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, retailers that received notifications for other products, and for less popular products via deep discounting.

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 the 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 focused primarily 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 and 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

[1] See discussion in the literature review section.

of heterogeneous retailers. Since MAP policies are typically confidential, I can only estimate, conservatively, that at least 590 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 pre-approve all retail websites; a new three-strike punishment protocol; higher specificity of the punishments upon 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 non-price 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). Further, 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, Anderson, and Coughlan 2016). Retailers violate MAP even though they sign authorized dealer agreements to follow the manufacturer's

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

[3] See Israeli, Anderson, Coughlan 2017 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).

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 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.

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[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.

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. But, 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 revises its channel agreements and policies in June 2012, and it requires its authorized retailers to re-sign 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 ecommerce agreement, distinct from the authorized dealer agreement. The ecommerce agreement required its retailers to go through an additional registration procedure to become authorized ecommerce 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.

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

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 timeframe. 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 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. In 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-

[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.

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 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 is 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 reduction in volume ordered from the manufacturer or dollars spent. Additionally, some evidence suggests 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), which takes an analytical approach, modeling MAP as an extension of Resale Price Maintenance (RPM), which can be used to legally maximize channel profits. Hence, Kali 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 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

[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 (HP) laboratories, to examine specifications for their MAP policy. Participants were grad students, and market reactions and outcomes were simulated in the lab.

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, Anderson, and Coughlan 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, in order 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. Further, consumers of that content may not even be aware that any laws were violated. Danaher, Smith, Telang, and Chen (2014), Adermon and Liang (2014), and Reimers (2016) find that new anti-piracy 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. In 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 post-violations 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.

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[10] While unauthorized retailers are not violating a MAP policy, since the policy does not apply to them, I use the term violations also 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.

The literature on digital enforcement relies heavily on crime and punishment deterrence research (Becker 1968; Stigler 1970; and others) and on relational contracts between principals and agents (Alchian and Demsetz 1972; Jensen and Meckling 1976; and others). 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 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 strike 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 (Shepard 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 (2017) 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 the 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 protection by imposing a lower bound on the advertised price. Due to the online environment and the fact that the policy requirement is about pricing which 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 (Antia and Frazier 2001; Antia, Bergen, Dutta, and Fisher 2006; Bergen, Heide, and Dutta 1998; Dutta, Bergen, and John 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 (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, Wathne, and Rokkan 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.

Finally, 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, Amir, and Ariely (2008), the mere reminder of compliance standards can decrease subsequent violation behavior.

To the best of 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 is available, it is challenging to form empirical inference due to 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. The first subsection describes the state of the industry and the manufacturer's policy change, which is the treatment evaluated in the paper. The second subsection 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 upon 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 recent years, 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

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

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 revises its agreement and policies and has its authorized dealers sign updated agreements (see Exhibit 1 for a timeline of the policy changes). The policy change includes two major components: a new dealer agreement with a standalone ecommerce 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 ecommerce 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.

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 ecommerce retailers to register their URLs and have them approved. These steps reduce information asymmetry and allow the manufacturer to segment ecommerce 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 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. There doesn't seem to be a single best practice in the durable goods industry overall, however. Firms vary in whether they use vague or specific description of the consequences of violations. A specific description usually defines the exact steps a manufacturer would take upon detecting a violation. Of 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. Exhibit 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 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 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. Exhibit 1 clarifies the timeline further.

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

3.2 Data description and Summary Statistics

The data for this study is 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 is unique because MAP policies are often confidential, and it is rare to observe communication between 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 top 20 in the world. The database contains 1,933,073 daily retailer X SKU observations, which include the price that was documented for that retailer X 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 X SKU X 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 X 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 a 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 2 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

[14] The MAP policy for this manufacturer is confidential as well. But the differences in the way the policy was stated are similar to the examples in Exhibit 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 is from the appropriate trade literature.
[16] The original dataset (2,132,043 observations) may contain more than one observation from the same retailer, SKU and market for a single day, due to 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 X 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 dataset to be more balanced and representative of the behavior in the market.

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 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. 57.5% of the observations are of authorized retailers.

I also compute for each retailer and SKU the number of days in a month the SKU appeared in the database (22.6 days on average, 23.4 for authorized and 21.5 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); 8% of retailers provide an online chat 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.

[18] This data was collected in September 2015 and in June 2016, using the Internet Archive the Wayback Machine. Data is available for 80% of retailers, which accounts for 95% of the observations. Missing data was a result of lack of archive information and since some websites have been removed by 2015.
[19] I obtain the relevant top retailers list from the relevant trade publications.

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 that 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. Lastly, 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 due to the policy change. Online, service can manifest itself by offering a larger assortment size or having a 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. Further, manufacturers must treat all their authorized retailers uniformly, and 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 with other unobservable changes in the market, such as

[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.

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

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

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 in a difference-in-differences setting. I show that the trend in unauthorized retailers is approximately similar to the trend in authorized retailers in absence of the policy change shock, which allows me to use the difference-in-difference 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 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 indirectly have an 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. Further, 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 channel or excess inventory in the unauthorized channel due to 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. Online Appendix Table A1 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 in Tables 3 and 4 below suggest that unauthorized retailers do not increase their violation rates after the policy change; instead, they either don't change their violation rate or they decrease it.

5. Data Analysis

I organize the analysis into two subsections. The goal of the first subsection is to measure the overall effect of the policy 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. I then investigate which retailers and SKUs were more likely to be affected, and the role of notification emails. The second subsection 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 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. Exhibit 3 plots the trends for both authorized and unauthorized retailers for the various outcome variables. Throughout the panels, the horizontal axis displays the month-year. 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 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. Panel A of Exhibit 3 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 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 a 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 the robustness section.

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

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

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 $Authorized_r xPost_m$ indicates whether month *m* occurs following

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

the policy change for the authorized group. X_{rsm} are control variables that 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 are SKU-level fixed effects. Finally, ϵ_{rsm} is the error term. I cluster the standard errors by retailer x 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 due to 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 4 percentage points in violation rates among authorized retailers (p-val=0.002). Since the unconditional average violation rate among authorized retailers before June 2012 was 8.5%, this finding suggests a reduction to around 4% monthly average violation rate for an authorized retailer and a 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 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 4 products for the authorized retailers following the policy change, compared with unauthorized retailers (p-val=0.016). Lastly, 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-val<0.001).

Overall, with respect to the direct effect of the policy change on MAP compliance, I find a reduction of about 4 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-val<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 4 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, Fong, Simester, and Tucker [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, Duflo, and Mullainathan [2004], in Table 3); control for additional time invariant characteristics (Table A9 in the Online Appendix);

[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 proportion smaller 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.

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 1 day across specifications.

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: $AuthorizedxPostxCharacteristic$, $PostxCharacteristic$, and $AuthorizedxCharacteristic$ 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 $AuthorizedxPost$ interaction. I use the regressions in columns 1-4 of Table 3 as a baseline for comparison.

Heterogeneity in Retailer and Product Characteristics

I first look at 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 don't believe the manufacturer will terminate them due to 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, top authorized retailers' violation rates are higher than non-top authorized retailers by 17 percentage points, but the depth of violations does not differ between the two groups. Regarding SKUs, top authorized retailers were likely to carry 9.7 SKUs more than authorized non-top retailers before the policy change, and SKU availability was 8 days shorter on average. After the policy change, non-top authorized retailers' violation rates reduce by 3.8 percentage points (p-val<0.001), while top authorized retailers' behavior is not differentially affected by the policy change.[25] Top authorized retailers' violation depth increases by 8.6 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 groups. Finally, top authorized retailers' SKU availability increased by 3 days (p-val<0.001) compared with 2.6 days for non-top authorized retailers (p-val<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 was collected from January 2011, or June 2012 if that wasn't 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 21 percentage points higher than those of authorized non-service providers, but there was no significant

[25] This result is achieved by summing the differential coefficients for this group after the policy change: *AuthorizedXPost*, *AuthorizedXpostXYes*, and *PostXYes*, and computing the appropriate standard errors.
[26] Note that top retailers who are unauthorized do not violate, and hence there are omitted variables in this regression.

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 2 percentage point with p-val=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-val=0.010), and for a longer period of time (5.1 days, p-val<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 the 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. Before the policy change, highly distributed SKUs sold by authorized retailers had marginally lower violation rates (by 7 percentage points, p-val=0.077), greater extent of violations compared with narrowly distributed SKUs sold by authorized retailers (by 33 percentage points, p-val<0.001), and those highly distributed products were available for a shorter period of time (by 1.8 days, p-val=0.008). 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-val<0.001) compared with before the policy change. For highly distributed products, violation depth increase only by 17 percentage point after the policy change (p-val=0.036). In addition, SKU availability for popular products increases by 2.3 days (p-val<0.001). For narrowly distributed products, there is a reduction of 1.8 days (p-val=0.056) in availability.

Put together, these results suggest that the policy is effective where it matters 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 due to the policy among top retailers and for all product categories, by which they exhibit 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).

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 pre-approve all 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 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 that, 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 non-dual 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 non-dual 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.

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-val=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 violations rate (-2.3 percentage points, p-val=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-val<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 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. Further, the significant reduction in overall violation rate and improvement in service variables occurs only after the policy change (see 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.

5.2 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 non-negative 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 X SKU combinations that were observed both before and after the policy change:

$$\%\Delta\ Average\ Price_{rs} = \alpha + \beta\ Authorized_r + \theta\ X_{rs} + f_s + \epsilon_{rs} \qquad (3)$$

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. X_{rsm} are control variables that 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 due to 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.

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 ecommerce agreement from its main dealer agreement, and required ecommerce 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 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 6 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 4 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 due to 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 in order 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, anti-pirating laws), and this study suggests these are more effective than a general termination threat. Finally, when customizing policies from one environment to the other, one should consider all 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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TABLES

Table 1 – Summary Statistics

Panel A: Sample Level Characteristics

Variable	mean	median	sd	min	max	N	mean	sd	mean	sd
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

Variable	mean	median	sd	min	max	N	mean	sd	mean	sd
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	1311	517	795	382	227	278
# 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

Each observation is a R*etailer x SKU x 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 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. Charge for shipping is true if there was a shipping charge for the ***SKU x retailer x month*** combination. SKU availability indicates the number of days in a month in which the retailer offered the SKU. Violation Rate indicates how often the retailer advertised a price below MAP for the specific SKU during the month. 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 non-platform websites).

Table 2 - The Effect of Manufacturer Policy changes: difference-in-differences Analysis

	Violation Rate		Violation Depth		Assortment Size		SKU Availability	
Authorized	-.19***	-.16***	.00074	.0062	6.5***	2.6	1.8***	-.68***
	(.0095)	(.012)	(.0045)	(.0052)	(.37)	(1.6)	(.13)	(.14)
Authorized x Post	-.066***	-.041***	-.025***	-.0092	5.1***	4**	1***	1.2***
	(.014)	(.014)	(.0068)	(.0068)	(.7)	(1.7)	(.27)	(.26)
Assortment Size		-.0011***		-.0005***				.0019
		(.00022)		(.00009)				(.003)
Charge for Shipping		.072***		.0041				2.9***
		(.011)		(.0046)				(.15)
Retailer Shipping		-.0069		.018***		.58		-1.4***
		(.01)		(.0045)		(1.1)		(.14)
Days SKU offered		-.0031***		-.0013***				
		(.00022)		(.00013)				
Retailer all Appearances		-6e-05***		-2e-05***		.009***		.0074***
		(.00002)		(6.3e-06)		(.0021)		(.0002)
Number of Markets		.005		.0027		.15		-.62***
		(.0051)		(.0032)		(1)		(.094)
Constant	.31***	.43***	.056***	.1***	12***	7***	24***	21***
	(.011)	(.013)	(.0037)	(.0068)	(1.3)	(1.8)	(.22)	(.26)
R-squared	.13	.15	.12	.14	.15	.21	.23	.28
N cases	80064	80064	21337	21337	7187	7187	80064	80064
SKU Fixed Effects	+	+	+	+	-	-	+	+
Month-Year FE	+	+	+	+	+	+	+	+

* p<0.10, ** p<0.05, *** p<0.01

Table 2 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 a SKU in a month (columns 7-8). The subsample for violation depth analysis includes only retailer X SKU X 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 x Post* variable (row 2). In columns 1-4 and 7-8, standard errors are clustered by retailer X SKU, and there are SKU fixed effects. In columns 5-6, standard errors are clustered by retailer.

