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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 country- or 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 (4th 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 interpreted 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 full-time 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.

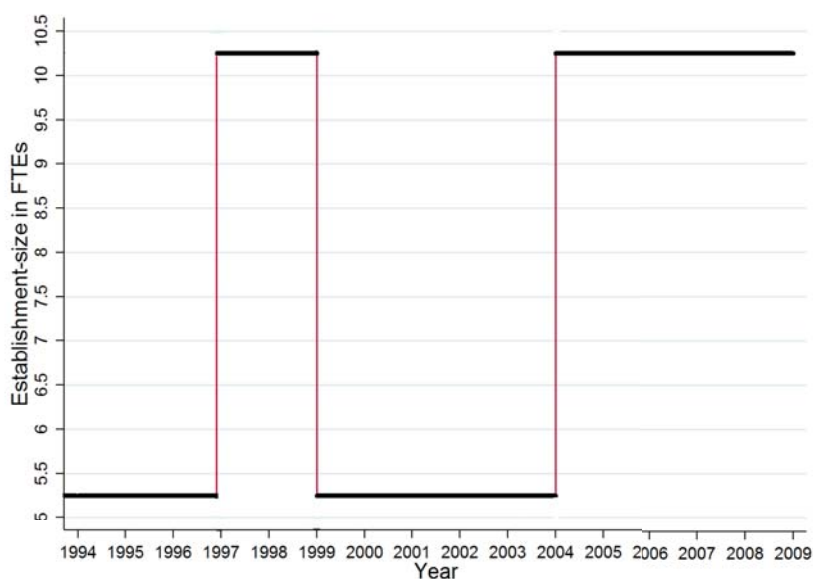
¹²Given that they comply with the moderate regulations of the Civil Code, which lays down a set of minimum standards that have to be met when dismissing workers, e.g. respecting the notice period (Bauer et al., 2007).

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 special 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 work 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 ≥ 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 reference day of the next survey date is an eight months gap. The gap between the reference 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.

²⁰I 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.

²²As mentioned in the previous section, the LIAB does not contain information about all employees in a given establishment and year.

²³This 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 self-select below the size threshold in order to escape EPL. To re-assure that this assumption holds, I conduct two tests. 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.

5.1 Empirical Model for the Establishment-Level Data

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

$$Y_{jt} = \alpha_t + f_t(F_{jt} - c_t) + \gamma D_{jt} + \epsilon_{jt} \quad (1)$$

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 γD_{jt} , where γ_t is the treatment effect for the respective period or 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.

5.1.1 Outcome Measures

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 interpreted 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 interpreted 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 Guertzen (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 Guertzen (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 sector-specific 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 employer-employee 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:

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

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

²⁹I 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 paid 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.

³¹If 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

³²Using 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.

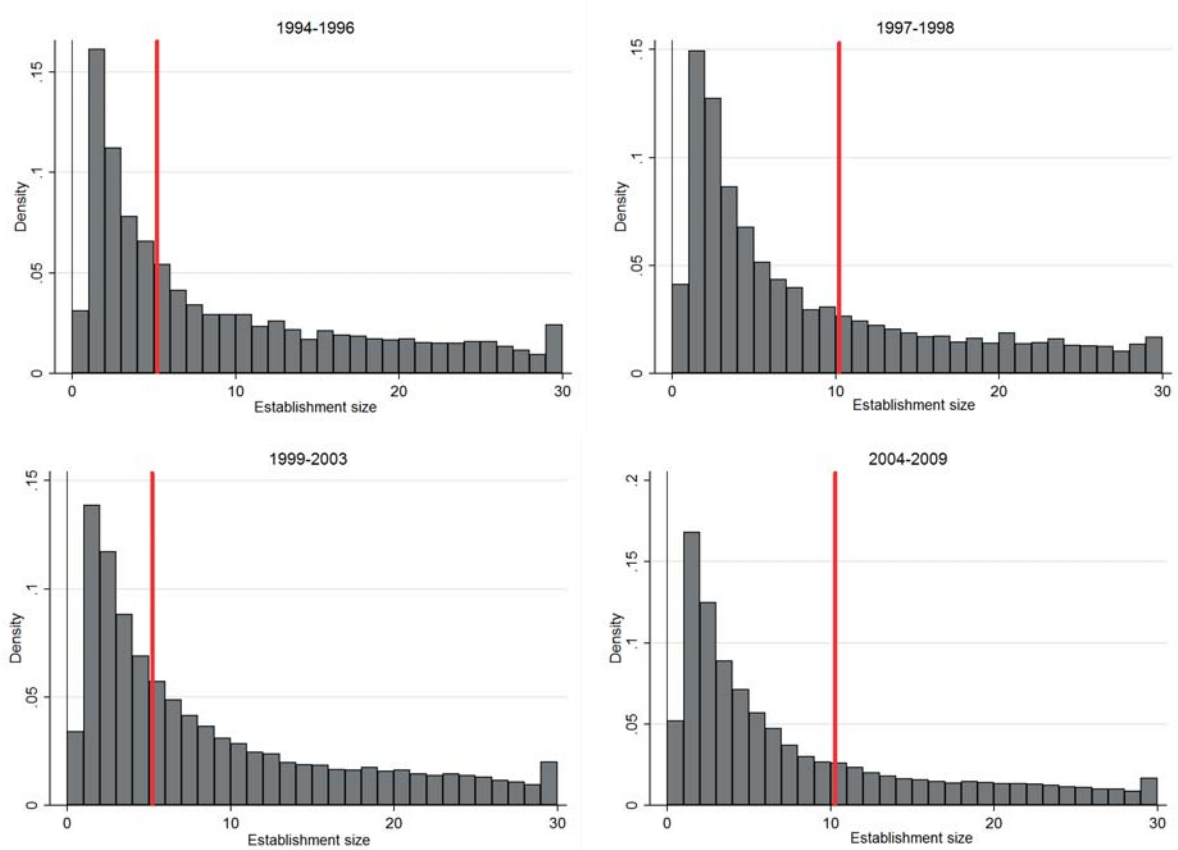
6 Results

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 sub-section. Subsequently, I present the main results of this paper: the effect of 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.

Table 2: McCrary test by year

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

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

³⁵As 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.

Table 3: Optimal bandwidth & functional form

Year	Capital stock	Investment flows	Expansion investment	ICT investment	Labor productivity	Establ.-level monthly wage	Indiv.-level wage	Job reallocation rate	Temporary workers	White-collar workers
	bandw. ff.	bandw. ff.	bandw. ff.	bandw. ff.	bandw. ff.	bandw. ff.	bandw. ff.	bandw. ff.	bandw. ff.	bandw. ff.
1994	9.81	10.55	-	-	8.75	7.70	3.01	7.82	-	-
1995	7.79	11.76	-	-	9.78	8.64	1.94	7.19	-	-
1996	9.23	8.29	12.24	-	8.62	7.47	2.79	4.98	18.92	-
1997	7.24	10.50	12.38	-	6.09	7.70	1.15	5.30	16.54	-
1998	6.57	9.93	11.45	-	7.57	7.81	1.30	4.34	17.20	-
1999	7.33	9.37	11.44	-	7.12	3.92	1.53	3.98	15.75	-
2000	7.14	8.22	8.22	9.39	5.35	3.14	1.88	4.69	13.63	-
2001	7.25	9.03	10.42	9.59	6.75	3.28	2.15	4.46	13.95	-
2002	7.12	9.57	9.02	10.27	6.22	3.02	1.34	4.34	12.71	-
2003	8.52	8.65	12.63	8.13	6.63	2.53	1.62	4.21	12.81	11.98
2004	6.62	9.85	11.60	9.07	6.43	7.76	2.55	3.97	13.73	12.69
2005	6.16	8.92	10.05	9.90	6.18	7.11	1.78	3.81	12.60	10.52
2006	6.50	8.26	6.92	9.92	5.37	7.10	2.18	3.80	11.80	11.45
2007	5.13	6.62	8.07	-	6.16	6.80	2.35	3.82	10.92	9.58
2008	6.23	7.91	7.68	-	5.76	7.01	1.82	3.64	10.62	10.42
2009	5.96	7.49	7.09	-	6.27	6.89	2.01	3.60	10.01	10.12

Note: Optimal bandwidth (bandw.) was chosen according to the bandwidth choice rule of Imbens and Kalyanam (2010). To find the optimal functional form (ff.), I run RDD regressions for each dependent variable including first (linear), second (squared) and third (cubic) order degrees polynomials of the centered running variable, i.e. establishment-size. I also allow these different functional forms to differ on each side of the threshold (inter.).

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 size-contingent 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 .

Table 4: Employment margin

	Job reallocation rate		Log(# of temp. workers)		Log(# of white-collar workers)	
Available time period	1994-2012		1996-2012		2003-2012	
	(1)	(2)	(3)	(4)	(5)	(6)
All periods (γ)	-0.008 (0.006)		0.003 (0.009)		-0.045*** (0.017)	
Period 1 (γ_1) (1994-1996)		0.014 (0.025)		0.019 (0.023)		- -
Period 2 (γ_2) (1997-1998)		-0.019 (0.024)		-0.032 (0.032)		- -
Period 3 (γ_3) (1999-2003)		-0.016 (0.011)		0.017* (0.010)		-0.025 (0.031)
Period 4 (γ_4) (2004-2012)		-0.006 (0.007)		-0.005 (0.014)		-0.048*** (0.018)
R^2_{adj}	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
N	30496	30496	53210	53210	31094	31094

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.

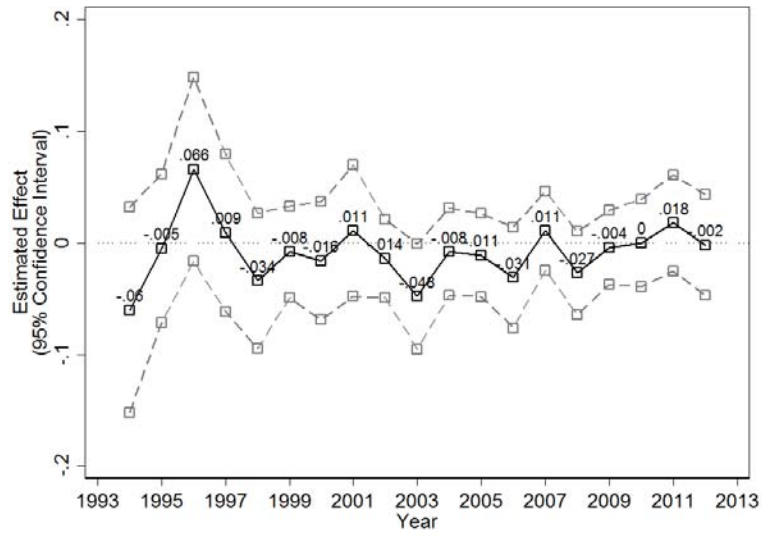
All in all, these results confirm the finding of Bauer et al. (2007) who do not find a statistically significant effect of EPL on establishment level job flows. Following the argument of Autor et al. (2007), it would not be surprising if EPL had not effect on the on the main outcomes analyzed in this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

6.4 The Effect of EPL on Capital Investments and Labor Productivity

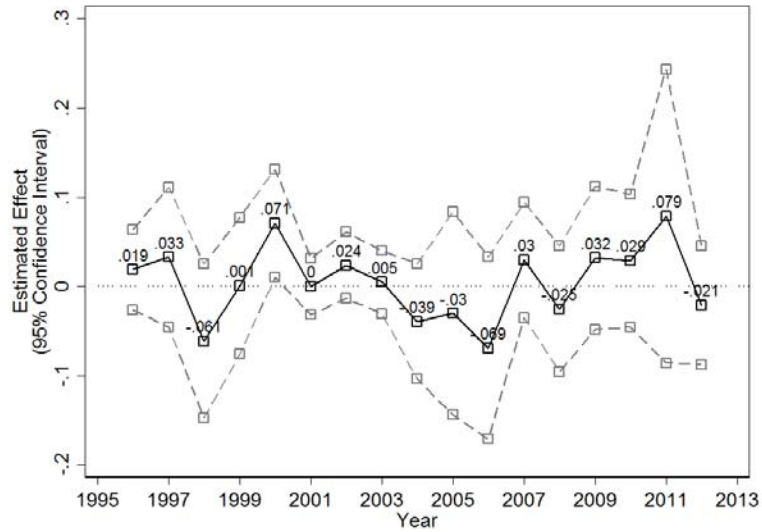
Table 5 shows the main results of this paper: the effect of EPL on capital investments and labor productivity. Columns 1 to 4 show the results of EPL on the logarithm of the capital stock and of the total capital investments (flow measure), respectively. Columns 1 and 3 show the results of the overall 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 re-regulated 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.

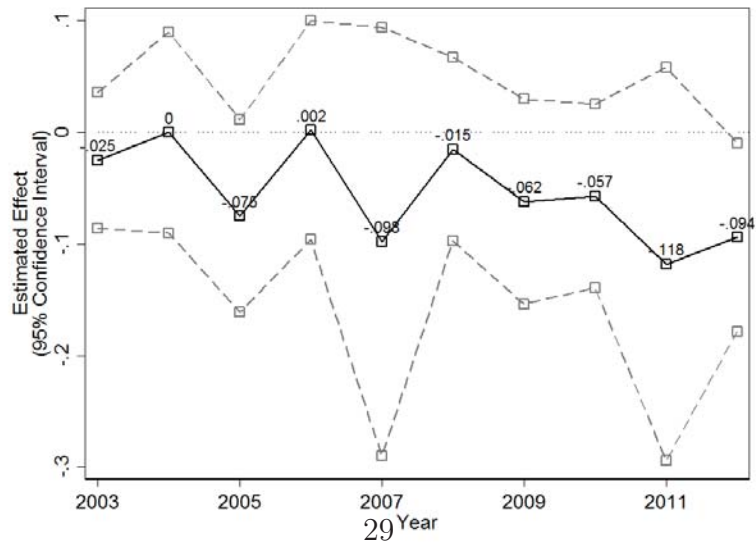
Figure 3: Year-specific effects of EPL on the employment margin



(a) Job reallocation rate



(b) Log(# of temporary workers)



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

Table 5: Investments and labor productivity

Time period	Stock measure		Flow measures							
	Log(capital stock)	Log(total capital investment)	Log(expansion investment)		Log(ICT investment)		Log(value-added)			
	1994-2012	1994-2012	1996-2012		2000-2006		1994-2012			
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
All periods (γ)	-0.038 (0.030)	-0.056 (0.092)	0.019 (0.102)		0.276** (0.135)		0.014 (0.025)			
Period 1 (γ_1) (1994-1996)	0.056 (0.096)	0.196 (0.302)	-0.410 (0.344)						0.112 (0.077)	
Period 2 (γ_2) (1997-1998)	-0.293*** (0.102)	-0.520* (0.297)	-0.621* (0.363)						-0.124 (0.079)	
Period 3 (γ_3) (1999-2003)	-0.074 (0.050)	-0.204 (0.133)	0.022 (0.165)		0.077 (0.161)				-0.062 (0.044)	
Period 4 (γ_4) (2004-2012)	0.021 (0.050)	0.198 (0.164)	0.168 (0.156)		0.856*** (0.270)				0.074** (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
N	58502	58502	106325	106325	54066	54066	22929	22929	53624	53624

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.

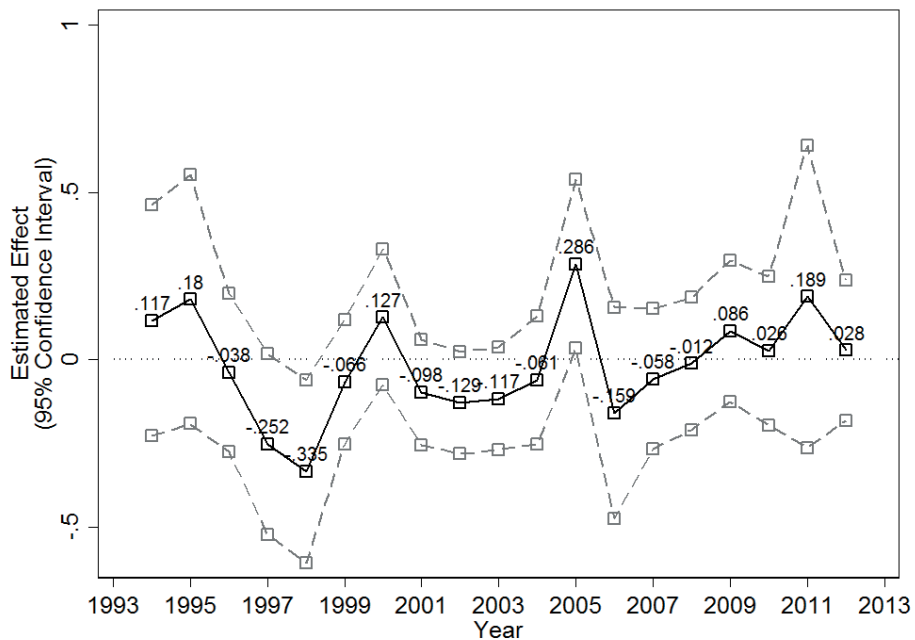
The period-specific treatment effects, γ_1 - γ_4 , do a statistically significant effect either.

Figures 4a and 4b show the year-specific treatment estimates of EPL for the capital stock and the total capital investments, respectively.

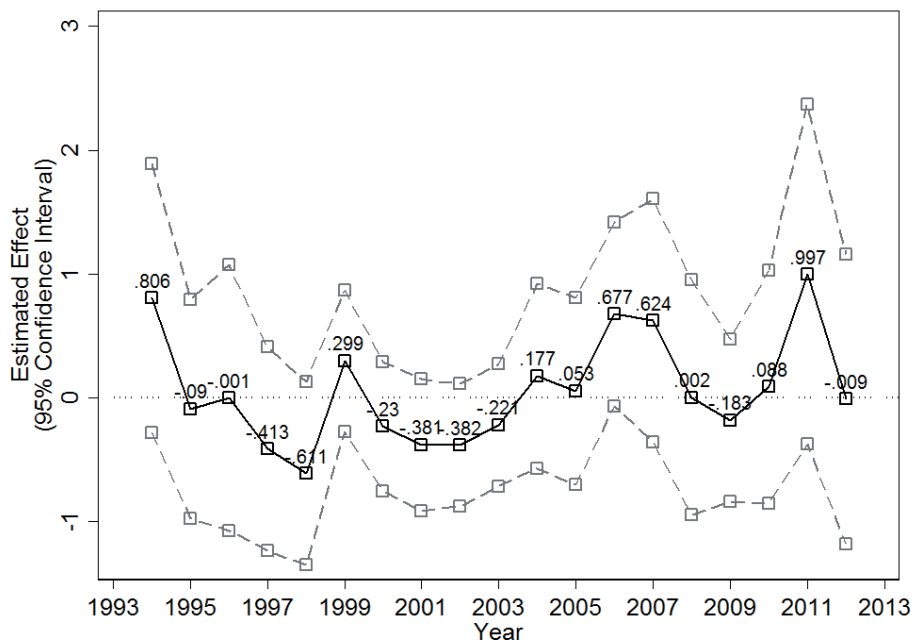
Examining the sub-components of capital investments, namely expansion investments and ICT investments, the overall treatment effect, γ , for both outcomes is positive but statistically insignificant (Table 5). The magnitude of the effect of EPL on ICT investments statistically significant and huge, suggesting 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

