

# Ex Post Merger Evaluation in the UK Retail Market for Books

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**Abstract:** The paper empirically evaluates the effects of a merger between two book retail chains in the UK. We build an original dataset of book titles with price data at the store level and at the national level. We then apply difference-in-differences techniques to assess the impact of the merger. Since titles become obsolete very quickly, we compare different titles before and after the merger in an hedonic approach. We undertake an ex post assessment of the impact of the merger both at the local level and at the national level. At the local level, we compare the changes in the price charged before and after the merger in the shops located in areas where both chains were present before the merger (where the merger could be expected to generate the strongest effect) and in areas where only one chain was present before the merger. Our results do not show any significant difference these two types of areas. To investigate the effects of the merger at the national level, we employ two distinct control groups, namely the competitors and the top-selling titles. In both cases we find that the merger did not result in an increase in prices.

*Keywords:* Mergers, Ex post Evaluation, Book market, Retail sector

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## **1. Introduction**

Mergers and acquisitions are important strategies firms use to restructure and curb economic efficiency through the exploitation of synergies and economies of scale and scope. At the same time, horizontal combinations might create or reinforce market power, which harm consumers and total welfare. Once consummated, mergers have long-term implications for the structure and the competitiveness of the industries where they take place and are difficult and costly to be reversed (Ottaviani and Wickelgren, 2011). Therefore, merger control plays a central role in modern competition policy: it is supposed to accurately screen and modify mergers so to minimize their negative impact on effective competition and, hence, consumers welfare. Due to its very nature, however, merger review is a speculative exercise as authorities are asked to predict the possible impact of the combination before it takes place and speculate on the possible developments of the industry.

Whether this speculative exercise is successful and, hence, merger policy is correctly enforced has been the focus of discussion both in the academic discourse and in the policy arena for decades (e.g. Crandall and Winston, 2003). More recently, competition authorities around the world have begun to put more emphasis on ex post evaluation exercises as a way to inform and improve the future policy enforcement through a learning process as well as a tool to reach transparency and accountability. Several authorities attempted to conduct in house studies (e.g. FTC, 1999; DG Comp, 2005; OFT, 2005) or commissioned such studies to external consultants and academic advisors (e.g. PricewaterhouseCoopers, 2005; Deloitte 2009; Buccirosi et al. 2007). At the same time several academic contributions have appeared, which tried to approach the problem of merger policy evaluation from different angles: from a case-by-case specific analysis (Ashenfelter and Hosken, 2011; Ashenfelter, Hosken and Weinberg, 2011), to a broader analysis of the merger control enforcement in a jurisdiction over a long time period (Bergmann et al. 2003; Atkas et al. 2007; Duso et al. 2007, 2011), to the long term effects of the policy in terms of deterrence of particular merger behaviours (Seldeslachts et al., 2009; Clougherty and Seldeslachts, 2012). Methodologically, several empirical approaches and econometric techniques have been employed for the ex post evaluation of merger policy such as structural models and simulations, evaluation methods (difference-in-difference analysis), event studies, and surveys (Buccirosi et al., 2007).

In this study we follow one strand of this growing literature and evaluate the merger between two large UK books retailers, Waterstone's and Ottakar's, which had been cleared by the UK

Competition Commission (CC) in 2006.<sup>1</sup> The aim is to provide an additional methodological piece on how to possibly perform a precise ex post evaluation in one particular retail market which is characterized by goods which quickly change over time.

A key feature of retail mergers is that they may have either local or national effects (or both), according to the level at which retail chains set prices. We therefore assess the impact of the merger both locally and nationally since price competition between bookshops might take place at either level. From the methodological point of view, this also allows us to provide alternative instruments to assess the effect of mergers when the aggregation level of the data differs. At the local level, we employ a Difference-in-Difference (DiD) approach comparing the price evolution of a selected sample of the merging parties' titles in areas where the merging firms competed before the merger (overlap areas) with the price evolution of the same sample of titles in areas where only one chain was present (non-overlap areas). We do not find any 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. At the national level, we also employ a DiD approach. We estimate the impact of the merger on the price of the selected titles relying on two different control groups: i) the same titles sold by the competitors and ii) the books in the top title category<sup>2</sup>, which are expected to be less affected by the merger given the high degree of competition from other retailers such as supermarkets and the internet. In both cases we find that the merger did not result in a price increase at the national level.

We conclude that the merger per se does not appear to have had any impact on price competition, even though this is observed to increase during the sample period mostly due to structural changes in the industry and, in particular, to the rapid growth of low-cost retailers, such as online bookstores and supermarkets. Therefore, the CC's decision to approve the merger seems to have been the appropriate one if indeed competition occurs at the price level. However, the merger might also 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. 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.

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<sup>1</sup> See HMV Group plc / Ottakar's plc, 12 May 2006, available at: [http://www.competition-commission.org.uk/assets/competitioncommission/docs/pdf/non-inquiry/rep\\_pub/reports/2006/fulltext/513.pdf](http://www.competition-commission.org.uk/assets/competitioncommission/docs/pdf/non-inquiry/rep_pub/reports/2006/fulltext/513.pdf).

<sup>2</sup> In any given year "top titles" are the two hundreds most sold books.

We contribute to the literature on ex post merger evaluation in several respects. From the methodological point of view, we add to the strand of literature that employs a DiD approach for assessing the validity of merger decisions by applying it to a setting where the limitations of this approach are not very severe.<sup>3</sup> In particular, as Friberg and Romahn (2012) point out, one challenge in the application of such methodology is the difficulty of properly identifying a before-merger period and an after-merger period. In our setting, however, the definition of the timing of the merger does not pose particular problems, also because the merger was cleared without any remedies.<sup>4</sup> Moreover, as discussed above, the different aggregation level of our dataset allows us to perform the DiD exercise using different control groups, both at the local and at the national level, thereby alleviating another potential shortcoming of this methodology (i.e. problems due to the choice of an appropriate control group).

Another contribution of our paper to the literature on ex post merger review lies on the fact that, to the best of our knowledge, this is the first paper to perform an analysis of the effect of a merger in the retailing sector. Merger activity in retailing industries has been relevant in recent years, but there is still little evidence on the effects of these mergers on prices and consumer welfare. The retail market for books is particularly interesting as a case study since it involves goods that have a very short life cycle. Books become obsolete very quickly: the novelty wears off and new products constantly appear on the markets. Hence, the price that consumers are willing to pay for a specific title might quickly decrease (often in a few months). On the other hand, we also observe that new top selling books are sometimes heavily discounted while deep range books are sold at the recommended retail price (RRP) with little promotions. Hence, books at different rankings are subjects to different marketing strategies also because best sellers and top books are sold not only by bookstores but also by supermarkets. Therefore, one cannot compare the same book in two different years if the same book does not generate the same interest.

From a methodological viewpoint, this feature of a short life cycle that the book market shares with other markets poses challenges to the empirical analysis.<sup>5</sup> In particular, the nature of the goods under examination does not allow us to employ a constant sample of products over time. For this reason, rather than only using the same titles before and after the merger, we

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<sup>3</sup> Other papers which pursued the DiD methodology in merger analysis are Focarelli and Panetta (2003); Hastings (2004); Chandra and Collard-Wexler (2009); Ashenfelter and Hosken (2010); Ashenfelter, Hosken and Weinberg (2011). See Weinberg (2008) and Hunter, Leonard and Olley (2008) for a survey.

<sup>4</sup> See Section 2.1 for further details on the merger.

<sup>5</sup> Another example of short-lived products are music records, movies and videogames (see Beck, 2007). We employed 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 (see Aguzzoni et al., 2011)

choose to compare different ones, but with very similar characteristics. Any difference in price due to these characteristics can be controlled for by using the hedonic price approach which models prices as a function of the characteristics of the products.<sup>6</sup> This allows us to compare the prices of products that vary over time and still identify the effect of a policy change (the merger decision) on these prices, since the regression accounts for the changes in the characteristics of the products that may impact on prices. A similar problem is faced by Ashenfelter, Hosken and Weinberg (2011), 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 which is representative of the entire population of titles in terms of observable characteristics.<sup>7</sup>

In the next Section we discuss the institutional setting, and in particular the characteristics of the book industry and the merger. We then present our empirical strategy in Section 3. Section 4 contains a description of our dataset. We discuss the results of our econometric analysis in Section 5. Finally, 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. They work with authors and produce the 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 represent 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) but also internet shops and other retailers.

