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Labor market adjustments after a large import shock: Evidence from the German clothing industry and the Multi-Fibre Arrangement*

Nils Braakmann and Joachim Wagner

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Abstract

This paper considers labor market adjustments following a large import shock in the German clothing industry caused by the phasing out of the Multi-Fibre Arrangement. Using the German shoe industry as a control group and administrative data, we study adjustments on the individual and firm level using difference-in-differences. Our results suggest relatively small increases in unemployment risk and no wage effects on the individual level, despite a 30% increase in plant mortality. Part of the different effects found for plants and individuals can be attributed to the reallocation of workers into other industries and occupations.

* All calculations were performed using Stata 10.1 and 11 SE. All do-files are available from the first author on request. This study uses the weakly anonymous BA-Employment Panel (Years 2000 – 2006) and the Establishment Panel History (Years 2000 – 2006). Data access was provided via remote data access at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB). See http://fdz.iab.de for details.

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1. Introduction

Globalization and its impact on the employment prospects and wages of domestic workers have been a mayor source of concern in many industrialized countries. In this paper, we exploit a large import shock caused by the phasing-out of the Multi-Fibre-Agreement that hit the German clothing industry after 2002. Using a non-affected but similar industry, the shoe industry, as a control group, we study labor market adjustments on both the firm and the individual level. Adjustments on the firm level, either through firm exits or through behavioral changes by surviving firms provide evidence on the labor demand changes in the affected industry, while the individual level results tell us about the fate of the workers that were employed in the industry before the import shock, e.g., whether they became long-term unemployed or whether they were able to find employment in another industry. As we will see in this paper, distinguishing between these effects is potentially important as even large negative changes to labor demand in an affected industry may lead to relatively minor effects on the individual level. To the best of our knowledge this paper is also the first to study labor market consequences of globalization on both levels.

Most of the previous evidence concerned with the relationship between some aspect of globalization and domestic labor markets used aggregate data and estimated industry or nation wide production or labor demand functions (see Feenstra and Hanson, 1996a,b, 1999, or Morrison-Paul and Siegel, 2001, for prominent examples and Feenstra and Hanson, 2001, for a survey of the evidence). A general conclusion drawn from this literature, which is focused primarily on the US, is that trade in intermediate inputs may explain some part of the increases in wage inequality among American workers that have been observed in recent years (see Katz and Autor, 1999, for an overview), while trade in final goods plays a

somewhat smaller role (Feenstra and Hanson, 2001). A potential problem with these industry-level studies is the possibility of aggregation bias and the likely endogeneity of industry level offshoring and industry-level employment and wages, which makes causal statements at least difficult.

In recent years, there has also been a rise in studies employing micro-econometric methods. These typically use individual or firm-level data on employment and wages combined with industry-wide measures of international outsourcing based on input/output tables, e.g., the share of intermediate inputs from a foreign industry in an industry's total output as in Geishecker and Görg (2004) or Egger et al. (2007). While these studies are less prone to the endogeneity issues mentioned above as industry level offshoring and individual wages are measured on different levels, a potential problem remains: As these studies typically relate industry measures of outsourcing to wages of individuals *employed in that industry*, labor market adjustments working through the reallocation of workers across industries cannot be studied (see, e.g, the remarks by Geishecker and Görg, 2004, pp. 247-8).

The paper that is closest in spirit to the approach pursued in this study is Hsieh and Woo (2005) who exploit China's opening to foreign investors in 1980 to study the effects of international outsourcing on the labor market in Hong Kong. Their findings using industry level data suggest that large shares of the observed demand changes can be related to (potential) offshoring to China. Compared to their paper, our study adds the explicit distinction between industry level adjustments working through firm exits or labor demand changes and changes in individual labor market prospects for workers who were employed in the affected industry. We also employ a control group of workers and firms from a similar, but unaffected industry to strengthen the causal interpretations of our results.

In contrast to most of the microeconometric literature, this paper exploits a quasi-experimental event that led to a large import shock in the German clothing industry. Specifically, we consider the effects of a large increase in imports following import quota changes for textiles and clothing through the phasing-out of the Multi-Fibre Arrangement agreed to in the Uruguay Round negotiations of GATT and laid down in the WTO Agreement on Textiles and Clothing. Under this agreement, import quotas for textiles and clothing were substantially lowered on January 1st, 2002 and completely lifted on December 31st, 2004 (see European Commission, 2000). The agreement led to a massive increase in imports from the year 2000 to 2006 (see Table 1). At the same time, domestic production of work wear, other outerwear and accessories dropped by approximately 50%. Note that this import shock is not directly related to the optimizing behavior of German clothing firms, which makes import changes plausibly exogenous with respect to the firm or the individual level.

[Table 1 around here.]

The effects of the agreement have already been studied on the firm level by Raff and Wagner (2009) who use an oligopoly model with heterogeneous firms to derive predictions about firm productivity and survival. Testing their predictions on enterprise data from German official statistics, they find that the least efficient domestic firms exit the market, while the average output of the surviving firms was lowered and their average productivity rose. These results are in line with their model's predictions. However, given data constraints, in particular the fact that their sample covers only firms with 20 or more employees, they cannot distinguish between firms that cease to exist and firms that drop below 20 employees. They also do not study possible labor demand changes in the clothing industry.

In this paper, we are interested in the labor market adjustments following the import shock caused by the phasing-out of the Multi-Fibre agreement. To identify these effects, we rely on (regression-adjusted) difference-in-differences estimators using the German shoe industry as a control group. As we demonstrate in section 2, the shoe industry, which was not affected by the changes in import quotas, has a relatively similar cost structure as the clothing industry and followed historically similar trends in sales, exports and a variety of other characteristics.

