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Employment Protection, Capital Investments and Labor Productivity

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Employment Protection, Capital Investments and Labor Productivity^{*}

Johanna Kemper[†]

Abstract

In the this paper, I analyze the effect of Employment Protection Legislation (EPL) on investments in physical capital and labor productivity by exploiting the fact that small establishments in Germany below a given size threshold are exempted from certain parts of EPL. I do this by means of an Regression Discontinuity Design (RDD) and using establishment-level data for the period 1994-2012. Following the implications of the theoretical literature, I also analyze whether or not EPL affects the employment margin and conduct an implicit test for the possibility of a negative impact of EPL on investments due to hold-up by using linked employer-employee data. I do not find a statistically significant threshold effect on any of these outcomes– also not when analyzing the effect of EPL by industry. The results of EPL on investments and labor productivity are consistent with the predictions of the literature that states if EPL does not affect the employment margin, it should also not impact any other margin of non-labor adjustment.

Keywords: Employment protection, investments, labor productivity

JEL-Codes: J08, J65, J24

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1 Introduction

If the cost of Employment Protection Legislation (EPL) cannot be internalized through an optimally designed wage contract, EPL acts as a tax on separations and raises labor adjustment costs (Lazear, 1990; Autor et al., 2007). Given that EPL increases the cost of employing workers, firms may search for strategies to adjust their cost structure, for example through more flexible forms of employment (e.g. by outsourcing employment to temporary work agencies (Autor, 2003)), or by substituting labor with capital.

To analyze the impact of EPL on firm investments in physical capital and the consequences on other margins, such as labor productivity, is the aim of this paper. Thereby, the following questions shall be addressed. Does EPL induce firms to increase investments in physical capital to substitute labor with capital? Or does it reduce the incentives for firms to invest in physical capital because the workers who are protected by EPL exploit their relatively stronger bargaining position to extract higher wages? What does this imply for labor productivity?

In a perfectly competitive market without financial or labor market frictions, firms would react to EPL by substituting the relatively more expensive factor labor with capital. If firms do not completely substitute the factor labor with capital, this should also increase labor productivity, provided that capital and labor are complements. In contrast, in an imperfectly competitive market with search and matching frictions, workers could use the protection through EPL to bargain higher wages and thereby reduce firms' profits. This so-called *hold-up problem* leads to an under-investment in physical capital; with a potentially negative effect on labor productivity if capital and labor are complements. However, in the long run, when investments are no longer irreversible and firms cannot be hold up by their workers, EPL may lead to a substitution of labor with capital (Cingano et al., 2010). Finally, if specific human capital and physical capital are complements, a positive impact of EPL on investments and labor productivity could also arise in an imperfectly competitive market (Janiak and Wasmer, 2014).

Few studies have analyzed these links at the firm-level. Their findings are mixed regarding the effect of EPL on investments in physical capital. Autor et al. (2007) and Cingano et al. (2016), who exploit variation in EPL in the US and Italy respectively, find a positive effect of EPL on investments in physical capital. In contrast, other studies that all use cross-country data find that EPL decreases investments in physical capital (Calcagnini et al., 2009, 2014; Cingano et al., 2010). The effect of EPL on productivity is not clear-cut either. While most studies (Autor et al., 2007; Bassanini et al., 2009; Okudaira et al., 2013; Cingano et al., 2016) find a negative effect on total factor productivity (TFP), results are mixed regarding its effect on labor productivity. While Autor et al.

(2007) find a positive effect, Okudaira et al. (2013) find a negative effect of EPL on labor productivity. Finally, Cingano et al. (2016) do not find a statistically significant effect on labor productivity (as measured by total value added, holding the factor labor constant).

With this paper, I contribute to this still relatively small field in the literature by analyzing the effect of EPL on firm investments in total physical capital and its components, namely expansion investments and investments in information and communication technology (ICT), as well as the impact of EPL on labor productivity. Following the implications of the theoretical literature, I also analyze whether or not EPL affects the employment margin and conduct an implicit test for the possibility of a negative impact of EPL on investments due to hold-up.

To identify the effect of EPL on these outcomes, I exploit EPL exemptions of small establishments below a certain size threshold in Germany by means of a regression discontinuity design (RDD). For the analysis, I use a linked employer-employee dataset (LIAB) for Germany for the time period 1994-2012, which is provided by the Institute for Employment Research (IAB) (Klosterhuber et al., 2013).

I do not find a statistically significant effect of EPL on any of these outcomes– also not when analyzing the effect of EPL by industry. EPL neither affects capital investments nor labor productivity, nor does it have a statistically significant effect on employment or wages. This result does not change when varying the bandwidth used in the RDD estimation, when omitting observations just around the establishment size threshold or including covariates as robustness checks.

Even if the results for capital investments are not in line with the main findings of the other two empirical studies that exploit variation in EPL within a single country (Autor et al., 2007; Cingano et al., 2016), the results for labor productivity resemble those of Cingano et al. (2016). Autor et al. (2007) find a statistically significant impact of EPL on capital investments for only one out of three policies that represent EPL in the US. Overall, these results are consistent with the predictions of the literature (e.g. Lazear (1990); Autor et al. (2007)) that states if EPL does not affect the employment margin, it should also not impact any other margin of non-labor adjustment.

Despite the statistically insignificant result, this paper contributes to the literature in three ways. First, to the best of my knowledge, it is the first paper to analyze the effect of EPL on investments in physical capital and on labor productivity for Germany – a country that has one of the highest levels of EPL among all OECD countries (OECD, 2017). Second, to assess the effect of EPL on these outcomes, the paper exploits variation within a single country by means of an RDD. Besides Cingano et al. (2016), who analyze the effect of EPL on these outcomes in Italy, and Autor et al. (2007), who focus on the US, it is the third study that analyzes these outcomes by exploiting variation within a

single country. Complementing the existing literature in this way appears particularly relevant as the literature based on country-level data suggests a negative impact of EPL on investments, while these two studies find a positive effect. Third, the rich data set also allows analyzing the effect of EPL on interrelated outcomes, namely the employment margin and wages.

The paper is organized as follows. I introduce the theoretical and empirical literature in the second section. In the third section, I explain the details regarding the institutional framework and EPL in Germany. In the subsequent two sections, I introduce the data and the models used for the empirical analysis in the sixth section. In the empirical part, I follow the theoretical line of argumentation by first analyzing the effect of EPL on the employment margin, followed by the analysis of its effect on capital investments and labor productivity. As an extension, I also analyze the impact of EPL on wages. Section seven discusses the results shortly, before section eight concludes.

2 Related Literature

In this chapter, I describe the theoretical and empirical literature on the effect of EPL on physical capital and productivity in more detail. Thereby, I constrain this summary of the empirical literature to studies that use firm-level data.

By imposing restrictions on the hiring and firing of workers, EPL increases the cost of employing workers. If firms can transfer the cost of EPL onto workers, for example through lower entry wages, the employment margin as well as other margins should remain unaffected (Lazear, 1990). If this is not the case, for example due to legal and red tape costs, EPL acts as a tax on separations. In turn, this should lead to a reduction in the number of hirings and dismissals with an ambiguous effect on total employment.¹. If EPL impedes labor adjustment, other margins of non-labor adjustment should also be affected – for example investments in physical capital and labor productivity (Autor et al., 2007).

Following this line of argumentation, theoretical considerations regarding the effect of EPL on capital investments and labor productivity rest upon the assumption that EPL *either* affects the employment *or* the wage margin.

¹On the one hand, EPL makes firms more hesitant to hire workers or create jobs. On the other hand, it lowers the productivity threshold beyond which a worker is dismissed, which increases tenure of incumbent workers(Autor et al., 2007).

2.1 EPL and Capital Investments

The effect of EPL on physical capital investments is ambiguous from a theoretical perspective– at least in the short run.

In a perfectly competitive model without financial or labor market frictions, EPL increases the cost of labor, which gives firms an incentive to substitute labor with capital, i.e. to invest in physical capital (machines, computers, etc.) (Cingano et al., 2010, 2016).

In an imperfectly competitive market, EPL may decrease investments in capital. In this setting, firm-worker pairs generate quasi-rents that are allocated through a bilateral bargaining process. EPL increases the outside option of insiders, i.e. the workers who are protected through EPL, and lowers that of firms, as dismissing these workers entails legal or red tape costs. If investments increase the productivity of the insiders' jobs, they can use this protection to "hold-up" firms to bargain higher wages. Thereby, insiders lower firms' private returns on investments and their incentives to invest (Autor et al., 2007; Cingano et al., 2010).²

According to Janiak and Wasmer (2014), even in an imperfectly competitive labor market, EPL could have a positive impact on investments in physical capital if specific human capital and physical capital are complements.³

In the long run, when investments are not irreversible and firms cannot be held up by workers, EPL may lead to substitution of labor with capital, i.e. firm investments in capital increase (Cingano et al., 2010; Janiak and Wasmer, 2014).

The impact EPL on physical capital investments is also ambiguous from an empirical perspective. Only five papers that use firm-level data analyze the impact of EPL on firm investments in physical capital. Two of these studies find that EPL increases firm investments in physical capital: Autor et al. (2007) who analyze the effects of the introduction of wrongful discharge protection in some US states, and Cingano et al. (2016) who analyze firm-level data from Italy and exploit the variation of a reform that increased dismissal costs for firms below the size threshold of 15 employees. The three remaining

²Card et al. (2014) argue that the severity of this hold-up problem depends on the reversibility of the investments before entering the wage bargain. If investments take time to accumulate and are irreversible, the firm's threat-point of liquidating (part of) the capital stock is not credible. In that case, workers can extract part of the returns on investment that are sunk without contributing to the cost, which lowers firms' incentives to invest. In contrast, if the firm can fully deduce the investment cost before bargaining starts, this may mitigate the hold-up problem up to the point without hold-up, where the bargaining outcome is "efficient", thereby yielding positive investment levels.

³Two effects are relevant: (i) EPL increases the incentive to invest in specific human capital (Wasmer, 2006), (ii) EPL increases the share of senior workers with more specific human capital. But since the demand for specific human capital is non-monotonic in EPL ("hump-shaped"), so is the effect of EPL on investments in physical capital if specific human capital and physical capital are complements. In contrast, if senior workers' specific human capital and physical capital are complements, Janiak and Wasmer (2014) predict a positive monotonic relation between capital investments and EPL. However, if specific human capital and physical capital are no complements, this effect could also be reversed.

studies come to the opposite conclusion. Using firm-level data for 10 European countries and the OECD's Employment Protection Index to identify the effect of EPL on capital, Calcagnini et al. (2009, 2014) find that EPL decreases capital investments. Cingano et al. (2010), who exploit between-sector variation in the bindingness of EPL by using data for 17 European countries, come to the same conclusion. Papers that use countryor industry-level data are in line with these mixed findings.⁴

In this paper, I also analyze a specific component of capital investments, namely expansion and ICT investments. While a positive impact of EPL on expansion investment may speak for the substitution hypothesis, an increase in ICT investment could speak for the complementarity hypothesis proposed above, as ICT investments often require specific human capital. Otherwise, the same theoretical predictions made above should also apply for these two sub-components of capital investments (see, e.g., Cette et al., 2016). To the best of my knowledge, none of the existing studies use firm-level data in their analysis.⁵

2.2 EPL and Labor Productivity

The effect of EPL on labor productivity is also ambiguous from a theoretical perspective. There are at least four explanations for a positive effect of EPL on labor productivity. First, if EPL increases investments in physical capital, this could also increase labor productivity if physical capital and workers' skills are complementary.⁶ Second, if EPL makes firms more selective in the recruiting process, this could lead to an advantageous compositional shift in labor productivity. Third, if EPL drives inefficient firms out of the market, it could have a positive effect on labor productivity (Autor et al., 2007; Cingano et al., 2016). Finally, through its positive effect on tenure, EPL may increase the incentives for firms to invest in their workers' specific human capital, which could increase their labor productivity (Jansen, 1998; Belot et al., 2007).

EPL could also decrease labor productivity. First, the converse to the former argument: if EPL decreases investments in physical capital, this could also decrease labor productivity. Second, as EPL lowers the productivity threshold at which firms are willing

⁴For more details, c.f. Cingano et al. (2010, 2016).

⁵Few studies have analyzed the effect of EPL on ICT investments. Using cross-country data for 13 OECD countries, J. (2005) find that EPL lowers ICT investments. This finding is confirmed in a study by Gust and Marquez (2004), who use cross-country data for 13 industrial countries, as well as in a study by Cette and Lopez (2012), who use cross-country data for 11 industrial countries. In contrast, using cross-country data for 10 OECD countries, Guerrieri et al. (2011) find that the degree of labor market flexibility does not affect ICT investments. This finding is confirmed by Cette et al. (2016), who exploit within-country and -industry variations across time using data from 14 industrial countries.

⁶EPL decreases labor productivity only if labor and capital are perfect substitutes. This corresponds to a case where a firm replaced all labor by capital. In most cases, however, not all workers are replaced with capital. If capital and labor are complementary, this could also increase labor productivity.

to dismiss workers, they may retain relatively more unproductive workers, which could lead to a decline in labor productivity as well as the marginal productivity of capital, as argued in the previous section (Autor et al., 2007).

The ambiguity regarding the link of EPL on labor productivity also persists in the empirical literature. While most of the studies that use firm-level data find a negative effect of EPL on TFP, the effect on labor productivity is not clear-cut. Besides capital investments Autor et al. (2007) also analyze the effect of EPL on TFP. They find that the increase in capital investments and non-production worker employment through EPL leads to an increase in labor productivity but to a decline in TFP, and hence efficiency. Cingano et al. (2016) confirm these findings, as do Bassanini et al. (2009), who use firm-level data for 11 OECD countries and exploit between-sector differences in the bindingness of EPL. Moreover, using firm-level data for Japan and exploiting variations in court decisions, Okudaira et al. (2013) find that an increase in pro-worker decisions by a labor court reduces TFP growth and labor productivity. Papers that use country- or industry-level data confirm these findings.⁷

Hence, from a theoretical and empirical perspective, neither the effect of EPL on investments in physical capital nor that on labor productivity is clear-cut.

2.3 Hypotheses

Based on the theoretical assumptions formulated in the previous section, I propose two hypotheses, which I constrain to channels through which EPL affects both investment and labor productivity.

Hypothesis 1: EPL increases the incentive to substitute workers with capital. This raises investments in physical capital and thereby labor productivity.

Hypothesis 2: If insiders exploit the protection through EPL to bargain higher wages (hold-up), this decreases the incentive to invest in physical capital and also reduces labor productivity.

⁷Micco and Pages (2004), who rely on the same identification strategy as Bassanini et al. (2009) but use industry-level data for a cross-section of 18 developing and developed countries, show that EPL lowers labor productivity. Conti and Sulis (2016) find that TFP growth is more negatively affected in countries with high levels of EPL and in sectors with high human capital intensity and where technology adaptation is relatively more important.

2.4 Extensions of the Paper

Since the theoretical considerations regarding the effect of EPL on capital investments and labor productivity rest upon the assumption that EPL *either* affects the employment *or* the wage margin, I also test the impact of EPL on the employment and wage margin in the empirical part of the paper. Thereby, I first test whether or not EPL affects the employment margin. If it does, I expect it to also have an impact on capital investments and labor productivity. A negative impact on the employment margin could speak for a substitution effect (labor for capital), while a positive impact of EPL on employment could speak for a complementarity effect.

In the last part of the paper, I analyze the impact of EPL on wages at the establishment level and on individual wages of incumbent workers to test for the possibility of hold-up. As mentioned before, one reason why firms do not raise investments in response to EPL could be because they are "hold-up" by their workers who are protected through EPL and who exploit this protection to claim higher wages– with a negative impact on investments.

However, in case EPL increases wages, this does not automatically imply hold-up. For a hold-up problem to arise, the firm must make a non-contractible investment before the wage bargain starts (Card et al., 2014). Through the non-contactable investment, the firm cannot protect itself against the risk of being hold-up by the workers. However, as EPL increases the bargaining power of the protected workers in general, they can exploit this protection to claim higher wages without upfront investments of the firm. Hence, analyzing wages in isolation is insufficient to test for hold-up. Therefore, in an imperfectly competitive market, higher wages are only an indication for hold-up if the effect of EPL on capital investments is negative.⁸ Hence, the extension of this paper is an implicit and not an explicit test for hold-up.

3 Institutional Background

EPL for regular contracts in Germany is one of the highest (4^{th} rank) among all OECD countries. According to the OECD Index of Employment Protection, the index value of EPL for regular contracts in Germany was 2.68 in 2013 (unchanged since 1994), compared

⁸In contrast, in a perfectly competitive market, wages could also increase, because EPL may increase labor productivity through more investments in capital. Interpreting this wage increase as an indication of hold-up would be wrong. As before, a look at the other margins, capital investments and labor productivity, helps to disentangle the effect of an increase in wages through higher productivity from that through hold-up. Accordingly, if EPL increases capital investments and labor productivity, an increase in wages cannot be interpret as hold-up.

to the OECD average of 2.04. In contrast, temporary contracts have been deregulated over the years: from an index value of 3.25 in 1994 to 1.13 in 2013 (OECD, 2017).⁹

3.1 Employment Protection and Firm-Size Exemptions in Germany

Employment protection in Germany is regulated by the German Protection Against Dismissal Act (PADA). It applies to all workers with a regular contract and prohibits dismissals without a justified reason. There are three different categories of dismissals for a justified reason: first, dismissal for personal reasons, such as the worker's incapability or health problems; second, dismissal for personal misconduct, such as unlawful behavior; and finally, dismissal for operational requirements of the firm. To justify operational requirements as a reason for dismissal, the firm either has to downsize or restructure its business (Bauer et al., 2007).

In case a justified reason for a dismissal is given, the firm does not have to pay firing costs. However, even if a justified reason is given, two constraints remain. First, before dismissing a worker, the firm has to consider other employment options within the firm, including the option of re-training the worker. Second, the firm cannot choose which worker to dismiss first, but has to consider certain social criteria for redundancy, such as age of the worker, number of dependents, tenure, etc. (Jahn, 2005; Bauer et al., 2007). In addition, if a dismissed worker brings his case to court, the firm faces a substantial amount of uncertainty about the outcome and associated costs (redundancy payment, continued payment of salary, etc.). Despite the existence of some guidelines in the case law about what to consider as just cause, each case depends to a large extent on the judges' interpretation of the law, and judges often consider dismissals unfair (Bauer et al., 2007). Despite the government's efforts to reform EPL since the beginning of the 1990s, Germany has one of the highest numbers of dismissal cases that are brought to court.¹⁰

If a dismissal is considered unfair, the worker has the right to be reinstated (Bauer et al., 2007). However, over 80% of the wrongful employment lawsuits are solved outside of court and end with a severance pay (OECD, 2010). The PADA stipulates severance payments in just a few cases: if both parties choose a solution outside of court, or in case of a certain age and tenure of the worker.¹¹.

⁹Since the implementation of the so-called "Hartz reform package" in the early 2000s as a means to lower the high-unemployment, Germany has a dual-type EPL, with relatively well-protected regular contracts and a low protection of temporary contracts. While the reform left the protection of regular contracts more or less unchanged, temporary contracts and employment through temporary work agencies (TWAs) were substantially deregulated (more details can be found in section A of the Appendix).

¹⁰About 6 cases per 1,000 employees, versus less than two cases in the other OECD countries on average (OECD, 2010).

¹¹The severance pay stipulated by the PADA amounts to half of a monthly salary for every year

Hence, not only do firms face high legal hurdles when they want to dismiss workers for a just cause, but even if such a cause is given, a significant amount of uncertainty remains about the outcome and the costs involved if a dismissed worker appeals to a labor court.

3.1.1 Firm-size Exemptions

The PADA does not apply to establishments below a certain size threshold. Establishments below this size threshold can dismiss any worker without a particular reason.¹² The exemption from the PADA applies to establishments instead of firms. If a firm owns several establishments that are geographically separated, the size rule applies to each establishment separately.

The establishment size defined by the PADA is a "static" concept. If a worker is dismissed, it is the *usual* establishment size and not the actual size on the day of the dismissal that is relevant. Hence, the usual establishment size that is relevant for the application of the PADA is calculated based on the number of employed workers in the past and estimations for the future.¹³ As a consequence, an establishment cannot switch between being and not being exempted from one day to the other. Exact rules how the threshold is calculated can be found in section 4.1.2.¹⁴

3.1.2 Changes of the Firm-size Exemptions Over Time

According to the rules of the PADA, the establishment size is calculated based on fulltime equivalents (FTEs). In the time period analyzed in this paper, the size threshold was changed three times: in October 1, 1996; January 1, 1999; and January 1, 2004. Before October 1, 1996, establishments with up to five FTE employees were exempted from the PADA. From October 1996 onwards, the size threshold was increased, exempting establishments with up to 10 FTE employees (see also Figure 1). However, workers who were contracted before October 1996, remained protected for a transition period of three years. Hence, only new hires in existing establishments or newly founded firms were directly affected by this change. Before the end of the three-year transition period, the

worked in the firm. If a labor court finds it unacceptable to reinstate a worker after a dismissal was declared invalid, it can condemn the firm to pay a higher severance payment. Normally up to 12 months' salary. If a worker has worked for at least 15 years for the same firm and is over 50 years old, the severance payment amounts to 15 months' salary. And if the worker has worked for at least 20 years at the same firm and is over 55 years old, the severance payment amounts to 18 months' salary.

¹²Given that they comply with the moderate regulations of the Civil Code, which lays down a set of minimum standards that have to be met when dismissing workers, e.g. respecting the notice period (Bauer et al., 2007).

¹³The law does not stipulate how far back in the past the firm size has to be considered.