Retailers can be classified into three broad groups:

- a) retailers specialized in the sales of books, as well as small independent bookshops;
- b) non-specialist retailers for which books are either an important category, or for which books are part of a wide range of goods, such as supermarkets and major multiples; and
- c) online only book retailers.

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<sup>6</sup> See Pakes (2003).

<sup>7</sup> See Section 4 for a description of the criteria adopted for the selection of post-merger titles.

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 (mainly best-selling) titles.

In the retail book market, pricing takes the form of setting the level of the discount off the recommended retail price (RRP), which is printed on the book by the publishers and acts as a ceiling for the retail price. Publishers generally set the RRPs according to estimates on what the market would bear (taking into account the expected discounts offered by retailers) and to cost-related demand shifters (type of binding, presence of colored images, etc.).<sup>8</sup>

In the UK market, discounts are generally larger for bestsellers than for deep-range titles. In 2005, the average discount on RRPs, across all retailers, was equal to 40% for the former category and 10% for the latter.<sup>9</sup> Our empirical framework enables us to analyze pricing strategies by title category, where categories are defined on the basis of sale ranking.<sup>10</sup>

The prices offered by retailers are, to some extent, influenced by the discounts they are able to negotiate upstream. In general, independent bookshops receive the lowest discounts and supermarkets and book clubs the highest. 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 and bestseller lists in newspapers and magazines, direct advertising to consumers, and publicity events (e.g. book signings and author readings).<sup>11</sup>

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 for book retailers in 2005, the year when the merger was announced.

[Insert Table 1 here]

The main trends, up to 2005, had 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). Anecdotal evidence from a survey that we ran on market participants suggests that growing pressure from online retailers from

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<sup>8</sup> 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.

<sup>9</sup> See HMV Group plc / Ottakar's plc, 12 May 2006, available at: [http://www.competition-commission.org.uk/assets/competitioncommission/docs/pdf/non-inquiry/rep\\_pub/reports/2006/fulltext/513.pdf](http://www.competition-commission.org.uk/assets/competitioncommission/docs/pdf/non-inquiry/rep_pub/reports/2006/fulltext/513.pdf).

<sup>10</sup> See Section 4.2 for a definition of the categories used in our empirical analysis.

<sup>11</sup> See Sorensen (2007) for an assessment of the impact of bestseller lists on book sales.

supermarkets in the years under examination.<sup>12</sup> This seems to be due to their aggressive discounts policy.<sup>13</sup>

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 were considered).

## **2.1 The Merger**

On August 2005 two of the major book retailers, Waterstone's and Ottakar's, announced their willingness to merge. The Competition Commission opened a merger procedure and, on May 12th 2006, cleared the merger unconditionally. At the time of the merger, Waterstone's was controlled by the HMV Group.<sup>14</sup> Waterstone's, the book-retailing segment, had 190 stores in the UK with a selection of titles generally carrying between 30,000 and 40,000. Ottakar's was established in 1987 with the aim to create a chain of bookshops in market towns throughout the UK. Since then, it had grown both organically and through acquisitions and, on December 31st 2005, it had 141 stores carrying 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 to further segment the market between best-sellers (the 5,000 top-selling titles in a given calendar year) and deep-range titles (the remaining titles). The CC found evidence that the competitive conditions on the two segments could differ as supermarkets and internet retailers' commercial offer focused on best sellers, but it then rejected this definition since there were no retailers selling only deep-range titles and the distinction between deep-range and best-seller titles was somewhat arbitrary.

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 found 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 CC analyzed the possible difference in range at the parties' stores in overlapping locations with respect to non-overlapping ones but it concluded the presence of a competing

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<sup>12</sup> See Appendix 4.

<sup>13</sup> We cannot investigate this issue since we only have aggregate data on all competing retailers, including specialist ones.

<sup>14</sup> 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.

Waterstone's or Ottakar's store nearby made no difference on the size of the range stocked, and this was mostly determined by the size of the store.

The CC also looked at several other factors potentially affecting the service quality within stores: number of staff, level of staff experience, book signings, opening hours, and the number and timing of refurbishments relative to competitor store openings. It found noticeable differences between overlap and non-overlap locations only in relation to book signings and store refurbishments but, based on the results of customer surveys, it concluded that these were not key competitive variables since not central to a bookshop's offer for most consumers.

### **3. Empirical Strategy**

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 price 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.

During its investigation, the CC analyzed the geographic extent of price competition and concluded that Waterstone's and Ottakar's stores applied uniform national prices before the merger. Nonetheless, we want to allow for the possibility that the parties had moved to a locally-based price competition following the merger. Since our choice of the empirical strategy depends on whether competition, over the period examined, took place at the local or at the national level, as a preliminary step we evaluate the geographic extent of price competition. To address this issue we undertake a statistical analysis of price variability across Waterstone's and Ottakar's stores (see Section 5.1). As we show below, the results are not conclusive. We find some variability, which indicates that prices are to some extent set on the basis of the local market conditions. Yet, the degree of this variability is limited and may be due, at least partially, to the presence of bundle sales<sup>15</sup>. Hence, as we could not establish the exact geographic dimension of price competition, we carry out two distinct analyses: one on discounts at the local level and one on discounts aggregated nationally. For the purpose of

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<sup>15</sup> When two or more books are sold in bundles ("buy 3 pay 2") the size of the discount attributed to each book could be somehow arbitrary as it depends on the bundle itself. Hence two bookstores adopting the same bundle policy might register different discounts for the same book depending on the type of bundles sold.

this paper, this allows us to show how one can run different types of empirical exercises when the data availability varies in the degree of aggregation.

### 3.1 Local Competition: The Empirical Strategy

To evaluate the merger's effect on discounts at the local level we carry out a DiD analysis in which we compare the change in discounts before and after the merger in the overlap areas (the treatment group) with the change in discounts over the same period in the non-overlap locations (the control group). 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. We therefore adopt the following general estimation equation:<sup>16</sup>

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

where  $disc_{ist}$  is the discount on the recommended retail price on title  $i$  granted in store  $s$  at time  $t$ ,  $\alpha$  is a constant,  $post\_merger_{ist}$  is a dummy equal to 1 for the titles observed in the post-merger period and 0 before, while  $overlap_s$  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, and  $\varepsilon_{ist}$  is the error term.<sup>17</sup>

Our key variable is the interaction between  $post\_merger_{ist}$  and  $overlap_s$ , whose coefficient ( $\delta$ ) measures the price change in overlap locations relative to the price change in non-overlap areas. 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. In what follows we refer to the interaction variable as *TrEff*. The  $post\_merger$  coefficient ( $\beta$ ) measures any price change (between the pre-merger and the post-merger period) common to all locations, while the coefficient  $\gamma$ , related to the *overlap* regressor, accounts for any idiosyncratic differences between overlap and non-overlap areas that are not related to the merger.

### 3.2 National Competition: The Empirical Strategy

<sup>16</sup> We estimate this equation using either fixed effects or random effects and we include a time trend and a time fixed-effect. These different specifications are discussed below.

<sup>17</sup> We test for autocorrelation in the error process by means of the Wooldridge test. We find that the null hypothesis of no autocorrelation is strongly rejected. We thus control for this problem by clustering the error at 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.

As for the analysis at the local level, we perform a DiD exercise also to investigate the effects of the merger on prices at the national level. A major issue in implementing this analysis is the identification of a suitable control group. We employ two different ones. First, we use the prices charged by the rival firms.<sup>18</sup> 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. Hence this would suggest that the prices of the competitors are not a valid control group. Nonetheless, according to the theoretical findings of Deneckere and Davidson (1985), the merging parties should increase prices by more than rivals, i.e. reduce their discounts less. Thus, comparing the change in prices of the merging parties to that of the competitors may still provide a useful estimate of the effects of the merger. In this case the general estimation equation is:

$$disc_{ijt} = \alpha + \beta \cdot post\_merger_{ijt} + \lambda \cdot merged_j + \delta \cdot post\_merger_{ijt} \times merged_j + \gamma \cdot X_i + \mu \cdot Z_t + \varepsilon_{ijt} \quad (2)$$

where  $disc_{ijt}$  is the discount on the recommended retail price on title  $i$  granted by retailer  $j$  at time  $t$ ,  $\alpha$  is a constant,  $post\_merger_{ijt}$  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 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. As above, the key variable is the interaction between  $post\_merger_{ijt}$  and  $merged_j$ , whose coefficient ( $\delta$ ) measures the price change, attributable to the merger, of the merging parties relative to the price change of competitors.<sup>19</sup> In what follows we refer to this interaction variable as *TrEffl*.