In section 3, we consider labor market adjustments on the firm level. Here, we use a 50% sample of all German plants and look at changes in both total employment and plant exit, where the latter is defined as employing less than one worker subject to social security contributions. These estimates provide evidence on the labor market adjustments that occurred through labor demand changes inside the German clothing industry. Note that these effects are not identical to the effects on workers that were employed in the clothing industry before the import shock, as these might have been able to find work in other industries, possibly at similar or even higher wages.

To capture this second dimension of labor market adjustment at the level of the individual, we use a 2% sample of German workers drawn from social security records in section 4. Here, we treat all individuals who worked in the clothing industry in the year 2000 as the treatment group and use individuals who were employed in the shoe industry at the same time as the control group. As outcomes, we look at changes in employment probabilities and wages as well as at the reallocation of individuals across industries or occupations.

Taken together, these estimates provide a comprehensive picture on the short-run labor market dynamics following a large import shock. Our results suggest strong increases in the likelihood of plant exits by between 3% and 7% in the treatment group relative to the control

group in each year and in particular after 2004, while average firm size was relatively unaffected, although the (non-significant) point estimates are generally negative. On the individual level, we find strong increases in individual unemployment risk by about 5% for the treatment group in 2002 that drops to a 2% higher unemployment risk relative to the control group in 2006. Conditional on being employed, we find no wage differences between treatment and control group. However, there is strong evidence for reallocation of workers in the treatment group across occupations after 2003. Conclusions from these results can be found in section 5.

2 The German clothing and shoe industries

Our empirical approach relies on comparing changes in the clothing industry over time with changes in the shoe industry over the same period. A crucial condition for this approach to be valid is that both industries would have followed the same trends in employment and wages in the absence of the phasing-out of the Multi-Fibre Agreement. While we cannot test this proposition directly as we do not observe counterfactual worlds, this section presents arguments as well as evidence based on long-term trends in favor of our approach.

Before going into the evidence, note first that there are clear economic arguments for similar trends in both industries: Both the consumption of shoes and clothing can be expected to follow similar trends over the business cycle as both shoes and clothing are durable consumer goods that underlie seasonal trends as well fashion cycles. Additionally, we can expect that the production of shoes and clothing is not too different from each other in terms of necessary qualifications – or at least more similar than, e.g., the production of clothes and the production of electronics.

Figures 1a and 1b present evidence in favor of these conjectures. The figures plot trends in monthly hours and wages (Figure 1a) as well as in sales and export sales (Figure 1b) in both industries for the period January 1995 to December 2001, which is directly before the phasing-out of the Multi-Fibre Agreement began. As level values for all variables are different in both industries, all variables have been transformed into an index, where the respective value in January 1995 serves as the base value and is set at 100. The figures clearly show similar seasonal as well as yearly trends in both industries, which supports the assumption of identical trends.

[Figures 1a and 1b around here.]

Table 2 presents further evidence on the cost shares of various inputs used in the production process. Large differences in the importance of different inputs could be problematic as these could cause divergent trends due to input price changes unrelated to the Multi-Fibre Agreement. However, while there are some small differences in the cost shares of the various inputs, the overall structure as well as the development over time looks similar in the shoe and the various clothing industries.

[Table 2 around here.]

Given this evidence, it seems reasonable to assume that the shoe industry may serve as a valid control group in our investigation, in particular as we also control for various observed and unobserved characteristics of the firm or the individual.

3 Firm level adjustments

3.1 Data and empirical approach

In this section we use a 50% sample of the population of German plants that employ at least one worker subject to social security contributions (effectively excluding only single person entrepreneurs and most government agencies), the *Establishment History Panel* (see Spengler, 2008, for details and Spengler, 2009, for the codebook and documentation). The data have been formed by aggregating social security records at the plant level and are provided and maintained by the research data center of the Federal Employment Agency in the Institute of Employment Research. Note that the data can be linked over time using plant identifiers, resulting in a panel data set from 1975 (West Germany) and 1992 (East Germany) onwards.

The data contain detailed information on industry affiliation and the workforce composition of the plant, including, e.g., the shares of workers with certain educational degrees, with various occupational positions, in certain age groups or with a certain nationality (see Spengler, 2009, for a full list) as well as quartiles of the age and wage distribution. However, we do not have information on average wages as the wage data are top censored at the contribution limit to social security. We also do not have any information on firm performance variables, like profitability, output, sales, exports or revenue. Additionally, the data do not contain information on physical capital.

We keep all firm-year-observations for the period 2000 to 2006 for all plants that were either active in the clothing or the shoe industry in the year 2000. To be specific, our treatment group is formed by all firms active in the 3-digit industry 182 “Manufacture of other wearing apparel and clothes”, which covers the production of clothes not made from leather or fur. As the control group, we use all firms that were active in the 3-digit-industry 193 “Manufacture

of footwear". This selection leaves us with 10,359 firm-year-observations for 2,126 plants in the treatment group and 2,028 firm-year-observations from 361 plants in the control group.

Tables 3a and 3b contain basic descriptive statistics for both groups in the base year 2000 and over the whole observation period. Note that plants in both industries are generally very similar, in particular with respect to the age and qualification structure of the respective workforce. Some differences can be seen for the share of women, although both industries predominantly employ female workers, and for average firm size.

[Tables 3a and 3b around here.]

Our estimation strategy relies on (regression-adjusted) difference-in-differences-estimators of the form

$$y_{it} = \eta_i + \beta' X_{it} + \delta * T_{it} + \tau * (D_i * T_{it}) + \varepsilon_{it}, \quad (1)$$

where y_{it} is the respective outcome of interest for firm i in year t, η_i is a firm-specific fixed effect, X_{it} contains time-varying control variables, T_{it} contains dummy variables for each year from 2001 to 2006, D_i is an indicator for the clothing industry and ε_{it} is a standard error term. Our parameters of interest are contained in τ, where each element gives the mean difference (adjusted for changes in X_{it}) in the respective outcome between the clothing and the shoe industry in the respective year. All standard-errors are adjusted for clustering on the firm-level, which allows for arbitrary heteroskedasticity and autocorrelation on the firm level (see Stock and Watson, 2008).

