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The Impact of E-book Distribution on Print Sales: Analysis of a Natural Experiment

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Abstract. Digital distribution introduces many new strategic questions for the creative industries—notably, how the use of new digital channels will impact sales in established channels. We analyze this question in the context of e-book and hardcover sales by exploiting a natural experiment that exogenously delayed the release of a publisher's new Kindle e-books in April and May 2010. Using new books released simultaneously in e-book and print formats in March and June 2010 as the control group, we find that delaying e-book availability results in a 43.8% decrease in e-book sales but no increase in print book sales on Amazon.com or among other online or offline retailers. We also find that the decrease in e-book sales is greater for books with less prerelease buzz. Together, we find no evidence of strong cannibalization between print books and e-books in the short term and no support for the sequential distribution of books in print versions followed by e-book versions.

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Keywords: digital distribution • channel • publishing industry • natural experiment

1. Introduction

Digital distribution channels introduce a variety of new opportunities and challenges for the creative industries (e.g., music, film, publishing). The book market represents one prominent (and understudied) setting where publishers face the challenge of selling books in both print and digital formats. Since the launch of the Kindle e-book reader platform in late 2007, the publishing industry has witnessed significant growth in e-book adoption and an associated growth in e-book sales. The Association of American Publishers reported that e-book sales revenue was $345 million from January to October 2010—a 171% growth from the same period in 2009—and that e-book sales made up nearly 9% of total trade book sales during this period in 2010 (Association of American Publishers 2010). Similarly, Amazon.com, the largest online seller of print and digital books, reported that Kindle sales surpassed Amazon's total hardcover sales in July 2010 and surpassed total print sales as of April 1, 2011 (Amazon.com 2011). Growth in the popularity of the e-book platform has continued, and from 2013 to 2015, e-books comprised about 20% of all book sales (Association of American Publishers 2016).

As the popularity of e-books grows, the book industry is engaged in an active debate about how the use of e-book channels will impact print sales. On one side of the debate, e-book platform providers, such as Amazon, claim that e-books do not cannibalize print sales, but rather represent mostly incremental sales. For example, Jeff Bezos, chief executive officer (CEO) of Amazon, observed in an earnings call with analysts (*Seeking Alpha* 2009):

> When people buy a Kindle, they actually continue to buy the same number of physical books going forward as they did before they owned a Kindle. And then incrementally, they buy about 1.6 to 1.7 electronic books, Kindle books, for every physical book that they buy.

On the other side of the debate, many book publishers worry that because print books and e-books offer essentially the same content in different formats, and that the consumption of e-books will cannibalize print book sales, particularly those that are priced significantly higher than their e-book counterparts. Carolyn Reidy, CEO of Simon & Schuster, put forward this view in a *New York Times* article (Rich 2009, p. WK3): "What a [book] consumer is buying is the content, not necessarily the format."

These sorts of cannibalization concerns have led many publishers to delay e-book releases in the hopes of not cannibalizing hardcover sales (see Table 1). However, discussions we have had with publishers suggest that these decisions have been made based on gut feel and instinct, rather than careful empirical analysis.

In this study we attempt to address the research question of whether and how e-books cannibalize print

Table 1. Delaying E-books

Event	Quote
September 2009: HarperCollins delays the e-book release of Sarah Palin's memoir by five months after the hardcover release date.	"The publishing plan is focused on maximizing velocity of the hardcover before Christmas." —Brian Murray, CEO, HarperCollins
November 2009: Viacom/Scribner delays the e-book release of Stephen King's new novel by six weeks after the hardcover release date.	"We think that this publishing sequence gives us the opportunity to maximize hardcover sales." —Adam Rothberg, Spokesperson, Scribner
Early 2010: Hachette Book Group delays the e-book release of nearly all of its titles by three to four months after the hardcover release date.	"I can't sit back and watch years of building authors sold off at bargain-basement prices." —David Young, CEO, Hachette
Early 2010: Simon & Schuster delays the e-book release for 35 major titles by four months after the hardcover release date.	"The right place for the e-book is after the hardcover but before the paperback." —Carolyn Reidy, CEO, Simon & Schuster

Sources. HarperCollins: https://www.wsj.com/articles/SB125427129354251281; Scribner: http://www.ew.com/article/2009/10/23/stephen-king-ebook-delay-price-wa/; Hachette and Simon & Schuster: https://www.wsj.com/articles/SB10001424052748704825504574584372263227740.

book sales by providing empirical evidence on how delaying the release of e-books impacts both hardcover sales and e-book sales. To do this, we use a novel natural experiment where, over the course of a two-month period, a particular publisher stopped providing Kindle e-books for its new releases to Amazon as a result of a dispute between the publisher and Amazon. During the dispute period, however, the publisher continued to provide its new releases in hardcover format to Amazon and other print book retailers. Because this publisher had previously released e-books at the same time as the print release, this event, which we describe in more detail below, creates a scenario where e-book release dates are exogenously delayed by between one to eight weeks (compared with hardcover release dates) for a set of titles, providing a unique opportunity to study the cross-channel effect between e-books and print books.

Our analyses show that, in total, delaying e-book release dates relative to their hardcover counterparts results in no increase in hardcover sales either on Amazon or among other physical and online retailers. However, delaying the e-book release date does result in a large *decrease* in e-book sales and total profit to the publisher.[1] The decrease in e-book sales is evident not only in the lost sales during the period of release delay but also through lower demand in the postrelease period. Additional analyses reveal that books with a smaller number of prerelease reviews suffer a greater decrease in e-book sales. In light of these findings, we suspect that some e-book buyers may stay loyal to the digital channel or format and wait to purchase e-books in the event of a delayed release, especially for books with more prerelease buzz. Furthermore, we find that delaying a Kindle release does not lead to an increase of e-book sales in a competing digital channel, Apple iBooks.