The DiD approach relies on the key assumption that the treatment (the merging parties) and control group (the competitors) are subject to a same common trend. However, this might not

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<sup>18</sup> Ashenfelter et al. (2011) use a similar control group to identify the effect of the merger between Maytag and Whirlpool. However, since in their case the merging firms are manufacturers, their control group are rivals' products within each appliance category. In our case, instead, we compare the same titles sold by competing retailers.

<sup>19</sup> 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 nationally. In order to control for this problem, we cluster the error at the level of title.

be the case, as the analysis in Section 5.3 suggests. Hence, we also perform a DiD exercise in which we use a different control group, namely the top-selling titles.<sup>20</sup> The top-sellers appear to be the category where the merging parties face the greatest competition, as these titles are sold by all types of retailers and, in particular, by supermarkets which have the most aggressive pricing policy. Therefore, the merger could be expected to have produced no effects, or very limited ones, on the prices of these titles. The estimation equation is then as follows:

$$disc_{ikt} = \alpha + \beta \cdot post\_merger_{ikt} + \lambda \cdot titlecategory_k + \delta \cdot post\_merger_{ikt} \times titlecategory_k + \gamma \cdot X_i + \mu \cdot Z_t + \varepsilon_{ikt} \quad (3)$$

where  $disc_{ikt}$  is the discount on the recommended retail price on title  $i$  in category  $k$  at time  $t$ ,  $\alpha$  is a constant,  $post\_merger_{ikt}$  is a dummy equal to 1 for the titles observed in the post-merger period and 0 before for category  $k$ ,  $titlecategory_k$  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  $\delta$  captures the effect of merger on these latter book categories (i.e. evergreen, best-seller, and deep-range books).<sup>21</sup> In the rest of the paper we refer to the interaction between  $post\_merger_{ikt}$  and  $titlecategory_k$  as *TrEff2*. This equation is run only on the merging parties' prices.

Notwithstanding its suitability as control group, we are aware that the post-merger sample of top-selling titles is rather limited and, hence, that small variations on the discount on a single title (or very few ones) may artificially increase/decrease the average discount of the category. In order to address this potential problem, we also carry out a before-and-after analysis, where we compare the pre-merger and post-merger prices of Waterstone's and Ottakar's, while controlling for the cost and demand factors that may have affected their prices independently of the merger. Despite its own limitations,<sup>22</sup> we consider it useful to run also a before-and-after exercise to verify the results of the DiD analysis.

### 3.3 Methodological Issues

**Random vs. Fixed-effects.** Given the panel nature of our data, one of the major methodological issues we have to address is whether to use random or fixed-effects. Ideally we would like to run a regression with fixed effects for each title, so as to capture all the title-

<sup>20</sup> As we will explain in Section 4.2, we identify four categories to classify the titles, namely best-seller, deep-range, evergreen and top-seller. The latter includes the first 200 best-selling titles in any calendar year.

<sup>21</sup> The error term is clustered at the title level.

<sup>22</sup> Although in principle it is possible to control for all changes in observable economic factors other than the merger that may affect prices, there might be other factors that are not observable and therefore cannot be controlled for, which may lead to biased estimates.

specific (unobserved and observed) characteristics that may affect their prices. However, the use of title-specific fixed-effects has the drawback that the effect of the merger on discounts is identified only from titles sold both before and after the merger, because for the books whose prices were observed in only one period the interaction variable is perfectly collinear with the title fixed effects (i.e. it is time-invariant).<sup>23</sup> This may affect the estimates because it reduces the 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.<sup>24</sup>

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 (this is relevant only when we ran the category-specific regressions). The titles which belong to (i) are mainly top-selling titles and, in very few cases, best-sellers and deep-range books, while the titles which belong to (ii) the problem spans across all the categories, since it is frequent that a title moves down or up the ranking from one year to the next. To overcome this problem, when we run the regressions for each category of titles separately, we use a random-effect specification within an hedonic pricing approach. Hence, instead of the title-specific fixed-effects, we include a set of observable characteristics that may affect a title's price.<sup>25</sup>

However, when we consider the titles altogether, the fixed-effects problem is less of a 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 (20 out of 200 titles). Hence, in the pooled regressions we opt for a specification with fixed-effects at title level.<sup>26</sup>

**Merger window.** A further methodological issue we have to address is related to the selection of the window of data surrounding the merger to be excluded from the analysis, since we do

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<sup>23</sup> In other words, for those titles the fixed-effects specification does not allow us to disentangle how much the prices changed as a result of the merger because the variance of the interaction regressor is already captured by the title's fixed effect.

<sup>24</sup> This limit may be serious as, in principle, it is possible that the merged parties may have chosen to increase only the prices of the new titles introduced after the merger was consummated while maintaining the prices of those titles published before the merger.

<sup>25</sup> 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. (2011), we then run the regressions on the sample of titles published before the merger using both a random-effect and a fixed-effect specification. We find that the estimate of *TrEff* is similar under both specifications, thereby suggesting that unobservable product characteristics do not result in bias of the estimator of the random-effect regression.

<sup>26</sup> To assess the validity of the fixed-effects approach we perform two distinct checks. First, we run a regression with a random-effect specification and conduct a variation of the Hausman test to evaluate the appropriateness of the RE estimation. The test indicates a fixed-effects specification as preferable in this case. Second, we run the RE specification on the entire sample and on a reduced sample where the titles published after the merger are dropped. We do not observe any relevant difference in the significance and magnitude of the coefficients of interest, which further confirms that the FE specification is appropriate as it does not neglect crucial information.

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 and run all regressions using these different samples. We find that the results are essentially unaffected by the size of the window. We therefore use a window that drops the least number of observations, i.e. the one that excludes only the 3 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.<sup>27</sup>

## 4. The Data

To perform the analyses described above, we built two different datasets, one with data on discounts at store level and one with average national discounts. We acquired data from Nielsen on the volumes and values of the books sold by 60 of HMV's stores for over 200 titles. Nielsen provided us with weekly figures on these volumes and values (and with data on some book specific characteristics) for each selected title both at store level and aggregated at the national level. The following two sections describe how we selected the sample of stores and of titles.

We also gathered information on market characteristics (such as population, GDP, internet penetration, etc.) both at the local and the national level that we used to select the 60 stores covered in our analysis and as control variables in the econometric exercises. All this data was collected from public sources. A description of the control variables that we collected data for is provided in Section 4.3 below.

### 4.1. The Choice of the Stores

The DiD analysis at the local level requires us to identify the stores to include in the treatment and in the control groups, where the treatment group consists of Waterstone's and Ottakar's stores from overlap areas and the control group consists of Waterstone's and Ottakar's stores from non-overlap areas.<sup>28</sup> Ideally, the stores in the control group should closely resemble, in

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<sup>27</sup> We are aware the CC's clearance was for an anticipated merger. However, according to press reports, Ottakar's agreed the takeover offer from Waterstone's on May 31<sup>st</sup> 2006 and all Ottakar's stores were rebranded as Waterstone's by November of the same 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.

<sup>28</sup> In the definition of overlap and non-overlap areas we follow a similar approach to the one adopted by the CC: "[...] 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

terms of demand and supply conditions, the ones in the treatment group, so that any post-merger difference between the two groups could be attributed to the merger. Hence, we select two groups of shops with homogeneous observable characteristics, so that we can assume that the non-observable characteristics are similarly distributed.

We use price data on a total of 60 stores. Accordingly, we build a sample in which the number of Waterstone's and Ottakar's stores is equally split between overlap and non-overlap areas (i.e. 30 and 30). For the overlap areas, we draw the 30 stores from 20 different areas (out of the 33 overlap areas identified by the CC).<sup>29</sup> For 10 overlap areas we select one store for each chain. Then, to increase the coverage of overlap areas, we draw the other five Waterstone's stores from the other five areas and the last five Ottakar's stores from another different set of five areas. Hence, we cover a total of 20 different overlap areas.<sup>30</sup> 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.