As outcomes, we consider both firm size, measured by the number of employees, and firm exits. In the latter case, we essentially estimate a linear probability model, which is innocuous as the main part of the model, specifically the time and group dummies and their interaction,

is saturated and hence non-parametric (see, e.g.., Angrist and Pischke, 2009, section 3.1.4). In the estimates for firm exits, we control for the structure of the workforce, firm size and age, while no control variables are used when looking at firm size. The latter decision is grounded in the idea that it does not make sense to control for the share of, e.g, high-skilled workers when looking at changes in firm size as both can be considered outcomes.

τ can be interpreted as a causal effect if (a) firms cannot select into or out of the treatment group, (b) firms cannot select into or out of the treatment period and (c) both treatment and control group would have experienced the same trends in the absence of treatment. The first two concerns are more relevant for cross-sectional difference-in-differences and are alleviated through the panel design of this study, which enables us to base group definitions on pre-treatment-industry affiliations and to use both pre- and post-treatment-observations for each firm. Additionally, by including a dummy and an interaction term for 2001, we explicitly allow for different trends in treatment and control group in the year before the phasing-out of the Multi-Fibre Agreement began. Given the evidence on pre-treatment trends in the previous section 2, we are quite confident that the common-trend assumption is likely to hold. Finally, note that controlling for enterprise-specific fixed-effects further alleviates concerns regarding the validity of the common-trend-assumption.

3.2 Results

Table 4 presents the results for the firm level estimates. For the likelihood of a firm exit, we observe strong increases in the clothing industry relative to the shoe industry in each year. In 2001 and 2003 the likelihood of a firm exit was approximately 4% higher, while after 2004 it rises to about 6% per year. Note also that there are particularly strong increases in the likelihood of a firm exit after the Multi-Fibre Agreement was fully phased-out in 2004. Over the whole observation period the results suggest a roughly 30% higher chance of a firm exit in

the treatment compared with the control group. This result is in line with the results by Raff and Wagner (2009) who found that 55% of the firms in their sample exited until 2006. The discrepancies in the results can be explained by the fact that Raff and Wagner (2009) look at enterprises while we look at plants. Additionally, their definition of a firm exit – a firm drops below 20 employees and consequently from their sample – and our definition are not identical. Given their higher cutoff point, it is in fact not surprising that their analysis leads to higher exit rates.

[Table 4 around here.]

Consider now changes in the firm size, displayed in the second column of table 4. Here, we find no significant changes in the clothing industry relative to the shoe industry. However, looking at the point estimates reveals a similar pattern as the one found for the likelihood of a firm exit: The point estimates are always negative and are also absolutely larger in 2002 and after 2004. Note also that the point estimates often suggest economically large effects that are insignificant due to low precision of the estimates.

Taken together, these results suggest rather large, negative effects of the rise in imports following the phasing-out of the Multi-Fibre agreement on firms that were active in the clothing industry in 2000. A question that follows directly from these results is what happened to the workers who worked in the clothing industry before 2002. In the following section, we provide evidence that most workers were able to find other job after some years, often in different occupations and industries, but at similar wages. However, the probability of unemployment at the end of our observation period is still 2 percentage points higher relative to the control group.

4 Individual level adjustments

4.1 Data and empirical approach

The individual level data used in this study comes from the so called employment panel of the Federal Employment Agency (*BA-Beschäftigtenpanel*) for the years 2000 to 2006. Information on an earlier version of the employment panel can be found in Koch and Meinken (2004), the current version is described (in German) in Schmucker and Seth (2009).

The individual data originates from social security information and is collected in the so called *employee history* by the Federal Employment Agency.[1] In Germany, employers are obliged by German law to deliver annual information on their employees, as well as additional information at the beginning and end of an employment, to social security. These notifications are used to calculate pensions, as well as contributions to and benefits from health and unemployment insurance. The data contain information on the begin and end of employment, daily wages, a person's age and sex, as well as several variables collected for statistical purposes, e.g. education or nationality. The resulting spell data cover approximately 75 - 80% of the German workforce, excluding free-lancers, the self-employed, civil servants and (unpaid) workers helping in family businesses (Koch and Meinken 2004, p. 317).

The employment panel is drawn from the employee history in a two step procedure. First, all persons born on one of seven specified dates are selected. As the German social security number is tied to the date of birth and does not change over time, it is possible to track those persons over time. Additionally, entries in and exits from the labor force are automatically covered by this procedure as new entrants born on one of these dates replace persons leaving the labor force. In a second step, the panel is formed by drawing four cross-sections per year -

[1] More information on person-level data from German social security records can be found in Bender at al. (2000).

- on the last day of March, June, September and December respectively - from this data. Finally, if a person receives unemployment benefits or is in an active labor market program on one of those days, an artificial observation indicating this fact is generated from other data sources of the Federal Employment Agency. The resulting panel is unbalanced due to entries into and exits from the labor force. However, there is no missing information due to non-response.

Similar to the previous section, we keep all individual-quarter-observations for the period 2000 to 2006 for all individuals that were either employed in the clothing or the shoe industry in the year 2000. Our treatment group is again formed by individuals who worked in the 3-digit industry 182 "Manufacture of other wearing apparel and clothes", while the control group consists of all individuals who worked in the 3-digit-industry 193 "Manufacture of footwear". After restricting the sample to full-time workers aged between 25 and 65 years, we are left with 35,811 observations for 1,804 individuals in the treatment group and 10,713 observations for 494 individuals in the control group.

Tables 5a and 5b contain basic descriptive statistics for both groups, again for the base year 2000 and over the whole period. Note that we already observe some industry changes in table 5a, which is due to the fact that we have quarterly data. As already found on the firm level, both groups are relatively similar with respect to age and the educational structure of the workforce. Differences are again found for the share of women.

