 2013 and 2019 to account for differences in the size of occupations in the identification of the average treatment effect.

2.6 Results

2.6.1 Effects on Wages and Employment

Before examining the minimum wage effects on vacancies, I analyze the effects on real monthly wages and employment levels. The extent of adjustment reactions generally depends on whether the minimum wage is binding, that is, whether it increases wages at the lower end of the wage distribution. Existing studies on Germany suggest positive wage effects of the minimum wage for incumbent workers. At the regional level, Bossler and Gerner (2020) identify average monthly wage effects of between 4 and 6 percent. Caliendo et al. (2018) find a positive hourly wage effect in the lower half of the wage distribution, which does not translate into an increase in monthly wages. At the worker level, Bossler and Schank (2023) estimate a monthly wage effect of approximately 4 percent. I focus on effects for newly hired workers rather than incumbent workers, using monthly wages from their first social security record at a new employer. At the occupation level, the difference-in-differences model compares new-hire wage averages among occupations with different minimum wage exposures.

Table 2.2 shows the estimated minimum wage effects on log monthly hiring wages in column (1). The statistically significant negative coefficient of the bite-year interaction in 2013 ($2013 * bite$) suggests that wages in high-bite occupations developed differently than those in low-bite occupations before the introduction of the minimum wage. The political discussion and the final decision on the minimum wage were held in July 2014, which makes anticipatory effects plausible. Employers might have already increased wages for new contracts to the minimum wage level in 2014. Data limitations prevent testing placebo effects in previous years due to the structural break in the classification of occupations in the 2011–2012 period.[59]

The treatment coefficients in the post-treatment years reveal wage increases in affected occupations of approximately 9.2 percent in 2015 and approximately 13 percent in 2016 rela-

[59]See Appendix 2.B for details on the change to a new occupation classification scheme.

tive to 2014. This corresponds to an average treatment effect of 2.2 percent in 2015 and 2.9 percent in 2016.[60] Adjusting for the anticipatory wage increases in 2014, the average wage effect is 2.0 percent in 2015 and 4.8 percent in 2016. The results confirm the effectiveness of the minimum wage and that the magnitude of wage increases for hires at the occupational level is similar to the effects for incumbent workers at the regional or worker level in the literature.

The majority of existing studies on German minimum wages find a relatively small effect on employment (see Section 2.2). At the occupational level, Friedrich (2020) finds small effects on employment with regional heterogeneity. The number of marginal employment jobs decreased in eastern Germany, whereas regular employment in western Germany increased temporarily, which might reflect the upgrading of marginal jobs to regular jobs. In my case, employment is defined as the average number of jobs in an occupation for a given year.[61] The employment effects are estimated to be small and statistically insignificant each year, as shown in column (2) of Table 2.2. The signs of the coefficients possibly suggest a small negative effect, which aligns with the literature. While the minimum wage led to wage increases in affected occupations, the number of jobs in those occupations did not change substantially.

2.6.2 Effects on Vacancies

The absence of equilibrium employment effects does not rule out changes in firms' hiring behavior or the efficiency of the matching process. Thus, I examine the minimum wage effects on vacancies to investigate these adjustment channels. The analysis starts with the inflow of vacancy openings to shed light on temporal or structural changes in unsatisfied labor demand. I subsequently present the results on vacancy durations to uncover frictional effects affecting matching efficiency. Finally, I show effects on the stock of vacancies, which results from the interaction of unsatisfied labor demand and vacancy duration and thus combines the two effect channels. In addition, the stock of vacancies influences labor market tightness and thus can have repercussions on vacancy openings and the time required to fill a vacancy.

[60] The effect for an average affected occupation is calculated as the product of the average bite (0.22) and the value of the treatment coefficient.

[61] The number of jobs is measured in the middle of each quarter (i.e., 15 February, 15 May, 15 August, and 15 November), and the yearly averages are calculated from the quarterly values for each occupation.

Table 2.2: Annual Effects of the Minimum Wage Introduction on Wages and Employment

	(1) Log Hiring Wage	(2) Log Employment
2013 $*$ *bite*	-0.034***	-0.011
	(0.010)	(0.012)
2014 $*$ *bite*	base	base
	-	-
2015 $*$ *bite*	0.087***	-0.009
	(0.010)	(0.013)
2016 $*$ *bite*	0.123***	-0.003
	(0.012)	(0.024)
2017 $*$ *bite*	0.144***	-0.021
	(0.020)	(0.032)
2018 $*$ *bite*	0.152***	-0.057
	(0.028)	(0.043)
2019 $*$ *bite*	0.109***	-0.099*
	(0.031)	(0.054)
Observations	2,604	2,604
Clusters	372	372

NOTE: The table shows estimates of the treatment effects on the treated derived from difference-in-differences specifications with occupation-level fixed effects. Treatment intensity (bite) is measured continuously as the share of hires at or below the minimum wage in the year 2014. The coefficients indicate how the outcome variables develop with respect to the bite of the minimum wage and relative to the base year (2014). Standard errors (in parentheses) are clustered at the occupational level. The regressions are weighted by the occupational labor market size which is defined as the average number of jobs in each occupation over the years 2013 to 2019. The outcome variables are the natural logarithm of the monthly wages for new hires (Column 1) and the natural logarithm of the number of jobs per occupation (Column 2). Asterisks indicate the statistical significance level: *** $p<0.01$, ** $p<0.05$, and * $p<0.1$. *Sources:* Integrated Employment Biographies, 2013–2019.

Vacancy Creation

From a firm's perspective, opening a vacancy indicates the presence of effort to find a worker for a vacant position. This position may either have been newly created, have recently become vacant or be scheduled to become vacant in the near future due to termination, dismissal or redeployment of the previous employee. As described in Section 2.2, the expected effect of the minimum wage on the opening of vacancies is not obvious.

Regardless of the minimum wage, some occupations might experience greater long-term growth in the number of jobs than others, for example, due to technological change. With rising employment, ceteris paribus, the absolute turnover of employees also increases, which in turn influences the absolute number of opened vacancies. Unlike Kudlyak et al. (2022), who exploit variation in state-specific minimum wages, allowing them to include county-specific occupation fixed effects, county-specific time fixed effects, and occupation-specific time fixed effects, I cannot directly control for occupation-specific trend differences because the minimum wage level is fixed for all occupations in Germany; also, the inclusion of an occupation-time interaction would absorb the treatment effect. As a workaround, I calculate the log vacancy inflow share as a coefficient of the number of vacancy inflows ($vinf$) and the number of jobs per occupation $\left[Ln\left(\frac{\mathbf{N}_{o,y}^{vinf}}{\mathbf{N}_{o,y}^{jobs}}\right)\right]$.[62] The transformation aims to account for different (minimum wage-independent) growth trends of occupations by focusing on the ratio of unsatisfied labor demand (vacancies) to satisfied labor demand (filled jobs). Short-term changes in this ratio reveal temporary imbalances in labor demand revealing transitory adjustment mechanisms, whereas persistent changes reveal structural shifts in unsatisfied labor demand.

Table 2.3 presents annual minimum wage effects ($\delta_{o,y}$ in Equation 2.1) on the log inflow share per year. I choose 2013 as the base year because anticipatory effects on hiring wages already appeared in 2014 (see Section 2.6.1). Column (1) shows effects for all newly opened vacancies, regardless of their eventual reason for closure. Small but statistically insignificant positive effects are estimated for 2014 and 2015. Columns (2) and (3) show heterogeneous effects on newly opened vacancies that were eventually either successfully filled or canceled. While no statistically significant effect on the opening of successfully filled vacancies is estimated, the introduction of the minimum wage led to a significant increase in canceled vacancies in affected occupations, as shown by the positive and statistically significant effect at the 5 percent level from 2014 to 2017. I multiply the coefficients by the average bite of 0.22 to quantify the average treatment effect. This reveals an increase in the vacancy inflow share of canceled vacancies between 4.0 and 9.0 percent relative to 2013 for an averagely affected occupation.

[62] The number of jobs per year is measured as the mean value of the number of jobs at the middle of each quarter of a year.

Appendix Table 2.F1 shows estimates for the absolute number of vacancy inflows as a robustness check. This yields an average treatment effect on eventually canceled vacancies between 5.2 and 21.1 percent in the 2014–2017 period.[63] Hence, the results are qualitatively robust but at a relatively high level, possibly revealing different minimum wage-independent growth trends across occupations. Appendix Figure 2.F1 shows quarterly effect estimates according to Equation (2.2). The coefficients point qualitatively in the same direction but are not estimated to be statistically significant.

Appendix Table 2.F2 and Appendix Figure 2.F2 present yearly and quarterly effects on vacancy outflows, which resemble the results for inflows. Although statistically insignificant, the coefficients indicate increased outflows of canceled vacancies, whereas outflows of filled vacancies tend to decrease.

As a further robustness check, I limit the analysis sample to occupations with requirement levels 1 (Helper) and 2 (Professional). These occupations may be more homogeneous in terms of their characteristics such that low-bite occupations in this subsample represent a more suitable control group than low-bite occupations at requirement levels 3 (Specialist) and 4 (Expert). The average hiring bite in this subsample is 0.3, indicating that occupations with low requirement levels are, on average, more affected by the minimum wage. However, there are control-group occupations with low level of exposure to the minimum wage, as the bite in the first decile is less than 11.4 percent. Appendix Table 2.F3 shows annually estimated treatment effects on the log inflow share. The sample restriction reduces the number of occupations to 163. The estimated treatment effects are mostly not statistically different from zero, except for filled vacancies in 2016. The signs of the coefficients indicate similar effects to those of the full sample. The comparatively larger negative (although insignificant) effects on all vacancy openings may indicate a reduction in labor demand among high-bite occupations compared with low-bite occupations with low requirement levels. This might indicate either employment reduction or the substitution of workers by capital, which is consistent with empirical findings on skill upgrading (Clemens et al., 2021) and labor substitution in highly automatable jobs (Aaronson and Phelan, 2019; Lordan and Neumark, 2018) due to minimum wages.

[63] The average treatment effect for the absolute number of vacancies is calculated by multiplying the coefficients by the average bite and dividing by the average number of canceled vacancy inflows in 2013 ($\overline{CVI}_{2013} = 1.415$).

Table 2.3: Annual Effects of the Minimum Wage Introduction on Vacancy Inflows Shares

	(1) All	(2) Filled	(3) Canceled
$2013 * bite$	base	base	base
	-	-	-
$2014 * bite$	0.027	-0.018	0.182**
	(0.059)	(0.055)	(0.079)
$2015 * bite$	0.056	-0.004	0.229**
	(0.093)	(0.094)	(0.097)
$2016 * bite$	0.005	-0.120	0.313**
	(0.113)	(0.108)	(0.128)
$2017 * bite$	-0.012	-0.181	0.411**
	(0.176)	(0.162)	(0.197)
$2018 * bite$	-0.034	-0.230	0.439
	(0.245)	(0.227)	(0.289)
$2019 * bite$	-0.188	-0.463	0.427
	(0.445)	(0.450)	(0.453)
Observations	2,604	2,604	2,604
Clusters	372	372	372

NOTE: The table shows estimates of the treatment effects on the treated derived from difference-in-differences specifications with occupation-level fixed effects. Treatment intensity (bite) is measured continuously as the share of hires at or below the minimum wage in the year 2014. The coefficients indicate how the outcome variables develop with respect to the bite of the minimum wage and relative to the base year (2013). Standard errors (in parentheses) are clustered at the occupational level. The regressions are weighted by the occupational labor market size which is defined as the average number of jobs in each occupation over the years ranging from 2013 to 2019. The outcome variables are the natural logarithm of the share of vacancy inflows on all jobs within occupations in each year. Column (1) shows the effects on all newly opened vacancies. Columns (2) and (3) show the effects on newly opened vacancies that were ultimately filled or canceled. Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1. *Sources:* Integrated Employment Biographies + vacancies registered with the German Federal Employment Agency, 2013–2019.

In summary, two main findings emerge from the results. First, the minimum wage introduction has not substantially altered employers' vacancy posting behavior. On the one hand, this aligns with minor effects on equilibrium employment found at the establishment and regional levels (see Section 2.2), which are also evident at the occupational level (see Section 2.6.1). On the other hand, the results show that the minimum wage has not substantially affected the unrealized labor demand. I do not find a reduction in vacancies as shown in Kudlyak et al. (2022), at the occupational level. Compared with low-bite establishments, highly affected establishments might still reduce their overall vacancies but do not do so selectively for high-bite occupations. This seems plausible since establishments need a certain mix of workers in different occupations to produce their output. The substitution of labor with capital (Aaronson and Phelan, 2019; Lordan and Neumark, 2018), which changes the proportion of needed occupations, can be expected in the long run. In addition, the measure of minimum wage exposure in my study conceptually differs from the bite measure in Kudlyak et al. (2022). First, I use the distribution of hiring wages, not incumbent workers' wages, for the bite calculation. Second, my bite measure is continuous and includes the intensive margin of minimum wage exposure for each occupation.

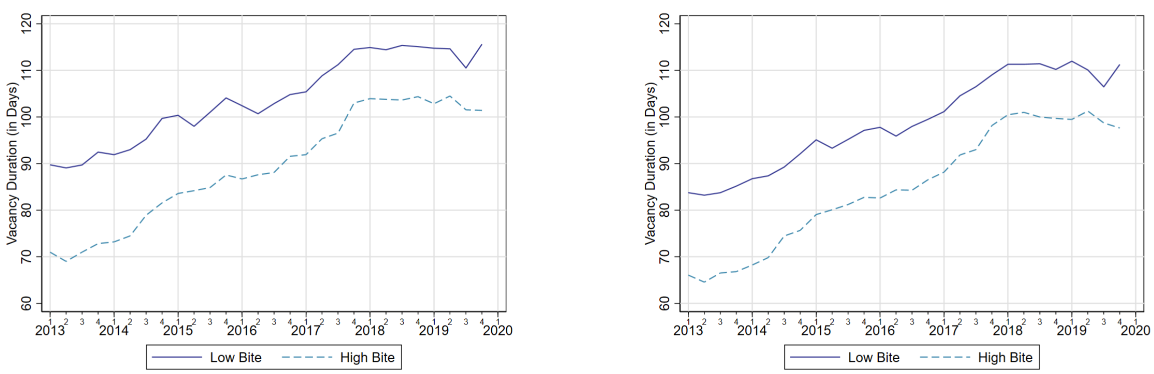
The second finding provides initial indications of greater frictions in the recruitment process of establishments. A vacancy's outcome – either filled or canceled —- is determined at closure and unknown to the employer at opening. The ex post analysis, however, suggests that a larger proportion of vacancies in high-bite occupations were eventually canceled. Increased cancellations in 2014 may also be caused by structural reasons; e.g., an initially planned vacancy may have been abandoned owing to the introduction of the minimum wage. However, the increased cancellations of vacancies opened after the introduction of the minimum wage reflect frictional reasons. A firm would have liked to fill a vacant position even after the introduction of the minimum wage but eventually had to cancel the vacancy, for example, because no sufficiently productive candidate applied. The following section examines these frictional effects using vacancy durations.

Completed Vacancy Durations

To analyze the effect of the minimum wage on the durations of vacancies, I focus on successfully filled vacancies. The latter represent completed matching processes of labor supply and demand. In contrast, for eventually canceled vacancies, it is not discernible if the search process was stopped because no suitable worker could be found or if the underlying job became obsolete. From the firm's perspective, the duration of a recruitment process directly influences its vacancy costs (see Section 2.2). Through the labor demand channel, the minimum wage can increase the search duration, for example, through increased hiring standards (Butschek, 2022; Clemens et al., 2021) or screening intensity (Gürtzgen et al., 2016).

Figure 2.2 shows the quarterly mean values of the total vacancy duration and the FEA vacancy duration in high- and low-bite occupations. The durations are measured quarterly at the time of vacancy opening. Therefore, for each quarter, the curves show the duration of those (completed) vacancies opened in the respective quarter. The curves illustrate bite-dependent level differences. On average, low-bite occupations have longer vacancy durations. The slopes of both curves point to increasing vacancy durations over time, which coincides with the sharp increase in labor market tightness in Germany between 2012 and 2019 (see Bossler and Popp, 2024; Börschlein et al., 2024). The increase in vacancy durations between the third quarter of 2014 and the fourth quarter of 2015 could indicate a minimum wage effect. The level differences between total vacancy durations and FEA vacancy durations illustrate the different measurement concepts (see Section 2.4). While the total vacancy duration also includes the planned search duration before the desired start of employment, the FEA vacancy duration only refers to unplanned search durations, i.e., from the desired start of employment to the actual deregistration of the vacancy.

Figure 2.2: Duration of Opening Vacancies in Low and High Bite Occupations



(a) Total Duration

(b) FEA Duration

NOTE: The figures show the mean duration of vacancies that were ultimately successfully filled measured at the opening quarter (inflow). Panel (a) shows the total vacancy duration, which is defined as the number of days between opening and closing. Panel (b) shows the FEA vacancy duration, which is defined as the number of days between the desired start of employment and closing. The sample is divided at the median of the bite distribution.

Table 2.4 shows the estimated effects of the minimum wage introduction on log vacancy durations. The treatment effects are estimated to be positive and significantly different from zero for all three measures of vacancy duration. The effects range between 11.0 and 11.6

percent in 2014 relative to the base year 2013, which corresponds to an average treatment effect of approximately 2.4 to 2.6 percent and may indicate anticipation effects. In the two years following the introduction of the minimum wage, the average treatment effect increases to values between 5.1 and 6.1 percent. The further increase to approximately 8.4 to 8.5 percent in 2017 could be caused by the first increase in the minimum wage in January 2017. However, the further substantial increase in 2018 does not coincide with an adjustment to the minimum wage, which was not raised until January 2019.

To examine the timing of the effects and possible placebo or anticipation effects in 2014 in more detail, Figure 2.3 shows the estimated effects for the total duration and the FEA duration at the quarterly level (Equation 2.2). The outcome variables are adjusted for quarter-bite-specific seasonal variation according to Equation (2.3). No statistically significant (placebo) effects are estimated in the quarters of 2013 relative to the base period (2014/Q1). The effects increase significantly in the third quarter of 2014, which coincides with the legislative decision for the minimum wage introduction that has been applicable since 2015. Therefore, employees and/or firms appear to have already adjusted their search and hiring behavior at the time of the decision for the minimum wage, impacting the duration of newly opened vacancies. The quarterly effects remain relatively stable in 2015 and 2016 at approximately 20 percent (Total Duration) and 25 percent (FEA Duration), respectively. The further gradual increase in the effects in 2017 could be related to the increase in the minimum wage. However, the distinct increase in standard errors from 2017 onward points to possible unobserved time-varying factors that increasingly influenced the vacancy durations of firms differently than the minimum wage. One factor causing the unobserved heterogeneity could be the further notable increase in labor market tightness in Germany from 2017 onward.

While the above analysis focused on durations of newly opened vacancies, I analyze the durations of closing vacancies as a robustness check. The two measurement concepts differ concerning the temporal allocation of vacancies. For the duration of opening vacancies at time t, all the vacancies that were opened in t are considered, whereas for the duration of closing vacancies at time t, all the vacancies that were closed in t are considered. This means that the opening time is fixed for the former and that the closing time is fixed for the latter in each period. Both measurement concepts offer advantages and disadvantages with respect

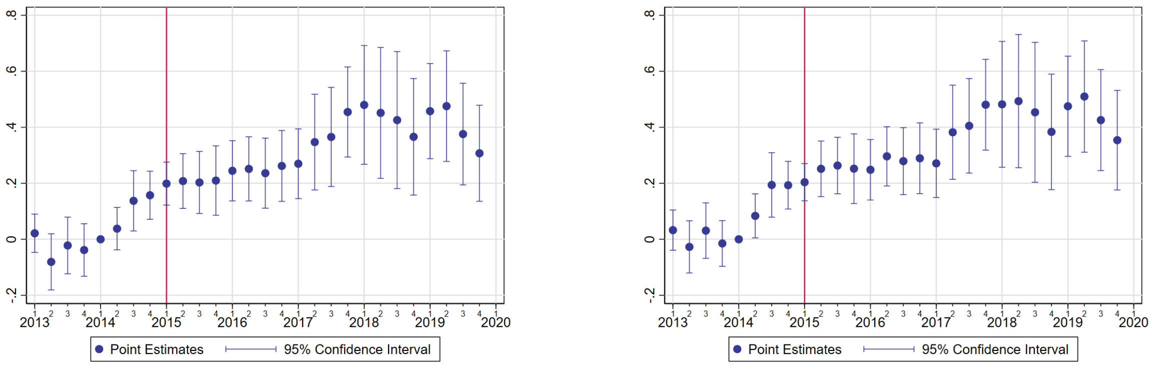
Table 2.4: Annual Effects of the Minimum Wage Introduction on the Durations of Opening Vacancies

	(1) Total	(2) FEA	(3) BLS
2013 $*$ *bite*	base	base	base
	-	-	-
2014 $*$ *bite*	0.115***	0.116***	0.110***
	(0.027)	(0.026)	(0.026)
2015 $*$ *bite*	0.233***	0.237***	0.236***
	(0.035)	(0.036)	(0.035)
2016 $*$ *bite*	0.277***	0.272***	0.277***
	(0.045)	(0.043)	(0.043)
2017 $*$ *bite*	0.388***	0.380***	0.388***
	(0.066)	(0.064)	(0.065)
2018 $*$ *bite*	0.458***	0.448***	0.453***
	(0.106)	(0.107)	(0.106)
2019 $*$ *bite*	0.429***	0.431***	0.439***
	(0.076)	(0.076)	(0.076)
Observations	2,604	2,604	2,604
Clusters	372	372	372

NOTE: The table shows estimates of the treatment effects on the treated derived from difference-in-differences specifications with occupation-level fixed effects. Treatment intensity (bite) is measured continuously as the share of hires at or below the minimum wage in the year 2014. The coefficients indicate how the outcome variables develop with respect to the bite of the minimum wage and relative to the base year (2013). Standard errors (in parentheses) are clustered at the occupational level. The regressions are weighted by the occupational labor market size which is defined as the average number of jobs in each occupation over the years ranging from 2013 to 2019. The outcome variables are the natural logarithm of the vacancy durations of newly opened vacancies. The coefficients show, for each year, the minimum wage effects on the duration of those (completed) vacancies opened in the respective year. The columns show the effects on total vacancy duration (1), FEA vacancy duration (2), and BLS vacancy duration (3). Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1. *Sources:* Integrated Employment Biographies + vacancies registered with the German Federal Employment Agency, 2013–2019.

to minimum wage analysis. Considering the duration of opening vacancies allows to clearly define whether a firm was aware of the minimum wage that was already in force or was to be introduced soon (e.g., from the 3rd quarter of 2014) at the start of the personnel search. Thus, this concept allows for a clearer distinction of search processes affected by the minimum wage over their entire duration. In contrast, measuring the duration of vacancies at the time

Figure 2.3: Quarterly Effects of the Minimum Wage Introduction on the Durations of Opening Vacancies



(a) Total Durations (b) FEA Durations

NOTE: The figure shows estimates of the treatment effects on the treated derived from difference-in-differences specifications with occupation-level fixed effects estimated on the quarterly level. Treatment intensity (bite) is measured continuously as the share of hires at or below the minimum wage in the year 2014. The coefficients indicate how the outcome variables develop with respect to the bite of the minimum wage and relative to the base period (2014/Q1). The vertical bars indicate 95-percent confidence intervals. Standard errors (in parentheses) are clustered at the occupational level. The regressions are weighted by the occupational labor market size, which is defined as the average number of jobs in each occupation over the years ranging from 2013 to 2019. The outcome variables are the natural logarithm of the vacancy durations measured at the quarter of vacancy opening. Panel (a) shows the effects on the total vacancy duration. Panel (b) shows the effects on the BLS vacancy duration *Sources:* Integrated Employment Biographies + vacancies registered with the German Federal Employment Agency, 2013–2019.

of closure allows a clearer distinction of which vacancies were not affected by the minimum wage over their entire duration (e.g., up to the 3rd quarter of 2014).

Appendix Table 2.F5 shows the annual minimum wage effects on the durations of closing vacancies. In terms of direction and magnitude, the results correspond to the effects on the durations of opening vacancies but are shifted back by one year owing to the different measurement concepts. Personnel searches that started in 2015 were clearly and completely affected by the minimum wage throughout their entire duration. However, vacancies closed in 2015 were not necessarily affected by the minimum wage for their entire duration, depending on when the search began. Therefore, effects on the duration of closing vacancies manifest gradually after the minimum wage introduction, as an increasing proportion of closing vacancies were affected over their entire duration.

Both measurement concepts show differences in the estimated minimum wage effects between total search durations and FEA search durations. These can be interpreted as follows. For vacancies where the search for personnel began in 2015, the minimum wage had roughly the same effect on the total vacancy duration and the unplanned search duration (FEA duration). This reflects that the end of a newly started search lies further in the future in relation to the base year. The search takes longer, and the search frictions of newly started searches increase. For vacancies that were filled in 2015, the minimum wage had a greater effect on the total vacancy duration than on the unplanned search duration (FEA duration). This reflects the fact that the start of the personnel search for a closed vacancy lied further in the past in relation to the base year. However, the timing of the planned start of employment shifted to a smaller extent as a result of the minimum wage. This finding indicates that firms have started looking for staff earlier, possibly indicating increased screening of applicants. The unplanned search duration is also extended, which may indicate labor supply effects (e.g., due to less turnover or less sufficiently productive applicants).

As with the analysis of vacancy openings, I estimate the minimum wage effects restricted to occupations with requirement levels 1 (Helper) and 2 (Professional) as a further robustness check. Appendix Table 2.F4 shows the annual estimated treatment effects on the log vacancy durations of opening vacancies, revealing no substantial deviations from the result for the overall sample.

Job-Vacancy Rate

The previous analyses of vacancy openings, closings, and durations each focused on a specific aspect of the matching process. Most of the results point to increased frictions due to minimum wages. The following analysis intends to measure the consolidated influence of the minimum wage on the mismatch between labor demand and supply and the degree of frictions in the matching process. To quantify the consolidated effects, I use the stock of pending, unfilled vacancies, which results from the interaction of the previously analyzed vacancy flows and durations. Adjustments to vacancy flows affect the stock of vacancies. Moreover, increasing vacancy durations automatically increases the stock of vacancies, ceteris paribus. Hiring difficulties can in turn have a dampening effect on vacancy openings (Le Barbanchon et al., 2024) and eventually reduce the vacancy stock.

To analyze the minimum wage effects on the stock of vacancies independently of different occupational growth trends, I calculate the job vacancy rate as *JVR = (number of registered vacancies)/(number of occupied positions + number of registered vacancies)*. This measure, which is also used in official statistics of the European Union (EUROSTAT), shows what proportion of the total labor demand is accounted for by vacancies, i.e., by unrealized labor demand. It allows the comparison of labor markets of different sizes and growth rates and can serve as an indicator of the extent of frictions in the labor market.

The administrative vacancy data utilized in this study encompass only those vacancies registered with the FEA. On the basis of survey data from the IAB Job Vacancy Survey (JVS), the ratio of reported vacancies to all vacancies in Germany between 2013 and 2019 is estimated to range between 41 and 49 percent (Bossler et al., 2020). While the reporting rate in the aggregate is, therefore, relatively high, it could differ across occupations. For example, Kettner and Stops (2009) demonstrate that the placement service of the FEA is utilized less frequently for occupations with complex tasks, while the majority of vacancies for low-complexity jobs are reported to the FEA. Obviously, occupational differences in the reporting rates bias the absolute numbers of posted vacancies per occupation in my data. However, this is not necessarily a threat to identification for two reasons. First, the difference-in-differences approach controls for time-constant level differences between groups. Second, the JVRs do not show different growth trends with respect to the minimum wage bite of occupations (see Appendix Figure 2.F3). Nevertheless, occupations with low registration rates might bias the estimated effects downward because minimum wage-related adjustments are not reflected in registered vacancies, even if these occupations' actual (nonregistered) vacancies are affected. Calculating the registration rate at a detailed occupational level (3+5-digit) is not possible with the currently available data.[64] As a workaround, I calculate yearly vacancy yields as *VY = hires / filled vacancy outflows* to approximate the registration rates per occupation. The underlying assumption is that high vacancy yields in some occupations, i.e., many hires

[64] On the basis of the JVS, registration rates can be calculated representatively for economic sectors, establishment size classes and eastern/western Germany but not at the occupation level.

for few observed vacancies, might indicate that the majority of vacancies in these occupations are not reported to the FEA. I exclude the 10 percent of occupations with the highest vacancy yields and four occupations with higher average vacancy outflows than observed hires as a robustness check.

Table 2.5: Annual Effects of the Minimum Wage Introduction on the Job-Vacancy-Rate

	(1) JVR Full Sample	(2) JVR Trimmed Sample
$2013 * bite$	base -	base -
$2014 * bite$	0.029 (0.080)	0.060 (0.097)
$2015 * bite$	0.188* (0.109)	0.292** (0.114)
$2016 * bite$	0.216 (0.141)	0.310** (0.156)
$2017 * bite$	0.302 (0.188)	0.460** (0.191)
$2018 * bite$	0.394 (0.270)	0.554*** (0.185)
$2019 * bite$	0.263 (0.452)	0.554*** (0.213)
Observations	2,604	2,352
Clusters	372	336

NOTE: The table shows estimates of the treatment effects on the treated derived from difference-in-differences specifications with occupation-level fixed effects. Treatment intensity (bite) is measured continuously as the share of hires at or below the minimum wage in the year 2014. The coefficients indicate how the outcome variables develop with respect to the bite of the minimum wage and relative to the base year (2013). Standard errors (in parentheses) are clustered at the occupational level. The regressions are weighted by the occupational labor market size, which is defined as the average number of jobs in each occupation over the years ranging from 2013 to 2019. The outcome variables are the job-vacancy rate (JVR), which is calculated as the stock of vacancies relative to the sum of occupied positions, and the stock of vacancies. Column (1) shows the effects on the unrestricted analysis sample. Column (2) shows the effects of the trimmed sample excluding the 10 percent of occupations with the highest vacancy yields. Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1. *Sources:* Integrated Employment Biographies + vacancies registered with the German Federal Employment Agency, 2013–2019.

Table 2.5 shows the estimated effects of the minimum wage introduction on the log JVR for the overall sample and the robustness check with a trimmed sample. No statistically significant pretreatment effects are identified in 2014 in either specification. The coefficients are positive in the year when the minimum wage was introduced and are estimated to be statistically significant at the 10 percent and 5 percent levels, respectively. The effects increased further in 2017 in particular, which may indicate an effect of the minimum wage increase. The further increase in effects in 2018 and 2019 should, in turn, be interpreted with caution because of the enormous increase in labor market tightness during these years (see Bossler and Popp, 2024). The average treatment effect is approximately 4.1 percent (full sample) and 6.1 percent (trimmed sample) in 2015.[65]

In summary, the analyses of vacancy flows, durations, and stocks point to growing difficulties in the matching process due to the introduction of the minimum wage. The total number of vacancies opened in minimum wage occupations does not appear to have changed significantly. This result is consistent with the insignificant effects on total employment in minimum wage occupations (see Table 2.2). In terms of labor demand, the minimum wage introduction, therefore, does not appear to have led to any significant structural adjustments. At the same time, a larger proportion of vacancies opened in minimum wage occupations were eventually canceled unsuccessfully instead of being filled. Furthermore, the positive effects on vacancy durations show that employers spent more time filling vacancies in minimum wage occupations. The increased difficulties in filling vacancies are also reflected in the stock of vacancies in minimum wage occupations, shown by the increase in job vacancy rates. Overall, the introduction of the minimum wage, therefore, appears to have had a frictional rather than a structural impact on matching processes in the labor market. In the following sections, I examine the possible mechanisms underlying the increase in frictions.

