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Working Paper

Do public bank guarantees affect labor market outcomes? Evidence from individual employment and wages

IWH Discussion Papers, No. 7/2024

Provided in Cooperation with:

Halle Institute for Economic Research (IWH) – Member of the Leibniz Association

Suggested Citation: Baessler, Laura; Gebhardt, Georg; Gropp, Reint; Güttler, André; Taskin, Ahmet (2024) : Do public bank guarantees affect labor market outcomes? Evidence from individual employment and wages, IWH Discussion Papers, No. 7/2024, Halle Institute for Economic Research (IWH), Halle (Saale), <https://nbn-resolving.de/urn:nbn:de:gbv:3:2-1054621>

This Version is available at:

<https://hdl.handle.net/10419/287750>

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Do Public Bank Guarantees Affect Labor Market Outcomes? Evidence from Individual Employment and Wages

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ISSN 2194-2188

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Abstract

We investigate whether employees in Germany benefit from public bank guarantees in terms of employment probability and wages. To that end, we exploit the removal of public bank guarantees in Germany in 2001 as a quasi-natural experiment. Our results show that bank guarantees lead to higher employment, but lower wage prospects for employees after working in affected establishments. Overall the results suggest that employees do not benefit from bank guarantees.

Keywords: bank guarantees, credit, employment, public banks, wages

JEL classification: E240

1 Introduction

When governments introduce bank guarantees, they routinely justify them as being in the public interest because employees of the bank's borrower benefit. In this paper, we investigate whether this claim is indeed true.

Various studies demonstrate different benefits as well as disadvantages of bank guarantees. The advantages are mainly stabilizing, counter-cyclical macroeconomic effects (e.g. Micco and Panizza, 2006; Bertay, Demirgüç-Kunt, and Huizinga, 2015; Cull and Pería, 2013; Coleman and Feler, 2015). Besides those benefits, there is growing evidence that public banks and guarantees are subject to political lending, lead to inefficient capital allocation, and reduce productivity growth (La Porta, Lopez-De-Silanes, and Shleifer, 2002; Sapienza, 2004, Claessens, Feijen, and Laeven, 2008; Carvalho, 2014; Englmaier and Stowasser, 2017; Gropp, Guettler, and Saadi, 2020). Yet, little is known about their real effects on the borrower's employees' labor market outcomes.

In this paper, we empirically analyze the impact of public bank guarantees on labor market outcomes at the individual level. To do so, we follow existing literature to exploit the natural experiment of the removal of public bank guarantees in 2001 based on the decision of the European Court of Justice (M. Fischer et al., 2014; Gropp, Gruendl, and Guettler, 2014; Gropp, Guettler, and Saadi, 2020). This change was made outside any crisis, which allows us to investigate the effect during good times.

We use bank-firm-level data from all firms borrowing from savings banks in Germany and merge this data at industry-location level with individual-level data from the Institute for Employment Research of the German Federal Employment Agency (IAB). We exploit the panel structure by investigating the labor market outcomes of two cohorts, thereby holding the labor force composition constant. The first cohort of employees in 2000 targets the period over which the bank guarantees removal happened. The second cohort of employees in 1988 covers a period when bank guarantees were still in place. To identify the effect of bank guarantees on labor market outcomes we compare the relative outcomes of the two cohorts using a triple difference approach. We are the first to use this employees' perspective approach to analyze the impacts of bank guarantees on labor

market outcomes.

Overall, our results suggest diverging effects on employment probability and wages. We find that bank guarantees decrease the employment prospects of the workforce but raise the wage prospects of the employed.

Regarding employment probabilities, we observe that for those individuals that work in savings bank-dependent establishments employment prospects improve relative to other industry-locations when public guarantees were lifted. In the cohort employed in 1988, when bank guarantees were still in place, the employees in industry-locations that rely on financing from public banks have a lower probability of still being employed after six years compared to employees in industry-location groups not relying on public banks. They also are more likely to have had spells of unemployment in the past. The cohort of 2000, the first that is affected by the removal of the bank guarantees, closely resembles the 1988 cohort in that the public bank-dependent employees are more likely to have had spells out of employment in their past. Their prospects going forward under the new banking regime, however, look significantly different. There, the probability of remaining in employment is now even higher than their peers who are not public bank financed. A triple differences approach yields a coefficient that implies a significant two percentage point improvement of the employment probability of the 2000 employed financed by public banks relative to their peers not financed by public banks. This effect is driven by a higher probability of remaining at the firm rather than by an increased probability of finding employment at another firm.

We do not find a similarly positive development for wage growth. For the 1988 cohort, there was no difference in wage growth between the industry-locations groups covered by bank guarantees and those that were not. For the 2000 cohort, however, we find a negative effect on wage growth for the public bank-financed groups. Hence, the triple difference estimate delivers a significantly negative effect of bank guarantees on wage prospects. Again, this is driven by the employees staying at their firm. These employees experience a significant one percent lower wage growth relative to their peers in groups not financed by public banks. Overall, the wage loss is small relative to the employment gain. Thus, in the triple difference approach, we observe a ten percent improvement in the wage sum

earned after six years for the 2000 cohort in the public bank financed industry-locations. Our results are robust to several checks. First, to rule out that the effect is caused by bad luck in the choice of the 1988 or 2000 cohort, we test several alternative cohorts, which all show similar patterns. Further, we argue, why we have no reason to believe in other reforms influencing our results. We restrict our data in various ways to eliminate the possibility that specific subgroups mainly drive the results. We test our treatment variable by restricting the samples to more extreme treatment values on both ends. As theory suggests, the results demonstrate higher treatment effects than our main analysis.

We contribute to several strands of the literature. Our paper is most closely related to La Porta, Lopez-De-Silanes, and Shleifer (2002) and Coleman and Feler (2015). Coleman and Feler (2015) provide evidence that localities with higher shares of government bank branches receive more lending and show larger relative increases in the number of employees and wages during the financial crisis in 2008/2009. Using variation over government-owned firms in 92 countries La Porta, Lopez-De-Silanes, and Shleifer (2002), among other things, show that government ownership of banks is associated with lower growth of per capita income and lower growth of productivity. We contribute to this literature by examining the effects of public bank guarantees on an individual level allowing for a policy evaluation during good times from an employee's perspective.

Our research further contributes to the literature on evaluating public banks with guarantees in place. Gropp, Gruendl, and Guettler (2014) use the same natural experiment of the removal of public bank guarantees in 2001 as our research, investigating the bank's risk-taking behavior. They find that the removal of bank guarantees leads to reduced credit risk for savings banks. Investigating public bank's lending behavior during crisis Bertay, Demirgüç-Kunt, and Huizinga (2015), Cull and Pería (2013) and Micco and Panizza (2006) find evidence that lending by public banks is more counter-cyclical than by private banks. This stabilizing effect is one of the public banks' major benefits. Contrarily, during good times, there is evidence that public banks and guarantees are subject to political lending, leading to inefficient capital allocation and reduced productivity growth. On a more aggregated level, Barth, Caprio, and Levine (2001) find that government own-

ership of banks is correlated with less developed financial systems. Gropp, Guettler, and Saadi (2020) find that public bank guarantees in Germany incentivize public banks to screen and monitor borrowers less. Thus, guarantees reduce allocation efficiency and potentially hinder economic growth.

There is further research on the political lending behavior of public banks. Sapienza (2004) suggests that public banks in Italy charge lower interest rates for comparable firms than private banks. Further, she finds that the political affiliations of state-owned banks matter. The higher the electoral results of the affiliated party within the region of the bank, the lower the interest rates this bank charges its borrowers. Similar Dinç (2005) investigates cross-country lending behavior of public banks and finds those banks increased their lending in election years by 0.5% of the median country's GDP relative to private banks. Carvalho (2014) investigates public banks in Brazil. He finds that firms borrowing from public banks expand employment before local elections. Likewise, Englmaier and Stowasser (2017) find evidence for political lending. They show that local elections influence the lending behavior of savings banks in Germany, negatively affecting their profitability.

With our research, we further contribute to the general field of evaluating financial regulations and interventions regarding their impact on labor market outcomes (e.g., Berger and Roman, 2017; Bai, Carvalho, and Phillips, 2018; Fonseca and van Doornik, 2019; Barbosa, Bilan, and Célérier, 2019).

Moreover, our paper is related to research on the effect of the quantity and quality of the firm-bank relationship on employment and wage in those firms (e.g., Chodorow-Reich, 2014; Bentolila, Jansen, and Jiménez, 2018; Behr, Norden, and Freitas Oliveira, 2022)

2 Data

2.1 Data Sets

To investigate the effect of bank guarantees on individual labor market outcomes, we merge proprietary loan information data with the anonymous version of the Sample of

Integrated Labor Market Biographies (SIAB) hosted by the Institute for Employment Research of the German Federal Employment Agency (IAB).

Proprietary loan information This source has already been used by Gropp, Guettler, and Saadi (2020) for analyzing the effect of bank guarantees on capital allocation. It provides self-reported balance sheet information of firms borrowing from savings banks on a firm-bank-year level. Savings banks are public banks, making up about one-third of the entire banking market in Germany (Gropp, Guettler, and Saadi, 2020). Further, savings banks are locally oriented and mainly serve small- and medium-sized firm enterprises. The balance sheet information includes the outstanding loan volume from savings banks and separately from all other banks. Those variables are the basis for the calculation of firms' and groups' savings bank dependence.

Sample of Integrated Labor Market Biographies (SIAB) The individual-level records in SIAB are generated from the Integrated Employment Biographies (IEB), a dataset sponsored by the Institute for Employment Research (IAB) of the Federal German Labor Agency. The IEB is a combination of different sources. The main source is the Employee History data ("Beschäftigten-Historik" (BeH)). The employment history results from the obligation of establishments to report on employees liable to social security. In that regard, establishments report on employees entering and leaving the organization.¹ In addition, there are sources of records from individuals receiving benefits, searching for a job, or participating in employment and training measures.² The SIAB data represent a 2% random population sample and is organized as spell data. Every episode in the individual's biography, such as employment or benefit receipt, is recorded as separate observations with exact start and end dates. The demographic variables for each observation vary depending on the source and include information like gender, age, family status, education, nationality, and place of residence. Additionally, if an individual is employed, SIAB links to employer information from the Establishment History Panel (BHP). BHP

¹An establishment is thereby defined as a regionally and economically delimited unit. Several establishments may belong to the same firm.

²Note that none of the mentioned data sources cover self-employed and government employees.

also derives from the previously mentioned Employee History. It includes further information about establishments, such as the number of (full-time) employees and the average wages of the employees (Frodermann et al., 2021).