Table 3 - Robustness: Ignoring Time Series Information

	Retailer Composition				Retailer X SKU Composition			
	Violation Rate	Violation Depth	Assortment Size	SKU Availability	Violation Rate	Violation Depth	Assortment Size	SKU Availability
Authorized	-.11***	.011	2.9	-.98***	-.15***	.0049	2.9	-1.8***
	(.012)	(.0099)	(1.9)	(.22)	(.016)	(.016)	(1.9)	(.28)
Post	-.018	-.032***	-2.3***	-8.9***	-.031**	-.02***	-2.3***	-9.2***
	(.012)	(.0075)	(.8)	(.26)	(.013)	(.0063)	(.8)	(.34)
Authorized x Post	-.039***	.02*	2.3	2.1***	-.026*	.0027	2.3	2.6***
	(.013)	(.011)	(1.7)	(.34)	(.013)	(.011)	(1.7)	(.43)
Assortment Size	-.0029***	.00067***		.072***	-.0018***	.00071*		.065***
	(.00024)	(.00026)		(.0057)	(.00034)	(.0004)		(.0077)
Charge for Shipping	.093***	-.011		3.8***	.11***	.022		4.1***
	(.016)	(.0088)		(.3)	(.021)	(.014)		(.38)
Retailer Shipping	.018*	.0031	2.3**	-2***	-.0047	-.0087	2.3**	-1.5***
	(.011)	(.0082)	(1.2)	(.22)	(.015)	(.011)	(1.2)	(.29)
Days SKU offered	-.0034***	-.002***			-.0027***	-.001**		
	(.00055)	(.0004)			(.00067)	(.00046)		
Retailer all Appearances	-.00003	-.00004***	.012***	.0086***	-.00007***	-.00003	.012***	.0094***
	(.00002)	(.00001)	(.0023)	(.0003)	(.00002)	(.00002)	(.0023)	(.0004)
Number of Markets	-.0038	.0012	.57	-.28**	.013*	.015	.57	-.12
	(.0056)	(.0054)	(1.2)	(.13)	(.0076)	(.01)	(1.2)	(.16)
Constant	.39***	.17***	2	15***	.4***	.11***	2	14***
	(.016)	(.014)	(1.4)	(.27)	(.021)	(.018)	(1.4)	(.34)
R-squared	.16	.18	.28	.35	.17	.12	.28	.38
N cases	7910	2931	487	7910	5106	1422	487	5106
SKU Fixed Effects	+	+	-	+	+	+	-	+

* p<0.10, ** p<0.05, *** p<0.01

Table 3 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 (column 1,5), the average monthly violation depth (column 2,6), the average assortment size (column 3,7), and the number of appearances of a SKU in a month (column 4,8). In columns 1-4 I use any SKU for a retailer that appeared both before and after the policy change. In 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 sub samples. The subsample for violation depth analysis includes only retailer X SKU X month combinations where a violation occurred (and is limited only to retailer X 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 x 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 5,6,8), standard errors are clustered by retailer X SKU, and there are SKU fixed effects. In column 3 (and 7), standard errors are clustered by retailer.

Table 4 - Heterogeneity in response to the policy change

Panel A: Top Sellers and Service Providers

	Top Sellers				**Service Providers**			
	Violation Rate	**Violation Depth**	**Assortment Size**	**SKU Availability**	**Violation Rate**	**Violation Depth**	**Assortment Size**	**SKU Availability**
Authorized	-.1***	.011	1.3	-.98***	-.28***	-.0022	1.9	-2***
	(.012)	(.01)	(2.1)	(.24)	(.023)	(.021)	(2.9)	(.58)
Post	-.032***	-.047***	-2.5***	-9.6***	-.047**	-.022**	-.97	-6.9***
	(.012)	(.0084)	(.95)	(.26)	(.02)	(.011)	(.9)	(.46)
Authorized x Post	-.038***	.0084	2.5	3***	.037	.012	6.6***	5.1***
	(.013)	(.012)	(2.1)	(.38)	(.024)	(.031)	(2.5)	(.71)
Authorized x Post x Yes	.023	.086***	-.23	-3.3**	-.08***	.04	-3.3	-1.5*
	(.035)	(.02)	(2.8)	(1.4)	(.029)	(.033)	(3.4)	(.82)
Post x Yes	.011		1.1	2.5*	.023	-.047***	-2.9	-4.1***
	(.032)		(.95)	(1.3)	(.024)	(.016)	(1.8)	(.57)
Authorized x Yes	.27***		9.7**	-8.4***	.21***	-.0029	-1.4	.71
	(.033)		(3.9)	(1.4)	(.025)	(.023)	(3.4)	(.6)
Assortment Size	-.0025***	.00089***		.078***	-.0025***	.00083***		.07***
	(.00024)	(.00027)		(.0058)	(.00024)	(.00027)		(.0059)
Charge for Shipping	.057***	-.0056		3.5***	.054***	-.0053		3.3***
	(.016)	(.0096)		(.32)	(.016)	(.01)		(.32)
Retailer Shipping	-.0038	.00087	3.1**	-1.5***	-.01	.0012	3.3**	-1.4***
	(.011)	(.0091)	(1.3)	(.23)	(.011)	(.0091)	(1.3)	(.23)
Days SKU offered	-.0041***	-.0023***			-.0041***	-.0026***		
	(.00056)	(.00042)			(.00056)	(.00044)		
Retailer All Appearances	4.5e-06	-.000037***	.013***	.0084***	.000017	-.000031**	.013***	.0086***
	(.000018)	(.000013)	(.0024)	(.00032)	(.000019)	(.000013)	(.0024)	(.00032)
Number of Markets	.0051	-.0057	-.81	-.075	.0096*	-.0082	-.89	-.13
	(.0058)	(.0058)	(1.3)	(.13)	(.0058)	(.006)	(1.3)	(.13)
Chat Tool	-.052***	-.021**	-1.4	1.2***	-.05***	-.022**	-1.2	1.1***
	(.01)	(.0096)	(1.9)	(.26)	(.01)	(.0097)	(1.9)	(.26)
Call Center	-.04***	-.014	5.8***	-2.3***	-.11***	.003	7.5***	-.28
	(.015)	(.0091)	(1.6)	(.25)	(.022)	(.012)	(1.9)	(.38)
Showroom	-.052***	.016*	-3.1*	-.17	-.054***	.017*	-2.7	-.2
	(.0089)	(.009)	(1.8)	(.2)	(.0088)	(.0091)	(1.9)	(.2)
Top Retailer	-.25***	.015	-6.3***	8.2***	.014	.052***	3.3	-.52*
	(.028)	(.014)	(1.7)	(1.3)	(.01)	(.013)	(3.9)	(.29)
Constant	.42***	.19***	1.3	16***	.46***	.19***	.24	15***
	(.02)	(.015)	(1.6)	(.33)	(.022)	(.015)	(1.5)	(.36)
R-squared	.16	.21	.32	.37	.17	.21	.32	.38
N cases	7537	2720	431	7537	7537	2720	431	7537
Retailer X SKU FE	+	+	-	+	+	+	-	+
Retailer Fixed Effects	-	-	+	-	-	-	+	-

Panel B: Distribution Intensity and Dual Status Retailers

	Distribution				Dual Status Retailers			
	Violation Rate	Violation Depth	Assortment Size	SKU Availability	Violation Rate	Violation Depth	Assortment Size	SKU Availability
Authorized	-.044	-.32***		.76	-.15***	-.00027	2.7	-.034
	(.039)	(.083)		(.66)	(.012)	(.012)	(2)	(.24)
Post	-.041	-.19**		-8.8***	-.017	-.033***	-2.3***	-8.9***
	(.036)	(.082)		(.77)	(.012)	(.0075)	(.8)	(.26)
Authorized x Post	.0082	.37***		-1.8*	-.013	.021	2.7	-.11
	(.046)	(.088)		(.95)	(.013)	(.013)	(1.8)	(.36)
Authorized x Post x Yes	-.057	-.35***		4.3***				
	(.048)	(.089)		(1)				
Post x Yes	.03	.16**		-.22	-.066***	-.00099	-.91	6.2***
	(.038)	(.082)		(.81)	(.015)	(.016)	(3.4)	(.46)
Authorized x Yes	-.07*	.33***		-1.8***				
	(.04)	(.084)		(.68)				
Assortment Size	-.003***	.00058**		.074***	-.003***	.00069***		.077***
	(.00025)	(.00024)		(.0057)	(.00024)	(.00025)		(.0056)
Charge for Shipping	.092***	-.0084		3.8***	.095***	-.0088		3.7***
	(.016)	(.0087)		(.3)	(.016)	(.0087)		(.3)
Retailer Shipping	.019*	-.00024		-2***	.017	-.00016	2.2*	-1.9***
	(.011)	(.0083)		(.22)	(.011)	(.0083)	(1.2)	(.21)
Days SKU offered	-.0033***	-.0021***			-.0032***	-.0021***		
	(.00056)	(.00039)			(.00057)	(.00039)		
Retailer All Appearances	-.000026	-.000046***		.0087***	-.000026	-.000045***	.011***	.0086***
	(.000018)	(.000013)		(.00031)	(.000018)	(.000013)	(.0023)	(.00031)
Number of Markets	-.0039	-.0067		-.19	-.0071	-.0073	.23	-.22*
	(.0057)	(.0055)		(.13)	(.0057)	(.0055)	(1.2)	(.13)
Top Retailer	.39***	.2***		15***	.023**	.065***	3.1	-.52*
	(.017)	(.015)		(.28)	(.009)	(.012)	(3.8)	(.28)
Dual Status					.099***	.013	.065	-2.5***
					(.013)	(.011)	(2.8)	(.28)
Constant	-.00034	.06***		-.68**	.39***	.19***	2.5*	14***
	(.009)	(.012)		(.27)	(.017)	(.014)	(1.5)	(.28)
R-squared	.16	.2		.35	.16	.19	.28	.37
N cases	7910	2931		7910	7910	2931	487	7910
Retailer X SKU FE	+	+	-	+	+	+	-	+
Retailer Fixed Effects	-	-	+	-	-	-	+	-

Panel C: Retailers that Received Test Period Emails

	Violation Rate	Violation Depth	Assortment Size	SKU Availability
Authorized	-.12***	.024**	2.1	-1.3***
	(.012)	(.012)	(1.9)	(.25)
Post	-.018	-.031***	-2.3***	-8.9***
	(.012)	(.0075)	(.8)	(.26)
Authorized x Post	-.033**	.0062	1.7	2.7***
	(.014)	(.014)	(1.6)	(.39)
Authorized x Post X Email sent for this SKU	-.15***	-.036		-2.7***
	(.021)	(.023)		(.86)
Authorized x Post X Retailer Received Email	.0095	.031*	2	-.91**
	(.014)	(.018)	(4.1)	(.46)
Email sent for this SKU	.048**	-.018		1.3***
	(.023)	(.012)		(.42)
Retailer Received Email	.024*	-.028**	4.4	.6**
	(.013)	(.014)	(3)	(.26)
Assortment Size	-.003***	.0008***		.072***
	(.00025)	(.00027)		(.0058)
Charge for Shipping	.091***	-.011		3.8***
	(.016)	(.0088)		(.3)
Retailer Shipping	.017	.0063	2.1*	-2***
	(.011)	(.0084)	(1.2)	(.22)
Days SKU offered	-.0035***	-.002***		
	(.00056)	(.00039)		
Retailer All Appearances	-.00003*	-.00004***	.011***	.0086***
	(.000018)	(.000013)	(.0023)	(.00031)
Number of Markets	-.0041	.001	.47	-.29**