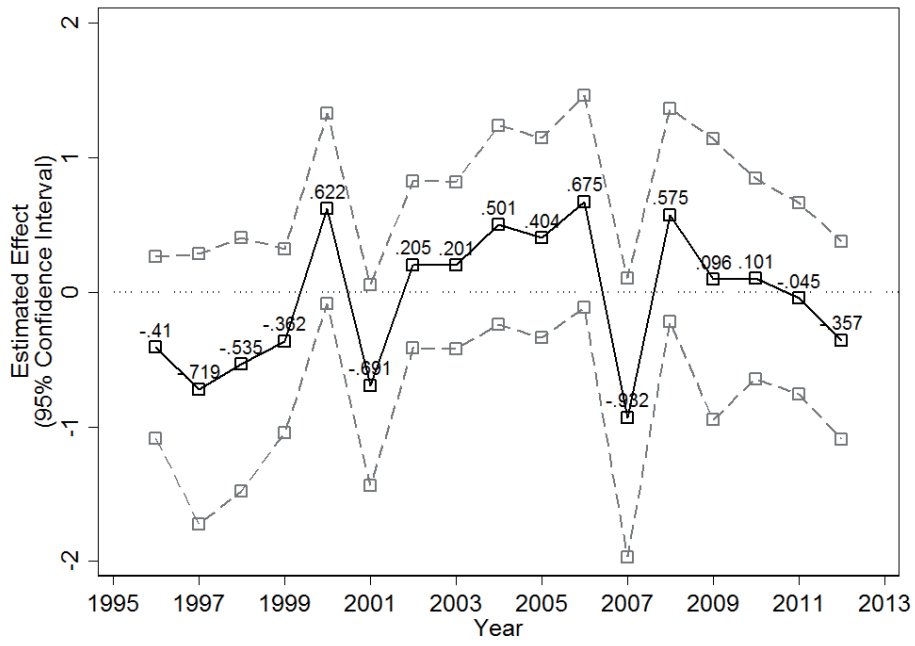


(a) Capital stock

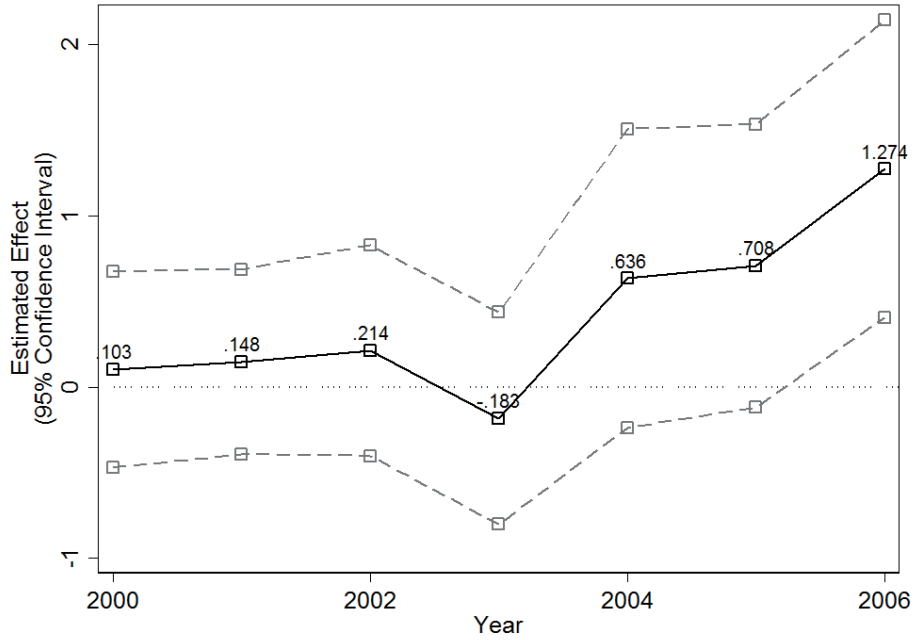


(b) Capital investment flows

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

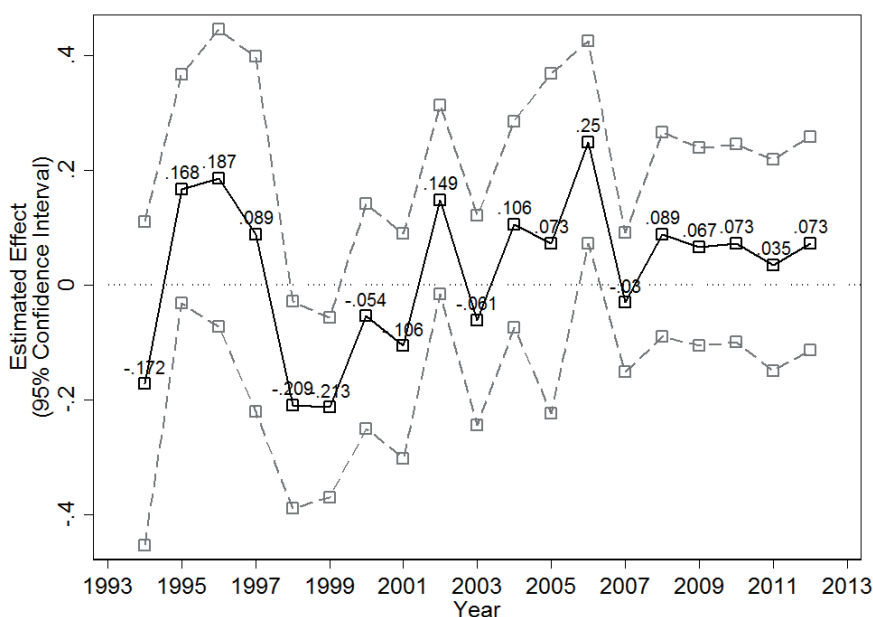


(a) Expansion investment



(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 full-time 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).

Table 6: Establishment- and individual-level wage

	Establishment-level		Individual-level	
	Log(<i>gross wage_{jt}</i>)		Log(<i>wage_{ijt}</i>)	
Available time	1994-2009			
All periods (γ)	-0.006 (0.018)		-0.018 (0.046)	
Period 1 (γ_1) (1994-1996)		0.127 (0.084)		0.001 (0.088)
Period 2 (γ_2) (1997-1998)		0.037 (0.049)		-0.115 (0.107)
Period 3 (γ_3) (1999-2003)		-0.079*** (0.027)		-0.154** (0.072)
Period 4 (γ_4) (2004-2009)		0.035 (0.027)		0.045 (0.068)
R_{adj}^2	0.611	0.611	0.015	0.016
AIC	54226	54219	31422	31410
# of establishments	33267	33267	1912	1912
# of persons	-	-	20188	20188

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

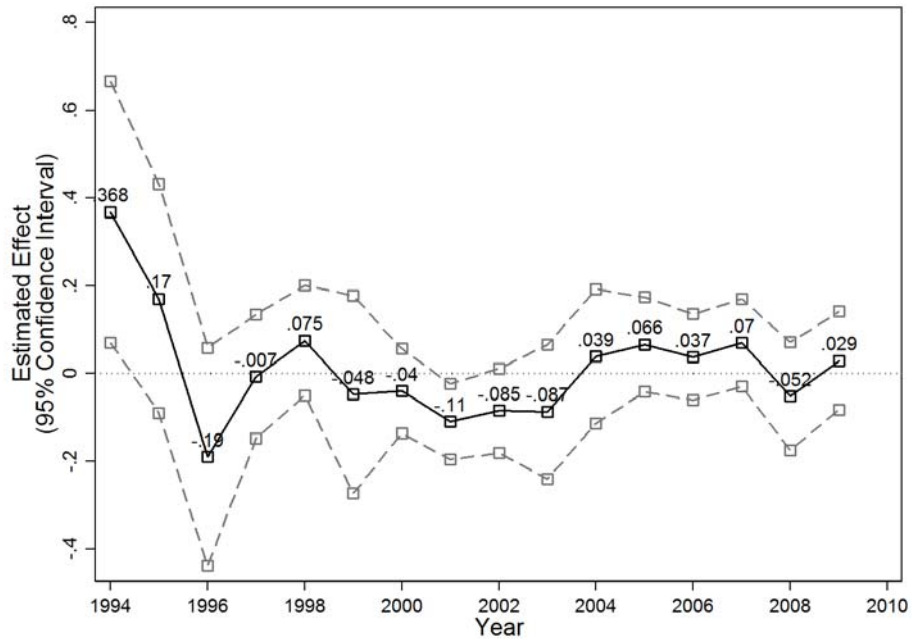
⁴¹This could be the result of over-fitting of the model the model.

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).

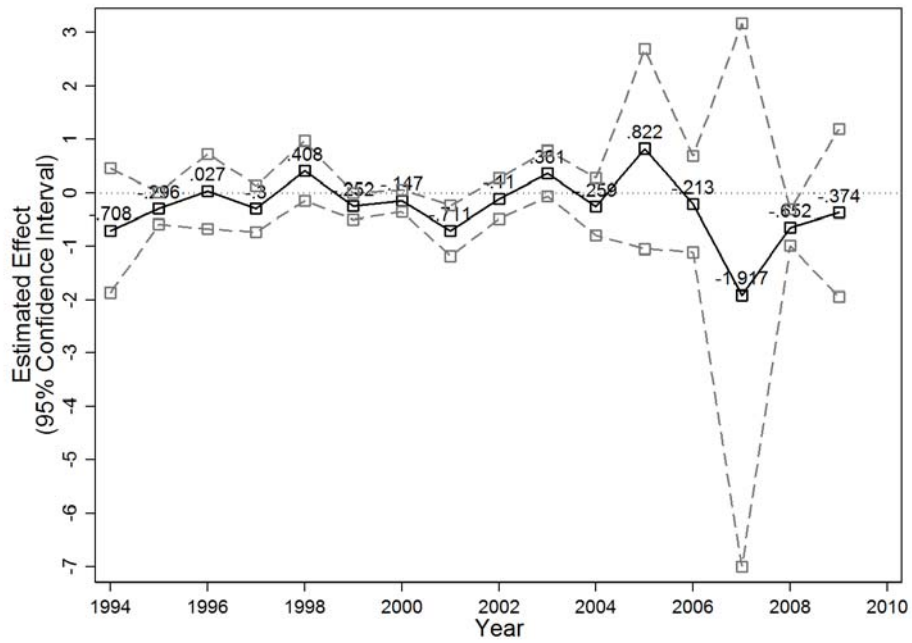
All in all, I neither find a statistically significant effect of EPL on wages at the establishment level, nor on the insiders' wages at the individual worker level. From a theoretical perspective, the nonexistence of an effect of EPL on wages suggests that hold-up is not a problem in this paper. This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

Figure 7: Year-specific effects of EPL on the establishment- and individual-level wage



(a) Establishment-level (gross) wage



(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 (with 5-20 workers) 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. In the first 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

Table 7: Weighting scheme for part-time workers

	Before Oct. 1996	Between Oct.96 & Dec.98		Between Jan.99 & Dec.03		After Jan 04	
Thershold (# of FTEs)	5.25	10.25		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	<20h-0.5	<25h-0.6	<20h-0.5	<25h-0.6
& weights (% of a FTE)	>15h-1.0	<20h- 0.5	<25h- 0.6	<30h-0.75	<30h-0.75	<30h-0.75	<30h-0.75
(Appl. weight= PADA)		≤30h-0.75	≤30h-0.8	≤30h-0.75	≤30h-0.75	≤30h-0.75	≤30h-0.75
		>30h- 1	>30h-1	>30h-1	>30h-1	>30h-1	>30h-1

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

West		East	
Time period	Upper earning limit (Euros per year)	Time period	Upper earning limit (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).

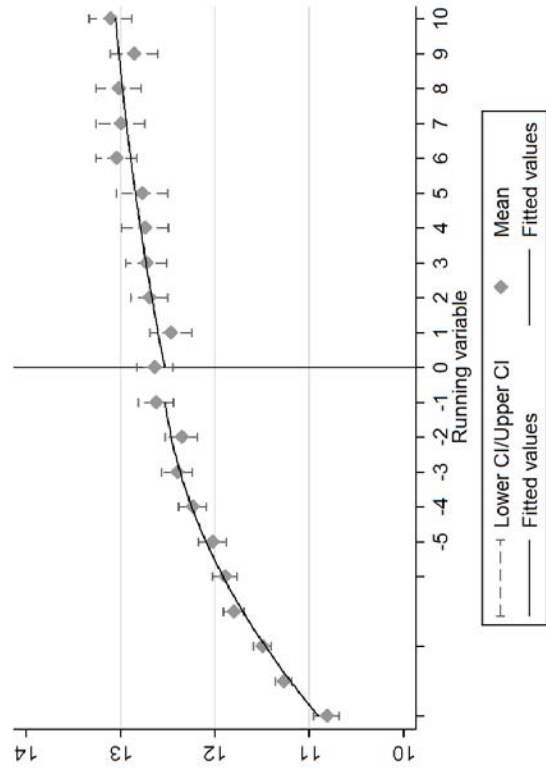
C Summary statistics and Graphs Descriptive Evidence for Treatment Effect

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.

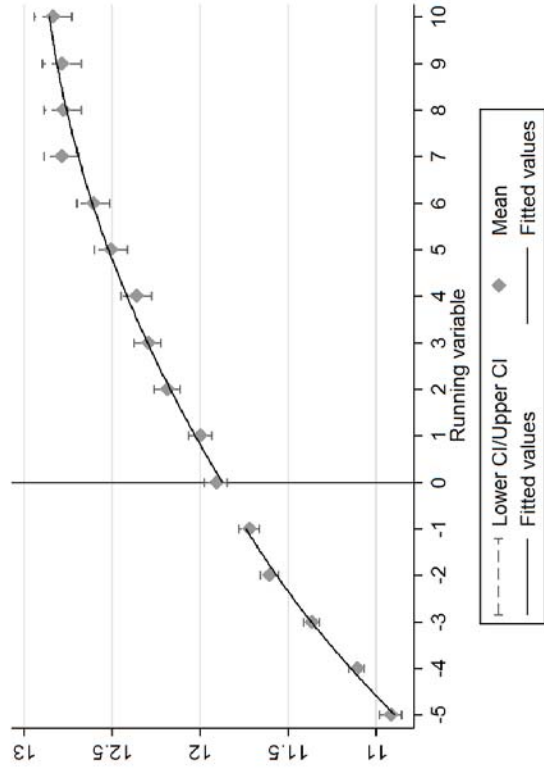
Table 9: Descriptive statistics by treatment and period

Variable	Periode 1				Periode 2				Periode 3				Periode 4			
	Below threshold		Above threshold		Below threshold		Above threshold		Below threshold		Above threshold		Below threshold		Above threshold	
	N	Mean SD	N	Mean SD	N	Mean SD	N	Mean SD	N	Mean SD	N	Mean SD	N	Mean SD	N	Mean SD
Log(capital stock)	1778	12.6 1.5	2932	11.7 1.4	997	12.8 1.4	2225	12.2 1.4	8273	12.4 1.4	13647	11.4 1.3	7733	12.8 1.4	18161	12.0 1.3
Log(investment flows)	2415	7.1 5.1	4290	5.478 4.92	1705	7.7 5.9	5815	5.3 4.9	11318	6.2 5.2	19046	4.2 4.8	12219	6.7 5.3	48128	4.6 4.9
Log(expansion inv.)	1087	3.277339 4.8	2049	2.7 4.2	1201	5.6 5.2	2942	4.8 4.7	6694	4.9 5.1	7983	4.1 4.7	8104	4.8 5.3	25027	3.7 4.6
Log(ICT inv.)	-	-	-	-	-	-	-	-	6806	5.1 4.1	5790	6.2 4.3	8202	5.0 4.1	2841	6.4 4.3
Log(labor productivity)	1298	13.0 1.2	2308	11.6 1.3	711	12.3 0.8	1824	12.5 1.0	6387	12.8 1.1	13569	11.5 1.2	7167	13.4 0.8	17963	12.4 0.9
Log(gross wage)	1096	9.8 0.6	2080	8.2 1.1	910	10.3 0.4	2385	9.2 0.7	3488	9.6 0.5	6781	8.7 0.7	8298	10.3 0.5	25179	9.0 0.8
Log(individ. wage)	509	4.12 0.48	597	4.25 0.43	676	4.25 0.36	409	4.24 0.40	3822	4.11 0.49	3967	4.15 0.48	5122	4.14 0.58	5086	4.18 0.58
Reallocation rate	714	0.1 0.2	1361	0.2 0.3	535	0.2 0.2	949	0.2 0.3	3922	0.2 0.2	9772	0.2 0.3	4710	0.1 0.2	7784	0.1 0.2
Log(# temporary workers)	714	0.1 0.4	1239	0.0 0.1	858	0.2 0.5	1983	0.1 0.3	6985	0.1 0.4	14639	0.0 0.1	7705	0.2 0.5	17435	0.1 0.3
Log(# of white collar workers)	-	-	-	-	-	-	-	-	3689	0.04 0.2	1828	0.2 0.6	16297	0.2 0.5	7478	0.3 0.7

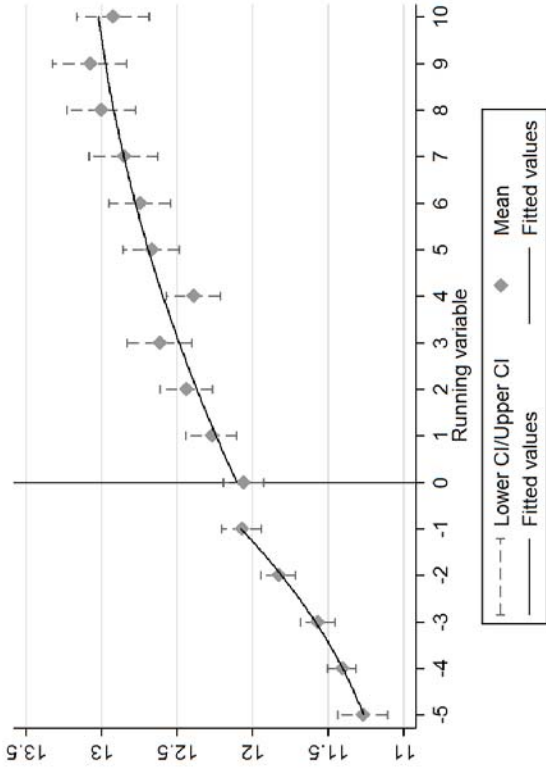
Figure 8: Descriptive evidence for treatment effect: Log(capital stock)



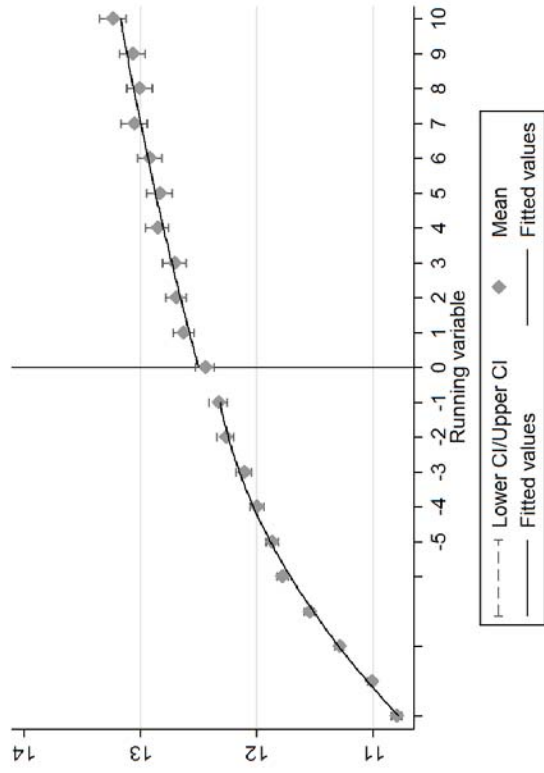
(a) 1st Period (1994-1996)



(b) 2nd Period (1997-1998)

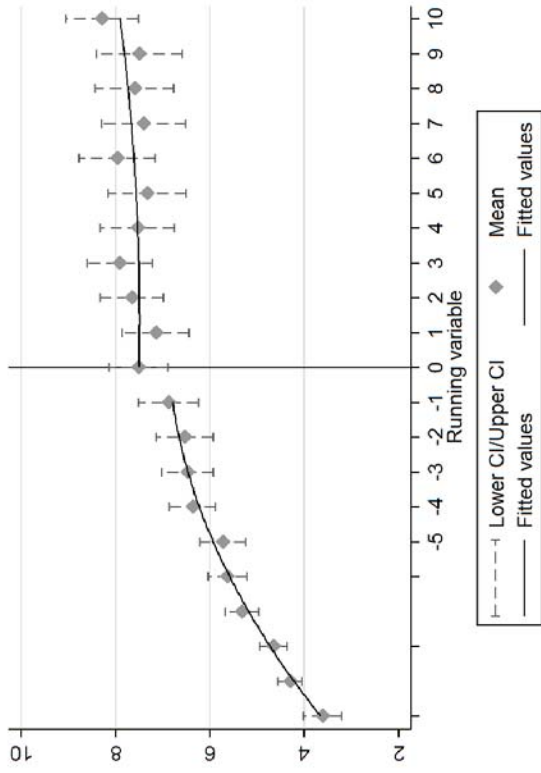


(c) 3rd Period (1999-2003)

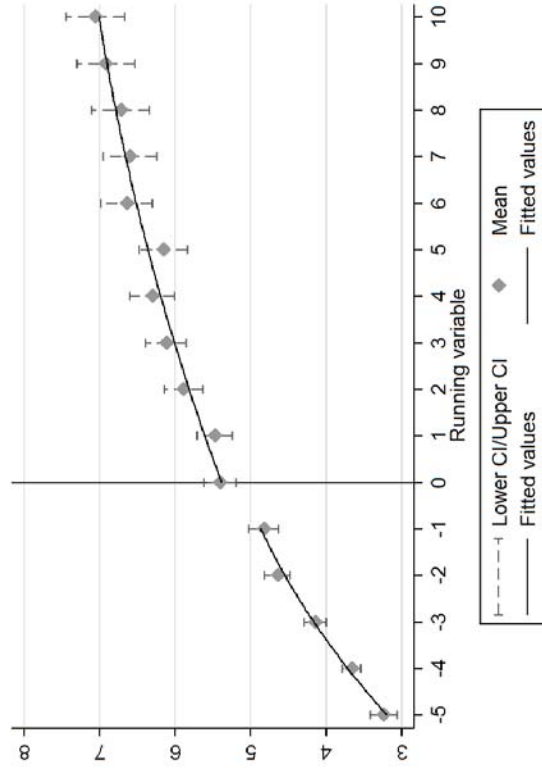


(d) 4th Period (2004-2012)

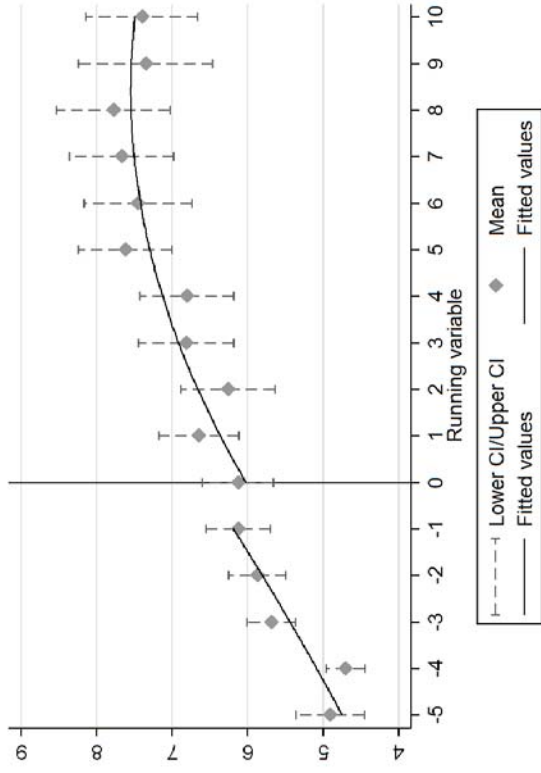
Figure 10: Descriptive evidence for treatment effect: Log(total capital investment)



