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Bargaining Power and Local Heroes*

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Abstract

Retailer bargaining power is an important aspect of many international antitrust investigations. Size and market share analysis are often the cornerstones of bargaining power identification. However, other factors, like consumer behavior,i.e. "one-stop shopping", can heavily affect the bargaining environment. We show and quantify, analyzing a natural experiment of a supply boycott of small local beer breweries towards a national retailer, that "one-stop shopping" leads to severe purchasing externalities, which is a novel finding. This results in a shift in the bargaining position in favor of the manufacturer, which is relevant for antitrust investigations. Neglecting those externalities in investigations and focusing mainly on size arguments may lead to a wrong assessment of bargaining power. Therefore a careful a case by case analysis of bargaining power is necessary in antitrust investigations.

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1 Introduction

Recent antitrust investigations in retail highlight the importance of buyer power, or to put if differently, the significance of retailer bargaining power towards manufacturers as a potential source of harm for competition in those markets (e.g., Australian Competition and Consumer Commission 2008, Competition Commission 2008, European Commission 1999, 2000, German Federal Cartel Office 2008, Rodriguez 2006).[1] However, those investigations lack a clear understanding of how to quantify the distribution of bargaining power along the value chain, which leads to a rather strong reliance on easy available indicators such as size or market shares.[2] This is not satisfactory, given that recent literature indicates the complexity of the distribution of bargaining power, which is driven by other factors like alternative supply channels due to private labels (Smith and Thanassoulis 2009), pivotal sizes (Raskovich 2003) or customers' behavior (Campo et al. 2000). In particular, the latter is important, since the phenomenom of "one-stop shopping" behavior, i.e. the concentration of purchases, is prevalent in retail and leads to those complementarities (e.g. Caprice and von Schlippenbach 2012).[3] There is still a controversy of how these complementarities, caused by "one-stop shopping" behavior, translate into a more favorable position for either the retailer or the manufacturer (von Schlippenbach and Wey 2012, Caprice and von Schlippenbach 2012, Johansson 2012).

This study now aims at closing this gap and provides an empirical investigation of bargaining in retail. Most importantly, it quantifies the size of complementarities in purchases that arise due to one-stop shopping behavior empirically. This has – to the best of our knowledge – not been quantified before. Most empirical studies of bargaining power concentrate on one product categorie and therefore do not provide a connection between

[1] Although there is a large interest of antitrust authorities, there is a debate on the welfare effects of buyer power. For instance, Inderst and Mazzarotto (2008) discuss potential theories of harm as well as potential welfare enhancing effects of buyer power.

[2] In the merger cases REWE/Meinl and Promodes/Carrefour the European Commission (1999, 2000) used the threshold of 22 % of manufacturers' sales as the most important indicator to derive buyer power (For a discussion, see Dobson 2005). Relying on such indicators is not uncommon. The German Federal Cartel Office (2010) also uses in the merger case EDEKA/Trinkgut a market shares analysis. This does not mean that the authorities exclusively rely on these measures. They also use other indicators derived by the theory. However, a sound framework for analysis is still missing.

[3] The original argument of complementarities in consumption traces back to Stahl (1982). He uses the non-convexity of transportation costs to show that customers concentrate their purchases.

the retailer-supplier relationship and consumer behavior, because of the missing of other product categories complementarities between goods cannot be taken into account. This is where our paper adds to the literature. We show how complementarities based on consumer behavior effect retailers' revenues and can have significant effects on its bargaining power.

The results indicate that complementarities are large and have a severe impact on the overall store revenues of a retailer. In the second step, this information is used to show the impact of those complementarities on the bargaining positions (i.e. the differences between agreement and disagreement profits) between retailer and manufacturers. Given the found size of the complementarities it is not surprising that the bargaining situation is heavily affected and crucially affects the bargaining situation. This understanding is crucial to improve the precision of any further antitrust analysis of bargaining power analysis in retail markets that relies on a precise definition of the initial bargaining positions.

The analysis takes advantage of two particular case studies which are quasi natural experiments allowing the identification of bargaining positions affected by complementarities in purchases. In particular, there were two independent breakdowns of supply negotiations between a large German retailer and two local breweries.[4] The final decisions to end those negotiations were driven by the manufacturers and were not, anticipated by the retailer so that we claim exogeneity of those events and use them as two natural experiments. The framework for the analysis is a bargaining framework in which the study identifies the different bargaining positions. Our difference-in-differences and panel fixed effects estimates clearly show that at least in one case, there is a significant loss in revenues for the stores in the treatment group for products in the non-beer categories. These losses exceed the retailer's initial revenues generated by the particular beer brands and so indicate the existence of "one-stop shopping" driven complementarities. This implies a favorable bargaining position of the manufacturer. The observed effect is important for the antitrust in at least two regards: First, the ignorance of consumer behavior can lead to a wrong assessment of bargaining power. Secondly even comparatively small manufacturers can obtain a favorable bargaining position, such that size-related measures can indicate wrong patterns.

[4]Consumers in these regional markets became aware of these boycotts, after the supply of the products stopped. This is documented in regional newspapers that covered the story. Moreover, there were lively discussions in regional social networks after the events.

The paper relates to different strands of literature. First, it relates and is motivated by the literature empirically analyzing bargaining power of retailer (i.e. buyer power). It is striking that although buyer power is seen as a potential source of harm for antitrust authorities[5], there are only few empirical studies that aim in identifying buyer power. Buyer-size effects are identified in empirical studies using intra-industry data as in Chipty (1995).[6] Chipty (1995) uses multi-country data from the cable TV industry to estimate how the number of channels provided and the prices of programs depend on company size. The conclusion of his study is that large downstream cable operators provide more channels and subscriptions at all prices compared to their smaller competitors. Main reasons for these observation are scale economies of larger cable operators and their superior bargaining power when negotiating for program content. Ellison and Snyder (2011) show that large drugstores do not receive any discounts when there is a monopolist supplier for antibiotics, but they receive small discounts from suppliers facing significant competitive pressure. However, the most sophisticated analysis is by Draganska et al. (2011). They provide a structural econometric analysis of bargaining power between manufacturers and retailers to analyze the German coffee market and provide some evidence on the determinants of bargaining power, which are for example firm size and the introduction of private labels. They identify a full Nash-Bargaining Framework and find for various retailer-manufacturer bargaining situations, different realizations of bargaining power. Interestingly, the distribution of bargaining power is not clearly in favor of either manufacturer or retailer, but depends on different combinations. However, they focus only on a specific category and cannot take into account complementarities with other products.[7] As a result, the important connection of the supplier-retailer relationship with consumer behavior is missing. Complementarities between products or product categories arising from consumer behavior as one-stop-shopping can be an important determinant of a retailer's bargaining power.

[5]See OECD (2008) summarizing the discussion among antitrust authorities. See Inderst and Mazzarotto (2006) for a survey of the potential sources of harm of buyer power as well as potential positive, welfare enhancing effects

[6]Another important finding of Chipty (1995) is the fact, that large buyers compared to their smaller competitors, usually get discounts, only in case the upstream market is characterized by sufficient competition.

[7]Another structural estimation incorporating bargaining theory in the framework is Grennan (2013), who investigates medical device business-to-business markets (coronary stents). He also does not consider any interrelations with other products, however, in his case, those are contrary to retail markets, probably not relevant.