Overall, 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 decided the number of areas and the type of stores required, we have to choose the method for the selection of specific areas and stores to be included in our sample. To this aim we follow an approach based on the so called Propensity Score Matching (PSM).<sup>31</sup> 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 using these predicted probabilities. The intuition of PSM applied to this case is the following. The treatment is the presence of both chains in the same area. We postulate that, although we observe only some "treated areas", the overlap and non-overlap areas may have similar probability of treatment as they share similar demand and supply conditions.

We consider a wide range of local market conditions (population, presence of universities, gross value added, internet penetration, average house prices, etc.) and, based on these, we estimate the predicted probability of treatment (propensity score). Then, for each treated (overlap) area we select the control (non-overlap) area that exhibits the closest propensity

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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, pag. 5).

<sup>29</sup> Cfr. Appendix E of the CC decision.

<sup>30</sup> Differently, we could have decided, for all areas, to draw both a Waterstone's and an Ottakar's store. However, this approach would have limited the analysis to only 15 areas (less than half of the 33 identified areas). We thus decided to increase the scope of our analysis and followed the above proposed selection method.

<sup>31</sup> For an introduction to Propensity Score Matching see Cameron and Trivedi (2005) p. 873.

score. Appendix 1 contains a more detailed description of the methodology, as well as the final list of stores resulting from the matching process.

#### **4.2. The Choice of the Titles**

During its inquiry, the CC considered whether there was a separate market for best-seller and deep-range titles.<sup>32</sup> Although it concluded that this was not the case, the CC recognized that the merger may have had a different impact on these titles because of the lower degree of competition that characterised the sale of deep-range titles. Indeed, while best-seller titles faced strong and growing competition, in particular, from supermarkets (and also from non-specialist and internet retailers), deep-range titles appeared less affected because supermarkets and non-specialists stores stocked only a small number of such titles. There is no evidence that the competitive framework between best-seller and deep-range titles has significantly changed since the merger (though supermarkets and non-specialist retailers apparently enlarged the range of the deep-range titles in stock in the recent years). Therefore, we have to account for this difference in our analysis.

In addition, we notice that the best-seller category includes a wide range of titles ranking from 1 to 5,000. These titles differ greatly with respect to the volume sold. For instance, in 2007 the volume of the 200 top-selling titles represented around 33% of the total sales of best-sellers with an average of more than 214,000 copies sold. On the other hand, the 200 books ranked from 4,800 to 5,000 represent just 1% of the total sales and show an average of 7,100 copies sold per title. This lead us to suspect that the retailers' pricing policy may significantly differ even within the best-seller category. In particular, since supermarkets, which are by far the strongest price competitors of the specialist retailers, tend to concentrate their offer on the very top-selling titles, the pricing strategy of the merging parties for these titles might be different from the one adopted on "standard" best-sellers. To capture these potential differences we define a "top-seller" category, which includes the first 200 top-selling titles for a particular calendar year.

We also identify a further set of titles that may show a pricing policy different from "standard" best-sellers, which we call "evergreen". Evergreen are those titles that are successfully sold for a long period of time. More specifically, this category includes those titles that were among the best-sellers for the entire period under analysis (2004-2007). Unlike

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<sup>32</sup> The CC defined best-seller books as the 5,000 top-selling titles for a particular calendar year and deep-range books as the titles ranked below 5,000. 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.

most best-sellers, that may perform well in terms of sales during one year and then fall out of the best seller group (i.e. be ranked below 5000) the next one, evergreen titles are sold in reasonable high volumes for many years and appear to be consistently among the best-sellers for a longer period of time. These titles might be in principle subject to a stronger competitive pressure compared to “standard” best-sellers, because they are traded in most of the retail channels. A possible explanation is that stocking evergreen titles might be less risky for those retailers that have limited shelf space for books (i.e. non-specialist retailers and supermarkets), as they can guarantee a quite stable flow of sales. As a result, our definition of best sellers is somewhat different from the CC’s one because it includes titles ranked 200 to 5,000 which are not evergreen.

It must be noted that the status of a title (i.e. whether it is a best-seller, a top-seller, an evergreen or a deep-range title) may vary over time.<sup>33</sup> For example, books that are best-sellers in a given year may become deep-range in the following year, similarly top-sellers may quickly fall down the ranking after the year of publication, thereby becoming best-seller or deep-range ones. 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 enter the sample after 2004.

Finally, 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.<sup>34</sup> 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.<sup>35</sup>

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<sup>33</sup> Clearly, with the exception of evergreen titles which are, by definition, best-sellers in every year.

<sup>34</sup> See Clerides (2002).

<sup>35</sup> 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.

We completed our dataset by collecting information for each title in the sample on a number of title-specific characteristics, namely the date of publication, the number of pages, whether it is part of a series and whether it contains figures.<sup>36</sup>

Subject to all the above criteria we asked Nielsen to randomly select 200 titles. The table below provides some statistics of the selected sample.

[insert Table 2 here]

Since the type-status of a title may vary from one year to another, the composition of the sample may change too. Given the concerns raised by the deep range titles in the CC's inquiry, we made sure that around 50% of the titles in each calendar year were deep-range, so as to have a representative sample of this category. The table below reports the number of titles included in each category for each year.<sup>37</sup> The annual size of the sample increases over time as titles published after 2004 are progressively added to the sample.

[insert Table 3 here]

Appendix 2 lists the 200 titles on which we perform the econometric analyses.

### 4.3. The Control Variables

Besides the title-specific control variables described in the previous subsection, we also built a large dataset of variables to control for demand and supply conditions. We consider several factors that may potentially affect demand and supply in the retailing book market. With respect to the demand-side, we collected information on (i) population; (ii) population density; (iii) average sales of books in volumes; (iv) gross value added; (v) number of universities; (vi) level of education. 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. With regard to the supply side, we gathered data on: (i) potential cost shifters (i.e. the cost of paper and average house prices<sup>38</sup>); (ii) a measure of the intensity of competition (i.e. the number of retailers operating in a given area); and (iii) the diffusion of internet sales (which we proxy through the level of internet penetration).

These variables were collected both at the local and the national level.<sup>39</sup> The table below provides a description of all these control variables.

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<sup>36</sup> If a book contains figures, publishers need to print coloured versions 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.

<sup>37</sup> Whenever a title could be classified both as evergreen and top-seller, we assume that the latter classification prevails.

<sup>38</sup> As a proxy of the cost incurred for opening and running a store.

<sup>39</sup> Except for the cost of paper (*woodpulp*) which is a commodity traded internationally (source: *World Bank*).

[insert Table 4 here]

#### 4.4. Other Data-Related Issues

**Categories.** As we show later, the different categories of titles seem to exhibit different discount patterns over time. Hence, a pooled regression on all titles might not capture the price movements specific to each category. For this reason we run the regression on each category separately. Nonetheless, we are aware that the classification of the titles we adopted is somewhat arbitrary and that it may not necessarily reflect exactly how retailers set their pricing strategies. Therefore, we also perform a pooled regression run on all the titles together.

**Data aggregation.** Throughout the analysis we aggregate data at the monthly level, even though Nielsen provided us with weekly figures. This choice was mainly driven by the fact that the weekly sales of a title relative to its monthly sales are lower and present a higher variation, which might introduce too much undue variation in the sample. In particular, when the volumes sold are low, the average price is more affected by typing errors and by the nature of the sales (i.e. whether stand-alone or part of a bundle). By aggregating at the monthly level, the average price is less affected by measurement errors or the presence of bundle sales.

### 5. Empirical Results

In this section we present the main results of the empirical analysis. Before doing that, we start by briefly discussing the issue of whether price competition is at the local or at the national level.

#### 5.1 Local vs. National Price Competition

To ascertain the geographic extent of price competition among retailers, 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.<sup>40</sup> For each title in a given month, we consider

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<sup>40</sup> 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 higher than the one applied to stand-alone purchases. This implies that the more titles a store sells through bundle-offers, the lower 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

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. If competition takes place mainly at the national level, one would expect low variability across stores and, thus, that the distribution of the standard deviation concentrates around 0. By contrast, if retailers compete locally, one would expect to observe a density function of the standard deviation which is more evenly distributed.