	(.0056)	(.0054)	(1.2)	(.13)
Constant	.4***	.16***	2.5*	15***
	(.017)	(.014)	(1.5)	(.28)
R-squared	.16	.18	.29	.35
N cases	7910	2931	487	7910
SKU FE	+	+	-	+
Retailer Fixed Effects	-	-	+	-

* p<0.10, ** p<0.05, *** p<0.01

Table 4 contains the results of equation (1), where instead of multiple month-year dummies, I have a single "Post" dummy, and instead of the *Authorized x 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 ignore 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 results for top sellers and for service providers, Panel B presents results for highly distributed products and for dual status retailers, Panel C presents results for retailers that received emails during the test period. The dependent variables are: the average monthly violations rate (column 1,5), the average monthly violation depth (column 2,6), the average assortment size (column 3,7), and the number of appearances of a SKU in a month (column 4,8). The subsample for violation depth analysis includes only retailer X SKU X 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 x Post x Characteristic* variable (rows 3 and 4). 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 5,6,8), standard errors are clustered by retailer X SKU, and there are retailer X SKU fixed effects. In column 3 (and 7), standard errors are clustered by retailer, and there are retailer fixed effects.

Table 5 - Change in Prices After the Policy Change

	%ΔAveragePrice			
	(1)	**(2)**	**(3)**	**(4)**
Authorized	.027***	.025***	.018**	.019***
	(.006)	(.006)	(.0071)	(.0072)
Assortment Size			.00086***	.00065***
			(.00025)	(.00022)
Charge for Shipping			.01	.017**
			(.0068)	(.0068)
Retailer Appearances			-3.4e-06	-5.4e-06
			(8.6e-06)	(8.6e-06)
Number of Markets			-.0022	-.00021
			(.0052)	(.0049)
Constant	.023***	.024***	.0064	.0055
	(.0041)	(.0043)	(.0081)	(.0082)
R-squared	.0071	.17	.02	.18
N cases	2542	2542	2542	2542
SKU Fixed Effects	-	+	-	+

* p<0.10, ** p<0.05, *** p<0.01

Table 5 contains the results of equation (3), 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.

Exhibits

Exhibit 1 - Timeline of Manufacturer's Policy Change

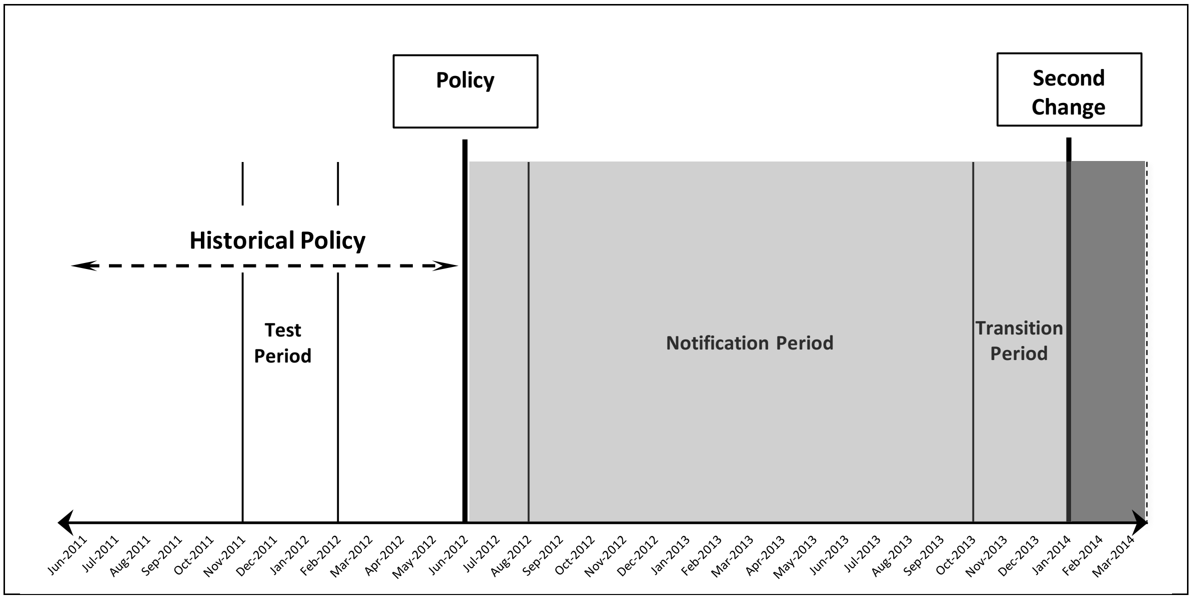


Exhibit 2 - Punishment Descriptions in Sample Policies

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: http://www.lg.com/us/commercial/display/heb2bmap (downloaded 08/01/2015)

Samsung Techwin America ("STA") Policy:

Effective September 16, 2013:

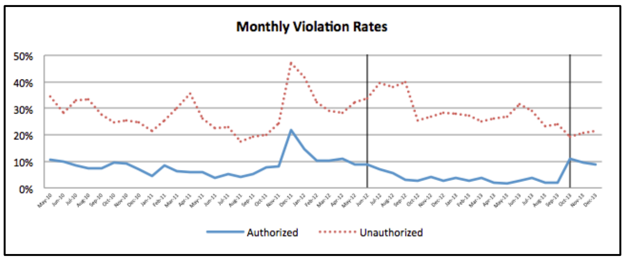
I) In the event a dealer or distributor chooses not to follow the MAP policy, sanctions will be unilaterally imposed by STA. Intentional and/or repeated failure to abide by this policy will result in termination of dealership or distributorship. STA does not intend to do business with dealers and/or 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://www.samsung-security.com/en/sales-and-services/map-policy.aspx (downloaded 08/01/2015)

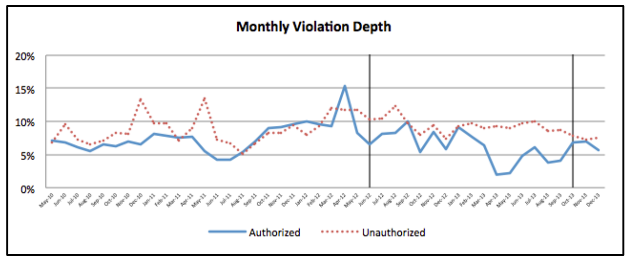
Exhibit 3 - Outcome Variables Trends Charts

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 a SKU appears in a month.

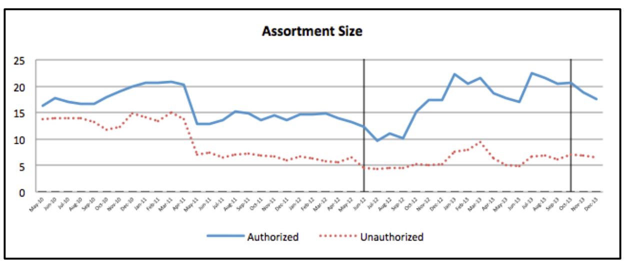
Panel A:



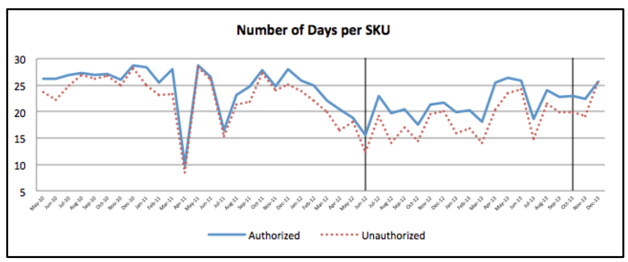
Panel B:



Panel C:



Panel D:



Online MAP Enforcement: Evidence from a Quasi-Experiment
ONLINE APPENDIX

ONLINE APPENDIX A: Robustness Section

1. Sensitivity Around the Policy Change Date

My main analysis defines the "post" period starting June 2012, when the manufacturer introduces the new agreements and policies. However, other dates may be relevant. For example, there may have been rumors circulating about the changes before June 2012 (the policies were written in March 2012), or perhaps the relevant beginning of the policy change is once emails were started, at the end of July 2012. I therefore define four different beginnings of the post period: April 2012, May 2012, July 2012, and August 2012. The results of this sensitivity analysis are reported in Table A3. For the majority of these variables, I find effects similar in sign and magnitude compared to the main results in the month before and after June. For violation depth, however, I obtain (marginally) statistically significant results only for the May 2012 definition.

In subsequent tests I change the definition of post to end later or begin as early as the test period. I also run a series of placebo tests that define the post period to be when no policy change takes place, to rule out systematic changes in the data.

2. Definition of the "Post" Period

In the main analysis, I define the post period to be from the time of the policy change until the beginning of the transition period in order to isolate the effect of the policy change and to avoid contamination of the estimate. However, to measure long-term effects of the policy change regardless of other interventions, I wanted to estimate the treatment effect for the rest of the sample. Therefore, I reran the analysis by defining the post period until the end of the sample – June 2012 to December 2013. The results of this analysis appear in columns 1-4 of Panel A of Table A4.

Recall that the manufacturer conducts a test at the end of November 2011. During this test, the manufacturer sends emails on four occasions in the weeks of November 27, 2011 and January 17, 2012. While there are no sustained consequences to those emails, they may have affected the authorized retailers' behavior. Further, the information regarding a change of policies and agreements in June 2012 may have become available before June. I therefore run the analysis again, this time defining December 2011 as the beginning of the post period. The results of this analysis appear in columns 5-8 of Panel A of Table A4.

Overall, the results are directionally consistent with the treatment effects of Table 2. There are some differences in terms of effect size and statistical significance mainly for the violation rate and depth. For violation rate, Table 2 suggests a treatment effect of 4 percentage points reduction in violation rate, but when I define the post period to end later, that reduction is 1.8 percentage points and no longer statistically significant (p-value=0.162). If I move the beginning of the post period to the test period, the effect is a reduction of 3.2 percentage points in violation rate. As illustrated in Exhibit 3 Panel A, the reason for the smaller reduction when the time period is extended to include the transition period is that during that time the violation rates rose again. For violation depth, the results are statistically not different than zero.

The fact that violation rates decrease by a lower amount if I include the period of October 2013 to December 2013, suggests that once enforcement via MAP notification emails have stopped, the effect of the policy change was reduced and violation rates were increased again. During October, the manufacturer started signing the retailers on new agreements; however it did not inform retailers that the notification emails would be halted for a three-month period. This suggests that emails are an important component of the policy change, presumably due to the increased credibility of the punishment. Another explanation of the effectiveness of the email notifications is that these emails prompt an internal investigation within the retailer organization (see discussion in section 5.2).

To rule out the explanation that the reduction in violations as well as improvement in service variables are due to the test period emails, I add another test in Panel B of Table A4. I include two interactions with Authorized – one after the Test and one after the policy change, the results suggests that most of the changes occur after the policy change in June 2012.