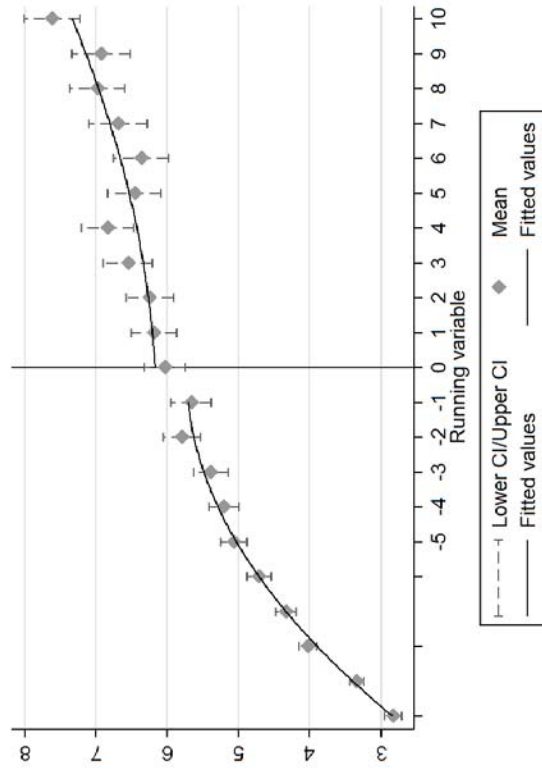
(a) 1st Period (1994-1996)



(b) 2nd Period (1997-1998)

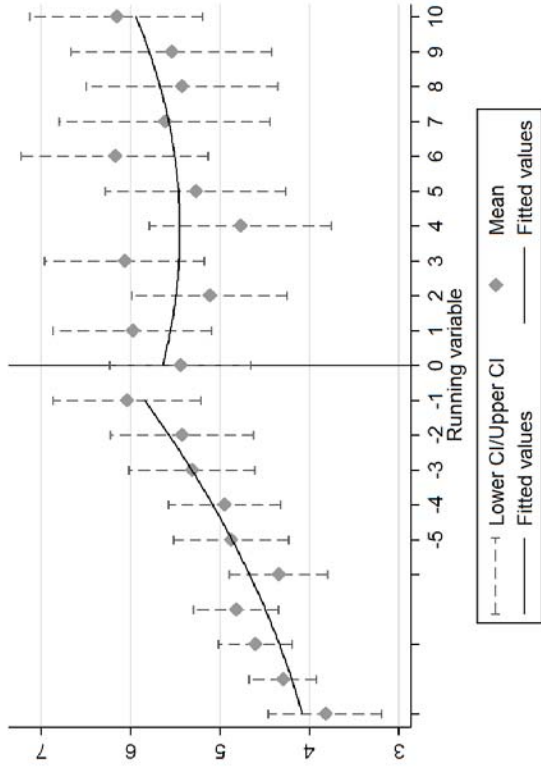


(c) 3rd Period (1999-2003)

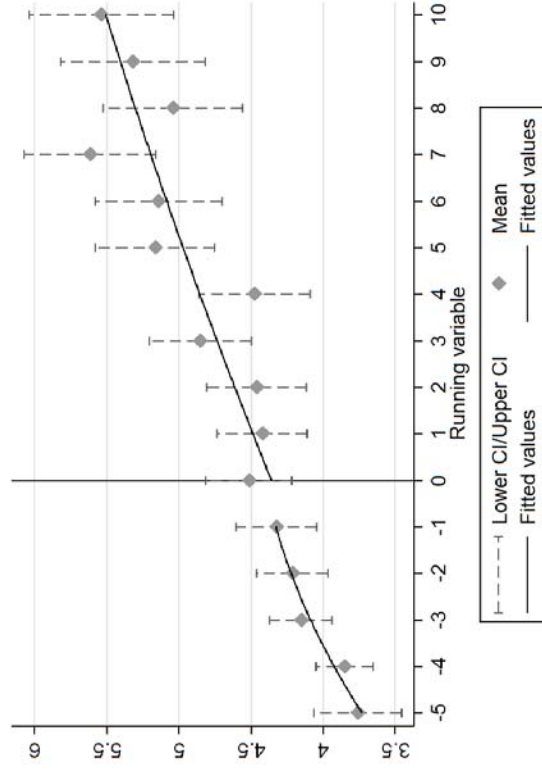


(d) 4th Period (2004-2012)

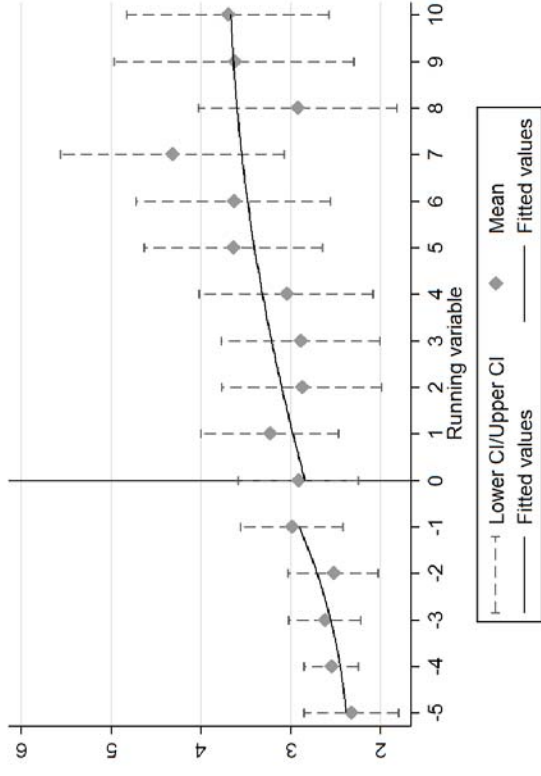
Figure 12: Descriptive evidence for treatment effect: Log(expansion investment)



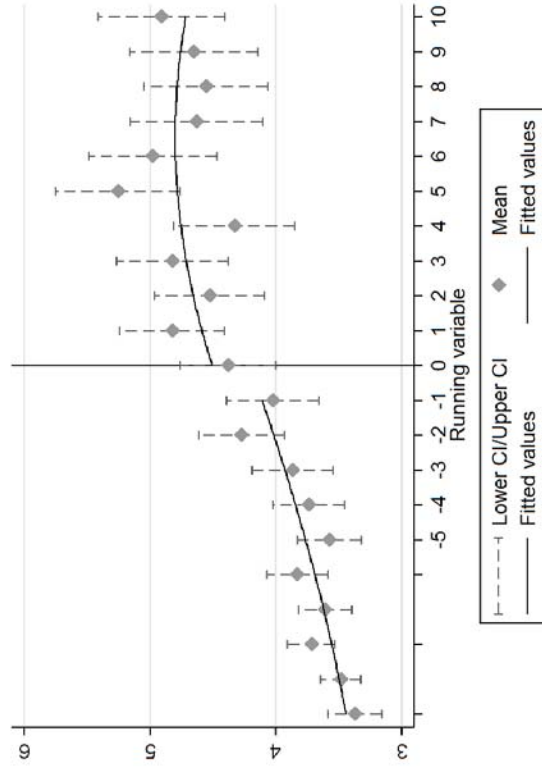
(a) 1st Period (1994-1996)



(b) 2nd Period (1997-1998)

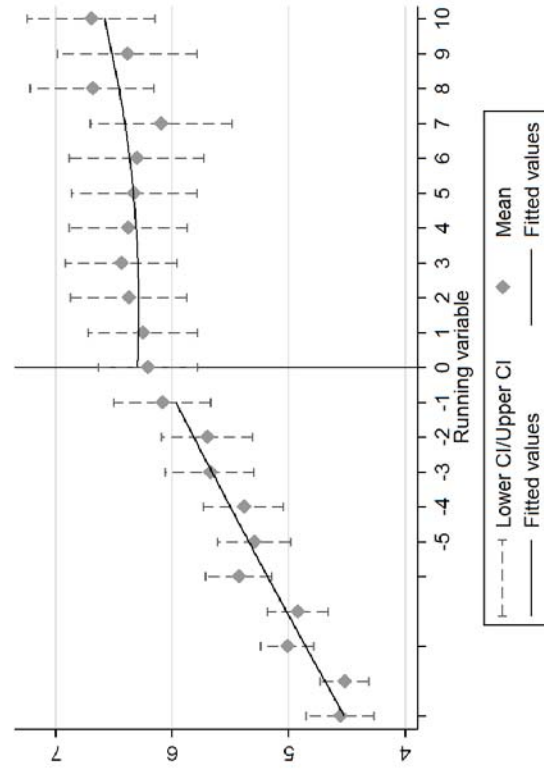


(c) 3rd Period (1999-2003)

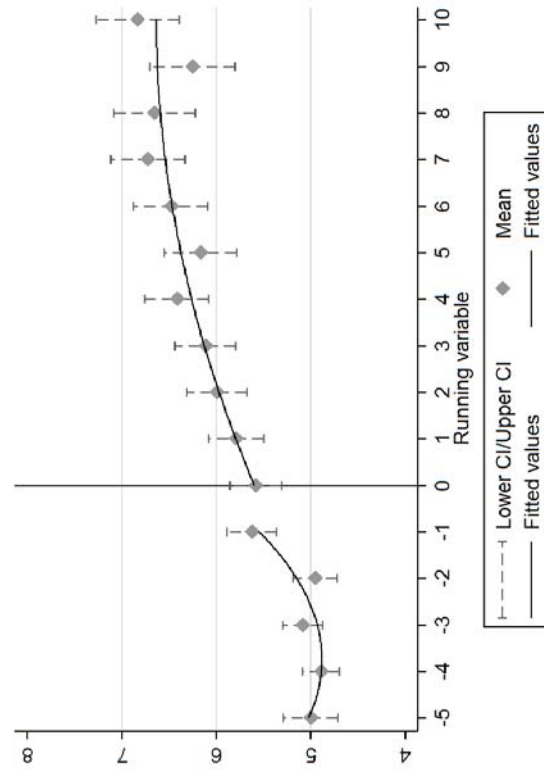


(d) 4th Period (2004-2012)

Figure 14: Descriptive evidence for treatment effect: Log(ICT investment)

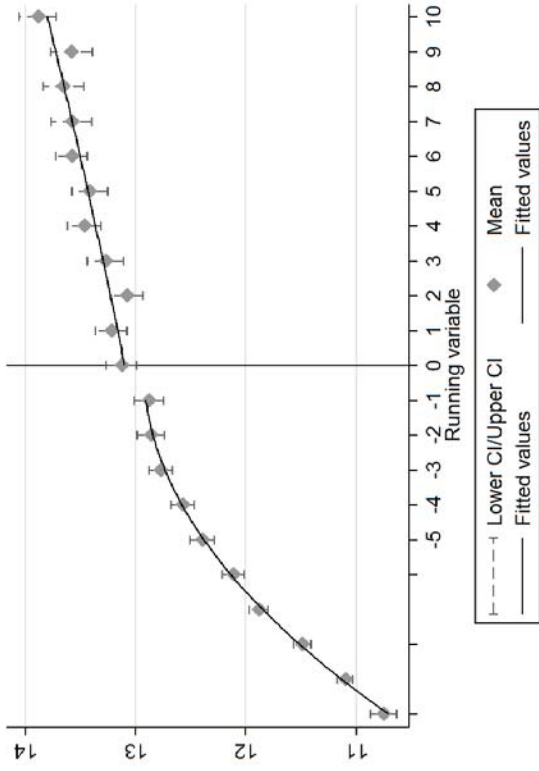


(a) 3rd Period (1999-2003)

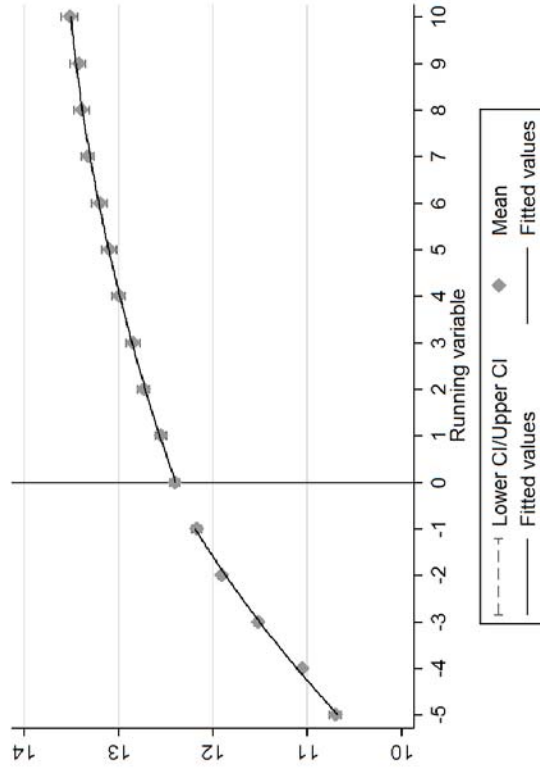


(b) 4th Period (2004-2012)

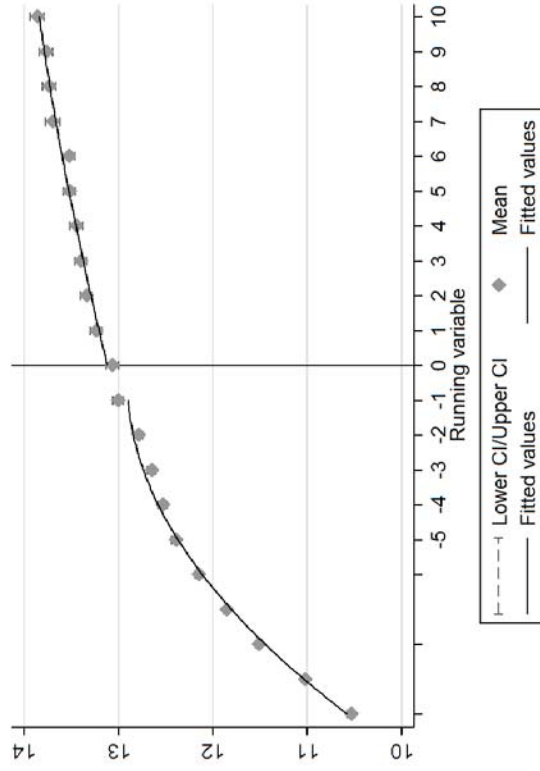
Figure 16: Descriptive evidence for treatment effect: Log(labor productivity)



(a) 1st Period (1994-1996)

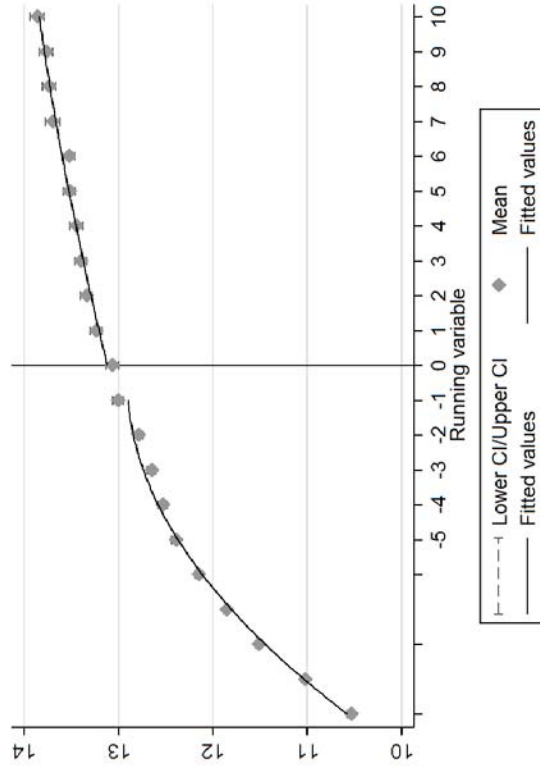


(b) 2nd Period (1997-1998)



(c) 3rd Period (1999-2003)

(d) 4th Period (2004-2012)



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 paid a wage above the collectively agreed one (*wage cushion*), and the share of females (*share fem.*).

Table 10: Continuity of covariates: by industry I

Year	Food, bev.	Textiles	Wood	Paper	Chem & fuel	Rubber & plastics	Other non-metal	Basic metals	Fabric. Metal ex mach.	Mach n.e.c.	Electric. & optical	Transp. Recycl. & equip.	Constr. Trade & restau-rant	Trans & comm.	Finan. & insur. busin. serv.	Social & pers. serv.		
1994	Treat	-0.353	-0.856	0.959	-	-	0.710	0.782	0.508	-6.460**	0.458	-0.641	-7.961	0.316	-0.742***	0.633	0.929**	-0.326
		(0.645)	(1.170)	(0.916)	(0.974)	-	(1.275)	(0.908)	(1.191)	(2.783)	(0.719)	(0.782)	(11.001)	(0.409)	(0.277)	(0.658)	(0.365)	(0.342)
	R^2	0.011	0.063	0.015	0.015	-	0.028	0.007	0.002	0.102	0.006	0.013	0.088	0.014	0.007	0.018	0.013	0.011
	N	1284	1284	1284	1284	-	1284	1284	1284	1284	1284	1284	1284	1284	1284	1284	1284	1284
1995	Treat	-0.410	0.762	-0.153	-0.040	0.324	1.530	-1.675**	-0.202	0.634	0.019	-0.167	-0.612	0.002	-0.081	-0.104	0.478*	-0.679***
		(0.451)	(0.832)	(0.573)	(0.760)	(1.368)	(0.942)	(0.699)	(0.647)	(0.975)	(0.491)	(0.610)	(1.098)	(0.275)	(0.194)	(0.455)	(0.277)	(0.257)
	R^2	0.024	0.012	0.002	0.010	0.035	0.013	0.025	0.025	0.036	0.003	0.007	0.010	0.038	0.005	0.005	0.010	0.026
	N	2828	2828	2828	2828	2828	2828	2828	2828	2828	2828	2828	2828	2828	2828	2828	2828	2828
1996	Treat	-0.807	0.634	-0.506	-0.258	1.641	-3.370	1.863***	0.070	-1.047	0.645	-0.106	0.932	-0.238	0.280	0.102	0.173	-0.366
		(0.556)	(0.956)	(0.657)	(0.980)	(2.567)	(2.739)	(1.054)	(0.690)	(0.721)	(0.462)	(0.645)	(1.108)	(0.271)	(0.206)	(0.521)	(0.287)	(0.284)
	R^2	0.029	0.002	0.005	0.013	0.050	0.041	0.020	0.022	0.038	0.017	0.003	0.026	0.033	0.004	0.006	0.007	0.021
	N	3360	3360	3360	3360	3360	3360	3360	3360	3360	3360	3360	3360	3360	3360	3360	3360	3360
1997	Treat	2.114***	0.462	-1.334**	-1.355	-1.092	1.090	-0.319	-0.239	-1.297*	0.671	-1.057	-0.339	-0.186	0.387	-0.183	1.295***	
		(0.541)	(1.196)	(0.657)	(1.180)	(0.739)	(1.294)	(1.457)	(0.708)	(0.679)	(0.983)	(1.957)	(0.261)	(0.257)	(0.554)	(0.340)	(0.374)	
	R^2	0.048	0.012	0.007	0.007	0.034	0.051	0.016	0.026	0.029	0.035	0.020	0.003	0.010	0.008	0.007	0.026	
	N	3906	3906	3906	3906	4174	3906	3906	3906	3906	3906	3906	3906	3906	3906	3906	3906	3906
1998	Treat	-0.381	2.221**	0.260	1.217	-1.092	-1.415	1.667	-0.180	0.123	-0.277	-0.434	-0.054	1.346	0.316	-0.288	0.471	-0.392
		(0.499)	(1.127)	(0.597)	(0.984)	(0.739)	(1.087)	(1.088)	(0.585)	(0.509)	(0.823)	(0.398)	(0.499)	(1.682)	(0.257)	(0.250)	(0.530)	(0.339)
	R^2	0.034	0.033	0.004	0.020	0.034	0.053	0.020	0.020	0.025	0.045	0.009	0.002	0.025	0.009	0.002	0.005	0.019
	N	4174	4174	4174	4174	4174	4174	4174	4174	4174	4174	4174	4174	4174	4174	4174	4174	4174
1999	Treat	-0.577	-0.317	0.215	0.233	0.742	-2.507***	-0.345	0.403	-0.356	-0.500	0.368	-0.279	-0.290	-0.015	0.076	-0.228	0.331*
		(0.401)	(0.766)	(0.385)	(0.572)	(0.579)	(0.944)	(0.631)	(0.498)	(0.365)	(0.465)	(0.309)	(0.721)	(0.873)	(0.184)	(0.152)	(0.361)	(0.197)
	R^2	0.017	0.007	0.004	0.016	0.019	0.029	0.007	0.016	0.024	0.033	0.008	0.019	0.001	0.017	0.002	0.001	0.004
	N	5505	5505	5505	5505	5505	5505	5505	5505	5505	5505	5505	5505	5505	5505	5505	5505	5505
2000	Treat	-0.192	-0.275	-0.329	1.310**	-0.189	-1.328	-0.011	-0.266	-0.263	-0.165	0.074	0.120	0.646	-0.008	0.317**	-0.127	0.280
		(0.381)	(0.801)	(0.435)	(0.565)	(0.715)	(0.893)	(0.677)	(0.573)	(0.377)	(0.474)	(0.337)	(0.781)	(0.844)	(0.187)	(0.155)	(0.373)	(0.198)
	R^2	0.015	0.002	0.006	0.020	0.012	0.030	0.012	0.014	0.028	0.023	0.008	0.009	0.006	0.013	0.003	0.001	0.017
	N	5976	5976	5976	5976	5976	5976	5976	5976	5976	5976	5976	5976	5976	5976	5976	5976	5976
2001	Treat	-0.250	0.294	-0.194	0.841	0.758	0.033	1.051*	0.573	-0.337	-0.696	0.061	-0.878	0.571	-0.225	0.249	-0.924**	0.187
		(0.331)	(0.826)	(0.448)	(0.542)	(0.630)	(0.675)	(0.607)	(0.585)	(0.391)	(0.555)	(0.357)	(0.996)	(0.867)	(0.187)	(0.153)	(0.402)	(0.184)
	R^2	0.021	0.011	0.005	0.008	0.033	0.039	0.016	0.017	0.021	0.031	0.009	0.009	0.008	0.012	0.001	0.004	0.017
	N	6169	6169	6169	6169	6169	6169	6169	6169	6169	6169	6169	6169	6169	6169	6169	6169	6169