We believe this study makes several key contributions. First, to the best of our knowledge, this study is the first one to investigate the sales performance of the sequential distribution of digital and physical products in the publishing industry. Our empirical results offer important managerial implications for book publishers and potentially firms in other creative industries as well. The key implication is that the strategy of delaying the release of e-books could do more harm than good by not increasing physical sales but significantly reducing digital sales. Second, our study adds to a growing academic literature analyzing the impact of digital distribution channels on existing sales channels. We extend this literature by using a novel natural experiment and sales data collected from the understudied electronic publishing industry, an industry that is growing in importance. Third, these results suggest that consumers can form strong channel (or format) preferences between physical and digital products and even strong platform preferences among different digital providers. Finally, our framework of how cross-channel effects are moderated by different demand-related characteristics can assist academics and managers in designing optimal digital release strategies for digital distribution channels.

2. Literature Review and Theoretical Background

Our research is related to the information systems and marketing literature analyzing cross-channel cannibalization (Deleersnyder et al. 2002, Smith and Telang 2009, Gong et al. 2015, among many others) and optimal timing of sequential distribution (e.g., Moorthy and Png 1992, Lehmann and Weinberg 2000, August et al. 2015).

One prominent research stream on cross-channel cannibalization investigates whether and how the addition of the Internet channel cannibalizes sales in established offline channels. Deleersnyder et al. (2002) find that the introduction of online newspapers

resulted in a relatively small cannibalization of physical newspaper sales, Biyalogorsky and Naik (2003) find that the introduction of online storefronts for music did not significantly cannibalize physical record sales, and Waldfogel (2009) shows that YouTube viewing has only a small negative impact on television viewing. In recent years, there have also been studies that investigate how the opening of an offline retail store affects online sales in the growing omnichannel retailing literature (e.g., Brynjolfsson et al. 2013). For instance, Avery et al. (2012) find that the introduction of a retail store selling high-end apparel, accessories, and home furnishings cannibalizes sales in the catalog channel but not the Internet channel in the short run and increases sales in both direct channels in the long run. In addition, Brynjolfsson et al. (2009) show that the cross-channel competition between online and offline retailers is more significant for mainstream products compared with niche products.

For information goods, there is a large literature on how digital distribution channels influence sales in existing channels. The impact of digital piracy on physical sales of music and movies has been extensively studied (for reviews of this literature, see Oberholzer-Gee and Strumpf 2010, Danaher et al. 2014), with the vast majority of these studies finding that piracy harms physical sales. Danaher et al. (2010) examine whether legitimate distribution channels such as Apple's iTunes Store impact piracy and (notably for our study) impact physical sales. They show that the removal of legal video content on iTunes is associated with a significant increase in the demand for pirated content but has no statistical impact on physical DVD sales. Similarly, Danaher et al. (2015) analyze how the addition of ABC television series to the Hulu platform impacted piracy and DVD sales, finding that the use of digital distribution significantly reduced the demand for pirated content but had no impact on DVD sales. Finally, Gong et al. (2015) study the cross-channel cannibalization between digital rentals and digital downloads of movies by analyzing the impact of price discounts on own- and cross-channel sales, finding that price promotions for digital downloads lead to increased digital rentals.

Although cross-channel effects have been studied in the context of movies and television shows, they have received limited attention in the context of the publishing industry. One exception is the study by Kannan et al. (2009), who investigate the optimal pricing strategies of the National Academies Press, which sells both print and digital books through its official website. Another related study by Ghose et al. (2006) examine the online sales of both used and new print books. They find that Internet channels for used books result in a relatively small cannibalization of new book sales. Our study extends this prior work by providing empirical evidence of cross-channel effects between e-books and print books sold in both online and offline stores.

Our study also contributes to the literature on the optimal timing of sequential distribution. In this literature, Moorthy and Png (1992) analyze how a seller can sequentially introduce a high-end and a low-end version of a durable product, finding that sequential introduction is preferred over simultaneous introduction when cannibalization is a concern. In the movie industry, release windows for different channels are an important business decision for practitioners and have been extensively investigated in the literature (e.g., Lehmann and Weinberg 2000, Prasad et al. 2004, August et al. 2015). Movies are traditionally released first in theaters and later in home video channels (purchase and rentals). However, several analytical and empirical studies suggest that simultaneous distributions may outperform sequential distributions. For instance, Hennig-Thurau et al. (2007) conduct a conjoint analysis and show that a simultaneous release of movies in theaters and on home video can be optimal for movie studios but will hurt other players such as theater chains. In their theoretical work, Calzada and Valletti (2012) also find that the simultaneous release of theatrical and video versions of movies can be optimal when the movie studio is integrated with the exhibition and distribution channels. In addition, Mukherjee and Kadiyali (2011) empirically test cross-channel substitution between two post-theatrical channels, home video purchases and rentals, and show that windowing (or sequential distribution) reduces the sum of revenues across two channels.

In the traditional publishing industry, books are often published sequentially first in hardcover and then in paperback formats, which follows the logic that the most expensive format should be released first (Hillary 2015). To avoid cannibalization between e-books and print books, publishers have been releasing them sequentially in general and delaying the release of e-books in particular. This study thus adds to the literature on optimal timing of sequential distribution by comparing sequential and simultaneous distributions of print books and e-books. Our research differs from prior movie studies in that we use a novel natural experiment that creates an exogenous and unintended delay in the availability of some e-book titles. This quasi-experimental setting allows us to establish causality and avoid many confounding factors common in related cross-channel settings.

From a theoretical perspective, when the digital release of a book is delayed, it may or may not increase the sales of its print version. Previous research on product stockouts has shown that consumers who face stockouts of a certain brand may switch to other sizes and varieties of the same brand (e.g., Emmelhainz et al. 1991). Similarly, e-book consumers may switch from

the focal title's e-book to its print book when its e-book is delayed, simply because e-books and print books offer the same content and could be strong substitutes for each other. If this happens, then the print sales of the book would increase when e-books were delayed. On the other hand, there are reasons to believe that e-book and print markets could be distinct as a result of e-book consumers' preferences for digital distribution channels. Marketing theory shows that markets can be segmented based on a consumer's channel preferences (Kotler 2002). Thus, it is possible that some e-book consumers may not consider print books as substitutes for e-books. In this case, the sales of the print book might not change, and digital sales could fall if consumers were unwilling to wait for the eventual digital release of the book.

