Aguzzoni, Luca; Argentesi, Elena; Ciari, Lorenzo; Duso, Tomaso; Tognoni, Massimo

Working Paper

Ex-post Merger Evaluation in the UK Retail Market for Books

Quaderni - Working Paper DSE, No. 889

Provided in Cooperation with:
University of Bologna, Department of Economics

Suggested Citation: Aguzzoni, Luca; Argentesi, Elena; Ciari, Lorenzo; Duso, Tomaso; Tognoni, Massimo (2013) : Ex-post Merger Evaluation in the UK Retail Market for Books, Quaderni - Working Paper DSE, No. 889, Alma Mater Studiorum - Università di Bologna, Dipartimento di Scienze Economiche (DSE), Bologna, https://doi.org/10.6092/unibo/amsacta/3728

This Version is available at:
https://hdl.handle.net/10419/159728

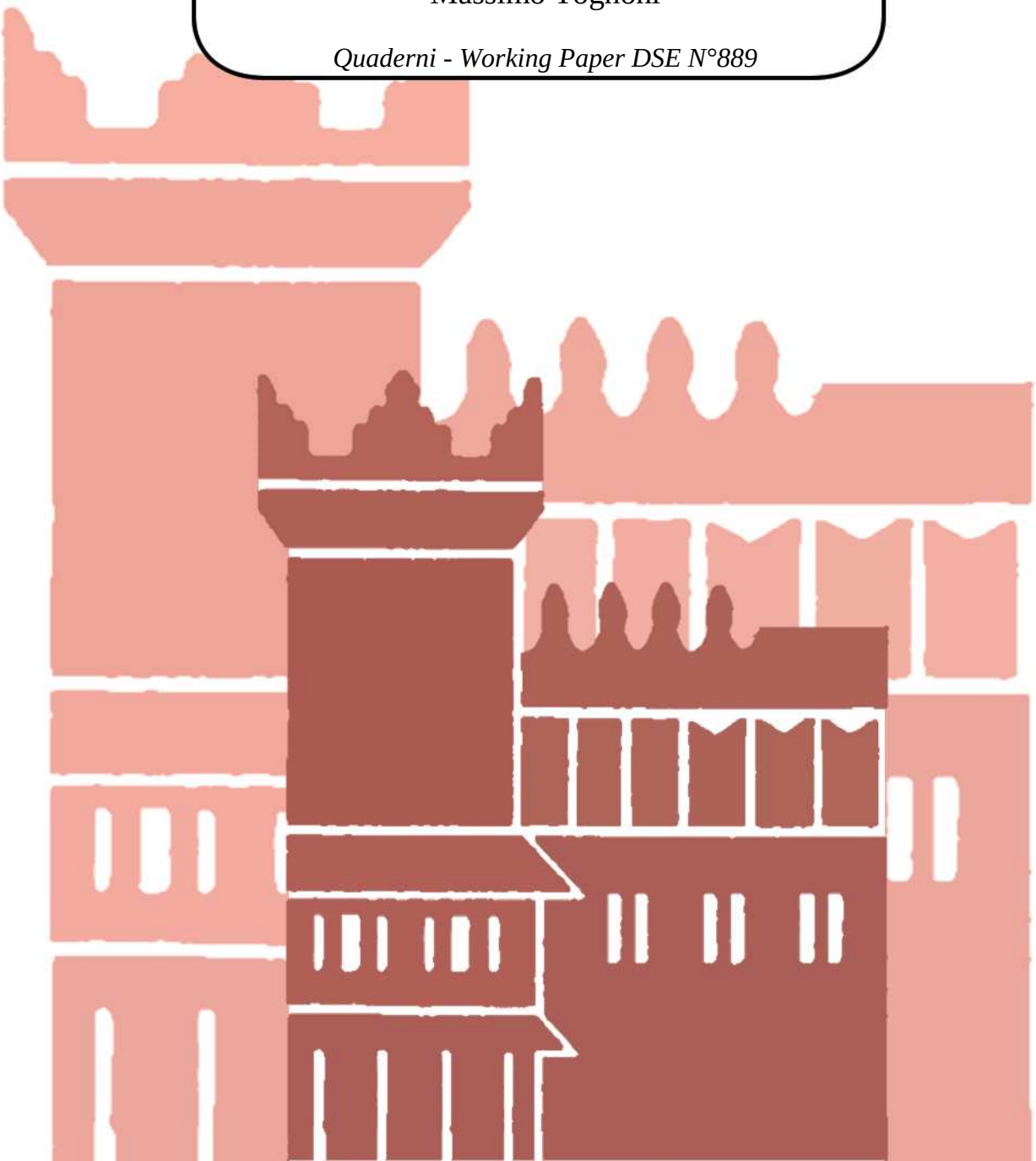
ISSN 2282-6483

Alma Mater Studiorum - Università di Bologna
DEPARTMENT OF ECONOMICS

Ex-post Merger Evaluation in the UK Retail Market for Books

Luca Aguzzoni
Elena Argentesi
Lorenzo Ciari
Tomaso Duso
Massimo Tognoni

Quaderni - Working Paper DSE N°889



Ex-post Merger Evaluation in the UK Retail Market for Books

Luca Aguzzoni
LEAR

Elena Argentesi*
University of Bologna

Lorenzo Ciari
European Bank for Reconstruction and Development

Tomaso Duso
Deutsches Institut für Wirtschaftsforschung (DIW Berlin) and
Düsseldorf Institute for Competition Economics (DICE)

Massimo Tognoni
UK Competition Commission

June 14, 2013

Abstract: This paper empirically evaluates the price effects of the merger of two major book retail chains in the UK: Waterstone's and Ottakar's. We employ differences-in-differences techniques and use a rich dataset containing monthly scanner data information on a sample of 200 books sold in 60 stores in 50 different local markets for a period of four years around the merger. Since retail mergers may have either local or national effects (or both) according to the level at which retail chains set prices, we undertake an ex-post assessment of the impact of the merger at both levels. At the local level, we compare the changes in the average price charged before and after the merger in the shops located in overlap areas –i.e. areas where both chains were present before the merger– and in non-overlap areas –i.e. areas where only one chain was present before the merger. At the national level, we employ two distinct control groups to evaluate the merger, namely the competitors and the top-selling titles. We find that the merger did not result in an increase in prices either at the local or at the national level. We also perform heterogeneous treatment effects estimations in order to assess whether the effect of the merger differs along various dimensions of heterogeneity that are present in our data.

Keywords: Mergers, Ex-post Evaluation, Book market, Retail sector
JEL Classification: K21, L24, L44, D22, O32

∗ Corresponding author: Elena Argentesi, Department of Economics, University of Bologna, Piazza Scaravilli 2, 40126 Bologna, Italy, Tel: + 39 051 2098661, Fax: +39 051 2098040, E-Mail: elena.argentesi@unibo.it. This paper is partially based on a research project we undertook for the UK Competition Commission (CC). Paolo Buccirossi and Cristiana Vitale were coauthors of that report and offered continuous guidance for this paper; therefore we extend our sincere gratitude for their council and contributions. We are also grateful to Luca Barbarito, Peter Haan, Cristian Huse, Adam Lederer, Chiara Monfardini, Massimo Motta, Amrita Ray Chaudhuri, and Daniel Rubinfeld for very useful suggestions. We gratefully acknowledge comments from seminar participants at the 2012 CRESSE Conference, Jornadas de Economia Industrial 2012, SIE Conference, DIW Berlin, Time Series and Competition Policy Workshop (Stellenbosch University), University of Padua, and University of Bologna. We also wish to thank the CC's staff for the support provided during the course of this study, and Carlotta Dandolo, Giulio Altomari, Roberto Alimonti and Roberto Cervone for their excellent research assistance.

1. Introduction

There is a growing interest in retrospective merger studies, which comes principally from the need to understand how mergers alter market structure and welfare. Yet, additional motivation behind this increased interest lies in the perceived need for antitrust agencies to check and improve the effectiveness of their decision making (e.g. Kovacic, 2009). Moreover, from an academic perspective, retrospective merger evaluations are seen as useful tools to validate structural models for merger simulation, which are increasingly used as an ex-ante instrument to assess policy changes.[1]

Despite a growing number of studies that analyze the price effect of mergers in a variety of industries, little work exists on the ex-post evaluation of mergers in the retailing sector.[2] This is particularly surprising since not only do retail markets constitute a significant part of the economy in terms of value added and employment in all developed countries but these markets are experiencing a great deal of merger activity. Moreover, as noted by Hosken et al. (2012), mergers in retailing are often subject to antitrust scrutiny. For instance, out of 176 grocery markets subject to merger control by the US Federal Trade Commission between 1996 and 2011, 152 were challenged.[3] Similarly, the European Commission (EC) reviewed an increasing number of retailing mergers over the past two decades. Out of the 167 mergers in retail trade analyzed by the EC between 1990 and 2008, 13 were denied or approved with specific conditions and obligations.

Mergers in retailing sectors present some specific features that differentiate them from concentrations in other markets and that should be considered in the merger review process as well as in any retrospective study.[4] In particular, they are characterized by dispersed buyers and sellers (Davis, 2006): Consumers tend to make their purchases within their local shopping location and retail businesses generally have multiple outlets across a country. Yet, retail offers may be set either nationally or locally, which creates an interplay between local and

[1] See Nevo and Whinston (2010) and Weinberg (2011). For a recent contribution which uses ex-post merger analysis to validate a simulation model, see Björnerstedt and Verboven (2012).

[2] Among these very few papers, Hastings (2004) analyzes the effects of a merger in the gasoline retail market in the US. Skrainka (2012) studies instead the effects of the merger between two UK grocery retailers using consumer data. Ashenfelter, Hosken and Weiberg (2009) and Duso (2012) present a review of the recent literature on ex-post merger studies.

[3] See Horizontal Merger Investigation Data, Fiscal years 1996 – 2011, Federal Trade Commission Table 4.2. Available at: http://www.ftc.gov/os/2013/01/130104horizontalmergerreport.pdf

[4] Indeed, the opening passage of the recent joint UK Competition Commission/Office of Fair Trading (OFT) report on retail mergers (Competition Commission and Office of Fair Trading, 2011; p. 4) reads: "Retail mergers account for a significant number of cases that come before the Office of Fair Trading and the Competition Commission.... Moreover, some of the questions that such mergers raise are largely specific to the sector."

national competition. This aspect is particularly relevant for merger assessment, since it implies that any concentration may impact competition at different geographical levels. This feature should therefore be taken into account in the design of any retrospective evaluation exercise in these industries.[5]

The focus of the few existing academic papers and most of the policy studies on retailing industries is on mergers in the food sector. However, other retail markets are also important. One example is the market for books, which shares several peculiar features with the markets for creative goods such as music records, movies, video games, and software. Given the unpredictable and usually short lasting popularity, books are generally characterized by a very short life cycle (like fashion products) as well as by uncertain demand and short periods of high profitability.[6] Moreover, they can be considered to be experience goods that are not typically purchased more than once. The fact that books are short-lived and have largely volatile sales poses particular challenges for the analysis of the effect of competition on prices in this industry. The same book title may have a different value to consumers in different periods of time and, therefore, retailers' pricing policy may change accordingly. Moreover, publishers have a rapidly changing portfolio of books. Therefore, for ex-post merger analysis, it would be incorrect to identify the effect of the merger by simply looking at the price evolution of a constant sample of products.

The above considerations suggest the need of expanding the existing evaluation methodologies and the importance of providing new empirical evidence to improve our understanding of competition in retailing sectors and, specifically, cultural goods markets. This study tries to address these issues by analyzing the effect of a consummated merger in the retail book sector. Waterstone's acquisition of Ottakar's –two of the major book retailers in the UK at that time– was announced in 2005 and cleared by the UK Competition Commission (CC) in 2006. In its investigation, the CC identified problematic local markets where both the merging parties competed before the merger (overlap areas) and where the concentration was expected to have the potential for significant anti-competitive effects.

Hence, to identify the causal effect of this concentration, we compare the price evolution of the products sold by the merging parties in the overlap areas with the price

[5] Other peculiarities of retail industries are considered in previous studies. One such peculiarity is the presence of vertical structures with relevant buyer power (Inderst and Shaffer, 2007). This aspect is analyzed by Pita Barros et al. (2006), who empirically investigate the effect of a merger on the bargaining power of retailers vis-à-vis producers in the food retailing sector. Davis (2005) studies the impact of local competition on prices in the movie theater market.

[6] See Canoy et al. (2006) and Beck (2007) for in-depth descriptions of the features of the book industry.

evolution of their products in areas where only one chain was present pre-merger (non-overlap areas) by employing a differences-in-differences (DiD) approach.[7] We quantify the price effects of the merger by using a rich dataset containing scanner data information on a sample of 200 books, sold in 60 stores, in 50 different local markets over a period of four years around the merger (2004-2007). Because of the peculiarities of the market for books discussed above, we look at the effect of the merger on the prices of a selected sample of titles that varies year by year instead of comparing the same set of titles before and after the merger in a fixed-effect framework. Thus, the price of different books is modeled by means of a hedonic approach as a function of the products' characteristics (e.g. Pakes, 2003).[8] This enables us to more correctly identify the effect of a policy change –i.e. the merger decision– on the price level, since any price difference due to the changes in the products' characteristics is accounted for in the regression.

As outlined in a joint report by the CC and the Office of Fair Trading (Competition Commission and Office of Fair Trading, 2011), it is often difficult to find evidence of local effects of mergers because of data limitations. On this issue, our case study has a major advantage with respect to previous studies since we have accurate data at different levels of aggregation, which allow us to assess both the local and the national effect of the concentration. Moreover, the peculiar structure of our database helps us to alleviate the main potential shortcoming of the DiD methodology: the choice of the appropriate control group.[9] As discussed by Nevo and Whinston (2010), finding a suitable control group to estimate the causal effect of a merger on prices is difficult in many industries. In this respect, the features of the retailing sector make it a good field of application, since one can exploit the variation in the competitive conditions across local markets (Hastings, 2004 and Choné and Linnemer,

[7] Other papers pursing the DiD methodology in merger analysis are Focarelli and Panetta (2003); Hastings (2004); Chandra and Collard-Wexler (2009); Ashenfelter and Hosken (2011); Ashenfelter, Hosken and Weinberg (2013); Choné and Linnemer (2012). See Weinberg (2008) and Hunter, Leonard and Olley (2008) for a survey. A growing strand of literature instead follows a structural approach; see Friberg and Romahn (2012), Skrainka (2012), and Björnerstedt and Verboven (2012).

[8] A similar problem is faced by Ashenfelter et al. (2013), who analyze the price effects of a merger between two appliance manufacturers in the US. Dealing with products with short lifetimes, they also use a model with product characteristics to account for product quality. Unlike them, however, we explicitly build a post-merger sample of titles that reflects the distribution of the observable characteristics in the entire population of titles. Our methodological framework could be applied to other mergers in this industry, or in related industries sharing the same features. Indeed, we employ a similar methodology in the assessment of the effects of another merger between two retailers of videogames (GAME Group plc and Game Station) in the UK (Aguzzoni et al., 2011).

[9] As Friberg and Romahn (2012) point out, another challenge in the application of such methodology is the difficulty of properly identifying before- and after-merger periods. In our setting, however, the definition of the timing of the merger does not pose particular problems, especially because the merger was cleared without any remedies.

2012). More specifically, we make an accurate choice of counterfactual areas by using a Propensity Score Matching (PSM) methodology, which allows us to select non-overlap areas that closely match overlap areas in terms of observable demand and supply characteristics.

In addition to analyzing the effects of the merger on local pricing, the structure of our dataset allows us to also investigate the country-wide effect of the merger on prices. The national analysis provides important complementary evidence on the effect of the merger by using a similar empirical framework as for the local analysis, but with a different level of data aggregation and with different counterfactuals. In particular, we estimate the impact of the merger on the aggregate national price of the same selection of titles relying on two different control groups: i) the same titles sold by the competitors and ii) a sample of the most sold books in a given year, which are expected to be less affected by the merger given the greater degree of competition they face from other retailers, including supermarkets and the internet.[10]

At the local level, the results of our average treatment effect analysis show that there is no significant difference in prices after the merger between non-overlap and overlap areas where the merger should have been reasonably expected to generate the strongest effect. Yet, to more precisely identify the impact of the merger on the price of differentiated products, we further estimate heterogeneous treatment effects by exploiting the richness of our data along different dimensions: firm-specific, book-specific, and market-specific. First, the analysis of firm-specific variables suggests that there was a price convergence among the merging parties in overlap areas after the merger for most book categories. Hence, despite the overall effect of the merger on prices is not significant, we find some evidence that Waterstone's stores increased prices while Ottakar's stores reduced them in local overlap areas. Second, we perform book-specific heterogeneous treatment effects in order to investigate whether the merger influences products existing prior to the merger differently from products introduced after the merger as suggested by Ashenfelter et al. (2013). In particular, we look at the timing of discounts retailers set over the prices recommended by the publishers on books released post-merger. Finally, market-specific variables that should capture the intensity of local competition do not seem to play a significant role in explaining the causal effect of the merger.

[10] While the former set of control groups is used in earlier literature, using products exposed to a different degree of competition as a control group is a methodological innovation of our approach.

At the national level, by using two different control groups, we do not find any significant price increases due to the merger. The dynamics of prices in this industry seem instead to be driven mainly by structural changes and, in particular, by the rapid growth of low-cost retailers, such as online bookstores and supermarkets, and the merger does not appear to have had any adverse impact on price competition. Therefore, the CC's decision to approve the merger seems to have been appropriate as long as the price effect is concerned.

The paper is structured as follows. In the next Section we discuss the institutional setting and, in particular, the characteristics of the book industry and the merger. Section 3 contains a description of our dataset. We then present our main empirical analysis in Section 4. We present the analysis on the effect of the merger at the national level in Section 5. Section 6 concludes.

2. The Book Industry and the Merger

The supply chain of the book industry is characterized by three groups of players: publishers, wholesalers, and retailers. Publishers lie at the top of the value chain, working with authors and producing books. Although the UK has over 10,000 publishers, in 2005 the ten largest groups represented more than half of total consumer sales, both by value and volume. Wholesalers are the bridge between publishers and retailers, as they non-exclusively purchase from the former and sell to the latter. They mainly supply independent bookshops (i.e. retailers with up to five outlets), although they also supply internet shops and other retailers.

Retailers can be broadly classified into four groups: 1) retailers specialized in the sales of books, as well as small independent bookshops; 2) non-specialist retailers for which books are an important category; 3) retailers for which books are part of a wide range of goods, such as supermarkets and major multiples; and 4) online book retailers. These categories differ in the range of titles they hold: specialist shops and online retailers offer a large selection, whilst supermarkets and major multiples hold fewer titles, mainly the market's current best-sellers.