Table 11: Continuity of covariates: by industry II

Year	Food, bev.	Textiles	Wood	Paper & fuel	Chem & plastics	Rubber & non-plastics	Other metal	Basic metals	Fabric. Metal ex mach.	Mach n.e.c. & optical	Electric. & equip.	Transp. Recycl. Constr.	Trade & restaur. comm. busin.	Finan. & insur.	Social & pers. serv. serv.
2002	Treat	0.682** (0.315)	0.779* (0.410)	0.987* (0.510)	0.504 (0.685)	-0.769 (0.788)	0.954* (0.576)	1.158* (0.592)	-0.056 (0.393)	-0.293 (0.523)	-0.991** (1.043)	-0.117 (1.025)	0.131 (0.149)	-0.262 (0.314)	-0.299* (0.168)
	R^2_{pseudo}	0.026	0.010	0.013	0.016	0.022	0.009	0.019	0.016	0.027	0.009	0.011	0.003	0.004	0.012
	N	6771	6771	6771	6771	6771	6771	6771	6771	6771	6771	6771	6771	6771	6771
2003	Treat	-0.013 (0.346)	-0.175 (0.937)	0.658 (0.457)	-0.562 (0.704)	0.366 (0.777)	1.178** (0.589)	0.276 (0.595)	-0.008 (0.394)	-0.990* (0.567)	-0.121 (0.356)	-2.177 (1.464)	-0.158 (0.153)	-0.423 (0.333)	0.316* (0.171)
	R^2_{pseudo}	0.019	0.007	0.006	0.047	0.010	0.006	0.013	0.013	0.035	0.008	0.015	0.004	0.003	0.010
	N	6639	6639	6639	6639	6639	6639	6639	6639	6639	6639	6639	6639	6639	6639
2004	Treat	0.630 (0.402)	0.521 (1.122)	-0.154 (0.528)	-0.098 (0.660)	0.661 (0.658)	1.077 (0.669)	0.500 (0.675)	-0.325 (0.436)	0.793 (0.504)	0.626 (0.404)	-0.041 (1.130)	0.187 (0.182)	-0.224 (0.379)	0.271 (0.226)
	R^2_{pseudo}	0.019	0.013	0.001	0.009	0.030	0.019	0.004	0.019	0.034	0.011	0.008	0.003	0.002	0.008
	N	7292	7292	7292	7292	7292	7292	7292	7292	7292	7292	7292	7292	7292	7292
2005	Treat	0.459 (0.432)	0.069 (1.040)	-0.191 (0.559)	0.477 (0.710)	-0.572 (0.920)	0.891 (0.752)	0.123 (0.744)	0.843* (0.451)	-0.946* (0.551)	0.708 (0.444)	-1.192 (1.375)	0.234 (0.196)	-0.240 (0.421)	-0.139 (0.244)
	R^2_{pseudo}	0.013	0.003	0.002	0.011	0.022	0.010	0.002	0.013	0.029	0.003	0.021	0.002	0.006	0.012
	N	5773	5773	5773	5773	5773	5773	5773	5773	5773	5773	5773	5773	5773	5773
2006	Treat	-0.209 (0.398)	-0.604 (0.918)	0.037 (0.551)	-0.694 (0.744)	-1.032 (0.960)	-0.253 (0.767)	1.214 (0.807)	0.270 (0.449)	0.452 (0.543)	0.923** (0.443)	-0.191 (1.463)	0.198 (0.193)	-0.076 (0.399)	-0.239 (0.252)
	R^2_{pseudo}	0.012	0.003	0.002	0.012	0.044	0.015	0.011	0.015	0.031	0.005	0.007	0.003	0.002	0.014
	N	5897	5897	5897	5897	5897	5897	5897	5897	5897	5897	5897	5897	5897	5897
2007	Treat	-0.582 (0.448)	0.389 (0.890)	-0.801 (0.611)	1.096 (0.847)	-0.237 (1.054)	0.206 (0.732)	0.691 (0.833)	0.406 (0.495)	1.094* (0.628)	0.345 (0.457)	3.429 (3.086)	-0.024 (0.202)	-0.127 (0.447)	0.531** (0.257)
	R^2_{pseudo}	0.002	0.003	0.003	0.009	0.034	0.057	0.021	0.014	0.020	0.003	0.043	0.003	0.004	0.010
	N	4588	4588	4588	4588	4588	4588	4588	4588	4588	4588	4588	4588	4588	4588
2008	Treat	0.557 (0.450)	0.131 (0.870)	-0.573 (0.572)	-0.085 (0.786)	0.224 (0.986)	0.211 (0.721)	-0.050 (0.811)	-0.087 (0.420)	0.203 (0.607)	-0.271 (0.619)	-0.564** (0.243)	-0.119 (0.202)	-0.578* (0.345)	0.564** (0.246)
	R^2_{pseudo}	0.009	0.001	-0.002	0.051	0.023	0.002	0.026	0.016	0.028	0.023	-0.004	0.002	0.004	0.009
	N	5231	5231	5231	5231	5231	5231	5231	5231	5231	5231	5231	5231	5231	5231
2009	Treat	0.236 (0.510)	0.919 (0.985)	-0.981 (0.645)	0.958 (0.863)	-0.494 (1.329)	-0.372 (0.827)	0.556 (0.817)	0.340 (0.469)	-0.232 (0.700)	-0.022 (0.776)	-0.054 (0.283)	0.016 (0.233)	-0.249 (0.403)	-0.049 (0.276)
	R^2_{pseudo}	0.007	0.004	-0.004	0.041	0.007	0.002	0.011	0.014	0.033	0.022	-0.003	0.002	0.005	0.007
	N	3512	3512	3512	3512	3512	3512	3512	3512	3512	3512	3512	3512	3512	3512

Table 12: Continuity of covariates: by other establishment characteristics

Year		Work council	Busin. LLC.	Collect. agr.	Wage cushion	Share fem.	Year		Work council	Busin. LLC.	Collect. agr.	Wage cushion	Share 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_{adj}						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		
	N	217	2745	1073	1057	1068		N	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		
	N	2076	1199	2087	2067	2049		N	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^2_{adj}					0.039		R^2_{adj}						0.024
	R^2_{pseudo}	0.064	0.130	0.049	0.059			R^2_{pseudo}	0.085	0.098	0.017	0.020		
	N	1010	3650	3075	3057	3094		N	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	
		(0.472)	(0.211)	(0.242)	(0.290)	(3.700)			(0.378)	(0.171)	(0.188)	(0.256)	(2.753)	
	R^2_{adj}					0.035		R^2_{adj}						0.023
	R^2_{pseudo}	0.093	0.135	0.053	0.054			R^2_{pseudo}	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	
		(0.423)	(0.142)	(0.177)	(0.246)	(2.747)			(0.392)	(0.182)	(0.195)	(0.252)	(2.863)	
	R^2_{adj}					0.034		R^2_{adj}						0.016
	R^2_{pseudo}	0.064	0.103	0.041	0.026			R^2_{pseudo}	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**	
		(0.360)	(0.144)	(0.158)	(0.205)	(2.405)			(0.399)	(0.180)	(0.192)	(0.257)	(2.842)	
	R^2_{adj}					0.021		R^2_{adj}						0.013
	R^2_{pseudo}	0.053	0.108	0.034	0.020			R^2_{pseudo}	0.078	0.082	0.008	0.008		
	N	4183	5665	4390	2730	4408		N	4381	5082	4374	4355	4387	
2001	Treat	-0.052	-0.278**	-0.155	0.266	4.607*	2009	Treat	-0.466	0.220	-0.110	0.569**	0.224	
		(0.366)	(0.139)	(0.155)	(0.199)	(2.423)			(0.411)	(0.207)	(0.204)	(0.284)	(2.992)	
	R^2_{adj}					0.019		R^2_{adj}						0.018
	R^2_{pseudo}	0.045	0.106	0.028	0.030			R^2_{pseudo}	0.063	0.059	0.003	0.003		
	N	4465	5826	4698	2969	4712		N	3500	3398	3504	3483	3507	

Table 13: Continuity of covariates: individual data

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

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).

Table 14: Robustness tests using Donut regressions, covariates and industry dummies: Investments and labor productivity:

	Log(capital stock)			Log(investment flows)			Log(expansion investments)			Log(ICT investments)			Log(Value added)		
	Donut	Cov.	Ind. dummies	Donut	Cov.	Ind. dummies	Donut	Cov.	Ind. dummies	Donut	Cov.	Ind. dummies	Donut	Cov.	Ind. dummies
Available time	1994-2012			1994-2012			1996-2012			2000-2006			1994-2012		
All periods (γ)	-0.071 (0.043)			-0.013 (0.130)			-0.046 (0.130)			0.324* (0.192)			0.002 (0.039)		
Period 1 (γ_1)	0.025 (0.138)	1.601* (0.817)	0.041 (0.095)	-0.080 (0.438)	-1.437 (1.801)	0.076 (0.301)	-0.466 (0.445)	-0.002 (0.456)	-0.474 (0.351)				-0.051 (0.109)	0.439 (0.327)	0.092 (0.077)
Period 2 (γ_2)	-0.216 (0.140)	-0.340** (0.150)	-0.300*** (0.100)	-0.402 (0.352)	-0.560 (0.425)	-0.557* (0.295)	-0.694 (0.428)	-1.052* (0.541)	-0.710* (0.363)				-0.107 (0.108)	-0.175 (0.110)	-0.128* (0.077)
Period 3 (γ_3)	-0.150** (0.076)	-0.084 (0.062)	-0.089* (0.050)	-0.045 (0.191)	-0.204 (0.177)	-0.203 (0.133)	-0.261 (0.221)	0.155 (0.218)	-0.012 (0.164)				-0.120 (0.077)	-0.061 (0.051)	-0.055 (0.044)
Period 4 (γ_4)	0.019 (0.071)	0.012 (0.054)	0.016 (0.049)	0.206 (0.247)	0.270 (0.182)	0.196 (0.166)	0.252 (0.195)	-0.033 (0.174)	0.245 (0.157)				0.117** (0.053)	0.076** (0.037)	0.074** (0.036)
R^2_{adj}	0.148	0.148	0.162	0.066	0.066	0.076	0.024	0.024	0.034	0.031	0.031	0.054	0.388	0.388	0.4
AIC	172091	172091	123851	580760	580763	397519	292867	292865	198477	115320	115320	76190	130787	130784	144220
N	50907	50907	37357	96866	96866	66496	49120	49120	33387	20289	20289	13443	46907	46907	34068
			56671	102912	102912	52266	52266	52266	52266	22402	22402	22402	52040	52040	52040

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.

Table 15: Robustness tests using Donut regressions, covariates and industry dummies: Establishment-level and individual-level wage

	Firm-level $\text{Log}(\text{gross wage}_{it})$		Individual-level $\text{Log}(\text{wage}_{ijt})$	
	Donut	Cov. Ind. dummies	Donut	Cov. Ind. dummies
Available time	1994-2009		1994-2009	
All periods (γ)	0.045 (0.038)		-0.033 (0.268)	
Period 1 (γ_1)	0.027 (0.253)	0.356 (0.473)	0.083 (0.085)	0.072 (0.789)
Period 2 (γ_2)	0.136 (0.084)	0.043 (0.057)	0.087* (0.051)	-0.134 (0.103)
Period 3 (γ_3)	-0.037 (0.065)	-0.017 (0.028)	-0.089*** (0.026)	-0.044 (0.059)
Period 4 (γ_4)	0.065 (0.052)	0.025 (0.027)	0.040 (0.027)	0.053 (0.061)
R_{adj}^2	0.628	0.646	0.631	0.2579
AIC	46220	31477	48752	13046
N	27737	24094	31064	1374

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.

Table 16: Robustness tests using Donut regressions, covariates and industry dummies: Employment margin

	Job reallocation rate		# of temp. workers		# of white-collar workers		
	Donut	Cov.	Donut	Cov.	Donut	Cov.	Ind. dummies
Available time							1996-2012
All periods (γ)	-0.010 (0.009)		0.008 (0.013)		-0.018 (0.028)		
Period 1 (γ_1)	0.003 (0.041)	-0.032 (0.027)	0.015 (0.036)	0.006 (0.009)	0.011 (0.021)		
Period 2 (γ_2)	-0.027 (0.039)	-0.043 (0.031)	0.002 (0.049)	-0.058 (0.043)	-0.029 (0.031)		
Period 3 (γ_3)	-0.015 (0.018)	-0.015 (0.012)	0.003 (0.015)	0.023* (0.012)	0.016 (0.010)	0.001 (0.070)	-0.026 (0.033)
Period 4 (γ_4)	-0.007 (0.011)	-0.006 (0.007)	0.012 (0.022)	-0.005 (0.015)	0.003 (0.015)	-0.019 (0.029)	-0.050*** (0.019)
R^2_{adj}	0.015	0.014	0.038	0.039	0.052	0.044	0.137
AIC	-4579	-4573	25161	27287	26542	35845	37239
N	24490	24490	45874	40378	49467	26692	28956

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.

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

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

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

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.

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

Table 17: Robustness tests using different fractions of the optimal bandwidth: Capital stock & total investment flows

	Stock measure				Flow measures							
	Log(capital stock)				Log(total capital investment)							
	1994-2012				1994-2012							
Time period	75% of opt. bandw.	50% of opt. bandw.	25% of opt. bandw.	75% of opt. bandw.	50% of opt. bandw.	25% of opt. bandw.	75% of opt. bandw.	50% of opt. bandw.	25% of opt. bandw.	75% of opt. bandw.	50% of opt. bandw.	25% of opt. bandw.
Bandwidth	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
All periods (γ)	-0.059* (0.033)		-0.027 (0.039)		-0.055 (0.046)		-0.065 (0.091)		-0.041 (0.105)		0.034 (0.140)	
Period 1 (γ_1)		0.005 (0.098)		0.057 (0.107)		-0.018 (0.139)		0.142 (0.321)		0.492 (0.366)		0.609 (0.464)
Period 2 (γ_2)		-0.335*** (0.118)		-0.410*** (0.164)		-0.507*** (0.203)		-0.340 (0.337)		-0.167 (0.412)		-0.714 (0.610)
Period 3 (γ_3)		-0.069 (0.050)		-0.060 (0.058)		-0.061 (0.068)		-0.152 (0.135)		-0.258* (0.146)		-0.269 (0.193)
Period 4 (γ_4)		-0.020 (0.053)		0.018 (0.061)		-0.002 (0.079)		0.036 (0.151)		0.150 (0.175)		0.381* (0.231)
R^2_{adj}	0.129	0.129	0.108	0.108	0.056	0.056	0.044	0.044	0.043	0.043	0.023	0.024
AIC	157538	157536	111194	111192	52212	52212	453598	453602	316047	316047	145527	145525
N	46422	46422	32700	32700	15347	15347	75156	75156	52340	52340	23964	23964

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.

Table 18: Robustness tests using different fractions of the optimal bandwidth: Expansion & ICT investment

Time period	Log(expansion investment)				Log(ICT investment)							
	1996-2012				2000-2006							
	75% of opt. bandw.	50% of opt. bandw.	25% of opt. bandw.	75% of opt. bandw.	50% of opt. bandw.	25% of opt. bandw.	75% of opt. bandw.	25% of opt. bandw.				
Bandwidth	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
All periods (γ)	-0.037 (0.108)		-0.133 (0.128)		0.056 (0.175)		0.290** (0.137)		0.225 (0.157)		0.165 (0.200)	
Period 1 (γ_1)		-0.467 (0.378)		-0.439 (0.402)		-0.293 (0.569)						
Period 2 (γ_2)		-0.549 (0.394)		-0.572 (0.626)		0.404 (0.956)						
Period 3 (γ_3)		0.068 (0.178)		0.008 (0.195)		0.228 (0.257)		0.067 (0.167)		0.021 (0.182)		-0.089 (0.236)
Period 4 (γ_4)		0.017 (0.168)		-0.174 (0.200)		-0.086 (0.270)		0.832*** (0.252)		0.889*** (0.310)		0.808** (0.369)
R^2_{adj}	0.02	0.02	0.021	0.021	0.017	0.016	0.022	0.023	0.019	0.02	0.01	0.01
AIC	253771	253774	174019	174023	84374	84379	107902	107897	82562	82558	41918	41916
N	42446	42446	29057	29057	14030	14030	18972	18972	14525	14525	7353	7353

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.

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

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

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.

Table 21: Robustness tests using different fractions of the optimal bandwidth: Individued wage

	Imputed wage											
	Censored wage						Imputed wage					
	Log($wage_{ijt}$)						Log($wage_{ijt}$)					
Time period	1994-2009						1994-2009					
Bandwidth	150% of opt. bandw.	200% of opt. bandw.	300% of opt. bandw.	150% of opt. bandw.	200% of opt. bandw.	300% of opt. bandw.	150% of opt. bandw.	200% of opt. bandw.	300% of opt. bandw.	150% of opt. bandw.	200% of opt. bandw.	300% of opt. bandw.
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
All periods (γ)	-0.033 (0.033)	-0.028 (0.031)	-0.028 (0.031)	-0.042* (0.025)	-0.042* (0.025)	0.004 (0.071)	-0.039 (0.035)	-0.004 (0.082)	-0.040 (0.032)	-0.006 (0.096)	-0.045* (0.027)	0.004 (0.071)
Period 1 (γ_1)		-0.004 (0.082)		-0.006 (0.096)		0.004 (0.071)		-0.004 (0.082)		-0.006 (0.096)		0.004 (0.071)
Period 2 (γ_2)		-0.055 (0.098)		-0.234* (0.120)		-0.211** (0.105)		-0.055 (0.098)		-0.234* (0.120)		-0.211** (0.105)
Period 3 (γ_3)		-0.120*** (0.044)		-0.119*** (0.039)		-0.111*** (0.033)		-0.117** (0.046)		-0.116*** (0.040)		-0.105*** (0.034)
Period 4 (γ_4)		0.042 (0.056)		0.033 (0.046)		0.001 (0.036)		0.026 (0.058)		0.028 (0.052)		-0.011 (0.038)
R^2_{adj}	0.019	0.019	0.031	0.032	0.043	0.044	0.017	0.017	0.027	0.027	0.041	0.041
AIC	44564	44546	58368	58341	87084	87062	46081	46069	60264	60242	90125	90110
N	28834	28834	37658	37658	55942	55942	28783	28783	37724	37724	56064	56064

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.

