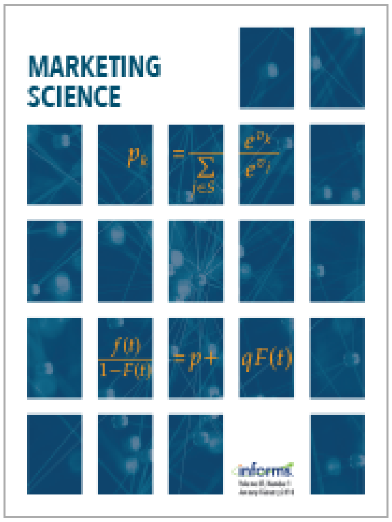

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Online MAP Enforcement: Evidence from a Quasi-Experiment

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Abstract. This paper investigates a manufacturer's ability to influence compliance rates among its authorized online retailers by exploiting changes in the minimum advertised price (MAP) policy and in dealer agreements. MAP is a pricing policy widely used by manufacturers to influence prices set by their downstream partners. A MAP policy imposes a lower bound on advertised prices, subjecting violating retailers to punishments such as termination of distribution agreements. Despite this threat, violations are common. I uncover two key elements to improve compliance: customization to the online environment and credible monitoring and punishments. I analyze the pricing, enforcement, and channel management policies of a manufacturer over several years. During this period, new channel policies take effect, providing a quasi-experiment. The new policies lead to substantially fewer violations. With improved compliance, channel prices increase by 2% without loss in volume. The reduction in violations is particularly stark among authorized retailers with lower sales volume, those that previously operated unapproved websites, and those that have received violation notifications for the specific product before. Moreover, low service providers improve their service. At the same time, there is an increase in opportunistic behavior among top retailers, or retailers that received notifications for other products, and for less popular products via deep discounting.

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Keywords: pricing policy • legal • pricing • channel relationships • quasi-experiment • e-commerce

1. Introduction

Manufacturers today are finding the task of controlling their brands and products increasingly challenging. In the era of omni-channel retailing and intense online distribution, customers can purchase the same items through many different outlets at the same time. On one hand, online distributor and retailer activities are highly visible, since a simple search can reveal the way they display and price a specific product on a specific website. On the other hand, online retailers may alter prices often or may sell a particular product on many different websites, behaviors that hinder manufacturers' ability to monitor retailers' actions, detect violations, and then enforce their policies. Thus, while digital technologies reduce the cost of *monitoring* and *detection*, the question of how to effectively *monitor* and *enforce* digital policies remains open.

Past research on digital enforcement was primarily focused on copyright enforcement and piracy in creative arts industries such as music, books, movies, and images. These infractions are commonly committed by individual offenders who have no contractual relationship with the content creators or owners of the work.[1] The research on copyright enforcement, therefore, typically investigates the effects of legislators' changes in copyright policies, and generally shows increased sales of legitimate content because end-consumers change their behavior, not necessarily because pirates commit fewer violations.

This paper focuses a different lens on digital enforcement by investigating whether manufacturers can decrease violations among their legitimate retailers through improving their ability to digitally monitor and enforce minimum advertised price (MAP) policies. MAP policies allow manufacturers to unilaterally impose a lower bound on retailers' advertised prices and are widely used in online marketplaces for consumer durable goods such as electronics, cameras, appliances, sporting goods, and toys, as well as in business-to-business online markets. By imposing a lower bound, manufacturers can protect retail margins and brand image, coordinate prices across different online and offline channels and outlets, and ensure participation of heterogeneous retailers. Since MAP policies are typically confidential, I can only estimate, conservatively, that at least 600 manufacturers use such policies with their downstream channel partners.[2]

In this paper, I study the ability of manufacturers to improve online MAP compliance via investing in monitoring and enforcement mechanisms. Specifically, I observe the interactions between a durable goods manufacturer and hundreds of its online retail partners between May 2010 and December 2013. During this time, the manufacturer changes its MAP policy and its distribution agreement. These changes include a requirement to preapprove all retail websites, a new three-strike punishment protocol, higher specificity of the punishments on each violation, and sending out notification emails when violations occur. I examine the retailers' response to these changes.

While MAP is widely used in practice, the academic literature on MAP violations is quite limited. There is, however, a large body of literature on resale price maintenance (RPM),[3] which is a vertical restraint similar to MAP. That literature discusses RPM's ability to coordinate the retail channel, prevent deep discounting and free riding, and motivate nonprice competition. The main difference between MAP and RPM is that RPM sets bounds on resale prices, as opposed to advertised prices only, and thus was per se illegal between 1911 to 2007, while MAP has been considered a legal policy since 1919. In an online setting, MAP becomes essentially a minimum RPM policy because any posted price is typically considered an advertised price. Past theoretical literature considers these policies self-enforcing (Telser 1980) and treats RPM and MAP prices as dictated by the manufacturer, and does not consider the possibility of violations (e.g., Kali 1998). Furthermore, there is no systematic evidence of MAP violations in offline channels.

At the same time, opportunistic retailers in online environments often advertise products with prices below the MAP, thus violating the policy (Pereira 2008, Barr 2012, Israeli et al. 2016). Retailers violate MAP even though they sign authorized dealer agreements to follow the manufacturer's policies, which include MAP policies that describe the consequences of violations, such as halting product shipments for a set period or terminating that retailer as a distributor. MAP violations may be attributed to manufacturers' failure to invest in either monitoring or enforcement efforts, which prevents them from acquiring detailed information on advertised retail prices. Alternatively, manufacturers may become aware of violations, but are unable or unwilling to enforce their MAP policy. This view is documented in academic papers that often abstract to parsimonious models that only consider a reduction of asymmetric information and enforcement severity, certainty, and costs as mechanisms to prevent opportunism (see Becker 1968, Stigler 1970, Alchian and Demsetz 1972; and others). However, retailers often commit violations even though a MAP policy is a clear legal document, and despite substantial investments manufacturers make in monitoring and enforcement.

Manufacturers seek a way to effectively enforce MAP and achieve compliance within online markets, which account for a significant fraction of sales. Accordingly, my analysis uncovers two key elements of successful channel policies to enforce pricing: customizing channel policies to the online retail environment, and improving the credibility of monitoring and the punishment.[4]

One reason that enforcing MAP among online retailers is so difficult is sheer volume: the vast distribution through online channels hinders manufacturers' ability to monitor retailer actions, and retailers sometimes take advantage of that fact. Without an automated monitoring system, a manufacturer has to check the advertised price for each of its stock-keeping units (SKUs) on each website where its products are sold. In addition, savvy retailers may choose to advertise their products under multiple domain names,[5] which manufacturers might not track. In such cases, even if the manufacturer obtains the advertised prices of all its SKUs, it often does not know which website is associated with which retailer. Thus, the online channel requires greater transparency to alleviate this asymmetric information.

Monitoring alone is not enough to achieve compliance to MAP policy, however; retailers must believe that the threat of punishment is credible and that the manufacturer will enforce the policy. During the sample period, the manufacturer I observe invests in obtaining detailed information on the pricing behavior of downstream retailers. In addition, employees spend a substantial fraction of their time monitoring and enforcing the MAP. Initially, these investments have little impact on compliance with the MAP policy. For example, at the beginning of the sample (May 2010), the manufacturer lacks any automated enforcement method. In November 2011, the manufacturer institutes a two-month test period in which notification emails are sent to violating retailers, resulting in a short-term reduction in violations. Yet, in subsequent weeks, these retailers commit additional violations, revealing that investments in monitoring and enforcement are insufficient for achieving long-term compliance.

To improve long-term compliance, the manufacturer substantially revised its channel agreements and policies in June 2012, and it required its authorized retailers to resign these agreements.[6] The focus of this paper is the effect of this policy change on MAP compliance. I treat the manufacturer's policy change as a quasi-experiment that allows me to explore the market's reaction to the change.

The revised policies of June 2012 include two main changes. First, the manufacturer created a standalone e-commerce agreement, distinct from the authorized dealer agreement. The e-commerce agreement required its retailers to go through an additional registration procedure to become authorized e-commerce retailers, and to preapprove all domain names. This change allowed the manufacturer to address the challenges of the online channel directly and adapt the agreement to fit the current retail environment. With this customization, the manufacturer complemented its monitoring efforts by discerning which websites belong to which authorized partner. This change also allowed the manufacturer to correctly identify websites of retailers that do not have a distribution authorization agreement, namely, unauthorized retailers. This step improved transparency and the credibility of the manufacturer's ability to monitor MAP.

Second, the manufacturer revised the MAP policy. The original policy threatened termination of a violating retailer's authorization to sell a product, a product line, or all of the manufacturer's products as possible punishments, but did not specify a time frame. The new policy introduces a three-strikes enforcement protocol, with detailed explanations of the consequences of each violation. The punishment for continuous violations under the new policy is termination, similar to that before the policy change. Additionally, under the new MAP policy, violating retailers receive an email notifying them of a violation. Specifying clear consequences and sending intermediate notification emails allows the manufacturer to credibly signal its commitment to enforcing the policy. Importantly, the threat of punishment appears to be more credible after the policy change even though the punishment in both the original and the updated policy are the same.

Note that, because manufacturers must treat all of their authorized retailers uniformly, the policies must be the same across authorized retailers over a given period of time. My empirical methodology exploits the fact that manufacturers hold direct legitimate authority over authorized retailers. By contrast, unauthorized retailers' actions are not directly governed by a MAP policy. I show that such retailers can serve as a control group within my quasi-experiment. I study changes over time (before versus after) in outcome variables in a difference-in-differences setting.[7] In particular, I compare the difference in outcome variables such as violation rates before and after the policy change between authorized ("treated") and unauthorized ("control") retailers. The difference-in-differences approach captures the effect of the changes in legal documents by comparing the violation rates and depths and other retail variables before versus after the changes in agreements and policies (first difference); and comparing authorized versus unauthorized retailers (second difference).

The empirical methodology does not assume that the unauthorized group is ex ante identical to the treatment group of authorized retailers; indeed, authorization is not randomly assigned. The difference-in-differences methodology accounts for the fact that authorized and unauthorized retailers are potentially different in various confounding characteristics. I only assume that the trends in behavior are similar before the intervention. Specifically, the identifying assumption for the difference-in-differences approach to measure the effect of interventions is that the trend in unauthorized retailers is approximately similar to the trend in authorized retailers in the absence of the intervention shock. This premise is also confirmed in my data.

Overall, I find improvements in compliance among authorized retailers following the policy change. Before the policy change, average violation rates in the authorized channel were 8.5%. Using the difference-in-differences approach, I find a persistent reduction of 40%–80% in violation rates among authorized retailers after the new channel policies were introduced. This effect is economically meaningful and robust to a variety of tests and specifications. The increased compliance leads to an average price increase of 2% among authorized retailers, but no systematic evidence suggests a reduction in volume ordered from the manufacturer or dollars spent. Additionally, some evidence suggests an improvement in service outputs, such as assortment size and duration of product availability following the policy change.

I find that the reduction in violations is particularly stark among three types of authorized retailers: those that are not top sellers in the category; those that previously operated unapproved websites; or those that received notification for a particular SKU in the test period. In addition, retailers that previously did not provide services improve certain elements of their service following the policy change. These findings suggest that the policy is effective for retailers who are less committed in the first place, and those that exhibited opportunistic behavior in the past. At the same time, there is no reduction in violation rates after the policy change among top authorized retailers; for retailers that received violation-notification emails on other SKUs during the test period; and for narrowly distributed products, although those groups exhibit higher opportunistic behavior by providing higher discounts when violating MAP after the policy change.

Moreover, I find that notification emails serve as effective warnings to authorized retailers that violate MAP price following the policy changes. This is in contrast to the test period, in which the same monitoring and notification tools were used, and emails were sent out to violators but did not have a sustained impact. I attribute the change in the emails' effectiveness to the new agreements and policies. An indication that emails are an important component of enforcement is that once the emailing feature temporarily stopped, violation rates increased again among authorized retailers.