In the UK retail book market, as well as in other countries, pricing takes the form of setting the level of the discount off the recommended retail price (RRP), which is usually printed on the book by the publishers and acts as a ceiling for the retail price.[11] Publishers generally set the RRPs according to estimates on what the market would bear (taking into

[11] Davies et al. (2004) and an OFT report (Office of Fair Trading, 2008) prepared by the Centre of Competition Policy at the University of East Anglia also provide in-depth overviews of pricing policies and regulations in the UK book market.

account the expected discounts offered by retailers) and to cost-related demand shifters (type of binding, presence of colored images, etc.).[12] In the UK market, discounts differ by title categories, which are defined on the basis of the sales' ranking. In particular, discounts are generally larger for bestsellers –i.e. the top 5,000 titles sold in one year– than for deep-range titles –i.e. the remaining titles.[13] The prices offered by retailers depend, to some extent, on the discounts they are able to negotiate upstream. In general, independent bookshops receive the smallest discounts, while supermarkets and book clubs negotiate the largest. The structure of discounts comprises a standard discount, typically over the entire publisher's range, and a promotional discount for some specific titles. Price-promoted books are generally prominently displayed by retailers. Nonetheless, there are other activities to attract consumers, including book reviews, bestseller lists in print media, direct advertising to consumers, as well as publicity events (e.g. book signings and author readings).[14]

We analyze the UK book industry around the time when the merger between two of the major book retailers (Waterstone's and Ottakar's) took place. Table 1 reports the national market shares in 2005, the year when the merger was announced. The main trends, up to 2005, has been a sharp growth in the market share of supermarkets and online retailers (both increased by 4% between 2001 and 2005), and a decrease in the share of non-internet distance sellers (principally book clubs).[15]

[Insert Table 1 here]

With regard to the degree of concentration, at the time of the merger the combined share of the merging parties was 24%. The shares of the four largest retailers (i.e. WHSmith, Waterstone's, Ottakar's and Borders) summed up to 45%; 55% if only deep-range books are considered.

[12] Clerides (2002) provides evidence of the fact that book prices seem to depend more on cost-related demand shifters than on pure demand shifters (new editions, author's previous publications). Beck (2004) empirically analyzes the role of resale price maintenance in the book industry.

[13] The cut-off of 5,000 was identified by the CC as the threshold separating these two categories, because it appeared to be the point at which discounting began to level off (Cfr. "HMV Group plc and Ottakar's plc Proposed acquisition of Ottakar's plc by HMV Group plc through Waterstone's Booksellers Ltd", 12th of May 2006, p. 14).

[14] See Sorensen (2007) for an assessment of the impact of bestseller lists on book sales.

[15] Anecdotal evidence from a survey that we ran on market participants suggests that there was a growing competitive pressure from online retailers and from supermarkets during the years under examination. This seems to be due to their aggressive discounts policy. For more details see Aguzzoni et al. (2011).

2.1 The Merger

In August 2005 two of the major book retailers, Waterstone's and Ottakar's, announced their intention to merge. At the time of the merger announcement, Waterstone's was controlled by the HMV Group.[16] Waterstone's, the book-retailing segment, had 190 stores in the UK, each with a selection of titles, generally, between 30,000 and 40,000. Ottakar's was established in 1987 with the aim of creating a chain of bookshops in market towns throughout the UK. After opening its first store, it grew both organically and through acquisitions, reaching 141 stores by December 31, 2005. Its stores averaged between 20,000 and 30,000 titles.

The CC defined the product market as the retail sale of new books to consumers. It also considered segmenting the market between bestsellers and deep-range titles. The CC found evidence that the competitive conditions on the two segments could differ as supermarkets and internet retailers' commercial offer focused on bestsellers, but it then rejected this definition since there were no retailers selling only deep-range titles and the distinction between deep-range and bestseller titles was somewhat arbitrary. Nevertheless, in our analysis we account for the possibility that different title categories are subject to different competitive conditions and are therefore affected by the merger in a different way. In order to do so, we also perform our empirical exercise on each category separately. Concerning the geographical dimension, the CC also considered whether competition was at the national or local level by examining three dimensions of competition: prices, range of titles stocked, and service quality. The CC claimed that the parties usually set uniform national prices and, as a result, local competition was generally in terms of titles range and service quality.

The merger investigation lead the CC to conclude that the proposed merger was not expected to result in an substantial lessening of competition in the market for the retail sale of new books (best-sellers or deep-range titles) at a local, regional or national level. As a result, the CC cleared the merger unconditionally on May 12, 2006.

3. Data and Sample Selection

To perform our empirical analysis, we built two different datasets, one at the store level and one at the national level. We acquired from Nielsen data on the volumes and values for a sample of 200 book titles sold by 60 of Waterstone's and Ottakar's stores. Nielsen provided

[16] The HMV group is a global entertainment retail chain. It bought the Waterstone's chain in 1998 and merged it with its own bookstore chain called Dillon. The HMV group eventually sold the Waterstone's chain in 2011.

us with weekly figures for these variables, as well as data on several book specific characteristics for each selected area, from the first week of January 2004 through the last week of December 2007.[17] Moreover, we obtained data on nationally aggregated volumes and values for the same sample of titles over the same time-span, separately for the merging parties and for the entire market. Value and volume figures for the competitors are then obtained as difference between the data of the entire market and those of the merging parties. Sections 3.1 and 3.2 describe how we selected the sample of stores and titles.

To complete our data, we also gathered information from public sources on socio-economic characteristics (such as population, GDP, internet penetration, etc.) –both at the local and the national level– that we used in the selection of the 60 stores covered in our analysis as well as additional control variables in the econometric exercises. A description of these control variables is provided in section 3.3.

3.1. The Choice of the Stores

The DiD analysis at the local level requires the identification of stores to include in the treatment and control groups. We define the treatment group as the Waterstone's and Ottakar's stores from overlap areas, i.e. local areas where both chains were present before the merger, and the control group as the Waterstone's and Ottakar's stores from non-overlap areas.[18] For the years 2004-2007, the Nielsen Bookscan panel, from which our data are retrieved, contains a total of 359 Waterstone's and Ottakar's stores located in 203 different areas (at the local authority level), of which 33 are the overlap areas identified by the CC and 170 are non-overlap areas.[19] While ideally we would like to have data on all stores in all areas, due to budget constraints this was unfeasible. Accordingly, we built a sample of 60 stores in which the number of Waterstone's and Ottakar's outlets is equally split between overlap and non-overlap areas (i.e. 30 and 30). For the overlap areas, we draw the 30 stores

[17] Throughout the analysis we aggregate data at the monthly level, since the weekly data might have too much undue variation that may not reflect actual changes in the pricing policies of the retailers. In particular, when the volumes sold are low, the average price is affected by typing errors and by the nature of the sales (i.e. whether stand-alone or part of a bundle). Aggregating at the monthly level alleviates this problem.

[18] The CC noticed: "… local competition, to the extent that it exists, is concentrated on nearby stores (within the same shopping location), and also encompasses out-of-town stores which may themselves be shopping destinations, such as supermarkets and Borders' superstores. Waterstone's and Ottakar's do not in general have such destination stores and therefore our assessment of local competition was focused on nearby locations, in particular 33 overlap areas, located over the entire breadth of Great Britain." (Cfr. "HMV Group plc and Ottakar's plc Proposed acquisition of Ottakar's plc by HMV Group plc through Waterstone's Booksellers Ltd", 12th of May 2006, p. 5).

[19] Cfr. Appendix E of the CC decision.

from 20 different areas. We select one store for each chain in 10 overlap areas. Then, to increase the coverage of overlap areas, we draw five additional Waterstone's stores from five overlap areas and the last five Ottakar's stores from a different set of five overlap areas. Hence, we cover a total of 20 different overlap areas. As for the control group, we select 30 non-overlap areas: 15 in which we observe only Waterstone's stores and 15 with only Ottakar's stores.

To summarize, the 60 stores in our sample are selected from a total of 50 areas of which 20 are overlap areas and 30 non-overlap areas. Once the number of areas and the type of stores required was decided, we chose a method to select the specific areas to be included in our sample. The key challenge is to choose areas for the control group that closely resemble the ones chosen for the treatment group in terms of demand and supply conditions such that any post-merger difference between the two groups can be attributed to the merger. Hence, we select two groups of areas with homogeneous observable characteristics and we assume that the non-observable characteristics are similarly distributed.

To this aim we use the Propensity Score Matching (PSM) methodology.[20] PSM postulates that the probability of treatment depends on observable characteristics and the actual assignment is random once one accounts for the predicted probability of treatment. It is then possible to build a control group using these predicted probabilities. In our case, the treatment is the presence of both chains in the same area. Hence, we postulate that the overlap and non-overlap areas may have similar probability of treatment as they share similar demand and supply conditions, although we observe only some treated areas.

In our selection mechanism, we use all 203 areas in the Nielsen panel and for each of them we collect a wide range of information on local market conditions that might affect the demand and supply sides in book markets (population, presence of universities, gross value added, internet penetration, average house prices, etc.; see Section 3.3. for a description of these variables). Based on these observables, we first estimate the predicted probability of treatment (propensity score) for each area. Out of 33 overlap areas we could only select 20 that matched our requirements. Indeed we first exclude three overlap area since their estimated propensity score is outside the common support (they do not have any "close" match in the non-overlap sample). Then, due to data limitations (either the shop was not in Nielsen Bookscan panel or it had closed), we dropped 10 additional localities. Finally, for

[20] For a more detailed description of the application of the PSM, see Appendix 1.

each of the selected overlap areas we identified the closest match among the non-overlap areas, i.e. the area that exhibits the closest propensity score.[21]

3.2. The Choice of the Titles

During its inquiry, the CC considered whether there was a separate market for bestselling and deep-range titles; the latter being those books ranked 5,001 or lower in sales. Although it concluded that this was not the case, the CC recognized that the merger may have had a different impact on these titles due to the lower degree of competition that characterizes the sale of deep-range titles. Indeed, while bestselling titles faced strong and growing competition, in particular from supermarkets, non-specialist stores, and internet retailers, deep-range titles appeared less affected by these competitive constraints because supermarkets and non-specialists stores stocked only a small number of such titles.[22] There is no evidence that the competitive framework between bestseller and deep-range titles has significantly changed since the merger, although supermarkets and non-specialist retailers have apparently enlarged the range of the deep-range titles kept in stock.[23] Therefore, we want to account for these possible differences in our analysis.

Excluding titles in the "deep-range" category, we are left with books in the top 5,000 of annual sales. Although these books sell broadly, these titles differ greatly with respect to the volumes sold. For instance, in 2007 the volume of the 200 most sold titles represented around 33% of the total sales of bestsellers, with an average of more than 214,000 copies sold. On the other hand, the 200 books ranked from 4,800 through 5,000 represent just 1% of the total sales, with an average of 7,100 copies sold per title. Hence, we suspect that the retailers' pricing policy may significantly differ across these 5,000 titles.

First, we identify a set of titles that may be characterized by a particular pricing policy, which we call "evergreen". These titles are sold in reasonable high volumes for many years and appear to be consistently among the bestsellers for a longer period of time. In principle, they are subject to unique competitive pressures because evergreens are sold across a variety of retail channels that otherwise devote limited space to books–i.e. non-specialist retailers and

[21] From the non-overlap areas we exclude the London ones, as there was no overlap area located in London. The matching between overlap and non-overlap is without replacement; that is a non-overlap area cannot be matched with more than one overlap area.
[22] Despite the fact that specialist retailers face competition on deep-range titles from Internet retailers, discounts on this category of titles are much smaller than on bestsellers.
[23] See Clark and Phillips (2008), p. 243.

supermarkets– as they can guarantee a quite stable flow of sales. For the purpose of this paper, we include in this category those titles that were ranked among the 5,000 most sold books across the entire period under analysis.

Of the remaining books, we define "top-sellers" to be those titles that were ranked among the 200 most sold books in any given calendar year. Indeed, supermarkets –which are by far the strongest price competitors of the specialist retailers– tend to concentrate their offers for those books that many consumers are interested in purchasing and have a high position in the sales rankings. Therefore, the pricing strategy of the merging parties for these titles might be different from the one adopted for other frequently sold books. In the event that a book could be included in either the evergreen or top-seller category, we define it to be a "top-seller" for that specific calendar year. This leaves us with the "bestseller" category, which includes all top 5,000 titles that are not already defined to be "evergreen" or "top-seller". As a result, our definition of bestsellers is somewhat different from the CC's one because it includes titles ranked 200 to 5,000 that are not evergreen.

Other than those books that have earned the status of "evergreen" in our sample, it is important to clarify that titles can change status from one year to the next, varying between best seller, top-seller, and deep-range. For example, bestsellers may move up or down the ranks, with the potential of becoming either a top-seller or deep-range title in the following year. To account for this fact, we selected a sample of titles that are representative of the different categories for each of the years under examination. This results in an unbalanced panel, where some titles are observed for the entire period, while others are only observed for some years.

We also consider other potential sources of differences in the pricing policies, i.e. the type of binding (hardcover vs. paperback) and the genre. The type of binding represents a way of discriminating among consumers with heterogeneous valuation and it is often connected to inter-temporal pricing policies aimed at exploiting the different willingness to pay of consumers (Clerides, 2002). It is less clear whether retailers adopt different pricing strategies according to the genre, but we think it is worth investigating also this aspect. Therefore, in order to assess any potential effect of the merger that may affect only books with a specific characteristic and to offer a reasonable representation of the universe of books sold, we

include in our sample titles with different types of binding and genre.[24] We complete our dataset by collecting information for each title in the sample on a number of title-specific characteristics such as the date of publication, the number of pages, whether it is part of a series and whether it contains figures.[25]

Taking into consideration all the above criteria, we asked Nielsen to randomly select 200 titles subject to the following conditions. At least 20 titles had to be evergreen; for each year at least 10 books had to be top sellers; around 50% of our sample should consist of deep range titles (to reflect CC's concerns over this type of titles); each year some newly published books should be introduced. In addition to these constraints, we also made sure that the distribution of the main characteristics for each title category in our sample was similar to the distribution of characteristics in the population of all books sold in the UK. Some statistics of the selected sample are reported in table 2.

[insert Table 2 here]

Since the sales of each title vary over time, the composition of the sample in terms of title category may vary too. Table 3 reports the number of titles included in each category for each year. The annual size of the sample increases over time as titles published after 2004 are progressively added to the sample. Appendix 2 lists all 200 titles on which we perform our econometric analyses.

[insert Table 3 here]

3.3. The Control Variables

In addition to the title-specific control variables described in the previous subsection, we also built a large dataset of variables to control for the areas' demand and supply conditions. We consider several factors that may potentially affect demand and supply in the book retailing market. With respect to the demand side, we collected information on (i) population; (ii) population density; (iii) average sales of books in volumes; (vi) gross value added (GVA); (v) number of universities; (vi) level of education; and (vii) the diffusion of internet sales (which

[24] We employ Nielsen classification of genres, which distinguishes book titles in four macro-categories: (i) Children's, Young Adult & Educational, (ii) Adult Fiction, (iii) Adult Non-Fiction: Trade, and (iv) Adult Non-Fiction: Specialist.

[25] If a book contains figures, publishers need to print in color, which in turn may raise the RRP. We do not know whether this may also affect the way retailers set the discounts, however we believe it is worth including this characteristic in the regression as a control.

we proxy through the level of internet penetration). The first four variables are mainly aimed at controlling for the dimension of the market, while the latter should provide an indication on the local population's propensity to buy books –i.e. the demand conditions. With regard to the supply side, we gathered data on: (i) potential cost shifters such as the cost of paper and average house prices as a proxy of the cost incurred for opening a store; and (ii) a measure of the intensity of competition –i.e. the number of different retailers operating in a given area and their entry and exit rates. Except for the cost of paper, which is an internationally traded commodity, as well as for area-specific variables (such as those related to the intensity of competition), all other variables were collected both at the local and the national level. All variables are described more in-depth in table 4. Tables 4a and 4b provide descriptive statistics for the set of variables used in the local and in the national analysis respectively.

[insert Table 4 here]

[insert Table 4a here]

[insert Table 4b here]

4. Empirical Analysis

The analysis we undertake aims at evaluating the effects of the Waterstone's/Ottakar's merger on the retail market for books in the UK. Since commercial data providers only hold data on prices, while all the information on the range of books stocked and on the quality of service are held by the retailers themselves, our econometric analysis focuses on the effects of the merger on the price dimension. The dependent variable of interest is the discount applied to RRP, because this is the variable retailers compete on. Nonetheless, for the sake of simplicity, we often refer to prices and price competition in the text.

To get a comprehensive picture of the merger's effect, we perform two distinct econometric analyses. Our preferred approach in terms of the richness of the data and of the identification strategy examines the impact of the merger at the local level. It compares prices in areas that should have been more affected by the merger (overlap areas) with prices in areas that should have been less affected by the merger (non-overlap areas) using the DiD methodology. Of course, this analysis is only meaningful in the presence of some price variability at the local level. Therefore, as a preliminary step, we perform a statistical analysis to observe whether there is sufficient price variability across local markets. In addition to the analysis of prices at the local level, we also measure the effect of the merger on nationally

aggregated prices, using two different control groups: the prices set by the competitors and the prices of top-titles. This complementary empirical exercise should provide important additional information by measuring the merger's effect on prices at a different aggregation level. Clearly, this implies that it relies on a different identification strategy. In the remaining of this section, we describe the econometric methodology used in our main analysis, i.e. the DiD at the local level. In Section 5 we describe the methodology used in the DiD at the national level.

4.1 Local Price Variability

We calculate the standard deviation of the discounts granted by Waterstone's and Ottakar's across the 60 stores in our sample for each title and for each month.[26] For each title in a given month, we consider the average percentage discount applied by each store selling that title. We then compute the standard deviation of these percentage discounts across stores. Finally, we estimate the distribution function of the standard deviation using a kernel density estimator. Figure 1 presents the distribution of the standard deviation of the discount across stores for all the titles together pre- and post-merger as well as for each of the four categories of titles. The standard deviation across stores is slightly lower (more concentrated around 0) in the post-merger period. However, the reduction in the variability after the merger is limited, which does not suggest any relevant change in the geographic scope of competition.

[insert Figure 1 here]

The discount variability seems to substantially differ across categories over the whole period, with a higher variability for top-titles and evergreens, but less disperse distribution for bestsellers and deep-range titles. Yet, we observe a significant difference in the distribution between the pre-merger and the post-merger period only for top-selling titles for which the variability is in both periods relatively high.[27]

[26] The analysis of the price dispersion across stores might be affected, at least to some extent, by the presence of bundle discounted sales in the dataset. Bundle discounted sales, such as "3 for 2", are common across retailers and they are largely used as a promotional activity. Titles included in these bundles are effectively sold at a discount, which is greater than the one applied to stand-alone purchases. This implies that the more titles that a store sells through bundle-offers, the lower is its average selling price. As a consequence, some price dispersion across stores may be the result of different successful promotional bundle campaigns, rather than of different pricing policies. Unfortunately, we could not control for this problem, as Nielsen does not collect information on whether a book is sold as stand-alone or as part of a bundle offer. Therefore, the results relative to the price dispersion across stores must be interpreted cautiously.

[27] In Appendix 3, we further investigate the aspect of the geographic aspect of price competition through the analysis of percentiles distribution.