Complementarities and their relation to customer behavior have been examined in the theoretical literature to some extent. Generally, consumers prefer stores offering a wide range of products to smaller stores with less products, which has been formalized in a seminal paper by Stahl (1982). The reason behind this effect is the convexity of transport costs. These costs imply that consumers concentrate their purchases avoiding those costs. The paper of Klemperer (1992) uses a similar argument introducing fixed shopping costs. Those also lead to a concentration of sales. When competitors choose similar product lines, consumers tend to single home, whereas in the case of diverse product lines, consumers are more likely to multihome and divide their shoppings to several stores. These complementarities between goods are important and can change firms' strategies substantially, as has been described, for instance, by Lal and Matutes (1994). They show that offering some goods below marginal cost can be a profitable strategy, because consumers who enter shops due to these special offers often buy additional products, which are not offered with discounts. As a result, retailers gain additional profits because of the products bought by customers who entered the store to purchase the products sold below marginal cost. In the context of retail markets the concentration of purchases, which is described as the consequence of relevant complementarities, is also often referred to as "one stop shopping". This is a form of consumer behavior, where consumers purchase all their weekly groceries from a single retailer (e.g., Competition Commission, 2000: 30).

These complementarities now have significant effects on the shopping behavior and in particular on the switching behavior across products and retail locations. Sloot et al. (2005) show in an empirical study using information of customer surveys, that consumers are more loyal to so called high equity brands than to low equity brands. Consequently, the probability of consumers to switch stores, highly depends on the type of the good, whether it is characterized as high equity or low equity brand. Still they cannot quantify the size and value of the externality. Empirical, descriptive, evidence for one-stop-shopping and also two-stop-shopping is reported in Smith and Thomassen (2012). Delisting a certain product with a large market share in local markets can harm overall store revenues significantly, because one-stop-shoppers switch to an other store where they can find the preferred product.[8] Therefore, it gets evident that complementarities in the form of "one

[8]See also Campo et al. (2000) who come to a similar result that consumer reaction due to stock-outs may be harmful for retailers and manufacturers. However, they derive their conclusions only by using consumer survey data and cannot, as the other literature does quantify the size of externalities.

stop shopping" are heavily affecting the competitive environment[9] and probably also the bargaining power distribution between retailer and manufacturers. Still the literature has not yet quantified the particular size of an externality.

This impact of complementarities resulting of one-stop shopping on the bargaining situation is analyzed by some recent, however, entirely theoretical literature. For instance, Caprice and von Schlippenbach (2013) analyze how one-stop-shopping behavior, i.e. the complementarities between formerly independent products, has an impact on retailers bargaining position. They analyze the role of slotting fees in a two part tariff framework where there are two manufacturers and one retailer. They show that due to complementarities the retailer and one manufacturer can extract rents from a second manufacturer. Differently, von Schlippenbach and Wey (2011) show that "one-stop shopping preferences" can lead to a worsening of the retailer's outside options in a bargaining situation, while Johansen (2011) shows in a framework with single- and multi-product retailer, that an enhancing share of "one-stop-shoppers" leads to an increase of multi-product retailers' bargaining power against their suppliers. As a result, retailers' bargaining power is to a certain degree related to consumer behavior.

Our paper relates in two aspects on the discussed literature. First, it quantifies the size of the externalities in purchases that result of the "one-stop shopping" and second it shows how those externalities affect the bargaining positions. To the best of our knowledge this has not been tested empirically before before.

The next section briefly discusses the data and the natural experiment case, which is followed by the empirical strategy. Afterwards, the results are presented and finally, a conclusion is drawn.

[9]For the effects of complementarities on merging behavior, also see Beggs (1994). For the effect of particular "one stop shopping" on mergers in retail, see Johansen and Nilssen (2013).

2 The Natural Experiment

2.1 The Beer Market

Our analysis takes advantage of the fact that two regional beer breweries stopped supplying a specific retailer within a relatively short period of time. This retailer offers a full assortment including for the drink category a range of different beer brands.

The beer market is very well suited for this kind of analysis, because there are several national and international brands as well as many small regional or even local brands. In particular, the variety of breweries in the German market is high with 1,341 breweries in 2011, which is followed by the British market with 946 breweries and 442 breweries in France (Brewers of Europe 2012). Given the information about the cooperating retailer, these brands often have significant market shares in their local markets and are serious competitors of the national and international brands within these markets. In general, one can argue that regional beer is a specialty and has a unique sale point, which may also apply to other kinds of products like special regional cheese, wines or similar products.[10]

The regional focus of the particular local beer brands is a special feature of the German beer market. There are many city specific or county specific small beer breweries that focus entirely on the local market. They are often important for the cities' identity. To give a striking example, Cologne and Duesseldorf are two very close cities. However, their residents prefer two highly different kinds of beer with several regional brands. While in Cologne people drink "Koelsch", a bright and light beer, people in Duesseldorf drink "Alt", a very heavy and dark beer. People in those cities usually treat it as an offense to be offered the "wrong" kind of beer. Besides, that both cities are very similar culturally.

2.2 The observed case

We observe two independent cases of supplier boycotts, which are, however, similar in their nature.

These suppliers' boycotts happened due to failed negotiations about conditions for the

[10]We thank Tore Nilssen for this comment.

further supply of beer. They were unexpected by the retailer, and only affected, due to the beers' local focus, only a subset of overall retail outlets. Therefore, this setting constitutes a natural experiment. In particular, there were two independent breweries, each offering one beer brand (Brand A by supplier A and brand B by supplier B).[11] An important aspect of these two cases is that both suppliers are regional brands that only deliver beer to some stores of this supermarket chain. As a result, we are able to create a treatment group as well as a control group to apply the difference-in-differences technique as well as panel models.

Our dataset contains information about 61 southwest branches of a large German supermarket chain between January 2010 and April 2012. Given the typical broad segments of supermarkets, discounters and full-line distributers, the observed supermarket chain can be claimed to be a full-line distributor.[12]

We observe store level beer revenues and overall revenues as well as the revenues of our two particular beer brands. Furthermore, we know in which county (Landkreis) the branch is located and we also know the number of competing supermarkets in the area. Descriptive statistics for the dataset are in table (1).

In the first case the brewery stopped delivery of its brand (brand A) on June 1st, 2011. Our treatment group encompasses nine stores and our control group 22 stores. The second refusal of delivery affected 53 retail outlets and the control group includes seven stores (brand B). In the second case the brewery stopped supply on January 1st, 2012. Our sample continuously comprises all stores, therefore, the stores delivering brand a are also in the control group for brand b and vice versa.[13] One particularity of the described cases is that they have been accompanied by a large interest of the local press. Table (2) shows the stores in our dataset.

As can be obtained from the table, we have an overlap between our treatment groups. However, the overlap is rather small and given that we can use the stores of treatment B as a control group, for treatment A, because until January 2012 there is no treatment for

[11] Of course, the retailer offered a variety of different beer brands.

[12] However, one should note that the supermarkets can be characterized as large supermarkets or hypermarkets.

[13] However, given that the boycott of brand B is later in time, we do not use the observations were brand b has a boycott treatment for the estimation of case A to avoid bias. Still we also provide joint estimates of both treatments, which provide the same results as the single estimations of the cases individually.

group B, the overlap should not cause severe problems within our estimations. In different words, due to the timing of the two boycotts, we can use the stores from treatment B combined with the no treatment stores as control group for case A. For case B we add the treatment A stores to our no treatment stores to create the control group. As discussed before, due to the small overlap this fact should not cause major problems. We also have another information available since the store managers were obliged to identify their main perceived competitors. Since there were some stores that did not face a competitor, we did not estimate the effects for those stores since consumers do not have easily available switching opportunities.

