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Chapter 1

Introduction

1.1 Motivation and background

Work is a central activity in most peoples' lives. Today, the majority of the working-age population participates in the labor market (Eurostat, 2013) and spends a considerable amount of their time on the job (OECD, 2015). For example, expected lifetime working hours for U.S. men born between 1950 and 1970 amount to around 74,000 to 83,000 hours (Hazan, 2009). Work also constitutes the primary source of income, accounting for around 60 to 80% (Gollin, 2002; Karabarbounis and Neiman, 2014), and significantly shapes individuals' life satisfaction beyond providing the means to consume goods and services (Layard, 2011, p. 67).

In light of this, the Great Recession of 2007–2009[1] has drastically illustrated the consequences of poor labor market performance for many societies around the world (see, for example, Elsby et al. (2011) or Bentolila et al. (2012) for assessments of the labor market crisis). Although labor market conditions have generally improved in the aftermath of this recession, recovery is still incomplete in most Western countries, with current unemployment rates exceeding pre-crisis levels and a significant amount of individuals, especially the low-skilled, being

[1] According to the National Bureau of Economic Research, the U.S. recession started after the economy's peak in December 2007 and ended in June 2009 (NBER, 2010). For the Euro Area, the Centre for Economic Policy Research dated the recession from the first quarter of 2008 to the second quarter of 2009 (CEPR, 2010), which was succeeded by another recession starting after the economy's peak in the third quarter of 2011 and ending in the first quarter of 2013 (CEPR, 2015).

trapped in long-term unemployment (OECD, 2015).

Persistent unemployment in the aftermath of recessions has been generally attributed to cyclical or structural explanations, and suitable macro policy responses differ substantially with regard to the causes of unemployment (Lazear, 2014). For the recession of 2007–2009, evidence, in particular from the U.S., suggests cyclical deficient labor demand to be the key explanation (see, among others, Lazear and Spletzer, 2012; Rothstein, 2015). Sahin et al. (2014), for example, provide evidence of cyclical mismatch at the industry and occupational level, i.e., misallocation of vacancies and job seekers across industries and occupations, which increased throughout the recession but decreased thereafter.

Independent of this current labor market crisis, however, substantial structural changes in firms' labor demand behavior have been well observed in many Western countries over the past decades. Technological change and continued globalization have substantially lowered firms' demand for routine tasks, which has decreased employment prospects for medium-skilled relative to high- and low-skilled workers and induced polarization of Western labor markets (Acemoglu and Autor, 2011; Goos et al., 2014).

Pecuniary and non-pecuniary consequences of unemployment can be numerous, and in some cases long-lasting. In addition to the (temporary) reduction in income and consumption, unemployment has been found to serve as a screening signal for firms, with longer spells of unemployment significantly lowering the likelihood of receiving job interview offers (Kroft et al., 2013; Eriksson and Rooth, 2014), and to lead to sustained earnings losses even after re-entering employment (Couch and Placzek, 2010). In addition, unemployment has been found to lower the physical (Sullivan and von Wachter, 2009) and mental health of the people concerned (Marcus, 2013) as well as to exert direct negative effects on wellbeing beyond the effect that stems from the loss of income (Winkelmann and Winkelmann, 1998; Kassenboehmer and Haisken-DeNew, 2009).

In addition to the consequences of job loss for the individuals concerned, unemployment also affects societies at large. High levels of unemployment have been shown to reduce aggregate output and income, create inequality and deteriorate societies' human capital (Layard et al., 2005). Moreover, unemployment has been shown to reduce nations' wellbeing to an extent beyond the fall in GDP and the increase in the number of the unemployed, which gives rise to the existence of psychic costs of recessions even for those people not subject to unemployment (Di Tella et al., 2003).

Against the backdrop of these substantial costs of unemployment, the recent recession has fueled discussions about the labor market effects of governmental policies regarding the unemployed, especially with respect to the design of the unemployment insurance (UI) system, which varies over the business cycle in some countries but remains constant in others (Schmieder et al., 2012). As a large literature has shown that more generous UI prolongs individuals' duration of nonemployment (see, for example, Card et al., 2007; Schmieder et al., 2012), debates about the optimal design of UI and suitable policy reforms to counteract moral hazard behavior continue to shape the scientific and public discussion.

The assertiveness of intended policy reforms may, however, be limited during times of high unemployment. Recessions have been shown to undermine peoples' trust in public institutions, which may constrain policy makers' abilities to enforce reforms (Stevenson and Wolfers, 2011). Societies' distrust in public institutions may thus lower the quality of the economic policies pursued (Easterly and Levine, 1997) and weaken democratic governance as a whole (Almond and Verba, 1963), but public institutions may also shape peoples' trust in turn (Alesina and Angeletos, 2005; Aghion et al., 2010). Irrespective of this reciprocal relationship, high levels of trust within a society reduce individuals' transaction costs and facilitate economic activity (Arrow, 1972; Knack, 2001), thus triggering positive and long-lasting effects on a country's economic performance (Knack and Keefer, 1997; Algan and Cahuc, 2010).

The importance of work in peoples' lives and the substantial costs of unemployment for the individuals concerned, but also societies at large, highlight the relevance of further improving the scientific understanding of the functioning of labor markets. Against this backdrop, the present dissertation aims at contributing to the understanding of central labor market mechanisms by analyzing open questions on (i) determinants of firms' labor demand, (ii) unemployed individuals' job search behavior and (iii) the state's role in shaping peoples' trust and, thereby, affecting labor market outcomes and economic performance in general.

1.2 Research questions and main findings

The following section describes and motivates each chapter of this dissertation in more detail, presents the respective empirical strategies chosen to answer the research questions raised and summarizes the main findings of each study.

Chapter 2: Heterogeneity in the Wage Elasticity of Labor Demand[2]

Chapter 2, a joint work with Andreas Peichl (ZEW Mannheim) and Sebastian Siegloch (University of Mannheim), contributes to the understanding of firms' labor demand behavior by rigorously investigating one key parameter of interest in labor economics as well as many other related disciplines, the own-wage elasticity of labor demand. Among others, firms' labor demand responses to wage changes have been shown to crucially influence the outcomes of labor market reforms (Hamermesh, 1993) as well as to point to structural changes in production arising from, for example, skill-biased technological change or globalization.

While the importance of this parameter is reflected by the large number of studies in the literature devoted to the estimation of labor demand elasticities, heterogeneity in the estimates as well as in researchers' beliefs about the size of this parameter (see Fuchs et al., 1998) is apparent. Against this backdrop, Chapter 2 conducts a comprehensive *meta-regression analysis* of the corresponding literature to investigate different dimensions of heterogeneity in this elasticity and thereby explain the diverse estimates of and beliefs about this crucial parameter.

Using different meta-regression techniques and information from 1,334 estimates of the elasticity obtained from 151 different micro-level studies, the results of this chapter demonstrate that there is no central, statistically defined elasticity of labor demand. Rather, heterogeneity in the estimates is natural to a considerable extent and can be explained by different theoretical concepts of the elasticity, heterogeneity in the characteristics of the workforce, differences between industries and countries, as well as changes in the labor demand behavior of firms over time. Researchers and policy makers should hence carefully acknowledge these dimensions of heterogeneity when evaluating the effects of intended reforms or

[2] This chapter has been published as "The Own-Wage Elasticity of Labor Demand: A Meta-Regression Analysis", see Lichter et al. (2015).

when calibrating models. The analysis, however, additionally shows that some part of the prevalent heterogeneity is also explained by the different empirical procedures applied or the different types of data used, implying undesired discretion for researchers to produce estimates in line with their assumptions. This potential problem is corroborated by evidence of considerable publication (or reporting) bias in the estimates, suggesting selection with respect to the empirical results that are reported or published.

Chapter 3: The Effects of Exporting on Labor Demand[3]

While Chapter 2 explores various sources of heterogeneity in the estimates of the own-wage elasticity of labor demand, Chapter 3, a joint work with Andreas Peichl (ZEW) and Sebastian Siegloch (University of Mannheim), adds to the literature by investigating the effects of exporting, one key feature of globalization, on the wage elasticity of labor demand. The analysis is motivated by growing concerns that rising trade volumes, despite being beneficial for societies at large, may have increased the responsiveness and vulnerability of employment to shocks (see, for example, Rodrik, 1997, for an early argument).

In light of this, Chapter 3 relies on detailed administrative linked employer-employee panel data to explore a long-known theoretical mechanism that may explain more elastic employment responses to wage shocks for exporting than non-exporting firms. In order to analyze the suggested mechanism, labor demand models are estimated by means of *fixed effects OLS* and *instrumental variables* techniques to capture time-invariant as well as time-variant plant characteristics that may affect both firms' selection into the export market as well as the extent of exporting.

The results of Chapter 3 provide new insights about the consequences of globalization for workers by providing empirical evidence that exporting, both at the extensive and intensive margin, renders firms' demand for labor more elastic. Building upon the theoretical model of Krishna et al. (2001) and recent evidence demonstrating that a country's product demand becomes less price elastic with rising per-capita income, it is shown that this finding can be explained by

[3] An earlier version of this chapter circulates as "Exporting and Labor Demand: Micro-Level Evidence from Germany", see Lichter et al. (2014).

one of the long-known Marshall-Hicks laws of derived demand: exporting firms in high-income countries are exposed to an overall more price elastic product demand than a comparable firm only serving its domestic market. This translates into more wage elastic demand for labor, unconditional on output. Using industry-level data on country-specific trade-flows, evidence for the proposed mechanism is corroborated by showing that those exporting firms selling a relatively large share of their output to low- and medium-income countries, thus serving markets with relatively price elastic product demand, react particularly elastically in their demand for labor with respect to changes in wages.

Chapter 4: Benefit Duration and Job Search[4]

Chapter 4, which is single-authored, takes account of the long-lasting consequences of unemployment by analyzing individuals' job search behavior in response to the generosity of unemployment insurance (UI). Although UI may allow individuals to actively search for suitable reemployment opportunities by temporarily compensating for income losses, a large literature has established that the extent of UI coverage significantly affects the duration of nonemployment and hence partly offsets the intended policy effects. While scholars have mainly attributed this effect to lower job search effort and a UI-induced moral hazard, evidence supporting the assumed relationship is particularly scarce.

Against this backdrop, the analysis detailed in Chapter 4 provides direct evidence of the effect of UI generosity on the job search behavior of unemployed individuals. In order to identify this direct effect, survey data from Germany is used that provide detailed information on the job search strategies of unemployed individuals at the beginning of their unemployment spell. Using quasi-experimental variation in the potential benefit duration for one particular group of workers arising from a unique legislative episode during the time of the survey period, *difference-in-differences* techniques are applied to identify potential disincentive effects of UI.

Overall, the results of the chapter extend the understanding of unemployed individuals' job search behavior. In line with the theoretical predictions of a non-

[4] This chapter is based on a (so far unpublished) manuscript titled "Benefit Duration and Job Search Effort: Evidence from a Natural Experiment", see Lichter (2015).

stationary job search model, the empirical results provide considerable evidence in favor of a UI-induced moral hazard. Increases in the potential benefit duration substantially lower individuals' job search effort, which is measured by means of the number of applications filed as well as the probability of applying for jobs that require moving. By providing direct evidence on disincentive effects of UI, the chapter thus complements evidence on prolonged spells of unemployment in response to more generous benefits by verifying one of the theoretically expected mechanisms explaining this finding. The results of the analysis prove robust to a variety of sensitivity checks and suggest the moral hazard to be particularly strong for those individuals who spent less time (or effort) on their education.

Chapter 5: The Economic Costs of Mass Surveillance[5]

Finally, Chapter 5 of this dissertation, a joint work with Max Löffler (ZEW Mannheim) and Sebastian Siegloch (University of Mannheim), turns to exploring the remarkable relationship between societies' level of trust and economic growth as well as the observed interaction effects between governmental policies, trust and economic performance. By investigating the effects of state surveillance, a feature of many (authoritarian) countries, on peoples' trust and regional economic performance in a single-country setting, this chapter significantly adds to the understanding of this important relationship.

To assess the long-term consequences of state surveillance, the analysis exploits county-level variation in the number of spies of the Ministry for State Security (*Stasi*) in the former socialist German Democratic Republic (East Germany) in combination with information on individuals' trust and economic performance in different regions after reunification. Potential non-randomness in the allocation of spies into counties is accounted for by implementing two distinct research designs. Different average levels in the intensity of spying across East German states (*Bezirke*) due to the territorial organizational structure of the state security service facilitate the use of a *border discontinuity design.* Harmonized county-level data for pre- and post-treatment years further enable the estimation of *fixed effects panel data* models (in the spirit of Moser et al., 2014), which allows controlling for

[5] An earlier version of this chapter circulates as "The Economic Costs of Mass Surveillance: Evidence from Stasi Spying in East Germany", see Lichter et al. (2015).

county fixed effects in the regressions.

The results provided in this chapter of the dissertation offer direct evidence on the substantial costs of state surveillance. It is shown that the extent of spying in socialist East Germany has had long-lasting negative consequences on peoples' trust in fellow citizens and public institutions even years after the end of the communist regime. The results of this analysis further highlight that diminished trust in turn impedes regions' economic performance, with self-employment rates and population growth, for example, being significantly lower in regions that were subject to high levels of state surveillance. Overall, these findings thus verify and extend cross-country evidence on the positive and long-run association between the quality of public institutions and economic growth by providing within-country evidence on governmental policies' effects on trust and, hence, on economic performance.

General findings of the dissertation.

The importance of work for today's societies and the particular role of public policy in shaping labor market outcomes and thereby peoples' trust, and vice versa, motivates the four quite different chapters of this dissertation. By contributing to the understanding of central labor market mechanisms, the present studies all highlight the importance of sound policy making and provide important implications thereof.

The findings of Chapter 2 illustrate that firms' labor demand behavior is significantly affected by labor market institutions, with the absolute value of the elasticity of labor demand being negatively correlated to the level of employment protection legislation in a given country. The results of Chapters 2 and 3 further point to important heterogeneities in firms' labor demand elasticities arising from differences in production technologies, workforce characteristics or the engagement in international trade, among others, which may significantly impact the effectiveness of labor market policies in turn (Hamermesh, 1993). Under these circumstances, the optimal design of labor market policies may thus differ with respect to the characteristics of a firm or an industry. With Lee and Saez (2012), for example, demonstrating that the optimal design of minimum wage policies depends on firms' labor demand elasticities, the results of Chapter 3 suggest different optimal minimum wage policies for trade-exposed and trade-sheltered sectors. In light of this, policy makers should thus take account of potential heterogeneities in firm responses to policy reforms in order to avoid unintended consequences of their interventions.

Chapter 4 provides exemplary evidence for unintended effects of labor market policies, with an extension of the potential benefit duration being found to reduce job search effort of the unemployed. As this finding can be interpreted as evidence in favor of a UI-induced moral hazard, the results of this chapter emphasize the need to carefully balance supportive and restrictive policies associated with the UI scheme. Evidence from Black et al. (2003), for example, stresses that mandatory participation in training systems may reduce UI-induced moral hazard and shorten nonemployment durations. Therefore, an extension of the potential benefit duration might be preferably combined with policies that suppress moral hazard. In addition to highlighting the direct (unintended) effects of labor market policies, the results of Chapter 5 of this dissertation further point to the overall importance of governmental policy for a nation's economic performance and wellbeing. State surveillance, a feature of many (authoritarian) countries, is found to erode peoples' trust in their fellow citizens and public institutions and, thereby, to hamper the performance of the labor market and suppresses economic growth.

While the results of this dissertation thus highlight that sound governmental policy may significantly improve the efficiency of the labor market and shape societies' economic wellbeing in general, it has been argued that any intended policy reform should be assessed against the constraints faced by politicians as well as the (long-term) political consequences of these actions (Dixit, 1997; Acemoglu and Robinson, 2013). If, for example, labor market policy reforms strengthen "the already dominant groups in society" (Acemoglu and Robinson, 2013, p. 175), unintended political consequences may emerge from these policies that may eventually outweigh the potential efficiency gains in the labor market. While such an analysis demands the foundation of a new conceptual framework (Acemoglu and Robinson, 2013), it appears that (labor) economists might gain important new insights by assessing labor market policies against this broader context in future research.

Chapter 2

Heterogeneity in the Wage Elasticity of Labor Demand*

2.1 Introduction

The own-wage elasticity of labor demand is a key parameter of interest in labor economics crucially influencing the effectiveness of many labor market policies (Hamermesh, 1993) and pointing to structural changes in production due to skill-biased technological or organizational change. It also plays a key role in many other fields besides labor economics. Firms' labor demand responses to wage rate changes have gained increasing attention in public finance, with own-wage elasticities of labor demand serving as an important input in optimal tax models of individuals and firms (Jacquet et al., 2012; Riedel, 2011) as well as determining the deadweight loss due to taxation. In international economics, the wage elasticity of labor demand serves as an important parameter in theoretical models of international trade (Rauch and Trindade, 2003) as well as when assessing the effects of globalization on the volatility of employment and wages (Rodrik, 1997). Moreover, estimates of wage elasticities of labor demand are used to calibrate macro and computable general equilibrium (CGE) models in various fields, typically using "guestimated" elasticities (Boeters and Savard, 2013).

* The following chapter has been published as "The Own-Wage Elasticity of Labor Demand: A Meta-Regression Analysis" (joined with Andreas Peichl and Sebastian Siegloch, see Lichter et al. (2015)).

The importance of this parameter is reflected by the enormous number of studies devoted to the estimation of firms' labor demand responses to wage changes. Despite extensive research, heterogeneity in the estimates of the own-wage elasticity of labor demand is apparent, with most estimates ranging between zero and minus one. Correspondingly, Fuchs et al. (1998) show that beliefs about the size of the own-wage elasticity are widely dispersed among economists. In this paper, we explore different sources of heterogeneity in the estimates by conducting a comprehensive meta-regression analysis of the relevant literature. Using information from a total of 151 micro-level studies and 1,334 elasticity estimates, we account for sources of heterogeneity due to the type of elasticity being estimated and the empirical specification being used.

Specifically, we test whether empirical findings back up theory: given different theoretical concepts of the labor demand elasticity, we expect some heterogeneity in the estimates. We investigate how much of this heterogeneity can be explained by the empirical specification of the labor demand model or by characteristics of the dataset. Moreover, we analyze whether the elasticity of labor demand differs for various types of workers, industries or countries and whether the elasticity has increased over time (for example, due to technological change or increasing globalization). In addition, we explicitly test for publication selection (or reporting) bias, given that journals' preference to publish statistically significant results (DeLong and Lang, 1992) and economists' strong beliefs in particular economic relationships might prompt researchers to report, and journals to publish, expected empirical results only (Card and Krueger, 1995; Franco et al., 2014). With respect to the own-wage elasticity of labor demand, there is unanimous belief in a negative relationship between real wages and labor demand, and thus, a negative own-wage elasticity. With his seminal contribution, Hamermesh (1993) has further shaped this belief by providing an interval, ranging from -0.15 to -0.75, of likely values for the constant-output elasticity of labor demand. In our study, we therefore explicitly test whether there is evidence of publication bias in this strand of the literature.

Our meta-regression analysis offers six key results. First, a considerable share of the variation in the estimates can be explained by the different concepts of elasticities applied: according to labor demand theory, we find that the elasticity of labor demand is smaller in the short than the intermediate and long run and that the total elasticity of demand – obtained from a structural model – exceeds the constant-output elasticity. Second, firms' responses to wage changes are dependent on worker characteristics, with the elasticity of labor demand being higher for low-skilled and atypical workers compared to the average worker. Third, we find sizeable differences in elasticity estimates across industries and countries, with labor demand being particularly elastic in countries with low levels of employment protection legislation. Fourth, labor demand has become more elastic over time, possibly due to technical progress and increased globalization. Thus, variation in the estimates of the labor demand elasticity is natural to a considerable extent. There is no central elasticity of labor demand; rather, researchers need to carefully assess which type of elasticity to estimate in a given context or adapt when calibrating a model.

However, differences in the estimates are (fifth) also due to differences regarding the empirical specification of the labor demand model and the type of data used: structural-form models better correspond to theory, while estimates based on industry-level data understate firms' labor demand responses to changes in the wage rate. Sixth, the results of our analysis also point to substantial upward publication (or reporting) bias, especially in reduced-form models.

The remainder of this paper is structured as follows. In Section 2.2, we explore various dimensions of heterogeneity in the estimates of the elasticity (2.2.1) and provide descriptive statistics for our meta data (2.2.2). In Section 2.3.1, we introduce our meta-regression model and the underlying estimation strategy. We present and discuss our results in Section 2.3.2, while investigating the presence of publication (or reporting) bias in Section 2.3.3. Section 2.4 concludes.

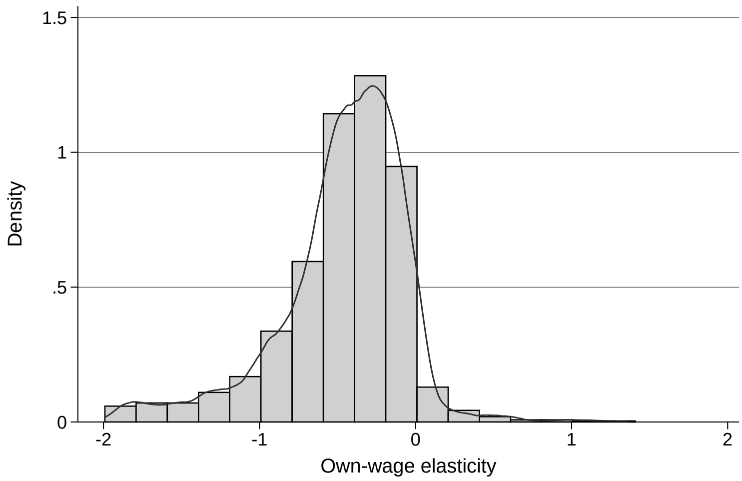
2.2 The meta sample & sources of heterogeneity

The data for our meta-analysis are collected by thoroughly examining the literature on labor demand and related topics.[1] Overall, we identify 151 studies that

[1] In detail, all studies included in our data are either listed in google scholar or given in the reference list of previously identified papers. In addition, we rely on the excellent survey of earlier empirical labor demand studies by Hamermesh (1993) to identify relevant studies published prior

provide micro-level estimates of the own-wage elasticity of labor demand. As most studies supply more than one elasticity estimate, the sample comprises those estimates that differ in an important source of heterogeneity only. Thus, we include all estimates from a particular study in case where they are derived from different specifications of the theoretical and empirical model, estimation procedures applied, or when they are worker-, industry-, time-, or country-specific. In contrast, if estimates only differ due to minor variations in the specification[2], the authors' preferred estimate is used. If there is no preferred estimate, we rely on the most comprehensive specification. Overall, this leaves us with 1,334 estimates of the own-wage elasticity. Tables 2.6.1 and 2.6.2 in the Appendix list the dimensions of heterogeneity and the particular source, i.e., the corresponding table or passage, for each estimate included in our meta-regression analysis.

Figure 2.2.1: Distribution of labor demand elasticities



Notes: The figure plots the distribution of estimates collected for the meta-regression analysis. For illustrative purposes, estimates exceeding the absolute value of 2 are excluded from this figure.

Figure (2.2.1) shows the distribution of labor demand elasticities in our data.[3] The mean (median) own-wage elasticity is -0.551 (-0.420), the standard deviation

to 1993.

[2] For example, due to the inclusion or exclusion of a specific control variable.

[3] For the sake of clarity, this graph does not include estimates of the own-wage elasticity of labor demand that exceed the value of two in absolute terms (N=55).

is 0.747 and the vast majority of estimates (83%) lies within the interval of minus one and zero.

2.2.1 Sources of heterogeneity

Given the widespread estimates, we identify likely sources of heterogeneity in the own-wage elasticity of labor demand: (i) labor demand theory, (ii) the empirical specification, (iii) the underlying data, (iv) characteristics of the workforce, and (v) variation across industries and countries as well as over time. Whereas (i) points to variation in the concept of the elasticity being estimated, (ii) and (iii) indicate different empirical strategies. Lastly, categories (iv) and (v) account for potential heterogeneity across groups and over time.

Labor demand theory. Heterogeneity in elasticity estimates is implied by theory. Firms' labor demand responses are more limited in the *short run* than in the *intermediate* and *long run*. In the short run, firms are assumed not to fully adjust the stock of labor employed when facing changes in the wage rate. For example, adjustment costs due to institutional regulations, such as employment protection legislation, limit firms' responses. In turn, firms are assumed to adjust the stock of labor and materials to the optimal level in the intermediate run, whereas the stock of capital remains fixed. Adjustments of the capital stock only occur in the long run. Limited flexibility in the adjustment of production inputs should thus translate into a lower own-wage elasticity of labor demand in the short run compared to the intermediate and long run.[4] Moreover, the *total* (unconditional) elasticity of labor demand should further exceed the *constant-output* (conditional) elasticity of labor demand. The conditional elasticity indicates the substitution effect between labor and other inputs of production at a given level of output and is determined by minimizing the costs of production conditional on output. The unconditional elasticity, in turn, reflects labor demand responses to wage rate changes where firms maximize profits and covers both the substitution and scale effect.

[4] For the purpose of our empirical analysis, we thus classify each estimate by means of the (dis)equilibrium state of labor and capital. Note that labor demand adjusts to the optimal level in a static labor demand model by definition, such that short-run labor demand can be only modeled in a dynamic model of labor demand.

The empirical specification. Differences regarding the empirical specification and identification of the labor demand model constitute another likely source of heterogeneity in the estimates of the labor demand elasticity.

Structural-form models usually apply the dual approach, minimizing costs conditional on output to derive labor demand functions.[5] Costs are specified by means of a linear second-order approximation to an arbitrary cost function in the following general form

$$C = C(\mathbf{w}, Y, Z),$$

with $\mathbf{w}$ denoting a vector of input prices of the production factors, Y denoting output, and Z capturing other variables affecting production, such as technological change over time or capital in case being specified as a quasi-fixed input factor reflecting an intermediate- rather than a long-run perspective, in which capital is a flexible input factor.[6] By minimizing costs and applying Shephard's Lemma, fully specified estimable factor demand equations are obtained

$$\mathbf{X} = f(\mathbf{w}, Y, Z).$$

Demand for input factor i thus depends on input prices, output, Z and the parameters of the assumed cost function. Own-wage elasticities can be calculated by using parameter estimates of the factor demand equations. Structural-form models thus provide an explicit framework to infer parameters of production that eventually determine the relevant elasticities of demand (Hamermesh, 1993, p.38).

Reduced-form models in turn lack a specific theoretical structure. Given firms' cost of production absent any specific functional form, $C(\mathbf{w}, Y)$, conditional factor demand equations can be derived by minimizing costs and applying Shephard's

[5] Less frequently, researchers also model complex production functions to obtain fully specified models of unconditional factor demand. See, for example, Kim (1988).

[6] Generalized Leontief, Translog and Box-Cox cost functions constitute the most common specifications in the literature, although many other specifications exist. See Diewert and Wales (1987) or Koebel et al. (2003) for details.

Lemma:

$$\mathbf{X} = \mathbf{X^d}(\mathbf{w}, Y).$$

Taking logarithms yields estimable log-linear specifications of factor demand, with the estimated coefficients of the factor prices representing the respective elasticities. Estimates of the total elasticity of labor demand are obtained when estimating the same factor demand specifications, but with the output variable dropped (Hamermesh, 1993, p.74). Due to lacking theoretical structure, reduced-form specifications of labor demand thus allow researchers considerable discretion regarding the inclusion of additional control variables in the empirical model.

Identification of both types of labor demand models often hinges on the assumption that wages are unaffected by demand and hence exogenously given to the individual firm. When relying on structural modeling, this problem is often assumed away, given that the theoretical model should stipulate the correct relationship between wages and employment.[7] In reduced-form models, however, endogeneity due to reverse causality/simultaneity is a first-order concern. Given the positive relationship between labor supply and wages, endogeneity would result in upward biased estimates of the own-wage elasticity of labor demand. In practice, many studies assume that wages are exogenous from the perspective of the individual employer (Hamermesh, 1993). While this assumption seems to be quite strong, it is less likely to hold when estimating labor demand at the industry level. Consequently, the validity of the wage exogeneity assumption is widely discussed in most current papers, and many attempts have been made to find instruments for the wage rate. However, credible instruments are still scarce. Often, researchers deal with endogeneity concerns in labor demand models by using lagged values of the wage rate as instruments. However, serious concerns have been raised about the validity of lagged endogenous variables as instruments (Angrist and Krueger, 2001, p.76f.). Due to the importance of addressing endogeneity concerns when estimating labor demand functions, we pay special attention to the wage treatment and the exogeneity assumption when running our meta analysis.

[7] Note that this assumption may be justified on theoretical grounds but may still lead to biased estimates when bringing the model to the data.

The dataset. Precise information on wages (and employment) is essential when estimating the elasticity of labor demand. In contrast to survey data, measurement error in wages is minimized when using information from *administrative* sources. Different sources of data may thus add to the heterogeneity in the estimates of the own-wage elasticity. Heterogeneity may likewise arise from differences in the level of observation. In his seminal work, Hamermesh (1993) reasons that *industry-level data* estimates of the own-wage elasticity cannot account for employment shifts within a given sector/industry and hence understate firms' employment responses to changes in wages. Studies using industry-level data are hence expected to provide downward biased estimates. Lastly, unobservable heterogeneity across firms (such as productivity differences) may affect employment, wages and hence the elasticity of labor demand. By relying on *panel* rather than *time-series* or *cross-sectional* data, researchers can easily account for unobservable firm- or industry-fixed effects and thus a potential form of bias in the estimates of the parameter of interest.

Workforce characteristics. Labor is not a homogenous production factor, and we expect labor demand elasticities to vary by worker types. For example, it is generally believed that firms' demand for low-skilled labor is more responsive to changes in the wage rate than the demand for medium- or high-skilled workers, given that low-skilled tasks may be more easily executed by machines or outsourced to low-income countries. In our meta-regression, we thus differentiate among *low-skilled*, *high-skilled* and *overall labor demand*.[8] We also distinguish the average worker from workers in *blue- or white-collar occupations*. Likewise, we test whether firms' demand for *female* labor and workers on *atypical contracts* is more elastic than for the average worker.

Variation across industries, countries and over time. Sectoral differences in labor demand are likely to contribute to the heterogeneity of own-wage elasticity estimates, given that some sectors are more dependent on domestic labor than

[8] We use overall demand as a category due to the fact that many studies do not account for heterogeneous types of labor and obtain elasticities for the overall workforce. Differences in the own-wage elasticity for low- and high-skilled labor are thus relative to the overall workforce, which represents medium-skilled workers on average.

others, e.g., due to differences in the capital to labor ratio or divergent opportunities to outsource parts of the production process. We therefore account for *sectoral differences* in the elasticity up to the 2-digit level.[9] *Cross-country differences* in institutional regulations regarding employment protection and dismissal may further crucially affect firms' labor demand behavior in response to changes in the wage rate. Moreover, the acceleration of international production sharing, global competition and technological advances may have rendered firms' demand for labor more elastic over time. Controlling for the *study's year of publication* to account for methodological advances in the literature, we analyze whether the magnitude of the elasticity of labor demand increases with the *mean year of observation* covered in the respective dataset.

Additional sources of heterogeneity. We stress that there are more dimensions of heterogeneity worth exploring: the presence of collective bargaining agreements at the firm or industry level may limit firms' employment responses but may also lead to wage moderation. Accordingly, as multinational firms are assumed to relocate production processes at lower costs, they may respond differently to changes in the wage rate compared to domestic firms. However, due to a limited number of studies explicitly distinguishing unionized from non-unionized and multinational from domestic firms, we have to discard these likely source of heterogeneity from our analysis. In addition, we do not explicitly control for firm size in this analysis. As the assignment mechanism of firms into different size classes is study-specific and the number of studies accounting for firm size is small, creation of non-overlapping and sizeable groups in our meta-analysis is unfeasible.

2.2.2 Descriptive statistics

Table 2.2.1 provides descriptive statistics of the explanatory variables used in the meta-regression.[10] We differentiate between two samples: the full sample covers all estimates obtained from the literature (N=1,334), whereas the baseline sample is

[9] Note that many studies focus on one-digit sectors or do not account for sectoral differences at all. Thus, we control for sectoral differences with respect to the overall economy.

[10] Tables 2.6.3 and 2.6.4 provide the characteristics of the explanatory variables for each paper included in the meta-regressions.

restricted to those estimates with a given or calculable standard error (N=890).[11]

With respect to theory, we first note that around 80% of the estimates refer to the intermediate or long run. Moreover, estimates of the constant-output elasticity of labor demand outnumber those of the total demand elasticity, indicating the literature's focus on the identification of long-run patterns of factor substitutability. Turning to the empirical specification, the majority of estimates come from reduced-form models of labor demand. Given that structural-form models account for the conceptual differences between the conditional and unconditional elasticity more explicitly, we allow for interdependencies between the empirical and theoretical specifications in our meta-regression analysis by interacting the latter variables. In terms of identification, most studies rely on the assumption that wages are exogenous to the firm or industry, with less than one-fifth of the estimated elasticities stemming from specifications where the wage variable has been instrumented.

Regarding the data, we observe that more elasticities are estimated using administrative rather than survey data and using variation at the industry level rather than at the firm level. Indeed, industry-level estimates are very rarely based on survey data. In our analysis, we account for this fact by including an interaction term of the data source and the unit of observation. Furthermore, panel data estimates constitute more than three-quarters of all elasticities in our analysis, with the majority stemming from specifications that account for unit-fixed effects.

The studies covered in our meta sample also account for a variety of worker characteristics: in terms of skills, 6.1% and 10.2% of the elasticity estimates in our baseline sample explicitly refer to high- and low-skilled labor, respectively. Likewise, explicit elasticities are given for blue- and white-collar workers, females and employees on atypical contracts. Moreover, it is apparent that the majority of studies has focused on the manufacturing sector, while rather few estimates refer to the service or construction sectors. Around one-third of the estimates apply to the overall economy.

[11] For the meta-analysis conducted below, standard errors are necessary to account for heteroscedasticity by applying Weighted Least Squares (WLS), using the inverse of the error term variances as the corresponding weights.

Table 2.2.1: Explanatory variables for heterogeneity in labor demand elasticities

	Baseline Sample		Full Sample	
Explanatory variable	Mean	Std. Deviation	Mean	Std. Deviation
Specification				
Time period				
Short-run elasticity	0.197	0.398	0.163	0.369
Intermediate-run elasticity	0.454	0.498	0.372	0.484
Long-run elasticity	0.349	0.477	0.465	0.499
Total demand elasticity (opposed to: constant-output elasticity)	0.211	0.408	0.156	0.363
Structural-form model (opposed to: reduced-form model)	0.372	0.484	0.475	0.500
Instrumenting wages (opposed to: exogenous wage)	0.161	0.367	0.177	0.382
Dataset				
Administrative data (opposed to: survey data)	0.784	0.412	0.812	0.391
Industry-level data (opposed to: firm-level data)	0.626	0.484	0.695	0.461
Panel data specification				
No panel data	0.165	0.372	0.275	0.447
Panel data/No fixed effects	0.116	0.320	0.113	0.317
Panel data/Fixed effects	0.719	0.450	0.612	0.488
Workforce characteristics				
Skill level				
All workers	0.837	0.370	0.854	0.353
High-skilled workers	0.061	0.239	0.055	0.228
Low-skilled workers	0.102	0.303	0.091	0.288
Female worker	0.033	0.178	0.022	0.146
Atypical employment	0.065	0.247	0.044	0.206
Worker type				
All workers	0.899	0.302	0.921	0.269
Blue-collar workers	0.062	0.241	0.047	0.212
White-collar workers	0.039	0.194	0.032	0.175
Industry (One-digit level)				
All	0.341	0.474	0.311	0.463
Manufacturing	0.544	0.498	0.596	0.491
Service	0.045	0.207	0.035	0.184
Construction	0.058	0.235	0.039	0.194
Other (Mining, Wholesale, Transportation, Electricity & Water)	0.012	0.110	0.019	0.135
Country (Aggregated)				
Continental European countries	0.299	0.458	0.253	0.435
Northern European countries	0.030	0.172	0.062	0.240
United Kingdom/Ireland	0.070	0.255	0.053	0.223
Southern European countries	0.023	0.148	0.030	0.171
USA/Canada	0.175	0.380	0.245	0.430
Asia	0.027	0.162	0.029	0.166
Latin America	0.070	0.255	0.062	0.242
Eastern European countries	0.101	0.302	0.070	0.256
Africa	0.029	0.168	0.021	0.143
Aggregate data	0.176	0.381	0.175	0.380
Mean year of observation	1989	9.7	1985	12.8
Mean year of publication	2002	7.6	2000	9.8

Notes: The baseline sample covers 890 observations and includes all point estimates with a given or calculable standard error. The full sample (N = 1,334) further includes all point estimates without a given or computable standard error.

Our meta data includes estimates of the wage elasticity of labor demand for 37 countries as well as estimates based on aggregate OECD or European data.[12] To simplify representation, mean values and standard deviations are given at an aggregate level in Table 2.2.1, with countries clustered by geographical location.[13] We note that a large share of estimates relate to Continental European countries[14] as well as the US and Canada, amounting to about 50% of the total estimates. By contrast, only a few elasticity estimates are given for Southern European, African or Asian countries. Lastly, we emphasize that the meta data cover studies published over more than four decades from 1971 to 2012.[15] The mean year of data in the respective studies is 1989 in the baseline and 1985 in the full sample.

2.3 Meta-regression analysis

Having identified likely sources of heterogeneity, we next turn to our meta-regression analysis. In Section 2.3.1, we briefly present the meta-regression model and estimation techniques. Section 2.3.2 presents the results, discusses the identified dimensions of heterogeneity and checks the sensitivity of our results. We subsequently test for the presence of publication selection bias in Section 2.3.3.

2.3.1 The regression model

In line with standard meta-regression analysis techniques (e.g., Card et al., 2010; Feld and Heckemeyer, 2011), we assume that the i^{th} estimate of the own-wage elasticity collected from study s, denoted η_{is}, is obtained by means of an econometric procedure such that the estimate of the elasticity varies around its true value (η_0) due to sampling error (ϵ_{is}) and is driven by study- ($\mathbf{X}'$) and estimate-specific ($\mathbf{Z}'$) effects, as introduced in the previous section. The regression model

12 Table 2.5.2 provides the number of estimates obtained for each country.

13 Precisely, we group elasticities for Germany, France, Belgium, the Netherlands and Luxembourg to Continental Europe, whereas Denmark, Norway, Finland and Sweden constitute the Nordic European countries. We further combine the estimates from Italy, Spain and Portugal to Southern Europe and group elasticities from Turkey, Macedonia and the former CIS states to Eastern Europe.

14 Here, the share of elasticities based on German data is particularly high.

15 Table 2.5.2 provides the year of publication for the studies covered in the meta data.

thus reads as follows:

$$\eta_{is} = \eta_0 + \boldsymbol{\beta}\mathbf{X'}_i + \boldsymbol{\delta}\mathbf{Z'}_{is} + \epsilon_{is}. \tag{2.3.1}$$

We account for heteroscedasticity in the meta-regression model in the estimation: the variance of the individual estimate of the elasticity (η_{is}) decreases with the size of the underlying sample, which differs between studies and/or within a single study in our sample: $V(\epsilon_{is}|\mathbf{X'}_i, \mathbf{Z'}_{is}) = \sigma^2_{\epsilon_{is}}$. The specific form of heteroscedasticity is given by the standard error of the estimate and is thus known when applying meta-regression techniques. As a consequence, we estimate equation (2.3.1) by WLS, using the inverse of the error term variances, i.e., the inverse of the squared standard error of the parameter estimate.[16] To control for study dependence in the estimates, standard errors are clustered at the study-level. In order to provide evidence for the robustness of our results, we also estimate our model for the full sample (including those elasticities without a standard error) by simple OLS, using the inverse of the number of observations taken per study as the corresponding weight.[17]

2.3.2 Results

The baseline results of our meta-regression analysis are presented in Table 2.3.1. We begin by separately analyzing the effects of different dimensions of heterogeneity on the own-wage elasticity of labor demand: namely, (i) the theoretical and empirical specification, (ii) characteristics of the dataset applied, and (iii) features of the workforce (columns (1) to (3)). Subsequently, we simultaneously account for all dimensions of heterogeneity in one model (column (4)) and additionally control for variation across industries and countries as well as over time in our most comprehensive specification (column (5)).

Column (1) shows that the empirical evidence backs theory: firms' labor demand responses to changes in the wage rate are more elastic in the intermediate and long run than in the short run since costs prevent firms from immediate adjustments to the optimal level of employment. However, intermediate- and long-run

[16] Stanley and Doucouliagos (2015) show that this estimator is preferable to other standard meta-regression estimators. Nonetheless, we test the sensitivity of our results by applying different estimators used in meta-studies below (see Table 2.3.2).

[17] See Tables 2.6.1 and 2.6.2 for the number of estimates taken per study.

elasticities are quite similar in magnitude. Our results further show that the total (unconditional) elasticity of labor demand exceeds the constant-output elasticity in absolute terms when being derived from a structural-form model of labor demand. In contrast, estimates of the total and constant-output elasticity of labor demand do not differ when being obtained from reduced-form models. Estimates from structural-form models thus tend to better comply with theory. As detailed in Section 2.2.1, a possible explanation for this finding lies in the empirical specifications of both models. Whereas structural-form estimates for unconditional and conditional elasticities are based on differing functional forms, reduced-form specifications of labor demand merely incorporate an additional control variable to capture firms' output when conditional rather than unconditional elasticities shall be obtained. As concerns the heterogeneity due to differing assumptions regarding the identification of the labor demand model, we find no statistically significant differences in the estimates with respect to the two polar assumptions about wage exogeneity. The results suggest, however, that estimates from specifications with instrumented wage variables exceed those estimates where wages are assumed to be exogenous.

We next investigate whether heterogeneity in the estimates of the elasticity of labor demand is data driven. The results displayed in column (2) suggest that the characteristics of the dataset add little to the heterogeneity in the estimates. However, data-driven heterogeneity becomes more important when controlling for the year of publication (see column (5)) since detailed firm-level data from administrative sources have only become available in recent years.

In line with our expectations, characteristics of the workforce are important determinants for the heterogeneity in the estimates. The results given in column (3) show that demand for high-skilled (low-skilled) workers is less (more) elastic than for the overall workforce. For low-skilled workers, more elastic demand may, for example, reflect higher substitutability of low-skilled tasks with capital as well as increasing possibilities of offshoring these tasks. In addition, demand for females and workers on atypical contracts is also more price elastic. For the latter group, one potential explanation is found in lower firing costs for the marginal and temporary employed. When controlling for worker characteristics only, we further note that estimates of the elasticity for both blue- and white-collar workers exceed

Table 2.3.1: Baseline results: meta-regression analysis

Dependent variable: Labor Demand Elasticity (η)	(1)	(2)	(3)	(4)	(5)
Specification					
Time period (omitted: Short-run)					
Intermediate-run	-0.243***			-0.139***	-0.114**
	(0.084)			(0.052)	(0.045)
Long-run	-0.302***			-0.150***	-0.151***
	(0.058)			(0.041)	(0.046)
Labor demand model (omitted: Conditional/Reduced-form)					
Conditional/Structural-form	0.203***			0.022	-0.049
	(0.075)			(0.055)	(0.070)
Unconditional/Reduced-form	0.009			-0.028	-0.009
	(0.054)			(0.052)	(0.027)
Unconditional/Structural-form	-0.123**			-0.389***	-0.150
	(0.053)			(0.078)	(0.103)
Instrumenting wages	-0.113			-0.117*	0.008
	(0.077)			(0.064)	(0.013)
Dataset					
Panel data specification (omitted: No panel data)					
Panel data/No unit-fixed effects		0.083		-0.060	-0.266**
		(0.086)		(0.064)	(0.123)
Panel data/Unit-fixed effects		-0.012		-0.144**	-0.249**
		(0.042)		(0.058)	(0.121)
Industry-level data		0.037		-0.075	-0.067
		(0.088)		(0.074)	(0.081)
Administrative data		0.267***		0.113***	-0.116
		(0.065)		(0.039)	(0.114)
Industry-level, admin data		-0.128		-0.020	0.255*
		(0.092)		(0.074)	(0.148)
Workforce characteristics					
Skill level (omitted: All workers)					
High-skilled workers			0.320***	0.162**	0.044
			(0.080)	(0.070)	(0.079)
Low-skilled workers			-0.409***	-0.271***	-0.213***
			(0.032)	(0.041)	(0.035)
Demand for female workers			-0.118***	-0.118***	-0.174***
			(0.042)	(0.045)	(0.031)
Atypical employment			-0.745***	-0.614***	-0.539***
			(0.038)	(0.055)	(0.046)
Worker characteristics (omitted: All workers)					
Blue-collar workers			-0.420***	-0.333***	-0.075
			(0.035)	(0.068)	(0.054)
White-collar workers			-0.314***	-0.238***	-0.062
			(0.076)	(0.051)	(0.056)
Estimates' mean year of observation (centralized)					-0.008*
					(0.004)
Constant	-0.077***	-0.287***	-0.094***	0.019	-0.354*
	(0.028)	(0.072)	(0.023)	(0.065)	(0.193)
Industry dummy variables	No	No	No	No	Yes
Year of publication dummy variables	No	No	No	No	Yes
Country dummy variables	No	No	No	No	Yes
No. of observations	890	890	890	890	890
Adjusted R-Squared	0.366	0.227	0.455	0.636	0.850

Notes: The table shows estimates based on regression model (2.3.1) for the baseline sample. Columns (1) through (5) are estimated by Weighted Least Squares using squared inverse standard errors of the estimates as weights. Standard errors (in parentheses) are clustered at the study level. Significance levels are 0.1 (*), 0.05 (**) and 0.01 (***).

the estimates for the overall workforce (not differentiating by collar type).[18]

We next include all three dimensions of heterogeneity in one regression. The results given in column (4) show that most of the previous findings prevail. Thus, we further add industry and country dummy variables to our regression in column (5), given that industries differ in terms of labor intensity and cross-national differences in labor market institutions are likely to affect firms' labor demand behavior. Moreover, we analyze whether labor demand has become more elastic over time. To identify potential shifts in the own-wage elasticity of labor demand over recent decades, we control for both the mean year of observation underlying the particular point estimate and the study's year of publication to capture methodological advances. Again, the results only slightly change: empirical evidence backs theory as firms' labor demand responses to changes in the wage rate are more limited in the short run compared to the intermediate or long run. Moreover, we offer clear evidence that demand for low-skilled and atypical workers is more elastic than for the overall workforce. However, our results also point to data-driven heterogeneity, given that industry-level estimates from administrative data sources are particularly small in absolute terms. This finding is in line with Hamermesh (1993), who argues that industry-level estimates understate firms' employment responses to changes in wages since intra-industry shifts in employment are not accounted for.