Our finding that the effects of delaying digital releases can be moderated by prerelease buzz is related to several papers documenting similar types of effects. Simester et al. (2009) show that the effects of current advertising on future sales are moderated by brand awareness among consumers. Earlier studies such as Elberse and Eliashberg (2003) also report that marketing buzz can directly affect product sales. With the rise of Internet channels, online word of mouth has become an important indicator of consumers' awareness and an important driver of product sales (e.g., Luan and Sudir 2010). Publishers often actively court, and in some cases pay, top reviewers to create online word of mouth for their new books (Coster 2006). Furthermore, our result that delaying digital releases leads to no increase in print sales is consistent with the literature suggesting that consumers potentially have a strong preference for their chosen channel. Vernik et al. (2011) show that music consumers may have a strong preference for the digital format, and this preference can drive product sales. Frambach et al. (2007) demonstrate that consumers can develop higher preferences for the online purchasing channel when they have a more favorable Internet experience. Zentner et al. (2013) find that online channels could change consumers' consumption patterns and product choices.

3. Data and Description of Natural Experiment

An ideal starting point for understanding how delayed release dates for e-books affect sales would be a pure experiment where a publisher randomly assigns books to the treatment group with different amounts of delay. Because of the business consequences involved in this setup, most publishers are reluctant to implement this type of randomized experiment. Lacking these data, our research employs a natural experiment that approximates this ideal setup.

Specifically, we use data from a publisher that stopped distributing new Kindle titles to Amazon from April 1, 2010, to June 1, 2010. Prior to April 1, the policy of this publisher was to release a Kindle title on the same day the book was to be initially released in print (typically in a hardcover version). Starting on April 1, as a result of a dispute with Amazon, this publisher stopped releasing new Kindle titles to Amazon while still releasing new print titles to Amazon. On June 1, 2010, the publisher returned to the Kindle store, releasing all Kindle titles that had been released in print from April 1 through May 31 and returning to its previous policy of simultaneously releasing both Kindle e-books and their corresponding print (hardcover) versions on the same day.[2] During this event, the availability of both digital and print books in other channels remained the same over time.

This event results in the release schedule of Kindle and print titles shown in Table 2. Notably for our purposes, this event creates a "natural experiment" where certain Kindle titles are delayed by between one and eight weeks relative to their print counterparts.

To analyze the results of this "experiment," we obtained data from the publisher in question, covering Amazon print and Kindle unit sales, prices, and release dates for all newly released books from March 1, 2010, through June 30, 2010. To assess how this experiment would affect the print sales in other channels (e.g., offline/local stores), we also purchased weekly sales data from Nielsen BookScan, which is the largest continuous book sales tracking service in

Table 2. Kindle Release "Experiment"

Week(s)	Print release	Kindle release	Kindle delay
Before April 1, 2010	Print and Kindle titles released same day		0 weeks
Week of April 4	April 4	June 1	8 weeks
Week of April 11	April 11	June 1	7 weeks
Week of April 18	April 18	June 1	6 weeks
Week of April 25	April 25	June 1	5 weeks
Week of May 2	May 2	June 1	4 weeks
Week of May 9	May 9	June 1	3 weeks
Week of May 16	May 16	June 1	2 weeks
Week of May 23	May 23	June 1	1 week
After June 1, 2010	Print and Kindle titles released same day		0 weeks

the world, tracking approximately 85% of print book sales in the United States. Book retailers participating in the BookScan service include Amazon.com, Barnes & Noble, B&N.com, Kmart, Starbucks, Target, Toys "R" US, Walmart, and many others. With the BookScan print sales data, we are able to investigate the substitution not only between Amazon print and digital sales but also between overall print and digital sales.[3]

We then divide this sample into two groups:

1. *Control group*: new books that are released from March 1 to March 31 and from June 1 to June 30. These books are released simultaneously in both print and Kindle formats.

2. *Experiment group*: new books that are released from April 1 to May 31. These books are released first in print and then one to eight weeks later (on June 1) in Kindle format.

Overall, we have 83 "control" titles and 99 "experiment" titles.

3.1. Comparing the Control and Experiment Groups

On the basis of discussions with the publisher in question, there is no reason to believe that the control group will be systematically different from the experiment group. Book release schedules are set at least six months in advance, they did not change as a result of the dispute, and the timing of the dispute was not influenced by the publisher's upcoming releases.

The publisher's statements that the control and treatment groups are similar is consistent with available data regarding whether books in the control group have a similar profile to books in the experiment group on observable dimensions—which in our case include the list price, genre category (fiction or nonfiction), prerelease buzz, book weight, height, length, width, and number of pages. Summary statistics on these dimensions are provided in Table 3 separately for books in the control group and books in the treatment group. *Fiction* is a dummy variable that equals 1 if the genre of a book belongs to the fiction category and 0 otherwise.[4] *PrereleaseReviews* is the number of Amazon reviews prior to print release. Other variables are either count or continuous variables. The statistics in Table 3 show that the control and experiment titles are very similar along all these observable dimensions.

We can also empirically test whether any of the observable variables can be used to predict a particular book belonging to the experiment group. To accomplish this, we estimate the following probit model:

$$P(Experiment_i = 1 \mid Z_i) = \Phi(Z_i\beta), \tag{1}$$

where $Experiment_i$ is an indicator of whether book i belongs to the experiment group, and Z_i is a vector of independent variables described in Table 3. The results from estimating this model are reported in Table 4.

The results in Table 4 show that none of the coefficients is statistically significant at the 5% level. This is again consistent with comments from our publisher that books in the control group are materially similar to—and thus a reasonable control for—books in the experiment group.

3.2. Descriptive Analyses of Book Sales

When studying the sales patterns of these books, we focus on the sales in the first 20 weeks since each book's initial release in its respective channel. We do this because this is the period where the majority of sales occur, allowing us to keep the sales numbers comparable across different books. Figure 1 presents the average weekly sales for books in our sample in the Amazon print, BookScan print, and Kindle digital formats. Amazon print unit sales are included in the BookScan print unit sales, so the curve for Amazon print is always below the curve for BookScan print.