3. Placebo Tests

I repeat the analysis with two "placebo interventions," similarly to the robustness test in Anderson, Fong, Simester, and Tucker (2010).[27] I define the post period to begin a year prior to the policy change (June 2011) or 18 months prior to the policy change (December 2010), and end at the time of the policy change. If there were no differences in the trends between the authorized and unauthorized groups, I should expect that the treatment effect would not be statistically different from zero. The sample used for these analyses is smaller, because it ends in May 2012, the placebo "pre" period is May 2010 to May 2011 (November 2010), and the placebo post period is June 2011 (December 2010) to May 2012. The results of this estimation appear in Table A5. The "placebo intervention" in columns 1-4 starts in June 2011, and the "placebo intervention" in columns 5-8 begins in December 2010.

For the majority of the outcome variables, the "treatment effect" for either placebo interventions is not statistically different than zero, which suggests that the trends of the two retailers groups were similar in the earlier period. However, the "SKU availability" variable has a non-zero coefficient for the first placebo interventions, although this coefficient is negative, compared to our positive estimate. Overall, I conclude that the treatment effects I obtain in Table 2 and in the robustness tests should be attributed to the policy change.

4. Group Composition

A typical assumption in a difference-in-differences setting is that the composition of the groups did not change. That is not guaranteed in our case, since retailers may stop selling product for a period of time, and new retailers may appear in the data (especially unauthorized retailers). Looking at the average violation behavior mitigates some of this concern, and is similar in nature to looking at average employment rates (such as in Card [1992] for example).

In order to address this concern, I reanalyze the baseline regressions, to include only retailers that appeared both before and after the policy change took place. This reduces the number of Retailer X SKU X month observations from 80,064 to 66,723. The results of this analysis appear in columns 1-4 of in Table A6. For violation depth, since the observations are limited only to cases where there was a violation, I limit the sample to include only retailers that violate both before and after the policy change. I also run the analysis on an additional subsample, limiting observations to those retailers that offer the same SKU both before and

[27] Anderson et al. (2010) employ a difference-in-differences strategy, and repeat their analysis in periods with no interventions in one of their robustness tests.

after the policy change (columns 5-8 of Table A6), which further reduces the number of observations to 48,224. For violation depth, I limit the sample to include only retailers that violated MAP for a certain SKU both before and after the policy change. For assortment size, since the regression is in the retailer level there is no difference between the subsamples.

While the results are directionally robust and overall obtain similar magnitudes, there are some differences. The effect on the depth of violations becomes statistically different than zero only when I look at the same retailer and SKU combinations (a reduction of 1.8 percentage points in the depth of violations). The effect on violation rate is a decrease of about 6 percentage points (compared to 4 percentage points in Table 2), the effect on assortment size is 3.6 SKUs (compared to 4 in Table 2) and the effect on the duration a SKU is available is about 2 days (compared to 1.2 in Table 2).

In the main analysis there are two retailers that the manufacturer decided to terminate as ecommerce retailers, and thus have turned to unauthorized retailers in late January 2013. Since these retailers "switch" groups due to the policy change (but at a delayed period of time), I exclude them from the data starting February 2013. As a robustness check, I also re-estimate my main result and my group composition analyses using the data to include a post period of June 2012-January 2013. The results (reported in Table A7) are robust in magnitude and direction for all variables to my main results, and are statistically significant for violation rates (a decrease of 5-9 percentage points in violation, depending on the specification), and for SKU appearances (1.3-2 days). In addition, if I chose to include the two retailers in the period of February 2013-Decemeber 2013, but in the unauthorized retailers group, my main results are replicated in terms of magnitude and significance (reported in Table A8). The fact the manufacturer terminated these retailers enhances the credibility of the threat, since it demonstrates that it is willing to execute the punishment.[28]

5. SKUs Sold by Both Authorized and Unauthorized Retailers

To verify that the results are not driven by differences in product assortment of authorized versus unauthorized retailers, I investigate whether such differences exists. I find that out of 144 SKUs 12 SKUs are sold only by authorized retailers. Four additional SKUs are sold only by unauthorized retailers in the period after the policy change. I re-estimate the regressions from the previous subsection, excluding observations of the 16 SKUs that are not sold by both authorized and unauthorized retailers both before and after the policy change. The results hold in sign, magnitude and significance.

6. Trend Comparison

The goal of this robustness test is to evaluate whether there were differences in the trend before the first intervention took place between the authorized and unauthorized retailers. I estimate the following regression:

$$y_{rsm}=\alpha +\beta\, Authorized_r+\Sigma\, \gamma_i\, Month_i+\Sigma\, \delta_i\, Authorized_r\, Month_i +\theta\, X_{rsm}+f_s+\epsilon_{rsm}$$

where y_{rsm}, $Authorized_r$, X_{rsm}, f_s, ϵ_{rsm} are defined as above and $Month_i$ are dummy variables that indicate the month-year of the observation for. This regression is estimated only using observations in the months prior to the first intervention. The coefficients of interests are the δ_i, which ideally should not be statistically different than zero. I run this analysis for all 4 of the outcome variables (unreported). While for assortment

[28] Note, however, that the manufacturer did not advertise this termination to other retailers.

none of the 24 interaction coefficients is statistically different than zero, for other variables there are some coefficients statistically different than zero. Therefore, I reject the hypothesis that the δ_i's are jointly statistically equal to zero for these variables. I focus my discussion on the violation rates and the duration variable, since they were robust throughout other specifications.

For violation rates, 9 of the 24 δ_i's are significantly different than zero at the 5% level (and an additional one at the 10% level). I evaluate whether these coefficients that are different from zero explain the effect of reduction in violation rates. All of these coefficients are positive, which may potentially cause a positive bias of an increase in violation rates among authorized retailers. However, the effect on violation rates is the opposite: violation rates of treated authorized retailers drop compared to their unauthorized counterparts following the policy change. Therefore, I believe that a bias due to the difference in trends does not explain my results.

For the duration of SKU availability, 9 of the 24 δ_i's are significantly different than zero at the 5% level. Six of these coefficients are positive, and the rest are negative. Thus, it is difficult to conclude whether this bias is the source of the results. Some of this concern is mitigated with the placebo intervention tests in section 3 of the robustness test.

7. Ignoring Time Series Information

One criticism of difference-in-differences estimators in which a long time series is used, is that the outcomes may be serially correlated, and thus the resulting standard errors are inconsistent (Bertrand et al., 2004). To address this concern, I cluster the standard errors by retailer and SKU combination. In addition, Bertrand et al. suggest a simple solution to mitigate the correlation concern that works also for a small number of clusters: collapsing the time series information into “before” and “after” periods and clustering the standard errors to account for the smaller sample. For robustness, I follow that approach, while keeping the group composition constant in two different subsample definitions. First, I limit the sample only to retailers I observe both before and after the policy change. Second, I limit the sample further to include only retailer and SKU combinations that are observed both before and after the policy change. For each retailer and SKU I average the outcome variables before and after the policy change took place and use these two observations in my regressions.

I estimate a variation of equation (1) of the main manuscript, where instead of multiple month-year dummies, I have a single “post” dummy, and the results are reported in Table 3. Columns 1-4 report the results for the subsample of the same retailers and columns 5-8 report the results for the subsample of retailer and SKU combinations. The treatment effect remains similar in magnitude and statistically different from zero for violation rates (a reduction of 2.6-3.9 percentage points) and for SKU availability (2.1-2.6 days). For the assortment size, the results are no longer significant, however the test is relatively low powered. Note that when limiting the sample to only the same SKU and retailer combinations (column 6) there is an overall reduction in violation rates of 3.1 for all retailers after the policy change, suggesting that for SKUs that were sold over the entire period there is a downward trend in violations for both authorized and unauthorized retailers. However, for authorized retailers there is an additional 2.6 decrease in violation rates above and beyond that of unauthorized retailers.

8. Time-invariant characteristics

To better control for individual level time-invariant heterogeneity I re-estimate the regressions with retailer X SKU fixed effects, since the main unit of observation is a retailer X SKU combination. When adding these fixed effects, the main effect of being an authorized retailer and other retailer specific characteristics are collinear with the fixed effect and thus are dropped out. Of course, for the outcome variable of assortment size there cannot be retailer X SKU fixed effects. Instead, I add retailer fixed effects to this regression.

I keep the specification of the previous subsection while adding the fixed effects as described. The results are reported in Table A9. Columns 1-4 report the results for the subsample of the same retailers and columns 5-8 report the results for the subsample of retailer and SKU combinations. The treatment effect remains similar in magnitude and statistically different from 0 for violation rates (a reduction of 4 percentage points) and for SKU availability (1.5 days). For the assortment size, the results are no longer significant, however the test is relatively low powered

9. Common support on observables

One source of bias when using outcomes in a control group to compute the counterfactual for the treated group is that they may have non-overlapping support on observables. While I do control for observable characteristics, the concern is that due non-overlapping support the regression model specification will produce inappropriate extrapolation to predict the control group outcomes.

To address this concern, I utilize a propensity score approach. For each retailer X SKU combination, I compute the propensity of that combination to be in the treatment group. In other words, I compute the likelihood of that combination to be of an authorized retailer using a logistic regression. This allows me to compute the propensity score for each combination. I then employ two different methodologies to re-estimate the treatment effect on the authorized retailers for the outcome variable of violation rates. Following the recommendation in Heckman, Ichimura, and Todd (1997), I exclude observations with weak common support. That is, I re-estimate the model in the previous subsection excluding treatment observations without comparable control observations and control observations without comparable treatment observations. I drop authorized retailer observations with propensity score that is higher than the maximum propensity score of the unauthorized retailers, and unauthorized retailers with propensity scores that are lower than the minimum propensity score of authorized retailers. This regression that restricts the sample to observations with common support is reported in column 2 of Table A10. Column 1 reports the baseline results from subsection 8 as comparison. In addition, columns 3 and 4 report results of nearest neighbor matching based on the computed propensity score. Again, observations are limited to having common support. Column 3 reports one-to-one nearest neighbor matching, and column 4 reports nearest neighbor matching using Mahalanobis distance as a distance metric.

Throughout these specifications I find a reduction of about 7-8 percentage points in violation rates among authorized retailers. Note that the point estimates of reduction in violation rates are higher once I limit the sample to observations with common support. This is because violation rates among authorized retailers in this commons-support sample were 12.7% on average before the policy change.

ONLINE APPENDIX B: The Effect of a Violation Notification Email: Event Study

To isolate the effect of sending a notification email to a violating retailer from the other components of the updated agreements and policy changes of June 2012, I employ an event study methodology.[29] I examine the change in violation rates among authorized retailers that violated MAP and received a notification. The main concern is that comparing pre-post changes may be confounded with other unobservable changes that coincide with the email. I therefore treat each date an email was sent as a separate event, and compare violation rates before and after the notification. I exploit the fact that these events occur in different points in time to average the effect of email events, and control for time specific effects of violation behavior. This fact mitigates the concern that sending emails coincides with other events. I also use the average violation rates of non-violating authorized retailers and of unauthorized retailers as a proxy for the overall market violation behavior.

The main manuscript focused on the effect of the June 2012 MAP policy change, a change that included updated agreements, updated policies, and new work flows. In this Appendix, I examine the effect of one major addition to the MAP policy: sending MAP violation notification emails. Following the policy change, I document 43 instances when the manufacturer sent such emails (a total of 124 emails were sent in this period), and 4 instances when emails were sent during a test period before the policy change (a total of 49 emails were sent during the test period). I treat the dates of each email sent as a separate event, and compare violation rates before and after the notification. I use daily data surrounding the email events in an "event study" manner to examine the effect of the emails on retailer's compliance.