Establishment Panel The IAB Establishment Panel is a dataset from a survey of German establishments conducted by the IAB. About 16,000 establishments are surveyed every year with the objective of obtaining information on the demand side of the labor market. The survey was first established in 1993 and among other things contains questions about employment development, business policy and business development, investment activities, personnel structure, recruitment, wages and salaries, and general data on the establishment (G. Fischer et al., 2009; Ellguth, Kohaut, and Möller, 2014).

2.2 Sample Selection

Proprietary loan information As described in the previous subsection, the proprietary loan information data is at the firm-bank-year level. We use it to derive the pre-2001 savings bank dependence of firms. Therefore, we create a firm-level data set by averaging relevant variables on the firm level over the period 1995 to 2000. The obtained data set contains loan information of 156,220 firms.

We do not have any direct firm-establishment link between the loan information and the data from IAB, so we further aggregate the loan information on the 2-digit industry code and location level ³ using the last observed industry of each firm and the location of the bank. The resulting data set covers 304 of 401 existing counties following the territorial allocations as of 31.12.2019 and 57 of 60 available 2-digit industry sectors. This leads to 3,577 industry-location groups with treatment intensity available.

Sample of Integrated Labor Market Biographies (SIAB) For preparing the SIAB sample, we mainly follow the approach from Dauth and Eppelsheimer (2020). The approach is characterized by two main transformations of the data. This is first the

³Firm location information is based on the borrowing bank, which is usually responsible for several counties. Our location information, therefore, summarizes several counties. In total, we observe 74 different locations. For the remainder of this article, we will just refer to them as location.

creation of a yearly panel and second the imputation of wages above the contribution assessment ceiling.⁴

To create a yearly panel, we first remove all parallel episodes from individuals working for two employers simultaneously keeping only the one with the highest wage. Second, we use the cutoff date of June 30 to choose the spell that represents the relevant year for an individual.

We impute the top coded wages following the two-step procedure from Dauth and Epelsheimer (2020) that is based on Dustmann, Ludsteck, and Schönberg (2009) and Card, Heining, and Kline (2013).⁵ Finally, wages are deflated using the consumer price index from the Federal Statistical Office.

We only keep observations from the source of employment history data to avoid any bias due to additional data sources over time and reporting changes, such as the reporting of the marginal part-time employees since 1999. For the same reason, we focus on social security-liable employees and interpret every other employment status as non-employed.

We match the savings bank dependence information from the loan data set on the industry-location level with IAB data, using the last industry and location observed for each establishment in the IAB data.

We arrange our data with the objective to follow up on the labor market outcomes of individuals after employment in a treated or control establishment. We do so for two different cohorts: one for all employed in the year 1988, when bank guarantees were applicable, and one for all employed in the year 2000, shortly before the removal of bank guarantees. For every individual employed during the relevant cohort year, we match observations from previous and following social-security liable employments. We interpret every individual-year observation not matched as non-employment.⁶

We restrict the employees in the relevant cohort year to social security-liable full-time

⁴The IAB data is recorded for social security payment purposes. The size of the payment depends on the salary level. There are lower and upper limits for social security contributions. The wage information is cut at this so-called contribution assessment ceiling.

⁵According to their approach, we run Tobit wage regression for clusters of observations by year, East or West Germany location, and education group. In the regression, we control for the individual characteristics of gender, age, and job duration.

⁶As we use the longitudinal version of SIAB, for every individual observed in the data, we can identify all years they are employed in social security liable occupations. Thus, we may interpret every missing individual-year observation as non-employment.

workers aged between 30 and 53. To avoid any side effects of the German reunification, we further restrict the employees in 2000 to observations from individuals working in Western German firms.

Finally, we receive a balanced panel data with 178,343 individual observations per year for the cohort from 2000 and 150,022 individual observations per year for the cohort from 1988.

Establishment Panel We assign either treatment or control status to each industry-location group in the establishment panel based on the calculated value in SIAB. We are able to assign a treatment status to 14,739 unique establishments over the time period of 1995 to 2006.

3 Identification Strategy

3.1 The Removal of Public Bank Guarantees

To identify the effect of public bank guarantees on labor market outcomes, we build upon the identification strategy and results from Gropp, Guettler, and Saadi (2020). As outlined in the introduction, they investigate the effect of public bank guarantees on capital allocation. Equally to their approach, we exploit the removal of public bank guarantees (in Germany called "Gewährträgerhaftung") in 2001 as a natural experiment. Until 2001, the German government was liable for any obligations of public banks. For many years commercial banks claimed that this would distort competition. In 2001 the European Court of Justice came to the same conclusion and judged that public bank guarantees in Germany violate anti-subsidy rules. In the transition period between 2001 and 2005, guarantees affected fewer loans step by step until their final removal in 2005. The main benefit of the change is that it happened during quiet times without interaction of any crisis.

Gropp, Guettler, and Saadi (2020) outline that looking forward to the complete removal of public bank guarantees in 2005, banks' behavior already changed in advance. Public banks' lending decisions were therefore already affected since the European Court decision in 2001. Other banks than public banks have not been affected by the change in funding

norms and enable us to use them as the control group.

3.2 Assignment of Treated and Control Group

To identify establishments highly dependent on public banks, we use the proprietary loan information data and calculate the share of outstanding loans from savings banks compared to other banks. Following the approach from Gropp, Guettler, and Saadi (2020), we calculate the average outstanding savings bank loans and average loans from other banks between 1995 and 2000 for each firm.

In the next step, we aggregate the average loan volume from savings banks and the average loan volume from other banks at the industry-location level. At the aggregated level we calculate the variable *savings banks dependence* as the ratio of the average savings bank loans to the average total bank loans for each industry-location group (see Equation 2).⁷ We merge the *savings banks dependence* with SIAB data at the industry-location level. Observations in SIAB data from an industry-location group that is potentially covered by the loan data, but the particular combination of industry code and the location is not available, are replaced with a zero *savings banks dependence* in SIAB data.⁸ In total we have a treatment intensity available for 3.619 different industry-location groups.

To account for the fact that the loan information data only contains firms that borrowed from savings banks between 1995 and 2000 at least once, we calculate a proxy for the share of firms within an industry-county group that borrowed from savings banks.⁹ We compare the personnel expenses derived from our balance sheet data with the total sum of wages paid at the industry-county level. In the first case, we need to account for employer contribution to social security, which was approximately 20 percent of the personnel expenses in the year 2000. We can approximate the latter with the SIAB data. As the data is a 2% sample of the whole population of German establishments, we multiply the wage

⁷To test that the savings bank dependence also works on an aggregated level, we reproduce the analysis regarding average loan volume from Gropp, Guettler, and Saadi (2020). We observe a slightly higher decrease in loan volume after the removal of bank guarantees on the aggregated level than on the firm level.

⁸As potentially covered, we define combinations of industry-county groups, for which we observe at least one firm with the respective industry code and at least one firm located in this county in our proprietary loan data set.

⁹As the definition of a firm in the proprietary loan data does not equal the definition of an establishment in IAB data, we can not use the number of firms directly.

sums of each industry-locations group by 50 to estimate the true wage sum.

For our final *treatment intensity* calculation, we multiply the *savings banks dependence* with the share of total wages covered in the loan information data. The resulting formula for calculating treatment intensity is specified as follows:

$$treatment\ intensity_{jl} = savings\ bank\ dependence_{jl} * \frac{0.80 \cdot wage\ sum_{jl}\ LoanData}{50 \cdot wage\ sum_{jl}\ SIAB} \quad (1)$$

with

$$savings\ bank\ dependence_{jl} = \frac{loans\ from\ savings\ banks_{jl}\ 1995-2000}{loans\ from\ all\ banks_{jl}\ 1995-2000}, \quad (2)$$

where jl represents the aggregation at industry j and location l level.

To measure the effect of bank guarantees on labor market outcomes we exploit the variance in treatment intensity. [Table 1](#) displays the average and distribution of the two variables *savings banks dependence* and *treatment intensity* for the year 2000 after merging with the individual-year level SIAB data. We observe a significant variation in the *savings bank loan dependence*. The *treatment intensity* gets scaled down to lower rates when multiplying with the wage share covered by savings banks. Still, we may obtain an adequate variation in our treatment.

Table 1: Distribution of treatment intensity and savings bank dependence in IAB data for the year 2000

	count	mean	sd	p10	p50	p90
savings banks dependence	178,343	0.4187	0.2525	0.0310	0.4136	0.7736
treatment intensity	178,343	0.0708	0.1040	0.0001	0.0308	0.1945

For our main analysis, we are interested in the overall effect of introducing or removing public bank guarantees. Therefore we transform the *treatment intensity* ranging from zero to one, to a *treatment dummy* variable using the median as a split. We are less interested in the causal effect when increasing the treatment dose, which would be an increase in

the dependence on savings banks' loans in our case.¹⁰

Finally, as our approach is to follow up on labor market outcomes of individuals after working in a treated or control establishment, we fix the treatment status of each individual based on the employer in the relevant cohort year. Consequently, a treated individual in our sample is defined as a person who, in the relevant cohort year, works in an establishment allocated to an industry-location highly dependent on savings banks. For reasons of simplification, we just refer to the treated and control for the remainder of this article.

3.3 Main Specification

We use a triple-difference approach to identify the causal effects of bank guarantees on labor market outcomes. Therefore we introduce a second cohort from 1988¹¹. We compare the two difference-in-difference results from the two cohorts using a triple difference approach. The cohort from 1988 may thereby be also interpreted as a placebo test with a placebo treatment taking place in 1988.

This type of triple difference was first introduced by Gruber (1994) and since then used by several authors such as lately Walker (2013). The approach of Walker (2013), who uses longitudinal data on workers and firms to estimate the economic costs of worker reallocation caused by the Clean Air Act Amendments in 1990, is similar to our identification.

Olden and Møen (2022) have recently formally derived the triple difference estimator and the required assumptions. They have shown that the triple difference estimator may be interpreted as the difference between two difference-in-differences. Yet, it relies on only one common trend assumption. This common trend assumption requires the bias in the two difference-in-differences to be the same. In our case, the bias represents the different trends we observe for the treatment and control groups.

Transferred to our setup, the identifying assumption is a common trend of the 1988 and

¹⁰The median is defined over the period 1955-2006 before the removal of guarantees. It splits the data into two almost equal parts on an individual level.

¹¹We chose this period because it is the closest time frame over 12 years that does not overlap with the periods of interest in our 2000 cohort.