¹⁴I want to thank Thomas Klaes (labor lawyer) for providing me with the information regarding the calculation of the size threshold.

size threshold was reduced back down to the previous threshold of five FTE employees on January 1, 1999. Finally, on January 1, 2004, the size threshold was again lifted to exempt establishments with up to 10 FTE employees. This time, the old size threshold of 5 FTEs remained valid for all workers in a given establishment who were contracted before January 1, 2004, until the number of workers contracted before the change dropped below the old size threshold. For all workers contracted on January 1, 2004 or thereafter, the new size rule applied.¹⁵ Due to a special weighting scheme for part-time workers, the exact size threshold beyond which the PADA applies becomes 5.25 FTEs or 10.25 FTEs, depending on the respective point in time (more details are specified in section 4.1.2)



Figure 1: Changes of the establishment size threshold over time



3.1.3 Could the Changes in the Firm-size be Anticipated?

While the first and second changes in the size thresholds were discussed in public, the third change was somehow predictable. Bauer et al. (2007) state that intense discussions about the first change in the size threshold (October 1, 1996) started around April 1996, in the course of the election campaign for the general elections taking place in September of the same year. The second change in the threshold (January 1, 1999) was also known to

¹⁵For example, in an establishment with a total of 11 workers, 6 incumbents and 5 newly hired workers, all workers enjoyed protection under the PADA after January 1, 2004. If one of the newly hired workers was dismissed, all newly hired workers would lose the protection of the PADA (threshold of 10 FTEs would no longer be surpassed), but nothing would change for the incumbents. However, in case of a dismissal of one incumbent, all workers would no longer be protected by the PADA (the number of incumbents would fall below the old threshold of five FTEs, and the number of newly hired below the 10 FTEs threshold).

the public, as it was part of the election campaign of the center-left party, which had good chances of winning the general elections. In contrast to the first two changes, the last change in the size threshold (January 1, 2004) was not announced much in advance, but it was not surprising, since the center-left government, which had been re-elected in 2002, started labor market reforms in 2002 with the announcement that it would be introducing further reforms successively (the so-called "Hartz reform package") (Eichhorst and Marx, 2011). Given the pro-business orientation of the center-right coalition (elected in 2005), a repeated downward change of the size threshold was unlikely in the years after 2005.¹⁶

3.2 Other Regulations Applying at the Same Size Thresholds

Two other regulations apply at the size thresholds of 5 or 10 FTE workers. First, establishments with 5 or more full-time workers are allowed (but not obliged) to appoint one person as a work council.¹⁷ Work councils have two main rights: first, participation rights, which ensure that the work council must be informed and consulted about specific topics, such as the economic situation of the firm, changes in the workplace, or dismissals, and can make proposals without binding character to the employer; and second, co-determination rights, which give work councils a say in a range of issues including disciplinary rules and working time. A work council can prevent dismissals only in specials cases in which the decision taken by the firm is incompatible with existing agreements or practices. On the other hand, work councils have a co-determination right in the development of social selection criteria for dismissals (Koller et al., 2007; Fulton, 2015). According to Fulton (2015), larger firms are more likely to have work councils than small firms.¹⁸ Around three quarters of all work councils are members of unions. The size threshold for appointing work councils has not changed over time.

The second rule that applies at the same threshold as EPL is that establishments with more than 10 FTE workers have to have a break room (Koller et al., 2007), which is unlikely to have consequences for the empirical analysis.

¹⁶In the years after the re-election in 2002, the center-left party continuously lost popularity. As a consequence of this loss of popularity, chancellor Schroeder finally asked for a vote of confidence of the parliament in 2005. As he did not win the majority of votes, he decided to call early general elections. The result of these elections was a coalition between the Christian Democrats and Social Democrats with Angela Merkel as chancellor (Brettschneider et al., 2007).

¹⁷The number of allowed work councils increases with the firm size: one works council in establishments with 5-20 full-time workers, three in establishments with 21-50 full-time workers, etc.

¹⁸He states that in 2013, only 9% (10%) of all eligible firms with less than 500 workers had a work council in the West (East) of Germany, but they covered 43% (34%) of all workers. Among firms with \geq 500 workers, 87% (89%) had a work council in the West (East).

4 Data and Sample

The data used in this paper is the longitudinal version of the LIAB 1993-2014 from the Institute for Employment Research (IAB) (Klosterhuber et al., 2013). It combines person data from the social security register with survey data from the IAB establishment panel.

4.1 Establishment Sample

In the first part of the paper, I only use data from the IAB establishment panel. The IAB establishment panel is a representative survey conducted among German establishments with at least one worker liable to social security. It is conducted on an annual basis; the reference day is June 30. The sample drawn for the LIAB is stratified according to industry, firm size, and federal state (Klosterhuber et al., 2013).

4.1.1 Sample Composition

In the sample of establishments for the first part, I include all firms of the IAB establishment panel that were observed at least two years in a row. This is necessary since my major dependent variables, investments and labor productivity, refer to the pre-survey year, while variables necessary to calculate establishment size in FTE employees refer to the survey year. Concretely, if establishment j is surveyed in year t, it is asked about the investments and sales made in the previous year, t-1. Consequently, I have to construct my running variable, establishment size, so that it refers to the year preceding the survey date for the empirical analysis. Therefore, I use information from the survey conducted in the pre-survey year (t-1) to calculate the establishment size. I conduct cross-checks if the number workers in the survey in year t-1 is in accordance with the number of workers in year t.¹⁹ Having precise information regarding the worker categories that are relevant for the calculation of the size threshold is important since the identification strategy of this paper hinges on the exact calculation of the establishment size.

One drawback of the IAB establishment panel is that I do not have information about the number of workers in an establishment throughout the year, but only at the end of June of each year. This could introduce measurement error in my running variable. Using the linkage between IAB and the social security data is not an alternative, as the LIAB does not contain information about all employees in a given establishment and year.

¹⁹Even though the IAB establishment panel also contains information about the number of workers in the previous year, t-1, in the survey of the year t, using this data for the calculation of the establishment size has two drawbacks. First, in many cases, information on owners, family workers, and apprentices referring to year t-1 is missing in the survey conducted in year t. Second, establishments surveyed in year t are not asked about the number of part-time workers in year t-1.

However, since the establishment size defined by the PADA is a rather "static" concept, I do not consider this a big problem.

Because the PADA does not apply to the public sector, except in special cases (mixed forms of public-private enterprises), nor to the maritime and nautical sectors, I exclude all establishments from these sectors. Following (Bauer et al., 2007), I also exclude establishments from the heavily subsidized agricultural and mining and quarrying sectors. Omitting the sectors and restricting the sample to firms that I observe at least twice and without missing values for the running variable of establishment size, I am left with a final sample containing 173,641 (67%) of the original 259,805 establishments. I keep observations for the time period 1994-2012. I drop observations with the reference years 1993 and 2013 because of a too low number of observations to estimate the RDD model with year-specific treatment variables that I will introduce in the next section.

For a subsample analysis by industry, I use the time-consistent industry classification provided by the IAB (Eberle et al., 2011).

Since the reference day of the IAB is June 30th, it does not coincide with the dates when the establishment size thresholds defined by the PADA changed. Between the first change on October 1, 1996 and the references day of the next survey date is an eight months gap. The gap between the references day of the IAB and the change of the size threshold is lower for the other two changes on January 1, 1999 and January 1, 2004, namely six months. As we will see in section 5, the changes are only marginally important for the identification strategy.

4.1.2 Calculating the Establishment Size According to the Rules of the PADA²⁰

As the unit of observation in the data used in this paper is the establishment, it is in accordance with the legal unit referred to in the PADA. The PADA only refers to dependent employees with a regular contract. Consequently, owners, family members, and apprentices are not included in the size calculation of the establishment size. On the other hand, temporary workers are included, even if the rules of the PADA do not apply to them. The reason for this is that firms would have an even higher incentive to substitute regular for temporary workers if the latter did not have to be counted in the size calculation for the threshold. Regular and temporary workers are only counted if they have been employed for longer than six months (probationary period). The PADA defines establishment size in terms of full-time equivalents (FTEs). One FTE is equal to 40 hours a week.

 $^{^{20}{\}rm I}$ want to thank Thomas Klaes (labor lawyer) for providing me with the information regarding the calculation of the size threshold.

As I do not have information on the tenure of individual workers, I count each temporary or regular worker as one FTE. Part-time workers, i.e. persons who work 30 hours per week or less, are not counted according to a weighting scheme that is stipulated by the PADA.²¹

4.2 Linked Employer-Employee Sample

In the second part of the paper, I complement the analysis for wages at the establishment level with an analysis of the wages at the individual workers level, using the actual employer-employee linkage of the LIAB. This allows me to test for a number of issues in the establishment level estimation. First, to ensure that I have only "insiders" in my sample (i.e. EPL applies to them), I only include full-time employees who have been employed in the same establishment for *at least* one year. This allows me to disentangle possible effects of EPL on entry-wages from those on insiders. It also allows me to control for individual worker characteristics. For the analysis, I only use data for the period 1994-2009 from the longitudinal version of the LIAB 1993-2010, because the LIAB 1993-2014 does not contain certain variables that are critical for the analysis.

4.2.1 Sample Composition

From the sample of the social security register data, which contains data for more than 24 million persons for multiple spells (over 43 mn. observations in total) for the time period 1993-2010, I drop all observations without an establishment identifier for the IAB establishment panel.²² Since the data contains multiple spells for one individual, I only keep individuals who have been employed by one establishment throughout the year.²³ I only keep full-time workers to be able to disentangle the effects of EPL on the entry wage of new hires from that on incumbents.²⁴ Following the rules of the PADA, I also exclude the following groups: family workers, apprentices, and trainees. The big drawback of the person data is that I do not have information regarding whether a full-time worker is employed with a temporary contract or not. As EPL does not apply to workers with a

²¹The IAB establishment panel has information on the number of part-time workers and their weekly working hours grouped into three categories. However, the weighting scheme does not correspond to the weighting scheme indicated by the PADA. I try to approximate the weighting scheme specified by the PADA by re-weighting the data accordingly. The weighting scheme for part-time workers was altered with each change of the size threshold. Table 7 in the Appendix summarizes the weighting scheme.

 $^{^{22}\}mathrm{As}$ mentioned in the previous section, the LIAB does not contain information about all employees in a given establishment and year.

 $^{^{23}\}mathrm{This}$ way, I keep 30% of all persons included in the original sample.

²⁴As mentioned in section 4.2, if the costs through stricter EPL can be undone through an optimally designed work contract, one would expect the entry wage of new hires to be lower than that of incumbents (Lazear, 1990).

temporary contract, they should not be in the sample. The minimum that I can do is to exclude all workers in firms that belong to the industry sector of temporary working agencies.

I link this sample to the establishment data sample from the previous section and only keep establishments with no more than 50 FTE workers for efficiency reasons, leaving me with 197,905 observations for individuals (roughly 8% of the total sample).

5 Empirical Methodology

I evaluate the effect of EPL on the outcome variables by means of an RDD. Because the assignment-to-treatment variable is a law, this is a *sharp* RDD. This also implies that the probability of being subject to EPL changes from zero to one at the threshold. Thereby, the identification strategy fulfills one of the two assumptions needed for the validity of the RDD. That is, the probability to receive treatment is continuous except at the threshold. The second assumption needed for the validity of the RDD is the local continuity restriction, which guarantees the comparability of establishments in a small interval around the threshold, so that they are "as good as randomly assigned" around the threshold. This also implies that establishments do not endogenously selfselect below the size threshold in order to escape EPL. To re-assure that this assumptions holds, I conduct two test. The McCrary test (McCrary, 2008) that tests whether the establishment size distribution is continuous at the size threshold. In addition, I also test whether establishment characteristics are continuous at the size threshold. The results are provided in section 4.6.1.

If these two assumptions are met, the RDD identifies the local average treatment effect of EPL on the respective outcomes (Lee and Lemieux, 2010).

Since I assess the effect of EPL on the respective outcomes by means of an RDD, I only exploit the cross-sectional variation of the size threshold for each year between 1994 and 2012. As explained in the next subsection, I run three different variants of the RDD regression: one that estimates one treatment effect for all years; another accounting for period-specific treatment effects; and, lastly, a model with year-specific treatment effects.

Empirical Model for the Establishment-Level Data 5.1

To investigate the impact of EPL on capital investments, labor productivity, and other measures of interest, I estimate the following model:

$$Y_{jt} = \alpha_t + f_t (F_{jt} - c_t) + \gamma D_{jt} + \epsilon_{jt}$$

$$\tag{1}$$

Where Y_{jt} is the dependent variable of establishment j in year t. $f_t(\cdot)$ approximates the year-specific functional forms of the centered running variable. The size of establishment j at time t is $(F_{jt}-c_t)$ is centered by subtracting the threshold value c_t beyond which EPL applies in year t. D_{jt} denotes a dummy variable that takes the value 1 if establishment j in year t is above the size threshold and 0 if it is below the threshold. Consequently, γ is the main coefficient of interest, which quantifies the effect of EPL on the respective outcome variable.

Besides model 1 with one treatment dummy for the entire time period, I also estimate a model with period-specific treatment effects – for the four periods, 1994-1996, 1997-1998, 1999-2003, and 2004-2012 and a model with year-specific treatment effects. Both are estimated using $\gamma_t D_{jt}$ instead of $gammaD_{jt}$, where γ_t is the treatment effect for the respective period or year year t.

Table 9 summarizes the respective size thresholds by period.

Table 1: Periods and size thresholds											
	Time	Size threshold	Date of reform								
Period 1	94-96	5.25									
Period 2	97-98	10.25	Oct.96								
Period 3	99-03	5.25	Jan 99								
Period 4	04-09	10.25	Jan 04								

I estimate model 1 by OLS using robust standard errors clustered at the establishment level, using the different dependent variables, which I will present in the following.

Outcome Measures 5.1.1

Before presenting details about the outcome measures, I want to emphasize that I cannot use dependent variables in per-worker terms, as my identification strategy depends on the establishment size, which is measured by the number of employees. However, because I control for establishment size by including polynomials of the running variable, the estimated treatment effects of EPL on the respective outcome can be interpret as holding labor constant (Cingano et al., 2016). As an example, the coefficients of the treatment

effect on the capital stock and value-added can be interpret as the effect of EPL on capital deepening and labor productivity.

Capital Investments: Total capital investments comprise investments in real-estate, buildings, ICT, production plants, office- and factory equipment, transport systems, and means of transport. I specify four different variables for capital investments: the logarithm of the capital stock, the logarithm of total capital investments as a flow measure, and the logarithm of its sub-components, expansion investment and ICT investments (both also flow measures). All investment variables are deflated by the producer price index for investment goods from the German Federal Statistical Office (normalized to 100 in 2010).

To build the capital stock, I use the perpetual inventory method.²⁵ For total capital investments (flows), I take the logarithm of year-by-year investment flows. Both variables are available for the time period 1994-2012.

To construct the two sub-components of capital investment, expansion and ICT investments (both also flow measures), I use information about the share of total capital investments that is spent for expansion and for ICT investments, respectively, together with the information on total capital investments to build each variable.²⁶ As these variables are reported in shares, their quality may be less precise. The variable expansion investments is available for the time period 1996-2012, while ICT investments is only available for 2000-2006.

Labor Productivity: To measure labor productivity, I use the logarithm of total value added. Total value added is calculated by subtracting the costs of intermediary inputs from the annual sales.²⁷ It is deflated by the aggregate producer price index from the German Federal Statistical Office (normalized to 100 in 2010).²⁸ Unfortunately, the information to build the labor productivity measure is only available for about half of the establishments in the sample. This variable is available for the time period 1994-2012.

Other outcomes: To account for the effect of EPL on the employment margin, I use three different measures: the job reallocation rate, the logarithm of the number of

²⁵Following Guertzgen (2009), I approximate the initial value of the capital stock by dividing investment expenditures in each establishment's first observation year by a pre-period growth rate of investment, g, and a depreciation rate of capital, δ . This implies the assumption that investment expenditures have grown at a constant rate g. Hence, the initial capital stock of firm j in year 1 is $K_{j1} = \frac{I_{j1}}{(\delta+g)}$, with I_{j1} real investment of firm j in year 1. As Guertzgen (2009) proposes, I set $\delta = 0.1$ and g = 0.05. In the subsequent years, the capital stock of firm j in year t is calculated following Griliches (1980): $K_{jt} = K_{jt-1}(1-\delta) + I_{jt}$, with I_{jt} real investment of firm j in year t.

²⁶Regarding expansion investments, the questionnaire of the IAB panels asks the following question: "What share of total investments made was attributed to the expansion of your establishment?"

²⁷The intermediary inputs comprise goods and services provided by other firms, raw materials and supplies, goods for resale, hiring and leasing costs, etc.

²⁸Unfortunately, the German Federal Statistical Office could not provide me with disaggregated sectorspecific producer price indexes for the observation period of this paper.

temporary workers, and of the number of white-collar workers. Following Davis and Haltiwanger (1992), the job reallocation rate of establishment j in year t is calculated as $\frac{2|(F_{jt}-F_{jt-1})|}{(F_{jt}+F_{jt-1})}$. White-collar workers are defined as the number of workers with at least a tertiary degree in establishment j. I further analyze wages on both the firm and the individual level; the latter is discussed in the next section. To account for the wage at the establishment level, I use the logarithm of the wage bill of establishment j in the month of June in year t. It is deflated using the same producer price index as for value added.²⁹

5.2 Empirical Model for Linked Employer-Employee Data

As mentioned in section 4.2, I also analyze the effect of EPL on wages to test for the possibility of hold-up. This extension of the paper is an implicit and not an explicit test for hold-up.

Besides analyzing the gross wage at the establishment level, I use matched employeremployee data to estimate the effect of EPL on the individual worker's wage. In contrast to establishment-level data, using individual-level data has several advantages. First, as I only include persons that have been employed for *at least* one year in the same establishment, I can better control for the effect of EPL on wages of insiders, i.e., those workers who are protected by EPL. Second, I only include full-time workers. In contrast to using establishment data, this allows disentangling the effect of EPL on the wage from other effects that influence the wage, such as the use of part-time work. Third, this allows to control for worker heterogeneity by including different worker characteristics as covariates, thereby increasing estimation efficiency.

To estimate the effect of EPL at the worker level, I use the LIAB. I do this by estimating the following model:

$$\ln w_{ijt} = \mu_t + f_t(F_{jt} - c_t) + \gamma D_{jt} + \epsilon_{ijt}$$
⁽²⁾

Where w_{ijt} is the logarithm of the real gross daily wage of worker *i* in establishment *j* at time *t*. It includes fringe benefits and bonus payments that are subject to social security contributions. Because worker wages in LIAB are top-coded, I will also run estimations using imputed wages. The procedure is described further below. As in the model for the establishments, $f_t(\cdot)$ approximates the year-specific functional forms of the centered running variable. $(F_{jt} - c_t)$ is the size of establishment *j* at time *t*, which is centered by c_t , which is the threshold value beyond which EPL applies in year *t*. D_{jt} is the treatment dummy, which is 1 if worker *i* in establishment *j* is above the size threshold at time *t*, and 0 if he is in an establishment below the size threshold. Therefore, γ quantifies the effect

 $^{^{29}}$ I use the producer instead of the consumer price index because I analyze the firm side.

of EPL on the worker's wage. As before, I also estimate model 2 with period-specific and year-specific treatment effects ($\gamma_t D_{jt}$).

As a robustness check, I also include worker and establishment characteristics in model 2. The worker characteristics include age, age squared, tenure, tenure squared, education and a dummy for females.³⁰ The establishment characteristics include dummies accounting for whether an establishment is subject to a collective bargaining agreement, whether it has a work council, whether it is a private limited company and the founding year of the establishment.

The wages in the social security register data are top-coded, i.e. right-censored, as there is an upper contribution limit to the social security system Klosterhuber et al. (2013).³¹ In my sample, censoring affects about 6.6% of all observations.

To replace the right-censored wages, I use a single imputation method, as proposed by Gartner (2005). Therefore, a Tobit model is estimated with the logarithm of the daily wage as dependent variable and including individual (education, age, occupation, tenure, region of workplace, position category) and establishment characteristics (business form, whether the establishment had a works council, was subject to a collective agreement, and payed a wage cushion, and the share of females). The censored wages are replaced by wages that are randomly drawn from a truncated normal distribution, where the predicted values from the Tobit regression are used to construct the moments of the distribution. The lower truncation point of this contribution is defined by the upper contribution limit of the social security system (Guertzgen, 2009). However, using single imputation has the drawback that it could lead to biased variance estimations. Since multiple or Bayesian imputation methods would have been computationally intensive, I chose this simpler method. Because of the methodological doubts, I run the regressions using the imputed wage as dependent variable only as a "robustness check" with the explicit statement that the results have to be interpreted with caution.

³⁰Tenure is defined as the number of days for which a person has worked in the respective establishment. Education is a categorical variable with six categories, where 1=primary/lower secondary school without vocational education (VET) degree, 6= university degree.

 $^{^{31}}$ If the gross wage surpasses this upper bound, this part of the wage will not be relevant for the calculation of the social security contributions (i.e. pension and unemployment contributions). This upper contribution limit was 46,630 Euro in Western Germany (36,199 Euros in the East) in 1994, and 67,200 Euro in the West (57,600 Euros in the East) in 2012 (for more details, see Table 8 in the Appendix).

5.3 Optimal Bandwidth and Functional Form

The optimal bandwidth is estimated for each year separately using the bandwidth selector proposed by Imbens and Kalyanaraman (2011).³² In order to analyze the optimal functional form, I test six alternative specifications of the function $f_t(\cdot)$ in equations 1 and 2. Since I allow these functional forms to differ across time, the corresponding estimations are done for each year separately in the sample of establishments within the above-mentioned optimal bandwidth. The first three specifications constrain the parameters of $f_t(\cdot)$ to be the same for a linear, quadratic, or cubic polynomial of the assignment variable. By adding interaction terms of the linear, squared and cubic polynomial of the assignment variable, I allow the functional forms to differ on either side of the size threshold. The optimal functional form is determined by means of the Akaike Information Criterion (AIC), as suggested by Lee and Lemieux (2010). The results can be found in Table 3.

5.4 Robustness Tests

To test the robustness of the results, I conduct several tests. First, I estimate so-called *Donut regressions* for each outcome (see, e.g., Barreca et al., 2011). The model for the Donut regressions simply leaves out establishments that are one FTE unit above or below the size threshold. Since the observations that are closest to the threshold are most susceptible to manipulation, not including these observations in the regression is a way to control for selection of firms above or below the threshold, because one needs stronger assumptions in order to get consistent results for the treatment effect. Second, I include covariates and industry dummies in the RDD regressions. This should only affect the standard errors but not coefficient estimates in a valid RDD setting (Lee and Lemieux, 2010). Third, I vary the size of the bandwidth by taking different fractions of the optimal bandwidth (75%, 50% and 25% of the optimal bandwidth for the establishment-level estimations and 150%, 200% and 300% of the optimal bandwidth in case of the individual-level data).³³

 $^{^{32}}$ Using a bandwidth estimator that allows the bandwidth to vary on each side of the threshold would have been better. Unfortunately, the bandwidth selector proposed by Calonico et al. (2014) did not work on the computers in the IAB research data center.

³³In case of the individual-level data, I have to increase the size of the bandwidth, because the starting bandwidth is already very small.

6 Results

In the first part of this section, I show that selection is not a problem in my data. In the second and third part, I show the optimal bandwidth and functional forms, as well as the summary statistics. Following the line of argumentation of the theoretical literature, I start with an analysis of EPL on the employment margin in the fourth subsection. Subsequently, I present the main results of this paper: the effect or EPL on capital investments and labor productivity. The sixth sub-section shows the results of an analysis of the effect of EPL on wages. After a short discussion in sub-section seven, I conclude in the last part of the paper.