2.7 Underlying Mechanisms

In this section, I use observable worker transitions between employers and occupations to explore which labor market mechanisms might underlie the results of the vacancy analysis (Section 2.7.1). In addition, I complement the findings with an analysis of survey data on recruitment processes, which makes it possible to differentiate between supply and demand effects (Section 2.7.2). I also exploit worker flows between low-bite and high-bite occupations

[65]The average bite in the trimmed sample is 0.21.

to examine a potential risk to the identification strategy, namely, the occurrence of spillover effects between occupations. The results show that the minimum wage introduction did not substantially change the structure of worker flows between occupations with different bites; albeit there was a small temporal increase in worker flows from low-bite to high-bite occupations in 2015. Details of the spillover analysis including a theoretical discussion, the identification strategy and effect estimates, can be found in Appendix 2.G.

2.7.1 Worker Transitions

In contrast to vacancies, completed worker transitions not only indicate the existence of a (temporary) imbalance between supply and demand but also represent observable results of adjustment processes in the labor market. Even without substantial equilibrium effects (e.g., total employment), the minimum wage can still affect worker transitions within and between affected occupations. The expected direction of minimum wage effects on labor market flows is thereby a priori not obvious. From a theoretical point of view, transitions might decrease or increase depending on the magnitude of effects from adverse underlying channels.

To investigate this adjustment channel at the occupational level, I focus on between-establishment flows. Comparing high-bite to low-bite occupations, I estimate the minimum wage effect on employer-to-employer (EE) transitions and additionally distinguish between EE transitions that involve an occupational change (EE transition between occupations) and EE transitions that involve workers staying in the same occupation at a different establishment (EE transition within occupation).[66]

Table 2.6 shows the estimated effects of the minimum wage introduction on EE transitions. Column (1) contains the treatment effects on the share of EE transitions among all employees per occupation. In the year of the minimum wage introduction, there is a zero effect compared with the base year. Despite the introduction of the minimum wage, there does not appear to have been any significant change in transitions in minimum wage occupations between 2014 and 2015 compared with occupations only slightly affected by the minimum wage. While negative effects are evident in the following years, these effects are only estimated to be statistically significant in 2017 and 2019. The average treatment effect for the

[66]The occupational change can be to any occupation, not only from high-bite to low-bite occupations.

Table 2.6: Annual Effects of the Minimum Wage Introduction on Job-to-Job Transitions

	(1) EE-Transitions (All)	(2) EE-Transitions (Between Occupations)	(3) EE-Transitions (Within Occupations)
2014 $*$ *bite*	base	base	base
	-	-	-
2015 $*$ *bite*	-0.000	-0.001	0.000
	(0.006)	(0.004)	(0.002)
2016 $*$ *bite*	-0.012	-0.010**	-0.002
	(0.008)	(0.005)	(0.004)
2017 $*$ *bite*	-0.012*	-0.013**	0.001
	(0.007)	(0.005)	(0.003)
2018 $*$ *bite*	-0.006	-0.014**	0.008
	(0.010)	(0.006)	(0.006)
2019 $*$ *bite*	-0.022***	-0.010*	-0.012**
	(0.008)	(0.005)	(0.006)
Observations	2,232	2,232	2,232
Clusters	372	372	372

NOTE: The table shows estimates of the treatment effects on the treated derived from difference-in-differences specifications with occupation-level fixed effects. Treatment intensity (bite) is measured continuously as the share of hires at or below the minimum wage in the year 2014. The coefficients indicate how the outcome variables develop with respect to the bite of the minimum wage and relative to the base year (2014). Standard errors (in parentheses) are clustered at the occupational level. The regressions are weighted by the occupational labor market size, which is defined as the average number of jobs in each occupation over the years ranging from 2013 to 2019. The outcome variables are the share of EE transitions on all filled jobs per occupation within a year. The columns show the effects on all EE transitions (1), on EE transitions that comprise the change of occupation (2), and EE transitions within the same occupation (3). Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1. *Sources:* Integrated Employment Biographies, 2013–2019.

coefficient in 2017 can be quantified as approximately -1.86 percent in 2017 and 3.4 percent in 2019. Together with the effect on opened vacancies, the negative effects on transitions can be interpreted as an increase in employment stability in minimum wage occupations. This might, in turn, indicate increasing match quality resulting from intensified applicant screening and increased hiring standards, for example. The results are in line with empirical minimum wage studies, which show increased job tenure and lower job turnover in response to minimum wages (Dube et al., 2007; Dube et al., 2016; Jardim et al., 2018).

Columns (2) and (3) show heterogeneous effects for EE transitions that are associated with a change in workers' occupation (between occupations) and for those that take place within an occupation. For EE transitions between occupations, negative, statistically significant effects are estimated from 2016 to 2019, which correspond to average treatment effects between -2.3 and -3.2 percent. This finding is consistent with empirical evidence of lower occupational mobility due to minimum wages (Liu, 2022). The effects on EE transitions within occupations, on the other hand, are close to zero and statistically insignificant. These differences support the argument that minimum wage occupations became more attractive. Employees thus had less incentive to look for a new job in a different occupation than they would without the minimum wage. Note that this result implies that the reallocation of employees between firms that has been documented by Dustmann et al. (2022) does not carry over to the occupational level. However, this does not contradict the findings of Dustmann et al. (2022). Minimum wage effects at the occupation level cannot be transferred to the establishment level because establishments can consist of a mix of occupations with different exposures to the minimum wage.

Table 2.7 shows additional analyses of transitions into nonemployment (EU Transition) and out of nonemployment (UE Transitions).[67] The expected directions of the effect are not clear a priori. For example, the increased attractiveness of minimum wage occupations means that fewer workers might (voluntarily) switch to nonemployment. Moreover, a labor demand effect can lead to more transitions to nonemployment. On the other hand, better matching can lead to a negative effect on EU transitions. The estimates for EU transitions show negative, statistically significant coefficients for the years following the introduction of the minimum wage. This channel confirms previous findings on the increased stability of employment relationships in minimum wage occupations. Furthermore, the results are in line with the literature indicating minimum wage adjustments via the margin of hirings rather than firings (Bossler and Gerner, 2020; Gopalan et al., 2021; Jardim et al., 2018). Column (2) shows the estimated effects on UE transitions. Theoretically, from a labor supply perspective one might expect a positive effect on these transitions, as unemployed people may have greater incentives to accept a minimum wage job due to its increased attractiveness. From a labor

[67]For UE transitions I do not distinguish whether a worker might have worked in the same occupation before unemployment.

Table 2.7: Annual Effects of the Minimum Wage Introduction on Transitions from/to Unemployment

	(1) EU-Transitions	(2) UE-Transitions
2014 ∗ *bite*	base	base
	-	-
2015 ∗ *bite*	-0.004	0.003
	(0.002)	(0.004)
2016 ∗ *bite*	-0.016***	-0.010**
	(0.004)	(0.005)
2017 ∗ *bite*	-0.026***	-0.016**
	(0.006)	(0.008)
2018 ∗ *bite*	-0.030***	-0.027***
	(0.006)	(0.009)
2019 ∗ *bite*	-0.030***	-0.038***
	(0.007)	(0.010)
Observations	2,232	2,232
Clusters	372	372

NOTE: The table shows estimates of the treatment effects on the treated derived from difference-in-differences specifications with occupation-level fixed effects. Treatment intensity (bite) is measured continuously as the share of hires at or below the minimum wage in the year 2014. The coefficients indicate how the outcome variables develop with respect to the bite of the minimum wage and relative to the base year (2014). Standard errors (in parentheses) are clustered at the occupational level. The regressions are weighted by the occupational labor market size, which is defined as the average number of jobs in each occupation over the years ranging from 2013 to 2019. The outcome variables are the share of transitions on all filled jobs per occupation within a year. Column (1) shows the effects on transitions between employment and unemployment. Column (2) shows the effects on transitions between employment and unemployment. Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1. *Sources:* Integrated Employment Biographies, 2013–2019.

demand perspective, one might expect negative effects on these transitions due to increased hiring standards. The results suggest that the demand channel appears to predominate here.

2.7.2 Complementary Analysis of Hiring Processes via Survey Data

While the analysis of worker transitions provides insights into the dominant driving forces behind the increase in labor market frictions, it is hardly possible to separate labor supply effects from labor demand effects. Thus, it remains unclear whether a transition was triggered by the decisions of a worker or a firm or by repercussions of one side's adjustment on the other. To take a targeted look at minimum wage-related adjustments of labor supply and to confirm the hypothesis of intensified screening on the demand side, I employ survey data from the IAB Job Vacancy Survey (JVS). The JVS is a representative, quarterly establishment survey that gathers information on the most recent instance of an establishment's new hire, which is administered in the fourth quarter of each year. In this survey, establishments are questioned about the search and hiring process for the latest case of a new hire subject to social insurance contributions in the past 12 months. The questionnaire includes questions on the characteristics of the search process, the hired applicant, and features of the filled position.

Using information on new hires from 2014 to 2016, I divide the sample into new hires either below or exactly at the minimum wage (treatment group) and new hires above the minimum wage (control group).[68] To estimate the minimum wage effect on hiring processes, I regress the characteristics of new hires (y) on year dummies ($year$), a treatment dummy ($treated$), the interaction of years and the treatment dummy ($year_t * treated$) and a set of control variables (x) according to Equation (2.4):

$$y_h = \alpha * year_t + \beta * treated + \delta_t * year_t * treated + \sum_{i=1}^{I} \gamma_i x_i + \varepsilon_h \tag{2.4}$$

The control variables include dummies for establishment size classes, the federal state, the industrial sector, the legal form of the establishment, and the level of job requirements at the filled position. These variables are included to account for compositional effects, as the individual annual waves of the IAB Job Vacancy Survey represent unconnected cross-sections, and it is therefore not possible to include fixed effects specific to new hires. The coefficient of the year–treatment interaction identifies the effect of minimum wage hires compared with hires above the minimum wage and relative to the base year 2014.

Table 2.8 shows the estimated effects for δ_t on four characteristics of hiring processes. The number of applicants in the recruiting process (Column 1) can indicate changes in labor

[68] The treatment group comprises new hires with hourly wages of up to 8.5 euros per working hour, whereas the control group comprises new hires with hourly wages of between 8.51 and 10.38 euros per hour. The upper limit of the control group corresponds to the low-wage threshold in Germany. This approach is intended to create homogeneity between the new hires in the treatment and control groups, which differ mainly in their minimum wage exposure.

Table 2.8: Effects of the Minimum Wage Introduction on Hiring Processes

	(1) Number of Applicants	(2) Share of Invited Applicants	(3) Share of suitable Applicants	(4) Compromises in Recruitment
$2015 * bite$	-1.114	-0.026	0.004	-0.063**
	(1.203)	(0.029)	(0.029)	(0.030)
$2016 * bite$	0.026	0.000	-0.001	-0.041
	(1.310)	(0.031)	(0.031)	(0.033)
Observations	3,570	3,269	3,245	4,394

NOTE: The table shows estimated treatment effects from difference-in-differences estimations on the level of new hires. The coefficients show how the outcome variables develop with respect to the bite of the minimum wage and relative to the base year (2014). The bite is calculated from nominal wages (w) of new hires reported by establishments in the questionnaire. Treatment group: w=[0,8.50]. Control group: w=[8.51,10.38]. Control variables are dummies for establishment size classes, federal state, industrial sector, legal form of the establishment and level of job requirements at the filled position. Standard errors are in parentheses. Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1. *Data source:* IAB Job-Vacancy-Survey 2014–2016.

supply available for minimum wage jobs. The negative effect seen in 2015 indicates that the introduction of the minimum wage has not attracted more applicants for new minimum wage jobs; however, this effect is estimated statistically insignificant. Again, the reduction in turnover may mean that there are not more applicants available despite the higher level of job attractiveness due to the minimum wage. There are also no significant effects on the share of invited applicants out of all applicants (Column 2), which can serve as an indicator of screening intensity. In this case, higher recruitment standards could lead to a larger proportion of applicants being screened out before the interview stage. The proportion of applicants who are subjectively perceived by the establishment as being suitable for the position (Column 3) also does not change after the introduction of the minimum wage. However, the effect on the necessity or willingness to make compromises in the recruitment process concerning the characteristics of the person hired (Column 4) is estimated to be negative and statistically significant in 2015. These compromises can be, for example, deviations in the candidate's qualifications or professional experience. The effect suggests that firms are less willing to make such compromises after the introduction of the minimum wage, which in turn argues in favor of stricter recruitment standards. Importantly, the survey information covers only new hires subject to social insurance contributions, thus limiting the comparability with the other analyses in this study.

2.8 Conclusion

According to search-and-matching theory, a binding minimum wage influences not only employers' labor demand decisions but also the matching process itself, for example., by intensifying the screening of applicants or the search behavior of employees. Such adjustments can affect the efficiency of matching in the labor market and thus represent relevant channels of adjustment that may not necessarily result in equilibrium employment responses.

In this paper, I examine the effects of the introduction of the statutory minimum wage in Germany on vacancy openings, as well as their durations and stocks. I employ novel data on the universe of vacancies registered with the Federal Employment Agency combined with administrative data on individual employment histories to estimate minimum wage effects in difference-in-differences specifications on the level of occupations. Comparing occupations of varying exposure to the minimum wage shows that the introduction of the wage floor led to increasing frictions in matching processes. Although the total number of newly opened vacancies did not decrease substantially in affected occupations, a larger share of these vacancies ended in cancellations without being filled. Furthermore, the duration of filled vacancies increased by approximately 5–6 percent in occupations with average exposure to the minimum wage, further emphasizing intensified search frictions.

The findings reveal that, from firms' perspectives, the introduction of the minimum wage in Germany not only resulted in higher wage costs but also implied increased hiring costs, as the latter are positively correlated with vacancy durations (Carbonero and Gartner, 2022). From the perspective of affected workers, the minimum wage introduction provided a wage increase, but they might face stricter selection processes to secure a job opportunity (Butschek, 2022). This might reduce the employability of specific groups like low-skilled workers (Clemens et al., 2021) and workers in highly automatable jobs (Aaronson and Phelan, 2019; Lordan and Neumark, 2018) due to the minimum wage.

Complementary analyses of worker transitions indicate a slight decline in transitions between employers, particularly when these involve a change in occupation. This suggests that the minimum wage has reduced labor turnover. On the one hand, this may signal improved match quality (Dube et al., 2007; Dube et al., 2016); on the other hand, the minimum wage could disadvantage unemployed individuals as shown by lower transition rates from unemployment and reduced firms' willingness to compromise in recruitment processes.

In summary, this study underscores that evaluating minimum wage policy solely through equilibrium wage and employment effects provides an incomplete picture. The findings highlight the importance of vacancies as a critical adjustment channel, revealing that despite modest realized employment effects, the introduction of the minimum wage in Germany has significantly impacted the unrealized side of labor demand through heightened search and matching frictions.

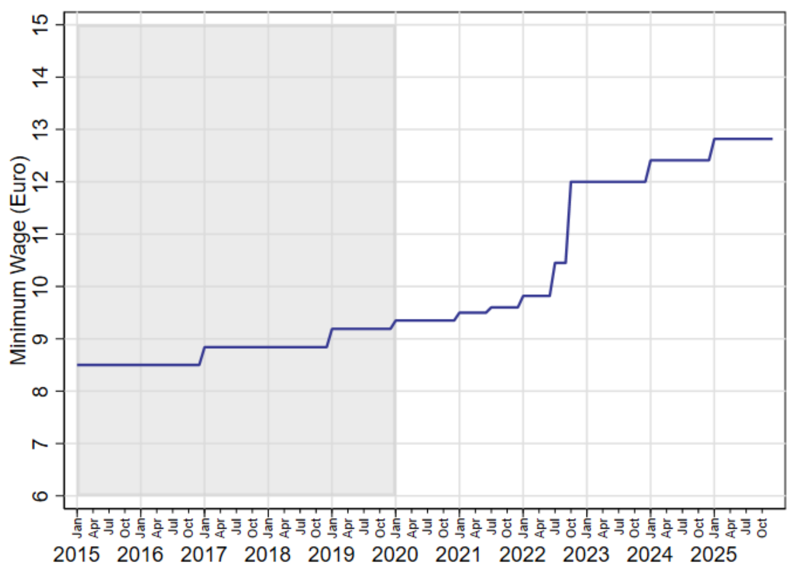
The unique database utilized in this study enables precise measurement of vacancy flows, stocks, and durations. However, it is restricted to vacancies reported to the Federal Employment Agency, which represented 41–49 percent of total vacancies during the analysis period (Bossler et al., 2020). This limitation introduces two key challenges. First, the findings may not fully capture the effects on overall unsatisfied labor demand. Second, potential shifts in firms' reporting behaviors in response to the minimum wage could introduce bias into the estimates. Furthermore, a structural break in occupational classification limits the length of the pre-treatment period, making it more difficult to distinguish anticipation effects in 2014 from placebo effects. Extending the pre-treatment period would allow for a clearer identification of these dynamics.

Future research could benefit from exploring effect heterogeneities related to labor market concentration to uncover variations in adjustment mechanisms between monopsonistic and competitive occupational labor markets. Additionally, adopting a task-based approach to classify occupations may shed light on whether differences in occupational substitutability influence demand-driven minimum wage effects on vacancies.

Appendix - Chapter 2

2.A Level of the Minimum Wage in Germany

Figure 2.A1: Development of the Minimum Wage Level



Note: The line illustrates the level of the German minimum wage from 2015 to 2025. The gray-shaded area encompasses the period of analysis in this paper, extending until the end of 2019.

2.B Structural Break in the Occupation Variable in Administrative Data

A new occupational classification was introduced in 2011 to account for changing occupational structures in the labor market. Employers were required to submit social insurance reports according to the new occupational codes as of December 2011. Until May 2012, however, they were allowed to submit social insurance reports without specifying the occupational code, affecting about 23 percent of reports during this period. In addition, updating effects occured until the end of 2012, because employers updated outdated occupational information of their employees, which no longer corresponded to the occupations actually performed, in the course of the classification change. For these reasons, a substantial structural break occures in the time series, severely limiting comparability. Recoding of the old occupational classification (KldB 1988) into the new occupational classification (KldB 2010) is not possible due to serious methodological differences (Bertat et al., 2013).

Missing occupational codes pose significant challenges for my occupational-level analysis for several reasons. First, they prevent differentiation of occupational transitions, as changes cannot be tracked when the occupational code is missing in a preceding spell. Second, hires with missing occupational data must be excluded, as they cannot be assigned to any occupation. This exclusion skews the number of hires per occupation and distorts the calculation of average hiring wages within occupations. Third, missing occupational information biases the number of vacancies per occupation and potentially also the estimation of average vacancy durations. These gaps collectively undermine the accuracy and reliability of the analysis.

An analysis of the 2012 data reveals that observations with missing occupational information are approximately twice as frequent in 2012 compared to later years. Additionally, the mean hiring wage for these observations is significantly higher in 2012 than in subsequent years. This indicates that the distribution of missing occupational data in 2012 differs substantially from later years, where missing values are less common and concentrated among lower hiring wages. Such discrepancies introduce spurious negative wage effects of the minimum wage in 2012 when estimating a difference-in-differences model. To address these issues, I exclude 2012 from my analysis sample and estimate all specifications using data from 2013 to 2019.

2.C Details on the Administrative Vacancy Data

The data on registered job vacancies is sourced from the Federal Employment Agency's central placement, counseling, and information system (VerBIS) and is provided as largely unprocessed raw data. The data processing for this study builds upon the foundational work of (Börschlein and Popp, 2024), who developed a heuristic to transform this process data into a spell dataset at the individual vacancy level. This manuscript is planned for publication as an FDZ method report at the IAB. In the following, I outline the characteristics of the data and the primary processing steps applied to the vacancy dataset used in this study. The heuristic has been adapted for this study at some steps, which essentially concerns the exclusion of certain vacancy types and occupations.

The vacancy data is collected for the purpose of placement and published on the Job Board of the Federal Employment Agency. This Job Board contains information on job vacancies posted either by establishments themselves or by agency workers who support establishments in their search for new employees. Establishments can use the FEA Job Board in two ways: by engaging directly with the FEA for assistance or by independently posting self-administered job advertisements without the FEA's direct involvement in the vacancy-filling process. For vacancies managed through direct FEA assistance, postings are administered by FEA staff. In contrast, self-administered postings are fully managed by the establishments. Additionally, establishments may commission the FEA to actively search for matches to their vacancies. Consequently, the data encompass three distinct types of vacancy postings:

1. A vacancy posting is administered by the FEA and the FEA has a contract with the establishment to find a match.

2. A vacancy posting is administered by the FEA but the FEA does not have a contract with the establishment to find a match.

3. A vacancy posting is not administered by the FEA.

Information on the reason for closing a vacancy is available only for vacancies with a placement order, corresponding to type 1. For other types, it is not possible to determine whether a vacancy was successfully filled or canceled. Additionally, approximately 40 percent of vacancies without a placement order consist of only a single spell, making it impossible to capture the end of their lifecycle. Due to these data quality limitations, I restrict my analysis to vacancies with placement orders. The resulting sample comprises approximately 16.4 million vacancy observations from 2013 to 2019. I further limit the analysis to vacancies for jobs subject to social security contributions and marginal employment (commonly referred to as "Mini-Jobs" in Germany), excluding those for apprenticeships and internships. This results in a sample size of about 13.0 million vacancy observations, accounting for 79 percent of the original data.

The raw vacancy dataset consists of vacancy packages, which may represent either a single vacancy or multiple vacancies posted by an establishment. The dataset is structured as an event-based spell dataset, meaning a new spell is generated for a vacancy package whenever one of its characteristics changes. Such changes can include the opening of an additional vacancy, the closing of a vacancy, or updates to features such as job type, working time, or other attributes.

The progression of a vacancy package can be tracked using four key variables related to the number of individual vacancies. The variable V_{total} represents the total number of vacancies booked within the package at a given time, regardless of how many remain open. The variable V_{open} indicates the number of vacancies currently open, i.e., still to be filled. Meanwhile, V_{filled} and V_{canceled} reflect the number of vacancies that have already been filled or canceled within the package at the time of a given booking.[69] By combining these variables with the corresponding date information for each booking, it is possible to determine the unique start and end dates for each vacancy as well as its completion status (filled or canceled). However, some vacancy packages exhibit inconsistent booking patterns, making them unsuitable for analysis. To address this, we apply a set of 10 filters to exclude such problematic cases.

1. $V_{\text{total}}|V_{\text{open}}|V_{\text{filled}}|V_{\text{canceled}} < 0$: Stock values for vacancies cannot be negative.

2. $V_{\text{total}} \neq V_{\text{open}} + V_{\text{filled}} + V_{\text{canceled}}$: The total number of vacancies must equal the sum of open, filled, and canceled vacancies within a package.

[69] For clarity, I use simplified variable names that differ from the original dataset.

3. $V_{\text{total}} = 0$: A vacancy package must include at least one vacancy, whether open, filled, or canceled.

4. V_{total} decreases: The total number of vacancies cannot decrease over time; it may only remain constant (when vacancies are filled or canceled) or increase (when new vacancies are added).

5. V_{filled} or V_{canceled} decreases: The number of filled or canceled vacancies must remain constant or increase over time; decreases are not allowed.

6. $V_{\text{open}} > 0$ in the last spell of a closed package: Closed packages should have no open vacancies.

7. $V_{\text{filled}}|V_{\text{canceled}} > 0$ in the first spell of a package: Vacancies cannot appear as already filled or canceled in the first spell, as the package must have been opened earlier for such changes to occur.

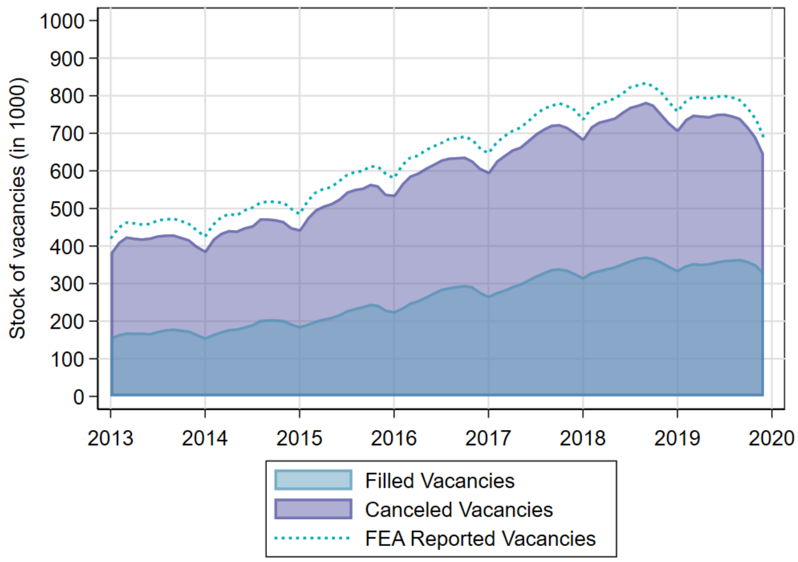
8. $(V_{\text{filled}} + V_{\text{canceled}})^t > V_{\text{open}}^{t-1}$: The combined number of filled and canceled vacancies in a spell cannot exceed the number of open vacancies in the previous spell.

9. V_{open} decreases without an increase in V_{filled} or V_{canceled}: Open vacancies can only decrease when they are either filled or canceled.

10. V_{open} and V_{canceled} increase simultaneously: Simultaneous opening and canceling of vacancies within the same package indicates redundant booking behavior.

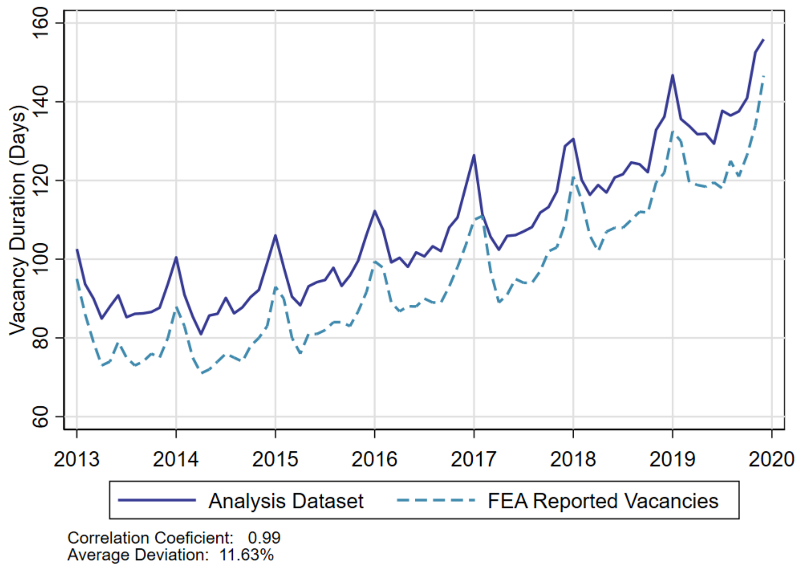
To calculate the stock and duration of individual vacancies, it is essential to disentangle their lifespans. However, when a vacancy package includes multiple individual vacancies opened and/or closed at different times, the exact opening and closing dates for each vacancy are not explicitly defined. To address this ambiguity, we apply a first-in, first-out (FIFO) principle: the first closing event within a vacancy package is assigned to the first opened vacancy, the second closing event to the second opened vacancy, and so forth. This approach assumes that establishments seeking multiple workers for the same occupation fill the longest-standing vacancy first. After disentangling these vacancy packages, the dataset consists of individual vacancies with clearly defined opening and closing dates. This allows for precise calculation of the daily stock of open vacancies throughout the analysis period. Notably, approximately 18.5 percent of individual vacancies originate from packages containing multiple vacancies and were separated using the FIFO principle.

Figure 2.C1: Stock of Vacancies in the Analysis Dataset and Official FEA Statistics



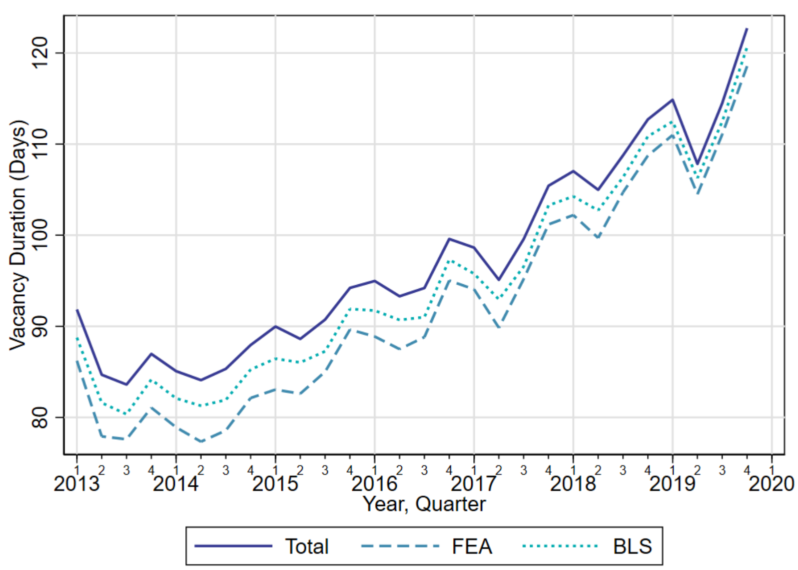
Note: The areas show monthly stocks of open vacancies that were ultimately filled or canceled in the analysis dataset. The area of filled vacancies is stacked on the area of canceled vacancies such that the total height of both areas represents the sum of filled and canceled vacancies. The dashed line shows the stock of vacancies from official statistics of the Federal Employment Agency (FEA).

Figure 2.C2: Vacancy Durations for Filled SIC Vacancy Outflows (Analysis Dataset and Official FEA Statistics)



Note: The figure shows vacancy durations for filled vacancies at the time of vacancy closing in the analysis dataset (solid line) and official statistics of the Federal Employment Agency (FEA)(dashed line). The durations are measured according to the FEA definition.

Figure 2.C3: Vacancy Durations for Filled Vacancy Outflows (Analysis Sample)



Note: The figure shows mean vacancy durations for filled vacancies at the time of vacancy closing. The durations are calculated based on the quarterly aggregated analysis sample for 372 occupations. The lines show the total vacancy durations (solid line), the FEA vacancy duration (dashed line) and the BLS vacancy duration (dotted line).

2.D Details on the IEB Data

The Integrated Employment Biographies (IEB) dataset combines information on employment records and recipients of transfer payments from various administrative sources (see Müller and Wolter, 2020). Employment data is collected through mandatory employer reports to the German social security system, covering all employees subject to social security contributions and marginal workers employed full- or part-time. The dataset includes firm and individual identifiers and follows a spell-based structure, with each spell representing an employment period at the daily level. For each employment spell, detailed information is available, including the type of notification, daily wage, 5-digit occupation code, type of employment (social security obligation, mini-job, trainees, and others), and workplace location, among other variables.

Identification of Connected Employment Periods

To identify new hires and worker flows between occupations and establishments, I first identify coherent spells, referring to an uninterrupted employment period of a worker with an establishment. Because establishment identifiers may change over time, I consolidate them using worker flow information from the Establishment History Panel (BHP) as outlined in Hethey and Schmieder (2010). Following Jaenichen (2018), an employment period is defined by a spell of employment lasting at least 3 days at a new firm, with subsequent spells of employment at the same firm within 32 days being assigned to the same employment period. Additionally, to distinguish re-employments from actual new hires after employment interruptions, I follow Gürtzgen and Küfner (2021) and pool subsequent employment spells at the same establishment if:

- the length of the gap between two employment spells is at most 365 days after an illness,
- the new employment spell occurs within 1096 days after a mother has left the establishment to give birth (for the identification, see Müller and Strauch, 2017),
- the new employment spell occurs within 1096 days after an employee has left the establishment for parental leave.