2000 cohorts. In other words, the bias between treated and control should be the same for both cohorts. In [section 4](#) we can confirm that the two cohorts show comparable differences between treated and control groups. Any differences constant over time are anyways captured by the elements of the triple difference specification. To further control for time-varying influences we add *age* and *agesquared* as controls to the triple difference design. In addition, we add industry-year and county-year fixed effects.¹² The latter accounts for savings banks investing in different industries and being active to various extents in different counties, who potentially follow different trends (for more information see Appendix section A).

Moreover, we conduct an event study in each section of our results. This event study enables us to visually compare the trends of the two cohorts in the five years before the (placebo) treatment. The estimates always show very similar trends for both cohorts before the intervention. We can conclude that the bias between the treatment and control cohort is the same for both cohorts, which is the necessary identifying assumption for applying triple-difference.

As a robustness check, we test alternative cohorts from different years to rule out that this happens accidentally for our chosen time frame. All time frames present similar event studies and results.

Finally, we derive the following specification for the triple difference approach:

$$\begin{aligned}
Y_{it} = & \beta_1 \cdot \textit{treated}_{ijlc} & (3) \\
& + \beta_2 \cdot \textit{treated}_{ijlc} \cdot \textit{6yearsafter}_{tc} \\
& + \beta_3 \cdot \textit{treated}_{ijlc} \cdot \textit{guarantees}_c \\
& + \beta_4 \cdot \textit{guarantees}_c \cdot \textit{treated}_{ijlc} \cdot \textit{6yearsafter}_{tc} \\
& + \textit{age}_{it} + \textit{agesquared}_{it} + \alpha_{jtc} + \alpha_{ltc} + \epsilon_{it} ,
\end{aligned}$$

where Y_{it} is the outcome of interest, such as the wage or employment status of individual i in year t , $\textit{treated}_{ijlc}$ equals one if an individual i is employed in an establishment associated

¹²Note that the fixed effects are based on the industry and location information of the establishment an individual is employed at in the cohort year (1988 respectively 2000) Thus, they capture the trends of all individuals engaged in an establishment from a particular location or industry in the year 2000, even though individuals may change the establishment or become non-employed afterward.

with a treated industry-location group jl in the cohort year c and zero if it is employed in a control establishment in the cohort year, $6yearsafter_{tc}$ is a variable that equals one for the year, six years after the base year, which is 2006 for our 2000 cohort and 1994 for our 1988 cohort, $guarantees_c$ equals one for the 1988 cohort when guarantees were in place over the whole observation period. The variable equals zero for all individual-year observations from the 2000 cohort. age and $agesquared$ represent the age and the squared age of an individual in year t . α_{jtc} are industry-year fixed effects and α_{ltc} location-year fixed effects. The coefficient of interest is β_4 , which identifies the causal effect of public bank guarantees on the outcome by estimating the relative change¹³ in outcome for the 2000 cohort compared to the 1988 cohort.

Regarding inference, Olden and Møen (2022) have outlined similar issues as for difference-in-difference analysis (shown by, e.g., Bertrand, Duflo, and Mullainathan, 2004, Cameron and Miller, 2015). We address the serial correlation issue with micro-panel data by limiting our main regression to a two-year comparison. We compare the outcomes in the year before the removal of guarantees (2000) and respectively the placebo treatment (1988) with those six years later (2006, respectively 1994). To address potential remaining within-group correlation, we cluster standard errors at the treatment level (industry-location level).¹⁴

4 Descriptive Statistics

Table 2 presents the summary statistics of the 156,220 firms we observe in the loan information data after averaging firm data from 1995 to 2000. Customers of savings banks are mainly small and medium-sized companies with an average expenditure for personnel per year of 1.13 million and average yearly sales of 5.36 million. The calculated average loan volume from savings banks and other banks is the basis for our treatment intensity calculation, as explained in subsection 3.2.

Table 3 shows average values of the most important variables for the treated and control groups, separately for both cohorts. Regarding the difference between treatment and

¹³Relative regarding the difference between treatment and control group.

¹⁴We observe 3.619 different industry-location groups in our data set, which is, according to Bertrand, Duflo, and Mullainathan (2004) sufficient to use clustering as a correction for within-group correlation.

control groups economically relevant are in particular the daily imputed wage, the share of low-skilled workers employed, no. of (full-time) employees in establishments¹⁵ and employment history.

Table 2: Characteristics of firms in proprietary loan data sample

	no. of observations	mean	sd	min	p50	max
loans all banks (in millions)	156,220	1.693	18.735	0.000	0.277	2,677.404
loans savings banks (in millions)	156,220	0.508	2.376	0	0.141	407.425
personnel expenditure per year (in millions)	146,625	1.130	9.444	0.000	0.236	1,613.228
sales per year (in millions)	154,417	5.362	71.438	0.000	0.895	15,251.310
no. of employees	45,597	77	1,820	0	17	377,450

Table 3: Averages of most important variables in treatment and control group over five years before the (placebo) intervention
For further explanation of variables see Appendix B.

	1988 Cohort			2000 Cohort		
	Control	Treated	Difference	Control	Treated	Difference
treatment intensity	0.008	0.131	-0.123***	0.009	0.132	-0.123***
daily imp. wage	119.613	102.970	16.644 ***	130.633	110.886	19.747***
age	38.886	38.846	0.041***	37.988	37.801	0.188***
duration of non-employment years in estab.	0.494	0.551	-0.056***	0.638	0.687	-0.049***
no. employees in estab.	2,967.471	479.670	2,487.801***	1,992.840	403.749	1,589.091***
no. of FTE in estab.	2,750.377	431.880	2,318.497***	1,768.428	342.805	1,425.623***
establishment age	9.906	9.544	0.361***	17.719	16.886	0.833***
part-time	0.0117	0.0118	-0.0001	0.0192	0.0179	0.0013***
woman	0.2819	0.2887	-0.0069***	0.3207	0.3020	0.0187***
low skilled	0.7750	0.8385	-0.0635***	0.7548	0.8270	-0.0722***
working in cohort year estab. employed	0.7964	0.7666	0.0298***	0.7072	0.7003	0.0069***
	0.9176	0.9082	0.0094***	0.8937	0.8856	0.0081***

As outlined in [subsection 3.3](#) we use a triple difference approach to estimate the causal effect of bank guarantees on labor market outcomes. The triple difference can be interpreted as the difference between two difference-in-differences. The first difference-in-difference is calculated from the 1988 cohort and the second from the 2000 cohort. For this approach, the similarity of the two cohorts is central. The differences between treated and control groups should be the same for both cohorts. Therefore, we compare the differences between the control and treated groups of both cohorts.

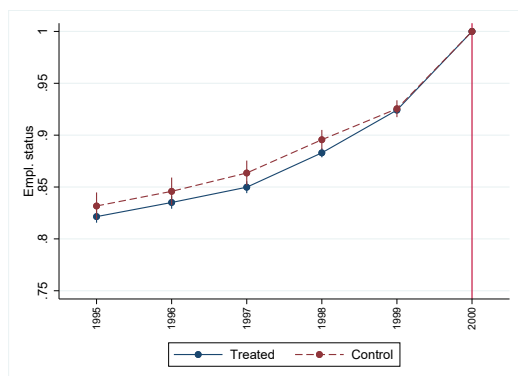
For most variables, the sign and size of the difference are very similar to the 1988 cohort.

¹⁵This difference is overestimated due to the individual-level data.

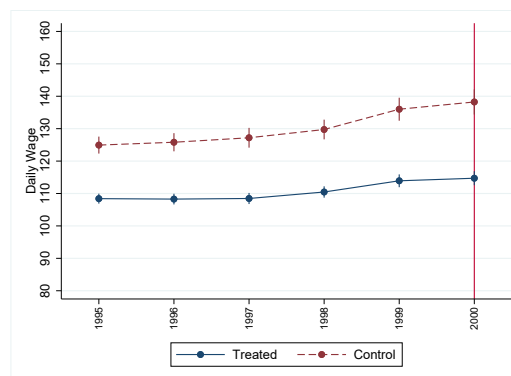
Exceptions are the yearly wages, the average number of employees in establishments, establishment age, the share of women, and the years an individual already working in the establishment.

The triple difference approach already captures any potential omitted variables constant over time, such as whether individuals come from small or large establishments, younger or older establishments, the share of women in the establishments, and how many years employees have already been in the establishment. Moreover, the triple difference specification controls for any economic trends. Therefore, we only add age as an additional time-varying control variable. We can not control for wages, as we use this variable on the left-hand side of our regressions. Anyhow, we closely observe common pre-trends of the 1988 and 2000 cohorts in this respect.

The charts in [Figure 1](#) describe our primary outcome variables, employment status, and wages in the years before the removal of guarantees. The employment status is a dummy variable that equals one when an individual is employed in a social security liable occupation and zero otherwise. As described in the previous section, the wage is a deflated and imputed daily value. [Figure 1a](#) illustrates the set up of the data set, with all individuals employed in 2000. The previous years, therefore, show values below one, indicating the share of individuals employed. The employment may be either in the same establishment as the one in the year 2000 or in another establishment. We observe significant differences in the level of employment status as well as wages between the treatment and control groups. Individuals working in treated establishments in 2000 tended to have higher non-employment probabilities in the five years before. Average salaries in treated establishments are generally significantly lower than in control establishments ([Figure 1b](#)).



(a) employment status over time



(b) average daily (imputed, deflated) wage

Figure 1: Main outcome variables in the five years before the removal of public bank guarantees

Using the establishment panel data we create further descriptive statistics to verify the identification of the treatment and control groups. In the survey, establishments are asked whether they have difficulties with access to financing. One would expect that in particular in the following years after the removal of bank guarantees, treated establishments have more often financing problems, as the loan volume issued by public banks decreased (Gropp, Guettler, and Saadi, 2020). The data confirms our expectations. In total 6,027 establishments have been asked about financing issues in the years 2004 and 2005. In the treated groups 11.77 percent of establishments stated that they experience financing issues, whereas, in the control groups, only 7.60 percent answered the question with "yes". The difference is significant at the 5 percent level.¹⁶

¹⁶We are not able to conduct a comparison before and after the removal of guarantees because the questions about financing issues have only been part of the survey in the years 2004 and 2005.

5 Results

5.1 Employment Effect

As outlined in the introduction, a common argument for bank guarantees are benefits for employees, which is, in the first place generating and preserving jobs with a fair salary. Therefore, we start our analysis by investigating the effect of bank guarantees on employment prospects holding constant the workforce.