Figure 1 presents the distribution of the standard deviation of the discount across stores for all the titles together pre and post-merger. 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]

We also estimate the distribution function of the standard deviation for each of the four categories of titles (see Figure 2). We observe a significant difference in the distribution between the pre-merger and the post-merger period only for top-selling titles, but the variability is always relatively high. From Figure it also emerges that the discount variability seems to differ across categories over the whole period. This suggests that the geographic scope of the pricing policy of Waterstone's and Ottakar's might differ depending on the type of title. In particular, top-selling titles and, to a lesser extent, evergreen ones show a relatively marked standard deviation, which suggests that discounts were set locally. Instead, the discount variability of best-sellers and deep-range titles appears to be lower and to concentrate around 0, which may indicate that for these titles the parties tended to adopt a uniform pricing strategy across the UK.<sup>41</sup>

[insert Figure 2 here]

Two conclusions can be drawn from the previous analysis on the discount variability across stores. First, we do not find evidence of any relevant change between the pre-merger and post-merger situation with respect to the geographic scope of the pricing policies of Waterstone's and Ottakar's. Second, we observe some discount variability across stores which may indicate that price competition takes place at the local level to some extent both before as well as after the merger. However, the degree of local competition on discounts seems to vary significantly

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whether a book is sold stand-alone or as part of a bundle offer. Therefore, the results relative to the price dispersion across stores must be interpreted cautiously.

<sup>41</sup> This aspect was further investigated through the analysis of percentiles distribution (see Appendix 3).

across categories, with deep-range titles at the lowest extreme and top-selling titles at the highest. Overall, the evidence on whether price competition takes place at the local or at the national level cannot be considered conclusive. Hence, we will assess the effects of the merger on prices both on individual stores' prices and on nationally aggregated prices.

## 5.2 Local Price Competition

As a preliminary step, we plot the monthly average discounts in the overlap and non-overlap locations (see Figure 3). 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. We find that discounts in overlap areas post-merger were not systematically lower than those observed in non-overlap areas and, moreover, that they tended to follow broadly the same pattern. Interestingly, the discounts on best-sellers and deep-range titles seem to have slightly decreased over time. Such a trend apparently started well before the merger. Discounts on evergreen titles decreased over time as well, although the trend is less marked. Instead, the discount pattern for top-selling titles is less clear.<sup>42</sup>

[insert Figure 3 here]

Table 5 reports the results of the DiD regression (estimation equation 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 *TrEff* is not significantly different from zero. Therefore, the merger does not seem to have had a different impact in overlap and non overlap areas. We also consider whether the effects of the merger materializes only in the overlap areas where the merging parties close a store after the merger. The coefficient *TrEff\_closed*, which captures the effect of the merger in those areas, is not significant, thereby indicating that not even in those areas do we observe a systematic difference in the prices before the merger relative to those after the merger.

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 extensively discussed above, for these specifications we use a random-effect specification.

[insert Table 5 here]

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<sup>42</sup> 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 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.

Again, the terms of interest, i.e. the coefficients for *TrEff* and *TrEff\_closed*, 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. We find a significant and negative time trend for all categories, except the evergreen titles, which is broadly consistent with the graphical analysis. In addition, we also look for shifts in this trend after the merger (the *post\_merger* coefficient), and we find statistically significant effects only for deep-range titles, which exhibit a positive shift (+ 2,5%), and for top-selling titles, which have a negative shift (- 2%).<sup>43</sup> We do not extensively report results on all other control variables, which mostly conforms expectations.<sup>44</sup>

In conclusion, the merger does not seem to have adversely affected prices in the overlapping areas where it could have been expected to generate the strongest effects due to the increase in the level of market concentration. This does not rule out the possibility that the parties changed their pricing strategy at the national level as a result of the merger. In order to address this issue, in the next section we analyse the price data aggregated nationally.

### 5.3. National Price Competition

The analysis of the effect of the merger at the national level is conducted using a DiD methodology with two different groups as controls for permanent time-varying factors: (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. As a robustness check, we also perform a standard before-and-after analysis which compares the discounts applied by the merging parties before and after the merger.

We have data on the national volume and value for the 200 titles we selected, 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

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<sup>43</sup> As a further check we also run the same regressions using time fixed-effects (i.e. we introduced a dummy for each month) instead of a linear trend and the results are broadly similar (see Table A5.1 in Appendix 5).

<sup>44</sup> 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 the merger 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 differ 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.

parties. Similarly to the analysis of price at the local level, we start by plotting and comparing graphically the average discounts applied by the merging parties and the competitors.<sup>45,46</sup>

[insert Figure 4 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 started indicatively around the beginning of 2005, i.e. well before the merger was consummated. Hence, 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 (which was issued in May 2006).<sup>47</sup> Whatever the cause of these trends, there is an issue that makes it difficult to draw any conclusions from the graphs presented above. 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,<sup>48</sup> the market shares held by supermarkets and internet retailers have continuously increased over the past years (Figure 5).

[insert Figure 5 here]

This observation, in combination with the fact that supermarkets and internet retailers tend to apply higher discounts than specialist and non-specialist book retailers,<sup>49</sup> may explain, at least partially, the increase in the average discounts in the "competitors" category.<sup>50</sup> In other words, the apparent increasing trend may simply be due to a change in the composition of the group, where the increasing weight of supermarkets and internet retailers drives the observed pattern of the average discount.

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<sup>45</sup> In the econometric analysis the information of Waterstone's and Ottakar's are aggregated and considered as a common company both pre- and post-merger. This is done in the light of the fact the parties seem to adopt similar pricing strategies over the whole period under examination. Nonetheless, we also run the regressions by keeping Waterstone's and Ottakar's separate and the results do not appear to change significantly.

<sup>46</sup> The distribution of monthly national discounts for all titles together is provided in Appendix 5 (see Figure A5.1).

<sup>47</sup> Some studies found evidence of anticipatory price increases before the parties were legally granted permission to merge (e.g. Weinberg, 2008). However, even considering the first announcement of Waterstone's bid for Ottakar's was made 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.

<sup>48</sup> 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.

<sup>49</sup> Anecdotal evidence on this is provided by our survey of market participants (see Appendix 4). 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.

<sup>50</sup> 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.

The fact that the discounts of the merging parties and the competitors show two diverging trends, which started even before the merger, poses concerns on the validity of the DiD approach. Indeed, the common trend assumption which lies at the very base 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.<sup>51</sup>

Table 3 presents the results of this econometric exercise (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).<sup>52</sup>  $TrEff_i$  is not significant in this specification suggesting that the merger does not appear to have had any relevant impact on discounts. Consistent with the results of the pooled regression, the analysis at the category level (from column (2) to (5)) shows that  $TrEff_i$  is never significant and that the coefficients associated to 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\_merger* 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.

[insert Table 6 here]

We then look at a different control group, the top-selling titles.<sup>53</sup> We choose these titles because, even if the merger may have affected them, it is likely that this effect is limited since these titles are sold through all retail channels including internet retailers and supermarkets that tend to adopt aggressive pricing policies. In Figure 6 we plot the average discounts applied by the merging parties for each category. 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 6 here]

The results of the DiD analysis with the top-selling titles as the control group (equation 3) are presented in Table 4. Columns (1), (2), and (3) report the estimates of the regressions with best-sellers, deep-range books and evergreen books as the treatment group respectively.

[insert Table 7 here]

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<sup>51</sup> 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. Hence, the results have to be considered with caution.

<sup>52</sup> We specify a fixed effect for each combination of titles and retailers (i.e. whether it is sold by the merging parties or by the competitors).

<sup>53</sup> The regression is run only on the merging parties' data.

The coefficient  $TrEFF_2$ , that measures the effect of the merger relative to the top category, is never significant, thereby indicating no negative impact of the merger on the discounts applied to best-seller, deep-range and evergreen titles. We also check these estimates by using a specification with time fixed-effects instead of a linear trend and we find similar results.<sup>54</sup>

Overall, even though some caution in the interpretation of the estimates is required because of the limited size of the post-merger sample for top-selling titles, 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.

**Robustness Check: Before-and-After Analysis.** As a final check, we also perform a standard before-and-after regression on the merging parties' data to assess the effect of the merger. In this setting we impose a trend to account for the long term evolution of the discount and the effects of the merger are captured by a dummy, which is equal to 1 for the observations after the merger and 0 otherwise (i.e. the potential impact of the merger is measured by a vertical shift in the trend). The results of these estimates confirm that the merger does not seem to have adversely affected prices.<sup>55</sup> Moreover, we also controlled for changes in the slope of the trend. In this case a significant and negative coefficient would indicate that the merger may have speeded up the decreasing pattern of the discounts. As before, we find that following the merger there was no significant change in the trend of the discounts.