In this procedure, the occurrence of illness, childbirth, and parental leave are identified using various types of notifications for transfer payments in the dataset. Periods of illness of more than six weeks are identified by notifications of type 51 indicating an "interruption notification due to receipt of or entitlement to compensation benefits" if the spell does not simultaneously indicate that a mother has left the establishment to give birth (according to Müller and Strauch, 2017). Periods of parental leave are identified by notifications of type 52 indicating an "interruption notification due to parental leave".

Based on the connected employment periods, I determine the start and end dates of employment relationships, track occupation changes within a relationship, and calculate wages for each employment period. When multiple social security notifications occur within the same employment relationship in a year, I identify a primary spell, as described in the next section. Following Gürtzgen and Küfner (2021), I exclude individuals with over 15 short employment spells (lasting fewer than three days) or more than 100 employment relationships. This minimizes the influence of rare, atypical employment biographies, which may also result from measurement errors affecting the count of hirings. Furthermore, I exclude employment periods shorter than 30 days to avoid skewing the analysis with short and potentially unstable employment relationships.

Concatenation of Subspells and Exclusions

After consolidating related employment spells, I create an annual panel at the level of employment relationships by retaining a single spell per year for each employment period of an individual. If an employer submits multiple social security notifications for the same employee in a given year, the dataset contains multiple spells for that year. Employers are required to submit an annual notification for employment relationships extending beyond December 31$^{\text{st}}$. Termination notifications are submitted when an employment relationship ends, and additional notifications may arise due to changes in employment type, health insurance adjustments, or periods of illness with wage replacement benefits. To identify the primary spell within an employment period for each year, I prioritize the spell with the highest daily wage. If duplicates remain after this step, I retain the longest spell, followed by prioritization based on notification types (annual notification, termination notification, and other notification types).

Any remaining duplicates are resolved through random selection. The resulting dataset includes employment periods with exactly one spell per year, covering the entire duration of each employment relationship. While individuals may hold multiple simultaneous jobs with different employers, overlapping spells within the same employer are excluded to maintain data consistency.

Identification of Worker Flows

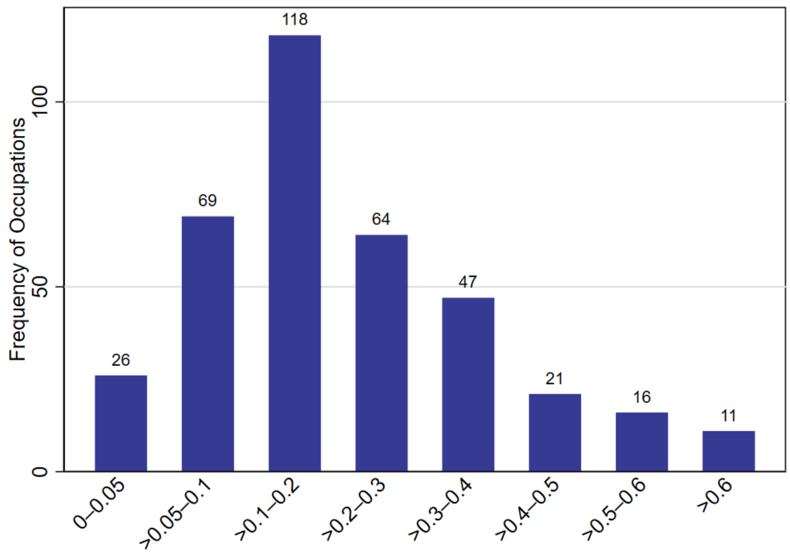
In order to analyze spillover effects between high-bite and low-bite occupations and to examine the underlying mechanisms of the minimum wage effects on vacancies (see Appendix 2.G and Section 2.7.1), I define worker flows between occupations and employers from the disaggregated individual-level data.

An inflow into occupation o occurs when a worker is employed in occupation o in year t but had no employment spell in occupation o during the 365 days preceding the start of employment in year t. Conversely, an outflow from occupation o is identified when a worker was employed in occupation o in year *t-1* but has no employment spell in that occupation within the 365 days following the end of employment in year *t-1*. These occupational flows are measured independently of whether the employment change involves a new employer (new hire) or takes place within the same employment relationship.

I also identify movements of workers between employers, which may be either concurrent with a change in occupation or occur within the same occupation. To conceptually distinguish the difference in worker movements between occupations and worker movements between employers, I use the term "transition" for the latter as opposed to "flow" for occupational changes. Additionally, I distinguish transitions between different employers (EE-transitions) and from non-employment (UE-transition) as well as into non-employment (EU-transition). An EE-transition is identified when a worker is newly hired at establishment e and was not employed at establishment e in the 365 days before, and was not or no longer than 92 days unemployed before the hire. Periods of unemployment shorter than three months are generally considered as frictional or search unemployment between two consecutive jobs in contrast to structural unemployment. Furthermore, I differentiate between EE-transitions where the employee did not work in the same occupation in the 365 days before the new hire (EE-transition between occupations) and EE-transitions where the employee was already working at another firm in the same occupation in the 365 days before the new hire (EE-transition within occupation). A EE-transition between occupations is basically a new hire in combination with an occupational inflow. Similarly, an EE-transition within occupation is a new hire without an occupational inflow.

2.E Hiring Bite Descriptives

Figure 2.E1: Frequency of Occupations by the Hiring Bite of the Minimum Wage



Note: The bars show the number of occupations within bite ranges. The bite is calculated as the share of workers hired at hourly wages below 8.50 Euro in 2014. The distribution of the bite variable is divided into eight groups for illustrative purposes.

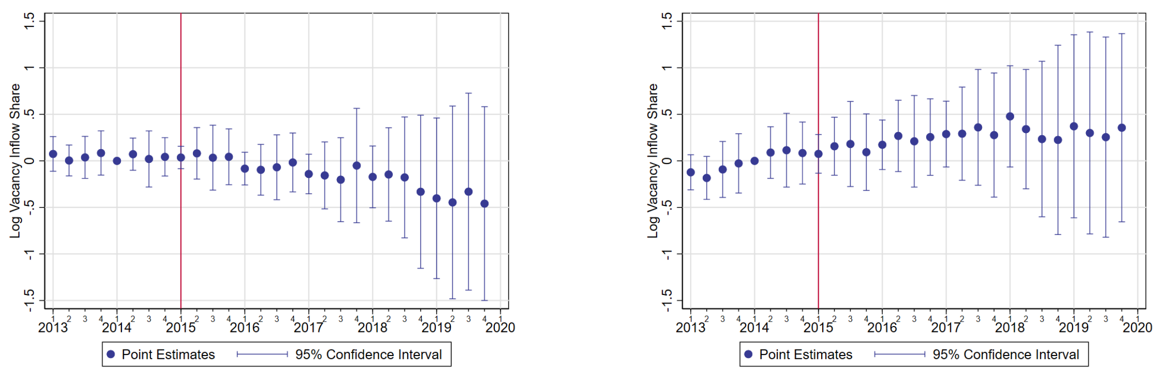
2.F Vacancy Effects: Additional Results

Table 2.F1: Annual Effects of the Minimum Wage Introduction on the Number of Vacancy Inflows

	(1) All	(2) Filled	(3) Canceled
$2013 * bite$	base -	base -	base -
$2014 * bite$	0.353 (0.276)	0.020 (0.158)	0.334** (0.159)
$2015 * bite$	1.115* (0.653)	0.342 (0.371)	0.773** (0.332)
$2016 * bite$	1.337 (0.851)	0.090 (0.431)	1.246** (0.528)
$2017 * bite$	1.375 (1.371)	0.016 (0.709)	1.359* (0.764)
$2018 * bite$	0.959 (1.490)	-0.411 (0.940)	1.370* (0.758)
$2019 * bite$	0.549 (2.375)	-1.035 (1.641)	1.584* (0.891)
Observations	2,604	2,604	2,604
Clusters	372	372	372

NOTE: The table shows estimates of the treatment effects on the treated derived from difference-in-differences specifications with occupation-level fixed effects. Treatment intensity (bite) is measured continuously as the share of hires at or below the minimum wage in the year 2014. The coefficients indicate how the outcome variables develop w.r.t. the bite of the minimum wage and relative to the base year (2013). Standard errors (in parentheses) are clustered at the occupational level. The regressions are weighted by the occupational labor market size which is defined as the average number of jobs in each occupation over the years 2013-2019. The outcome variables are the absolute number of vacancy inflows per occupation. Column (1) shows the effects on all opened vacancies. Columns (2) and (3) show the effects on opened vacancies that were ultimately filled or canceled. Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1. *Sources:* Integrated Employment Biographies + vacancies registered with the German Federal Employment Agency, 2013-2019.

Figure 2.F1: Quarterly Effects of the Minimum Wage Introduction on the Share of Vacancy Inflows



(a) Filled Vacancies

(b) Canceled Vacancies

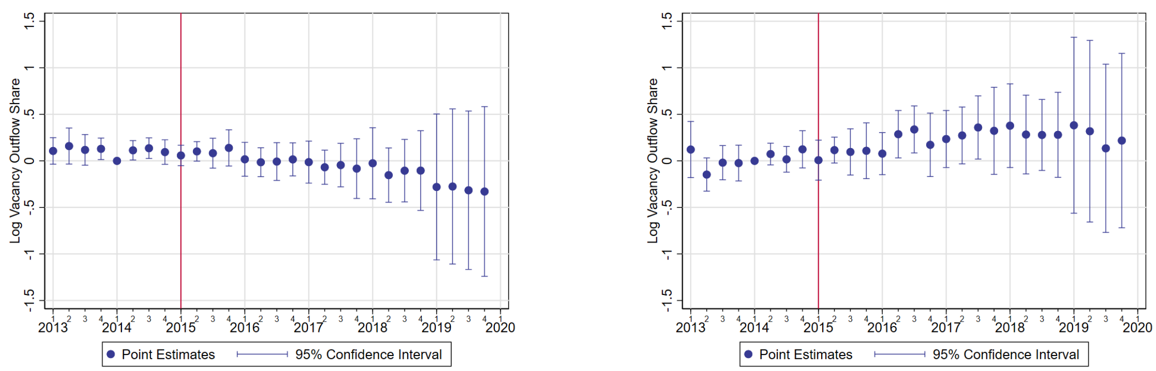
Note: The figure shows estimates of the treatment effects on the treated derived from difference-in-differences specifications with occupation-level fixed effects estimated on the quarterly level. To account for bite-specific seasonal variation, the outcome variables are adjusted for a quarter-bite-specific trend as shown in Equation (2.3). Treatment intensity (bite) is measured continuously as the share of hires at or below the minimum wage in the year 2014. The coefficients indicate how the outcome variables develop w.r.t. the bite of the minimum wage and relative to the base period (2014/Q1). The vertical bars indicate 95% confidence intervals. Standard errors (in parentheses) are clustered at the occupational level. The regressions are weighted by the occupational labor market size which is defined as the average number of jobs in each occupation over the years 2013-2019. The outcome variables are the natural logarithm of the share of vacancy inflows on all jobs within occupations in each quarter. Panel (a) shows the effects on newly opened vacancies that were ultimately filled. Panel (b) shows the effects on newly opened vacancies that were ultimately canceled. *Sources:* Integrated Employment Biographies + vacancies registered with the German Federal Employment Agency, 2013-2019.

Table 2.F2: Annual Effects of the Minimum Wage Introduction on the Share of Vacancy Outflows

	(1) All	(2) Filled	(3) Canceled
2013 * *bite*	base	base	base
	-	-	-
2014 * *bite*	-0.016	-0.043	0.075
	(0.064)	(0.059)	(0.099)
2015 * *bite*	0.004	-0.030	0.100
	(0.099)	(0.093)	(0.129)
2016 * *bite*	-0.017	-0.126	0.221
	(0.118)	(0.111)	(0.145)
2017 * *bite*	-0.032	-0.178	0.310
	(0.165)	(0.150)	(0.191)
2018 * *bite*	-0.060	-0.216	0.318
	(0.200)	(0.190)	(0.224)
2019 * *bite*	-0.217	-0.439	0.284
	(0.440)	(0.429)	(0.469)
Observations	2,604	2,604	2,602
Clusters	372	372	372

NOTE: The table shows estimates of the treatment effects on the treated derived from difference-in-differences specifications with occupation-level fixed effects. Treatment intensity (bite) is measured continuously as the share of hires at or below the minimum wage in the year 2014. The coefficients indicate how the outcome variables develop w.r.t. the bite of the minimum wage and relative to the base year (2013). Standard errors (in parentheses) are clustered at the occupational level. The regressions are weighted by the occupational labor market size, which is defined as the average number of jobs in each occupation over the years 2013-2019. The outcome variables are the natural logarithm of the ratio of vacancy outflows to the number of jobs per occupation. Column (1) shows the effects on all closed vacancies. Columns (2) and (3) show the effects on closed vacancies that were filled or canceled. Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1. *Sources:* Integrated Employment Biographies + vacancies registered with the German Federal Employment Agency, 2013-2019.

Figure 2.F2: Quarterly Effects of the Minimum Wage Introduction on the Share of Vacancy Outflows



(a) Filled Vacancies (b) Canceled Vacancies

Note: The figure shows estimates of the treatment effects on the treated derived from difference-in-differences specifications with occupation-level fixed effects estimated on the quarterly level. To account for bite-specific seasonal variation, the outcome variables are adjusted for a quarter-bite specific trend as shown in Equation (2.3).Treatment intensity (bite) is measured continuously as the share of hires at or below the minimum wage in the year 2014. The coefficients indicate how the outcome variables develop w.r.t. the bite of the minimum wage and relative to the base period (2014/Q1). The vertical bars indicate 95% confidence intervals. Standard errors (in parentheses) are clustered at the occupational level. The regressions are weighted by the occupational labor market size, which is defined as the average number of jobs in each occupation over the years 2013-2019. The outcome variables are the natural logarithm of the share of vacancy outflows on all jobs within occupations in each quarter. Panel (a) shows the effects on closed vacancies that were filled. Panel (b) shows the effects on closed vacancies that were canceled. *Sources:* Integrated Employment Biographies + vacancies registered with the German Federal Employment Agency, 2013-2019.

Table 2.F3: Annual Effects of the Minimum Wage Introduction on Vacancy Inflows Shares in Occupations with Low Requirement Levels

	(1) All	(2) Filled	(3) Canceled
$2013 * bite$	base -	base -	base -
$2014 * bite$	-0.069 (0.074)	-0.096 (0.071)	0.056 (0.085)
$2015 * bite$	-0.067 (0.107)	-0.140 (0.110)	0.147 (0.106)
$2016 * bite$	-0.127 (0.131)	-0.235* (0.130)	0.199 (0.133)
$2017 * bite$	-0.181 (0.203)	-0.300 (0.189)	0.220 (0.228)
$2018 * bite$	-0.191 (0.295)	-0.320 (0.278)	0.255 (0.343)
$2019 * bite$	-0.352 (0.539)	-0.537 (0.552)	0.186 (0.539)
Observations	1,141	1,141	1,141
Clusters	163	163	163

NOTE: The table shows estimates of the treatment effects on the treated derived from difference-in-differences specifications with occupation-level fixed effects. Treatment intensity (bite) is measured continuously as the share of hires at or below the minimum wage in the year 2014. The coefficients indicate how the outcome variables develop w.r.t. the bite of the minimum wage and relative to the base year (2013). Standard errors (in parentheses) are clustered at the occupational level. The regressions are weighted by the occupational labor market size, which is defined as the average number of jobs in each occupation over the years 2013-2019. The outcome variables are the natural logarithm of the share of vacancy inflows on all jobs within occupations in each year. Column (1) shows the effects on all newly opened vacancies. Columns (2) and (3) show the effects on newly opened vacancies that were ultimately filled or canceled. The estimation sample is restricted to occupations with task requirement levels 1 and 2. Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1. *Sources:* Integrated Employment Biographies + vacancies registered with the German Federal Employment Agency, 2013-2019.

Table 2.F4: Annual Effects of the Minimum Wage Introduction on the Durations of Opening Vacancies in Occupations with Low Requirement Levels

	(1) Total	(2) FEA	(3) BLS
$2013 * bite$	base	base	base
	-	-	-
$2014 * bite$	0.116***	0.114***	0.109***
	(0.028)	(0.028)	(0.027)
$2015 * bite$	0.195***	0.186***	0.190***
	(0.035)	(0.036)	(0.034)
$2016 * bite$	0.194***	0.178***	0.189***
	(0.043)	(0.041)	(0.040)
$2017 * bite$	0.268***	0.254***	0.265***
	(0.071)	(0.072)	(0.073)
$2018 * bite$	0.314**	0.299**	0.308**
	(0.121)	(0.122)	(0.122)
$2019 * bite$	0.293***	0.279***	0.294***
	(0.069)	(0.071)	(0.073)
Observations	1,141	1,141	1,141
Clusters	163	163	163

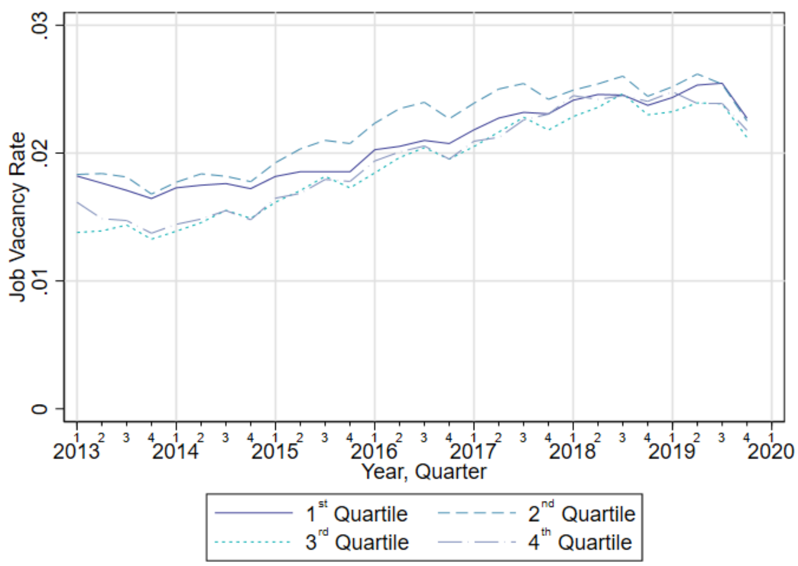
NOTE: The table shows estimates of the treatment effects on the treated derived from difference-in-differences specifications with occupation-level fixed effects. Treatment intensity (bite) is measured continuously as the share of hires at or below the minimum wage in the year 2014. The coefficients indicate how the outcome variables develop w.r.t. the bite of the minimum wage and relative to the base year (2013). Standard errors (in parentheses) are clustered at the occupational level. The regressions are weighted by the occupational labor market size, which is defined as the average number of jobs in each occupation over the years 2013-2019. The outcome variables are the natural logarithm of the vacancy durations of newly opened vacancies. The coefficients show, for each year, the minimum wage effects on the duration of those (completed) vacancies opened in the respective year. The columns show the effects on total vacancy duration (1), FEA vacancy duration (2), and BLS vacancy duration (3). The estimation sample is restricted to occupations with task requirement levels 1 and 2. Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1. *Sources:* Integrated Employment Biographies + vacancies registered with the German Federal Employment Agency, 2013-2019.

Table 2.F5: Annual Effects of the Minimum Wage Introduction on the Durations of Closing Vacancies

	(1) Total	(2) FEA	(3) BLS
$2013 * bite$	base -	base -	base -
$2014 * bite$	0.026 (0.041)	-0.001 (0.048)	0.010 (0.044)
$2015 * bite$	0.132*** (0.044)	0.103** (0.047)	0.120*** (0.044)
$2016 * bite$	0.254*** (0.057)	0.244*** (0.062)	0.255*** (0.058)
$2017 * bite$	0.331*** (0.064)	0.298*** (0.065)	0.324*** (0.062)
$2018 * bite$	0.407*** (0.076)	0.374*** (0.075)	0.393*** (0.075)
$2019 * bite$	0.498*** (0.077)	0.476*** (0.072)	0.494*** (0.074)
Observations	2,604	2,604	2,604
Clusters	372	372	372

NOTE: The table shows estimates of the treatment effects on the treated derived from difference-in-differences specifications with occupation-level fixed effects. Treatment intensity (bite) is measured continuously as the share of hires at or below the minimum wage in the year 2014. The coefficients indicate how the outcome variables develop w.r.t. the bite of the minimum wage and relative to the base year (2013). Standard errors (in parentheses) are clustered at the occupational level. The regressions are weighted by the occupational labor market size, which is defined as the average number of jobs in each occupation over the years 2013-2019. The outcome variables are the natural logarithm of the vacancy durations of closing vacancies. The coefficients show, for each year, the minimum wage effects on the duration of those (completed) vacancies closed in the respective year. The columns show the effects on total vacancy duration (1), FEA vacancy duration (2), and BLS vacancy duration (3). Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1. *Sources:* Integrated Employment Biographies + vacancies registered with the German Federal Employment Agency, 2013-2019.

Figure 2.F3: Job Vacancy Rate by Bite Quartile



Note: The figure shows the quarterly average job vacancy ratio by quartiles of the bite distribution. *Sources:* Integrated Employment Biographies + vacancies registered with the German Federal Employment Agency, 2013-2019.

2.G Occupational Reallocation Indicating Spillover

The identification of causal effects within a Neyman-Rubin causal model framework relies on the assumption that the treatment assigned to some unit must not affect the potential outcome of some other unit (SUTVA assumption, see Rubin, 1980). In the context of minimum wage evaluation, this assumption implies that the outcomes in occupations that are not (or only weakly) affected by the minimum wage must not be influenced by the minimum wage effect on highly affected occupations. Otherwise, treatment effects can spillover to the control group (low-bite occupations). Such spillover effects can bias average treatment effects in a difference-in-differences model, as the potential outcome of the control group is affected by the treatment such that the control group does not identify the counterfactual trend (Imbens and Rubin, 2015).

If the treatment affects the control group in the same direction as the treatment group, average treatment effects are biased downwards. In turn, the average treatment effects are biased upwards, if the treatment affects the control group in the opposite manner to how it affects the treatment group. The direction of the induced bias is not clear in general. In the minimum wage literature, spillover effects are typically analyzed in the context of wages and employment. For instance, wage spillovers may arise when firms seek to preserve wage differentials between skill groups or when labor demand shifts from minimum wage workers to other groups (Bossler and Schank, 2023). Positive wage spillovers are commonly observed among employees earning slightly above the minimum wage (e.g., Cengiz et al., 2019; Demir, 2024; Gopalan et al., 2021; Neumark et al., 2004) and can extend to about the midpoint of the wage distribution (Bossler and Schank, 2023; Gregory and Zierahn, 2022). Some studies also show positive spillover effects on employment for employees paid just above the minimum wage (Cengiz et al., 2019; Gregory and Zierahn, 2022), while negative employment spillover effects also occur in the upper part of the wage distribution (Aretz et al., 2013; Gregory and Zierahn, 2022).

Spillover effects between occupations are especially likely in labor markets that are not strongly segmented. Such spillovers can arise when employees can transition between occupations without significant retraining or additional qualifications or when firms can substitute one occupation for another. On the labor supply side, workers may be drawn to minimum-wage occupations as these jobs become more attractive due to higher wages after the minimum wage introduction. This shift can reduce labor supply in occupations that are not affected by the minimum wage. On the labor demand side, recent studies show increasing capital investments (Harasztosi and Lindner, 2019) and the substitution of labor by capital in highly automatable occupations (Aaronson and Phelan, 2019; Lordan and Neumark, 2018) due to a minimum wage increase.

To uncover potential spillover effects on the occupational level, I utilize worker flows between occupations. Structural changes in worker flows between high-bite and low-bite occupations after the minimum wage introduction would indicate spillover effects. It is noteworthy that changes in worker flows between occupations themselves are not an indicator of spillovers but merely indicate occupation-specific reallocation of employees. Only if this minimum-wage-induced reallocation occurs between high-bite and low-bite occupations, the minimum wage affects the labor supply available to low-bite occupations, resulting in spillover effects.

To quantify the degree of spillovers, I construct a measure that compares the bite of a worker's occupation before and after the worker's occupation changes. Therefore, I identify worker flows between occupations at the individual level using the IEB data (see Appendix 2.D. A flow captures the change of a worker's occupation and does not distinguish whether the occupational change is associated with a change of the employer or occurs within the same firm. By measuring all reallocations between occupations, not only between employers and occupations, this flow measure also includes the substitution of occupations within firms.[70] Each worker can have multiple simultaneous jobs in possibly different occupations per year. An inflow into occupation o is identified for a worker that is employed in occupation o in year t but had no employment spell in occupation o in the previous 365 days. Similarly, an outflow from occupation o is identified when a worker worked in occupation o in the year $t-1$, but no employment spell in occupation o is observed in the 365 days after the end of the employment spell in occupation o for that worker in the year t. The reference periods of

[70]This definition differs from worker flows between employers, which are analyzed in Section 2.7.1. To distinguish between the two concepts, I use the terms "flows" of workers between occupations (regardless of the change of employer) and "transitions" of workers between employers (regardless of the change of occupation), which is used in Section 2.7.1.

the flows are chosen to capture potential minimum wage effects expected from the year 2015 onward.[71] Since my analysis sample starts in the year 2013, flows are identified for the year 2014 onwards.

For each identified inflow between 2014 and 2019, I compare the bite of each worker's previous occupation to the bite of the occupation the worker flows into. The bite always refers to the hiring bite in the year 2014, and is held constant for each occupation, as described in Section 2.4.2. If a worker had multiple jobs in different occupations in $t-1$, the bite is calculated as the average of the bites of all jobs in $t-1$. The difference between the bite in $t-1$ and the bite of the inflow occupation o then yields the bite gain from an inflow into occupation o in t. Similarly, the bite gain from an outflow from occupation o is derived from the difference between the bite of occupation o in t and the bite of the occupation in which the worker is employed in year $t+1$.[72] Aggregated at the occupational level, the bite gain thus indicates, for each occupation, the average bite difference for inflows into or outflows from that occupation. A positive (negative) average bite gain for inflows indicates that workers flowing into occupation o on average come from occupations with lower (higher) bites. Similarly, a positive (negative) bite gain for outflows indicates that workers leaving occupation o on average flow into occupations with lower (higher) bites. Changes in the average bite gain are used to identify whether systematic differences in worker flows between occupations with respect to minimum wage exposure emerge after the minimum wage introduction.

Table 2.G1 shows estimates of the minimum wage effect on the average bite gain from inflows (Column 1) and outflows (Column 2) from difference-in-difference models according to Equation (2.1). The positive treatment effect for inflows in 2015 indicates that workers flowing into high-bite occupations in 2015, on average, more often come from occupations with a lower bite compared to inflows in 2014. In the later years (except 2019), the treatment effects are not estimated to be statistically significant, and their estimates are close to zero. The effect of 0.006

[71] Consider the following example to clarify the assignment of flows to years: An inflow is assigned to the year 2015 if a worker works in occupation o at any time in 2015 and no employment in occupation o is observed for this worker in the 365 days prior to the start of the employment spell in o in 2015. An outflow is assigned to the year 2015 if a worker worked in occupation o at any time in 2014, and no employment spell is observed for this worker in occupation o in the 365 days after the end of the employment spell in o in 2014.

[72] The average bite of the jobs in $t+1$ is used for outflows of workers with multiple employment spells in $t+1$.

Table 2.G1: Annual Effects of the Minimum Wage Introduction on the Average Bite Gain from Occupational Flows

	(1) Bite Gain Inflow	(2) Bite Gain Outflow
$2014 * bite$	base	base
	-	-
$2015 * bite$	0.006***	0.002
	(0.002)	(0.002)
$2016 * bite$	0.001	-0.001
	(0.002)	(0.002)
$2017 * bite$	0.002	-0.001
	(0.004)	(0.004)
$2018 * bite$	-0.002	-0.004
	(0.005)	(0.003)
$2019 * bite$	0.007**	-0.017***
	(0.003)	(0.005)
Observations	2,232	2,232
Clusters	372	372

NOTE: The table shows estimates of the treatment effects on the treated derived from difference-in-differences specifications with occupation-level fixed effects. Treatment intensity (bite) is measured continuously as the share of hires at or below the minimum wage in the year 2014. The coefficients indicate how the outcome variables develop w.r.t. the bite of the minimum wage and relative to the base year (2013). Standard errors (in parentheses) are clustered at the occupational level. The regressions are weighted by the occupational labor market size, which is defined as the average number of jobs in each occupation over the years 2013-2019. The outcome variables are the average bite gain from workers that work in occupation o in t but worked in a different occupation in $t-1$ (Inflow, Column 1) and the average bite gain from workers that worked in occupation o in $t-1$ but work in a different occupation in t (Outflow, Column 2). Asterisks indicate the statistical significance level: *** p<0.01, ** p<0.05, and * p<0.1. *Sources:* Integrated Employment Biographies + vacancies registered with the German Federal Employment Agency, 2013-2019.

shows that the mean occupational bite of all employees flowing into occupations with bite=1 was 0.006 higher in 2015 than for inflows into occupations with bite=0 and relative to the base year 2014. The marginal effect is, therefore, relatively small in relation to the mean bite (0.22). Thus, there are indications of minor short-term spillover effects. In subsequent years (2016-2018), the point estimates are even closer to zero and statistically insignificant. The effect on the bite gain of outflows is even smaller and is estimated to be statistically insignificant in the years 2015 to 2018. Therefore, the introduction of the minimum wage did not substantially change the structure of worker flows between occupations with different bites, which argues against significant spillover effects. Still, there are large and statistically significant effects in 2019. A possible explanation is that the data show indications of temporary recoding of certain occupations in 2018, so that some occupations are assigned a different occupational code. This could lead to the erroneous identification of in- or outflows that do not represent actual employment flows between the occupations. Nevertheless, I argue that a possible structural break in 2018 does not invalidate the analysis of spillover effects for the minimum wage, as these should occur close to the time of the minimum wage introduction.

Chapter 3

Scarce Workers, High Wages?

Abstract[*]

Labor market tightness tremendously increased in Germany between 2012 and 2022. We analyze the effect of tightness on wages by combining social security data with unusually rich information on vacancies and job seekers. Instrumental variable regressions reveal positive elasticities between 0.004 and 0.011, implying that higher tightness explains between 7 and 19 percent of the real wage increase. We report greater elasticities for new hires, high-skilled workers, the Eastern German labor market, and the service sector. In particular, tightness raised wages at the bottom of the wage distribution, contributing to the decline in wage inequality over the last decade.

JEL Classification: J31, J63, J64

Keywords: labor market tightness, wages, labor shortage, occupations, wage inequality

[*] This chapter is joint work with Mario Bossler and Martin Popp. The paper was submitted and went under review in the *Journal of Labor Economics* in August 2024.

3.1 Introduction

While the labor market in reunified Germany was plagued by high unemployment figures until the mid-2000s, the subsequent one-and-a-half decades were – notwithstanding several crises – characterized by robust GDP growth and a substantial increase in employment. This turnaround is widely attributed to a sustained period of wage restraint (Dustmann et al., 2014; Hoffmann and Lemieux, 2016) and a set of labor market reforms (called "Hartz laws") which aimed at reducing structural unemployment by flexibilizing the labor market (Krause and Uhlig, 2012; Launov and Wälde, 2016; Bradley and Kügler, 2019; Hochmuth et al., 2021). As a result, firms were posting an increasing number of vacancies, and – bolstered by the long-term demographic decline of domestic workers – the number of unemployed fell. Consequently, the formerly slack labor market in Germany began to tighten. The tightening continued in the following years and accelerated during the mid-2010s. In 2022, labor market tightness in Germany is at an all-time high, implying that the number of vacancies per job seeker has tripled since 2010.