[Tables 5a and 5b around here.]

We again use (regression-adjusted) difference-in-differences-estimators of the form

$$y_{it} = \eta_i + \beta' X_{it} + \delta * T_{it} + \tau * (D_i * T_{it}) + \varepsilon_{it}, \quad (1)$$

where y_{it} is the respective outcome of interest for individual i in year t, η_i is an individual-specific fixed effect, X_{it} contains time-varying control, variables, T_{it} contains dummy variables for each year from 2001 to 2006, D_i is an indicator for the clothing industry and ε_{it} is an error term. Our parameters of interest are again contained in τ. All standard errors are adjusted for clustering on the individual-level.

As outcomes we consider wages, employment (with unemployment as the omitted alternative) as well as changes in 2- or 3-digit occupations or industries relative to the year 2000. The first analysis captures possible wage adjustments within the clothing industry as well as effects working through the relocation of displaced workers in worse-paying jobs. With respect to the employment estimates, we have to omit individuals changing into non-employment as these cannot be distinguished from individuals becoming self-employed, lifetime civil servants or migrate to another country. Note, however, that the last two alternatives can be expected to be rather rare among former clothing workers. Additionally, there is no way to distinguish workers going into (early) retirement from discouraged workers. The estimates relating to industry and occupation changes capture labor market adjustment working through the reallocation of workers. Changes on the 2-digit level are equivalent to broader occupation/industry changes, e.g., from the clothing industry to the motor vehicles, while changes on the 3-digit level are equivalent to changes, e.g., from the manufacture of passenger vehicles to the manufacture of car parts.

Included control variables are years of potential experience as a second order polynomial, measured as age – typical years of education for an individual with the respective degrees – 6, as well as dummies for having completed a university degree and for not having received any form of post-school-education. All estimates additionally include quarter-of-year-dummies to control for seasonal effects.

The crucial assumption is again that both groups would have experienced the same trends in the absence of the phasing-out of the Multi-Fibre agreement. Given the relative large similarities in observable characteristics displayed in table 5, the fact that wages and annual hours worked were historically similar in the clothing and shoe industry and the fact that both groups worked in relatively similar industries in terms of required inputs, this assumption seems again warranted.

4.2 Results

Table 6 displays estimation results for the difference-in-differences estimates by standard fixed effects regressions for wages, employment probabilities and the probability to work in a different 2- or 3-digit occupation or industry as in the year 2000. Starting with the wages estimates, we notice no pattern of results that would suggest an earnings disadvantage for the treatment relative to the control group. The parameters are generally insignificant and there is also no consistent negative pattern existent in the point estimates. Looking at employment, however, reveals negative employment effects for the treatment group in every year. Specifically, we observe a strong increase in unemployment in 2002, the year where the import quotas were substantially lowered, relative to both 2000 and 2001. There is also a decline in the employment probabilities of the treatment group in 2001, which can be taken as a sign for anticipation effects. From 2003 on, we observe relative increases in the employment prospects for the treatment group, whose relative disadvantage compared to the control group drops to a 2 percentage points lower employment probability by 2006. Note that there is also no further increase in unemployment accompanying the complete lifting of quotas in the end of 2004.

[Table 6 around here.]

A possible explanation for the relatively weak employment effects can be seen in the last four columns of table 6: For the treatment group, we observe a much higher chance to work in a different occupation than in the base year 2000 for every year after 2002. Looking at industry changes, we see that the probability of working in a different industry than in 2000 steadily increases for the treatment group up to the year 2003 and declines afterwards, which follows a similar pattern than the individual employment prospects.

Taken together, these results suggest that some members of the treatment group could avoid unemployment by changing into another industry in the first years under study. They also suggest that, after the initial shock, many of the original clothing workers returned to the clothing industry, although in different occupations. However, the probability of unemployment in the treatment group remained about 2 percentage points higher than in the control group. The fact that we do not find any wage effects is hardly surprising given the relatively institutionalized nature of the German labor market (see, e.g., OECD, 2004, chapter 3). Compared with the much larger effects on plant survival, these results also speak for the labor market's ability to reallocate workers across different industries when the need arises.

5 Conclusion

In this paper, we considered the consequences of a large import shock that hit the German clothing industry in 2002 and 2004. Using social security data on both the individual and the firm level and workers and firms from an unaffected, but similar industry as the control group, we showed that, despite a high level of firm mortality, individual employment prospects were harmed to much lesser extent. While almost 30% of the plants that were active in the clothing

industry in 2000 exited the market, individual employment probabilities for workers who were employed in the clothing industry in 2000 dropped by at most 5 percentage points and even recovered by another 3 percentage points up to 2006. Part of this recovery was most likely related to a much higher chance of an occupation or industry change in the treatment group. However, these changes do not seem to have been accompanied by wage penalties.

As a whole, these results demonstrate the ability of even a highly regulated labor market to reallocate workers when one industry is hit by an exogenous shock. One should keep in mind though that a part of the relatively mild effects of the shock considered here can be attributed to the fact that the clothing industry is relatively small compared to the German economy, which means that general equilibrium effects were probably rather small. Obviously, the case could have been different if, e.g., 30% of the German car manufacturers closed down. On a more methodological level, our results also suggest that it is potentially important to distinguish between the firm and the individual level when looking at the effects of globalization. In our case, the effects for firms in the respective industry were large, while individual effects were often small and to some degree temporary.