Because of the nature of our data set all observed individuals are employed in the cohort year. Thus we do not receive any additional variation from observations in the cohort year. For this reason, we drop observations from the cohort year, which leaves us with the observations six years after the placebo intervention (1994) and six years after the removal of guarantees (2006). This simplifies Equation 3 by omitting the variable $6yearsafter_{tc}$. Our outcome variable Y_{it} in the specification is the employment status, which equals one if an individual is employed in a social-security-liable occupation and zero otherwise.

Table 4:

Effect of public bank guarantees on the employment probability of (former) employees: comparison to six years after the removal/placebo

Column (1) of this table shows the estimates for the cohort 1988. Respectively column (2) displays the estimates for the cohort from 2000. The third column uses a simplified version of Equation 3 comparing the outcomes of the two cohorts. The sample is the individual-level balanced panel from IAB. It is restricted to the years six years after the (placebo) intervention (1994 and 2006). As an outcome, we analyze an individual's employment status, which equals one in the case of social-security liable employment and zero otherwise. *Treated* is a dummy variable equal to one for individuals working in an establishment in 2000, allocated to an industries-location group borrowing from savings banks more than the median group and zero otherwise. *Guarantee* is a dummy variable that equals one for placebo years 1988 and 1994, where guarantees were in place. The dummy variable *6yearsafter* equals one for 2006 and 1994, respectively, six years after the removal or placebo treatment. All other variables are as defined in section B in the appendix. Standard errors, shown in parenthesis, are two-way clustered at the industry and location level. * show percent levels with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent variable: employment status			
	(1)	(2)	(3)	(4)
	Cohort 1988	Cohort 2000	Triple Diff.	Triple Diff. with controls
treated	-0.0153*** (0.0041)	0.0046 (0.0036)	0.0046 (0.0036)	0.0045 (0.0036)
treated X guarantees			-0.0200*** (0.0051)	-0.0202*** (0.0050)
age	No	No	No	Yes
age squared	No	No	No	Yes
industry-year FE	Yes	Yes	Yes	Yes
county-year FE	Yes	Yes	Yes	Yes
R-squared	0.0159	0.0178	0.0169	0.0494
N. of observations	150,022	178,343	328,365	328,365

The results of the employment prospect analysis are displayed in Table 4. We apply

a three-step procedure to understand the underlying dynamics of the effect. We first investigate employment probabilities with bank guarantees in place using a difference-in-difference approach with the data from the 1988 cohort (column (1)). That means we also drop the variable $guarantees_c$ in the specification. We apply the same procedure for the 2000 cohort (column (2)). In a third step, we quantify the difference between the 1988 and 2000 cohorts using the above-described specification (column (3)).

We observe that for the 1988 cohort with bank guarantees in place, the probability of being employed six years after working in a treated establishment is about 1.5 percentage points lower compared to those working at control establishments (column (1)). This is a sizable effect given that about 20% of the observed individuals in our data set are non-employed after six years. In contrast, the 2000 cohort, affected by the removal of bank guarantees, shows a positive coefficient (column (2)) indicating better employment prospects for treated than for control individuals. Taken together using the triple difference specification, we find that bank guarantees reduce employment probability in the long term by about 2 percentage points (column (3)).

We complement our results by investigating the development of the effect over several years using an event study design separately for the 1988 and the 2000 cohorts. We do so, first, to investigate whether the triple difference approach fulfills the required common trend assumption and second, to gain insights into the timing of the effect after the removal of public bank guarantees. To obtain yearly estimates, we remove the third difference represented by the variable $guarantees_c$ from [Equation 3](#) and replace the variable $6yearsafter_{tc}$ with dummy variables for each year $year_t$. [Figure 2](#) shows the results of the two event studies, where the 1988 cohort (guarantees) is displayed in grey and the 2000 cohort (guarantees removal) is represented in blue. The basic year 1988 and respectively 2000 is marked with a red line. The negative and positive numbers at the x-axis display the leads and lags.

Looking at the yearly coefficients, six years before and five years after the removal of guarantees (blue line), respectively, and the guarantees data (grey line), we observe very similar pre-trends for the two cohorts regarding the employment history of employees. This underlines the common trend assumption for using the triple difference approach.

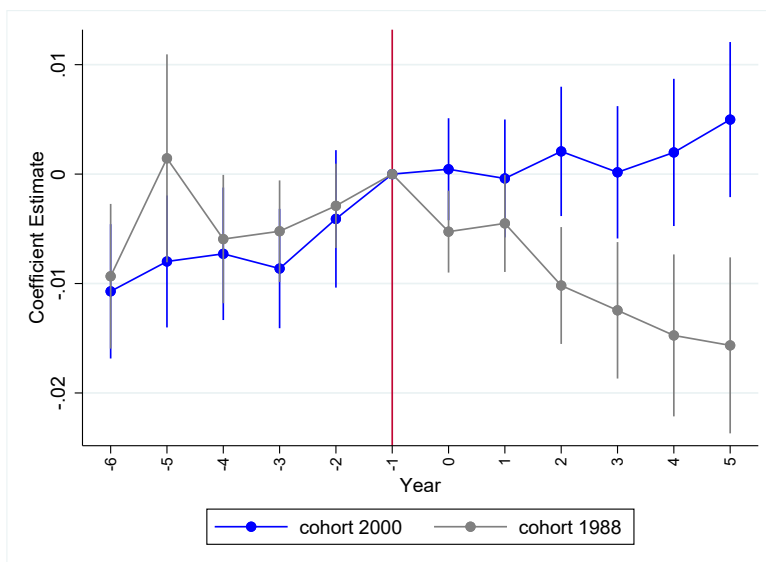


Figure 2: Event Study Results For the 1988 (guarantees) and 2000 Cohort (removal of guarantees) regarding Employment Probability

The graph illustrates our previous findings that employees in establishments financed by savings banks have lower future employment probabilities compared to the control group. After the removal, the employment prospects develop more positively. This effect especially becomes apparent approximately three years after the removal of guarantees. After six years without guarantees, treated employees have even slightly better prospects than their counterparts from the control group.

5.2 Wage Effect

We continue our analysis of the effects of bank guarantees on labor market outcomes for the workforce by investigating wage developments of employed.

Table 5 represents the results of the wage analysis. As a basis, we use Equation 3 replacing the outcome Y_{it} with the log wage of individuals employed in a social-security-liable occupation. Thus, the size of our sample shrinks to only those employed.¹⁷ Equivalent to the approach in subsection 5.1 we apply a three-step procedure to gain insights into the sources of the wage effect. Therefore, we use a specification without the third difference ($guarantees_c$) in the first and second columns.¹⁸

¹⁷This also means that our data set is not balanced anymore.

¹⁸But in contrast to the employment analysis, we now include both the cohort year and the observations six years later because wages obviously differ in the cohort year.

Table 5:

Effect of public bank guarantees on the wage of employees: comparison to six years after the removal/placebo

Column (1) of this table shows the difference-in-difference estimates for the cohort 1988. Respectively column (2) displays the difference-in-difference estimates for the cohort from 2000. The third column uses Equation 3 for triple difference estimation comparing the two cohorts. The sample is the individual-level balanced panel from IAB. It is restricted to the year of the cohort and the year six years later (1988 and 1994, 2000 and 2006). As an outcome, we analyze an employee's imputed log wage. *Treated* is a dummy variable equal to one for individuals working in an establishment in 2000, allocated to an industries-location group borrowing from savings banks more than the median group and zero otherwise. *Guarantee* is a dummy variable that equals one for placebo years 1988 and 1994, where guarantees were in place. The dummy variable *6yearsafter* equals one for 2006 and 1994, respectively, six years after the removal or placebo treatment. All other variables are as defined in section B in the appendix. Standard errors, shown in parenthesis, are two-way clustered at the industry and location level. * show percent levels with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent variable: log wage			
	(1)	(2)	(3)	(4)
	Cohort 1988	Cohort 2000	Triple Diff.	Triple Diff. with controls
treated	-0.0446*** (0.0073)	-0.0474*** (0.0072)	-0.0474*** (0.0072)	-0.0478*** (0.0073)
treated X 6-years-after	0.0022 (0.0034)	-0.0079** (0.0035)	-0.0079** (0.0035)	-0.0074** (0.0035)
treated X guarantees			0.0028 (0.0066)	0.0034 (0.0065)
treated X guarantees X 6-years-after			0.0101** (0.0048)	0.0094** (0.0048)
age	No	No	No	Yes
age squared	No	No	No	Yes
industry-year FE	Yes	Yes	Yes	Yes
county-year FE	Yes	Yes	Yes	Yes
R-squared	0.1777	0.1975	0.1896	0.1921
N. of observations	268,712	318,824	587,536	587,536

We observe, that bank guarantees lead to a 1 percent increase in wages (column (3)). This effect is mainly driven by a drop in wages for the 2000 cohort after the removal of bank guarantees (column (2)). On the other hand, bank guarantees for the cohort 1988 lead to similar wage development than in the control group (column (1)).

We can conclude that part of the additional loan volume issued while bank guarantees were in place is transferred to employees in the form of higher wages. Nevertheless, considering the previous chapter's results, this comes at the price of less job security.

To gain deeper insights into the effect we repeat the event studies for the two cohorts as done in the previous chapter. The results are displayed in Figure 3. We can see from the graph that even though we observe larger and smaller gaps between the 1988 and 2000 cohorts, the directions and trends of wages before the (placebo) intervention are the same for both groups. They keep similar until three years after the removal of guarantees and then diverge in opposite directions. This supports the common trend assumption and

reveals that the removal of guarantees leads to a reduction in wages approximately four years after working in a treated establishment.