Two conclusions can be drawn from this preliminary analysis on the discount variability across stores. First, we observe some discount variability across stores that might be consistent with price competition taking place at the local level, both before and after the merger. Moreover, the extent of local competition on discounts seems to vary significantly across categories, with deep-range titles at the lowest extreme and top-selling titles at the highest. This suggests that the parties tended to adopt a more uniform pricing strategy across the UK for the former category of titles, while for the latter titles' category discounts seem to be set more frequently set at local level. Pricing policies for bestsellers and evergreen titles lie somewhere in-between. Overall, we cannot rule out the possibility that prices are, at least to some extent, set at the store level, since we find evidence of some variability in local prices.

4.2 Local Analysis: Average Treatment Effect

In Figure 2, we plot the monthly average discounts in the overlap and non-overlap locations for the different categories of titles. If the merger increased the price –i.e. reduced the discount– we would expect to observe an increase in the vertical distance between overlap and non-overlap lines after the merger. Yet apparently, discounts in overlap areas tend to follow broadly the same pattern as those in non-overlap areas and do not seem to be systematically lower post-merger. We observe different patterns across categories. The discounts on bestsellers and deep-range titles seem to have slightly decreased over time. Apparently this trend started well before the merger. Discounts on evergreen titles decreased over time as well, although the trend is less marked. The discount pattern for top-selling titles is less clear.[28]

[insert Figure 2 here]

The hypothesis we test is that, if local managers were free to set prices at the store level and the merger had anticompetitive effects, these effects should have been larger in the overlap areas, because of the reduction in local competition brought about by the merger. We therefore estimate the following regression:

[28] The more pronounced volatility is partially due the fact that our sample for these titles is small, which implies that a change in the discount that is applied only to a few titles may significantly affect the average. In particular, in the first four months of 2007 we have only one top-selling title in our sample, which was sold at a very high discount; this explains the sudden increase we observe at the beginning of 2007. This occurrence could affect the results for those titles. Therefore, when running the econometric exercise on top-selling titles, we exclude the data for the first four months of 2007.

$$disc_{ist} = \alpha + \beta \cdot post_t + \lambda \cdot overlap_s + \delta \cdot post_t \times overlap_s + \gamma \cdot X_i + \mu \cdot Z_{st} + \eta_t + \nu_{is} + \varepsilon_{ist} \quad (1)$$

where $disc_{ist}$ is the discount on the recommended retail price on title *i* granted in store *s* at time *t*, $post_t$ is a dummy equal to 1 for the titles observed in the post-merger period and 0 before, $overlap_s$ is a dummy equal to 1 for the titles sold in overlapping stores and 0 otherwise, X_i is a set of title-specific control variables, Z_{st} is a set of variables aimed at controlling for changes across time in local market features, η_t is a time trend, ν_{is} is a title-store fixed effect and ε_{ist} is the error term.[29]

Our key variable is the interaction between $post_t$ and $overlap_s$, whose coefficient (δ) measures the price change in overlap locations relative to the price change in non-overlap areas: the average treatment effect (ATE). This coefficient quantifies the additional variation experienced by the prices in the overlap areas with respect to the average price change in the non-overlap areas. The *post* coefficient (β) measures any price change (between the pre-merger and the post-merger period) common to all locations, while the coefficient λ, related to the *overlap* regressor, accounts for any idiosyncratic differences between overlap and non-overlap areas that are not related to the merger.

We perform a pooled regression on all the titles together in order to quantify the overall effect of the merger. We also run the regressions on each category separately, as the different categories of titles face different competitive conditions and are therefore expected to exhibit dissimilar discount patterns over time.

In our baseline specification , we run a regression with fixed-effects for each title/store, so as to capture all the time-invariant title/store-specific (unobserved and observed) characteristics that may affect their prices. However, the use of title/store-specific fixed-effects implies that the effect of the merger on discounts is solely identified from titles sold both before and after the merger, because the interaction variable is perfectly collinear with the title/store fixed-effects –i.e. it is time-invariant– for those books whose prices were observed only before or after the merger. This may affect the estimates because it reduces the

[29] We test for autocorrelation in the error process by means of the Wooldridge test. We strongly reject the null hypothesis of no autocorrelation. We thus cluster the error terms at the title and store level. As a further robustness check we also estimate regressions in which we impose an AR(1) error structure on the model. The resulting estimates are similar to those obtained by clustering the error terms. Finally, we also used monthly dummies as time fixed-effects instead of the monthly trend. Results are again unchanged.

size of the sample of titles on which the effects of the merger are actually measured and it does not allow us to capture any change in the prices of the titles published after the merger.[30]

In our sample this problem arises from two different sources: (i) some of the titles included were published after the merger, and (ii) some of the titles included changed category over the period examined, which is clearly only relevant when we run the category-specific regressions. The titles that belong to (i) are mainly top-selling titles and, in very few cases, bestsellers and deep-range books, while for the titles which belong to (ii) the problem spans across all the categories, since titles frequently move up or down the rankings from one year to the next. When we pool all titles together, the fixed-effects problem is not a big concern since the potential distortion comes only from titles that are published after the merger. These titles represent only a small fraction of the sample (19 out of 200 titles). Hence, in the pooled regressions we opt for a specification with fixed-effects at title/store level. When instead we run the regressions for each category of titles separately, the fact that a large proportion of titles change category over time, and in particular before and after the merger, poses a serious problem to the fixed-effect estimation. To overcome this problem, instead of the title/store-specific fixed-effects, we include a set of observable characteristics (both title-specific and store-specific) that may affect a title's price, using a random-effect specification within a hedonic pricing approach.[31]

A further methodological issue we address is related to the selection of the window of data surrounding the merger to be excluded from the analysis, since we do not know when the merging parties started operating as a single entity. We consider two possible windows (6 and 12 months) around the date of the merger clearance. We run all regressions using these two different samples where we dropped all observations inside the chosen window. We find that the results are essentially unaffected by the size of the window. We therefore only report results based on the window that drops the least number of observations, i.e. the one that

[30] In order to assess the impact of the merger on the titles published after the merger, we undertake a specific analysis that is described in Section 4.3.

[31] The random-effect specification has a potential drawback as the estimator can be biased if there are unobservable characteristics that systematically changed after the merger occurred. Following the approach of Ashenfelter et al. (2013), we run the regressions on the sample of titles that remained in the same category before and after the merger using both a fixed-effect specification and a random-effects specification with title and store characteristics. The results of this comparison are presented in Appendix 4. We find that the estimate of treatment effect is similar under both specifications, thereby suggesting that unobservable product characteristics do not result in bias of the estimator of the random-effect regression.

excludes the 6 months around the merger date (3 months before and 3 months after the clearance), as this window allows a more efficient exploitation of the dataset.[32]

Table 5 reports the results of the DiD regression (1). In the first column, we use the full sample of all titles with fixed-effects for each combination of stores and titles. The coefficient for *post×overlap* is not significantly different from zero. Therefore, the merger does not seem to have had a different impact in overlap and non overlap areas. Columns 2 to 5 report the results of the regressions run on best-selling titles, deep-range titles, evergreen titles, and top-selling titles respectively. As discussed above, for these specifications we use a random-effect specification.

[insert Table 5 here]

Again, the terms of interest, i.e. the coefficient estimates for *post×overlap*, are never statistically significant, which seems to confirm that the merger did not adversely affect the discounts applied by the merging parties in the overlap areas, on average. We find a significant and negative time trend for all categories, except the evergreen titles, which is broadly consistent with the graphical analysis discussed above. In addition, we also look for shifts in this trend after the merger (the *post* coefficient), and we find statistically significant effects only for deep-range titles, which exhibit a positive shift (+ 2.4%), and for top-selling titles, which have a negative shift (– 3.5%).[33] We do not extensively report results on all other control variables, which mostly conform expectations.[34] In conclusion, the merger does not seem to have on average adversely affected prices in the overlapping areas where it could

[32] We are aware the CC cleared an anticipated merger. However, according to press reports, Ottakar's agreed the takeover offer from Waterstone's on May 31, 2006, and all Ottakar's stores were rebranded as Waterstone's by November of that year, i.e. few months after the clearance. This indicates that the parties merged soon after the clearance and, thus, that this may act as a good proxy of the date when the merger actually occurred. Moreover, the fact that the estimates are essentially unaffected even if we consider a 12-month window supports the view that the results are not sensitive to the exact identification of the merger date.

[33] As a further check we also run the same regressions using time fixed-effects (i.e. we introduce a dummy for each month) instead of a linear trend and the results are broadly similar (results are available upon request).

[34] We observe some common and statistically significant effects relative to the title characteristics. First, around Christmas the discounts (*season*) tend to be higher (except for deep-range titles). Second, paperback titles (*paperback*) are associated with lower discounts compared to hardcover titles (except for the top-selling titles). Third, the discounts appear to be lower (except for deep range books) as the time elapsed from the publication (*elapsed_year*) increases. Fourth, the publication of a new title is on average accompanied by promotional discounts (except for the deep-range category) as shown by the sign of the coefficient *just_pub*. Fifth, when a book contains figures (*figure*) the discount is on average lower. Sixth, titles that are part of series (*series*) are usually sold at a higher discount. Finally, the estimates indicate that Waterstone's stores (the coefficient of the dummy *waterstone*) before rebranding applied on average a discount 1.6% higher than Ottakar's shops. The other control variables included in the model are, instead, mostly not significant and, even when they are, the sign of the coefficients differs across categories. In particular, the variables controlling for local market features (i.e. nature and number of competing retailers, population, property price, urban vs. rural area, presence of universities and degree of education) do not seem to play a role in how the discounts were set.

have been expected to generate the strongest effects due to the increase in the level of market concentration.

4.3 Local Analysis: Heterogeneous Treatment Effect

The fact that we do not observe any significant average result might simply mean that our simple framework fails to model some important underlying heterogeneity in the merged entity's conduct post-merger. Hence, we further exploit the richness of our dataset and we perform heterogeneous treatment effects estimations to more precisely investigate the impact of the merger on prices. In particular, we assess whether the effect of the merger differs between overlap and non-overlap areas along three dimensions: book-specific, firm-specific, and market-specific. There are good reasons to expect possible heterogeneous effects of the merger along these dimensions. The merger might for instance only affect the merging firms' pricing strategy for the new products released after the merger as compared to old products; it can affect the extent of price differentiation between the merging firms if they internalize the externalities they exert on each other's products; and finally it might have differential effects depending on the competitive conditions of local markets.

Table 6 reports the estimated coefficients for several heterogeneous treatment effects. In the first set of regressions our key variable *post×overlap* is interacted with firm-specific dummy variables. We try to identify whether there was a different pricing response to the merger between Waterstone's and Ottakar's stores if compared to their average behavior in non-overlapping areas. The results of these regressions suggest that there has been a convergence of the prices between the two merged chains in overlap areas after the merger. In particular, Waterstone's stores reduced the discounts by 1.5% on average, while Ottakar's stores increased them by 0.9%, when compared to non-overlap areas. This might have important policy implication as it has long been recognized that uniform prices can have a mixed effect on consumers' surplus (e.g. Hausman and Mackie-Mason, 1988): some customers might be better off while others are worse off. Yet, to draw clear welfare conclusions about this change in pricing strategy would require an analysis of sales volumes for all book titles, which is unfortunately infeasible with the data at hand.

[insert Table 6 here]

We further consider whether the effect of the merger mainly materializes in the overlap areas where the merging parties closed a store after the merger. Yet, the coefficient

post×overlap×closed, which captures the effect of the merger in those areas, is not significantly different from zero, thereby indicating that not even in those locations where there was a reduction in the number of stores due to the exit of one of the merging parties can we observe a systematic difference in the prices after the merger.

The second set of regressions deals with title-specific heterogeneity. As Ashenfelter et al. (2013) point out, it is important to explore whether the merger influences products existing prior to the merger differently from products introduced after the merger in order to correctly assess its effects. Also, with short-life cycle goods the firms' pricing strategy in relation to a specific product may vary over time to reflect different demand and supply conditions. In the book industry, for example, we observe (see footnote 34) that retailers tend to grant higher discounts on titles that have been just published –i.e. in the two months following publication. The merger may have then had an impact on the parties' pricing incentives, which vary depending on the elapsed time since the book publication. To capture this heterogeneous effect on initial discounts for products released after the merger, we partition the treatment effect into three components: (i) the component capturing changes in prices for those titles released before the merger (*post×overlap×released_pre*), (ii) the component capturing changes in prices for those titles released after the merger but only considering the first two months after release (*post×overlap×released_post×just_pub*), and (iii) the component which is identified by the changes in prices for those titles released after the merger, but only considering the period following the second month after release. However, in this case we must exert a note of caution, as we have only a few books published post merger in our dataset, all of which essentially belong to the top-sellers category. Hence the empirical identification of this effect might not be particularly robust. In fact, we only show estimates for top-selling titles.

The positive and significant coefficient of *post×overlap×released_post×just_pub* suggests that stores in overlap areas seem to price newly released books more aggressively after the merger. Instead, the negative and significant coefficient of *post×overlap×released_post×non_just_pub* indicates that stores in overlap areas set significantly higher prices for titles published after the merger two months after the release date. This result seems to suggest that the merger might have also affected the firms' incentives in relation to the timing and duration of discounts that retailers offer. This result should be taken with caution given the limited number of titles released post merger in our

sample. Nonetheless we think that the mergers' impact on the timing of discounts/prices in short-life product markets may be a fruitful line for future empirical and theoretical research.

The third set of heterogeneous treatment effect regressions deal with a market-specific heterogeneity. Similarly to Hosken et al. (2012), we look at whether the effect of the merger on prices differs according to the intensity of competition as measured by the number of competitions present in the market before the merger.[35] In particular, we define the variables *post×overlap×high_comp*, *post×overlap×medium_comp* and *post×overlap×low_comp*, which represent the effect of the merger in overlap areas with high, medium, and low number of competitors respectively.[36] Moreover, we investigate whether the merger had a differential impact depending on the dynamics of entry and exit. More precisely, we define the following variables: *post×overlap×high_entry/ post×overlap×high_exit* represents the effect of the merger in the areas where the entry/exit of 3 or more competitors occurred after the merger; *post×overlap×medium_entry/ post×overlap×medium_exit* represents the effect in the areas where there was entry or exit of 1-2 competitors after the merger.[37] The expectation is that entry can mitigate the potential anti-competitive effects of the merger. However, these latter interactions should be interpreted cautiously as there is a potential endogeneity problem (i.e. entry and exit may be triggered by the pricing conduct of the merged entity following the transaction). Nevertheless, the coefficient estimates for these two sets of interaction variables are mostly not significant, indicating that market-specific characteristics that should capture differences in the areas' competitive conditions do not seem to play a significant role in explaining the effect of the merger at local level.

5. National Analysis

So far our analysis focuses on the effect of the merger on local competition. However, the concentration might also have had an aggregate effect on national prices, which would not be identified through our local analysis. Therefore, we perform a complementary DiD analysis to investigate the merger's effect at the national level. A major issue in implementing this

[35] Unfortunately, information on market shares in local areas is not available in our data. Hence, we must rely on the simple count of the number of competitors instead of a more precise measure of market concentration as used by Hosken et al. (2012) to analyze the heterogeneous effect of mergers in the US grocery retailing markets.

[36] Areas are assigned to one of the three categories (high, medium, low) on the basis of the number of competitors that were present in each area before the merger. We define high, medium, and low based on the 33rd and 66th percentile of the distribution of the number of competitors in all the areas.

[37] These variables refer to generic bookstores like WHSmith since other types of competitors (e.g. supermarkets) do not have any significant variation in entry/exit episodes in the areas considered.

alternative analysis is the identification of a suitable counterfactual. To enhance the robustness of our results, we employ two different control groups: (i) the same titles sold by the competitors of Waterstone's and Ottakar's; and (ii) the top-selling titles sold by the merging parties. We describe the rationale behind both methodologies in turn.

5.1 Competitors' Titles as Control Group

As a first control group for our DiD analysis with nationally aggregate prices, we use the prices charged by the rival firms.[38] This control allows us to disentangle the merger effect from any common factors affecting both the treatment and the control group. Indeed exogenous supply or demand shocks affecting the whole industry should be expected to hit in a similar way the prices of the merging parties and those of their competitors. However, if firms compete on prices, the discounts applied by all retailers in the market are likely to be correlated and, thus, the merger may affect not just the discounts granted by the parties, but also those granted by their competitors. This would suggest that the prices of the competitors are not a valid control group. Yet, according to the standard theoretical merger model by Deneckere and Davidson (1985) where firms compete in prices and goods are differentiated, any price change by the merging parties post-merger should be followed by a price change in the same direction by their rivals, but of a lesser magnitude. Comparing the change in prices of the merging parties to that of the competitors may therefore provide a useful indication as to whether the merger produced negative price effects. If we were to measure a positive average treatment effect, we might conclude that the merger resulted in higher prices even though we would be unable to measure the magnitude of the overall merger's effect on prices. In this case the general estimation equation is:

$$disc_{ijt} = \alpha + \beta \cdot post_t + \lambda \cdot merged_j + \delta \cdot post_t \times merged_j + \gamma \cdot X_i + \mu \cdot Z_t + \eta_t + \nu_i + \varepsilon_{ijt} \qquad (2)$$

where $disc_{ijt}$ is the discount on the recommended retail price on title i granted by retailer j at time t, $post_t$ is a dummy equal to 1 for the titles observed in the post-merger period and 0 before at retailer j, $merged_j$ is a dummy equal to 1 for the titles sold by the merging parties and 0 otherwise and measures the time-invariant difference between the merging parties and

[38] Using competitors' products as a control group is a common practice in this literature. For instance, see McCabe (2002). Ashenfelter et al. (2013) use a similar control group to identify the effect of the merger between Maytag and Whirlpool. However, in their case as the merging firms are manufacturers, their control group were rivals' products within each appliance category. In our case, instead, we compare the same titles sold by competing retailers.

their competitors. X_i is a set of title-specific control variables and Z_t is a set of variables aimed at controlling for changes across time in the demand and supply conditions at the national level, η_t is a time trend, v_i is a title fixed-effect,[39] and ε_{ist} is the error term.[40] The key variable of interest is the interaction between $post_t$ and $merged_j$, whose coefficient (δ) measures the price change of the merging parties relative to the price change of competitors attributable to the merger.

Similar to the local level analysis, we start by plotting and graphically comparing the average discounts applied by the merging parties and the competitors (figure 3).

[insert Figure 3 here]

The discount patterns of the merging parties and competitors diverge over time. The former decreased their discounts, while the latter increased them. This appears to hold for all categories, except for top-selling titles, for which no clear trend can be identified either for the merging parties or their competitors. The diverging trend seems to start indicatively around the beginning of 2005, which is well before the merger was consummated. This may hardly be the result of the merger because it would imply that the parties started acting as a single entity one and a half year before the CC's decision.[41] We consider it more likely that the observed trends are the results of structural changes in the supply-side of the market. The "competitors" group contains a wide set of retailers, ranging from specialist and non-specialist chains to supermarkets and internet retailers. According to the data provided by the Booksellers Association,[42] the market shares held by supermarkets and internet retailers have continually increased over the past years, as shown in figure 4.

[insert Figure 4 here]

[39] Similarly to the local analysis, in the regressions by category we replace the fixed-effect with product characteristics. A comparison of the estimated price effect using these two different specifications on the sample of titles that remain in the same category before and after the merger is presented in Appendix 4.