The regression results further show that labor demand elasticities vary considerably by industry.[19] Figure 2.3.1 plots differences in the industry-specific own-wage elasticity with respect to the elasticity for all sectors.[20] The graph shows that the elasticity of labor demand is significantly larger in the construction sector (F), overall manufacturing (C), and for manufactures of basic metals (ISIC 24) and metal products (ISIC 25) - two industries that are particularly labor intensive and where production has shifted to low-wage countries in recent decades.

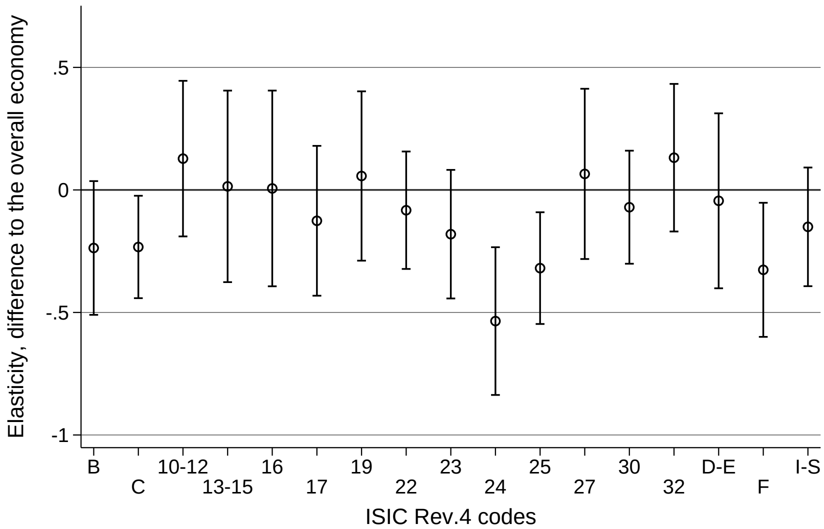
Due to advances in technology and increasing globalization, it is widely believed that labor demand has become more elastic over time. Our meta-regression ana-

[18] While this finding is rather unexpected, we stress that the difference in the elasticity for white-collar workers and the average worker vanishes when controlling for the study's year of publication.

[19] The corresponding regression results are available upon request.

[20] For the sake of clarity, this graph only displays the difference in the own-wage elasticity only for those industries in which more than two estimates were given from at least two different studies.

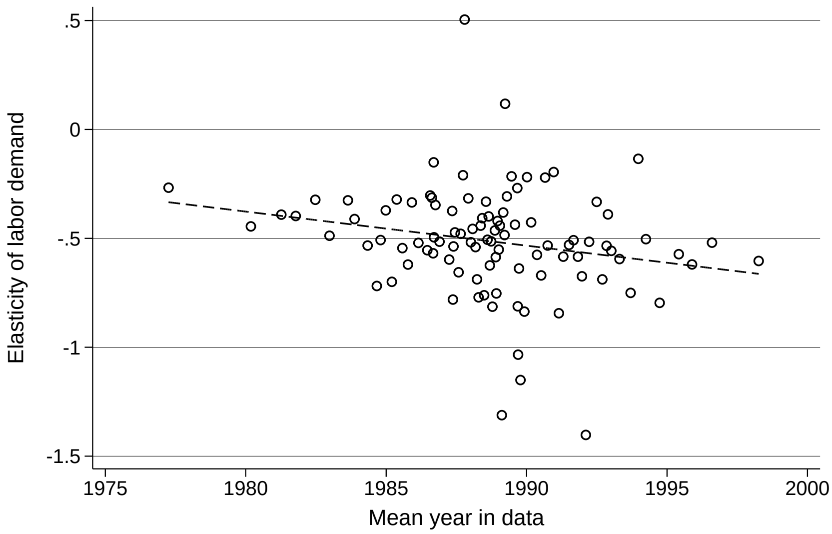
Figure 2.3.1: Industry-specific own-wage elasticities



Notes: The figure shows the differences in industry specific labor demand elasticities relative to the overall elasticity. Industry codes refer to Mining (B); Manufacturing (C); Manufacture of food, beverages, tobacco (10-12); Manufacture of textiles, apparel, leather (13-15); Manufacture of wood & wood products (16); Manufacture of paper & paper products (17); Manufacture of chemicals & chemical products (20); Manufacture of rubber & plastic products (22); Manufacture of non-metallic mineral products (23); Manufacture of basic metals (24); Manufacture of metal products (25); Manufacture of electrical equipment (27); Manufacture of transport equipment (30); Other manufacturing (32); Electricity, gas and water supply (D-E); Construction (F); Service (I-S).

lysis provides support for this view, with column (5) showing that – controlling for all other dimensions of heterogeneity – the elasticity of labor demand has increased in absolute terms over recent decades. Figure (2.3.2) illustrates this development, grouping observations according to the mean year of the underlying data and controlling for other sources of heterogeneity.

Figure 2.3.2: The elasticity of labor demand over time



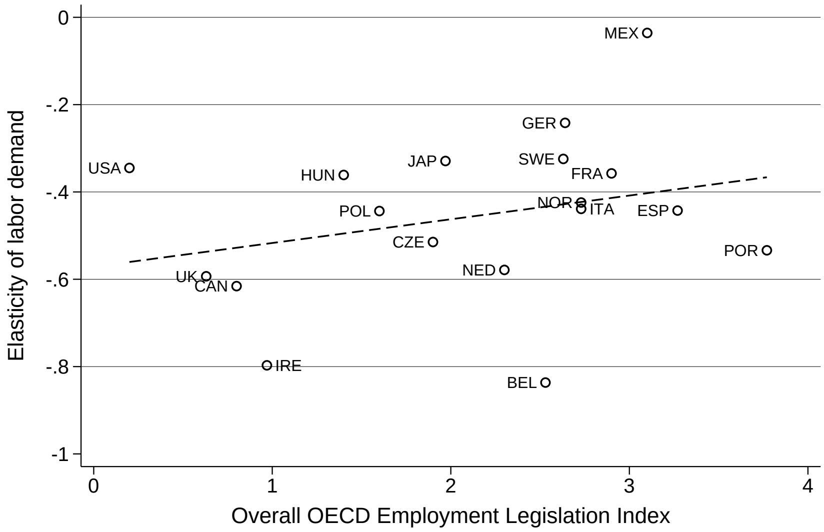
Notes: The figure shows the evolution of the elasticity of labor demand over time, controlling for all other sources of heterogeneity as shown in the baseline model (column (5) of Table 2.3.1). The graph groups the (mean) year of observation into 100 equally sized bins. Data bins are depicted by circles, the linear fit is indicated by the line.

We further find substantial differences in the labor demand elasticity across countries.[21] To illustrate these differences, Figure (2.3.3) plots the predicted labor demand elasticities against the country-specific OECD Employment Legislation Index. The graph shows a positive relationship between overall employment protection and the wage elasticity, with labor demand being less elastic in countries that have rather strict rules of employment protection legislation (for example, Spain and Mexico). In contrast, labor demand is more elastic in those countries that have weak rules on employment protection (for example, the UK and Canada). Differences in employment protection legislation among countries may

[21] The corresponding full regression results are available upon request.

thus contribute to the country-specific estimates of the labor demand elasticity.

Figure 2.3.3: The elasticity of labor demand and employment protection legislation



Notes: The figure plots predicted labor demand elasticities by country against a country-specific measure of the strictness of employment protection legislation. Predictions are based on the the preferred specification (column (5) of Table 2.3.1). The measure of employment protection legislation is calculated as the average of the OECD Employment Legislation Index for the late-1980s, late-1990s and 2003 (see Table 2.A2.4 of the OECD Employment Outlook 2004).

Overall, our analysis shows that heterogeneity in the estimates of the own-wage labor demand elasticity is natural to a considerable extent: heterogeneity is implied by different theoretical concepts of the elasticity, and responsiveness crucially depends on worker characteristics, with elasticities being larger for low-skilled and atypical workers. Moreover, estimates vary across industries and countries and have increased over time, supporting hypotheses concerning the effects of technical progress and globalization on labor demand. Thus, researchers need to carefully assess which elasticity to estimate in a given context or adapt when calibrating a model. Yet, heterogeneity is also due to researchers' choices regarding the empirical specification of the labor demand model and the dataset applied. Our analysis highlights that structural-form models better correspond to theory, and estimates based on industry-level data are considerably downward biased.

Sensitivity analysis In the preceding analysis, we have identified various factors causing heterogeneity in the estimates of the wage elasticity of labor demand. Next, we test the sensitivity of our results when (i) restricting the sample along various dimensions and (ii) using different estimators.

Recall that our sample includes all estimates of the wage elasticity of labor demand from a particular study when being derived from different specifications of the theoretical and empirical model, estimation procedures applied or in case being worker-, industry-, time- or country-specific, leading to 890 observations. However, some studies excessively contribute to the number of observations by providing, for example, estimates of the elasticity of labor demand for each single year in the underlying dataset.[22] Thus, in order to test the robustness of our results, we limit the number of estimates included in our meta-regression analysis along three dimensions. We begin by limiting the number of estimates by applying stricter selection rules. For example, in the case when the estimate of labor demand is given for many different years, only the estimate of the mean year is taken, reducing the number of observations in our meta data to 609.[23] We further drop estimates that are statistically insignificant and randomly take two estimates from each study.[24] From columns (1) to (3) of Table 2.3.2, we infer that restricting the data along these three dimensions does not significantly affect the conclusions of our analysis.

The sensitivity of our results is further tested by applying simple OLS and 'random effects' meta-regression techniques. When OLS is used, observations are weighted by the inverse of the study's number of elasticities included. In turn, 'random effects' meta-regressions estimate an additional between-study variance term to cover differences in the estimates beyond pure sampling error and those captured by the control variables (Feld and Heckemeyer, 2011). Columns (4) and (5) present the OLS results for the full and baseline sample, the former including all

[22] For example, Hijzen and Swaim (2010) provide estimates of the conditional and unconditional elasticity of labor demand for each single year from 1983 to 2002.

[23] Additional examples are studies that show the robustness of their results by obtaining estimates of the elasticity of labor demand by using cost and employment shares in structural-form models or by applying various lags when differencing the data.

[24] For the latter approach, we limit the control variables according to the specification provided in Column (4) of Table 2.3.1, given that the number of observations drops to 197. All other regressions in this section are based on our most comprehensive model.

Table 2.3.2: Sensitivity analysis: reduced samples and different estimators

Dependent variable: Labor Demand Elasticity (η)	WLS N=609 (1)	WLS T-value> 2 (2)	WLS N=197 (3)	OLS N=1334 (4)	OLS N=890 (5)	RE Meta (6)
Specification						
Time Period (omitted: Short-run)						
Intermediate-run	-0.110**	-0.183***	-0.099*	-0.305***	-0.214**	-0.174***
	(0.048)	(0.053)	(0.059)	(0.113)	(0.085)	(0.041)
Long-run	-0.147***	-0.253***	-0.131***	-0.434***	-0.275***	-0.242***
	(0.044)	(0.074)	(0.041)	(0.095)	(0.063)	(0.034)
Labor demand model (omitted: Cond./Reduced-form)						
Conditional/Structural-form	-0.066	-0.036	-0.175*	0.050	0.117	-0.015
	(0.076)	(0.084)	(0.095)	(0.073)	(0.071)	(0.047)
Unconditional/Reduced-form	0.015	-0.033***	-0.066	-0.193**	-0.042	-0.030
	(0.038)	(0.013)	(0.066)	(0.090)	(0.054)	(0.035)
Unconditional/Structural-form	-0.184	-0.129	-0.526***	0.381*	-0.000	-0.099
	(0.113)	(0.121)	(0.105)	(0.226)	(0.124)	(0.192)
Instrumenting wages	0.001	0.008	-0.152**	-0.247***	-0.247***	-0.064*
	(0.012)	(0.014)	(0.069)	(0.075)	(0.076)	(0.038)
Dataset						
Panel data specification (omitted: No panel data)						
Panel data/No unit-fixed effects	-0.297***	-0.364***	-0.190**	0.140*	0.043	-0.153*
	(0.108)	(0.091)	(0.085)	(0.074)	(0.110)	(0.084)
Panel data/Unit-fixed effects	-0.310***	-0.337***	-0.194***	0.045	-0.007	-0.212***
	(0.100)	(0.087)	(0.071)	(0.083)	(0.095)	(0.080)
Industry-level data	-0.071	-0.110*	-0.092	-0.148	-0.207**	-0.010
	(0.075)	(0.062)	(0.092)	(0.109)	(0.089)	(0.071)
Administrative data	-0.130	-0.147	0.006	-0.405***	-0.194***	-0.150***
	(0.103)	(0.114)	(0.087)	(0.094)	(0.072)	(0.056)
Industry-level admin data	0.328**	0.332**	0.121	0.478***	0.369***	0.164**
	(0.138)	(0.137)	(0.130)	(0.134)	(0.130)	(0.079)
Workforce characteristics						
Skill level (omitted: All workers)						
High-skilled workers	0.046	-0.012	0.344***	-0.016	-0.055	0.003
	(0.086)	(0.100)	(0.079)	(0.096)	(0.089)	(0.046)
Unskilled workers	-0.270***	-0.227***	-0.330***	-0.285***	-0.162**	-0.139***
	(0.095)	(0.040)	(0.084)	(0.098)	(0.079)	(0.035)
Demand for female workers	-0.174***	-0.168***	-0.041	-1.323	-1.436	-0.295***
	(0.030)	(0.024)	(0.035)	(0.851)	(0.868)	(0.079)
Atypical employment	-0.539***	-0.548***	-0.391	-0.446*	-0.325	-0.403***
	(0.047)	(0.037)	(0.384)	(0.262)	(0.304)	(0.049)
Worker characteristics (omitted: All workers)						
Blue-collar worker	-0.054	0.002	-0.320***	-0.160	-0.370***	-0.121*
	(0.066)	(0.071)	(0.055)	(0.140)	(0.107)	(0.067)
White-collar worker	-0.012	0.015	-0.225***	0.106	-0.027	-0.082
	(0.068)	(0.072)	(0.069)	(0.114)	(0.104)	(0.073)
Estimates' mean year of observation (centralized)	-0.008	-0.008*		-0.008**	-0.014**	-0.016***
	(0.005)	(0.005)		(0.004)	(0.006)	(0.003)
Constant	0.211	0.414***	0.121	0.511***	-1.492***	0.387*
	(0.140)	(0.131)	(0.086)	(0.148)	(0.267)	(0.234)
Industry dummy variables	Yes	Yes	No	Yes	Yes	Yes
Year of publication dummy variables	Yes	Yes	No	Yes	Yes	Yes
Country dummy variables	Yes	Yes	No	Yes	Yes	Yes
No. of observations	609	627	197	1,334	890	890
Adjusted R-Squared	0.827	0.856	0.550	0.288	0.281	–

Notes: The table shows estimates based on regression model (2.3.1) for the various samples and different estimation techniques as indicated in the table head. Standard errors (in parentheses) are clustered at the study level. Significance levels are 0.1 (*), 0.05 (**) and 0.01 (***).

1,334 observations. In line with previous results, the results do not significantly differ. Notably, the results in column (4) and (5) provide evidence for higher elasticities of labor demand when instrumenting the wage rate. Column (6) further shows that our findings remain unaffected when applying 'random effects' meta-regression techniques, thus underlining the robustness of our results.[25]

2.3.3 Publication selection bias

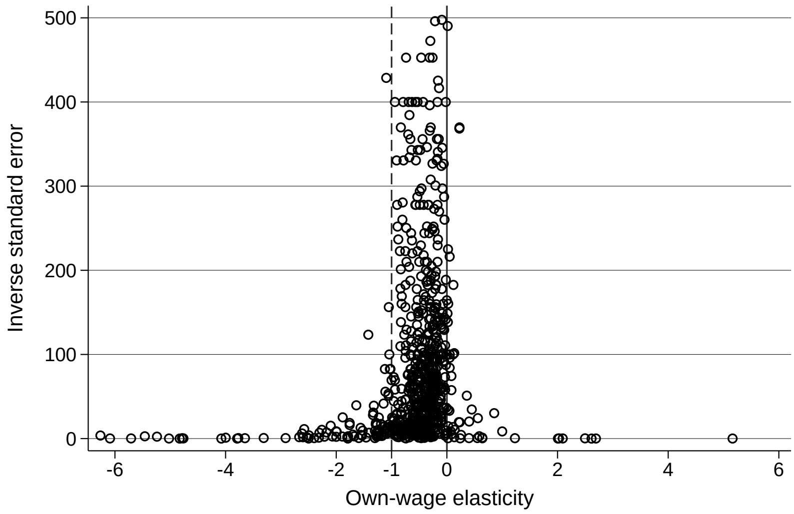
In the second part of our analysis, we evaluate whether publication selection bias is present in the empirical literature on labor demand. Journals' tendency to publish statistically significant results as well as researchers' strong beliefs in particular economic relationships and distaste for publishing null findings might induce a selection process of empirical findings that biases the true population parameter and hence limits knowledge about a particular economic relationship (DeLong and Lang, 1992; Franco et al., 2014).

One common method for detecting publication selection bias is to analyze the relationship between the estimated coefficient and its standard error (Card and Krueger, 1995; Stanley and Doucouliagos, 2015). In the absence of publication bias, there should be no systematic relationship between estimates and standard errors. However, if authors (journals) tend to only report (publish) results that are at least significant at the 10% level, implying a t-value (t) of about 1.6, a tendency to report significant results will induce a correlation between the elasticity estimate (b) and its standard error (SE), given that $t = b/SE$ (Card and Krueger, 1995). As the elasticity of labor demand is generally believed to be negative ($b < 0$), we expect to find a negative relationship between the standard error and the elasticity estimate in case of publication bias.

"Funnel plots" are a first approach to visualize publication bias by plotting point estimates against the inverse of the standard error (Sutton et al., 2000). If there is no publication bias, the graph is expected to be funnel-shaped, i.e., low-precision estimates should be widely dispersed. However, when plotting the elasticity estimates against the inverse of their standard errors, the distribution is asymmetric and skewed to the left (Figure 2.3.4). As this asymmetry reflects

[25] The full regression results are provided upon request.

Figure 2.3.4: Funnel plot for publication bias



Notes: The figure plots point estimates of the labor demand elasticity against the inverse of the corresponding standard error. For illustrative purposes, high-precision estimates with an inverse standard error greater than 500 are excluded. The funnel plot is meant to visualize publication bias. In the absence of publication bias, low-precision estimates should be widely dispersed.

publication (or reporting) bias, researchers seem to be inclined to frame their empirical specification in such a way that they obtain negative wage elasticities that are in line with theory (see Card and Krueger, 1995).

Despite the visual evidence, we also test for publication bias within our most comprehensive meta-regression specification, presented in column (5) of Table 2.3.1. According to random sampling theory, point estimates and respective standard errors should be independent. However, column (1) of Table 2.3.3 shows that the standard error has a particularly strong and statistically significant effect on the own-wage elasticity of labor demand in our model.[26] As expected, the sign is negative, reflecting the assumed negative elasticity and suggesting significant publication bias in the estimates towards more negative elasticities.

Given this evidence, we analyze whether publication bias is less prevalent in peer-reviewed journals and differs with the quality of the journal. We thus control

[26] As the empirical results concerning the sources of heterogeneity prevail, we limit our presentation to those variables indicating publication bias only. The full regression results are provided upon request.

Table 2.3.3: Testing for publication selection bias

Dependent variable: Labor Demand Elasticity (η)	WLS (1)	WLS (2)	WLS (3)	WLS (4)	WLS (5)
Standard error	-1.053***	-1.111**	-0.985***	-1.449***	-1.417***
	(0.274)	(0.427)	(0.296)	(0.313)	(0.346)
Normalized impact factor		-0.164			
		(0.156)			
Std. error*Normalized impact factor		0.287			
		(0.895)			
Std. error*Short-run elasticity			-0.462		-0.119
			(0.640)		(0.636)
Std. error*Structural-form model				0.913*	0.882*
				(0.513)	(0.521)
Constant	-0.374**	-0.327*	-0.372**	-0.390**	-0.389**
	(0.175)	(0.178)	(0.174)	(0.181)	(0.182)
No. of observations	890	890	890	890	890
Adjusted R-Squared	0.855	0.856	0.855	0.856	0.856

Notes: The table shows the estimates of the baseline model (cf. column (5) of Table 2.3.1), additionally controlling for (interactions of) the respective standard error of the estimate. Standard errors (in parentheses) are clustered at the study level. Significance levels are 0.1 (*), 0.05 (**) and 0.01 (***).

for the impact factor of the respective journal within which the own-wage elasticity estimate was published and interact the standard error with the impact factor variable.[27] The results in column (2) show that the journal's impact factor has no statistically significant effect on the extent of publication bias.

We further evaluate whether publication bias is driven by the theoretical or empirical specification of the labor demand model. Precisely, we analyze whether publication bias is stronger for estimates of the short-run rather than the intermediate- and long-run elasticity of labor demand and whether it is less pronounced when the elasticity estimate is obtained from a structural-form model. We expect that it is more likely to estimate a non-negative or insignificant elasticity in the short run because these estimates should be lower in theory. In addition, publication bias should be less present in structural-form models where modeling choices are constrained by theory. Column (3) shows that publication bias is stronger, albeit not statistically significant, for estimates of the short-run rather than intermediate- and long-run elasticity. However, column (4) reports evidence that publication bias is much weaker when the elasticity is derived from a structural-form model rather than a reduced-form model. Column (5) shows that the latter effect remains

[27] In detail, we use the IDEAS/RePEc Simple Impact Factor as of October 23, 2013. The impact factor is normalized to a range between zero and one.

statistically significant when including both interaction terms in one regression.

2.4 Conclusion

The own-wage elasticity of labor demand serves as a key parameter in economic research and policy analysis crucially influencing the effectiveness of policy reforms and the outcomes of many economic models. This importance is reflected by a large number of empirical studies devoted to the estimation of labor demand elasticities. Nonetheless, heterogeneity in the estimates of the own-wage labor demand elasticity has been apparent. Building on detailed information from 151 different micro-level studies, this paper uses meta-regression techniques to identify sources of heterogeneity affecting the estimates of the elasticity of labor demand.

Our analysis provides six key findings. First, heterogeneity in the estimates of labor demand can be explained by the different concepts of elasticities applied. Second, labor demand responses to wage changes depend on worker characteristics, with elasticities being higher for low-skilled and atypical workers. Third, labor demand elasticities are industry- and country-specific, with low levels of employment protection legislation implying more elastic demand for labor. Fourth, firms' labor demand has become more elastic over time, supporting hypotheses concerning the effects of technical progress and globalization on labor demand. Hence, heterogeneity in the estimates of the elasticity of labor demand is natural to a considerable extent. Fifth, some differences in the estimates are due to the estimation procedure applied and the type of data used. More precisely, the results show that estimates from structural labor demand models better correspond to theory and suggest that instrumenting the wage variable leads to higher estimates of the own-wage elasticity. Moreover, industry-level estimates are lower in absolute terms compared to firm-level estimates. Sixth, and even more worryingly, our analysis also points to substantial publication (or reporting) bias, especially in reduced-form models.

Several important conclusions can be drawn from this analysis. Our findings highlight that the prevalent heterogeneity in the estimates of the labor demand elasticity has to be taken into account. There is no such thing as a central elasticity of labor demand; rather, researchers need to precisely determine the type of elasticity that best corresponds with their analysis. Moreover, our analysis points to potential dangers in reporting biased elasticities. The choice of data and empirical specification applied seems to influence the estimated elasticities, which implies some arbitrariness and unwanted discretion for researchers to produce estimates that are in line with the priors. In particular, we find that industry-level elasticity estimates are downward biased, and estimates obtained from structural-form models better correspond with theory. This potential problem is corroborated by our finding of substantial publication bias, particularly present in reduced-form studies, where there is much more discretion in terms of the empirical specification applied.

2.5 Appendix

Table 2.5.1: Distribution of labor demand elasticities by sector/industry

	No. of estimates	
	Baseline Sample	**Full Sample**
All sectors	303	415
Mining (B)	3	9
Manufacturing (C)	378	557
Manufacture of food,beverages,tobacco (10-12)	6	20
Manufacture of textiles,apparel,leather (13-15)	6	23
Manufacture of wood & wood products (16)	3	11
Manufacture of paper & paper products (17)	7	17
Printing (18)	1	5
Manufacture of coke & petroleum (19)	2	2
Manufacture of chemicals & chemical products (20)	16	22
Manufacture of rubber & plastic products (22)	2	7
Manufacture of non-metallic mineral products (23)	11	21
Manufacture of basic metals (24)	8	32
Manufacture of metal products (25)	6	10
Manufacture of electrical equipment (27)	5	9
Manufacture of machinery (28)	10	21
Manufacture of transport equipment (30)	8	14
Other manufacturing (32)	15	24
Electricity, gas and water supply (D-E)	5	9
Construction (F)	52	52
Wholesale (G)	3	3
Transportation (H)	0	4
Service (I-S)	36	43
Information and communication (J)	1	1
Financial & insurance services (K)	3	3

Notes: The baseline sample covers 890 observations and includes all estimates of the own-wage elasticity with a given or calculable standard error. The full sample (N=1,334) further includes all point estimates without a given or computable standard error. Industrial classification according to ISIC Rev.4 of the United Nations Statistics Division. Due to changes in the ISIC classification over time, industries $10-12$, $13-15$, $D-E$ had to be pooled.

Table 2.5.2: Distribution of estimates by year of publication and country

	No. of estimates			No. of estimates	
	Baseline Sample	**Full Sample**		**Baseline Sample**	**Full Sample**
Year					
1971	0	4	1995	6	7
1974	0	4	1996	19	19
1975	0	5	1997	28	28
1977	0	2	1998	57	70
1979	0	9	1999	16	34
1980	10	12	2000	8	22
1981	5	95	2001	77	79
1983	0	2	2002	13	33
1984	18	22	2003	65	96
1985	2	17	2004	33	52
1986	38	44	2005	71	73
1987	1	17	2006	46	47
1988	12	20	2007	47	50
1989	0	2	2008	78	91
1990	1	16	2009	6	6
1991	8	9	2010	167	237
1992	16	51	2011	7	7
1993	19	19	2012	14	31
1994	2	2			
Country					
Aggregate Data	138	202	Lithuania	2	2
Aggr. European Data	19	32	Macedonia	2	4
Argentina	4	6	Mauritius	2	2
Belgium	6	10	Mexico	7	7
Bulgaria	2	2	Netherlands	5	10
Canada	4	40	Norway	3	4
Chile	2	2	Peru	13	13
China	1	1	Poland	7	7
Colombia	31	50	Portugal	3	3
Czech Republic	9	9	Romania	1	2
Denmark	1	2	Slovak Republic	6	6
Finland	1	2	Slovenia	1	2
France	12	16	South Korea	4	4
Germany	243	302	Spain	6	23
Ghana	0	2	Sweden	22	74
Hungary	9	9	Tunisia	24	24
India	3	3	Turkey	51	51
Ireland	5	5	United Kingdom	57	65
Italy	11	14	United States	152	287
Japan	16	30	Uruguay	5	5

Notes: The baseline sample covers 890 observations and includes all estimates of the own-wage elasticity with a given or calculable standard error. The full sample (N=1,334) further includes all point estimates without a given or computable standard error.

2.6 Data Appendix

Table 2.6.1: Dimensions of heterogeneity and source (baseline sample)

Study	Year	Specification heterogeneity in							Estimates	Source
		Theory	Empirics	Data	Worker Type	Sector	Country	Time period		
Field and Grebenstein (1980)	1980					10			10	Tab. 2, Col. (3)
Denny et al. (1981)	1981								1	Tab. 4, Col. (2)
Grant and Hamermesh (1981)	1981				4				4	Tab. 3, Col. (1,2,3,4)
Atkinson and Halvorsen (1984)	1984								1	Tab. 3, Col. (2)
Nissim (1984)	1984	2			3				6	Tab. 3
Symons and Layard (1984)	1984		1				6			Tab. 1
			1				5		11	Tab. 2
Mairesse and Dormont (1985)	1985							2	2	Tab. 6, Col. (1,2)
Allen (1986)	1986			6						Tab. 4, Col. (1,2,3,5,6,7)
				6	3					Tab. 7, Col. (1,2,3,5,6,7)
				6	2				36	Tab. A1, Col. (1,2,3,5,6,7)
Halvorsen and Smith (1986)	1986								1	Tab. 2, Col. (2)
Kokkelenberg and Choi (1986)	1986								1	Tab. 3
Wadhwani (1987)	1987								1	Tab. 2
Kim (1988)	1988	2							2	Tab. 2 & 3, Col. (2)
Morrison (1988)	1988	2	1				2			Tab. 2, Col. (1-4)
		2	1				2		8	Tab. 2,Col. s (9-12)
Pencavel and Holmlund (1988)	1988	2							2	Tab. 1, Col. (2,4)
Wadhwani and Wall (1990)	1990								1	Tab. 2, Col. (1)
Arellano and Bond (1991)	1991	1	7							Tab. 4, Col. (1,2,4)

Table 2.6.1: continued

Study	Year	Specification heterogeneity in: Theory	Empirics	Data	Worker Type	Sector	Country	Time period	Estimates	Source
										Tab. 5
		1							8	Text, p. 291
Griffin (1992)	1992		2		4	2			16	Tab. 2 & 4, Col. (2,4)
Dunne and Roberts (1993)	1993		3		2			2		Tab. A2 & A3
			2		2				16	Tab. A2 & A3
Wolfson (1993)	1993					3			3	Tab. 6, Col. (1,3,5)
Fitzroy and Funke (1994)	1994		2						2	Tab. 3, Col. (1,2)
Konings and Vandenbussche (1995)	1995					2			2	Tab. 4 & 6, Col. (2)
Lindquist (1995)	1995		2						2	Tab. 3.8, Col. (1,5)
Draper and Manders (1996)	1996				2	2			4	Tab. 2, Col. (1,2)
Griffin (1996)	1996			2	6				12	Tab.1 & 2, Col. (2)
Terrell (1996)	1996		3						3	Tab. 3, Col. (1,4,7)
Cahuc and Dormont (1997)	1997	1	3							Tab. 4, Col. (1,2,3)
		1							4	Tab. 4, Col. (6)
Falk and Koebel (1997)	1997				3	5			15	Tab. 4
Konings and Roodhooft (1997)	1997	2							2	Tab. 5 & 6 , Col. (1)
Revenga (1997)	1997		2	1						Tab. 4, Col. (5,6)
		2	1							Tab. 7, Col. (1,2)
			1						5	Tab. 7, Col. (6)
VanReenen (1997)	1997		3							Tab. 3, Col. (3,4,5)
			1						4	Tab. 4, Col. (2)
Blechinger et al. (1998)	1998						2		2	Tab. A13, Col. (1,2)
FitzRoy and Funke (1998)	1998				3		2		6	Tab. 2,3
Hatzius (1998)	1998		2				2		4	Tab.6 & 7, Col. (2,6)

Table 2.6.1: continued

Study	Year	Specification heterogeneity in: Theory	Empirics	Data	Worker Type	Sector	Country	Time period	Estimates	Source
Hine and Wright (1998)	1998								1	Tab. 2, Col. (1)
Koebel (1998)	1998					23			23	Tab. 3, Col. (2)
Milner and Wright (1998)	1998							2		Tab. 2, Col. (2)
									2	Tab. 2, Col. (6)
Roberts and Skoufias (1998)	1998		2		2					Tab. 1, Col. (1,2)
			7		2				18	Tab. 2
Rottmann and Ruschinski (1998)	1998								1	Tab. 1, Col. (1)
Abraham and Konings (1999)	1999								1	Tab. 7, Col. (3)
Allen and Urga (1999)	1999	2							2	Tab. 5, Col. (1,2)
Bellmann et al. (1999)	1999				6				6	Tab. A1
Blechinger and Pfeiffer (1999)	1999					2			2	Tab. 2, Col. (6)
Falk and Koebel (1999)	1999				3				3	Tab. 4, Col. (2)
Funke et al. (1999)	1999								1	Tab. 3, Col. (5)
Greenaway et al. (1999)	1999								1	Tab. 2, Col. (5)
Bellmann and Schank (2000)	2000				6				6	Tab. 3
Braconier and Ekholm (2000)	2000					2			2	Tab. 2, Col. (2,3)
Addison and Teixeira (2001)	2001						4		4	Tab. 4
Falk (2001)	2001								1	Tab. 7
Falk and Koebel (2001)	2001		2		3					Tab. 3
					3				9	Tab. B2
Krishna et al. (2001)	2001		2			10				Tab. 2, Col. (1,3)
					3	10				Tab. 4, Col. (1,3,5)
			1						51	Tab. 5, Reg(B)
Slaughter (2001)	2001	2	3		2				12	Tab. 2
Bellmann et al. (2002)	2002				3		2		6	Tab. A3 & A4
Falk and Koebel (2002)	2002				3				3	Tab. 5
Koebel (2002)	2002				4				4	Tab. 7, Col. (1)
Bruno et al. (2003)	2003	2	2				8		32	Tab. 1b-8b
Koebel et al. (2003)	2003		3	2	3				18	Tab. 4 & 5

Table 2.6.1: continued

Study	Year	Specification heterogeneity in							Estimates	Source
		Theory	**Empirics**	**Data**	**Worker Type**	**Sector**	**Country**	**Time period**		
Barba Navaretti et al. (2003)	2003						11		11	Tab. 2
Ogawa (2003)	2003					4			4	Tab. 3
Bernal and Cardenas (2004)	2004		2	1						Tab. 4.7, Col. (3,5)
			3	1					5	Tab. 4.8, Col. (4,5,6)
Cassoni et al. (2004)	2004			1						Tab. 8.3, Col. (5)
		2		1				2		Tab. 5, Col. (3,6)
									5	Tab. 8.6
Falk and Koebel (2004)	2004				3	2			6	Tab. 4 & 5
Konings and Murphy (2004)	2004	2				3			6	Tab. 5,6
Mondino and Montoya (2004)	2004		2	2					4	Tab. 6.7, Col. (2,3,4,5)
Saavedra and Torero (2004)	2004			1						Tab. 2.4, Col. (1)
				2				3	7	Tab. 2.5
Addison and Teixeira (2005)	2005			2			2		4	Tab. 1 & 2
Amiti and Wei (2005)	2005	4	1			2				Tab. 9b & 10b
		2	1			2			12	Tab. 9a & 10a
Arnone et al. (2005)	2005	2							2	Tab. 2, Col. (1,2)
Basu et al. (2005)	2005	1					2	4		Tab. 4
		1					2	3		Tab. 4
		1					2	3		Tab. 4
		1					1	2		Tab. 4
		1					1	1	23	Tab. 4
Becker et al. (2005)	2005						2		2	Tab. 4 & 5
Bruno and Falzoni (2005)	2005	2	3						6	Tab. 4
Fajnzylber and Maloney (2005)	2005				2		3		6	Tab. 1
Falk and Wolfmayr (2005)	2005		4						4	Tab. 5, Col. (1)
Fu and Balasubramanyam (2005)	2005								1	Tab. 3, Col. (7)
Görg and Hanley (2005)	2005		2						2	Tab. 2, Col. (1,2)

Table 2.6.1: continued

Study	Year	Specification heterogeneity in: Theory	Empirics	Data	Worker Type	Sector	Country	Time period	Estimates	Source
Hijzen et al. (2005)	2005		3		3					Tab. 5, Col. (1,2,3,5,6,7)
									9	Tab. 6, Col. (4,5,6)
Amiti and Wei (2006)	2006	2	2			1				Tab. 11, Col. (1,3,4,6)
		2				1				Tab. 12, Col. (2,3)
		2				1				Tab. 12, Col. (5,6)
		2	2			1				Tab. 13, Col. (1,2,4,5)
		2				1				Tab. 14, Col. (1,2)
		2				1			16	Tab. 14, Col. (5,6)
Bellmann and Pahnke (2006)	2006		2		3		2			Tab. 1 & 2 & 3, Col. (3,6)
									12	Tab. 4-9,Col. (5)
Blien et al. (2006)	2006	1								Tab. 3a
		1				3			4	Tab. 5, Col. (1,3,5)
Ekholm and Hakkala (2006)	2006		2		3				6	Tab. 3A & A2c
Harrison and McMillan (2006)	2006			2					2	Tab. 5,6, Col(1)
Koebel (2006)	2006		2		3				6	Tab. 1,3
Crino (2007)	2007		2		3					Tab. 6, Model(2)
									6	Tab. 8, Model(3)
Haouas and Yagoubi (2007)	2007		2			6				Tab. 2, Col. (1,5)
					2	6			24	Tab4, Col. (1,5)
Hasan et al. (2007)	2007		3						3	Tab. 3 & 5 & 6, Col. (1)
Lachenmaier and Rottmann (2007)	2007					1				Tab. 2, Col. (1)
								1		Tab. 5, Col. (1)
							2		4	Tab. 5, Col. (2,3)

Table 2.6.1: continued

Study	Year	Specification heterogeneity in: Theory	Empirics	Data	Worker Type	Sector	Country	Time period	Estimates	Source
Molnar and Taglioni (2007)	2007		2				3			Tab. 4
		2	2				1		10	Tab. 6 & 7, Col. (1)
Aguilar and Rendon (2008)	2008		2						2	Tab. 2, Col. (5,6)
Jacobi and Schaffner (2008)	2008		3		5		2	2	60	Tab. 2 & 3
Micevska (2008)	2008		2						2	Tab. 5, Col. (2,4)
Onaran (2008)	2008	1			1		2			Tab. 3a, Col. (3,6)
		1			1		4			Tab. 3a, Col. (1,2,5,8)
		1			1		4			Tab. 3 & Cont., Col. (2,3,6,7)
					1		4		14	Tab. 3 & Cont., Col. (1,2,5,8)
Godart et al. (2009)	2009		3						3	Tab. 3, Col. (5)
										Tab. 8, Col. (5,6)
Görg et al. (2009)	2009		1							Tab. 2, Col. (3)
			2			1			3	Tab. A1, Col. (5,6)
Aguilar and Rendon (2010)	2010		2		2				4	Tab. 2, Col. (5,6)
Brixy and Fuchs (2010)	2010	2							2	Tab. 8, Col(2,3)
Buch and Lipponer (2010)	2010					3			3	Tab. 5, Col. (1,2,3)
Freier and Steiner (2010)	2010				8		2		16	Tab. Appendix
Hakkala et al. (2010)	2010				4					Tab. 2, Col. (2)
									4	Tab. 3, Col. (1,2,3)
Hijzen and Swaim (2010)	2010	2	1					20		Tab. 3, Col. (1,7)
		2	1					18		Tab. 3, Col. (4,10)
			1			2		20		Tab. 4, Col. (4,7)
			1					16	132	Tab. 4, Col. (10)
Senses (2010)	2010	2	2	1						Tab. 1, Col. (1)
		2		1					6	Tab. 1, Col. (1)

Table 2.6.1: continued

Study	**Year**	**Specification heterogeneity in**							**Estimates**	**Source**
		Theory	**Empirics**	**Data**	**Worker Type**	**Sector**	**Country**	**Time period**		
Bohachova et al. (2011)	2011		3						3	Tab. 2, Col. (1,2,3)
Mitra and Shin (2011)	2011	2	2						4	Tab. 5, Col. (1,2,5,6)
Ayala (2012)	2012	2	3						6	Tab. 7
Crino (2012)	2012				3				3	Tab. 5, Col. (10,11,12)
Kölling (2012)	2012					5			5	Tab. 5, Col. (1)

Table 2.6.2: Dimensions of heterogeneity and source (estimates without std. error)

Study	Year	Specification heterogeneity in							Estimates	Source
		Theory	Empirics	Data	Worker Type	Sector	Country	Time period		
Tinsley (1971)	1971	2						2	4	Tab. 3.5
Nadiri and Rosen (1974)	1974				2	2			4	Tab. 3
Berndt and Wood (1975)	1975							5	5	Tab. 5
Kesselman et al. (1977)	1977				2				2	Text, p. 344
Berndt and Khaled (1979)	1979		5						5	Tab. 5, Col. (1,3,4,5,6)
Magnus (1979)	1979		4						4	Tab. 4
Clark and Freeman (1980)	1980		2						2	Tab. 2, Equations (1,2)
Anderson (1981)	1981							3	3	Tab. 7.4
Denny et al. (1981)	1981	2				18	2		72	Tab. 11.1 & 11.3
Morrison and Berndt (1981)	1981	2			1					Tab. 2, Col. (1,3)
		2			2				6	Tab. 4, Col. (1,3)
Norsworthy and Harper (1981)	1981							3		Calc. from Tab. 9.2
		2						3	9	& 9.4, Col. (A,E,F)
Pindyck and Rotemberg (1983)	1983	2							2	Tab. 2
Nelson (1984)	1984							3	3	Text, p. 63
Nickell (1984)	1984								1	Text, p. 548
Carruth and Oswald (1985)	1985	3	1							Tab. 2
		2	1						5	Tab. 5
Faini and Schiantarelli (1985)	1985	2							2	Tab. 3, Col. (4)
Segerson and Mount (1985)	1985		2					4	8	Tab. 4,6
Morrison (1986)	1986	2	3						6	Tab. 2
Chung (1987)	1987		4						4	Tab. 5
Diewert and Wales (1987)	1987		5					2	10	Tab. 2 & 4
McElroy (1987)	1987		2						2	Calc. from Tab. 2

Table 2.6.2: continued

Study	Year	Specification heterogeneity in							Source	Estimates
		Theory	Empirics	Data	Worker Type	Sector	Country	Time period		
Baltagi and Griffin (1988)	1988								1	(Hamermesh, 1993, Tab.3.2)
Burgess (1988)	1988								1	Text, p. 90
Daughety and Nelson (1988)	1988							4	4	Tab. 2, Col. (2)
Deno (1988)	1988								1	Tab. 3, Col. (2)
Pencavel and Holmlund (1988)	1988								1	Text, p. 1113
Flaig and Steiner (1989)	1989								1	Text, p. 404
Kokkelenberg and Nguyen (1989)	1989								1	Tab. 4, Col. (2)
Nakamura (1990)	1990					7		2	14	Tab. 3, Col. (5)
Nickell and Symmons (1990)	1990								1	(Hamermesh, 1993, Tab.3.2)
Blanchflower et al. (1991)	1991								1	Text, p. 825
Bergström and Panas (1992)	1992					8		4	32	Tab. 4
Bresson et al. (1992)	1992				1					Tab. 2, Col. (2)
					2				3	Tab. 4
Konings and Roodhooft (1997)	1995								1	Text, p. 11
FitzRoy and Funke (1998)	1998				3		2		6	Tab. 4
Koebel (1998)	1998					6			6	Tab. 3
Rottmann and Ruschinski (1998)	1998								1	Tab. 2
Mellander (1999)	1999	2			3			1		Tab.7a & b
		2			3			1		Tab. 8a & b
		2			3			1	18	Tab. 9a & b
Ryan and Wales (2000)	2000		6					2	12	Tab. 2 & 3
Teal (2000)	2000				2			2	2	Tab. 6
Flaig and Rottmann (2001)	2001	2							2	Tab. 3
Bauer and Riphahn (2002)	2002								1	Tab. 1, Col. (1)
Cuyvers et al. (2005)	2002	2		1		6				Tab. 4
				1					13	Tab. 6, Col (3)
Kölling and Schank (2002)	2002				3	2			6	Tab. 4 & 5

Table 2.6.2: continued

Study	Year	Specification heterogeneity in Theory	Empirics	Data	Worker Type	Sector	Country	Time period	Source	Estimates
Bruno and Falzoni (2003)	2003	2							2	Tab. 4
Koebel et al. (2003)	2003		3	2	3				18	Tab. 3
Barba Navaretti et al. (2003)	2003						11		11	Tab. 3
Bernal and Cardenas (2004)	2004	2			2			4		Tab. 4.4 & 4.5
		1							17	Tab. 4.9
Mondino and Montoya (2004)	2004		2						2	Tab. 6.12, Col. (1)
Arnone et al. (2005)	2012	2							2	Text, pp. 735;738
Harrison and McMillan (2006)	2006								1	Tab. A6
Benito and Hernando (2007)	2007				3				3	Text, p.300
Addison et al. (2008)	2008				4	2			8	Tab. 6 & 7
Benito and Hernando (2008)	2008								1	Text, pp. 291
Micevska (2008)	2008		2						2	Tab. 6
Onaran (2008)	2008						2		2	Tab. 3
Brixy and Fuchs (2010)	2010		2							Tab. 4,Col (5,6)
							2		4	Tab. 5,Col (5,6)
Buch and Lipponer (2010)	2010								1	Tab. 4
Hijzen and Swaim (2010)	2010	2	2					16	64	Tab. 5
Muendler and Becker (2010)	2010								1	Tab. 7, Col. (1)
Ayala (2012)	2012		2						2	Tab. 8, Col. (3,5)
Peichl and Siegloch (2012)	2012				3				3	Tab. 1
Sala and Trivin (2012)	2012	2	2					2		Tab. 5 & 7
		2						2	12	Tab. 5&7

Table 2.6.3: Empirical studies with given or calculable standard errors

Study	**Model specifics**		**Data**	
	Theoretical model	**Empirical specification**	**Characteristics**	**Period**
Field and Grebenstein (1980)	long-run, conditional	structural, exogenous wage, no FE	industry-level, cross-section, admin	1971
Denny et al. (1981)	long-run, conditional	structural, exogenous wage, no FE	firm-level, time-series, admin	1952-1976
Grant and Hamermesh (1981)	long-run, conditional	structural, exogenous wage, no FE	industry-level, cross-section, admin	1969
Atkinson and Halvorsen (1984)	long-run, conditional	structural, exogenous wage, no FE	firm-level, cross-section, survey	1970
Nissim (1984)	short-/intermediate-run, conditional	structural-form, endogenous wage, no FE	industry-level, time-series, admin	1963-1978
Symons and Layard (1984)	long-run, unconditional	reduced-form, en-/exogenous, no FE	industry-level, time-series, admin	1956-1980
Mairesse and Dormont (1985)	short-run, unconditional	reduced-form, exogenous wage, FE	firm-level, panel, survey	1970-1979
Allen (1986)	long-run, conditional	structural-form, exogenous wage, no FE	firm-level, cross-section, survey	1972/1974
Halvorsen and Smith (1986)	long-run, conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1954-1974
Kokkelenberg and Choi (1986)	long-run, conditional	structural-form, exogenous wage, no FE	firm-level, cross-section, admin	1970