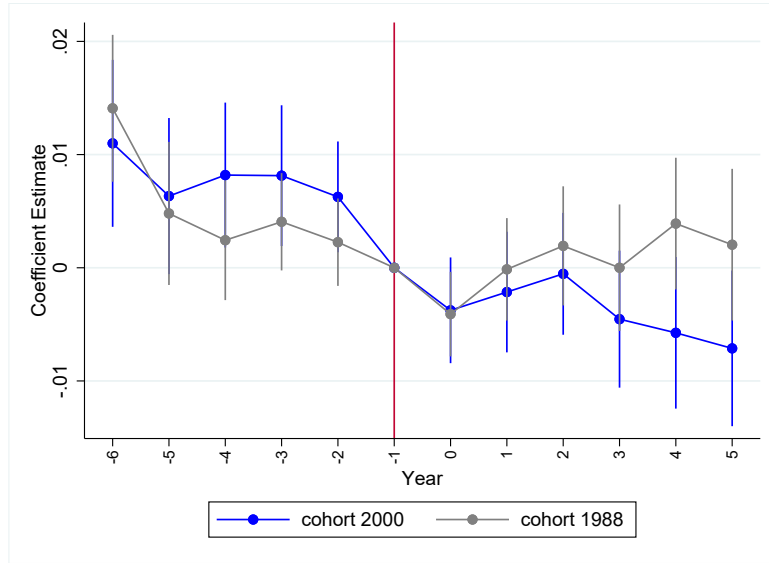


Figure 3: Event Study Results For the 1988 (guarantees) and 2000 Cohort (removal of guarantees) regarding Wages

5.3 Wage Sum Effect

In this section, we summarize previous findings and quantify the overall effects of bank guarantees on an individual's wage sum. The wage sum represents an individual's income from work and is defined as zero in case of non-employment. We plug in this variable for the outcome Y_{it} in Equation 3. The estimated coefficients may be found in Table 6. The results show the joint effect of bank guarantees on an individual's labor market outcomes stemming from employment probability and wage prospects.

We find that bank guarantees lower wage sum prospects by about 9 percent (column (3)). The results are statistically significant at the 1 percent level. Consequently, the worse employment probabilities when guarantees are in place offset the wage benefits and lead to an overall negative effect of bank guarantees on the labor market prospects of the workforce.

Table 6:

Effect of public bank guarantees on the wage sum of (former) employees: comparison to six years after the removal/placebo

Column (1) of this table shows the difference-in-difference estimates for the cohort 1988. Respectively column (2) displays the difference-in-difference estimates for the cohort from 2000. The third column uses Equation 3 for triple difference estimation comparing the two cohorts. The sample is the individual-level balanced panel from IAB. It is restricted to the year of the cohort and the year six years later (1988 and 1994, 2000 and 2006). As an outcome, we analyze an individual's wage sum, which equals the log wage for employees and zero for non-employed. *Treated* is a dummy variable equal to one for individuals working in an establishment in 2000, allocated to an industries-location group borrowing from savings banks more than the median group and zero otherwise. *Guarantee* is a dummy variable that equals one for placebo years 1988 and 1994, where guarantees were in place. The dummy variable *6yearsafter* equals one for 2006 and 1994, respectively, six years after the removal or placebo treatment. All other variables are as defined in section B in the appendix. Standard errors, shown in parenthesis, are two-way clustered at the industry and location level. * show percent levels with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent variable: wage sum (wage if employed, zero otherwise)			
	(1)	(2)	(3)	(4)
	Cohort 1988	Cohort 2000	Triple Diff.	Triple Diff. with controls
treated	-0.0446*** (0.0073)	-0.0474*** (0.0072)	-0.0474*** (0.0072)	-0.0489*** (0.0073)
treated X 6-years-after	-0.0615*** (0.0194)	0.0276* (0.0166)	0.0276* (0.0166)	0.0296* (0.0164)
treated X guarantees			0.0028 (0.0066)	0.0047 (0.0065)
treated X guarantees X 6-years-after			-0.0890*** (0.0241)	-0.0933*** (0.0238)
age	No	No	No	Yes
age squared	No	No	No	Yes
industry-year FE	Yes	Yes	Yes	Yes
county-year FE	Yes	Yes	Yes	Yes
R-squared	0.1241	0.1445	0.1354	0.1552
N. of observations	300,044	356,686	656,730	656,730

As in the sections before, we analyze wage sum trends over several years using an event study design for the 1988 and 2000 cohorts separately. The resulting coefficients are plotted in Figure 4. The graph confirms similar wage sum trends for employees before the (placebo) intervention. We can assume that the common trends assumption required for applying triple difference is thus fulfilled. After the removal of bank guarantees, the wages of some individuals from the two cohorts develop in opposite directions. This development slowly starts in the first year after the removal and significantly increases three to six years afterward. These trends yield the estimated negative effect of bank guarantees on the wage sum and illustrate that it mainly derives from comparably negative wage sum prospects for the 1988 cohort.

To sum up, the additional loan volume issued when bank guarantees are in place is not for the benefit of the overall workforce. Only a special part of the workforce benefits from bank guarantees in the form of higher wages. On the other side large parts of the

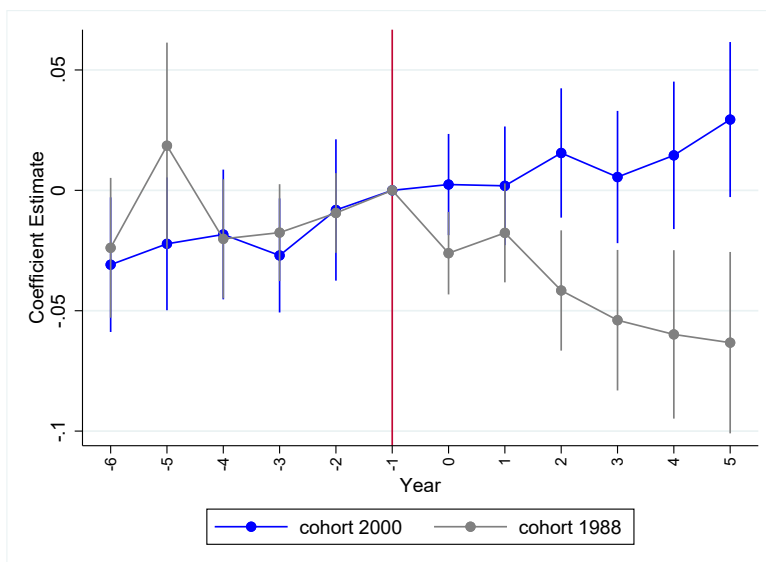


Figure 4: Difference-In-Difference Results For 1988 and 2000 Cohort

workforce experience higher probabilities of job loss when guarantees are in place. In this section, we showed that overall less money is transferred in the form of wages to the workforce when guarantees are in place.

In the following sections, we further investigate the sources of the observed opposing effects.

5.4 The Source of Job Security: Analysis on Stayers and Leavers

As outlined in [subsection 5.1](#) we find that after the removal of bank guarantees, employment prospects increase. There are two potential sources for this effect. Either employees stay longer in the treated establishment or they have a higher probability of finding alternative employers potentially before or at least shortly after a period of unemployment. This section aims to shed light on those mechanisms.

For our analysis, we replace the outcome variable Y_{it} in the triple difference approach ([Equation 3](#)) with the variable $baseYearEstab$. This variable is a dummy that equals one in case an individual is still employed in the same establishment as in the cohort year. As in [subsection 5.1](#) we only consider the period six years after the (placebo) intervention for our analysis, as all individuals are employed in the cohort establishment in the cohort year, which leads to the simplified version of [Equation 3](#) without the variable $6yearsafter_{tc}$.

The results are displayed in [Table 7](#). We observe that with guarantees in place, people are about 2.5 percentage points less likely to stay with the same employer six years after the intervention (columns (3) and (4)). This effect derives from a significantly negative effect of bank guarantees on "staying" represented by the 1988 cohort (column (1)). After the removal of guarantees, the probability of staying in affected establishments is not significantly different from the control group (column (2)).

Table 7: Effect of public bank guarantees on the probability to stay in the same establishment: six years after the removal/placebo

Column (1) of this table shows the estimates for the cohort 1988. Respectively column (2) displays the estimates for the cohort from 2000. The third column uses a simplified version of [Equation 3](#) comparing the outcomes of the two cohorts. The sample is the individual-level balanced panel from IAB. It is restricted to the years six years after the (placebo) intervention (1994 and 2006). As an outcome, we analyze an individual's "staying status". The variable equals one if an individual is employed in the same establishment as in the cohort years 1988, respectively 2000, and zero otherwise. *Treated* is a dummy variable equal to one for individuals working in an establishment in 2000, allocated to an industries-location group borrowing from savings banks more than the median group and zero otherwise. *Guarantee* is a dummy variable that equals one for placebo years 1988 and 1994, where guarantees were in place. The dummy variable *6yearsafter* equals one for 2006 and 1994, respectively, six years after the removal or placebo treatment. All other variables are as defined in section *B* in the appendix. Standard errors, shown in parenthesis, are two-way clustered at the industry and location level. * show percent levels with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent variable: staying status (one if still in cohort estab., zero otherwise)			
	(1)	(2)	(3)	(4)
	Cohort 1988	Cohort 2000	Triple Diff.	Triple Diff. with controls
treated	-0.0332*** (0.0078)	-0.0078 (0.0080)	-0.0078 (0.0080)	-0.0083 (0.0080)
treated X guarantees			-0.0255*** (0.0091)	-0.0249*** (0.0091)
age	No	No	No	Yes
age squared	No	No	No	Yes
industry-year FE	Yes	Yes	Yes	Yes
county-year FE	Yes	Yes	Yes	Yes
R-squared	0.0454	0.0517	0.0525	0.0609
N. of observations	150,022	178,343	328,365	328,365

To gain a full picture, we repeat the same analysis with a data set restricted to employed only. The results displayed in [Table 8](#) are not significant but indicate that the increased probability to stay at the same establishment after the removal of guarantees not only stems from individuals who would otherwise become non-employed but also partly from those who would alternatively switch to another employer.

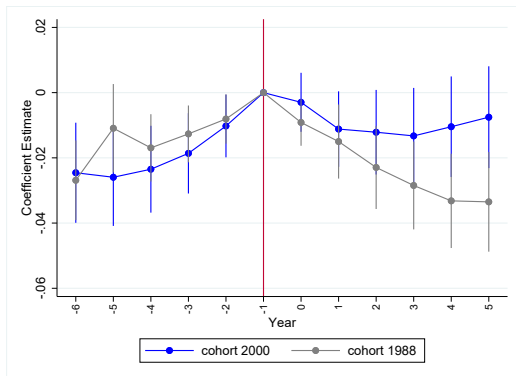
In [Figure 5](#), we plot event study results for both above analyses regarding staying with the same employer, using first the whole data set ([Figure 5a](#)) and second restricting it to employed only ([Figure 5b](#)). For both, we can confirm similar trends before the intervention. The central part of the positive effect of the guarantees' removal on the staying probabil-

ity seems to develop about 3 years after the intervention. Looking only at employed, the effect develops slightly later in time. Hence, it takes longer for the affected establishments to become more attractive to those employees who tend to switch to other establishments.