Table 22: Robustness tests using different fractions of the optimal bandwidth: Job reallocation rate, temporary work & white-collar employment

Time period	Job Reallocation rate							# of temporary workers					# of white-collar workers		
	1994-2012							1996-2012					2003-2012		
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Bandwidth	75% of opt. bandw.	50% of opt. bandw.	25% of opt. bandw.	75% of opt. bandw.	50% of opt. bandw.	25% of opt. bandw.	75% of opt. bandw.	50% of opt. bandw.	25% of opt. bandw.	75% of opt. bandw.	50% of opt. bandw.	25% of opt. bandw.	75% of opt. bandw.	50% of opt. bandw.	25% of opt. bandw.
All periods (γ)	-0.009 (0.006)	-0.007 (0.008)	-0.018 (0.013)	-0.178** (0.085)	0.005 (0.008)	0.026 (0.020)	0.005 (0.010)	0.023 (0.018)	0.005 (0.014)	-0.032* (0.017)	-0.045** (0.018)	-0.072** (0.028)			
Period 1 (γ_1)	0.015 (0.023)	0.052 (0.033)	-0.059 (0.029)	-0.036 (0.029)	-0.059 (0.062)	-0.045 (0.038)	-0.056 (0.040)	0.023 (0.016*)	0.031 (0.028)	0.023 (0.015)	0.031 (0.028)	0.031 (0.028)	0.031 (0.028)	0.031 (0.028)	0.031 (0.028)
Period 2 (γ_2)	-0.034 (0.024)	-0.017 (0.014)	-0.017 (0.019)	-0.023 (0.019)	0.017** (0.008)	0.017** (0.008)	0.016* (0.010)	0.023 (0.010)	0.023 (0.010)	0.023 (0.010)	0.023 (0.010)	0.023 (0.010)	0.023 (0.010)	0.023 (0.010)	0.023 (0.010)
Period 3 (γ_3)	-0.014 (0.011)	-0.006 (0.008)	-0.004 (0.010)	-0.004 (0.010)	0.000 (0.018)	0.000 (0.018)	0.000 (0.018)	0.000 (0.018)	0.000 (0.018)	0.000 (0.018)	0.000 (0.018)	0.000 (0.018)	0.000 (0.018)	0.000 (0.018)	0.000 (0.018)
Period 4 (γ_4)	0.012 (0.012)	0.012 (0.012)	0.012 (0.012)	0.012 (0.012)	0.012 (0.012)	0.012 (0.012)	0.012 (0.012)	0.012 (0.012)	0.012 (0.012)	0.012 (0.012)	0.012 (0.012)	0.012 (0.012)	0.012 (0.012)	0.012 (0.012)	0.012 (0.012)
R^2_{adj}	-6171	-6168	-4268	-4268	-1834	-1833	23143	23144	15974	15976	9319	9320	32348	32350	21935
AIC	22380	22380	14359	14359	7091	7091	42050	42050	27482	27482	12591	12591	22452	22452	14405
N															

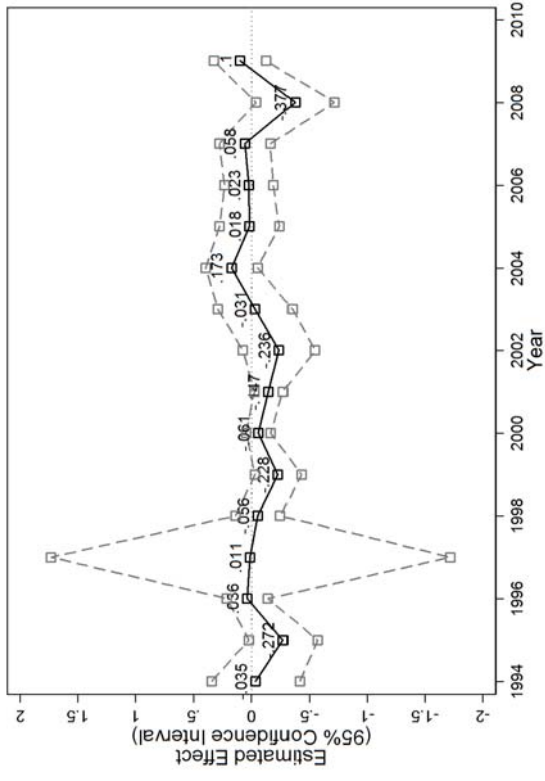
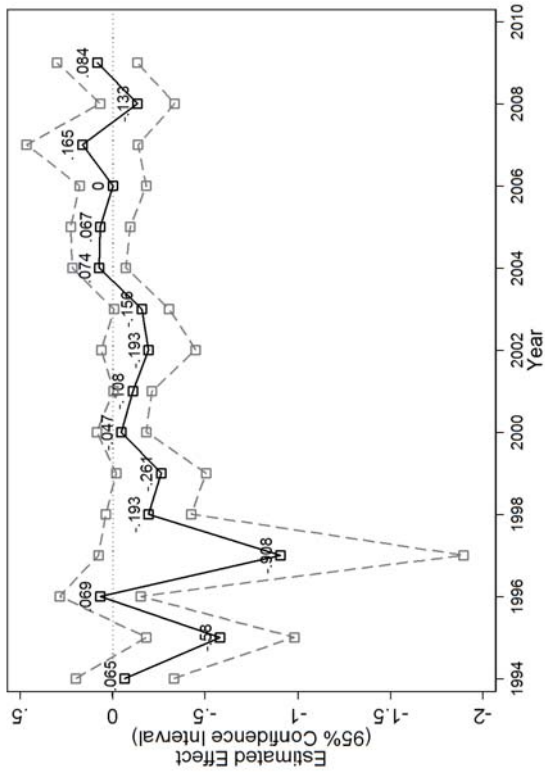
Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The table shows the treatment effect of EPL on the respective outcome, estimated by OLS. Robust standard errors clustered at the establishment level are reported in brackets. N refers to the number of observations at the (year-specific) optimal bandwidths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

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

	Imputed wage			
	Individual-level			
	Log($wage_{ijt}$)			
	Baseline		Donut	
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
N	19786	19786	8787	8787

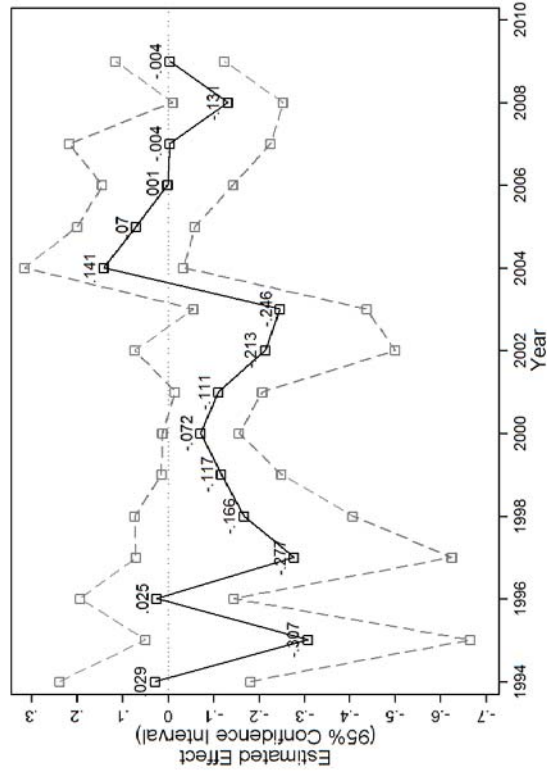
Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The table shows the treatment effect of EPL on the respective outcome, estimated by OLS. Robust standard errors clustered at the establishment level are reported in brackets. N refers to the number of observations at the (year-specific) optimal bandwidths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

Figure 18: Year-specific effects of EPL using different bandwidths: Individual wage



(b) 200% of opt. bandwidth

(a) 150% of opt. bandwidth



(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.

Table 24: Investment flows by industry I

Industry	Electr., Gas; Water, Sewerage; Waste & Recycl.		Food Prod.; Beverages; Tobacco		Textile, Apparel & Leather		Wood & Paper Prod; Printing		Coke & Petrol. Prod.; Chemicals; Pharmac.	
	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut
All periods (γ)	1.767*** (0.693)	2.280*** (1.044)	-0.539 (0.528)	-0.093 (0.647)	-1.591 (1.114)	-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*** (1.071)	3.987*** (1.528)	-0.845 (0.766)	-0.949 (1.002)	1.724 (3.150)	5.287 (4.937)	-0.346 (0.890)	0.270 (1.111)	0.504 (1.231)	1.133 (1.804)
Period 4 (γ_4)	0.681 (0.955)	1.181 (1.465)	-0.367 (0.684)	0.328 (0.838)	-2.006* (1.172)	-2.372 (1.561)	-0.432 (0.898)	-0.908 (1.124)	-0.068 (1.207)	-0.591 (1.442)
R^2_{adj}	0.117	0.12	0.088	0.1	0.08	0.082	0.092	0.099	0.083	0.093
AIC	6180	6178	13962	12353	3734	3477	11994	10917	5662	5049
N	988	988	2311	2047	629	587	1986	1812	909	810

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.

Table 25: Investment flows by industry II

Industry	Rubber & Plastic Prod.		Non-Metallic Mineral Prod.		Cement, Lime & Plaster; Non-Met. Mineral Prod. n.e.c.		Iron, Basic & Fabr. Metal Prod.		Computer, Electr. & Optical Prod.	
	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut
All periods (γ)	-0.418 (0.974)	-0.977 (1.172)	-4.293 (2.827)	-5.937 (4.042)	1.347* (0.808)	2.080** (1.012)	0.195 (0.413)	0.690 (0.509)	0.494 (1.015)	0.545 (1.395)
Period 3 (γ_3)	-0.442 (1.654)	-2.115 (2.115)	-3.858 (3.458)	-9.228 (6.548)	1.868 (1.194)	1.431 (1.702)	-0.304 (0.588)	0.561 (0.758)	1.899 (1.620)	3.047 (2.349)
Period 4 (γ_4)	-0.402 (1.328)	-0.327 (1.541)	-4.662 (3.896)	-4.329 (5.476)	0.921 (1.174)	2.548* (1.391)	0.581 (0.590)	0.770 (0.689)	-0.047 (1.196)	-0.217 (1.581)
R^2_{adj}	0.054	0.055	0.318	0.266	0.054	0.064	0.084	0.088	0.171	0.174
AIC	4682	4684	4278	1400	6096	5616	21891	19817	3505	3220
N	766	766	257	240	992	915	3592	3251	581	533

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.

Table 26: Investment flows by industry III

Industry	Electrical Equipment		Machinery & Equipment		Motor Vehicles & Other Transp. Equipm.		Furniture; Jewellery, Bijouterie & Rel. Articles		Repair & Install. Machinery & Equipment	
	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	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 (1.290)	1.876 (1.473)	-0.567 (1.175)	0.744 (1.749)	2.283** (1.004)	2.262* (1.350)	0.298 (0.745)	0.775 (1.130)	0.356 (1.631)	-1.483 (2.247)
Period 4 (γ_4)	-0.045 (1.582)	0.144 (1.739)	0.555 (0.850)	1.702 (1.074)	-0.079 (0.958)	0.164 (1.021)	-0.881 (0.776)	-0.852 (1.010)	-1.501 (1.212)	-1.611 (1.537)
R^2_{adj}	0.144	0.162	0.071	0.074	0.138	0.144	0.04	0.044	0.131	0.138
AIC	3418	3420	8391	7594	6711	6218	15802	14166	5365	4906
N	565	538	1364	1234	1104	1023	2640	2368	887	811

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.

Table 27: Investment flows by industry IV

Industry	Building & Construction		Special. Constr. Activities		Sale, Repair, Motor Vehicles		Wholesale		Retail Trade	
	Baseline	Domut	Baseline	Domut	Baseline	Domut	Baseline	Domut	Baseline	Domut
All periods (γ)	-0.257 (0.609)	-0.249 (0.865)	-0.079 (0.285)	0.005 (0.385)	-0.822* (0.486)	-1.549** (0.641)	0.357 (0.437)	0.523 (0.522)	0.115 (0.303)	0.141 (0.436)
Period 3 (γ_3)	-1.314 (0.815)	0.616 (1.300)	-0.296 (0.380)	-0.174 (0.583)	-0.684 (0.705)	-1.295 (1.042)	0.618 (0.615)	0.713 (0.733)	0.312 (0.385)	0.391 (0.561)
Period 4 (γ_4)	0.259 (0.810)	-0.557 (1.105)	0.101 (0.444)	0.110 (0.538)	-0.898 (0.673)	-1.663** (0.828)	0.204 (0.596)	0.413 (0.711)	-0.097 (0.493)	-0.208 (0.702)
R^2_{adj}	0.066	0.067	0.042	0.045	0.051	0.056	0.073	0.077	0.036	0.039
AIC	11604	11603	10140	10141	52532	47302	47304	19376	19378	17470
N	1899	1899	1660	1660	8762	7897	7897	3219	3219	2906

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The table shows the treatment effect of EPL on the respective outcomes, 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.

Table 28: Investment flows by industry V

Industry	Transport., Warehousing & Storage; Postal Serv.		Accommodation & Food Services		Information & Communication		Financial & Insurance Services		Real Estate Serv.	
	Baseline	Domut	Baseline	Domut	Baseline	Domut	Baseline	Domut	Baseline	Domut
All periods (γ)	0.855* (0.479)	0.773 (0.576)	-0.457 (0.435)	-0.591 (0.627)	1.234** (0.559)	1.728** (0.673)	-1.179 (0.724)	-0.515 (0.844)	0.008 (0.768)	-0.110 (0.995)
Period 3 (γ_3)	0.827 (0.672)	1.561* (0.804)	-0.772 (0.573)	-0.844 (0.826)	1.246* (0.711)	1.609* (0.893)	-2.009** (0.903)	-1.193 (1.216)	-0.065 (1.075)	-0.012 (1.278)
Period 4 (γ_4)	0.884 (0.679)	0.018 (0.813)	-0.047 (0.691)	-0.261 (0.973)	1.220 (0.912)	1.864* (1.059)	-0.304 (1.090)	0.024 (1.189)	0.070 (1.167)	-0.190 (1.500)
R^2_{adj}	0.089	0.089	0.056	0.058	0.073	0.073	0.047	0.05	0.174	0.18
AIC	20437	20439	18975	18975	20542	20543	18438	18439	13050	13052
N	3323	3323	3084	3084	3442	3442	3093	3093	2194	2194

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The table shows the treatment effect of EPL on the respective outcomes, 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.

Table 29: Investment flows by industry VI

Industry	Legal & Accounting Serv.		Management Consultancy Serv.		Architectural & Engineering Serv.		Scientific R&D Serv.		Advert. & Market Research; Other Prof., Scientific & Techn. Serv.	
	Baseline	Domut	Baseline	Domut	Baseline	Domut	Baseline	Domut	Baseline	Domut
All periods (γ)	0.200 (0.458)	0.265 (0.644)	-0.566 (0.981)	-1.074 (1.297)	0.061 (0.454)	0.182 (0.608)	1.088 (0.669)	0.873 (0.907)	0.455 (0.939)	-0.074 (0.904)
Period 3 (γ_3)	0.128 (0.633)	0.396 (1.095)	-0.484 (1.936)	-3.311 (2.589)	0.443 (0.713)	0.719 (1.023)	0.726 (1.041)	0.455 (1.393)	1.588 (1.684)	1.802 (1.833)
Period 4 (γ_4)	0.259 (0.647)	0.183 (0.833)	-0.601 (1.223)	-0.337 (1.548)	-0.277 (0.670)	-0.210 (0.838)	1.495 (0.955)	1.260 (1.247)	-0.200 (1.275)	-1.040 (1.245)
R^2_{adj}	0.065	0.065	0.07	0.07	0.078	0.074	0.156	0.165	0.09	0.093
AIC	12987	12989	11227	11229	5417	5419	5081	5082	14880	14881
N	2222	2222	1922	1922	894	894	841	841	2534	2534

Note: *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. The table shows the treatment effect of EPL on the respective outcomes, 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.

Table 30: Investment flows by industry VII

Industry	Travel & Other Serv.		Education		Health & Social Services		Arts, Entertainment & Recreation		Repair of Computers; Personal & Household Goods		Other Personal Serv.	
	Baseline	Donmit	Baseline	Donmit	Baseline	Donmit	Baseline	Donmit	Baseline	Donmit	Baseline	Donmit
All periods (τ)	-0.423 (0.587)	-0.520 (0.797)	-1.126** (0.552)	-1.308* (0.761)	-0.178 (0.321)	-0.367 (0.448)	-1.157 (0.813)	-1.359 (1.231)	1.317 (1.281)	1.229 (1.658)	0.633 (0.664)	0.312 (0.899)
Period 3 (τ_3)	-0.307 (1.045)	0.277 (1.785)	-1.134 (0.846)	-1.217 (1.291)	0.353 (0.474)	0.694 (0.714)	-0.571 (1.002)	-0.201 (1.585)	3.613** (1.643)	5.817*** (2.133)	-0.122 (0.771)	0.124 (1.157)
Period 4 (τ_4)	-0.463 (0.727)	-0.646 (0.917)	-1.120 (0.739)	-1.357 (0.964)	-0.622 (0.450)	-1.040* (0.589)	-2.123 (1.505)	-2.919 (2.060)	-0.316 (2.104)	-1.590 (2.916)	2.035 (1.308)	0.575 (1.535)
R^2_{adj}	0.035	0.035	0.021	0.016	0.032	0.033	0.07	0.073	0.246	0.239	0.033	0.028
AIC	15720	15722	12479	11322	44790	41308	6252	5778	2433	2323	10181	9510
N	2629	2439	2063	1871	7544	6963	1040	962	421	402	1749	1637

Note: ** $p < 0.05$, * $p < 0.1$. 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.

Table 31: Capital stock by industry I

Industry	Electr., Gas, Water, Sewerage; Waste & Recycl.		Food Prod.; Beverages; Tobacco		Textile, Apparel & Leather		Wood & Paper Prod.; Printing		Coke & Petrol. Prod.; Chemicals; Pharmac.	
	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut
All periods (γ)	-0.338 (0.290)	-0.730 (0.480)	-0.101 (0.170)	-0.129 (0.206)	0.044 (0.409)	-0.011 (0.670)	-0.158 (0.228)	-0.437 (0.311)	0.109 (0.324)	0.092 (0.396)
Period 3 (γ_3)	-0.320 (0.526)	-0.363 (0.987)	0.033 (0.292)	0.030 (0.388)	-0.049 (1.266)	0.472 (1.612)	-0.080 (0.369)	-0.084 (0.563)	-0.236 (0.435)	-0.110 (0.568)
Period 4 (γ_4)	-0.345 (0.386)	-0.834 (0.587)	-0.218 (0.220)	-0.218 (0.271)	0.057 (0.435)	-0.082 (0.740)	-0.205 (0.342)	-0.585 (0.455)	0.229 (0.404)	0.151 (0.480)
R_{adj}^2	0.25	0.249	0.279	0.279	0.216	0.216	0.231	0.233	0.305	0.288
AIC	2492	2494	2122	2124	5298	5299	4653	4655	1199	1201
N	678	678	575	575	1671	1671	1462	1462	343	343

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 fractional forms of the centered running variable were chosen according to the AIC.

Table 32: Capital stock by industry II

Industry	Rubber & Plastic Prod.		Cement, Lime & Plaster; Non-Met. Mineral Prod. n.e.c.		Iron, Basic & Fabr. Metal Prod.		Computer, Electr. & Optical Prod.		Electrical Equipment	
	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut
All periods (γ)	0.319 (0.339)	0.433 (0.442)	-0.126 (0.322)	0.423 (0.456)	0.232* (0.131)	0.259 (0.175)	0.801* (0.409)	0.745 (0.496)	0.383 (0.382)	0.307 (0.387)
Period 3 (γ_3)	-0.223 (0.454)	-0.553 (0.601)	-1.065*** (0.402)	-0.970 (0.741)	0.570*** (0.219)	0.749** (0.304)	1.744** (0.877)	1.691 (1.065)	0.188 (0.803)	-0.884 (0.586)
Period 4 (γ_4)	0.593 (0.505)	0.897 (0.643)	0.527 (0.528)	1.117* (0.628)	-0.053 (0.184)	-0.105 (0.234)	0.391 (0.466)	0.470 (0.524)	0.442 (0.366)	0.498 (0.422)
R_{adj}^2	0.273	0.274	0.275	0.28	0.21	0.213	0.219	0.222	0.395	0.4
AIC	1809	1808	1701	1699	8323	8316	7535	7527	1455	1452
N	519	519	481	481	2502	2502	2249	2249	428	428

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 fractional forms of the centered running variable were chosen according to the AIC.

Table 33: Capital stock by industry III

Industry	Machinery & Equipment		Motor Vehicles & Other Transp. Equipm.		Furniture; Jewellery, Bijouterie & Rel. Articles		Repair & Install. Machinery & Equipment		Building & Construction	
	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut
All periods (γ)	0.125 (0.215)	0.390 (0.326)	0.252 (0.248)	0.270 (0.301)	0.062 (0.168)	-0.052 (0.223)	0.008 (0.355)	0.178 (0.510)	-0.020 (0.226)	-0.196 (0.400)
Period 3 (γ_3)	-0.101 (0.394)	-0.291 (0.526)	0.128 (0.425)	0.472 (0.587)	0.205 (0.278)	0.018 (0.385)	0.398 (0.609)	0.509 (0.846)	-0.213 (0.241)	-0.076 (0.575)
Period 4 (γ_4)	0.243 (0.291)	0.668 (0.411)	0.184 (0.350)	0.184 (0.406)	-0.041 (0.269)	-0.094 (0.351)	-0.169 (0.450)	0.037 (0.662)	0.052 (0.276)	-0.221 (0.473)
R_{adj}^2	0.195	0.195	0.218	0.218	0.158	0.158	0.163	0.163	0.09	0.091
AIC	3356	3357	2989	2987	2829	2830	2638	2640	6299	6300
N	1005	1005	903	903	846	846	783	783	1993	1993

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 fractional forms of the centered running variable were chosen according to the AIC.

Table 34: Capital stock by industry IV

Industry	Special. Constr. Activities		Sale, Repair Motor Vehicles		Wholesale Trade		Retail Trade		Transport., Warehousing & Storage; Postal Serv.	
	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut
All periods (γ)	0.109 (0.092)	-0.018 (0.127)	-0.081 (0.138)	-0.141 (0.228)	-0.138 (0.159)	-0.159 (0.185)	0.062 (0.120)	0.062 (0.185)	-0.003 (0.140)	0.006 (0.168)
Period 3 (γ_3)	0.135 (0.136)	-0.018 (0.195)	0.001 (0.270)	-0.126 (0.423)	-0.215 (0.220)	-0.370 (0.300)	-0.067 (0.163)	0.070 (0.268)	0.334 (0.221)	0.655** (0.291)
Period 4 (γ_4)	0.084 (0.139)	-0.018 (0.191)	-0.128 (0.200)	-0.150 (0.314)	-0.107 (0.210)	-0.079 (0.249)	0.240 (0.192)	0.051 (0.280)	-0.207 (0.199)	-0.354 (0.223)
R^2_{adj}	0.099	0.103	0.049	0.053	0.187	0.2	0.115	0.126	0.158	0.166
AIC	12736	10804	5805	5060	7971	7130	11239	9382	7278	6615
N	4267	3628	1782	1538	2510	2253	3354	2813	2197	2012

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.