## 6. Conclusions

The ex post assessment of the mergers' 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 on the estimation of the effects of consummated mergers in these industries. Our paper tries to fill this gap by estimating the price effects of a merger, that took place in 2005, between two major bookstore chains in the UK: Waterstone's and Ottakar's. 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. Unlike studies on ex post merger

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<sup>54</sup> See Table A5.2 in Appendix 5.

<sup>55</sup> See Table A5.3 in Appendix 5.

evaluations in other sectors, our empirical framework takes into account this feature by performing an assessment of the effect of the merger both at the national and local level.

To do so, we build an original database with information on a set of book titles both at the store level and at the national level. In doing so, we take into account another important feature of the market under examination, namely the short life of books. Therefore we select a sample of books which does not remain constant over the whole period, using an 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 exercise where we compare the price change in overlap areas and in non-overlap areas before and after the merger. Our results show that the merged entity did not change her prices in a significant way after the merger. Also at the national level, results of two different DiD exercises (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.

Arguably, there are other dimensions of competition that we cannot explore due to data availability, such as the quality of the service, the range of the products in stock, and the customer service. 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.

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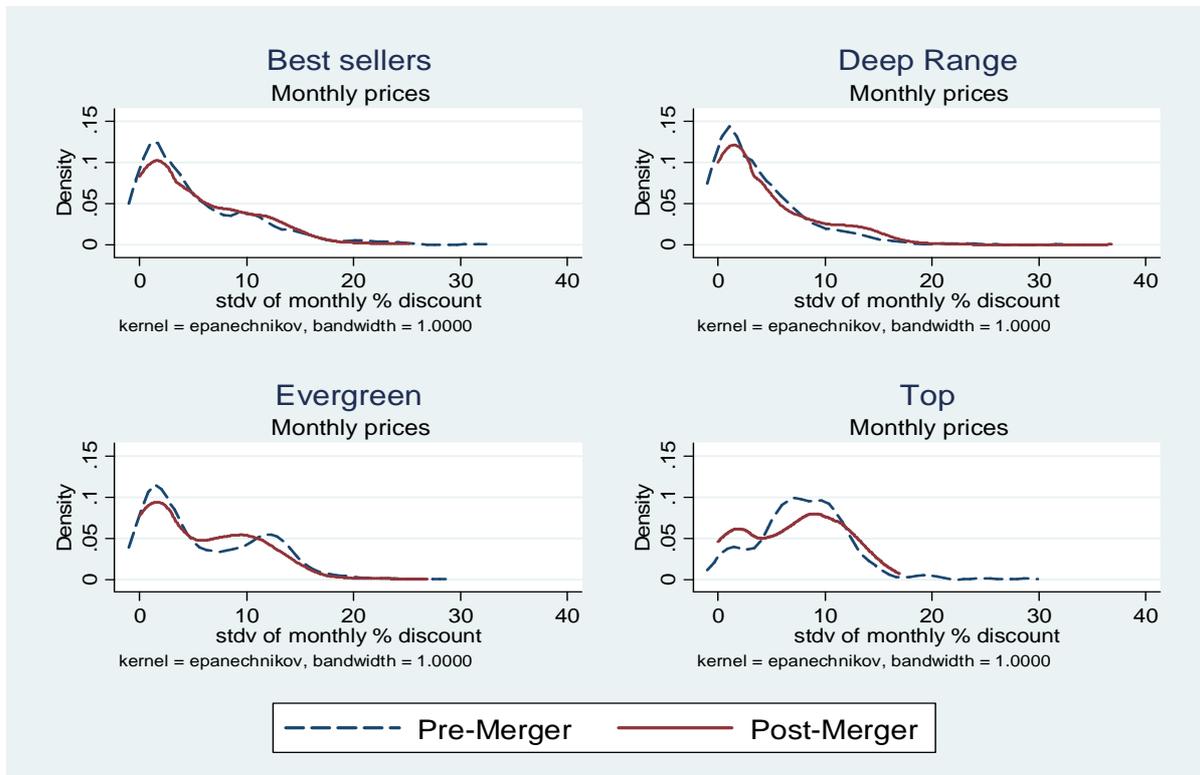
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## Figures and Tables

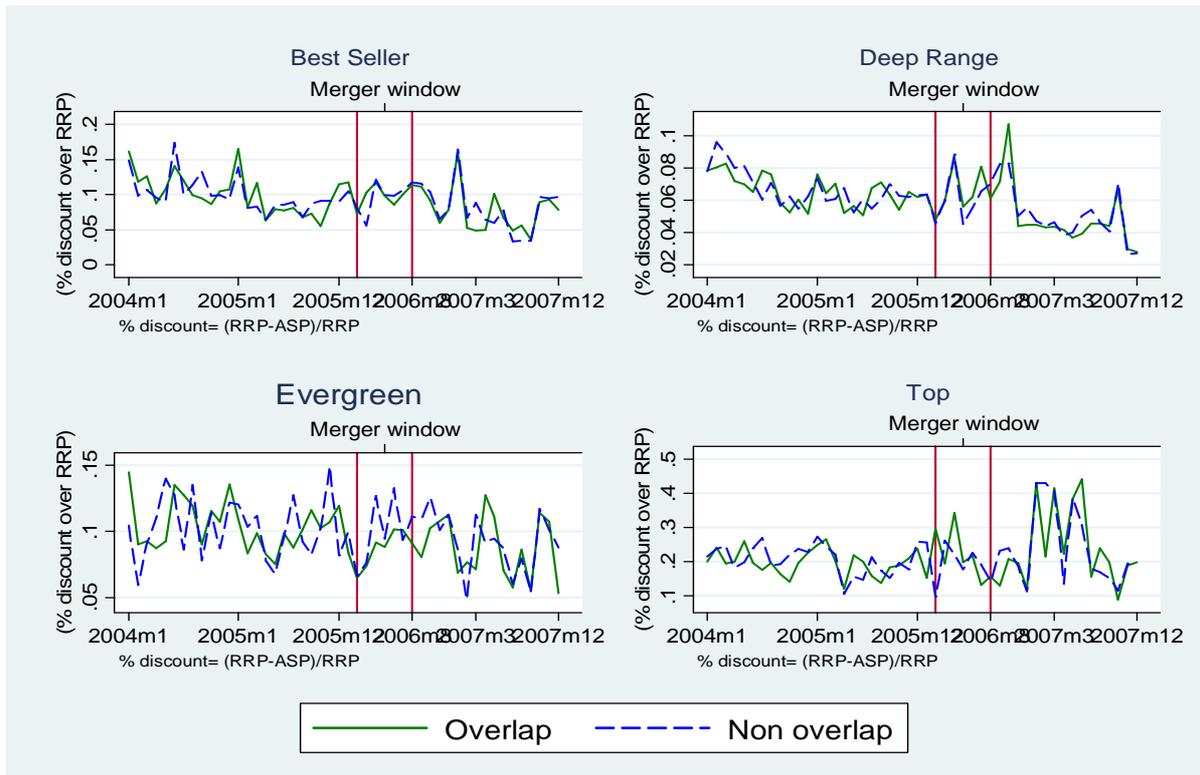
**Figure 1: Distribution of the Monthly Standard Deviation: pre vs. post merger**