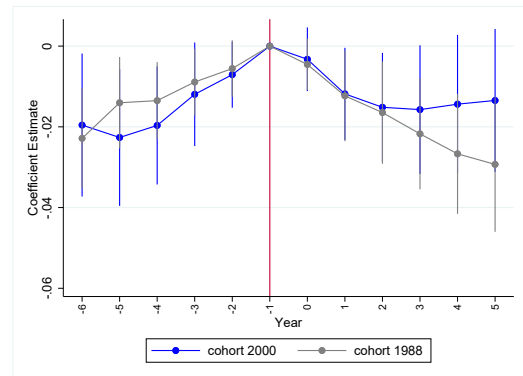
Table 8: Effect of public bank guarantees on the probability to stay in the same establishment or switch to another employer: six years after the removal/placebo

Column (1) of this table shows the estimates for the cohort 1988. Respectively column (2) displays the estimates for the cohort from 2000. The third column uses a simplified version of Equation 3 comparing the outcomes of the two cohorts. The sample is the individual-level balanced panel from IAB. It is restricted to the years six years after the (placebo) intervention (1994 and 2006). Further, we restrict the data to employed only. As an outcome, we analyze an individual's "staying status". The variable equals one if an individual is employed in the same establishment as in the cohort years 1988, respectively 2000, and zero otherwise. *Treated* is a dummy variable equal to one for individuals working in an establishment in 2000, allocated to an industries-location group borrowing from savings banks more than the median group and zero otherwise. *Guarantee* is a dummy variable that equals one for placebo years 1988 and 1994, where guarantees were in place. The dummy variable *6yearsafter* equals one for 2006 and 1994, respectively, six years after the removal or placebo treatment. All other variables are as defined in section B in the appendix. Standard errors, shown in parenthesis, are two-way clustered at the industry and location level. * show percent levels with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent variable: staying status (one if still in cohort estab., zero otherwise)			
	(1)	(2)	(3)	(4)
	Cohort 1988	Cohort 2000	Triple Diff.	Triple Diff. with controls
treated	-0.0291*** (0.0085)	-0.0133 (0.0090)	-0.0133 (0.0090)	-0.0141 (0.0091)
treated X guarantees			-0.0158 (0.0103)	-0.0148 (0.0104)
age	No	No	No	Yes
age squared	No	No	No	Yes
industry-year FE	Yes	Yes	Yes	Yes
county-year FE	Yes	Yes	Yes	Yes
R-squared	0.0499	0.0516	0.0569	0.0710
N. of observations	118,690	140,481	259,171	259,171



(a) whole data



(b) employed only

Figure 5: Event Study Results For the 1988 (guarantees) and 2000 Cohort (removal of guarantees) regarding Staying with the Same Employer

Combined with the results from the previous sections, we can conclude that employees have lower employment prospects when bank guarantees are in place. These stem from an increased probability of turning unemployed after working in an affected establishment.

After the removal the probability to turn non-employed decreases and also for those who would otherwise switch the establishment, the probability to stay with the treated employer tends to increase.

5.5 Stayers' Wages

In [subsection 5.2](#) we found a positive effect of bank guarantees on wage prospects. However, the effect has different underlying mechanics. It captures the wage effect of individuals staying in the same treated establishment as well as the wage development of individuals switching to another employer. The latter is usually accompanied by wage increases. We can not draw any conclusion about wage development within treated establishments from the previous analysis. Therefore, we investigate the wage effect of individuals staying in treated establishments in the long term in this section. We call the group of those individuals "stayers" for the remainder of the article.

To gain insights into the wage effect of stayers we restrict our data to individuals who stay with the same employer from 1995 to 2006 for the 2000 cohort and those who stay with the same employer from 1983 to 1994 for the 1988 cohort. We apply [Equation 3](#) investigating log wages as outcome again. The results in [Table 9](#) indicate significant positive effects of bank guarantees on the wages of stayers (column (3)). Which mainly derive from a sharp drop in stayers' wages after the removal of guarantees (column (2)). Instead with guarantees in place employees in treated establishments experience similar wage prospects to those in control establishments (column (1)). Overall, bank guarantees lead to a 1.4 percent increase in stayer's wages, which is higher than the result from [subsection 5.2](#). This is an indicator that the positive wage effect of guarantees is driven by individuals working in affected establishments in the long term, rather than better wage prospects after switching to other establishments.

Again, we conduct an event study design ([Figure 6](#)), which confirms almost the same relative wage prospects of individuals from the two cohorts before the intervention. We can further see, that the negative effect after the removal of guarantees develops after two to three years.

Taken together our findings substantiate our theory that establishments borrowing from

savings banks tend to use additional loan volume to grant higher wages, especially to a group of long-term employees.

Table 9:

Effect of public bank guarantees on stayers' wages: comparison to six years after the removal/placebo. Column (1) of this table shows the difference-in-difference estimates for the cohort 1988. Respectively column (2) displays the difference-in-difference estimates for the cohort from 2000. The third column uses Equation 3 for triple difference estimation comparing the two cohorts. The sample is the individual-level balanced panel from IAB. It is restricted to the year of the cohort and the year six years later (1988 and 1994, 2000 and 2006). Further, we restrict the data in this analysis to employees employed at the same establishment over the 12 years observation period. As an outcome, we analyze an individual's log wages. *Treated* is a dummy variable equal to one for individuals working in an establishment in 2000, allocated to an industries-location group borrowing from savings banks more than the median group and zero otherwise. *Guarantee* is a dummy variable that equals one for placebo years 1988 and 1994, where guarantees were in place. The dummy variable *6yearsafter* equals one for 2006 and 1994, respectively, six years after the removal or placebo treatment. All other variables are as defined in section B in the appendix. Standard errors, shown in parenthesis, are two-way clustered at the industry and location level. * show percent levels with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent variable: log wage			
	(1)	(2)	(3)	(4)
	Cohort 1988	Cohort 2000	Triple Diff.	Triple Diff. with controls
treated	-0.0421*** (0.0083)	-0.0277*** (0.0093)	-0.0277*** (0.0093)	-0.0286*** (0.0093)
treated X 6-years-after	-0.0003 (0.0035)	-0.0144*** (0.0036)	-0.0144*** (0.0036)	-0.0137*** (0.0035)
treated X guarantees			-0.0144 (0.0093)	-0.0129 (0.0093)
treated X guarantees X 6-years-after			0.0141*** (0.0052)	0.0130** (0.0051)
age	No	No	No	Yes
age squared	No	No	No	Yes
industry-year FE	Yes	Yes	Yes	Yes
county-year FE	Yes	Yes	Yes	Yes
R-squared	0.1789	0.2039	0.1920	0.1967
N. of observations	106,828	99,364	206,192	206,192

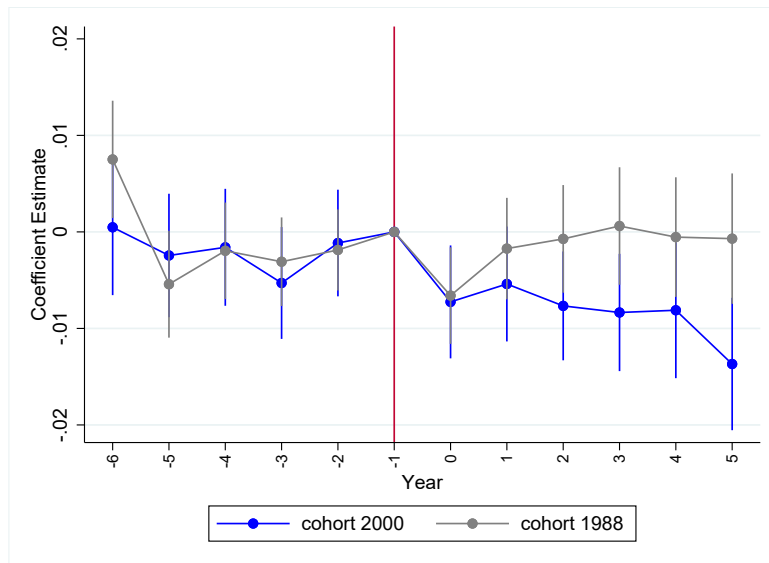


Figure 6: Event Study Results For the 1988 (guarantees) and 2000 Cohort (removal of guarantees) regarding Wages of Long-Term Employees

6 Robustness

6.1 Other Reforms and Events

We also considered other reforms during the nineties and since the start of the new millennium influencing our results. In this section, we argue why we have no reason to believe that the following reforms influence our results.

German reunification The German reunification significantly influenced labor market outcomes, in particular for East German citizens. Wages in East and West Germany converged during the nineties. We can not rule out that this wage increase in East Germany was the same for the treated and control groups.

Further, savings banks had different values and functions in East and West Germany before 1990, which potentially influences trends and characteristics of treated and control groups for our 1988 cohort.

For this reason, we removed observations from East Germany from our data allowing us for a clean analysis within West Germany.

Dotcom Bubble The start of the new millennium is marked by the so-called Dotcom Bubble. After the boom of internet-related business models, many companies shut down after the dot-com crash in 2000. As internet-related companies are not equally distributed to treated and control groups, we can not rule out a potential side effect of the Dotcom bubble. We test this by excluding computer and data-related industries from our analysis in [subsection 6.3](#). Our main result is robust to this restriction, so we can conclude that any influences of the dotcom bubble do not drive our result.

Labor market reforms So so-called "Hartz IV" Reform in Germany was implemented between 2003 and 2005 with the goal to counteract the rising unemployment rate. One might argue that this may influence the increased employment probability we observe in our main analysis.

There are several reasons speaking against this concern. First, the reform mainly ad-

dresses the unemployed, whereas we investigate the employment prospects of the already employed. Those should not be targeted or affected by the reform. Second, one must believe that the reform affects employees at treated and control establishments in different ways to bias our results. However, we can not think of any arguments that would support this idea. Third, studies show that the effects of the reform become visible from 2005 onward. But for most of our analysis, we can observe our effect involving earlier than 2005, which speaks against the reform driving our results.

6.2 Alternative Time Periods

One might argue that the chosen cohort from 1988 is an extraordinary period that does not represent employees' usual labor market biography when guarantees are in place. In this section, we argue and demonstrate, that this is not the case.

First, similar trends before the (placebo) intervention speak for the comparability of the cohorts. Anyhow, we test different alternative cohorts. We use several cohorts from later years that are temporally closer to the 2000 cohort. This also means that periods investigated in the event study overlap. [Figure 7](#) shows the event study results for different alternative periods beginning with a cohort from 1989 until 1994¹⁹. As an outcome, we use the wage sum, as it represents our overall results. In the figure lighter gray colors represent cohorts from earlier years; darker colors mark cohorts from later years. The red line represents the cohort 1988 from our main analysis. The blue line represents the 2000 cohort, whose observation period ranges from 1995 to 2006.

