

Empirical Effects of Resale Price Maintenance: Evidence from Fixed Book Price Policies in Europe

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Abstract

This paper investigates the effects of Resale Price Maintenance (RPM) agreements in Europe by studying a legally-permitted form of RPM in the book market. In particular, we study the effects of such agreements on prices and sales, finding that countries which have Fixed Book Prices policies witness higher book sales, relative to countries without such a policy, with no noticeable effect on the average price of books. We suggest the mechanism for these findings is that FBP policies promote non-price competition and a diverse network of retailers, increasing quantity, whilst a change in bargaining power reduces double marginalisation, offsetting upward pricing pressure from RPM. Whilst these results are limited to the book sector, and are based on a limited number of countries changing policy, they nonetheless highlight that RPM agreements have the ability to confer positive competition effects and absolute prohibition of such agreements may not always be warranted.

JEL classifications: L42, L81, C23

Keywords: Resale Price Maintenance, Fixed Book Prices, Competition Economics

1 Introduction

Resale Price Maintenance (RPM) is a vertical restraint whereby upstream manufacturers limit the ability of downstream retailers to set their own pricing policies for a product. In a number of jurisdictions RPM is prohibited based on arguments that these agreements lead to higher prices with little effect on service quality, thereby being to the detriment of consumers.¹ However, there is limited empirical evidence into the actual effects of RPM in practice, in particular, to evaluate whether the pro- or anti-competitive effects discussed in theory prevail in reality. This is primarily a result of the illegality of RPM, making it difficult to test. Existing studies either examine cases where RPM is prohibited (e.g. MacKay & Smith 2014) or study illegal instances of the use of RPM (e.g. Ippolito 1991), with the obvious pitfall that competition agencies are likely to focus their limited resources on the RPM violations which have the most likely anti-competitive effect.

To overcome this issue, we leverage a notable exception to the illegality of RPMs, in the European book sector, namely, the use of Fixed Book Price (FBP) policies. FBP policies currently exist in around one-third of EU nations and have a long history, with RPM in the book sector existing as early as 1829 (IPA 2014). Furthermore, book sales in Europe totalled €22.4 billion in 2019 (FEP-FEE 2021), highlighting the importance of this large industry. By studying the use of FBP policies, we can see the effect of RPM when it is permitted and examine whether the pro- or anti-competitive theoretical effects prevail in reality. Whilst many have hypothesised about the effects of FBP policy (e.g. Dearnley &

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¹This includes the European Union where RPM is designated a hardcore restriction under the Vertical Block Exemption Regulation (VBER). Whilst RPM is a hardcore restriction, the European Commission allows an exception for Fixed Book Price policies so long as cross-border trade between member states is not affected.

Feather 2002, van der Ploeg 2004, Berger & Hviid 2019), along similar lines to the pros and cons of RPM, very few have empirically tested its effects.

We exploit FBP policy variations on the basis of which to assess the impact of RPM on market outcomes. Using panel data from publicly available sources across Europe, we analyse the effects on book price and output sold, associated with the use of FBP policies in a two-way fixed effects specification. We find evidence to suggest that between 2008 and 2019, such policies had no noticeable effect on book prices, with evidence suggesting possible downward effects on prices, but resulted in increased book sales. Using a longer time series of data for book prices, available between 1996 to 2020, we continue to find no evidence for price effects of RPM across a wider sample of policy changes.² These findings are at odds with theoretical predictions which conclude that RPM leads to higher prices, although the theory acknowledges possible positive effects on book sales as a result of increased service provision. Instead, our empirical findings are consistent with the explanation that FBP policies led to changes in the market structure of the book sector, affecting the bargaining power of retailers, offsetting the increased price effect predicted by the theory. Furthermore, an increase in the number of bookshops, and competition over non-price competition which enhanced the retail experience for customers, may explain the increase in book sales.

To the best of our knowledge we are the first to use cross-country panel data on the use of FBP policies to evaluate the effects on price and output sold and draw conclusions from these findings to contribute to the debate on the effects of RPM. The findings in this paper allow us to contribute to the literature in two regards. Firstly, we show that there are positive effects to RPM agreements. Our findings suggest that outright bans on RPM may be imprudent, and to the potential detriment of consumers. We do, however, note that our findings are restricted to a narrow sector, which has unique features and are based on aggregated data and a limited number of policy changes. As such, we do not suggest that RPM is universally pro-competitive and should be permitted, without regard, in any sector. Instead, we believe that finding pro-competitive effects of RPM in a large industry in Europe gives impetus to competition authorities to consider the use of RPMs in certain industries based on an analysis of effects, rather than a blanket ban. Secondly, our findings demonstrate the positive impact that FBP policies have had across Europe in boosting book sales and the number of bookshops. This implies that one of the goals of FBP policies, to promote reading and access to literature, has succeeded. Furthermore, we do not find evidence of higher book price growth in FBP countries as a result of the policy.

In the next section we discuss the background behind FBP policies and why countries adopt such policies, additionally discussing the existing literature around both FBP and RPM before outlining the available data in Section 3. Section 4 provides descriptive statistics whilst the methodology used in this paper is set out in Section 5. Econometric results and discussion is provided in Section 6, alongside a mechanism which is used to explain our results. Robustness checks are presented in Section 7 and finally Section 8 provides concluding remarks and some areas for future research, noting the data limitations in this study and outlining steps for future research to investigate this area further using more refined data.

²We are unable to test the effects on book sales, in this extended time period, due to the unavailability of data.

2 Literature and institutional background

2.1 FBP background

FBP policies allow publishers to mandate that retailers (booksellers) sell their books at a specified price.³ Such policies can therefore be seen as minimum resale price maintenance agreements. This rule either exists as a legal requirement, for instance *Loi Lang* in France, or as a well-established agreement amongst publishers, such as in Norway where an agreement, *Bokavtalen*, between the Norwegian Bookstore Association and the Norwegian Publishers Association is in operation.

Different regimes have adopted different policies with respect to the stringency of FBP policies, such as the duration that fixed prices apply, what types of books (including whether the policy applies to e-books) and for what customers the law applies, and whether any discounts are permitted. This demonstrates that the intensity of FBP policies are not homogeneous but instead vary across regimes. Variation in the duration that FBP applies ranges from 6 months in Slovenia to 24 months in Austria, France and Spain, with other regimes having a duration in between. Consequently, most FBP policies last throughout the commercial life of the majority of books, with estimates for when the majority of sales take place, post publication, ranging from 4 to 6 months (Sorensen 2007) to two years (Poort et al. 2012).

Policymakers typically adopt FBP policies to protect the cultural integrity of their national book publishing market and to promote positive externalities that are associated with a strong and diverse book publishing and bookselling sector, which promote consumption and readership (Poort & van Eijk 2017, Ragazzo & Lima 2017).

A particular concern, and motivation for adopting FBP policies, is that low-service retailers heavily discount popular bestseller books and have a limited range of books on sale. This discounting is feared to lead to reduced margins in specialised bookshops (which stock a wider range of books and can be considered high-service booksellers) and would endanger their business model which, to some extent, relies on relatively stable sales of bestsellers to support the stocking of other, special interest books with more volatile sales behaviour. In the literature, this is often known as the cross-subsidisation argument and is an example of service provision that consumers *may* favour.

Other services provided by bookshops include knowledgeable staff, a wide range of low circulation titles, a pleasant and calm browsing and shopping experience, and reading recommendations. These types of services promote the value of books in general and help book sales across all retail formats. In addition, they help bring about a form of retail competition that is not based on low prices alone.

It is argued that heavy price discounting in the absence of an FBP policy can lead to (a) closure of specialised and bricks-and-mortar bookshops, ultimately limiting consumer access to a wide range of book titles, or at least (b) some niche books being unprofitable to stock, thereby limiting the breadth of literature available to consumers and as a result the breadth of titles published within national markets. Therefore, a particularly salient argument in favour of implementing FBP policies has been that the limitation of discounting would maintain bookseller profitability and hence preserve, or increase, the

³This is akin to agency model, where retailers earn royalties from selling books, equal to the difference between the stipulated consumer price and the wholesale price set by the publisher.

number of specialised bookshops along with the range of titles stocked.

On the other hand, those that oppose fixed book prices highlight that discounting can allow a wider range of consumers to access books and that FBP leads to artificially high retail prices at the expense of consumers which may be particularly prohibitive for low income individuals (Poort & van Eijk 2017). Furthermore, opponents of FBP highlight that the cross-subsidisation argument posited above is unlikely to work because there is no guarantee that bookshops will use monopoly profits from one genre of books to cross-subsidise less popular books, particularly when non-fiction books tend to have low price elasticities of demand and there is little cross-genre substitution (Canoy et al. 2006). Additionally, even if cross-subsidisation across titles did occur, then it may pose equity problems in that it results in a subsidy from low-income bestseller readers to high-income readers of specialist books.

2.2 FBP literature

To the best of our knowledge, we are the first to use cross-country panel data on the use of FBP policies to evaluate the effects on consumer welfare and draw conclusions from these findings to the RPM debate. Previously, studies have looked at FBP policies within-country and compared price and sales evolutions but have not used a well-specified counterfactual group, which limits their ability to make causal claims from their findings (Ball et al. 2008, Fishwick 2008, Løyland & Ringstad 2012, Kontolaimou et al. 2019). Within this literature, there are mixed findings: some find positive effects of FBP (Fishwick 2001, Fishwick 2008) whilst others find negative effects (Løyland & Ringstad 2012, Kontolaimou et al. 2019). The aim of this paper is to weigh in on this debate by employing robust econometric methods to investigate the effect of FBP on book prices and sales of books.

One paper that is more closely aligned with our methodology is Canoy et al. (2006), who use a two-way fixed effects model to investigate the effect of FBP policies on the number of (unique) book titles per 100,000 inhabitants. They control for real GDP per capita and average years of schooling and include year and country fixed-effects across 20 countries spanning 1975-1999. Their results suggest that FBP policies have no statistically significant effect on the number of book titles published, contrary to a motivating factor for implementing such policies.

In the e-book market, los Santos & Wildenbeest (2017) study the switch that five of the six largest publishers made from agency models, which contain an RPM component, to wholesale models, where RPM is absent. Originally, these publishers made the switch to the agency model in 2010, motivated by a fear that low e-book pricing was eroding consumers' perceptions of books, cannibalising sales of hardcover books and would lead to downward pressure on wholesale prices. However, following anti-trust enforcement in 2012, the publishers were forced to revert back to the wholesale model. Essentially these authors tested whether RPM in e-books produces positive or negative effects, using a difference-in-difference framework. Their findings suggest that the wholesale model is strongly associated with a significant reduction in e-book prices, with results suggesting that prices on Amazon decreased by 18%. However, they caution that the time period after the switch to the wholesale model may be insufficient to capture the potential incentives for retailers to increase prices. On the other hand, Gail & Klotz (2021) find the opposite effect: e-book prices were 36% cheaper, on average, under the agency model,

compared with the wholesale model. These authors use a slightly different approach, based on a large cross-sectional dataset of e-books, and their characteristics, sold on Amazon in the UK, rather than exploiting a natural experiment. Nonetheless, their findings are robust across a number of specifications and are in line with theoretical models which find that retail prices for e-books sold under the agency model are lower, in the long-run, compared with those sold under the wholesale model, where retailers exploit a lock-in effect under the latter model (Johnson 2020).