Note that the manufacturer had an authorized dealer agreement and a MAP policy in place for nearly seven years before it introduced the new agreements and policies. The original agreement facilitated selection of the channel partners and provided clear incentives for retailers to comply. During the two years before the policy change, the manufacturer systematically monitored online prices, and eliminated distribution through a violating distributor. Despite these measures, MAP violations continued. After June 2012, however, once the manufacturer established a clear set of channel agreements and policies both internally and externally, it significantly improved compliance among authorized retailers in the channel.

2. Related Literature

While MAP is widely used in practice, the academic literature on MAP is very limited. An exception is Kali (1998), who takes an analytical approach, modeling MAP as an extension of RPM, which can be used to legally maximize channel profits. Hence, Kali (1998) treats MAP as a solution to a pricing problem. Initially, MAP and RPM may have been viewed as self-enforcing policies,[8] but the prevalence of MAP violations in recent years has become a central concern for manufacturers. Another exception is the work by Charness and Chen (2002), looking at MAP policy design using a controlled laboratory experiment to investigate the question of how to achieve MAP compliance by manipulating the penalty upon violation.[9] The vast majority of penalties considered in their work were monetary fines, but their results show that pulling the product from the vendor achieved the highest compliance in the lab. I extend that research by studying the effect of real-world changes in channel policies and enforcement efforts and their effect on real market outcomes.

To date, the only empirical study on MAP that uses observational data (Israeli et al. 2016) documents how different retailer, product, and market characteristics correspond with MAP violations. They show differences in violation behavior among authorized and unauthorized retailers[10] and conjecture that, to achieve full channel compliance, authorized and unauthorized retailers should each be addressed separately. In contrast to the descriptive nature of that study, I use a quasi-experiment to study how manufacturers can effectively implement MAP policies in online environments.

The literature on digital enforcement focuses primarily on copyright enforcement and piracy in creative arts industries, infractions that are commonly committed by individual offenders who have no contractual relationship with the content creators or owners of the work. Furthermore, consumers of that content may not even be aware that any laws were violated. Danaher et al. (2014), Adermon and Liang (2014), and Reimers (2016) find that new antipiracy laws or new enforcement actions that delist infringing content can increase legitimate sales. The researchers attribute this increase in sales to consumers' awareness of the law or an increase in search frictions. By contrast, I focus on addressing the violators themselves (the authorized retailers), and not the end consumers. Note that, pirates and copyright violators are, effectively, the unauthorized retailers in my setting, and not the authorized retailers. That is, this paper sheds light on online enforcement when the offenders have a contractual relationship with the firm.[11] More recently, Luo and Mortimer (2017) investigate how the wording of messages regarding postviolations dispute resolution may result in higher settlement rates. My paper aims at improving the effectiveness of the policy to reduce violations in the first place.

The literature on digital enforcement relies heavily on crime and punishment deterrence research (Becker 1968, Stigler 1970) and on relational contracts between principals and agents (Alchian and Demsetz 1972, Jensen and Meckling 1976). The crime and punishment literature examines the effect of certainty, severity, and immediacy of punishment on the likelihood of engaging in illegal activity. *Certainty* refers to the probability of being punished, while *severity* and *immediacy* refer to the timing and onerousness of the punishment itself. In general, an improvement in any of these constructs will lead to fewer violations (Nagin 2013). In the setting I study, the manufacturer revises the MAP policy such that the consequences of a violation are clear and credible, thus increasing certainty, which should deter opportunistic behavior by retailers.

Interestingly, while the manufacturer's final punishment does not change, the wording and details of the punishment procedure do, and these changes improve the credibility of the punishment. Moreover, the punishment, which was initially vague ("may result in termination"), is now detailed and specific. Casey and Scholz (1991) show that when penalties and probabilities of getting caught are high, clarity of the punishment increases compliance, but when risks are low, vagueness is more likely to increase compliance. In my setting, if retailers perceive the risk to be low before the policy change, and the risk as high after the policy change, there is a fit between the level of vagueness and the level of risk, which would then imply deterrence both before and after the policy change, in contrast to my findings.

Additionally, the new punishment structure includes three strikes, such that the final strike leads to a termination of the retailer. Therefore, retailers may have perceived the first and second strikes in the new policy as less severe punishments compared with the original policy, potentially leading to more violations as retailers evaluate the costs and benefits of such behavior. While the theoretical literature that examines repeat-offender punishments is inconclusive, the empirical legal literature finds that three-strikes legislation either reduces the incidence of the effected crimes or has a null effect on crime (Shepherd 2002). In that case, however, the final punishment is typically more severe than the original punishment before the policy change, while in my setting, the final punishment is identical.

The economic literature on relational contracts (see Malcomson 2013 for an extensive overview) expands the principal-agent literature to study relationships between firms. One central aspect in this literature focuses on developing agreements that are self-enforcing because of a threat to terminate the relationship. As already mentioned, MAP is viewed as a self-enforcing mechanism, yet violations are common. A recent growing body of empirical literature investigates elements of self-enforcing agreements. For example, Kosova and Sertsios (2018) demonstrate that in the hotel-franchising industry, initial requirements in a contract can be used to increase the agent's ex post rents, which in turn should boost self-enforceability. In particular, they find that hotels that are farther away from headquarters are larger and higher quality, and produce higher revenues. However, they do not observe violations in contracted behavior but rather conjecture that initial contract requirements serve as a substitute to monitoring intensity and therefore mitigate agency problems. In my setting, the policy and initial requirements are uniform across all authorized retailers, and it provides them with the same margin of protection by imposing a lower bound on the advertised price. Because of the online environment and the fact that the policy requirement is about pricing that is observable, monitoring costs are also uniform across retailers.

Finally, I draw on literature concerning distribution channel management and coordination. Research on enforcement of manufacturers' contracts and policies has focused on gray markets, franchising, and exclusive territories (see Antia and Frazier 2001, Antia et al. 2006, Bergen et al. 1998, Dutta et al. 1994; and others), typically in offline settings. Most of that literature investigates what determines enforcement type, enforcement severity, or the tolerance to violations rather than the effects or effectiveness of enforcement (e.g., Antia and Frazier 2001, Bergen et al. 1998, Gilliland and Bello 2002). Other studies look at how channel partners view different control mechanisms and how likely they are to affect commitment or opportunistic behavior in a variety of market settings (see Anderson and Weitz 1992, Jap and Ganesan 2000, Murry and Heide 1998, Stump and Heide 1996; and others).

A few studies examine how control mechanisms and enforcement affect the behavior of a counterpart channel member (Heide et al. 2007, Wathne and Heide 2000; and Antia et al. 2006). This literature would predict deterrence both before *and* after the policy change in the setting I studied; it does not predict a difference between the periods. My study contributes to the literature by showing that the context and terms of the policy affects the manufacturer's ability to govern the market, above and beyond severity, credibility, and immediacy. Furthermore, the aforementioned literature relies on self-reported measures for both the dependent and independent variables, while I use observed data.

Lastly, one of the new features of the policy change I investigate is a notification email that the manufacturer sends to violating authorized retailers, which contains the MAP policy and reminds the retailers of expected behavior and consequences of violations. The notification potentially increases both the credibility and certainty of the enforcement threat by demonstrating to authorized retailers that their behavior is being monitored. As in Mazar et al. (2008), the mere reminder of compliance standards can decrease subsequent violation behavior.

To my knowledge, extant research in channel management uses self-reported survey data from various channel partners or lab experiments with hypothetical market conditions. Studies of actual manufacturer and retail behavior are difficult to execute because data on manufacturer restraints and partners' behavior is often proprietary or hard to obtain. Even when data are available, it is challenging to form empirical inference because of the limited variation in channel contracts and the endogenous behavior of channel partners. Manufacturers do not frequently vary contract terms over time or among channel partners; whether channel partners comply with contract terms is an endogenous choice. My paper attempts to overcome these limitations and empirically identify the effect of monitoring and enforcement of vertical restraints. While my study is limited to one manufacturer in a single industry, it is the first to use observed data to try and identify the effect of enforcement on violation behavior in the channel. I exploit my unique setting and data structure to employ a difference-in-differences methodology, which is commonly used to investigate the effect of interventions and evaluate policies in economics and marketing (see the canonical example of Card and Kruger 1994 and many others).

3. Institutional Details and Summary Statistics

This section describes the data on which this paper is based. Section 3.1 describes the state of the industry and the manufacturer's policy change, which is the treatment evaluated in the paper. Section 3.2 describes the data and provides summary statistics.

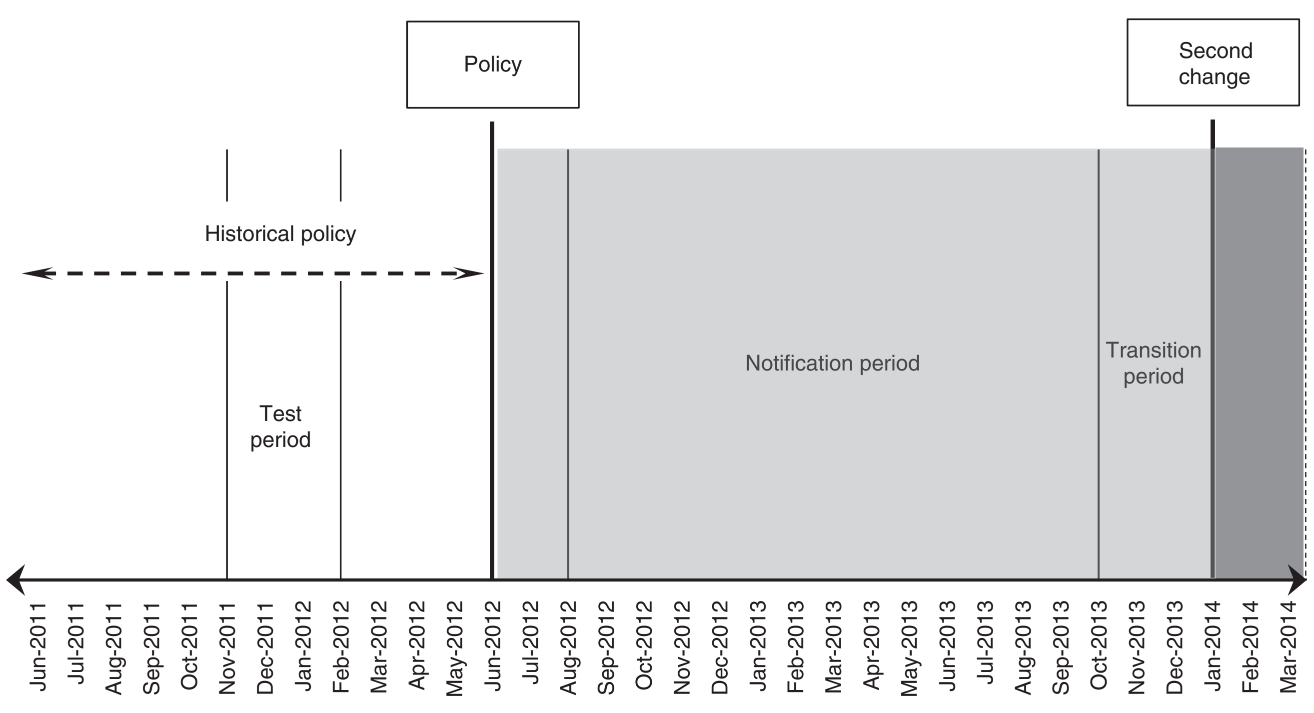
3.1. Institutional Details and the Manufacturer's Policy Change

To improve their efforts to monitor MAP compliance in online marketplaces, manufacturers typically hire third-party companies to track MAP prices on the Internet. These companies (such as Channel IQ, or Market Track) search the Internet for instances where a product under a MAP policy is offered for sale, and record the retailer's identity and the advertised price. Manufacturers typically pay the third-party companies to track a subset of SKUs and provide dashboards, spreadsheets, and screenshots that demonstrate the current pricing in the market. Sometimes manufacturers attempt to improve MAP compliance by updating their agreements with distributors and retailers, changing the wording of the MAP policies and the actions on MAP violations, as well as eliminating unauthorized distribution.