Exhibit A1 illustrates the event study graphically. The horizontal axis is the number of days since an email event occurred. Day 0 is the day the email was sent, day 7 is a week after the email was sent, and day -7 is a week prior to the event. The vertical axis is the proportion of MAP violations for a retailer X SKU combination within a group of retailers. The solid line is the group of authorized retailers receiving email notifications and that certain SKU, the dashed line is the group of all other authorized retailers and SKUs, and the dotted line is the group of unauthorized retailers. Each point in the graph is the average across the daily violations for the 43 events, and illustrates what fraction of the group was in violation of MAP. Prices are collected daily but emails are sent out only weekly, which means that emails aren't necessarily sent on the day of the violation. Therefore, there are cases where the blue solid line is not at 100% violation before day 0. Panel A includes the full sample of the 43 events that followed the policy change.

I compare the change in daily violations in the day before the email with the day after the email, and then to one, two, or three weeks following the violations. The reason to analyze the data by week is twofold: first, emails are sent out once a week; second, retailers are given 7 days to respond to a notification. For the full sample of 43 events, I find that violation rates decrease by 29 percentage points (86% violations in day -1 compared to 57% in day 1) in the day after violation notifications were sent. A week after the emails were sent, there is a 55 percentage point reduction in violations compared to day -1; and two weeks after the event, there is a reduction of 67 percentage points. After 3 weeks there is an 82 percentage point reduction,

[29] Which are commonly used in finance (e.g. MacKinlay 1997) to investigate the effect of different events on abnormal returns.

to a similar level of the group of all other authorized retailers.[30]

I also analyze the changes in violations using the following regression:

$$Violation_{rsd} = \alpha + \Sigma\beta_i Week_i + f_m + f_y + f_s + f_r + \epsilon_{rst} \quad (2)$$

where $Violation_{rsd}$ is an indicator that retailer *r* violated MAP for SKU *s* in day *d*, and $Week_i$ is an indicator of the number of weeks following an event. The variables f_m and f_y are month and year fixed effects, f_s are SKU fixed effects, and f_r are retailer fixed effects. The variable ϵ_{rst} is the error term, clustered by retailer X SKU. The regression is estimated only for authorized retailers. Table A11 presents the results of this regression, where the baseline week is the week before the email was sent (days -7 to 0, inclusive). Week 1 includes day 1 to day 7, week 2 includes day 8 to day 14, and so forth. The results of the regression reveal a similar trend to the results obtained by comparing only means – the reduction in violation rates increases and is sustained over time.

In Panel A of Exhibit A1 I present that in the 4 weeks before the event, the violation rates of the group of retailers and SKUs for which an email was sent is at 47%. A further investigation finds that this is partially driven by the first 5 events that took place during August and September 2012, and included a large number of retailers and SKUs for which there were violations. Panel B of Exhibit A1 restricts the sample to the later 38 events. In this case, the violation rates prior to the events for the group of retailers that receive emails is around 15%.[31,32]

As for the test period, I observe a similar pattern to the first 5 events, with higher violation rates among all groups (see Panel B of Exhibit A1). In particular, it is clear that the group of authorized retailers that did not receive emails (illustrated in the dashed line) actually has a violation rate of around 10%. The reason these retailers did not receive emails is that, at the time of the test period, a lack of transparency prevented the manufacturer from ascertaining whether they were authorized retailers. But by the time of the data analysis, policy change requirements ensured that authorized retailers were identified as such. During the test period I also observe that retailers violated MAP again shortly after they received an email, and two weeks out the violation rates remained at an average of 25%. In contrast, after the policy change all authorized retailers that did not receive emails did not violate MAP, and those that violate the policy reduce their violation rate almost completely after receiving an email.

The manufacturer used the same enforcement mechanism of email notifications both during the test period and following the policy change. In addition, we observe two periods after the policy change when no enforcement took place: one immediately after the policy change, before the emailing feature started, and one during the transition period. Throughout this time, the manufacturer continuously monitored the

[30] While we expect all other authorized retailers to have a 0% violation rate (otherwise they would have been sent an email), there are data collection issues that cause erroneous reporting of violations. Before emails are sent out there is a manual verification of the existence of violation and a proof of that violation. In absence of these, an email is not sent out. Hence, the authorized retailers' violation rate in the data is slightly higher than 0%.

[31] For the sample of 38 events I find that violation rates decrease by 30 percentage points in the day after violation notifications were sent. A week after the emails were sent, the reduction in violations compared to day -1 is 63 percentage points, 74 percentage points after two weeks, and 84 percentage points after 3 weeks. Regression estimates are similar to those reported in Table A11.

[32] Panel C of Exhibit A1 in the appendix provides a further look into the first two months of sending emails following the policy change. The axes and the lines are the same as in the other panels, but "Day 0" is the day of the second event. The vertical lines represent each of the events. As the graph demonstrates, the first event was sent 3 weeks prior to the second event (on day -20), and only starting the second event, emails would go out on a weekly basis (days 8, 13, and 20). The solid line represents only the retailers SKU combinations that received emails in event 2. The graph demonstrates that a subset of the retailers that violated MAP in event 2, kept violating MAP and kept receiving emails in future events. Events 4 and 5 seem to have caused a further reduction in the subset of retailers who violate. This pattern is observed only early after the policy change started, and after the first 5 events the reduction in violations seems to be more persistent. This also explains why the first events exhibit a high violation rate for a long period of time before each event. During this time, the manufacturer did not withhold product, and only sent out notifications.

market by collecting rich information about retailer pricing. Investment in monitoring alone did not reduce violation rates. For example, in Panel A of Exhibit 3 note that in the transition period, violation rates among authorized retailers spiked again to their level before the policy change.

The enforcement that took place during the test period was not effective in the long term due to misaligned channel agreements and policies. The periods with no enforcement after the policy change were also ineffective in achieving MAP compliance. Once enforcement efforts are complemented with appropriately designed channel policies and agreements, however, there is effective reduction in MAP compliance. I attribute the success of the enforcement emails to the increased credibility that the policy change facilitates. In particular, customizing the channel agreements to the online retail environment reduces asymmetric information, allowing more effective monitoring and enhanced ability to enforce the policy. Moreover, sending regular notification emails reinforces the credibility of the punishment and of an action by the manufacturer.

Although the lack of sustained compliance during the test period might be attributed to its short duration and the small number of notification emails sent out, note that in the weeks following the notifications sent during the test period, violating retailers committed a substantial number of violations. Further, the baseline of violation rates among the authorized retailers that did not receive emails in that period is at 10%. This is compared to a 1% violation rate for the equivalent group during the first five events that followed the policy change. The ability to reduce the violation rates of the authorized group was due to the improvement in both the credibility of the punishment and the transparency of the channel. In the test period, these authorized retailers did not receive emails even though they violated MAP because they were not identified properly. The change in policies and agreements essentially improved both the monitoring and the enforcement efforts, through improved information and punishment credibility.[33]

ONLINE APPENDIX C: The Effect on Manufacturer's Profit: an exploratory comparison

In this section I examine whether there is an effect of MAP on dollars spent or quantity ordered from the manufacturer. One of the reasons manufacturers avoid MAP is the fear 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 products. To test the effect on quantity and expenditure, I obtain the manufacturer's detailed sales report that includes the quantity and dollars spent for all orders of retailers 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.

I use linear regression and the equivalent Poisson regression models of the form:

$$y_{rst}=\alpha+\beta\, Treat+\theta\, ViolationRate_{rst}+f_s+f_r+\epsilon_{rst}$$

where y_{rs} is either the average quantity or dollars spent by retailer *r* for SKU *s* in period *t*, and $Treat$ is an indicator of whether period *i* is before or after the policy change (each retailer and SKU combination appears

[33] Another explanation of the effectiveness of the email notifications might be that these emails prompt an internal investigation within the retailer organization. For example, if the entity within the retailer's staff that receives the email is not the one that is in charge of pricing, or if advertising a price below MAP was a mistake. In most cases, these emails are being sent to the contact person in the company, but I do not observe their responsibilities.

at most twice).[34] $ViolationRate_{rst}$ is the average rate of violations for retailer *r* for SKU *s* in period t, f_s are SKU fixed effects, f_r are retailer fixed effects. ϵ_{rst} is the error term.

I find no evidence of a negative impact on quantity ordered or dollar spent when estimating the above equation. Both the linear and Poisson regression yield positive coefficients for β, although neither coefficient is statistically different than zero when appropriately accounting for standard errors (not reported). Therefore, I could not reject the null that a change in MAP policy has no impact on retailer's ordering behavior. Moreover, the point estimates of these coefficients are economically small and not meaningful. Although not statistically significant, the non-negative coefficient is consistent with the notion that a well-governed MAP policy is a desired outcome for both manufacturers and retailers. At the same time, I observe an increase of 2% in average prices among authorized retailers due to the increased compliance with MAP (reported in Table 5 in the manuscript). Even though the prices are higher, there in no evidence of an impact of MAP on quantity ordered.

Due to the structure and availability of the database, this test is exploratory in nature and may suffer from lack of statistical power. Specifically, the way that the quantity and dollar spent variables are aggregated leaves little variation. The patterns of ordering vary by retailers and products, and while some retailer-product combinations are observed in a similar frequency (e.g. every month or every quarter), others do not seem to have constant ordering patterns. Therefore, I computed average ordering monthly rates for the period before and after the policy change. In addition, the analysis is limited to the group of authorized retailers and products that are observed both before and after the policy change, and does not include a control group. Most of these issues are mitigated by using product and retailer fixed effects, but they take away from the statistical power of the test. Given all these inherent limitations in the data, the non-negative β coefficients are consistent with the view that increased average prices through MAP compliance do not have an adverse effect on volume.

[34] The sales report contains monthly data about purchases when they occur. There are some months when a retailer does not purchase any SKUs or does not purchase a certain SKU. When I compute the average quantity purchased I include all months in which either a purchase was reported or the retailer and SKU combination was observed in the database (inclusive). In months where there was no purchase reported but the retailer X SKU combination appear in the database, I set the quantity purchased to be zero.

Appendix Tables and Exhibits

Table A1 - The Effect of Manufacturer Policy Changes: Subsample with Shorter Post Period

		Violation Rate	Violation Depth	Assortment Size	SKU Availability
Post period: 3 months	Authorized x Post	-.087*** (.02)	-.028*** (.01)	-.64 (1.2)	2*** (.51)
	R-squared N cases	.14 57605	.12 16124	.19 5069	.31 57605
Post period: 2 months	Authorized x Post	-.076*** (.02)	-.026** (.012)	-.39 (1.2)	1.3** (.53)
	R-squared N cases	.14 56434	.12 15767	.19 4907	.31 56434
Post period: 1 month	Authorized x Post	-.055*** (.02)	-.031*** (.012)	.6 (1.2)	.8* (.47)
	R-squared N cases	.14 55510	.12 15511	.18 4770	.31 55510
SKU Fixed Effects		+	+	-	+

* p<0.10, ** p<0.05, *** p<0.01

Table A1 contains the results of equation (1), when the post period is defined for durations of 3, 2, or 1 month after the poicy change. The dependent variables are: the average monthly violations rate (column 1), the average monthly violation depth (column 2), the average assortment size (column 3), and the number of appearances of a SKU in a month (column 4). The subsample for violation depth analysis includes only retailer X SKU X 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 x Post*. The usual covariates are included in the analysis but nor reported. In columns 1,2 and 4, standard errors are clustered by retailer X SKU, and there are SKU fixed effects. In column 3, standard errors are clustered by retailer.