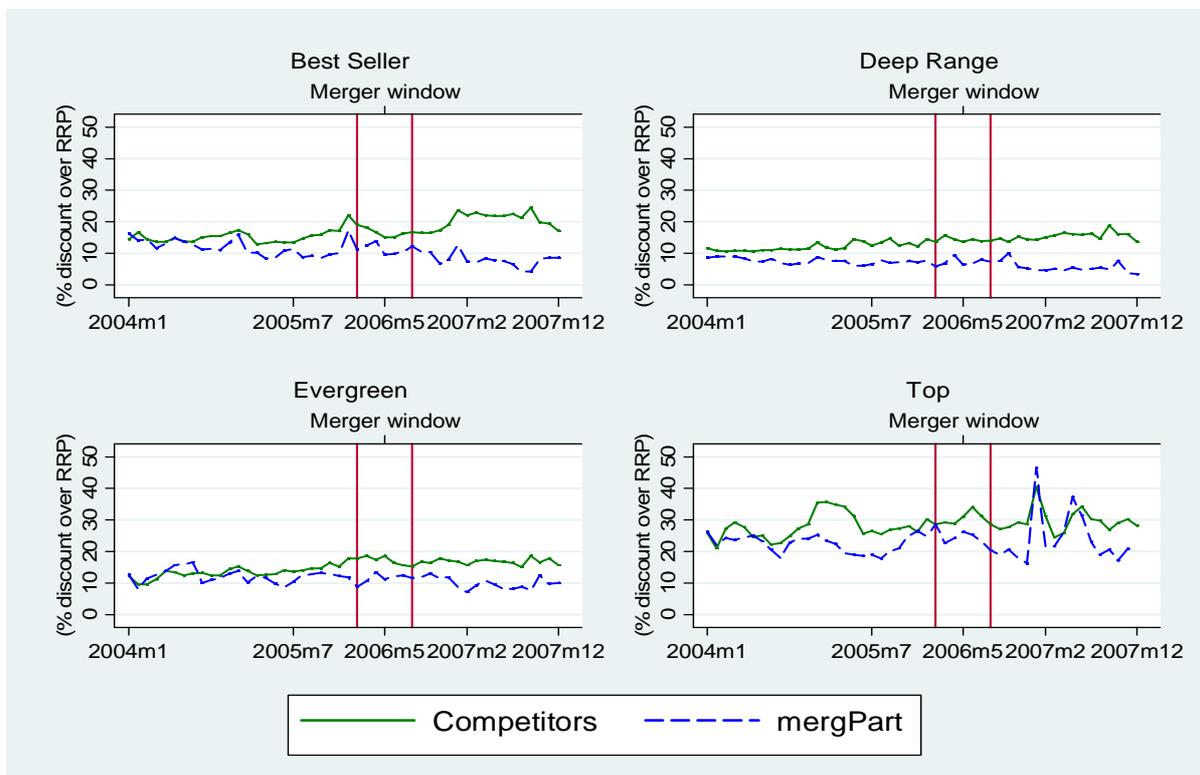
**Figure 2: Distribution of the Monthly Standard Deviation by Title-Category:  
pre vs. post-merger**



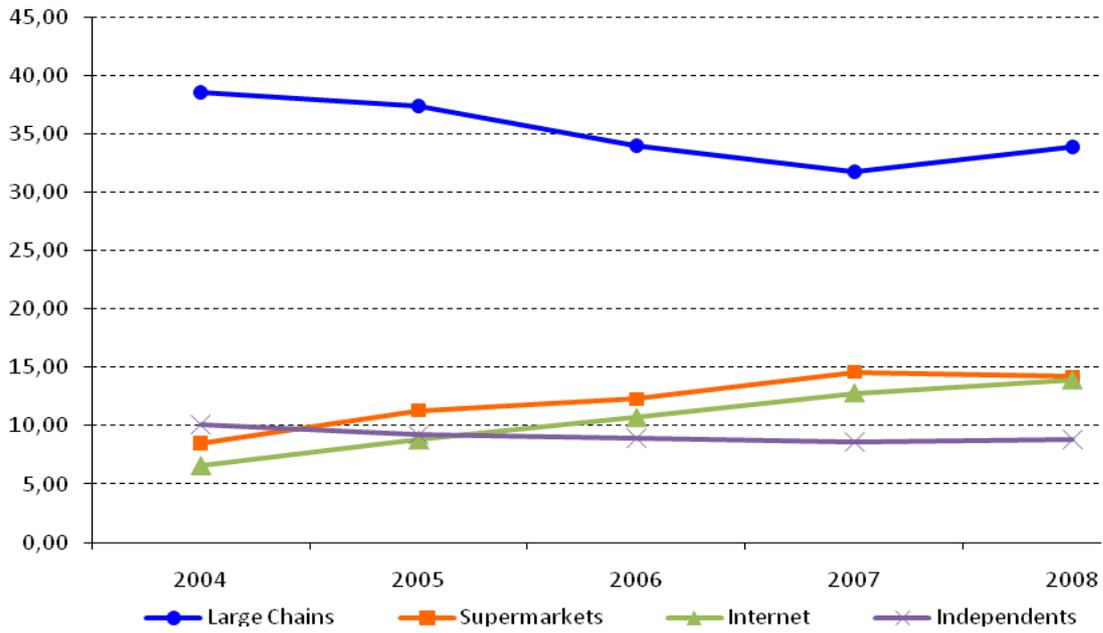
**Figure 3: Distribution of the Monthly Discounts: Overlap vs. Non-overlap Locations**



**Figure 4: Distribution of monthly national discounts: merging parties vs competitors**

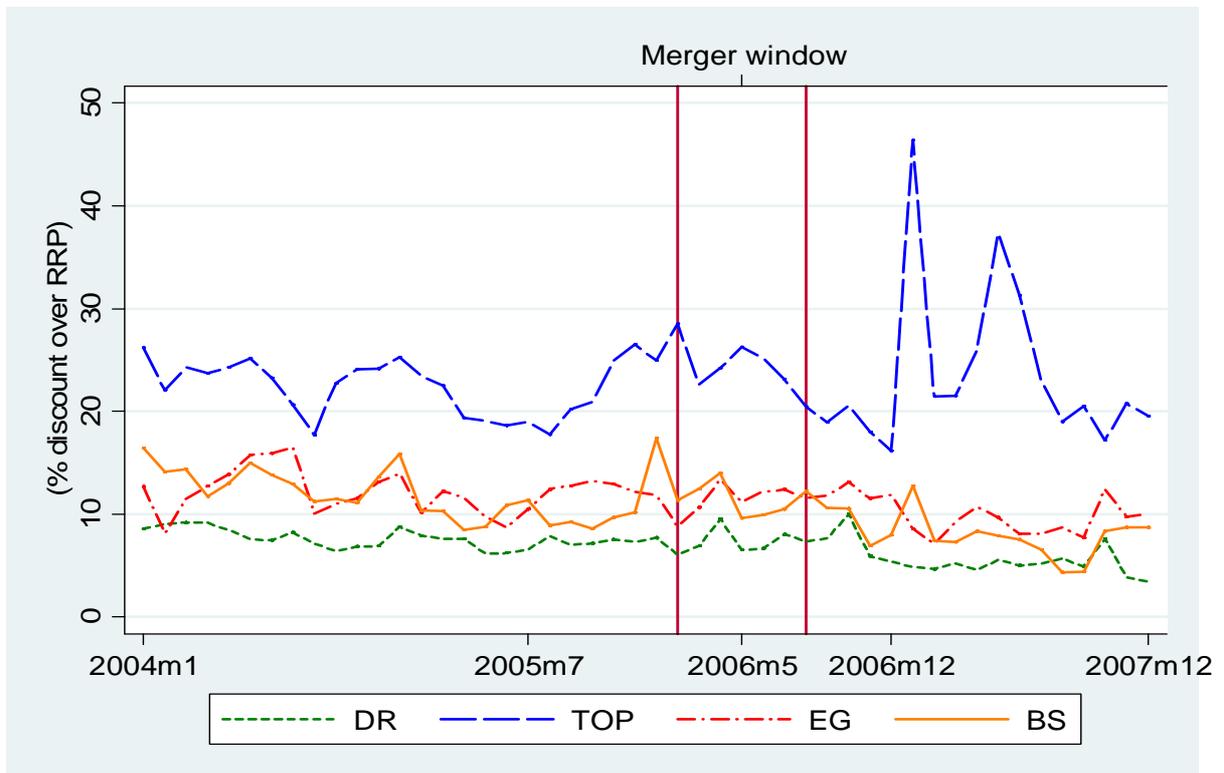


**Figure 5: Retailing book market: market share by volumes**



Source: <http://www.booksellers.org.uk/>

**Figure 6: Average discounts by category – the merging parties**



**Table 1: National market shares of retailers (based on the value of sales)**

<b>National market shares (in 2005)</b>	
Waterstone's	17%
Ottakar's	7%
Other specialist bookshops <sup>56</sup>	15%
Other stores including WHSmith	19%
Supermarkets	8%
Internet	8%
Book clubs and other distance sellers	15%
Other	10%
<b>Total</b>	<b>100%</b>

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

**Table 2: Number of titles by category and type of binding**

	<b>Hardback</b>	<b>Paperback</b>	<b>Total</b>
<b>Fiction</b>	6	56	<b>62</b>
<b>Specialist</b>	-	20	<b>20</b>
<b>Trade</b>	16	50	<b>66</b>
<b>Young</b>	6	46	<b>52</b>
<b>Total</b>	<b>28</b>	<b>172</b>	<b>200</b>

**Table 3: Number of titles by category and year**

	<b>Book Type</b>				<b>Total</b>
	<b>Best-seller</b>	<b>Deep-range</b>	<b>Evergreen</b>	<b>Top</b>	
2004	28	76	31	19	<b>154</b>
2005	48	82	35	14	<b>179</b>
2006	33	106	41	11	<b>191</b>
2007	23	125	41	10	<b>199</b>

<sup>56</sup> Including Borders and Blackwells.