The unprecedented levels of tightness are making recruitment more and more difficult for firms in Germany, while workers are finding it much easier to take up new jobs. Through the lens of different economic models, such as the standard supply-demand, monopsony, search-and-matching, or bargaining model, an increase in labor market tightness is supposed to translate into higher wage levels – in terms of either a new labor market equilibrium under (im-)perfect competition or an improved bargaining position of workers relative to firms. This so-called "wage-(setting-)curve relationship" raises the question of how much wages have risen due to the tightening of the German labor market. Unfortunately, estimating the relationship between tightness and wages proves difficult in two respects. Importantly, detailed information on the relevant numbers of vacancies and job seekers for regional labor markets by occupation is hardly available. Moreover, failure to trace out exogenous variation in labor market tightness to the individual worker will result in biased estimates.

In this paper, we analyze the effects of the tremendous increase in labor market tightness on wages in Germany from 2012 to 2022. Our analysis is based on longitudinal information on workers from the German social security records. We enrich this dataset with exceptionally rich information on vacancies and job seekers that allows us to directly calculate tightness for labor markets in terms of fine-grained combinations of occupations and regions. When estimating the wage-(setting-)curve relationship, we perform Mincer-type wage regressions with multidimensional fixed effects to absorb unobserved systematic differences across years, workers, labor markets, and firms. To address endogeneity from reverse causality and local productivity shocks, we additionally build on the popular leave-one-out instrumental variable strategy from Azar et al. (2022) and, in doing so, we are the first to transfer their design from concentration indices to ratios of vacancies to job seekers. Specifically, we instrument log labor market tightness in a particular occupation, region, and year by the average log tightness in all other regions for the very same occupation and year. By virtue of this leave-one-out design, our instrument harnesses only variation in tightness from national or non-local forces and is therefore immune against feedback effects or shocks from the same labor market.

Our OLS regressions with varying sets of control variables and fixed effects deliver statistically significant elasticities of labor market tightness on wages between 0.007 and 0.009. After successfully passing first-stage diagnostics, our IV regressions show the same positive sign but turn out somewhat higher, seemingly addressing downward bias from reverse causality. Our baseline IV regression with the full set of fixed effects and controls shows a significantly positive elasticity of 0.011, implying that an increase in labor market tightness by 100 log points raises the daily gross wages of regular full-time workers on average by 1.1 percent.

We acknowledge that our baseline IV estimate is upward-biased in the presence of occupation-specific productivity shocks at the national level, which our leave-one-out instrument does not protect against. Therefore, we use several proxies to control for these shocks with varying rigor. Under the most rigorous proxy (i.e., when conditioning on the number of vacancies in an occupation), the elasticity turns out lower but remains significantly positive with a value of 0.004, which plausibly forms a lower bound of the causal effect of log labor market tightness on log wages. Although the upper bound of 0.011 exceeds the lower bound by factor 2.6, both values imply positive but limited wage gains from the rising tightness. In light of our effect interval, the increase in tightness can explain between 7.4 and 19.1 percent of the rise in real wages in the German labor market between 2012 and 2022.

We perform a large variety of robustness checks to test the sensitivity of our baseline IV effect – namely in terms of narrower or broader labor market definitions, a flow-adjusted version of labor market tightness that takes additionally into account vacancies and job seekers from neighboring occupations, the handling of unregistered vacancies, trimming extreme values, and quadratic effects. In all checks, the effect of tightness on wages turns out to be in close proximity to our upper-bound elasticity of 0.011.

Further analyses of heterogeneous effects for subgroups show that the elasticities are comparatively higher for newly hired workers, specialists, experts, high-skilled workers, and workers in Eastern Germany and in the service sector. Moreover, an additional analysis by wage deciles highlights that workers in the low-wage segment particularly benefit from rising labor market tightness. In light of this pattern, we show that the rising tightness has contributed to the observed decline in wage inequality over the last decade. Moreover, final analyses of firm-level wage-setting indicate that most of our effect stems from low-paying firms raising their overall wage level in response to higher tightness across the set of employed occupations. Put differently, only a small share of the effect can be attributed to firms raising wages differently depending on the tightness increases in the respective occupations. In the context of Card et al. (2013), who describe wage inequality as a combination of worker-specific and firm-specific heterogeneities, our result shows that increasing labor market tightness indirectly reduces the variance of firm-specific wage premia and, thus, wage inequality.

Our study contributes to a better understanding of wage-setting policies, tight labor markets, and their mutual interdependencies. First, we add to the growing literature on the general consequences of scarcity of labor input. In recent years, the literature has gathered new momentum as labor has become increasingly scarce in U.S. and European markets (Abraham et al., 2020; Groiss and Sondermann, 2024). While search-and-matching theory advocates the use of tightness (i.e., the ratio of vacancies to job seekers or unemployed), many studies are resorting to alternative scarcity measures like employment rates, job-to-job transition rates, vacancy durations, or self-reported labor shortages. An increasing number of studies quantifies how scarce labor has manifested in lower (growth of) employment in the U.K. (Stevens, 2007), the U.S. (Beaudry et al., 2018), Germany (Bossler and Popp, 2024), and France (Le Barbanchon et al., 2024). While insufficient labor supply may negatively affect firms' productivity (Haskel and Martin, 1993), bargaining power (Hirsch et al., 2018; Pezold et al., 2023), and profits, Reder (1955) argues that firms may counteract hiring difficulties by either raising wages to retain and attract workers or by lowering hiring standards. Otherwise, firms might compensate for the lack of manpower by increasing capital usage (D'Acunto et al., 2020; Lipowski, 2024) or adjusting labor contracts of incumbent workers (Houseman et al., 2003; Fang, 2009; Healy et al., 2015).

Causal evidence on the effects of labor market tightness (or proxies thereof) on wages is small but rapidly growing. For the post-Covid U.S. economy, Autor et al. (2023) find that rising tightness (as a composite of job-to-job transitions and unemployment) primarily raised wages at the bottom of the wage distribution, which counteracted rising wage inequality. Using Danish register data at the firm level, Hoeck (2023) arrives at an elasticity of wages with respect to tightness between 0.01 and 0.02. For Germany, Linckh et al. (2024) build on cross-sectional information on 533 German workers in 2017/18 and trace out a positive but insignificant semi-elasticity of tightness (in levels) on entry wages (in logs).[73] Unlike the aforementioned studies, Bossler and Popp (2024) use an instrumental variable approach to explicitly address endogeneity from reverse causality and omitted variable bias from productivity shocks. Using a shift-share design at the firm level, they estimate a tightness elasticity of 0.01 for the German labor market in the years 2012-2019. However, their estimation at the firm level may conceal important heterogeneity in terms of socio-demographic worker characteristics. Against this background, our study is the first to combine rich information on vacancies and job seekers with an instrumental variable approach to determine the wage effects of labor market tightness on the level of workers. In doing so, we are able to differentiate tightness effects along many dimensions of workers while minimizing bias from spurious variation in tightness.

Second, our paper provides microfoundations for calibrating key parameters of search-and-matching models (Mortensen and Pissarides, 1994; Mortensen and Pissarides, 1999; Pissarides,

[73]For the German labor market, there are two further studies which analyze the effect of tightness on wages for particular groups of workers: For the years 1995-2014, Brunow et al. (2022) show that entry into a tight labor market (proxied by the ratio of unemployed to employed persons) is associated with a steeper wage development for young workers 10 years after entering the labor market. Kölling (2023) finds that firms with self-reported hiring problems pay a wage premium of around 4 percent to nurses during 2008-2018.

2000). In these models, job-finding rates of workers and job-filling rates of firms are directly related to the ratio of vacancies to job seekers. Under Nash bargaining, the standard DMP model propagates a positive relationship of labor market tightness on wages – commonly known as the "wage-setting curve". In line with Hoeck (2023), our lower- and upper-bound elasticities mirror a positively sloped but relatively flat wage-setting curve. The flat slope contradicts the calibration in Shimer (2005) but resembles the calibration by Hagedorn and Manovskii (2008), which performs well from an empirical point of view.

Third, our paper indirectly addresses the literature on the so-called "wage curve", a related but (compared to the wage-setting curve) differently nuanced concept of wage formation. The wage curve (Blanchflower and Oswald, 1995; Card, 1995; Nijkamp and Poot, 2005) connects wages to regional unemployment levels and can be derived from bargaining models (Nash, 1950; Nickell and Andrews, 1983) or efficiency wage models (Stiglitz, 1974; Yellen, 1984). The wage curve was first described by Blanchflower and Oswald (1994), who established a robust negative relationship between regional unemployment and wage levels. Evidence shows that the effect of unemployment on wages is negative but relatively weak in the German labor market (Bellmann and Blien, 2001; Baltagi et al., 2009; Baltagi et al., 2012; Bauer and Lochner, 2020). Relative to the literature on the wage curve, our tightness measure constitutes a more comprehensive measure for scarcity of labor than the mere number of unemployed due to consideration of the labor-demand side. Despite the different concepts, our results validate the available evidence on German wage curves in the sense that improved outside options for workers have a positive but rather limited effect on wages.

Fourth, we contribute to the literature on the development of the wage structure in Germany (Dustmann et al., 2009; Dustmann et al., 2014; Card et al., 2013). This literature has documented that wage inequality has been increasing from the 1990s up until the mid-2000s. The rising inequality was driven by declining real wages in the lower half of the wage distribution, while wages were stagnating in the middle and growing at the top (Dustmann et al., 2014). The literature provides various explanations for this phenomenon, namely (i) skill-biased and routine-biased technological change (Dustmann et al., 2009; Antonczyk et al., 2018), (ii) decline in collective bargaining along with increased flexibilization of such agreements (Dustmann et al., 2009; Dustmann et al., 2014), (iii) rising dispersion in firm-specific wage premia and increasing assortativeness in the matching of workers to plants (Card et al., 2013), and (iv) domestic outsourcing of service personnel (Goldschmidt and Schmieder, 2017). Biewen and Plötze (2019) show that the rise in income inequality stopped during the mid-2000s. Later, it was documented that the inequality even started to decline during the 2010s (Möller, 2016; Bossler and Schank, 2023), and the declining inequality not only referred to labor incomes but also to wages (Blömer et al., 2023; Drechsel-Grau et al., 2022; Fedorets et al., 2020). Part of this decline can be attributed to the 2015 minimum wage introduction (Bossler and Schank, 2023). However, the decline already started before the minimum wage was introduced, leaving room for other explanatory factors. Against this backdrop, our paper documents that the tremendously rising labor market tightness, from which workers in low-wage firms were particularly benefiting, contributed to the observed decline in wage inequality.

The study is structured as follows: Section 3.2 provides a succinct overview of the models that propagate a wage effect from tightening labor markets. Section 3.3 describes the administrative and survey-based data sources. In Section 3.4, we provide descriptive evidence on the nexus between labor market tightness and wages. In Section 3.5, we describe our empirical model and the design of our leave-one-out instrument. Section 3.6 presents the results of our empirical analyses and robustness checks and interprets the effect size. In Section 3.7, we present an evaluation of heterogeneous effects for different subgroups. Section 3.8 concludes.

3.2 Theoretical Background

Economic theory offers various frameworks that shed light on the relationship between the equilibrium outcomes of labor market tightness and wages. In this section, we briefly explain the predictions from these models to facilitate the interpretation of our later empirical results.

The Standard Model of Labor Demand and Labor Supply. In a competitive labor market, the equilibrium wage rate, which equals workers' marginal value product of labor, is determined by the intersection of the downward-sloping labor demand and the upward-sloping labor supply curve. A rise in labor market tightness is caused by more vacancies (e.g., from higher product demand) or fewer job-seeking individuals (e.g., due to demographic decline). If, for all wage levels and given employment, the number of vacancies (i.e., unrealized labor demand) increases, the labor demand curve shifts rightward. Vice versa, if the number of job seekers (i.e., unrealized labor supply) decreases, the labor supply curve shifts leftward. Both shifts cause wages to rise in the new market-clearing equilibrium. This wage rise turns out higher, the more wage-inelastic (i.e., the steeper) the demand and supply curves are.

Monopsonistic Labor Markets. In contrast to the competitive labor market, monopsonistic labor markets are characterized by only a limited number of employers (Boal and Ramson, 1997; Manning, 2003).[74] In the textbook monopsony model of a single firm (Robinson, 1933), the monopsonist retains wage-setting power when labor supply is less than perfectly elastic. Unlike firms in the competitive model, the monopsonist does not face an exogenously given market wage but chooses employment along an upward-sloping labor supply curve. Since the monopsonist can hire additional workers only by offering higher wages, the monopsonist chooses a profit-maximizing employment level below the competitive level, resulting in a market wage that is marked down relative to workers' marginal productivity.

If tightness increases in the monopsonistic labor market, wages will also rise. However, the first key difference to the competitive model is that the monopsonist raises wages by less when the marginal value product curve of the market (i.e., the labor demand curve in the competitive model) shifts rightward towards a new imperfectly competitive equilibrium. The reason is that, unlike firms in the competitive model, the monopsonist is not a wage taker who is bound to market forces. However, the monopsonist will still pay higher wages along the upward-sloping labor supply curve but will not fully meet the expansion in demand to avoid further wage increases. However, the wage effect does not necessarily fall short of the wage effect in the competitive model. In particular, the second key difference is that higher tightness is supposed to improve worker's bargaining position and thus flattens the labor supply curve to the firm, which comes along with higher wages by reducing markdowns.

Search and Matching. The Diamond-Mortensen-Pissarides (DMP) model offers a more nuanced understanding of the labor market by incorporating labor market frictions from search

[74] In a broader sense, monopsony power can also result from frictions in atomistic labor markets, for example due to low labor mobility allowing firms to exert wage-setting power.

and matching (Mortensen and Pissarides, 1999; Pissarides, 2000). These frictions impede the smooth clearing of the labor market, leading to a simultaneous coexistence of vacancies and unemployed individuals.

Since the search process is costly, a successful match yields a rent that is shared between workers and firms according to Nash bargaining. Under this exogenous bargaining rule, a rise in labor market tightness raises the job-finding rate of workers and, thus, provides workers with a better outside option (i.e., the present value of unemployment increases). Vice versa, higher tightness reduces the job-filling rate of firms, which worsens their outside option (i.e., the present value of an unfilled vacancy decreases). As a result, the model propagates a positive effect of labor market tightness on wages, which is commonly referred to as the "wage-setting curve" (Mortensen and Pissarides, 1994; Mortensen and Pissarides, 1999; Pissarides, 2000). In the standard DMP model, the slope of the wage-setting curve positively depends on the magnitude of vacancy-posting cost and the relative bargaining power of workers, with productivity being an important co-determining variable of the wage.

In the wage-posting version of the search-and-matching model, firms offer wages based on their profit maximization function (Burdett and Mortensen, 1998). An exogenous change in labour market tightness, e.g. an increase in vacancies, leads to an increase in the job-offer-arrival rate of workers. This reduces search frictions for workers, and firms must offer higher wages to attract new workers or retain existing workers.

Bargaining Models. In bargaining models, workers (or unions) and firms negotiate about the distribution of rents (Dunlop, 1944; Nickell and Andrews, 1983). In contrast to the DMP model, the bargaining power is not fixed. Higher labor market tightness improves the endogenous bargaining power of workers (relative to firms) due to more outside options. As a consequence, monopsony (or wage-setting) power of firms falls and workers will extract a larger fraction of the overall match surplus, resulting in higher wages.

Efficiency Wages. In efficiency wage models, firms with incomplete information raise wages above market-clearing levels for four reasons (Yellen, 1984): reduced shirking, improved morale, lower turnover, and better applicants. In these models, the wage acts as an incentive and selection device. However, when labor markets tighten, productivity and availability of workers suffer (due to better outside options). Consequently, firms may further increase the wage rate to uphold its functionality.

3.3 Data

In this paper, we assemble three different data sources to study the impact of labor market tightness on wages in the German labor market. First, we obtain data on wages and control variables from the Integrated Employment Biographies (IEB). Second, we use official statistics from the Federal Employment Agency to gather detailed information on registered job seekers and registered vacancies. Third, we extrapolate the number of registered vacancies using information on the shares of registered vacancies as of all vacancies from the IAB Job Vacancy Survey (JVS) to determine the total number of vacancies. For further details on our data, we refer the reader to Appendix 3.A.

Wages and Control Variables. The IEB is the administrative dataset of the German labor market, featuring daily information on the near-universe of individual employment spells (Müller and Wolter, 2020). The data cover workers' longitudinal employment histories, including their daily gross wages (up to a censoring limit), contract type, 5-digit occupation, workplace location, working time (full-time or part-time), gender, age, education, nationality, and a firm identifier.[75] Specifically, the IEB includes employment notifications of all workers subject to social security contributions, the reporting of which is mandatory for employers in Germany.

Our analysis focuses on employment spells that span June 30 of the years 2012-2022.[76] For lack of information on the number of individual working hours in the IEB, we follow standard practice and limit our analysis to full-time workers in regular employment to ensure uniform working hours. Throughout the study, we focus on workers in the non-agricultural private business sector.[77] When constructing our wage variable, we explicitly take into account special

[75]Note that the firm identifier refers to establishments which are local units of a company in our data.

[76]Although data availability would allow us to construct our measures of labor market tightness from 2010 onward, we begin our analysis in the year 2012 because there was a major structural break in 2011/12 in the occupation variable in the IEB data.

[77]Specifically, we keep the following NACE 2-digit codes: 5-82, 90, and 92-96.

payments (such as provisions or Christmas bonuses), which we distribute evenly over the observed spell length and, thus, translate into higher daily wages. To address right-censored wages above the upper earnings limit on social security contributions, we employ a two-step imputation technique based on the Tobit regression method as in Card et al. (2013).[78] We deflate our imputed wages by the national consumer price index to depict real wages. To reduce the computational burden, we draw a random 5 percent sample of workers to perform our descriptive and causal analyses.

Labor Market Definition. We combine detailed information on occupations and regions (in terms of workplace) to define labor markets. Regarding occupations, we leverage the rich occupational information available in the IEB, namely the German Classification of Occupations (KldB) from 2010, which allows us to distinguish between a maximum of 1,300 5-digit occupations. The first four digits classify occupations based on the area of expertise with increasing levels of detail. The fifth digit assigns the skill requirement level based on four categories: helpers, professionals, specialists, and experts.[79] In line with standard practice, we employ the leading three digits (occupational groups) along with the fifth digit (skill requirement level). This aggregation leads to $O = 431$ different occupations, which we refer to as "detailed 3-digit occupations" in the following. In terms of regions, we utilize the graph-theoretical approach proposed by Kropp and Schwengler (2016) to merge 401 administrative districts into commuting zones. Our optimization, which is guided by home-to-work commuting patterns provided by the Federal Employment Agency, results in $R = 52$ zones with strong within-zone but little between-zone commuting.[80] Taken together, our baseline definition of labor markets results in 16,980 occupation-by-zone combinations. To convince the reader that

[78] The implementation follows the imputation procedure of the Establishment History Panel (Ganzer et al., 2023). Initially, we run Mincer-type Tobit regressions (separately by year, gender, and education) on a set of individual controls to fit wages and derive leave-one-out average wages per firm (i.e., the average wage in a firm excluding the observation at hand). Subsequently, we re-run the Tobit regressions using the firm-level leave-one-out average wage as an additional covariate to account for firm-specific wage premia in the wage imputation.

[79] Helper occupations require zero or a maximum of one year's training. Professional occupations cover all jobs for which industrial, commercial, or other vocational training is required (excluding master craftsmen and technicians). Specialists hold a bachelor's degree or have completed master craftsman/technician training. Experts hold a master's degree or an equivalent diploma.

[80] Further details of the delineation of commuting zones are summarized in Appendix 3.B.

our empirical results are not driven by this specific delineation, we later show that our baseline results hardly change when applying broader or narrower definitions of labor markets.

Labor Market Tightness. We follow the method from Bossler and Popp (2024) and combine process and survey data to construct measures of labor market tightness at an exceptionally fine level. To begin with, we collect process data on posted vacancies that were registered with the Federal Employment Agency. We obtain official statistics on the stock of registered vacancies for a reference date in mid-June between 2012 and 2022, including details on the targeted occupation and commuting zone (in terms of workplace). Note that, in Germany, firms are not required to register vacancies with the Federal Employment Agency, resulting in incomplete vacancy data. To determine the overall stock of registered plus unregistered vacancies for each labor market and year, we divide the number of registered vacancies by the yearly share of registered vacancies from the IAB Job Vacancy Survey.

The IAB Job Vacancy Survey (IAB-JVS) is a representative and large-scale firm survey which, among other questions, asks firms about their numbers of registered and unregistered vacancies (Bossler et al., 2020). When constructing the yearly shares of registered vacancies among all vacancies, we differentiate between helpers, professionals, and the two groups of specialists and experts combined. The yearly shares of registered vacancies fluctuate slightly over time (see Appendix Table 3.A1) and have the following values on average for the years 2012-2022: helpers (45.1 percent), professionals (44.3 percent), and specialists along with experts (28.9 percent).

In contrast to vacancies, individuals must register as unemployed with the Federal Employment Agency to be eligible for benefits from unemployment insurance or social assistance. For each labor market and year, we extract official information on the number of job seekers, comprising registered unemployed individuals plus employed workers who actively seek employment through the Federal Employment Agency.[81] Abraham et al. (2020) demonstrate that the count of effective job seekers delivers greater explanatory power in the matching function than the mere tally of unemployed individuals. For each labor market (i.e., for each combination of detailed occupational group and commuting zone) and year, we divide the

[81] Upon registration at the Federal Employment Agency, job seekers must state their targeted occupation.

overall stock of registered plus unregistered vacancies by the count of job seekers to arrive at our measure of labor market tightness. Bossler and Popp (2024) show that the resulting ratio performs well as a measure for firms' difficulty in recruiting workers – in the sense that it positively correlates with pre-match hiring cost, search duration, and the number of search channels while it is negatively correlated with the number of applicants.

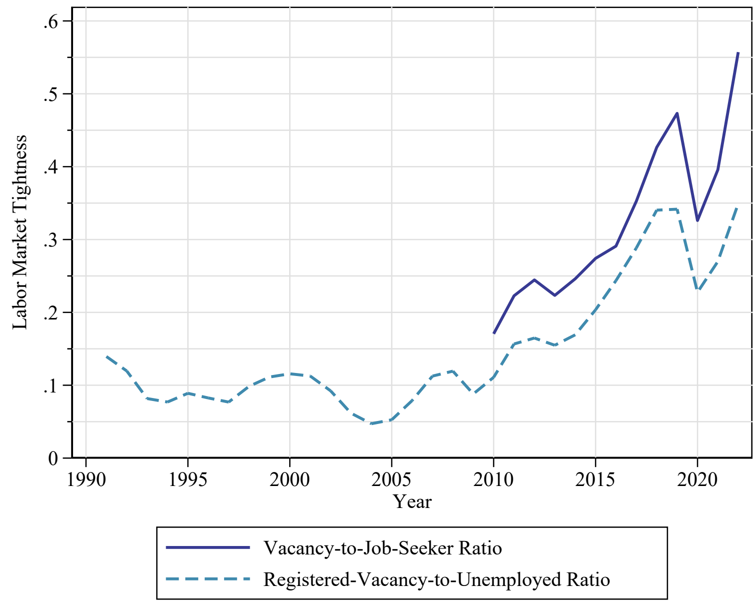
3.4 Descriptive Results

Before estimating the wage effect of labor market tightness in a regression framework, we scrutinize our data on tightness and wages descriptively. The descriptive analysis provides preliminary insights into the plausibility of the data, the aggregate developments, and the raw correlation between both variables. For more detailed descriptive evidence, we refer the reader to Appendix 3.D.

The Development of Labor Market Tightness. The development of the German labor market has been remarkable during the last decade, moving from what was once described as the "sick man of Europe" to an "economic superstar" (Dustmann et al., 2014). The German experience is characterized by robust employment growth, which surged from 42.0 million employees in 2012 to 46.8 million employees in 2022. Similarly, the number of unemployed declined from 2.90 million persons in 2012 to 2.42 million persons in 2022, mirroring the growth in employment and reflecting the demographic change of the German labor force. As a result, the expansion in employment was accompanied by a tremendous increase in the proportion of firms reporting increased labor shortages, which came along with a strongly increasing labor market tightness.

Figure 3.1 presents the development of labor market tightness in Germany. Our detailed measure of tightness relates the total number of vacancies to the number of job seekers in each labor market and is available from 2010 onward. The solid line shows the development of this measure when dividing the number of vacancies and job seekers at the national level. The dashed line illustrates the development of an alternative tightness measure relating the nationwide number of registered vacancies to the nationwide number of unemployed, each of which is available in aggregate data of the Federal Employment Agency since 1991. The

Figure 3.1: Labor Market Tightness in Germany over Time



NOTE: The figure illustrates the development of labor market tightness in Germany over time. The solid line refers to the economy-wide ratio of vacancies to job seekers. The dashed line shows the ratio of registered vacancies to unemployed persons and, thus, builds on aggregate information that is available already from 1991 onwards. *Data Source:* Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 1991-2022.

development of our baseline and the alternative tightness measure is remarkably similar. Despite taking into account employed job seekers in the denominator, our more comprehensive measure turns out to be higher since it features the overall number of vacancies in the numerator, which vastly exceeds the number of registered vacancies. The descriptive development demonstrates that our sample period covers an interesting period of a significant rise in labor market tightness, which has previously remained – by and large – constant. In 2010, our detailed measure of tightness was still about 0.17 at the national level, implying, on average, six job seekers for every vacant job. In 2012 (i.e., at the beginning of our regression sample), tightness rose to 0.24, averaging four job seekers per vacancy. Apart from a temporary slump during the Covid-19 pandemic, tightness rose tremendously in the following years to a value of 0.56 by 2022. At the current edge, there are fewer than two job seekers for every vacancy, representing more than a tripling in tightness since 2010. In precise terms, tightness rose by 229.4 percent during 2010-2022 and by 133.3 percent during 2012-2022.

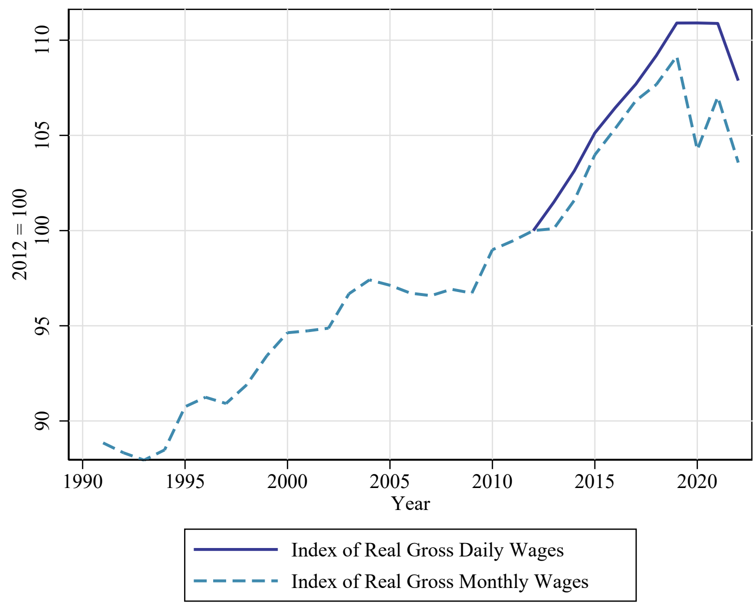
However, we observe remarkable differences in the level and development of tightness by the underlying requirement level (see Appendix Figure 3.D1). Among the four groups, tightness for helper occupations turns out by far the lowest, while the markets for professionals, specialists, and experts are generally tighter. During our period of analysis, all groups of requirement levels experienced a tremendous increase in tightness. Between 2010 and 2022, tightness for helpers and experts doubled while it tripled and quadrupled for specialists and professionals, respectively. For professionals, specialists, and experts, the number of vacancies even exceeds the number of job seekers at the current edge in 2022. When abstracting from the requirement level, we observe that all labor markets in terms of 2-digit occupations (see Appendix Figure 3.D2) or commuting zones (see Appendix Figure 3.D3) tightened. Importantly, however, there are substantial differences in the tightness increase across occupations and regions over time, which is the variation we are leveraging in our causal analysis.

The Development of Wages. Against the backdrop of the wage(-setting) curve, the main question of interest is to what extent the tremendous increase in tightness has led to wage increases. Figure 3.2 shows the development of average real wages since the beginning of the 1990s based on data from the German Federal Statistical Office and in the period of our analysis sample based on the IEB data. The resulting time series shows relatively flat growth in real wages during the 2000s, which has been argued to have contributed to the favorable development of the German labor market by increasing the country's competitiveness through lower labor costs (Dustmann et al., 2014). Most importantly for our analysis, however, wage growth accelerated during the 2010s, suggesting that some of the wage growth could be driven by rising labor market tightness. In sum, we observe that average real gross daily wages rose from 106.25 Euro to 114.62 Euro per day, that is, by 7.9 percent between 2012 and 2022.[82]

Raw Correlation of Labor Market Tightness and Wages. Before estimating the effect of labor market tightness on wages, we examine the raw correlation between the two variables.

[82]The qualitative assessment of the wage development remains unchanged (albeit steeper) when looking at nominal wages, as depicted in Appendix Figure 3.D4.

Figure 3.2: Real Wages in Germany over Time

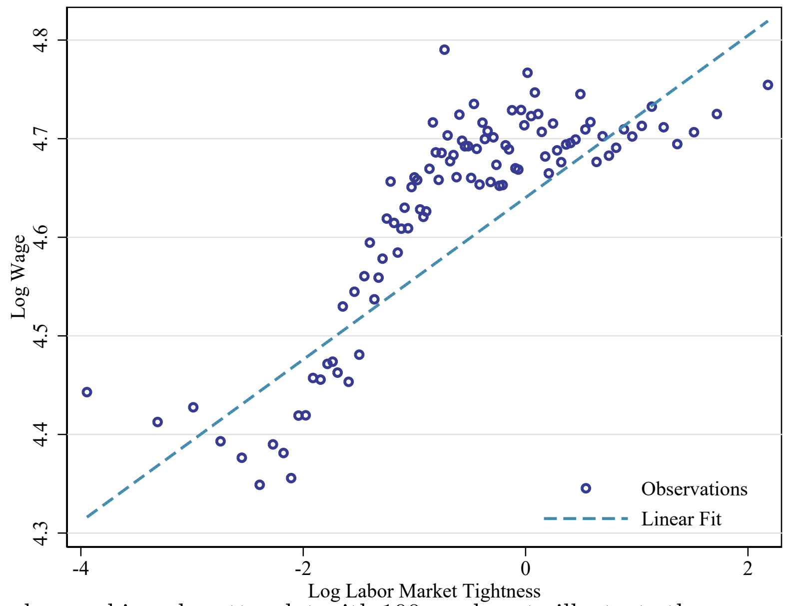


NOTE: The figure illustrates the development of wages in Germany over time. The solid line refers to real gross daily wages (including special payments) for regular full-time workers in the non-agricultural private business sector. The dashed line shows the index of real gross monthly wages for full-time workers excluding special payments. The latter time series refers to the manufacturing sector until 2006 and to manufacturing and services from 2007 onwards. *Data Source:* Integrated Employment Biographies + Official Data of the German Federal Statistical Office, 1991-2022.

Figure 3.3 depicts the raw correlation between both variables, displaying the raw average log wage for each percentile bin of the log tightness distribution together with a linear fit based on the respective bivariate linear regression. By visual inspection, the correlation between both variables is clearly positive, demonstrating that high tightness is associated with high wages. This aligns with our theoretical expectations that wages respond positively to a rising tightness. However, the picture does not necessarily imply that there is a causal effect of tightness on wages since the raw correlation may be confounded by uncontrolled variables (e.g., requirement level) or reverse causality.