3 Empirical Strategy

The study aims at analyzing whether purchasing complementarities due to "one-stop shopping" arise and affect the bargaining environment between retailer and manufacturer. In particular, we investigate if those complementarties are observable with products of small locally differentiated firms and can be translated into a favorable bargaining position when manufacturer negotiate with retailers. We exploit a particular off-equilibrium situation, namely the two cases of unexpected breakdowns of negotiations between a retailer and a manufacturer, which allows us to identify a treatment effect (i.e. the revenue difference of supply and no supply). This revenue difference allows us to gain important information on the revenue related profit difference between the agreement and the disagreement profit. This, ignoring the exploitation of bargaining power, defines the bargaining positions. However, we do not have any information on potential transfers between manufacturers and the retailer, such that we cannot infer the whole bargaining system, particularly the exogenous bargaining strength or bargaining power of the bargaining participants. The given information still allows us to show how different the dimensions of the outside options of retailers and manufacturers are. The magnitude of those outside options is one cornerstone in the assessment of bargaining power in competition analysis (e.g., Competition Commission 2008, Australian Competition and Consumer Commission 2008). Since -everything else equal- a decrease in the value of the outside option leads to an inferior bargaining position (see also Inderst and Mazzarotto 2006).

Although the results are technically identified only for the analyzed cases, they still allow

us to derive general conclusions for other markets. We take advantage that the manufacturers, i.e. the two breweries, each of which sells its products only at one specific local market, are typical small suppliers. Neither their absolute size nor their share of the whole product category is large. The only clear difference from any other small supplier is that each of the manufacturers sells a particular good, a locally differentiated beer, on a small market. Since local beer brands are usually strongly tied to the identity of the local markets, they provide an intense local differentiation strategy. In addition, there are no other relevant differences, so that we claim, that the identified treatment effect derives from the boycott of the small locally differentiated supplier. Therefore, the results are interpreted by taking into account this specific characteristic.

The following section first describes the theoretical bargaining framework and the corresponding elements we claim to identify. Then follows the main identification strategy. Finally, several robustness checks are conducted to test the validity of the identified treatment effects.

3.1 Theoretical Framework

To analyze the distribution of bargaining power between the mentioned nationwide active retailer and the two manufacturers, we apply the conceptual framework of a widely used Nash-bargaining framework between two parties[14]:

$$argmax_{p_M,T}(\underbrace{\Pi_R^A(p_M,T,E)-\Pi_R^{DA}}_{\Delta^R})^{\lambda}(\underbrace{\Pi_M^A(p_M,T)-\Pi_M^{DA}}_{\Delta^M})^{1-\lambda} \tag{1}$$

Equation (1) shows the bargaining framework when the retailer and the manufacturer bargain about the wholesale price (p_M) of the manufacturers' product for the retailer and potentially about a fixed transfer (T_i with $i \in (R,M)$) either from the retailer to the manufacturer or from the manufacturer to the retailer. The agreement profit ($\Pi_i(p_M,T,E)=\pi_i+E+T$) consists of a revenue related part, that is a function of the wholesale price $\pi_i(p_M)$, the above discussed directly determined transfer T_i and potential externalities in purchases E that captures the profits gained with revenues from other

[14]See for a in depth discussion of the asymmetric Nash-Bargaining solution Binmore et al. (1986). See also for similar applications in retail markets Draganska et al. (2011), Meza and Sudhir (2010).

products that has been caused by the customers attracted by the sales of the product which is analyzed.[15] The disagreement profit - ad definitionem- does not include neither the transfer nor the wholesale price not the externality. Importantly, the overall solution is determined by the product of the difference (Δ^i) between the agreement profit Π_i^A and the disagreement profit Π_i^{DA} for the retailer ($i = R$) and the bargaining manufacturer ($i = M$), which is also called endogenous bargaining power or the bargaining positions. These differences (Δ^R and respectively Δ^M) are weighted by a factor λ that is also known as the exogenous bargaining power (e.g., Binmore et al. 1986). This bargaining power or bargaining strength is exogenously given and depends on all other, non-observed factors as for instance different time preferences of the bargaining parties. As discussed by Inderst and Mazzarotto (2006), shifts in Δ^i, or to put it differently, the relative value of the disagreement profit compared to the agreement profit, indicate unambiguously shifts in bargaining power.[16]

We aim at identifying the revenue related profit (agreement and disagreement profit) in equation (1). This enables us to determine the relative differences between the agreement and the disagreement profit of both, the retailer and the manufacturers. Since the missing transfer information only indicates how the overall profit is distributed ($T \in [\Delta^R, \Delta^M]$) given the exogenous bargaining power ($T = \lambda\Delta^M - (1-\lambda)\Delta^R$), we can well define the positions in the bargaining game. Taking into account the above argument the unambiguous shifts of bargaining power, if everything else is held constant, the relative difference of the agreement and disagreement profit now shows the relative bargaining positions and power of the bargaining parties.

3.2 Identification Strategy

To identify the elements of the framework presented in the previous subsection, we use the natural experiment of termination of the negotiations between the retailer and the manufacturers. This situation can be seen as a natural experiment, since it is an off-

[15] We assume that externalities are only relevant for the retailers' product since we claim complementarities to be important such that the manufacturers sales are not subject to severe externalities.

[16] The literature calls the disagreement profit often "outside option" (e.g., Inderst and Mazzarotto, 2008).

equilibrium situation, that is exogenous for the treated entities (retail outlets).[17] Our interest lies in the revenue related difference between the agreement and the disagreement profits. The information we have is revenues on the retail outlet level. The data contains retail outlets that are either treated or non-treated. Given that the untreated retail outlets are located closely to the treated ones, we can easily assume, that they only differ with respect to their beer brand, but are otherwise affected by the same exogenous shocks. The availability of a beer brand in a particular region is -given the previously discussed structure of the German beer market- not driven by selection processes, but by historical developments.

The treatment of the retail outlets now leads to a change in the sales of: a) the particular beer brand b) the beer category and c) the retail outlet. The identification of the treatment effect is therefore a first step in the identification of agreement and disagreement profits. This allows us to identify profits on the same levels, from the brand to the outlet level.

Retailer's Profits

For both independent cases, we apply the so called difference-in-differences (DiD) approach. The main identification assumptions are that both the treatment and the control groups are exposed to the same unobservable impacts during the treatment and the non-treatment period and that the treatment is the only distinguishing element (for an in-depth discussion, see Angrist and Pischke 2007). This assumption is fulfilled, most probably, because we observe retail outlets in close neighboring counties that are structurally similar. We expect that major structural impacts are the same for both the treatment group and the control group. The strong locality of the beer taste and the consideration of beer as a regional identification good, allows the particular assumption that is the purchase of this beer brand and the non-availability afterwards is the only large distinguishing element. The model now is formalized as follows (Wooldridge, 2010: 147-149):

$$REV_j = \beta_{0_j} + \beta_{1_j} B + \beta_{2_j} T + \underbrace{\beta_{3_j}}_{TreatmentEffect} BxT + \beta_{TD_j} \sum TD + u \qquad (2)$$

with $j \in \{a, b, c\}$

[17] The theoretical model would predict a bargaining solution as long as there are positive net profits out of the bargaining. Therefore, a failure in the negotiations is off-equilibrium.

REV_j is our dependent variable. We define this variable according to three different specifications: The first definition considers REV_j as the overall store's revenues REV_a, the second considers the store's beer revenues REV_b, and the third one the store revenues without the beer revenues REV_c. Importantly, the store revenues comprise all revenues of the store across all product categories, the beer revenues contain all beer revenues including several beer brands. All measures of revenue are normalized[18], that is, the variable REV_j describes the revenue in the particular period divided by the store's average revenue over time. This is done to make the different revenues also comparable in the cross section. B is a dummy variable which takes the value one if a store belongs to our first treatment group, losing their supply of beer brand a and zero otherwise. This dummy variable measures differences between the two groups of supermarkets that possibly existed before the treatment. T is a dummy variable that indicates the treatment period and equals one if the stores of the treatment group are not supplied with brand a in the respective time period and takes the value zero otherwise. Such time dummy variables capture aggregate economic factors changing even without the delivery boycott. The most important term of the regression equation is BxT and the corresponding coefficient β_{3j}. This interaction term measures the treatment effect we are interested in. What is the difference in revenues per store between treatment group and control group caused by the loss of beer brand a? The particular interpretation depends on the exact definition of the dependent variable and is discussed in the following paragraphs. Additionally, we take into account time specific dummies (TD) that indicate the particular period, since the store and particularly the beer revenues may suffer from seasonality. Additionally, the term u_t is the standard error term satisfying the usual assumptions (Greene, 2008: 11-19).