Table 2.6.3: continued

Study	**Model specifics**		**Data**	
	Theoretical model	**Empirical specification**	**Characteristics**	**Period**
Wadhwani (1987)	long-run, unconditional	reduced-form, exogenous wage, no FE	industry-level, time-series, admin	1962-1981
Kim (1988)	long-run, (un)conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1948-1971
Morrison (1988)	short-/intermediate-/long-run, conditional	structural, endogenous wage, no FE	industry-level, time-series, admin	1955-1981
Pencavel and Holmlund (1988)	short-/intermediate-run, unconditional	reduced-form, endogenous wage, no FE	industry-level, time-series, admin	1951-1983
Wadhwani and Wall (1990)	short-run, unconditional	reduced-form, endogenous wage, FE	industry-level, panel, survey	1974-1982
Arellano and Bond (1991)	short-/long-run, unconditional	reduced-form, ex/endogenous wage, (no) FE	firm-level, panel, survey	1979-1984
Griffin (1992)	long-run, conditional	structural-form, exogenous wage, no FE	firm-level, cross-section, admin	1980
Dunne and Roberts (1993)	long-run, conditional	reduced-form, exogenous wage, (no) FE	firm-level, panel, survey	1975-1981
Wolfson (1993)	short-run, conditional	structural-form, endogenous wage, FE	firm-level, panel, survey	1976-1984
Fitzroy and Funke (1994)	short-run, conditional	reduced-form, endogenous wage, FE	industry-level, panel, admin	1979-1990

Table 2.6.3: continued

Study	Model specifics		Data	
	Theoretical model	Empirical specification	Characteristics	Period
Konings and Vandenbussche (1995)	long-run, conditional	reduced-form, endogenous wage, FE	firm-level, panel, survey	1982-1989
Lindquist (1995)	intermediate, conditional	structural-form, exogenous wage, FE	firm-level, panel, admin	1972-1990
Draper and Manders (1996)	long-run, conditional	structural-form, endogenous wage, no FE	industry-level, time-series, admin	1972-1993
Griffin (1996)	long-run, conditional	structural-form, exogenous wage, no FE	firm-/industry-level, cross-section, admin	1980
Terrell (1996)	long-run, conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1947-1971
Cahuc and Dormont (1997)	short-/intermediate-run, conditional	reduced-form, exogenous wage, (no) FE	firm-level, panel, survey	1986-1989
Falk and Koebel (1997)	long-run, conditional	structural-form, exogenous wage, no FE	industry-level, panel, admin	1977-1994
Konings and Roodhooft (1997)	short-/intermediate-run, conditional	reduced-form, endogenous wage, FE	firm-level, panel, admin	1989-1994
Revenga (1997)	intermediate-run, (un)conditional	reduced-form, exogenous wage, (no) FE	firm-/industry-level, panel, survey	1984-1990
VanReenen (1997)	short-run, unconditional	reduced form, ex/endogenous wage, FE	firm-level, panel, survey	1976-1982

Table 2.6.3: continued

Study	Model specifics		Data	
	Theoretical model	Empirical specification	Characteristics	Period
Blechinger et al. (1998)	long-run, conditional	structural-form, exogenous wage, FE	firm-level, panel, survey	1993-1995
FitzRoy and Funke (1998)	short-run, conditional	reduced form, endogenous wage, FE	industry-level, panel, admin	1991-1993
Hatzius (1998)	long-run, conditional	reduced-form, ex/endogenous wage, FE	firm-level, panel, survey	1974-1994
Hine and Wright (1998)	short-run, conditional	reduced-form, exogenous wage, FE	industry-level, panel, admin	1979-1992
Koebel (1998)	long-run, conditional	structural-form, exogenous wage, no FE	industry-level, panel, admin	1960-1992
Milner and Wright (1998)	short-run, conditional	reduced-form, exogenous wage, FE	industry-level, panel, admin	1972-1992
Roberts and Skoufias (1998)	long-run, conditional	reduced-form, exogenous wage, (no) FE	firm-level, panel, survey	1981-1987
Rottmann and Ruschinski (1998)	short-run, conditional	reduced-form, exogenous wage, FE	firm-level, panel, survey	1980-1992
Abraham and Konings (1999)	intermediate-run, conditional	reduced-form, exogenous wage, no FE	firm-level, panel, survey	1990-1995
Allen and Urga (1999)	short-/long-run, conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1965-1992

Table 2.6.3: continued

Study	**Model specifics**		**Data**	
	Theoretical model	**Empirical specification**	**Characteristics**	**Period**
Bellmann et al. (1999)	intermediate-run, conditional	structural-form, exogenous wage, no FE	firm-level, cross-section, admin	1995
Blechinger and Pfeiffer (1999)	long-run, conditional	reduced-form, exogenous wage, FE	firm-level, panel, survey	1992-1995
Falk and Koebel (1999)	long-run, conditional	structural-form, exogenous wage, FE	industry-level, panel, admin	1978-1999
Funke et al. (1999)	short-run, conditional	reduced-form, endogenous wage, FE	firm-level, panel, admin	1987-1994
Greenaway et al. (1999)	short-run, conditional	reduced-form, exogenous wage, FE	industry-level, panel, admin	1979-1991
Bellmann and Schank (2000)	intermediate-run, conditional	structural-form, exogenous wage, no FE	firm-level, cross-section, admin	1995
Braconier and Ekholm (2000)	long-run, conditional	reduced-form, exogenous wage, FE	firm-level, panel, survey	1970-1994
Addison and Teixeira (2001)	long-run, conditional	reduced-form, exogenous wage, no FE	industry-level, time-series, admin	1977-1997
Falk (2001)	intermediate-run, conditional	reduced-form, exogenous wage, FE	firm-level, panel, survey	1995-1997
Falk and Koebel (2001)	short-/intermediate-run, conditional	structural-form, exogenous wage, FE	industry-level, panel, admin	1976-1995

Table 2.6.3: continued

Study	**Model specifics**		**Data**	
	Theoretical model	**Empirical specification**	**Characteristics**	**Period**
Krishna et al. (2001)	intermediate-run, unconditional	reduced-form, ex/endogenous wage, FE	firm-level, panel, admin	1983-1986
Slaughter (2001)	intermediate-run, unconditional	reduced-form, exogenous wage, FE	industry-level, panel, admin	1961-1991
Bellmann et al. (2002)	intermediate-run, conditional	structural-form, exogenous wage, no FE	firm-level, panel, admin	1993-1998
Falk and Koebel (2002)	intermediate-run, conditional	structural-form, exogenous wage, FE	industry-level, panel, admin	1978-1990
Koebel (2002)	long-run, conditional	structural-form, exogenous wage, FE	industry-level, panel, admin	1978-1990
Bruno et al. (2003)	short-/long-run, conditional	reduced-form, exogenous wage, FE	industry-level, panel, admin	1970-1996
Koebel et al. (2003)	long-run, conditional	structural-form, exogenous wage, FE	industry-level, panel, admin	1978-1990
Barba Navaretti et al. (2003)	short-run, conditional	reduced-form, exogenous wage, FE	firm-level, panel, admin	1993-2000
Ogawa (2003)	short-run, conditional	reduced-form, endogenous wage, FE	firm-level, panel, survey	1993-1998
Bernal and Cardenas (2004)	short-run, conditional	reduced-form, ex/endogenous, (no) FE	firm-/industry-level, panel, survey	1978-1991

Table 2.6.3: continued

Study	Model specifics		Data	
	Theoretical model	**Empirical specification**	**Characteristics**	**Period**
Cassoni et al. (2004)	short-/long-run, conditional	structural-/reduced-form, ex/endogenous, FE	industry-level, panel, admin	1975-1997
Falk and Koebel (2004)	intermediate-run, conditional	structural-form, exogenous wage, FE	industry-level, panel, admin	1978-1994
Konings and Murphy (2004)	short-/long-run, conditional	reduced-form, exogenous wage, FE	firm-level, panel, admin	1993-1998
Mondino and Montoya (2004)	short-run, conditional	reduced-form, ex/endogenous wage, FE	firm-level, panel, survey	1990-1996
Saavedra and Torero (2004)	short-/long-run, conditional	reduced-form, exogenous wage, (no) FE	firm-/industry-level, panel, survey	1987-1997
Addison and Teixeira (2005)	short-/long-run, (un)conditional	reduced-form, endogenous wage, (no) FE	firm-/industry-level, panel/time-series, admin/survey	1977-2001
Amiti and Wei (2005)	short-/long-run, (un)conditional	reduced-form, exogenous wage, FE	industry-level, panel, admin	1995-2001
Arnone et al. (2005)	short-run, (un)conditional	reduced-form, endogenous wage, FE	firm-level, panel, survey	1998-2002
Basu et al. (2005)	short-/long-run, conditional	reduced-form, endogenous wage, FE	firm-level, panel, admin	1989-1993

Table 2.6.3: continued

Study	Model specifics		Data	
	Theoretical model	**Empirical specification**	**Characteristics**	**Period**
Becker et al. (2005)	intermediate-run, conditional	structural-form, exogenous wage, no FE	firm-level, cross-section, admin/survey	1998/2000
Bruno and Falzoni (2005)	short-/long-run, conditional	reduced-form , ex/endogenous wage, FE	industry-level, panel, admin	1970-1997
Fajnzylber and Maloney (2005)	long-run, unconditional	reduced-form, endogenous wage, FE	firm-level, panel, survey	1977-1995
Falk and Wolfmayr (2005)	long-run, conditional	reduced-form, exogenous wage, FE	industry-level, panel, admin	1995-2000
Fu and Balasubramanyam (2005)	short-run, conditional	reduced-form, exogenous wage, FE	industry-level, panel, survey	1987-1998
Görg and Hanley (2005)	short-run, conditional	reduced-form, ex/endogenous, FE	firm-level, panel, survey	1990-1995
Hijzen et al. (2005)	intermediate-run, conditional	structural-form, exogenous wage, (no) FE	industry-level, panel, survey	1982-1996
Amiti and Wei (2006)	short-/intermediate-run, (un)conditional	reduced-form, exogenous wage, FE	industry-level, panel, admin	1992-2000
Bellmann and Pahnke (2006)	short-run, conditional	reduced-form, exogenous wage, FE	firm-level, panel, admin	1996-2004
Blien et al. (2006)	short-/intermediate-run, conditional	reduced-form, exogenous wage, FE	firm-level, panel, admin	1993-2002

Table 2.6.3: continued

Study	Model specifics		Data	
	Theoretical model	Empirical specification	Characteristics	Period
Ekholm and Hakkala (2006)	intermediate, conditional	structural-form, exogenous wage, no FE	industry-level, panel, admin	1995-2000
Harrison and McMillan (2006)	intermediate-run, unconditional	reduced-form, exogenous wage, FE	firm-level, panel, survey	1982-1999
Koebel (2006)	long-run, conditional	structural-form, exogenous wage, FE	industry-level, panel, admin	1976-1995
Crino (2007)	intermediate-run, conditional	structural-form, ex/endogenous wage, FE	industry-level, panel, admin	1990-2004
Haouas and Yagoubi (2007)	intermediate-run, unconditional	reduced-form, exogenous wage, (no) FE	industry-level, panel, admin	1971-1996
Hasan et al. (2007)	intermediate-run, conditional	reduced-form, exogenous wage, FE	industry-level, panel, survey	1980-1997
Lachenmaier and Rottmann (2007)	long-run, conditional	exogenous wage, FE	firm-level, panel, survey	1982-2003
Molnar and Taglioni (2007)	short-/long-run, conditional	reduced-form, ex/endogenous, FE	industry-level, panel, admin	1993-2003
Aguilar and Rendon (2008)	long-run, unconditional	reduced-form, ex/endogenous wage, no FE	firm-level, cross-section, survey	2004
Jacobi and Schaffner (2008)	intermediate-run, conditional	structural-form, exogenous wage, FE	industry-level, panel, admin	1999-2005

Table 2.6.3: continued

Study	Model specifics		Data	
	Theoretical model	**Empirical specification**	**Characteristics**	**Period**
Micevska (2008)	short-run, conditional	reduced-form, exogenous wage, FE	firm-level, panel, admin	1994-1999
Onaran (2008)	short-/long-run, conditional	reduced-form, ex/endogenous wage, FE	industry-level, panel, admin	1999-2004
Godart et al. (2009)	short-run, conditional	reduced-form, exogenous wage, (no) FE	firm-level, panel, admin	1997-2005
Görg et al. (2009)	short-run, conditional	reduced-form, ex/endogenous wage, (no) FE	firm-level, panel, survey	1983-1998
Aguilar and Rendon (2010)	long-run, unconditional	reduced-form, ex/endogenous wage, no FE	firm-level, cross-section, survey	2004
Brixy and Fuchs (2010)	short-run, conditional	reduced-form, exogenous wage, FE	firm-level, panel, survey	2001-2006
Buch and Lipponer (2010)	short-run, conditional	reduced-form, endogenous wage, FE	firm-level, panel, admin	1997-2004
Freier and Steiner (2010)	intermediate-run, conditional	structural-form, exogenous wage, FE	industry-level, panel, admin	1999-2003
Hakkala et al. (2010)	short-run, conditional	reduced-form, endogenous wage, FE	firm-level, panel, admin	1990-2002
Hijzen and Swaim (2010)	intermediate-run, (un)conditional	reduced-form, ex/endogenous wage, FE	industry-level, panel, admin	1980-2002

Table 2.6.3: continued

Study	Model specifics		Data	
	Theoretical model	**Empirical specification**	**Characteristics**	**Period**
Senses (2010)	intermediate-run, conditional	structural-form, exogenous wage, FE	firm-level, panel, survey	1972-2001
Bohachova et al. (2011)	short-run, conditional	reduced-form, ex/endogenous wage, (no) FE	firm-level, panel, survey	2000-2008
Mitra and Shin (2011)	intermediate-run, (un)conditional	reduced-form, exogenous wage, (no) FE	firm-level, panel survey	2002-2008
Ayala (2012)	short-run, (un)conditional	reduced-form, ex/endogenous wage, FE	industry-level, panel, admin	1974-2009
Crino (2012)	intermediate-run, conditional	structural-form, exogenous wage, FE	industry-level, panel, admin	1990-2004
Kölling (2012)	intermediate-run, conditional	structural-form, exogenous wage, FE	firm-level, panel, survey	2000-2007

Table 2.6.4: Empirical studies without given or calculable standard errors

Study	Model specifics		Data	
	Theoretical model	**Empirical specification**	**Characteristics**	**Period**
Tinsley (1971)	short-/long-run, conditional	reduced-form, exogenous wage, no FE	industry-level, time-series, admin	1954-1965
Nadiri and Rosen (1974)	long-run, conditional	reduced-form, exogenous wage, no FE	industry-level, time-series, admin	1948-1974
Berndt and Wood (1975)	long-run, conditional	structural-form, endogenous wage, no FE	industry-level, time-series, admin	1947-1971
Kesselman et al. (1977)	long-run, conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1962-1971
Berndt and Khaled (1979)	long-run, conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1947-1971
Magnus (1979)	long-run, conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1950-1976
Clark and Freeman (1980)	long-run, conditional	reduced-form, exogenous wage, no FE	industry-level, time-series, admin	1950-1976
Anderson (1981)	long-run, conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1948-1971
Denny et al. (1981)	intermediate-/long-run, conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1949-1975
Morrison and Berndt (1981)	intermediate-/long-run, conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1952-1971

Table 2.6.4: continued

Study	Model specifics		Data	
	Theoretical model	Empirical specification	Characteristics	Period
Norsworthy and Harper (1981)	short-/long-run, conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1958-1977
Pindyck and Rotemberg (1983)	intermediate-/long-run, conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1948-1971
Nelson (1984)	long-run, conditional	structural-form, exogenous wage, no FE	firm-level, panel, survey	1953-1982
Nickell (1984)	long-run, conditional	reduced-form, exogenous wage, no FE	industry-level, time-series, admin	1958-1974
Carruth and Oswald (1985)	short-/long-run, unconditional	reduced-form, endogenous wage, no FE	industry-level, time-series, admin	1950-1980
Faini and Schiantarelli (1985)	long-run, (un)conditional	reduced-form, exogenous wage, FE	industry-level, panel, admin	1970-1979
Segerson and Mount (1985)	intermediate-run, unconditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1961-1977
Morrison (1986)	intermediate/long-run, conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1949-1980
Chung (1987)	long-run, conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1947-1971
Diewert and Wales (1987)	long-run, conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1947-1971

Table 2.6.4: continued

Study	Model specifics		Data	
	Theoretical model	**Empirical specification**	**Characteristics**	**Period**
McElroy (1987)	long-run, conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1947-1971
Baltagi and Griffin (1988)	long-run, conditional	structural-form, exogenous wage, FE	firm-level, panel, survey	1951-1978
Burgess (1988)	long-run, unconditional	reduced-form, endogenous wage, no FE	industry-level, time-series, admin	1963-1982
Daughety and Nelson (1988)	long-run, conditional	structural-form, exogenous wage, no FE	firm-level, panel, survey	1953-1982
Deno (1988)	long-run, unconditional	structural-form, exogenous wage, no FE	industry-level, panel, admin	1970-1978
Pencavel and Holmlund (1988)	long-run, unconditional	reduced-form, endogenous, no FE	industry-level, time-series, admin	1951-1983
Flaig and Steiner (1989)	long-run, conditional	reduced-form, exogenous wage, no FE	industry-level, time-series, admin	1963-1986
Kokkelenberg and Nguyen (1989)	long-run, conditional	structural-form, exogenous wage, no FE	firm-level, panel, survey	1972-1981
Nakamura (1990)	long-run, conditional	structural-form, exogenous wage, no FE	industry-level, panel, admin	1964-1982
Nickell and Symmons (1990)	long-run, unconditional	reduced-form, exogenous wage, no FE	industry-level, time-series, admin	1962-1984

Table 2.6.4: continued

Study	**Model specifics**		**Data**	
	Theoretical model	**Empirical specification**	**Characteristics**	**Period**
Blanchflower et al. (1991)	long-run, unconditional	reduced-form, exogenous wage, no FE	firm-level, cross-section, survey	1984
Bergström and Panas (1992)	long-run, conditional	structural-form, exogenous wage, no FE	industry-level, panel, admin	1963-1980
Bresson et al. (1992)	long-run, conditional	reduced-form, endogenous wage, FE	firm-level, panel, survey	1980-1983
Konings and Roodhooft (1997)	long-run, conditional	reduced-form, endogenous wage, FE	firm-level, panel, admin	1989-1994
FitzRoy and Funke (1998)	long-run, conditional	reduced-form, endogenous wage, FE	industry-level, panel, admin	1991-1993
Koebel (1998)	long-run, conditional	structural-form, exogenous wage, no FE	industry-level, panel, admin	1960-1992
Rottmann and Ruschinski (1998)	long-run, conditional	reduced-form, exogenous wage, FE	firm-level, panel, survey	1980-1992
Mellander (1999)	intermediate-/long-run, conditional	structural-form, endogenous wage, FE	industry-level, panel, admin	1985-1995
Ryan and Wales (2000)	long-run, conditional	structural-form, exogenous wage, no FE	industry-level, time-series, admin	1947-1971
Teal (2000)	long-run, conditional	structural-form, exogenous wage, FE	firm-level, panel, survey	1991-1995

Table 2.6.4: continued

Study	Model specifics		Data	
	Theoretical model	Empirical specification	Characteristics	Period
Flaig and Rottmann (2001)	intermediate-/long-run, conditional	structural-form, endogenous wage, no FE	industry-level, panel, admin	1968-1995
Bauer and Riphahn (2002)	long-run, conditional	reduced-form, endogenous wage, FE	industry-level, panel, admin	1977-1994
Cuyvers et al. (2005)	intermediate-/long-run, conditional	structural-form, endogenous wage, no FE	firm-level, panel, survey	1994-1998
Kölling and Schank (2002)	intermediate-run, conditional	structural-form, exogenous wage, FE	firm-level, panel, admin	1994-1997
Bruno and Falzoni (2003)	short-/intermediate-run, conditional	structural-form, endogenous wage, FE	industry-level, panel, survey	1982-1994
Barba Navaretti et al. (2003)	long-run, conditional	reduced-form, exogenous wage, FE	firm-level, panel, admin	1993-2000
Bernal and Cardenas (2004)	intermediate-/long-run, conditional	structural-/reduced-form, exogenous wage, no FE	industry-level, time-series, survey	1976-1991
Mondino and Montoya (2004)	long-run, conditional	reduced-form, ex/endogenous wage, FE	firm-level, panel, survey	1990-1996
Arnone et al. (2005)	long-run, (un)conditional	reduced-form, endogenous wage, FE	firm-level, panel, survey	1998-2002
Harrison and McMillan (2006)	intermediate-run, conditional	structural-form, exogenous wage, FE	firm-level, panel, survey	1982-1999

Table 2.6.4: continued

Study	Model specifics		Data	
	Theoretical model	**Empirical specification**	**Characteristics**	**Period**
Benito and Hernando (2007)	long-run, conditional	reduced-form, endogenous wage, FE	firm-level, panel, survey	1985-2000
Addison et al. (2008)	intermediate-run, conditional	structural-form, exogenous wage, FE	firm-level, panel, admin	1993-2002
Benito and Hernando (2008)	long-run, conditional	reduced-form, endogenous wage, FE	firm-level, panel, survey	1985-2001
Micevska (2008)	long-run, conditional	reduced-form, exogenous wage, FE	firm-level, panel, admin	1994-1999
Onaran (2008)	short-run, conditional	reduced-form, endogenous wage, FE	industry-level, panel, admin	1999-2004
Brixy and Fuchs (2010)	long-run, conditional	reduced-form, exogenous wage, (no) FE	firm-level, panel, survey	2001-2006
Buch and Lipponer (2010)	long-run, conditional	reduced-form, endogenous wage, FE	firm-level, panel, admin	1997-2004
Hijzen and Swaim (2010)	short-/long-run, conditional	reduced-form, ex/endogenous wage, FE	industry-level, panel, admin	1980-2002
Muendler and Becker (2010)	intermediate-run, conditional	structural-from, exogenous wage, FE	firm-level, panel, survey	1996-2001
Peichl and Siegloch (2012)	long-run, conditional	structural-form, exogenous wage, no FE	firm-level, panel, admin	1996-2007

Table 2.6.4: continued

Study	Model specifics		Data	
	Theoretical model	Empirical specification	Characteristics	Period
Sala and Trivin (2012)	long-run, (un)conditional	reduced-form, ex-/endogenous wage, FE	industry-level, panel, admin	1964-2007

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Chapter 3

The Effects of Exporting on Labor Demand*

3.1 Introduction

The worldwide volume of exports has dramatically increased over recent decades. Although the total benefits of globalization are believed to exceed the losses, most scholars assume that globalization increased the responsiveness and hence the vulnerability of employment to shocks (see Rodrik (1997), for an early argument). While (accelerating) international trade is a relatively modern phenomenon, a theoretical mechanism explaining more elastic employment responses to wage shocks has long been known: one of the Hicks-Marshall laws of derived demand states that the unconditional, long-run own-wage elasticity of labor demand is higher, ceteris paribus, the higher the price elasticity of product demand (Hamermesh, 1993). Exporting firms have been shown to face destination-specific price elasticities of product demand, which are decreasing in per-capita income of the destination country (Markusen, 2013; Simonovska, 2015). Hence, exporters in high-income countries are exposed to an overall more price elastic product demand compared to a firm that only serves the domestic market. As a consequence, exporters should also face a higher elasticity of labor demand.

* This chapter is based on a joint work with Andreas Peichl and Sebastian Siegloch, circulating as Lichter et al. (2014).

Despite its importance, the effect of firms' export behavior on the elasticity of labor demand has not been explicitly investigated to date. In this paper, we explore this relationship using administrative linked employer-employee data from Germany. To theoretically corroborate our hypothesis, we follow Krishna et al. (2001), who show that the implication of the Hicks-Marshall law of demand holds true in a model with firms exhibiting some price-setting power. Allowing for non-homothetic consumer preferences across countries, as in Markusen (2013), more elastic product demand for exporting compared to non-exporting firms transmits into higher own-wage elasticities for firms engaged in exporting.[1] Empirically, we use German administrative linked employer-employee data from 1996 to 2008 and industry-level data on trade flows to test our hypothesis and explicitly analyze the effect of exporting at both the extensive and intensive margins on the elasticity of labor demand. Focusing on Germany holds particular interest in this context, given that the German economy is heavily reliant on exports, with the export share on national GDP amounting to around 50% and around one-quarter of all jobs depending on exporting (Yalcin and Zacher, 2011). Moreover, Germany is a high-income country with trade ties to many low- and medium-income countries, thus making it a suitable candidate to test our theoretical predictions.

Indeed, we find that exporting at both the extensive and intensive margins has a positive effect on the absolute value of the unconditional own-wage elasticity of labor demand: The mean elasticity of labor demand for exporting plants is around one-third higher than the mean elasticity for non-exporters (-0.66 vs. -0.47). Our results are not driven by selection into exporting as we control for firm fixed effects and find a similar effect of exporting (at the intensive margin) when limiting the analysis to exporting firms only. Moreover, we show that our results are robust to endogeneity concerns by applying an instrumental variables strategy in the spirit of Autor et al. (2013), using U.S. exports to China as an instrument. Relying on industry-level data on export flows by destination countries, we further demonstrate that exporting increases the elasticity of labor demand for those firms that export a relatively large share of their output to low- and

[1] When speaking of higher/larger or lower/smaller own-wage elasticities, we refer to absolute values throughout the paper. Hence, a higher wage elasticity means 'more negative' and thus de facto a wage elasticity with a lower value.

medium income countries. In turn, we find no effect of exporting on the elasticity of labor demand in case exports are primarily destined for high-income markets, i.e., markets resembling the domestic one. Our findings hold true for single-plant firms, whereas exporting has no effect on the elasticity of labor demand of plants belonging to a multi-plant firm.[2] Given that exporting is usually the first step when becoming an international actor in the product market, these results suggest that multi-plant firms have accommodated export-induced volatility by adjusting its production processes and structure. When accounting for worker heterogeneity, we further find that medium-skilled workers are mostly affected by exporting, whereas demand for high-skilled labor does not respond to exporting behavior. These findings are in line with routine-based technological change, which has been shown to hit medium-skilled workers in particular. In line with our hypothesis, we further verify that the results are not due to differences in the conditional elasticity of labor demand, given that estimates conditional on output are not statistically different for exporting and non-exporting plants. We take these results as suggestive evidence in favor of our proposed mechanism: different scale effects for exporters and non-exporters drive differences in unconditional labor demand elasticities.

Overall, we add to the existing literature in three ways. First, we propose and verify an important mechanism of how exporting behavior – a central element of globalization – affects workers in the national labor market through higher labor demand elasticities. This channel is relevant for both theoretical models of international trade and policy analysis. For example, with the optimal minimum wage policy depending on the actual size of the (low-skilled) wage elasticity of labor demand (Lee and Saez, 2012), optimal policy interventions might be different in trade-exposed and trade-sheltered sectors. Second, our study adds to the growing literature on the characteristics of exporting firms. It has been established that exporting firms differ considerably from those merely serving the domestic market (see Bernard et al. (2007) for an overview). Nonetheless, it is important to establish a causal interpretation for these differences, as the decision to export is clearly endogenous (Bernard and Jensen, 1995; Clerides et al., 1998). Therefore, we explicitly address firms' selection into exporting and endogeneity concerns in our empirical analysis by applying a firm fixed effects, instrumental variables estimator.

[2] Note that labor demand is generally more elastic in multi-plant plants.

Finally, to the best of our knowledge, this is the first study on globalization and (the elasticity of) labor demand to use administrative linked employer-employee data. In recent decades, the literature has moved from using country- to industry- and firm-level data. By using administrative linked employer-employee micro-level panel data, we are able to base our estimations on a rich set of establishments and their employees, thus analyzing differential effects of exporting on heterogeneous types of plants and workers.[3]

The remainder of this paper is structured as follows. Section 3.2 provides a discussion of the related literature, focusing on the effects of globalization on the elasticity of labor demand, as well as differences between exporting and non-exporting plants. We subsequently present the theoretical mechanism and empirical model in Section 3.3. Section 3.4 describes the dataset used in our analysis and provides descriptive evidence on the plants' export behavior and performance. We present and discuss our empirical results in Section 3.5, placing particular emphasis on the issue of endogeneity and heterogenous effects of exporting for different types of plants and workers, before Section 3.6 concludes.

3.2 Related literature

We combine two broad strands of related literature in our paper: studies analyzing (i) the effects of globalization on the elasticity of labor demand; and (ii) the differences between exporting and non-exporting firms, as well as the causal effect of exporting on firm behavior.

The analysis of different features of globalization and their corresponding effects on labor demand has attracted much attention in the literature. While Slaughter (2001) shows that (non-production) production labor has (not) become more elastic in manufacturing industries over time in the U.S., he finds only weak evidence for a direct effect of trade. Exploiting exogenous variation due to trade policy reforms in low- and middle-income countries, several studies analyze the causal effect of trade liberalization on the elasticity of labor demand. Empirical

[3] Note that the dataset focuses on the establishment rather than the aggregate, namely the firm, and that the terms "plant" and "establishment" are used interchangeably in this paper. The dataset yet allows us to distinguish between single- and multi-plant firms in our analysis.

evidence is mixed, with Krishna et al. (2001) as well as Fajnzylber and Maloney (2005) finding no significant empirical link between trade liberalization and the elasticity of labor demand, whereas Hasan et al. (2007) and Mitra and Shin (2012) show that corresponding reforms in India and South Korea rendered the demand for labor more elastic.[4] Focusing on key aspects of globalization, several studies analyze the labor demand effects of firms' decision to outsource production processes, with the results suggesting that labor demand elasticities for (un-)skilled workers increase (decrease) (Hijzen et al., 2005; Senses, 2010; Hijzen and Swaim, 2010), albeit not in every country (Fajnzylber and Fernandez, 2009). Other studies investigate whether labor demand elasticities differ between multinational and domestic firms, yet no conclusive evidence has been found.[5]

Regarding the second strand of the literature, a variety of stylized facts has been established concerning the differences between exporting and non-exporting firms. Among others, exporting firms are larger in terms of both output and employment, more productive and pay higher wages than comparable non-exporting firms (see, for example, Bernard and Jensen, 1995; Bernard et al., 2007).[6] However, most differences do not stem from the mere act of exporting goods to foreign markets. For example, Clerides et al. (1998) and Bernard and Jensen (1999) show that only the most productive firms select into exporting, whereas no significant productivity gains occur after entering the export market.[7] It has further been established that exporting firms' prices are destination-specific, with Manova and Zhang (2012) showing that firms charge higher prices for the same product in richer and less

[4] Clearly, these studies are related to our work as trade liberalization increases firms' opportunities to sell their goods abroad, among others. However, the respective studies do not explicitly derive the effect of exporting on the elasticity of labor demand, but rather the overall effect of trade openness. Only Mitra and Shin (2012) analyze the interaction effects of trade liberalization reforms, importing and exporting behavior to some extent.

[5] Evidence ranges from findings on higher absolute own-wage labor demand elasticities for multinational compared to domestic firms (Fabbri et al., 2003; Görg et al., 2009; Hakkala et al., 2010), no significant differences (Buch and Lipponer, 2010) to less elastic demand for labor by multinationals (Barba Navaretti et al., 2003).

[6] For Germany, Schank et al. (2007) – controlling for observable and unobservable worker and firm characteristics – find wages to slightly increase with the firm's export share in total sales. Wagner (2007) reports productivity differences between exporting and non-exporting firms.

[7] Aw et al. (2000) and Delgado et al. (2002) find similar evidence; however, Van Biesebroeck (2005) and De Loecker (2007) report productivity gains from exporting for Sub-Saharan African manufacturing firms and Slovenian firms during the transition from a plan to market economy, respectively.

remote countries, among others. Simonovska (2015) relates country-specific prices to per-capita income differentials, with prices for the same product being higher in richer and less price elastic countries.

Related to our work, recent studies have investigated the relationship between the firm's export behavior and volatility in sales and employment. Using panel data on French manufacturing firms, Vannoorenberghe (2012) shows that firms' sales volatility increases with the export share. Nguyen and Schaur (2012) find similar evidence for Danish firms, yet show that the overall higher sales volatility for exporting rather than non-exporting firms is mainly driven by firms that do not continuously export. Focusing on employment, Kurz and Senses (2015) find a non-monotonic effect of exporting on the volatility of employment for U.S. manufacturing firms. We add to this evidence by providing an in-depth analysis of plant-level sources of employment volatility, with the own-wage elasticity of labor demand serving as an important proxy for employment volatility and wage pressure. Moreover, by focusing on the elasticity of labor demand, we are further able to propose a new channel determining the effects of exporting on labor demand, while accounting for endogeneity concerns in the empirical analysis.

3.3 Theoretical background and empirical model

In order to derive our hypothesis, we follow Krishna et al. (2001) and model firms' demand for labor in a monopolistic competitive product market setting, assuming that there are no strategic interactions between firms. Firm i maximizes profits by selling its product at either the domestic or at foreign markets, facing product demand given by:

$$p_i = \theta \overline{p} Q_i^{-\frac{1}{\epsilon_i}}, \tag{3.3.1}$$

with p_i denoting the price charged by firm i, $\overline{p}$ the average global product price, θ a scaling factor, Q_i the firm's output, and ϵ_i the price elasticity of product demand.[8] In line with Markusen (2013), we assume that consumer preferences are non-homothetic across countries, such that the price elasticity of product demand

[8] As noted by Krishna et al. (2001), this framework approximates a setting with a large number of varieties in the product market, where each firm is an infinitesimal player but has some power concerning the pricing of its product.

is country-specific and decreasing in per-capita income.[9] With each firm serving its domestic and different foreign markets to a varying extent, the elasticity of product demand becomes firm-specific, given by the sales-weighted average of the country-specific price elasticities of product demand. Firms located in high-income countries (such as Germany) and only serving their domestic market should thus face a less price elastic demand for their products compared to those that export some share of their output to foreign destinations, and to low- and medium-income countries in particular.

Assuming the production function to be Cobb-Douglas in variable inputs, $Q_i = \prod_{k=1}^{n} V_{ki}^{\alpha_k}$, it can be shown that – in line with the Hicks-Marshall law of derived demand – the absolute value of the elasticity of labor demand (η_{lli}) increases with the price elasticity of product demand[10]:

$$\frac{\partial \left|\eta_{lli}\right|}{\partial \epsilon_i} = \frac{\alpha_l}{\epsilon_i^2 \left[1 - \left(1 - \frac{1}{\epsilon_i}\right)\left(\sum_{k=1}^{n} \alpha_k\right)\right]^2} > 0. \tag{3.3.2}$$

Hence, a more price elastic product demand faced by exporting compared to non-exporting firms translates into a higher own-wage elasticity of labor demand as already stipulated by the Hicks-Marshall law of derived demand.

The empirical set-up. In line with firms' maximization of profits, the empirical specification of the unconditional labor demand model is:

$$\ln l_{it} = \delta \ln w_{it} + \beta \ln w_{it} e_{it} + \gamma e_{it} + \boldsymbol{\beta} \mathbf{X'}_{it} + \eta_i + \boldsymbol{\varphi}_{jt} + \epsilon_{it}. \tag{3.3.3}$$

The term $\ln l_{it}$ denotes the logarithm of establishment i's overall employment at time t, $\ln w_{it}$ the inflation-adjusted log mean wage rate and e_{it} the respective export variable, defined by either the export status or the export share in total sales. $\mathbf{X_{it}}$ is a row vector of additional covariates, including log investments of the

[9] This assumption is in line with recent empirical evidence showing that the price elasticity of demand is decreasing with per-capita income and that (exporting) firms set higher prices in richer countries (Simonovska, 2015; Manova and Zhang, 2012).

[10] A more detailed derivation is provided in Appendix 3.7.3.

previous year[11], the share of intermediate inputs used in production and dummy variables indicating whether wages are set under some form of collective bargaining agreement (CBA) and whether the plant belongs to a multi-plant firm. We also include establishment fixed effects (η_i) as well as industry-year fixed effects, which are summarized by row vector $\boldsymbol{\varphi}_{jt}$; the error term is denoted by ε_{it}.

In addition to estimating equation (3.3.3) for total employment, it can be modified for N heterogenous types of labor as follows:

$$\ln l_{it}^s = \sum_{k=1}^{N}(\delta_{sk} \ln w_{it}^k) + \beta_s \ln w_{it}^s e_{it} + \lambda_s e_{it} + \boldsymbol{\gamma_s} \mathbf{X}'_{it} + \eta_i + \boldsymbol{\varphi}_{jt} + \epsilon_{ist} \quad \forall s. \quad (3.3.4)$$

The dependent variable becomes the log number of employees of labor type s, $\ln l_{it}^s$. We further control for the average wage of each skill group and interact the skill-specific log wage ($\ln w_{it}^s$) with the export variable. The remaining variables are defined as before.

As both models are estimated in logarithms, estimates of the overall and skill-specific own-wage elasticities of labor demand are given by:

$$\left.\frac{\partial \ln l_{it}}{\partial \ln w_{it}}\right|_e = \delta + \beta e_{it} \quad \text{and} \quad \left.\frac{\partial \ln l_{it}^s}{\partial \ln w_{it}^s}\right|_e = \delta_{ss} + \beta_s e_{it}, \qquad (3.3.5)$$

with βe_{it} and $\beta_s e_{it}$ representing the differential effect of exporting on the elasticity of labor demand, respectively. In turn, the own-wage elasticity of labor demand for firms only serving their domestic market is given by the parameters δ and δ_{ss}.

Estimation and identification. We estimate the overall labor demand model (equation (3.3.3)) by means of fixed effects OLS and instrumental variables. The set of demand equations for heterogeneous types of labor (equation (3.3.4)) is estimated by fixed effects OLS and SUR (Zellner, 1962), with the latter estimator explicitly accounting for the correlation of the error terms of each demand function within the same establishment. By only using within-establishment variation to identify the effects of exporting on labor demand, we account for time-invariant self-selection into exporting. Establishment fixed effects additionally control for

[11] As we do not observe capital prices, we assume that capital is quasi-fixed and thus control for the level of capital, measured by means of the log investments of the previous year.

unobserved time-invariant confounders such as plant location or product quality, which might affect both left- and right-hand side variables. Industry-specific shocks are captured by industry-year dummy variables.[12]

Regarding identification of the empirical models, we follow standard practice (see, for example, Hijzen and Swaim, 2010; Senses, 2010) and assume that the individual establishment faces perfectly elastic labor supply, such that wages are exogenously given for the individual plant and shifts in labor supply, measured by means of changes in the wage rate, trace out the labor demand curve (Slaughter, 2001).[13] However, the establishment's export behavior and its demand for labor might depend on unobserved *time-varying* plant-level factors, notably productivity gains, which are not captured by plant fixed effects. Hence, the establishment's export share in total sales (e_{it}), as well as the corresponding interaction term with the wage rate ($\ln w_{it}e_{it}$), might be endogenous, which could bias our estimates.[14] We explicitly account for this source of endogeneity by applying an instrumental variables (IV) strategy. Conceptually, we follow Autor et al. (2013), who instrument U.S. imports of Chinese goods by changes in other high-income countries' imports stemming from China.[15] We adjust their approach to our research ques-

[12] As highlighted below, we account for endogeneity concerns regarding the plants' export behavior by using 2-digit industry-level trade flows from the U.S. to China as an instrument. In order to simultaneously account for industry-specific trends in the IV approach, we aggregate industries within the manufacturing sector, differentiating cars, steel, durables, food and non-durables. Note that the reported OLS results are robust to the inclusion of two-digit industry-year fixed effects.

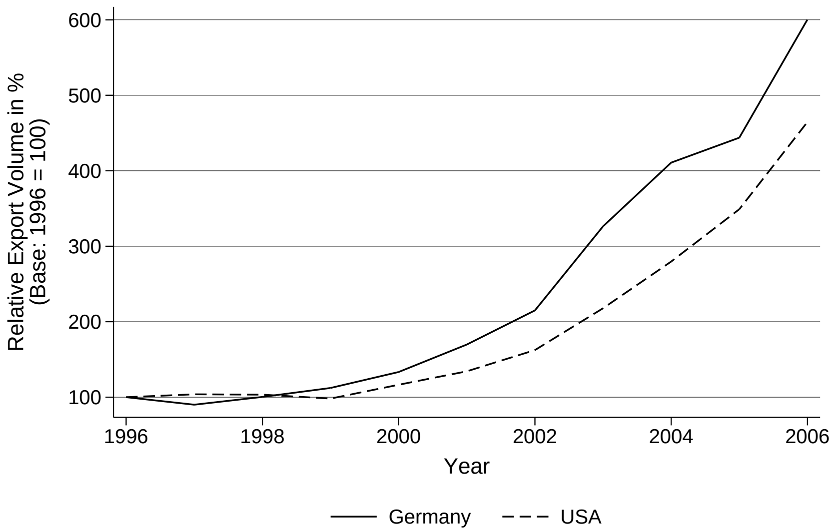
[13] Hamermesh (1993) argues that the validity of this identifying assumption depends on the level of aggregation in the data. As our analysis is based upon establishment-level data, simultaneity bias arising from incorrectly assuming perfectly elastic labor supply should only reflect minor concern in our study. We note that a common method to deal with endogeneity concerns in labor demand models has been the use of lagged values of the wage rate as an instrument. However, given that the "use of lagged endogenous variables as instruments is problematic if the equation error or omitted variables are serially correlated" and hence relies on "atheoretical and hard-to-assess assumptions about dynamic relationships" (Angrist and Krueger, 2001, p.76f.), we do not apply this procedure. Besides these studies, to the best of our knowledge, all other existing estimates of labor demand elasticities at the plant-level rely on the assumption of exogenous wages for identification (see also Senses, 2010).

[14] Imagine unobserved gains in productivity causing a higher export share and higher level of employment. For a given wage rate, this would induce an upward bias of the interaction term between wages and the export share. Therefore, accounting for this sort of endogeneity should lead to more negative estimates of the interaction term. Consequently, our fixed effects results serve as lower bound estimates.

[15] Note that Dauth et al. (2014) also employ a similar instrumental variables strategy when

tion by instrumenting the individual German establishment's export share in total sales with the corresponding industry's value of U.S. exports (in logs) destined for China. Given that China's demand for foreign goods should similarly affect U.S. and German plants, there is a high correlation between German and U.S. export shares to China as shown in Figure 3.3.1.[16] Arguably, U.S. industry-level ex-

Figure 3.3.1: Overall U.S. and German exports to China



Notes: The figure plots the relative increase in export flows from Germany and the U.S. to China from 1996 to 2006. The corresponding trade data are provided by the United Nations Statistics Division.

port volumes to China are not correlated with establishment-specific productivity gains in German establishments and should affect employment decisions only via the establishments' export behavior. We derive the instrument at the two-digit industry level (22 industries within the manufacturing sector), using yearly UN Comtrade data provided by the United Nations Statistics Division (UNSD).[17] We instrument both the export share as well as the interaction term of the export share and the wage rate by following the procedure suggested in Wooldridge (2010). As

analyzing the effects of trade integration on local German labor markets.

[16] Table 3.7.1 in the Appendix shows the correlations at the industry level.

[17] Information are given at the HS Classification level and transformed to the ISIC level using UNSD correspondence tables.

the instrument is derived at the 2-digit industry-level, we cluster standard errors accordingly.[18]

3.4 Data and descriptive statistics

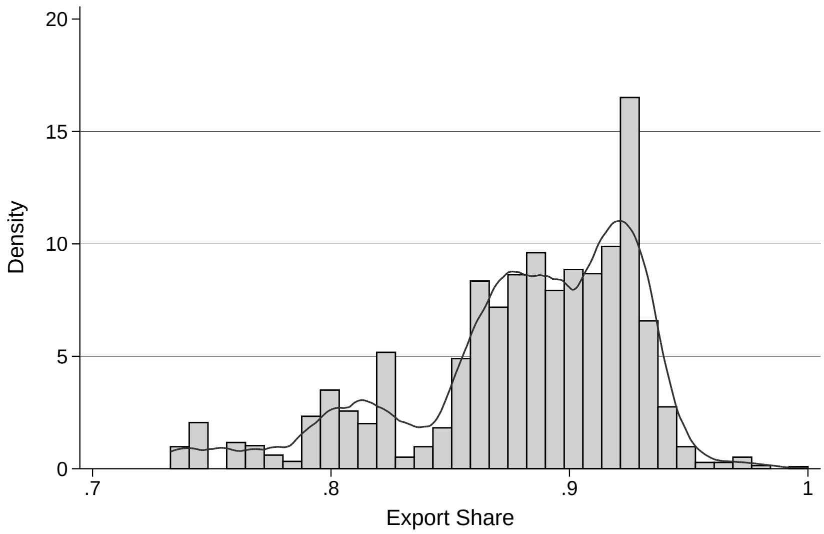
3.4.1 Data sources

For the purpose of our study we use administrative linked employer-employee data (*LIAB*) from Germany, provided by the Institute of Employment Research (IAB)[19], and link it with industry-level export data from UN Comtrade and the German Federal Statistical Office. As noted before, the case of Germany holds particular interest as its economy heavily depends on the export of goods and services, the export share in national GDP (approximately 50%) being considerably higher compared to most other developed countries.[20] Moreover, although most exports are destined for high-income countries, industry-level exports to medium- and low-income countries range between 5 and 25% (see Figure 3.4.1). Germany's strong reliance on exporting is further reflected by the fact that around one-quarter of all jobs depend either directly or indirectly on exports (Yalcin and Zacher, 2011).

Utilizing linked employer-employee data is crucial for our study, as we need to observe both individual-specific variables such as employees' occupations, qualifications and wages, as well as establishment information on output or export intensity to analyze the effects of exporting on the elasticities of labor demand for heterogeneous types of workers and plants. The underlying *employee data* is a two percent random sample of the administrative employment statistics of the German Federal Employment Agency, which covers all employees paying social security contributions (payroll taxes) or receiving unemployment benefits.[21] Among others, the dataset comprises detailed information on the individuals' qualification and occupation, their employment type (full-time, part-time or irregular employment), as well as their daily wage, right-censored at the upper earnings threshold

[18] Note that results remain statistically significant when clustering at the establishment level.
[19] See Alda et al. (2005) for detailed information on this dataset.
[20] See Figure 3.7.1 in the Appendix for a cross-country comparison.
[21] Thus, the dataset does not cover self-employed or civil servants as they are not subject to social security contributions.

Figure 3.4.1: Industry export shares to high-income countries



Notes: The figure plots the distribution of export shares to high-income countries across different industries over time. Country-specific trade flows are provided by the Federal Statistical Office. Information on per-capita income by destination country is obtained from the World Development Indicators (World Bank).

of social security contributions. In turn, the *IAB establishment panel* is a representative, stratified, random sample of German establishments with at least one employee liable to social security. As the name indicates, the dataset focuses on the establishment rather than the aggregate, namely the firm. It has covered West and East German establishments since 1996 and contains various information on the establishments' business and employment structure, including data on investments, turnover, staff and the export share in total sales.