Table 3. Summary Statistics

	Variable	Median	Mean	SD	Min	Max	Obs.
Control group	*List Price* ($)	26.0	26.2	3.1	16.0	45.0	83
	Fiction	0	0.5	0.5	0	1.0	83
	PrereleaseReviews	2	7.9	27.7	0	243	83
	Weight (pounds)	1.1	1.2	0.4	0.5	3.6	83
	Height (inches)	1.3	1.3	0.3	0.8	2.5	83
	Length (inches)	9.0	8.8	0.6	6.9	10.0	83
	Width (inches)	6.1	6.0	0.4	4.8	7.8	83
	Pages	304.0	349.6	128.1	160.0	1,184.0	83
Experiment group	*List Price* ($)	26.0	25.9	2.0	22.0	38.0	99
	Fiction	0	0.4	0.5	0	1.0	99
	PrereleaseReviews	2	3.8	5.5	0	26	99
	Weight (pounds)	1.2	1.1	0.3	0.3	2.3	99
	Height (inches)	1.3	1.3	0.2	0.8	2.3	99
	Length (inches)	9.1	8.9	0.4	7.1	10.6	99
	Width (inches)	6.2	6.1	0.4	5.1	8.0	99
	Pages	320.0	339.6	88.9	192.0	704.0	99

Table 4. Results from a Probit Model

	(1) *Experiment group dummy*
Constant	−3.806 (2.048)
ListPrice	−0.108 (0.064)
Fiction	−0.180 (0.212)
PrereleaseReviews	−0.020 (0.015)
Weight	−0.794 (0.554)
Height	1.118 (0.866)
Length	0.716 (0.393)
Width	0.013 (0.485)
Pages	0.000 (0.003)
Log likelihood	−117.5
No. of observations	182

Notes. Standard errors are in parentheses. None of the listed variables are significantly different from zero at the $p < 0.05$ level.

All three curves follow a similar and overall decreasing temporal pattern.

To get an initial assessment of whether there are differences in the sales patterns between the control and experiment groups, we present the summary statistics in Table 5 for sales of books in the control and experiment groups in the first 20 weeks since each book's release in its respective channel (print and Kindle). The number of observations for each variable is the number of book titles multiplied by 20 weeks. We make the following observations. First, looking at the control group, these summary statistics suggest that digital sales are a significant sales channel. Digital sales make up nearly half of total sales on Amazon for this publisher, which is generally consistent with available data regarding sales patterns (e.g., Amazon.com 2011). Second, a quick comparison of the control and experiment groups reveals that e-book sales in the experiment group are significantly lower than those in the control group regardless of whether the e-book sales are measured since the print release or Kindle release. We note that print and Kindle release dates are the same for the control group; the digital sales after print release are smaller than the digital sales after Kindle release for the experiment group because digital sales are zero in the initial few weeks after print release as a result of the treatment. Although the mean and median of Amazon print sales in the experiment group are slightly higher than in the control group in these summary statistics, they seem to be nowhere near large enough to compensate for the lost e-book sales. Third, the means of the BookScan print sales for these two groups are different, but the medians are quite close (173 versus 161), suggesting that a few very popular books in the control group are driving up the average sales (and standard deviation as well) recorded by BookScan.

4. Analyses and Results

Because our dependent variables (weekly print and digital sales) are count data and we find evidence of overdispersion, we estimate a negative binomial panel regression model (Hausman et al. 1984, Cameron and Trivedi 2013). Let Y_{it} represent the unit sales generated by a particular version (print or digital) of book i in week t and follow a Poisson distribution with the conditional mean μ_{it}:

$$f(Y_{it} \mid X_{it}) = \frac{e^{-\mu_{it}} \mu_{it}^{Y_{it}}}{Y_{it}!}, \quad Y_{it} = 0, 1, 2, 3, \ldots, \tag{2}$$

Figure 1. (Color online) Average Weekly Sales (Kindle Digital on the Secondary Axis)

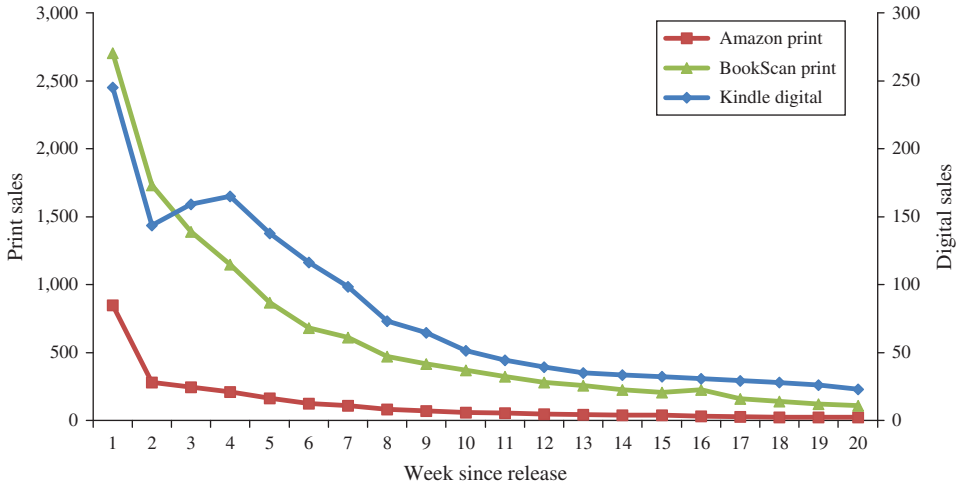

Table 5. Summary Statistics on Book Sales

	Sales channel	Median	Mean	SD	Min	Max	Obs.
Control group	*BookScan print sales*	173	801.7	2,748.0	2	38,310	1,660
	Amazon print sales	32	123.7	483.6	0	10,408	1,660
	Digital sales	20	125.2	417.6	0	9,066	1,660
Experiment group	*BookScan print sales*	161	472.2	1,207.4	1	23,054	1,980
	Amazon print sales	34	132.5	364.3	0	5,573	1,980
	Digital sales after print release	8	36.2	86.9	0	1,247	1,980
	Digital sales after Kindle release	13	40.1	86.9	0	1,247	1,980