After identifying the treatment effect on the revenue measure defined, we calculate the impact for the relevant profits. We use common industry revenue to profit ratios (PSR) and multiply those with the revenues to derive an approximation for profits ($\pi = REV_j * PSR$). To tackle the issue of measurement error that arises due to this imputation, we apply a sensitivity analysis of the results regarding this imputation.[19] In particular, we use

[18]We normalize each value by dividing the particular month's value by the average value across all months.

[19]Since balance sheet data is available for firms that are above a certain size. This allows us to calculate Revenue-Profit-Ratios for the retailer and one manufacturer. We use this as a benchmark in the sensitivity analysis.

bandwidths of the PSR variable to indicate the sensitivity of this imputation assumption. Moreover, we have to assume constant marginal costs of production and provision in the relevant range of quantities, which should be easily met, given the small overall revenues with the beer brand in comparison with the retail outlet.

Given the definition of the revenues, the treatment effect (β_{3_j}) enables us to calculate the difference between the agreement profit (π_R^A) and disagreement profit of the retailer π_R^{DA} regarding this specific case by multiplying one plus the treatment effect ($1 + \beta_{3_j}$) with the revenues and the profit/sales ratio. First we are interested in the impact on the overall outlets revenues. Given that the disagreement profit is often defined as strictly non-negative the treatment effect captures the revenue related profit π_R^{DA} as well as the possible externalities (E) and the profit which is unaffected by the revenues in the beer category (Ψ):

$$\pi_R^{DA} + E + \Psi = (1 + \beta_{3_a}) * REV * PSR_R \tag{3}$$

Given that the disagreement profit is at least equal to zero, but may be larger since the retailer might compensate some losses with other products, any negative value is the lower bound for the absolute value of the Externality E. The agreement profit – without the externality– is calculated similarly, by multiplying the revenues with the profit/revenue ratio:

$$\pi_R^A + \Psi = REV_a * PSR_R \tag{4}$$

The corresponding difference of revenue related profits δ_R is given by :

$$\delta_R = \pi_R^A + E + \Psi - \pi_R^{DA} - \Psi = (1 + \beta_{3_a}) * REV_a * PSR_R - REV_a * PSR_R \tag{5}$$

Using the specification REV_b, only considering beer category sales, the profits without the externalities are identified. However, since we know that there were special offers, the assumption of equal profit to revenue ratios with the beer brands is probably not satisfied as the revenues with other beer brands that are triggered by a marketing campaign are probably – everything else equal– lower as those ratios generated without a marketing campaign. The profits are then equal to:

$$\pi_R^{DA} = (1 + \beta_{3_b}) * REV_b * PSR_R \tag{6}$$

$$\pi_R^A = REV_b * PSR_R \tag{7}$$

Considering only revenues in the outlet without the beer category (REV_c) identifies the size of the externality:

$$E = (1 + \beta_{3c}) * REV * PSR_R \tag{8}$$

Manufacturers' Δ

Since we do not have information on the revenues or profits of the manufacturers, we need several assumptions to derive the revenue related difference between agreement and disagreement profits (δ_M). The information on the pre-boycott sales with the particular product and the post-boycott sales of the product allows an identification of the lower bound of the bargaining power distribution. The pre-boycott sales are the benchmark for the agreement profit. Therefore, although we cannot infer the exact size of the difference between the agreement and disagreement profits, we can provide an upper bound of those profits. Clearly, this assumes that there are no large positive externalities for the manufacturer from being listed at the retailer for the manufacturer. That means, as long as the sales in the retailer store are not leading to additional sales for the manufacturer elsewhere (e.g., introducing the product to new loyal customers) as a kind of positive externality, the disagreement profit is at worst zero. This assumption seems to be reasonable as the potential customer loyalty or advertisement effects can be supposed to be long-run effects, which are not relevant given the timing of the data. If there is some compensation elsewhere due to anterior capacity constraints, it is even positive.

The revenue related agreement profit for the manufacturers are derived by the product of the revenues of the retailer and one minus the average retailer profit, multiplied with a typical profit revenue ratio:

$$\pi_M^A = REV_b * (1 - PSR_R) PSR_M \tag{9}$$

The disagreement profit is, as discussed earlier, defined as the zero such that we can define

the manufacturers profit differences as:

$$\delta_M = \pi_M^A - 0 \tag{10}$$

We repeat all estimations and computations for the case of beer brand b using treatment group b and control group b.

3.3 Robustness Checks

To check for robustness, we apply several additional techniques and estimations. The robustness checks all try to ensure that the treatment effect is identified correctly. All profit computations then are the same as in subsection 4.2.

The first check tries to verify that our DiD estimations do not suffer from autocorrelation highlighted by Bertrand et al. (2004). As they show, this source of bias may lead to underestimated standard errors. To correct for this, estimations are repeated, as they suggest, first, with clustered boostrapped standard errors and, second, with only taking into account averaged pre- and post treatment period values. This should then help to tackle the problem of autocorrelation.

However, it is still possible that unobserved heterogeneity may bias the estimation of the treatment effect. Using the revenues as an index already helps to tackle pure size effects. Also the regional closeness of the stores with a very similar economic and cultural environment should reduce that unobserved heterogeneity. However, it cannot be ruled out that regional specificities such as the quality of the store managers or other factors may have an impact on the store performance. To account for time invariant unobserved heterogeneity, we apply a fixed effects estimation, which requires some small adjustments from estimation equation (2) since time-invariant effects cannot be considered:

$$REV_{t_j} = \beta_{0_j} + \beta_{3_j} AxT_t + \beta_{TD_j} Trend + FE + u \tag{11}$$

The notation in equation (11), however, remains similar to equation (2) with the constant β and the treatment effect β_3 of the interaction of the treatment subjects with the treatment period. Clearly, the treatment group and treatment time variable are not included,

as they are time invariant. The subscript t is added to indicate the panel structure. The fixed effects (FE) are on a retail outlet level and the time panel dimension remains monthly. The variable TD captures trend-specific due to time dummies ($Trend$). The variable u is the error term.

4 Results

4.1 Basic Results

Table (3) provides the estimates of the basic equation described in the empirical strategy section with heteroskedasticity-robust standard errors. For both cases (Case A & Case B) column (1) shows the analysis on the overall stores revenues. The results show that the sales are increasing over time with higher revenues in the post-treatment period than in the pre-treatment period with both effects being highly significant at the 1 % level (Treatment A, Coeff.: 0.3023, SE.: 0.0213; Treatment B Coeff.: 0.0881, SE.: 0.0128). In Case A the treated group has significantly more revenues (Group A, Coeff.: 0.0121, SE: 0.0121), while Group B is statistically not different from the untreated group (Group B, Coeff.: 0.0036, SE:0.0039). The treatment effects are, for Case A and Case B, both significant and negative. Case A shows a negative and weak significant effect with significance at the 10 % level (Treatment A x Group A, Coeff.: -0.0400, SE: 0.0211). Case B shows a slightly smaller effect than Case A, however, it is strongly significant at the 1% level (Treatment B x Group B, Coeff.: -0.0279, SE.: 0.0079). The explanatory value of both cases estimation is rather high with R^2 clearly above 70 %. The treatment effects' found are not only statistically significant, but are relevant from an economic view since the lost sales due to the boycott outnumber the anterior revenues with the corresponding brands. This speaks clearly in favor of externalities of the beer brands with sales with other products.