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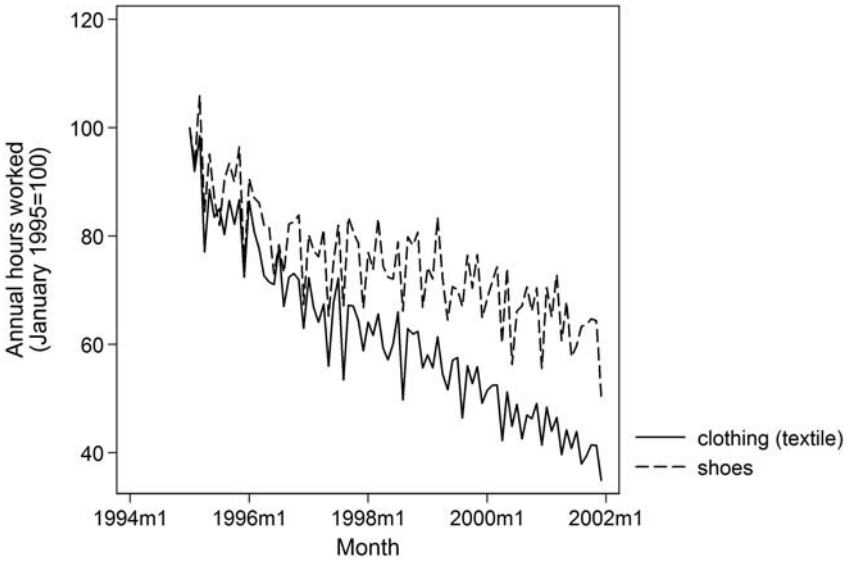
Table 1: Imports and domestic production in the German clothing and shoe industry 2000-2007

	2000	2001	2002	2003	2004	2005	2006
Imports (1000 Euro, 2000 prices)							
Work wear	3438.6	3804.4	3658.8	3961.9	4335.2	4441.1	4685.4
Other outerwear	83881.0	84124.2	81776.7	81834.1	84495.4	83949.5	88728.1
Underwear	59170.6	57384.2	55722.0	56695.1	56651.2	57562.3	62625.2
Other / accessories	23500.9	20971.0	19018.8	19229.3	19746.5	21450.7	23215.5
Shoes	45391.1	46056.6	43975.5	42197.0	40979.4	43055.2	45386.0
Domestic production (1000 Euro, 2000 prices)							
Work wear	1085.9	1018.1	979.9	996.3	804.5	893.5	968.0
Other outerwear	18485.3	17644.7	14970.6	12745.3	12697.4	12012.5	9431.7
Underwear	9527.6	8531.6	7428.7	6941.1	6336.3	5211.6	5195.4
Other / accessories	3756.9	3060.1	2650.9	2496.1	2163.5	1941.5	2081.9
Shoes	10988.0	10479.5	10015.3	8374.9	8543.0	8005.1	7141.4
Price index import goods							
Work wear	100.0	95.7	89.5	80.4	75.8	75.1	73.8
Other outerwear	100.0	101.0	101.1	96.8	95.1	95.3	95.8
Underwear	100.0	101.2	101.5	100.4	99.5	99.2	99.9
Other / accessories	100.0	105.1	105.4	100.4	97.6	97.4	98.8
Shoes	100.0	101.3	102.9	103.2	102.8	103.8	104.8
Price index domestic production							
Work wear	100.0	99.2	99.9	100.0	100.7	101.7	101.6
Other outerwear	100.0	100.2	100.7	99.8	97.5	97.5	98.0
Underwear	100.0	100.3	100.7	100.8	99.5	101.0	102.0
Other / accessories	100.0	102.7	103.3	104.3	107.2	109.8	111.5
Shoes	100.0	103.1	104.8	104.7	104.8	106.0	107.4

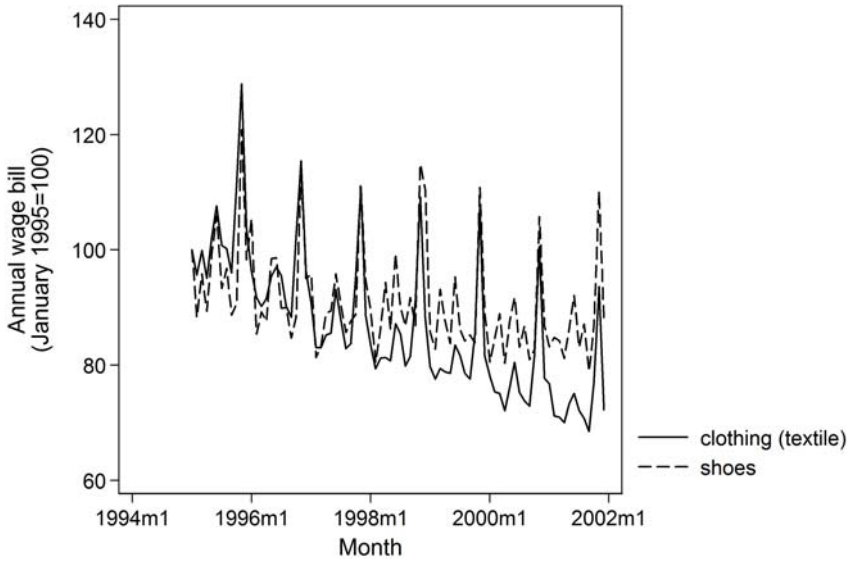
Source: Statistisches Bundesamt, *Außenhandels*- and *Preisstatistik*, own calculations.

Figure 1a: Trends in employment and wages in the German clothing and shoe industry, 1995-2001