where the Poisson parameter μ_{it} follows a gamma distribution with parameters (λ_{it}, δ_i), $\lambda_{it} = \exp(X_{it}\beta + \varepsilon_{it})$, and δ_i is the dispersion parameter. The explanatory variables X_{it} include our main variable of interest, $Experiment_i$, a dummy variable that equals 1 if book i belongs to the experiment group; $WeeksSinceRelease_{it}$, the number of weeks from the release of the specific version of book i until week t;[5] $PrintPrice_{it}$, the natural log of price for the print version of book i in week t on Amazon; $DigitalPrice_{it}$, the natural log of price for the digital version of book i in week t; $Reviews_{it}$, the natural log of the number of Amazon new reviews received by book i in week t; $Rating_{it}$, the average rating of all Amazon reviews received by book i until week t;[6] $PrereleaseReviews_i$, the natural log of the number of Amazon reviews before the print release; a series of book-level characteristics including $Fiction_i$, $Weight_i$, $Height_i$, $Length_i$, $Width_i$, and $Pages_i$; and time dummies (e.g., weekly dummies that control for time effects such as trends and seasonality).

An advantage of this model is that it allows us to directly control for any factors that may have affected

Table 6. Effects of the Delay of Digital Release on Print and Digital Sales

	(1) BookScan print sales	(2) Amazon print sales	(3) Digital sales after print release	(4) Digital sales after Kindle release
Experiment	−0.327	−0.035	−0.577**	−0.657**
	(0.195)	(0.156)	(0.162)	(0.218)
WeeksSinceRelease	−0.124**	−0.136**	−0.070**	−0.126**
	(0.021)	(0.016)	(0.016)	(0.022)
PrintPrice	−1.499**	−2.233**	0.910	−0.519
	(0.234)	(0.319)	(0.496)	(0.402)
DigitalPrice	−0.099	0.433	−4.556**	−2.201**
	(0.331)	(0.417)	(1.000)	(0.619)
Reviews	0.148**	0.167**	0.259**	0.197**
	(0.038)	(0.038)	(0.052)	(0.038)
Rating	−0.051	0.205	−0.057	0.169
	(0.128)	(0.139)	(0.100)	(0.121)
PrereleaseReviews	0.348**	0.298**	0.382**	0.515**
	(0.064)	(0.065)	(0.082)	(0.082)
Fiction	−0.092	−0.492**	1.134**	1.137**
	(0.208)	(0.159)	(0.199)	(0.223)
Weight	0.416	−0.065	−0.332	0.211
	(0.541)	(0.416)	(0.425)	(0.522)
Height	0.618	−0.272	−0.001	0.955
	(0.897)	(0.591)	(0.702)	(0.777)
Length	0.694**	0.737**	1.018**	1.021**
	(0.228)	(0.208)	(0.291)	(0.277)
Width	−0.944**	−0.876**	−0.905*	−1.028**
	(0.346)	(0.292)	(0.401)	(0.391)
Pages	0.001	0.004*	0.006**	0.002
	(0.002)	(0.002)	(0.002)	(0.002)
No. of observations	3,640	3,640	3,640	3,640

Notes. Robust standard errors are in parentheses. Time dummies are included in the estimation.
**Significantly different from zero at the $p < 0.01$ level; *significantly different from zero at the $p < 0.05$ level.

book sales during this time frame that may differ between the control and experiment groups. We note that if sales of the control books are statistically representative of what sales of the experiment books would have been if the Kindle version had been released along with the print version (as we believe is the case), then having $Experiment_i$ only in the explanatory variable X_{it} is sufficient to test the effects of the delay of digital releases.

As our data are a book-week panel, we report the results from the population averaged negative binomial estimators for panel models (Cameron and Trivedi 2013) to account for any potential within-panel correlation. Alternatively, we could employ random effects estimators for the panel model, which produce the subject-specific estimates (i.e., what would happen to a particular book if its digital release was delayed). Since we are more interested in the effect of the digital release delay on the general population, or what would happen if the digital release of an average book were delayed, we present the results from the population-averaged panel model. Note that the population-averaged estimates and subject-specific estimates are equivalent for linear models but not for nonlinear models such as negative binomial regressions.

4.1. The Effects on Print Sales and Digital Sales

Table 6 reports our main results from estimating the negative binomial panel regression model with four different dependent variables. Columns (1)–(3) examine the weekly print and digital sales in weeks 1–20 after print release. As the digital sales of the initial few weeks (since print release) are zero for the experiment group, we examine the digital sales in weeks 1–20 after Kindle release in column (4), so that the experiment group also has 20 weeks of positive sales and the sales patterns are aligned between the two groups. The coefficient estimates on the *Experiment* variable in columns (1) and (2) are both statistically insignificant, indicating that delaying Kindle releases does not lead to a significant change in overall and Amazon print sales, consistent with our summary statistics above. This implies that when the digital version of a book is unavailable on Amazon, consumers do not seem to switch to buying print books from the same retailer (Amazon) or from other online or brick-and-mortar print book retailers. The coefficient estimates on the *Experiment* variable in columns (3) and (4) are both negative and statistically significant at the 1% level.[7] The coefficient estimate of −0.577 in column (3) means that delaying Kindle releases leads to a 43.8% (i.e., $e^{-0.577} - 1 = -0.438$) decrease in digital sales after print release; the coefficient estimate of −0.657 in column (4) means that delaying Kindle releases leads to a 48.2% (i.e., $e^{-0.657} - 1 = -0.482$) decrease in digital sales after Kindle release. The lost sales in the period of release delay alone could potentially lead to the results reported in column (3), but the analysis in column (4) further indicates that the experiment group also faces lower demand after the Kindle e-book is released. In sum, these results suggest that delaying the digital release of books results in a decrease in digital sales but no increase in print sales.