[40] Like for the analysis at the local level, we test for autocorrelation in the error process and find that the null hypothesis of no autocorrelation is strongly rejected when the data are aggregated at the national level. To control for this issue, we cluster the error at the title level.

[41] Some studies find evidence of anticipatory price increases before the parties were legally granted permission to merge (e.g. Weinberg, 2008). However, even if one considers the first announcement of Waterstone's bid for Ottakar's in August 2005 as the date when the parties started acting as a single entity, the diverging trend began some 8 months earlier. This seems to rule out the hypothesis that the merger triggered the negative trend in the discounts.

[42] Booksellers Association website: http://www.booksellers.org.uk/Industry-Info/Industry-Reports/Book-Industry-Statistics/UK-Book-Sales---Retail-1999-2008.aspx, visited October 2010.

This observation, in combination with the fact that supermarkets and internet retailers tend to apply higher discounts than specialist and non-specialist book retailers,[43] may explain, at least partially, the increase in the average discounts for the "competitors" category.[44] In other words, the apparent increasing trend may simply be due to a change in the composition of the group of competitors, where the increasing weight of supermarkets and internet retailers drives the observed pattern of the average discount.

The fact that the discounts of the merging parties and the competitors show two diverging trends, which started before the merger, poses concerns on the validity of the DiD approach. Indeed, the common trend assumption that lies at the very heart of the DiD methodology seems to be violated in this case. We try to address this issue by imposing two distinct trends, one for the merging parties (*month_t_merged*) and one for the competitors (*month_t_comp*), so as to isolate the effect of the trends from that of the merger.[45]

[insert Table 8 here]

Table 8 presents the results of the estimation of equation (2). In the pooled regression (column 1) with fixed-effects, the coefficients of both these trends are significant and have the expected sign (negative for the merging parties and positive for the competitors). The coefficient's estimate for *post×merged* is not significant in this specification suggesting that the merger does not appear to have had any differential impact on discounts, either between the merging parties or their competitors. Consistent with the results of the pooled regression, the analysis at the category level (columns (2) to (5)) shows that the coefficient's estimate for *post×merged* is never significant and that the coefficients associated with the trends, when statistically significant, are negative for the merging parties and positive for the competitors. The only exception is for the top-selling titles, where both trends are negative and significant. Finally, the *post* dummy, which should capture any common deviation from the trend after the merger, is not significant either in the pooled regression or in the category-specific one. Overall, this analysis suggests that the merger did not affect national prices.

[43] Anecdotal evidence on this is provided by a survey of market participants that we conducted for the CC (see Aguzzoni et al., 2011). Clay et al. (2002) instead find that, in the US market, online and physical stores have similar prices, although online prices are characterized by a higher dispersion.

[44] This issue could have been addressed by splitting the data by retail channel and using only the large chains and independent shops channels as a control group. Unfortunately, Nielsen could not provide us with the data by retail channel because of confidentiality reasons.

[45] The resulting estimates might be, nonetheless, biased as the linear trends may not be able to fully capture the different dynamic of discounts for the merged parties and for the competitors.

5.2 Top-selling Titles as Control Group

To overcome the issues that arise when using competitors' titles as a control group, we also perform an additional DiD estimation in which we use the top-selling titles as a control group. The top-sellers appear to be the category where the merging parties face the fiercest competition, as these titles are sold by all types of retailers and, in particular, by supermarkets which have the most aggressive pricing policy. Accordingly, the size of the discount is the largest for this category, as we see in figure 5 where the average discounts applied by the merging parties for each category are plotted. Therefore, the merger could be expected to have the most limited effect on the prices of these titles. In figure 5 we can also observe that, although the pre-merger price patterns in the control and the treatment groups vary across categories, they do not show diverging trends, which suggests that the assumption of common trends may be reasonable in this case.

[insert Figure 5 here]

The estimating equation is then as follows:

$$disc_{ikt} = \alpha + \beta \cdot post_t + \lambda \cdot non\_top_k + \delta \cdot post_t \times non\_top_k + \gamma \cdot X_i + \mu \cdot Z_t + \eta_t + \varepsilon_{ikt} \qquad (3)$$

where $disc_{ikt}$ is the discount on the recommended retail price on title i in category k at time t, α is a constant, $post_t$ is a dummy equal to 1 for the titles observed in the post-merger period and 0 before for category k, non_top_k is a dummy equal to 1 for the titles in category k other than top-sellers and captures the systematic difference –i.e. not related to the merger– between top-selling titles and other categories of titles, and the coefficient δ captures the effect of merger on these latter book categories –i.e. evergreen, bestseller, and deep-range books.[46] This equation is run only on the merging parties' prices.

The results of the DiD analysis with the top-selling titles as the control group equation (3) are presented in table 9. Columns (1), (2), and (3) report the estimates of the regressions with bestsellers, deep-range books and evergreen books as the treatment group respectively. The coefficient estimate for *post*×*non_top* that measures the effect of the merger relative to the top category, is never significant, thereby indicating that the merger did not differentially affect the discounts applied to bestseller, deep-range and evergreen titles if compared to top-titles. We also check these estimates by using a specification with time fixed-effects instead of a linear trend and we find similar results. Overall, even though some caution in the

[46] Also in this case, the error term is clustered at the title level.

interpretation of the estimates is required because of the limited size of the post-merger sample for top-selling titles and, hence, that small variations on the discount on very few titles may artificially increase/decrease the average discount of the category, our results show that the merger did not produce any negative effect on prices at the national level. This is consistent with the outcome of the DiD regression that uses the prices of the competitors as control group.

[insert Table 9 here]

5.3. Heterogeneous Treatment Effects

To mimic the heterogeneous treatment effects regressions performed at the local level, we also try similar specifications with the aggregated national data. In particular, we look at whether the national discounts of Waterstone's and Ottakar's in the different categories converged after the merger when compared to their competitors.[47] Moreover, we analyze whether the timing of the discounts on books published post merger changed at the national level as well. In both cases, the results reported in table 9 suggest that the effects are generally not significant. The apparent lack of significant heterogeneous treatment effects at the national level (in contrast to what we observe at the local level) is not necessarily worrying. Indeed, this additional analysis should identify complementary nation-wide effects that are derived through a different empirical analysis, performed with different empirical strategies – in particular with different samples and control groups. Overall, our findings suggest that the merger did not affect aggregate national prices, while it affects to some extent competition at the local level by altering the way the merging parties set prices in the overlap areas, though the average effect is insignificant also at this level of aggregation.

6. Conclusions

The ex-post assessment of merger effects is an important and increasingly used tool to inform and guide the decision making of antitrust agencies in prospective merger cases. Despite the large number of mergers in the retailing sector that antitrust authorities have decided upon in recent years, there is lack of empirical work estimating the effects of consummated mergers in these industries. Our paper tries to fill this gap by econometrically analyzing the price effects

[47] We also performed the same exercise using top sellers as control group, and the results are qualitatively similar.

of the merger between two major bookstore chains in the UK –Waterstone's and Ottakar's– that took place in 2006. A peculiar characteristic of mergers in retail industries is the fact that they can exert their influence at different geographical levels since retail chains may set their pricing policies either at the national or local level due to the dispersed distribution of buyers and sellers. Unlike previous retrospective merger studies in other sectors, our empirical framework makes use of this feature to more precisely identify the effect of the merger. The availability of data at both the local and national level coupled with different identification strategies and, in particular, different counterfactuals, allows us to perform complementary assessments of the effect of the merger, which give a broader picture of its implications.

We build an original database with rich information on a set of 200 book titles both at the store level and at the national level. In doing so, we take into account another distinguishing feature of the market under examination, namely the short-life nature of books. Therefore we select a sample of titles that does not remain constant over the whole period, using a hedonic price approach to account for the changes in the characteristics of the products that may impact on prices.

As for the effects of the merger on local competition, we perform a DiD analysis where we compare the price change before and after in areas where both chains are present before the merger (overlap areas) to the price change before and after the merger in non-overlap areas where only one of the merging parties is present. Our results show that the merged parties did not change their prices on average, in a significant way, after the merger in those overlapping locations where it might have been expected to do so. Also at the national level, results of two different DiD analysis – one with competitors as control group, and one with top-selling titles as control group– do not show any significant effect of the merger on prices.

We further exploit the richness of our data to empirically identify a differential response to the concentration by the merging parties. After the merger, Ottakar's –the perceived premium chain– significantly decreased prices while Waterstone's significantly increased them in the overlap areas. We also find evidence that the top-selling titles released after the merger seem to differ in overlap areas in the timing of the discounts even though this effect is only identified by very few observations and, hence, its robustness is questionable.

Arguably, the merger might have affected competition along different dimensions. In particular, it might have led to a reduction of the titles on offer and to a standardization of the

range stocked, which could have had a negative impact on consumer welfare.[48] Unfortunately, due to the unavailability of data on variables other than prices, we were not in the position to assess the magnitude of this effect and how it may have impacted on consumer welfare, nor to ascertain whether it has been caused, or exacerbated, by the merger. Although these aspects do not seem to be crucial in the case under examination, as the survey of market participants seems to confirm, they might be relevant in other retail industries and therefore should be considered in further ex-post evaluation exercises.

[48] For instance, Inderst and Shaffer (2007) show in a theoretical model that retail mergers may reduce product variety.

References

Aguzzoni L., E. Argentesi, P. Buccirossi, L. Ciari, T. Duso, M. Tognoni, and C. Vitale, 2011, "The Ex-post Evaluation of Two Merger Decisions", a report prepared for the UK Competition Commission.

Ashenfelter O., and D. Hosken, 2011, "The Effect of Mergers on Consumer Prices: Evidence on Five Mergers on the Enforcement Margin", *Journal of Law and Economics*, 53 (3): 417-466.

Ashenfelter O., D. Hosken and M. Weinberg, 2013, "The Price Effects of a Large Merger of Manufacturers: A Case Study of Maytag-Whirlpool", *American Economic Journal: Economic Policy*, 5 (1): 239-61.

Ashenfelter O., D. Hosken and M. Weinberg, 2009, "Generating Evidence to Guide Merger Enforcement", *Competition Policy International*, Volume 5, No. 1.

Beck J., 2004, "Fixed, Focal, Fair? Book Prices Under Optional Resale Price Maintenance", *WZB Working Paper*, SP II 2004 – 15.

Beck J., 2007, "The Sales effect of Word of Mouth: A Model for Creative Goods and Estimates for Novels", *Journal of Cultural Economics*, 3(1): 5-23.

Björnerstedt J. and F. Verboven, 2012, "Does Merger Simulation Work? A "Natural Experiment" in the Swedish Analgesics Market", *CEPR Discussion Paper* No. DP9027.

Canoy et al., 2006, "The Economics of Books", in *Handbook of the Economics of Art and Culture*, by Ginsburgh V. and C. D. Throsby (eds.), vol. 1, pp. 721-761.

Chandra A. and A. Collard-Wexler, 2009, "Mergers in Two-Sided Markets: An Application to the Canadian Newspaper Industry", *Journal of Economics and Management Strategy*, 18(4), 1045-1070.

Chevalier J.A. and D. Mayzlin, 2006, "The Effect of Word of Mouth on Sales", *Journal of Marketing Research*, 43 (3): 345-354.

Choné P. and L. Linnemer, 2012, "A Treatment Effect Method for Merger Analysis with an Application to Parking Prices in Paris", *Journal of Industrial Economics*, 60 (4): 631-656.

Clark G. and A. Phillips, 2008, *Inside Book Publishing*, Routledge.

Clay K., R. Krishnan, E. Wolff, D. Fernandes, 2002, "Retail Strategies on the Web: Price and Non-Price Competition in the Online Book Industry", *Journal of Industrial Economics*, 50 (3): 351-367.

Clement M., D. Propp, A. Rott, 2007, "Do Critics Make Bestsellers? Opinion Leaders and the Success of Books", *Journal of Media Economics*, 20 (2): 77-105.

Clerides S., 2002, "Book Value: Intertemporal Pricing and Quality Discrimination in the US Market for Books", *International Journal of Industrial Organization*, 20 (10): 1385–1408.

Competition Commission and Office of Fair Trading, 2011, "Commentary on Retail Mergers", available at www.oft.gov.uk/shared_oft/mergers/oft1305-ccV1a.pdf.

Davis P., 2005, "The Effect of Local Competition on Admission Prices in the U.S. Motion Picture Exhibition Market", *Journal of Law and Economics*, 48 (2): 677-707.

Davis P., 2006, "Spatial competition in retail markets: movie theaters", *The RAND Journal of Economics* 37 (4): 964-982.

Davies S. W., H. Coles, M. Olczak, C. Pike, and C. Wilson, 2004, "The benefits from competition: some illustrative UK cases," DTI Economics Paper No. 9.

Deneckere R. and C. Davidson, 1985, "Incentives to form coalitions with Bertrand competition", *Rand Journal of Economics*, 16 (4): 473-486.

Duso T., 2012, "A Decade of Ex-post Merger Policy Evaluations: A Progress Report," in Dan Sjöblom Ed., *More Pros and Cons of Merger Control*, Stockholm: Swedish Competition Authority, 125-187.

Farrell J., P.A. Pautler, and M.G. Vita, 2009, "Economics at the FTC: Retrospective Merger Analysis with a Focus on Hospitals", *Review of Industrial Organization*, 35 (4): 369-385.

Focarelli D. and F. Panetta, 2003, "Are Mergers Beneficial to Consumers? Evidence from the Market for Bank Deposits", *American Economic Review*, 93 (4): 1152-1172.

Friberg R. and A. Romahn, 2012, "Ex-Post Merger Review and Divestitures", mimeo, Stockholm School of Economics.

Hausman J. and J. Mackie-Mason (1988), "Price Discrimination and Patent Policy," *Rand Journal of Economics*, 19: 253-265.

Hastings J. S., 2004, "Vertical Relationships and Competition in Retail Gasoline Markets: Empirical Evidence from Contract Changes in Southern California", *American Economic Review*, 94 (1): 317-328.

Hosken D., L.M. Olson, and L.K. Smith, 2012, "Do Retail Mergers Affect Competition? Evidence from Grocery Retailing", Federal Trade Commission, Bureau of Economics, Working Paper No. 31.

Hunter G., G.K. Leonard, and S. Olley, 2008, "Merger Retrospective Studies: A Review", *Antitrust*, 23 (1): 34–41.

Inderst R. and G. Shaffer, 2007, "Retail Mergers, Buyer Power and Product Variety", *Economic Journal*, 117 (1): 45–67.

Kovacic W., 2009, "Assessing the Quality of Competition Policy: The Case of Horizontal Merger Enforcement," *Competition Policy International*, 5, 129–150.

McCabe M., 2002, "Journal Pricing and Mergers: A Portfolio Approach", *American Economic Review*, 92 (1): 259-269.

Nevo A. and M.D. Whinston, 2010, "Taking the Dogma out of Econometrics: Structural Modeling and Credible Inference", *Journal of Economic Perspectives*, 24 (2): 69-82.

Office of Fair Trading, 2008, "An evaluation of the impact upon productivity of ending resale price maintenance on books," http://www.oft.gov.uk/shared_oft/economic_research/oft981.pdf.

Pakes A., 2003, "A Reconsideration Of Hedonic Price Indexes With An Application To PC's", *American Economic Review*, 93 (5): 1578-1614.

Schmidt-Stölting C., E. Blömeke, and M. Clement, 2011, "Success Drivers of Fiction Books: An Empirical Analysis of Hardcover and Paperback Editions in Germany", *Journal of Media Economics*, 24 (1): 24-47.

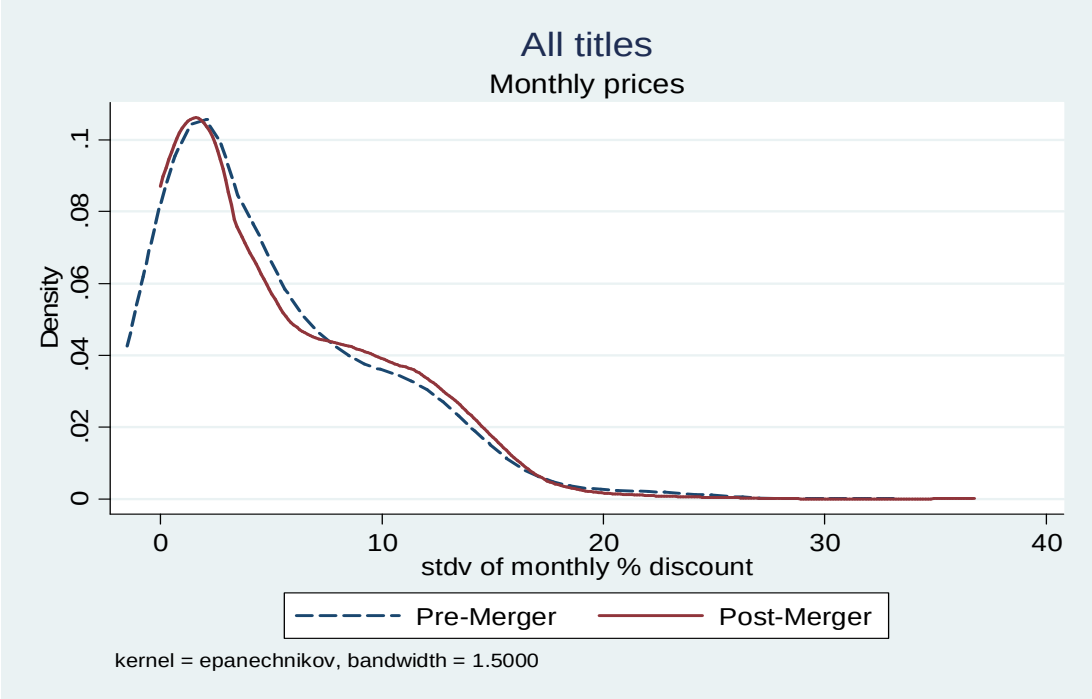
Skrainka B. S., 2012, "The Geography of Grocery Demand in the UK: An Evaluation of the 2003 Morrisons-Safeway Merger", mimeo, University of Chicago.

Sorensen A., 2007, "Bestseller Lists and Product Variety", *Journal of Industrial Economics*, 55 (4): 715-738.

Weinberg M., 2008, "The Price Effects of Horizontal Mergers", *Journal of Competition Law and Economics*, 4 (2): 433-447.