To analyze more precisely where revenues have been lost column (2) analyses the revenues in the beer category. As for overall retail outlets revenues, results in column (2) show that in both treatment periods the overall beer revenues are significantly higher (Treatment A, Coeff.: 0.2928, SE.: 0.0237; Treatment B Coeff.: 0.1921, SE.: 0.0193). Also regarding beer revenues, in Case A, the treatment group has higher revenues (Group A, Coeff.: 0.0203, SE.: 0.0065), while there is no statistically significant effect in Case B (Group B,

Coeff.: 0.0004, SE.: 0.0416). Interestingly, the treatment although negative in absolute terms is not statistically significant different from zero in both cases. In Case A it is not clear, whether the effect is only insignificant due to imprecise measurement since it is rather large but also subject to a large standard error (Treatment A x Group A, Coeff.: -0.0351, SE.: 0.0219). In Case B however, it is both, very small and subject to a rather larger standard error (Treatment B x Group B, Coeff.: -0.0028, SE.: 0.0126). This finding corresponds to the fact that special promotions occured in the beer category to stabilize sales after the one brand disappeared. Clearly, those promotions can be an expensive strategy. The rational for an investment into promotions, i.e. low prices (that may generate losses) into a specific category can be explained by so called "loss leader" products that are shown to work due to their low possibly loss generating price as an advertisement device, while the profit is gained by other products (Lal Matutes, 1994). In addition, the retailer may take into account the arising externality on other products when deciding on which product to put into the shelf. Since shelf space is limited, it may be that revenues are generated similarly with other than the two analyzed beer brands, but without such a strong externality on other products.

To analyze the impact of the two beer brands in both cases, column (3) takes into account only the store revenues without revenues in the beer category. The findings regarding the revenues in the treatment periods as well as between the groups remain unchanged. In both treatment periods revenues are higher (Treatment A, Coeff.: 0.3026, SE.: 0.0214; Treatment B Coeff.: 0.0850, SE.: 0.0129). With the treated group in Case A having slightly higher revenues than the untreated groups (Group A, Coeff.: -0.0117, SE.:0.0051) and no significant difference in Case B (Group B, Coeff.: 0.0036, SE.: 0.0040). Importantly, the treatment effects are significant in both cases (Treatment A x Group A, Coeff.: -0.0401, SE.: 0.0212, Treatment B x Group B, Coeff.: -0.0284, SE.: 0.0079). Since the dependent variable does not include any beer sales, the effect is on revenues with other products, which leads to an identification of externalities.

Table (4) now jointly estimates the impact of both cases to check whether the joint treatment of some of the outlets has an impact on the estimation of each case. The observed effect on the overall store revenues including beer remains the same (column 1), however, the effect on the beer revenues (column 2) becomes negative and significant for the first treatment effect (Coeff.: -0.0516, SE. 0.0169). This indicates in contrast to the previous estimations that the impact of the first treatment may be due to the impact on

the beer revenues. However, hardly unchanged results in column (3) for both treatments on the overall revenues without considering beer that are still significant and negative indicate that there is a relevant externality on the overall revenues in both cases.

The size of the effects of the estimations in table (3) are shown in table(5) for the the treatment effects of Case A and table (6) for the treatment effects of Case B. The quantification show the same as in the estimation tables before. While there is hardly any difference in the agreement and disagreement profit of the retailer without considering externalities when he bargains with the manufacturer for Case A (table 5, $\pi_R^A = 407,44$: $\pi_R^{DA} = 402,28$ to $\pi_R^A = 4074,35$: $\pi_R^{DA} = 4022,81$) and Case B (table 6, $\pi_R^A = 417,47$: $\pi_R^{DA} = 407,67$ to $\pi_R^A = 4174,33$: $\pi_R^{DA} = 4076,66$). The size of the externality clearly outsizes the overall revenue with the particular product (Case A: table (5), $E_A \in (596,57; 5965,72)$, Case B: table (6), $E_{A2} \in (423,25; 4232,51)$). This is shown in both cases for many different possible profit to revenue ratios used to determine the profits. Moreover, in both cases the size of the externality leads to the cases that the absolute difference in the δ's is larger for the retailer than for the manufacturer. However, for the first case, the profits earned are arguably small per outlet and month (table(6), $\delta_{MA1}^A \in (23,25; 53,14)$). Therefore, we doubt that Case A is robust, which we will show in the following robustness checks section. Therefore, we concentrate on the Case B. In the most extreme case the difference between retailer's agreement and disagreement profit is more than 24 times larger than profits of the manufacturer (table (6), $PRR\ Retailer = 0.10$ and $PRR\ Manufacturer = 0.07$ $\delta_R/\delta_{MA2}^A = 4160,51/169,63 = 24,52$). However, the ratio is highly dependent on the profit to revenue ratios as there are combinations for which the relationship is nearly equal (table (6), $PRR\ Retailer = 0.01$ and $PRR\ Manufacturer = 0.16$ $\delta_R/\delta_{MA2}^A = 416,05/387,74 = 1,07$). The most probable value, given the information of balance sheet data is, a PRR of 11% for the manufacturer and a PRR of 7% for the retailer, which leads to a ratios of approximately 1 : 11 ((table (6), $\delta_R/\delta_{MA2}^A = 2912,36/266,57 = 10.93$)) for the comparison of the difference between agreement and disagreement profit, which indicates a favorable bargaining position for the manufacturer. Still, we do not know how the exogenous bargaining power λ is distributed, but we can clearly see that the size of the externality is crucial to identify properly the bargaining framework.

4.2 Robustness Checks

In this section we show several robustness check to account for well known problems related to DiD estimations, such as the biased estimation of standard errors due to autocorrelation (Bertrand et al. 2004). Bertrand et al. (2004) propose clustered bootstrapped standard errors as well as the reduction of the time-series into a before and after treatment group.[20]

We apply those two methods as robustness checks. Table (7) provides the estimates with bootstrapped clustered standard errors. We consider 1000 bootstrap repetitions and cluster along the retail outlet dimension. The coefficients remain the same as in the basic estimation, however, standard errors change. The effect we are interested in is the treatment effect in each case. The weakly significant effect of table (3) in Case A disappears for all revenue measures. This indicates that the effect found in Case A is subject to autocorrelation. However, the effects found significant in table (3) for Case B remain significant in table (7). The negative impact on store revenues with beer (column 1, Treatment B x Group B, Coeff.: -0.0279, SE.: 0.0121) and the store revenues without beer (column 3, Treatment B x Group B, Coeff.: -0.0284, SE.: 0.0124) remain significant at the 5 % level. The standard errors, however, are slightly higher than in the baseline estimation, which indicates that autocorrelation maybe relevant in the baseline specification. Joint estimation of the effects presented in table (8) does not reveal any substantial difference to the estimation of the stand alone cases. The valuations of the treatment effects of estimations in table (7) for the second case remain the same as in table (6) as there have only been corrections of the standard errors. The first case, however, is not significant anymore so that one cannot quantify any effect.

Table (9) now applies the second correction for robustness regarding autocorrelation. Now the observations in the post- and pre-treatment periods are collapsed into two periods for each retail outlet. Analyzing the treatment effect it becomes evident that the size of the effect remains similar, albeit slightly larger in both Cases. As in the standard error correction via bootstrapping (table 7) the treatment effects in Case A disappear for each measure of revenue. Therefore, this result confirms the problem of autocorrelation and leads to the conclusion that treatment effects in Case A are not robust. Therefore, the

[20]However, they also point out that these techniques may suffer from a sample size that is too small, in particular with regard to the cross sectional dimension. Since our sample of retail outlet is not smaller than the typical 50 states dimension in policy analysis, we do not fear this problem

effect decreases to zero. The treatment effects in Case B, however, remain as in the previous specifications. There is a highly significant effect of the treatment effect on the overalls store revenues (Column 1, Treatment B x Group B, Coeff.: -0.0321, SE.: 0.0113), however no effect on the revenues with beer (Column 2, Treatment B x Group B, Coeff.: -0.0106, SE.: 0.0174). Column (3) shows the the externality on other products which is negatively significant (Treatment B x Group B, Coeff.: -0.0325, SE.: 0.0115). A potential limitation of this robustness check is that the R^2 is decreasing to 6-8 % for the store sales. The quantification of the treatment effects is only relevant for the second case since the first is not significant anymore. The results, however, are in line with the previously found effects and do not differ very much (table 10).