Table 35: Capital stock by industry V

Industry	Accommodation & Food Services		Financial & Insurance Serv.		Real Estate Serv.		Legal & Accounting Serv.		Management Consultancy Serv.	
	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut
All periods (γ)	0.023 (0.170)	0.242 (0.253)	-0.181 (0.266)	-0.221 (0.374)	-0.110 (0.344)	-0.317 (0.535)	-0.119 (0.136)	-0.382* (0.217)	0.275 (0.642)	-0.565 (0.916)
Period 3 (γ_3)	-0.112 (0.246)	0.021 (0.406)	0.138 (0.368)	0.097 (0.524)	0.109 (0.687)	-0.293 (0.912)	-0.367* (0.200)	1.940* (1.139)	0.292 (1.118)	
Period 4 (γ_4)	0.190 (0.265)	0.476 (0.381)	-0.417 (0.384)	-0.375 (0.498)	-0.254 (0.600)	-0.330 (0.859)	0.193 (0.180)	-0.120 (0.273)	-0.249 (0.804)	-0.763 (1.096)
R^2_{adj}	0.146	0.15	0.344	0.355	0.26	0.277	0.201	0.218	0.163	0.131
AIC	6381	5614	4452	4132	3510	3190	3415	2835	2038	1884
N	1794	1566	1301	1212	844	762	1255	1031	515	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 AIC.

Table 36: Capital stock by industry VI

Industry	Architectural & Engineering Serv.		Adverts. & Market Research; Other Prof., Scientific & Techn. Serv.		Education		Health & Social Services		Arts, Entertainment & Recreation	
	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut
All periods (γ)	-0.211 (0.142)	0.002 (0.199)	-0.183 (0.239)	-0.495 (0.556)	-0.047 (0.182)	-0.015 (0.301)	0.097 (0.149)	-0.067 (0.215)	-0.468 (0.317)	-0.283 (0.445)
Period 3 (γ_3)	-0.334 (0.271)	-0.118 (0.390)	1.026 (0.628)	0.026 (0.799)	0.601 (0.357)	0.500 (0.580)	0.123 (0.191)	-0.109 (0.306)	-0.745** (0.374)	-0.212 (0.624)
Period 4 (γ_4)	-0.108 (0.294)	0.090 (0.290)	-0.065* (0.279)	-0.938* (0.551)	-0.284 (0.287)	-0.081 (0.383)	0.081 (0.219)	-0.031 (0.289)	0.089 (0.152)	-0.209 (0.718)
R^2_{adj}	0.227	0.232	0.231	0.159	0.146	0.135	0.090	0.092	0.230	0.287
AIC	1069	1069	1498	1498	485	449	1040	992	545	482
N	1069	1069	1498	1498	485	449	1040	992	545	482

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.

Table 37: Labor productivity by industry I

Industry	Electr., Gas, Water, Sewerage; Waste & Recycl.		Wood & Paper Prod; Printing		Rubber & Plastic Prod.		Cement, Lime & Plaster; Non-Met. Mineral Prod. n.e.c.		Iron, Basic & Fabr. Metal Prod.	
	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut
All periods (γ)	0.213 (0.197)	-0.197 (0.401)	0.199 (0.131)	0.201 (0.173)	-0.026 (0.146)	0.008 (0.185)	-0.076 (0.300)	-0.104 (0.319)	0.122 (0.074)	0.098 (0.110)
Period 3 (γ_3)	0.782** (0.342)	0.452 (0.628)	0.504** (0.243)	0.644** (0.308)	0.214 (0.224)	-0.068 (0.325)	-0.466 (0.523)	-0.348 (0.688)	0.027 (0.109)	0.039 (0.202)
Period 4 (γ_4)	-0.212 (0.236)	-0.762 (0.479)	0.011 (0.141)	0.004 (0.202)	-0.156 (0.189)	0.041 (0.236)	0.158 (0.380)	-0.020 (0.390)	0.186* (0.112)	0.126 (0.144)
R^2_{adj}	0.37	0.376	0.467	0.489	0.613	0.624	0.292	0.313	0.591	0.607
AIC	2050	2046	2823	2819	1199	1105	1574	1412	5433	4900
N	652	552	1220	1079	533	482	510	454	2602	2334

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 are reported in brackets. N refers to the number of observations at the establishment level. Year-specific functional forms of the centered running variable were chosen according to the AIC.

Table 38: Labor productivity by industry II

Industry	Computer, Electr. & Optical Prod.		Electrical Equipment		Machinery & Equipment		Motor Vehicles & Other Transp. Equipm.		Furniture; Jewelry, Bijouterie & Rel. Articles	
	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut
All periods (γ)	0.290 (0.318)	0.416 (0.462)	-0.287 (0.210)	-0.025 (0.303)	-0.164 (0.132)	-0.084 (0.192)	-0.138 (0.145)	-0.243 (0.197)	-0.185** (0.087)	-0.114 (0.121)
Period 3 (γ_3)	0.135 (0.784)	0.265 (1.265)	0.070 (0.283)	0.997* (0.551)	-0.463* (0.268)	-0.328 (0.320)	-0.130 (0.252)	-0.294 (0.448)	-0.152 (0.135)	-0.113 (0.216)
Period 4 (γ_4)	0.371 (0.265)	0.486 (0.342)	-0.434 (0.271)	-0.239 (0.321)	-0.057 (0.149)	-0.020 (0.226)	-0.142 (0.186)	-0.230 (0.228)	-0.218* (0.123)	-0.115 (0.152)
R^2_{adj}	0.314	0.308	0.676	0.692	0.589	0.599	0.715	0.722	0.535	0.544
AIC	1443	1444	853	806	2236	2049	1944	1846	3864	3512
N	425	387	394	372	1065	966	868	813	1820	1631

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 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.

Table 39: Labor productivity by industry III

Industry	Repair & Instal. Machinery & Equipment		Building & Construction		Special. Constr. Activities		Sale, Repair Motor Vehicles		Retail Trade	
	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut
All periods (γ)	0.431* (0.227)	0.556 (0.370)	-0.085 (0.154)	-0.147 (0.187)	-0.058 (0.053)	-0.076 (0.078)	0.170 (0.113)	-0.048 (0.173)	-0.072 (0.095)	-0.204 (0.129)
Period 3 (γ_3)	0.559 (0.406)	0.696 (0.638)	-0.229 (0.403)	-0.337 (0.594)	-0.134* (0.079)	-0.110 (0.110)	0.153 (0.191)	-0.243 (0.320)	-0.173 (0.150)	-0.344 (0.221)
Period 4 (γ_4)	0.334 (0.218)	0.427 (0.367)	-0.044 (0.165)	-0.118 (0.207)	0.030 (0.077)	-0.038 (0.121)	0.181 (0.147)	0.066 (0.216)	0.040 (0.128)	-0.078 (0.168)
R^2_{adj}	0.317	0.302	0.3	0.413	0.464	0.487	0.366	0.405	0.324	0.345
AIC	1519	1521	1365	2377	8529	7233	4497	3747	10840	9327
N	473	412	1131	951	4070	3449	1636	1408	3636	3144

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 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.

Table 40: Labor productivity by industry IV

Industry	Transport., Warehousing & Storage; Postal Serv.		Accommodation & Food Services		Information & Communication		Real Estate Serv.		Legal & Accounting Serv.	
	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut
All periods (γ)	0.064 (0.150)	0.006 (0.178)	-0.327** (0.129)	-0.540** (0.254)	-0.320** (0.142)	-0.357** (0.160)	0.157 (0.251)	-0.018 (0.351)	-0.053 (0.087)	-0.294** (0.143)
Period 3 (γ_3)	0.148 (0.224)	0.130 (0.206)	-0.379* (0.200)	-1.029** (0.437)	-0.453 (0.279)	-0.643** (0.296)	0.628 (0.441)	0.341 (0.553)	-0.086 (0.139)	-0.383* (0.232)
Period 4 (γ_4)	-0.001 (0.190)	-0.066 (0.213)	-0.273 (0.168)	-0.085 (0.272)	-0.225 (0.153)	-0.216 (0.193)	-0.206 (0.317)	-0.316 (0.464)	-0.017 (0.109)	-0.197 (0.180)
R_{adj}^2	0.413	0.425	0.359	0.365	0.513	0.553	0.422	0.433	0.585	0.607
AIC	6689	6690	4329	4331	4137	4138	2602	2601	2072	1708
N	2210	2210	1540	1321	1495	1361	746	671	1129	919

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.

Table 41: Labor productivity by industry V

Industry	Management Consultancy Serv.		Architectural & Engineering Serv.		Scientific R&D Serv.		Advert. & Market Research; Other Prof., Scientific & Techn. Serv.		Travel & Other Serv.	
	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut
All periods (γ)	-0.381 (0.349)	-0.552 (0.434)	-0.095 (0.090)	-0.101 (0.133)	0.689* (0.366)	0.800 (0.537)	0.142 (0.176)	0.078 (0.223)	-0.064 (0.148)	0.000 (0.198)
Period 3 (γ_3)	0.425 (0.412)	0.203 (0.649)	-0.190 (0.147)	-0.204 (0.240)	0.911** (0.440)	1.081* (0.613)	-0.107 (0.297)	-0.016 (0.321)	0.024 (0.263)	0.240 (0.377)
Period 4 (γ_4)	-1.153*** (0.396)	-1.025** (0.499)	-0.009 (0.136)	-0.018 (0.181)	-0.138 (0.499)	-0.365 (0.931)	0.384* (0.227)	0.210 (0.359)	-0.116 (0.167)	-0.115 (0.218)
R_{adj}^2	0.369	0.383	0.513	0.511	0.263	0.282	0.571	0.572	0.255	0.254
AIC	1206	1197	3768	3400	1787	1569	1006	924	4745	4406
N	437	405	1575	1401	482	423	441	405	1592	1466

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.

Table 42: Labor productivity by industry VI

Industry	Education		Health & Social Services		Arts, Entertainment & Recreation		Repair of Computers; Personal & Household Goods		Other Personal Serv.	
	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut	Baseline	Donut
All periods (γ)	0.159 (0.164)	0.202 (0.256)	0.008 (0.084)	-0.088 (0.120)	-0.303 (0.519)	-1.131** (0.449)	-0.053 (0.212)	0.126 (0.278)	-0.094 (0.175)	-0.132 (0.234)
Period 3 (γ_3)	0.073 (0.253)	-0.070 (0.450)	-0.083 (0.112)	-0.148 (0.166)	0.296 (0.696)	-0.959 (0.750)	0.205 (0.366)	0.385 (0.381)	0.101 (0.195)	0.159 (0.293)
Period 4 (γ_4)	0.237 (0.231)	0.404 (0.302)	0.181 (0.121)	-1.493*** (0.513)	-1.493*** (0.513)	-1.272** (0.575)	-0.343 (0.432)	-0.114 (0.432)	-0.535 (0.332)	-0.882** (0.423)
R_{adj}^2	0.481	0.478	0.41	0.423	0.236	0.244	0.621	0.613	0.481	0.487
AIC	1322	1324	5555	4963	1128	974	552	523	2081	1930
N	505	439	2636	2348	331	291	235	221	839	781

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.

Figure 20: Descriptive evidence for treatment effect: Log(total investments)

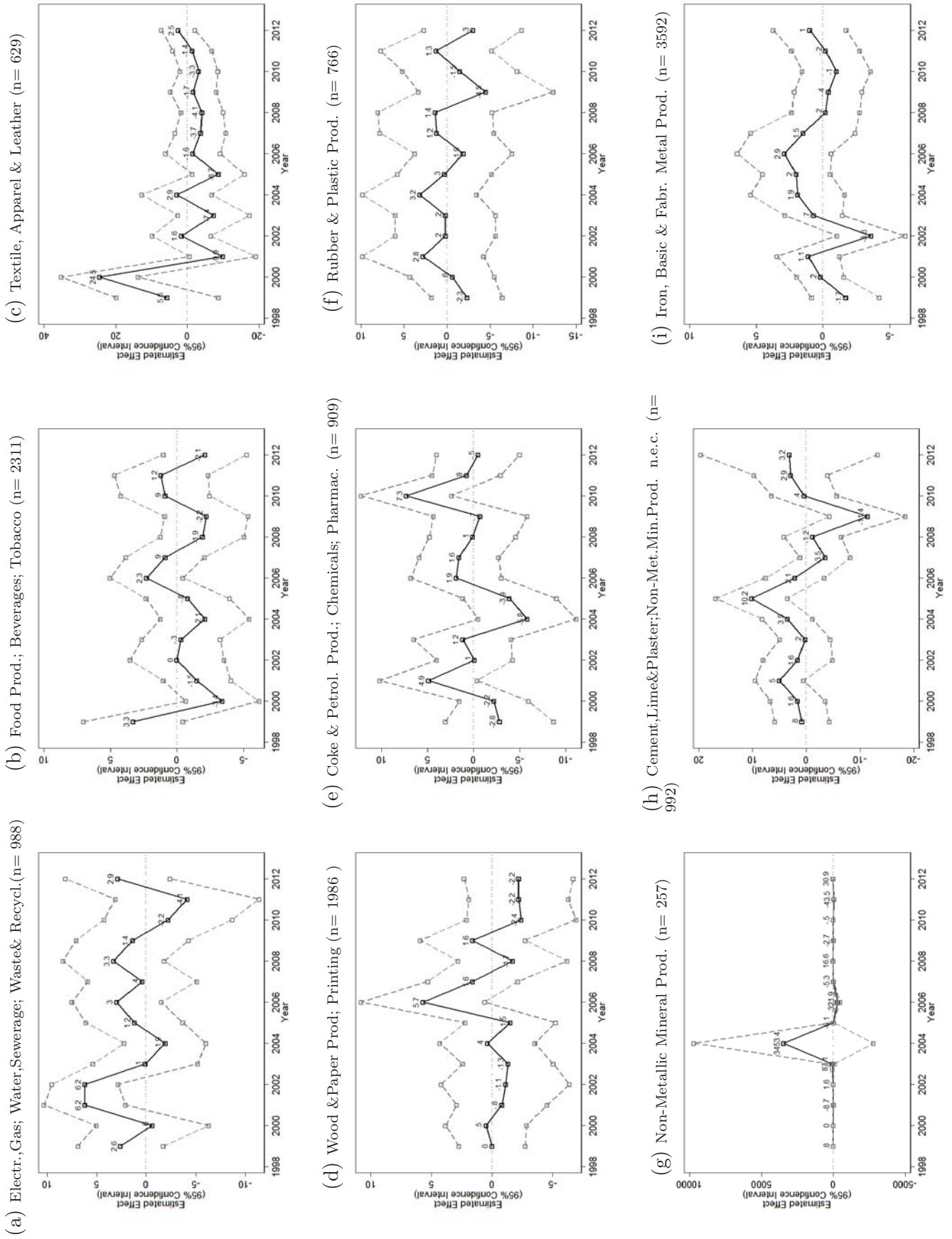


Figure 22: Descriptive evidence for treatment effect: Log(total investments)

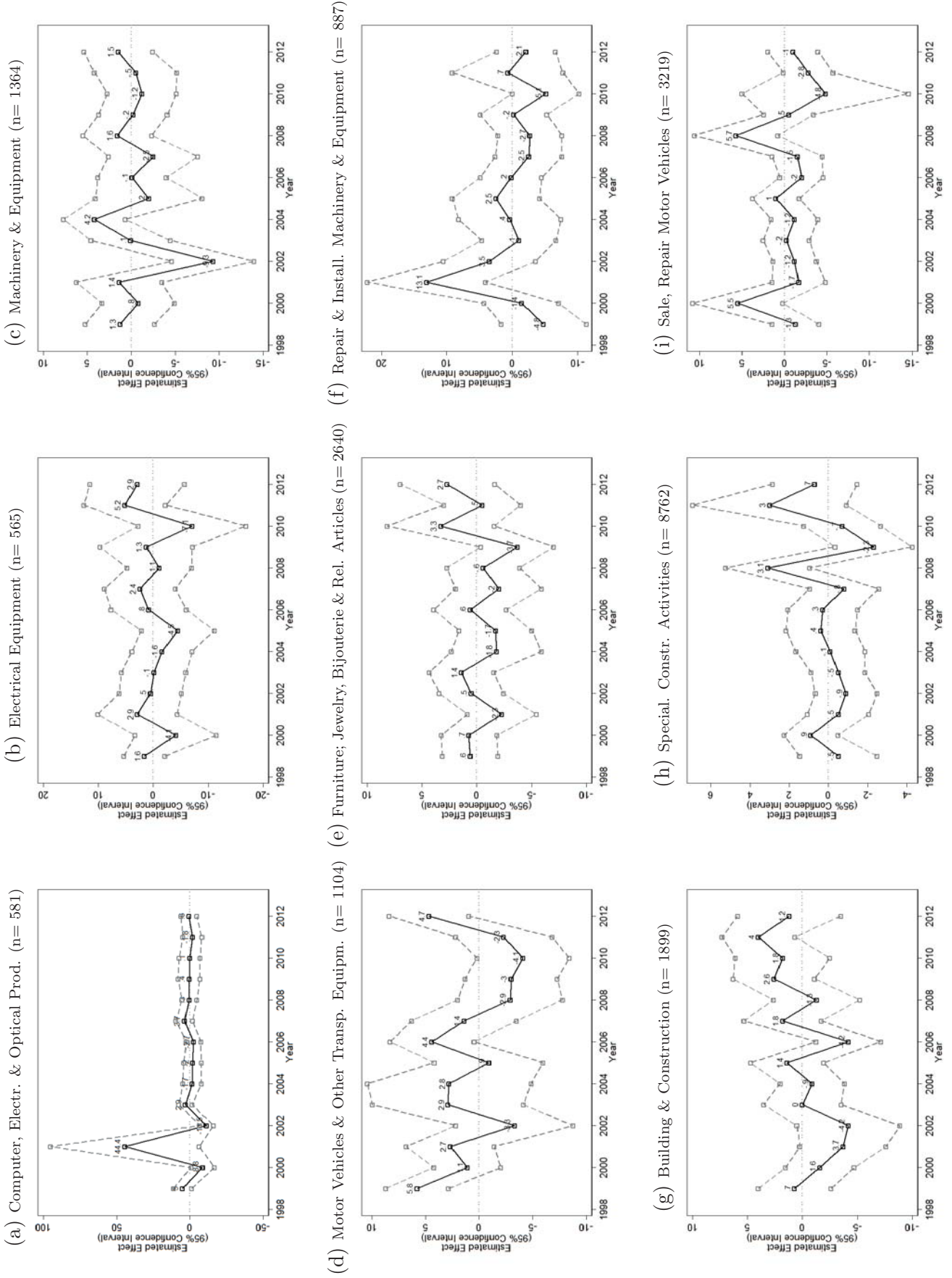


Figure 24: Descriptive evidence for treatment effect: Log(total investments)