Following common practice, we restrict our analysis to the manufacturing sector, given that it accounts for the majority of Germany's total exports and displays substantial heterogeneity in terms of employment, export intensity and output.[22] To account for heterogeneous effects of trade for differently skilled workers, we distinguish between low-, medium- and high-skilled workers. High-skilled employees hold either university, polytechnical or college degrees, whereas medium-skilled workers have either completed vocational training or obtained the highest Ger-

[22] Helpman et al. (2012) reason for substantial heterogeneity in Brazilian manufacturing firms, which we also find for the German case (see Table 3.4.2).

man high school diploma (Abitur). By contrast, unskilled workers have neither completed vocational training nor obtained the Abitur. We adjust all monetary variables for inflation, notably wages and output, relying on the German consumer price index obtained from the German Federal Statistical Office. Our sample spans the period of 1996 to 2008 and ultimately comprises 7,871 establishments, which are observed 3.24 times on average during this period. This amounts to 25,550 establishment-year and around 7 million worker-year observations.

3.4.2 Exporting and plant characteristics

It has been well established that exporting firms differ from non-exporting firms in many aspects. Thus, we present descriptive plant characteristics for five types of establishments: plants that (i) always export; (ii) never export; (iii) enter the export market; (iv) stop exporting goods; and (v) change the export status more than once within the sample.

Overall, our data confirms that the establishment's decision to engage in exporting is a rather long-term choice, given that 86.4% of the plants in our sample do not change the export status. In turn, 5.0 (3.4)% of the plants covered enter (exit) the export market, and around 5.2% export discontinuously. Despite the observed persistence in the plants' export status, the data displays considerable variation at the intensive margin of exporting, i.e., at the plants' export share in total sales.

Table 3.4.1: Variation in plants' export shares in total sales

Plant type	Always exporting	Never exporting	Enter exporting	Stop exporting	Discontinuous exporting
Number of plants	3,434	3,368	394	263	412
Export share					
Mean value (in %)	37.77	–	14.08	9.24	8.74
Mean value (in %), when exporting	37.77	–	23.81	18.21	16.72
Between-plants variation (std. deviation)	24.41	–	15.43	11.28	12.45
Within-plant variation (std. deviation)	8.50	–	14.50	11.78	11.13

Notes: This table provides information on the number of distinct types of plants considered in the analysis and indicates the respective export intensities of these plant types.

Table 3.4.1 presents the mean export share both conditional and unconditional on exporting, as well as the between- and within-plant variation. As expected, the export share in total sales is highest for continuously exporting plants (around 38%) and lower for plants that enter (around 24%) or exit the export market (around 18%) in the sample period, or export discontinuously (around 17%). In terms of variation, the data show that the export share in total sales substantially differs between plants and within the establishment over time. Between-plant variation in the export share is particularly strong for always-exporting plants, whereas within-plant variation in the export share is higher for those plants that change their export status.[23]

Table 3.4.2: Exporting and plant characteristics

Plant type	Always exporting	Never exporting	Enter exporting	Stop exporting	Discontinuous exporting
Number of plants	3,434	3,368	394	263	412
Number of workers					
Mean value	504.86	53.06	221.26	200.72	236.55
Between-plants variation (std. deviation)	1,493.29	179.36	627.22	333.54	711.85
Within-plant variation (std. deviation)	173.11	24.78	39.08	65.52	120.46
Skill composition of workforce					
High-skilled workers (mean)	0.102	0.036	0.080	0.065	0.070
Medium-skilled workers (mean)	0.718	0.857	0.776	0.781	0.774
Low-skilled workers (mean)	0.180	0.107	0.144	0.154	0.156
Average monthly wage (in logs)					
Mean value	7.905	7.550	7.741	7.726	7.731
Between-plants variation (std. deviation)	0.313	0.356	0.324	0.345	0.308
Within-plant variation (std. deviation)	0.049	0.070	0.054	0.064	0.060
Performance measures					
Output (in logs)	17.17	14.11	16.00	15.71	15.86
Investments per worker	27,388	9,767	14,425	14,665	9,907

Notes: The table provides descriptive statistics for the five distinct types of plants considered in the analysis. All monetary values are given in 2008 Euros.

Table 3.4.2 further shows that, according to our expectations, plant characteristics differ by export involvement. In line with the literature, our data show that exporting plants are – on average – larger in terms of both output and employment, pay higher wages and have higher investment rates per worker. Regarding workforce characteristics, continuously exporting plants are considerably larger (mean: 505) than all other types of establishments considered. Plants that never

[23] Figure 3.7.2 in the Appendix plots the distribution of the export share in total sales in our sample. However, recall that identification of the labor demand model is only based upon within-plant variation over time.

export are rather small, with an average of 53 employees. Plants that start or stop exporting within our sample, as well as those switching their export status more than once, are similar in terms of employment and medium-sized, with the average number of workers ranging between 201 and 237. Regarding worker heterogeneity, the share of both low- and high-skilled workers increases with export involvement. For example, the average share of high-skilled workers in the total workforce increases from around 3.6% for non-exporting plants to 7.0% and 10.2% for discontinuous and permanent exporters, respectively.

When focusing on wages, the observed pattern prevails. Average wages are highest (lowest) in plants that always (never) export within our sample, whereas wages for the other types of establishments lie in-between. Between-plant variation in wages is considerable and similar across establishment types. Within-plant variation in wages is smaller, yet sizeable in absolute terms. In terms of plant performance, average output of always-exporting plants considerably exceeds the output of all other types of plants, while investments per worker are lowest for never-exporting plants in our sample.

3.5 Empirical results

We start by presenting the results for total labor demand in Table 3.5.1. Recall that each specification contains industry-year fixed effects, capturing aggregate and industry-specific shocks over time, as well as establishment fixed effects. The overall unconditional wage elasticity for non-exporting plants is -0.471 in specification (1), reflecting a reasonable magnitude for a static long-run elasticity (Hamermesh, 1993; Lichter et al., 2015). More interestingly, we find that exporting has the expected effect on the own-wage elasticity of unconditional labor demand, as indicated by the negative and significant interaction term of the log wage and the export dummy: labor demand of exporting plants is considerably more elastic than labor demand of non-exporting firms. We next turn to the intensive margin, substituting the export dummy variable with the establishment's export share in total sales. The results provided in column (2) mirror those presented in column (1), with the establishment's export share having a positive and significant effect on the (absolute value of the) overall own-wage elasticity of labor demand. The own-wage elasticity of labor demand for the mean exporting establishment is -0.657, compared to -0.469 for a comparable non-exporting establishment.[24]

Selection into exporting. In the next two specifications, we check the robustness of the baseline results with respect to plants' selection into exporting. In column (3), we restrict the sample to exporting plants only, thus focusing on the potentially selected group of exporting establishments. We find that the wage elasticity increases compared to model (2), although we cannot reject that the two coefficients are identical. Moreover, the interaction term remains statistically significant, which indicates that labor demand elasticities increase with the exporting intensity, even within the group of exporters. This finding suggests that our effects are not driven by selection *into* exporting. Our model in column (4), within which we restrict the sample to establishments that do not change their export status over the observation period, corroborates this conjecture. Again, we find similar estimates compared to those of specification (2).

Instrumental variables. As discussed in Section 3.3, our fixed effects estimates presented before would be biased if both the establishment's employment decision and export behavior were affected by unobserved time-varying factors, such as plant-specific productivity shocks. In the following, we thus present the results from our IV approach. Recall that, in the spirit of Autor et al. (2013), we instrument the establishment's export share with the corresponding U.S. industry's export value to China. Whereas we reason that U.S. industry's trade volumes *are* correlated with the respective establishments' export shares, given that both are driven by China's demand for foreign goods, the instrument *is not* correlated with establishment-specific productivity shocks.

The instrument is available at the two-digit industry level, covering 1996 to 2006. Hence, to enable the comparison of point estimates, we first present baseline fixed effects results on the slightly restricted sample in column (5). In line with our previous results, the export share in total sales has a positive effect on the own-wage elasticity of unconditional labor demand. Moreover, the estimates in column (5) are of similar magnitude compared to those of the baseline estimation

[24] Conditional on exporting, the mean share of exports in total sales is 33%.

using the full sample in column (2). We report our IV estimates in column (6), noting that our model is well identified: clustering standard errors at the level of the instrument, the Kleibergen-Paap test statistics suggest that the excluded instruments are relevant and not weak.[25] Moreover, we find that the point estimate of the interaction term of the establishment's export share and the wage rate becomes more negative when accounting for endogeneity, which suggests that our OLS estimates are biased towards zero. However, when comparing OLS and IV results, we cannot reject that the parameters of the interaction terms in columns (5) and (6) are statistically identical, suggesting that instrumenting might not be necessary after all.[26] Since the following analyses are quite demanding in terms of statistical power (as we add another layer of interactions), we focus on the more efficient OLS estimates.

Table 3.5.1: Labor demand & exporting: Fixed Effects & IV results

	All plants		Export Only	$\Delta Export = 0$	IV Sample (1996–2006)	
Dependent variable:	OLS	OLS	OLS	OLS	OLS	2SLS
Ln(employees)	(1)	(2)	(3)	(4)	(5)	(6)
Ln(wage)	-0.471***	-0.469***	-0.960***	-0.449***	-0.471***	-0.270**
	(0.104)	(0.102)	(0.181)	(0.116)	(0.118)	(0.129)
× Export Dummy	**-0.199*****					
	(0.053)					
× Export Share		**-0.0057*****	**-0.0031****	**-0.0086*****	**-0.0056*****	**-0.0196*****
		(0.0018)	(0.0016)	(0.0028)	(0.0020)	(0.0055)
Firm-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	25,550	25,550	16,961	19,984	21,245	21,245
No. of plants	7,871	7,871	4,503	6,802	7,051	4,553
Underidentification						11.18
Weak identification						65.35

Notes: Dep. variable: Ln(employees). All specifications include establishment and industry-year fixed effects. The constant is omitted for the ease of presentation. We provide the Kleibergen-Paap statistics for the under-identification and weak identification tests in column (6). Standard errors (in parentheses) are clustered at the establishment level in columns (1)–(5) and clustered at the 2-digit industry level in column (6). Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

[25] The corresponding first-stage regressions are given in Table 3.7.5 in the Appendix.

[26] In detail, we cannot reject that the parameters are identical at the 95% confidence level.

Export destinations. In order to further test the suggested theoretical mechanism, we analyze whether the estimated effect indeed depends upon the (mean) per-capita income level of the plants' export destination countries. As information on export destination countries are missing in the LIAB data, we make use of detailed 2-digit industry-level data provided by the German Federal Statistical Office and calculate the industry-level share of exports to high-income countries. On average, around 88% of the total German manufacturing exports are destined for high-income countries, with the share varying considerably by industry though. We exploit this variation across industries by interacting our effect of interest ($\ln w_{it}e_{it}$) with the respective industry's export share to high-income countries, which is categorized into quartiles. As shown in column (1) of Table 3.5.2, we indeed find evidence in favor of the proposed channel: exporting has a positive and significant effect on the elasticity of labor demand for those firms which export a relative large share of their output to low- and medium-income countries.[27] In turn, we find no effect of exporting on the elasticity in case exports are primarily destined for high-income markets, i.e., in case the export market largely resembles the domestic one in terms of purchasing power. When focusing on exporting plants only, we find the same qualitative results (see column (2)).

Plant heterogeneity. Thus far, we have established that exporting at both the intensive and extensive margins positively affects the absolute value of the elasticity of labor demand, controlling for selection into exporting and endogeneity concerns. We next investigate heterogenous effects of exporting for different types of plants. Precisely, we analyze differential effects of exporting due to the plants' structure (single-plant versus multi-plant firms) and coverage by a collective bargaining agreement (CBA). We distinguish single-plant firms from plants belonging to a multi-plant firm to address concerns that the estimated effect may be only driven by multi-plant firms, capturing differences in firm structure rather than the effect of exporting, as those firms may more easily shift production between plants located in domestic or foreign locations and thus have more elastic demand for

[27] Note that the number of observations decreases slightly compared to the baseline model as information on country-specific export flows are missing for some 2-digit industries in 2008. When replacing the missing information with industry-level means, the results remain unaffected.

Table 3.5.2: Labor demand & exporting: destinations and plant heterogeneity

	Export destinations		Plant heterogeneity	
Dependent variable:	OLS			
Ln(employees)	(1)	(2)	(3)	(4)
Ln(wage)	-0.510***	-0.964***	-0.660***	-0.432***
	(0.129)	(0.104)	(0.128)	(0.098)
× Export Share × High-Income Market Share (Quartile 1)	**-0.0066*****	**-0.0038****		
	(0.0019)	(0.0017)		
× Export Share × High-Income Market Share (Quartile 2)	**-0.0070*****	**-0.0043*****		
	(0.0019)	(0.0016)		
× Export Share × High-Income Market Share (Quartile 3)	**-0.0052****	**-0.0027**		
	(0.0023)	(0.0021)		
× Export Share × High-Income Market Share (Quartile 4)	**-0.0033**	**-0.0011**		
	(0.0028)	(0.0027)		
× Single Plant			**0.2290*****	
			(0.0778)	
× Export Share × Multi-plant			**-0.0038**	
			(0.0023)	
× Export Share × Single-plant			**-0.0062*****	
			(0.0019)	
× CBA				**-0.0603**
				(0.0520)
× Export Share × No CBA				**-0.0045****
				(0.0018)
× Export Share × CBA				**-0.0072*****
				(0.0024)
Firm-level controls	Yes	Yes	Yes	Yes
Industry × Year FE	Yes	Yes	Yes	Yes
No. of observations	25,189	16,727	25,550	25,550
No. of plants	7,806	4,468	7,871	7,871

Notes: Dep. variable: Ln(employees). All specifications include establishment and industry-year fixed effects. In column (2), the sample is restricted to exporting plants only. Standard errors (in parentheses) are clustered at the establishment level in columns (1)–(4). Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

labor. Moreover, we account for the plants' coverage by a CBA, as our assumption about wage exogeneity is corroborated in a setting in which plants are subject to CBAs, and unions may limit firms' adjustment in employment.

The results displayed in column (3) of Table 3.5.2 show that exporting affects labor demand elasticities of single-plant firms in particular. In turn, exporting has no effect on the elasticity of labor demand for plants belonging to a multi-plant firm, albeit the elasticity is larger for those plants in general. Given that exporting is usually the first step when becoming an international actor in the product market, these results suggest that (i) more price elastic product demand transmits to more elastic labor demand for single-plant firms, as suggested by the proposed mechanism, whereas (ii) multi-plant firms have accommodated to export-induced volatility in the product market by adjusting their production processes and structure. When accounting for the plants' coverage by collective bargaining, the results presented in column (4) in turn provide no evidence for differential effects of exporting due to collective bargaining; the elasticity of labor demand increases with the export share in total sales in both types of plants.

Worker heterogeneity. As it has been shown that globalization affects different types of workers to varying extents (cf. Section 3.2), we further investigate differential effects of exporting for heterogeneous types of labor, distinguishing between low-, medium- and high-skilled workers. Table 3.5.3 presents the corresponding results obtained from fixed effects SUR.[28] In general, we infer from columns (1), (3) and (5) that demand is least elastic for high-skilled workers, whereas the elasticity for low-skilled workers is highest in absolute terms.[29] Moreover, we find that exporting primarily affects medium-skilled workers, with the interaction term of the corresponding wage rate and the export share being negative and statistically significant (see column (3)). Our results also suggest that demand for low-skilled workers becomes more elastic with an increasing export share in total sales, al-

[28] We report the corresponding estimates obtained from fixed effects OLS in Table 3.7.6 in the Appendix of this paper. Given that we do not find significant different results for overall labor demand when applying fixed effects IV and OLS, we do not apply our IV strategy to the model of heterogeneous labor demand.

[29] Note that regression results in columns (1), (3), and (5) are based on one common specification using SUR.

Table 3.5.3: Labor demand & exporting: worker heterogeneity

	High-skilled		Medium-skilled		Low-skilled	
Dep. var: ln(employees)	(1)	(2)	(3)	(4)	(5)	(6)
Ln(high-skilled wage)	**-0.354*** **	**-0.241** **	0.0421	0.0560	-0.120*	-0.147*
	(0.100)	(0.095)	(0.045)	(0.043)	(0.067)	(0.084)
Ln(medium-skilled wage)	0.298*	0.223	**-0.443*** **	**-0.480*** **	-0.306	-0.341
	(0.165)	(0.168)	(0.125)	(0.125)	(0.199)	(0.217)
Ln(low-skilled wage)	0.112**	0.110*	0.106***	0.118***	**-0.457*** **	**-0.495*** **
	(0.0495)	(0.0629)	(0.0350)	(0.0391)	(0.0984)	(0.122)
Ln(high-skilled wage)* Export Share	**-0.0013**					
	(0.0018)					
Ln(high-skilled wage)* Export Share* CBA		**-0.0025**				
		(0.0021)				
Ln(high-skilled wage)* Export Share* No CBA		**0.0000**				
		(0.0025)				
Ln(medium-skilled wage)* Export Share			**-0.0031*** **			
			(0.0012)			
Ln(medium-skilled wage)* Export Share* CBA				**-0.0023***		
				(0.0013)		
Ln(medium-skilled wage)* Export Share* No CBA				**-0.0038** **		
				(0.0018)		
Ln(low-skilled wage)* Export Share					**-0.0023**	
					(0.0014)	
Kn(low-skilled wage)* Export Share* CBA						**-0.0035***
						(0.0018)
Ln(low-skilled wage)* Export Share* No CBA						**-0.0001**
						(0.0020)
Ln(high-skilled wage)* CBA		-0.208		-0.0264		0.0520
		(0.128)		(0.0608)		(0.0970)
Ln(medium-skilled wage)* CBA		0.151		0.0445		0.0564
		(0.130)		(0.0858)		(0.166)
Ln(low-skilled wage)* CBA		0.0023		-0.0279		0.0566
		(0.072)		(0.0460)		(0.138)
Export Share	0.0126	0.0010	0.0253***	0.0313**	0.0179	0.0016
	(0.0151)	(0.0203)	(0.0092)	(0.0139)	(0.0109)	(0.0150)
CBA	-0.0306	0.459	-0.0197	0.0647	-0.0169	-1.300
	(0.0206)	(0.900)	(0.0151)	(0.654)	(0.0316)	(1.217)
Export Share*CBA		0.0218		-0.0125		0.0260
		(0.0219)		(0.0143)		(0.0183)
Plant-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	13,414	13,414	13,414	13,414	13,414	13,414
Breusch-Pagan Test	4773.7	4782.3	4773.7	4782.3	4773.7	4782.3

Notes: All estimates are obtained by means of fixed effects SUR. Additional plant-level controls are: the share of intermediate inputs, log investments of the previous year and a dummy variable indicating whether the plant belongs to a multi-plant firm. The constant is omitted for the ease of presentation. Standard errors (in parentheses) are jackknife-cluster robust at the plant level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

beit the effect is not statistically significant at conventional levels.[30] Demand for high-skilled workers is not affected by exporting.[31]

In the preceding analysis, we found no evidence for differential effects of exporting on total labor demand due to collective bargaining. However, we additionally analyze differential effects for heterogenous types of labor, given that unions may cover different workers to varying extents, with the corresponding results presented in columns (2), (4) and (6).[32] Focusing on establishments without CBAs, we find clear evidence that exporting only affects the elasticity of demand for medium-skilled workers. The parameters of the interaction terms between the corresponding log wage rate, the export share and the collective bargaining dummy variable show that there is no effect of exporting on the elasticity for low- and high-skilled workers. This skill difference could be explained with routine tasks (which are more common for medium-skilled workers) being more affected by globalization and increasing competition (Acemoglu and Autor, 2011). In contrast, the observed pattern changes when considering plants covered by CBA. While exporting still renders demand for medium-skilled labor more elastic, the demand for low-skilled workers is now also affected (see column (6)). Given that the median union member is medium-skilled, it seems that unions mitigate the pressure from globalization on their members by sharing the burden of higher volatility more equally across the skill distribution.[33]

Conditional labor demand. Although our findings are in line with the proposed mechanism, the results do not provide direct evidence for the Hicks-Marshall law, given that higher unconditional own-wage elasticities for exporting compared to non-exporting plants may be due to differences in the price elasticity of product demand or the conditional elasticity of labor demand. Unfortunately, due to a

[30] The p-value for the interaction term of the low-skilled wage rate and the export share given in column (5) is 0.104.

[31] We argue that establishment fixed effects should account for overall differences in the quality and composition of the workforce. However, controlling for plants' composition of the workforce does hardly affect our results.

[32] The regression results presented in columns (2), (4) and (6) are based on one common specification using SUR.

[33] Note that there are no observable differences in the composition of the workforce for plants with and without CBA (see Table 3.7.2 in the Appendix).

lack of price data, we cannot estimate the direct effect of exporting on the price elasticity of product demand. In this section, we thus provide estimates of the conditional own-wage elasticity of labor demand for exporting and non-exporting plants to implicitly verify our proposed mechanism.

To estimate the conditional labor demand elasticity we depart from a static, structural model of firm behavior within which firms are assumed to minimize costs given a constant level of output. We specify costs (C) by means of the flexible Generalized Leontief cost function as given by Diewert and Wales (1987):

$$C = \sum_i \alpha_i w_i + \sum_i \sum_j \alpha_{ii} w_j^{0.5} w_j^{0.5} Y + \sum_i (\beta_{YYi} w_i) Y^2, \qquad (3.5.1)$$

with w denoting the input prices of the production factors and Y plants' output. Applying Shephard's lemma to equation (3.5.1), input-output ratios are given by:

$$R_i = \frac{\alpha_i}{Y} + \alpha_{ii} + \sum_{j \neq i} \alpha_{ij} \left(\frac{w_j}{w_i} \right)^{0.5} + \beta_{YYi} Y \quad \forall i. \qquad (3.5.2)$$

These ratios are estimated by fixed effects SUR, separately for exporting and non-exporting plants and allowing for non-constant returns to scale while imposing linear homogeneity in input prices. The conditional own-wage labor demand elasticity ($\overline{\eta}_{ii}$) for input i is subsequently calculated by means of:

$$\overline{\eta}_{ii} = -0.5 \frac{Y}{X_i} \sum_{j \neq i} \alpha_{ij} \left(\frac{w_j}{w_i} \right)^{0.5}, \qquad (3.5.3)$$

the term X_i denoting the level of input good i.

Table 3.5.4 reports the point estimates and standard errors for the conditional elasticity of total as well as low-, medium- and high-skilled labor demand. Focusing on total employment, we report very similar values for exporting (-0.13) and non-exporting establishments (-0.17). The same picture emerges when considering conditional demand elasticities for the three different groups of workers. Point estimates for the elasticity of medium- and low-skilled labor demand are similar for exporting and non-exporting plants, and differences not statistically significant. For high-skilled labor, point estimates suggest that labor demand is less

Table 3.5.4: Conditional labor demand elasticities

Conditional Own-Wage Elasticity	Non-exporting plants	Exporting plants
Overall labor demand	-0.17	-0.13
	(0.12)	(0.05)
High-skilled labor demand	-0.41	-0.03
	(0.50)	(0.16)
Medium-skilled labor demand	-0.14	-0.11
	(0.10)	(0.06)
Low-skilled labor demand	-0.42	-0.33
	(0.36)	(0.09)

Notes: This table provides estimates of conditional labor demand elasticities for exporting and non-exporting plants. Standard errors (in parentheses) are obtained from bootstrapping using 400 replications.

elastic for exporting compared to non-exporting plants. Despite standard errors being rather large and the difference not being significant[34], this finding could be interpreted in favor of Matsuyama (2007), who suggests that exporting firms face more skill-intensive tasks (e.g., by requiring workers with foreign language skills or experience in international business) than non-exporting firms, which should translate into higher demand for skilled labor, conditional on output. Overall, we take these findings as suggestive evidence for our proposed mechanism in favor of the Hicks-Marshall law of derived demand, with exporting plants having more elastic unconditional demand for labor compared to non-exporting plants due to more price elastic product demand.

3.6 Conclusion

In this paper, we show that globalization increases worker vulnerability by demonstrating that plants' exporting activity has a positive and significant effect on the absolute value of the own-wage elasticity of labor demand. With the price elasticity of product demand being country-specific and decreasing in per-capita income, exporting firms located in high-income countries are exposed to an overall more

[34] In line with Koebel et al. (2003) we report larger standard errors for the own-wage elasticities of those inputs that have a small share in production.

price elastic product demand than a comparable firm only serving its domestic market. Building upon the theoretical model of Krishna et al. (2001) and assuming non-homothetic consumer preferences across countries, we show that more elastic product demand for exporting compared to non-exporting firms should lead to higher own-wage elasticities of labor demand for firms engaged in exporting, in line with the Hicks-Marshall law of derived demand.

Indeed, our empirical results confirm that exporting at both the intensive and extensive margins has a positive and significant effect on the unconditional own-wage elasticity of labor demand. Using industry-level data on country-specific trade flows we further corroborate our proposed mechanism by showing that exporting only renders firms' labor demand more elastic in case a relative large share of their output is destined for low- or medium-income countries. Accounting for plant heterogeneity, we further find that exporting only affects the labor demand of single-plant firms. Regarding heterogenous effects across worker types, our estimates show that exporting affects the demand for medium-skilled labor in particular, thus suggesting that workers performing routine tasks are most affected by globalization (Acemoglu and Autor, 2011). Finally, we verify that our results are not due to differences in the conditional elasticity of labor demand, taking this as suggestive evidence in favor of our proposed mechanism.

Our findings have important policy implications. As it has been shown that optimal minimum wage policies depend on the wage elasticity of labor demand (Lee and Saez, 2012), optimal strategies may be different for trade-exposed and trade-sheltered sectors. The same applies to other policies increasing the wage costs of employers. In terms of future research, it would be interesting to revisit our results using comparable datasets, yet with more information on the country-specific export pattern of each plant or firm, as well as for a developing country with strong trade ties to high-income countries, where our proposed mechanism suggests reverse effects of exporting on the elasticity of labor demand.

3.7 Appendix

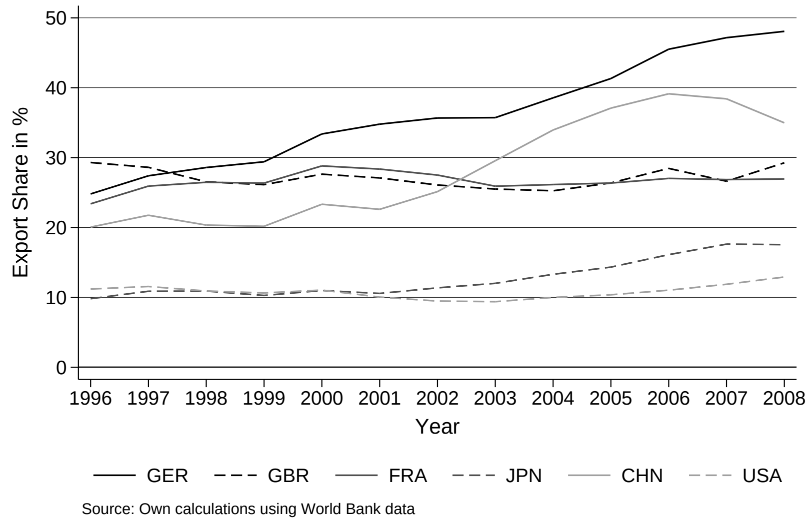
3.7.1 Descriptive statistics

Table 3.7.1: U.S. and German exports to China by industries

Industry	Correlation
Chemical Industries	0.982
Plastics/Rubber	0.911
Raw Hides, Skin, Leather	0.929
Wood and Wood Products	0.809
Textiles, Foodwear	0.964
Stone, Glas	0.955
Metals	0.991
Machinery	0.972
Transportation	0.782
Misc. Manufacturing	0.986

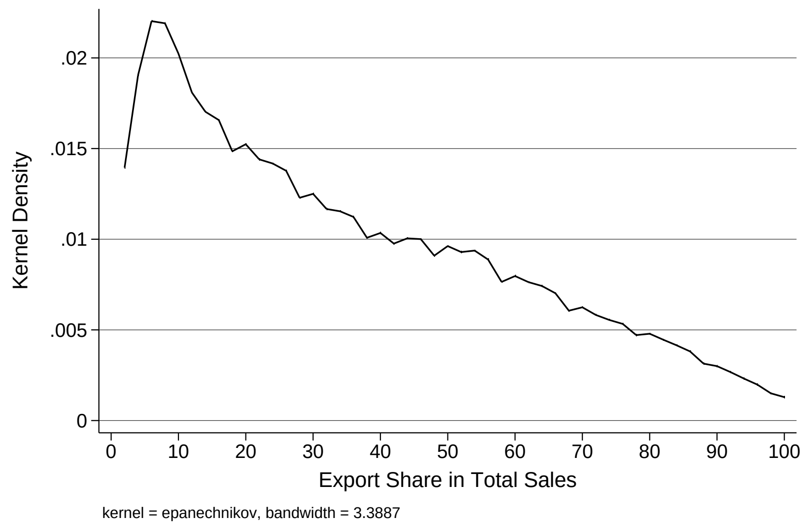
Notes: This table provides simple correlations for the industry-specific export flows (HS classification) of the U.S. and Germany to China. Correlations are based on data from the United Nations Statistics Division, 1996–2006.

Figure 3.7.1: Export share on national GDP



Notes: This figure shows the export share on national GDP for Germany, Great Britain, France, Japan, China and the U.S. from 1996 to 2008 using World Bank data.

Figure 3.7.2: Distribution of export shares across plants



Notes: The figure plots the distribution of the export share on total sales for firms covered in the estimation sample.

Table 3.7.2: Differences by plant types

Plant type	CBA/Exporting	No CBA/Exporting	CBA/Non-Exporting	No CBA/Non-Exporting
No. of employees	649.217	127.943	137.976	31.996
Share of worker				
High-skilled	0.101	0.092	0.041	0.043
Medium-skilled	0.712	0.755	0.836	0.847
Low-skilled	0.187	0.153	0.123	0.110

Notes: The table provides descriptive statistics – the mean number of employees and the mean skill composition – for the four different types of firms as indicated in the table head.

3.7.2 Additional regression results

Table 3.7.3: Full regression results of Table 3.5.1

	All plants		Export Only	Δ Export Status = 0	IV Sample (1996–2006)	
Dep. var:	OLS	OLS	OLS	OLS	OLS	2SLS
Ln(employees)	Col.(1)	Col.(2)	Col.(3)	Col.(4)	Col.(5)	Col.(6)
Ln(wage)	-0.471***	-0.469***	-0.960***	-0.449***	-0.471***	-0.270**
	(0.104)	(0.102)	(0.181)	(0.116)	(0.118)	(0.129)
× Export Dummy	**-0.199*** **					
	(0.053)					
× Export Share		**-0.0057*** **	**-0.0031** **	**-0.0086*** **	**-0.0056*** **	**-0.0196*** **
		(0.0018)	(0.0016)	(0.0028)	(0.0020)	(0.0055)
Export Dummy	1.579***					
	(0.409)					
Export Share		0.0454***	0.0246**	0.0677***	0.0443***	0.151***
		(0.0138)	(0.0123)	(0.0222)	(0.0156)	(0.0443)
CBA	0.0112	0.0119	0.0199	0.0273	0.0070	0.0063
	(0.0139)	(0.0140)	(0.0203)	(0.0189)	(0.0163)	(0.0107)
Ln(investments)	0.0347***	0.0347***	0.0425***	0.0330***	0.0349***	0.0364***
	(0.0032)	(0.0032)	(0.0043)	(0.0039)	(0.0037)	(0.0039)
Sh. intermediates	-0.0047	-0.0042	-0.0143	-0.0080	0.0017	0.0104
	(0.0140)	(0.0141)	(0.0179)	(0.0159)	(0.0156)	(0.0129)
Single-plant	-0.0006	0.0000	-0.0017	0.0050	0.0097	0.0101
	(0.0150)	(0.0150)	(0.0171)	(0.0157)	(0.0177)	(0.0151)
Industry × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	25,550	25,550	16,961	19,984	21,245	21,245
No. of plants	7,871	7,871	4,503	6,802	7,051	4,553
Underidentification						11.18
Weak identification						65.35
Endogeneity test (p-value)						0.041

Notes: Dep. variable: Ln(employees). All specifications include establishment and industry-year fixed effects. The constant is omitted for the ease of presentation. We provide the Kleibergen-Paap statistics for the under-identification and weak identification tests. Standard errors (in parentheses) are clustered at the establishment level in columns (1)–(5). Standard errors in column (6) are clustered at the 2-digit industry level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Table 3.7.4: Full regression results of Table 3.5.2

	Export destinations		Plant heterogeneity	
Dependent variable:	OLS	OLS	OLS	OLS
Ln(employees)	Col.(1)	Col.(2)	Col.(3)	Col.(4)
Ln(wage)			-0.660***	-0.432***
			(0.128)	(0.098)
× Export Share × High-Income Market Share	-0.0066***	-0.0038**		
(Quartile 1)	(0.0019)	(0.0018)		
× Export Share × High-Income Market Share	-0.0070***	-0.0043***		
(Quartile 2)	(0.0019)	(0.0016)		
× Export Share × High-Income Market Share	-0.052**	-0.0027		
(Quartile 3)	(0.0023)	(0.0021)		
× Export Share × High-Income Market Share	-0.0033	-0.0011		
(Quartile 4)	(0.0028)	(0.0027))		
× Single Plant			**0.2290*** **	
			(0.0778)	
× Export Share × Multi-plant			**-0.0038**	
			(0.0023)	
× Export Share × Single-plant			**-0.0062*** **	
			(0.0019)	
× CBA				**-0.0603**
				(0.0520)
× Export Share × No CBA				**-0.0045** **
				(0.0018)
× Export Share × CBA				**-0.0072*** **
				(0.0024)
CBA			0.0070	0.0063
			(0.0163)	(0.0107)
Ln(investments)			0.0349***	0.0364***
			(0.0037)	(0.0039)
Sh. intermediates			0.0017	0.0104
			(0.0156)	(0.0129)
Single-plant			0.0097	0.0101
			(0.0177)	(0.0151)
Industry × Year FE	Yes	Yes	Yes	Yes
No. of observations	25,189	16,727	25,550	25,550
No. of plants	7,806	4,468	7,871	7,871

Notes: Dep. variable: Ln(employees). All specifications include establishment and industry-year fixed effects. The constant is omitted for the ease of presentation. Standard errors (in parentheses) are clustered at the establishment level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Table 3.7.5: Instrumental variables regressions

(A) Regression for prediction of endogenous variable

Export share	
Ln(U.S. exports)	0.864*
	(0.445)
Ln(wage)	1.196
	(1.223)
CBA	0.231
	(0.337)
Ln(investments)	0.212**
	(0.091)
Sh. intermediates	1.699***
	(0.559)
Single-plant	-0.168
	(0.456)
Industry × Year	Yes

(B) First-stage estimation results from 2SLS

	Export Share	Ln(wage)* Export Share
Ln(wage)	-2.320**	-19.301**
	(1.001)	(7.928)
CBA	-0.229	-1.927
	(0.253)	(1.949)
Ln(investments)	-0.171*	-1.271*
	(0.086)	(0.674)
Sh. intermediates	-1.168**	-8.843**
	(0.428)	(3.375)
Single-plant firm	0.265	2.197
	(0.464)	(3.715)
$\text{Ln}(\widehat{\text{U.S. exports}})$	2.141***	8.628***
	(0.423)	(3.295)
$\text{Ln(Wage)*}\widehat{\text{Ln(U.S. exports)}}$	-0.001	1.014***
	(0.043)	(0.344)
Industry × Year	Yes	Yes
No. of observations	21,245	21,245
F-Test of excluded instruments	7.88	8.87

Notes: In Panel A (B) standard errors are clustered at the plant (two-digit industry) level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

Table 3.7.6: Effects by worker type: Fixed Effects results

Dep. var.: Ln(employees)	High-skilled		Medium-skilled		Low-skilled	
	(1)	(2)	(3)	(4)	(5)	(6)
Ln(high-skilled wage)	**-0.386*** **	**-0.273*** **	0.0422	0.0550	-0.0430	-0.0895
	(0.103)	(0.0967)	(0.0448)	(0.0428)	(0.0672)	(0.0834)
Ln(medium-skilled wage)	0.127	0.0430	**-0.488*** **	**-0.517*** **	-0.132	-0.178
	(0.170)	(0.172)	(0.126)	(0.125)	(0.196)	(0.216)
Ln(low-skilled wage)	0.0659	0.0742	0.0981***	0.114***	**-0.427*** **	**-0.461*** **
	(0.0485)	(0.0621)	(0.0344)	(0.0379)	(0.0962)	(0.119)
Ln(high-skilled wage) × Export Share	**-0.0024**					
	(0.0020)					
Ln(high-skilled wage) × Export Share × No CBA		**-0.0004**				
		(0.0025)				
Ln(high-skilled wage) × Export Share × CBA		**-0.0041***				
		(0.0023)				
Ln(medium-skilled wage) × Export Share			**-0.0025****			
			(0.0013)			
Ln(medium-skilled wage) × Export Share × No CBA				**-0.0039****		
				(0.0018)		
Ln(medium-skilled wage) × Export Share × CBA				**-0.0012**		
				(0.0016)		
Ln(low-skilled wage) × Export Share					**-0.0015**	
					(0.0014)	
Ln(low-skilled wage) × Export Share × No CBA						**0.0000**
						(0.0019)
Ln(low-skilled wage) × Export Share × CBA						**-0.0021**
						(0.0018)
Ln(high-skilled wage) × CBA		-0.212		-0.0234		0.0921
		(0.132)		(0.0602)		(0.0950)
Ln(medium-skilled wage) × CBA		0.166		0.0286		0.0581
		(0.128)		(0.0869)		(0.162)
Ln(low-skilled wage) × CBA		-0.0186		-0.0344		0.0438
		(0.0714)		(0.0450)		(0.133)
Export Share	0.0209	0.0040	0.0209**	0.0318**	0.0126	0.0013
	(0.0164)	(0.0208)	(0.0101)	(0.0142)	(0.0105)	(0.0145)
CBA	-0.0107	0.555	-0.0182	0.218	-0.0405	-1.563
	(0.0208)	(0.959)	(0.0150)	(0.684)	(0.0311)	(1.172)
Export Share × CBA		0.0315		-0.0214		0.0159
		(0.0234)		(0.0164)		(0.0177)
Plant-level controls	Yes	Yes	Yes	Yes	Yes	Yes
Industry × Year	Yes	Yes	Yes	Yes	Yes	Yes
No. of observations	13,414	13,414	13,414	13.414	13,414	13,414
No. of plants	4,058	4,058	4,058	4,058	4,058	4,058
Within R-Squared	0.0615	0.0645	0.0632	0.0643	0.0817	0.0825

Notes: Results are obtained by means of fixed effects OLS, standard errors are clustered at the plant level. Significance levels are 0.1 (*), 0.05 (**), and 0.01 (***).

3.7.3 Details on the theoretical model

Following Krishna et al. (2001), firms' are assumed to maximize profits while facing a less than infinitely elastic overall product demand curve of type:

$$p_i = \theta \overline{p} Q_i^{-\frac{1}{\epsilon_i}}, \tag{3.7.1}$$

with term p_i denoting the own price, $\overline{p}$ the average global product price, θ a scaling factor, Q_i the firm's output and ϵ_i the price elasticity of demand. Given non-homothetic consumer preferences across countries (Markusen, 2013) and each firm serving their domestic market and different foreign markets at varying extents, we modify the model by considering the price elasticity of product demand to be firm-specific.

The production function is Cobb–Douglas in variable inputs, and given by:

$$Q_i = \prod_{k=1}^{n} V_{ki}^{\alpha_k}, \tag{3.7.2}$$

where the term $V_{ki}^{\alpha_k}$ denotes the k^{th} input in production. Factor markets are assumed to be fully competitive, with the firm taking factor prices (w_k) as given. Partially differentiating profits with respect to the l^{th} input, labor, and equating it to zero, yields the following first order condition:

$$\theta \overline{p} Q_i^{1-\frac{1}{\epsilon_i}} \left(1 - \frac{1}{\epsilon_i}\right) \alpha_l V_{li}^{-1} = w_l. \tag{3.7.3}$$

Taking logs and reorganizing terms, this condition can be rewritten as:

$$\begin{aligned} \ln V_{li} = & -\frac{\ln\left(\theta\left(1-\frac{1}{\epsilon_i}\right)\alpha_l\right)}{\left[\alpha_l\left(1-\frac{1}{\epsilon_i}\right)-1\right]} + \frac{1}{\left[\alpha_l\left(1-\frac{1}{\epsilon_i}\right)-1\right]} \ln\left(\frac{w_l}{\overline{p}}\right) \\ & - \sum_{k\neq l} \frac{\alpha_k\left(1-\frac{1}{\epsilon_i}\right)}{\left[\alpha_l\left(1-\frac{1}{\epsilon_i}\right)-1\right]} \ln V_{ki}. \end{aligned} \tag{3.7.4}$$

Substituting the first order conditions for inputs $V_{k\neq l}$ into equation (3.7.4), the optimal labor demand function is given by means of:

$$\ln V_{li} = \delta_0 + \underbrace{\sum_{k=1}^{n} \frac{-\left[1-\left(1-\frac{1}{\epsilon_i}\right)\left(\sum_{k\neq l}\alpha_k\right)\right]}{\left[1-\left(1-\frac{1}{\epsilon_i}\right)\left(\sum_{k=1}^{n}\alpha_k\right)\right]}}_{\delta_k} \ln\left(\frac{w_k}{\overline{p}}\right), \tag{3.7.5}$$

with δ_0 and δ_k being functions of ϵ_i.
From equation (3.7.5), the own-wage elasticity of labor demand can be derived as:

$$\frac{\partial \ln V_{li}}{\partial \ln\left(\frac{w_l}{\overline{p}}\right)} = \eta_{lli} = \frac{-\left[1-\left(1-\frac{1}{\epsilon_i}\right)\left(\sum_{k\neq l}\alpha_k\right)\right]}{\left[1-\left(1-\frac{1}{\epsilon_i}\right)\left(\sum_{k=1}^{n}\alpha_k\right)\right]} < 0, \tag{3.7.6}$$

with labor demand decreasing in case wages increase. In line with the Hicks-Marshall law of derived demand, it can be further shown that the absolute value of the own-wage elasticity of labor demand increases with the price elasticity of product demand:

$$\frac{\partial\left|\eta_{lli}\right|}{\partial\epsilon_i} = \frac{\alpha_l}{\epsilon_i^2\left[1-\left(1-\frac{1}{\epsilon_i}\right)\left(\sum_{k=1}^{n}\alpha_k\right)\right]^2} > 0. \tag{3.7.7}$$

Given non-homothetic consumer preferences across countries, the absolute value of the country-specific price elasticity increases with per-capita income. Thus, firms located in high-income countries and serving its domestic market only face a less elastic demand for their products compared to those firms that export some share of their output to foreign destinations, especially to low- and medium-income countries. In accordance with equation (3.7.7), a more price elastic product demand faced by exporting rather than non-exporting firms should thus translate into a higher own-wage elasticity of labor demand.

Chapter 4

Benefit Duration and Job Search*

4.1 Introduction

A central challenge of unemployment insurance (UI) schemes is to allow unemployed individuals to actively search for suitable reemployment opportunities by partly compensating for income losses while at the same time repressing the unintended incentives to lower search intensity. Disincentive effects of UI systems, triggered by both the level of benefits as well as the potential benefit duration (PBD), have been, however, well identified by empirical research. In a nutshell, extensions of the PBD have been shown to significantly extend individuals' nonemployment duration, irrespective of personal characteristics or institutional regulations of the labor market (see, for example, Katz and Meyer, 1990; Card and Levine, 2000; Lalive et al., 2006; Van Ours and Vodopivec, 2006; Chetty, 2008; Schmieder et al., 2012, 2015).[1]

While standard job search theory shows that increases in the duration of nonemployment spells due to the extension of the PBD can be attributed to lower search effort and/or higher reservation wages, direct empirical evidence regarding

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* This chapter is based on a (so far unpublished) single-authored manuscript titled "Benefit Duration and Job Search Effort: Evidence from a Natural Experiment", see Lichter (2015).

[1] Card et al. (2007) show that the *extent* of the observed spike in exit rates prior to the expiration of benefits significantly depends on the measurement of individuals' unemployment spells: reemployment hazards increase significantly less than unemployment exit rates. Given that unemployment registration is not mandatory in many countries after benefit exhaustion, spikes in unemployment exit rates may hence overstate the extent of a UI-induced moral hazard.

the importance of reduced job search effort in contributing to this aggregate effect is sparse. Absent direct evidence, findings of prolonged spells of nonemployment are rather commonly interpreted as suggestive evidence of reduced search effort and the presence of moral hazard.[2] Two recent studies by Baker and Fradkin (2015) and Marinescu (2015) aim at filling this gap by relating state-level variation in the PBD in the U.S. to changes in state-level internet job search intensity, with both providing evidence of less job search in response to increases in the PBD during the time of the recent recession.

The present paper adds to this limited evidence by using quasi-experimental variation in the PBD for one specific age group of the unemployed in conjunction with detailed, direct information on individuals' search effort and reservation wage choices to provide causal evidence of the effect of benefit duration on job search behavior. Variation in the PBD comes from an unexpected and rapidly implemented policy change in Germany in late 2007. The new legislation was motivated by concerns of social injustice and took place during times of stable-macro-economics conditions. On December 11, 2007, only two months after the initial reform proposal, the then acting coalition of the Christian Democrats (CDU) and Social Democrats (SPD) issued a law that enabled the extension of the PBD for eligible workers aged 50 to 54 by twelve weeks (from 12 to 15 months), while the PBD for younger workers remained unaffected.[3]

Using data from the IZA Evaluation Dataset Survey, which covers a large sample of individuals registering as unemployed at the German Federal Employment Agency between June 2007 and May 2008, the present paper exploits this policy reform to investigate the effects of the PBD on job search behavior. Using unemployed individuals aged 45 to 49, who were not affected by the reform, as a control group allows for applying simple difference-in-differences techniques. Importantly, the swiftness of the political process and uncertainty about the design

[2] Chetty (2008) shows that the increase in unemployment duration due to more generous UI cannot be entirely attributed to moral hazard, but the role of liquidity effects must also be accounted for.