Since the results of Case B seem to be robust to autocorrelation and are only slightly changed from the main specification, we try to tackle one additional potential harm of the DiD estimations: unobserved heterogeneity. Therefore, we reestimate our model using fixed effects techniques. the results are shown in table (11). The findings confirm the previously found results. All treatment effects of Case A disappear, while the treatment effects of case B remain significant and hardly differ in absolute terms (Column 1, Treatment B x Group B, Coeff.: -0.0281, SE.: 0.0123, Column (3), Coeff.: -0.0282, SE.: 0.0126). This confirms the findings of the other robustness checks. The joint estimation in table(12) shows the same patterns as in the stand alone cases. Also the quantifications remain (13) consistent to the previously found effects for the second case, while they are not significant in the first.

Summarizing the results of the robustness checks, the main conclusion remains valid for Case B while they disappear for Case A. However, it is important to state that all methods are dependent on sample size. Given that the standard sample in DiD analysis comprises 50 states due to the fact that many applications of DiD use U.S. state level data, we think that the bias should be smaller in our larger sample. Therefore, the boycott of the local beer brand in Case B did have a significant impact on overall store revenues, but not on the stores' beer revenues. In particular, also the store revenues without beer faced a negative impact.

This leads to the interpretation that managers were able to compensate the beer revenues, for instance by special offers and commercials in the beer segment, but different, customers that did not spend as much money as the former local beer buyers in Case B

on other products. Since the externalities identified are rather strong this effect can be best explained by consumer behavior as "one-stop shopping". Clearly, we cannot identified the particular reasons, why people switched, but given the local differentiation of the beer and tendencies towards "one-stop shopping" it is probable that, the loss of valuable "one-stop shoppers" harmed the retailer.

5 Conclusion

The paper revealed important insides in the bargaining process between retailer and manufacturer. It takes advantage of two supply boycotts by strong regional beer brands, our local heroes. We can show that the supply boycotts of the beer brands lead to significant negative effects on the retailers revenues. This is surprising, because the manufacturers are clearly smaller measured in absolute terms of revenues than the retailer. The paper has shown for a particular case that an analysis of this process -for instance in antitrust investigations deriving a theory of harm using bargaining power as an argument- is incomplete without taking into account externalities, or to be more precise complementarities among the product the bargaining is about and the overall retailer's assortment. These externalities may as we have shown, arise from the widely discussed "one-stop-shopping" customer behavior. Therefore, this paper is, to the best of our knowledge,the first quantifying the impact of "one-stop-shopping" externalities. We show that in at least one of two natural experiment cases, externalities are existent and have a considerable large size. Given the information of the particular case, we explain this large effect with the strong loyalty of customers to "their" local beer brand. This local differentiation of beer brands may therefore be an important strategy for small sized firms to increase their brand value. This phenomenon is important in antitrust analysis in at least two regards.

First, antitrust analysis may be biased heavily neglecting the size of those externalities since -everything else equal- this externality is increasing the difference of agreement and disagreement profits of retailer, which is equal to a more favorable bargaining position of the manufacturer. Second, the importance of a size or market share driven analysis as an approximation for the bargaining positions may be overvalued since a differentiation strategy (in this case local differentiation) may be an option also for small manufacturers to leverage their relevance from the product sold to larger parts of the retailers assortment.

In the case of the externalities relevance, neglecting those externalities may therefore systematically underestimate the manufacturers bargaining position. This, however, does not mean that size is not important anymore, but that a bargaining position has to be analyzed more cautious. On the one hand, this finding makes general statements of bargaining power more difficult, since it suggest a case by case analysis, but on the other hand it may allow for a more differentiated analysis.

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A Appendix

Table 1: Descriptive Statistics

Variable	Obs.	Mean	Std. Dev.	Min	Max
Store Revenue	7,672	1,555,512	650,114.40	139,977.10	4,548,499
Beer Revenue	1,672	42,946.85	22,900.01	2,214.67	210,018.60
Beer Revenue Brand A	129	335.42	423.13	0.58	2,005.32
Beer Revenue Brand B	1,306	2,201.13	3,337.04	0.54	18,760.80

Table 2: Stores and Treatments

Treatment Status	Treatment Brand A	Treatment Brand B
Store Number	3, 7, 12, 15, 37, 49, 54, 56, 60	2-22, 24-29, 31-33, 35-36, 39, 42-61
Treatment Status	Both Treatments	No Treatment
Store Number	3, 7, 12, 15, 49, 54, 56, 60	1, 23, 30, 34, 38, 40, 41

Table 3: **Baseline DiD Estimates.**

CASE A			
	Store Revenues	*Store Beer Revenues*	*Store Revenues w/o Beer*
	(1)	(2)	(3)
Treatment A	0.3023***	0.2928***	0.3026***
	(0.0213)	(0.0237)	(0.0214)
Group A	0.0121**	0.0203***	0.0117**
	(0.0050)	(0.0065)	(0.0051)
Treatment A x Group A	-0.0400*	-0.0351	-0.0401*
	(0.0211)	(0.0219)	(0.0212)
Constant	0.9679***	0.8429***	0.9715***
	(0.0092)	(0.0123)	(0.0093)
R^2	0.7449	0.8113	0.7492
Observations	1288	1288	1288
CASE B			
	Store Revenues	*Store Beer Revenues*	*Store Revenues w/o Beer*
	(1)	(2)	(3)
Treatment B	0.0881***	0.1921***	0.0850***
	(0.0128)	(0.0193)	(0.0129)
Group B	0.0036	0.0004	0.0036
	(0.0039)	(0.0046)	(0.0040)
Treatment B x Group B	-0.0279***	-0.0028	-0.0284***
	(0.0079)	(0.0126)	(0.0079)
Constant	0.9665***	0.8453***	0.9700***
	(0.0099)	(0.0127)	(0.0100)
R^2	0.7275	0.8003	0.7316
Observations	1503	1503	1503

Hetroskedacity robust standard errors in parenthesis.

***, **, * statistically significant on the 1, 5, and 10% level.

Table 4: **Joint Estimates of Case A and Case B.**

	Store Revenues	*Store Beer Revenues*	*Store Revenues w/o Beer*
	(1)	(2)	(3)
Treatment A	0.3011***	0.2952***	0.3013***
	(0.0221)	(0.0242)	(0.0222)
Group A	0.0121**	0.0203***	0.0117**
	(0.0050)	(0.0065)	(0.0051)
Treatment A x Group A	-0.0323**	-0.0517***	-0.0315**
	(0.0150)	(0.0169)	(0.0150)
Treatment B	-0.2087***	-0.0962***	-0.2121***
	(0.0228)	(0.0263)	(0.0229)
Group B	0.0036	0.0005	0.0037
	(0.0039)	(0.0047)	(0.0039)
Treatment B x Group B	-0.0284***	-0.0035	-0.0289***
	(0.0080)	(0.0129)	(0.0081)
Constant	0.9648***	0.8425***	0.9683***
	(0.0098)	(0.0127)	(0.0099)
R^2	0.7303	0.8028	0.7342
Observations	1503	1503	1503

Clustered Bootstrapped standard errors in parenthesis. 1000 Replications. Clustering on outlets