The manufacturer I observe had a MAP policy in place since 2005, as well as an authorized dealer agreement, both of which were considered active as long as both parties complied with the terms. These allowed the manufacturer to select appropriate partners and provided the retailers with incentives to adhere to the policy. Initially, monitoring of MAP compliance was manual and sporadic. In the past decade, as the distribution grew and the online channel became important,[12] the manufacturer took additional actions to improve MAP compliance in the online channel. Eventually, the manufacturer added systematic and automatic monitoring of online prices, which revealed that its products were available on many more online outlets than previously identified. Not only did the manufacturer discover unauthorized retailers, but it also found out that several of the seemingly unauthorized websites were in fact the manufacturer's own retailers using unknown domain names. That is, authorized retailers used several different domain names and identities when selling the products, but those were unknown to the manufacturer.

The focus of this paper is June 2012, when the manufacturer revised its agreement and policies and had its authorized dealers sign updated agreements (see Figure 1 for a timeline of the policy changes). The policy change includes two major components: a new dealer agreement with a standalone e-commerce agreement, and an updated MAP policy and enforcement protocol. When revising the agreement, the manufacturer sought to reduce asymmetric information regarding the online presence of its products, both in terms of the online marketplaces where the product is being sold and in terms of the seller's identity. Therefore, the new agreement requires retailers to be preapproved to sell products online, in predetermined website addresses, and it restricts all e-commerce dealers from advertising products unless they carry a minimum of one month inventory. The agreement also requires retailers to commit to a predefined minimum dollar amount of inventory for a specified time range.

Figure 1. Timeline of the Manufacturer's Policy Change



Two components of the agreement are important in customizing it to the online retail environment: allowing retailers to opt out from the online *or* brick-and-mortar channel, and requiring e-commerce retailers to register their URLs and have them approved. These steps reduce information asymmetry and allow the manufacturer to segment e-commerce from brick-and-mortar retailers, thereby providing more transparency in the online marketplace and improved credibility of monitoring.

The updated MAP policy and enforcement protocol include a detailed explanation of the consequences of a violation. The policy outlines a three-strikes punishment structure with well-defined terms. Following the first violation, an authorized retailer loses product for 30 days, a second violation leads to cutting off distribution for 60 days, and a third violation results in termination of that retailer. In addition, when an authorized retailer violates MAP policy online, it receives a violation notification email. Importantly, while the MAP policy was updated, MAP prices remained static in the six months before the policy change and the six months after the policy change.

The main difference between the updated MAP policy and the original 2005 policy was the clear explanation of the expected consequences of violations. The original policy mentioned that a MAP violation "may result in termination of distribution of the product, the line, or complete termination," but did not specify detailed consequences. That is, the same potential punishment was a part of the original policy, but in the context of that policy, it did not deter violations. This suggests that the same termination threat did not seem credible in the historical policy, within the historical channel structure. Detailing the specific steps of punishment and including warning emails signal the manufacturer's commitment to enforcing the policy and enhance the credibility of the punishment.

Thus, the manufacturer I study moves from a vague MAP policy to a more specific description of the punishment. However, there does not seem to be a single best practice in the durable goods industry. Firms vary in whether they use vague or specific descriptions of the consequences of violations. A specific description usually defines the exact steps a manufacturer would take upon detecting a violation. Of the 462 MAP policies that were collected in an online search, 41% describe a specific punishment or timeline, and 59% include vague descriptions of potential punishments. There is no systematic difference in policies across industries and product categories. Figure 2 provides examples of specific and vague wordings. One specific punishment is LG's three-strikes policy, where for each violation there is an escalating punishment. Conversely, Samsung's policy is vague, stating that "sanctions will be unilaterally imposed" without specifying what those sanctions may entail.

In practice, the manufacturer in this research monitors prices of products that are subject to MAP daily, but sends notifications to violating authorized retailers on a weekly basis. A notification email indicates the occurrence of the violation, reminds the violating retailer of the MAP policy, and includes a proof of the violation using a screenshot from one of the retailer's URLs. For retailers that continuously violate MAP even after receiving a notification, the manufacturer applies the three-strikes policy and continues to monitor price changes. When dealing with unauthorized retailers, the manufacturer sends "cease and desist" letters through an attorney as an attempt to

Figure 2. Punishment Descriptions in Sample Policies

LG Electronics USA, Inc. ("*LGEUS*") *Policy*:
Effective March 1, 2012:
4. *Recourse*
Resellers advertising any LGEUS model below the MAP price listed in the MAP Schedules to be distributed to resellers by LGEUS from time to time will result in LGEUS taking the following unilateral actions unless such violation is determined by LGEUS to be a mistake, error or due to causes beyond the control of reseller:
a. The first violation will result in a formal warning letter being sent to the reseller.
b. The second violation will result in a warning letter to the reseller stating that any further violations will result in the reseller being placed on LGEUS' "Do Not Sell" ("DNS") list, which will prohibit authorized LGEUS distributors from selling products to said reseller
c. The third violation will result in the reseller being notified that the authorized LGEUS distributors have been notified their account has been added to the do not sell DNS list for a period of minimum of 6 months.
Source: https://web.archive.org/web/20160213070758/http://www.lg.com/us/commercial/display/heb2bmap (accessed June 10, 2018).

Samsung Techwin America ("*STA*") *Policy*:
Effective September 16, 2013:
(I) In the event a dealer or distributor chooses not to follow the MAP policy, sanctions will be unilaterally imposed by STA. Intentional and/or repeated failure to abide by this policy will result in termination of dealership or distributorship. STA does not intend to do business with dealers and/or distributors who compromise the perceived value of STA and its products. STA may monitor the advertised price of dealers or distributors, either directly or via the use of third party agencies. Third party agencies retained by STA may engage in monitoring of any advertisements.
Source: https://web.archive.org/web/20150910201955/https://www.samsung-security.com/en/sales-and-services/map-policy.aspx (accessed June 10, 2018).

force those retailers to stop selling its products. Unauthorized retailers on eBay are sanctioned by eBay's intellectual property infringement flow (eBay Verified Rights Owner program[13]) that removes the infringing webpages from eBay.

To inform retailers of the new agreements and policies, and verify that retailers fully understand them, the manufacturer held training sessions with its employees, intermediaries, and distributors. During these sessions, the manufacturer explained the reasons and motivation for the channel agreements and policies, and reviewed the application procedures in detail. The training process aligned the manufacturer's employees, agents, and retailers with the new policies and agreements. The new legal documents were effective June 2012, and the manufacturer launched the notification email system by the end of July 2012.

As already stated, the focus of this paper is the effect of the policy change of June 2012. Before the policy change, however, in November 2011, the manufacturer administered a two-month test period in which violation notification emails were sent out but the MAP policy did not change. I use that period to evaluate the effect of the emails in the absence of the other components of the policy change.

Note that 18 months after the policy change (in January 2014), the manufacturer modified the agreements and policies to significantly reduce the number of authorized online retailers. The three months prior to these changes was a transition period in which no notification emails were sent, but MAP monitoring continued. My main analysis examines data *before* that transition period and the subsequent second policy change. However, details about this later period can be found in Online Appendix B, which further explores the effect of the notification emails both before and after the policy change.

3.2. Data Description and Summary Statistics

The data for this study are provided by Channel IQ, a company that monitors and enforces MAP policies and collects data about online prices for its manufacturer clients, and from one of their manufacturer clients. The data are unique because MAP policies are often confidential, and it is rare to observe communication between a manufacturer and retailers.[14]

The database includes a durable goods manufacturer that sold 144 unique product SKUs via 99 authorized retailers and 454 unauthorized retailers over the period May 2010 to December 2013. The manufacturer is among the top 10 manufacturers in the industry[15] in terms of sales in North America, and in the top 20 in the world. The database contains 1,933,073 daily retailer × SKU observations, which include the price that was documented for that retailer × SKU combination in a specific day as well as the "MAP price," which is the price the manufacturer set as a lower bound on advertising price for the product for that time period.[16] I also observe whether the retailer is an authorized retailer of the manufacturer. For the difference-in-differences analysis, I collapse the data into 84,981 retailer × SKU × month combinations, out of which 80,064 are used for the main analyses from May 2010 to September 2013.[17]

I compute a variety of measures from the raw data. For each daily retailer × SKU observation, I define an indicator variable that indicates whether or not a MAP violation occurred that day. If violations occur, I also compute the depth of the violation, which is the percentage below MAP at which an SKU was priced. When I aggregate the data, I compute the average percentage of violations and average depth of violations for each month. For example, if for a particular SKU a retailer has 20 observations in a given month, and has violated MAP in two of them, the average rate of violations for that month for this SKU is 10%. Similarly, if the MAP price for that SKU is $100, and in each violation the product was offered at $80, the average depth of violations for that month is 20%.

Table 1 provides the summary statistics. Panel A provides sample level characteristics and panel B provides retailer level characteristics. Columns 2–7 provide aggregate level statistics, columns 8–9 provide statistics on the authorized retailers, and columns 10–11 provide statistics on the unauthorized retailers. The average percentage of violations in the monthly database is 16.1% (6.7% among authorized, 28.9% among unauthorized), and the average depth of violations is 8.3% (7.4% among authorized, 8.9% among unauthorized). I observe violations on 21,337 of the 80,064 monthly observations. A total of 57.5% of the observations are of authorized retailers.

For each retailer and SKU, I also compute the number of days in a month the SKU appeared in the database (22.6 days on average, 23.4 days for authorized, and 21.5 days for unauthorized). This variable proxies for the availability of the product for that retailer. To proxy for assortment size of a retailer, I compute the number of unique SKUs that each retailer offered during a month. A retailer offers 8.8 SKUs each month on average, an authorized retailer has an assortment size of 17 on average, and an unauthorized retailer assortment size is 7 on average. To proxy for distribution intensity of a product, I compute for each SKU how many retailers carry it; an average SKU is carried by 67 retailers. For each month, I compute the number of authorized and unauthorized retailers that were observed. I observe 174 retailers per month on average, out of which about 40% are authorized.

In addition, I collect retailer-specific data: 18% of retailers have a showroom in addition to their online website (47% of authorized retailers, 11% of unauthorized retailers); 8% of retailers provide an online chat

Table 1. Summary Statistics

Variable	Mean	Median	SD	Min	Max	N	Mean	SD	Mean	SD
				Panel A: Sample level characteristics						
Authorized	0.57	1	0.49	0	1	80,064	Yes		No	
Charge For Shipping	0.12	0	0.33	0	1	80,064	0.11	0.32	0.14	0.35
SKU Availability	22.6	28	9.84	1	31	80,064	23.4	9.35	21.5	10.4
Violation Rate	0.16	0	0.36	0	1	80,064	0.07	0.24	0.29	0.45
Violation Depth	0.08	0.05	0.13	6E–06	0.99	21,337	0.07	0.13	0.09	0.12
Top Retailer	0.16	0	0.37	0	1	80,064	0.28	0.45	0.00	0.03
Dual Status	0.19	0	0.39	0	1	80,064	0.32	0.47	0.00	0.00
Chat Tool	0.20	0	0.40	0	1	77,437	0.29	0.45	0.08	0.26
Showroom	0.35	0	0.48	0	1	77,437	0.49	0.50	0.15	0.36
Call Center	0.69	1	0.46	0	1	77,437	0.81	0.39	0.53	0.50
				Panel B: Retailer level characteristics						
Authorized	0.18	0	0.38	0	1	517	Yes		No	
Assortment Size	8.8	3	13.1	1	88	517	17	15.5	7	11.8
Retailer Shipping	0.46	0	0.50	0	1	517	0.55	0.50	0.44	0.50
Retailer Appearances	327	175	368	1	1,311	517	795	382	227	278
Number of Market	1.21	1	0.52	1	3	517	1.66	0.76	1.12	0.39
Top Retailer	0.02	0	0.15	0	1	517	0.12	0.33	0.002	0.05
Dual Status	0.06	0	0.23	0	1	517	0.33	0.47	0	0
Chat Tool	0.08	0	0.27	0	1	420	0.18	0.39	0.05	0.21
Showroom	0.18	0	0.39	0	1	420	0.47	0.50	0.11	0.31
Call Center	0.44	0	0.50	0	1	420	0.76	0.43	0.36	0.48

Notes. Each observation is a Retailer × SKU × month combination. The sample in the table includes all observations from May 2010 to September 2013. Panel A presents summary statistics for the entire sample, while panel B presents summary statistics by retailer in the sample. In addition, the four right columns present the mean and standard deviations separated by authorized (columns 8–9) and unauthorized (columns 10–11) retailers. Authorized indicates whether or not the retailer is an authorized dealer. The charge for shipping is true if there was a shipping charge for the SKU × retailer × month combination. SKU availability indicates the number of days in a month in which the retailer offered the SKU. The violation rate indicates how often the retailer advertised a price below MAP for the specific SKU during the month. The violation depth indicates what was the average percent discount below MAP that the retailer advertised for this SKU during this month. Top retailer is true for the top online retailers in terms of sales in this industry. Dual status is true for authorized retailers that ever sold on unapproved websites. Chat Tool, Showroom, and Call Center indicate whether the retailer offers these services. Assortment Size is the number of products a retailer holds in a certain month. Retailer Shipping is true if there is always a shipping charge for the retailer. Retailer Appearances is the number of days that the retailer was observed in the data. Number of Markets indicates the number of platforms on which the retailer sells their products (out of Amazon, eBay, and nonplatform websites).

tool (18% of authorized, 5% of unauthorized); and 44% of retailers have a call center (76% of authorized, 36% of unauthorized).[18] In addition, 70% of the top online retailers in terms of sales in this industry[19] sell the manufacturer's product, 93% of which are authorized retailers.

I also obtained detailed manufacturer sales reports that include the purchases of products for each of the retailers between July 2002 and December 2013. I use those data to investigate the effect of MAP compliance and increased prices on demand.

4. Estimation Approach

This section discusses the main identification strategy of my empirical analysis. I attempt to measure the overall effect of the June 2012 policy change on retailers' violation rates, violation depths, assortment size, and duration of product availability. I examine the rate of violation occurrences, since improving those rates was the main goal of the policy change. The effect of the policy on violation depth is also of interest, since retailers can react to the policy change by reducing prices less than in the past if they want to test the manufacturer's reaction, or more than in the past if they believe they will now be punished anyway. Last, I estimate the effect of the policy change on assortment size and duration of SKU availability as a proxy for service. If indeed, as predicted in theoretical papers, a well-governed MAP policy protects retail margin and thus moves retailers away from price competition to service competition, one would expect service to improve because of the policy change. Online, service can manifest itself by offering a larger assortment size or having an SKU available for purchase every day.[20]

The difficulty in computing the overall effect of the policy on authorized retailers' behavior is to find the appropriate counterfactual. Recall that the manufacturer's agreements and policies directly affect only the authorized retailers. Furthermore, manufacturers must treat all their authorized retailers uniformly, thus the policies must be the same across authorized retailers over a given time period.[21] However, I cannot simply compare the outcome variables of the authorized retailers group before and after the policy change, since I may be confounding the pre-post differences

Table 2. The Effect of Manufacturer Policy Changes: Difference-in-Differences Analysis

	Violation rate		Violation depth		Assortment size		SKU availability	
Authorized	−0.19***	−0.16***	0.00074	0.0062	6.5***	2.6	1.8***	−0.68***
	(0.0095)	(0.012)	(0.0045)	(0.0052)	(0.37)	(1.6)	(0.13)	(0.14)
Authorized × Post	−0.066***	−0.041***	−0.025***	−0.0092	5.1***	4**	1***	1.2***
	(0.014)	(0.014)	(0.0068)	(0.0068)	(0.7)	(1.7)	(0.27)	(0.26)
Assortment Size		−0.0011***		−0.0005***				0.0019
		(0.00022)		(0.00009)				(0.003)
Charge for Shipping		0.072***		0.0041				2.9***
		(0.011)		(0.0046)				(0.15)
Retailer Shipping		−0.0069		0.018***		0.58		−1.4***
		(0.01)		(0.0045)		(1.1)		(0.14)
Days SKU offered		−0.0031***		−0.0013***				
		(0.00022)		(0.00013)				
Retailer all Appearances		−6e−05***		−2e−05***		0.009***		0.0074***
		(0.00002)		(6.3e−06)		(0.0021)		(0.0002)
Number of Market		0.005		0.0027		0.15		−0.62***
		(0.0051)		(0.0032)		(1)		(0.094)
Constant	0.31***	0.43***	0.056***	0.1***	12***	7***	24***	21***
	(0.011)	(0.013)	(0.0037)	(0.0068)	(1.3)	(1.8)	(0.22)	(0.26)
R-square	0.13	0.15	0.12	0.14	0.15	0.21	0.23	0.28
N case	80,064	80,064	21,337	21,337	7,187	7,187	80,064	80,064
SKU fixed effect	+	+	+	+	−	−	+	+
Month-year F	+	+	+	+	+	+	+	+

Notes. This table contains the results of Equation (1) for four different dependent variables. The dependent variables are the average monthly violations rate (columns 1–2), the average monthly violation depth (columns 3–4), the average assortment size (columns 5–6), and the number of appearances of an SKU in a month (columns 7–8). The subsample for violation depth analysis includes only retailer × SKU × month combinations where a violation occurred. The assortment size analysis is done for a subsample of retailer and month observations. The treatment effect (δ) is the coefficient for the *Authorized × Post* variable (row 2). In columns 1–4 and 7–8, the standard errors are clustered by retailer × SKU, and there are SKU fixed effects. In columns 5–6, the standard errors are clustered by retailer. Standard errors are reported in parentheses.

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

with other unobservable changes in the market, such as demand shocks that may coincide with the policy change. Therefore, I need to find an appropriate comparable group to the group of authorized retailers that is subject to the same market forces, but is not directly affected by the policy change.

I use unauthorized retailers that operate in the same market as the authorized retailers to obtain the counterfactual against which to measure the treatment effects. I show that the unauthorized retailers can serve as a control group, which provides a quasi-experiment that allows me to employ a difference-in-differences approach. This approach accounts for the fact that authorized and unauthorized retailers are potentially different in various confounding characteristics. Specifically, I compare the difference in outcome variables such as violation rates before and after the policy change between authorized ("treated") and unauthorized ("control") retailers.

The industry and marketplace in which the manufacturer operates include a big unauthorized channel. These unauthorized retailers are not subject to the manufacturer's rules and regulations, but are subject to the same market forces as the authorized retailers since they operate in the same marketplace. In fact, unauthorized retailers may appear to consumers as authorized retailers, since consumers are not necessarily aware of the manufacturer's dealer agreements. An unauthorized retailer is any retailer that does not have a distribution authorization agreement with the manufacturer; it could be any entity that sells the product without authorization, regardless of scale or size—for example, Amazon.com is an unauthorized retailer for certain manufacturers.[22] Unauthorized retailers obtain their inventory through a legitimate authorized retailer, or through the gray market, and compete with both authorized and other unauthorized retailers. Since manufacturers do not hold legitimate power against the unauthorized channel, MAP policies do not apply to them and are therefore unenforceable in that channel. For simplicity, however, I use the term "violation" to indicate cases where unauthorized retailers advertise prices below MAP. Manufacturers can try to identify unauthorized retailers and combat them through trademark or intellectual property related legal suits, which are time-consuming and hard to prove.

I study changes over time (before versus after the policy change) in outcome variables, such as violation rates, between authorized and unauthorized retailers

Table 3. Robustness: Ignoring Time Series Information

	Retailer composition				Retailer × SKU composition			
	Violation rate	Violation depth	Assortment size	SKU availability	Violation rate	Violation depth	Assortment size	SKU availability
Authorized	−0.11***	0.011	2.9	−0.98***	−0.15***	0.0049	2.9	−1.8***
	(0.012)	(0.0099)	(1.9)	(0.22)	(0.016)	(0.016)	(1.9)	(0.28)
Post	−0.018	−0.032***	−2.3***	−8.9***	−0.031**	−0.02***	−2.3***	−9.2***
	(0.012)	(0.0075)	(0.8)	(0.26)	(0.013)	(0.0063)	(0.8)	(0.34)
Authorized × *Post*	−0.039***	0.02*	2.3	2.1***	−0.026*	0.0027	2.3	2.6***
	(0.013)	(0.011)	(1.7)	(0.34)	(0.013)	(0.011)	(1.7)	(0.43)
Assortment Size	−0.0029***	0.00067***		0.072***	−0.0018***	0.00071*		0.065***
	(0.00024)	(0.00026)		(0.0057)	(0.00034)	(0.0004)		(0.0077)
Charge for Shipping	0.093***	−0.011		3.8***	0.11***	0.022		4.1***
	(0.016)	(0.0088)		(0.3)	(0.021)	(0.014)		(0.38)
Retailer Shipping	0.018*	0.0031	2.3**	−2***	−0.0047	−0.0087	2.3**	−1.5***
	(0.011)	(0.0082)	(1.2)	(0.22)	(0.015)	(0.011)	(1.2)	(0.29)
Days SKU offered	−0.0034***	−0.002***			−0.0027***	−0.001**		
	(0.00055)	(0.0004)			(0.00067)	(0.00046)		
Retailer all Appearances	−0.00003	−0.00004***	0.012***	0.0086***	−0.00007***	−0.00003	0.012***	0.0094***
	(0.00002)	(0.00001)	(0.0023)	(0.0003)	(0.00002)	(0.00002)	(0.0023)	(0.0004)
Number of Market	−0.0038	0.0012	0.57	−0.28**	0.013*	0.015	0.57	−0.12
	(0.0056)	(0.0054)	(1.2)	(0.13)	(0.0076)	(0.01)	(1.2)	(0.16)
Constant	0.39***	0.17***	2	15***	0.4***	0.11***	2	14***
	(0.016)	(0.014)	(1.4)	(0.27)	(0.021)	(0.018)	(1.4)	(0.34)
R-square	0.16	0.18	0.28	0.35	0.17	0.12	0.28	0.38
N case	7,910	2,931	487	7,910	5,106	1,422	487	5,106
SKU fixed effect	+	+	−	+	+	+	−	+

Notes. This table contains the results of Equation (1), where instead of multiple month-year dummies, there is a single "Post" dummy, for four different dependent variables, limiting the sample only to retailers that appear both before and after the policy change took place, while ignoring time series information. I average the various outcome variables before and after the policy change took place (rather than having multiple observations before and after). The dependent variables are the average monthly violations rate (columns 1, 5), the average monthly violation depth (columns 2, 6), the average assortment size (columns 3, 7), and the number of appearances of an SKU in a month (columns 4, 8). In columns 1–4, I use any SKU for a retailer that appeared both before and after the policy change. Columns 5-8 limit the sample further and include only observations for which the retailer and SKU combinations appear both before and after the policy change. Since columns 3 and 7 use retailer level data, they are identical for each of the subsamples. The subsample for violation depth analysis includes only retailer × SKU × month combinations where a violation occurred (and is limited only to retailer × SKU combination with violations both before and after the policy change in column 6). The assortment size analysis is done for a subsample of retailer and month observations. The treatment effect (δ) is the coefficient for the *Authorized* × *Post* variable (row 3). The observations in these regressions are restricted to retailers that were observed both before and after the policy change took place. In columns 1, 2, and 4 (and 5, 6, and 8), the standard errors are clustered by retailer × SKU, and there are SKU fixed effects. In column 3 (and 7), the standard errors are clustered by retailer. Standard errors are reported in parentheses.

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

in a difference-in-differences setting. I show that the trend in unauthorized retailers is approximately similar to the trend in authorized retailers in the absence of the policy change shock, which allows me to use the difference-in-differences methodology despite the differences between the groups.

While the changes in agreements and policies directly impact only the authorized retailers, there could potentially be an indirect effect of the policy change on unauthorized retailers. In that case, the trends in the behavior of unauthorized retailers would be affected by the policy change and thus would not be a valid counterfactual to authorized retailers' behavior, which would threaten my identification strategy. I argue that that this is not likely, and try to mitigate this concern by estimating the effect of the policy change on retailers' behavior relatively close to the time of the policy change.

There are several ways in which the policy change could potentially have an indirect impact on unauthorized retailers. For example, the new policy might advocate against selling product to unauthorized retailers differently than the previous policy. However, this aspect of the policy did not change. Furthermore, even if the policy change did alter the attitude of authorized retailers toward unauthorized retailers, it is unclear that the outcome variables of violation rates or depths would have been differentially affected. Specifically, because consumers are unaware of the differences between authorized and unauthorized retailers, both types of retailers likely operate under the same demand-side forces. Regarding supply, both a reduction in inventory to the unauthorized

Table 4. Heterogeneity in Response to the Policy Change

Panel A: Top sellers and service providers								
	Top sellers				Service provider			
	Violation rate	Violation depth	Assortment size	SKU availability	Violation rate	Violation depth	Assortment size	SKU availability
Authorized	−0.1***	0.011	1.3	−0.98***	−0.28***	−0.0022	1.9	−2***
	(0.012)	(0.01)	(2.1)	(0.24)	(0.023)	(0.021)	(2.9)	(0.58)
Post	−0.032***	−0.047***	−2.5***	−9.6***	−0.047**	−0.022**	−0.97	−6.9***
	(0.012)	(0.0084)	(0.95)	(0.26)	(0.02)	(0.011)	(0.9)	(0.46)
Authorized × *Post*	−0.038***	0.0084	2.5	3***	0.037	0.012	6.6***	5.1***
	(0.013)	(0.012)	(2.1)	(0.38)	(0.024)	(0.031)	(2.5)	(0.71)
Authorized × *Post* × *Yes*	0.023	0.086***	−0.23	−3.3**	−0.08***	0.04	−3.3	−1.5*
	(0.035)	(0.02)	(2.8)	(1.4)	(0.029)	(0.033)	(3.4)	(0.82)
Post × *Yes*	0.011		1.1	2.5*	0.023	−0.047***	−2.9	−4.1***
	(0.032)		(0.95)	(1.3)	(0.024)	(0.016)	(1.8)	(0.57)
Authorized × *Yes*	0.27***		9.7**	−8.4***	0.21***	−0.0029	−1.4	0.71
	(0.033)		(3.9)	(1.4)	(0.025)	(0.023)	(3.4)	(0.6)
Assortment Size	−0.0025***	0.00089***		0.078***	−0.0025***	0.00083***		0.07***
	(0.00024)	(0.00027)		(0.0058)	(0.00024)	(0.00027)		(0.0059)
Charge for Shipping	0.057***	−0.0056		3.5***	0.054***	−0.0053		3.3***
	(0.016)	(0.0096)		(0.32)	(0.016)	(0.01)		(0.32)
Retailer Shipping	−0.0038	0.00087	3.1**	−1.5***	−0.01	0.0012	3.3**	−1.4***
	(0.011)	(0.0091)	(1.3)	(0.23)	(0.011)	(0.0091)	(1.3)	(0.23)
Days SKU offered	−0.0041***	−0.0023***			−0.0041***	−0.0026***		
	(0.00056)	(0.00042)			(0.00056)	(0.00044)		
Retailer All Appearances	4.5e−06	−0.000037***	0.013***	0.0084***	0.000017	−0.000031**	0.013***	0.0086***
	(0.000018)	(0.000013)	(0.0024)	(0.00032)	(0.000019)	(0.000013)	(0.0024)	(0.00032)
Number of Market	0.0051	−0.0057	−0.81	−0.075	0.0096*	−0.0082	−0.89	−0.13
	(0.0058)	(0.0058)	(1.3)	(0.13)	(0.0058)	(0.006)	(1.3)	(0.13)
Chat Tool	−0.052***	−0.021**	−1.4	1.2***	−0.05***	−0.022**	−1.2	1.1***
	(0.01)	(0.0096)	(1.9)	(0.26)	(0.01)	(0.0097)	(1.9)	(0.26)
Call Center	−0.04***	−0.014	5.8***	−2.3***	−0.11***	0.003	7.5***	−0.28
	(0.015)	(0.0091)	(1.6)	(0.25)	(0.022)	(0.012)	(1.9)	(0.38)
Showroom	−0.052***	0.016*	−3.1*	−0.17	−0.054***	0.017*	−2.7	−0.2
	(0.0089)	(0.009)	(1.8)	(0.2)	(0.0088)	(0.0091)	(1.9)	(0.2)
Top Retailer	−0.25***	0.015	−6.3***	8.2***	0.014	0.052***	3.3	−0.52*
	(0.028)	(0.014)	(1.7)	(1.3)	(0.01)	(0.013)	(3.9)	(0.29)
Constant	0.42***	0.19***	1.3	16***	0.46***	0.19***	0.24	15***
	(0.02)	(0.015)	(1.6)	(0.33)	(0.022)	(0.015)	(1.5)	(0.36)
R-square	0.16	0.21	0.32	0.37	0.17	0.21	0.32	0.38
N case	7,537	2,720	431	7,537	7,537	2,720	431	7,537
Retailer × SKU FE	+	+	−	+	+	+	−	+
Retailer fixed effects	−	−	+	−	−	−	+	−

channel or excess inventory in the unauthorized channel because of increased prices in the market would not likely be apparent immediately after the policy change, and would more likely to be a long-term process. I therefore mitigate the concern that unauthorized dealers might be indirectly affected by the policy change by shortening the length of the examined period after the policy change. Table A1 in the online appendix reports the estimates of that analysis. Finally, if the policy is effective and authorized retailers raise their prices, in absence of the excess inventory explanation, it is unclear why unauthorized retailers will start lowering their prices even more than before; the more likely change in behavior would be to follow the market and raise prices. In that case, my measured effects are a lower bound on the effect of the policy change on the authorized retailers. Empirically, the results reported below suggest that unauthorized retailers do not increase their violation rates after the policy change; instead, they either do not change their violation rate or they decrease it.

5. Data Analysis

I organize the analysis into three sections. The goal of Section 5.1 is to measure the overall effect of the policy

Table 4. (Continued)

Panel B: Distribution intensity and dual status retailers

	Distribution				Dual status retailer			
	Violation rate	Violation depth	Assortment size	SKU availability	Violation rate	Violation depth	Assortment size	SKU availability
Authorized	−0.044	−0.32***		0.76	−0.15***	−0.00027	2.7	−0.034
	(0.039)	(0.083)		(0.66)	(0.012)	(0.012)	(2)	(0.24)
Post	−0.041	−0.19**		−8.8***	−0.017	−0.033***	−2.3***	−8.9***
	(0.036)	(0.082)		(0.77)	(0.012)	(0.0075)	(0.8)	(0.26)
Authorized × Post	0.0082	0.37***		−1.8*	−0.013	0.021	2.7	−0.11
	(0.046)	(0.088)		(0.95)	(0.013)	(0.013)	(1.8)	(0.36)
Authorized × Post × Yes	−0.057	−0.35***		4.3***				
	(0.048)	(0.089)		(1)				
Post × Yes	0.03	0.16**		−0.22	−0.066***	−0.00099	−0.91	6.2***
	(0.038)	(0.082)		(0.81)	(0.015)	(0.016)	(3.4)	(0.46)
Authorized × Yes	−0.07*	0.33***		−1.8***				
	(0.04)	(0.084)		(0.68)				
Assortment Size	−0.003***	0.00058**		0.074***	−0.003***	0.00069***		0.077***
	(0.00025)	(0.00024)		(0.0057)	(0.00024)	(0.00025)		(0.0056)
Charge for Shipping	0.092***	−0.0084		3.8***	0.095***	−0.0088		3.7***
	(0.016)	(0.0087)		(0.3)	(0.016)	(0.0087)		(0.3)
Retailer Shipping	0.019*	−0.00024		−2***	0.017	−0.00016	2.2*	−1.9***
	(0.011)	(0.0083)		(0.22)	(0.011)	(0.0083)	(1.2)	(0.21)
Days SKU offered	−0.0033***	−0.0021***			−0.0032***	−0.0021***		
	(0.00056)	(0.00039)			(0.00057)	(0.00039)		
Retailer All Appearances	−0.000026	−0.000046***		0.0087***	−0.000026	−0.000045***	0.011***	0.0086***
	(0.000018)	(0.000013)		(0.00031)	(0.000018)	(0.000013)	(0.0023)	(0.00031)
Number of Market	−0.0039	−0.0067		−0.19	−0.0071	−0.0073	0.23	−0.22*
	(0.0057)	(0.0055)		(0.13)	(0.0057)	(0.0055)	(1.2)	(0.13)
Top Retailer	0.39***	0.2***		15***	0.023**	0.065***	3.1	−0.52*
	(0.017)	(0.015)		(0.28)	(0.009)	(0.012)	(3.8)	(0.28)
Dual Status					0.099***	0.013	0.065	−2.5***
					(0.013)	(0.011)	(2.8)	(0.28)
Constant	−0.00034	0.06***		−0.68**	0.39***	0.19***	2.5*	14***
	(0.009)	(0.012)		(0.27)	(0.017)	(0.014)	(1.5)	(0.28)
R-square	0.16	0.2		0.35	0.16	0.19	0.28	0.37
N case	7,910	2,931		7,910	7,910	2,931	487	7,910
Retailer × SKU FE	+	+		+	+	+	−	+
Retailer fixed effects	−	−		−	−	−	+	−

change on the authorized retailers, using a difference-in-differences analysis. I investigate the effect of the policy change on a variety of outcome variables: violation rate, violation depth, assortment, and duration of product availability. I discuss the identifying assumption of parallel trends and provide a series of robustness tests to validate my estimates. In Section 5.2, I then investigate which retailers and SKUs were more likely to be affected, and the role of notification emails. Section 5.3 is exploratory in nature and investigates the effect of the policy change on prices and demand as proxied by inventory ordered and dollars spent by retailers.

5.1. The Effect of the Policy Change: Difference-in-Differences

The identifying assumption for the difference-in-differences analysis is that unauthorized retailers' behavior is a valid counterfactual for authorized retailers' behavior. That is, the trend in behavior of unauthorized retailers is approximately similar to the trend for authorized retailers in the absence of the policy change shock. For the difference-in-differences treatment effect estimate to be valid, a parallel trend between the authorized and unauthorized dependent variable is required. Figure 3 plots the trends for both authorized and unauthorized retailers for the various outcome variables. In panels (A)–(D), the horizontal axis displays the month-year and the vertical lines indicate dates of special interest. The first line is in June 2012, the time the policy change took place, and the second line is in October 2013, when the transition period began before the second policy change. The solid line represents the group of authorized retailers

Table 4. (Continued)

Panel C: Retailers that received test period emails				
	Violation rate	Violation depth	Assortment size	SKU availability
Authorized	−0.12***	0.024**	2.1	−1.3***
	(0.012)	(0.012)	(1.9)	(0.25)
Post	−0.018	−0.031***	−2.3***	−8.9***
	(0.012)	(0.0075)	(0.8)	(0.26)
Authorized × *Post*	−0.033**	0.0062	1.7	2.7***
	(0.014)	(0.014)	(1.6)	(0.39)
Authorized × *Post* × *Email sent for this SKU*	−0.15***	−0.036		−2.7***
	(0.021)	(0.023)		(0.86)
Authorized × *Post* × *Retailer Received Email*	0.0095	0.031*	2	−0.91**
	(0.014)	(0.018)	(4.1)	(0.46)
Email sent for this SKU	0.048**	−0.018		1.3***
	(0.023)	(0.012)		(0.42)
Retailer Received Email	0.024*	−0.028**	4.4	0.6**
	(0.013)	(0.014)	(3)	(0.26)
Assortment Size	−0.003***	0.0008***		0.072***
	(0.00025)	(0.00027)		(0.0058)
Charge for Shipping	0.091***	−0.011		3.8***
	(0.016)	(0.0088)		(0.3)
Retailer Shipping	0.017	0.0063	2.1*	−2***
	(0.011)	(0.0084)	(1.2)	(0.22)
Days SKU offered	−0.0035***	−0.002***		
	(0.00056)	(0.00039)		
Retailer All Appearances	−0.00003*	−0.00004***	0.011***	0.0086***
	(0.000018)	(0.000013)	(0.0023)	(0.00031)
Number of Market	−0.0041	0.001	0.47	−0.29**
	(0.0056)	(0.0054)	(1.2)	(0.13)
Constant	0.4***	0.16***	2.5*	15***
	(0.017)	(0.014)	(1.5)	(0.28)
R-square	0.16	0.18	0.29	0.35
N case	7,910	2,931	487	7,910
SKU FE	+	+	−	+
Retailer fixed effects	−	−	+	−

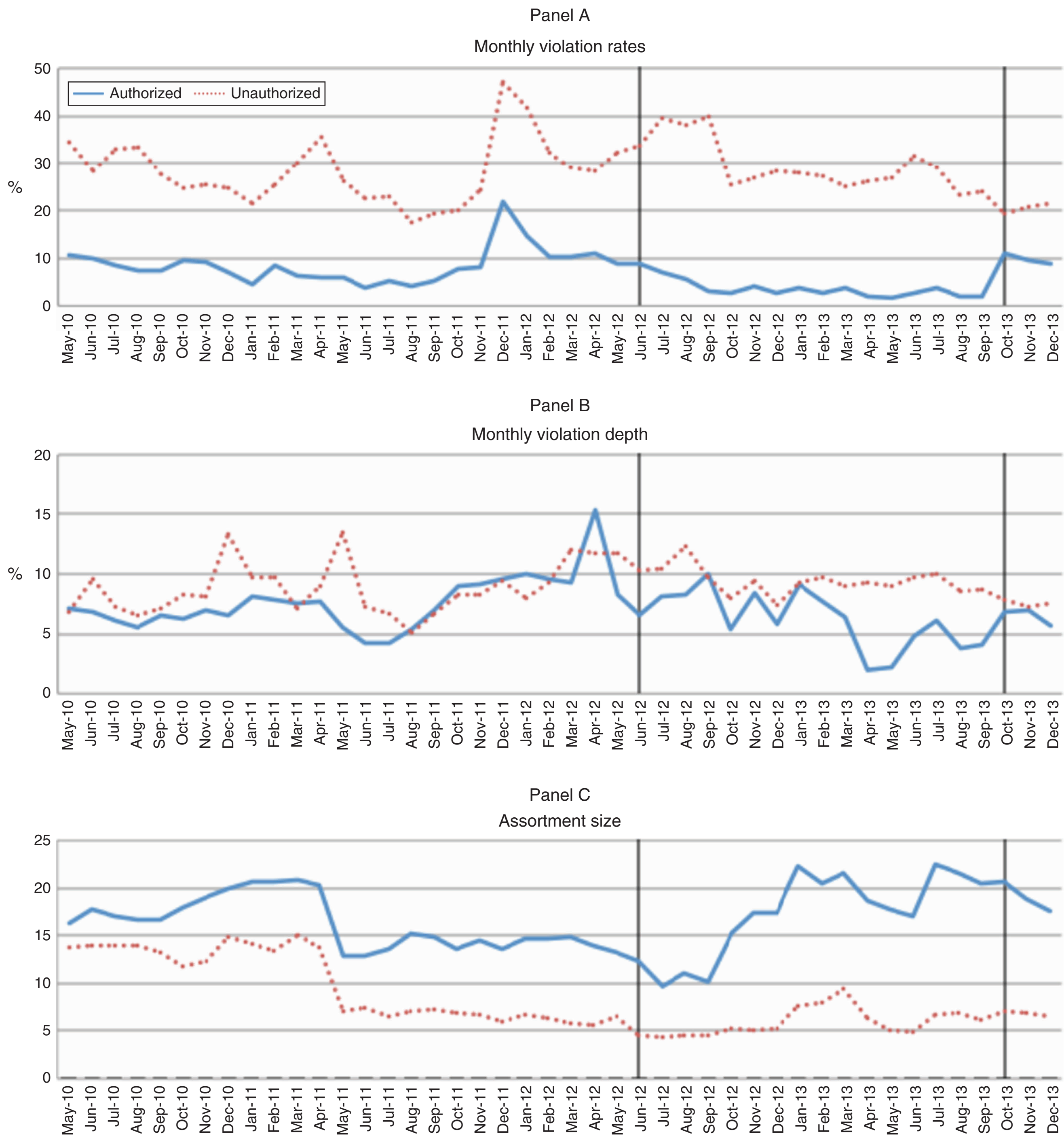
Notes. This table contains the results of Equation (1), where instead of multiple month-year dummies, I have a single "Post" dummy, and instead of the Authorized × Post interaction there are two interactions with a specific characteristic, for four different dependent variables, limiting the sample only to retailers that appear both before and after the policy change took place, while ignoring time series information. I average the various outcome variables before and after the policy change took place (rather than having multiple observations before and after). Panel A presents the results for top sellers and for service providers, panel B presents the results for highly distributed products and for dual status retailers, panel C presents the results for retailers that received emails during the test period. The dependent variables are the average monthly violations rate (columns 1, 5), the average monthly violation depth (columns 2, 6), the average assortment size (columns 3, 7), and the number of appearances of an SKU in a month (columns 4, 8). The subsample for violation depth analysis includes only retailer × SKU × month combinations where a violation occurred. The assortment size analysis is done for a subsample of retailer and month observations. The treatment effect (δ) is the coefficient for the *Authorized* × *Post* × *Characteristic* variable (rows 3 and 4 in Panels A and B, and rows 3–5 in Panel C). The observations in these regressions are restricted to retailers that were observed both before and after the policy change took place. In columns 1, 2, and 4 (and 5, 6, and 8), standard errors are clustered by retailer × SKU, and there are retailer × SKU fixed effects. In column 3 (and 7), standard errors are clustered by retailer, and there are retailer fixed effects. Standard errors are reported in parentheses.

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

and the dotted line represents the group of unauthorized retailers.

For most of the variables of interest, I observe parallel trends prior to June 2012. This can be seen in the chart, and when looking at the coefficient of correlation (R^2) of the regression of the series of the points depicted in the chart on each other. Figure 3, Panel A displays the average monthly violation rates, which seem to be parallel at first, but diverge starting June 2012 ($R^2 = 0.62$ for the data points before June 2012). Panel B displays the average depth of violations, and is limited only to observations where the advertised price

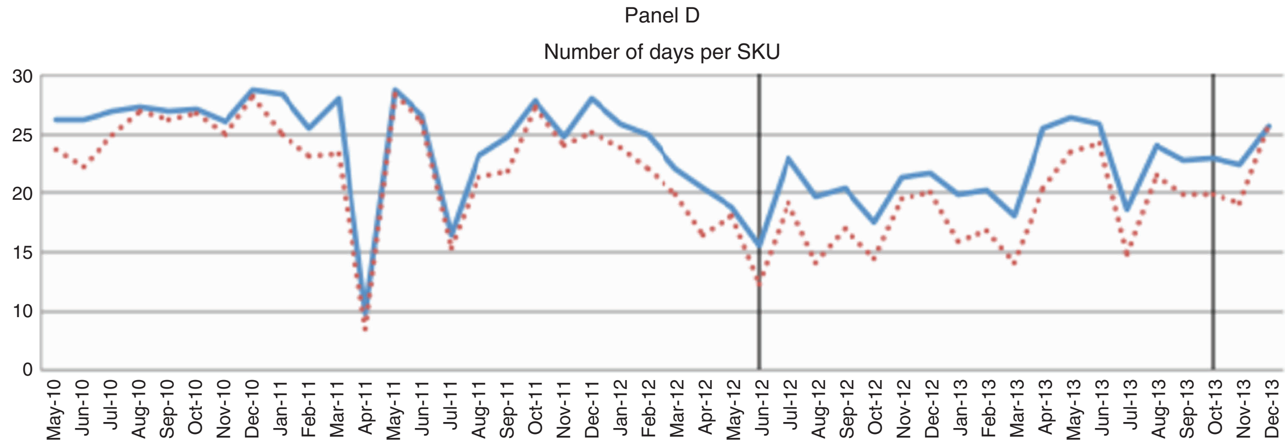
Figure 3. (Color online) Outcome Variables Trends Charts



was below MAP ($R^2 = 0.15$ for the data points before June 2012). Panel C plots the average assortment size for each retailer ($R^2 = 0.76$ for the data points before June 2012), and panel D plots the average number of days (duration) a retailer holds an SKU in a month ($R^2 = 0.92$ for the data points before June 2012).[23] For all of these, I also observe divergence toward the end of the sample. For panel D, I observe some divergence that begins before the policy change, around August 2011. It is hard to tell whether or not the trend is similar, and I investigate it further in Online Appendix A.

Overall, for the outcome variables of interest—violation rates, violation depth, assortment size, and duration of SKU availability—the trends among the authorized and unauthorized retailers seem to move together in a fairly systematic way. I believe that the

Figure 3. (Color online) (Continued)



Notes. The horizontal axis is the date and the vertical axis is the average variable of interest. Each point in the plot indicates the level for that variable in the data. The vertical lines represent the beginning of the policy change and the transition period, respectively. Each graph plots the authorized and unauthorized levels for each of the variables. Panel A presents the average monthly violation rate for the sample, panel B presents the average monthly depth of violations only for observations in violation of MAP, panel C presents the average monthly assortment size, and panel D presents the average number of days an SKU appears in a month.

similarity in trends warrants a difference-in-differences analysis. Therefore, I estimate the following general difference-in-differences model:

$$y_{rsm} = \alpha + \beta\, Authorized_r + \Sigma\gamma_i\, Month_i + \delta\, Authorized_r \times Post_m + \theta\, X_{rsm} + f_s + \epsilon_{rsm}, \quad (1)$$

where the dependent variable, y_{rsm} is either the percentage of violations, the depth of violations, or the number of days the SKU appears, for retailer r, SKU s, and month m. The independent variable $Authorized_r$ indicates whether retailer r is an authorized retailer of the manufacturer. $Month_i$ are dummy variables that indicate the month-year. The interaction $Authorized_r \times Post_m$ indicates whether the month m occurs following the policy change for the authorized group. Control variables X_{rsm} include retailer r's assortment size in month m, an indicator of whether retailer r charged for shipping for SKU s in month m, an indicator of whether or not retailer r charges for shipping, the number of days retailer r offered SKU s in month m, the overall appearance in days of the retailer in the database, and the number of markets in which the retailer r participated. The variable f_s is SKU-level fixed effects. Finally, ϵ_{rsm} is the error term. I cluster the standard errors by retailer × SKU to control for the correlation between retailer's choices over time, following Bertrand et al. (2004), since retailers are likely to make the same choice over time for a specific SKU. The parameter of interest is δ, the treatment effect. I also estimate a retailer month-level version of this model, where the dependent variable y_{rsm} is the assortment size, without SKU fixed effects and without controlling for assortment size. In that model, the standard errors are clustered by retailer. These specifications allow me to measure the treatment effect of the policy change on the authorized retailers within month-year and within SKU, such that the measured effect is not because of month or product differences.

Table 2 presents the results of the difference-in-differences analysis. In this table, the "pre" period is defined as October 2010 to May 2012, and the "post" period is June 2012 to September 2013. For violation rates (columns 1, 2), authorized retailers violate on average 16 percentage points less than unauthorized retailers. The treatment effect of the policy change is a reduction of about four percentage points in violation rates among authorized retailers (p-value = 0.002). Since the unconditional average violation rate among authorized retailers before June 2012 was 8.5%, this finding suggests a reduction of around a 4% monthly average violation rate for an authorized retailer and an SKU. For violation depth (columns 3, 4), there are no systematic differences between authorized and unauthorized retailers. In addition, once controlling for observable characteristics, the treatment effect on the average depth of violations is not statistically different than zero.