[3] As detailed below, workers were subject to the reform in case having had contributed to UI for at least 12 months within the last two years (eligibility constraint) and for 30 months within the last five years. Note that the reform also extended the PBD for eligible workers aged 58 and above. Given that this study bases on data covering unemployed individuals aged 16 to 54 only, the effects of this change are, however, not investigated.

and scope of the reform until its public announcement by December 11, 2007 limit the scope of adaptive behavior. Moreover, the reform's detachedness from actual labor market conditions allows comparing the job search behavior of the two groups prior to and after the reform net of any endogenous policy bias.

The results of this study show that unemployed individuals entitled to an additional twelve weeks of unemployment benefits exerted substantially lower levels of job search effort at the beginning of the unemployment spell compared to their untreated counterparts: they filed less job applications and were less likely to apply for jobs in distant areas. The effects are robust to the inclusion of a variety of personal and regional control variables and are of significant magnitude: the increase in the PBD by three months caused job applications to decrease by around 40% of a standard deviation, on average. Treatment effects are similar for females and males but substantially differ by skill. Whereas treated low- and medium-skilled individuals showed significantly less job search effort in response to the reform, the high-skilled unemployed did not reduce the number of filed applications. In contrast, the increase in the PBD had no effect on reservation wages, which – despite being counterintuitive to theory – is in line with recent evidence demonstrating limited responsiveness of individuals' reservation wage with respect to changes in UI parameters (see, among others, Krueger and Mueller, 2014; Schmieder et al., 2015).

Overall, the study offers considerable evidence for UI-induced moral hazard and strategic search behavior. Unemployed individuals respond to more generous UI by significantly reducing job search effort. These findings relate to early work by Barron and Mellow (1979), who report a negative relationship between UI payments and the time devoted to job search. Moreover, the results correspond to less direct evidence of moral hazard by Arni and Schiprowski (2015), who show that externally imposed changes in search effort affect job seekers' outcomes, and Black et al. (2003), who demonstrate that individuals leave unemployment upon receiving notice of required participation in reemployment services, i.e., in case costs of unemployment increase.

The paper proceeds as follows. Section 4.2 provides the theoretical foundation of this study by highlighting expected changes in job search behavior in response to an extension of the PBD. Section 4.3 offers a short overview about the key institutional characteristics of the German labor market and highlights the key features of the reform of interest. Information on the dataset are presented in Section 4.4, Section 4.5 provides the empirical model and details the underlying identification strategy. The results of this analysis are presented in Section 4.6, while Section 4.7 concludes.

4.2 Job search theory

According to the partial-equilibrium models of job search, increases in the PBD should lower job search effort and increase reservation wages. The theoretical framework by Schmieder et al. (2015) demonstrates the expected effects in a discrete-time setting.

Risk-neutral workers are assumed to become unemployed in period $t = 0$ and to maximize the present discounted value of income. They receive benefits b_t and choose search effort λ_t, which is normalized to reflect the probability of receiving a job offer, in each period. Effort choices generate search costs $\psi(\lambda_t)$, which are assumed to be an increasing, convex and twice differentiable function of the search effort applied. Constant UI benefits b are limited to P periods and replaced by an indefinite second tier payment $\underline{b}$ thereafter ($\underline{b} < b$), generating non-stationarity in the spirit of Van den Berg (1990).

Jobs offer a wage w_t^* that is drawn from a distribution with cumulative distribution function $F(w_t^*; \mu_t)$, assumed to vary over the spell of nonemployment t; for example, due to stigma effects. For simplicity, it is assumed that the distribution can be summarized by its mean in period t: μ_t. In this case, $w_t^* = \mu_t + u_t$, where $E[u_t|t] = 0$, such that u_t reflect random draws from the distribution. If a job is accepted, workers start at the beginning of the next period and are assumed to indefinitely stay with their new job.[4] Thus, the value of being employed, V^e, satisfies: $V^e(w^*) = \frac{1}{\rho}w^*$, with ρ indicating the common subjective discount rate.

Given that $V^e(w^*)$ increases with w^*, the optimal search strategy of a job seeker

[4] Van den Berg (1990) acknowledges potential criticism regarding this assumption as rejecting a job offer may be suboptimal to accepting it and quitting immediately thereafter, given that the latter case may result in a new spell of unemployment and thus extended benefits. However, given legal boundaries prohibiting or punishing such behavior in reality, the validity of this assumption seems justifiable.

thus comprises choosing effort to generate contact and specifying a reservation wage (ϕ_t) in each period such that all wage offers $w^* \geq \phi_t$ are accepted. The corresponding Bellman equation is then given as follows:

$$V_t^u = b_t + \max_{\lambda_t} \Big[-\psi(\lambda_t) + \frac{1}{1+\rho} V_{t+1}^u + \frac{\lambda_t}{1+\rho} \int_{\phi_t}^{\infty} \Big(V^e(w^*) - V_{t+1}^u \Big)\, dF_t(w^*) \Big].$$

The environment is assumed to become stationary for $t \geq T$: $b_t = \underline{b}$ and $F_t(w^*) = F_T(w^*)$. This in turn implies that the optimal search strategy is *constant* for $t \geq T$. Using that $V_t^u = V_T^u \; \forall \; t \geq T$, $\phi_t = \rho V_t^u$ holds true in stationarity. The optimal reservation wage can then be deduced from the Bellman equation:

$$\frac{\phi_T}{\rho} = b_T - \psi(\lambda_T) + \frac{1}{1+\rho}\frac{\phi_T}{\rho} + \frac{\lambda_T}{1+\rho} \int_{\phi_T}^{\infty} \Big(\frac{1}{\rho} w^* - \frac{\phi_T}{\rho} \Big)\, dF_T(w^*)$$

$$\phi_T = (1+\rho)(b_T - \psi(\lambda_T)) + \frac{\lambda_T}{\rho} \int_{\phi_T}^{\infty} \Big(w^* - \phi_T \Big)\, dF_T(w^*). \tag{4.2.1}$$

Optimal search intensity in stationarity is then obtained by differentiating equation (4.2.1) with respect to λ_T, yielding:

$$\psi'(\lambda_T)(1+\rho)\rho - \int_{\phi_T}^{\infty} \Big(w^* - \phi_T \Big)\, dF_T(w^*) = 0. \tag{4.2.2}$$

In the non-stationary segment ($t < T$), it in turn holds true that $\phi_t = \rho V_{t+1}^u$. Knowledge about ϕ_t and λ_t in period t, with the initial conditions resulting from equations (4.2.1) and (4.2.2) in $t = T$, allows derivation of the job seeker's optimal strategy in non-stationarity for period $t-1$:

$$\frac{\phi_{t-1}}{\rho} = b_t - \psi(\lambda_t) + \frac{1}{1+\rho}\frac{\phi_t}{\rho} + \frac{\lambda_t}{1+\rho} \int_{\phi_t}^{\infty} \Big(\frac{1}{\rho} w^* - \frac{\phi_t}{\rho} \Big)\, dF_t(w^*)$$

$$(1+\rho)\phi_{t-1} = (1+\rho)\rho(b_t - \psi(\lambda_t)) + \phi_t + \lambda_t \int_{\phi_t}^{\infty} (w^* - \phi_t)\, dF_t(w^*). \tag{4.2.3}$$

Optimal search effort in period $t-1$ can then be deduced by differentiating equation

(4.2.3) with respect to λ_{t-1}, which yields:

$$\rho(1+\rho)\psi'(\lambda_{t-1}) - \int_{\phi_{t-1}}^{\infty} (w^* - \phi_{t-1})\, dF_t(w^*) = 0. \tag{4.2.4}$$

Based on equation (4.2.3), it can then be shown that reservation wages increase in response to an extension of the PBD,

$$\frac{d\phi_t}{dP} = \frac{dV_{t+1}^u}{dP}\rho > 0, \tag{4.2.5}$$

in case there is some probability of remaining unemployed after the exhaustion of the PBD, in which case an extension of the PBD will increase the value of remaining unemployed in each period $t \leq P$: $\frac{dV_{t+1}^u}{dP} > 0$.
Using that

$$(1+\rho)\rho\psi'(\lambda_t) - \int_{\phi_t}^{\infty} (w^* - \rho V_{t+1}^u)\, dF_t(w^*) = 0,$$ [5]

it further follows that job search effort decreases in response to an increase in the PBD in the case where $\frac{dV_{t+1}^u}{dP} > 0 \;\forall\; t \leq P$:

$$(1+\rho)\rho\psi''(\lambda_t)\frac{d\lambda_t}{dP} = -\rho\frac{dV_{t+1}^u}{dP}\big(1 - F_t(\phi_t)\big)$$

$$\frac{d\lambda_t}{dP} = -\frac{dV_{t+1}^u}{dP}\frac{1 - F_t(\phi_t)}{(1+\rho)\psi''(\lambda_t)} < 0. \tag{4.2.6}$$

According to the model, an extension of the PBD as given in the empirical setting of this paper is thus expected to lower job search effort and to increase reservation wages of the individuals concerned. In the empirical part of this paper, these two hypotheses, i.e., relations (4.2.5) and (4.2.6), are tested.

4.3 The institutional setting

In Germany, all employees subject to social security contributions are covered by UI and are entitled to receive unemployment benefits if having had contributed to

[5] Note that this expression can be derived by differentiating equation (4.2.3) with respect to λ_t and is equal to equation (4.2.4) in period t.

UI for at least twelve months within the last two years preceding their job loss. The duration of benefits is subject to the number of months employed within a given time frame and increases with age. Monthly benefits amount to 60% (67% for recipients with children) of the last net wage, which is capped at the upper ceiling of the social security contributions, and payments are generally rescinded for up to twelve weeks if workers terminate their job themselves, which lowers the maximum benefit duration accordingly. Each recipient of unemployment benefits is further obliged to actively search for a job and to be at the Employment Service's disposal, while failure to comply with these requirements may result in benefit cuts.[6] Individuals who are not entitled for or exhaust their unemployment benefits may receive welfare benefits, which are granted for an unlimited period and designed to assure living at subsistence level.

UI benefit extension for older workers in 2007 The extension of the PBD for older workers was the result of an unexpected policy reform under the grand coalition of Christian Democrats (CDU) and Social Democrats (SPD) in late 2007. The remarkably rapid implementation of the reform proposal, uncertainty about the design and scope of the reform until its public announcement, and its detachedness from the business cycle allows for the investigation of the effects of the PBD on job search effort and reservation wages in absence of (the common challenges of) avoidance behavior and endogenous policy bias. Below, the key features of this reform are detailed.

Since their implementation in the early 2000s, the Social Democrats were heavily divided about the evaluation of their large, structural reforms that had made the German labor market much more flexible (*Hartz IV, Agenda 2010*, among others) but had marked a significant shift in the party's policy agenda, resulting in electoral defeats and a challenge to the identity of the party. On October 1, 2007, the then acting party leader of the Social Democrats, Kurt Beck, marked the party's public turn from its (more) liberal policy by calling for an extension of the PBD for older workers. The reform proposal was motivated on the grounds of social injustice concerns – long periods of UI contributions were ought to be re-

[6] Note that there is no general minimum number of applications required by law.

warded by extended PBD[7] – and was made during times of stable macro-economic conditions (see Figure 4.8.1 in the Appendix).

The initial proposal was met with considerable skepticism, from politicians in both the Christian Democratic and the Social Democratic parties. Disagreement about the proposal, and hence uncertainty about the implementation of the suggested reform, lasted for several weeks and raised rumors about the collapse of the acting coalition. To ease the growing tensions[8], both parties negotiated over pending disputes in a coalition meeting on the night of November 12, and a general decision in favor of an extension of the PBD was announced by the following morning. However, details about the *actual* changes of the UI scheme did not become public until December 11, 2007, when the corresponding law was issued to parliament.

Ultimately, the reform affected those unemployed individuals aged 50 or above who fulfilled the given entitlement criteria. PBD for workers aged 50 to 54 was extended by twelve weeks (from 12 to 15 months) if having had contributed to UI for at least 12 months within the last two years (eligibility constraint) and for 30 months within the last five years.[9] Likewise, UI benefit duration was extended from 18 to 24 months for all workers aged 58 or above if they had fulfilled the eligibility constraint and had contributed to UI for at least four out of the last five years. The reform also contained a transitional agreement which extended the PBD for those respective workers who were unemployed prior to the reform, fulfilled the entitlement criteria highlighted above and whose eligibility period was not exhausted by December 31, 2007.[10] The reform was passed by parliament on January 26, 2008 and retroactively extended back to January 1, 2008. Table 4.8.1 in the Appendix outlines the relationship between the claimant's age and length

[7] The reform proposal followed claims of the German Trade Union Confederation (*DGB*), who initially suggested the extension of the PBD for all workers aged 45 and above to up to 24 months.

[8] The coalition also disagreed about other pending topics, such as the introduction of minimum wages in the postal sector, for example.

[9] Note that the reform extended the qualifying period from three to five years, too.

[10] Hence, the reform subsequently extended the PBD for all eligible individuals who had become unemployed before January 1, 2008 and were entitled to receive benefit payments on December 31, 2007 by three months (see §434r, SGB III). However, note that this only applied to those individuals who fulfilled both criteria (above the respective age threshold and sufficient contributions to UI) at the time of unemployment registration.

of UI contributions and the PBD prior to (upper panel) and after the reform (lower panel). However, as the data used in this analysis focuses on unemployed individuals aged 16 to 54, this study exploits information about the reform for the younger of the two age groups only.

4.4 Data

In order to investigate the consequences of this reform, the analysis in this paper uses data from the IZA Evaluation Dataset Survey, which covers a large sample of individuals registering as unemployed at the German Federal Employment Agency between June 2007 and May 2008, i.e., prior to and after the reform (see Arni et al. (2014) for details). Designed to allow for the investigation of active labor market program (ALMP) effects, the dataset surveys prime-aged workers (aged 16 to 54) who enter unemployment, search for reemployment opportunities and qualify for participation in ALMPs. Individuals close to (early) retirement and all recipients of welfare benefits, who are thus not entitled for participation in ALMPs, are in turn not covered by the survey.

In order to obtain a representative sample of the unemployed population in this survey and to account for seasonal effects over one year, a random sample of unemployed individuals was drawn from the monthly unemployment inflow statistics of the German Federal Employment Agency in each month between June 2007 and May 2008. In total, 17,396 individuals were first interviewed around two months after becoming unemployed and were repeatedly questioned over time. For the present analysis, the first wave of the survey is exploited, which provides detailed information on individual job search behavior at the beginning of the unemployment spell.

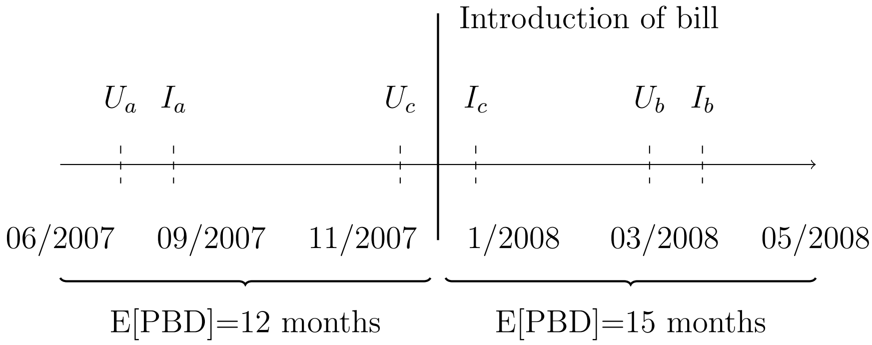
More precisely, the survey covers information on the number of applications, the filing of applications that require moving and the reservation wage, i.e., the indicated lowest wage rate at which an unemployed person would consider working. This information is supplemented by a large set of variables on the respondents' employment history, personal characteristics (e.g., the age, education or level of professional training) and personality traits, such as the locus of control or the Big Five. The data also include information on individuals' supervision intensity by the local Employment Agencies (the number of agency visits or received job offers, among others) and local labor market conditions, such as regional unemployment and vacancy rates.

For the empirical analysis presented below, all individuals who are already reemployed at the time of the first interview[11] – around 25% of the observations – or did not participate in the labor market are excluded.[12] Descriptive statistics for the estimation sample are provided in Table 4.8.2 in the Appendix.

4.5 Identification

The dataset allows observing the job search behavior of unemployed individuals who were interviewed prior to or after the public announcement of the reform and its details on December 11, 2007. Variation in the date of unemployment registration, the policy reform and the date of the interview provide a clear quasi-experimental setting to identify the effects of the PBD on job search effort.

Figure 4.5.1: Unemployment entry, interview date and expected benefit duration



Notes: The figure plots the setting of this analysis. For example, individuals $i \in \{a, b, c\}$ registered as unemployed at U_i and were interviewed at I_i. Expectations about the potential benefit duration change on December 11, 2007; the day the bill was introduced to the parliament.

Figure 4.5.1 illustrates the setting of the analysis. Individual a registered as unemployed (U_a) and was interviewed about her job search behavior (I_a) prior to

[11] On average, the interview is conducted around eight weeks after the individuals' unemployment registration.

[12] Note that both the probability of being reemployed at the time of the first interview as well as the probability of participating in the labor market are not affected by the reform of interest.

the reform, thus choosing her job search effort while expecting a PBD of twelve months. In turn, individual b became unemployed and chose job search effort while knowing about the extension of the PBD. For individual c, expectations about the PBD were updated after unemployment registration but prior to the interview. Some part of the relevant job search period was thus subject to the new PBD regime, whereas initial job search effort was chosen while expecting a PBD of 12 months. The job search effort of individual c may thus have converged towards the search effort level of individual b after the extension of the PBD became public.[13]

Based on this setting and in line with the empirical strategies pursued by Kyyrä and Ollikainen (2008) as well as Van Ours and Vodopivec (2008), a simple difference-in-differences strategy is applied to compare pre- and post-reform outcomes. Unemployed workers aged 50 to 54, who were interviewed after the announcement of the reform and hence gained knowledge about the extension of the PBD prior to choosing their job search behavior, constitute the *treatment group*. Same-aged individuals interviewed prior to the introduction of the reform serve as the *comparison group*.[14] Unemployed workers aged 45 to 49, interviewed prior to or after the reform, serve as *control groups* in order to account for any seasonal aggregate effects.

Eligible individuals As highlighted before, benefit duration in Germany is subject to the claimant's age and length of UI contributions within a given qualifying period. The reform of interest thus changed the PBD for a subset of individuals aged 50 to 54 only. Individuals were entitled to extended PBD if having had contributed to UI for at least 12 months within the last two years (eligibility constraint) and for 30 months within the last five years (coverage constraint). For the purpose of this analysis, all unemployed individuals that did not fulfill the contribution criteria were thus excluded, irrespective of the claimant's age. Unfortunately, the present dataset provides information on the respondents' last employment *period* only, which limits the analysis to those claimants who have ful-

[13] In the empirical analysis presented below, special attention is paid to those individuals whose expectations about the PBD updated after unemployment registration but prior to the interview.

[14] Note that the comparison group is equivalent to the treatment group observations measured pre-treatment.

filled both entitlement criteria without any interrupting period of non-employment. Compared to the entire eligible population, the individuals in this sample are thus positively selected with regard to their labor market history given that the sampled individuals were not subject to unemployment in the recent past. If the sampled individuals responded differently with regard to this reform compared to the eligible individuals not covered in the analysis, the estimates of this study may thus not provide the true treatment effect for the entire eligible population.

In general, heterogeneous responses by these two groups may be due to consequences and causes of prior unemployment experience. First, UI-induced moral hazard may be less (more) pronounced among the group of those eligible individuals who have experienced unemployment prior to the current unemployment spell if these individuals had encountered net (dis)utility from unemployment and include past experiences in their current decision about job search effort. Second, unobservable and observable differences between both groups may have caused prior unemployment spells and could affect individuals' responses with respect to the reform of the PBD.

The analysis presented below, however, suggests that past unemployment experience does not affect current choices about job search effort. UI-induced moral hazard is of similar magnitude for those individuals in the sample who have been unemployed prior to the current spell and those who have not. Evidence of more pronounced UI-induced moral hazard among the low- and medium-skilled compared to the high-skilled unemployed further implies that the sample may underestimate the overall treatment effect for the entire eligible population if the covered sample is positively selected on skills.

Empirical model and identification The present setting allows for testing the two hypotheses of the job search model presented in Section 4.2. Using *difference-in-differences* techniques, it is tested whether an extension in the PBD lowers the job search effort (cf. equation (4.2.6)) and increases the reservation wage (cf. equation (4.2.5)) of the individuals concerned. The underlying empirical specification reads as follows:

$$y_i = \alpha + \beta T_i + \gamma A_i + \delta(T_i \times A_i) + X_i'\rho + \varepsilon_i, \qquad (4.5.1)$$

with the dependent variable y_i indicating measures of job search effort or the reservation wage of individual i, T_i being a dummy variable indicating whether the individual was interviewed after the reform, and A_i indicating whether the individual is aged between 50 to 54. The treatment effect is given by δ, X_i' defines a vector of control variables and ε_i the error term.

Identification of the model rests upon the assumptions that (i) no observable or unobservable individual characteristics determined the allocation to the treatment or comparison group and (ii) potential changes in labor market conditions over the sampling period affected treatment and control groups to an equal extent. Put more precisely, except for differences in knowledge about the reform due to the timing of being interviewed/becoming unemployed, the comparison group should be highly similar to the treatment group. Moreover, changes in business cycle conditions should not have had asymmetric effects on treatment and control groups. The remainder of this section aims at validating these identifying assumptions.

Voluntary quits and strategic layoffs In order for the identifying assumptions to hold, layoffs have to be exogenous from the individual's perspective. As some workers may, however, potentially opt to become unemployed in response to the extension of the PBD, the treatment group may be self-selected in this respect. To account for potential selection, all workers that voluntarily quit their job or became unemployed by mutual agreement are therefore excluded from the sample. Excluding these individuals from the analysis further accounts for the fact that payments of UI can be suspended for up to twelve weeks if workers voluntarily opt out of employment, which lowers the PBD accordingly.

Strategic layoff decisions by firms may further violate the identifying assumption. If firms deliberately suspend dismissals of older workers (aged 50 or above) to allow for a longer PBD, allocation into the treatment and comparison group would be non-random. Due to the fast implementation of the reform, adaptive behavior of firms is highly unlikely, and strict dismissal laws impede strategic timing of layoffs in Germany. However, as a robustness check, the analysis is further limited to layoffs where strategic timing of terminations can be ruled out, focusing on those workers who became unemployed either due to plant closings or the expiration of a temporary contact. As detailed below, the results of the analysis remain unaffected in the cases where the analysis is limited to the respective subgroups.

Concurrent ALMP reforms Estimates would be biased if simultaneous reforms had occurred that asymmetrically affected treatment, comparison and control groups. Concurrent with the extension of the PBD, the government did indeed introduce labor market integration vouchers (*Eingliederungsgutscheine*). In brief, these vouchers slightly modified eligibility criteria for unemployed individuals aged 50 or above so that they could receive employment integration subsidies (*Eingliederungszuschüsse*). These subsidies have long been used as an ALMP instrument in Germany, and all unemployed individuals are allowed to file for integration subsidies in general. Approval, duration as well as the amount of the subsidy are subject to the discretion of the local Employment Agency and are dependent upon applicants' work productivity limitations, with the scope and availability of integration subsidies being extended for individuals aged 50 or above (since May 2007).

The existence of integration vouchers and extended subsidies for the unemployed aged 50 or above should, however, not impede the causal interpretation of the findings in this analysis. Given that all unemployed individuals in the treatment and the comparison group were potentially eligible for extended subsidies in general, potential effects arising from these subsidies should be captured by the parameter of the age group dummy and therefore not affect the treatment effect of interest. Moreover, the slight modifications in the eligibility criteria for subsidies invoked by the introduction of the integration voucher as of January 1, 2008 only had a marginal, negligible effect on take up rates. In 2008, the Federal Employment Agency granted 3,000 vouchers only, compared to more than 1.5 million ALMP measures in total (Statistics of the Federal Employment Agency).[15]

Observable characteristics by age group and interview period As highlighted above, besides differences in knowledge about the reform and the timing of becoming unemployed, the comparison and treatment group should be highly

[15] By April 2012, the voucher program was stopped. Over the course of its existence, a total of around 20,000 vouchers had been issued. The total number of subsidies granted was quite constant over the period of interest. Figure 4.8.2 in the Appendix shows the annual number of subsidies granted from 2006 to 2010.

similar in observable characteristics. Moreover, labor market conditions should be either constant over time or change to an equal extent for the treatment and control group. The IZA Evaluation dataset allows for extensive testing of both identifying assumptions. Table 4.5.1 shows (differences in) mean characteristics by age groups and within the treatment and control group prior to and after the reform.

Columns (1) to (3) show means for the two age groups and the results of a simple t-test (p-values) on the equality of the means for a large set of variables. Besides expected differences in age, it becomes apparent that both groups of individuals are not systematically different from each other. On average, individuals from both groups are married, completed an apprenticeship and generated a monthly net labor income of around 1,400 euros prior to unemployment, for example. Evaluated at the mean, both groups of workers come from comparable regions across Germany, with differences in local unemployment and vacancy rates being small and insignificant. Moreover, the individuals in both groups received equal supervision by local Federal Employment Agencies, for example, by means of the number of agency visits or job offers. Lastly, both groups are similar with respect to personality traits, measured by means of individuals' locus of control, extroversion or openness, among others.

It is further tested whether mean characteristics within one age group differ before and after the reform. Columns (4) to (9) show the corresponding results. Both of the two control groups as well as comparison and treatment group are highly similar in terms of observable personal characteristics. Most importantly, the comparison and treatment group neither differ in terms of personal characteristics nor personality traits when being compared at the mean. The only notable exception is the share of respondents that has been unemployed prior to this current spell, which is higher in both the control and treatment group after the reform but only significantly different in the latter group.

When focusing on regional characteristics and individual ALMP measures, differences in some variables become apparent. However, changes over time occur for both age cohorts symmetrically and to a similar extent. In detail, the data suggest local active labor market intensity, measured by means of the share of ALMP participants over the number of total unemployed individuals, to be higher after

Table 4.5.1: Observable characteristics by age and unemployment entry

	Individuals			Within Control Group			Within Treatment Group		
	Control group	Treatment Group	p-value	pre treatment	post treatment	p-value	pre treatment	post treatment	p-value
Personal characteristics									
Age	47.31	52.57	0.00	47.18	47.36	0.32	52.71	52.51	0.31
Male (no/yes)	0.45	0.42	0.46	0.39	0.48	0.14	0.39	0.44	0.47
Education	3.83	3.63	0.11	3.99	3.76	0.24	3.61	3.64	0.90
Skill level	2.09	2.11	0.62	2.12	2.08	0.53	2.16	2.09	0.31
Last log wage	7.07	7.10	0.58	7.10	7.06	0.58	7.10	7.10	1.00
Unemployed Before	0.65	0.62	0.48	0.60	0.67	0.27	0.52	0.66	0.04
Regional characteristics									
State of residence	8.11	8.36	0.45	7.57	8.32	0.14	7.94	8.54	0.23
Local unemployment rate	9.26	9.19	0.83	9.14	9.31	0.74	9.11	9.23	0.82
Local ALMP intensity	15.70	16.34	0.17	14.42	16.19	0.01	15.59	16.66	0.18
Individual ALMP measures									
Number of agency job offers	1.83	1.90	0.78	2.04	1.74	0.36	1.74	1.96	0.60
Number of agency visits	1.70	1.78	0.16	1.93	1.61	0.00	1.91	1.72	0.04
Personality traits									
Internal locus of control	5.90	5.90	0.99	5.88	5.91	0.81	5.99	5.86	0.29
Consciousness	6.39	6.26	0.10	6.38	6.40	0.89	6.41	6.20	0.09
Openness	4.95	4.91	0.72	4.91	4.97	0.66	4.97	4.89	0.64
Extraversion	5.00	5.03	0.75	4.90	5.04	0.31	5.13	4.99	0.31
Neuroticism	3.83	3.85	0.79	3.80	3.84	0.79	3.76	3.89	0.39
Weeks b/w UE and interview	7.67	7.83	0.39	9.32	7.03	0.00	9.95	6.93	0.00
Dependent variables									
Number of filed applications	15.58	16.65	0.62	12.96	16.59	0.18	21.95	14.39	0.04
Applying for distant jobs	0.15	0.15	0.96	0.10	0.17	0.08	0.24	0.11	0.01
Log reservation wage	7.00	7.00	0.93	7.01	6.99	0.78	7.00	7.00	0.94
Number of observations	324	274		90	234		82	192	

Notes: The table provides information on (differences) in means for (a) control and treatment group; (b) the control group before and after the reform; and (c) the comparison and treatment group. The total number of observations is 598. Note that the number of observations is slightly smaller for the reservation wage (N=559).

the reform, yet for both treatment and control group. Local unemployment rates, in turn, remain constant. The same pattern applies to individual-level measures of support by the local Federal Employment Agencies. On average, the number of visits at the local agency is slightly lower after the reform. These small difference may, however, be explained by the fact that the mean number of weeks elapsed between the individuals' unemployment registration and the interview decreased for both age groups from nine weeks prior to the reform to seven weeks afterwards.

Against the background of these similarities, it is further investigated whether treatment and control group would have followed the same trend in the outcome variables over time absent treatment. In order to investigate this identifying assumption, respondents are grouped according to their interview date and trends in the average job search intensity and the reservation wage are compared between treatment and control group.[16] For example, Figure 4.5.2 visualizes the mean number of applications for the treatment and control group over the course of the survey period.[17] First, the graph provides evidence in favor of a common trend for both groups absent the treatment. Average job search intensity is higher for the treatment than for the control group (cf. Table 4.5.1), but trends are highly similar for both groups prior to the reform. The same applies to our two other outcomes of interest (see Panels (a) of Figures 4.8.4 and 4.8.5).[18]

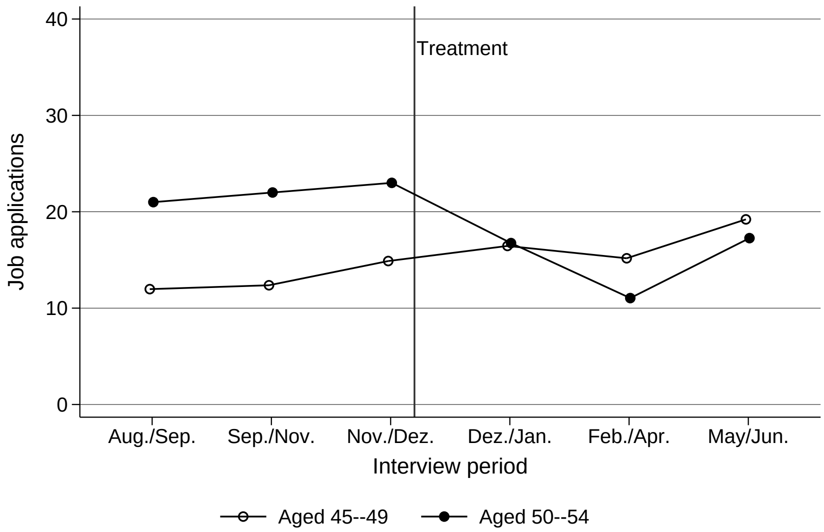
In addition to the visual evidence in favor of a common trend prior to the reform, Figure 4.5.2 also provides insights about the treatment effect of the reform. While the mean number of job applications increases slightly for the control group after the reform, mean job applications for the treated unemployed significantly decrease. As indicated in Panel (a) of Figure 4.8.4 in the Appendix, the same result is found for the second measure of job search effort, the probability of applying for a job in distant areas. In contrast, reservation wages for both the control and

[16] Recall that the underlying dataset is based on monthly-drawn random samples from the unemployment inflow statistics of the German Federal Employment Agency over the course of one year, such that interview dates vary for the respondents in the treatment and control groups.

[17] The corresponding figures for the two other outcomes of interest are provided in the Appendix of this paper (see Panels (a) of Figures 4.8.4, 4.8.5)

[18] Panels (b) of Figures 4.8.3, 4.8.4 and 4.8.5 in the Appendix further demonstrate that this result holds true when controlling for differences in observable characteristics for the respondents covered in each of the three periods prior to treatment.

Figure 4.5.2: Trends in the number of job applications



Notes: The graph plots variation in the mean number of job applications for treatment and control group over the survey period.

treatment group remain rather constant after the reform (see Figure 4.8.5).

4.6 Results

4.6.1 Baseline estimates

Table 4.6.1 provides the corresponding treatment effect estimates for the three outcomes of interest, the number of filed applications, the probability of applying for jobs that require moving and the reservation wage.

Column (1) of Panel A shows that the PBD has a negative and significant effect on the total number of applications. In this very simple model, the average number of filed applications drops by around 40% of a standard deviation in response to the reform. In columns (2) to (5), control variables are successively added to the model to check the robustness of this result. Adding personal characteristics, such as the individuals' gender, level of training or last wage prior to unemployment hardly changes the treatment effect (see Column (2)). The same conclusions arise when adding individual-level controls of ALMP intensity (Column (3)), or regional controls of the labor market to the model (Column (4)). As it has been shown that personality traits may affect job search behavior (Caliendo et al., 2015), information on individuals' personality traits are added in the most comprehensive specification. As displayed in Column (5), accounting for these variables, however, hardly affects the estimate.

Panel B of Table 4.6.1 presents the corresponding results when focusing on the probability of applying for jobs that require moving as the outcome variable of interest. The estimates show a statistically significant and robust negative effect of the PBD on the probability of applying for a job that requires moving. From the results of the simple model presented in Column (1), it can be inferred that the probability decreases by around 20% in response to the reform. In line with the results of Panel A, the effect is very robust with respect to the inclusion of additional covariates. Estimates of the treatment effect provided in Columns (2) to (5) do not change much when successively adding controls. Thus, the results for the two distinct measures of job search effort verify the prediction of the job search model presented in Section 4.2: an increase in the PBD lowers individual job search effort (cf. equation (4.2.6)).

The estimates presented in Panel C in turn provide no evidence in favor of higher reservation wages due to the increase in the PBD. The estimated treatment effect from the simple model presented in Column (1) is close to zero and statistically insignificant. This holds true when successively adding control variables to the model. While this result is thus in contrast to the prediction of the job search model presented before (cf. equation (4.2.5)), it is still in line with recent evidence by Krueger and Mueller (2014) and Schmieder et al. (2015), who show that reservation wages respond little over the spell of unemployment and with respect to changes in UI parameters. For example, because job seekers may potentially "anchor their reservation wage on their previous wage" (Krueger and Mueller, 2014, p.31). Overall, the moderate increase in the PBD is thus found to lower job search effort but keeps reservation wages unaffected.

Adjustment of job search behavior Due to the setup of the reform, the treatment group comprises a subset of individuals who learned about the reform

Table 4.6.1: The effects of benefit duration on job search

Panel A – Number of job applications	(1)	(2)	(3)	(4)	(5)
Date of Reform	3.638	6.410**	6.969**	7.419***	7.467***
	(2.685)	(2.804)	(2.874)	(2.863)	(2.790)
Age Group Dummy	8.996**	5.945	6.898	7.885*	6.702
	(3.699)	(4.712)	(4.484)	(4.628)	(4.793)
Treatment Effect	-11.199**	-10.766**	-12.628***	-12.914***	-12.021***
	(4.528)	(4.456)	(4.818)	(4.786)	(4.606)
Adjusted-R^2	0.005	0.044	0.125	0.134	0.151
Panel B – Distant applications	(1)	(2)	(3)	(4)	(5)
Date of Reform	0.071*	0.084*	0.095**	0.077	0.072
	(0.040)	(0.046)	(0.046)	(0.047)	(0.047)
Age Group Dummy	0.144**	0.185**	0.193**	0.197**	0.194**
	(0.057)	(0.079)	(0.079)	(0.081)	(0.080)
Treatment Effect	-0.205***	-0.211***	-0.223***	-0.222***	-0.218***
	(0.066)	(0.064)	(0.064)	(0.065)	(0.065)
Adjusted-R^2	0.013	0.152	0.155	0.146	0.151
Panel C – (Log) reservation wage	(1)	(2)	(3)	(4)	(5)
Date of Reform	-0.017	0.008	0.011	-0.002	-0.002
	(0.061)	(0.034)	(0.035)	(0.035)	(0.035)
Age Group Dummy	-0.013	0.065	0.057	0.039	0.046
	(0.080)	(0.055)	(0.055)	(0.055)	(0.055)
Treatment Effect	0.022	-0.027	-0.027	-0.022	-0.020
	(0.092)	(0.049)	(0.050)	(0.050)	(0.050)
Adjusted-R^2	-0.005	0.707	0.710	0.709	0.710
Individual controls	No	Yes	Yes	Yes	Yes
ALMP measures	No	No	Yes	Yes	Yes
Regional controls	No	No	No	Yes	Yes
Personality traits	No	No	No	No	Yes
Number of observations	598	598	598	598	598

Notes: The table provides the baseline results of the analysis based on equation (4.5.1). Standard errors (in parentheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4.6.2: Benefit duration & the number of applications: treatment duration

	(1)	(2)	(3)	(4)	(5)	(6)
Date of Reform	3.780	9.152***	3.780	8.304***	7.413	11.060 **
	(2.968)	(3.350)	(2.971)	(3.161)	(6.268)	(4.801)
Age Group Dummy	9.585***	5.306	9.585***	7.387	9.602***	7.975 *
	(3.701)	(4.645)	(3.705)	(4.759)	(3.699)	(4.677)
Treatment Effect	-12.555***	-13.000***	-12.555***	-12.377 **		
	(4.802)	(4.931)	(4.807)	(4.806)		
× UE after reform			3.674	-0.879		
			(5.578)	(5.048)		
... interview Dec-Jan					-10.539 **	-12.247***
					(4.541)	(4.495)
... interview Feb-Apr					-14.900***	-13.813***
					(5.421)	(5.286)
... interview May-Jun					-12.250	-12.781 *
					(7.792)	(7.444)
Adjusted-R^2	0.008	0.171	0.006	0.150	-0.000	0.152
Individual controls	No	Yes	No	Yes	No	Yes
ALMP measures	No	Yes	No	Yes	No	Yes
Regional controls	No	Yes	No	Yes	No	Yes
Personality traits	No	Yes	No	Yes	No	Yes
Number of observations	505	505	598	598	598	598

Notes: The table shows the regression results of equation (4.5.1), focusing on differential effects due to the timing/duration of the treatment. The dependent variable is the number of applications. In Columns (1) and (2), all individuals who became unemployed prior to the reform but were interviewed thereafter are dropped. Standard errors (in parentheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

after registering as unemployed but prior to the interview.[19] These individuals therefore started their initial job search while expecting a PBD of 12 months but learned about the reform during the relevant search spell. In this section, it is hence tested whether there are different treatment effects for the fully and partly treated individuals. It is further tested whether the treatment effect remains stable over the survey period by allowing for different treatment effects from December 11 to January, February to April and May to June.

In a first step, all individuals who learned about the reform after unemployment registration but prior to the interview were dropped from the sample. Columns

[19] Note that this group accounts for around 15% of the sample.

(1) and (2) of Table 4.6.2 indicate that the estimated effect remains virtually unchanged when using the reduced sample. In line with this result, columns (3) and (4) further show no evidence of heterogeneous treatment effects for the partly and fully treated. While the treatment effect may certainly depend on the relative time period between entry into unemployment, the reform and the interview date, a small sample size unfortunately precludes further analysis of this potential heterogeneity. Columns (5) and (6) in turn indicate that treatment effects do not differ with respect to the interview date, which corroborates the baseline findings and evidence in favor of UI-induced moral hazard.

4.6.2 Sensitivity of results and heterogeneous effects

The following section investigates the sensitivity of the previous results and tests for heterogeneous treatment effects. Sensitivity of the results is studied by (i) accounting for adaptive behavior, (ii) assessing potential biases due to strategic layoff decisions of firms, and (iii) testing the unconfoundedness assumption by means of a pseudo treatment test. Moreover, heterogeneous treatment effects for particular subgroups of the unemployed are provided to analyze the extent of UI-induced moral hazard for males and females, different skill groups and individuals with and without prior unemployment experience.

Salience of the reform The fast implementation of the policy reform limits the potential of adaptive behavior. Knowledge about important aspect of the reform, such as the reform's date of inception and its retroactive implementation, only became public on December 11, 2007. In conjunction with exact knowledge about the interview date, this allows for the precise definition of treatment and control groups. However, as a general agreement about the reform was already reached by November 12, 2007, adaptive behavior to this news cannot be entirely ruled out.

Although early adaption to the reform would blur the control group and would bias estimates towards zero, given that the control and treatment group would be more similar, the sensitivity of the results is tested when redefining treatment and control groups by November 12, 2007. Table 4.8.4 in the Appendix provides the corresponding results for the three outcomes of interest, obtained from the most simple and most comprehensive version of the empirical model. The estimates highlight that all qualitative results of the analysis remain robust to the redefinition of the reform's date.[20]

Strategic timing of layoffs As highlighted above, strategic timing of layoffs may impede the causal interpretation of the findings provided. Although strict employment protection laws in Germany limit the scope for strategic firing decisions of firms[21], the robustness of the findings is tested by limiting the analysis to those individuals who became unemployed due to plant closure, the termination of a temporary contract and alike. Although the number of observations decreases significantly, the results presented in Table 4.8.5 demonstrate that estimates remain robust to this constraint.

Pseudo treatment Identification of the underlying model further relies on the assumption that individuals are randomly assigned to treatment and control group and are similar in terms of observable and unobservable characteristics. While observable characteristics are indeed similar among treatment, comparison and control groups (cf. Table 4.5.1), unobservable variables may still violate the unconfoundedness assumption. Following Rosenbaum (1987), this assumption is indirectly tested by estimating the causal effect of the treatment for two groups of individuals that were unaffected by the reform (workers aged 40 to 44 and 45 to 49, respectively); with one of the two groups (the older age group) being arbitrarily considered as pseudo-treated. No evidence of any pseudo treatment effect on the outcomes would strengthen the claim of unconfoundedness. Table 4.8.6 shows support for the identifying assumption, given that pseudo-treatment effects for all three measures of job search effort are small and statistically insignificant.[22]

[20] However, point estimates are smaller and less precisely estimated, which suggest that the reform's date in the baseline regressions is correctly chosen.

[21] Dismissal of regular workers is subject to a variety of legal regulations. Advanced notice of layoff is required by law, with the period of notice increasing with workers' tenure (§622, German Civil Code). Additional rules (*Kündigungsschutzgesetz*) apply for plants that employ at least ten full-time equivalent workers. Rates of job destruction and creation mirror these legislative features of the German labor market: job and worker flow rates are around 50% lower than in the US (Bachmann et al., 2013).

[22] Note that, except for the mean age, both groups are highly similar with regard to observable characteristics. The corresponding descriptive statistics are available upon request.

Heterogeneous treatment effects Lastly, the presence of heterogeneous treatment effects is investigated, focusing on differential effects by gender, skill and prior unemployment experience. Estimates of the treatment effect on the number of job applications are provided for the most simple and most comprehensive specification, respectively. Corresponding results for the probability of applying for jobs that require moving are provided in Table 4.8.7 in the Appendix of the paper.

Table 4.6.3: Benefit duration & the number of applications: heterogeneous effects

Dep. Var.: Job applications	(1)	(2)	(3)	(4)	(5)	(6)
Date of Reform	3.363	7.419***	4.818*	7.316***	3.854	7.468***
	(2.770)	(2.783)	(2.866)	(2.776)	(2.830)	(2.795)
Age Group Dummy	8.992**	6.633	8.622**	6.649	8.751**	6.686
	(3.697)	(4.779)	(3.642)	(4.814)	(3.810)	(5.003)
Treatment × Female	-12.228***	-11.560***				
	(4.558)	(4.109)				
Treatment × Male	-9.578*	-12.568**				
	(5.543)	(6.179)				
Treatment × Low-Skilled			-11.900**	-11.200*		
			(4.772)	(5.771)		
Treatment × Medium-Skilled			-13.374***	-14.178***		
			(4.620)	(4.667)		
Treatment × High-Skilled			-1.546	-2.964		
			(8.410)	(8.566)		
Treatment × Not UE before					-13.286**	-12.100*
					(5.907)	(6.341)
Treatment × UE before					-9.787**	-11.975**
					(4.676)	(4.677)
Adjusted-R^2	0.008	0.150	0.017	0.154	0.004	0.150
Individual controls	No	Yes	No	Yes	No	Yes
ALMP measures	No	Yes	No	Yes	No	Yes
Regional controls	No	Yes	No	Yes	No	Yes
Personality traits	No	Yes	No	Yes	No	Yes
Number of observations	598	598	598	598	598	598

Notes: The table shows the regression results of equation (4.5.1), allowing for heterogeneous treatment effects by (a) gender, (b) education, and (c) prior unemployment experience. The dependent variable is the number of applications. Standard errors (in parentheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Columns (1) and (2) of Table 4.6.3 show that both females and males respond similarly to the extension of the PBD. The number of filed job applications decreases by around 9 and 7 applications, respectively. When focusing on individuals' skills (see Columns (3) and (4)), treatment effects are strong and significant for low- and medium-skilled workers but insignificant and small for the high-skilled, suggesting that UI-induced moral hazard is absent for those individuals who have invested more time and resources in their education.[23] Finally, when testing for heterogeneous effects by prior unemployment experience, the estimates provide no evidence that the previously unemployed react differently to the reform. The treatment effect is of similar magnitude for both types of individuals, which in turn suggests that potential biases due to the study's limitation on unemployed individuals who fulfilled both entitlement criteria without any interruption should be small (cf. Section 4.5).

4.7 Conclusion

To date, a large empirical literature has established that UI generosity significantly affects the duration of nonemployment. While this finding is usually attributed to UI-induced moral hazard, empirical evidence on the assumed relationship is scarce. Using quasi-experimental variation in the PBD for one specific age group of workers in Germany paired with direct information on the job search behavior of unemployed individuals, this paper complements the existing evidence by providing causal estimates of the effect of the PBD on job search effort and reservation wages.

The results of this analysis lend considerable support to the existence of UI-induced moral hazard, with the extension of the PBD leading to a considerable decrease in job search effort measured by the number of filed applications and the probability of applying for jobs that require moving. In line with recent evidence (see, among others, Krueger and Mueller (2014) and Schmieder et al. (2015)) but in contrast to standard job search theory, reservation wages, however, remain unaffected by the reform.

Overall, the study provides comprehensive evidence of strategic search beha-

[23] Note that low-skilled individuals have not completed any form of occupational training (∼8%), medium-skilled individuals have completed some form of apprenticeship (∼74%), and high-skilled unemployed hold some kind of college or university degree (∼18%).

vior. Unemployed individuals respond to more generous UI by reducing search effort, which highlights the trade-off faced by policy makers when designing UI schemes. While UI should allow individuals to actively search for suitable reemployment, disincentive effects arising from too generous UI should also be avoided in turn. Based on the findings of this study, future research might aim at estimating effort choices and reemployment probabilities due to changes in UI in one integrated framework.