2.3 RPM literature

There are both pro- and anti-competitive motivations and effects of RPM. Anti-competitive motivations include (i) sustaining a cartel (either upstream or downstream), (ii) foreclosure, (iii) forestalled innovation, and (iv) lack of price competition. Firstly, the cartel argument posits that RPM is a relatively efficient way to monitor compliance of any cartel agreement, helping to sustain the cartel from incentives of members to unilaterally reduce their price in secret (Telser 1960, Jullien & Rey 2000). Such cartels can be initiated by either the manufacturer or the retailer – where the retailer benefits from retailer collusion and deterring entry of low-priced rivals (Bauer & Yamey 1954) - depending on the relative market power of each. Secondly, profit margins for retailers can be boosted by a policy of RPM, offering an incentive for retailers not to stock competitor products, resulting in foreclosure (Lafontaine & Slade 2008). Thirdly, a dominant retailer may encourage RPM from a manufacturer in order to prevent innovation in distribution (such as the entry of discount retailers) which results in lower costs and thus prevents consumer-enhancing innovation by competing retailers (Marvel & McCafferty 1985). Finally, RPM agreements clearly limit the scope for price competition between retailers and thus softens competition along a paramount dimension of consumer welfare.

On the other hand, pro-competitive motivations include (i) preventing free-riding on service provision, (ii) encouraging retailers to maintain adequate inventory, (iii) elimination of the double marginalisation problem, and (iv) supporting the launch of new products. The most common reason usually put forward to support RPM is that by preventing price competition, retailers are instead induced to compete along other dimensions, most notably service quality. This can result in higher sales, assuming that customers prefer a high level of service or sales effort. In other words, RPM prevents low-price retailers from free-riding on the service efforts of high service retailers, a problem that is particularly noted when comparing bricks-and-mortar and online retailers.⁴ This argument is extended by Mathewson & Winter (1998) to include the effect of RPM on the number of shops which stock the RPM product. Relatedly, retailers, particularly discounters, may minimise their inventory to reduce costs. If consumers prefer shops to be well stocked then RPM can increase sales by incentivising retailers to compete along such a dimension (Deneckere et al. 1996, Deneckere et al. 1997). In the book market, it may be the case that one service dimension could be competition in stocking back-listed books (Davies et al. 2004), which make up a significant part of the market (80% for some bookstores (Utton 2000)).⁵

An unrelated benefit of RPM is the elimination of the double marginalisation problem

⁴Without RPM, consumers can shop around and use a retailer's technical expertise and service provision but then purchase the good at a cheaper retailer (who is offering a lower service offering).

⁵Some authors remain unconvinced that these service provision arguments have much effect in the book market (e.g. van der Ploeg 2004).

(Spengler 1950, Mathewson & Winter 1984, Rey & Vergé 2004). Without co-ordination, both the manufacturer and retailer set profit maximising prices, ignoring the externality that this imposes on the other party, with the final price being higher than if there was a vertically integrated monopolist. This higher price results in lower sales, so lower profits, and therefore both consumers and the sellers lose. RPM is one solution which eliminates the double marginalisation problem and permits lower consumer prices.⁶ Finally, there are arguments that RPM can aid in the launch of new products, which are often risky ventures and retailers may need a guaranteed margin to be incentivised to stock such products (Marvel & McCafferty 1984).

Overall, the theory produces both positive and negative outcomes of RPM, depending on the market structure and demand function. As such, it is not possible to evaluate the impact of RPM on consumer welfare based on theoretical arguments alone. There is extensive theoretical literature examining the impact of RPMs but a number of authors have highlighted the limited empirical evidence, leaving unsolved the question of whether the anti-competitive or pro-competitive effects of RPM have a greater influence (Ball et al. 2008, Poort & van Eijk 2017, MacKay & Smith 2017). This lack of empirical evidence provides the motivation for our empirical study on the effects of FBP policies across Europe.

Of the limited empirical evidence available, several studies find supportive evidence that RPM is motivated by a desire to enhance service and sales rather than for collusive reasons (Ippolito 1991, Mathewson & Winter 1998, Cooper et al. 2005, Beck 2004, Lafontaine & Slade 2008). Others find evidence of anti-competitive effects in certain product markets (Shepard 1978, Ornstein & Hanssens 1987, Mueller & Geithman 1991).

Beck (2004) studies the German book market and notes that whilst retail prices are clustered around focal prices, the evidence suggests that such prices are not the result of RPM-aided collusion, finding that books are often priced below the levels predicted by a hedonistic regression based on observable characteristics. Instead, he finds evidence that RPM may be used in response to demand uncertainty, particularly for titles from new authors.

Some of the empirical literature took the approach of examining legal cases of RPM infringements when RPMs are per se illegal. For instance, Ippolito (1991) studies a set of legal cases on RPM between 1976 and 1982, finding that most uses of RPM are consistent with a special service argument, with less than 15% of cases being consistent with a collusion argument. However, RPMs which have pro-competitive outcomes are unlikely to be prioritised for investigation by legal authorities, meaning that such samples are biased, being more likely to discover anti-competitive effects than would be the case across the wider economy. A more appropriate approach is to study the effect of RPMs around a policy change, such as a ban of RPM in some jurisdictions. This is the approach adopted by MacKay & Smith (2014) who take advantage of the differential application of the 2007 Leegin Supreme Court decision across US states which permitted RPM in some cases. Using a difference-in-difference approach across a broad variety of products they find evidence of higher prices, lower output and reduced consumer welfare when RPM is enforced under “rule of reason” (i.e. not presumed unlawful). However, the authors are unable to identify whether RPM agreements are actually being used in rule

⁶Note that price ceilings (rather than price floors as FBP policies are) are sufficient to solve the double marginalisation problem and that alternative solutions, such as the manufacturer setting a non-linear payment schedule, would also solve the problem (see Kwoka & Slade (2020) for more details).

of reason states, an issue that we do not face given that FBP agreements, where applied, are mandated between publishers and retailers.

3 Data

This study is interested in the impact of FBP policies on three key dependent variables: book prices, volume of book sales and the number of enterprises involved in selling books (the latter variable is used to outline a mechanism for explaining our results). We source publicly available data, as explained below.

Book prices Consumer price indices for books are commonly used in the FBP evaluation literature (e.g. Fishwick 2001, Dearnley & Feather 2002, Davies et al. 2004, Fishwick 2008, Løyland & Ringstad 2012, Kontolaimou et al. 2019). Therefore, data is collected from Eurostat’s Harmonised Index of Consumer Prices (HICP), for the price series on books, and is indexed to 2015 prices for each country. Fiction, educational textbooks, other non-fiction books as well as book-binding services and e-books are included in our price measure of books and prices are collected from both (physical) shops and online retailers.⁷ The natural logarithm of this variable is used in the econometric analysis.⁸

Unfortunately, we do not know the exact book list used to create the price index for each country. However, according to the HICP methodology, this price series relies, to some extent, on the prices of books contained in bestseller lists. We would expect the price of bestsellers to be most affected by an FBP policy, since such a policy would most likely seek to prevent heavy discounting on bestsellers. Therefore, a sample of book prices that includes mostly bestsellers would be expected to bias the effect of FBP policies upwards. If, instead, we find a negative effect for FBP policies on prices, in the bestseller-heavy sample, then we expect the true effect to be even lower in a more fully representative sample of book prices.⁹ However, most FBP agreements only apply to recently published books (usually within a couple of years of publication), which are perhaps more likely to be included in bestseller lists and thus included in our dataset (Sorensen 2007). This may suggest that our dataset roughly captures the books to which FBP policies apply.

Book sales volume indicator To the best of our knowledge there does not exist a publicly available dataset providing information on the number of books sold across Europe. Instead, we make use of turnover data for the retail sale of books in specialised stores¹⁰, and we deflate this data by the book price index such that it acts as a proxy for the volume of books sold. This proxy thus tells us the book-price inflation adjusted turnover in specialised bookstores. It should be noted that this variable is based on data from “stores which specialise in book sales” and hence does not include books sold at supermarkets and other non-specialised stores.¹¹ The natural logarithm of this variable is used in the econometric analysis.

⁷It is not possible to disaggregate our analysis by these sub-categories as the data is not publicly available. Additionally, prices are weighted by purchase share to reflect differences in prices between online and physical shops.

⁸To interpret the coefficient on the FBP dummy variable the coefficient needs to be exponentiated and subtracted by one. The transformed variable can then be interpreted as the percentage change in the outcome variable (price of book sales) as a result of the FBP policy variable changing from zero to one.

⁹Further discussion relating to this is provided in Section 6.

¹⁰This turnover data is sourced from Eurostat’s Structural Business Statistics database.

¹¹As mentioned above, we would expect the price of bestsellers to be most affected by an FBP policy. Therefore, dividing the turnover measure by an index which is likely distorted towards the price of bestsellers produces a conservative estimate for the effect on book sales. Again, further discussion relating to this is provided in Section 6.

Number of enterprises involved in selling books per capita Data is collected from Eurostat’s Structural Business Survey for the absolute number of firms engaged in the “retail sale of books in specialised stores” by country. This variable is then divided by the total population, sourced from Eurostat.

HHIs The Herfindahl Hirschman Index is created in each year for each of the 28 countries in our sample based on data from Bureau van Dijk’s Orbis dataset. We obtain all firms that are listed as being in the NACE sector of retail sale of books in specialised stores.¹² The market share for each firm is calculated as the sum of revenue within a country and year. Firms with missing revenue values are treated as having zero revenues and any negative revenue is recoded as being zero. The HHI is then calculated by taking the sum of the square of market share.

FBP policies The main independent variable is a dummy variable capturing whether a fixed book price policy was in force. This data was sourced from academic articles, legal sources, and industry and trade association reports (see Annex A1). If an FBP policy was changed mid-year, then the FBP policy only changes status in the next complete year to ensure that the full effect of the law change is realised.¹³

Control variables A number of other variables are obtained from Eurostat to control for more general changes in the economy and factors which are likely to affect the book sector. These variables are the Consumer Price Index (CPI), log of Gross Domestic Product (GDP), unemployment rate, total population, average labour cost and the percentage of the population who have never used the internet. Countries with higher inflation may see higher book prices, independent of FBP policy, whilst higher levels of GDP and lower levels of unemployment may affect the number of bookstores and the number of books read. A larger population is likely to affect book sales and the number of bookstores.¹⁴ Greater use of the internet proxies for the ability to purchase books online and/or to use digital reading services, such as e-books. Finally, higher labour costs may impact book prices and further acts as a proxy for average earnings which is also captured through the GDP variable.

All data is collected at annual frequency for the 27 EU member states plus the UK.¹⁵ Data is available for all series between 2008 and 2019 and the sample is strongly balanced, albeit with some observations missing for certain countries in particular years. For the price data series we are able to expand the time series data to 1996-2020, where an increased number of FBP adopters and revokers allow us to strengthen our conclusions.¹⁶ More details on the date and country availability of the data series is provided in Section 4 along with descriptive statistics (Table 1).

¹²It is inevitable that some firms will be listed in this sector whilst not truly having activities in this sector (or predominantly being involved in another sector). This is a well-known critique of using industry classifications to estimate market power (e.g. Affeldt et al. 2021).

¹³We conduct a robustness check, with the FBP status changing at the beginning of the year instead, and our results are unchanged.

¹⁴Particularly because in countries with very small domestic markets, the domestic publishing sector cannot achieve the economies of scale which are possible in larger markets.

¹⁵Austria, Belgium, Bulgaria, Croatia, Cyprus, Czech Republic, Denmark, Estonia, Finland, France, Germany, Greece, Hungary, Ireland, Italy, Latvia, Lithuania, Luxembourg, Malta, Netherlands, Poland, Portugal, Romania, Slovakia, Slovenia, Spain, Sweden and the United Kingdom. We thank the Slovenian Statistical Office for their assistance in providing the confidential data for Slovenia. Note that Netherlands is excluded from the book sales regressions given the absence of turnover data, Greece is excluded from the bookselling enterprises regressions due to structural breaks in the data series and Malta is excluded from the bookselling enterprises regressions given missing data between 2011 and 2019.

¹⁶Unfortunately, data is only available for the book sales and bookselling enterprises data series between 2008 and 2019.