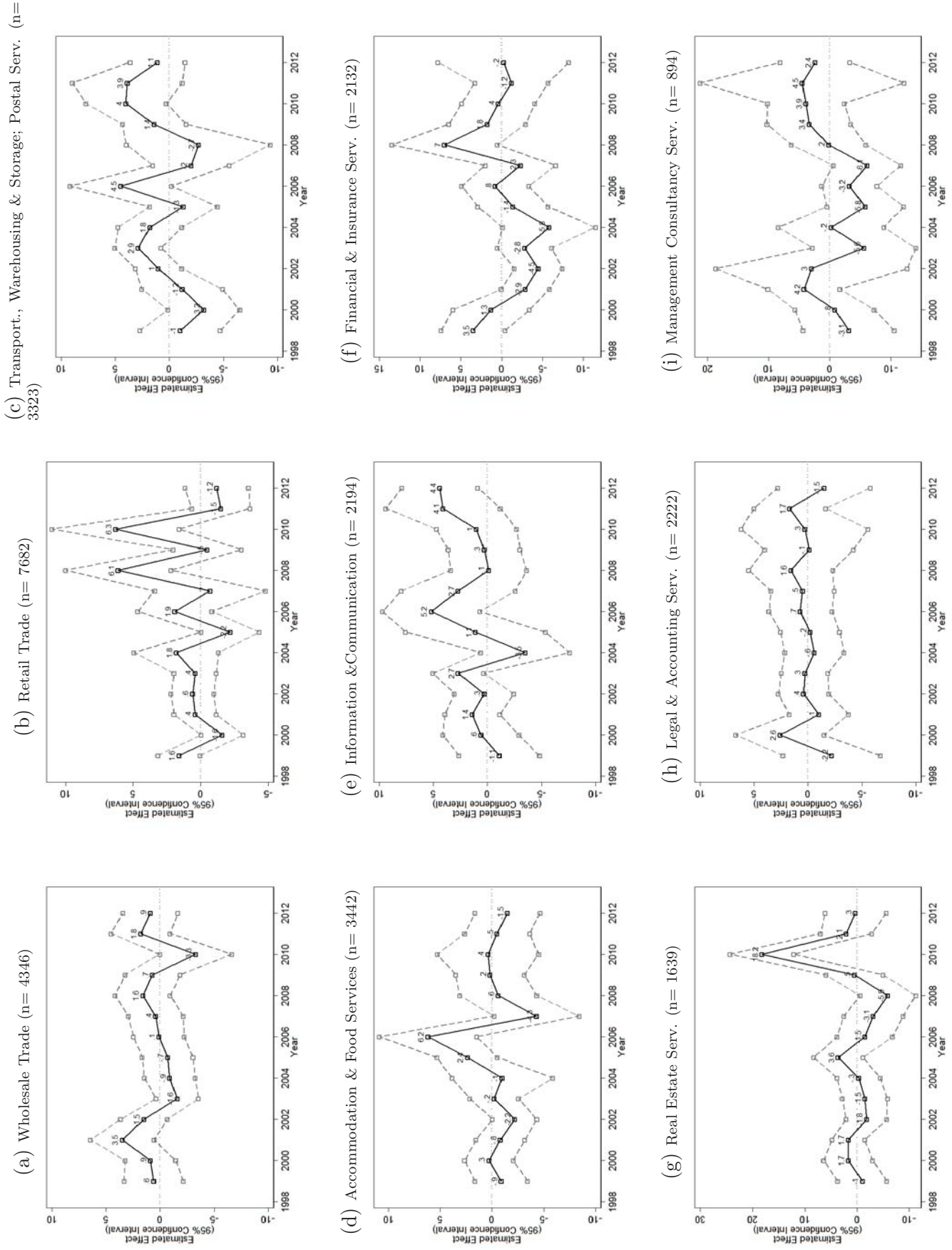
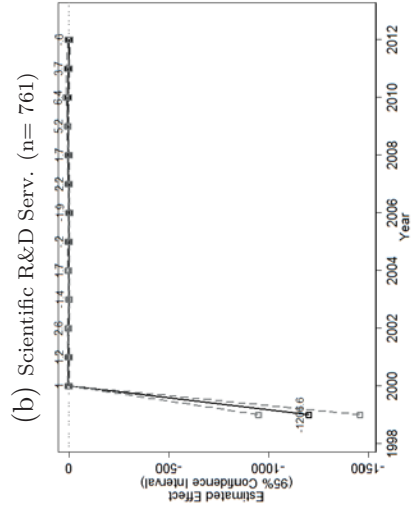
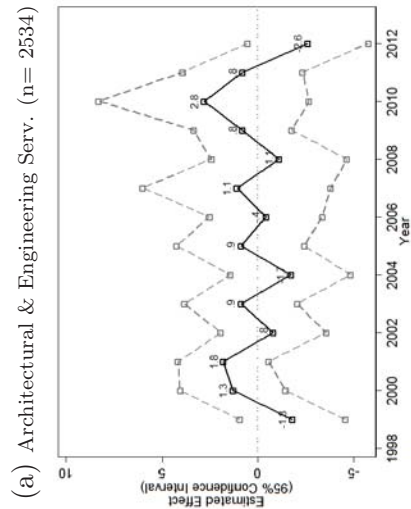


Figure 26: Descriptive evidence for treatment effect: Log(total investments)



(c) Advert. & Market Research; Other Prof., Scientific & Techn. Serv. (n= 869)

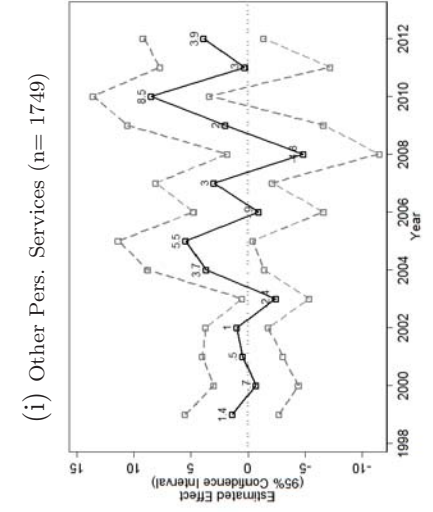
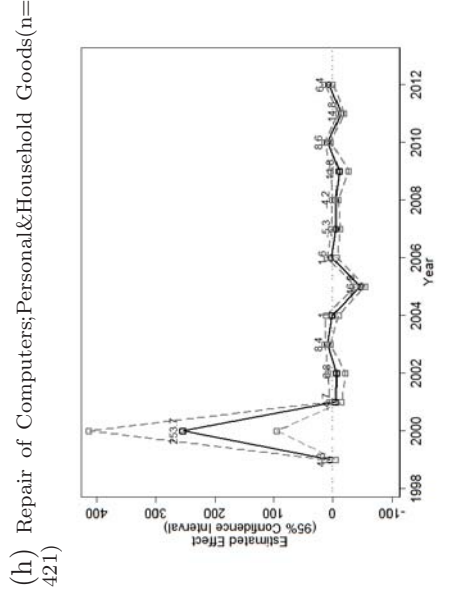
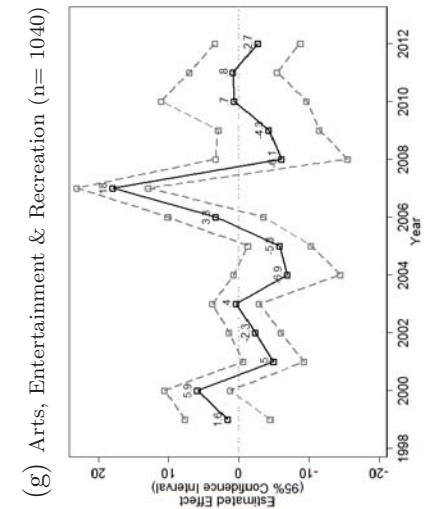
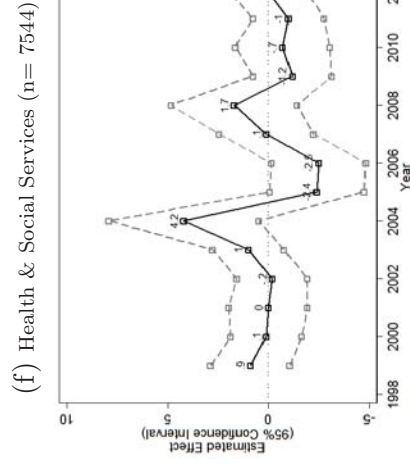
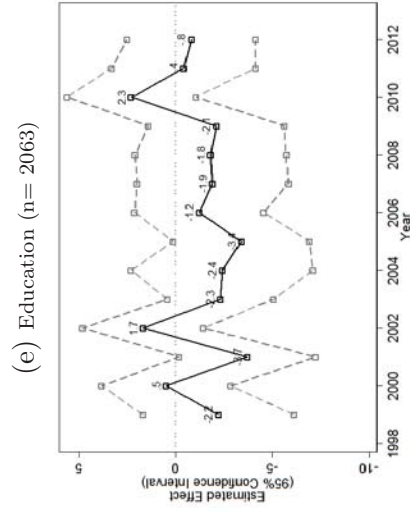
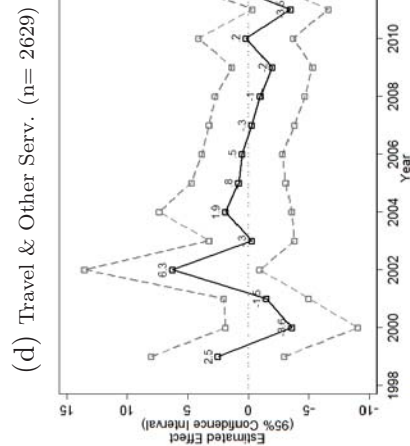
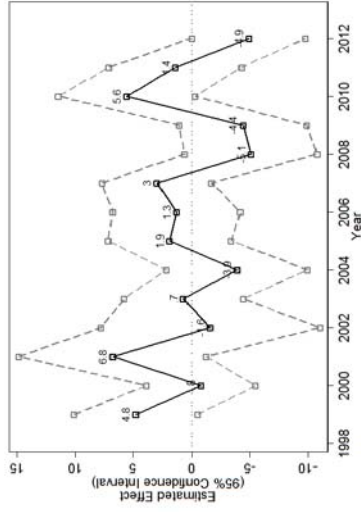


Figure 28: Descriptive evidence for treatment effect: Log(capital stock)

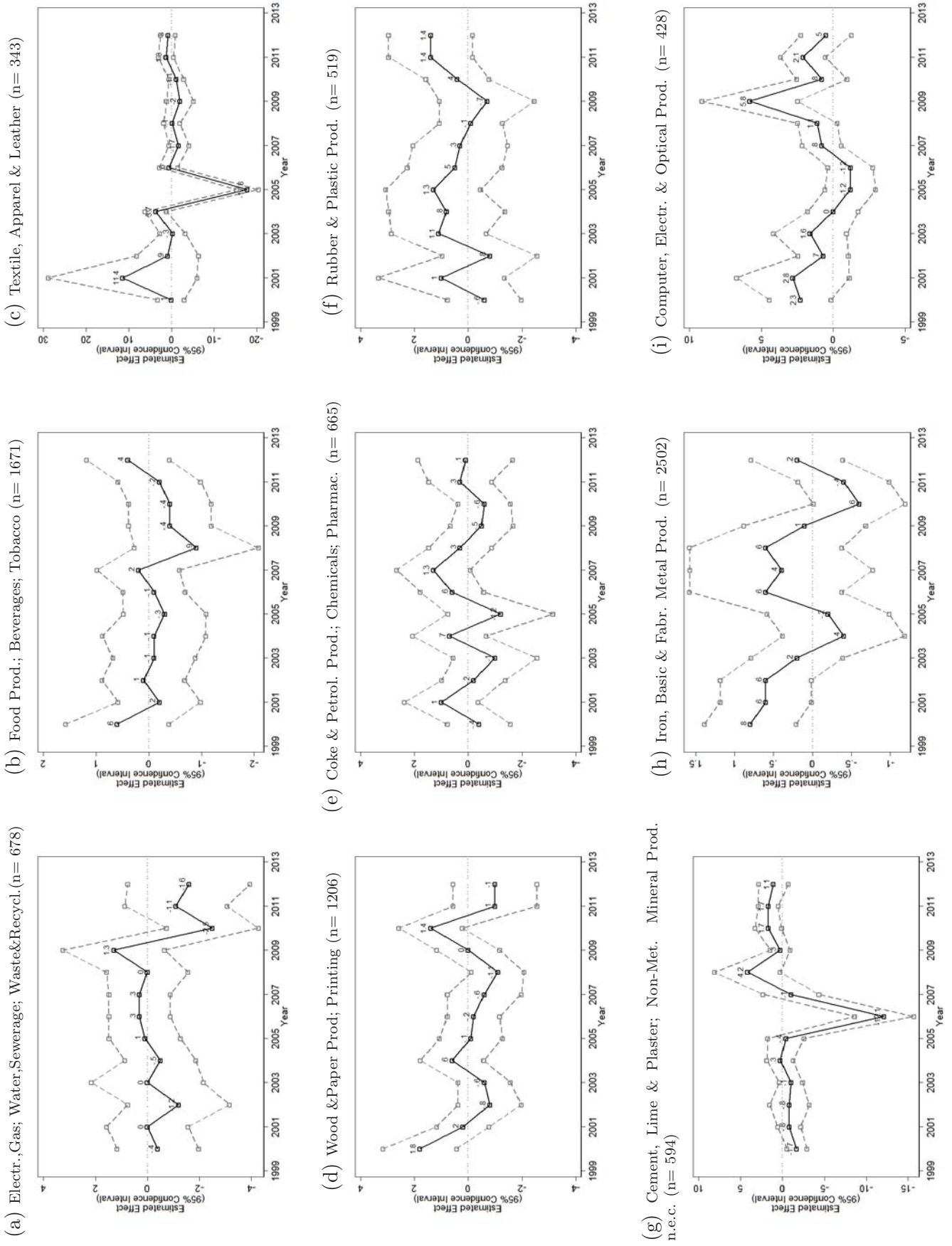
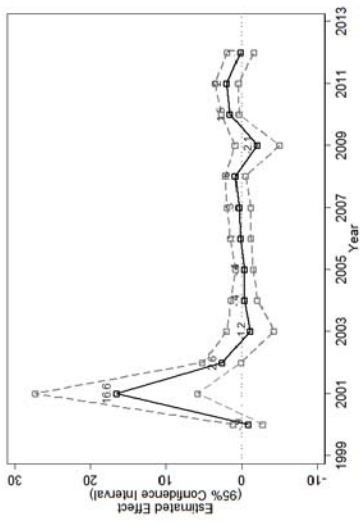
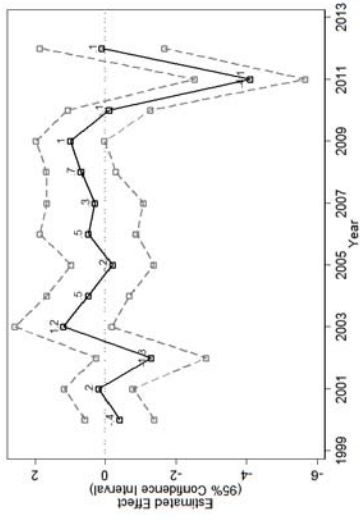


Figure 30: Descriptive evidence for treatment effect: Log(capital stock)

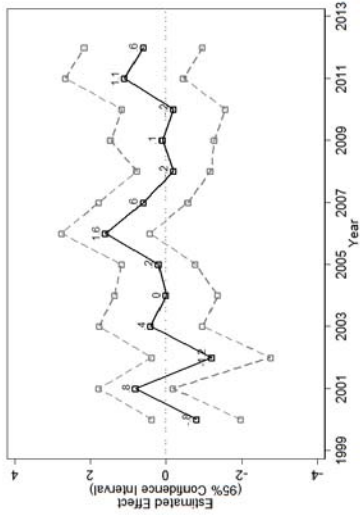
(a) Electrical Equipment (n= 354)



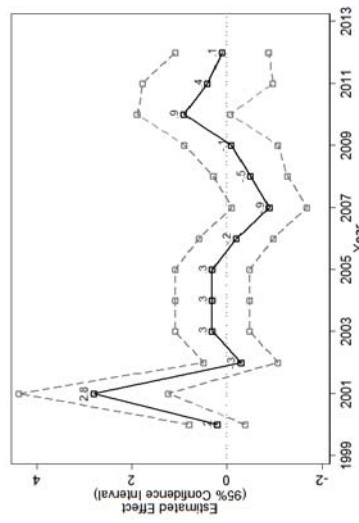
(b) Machinery & Equipment (n= 1005)



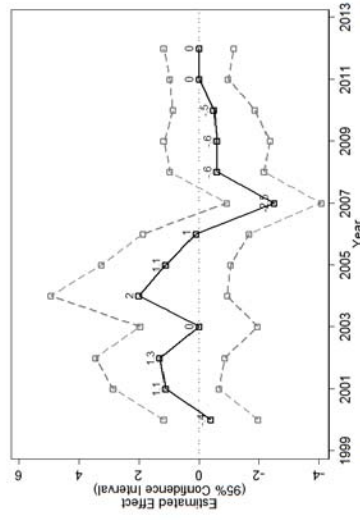
(c) Motor Vehicles & Other Transp. Equipm. (n= 846)



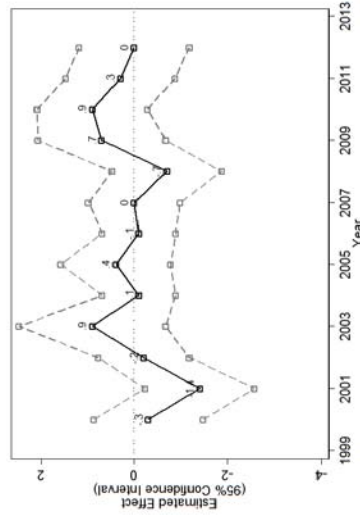
(d) Furniture; Jewellery, Bijouterie & Rel. Articles (n= 1993)



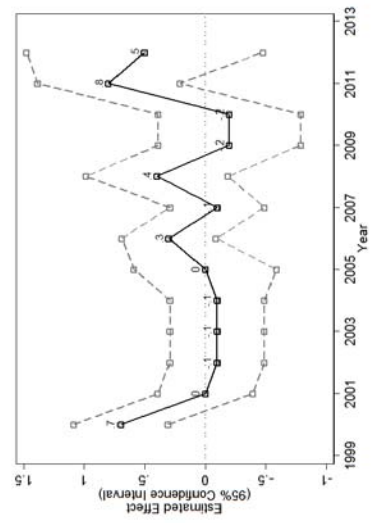
(e) Repair & Install. Machinery & Equipment (n= 550)



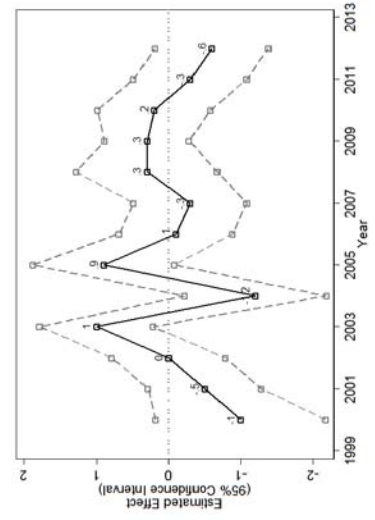
(f) Building & Construction (n= 1167)



(g) Special. Constr. Activities (n= 4267)



(h) Sale, Repair Motor Vehicles (n= 1782)



(i) Wholesale Trade (n= 2510)

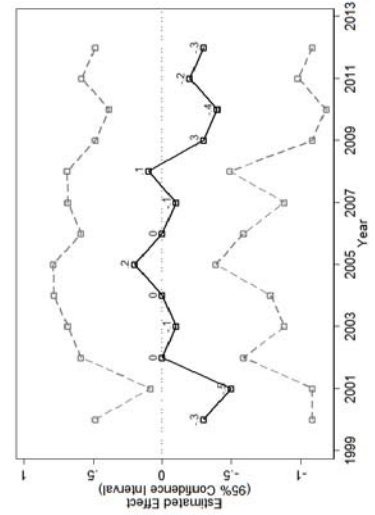
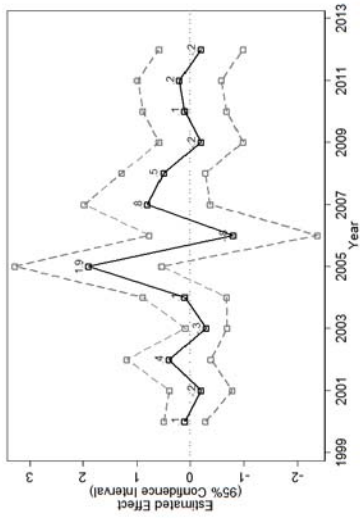
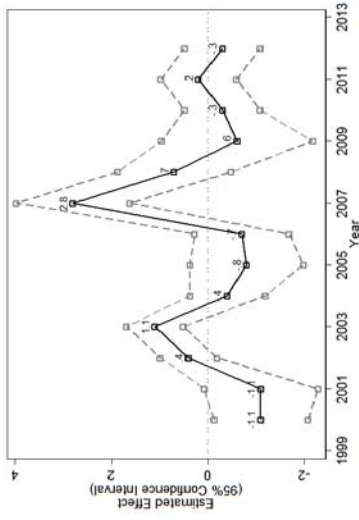


Figure 32: Descriptive evidence for treatment effect: $\text{Log}(\text{capital stock})$

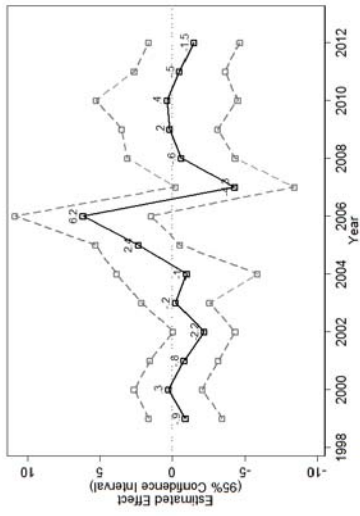
(a) Retail Trade (n= 3354)



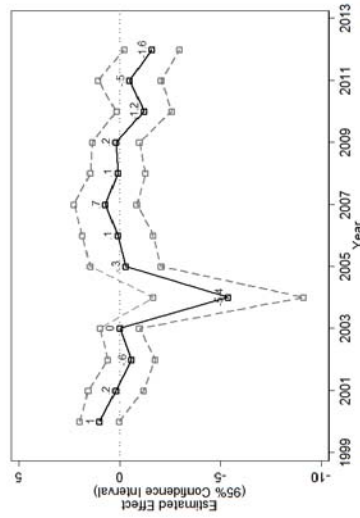
(b) Transport., Warehousing&Storage;Postal Serv.(n= 2197)



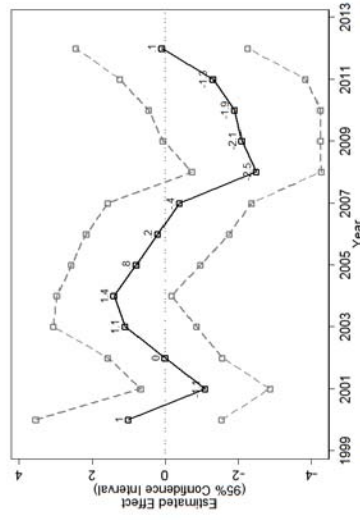
(c) Accommodation & Food Services (n= 1794)