We clearly see that all alternative cohorts are very similar to the 1988 cohort. Hence, we can conclude that the 1988 cohort represents employment history and prospects in times of guarantee well.

We repeat the same analysis with alternative cohorts after the removal of guarantees. The results are displayed in [Figure 8](#). Lighter, gray colors represent cohorts from earlier years (e.g. 2001), and darker gray colors cohorts from later periods (latest until 2006).

¹⁹Any later cohort would overlap with the bank guarantees removal

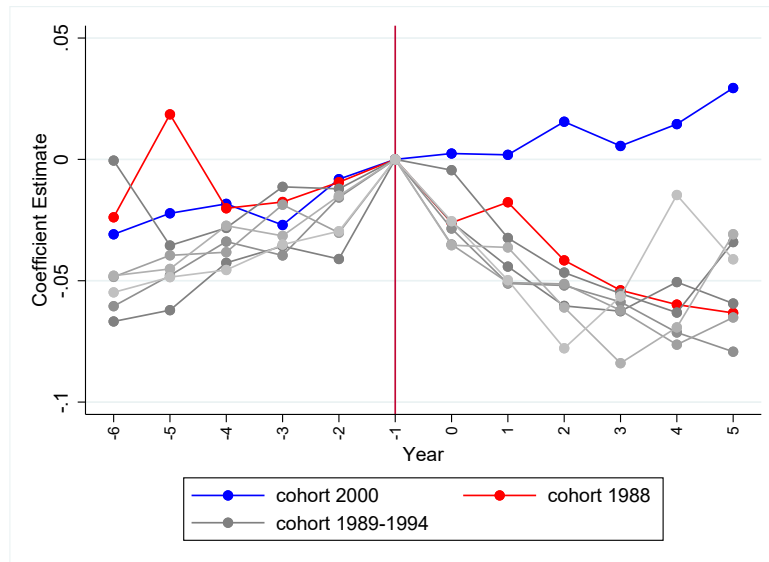


Figure 7: Event Study Results for Different Alternative "Guarantee" Cohorts regarding Wages Sum

Overall, we observe very similar development of the effect over years. The effects are slightly smaller for later periods. Still, the estimates six years after working in treated establishments, in any case, show that without guarantees the treated groups have similar labor market outcomes to the control group instead of falling behind them in times of guarantees.

6.3 Data Set Restrictions

In this section, we test our main results for robustness regarding sensitivity to certain subgroups. In the first part, we focus on the summarized results using the wage sum as the outcome of the specification. The results after different restrictions on the data are shown in [Table 10](#).

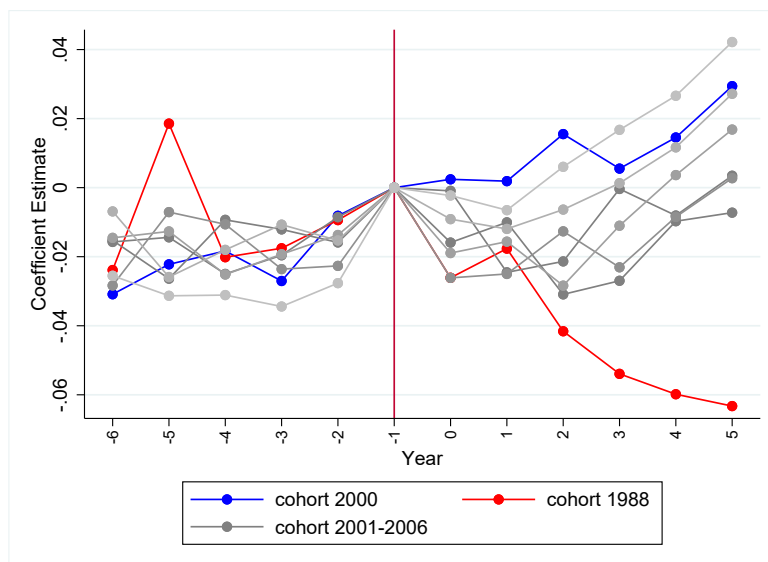


Figure 8: Event Study Results for Different Alternative "No Guarantee" Cohorts Regarding Wage Sum

Table 10:

Effect of public bank guarantees on the wages of (former) employees: comparison to six years after the removal/placebo

In this table, we present the results of the regression model Equation 3. The sample is the individual-level balanced panel from IAB. It is restricted to the years 2000 and 2006, presenting the pre- and post-outcomes during the removal of guarantees. We use data from 1988 and 1994 as a third difference, which may be interpreted as a placebo treatment. As an outcome, we analyze an individual's wage sum, which equals the log wage for employees and zero for non-employed. *Treated* is a dummy variable equal to one for individuals working in an establishment in 2000, allocated to an industries-location group borrowing from savings banks more than the median group and zero otherwise. *Guarantee* is a dummy variable that equals one for placebo years 1988 and 1994, where guarantees were in place. The dummy variable *6yearsafter* equals one for 2006 and 1994, respectively, six years after the removal or placebo treatment. All other variables are as defined in section B in the appendix. The different columns of the table show regression results after different restrictions of the data set. Standard errors, shown in parenthesis, are two-way clustered at the industry and location level. * show percent levels with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent variable: wage sum (wage if employed, zero otherwise)			
	(1)	(2)	(3)	(4)
	Manuf. Consumer Goods	Excl. Comp.	Men	Low Skilled
treated	-0.0819*** (0.0153)	-0.0476*** (0.0073)	-0.0399*** (0.0072)	-0.0381*** (0.0060)
treated X guarantees	0.0145 (0.0153)	0.0032 (0.0065)	0.0064 (0.0069)	0.0060 (0.0057)
treated X 6-years-after	0.0438 (0.0321)	0.0308* (0.0165)	0.0231 (0.0189)	0.0410** (0.0192)
treated X guarantees X 6-years-after	-0.1325*** (0.0493)	-0.0914*** (0.0239)	-0.0942*** (0.0278)	-0.0950*** (0.0270)
age	Yes	Yes	Yes	Yes
age squared	Yes	Yes	Yes	Yes
industry-year FE	No	Yes	Yes	Yes
county-year FE	Yes	Yes	Yes	Yes
R-squared	0.1561	0.1551	0.1549	0.1651
N. of observations	104,884	649,726	460,126	515,837

First, we restrict our data to the most balanced industry in terms of observations from treated and control groups (column (1)). With this analysis, we test, whether our results are driven by small unbalanced industries. The most balanced industry is the industry

of manufacturing consumer goods with about 49 percent of observations from the treated groups and 51 percent of observations from the control groups.²⁰ As in our main analysis, the results show a significant negative effect. The size of the effect is even larger than in our main analysis.

In column (2), we excluded the industries related to computer and other similar activities to rule out any side effects of the Dotcom Bubble on our results. We obtain almost the same coefficient as in our main analysis. We further restrict our data set to men only (column (3)), which delivers almost precisely the same results as in our main analysis.

In a fourth regression, we restrict to low-skilled employees to rule out that the effect is only driven by employees with certain skill levels. Again the results are very similar to those from the main analysis.

The second part of this section tackles the potential influence of part-time employees. As outlined in [subsection 2.2](#) we restrict the workforce in the cohort year to social security-liable full-time employees only. However, we allow them to switch to part-time employment afterward, which we interpret as employed as long as the employment is social security liable.

One might argue that the wage decrease after the removal of bank guarantees is driven by establishments pushing their employees into part-time employment to save personnel costs. Moreover, we have to rule out that the positive employment effect after the removal of bank guarantees is driven by part-time. Therefore we repeat our main analysis with all three outcomes, employment, wages, and wage sums. In terms of employment, we changed the interpretation of part-time employed to non-employment. For the wage analysis, we exclude part-time employees from the regression. In the wage sum regression, we interpret the wage of part-time employees as zero. The results are summarized in [Table 11](#).

All coefficients show similar values to those in our main analysis. We can conclude, that part-time does not play a role in the results we observe.

To sum up we showed that our results are not driven by specific subgroups. They stay robust over different industries, gender, and skill levels and are not driven by employees

²⁰As the industry is naturally balanced we do not use industry-year fixed effects.

switching to part-time employment.

Table 11:

Effect of public bank guarantees on the wages of (former) employees: comparison to six years after the removal/placebo

In this table, we present the results of the regression model [Equation 3](#). The sample is the individual-level balanced panel from IAB. It is restricted to the years 2000 and 2006, presenting the pre- and post-outcomes during the removal of guarantees. We use data from 1988 and 1994 as a third difference, which may be interpreted as a placebo treatment. We analyse different outcomes: employment status (column (1)), log imputed wages (column (2)) and wage sum (column (3)). The employment status equals one in the case of social-security liable full-time workers and zero otherwise (including part-time workers). The log wage represents the logarithmized imputed wage restricted to full-time employees only. The wage sum equals the log wage for employees and zero for non-employed and part-time workers. *Treated* is a dummy variable equal to one for individuals working in an establishment in 2000, allocated to an industries-location group borrowing from savings banks more than the median group and zero otherwise. *Guarantee* is a dummy variable that equals one for placebo years 1988 and 1994, where guarantees were in place. The dummy variable *6yearsafter* equals one for 2006 and 1994, respectively, six years after the removal or placebo treatment. All other variables are as defined in section *B* in the appendix. Standard errors, shown in parenthesis, are two-way clustered at the industry and location level. * show percent levels with * p<0.10, ** p<0.05, *** p<0.01.