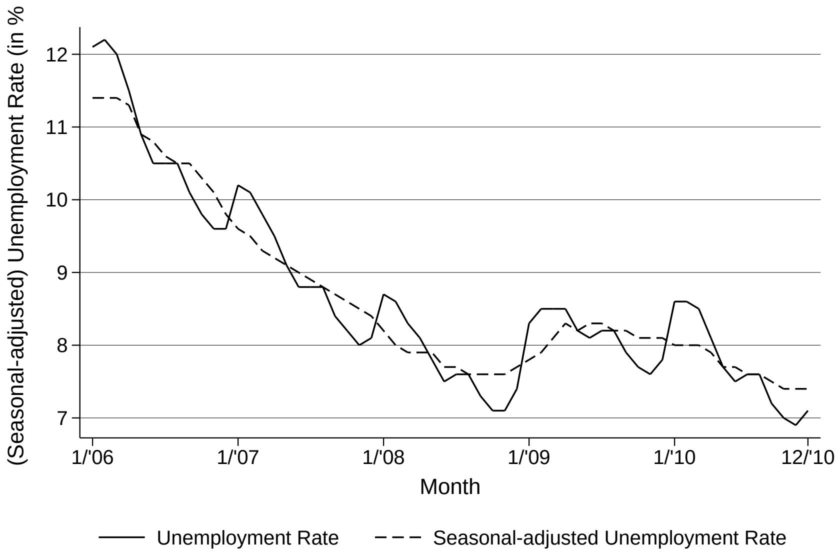
4.8 Appendix

Table 4.8.1: Claimants' age, length of UI contributions and PBD

Before January 1 2008							
Period of UI contribution (months)	12	16	20	24	30	36	
& Age of eligible person .. or above					55	55	
Potential Benefit Duration (PBD)	6	8	10	12	15	18	
Since January 1 2008							
Period of UI contribution (months)	12	16	20	24	30	36	48
& Age of eligible person .. or above					50	55	58
Potential Benefit Duration (PBD)	6	8	10	12	15	18	24

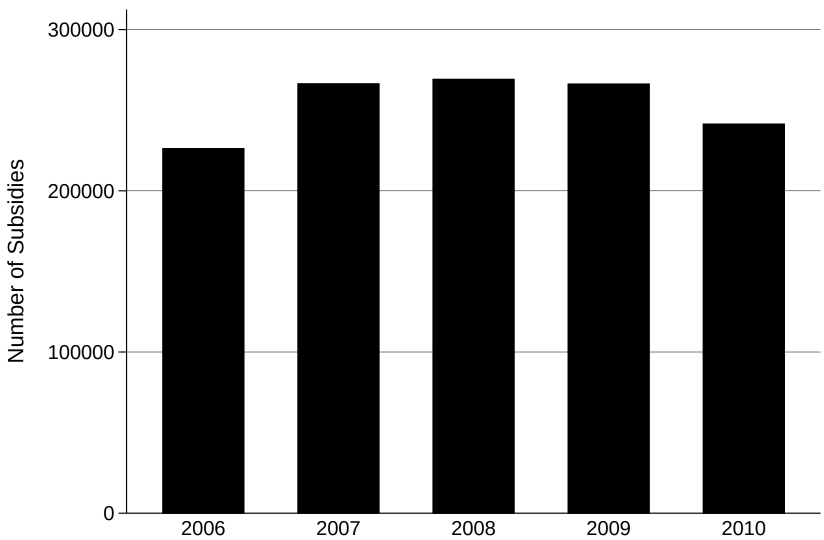
Notes: The table shows the relationship between the claimant's age, length of UI contributions and the potential benefit duration. Note that prior to the reform, the qualifying period determining the length of coverage was three years. It was extended to five years by January 1, 2008.

Figure 4.8.1: (Seasonal-adjusted) unemployment rate (2006–2010)



Notes: The graph plots monthly (seasonal-adjusted) unemployment rates from January 2006 to December 2010 for Germany. The data are provided by the German Federal Employment Agency.

Figure 4.8.2: Number of granted employment integration subsidies (2006–2010)



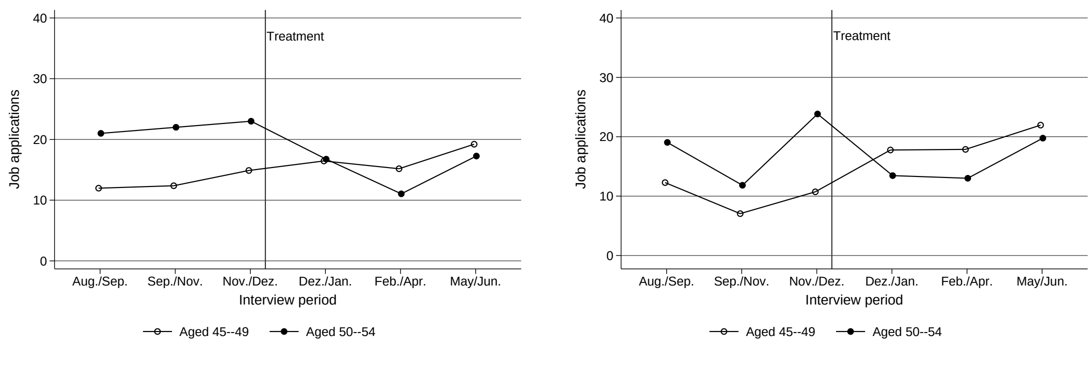
Notes: The graph plots the annual number of granted employment integration subsidies. The data are provided by the German Federal Employment Agency.

Table 4.8.2: Descriptive statistics for estimation sample

	Mean	Std Deviation	Minimum	Maximum	Observations
Dependent variables					
Number of filed applications	16.07	26.82	0.00	400.00	598
Applying for distant jobs	0.15	0.36	0.00	1.00	598
Log reservation wage	7.00	0.47	5.30	8.99	559
Personal characteristics					
Age	49.72	3.00	45.00	55.17	598
Male (no/yes)	0.44	0.50	0.00	1.00	598
Education	3.74	1.50	0.00	7.00	598
Skill level	2.10	0.50	1.00	3.00	598
Last log wage	7.08	0.56	5.08	9.21	598
Unemployed Before	0.64	0.48	0.00	1.00	598
Regional characteristics					
Local unemployment rate	9.23	3.93	3.00	17.00	598
Local ALMP intensity	15.99	5.60	7.00	30.00	598
State of residence	8.22	4.07	1.00	16.00	598
Individual ALMP measures					
Number of agency job offers	1.86	3.00	0.00	25.00	598
Number of agency visits	1.74	0.70	0.00	4.00	598
Personality traits					
Internal locus of control	5.90	0.94	1.33	7.00	598
Consciousness	6.33	0.94	1.00	7.00	598
Openness	4.93	1.25	1.00	7.00	598
Extraversion	5.01	1.06	1.00	7.00	598
Neuroticism	3.84	1.20	1.00	7.00	598

Notes: The table provides descriptive statistics for the underlying estimation sample. The number of observations is 598, except for the reservation wage (N=559).

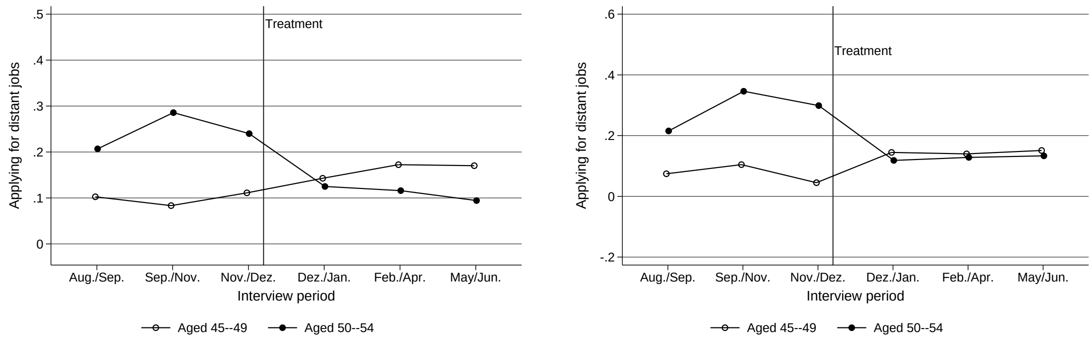
Figure 4.8.3: Trends in the number of job applications



(a) Means (b) Predicted Means

Notes: Panel (a) plots the variation in the mean number of job applications for treatment and control group over the survey period. Panel (b) plots the variation in predicted means, controlling for differences in observable characteristics across groups, over the same period.

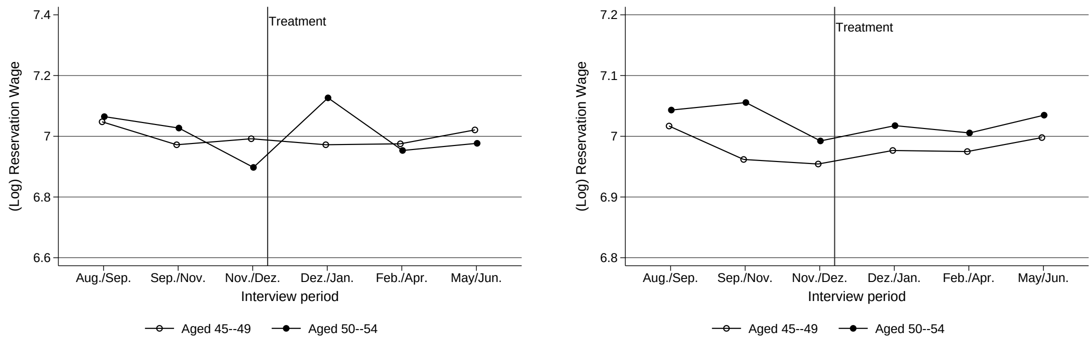
Figure 4.8.4: Trends in the probability of distant applications



(a) Means (b) Predicted Means

Notes: Panel (a) plots the variation in the mean probability of applying for distant jobs for treatment and control group over the survey period. Panel (b) plots the variation in predicted means, controlling for differences in observable characteristics across groups, over the same period.

Figure 4.8.5: Trends in the reservation wage



(a) Means (b) Predicted Means

Notes: Panel (a) plots the variation in the mean reservation wage for treatment and control group over the survey period. Panel (b) plots the variation in predicted means, controlling for differences in observable characteristics across groups, over the same period.

Table 4.8.3: Benefit duration & applying for distant jobs: treatment duration

	(1)	(2)	(3)	(4)	(5)	(6)
Date of Reform	0.067	0.072	0.067	0.068	0.096	0.100
	(0.043)	(0.055)	(0.043)	(0.053)	(0.070)	(0.072)
Age Group Dummy	0.131 **	0.186 **	0.131 **	0.177 **	0.131 **	0.179 **
	(0.058)	(0.087)	(0.058)	(0.081)	(0.058)	(0.081)
Treatment Effect	-0.192***	-0.212***	-0.192***	-0.204***		
	(0.069)	(0.068)	(0.069)	(0.067)		
× UE after reform			0.029	0.038		
			(0.083)	(0.086)		
... interview Dec-Jan					-0.150 **	-0.189***
					(0.075)	(0.072)
... interview Feb-Apr					-0.184 *	-0.182 *
					(0.097)	(0.093)
... interview May-Jul					-0.242***	-0.217 **
					(0.083)	(0.084)
Adjusted-R^2	0.011	0.150	0.006	0.144	0.003	0.139
Individual controls	No	Yes	No	Yes	No	Yes
ALMP measures	No	Yes	No	Yes	No	Yes
Regional controls	No	Yes	No	Yes	No	Yes
Personality traits	No	Yes	No	Yes	No	Yes
Number of observations	505	505	598	598	598	598

Notes: The table shows the regression results of equation (4.5.1), focusing on differential effects due to the timing/duration of the treatment. The dependent variable indicates whether individuals apply for jobs that require moving. In Columns (1) and (2), all individuals who became unemployed prior to the reform but were interviewed thereafter are dropped. Standard errors (in parentheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4.8.4: Benefit duration & job search: salience of reform

	Job applications		Distant applications		Reservation wage	
	(1)	(2)	(3)	(4)	(5)	(6)
Date of Reform	4.291	7.213***	0.070	0.051	-0.026	-0.023
	(2.626)	(2.611)	(0.044)	(0.046)	(0.069)	(0.039)
Age Group Dummy	9.364***	4.339	0.150**	0.172 *	0.026	0.048
	(3.467)	(5.298)	(0.068)	(0.091)	(0.093)	(0.059)
Treatment Effect	-10.399 **	-7.996 *	-0.191**	-0.177**	-0.029	-0.025
	(4.311)	(4.191)	(0.075)	(0.075)	(0.103)	(0.054)
Adjusted-R^2	0.001	0.146	0.007	0.141	-0.004	0.711
Individual controls	No	Yes	No	Yes	No	Yes
ALMP measures	No	Yes	No	Yes	No	Yes
Regional controls	No	Yes	No	Yes	No	Yes
Personality traits	No	Yes	No	Yes	No	Yes
Number of observations	598	598	598	598	559	559

Notes: The table shows the regression results of equation (4.5.1) when defining treatment and control groups by November 12, 2007. Standard errors (in parentheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4.8.5: Benefit duration & job search: accounting for selective layoffs

	Job applications		Distant applications		Reservation wage	
	(1)	(2)	(3)	(4)	(5)	(6)
Date of Reform	8.148	13.306*	0.095	0.205**	-0.034	0.006
	(6.112)	(7.046)	(0.069)	(0.089)	(0.101)	(0.075)
Age Group Dummy	3.206	3.888	0.063	0.335**	-0.066	0.012
	(2.557)	(7.978)	(0.088)	(0.134)	(0.141)	(0.111)
Treatment Effect	-10.714	-19.745*	-0.206**	-0.260**	0.097	0.008
	(6.587)	(10.082)	(0.102)	(0.110)	(0.160)	(0.108)
Adjusted-R^2	-0.002	0.331	0.024	0.136	-0.014	0.610
Individual controls	No	Yes	No	Yes	No	Yes
ALMP measures	No	Yes	No	Yes	No	Yes
Regional controls	No	Yes	No	Yes	No	Yes
Personality traits	No	Yes	No	Yes	No	Yes
Number of observations	200	200	200	200	190	190

Notes: The table shows the regression results of equation (4.5.1) when reducing the scope of strategic firm behavior. Standard errors (in parentheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4.8.6: Benefit duration & job search: pseudo treatment effects

	Job applications		Distant applications		Reservation wage	
	(1)	(2)	(3)	(4)	(5)	(6)
Date of Reform	-0.880	1.205	0.057	0.053	-0.044	-0.052
	(2.354)	(2.230)	(0.045)	(0.044)	(0.058)	(0.041)
Age Group Dummy	-1.303	-5.634	-0.026	-0.066	-0.014	-0.012
	(2.646)	(4.000)	(0.049)	(0.068)	(0.072)	(0.053)
Pseudo Treatment	3.621	4.597	0.009	0.025	0.018	0.047
	(3.129)	(3.193)	(0.061)	(0.058)	(0.085)	(0.048)
Adjusted-R^2	-0.001	0.113	0.002	0.140	-0.004	0.716
Individual controls	No	Yes	No	Yes	No	Yes
ALMP measures	No	Yes	No	Yes	No	Yes
Regional controls	No	Yes	No	Yes	No	Yes
Personality traits	No	Yes	No	Yes	No	Yes
Number of observations	639	639	639	639	595	595

Notes: The table shows the regression results of equation (4.5.1) when focusing on two groups of workers who were unaffected by the reform. Standard errors (in parentheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Table 4.8.7: Benefit duration & applying for distant jobs: heterogeneous effects

Dep. Var.: Distant Applications	(1)	(2)	(3)	(4)	(5)	(6)
Date of Reform	0.061	0.073	0.098**	0.072	0.076*	0.072
	(0.040)	(0.047)	(0.041)	(0.047)	(0.039)	(0.047)
Age Group Dummy	0.144**	0.195**	0.142**	0.194**	0.138**	0.193**
	(0.057)	(0.080)	(0.058)	(0.080)	(0.058)	(0.081)
Treatment X Female	-0.199***	-0.225***				
	(0.066)	(0.068)				
Treatment X Male	-0.203**	-0.211***				
	(0.079)	(0.075)				
Treatment X Low-Skilled			-0.307***	-0.265***		
			(0.088)	(0.098)		
Treatment X Medium-Skilled			-0.190***	-0.217***		
			(0.066)	(0.065)		
Treatment X High-Skilled			-0.189*	-0.204*		
			(0.113)	(0.111)		
Treatment X Not UE before					-0.224***	-0.220***
					(0.078)	(0.074)
Treatment X UE before					-0.188***	-0.217***
					(0.071)	(0.071)
Adjusted-R^2	0.031	0.149	0.077	0.148	0.019	0.149
Individual controls	No	Yes	No	Yes	No	Yes
ALMP measures	No	Yes	No	Yes	No	Yes
Regional controls	No	Yes	No	Yes	No	Yes
Personality traits	No	Yes	No	Yes	No	Yes
Number of observations	598	598	598	598	598	598

Notes: The table shows the regression results of equation (4.5.1), allowing for heterogeneous treatment effects by (a) gender, (b) education, and (c) prior unemployment experience. The dependent variable indicates whether individuals apply for jobs that require moving. Standard errors (in parentheses) are heteroscedasticity robust. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

Chapter 5

The Economic Costs of Mass Surveillance*

5.1 Introduction

Millions of individuals are or have been spied upon by their own government. According to the Democracy Index 2012, published by the Economist Intelligence Unit, 37 percent of the world population lives in authoritarian states. A key feature of these regimes is the aim to control all aspects of public and private life at all times. For this purpose, large-scale surveillance systems are installed that constantly monitor societal interactions, identify and silence political opponents, and establish a system of obedience by instilling fear (Arendt, 1951).[1] A large and growing literature suggests that such environments of distrust should exhibit adverse economic effects (Algan and Cahuc, 2010; Alesina and Giuliano, 2015). Spying is likely to destroy interpersonal and institutional trust, i.e., social capital. As all economic transactions in turn involve an element of trust between trading

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* This chapter circulates as “The Economic Costs of Mass Surveillance: Evidence from Stasi Spying in East Germany”, see Lichter et al. (2015).

[1] We acknowledge that democratic countries usually spy on their populations as well. Thus, it is obvious that there is no clear line between democracies and authoritarian states in this respect. In this paper, we are interested in the effect of surveillance on economic performance and leave definitional discussions aside. This also concerns the lively debate in political science on how to precisely define and distinguish different forms of authoritarian regimes, such as totalitarian, despotic or tyrannic systems.

partners (Arrow, 1972; Putnam, 1995), government surveillance should exhibit adverse economic effects.

Despite the prevalence of surveillance systems around the world, there is no empirical evidence on the effect of spying on economic performance. This is most likely due to the fact that it is challenging to establish a credible research design. The empirical challenge is to find random variation in surveillance intensities while keeping other policies affecting trust and economic performance constant. The common trend requirement with regard to other policies makes cross-country settings basically inviable as isolating the effect of spying from the authoritarian policy mix seems impossible. For credible single-country research designs, two conditions have to be met: (i) there should be observable variation in surveillance density (regionally or over time) and (ii) the variation in the intensity of the treatment has to have at least a random component.

In this paper, we aim to overcome these empirical challenges and estimate the effect of state surveillance on social capital and economic performance by using official data on the regional number of spies in the former socialist German Democratic Republic (GDR). We argue that the surveillance system implemented by the GDR regime from 1950 to 1990 was a setting that fulfills both conditions for a valid research design. The official state security service of the GDR, the Ministry for State Security (*Ministerium für Staatssicherheit*), commonly referred to as the Stasi, administered a huge network of spies called "unofficial collaborators" (*Informelle Mitarbeiter*, IM). These spies were ordinary people, recruited to secretly collect information on any societal interaction in their daily life that could be of interest to the regime. We use the substantial regional variation in the intensity of spying across GDR counties (*Kreise*) to estimate the effect of surveillance on long-term *post-regime* outcomes of social capital and economic performance, measured in the 1990s and 2000s, i.e., after the fall of the Iron Curtain and Germany's reunification.[2]

[2] An earlier study by Jacob and Tyrell (2010), and a recent paper by Friehe et al. (2015), which has been conducted simultaneously to and independently of our study, present cross-sectional OLS regressions showing that Stasi spying is negatively associated with some measures of personality traits and social capital – assuming the regional spy density to be random. We demonstrate below that the number of Stasi spies in a county was to a large part driven by state-level decisions and county characteristics.

Given that condition (i) is fulfilled, the remaining challenge for identification is to establish exogenous variation in the intensity of spying. Although historians and scholars from related disciplines have not yet identified an obvious regional allocation pattern of spies, it is a priori unlikely that the spy allocation was purely random. While we demonstrate that endogeneity is likely to drive estimates towards zero, yielding a lower bound, we implement two different research designs to overcome doubts on identification.[3]

The first design exploits the specific territorial-administrative structure of the secret security service. County offices of the Stasi were subordinate to the respective state (*Bezirk*) office and each state office in turn bore full responsibility to secure its territory, leading different state offices to administer different average levels of spy densities. Indeed, around 25% of the variation in the spy density at the county level can be explained with state fixed effects. We use the resulting discontinuities along state borders as the source of exogenous variation and limit our analysis to all contiguous county pairs that straddle a GDR state border (see Dube et al., 2010, for an application of this identification strategy in the case of minimum wages). Hence, identification comes from different intensities of spying, induced by different state surveillance strategies, within county pairs on either side of a state border. The identifying assumption is that border pair counties are similar in all other respects; we carefully test this assumption. An advantageous feature of our border discontinuity setting is that many of the GDR state borders do not exist anymore as GDR states were merged into much larger federal states after reunification. Indeed, around fifty percent of the counties straddling a former GDR state border in our sample are nowadays part of the same federal state.

For our second identification strategy, we follow Moser et al. (2014) and construct a county-level panel data set that covers both pre- and post-treatment years. This research design enables us to include county fixed effects to account for time-invariant confounders, such as a regional liberalism, which might have affected the allocation of Stasi spies and may also affect the economic prosperity of a county. Using pre-treatment data from the 1920s and early 1930s, this design enables us to directly test for pre-trends in the outcome variables. Reassuringly, the Stasi dens-

[3] Indeed, the results of our analysis suggest that simple (non-reported) OLS estimates are biased towards zero.

ity has no explanatory power for social capital and economic performance *prior* to the division of Germany, which strengthens the causal interpretation of our findings. Similarly, controlling for a large set of historical pre-treatment variables that account for persistent regional differences in economic potential, political ideology and social capital does not affect the estimates of our border pair research design qualitatively.

Overall, we find a negative and long-lasting effect of spying on both social capital and economic performance.[4] Using data from the German Socio-Economic Panel (SOEP), we find that more government surveillance leads to lower trust in strangers and stronger negative reciprocity. Both measures have been used as proxies for interpersonal trust in the literature (Glaeser et al., 2000; Dohmen et al., 2009). In line with evidence on the shaping of trust levels (Sutter and Kocher, 2007), the negative effect on interpersonal trust is strongest for the cohort who spent their entire childhood in the GDR. Looking at institutional trust, we find that both the intention to vote and engagement in local politics is significantly lower in counties with a high spy density even two decades after reunification. Using county-level data, we find that election turnout has been significantly lower in higher-spying counties in federal elections from 2002 onwards.

In terms of economic performance, we find a negative significant effect of the spy density on labor income as reported in the SOEP. Using administrative county-level data, we further show that self-employment rates and the number of patents per capita are significantly lower in higher-spying counties. Moreover, post-reunification unemployment is persistently higher in counties with high surveillance levels. Our estimates imply that abolishing state surveillance would, on average, have reduced the long-term unemployment rate by 1.8 percentage points, which is equivalent to a ten percent drop given the average unemployment level in East Germany since reunification. Last, we find significantly negative effects of the spy density on population: Stasi spying appears to be an important driver of the tremendous population decline experienced in East Germany after reunifica-

[4] The annual number of requests for disclosure of information on Stasi activity (*Bürgeranträge*) serves as a first indication that East German citizens are still affected by the surveillance of the Stasi, even twenty-five years after reunification. Figure 5.7.1 in the Appendix plots the annual number of requests filed from 1992 to 2012. Unfortunately, there is no regional information on these requests, which could provide an interesting outcome.

tion. We find that for both out-migration waves (1989–1992, and 1998–2009, see Fuchs-Schündeln and Schündeln, 2009), population losses were relatively stronger in higher-spying counties.

Overall, our paper contributes to the large literature documenting a long-term positive effect of the quality of political institutions (oftentimes used as measures of social capital) on economic performance using cross-country research designs (Mauro, 1995; Knack and Keefer, 1997; Hall and Jones, 1999; Sobel, 2002; Rodrik et al., 2004; Nunn, 2008; Nunn and Wantchekon, 2011; Acemoglu et al., 2015). We add to this strand of the literature in several ways. First, we study the impact of a certain element of (lacking) democracy – state surveillance – on economic performance. Second, we do this in a within-country setting, which is close to a natural experiment and therefore allows for a clean identification of our effect. Third, by using surveillance as our source of variation and rich survey data, we are able to directly link variations in social capital (trust) to changes in economic performance. In fact, using two-stage least squares and spy density as an instrument, we are able to show that higher trust has a positive and causal effect on income. Fourth, the persistence of the adverse economic effects documents the long-term costs of eroding social capital and the transmission of trust across generations (Guiso et al., 2006; Algan and Cahuc, 2010; Tabellini, 2010; Becker et al., 2015). Lastly, our study is related to the influential study by Alesina and Fuchs-Schündeln (2007). While their study shows that ideological indoctrination in the GDR had long-term effects on individual preferences, we show that the same is true for the type of governance used to strengthen the power of the regime.

The remainder of this paper is organized as follows. Section 5.2 presents the historical background and the institutional framework of the Stasi. Section 5.3 describes the data. Section 5.4 introduces our research design and explains the two different identification strategies. Results are presented in Section 5.5, before Section 5.6 concludes.

5.2 Historical background

After the end of World War II and Germany's liberation from the Nazi regime in 1945, the remaining German territory was occupied by and divided among the four Allied forces – the US, the UK, France and the Soviet Union. The boundaries between these zones were drawn along the territorial boundaries of 19th-century German states and provinces that had largely disappeared by then (Wolf, 2009). On July 1, 1945, roughly two months after the total and unconditional surrender of Germany, the division into the four zones became effective.

With the Soviet Union and the Western allies disagreeing over Germany's political and economic future, the borders of the Soviet occupation zone soon became the official inner-German border and eventually led to a 40-year long division of a society that had been highly integrated prior to its separation. In May 1949, the Federal Republic of Germany was established in the three western occupation zones. Only five months later, the German Democratic Republic, a state in the spirit of "real socialism"[5] and member state of the Warsaw pact, was founded in the Soviet ruled zone. Until the sudden and unexpected fall of the Berlin Wall on the evening of November 9, 1989 and the reunification of West and East Germany in October 1990, the GDR was a one-party dictatorship under the rule of the Socialist Unity Party (SED) and its secretaries general.

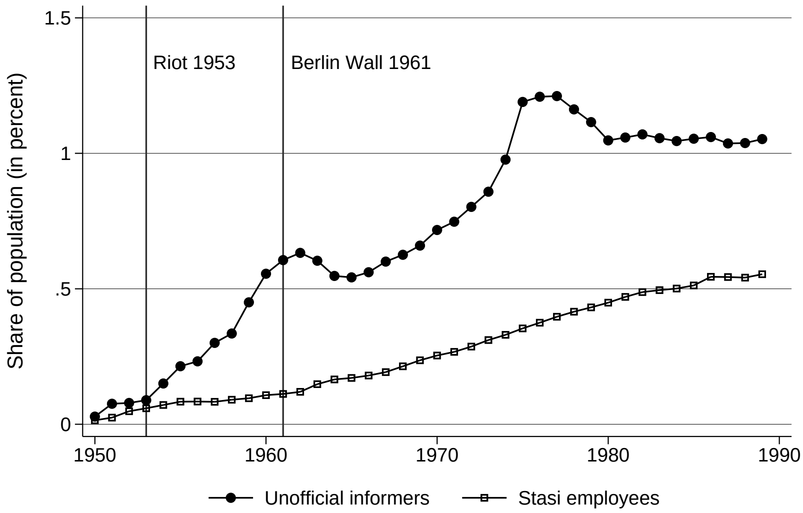
The regime secured its authority by means of a large and powerful state security service. The Ministry for State Security was founded in February 1950, just a few month after the GDR was constituted, and designed to "battle against agents, saboteurs, and diversionists [in order] to preserve the full effectiveness of [the] Constitution"[6]. It soon became a ubiquitous institution, spying on and suppressing the entire population to ensure and preserve the regime's power (Gieseke, 2014, p. 50ff.).

The party leaders' demand for comprehensive surveillance was reflected by the organizational structure of the Stasi. While the main administration was located in East Berlin, the Stasi also maintained offices in each capital of the fifteen states (*Bezirksdienststellen*), regional offices in most of the 226 counties (*Kreisdienststellen*) and offices in seven Objects of Special Interest, which were large and strategic-

[5] Erich Honecker, Secretary General of the SED between 1971–1989, introduced this term on a meeting of the Central Committee of the SED in May 1973 to distinguish the regimes of the Eastern bloc from Marxist theories on socialism.

[6] According to Erich Mielke, subsequent Minister for State Security from 1957 to 1989, on January 28, 1950 in the official SED party newspaper *Neues Deutschland* as quoted in Gieseke (2014, p. 12).

Figure 5.2.1: Share of Stasi employees & spies in the GDR population



Notes: Own calculations using data on the total number of spies (unofficial collaborators) from Müller-Enbergs (2008), information on the number of Stasi employees as reported in Gieseke (1996), and population figures from Statistical Yearbooks of the GDR.

ally important public companies (*Objektdienststellen*).[7] Following this territorial principle, state-level offices had to secure their territory and had authority over their subordinate offices in the respective counties. As a consequence, surveillance strategies differed in their intensities across GDR states. For instance, about one-third of the constantly-monitored citizens (*Personen in ständiger Überwachung*) were living in the state of Karl-Marx-Stadt (Horsch, 1997), which accounted for only eleven percent of the total population. Likewise, the state of Magdeburg accounted for 17 percent of the two million bugged telephone conversations, while this state only accounted for eight percent of the total GDR population. We exploit this variation in surveillance intensities across states for identification (see Section 5.4.2).

Over the four decades of its existence, the Stasi continuously expanded its competencies and duties as well as the surveillance of the population. The unforeseen national uprising on and around June 17, 1953 revealed the weakness of the secret security service in its early years and caused a subsequent transformation

[7] Note that the Stasi monitored economic activity but was not involved in economic production.

and expansion. The number of both official employees and unofficial collaborators continuously increased until the late 1970s and remained at a high level until the breakdown of the regime in 1989. Figure 5.2.1 plots the share of regular employees and unofficial collaborators in the population for the period of 1950 until 1989. In absolute terms, the Stasi listed 90,257 regular employees and 173,081 unofficial informants by the end of 1989, amounting to around 1.57 percent of the entire population.[8]

The Stasi's most important tool of surveillance and suppression, and its "main weapon against the enemy"[9] was the dense network of spies called unofficial collaborators. These spies were recruited from the population and instructed to secretly collect information about individuals in their own social network. Being friends, colleagues, neighbors or sport buddies of the individuals they spied on, collaborators were able to provide valuable personal information that complemented the Stasi's knowledge of the population and helped creating an overall picture about anti-socialist and dissident movements and hence guaranteed surveillance of the society's everyday life (Gieseke, 2014, p. 163ff.). At the same time, the threat of being denunciated and the concealed presence of the state security caused an atmosphere of mistrust and suspicion (Wolle, 2009; Gieseke, 2014).[10]

There were different reasons for serving as a collaborator. Some citizens agreed to cooperate due to ideological reasons, others were intrigued by personal and material benefits accompanied with their cooperation. However, the regime also urged citizens to act as unofficial collaborators by creating fear and pressure. The body of spies was administrated in a highly formalized way, with cooperation being sealed in written agreements and spies being tightly led by a responsible official

[8] Note that the number of regular employees of the Stasi was notably high when being compared to the size of other secret services in the Eastern Bloc (Gieseke, 2014, p. 72). Although figures on the number of spies in other communist countries during times of the Iron Curtain entail elements of uncertainty, the level of surveillance was comparable to the Soviet Union.

[9] Directive 1/79 of the Ministry for State Security for the work with unofficial collaborators (Müller-Enbergs, 1996, p. 305).

[10] For example, Wolle (2009) characterizes the society as deeply torn. Spies were oftentimes in close contact with the spied-upon person and citizens felt the Stasi's presence like a "scratching t-shirt" (Reich, 1997, p. 28). For less scientific documentations about the impact of the Stasi, see the Academy Award winning movie "The Lives of Others" and the recent TED talk "The dark secrets of a surveillance state" given by the director of the Berlin-Hohenschnhausen Stasi prison memorial, Hubertus Knabe.

Stasi agent (Gieseke, 2014, p. 114ff.).

5.3 Data

In this section, we briefly describe the various data sources collected for our empirical analysis. Section 5.3.1 presents information on our explanatory variable, the spy density in a county. Section 5.3.2 and Section 5.3.3 describe the data used to construct outcome measures and control variables. Detailed information on all variables are provided in Appendix Table 5.8.3. The Data Appendix 5.8 also provides details on the harmonization of territorial county borders over time.

5.3.1 Spy data

Information on the number of spies in each county is based on official Stasi records, published by the Agency of the Federal Commissioner for the Stasi Records (*BStU*) and compiled in Müller-Enbergs (2008). Although the Stasi was able to destroy part of its files in late 1989, much information was preserved when protesters started to occupy Stasi offices across the country. In addition, numerous shredded files could be restored after reunification. Since 1991, individual Stasi records are publicly available for personal inspection as well as requests from researchers and the media.

Given that the Stasi saw unofficial collaborators as their main weapon of surveillance, we choose the county-level share of unofficial collaborators in the population as our main measure of the intensity of surveillance. Most regular Stasi officers were based in the headquarter in Berlin, and only 10-12 percent of them were employed at the county level.[11] In contrast, the majority of all unofficial collaborators were attached to county offices. The Stasi differentiated between three types of unofficial collaborators: (1) collaborators for political-operative penetration, homeland defense, or special operations as well as leading informers, (2) collaborators providing logistics and (3) societal collaborators, i.e., individuals

[11] Nevertheless, we note that the county-level correlation between the number of spies and the number of regular employees is 0.85. This high correlation reflects the fact that regular Stasi employees administered the body of spies at the county level.

publicly known as loyal to the state. We use the first category of unofficial collaborators to construct our measure of surveillance density, as those were actively involved in spying and are by far the largest and most relevant group of collaborators. If an Object of Special Interest with a separate Stasi office was located in a county, we add the unofficial collaborators attached to these object offices to the county's number of spies.[12] As information on the total number of spies are not given for each year in every county, we use the average share of spies from 1980 to 1988 as our measure of surveillance.[13] The spy density in a given county was very stable across the 1980s, the within-county correlation being 0.93. For further details on our main explanatory variable, see Data Appendix 5.8.

Figure 5.3.1 plots the density of unofficial collaborators (spy category 1) for each county. Today, the number of spies is known for about ninety percent of the counties for at least one year in the 1980s. The density of spies differs considerably both across and within GDR states, with the fraction of unofficial collaborators in the population ranging from 0.12 to 1.03 percent and the mean density being 0.38 percent. The median is similar to the mean (0.36 percent), one standard deviation refers to 0.14 spies per capita.

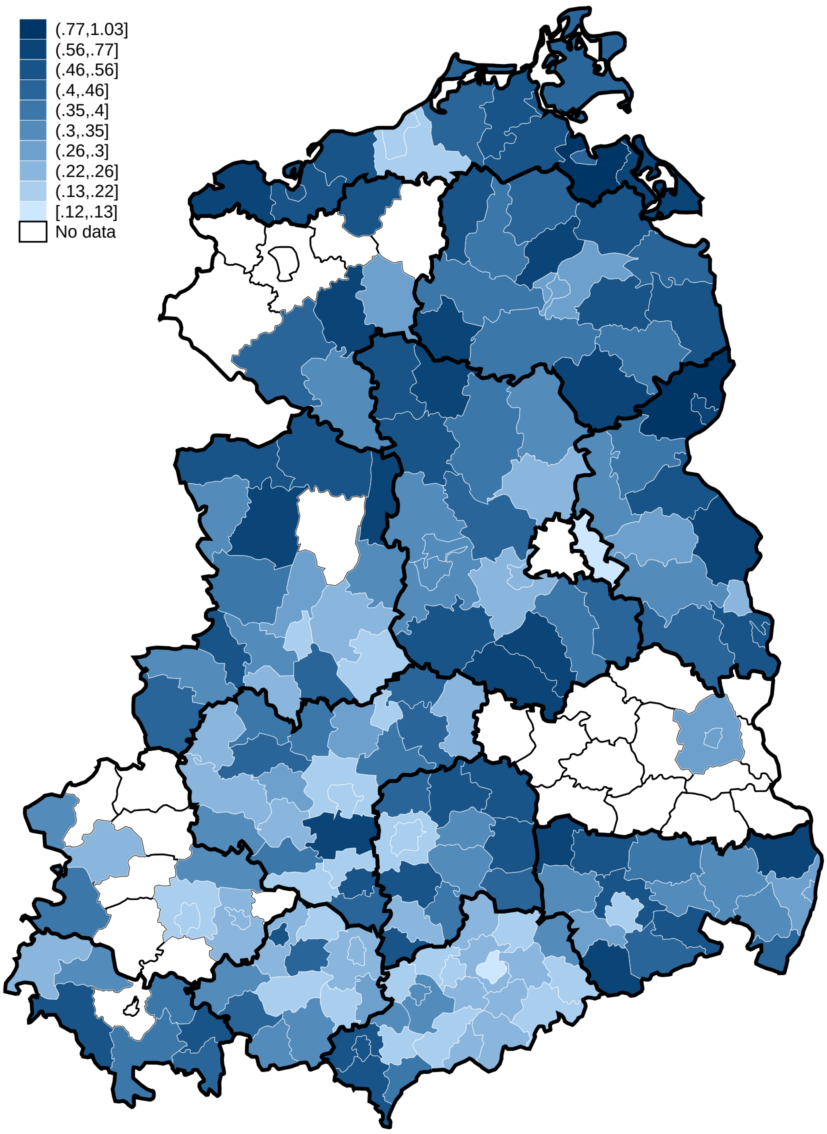
5.3.2 Individual-level data

For the empirical analysis presented below, we rely on two distinct datasets to estimate the effect of state surveillance on social capital and economic performance. First, we use information from the German Socio-Economic Panel Study (SOEP), a longitudinal survey of German households (Wagner et al., 2007). Established for West Germany in 1984, the survey covers respondents from the former GDR since June 1990. The SOEP contains information on the county of residence and when individuals have moved to their current home. We identify and select respondents living in East German counties in 1990 who have not changed residence in 1989 or 1990. We then follow these individuals from the 1990 wave of the SOEP over time. By exploiting a variety of different waves of the survey we are able to observe various measures of social capital as well as current gross labor income (see Section

[12] In the empirical analysis, we explicitly control for the presence of such offices in Objects of Special Interest.

[13] Data from earlier years is only available for a limited number of counties.

Figure 5.3.1: Percentage share of Stasi spies at the county level



Notes: This graph plots the county-level surveillance density measured by the average yearly share of unofficial collaborators relative to the population between 1980 and 1988. Thick black lines show the borders of the fifteen GDR states. White areas indicate missing data.

5.4.2 and Data Appendix 5.8).

In order to proxy interpersonal trust, we use two standard measures provided in the SOEP: (i) trust in strangers (see, e.g., Glaeser et al., 2000), and (ii) negative reciprocity (see, e.g., Dohmen et al., 2009). To capture trust in the political system, we investigate two measures as well. First, we take the survey question about the intention to vote if federal elections were held next Sunday. The question captures the stated preferences to participate in the most important German election.[14] Second, we exploit the question whether individuals are engaged in local politics.

Apart from these variables measuring social capital, we also take reported monthly gross labor income as a measure of individual economic performance. Moreover, we use the rich information of the SOEP to construct a set of individual control variables: gender, age, household size, marital status, level of education and learned profession. For the underlying survey questions, data years and exact variable definitions, see Data Appendix 5.8.

5.3.3 County-level data

For the second dataset, we compiled county-level data on various measures of economic performance (self-employment, patents, unemployment, population) as well as electoral turnout as a proxy for social capital. We collected county-level data for two time-periods, data from the 1990s and 2000s as well as pre-World War II data. Post-reunification data come from official administrative records; historical data come from various sources (see Data Appendix 5.8 for details).

In addition to our outcome variables, we further collect various county-level variables that we use as control variables to check the sensitivity of our estimates. These control variables are used in both individual- and county-level models.

In total, we construct three sets of control variables. The first set measures the strength of the *opposition* to the regime. As mentioned in Section 5.2, the national uprising on and around June 17, 1953 constituted the most prominent rebellion against the regime before the large demonstrations in 1989. The riot markedly changed the regime's awareness for internal conflicts and triggered the expansion of the Stasi spy network (cf. Figure 5.2.1). We use differences in the regional intensity of the riot to proxy the strength of the opposition. Specifically, we construct three control variables: (i) a categorical variable measuring the strike intensity with values "none", "strike", "demonstration", "riot", and "liberation of prisoners", (ii) a dummy variable indicating whether the regime declared a state

[14] We also use a measure of revealed preferences, i.e., electoral turnout, below.

of emergency in the county and (iii) a dummy equal to one if the Soviet military intervened in the county (for details on the source and the construction of the variables, see Appendix Table 5.8.3).

The second set of controls takes into account that the Stasi tried to protect certain firms in the industrial sector. Hence, our *industry* controls comprise (i) the share of employees in the industrial sector in 1989 and (ii) a dummy variable indicating whether a large enterprise from the uranium, coal, potash, oil or chemical industry was located in the county.

The third set of controls is intended to pick up historical and potentially persistent county differences in terms of economic performance and political ideology. It will be used in the models on the individual level in the absence of pre-treatment information on the outcomes. Our *pre World War II* controls include (i) the mean share of Nazi and Communist votes in the federal elections of 1928, 1930 and the two 1932 elections to capture political extremism (Voigtländer and Voth, 2012), (ii) average electoral turnout in the same elections to proxy institutional trust, (iii) the regional share of protestants in 1925 in order to control for differences in work ethic and/or education (Becker and Wößmann, 2009), (iv) the share of self-employed in 1933 to capture regional entrepreneurial spirit, and (v) the unemployment rate in 1933 to capture pre-treatment differences in economic performance.

5.4 Research designs

We present two distinct research designs to identify the causal effect of spying on social capital and economic performance. First, we lay out a very simple linear model as a benchmark and discuss potential threats to identification and likely biases (Section 5.4.1). Based on this discussion, we propose two empirical approaches intended to overcome endogeneity problems (Sections 5.4.2 and 5.4.3).

5.4.1 Linear model

To identify the long-term effects of surveillance, we regress various measures of social capital and economic performance on our measure of surveillance intensity.

The simplest model takes the following form:

$$Y_j = \alpha + \beta SPYDENS_c + \boldsymbol{V_j'\xi} + \varepsilon_j, \tag{5.4.1}$$

where Y_j measures an outcome that may either vary at the individual or county level, $j \in [i, c]$. Our main regressor is the spy density in county c, defined as the average number of spies per capita in each county of the GDR in the 1980s.[15] Vector $\boldsymbol{V_j}$ may contain control variables. In this simple model, identification comes from cross-sectional variation in the intensity of surveillance across GDR counties (see Jacob and Tyrell, 2010, and Friehe et al., 2015, for empirical applications of this model). Two main threats to identification are obvious: (i) selection out of treatment and (ii) omitted variable bias. We discuss these concerns in turn.

Selection out of treatment. If people moved away from counties with a high spying density, we would face a selection problem that could bias our estimates. However, the authoritarian regime controlled and limited external and internal migration in a very strict way, making residential sorting a secondary concern.

First, leaving the East German territory without permission was illegal throughout the existence of the GDR. Refugees could be sentenced to lengthy terms of imprisonment. However, about three million citizens had escaped to West Germany up until the early 1960s, which was the main reason for the construction of the Berlin Wall and the expansion of border fortifications in August 1961. Consequently, the large-scale installation of land-mines at the borderland and the regime's order for soldiers to shoot at refugees trying to pass the border led to a sharp drop in the number of refugees. The regime also often punished those individuals who applied for emigration visas, exposing people to considerable harassment in working and private life (Kowalczuk, 2009). Between 1962 and 1988, only around 18,000 individuals (0.1 percent of the population) managed to leave East Germany each year, either by authorized migration (*Übersiedler*) or illegal escape (see Panel (a) in Figure 5.7.2 in the Appendix). The share of refugees on the total number of migrants was around one-third.

Second, residential mobility within the GDR was highly restricted as all living

[15] Results do not change when using the density of specific years as a regressor.

space was administered by the regime. In every municipality, a local housing agency (*Amt für Wohnungswesen*) decided on the allocation of all houses and flats, whether privately, cooperatively or publicly owned. Every individual looking for a new apartment had to file an application at the local housing agency. Processing times often lasted several years and assignment to a new flat was usually subject to economic, political or social interests of the regime (Grashoff, 2011, p. 13f.). From 1975 to 1988, the average number of yearly applications was 755,000, constituting around 4.5 applications per 100 citizens (Steiner, 2006).[16] Panel (b) of Figure 5.7.2 in the Appendix shows the extent of residential mobility in the GDR. Mobility of East German citizens had been considerably lower compared to mobility in West Germany.

Observing the county-level number of spies in multiple years in the 1980s, we can further directly test whether the spy density affected population size. Reassuringly, we estimate a zero effect of the log number of spies on log population in a model with county and year fixed effects.[17] Hence, selection out of treatment does not seem to be an issue in our setting.

Confounding variables. The second, more serious threat to identification are regional confounders that have affected the allocation of Stasi spies in the 1980s and that affect our outcomes of interest after the fall of the Iron Curtain. Astonishingly, there is very little knowledge on what determined the regional spy density. There is some anecdotal evidence that the Stasi was particularly active in regions with strategically important industry clusters. In contrast, and a bit surprisingly, previous research could not establish a clear correlation between the size of the Stasi and the size of the opposition at the county level (Gieseke, 1995, p. 190).