4 Descriptive statistics

This section provides descriptive statistics of the dataset for the 2008 to 2019 sample. Table 1 displays summary statistics of the variables used in the econometric analyses. In total, across the 28 countries included in the dataset between 2008 and 2019, there are 336 observations. Table 13 in Annex A2 repeats this exercise for the book price data which is available from 1996 to 2020.

Table 1: Descriptive statistics, 2008-2019

Name	Description	Obs.	Mean	Std. Dev.	Min	Max
FBP	Dummy variable (0 = No FBP, 1 = FBP)	336	0.35	0.48	0	1
Book Price*	Annual average index (2015=100)	336	97.03	8.25	66.49	123.80
Book sales volume indicator*	Value (million euros, turnover adjusted by book price inflation)	289	583.54	863.29	7.72	4,125.21
Number of Bookselling Enterprises per 100,000 capita	Value	295	4.95	4.15	0.44	28.93
HHI	Herfindahl Hirschman Index	323	0.29	0.30	0.01	1
CPI	Annual average index (2015=100)	336	98.24	5.23	78.33	110.50
GDP*	Chain linked volumes, index (2015=100)	336	99.50	9.67	70.60	135.77
Unemployment Rate	Rate of unemployment as % of population	336	5.65	2.86	1.30	17.30
Total Population*	Value (millions)	336	18.10	23.00	0.41	82.20
Labour Cost	Index (2015=100)	336	100.63	8.60	70.10	140.30
Internet Access	% of total population who have never used the internet	336	21.17	13.52	2.00	64.00

Note: Sales volume data is missing for the Netherlands. Number of bookselling enterprises per (100,000) capita excludes Greece (data series breaks) and Malta (insufficient data). *This variable is summarised before the log-transformation has been applied (and used in the econometric model) for ease of reading. Source: Authors' calculations based on Eurostat data.

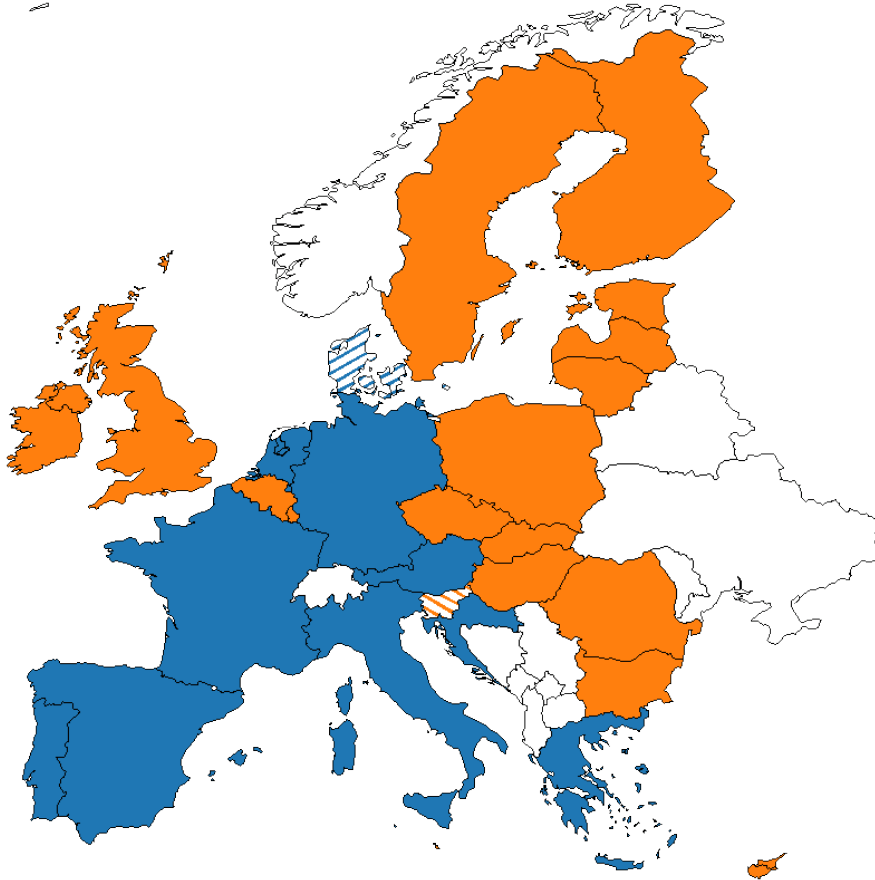
We see a general decline in output of books sold over time, with total quantity declining by 13.4% between 2008 and 2019. In Figure 3, we can see that this decline mainly occurred in the larger markets, with the exceptions of France and Poland. In terms of price (Figure 4), there is a general increase in price over time, with the exceptions of Greece, Hungary, Portugal and Slovenia which see declines over time. On average, book prices rose by 12.7% over the sample period. Finally, we see a general decline in the number of bookselling enterprises per 100,000 capita (Figure 5), with the number of bookselling enterprises per capita declining by 29.7% over the sample period.

There is high variability in the HHI over time within countries (Figure 6). This is largely a result of the construction of the measure, which relies on firm-level revenues from the Orbis dataset, where revenues fluctuate over time and may not provide a stable picture when companies enter and exit.

In terms of variation in the use of FBP policy, during the 2008-2019 period of study, there is one instance of adoption of FBP laws (Slovenia in 2015) and one instance of revocation

of FBP laws (Denmark in 2010).¹⁷ Between 1996 and 2020, we see 6 instances of adoption of FBP laws (Austria in 2001; Belgium in 2020; Croatia in 2008; Greece in 1998; Italy in 2002; and Slovenia in 2015)¹⁸ and 2 instances of revocation of FBP laws (Denmark in 2010; Hungary in 2007). Greece revoked the FBP policy for certain categories of books in 2015 but re-enacted (adopted) the policy again in 2019. The Greek revocation did not apply to literature titles, where FBP still applied and given the importance of such titles in our dataset, and the inability to separate literature books from other categories, we ignore this change.¹⁹ The status of FBP policies across Europe, between 2008 and 2019, is shown in Figure 1 overleaf and, between 1996 and 2020, in Figure 2 in Annex A1.

Figure 1: FBP status across the EU27+UK, 2008-2019



Note: Orange denotes countries which have never had an FBP policy between 2008 and 2019; shaded orange are countries which adopted an FBP policy; blue are countries which have always had an FBP policy; and shaded blue are countries which revoked an FBP policy. Source: Authors' illustration.

¹⁷Note, the Slovenian FBP adoption occurred in August 2014 but we set the FBP variable to change in 2015 to ensure that the effect of the policy adoption is fully observed in our annual data, as described in Section 4.

¹⁸Austria's policy was introduced in June 2000, Belgium's in April 2019, Croatia's in April 2007 and Italy in September 2001.

¹⁹Greece is still included as an adopter in the long book price series, where it adopted an FBP policy in 1998. As a robustness check, we completely exclude Greece from our analysis and find that our results are largely unaffected, and the main conclusions hold.

5 Methodology

Let Y_{it} be the outcome of interest (book prices, book sales, number of bookselling enterprises, HHI) in country i at time t and FBP_{it} be a dummy variable for whether the intervention has affected country i at time t . We then estimate the following two-way fixed effects model, where α_i and γ_t are fixed effects for countries and time, respectively and X_{it} are the relevant country-level control variables and ϵ_{it} is an error term:

$$Y_{it} = \alpha_i + \gamma_t + \beta FBP_{it} + \delta X_{it} + \epsilon_{it} \quad (1)$$

The estimated impact of a FBP policy is given by the difference-in-difference estimate of β . Standard errors are made robust and are clustered at the country level (Bertrand et al. 2004).²⁰

We assume that FBP treatment is independently assigned and is not endogenous on book price, book sales, number of bookselling enterprises or HHI. We present evidence in Section 7.3 from balancing tests, which supports this assumption, but, furthermore, note that even if treatment was not independent, the resulting bias would run in the opposite direction to our findings. In other words, if FBP was more likely in a given country because the price of books were perceived as being too low, then we would expect to see upward bias in the coefficient on prices. This suggests that our estimates are conservative and further supports our overall conclusions.

6 Results and discussion

6.1 Book sales

For book sales, we see a positive, statistically significant, coefficient attached to the FBP policy variable, even in the parsimonious model (Table 2, column 1). The following columns of Table 2 add a number of controls, including CPI, GDP, unemployment, total population, labour cost, internet access and net earnings (columns 2 to 7), with the coefficient of interest remaining positive and statistically significant. This indicates that FBP policies result in a greater volume of book sales compared to countries without FBP policies. Given the general decline in book sales over time, this is more precisely stated as FBP policies result in book sales declining less than would be the case without such a policy. Adding additional controls to the model does not cause a large change in the magnitude of the coefficient of interest. Our preferred specification (column 7) indicates that countries with an FBP policy see a sizeable increase of 16.1% in the volume of book sales, relative to countries without an FBP policy.

As discussed, we would expect the price of bestsellers to be most affected by an FBP policy. Consequently, dividing the turnover measure by an index more heavily weighed by the price of bestsellers would produce a conservative estimate for the effect on book

²⁰Rokicki et al. (2018) find that cluster-robust standard errors work sufficiently well so long as the number of countries is greater than 20 and the sample is balanced; there are 28 countries in our dataset which is balanced. Whilst it is also noted that cluster-robust standard errors are sensitive when the proportion of treated clusters is small (Conley & Taber 2011), this is a greater issue when the overall number of clusters is small. In any case, in Section 7 we check the sensitivity of our findings using bootstrap and Wild bootstrap standard errors and find that our conclusions are unchanged, with the exception of the findings on bookselling enterprises, which is statistically insignificant when using a Wild bootstrap.

sales. Therefore, we expect that our finding for book sales is an underestimate of the true effect, had the turnover measure been divided by a more appropriate price index.

Next, we compare Slovenia, which adopted an FBP policy, and Denmark, which revoked an FBP policy, to examine whether there is a difference in effect between these two groups.²¹ The positive effect on book sales occurs across both adopters and revokers (Table 3).

Table 2: Effect of FBP policy on book sales (volume)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FBP	0.124*** (0.0291)	0.114*** (0.0193)	0.120*** (0.0219)	0.131*** (0.0199)	0.130*** (0.0225)	0.137*** (0.0186)	0.149*** (0.0221)
CPI		3.372** (1.272)	3.123** (1.239)	3.024** (1.152)	3.015** (1.179)	1.431 (1.031)	1.388 (0.906)
Log GDP			0.861 (0.583)	0.307 (0.627)	0.334 (0.608)	0.201 (0.476)	0.296 (0.501)
Unemployment Rate				-0.0375* (0.0185)	-0.0363* (0.0190)	-0.0342* (0.0184)	-0.0250 (0.0206)
Population					-0.110 (0.470)	0.362 (0.609)	-0.161 (0.565)
Labour Cost						0.00963*** (0.00345)	0.00952*** (0.00335)
Internet Access							0.00799 (0.00503)
Observations	289	289	289	289	289	289	289

Note: Netherlands is not included in the sample as turnover data is not available. Some country-year observations for book sales volume are missing from the underlying data source. Standard errors are cluster-robust. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 3: Effect of FBP policy on book sales (volume) – adopters versus revokers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Adopters (n=277)	0.0857 (0.0577)	0.116* (0.0567)	0.127** (0.0533)	0.134** (0.0494)	0.134** (0.0493)	0.134*** (0.0475)	0.115** (0.0467)
Revokers (n=277)	0.152*** (0.0535)	0.0868 (0.0654)	0.0864 (0.0595)	0.104* (0.0546)	0.100* (0.0561)	0.117** (0.0490)	0.164*** (0.0507)

Note: Slovenia adopts the policy whilst Denmark revokes. Netherlands is not included in the sample as turnover data is not available. Some country-year observations for book sales volume are missing from the underlying data source. Standard errors are cluster-robust. ***p < 0.01, **p < 0.05, *p < 0.1.