	Dependent variable:		
	Empl. Status	Log Wage	Wage Sum
treated	-0.0002 (0.0002)	-0.0478*** (0.0073)	-0.0489*** (0.0073)
treated X guarantees	0.0003 (0.0003)	0.0034 (0.0065)	0.0047 (0.0065)
treated X 6-years-after	0.0049 (0.0038)	-0.0092** (0.0037)	0.0299* (0.0169)
treated X guarantees X 6-years-after	-0.0206*** (0.0052)	0.0107** (0.0050)	-0.0923*** (0.0243)
age	Yes	Yes	Yes
age squared	Yes	Yes	Yes
industry-year FE	Yes	Yes	Yes
county-year FE	Yes	Yes	Yes
R-squared	0.1742	0.1986	0.1694
N. of observations	656,730	575,315	656,730

6.4 Extreme Treatment Intensity Values

As a robustness check for the treatment calculation, we replicate our results using the 25 percentile of the treatment intensity for defining the control group and the upper 75 percentile for determining the treatment group. This reduces our data set to approximately half the observations (332,668 observations). According to our theory, the effect of the financial constraints due to the removal of bank guarantees should be larger, the higher establishments depend on savings banks loans. On the other hand, the less an establishment borrows from savings banks, the less they should be affected by guarantee removal. Therefore, we expect the estimated coefficients of interest to increase when considering more extreme values of treatment intensity. The results of our analysis using [Equation 3](#) with the adopted treatment definition is displayed in [Table 12](#). They mostly confirm our expectations. We observe an about 0.3 percentage points larger effect for

employment prospects, which is a 17 percent increase of our main result using the median for treatment dummy definition (column (1)). We can not reproduce the results from the main analysis regarding wages (column (2)). This may be due to the special subgroups of "stayers" driving the results, who are potentially less present in the more extreme treatment cases. Nevertheless, we see increased effects in the total wage sums (column (3)). We draw two important conclusions from this robustness check. First, it confirms that our definition of treatment intensity works and shows the expected behavior. Second, it demonstrates the theoretical mechanisms we assume behind, that labor market outcomes are more affected, the higher firms depend on savings bank loans.

Table 12:

Effect of public bank guarantees on different labor market outcomes: comparison to six years after the removal/placebo using the 25th/75th percentile for treatment dummy definition

In this table, we present the results of the regression model [Equation 3](#). The sample is the individual-level balanced panel from IAB. It is restricted to the years 2000 and 2006, presenting the pre- and post-outcomes during the removal of guarantees. We use data from 1988 and 1994 as a third difference, which may be interpreted as a placebo treatment. As an outcome, we analyze an individual's wage, which equals the log wage for employees and zero for non-employed. *Treated* is a dummy variable equal to one for individuals working in an establishment in 2000, allocated to an industries-location group borrowing from savings banks more than the median group and zero otherwise. The variable equals zero for individuals working in an establishment, with lower dependence on savings bank loans than the 25th percentile. *Guarantee* is a dummy variable that equals one for placebo years 1988 and 1994, where guarantees were in place. The dummy variable *6yearsafter* equals one for 2006 and 1994, respectively, six years after the removal or placebo treatment. All other variables are as defined in section *B* in the appendix. Standard errors, shown in parenthesis, are two-way clustered at the industry and location level. * show percent levels with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	Dependent variable:		
	Empl. Status	Log Wage	Wage Sum
treated	0.0002 (0.0004)	-0.1063*** (0.0146)	-0.1052*** (0.0147)
treated X guarantees	-0.0001 (0.0007)	0.0045 (0.0120)	0.0040 (0.0122)
treated X 6-years-after	-0.0092 (0.0076)	0.0076 (0.0072)	-0.0164 (0.0351)
treated X guarantees X 6-years-after	-0.0235** (0.0101)	-0.0022 (0.0099)	-0.1130** (0.0473)
age	Yes	Yes	Yes
age squared	Yes	Yes	Yes
industry-year FE	Yes	Yes	Yes
county-year FE	Yes	Yes	Yes
R-squared	0.1533	0.2288	0.1621
N. of observations	332,668	298,433	332,668

6.5 Continuous Treatment Intensity

We verify the behavior of the treatment variable further by using a continuous treatment intensity instead of the dummy variable generated using the median treatment intensity.

This allows us to use additional variation in the treatment intensity.

The results are shown in [Table 13](#). Compared to our main regression we observe much higher coefficients, which is because they reflect the difference between long-term labor market outcomes of individuals working in establishments with no dependence on savings banks' loans and individuals working in establishments that exclusively depend on those loans. However, the effect on wages is essentially zero, as for the higher percentile analysis ([subsection 6.4](#)). This is because the effect size does not necessarily grow with higher dependence on savings banks loans but instead on personal relationships and a special small subgroup of the employees.

Overall the results using a continuous treatment intensity show that we calculated a reasonable treatment variable that demonstrates the expected behaviour.

Table 13:

Effect of public bank guarantees on different labor market outcomes: comparison to six years after the removal/placebo using a continuous treatment intensity

In this table, we present the results of a slightly modified version of the regression model [Equation 3](#). The sample is the individual-level balanced panel from IAB. It is restricted to the years 2000 and 2006, presenting the pre- and post-outcomes during the removal of guarantees. We use data from 1988 and 1994 as a third difference, which may be interpreted as a placebo treatment. As an outcome, we analyze an individual's wage, which equals the log wage for employees and zero for non-employed. *Treatmentintensity* is a fixed continuous treatment variable for individuals working in an establishment in 2000. The treatment intensity displays the degree to which the industry-location the establishment is allocated depends on borrowing from savings banks. The variable equals zero for individuals working in an establishment, with lower dependence on savings bank loans than the 25th percentile. *Guarantee* is a dummy variable that equals one for placebo years 1988 and 1994, where guarantees were in place. The dummy variable *6yearsafter* equals one for 2006 and 1994, respectively, six years after the removal or placebo treatment. All other variables are as defined in section *B* in the appendix. Standard errors, shown in parenthesis, are two-way clustered at the industry and location level. * show percent levels with * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

	(1)	(2)	(3)
	Empl. Status	Log Wage	Wage Sum
treatment intensity	-0.0003 (0.0009)	-0.2529*** (0.0269)	-0.2544*** (0.0276)
guarantees X treatment intensity	0.0009 (0.0014)	0.0583** (0.0229)	0.0625*** (0.0241)
6-years-after X treatment intensity	-0.0026 (0.0152)	0.0018 (0.0157)	0.0457 (0.0682)
guarantees X 6-years-after X treatment intensity	-0.0659*** (0.0233)	-0.0120 (0.0216)	-0.3214*** (0.1072)
age	Yes	Yes	Yes
age squared	Yes	Yes	Yes
industry-year FE	Yes	Yes	Yes
county-year FE	Yes	Yes	Yes
R-squared	0.1519	0.1922	0.1553
N. of observations	656,730	587,536	656,730

7 Conclusion

In this article, we investigate the labor market effects of bank guarantees for employees. Taken together, we find that bank guarantees cause an overall wage sum loss of 9 percent for employees in treated establishments compared to their counterparts employed in control establishments. This negative effect stems from lower employment prospects caused by a higher probability of becoming non-employed within six years after working in an affected establishment. On the contrary, the wage prospects of employees are better when guarantees are in place. This effect is mainly driven by individuals who stay at treated establishments for a relatively long period. The wage result suggests that at least part of the additional loan volume issued to the market when bank guarantees are applicable is transferred to employees in the form of higher wages. Nevertheless, this comes at the cost of job security.

Our analysis shows that during good times the intention of bank guarantees to provide benefits to employees is at least not applicable to all employees. We can not make any conclusions about job-finding probabilities for the unemployed, but we provide proof that employees working in affected establishments have, on average, lower wage sum prospects. The removal of bank guarantees, therefore, was positive for workers in affected establishments and lifted their wage sum prospects even slightly above those from the control group.

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Appendix

A: Distribution of Treated and Control Group over State and Industry

Treated and control group over states

Table 14: Distribution of observations over industries: treated versus control group

	Control		Treated	
	No. of obs.	Share	No. of obs.	Share
Schleswig-Holstein	4,154	4.70	2,681	2.98
Hamburg	6,290	7.12	1,230	1.37
Lower Saxony	5,630	6.38	12,718	14.12
Bremen	2,132	2.41	710	0.79
North Rhine-Westphalia	30,811	34.89	13,855	15.39
Hesse	9,463	10.72	9,179	10.19
Rhineland-Palatinate	5,212	5.90	4,594	5.10
Baden-Wuerttemberg	9,023	10.22	24,042	26.70
Bavaria	13,629	15.43	19,069	21.18
Saarland	1,959	2.22	1,962	2.18
Total	88,303	100.00	90,040	100.00

Treated and control group over summarized 2-digit industry groups

Table 15: Distribution of observations over state: treated versus control group

	Control		Treated	
	No. of observations	Share	No. of observations	Share
Agriculture, hunting, forestry, and fishing	253	0.29	1,066	1.18
Mining and quarrying	1,388	1.57	185	0.21
Manuf. food, beverage, and tobacco	1,428	1.62	3,468	3.85
Manuf. textile and textile products	422	0.48	1,108	1.23
Manuf. leather and and leather products	185	0.21	50	0.06
Manuf. wood and wood products	190	0.22	1,243	1.38
Manuf. pulp, paper and paper products; publishing and printing	1,654	1.87	2,573	2.86
Manuf. coke, refined petroleum, nuclear fuel and chemical products	3,477	3.94	1,149	1.28
Manuf. rubber and plastic product	592	0.67	2,712	3.01
Manuf. other non-metallic mineral products	628	0.71	1,553	1.72
Manuf. basic metals and fabricated metal products	3,584	4.06	6,778	7.53
Manuf. machinery and equipment n.e.c.	2,523	2.86	7,076	7.86
Manuf. electrical and optical equipment	4,736	5.36	4,971	5.52
Manuf. Transport Equipments	6,429	7.28	882	0.98
Manufacturing n.e.c. (Miscellaneous)	525	0.59	1,801	2.00
Electricity, gas, and water supply	2,151	2.44	74	0.08
Construction	1,665	1.89	11,685	12.98
Retail trade, wholesale and repair of machinery	5,072	5.74	10,457	11.61
Retail trade, Wholsale	2,533	2.87	7,732	8.59
Hotels and Restaurants	1,216	1.38	2,502	2.78
Transport, storage, and communication	5,785	6.55	4,330	4.81
Financial intermediation	7,330	8.30	90	0.10
Real estate, renting and business activities	12,303	13.93	5,782	6.42
Public administration and defence; compulsory social security	8,800	9.97	0	0.00
Education	2,928	3.32	274	0.30
Health and social work	6,142	6.96	8,290	9.21
Other community, social and personal service activities	4,068	4.61	2,052	2.28
Private households with employed persons	0	0.00	157	0.17
Extra-territorial organizations and bodies	296	0.34	0	0.00
Total	88,303	100.00	90,040	100.00

B: Definition of Variables

Table 16: Description of variables

Variable	Definition
age	age individuals
age squared	squared age of individuals
duration of non-employment	duration of non-employment in the five years before the (placebo) intervention in years
years in estab.	years an individual is employed in the current establishment
low-skilled	based on the last employment's task description; equals one for unskilled and skilled tasks, and zero for (highly) complex tasks
working in cohort year estab.	variable equals one in a certain year if an individual is working in the same establishment as in the cohort year.

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ISSN 2194-2188



The IWH is funded by the federal government and the German federal states.