Table A2 - The Effect of Manufacturer Policy Changes: Subsample of Authorized Retailers

	Violation Rate		Violation Depth		Assortment Size		SKU Availability	
Post	-.05*** (.0047)	-.057*** (.005)	-.023*** (.0067)	-.029*** (.0065)	.94 (1.5)	-.012 (1.4)	-3.4*** (.18)	-3.7*** (.18)
Assortment Size		-.000058 (.00018)		.000029 (.00014)				.011** (.0044)
Charge for Shipping		.005 (.0094)		.0068 (.0082)				2.6*** (.19)
Retailer Shipping		.037*** (.008)		.019** (.0087)		2.1 (2.2)		-2.2*** (.23)
Days SKU offered		-.0014*** (.00018)		-.0017*** (.00022)				
Retailer all Appearances		-.00004*** (.000014)		-.00002 (.000014)		.017*** (.0036)		.0057** (.00033
Number of Markets		.0069 (.0052)		.0056 (.0061)		-2.3 (1.6)		-.042 (.14)
Constant	.084*** (.004)	.13*** (.011)	.081*** (.0038)	.12*** (.013)	16*** (1.1)	2.7 (2.6)	25*** (.089)	20*** (.32)
R-squared N cases	.04 45981	.049 45981	.19 8415	.2 8415	.001 2800	.13 2800	.053 45981	.081 45981
SKU Fixed Effects	+	+	+	+	-	-	+	+
Month-Year FE	+	+	+	+	+	+	+	+

* p<0.10, ** p<0.05, *** p<0.01

Table A2 contains the results of equation (1), limited only to the authorized retailer sample 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 a SKU in a month (columns 7-8). The subsample for violation depth analysis includes only retailer X SKU X 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 *Post* variable (row 1). In columns 1-4 and 7-8, standard errors are clustered by retailer X SKU, and there are SKU fixed effects. In columns 5-6, standard errors are clustered by retailer.

Table A3 - Robustness: Sensitivity Around "Post" Month

	Violation Rate				Violation Depth			
	April 2012	**May 2012**	**July 2012**	**August 2012**	**April 2012**	**May 2012**	**July 2012**	**August 2012**
Authorized	-.16***	-.16***	-.16***	-.16***	.0054	.0074	.0053	.005
	(.012)	(.012)	(.012)	(.012)	(.0052)	(.0052)	(.0052)	(.0052)
Authorized x Post	-.036***	-.042***	-.04***	-.035***	-.0053	-.012*	-.0064	-.0058
	(.013)	(.013)	(.014)	(.013)	(.0065)	(.0067)	(.0069)	(.0071)
Assortment Size	-.0011***	-.0011***	-.0011***	-.0011***	-.00054***	-.00052***	-.00053***	-.00053***
	(.00022)	(.00022)	(.00022)	(.00022)	(.000089)	(.00009)	(.00009)	(.000091)
Charge for Shipping	.072***	.072***	.072***	.072***	.0042	.004	.0042	.0042
	(.011)	(.011)	(.011)	(.011)	(.0046)	(.0046)	(.0046)	(.0046)
Retailer Shipping	-.0067	-.0071	-.0068	-.0065	.018***	.018***	.018***	.018***
	(.01)	(.01)	(.01)	(.01)	(.0045)	(.0045)	(.0045)	(.0045)
Days SKU offered	-.0031***	-.0031***	-.0031***	-.0031***	-.0013***	-.0013***	-.0013***	-.0013***
	(.00022)	(.00022)	(.00022)	(.00022)	(.00013)	(.00013)	(.00013)	(.00013)
Retailer all Appearances	-.00006***	-.00006***	-.00006***	-.00006***	-.00002***	-.00002***	-.00002***	-.00002***
	(.000018)	(.000018)	(.000018)	(.000018)	(6.3e-06)	(6.3e-06)	(6.3e-06)	(6.3e-06)
Number of Markets	.0048	.005	.0049	.0046	.0026	.0027	.0026	.0025
	(.0051)	(.0051)	(.0051)	(.0051)	(.0032)	(.0032)	(.0032)	(.0032)
Constant	.43***	.43***	.43***	.44***	.1***	.1***	.1***	.1***
	(.014)	(.013)	(.013)	(.013)	(.0068)	(.0068)	(.0068)	(.0068)
R-squared	.15	.15	.15	.15	.14	.14	.14	.14
N cases	80064	80064	80064	80064	21337	21337	21337	21337
SKU FE	+	+	+	+	+	+	+	+
Month-Year FE	+	+	+	+	+	+	+	+

	Assortment Size				SKU Availability			
	April 2012	**May 2012**	**July 2012**	**August 2012**	**April 2012**	**May 2012**	**July 2012**	**August 2012**
Authorized	2.5	2.6	2.6*	2.6*	-.72***	-.65***	-.65***	-.62***
	(1.6)	(1.6)	(1.5)	(1.5)	(.14)	(.14)	(.14)	(.14)
Authorized x Post	3.5**	3.7**	4.3**	4.7***	1.1***	.99***	1.1***	1***
	(1.6)	(1.6)	(1.7)	(1.8)	(.25)	(.26)	(.26)	(.25)
Assortment Size					.0021	.0023	.002	.0022
					(.003)	(.003)	(.003)	(.003)
Charge for Shipping					2.9***	2.9***	2.9***	2.9***
					(.15)	(.15)	(.15)	(.15)
Retailer Shipping	.56	.57	.59	.6	-1.4***	-1.4***	-1.4***	-1.4***
	(1.1)	(1.1)	(1.1)	(1.1)	(.14)	(.14)	(.14)	(.14)
Retailer all Appearances	.009***	.009***	.009***	.009***	.0074***	.0074***	.0074***	.0074***
	(.0021)	(.0021)	(.0021)	(.0021)	(.00018)	(.00018)	(.00018)	(.00018)
Number of Markets	.16	.15	.14	.14	-.62***	-.61***	-.62***	-.61***
	(1)	(1)	(1)	(1)	(.094)	(.094)	(.094)	(.094)
Constant	7***	7***	7***	7.1***	21***	21***	21***	21***
	(1.8)	(1.8)	(1.8)	(1.8)	(.27)	(.27)	(.26)	(.26)
R-squared	.21	.21	.21	.21	.28	.28	.28	.28
N cases	7187	7187	7187	7187	80064	80064	80064	80064
SKU FE	-	-	-	-	+	+	+	+
Month-Year FE	+	+	+	+	+	+	+	+

* p<0.10, ** p<0.05, *** p<0.01

Table A3 contains the results of equation (1) for four different dependent variables for different definitions of "Post". The dependent variables are (clockwise starting from the top left panel): average monthly violations rate, average monthly violation depth, average assortment size, and number of appearances of a SKU in a month. The columns define the first month "post" policy change as April 2012, May 2012, July 2012 or August 12. The subsample for violation depth analysis includes only retailer X SKU X month combinations where a violation occurred. The assortment size analysis is done for a subsample of retailer and month observations. For violation rates, depth, and number of appearances, standard errors are clustered by retailer X SKU, for "assortment size" dependent variable standard errors are clustered by retailer.

Table A4 - Robustness: Definition of "Post" Period

Panel A: Changing the Definition of "Post"

	"Post" Period Ends Later				"Post" Period Starts Earlier			
	Violation Rate	**Violation Depth**	**Assortment Size**	**SKU Availability**	**Violation Rate**	**Violation Depth**	**Assortment Size**	**SKU Availability**
Authorized	-.16*** (.012)	.0073 (.0051)	2.6 (1.6)	-.68*** (.14)	-.16*** (.013)	.006 (.0051)	2.4 (1.7)	-.76*** (.14)
Authorized x Post	-.018 (.013)	-.01 (.0064)	4.3** (1.7)	.94*** (.24)	-.032** (.013)	-.0057 (.006)	3.4** (1.5)	1.1*** (.23)
Assortment Size	-.0011*** (.00022)	-.00053*** (.000087)		.0013 (.003)	-.0011*** (.00022)	-.00054*** (.00009)		.0022 (.003)
Charge for Shipping	.072*** (.011)	.004 (.0044)		2.7*** (.15)	.073*** (.011)	.0042 (.0046)		2.9*** (.15)
Retailer Shipping	-.0057 (.0099)	.017*** (.0044)		-1.4*** (.14)	-.0064 (.01)	.018*** (.0045)	.55 (1.1)	-1.4*** (.14)
Days SKU offered	-.0029*** (.00021)	-.0012*** (.00012)	.66 (1.1)		-.0031*** (.00022)	-.0013*** (.00013)		
Retailer all Appearances	-.00005*** (.000017)	-.00002*** (6.0e-06)		.0074*** (.00018)	-.00006*** (.000018)	-.000017*** (6.3e-06)	.009*** (.0021)	.0074*** (.00018)
Number of Markets	.0024 (.0049)	.0029 (.003)	.0089*** (.0021)	-.56*** (.09)	.0044 (.0051)	.0026 (.0032)	.18 (1)	-.61*** (.094)
Constant	.43*** (.013)	.099*** (.0065)	.23 (1)	21*** (.26)	.43*** (.014)	.1*** (.0068)	7.1*** (1.8)	21*** (.27)
R-squared	.14	.14	.21	.27	.15	.14	.21	.28
N cases	84981	22657	7617	84981	80064	21337	7187	80064
SKU FE	+	+	-	+	+	+	-	+
Month-Year FE	+	+	+	+	+	+	+	+

Panel B: Differences between Test and Policy Change

	Including Both Post interactions			
	Violation Rate	**Violation Depth**	**Assortment Size**	**SKU Availability**
Authorized	-.16*** (.013)	.006 (.0051)	2.4 (1.7)	-.76*** (.14)
Authorized x Post Test	-.0038 (.016)	.0011 (.0092)	1 (1.4)	.48* (.28)
Authorized x Post Policy	-.038** (.015)	-.01 (.01)	3.2* (1.6)	.79** (.33)
Assortment Size	-.0011*** (.00022)	-.00053*** (.00009)		.0018 (.003)
Charge for Shipping	.072*** (.011)	.0041 (.0046)		2.9*** (.15)
Retailer Shipping	-.007 (.01)	.018*** (.0045)	.58 (1.1)	-1.4*** (.14)
Days SKU offered	-.0031*** (.00022)	-.0013*** (.00013)		
Retailer all Appearances	-.00006*** (.000018)	-.00002*** (6.3e-06)	.009*** (.0021)	.0074*** (.00018)
Number of Markets	.005 (.0051)	.0027 (.0032)	.15 (1)	-.62*** (.094)
Constant	.43*** (.014)	.1*** (.0068)	7.1*** (1.8)	21*** (.27)
R-squared	.15	.14	.21	.28
N cases	80064	21337	7187	80064
SKU FE	+	+	-	+
Month-Year FE	+	+	+	+
Combined Authorized x Post	-.042*** (.014)	-.009 (.007)	4.2** (1.77)	1.3 (.27)

* p<0.10, ** p<0.05, *** p<0.01

Table A4 contains the results of equation (1) for four different dependent variables for two different definitions of "Post". The dependent variables are: the average monthly violations rate (column 1,5), the average monthly violation depth (column 2,6), the average assortment size (column 3,7), and the number of appearances of a SKU in a month (column 4,8). Panel A presents two different definitions of the "Post" period. In columns 1-4 "Post" period ends at the end of the database, but starts with the policy change. In columns 5-8 "Post" period ends before the transition period begins, but starts after the test period. Panel B extends columns 5-8 of Panel A by including one interaction with

Authorized for after the policy change, and one for the test period. The subsample for violation depth analysis includes only retailer X SKU X 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 x Post* variable (row 2). In columns 1,2 and 4 (and 5,6,8), standard errors are clustered by retailer X SKU, and there are SKU fixed effects. In column 3 (and 7), standard errors are clustered by retailer.