Panel (a): Monthly hours worked



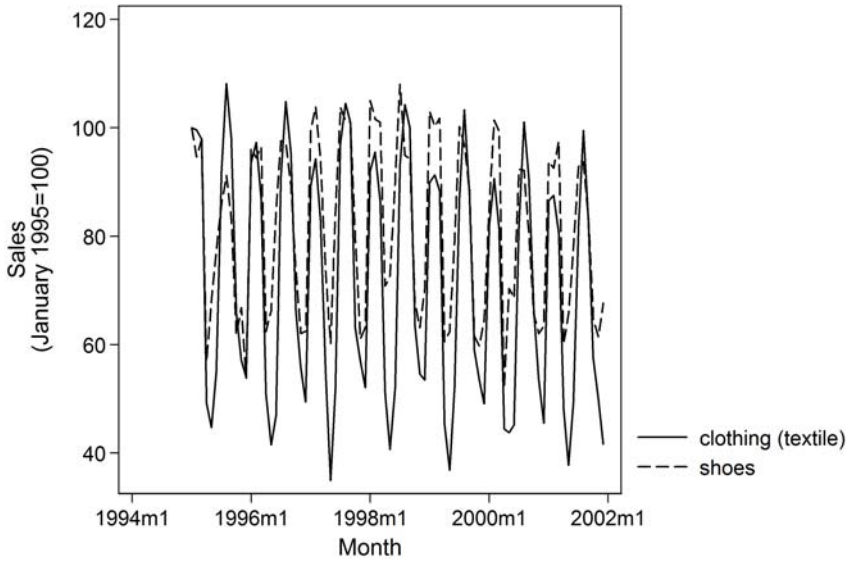
Panel (b): Monthly wage bill



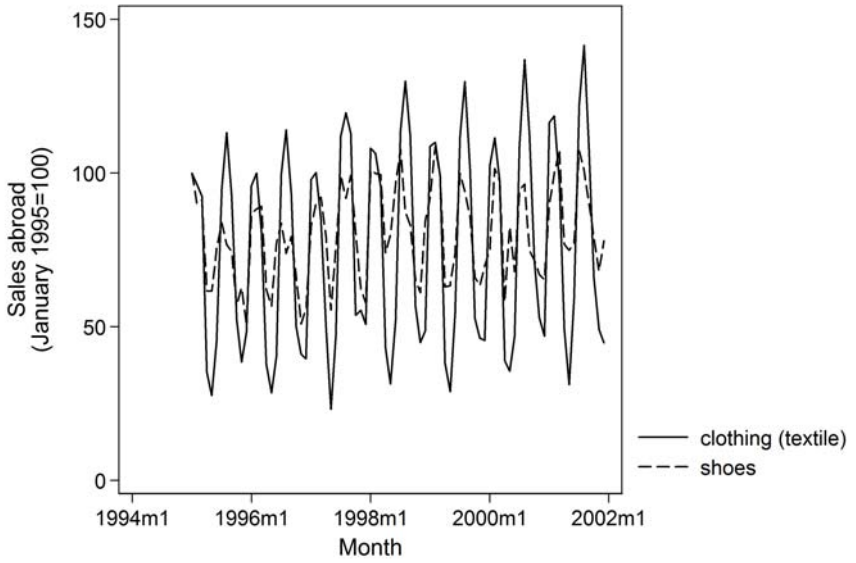
Source: Statistisches Bundesamt, *Monatsbericht für Betriebe im Verarbeitenden Gewerbe*, own calculations

Figure 1b: Trends in sales and exports in the German clothing and shoe industry, 1995-2001

Panel (c): Monthly sales



Panel (d): Monthly export sales



Source: Statistisches Bundesamt, *Monatsbericht für Betriebe im Verarbeitenden Gewerbe* , own calculations

Table 2: Cost shares of selected inputs (% of gross production value) in the German clothing and shoe industry

	2000	2001	2002	2003	2004	2005	2006
Material							
Work wear	40.9	40.5	42.4	41	42.3	46.7	45.7
Other outerwear	41.2	40.3	38.8	39.2	41.5	42.2	43.3
Underwear	34.4	34.5	33.2	34.2	33.3	32.8	31
Other / accessories	30.9	31	33.9	40.7	37.8	37.7	37.7
Shoes	45.9	50.4	47.6	45.8	45.5	45.9	45.6
Energy							
Work wear	.5	.5	.5	.5	.5	.6	.6
Other outerwear	.3	.3	.4	.4	.4	.4	.4
Underwear	.6	.5	.5	.6	.6	.6	.7
Other / accessories	.7	.6	.5	.6	.6	.6	.7
Shoes	.4	.4	.4	.5	.5	.5	.5
Goods for resale							
Work wear	7.5	8.1	8.3	10.4	9.9	9.8	9.3
Other outerwear	5	6.8	7.4	9.2	7.2	5.5	6
Underwear	12.7	13.4	13	12.7	12.8	13	14.3
Other / accessories	15.1	17.4	13	8	10	10.5	10.1
Shoes	13.3	11.4	12.7	14.2	15.2	14.7	15
Costs for work performed by other firms							
Work wear	8.7	9.7	9.4	10.4	10.3	10.6	11.9
Other outerwear	15.1	16.1	15.6	12.8	12.4	12.2	11.4
Underwear	10.7	9.9	10.5	8.7	9.5	9.2	9.7
Other / accessories	6.2	5.3	4.5	3.8	3.8	4.4	3.8
Shoes	4.6	4.6	5	4	4.1	4.2	4.7
Gross wages							
Work wear	16.8	17.6	16.4	17.5	16.6	15.6	14.7
Other outerwear	13.5	13.1	13.4	13	12.8	12.3	12
Underwear	18.8	17.8	17.4	16.7	16.9	16.6	17.4
Other / accessories	22.2	19.7	19.1	18	17.1	16.8	16.5
Shoes	14.7	13.8	13.7	13.5	12.8	12.2	12

Source: Statistisches Bundesamt, *Kostenstrukturerhebung im Verarbeitenden Gewerbe*, own calculations.