Finally, it is worth noting that the relationship between tightness and wages is far from perfectly linear. While wages seem to rise in response to tightness, the wage curve becomes flatter beyond a certain level of tightness. This particular pattern suggests that it might be

Figure 3.3: Raw Correlation of Labor Market Tightness and Wages



NOTE: The figure shows a binned scatterplot with 100 markers to illustrate the raw correlation between log labor market tightness and log real daily wages. *Data Source:* Integrated Employment Biographies + Official Statistics of Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

worth allowing for a non-linear relationship of both variables, which we will address when estimating heterogeneous effects in Section 3.7.

3.5 Empirical Model

Our descriptive analysis suggests a positive relationship between labor market tightness and wages. To estimate the causal effect of higher labor market tightness on wages, we employ high-dimensional fixed-effects regression models using the following log-linear Mincer-type wage equation:

$$ln\, W_{it} = \alpha \cdot ln\, \theta_{ort} + X_{it}\beta + \gamma_i + \delta_t + \phi_{or} + \psi_j + \varepsilon_{it} \tag{3.1}$$

We regress daily wages W of worker i in year t on labor market tightness θ (all in logs) while allowing for an idiosyncratic error term ϵ. Our measure of labor market tightness is defined as the ratio of vacancies to job seekers for each combination of occupation o and region r: $\theta_{ort} = \frac{V_{ort}}{U_{ort}}$. The occupation identifier o corresponds to the occupation that worker

i has in year t such that $o = o(i,t)$.[83] Our main coefficient of interest is α, which denotes the elasticity of wages with respect to labor market tightness. In our first and all other specifications, we follow standard practice and control for worker fixed effects γ_i and year fixed effects γ_t to absorb unobserved heterogeneity across workers and time. In addition, we control for a labor-market fixed effect ϕ_{or} to ensure that our effect is driven by variation in tightness over time rather than by occupational or regional mobility of workers between labor markets. In our second specification, we additionally include a set of time-varying variables X_{it} to control for omitted variable bias, namely education, age, squared age, new hire, and a binary variable indicating whether the firm is located in Western or Eastern Germany. In our third specification, we initially dispense with control variables but add firm fixed effects ψ_j to absorb unobserved heterogeneity between employers, thus ruling out that the effect is driven by mobility between employers. The firm identifier j corresponds to the firm that employs worker i in year t such that $j = j(i,t)$. In our fourth and preferred specification, we include both control variables and firm fixed effects. Thus, our effect is identified by comparing changes in workers' wages in labor markets with a changing tightness while netting out composition effects by time, labor market, firm, and socio-demographic characteristics. For inference, we allow for a cluster-robust covariance matrix of the error term, defining clusters at the level of occupation-by-region cells (i.e., at the level of variation of our explanatory variable).

Threats to the Identification. Guided by the theoretical models in Section 3.2, we explicitly acknowledge that labor market tightness is an equilibrium outcome that is itself influenced by economic forces. The first threat to the identification is reverse causality between wages and tightness, which the inclusion of fixed effects does not protect against. Specifically, higher wages may raise labor supply, thus increasing the number of job seekers in the market. Vice versa, higher wages also reduce labor demand via the job creation curve. As shown by Bassier et al. (2023), increased wages reduce vacancy durations and, consequently, the number of vacancies in the market. Both the reverse labor supply and the labor demand channel imply a negative feedback effect on labor market tightness, which would manifest in a downward-biased elasticity. The feedback effect of a single atomistic firm is certainly

[83] The region identifier r corresponds to the workplace location of worker i in year t.

negligible.[84] However, when firms are not small in relation to the market or act in concert, their wage-setting policies may have a reverse effect on labor market tightness. To minimize downward bias from reverse causality, we block these feedback mechanisms by instrumenting the explanatory tightness variable in a two-stage least squares regression. We draw on an internal leave-one-out instrumental variable strategy, which became popular through several recent monopsony studies that analyze the effect of higher labor market concentration on wages (e.g., Azar et al., 2022; Bassanini et al., 2024), and transfer this design to the analysis of labor market tightness. Specifically, we instrument any value of log tightness in a certain region c by the average of the log tightness in all other regions for the same occupation and time period:

$$Z^1_{oct} \equiv \overline{ln\,\theta}^{-c}_{ot} = \frac{\sum_{r \neq c} ln\, \frac{V_{ort}}{U_{ort}}}{R-1} \tag{3.2}$$

Favorably, the instrument delivers variation in local labor market tightness that is driven by national, non-local forces in the respective occupations. Due to the leave-one-out property (i.e., by excluding vacancies and job seekers from the focal region), the instrument is immune against changes in occupations in the very same region and, thus, protects against a feedback effect of wages on tightness in the very same labor market.

By construction, calculating the leave-one-out average assigns all other regions the same weight. To ensure that larger labor markets receive a correspondingly higher weight, we construct the following alternative leave-one-out instrument for region c:

$$Z^2_{oct} \equiv ln\,\theta^{-c}_{ot} = ln\,\frac{V^{-r}_{ot}}{U^{-r}_{ot}} = ln\left(\frac{\sum_{r \neq c} V_{ort}}{\sum_{r \neq c} U_{ort}}\right) \tag{3.3}$$

This "sum-based" instrument does not rely on averaging tightness but, for the very same occupation and time period, adds up the number of vacancies and job seekers in all other regions and calculates the ratio of both leave-one-out sums. Thus, larger regions with many vacancies or job seekers contribute more strongly to the instrument than smaller regions.

[84] This assumption is less likely to hold when using (self-reported) measures of labor shortages or hiring difficulties at the firm level as explanatory variable because firms may face these problems simply because they are low-paying. By instead using tightness at the market level to measure the scarcity of the labor input, we already rule out reverse causality from wage policies of single atomistic firms, which cannot reasonably influence market outcomes.

The second threat to our identification is omitted variable bias from productivity shocks. If productivity increases at the local or national level, the labor demand curve shifts rightward, leading to a simultaneous rise in wages and vacancies. Thus, if productivity shocks are not properly controlled for, the estimated elasticity will be upward-biased. Favorably, our instrumental variable protects against omitted variable bias from local productivity shocks since the leave-one-out average always excludes the focal region. However, our instrument may pick up productivity shocks if these shocks correlate across regions (i.e., if there are national shocks to productivity in the respective occupation).

We acknowledge that our instrument is not fully exogenous in the presence of occupation-wise productivity shocks at the national level. Since we do not observe productivity directly, we perform four different checks with different proxies for national productivity shocks to empirically assess the magnitude of this bias. First, we control for occupation-by-year fixed effects in our OLS specification.[85] Second, we directly control for log productivity at the firm (rather than occupation) level in our IV specification by combining our IEB data with survey information from the IAB Establishment Panel. Third, we control for industry-by-year fixed effects, which assumes that productivity shocks are industry-specific, in our IV specification. Fourth, and most rigorously, we control for the leave-one-out sum of vacancies in the respective occupation-by-year cell in our IV specification. By controlling for these vacancies, we safeguard that the variation in tightness stems exclusively from supply-side changes in the number of job seekers which are plausibly unrelated to productivity shocks. Taken together, these checks will allow us to establish a lower bound for the elasticity of wages with respect to tightness.

Heterogeneous Effects. When estimating heterogeneous effects of the relationship between tightness and wages, we reformulate Equation (3.1) and run the following interacted model:

$$ln\, W_{it} = \sum_{s=1}^{S} \alpha_s \cdot D_{it}^{s} \cdot ln\, \theta_{ort} + X_{it}\beta + \gamma_i + \delta_t + \phi_{or} + \psi_j + \varepsilon_{it} \tag{3.4}$$

[85] Note that we cannot control for occupation-by-year fixed effects in our IV specification since our instrument is essentially defined at this level.

where D^s represents a dummy variable that is 1 when the underlying categorical variable has value s (zero otherwise). In this formulation, α_s directly captures the tightness effects of the respective subgroup s without relying on a certain reference group. Note that the group identifier D is always included in the set of control variables or absorbed by the fixed effects. Thus, the base effect for all interacted variables is always controlled for. For our instrumental variable strategy, we interact our baseline instrument Z_{ort} by the respective group identifier D^s, resulting in S instruments for the S potentially endogenous variables of interest.

3.6 Empirical Results

OLS. We begin our regression analysis by estimating the wage elasticity with respect to labor market tightness from OLS-based fixed effects regression, as specified in Equation (3.1). Table 3.1 presents the respective results for our four different specifications.[86] Column (1) presents the regression with year, worker, and labor-market fixed effects. The elasticity turns out to be positive, implying that a doubling in labor market tightness (i.e., an increase by 100 percent) raises the daily gross wages of full-time workers by 0.8 percent. The effect size remains fairly constant when we additionally control for socio-demographic characteristics in Column (2) or firm fixed effects in Column (3). In Column (4), we present our most comprehensive OLS specification with socio-demographic controls and year, worker, labor-market, and firm fixed effects. Again, the effect remains fairly robust at an elasticity of 0.074. In sum, the OLS regressions point towards an elasticity of wages with respect to tightness in the range of 0.007-0.009, which hints towards a positive but rather small effect. Finally, note that all tightness effects are statistically significant at 1 percent levels.

IV. As mentioned in Section 3.5, the OLS-based elasticities may be biased due to reverse causality or uncontrolled productivity shocks. To counteract this bias, we instrument log labor market tightness by the leave-one-out average of log tightness in all other regions but for the very same occupation and time period, as given by Equation (3.2). Table 3.2 displays the IV results. Column (1) delivers an elasticity of 0.0124 from the most parsimonious specification with year, worker, and labor-market fixed effects only. The respective first-stage coefficient

[86]Full tables of the regression estimates are presented in Appendix 3.E.

Table 3.1: OLS Effects of Labor Market Tightness on Wages

	(1) Log Wage	(2) Log Wage	(3) Log Wage	(4) Log Wage
Log Tightness	0.0079***	0.0088***	0.0065***	0.0074***
	(0.0008)	(0.0006)	(0.0006)	(0.0005)
Year FE	yes	yes	yes	yes
Worker FE	yes	yes	yes	yes
Labor Market FE	yes	yes	yes	yes
Firm FE			yes	yes
Controls		yes		yes
Observations	8,584,726	8,584,726	8,454,953	8,454,953
Clusters	13,806	13,806	13,716	13,716

NOTE: The table displays OLS regressions of log daily wages of regular full-time workers on log labor market tightness. Control variables include binary variables for new hires, workplace location in Eastern Germany, three levels of professional education and continuous variables for age and squared age. Labor markets are combinations of detailed 3-digit occupations and commuting zones. Standard errors (in parentheses) are clustered at the labor market level: *= p<0.10. ** = p<0.05. *** = p<0.01. Including firm fixed effects reduces the number of observations and clusters due to singleton groups, which are excluded from the estimation. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

is significantly positive and large (0.89), corroborating that our leave-one-out instrument adequately predicts changes in labor market tightness. Favorably, the first stage is sufficiently strong, as indicated by an F-Statistic of the excluded instrument of 2,847. In Columns (2) and (3), we separately include socio-demographic controls and firm effects, which lead to slightly smaller coefficients. In our baseline specification in Column (4) with socio-demographic controls as well as year, worker, labor-market, and firm fixed effects, we arrive at a similar elasticity of 0.011, implying that an increase in labor market tightness by 100 percent raises daily wages of workers ceteris paribus by 1.1 percent. In this baseline specification, the first-stage coefficient is 0.88 and the F Statistic is 2,740, thus lending credence to the plausibility of our IV approach. Finally, note that all IV effects are statistically significant at 1 percent levels.

In sum, the elasticities of the IV estimation turn out to be somewhat larger than their OLS counterparts. The larger IV elasticities suggest that the leave-one-out instrument successfully addresses reverse causality, which manifests in downward-biased OLS estimates.

Table 3.2: IV Effects of Labor Market Tightness on Wages

	(1) Log Wage	(2) Log Wage	(3) Log Wage	(4) Log Wage
Log Tightness	0.0124*** (0.0018)	0.0114*** (0.0014)	0.0114*** (0.0015)	0.0113*** (0.0012)
Year FE	yes	yes	yes	yes
Person FE	yes	yes	yes	yes
Labor Market FE	yes	yes	yes	yes
Firm FE			yes	yes
Controls		yes		yes
Observations	8,584,317	8,584,317	8,454,543	8,454,543
Clusters	13,783	13,783	13,692	13,692
First-Stage Coefficient	0.8914***	0.8915***	0.8814***	0.8814***
First-Stage F-Statistic of Excluded Instrument	2,847	2,848	2,737	2,740

NOTE: The table displays IV regressions of log real daily wages of regular full-time workers on log labor market tightness. The instrumental variable refers to leave-one-out averages of labor market tightness in all other commuting zones but for the same occupation and time period. Control variables include binary variables for new hires, workplace location in Eastern Germany, three levels of professional education, and continuous variables for age and squared age. Labor markets are combinations of detailed 3-digit occupations and commuting zones. Standard errors (in parentheses) are clustered at the labor market level: *=p<0.10. **=p<0.05. ***=p<0.01. Including firm fixed effects reduces the number of observations and clusters due to singleton groups, which are excluded from the estimation. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Productivity Shocks at the National Level. While our instrument counteracts reverse causality and omitted variable bias from local productivity shocks, it is not immune against occupational productivity shocks at the national level. To assess the magnitude of this potential upward bias, we perform four robustness checks with differently rigorous proxies for national productivity shocks in Table 3.3.[87]

In Column (1), we address productivity shocks by occupations at the national level by additionally controlling for occupation-by-year fixed effects in our preferred OLS specifica-

[87]Full regression tables including the coefficients of the control variables are presented in Appendix Table 3.E3.

tion.[88] By virtue of the occupation-by-year fixed effects, our OLS estimate slightly decreases from 0.0074 to 0.0060, indicating only little omitted variable bias from national productivity shocks. In Column (2), we directly control for productivity, albeit not at the occupational but at the firm level. To do so, we build on the sub-sample of IEB workers whose firms were interviewed in the IAB Establishment Panel. The combination of both datasets forms the so-called "Linked-Employer-Employee Dataset of the IAB" (LIAB), which allows us to calculate firm productivity by dividing annual revenues by headcount employment. When controlling for log firm productivity at the worker level, the IV elasticity slightly decreases from 0.011 to 0.010.[89] In Column (3), we alternatively include industry-by-year fixed effects in our IV specification. As a consequence, our baseline IV elasticity shrinks from 0.011 to 0.006. In Column (4), we perform the most rigorous approach to address productivity shocks in our IV setting by controlling for the sum of vacancies in all other regions but for the very same occupation and time period. In doing so, we ensure that the identifying variation purely stems from variation in the supply side of the labor market and arrive at a significantly positive elasticity of 0.004.

In line with the theoretical expectation of an upward bias from national (occupation-specific) productivity shocks, the inclusion of each of the four productivity proxies results in lower elasticities than our baseline IV elasticity of 0.011, which we view as an upper bound of the causal effect of tightness on wages. We choose a conservative approach and view the elasticity of 0.004 from the most rigorous check, that is, when controlling for the leave-one-out sum of vacancies, as lower bound. In the following, since it is *a priori* unclear which of our three proxies performs best (i.e., productivity is controlled for without eliminating other useful variation in tightness), we proceed with reporting robustness checks and heterogeneity analyses only for our baseline IV specification representing the upper bound.

[88]Note that occupation-by-year fixed effects may not only absorb confounding productivity shocks but also absorb desired mediating variation in tightness from occupation-wise shocks to vacancies and job seekers at the national level.

[89]As surveyed firms in the IAB Establishment Panel may constitute a non-random sample of firms, we employ survey weights when estimating the effect of labor market tightness on wages using the LIAB sample. The baseline effects in the LIAB sample are 0.0064 (OLS) and 0.0122 (IV), that is, in close proximity to the baseline effects of the 5 percent IEB sample.

Table 3.3: Addressing Productivity Shocks at the National Level

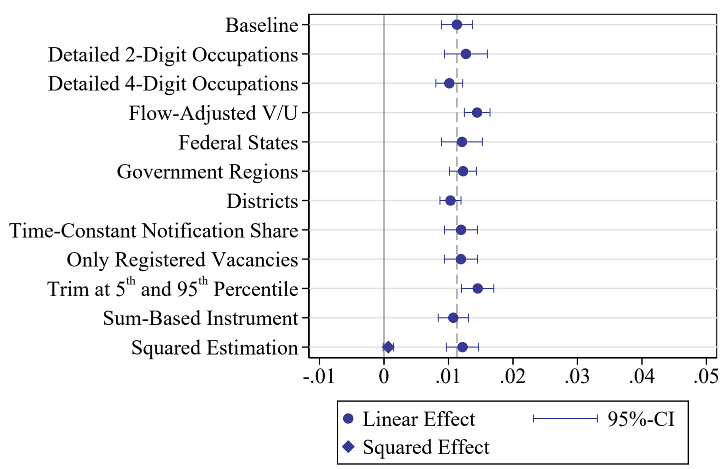
	(1) Log Wage	(2) Log Wage	(3) Log Wage	(4) Log Wage
Log Tightness	0.0060*** (0.0004)	0.0101*** (0.0028)	0.0057*** (0.0011)	0.0044*** (0.0017)
Log Firm Productivity	- -	0.0214*** (0.0021)	- -	- -
Log Leave-One-Out Sum V	- -	- -	- -	0.0114*** (0.0016)
Year FE	yes	yes	yes	yes
Worker FE	yes	yes	yes	yes
Labor Market FE	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Occupation-by-Year FE	yes			
Industry-by-Year FE			yes	
Controls	yes	yes	yes	yes
Source	5% IEB	LIAB	5% IEB	5% IEB
Survey Weights		yes		
Observations	8,454,869	3,117,429	8,454,541	8,454,541
Clusters	13,698	8,865	13,692	13,692
First-Stage Coefficient		0.8569***	0.8558***	0.7920***
First-Stage F-Statistic of Excluded Instrument		2,098	2,679	1,629

NOTE: The table displays OLS and IV regressions of log real daily wages of regular full-time workers on log labor market tightness. The instrumental variable refers to leave-one-out averages of labor market tightness in all other commuting zones but for the same occupation and time period. Control variables include binary variables for new hires, workplace location in Eastern Germany, three levels of professional education, and continuous variables for age and squared age. Labor markets are combinations of detailed 3-digit occupations and commuting zones. Column (1) shows OLS estimates when additionally controlling for detailed 3-digit-occupation-by-year fixed effects. Column (2) features IV estimates when controlling for log firm productivity in the LIAB worker sample. Column (3) displays IV estimates when controlling for 1-digit industry-by-year fixed effects (based on the NACE 2.0 classification). Column (4) presents IV estimates when conditioning on the log leave-one-out sum of the number of vacancies in all other commuting zones but for the very same occupation and time period. Standard errors (in parentheses) are clustered at the labor market level: *=p<0.10. **=p<0.05. ***=p<0.01. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey + Linked Employer-Employee Dataset of the IAB, 2012-2022.

Further Robustness Checks. In addition to addressing the role of productivity shocks, we test the robustness of our baseline instrumental variable effect of tightness on wages in various other dimensions, as illustrated in Figure 3.4.

First, we use a broader definition of occupational labor markets using the leading two (rather than three) digits ("occupational main group") along with the fifth digit ("requirement level") to form 132 "detailed 2-digit occupations". Second, we use a narrower definition of occupational labor markets and exploit all five digits of the occupational classification, resulting in labor markets for 1,300 different occupational types ("detailed 4-digit occupations"), which leads to a total number of 36,596 occupational labor markets. Third, we build a flow-adjusted measure of occupational labor market tightness to account for the fact that individuals can search for a job in a neighboring occupation (see Appendix 3.C). Specifically, we additionally include vacancies and job seekers in all other detailed 3-digit occupations with weights that refer to transition probabilities from moving from the focal to the neighboring detailed 3-digit occupation.

Figure 3.4: Robustness Checks



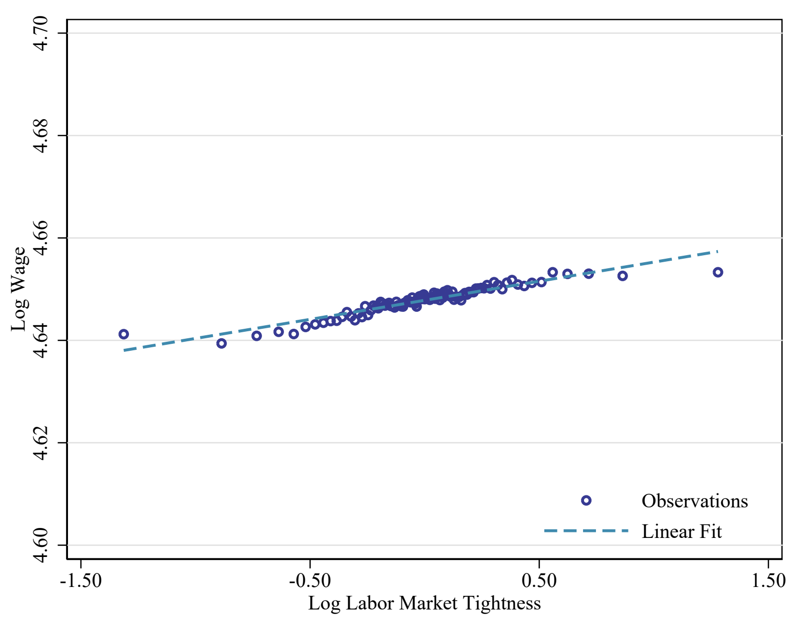
Note: The figure displays estimated elasticities and 95% confidence intervals from IV regressions of log real daily wages of regular full-time workers on log labor market tightness for a variety of specifications to test the sensitivity of our baseline IV effect. Each specification includes fixed effects for years, workers, labor markets, and firms as well as control variables. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Fourth, we rely on an administrative rather than a functional delineation of regions by using 16 federal states, which allows for the possibility that individuals search for a job at a longer regional distance. Fifth, we use another administrative delineation of 38 government regions. Sixth, we employ 400 administrative districts to delineate regions at a very narrow level.

Seventh, we construct the total number of vacancies per labor market by using time-constant instead of time-varying registration shares from the IAB Job Vacancy Survey (see Appendix Table 3.A1). While this robustness check does not account for temporal variation in registration shares with the Federal Employment Agency, it may reduce potential measurement errors in the time-varying registration shares over time. Eighth, we solely rely on administratively registered vacancies without calculating the total number of vacancies from registration shares of the IAB Job Vacancy Survey.

Ninth, we trim the dependent variable and the explanatory variable below the 5^{th} and above the 95^{th} percentile to ensure that the result is not driven by outliers. Tenth, we use our sum-based leave-one-out instrument, as given by Equation (3.3), to ensure that larger labor markets are attributed more importance in the leave-one-out calculation.

Figure 3.5: Conditional Relationship between Labor Market Tightness and Wages



NOTE: The figure shows a binned scatterplot with 100 markers to illustrate the correlation between log labor market tightness (instrumented by the leave-one-out average) and log real daily wages, after conditioning on control variables. *Data Source:* Integrated Employment Biographies + Official Statistics of Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Eleventh, we estimate an alternative specification with an additional quadratic effect of tightness on wages. While we do not have a theoretical expectation for the functional form of the relationship, this specification is empirically motivated by our descriptive scatter plot in Figure 3.3 that suggested a concave relationship. We use the same instrumental variable specification as in our baseline regression but add a quadratic term for labor market tightness along with the squared instrumental variable. The results, as illustrated in the last row of Figure 3.4, show an unchanged linear effect and a quadratic effect which is virtually zero, rejecting the hypothesis of a quadratic effect pattern. Since the absence of a quadratic relationship contradicts our initial visual inspection, we replicate the graphical illustration after conditioning on observed and unobserved heterogeneity and after accounting for the instrumentation. Figure 3.5 presents the respective binned scatter plot, which – in line with the regression – shows a fairly linear relationship between tightness and wages. In addition, the graphical illustration indicates that the slope is much flatter (compared with the raw correlation) after conditioning for all the confounding forces, which is in line with our baseline interpretation of only a slightly positive wage elasticity.

In summary, the estimated coefficients from our eleven robustness checks are very similar to the baseline elasticity of 0.011. Z-tests comparing each robustness check coefficient to the baseline coefficient show that none of the (linear) coefficients are statistically different from the baseline at the 5 percent level. However, the coefficients from the flow-adjusted specification and the trimmed sample are statistically different from the baseline at the 10 percent level.

Interpretation of Effect Size. Using OLS and IV regressions, we provide an interval for the causal effect of log labor market tightness on log wages between 0.004 (lower bound) and 0.011 (upper bound). Although the upper bound exceeds the lower bound by factor 2.6, both values imply that there were positive but rather limited wage increases from tremendously rising tightness in Germany. Specifically, the bounded interval implies that the observed increase in aggregate tightness in the German economy between 2012 and 2022 by 133.3 percent (see Figure 3.1) raised, on average, gross wages of regular full-time workers ceteris paribus by 0.6 to 1.5 percent. Against the backdrop that real wages grew by 7.9 percent during the same period (see Figure 3.2), the increase in tightness can explain only between 7.4 and 19.1 percent of the wage rise in the German economy.[90]

[90] In absolute terms, real annual wages grew on average by 3,055 Euro between 2012 and 2022, of which the rise in tightness can explain between 209 and 569 Euro.

Our results can help to calibrate the parameters in search-and-matching models. The wage-setting curve in the Diamond-Mortensen-Pissarides model propagates a positive linear effect of tightness (in levels) on the wage rate (in levels and normalized by productivity), which is the product of workers' relative bargaining power and vacancy-posting cost. Following standard practice, however, we have estimated elasticities of wages with respect to tightness using log-linear models. To translate log effects into level effects, we weight our upper- and lower-bound elasticities by the 2012 ratio of daily wages and aggregate tightness. After normalizing by average daily gross value added per worker, our bounds correspond to a coefficient for the tightness level between 0.013 and 0.032.[91] Specifically, these bounds imply a relatively flat wage-setting curve and are markedly smaller than the calibrated value of 0.153 by Shimer (2005). This higher value can partly explain why the Shimer model generates more wage volatility than what empirical evidence suggests. By contrast, our bounded interval includes the calibrated value of 0.030 from Hagedorn and Manovskii (2008), whose model performs better in terms of observed wage volatility.[92]

Building on our theoretical considerations of Section 3.2, we want to highlight several explanations as to why the effect of tightness on wages turns out rather small. First, through the lens of the standard LS-LD framework, our limited wage effect can be attributed to relatively elastic (i.e., flat) labor supply or labor demand curves. When labor demand shifts rightward along the upward-sloping labor supply curve, the presence of strong substitution effects from leisure towards labor (which render labor supply relatively elastic) would require only small wage increases to clear the market.[93] Similarly, when labor supply shifts leftward along the labor demand curve, the presence of strong substitution or scale effects away from labor (which render labor demand relatively elastic) would lower the necessity for wage increases. Second,

[91] Using information from the German Statistical Agency from 2012 (Destatis, 2013), we calculate the average daily gross value added per worker by dividing the overall gross value added (2,363.9 billion Euro) by the workforce size (41.586 million workers) and 365 days per year.

[92] Building on Hosios' (1990) rule, Shimer (2005) assigns vacancy-posting cost a value of 0.213 (as a fraction of a worker's productivity). At the same time, he sets a comparatively high value of 0.72 for workers' relative bargaining power, thus specifying a relatively steep wage-setting curve. Hagedorn and Manovskii (2008) choose higher vacancy-posting cost of 0.548 but assign workers a much lower relative bargaining power of 0.052, which mirrors a relatively flat wage-setting curve.

[93] Alternatively, when higher wages make workers reduce their supplied working hours (i.e., in the presence of strong income effects), firms are disincentivized to raise wages. Empirical evidence, however, suggests that income effects tend to be small (Bargain et al., 2014; Bargain and Peichl, 2016).

the limited wage effect can also be rationalized on the grounds of monopsony or bargaining models. In monopsony models, firms may maximize profits by voluntarily accepting lower employment levels (and forego revenues) to avoid costly wage increases. Relatedly, when firms command substantial bargaining power, the effect of higher tightness will not translate into markedly higher wages in the standard-DMP model.[94] Third, along the lines of the standard DMP model, firms with low vacancy posting cost (or search durations) have less incentive to increase wages to timely fill their vacancies.

3.7 Heterogeneous Effects

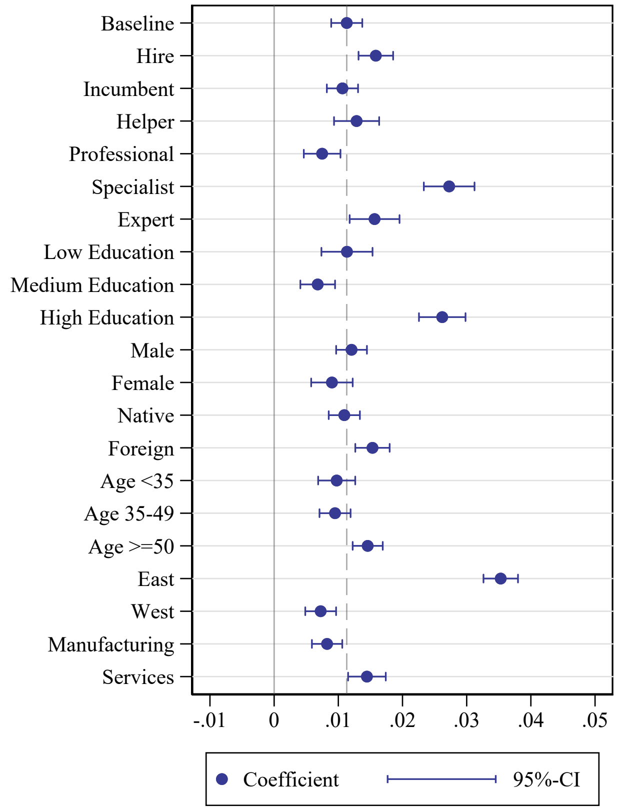
Subgroup Analysis. We estimate heterogeneous effects by interacting labor market tightness with the respective group dummies, as described by Equation (3.4). Figure 3.6 presents the elasticities from these subgroup analyses. The effect turns out markedly larger for new hires than for incumbent workers.[95] This result is in line with a number of studies that show that wages of job changers are significantly more flexible than the wages of incumbent employees (Pissarides, 2009; Haefke et al., 2013; Bassanini et al., 2023).

Next, we differentiate the effects by job requirement level. The effect for helpers roughly corresponds to the baseline effect, while the effect for professionals turns out markedly smaller. The effect for specialists is considerably larger, and the effect for experts is slightly larger than in the baseline. The heterogeneous effects by professional education level closely mirror this pattern: the effect for low-skilled workers is close to the baseline, whereas the effect for medium- and high-skilled workers are smaller and, respectively, larger than the baseline. Through the lens of the LS-LD framework, the larger effect for specialists, experts, and high-skilled can be explained by the lower substitutability of complex tasks (i.e., the labor demand curve for these workers is relatively inelastic/steep), whereas helpers and low-skilled workers perform easier tasks with more scope for substitution. Labor demand theory also helps to

[94]However, monopsony power does not necessarily have only a moderating influence on the effect of labor market tightness on wages. Monopsony power also constitutes a mediating force when higher tightness counteracts monopsony power and thereby raises marked-down wages. In the textbook monopsony model, this channel manifests in a flattening labor supply curve to the single firm. In the standard-DMP model, this channel is ruled out by treating workers' and firms' relative bargaining power as exogenous.

[95]We define a hire as a worker who is employed in a certain firm on June 30 of year t but not on June 30 in year $t-1$.