The results for the control variables are largely consistent with expectations. First, the coefficient estimates on *WeeksSinceRelease* in all columns are negative and statistically significant at the 1% level. The coefficient estimate of −0.124 in column (1) of Table 6 means that print sales decline at roughly 11.7% (i.e., $e^{-0.124} - 1 = -0.117$) per week, as shown in Figure 1. Second, consistent with expectations, the coefficient estimates on *PrintPrice* are negative and statistically significant at the 1% level in columns (1) and (2) when the dependent variables are either BookScan print sales or Amazon print sales; the coefficient estimates on *DigitalPrice* are negative and statistically significant at the 1% level in columns (3) and (4) when the dependent variable is digital sales. On the other hand, the coefficient estimates on *DigitalPrice* in the regressions of print sales and the coefficient estimates on *PrintPrice* in the regressions of digital sales are all statistically insignificant. Third, the coefficient estimates on *Reviews* are positive and statistically significant at the 1% level in all regressions, but the coefficient estimates on *Rating* are all statistically insignificant. This suggests that weekly book sales are most closely associated with the number of weekly new reviews on Amazon. In addition, the coefficient estimates on *PrereleaseReviews* are all positive and statistically significant at the 1% level, implying that the prerelease buzz of a book is predictive of both its print and digital sales after release. Fourth, the coefficient estimates on *Fiction* are statistically significant at the 1% level in columns (2)–(4) but not in column (1). This suggests that the aggregate print sales of fiction and nonfiction books are similar in all channels, but consumers on Amazon seem to purchase fewer fiction print books than nonfiction print books and more fiction e-books than nonfiction e-books. Finally, among the other book characteristics, *Length* and *Width* are statistically correlated with the book sales of both formats.

4.2. Interactions with Prerelease Reviews and Genre Category

To further examine how the effects of the delay of digital releases vary across different types of books, we test whether two exogenous demand-related characteristics, prerelease buzz and the genre of a book, moderate our treatment effects.[8] Note that the number of Amazon reviews before the print release is not driven by sales after the book's release; hence, it is an exogenous variable that is not directly affected by whether the book's digital version is delayed. To conduct these

Table 7. Interactions Between *Experiment* and *PrereleaseReviews*

	(1) BookScan print sales	(2) Amazon print sales	(3) Digital sales after print release	(4) Digital sales after Kindle release
Experiment	−0.442	−0.189	−0.924**	−1.131**
	(0.254)	(0.217)	(0.221)	(0.273)
PrereleaseReviews	0.294**	0.235**	0.216*	0.319**
	(0.083)	(0.082)	(0.097)	(0.085)
Experiment × *PrereleaseReviews*	0.099	0.129	0.310*	0.395**
	(0.121)	(0.119)	(0.132)	(0.141)
WeeksSinceRelease	−0.124**	−0.136**	−0.073**	−0.122**
	(0.021)	(0.016)	(0.016)	(0.021)
PrintPrice	−1.500**	−2.251**	0.931	−0.614
	(0.235)	(0.320)	(0.480)	(0.392)
DigitalPrice	−0.111	0.431	−4.932**	−2.177**
	(0.330)	(0.409)	(0.918)	(0.536)
Reviews	0.150**	0.169**	0.272**	0.200**
	(0.039)	(0.040)	(0.052)	(0.038)
Rating	−0.053	0.203	−0.053	0.185
	(0.126)	(0.137)	(0.095)	(0.117)
Fiction	−0.071	−0.462**	1.204**	1.251**
	(0.206)	(0.158)	(0.203)	(0.218)
Weight	0.407	−0.080	−0.485	0.078
	(0.553)	(0.443)	(0.458)	(0.566)
Height	0.592	−0.301	0.037	1.030
	(0.897)	(0.594)	(0.713)	(0.782)
Length	0.682**	0.741**	1.160**	1.019**
	(0.232)	(0.207)	(0.293)	(0.280)
Width	−0.908**	−0.855**	−0.956*	−0.992**
	(0.345)	(0.288)	(0.399)	(0.384)
Pages	0.001	0.004*	0.006**	0.002
	(0.002)	(0.002)	(0.002)	(0.002)
No. of observations	3,640	3,640	3,640	3,640

Notes. Robust standard errors are in parentheses. Time dummies are included in the estimation.
**Significantly different from zero at the $p < 0.01$ level; *significantly different from zero at the $p < 0.05$ level.

two analyses, we add the interaction terms of *Experiment* × *PrereleaseReviews* and *Experiment* × *Fiction* to the list of explanatory variables X_{it}, respectively, and then estimate the model for the same four dependent variables.

In Table 7, we investigate how the level of prerelease buzz moderates the effects of the delay of digital releases. In our sample of 182 books, the average number of prerelease Amazon reviews is 5.7,[9] and the minimum and maximum of prerelease Amazon reviews are 0 and 243, respectively. The coefficient estimates on *Experiment* × *PrereleaseReviews* imply that the effect of the delay of digital releases on print sales does not vary for books with different levels of prerelease buzz (columns (1) and (2)), but the effect on digital sales varies for books with different levels of prerelease buzz (columns (3) and (4)). Specifically, the coefficient estimate on the interaction term in column (3) is 0.310 and statistically significant at the 5% level, while the coefficient in column (4) is 0.395 and statistically significant at the 1% level. Since the main effects of *Experiment* on digital sales in columns (3) and (4) are negative, a positive and significant interaction effect implies that the negative effect of the treatment is weaker for books with more prerelease buzz and stronger for books with less prerelease buzz. In other words, delaying Kindle releases reduces the digital sales of books with less prerelease buzz more than it does for other books.

Table 8. Interpreting Interaction Effects Between *Experiment* and *PrereleaseReviews*

	No. of prerelease reviews	Change in digital sales after print release (%)	Change in digital sales after Kindle release (%)
25th percentile	0	−60.3	−67.7
Median	2	−44.2	−50.2
75th percentile	5	−30.8	−34.5

To put these coefficient estimates into their proper economic perspective requires more effort than for interactions in linear models. For nonlinear models,