Before investigating the effects of spying on social capital and economic outcomes, we thus try to explain the regional variation in the spy density, which will be our treatment variable later on. Therefore, we run simple OLS regressions of

[16] Some citizens tried to elude the governmental allocation by illegal and unseen movements into dilapidated flats. There are no official records about the actual number of illegal squatters. Estimates for the city of Rostock show that the share of squatters within the population was small, amounting to 0.28 percent in early 1990 (Grashoff, 2011, p. 76).

[17] The corresponding regression results are available upon request.

the spy density on six sets of potential explanatory variables and check the explanatory power of the model as indicated by the R^2 measure. Table 5.4.1 shows the results, while full regression outputs are provided in Appendix Table 5.7.1. We start off by explaining the spy density with a constant and a dummy variable, which is equal to one if one of the seven Objects of Special Interest, that is, a large public company of strategic importance, was located in the county.[18] In the next specification, we add dummy variables for the fifteen GDR states. The R^2 measure in column (2) shows that around 25 percent of the county-level variation can be explained by differences across GDR states. This is suggestive evidence in line with the claim of historians that county offices responded to higher-ranked state offices and that decisions made at the state level indeed affected county-level outcomes. We will exploit this feature in our border discontinuity design presented in Section 5.4.2.

Table 5.4.1: The allocation of Stasi spies

	(1)	(2)	(3)	(4)	(5)	(6)
GDR state FE	No	Yes	Yes	Yes	Yes	Yes
County size controls	No	No	Yes	Yes	Yes	Yes
Opposition controls	No	No	No	Yes	Yes	Yes
Industry controls	No	No	No	No	Yes	Yes
Pre World War II controls	No	No	No	No	No	Yes
Observations	187	187	187	187	187	187
R^2	0.033	0.298	0.529	0.540	0.545	0.561
Adjusted R^2	0.028	0.237	0.481	0.475	0.474	0.473

Notes: This table demonstrates the power of different sets of county-level control variables in explaining the county spy density using a simple OLS regression. Every specification includes a constant and a dummy for Objects of Special Interest (*Objektdienststellen*). Full regression results are shown in Appendix Table 5.7.1.

In the third specification of Table 5.4.1, we add variables controlling for the *size* of the county. While the spy density already accounts for differences in county population, we add the log mean county population in the 1980s and the log square meter area of the county as regressors. We find that controlling for size – and

[18] As described in Section 5.3.1, the Stasi maintained offices in these objects, which recruited their own spies. As we add the spies working in these objects to the number of spies in the respective county offices, we control for Objects of Special Interest with a dummy variable in all regressions below.

in particular population – increases the explanatory power substantially, raising the R^2 to 0.53. Moreover, the results show that the spy density is decreasing in the population (cf. Appendix Table 5.7.1), which could be rationalized with an economies of scale argument. Overall, column (3) suggests that it might be important to control for county size when identifying the effect of the Stasi on our outcomes. We test this assertion below.

In columns (4) to (6) we sequentially add *opposition*, *industrial* and *pre World War II* controls (see Section 5.3.3). In total, neither of the three sets of control variables adds much to the explanatory power of the model. Nevertheless, we test the sensitivity of our results with respect to the inclusion of the control variable sets in both research designs laid out below.

Unobserved confounders and potential bias. While controlling for observable potential confounders may demonstrate the robustness of estimates, it is impossible to prove that there are no unobservable variables biasing simple OLS results in our setting. Let us assume that there is a systematic confounding variable Z_c, such as capitalist spirit or strive for freedom, that varies across regions. Given that measures of liberal attitudes are usually positively correlated with social capital and economic performance in democratic countries, it is likely that an unobserved confounder with a positive (negative) correlation with the spy density also has a positive (negative) correlation with our outcomes. With this claim in mind, we study the potential endogeneity bias more formally. We rewrite the error term of equation (5.4.1) as $\varepsilon_j = \gamma Z_c + \eta_j$, with γ being the effect of the unobserved capitalist spirit on Y_j and η_j being noise. In such a case, the OLS estimate would be given by:

$$\begin{aligned}\beta^{OLS} &= \frac{Cov(SPYDENS_c, \varepsilon_j)}{Var(SPYDENS_c)} \\ &= \beta + \gamma \frac{Cov(SPYDENS_c, Z_c)}{Var(SPYDENS_c)} + \underbrace{\frac{Cov(SPYDENS_c, \eta_j)}{Var(SPYDENS_c)}}_{=0}. \end{aligned} \tag{5.4.2}$$

If, as argued above, the effect of capitalist spirit on the outcome γ and the covariance between capitalist spirit and the spying density $Cov(SPYDENS_c, Z_c)$

have the same sign, and if, as suggested by the theory of social capital, $\beta < 0$, the estimate β^{OLS} will be biased towards zero and underestimate the effect of spying on our outcomes.[19]

In the following subsections, we present two research designs which are intended to better account for unobserved confounders and limit the potential endogeneity bias.

5.4.2 Border discontinuity design

Our first identification strategy exploits the territorial-administrative structure of the Stasi and the fact that about 25 percent of the county-level variation in the spy density can be explained with GDR state fixed effects (cf. Table 5.4.1, column (2)). As the Stasi's county offices were subordinate to the respective state office, different GDR states administered different average levels of spy densities across states. We use the resulting discontinuities along state borders as a source of exogenous variation. We follow Dube et al. (2010) and limit our analysis to all contiguous counties that straddle a GDR state border, thus identify the effect of spy density on our outcome variables by comparing county pairs on either side of a state border.[20] The identifying assumption is that the county on the lower-spy side of the border is similar to the county on the higher-spy side in all other relevant characteristics. While such an assumption can be quite strong in similar border research designs, it might be less critical in our case given that we focus on post-GDR outcomes and many GDR state borders do not exist anymore. In fact, after reunification the fifteen GDR states merged into six federal states, and around half of the counties straddling a GDR border in our sample belong to the same federal state in post-reunification Germany.

Formally, we regress individual outcome i in county c, which is part of a border pair b, on the spy density in county c and border pair dummies ν_b:

$$Y_{icb} = \alpha + \beta SPYDENS_c + \boldsymbol{X_i'}\boldsymbol{\delta} + \boldsymbol{K_c'}\boldsymbol{\phi} + \nu_b + \varepsilon_{icb}. \tag{5.4.3}$$

[19] The results of our analysis indeed suggest that OLS estimates are biased towards zero. OLS regression results are available upon request.

[20] If a county has several direct neighbors on the other side of the state border, we duplicate the observation. See below for a discussion.

As outcome variable, Y_{icb}, we use trust in strangers, extent of negative reciprocity, intention to vote in elections, engagement in local politics and individual gross income (see Section 5.3.2).

As mentioned before, the identifying assumption in the border discontinuity design is that counties on either side of a border differ systematically in their spy density since they belonged to different GDR states. Apart from that, there should be no systematic differences between the counties straddling a former state border. However, there might be *persistent* compositional or historical differences within county-border pairs which affected the spy allocation in the 1980s as well as the post-reunification outcomes. For that reason, we add two sets of control variables as a sensitivity check. First, vector $\boldsymbol{X_i}$ accounts for compositional differences in the population and includes individual information provided by the SOEP on age, gender, marital status, education and learned profession. Second, vector $\boldsymbol{K_c}$ controls for potential county-level differences within a border pair. It is important to understand that in order to invalidate our research design, these differences must (i) have influenced the spy allocation in the 1980s and (ii) affect outcome variables after reunification, making these factors *time-persistent* per definition. As a consequence, we include the county size, opposition, industry and pre-World War II controls that we use above to explain the variation in spy density (cf. Table 5.4.1).[21]

We use the cross-sectional weights provided by the SOEP to make the sample representative for the entire population. Given that we duplicate observations in counties that neighbor multiple counties in a different state, we adjust cross-sectional weights by dividing them through the number of duplications in our baseline specification and cluster standard errors at the border pair and the individual level. We test the robustness of our results by (i) disregarding cross-sectional weights and only accounting for duplications and (ii) by using original cross-sectional weights, not adjusting for duplicates. Results (shown in Appendix Table 5.7.2) prove to be robust to these modifications.

Table 5.4.2 further provides a test of the validity of our research design by checking whether counties straddling a state border are indeed similar. Based on

[21] Recall that we further control for Objects of Special Interest in all regressions (see Sections 5.3.1 and 5.4.1).

the GDR state average spy density, we assign one county in a border pair to either the higher- or the lower-spy state side. Table 5.4.2 shows the differences between higher- and lower-spying counties in terms of the spy density and all other control variables used in regression equation (5.4.3). We also test whether the differences are statistically significant by running simple bivariate OLS regressions of each characteristic on a dummy variable for higher/lower spy density counties.[22] We find that the spy density is indeed significantly higher in counties located in higher-spying GDR states. Moreover, apart from population, all control variables seem to be well balanced between the higher and lower spy density side.[23] The fact that the county population is slightly higher on the lower-spy side is in line with the results from Table 5.7.1: the spy density was lower in cities. For that reason, we test the sensitivity of our results with respect to population and county size as controls.

5.4.3 Panel data design

In Section 5.4.1, we discussed that time-persistent confounders that have affected the spy allocation and are still affecting post-reunification outcomes are a potential threat to identification. Given that the social capital measures obtained from the SOEP are only observed post-treatment, we cannot account for these time-persistent potential confounders by including county fixed effects.

However, certain outcomes such as measures of economic performance or political participation can be observed pre-treatment. Using county-level outcome variables from the late 1920s and early 1930s, we apply a panel data research design following Moser et al. (2014) that allows us to include county fixed effects to account for any time-invariant confounder.[24] The panel data model reads as follows:

$$Y_{ct} = \alpha + \sum_{t} \beta_t SPYDENS_c \times \tau_t + \boldsymbol{L'_{ct}\zeta} + \rho_c + \tau_t + \varepsilon_{ct}. \tag{5.4.4}$$

[22] Note that we use the same weights as in the regression.

[23] Note that Table 5.4.2 is based on the SOEP sample. As the SOEP does not sample individuals from every county, there is a small difference in the mean spy density in this sample compared to the overall mean (cf. Section 5.3.1).

[24] Note that many (though not all) potential confounders are likely to be time-invariant by definition, since they must have affected the spy allocation in the 1980s and outcomes in the 1990s and 2000s.

Table 5.4.2: Descriptive statistics for the border pair sample

	Mean	SD	Mean by county type: Low-spying	High-spying	Difference: Δ	p-value
Spy density	0.36	0.13	0.34	0.38	-0.04	0.04
County variables						
Log mean population 1980s	11.14	0.72	11.23	11.04	0.19	0.13
Log county size	6.14	0.52	6.10	6.19	-0.10	0.28
Object of Special Interest	0.03	0.17	0.01	0.04	-0.03	0.31
Share indust. employment 1989	45.70	12.15	45.85	45.55	0.30	0.89
Important industries 1989	0.25	0.44	0.19	0.31	-0.12	0.11
Uprising 1953: None	0.29	0.46	0.27	0.31	-0.04	0.57
Uprising 1953: Strike	0.25	0.44	0.24	0.27	-0.03	0.69
Uprising 1953: Demonstration	0.13	0.34	0.12	0.15	-0.03	0.62
Uprising 1953: Riot	0.25	0.43	0.31	0.18	0.13	0.07
Uprising 1953: Prisoner liberation	0.07	0.26	0.06	0.09	-0.03	0.51
State of emergency 1953	0.75	0.43	0.79	0.72	0.07	0.32
Military intervention 1953	0.57	0.50	0.54	0.60	-0.06	0.49
Electoral turnout 1928–32	84.10	3.64	83.69	84.52	-0.83	0.19
Vote share KPD 1928–32	15.45	6.83	15.71	15.20	0.52	0.66
Vote share NSDAP 1928–32	25.36	3.84	25.54	25.17	0.36	0.59
Share self-employed 1933	15.75	2.52	15.92	15.57	0.35	0.43
Share protestants 1925	91.77	3.85	91.23	92.30	-1.07	0.11
Share unemployed 1933	16.80	5.45	17.16	16.44	0.72	0.45
Individual characteristics (in 1990)						
Male (in percent)	46.56	49.89	45.89	47.52	-1.64	0.29
Age	46.58	18.72	46.75	46.32	0.44	0.80
Household size	2.72	1.16	2.67	2.80	-0.13	0.20
Share of singles	21.08	40.79	22.66	18.80	3.86	0.23
Share of married	59.29	49.13	56.92	62.71	-5.79	0.11
Other marital status	19.63	39.72	20.42	18.49	1.94	0.45
Share of low-skilled	45.45	49.80	42.92	49.11	-6.19	0.33
Share of medium-skilled	34.42	47.52	34.34	34.52	-0.18	0.94
Share of high-skilled	20.13	40.10	22.74	16.37	6.36	0.20
Blue-collar worker	51.50	49.98	49.41	54.51	-5.10	0.16
Self-employed	2.62	15.98	3.24	1.73	1.51	0.15
White-collar worker	23.51	42.41	25.08	21.23	3.85	0.35
Civil servant	0.25	5.01	0.09	0.49	-0.40	0.25
Other/unknown	22.12	41.51	22.18	22.04	0.14	0.96

Notes: The contiguous border pair sample covers 134 counties. Lower-spying and higher-spying counties are determined by means of the population-weighted GDR state average of the county-level spy density in the border pair sample. Lower-spying counties include 1,131 individuals, higher-spying counties 748 individuals. Descriptive statistics on individual characteristics are based on the 1990 wave of the SOEP data and calculated using cross-sectional weights, adjusted for duplications of counties that are part of multiple border pairs. The corresponding p-values are based on OLS regressions of individual characteristics on an indicator variable for lower-/higher spy density counties, clustering standard errors at the county and person level. For information on all variables, see Appendix Table 5.8.3.

Outcomes Y_{ct} are county c's election turnout, self-employment rate, number of patents per capita, unemployment rate and log population in year t (see Section 5.3.3).

We allow the effect of spying to evolve over time by interacting the time-invariant spy density $SPYDENS_c$ with year dummies τ_t. Coefficients $\beta_t, \forall t \geq 1989$ show the treatment effect after reunification and demonstrate the potential persistence of the effect. Moreover, coefficients $\beta_t, \forall t < 1989$ provide a direct test of the identifying assumption. If the surveillance levels in the 1980s had an effect on social capital or economic outcomes *prior* to fall of the Iron Curtain, this would be an indication that spies were not allocated randomly with respect to the outcome variable. Hence, we need to have flat, insignificant pre-trends to defend our identifying assumption.[25]

Year fixed effects τ_t account for trends in outcome variables over time. In our preferred specification, we even allow for heterogeneous and flexible trends by region (see below). County fixed effects ρ_c account for persistent confounding variables such as geographic location or regional liberalism. Note that identification in this panel model is somewhat more subtle than in the standard case since the Stasi density is constant across the panel and identification cannot be within-county as a consequence. Instead, the model is identified by exploiting cross-sectional variation in post-treatment adjustment paths. The interactions of the spy density with the year dummies thus capture the potential relationship between state surveillance in the 1980s and different adjustment paths after reunification relative to the initial base levels prior to the treatment.

Although we account for county fixed effects, we test the robustness of our results and include several sets of control variables, which are captured in $\boldsymbol{L_{ct}}$. In all specifications, and as done above, we control for the presence of an Object of Special Interest in county c by interacting a dummy variable with year dummies after the treatment ($t \geq 1989$). Moreover, we account for both county size and regional trends. Clearly, rural and urban jurisdiction are likely to show different

[25] We omit the spy density for the last pre-treatment year and normalize β_t to zero in the respective year. With the exception of the regression for population, our pre-treatment variables are measured prior to World War II. For unemployment, we only observe one pre-treatment year (1933). While this is sufficient to identify county fixed effects, we cannot test for pre-trends regarding regional unemployment in this model.

economic developments in the 1990s and 2000s independent of the Stasi density. The same is true for certain regions. Given that both population and GDR states explain up to 50 percent of the spy density variation (cf. Table 5.4.1), it is crucial to account for both regional and county-size trends. Concretely, we add GDR state times year fixed effects to the model[26] as well as size controls (log mean population in the 1980s and log county area) interacted with a dummy indicating the post-treatment period. In our richest and preferred specification, we also add the opposition and industry controls as used in Table 5.4.1 – each variable interacted with a post-treatment dummy. Lastly, we apply two other sensitivity checks: First, we add current population to the model. Despite being a potential outcome and hence being considered as a *bad control*, we test the robustness of our results when controlling for current population size, which captures different regional adjustment paths and also accounts for selection out of counties. Second, we control for federal and state transfers as well as investment subsidies paid to East German counties after reunification.

5.5 Results

In the following section, we present the empirical results. First, we focus on the effect of the spy density on individual-level measures of interpersonal and institutional trust (Section 5.5.1). In Section 5.5.2, we investigate how governmental surveillance affects economic performance. Last, we test the theoretical mechanism between government surveillance, social capital, and economic performance using the spy density as an instrument for trust (see Section 5.5.3).

5.5.1 The effects of surveillance on social capital

We apply the border discontinuity design (see equation (5.4.3)) to identify the effect of spying on measures of interpersonal and institutional trust. For each outcome, we estimate three specifications of the model: (i) only controlling for border pair fixed effects, (ii) adding individual characteristics to pick up compositional

[26] For the pre-war periods, we use German states and Prussian provinces from the time of the Weimar Republic.

differences in the population, and (iii) additionally including county-level controls to capture differences in county size, oppositional strength, industry composition, and persistent political ideology and economic performance (as captured by the pre World War II controls).

Panel A of Table 5.5.1 presents the results for our measures of interpersonal trust. We find that spying significantly affects both of our outcomes, trust in strangers and negative reciprocity. Results are significant in our leanest specification and also conditional on individual- and county-level controls, the latter specification being our preferred one.[27] For a one standard deviation increase in the spy density (see Table 5.4.2), the estimate in column (3) implies that the probability to trust would be around four percentage points lower, which is a large effect given that the average probability is fourteen percent. When focusing on reciprocity, we find that a one standard deviation increase in the spy density raises negative reciprocity by 0.7 points, the mean level of reciprocity being 9.2 points.

In Panel B of Table 5.5.1, we test for heterogeneous effects by age. We interact our main regressor with cohort dummies for individuals born (i) before 1940, (ii) between 1940 and 1961, and (iii) after 1961. Psychological and economic research has shown that trust is shaped during adolescence and does not change much after the age of 21 (Sutter and Kocher, 2007). With the Berlin Wall being built in 1961, this implies that the youngest cohort in our analysis was fully socialized in the GDR and should have been influenced most by Stasi spying. The second cohort, born between 1940 and 1961, was predominantly socialized in the immediate aftermath of World War II and during the first years of the GDR. In contrast to the youngest cohort, these respondents (or their families) also had the chance to move out of the GDR prior to the construction of the Berlin Wall in 1961. Lastly, people born before 1940 experienced World War II and reached adulthood prior to 1961. Interestingly, and in line with our expectations, we find that the negative effect of spying on trust is strongest – and statistically significant – for the youngest cohort. When focusing on negative reciprocity, we find the medium cohort to be most affected, although point estimates for all three cohorts are not statistically

[27] Given that trust in strangers is a binary outcome and negative reciprocity is measured on a 21 point scale, we estimate equation (5.4.3) using Ordinary Least Squares to ease interpretation. We find similar results when estimating a binary probit model for trust and an ordered probit model for reciprocity (cf. Table 5.7.2 in the Appendix).

Table 5.5.1: The effects of spying on interpersonal trust

	Trust in strangers			Negative reciprocity		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – Baseline effects						
Spy density	-0.293**	-0.279*	-0.319*	5.120***	4.912***	5.283***
	(0.141)	(0.154)	(0.184)	(1.803)	(1.698)	(1.747)
Adjusted-R^2	0.061	0.090	0.106	0.063	0.130	0.142
Panel B – Heterogeneous effects by age						
Spy density × Born bef. 1940	-0.241	-0.191	-0.254	4.286*	3.421	4.424*
	(0.191)	(0.219)	(0.241)	(2.346)	(2.550)	(2.288)
Spy density × Born 1940–1961	-0.245	-0.237	-0.285	6.530***	5.812***	6.444***
	(0.172)	(0.176)	(0.205)	(2.054)	(2.028)	(2.167)
Spy density × Born after 1961	-0.604**	-0.631***	-0.604***	4.201	3.527	4.008
	(0.255)	(0.226)	(0.222)	(3.241)	(3.205)	(3.549)
Adjusted-R^2	0.068	0.094	0.108	0.082	0.134	0.146
Panel C – Heterogeneous effects by mobility						
Spy density × Stayed	-0.321**	-0.318**	-0.338*	5.325***	4.865***	5.311***
	(0.137)	(0.153)	(0.188)	(1.781)	(1.734)	(1.811)
Spy density × Moved	-0.197	-0.125	-0.189	3.839	4.866	4.968
	(0.236)	(0.229)	(0.237)	(4.101)	(3.668)	(3.655)
Adjusted-R^2	0.063	0.092	0.108	0.063	0.129	0.142
Individual controls		Yes	Yes		Yes	Yes
County size controls			Yes			Yes
Opposition controls			Yes			Yes
Pre World War II controls			Yes			Yes
Industry controls			Yes			Yes
Number of observations	3,389	3,389	3,389	3,014	3,014	3,014

Notes: This table shows the β coefficients of the border pair model laid out in equation (5.4.3) using SOEP data. All specifications include border pair fixed effects and a dummy variable indicating the presence of an Object of Special Interest. Standard errors are two-way clustered at the border pair and the individual level with usual confidence levels (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We restrict the sample to border pairs for which we observe individuals in both counties along the border. All specifications use cross-sectional weights adjusted for duplicates of counties that are part of multiple border pairs. For detailed information on the control variables, see Data Appendix 5.8.

different from each other. In Panel C, we split the sample by individuals' moving decision after reunification. We will discuss these effects in Section 5.5.2, when looking at the population effect of state surveillance.

Next, we turn to institutional trust with Table 5.5.2 providing the results. We find a significant negative effect of the spy density on the intention to vote in elections throughout all specifications. This effect is driven by the medium cohort. On average, a one standard deviation increase in the intensity of spying leads to a decrease in the *intention* to attend elections of seven percentage points. In contrast, when looking at engagement in local politics, the young and the old cohorts seem to be negatively affected, while the overall average effect is negative but statistically insignificant.

While the intention to vote is a soft measure of institutional trust capturing stated preferences, we can use administrative data on electoral turnout to check whether intentions actually translate into real political participation. Given that county-level data on voter turnout are available since the 1920s, we apply our panel data model (see equation (5.4.4)), which allows us to control for time-invariant political preferences and historical differences in social capital by adding county fixed effects.

Figure 5.5.1 plots the corresponding β coefficients obtained from our preferred specification, i.e., when adding the full set of control variables: county size, opposition and industry controls as well as state times year fixed effects. Table 5.7.3 in the Appendix presents the corresponding regression results and shows that we find similar effects for leaner specifications as soon as we control for different trends in county size after reunification. Our results clearly indicate that the electoral turnout starts to decline in the 1990s for counties with a higher spy density. In the 2000s, voter turnout is about 4.8 percentage points lower relative to low-spying counties. For a one standard deviation increase in the spy density, average electoral turnout would be about 0.7 percentage points lower.

The figure also contains information on the potential endogeneity of the intensity of surveillance. If estimates of the intensity of spying were significant prior to World War II, the allocation of spies would have responded to pre-treatment trends in electoral turnout and would thus have been endogenous in this respect. While we indeed find a lower turnout in the 1930 election, significant at the ten

Table 5.5.2: The effects of spying on institutional trust

	Attend elections			Engagement in local politics		
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A – Baseline effects						
Spy density	-0.434*	-0.335*	-0.537**	-0.040	-0.027	-0.195
	(0.222)	(0.186)	(0.252)	(0.131)	(0.116)	(0.123)
Adjusted-R^2	0.053	0.137	0.146	0.020	0.125	0.134
Panel B – Heterogeneous effects by age						
Spy density × Born bef. 1940	-0.268	-0.084	-0.292	-0.385**	-0.246	-0.424***
	(0.243)	(0.209)	(0.264)	(0.160)	(0.155)	(0.158)
Spy density × Born 1940–1961	-0.622***	-0.597**	-0.800***	0.135	0.140	-0.016
	(0.239)	(0.240)	(0.287)	(0.151)	(0.131)	(0.137)
Spy density × Born after 1961	-0.163	-0.023	-0.271	-0.022	-0.092	-0.259*
	(0.313)	(0.298)	(0.336)	(0.163)	(0.142)	(0.147)
Adjusted-R^2	0.079	0.145	0.153	0.035	0.132	0.141
Panel C – Heterogeneous effects by mobility						
Spy density × Stayed	-0.404*	-0.314	-0.529**	-0.057	-0.051	-0.227*
	(0.229)	(0.193)	(0.247)	(0.134)	(0.117)	(0.125)
Spy density × Moved	-0.572*	-0.339	-0.508	0.043	0.103	-0.002
	(0.334)	(0.311)	(0.375)	(0.169)	(0.167)	(0.161)
Adjusted-R^2	0.053	0.138	0.149	0.020	0.126	0.135
Individual controls		Yes	Yes		Yes	Yes
County size controls			Yes			Yes
Opposition controls			Yes			Yes
Pre World War II controls			Yes			Yes
Industry controls			Yes			Yes
Number of observations	3,116	3,116	3,116	3,563	3,563	3,563

Notes: This table shows the β coefficients of the border pair model laid out in equation (5.4.3) using SOEP data. All specifications include border pair fixed effects and a dummy variable indicating the presence of an Object of Special Interest. Standard errors are two-way clustered at the border pair and the individual level with usual confidence levels (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We restrict the sample to border pairs for which we observe individuals in both counties along the border. All specifications use cross-sectional weights adjusted for duplicates of counties that are part of multiple border pairs. For detailed information on the control variables, see Data Appendix 5.8.

Figure 5.5.1: The effect of spying on electoral turnout

Notes: The graph plots the point estimates and corresponding 95 % confidence intervals of the spy density interacted with year dummies; see regression model (5.4.4). The specification includes county fixed effects and state times year fixed effects as well as controls for Objects of Special Interest, county size, opposition and industry composition. See specification (6) in Table 5.7.3 for details.

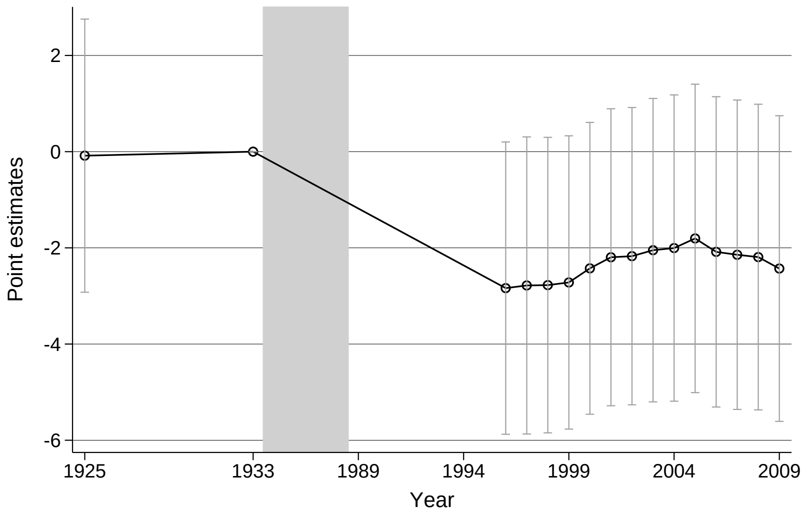
percent level, the remaining pre-treatment effects both before and after 1930 are insignificant and small. This suggests that the spy allocation was not systematically determined by pre World War II trends in institutional trust, which is crucial for establishing causality in our panel model.

5.5.2 The effects of surveillance on economic performance

Theoretically, we expect government surveillance to deteriorate social capital, which in turn leads to lower economic performance. While we have demonstrated the first part of this mechanism in the previous section, we now turn to the economic effects of state surveillance. First, we look at the direct effect of spying on economic outcomes, hence we estimate reduced form effects.

We begin by analyzing the effect of spying on entrepreneurial activity, given that lacking trust results in extensive monitoring of "possible malfeasance by partners, employees, and suppliers [and] less time to devote to innovation in new products or processes" (Knack and Keefer, 1997). Indeed, many studies have shown that more trustful people are more likely to become entrepreneurs (Welter, 2012; Caliendo et al., 2014). Hence, we consider two outcomes related to entrepreneurial activity, county-level self-employment rates and the number of patents per 100,000 inhabitants.

Figure 5.5.2: The effect of spying on self-employment rates

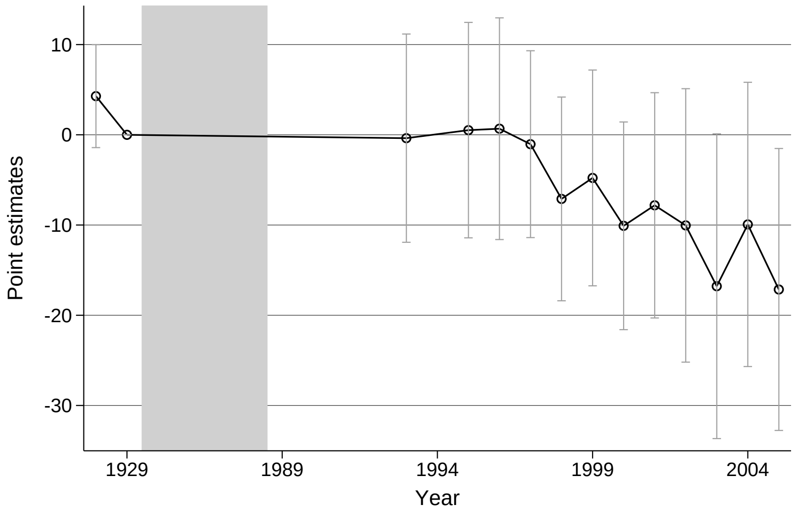


Notes: The graph plots the point estimates and corresponding 95 % confidence intervals of the spy density interacted with year dummies; see regression model (5.4.4). The specification includes county fixed effects and state times year fixed effects as well as controls for Objects of Special Interest, county size, opposition and industry composition. See specification (6) in Table 5.7.4 for details.

Figures 5.5.2 and 5.5.3 plot the respective regression estimates; full regression results are shown in Appendix Tables 5.7.4 and 5.7.5. We find that the self-employment rate is significantly lower (at the ten percent level) the higher the county's spy density. This negative effect is quite persistent and varies around −2.5 percentage points.[28] This estimate implies that for a one standard deviation increase in the spy density, the self-employment rate would be around 0.4 percentage points lower. Reassuringly, we detect no significant pre-trend, which implies that our estimates are not driven by different pre-treatment trends in entrepreneurial spirit.

[28] However, as shown in Appendix Table 5.7.4, we lose precision when including county size controls.

Figure 5.5.3: The effect of spying on patents per 100,000 inhabitants



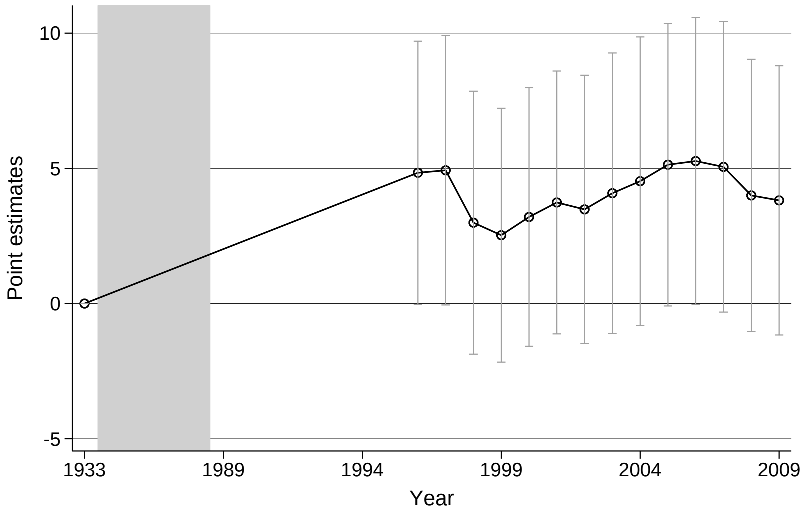
Notes: The graph plots the point estimates and corresponding 95 % confidence intervals of the spy density interacted with year dummies; see regression model (5.4.4). The specification includes county fixed effects and state times year fixed effects as well as controls for Objects of Special Interest, county size, opposition and industry composition. See specification (6) in Table 5.7.5 for details.

When looking at patents in Figure 5.5.3, we see no effect of spying on innovativeness in the first years after reunification. However, starting in 1997, the number of patents per capita in counties with a high spy density starts to drop. In 2005, the last year of our data, the point estimate is around -17, which implies that a one standard deviation decrease in the intensity of spying would, on average, lead to 2.4 patents more per 100,000 inhabitants, which is an increase of about twenty percent.

With entrepreneurial spirit lagging behind in counties with a high spy density, we can expect more comprehensive measures of economic performance to be lower as well. Ideally, we would look at the effect of spy density on GDP. Unfortunately, there is no pre World War II county-level measure available that is comparable to today's GDP. Hence, we take two other proxies for economic performance for which pre-treatment information is available. First, we look at the counties' unemployment rates and then at population size, which has been used as a proxy for regional growth (Redding and Sturm, 2008).

Figures 5.5.4 and 5.5.6 as well as Appendix Tables 5.7.6 and 5.7.7 show the

Figure 5.5.4: The effect of spying on unemployment rates

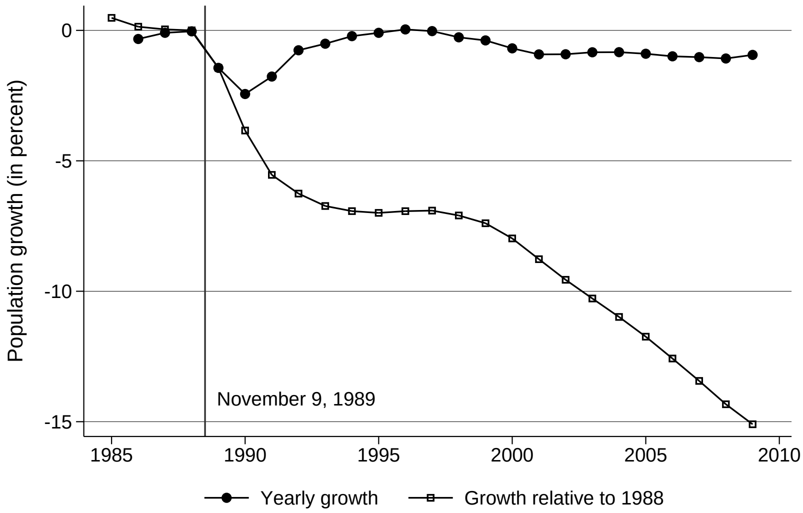


Notes: The graph plots the point estimates and corresponding 95 % confidence intervals of the spy density interacted with year dummies; see regression model (5.4.4). The specification includes county fixed effects and state times year fixed effects as well as controls for Objects of Special Interest, county size, opposition and industry composition. See specification (6) in Table 5.7.6 for details.

results. Figure 5.5.4 shows that unemployment is indeed higher in counties with a high spy density. The effect is persistent and oscillates around 4.7 percentage points. A one standard deviation increase in the spy density leads to an increase in the unemployment rate of 0.7 points. Unfortunately, there is only one reliable pre-treatment observation for the unemployment rate. While we can still identify the effect of spying in our panel research design, we cannot check for pre-trends in unemployment.

Next, we investigate the effect of state surveillance on county population. Average yearly and cumulated county-level population growth since the mid-1980s are depicted in Figure 5.5.5. The graph shows two emigration waves after the fall of the Iron Curtain – a severe and temporary one immediately after reunification (between 1989 and 1992) and a moderate and persistent one starting in 1998. Fuchs-Schündeln and Schündeln (2009) investigate the age, skill, and gender composition of these two migration waves in detail. They find that in the first wave it was rather the low-skilled who moved, while the second wave of migrants was driven by more educated and younger individuals.

Figure 5.5.5: Average county-level population growth in East Germany



Notes: The graph shows yearly and cumulative average population growth for East German counties from 1985 to 2009. Cumulative growth is measured relative to the year 1988.

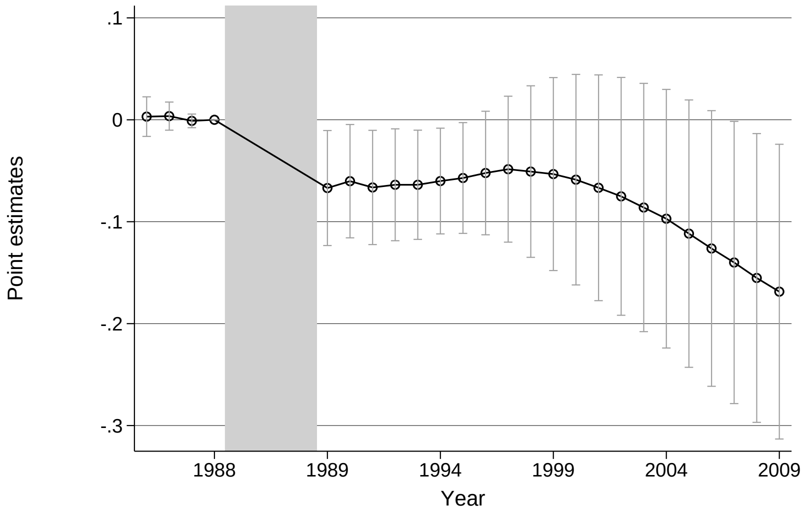
In Figure 5.5.6 and the corresponding Table 5.7.7, we test whether these two emigration waves can be related to the intensity of Stasi spying in GDR counties. Using yearly county-level population data from 1985 to 1988 as our pre-treatment observations, we indeed find a negative population effect of spying that can be related to the two migration waves.[29] First, population in higher-spying counties sharply drops in the first post-treatment year 1989.[30] This implies that the initial emigration wave was significantly driven by people leaving high-spying counties. For a one standard deviation decrease in the spy density, the population would be 0.9 percent higher. Given that the average population loss in 1989 was 1.5 percent, this is a substantial effect.

Further note that the effect of spying is flat after 1989. From 1990 to 1997, we do not see a significantly different population effect between high- and low-

[29] Note that effects are always relative to lower spying counties. Hence, a negative population effect does not need to result in a lower number of inhabitants if population levels increased in lower-spying counties. Given that populations dropped in almost all counties, the most relevant interpretation of a negative effect seems to be a faster decline in population.

[30] Population is measured on December 31, 1989, hence hardly two months after the fall of the Berlin Wall. However, many people already tried to escape from the GDR in the summer of 1989 either via Hungary and Austria or by fleeing to the West German embassies in Warsaw, Prague, and Budapest.

Figure 5.5.6: The effect of spying on log population



Notes: The graph plots the point estimates and corresponding 95 % confidence intervals of the spy density interacted with year dummies; see regression model (5.4.4). The specification includes county fixed effects and state times year fixed effects as well as controls for Objects of Special Interest, county size, opposition and industry composition. See specification (6) in Table 5.7.7 for details.

spying counties in addition to the initial population outflow. In other words, the population response driven by spying was immediate. In 1998, i.e., the first year of the second emigration wave, the effect of spying on population size starts to decline again and continues to do so until 2009. Given that the overall population loss in 2009 for East German counties was fifteen percent (relative to 1988, see Figure 5.5.5), we use back-of-the-envelope calculations to assess how much of this decline can be attributed to spying. Given that the mean spy density is 0.38, the point estimate for the year 2009 of -0.169 implies that the population would, on average, be 6.6 percent higher in the absence of any spying. Hence, about forty percent of the overall decline can be explained by people moving away from former high-spying counties.

The strong population effect of spying gives rise to the question of how much of our effects on social capital and other economic outcomes are driven by selection out of high-spying counties. For the panel estimates, we show that results for all outcomes are robust to the inclusion of the current population as a control variable, acknowledging that this only accounts for the population drop but not for potential differences in the composition of emigrants. Moreover, we can re-assess the timing of the effects, bearing in mind that the first wave of migrants was rather negatively selected in terms of education, while the second wave was positively selected (Fuchs-Schündeln and Schündeln, 2009). For unemployment and self-employed, we find the strongest negative effects in 1996 and 1997, hence prior to the second migration wave. Given that the stayers were positively selected in the first wave, it is possible that the true effect of spying is even more negative than estimated. In terms of patents, it is interesting to see that the decline actually starts with the beginning of the second migration wave. Hence, it is possible that the effect of spying on patents is of second order and triggered by the emigration of young and highly educated people.

In terms of social capital, we can go a bit further in assessing the potential selection effect. First, note that Table 5.4.2 indicates that the initial level in terms of education and learned occupation was not statistically different between higher and lower spy density counties. Second, we interact the spy density with a dummy variable indicating whether that individual moved out of the 1989 county of residence; see Panels C of Tables 5.5.1 and 5.5.2. Our results show no significantly different effects between movers and stayers.[31] This suggests that the compositions of movers is not different from stayers in terms of social capital and our findings are not driven by selection of movers.

5.5.3 Linking spying, trust and economic performance

In the previous two sections, we provided evidence of negative effects of spying on social capital and economic performance. In a last step, we aim at documenting the theoretical mechanism between government surveillance, social capital, and economic performance. We use gross labor income reported in the SOEP as our measure of economic performance.

First, we estimate the reduced form effect of spying on income (see Table 5.5.3, columns (1) to (3)). As expected, we find a negative significant effect of the spy

[31] Given that the group of movers is much smaller, we do not obtain statistically significant effects for them.

Table 5.5.3: The effect of spying on monthly gross labor income

	Reduced form			2SLS	
	(1)	(2)	(3)	(4)	(5)
Dependent variable	Income	Income	Income	Trust	Income
Spy density	-1.043 *	-0.776 *	-0.915 **	-0.744 ***	
	(0.560)	(0.423)	(0.416)	(0.245)	
Trust in strangers					1.354 *
					(0.725)
Individual controls		Yes	Yes	Yes	Yes
County size controls			Yes	Yes	Yes
Opposition controls			Yes	Yes	Yes
Pre World War II controls			Yes	Yes	Yes
Industry controls			Yes	Yes	Yes
Number of observations	1,773	1,773	1,773	1,743	1,743
Adjusted-R^2	0.084	0.313	0.341	0.134	
F-Test				9.237	

Notes: This table shows the β coefficients of the border pair model laid out in equation (5.4.3) using SOEP data. All specifications include border pair fixed effects and a dummy variable indicating the presence of an Object of Special Interest. Standard errors are two-way clustered at the border pair and the individual level with usual confidence levels (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We restrict the sample to border pairs for which we observe individuals in both counties along the border. All specifications use cross-sectional weights adjusted for duplicates of counties that are part of multiple border pairs. For detailed information on the control variables, see Data Appendix 5.8.

density on log gross income of -0.915. The effect implies that a one standard deviation increase in the spy density leads to a gross income loss of twelve percent. To test the suggested channel with surveillance affecting trust, and trust affecting income, we run a two-stage least squares regression of income on trust using the spy density as an instrument. Column (4) of Table 5.5.3 shows the first-stage result of the regression.[32] The F-test of the first-stage regression is 9.2, which suggests that the instrument has reasonable power. The second-stage results are presented in column (5). Using the change in trust induced by a one standard deviation *decrease* in surveillance, the first stage implies a ten percent increase in the probability to trust strangers, which in turn increases gross income by 15.1 percent.

[32] As found in Table 5.5.1, spying has a significantly negative effect on trust. Note that the point estimate is twice as high and more significant despite the smaller sample. This can be explained by the fact the 2SLS sample is restricted to individuals with positive labor income who are, on average, younger compared to the full estimation sample. In fact, the magnitude of the first-stage estimate is comparable to the one found for the youngest cohort.

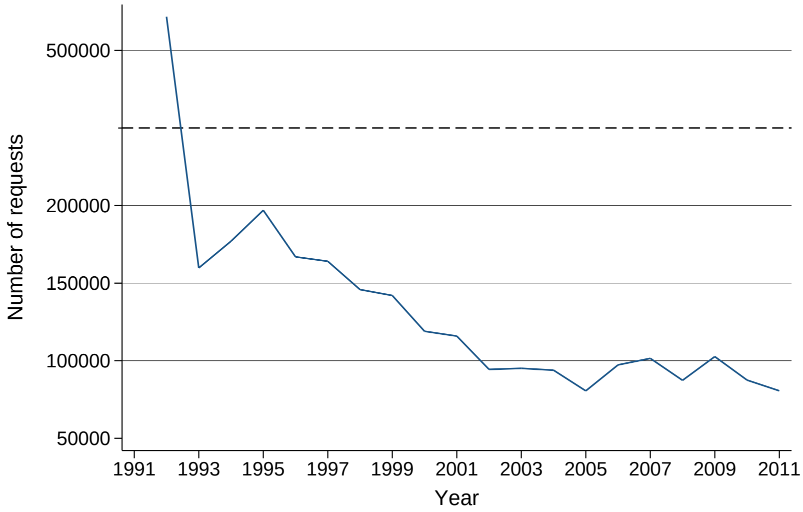
5.6 Conclusion

In this paper, we estimate the effect of state mass surveillance on social capital and economic performance by exploiting county-level variation in the number of spies per capita in the former socialist German Democratic Republic. To account for the potentially non-random regional allocation of spies, we implement two different research designs. First, we exploit discontinuities at state borders arising from the administrative-territorial structure of the Ministry for State Security. Second, we set up a long-term panel with pre World War II measures of social capital and economic performance. This allows us to control for county fixed effects and identify the effect of cross-sectional spy density variation through different adjustments paths after the breakdown of the socialist regime. Moreover, we are able to inspect potential pre-treatment trends in outcome variables, which would invalidate our identifying assumption.

The results of our analysis show that more intense state surveillance had negative and long-lasting effects on both social capital and economic performance. Our estimates imply that an abolishment of all spying activities would have led to an increase in electoral turnout of 1.8 percentage points. Moreover, it would have increased regional innovativeness and entrepreneurship through more patents per capita and higher self-employment rates. Eventually, the average unemployment rate would have been about 1.8 percentage points lower, which is equivalent to a ten percent drop compared to the average in East Germany. We also find that Stasi spying can explain a large part of the decline in population levels in East Germany. Hence, we show that the former East German regime did not only have a long-lasting impact on political preferences (Alesina and Fuchs-Schündeln, 2007), but it also eroded institutional and interpersonal trust, which in turn has long-lasting negative effects on the society and the economy.