Figure 3.6: Heterogeneous Effects



NOTE: The figure displays estimated elasticities and 95% confidence intervals from IV regressions of log real daily wages of regular full-time workers on log labor market tightness to examine the heterogeneity regarding different characteristics (see Equation 3.4). Each specification includes fixed effects for years, workers, labor markets, and firms as well as control variables. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

reconcile why professionals and medium-skilled workers feature the smallest effects on wages. Due to the popularity of the dual vocational system in Germany, professionals and medium-skilled workers represent the largest groups in the German workforce. Thus, if wages for these large groups of workers rise, the wage bill increases by a relatively large margin. Consequently, labor demand for professionals and medium-skilled workers becomes relatively elastic due to large scale effects, limiting the scope for wage increases.[96]

[96]For Germany, Peichl and Popp (2022) and Popp (2023) show that demand for medium-skilled workers is more elastic than for low- and high-skilled workers due to relatively large scale effects.

We observe only marginal differences between men and women, namely that women's wages react slightly less strongly to changes in labor market tightness than men's wages. In terms of nationality and age, the effects are slightly greater for foreigners and for workers over the age of 50.

For workers in Eastern Germany, our effect turns out almost four times as large as for workers in Western Germany. A leading explanation for this pattern is the observation that monopsony power is more pronounced in Eastern Germany (Bachmann et al., 2022) and, thus, there is more scope for higher tightness to counteract markdowns.[97] In the service sector, the effect is markedly higher than in the manufacturing sector. This difference seems plausible, as service occupations are relatively labor-intensive and, thus, can be less easily substituted by capital than occupations in the manufacturing sector, manifesting in a relatively inelastic labor demand curve.

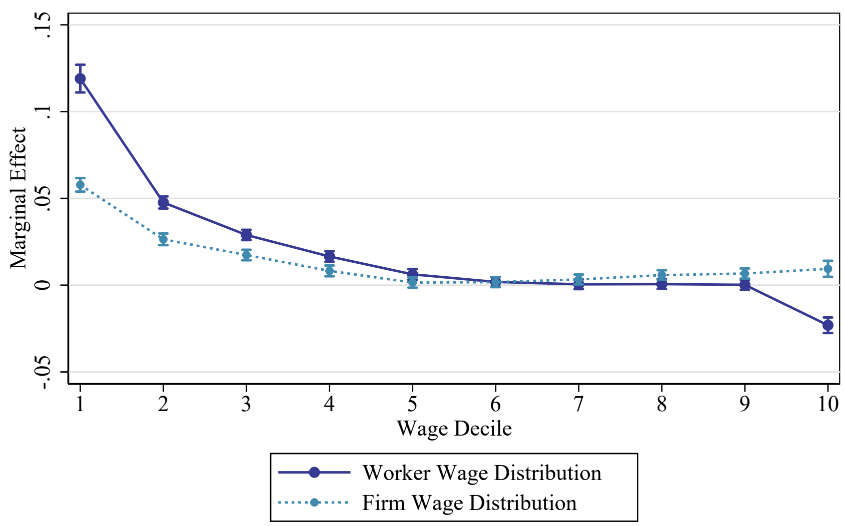
Heterogeneous Effects along the Wage Distribution. In an additional heterogeneity analysis, we examine differences in the effect magnitude along the wage distribution (see Appendix 3.F for further details). A large empirical literature established an increased wage inequality in Germany during the 1990s and 2000s (Dustmann et al., 2009; Dustmann et al., 2014; Goldschmidt and Schmieder, 2017; Antonczyk et al., 2009). Remarkably, wage inequality began declining from 2010 onwards, particularly due to wage increases at the bottom of the wage distribution (Blömer et al., 2023; Drechsel-Grau et al., 2022; Fedorets et al., 2020), which can be partly explained by the introduction of the statutory minimum wage (Bossler and Schank, 2023). In addition to the effects of the minimum wage, the enormous tightening of the labor market could also have contributed to falling wage inequality. This inequality-reducing effect was first highlighted for the U.S. by Autor et al. (2023) with data from after the Covid-19 pandemic.

We build on the work of Autor et al. (2023) and estimate the effect of labor market tightness on wages along the wage distribution. Specifically, we analyze the tightness elasticity along the worker-level wage distribution to identify potentially inequality-reducing effects. Moreover, we also analyze the respective elasticity along the firm-level wage distribution to

[97]The prerequisite for this mechanism is that the effect on labor supply elasticity as a result of higher outside options dominates.

identify the tightness response from the perspective of firm-level wages. We split the sample into decile groups of the (worker- and firm-level) wage distribution, which we then interact with labor market tightness and our leave-one-out instrument.[98]

Figure 3.7: Heterogeneous Effects along the Wage Distribution



NOTE: The figure displays estimated elasticities and 95% confidence intervals from IV regressions of log real daily wages of regular full-time workers on log labor market tightness for ten decile groups along the worker wage distribution (solid line) and the firm wage distribution (dashed line). Workers are assigned into ten decile groups based on their real daily gross wage when they first appear in the IEB as full-time workers during 2012-2022. Firms are assigned into ten decile groups based on the average real daily wage of their full-time workers when the firm first appears in the IEB sample during 2012-2022. Both curves refer to the baseline specification with fixed effects for years, workers, labor markets, and firms as well as control variables. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Figure 3.7 illustrates the estimated tightness elasticities along the wage distribution.[99] The estimates represent the interacted baseline IV specification, including control variables, fixed effects for years, workers, labor markets, and firms. The solid line displays the elasticities for each decile group of the workers' wage distribution. An increase in labor market tightness by 100 percent raises the wages of workers in the lowest decile group by roughly 12 percent.

[98] For the analysis of the worker wage distribution, we divide workers into ten equally-sized groups based on their real daily gross wage when they first appear in the IEB as full-time workers during 2012-2022. The use of predetermined wages ensures that workers do not switch decile groups during our period of analysis. For the analysis of the firm wage distribution, we divide workers into ten groups based on their firm's average real daily wage from the firm's first appearance during 2012-2022.

[99] The corresponding regression tables and first-stage estimates are shown in Appendix Tables 3.E10 and 3.E11.

This effect rapidly falls along workers' wage distribution. The effects for the sixth up to the ninth decile group of workers are still slightly positive but no longer statistically different from zero, while the effect in the tenth and highest decile group of workers turns out negative.[100][101]

The dashed line depicts the tightness elasticities for different firm-level wage decile groups. The pattern largely resembles the pattern from the wage distribution of workers: An increase in labor market tightness by 100 percent raises average wages of firms in the lowest decile group by roughly 6 percent. The effect stays significantly positive in the second, third, and fourth decile group of firms but turns insignificant in the middle of the wage distribution of firms. Unlike for workers, we observe a slightly positive effect in the top three decile groups of firms.

Taken together, the positive wage effects at the bottom of workers' wage distribution point towards an inequality-reducing effect of the rise in labor market tightness. During our period of analysis between 2012 and 2022, real wage dispersion declined by 24.0 percent (when comparing average wages in the highest decile group with average wages in the lowest decile group, see Appendix Figure 3.F3). In order to determine what proportion of the decline in wage dispersion can be attributed to the increase in tightness, we calculate the development of wage dispersion at constant tightness. For each decile group, the counterfactual is calculated by subtracting the product of the wage elasticity with respect to tightness and the observed tightness increase. Of these 24.0 percent, about 12.2 percentage points can be ascribed to the increased tightness (see Appendix Figure 3.F4). Our finding that primarily low-paying firms raised wages in response to rising tightness shows that the reduction in wage inequality might be driven by wage increases between (rather than within) firms. Overall, our results along the

[100] Note that the negative effect at the very top of the distribution could be driven by the top-coding of wages at the social security limit above which we impute wages (see Section 3.3) or mean reversion.

[101] Our finding that low-wage workers benefit the most from rising tightness does not contradict the results from our subgroup analysis, which finds lower elasticities for helpers and professionals than for specialists and experts. Appendix Figure 3.F2 shows that helpers and professionals are not only represented at the lower end of the wage distribution. In particular, professionals account for more than fifty percent of workers in each of the bottom eight decile groups while still accounting for more than 40 and 20 percent in the top two decile groups, respectively. Against the backdrop of this pattern, our analysis by workers' decile groups suggests that those workers who earn comparatively low wages within their requirement level benefit most from the increase in labor market tightness. Moreover, when simultaneously differentiating our effects by workers' decile groups and requirement level (see Figure 3.F1), it turns out that, among the lowest decile groups, specialists and experts benefit more strongly from rising tightness than helpers or professionals.

wage distribution lend credence to the notion that higher tightness improved the bargaining power of workers, raising marked-down wages in the subgroup of firms with wage-setting power but less so in the subgroup of firms that pay competitively.

Firm-Level Wage Setting. Our finding that primarily low-paying firms raised wages in response to tightness prompts the question of the role of firms in the wage-setting process. Firms explain a large share of the total variation in wages, as they pay a firm-specific premium through rent sharing (Van Reenen, 1996) or a match-specific premium (Abowd et al., 1999; Card et al., 2013). In addition, firm-level institutions, such as collective bargaining, are reducing wage dispersion (Freeman, 1982), in particular within firms. From our analysis of effects along the wage distribution, we observe pronounced wage increases in low-paying firms, but it remains open whether these firms differentiate between different occupational groups within the same workplace when setting wages. This culminates in the question of whether firms only raise wages in response to an increasing tightness of a certain occupational group or whether they pay all employees a raise, even if the occupation of the person in question has not experienced an increase in tightness but tightness of co-workers.

We take two approaches to address the question of firm-specific wage setting, both displayed in Table 3.4. First, we re-examine the baseline wage effect (as displayed in Column (4) of Table 3.2) and further include firm-by-year fixed effects. These fixed effects eliminate the average wage change that occurs within firms over time. Hence, the resulting effect only captures variation from differential tightness increases of different occupations within a firm. Second, we estimate the effect of tightness on the average wage of all workers in a firm.[102] In doing so, we examine whether all the co-workers benefit from a tightness increase even if the respective coworkers' tightness does not itself change.

Upon inclusion of firm-by-year fixed effects, our IV effect drastically shrinks in Column (1), indicating that firms hardly differentiate between occupations when the employed occupations experience a differential tightness increase. For example, a manager in a firm receives only a slightly higher pay growth than his or her secretaries if the tightness of managers increases

[102]Note that we partial out an individual-level fixed effect before averaging wages at the firm level. This procedure ensures that we control for the same granularity of individual heterogeneity as in the baseline estimation.

Table 3.4: IV Regressions of Wage Setting at the Firm Level

	(1) Log Wage	(2) Log $\overline{\text{Wage}}^{\text{Firm}}$
Log Tightness	0.0024**	0.0088***
	(0.0011)	(0.0012)
Year FE		yes
Worker FE	yes	yes
Labor Market FE	yes	yes
Firm FE		yes
Firm-by-Year FE	yes	
Controls	yes	yes
Observations	6,424,250	8,454,541
Clusters	12,676	13,692
First-Stage Coefficient	0.8597***	0.8814***
First-Stage F-Statistic of Excluded Instrument	3,247	2,740

NOTE: The table displays IV regressions of log (average firm-level) real daily wages of regular full-time workers on log labor market tightness. The instrumental variable refers to leave-one-out averages of labor market tightness in all other commuting zones but for the same occupation and time period. Control variables include binary variables for new hires, workplace location in Eastern Germany, three levels of professional education, and continuous variables for age and squared age. Labor markets are combinations of detailed 3-digit occupations and commuting zones. Column (1) shows IV estimates when additionally controlling for firm-by-year fixed effects. Column (2) displays IV estimates when regressing the firm-level average of real daily wages on log labor market tightness. Standard errors (in parentheses) are clustered at the labor market level: *=p<0.10. **=p<0.05. ***=p<0.01. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

compared to those of secretaries. In line, the use of firm-level averages of wages in Column (2) demonstrates that the average wage of all workers rises almost as much as the individual worker's wage when labor markets tighten. In our example, it implies that the secretary also benefits from the tightness of managers.

3.8 Conclusion

Combining exceptionally detailed information on vacancies and job seekers with administrative employment data, we estimate the effect of increased scarcity of the labor input on workers' compensation in Germany for the years 2012 to 2022. Using high-dimensional fixed effects and a new leave-one-out instrument, our empirical analysis delivers a positive upper-bound elasticity of wages with respect to labor market tightness of 0.011. Controlling for potentially confounding (occupation-specific and nationwide) productivity shocks, we arrive at a lower-bound elasticity of 0.004. Taken together, we conclude that labor market tightness has a statistically significant but modest impact on wages. Our estimated effect range implies that the observed increase in labor market tightness by around 130 percent between 2012 and 2022 explains between 7 and 19 percent of the rise in average real wages during the same period.

However, our results point out several interesting heterogeneities which reveal somewhat stronger wage effects. We find particularly large elasticities for newly hired workers, high-skilled workers, the Eastern German labor market, and the service sector. We also observe that wage increases are significantly stronger at the lower end of the wage distribution, which is mainly driven by wage increases from low-paying firms. At the same time, the effects are virtually zero above the fourth decile of wages. Thereby, we document an inequality-reducing effect of higher labor market tightness, thus corroborating evidence from Autor et al. (2023) for the U.S. labor market.

Our results provide some important implications. First, our positive but limited tightness effect mirrors a relatively flat wage-setting curve. Specifically, our upper- and lower-bound log-log elasticities translate into linear coefficients in the range between 0.013 and 0.032, and these relatively low values may guide researchers to their calibrate search-and-matching models. Second, to put our wage effects into perspective, it is worth exploring whether the pay increases were sufficient to overcome firms' hiring frictions that usually arise in tight labor markets (Le Barbanchon et al., 2024). Building on the same data, Bossler and Popp (2024) find that, holding all other things equal, the doubling in tightness reduced firms' employment growth on average by 5 percent between 2012 and 2019. When abstracting that our time horizon is three years longer, our estimated wage increases between 0.6 and 1.5 percent (along with potential improvements in non-monetary job amenities) in response to higher tightness were seemingly not strong enough to maintain firms' employment growth. Third, since we do not find any indications of non-linearities in the tightness elasticity, our estimates can provide tentative guidance for what will happen when the German labor market keeps tightening in the upcoming years due to intensifying demographic decline: namely tangible but limited wage increases (despite muted employment growth) that further reduce wage inequality at the bottom end of the wage distribution.

Appendix - Chapter 3

3.A Data on Vacancies: Further Details

The official statistics of the German Federal Employment Agency (FEA) collect information on registered vacancies. Registered vacancies refer to those job openings that firms have forwarded to the FEA to facilitate the recruitment of suitable candidates. Upon registration, the firms have to state their targeted 5-digit occupation and the district of the workplace. Vacancies that have not been registered with the FEA, such as those posted exclusively through other channels like newspapers or private online job boards, are not included in the Offical Statistics of the Federal Employment Agency.

To additionally take into account unregistered vacancies, we leverage additional information from the IAB Job Vacancy Survey (IAB-JVS). The IAB Job Vacancy Survey is a quarterly business survey that focuses on labor demand and recruitment practices. In the last quarter of the year, about 15,000 firms disclose the number and structure of their vacancies. Firms are requested to distinguish between their registered and unregistered vacancies and further categorize them based on the requirement level of the associated jobs. Importantly, this information allows us to construct the share of registered vacancies in all vacancies separately by requirement level. As the survey only began differentiating between specialists and experts in 2015, we pool this information and compute notification shares for specialists and experts as a whole.

Table 3.A1 of this appendix illustrates the evolution of notification shares by requirement level. The notification shares exhibit some temporal variation and generally decline with an increasing level of requirement. While almost half of the vacancies for helper occupations

are registered with the FEA, the notification share of vacancies for professionals tends to be slightly lower. For specialists and experts, less than one in three vacancies is registered with the Federal Employment Agency.

To quantify the overall number of registered and unregistered vacancies in a given labor market and year, we make use of the above-mentioned sources and follow the approach from (Bossler and Popp, 2024). First, we extract the number of registered vacancies for each combination of 5-digit occupation, district, and year from the Federal Employment Agency's Official Statistics. Following standard practice of the FEA, we apply the following filters when drawing these data: we exclude vacancies with an employment duration of less than seven days, subsidized vacancies, vacancies for freelancers, and vacancies from private employment agencies. Second, we divide the resulting number of registered vacancies for each labor market and year by the corresponding notification share. When dividing by the yearly notification share, we use the requirement level (i.e., the fifth digit of the KldB occupation variable) to differentiate between the notification shares of helpers, professionals, and specialists along with experts. As a result, we arrive at the overall number of registered and unregistered vacancies for each combination of 5-digit occupation, district, and year. In the third and final step, we aggregate these yearly numbers for each combination of detailed 3-digit occupation and commuting zone to conform with our baseline definition of a labor market.

Table 3.A1: Shares of Registered Vacancies in All Vacancies by Requirement Level

Year	Helpers	Professionals	Specialists and Experts
2012	36.0	45.0	33.6
2013	44.2	47.0	25.7
2014	48.0	41.9	29.9
2015	48.1	46.5	29.3
2016	53.4	50.5	36.6
2017	52.3	46.4	31.1
2018	44.0	46.2	32.7
2019	43.5	41.9	30.4
2020	45.0	39.1	26.0
2021	46.5	43.0	27.3
2022	43.1	41.8	26.9
2012-2022	45.8	44.5	29.9

NOTE: The table shows the yearly percentage shares of registered vacancies in all vacancies, separately by requirement levels (i.e., the fifth digit of the KldB-2010 occupation variable). Helper occupations require no training or only a maximum of one year's training. The group of professionals includes all activities with industrial, commercial or other vocational training (excluding master craftsmen and technicians). Specialist occupations necessitate a bachelor degree or the completion of master craftsman/technician training. Experts hold a master degree or an equivalent diploma. *Source:* IAB Job Vacancy Survey, 2012-2022.

3.B Delineation of Commuting Zones

The definition of a region should mirror the spatial clustering of economic activities as precisely as possible. A "functional region" is defined as a cluster of neighboring areas in which a large fraction of economic activities like commuting and trade occurs among resident workers and businesses. Thus, functional regions quite accurately capture the spatial dimension of economic flows. By contrast, administrative delineations, such as districts which are formed by political boundaries, often fall short in capturing economic interactions well. Consequently, a large fraction of economic exchanges occurs across rather than within these administrative borders.

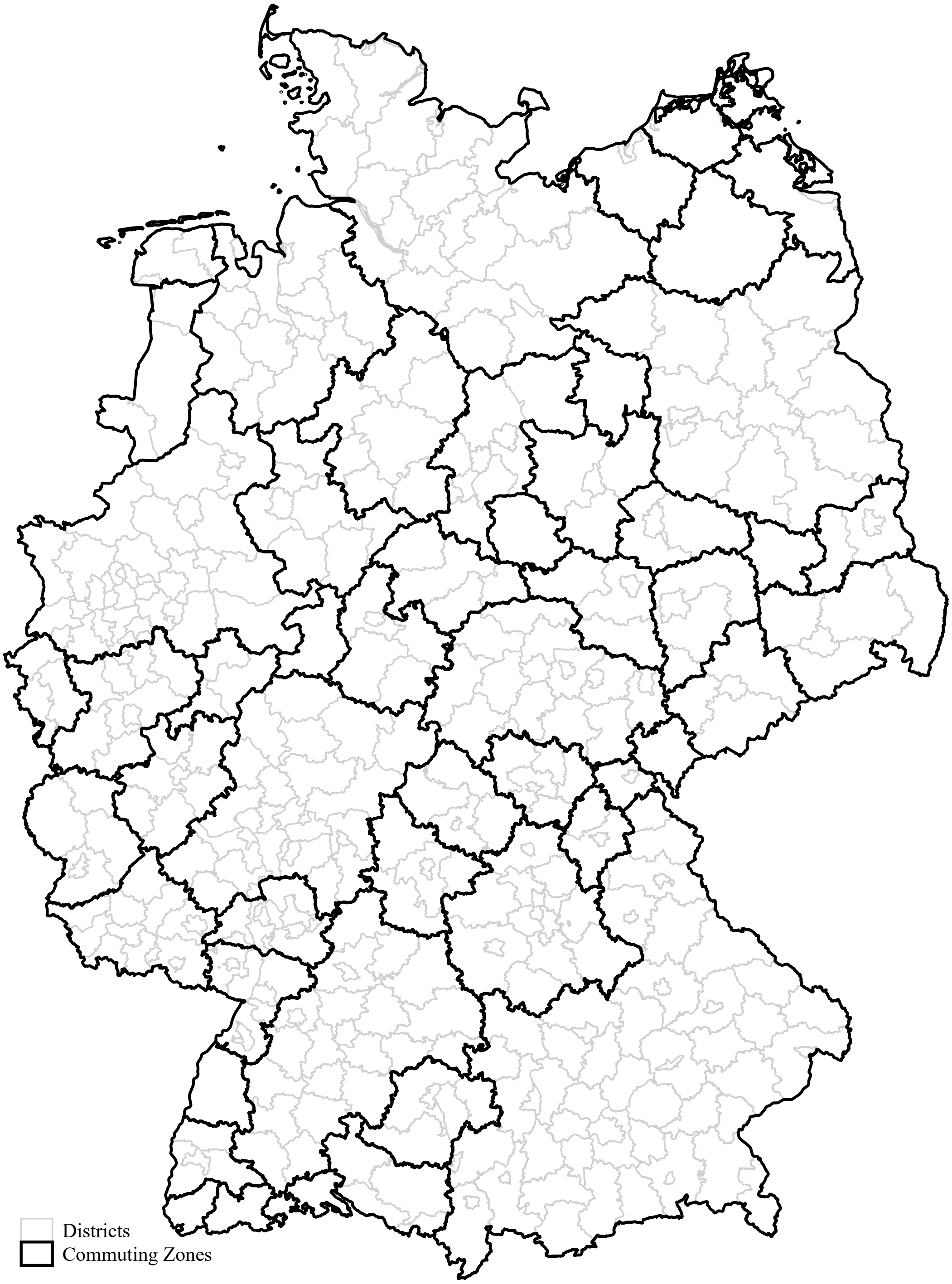
Relying on administrative regions can lead to a mismeasurement of labor market tightness. Assume that there are two cities with many people commuting from city 1 to work for firms in the neighboring city 2 (and vice versa). In such an environment, calculating labor market tightness in city 1 based on vacancies and job seekers only from city 1 mistakenly ignores that vacancies and job seekers from city 2 constitute additional outside options for workers and firms in city 1. When tightness in city 2 is lower (higher) than in city 1, the ratio of vacancies from city 1 to job seekers from city 1 will overestimate (underestimate) the true labor market tightness in city 1 (which must also reflect the vacancies and job seekers from the closely connected city 2).

To address this issue, we create functional labor market regions based on observed home-to-work commuting patterns. These commuting zones are designed to have relatively many connections within zones and relatively few connections between zones to minimize cross-border flows. We employ a graph-theoretical method developed by (Kropp and Schwengler, 2016) to merge 401 administrative districts into more appropriate commuting zones. This method involves several steps: First, we calculate a matrix of bi-directional commuting flows among administrative regions and identify dominant flows between regions to guide potential mergers. Mergers occur when the dominant flow exceeds a certain threshold, resulting in a consolidated flow matrix. This process is iteratively repeated until further mergers are unnecessary. Second, we repeat the first step with varying threshold values to propose multiple meaningful delineations and select the delineation that delivers the highest modularity value. The concept of modularity is a popular measure to determine the degree of clustering in networks of connections. Specifically, the modularity concept compares the number of ties inside a cluster with the expected number of ties if the network of ties between clusters was random.[103] Third, we ensure that all districts within the newly delineated commuting zone form a coherent area.

We implement this procedure using German FEA data on commuting patterns of the universe of contributory workers in the years 2012-2022. Our starting point refers to 401 administrative districts (i.e., 3-digit NUTS regions), which initially achieve a modularity value of 0.603. After gradually increasing the merger threshold, we find that a threshold of 8 percent yields generates the delineation with the highest modularity ($Q = 0.838$). After eight iterations of merging dominant flows above this threshold, we arrive at 52 commuting zones with strong internal interactions but limited connections between zones (see Appendix Figure 3.B1). The use of these commuting zones rather than districts reduces the share of commuters between regions from 39.0 to 10.8 percent.

[103] The value of modularity Q equals zero when the delineation does not perform better than a random delineation. Q approaches the maximum value of $Q = 1$ when the network itself is strongly modular (i.e., there are many ties within clusters) and was correctly delineated by the procedure. Values of Q usually range between 0.3 and 0.7.

Figure 3.B1: Delineation of Commuting Zones



NOTE: The figure illustrates the delineation of commuting zones based on 401 German districts (NUTS-3 regions). The districts were merged into 52 commuting zones using the graph-theoretical method (Kropp and Schwengler, 2016) and register data on German commuting patterns between 2012 and 2022. *Data Source:* Official Statistics of the German Federal Employment Agency, 2012-2022.

3.C The Construction of Flow-Adjusted Labor Market Tightness

When examining the effects of labor market tightness, researchers usually employ regions or, in rare circumstances, occupations to delineate the relevant labor market. However, in doing so, labor markets are divided into mutually exclusive segments. This definition rules out that vacancies and job seekers compete with similar vacancies or job seekers from a different labor market (i.e., a neighboring region or a closely related occupation) in the recruitment and search process, respectively. The mutually exclusive concept of a labor market may result in an overly narrow definition of labor markets but it could, in principle, also be too broad if workers (firms) only search (recruit) within a subsegment of the respective market.

By virtue of our functional delineation of 52 commuting zones (see Appendix 3.B), we already account for the fact that labor markets are functioning across administratively defined districts. By contrast, the occupational dimension of our baseline labor markets is based on 431 detailed 3-digit occupations, which stem from the German Classification of Occupations 2010 (KldB-2010). Note that the aim of this classification already is to group occupations with similar tasks into the same categories (Federal Employment Agency, 2011). Nevertheless, the mutual exclusiveness of this delineation cannot rule out that workers and firms search and recruit from neighboring occupations.

To overcome this shortcoming, we implement a data-guided approach and take vacancies and job seekers outside the focal occupation as additional outside options into account. Specifically, we closely follow Bossler and Popp (2024) and construct a flow-adjusted version of labor market tightness that builds on occupational mobility patterns to determine weights for vacancies and job seekers in neighboring occupations. Their approach is inspired by Arnold (2021) who accounts for jobs in neighboring markets when calculating indices of labor market concentration.

The underlying idea of the flow adjustment is that the relative value of vacancies and job seekers in different occupations can be inferred from labor market flows within and between these occupations. Let $P(h|o)$ denote the probability that a worker in occupation o in year t is employed in occupation h in year $t+1$. When recruiting (searching for a job in) occupation o, the firm's (worker's) relative value of a job seeker (vacancy) in occupation h (compared to occupation o) then is:

$$\omega_{oh} = \frac{P(h|o)}{P(o|o)} \cdot \frac{L_o}{L_h} \tag{3.C.1}$$

To calculate weights from these flows, it is necessary to consider that flows from one market to another depend on the relative size of the markets. Therefore, we normalize relative transition probabilities by employment in the respective occupations. Note that, by construction, the occupation in question always receives a weight of one, i.e., $\omega_{oo} = 1$. When determining the transition probabilities with the administrative IEB data, we pool mobility patterns over labor market regions and the years 2012-2022 to arrive at a stable weighting matrix for mobility between all detailed 3-digit occupations.

Given the weighting matrix, the flow-adjusted number of vacancies in occupation o and region r

$$\tilde{V}_{ort} = \sum_{h=1}^{H} \omega_{oh} V_{ort} \tag{3.C.2}$$

is calculated as the weighted sum of vacancies in the same occupation and all other occupations in region r and time t. By construction, the number of flow-adjusted vacancies always exceeds the number of actual vacancies in a labor market because the flow adjustment takes into account that workers can fill vacancies not only in the same occupation but also in neighboring occupations.

Similarly, the flow-adjusted number of job seekers in occupation o and region r is

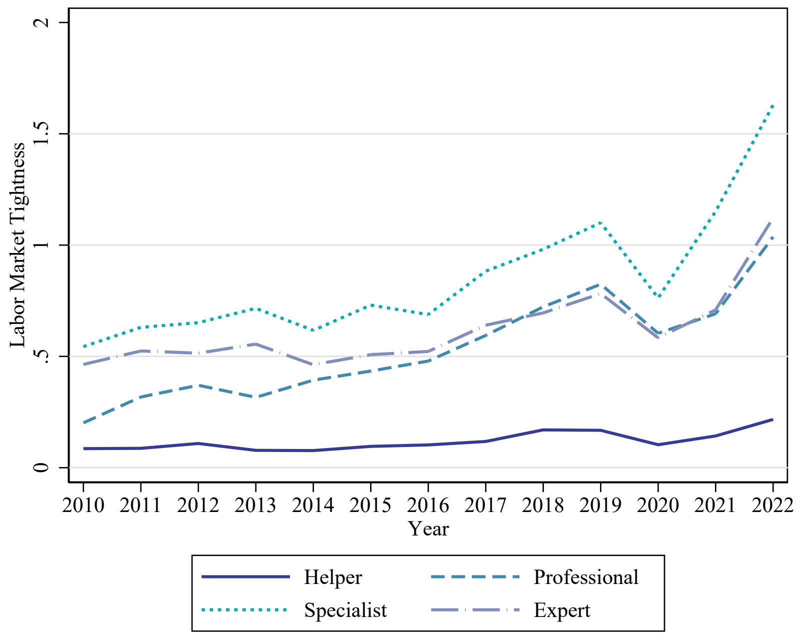
$$\tilde{U}_{ort} = \sum_{h=1}^{H} \omega_{oh} U_{ort} \tag{3.C.3}$$

which is the sum of job seekers in the same occupation and job seekers of all other occupations in region r and time t. Again, the number of flow-adjusted job seekers always exceeds the number of actual job seekers in a labor market because the flow adjustment considers that firms can recruit job seekers not only from the same occupation but also from neighboring occupations.

Note that, when there are no flows between occupations, the weight is zero. In this case, the neighboring occupations do not constitute a relevant outside options and the flow-adjusted measure pins down to the baseline measure, which relies only on the vacancies and job seekers from the focal occupation. When there are random flows between occupations, all occupations receive the same weight. Then, the flow-based number of vacancies and job seekers collapses to the factual number of vacancies and job seekers in the commuting zone.