6.2 Book prices

For book prices, we see a negative, statistically significant, coefficient attached to the independent variable across all models in the shorter data series (2008-2019). The interpretation of this is that countries with an FBP policy have lower prices (relative to

²¹When focusing on adopters (Slovenia) we drop the revoker (Denmark), and vice versa.

their 2015 baseline level) than non-FBP countries, or in other words, countries with FBP policies have lower price growth than countries without FBP policies.²²

Looking through the results individually, we see that even in the parsimonious model (Table 4, column 1) there is a statistically significant negative coefficient. Clearly, any change in the growth rate of book prices may simply reflect average changes in general price levels, so we next control for CPI (column 2), again finding a negative, statistically significant, coefficient on FBP. Additional controls for GDP, unemployment, total population, labour cost and internet access (columns 3 to 7) similarly yield a negative coefficient. Note, that adding additional controls does not cause a large change in the magnitude of the coefficient of interest.²³ In our preferred specification (column 7), we can see that having an FBP policy leads to lower book price growth (relative to 2015 prices) of 6.9% in FBP countries compared with non-FBP countries.

We again present the disaggregation by Slovenia (adopter) and Denmark (revoker) which shows that both adopters and revokers experience a statistically significant reduction in book price growth under an FBP policy compared to non-FBP countries (Table 5). In other words, adopting an FBP policy results in lower book price growth, whilst revoking an FBP policy results in higher book price growth. However, this result is not statistically significant for the revokers sub-sample in specification (7) when including lack of access to the internet as a control variable (which is itself statistically significant).

On the other hand, focusing on the extended dataset available for book prices, which exists from 1996 to 2020, we do not see strong evidence for any difference in the growth rate of book prices between FBP and non-FBP countries. In this extended sample, we have an increased number of adopters/revokers. During this period, there are 6 instances of adoption of FBP laws (Austria, Belgium, Croatia, Greece, Italy and Slovenia) and 2 instances of revocation of FBP laws (Denmark and Hungary).

We re-estimate equation (1) using this full series of pricing data. It can be seen in Table 6 that there is some evidence that FBP countries have a lower book price than non-FBP countries. However, this is only the case in the parsimonious model (column 1) and the full specification (column 7). Caution should be exercised when looking at this full specification (column 7), as the internet access variable has a more limited time span (available only from 2005), which reduces the overall sample size and may exclude certain changes in FBP policy.

It can be seen, in Table 7, that there is no effect of FBP policies on price growth for Denmark, which revoked, but there is some evidence that Slovenia, which adopted FBP policies, saw negative growth in FBP prices, although again, this is only the case for the full specification (with a reduced sample). The revoker result may make sense following the literature on post-collusion strategies which find that firms do not fully return to competitive outcomes (Harrington Jr. 2004).

The insignificant finding may reflect weaknesses in the data. However, recall that our book price data is likely biased towards the price of bestsellers. This ought to increase the confidence in our finding that there is no effect of the FBP policy on book prices,

²²The data available to us is insufficient for us to comment on the difference in the absolute level of book prices between FBP and non-FBP countries. We investigated the existence of price quote data (underlying the HICP calculations) and price level data provided by Eurostat but unfortunately neither source was available for the full set of countries included in this study.

²³The estimated effect on FBP is largely insensitive to the inclusion of additional variables, suggesting that the identifying assumption is confirmed by this form of the coefficient comparison test (Pei et al. 2019).

as any positive effect on book prices would be particularly expected on the price of bestsellers.²⁴ That no such finding is discovered boosts the confidence in the results and we might expect to see a stronger (and potentially statistically significant result) in a more representative sample of book prices.²⁵

Overall, given the evidence from the extended time period sample, and due to concerns around robustness for the restricted sample of book price (see Section 7), we conclude that countries which implement FBP policies do not see the prices of books grow faster, or slower, than non-FBP countries. This finding is somewhat surprising given that the policy is designed to reduce the ability of booksellers to discount book prices. We use insights from the vertical contracts literature to explain such a finding, suggesting that a potential mechanism for this result is that FBP policies result in greater upstream (publisher) bargaining power, which allows the publisher to reduce the wholesale price to reduce double marginalisation, resulting in lower consumer prices.

Table 4: Effect of FBP policy on book prices

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FBP	-0.0796*** (0.0210)	-0.0806*** (0.0169)	-0.0803*** (0.0163)	-0.0788*** (0.0167)	-0.0808*** (0.0166)	-0.0803*** (0.0173)	-0.0719** (0.0299)
CPI		0.451 (0.713)	0.439 (0.689)	0.414 (0.681)	0.403 (0.681)	0.0487 (0.771)	-0.0103 (0.777)
Log GDP			0.0288 (0.145)	-0.0639 (0.196)	-0.00889 (0.188)	-0.0186 (0.212)	0.0523 (0.174)
Unemployment Rate				-0.00691 (0.00710)	-0.00442 (0.00631)	-0.00370 (0.00677)	0.00188 (0.00624)
Population					-0.205 (0.354)	-0.135 (0.330)	-0.482 (0.324)
Labour Cost						0.00212 (0.00169)	0.00215 (0.00166)
Internet Access							0.00517*** (0.00176)
Observations	336	336	336	336	336	336	336

Note: Standard errors are cluster-robust. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 5: Effect of FBP policy on book price – adopters versus revokers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Adopters (n=324)	-0.0994*** (0.0215)	-0.0927*** (0.0186)	-0.0919*** (0.0178)	-0.0908*** (0.0175)	-0.0910*** (0.0178)	-0.0916*** (0.0165)	-0.0983*** (0.0149)
Revokers (n=324)	-0.0404** (0.0157)	-0.0491*** (0.0153)	-0.0491*** (0.0150)	-0.0469*** (0.0152)	-0.0508*** (0.0131)	-0.0487*** (0.0131)	-0.0180 (0.0163)

Note: Slovenia adopts the policy whilst Denmark revokes. Standard errors are cluster-robust. ***p < 0.01, **p < 0.05, *p < 0.1.

²⁴los Santos & Wildenbeest (2017) find that discounting is higher for more popular books than less popular books and that there is a general pattern of bestsellers being used as loss leaders.

²⁵Recall that bestsellers are more likely to be discounted than special interest books.

Table 6: Effect of FBP policy on book prices (full series – 1996-2020)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FBP	-0.0595** (0.0270)	-0.0374 (0.0427)	-0.0362 (0.0438)	-0.0393 (0.0405)	-0.0417 (0.0394)	-0.0467 (0.0369)	-0.0683** (0.0315)
CPI		1.509*** (0.280)	1.450*** (0.298)	1.240*** (0.321)	0.886** (0.335)	0.717 (0.459)	0.0980 (0.731)
Log GDP			0.0697 (0.106)	0.0571 (0.135)	0.184 (0.132)	0.193 (0.139)	0.244 (0.179)
Unemployment Rate				-0.00160 (0.00555)	0.00624 (0.00603)	0.00694 (0.00632)	0.0118 (0.00778)
Population					-0.549** (0.246)	-0.559** (0.248)	-0.699** (0.274)
Labour Cost						0.00116 (0.00182)	0.00199 (0.00194)
Internet Access							0.00485** (0.00183)
Observations	673	673	669	651	651	650	416

Note: Standard errors are cluster-robust. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 7: Effect of FBP policy on price, disaggregated by FBP status, (full series - 1996-2020)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Adopters	-0.0662 (0.0486)	-0.0369 (0.0300)	-0.0344 (0.0297)	-0.0380 (0.0277)	-0.0374 (0.0290)	-0.0442 (0.0297)	-0.0973*** (0.0325)
Sample size	628	628	624	606	606	605	386
Revokers	-0.0355 (0.0366)	-0.0235 (0.105)	-0.0265 (0.100)	-0.0317 (0.0886)	-0.0397 (0.0823)	-0.0435 (0.0749)	-0.0249 (0.0262)
Sample size	529	529	525	509	509	509	328

Note: Adopters are Austria, Belgium, Croatia, Greece, Italy and Slovenia. Revokers are Denmark and Hungary. Standard errors are cluster-robust. ***p < 0.01, **p < 0.05, *p < 0.1.

6.3 Potential mechanism

Overall, we conclude that FBP policies have no noticeable effect on the growth rate of book prices but do result in a greater output of books. This raises two interesting questions: (1) why do we not see FBP policies increasing prices more relative to non-FBP countries, as we would expect, and (2) why do FBP countries have a higher book sales volume relative to non-FBP countries when there is no change in price?

We turn to the literature on vertical contracts to help explain the first question. Johnson (2017) considers the pricing implications under an agency model (the prevailing model under FBP policy), where upstream firms (publishers) set final retail prices after negotiating the retailer's (booksellers) royalty share, compared with the wholesale model (no-FBP policy), where downstream retailers set the final retail prices after negotiating the wholesale price. He finds that take-it-or-leave-it offers by the bargaining entity leads

to lower retail prices under the agency model than the wholesale model, so long as the publisher is the entity making the offers. De los Santos et al. (2018) extend this model to consider other types of arrangements, with the key insight being that whether prices are higher or lower under agency versus wholesale models depend upon the relative bargaining power between publishers and retailers. They theoretically show that if the upstream firm (publisher) has bargaining power vis-a-vis the downstream firm (bookseller), then agency prices would be lower than wholesale prices, with knock-on effects to final retail prices. The opposite also holds. If the downstream firm (bookseller) has bargaining power relative to the upstream seller, then agency model prices would be higher than wholesale model prices. The intuition behind this is that the price-setting firm has an incentive to set input prices to reduce the double marginalisation problem, thereby reducing final retail prices.

This argument may help explain why some studies, such as los Santos & Wildenbeest (2017), find that a switch from the agency model (i.e. FBP policies) to the wholesale model (i.e. non-FBP policies) results in a reduction in prices - contrary to the evidence found here. Their study focuses on the short-term effects of the switch and, as acknowledged by the authors, does not reflect longer term structural changes to the market structure of the book industry and therefore relative bargaining powers which can affect the final retail price. Instead, our results are more closely aligned with other findings in the literature (e.g. Gail & Klotz 2021).

To see if such an argument can explain our findings, we wish to test whether FBP policies affect the market power of booksellers in the market, thereby altering the relative bargaining power between upstream and downstream firms. To test this hypothesis, we estimate bookseller market power in each country, over time, proxied by the HHI and estimate a similar two-way fixed effects model, as above. As an additional, but indirect, test of this hypothesis, we also estimate the effect of FBP policies on the number of firms operating in the bookselling retail market. Whilst this does not directly test the hypothesis – as the number of firms in the market does not perfectly correlate with concentration – it does provide an indication of what is happening in the market and provides further supportive evidence. Moreover, a great number of bookshop retailers is likely to increase the availability of access to books to the wider population and may directly explain an increase in the number of books sold (thereby answering our second question).

The results, shown in Tables 8 and 10, provide evidence that FBP countries have a lower HHI (in the bookselling retail sector) and a greater number of bookselling retailers compared with non-FBP countries. The existence of an FBP policy results in a 0.118 unit reduction in HHI and a 0.636 unit increase in bookselling retailers per 100,000 capita (column 6).²⁶ This indicates that the market power of retailers is lower in countries with FBP policies. Disaggregating these results by adopter/revoker sub-sample shows that the effect is stronger for revokers (Denmark) than adopters (Slovenia), although a number of results for number of bookselling enterprises are no longer statistically significant when disaggregated (Tables 9–11).