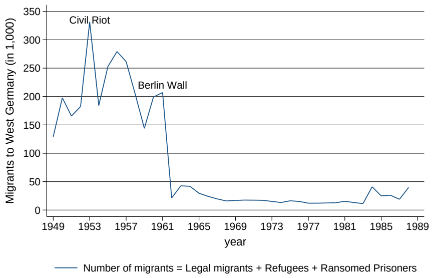
5.7 Appendix

Figure 5.7.1: Annual number of requests for inspection of Stasi files

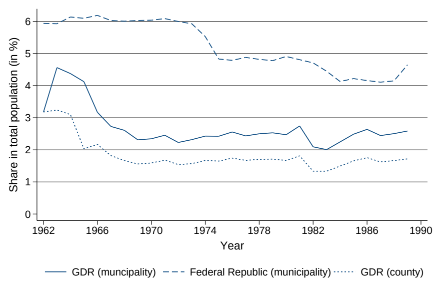


Notes: The graph plots the annual number of requests for disclosure of information on Stasi activity. Data are provided by the Agency of the Federal Commissioner for the Stasi Records (BStU).

Figure 5.7.2: Migration in socialist East Germany



(a) External Migration



(b) Internal Migration

Notes: Panel (a) is based on own calculations using data from Rühle and Holzweißig (1988), Ritter and Lapp (1997) and monthly announcements of the West German Federal Ministry for Displaced Persons, Refugees and War Victims. Panel (b) is based on own calculations using data from the Statistical Yearbooks of the German Democratic Republic and the Federal Statistical Office of Germany.

Table 5.7.1: The allocation of Stasi spies: full regression results

	(1)	(2)	(3)	(4)	(5)	(6)
Object of Special Interest	0.160	0.172	0.261***	0.267***	0.265***	0.272***
	(0.124)	(0.114)	(0.067)	(0.069)	(0.070)	(0.076)
Log mean population 1980s			-0.134***	-0.144***	-0.148***	-0.135***
			(0.015)	(0.016)	(0.016)	(0.021)
Log county size			-0.006	-0.002	0.001	0.003
			(0.011)	(0.012)	(0.012)	(0.014)
Uprising 1953: Strike				0.004	-0.001	-0.005
				(0.024)	(0.026)	(0.027)
Uprising 1953: Demonstration				0.002	-0.006	-0.014
				(0.026)	(0.027)	(0.031)
Uprising 1953: Riot				-0.030	-0.035	-0.041
				(0.037)	(0.038)	(0.038)
Uprising 1953: Prisoner liberation				0.002	-0.005	-0.015
				(0.034)	(0.035)	(0.038)
Military intervention 1953				0.037	0.037	0.045*
				(0.023)	(0.023)	(0.024)
State of emergency 1953				-0.014	-0.009	-0.009
				(0.026)	(0.027)	(0.029)
Share indust. employment 1989					0.001	0.001
					(0.001)	(0.001)
Important industries 1989					-0.003	-0.011
					(0.022)	(0.022)
Mean electoral turnout 1928–1932						0.001
						(0.006)
Mean vote share NSDAP 1928–1932						0.006**
						(0.003)
Mean vote share KPD 1928–1932						-0.000
						(0.003)
Share protestants 1925						-0.001
						(0.001)
Share unemployed 1933						0.001
						(0.004)
Share self-employed 1933						0.004
						(0.009)
GDR state FE	No	Yes	Yes	Yes	Yes	Yes
Observations	187	187	187	187	187	187
R^2	0.033	0.298	0.529	0.540	0.545	0.561
Adjusted R^2	0.028	0.237	0.481	0.475	0.474	0.473

Notes: This table shows the simple OLS coefficients of regressing the mean county-level spy density in the 1980s on different sets of control variables. For details on the source and construction of the variables, see Appendix Table 5.8.3.

Table 5.7.2: The effects of spying on trust: robustness checks

	Border pairs (OLS)					Border pairs (Probit)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Weights	Adj.	Adj.	Adj.	Cross	None	Adj.	Adj.
Panel A – Trust in strangers							
Spy density	-0.293**	-0.279*	-0.319*	-0.422**	-0.088	-1.611**	-1.689**
	(0.141)	(0.154)	(0.184)	(0.191)	(0.129)	(0.734)	(0.695)
Observations	3,389	3,389	3,389	3,389	3,389	3,389	3,389
Adjusted-R^2	0.061	0.090	0.106	0.078	0.076		
Pseudo-R^2						0.088	0.155
Panel B – Negative reciprocity							
Spy density	5.120***	4.912***	5.283***	5.877***	5.439***	1.161***	1.203***
	(1.803)	(1.698)	(1.747)	(1.878)	(1.849)	(0.376)	(0.350)
Observations	3,014	3,014	3,014	3,014	3,014	3,014	3,014
Adjusted-R^2	0.063	0.130	0.142	0.146	0.125		
Pseudo-R^2						0.015	0.034
Panel C – Attend elections							
Spy density	-0.434*	-0.335*	-0.537**	-0.578**	-0.297	-1.226**	-1.753**
	(0.222)	(0.186)	(0.252)	(0.256)	(0.209)	(0.560)	(0.697)
Observations	3,116	3,116	3,116	3,116	3,116	3,116	3,116
Adjusted-R^2	0.053	0.137	0.146	0.135	0.102		
Pseudo-R^2						0.058	0.152

continued

Individual controls		Yes	Yes	Yes	Yes		Yes
County size controls			Yes	Yes	Yes		Yes
Opposition controls			Yes	Yes	Yes		Yes
Pre W II controls			Yes	Yes	Yes		Yes
Industry controls			Yes	Yes	Yes		Yes

Table 5.7.2 continued

	Border pairs (OLS)					Border pairs (Probit)	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Weights	Adj.	Adj.	Adj.	Cross	None	Adj.	Adj.
Panel D – Engagement in local politics							
Spy density	-0.040	-0.027	-0.195	-0.180	-0.083	-0.203	-2.109***
	(0.131)	(0.116)	(0.123)	(0.127)	(0.141)	(0.647)	(0.603)
Observations	3,563	3,563	3,563	3,563	3,563	3,563	3,563
Adjusted-R^2	0.020	0.125	0.134	0.115	0.100		
Pseudo-R^2						0.062	0.250
Panel E – Monthly gross labor income							
Spy density	-1.043*	-0.776*	-0.915**	-0.671	-0.666*		
	(0.560)	(0.423)	(0.416)	(0.470)	(0.388)		
Observations	1,773	1,773	1,773	1,773	1,773		
Adjusted-R^2	0.084	0.313	0.341	0.325	0.266		
Pseudo-R^2							
Individual controls		Yes	Yes	Yes	Yes		Yes
County size controls			Yes	Yes	Yes		Yes
Opposition controls			Yes	Yes	Yes		Yes
Pre WW II controls			Yes	Yes	Yes		Yes
Industry controls			Yes	Yes	Yes		Yes

Notes: This table shows the β coefficients of the border pair model laid out in equation (5.4.3) using SOEP data. All specifications include border pair fixed effects and a dummy variable indicating the presence of an Object of Special Interest. Standard errors are two-way clustered at the border pair and the individual level in specifications (1)-(5) and one-way clustered at the border pair level only in specifications (6)-(7). We use the usual confidence levels (* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$). We restrict the sample to border pairs for which we observe individuals in both counties along the border. Columns (1)-(3) and (6)-(7) present estimates using cross-sectional weights, adjusted for duplications of counties that are part of multiple border pairs. Estimates in column (4) use unadjusted cross-sectional weights, column (5) shows unweighted regression results but adjusts for duplicates. Specifications (6)-(7) present ordered probit results if negative reciprocity is the outcome variable. For detailed information on the control variables, see Data Appendix 5.8.

Table 5.7.3: The effect of spying on electoral turnout

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spy density × 05/1924	0.327	0.327	-1.305	-1.305	-1.305	-1.343	-1.305
	(1.515)	(1.516)	(1.460)	(1.462)	(1.463)	(1.453)	(1.462)
Spy density × 12/1924	0.341	0.341	-0.972	-0.972	-0.972	-1.022	-0.972
	(1.420)	(1.421)	(1.452)	(1.454)	(1.455)	(1.442)	(1.454)
Spy density × 1928	2.316	2.316	0.354	0.354	0.354	0.357	0.354
	(1.661)	(1.662)	(1.581)	(1.583)	(1.584)	(1.584)	(1.583)
Spy density × 1930	-2.379*	-2.379*	-2.192*	-2.192*	-2.192*	-2.183*	-2.192*
	(1.219)	(1.219)	(1.164)	(1.166)	(1.166)	(1.166)	(1.165)
Spy density × 07/1932	-0.479	-0.479	-0.239	-0.239	-0.239	-0.231	-0.239
	(0.811)	(0.811)	(0.580)	(0.580)	(0.581)	(0.581)	(0.580)
Spy density × 1990	-1.888	-5.534**	-0.554	-0.745	-0.480	-0.510	
	(2.340)	(2.647)	(2.101)	(2.166)	(2.162)	(2.166)	
Spy density × 1994	-2.894	-6.540**	-2.710	-2.901	-2.635	-2.667	
	(2.340)	(2.740)	(2.212)	(2.278)	(2.256)	(2.223)	
Spy density × 1998	-6.505***	-10.151***	-1.585	-1.776	-1.511	-1.558	-0.866
	(2.202)	(2.640)	(1.975)	(2.037)	(2.036)	(2.072)	(2.071)
Spy density × 2002	0.963	-2.683	-3.988*	-4.179**	-3.914*	-3.919*	-3.082
	(2.170)	(2.641)	(2.041)	(2.092)	(2.086)	(2.047)	(2.087)
Spy density × 2005	0.500	-3.147	-3.726*	-3.917*	-3.652*	-3.592*	-2.643
	(2.053)	(2.513)	(2.067)	(2.124)	(2.134)	(2.038)	(2.166)
Spy density × 2009	2.924	-0.723	-4.878**	-5.069**	-4.804**	-4.644**	-2.971
	(2.335)	(2.779)	(2.190)	(2.268)	(2.269)	(2.201)	(2.273)
Post × Object × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post × County size controls		Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE			Yes	Yes	Yes	Yes	Yes
Post × Opposition controls				Yes	Yes	Yes	Yes
Post × Industry controls					Yes	Yes	Yes
Log current population						Yes	
Post × Transfers							Yes
Observations	2232	2232	2232	2232	2232	2230	1858
Adjusted R^2	0.826	0.829	0.919	0.920	0.921	0.923	0.930

Notes: This table shows the β coefficients of the panel data model laid out in equation (5.4.4). All specifications include county and year fixed effects. Standard errors are clustered at the county level with the usual confidence levels (* $p < .1$, ** $p < .05$, *** $p < .01$). The Stasi share-year interaction for November 1932 is omitted. The district of East Berlin is excluded from the data because East and West Berlin cannot be separated after reunification. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq 1989$). Object stands for Object of Special Interest. County size controls include log county area and log mean 1980s population. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. For detailed information on the control variables, see Data Appendix 5.8.

Table 5.7.4: The effect of spying on self-employment rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spy density × 1925	-1.353	-1.353	-0.083	-0.083	-0.083	0.023	-0.083
	(1.330)	(1.330)	(1.437)	(1.439)	(1.439)	(1.441)	(1.440)
Spy density × 1996	-4.155***	-2.938*	-2.188	-2.468	-2.837*	-2.769*	-2.773*
	(1.117)	(1.512)	(1.455)	(1.517)	(1.540)	(1.421)	(1.532)
Spy density × 1997	-4.197***	-2.979*	-2.132	-2.412	-2.782*	-2.708*	-2.733*
	(1.164)	(1.558)	(1.485)	(1.544)	(1.565)	(1.454)	(1.557)
Spy density × 1998	-4.061***	-2.843*	-2.125	-2.405	-2.774*	-2.704*	-2.755*
	(1.170)	(1.571)	(1.479)	(1.536)	(1.557)	(1.453)	(1.556)
Spy density × 1999	-3.949***	-2.731*	-2.070	-2.349	-2.719*	-2.652*	-2.705*
	(1.203)	(1.607)	(1.474)	(1.529)	(1.545)	(1.445)	(1.541)
Spy density × 2000	-3.914***	-2.697	-1.777	-2.056	-2.426	-2.366	-2.433
	(1.229)	(1.633)	(1.460)	(1.517)	(1.538)	(1.437)	(1.532)
Spy density × 2001	-3.431***	-2.213	-1.546	-1.826	-2.196	-2.145	-2.218
	(1.234)	(1.633)	(1.489)	(1.545)	(1.565)	(1.469)	(1.558)
Spy density × 2002	-3.332***	-2.115	-1.523	-1.803	-2.173	-2.133	-2.160
	(1.241)	(1.629)	(1.493)	(1.547)	(1.567)	(1.477)	(1.555)
Spy density × 2003	-3.214**	-1.996	-1.399	-1.678	-2.048	-2.022	-2.041
	(1.286)	(1.669)	(1.528)	(1.582)	(1.599)	(1.513)	(1.594)
Spy density × 2004	-3.213**	-1.995	-1.355	-1.635	-2.004	-1.992	-1.946
	(1.317)	(1.691)	(1.541)	(1.597)	(1.614)	(1.526)	(1.600)
Spy density × 2005	-2.779**	-1.562	-1.154	-1.434	-1.804	-1.810	-1.728
	(1.348)	(1.716)	(1.558)	(1.611)	(1.625)	(1.540)	(1.614)
Spy density × 2006	-3.086**	-1.868	-1.434	-1.714	-2.083	-2.107	-2.021
	(1.354)	(1.715)	(1.571)	(1.620)	(1.635)	(1.558)	(1.627)
Spy density × 2007	-2.935**	-1.717	-1.494	-1.774	-2.143	-2.184	-2.132
	(1.311)	(1.676)	(1.562)	(1.612)	(1.630)	(1.560)	(1.619)
Spy density × 2008	-2.560**	-1.342	-1.542	-1.822	-2.192	-2.251	-2.186
	(1.297)	(1.668)	(1.542)	(1.593)	(1.611)	(1.539)	(1.601)
Spy density × 2009	-2.484*	-1.266	-1.781	-2.061	-2.430	-2.507	-2.413
	(1.299)	(1.676)	(1.539)	(1.593)	(1.611)	(1.541)	(1.612)
Post × Object × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post × County size controls		Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE			Yes	Yes	Yes	Yes	Yes
Post × Opposition controls				Yes	Yes	Yes	Yes
Post × Industry controls					Yes	Yes	Yes
Log current population						Yes	
Post × Transfers							Yes
Observations	2976	2976	2976	2976	2976	2976	2974
Adjusted R^2	0.877	0.878	0.915	0.916	0.917	0.920	0.918

Notes: This table shows the β coefficients of the panel data model laid out in equation (5.4.4). All specifications include county and year fixed effects. Standard errors are clustered at the county level with the usual confidence levels (* $p < .1$, ** $p < .05$, *** $p < .01$). The Stasi share-year interaction for 1933 is omitted. The district of East Berlin is excluded from the data because East and West Berlin cannot be separated after reunification. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq 1989$). Object stands for Object of Special Interest. County size controls include log county area and log mean 1980s population. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. For detailed information on the control variables, see Data Appendix 5.8.

Table 5.7.5: The effect of spying on patents per 100,000 inhabitants

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spy density × 1928	3.685*	3.685*	4.285	4.285	4.285	4.289	4.285
	(1.952)	(1.953)	(2.887)	(2.891)	(2.892)	(2.895)	(2.893)
Spy density × 1993	-4.891	-0.411	-2.008	-0.966	-0.373	-0.396	
	(4.463)	(5.511)	(5.898)	(5.834)	(5.850)	(6.050)	
Spy density × 1995	-3.361	1.118	-1.118	-0.076	0.516	0.507	0.714
	(5.416)	(5.857)	(6.659)	(6.296)	(6.051)	(6.132)	(6.097)
Spy density × 1996	-5.109	-0.629	-0.958	0.084	0.677	0.678	0.519
	(5.668)	(6.146)	(6.825)	(6.446)	(6.228)	(6.340)	(6.302)
Spy density × 1997	-6.052	-1.572	-2.673	-1.631	-1.038	-1.029	-1.134
	(5.128)	(5.462)	(5.722)	(5.411)	(5.249)	(5.378)	(5.332)
Spy density × 1998	-13.433**	-8.954	-8.741	-7.699	-7.106	-7.102	-7.578
	(5.471)	(6.239)	(6.380)	(5.974)	(5.722)	(5.803)	(5.678)
Spy density × 1999	-9.350	-4.870	-6.416	-5.374	-4.781	-4.782	-5.261
	(5.848)	(6.576)	(6.708)	(6.284)	(6.061)	(6.183)	(6.036)
Spy density × 2000	-13.858**	-9.378	-11.724*	-10.682*	-10.089*	-10.102*	-10.335*
	(5.862)	(6.603)	(6.572)	(6.052)	(5.833)	(5.925)	(5.832)
Spy density × 2001	-10.716	-6.236	-9.451	-8.409	-7.816	-7.846	-7.741
	(6.538)	(7.167)	(6.868)	(6.515)	(6.327)	(6.456)	(6.282)
Spy density × 2002	-13.265*	-8.785	-11.676	-10.633	-10.041	-10.088	-9.837
	(7.870)	(8.427)	(8.366)	(7.911)	(7.681)	(7.751)	(7.561)
Spy density × 2003	-18.940**	-14.460*	-18.417*	-17.375*	-16.782*	-16.852*	-16.317*
	(8.013)	(8.282)	(9.498)	(8.928)	(8.560)	(8.602)	(8.391)
Spy density × 2004	-12.959	-8.479	-11.568	-10.526	-9.934	-10.027	-9.194
	(7.905)	(8.410)	(8.674)	(8.220)	(7.984)	(8.091)	(7.935)
Spy density × 2005	-15.291*	-10.811	-18.778**	-17.736**	-17.143**	-17.267**	-16.280**
	(8.377)	(8.877)	(8.548)	(8.114)	(7.921)	(7.987)	(7.855)
Post × Object × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post × County size controls		Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE			Yes	Yes	Yes	Yes	Yes
Post × Opposition controls				Yes	Yes	Yes	Yes
Post × Industry controls					Yes	Yes	Yes
Log current population						Yes	
Post × Transfers							Yes
Observations	2604	2604	2604	2604	2604	2604	2418
Adjusted R^2	0.427	0.430	0.504	0.509	0.511	0.513	0.532

Notes: This table shows the β coefficients of the panel data model laid out in equation (5.4.4). All specifications include county and year fixed effects. Standard errors are clustered at the county level with the usual confidence levels (* $p < .1$, ** $p < .05$, *** $p < .01$). The Stasi share-year interaction for 1929 is omitted. The district of East Berlin is excluded from the data because East and West Berlin cannot be separated after reunification. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq 1989$). Object of SI stands for Object of Special Interest. County size controls include log county area and log mean 1980s population. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. For detailed information on the control variables, see Data Appendix 5.8.

Table 5.7.6: The effect of spying on unemployment rates

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Spy density × 1996	21.075***	10.195***	4.287*	4.734*	4.838*	4.741**	4.916**
	(3.016)	(3.296)	(2.453)	(2.510)	(2.466)	(2.327)	(2.465)
Spy density × 1997	20.269***	9.388***	4.377*	4.824*	4.928*	4.822**	4.990**
	(3.058)	(3.303)	(2.515)	(2.564)	(2.524)	(2.387)	(2.517)
Spy density × 1998	19.101***	8.220**	2.440	2.887	2.991	2.891	3.042
	(2.940)	(3.242)	(2.448)	(2.494)	(2.465)	(2.315)	(2.461)
Spy density × 1999	19.261***	8.380***	1.977	2.424	2.528	2.433	2.574
	(2.825)	(3.189)	(2.345)	(2.409)	(2.379)	(2.223)	(2.377)
Spy density × 2000	20.078***	9.197***	2.650	3.097	3.202	3.119	3.221
	(2.827)	(3.215)	(2.379)	(2.451)	(2.423)	(2.279)	(2.418)
Spy density × 2001	21.204***	10.324***	3.188	3.635	3.739	3.675	3.734
	(2.825)	(3.193)	(2.416)	(2.496)	(2.464)	(2.335)	(2.466)
Spy density × 2002	20.811***	9.931***	2.932	3.379	3.483	3.438	3.504
	(2.841)	(3.185)	(2.459)	(2.544)	(2.515)	(2.367)	(2.519)
Spy density × 2003	22.497***	11.617***	3.529	3.976	4.081	4.061	4.086
	(3.081)	(3.397)	(2.586)	(2.660)	(2.629)	(2.506)	(2.629)
Spy density × 2004	23.330***	12.450***	3.975	4.422	4.526*	4.531*	4.567*
	(3.137)	(3.458)	(2.655)	(2.732)	(2.704)	(2.584)	(2.701)
Spy density × 2005	22.625***	11.744***	4.584*	5.031*	5.136*	5.175**	5.187*
	(2.999)	(3.357)	(2.585)	(2.674)	(2.648)	(2.528)	(2.638)
Spy density × 2006	23.246***	12.366***	4.719*	5.166*	5.270*	5.342**	5.298**
	(2.972)	(3.381)	(2.642)	(2.714)	(2.689)	(2.594)	(2.683)
Spy density × 2007	23.237***	12.357***	4.504*	4.951*	5.055*	5.159*	5.025*
	(2.927)	(3.379)	(2.670)	(2.743)	(2.722)	(2.626)	(2.727)
Spy density × 2008	21.914***	11.034***	3.448	3.895	3.999	4.138*	3.969
	(2.766)	(3.249)	(2.494)	(2.574)	(2.553)	(2.447)	(2.561)
Spy density × 2009	20.537***	9.657***	3.264	3.711	3.816	3.985*	3.916
	(2.653)	(3.118)	(2.460)	(2.547)	(2.523)	(2.404)	(2.531)
Post × Object × Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Post × County size controls		Yes	Yes	Yes	Yes	Yes	Yes
State × Year FE			Yes	Yes	Yes	Yes	Yes
Post × Opposition controls				Yes	Yes	Yes	Yes
Post × Industry controls					Yes	Yes	Yes
Log current population						Yes	
Post × Transfers							Yes
Observations	2790	2790	2790	2790	2790	2790	2788
Adjusted R^2	0.609	0.656	0.823	0.829	0.829	0.837	0.829

Notes: This table shows the β coefficients of the panel data model laid out in equation (5.4.4). All specifications include county and year fixed effects. Standard errors are clustered at the county level with the usual confidence levels (* $p < .1$, ** $p < .05$, *** $p < .01$). The Stasi share-year interaction for 1933 is omitted. The district of East Berlin is excluded from the data because East and West Berlin cannot be separated after reunification. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq 1989$). Object of SI stands for Object of Special Interest. County size controls include log county area and log mean 1980s population. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. For detailed information on the control variables, see Data Appendix 5.8.

Table 5.7.7: The effect of spying on log population

	(1)	(2)	(3)	(4)	(5)	(6)
Spy density × 1985	-0.009	-0.009	0.003	0.003	0.003	0.003
	(0.009)	(0.009)	(0.010)	(0.010)	(0.010)	(0.010)
Spy density × 1986	-0.003	-0.003	0.004	0.004	0.004	0.004
	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)	(0.007)
Spy density × 1987	-0.002	-0.002	-0.001	-0.001	-0.001	-0.001
	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)	(0.003)
Spy density × 1989	0.016 ***	-0.063 **	-0.069 **	-0.070 **	-0.067 **	
	(0.005)	(0.025)	(0.027)	(0.028)	(0.029)	
Spy density × 1990	0.029 ***	-0.050 **	-0.062 **	-0.064 **	-0.060 **	
	(0.007)	(0.024)	(0.027)	(0.028)	(0.028)	
Spy density × 1991	0.023 ***	-0.056 **	-0.068 **	-0.070 **	-0.066 **	
	(0.008)	(0.024)	(0.028)	(0.028)	(0.028)	
Spy density × 1992	0.028 ***	-0.051 **	-0.065 **	-0.067 **	-0.064 **	
	(0.009)	(0.024)	(0.027)	(0.027)	(0.028)	
Spy density × 1993	0.031 ***	-0.048 **	-0.065 **	-0.067 **	-0.064 **	
	(0.011)	(0.024)	(0.026)	(0.027)	(0.027)	
Spy density × 1994	0.041 ***	-0.037	-0.062 **	-0.063 **	-0.060 **	
	(0.015)	(0.025)	(0.025)	(0.026)	(0.026)	
Spy density × 1995	0.055 ***	-0.024	-0.059 **	-0.060 **	-0.057 **	-0.083 **
	(0.019)	(0.027)	(0.026)	(0.027)	(0.028)	(0.033)
Spy density × 1996	0.071 ***	-0.008	-0.054 *	-0.056 *	-0.052 *	-0.082 **
	(0.025)	(0.031)	(0.029)	(0.030)	(0.031)	(0.035)
Spy density × 1997	0.088 ***	0.009	-0.050	-0.052	-0.048	-0.078 **
	(0.030)	(0.035)	(0.034)	(0.036)	(0.036)	(0.039)
Spy density × 1998	0.101 ***	0.022	-0.052	-0.054	-0.051	-0.084 *
	(0.036)	(0.039)	(0.041)	(0.042)	(0.043)	(0.044)
Spy density × 1999	0.112 ***	0.033	-0.055	-0.057	-0.053	-0.086 *
	(0.041)	(0.043)	(0.046)	(0.048)	(0.048)	(0.049)
Spy density × 2000	0.118 ***	0.039	-0.060	-0.062	-0.059	-0.090 *
	(0.044)	(0.046)	(0.050)	(0.052)	(0.052)	(0.052)
Spy density × 2001	0.119 **	0.040	-0.068	-0.070	-0.067	-0.096 *
	(0.048)	(0.049)	(0.054)	(0.056)	(0.056)	(0.055)
Spy density × 2002	0.118 **	0.039	-0.077	-0.078	-0.075	-0.103 *
	(0.050)	(0.052)	(0.057)	(0.059)	(0.059)	(0.058)
Spy density × 2003	0.117 **	0.038	-0.088	-0.089	-0.086	-0.111 *
	(0.053)	(0.054)	(0.060)	(0.061)	(0.062)	(0.061)

Spy density × 2004	0.114 **	0.035	-0.099	-0.100	-0.097	-0.119 *
	(0.055)	(0.056)	(0.062)	(0.064)	(0.064)	(0.063)
Spy density × 2005	0.110 *	0.031	-0.113 *	-0.115 *	-0.112 *	-0.133 **
	(0.057)	(0.058)	(0.064)	(0.066)	(0.066)	(0.066)
Spy density × 2006	0.106 *	0.027	-0.128 *	-0.130 *	-0.126 *	-0.145 **
	(0.059)	(0.060)	(0.067)	(0.068)	(0.069)	(0.067)
Spy density × 2007	0.101 *	0.023	-0.142 **	-0.143 **	-0.140 **	-0.156 **
	(0.061)	(0.062)	(0.068)	(0.070)	(0.070)	(0.069)
Spy density × 2008	0.095	0.016	-0.157 **	-0.158 **	-0.155 **	-0.172 **
	(0.062)	(0.064)	(0.070)	(0.071)	(0.072)	(0.071)
Spy density × 2009	0.090	0.011	-0.170 **	-0.172 **	-0.169 **	-0.177 **
	(0.064)	(0.066)	(0.071)	(0.073)	(0.073)	(0.073)
Post × Object × Year FE	Yes	Yes	Yes	Yes	Yes	Yes
Post × County size controls		Yes	Yes	Yes	Yes	Yes
State × Year FE			Yes	Yes	Yes	Yes
Post × Opposition controls				Yes	Yes	Yes
Post × Industry controls					Yes	Yes
Post × Transfers						Yes
Observations	4650	4650	4650	4650	4650	3532
Adjusted R^2	0.528	0.548	0.703	0.703	0.704	0.799

Notes: This table shows the β coefficients of the panel data model laid out in equation (5.4.4). All specifications include county and year fixed effects. Standard errors are clustered at the county level with the usual confidence levels * $p < .1$, ** $p < .05$, *** $p < .01$). The Stasi share-year interaction for 1933 is omitted. The district of East Berlin is excluded from the data because East and West Berlin cannot be separated after reunification. Post is a dummy for the period after the fall of the Berlin Wall ($t \geq 1989$). Object of SI stands for Object of Special Interest. County size controls include log county area and log mean 1980s population. State refers to GDR states in the 1980s and post-reunification, and to Weimar provinces prior to World War II. For detailed information on the control variables, see Data Appendix 5.8.

5.8 Data Appendix

This appendix provides additional information on the different data sets and variables used for our empirical analysis. We present descriptive statistics for our outcome measures as well as definitions of the used control variables and detailed information on the data sources in Section 5.8.1. In Section 5.8.2, we describe the harmonization of the county-level data to the administrative territorial structure and county border definitions as of October 1990.

5.8.1 Descriptive statistics and data sources

Table 5.8.1 shows descriptive statistics for outcome variables at the county level, Table 5.8.2 for outcomes at the individual level. Table 5.8.3 describes all variables used and lists the respective sources.

The sets of control variables listed in the result tables for both SOEP and panel regressions are defined as follows. *County size controls* include log county area and log mean population in the 1980s. *Opposition controls* account for the intensity of the uprising in 1953 and include uprising intensity (four dummy variables) as well as two dummy variables for state of emergency and Soviet military intervention. *Industry controls* include the industrial employment share in September 1989 and a dummy variable equal to one if a strategically important industry (coal, oil, uranium, chemical, potash) was present in the county. *Transfers* are measured after 1994 and comprise federal and state transfers as well as investment subsidies paid to the counties. *Pre World War II controls* account for unemployment and self-employment in 1933, the share of protestants as of 1925, and the average election turnout as well as the average vote share of the Communist party and the Nazi party in the federal elections from 1928 to 1932. *Individual controls* include gender, age (and age squared), education (six dummy variables), learned profession (four dummy variables), household size (three dummy variables), marital status (two dummy variables), and SOEP wave fixed effects.

Table 5.8.1: Descriptive statistics on panel outcomes and controls

	Mean	SD	Min	Max	N
Electoral turnout	77.3	7.4	56.6	92.6	2,232
Self-employment rate	11.4	3.6	5.0	31.8	2,976
Patents per 100,000 inhabitants	11.9	14.6	0.0	212.6	2,604
Unemployement rate	18.4	4.0	3.7	31.3	2,790
Log population	10.9	0.6	9.6	13.2	4,650
Stasi share	0.4	0.1	0.1	1.0	186
Dummy: Object of Special Interest	0.0	0.2	0.0	1.0	186
Log mean population 1980s	11.0	0.6	9.8	13.2	186
Log county size (in sqm)	6.0	0.8	3.3	7.1	186
Uprising intensity 1953	1.4	1.4	0.0	4.0	186
Dummy: State of Emergency 1953	0.5	0.5	0.0	1.0	186
Dummy: Military intervention 1953	0.7	0.5	0.0	1.0	186
Share indust. employment 1989	45.3	13.6	16.8	74.5	186
Dummy: Important industries 1989	0.2	0.4	0.0	1.0	186
Log transfers	16.9	0.7	15.6	19.9	2,788
Log investment subsidies	16.2	0.7	14.6	19.1	2,788

Notes: This table presents descriptive statistics on outcome and control variables at the county level. Information on the respective years covered are provided in Appendix Table 5.8.3.

Table 5.8.2: Descriptive statistics on SOEP outcome variables

	Mean	SD	Min	Max	N
Dummy: Trust in strangers	0.14	0.35	0.00	1.00	3,389
× Born before 1940	0.18	0.38	0.00	1.00	895
× Born 1940–1961	0.14	0.34	0.00	1.00	1,867
× Born after 1961	0.11	0.31	0.00	1.00	627
× Stayed in county	0.15	0.36	0.00	1.00	2,713
× Moved	0.11	0.31	0.00	1.00	676
Negative reciprocity	9.22	4.23	3.00	21.00	3,014
× Born before 1940	8.80	4.41	3.00	21.00	754
× Born 1940–1961	9.10	4.11	3.00	21.00	1,673
× Born after 1961	10.09	4.25	3.00	21.00	587
× Stayed in county	9.26	4.20	3.00	21.00	2,443
× Moved	9.03	4.40	3.00	21.00	571
Dummy: Attend elections	0.71	0.45	0.00	1.00	3,116
× Born before 1940	0.80	0.40	0.00	1.00	789
× Born 1940–1961	0.69	0.46	0.00	1.00	1,732
× Born after 1961	0.65	0.48	0.00	1.00	595
× Stayed in county	0.69	0.46	0.00	1.00	2,484
× Moved	0.77	0.42	0.00	1.00	632
Dummy: Engagement in local politics	0.11	0.31	0.00	1.00	3,563
× Born before 1940	0.13	0.33	0.00	1.00	926
× Born 1940–1961	0.12	0.32	0.00	1.00	1,959
× Born after 1961	0.06	0.24	0.00	1.00	678
× Stayed in county	0.11	0.32	0.00	1.00	2,890
× Moved	0.09	0.29	0.00	1.00	673
Log monthly gross labor income	7.52	0.66	4.09	9.52	1,773

Notes: This table presents descriptives statistics for the SOEP outcome variables. For information on the respective years covered, see Appendix Table 5.8.3.

Table 5.8.3: Data sources and variable construction

Variable	Years	Source
Panel A – Stasi data (see Section 5.3.1)		
Spy density	1980–1988	The main explanatory variable of interest, regional spy density, is calculated as the average spy density at the county level in the period 1980–1988 (see Section 5.3.1 for details). Data on spies, called unofficial collaborators, are based on official Stasi records published by the Agency of the Federal Commissioner for the Stasi Records (*Bundesbeauftragter fr die Unterlagen des Staatssicherheitsdienstes der ehemaligen Deutschen Demokratischen Republik, BStU*) and compiled by Müller-Enbergs (2008). Population figures come from the Statistical Yearbooks of the GDR. Our measure of spy density covers unofficial collaborators for political-operative penetration, homeland defense, or special operations, as well as leading informers (*IM zur politisch-operativen Durchdringung und Sicherung des Verantwortungsbereiches, IM der Abwehr mit Feindverbindung bzw. zur unmittelbaren Bearbeitung im Verdacht der Feindttigkeit stehender Personen, IM im besonderen Einsatz, Fhrungs-IM*). In cases where Stasi held offices in Objects of Special Interest, the number of spies attached to these offices was added to the number of spies in the respective county.
Panel B – Individual SOEP data (see Section 5.3.2)		
Attend elections	2005, 2009	The question exploited reads as follows: “If the next election to the German ‘Bundestag’ were next Sunday, would you vote?”. Response options were given on a five-point scale to allow respondents to express different levels of conviction (not) to vote (“in no case”, “probably not”, “possibly”, “probably”, “in any case”). We construct a zero/one dummy grouping the former three and the latter two response options.

continued

Table 5.8.3 continued

Variable	Years	Source
Engagement in local politics	2001, 2007	Respondents are questioned about their involvement in citizen's groups, political parties and local governments (the question reads: "Which of the following activities do you take part in during your free time?"). Response options vary on a four point scale indicating weekly, monthly, less often or no involvement at all. We construct a zero/one dummy variable indicating whether respondents are involved at all. Note that information on individuals' engagement in local politics is provided in additional waves as well. We choose the years of 2001 and 2007 to cover similar points in time with all of our four measures of social capital.
Labor income	2003, 2008	Information on current monthly gross labor income is provided in every wave for East German respondents since 1992. As we aim to identify the direct relationship between surveillance, trust, and economic performance, we focus on those two waves in which both trust and wages can be observed.
Reciprocity	2005, 2010	We use three statements on negative reciprocity, response options varying on a seven-point scale. We follow Dohmen et al. (2009) by combining the three questions into one single measure. The respective questions read as follows: (i) "If I suffer a serious wrong, I will take revenge as soon as possible, no matter what the cost," (ii) "If somebody puts me in a difficult position, I will do the same to him/her," and (iii) "If somebody offends me, I will offend him/her back."
Trust in strangers	2003, 2008	The question on interpersonal trust reads as follows: "If one is dealing with strangers, it is better to be careful before one can trust them." Response options were given on a four-point scale, allowing the respondents to totally or slightly agree, or totally or slightly disagree with the given statements. To simplify interpretation of our estimates we group the first and latter two answers.

continued

Table 5.8.3 continued

Variable	Years	Source
Control variables		The set of control variables includes information on the respondents' age, sex, household size, marital status, education and learned profession. As different measures of social capital are measured in various waves of the survey; samples slightly differ for the outcome variables of interest.
Panel C – County-level data (see Section 5.3.3)		
Election turnout	1924–1932	We use election turnout in the federal elections in the Weimar Republic in 05/1924, 12/1924, 1928, 1930, 07/1932 and 11/1932. The data is provided in the replication data of King et al. (2008), available at the Harvard Dataverse, handle: hdl/1902.1/11193.
	1990–2009	Data on regional election turnout in the federal elections in 1990, 1994, 1998, 2002, 2005 and 2009 are provided by the Federal Returning Officer (*Bundeswahlleiter*).
Industry controls	1989	Industry composition is measured by means of the share of employees in the industrial sector as of September 1989, reported in Rudolph (1990). We further collect information from various sources whether large enterprises from the uranium, coal, potash, oil or chemical industry were located in the respective county. We construct a zero/one dummy based on this data.
Patents	1928–1929	We approximate county-level patent filings in 1928 and 1929 with data on high-value patents provided by Jochen Streb. High-value patents are defined as patents with a life span of at least ten years (Streb et al., 2006).
	1993–2005	Information on post re-unification patent filings come from the German Patent and Trade Mark Office (*Deutsches Patent- und Markenamt*). Yearly data are provided for 1995–2005; for 1992–1994 the aggregated number of patents is given. We assign the average number of patents to the year of 1993.

continued

Table 5.8.3 continued

Variable	Years	Source
Political ideology	1928–1932	We proxy historic political ideology by the mean vote shares of the Communist party (*Kommunistische Partei Deutschlands, KPD*) and the Nazi party (*Nationalsozialistische Deutsche Arbeiterpartei, NSDAP*) in the federal elections in 1928, 1930, 07/1932 and 11/1932 to construct two distinct measures of political ideology. Data on Weimar Republic election results are based on King et al. (2008).
Population	1925–1933	Population figures for the Weimar Republic are obtained from King et al. (2008) and Falter and Hänisch (1990).
	1980–1989	Data collected from the Statistical Yearbooks of the German Democratic Republic (*Statistische Jahrbcher der Deutschen Demokratischen Republik*).
	1990–2009	Collected from the Regional Database Germany (*Regionaldatenbank Deutschland*), the Statistical Offices of the Federal States (*Statistische Landesmter*) and the Working Group Regional Accounts (*Arbeitskreis Volkswirtschaftliche Gesamtrechnungen der Lnder*).
Religion	1925	The share of protestants in the population was published in the 1925 census of the Weimar Republic (*Volkszhlung 1925*). Our data stems from King et al. (2008).
Revenues	1995–2009	Data on revenues are obtained from the Regional Database Germany (*Regionaldatenbank Deutschland*). Revenues cover monetary transfers from the federal and state level (*allgemeine Zuweisungen und Umlagen von Bund, Land, Gemeinden/Gemeindeverbnden*) as well as investment subsidies granted to the counties (*Zuweisungen und Zuschsse fr Investitionsfrderungen*).
Self-employment	1925, 1933	County-level self-employment rates from the 1925 and 1933 censuses of the Weimar Republic (*Volks- und Berufszhlung 1925 und 1933*). Data for 1925 are obtained from Falter and Hänisch (1990); data for 1933 from King et al. (2008). Note that numbers for 1925 refer to households and should be considered as an approximation of the self-employment rate.

continued

Table 5.8.3 continued

Variable	Years	Source
	1996–2009	County-level data on the share of self-employed is available in the INKAR data base of the Federal Institute for Research on Building, Urban Affairs and Spatial Development (*Bundesinstitut fr Bau-, Stadt- und Raumforschung, BBSR*).
Unemployment	1933	County-level unemployment rates are based on the 1933 census of the Weimar Republic (*Volks- und Berufszhlung 1933*), provided in King et al. (2008).
	1996–2009	Monthly county-level unemployment rates are made available from March 1996 to December 2009 by the Federal Employment Agency (*Bundesagentur fr Arbeit*). We calculate yearly means from this data.
Uprising	1953	We use cartographic statistics published by the former West German Federal Ministry of Intra-German Relations (*Bundesministerium fr gesamtdeutsche Fragen*) to create two dummy variables indicating whether the regime declared a state of emergency and whether the Soviet military intervened in the particular county. In addition, the data provides an ordinal, additive measure of strike intensity ("none", "strike", "demonstration", "riot", "liberation of prisoners"). The map is available in the archives of the Federal Foundation for the Reappraisal of the SED Dictatorship (*Bundesstiftung zur Aufarbeitung der SED-Diktatur*), signature: EA 111 1889.

5.8.2 Redrawn county borders and data harmonization

We combine county-level data from various sources and decades in this study. Since 1925, the first year in our data set, county borders have been redrawn multiple times. To account for these territorial changes, we harmonize all county-level data to borders as of October 1990.

The Federal Institute for Research on Building, Urban Affairs and Spatial Development (*BBSR*) provides population and area weighting factors for all county border reforms from 1991 onwards to harmonize the data. We rely on population weights because population shares yield the most accurate harmonization of different border definitions with regard to our outcomes, which are mainly driven by people, not space. The outlined procedure is important as the number of East German counties was gradually reduced from 216 at the time of the German reunification to 87 in 2009 (the boroughs of East Berlin counting as one single county). Of course, this harmonization is only valid when looking at county-level aggregates and not individual data. The panel dimension of the SOEP, however, allows us to identify individuals' county of residence prior to the fall of the Berlin Wall.

Unfortunately, there are no administrative weighting factors available for the harmonization of county borders prior to reunification. However, there were only minor territorial reforms between 1953 and 1990, the period we cover with our GDR data. In ten cases, neighboring counties were merged together. In five cases, bigger cities became independent from the surrounding county (*Stadtkreise*). We manually account for these administrative changes using detailed maps and other historical sources. When merging two counties, we always use the maximum for each of the three riot variables (state of emergency, Soviet military intervention, strike intensity). In case new counties were constituted, we assign historical values of the emitting county to the created one.

When harmonizing data from the Weimar Republic with county borders as of 1990, considerable administrative territorial reforms have to be taken into account. Due to the lack of adequate population weighting factors, the harmonization is based on geospatial area weighting factors (Goodchild and Lam, 1980). We merge the corresponding shapefiles from the Weimar Republic with the shapefile for 1990 to determine weighting factors that allow to adjust the historical data to the county borders as of 1990. Given that most of our outcomes and control variables refer to people and not space, it needs to be stressed that this procedure is afflicted with some degree of imprecision. Given the long time span, the numerous territorial reforms, and the lack of population weighting factors, this procedure is, however, the most accurate harmonization procedure we can apply.

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Curriculum Vitae

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Personal Details

Born	November 22, 1985 in Bergisch Gladbach (NW, Germany)
Citizenship	German
Languages	German (native), English (fluent), French (basics)

Current Position

Since 09/2015	**Research Associate** Institute for the Study of Labor (IZA), Bonn, Germany
Since 10/2011	**Ph.D. Student in Economics** University of Cologne, Germany Advisors: Prof. Dr. Michael Krause (U Cologne) Prof. Dr. Gerard A. Pfann (U Maastricht)

Education

10/2005-08/2011	**Diplom-Volkswirt** (equiv. M.A. Economics) University of Cologne, Germany
09/2007-02/2008	**Graduate Studies in Economics** Trinity College Dublin, Ireland

Research Interests

Labor Economics, Political Economy

Publications

A. Refereed International Journals

2015	The Own-Wage Elasticity of Labor Demand: A Meta-Regression Analysis, *European Economic Review*, Vol. 80, pp. 94–119. (with A. Peichl & S. Siegloch)

B. In the Editorial Process

Exporting and Labor Demand: Micro-Level Evidence from Germany, ZEW Discussion Paper No. 14-013 [revise and resubmit, Canadian Journal of Economics] (with A. Peichl and S. Siegloch)

C. Work in Progress

The Economic Costs of Mass Surveillance: Insights from Stasi Spying in East Germany, IZA Discussion Paper No. 9245 (with M. Löffler and S. Siegloch)

Productivity Effects of Air Pollution: Evidence from Professional Soccer, IZA Discussion Paper No. 8964 (with N. Pestel and E. Sommer)

Benefit Duration and Job Search Effort: Evidence from a Natural Experiment, mimeo

Policy Projects

2015	People to jobs, or jobs to people? (IZA on behalf of Randstad)
2011-2014	NEUJOBS - Employment 2025 (IZA and 28 partners on behalf of the European Commission)

Presentations

Conferences and Workshops

2015	SOLE Meetings, Montreal (Canada) ZEW Lunch Seminar, Mannheim (Germany) IIPF Annual Congress, Dublin (Ireland)
2014	SOLE Meetings, Arlington (USA) IZA European Summer School in Labor Economics, Buch (Germany)
2013	CMR Lunch Seminar, Cologne (Germany) MAER-Net Colloquium, Greenwich (United Kingdom) 3rd Linked-Employer-Employee Data Workshop, Lisbon (Portugal) CESifo-Delphi Conference, Munich (Germany) 6th RGS Doctoral Conference in Economics, Bochum (Germany)
2012	NEUJOBS conference, Bratislava (Slovak Republic) 20 Jahre IAB-Betriebspanel, Nuremberg (Germany)

Further Training

2011/12	"Applied Econometrics for PhDs", Jun.-Prof. Dr. O. Badunenko, University of Cologne
2011/12	"Game Theory", Prof. Dr. B. Rockenbach, University of Cologne
2012	"Advanced Econometrics: Linear Models", Jun.-Prof. Dr. O. Badunenko, University of Cologne
2012	"Empirical Economics: Methods and Applications in Industrial Economics", Prof. Dr. S. Prantl, University of Cologne
2012	"Identification Strategies in Econometrics", Prof. D. Jaeger, University of Cologne
2012	"Theory and practice of program evaluation", G. Imbens, F. Mealli and A. Flores-Lagunes, Luxembourg
2012	"EUROMOD training course", Institute for Social and Economic Research, Colchester (UK)

Scholarships

10/2011-10/2014	**IZA Scholarship for Ph.D. Students** Institute for the Study of Labor (IZA), Bonn
09/2008-03/2009	**Sokrates/Erasmus-Scholarship for Studying Abroad** European Commission, Brussels (Belgium)

Earlier Positions

10/2011-08/2015	**Resident Research Affiliate** Institute for the Study of Labor (IZA), Bonn
09/2010-09/2011	**Student Research Assistant** Institute for the Study of Labor (IZA), Bonn
10/2005-06/2007	**Student Research Assistant** Institute for Economic Policy (IWP), Cologne

Non-Academic Work Experience

05/2010-08/2010	**Internship** RWE Power AG, Cologne
03/2006-04/2006	**Internship** District Government, Cologne

Miscellaneous

Software	MS Office, Latex, STATA

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