Table A5 - Robustness: Placebo Test

	"Placebo Intervention": June 2011				**"Placebo Intervention": December 2010**			
	Violation Rate	**Violation Depth**	**Assortment Size**	**SKU Availability**	**Violation Rate**	**Violation Depth**	**Assortment Size**	**SKU Availability**
Authorized	-.16***	-.0033	2	-.36**	-.16***	.0012	.89	-.87*
	(.015)	(.0067)	(2.5)	(.16)	(.016)	(.0064)	(2.5)	(.19)
Authorized x Post	-.0032	.0077	2	-.51***	-.0024	-.00031	2.9*	.39**
	(.014)	(.0078)	(2)	(.18)	(.011)	(.0063)	(1.7)	(.18)
Assortment Size	.000064	-.00025**		-.0099***	.000061	-.00024**		-.011
	(.00034)	(.00012)		(.0035)	(.00034)	(.00012)		(.003
Charge for Shipping	.075***	-.0048		.67***	.075***	-.0048		.69**
	(.014)	(.0058)		(.18)	(.014)	(.0058)		(.18)
Retailer Shipping	-.013	.03***		-1.2***	-.013	.03***		-1.2*
	(.011)	(.0053)		(.15)	(.011)	(.0053)		(.15)
Days SKU offered	-.0037***	-.00093***	-.45		-.0037***	-.00092***	-.44	
	(.00028)	(.00014)	(1.2)		(.00028)	(.00014)	(1.2)	
Retailer all Appearances	-.000033*	-.000017**		.0075***	-.000033*	-.000017**		.0075
	(.00002)	(7.5e-06)		(.00019)	(.00002)	(7.6e-06)		(.000
Number of Markets	.0032	-.0013	.0078***	-.71***	.0032	-.0014	.0078***	-.7**
	(.006)	(.0038)	(.0023)	(.093)	(.006)	(.0038)	(.0023)	(.094
Constant	.39***	.095***	.48	21***	.39***	.093***	.47	21***
	(.017)	(.008)	(1)	(.29)	(.017)	(.008)	(1)	(.29)
R-squared	.14	.13	.17	.3	.14	.13	.17	.3
N cases	53957	14994	4557	53957	53957	14994	4557	53957
SKU FE	+	+	-	+	+	+	-	+
Month-Year FE	+	+	+	+	+	+	+	+

* p<0.10, ** p<0.05, *** p<0.01

Table A5 contains the results of equation (1) for four different dependent variables for two different definitions of "Post". The dependent variables are: the average monthly violations rate (column 1,5), the average monthly violation depth (column 2,6), the average assortment size (column 3,7), and the number of appearances of a SKU in a month (column 4,8). In columns 1-4, I define a placebo intervention, where "Post" is defined to begin exactly a year before the policy change takes place and ends a year later. In columns 5-8 I define another placebo intervention, where the "Post" period begins eighteen month prior to the policy change and ends at the time of the policy change. The subsample for violation depth analysis includes only retailer X SKU X 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 x Post* variable (row 2). In columns 1,2 and 4 (and 5,6,8), standard errors are clustered by retailer X SKU, and there are SKU fixed effects. In column 3 (and 7), standard errors are clustered by retailer.

Table A6 - Robustness: Group Composition

	Retailer Composition				**Retailer X SKU Composition**			
	Violation Rate	**Violation Depth**	**Assortment Size**	**SKU Availability**	**Violation Rate**	**Violation Depth**	**Assortment Size**	**SKU Availability**
Authorized	-.13***	.0015	3.5*	-1.2***	-.15***	-.012	3.5*	-1.6***
	(.013)	(.0062)	(1.8)	(.16)	(.018)	(.01)	(1.8)	(.19)
Authorized x Post	-.062***	-.0041	3.6**	2***	-.063***	-.018**	3.6**	2.2***
	(.015)	(.0072)	(1.7)	(.28)	(.018)	(.0091)	(1.7)	(.36)
Assortment Size	-.0022***	-5.0e-06		.0095***	-.0016***	.00012		.012***
	(.00021)	(.00012)		(.0034)	(.00029)	(.00017)		(.0043)
Charge for Shipping	.048***	.0067		2.5***	.058***	.019**		2.4***
	(.012)	(.0053)		(.16)	(.014)	(.0082)		(.19)
Retailer Shipping	.016	.013**	2.5*	-1.6***	.018	.00018	2.5*	-1.4***
	(.011)	(.0055)	(1.3)	(.16)	(.014)	(.0081)	(1.3)	(.2)
Days SKU offered	-.0018***	-.0011***			-.0015***	-.00067***		
	(.00024)	(.00016)			(.00028)	(.0002)		
Retailer all Appearances	2.6e-06	-.000016*	.0084***	.0077***	-.000033	-1.6e-06	.0084***	.008***
	(.00002)	(8.4e-06)	(.0023)	(.00023)	(.000026)	(.000012)	(.0023)	(.00029)
Number of Markets	-.00054	.0035	-.099	-.53***	.0069	.0096	-.099	-.47***
	(.0052)	(.0037)	(1.1)	(.097)	(.0064)	(.0066)	(1.1)	(.12)
Constant	.33***	.087***	4.8**	22***	.32***	.069***	4.8**	22***
	(.016)	(.0089)	(2)	(.32)	(.021)	(.012)	(2)	(.41)
R-squared	.13	.17	.22	.28	.13	.13	.22	.28
N cases	66723	15490	5424	66723	48224	8429	5424	48224
SKU FE	+	+	-	+	+	+	-	+
Month-Year FE	+	+	+	+	+	+	+	+

* p<0.10, ** p<0.05, *** p<0.01

Table A6 contains the results of equation (1) for four different dependent variables, limiting the sample only to retailers that appear both before and after the policy change took place. The dependent variables are: the average monthly violations rate (column 1,5), the average monthly violation depth (column 2,6), the average assortment size (column 3,7), and the number of appearances of a SKU in a month (column 4,8). In columns 1-4 I use any SKU for a retailer that appeared both before and after the policy change. In 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 sub samples. The subsample for violation depth analysis includes only retailer X SKU X month combinations where a violation occurred (and is limited only to retailer X 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 x Post* variable (row 2). 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 5,6,8), standard errors are clustered by retailer X SKU, and there are SKU fixed effects. In column 3 (and 7), standard errors are clustered by retailer.

TABLE A7 – SUBSAMPLE MAY 2010 – JANUARY 2013

Panel A: The Effect of Manufacturer Policy changes: difference-in-differences Analysis

	Violation Rate		**Violation Depth**		**Assortment Size**		**SKU Availability**	
Authorized	-.19***	-.16***	-.00023	.0038	6.5***	2.8*	1.8***	-.7***
	(.0095)	(.012)	(.0044)	(.0053)	(.37)	(1.6)	(.13)	(.14)
Authorized x Post	-.072***	-.051***	-.017**	-.0061	2.9***	2	1.3***	1.3***
	(.015)	(.015)	(.0078)	(.0078)	(.8)	(1.4)	(.34)	(.33)
Assortment Size		-.00063**		-.00034***				.0019
		(.00027)		(.0001)				(.0031)
Charge for Shipping		.073***		-.00072				2.1***
		(.012)		(.0051)				(.16)
Retailer Shipping		-.0088		.023***		.25		-1.6***
		(.011)		(.0049)		(1.1)		(.15)
Days SKU offered		-.0032***		-.0012***				
		(.00024)		(.00013)				
Retailer all Appearances		-.00005**		-.00002***		.0082***		.0075***
		(.000019)		(7.0e-06)		(.002)		(.00019)
Number of Markets		.0048		.0015		.41		-.6***
		(.0056)		(.0036)		(.92)		(.096)
Constant	.3***	.41***	.059***	.097***	12***	7.3***	24***	21***
	(.011)	(.014)	(.0036)	(.0072)	(1.3)	(1.8)	(.22)	(.27)
R-squared	.13	.14	.11	.13	.15	.2	.25	.3
N cases	65884	65884	18087	18087	5931	5931	65884	65884
SKU Fixed Effects	+	+	+	+	-	-	+	+
Month-Year FE	+	+	+	+	+	+	+	+

Panel B: Robustness: Group Composition

	Retailer Composition				**Retailer X SKU Composition**			
	Violation Rate	**Violation Depth**	**Assortment Size**	**SKU Availability**	**Violation Rate**	**Violation Depth**	**Assortment Size**	**SKU Availability**
Authorized	-.13***	-.0034	3.6*	-1.2***	-.15***	-.0095	3.6*	-1.8***
	(.014)	(.0065)	(1.9)	(.17)	(.02)	(.012)	(1.9)	(.22)
Authorized x Post	-.092***	.00011	1.7	2***	-.082***	-.012	1.7	2***
	(.016)	(.0084)	(1.5)	(.35)	(.019)	(.0099)	(1.5)	(.42)
Assortment Size	-.0023***	.00032**		.016***	-.0017***	.0004*		.026***
	(.00026)	(.00016)		(.0036)	(.00037)	(.00023)		(.0046)
Charge for Shipping	.033***	.0064		2.3***	.056***	.021**		2.3***
	(.012)	(.0061)		(.17)	(.015)	(.0095)		(.2)
Retailer Shipping	.018*	.014**	2.8**	-1.6***	.012	-.00066	2.8**	-1.9***
	(.011)	(.0061)	(1.3)	(.16)	(.015)	(.0095)	(1.3)	(.22)
Days SKU offered	-.0018***	-.00098***			-.0015***	-.00073***		
	(.00027)	(.00018)			(.00033)	(.00023)		
Retailer all Appearances	.000022	-.000021**	.0072***	.0076***	-.000046	-.000013	.0072***	.008***
	(.000022)	(9.7e-06)	(.0022)	(.00025)	(.000029)	(.000014)	(.0022)	(.00034)
Number of Markets	.00045	.0037	.18	-.49***	.0095	.01	.18	-.44***
	(.0057)	(.0043)	(.99)	(.1)	(.0072)	(.0082)	(.99)	(.13)
Constant	.3***	.084***	4.9**	21***	.32***	.072***	4.9**	21***
	(.017)	(.0099)	(2.1)	(.34)	(.025)	(.014)	(2.1)	(.47)
R-squared	.13	.18	.22	.3	.14	.11	.22	.29
N cases	52618	12454	4376	52618	35736	6593	4376	35736
SKU FE	+	+	-	+	+	+	-	+
Month-Year FE	+	+	+	+	+	+	+	+

* p<0.10, ** p<0.05, *** p<0.01

Table A7 replicates the results of the main analysis for the subsample of May 2010 – January 2013. Panel A is based on the same analysis of Table 2 and Panel B is based on Table A6.