We conclude that, whilst there are flaws to both tests, the fact that both results supports the bargaining power hypothesis provides strong evidence in favour of this argument. The

²⁶In other words, this is an increase of around 6 bookselling retailers per million population (an average increase of 10%), a not insignificant number. Note that we do not control for total population in these models as we are dealing with a per capita measure. Malta and Greece are excluded from the sample due to insufficient data.

intuition is that FBP policies discourage fierce price competition, which results in reduced exit of bookselling retailers relative to non-FBP countries. Consequently, with a wider range of retailers to sell to, the bargaining power of publishers is increased and they lower the retailer's royalty share, reducing the extent of double marginalisation and lowering final consumer prices. Despite receiving a lower royalty share, existing retailers benefit from an increased quantity of output sold following from lower consumer prices, which may rise profits despite lower margins. With price competition not being permitted, such retailers can only compete on non-price dimensions, which might explain the increase in book sales volume relative to non-FBP countries. Furthermore, the increase in the number of bookselling retailers may have increased the number, and distribution, of shops, providing access to books and supported the increase in book sales observed.²⁷

Table 8: Effect of FBP policy on the HHI

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
FBP	-0.107*** (0.0333)	-0.102*** (0.0241)	-0.104*** (0.0245)	-0.106*** (0.0252)	-0.118*** (0.0292)	-0.118*** (0.0304)	-0.118*** (0.0342)
CPI		-1.028 (1.152)	-0.885 (1.199)	-0.931 (1.202)	-0.992 (1.167)	-1.249 (1.217)	-1.246 (1.217)
Log GDP			-0.184 (0.274)	0.0648 (0.471)	0.302 (0.505)	0.258 (0.486)	0.255 (0.478)
Unemployment Rate				0.0178 (0.0190)	0.0289 (0.0199)	0.0292 (0.0199)	0.0289 (0.0204)
Population					-0.890 (0.923)	-0.811 (0.919)	-0.789 (0.846)
Labour Cost						0.00213 (0.00257)	0.00213 (0.00255)
Internet Access							-0.000314 (0.00433)
Observations	323	323	323	323	323	323	323

Note: Standard errors are cluster-robust. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 9: Effect of FBP policy on the HHI – adopters versus revokers

	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Adopters (n=312)	-0.0701* (0.0409)	-0.0819** (0.0383)	-0.0826** (0.0377)	-0.0857** (0.0378)	-0.0899** (0.0366)	-0.0882** (0.0367)	-0.0892** (0.0384)
Revokers (n=313)	-0.154*** (0.0428)	-0.132*** (0.0380)	-0.136*** (0.0396)	-0.138*** (0.0392)	-0.158*** (0.0466)	-0.159*** (0.0468)	-0.161** (0.0615)

Note: Slovenia adopts the policy whilst Denmark revokes. Standard errors are cluster-robust. ***p < 0.01, **p < 0.05, *p < 0.1.

²⁷ Again, data limitations prevent us commenting on the number of individual bookshops which exist in each country.

Table 10: Effect of FBP policy on the number of bookselling enterprises per 100,000 capita

	(1)	(2)	(3)	(4)	(5)	(6)
FBP	0.485** (0.207)	0.456*** (0.0831)	0.473*** (0.0599)	0.525*** (0.0627)	0.526*** (0.0648)	0.636*** (0.223)
CPI		11.59 (10.30)	11.67 (10.13)	10.58 (9.505)	10.27 (9.076)	10.17 (8.713)
Log GDP			2.957 (3.708)	-0.658 (2.139)	-0.658 (2.176)	-0.985 (2.510)
Unemployment Rate				-0.234 (0.155)	-0.232 (0.151)	-0.228 (0.143)
Labour Cost					0.00195 (0.0158)	0.00341 (0.0184)
Internet Access						0.0485 (0.0616)
Observations	295	295	295	295	295	295

Note: Total population is not included as a control as this is a per capita measure. Sample excludes Malta (insufficient observations) and Greece (structural breaks in data series). Standard errors are cluster-robust. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 11: Effect of FBP policy on the number of bookselling enterprises per 100,000 capita – adopters versus revokers

	(1)	(2)	(3)	(4)	(5)	(6)
Adopters (n=283)	0.210 (0.308)	0.363 (0.411)	0.432 (0.490)	0.481 (0.485)	0.481 (0.485)	0.428 (0.405)
Revokers (n=283)	0.795* (0.390)	0.527 (0.587)	0.470 (0.636)	0.531 (0.562)	0.533 (0.554)	0.845*** (0.287)

Note: Slovenia adopts the policy whilst Denmark revokes. Total population is not included as a control as this is a per capita measure. Sample excludes Malta (insufficient observations) and Greece (structural breaks in data series). Standard errors are cluster-robust. ***p < 0.01, **p < 0.05, *p < 0.1.

7 Robustness Checks

A number of robustness checks are conducted, including a common (or parallel) trends check, a pre-intervention placebo test, a series of falsification tests, a balancing test, the exclusion of one country at a time and robustness checks given the staggered nature of adoption/revocation in the full price series data (including the Goodman-Bacon decomposition). Additionally, we check the sensitivity of our findings using bootstrap and Wild bootstrap standard errors (Cameron et al. 2008) and find that our conclusions are unchanged, with the exception of the findings on bookselling enterprises, which is statistically insignificant when using a Wild bootstrap. The Wild bootstrap procedure is applied using the null hypothesis of no effect imposed, with Rademacher weights.

7.1 Common trends assumption

In support of the common trends assumption, we estimate the same models as in the main analysis but include treatment leads and lags, as in Autor (2003). That is, we estimated an augmented regression including all sets of controls, with lags and leads, of regression (1), of form:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{\tau=-q}^{-1} \lambda_{\tau} FBP_{i\tau} + \sum_{\tau=0}^m \beta_{\tau} FBP_{i\tau} + \delta X_{it} + \epsilon_{it} \quad (2)$$

Where $\sum_{\tau=-q}^{-1} \lambda_{\tau} FBP_{i\tau}$ represents leads of the treatment indicator, q is the number of leads, $\sum_{\tau=0}^m \beta_{\tau} FBP_{i\tau}$ represents lags of the treatment and m is the number of lags. In this study, we set $q = m = 3$. For such a test to support the common trends assumption, the coefficients on the leads ought to be insignificant and the coefficients on the lags different.

For all four variables of interest (book price, book sales, number of bookselling enterprises per capita and HHI), the coefficients on all three leads are found to be statistically insignificant (this is shown graphically in Figures 7, 8, 9 and 10 in Annex A3), suggesting the common trends assumption is not violated. However, for the restricted price series data the test only passes with 1 lag, which emphasises our focus on the long price data series, and the conclusion of no noticeable price effects.

7.2 Falsification tests

We employ four falsification tests, to improve the confidence that our results are not spurious. The first three tests adjust the outcome variable, whilst the latter adjusts the dependent variable (FBP status). Firstly, we replace the dependent variable with the log of prices for all items in the HICP and find statistically insignificant results.²⁸ Secondly, we replace our book sales variable with a measure capturing turnover from newspapers and stationery divided by the newspaper price series and again find insignificant results.²⁹ Thirdly, we replace the dependent variable of number of bookselling enterprises with the number of retail enterprises engaged in the sale of newspapers and stationery (per capita) and again find insignificant results.³⁰ Finally, we keep only pre-treatment observations for Denmark and Slovenia and adjust the period in which they changed FBP status by one year. We then re-estimate (1) for the three variables of interest and again find insignificant results on the FBP variable. Likewise, the same procedure is repeated for the pre-treatment observations of Austria, Belgium, Croatia, Denmark, Greece, Hungary and Slovenia for the full sample for book prices. Again, insignificant results are found on the FBP variable. These findings all lend further support to our causal interpretation of our main results.

²⁸CPI is excluded as a dependent variable. This finding holds in both the full and limited sample.

²⁹These measures of retail sales of newspapers and stationery and prices of newspapers and periodicals, are sourced from Eurostat and excludes book sales/prices. We study newspapers for our falsification test as these items are not affected by FBP laws and are similar to books in nature. Croatia and Romania are excluded due to breaks in the time series.

³⁰Sourced from Eurostat's Structural Business Statistics, we similarly exclude Greece and Malta from the sample for the same reasons they are excluded in the number of bookseller enterprises regressions.

7.3 Balancing test

A balancing test whereby the control variables are placed on the left-hand side of the regression, instead of the outcome variables is employed to provide further confirmation of the suitability of our identifying assumption (Pei et al. 2019). We conduct this test using the full range of control variables used in our benchmark model and find individually insignificant coefficients on the causal variable of interest (shown in Table 14, Annex A3) for each of the control variables.³¹ Additionally, a joint significance test is carried out for our preferred specification (including all control variables listed below) along the lines of the right-hand side balancing test outlined in Pei et al. (2019). The p-value from this joint significance test stands at 0.9407 for the 2008 to 2019 data and 0.9111 for the 1996 to 2020 data. These findings thereby provide further confirmation of the suitability of our identifying assumption.

7.4 Excluding one country at a time

We check whether our main results are sensitive to the exclusion of a single country, to ensure that the results are not being driven by a single country (shown in Table 15). For book price (full sample), exclusion of a single country yields a very similar coefficient of interest across samples. For book sales, exclusion of a single country again yields a very similar coefficient of interest across samples, which remains statistically significant. For bookselling enterprises, exclusion of a single country yields similar results across samples with the exception of excluding Cyprus or Denmark, when the results become statistically insignificant. Finally, for HHI, exclusion of a single country yields similar results across all samples.

7.5 Staggered treatment concerns

There has been much recent literature on the debate around staggered two-way fixed effects research designs, with Goodman-Bacon (2021) noting that the two-way fixed effects estimator (used in this paper) is a weighted average of all possible two-group/two-period difference-in-difference estimators. This itself, is an extension of the result that the two-way fixed effects estimator yields an average of treatment effects across all groups and times, some of which can have negative weights (De Chaisemartin & d’Haultfoeuille 2020). Consequently, the two-way fixed estimator is skewed by comparisons between the earlier-treated and later-treated observations. Furthermore, with treatment effect heterogeneity, either across groups or across time, staggered treatment timing leads to biased two-way fixed effects (Baker et al. 2022). To ensure that these critiques do not affect the results of our analysis, we have already estimated our model separately for adopters and revokers and can see from these results that there is in fact heterogeneous effects of adoption/revocation.

Given the staggered adoption/revocation of countries in the long price series, we also conduct the Goodman-Bacon decomposition to ensure that this staggered timing does not bias our results. The majority of the variation in the adoption sample comes from comparing always-FBP with the adopters (32.84%) and never-FBP with the adopters

³¹Unemployment is only significant at the 10% significance level, in Panel A, and not the conventional 5% level.

(63.47%) whilst only a small proportion (3.11%) comes from comparisons between the timing groups. Similarly, for the revocation sample, the majority of variation comes from comparing always-FBP with the revokers (27.01%) and never-FBP with the revokers (65.53%) with only 3.14% coming from comparison between the timing groups. The full decompositions are provided in Table 16 and Table 17 (Annex A3), demonstrating that very little of the variation originates from variation in staggered adoption.

Additionally, to reduce concern that the treatment effect variable (FBP) is picking up time-invariant cross-country differences among the always-treated sample of countries, we re-estimate the models where the treatment group only contains countries that experienced a switch in the treatment status against (a) a control group which only contains never-treated countries, and (b) a control group which only contains always-treated countries. Focusing on the parsimonious model, given the limited sample sizes, we find qualitatively similar results, strengthening the credibility and validity of the estimates (see Table 18 in Annex A3).

8 Concluding remarks

This paper is the first to evaluate the effects of Fixed Book Price policies in a multi-country study to inform upon the RPM debate. Our findings suggest that FBP (a form of RPM) may lead to a larger volume of output sold, relative to countries without an FBP policy, with no effect on product prices. These results are robust to a number of specification tests, including falsification tests, common trends assumptions and the balancing test. We explore the mechanism for such a finding and provide evidence that suggests this effect arises from an increase in the number of bookselling enterprises, which could be increasing access to books and thus increasing sales. The finding of no effect on book prices is somewhat surprising, given the aim of FBP policies in preventing discounting. We suggest that it may arise due to a reduction in retailer bargaining power which allows publishers to reduce the double marginalisation problem and thereby lower consumer prices, this is supported by evidence showing a decline in retailer HHI for FBP policy countries.