Table 3a: Descriptive statistics, firm level, 2000

Variable	Clothing industry		Shoe industry		P-Value mean
	Mean	Std.Dev.	Mean	Std.Dev.	difference[a]
Firm age (years)	12.93	9.76	15.19	9.00	0.0000
Firm size	17.81	56.31	37.32	133.33	0.0065
Share of German workers	91.08	22.62	94.27	16.14	0.0012
Share of women	84.25	26.68	61.17	31.65	0.0000
Share of full-time workers	47.40	39.98	60.73	35.99	0.0000
Share of white collar workers	15.84	28.03	20.43	28.93	0.0053
Share of blue-collar workers	21.34	32.85	24.01	30.28	0.1267
Share of university graduates	1.417	7.928	.87	3.86	0.0416
Share of low-qualified workers	20.02	32.02	23.30	31.04	0.0654
Share of workers below 30 years of age	17.53	28.95	16.97	24.10	0.6927
Share of workers between 40 and 54 years of age	24.85	30.02	20.98	24.54	0.0077
Share of workers above 55 years of age	21.39	29.59	23.57	29.08	0.1895
Number of firms	2,126		361		

[a] P-values are from a standard two sample mean comparison test assuming unequal variances in both groups.

Table 3b: Descriptive Statistics, firm level, 2000 to 2006

Variable	Clothing industry		Shoe industry	
	Mean	Std.Dev.	Mean	Std.Dev.
Firm age (years)	16.08	9.97	18.00	9.13
Firm size	19.05	56.15	42.47	167.53
Share of German workers	91.76	21.28	94.75	13.47
Share of women	84.31	25.12	61.82	29.08
Share of full-time workers	44.52	38.59	58.93	35.21
Share of white collar workers	15.72	26.62	22.40	29.29
Share of blue-collar workers	19.98	31.34	21.64	28.24
Share of university graduates	1.48	7.46	1.02	4.89
Share of low-qualified workers	18.53	30.24	20.51	29.11
Share of workers below 30 years of age	15.40	26.75	16.48	22.33
Share of workers between 40 and 54 years of age	26.43	29.22	22.67	23.92
Share of workers above 55 years of age	23.27	29.97	23.80	28.47
Number of firms	2,126		361	
Number of firm-year-observations	10,359		2,028	

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Table 4: Firm-level estimates for exits and employment, FE-Regression

	Firm exit	Total employment (number of workers)
Interaction treatment*2001	0.0360***	-0.4588
	(0.0127)	(1.0850)
Interaction treatment*2002	0.0538***	-1.9021
	(0.0150)	(1.6164)
Interaction treatment*2003	0.0369**	-0.7148
	(0.0167)	(2.3517)
Interaction treatment*2004	0.0585***	-3.1520
	(0.0167)	(3.5669)
Interaction treatment*2005	0.0564***	-3.9984
	(0.0160)	(4.1917)
Interaction treatment*2006	0.0653***	-5.7841
	(0.0210)	(5.3233)
2001	0.0195*	-0.4544
	(0.0105)	(1.0581)
2002	0.0172	-0.7450
	(0.0123)	(1.5440)
2003	0.0192	-2.8376
	(0.0146)	(2.3071)
2004	0.0015	-1.1101
	(0.0152)	(3.5215)
2005	-0.0209	-1.5257
	(0.0163)	(4.1120)
2006	(dropped)	-0.5380
		(5.2233)
Firm age (years)	0.0269***	
	(0.0038)	
Firm age (squared)	-0.0002***	
	(0.0001)	
Firm size	-0.0010***	
	(0.0003)	
Firm size (squared)	0.0000***	
	(0.0000)	
Share of German s(percent)	-0.0003	
	(0.0003)	
Share of women (percent)	-0.0002	
	(0.0003)	
Share of full-time workers (percent)	-0.0006***	
	(0.0002)	
Share of white-collar workers (percent)	0.0006*	
	(0.0003)	
Share of blue-collar workers (percent)	0.0003	
	(0.0002)	
Share of high-qualified workers (percent)	0.0013	
	(0.0009)	
Share of low-qualified workers (percent)	0.0002	
	(0.0002)	
Share of workers below 30 year sof age (percent)	0.0001	
	(0.0002)	
Share of workers 45 to 54 years of age (percent)	-0.0005***	
	(0.0002)	
Share of workers above 54 years of age (percent)	-0.0002	
	(0.0002)	
Constant	-0.1917**	25.4044***
	(0.0772)	(0.4736)
Regional dummies (*Bundesland*)	(included)	(included)
No. of firms	2,487	2,487
Firm-year-observations	12,387	12,387

Coefficients, standard errors adjusted for clustering on the firm level in parentheses. */**/*** denote significance on the 10%, 5% and 1% level.

Table 5a: Descriptive Statistics, individual level, 2000

Variable	Treatment group			Control group			P-Value: means different[a]
	Obs	Mean	Std.dev	Obs.	Mean	Std.dev.	
Ln(Monthly real wage)	6232	7.23	.80	1770	7.40	.71	0.0000
Monthly real wage (Euro, 2000 prices)	6232	1757.57	1013.04	1770	1983.73	1059.93	0.0000
Employed (1 = yes)	6422	.97	.17	1813	.98	.15	0.1622
Occupation change after 2000 (1 = yes, 2-digit occupations)	6422	.05	.23	1813	.05	.21	0.1048
Occupation change after 2000 (1 = yes, 3-digit occupations)	6422	.06	.24	1813	.05	.21	0.0080
Industry change after 2000 (1 = yes, 2-digit industries)	4836	.11	.31	1370	.08	.28	0.0031
Occupation change after 2000 (1 = yes, 3-digit industries)	4836	.11	.31	1370	.08	.28	0.0027
Age (years)	6422	41.10	11.40	1813	40.78	10.89	0.3244
Potential experience (years)	5702	29.62	11.67	1669	29.19	11.34	0.2264
No-Post-School Education (1 = yes)	6422	.25	.43	1813	.35	.48	0.0000
Vocational training (1 = yes)	6422	.58	.49	1813	.50	.50	0.0000
University graduate (1 = yes)	6422	.03	.17	1813	.049	.22	0.0039
Male (1 = yes)	6422	.20	.40	1813	.39	.49	0.0000
Number of Individuals	1,804			494			

Note: Data are measured on a quarterly base.

[a] P-values are from a standard two sample mean comparison test assuming unequal variances in both groups.