***, **, * statistically significant on the 1, 5, and 10% level.

Table 5: Profits Case A

	Profits Retailer									
	In Euros per month and outlet, for profit to revenue ratios (PRR)									
	0.01	**0.02**	**0.03**	**0.04**	**0.05**	**0.06**	**0.07**	**0.08**	**0.09**	**0.10**
δ_R	595,08	1190,16	1785,24	2380,32	2975,40	3570,48	4165,56	4760,64	5355,72	5965,72
	(237,05)	(474,10)	(711,15)	(948,20)	(1185,24)	(1422,29)	(1659,34)	(1896,39)	(2133,44)	(2370,49)
π_R^A	407,44	814,87	1222,31	1629,74	2037,18	2444,61	2852,05	3259,48	3666,92	4074,35
	(182,60)	(365,20)	(547,80)	(730,40)	(913,00)	(1095,60)	(1278,20)	(1460,80)	(1643,39)	(1825,99)
π_R^{DA}	402,28	804,56	1206,84	1609,12	2011,41	2413,69	2815,97	3218,25	3620,53	4022,81
	(184,25)	(368,49)	(552,74)	(736,98)	(921,23)	(1105,48)	(1289,72)	(1473,97)	(1658,21)	(1842,46)
E_A	596,57	1193,14	1789,72	2386,29	2982,86	3579,43	4176,00	4772,58	5369,15	5965,72
	(240,18)	(480,35)	(720,53)	(960,70)	(1200,88)	(1441,05)	(1681,23)	(1921,40)	(2161,58)	(2401,75)
	Profits Manufacturer A									
	In Euros per month and outlet, for profit to revenue ratios (PRR)									
	0.07	**0.08**	**0.09**	**0.10**	**0.11**	**0.12**	**0.13**	**0.14**	**0.15**	**0.16**
$\delta_{MA}^A = \pi_{MA}^A$	23,25	26,57	29,89	33,22	36,54	39,86	43,18	46,50	49,82	53,14
	(31,44)	(35,93)	(40,42)	(44,91)	(49,41)	(53,90)	(58,39)	(62,88)	(67,37)	(71,86)
π_{MA}^{DA}	0	0	0	0	0	0	0	0	0	0

Standard Errors are in parenthesis.

Table 6: Profits Case B

	Profits Retailer									
	In Euros per month and outlet, for profit to revenue ratios (PRR)									
	0.01	**0.02**	**0.03**	**0.04**	**0.05**	**0.06**	**0.07**	**0.08**	**0.09**	**0.10**
δ_R	416,05	832,10	1248,15	1664,21	2080,26	2496,31	2912,36	3328,41	3744,46	4160,51
	(163,55)	(327,10)	(490,66)	(654,21)	(817,76)	(981,32)	(1144,87)	(1308,42)	(1471,97)	(1635,52)
π_R^A	417,47	834,95	1252,41	1669,89	2087,36	2504,84	2922,31	3339,78	3757,26	4174,73
	(184,95)	(369,90)	(554,85)	(739,80)	(924,75)	(1109,71)	(1294,66)	(1479,61)	(1664,56)	(1849,51)
π_R^{DA}	407,67	815,33	1223,00	1630,66	2038,33	2446,00	2853,66	3261,33	3668,99	4076,66
	(187,23)	(374,46)	(561,70)	(748,93)	(936,16)	(1123,39)	(1310,62)	(1497,86)	(1685,09)	(1872,32)
E_A	423,25	846,50	1269,75	1693,01	2116,26	2539,51	2962,76	3386,01	3809,26	4232,51
	(170,40)	(340,80)	(511,19)	(681,59)	(851,99)	(1022,39)	(1192,79)	(1363,18)	(1533,58)	(1703,98)
	Profits Manufacturer A									
	0.07	**0.08**	**0.09**	**0.10**	**0.11**	**0.12**	**0.13**	**0.14**	**0.15**	**0.16**
	In Euros per month and outlet, for profit to revenue ratios (PRR)									
$\delta_{MAB}^A = \pi_{MAB}^A$	169,63	193,87	218,10	242,33	266,57	290,80	315,03	339,27	363,50	387,74
	(244,45)	(279,38)	(314,30)	(349,22)	(384,14)	(419,06)	(453,99)	(488,91)	(523,83)	(558,75)
π_{MAB}^{DA}	0	0	0	0	0	0	0	0	0	0

Standard Errors are in parenthesis.

Table 7: **DiD Estimates, with Bootstrapped Standard Errors.**

CASE A			
	Store Revenues	*Store Beer Revenues*	*Store Revenues w/o Beer*
	(1)	(2)	(3)
Treatment A	0.3023***	0.2928***	0.3026***
	(0.0199)	(0.0189)	(0.0193)
Group A	0.0121	0.0203*	0.0117
	(0.0131)	(0.0123)	(0.0123)
Treatment A x Group A	-0.0400	-0.0351	-0.0401
	(0.0370)	(0.0425)	(0.0343)
Constant	0.9679***	0.8429***	0.9715***
	(0.0088)	(0.0126)	(0.0089)
R^2	0.7449	0.8113	0.7492
Observations	1288	1288	1288
CASE B			
	Store Revenues	*Store Beer Revenues*	*Store Revenues w/o Beer*
	(1)	(2)	(3)
Treatment B	0.0881***	0.1921***	0.0850***
	(0.0148)	(0.0224)	(0.0152)
Group B	0.0036**	0.0004	0.0036**
	(0.0018)	(0.0027)	(0.0018)
Treatment B x Group B	-0.0279**	-0.0028	-0.0284**
	(0.0121)	(0.0183)	(0.0124)
Constant	0.9665***	0.8453***	0.9700***
	(0.0101)	(0.0122)	(0.0098)
R^2	0.7275	0.8003	0.7316
Observations	1503	1503	1503

Clustered Bootstrapped standard errors in parenthesis. 1000 Replications. Clustering on outlets

***, **, * statistically significant on the 1, 5, and 10% level.

Table 8: **DiD Estimates, with Bootstrapped Standard Errors. Joint Estimates of Case A and Case B.**

	Store Revenues	*Store Beer Revenues*	*Store Revenues w/o Beer*
	(1)	(2)	(3)
Treatment A	0.3011***	0.2952***	0.3013***
	(0.0216)	(0.0188)	(0.0214)
Group A	0.0121	0.0203*	0.0117
	(0.0131)	(0.0115)	(0.0129)
Treatment A x Group A	-0.0323	-0.0517	-0.0315
	(0.0321)	(0.0351)	(0.0318)
Treatment B	-0.2087***	-0.0962***	-0.2121***
	(0.0246)	(0.0270)	(0.0239)
Group B	0.0036*	0.0005	0.0037*
	(0.0020)	(0.0028)	(0.0021)
Treatment B x Group B	-0.0284**	-0.0035	-0.0289**
	(0.0135)	(0.0194)	(0.0143)
Constant	0.9648***	0.8425***	0.9683***
	(0.0099)	(0.0122)	(0.0104)
R^2	0.7303	0.8028	0.7342
Observations	1503	1503	1503

Clustered Bootstrapped standard errors in parenthesis. 1000 Replications. Clustering on outlets

***, **, * statistically significant on the 1, 5, and 10% level.

Table 9: **DiD Estimates, Collapsed Before and After Treatment**

CASE A			
	Store Revenues	*Store Beer Revenues*	*Store Revenues w/o Beer*
	(1)	(2)	(3)
Treatment A	0.0478***	0.1042***	0.0463***
	(0.0068)	(0.0096)	(0.0068)
Group A	0.0097	0.0102	0.0095
	(0.0128)	(0.0187)	(0.0128)
Treatment A x Group A	-0.0409	-0.0296	-0.0411
	(0.0267)	(0.0352)	(0.0266)
Constant	0.9841***	0.9827***	0.9842***
	(0.0033)	(0.0039)	(0.0033)
R^2	0.2826	0.5072	0.2705
Observations	109	109	109
CASE B			
	Store Revenues	*Store Beer Revenues*	*Store Revenues w/o Beer*
	(1)	(2)	(3)
Treatment B	0.0418***	-0.0884***	0.0454***
	(0.0080)	(0.0120)	(0.0083)
Group B	0.0074*	0.0068	0.0074*
	(0.0040)	(0.0072)	(0.0040)
Treatment B x Group B	-0.0321***	-0.0106	-0.0325***
	(0.0113)	(0.0174)	(0.0115)
Constant	0.9940***	1.0126***	0.9935***
	(0.0013)	(0.0020)	(0.0014)
R^2	0.0627	0.4298	0.0793
Observations	109	109	109

Heteroskedasticity-robust standard errors in parenthesis.