**Table 4: Description of control variables**

<b>Variable</b>	<b>Description</b>
month_t	month trend
closed	dummy for closures in the area
TrEff_closed	dummy which combines TrEff and Closed
season	seasonal dummy (christmas period)
trading_m1	number of stores for specialist retailers
trading_m2	number of stores for non-specialist retailers
trading_m3	number of supermarkets <sup>57</sup>
woodpulp	cost of paper
internet	internet penetration
house_price	house prices
waterstone	dummy for Waterstone's stores (before rebranding)
avgsales_area	average sales per location <sup>58</sup>
population	population
pop_density	population density
urban area	dummy for urban areas
universities	number of universities
Education	average level of education <sup>59</sup>
GVA <sup>60</sup>	gross value added
classD1, D2, D3 and D4	genre (D1=Fiction, D2= Specialist, D3=Trade, D4=Young)
series	dummy for titles which are part of a series
figure	dummy for titles containing figures
pages	number of pages
elapsed_year	years elapsed since the publication
just_pub <sup>61</sup>	dummy for the first 2 months after publication
paperback	dummy for paperback titles

<sup>57</sup> This variable also includes other retailers (such as DIY chains) that sell books as a part of a wide range of goods.

<sup>58</sup> Average sales (volumes) of Waterstone's and Ottakar's stores per area (in 2005).

<sup>59</sup> The level of education was 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).

<sup>60</sup> GVA measures the contribution to the economy of each individual producer, industry or sector, and it is used in the estimation of Gross Domestic Product (GDP). The link between GVA and GDP can be defined as:  $GVA + \text{taxes on products} - \text{subsidies on products} = \text{GDP}$  (see the *Office for National Statistics*).

<sup>61</sup> We observed that retailers may grant higher discount on titles that are just published. To capture this aspect we then introduced a dummy equal to 1 for the first two months since a title is published and 0 otherwise.

**Table 5: DiD on local prices - Overlap vs. Non-overlap Areas**

	All titles (1)	Best Sellers (2)	Deep-range (3)	Evergreen (4)	Top-Sellers (5)
		0.382 (1.007)	0.118 (0.531)	0.0919 (0.277)	0.590 (1.274)
post_merger	1.261*** (5.70)	0.569 (1.147)	2.449*** (10.68)	-0.293 (-0.796)	-1.989** (-2.118)
<b>TrEff</b>	<b>-0.252</b> <b>(-1.16)</b>	<b>-0.0703</b> <b>(-0.134)</b>	<b>-0.164</b> <b>(-0.732)</b>	<b>0.0961</b> <b>(0.258)</b>	<b>0.0132</b> <b>(0.0178)</b>
closed	0.045 (0.13)	-0.0517 (-0.0887)	-0.295 (-0.804)	0.0142 (0.0243)	0.501 (0.606)
TrEff_closed	0.051 (0.11)	<b>0.463</b> <b>(0.472)</b>	<b>0.368</b> <b>(0.764)</b>	<b>0.161</b> <b>(0.221)</b>	<b>-1.157</b> <b>(-0.756)</b>
constant	18.410*** (5.64)	9.616*** (4.125)	14.90*** (11.44)	5.404*** (2.813)	12.34*** (3.863)
observations	176,668	37,981	59,548	57,974	20,754
number of id	11,842	4,549	6,916	2,445	2,930
R-squared	0.061				
cluster	ISAN*ISBN <sup>62,63</sup>	ISAN*ISBN	ISAN*ISBN	ISAN*ISBN	ISAN*ISBN
Time Trend	YES	YES	YES	YES	YES
Effects	Fixed: ISAN*ISBN	Random	Random	Random	Random

The dependent variable is the price discount. In all columns we control for a monthly time trend, trading\_m1, trading\_m2, trading\_m3, a seasonal dummy, the price of wood pulp, the internet penetration rate, the housing price, gross value added, and the years elapsed since publication. 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 (see Table 4 for the description of control variables). Robust t-statistics (columns 1) and z-statistic (columns 2 to 5) in parentheses, The symbols \*\*\*, \*\*, and \* represent significance at the 1%, 5%, and 10% level respectively.

<sup>62</sup> ISAN is the Nielsen's unique identifier of a store.

<sup>63</sup> ISBN is the Nielsen's unique identifier of a title.

**Table 6: DiD on national prices (competitors as control group)**

	All titles (1)	Best Sellers (2)	Deep Range (3)	Evergreen (4)	Top Range (5)
Merged		0.225 (0.0784)	-3.861*** (-2.749)	0.00529 (0.00277)	-1.483 (-0.526)
post_merger	-0.732 (-0.766)	-3.562 (-1.538)	-0.360 (-0.269)	-1.668 (-1.262)	-4.795 (-0.958)
<b>TrEff<sub>1</sub></b>	<b>0.108</b> <b>(0.0890)</b>	<b>0.692</b> <b>(0.229)</b>	<b>0.646</b> <b>(0.421)</b>	<b>0.365</b> <b>(0.176)</b>	<b>-2.751</b> <b>(-0.470)</b>
month_t_merged	-0.134*** (-3.294)	-0.349*** (-3.223)	-0.109*** (-2.845)	0.0138 (0.129)	-0.445** (-2.312)
month_t_comp	0.0721* (1.922)	-0.0198 (-0.234)	0.0688 (1.420)	0.192*** (2.734)	-0.311* (-1.652)
constant	218.1*** (3.318)	21.77 (0.485)	47.39** (2.165)	60.62** (2.047)	-146.5 (-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
Time Trend	YES	YES	YES	YES	YES
Effects	Fixed:	Random	Random	Random	Random
	ISBN*retailer				

The dependent variable is the price discount. In all columns we control for a monthly time trend, trading\_m1, trading\_m2, trading\_m3, a seasonal dummy, the price of wood pulp, the internet penetration rate, the housing price, GVA (?), and the years elapsed since publication. In the random effects specifications (columns 2 to 5) we additionally control for Woodpulp; Pages; Series; figure; paperback; classD2; classD3; classD4; house\_price; internet; GVA; elapsed\_year; just\_pub; Season (see Table 4 for the description of control variables). Robust t-statistics (columns 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 (top titles as control group)**

	<b>Best Seller (1)</b>	<b>Deep Range (2)</b>	<b>Evergreen (3)</b>
<b>TrEFF<sub>2</sub></b>	<b>1.431</b>	<b>6.322</b>	<b>5.164</b>
	<b>(0.36)</b>	<b>(1.52)</b>	<b>(1.40)</b>
titlecategory	-9.144***	-17.679***	-4.714**
	(-3.73)	(-7.32)	(-2.08)
post_merger	-6.463	-4.812	-6.841
	(-1.35)	(-1.09)	(-1.59)
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
Time Trend	YES	YES	YES
Individual Effects	Random	Random	Random

The dependent variable is the price discount. In all columns we control for a monthly time trend, trading\_m1, trading\_m2, trading\_m3, a seasonal dummy, the price of wood pulp, the internet penetration rate, the housing price, GVA (?), and the years elapsed since publication. In the random effects specifications (columns 2 to 5) we additionally control for Woodpulp; Pages; Series; figure; paperback; classD2; classD3; classD4; house\_price; internet; GVA; elapsed\_year; just\_pub; season (see Table 4 for the description of control variables). Robust z-statistic are reported in parentheses, The symbols \*\*\*, \*\*, and \* represent significance at the 1%, 5%, and 10% level respectively.