Figures and Tables

Figure 1: Distribution of the Monthly Standard Deviation: pre *vs.* post-merger



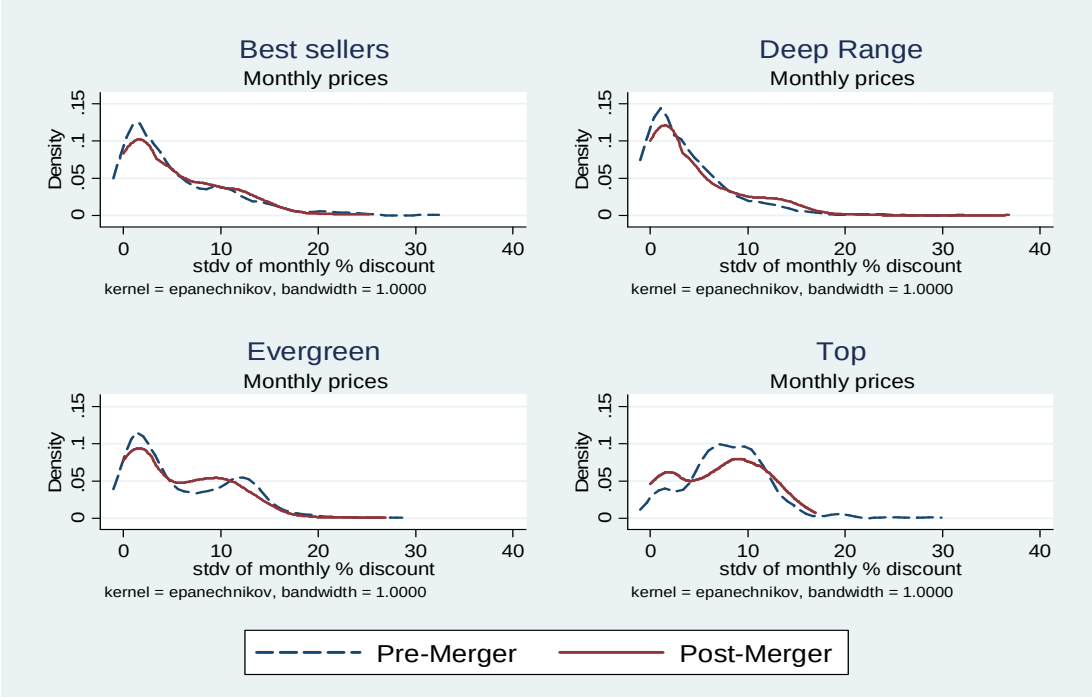


Figure 2: Distribution of the Monthly Discounts: Overlap vs. Non-overlap Locations

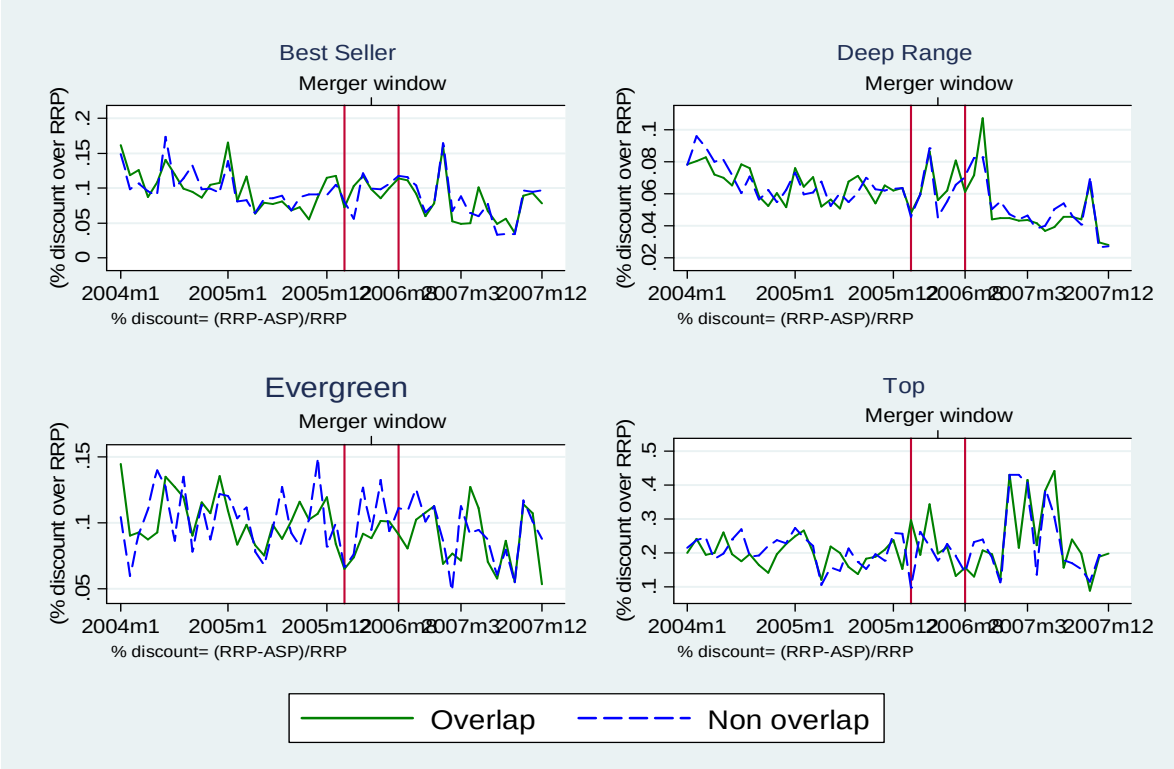


Figure 3: Distribution of monthly national discounts: merging parties *vs* competitors

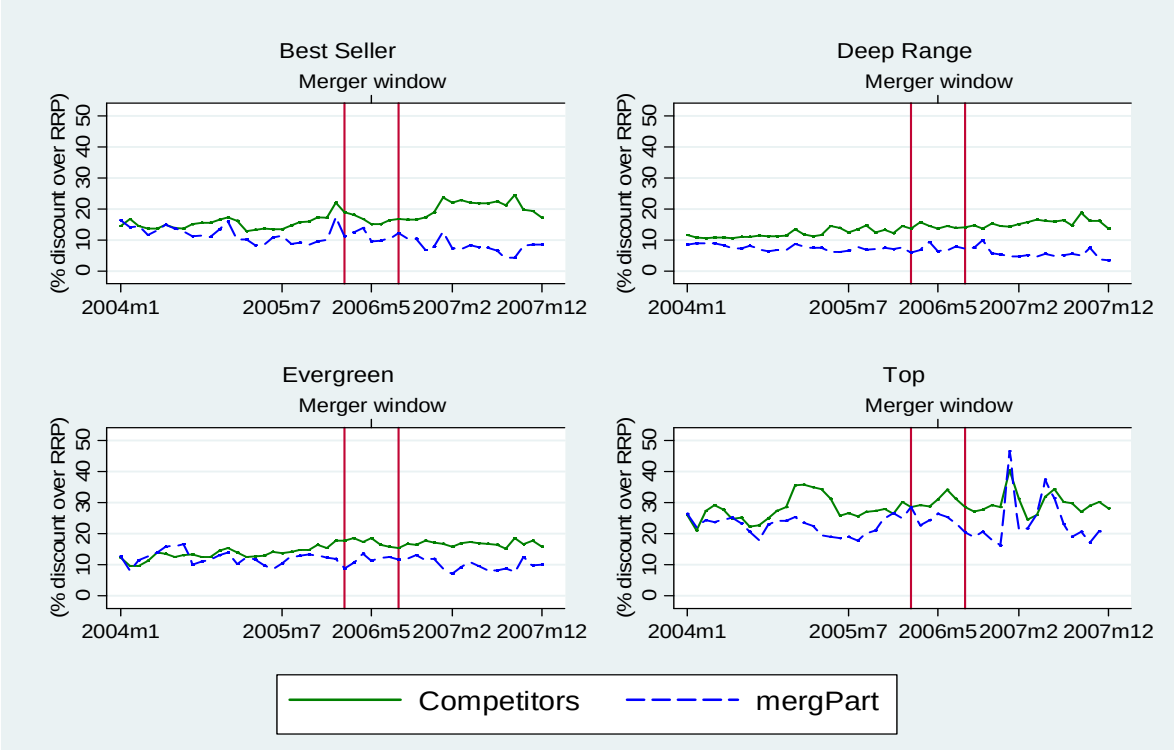
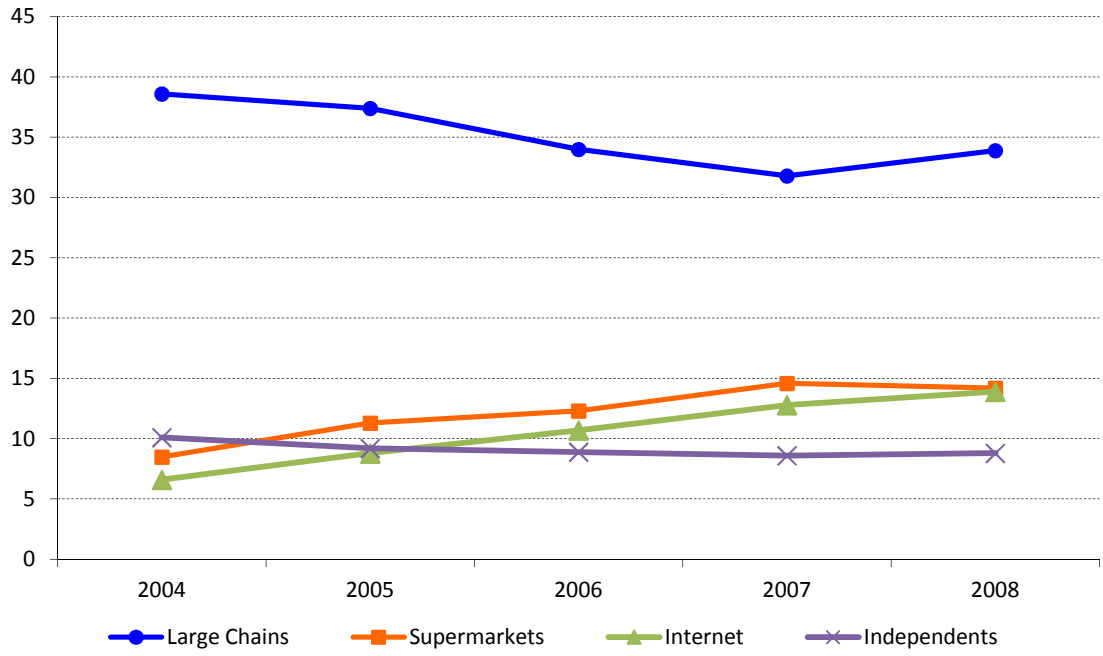


Figure 4: Retailing book market: market share by volumes



Source: *http://www.booksellers.org.uk/*

Figure 5: Average discounts by category – the merging parties

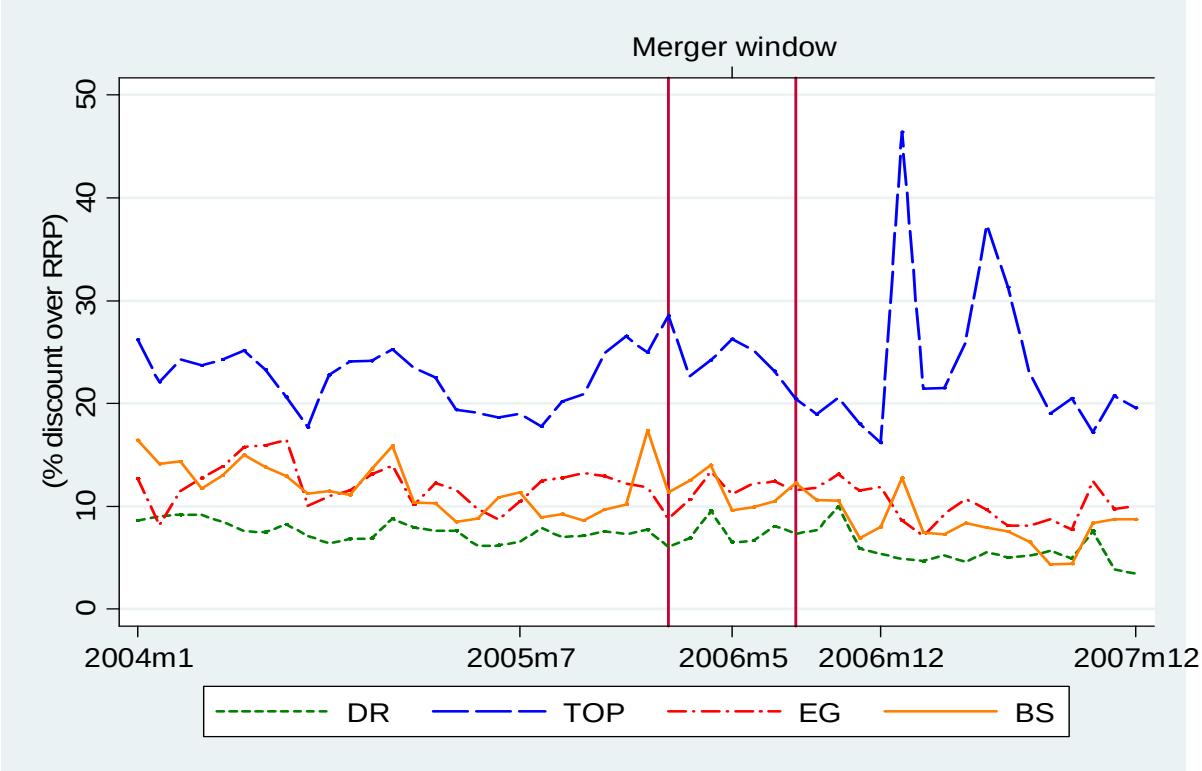


Table 1: National Market Shares of Retailers (based on the value of sales in 2005)

Firm	Market shares
Waterstone's	17%
Ottakar's	7%
Other specialist bookshops, including Borders and Blackwells	15%
Other stores, including WHSmith	19%
Supermarkets	8%
Internet	8%
Book clubs and other distance sellers	15%
Other	10%
Total	100%

Source: CC's calculations based on TNS and Nielsen Bookscan data.

Table 2: Number of titles by genre and type of binding

	Hardcover	Paperback	Total
Fiction	6	56	62
Specialist	-	20	20
Trade	16	50	66
Young	6	46	52
Total	28	172	200

Table 3: Number of titles by category and year

	Book Type				
	Bestseller	Deep-range	Evergreen	Top-sellers	Total
2004	40	76	20	18	154
2005	65	82	20	12	179
2006	54	106	20	11	191
2007	44	126	20	10	200

Table 4: Description of control variables

Variable	Description	Source
overlap	Dummy=1 if overlap area	
post	Dummy=1 if post merger (after June 2006)	
discount	[RRP – (sales values/sales volumes)]/RRP*100	Nielsen Bookscan
month_t	Monthly trend	
closed	Dummy=1 if Waterstone's or Ottakar's closed a shop in the area	
season	Seasonal dummy for Christmas period	
waterstone	Dummy for Waterstone's stores (before rebranding)	
trading_m1	Number of stores for specialist retailers	
trading_m2	Number of stores for non-specialist retailers	
trading_m3	Number of supermarkets including other retailers (e.g. DIY chains) that sell books as a part of a wide range of goods	
entry1	Number of stores for specialist retailers that have entered the market over the last three months	
entry2	Number of stores for non-specialist retailers that have entered over the last three months	
entry3	Number of supermarkets including other retailers (e.g. DIY chains), selling books as a part of a wide range of goods, that have entered over the last three months	
exit1	Number of stores for specialist retailers that have exited over the last three months	
exit2	Number of stores for non-specialist retailers that have exited over the last three months	
exit3	Number of supermarkets including other retailers (e.g. DIY chains), selling books as a part of a wide range of goods, that have exited over the last three months	
classD1, D2, D3,D4	Genre (D1=Fiction, D2= Specialist, D3=Trade, D4=Young)	
Series	Dummy=1 for titles which are part of a series	
Figure	Dummy=1 for titles containing	

	figures	
Pages	Number of pages	
elapsed_year	Years elapsed since the publication	
just_pub	Dummy=1 for the first 2 months after publication	
paperback	Dummy=1 for paperback titles	
avgsales_area	Average sale volumes of Waterstone's and Ottakar's stores per area (in 2005)	
woodpulp	Cost of paper	World Bank
internet	Internet penetration	Internet Access, Households and Individuals, Office of National Statistics
house_price	House prices	http://www.landregistry.gov.uk/ for England and Wales, and www.ros.gov.uk for Scotland
population	Population	UK Office for National Statistics, Neighbourhood Statistics section or Scottish Neighbourhood Statistics when relevant (2001 census)
pop_density	Population density	UK Office for National Statistics, Neighbourhood Statistics section or Scottish Neighbourhood Statistics when relevant (2001 census)
urban area	Dummy=1 for areas with number of inhabitants per hectare>1.5 (OECD definition)	UK Office for National Statistics, Neighbourhood Statistics section or Scottish Neighbourhood Statistics when relevant (2001 census)
universities	Number of universities	http://www.lovemytown.co.uk/Universities/UniversitiesTable1.asp
education	Average level of education measured using 7 levels as defined by the National Qualification Framework. These levels range from 1 (secondary education - GCSE- with marks below or equal to D) to 7 (Doctoral degree).	UK Office for National Statistics, Neighbourhood Statistics section or Scottish Neighbourhood Statistics when relevant (2001 census)
GVA	Gross value added. It measures the contribution to the economy of each individual producer, industry or sector. GVA = GDP–taxes on products +subsidies on products.	UK Office for National Statistics, Neighbourhood Statistics section or Scottish Neighbourhood Statistics when relevant (2001 census)

Table 4a: Descriptive statistics of control variables for local analysis

Variable	Obs.	Mean	Std. Dev.
overlap	207690	0.49	0.50
post	207690	0.39	0.49
discount	207690	10.58	12.93
month_t	207690	24.25	13.33
closed	207690	0.07	0.25
season	207690	0.19	0.40
waterstone	207690	0.54	0.50
trading_m1	207690	0.91	1.45
trading_m2	207690	9.58	7.19
trading_m3	207690	7.49	6.15
entry1	204013	0.02	0.12
entry2	204013	0.09	0.29
entry3	204013	0.05	0.22
exit1	204013	0.01	0.09
exit2	204013	0.04	0.20
exit3	204013	0.03	0.18
classD1	207690	0.31	0.46
classD2	207690	0.09	0.29
classD3	207690	0.28	0.45
classD4	207690	0.31	0.46
series	207690	0.35	0.48
figure	207690	0.51	0.50
pages	207690	301.71	236.79
elapsed_year	207690	3.34	3.47
just_pub	207690	0.07	0.25
paperback	207690	0.90	0.29
avgsales_area	207690	190506.50	86660.03
woodpulp	207690	5459.25	512.25
internet	207690	57.25	6.17
house_price	207690	195713.70	65260.84
population	207690	182723.40	102776.00
pop_density	207690	14.75	12.78
urban area	207690	0.86	0.34
universities	207690	0.85	0.73
education	207690	20.53	6.02
GVA	207690	17953.33	3979.17

Table 4b: Descriptive statistics of control variables for national analysis

Variable	Obs.	Mean	Std. Dev.
post	24236	0.46	0.50
discount	23461	11.27	11.56
month_t	24236	26.21	13.47
season	24236	0.17	0.37
waterstone	24236	0.33	0.47
classD1	24236	0.29	0.45
classD2	24236	0.11	0.31
classD3	24236	0.31	0.46
classD4	24236	0.29	0.45
series	24236	0.32	0.47
Figure	24236	0.53	0.50
Pages	24236	313.23	265.84
elapsed_year	24236	3.81	3.89
just_pub	24236	0.04	0.20
paperback	24236	0.88	0.32
woodpulp	24236	5527.23	527.41
internet	24236	56.52	4.18
house_price	24236	199573.00	15123.11
GDP per capita	24236	0.021	0.001

Table 5: DiD on Local Prices - Average Treatment Effect

	All titles (1)	Bestsellers (2)	Deep-range (3)	Evergreen (4)	Top-sellers (5)
Overlap		0.369	0.121	0.041	0.503
		(0.974)	(0.540)	(0.122)	(1.085)
Post	1.361***	0.987*	2.363***	-0.387	-3.469***
	(5.94)	(1.881)	(9.789)	(-1.015)	(-3.469)
overlap×post	-0.290	-0.061	-0.148	0.112	-0.147
	(-1.38)	(-0.119)	(-0.684)	(0.310)	(-0.202)
month_t	-0.117***	-0.221***	-0.116***	-0.009	-0.008
	(-10.77)	(-13.370)	(-14.410)	(-0.572)	(-0.329)
Constant	18.701***	10.88***	14.83***	5.596***	11.27***
	(5.77)	(4.604)	(11.23)	(2.844)	(3.408)
Observations	172,991	37,094	58,098	56,955	20,433
Number of id	11,833	4,544	6,909	2,445	2,930
R-squared	0.061				
Cluster	ISAN×ISBN	ISAN×ISBN	ISAN×ISBN	ISAN×ISBN	ISAN×ISBN
Effects (ISAN×ISBN)	Fixed	Random	Random	Random	Random

Notes: The dependent variable is the price discount. In all columns we control for the following variables (see Table 4 for the description of control variables): trading_m1, trading_m2, trading_m3, entry (1, 2, 3), exit (1, 2, 3), season, woodpulp, internet, the housing price, GVA, and elapsed_year. In the random effects specifications (columns 2 to 5) we additionally control for waterstone, avgsales_area, population, pop_density, urban area, universities, education, classD2, classD3, classD4, series, figure, pages, paperback. Robust t-statistics (column 1) and z-statistic (columns 2 to 5) in parentheses. The symbols ***, **, and * represent significance at the 1%, 5%, and 10% level respectively. ISAN is the Nielsen's unique identifier of a store while ISBN is the unique identifier of a title.