6.1 Bunching

Showing that establishments do not select below the size threshold or stay intentionally small to avoid being subject to EPL is important to prove the validity of the RDD. Figure 2 shows the distribution of the running variable establishment size for the four periods: (1) 1994-1996, (2) 1997-1998, (3) 1999-2003, and (4) 2004-2012. If selection was a problem, one would observe a jump at the respective size threshold, each marked with a vertical line. The vertical line corresponds to establishments with more than 5.25 FTE workers in the first and third period, and 10.25 FTE workers in the second and fourth period. However, none of the densities depict a noticeable jump at the respective size threshold.



Figure 2: Density of the running variable

The McCrary test (McCrary, 2008) is a formal test to show that selection out of EPL, i.e. below the threshold, is not a problem in the data that I use. Under the null hypothesis, it tests whether the distribution of the running variable is continuous around the threshold. Rejecting the null hypothesis of continuity would yield evidence for selection around the threshold.

For the validity of this test, the selection can only occur in one direction. This requirement is fulfilled in this paper, since selection of establishments into EPL seems rather implausible. A negative sign of the discontinuity estimate corresponds to a selection below the size threshold, while a positive sign corresponds to a selection above it. I conduct this test for establishments with 20 or fewer FTE workers for each year separately.³⁴

Table 2 shows the results of the McCrary test for the years 1994-2012. Column 2 shows the discontinuity estimates and the corresponding standard errors, and column 3 the corresponding t-values. The null hypothesis of continuity of the running variable can only be rejected in the year 2005, implying a statistically significant (10%-level) jump at the threshold. However, the sign of the effect indicates that establishments select into EPL. Since this does not imply that establishments try to circumvent EPL, this kind of

³⁴Using the average of the optimal bandwidth of all dependent variables yields similar results.

Lan	ie 2. Micerary	test by year
Year	Discont. est.	T-ratio
	(se)	
1994	-0.2	1.04
	(0.2)	
1995	-0.11	0.72
	(0.15)	
1996	-0.05	0.39
	(0.13)	
1999	0.07	0.66
	(0.11)	
2000	-0.07	0.76
	(0.1)	
2001	0.05	0.47
	(0.1)	
2002	0.07	0.75
	(0.09)	
2003	-0.07	0.70
	(0.09)	
1997	-0.28	1.43
	(0.19)	
1998	-0.14	0.73
	(0.19)	
2004	-0.12	0.90
	(0.13)	
2005	0.24	1.83
	(0.13)	
2006	-0.04	0.33
	(0.13)	
2007	-0.2	1.59
	(0.13)	
2008	0.05	0.41
	(0.13)	
2009	0.05	0.38
	(0.14)	
2010	0.06	0.6
-	(0.1)	
2011	-0.08	0.64
	(0.13)	-
2012	0.11	0.58
	(0.19)	
	(0.10)	

Table 2: McCrary test by year

Note: **** p < 0.01, *** p < 0.05, *p < 0.1. The table shows the results of the McCrary test. A statistically significant discontinuity estimate implies rejection of the null hypothesis of continuity of the distribution. A negative sign of a statistically significant continuity estimate implies a selection problem.

selection should not be a threat to my analysis. The results for the McCrary test in all other years do not show evidence for a discontinuity of the establishment size distribution. Hence, according to these results, selection does not seem to be a problem.

Another way to analyze the validity of the RDD is to show that establishments just above and below the size threshold do not differ substantially in terms of observable characteristics. A continuous relation of establishment characteristics just below and above the size threshold would imply that they are as good as randomly assigned around the threshold (Lee and Lemieux, 2010).

I conduct this test by estimating RDD models for each year between 1994 and 2012, using different establishment characteristics as outcomes. These include the industry sector of the establishment and the following other establishment characteristics: whether a company had a works council (*works council*), whether its business form is that of a private limited company (*busin. LLC.*), whether it was subject to a collective agreement (*collect. agr.*), whether it payed a wage above the collectively agreed one (*wage cushion*), and the share of females (*share fem.*). The results of this test do not indicate systematic differences of establishments above and below the size threshold for any of these characteristics over time. The discontinuity estimates are only statistically significant in a few cases. The results can be found in Tables 10, 11, and 12 in the Appendix.

In summary, based on the results of this section, I conclude that there is no evidence for a systematic selection of establishments below the size threshold to escape the higher costs from being subject to EPL.

6.2 Optimal Bandwidth and Functional Form for the RDD

For each regression, I selected the optimal bandwidth for each year separately using the optimal bandwidth selector by Imbens and Kalyanaraman (2011). The drawback of this bandwidth selector is that it does not allow me to estimate a separate bandwidth for observations above and below the threshold.³⁵ The optimal functional form for each year was chosen by means of the AIC, as suggested by Lee and Lemieux (2010). Therefore, I run RDD regressions for each dependent variable including polynomials of the centered running variable of the first (linear), second (squared), and third (cubic) order, as well as their interactions. The optimal bandwidth and functional forms for each variable can be found in Table 3.

Table 9 in the Appendix shows the summary statistics for all outcome measures for each of the four periods. The outcomes are presented for establishments below and above

 $^{^{35}}$ As mentioned earlier, I could not use the bandwidth selector proposed by Calonico et al. (2014) as it did not work on the computers of the IAB data center. In contrast to the selector by Imbens and Kalyanaraman (2011), this selector allows the bandwidth to differ on each side of the threshold.

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	stoc	CK	NOH	NS	INVESU	nent	INVEST	ment	product	IVITY	monthly	wage	wag	0	cation i	ate	worke	IS	M	orkers
Optimal	bandw.	Ĥ.	bandw.	Ĥ.	bandw.	Ĥ.	bandw.	Ĥ.	bandw.	ff.	bandw.	ff.	bandw.	Ĥ.	bandw.	Ĥ.	bandw.	Ĥ.	bandw.	ff.
1994	9.81	squ.	10.55	lin.	1	1	1	1	8.75	cub.	7.70	squ.	3.01	lin.	7.82	lin.		,		1
1005	01		92 11						0 10		700		101	int.	1					
CRAT	1.19	int.	11.70	.nbs	I	ı	I	I	9.10	int.	ð.04	squ. int	1.94	III.	1.19			1	I	I
1996	9.23	lin.	8.29	lin.	12.24	lin.	I	I	8.62	lin.	7.47	cub.	2.79	lin.	4.98	lin.	18.92	squ.	I	I
		int.										int.								
1997	7.24	lin.	10.50	cub.	12.38	lin.	I	I	6.09	cub.	7.70	cub.	1.15	.nbs	5.30	cub.	16.54	lin.	I	ı
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OFFI	10.0	.nhe	9.30	int.	07.11	int.	I	I	10.1	cup.	10.1	cup.	1.00	int.	4.04		07.11	cup.	I	1
1999	7.33	squ.	9.37	squ.	11.44	lin.	ı	ı	7.12	squ.	3.92	cub.	1.53	lin.	3.98	cub.	15.75	lin.		ı
2000	7.14	squ.	8.22	lin.	8.22	lin.	9.39	lin.	5.35	cub.	3.14	squ.	1.88	int. cub.	4.69	lin.	13.63	lin.		I
				int.										int.						
2001	7.25	squ.	9.03	lin.	10.42	.nbs	9.59	lin.	6.75	cub.	3.28	squ.	2.15	squ.	4.46	squ.	13.95	lin.	I	I
2002	7.12	cup	9.57	int.	9.02	lin	10.27	lin	6.22	cub	3.02	IIUS	1.34	int. lin	4.34	lin	12.71	lin		1
				5								5		int.	-		-			
2003	8.52	lin.	8.65	squ.	12.63	lin.	8.13	lin.	6.63	squ.	2.53	squ.	1.62	cub.	4.21	lin.	12.81	lin.	11.98	lin.
2004	6.62	int.	0.85	1105	11 60	int. lin	0 07	lin	6 43	int.	7 76	int.	2.55	int.	3 97	lin	13 73	lin	12.60	lin
				5						5		int.								int.
2005	6.16	squ.	8.92	squ.	10.05	lin.	9.90	squ.	6.18	squ.	7.11	cub.	1.78	squ.	3.81	lin.	12.60	lin.	10.52	.nbs
				int.						int.				int.						int.
2006	6.50	squ.	8.26	squ.	6.92	lin.	9.92	squ.	5.37	cub.	7.10	cub.	2.18	lin.	3.80	lin.	11.80	squ.	11.45	squ.
						int.		int.												int.
2007	5.13	cub.	6.62	squ.	8.07	cub.	I	ı	6.16	lin.	6.80	cub.	2.35	lin.	3.82	.nbs	10.92	squ.	9.58	cub.
										int.				int.						int.
2008	6.23	lin.	7.91	squ.	7.68	.nbs	I	I	5.76	squ.	7.01	squ.	1.82	cub.	3.64	lin.	10.62	cub.	10.42	squ.
	1		1			,				;		int.		int.						;
2009	5.96	.nbs	7.49	squ.	7.09	cub.	I	ı	6.27	lin.	6.89	cub.	2.01	cub.	3.60	squ.	10.01	cub.	10.12	lin.
		int.								int.				int.						int.
Note: Optim second (squar	al bandwidtl red) and thir	h (bandw.) d (cubic)) was chosen order degree	according s polynomì	to the ban als of the co	dwidth chc entered rur	ice rule of ming variab	Imbens and de, i.e. esta	Kalyanaran blishment-siz	an (2010) e. I also a	. To find the llow these di	e optimal fferent fur	functional fo ictional forn	orm (ff.), I is to differ	run RDD re on each side	gressions of the th	or each dep eshold (inte	endent va r.).	riable incluc	ling first (linear),

Table 3: Optimal bandwidth & functional form

the size threshold separately, using the respective optimal bandwidth. Figures that show the mean values of the main outcome variables for capital investments and the labor productivity variable along with their standard errors can be found Figures 8 to 16 in the Appendix.

6.3 The Effect of EPL on the Employment Margin

As mentioned in section 2, Autor et al. (2007) argue that if EPL does not lead to higher adjustment costs (e.g. because the costs can be undone by lower wages), thereby reducing hiring and dismissals, EPL should also not affect other margins such as labor productivity or investments.³⁶ Following this line of argumentation, a first logic step before analyzing the effect of EPL on capital investments and labor productivity is to check its effect on the employment margin- which I will do in the following.

According to the results of Figure 2 and the year-specific McCrary tests, EPL does not seem to affect the size distribution of the establishments. Otherwise, we would have observed evidence for a discontinuity around the size threshold. Given the theoretical predictions of the effect of EPL on employment, one would have expected that sizecontingent employment regulations impede the growth of establishments and generate discontinuities in the establishment size distribution (Cingano et al., 2016). The result of an absence of a discontinuity is in line with the study of Bauer et al. (2007), who show in their analysis of the effect of EPL in Germany that establishments just below the size threshold are not less likely to grow.

However, these findings do not necessarily imply that EPL does not have consequences for the employment decisions of establishments. Even though Bauer et al. (2007) do not find a statistically significant effect of EPL on the job flow rate, nor on hirings and firings, Bauernschuster (2013) who analyses the effect of the 2004 reform of the establishment size threshold finds a statistically significant positive effect on hirings. In addition, the results of a number of unpublished studies that analyze the threshold effects of the PADA on different employment outcomes (i.e., employment stability, hirings and separations, use of temporary contrasts) provide mixed evidence.³⁷

Following Autor et al. (2007), Cingano et al. (2016) and Cingano et al. (2016) I analyze

³⁶In their paper, Autor et al. (2007), analyze the effect the adaption of wrongful-discharge protection by state courts in the US on productivity and capital investments. Trading arguments of models assuming a competitive labor market against the predictions of a Diamond-Mortensen-Pissarides model, which is based on the assumption of an imperfectly competitive labor market, they argue the following: if EPL does not reduce hirings and separations, both models predict that EPL should not have an effect on outcomes such as productivity.

³⁷Among others, these include Priesack (2015); Bellmann et al. (2014); Bauernschuster (2009); Boockmann et al. (2008); Verick (2004). However, Verick (2004) and Priesack (2015) also do not find an effect of EPL on job flows or firm growth.

the effect of EPL on the job reallocation rate. In addition, I also analyze the number of temporary workers, and white-collar workers. From a theoretical perspective, I expect a negative treatment effect of EPL on the job reallocation rate, while the impact on the two other margins is expected to be positive.

	Tabi	c i. Link	noy month in	101811		
	Job re	eallo-	Log(# of the test of	of temp.	Log(# of v	white-collar
	cation	rate	worl	kers)	worl	xers)
Available time period	1994-	2012	1996	-2012	2003-	-2012
	(1)	(2)	(3)	(4)	(5)	(6)
All periods (γ)	-0.008		0.003		-0.045***	
	(0.006)		(0.009)		(0.017)	
${\bf Period}{\bf 1}(\gamma_1)$		0.014		0.019		-
(1994-1996)		(0.025)		(0.023)		-
${\bf Period}{\bf 2}(\gamma_2)$		-0.019		-0.032		-
(1997-1998)		(0.024)		(0.032)		-
${\bf Period}{\bf 3}(\gamma_3)$		-0.016		0.017^{*}		-0.025
(1999-2003)		(0.011)		(0.010)		(0.031)
Period 4 (γ_4)		-0.006		-0.005		-0.048***
(2004-2012)		(0.007)		(0.014)		(0.018)
R_{adj}^2	0.014	0.014	0.034	0.034	0.038	0.038
AIC	-6383.839	-6379.73	31585.483	31587.494	43013.475	43015.198
Ν	30496	30496	53210	53210	31094	31094

Table 4. Employment margin

Note: $^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$. The table shows the treatment effect of EPL on the respective outcome, estimated by OLS. Robust standard errors clustered at the establishment level are reported in brackets. N refers to the number of observations at the (year-specific) optimal bandwidths summed over all years. Year-specific functional forms of the centered running variable were chosen according to the AIC.

Columns 1 and 2 in Table 4 show the results of RDD regressions quantifying the local average treatment effect of EPL on the job reallocation rate across all years (γ) (column 1) and for each period separately (column 2). The result across all years is negative but statistically insignificant and close to zero. The period-specific treatments are all insignificant and negative, except in the first period where the coefficient is positive. All are relatively close to zero. Figure 3a shows the year-specific treatment results (γ_t) of EPL on the job reallocation rate for the years 1994-2012. It underlines the finding of a statistically insignificant effect of EPL on the job reallocation rate. The results stay statistically insignificant when varying the size of the bandwidth, using Donut regressions or including establishment covariates or industry dummies (see Tables 16 and 22 in the Appendix). All in all, there is no statistical evidence that EPL affects job reallocation.³⁸

Table 4 also shows the results of an explicit test for the option that establishments could try to circumvent the costs of EPL by increasing the number of temporary work-

³⁸I also run regressions with the number of hirings or firings as a dependent variable. The effects are negative but also statistically insignificant.

ers, for whom EPL does not apply. The effect across all years (γ) shown in column 3 is close to zero and statistically insignificant, same for the period effects. Using Donut regressions or including covariates or industry dummies in the regression does not substantially change the overall picture. Varying the size of the bandwidth slightly increases the magnitude of the effect (see Tables 16 and 22 in the Appendix). Overall, there is no statistically significant evidence that establishments use temporary employment as a means to circumvent EPL.³⁹

To test whether establishments increased the quality of hirings to reduce the expected dismissal costs, I analyze the effect of EPL on white-collar employment. The overall effect, γ , is negative, statistically significant, but very low in magnitude. Furthermore, Figure 3c does not point towards a statistically significant effect of EPL on the use of temporary workers. When including covariates or industry dummies, only the treatment effect for the last period stays statistically significant. The treatment across all years decreases further when using 75% of the optimal bandwidth (see Tables 16 and 22 in the Appendix).

In addition to these three outcomes, I also analyzed the effect of EPL on hirings and separations. I did not find a robust statistically significant effect of EPL on these outcomes. However, the drawback of the IAB establishment panel is that it only provides information about the number of hires and separations in the first half of the year.

All in all, these results confirm the finding of Bauer et al. (2007) who do not find a statistically significant effect of EPL on establishment level job flows. Following the argument of Autor et al. (2007), it would not be surprising if EPL had not effect on the on the main outcomes analyzed in this paper, capital investments and labor productivity.

6.4 The Effect of EPL on Capital Investments and Labor Productivity

Table 5 shows the main results of this paper: the effect of EPL on capital investments and labor productivity. Columns 1 to 4 show the results of EPL on the logarithm of the capital stock and of the total capital investments (flow measure), respectively. Columns 1 and 3 show the results of the overall treatment effect, γ , and columns 2 and 4 display period-specific treatment effects, γ_p . EPL seems to reduce capital stock by roughly 3.8% and the capital investment flows by 5.6%. Neither effect is statistically significant.

³⁹As briefly summarized in the Appendix, temporary contracts were deregulated and again reregulated at roughly the same points in time as EPL. It is only since 2002 that temporary contracts and temporary work agencies are more rigorously deregulated.



Figure 3: Year-specific effects of EPL on the employment margin

(c) Log(# of white-collar workers)

						-		•		
	Stock	measure				Flow	measure	8		
	Log(capital	Log(total capital Log		Log(ex	log(expansion Log		g(ICT Log(valı		ue-added)
	st	ock)	inves	tment)	invest	ment)	inves	tment)		
Time period	1994	4-2012	1994	-2012	1996	-2012	2000)-2006	1994	4-2012
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
All periods (γ)	-0.038		-0.056		0.019		0.276**		0.014	
	(0.030)		(0.092)		(0.102)		(0.135)		(0.025)	
Period 1 (γ_1)		0.056		0.196		-0.410				0.112
(1994-1996)		(0.096)		(0.302)		(0.344)				(0.077)
Period 2 (γ_2)		-0.293***		-0.520*		-0.621*				-0.124
(1997-1998)		(0.102)		(0.297)		(0.363)				(0.079)
Period 3 (γ_3)		-0.074		-0.204		0.022		0.077		-0.062
(1999-2003)		(0.050)		(0.133)		(0.165)		(0.161)		(0.044)
Period 4 (γ_4)		0.021		0.198		0.168		0.856***		0.074^{**}
(2004-2012)		(0.050)		(0.164)		(0.156)		(0.270)		(0.036)
R^2_{adj}	0.137	0.137	0.063	0.063	0.023	0.023	0.029	0.029	0.371	0.371
AIC	198002	197997	638334	638332	322661	322660	130341	130336	149720	149714
Ν	58502	58502	106325	106325	54066	54066	22929	22929	53624	53624

Table 5: Investments and labor productivity

Note: ***p < 0.01, **p < 0.05, *p < 0.1. The table shows the treatment effect of EPL on the respective outcome, estimated by OLS. Robust standard errors clustered at the establishment level are reported in brackets. N refers to the number of observations at the (year-specific) optimal bandwidths summed over all years. Year-specific functional forms of the centered running variable were chosen according to the AIC.

The period-specific treatment effects, γ_1 - γ_4 , do a statistically significant effect either. Figures 4a and 4b show the year-specific treatment estimates of EPL for the capital stock and the total capital investments, respectively.

Examining the sub-components of capital investments, namely expansion investments and ICT investments, the overall treatment effect, γ , for both outcomes is positive but statistically insignificant (Table 5). The magnitude of the effect of EPL on ICT investments statistically significant and huge, suggesting that EPL increases ICT investments by 27.6%. In contrast, the magnitude of the coefficient of expansion investments is rather small. The effect is statistically insignificant.

Figure 5a shows the year-specific treatment effects for the effect of EPL on expansion investments and Figure 5b shows its effect on ICT investments.

Figure 4: Year-specific effects of EPL on the capital stock and total capital investment flows



(b) Capital investment flows


Figure 5: Year-specific effects of EPL on expansion investments and ICT investments

(b) ICT investment



Figure 6: Year-specific effects of EPL on labor productivity

Finally, Table 5 shows the effect of EPL on the measure for labor productivity, the logarithm of total value added. The overall treatment effect, γ , is positive but statistically insignificant. Figure 6 shows the year-specific treatment effect of EPL on labor productivity.

6.4.1 Robustness Checks

To test the robustness of these results, Tables 17, 18, and 19 show how the treatment effect estimate changes when I vary the size of the bandwidth. Therefore, I re-estimate each model shown in Table 5, taking 75%-/50% and 25% of the year-specific optimal bandwidth of the assignment variable for each dependent variable. The general finding of this test is that the magnitude of almost all outcome measures decreases when the size of the bandwidth is lowered. The results can be found in Appendix 4F.

As an additional robustness test, I run Donut regressions by leaving out establishments that are one FTE below or above the size threshold. I also run RDD regressions including establishment covariates and industry dummies. The results do not change the overall picture that I just described. The results can be found in Table 14 in the Appendix.

6.4.2 Summary of Findings

All in all, these results do not provide evidence for a statistically significant effect of EPL on capital investments or labor productivity. A robust empirical effect would be found in every single year, which is not the case for any of the investment variables or for the

variable measuring labor productivity. One drawback of this analysis is undoubtedly the quality of the expansion and ICT investment variable, as well as the volatile nature of the flow measures for investments in general.

This finding is not in line with any of the published empirical studies that I introduced in chapter 2. However, even if insignificant and showing the reverse sign, the magnitude of the effect of EPL on the capital stock is somewhat comparable to the results of Cingano et al. (2016) and Autor et al. (2007) (in particular when varying the bandwidth), who find that EPL increases the capital stock by roughly 5% and 6.5%, respectively. However, I must mention that Autor et al. (2007) find a statistically significant effect only for one out of three policies that increase EPL in the US.

Only one study analyzes the impact of EPL on value added as a measure for labor productivity (Cingano et al., 2016). They do not find a statistically significant effect of EPL on value added either. However, in contrast to my finding, their estimate is negative, although it has a somewhat comparable magnitude (1.3% compared to 1.4% in this paper).

6.5 Industry-specific effects: The Effect of EPL on Capital Investments and Labor Productivity

EPL may affect certain industries differently. Consequently, conducting the same analysis for the effect of EPL on investment flows, capital stock and labor productivity from the previous section by industry may reveal some heterogeneous effects, since EPL is more binding in some industries than in others.

However, analyzing the disaggregated effect of EPL by industry does not change the conclusion of the previous section. While the effect of EPL across all years is statistically significant for some industries, this effect is not robust for the period- or year-specific treatment effects. The tables and figures of this exercise are provided in Appendix G.

Overall, the results of disaggregating the effect of EPL on investments and labor productivity supports the conclusion from the previous section: that EPL does not affect any of these outcomes.

6.6 Extension: The Effect of EPL on Wages

In this subsection, I conduct an implicit test of whether workers exploit the protection of EPL to "hold-up" their employers in order to extract rents from the surplus of the establishment-worker match. The aim is to determine whether establishments just above the size threshold, relative to those just below, pay higher wages on average. In addition to estimating the effect of EPL at the establishment level, I also assess its effect on wages at the individual-level using matched employer-employee data. Since I only include fulltime employees who have been employed in the same establishment for *at least* one year in my analysis, I assure that I have only "insiders" in my sample (i.e. EPL applies to them). This allows me to disentangle possible effects of EPL on entry-wages from those on insiders. It also allows me to control for individual worker characteristics.