3.D Descriptive Results: Further Evidence

Figure 3.D1: Labor Market Tightness by Job Requirement Level over Time

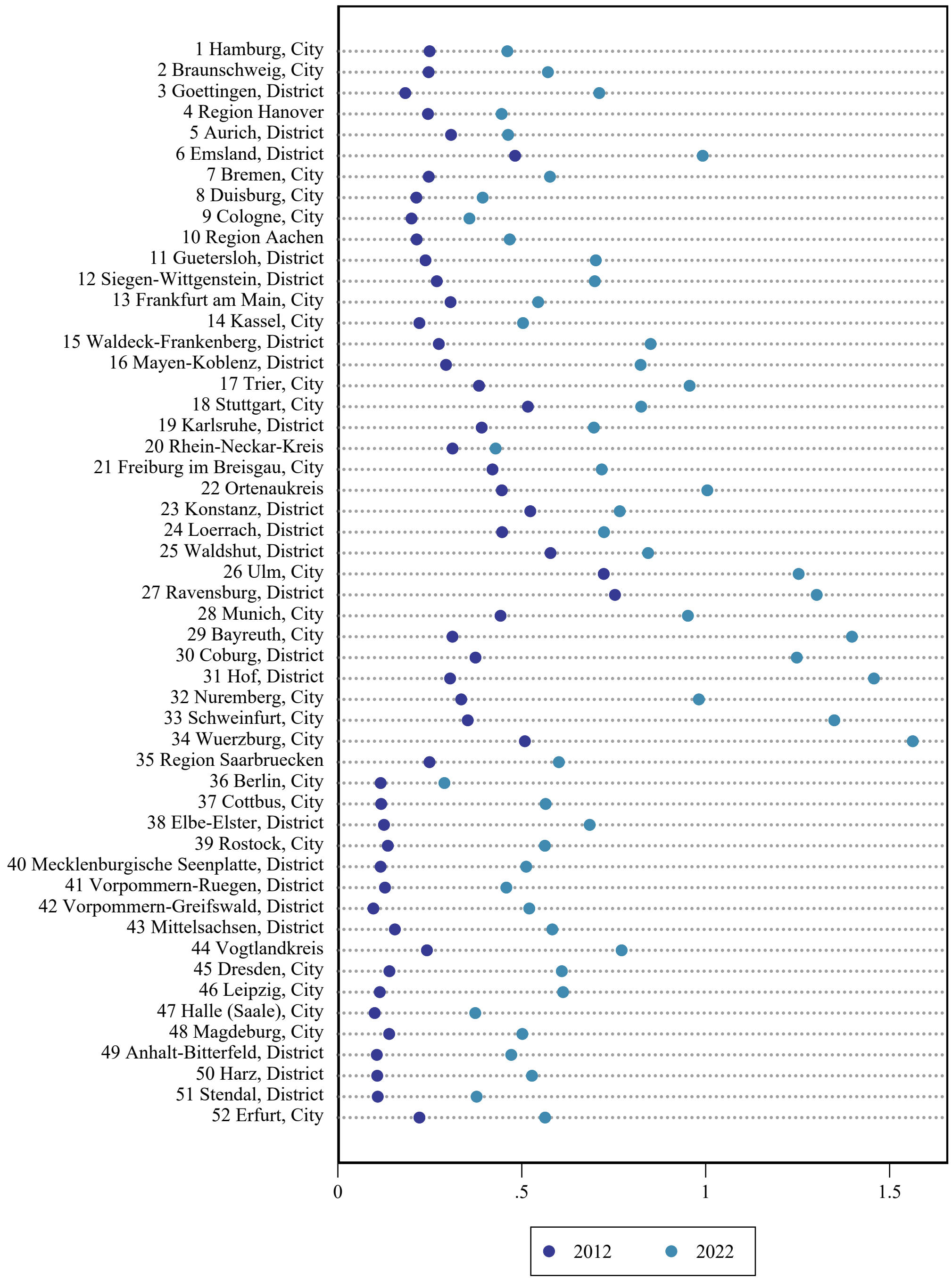


NOTE: The figure illustrates the development of labor market tightness (= vacancies/job seekers) in Germany over time by requirement levels (i.e., the fifth digit of the KldB-2010 occupation variable). *Data Source:* Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Figure 3.D2: Labor Market Tightness by 2-Digit Occupation

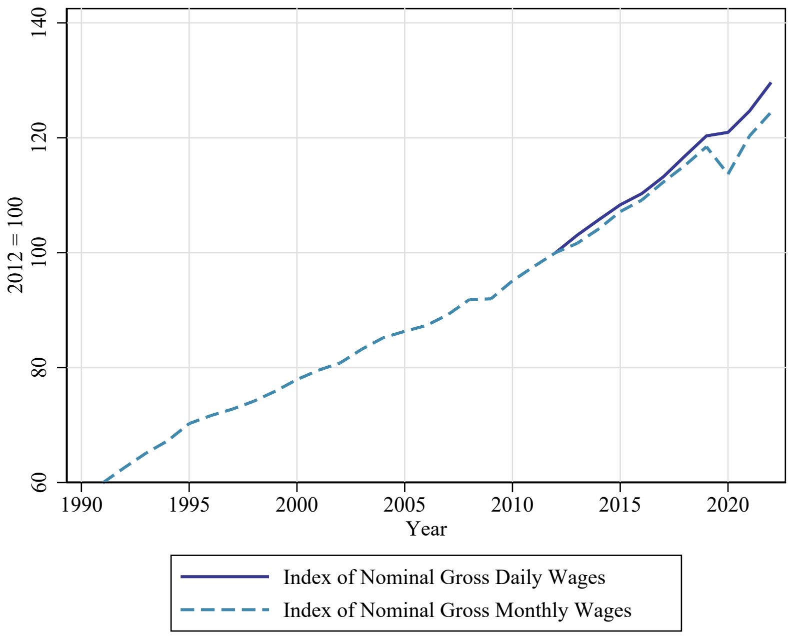
NOTE: The figure illustrates the development of labor market tightness (= vacancies/job seekers) in Germany between 2012 and 2022 by 2-digit occupations. *Data Source:* Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Figure 3.D3: Labor Market Tightness by Commuting Zone



NOTE: The figure illustrates the development of labor market tightness (= vacancies/job seekers) in Germany between 2012 and 2022 by commuting zones. Commuting zones are delineated using the graph-theoretical approach proposed by Kropp and Schwengler (2016). *Data Source:* Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Figure 3.D4: Nominal Wages in Germany over Time



NOTE: The figure illustrates the development of wages in Germany over time. The solid line refers to nominal gross daily wages (including special payments) for regular full-time workers in the non-agricultural private business sector. The dashed line shows the index of nominal gross monthly wages for full-time workers excluding special payments. The latter time series refers to the manufacturing sector until 2006 and to manufacturing and services from 2007 onwards. *Data Source:* Integrated Employment Biographies + Official Data of the German Federal Statistical Office, 1991-2022.

Table 3.D1: Descriptive Statistics

	Mean	P25	P50	P75	SD	N
Labor Market Tightness	0.97	0.27	0.56	1.12	1.41	8,885,219
Gross Daily Wage (in 2012-Euro)	118.06	73.16	100.50	143.43	69.12	8,907,310
Hire	0.18	-	-	-	0.39	8,907,310
Job Requirement Level						
Helper	0.12	-	-	-	0.33	8,907,310
Professional	0.58	-	-	-	0.49	8,907,310
Specialist	0.16	-	-	-	0.37	8,907,310
Expert	0.14	-	-	-	0.35	8,907,310
Education Level						
Low	0.07	-	-	-	0.26	8,907,310
Medium	0.72	-	-	-	0.45	8,907,310
High	0.20	-	-	-	0.40	8,907,310
Female	0.26	-	-	-	0.44	8,907,310
Foreign	0.10	-	-	-	0.30	8,907,310
Age	42.96	33.00	43.58	52.66	11.68	8,907,310
East Germany	0.15	-	-	-	0.36	8,907,310
Manufacturing Sector	0.45	-	-	-	0.50	8,907,310

NOTE: The table displays descriptive sample means, selected percentiles, standard deviations, and the numbers of observations of the relevant variables. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Table 3.D2: Descriptive Statistics of Main Variables by Subgroups

	Mean	P50	SD	Min	Max	N
Gross Daily Wage (in 2012-Euro)						
Overall	118.06	100.50	69.12	0.01	1845.87	8,907,310
Hire	96.70	80.58	58.69	0.01	1403.89	1,613,069
Incumbent	122.79	105.01	70.35	0.01	1845.87	7,294,241
Helper	75.49	70.63	29.03	0.01	522.24	1,084,707
Professional	99.46	91.29	43.85	0.01	1033.36	5,131,582
Specialist	150.46	138.03	71.50	0.01	1124.33	1,448,068
Expert	194.30	176.67	97.76	0.33	1845.87	1,242,953
Low Education	82.04	74.20	40.21	0.01	1586.55	661,026
Medium Education	103.88	94.02	47.53	0.01	1121.69	6,449,947
High Education	182.25	163.14	98.30	0.01	1845.87	1,796,337
Male	124.73	105.55	72.03	0.01	1586.55	6,569,545
Female	99.32	86.59	56.08	0.01	1845.87	2,337,765
Native	120.09	102.43	69.74	0.01	1845.87	8,021,830
Foreign	99.69	84.03	60.14	0.01	1221.98	885,480
Age <35	98.70	89.25	46.89	0.01	1104.98	2,665,428
Age 35-49	126.34	106.85	74.34	0.01	1564.56	3,297,593
Age >=50	126.33	106.35	76.08	0.01	1845.87	2,944,289
East	88.40	74.82	47.73	0.01	1102.33	1,337,439
West	123.31	105.34	70.96	0.01	1845.87	7,569,871
Manufacturing	125.56	108.60	68.08	0.01	1790.24	3,981,248
Services	112.01	92.48	69.36	0.01	1845.87	4,926,062
Labor Market Tightness						
Overall	0.97	0.56	1.41	0.00	219.00	8,885,219
Hire	0.87	0.50	1.30	0.00	148.00	1,610,120
Incumbent	0.99	0.57	1.44	0.00	219.00	7,275,099
Helper	0.29	0.17	0.46	0.00	54.00	1,084,089
Professional	1.05	0.64	1.38	0.00	148.00	5,125,371
Specialist	1.11	0.74	1.64	0.00	219.00	1,437,641
Expert	1.02	0.53	1.65	0.00	89.00	1,238,118
Low Education	0.73	0.40	1.14	0.00	148.00	660,135
Medium Education	1.00	0.58	1.43	0.00	219.00	6,435,201
High Education	0.94	0.55	1.44	0.00	148.00	1,789,883
Male	1.07	0.62	1.53	0.00	219.00	6,552,179
Female	0.67	0.43	0.98	0.00	148.00	2,333,040
Native	0.97	0.57	1.42	0.00	219.00	8,000,882
Foreign	0.90	0.50	1.32	0.00	148.00	884,337
Age <35	1.02	0.60	1.45	0.00	148.00	2,659,874
Age 35-49	0.94	0.55	1.38	0.00	148.00	3,289,441
Age >=50	0.94	0.54	1.42	0.00	219.00	2,935,904
East	0.81	0.44	1.25	0.00	89.00	1,332,228
West	0.99	0.58	1.44	0.00	219.00	7,552,991
Manufacturing	1.20	0.70	1.63	0.00	219.00	3,969,420
Services	0.78	0.47	1.18	0.00	219.00	4,915,799

Note: The table displays descriptive sample means, medians, standard deviations, extreme values, and the number of observations of the main variable by subgroups. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

3.E Full Regression Tables

Table 3.E1: OLS Effects of Labor Market Tightness on Wages

	(1) Log Wage	(2) Log Wage	(3) Log Wage	(4) Log Wage
Log Tightness	0.0079***	0.0088***	0.0065***	0.0074***
	(0.0008)	(0.0006)	(0.0006)	(0.0005)
Hire	-	-0.0428***	-	-0.0384***
	-	(0.0007)	-	(0.0007)
Eastern Germany	-	-0.0328***	-	-
	-	(0.0026)	-	-
Age	-	0.0648***	-	0.0525***
	-	(0.0033)	-	(0.0047)
Age^2	-	-0.0006***	-	-0.0005***
	-	(0.0000)	-	(0.0000)
Low Education	-	base	-	base
	-	-	-	-
Medium Education	-	0.1614***	-	0.1412***
	-	(0.0042)	-	(0.0061)
High Education	-	0.3777***	-	0.3767***
	-	(0.0082)	-	(0.0115)
Year FE	yes	yes	yes	yes
Worker FE	yes	yes	yes	yes
Labor Market FE	yes	yes	yes	yes
Firm FE			yes	yes
Controls		yes		yes
Observations	8,584,726	8,584,726	8,454,953	8,454,953
Clusters	13,806	13,806	13,716	13,716

NOTE: The table displays OLS regressions of log daily wages of regular full-time workers on log labor market tightness. Control variables include binary variables for new hires, workplace location in Eastern Germany, three levels of professional education and continuous variables for age and squared age. Labor markets are combinations of detailed 3-digit occupations and commuting zones. Standard errors (in parentheses) are clustered at the labor market level: *=p<0.10. **=p<0.05. ***=p<0.01. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Table 3.E2: IV Effects of Labor Market Tightness on Wages

	(1) Log Wage	(2) Log Wage	(3) Log Wage	(4) Log Wage
Log Tightness	0.0124***	0.0114***	0.0114***	0.0113***
	(0.0018)	(0.0014)	(0.0015)	(0.0012)
Hire	-	-0.0428***	-	-0.0384***
	-	(0.0007)	-	(0.0007)
Eastern Germany	-	-0.0328***	-	-
	-	(0.0026)	-	-
Age	-	0.0648***	-	0.0525***
	-	(0.0033)	-	(0.0047)
Age^2	-	-0.0006***	-	-0.0005***
	-	(0.0000)	-	(0.0000)
Low Education	-	base	-	base
	-	-	-	-
Medium Education	-	0.1614***	-	0.1412***
	-	(0.0042)	-	(0.0061)
High Education	-	0.3780***	-	0.3770***
	-	(0.0082)	-	(0.0115)
Year FE	yes	yes	yes	yes
Worker FE	yes	yes	yes	yes
Labor Market FE	yes	yes	yes	yes
Firm FE			yes	yes
Controls		yes		yes
Observations	8,584,317	8,584,317	8,454,543	8,454,543
Clusters	13,783	13,783	13,692	13,692
First-Stage Coefficient	0.8914***	0.8915***	0.8814***	0.8814***
First-Stage F-Statistic of Excluded Instrument	2,847	2,848	2,737	2,740

NOTE: The table displays IV regressions of log real daily wages of regular full-time workers on log labor market tightness. The instrumental variable refers to leave-one-out averages of labor market tightness in all other commuting zones but for the same occupation and time period. Control variables include binary variables for new hires, workplace location in Eastern Germany, three levels of professional education, and continuous variables for age and squared age. Labor markets are combinations of detailed 3-digit occupations and commuting zones. Standard errors (in parentheses) are clustered at the labor market level: *=p<0.10. **=p<0.05. ***=p<0.01. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Table 3.E3: Addressing Productivity Shocks at the National Level

	(1) Log Wage	(2) Log Wage	(3) Log Wage	(4) Log Wage
Log Tightness	0.0060***	0.0101***	0.0057***	0.0044***
	(0.0004)	(0.0028)	(0.0011)	(0.0017)
Log Firm Productivity	-	0.0214***	-	-
	-	(0.0021)	-	-
Log Leave-One-Out Sum V	-	-	-	0.0114***
	-	-	-	(0.0016)
Hire	-0.0378***	-0.0328***	-0.0375***	-0.0384***
	(0.0007)	(0.0016)	(0.0007)	(0.0007)
Age	0.0531***	0.0746	0.0525***	0.0526***
	(0.0048)	(0.1112)	(0.0046)	(0.0047)
Age^2	-0.0005***	-0.0004***	-0.0005***	-0.0005***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Low Education	base	base	base	base
	-	-	-	-
Medium Education	0.1418***	0.1756***	0.1396***	0.1411***
	(0.0061)	(0.0271)	(0.0062)	(0.0061)
High Education	0.3707***	0.3858***	0.3736***	0.3764***
	(0.0114)	(0.0410)	(0.0116)	(0.0115)
Year FE	yes	yes	yes	yes
Worker FE	yes	yes	yes	yes
Labor Market FE	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes
Occupation-by-Year FE	yes			
Industry-by-Year FE			yes	
Controls	yes	yes	yes	yes
Source	5% IEB	LIAB	5% IEB	5% IEB
Survey Weights		yes		
Observations	8,454,869	3,117,429	8,454,541	8,454,541
Clusters	13,698	8,865	13,692	13,692
First-Stage Coefficient		0.8569***	0.8558***	0.7920***
First-Stage F-Statistic of Excluded Instrument		2,098	2,679	1,629

NOTE: The table displays OLS and IV regressions of log real daily wages of regular full-time workers on on log labor market tightness. The instrumental variable refers to leave-one-out averages of labor market tightness in all other commuting zones but for the same occupation and time period. Control variables include binary variables for new hires, workplace location in Eastern Germany, three levels of professional education and continuous variables for age and squared age. Labor markets are combinations of detailed 3-digit occupations and commuting zones. Column (1) shows OLS estimates when additionally controlling for detailed 3-digit-occupation-by-year fixed effects. Column (2) features IV estimates when controlling for log firm productivity in the LIAB worker sample. Column (3) displays IV estimates when further controlling for 1-digit industry-by-year fixed effects (based on the NACE 2.0 classification). Column (4) presents IV estimates when conditioning on the log leave-one-out sum of the number of vacancies in all other commuting zones but for the very same occupation and time period. Standard errors (in parentheses) are clustered at the labor market level: *=p<0.10. **=p<0.05. ***=p<0.01. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey + Linked Employer-Employee Dataset of the IAB, 2012-2022.

Table 3.E4: Robustness Checks I

	(1) **Detailed 2-Digit Occupations**	(2) **Detailed 4-Digit Occupations**	(3) **Flow-Adjusted V/U**	(4) **Federal States**	(5) **Government Regions**	(6) **Districts**
	Log Wage	Log Wage	Log Wage	Log Wage	Log Wage	Log Wage
Log Tightness	0.0127***	0.0101***	0.0144***	0.0121***	0.0123***	0.0103***
	(0.0017)	(0.0011)	(0.0010)	(0.0016)	(0.0011)	(0.0008)
Hire	-0.0384***	-0.0382***	-0.0379***	-0.0384***	-0.0383***	-0.0377***
	(0.0009)	(0.0006)	(0.0006)	(0.0009)	(0.0006)	(0.0005)
Age	0.0518***	0.0501***	0.0505***	0.0518***	0.0514***	0.0488***
	(0.0045)	(0.0049)	(0.0046)	(0.0046)	(0.0046)	(0.0054)
Age^2	-0.0005***	-0.0005***	-0.0005***	-0.0005***	-0.0005***	-0.0005***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Low Education	base	base	base	base	base	base
	-	-	-	-	-	-
Medium Education	0.1428***	0.1386***	0.1416***	0.1424***	0.1407***	0.1361***
	(0.0068)	(0.0061)	(0.0059)	(0.0077)	(0.0051)	(0.0040)
High Education	0.3790***	0.3727***	0.3764***	0.3791***	0.3756***	0.3649***
	(0.0125)	(0.0118)	(0.0114)	(0.0135)	(0.0102)	(0.0087)
Year FE	yes	yes	yes	yes	yes	yes
Worker FE	yes	yes	yes	yes	yes	yes
Labor Market FE	yes	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes	yes
Observations	8,591,598	8,131,060	8,616,432	8,552,284	8,471,619	7,692,410
Clusters	6,112	23,919	33,966	5,498	12,204	63,707
First-Stage Coefficient	0.9084***	0.8434***	0.8360***	0.8591***	0.9381***	0.9434***
First-Stage F-Statistic of Excluded Instrument	1,863	3,576	6,225	2,197	3,452	2,558

NOTE: The table displays IV regressions of log real daily wages of regular full-time workers on log labor market tightness. The instrumental variable refers to leave-one-out averages of labor market tightness in all other commuting zones but for the same occupation and time period. Control variables include binary variables for new hires, workplace location in Eastern Germany, three levels of professional education, and continuous variables for age and squared age. Labor markets are combinations of detailed 3-digit occupations and commuting zones. Standard errors (in parentheses) are clustered at the labor market level: *=p<0.10. **=p<0.05. ***=p<0.01. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Table 3.E5: Robustness Checks II

	(1) **Time-Constant Notification Share** Log Wage	(2) **Only Registered Vacancies** Log Wage	(3) **Trim at 5th and 95th Percentile** Log Wage	(4) **Sum-Based Instrument** Log Wage	(5) **Squared Estimation** Log Wage
Log Tightness	0.0120***	0.0119***	0.0145***	0.0107***	0.0122***
	(0.0013)	(0.0013)	(0.0013)	(0.0012)	(0.0013)
(Log Tightness)^2					0.0007
					(0.0004)
Hire	-0.0384***	-0.0384***	-0.0273***	-0.0384***	-0.0384***
	(0.0007)	(0.0007)	(0.0005)	(0.0007)	(0.0007)
Age	0.0525***	0.0525***	0.0482***	0.0525***	0.0525***
	(0.0047)	(0.0047)	(0.0039)	(0.0047)	(0.0047)
Age^2	-0.0005***	-0.0005***	-0.0005***	-0.0005***	-0.0005***
	(0.0000)	(0.0000)	(0.0000)	(0.0000)	(0.0000)
Low Education	base	base	base	base	base
	-	-	-	-	-
Medium Education	0.1411***	0.1411***	0.0362***	0.1412***	0.1413***
	(0.0061)	(0.0061)	(0.0021)	(0.0061)	(0.0061)
High Education	0.3770***	0.3770***	0.1253***	0.3769***	0.3771***
	(0.0115)	(0.0115)	(0.0030)	(0.0115)	(0.0115)
Year FE	yes	yes	yes	yes	yes
Worker FE	yes	yes	yes	yes	yes
Labor Market FE	yes	yes	yes	yes	yes
Firm FE	yes	yes	yes	yes	yes
Controls	yes	yes	yes	yes	yes
Observations	8,454,549	8,454,549	6,862,884	8,454,700	8,454,543
Clusters	13,692	13,692	12,581	13,699	13,692
First-Stage Coefficient (Log Tightness)	0.8777***	0.8805***	0.8127***	0.8979***	0.8823***
First-Stage F-Statistic of Excluded Instrument (Log Tightness)	2,276	2,305	2,595	3,624	1,380
First-Stage Coefficient (Log Tightness)^2					0.8065***
First-Stage F-Statistic of Excluded Instrument (Log Tightness)^2					1,020

Note: The table displays IV regressions of log real daily wages of regular full-time workers on log labor market tightness. The instrumental variable refers to leave-one-out averages of labor market tightness in all other commuting zones but for the same occupation and time period. Control variables include binary variables for new hires, workplace location in Eastern Germany, three levels of professional education, and continuous variables for age and squared age. Labor markets are combinations of detailed 3-digit occupations and commuting zones. Standard errors (in parentheses) are clustered at the labor market level: *=p<0.10. **=p<0.05. ***=p<0.01. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Table 3.E6: Heterogeneous Effects I

		(1) Log Wage		(2) Log Wage		(3) Log Wage
Log Tightness * ...	Hire	0.0158***	Helper	0.0128***	Low Education	0.0113***
		(0.0014)		(0.0018)		(0.0020)
Log Tightness * ...	Incumbent	0.0106***	Professional	0.0075***	Med. Education	0.0068***
		(0.0012)		(0.0015)		(0.0014)
Log Tightness * ...			Specialist	0.0273***	High Education	0.0262***
				(0.0020)		(0.0019)
Log Tightness * ...			Expert	0.0156***		
				(0.0020)		
Hire		-0.0349***		-0.0384***		-0.0383***
		(0.0007)		(0.0007)		(0.0007)
Age		0.0524***		0.0529***		0.0528***
		(0.0048)		(0.0048)		(0.0048)
Age^2		-0.0005***		-0.0005***		-0.0005***
		(0.0000)		(0.0000)		(0.0000)
Low Education		base		base		base
		-		-		-
Medium Education		0.1407***		0.1413***		0.1381***
		(0.0061)		(0.0061)		(0.0055)
High Education		0.3763***		0.3765***		0.3832***
		(0.0114)		(0.0115)		(0.0111)
Year FE		yes		yes		yes
Worker FE		yes		yes		yes
Labor Market FE		yes		yes		yes
Firm FE		yes		yes		yes
Controls		yes		yes		yes
Observations		8,454,541		8,454,541		8,454,541
Clusters		13,692		13,692		13,692

NOTE: The table displays IV regressions of log real daily wages of regular full-time workers on log labor market tightness. The instrumental variable refers to leave-one-out averages of labor market tightness in all other commuting zones but for the same occupation and time period. Control variables include binary variables for new hires, workplace location in Eastern Germany, three levels of professional education, and continuous variables for age and squared age. Labor markets are combinations of detailed 3-digit occupations and commuting zones. Standard errors (in parentheses) are clustered at the labor market level: * = p<0.10. ** = p<0.05. *** = p<0.01. *Sources:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Table 3.E7: Heterogeneous Effects II

		(1) Log Wage		(2) Log Wage		(3) Log Wage
Log Tightness * ...	Male	0.0121*** (0.0012)	Native	0.0109*** (0.0012)	Age <35	0.0097*** (0.0015)
Log Tightness * ...	Female	0.0090*** (0.0017)	Foreign	0.0153*** (0.0014)	Age 35-49	0.0095*** (0.0012)
Log Tightness * ...					Age >=50	0.0146*** (0.0012)
Hire		-0.0384*** (0.0007)		-0.0384*** (0.0007)		-0.0384*** (0.0007)
Age		0.0525*** (0.0047)		0.0525*** (0.0047)		0.0532*** (0.0049)
Age^2		-0.0005*** (0.0000)		-0.0005*** (0.0000)		-0.0005*** (0.0000)
Low Education		base -		base -		base -
Medium Education		0.1412*** (0.0061)		0.1410*** (0.0061)		0.1411*** (0.0061)
High Education		0.3770*** (0.0115)		0.3768*** (0.0115)		0.3767*** (0.0115)
Year FE		yes		yes		yes
Worker FE		yes		yes		yes
Labor Market FE		yes		yes		yes
Firm FE		yes		yes		yes
Controls		yes		yes		yes
Observations		8,454,541		8,454,541		8,454,541
Clusters		13,692		13,692		13,692

NOTE: The table displays IV regressions of log real daily wages of regular full-time workers on log labor market tightness. The instrumental variable refers to leave-one-out averages of labor market tightness in all other commuting zones but for the same occupation and time period. Control variables include binary variables for new hires, workplace location in Eastern Germany, three levels of professional education, and continuous variables for age and squared age. Labor markets are combinations of detailed 3-digit occupations and commuting zones. Standard errors (in parentheses) are clustered at the labor market level: *=p<0.10. **=p<0.05. ***=p<0.01. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Table 3.E8: Heterogeneous Effects III

		(1) Log Wage		(2) Log Wage
Log Tightness * ...	East	0.0353***	Manufacturing	0.0082***
		(0.0014)		(0.0012)
Log Tightness * ...	West	0.0072***	Services	0.0145***
		(0.0012)		(0.0015)
Hire		-0.0383***		-0.0383***
		(0.0007)		(0.0007)
Age		0.0525***		0.0525***
		(0.0046)		(0.0048)
Age^2		-0.0005***		-0.0005***
		(0.0000)		(0.0000)
Low Education		base		base
		-		-
Medium Education		0.1419***		0.1411***
		(0.0061)		(0.0061)
High Education		0.3781***		0.3768***
		(0.0115)		(0.0115)
Year FE		yes		yes
Worker FE		yes		yes
Labor Market FE		yes		yes
Firm FE		yes		yes
Controls		yes		yes
Observations		8,454,541		8,454,543
Clusters		13,692		13,692

NOTE: The table displays IV regressions of log real daily wages of regular full-time workers on log labor market tightness. The instrumental variable refers to leave-one-out averages of labor market tightness in all other commuting zones but for the same occupation and time period. Control variables include binary variables for new hires, workplace location in Eastern Germany, three levels of professional education, and continuous variables for age and squared age. Labor markets are combinations of detailed 3-digit occupations and commuting zones. Standard errors (in parentheses) are clustered at the labor market level: *=p<0.10. **=p<0.05. ***=p<0.01. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Table 3.E9: Heterogeneous Effects - First-Stage Regressions

		(1) Log Tightness
Z^1 * Hire	Coefficient	0.9280***
	F-Statistic of Excluded Instrument	3,332
Z^1 * Incumbent	Coefficient	0.9018***
	F-Statistic of Excluded Instrument	4,028
Z^1 * Helper	Coefficient	0.8637***
	F-Statistic of Excluded Instrument	347
Z^1 * Professional	Coefficient	0.8350***
	F-Statistic of Excluded Instrument	2,449
Z^1 * Specialist	Coefficient	0.8146***
	F-Statistic of Excluded Instrument	653
Z^1 * Expert	Coefficient	0.9328***
	F-Statistic of Excluded Instrument	357
Z^1 * Low Education	Coefficient	0.8696***
	F-Statistic of Excluded Instrument	5,353
Z^1 * Medium Education	Coefficient	0.8614***
	F-Statistic of Excluded Instrument	8,569
Z^1 * High Education	Coefficient	0.9138***
	F-Statistic of Excluded Instrument	2,586
Z^1 * Male	Coefficient	0.8925***
	F-Statistic of Excluded Instrument	8,270
Z^1 * Female	Coefficient	0.8622***
	F-Statistic of Excluded Instrument	7,391
Z^1 * Native	Coefficient	0.8862***
	F-Statistic of Excluded Instrument	10,447
Z^1 * Foreign	Coefficient	0.8844***
	F-Statistic of Excluded Instrument	8,677
Z^1 * Age <35	Coefficient	0.9390***
	F-Statistic of Excluded Instrument	5,977
Z^1 * Age 35-49	Coefficient	0.9297***
	F-Statistic of Excluded Instrument	4,191
Z^1 * Age >=50	Coefficient	0.9597***
	F-Statistic of Excluded Instrument	4,615
Z^1 * East	Coefficient	1.0428***
	F-Statistic of Excluded Instrument	3,318
Z^1 * West	Coefficient	0.8777***
	F-Statistic of Excluded Instrument	4,921
Z^1 * Manufacturing	Coefficient	0.9256***
	F-Statistic of Excluded Instrument	7,989
Z^1 * Services	Coefficient	0.8477***
	F-Statistic of Excluded Instrument	8,695

NOTE: The table displays estimated coefficients and F-statistics of the excluded instruments of the first-stage regressions of log labor market tightness on the leave-one-out averages of log labor market tightness in all other commuting zones but for the same occupation and time period (Z^1). The first stages are estimated separately for each subgroup. Standard errors (in parentheses) are clustered at the labor market level: *=p<0.10. **=p<0.05. ***=p<0.01. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Table 3.E10: Heterogeneous Effects along the Wage Distribution

	(1) **Worker Distribution** Log Wage	(2) **Firm Distribution** Log Wage
Log Tightness * Wage Decile Group 1	0.1190*** (0.0041)	0.0578*** (0.0020)
Log Tightness * Wage Decile Group 2	0.0476*** (0.0018)	0.0264*** (0.0017)
Log Tightness * Wage Decile Group 3	0.0289*** (0.0015)	0.0174*** (0.0015)
Log Tightness * Wage Decile Group 4	0.0165*** (0.0015)	0.0082*** (0.0016)
Log Tightness * Wage Decile Group 5	0.0062*** (0.0016)	0.0015 (0.0015)
Log Tightness * Wage Decile Group 6	0.0019 (0.0015)	0.0017 (0.0015)
Log Tightness * Wage Decile Group 7	0.0005 (0.0014)	0.0033** (0.0014)
Log Tightness * Wage Decile Group 8	0.0006 (0.0014)	0.0058*** (0.0014)
Log Tightness * Wage Decile Group 9	0.0002 (0.0014)	0.0067*** (0.0015)
Log Tightness * Wage Decile Group 10	-0.0231*** (0.0023)	0.0094*** (0.0024)
Hire	-0.0371*** (0.0007)	-0.0379*** (0.0007)
Age	0.0505*** (0.0044)	0.0523*** (0.0045)
Age^2	-0.0005*** (0.0000)	-0.0005*** (0.0000)
Low Education	base -	base -
Medium Education	0.1297*** (0.0059)	0.1391*** (0.0062)
High Education	0.3641*** (0.0110)	0.3760*** (0.0115)
Year FE	yes	yes
Worker FE	yes	yes
Labor Market FE	yes	yes
Firm FE	yes	yes
Controls	yes	yes
Observations	8,454,541	8,454,541
Clusters	13,692	13,692

NOTE: The table displays IV regressions of log real daily wages of regular full-time workers on log labor market tightness for ten decile groups along workers' wage distribution (Column 1) and firms' wage distribution (Column 2). Workers are assigned into ten decile groups based on their real daily gross wage when they first appear in the IEB as full-time workers during 2012-2022. Firms are assigned into ten decile groups based on the average (real daily) wage of their (full-time) workers when the firms first appear in the IEB sample during 2012-2022. Both estimations refer to the baseline specification with fixed effects for years, workers, labor markets, and firms as well as control variables. Standard errors (in parentheses) are clustered at the labor market level: *=p<0.10. **=p<0.05. ***=p<0.01. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Table 3.E11: Heterogeneous Effects along the Wage Distribution - First-Stage Regressions