Table 6: DiD on Local Prices – Heterogeneous Treatment Effects

	All titles (1)	Bestsellers (2)	Deep-range (3)	Evergreen (4)	Top-sellers (5)
WATERSTONE'S/OTTAKAR'S					
overlap×post×Waterstones	-1.496***	-1.536**	-0.560**	-1.166***	-0.815
	(-5.750)	(-2.375)	(-2.118)	(-2.723)	(-0.942)
overlap×post×Ottakars	0.927***	1.540***	0.237	1.430***	0.542
	(3.710)	(2.771)	(0.905)	(3.364)	(0.650)
BOOKS PUBLISHED POST MERGER					
overlap×post×released_post×just_pub					1.648**
					(2.245)
overlap×post×released_post×non_just_pub					-2.248***
					(-2.816)
overlap×post×released_pre	-0.284	-0.097	-0.146	0.112	0.440
	(-1.35)	(-0.184)	(-0.674)	(0.310)	(0.331)
STORE CLOSURES					
overlap×post×closed	-0.363	0.183	0.146	0.166	-1.774
	(-0.78)	(0.182)	(0.298)	(0.224)	(-1.139)
overlap×post×non_closed	-0.282	-0.091	-0.192	0.105	0.039
	(-1.30)	(-0.174)	(-0.863)	(0.285)	(0.052)
SPECIALIZED + GENERICS					
overlap×post×high comp	-0.301	-0.087	-0.131	0.112	-0.630
	(-1.03)	(-0.133)	(-0.476)	(0.240)	(-0.682)
overlap×post×medium comp	-0.345	-0.409	-0.141	0.156	-0.204
	(-1.21)	(-0.620)	(-0.493)	(0.334)	(-0.230)
overlap×post×low comp	-0.155	0.703	-0.204	0.027	0.760
	(-0.44)	(0.863)	(-0.552)	(0.049)	(0.729)
SUPERMARKETS					
overlap×post×high comp	-0.067	0.156	-0.193	0.329	-1.284
	(-0.25)	(0.249)	(-0.705)	(0.717)	(-1.388)
overlap×post×medium comp	-0.357	-0.480	0.032	0.075	0.584
	(-1.04)	(-0.612)	(0.096)	(0.138)	(0.573)
overlap×post×low comp	-0.530*	-0.025	-0.239	-0.123	0.631
	(-1.77)	(-0.036)	(-0.791)	(-0.261)	(0.707)
ENTRY					
overlap×post×high entry	-0.866**	-0.365	-0.234	-0.050	-2.264
	(-1.97)	(-0.388)	(-0.561)	(-0.074)	(-1.599)
overlap×post×medium entry	-0.023	0.424	-0.391	0.317	-0.659
	(-0.07)	(0.589)	(-1.302)	(0.607)	(-0.639)
overlap×post×no entry	-0.275	-0.189	-0.002	0.065	0.514
	(-1.11)	(-0.322)	(-0.008)	(0.160)	(0.655)
EXIT					
overlap×post×medium exit	-0.601*	-0.212	-0.086	-0.338	0.035
	(-1.66)	(-0.269)	(-0.237)	(-0.607)	(0.032)
overlap×post×no exit	-0.195	-0.012	-0.169	0.245	-0.202
	(-0.87)	(-0.021)	(-0.742)	(0.645)	(-0.268)

Notes: The dependent variable is the price discount. We only report coefficients for the interaction variables that represent heterogeneous treatment effects. In all columns we control for the following variables (see Table 4 for the description of control variables): month_t, trading_m1, trading_m2, trading_m3, entry (1, 2, 3), exit (1, 2, 3), season, woodpulp, internet, the housing price, GVA, and elapsed_year. In the random effects specifications (columns 2 to 5) we additionally control for waterstone, avgsales_area, population, pop_density, urban area, universities, education, classD2, classD3, classD4, series, figure, pages, paperback. Robust t-statistics (column 1) and z-statistic (columns 2 to 5) in parentheses. The symbols ***, **, and * represent significance at the 1%, 5%, and 10% level respectively.

Table 7: DiD on National Prices – Average Treatment Effect
Competitors as control group

	All titles (1)	Bestsellers (2)	Deep-range (3)	Evergreen (4)	Top-sellers (5)
merged		0.225	-3.861***	0.005	-1.483
		(0.078)	(-2.749)	(0.003)	(-0.526)
post	-0.732	-3.562	-0.360	-1.668	-4.795
	(-0.766)	(-1.538)	(-0.269)	(-1.262)	(-0.958)
post×merged	0.108	0.692	0.646	0.365	-2.751
	(0.089)	(0.229)	(0.421)	(0.176)	(-0.470)
month_t_merged	-0.134***	-0.349***	-0.109***	0.014	-0.445**
	(-3.294)	(-3.223)	(-2.845)	(0.129)	(-2.312)
month_t_comp	0.072*	-0.020	0.069	0.192***	-0.311*
	(1.922)	(-0.234)	(1.420)	(2.734)	(-1.652)
constant	218.100***	21.770	47.390**	60.62**	-146.500
	(3.318)	(0.485)	(2.165)	(2.047)	(-1.571)
observations	13,346	2,417	7,173	2,913	814
R-squared	0.064				
number of id	400	156	270	82	98
cluster	ISBN	ISBN	ISBN	ISBN	ISBN
Effects (ISBN×retailer)	Fixed	Random	Random	Random	Random

Notes: The dependent variable is the price discount. In all columns we control for the following variables (see Table 4 for the description of control variables): season, woodpulp, ip, avg_hp, gdp_pc, just_pub, and elapsed_year. In the random effects specifications (columns 2 to 5) we additionally control for pages, series, figure, paperback, classD2, classD3, classD4. Robust t-statistics (column 1) and z-statistic (columns 2 to 5) in parentheses. The symbols ***, **, and * represent significance at the 1%, 5%, and 10% level respectively.

Table 8: DiD on National Prices – Average Treatment Effect Top-selling Titles as Control Group

	Bestsellers	**Deep-range**	**Evergreen**
	(1)	**(2)**	**(3)**
non_top	-9.144***	-17.679***	-4.714**
	(-3.73)	(-7.32)	(-2.08)
post	-6.463	-4.812	-6.841
	(-1.35)	(-1.09)	(-1.59)
overlap×non_top	1.431	6.322	5.164
	(0.36)	(1.52)	(1.40)
month_t	-0.323**	-0.210***	0.016
	(-2.42)	(-3.82)	(0.10)
Constant	53.245	19.447	35.875
	(0.82)	(0.70)	(0.66)
Observations	1,526	3,457	1,696
number of id	127	184	90
Cluster	ISBN	ISBN	ISBN
Individual Effects	Random	Random	Random

Notes: The dependent variable is the price discount. In all columns we control for the following variables (see Table 4 for the description of control variables): season, woodpulp, ip, avg_hp, gdp_pc, just_pub, elapsed_year, pages, series, figure, paperback, classD2, classD3, classD4. Robust z-statistic are reported in parentheses. The symbols ***, **, and * represent significance at the 1%, 5%, and 10% level respectively.

Table 9: DiD on National Prices – Heterogeneous Treatment Effects
Competitors as control group

	All titles	Bestsellers	Deep-range	Evergreen	Top-sellers
	(1)	(2)	(3)	(4)	(5)
WATERSTONE'S/OTTAKAR'S					
post×merged×Waterstones	0.236	0.243	0.685	0.452	-1.301
	(0.210)	(0.088)	(0.453)	(0.248)	(-0.239)
post×merged×Ottakar	0.916	1.848	0.840	1.813	1.592
	(0.816)	(0.712)	(0.558)	(0.994)	(0.344)
BOOKS PUBLISHED POST MERGER					
post×merged×released_post×just_pub					8.571*
					(1.880)
post×merged×released_post×non_just_pub					3.353
					(0.696)
post×merged×released_pre					-5.850
					(-1.030)

Notes: The dependent variable is the price discount. We only report coefficients for the interaction variables that represent heterogeneous treatment effects. In all columns we control for the following variables (see Table 4 for the description of control variables): season, woodpulp, ip, avg_hp, gdp_pc, just_pub, and elapsed_year. We impose two distinct time trends for the merging parties and for competitors. In the random effects specifications (columns 2 to 5) we additionally control for pages, series, figure, paperback, classD2, classD3, classD4. Robust t-statistics (column 1) and z-statistic (columns 2 to 5) in parentheses. The symbols ***, **, and * represent significance at the 1%, 5%, and 10% level respectively.

Appendix

Appendix 1. Selection of areas using propensity score matching

This Appendix describes the methodology used in the selection of the areas. In its database, Nielsen Bookscan, Nielsen collects information on book sales from a wide panel of Waterstone's and Ottakar's stores. As regards the pre-merger period, this panel includes 359 Waterstone's and Ottakar's stores located in 203 different areas[49] (defined at the local authorities level), of which 33 were overlap areas (as defined by the CC) and 170 were non-overlap ones. To select the 60 stores for our analysis we followed an approach based on the *Propensity Score Matching* (henceforth PSM). PSM has its roots in the *Matching* literature and it was developed as a mean to correct for sample selection bias that may affect the estimate of the treatment effects. In non-experimental studies the assignment of subjects to the treatment and control groups is not random, thus the estimate of a causal effect obtained by comparing a treatment group with a non-experimental comparison group could be biased because of systematic differences between the two groups. In other words, units receiving treatment and those excluded from treatment may differ not only in their treatment status but also in other characteristics that affect both participation and the outcome of interest.

The bias can be reduced if the comparison of outcomes is performed using treated and control groups which are as similar as possible. It might be relatively simple to assign a comparison unit based on a single observable characteristic. However, if the matching process is to be effective in mitigating the potential bias, one needs to consider a full range of factors across which the treatment and control group might differ. PSM allows this matching problem to be reduced to a single dimension.

Under the PSM the degree of closeness among groups is measured by the *propensity score*, i.e. the probability of treatment, given a set of observed characteristics. The idea is that all relevant differences between the groups pre-treatment can be captured by observable characteristics in the data[50] and these characteristics can be used to estimate the *propensity score*. Through this approach a propensity score (which ranges from 0 to 1) is attached to every unit and the treatment and control group are then matched based on it.

[49] From our selection we excluded all the shops in the London area as, although both Waterstone's and Ottakar's operated stores in that area, it was not considered an overlap location by the CC.
[50] Once accounted for these differences, one can take assignment to treatment to have been random.

A fundamental requirement for this method is that the predicted probabilities of treatment, for control and treated units, must have a wide common support region, i.e. the existence of a substantial overlap between the propensity scores of control and treated units. In practice, we applied PSM accordingly to these steps: (1) Identify the relevant explanatory variables; (2) Estimate the predicted probability (*pscore*) of assignment to treatment for all areas; and (3) Match (without replacement) each treated area with the control area that has the closest *pscore*.

In the first step the aim is to select all the observable explanatory variables that characterize the book retailing market at the local level (hence, we need variables that vary at the local level). These variables can be broadly classified in two groups: (i) factors that may impact on the demand and (ii) factors that may affect the supply.

Using the above variables, we estimated the predicted probability of being in an overlap area running a logistic regression on the discrete dependent variable of treatment assignment.[51] The results from this regression can be found in Table A1.1.

[51] The dependent variable is the treatment status (overlap area = 1; non-overlap area = 0).

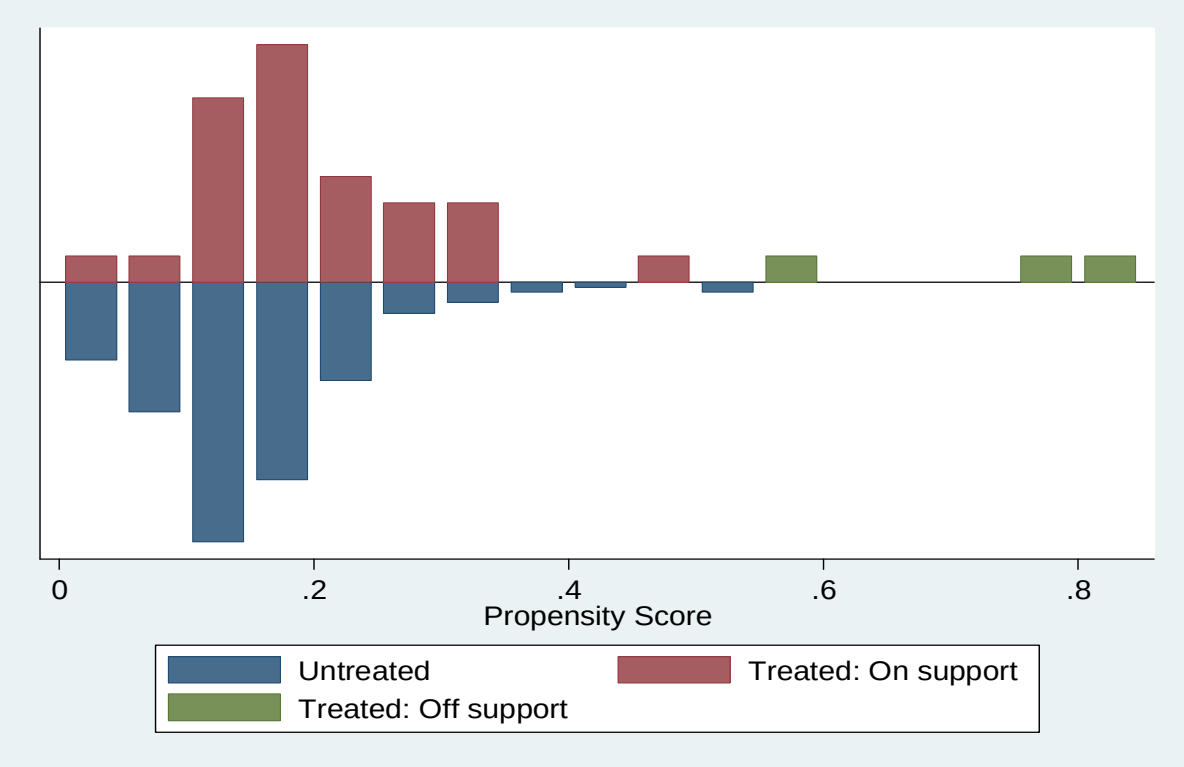
Table A1.1: **Propensity Score Matching, estimation results**

	dep variable: Overlap
Population	0.000001
	(0.27)
pop_density	-0.0002
	(-0.01)
Avgsales	0.0000006
	(0.09)
Universities	-0.00950
	(-0.01)
Education	-0.00379
	(-0.03)
GVA (2004)	0.00008
	(0.51)
Internet (2005)	-0.0419
	(-0.27)
house_price (2004)	-0.000005
	(-0.37)
trading_m1	-0.0170
	(-0.12)
trading_m3	-0.0189
	(-0.30)
Scotland	-0.412
	(-0.33)
Constant	1.659
	(0.22)
Observations	50

Notes: t-statistics in parentheses.

After the regression, each local areas is assigned a probability of treatment. By looking at the distribution of these predicted probabilities (see Figure A1.1) we can check if the common support requirement is satisfied. We conclude that there is substantial overlap and we are then reassured that we can find a sufficient number of treated local areas with a close enough match in the control group.

Figure A1.1: *Pscore* distribution by groups and common support



The selection of the treated areas was also constrained by data availability[52] and out of the 33 overlap local areas we could use only 20. For each of the 20 selected local areas we found the closest match in the non-overlap areas following the PSM approach. Table A1.2 presents the final list of areas from this matching process.

[52] Some shops closed, or were not surveyed by Nielsen Bookscan.

Table A1.2: Store Matching Outcome

	Treated			Control		
	Location	**pscore**	**store**	**location**	**pscore**	**store**
a) areas where we selected only a Waterstone's store	Southend-on-Sea	0.110	W	Oxford	0.111	W
	Worcester	0.269	W	Nottingham	0.256	W
	Canterbury	0.308	W	Bournemouth	0.300	W
	Kings Lynn	0.192	W	Bath	0.189	W
	Milton Keynes	0.102	W	Romford	0.103	W
b) areas where we selected only a Ottakar's store	Folkestone	0.222	O	Dumfries	0.222	O
	Bromley	0.033	O	Barnet	0.033	O
	Cheltenham	0.169	O	High Wycombe	0.171	O
	Guildford	0.162	O	Barnstaple	0.159	O
	Harrogate	0.115	O	Staines	0.115	O
c) areas where we selected both a Waterstone's and a Ottakar's store	Aberdeen	0.469	W	Bristol	0.508	W
	Aberdeen	0.469	O	Newport	0.382	O
	Chelmsford	0.285	W	Stirling	0.294	W
	Chelmsford	0.285	O	Elgin	0.279	O
	Coventry	0.167	W	Chichester	0.167	W
	Coventry	0.167	O	Newton Abbot	0.165	O
	Inverness	0.230	W	Winchester	0.233	W
	Inverness	0.230	O	Loughborough	0.230	O
	Huddersfield	0.071	W	Stockport	0.072	W
	Huddersfield	0.071	O	St Albans	0.071	O
	Crawley	0.203	W	Derby	0.202	W
	Crawley	0.203	O	Ashford	0.204	O
	Lancaster	0.198	W	Wolverhampton	0.194	W
	Lancaster	0.198	O	Andover	0.198	O
	Meadowhall	0.182	W	Stoke On Trent	0.188	W
	Meadowhall	0.182	O	Carlisle	0.186	O
	Norwich	0.309	W	Leicester	0.316	W
	Norwich	0.309	O	Aberystwyth	0.315	O
	Epsom	0.120	W	Bedford	0.116	W
	Epsom	0.120	O	Bishop's Stortford	0.123	O

A graphical representation of this selection can also be found in Figure A1.2, where it is possible to see that the matched overlap and non-overlap localities are equally spread around the UK (the only exception is for Wales, where there were no overlap areas).