TABLE A8 – SAMPLE INCLUDING “SWITCHED” RETAILERS

Panel A: The Effect of Manufacturer Policy changes: difference-in-differences Analysis

	Violation Rate		**Violation Depth**		**Assortment Size**		**SKU Availability**	
Authorized	-.19*** (.0095)	-.16*** (.012)	.00075 (.0045)	.0058 (.0052)	6.5*** (.37)	2.4 (1.6)	1.8*** (.13)	-.58*** (.14)
Authorized x Post	-.055*** (.013)	-.032** (.013)	-.026*** (.0068)	-.011 (.0068)	4.8*** (.7)	3.7** (1.7)	1.4*** (.27)	1.7*** (.26)
Assortment Size		-.0012*** (.00022)		-.00052*** (.00009)				-.0034 (.0031)
Charge for Shipping		.074*** (.011)		.0039 (.0046)				3*** (.16)
Retailer Shipping		-.0066 (.01)		.018*** (.0045)		.61 (1.1)		-1.4*** (.14)
Days SKU offered		-.003*** (.00022)		-.0013*** (.00013)				
Retailer all Appearances		-.000064*** (.000018)		-.000017*** (6.3e-06)		.0093*** (.0021)		.0073*** (.00018)
Number of Markets		.0041 (.0051)		.0028 (.0032)		.19 (1)		-.68*** (.094)
Constant	.31*** (.011)	.44*** (.013)	.056*** (.0037)	.1*** (.0068)	12*** (1.3)	6.8*** (1.8)	24*** (.22)	21*** (.27)
R-squared N cases	.13 80656	.15 80656	.12 21507	.14 21507	.15 7202	.21 7202	.23 80656	.28 80656
SKU FE	+	+	+	+	-	-	+	+
Month-Year FE	+	+	+	+	+	+	+	+

Panel B: Robustness: Group Composition

	Retailer Composition				**Retailer X SKU Composition**			
	Violation Rate	**Violation Depth**	**Assortment Size**	**SKU Availability**	**Violation Rate**	**Violation Depth**	**Assortment Size**	**SKU Availability**
Authorized	-.13*** (.013)	.0011 (.0062)	3.4* (1.8)	-1.1*** (.16)	-.15*** (.018)	-.008 (.0098)	3.4* (1.8)	-1.6*** (.19)
Authorized x Post	-.053*** (.014)	-.006 (.0073)	3.3* (1.8)	2.6*** (.28)	-.052*** (.017)	-.021** (.0091)	3.3* (1.8)	2.9*** (.36)
Assortment Size	-.0022*** (.0002)	-8.3e-06 (.00012)		.003 (.0035)	-.0017*** (.00028)	.00019 (.00016)		.0043 (.0045)
Charge for Shipping	.049*** (.012)	.0066 (.0053)		2.6*** (.16)	.059*** (.014)	.019** (.0082)		2.5*** (.19)
Retailer Shipping	.017 (.011)	.013** (.0055)	2.5* (1.3)	-1.6*** (.16)	.018 (.014)	-.00071 (.0081)	2.5* (1.3)	-1.4*** (.2)
Days SKU offered	-.0017*** (.00023)	-.0011*** (.00016)			-.0013*** (.00028)	-.00063*** (.0002)		
Retailer all Appearances	-1.0e-06 (.00002)	-.000015* (8.4e-06)	.0086*** (.0023)	.0075*** (.00023)	-.000037 (.000025)	-5.7e-06 (.000012)	.0086*** (.0023)	.0079*** (.00029)
Number of Markets	-.0014 (.0052)	.0036 (.0037)	-.045 (1.1)	-.6*** (.098)	.0056 (.0064)	.01 (.0065)	-.045 (1.1)	-.55*** (.12)
Constant	.33*** (.015)	.086*** (.0089)	4.6** (2.1)	22*** (.32)	.33*** (.021)	.063*** (.012)	4.6** (2.1)	22*** (.41)
R-squared N cases	.13 67315	.17 15660	.21 5439	.28 67315	.13 48860	.15 8794	.21 5439	.28 48860
SKU FE	+	+	-	+	+	+	-	+
Month-Year FE	+	+	+	+	+	+	+	+

* p<0.10, ** p<0.05, *** p<0.01

Table A8 replicates the results of the main analysis for a sample where the two authorized retailers that were terminated are switched to unauthorized in the data (instead of being dropped from the data starting February 2013, as in the main analyses). Panel A is based on the same analysis of Table 2 and Panel B is based on Table A6.

Table A9 - Robustness: Time Invariant Characteristics

	Retailer Composition				Retailer X SKU Composition			
	Violation Rate	**Violation Depth**	**Assortment Size**	**SKU Availability**	**Violation Rate**	**Violation Depth**	**Assortment Size**	**SKU Availability**
Authorized x Post	-.073***	-.021*	3*	2.8***	-.07***	-.022**	3*	2.7***
	(.017)	(.011)	(1.6)	(.39)	(.016)	(.011)	(1.6)	(.38)
Assortment Size	.000037	.000087		.062***	.000045	.00017		.074***
	(.00018)	(.00018)		(.0049)	(.0002)	(.00023)		(.0054)
Charge for Shipping	.022*	.021**		1.9***	.031**	.03**		1.7***
	(.012)	(.0086)		(.25)	(.013)	(.012)		(.28)
Days SKU offered	-.00069***	-.000091			-.0006***	-.00016		
	(.00018)	(.00016)			(.00021)	(.00021)		
Constant	.18***	.075***	14***	25***	.16***	.063***	14***	26***
	(.0097)	(.0071)	(1.1)	(.28)	(.011)	(.0099)	(1.1)	(.32)
R-squared	.67	.73	.6	.47	.62	.57	.6	.42
N cases	66723	15490	5424	66723	48224	8429	5424	48224
Retailer X SKU FE	+	+	-	+	+	+	-	+
Retailer Fixed Effects	-	-	+	-	-	-	+	-
Month-Year FE	+	+	+	+	+	+	+	+

* p<0.10, ** p<0.05, *** p<0.01

Table A9 contains the results of equation (1), where instead of multiple month-year dummies, I have 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 ignore 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 (column 1,5), the average monthly violation depth (column 2,6), the average assortment size (column 3,7), and the number of appearances of a SKU in a month (column 4,8). In columns 1-4 I use any SKU for a retailer that appeared both before and after the policy change. In 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 sub samples. The subsample for violation depth analysis includes only retailer X SKU X month combinations where a violation occurred (and is limited only to retailer X 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 x Post* variable (row 2). 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 5,6,8), standard errors are clustered by retailer X SKU, and there are retailer X SKU fixed effects. In column 3 (and 7), standard errors are clustered by retailer, and there are retailer fixed effects.

Table A10 - Robustness: Common Support on Observables

	All Observations	**Common Support**	**Common Support + Nearest Neighbor Matching**	**Common Support + Mahalanobis Distance Matching**
Post	-.015	.0065	.081*	.065
	(.017)	(.017)	(.041)	(.04)
Authorized x Post	-.04**	-.084***	-.15***	-.14***
	(.018)	(.019)	(.038)	(.038)
Assortment Size	-.00091	-.0011	-.0017**	-.002**
	(.00061)	(.00064)	(.00071)	(.0008)
Charge for Shipping	.013	.014	-.053	.0075
	(.038)	(.046)	(.091)	(.12)
Days SKU offered	-.0013	-.00086	.00065	-.0011
	(.00085)	(.0009)	(.0016)	(.0014)
Constant	.23***	.22***	.15***	.19***
	(.021)	(.021)	(.044)	(.032)
R-squared	.8	.79	.66	.67
N cases	5106	4274	4772	4772
Retailer X SKU FE	+	+	+	+

* p<0.10, ** p<0.05, *** p<0.01

Table A10 contains the results of equation (1), where instead of multiple month-year dummies, I have a single “Post” dummy, for four different dependent variables, limiting the sample only to retailer and SKU combinations that appear both before and after the policy change took place, while ignore 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 variable is the average monthly violations rate. In column 1 I report the result from Column 5 of Table 7, in Columns 2-4 I restrict the sample to contain only observation with common support. In Columns 3-4 I match each treatment observation to the nearest neighbor, either using Euclidian (column 3) or Mahalanobis distance (column 4). The treatment effect (δ) is the coefficient for the *Authorized x Post* variable (row 2). The observations in these regressions are restricted to retailers and SKUs that were observed both before and after the policy change took place. Standard errors are clustered by retailer X SKU, and there are retailer X SKU fixed effects.

Table A11 - Event Study: Changes in Violations Following Notification Email

	1	2	3	1	2	3
Week 1	-.033	-.18***	-.17***	-.36	-1.9***	-2***
	(.054)	(.038)	(.038)	(.29)	(.34)	(.36)
Week 2	-.2***	-.31***	-.28***	-1.2***	-3.6***	-3.7***
	(.047)	(.037)	(.04)	(.32)	(.53)	(.56)
Week 3	-.35***	-.41***	-.36***	-2.4***	-6.2***	-6.3***
	(.037)	(.033)	(.031)	(.44)	(.6)	(.8)
Week 4	-.4***	-.48***	-.41***	-3***	-7.5***	-8.3***
	(.036)	(.034)	(.033)	(.49)	(.95)	(1)
Constant	.53***	.62***	.83***	.35**	.89***	1.2
	(.031)	(.028)	(.097)	(.17)	(.19)	(3.5)
R-squared	.19	.56	.59	.19	.57	.61
N cases	3817	3817	3817	3817	3306	3306
SKU X Retailer FE	-	+	+	-	+	+
Month-Year FE	-	-	+	-	-	+

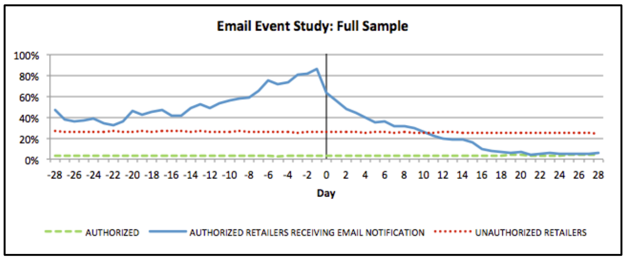
* p<0.10, ** p<0.05, *** p<0.01

Table A11 contains the results of equation (2) including the entire 4 weeks after the notification email, for authorized retailers. The dependent variable is the violation rate. Standard errors are clustered by retailer X SKU, and there are month, year, retailer and SKU fixed effects. Column 1-4 report ordinary least squares results, and columns 5-8 report Logit results. The Logit results are overall consistent with the linear results. For example, the associated odds ratios for column 3 for example are .13, .024, .002, .002, suggesting that the odds of a violation decrease to 87% in week1, 97% in week two and are virtually non-existent in weeks 3 and 4.

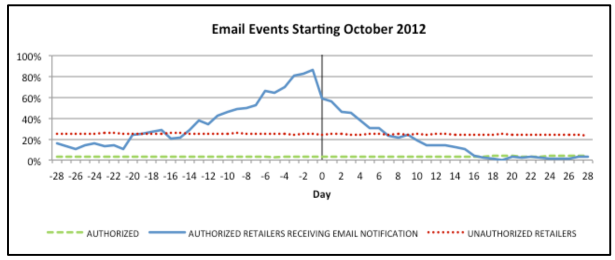
<u>Exhibit A1 - Email Event Study Charts</u>

The horizontal axis is the number of days since an email event occurred. Day 0 is the day the email was sent (email event), day 7 is a week after the email was sent, and day -7 is a week prior to the event. The vertical axis is the proportion of violations in each group. The solid blue line is the group of authorized retailers receiving email notifications and that certain SKU, the dashed green line is the group of all other authorized retailers and SKUs, and the dotted red line is the group of unauthorized retailers. Each point in the graph is the average across the daily violations for the events plotted in that graph, and illustrates what fraction of the group was in violation of MAP. Top left panel present the 43 events of the policy change period, and top right panel presents the last 38 events of the policy change period. The bottom left panel presents the second event and illustrates events 1 through 5 for the group which were contacted in event 2, and the top right panel presents the events of the test period.

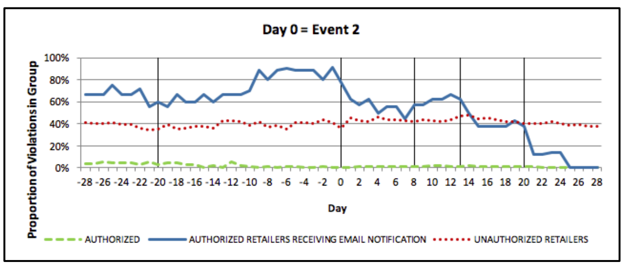
Panel A:



Panel B:



Panel C:



Panel D: