The Mechanics of Price Adjustment: New Evidence on the (Un)Importance of Menu Costs

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This paper examines nominal price rigidities in an environment, e-commerce, where literal menu costs can be assumed not to exist. We argue that if we can empirically show that nominal rigidities do still exist in the e-commerce environment, then it implies that other kinds of costs besides menu costs, such as management costs, must be causing these nominal rigidities. This evidence is of importance because of the central role that menu costs play in Keynesian macroeconomics. In this paper we examine the price changing behavior of two leading online booksellers—Amazon.com and BarnesandNoble.com—and find strong evidence that nominal price rigidities do indeed persist on the Internet. Copyright © 2007 John Wiley & Sons, Ltd.

INTRODUCTION

Menu costs play a central role in Keynesian macroeconomics. Keynesian theorists have argued that they provide an important nominal price rigidity that can cause business cycle dynamics (see Gordon, 1990 for a review). Taken literally, ‘menu costs’ refer to the cost of printing a new menu every time a price is changed. In an attempt to directly measure these costs Levy \textit{et al.} (1997) and Dutta \textit{et al.} (1999) consider four constituents of menu costs: (1) the labor cost of changing shelf prices, (2) the costs of printing and delivering new price tags, (3) the costs of mistakes made during the price change process, and (4) the cost of in-store supervision of the price change process’ (Levy \textit{et al.}, 1997, p. 798). Levy \textit{et al.} (1997) and Dutta \textit{et al.} (1999) argue that these types of costs are likely to be particularly important in settings with ‘multi-product posted prices’ such as large retail stores with many products. An important critique of the Keynesian menu cost theories is whether these, which would appear to be relatively trivial, costs can indeed be responsible for business cycles and the non-neutrality of monetary policy.

At the theoretical level, one Keynesian response to this critique (Akerlof and Yellen, 1985; Mankiw, 1985) has been that even small nominal rigidities can lead to large business cycle effects. A second response from authors like Gordon (1990), Ball and Mankiw (1994), and Mankiw and Reis (2002) has been that ‘menu costs’ should be interpreted more broadly than only the labor intensive costs listed by Levy \textit{et al.} (1997) and Dutta \textit{et al.} (1999) above. Gordon (1990, p. 1145) for example argues that nominal rigidities to changing prices within the firm can arise from ‘the entire range of costs that managers must incur
whenever nominal prices are changed’. These types of non-menu costs can include ‘management costs’ (e.g. the cost of managers’ time and information processing costs, etc.)

Evidence showing that price rigidities are caused not only by ‘menu costs’ but also by other types of costs such as ‘management costs’ could be important for at least two reasons. Firstly, the existence of other costs, in addition to menu costs, would bolster the New Keynesian case by providing additional causes of nominal rigidities. Secondly, it is also possible that the nominal rigidities caused by non-menu costs such as management costs may be qualitatively different from those caused by menu costs. This could then have implications for the way in which nominal rigidities are modeled in macroeconomic models.

The literature until now, however, has had some difficulty in distinguishing between menu costs and non-menu costs (e.g. management costs) as causing nominal rigidities. This is because in most firms all of these costs are usually associated with administering any particular price change. The Internet, however, provides an environment where ‘menu costs’ as defined by Levy et al. (1997) are almost wholly absent. For instance, the largest element of menu costs, according to Levy et al. (1997), consists of ‘labor cost of price changes’ followed by ‘labor cost of sign changes’. These are precisely the kinds of costs that are altogether absent in the online environment. A business to consumer (B2C) e-commerce firm does not require labor to change the price on its shelves, nor does it need to print new price tags for its products. Once the price changes are decided upon, all it takes to change prices are a few strokes in a computer keyboard. Furthermore, if a multi-product e-commerce retailer needs to change only the price of one product, it does not have to print and distribute a whole new menu. It can easily change the price of that product while leaving the others unchanged.

Therefore if the menu costs identified by Levy et al. (1997) and Dutta et al. (1999) do, in fact, effectively capture the main costs that cause nominal rigidities, then nominal rigidities should be negligible on the Web. In other words, if we can empirically show that nominal rigidities do exist on the Web, then it implies that other kinds of costs besides menu costs, such as management costs, must be causing these rigidities. In this paper we examine the price changing behavior of two leading online booksellers—Amazon.com—and BarnesandNoble.com—and find strong evidence that nominal price rigidities do persist on the Internet. While we have briefly examined similar data in Chakrabarti and Scholnick (2005), the current paper provides a far more detailed analysis, with several new empirical tests.

Our data consist of weekly prices at Amazon and Barnes and Noble for a random sample of over 3000 books for over a year.¹ We find that, much like the Israeli wine and meat stores in Lach and Tsiddon (1996) and the Canadian newspaper chains in Fisher and Konieczny (2000), both online retailers exhibit marked within-store synchronization in their price change behavior, which indicates nominal price rigidities.

We interpret the evidence in the following manner. We believe that this new evidence suggests that we should look beyond the usual suspects of narrowly defined menu costs in our search for explanations of price rigidities. The price rigidities we detect could be caused by any costs other than those menu costs that are absent in the e-commerce environment. Furthermore, if these non-menu costs (e.g. management costs) are shown to cause price rigidities in e-commerce retailers, then it is possible that they will also exist in other large multi-product stores—stores that make up a significant sector of the economy. If it is indeed true that price adjustment costs such as management costs are significant across large sectors of the economy, then this will have important implications for our understanding of the mechanics of price adjustment.

Our finding that price rigidities persist in an environment free of menu costs could have important implications for the New Keynesian research agenda. Mankiw and Reis (2002), for example, have recently proposed a model based on ‘sticky information’ rather than the standard New Keynesian model based on ‘sticky prices’, which they argue provides a macromodel that better replicates actual data. The key assumption of their sticky information model is that price rigidities arise because ‘firms gather information and recompute optimal prices slowly over time’ (p. 7)—a very different assumption from a simple menu cost view of nominal rigidities, where firms adjust prices as soon as the benefits of adjusting prices outweigh the menu costs. Empirical results such as ours can aid macroeconomists understand better the causes of nominal rigidities.

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The paper is organized as follows. The next section discusses the relevant literature and places the present paper in context. The third section describes our data and the data collection mechanism. The fourth section describes the statistical tests carried out to detect the presence of synchronization in online price setting. The fifth section concludes.

LITERATURE SURVEY

Nominal Rigidities and Keynesian Macroeconomics

The question of nominal rigidities is central to Keynesian and New Keynesian macroeconomics. It can be argued that it is the belief in sticky prices that distinguishes New Keynesian from New Classical macroeconomists. For instance, in their review of the New Keynesian Monetary Policy literature, Clarida et al. (1999, p. 1662) state that this approach 'is based on the idea that temporary nominal price rigidities provide the key friction that gives rise to non-neutral effects of monetary policy'.

Given the central role of nominal rigidities in the Keynesian and New Keynesian doctrines, the search for the reasons of such rigidities has been justifiably extensive. ‘Menu costs’ have formed the standard argument, much to the discomfort of many who think they are too small and inconsequential to deserve credit for economy-wide business cycles. A theoretical Keynesian response from Akerlof and Yellen (1985) and Mankiw (1985) has been that individual firms face small (if any) change in profits by delaying adjusting prices to take into account menu costs, but collectively this delay can result in large business cycle effects.

More recent research has focused directly on the specific mechanics of price changes within firms, and has followed two distinct paths. The first has examined, at a theoretical level, the impact of widening the definition of menu costs, to examine how a larger variety of ‘management costs’ involved in changing prices could affect price-changing dynamics. The second has been an empirical investigation into the actual magnitude of menu costs within firms.

At the theoretical level, several authors emphasize that the costs of a price change extend beyond those associated with actually updating labels, menus, catalogs, etc. (see Gordon, 1990). Sheshinski and Weiss (1992) discuss ‘decision costs’ as distinct from ‘menu costs’ as an important element of the costs of changing prices. Ball and Mankiw (1994) actually go to the extent of suggesting that menu costs should be seen as a ‘metaphor’ or ‘parable’ for other kinds of costs within a firm that result in similar nominal price frictions. In fact, they ‘... suspect that the most important costs of price adjustment are the time and attention required of managers to gather the relevant information and make and implement decisions’. Mankiw and Reis (2002) have recently pointed out that changing the type of costs of price adjustment from ‘sticky prices’ to ‘sticky information’ can change not only the magnitude but also the dynamics of monetary policy shocks.

The empirical stream of recent menu cost research has focused on examining the actual process of price adjustment within firms. Levy et al. (1997) and Dutta et al. (1999) take up the challenge of actually measuring ‘menu costs’ using data from five multi-store supermarket chains and at a large US drugstore chain, respectively. They find that price changing at a major store is a complex and multi-step process requiring considerable resources with costs averaging to over 50 cents per price change. In their survey of corporate price setters, using interview data, Blinder et al. (1998) find that the ‘price adjustment costs’ include management costs like ‘decision making time of executives’ over and above menu costs like ‘printing new catalogs, new price lists, new packaging, etc.’ (see also Blinder, 1991). Using an ‘ethnographic’ interview based methodology, Zbaracki et al. (2004) examine price changing decisions in an industrial manufacturer and conclude that menu costs form only a small proportion of the total costs of changing prices compared to other types of costs such as ‘managerial and customer costs’.

Thus, while there has been theoretical discussion about the possible implications of non-menu costs such as management costs on price adjustment dynamics (e.g. Ball and Mankiw, 1994; Mankiw and Reis, 2002), as well as ethnographic evidence (e.g. Blinder et al., 1998; Zbaracki et al., 2004) that such costs do exist in firms, there has been, until now, no econometric study using a large sample of actual price changes to test whether observed price rigidities can be ascribed to non-menu costs. This is because in most firms menu costs and other costs such as management costs invariably occur.
simultaneously, thus separating their effects on any observed price rigidities is difficult. The Internet, however, provides an environment that is largely free of menu costs while management costs are still possibly present.

**Nominal Rigidity in Multi-product Firms—Staggering vs. Synchronization**

While much of the initial work on nominal price rigidities focused on the single product firm, more recent research has examined price rigidities within multi-product firms (such as the firms examined here). Sheshinski and Weiss (1992) have developed theoretical work on this issue, while Lach and Tsiddon (1996) and Fisher and Konieczny (2000) have provided empirical evidence. The key distinction emphasized by these authors is whether multi-product firms stagger or synchronize their price changes for all of their different products.

In their theoretical paper, Sheshinski and Weiss (1992) emphasize the effects of economies of scale in price adjustment in a multi-product firm setting. According to them, economies of scale in price adjustment would cause the price change process to exhibit synchronization, whereby the firm would delay changing prices for individual products, but rather would execute price changes for many of its products simultaneously. Economies of scale in adjusting prices could be caused by any factor causing nominal rigidities, including menu costs and/or management costs. Without such economies of scale in price adjustment (if, for example, there were no menu costs or management costs), the price change process would be staggered over time. The timing of price changes of individual products would be unrelated to each other, but would rather be determined by other product-specific factors, such as changes in the relevant supply and demand conditions for each product.

More recent empirical papers examining nominal rigidities in multi-product firms (e.g. Lach and Tsiddon, 1996; Fisher and Konieczny, 2000) have been based on the Sheshinski and Weiss (1992) argument that price synchronization within firms is an indication of nominal rigidities. Lach and Tsiddon (1996) examine actual price data of several meat and wine products sold by different food stores in Israel, and use a battery of tests to distinguish whether the price changes within stores (and between stores) are either synchronized or staggered over time. They find evidence of within-store synchronization in the timing of price changes, which they interpret as supporting the menu cost view of nominal rigidities. Fisher and Konieczny (2000) also find evidence of synchronization of price changes over time among Canadian newspapers belonging to different newspaper groups. They also hold menu costs responsible for the price rigidity that they detect.

In the discussion of the multi-product firm, evidence of the synchronization of price changes within a store has been taken to imply the existence of nominal rigidities which would thus support the New Keynesian position. However, it is theoretically possible that if the price changes of all the products in all the stores were synchronized (i.e., if the prices of all books in all bookstores change at the same time) and if this response occurred instantaneously following an exogenous monetary policy shock, then the aggregate price level would respond fully and without a lag to the monetary policy shock. This would thus refute the central New Keynesian price rigidity argument, and would support the view that Monetary Policy was neutral. It is for this reason that Lach and Tsiddon (1996) argue that their empirical evidence of both within-store synchronization and across-store staggering of price changes is consistent with the nominal rigidity literature.

The current paper will ask the same question as Lach and Tsiddon (1996)—are price changes synchronized or staggered within a multi-product store—and will use much of the econometric methodology developed in that paper. However, it will pose the question in an environment where price rigidity cannot be caused by ‘menu costs’.

**DATA AND DATA COLLECTION**

The two stores examined here, Amazon.com and Barnes and Noble.com, largely dominate the online book retailing industry in the US. Based on online sales for all e-retailers in the US in 2000, the Amazon group is ranked second and Barnes and Noble is ranked ninth. The next highest ranked e-bookstore, Borders.com, is ranked 87th (Russell Reynolds Associates, 2001). The data set we use here comprises weekly observations on the prices of 3124 books from Amazon and Barnes and Noble for a period of 57 weeks. The data
collected here come entirely from the websites of the two companies. The prices that we capture are the prices available to any general consumer. The data collection technique is outlined below.

The product specifications for a book (i.e. the physical characteristics of a book) are completely captured by what is known as the International Standard Book Number or the ISBN of a book. The book market, therefore, provides us with a setting where the products being sold in the two stores in question are exactly identical—a feature that is difficult to obtain for most other product categories. In order to construct our data set of books, we begin by building a sample of ISBNs obtained from an online book-selling site ‘even better.com’—a source independent of the two bookstores under study. From the list of books available at this site, we generate a large sample of ISBNs of books belonging to different subject categories using proportionate random sampling. Having created this list of ISBNs we develop and employ a ‘bot’—a Web based program—to automatically extract the necessary information for each book in our sample from the two websites, once every week (every Monday night). Allowing the bot to run its weekly data extraction process for over a year we create our database for price observations for the two stores. Our data, therefore, are weekly in frequency and cover the period from March 20, 2000 to April 22, 2001.

From the initial random sample of ISBNs we select a sample of 3124 books for analysis. These are the books in our sample for which price data are recorded for both stores for each of the 57 weeks. The books in our sample are important enough that both stores make them available for sale on their websites each week throughout our sample period. We thus study prices for identical products—3124 books that are sold in both stores. The prices that we record for each book are the prices offered to any visitor to the websites. The recorded price does not include any discounts offered to consumers from belonging to a frequent buyer program.

In our investigation of the nature of price changes at these two online retailers, we follow the standard approach in the area and focus on the number of books changing prices in a week at a particular store, rather than the actual price data. This is because, as in the extant literature (see Lach and Tsiddon, 1996; Fisher and Konieczny, 2000), we focus particularly on the timing of price changes, rather than their magnitude. We, therefore, create a panel of zeros and ones for each store—for 56 weeks (dropping the first week as we compute changes) and 3124 books—that serve as our data for the subsequent analysis. In this database, one signifies that a particular book changed its price during the past week at a particular store while zero indicates no change in the price of the book since the previous price observation a week earlier. Our database can therefore be formally described as the set:

\[ D = \{X_{ijt} \in \{0, 1\} : i \in \{\text{Amazon, Barnes&Noble}\}, j \in \{1, 2, \ldots, 3124\}, t \in \{1, 2, \ldots, 56\}\} \]

Table 1 presents the descriptive statistics for the data.

### DATA ANALYSIS

In this section we present evidence from a battery of tests that suggest the presence of a high degree of synchronization in price changes, and thus nominal rigidities, at the two online bookstores under investigation. In order to study the nature of

<table>
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<tr>
<th>Number of books</th>
<th>Amazon</th>
<th>Barnes and Noble</th>
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<tr>
<td>Number of weeks</td>
<td>56</td>
<td>56</td>
</tr>
<tr>
<td>Total number of changes</td>
<td>7759</td>
<td>7160</td>
</tr>
<tr>
<td>Average proportion of books changing prices per week</td>
<td>0.04</td>
<td>0.04</td>
</tr>
<tr>
<td>Average number of changes per book</td>
<td>2.48</td>
<td>2.29</td>
</tr>
<tr>
<td>Standard deviation of number of changes</td>
<td>0.21</td>
<td>0.20</td>
</tr>
<tr>
<td>Maximum number of changes (in a particular book price in 56 weeks)</td>
<td>16</td>
<td>17</td>
</tr>
<tr>
<td>Minimum number of changes</td>
<td>0</td>
<td>0</td>
</tr>
</tbody>
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price changes in the two online stores, we largely follow the methodology used in Lach and Tsiddon (1996) and Fisher and Konieczny (2000). The results of the various tests used here not only corroborate one another but also indicate the robustness of our findings.

**A Graphical Description of the Data**

We begin by computing the proportion of books that changed prices in a particular week at each of the two stores. Figure 1 shows the proportion of books changing prices in each of the 56 weeks under study. Figure 1 immediately captures the synchronicity of price changes in both the stores. In 43 of the 56 weeks Amazon changes less than 3% of its book prices while in the three weeks with maximum price changes it changes the prices of well over a quarter of its books—in fact the maximum change exceeds 37%. While Barnes and Noble changes book prices less frequently than Amazon, the non-uniformity in the proportion of book prices changed is no less spectacular in its case. In 49 of the 56 weeks the price changes occur for less than 5% of its books while in the top two weeks it changes well over 20% of its book prices—the maximum being close to 30%.

The statistical analysis that follows is largely based on the graphical evidence provided in Figure 1. There are three main elements in the analysis. The following three sub-sections provide different types of tests to examine price staggering or synchronization within each of the individual stores. The following fourth sub-section examines synchronization and staggering between the two stores to test for the possible impact of a common exogenous shock, particularly during the episodes of large number of changes in the two stores. Finally, the following fifth sub-section runs a robustness check on our results by omitting the episodes of large changes altogether and examining if the results would continue to hold even in these deliberately unfavorable settings.

**Volatility of the ‘Proportion of Price Change’ Series for Each Store**

In order to evaluate the extent of synchronization or staggering present in the two series displayed in Figure 1 it is useful to begin with a descriptive approach suggested by Fisher and Konieczny (2000). These authors suggest comparing the actual volatility of these two series with two extreme cases, the cases of perfect staggering and perfect synchronization. The case of perfect staggering would imply that the same proportion of books (the average proportion of 4% for both stores) change price every week. In other words the
perfect staggering case would be displayed in Figure 1 as a straight line at the 4% level for both stores, which implies a standard deviation of zero. In such a world there would be no economies of scale in adjusting prices.

The opposite case of perfect synchronization implies that all price changes occur together in some weeks with no price movements in other weeks. This implies a world of extremely high economies of scale in adjusting prices. Following Fisher and Konieczny (2000) we argue that since on an average there are 2.48 price changes per book at Amazon and 2.29 price changes at Barnes and Noble, a case of perfect synchronization would imply a series of three ones and 53 zeros in either case. The standard deviation of this series would then be the standard deviation consistent with perfect synchronization. Clearly the observed standard deviation would lie between these two extremes. Its relative distance from the perfect staggering case (relative to its distance from the perfect synchronization case) would then provide an indication of the degree of synchronization in price changes. These results are shown in Table 2.

As Table 2 indicates, the distance of Amazon’s price changes from the perfect staggering case is over 33% while that for Barnes and Noble 20%. These figures should be viewed in light of other studies. Fisher and Konieczny (2000) in their study of price changes in Canadian newspapers find a maximum deviation of 20.9% from the perfect staggering case. Thus it appears in the case of online book retailing with virtually no menu costs, the degree of price synchronization is at least as high if not significantly higher than that found by Fisher and Konieczny (2000).

The comparisons of the actual data to the perfect staggering and perfect synchronization cases are, however, essentially descriptive, and cannot provide formal statistical evidence of lack of perfect staggering. Such evidence, however, is provided by \( \chi^2 \) tests of the variance of the time series of proportion of books changing prices. If these standard deviations are significantly different from zero, then it shows that they are, indeed, significantly different from the case of perfect staggering of prices which would imply zero standard deviations. The \( \chi^2 \) tests for both Amazon and Barnes and Noble indicate that the variances are significantly positive at 1% level providing our first statistical evidence of the lack of perfect staggering (and, hence, the presence of synchronization) in the price-setting behavior of these two online booksellers. 7

### Tests of Synchronization

While the above tests examined the entire series displayed in Figure 1 for each store, given our data it is also possible to test hypotheses for each particular week. We can use the binomial distribution to test for the presence of within-store synchronization in price changes. If the price changes are indeed staggered evenly over time, then during any particular week, the proportion of books changing prices would follow the binomial distribution with the number of trials equal to the number of books and the probability of success for a store equal to the ratio of total price changes during the entire sample period at the store to the total number of observations from that store. In the cases of both Amazon and Barnes and Noble this is 4%.

Thus the null hypothesis, once again, is that of no synchronization, and the \( p \)-value of the observed proportion of price changes would indicate the probability of the event occurring under the null hypothesis. This is a two-way test. Therefore a very low or a very high \( p \)-value would both signify lower probability of the null hypothesis being correct. Table 3 shows the distribution of binomial \( p \)-values for each store. In the case of Barnes and Noble, over three-fourths of the observations (43 out of 56) lie in the two 5% level of confidence critical regions. For Amazon the results are even more unequivocal—all values fall

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<tr>
<th>Table 2. A Comparison of Standard Deviations</th>
<th>Amazon</th>
<th>Barnes and Noble</th>
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<tr>
<td>Standard deviation in actual data</td>
<td>0.076</td>
<td>0.045</td>
</tr>
<tr>
<td>Standard deviation assuming perfect staggering</td>
<td>0.000</td>
<td>0.000</td>
</tr>
<tr>
<td>Standard deviation assuming perfect synchronization</td>
<td>0.225</td>
<td>0.225</td>
</tr>
<tr>
<td>% Difference (actual data from perfect staggering)</td>
<td>33.7</td>
<td>20.0</td>
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in the 5% tails, in fact all of them fall in the 1% tails. Once again this seems to indicate considerable level of synchronization in price changes over weeks in the two online stores.

Do Books Change Prices at the Same Time? Tests of Pair-Wise Correlations

The final test of within-store staggering that we apply to our data is that of pair-wise correlation in the timing of price changes as outlined in Lach and Tsiddon (1996). The object of interest here is the extent to which different pairs of books exhibit co-movement in their price change pattern. The null hypothesis is that for any pair of books, the decision to change prices for one book over the 56 weeks under study is independent from that for the other book. A sufficiently high level of correlation for several pairs would therefore lead to a rejection of the null hypothesis. However, since the distribution of the relevant statistic in this case is a non-standard one, we employ Monte-Carlo simulations to generate the distribution and compare our observed values against this distribution.

Given the large number of books in our sample and the fact that the majority of the books do not change prices at all during the entire sample period, we start off by drawing a random sub-sample of 100 books separately for Amazon and for Barnes and Noble from amongst those books in our larger sample that experience at least one price change during the sample period. For these 100 chosen books we then compute the indicator function \( S_i(j,k) \) for store \( i \in \{ \text{Amazon, Barnes & Noble} \} \), week \( t \in \{1,2,\ldots,56\} \) and for books \( j,k \in \{1,2,\ldots,100,j \neq k\} \) for the 4950 distinct pairs of books. The indicator function \( S_i(j,k) \) is unity if \( X_{j|t} = X_{k|t} \) and zero otherwise. The mean of \( S_i(j,k) \) over time, \( S_i(j,k) \), is then the proportion of synchronization between the two book prices at store \( i \). The average of these indicator functions \( S_i = \Sigma_j \Sigma_{k \neq j} S_i(j,k) \) over all the 4950 pairs turn out to be 0.85 for Amazon and 0.88 for Barnes and Noble. While these numbers are apparently quite high, it is not possible to formally reject the null hypothesis of ‘no synchronization’ without looking at the distribution of this statistic, \( S_i \). Consequently, we generate a Monte-Carlo distribution of this statistic under the null hypothesis of the ‘no synchronization’ in the following manner.

In order to generate the Monte-Carlo distribution of the statistic \( S_i \), we start off by estimating the Markov-chain transformation matrix of price changes in the two stores shown in the two panels of Table 4. These transformation matrices indicate the probabilities for a book to take a ‘price change’ state or a ‘no change’ state in a week given its state in the previous week. Since our sub-sample comes from books that record at least one change during the entire period, the estimated transformation matrix for these books is different from that of the entire sample. Consequently, we estimate the transformation matrix from the sub-sample consisting of books that experience at least one change during the sample period. Using this transformation matrix and the proportion of books that change price in the first week (amongst those that change price at least once during the entire period) we then create an array of zeros and ones for 100 books and 56 weeks using draws from random number generators. This process obviously assumes independence and not synchronization between the different books in the timing of their price changes. Next we compute the value of \( S_i \) from this array just as we do from our sub-sample. We repeat the entire process 5000 times to generate

<table>
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<th>Table 3. Distribution of Binomial p-Values</th>
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<tr>
<td>Number of observations</td>
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<tr>
<td>Amazon</td>
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<td>Barnes and Noble</td>
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<th>Table 4. Markov-chain Transition Matrices</th>
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<td>-------------------------------------------</td>
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<tr>
<td>Panel A: Amazon</td>
</tr>
<tr>
<td>No change in previous week</td>
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<tr>
<td>Price change in previous week</td>
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<tr>
<td>Panel B: Barnes and Noble</td>
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<tr>
<td>No change in previous week</td>
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<tr>
<td>Price change in previous week</td>
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distributions of $S_i$'s for one store under the null hypothesis. The distribution for the other store is similarly obtained too. Figure 2 presents the histograms and the descriptive statistics of the resulting distributions. The 95% critical value of the distribution for Amazon is 0.434 and the 99% critical value is 0.443 while those for Barnes and Noble are 0.445 and 0.453, respectively. Clearly then, this test, like all others, firmly rejects the hypothesis of ‘no synchronization’ for both stores.

Taken together the battery of tests described in the preceding three sub-sections establishes, we believe, beyond ‘reasonable doubt’, that there is a significant element of within-store synchronization in the price-setting behavior at the two online stores.

**Focusing on the ‘Spikes’—Reaction to Common Shocks or Competitive Pressures**

While the above empirical sections have examined within-store price staggering, it is also important to examine staggering between stores. As noted by Lach and Tsiddon (1996) and others, an important requirement for the non-neutrality of money is that all price setters do not adjust all their prices simultaneously in response to a common shock. Thus if our evidence were to show that the prices of all books in our sample in all (both) stores changed price simultaneously, this could be interpreted as a reaction to a large common external shock. An examination of Figure 1 indicates that there are three episodes where either one or both stores change a large proportion of their prices, weeks 4 and 6, weeks 19 and 20, and weeks 41 and 43 (counting the first week of change data, i.e. second week of the price data, as week 1).

Over 33% of the price changes on week 6 at Amazon were for books that changed prices two weeks before at Barnes and Noble. This amounts to 282 books or about 9% of the entire sample. About 66% of the books for which Amazon changed prices on week 20 were the ones with price changes at Barnes and Noble a week earlier. This amounts to 785 books or about 25% of the entire sample. In these two episodes Barnes and Noble changed its prices and was followed rapidly by Amazon. The episode in weeks 41 and 43 is different from the other two episodes in that Amazon raised its prices without a response from Barnes and Noble, and then changed its prices again. In week 43, over 95% of the Amazon price changes were books whose prices were changed two weeks earlier—in most cases the price was brought back exactly to the pre-change levels.

Given the limitations of our data, it is not possible to identify specifically what caused these spikes. However, the data indicate that some element of a competitive relationship between the two stores is possibly driving the behavior in Figure 1, with Amazon responding to price changes by Barnes and Noble (e.g. weeks 6 and 20). As for weeks 41 and 43 at Amazon this could very well indicate a short-term price change to take advantage of the Christmas season, with no apparent reaction from Barnes and Noble. In any case these data do seem to be inconsistent with the view that these changes are a reaction to a ‘common shock’ with all prices in all stores changing simultaneously. Thus there is little support for the monetary neutrality view in our data.

Another way of examining whether there is between-store synchronization is to examine the
correlation between the two series in Figure 1. The correlation between the proportions of books changing prices in the two stores is a low 0.05. Thus there is no evidence of ‘simultaneous moves’ by the two stores. The time series of the average proportion of prices changed in both the two stores in the 56 weeks is smoother than the proportion in either store. The average has a coefficient of variation of 1.06, lower than both the individual proportions (1.71 for Amazon and 1.10 for Barnes and Noble). Thus it is hard to believe that the observed within-store synchronization is the result of an external common shock that would in turn result in the aggregate (book) price level of these stores responding instantly and fully to that shock.

Robustness Check—are the ‘Spikes’ Driving Our Results?

This sub-section asks the question of whether our strong within-store price change synchronization results in the second, third, and fourth sub-sections of this section are driven by just those few weeks of data where there are very high spikes, or whether they are valid throughout the data. It is conceivable that these results may be caused by the few ‘spikes’—or weeks with abnormally high numbers of price changes at either stores—identified in the previous sub-section. In order to check if this is indeed the case, we replicate our different tests of within-store synchronization, but in this case we drop those weeks with the ‘spikes’ from the data set. Thus we drop the observations for weeks 6, 20, 41, and 43 for Amazon and weeks 4 and 19 for Barnes and Noble and re-do our analysis.

After dropping the weeks with the ‘spikes’, the standard deviation for the changes (Table 2, Row 1) drops to 0.031 for Amazon and to 0.016 for Barnes and Noble. Even after this reduction, however, the percentage difference from staggering (Table 2, Row 4) remains at a substantial 16.3% for Amazon and 8.3% for Barnes and Noble. These results still compare well with those of Fisher and Konieczny (2000) who detect synchronization with figures ranging from 4.4 to 20.4%. The $\chi^2$ tests continue to reject the ‘perfect staggering’ null hypothesis of no variation.

As for the binomial tests, the hypothesis of staggering is rejected in 41 weeks out 52 (over 75%) for Amazon and 35 weeks out of 54 (about 65%) for Barnes and Noble. These results also compare well with the relevant figures in Lach and Tsiddon (1996) that range from around 65 to 95%.