Table 9. Interactions Between *Experiment* and *Fiction*

	(1) BookScan print sales	(2) Amazon print sales	(3) Digital sales after print release	(4) Digital sales after Kindle release
Experiment	−0.335	−0.047	−0.690**	−0.768**
	(0.283)	(0.216)	(0.221)	(0.281)
Fiction	−0.102	−0.508*	1.007**	1.016**
	(0.343)	(0.225)	(0.257)	(0.321)
Experiment × *Fiction*	0.017	0.027	0.254	0.218
	(0.357)	(0.279)	(0.350)	(0.403)
WeeksSinceRelease	−0.124**	−0.136**	−0.069**	−0.126**
	(0.021)	(0.016)	(0.016)	(0.021)
PrereleaseReviews	0.350**	0.300**	0.396**	0.540**
	(0.066)	(0.063)	(0.084)	(0.081)
PrintPrice	−1.498**	−2.230**	0.858	−0.502
	(0.234)	(0.319)	(0.498)	(0.402)
DigitalPrice	−0.100	0.434	−4.478**	−2.208**
	(0.331)	(0.416)	(1.045)	(0.606)
Reviews	0.148**	0.167**	0.255**	0.198**
	(0.038)	(0.038)	(0.051)	(0.038)
Rating	−0.051	0.206	−0.068	0.169
	(0.128)	(0.139)	(0.103)	(0.122)
Weight	0.414	−0.068	−0.209	0.165
	(0.543)	(0.417)	(0.452)	(0.520)
Height	0.614	−0.278	−0.030	0.950
	(0.875)	(0.581)	(0.724)	(0.764)
Length	0.696**	0.741**	0.988**	1.063**
	(0.229)	(0.211)	(0.300)	(0.277)
Width	−0.947**	−0.881**	−0.933*	−1.077**
	(0.360)	(0.301)	(0.414)	(0.399)
Pages	0.001	0.004*	0.006**	0.002
	(0.002)	(0.002)	(0.002)	(0.002)
No. of observations	3,640	3,640	3,640	3,640

Notes. Robust standard errors are in parentheses. Time dummies are included in the estimation.
**Significantly different from zero at the $p < 0.01$ level; *significantly different from zero at the $p < 0.05$ level.

such as negative binomial regressions, there is no single economic interpretation of the interaction effect, as it varies for different combinations of the two interacting variables (Hilbe 2011, appendix A). In Table 8, we report the effects of the delay of digital releases on digital sales at different levels of prerelease buzz. We observe a decreasing trend in the magnitude of the treatment effect as the number of prerelease reviews increases from zero to five. However, the change in the treatment effect is nonlinear in the levels of *PrereleaseReviews*. Note that according to the main results from Table 6, the average effect of the delay of digital releases is −43.8% on digital sales after print release and −48.2% on digital sales after Kindle release.

In Table 9, we investigate how the genre of a book moderates the effect of the delay in digital releases. In both the control and experiment groups, roughly 40% of books are fiction and the remaining 60% are nonfiction (e.g., biographies and memoirs, business and investing, health, mind and body). The interaction terms in all four columns of Table 9 are statistically insignificant, although the main effects of *Experiment* and *Fiction* remain largely similar as in Table 6. These results suggest that the effects of the delay of digital releases do not vary between fiction and nonfiction titles.

4.3. Digital Sales in Other Channels

Although our results suggest that there is no substitution from Kindle sales to online or offline print sales when the Kindle release is delayed, it is still possible that consumers may purchase e-books from other digital channels. A potential confounding factor in our experiment is that Apple's iBookstore opened in early April 2010 and included content from this publisher. One could argue that at least part of the drop in Kindle sales for the experiment group could be attributed to the opening of the iBookstore if the decrease in sales came from consumers who substituted the (unavailable) Kindle channel with the iBookstore. We first note that the market share held by

iBooks in our study period (Carnoy 2010, Hoffelder 2015) is unlikely to explain the drop in e-book sales that we observe. In addition, iBookstore purchases can only be viewed on Apple Mac and iOS (e.g., iPhone, iPod Touch, iPad) devices, reducing the potential market segment that could make the trade-off between the Kindle and iBookstore. We also note that we have already controlled for this effect in part by adding a set of time dummy variables, as this event is likely to have a systematic shock on the demand of all books.

However, to directly evaluate the extent to which consumers switch from Kindle to iBooks in the event of Kindle release delay, we obtained this publisher's sales on the iBookstore for the control and experiment titles in our sample. With these data we run a regression similar as in Table 6 but replace the dependent variable with weekly iBook sales. This test would reveal whether there is a significant difference in the iBook sales between the control and experiment groups.

Table 10 reports the results of this regression. The number of observations is smaller than 3,640 because iBook sales started in April 2010, and all observations in March are dropped from the analysis. The coefficient estimate on *Experiment* is statistically insignificant, suggesting that consumers do not switch from Kindle to iBook when the Kindle release is delayed. We also note that the results for the control variables are largely consistent with those in Table 6.[10]

Table 10. iBook Sales

	(1) *iBook sales after print release*
Experiment	−0.183 (0.280)
WeeksSinceRelease	−0.056 (0.031)
PrintPrice	0.046 (0.304)
DigitalPrice	−0.814 (0.508)
Reviews	0.215** (0.067)
Rating	−0.278 (0.148)
PrereleaseReviews	0.604** (0.088)
Fiction	−0.047 (0.315)
Weight	0.566 (0.782)
Height	0.104 (1.234)
Length	0.539 (0.431)
Width	−0.622 (0.475)
Pages	−0.001 (0.003)
No. of observations	3,512

Notes. Robust standard errors are in parentheses. Time dummies are included in the estimation.

**Significantly different from zero at the $p < 0.01$ level; *significantly different from zero at the $p < 0.05$ level.

5. Discussion

Our research analyzes how the availability of a product in a digital channel impacts sales in physical and other digital channels—in our case, in the digital and physical channels for books. This question is important to both managerial and academic audiences. From a managerial standpoint, content owners across a variety of industries are making decisions about whether, when, and how to include digital products in their existing set of distribution channels, and where to place these products into their existing product release cycles. These decisions are particularly salient for book publishers, many of whom have experimented with releasing e-book versions in between the (high margin) hardcover release date and the (lower margin) paperback release date. From an academic perspective, our work adds to a growing literature analyzing the impact of new digital distribution channels on physical sales.

We analyze this question using a novel natural experiment where a publisher stopped releasing new e-book content to Amazon for a period of about two months. Because this publisher still released print copies of its titles to Amazon during this period, the net effect of this event is that books released during this time frame were delayed on the electronic channel relative to the print channel by between one and eight weeks.