As for the assortment size (columns 5, 6), conditional on the additional control variables, there is no statistically significant difference between authorized and unauthorized retailers. The treatment effect suggests an increase of four products for the authorized retailers following the policy change, compared with unauthorized retailers (p-value = 0.016). Last, the duration of SKU availability (columns 7, 8) is on average 0.7 fewer days per month for authorized retailers compared with unauthorized retailers. The treatment effect is an increase of 1.2 days per SKU on average (p-value < 0.001).

Overall, with respect to the direct effect of the policy change on MAP compliance, I found a reduction of about four percentage points in violation rates (a decrease of almost 50% on average), and no effect on the depth of violations. In addition, the policy change seems to increase the availability of a product within a retailer and the assortment size an authorized retailer carries.[24]

For comparison, I also report the results of a regression that limits the sample only to the group of authorized retailers, and compares the outcome variables before and after the policy change for that group (Table A2 in the online appendix). While the estimates for violation rates are consistent with those obtained by the difference-in-differences analysis, estimates for the other outcome variables differ. Violation depths are estimated to decrease by 2.9 percentage points (p-value < 0.001), compared with no significant difference obtained in the difference-in-differences analysis. There is no significant difference in assortment size, compared with an increase of four products in the difference-in-differences analysis. Finally, there is a reduction of 3.7 days in duration compared with an increase of 1.2 days in the difference-in-differences analysis.

To validate my results, I carry out a series of robustness tests, which are detailed in the robustness section in Online Appendix A. I test for sensitivity around the policy change date (Table A3 in the online appendix); vary the definition of the "post" period (Table A4 in the online appendix); run placebo tests (a la Anderson et al. 2010, in Table A5 in the online appendix); verify the group composition (Table A6 in the online appendix) and the SKU composition; compare trends across authorized and unauthorized retailers; construct a dataset that ignores time series information (a la Bertrand et al. 2004, in Table 3); control for additional time invariant characteristics (Table A9 in the online appendix); and address concerns of common support on observables (Table A10 in the online appendix). Overall, I confirm the main results: authorized retailers' violation rates decrease by 40%–80% following the policy change while violation depth is unaffected. In addition, authorized retailers' assortment sizes increase by 3–4 SKUs and the availability of their SKUs increase as well by about one day across specifications.

5.2. Heterogeneity in Response to the Policy Change

After establishing that the policy is indeed effective in reducing violations and potentially improving services, I examine whether the policy differentially affects different retailers. In particular, I examine variation across sales levels, service levels, product popularity, authorized retailers' use of unregistered websites, and prior communication on violations in the test period. To do so, I construct interactions from the type *Authorized* × *Post* × *Characteristic*, *Post* × *Characteristic*, and *Authorized* × *Characteristic* that allow me to examine the treatment effect on the group with the particular characteristic and compare that with the group without that characteristic. I use these in addition to the *Authorized* × *Post* interaction. I use the regressions in columns 1–4 of Table 3 as a baseline for comparison.

5.2.1. Heterogeneity in Retailer and Product Characteristics. I first look at the top retailers, which are defined as those with the highest sales in the industry. These retailers are likely to have higher commitment to the category in general, offer and sell more volume, and may act as industry-building brands. A priori the predictions regarding who will be affected more by the policy are unclear. It could be that top authorized retailers are less threatened by the policy since they do not believe the manufacturer will terminate them because of the volume they carry and their brand value in the industry and thus do not react to the change; or that top retailers now believe that the manufacturer will punish them and thus the policy change will deter them from violating MAP. As for the non-top authorized retailers, since they are less significant and less committed to the industry, they might decide to take the risk and be terminated from the manufacturer's authorized list, or they might be threatened by the new policy and improve their compliance behavior.

Columns 1–4 of panel A of Table 4 report the results for top retailers. Prior to the policy change, there were no statistically significant differences between top and non-top authorized retailers on any of the outcome variables. After the policy change, non-top authorized retailers' violation rates were reduced by 3.8 percentage points (p-value < 0.001), while top authorized retailers' behavior is not differentially affected by the policy change.[25] Top authorized retailers' violation depth increases by 9.5 percentage points after the policy change, while non-top authorized retailers violation depth remains as it was before the policy change.[26] There is no significant difference in the assortment size before and after the policy change for either group. Finally, non-top authorized retailers' SKU availability increased by three days (p-value < 0.001) compared with 2.2 days for top authorized retailers (p-value $<$ 0.001). Overall, there are improvements across both types of retailers—non-top authorized retailers violate less and carry product for a longer time period after the policy change, and top retailers make SKUs available longer. However, top retailers violate to a greater depth after the policy is implemented.

I next examine services. I construct an indicator variable that equals 1 if the retailer provides any service (chat, call center, or showroom) and equals 0 otherwise. The data were collected from January 2011, or June

2012 if that was not available. The results are robust to different definitions of this variable (for example, an indicator for whether 0, 1, 2, or all services are provided). Similar to the top-retailer characteristic, a priori there is no clear prediction of which authorized retailers will be affected more. Those that provide services made more investments and commitments compared with those that did not. The results are reported in panel A of Table 4, columns 5–8. Before the policy change, authorized service providers' violation rates were 7 percentage points higher than those of authorized non-service providers, but there was no significant difference in violation depth, SKU availability, or assortment size. After the policy change, there is no difference in violation rates for either group (authorized service providers reduce violations by two percentage points with p-value = 0.349). There is also no difference in violation depths among these groups. However, there are some changes regarding services after the policy change—those non-service providers authorized retailers also carry more products on average (6.6, p-value = 0.010), and for a longer period of time (5.1 days, p-value < 0.001) after the policy change. At the same time, there are no differences in the behavior of the authorized service providers before and after the policy change.

Overall, I find that the policy change affects those authorized retailers that do not provide services more than those that provide services and causes them to make improvements in certain services. This suggests that the policy affects retailers the manufacturer likely had trouble identifying in the first place because they did not have a physical presence or convenient contact capability, or that otherwise seemed less committed. All else equal, before the policy change, top authorized retailers or those who provided service were more likely to violate MAP than other authorized retailers. Interestingly, both the top authorized retailers and the authorized service providers did not change their rate of violations, but the top authorized retailers group increased the depth of violation after the policy change.

Next, I examine the effect of the policy change on more popular versus more niche products. These results are reported in panel B of Table 4, in columns 1–4. I construct indicators of below and above the median (median = 41) distribution and interact those with the appropriate variables. On one hand, more popular items have higher demand and perhaps retailers do not need to violate MAP to draw consumers to purchase the product; on the other hand, lowering the price might generate more demand to the violating retailer. In terms of monitoring, a product offered by many retailers may be more visible, and if any violation occurs, other retailers may be notifying the manufacturer of violations, whereas violating on niche products might be less observable to other retailers and entail lower risk. I find that after the policy change, there is no difference in violation rates for highly distributed or narrowly distributed products. At the same time, once they violate MAP on the narrowly distributed products, violation depths are higher by 37 percentage points (p-value < 0.001) compared with before the policy change. For highly distributed products, violation depth increases only by 17 percentage points after the policy change (p-value = 0.036). In addition, SKU availability for popular products increases by 2.3 days (p-value < 0.001). For narrowly distributed products, there is a reduction of 1.8 days (p-value = 0.056) in availability.

Taken together, these results suggest that the policy is effective where it matters the most—there are bigger reductions in violation rates among those retailers that have lower overall sales in the product category. Additionally, the policy improves the level of service provided by low service or low sales retailers by increasing SKU availability and the assortment size. Improvements in SKU availability are also observed for highly visible products that are available in many retail outlets, but for these products, violation depths are higher after the policy change. For top retailers and for retailers that provide services, there is no significant reduction in violation rates. At the same time, there is an increase in opportunistic behavior because of the policy among top retailers and for all product categories, by which they exhibit a higher depth of violations after the policy change. Presumably, this is due to the clearer and escalating nature of the punishment structure after the policy change, in line with Gneezy and Rustichini (2000).

5.2.2. Heterogeneity in Past Retailer Behavior. I examine the effect of the policy change on the authorized retailers that appeared to have unauthorized websites before the policy change. As detailed above, the policy required retailers to preapprove all of the domain names that they use as part of the new agreements. While it is unobservable to the company or to the econometrician whether or not retailers have approved all of the domain names through which they sell the product, I attempt to match as many domain names as possible with an authorized retailer. To do so, I examined the names of the websites, physical addresses that were mentioned, logos of the retailer brand, imagery, and any other indicator of the source of the retailer. Based on this information, I updated the indicator of whether or not a retailer is authorized. Importantly, I updated the authorization status for retailers such that throughout the entire data analysis a retailer has the same status as they do at the end of the data period. However, I do observe whether a website was presumed to be owned by an unauthorized retailer before the policy change. I use that information to construct an indicator on whether or not a retailer had more than one authorization status before the policy change. I name these retailers "dual status retailers," and examine whether the policy differentially affected these retailers.

The results are reported in Table 4, panel B, columns 5–8. Note that only authorized retailers could be dual status retailers. Before the policy change, dual status retailers' violation rates were higher by 10 percentage points compared to nondual status authorized retailers, and carried products for 2.5 fewer days. After the policy change, the dual status retailers reduce their violations by 6.6 percentage points, and increase the number of days of SKU availability by 6.2 days. At the same time there is no difference in any of the outcome variables for nondual status authorized retailers. This suggests that the policy was effective in addressing the challenge of the online environment that it is harder to track which website belongs to which retailer. One potential reason for this finding is that the increase in credibility of monitoring and enforcement as well as the explicit request to preapprove websites may have convinced these retailers that their behavior is being watched.

5.2.3. Heterogeneity in Past Retailer and Manufacturer Interactions. I examine the effect of the policy change on the retailers that violated MAP in the test period, and received notification emails from the manufacturer. Since retailers receive emails for a specific product violation, I create two variables, one indicating whether the retailer received an email during the test period, and one indicating whether they received an email for this particular SKU during the test period. The results are reported in panel C of Table 4. First, note that, by construction, before the policy change, authorized retailers that received notification emails were more likely to violate MAP than those that did not receive emails only for the SKUs for which they received an email. Authorized retailers that did not receive emails in the test period reduced their violations by 3.3 percentage points (p-value = 0.018) following the policy change. At the same time, retailers that received emails for other SKUs during the test period do not significantly reduce their violation rate (−2.3 percentage points, p-value = 0.144), and those that received emails for violation of a particular SKU reduce violations for that specific SKU by 17.4 percentage points (p-value < 0.001). In addition, while retailers that received an email for other SKUs increase the depth of violations by 3.8 percentage points, those that received an email for this particular SKU and those that did not receive any emails in the test period do not reduce the depth of their violations once they violate. Finally, while retailers that received an email for this particular SKU did not change its availability, those that received emails for other SKUs increased the availability by 1.8 days, and those that did not receive any emails in the test period increased their availability by 2.7 days.

These results show that notifications during the test period, before there were any changes in the policies and agreement, had a lasting effect on retailers. The effects are particularly stark on those SKUs for which retailers received notifications. Perhaps these retailers believed that those SKUs are being more tightly monitored and therefore modified their behavior on these SKUs to stay under the radar. Note that retailers that received emails during the test period may be more likely to violate MAP in the first place, and the fact that the initial notification emails had a differential effect on their behavior suggests some learning for these particular SKUs.

Finally, changes above and beyond sending an email were required, as retailers improve their violation behavior after the policy change, whether they previously received a notification email or not. Furthermore, the significant reduction in overall violation rate and improvement in service variables occurs only after the policy change (see the discussion in the online appendix and Table A4). Online Appendix B further explores the effect of the notification emails both before and after the policy change, and also concludes that those emails are more effective once the terms of engagement were changed as well via the new policy.

5.3. The Effect on Manufacturer's Profit: An Exploratory Comparison

In this section I examine whether MAP enforcement affects dollars spent or quantity ordered from the manufacturer. One of the reasons manufacturers avoid MAP is the concern of lower demand and dampened profits. While MAP is used to protect retailer margin and allow inclusion of more retailers into the market, it may deter other retailers from selling the manufacturer's products. To test the effect on quantity and expenditure, I obtained the manufacturer's detailed sales report that includes the quantity and dollars spent for all retailer orders between July 2002 and December 2013. I investigate the effect of the policy change in June 2012 on retailer purchase behavior using the data through September 2013. This analysis is detailed in Online Appendix C.