As described in section 2, in an imperfectly competitive market, hold-up is one reason why firms do not invest in capital. In the same section, I also stated that finding evidence for higher wages does not automatically imply hold-up. Only if I found a negative effect on capital investments at the same time, this could be an *indication* for hold-up, but still no causal effect. Given the statistically insignificant impact on investments in the previous section, any evidence of EPL on wages found in this section cannot be related to hold-up.

Before discussing the results, I have to prove that a non-random selection of workers does not invalidate my analysis.⁴⁰ To test whether this kind of selection is a problem in my case, I run RDD regressions for each year separately to test whether worker characteristics (that is, education, age, tenure, and gender) in establishments above and below the size threshold differ substantially. The threshold estimates become statistically significant only in some cases, so I conclude that worker sorting is not a problem in my case (see Table 13 in the Appendix for more details).

Table 6 shows the results for the effect of EPL on wages. Column 1 shows the results of an RDD regression on gross wages at the establishment level. The treatment effect across all years is negative, statistically insignificant, and low in scale (0.6%). Considering the period-specific treatment effects, it becomes clear that the negative effect across all years is driven by the negative coefficient of the third period, which is statistically significant. The period-specific treatment effects in all other periods are positive but statistically insignificant. When re-estimating the model using the establishment-level data for larger bandwidths, running Donut regressions or including covariates does not substantially change the results (see Tables 15 and 20).

⁴⁰If, for example, low-productivity workers intentionally chose to work for establishments above the threshold because they value job security, I would not identify the causal effect of EPL on the workers' wage, but the effect resulting from a different composition of the workforce in establishments subject to EPL relative to those that are not (Leonardi and Pica, 2013).

			T 1' ' 1	11 1
	Establis	nment-level	Individi	ial-level
	$\log(gros$	$s \ wage_{jt})$	$\log(wag)$	e_{ijt})
Available time		1994-2	2009	
All periods (γ)	-0.006		-0.018	
	(0.018)		(0.046)	
${\bf Period}{\bf 1}(\gamma_1)$		0.127		0.001
(1994-1996)		(0.084)		(0.088)
${\bf Period}{\bf 2}(\gamma_2)$		0.037		-0.115
(1997-1998)		(0.049)		(0.107)
${\bf Period}{\bf 3}(\gamma_3)$		-0.079***		-0.154**
(1999-2003)		(0.027)		(0.072)
Period 4 (γ_4)		0.035		0.045
(2004-2009)		(0.027)		(0.068)
R^2_{adj}	0.611	0.611	0.015	0.016
AIC	54226	54219	31422	31410
# of stablishments	33267	33267	1912	1912
# of persons	-	-	20188	20188

Table 6: Establishment- and individual-level wage

Note: ***p < 0.01, **p < 0.05, *p < 0.1. The table shows the treatment effect of EPL on the respective outcome, estimated by OLS. Robust standard errors clustered at the establishment level are reported in brackets. N refers to the number of observations at the (year-specific) optimal bandwidths summed over all years. Year-specific functional forms of the centered running variable were chosen according to the AIC.

The effect of EPL on the individual wage is shown in column 3. The treatment effect across all years is also negative and statistically insignificant. As in the establishment-level model, the treatment effect of the third period is statistically significant and negative. The estimate for the second period is also negative, but insignificant, as the other two period effects.

Figures 7a and 7b show the year-specific treatment effects for the establishment and individual worker level wages, respectively. Especially in the beginning of the 2000s, the treatment estimates of the individual wage are huge.⁴¹ Both Figures confirm the statistically insignificant effect of EPL on wages.

In section 5.2, I mentioned that the individual level wages in the LIAB are censored and that I imputed the wages. However, since only 6.6% of the wages in my sample are censored, this should not have a large impact on my results. When running regressions using imputed values for the wages that were above the upper contribution limit of the social security system, the treatment effect across all years becomes statistically significant (5%-level), suggesting a 10.4% decrease in the wage in response to EPL (see Table 23 in the Appendix). However, of the period-specific treatment effects, only that

⁴¹This could be the result of over-fitting of the model the model.

for the third period is statistically significant. Also the year-specific treatment effects do not show a throughout statistically significant effect of EPL on individual wages. The robustness checks for the non-imputed and the imputed individual wages do not change the overall conclusion (see Tables 21 in the Appendix).

All in all, I neither find a statistically significant effect of EPL on wages at the establishment level, nor on the insiders' wages at the individual worker level. From a theoretical perspective, the nonexistence of an effect of EPL on wages suggests that hold-up is not a problem in this paper. This finding is further confirmed by the finding of a statistically insignificant effect of EPL on investments found in the previous section.

Finally, I want to state that the majority of the scarce empirical literature that analyzes the effect of EPL on wages and labor costs finds evidence for a negative effect. Only one study finds a positive effect, and one no statistically significant effect at all (for more information, see Cingano et al., 2016).





(b) Individual-level wage

7 Discussion

In summary, I do not find a statistically significant effect of EPL on any of the outcomes – capital investments, labor productivity, and employment margin. This result is consistent with the reasoning of Autor et al. (2007): if EPL does not affect the employment margin, it has no effect on other margins of non-labor adjustment such as capital investments and labor productivity. In addition, I do not find empirical evidence for hold-up either, which is, from a theoretical perspective, also consistent with the zero effect on capital investments and labor productivity.

Changes of the size threshold over time and adaptation of expectations

One could argue that I do not find a statistically significant effect on investments or labor productivity because repeated changes of the threshold over time have made establishments reluctant to adjust investments or other variables affecting labor productivity – especially because investment decisions take time and are not easily changed from one day to the other. One argument against this doubt is that the effect of EPL on capital investments is also not statistically significant before the first change in the threshold in the first period. This is slightly different in case of the labor productivity, where the treatment effect in the first and the last periods is statistically significant. The fact that the first period treatment dummy stays statistically significant when decreasing the bandwidth further supports this argument, while the results of a Donut regression and a regression using industry dummies go against it, as the first period treatment effect becomes statistically insignificant in these specifications. A second argument is that I analyzed the same outcomes in an earlier version of this paper, where I exploited the change in the legislation by means of a difference-in-discontinuities design and did not find a statistically effect on any of these outcomes in the first period, but also not in the other periods (which is again less likely, because of an adaptation of expectations).⁴²

⁴²A weaker but still valid argument against these doubts is that if establishments became reluctant over time to adjust their strategies to the changes in the threshold, they should have adjusted their beliefs about the likelihood of a fourth change of the size threshold after the election of the coalition between the center-right and center-left party in 2005. As argued in chapter 3, a repeated downward change of the size threshold was highly unlikely under this pro-business government with Angela Merkel as chancellor. Thus, the establishments should have adjusted their behavior accordingly by the end of the observation period. Even if the statistically significant treatment effects in the last period for total capital investment flows and expansion investments represent such an adjustment of expectations, these treatment effects are not robust when varying the bandwidth, and there is no such effect on the capital stock.

Threshold effects of works councils applying at the same size thresholds

In section 3.2, I mentioned that the right to elect a work council applies at the same size threshold as EPL in the first and third period. Since the effect of a work council affects all outcomes analyzed here in the same direction as EPL, it would at most bias the results upwards. Since I do not find an effect on any of the outcomes, this should not be a problem. Moreover, only about 6% of the establishments below the size of 30 FTEs have a work council in the data sample that I use, which is in line with the results of Addison et al. (2001). Using the same data (IAB establishment panel), the authors find that only 4.7% of the establishments with between 5 and 20 workers have a work council.

It should be mentioned that Addison et al. (2007) analyze the effect of work councils on investments in Germany, using the same establishment panel as I do. They do not find a statistically significant effect of work councils on investments. In contrast, Addison et al. (2001) find a statistically significant effect of work councils on wages and other outcomes. However, since only 4.7% of the establishments with 5-20 workers in their sample have a work council, some of the effects they find are only valid for larger establishments are not statistically significant for smaller establishments (with 5-20 workers).

8 Conclusion

In this paper, I analyzed the effect of EPL on capital investments and labor productivity by means of an RDD. I the fist part of the paper I looked at the effect of EPL on the employment margin. I did not find evidence that EPL increases job reallocation, the amount of temporary or white-collar workers. In line with the predictions of the literature (e.g. Lazear (1990); Autor et al. (2007))– that states if EPL has no impact on the employment margin, also other margins of non-labor adjustment should not be affected– I did not find statistically significant evidence for an impact of EPL on capital investments or labor productivity– also not when analyzing the effect of EPL by industry. In an extension of the paper, I conducted an implicit test for hold-up by investigating whether establishments above the size threshold for which EPL applies pay higher wages. Consistent with EPL not having an effect on capital investments, I found no evidence for higher wages.

A direct consequence of these findings from a theoretical stance, is that employers shift the costs incurred by EPL onto the workers via lower entry wages. Since I only focused on incumbent workers, this I did not analyze this channel. However, future research should definitely examine this margin. As mentioned earlier, Leonardi and Pica (2013) analyze this channel for Italy and find statistically significant evidence that firms transfer some of the costs of EPL onto workers via lower entry wages.

Further limitations of this paper include the following. I did not control for financial market imperfections and the role of debt-financing. Cingano et al. (2016) find that, among firms with a relatively low capital stock, capital deepening is more pronounced if a firm is not credit constrained. Moreover, I did not control for whether EPL leads to a negative selection of the workforce, which would decrease labor productivity.

Another possible explanation for why I did not find a statistically significant effect of EPL on any of the analyzed margins is that I miss-specified the establishment size threshold because of measurement error in the variables or due to missing information on the exact working time of every single part-time worker or the number of employees in establishments throughout the entire year.

Alternatively, it could also be that the threshold exempting smaller establishments from EPL is simply not relevant for the establishments of that size. Schramm and Endemann (2010) who conducted a survey among human resource managers argue that according to the subjective judgments of these managers, EPL in Germany may in general have a relatively low consequences for the employment strategies of firms (hence, establishments), also because firms have a preference for stable employment relations anyways. According to their results, firms judged factors such as the overall business environment or availability of qualified workers as much more important.

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A Background Information: Labor Market Reforms in Germany in the Time between 1994 and 2010

In the 1990s and early 2000s, Germany was called the "sick man of Europe" (Dustmann et al., 2014). Economic growth was low (1.4% p.a., 1994-2005) and unemployment high (10.6% in 1994 and 13% in 2005)(Office, 2016, 2017). To make the labor market more flexible, the German center-right government under chancellor Kohl (Christian Democrats) lowered the replacement rates for unemployment in 1994, and increased the firm size threshold on October 1st, 1996. In 1997, the government extended the maximum duration of temporary contracts and agency work assignments and removed the qualification safeguard clause in the unemployment insurance (Eichhorst and Marx, 2011).

After the Red-Green coalition (of Social Democrats and the Green Party) was elected in 1998, it not only lowered the firm size threshold of EPL to its former level, but it also restricted the use of temporary contracts to initial hiring only. The economic downturn in 2001 later led to a paradigm shift of this coalition. After it was re-elected in 2002, it implemented the so-called "Hartz reforms" in three steps between January 2003 and January 2005. Among other things, the reforms deregulated the temporary work sector (fixed-term contracts and temporary agency work), introduced marginal part-time jobs (Minijobs), shortened the duration of unemployment benefits, and merged the unemployment assistance and social assistance, thereby lowering unemployment benefits (Eichhorst and Marx, 2011; Engbom et al., 2015).

In 2005, however, the new coalition of Christian Democrats and Social Democrats focused more on equity and social justice. For example, it extended unemployment benefits for older workers, implemented measures to help the long-term unemployed out of unemployment, and started first discussions about the introduction of a minimum wage, especially since the scope of low-paid work raised doubts about unfair wages (Eichhorst and Marx, 2011).

B Complementary Tables for Sections 2-5

	Before Oct. 1996	Between Oc	t.96 & Dec.98	Between Ja	an.99 & Dec.03	Aft	er Jan 04
Thershold ($\#$ of FTEs)	5.25	1	0.25		5.25	5.25 (old)/ 10.25 (new)
Weighting scheme	PADA	PADA	Appl.	PADA	Appl.	PADA	Appl.
Working hours/ week	<15h -0.0	<10h -0.25	$<\!15h-\!0.30$				
& weights (% of a FTE)	>15h-1.0	$<\!\!20h-0.5$	$<\!\!25h\!-0.6$	$<\!20h-\!0.5$	$<\!\!25h\!-\!0.6$	$<\!\!20h\!-\!\!0.5$	$<\!\!25h\!-\!0.6$
	(Appl. weight=	${\leq}30\mathrm{h}{-}0.75$	${\leq}30\mathrm{h}{-}0.8$	\leq 30h–0.75	${\leq}30\mathrm{h}{-}0.75$	\leq 30h–0.75	${\leq}30\mathrm{h}{-}0.75$
	PADA)	>30h-1	>30h-1	>30h-1	>30h-1	>30h-1	>30h-1

Table 7: Weighting scheme for part-time workers

Note: The table shows the weighting scheme for part-time workers according to the rules of the PADA and the applied weighting scheme in the empirical analysis (Appl.). The weighting scheme of the PADA and that in the IAB data do not coincide. Instead, weighting scheme of the data was adjusted to best represent that of the PADA.

Table 8: Upper contribution limits to the social security system

I	Vest	l	East
Time period	Upper earning limit	Time period	Upper earning limit
	(Euros per year)		(Euros per year)
1.1 31.12.1994	46'630	1.1 31.12.1994	36'199
1.1 31.12.1995	47'857	1.1 31.12.1995	39'267
1.1 31.12.1996	49'084	1.1 31.12.1996	41'721
1.1 31.12.1997	50'311	1.1 31.12.1997	43'562
1.1 31.12.1998	51'538	1.1 31.12.1998	42'949
1.1 31.12.1999	52'152	1.4 31.12.1999	44'176
1.1 31.12.2000	52'765	1.1 31.12.2000	43'562
1.1 31.12.2001	53'379	1.1 31.12.2001	44'789
1.1 31.12.2002	54'000	1.1 31.12.2002	45'000
1.1 31.3.2003	61'200	1.1 31.3.2003	51'000
1.4 31.12.2003	61'200	1.4 31.12.2003	51'000
1.1 31.12.2004	61'800	1.1 31.12.2004	52'200
1.1 31.12.2005	62'400	1.1 31.12.2005	52'800
1.1 31.12.2006	63'000	1.1 31.12.2006	52'800
1.1 31.12.2007	63'000	1.1 31.12.2007	54'600
1.1 31.12.2008	63'600	1.1 31.12.2008	54'000
1.1 31.12.2009	64'800	1.1 31.12.2009	54'600
1.1 31.12.2010	66'000	1.1 31.12.2010	55'800
1.1 31.12.2011	66'000	1.1 31.12.2011	57'600
1.1 31.12.2012	67,200	1.1 31.12.2012	57'600

Source: (IAB, 2017).

C Summary statistics and Graphs Descriptive Evidence for Treatment Effect

The following figures provide visual tests for a discontinuity at the threshold by showing averages of the dependent variables for different bins around the threshold, where bins are defined by one FTE employee. The figures display the mean of the dependent variable as well as the standard error of the mean. Additionally, the figures display a quadratic approximation on each side of the threshold.

Table 9: Descriptive statistics by treatment and period















D Continuity of covariates

One way to analyze the validity of the RDD is to show that establishments just above and below the size threshold do not differ substantially in terms of observable characteristics. A continuous relation of establishment characteristics just below and above the size threshold would imply that they are as good as randomly assigned around the threshold (Lee and Lemieux, 2010).

I conduct this test by estimating RDD models for each year between 1994 and 2012, using different establishment characteristics as outcomes. These include the industry sector of the establishment and the following other establishment characteristics: whether a company had a works council (*works council*), whether its business form is that of a private limited company (*busin. LLC.*), whether it was subject to a collective agreement (*collect. agr.*), whether it payed a wage above the collectively agreed one (*wage cushion*), and the share of females (*share fem.*).

	Finan. Social &	&insur. pers.	busin. serv. serv.	0.929^{**} -0.326	(0.365) (0.342)	0.013 0.011	1284 1284	0.478* -0.679***	(0.277) (0.257)	0.010 0.026	2828 2828	0.173 -0.366	(0.287) (0.284)	0.007 0.021	3360 3360	-0.183 1.295^{***}	(0.340) (0.374)	0.004 0.026	3906 3906	-0.192 0.392	(0.339) (0.347)	0.005 0.019	4174 4174	0.331^{*} -0.252	(0.200) (0.197)	0.004 0.022	5505 5505	0.280 -0.380*	(0.198) (0.198)	0.004 0.017	5976 5976	* 0.187 -0.060	(0.184) (0.175)	0.002 0.017	0718 07180
	Trans	1- Åz	comm.	*0.633	(0.658)	0.018	1284	-0.104	(0.455)	0.005	2828	0.102	(0.521)	0.006	3360	0.387	(0.554)	0.007	3906	0.471	(0.530)	0.002	4174	-0.228	(0.361)	0.001	5505	-0.127	(0.373)	0.001	5976	-0.924^{**}	(0.402)	0.004	G160
	Constr. Trade	& restau	rant	0.316 -0.742**	(0.409) (0.277)	0.014 0.007	1284 1284	0.002 -0.081	(0.275) (0.194)	0.038 0.005	2828 2828	-0.238 0.280	(0.271) (0.206)	0.033 0.004	3360 3360	-0.339 -0.186	(0.261) (0.257)	0.031 0.008	3906 3906	0.316 -0.288	(0.257) (0.250)	0.025 0.009	4174 4174	-0.015 0.076	(0.184) (0.152)	0.017 0.002	5505 5505	$-0.008 0.317^{**}$	(0.187) (0.155)	0.013 0.003	5976 5976	-0.225 0.249	(0.187) (0.153)	0.012 0.001	6160 6160
try I	sp. Recycl.			1 -7.961	2)(11.001)	0.088	1284	7 -0.612	(1.098)	0.010	2828	6 0.932	5)(1.108)	0.026	3360	-1.057	(1.957)	0.010	3906	4 1.346	9) (1.682)	0.025	4174	9 -0.290	(0.873)	0.001	5505	0.646	(0.844)	0.006	5976	8 0.571	(0.867)	0.008	6160
by indus	ctric. Trans	equip	cal	8 -0.64	(0.78) (0.78)	0.013	4 1284	9 -0.16	91) (0.61)	0.007	8 2828	1 -0.10	(0.64)	5 0.003	3360	15 0.671	(0.63) (0.63)	0.003	5 3906	$34 -0.05^{4}$	98) (0.49)	9 0.002	4 4174	8 -0.279	(0.72)	8 0.019	5 5505	$^{7}4$ 0.120	37) (0.78	8 0.009	5976	1 -0.878	57) (0.99)	900 [.] 006	6160
riates: b	ach Elec	e.c. &	opti	$.460^{**}0.45$.783) (0.7	102 0.00	84 128	534 0.01	.975) (0.4	0.00 0.00	28 2828	070 0.44	(895) (0.4	0.01 0.01	60 336(047 0.64	(0.5) (0.5)	0.02 0.02	06 3900	.277 -0.4	.823) (0.3	0.00	74 417	500 0.36	.465) (0.3)	0.00	05 550	.165 0.07	.474) (0.3	0.00 0.00	76 597(696 0.06	(555) (0.3)	0.00	60 61 <i>6</i> 1
r of cova	abric. M	Metal n.	x mach.	.508 -6	1.191) (2)	0.002 0.	284 12	0.202 0.0	0.647) (0	0.025 0.0	2828 28	.863*** 0.0	0.721) (0	0.038 0.0	360 33	1.297* -1	0.679) (0	0.029 0.0	3906 39	.123 -0	0.509) (0	0.025 0.0	174 41	0.356 -0	0.365) (0	0.024 0.0	505 55	0.263 -0	0.377) (0	0.028 0.0	976 59	0.337 -0	0.391) (0	0.021 0.0	160 G1
0: Continuity	Other Basic I	non- metals 1	$metal \in$	0.710 0.782 ((1.275)(0.908) (0.028 0.007 0	1284 1284 1	1.530 -1.675**-	(0.942)(0.699) (0.013 0.025 0	2828 2828 2	-0.646 -0.380 1	(1.054)(0.690) (0.020 0.022 (3360 3360 5	-0.319 -0.239 -	(1.457)(0.708) (0.016 0.026 (3906 3906 3	1.667 -0.180 ((1.088)(0.585) (0.020 0.020 (4174 4174 4	-0.345 0.403 -	(0.631)(0.498) (0.007 0.016 (5505 5505 5	-0.011 -0.266 -	(0.677)(0.573) (0.012 0.014 (5976 5976 5	1.051* 0.573 -	(0.607)(0.585) (0.016 0.017 (6160 6160 6
Table 1	er Chem Rubber	k_c k_c	fuel plastics	- 6	74)		1	10 - 0.324	30) - (1.368)	0^{-} 0.035	- 2828	8 1.641 -3.370	30) (2.567) (2.739)	3 0.050 0.041	3360 3360	55 - 1.092 1.090	(0.739)(1.294)	7 0.034 0.051	4174 3906	7 -1.092 -1.415	34) (0.739) (1.087)	0 0.034 0.053	4174 4174	3 0.742 -2.507***	(20, 0.579)(0.944)	5 0.019 0.029	5505 5505	0^{**} - 0.189 - 1.328	35) (0.715) (0.893)	0 0.012 0.030	5976 5976	1 0.758 0.033	(0.630) (0.675) (0.675) (0.675)	8 0.033 0.039	6160 6160
	ood Pape			.856 0.959	.916) (0.97	015 0.01	84 1284	.153 -0.04	.573) (0.70	002 0.010	28 2828	.506 - 0.25	(0.98) (0.98)	005 0.01	60 3360	.334**-1.35	.657) (1.18	00.0 200	06 3906	260 1.21	(0.98) (0.98)	0.020	74 4174	215 0.23	(385) (0.57)	0.01 0.01	05 5505	.329 1.310	(435) (0.50	0.020	176 5976	.194 0.84	.448) (0.54	0.00 0.003	60 6160
	Textiles W			-0.353 -0	(1.170) (0)	0.063 0.4	1284 12	0.762 -0	(0.832) $(0$	0.012 0.0	2828 28	0.634 -0	(0.956) $(0$	0.002 0.4	3360 33	0.462 -1	(1.196) (0)	0.012 0.4	3906 35	2.221^{**} 0.	(1.127) (0)	0.033 0.4	4174 41	-0.317 0.	(0.766) $(0$	0.007 0.4	5505 55	-0.275 -0	(0.801) $(0$	0.002 0.4	5976 55	0.294 -0	(0.826) $(0$	0.011 0.	6160 G1
	Food,	bev.		0.074	(0.645)	0.011	1284	-0.410	(0.451)	0.024	2828	-0.807	(0.556)	l_{l_0} 0.029	3360	2.114^{***}	(0.541)	l_{lo} 0.048	3906	-0.381	(0.499)	l_{lo} 0.034	4174	-0.577	(0.401)	l_{o} 0.017	5505	-0.192	(0.381)	$l_{o} 0.015$	5976	-0.250	(0.331)	$l_{o} 0.021$	6160
	Year			1994 Treat		R^2_{pseud}	Z	1995 Treat		R^2_{nsend}	Z	1996 Treat		R^2_{pseud}	Z	1997 Treat		R^2_{pseud}	Z	1998 Treat		R^2_{pseud}	Z	1999 Treat		R^2_{pseud}	Z	2000 Treat		R^2_{pseud}	Z	2001 Treat		R^2_{pseud}	Z