Evidence presented here provides a preliminary insight into this policy, albeit with acknowledged data limitations. Consequently, there are a number of avenues for future research to explore. Firstly, our data is only available at an aggregated level, we do not have a direct measure of quantity and the pricing data is potentially distorted towards bestsellers. Future work should examine book-level data, which is of finer and more precise quality and is available for a broad selection of books. Secondly, with more granular data, it would be possible to look at sales location data for individual bookshops and other (non-exclusive) retailers of books. Thirdly, if longer time series can be obtained, particularly for the book sales data, then more FBP change countries can be examined (e.g. Croatia in 2007 and Hungary in 2006), to provide further evidence. Finally, it might also be interesting to examine the effect of FBP policies on the variety of book titles published, potentially using ISBN data.³²

Given the limited quality of the data, we do not put much emphasise on the exact parameter estimates but instead focus on the evidence that these results provide in the

³²Whilst this has been done at the aggregate level (Canoy et al. 2006), it would be interesting to study at the individual book-level.

sense that FBP policies suggest an increased volume of book sales, with no evidence found for an increase in prices. Despite the shortcomings in data, we provide a first empirical look at the effect of FBP policies and the implications this has for policy decisions related to RPM. In itself, these results demonstrate that FBP policies may have positive effects in the book market and provide evidence to support their use. More broadly, the results show that there are potentially positive effects from a system of resale price maintenance. Whilst this finding is only directly relevant in a specific market (the book market), it might suggest that the per se illegality imposed by the EU could be excessive and may be detrimental to welfare. Instead, taking an effects based approach may lead to increases in consumer welfare, an approach taken by the US following the Leegin case.

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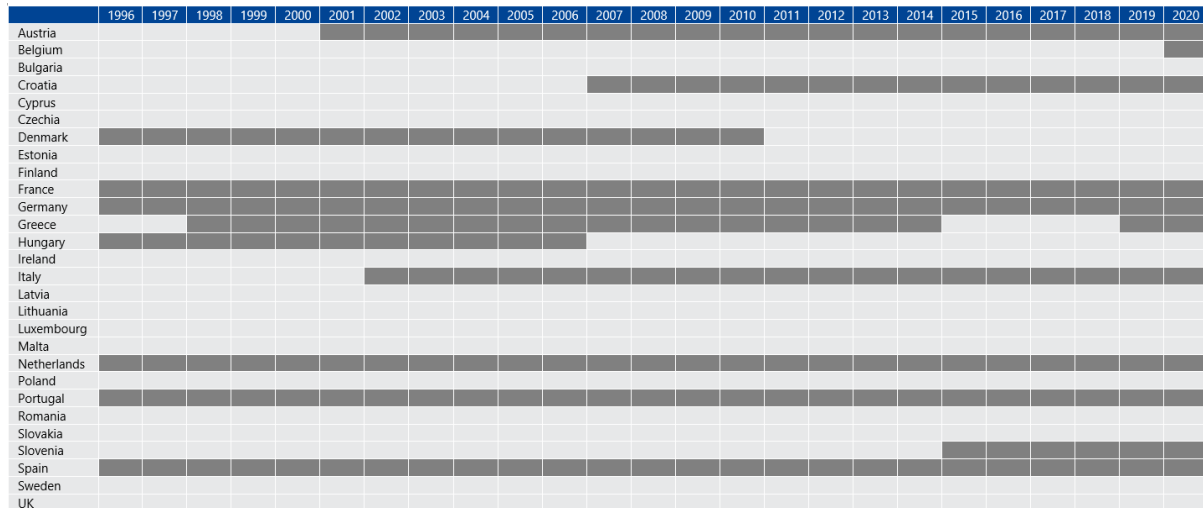
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A Annex

A.1 FBP policies

Figure 2: FBP policies, 1996-2020



Note: Dark shading shows when a country is operating under an FBP policy. See Table 12 for more detail. Earliest record of entry into our database is 1996; some FBP policies may have been in place before this but for these purposes will assume to have started in 1996. Source: Author's analysis based on academic articles, government legal sources, the Federation of European Publishers, and industry reports.

Table 12: Fixed Book Price regimes between 1996 and 2019

Country	Period of FBP
Austria	June 2000 - Present
Belgium	April 2019 - Present ³³
Bulgaria	Never
Croatia	April 2007 - Present
Cyprus	Never
Czechia	Never
Denmark	Pre-1996 - January 2011
Estonia	Never
Finland	Never
France	Pre-1996 - Present
Germany	Pre-1996 - Present
Greece	January 1998 - Present ³⁴
Hungary	Pre-1996 - October 2006 ³⁵
Ireland	Never
Italy	2002 - Present ³⁶
Latvia	Never
Lithuania	Never
Luxembourg	Never ³⁷
Malta	Never
Netherlands	Pre-1996 - Present ³⁸
Poland	Never
Portugal	1996 - Present
Romania	Never
Slovakia	Never
Slovenia	March 2014 - Present ³⁹
Spain	Pre-1996 - Present
Sweden	Never
United Kingdom	Never

Note: Earliest record of entry into our database is 1996; some FBP policies may have been in place before this but for these purposes will assume to have started in 1996. Source: Author's analysis based on academic articles, government legal sources and industry reports.

³³FBP policy was adopted in Wallonia in January 2018 but the rest of the country did not follow suit until 5th April 2019. Given that the policy started mid-way through the year, it has been excluded from the treatment group.

³⁴Greece revoked the FBP policy for certain categories of books in 2015 but re-enacted (adopted) the policy again in 2019. The Greek revocation did not apply to literature titles, where FBP still applied and given the importance of such titles in our dataset, and the inability to separate out literature books from other categories, we ignore this change.

³⁵An agreement existed which was declared unlawful by GVH (Hungarian Competition Agency) and repealed on 11th April 2006 and upheld on appeal on 15th October 2006.

³⁶Article 11 of the Law No.62 introduced FBP in March 2001, which came into effect on 1st September 2001. We therefore set the FBP variable to change in 2002 to fully capture the effect of the policy. Note that several decrees extended the FBP law up until 1st January 2005 when the law became permanent. Over time, various exemptions have made the law more/less flexible. The Levi Law, introduced in September 2011, defined the maximum discount retailers could apply.

³⁷Informal agreements exist but there is not widespread compliance. Political discussions are ongoing regarding the potential adoption of an FBP policy (private correspondence with Fédération Luxembourgeoise des Editeurs de Livres).

³⁸Law came into force on 1st January 2005. Prior to 2005 book prices were agreed in negotiations between publishers and booksellers.

³⁹There was a transitional period of 6 months, such that the law came into force on 15th August 2014 (IPA 2014).

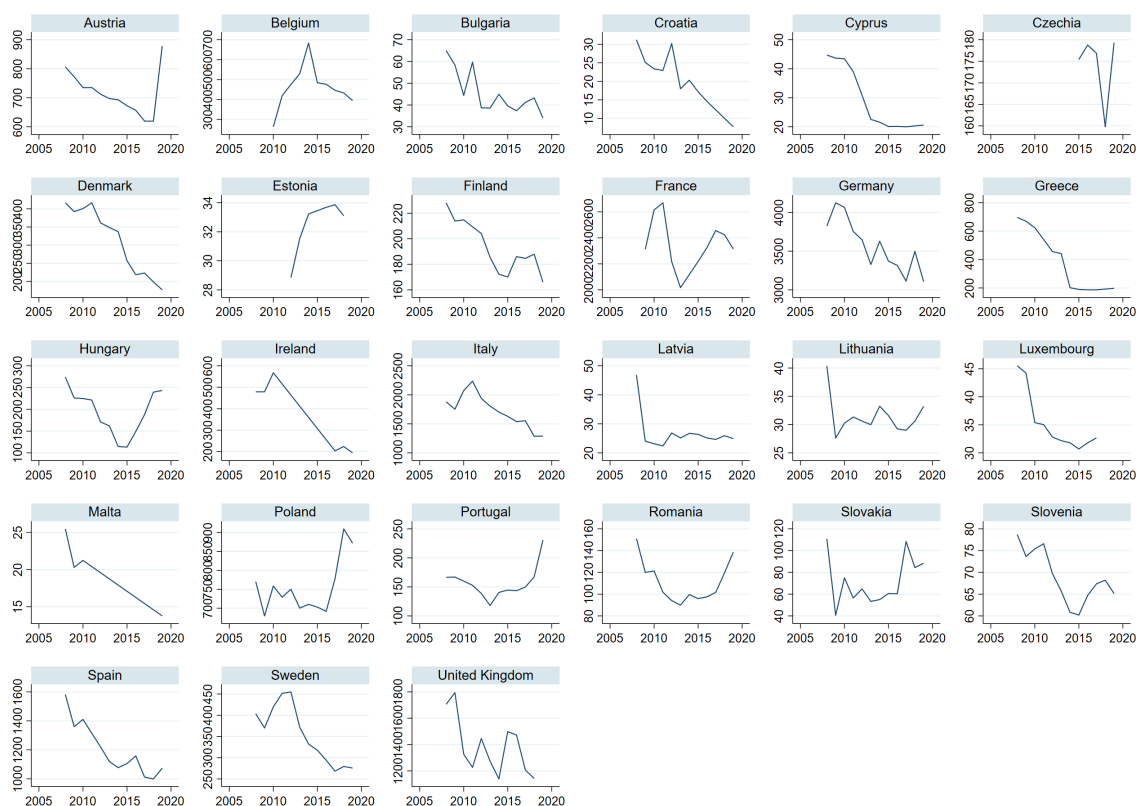
A.2 Descriptive statistics

Table 13: Descriptive statistics for book price series data, 1996 to 2020

Name	Description	Obs.	Mean	Std. Dev.	Min	Max
FBP	Dummy variable (0 = No FBP, 1 = FBP)	700	0.33	0.47	0	1
Book Price*	Annual average index (2015=100)	673	88.45	15.31	27.52	162.00
CPI	Annual average index (2015=100)	696	86.21	16.54	3.25	113.15
GDP*	Chain linked volumes, index (2015=100)	695	89.76	17.12	37.21	136.22
Unemployment Rate	Rate of unemployment as % of population	670	5.39	2.61	1.30	17.30
Total Population*	Value (millions)	700	17.82	22.55	0.38	83.17
Labour Cost	Index (2015=100)	690	89.58	18.61	2.50	151.50
Internet Access	% of total population who have never used the internet	418	23.00	15.64	1.00	74.00