		(1) **Worker Distribution** Log Tightness	(2) **Firm Distribution** Log Tightness
Z^1 * Decile Group 1	Coefficient	0.9163***	0.9287***
	F-Statistic of Excluded Instrument	1,975	1,827
Z^1 * Decile Group 2	Coefficient	0.9199***	0.9217***
	F-Statistic of Excluded Instrument	2,263	2,350
Z^1 * Decile Group 3	Coefficient	0.9210***	0.9184***
	F-Statistic of Excluded Instrument	2,300	2,339
Z^1 * Decile Group 4	Coefficient	0.9016***	0.8958***
	F-Statistic of Excluded Instrument	2,672	2,548
Z^1 * Decile Group 5	Coefficient	0.8811***	0.8794***
	F-Statistic of Excluded Instrument	2,942	3,034
Z^1 * Decile Group 6	Coefficient	0.8625***	0.8704***
	F-Statistic of Excluded Instrument	2,718	2,933
Z^1 * Decile Group 7	Coefficient	0.8517***	0.8590***
	F-Statistic of Excluded Instrument	2,253	2,253
Z^1 * Decile Group 8	Coefficient	0.8496***	0.8644***
	F-Statistic of Excluded Instrument	1,938	1,742
Z^1 * Decile Group 9	Coefficient	0.8526***	0.8619***
	F-Statistic of Excluded Instrument	1,333	1,107
Z^1 * Decile Group 10	Coefficient	0.8895***	0.8750***
	F-Statistic of Excluded Instrument	422	414

NOTE: The table displays estimated coefficients and F-statistics of the excluded instruments of the first-stage regressions of log labor market tightness on the leave-one-out averages of log labor market tightness in all other commuting zones but for the same occupation and time period (Z^1). The first stages are estimated separately for each wage decile group. Column 1 shows the first stage estimations for ten decile groups along the worker wage distribution. Column 2 shows the first-stage estimations for ten decile groups along the firm wage distribution. Workers are assigned into ten decile groups based on their real daily gross wage when they first appear in the IEB as full-time workers during 2012-2022. Firms are assigned into ten decile groups based on the average (real daily) wage of their (full-time) workers when the firms first appear in the IEB sample during 2012-2022. Standard errors (in parentheses) are clustered at the labor market level: *=p<0.10. **=p<0.05. ***=p<0.01. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

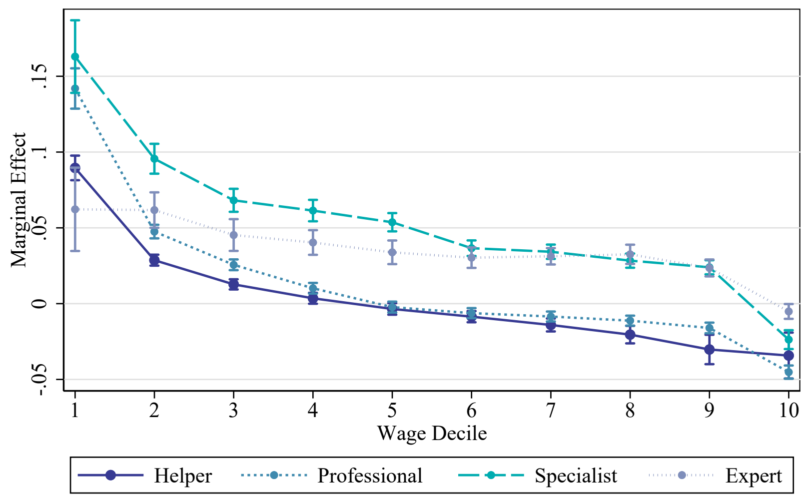
Table 3.E12: IV Regressions of Wage Setting at the Firm Level

	(1) Log Wage	(2) Log $\overline{\text{Wage}}^{\text{Firm}}$
Log Tightness	0.0024**	0.0088***
	(0.0011)	(0.0012)
Hire	-0.0497***	-0.0120***
	(0.0009)	(0.0003)
Age	0.0501***	0.0223***
	(0.0041)	(0.0013)
Age²	-0.0005***	-0.0002***
	(0.0000)	(0.0000)
Low Education	base	base
	-	-
Medium Education	0.1407***	0.0371***
	(0.0079)	(0.0016)
High Education	0.3843***	0.0705***
	(0.0143)	(0.0020)
Year FE		yes
Worker FE	yes	yes
Labor Market FE	yes	yes
Firm FE		yes
Firm-by-Year FE	yes	
Controls	yes	yes
Observations	6,424,250	8,454,541
Clusters	12,676	13,692
First-Stage Coefficient	0.8597***	0.8814***
First-Stage F-Statistic of Excluded Instrument	3,247	2,740

NOTE: The table displays IV regressions of log (average firm-level) real daily wages of regular full-time workers on log labor market tightness. The instrumental variable refers to leave-one-out averages of labor market tightness in all other commuting zones but for the same occupation and time period. Control variables include binary variables for new hires, workplace location in Eastern Germany, three levels of professional education, and continuous variables for age and squared age. Labor markets are combinations of detailed 3-digit occupations and commuting zones. Column (1) shows IV estimates when additionally controlling for firm-by-year fixed effects. Column (2) displays IV estimates when regressing the firm-level average of real daily wages on log labor market tightness. Standard errors (in parentheses) are clustered at the labor market level: *=p<0.10. **=p<0.05. ***=p<0.01. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

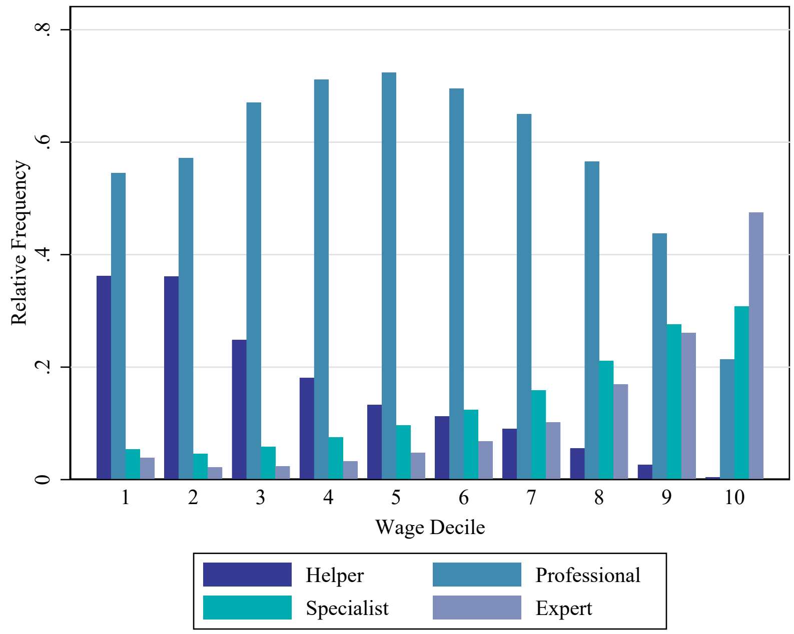
3.F Heterogeneous Effects along the Wage Distribution: Further Details

Figure 3.F1: Heterogeneous Effects along the Wage Distribution by Requirement Level



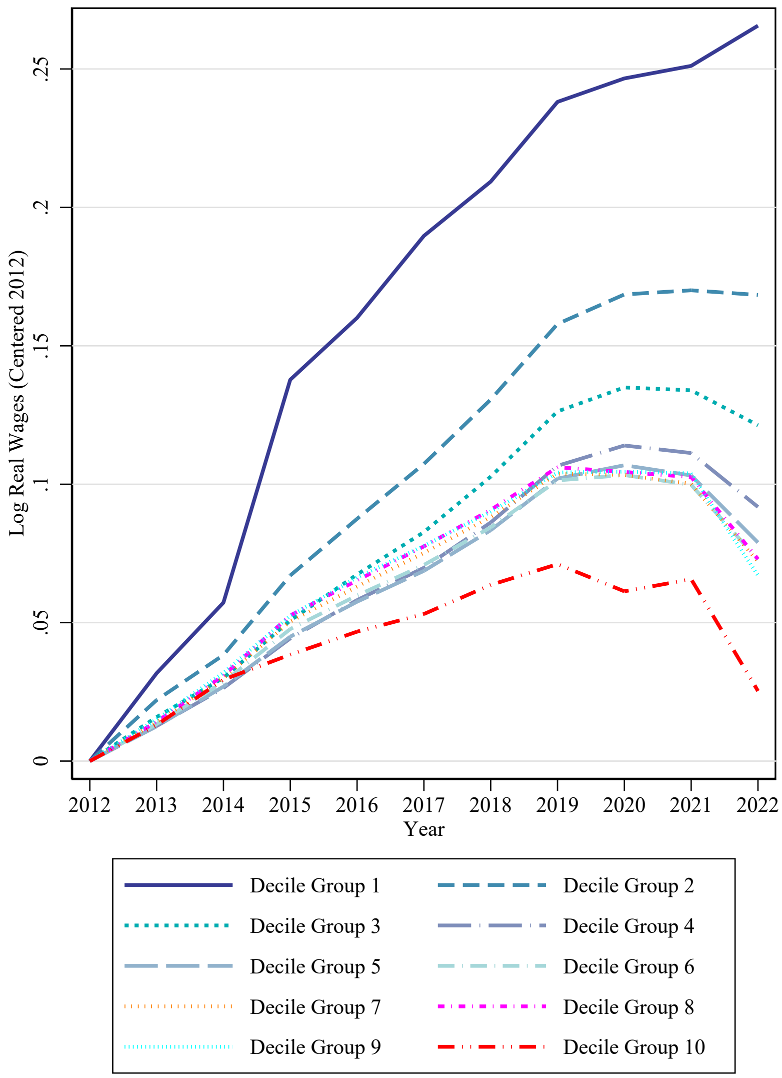
Note: The figure displays estimated elasticities and 95% confidence intervals from IV regressions of log real daily wages of regular full-time workers on log labor market tightness for ten decile groups along the worker wage distribution and for each of the four job requirement levels as stated in the legend. Workers are assigned into ten decile groups based on their real daily gross wage when they first appear in the IEB as full-time workers during 2012-2022. The curves refer to the baseline specification with fixed effects for years, workers, labor markets, and firms as well as control variables. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Figure 3.F2: Requirement Levels by Decile Groups



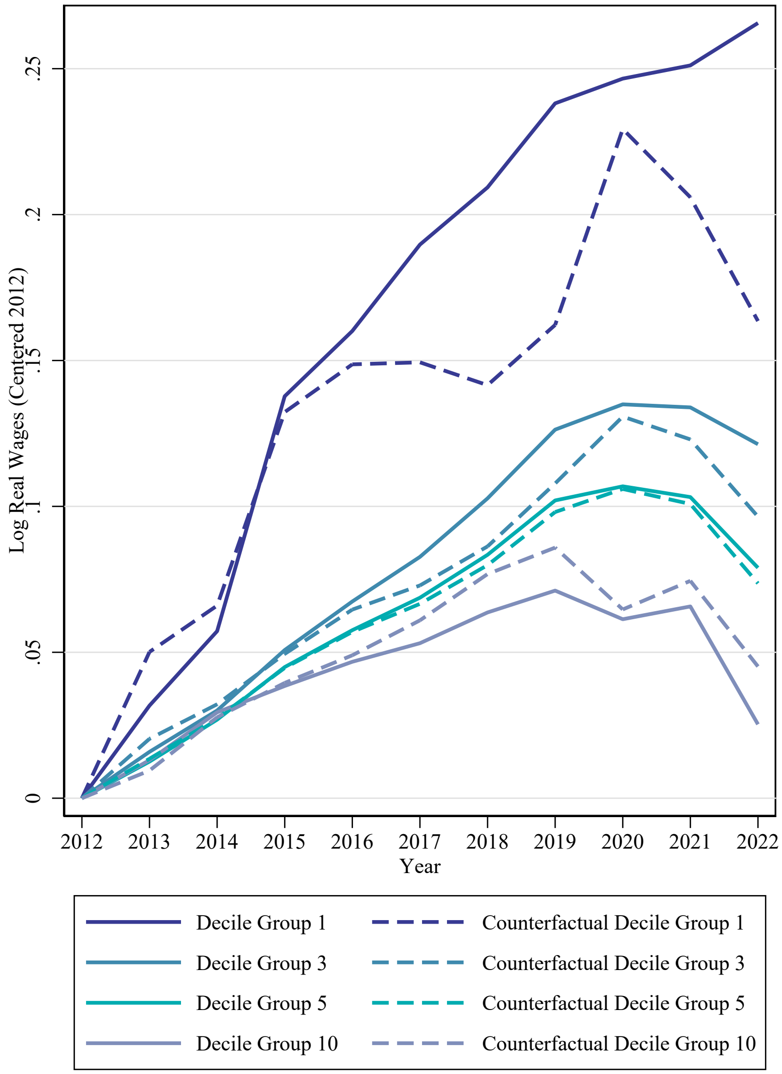
NOTE: The figure illustrates the share of the four different requirement levels in employment, separately by decile groups of workers' wage distribution. Requirement levels refer to the fifth digit of the KldB-2010 occupation variable. *Data Source:* Integrated Employment Biographies, 2012-2022

Figure 3.F3: Real Wage Growth By Decile Group



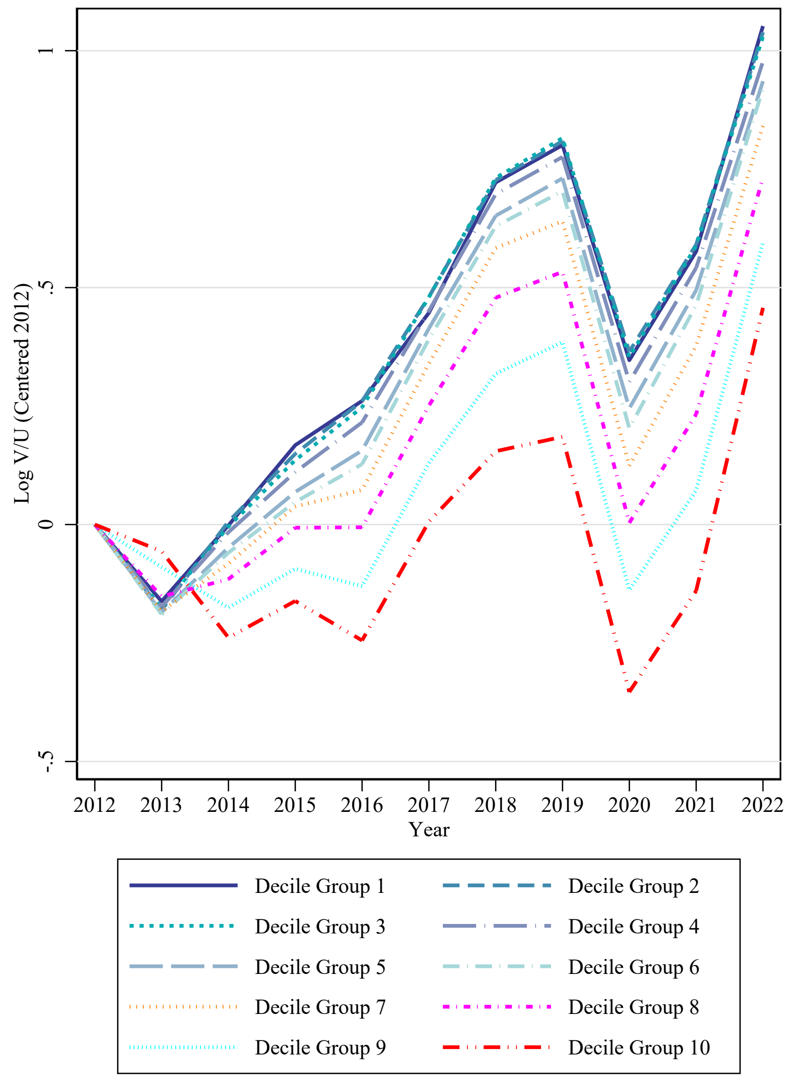
NOTE: The figure illustrates the average growth of real daily wages for ten decile groups of workers' wage distribution since 2012 (in percent). The respective time series of log real wages are centered at 2012 by subtracting the 2012 log real wage of the respective decile group *Data Source:* Integrated Employment Biographies, 2012-2022.

Figure 3.F4: Counterfactual Real Wage Growth in Absence of Tightness Increase



NOTE: The figure compares the wage growth in the first, second, third, and tenth decile group since 2012 with a counterfactual scenario in which labor market tightness is held constant. The solid lines illustrate average growth of real daily gross wages since 2012 (in percent). The dashed lines illustrate the counterfactual wage growth in the absence of a tightness increase. For each decile group, the counterfactual is calculated by subtracting the product of the wage elasticity with respect to tightness and the observed tightness increase (see Appendix Figure 3.F5) from observed values. *Data Source:* Integrated Employment Biographies + Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Figure 3.F5: Labor Market Tightness Growth by Decile Group



Note: The figure illustrates the average growth of labor market tightness for ten decile groups of workers' wage distribution since 2012 (in percent). The respective time series of log labor market tightness are centered at 2012 by subtracting the 2012 log tightness of the respective decile group *Data Source:* Official Statistics of the German Federal Employment Agency + IAB Job Vacancy Survey, 2012-2022.

Overall Conclusion and Outlook

The frictional nature of the labor market is well-established, and modern economic theory has evolved to account for these complexities. As a result, contemporary models can effectively replicate and explain real-world labor market phenomena, such as persistent unemployment and the coexistence of vacancies. However, as the different perspectives in the three essays of this dissertation demonstrate, the relationship between frictions and labor market outcomes is inherently multidimensional and bidirectional, influencing both market mechanisms and effects of labor market institutions, such as minimum wage policies. Various forms of frictions can trigger market adjustments, which in turn shape the evolution of labor market frictions themselves. Consequently, isolating specific cause-and-effect mechanisms and quantifying their causal impacts in ex post analyses primarily remain empirical questions.

This doctoral thesis provides valuable insights to better understand the complex interplay between wages and labor market frictions from three distinct perspectives. To achieve this, my co-authors and I exploit two major changes in the German labor market to empirically analyze three causal relationships. On the one hand, we leverage the introduction of the statutory minimum wage as a significant exogenous wage shock to assess its effects within a frictional market (Chapter 1) and on a specific form of labor market friction (Chapter 2). On the other hand, we leverage the substantial increase in labor market tightness as a hiring friction to explore its impact on market wages (Chapter 3).[104]

[104] Labor market tightness is defined as the ratio of job vacancies to job seekers.

In the following, I discuss the main findings of each essay, derive possible policy implications and suggest avenues for future research. Afterwards, I briefly discuss the role of high quality administrative and survey data for empirical research from the perspective of my dissertation. Finally, I summarise the implications derived from the results in the form of a condensed outlook on future research opportunities.

Labor Market Dynamics Affect Long-Term Effects of Policy Interventions

Chapter 1 (co-authored by Mario Bossler) of this dissertation proposes a novel method for estimating long-term effects of Germany's 2015 minimum wage introduction, accounting for dynamic selection between the groups of strongly treated and non-treated, or weakly treated individuals. This dynamic selection arises primarily from the frictional nature of the labor market, where wage shocks unrelated to the minimum wage can cause workers to move into the group of minimum wage workers, while others transition out of this group due to wage growth.

Our approach enables the reclassification of minimum wage exposure while accounting for wage dynamics unrelated to the minimum wage. The relevance of our method for minimum wage evaluation stems from relatively strong upward wage dynamics at the lower end of the wage distribution in Germany (Bossler and Schank, 2023; Naguib, 2022). As our results show, these dynamics significantly affect the composition of the treatment and control groups, particularly in the long term. While most existing studies focus on the short-term effects of the German minimum wage introduction, our approach is designed for long-term evaluation. The results of our machine learning-based prediction model indicate that, starting approximately two years after the initial treatment, the predicted bites capture minimum wage exposure with lower error rates compared to the conventional time-constant pre-treatment bites.

The applicability and added value of our method explicitly depend on the underlying research question, particularly regarding the target group for effect estimation. The effects estimated using our dynamic, predicted bite refer to the contemporaneously affected individuals in a given period. In other words, we estimate the treatment effect on the contemporaneously treated units, while conventional analyses estimate the treatment effect on the initially treated units. Our approach isolates the minimum wage effect on individuals affected at a point in time after the introduction of the minimum wage, whereas the conventional approach estimates the total effect on those initially affected at the time of introduction, which can also include wage dynamics unrelated to the minimum wage. Ultimately, it is up to the researcher to determine which method better aligns with their analytical objective or policy-relevant question.

Using a time-varying measure, the study finds statistically significant positive wage effects of the minimum wage introduction. However, these effects are smaller and remain relatively constant over time compared to estimates derived from conventional measures. The different effect sizes of our method and the conventional approach can be partly attributed to the distinctions outlined above. While our reclassification adjusts for wage dynamics unrelated to the minimum wage, the conventional time-constant pre-treatment bite measure may overestimate wage effects by failing to account for such unrelated wage developments.

The second reason for the smaller effect size lies in the inclusion of labor market entrants when predicting the bite. In the conventional approach, focusing on individuals initially affected by the minimum wage introduction leads to an increasingly selective treatment group over time, as labor market exits remove less stable employment relationships from the group. In contrast, our prediction model allows for including labor market entrants in each period. This dynamic reclassification avoids the distortion caused by excluding new entrants and enables to estimate the genuine effects of the minimum wage on a more unbiased group of affected individuals.

The biggest challenge of our proposed approach lies in selecting prediction variables that satisfy several necessary conditions and assumptions. First, these variables must not be influenced by the minimum wage introduction, as this would create an endogeneity problem that our LASSO estimation method cannot solve. For future research, it would be promising to combine prediction and causal parameter estimation in the same model using the advanced double machine learning technique, which can mitigate regularization bias and address endogeneity issues (Chernozhukov et al., 2018).

Second, we must assume that the predictive variables have the same influence on wage development – and thereby on the bite – after the minimum wage introduction as they did before, as our model coefficients are estimated based only on the pre-treatment period. Third, minimum wage-independent wage dynamics can only be captured in bite predictions if they are explainable through the selected set of predictive variables. Future research should substantially expand the set of predictive variables by incorporating individual- and establishment-specific information from various administrative and survey datasets. This would enhance the model's ability to explain wage dynamics and improve the accuracy and robustness of bite predictions.

Although this study focuses on the German minimum wage introduction, the issues addressed are relevant for empirical ex post evaluations of other policy interventions that affect units continuously and dynamically over time. In such cases, the analytical approach must align with the desired target group. If the objective is to estimate effects on contemporaneously affected individuals long after the policy's implementation and this group cannot be deterministically identified, a probability-based reclassification is required. This dissertation presents one possible approach; however, it is neither the only applicable method nor necessarily the most effective. Future research should explore alternative machine learning techniques, such as generalized random forests (Athey et al., 2019), to further improve predictive performance.

Overall, the results underscore the importance of accounting for labor market dynamics and the changing composition of affected and unaffected groups in long-term minimum wage evaluations. Specifically, short-term effects on workers initially affected by the policy cannot be extrapolated to capture long-term effects on those affected at later points in time. Changing labor market conditions in Germany, such as demographic shifts and increasing worker scarcity, further highlight the need for thorough long-term policy evaluations.

Wage Shocks Affect Search Fricitons

Although the effects of the German statutory minimum wage on equilibrium employment have generally been found to be small in most empirical ex post evaluations, Chapter 2 of this dissertation demonstrates that the policy intervention significantly impacted the unrealized side of labor demand – namely, vacancies. Motivated by previous findings of stricter hiring standards (Butschek, 2022; Clemens et al., 2021) and reduced occupational mobility (Liu, 2022) due to minimum wages, this study shows the effects of the minimum wage introduction on matching processes. Specifically, the analysis reveals that the policy intervention increased the share of canceled vacancies by 4–9 percent and extended the duration of successfully filled vacancies by approximately 5–6 percent in occupations with average exposure to the minimum wage. The existing literature on this channel of effect is limited. While Kudlyak et al. (2022) examine the effects of minimum wages on vacancy creation in the U.S., my study is the first to analyze both vacancy creation and, for the first time, vacancy durations in the context of a nationwide minimum wage policy, as seen in Germany.

The results have several important implications. First, even in the absence of equilibrium employment effects, behavioral adjustments by workers and firms influence the process of filling job vacancies, making the unrealized side of labor demand a meaningful adaptation channel for minimum wage policies. The increased share of canceled vacancies and the longer duration of filled vacancies suggest that the introduction of the minimum wage in Germany reduced matching efficiency by increasing search frictions and complicating the hiring process.

For future policy decisions on minimum wage adjustments, these results imply that a comprehensive minimum wage evaluation should not only focus on realized employment effects but also consider the impacts on the matching process. The minimum wage, as outlined in the German Minimum Wage Act, aims to provide adequate worker protection, promote fair competition, and protect employment. On the one hand, the minimum wage addresses market failures by reducing monopsonistic exploitation in highly concentrated labor markets, ensuring that workers receive wages closer to their marginal productivity (Azar et al., 2023; Popp, 2021). On the other hand, the minimum wage has reduced employers' willingness to make compromises in hiring processes, as shown in the complementary analysis using establishment survey data in Chapter 2. This could potentially decrease the employability of certain worker groups, particularly low-skilled workers (Clemens et al., 2021) and those in highly automatable jobs (Aaronson and Phelan, 2019; Lordan and Neumark, 2018). In future research on vacancy effects, these heterogeneous effects should be examined more thoroughly, possibly by classifying occupations based on their skill requirements and automation potential. This could reveal effect heterogeneities for low-skill vacancies in highly automatable occupations.

From the employer's perspective, the longer vacancy durations imply increasing hiring costs, in addition to the wage costs increases due to the minimum wage. Hence, the overall costs of the minimum wage for affected employers rise. At the same time, the analysis of worker transitions in this study points to more stable employment relationships, which could indicate higher matching quality (Dube et al., 2007; Dube et al., 2016) after the minimum wage introduction. For a comprehensive policy evaluation, these adverse effects on the labor demand side should also be taken into account.

Regarding the frictionality of the labor market, the results suggest that the prolongation of hiring processes in minimum wage occupations may have contributed to the observed increase in labor market tightness during the second half of the 2020s. However, this hypothesis remains for potential analysis in future research. In Chapter 3, we show that while labor market tightness for low-skill jobs is relatively low in absolute terms, it has still doubled in relative terms between 2010 and 2022.

Overall, the results highlight the duality and multidimensionality of the relationship between wages and frictions. The introduction of the minimum wage, which can also be viewed as an exogenous wage shock, has increased the frictionality of the matching process and may have contributed to the rise in labor market tightness. At the same time, existing studies suggest that minimum wages can reduce market imperfection by mitigating monopsonistic exploitation. This dual impact demonstrates that minimum wage policies can simultaneously reduce one form of market imperfection while potentially creating adverse effects on other imperfections, such as search and hiring frictions.

Hiring Frictions Affect Wage Dynamics

Chapter 3 (co-authored by Mario Bossler and Martin Popp) shifts the perspective to examine the reciprocal relationship between wages and frictions from the opposite side. Using labor market tightness, we demonstrate how this hiring friction affects market wages in Germany. While the previous chapters used the introduction of the minimum wage as an exogenous wage shock, we build on the popular leave-one-out instrumental variable strategy from Azar et al. (2022) to eliminate reverse causality between labor market tightness and wages. The results show that the doubling of labor market tightness between 2012 and 2022 in Germany contributed to wage growth, accounting for between 7 and 19 percent of the wage increases overall.

Moreover, our analysis of several subgroups reveals stronger wage elasticities for newly hired workers, high-skilled workers, the Eastern German labor market, and the service sector. Furthermore, we observe substantially stronger effects at the lower end of the wage distribution, primarily due to wage increases of low-paying firms.

The results imply that the widely discussed worker scarcity, which the German Minister for Economic Affairs has referred to as "Germany's biggest risk to growth" (Alkousaa, 2024), offers benefits from the perspective of employees. On the one hand, workers benefit from a wide range of employment opportunities and, on the other, from wage increases. From a social policy standpoint, the disproportionately strong effect for subgroups with lower average wages – particularly in Eastern Germany, the service sector, and the overall low-wage sector – can be considered a desirable outcome.

Since the beginning of 2023, the number of job vacancies has declined significantly, leading to a relaxation in labor market tightness, although it remains at a very high level (Gürtzgen et al., 2024). A recent study by the DIW estimates that in 2024, the opportunity costs of the skilled labor shortage will amount to 49 billion euros compared to the production potential of the German economy (Burstedde and Kolev-Schaefer, 2024). However, the skilled labor shortage should not be equated with labor market tightness, as the former results not only from an overall shortage of available workers but also from a skill mismatch between supply and demand. For future analyses, it would be useful to consider the extent of skill mismatch at the occupational level in addition to labor market tightness, in order to identify heterogeneous wage effects for occupations particularly affected by tightness and skill mismatches.

If the relaxation of labor market tightness continues and intensifies, the need for employers to compete for the few available workers through attractive wage offers and possible compromises on applicant quality will decrease. At the same time, the bargaining position of employees and trade unions in wage and collective bargaining negotiations may weaken. As a result, wage increases driven by labor market tightness could potentially slow down in the future.

Overall, Chapter 3 further emphasizes the multidimensional relationship between frictions and wages. While worker scarcity undoubtedly poses significant challenges for employers in the competition for limited labor, the hiring friction enhanced the bargaining position of workers. They benefited not only from a variety of job opportunities but also from wage growth. Therefore, while a frictional labor market may prevent the most economically efficient outcomes, it can simultaneously provide advantages for certain agents in the labor market.

The Role of High Quality Data

This dissertation relied heavily on the availability of large-scale administrative datasets and thus underscores the significant value of high-quality data for both predictions and causal analyses. Data from the German social security system and the Federal Employment Agency's vacancy records provide rich, longitudinal information, allowing insights into individual employment histories and firm- or occupation-level dynamics. These datasets enable precise tracking of variables such as wages, job transitions, and vacancy developments, which are crucial for evaluating long-term labor market policies.

However, this thesis also highlights areas where additional data would significantly enhance empirical analyses. For example, incorporating information on working hours would allow for precise hourly wage calculations, essential for evaluating minimum wage effects, particularly in light of recent incremental increases. High-quality survey data, such as the Job Vacancy Survey, offers complementary insights into establishment characteristics and recruitment processes. Combining both administrative data and survey information opens new possibilities for understanding labor market dynamics, enabling more comprehensive analyses of policies like the minimum wage.

Outlook

The findings of this dissertation underscore the complex interplay between wages and labor market frictions, highlighting both the relevance of this topic and the potential for future research. As labor markets develop dynamically depending on the prevailing market environment and business cycle fluctuations, understanding the role of frictions for employment dynamics and amplification of exogenous shocks remains highly relevant for policymakers and researchers alike. For instance, demographic shifts and technological advancements are reshaping labor supply and demand, potentially amplifying labor market tightness and its implications for wage dynamics. Future research could build on these findings to examine how these structural changes affect both equilibrium employment and wage inequality across diverse labor market segments.

Moreover, the introduction of minimum wages, a key policy instrument analyzed in this dissertation, provides fertile ground for further study. While this research explored both short- and long-term effects on wages and vacancies, future work could delve deeper into heterogeneous effects across skill groups, industries, and geographic regions. Additionally, the role of employer adjustments, such as automation and changes in hiring standards, warrants further exploration to understand their impact on labor market matching efficiency and worker mobility.

An essential avenue for future research lies in better integrating the role of institutional factors and frictions into dynamic labor market models. For instance, interactions between minimum wages, labor market tightness, and occupational mobility could be examined to shed light on broader macroeconomic outcomes.

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