						TONTO		INTITATI	n n n	VALIAUT	vu	nenniii	1 TT					
Year	Food,	Textile	s Wood	Paper	Chem	Rubbei	r Other	Basic	Fabric.	Mach	Electric	. Transp	. Recycl.	. Constr.	Trade	Trans Fi	nan.	Social $\&$
	bev.				&	&	non-	metals	Metal	n.e.c.	&r	equip.			& restau-	- &	nsur.	pers.
					fuel	plastics	s metal		ex mach		optical				rant	comm. bu	sin. serv.	serv.
2002 Treat	0.682^{*}	*0.355	0.779^{*}	0.987^{*}	0.504	-0.769	0.954^{*}	1.158^{*}	-0.056	-0.293	-0.991^{**}	• -0.991	-0.117	0.147	0.131	-0.262 -0.	093	-0.299*
-	(0.315)	(0.824)	(0.410)	(0.510)	(0.685)	(0.788)	(0.576)	(0.592)	(0.393)	(0.523)	(0.392)	(1.043)	(1.025)	(0.185)	(0.149)	(0.314)(0.	173)	(0.168)
R^2_{pseudc}	0.026	0.010	0.004	0.013	0.016	0.022	0.009	0.019	0.016	0.027	0.009	0.007	0.011	0.004	0.003	0.004 0.0)04	0.012
N	6771	6771	6771	6771	6771	6771	6771	6771	6771	6771	6771	6771	6771	6771	6771	6771 67	71	6771
2003 Treat	-0.013	-0.175	0.658	0.685	-0.562	0.366	1.178^{**}	*0.276	-0.008	-0.990*	0.121	-2.177	-3.148*	-0.032	-0.158	-0.423 0.0)13	0.316^{*}
	(0.346)	(0.937)	(0.457)	(0.559)	(0.704)	(0.777)	(0.589)	(0.595)	(0.394)	(0.567)	(0.356)	(1.464)	(1.646)	(0.186)	(0.153)	(0.333)(0.	177)	(0.171)
R^2_{pseudc}	0.019	0.007	0.006	0.006	0.047	0.010	0.006	0.013	0.013	0.035	0.008	0.015	0.026	0.003	0.004	0.003 0.(002	0.010
Ν	6639	6639	6639	6639	6639	6639	6639	6639	6639	6639	6639	6639	6639	6639	6639	6639 66	39	6639
2004 Treat	0.630	0.521	-0.154	-0.098	0.661	0.333	1.077	0.500	-0.325	0.793	0.626	-0.041	1.073	0.045	0.187	-0.224 -0.	635^{***}	0.271
	(0.402)	(1.122)	(0.528)	0.660)	(0.658)	(0.852)	(0.669)	(0.675)	(0.436)	(0.504)	(0.404)	(1.130)	(1.205)	(0.217)	(0.182)	(0.379)(0.379)	(210)	(0.226)
R^2_{pseudc}	0.019	0.013	0.001	0.009	0.030	0.019	0.004	0.017	0.019	0.034	0.011	0.008	0.018	0.005	0.003	0.002 0.(900	0.008
Z	7292	7292	7292	7292	7292	7292	7292	7292	7292	7292	7292	7292	7292	7292	7292	7292 72	92	7292
2005 Treat	0.459	0.069	-0.191	0.622	0.477	-0.572	0.891	0.123	0.843^{*}	-0.946*	0.708	-1.192	0.399	-0.311	0.234	-0.240 -0.	274	-0.139
	(0.432)	(1.040)	(0.559)	(0.710)	(0.681)	(0.920)	(0.752)	(0.744)	(0.451)	(0.551)	(0.444)	(1.375)	(1.350)	(0.236)	(0.196)	(0.421)(0.	230)	(0.244)
R^2_{pseudc}	0.013	0.003	0.002	0.011	0.022	0.010	0.002	0.013	0.030	0.029	0.003	0.003	0.021	0.003	0.002	0.006 0.(01	0.012
Z	5773	5773	5773	5773	5773	5773	5773	5773	5773	5773	5773	5773	5773	5773	5773	5773 57	73	5773
2006 Treat	-0.209	-0.604	0.037	-0.694	1.649^{**}	*-1.032	-0.253	1.214	0.270	0.452	0.923^{**}	0.423	-0.191	-0.231	0.198	-0.076 -0.	192	-0.239
	(0.398)	(0.918)	(0.551)	(0.744)	(0.751)	(0.960)	(0.767)	(0.807)	(0.449)	(0.543)	(0.443)	(1.098)	(1.463)	(0.229)	(0.193)	(0.399)(0.	226)	(0.252)
R^2_{pseudc}	0.012	0.003	0.002	0.012	0.044	0.015	0.011	0.016	0.015	0.031	0.005	0.007	0.000	0.004	0.003	0.002 0.()02	0.014
Z	5897	5897	5897	5897	5897	5897	5897	5897	5897	5897	5897	5897	5897	5897	5897	5897 58	97	5897
2007 Treat	-0.582	0.389	-0.801	1.096	1.250	-0.237	0.206	0.691	0.406	1.094^{*}	0.345	0.627	3.429	-0.479*	-0.024	-0.127 -0.	180	0.531^{**}
	(0.448)	(0.890)	(0.611)	(0.847)	(0.774)	(1.054)	(0.732)	(0.833)	(0.495)	(0.628)	(0.457)	(1.249)	(3.086)	(0.251)	(0.202)	(0.447)(0.	253)	(0.257)
R^2_{pseudc}	0.002	0.003	0.003	0.009	0.034	0.057	0.009	0.021	0.014	0.020	0.003	0.006	0.043	0.004	0.003	0.004 0.0	00(0.010
Z	4588	4588	4588	4588	4588	4588	4588	4588	4588	4588	4588	4588	4588	4588	4588	4588 45	88	4588
2008 Treat	0.557	0.131	I	-0.573	-0.085	0.224	0.211	-0.050	-0.087	0.203	-0.271	I	I	-0.564^{**}	0.119	-0.578*-0.	101	0.564^{**}
	(0.450)	(0.870)	I	(0.572)	(0.786)	(0.986)	(0.721)	(0.811)	(0.420)	(0.607)	(0.619)	I	I	(0.243)	(0.202)	(0.345)(0.	(280)	(0.246)
R^2_{pseudc}	0.009	0.001	I	0.002	0.051	0.023	0.002	0.026	0.016	0.028	0.023	I	I	0.004	0.002	0.004 0.0	005	0.009
Z	5231	5231	I	5231	5231	5231	5231	5231	5231	5231	5231	I	I	5231	5231	5231 52	31	5231
2009 Treat	0.236	0.919	I	0.981	0.958	-0.494	-0.372	0.556	0.340	-0.232	-0.022	I	I	-0.054	0.016	-0.249 0.1	42	-0.049
	(0.510)	(0.985)	ı	(0.645)	(0.863)	(1.329)	(0.827)	(0.817)	(0.469)	(0.700)	(0.776)	I	I	(0.283)	(0.233)	(0.403)(0.	(331)	(0.276)
$ R^2_{pseudc}$	0.007	0.004	ı	0.004	0.041	0.007	0.002	0.011	0.014	0.033	0.022	I	I	0.003	0.002	0.005 0.(01	0.007
Ż	3512	3512	ı	3512	3512	3512	3512	3512	3512	3512	3512	ı	ı	3512	3512	3512 35	12	3512

Table 11: Continuity of covariates: by industry II

Year		Work	Busin.	Collect.	Wage	Share	Year		Work	Busin.	Collect.	Wage	Share
		council	LLC.	agr.	cushion	fem.			council	LLC.	agr.	cushion	fem.
1994	Treat	0.404	-0.349	-	-	-8.380*	2002	Treat	-0.080	-0.189	-0.149	-0.083	-1.018
		(2.047)	(0.716)	-	-	(4.559)			(0.367)	(0.133)	(0.155)	(0.203)	(2.395)
	R^2_{adj}			-	-	0.038		R^2_{adi}					0.020
	R^2_{pseudo}	0.182	0.289	-	-			R^2_{pseudo}	0.079	0.094	0.040	0.028	
	N	185	247	-	-	995		N	4665	6464	4659	2925	4673
1995	Treat	-0.881	0.254	-0.500*	-0.559*	-4.551	2003	Treat	0.148	-0.102	-0.161	-0.048	3.261
		(1.689)	(0.202)	(0.300)	(0.292)	(4.552)			(0.353)	(0.136)	(0.139)	(0.175)	(2.143)
	R^2_{adj}					0.025		R^2_{adj}					0.017
	R^2_{pseudo}	0.151	0.115	0.062	0.070			R^2_{pseudo}	0.077	0.100	0.030	0.050	
	Ν	217	2745	1073	1057	1068		Ν	5286	6290	5649	5644	5655
1996	Treat	0.784	-0.196	0.100	-0.135	1.457	2004	Treat	0.295	0.206	0.428^{**}	0.365	4.603^{*}
		(0.526)	(0.410)	(0.220)	(0.255)	(3.485)			(0.338)	(0.162)	(0.171)	(0.235)	(2.424)
	R^2_{adj}					0.038		R^2_{adj}					0.020
	R^2_{pseudo}	0.061	0.107	0.026	0.038			R^2_{pseudo}	0.097	0.127	0.032	0.025	
	Ν	2076	1199	2087	2067	2049		Ν	5915	6917	5916	3681	5928
1997	Treat	-0.316	-0.246	0.430^{*}	0.367	8.775**	2005	Treat	0.194	0.280	0.222	0.137	1.796
		(1.432)	(0.226)	(0.255)	(0.284)	(3.825)			(0.353)	(0.175)	(0.184)	(0.236)	(2.657)
	R_{adj}^2					0.039		R_{adj}^2					0.024
	R_{pseudo}^2	0.064	0.130	0.049	0.059			R_{pseudo}^2	0.085	0.098	0.017	0.020	
	Ν	1010	3650	3075	3057	3094		Ν	4866	5505	4876	4862	4881
1998	Treat	0.215	0.058	0.409*	0.113	0.135	2006	Treat	-0.224	0.221	0.102	0.533**	-0.354
	-2	(0.472)	(0.211)	(0.242)	(0.290)	(3.700)			(0.378)	(0.171)	(0.188)	(0.256)	(2.753)
	R_{adj}^2					0.035		R_{adj}^2			0.010		0.023
	R_{pseudo}^2	0.093	0.135	0.053	0.054			R_{pseudo}^2	0.088	0.099	0.012	0.015	
	N	3033	3953	3057	3057	3065		N	4786	5747	4787	4754	4797
1999	Treat	0.231	-0.007	-0.218	-0.161	0.203	2007	Treat	0.430	0.152	-0.133	-0.041	0.713
	D 2	(0.423)	(0.142)	(0.177)	(0.246)	(2.747)		D2	(0.392)	(0.182)	(0.195)	(0.252)	(2.863)
	R_{adj}^2		0.400			0.034		R_{adj}^2					0.016
	R_{pseudo}^2	0.064	0.103	0.041	0.026			R_{pseudo}^2	0.051	0.066	0.006	0.005	
	N	3079	5313	3127	1901	3129		N	3940	4465	3941	3931	3950
2000	Treat	0.135	-0.013	-0.059	-0.203	1.380	2008	Treat	-0.354	0.155	0.320^{*}	-0.012	6.921**
	D ²	(0.360)	(0.144)	(0.158)	(0.205)	(2.405)		D2	(0.399)	(0.180)	(0.192)	(0.257)	(2.842)
	R_{adj}^2	0.050	0.100	0.004	0.000	0.021		R_{adj}^2	0.070	0.000	0.000	0.000	0.013
	R_{pseudo}^2	0.053	0.108	0.034	0.020	1.100		R_{pseudo}^2	0.078	0.082	0.008	0.008	1007
0001	N TT (4183	5005	4390	2730	4408	2000	IN TT (4381	5082	4374	4355	4387
2001	Ireat	-0.052	-0.278***	(0.155)	0.266	4.607*	2009	Ireat	-0.400	0.220	-0.110	0.569	0.224
	D^2	(0.366)	(0.139)	(0.155)	(0.199)	(2.423)		D2	(0.411)	(0.207)	(0.204)	(0.284)	(2.992)
	Γ_{adj}^{2}	0.045	0.100	0.000	0.020	0.019		Γ_{adj}^{-}	0.002	0.050	0.002	0.002	0.018
	R ² _{pseudo}	0.045	0.100	0.028	0.030	4710		κ_{pseudo}	0.063	0.059	0.003	0.003	9507
	IN	4405	<u> </u>	4098	2969	4/12		IN	3500	3398	3504	3483	3507

Table 12: Continuity of covariates: by other establishment characteristics

	2009	0.190	(0.232)	0.001	3404	1165	2009	1.916	(2.043)	0.008	11024	1452	2009	-1283.0	(783.9)	0.010	26972	1452	2009	-0.183	(0.113)	0.009	1939	1452
	2008	-0.335	(0.237)	0.007	3615	1285	2008	-0.863	(1.654)	0.000	12249	1622	2008	-341.0	(654.5)	0.006	29947	1622	2008	0.099	(0.104)	0.008	2179	1622
	2007	-0.131	(0.241)	0.001	4746	1631	2007	-0.394	(1.681)	0.013	15348	2038	2007	692.8	(558.3)	0.010	37497	2038	2007	0.144	(0.100)	0.007	2809	2038
	2006	-0.185	(0.240)	0.001	3786	1309	2006	0.609	(2.185)	0.002	12412	1641	2006	216.2	(591.8)	0.003	30125	1641	2006	0.105	(0.112)	0.003	2260	1641
	2005	-0.376	(0.270)	0.002	3839	1325	2005	2.364	(1.929)	0.003	11983	1594	2005	628.0	(678.6)	0.002	29237	1594	2005	0.053	(0.127)	0.001	2223	1594
lata	2004	-0.083	(0.234)	-0.002	4703	1557	2004	0.141	(1.971)	0.001	14060	1861	2004	1236.5^{*}	(649.3)	0.016	33998	1861	2004	-0.132	(0.111)	0.010	2591	1861
vidual d	2003	-0.098	(0.218)	0.000	3271	1237	2003	1.104	(1.963)	-0.002	11083	1470	2003	38.5	(469.2)	-0.002	26649	1470	2003	0.061	(0.116)	0.003	2093	1470
es: indiv	2002	0.330	(0.242)	0.001	2783	1088	2002	-0.764	(1.937)	0.003	9793	1299	2002	-55.5	(533.8)	-0.000	23402	1299	2002	0.417^{***}	(0.132)	0.043	1783	1299
covariat	2001	-0.123	(0.172)	-0.000	4526	1680	2001	-0.836	(1.828)	-0.001	14895	1971	2001	193.8	(394.1)	-0.001	35255	1971	2001	0.135	(0.101)	0.015	2769	1971
uity of	2000	-0.076	(0.153)	0.001	4649	1699	2000	1.933	(1.635)	-0.000	15171	2006	2000	90.1	(301.5)	0.001	35532	2006	2000	0.085	(0.079)	0.005	2836	2006
Contin	1999	0.098	(0.157)	0.001	2343	904	1999	-1.847	(2.308)	0.009	7800	1043	1999	420.0	(496.6)	0.004	18250	1043	1999	0.398^{***}	(0.125)	0.018	1455	1043
Lable 13:	1998	0.177	(0.351)	-0.004	1675	639	1998	-2.250	(3.101)	-0.001	5289	711	1998	2036.9^{**}	(840.8)	0.057	12379	711	1998	0.420^{**}	(0.178)	0.041	915	711
	1997	0.696	(1.023)	0.029	889	329	1997	-8.542	(8.122)	0.007	2789	374	1997	-2318.9	(1465.0)	0.033	6494	374	1997	-0.084	(0.279)	0.049	452	374
	1996	0.453	(0.332)	0.013	1827	651	1996	-0.069	(2.743)	0.002	5622	755	1996	-308.0	(502.7)	0.010	13185	755	1996	0.051	(0.147)	0.007	1083	755
	1995	-0.178	(0.365)	0.019	330	156	1995	-4.420	(5.310)	0.011	1313	176	1995	-440.2	(913.1)	0.019	3205	176	1995	0.728^{***}	(0.238)	0.065	246	176
	1994	-0.547	(0.305)	0.021	342	162	1994	-1.104	(3.788)	0.0163	1307	175	1994	152.0	(637.1)	0.0184	3211	175	1994	0.049	(0.233)	0.0329	234	175
	Education	Treat		R^2_{adi}	AIČ	Z	Age	Treat		R^2_{adi}	AIČ	N	Tenure	Treat		R^2_{adi}	AIČ	N	Female	Treat		R^2_{pseudo}	AIC	Ν

E Robustness Tests Using Donut Regressions, Covariates and Industry Dummies

To test the robustness of the baseline results of this paper results, I conduct several tests. First, I estimate so-called *Donut regressions* for each outcome (see, e.g., Barreca et al., 2011). The model for the Donut regressions simply leaves out establishments that are one FTE unit above or below the size threshold. Since the observations that are closest to the threshold are most susceptible to manipulation, not including these observations in the regression is a way to control for selection of firms above or below the threshold, because one needs stronger assumptions in order to get consistent results for the treatment effect. Second, I include covariates and industry dummies in the RDD regressions. This should only affect the standard errors but not coefficient estimates in a valid RDD setting (Lee and Lemieux, 2010).

••		ıd.	nnies				092	(22)	128^{*}	(22)	055	044)	74**	036)	385	1220	040	ber of	
tivity	dded)	v. Ir	dun	2			39 0.4	27) (0.4	75 -0.	10) (01	61 -0.	51) (0.4	5** 0.0	37) (0.4	4 0.	73 144	68 52	o the num	
oduc	/alue ac	Co		994 - 201			1 0.45	9) (0.32)	7 -0.1	8) (0.1]	0.0- 00	7) (0.0	** 0.07(3) (0.03	8 0.4	84 874	17 340	N refers to	
ior pi	$Log(\Lambda$	Donut		11		(6	-0.05	(0.10)	-0.10	(0.10)	-0.12	(0.0)	0.117	(0.05)	8 0.38	37 13078	7 4690	brackets.	
nd lał			SS		00.0	(0.03)							*		0.38	3 13078	4690	ported in	
ints a	nts)	Ind.	dummie								0.013	(0.150)	0.850^{**}	(0.253)	0.133	124825	22402	level are re	
estme	nvestme	Cov.		-2006							-0.167	(0.215)	045***	(0.313)	0.054	76190	13443	olishment	
S: Inv	g(ICT in	ut		2000							0.188	0.225) (.774* 1	0.411 (0.031	15320	20289	t the estal the AIC.	
mmies	Log	Don			.324*	0.192)					-		0	<i>–</i>	0.031 (153201	20289 2	clustered a ording to 1	
ry du	ents)	Ind.	mmies			<u> </u>	0.474	(.351)	.710*	(.363)	0.012	(.164)	.245	.157)	0.034	11161 1	2266	ard errors chosen acc	
ndust	nvestme	lov.	qu	012			.002 -(456) (C	052* -0	541) (0	155 -(218) (C	033 0	174) (C	027 0	8477 3.	3387 5	bust stand iable were	
and ii	ansion i			1996-20			.466 -0	445)(0.	.694 -1.	428)(0.	.261 0.	221 (0.	252 -0	195)(0.	024 0.	2865 198	120 33	y OLS. Ro inning var	
iates	og(exp	Donut			.046	(130)	0-	0)	0-	0)	0-	0)	0	0)	024 0.	286729	0120 49	stimated by centered r	
covar		.p	mies		<u>_</u>	0	920	301)	57*	295)	203	[33]	196	(99)	0 220	352 29	912 46	utcome, es ms of the	
sions,	t flows)	v. In	dum	2			37 0.0	0.1) (0.5	60 -0.5	25) (0.2	04 -0.5	(0.1) (77	70 0.1	32) (0.1	76 0.0	619 616	96 102	spective o	
egress	restmen	Co		994-201			80 -1.4	38) (1.8)	02 -0.5	52)(0.42)	45 -0.2	(0.1)	0.2	$\frac{17}{0.18}$	36 0.0 [°]	63 3975	56 664	on the re pecific fun	
nut r	Log(inv	Donut		1	13	30)	-0.0	(0.43	-0.4	(0.3)	-0.0-	(0.16)	0.2((0.24)	36 0.00	60 5807	56 968t	ect of EPI ars. Year-s	
ng Do			ies		-0.0	(0.1;		()	*	()	*	()			0.0(2 5807	1 968	atment eff over all yea	
s usir	·k)	Ind.	dumm				0.041	(0.095	-0.300*	(0.100)	-0.089	(0.050)	0.016	(0.045	0.162	18923	5667	ws the tre summed c	
ss test	tal stoc	Cov.		-2012			1.601^{*}	(0.817)	0.340^{**}	(0.150)	-0.084	(0.062)	0.012	(0.054)	0.159	123851	37357	e table sho andwidths	
ustnes	.og(capi	ut		1994			0.025	0.138)	0.216 -	0.140)	0.150^{**}	0.076)	0.019	0.071)	0.148	72091	50907	< 0.1. Th optimal b	
Robı		Don:			0.071	(0.043)	-	-		-	-0	-	-	ī)	0.148	72091 1	20609	$< 0.05, {}^{*}p$ ar-specific)	
le 14:				time	$\frac{1}{1} (\gamma) = 0$	<u> </u>	(1,1)		(γ_2)		(γ_3)		(γ_4)			1	кЭ Сл	$(0.01, *^*p)$ at the (yes	
Tab				Availabl€	All perio		Period 1		Period 2		Period 3		Period 4		R^2_{adi}	AIČ	7	Note: $*** p < box{observations}$	