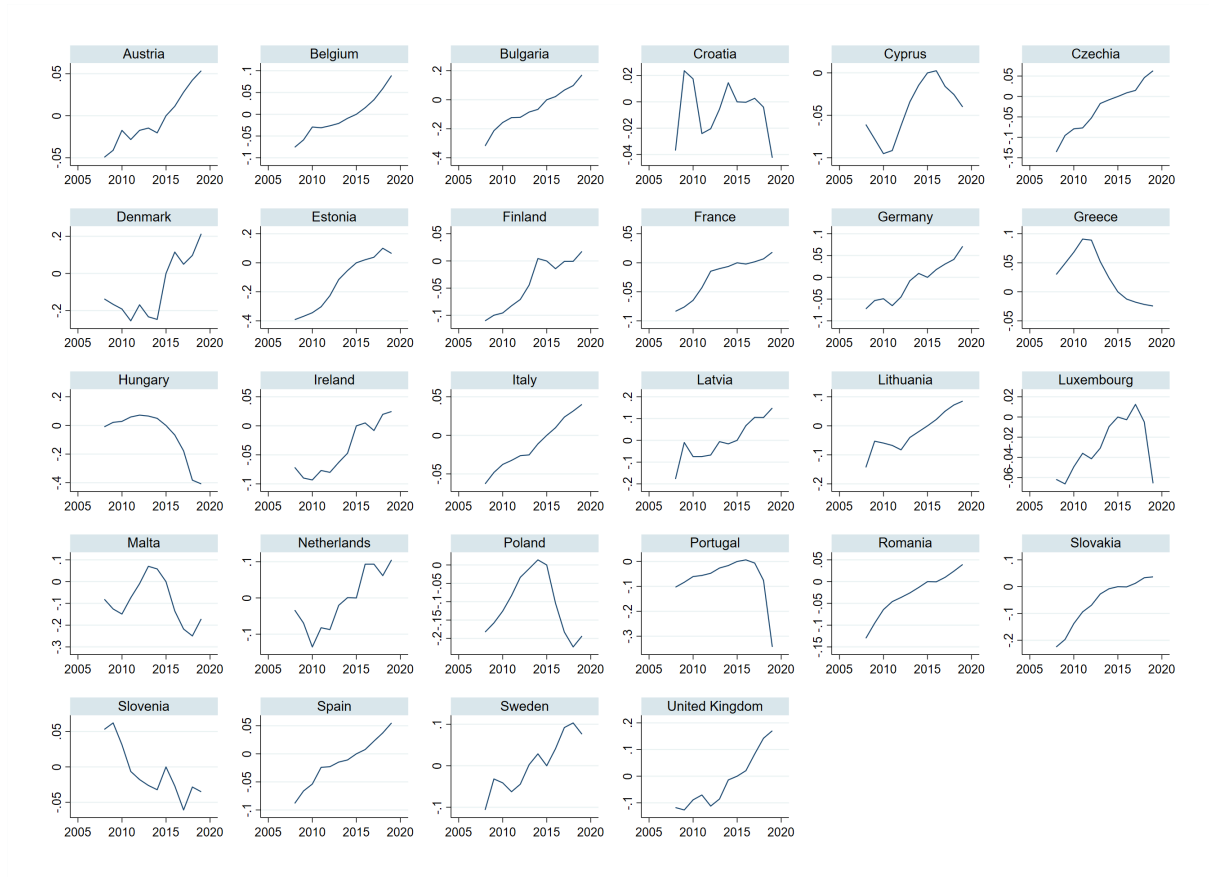
Note: Internet access is not available prior to 2006. *This variable is summarised before the log-transformation has been applied (and used in the econometric model) for ease of reading. Source: Authors' calculations based on Eurostat data.

Figure 3: Country-level trends in output of books sold



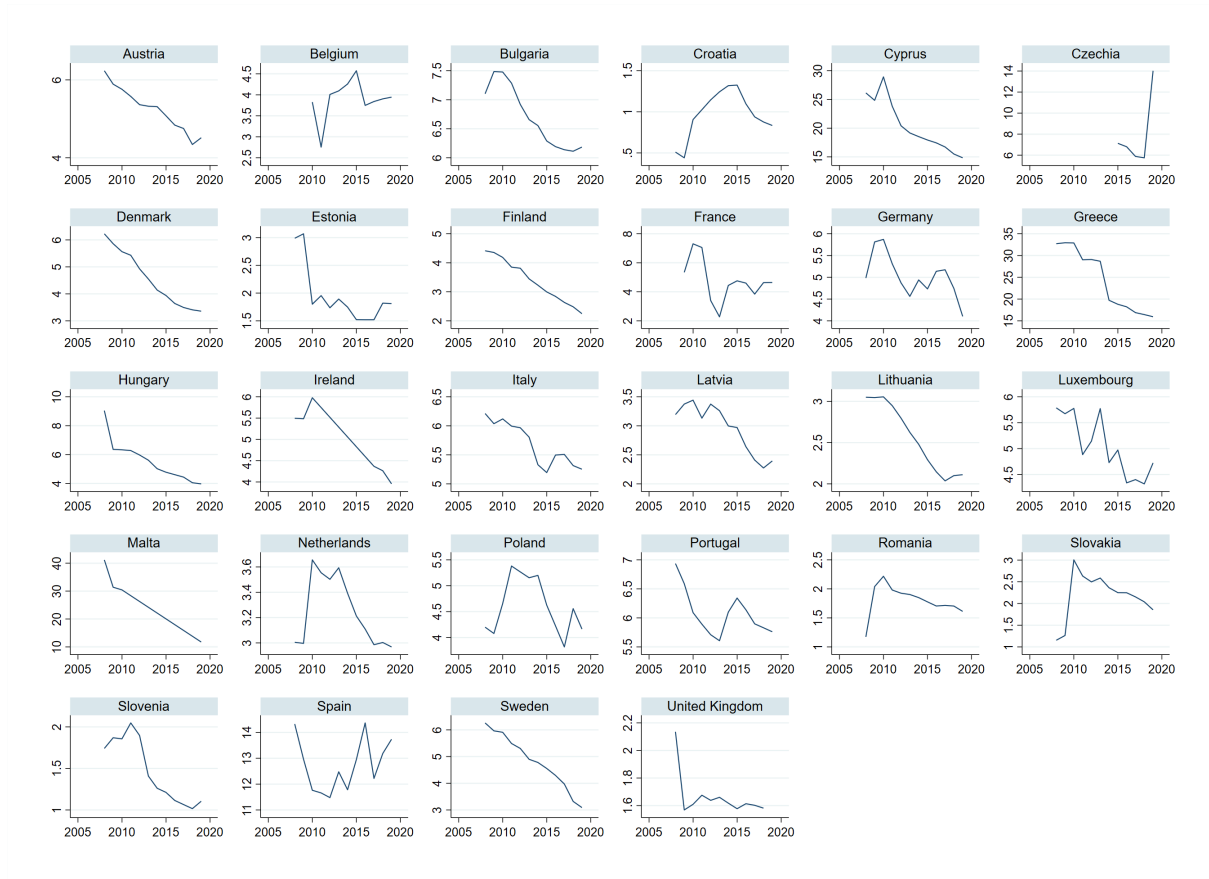
Note: Figure shows the natural log of the quantity variable.

Figure 4: Country-level trends in book price sold



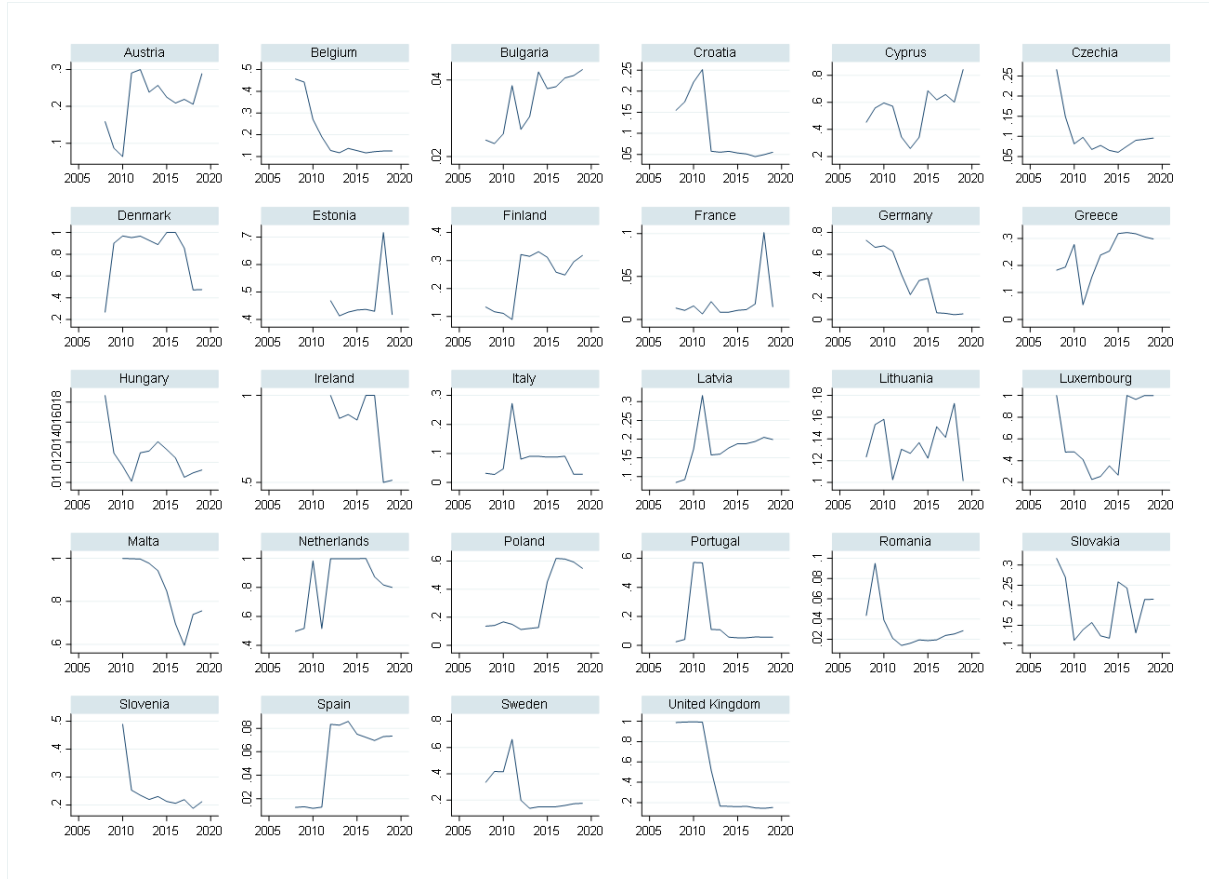
Note: Figure shows the natural log of the price variable, indexed to 2015.

Figure 5: Country-level trends in number of bookselling enterprises



Note: Figure shows the number of bookselling enterprises per 100,000 capita.

Figure 6: Country-level trends in the HHI



Note: Figure shows the level of HHI.

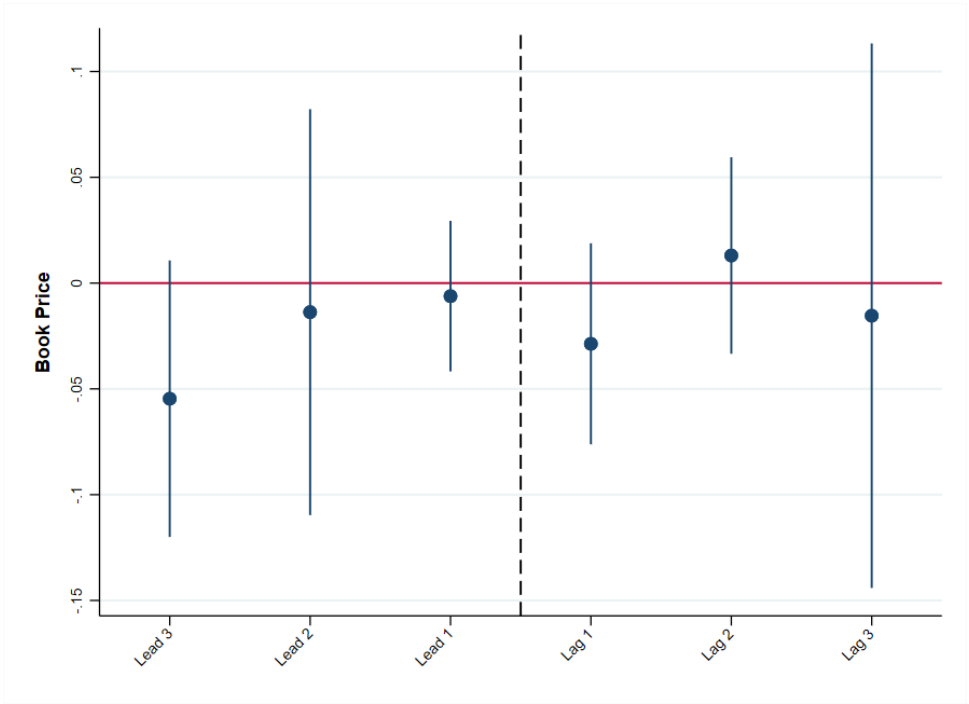
A.3 Robustness test results

Table 14: Balancing tests

	(1) Log GDP	(2) CPI	(3) Unemploy- ment	(4) Labour Cost	(5) Population	(6) Internet Access
Panel A: Individual Significance Tests, 2008 to 2019						
FBP	-0.0117 (0.0135)	0.00229 (0.0113)	0.360* (0.203)	0.235 (1.851)	-0.00880 (0.00615)	-2.418 (2.684)
Observations	336	336	336	336	336	336
R-squared	0.576	0.868	0.408	0.345	0.078	0.814
Panel B: Individual Significance Tests, 1996 to 2020						
FBP	-0.0189 (0.0350)	-0.0298 (0.0389)	0.134 (0.376)	-3.750 (3.225)	-0.00316 (0.0216)	-3.019 (3.494)
Observations	695	696	670	690	700	418
R-squared	0.761	0.869	0.222	0.720	0.096	0.852

Note: Standard errors are cluster-robust. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Figure 7: Common trends (full specification) - Book Price



Note: Book price data is based on 1996 to 2020.