I find no evidence of a negative impact on quantity ordered or dollars spent. Therefore, I could not reject the null that a change in MAP policy has no impact on retailers' ordering behavior. Moreover, the point estimates of these coefficients are economically small and not meaningful. Although not statistically significant, the nonnegative coefficient is consistent with the notion that a well-governed MAP policy is a desired outcome for both manufacturers and retailers.

I also measure the effect of the policy change on price. As MAP violations decrease, I expect average prices in the channel to increase. To assess the increase in prices, I estimate a linear regression model of the percentage change in average monthly prices after the policy change, for retailer × SKU combinations that were observed both before and after the policy change

$$\%\Delta\, Average\ Price_{rs} = \alpha + \beta\, Authorized_r + \theta X_{rs} + f_s + \epsilon_{rs}, \tag{2}$$

where $\%\Delta Average\ Price$ is the percentage change in average monthly prices of retailer r for SKU s in the period after the policy changed compared with the period before. $Authorized_r$ indicates whether retailer r is an authorized retailer of the manufacturer. Control variables X_{rsm} include retailer r's average assortment size, an indicator whether or not retailer r charges for shipping, the overall appearance in days of the retailer in the database, and the number of markets the retailer r participated in. The f_s are SKU level fixed effects. The ϵ_{rst} is the error term. I compute robust standard errors. The coefficient of interest is β that measures the average change in prices for authorized retailers due to the policy change, within SKUs.

I observe an increase of 2% in average prices among authorized retailers because of the increased compliance with MAP (reported in Table 5). Even though the prices are higher, there is no evidence of an impact of MAP on quantity ordered. With regard to cost, the manufacturer paid Channel IQ for their tracking services both before and after the policy change. The additional direct costs of enforcement are mainly the time the firm spent verifying the violations before sending out emails and following up with punishments, which amounts to about an hour per week.

Table 5. Change in Prices After the Policy Change

	%ΔAveragePrice			
	(1)	(2)	(3)	(4)
Authorized	0.027***	0.025***	0.018**	0.019***
	(0.006)	(0.006)	(0.0071)	(0.0072)
Assortment Size			0.00086***	0.00065***
			(0.00025)	(0.00022)
Charge for Shipping			0.01	0.017**
			(0.0068)	(0.0068)
Retailer Appearances			−3.4e−06	−5.4e−06
			(8.6e−06)	(8.6e−06)
Number of Market			−0.0022	−0.00021
			(0.0052)	(0.0049)
Constant	0.023***	0.024***	0.0064	0.0055
	(0.0041)	(0.0043)	(0.0081)	(0.0082)
R-square	0.0071	0.17	0.02	0.18
N cases	2,542	2,542	2,542	2,542
SKU fixed effect	−	+	−	+

Notes. This table contains the results of Equation (2), where the dependent variable is the average change in prices for a retailer and SKU following the policy change. The observations in these regressions are restricted to retailers and SKU combinations that were observed both before and after the policy change took place. In columns 2 and 4, I control for SKU fixed effects. I compute robust standard errors. Standard errors are reported in parentheses.

$^{*}p < 0.10$; $^{**}p < 0.05$; $^{***}p < 0.01$.

6. Conclusion

In this paper I investigate a manufacturer's ability to influence compliance rates among authorized retailers in the online channel by exploiting changes in the MAP policy and in dealer agreements. I demonstrate that initial investments in monitoring and enforcement may be insufficient to achieve compliance with MAP. Effective governance of MAP may also require additional changes in channel policies and agreements. In particular, I discuss two key elements of successful channel policies: customizing the policies to the online retail environment, and improving the credibility of the monitoring and punishment. Addressing the challenges of the online retail environment by customizing the procedures to that environment reduces adverse selection concerns, and credible threats reduce moral hazard among opportunistic retailers.

Specifically, the manufacturer examined in this analysis separated the e-commerce agreement from its main dealer agreement, and required e-commerce dealers to preapprove the domain names through which they offer the manufacturer's products. These particular changes directly addressed the challenges of the online environment, and increased channel transparency through informing the manufacturer of the retailers' online presence. The MAP policy was modified to include a detailed explanation of the consequences of violations, a three-strike policy, and added the provision of warning emails. The new policy created a credible commitment on the manufacturer's behalf and enhanced the credibility of the punishment even though the same final punishment of termination was employed in the original policy. Notably, the manufacturer further increased the certainty and credibility of enforcement actions by following up on the policy and terminating two authorized online retailers six months after the policy change.

To illustrate these points, I analyze a quasi-experiment prompted by a manufacturer's change in channel policies. I exploit the fact that manufacturers can only intervene and have legitimate power over the authorized channel to employ a difference-in-differences approach. I find that authorized retailers reduce their violation rates by 40%–80% following the policy change. This effect is robust to a variety of tests and specifications. In addition, authorized retailers' assortment sizes increase (by four SKUs on average) and the availability of their SKUs increase as well (by 1.2 days on average). Interestingly, the reductions in violation rates diminish once the manufacturer halts the email notification system. While average prices increase by 2% among authorized retailers because of the policy change, my preliminary analysis finds no evidence of a change in quantities ordered by retailers following the introduction of the updated agreements and policies. While the overall effect is a reduction in violation rates, and no effect on the depth of violations, I find differential effects by retailers and product characteristics. The reduction in violations is particularly stark among authorized retailers with lower total sales, those that previously operated unauthorized websites, and those that previously received a notification for a particular SKU. There is also evidence that low service providers try to improve elements of service after the policy change. On the other hand, the depth of violations is higher after the policy change among top retailers, retailers that received notification emails for other SKUs, and for narrowly distributed products, while their violation rates remain unaffected.

A limitation of my study is that the manufacturer made several changes simultaneously, which prevents me from being able to separately identify the effects of different factors that influence MAP violation rates. I show that emails during the test period were less effective in changing retailers' behavior than the combination of emails with the policy change. In the online appendix, I attempt to isolate the effect of the email notifications by investigation of violation rates in the days before and after a notification was sent. I find that within a week of the notification, violations drop by more than 50% among the authorized retailers that received an email. Within three weeks of the notification, violation rates in this group reduce to the level of other authorized retailers in the market. This effect of the notification persists for at least four weeks following the notification. I attribute the sustained effectiveness of these enforcement emails to the policy change.

While this research is based on data from a single manufacturer and is limited to the actions of this manufacturer, the findings suggest that other manufacturers also have the ability to effectively intervene and reduce violation rates within their authorized channel. As for the unauthorized channel, the prevalence of such retailers in distribution channels remains a problem for manufacturers, and further research is warranted to resolve it.

My findings are generalizable to other policies and contracts. The manufacturer's ability to effectively improve compliance may also be extended to other policies and contracts on partners' observable actions in which the manufacturer has a technology to monitor and measure partners' behavior. In such cases, the design of the contract, incentives, and punishments should be such that there is transparency of partners' actions, and that the monitoring and enforcement seem credible to these partners. Several digital copyright enforcement policies contain a three-strikes enforcement protocol (e.g., YouTube, antipirating laws), and this study suggests these are more effective than a general termination threat. Finally, when customizing policies from one environment to another, one should consider all of the implications of the environment change in order for the policy to still be effective. For example, implementation of exclusive territory restrictions in an online channel, or implementation of pricing and copyrights programs in digital compared to physical media, require customization to the new environment.

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Endnotes

[1] See the discussion in Section 2.

[2] I arrived at this conclusion after collecting more than 460 publicly posted online MAP policies and conducting an extensive online search that included press releases, articles, and industry forums.

[3] See Israeli et al. (2016) for an overview of the literature on RPM. The vast majority of the literature on RPM is theoretical. There are two published empirical studies that examine lawsuits regarding price fixing, and one that examines whether RPM prohibition affects video game prices (though the authors do not observe whether the video games were subject to RPM to begin with).

[4] Credible threats have been explored in the academic literature in sociology, law, economics, and marketing. In particular, the literature discusses the importance of a threat's certainty on enforcement (Becker 1968, Stigler 1970, Antia et al. 2006). However, I demonstrate that the same punishment becomes more credible and certain once channel policies and agreements are updated.

[5] At a marginal cost, compared with the cost of opening an additional brick-and-mortar location, for example.

[6] A policy is unilateral and imposed by the manufacturer; an agreement is bilateral and agreed on by all parties. For brevity, I use the term "policy change" when referring to changes in both policies and agreements.

[7] I study changes over time, rather than cross-sectional variation in contemporaneous MAP policies. There cannot be authorized retailers' control and treatment groups in a single period of time, each with a different policy.

[8] A self-enforcing agreement was first modeled and analyzed by Telser (1980). Klein and Murphy (1988) make a similar argument, that optimally compensating retailers using vertical restraints is an effective enforcement mechanism.

[9] The experiment was conducted in Hewlett-Packard laboratories, to examine specifications for their MAP policy. Participants were grad students, and market reactions and outcomes were simulated in the lab.

[10] While unauthorized retailers are not violating a MAP policy, since the policy does not apply to them, I also use the term violations for any case where a price is advertised below MAP by unauthorized retailers.

[11] For example, how to address infringing content on YouTube, or intellectual property infringement on selling platforms such as eBay and Amazon. Note, for example, that the current YouTube copyright infringement policy includes a three-strikes protocol for violators.

[12] In the timeframe studied in this paper, close to 20% of the manufacturer's business is estimated to be sold in the online channel.

[13] For details, see http://pages.ebay.com/help/community/vero-aboutme.html.

[14] The MAP policy for this manufacturer is confidential as well. Yet the differences in the way the policy was stated are similar to the examples in Figure 2.

[15] For confidentiality reasons, I cannot reveal the identity of the manufacturer or the industry in which they operate. The data on sales ranking are from the appropriate trade literature.

[16] The original data set (2,132,043 observations) may contain more than one observation from the same retailer, SKU, and market for a single day, due to the Channel IQ data collection process. To balance the data, I collapse these observations into a single observation for a retailer, SKU, and market, selecting the lowest documented price for each day. For this manufacturer, over 92% of the retailers sell a certain SKU in a single outlet. Therefore, I collapse each daily observation into a retailer × SKU observation, again maintaining the observation with the lowest advertised price.

[17] Since not all retailers and SKU combinations are observed daily, I find a monthly data set to be more balanced and representative of the behavior in the market.

[18] These data were collected in September 2015 and in June 2016, using the Internet Archive, the Wayback Machine. Data are available for 80% of the retailers, which accounts for 95% of the observations. Missing data was a result of a lack of archive information and some websites were removed by 2015.

[19] I obtained the relevant top retailers list from the relevant trade publications.

[20] For example, Amazon.com prides itself on having the widest and broadest assortment, which leads to convenience, "a one stop shop," thus better service.

[21] I study changes over time, rather than cross-sectional variation in contemporaneous MAP policies. There cannot be authorized retailers control and treatment groups in a single period of time, each with a different policy.

[22] See Levy (2016) on Birkenstock's announcement that they would stop supplying products to Amazon.com.

[23] The duration of an SKU's availability may indicate how large the inventory the retailer carries is and whether it runs out of product.

[24] The results reported in this paper were obtained using a linear regression specification for the four outcome variables. Since violation rates reflect proportions with mass at zero (no violations) and one (always violations), I also estimate the main results of this paper using an appropriate zero-one inflated beta specification. The regression reveals that the treatment effect is a reduction of about 15 percentage points (p-value < 0.001) in violation rates for observations with a proportion smaller than or equal to 1 and no statistically significant change for observations with proportion 0. This is consistent with the average reduction in violation rates reported in the main results of the paper.

[25] This result is achieved by summing the differential coefficients for this group after the policy change: *Authorized* × *Post*, *Authorized* × *Post* × *Yes*, and *Post* × *Yes*, and computing the appropriate standard errors.

[26] Note that the top retailers who are unauthorized do not violate, and hence there are omitted variables in this regression.

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