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		F IIT	n-leve			Indivic	lual-level	
		Log(gro.	ss wage _{jt}	($Log(\iota$	$wage_{ijt})$	
	Don	nut	Cov.	Ind.	Doi	nut	Cov.	Ind.
				dummies				dummies
Available time		199_{-}	4-2009			199_{-1}	4-2009	
All periods (γ)	0.045				-0.033			
	(0.038)				(0.268)			
Period 1 (γ_1)		0.027	0.356	0.083		-0.003	0.072	-1.262
		(0.253)	(0.473)	(0.085)		(0.205)	(0.789)	(0.509)
Period 2 (γ_2)		0.136	0.043	0.087^{*}		-2.021	-0.134	-0.273
		(0.084)	(0.057)	(0.051)		(1.424)	(0.101)	(0.103)
Period 3 (γ_3)		-0.037	-0.017	-0.089***		0.065	-0.044	-0.076
		(0.065)	(0.028)	(0.026)		(0.669)	(0.059)	(0.058)
Period 4 (γ_4)		0.065	0.025	0.040		-0.035	0.053	0.050
		(0.052)	(0.027)	(0.027)		(0.350)	(0.061)	(0.056)
R^2_{adi}	0.628	0.628	0.646	0.631	0.021	0.021	0.0451	0.2579
AIČ	46220	46223	31477	48752	15006	15009	16817	13046
Ν	27737	27737	24094	31064	9189	9189	1711	1374
Note: *** $p < 0.01$, ** estimated by OLS. Ro the number of observat forms of the centered	p < 0.05, *p < obst standard tions at the (ye	(0.1. The ta errors cluste errors cluste ar-specific) of were chos	uble shows th ered at the es optimal band	te treatment effe stablishment leve lwidths summed to the AIC	ct of EPL on l are reported over all years.	the respecti 1 in brackets. Year-specifi	ive outcome, . N refers to ic functional	
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Tabl	e 16: Ro	bustness	tests usin.	g Donut r	egression	s, covaria	ttes and i	ndustry du	ummies: I	Imploym	ent margin	
		Job realle	ocation rat	e		# of tem	p. workei	S	#	^ℓ of white-	collar work	ers
	Do	nut	Cov.	Ind.	Doi	nut	Cov.	Ind.	Dor	ut	Cov.	Ind.
				dummies				dummies				dummies
Available time		199_{c}	4-2012			1996	j-2012			199	6-2012	
All periods (γ)	-0.010 (0.009)				0.008 (0.013)				-0.018 (0.028)			
Period 1 (γ_1)		0.003	-0.032	-0.012	~	0.015	0.006	0.011	~			
		(0.041)	(0.027)	(0.021)		(0.036)	(0.009)	(0.021)				
Period 2 (γ_2)		-0.027	-0.043	-0.037		0.002	-0.058	-0.029				
		(0.039)	(0.031)	(0.023)		(0.049)	(0.043)	(0.031)				
Period 3 (γ_3)		-0.015	-0.015	-0.011		0.003	0.023^{*}	0.016		0.001	-0.026	-0.029
		(0.018)	(0.012)	(0.010)		(0.015)	(0.012)	(0.010)		(0.070)	(0.033)	(0.031)
Period 4 (γ_4)		-0.007	-0.006	-0.003		0.012	-0.005	0.003		-0.019	-0.050***	-0.053***
		(0.011)	(0.007)	(0.001)		(0.022)	(0.015)	(0.015)		(0.029)	(0.019)	(0.018)
R^2_{adi}	0.015	0.014	0.018	0.019	0.038	0.038	0.039	0.052	0.044	0.044	0.064	0.137
AIČ	-4579	-4573	-7536	-7151	25161	25167	27287	26542	35845	35847	38705	37239
Ν	24490	24490	22010	28391	45874	45874	40378	49467	26692	26692	28572	28956
Note: *** $p < 0.01$, ** level are reported in running variable were	p < 0.05, $*p < brackets$. N r chosen accor	<pre>< 0.1. The tal efers to the r ding to the A</pre>	ble shows the t number of obse AIC.	reatment effect ervations at th	of EPL on th e (year-specif	te respective c ic) optimal b	outcome, estin andwidths su	nated by OLS. I ummed over all	Robust stands years. Year-s	vrd errors clus pecific functic	tered at the estand	blishment e centered

F Robustness Tests: Varying the Optimal Bandwidth

As a further test to check the robustness of the baseline results of this paper, I vary the size of the bandwidth by taking different fractions of the optimal bandwidth (75%, 50% and 25% of the optimal bandwidth for the establishment-level estimations and 150%, 200% and 300% of the optimal bandwidth in case of the individual-level data).⁴³

			Log(lab	or producti	vity)	
Time period			-	1994-2012		
Bandwidth	75% of a	opt. bandw.	50% of c	pt. bandw.	25% (of opt. bandw.
	(1)	(2)	(3)	(4)	(5)	(6)
All periods (γ)	-0.008		0.011		-0.014	
	(0.024)		(0.029)		(0.040)	
Period 1 (γ_1)		0.160^{*}		0.149^{*}		0.325^{**}
		(0.089)		(0.089)		(0.134)
Period 2 (γ_2)		-0.101		-0.206*		-0.254*
		(0.085)		(0.111)		(0.149)
Period 3 (γ_3)		-0.093**		-0.033		-0.101
		(0.041)		(0.048)		(0.066)
Period 4 (γ_4)		0.052		0.043		0.031
		(0.034)		(0.040)		(0.057)
R^2_{adj}	0.347	0.347	0.251	0.251	0.138	0.138
AIC	112883	112875	72555	72554	34012	34006
Ν	40319	40319	26210	26210	12374	12374

Table 19: Robustness tests using different fractions of the optimal bandwidth: Labor productivity

Note: ${}^{***}p < 0.01$, ${}^{**}p < 0.05$, ${}^{*}p < 0.1$. The table shows the treatment effect of EPL on the respective outcome, estimated by OLS. Robust standard errors clustered at the establishment level are reported in brackets. N refers to the number of observations at the (year-specific) optimal bandwidths summed over all years. Year-specific functional forms of the centered running variable were chosen according to the AIC.

 43 In case of the individual-level data, I have to increase the size of the bandwidth, because the starting bandwidth is already very small.

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$\begin{array}{ c c c c c c c c c c c c c c c c c c c$	ital				Flow me	asures		
$\begin{array}{ c c c c c c c c c c c c c c c c c c c$					Log(total	capital		
$\begin{array}{ c c c c c c c c c c c c c c c c c c c$					investn	lent)		
$\begin{tabular}{ c c c c c c c c c c c c c c c c c c c$	12				1994-5	2012		
$\begin{array}{ c c c c c c c c c c c c c c c c c c c$	bandw. 25% of op	t. bandw.	75% of opt.	bandw.	50% of opt.	. bandw.	25% of of	ot. bandw.
$ \begin{array}{c c c c c c c c c c c c c c c c c c c $	(4) (5)	(9)	(2)	(8)	(6)	(10)	(11)	(12)
$ \begin{array}{c ccccccccccccccccccccccccccccccccccc$	-0.055		-0.065		-0.041		0.034	
$ \begin{array}{c c c c c c c c c c c c c c c c c c c $	(0.046)	<u> </u>	(0.091)		(0.105)		(0.140)	
$\begin{array}{c ccccccccccccccccccccccccccccccccccc$.057	-0.018	0.	142		0.492		0.609
$\begin{array}{c cccc} \text{Period 2} (\gamma_2) & -0.335^{***} & -0.410^{**} \\ & & (0.118) & (0.164) \\ \text{Period 3} (\gamma_3) & -0.069 & -0.060 \\ & & (0.050) & (0.058) \\ \text{Period 4} (\gamma_4) & -0.020 & 0.018 \\ \end{array}$.107)	(0.139)	(0.	321)	<u> </u>	0.366)		(0.464)
Period 3 (γ_3) (0.118) (0.164) Period 3 (γ_3) -0.069 -0.060 Period 4 (γ_4) -0.020 0.018	410**	0.507**	0-	.340	. 1	-0.167		-0.714
Period 3 (γ_3) -0.069 -0.060 Period 4 (γ_4) -0.020 0.018	.164)	(0.203)	.0)	337)	<u> </u>	0.412)		(0.610)
Period 4 (γ_4) (0.050) (0.058) 0.020 0.018	090.0	-0.061	0-	.152	T	0.258^{*}		-0.269
Period 4 (γ_4) -0.020 0.018	.058)	(0.068)	.0)	135)	<u> </u>	0.146)		(0.193)
	.018	-0.002	0.	036	•	0.150		0.381^{*}
(0.053) (0.061)	.061)	(0.079)	(0.	151))	0.175)		(0.231)
R_{adi}^2 0.129 0.129 0.108 0.108 0.056	.108 0.056	0.056	0.044 0.	044	0.043	0.043	0.023	0.024
AIČ 157538 157536 111194 111192 52212	1192 52212	52212	453598 45 ;	3602	316047 3	316047	145527	145525
N 46422 46422 32700 32700 15347	2700 15347	15347	75156 75	156	52340	52340	23964	23964

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5 5, 5 level are reported in brackets. N reters to the variable were chosen according to the AIC.

tment				pt. bandw.	(12)							-0.089	(0.236)	0.808^{**}	(0.369)	0.01	41916	7353	establishment ntered muning
T invest				25% of o	(11)	0.165	(0.200)									0.01	41918	7353	ustered at the orms of the ce
sion & IC	(ICT	$\operatorname{sment})$	-2006	ot. bandw.	(10)							0.021	(0.182)	0.889^{***}	(0.310)	0.02	82558	14525	dard errors cl c functional fe
: Expans	Log	invest	2000	50% of ol	(6)	0.225	(0.157)	r.								0.019	82562	14525	3. Robust stan s. Year-specifi
ndwidth				pt. bandw.	(8)							0.067	(0.167)	0.832^{***}	(0.252)	0.023	107897	18972	mated by OLS d over all year
ptimal ba				75% of of	(2)	0.290^{**}	(0.137)									0.022	107902	18972	outcome, esti widths summe
of the op				ot. bandw.	(9)			-0.293	(0.569)	0.404	(0.956)	0.228	(0.257)	-0.086	(0.270)	0.016	84379	14030	the respective optimal band
ractions				25% of of	(5)	0.056	(0.175)									0.017	84374	14030	ect of EPL on (year-specific)
lifferent f	cpansion	$\operatorname{tment})$	-2012	pt. bandw.	(4)			-0.439	(0.402)	-0.572	(0.626)	0.008	(0.195)	-0.174	(0.200)	0.021	174023	29057	treatment effort vations at the
s using d	Log(ex	invest	1996	. 50% of ol	(3)	-0.133	(0.128)									0.021	174019	29057	able shows the imber of obser
mess test				pt. bandw.	(2)			-0.467	(0.378)	-0.549	(0.394)	0.068	(0.178)	0.017	(0.168)	0.02	253774	42446	0 < 0.1. The transformed to the tense to the metric section to the metric section.
: Robust				75% of o	(1)	-0.037	(0.108)	к к				-	-			0.02	253771	42446	$p_{\rm res}^{**} p < 0.05, p_{\rm r}^{*}$
Table 18.			Time period	Bandwidth		All periods (γ)		Period 1 (γ_1)		Period 2 (γ_2)		Period 3 (γ_3)		Period 4 (γ_4)		R^2_{adi}	AIČ	Ν	Note: $*** p < 0.01$, ' level are reported in

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variable were chosen according to the AIC.
			Log	(gross wage	$_{jt})$	
Time period				1994-2009		
Bandwidth	75% of a	opt. bandw.	50% of a	opt. bandw.	25%	of opt. bandw.
	(1)	(2)	(3)	(4)	(5)	(6)
All periods (γ)	-0.007		-0.027		-0.018	
	(0.018)		(0.020)		(0.028)	
Period 1 (γ_1)		0.146^{*}		0.014		0.082
		(0.087)		(0.057)		(0.081)
Period 2 (γ_2)		-0.051		-0.023		-0.033
		(0.043)		(0.053)		(0.089)
Period 3 (γ_3)		-0.064**		-0.101**		-0.099**
		(0.031)		(0.040)		(0.050)
Period 4 (γ_4)		0.007		-0.005		0.010
		(0.025)		(0.028)		(0.039)
R_{adj}^2	0.58	0.58	0.514	0.514	0.386	0.387
AIC	38360	38355	22662	22663	10606	10607
Ν	23650	23650	14880	14880	7369	7369

Table 20: Robustness tests using different fractions of the optimal bandwidth: Establishment-level wage

Note: ${}^{***}p < 0.01$, ${}^{**}p < 0.05$, ${}^{*}p < 0.1$. The table shows the treatment effect of EPL on the respective outcome, estimated by OLS. Robust standard errors clustered at the establishment level are reported in brackets. N refers to the number of observations at the (year-specific) optimal bandwidths summed over all years. Year-specific functional forms of the centered running variable were chosen according to the AIC.

			Censo	red wage					Impu	ted wage		
			Log($(wage_{ijt})$					Log	$(wage_{ijt})$		
Time period			195	14-2009					199	94-2009		
Bandwidth	150% of	opt. bandw.	. 200% of	opt. bandw.	300% of op	t. bandw.	150% of of	ot. bandw.	200% of o	pt. bandw.	300% of c	pt. bandw.
	(1)	(2)	(3)	(4)	(5)	(9)	(2)	(8)	(6)	(10)	(11)	(12)
All periods (γ)	-0.033		-0.028		-0.042*		-0.039		-0.040		-0.045^{*}	
	(0.033)		(0.031)		(0.025)		(0.035)		(0.032)		(0.027)	
Period 1 (γ_1)		-0.004		-0.006		0.004		-0.004		-0.006		0.004
		(0.082)	-	(0.096))	(0.071)		(0.082)		(0.096)		(0.071)
Period 2 (γ_2)		-0.055	-	-0.234^{*})- -	0.211^{**}		-0.055		-0.234^{*}		-0.211^{**}
		(0.098)	-	(0.120)		(0.105)		(0.098)		(0.120)		(0.105)
Period 3 (γ_3)		-0.120^{***}		-0.119^{***}	0-	.111***	·	-0.117^{**}	1	0.116^{***}		-0.105^{***}
		(0.044)		(0.039)		(0.033)		(0.046)		(0.040)		(0.034)
Period 4 (γ_4)		0.042		0.033		0.001		0.026		0.028		-0.011
		(0.056)		(0.046)		(0.036)		(0.058)		(0.052)		(0.038)
R^2_{adi}	0.019	0.019	0.031	0.032	0.043	0.044	0.017	0.017	0.027	0.027	0.041	0.041
AIČ	44564	44546	58368	58341	87084	87062	46081	46069	60264	60242	90125	90110
Ν	28834	28834	37658	37658	55942	55942	28783	28783	37724	37724	56064	56064
Note: $***^{p} < 0.01, **$	$^{*}p < 0.05, ^{*}p$	< 0.1. The table	le shows the tr	eatment effect of	EPL on the resp	sective outcom	ie, estimated b	y OLS. Robust	standard error	s clustered at th	ıe establishment	c level are reported

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in brackets. N refers to the number of observations at the (year-specific) optimal bandwidths summed over all years. Year-specific functional forms of the centered running variable were chosen according to the AIC.

employme	nt																	
			Job Rea	ullocation					# of ten	nporary					# of whi	te-collar		
			Γέ	ate					worł	ters					work	Kers		
Time period			1994	-2012					1996-	2012					2003-	2012		
Bandwidth	75% of ol	ot. bandw.	50% of of	pt. bandw.	25% of of	pt. bandw.	75% of op	t. bandw.	50% of op ¹	t. bandw.	25% of op ¹	t. bandw.	75% of opt	. bandw.	50% of opt	t. bandw.	25% of opt	. bandw.
	(1)	(2)	(3)	(4)	(5)	(9)	(2)	(8)	(6)	(10)	(11)	(12)	(13)	(14)	(15)	-		
All periods (γ)	-0.009		-0.007		-0.018		0.005		0.005		0.005		-0.032^{*}		-0.045^{**}		0.072**	
	(0.006)		(0.008)		(0.013)		(0.008)		(0.010)		(0.014)		(0.017)		(0.018)		(0.028)	
Period 1 (γ_1)		0.015		0.052		-0.178^{**}		0.026		0.023		0.031						
		(0.023)		(0.033)		(0.085)		(0.020)		(0.018)		(0.028)						
Period 2 (γ_2)		-0.034		-0.036		-0.059		-0.045		-0.056	'	0.112^{**}						
		(0.024)		(0.029)		(0.062)		(0.038)		(0.040)		(0.051)						
Period 3 (γ_3)		-0.014		-0.017		-0.023		0.017**		0.016^{*}		0.023		-0.025		-0.017		-0.032
		(0.011)		(0.014)		(0.019)		(0.008)		(0.010)		(0.015)	<u> </u>	0.028)		(0.025)		(0.037)
Period 4 (γ_4)		-0.006		-0.004		0.000		-0.005		-0.003		-0.003	1	0.033^{*}		-0.052^{**}	·	0.083^{**}
		(0.008)		(0.010)		(0.018)		(0.014)		(0.021)		(0.028)	<u> </u>	0.020)		(0.023)		(0.034)
R^2_{adi}	0.012	0.012	0.012	0.012	0.02	0.02	0.032	0.032	0.026	0.026	0.019	0.019	0.03	0.03	0.024	0.024	0.013	0.013
AIČ	-6171	-6168	-4268	-4268	-1834	-1833	23143	23144	15974	15976	9319	9320	32348	32350	21935	21936	10865	10866
Ν	22380	22380	14359	14359	7091	7091	42050	42050	27482	27482	12591	12591	22452	22452	14405	14405	6843	6843
Note: $^{***}p < 0.01$, ' (year-specific) optim	p < 0.05, p al bandwidths	< 0.1.The tab summed over	ole shows the t all years. Year	reatment effect r-specific function	of EPL on tl onal forms of	he respective c the centered r	outcome, estim: unning variable	ated by OLS.	Robust standa: according to th	rd errors cluste e AIC.	ared at the est	ablishment lev	el are reported	in brackets.	N refers to th	ie number of ol	oservations at	the

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		Imputed	l wage	
		Individua	al-level	
	L	$og(wage_{ijt})$)	
	Bas	eline	Do	nut
Available time	1994	-2009	1994	-2009
All periods (γ)	-0.104**		-0.159	
	(0.048)		(0.405)	
Period 1 (γ_1)		0.001		-0.003
		(0.088)		(0.205)
Period 2 (γ_2)		-0.115		-2.021
		(0.107)		(1.425)
Period 3 (γ_3)		-0.163**		0.089
		(0.076)		(0.701)
Period 4 (γ_4)		-0.088		-0.258
		(0.085)		(0.686)
R_{adj}^2	0.016	0.016	0.022	0.022
AIC	31677	31678	14677	14680
Ν	19786	19786	8787	8787

Table 23: Baseline& Donut regressions: Imputed individual-level wage

Note: $^{***}p < 0.01, \,^{**}p < 0.05, \,^*p < 0.1$. The table shows the treatment effect of EPL on the respective outcome, estimated by OLS. Robust standard errors clustered at the establishment level are reported in brackets. N refers to the number of observations at the (year-specific) optimal bandwidths summed over all years. Year-specific functional forms of the centered running variable were chosen according to the AIC.





(c) 300% of opt. bandwidth

G Analyzing the Effect of EPL on Capital Investments and Labor Productivity by Industry

EPL may affect certain industries differently. Consequently, conducting the same analysis for the effect of EPL on investment flows, capital stock and labor productivity from the previous section by industry may reveal some heterogeneous effects, since EPL is more binding in some industries than in others.