Figure 8: Common trends (full specification) - Book Sales

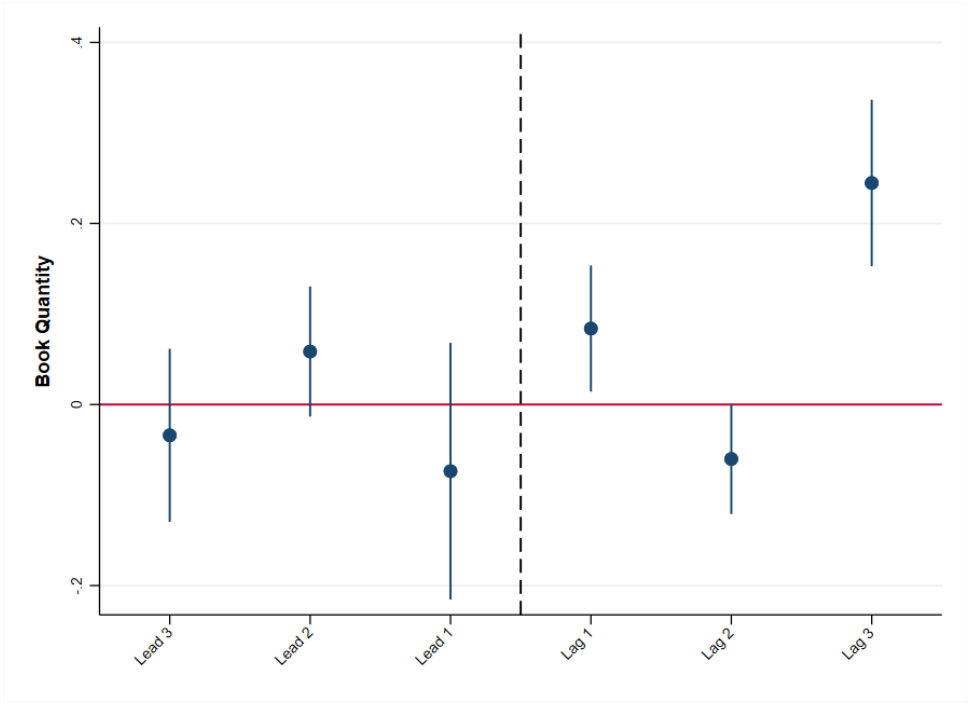


Figure 9: Common trends (full specification) - Bookshops

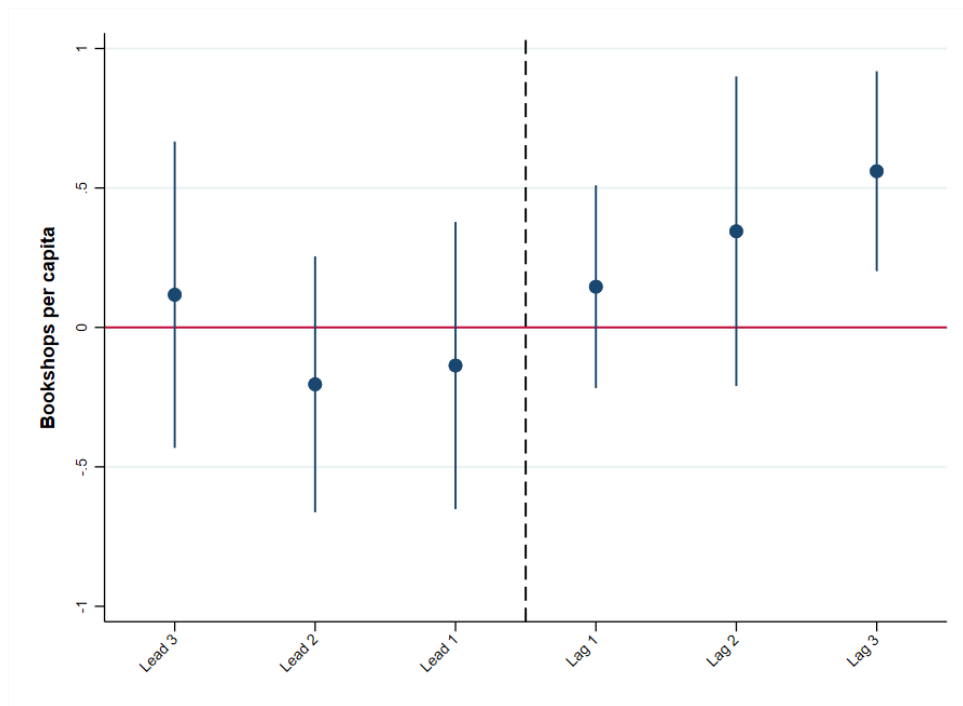


Figure 10: Common trends (full specification) - HHI

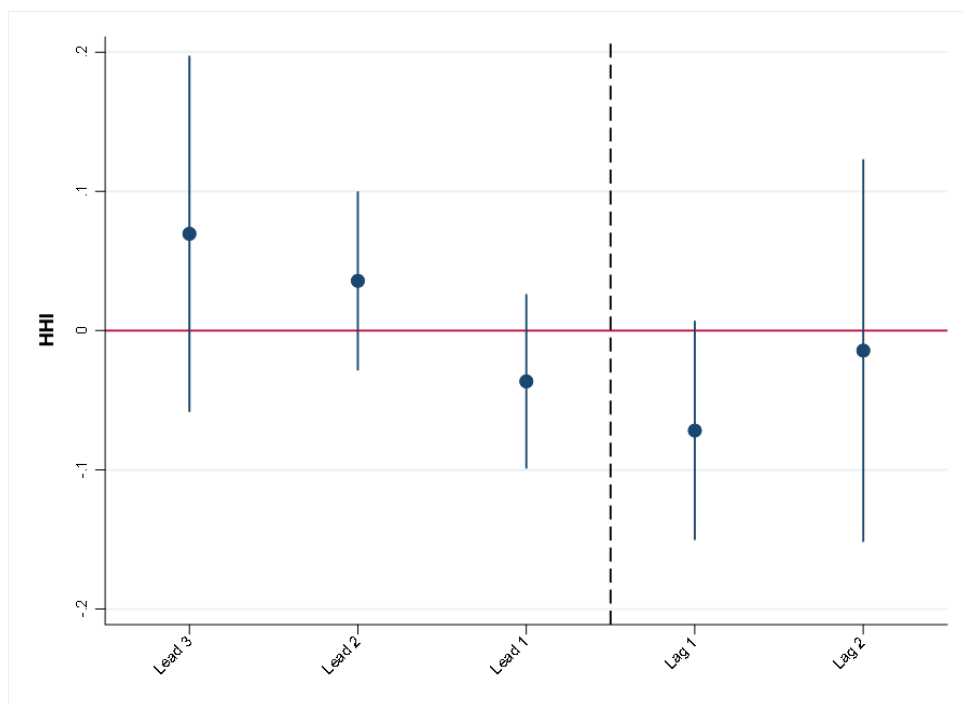


Table 15: Excluding one country at a time

Excluded Country	Price	Sales	Enterprises	HHI
Austria	-0.0714**	0.149***	0.639**	-0.123***
Belgium	-0.0714**	0.143***	0.639***	-0.113***
Bulgaria	-0.0691**	0.146***	0.642***	-0.119***
Croatia	-0.0726**	0.143***	0.664***	-0.119***
Cyprus	-0.0695**	0.150***	0.423	-0.114***
Czechia	-0.0720**	0.149***	0.634**	-0.116***
Denmark	-0.0983***	0.115**	0.428	-0.0892**
Estonia	-0.0708**	0.149***	0.646***	-0.117***
Finland	-0.0717**	0.151***	0.648**	-0.124***
France	-0.0722**	0.152***	0.661***	-0.119***
Germany	-0.0708**	0.150***	0.651***	-0.112***
Greece	-0.0751**	0.137***	0.636***	-0.116***
Hungary	-0.0785**	0.153***	0.619***	-0.118***
Ireland	-0.0725***	0.150***	0.627***	-0.119***
Italy	-0.0708**	0.147***	0.668***	-0.122***
Latvia	-0.0709**	0.146***	0.633***	-0.121***
Lithuania	-0.0714**	0.147***	0.632***	-0.118***
Luxembourg	-0.0712**	0.154***	0.643***	-0.120**
Malta	-0.0761**	0.148***	0.636***	-0.124***
Netherlands	-0.0710**	0.149***	0.617***	-0.128***
Poland	-0.0749**	0.153***	0.623***	-0.113**
Portugal	-0.0728**	0.155***	0.656***	-0.123***
Romania	-0.0698**	0.157***	0.734**	-0.120***
Slovakia	-0.0730**	0.150***	0.615***	-0.117***
Slovenia	-0.0180	0.164***	0.845***	-0.161**
Spain	-0.0715**	0.150***	0.673***	-0.117***
Sweden	-0.0711**	0.149***	0.656**	-0.112***
United Kingdom	-0.0714**	0.153***	0.637***	-0.110***

Note: Greece and Malta were excluded from bookselling enterprises regressions. Standard errors are cluster-robust. ***p < 0.01, **p < 0.05, *p < 0.1.

Table 16: Goodman-Bacon decomposition of adopter results, 1996 to 2020

	Beta coefficient	Total Weight
Timing groups	-0.000377	3.11%
Always versus Timing	-0.0512	32.84%
Never versus Timing	-0.1409	63.47%
Always versus Never	5.1469	0.18%
Within	-0.5689	0.40%

Note: Standard errors are cluster-robust.

Table 17: Goodman-Bacon decomposition of revoker results, 1996 to 2020

	Beta coefficient	Total Weight
Timing groups	-0.259	3.29%
Always versus Timing	-0.0979	23.84%
Never versus Timing	-0.00599	68.59%
Always versus Never	-3.323	0.37%
Within	0.612	3.91%

Note: Standard errors are cluster-robust.

Table 18: Sample decomposition: treatment versus never treated and treatment versus always treated

Control Group	Price (1996 to 2020)	Price (2008 to 2019)	Sales	Enterprises	HHI
Never Treated	-0.040	-0.082**	0.114**	0.569***	-0.129***
Always Treated	-0.069**	-0.090***	0.164***	0.396	-0.064

Note: ***p < 0.01, **p < 0.05, *p < 0.1.