Our results show that delaying the publication of the e-book relative to its print version causes a large and persistent decrease in digital sales. This negative impact on digital sales is more pronounced for books with less prerelease buzz. However, the effects of the digital release delay do not vary with the genre category (fiction versus nonfiction) of the book. Contrary to the common belief in the industry that delaying digital releases increases hardcover sales, we find no significant increase in print sales in both online and offline channels.

These results point to the possibility that, in general, consumers may be relatively more tied to their mode of consumption (physical or digital) than they are to a particular product. Said another way, when facing new digital channels, publishers and other media firms have frequently conceptualized the consumer's decision process as being driven by product choice first and then channel choice. This conceptualization is seen in the frequent strategy to delay digital availability as a way of retaining physical sales. Our results suggest that, in general, consumers choose their channel (digital versus physical) first and then restrict their choice set to the products available in that channel. To be clear,

our results do not suggest that digital channels will not cannibalize aggregate physical sales in the long term; they certainly will. Rather, we believe our results suggest that as a managerial question, given that digital channels exist and that some consumers have preferences for these channels versus physical channels, refusing to provide books and other products in digital channels is unlikely to result in increased short-term physical sales for a particular title.

Our results are not without limitations. First, and most notably, our results are based on the assumption that the books released immediately before and after our experiment period are good controls for the books released during the experiment. We believe this is true based on the empirical tests above and based on conversations with the publisher in question. However, we cannot conclusively rule out the possibility that unobserved differences between the control and experiment groups may be driving some of our results.

Second, our results are situated within a particular market (books) and at a particular stage of development of that market. E-book adoption has experienced rapid growth, from 9% of all book sales in 2010 (Association of American Publishers 2010) to 20% in 2015 (Association of American Publishers 2016); interaction between these two channels has increased in importance in the publishing industry. Our results suggest that as the e-book market grows, it is likely that delaying e-book releases will result in more substantial decreases in digital sales without significantly improving print book sales. Nonetheless, it would be useful for future research to investigate how the cannibalization between print books and e-books evolves at different stages of e-book adoption.

Finally, our natural experiment is slightly different from officially delaying the digital release because no specific future release time is set. However, in our context, because consumers face more uncertainty regarding whether the digital release would ever be available, they are more likely to switch to other formats or channels than they would be if they knew for certain when the digital content would be available. Because we still observe no substitution from Kindle to print or iBook sales, we believe our result of lost digital sales is likely to hold when the future release date is known to consumers.

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Endnotes

[1] We note that these lower e-books sales will also lead to lower total profits in our setting, given that publishers' margins on e-books are very similar to their margins on higher-priced print books. See, for example, Rich (2010), who notes that publisher profit (before overhead) on a $26.00 print book is approximately $4.05, while publisher profit on the same book sold in e-book format is between $3.51–$4.26 for a $9.99 e-book and $4.56–$5.54 for the same e-book sold at $12.99.

[2] Just as important for our discussion below, from discussions with the publisher in question and searches of press articles in both LexisNexis and ProQuest, there was no prior public indication that Kindle titles were going to be removed from Amazon for this publisher, and the first time there was any discussion of the return date for this publisher was on May 26, just five days prior to their restoration on Amazon. As such, prior to April 1, consumers had no expectation that Kindle titles would be removed, and prior to May 26, consumers had no information about when Kindle titles would be returned to the Amazon store.

[3] According to news articles in August 2010 (Carnoy 2010) and October 2015 (Hoffelder 2015), Amazon Kindle had a market share of 70%–80% in the e-book market. The other two main players in this market are the Apple iBook and Barnes & Noble Nook, which accounted for 10%–12% and 7%–8% of e-book sales, respectively. In Section 4.3, we analyze the iBook sales data acquired from the publisher and show that e-book sales in channels other than Amazon Kindle are unlikely to affect our results.

[4] There are many subgenres in either fiction or nonfiction categories. Because our sample size is relatively small compared with the number of subgenres, we do not add subgenre dummies but only compare fiction against nonfiction in our analyses.

[5] As a robustness check, we also include a squared time trend term, $WeeksSinceRelease^2_{it}$, in addition to the linear time trend term, $WeeksSinceRelease_{it}$; our results remain largely similar to those reported in Table 6 and are available upon request.

[6] There are many observations that have zero new reviews in a given week. To avoid missing values for the average rating variable, we control for the average rating of all Amazon reviews received by book i until week t, instead of the average rating of new Amazon reviews received by book i in week t.

[7] The magnitude of the coefficient estimate on *Experiment* in column (4) is larger than that in column (3) because in column (4), the digital sales after Kindle release are consistently lower for the experiment group than for the control group, but in column (3), the digital sales of the experiment group can be larger in some weeks than that of the control group (e.g., for a book whose digital release is delayed by eight weeks, its digital sales after *print* release in week 9 are likely to be larger than the digital sales of a control book in the corresponding week), although the digital sales of the initial few weeks are zero for the experiment group.

[8] In untabulated results, we examine two other exogenous moderators that are related to the popularity of a book's author and thus may also be good measures of anticipated demand. Without access to data on each author's prior sales, we utilize the number of prior books as a proxy of productivity and the number of customer reviews as a proxy of book sales. We first collect the list of prior books published by each author in our sample and then collect all the customer reviews received by those prior books on Amazon before our study period. We construct two variables, $AuthorPriorBooks_i$ (the number of prior books by the author of book i) and $AuthorPriorBookReviews_i$ (the number of Amazon reviews on all prior books by the author of book i), and we conduct similar analyses outlined in this section to test the interactions between the *Experiment* variable and these two variables. We do not find any significant interactions from these analyses. These findings suggest that the reviews on the quality of a book itself (i.e., prerelease buzz) seem to play a more important role

than the profile of the book's author in the event of a digital release delay.

[9] By contrast, the average number of Amazon reviews received by the books in our sample reaches 28.5 by the end of week 20.

[10] We further note that our analyses in Table 10 do not directly address the question of how different user characteristics or preferences explain a user's choice of selecting between the Amazon Kindle platform and the Apple iBook platform.

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