Industry	Electr.,	Gas; Wate	er, Sewer	tge; Wasi	te & Rec	vel. Fc	T od Prod.	able 2 Beverag	4: Inv es; Tobac	/estm	ent flo ^{Textile, A}	ws by	indust	ry I ^{Wood}	&Paper	Prod; Prir	ting	Coke & P	etrol. Pre	od.; Chem	icals; Pharmac.
,	Bat	seline		Dom	at .		Baseline		Donut		Baseline	D	onut	Base	line	Don	at o	Baseli	ine		Donut
All periods (γ)	1.767^{**} (0.693)		2.280^{*} (1.044	* ~		-0.5 (0.5	339 28)	-0- (0.6	093 347)	-1.5 (1.1	691 14)	-1.726 (1.511)		-0.396 (0.619)		-0.542 (0.801)		0.105 (0.908)		-0.143 (1.135)	
Period 3 (γ_3)	~	3.500**	*	(r)	.987*** /1 F00/	-	-0.5	345 (20)	-0.5)49 \	1.72	, , ,	5.287	~	-0.346	~	0.270		0.504	~	1.133
Period 4 (γ_4)		0.681			(1.328) 1.181		-0 -0	00) 367	0.3	28	(3.13 -2.00	() *0	(4.937) - 2.372		(U.89U) -0.432		(1111.1)		(1.231)		(1.804) -0.591
		(0.955)			(1.465)		(0.6	84)	(0.8	38)	(1.17)	2)	(1.561)		(0.898)		(1.124)	Ŭ	(1.207)		(1.442)
R^2_{adj}	0.117	0.12	0.133		0.134	0.0	88 0.0	88 0	.1	1 0.0	8 0.08	1 0.082	0.086	0.092	0.092	0.099	0.099	0.083	0.082	0.093	0.093
AIC	6180 988	6178 988	5353 856		5353 856	23 13	62 139 11 23	11 20 20 20 20 20 20 20 20 20 20	353 120 47 20	53 37. 47 62	34 373 9 629	1 3477 587	3475 587	11994 1986	11996 1986	10917 1812	10919 1812	5662 909	5664 909	5048 810	5049 810
Note: $p < 0.01, p > 1$ (year-specific) optimal	$p < 0.05$, $*_p \cdot p$ bandwidths	< 0.1. The t _i summed over	able shows th all years. Ye	e treatment >ar-specific f	effect of EP unctional for	L on the res ms of the ce	pective outco ntered runnir	me, estimate 1g variable w	ere chosen ac	obust standa cording to th	rd errors cluste e AIC.	red at the esta	blishment leve	l are reported	in brackets.	N refers to th	e number of	observations	at the		
							Ta	ble 2	5: Inv	estme	int flor	vs bv	indust	rv II							
Industry	Ru	bber & F	lastic Prc	ъd.	Nor	-Metallic	Mineral	Prod.	Cemen	t, Lime &	Plaster; N	on-Met. M	ineral Pro	J. n.e.c.	Iron, Ba	sic & Fab	r. Metal I	Prod. C	Computer	, Electr. 8	z Optical Prod.
	Base	oline	Doi	out	Bas	eline	Ď	onut	Ba	seline		Dor	nt		Basel	ine	Donu	t	Baseli	ne	Donut
All periods (γ)	-0.418		-0.977 (1 179)		-4.293		-5.937 (4.042)		1.347* (0.808)		2.080** (1.012)				0.195 (0.413)		0.690		0.494 1 015)).545 305)
Period 3 (γ_3)	(+ 10.0)	-0.442	(=)	-2.115		-3.858	(==0)	-9.228	(000.0)	1.868	(21011)		1.431		(011.0)	-0.304	(2000-0	0.561	(01011	1.899	3.047
Domind $A(\infty)$		(1.654)		(2.115)		(3.458)		(6.548)		(1.194)			(1.702) $2 \pm 48*$			(0.588)	0	0.758)	0	1.620	(2.349)
1 (14)		(1.328)		(1.541)		(3.896)		(5.476)		(1.174)			(1.391)			(0.590))	0.689)	·	1.196)	(1.581)
R^2_{adj}	0.054	0.053	0.055	0.055	0.318	0.315	0.266	0.264	0.054	0.053	0.064		0.063		0.084	0.084	0.088	0.088	0.171	0.171	0.174 0.176
AIC	4682 766	4684 766	4277 699	4278 699	1491 257	1493 257	1397 240	1400 240	6096 992	6097 992	5616 915		5618 915		21891 3592	21891 3592	19817 3251	19819 3251	3505 581	3506 581	3220 3220533 533
Note: ${}^{***}_{p} < 0.01, {}^{**}_{j}$ (year-specific) optimal	$p < 0.05$, * $p \cdot p$ bandwidths	< 0.1. The ti summed over	able shows th : all years. Ye	e treatment par-specific f	effect of EP unctional for	L on the res ms of the ce	pective outco ntered runnin	ome, estimato 1g variable w	ad by OLS. F ere chosen ac	obust standa cording to th	rd errors clust e AIC.	ared at the esta	blishment leve	l are reported	in brackets.	N refers to th	ie mmber of	observations	at the		
							Tal	ble 26	i: Invo	estme	nt flov	vs by i	ndustr	III V							
Industry	E	lectrical <u>F</u>	Jquipment		Ma	chinery &	Equipme	ent	Motor V	phicles &	Other Trar	sp. Equipr	1. Furnitı	ue; Jewele	ry, Bijout	erie & Rel	. Articles	Repair of	& Install.	Machiner	y & Equipment
	Base	line	Don	ut	Base	line	Doi	ut	Base	line	D	onut	Ba	seline		Donut		Bas	eline		Donut
All periods (γ)	0.209 (1.061)		0.826 (1.143)		0.213 (0.697)		1.483 (0.902)		0.872 (0.737)		0.849 (0.835)		-0.298 (0.526)		-0.189 (0.747)			-0.974 (0.961)		-1.585 (1.223)	
Period 3 (γ_3)		0.517		1.876		-0.567		0.744		2.283**		2.262*		0.298		0.	75		0.356		-1.483
Period 4 (γ_4)		(1.290) - 0.045 (1.582)		(1.473) 0.144 (1.739)		(1.175) 0.555 (0.850)		(1.749) 1.702 (1.074)		(1.004) -0.079 (0.958)		(1.350) 0.164 (1.021)		(0.776) -0.881 (0.776)			.30) 352 110)		(1.631) -1.501 (1.212)		(2.247) -1.611 (1.537)
R ² A TC	0.144	0.142	0.162	0.162	0.071	0.071	0.074	0.074	0.138	0.14	0.143 6918	0.144	0.04	0.041	0.044	0.0	, 141 	0.131	0.131	0.138	0.137
N	565 565	565 565	5244 538	5240 538	0091 1364	0092 1364	1234 1234	1234 1234	1104	1104	0210 1023	0210 1023	2640 2640	2640 2640	14100 2368	14. 23	68	6066 887	0000 887	4900 811	4905 811
Note: $**^{p} < 0.01, *^{s}_{p}$ (year-specific) optimal	0 < 0.05, $p < bandwidths s$	c 0.1. The ta ummed over	ble shows the all years. Yea	treatment e u-specific fu	effect of EPL actional form	on the respe s of the cent	ctive outcom ered running	e, estimated variable wer	by OLS. Rob e chosen acco	ust standard rding to the /	errors clustere MIC.	l at the establi	shment level ar	e reported in	orackets. N r	efers to the m	unber of obse	rvations at th	le		

Industry	Bi	ulding&	Construct.	ion	Spec	ial. Cons	tr. Activ	ities	Sa	le, Repair	· Motor V	/ehicles			Wholesale	0			Retail T	rade	
	Bas	eline	Do	nut	Base	line	Do	mut	Bat	seline		Donut		Baseline		Donut		Baselin	le	Donu	t
All periods (γ)	-0.257		-0.249 (0.865)		-0.079		0.005		-0.822*		-1.549* (0.641	* _	0.3	57 37)	0.0	(23 (29)	0.0	.155 303)		0.141	
Period 3 (γ_3)	(000.0)	-1.314	(000.0)	0.616	(007.0)	-0.296	(000.0)	-0.174	(001.0)	-0.684	110.01	-1.25	5	0.6	18 (0.	0.'	713 10.)).312	(001-0	0.391
		(0.815)		(1.300)		(0.380)		(0.583)		(0.705)		(1.04)	2)	(0.6)	15)	.0)	733)	0)	(.385))	0.561)
Period 4 (γ_4)		0.259		-0.557		0.101		0.110		-0.898		-1.665	*	0.2	04	Ō	413	, Ţ	0.097		-0.208
		(0.810)		(1.105)		(0.444)		(0.538)		(0.673)		(0.82)	8)	(0.5)	6 ()	(0)	711)	0)	(.493)	<u> </u>	0.702)
R^2_{adj}	0.066	0.067	0.07	0.07	0.042	0.042	0.045	0.045	0.051	0.051	0.056	0.05	6 0.0	73 0.0	73 0.0	0.0	077 0.	.036 ().036	0.039	0.039
AIC	11604 1899	11603 1899	10140 1660	10141 1660	52532 8762	52533 8762	47302 7897	47304 7897	19376 3219	19378 3219	17476 2906	1747 2900	2 - 264	67 264 16 43^2	68 24 40 40	141 24 15 40	443 45 015 7	5812 4 682 3	$5813 \le 7682$	40991 6883	40993 6883
Note: *** $p < 0.01$, ** (year-specific) optimal	1 < 0.05, *p bandwidths	< 0.1. The summed ove	table shows the r all years. Y	he treatment (ear-specific fu	effect of EPL inctional form	on the respe is of the cent	sctive outcor sered running	me, estimatec g variable we	by OLS. R e chosen ac	obust standa cording to the	rd errors clu 3 AIC.	stered at the	establishme	nt level are r	eported in b	rackets. N r	efers to the	number of c	observations	at the	
						É	و ما ما م	0. T.	000000		ļ		+ 1 T								
						T	ante z	0. 111	CSUILIC	TIL TIO	vu sw	snntt	uly v								
Industry	Transport	., Wareho	ousing & S	torage; Po	stal Serv.	Accomn	nodation	& Food S	arvices	Informa	ation & Co	ommunica	tion	Financia	al & Insu:	rance Serv	vices		Real Esta	te Serv.	
	Basel	ine		Donut		Base	sline	Don	ut	Baseli	ne	Dont	t	Baseli	ine	Dom	ut	Basel	line	Dom	ıt
All periods (γ)	0.855*		0.773 0.573			-0.457		-0.591		1.234** (0 == 0)		1.728**		-1.179		-0.515		0.008		-0.110	
	(0.419)	100 0	(016.0)	τ Σ	*	(0.400)	0000	(170.0)	1100	(ecc.u)	*010 F	(e10.0)	*000	0.124)	**000	(0.044)	001 1	(001.0)	1000	(0.339)	0100
Ferrod 5 (γ_3)		122.0		1.00. /0 en			-0.112		-0.844		1.240° (0.711)			1	2.000 (eno		-1.193		-0.005		-0.012
Period 4 (γ_A)		0.884		0.01	r ~		-0.047		-0.261		1.220		1.864*		-0.304		0.024		020.0		-0.190
		(0.679)		(0.81)	3)		(0.691)		(0.973)		(0.912)	_	(1.059)	0	(1.090)		(1.189)		(1.167)		(1.500)
R^2_{adj}	0.089	0.089	0.092	0.09	12	0.056	0.056	0.058	0.058	0.073	0.072	0.073	0.073	0.047	0.047	0.05	0.05	0.174	0.173	0.18	0.18
AIC	20437	20439	18975	1897	122	20542	20543	18438	18439	13050	13052	12057	12059	12879	12878	12068	12069	10027	10029	9366	9368
Ν	3323	3323	3084	308	4	3442	3442	3093	3093	2194	2194	2027	2027	2132	2132	2001	2001	1639	1639	1531	1531
Note: ${}^{**}_{p} < 0.01, {}^{**}_{p}$ (year-specific) optimal f	< 0.05 , $*_p <$ and widths st	0.1. The tab immed over a	le shows the tı ∐ years. Year∹	reatment effect specific function	t of EPL on th onal forms of th	e respective c he centered ru	outcome, esti unning variab	imated by OL ble were chose	 Robust str according to 	o the AIC.	clustered at t	he establishm	ent level are 1	eported in br	ackets. N ref	ers to the nu	mber of obse	rvations at t	he		
						Ta	uble 29	9: Inv	estme	nt flov	vs by	indust	Iry VI								
Industry	Legal &	z Account.	ing Serv.	Mani	agement Co	msultancy	Serv.	Architectui	al & Engi	neering Sei	· A.	Scientific	R&D Ser	v.	Advert. &	z Market I	Research; (Other Pro	f., Scientif	ic & Tech	1. Serv.
	Baselin	0	Donut	Ba	seline	Doni	ut	Baselin		Donut	ц.	3aseline	De	mut	Basel	ine			Donut		
All periods $(\gamma) = (1)^{1/2}$).200).458)	0.6	265 344)	-0.566 (0.981)		-1.074 (1.297)		(0.061) (0.454)	o j	182 508)	1.08 (0.66)	8 (6	(0.9073)		0.455 (0.939)		-0.074 0.904				
Period 3 (γ_3)	O .	.128	0.39		-0.484	~	-3.311	0	443	0.71	19	0.726	~	0.455		1.588			1.802	0	
	00	.633)	(1.09	(2) (2)	(1.936)		(2.589)	0,	.713)	(1.0)	33)	(1.041)		(1.393)		(1.684)			(1.83)	~ ·	
Period 4 (γ_4)) (j	.259 647)	0.18	2 62	-0.001 (1.223)	-	-0.337 (1.548)	7 Q	.277	-0.2	01	1.495 (0.955)		1.200 (1.247)		-0.200 (1.275)			-1.04		
R^2_{adi} (0.065 0	.065 0.	0.0 70	7 0.07	0.069	0.068	0.069	0.078 0	0.18 0.1	074 0.07	74 0.150	6 0.155	0.165	0.164	0.09	0.09	0.093		0.09	1	
AIC	2987 1: 2222 2	222 11 222 15	227 1125 122 1922	29 5417 2 894	5419 894	5081 841	5082 841	14880 1- 2534 2	1881 13 534 20	597 135 312 231	98 450 2 761	5 4505 761	4055 684	4056 684	$5144 \\ 869$	5145 869	4855 821		4855 821		
Note: $***_p < 0.01$, $**_p < 0.01$, $**_p < (year-specific) optimal bas$	0.05 , $*_p < 0.1$ idwidths sume	. The table sh	hows the treatm	nent effect of El	PL on the respe	ctive outcome,	, estimated by	' OLS. Robust	standard erro	rs clustered at	the establishn	nent level are r	enorted in hra	ckots. N refers	to the numb	ar of obcorrecti					

		ut			0.124	(1.157)	0.575	(1.535)	0.028	9512	1637	
	onal Serv.	Dom	0.312	(0.899)		_		_	0.028	9510	1637	
	ther Perso	sline			-0.122	(0.771)	2.035	(1.308)	0.034	10180	1749	
	0	Base	0.633	(0.664)					0.033	10181	1749	-
	nal & Household Goods	Donut			5.817^{***}	(2.133)	-1.590	(2.916)	0.245	2320	402	cific) optimal bandwidths summe
	ers; Persoi		1.229	(1.658)					0.239	2323	402	the (year-spe
	f Compute	sline			3.613^{**}	(1.643)	-0.316	(2.104)	0.247	2433	421	bservations at
1	Repair o	Base	1.317	(1.281)					0.246	2433	421	e number of o
	creation	mut			-0.201	(1.585)	-2.919	(2.060)	0.074	5778	962	N refers to the
	nent & Re	Do	-1.359	(1.231)					0.073	5778	962	in brackets.]
2	Intertainn	seline			-0.571	(1.002)	-2.123	(1.505)	0.07	6253	1040	l are reported
	Arts, I	Ba	-1.157	(0.813)					0.07	6252	1040	dishment leve
	vices	onut			0.694	(0.714)	-1.040^{*}	(0.589)	0.034	41305	6963	l at the estab
	Social Ser		-0.367	(0.448)		_		_	0.033	41308	6963	arors clustere
	Health &	aseline	~	(0.353	(0.474	-0.622	(0.450)	0.032	() 44789	7544	ist standard e
)		B	-0.178	(0.321)				_	0.032	44790	7544	v OLS. Robu
		Donut	×	_	-1.217	(1.291)	-1.357	(0.964)	0.015	11324	1871	, estimated by
	cation		-1.308	(0.761)		_		_	0.016	11322	1871	ive outcome z to the AIC
	Edu	eline			-1.134	(0.846)	-1.120	(0.739)	0.02	12481	2063	n the respec sen accordin
		Bas	-1.126^{**}	(0.552)					0.021	12479	2063	fect of EPL o ble were chos
	v.	mut			0.277	(1.785)	-0.646	(710.0)	0.032	14582	2439	s treatment ef running varia
	Other Ser	Dc	-0.520	(0.797)					0.032	14581	2439	the shows the the centered
	Travel &	seline			-0.307	(1.045)	-0.463	(0.727)	0.035	15722	2629	< 0.1. The ta onal forms of
		Bas	-0.423	(0.587)					0.035	15720	2629	p < 0.05, p . pecific function
	Industry		All periods (γ)		Period 3 (γ_3)		Period 4 (γ_4)		R^2_{adi}	AIČ	N	Note: $*** p < 0.01$, $**$ over all vears. Year-si

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							L	able 3.	1: Car	ital s	tock l	bui yc	ustry	I								
Industry	Electr.	Gas; Wat	ter, Sewera	ige; Wast	e & Recy	cl. Fo	od Prod.; j	Beverages;	Tobacco	Text	ile, Appai	rel & Leat.	her	Wood &	Paper P ₁	od; Print	ing C	oke & Pe	trol. Pro	d.; Chem	icals; Phar:	mac.
	Base	eline		Donut			Baseline	Ď	onut	Base	aline	Dom	at	Baselin	ы	Donu		Baselir	le		Donut	
All periods (γ)	-0.338 (0.290)		-0.730 (0.480)			-0.1	01 70)	-0.129 (0.206)		0.044 (0.409)		-0.011 (0.670)		-0.158 0.228)	' 3	0.437).311)).109).324)		0.092 0.396)		
Period 3 (γ_3)		-0.320		-U).363		0.03;	3	0.030		-0.049		0.472		0.080		0.084	T	0.236		-0.110	
		(0.526)		0)	(286)		(0.29)	2)	(0.388)		(1.266)		(1.612)	0	0.369)	S	0.563)	U	(.435)		(0.568)	
Period 4 (γ_4)		-0.345		-0).834		-0.18	99	-0.218		0.057		-0.082	1	0.205	1	0.585	0).229		0.151	
		(0.386)		0)	.587)	_	(0.22)	()	$(0.271)_{1}$		(0.435)		(0.740)		0.342)	-	(.455)	9	(.404)		(0.480)	
R^2_{adj}	0.25	0.249	0.279	0	.279	0.2	16 0.21	6 0.231	0.23	0.305	0.303	0.288	0.286	0.245	0.245 ().267 ().267 ().296 (.297	0.308	0.308	
AIC	2492 678	$2494 \\ 678$	2122 575	.4 -2	2124 575	167	38 5299 71 1671) 4653 ! 1462	4655 1462	1199 343	1201 343	1084 310	1086 310	4097 1206	4099 1206	3624 1069	3625	2328 665	2336 665	2066 589	2068 589	
Note: $*** p < 0.01$, $**$ over all years. Year-sp	> < 0.05, $p < ecific function$	< 0.1. The ta- nal forms of	the shows the 1 the centered r	treatment eff unning varia	fect of EPL c ble were cho	in the respection seen according	tive outcome, , g to the AIC.	estimated by O	LS. Robust sta	ndard errors	clustered at th	ie establishme	nt level are re	ported in bra	kets. N refer	s to the num	ber of observa	tions at the (year-specific	c) optimal ba	ndwidths sumn	ned
							Ë	able 35	2: Cap	ital st	tock b	y indı	ustry	II								
Industry	Ru	bber & F	Plastic Proc		Cement,	Lime & .	Plaster; Nc	m-Met. Mi	neral Prod.	n.e.c.	Iron, Ba	sic & Fabr	. Metal P	rod. (Jomputer,	Electr. 8	z Optical	Prod.	Ele	sctrical Ec	quipment	
	Base	sline	Don	ut	Bas	teline		Dor	ut		Baseli	ne	Donu	t	Baselin	ıe	Donut		Baseli	ne	Donut	
All periods (γ)	(0.339)		0.433 (0.442)		-0.126 (0.322)		0.423 (0.456)				0.232* 0.131)		0.259 (0.175)		(801* 0.409))	0.745 0.496).383).382))	0.307 0.387)	
Period 3 (γ_3)	()	-0.223	()	-0.553	()	-1.065^{**}	*		-0.970		0	.570***	0	.749**	1	744**		L.691	(0.188	-0	.884
		(0.454)		(0.601)		(0.402)			(0.741)		-	(0.219)	<u> </u>	0.304)	0	0.877)	0	(290.1)	(0.893)	(0.	(586)
Period 4 (γ_4)		0.593		0.897		0.527			1.117^{*}			-0.053 (0.184)		0.105).470).466)	0 3	0.391)	0.442 0.366)	0 0	(498)
$R^2_{n,x}$	0.273	0.274	0.275	0.28	0.144	0.156	0.163		0.174	+	0.21	0.213	0.219	0.222	0.395	0.4	0.403 (0.406 (0.301	0.299	0.321 0.	323
AIC	1809	1808	1701	1699	2008	2001	1786		1780		8323	8316	7535	7527	1455	1452	1342	1341	1132	1134	1062 1	062
Z	519	519	481	481	594	594	532		532		2502	2502	2249	2249	428	428	392	392	354	354	336	336
Note: ${}^{***}_{p} < 0.01$, ${}^{**}_{j}$ over all years. Year-st	$p < 0.05$, $p < p_{cific}$ function	Content of the tal to the tal to the term of term o	ble shows the t the centered ru	reatment effe unning variat	set of EPL of sle were chos.	1 the respect en according	ive outcome, et to the AIC.	stimated by OL	S. Robust stan	lard errors cl	ustered at the	establishment	level are rep.	orted in brack	tts. N refers t	o the number	of observatio	as at the (ye	ar-specific) o	optimal band	wid ths summed	_
							$\mathrm{T}_{\hat{o}}$	uble 33	: Capi	tal st	ock b	y indu	Istry 1	II								
Industry	Mau	chinery &	z Equipmen	1t	Motor V_{ϵ}	phicles &	Other Trai	1sp. Equip1	n. Furnit:	ne; Jewel	ery, Bijou	terie & Re	 Articles 	Repair	& Install.	Machiner	y & Equi	pment	Build	ling & Co	Instruction	
	Base	line	Dom	ut	Basei	ine	D	onut	Ba	seline		Donut		Bas	eline		Donut		Baseli	ne	Donut	
All periods (γ)	0.125		0.390		0.252		0.270		0.062		-0.052			0.008		0.178		13	0.020		-0.196	
Period 3 (∞)	(017.0)	-0.101	(026.0)	-0.291	(01-7-10)	0.128	(TOG-O)	0.472	(0017-0)	0.205	(077.0)	0	018	(000.0)	0.398	(nten)	0.50	- 6	(077.0	-0.213	.u.4uu) -0	076
		(0.394)		(0.526)		(0.425)		(0.587)		(0.278)		0	385)		(0.609)		(0.84)	(9	0	(0.241)	0)	575)
Period 4 (γ_4)		0.243		0.668		0.328		0.184		-0.041		0-	094		-0.169		0.03	7		0.052	0-	.221
0-		(0.291)		(0.411)		(0.350)		(0.406)		(0.269)		(0.	351)		(0.450)		(0.66	2)		(0.276)	0)	(473)
R^2_{adj}	0.195	0.195	0.216	0.218	0.359	0.358	0.364	0.364	0.158	0.158	0.164	0 i	163	0.09	0.091	0.107	0.10	91	0.161 2270	0.161	0.174 0.	.173
N	3350 1005	3357 1005	903 903	2987 903	2829 846	283U 846	2038 783	2040 783	0299 1993	030U 1993	574U 1806	18.0	742 306	1804 550	550 550	1590 488	461 488		387U 1167	3872 1167	3291 5 983 5	29.5
	CONT	CONT	909	- eng	040	040	100	0.01	066T	CRAT	ποτ	Ť	200	000	000	400	1007	_	-	1011	1011 1011	

 $\frac{R_{adf}^2}{R_{adf}^2} = \frac{(0.231)}{105} \frac{(0.216)}{2035} \frac{(0.216)}{0.218} \frac{(0.359)}{0.359} \frac{(0.354)}{0.356} \frac{(0.440)}{0.364} \frac{(0.158)}{0.356} \frac{(0.240)}{0.364} \frac{(0.364)}{0.161} \frac{(0.161)}{0.161} \frac{(0.$

Industry	Special	l. Constr	r. Activit	ies	Sale,]	Repair M	lotor Veh	icles	1 2 2 2 2 2 2 2 2 2 2 2 2 2 2 2 2 2 2 2	Wholesa.	le Trade			Retail	Trade		Transpor	t., Wareh	iousing & St	orage; Postal Serv.
	Baselin	le	Don	ut	Basel	ine	Dor	iut	Base.	line	Doi	nut	Base	eline	Doi	nut	Basel	ine		Donut
All periods (γ)	0.109		-0.018		-0.081		-0.141		-0.138		-0.159		0.062		0.062		-0.003		0.006	
	(0.092)	<u> </u>	(0.127)		(0.138)		(0.228)		(0.159)		(0.185)		(0.120)		(0.185)		(0.140)		(0.168)	
Period 3 (γ_3)	0).135		-0.018		0.001		-0.126		-0.215		-0.370		-0.067		0.070		0.334		0.655^{**}
	<u>)</u>	(136)		(0.195)		(0.270)		(0.423)		(0.220)		(0.300)		(0.163)		(0.268)		(0.221)		(0.291)
Period 4 (γ_4)	0).084		-0.018		-0.128		-0.150		-0.107		-0.079		0.240		0.051		-0.207		-0.354
	<u>)</u>	(139)		(0.191)		(0.200)		(0.314)		(0.210)		(0.249)		(0.192)		(0.280)		(0.199)		(0.223)
R^2_{adi}	0.099 0	0.099	0.103	0.103	0.049	0.049	0.053	0.052	0.187	0.187	0.2	0.2	0.115	0.115	0.126	0.126	0.158	0.16	0.166	0.17
AIČ	12736 1	2738	10804	10806	5805	5807	5060	5062	7971	7973	7130	7131	11239	11239	9382	9384	7278	7275	6624	6615
N	4267 4	4267	3628	3628	1782	1782	1538	1538	2510	2510	2253	2253	3354	3354	2813	2813	2197	2197	2012	2012
Note: ${}^{***}_{p < 0.01}$, ${}^{**}_{l}_{l}$	0 < 0.05, *p < 0.05	.1. The tabl	le shows the	treatment e	ffect of EPL c	on the respec	ctive outcom	e, estimated	by OLS. Rob	ust standarc	1 errors clust	'ered at the e	stablishment	level are rep	orted in brac	kets. N refer	s to the numb	er of observa	ations at the	
(year-specific) optima.	. Dandwidtns sun	nmed over a	ul years. Yea	ar-specinc iu.	ncuonal lorm:	s of the centa	ered running	variable wer	e cnosen acco	rding to the	AIC.									
							Tab	de 35:	Capi	ital st	ock b	v indı	lstrv	Λ						