Figure A1.2: Geographic distribution of Treatment and Control Areas

Treated local areas

Control local areas

For the selected localities we also tested the equality of means for the relevant explanatory variables and verified if the means across the two groups were not statistically different (see Table A1.3).

Table A1.3: Test on equality of means for explanatory variables

Variable	Mean		t-test	
	Treated	Control	t	p>t
Pscore	0.196	0.203	-0.26	0.793
Population	180000	170000	0.48	0.633
pop_density	14.051	13.62	0.11	0.912
Universities	0.75	0.73333	0.08	0.937
Education	19.9	20.6	-0.38	0.702
Avgsales	180000	180000	-0.01	0.989
GVA_2004	16876	16630	0.22	0.824
internet_2005	56.4	56.833	-0.39	0.702
house_price_2004	180000	190000	-0.61	0.548
trading_m1	6.15	6.3667	-0.15	0.883
trading_m3	15.15	14.767	0.11	0.916
Scotland	0.1	0.1	0	1

Appendix 2. List of book titles for Waterstone's/Ottakar's merger

Table A2.1 below lists the 200 titles on which we performed the econometric analyses.

Table A2.1: List of the titles included in our dataset

	Title	Author
1	7000 Baby Names: Classic and Modern	Spence, Hilary
2	Adventure of English, The	Bragg, Melvyn
3	Allen Carr's Easy Way to Stop Smoking	Carr, Allen
4	Amber the Orange Fairy: Rainbow Magic	Meadows, Daisy
5	Angel	Price, Katie
6	Angels: Miniature Editions	.
7	Animal Discovery Cards: Baby Einstein S.	Aigner-Clark, Julie
8	Art of Drawing Manga, The	Krefta, Ben
9	Atonement	McEwan, Ian
10	Bad Beginning, The: Series of Unfortunate Events	Snicket, Lemony
11	Bare Bones	Reichs, Kathy
12	Beginner's French: Teach Yourself Languages	Carpenter, Catrine
13	Bible Code, The	Drosnin, Michael
14	Blow Fly	Cornwell, Patricia
15	BMA Concise Guide to Medicines and Drugs	Henry, John A.
16	Body Double	Gerritsen, Tess
17	Body Shape Bible, The: Forget Your Size Discover Your Shape Transform Yourself	Constantine, Susanna
18	Bond Assessment Papers: Second Papers in Maths 8-9 Years: Bond Assessment Papers S.	Baines, Andrew & Bon
19	Broker, The	Grisham, John
20	Brother's Journey, A: Surviving a Childhood of Abuse	Pelzer, Richard B.
21	Brussels and Bruges: AA Citypacks	Franquet, Sylvie & S
22	Castle of Wizardry: Belgariad S.	Eddings, David
23	Cause of Death	Cornwell, Patricia
24	Change Your Life in Seven Days	McKenna, Paul
25	Chapter House Dune:(Bk. 6) :Gollancz S.F.	Herbert, Frank
26	Child Called It, A	Pelzer, Dave
27	Cigars of the Pharoah: The Adventures of Tintin S.	Herge
28	Coast	Somerville, Christop
29	Coming Out	Steel, Danielle
30	Complete Beginners' Cookbook	Watt, Fiona
31	Concise Colour Medical Dictionary: Oxford Paperback Reference S.	Martin, Elizabeth

	Title	Author
32	Concise Oxford Spanish Dictionary	.
33	Confusion, The	Stephenson, Neal
34	Contest	Reilly, Matthew
35	Cranks Recipe Book, The	Canter, David
36	Crucible, The: A Play in Four Acts: Penguin Modern Classics	Miller, Arthur
37	Curious Incident of the Dog in the Night-time, The	Haddon, Mark
38	Dark is the Moon: View from the Mirror S.	Irvine, Ian
39	Dark Tower,The: D rawing of the Three (Bk. 2)	King, Stephen
40	Devil's Disciples, The: The Life and Times of Hitler's Inner Circle	Read, Anthony
41	Diaries 1969-1979:The Python Years	Palin, Michael
42	Dr. Gillian McKeith's Ultimate Health Plan: The Diet Programme That Will Keep You Slim for Life	McKeith, Gillian
43	Duck: My Thomas Story Library	Awdry, W.
44	Elder Gods, The	Eddings, David & Edd
45	Electrician's Guide to the Building Regulations (Approved Document P, Electrical Safety in Dwellings)	.
46	Elegance	Tessaro, Kathleen
47	English Grammar in Use with Answers: A Self-study Reference and Practice Book for Intermediate Students of English	Murphy, Raymond
48	English Passengers	Kneale, Matthew
49	Enormous Crocodile, The	Dahl, Roald
50	Essential Costa Brava: AA Essential S.	Kelly, Tony
51	Essential Teaching Skills	Kyriacou, Chris
52	Face the Fire: Three Sisters Island	Roberts, Nora
53	Faithless	Slaughter, Karin
54	False Impression	Archer, Jeffrey
55	Farm: Usborne Look and Say	.
56	Filth	Welsh, Irvine
57	Flat Stanley in Space	Brown, Jeff
58	GCSE Double Science: Chemistry Revision Guide - Higher (Pt. 1 & 2)	Parsons, Richard
59	Girls Only! All About Periods and Growing-up Stuff	Parker, Victoria
60	Girls Out Late	Wilson, Jacqueline
61	Girls under Pressure	Wilson, Jacqueline
62	Good Night, Gorilla	Rathmann, Peggy
63	Gordon Ramsay's Playing with Fire: Raw, Rare to Well Done	Ramsay, Gordon
64	Great Lies to Tell Small Kids	Riley, Andy
65	Harry Potter Pbk Boxed Set	Rowling, J.K.
66	High Fidelity	Hornby, Nick

	Title	Author
67	High Hopes	Hopkins, Billy
68	Highest Tide, The	Lynch, Jim
69	Holy Bible, The: King James Version: Authorized King James Version	.
70	Holy Blood and the Holy Grail, The	Baigent, Michael & L
71	Horrid Henry and the Mega-mean Time Machine: (Bk. 13) : Horrid Henry	Simon, Francesca
72	Horrid Henry Meets the Queen: (Bk . 12) : Horrid Henry	Simon, Francesca
73	How to Boil an Egg:... And 184 Other Simple Recipes for One	Arkless, Jan
74	Humble Pie	Ramsay, Gordon
75	I Am Too Absolutely Small for School: Charlie & Lola	Child, Lauren
76	I Know You Got Soul	Clarkson, Jeremy
77	I Love Capri	Jones, Belinda
78	IEE on Site Guide (BS 7671: 2001 16th Edition Wiring Regulations Including Amendment 2: 2002)	.
79	"In the Night Garden" Little Library: Little Library: In the Night Garden	.
80	Innocent Graves	Robinson, Peter
81	Internet for Dummies, The: For Dummies S.	Levine, John R. & Yo
82	Introduction to Buddhism: An Explanation of the Buddhist Way of Life	Kelsang Gyatso, Gesh
83	Introductory Guide to Anatomy and Physiology, An	Tucker, Louise
84	Invisible Boy, The: Magical Children S.	Gardner, Sally
85	It's Not About the Bike: My Journey Back to Life	Armstrong, Lance
86	Jasper's Beanstalk: Jasper	Butterworth, Nick &
87	Jolly Postman, or, Other People's Letters, The: Or, Other People's Letters: Viking Kestrel picture books	Ahlberg, Allan & Ahl
88	Jonathan Strange and Mr. Norrell	Clarke, Susanna
89	Jose Mourinho: Made in Portugal - the Authorised Biography	Lourenco, Luis & Mou
90	Kalahari Typing School for Men,The:No.1 Ladies' Detective Agency S.	McCall Smith, Alexan
91	Kama Sutra, The: Great Sex S.	Hooper, Anne
92	Krakatoa: The Day the World Exploded	Winchester, Simon
93	KS1 Maths: Question Book (Pt. 1 & 2)	Parsons, Richard
94	KS2 Science: SAT's Practice Papers - Levels 3-5 (bookshop)	Parsons, Richard
95	KS3 Science: Revision Guide - Levels 5-7	Parsons, Richard & G
96	Last Juror, The	Grisham, John
97	Last Term at Malory Towers: Malory Towers S.	Blyton, Enid
98	Learning to Counsel: Develop the Skills You Need to Counsel Others	Sutton, Jan & Stewar
99	Letter from America:1946-2004	Cooke, Alistair
100	Little Miss Scary: Little Miss library	Hargreaves, Roger
101	Lord of the Rings, The: Return of the King (v.3)	Tolkien, J. R. R.
102	Lost for Words: The Mangling and Manipulating of the English Language	Humphrys, John

	Title	Author
103	Lovely Bones, The	Sebold, Alice
104	Low-Fat Meals in Minutes: "Australian Women's Weekly" Home Library	Tomnay, Susan
105	Magician's Nephew, The: Chronicles of Narnia S.	Lewis, C.S.
106	Mammoth Book of Extreme Science Fiction, The: Mammoth Book of S.	.
107	Man Called Cash, The: The Life, Love and Faith of an American Legend	Turner, Steve
108	Memoirs of a Geisha	Golden, Arthur
109	Monkey Puzzle	Donaldson, Julia
110	Moondust: In Search of the Men Who Fell to Earth	Smith, Andrew
111	Mr. Christmas	Hargreaves, Roger
112	Mr. Fussy: Mr. Men Library	Hargreaves, Roger
113	Mr. Perfect	Robinson, Catherine
114	Mr. Uppity: Mr. Men Library	Hargreaves, Roger
115	New First Aid in English, The	Maciver, Angus
116	New Pocket Dog Training	Fogle, Bruce
117	New Rector, The: Tales from Turnham Malpas	Shaw, Rebecca
118	Next Accident, The	Gardner, Lisa
119	Nursing Calculations	Gatford, J.D. & Phil
120	Nursing Practice: Hospital and Home - The Adult	Alexander, Margaret
121	Office 2003 in Easy Steps: Colour Edition:In Easy Steps S.	Copestake, Stephen
122	One Child	Hayden, Torey L.
123	One Hundred Ways for a Cat to Train Its Human	Haddon, Celia
124	One Hundred Years of Solitude	Garcia Marquez, Gabriel
125	Other Side of the Story, The	Keyes, Marian
126	Other Woman, The	Green, Jane
127	Oxford English Minidictionary	.
128	Oxford French Verbpack, The	.
129	Oxford Reading Tree: Stage 4: Storybooks: the Storm	Hunt, Roderick
130	Pale Horseman,The (Hardcover)	Cornwell, Bernard
131	Pale Horseman,The (Paperback)	Cornwell, Bernard
132	Peekaboo Farm!: Peekabooks S.	.
133	Philip's Motoring Atlas Britain 2006:Philip's Road Atlases	.
134	Philosophy: The Basics: Basics (Routledge Paperback)	Warburton, Nigel
135	Picking Up the Pieces	Britton, Paul
136	Pippi Longstocking	Lindgren, Astrid
137	"Playboy": Bartender's Guide	Mario, Thomas
138	Precious Time	James, Erica
139	Pregnancy Questions and Answer Book, The	Lees, Christoph & Re
140	Prince, The	Machiavelli, Niccolo

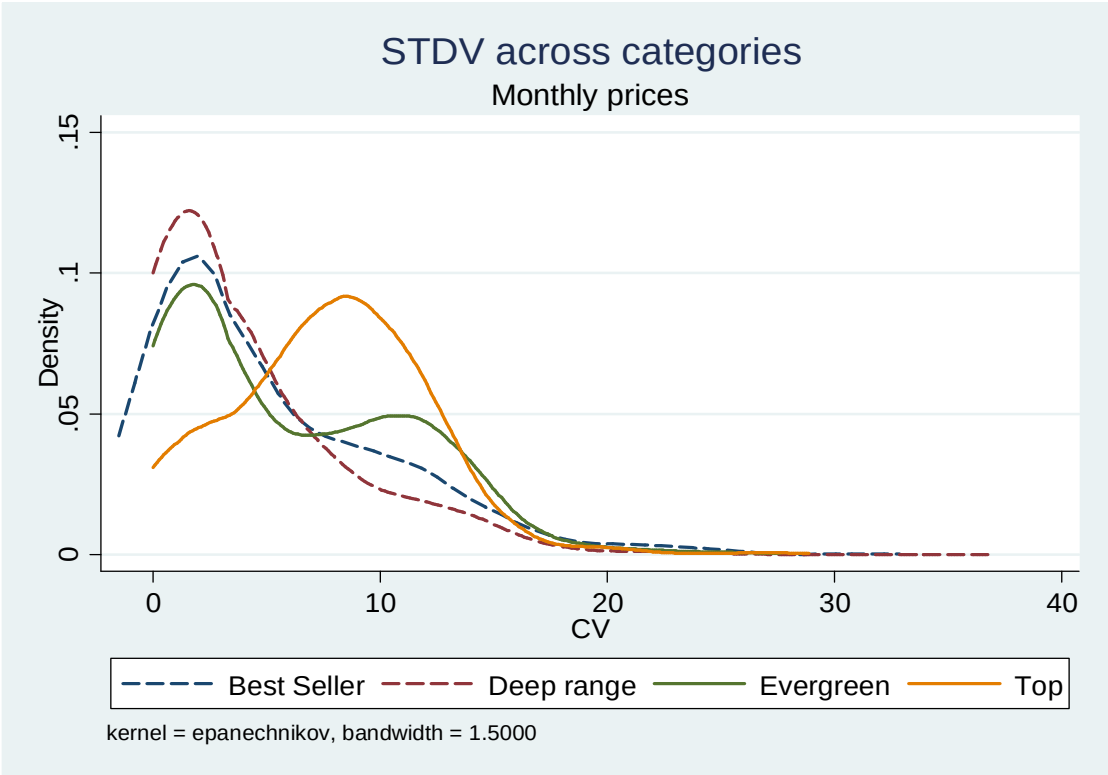
	Title	Author
141	Q Pootle 5	Butterworth, Nick
142	Quick Course in Microsoft Excel 2000:Quick Course	.
143	Really Rotten Experiments: Horrible Science S.	Arnold, Nick
144	Rebecca	Du Maurier, Daphne
145	Recoil	McNab, Andy
146	Restaurant Guide,The:2004:AA Lifestyle Guides	.
147	Revenge of the Middle-aged Woman	Buchan, Elizabeth
148	Rick Stein's Mediterranean Escapes	Stein, Rick
149	River Cottage Meat Book, The	Fearnley-Whittingsta
150	Ronnie: The Autobiography of Ronnie O'Sullivan	O'Sullivan, Ronnie
151	Rottweiler, The	Rendell, Ruth
152	Rough Guide to Venice, The: Rough Guide Travel Guides	Buckley, Jonathan
153	RSPB Pocket Birds	Elphick, Jonathan &
154	Rules of Management: The Definitive Guide to Managerial Success	Templar, Richard
155	Russian Dictionary: Collins GEM	.
156	Salisbury and The Plain, Amesbury:1: 50 000:OS Landranger Map	.
157	Savage Stone Age, The: Horrible Histories S.	Deary, Terry
158	Secret of Crickley Hall, The	Herbert, James
159	Sexual Life of Catherine M, The	Millet, Catherine
160	Sharon Osbourne Extreme: My Autobiography	Osbourne, Sharon
161	Shopaholic and Sister	Kinsella, Sophie
162	Silly Verse for Kids: Puffin Books	Milligan, Spike
163	Silver Spoon, The	.
164	Smelly Slugsy: Read-to-Me Scented Storybook: "Fifi and the Flowertots"	.
165	Social Work: Themes, Issues and Critical Debates	.
166	Sorceress	Rees, Celia
167	South Africa: AA Explorer S.	Shales, Melissa
168	Sovereign: Shardlake	Sansom, C.J.
169	Spanish Verb Tenses: Practice Makes Perfect Series	Richmond, Dorothy De
170	Storm of Swords, A: (1) :Song of Ice and Fire	Martin, George R.R.
171	SUMO (Shut Up, Move On):The Straight Talking Guide to Creating and Enjoying a Brilliant Life	McGee, Paul
172	Taking ,The	Koontz, Dean
173	Tao of Pooh and Te of Piglet, The: Wisdom of Pooh S.	Hoff, Benjamin
174	Thief of Time: A Discworld Novel	Pratchett, Terry
175	This Little Puffin: Finger Plays and Nursery Games:Puffin Books	Matterson, Elizabeth
176	Thousand Days in Venice, A: An Unexpected Romance	de Blasi, Marlena
177	Thud!: Discworld Novels	Pratchett, Terry

	Title	Author
178	Time and Chance	Penman, Sharon K.
179	Times Tables: Magical Skills (Level 2) :Magical skills	Fidge, Louis & Broad
180	Trojan Odyssey	Cussler, Clive
181	Truth, The: Discworld Novels	Pratchett, Terry
182	Twelfth Card, The	Deaver, Jeffery
183	Twilight Children: Three Voices No One Heard - Until Someone Listened	Hayden, Torey L.
184	Twist of Gold	Morpurgo, Michael
185	Twisted: Collected Stories of Jeffery Deaver	Deaver, Jeffery
186	Ultimate Dinosaur Glow in the Dark Sticker Book, The: Ultimate Stickers	.
187	Under Orders	Francis, Dick
188	Understanding Health and Social Care: An Introductory Reader: Published in Association with the Open University	.
189	Unlocking Formative Assessment: Practical Strategies for Enhancing Pupils' Learning in the Primary Classroom	Clarke, Shirley
190	Untouchable: Alpha Force S.	Ryan, Chris
191	Usborne Complete Book of Drawing: Usborne Activity Books	.
192	Vesuvius Club, The: A Lucifer Box Novel	Gatiss, Mark
193	Vieira: My Autobiography	Vieira, Patrick
194	Wasp Factory, The	Banks, Iain
195	Wedding Flowers: Over 80 Glorious Floral Designs for That Special Day	Roberts, Stephen
196	Wee Free Men, The	Pratchett, Terry
197	Wide Sargasso Sea: Student Edition: Penguin Modern Classics	Rhys, Jean
198	Wide Window, The: Series of Unfortunate Events	Snicket, Lemony
199	"York Notes on ""An Inspector Calls"": York Notes"	Scicluna, John
200	Yorkshire Dales: Walks: Pathfinder Guide	Conduit, Brian & Mar

Appendix 3. Local vs. national price competition for Waterstone's/Ottakar's merger

This Appendix presents some further results on the analysis of the geographic scope of price competition among book retailers. Figure A3.1 below compares the distribution of the discount's standard deviation (calculated over the entire period) for the four title categories.