***, **, * statistically significant on the 1, 5, and 10% level.

Table 10: Profits Case B Before and After Treatment

	Profits Retailer									
	In Euros per month and outlet, for profit to revenue ratios (PRR)									
	0.01	**0.02**	**0.03**	**0.04**	**0.05**	**0.06**	**0.07**	**0.08**	**0.09**	**0.10**
δ_R	474,59	949,18	1423,77	1898,36	2372,95	2847,54	3322,12	3796,71	4271,30	4745,89
	(181,65)	(363,30)	(544,95)	(726,60)	(908,25)	(1089,90)	(1271,54)	(1453,19)	(1634,84)	(1816,49)
π_R^A	412,77	825,54	1238,31	1651,09	2063,86	2476,63	2889,4	3302,17	3714,94	4127,71
	(170,10)	(340,21)	(510,31)	(680,42)	(850,52)	(1020,63)	(1190,73)	(1360,84)	(1530,94)	(1701,05)
π_R^{DA}	399,37	798,75	1198,12	1597,49	1996,87	2396,24	2795,61	3194,98	3594,36	3993,73
	(171,56)	(343,12)	(514,68)	(686,24)	(857,80)	(1029,37)	(1200,93)	(1372,49)	(1544,05)	(1715,61)
E_A	485,49	970,98	1456,46	1941,96	2427,44	2912,93	3398,41	3883,90	4369,39	4854,88
	(190,70)	(381,41)	(572,11)	(762,81)	(953,52)	(1144,22)	(1334,93)	(1525,63)	(1716,33)	(1907,04)
	Profits Manufacturer B									
0.07	**0.08**	**0.09**	**0.10**	**0.11**	**0.12**	**0.13**	**0.14**	**0.15**	**0.16**	
	In Euros per month and outlet, for profit to revenue ratios (PRR)									
$\delta_{MAB}^A = \pi_{MAB}^A$	172,31	196,92	221,54	246,16	270,77	295,39	320,00	344,62	369,23	393,85
	(245,82)	(280,94)	(316,05)	(351,17)	(386,29)	(421,41)	(456,52)	(491,64)	(526,76)	(561,88)
π_{MAB}^{DA}	0	0	0	0	0	0	0	0	0	0

Standard Errors are in parenthesis. Values comprise average values per month and outlet.

Table 11: **Fixed Effect Regression.**

CASE A			
	Store Revenues	*Store Beer Revenues*	*Store Revenues w/o Beer*
	(1)	(2)	(3)
Treatment A	0.0524***	0.4698***	0.0406**
(1 lag)	(0.0162)	(0.0244)	(0.0162)
Treatment A x Group A)	-0.0432	-0.0359	-0.0432
(1 lag)	(0.0415)	(0.0440)	(0.0415)
Constant	0.8991***	0.7656***	0.9029***
	(0.0060)	(0.0067)	(0.0061)
R^2	0.7646	0.8094	0.7694
Observations	1182	1182	1182
CASE B			
	Store Revenues	*Store Beer Revenues*	*Store Revenues w/o Beer*
	(1)	(2)	(3)
Treatment B	0.2105***	0.2522***	0.2091***
(1 lag)	(0.0123)	(0.0161)	(0.0127)
Treatment B x Group B	-0.0281**	-0.0179	-0.0282**
(1 lag)	(0.0123)	(0.0203)	(0.0126)
Constant	0.8990***	0.7655***	0.9028***
	(0.0061)	(0.0070)	(0.0062)
R^2	0.7458	0.7964	0.7505
Observations	1343	1343	1343

Heteroskedasticity-robust standard errors in parenthesis. Fixed Effects on outlet level.

***, **, * statistically significant on the 1, 5, and 10% level.

Table 12: **Fixed Effect Regression. Joint Estimates of Case A and Case B.**

	Store Revenues	*Store Beer Revenues*	*Store Revenues w/o Beer*
	(1)	(2)	(3)
Treatment A (1 lag)	0.1286***	0.1992***	0.1265***
	(0.0106)	(0.0116)	(0.0108)
Treatment A x Group A (1 lag)	-0.0361	-0.0567	-0.0352
	(0.0355)	(0.0381)	(0.0356)
Treatment B (1 lag)	0.0870***	0.0611***	0.0876***
	(0.0104)	(0.0184)	(0.0106)
Treatment B x Group B (1 lag)	-0.0287**	-0.0188	-0.0288**
	(0.0133)	(0.0209)	(0.0135)
Constant	0.8992***	0.7658***	0.9030***
	(0.0060)	(0.0069)	(0.0062)
R^2	0.7489	0.7992	0.7534
Observations	1343	1343	1343

Robust standard errors in parenthesis.

***, **, * statistically significant on the 1, 5, and 10% level.

Table 13: Profits Case B Fixed Effects Regression

	Profits Retailer									
	0.01	**0.02**	**0.03**	**0.04**	**0.05**	**0.06**	**0.07**	**0.08**	**0.09**	**0.10**
	In Euros per month and outlet, for profit to revenue ratios (PRR)									
δ_R	418,74	837,48	1256,22	1674,95	2093,69	2512,43	2931,17	3349,91	3768,65	4187,39
	(164,61)	(329,22)	(493,83)	(658,44)	(823,04)	(987,65)	(1152,26)	(1316,87)	(1481,48)	(1646,09)
π_R^A	417,47	834,95	1252,42	1669,89	2087,36	2504,84	2922,31	3339,78	3757,26	4174,73
	(184,95)	(369,90)	(554,85)	(739,80)	(924,75)	(1109,71)	(1294,67)	(1479,61)	(1664,56)	(1849,51)
π_R^{DA}	401,47	802,95	1204,42	1605,895	2007,37	2408,84	2810,32	3211,79	3613,26	4014,74
	(184,39)	(368,78)	(553,16)	(737,55)	(921,94)	(1106,33)	(1290,72)	(1475,10)	(1659,49)	(1843,88)
E_A	420,07	840,13	1260,20	1680,26	2100,33	2520,40	2940,46	3360,53	3780,59	4200,66
	(169,12)	(338,23)	(507,35)	(676,46)	(845,58)	(1014,69)	(1183,81)	(1352,92)	(1522,04)	(1691,16)
	Profits Manufacturer B									
	0.07	**0.08**	**0.09**	**0.10**	**0.11**	**0.12**	**0.13**	**0.14**	**0.15**	**0.16**
	In Euros per month and outlet, for profit to revenue ratios (PRR)									
$\delta_{MAB}^A = \pi_{MAB}^A$	169,63	193,87	218,10	242,33	266,57	290,80	315,03	339,27	363,50	387,74
	(244,45)	(279,38)	(314,30)	(349,22)	(384,14)	(419,06)	(453,99)	(488,91)	(523,83)	(558,75)
π_{MAB}^{DA}	0	0	0	0	0	0	0	0	0	0

Standard Errors are in parenthesis. Values comprise average values per month and outlet.