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Table	

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$ \begin{array}{ c c c c c c c c c c c c c c c c c c c$	Accomm	ΤĂ	odation &	¿ Food S	ervices	Finan	cial & Ins	surance St	erv.		Real Esta	te Serv.		Lega	l & Accoi	inting Sei	rv.	Manage	ment Cor	Isultancy	Serv.
$ \begin{array}{ c c c c c c c c c c c c c c c c c c c$	Baseline Donut	ine Donut	Donut	ut		Basel	line	Doni	ut	Basel	line	Dom	ut	Baseli	ne	Don	ut	Basel	ine	Dom	ıt
$ \begin{array}{ c c c c c c c c c c c c c c c c c c c$	0.023 0.242	0.242	0.242			-0.181		-0.221		-0.110		-0.317		-0.119		-0.382^{*}		0.275		-0.565	
$ \begin{array}{c ccccccccccccccccccccccccccccccccccc$	(0.170) (0.253)	(0.253)	(0.253)			(0.266)		(0.374)		(0.344)		(0.535)		(0.136)		(0.217)		(0.642)		(0.916)	
$ \begin{array}{c ccccccccccccccccccccccccccccccccccc$	-0.112 0.021	-0.112 0.021	0.021	0.021			0.138		0.097		0.109		-0.293		0.367^{*}		-0.567*		1.940^{*}		0.292
$\begin{array}{c c c c c c c c c c c c c c c c c c c $	(0.246) (0.406)	(0.246) (0.406)	(0.406)	(0.406)			(0.368)		(0.524)		(0.687)		(0.912)		(0.200)		(0.312)		(1.139)		(1.118)
$ \begin{array}{c ccccccccccccccccccccccccccccccccccc$	0.190 0.476	0.190 0.476	0.476	0.476			-0.417		-0.375		-0.254		-0.330		0.193		-0.120		-0.249		-0.763
$\begin{array}{ c c c c c c c c c c c c c c c c c c c$	(0.265) (0.381)	(0.265) (0.381)	(0.381)	(0.381)			(0.384)		(0.498)		(0.600)		(0.859)		(0.180)		(0.273)		(0.804)		(1.096)
$ \begin{array}{c ccccccccccccccccccccccccccccccccccc$	0.146 0.146 0.15 0.15 $($	0.146 0.15 0.15 0	0.15 0.15 (0.15 ().344	0.345	0.355	0.355	0.26	0.259	0.278	0.277	0.201	0.204	0.218	0.219	0.163	0.17	0.131	0.131
1301 1301 1212 1212 844 762 762 1255 1255 1031 1031 515 515 479 479	6381 6383 5614 5615	6383 5614 5615	5614 5615	5615		4452	4452	4132	4133	3510	3512	3190	3192	3415	3410	2835	2835	2038	2034	1884	1886
	1794 1794 1566 1566	1794 1566 1566	1566 1566	1566		1301	1301	1212	1212	844	844	762	762	1255	1255	1031	1031	515	515	479	479

Note: "" p < 0.01, " p < 0.05, " p < 0.1. The table shows the treatment effect of EPL on the respective outcome, estimated by OLS. Robust standard errors clustered at the establishment level are reported in brackets. N refers to the number of observations at the (year-specific) optimal bandwidths summed over all years. Year-specific functional forms of the centered running variable were chosen according to the ALC.

Table 36: Capital stock by industry VI

Industry	Architec	tural & E	Sngineerin	g Serv.	Advert. &	Market.	Research; Other Pr	of., Scientific & Techn. Serv.		Educat	ion		Healt	h &Social	Services	1	Arts, Ente	ertainment	& Recret	ntion
	Base	line	Don	nt	Baselin	16		Donut	Baseli	DIG	Donu	+	Baselir		Donut		Baseli	ne	Donut	
All periods (γ) ,	-0.211		0.002		-0.183		-0.495		-0.047		-0.015		0.097	Ŷ	0.067	Ŷ	0.468		0.233	
	(0.142)		(0.199)		(0.239)	-	(0.356)		(0.182)		0.301)	_	0.149	0	.215)	8	0.317)	-	0.445	
Period 3 (γ_3)		-0.334		-0.118		1.026		0.770		0.601		0.500		.123	Ŷ	0.109	Ŷ	0.745**	Ţ	0.212
		(0.271)		(0.390)	-	0.628		(0.799)		0.357)		0.580)	č	(191)	9	.306)		0.374)	Ű	(.624)
Period 4 (γ_4)		-0.108		0.090	Ť).665*		-0.938*		0.438^{*}		0.284		.081	Ŷ	0.051		0.089	Ţ	0.269
		(0.204)		(0.260)	Č	0.379		(0.551)		0.267)	Ŭ	0.383	Ű	(219)	9	.289)	Ŭ	0.612)	9	0.718)
R^2_{odi}	0.227	0.227	0.232	0.231	0.146 (0.159	0.135	0.143	0.028	0.034	0.035	0.037	0.090 (0 680.	.093 0	.092 0	0.230	0.231	0.237 (0.235
AIČ	5025	5026	4465	4466	1546	1539	1440	1436	3643	3637	3127	3127	12475 1	2477 1	1116 1	1118	1986	1987	1768	1770
N	1669	1669	1498	1498	485	485	449	449	1040	1040	902	902	3731	1731 3	323 3	323	545	545	482	482
Note: $**^{p} < 0.01, *^{p}$	p < 0.05, p	· 0.1. The ta	ble shows the) treatment e.	flect of EPL on	the respect	ive outcome, estimated by	OLS. Robust standard errors clustered a	t the establish	nent level ar	a reported in	brackets. N	refers to the r	umber of obs	ervations at	the				

Note: ""p < 0.01, "p < 0.05," p < 0.1. The table shows the treatment effect of BPL on the respective outcome, estimated by OLS. Robust standard errors (var-specific) optimal bandwidths summed over all years. Year-specific functional forms of the currence running variable were chosen according to the AIC.

eriods (γ) d 3 (γ_3)	Elecut., Ga	us; Water,	Sewerage;	Waste & Re	scycl. V	Vood & Pap	per Prod;	Printing	Rı	ubber & P	lastic Pro	òd.	Cement, L	ime & Pla	ster; Non-N	fet. Minera	l Prod. n.e	.c. Iron	, Basic & F	br. Metal	Prod.
eriods (γ) d 3 (γ_3)	Baselin	ne		Donut		Baseline		Donut	Bas	eline	Don	nt	Baselin	le		Donut			aseline	Dom	t
d 3 (₇₃)	0.213		-0.197 0.401)		0.15	66	0.20	1 3)	-0.026		0.008 (0.185)		-0.076	0- 0)	.104 319)			0.125		0.098 (0.110)	
	0.	.782**	(+0++0	0.452		0.504	**	0.644**	(0110) :	0.214	(00.10)	-0.068	-	0.466	(010	-0.3	48		0.027	(0110)	0.039
	<u>ت</u>	0.342)		(0.628)		(0.24)	3)	(0.308)		(0.224)		(0.325)	<u>ت</u> `	0.523)		39·0)	88) 38)		(0.109)		(0.202)
(4 (74)	' =	-0.212 0.236)		-0.762 (0.479)		(0.14)	1 (1	(0.202)		-0.156 (0.189)		(0.236)		0.158 0.380		-0.0 (0.39	00		0.186° (0.112)		0.126 (0.144)
	0.37	0.376	0.386	0.389	0.46	37 0.46	9 0.48	3 0.482	0.613	0.613	0.624	0.624	0.292 (0.293 0.	314	0.31		0.59	0.591	0.607	0.607
	2050 652	2046 652	1776 552	1774 552	282	3 2819 0 1220	9 $250.$ 0 1075	4 2501 3 1079	533	1200 533	1105 482	1107 482	1574 510	1574 1 510 4	412 154	141 45,	4	5433 2602	5434 2602	4898 2334	4900 2334
* $p < 0.01$, ** p lustered at the . 1 over all years.	 < 0.05, *p < 6 setablishment 1 Year-specific fi 	 The tabl- level are rept unctional forr 	e shows the tre prted in bracker is of the center	atment effect of ts. N refers to 1 red running varia	EPL on the resi the number of ol ble were chosen	bective outcom bervations at according to t	ne, estimated the (year-spe he AIC.	by OLS. Robus scific) optimal 1 8: Lal	t standard andwidths OT DT	oduct	ivitv	by inc	dustry	/ II				_			
TV	Compute	ar. Electr	. & Optice	al Prod.	Elect	rical Equi	ipment		Machine	irv & Eau	upment	Mot	tor Vehicle	es & Othe	r Transp.	Equipm.	Furniture	: Jewelerv	. Bijouteri	& Rel. A	rticles
	Base	line	Don	nut	Baseline		Donut		Baseline	- F	Donut		Baseline		Donut		Base	line		Donut	
riods (γ)	0.290 (0.318)		0.416 (0.462)		-0.287 0.210)	.0- (0.:	.025 303)	-0.	164 32)	-0.0)84 92)	-0-	138 45)	-0.2	43 97)		-0.185^{**} (0.087)		-0.114 (0.121)		
1 3 (₇₃)	<u> </u>	0.135	~	0.265	0	020).0	*266	-0.4	63* 	;;0-	328	1.0-	30	Ϋ́	.294	~	-0.152	·	-0.11	m 3
$14(\gamma_4)$		(0.784) 0.371		(1.265) 0.486	<u>,</u> 0	283) 434	<u>0</u> 9	.239 .239	-0.0	68) 157	-0.0	320) 320	0.7 -0.1	52) 42	<u> </u>			$(0.135) - 0.218^{*}$		0.216	20
		(0.265)		(0.342)	(0.	271)	(0.	321)	(0.1)	49)	(0.2)	26)	(0.1)	86)	0)	.228)		(0.123)		(0.152)	
	0.314	0.312	0.308	0.307	0.676 0.	677 0.4	692 0.	.695 0.5	3.0 68	59 0.59	99 0.5	0.7	715 0.7	15 0.72	22 0	.721	0.535	0.535	0.545	0.544	
	1443 425	1444 425	1345 387	1347 387	853 8 394 3	553 8 94 3	306 8 172 3	302 22 372 10	36 22: 55 10(34 20: 35 96	49 20 6 96	36 19 36 86	44 19. 38 86	46 184 8 81;	9.00	813	3864 1820	3866 1820	3510 1631	3512 1631	
p < 0.01, [*] p $p < 0.01, $ [*] ecific) optimal	p < 0.05, $p < p$ bandwidths is	< 0.1. The t summed ove.	able shows the r all years. Ye.	e treatment effe ar-specific func	ct of EPL on t tional forms of 1	he respective the centered 1	outcome, es running varis	timated by OI able were chose	S. Robust sts in according t	andard errors the AIC.	clustered at	t the establis	hment level a	re reported in	brackets. N r	efers to the m	mber of obser	vations at th	0		
						Ta	uble 3	9: Lab	or pro	oducti	ivity	by inc	lustry	III							
try	Repair &	z Install.	Machinery	y & Equipn	nent	Building ,	& Constr	ruction	Sp	ecial. Con	nstr. Act	ivities	Sal	e, Repair	Motor Vel	nicles		Ré	tail Trade		
	Base	line		Donut		3aseline		Donut	B	seline		Donut	Βâ	seline	D	nut	Baseli	ne		Donut	
riods (γ)	0.431^{*} (0.227)		0.556 (0.370)		-0.05 (0.15	35 4)	-0.14 (0.18)	47 7)	(0.053)		-0.076 (0.078		0.170 (0.113)	_	-0.048 (0.173)		-0.072 (0.095)		0.204 0.129)		
$13 (\gamma_3)$		0.559		0.696		-0.22	6	-0.337		-0.134^{*}	~	-0.110	_	0.153		-0.243		-0.173		-0.344	
		(0.406)		(0.638)		(0.40)	3)	(0.594)		(0.070)	_	(0.110)		(0.191)		(0.320)	0	(0.150)		(0.221)	
$14(\gamma_4)$		0.334		0.427		-0.04	14	-0.118		0.030		-0.038	~~~	0.181		0.066		0.040		-0.078	
		(0.218)		(0.367)		(0.16)	5)	(0.207		(0.077)		(0.121		(0.147)		(0.216)		0.128)		(0.168)	
	0.317	0.316	0.302	0.3 1265	0.34	7 0.34	7 0.41 7 937'	3 0.412 7 9270	0.464 8520	0.464	0.487	7934	0.366	0.366	0.405	0.405	0.324	0.324 10841	0.345 0227	0.345	
	473	473	419	412	113	1131	1 951	951	4070	4070	3440	3440	1674	1636	1408	1408	3636	11001	000 J	070C	

Inductive	Granor	nt Wondhor	icine & Cton	no. Doctol	Contro	Tab.	le 40 :	Labc	or pro-	ductiv	$\frac{1}{1}$ ity by	<u>v</u> indu	Istry IV	~	2 col Fetot	Come		Iomo	1 & Accord	tine Con	
Industry	Iranspc	rt., Warehot	ISING & STOL	age; Fostal	Serv.	Accomme	odation &	Food Sei	VICES	Infor	mation &	Communit	cation		teal Estat	e Serv.	-	Lega	I & Accou	nting Ser	
	Bas	eline	Ω	onut		Baselin	e	Dont	ŧ	Base	line	Õ	nut	Base	ine	Donu		Baseli	ne	Don	t
All periods (γ)	0.064 (0.150)	- <u>9</u>).006).178)			.327** \.129)		0.540^{**} 0.254		-0.320^{**} (0.142)		-0.357^{**} (0.160)		0.157 (0.251)		-0.018 0.351)		.0.053 0.087)	Ϋ́	0.143	
Period 3 (γ_3)		0.148		0.130		ې ۲	0.379*		1.029^{**}		-0.453		-0.643**		0.628		0.341		-0.086		-0.383^{*}
		(0.224)		(0.296)		J)	(0.200)		(0.437)		(0.279)		(0.296)		(0.441)	-	0.553)	0	(0.139)		(0.232)
Period 4 (γ_4)		-0.001		-0.066		Ĩ	0.273		-0.085		-0.225		-0.216		-0.206		0.316		-0.017		-0.197
		(0.190)		(0.213)		J	(1.168)		(0.272)		(0.153)		(0.193)		(0.317)	<u> </u>	0.464)	<u> </u>	(0.109)		(0.180)
R^2_{adj} AIC	0.413 6689	0.413 (6690 ().425 3196	0.425 6198) 7).359 (1329 4).359 4331	0.365 3795	0.368 3792	0.513 4137	0.513 4138	0.553 3632	0.554 3632	0.422 2602	0.424 2601	0.433 (2363	0.433 (2364 (0.585 2072	0.585 2073	0.607 1706	0.607 1708
Ν	2210	2210	2045	2045		1540	1540	1321	1321	1495	1495	1361	1361	746	746	671	671	1129	1129	919	919
Note: $*** p < 0.01$, (year-specific) optim	$p < 0.05, p^*$ d bandwidths	< 0.1. The table summed over all	shows the treatr years. Year-spec	nent effect of E. ific functional fe	PL on the res urms of the ce	pective outco ntered runnir.	me, estimate g variable we	d by OLS. Re sre chosen acc	obust standar ording to the	d errors cluste AIC.	ered at the es	tablishment le	evel are reported	l in brackets.	N refers to th	e number of e	observations a	at the			
						Tab	ile 41:	Lab(or pro	ducti	vity b	y indı	ustry V	r							
Industry	Mana	gement Const	ultancy Serv.	Archit	ectural &	Engineerit	lg Serv.	Sci	ientific R&	JD Serv.	ΡЧ	vert. & Mi	arket Resear	ch; Other	Prof., Scie	tific & Te	chn. Serv.		Travel & C	ther Serv	
_	Base	line	Donut	B	seline	Doi	ut	Baseli	ne	Donut		Baseline			Donut			Bas	seline	Dor	nt
All periods (γ)	-0.381	Î	0.552	-0.095		-0.101		0.689^{*}		0.800	0	142	0.078					-0.064		0.000	
_	(0.349)	E).434)	(060.0)	_	(0.133)		(0.366)	<u> </u>	(0.537)	0	(921)	(0.223)					(0.148)		(0.198)	
Period 3 (γ_3)		0.425	0.2(33	-0.190		-0.204)).911**	-i -	081*	-0.1	07		0	016			0.024		0.240
		(0.412)	(0.64)	(61	(0.147)		(0.240)		(0.440)	0	.613)	(0.2	67)		0)	321)			(0.263)		(0.377)
Period 4 (γ_4)		-1.153***	-1.02	**0	-00.00		-0.018		-0.138	Ϋ́).365	0.35	17*		0	210			-0.116		-0.115
0.4	0000	(0.396)	(0.45	(<u>)</u>	(0.136)		(181.0)	0000	(0.499)	0)	.931)	(0.2.	27)		0	359)		1	(0.167)	. 10 0	(0.218)
R^{ℓ}_{adj}	0.369	0.383	0.384 0.38	88 0.513 - 0.513	0.513	0.511	0.511	0.263	0.265	0.282 0	284 0.	571 0.5	72 0.573		o' '	572		0.255	0.255	0.254	0.254
ALC N	437	437	405 400	1 3/08 5 1575	3709 1575	3400 1401	$3402 \\ 1401$	1787 482	1/80 482	1509 I 423 A	123 14	JU6 10(41 44	1 405 J24		2 4	05		4/45 1592	$\frac{4}{1592}$	4400 1466	4407 1466
Note: ${}^{***}_{p} < 0.01, {}^{**}_{p}$ over all years. Year-sp	< 0.05, * $p < 0.05$	1.1. The table show forms of the cen	vs the treatment e tered running vari	ffect of EPL on t able were chosen	he respective (according to 1	outcome, estim the AIC.	ated by OLS.	Robust stand	ard errors clus	tered at the es	tablishment le	vel are reported	1 in brackets. N 1	efers to the m	mber of observ	ations at the (year-specific)	optimal band	widths summe	Ŧ	
						Tab	le 42:	Labo	r pro	ductiv	ritv by	r indu	strv V	—							
Industry		Educati	on	Ħ	ealth & S	ocial Serv	ices	Arts.	Entertain	1 ment & I	Secreation	Rep	hir of Comp	- uters: Per	sonal & H	ousehold (Goods	ð	ther Perso	nal Serv.	
Baselin		Donut		3aseline	Ď	onut	Bas	seline		Donut		Baseline		onut		Basel	ine		Donut		
All periods (γ)	0.159).202	0.008		-0.088	-	-0.303		-1.131	**	-0.0	53 0)	0.126	-			0.094	ΤS	0.132	
Period 3 (γ_{2})	(101.104)	0.073	.0.0- -0.0	70 (U.U54	0.083	(071.0)	-0.148	(RIG.U)	0.296	(0.44:	ربا -0.91	2.0) [0.2.	0.205	(0.278)		0.385	<u>۔</u>	(e)T'N	0.101	1.234)	0.159
		(0.253)	(0.45	(0)	(0.112)		(0.166)		(969.0)	_	(0.75	(0)	(0.366)	_		(0.381))	(0.195)	0	(0.293)
Period 4 (γ_4)		0.237	0.40)4	0.181		0.031		-1.493**	**	-1.27	2**	-0.343			-0.114			-0.535	Ţ).882**

					0.159	0.293)).882**	0.423)	0.487	1930	781
	onal Serv.	ut	-0.132	(0.234)		_	Ť	_	0.485	1931	781
)ther Pers	Dont		-	0.101	(0.195)	-0.535	(0.332)	0.482	2080	839
			-0.094	(0.175)					0.481	2081	839
	& Household Goods	Baseline			0.385	(0.381)	-0.114	(0.432)	0.612	527	221
	tters; Personal	mut	0.126	(0.278)					0.613	523	221
- · ·	of Compu	Do			0.205	(0.366)	-0.343	(0.277)	0.62	553	235
	Repair	eline	-0.053	(0.212)					0.621	552	235
ر الاتراك	reation	Bas			-0.959	(0.750)	-1.272**	(0.575)	0.242	974	291
	ent & Rec	ut	-1.131^{**}	(0.449)					0.245	974	291
	Arts, Entertainme	Don			0.296	(0.696)	-1.493^{***}	(0.513)	0.244	1123	331
	ervices Arts,]	seline	-0.303	(0.519)					0.236	1128	331
		Donut Base			-0.148	(0.166)	0.031	(0.168)	0.423	4964	2348
	ocial Servi		-0.088	(0.120)					0.423	4963	2348
	ealth & So	le D			-0.083	(0.112)	0.181	(0.121)	0.411	5554	2636
	H	seline	0.008	(0.084)					0.41	5555	2636
		Bas			-0.070	(0.450)	0.404	(0.302)	0.478	1166	439
	ucation	onut	0.202	(0.256)		_		_	0.478	1165	439
	Edu	Ц		_	0.073	(0.253)	0.237	(0.231)	0.48	1324	505
		ne) 0.159	(0.164)					0.481	1322	505
	Industry	Baselin	All periods (γ)		Period 3 (γ_3)		Period 4 (γ_4)		R^2_{adi}	AIČ	Ν

Note: *** p < 0.01, ** p < 0.05, ** < 0.1. The table shows the treatment effect of EPL on the respective outcome, estimated by OLS. Robust standard arrors clustered at the establishment level are reported in brackets. N refers to the number of observations at the (year-specific) optimal bandwidths summed over all years. Yar-specific functional forms of the centered running variable were chosen according to the AIC.







Year

Year

2004 2006 Year























Figure 34: Descriptive evidence for treatment effect: Log(capital stock)











Figure 40: Descriptive evidence for treatment effect: Log(labor productivity)