As for the pair-wise correlations, the observed values in the reduced sample are, as expected, a little lower than in the total sample (0.78 for Amazon and 0.83 for Barnes and Noble), but are still comfortably rejected at the 1% level by the Monte-Carlo simulations using the new transition matrices. This confirms that the evidence of synchronicity found in the test is not driven by a few episodes of large changes but is inherent throughout the data.

Finally, with the elimination of the weeks with the ‘spikes’, the correlation between the proportion of books changing prices at the two stores drops further to –0.06 providing no support to a ‘common shocks’ view. Thus even in the most unfavorable scenario for our results, when we deliberately omit the weeks with the most changes, our conclusions remain unaffected.

CONCLUSIONS

We examine the price change behavior in two leading online booksellers—Amazon.com and BarnesandNoble.com—and find evidence of price change synchronization. Using largely the methodology of Lach and Tsiddon (1996) we find that the degree of synchronization is no less marked than that in Lach and Tsiddon (1996) or Fisher and Konieczny (2000). Price synchronization and nominal rigidity therefore clearly persist in the e-commerce environment, where menu costs associated with changing prices are almost non-existent. Consequently, menu costs cannot be the generator of the price rigidity observed here.

The existing literature has theorized that in New Keynesian models, ‘menu costs’ should not be construed literally, but rather should include the full set of costs to management involved in changing prices (Ball and Mankiw, 1994; Mankiw and Reis, 2002). The literature has also provided interview type evidence that management costs of changing prices are of importance to firms (Blinder et al., 1998; Zbaracki et al., 2004). Using data from e-commerce, however, allows us for the first time to show a direct empirical link between non-menu...
cost (e.g. management costs) and observed price rigidities in multi-product firms.

We are not able to specify exactly the kinds of costs that are causing the observed price rigidities at Amazon.com and Barnes and Noble.com. Our evidence only allows us to specifically exclude elements of menu costs (like, for instance, those listed by Levy et al., 1997; Dutta et al., 1999), which are largely absent on the Internet, from the list of suspects. However, given the fact that the firms in our sample are similar to large multi-product retailers in the diversity of their products, we suspect that costs associated with information processing and deciding upon optimal prices are most important here. Decision-making time and expertise are, therefore, the costly inputs. The price change synchronization observed here may also be indicative of the fact that in a multi-product firm, price change decisions are made not at the individual product level but in some product classes.

The findings in this paper also have implications for other elements of the literature. Kashyap (1995), for example, has raised the question of whether nominal price rigidities caused by ‘management costs’ could be reduced by increased computerization. The environment from which we draw our data, the e-commerce business, is very highly computerized. Computerization itself, therefore, does not seem to be enough to eliminate price rigidities.

This paper contributes to the ongoing research agenda to specifically link different causes of nominal rigidities (e.g. menu and/or management costs) with specific price adjustment dynamics and, in turn, to link these to dynamic macroeconomic models. To the extent that alternative price adjustment dynamics lead to important differences in our understanding of the macroeconomy, further research should investigate the different causes of nominal price rigidities (e.g. menu or management costs), and the specific price dynamics they cause. Our result, that price rigidities persist in an environment with little or no menu costs, is a step in that direction.

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NOTES

1. Kauffman and Wood (2007, this issue) also examine price data collected from a large number of books and CDs sold on the Internet. They conclude that tacit collusion exists among different Internet retailers.

2. Zbaracki et al. (2004) have also demonstrated the link between managerial costs and price rigidity.

3. Lach and Tsiddon (2007, this issue) find that the average price change for products within a multi-product store is not ‘small’, a finding that they argue is consistent with menu cost models in multi-product firms.

4. In a more recent paper on Canadian newspaper pricing, Fisher and Konieczny (2006) stress other aspects of price determination like the frequency of revisions in optimal price policy.

5. Axarloglou (2007, this issue) using data from traditional book and music stores examines both within-store price change synchronization and between-store price change synchronization. He finds that both significantly impact the probability of a specific product changing price.

6. The original address of this site was www.evenbetter.com. However, during our study it was acquired by a more general comparison shopping site, Deal-Time.com (www.dealtime.com).

7. In a related test, Lach and Tsiddon (1996) suggest comparing each of the series in Figure 1 with an average across all the stores in the sample. We conducted such a test, and using the methodology outlined in Lach and Tsiddon (1996) we can again reject the hypothesis of no synchronization for each store. We do not report these results, however, because we have only two stores in our sample from which to derive an average, unlike Lach and Tsiddon who have a large number of stores in their database.

REFERENCES


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