Figure A3.1: Distribution of monthly standard deviation (comparison by title category)



As already stressed, the discount variability differs across categories. Top-selling titles and, to a lesser extent, evergreen titles have a high standard deviation, while the discount variability of bestsellers and deep-range titles appears to be lower and concentrated around 0. This latter result suggests that for these titles price competition mainly occurs at national level. However, we cannot say, in particular for deep-range titles, whether this low variability was due to a strict application of a centrally set pricing policy, or to the fact that local conditions did not vary much (for example, because the demand for deep-range titles was scarcely elastic over the entire nation).

To further investigate the issue of local price variation, we also examined the percentiles of the discount distribution. For each title in each month, we derived the percentiles of this distribution and analyzed them graphically. A higher vertical difference between percentiles would suggest higher dispersions of the discount across stores.

In the Figure A3.2 we plot a relatively narrow interval (percentiles 25th 50th and 75th), whereas in Figure A3.3 we plot a larger interval (percentiles 10th 50th and 90th).

Figure A3.2: Percentiles of the discount distribution (25th, 50th, and 75th)

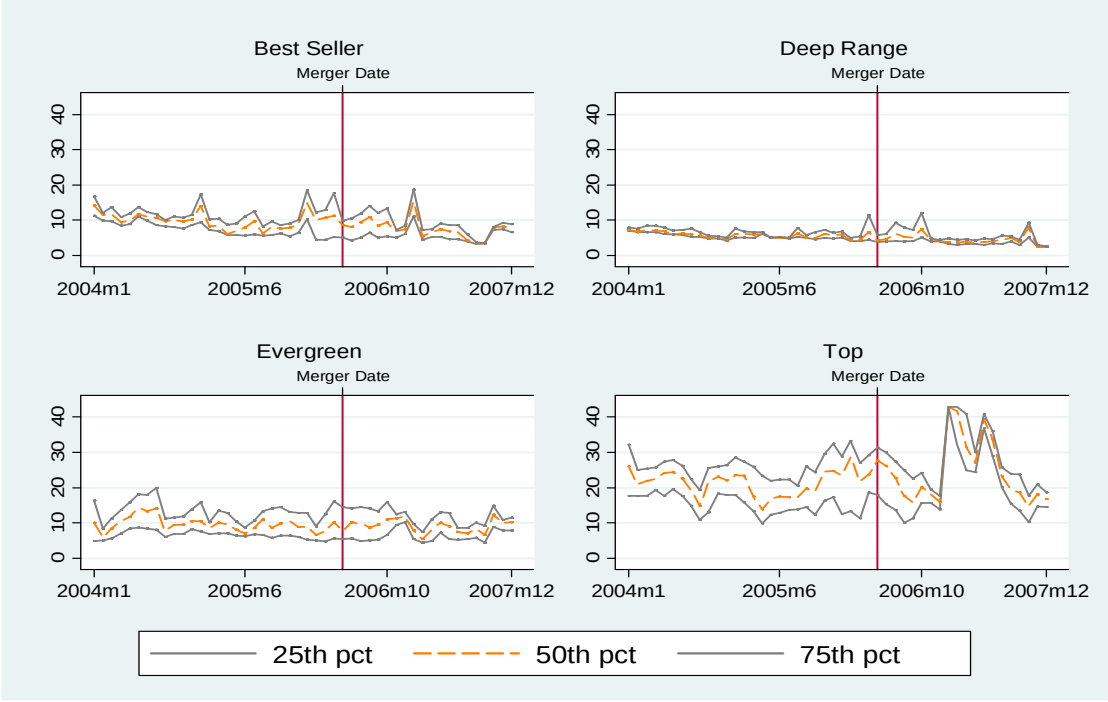
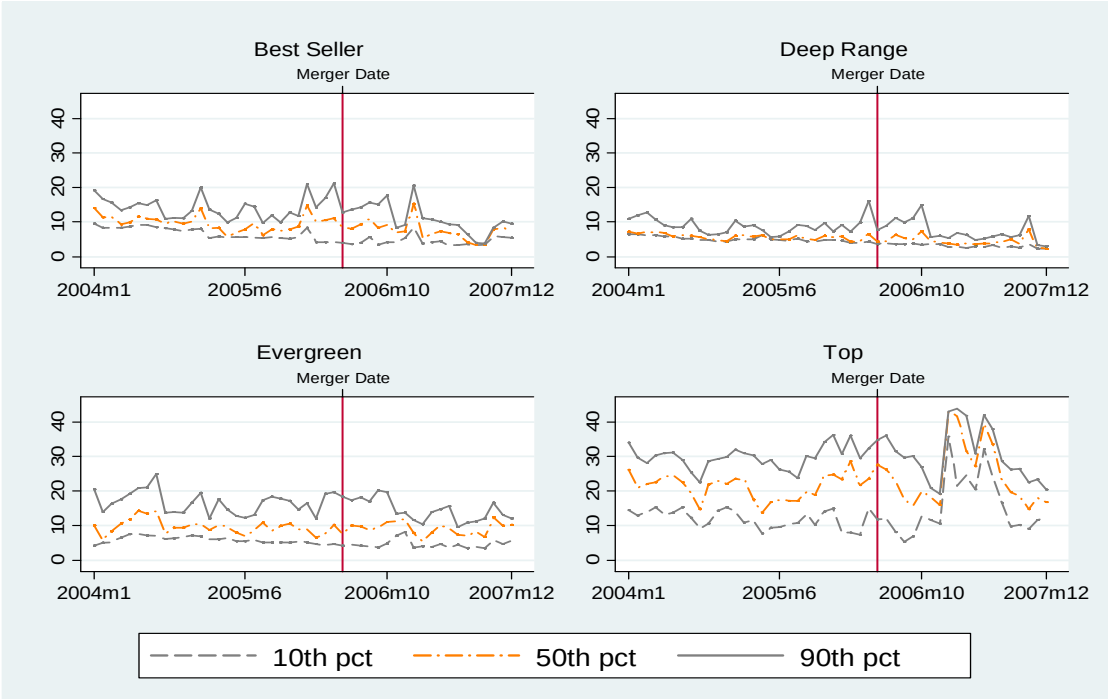


Figure A3.3: Percentiles of the discount distribution (10th, 50th, and 90th)

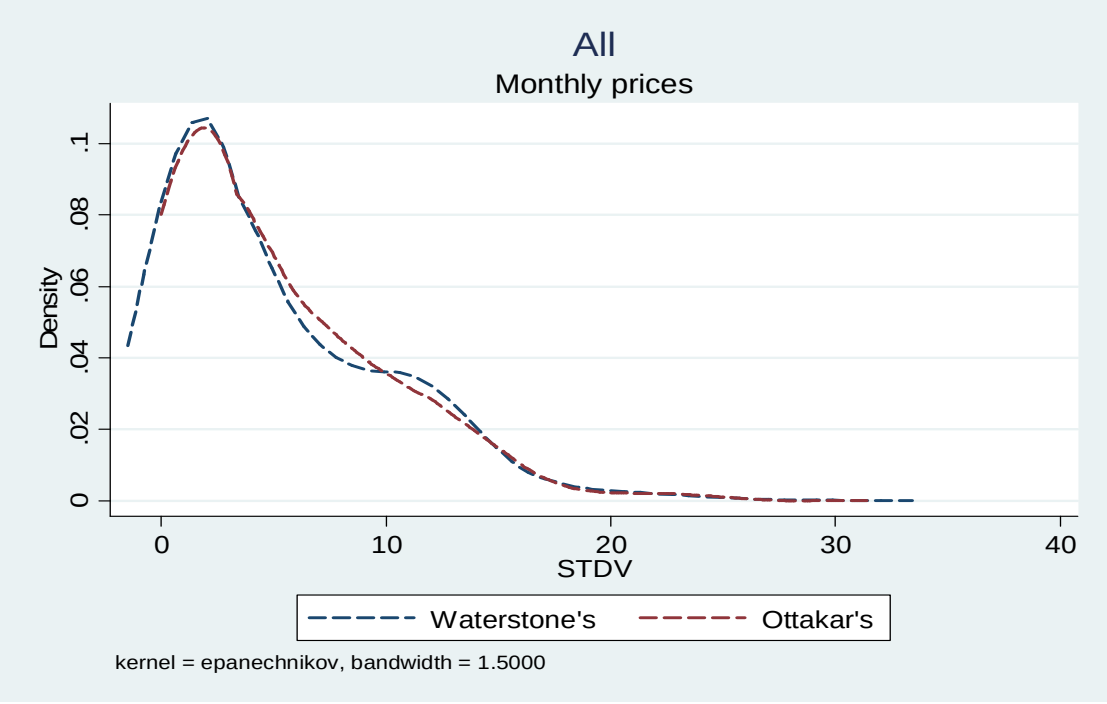


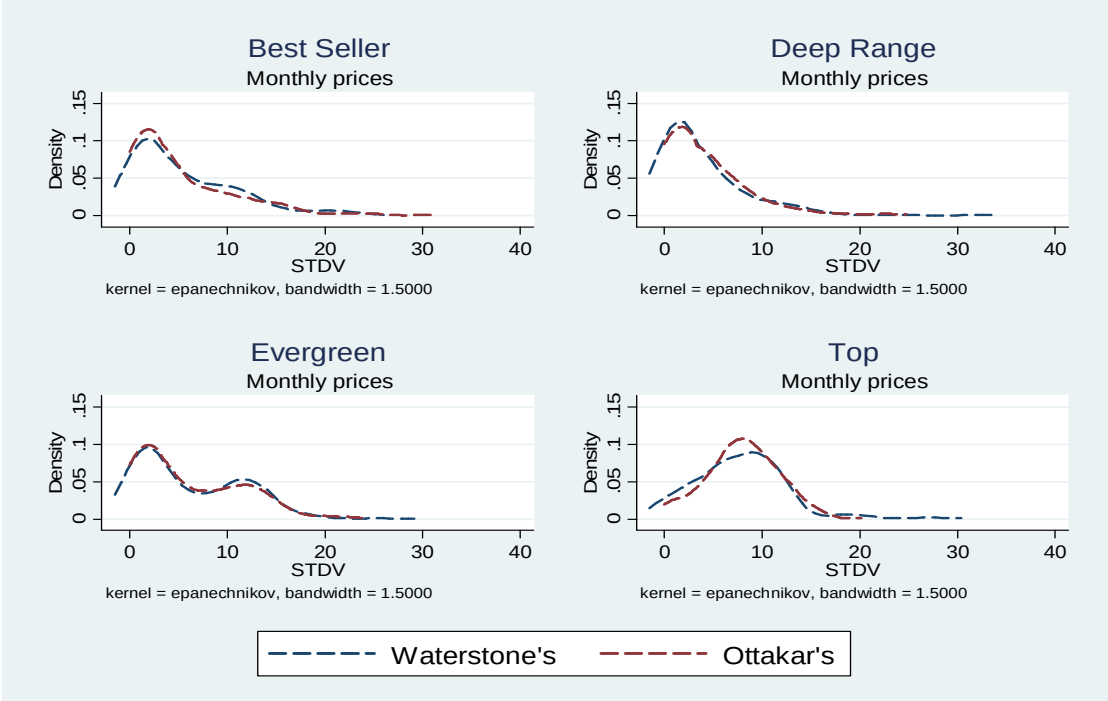
The percentiles analysis confirms the previous results. We observe a high variability for evergreen and top-selling titles, a lower one for best-sellers and a very low one for deep range titles.

Finally, we also verified whether there was any difference in the pricing policies adopted by Waterstone's and Ottakar's before the merger. This was done in order to check the opinions expressed by some market participants[53] who claimed that Ottakar's tended to have a more local-oriented pricing policy. Hence, we computed the discount variability across Waterstone's stores before and after the merger and compared it with the same figures for Ottakar's. In figure A3.4 we plot the distribution of the discount standard deviations across Waterstone's and Ottakar's stores before the merger.

[53] These opinions were expressed to both the CC during its inquiry and to us in the responses of our questionnaires.

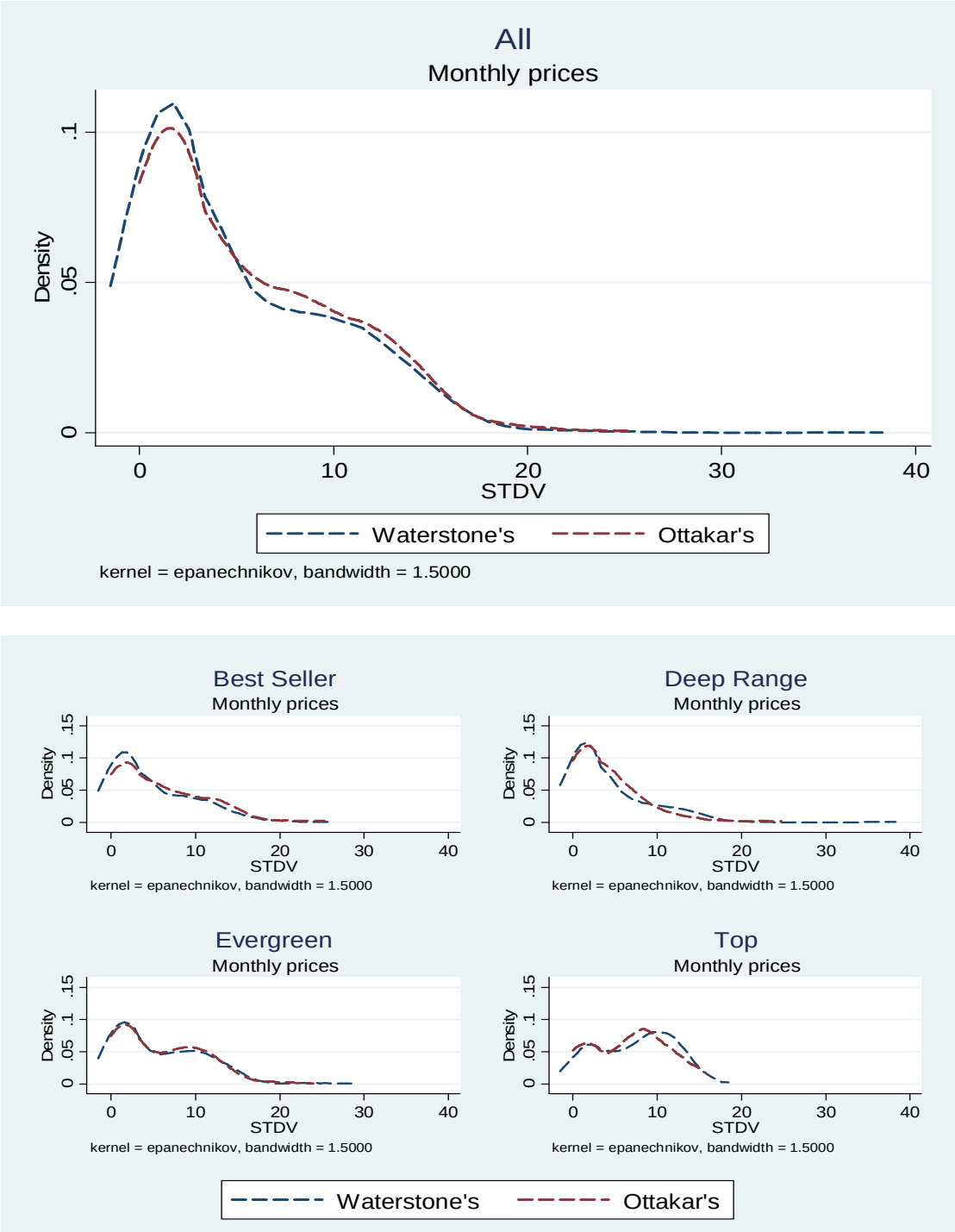
Figure A3.4: Distribution of monthly standard deviation before merger: Waterstone's vs Ottakar's





The graphical inspection shows no significant difference between the merged parties, suggesting that before the merger the extent to which Waterstone's and Ottakar's adopted local pricing was similar. As expected, this holds all the more for the discounts applied once the merger was consummated (see the figure A3.5 below).

Figure A3.5: Distribution of monthly standard deviation after the merger: Waterstone's vs Ottakar's



Appendix 4. Comparison of estimated effects using title/store fixed-effects and title/store characteristics

This Appendix presents a comparison of the estimated price effects of the merger under two different specifications. In the first one, we use product fixed-effects to control for unobserved heterogeneity, while in the second one we include a set of time invariant product characteristics that may affect a title's price and estimate a random-effects model. All specifications are estimated on the set of titles that are in the same category both before and after the merger, because it is only for these titles that the price effects are identified in the presence of product fixed-effects. We show this comparison for all categories but top-sellers, since very few titles are in this category both in the pre- merger and in the post-merger period, and therefore the fixed-effects specification is not feasible.

The upper part of table A4.1 shows the comparison for the local analysis. In this set of regressions, we use either title/store fixed-effects, or a random-effects specification with title and store characteristics. The fact that the estimate of treatment effect is similar under both specifications suggests that unobservable product characteristics do not result in bias of the estimator of the random-effect regression.[54]

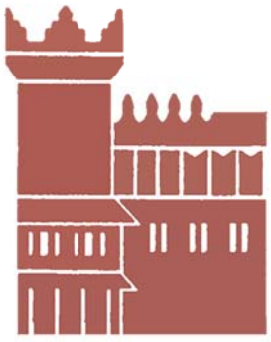
Table A4.1: Comparison of estimated effects using fixed-effects and product characteristics

	Deep-range		**Evergreen**		**Bestsellers**	
LOCAL ANALYSIS						
post×overlap	-0.245	-0.193	0.039	0.090	-0.701	-0.127
	(-1.02)	(-0.84)	(0.10)	(0.24)	(-1.11)	(-0.22)
Title/store fixed-effects	YES	NO	YES	NO	YES	NO
Title and Store Characteristics	NO	YES	NO	YES	NO	YES
Observations	53,577	53,577	57,974	57,974	16,912	16,912
NATIONAL ANALYSIS						
post×merged	0.814	0.798	0.365	0.386	2.301	2.375
	(0.522)	(0.510)	(0.176)	(0.186)	(0.781)	(0.801)
Title fixed-effects	YES	NO	YES	NO	YES	NO
Title Characteristics	NO	YES	NO	YES	NO	YES
Observations	6,422	6,422	2,913	2,913	1,041	1,041

Notes: In the local analysis, the title and store characteristics used are: waterstone, avgsales_area, population, pop_density, urban area, universities, education, classD2, classD3, classD4, series, figure, pages, paperback. In the national analysis, the title characteristics used are: pages, series, figure, paperback, classD2, classD3, classD4. Robust t-statistics (for fixed-effects specifications) and z-statistic (for random-effects specifications) in parentheses.

[54] The category that exhibits the larger difference in the two coefficients is the bestsellers'. However these results have to be taken more cautiously because only a small proportion of titles (27 out of 62) belong to this category both in the pre-merger and in the post-merger period.

The bottom part of table A4.1 shows instead the comparison for the national analysis, using competitors as the control group. Again, we compare a specification with title fixed-effects with a random-effects specification with title characteristics. Also in this set of regressions, the estimated coefficients are similar under the two different specifications.



Alma Mater Studiorum - Università di Bologna
DEPARTMENT OF ECONOMICS

Strada Maggiore 45
40125 Bologna - Italy
Tel. +39 051 2092604
Fax +39 051 2092664
http://www.dse.unibo.it