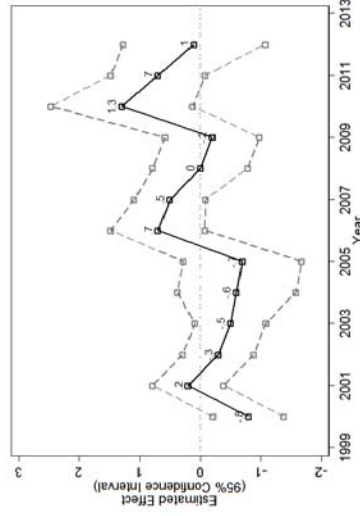
(d) Financial & Insurance Serv. (n= 1301)



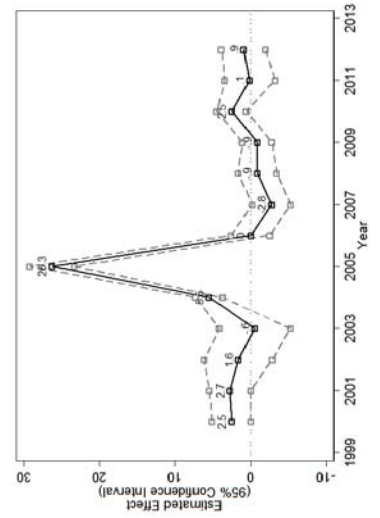
(e) Real Estate Serv. (n= 844)



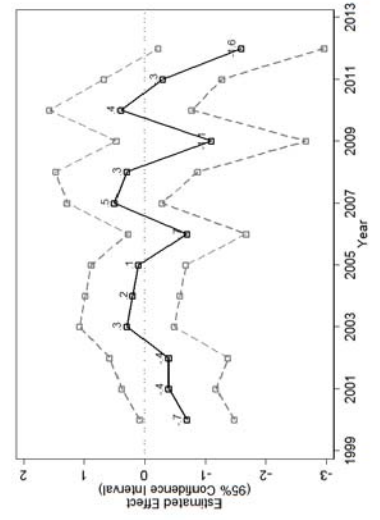
(f) Legal & Accounting Serv. (n= 1255)



(g) Management Consultancy Serv. (n= 515)



(h) Architectural & Engineering Serv. (n= 1669)



(i) Advert. & Market Research; Other Prof., Scientific & Techn. Serv. (n= 485)

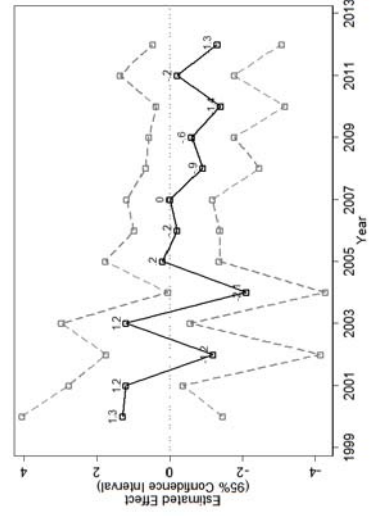


Figure 34: Descriptive evidence for treatment effect: Log(capital stock)

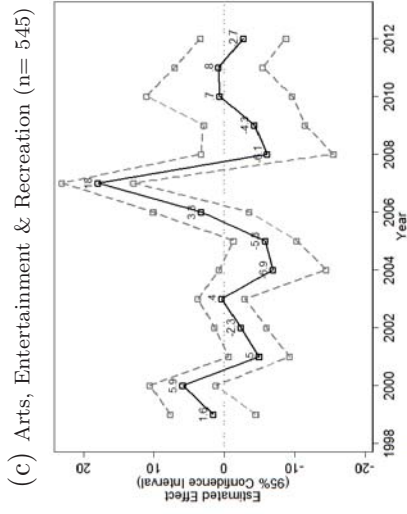
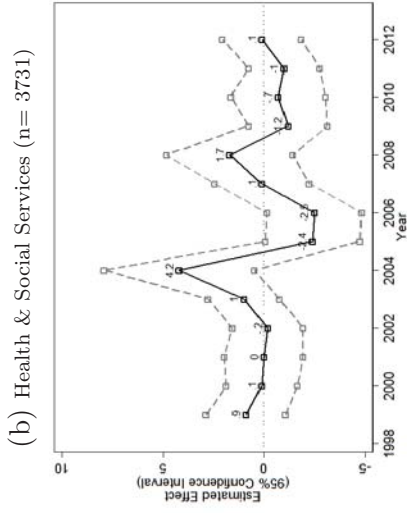
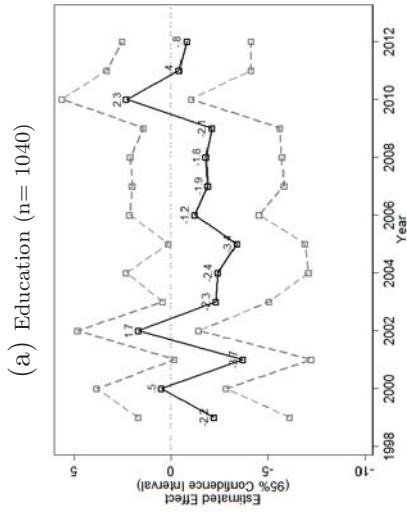
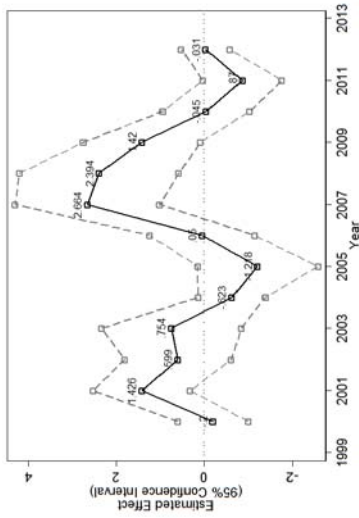
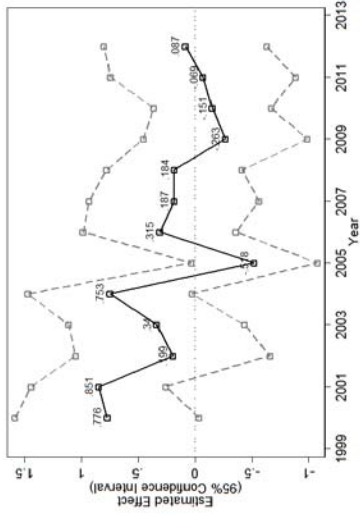


Figure 36: Descriptive evidence for treatment effect: $\text{Log}(\text{labor productivity})$

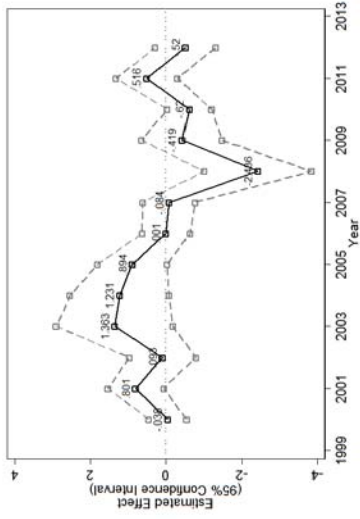
(a) Electr., Gas; Water, Sewerage; Waste & Recycl. (n=652)



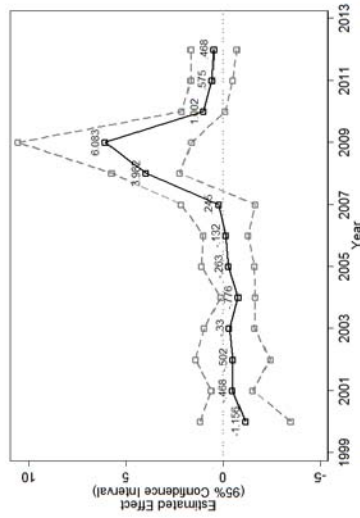
(b) Wood & Paper Prod; Printing (n=1220)



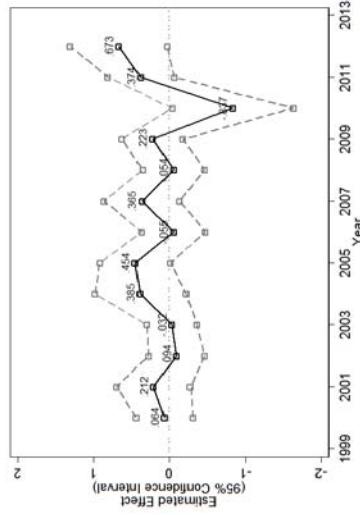
(c) Rubber & Plastic Prod. (n=533)



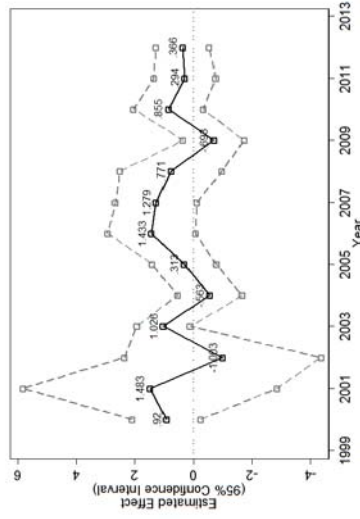
(d) Cement, Lime & Plaster; Non-Met. Mineral Prod. n.e.c. (n=510)



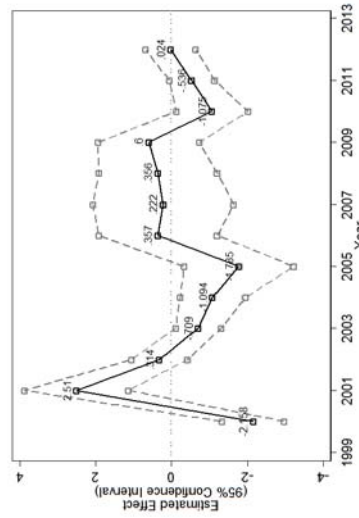
(e) Iron, Basic & Fabr. Metal Prod. (n=2602)



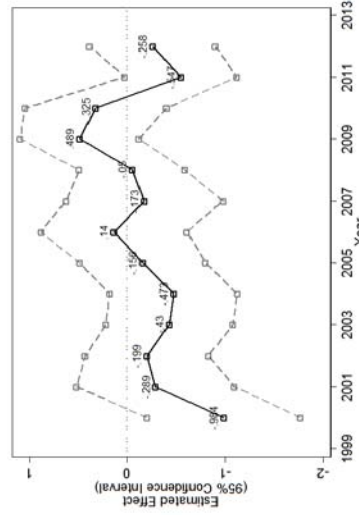
(f) Computer, Electr. & Optical Prod. (n=425)



(g) Electrical Equipment (n=394)



(h) Machinery & Equipment (n=1065)



(i) Motor Vehicles & Other Transp. Equipm. (n=868)

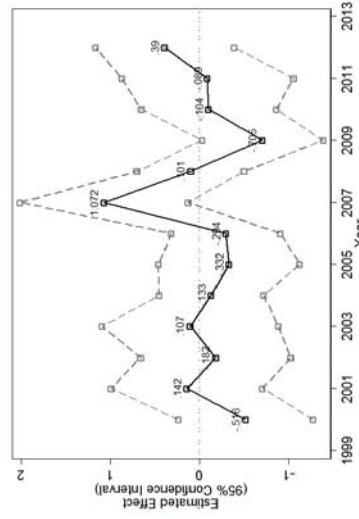


Figure 38: Descriptive evidence for treatment effect: Log(labor productivity)

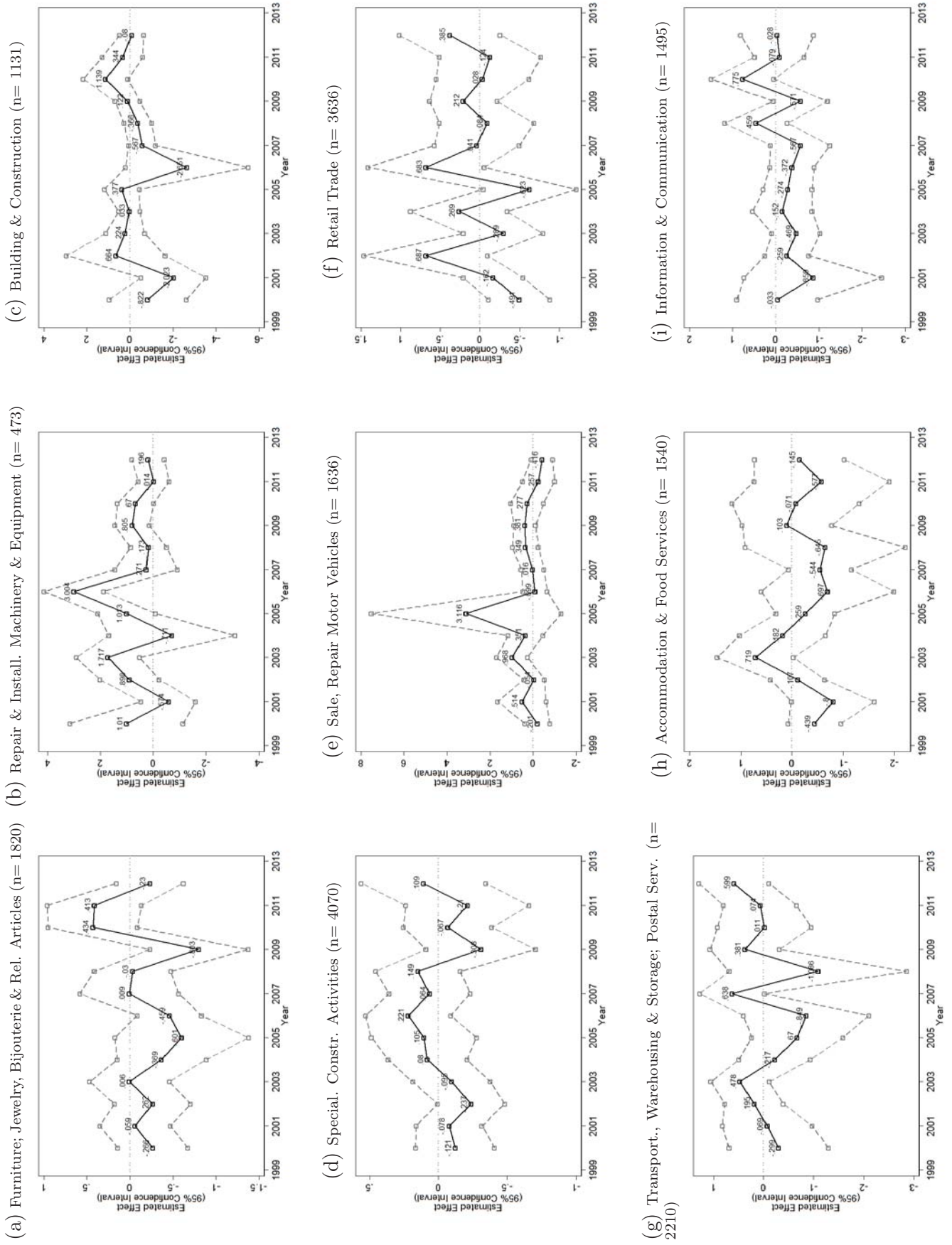
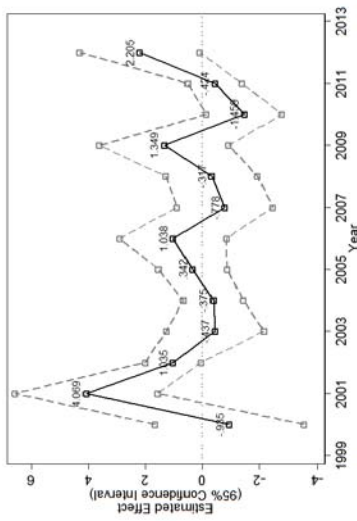
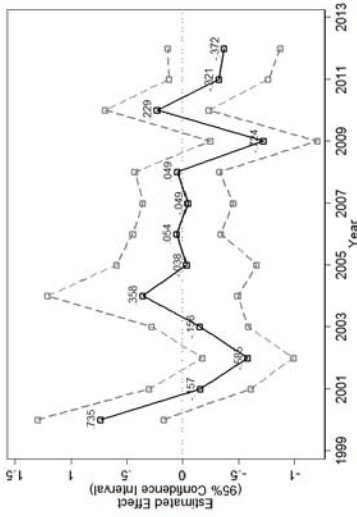


Figure 40: Descriptive evidence for treatment effect: Log(labor productivity)

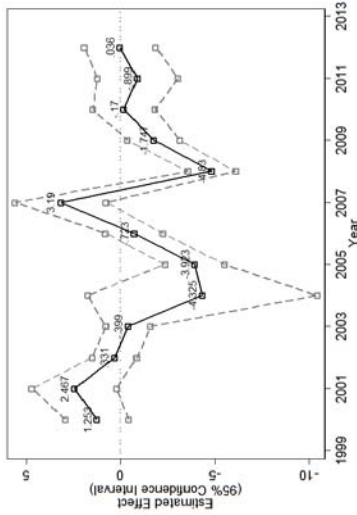
(a) Real Estate Serv. (n= 746)



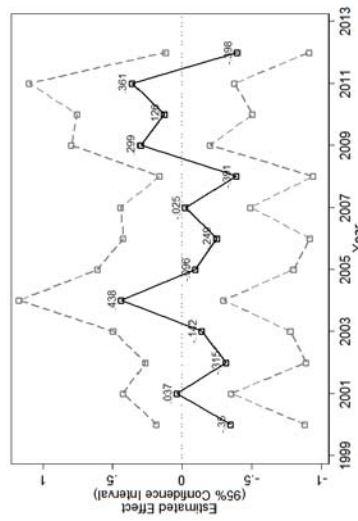
(b) Legal & Accounting Serv. (n= 1129)



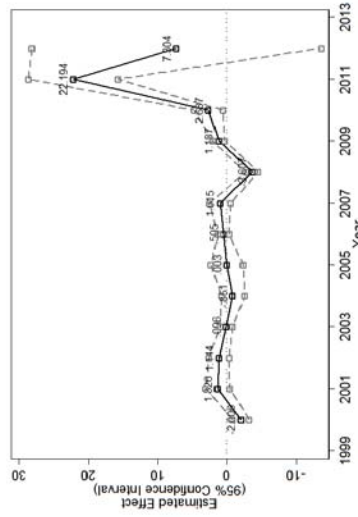
(c) Management Consultancy Serv. (n= 437)



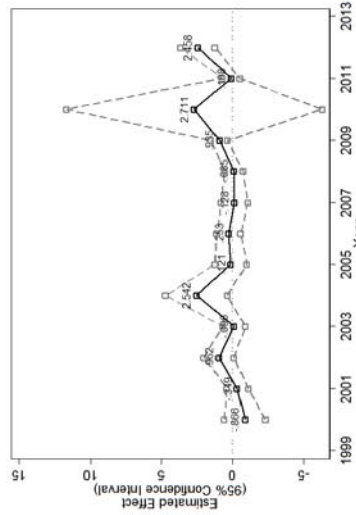
(d) Architectural & Engineering Serv. (n= 1575)



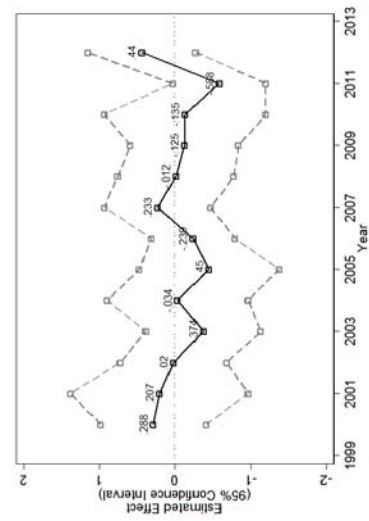
(e) Scientific R&D Serv. (n= 482)



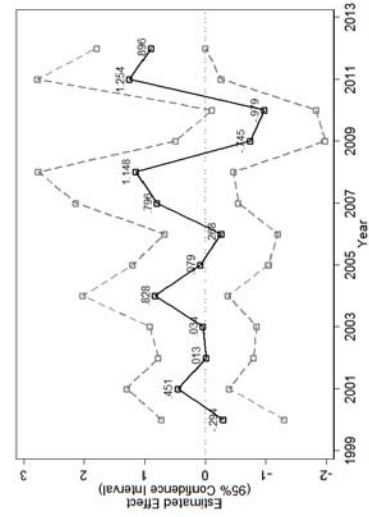
(f) Advert. & Market Research; Other Prof., Scientific & Techn. Serv. (n= 441)



(g) Travel & Other Serv. (n= 1592)



(h) Education (n= 505)



(i) Health & Social Services (n= 2636)

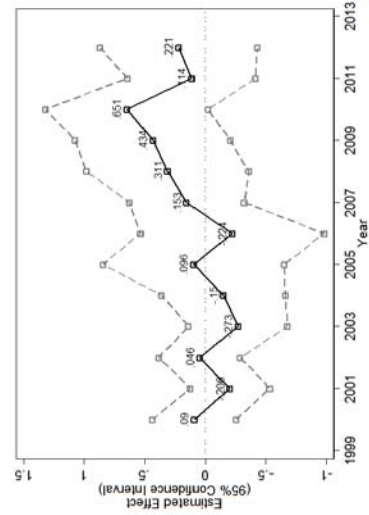


Figure 42: Descriptive evidence for treatment effect: Log(labor productivity)