Table 5b: Descriptive Statistics, individual level, 2000 to 2006

Variable	Treatment group			Control group		
	Obs	Mean	Std.dev	Obs.	Mean	Std.dev.
Ln(Monthly real wage)	32,798	7.33	0.75	10,172	7.48	0.65
Monthly real wage (Euro, 2000 prices)	32,798	1896.44	1060.68	10,172	2087.50	1073.54
Employed (1 = yes)	35,811	0.94	0.24	10,713	0.96	0.19
Occupation change after 2000 (1 = yes, 2-digit occupations)	35,811	0.20	0.40	10,713	0.14	0.35
Occupation change after 2000 (1 = yes, 3-digit occupations)	35,811	0.22	0.42	10,713	0.16	0.36
Industry change after 2000 (1 = yes, 2-digit industries)	34,223	0.30	0.46	10,271	0.26	0.44
Occupation change after 2000 (1 = yes, 3-digit industries)	34,223	0.30	0.46	10,271	0.26	0.44
Age (years)	35,811	41.70	10.97	10,713	41.85	10.34
Potential experience (years)	31,193	30.26	11.21	9,648	30.30	10.93
No-Post-School Education (1 = yes)	35,811	0.21	0.41	10,713	0.30	0.46
Vocational training (1 = yes)	35,811	0.58	0.49	10,713	0.52	0.50
University graduate (1 = yes)	35,811	0.03	0.18	10,713	0.05	0.22
Male (1 = yes)	35,811	0.19	0.39	10,713	0.41	0.49
Number of Individuals	1,804			494		

Table 6: Individual-level estimates for wages and employment, FE-Regression

	Log monthly wages	Employment	Occupation change (relative to 2000)		Industry change (relative to 2000)	
			2-digit-level	3-digit-level	2-digit-level	3-digit-level
Interaction treatment*2001	-0.0031	-0.0372***	0.0022	0.0038	0.0335***	0.0337***
	(0.0152)	(0.0080)	(0.0113)	(0.0122)	(0.0125)	(0.0126)
Interaction treatment*2002	-0.0164	-0.0502***	0.0155	0.0209	0.0508***	0.0510***
	(0.0213)	(0.0116)	(0.0149)	(0.0160)	(0.0179)	(0.0180)
Interaction treatment*2003	-0.0259	-0.0358**	0.0419**	0.0529***	0.0684***	0.0687***
	(0.0240)	(0.0142)	(0.0163)	(0.0173)	(0.0216)	(0.0216)
Interaction treatment*2004	0.0069	-0.0167*	0.0528***	0.0655***	-0.0472*	-0.0469*
	(0.0207)	(0.0088)	(0.0189)	(0.0199)	(0.0259)	(0.0259)
Interaction treatment*2005	0.0349	-0.0190**	0.0478**	0.0577**	-0.0655**	-0.0654**
	(0.0228)	(0.0093)	(0.0221)	(0.0231)	(0.0285)	(0.0285)
Interaction treatment*2006	0.0248	-0.0186**	0.0558**	0.0662***	-0.0433	-0.0451
	(0.0254)	(0.0094)	(0.0245)	(0.0257)	(0.0298)	(0.0298)
Potential Experience	0.0257**	0.0755***	0.0440***	0.0486***	-0.0458***	-0.0459***
	(0.0100)	(0.0083)	(0.0096)	(0.0097)	(0.0107)	(0.0107)
Potential experience (squared)	-0.0006***	-0.0001***	-0.0004***	-0.0004***	-0.0003**	-0.0003**
	(0.0001)	(0.0000)	(0.0001)	(0.0001)	(0.0001)	(0.0001)
No post-school Education	-0.3007***	0.2454***	-0.0103	-0.0057	-0.2050***	-0.2051***
	(0.0577)	(0.0250)	(0.0324)	(0.0331)	(0.0351)	(0.0351)
University Graduate	0.2146*	0.4917***	0.1623*	0.2084**	-0.2890***	-0.2893***
	(0.1159)	(0.0506)	(0.0855)	(0.0905)	(0.0842)	(0.0842)
2001	0.0145	-0.0673***	0.0187	0.0195	0.1161***	0.1161***
	(0.0149)	(0.0106)	(0.0125)	(0.0130)	(0.0135)	(0.0135)
2002	0.0539**	-0.1570***	0.0260	0.0284	0.2407***	0.2409***
	(0.0237)	(0.0190)	(0.0206)	(0.0213)	(0.0233)	(0.0233)
2003	0.0498	-0.2601***	0.0116	0.0085	0.3949***	0.3951***
	(0.0308)	(0.0267)	(0.0273)	(0.0279)	(0.0311)	(0.0311)
2004	0.0730**	-0.2771***	0.0219	0.0159	0.5123***	0.5126***
	(0.0368)	(0.0322)	(0.0360)	(0.0365)	(0.0403)	(0.0403)
2005	0.0588	-0.3387***	0.0378	0.0350	0.6271***	0.6277***
	(0.0454)	(0.0399)	(0.0445)	(0.0451)	(0.0487)	(0.0487)
2006	0.0788	-0.4051***	0.0334	0.0275	0.7042***	0.7067***
	(0.0535)	(0.0474)	(0.0525)	(0.0532)	(0.0564)	(0.0564)
Constant	7.3541***	-1.0930***	-0.7745***	-0.8949***	1.6782***	1.6802***
	(0.2431)	(0.2180)	(0.2309)	(0.2327)	(0.2493)	(0.2492)
Quarter of year dummies	Included	Included	Included	Included	Included	Included
No. of individuals	2,147	2,149	2,149	2,149	2,127	2,127
Individual-quarter-obs.	38,640	40,811	40,811	40,811	38,988	38,988

Coefficients, standard errors adjusted for clustering on the person level in parentheses. */**/*** denote significance on the 10%, 5% and 1% level. The difference in the number of individuals between wage estimations and the other estimations is caused by two individuals with only one wage observation.

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