Channel Management and MAP: Evidence from a Quasi-Experiment

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ABSTRACT

Minimum Advertised Price (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. This paper shows that the mere fact that manufacturers monitor pricing and have a contractual threat to terminate distribution may be insufficient in achieving compliance, and that the context and terms of the policy affect manufacturers' ability to govern MAP. Two key elements to improve compliance are uncovered: 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 retailers that do not provide services or have lower sales volume and for popular products. At the same time, there is an increase in opportunistic behavior among top retailers or high service providers, and for less popular products via deeper discounts.

1. Introduction

Minimum Advertised Price (MAP) policies are widely used in online marketplaces for consumer durable goods such as electronics, cameras, appliances, sporting goods and toys as well as in B2B online markets. MAP allows manufacturers to unilaterally impose a lower bound on retailers' advertised prices, enabling manufacturers to (among other things) protect retail margins and brand image, and to coordinate prices across different online and offline channels and outlets. The exact prevalence of MAP is difficult to measure since the policies are typically confidential. However, a conservative lower bound is that at least 540 manufacturers use MAP policies with their downstream channel partners.[1]

This paper examines whether manufacturers can influence MAP compliance. While retailers sign authorized dealer agreements to follow the manufacturer's policies, opportunistic retailers often advertise products with prices below the MAP, thus violating the policy (Pereira 2008, Barr 2012, Israeli, Anderson and Coughlan 2016). They do this even though MAP policies always describe the consequences of violations, such as halting product shipments for a set period, or terminating that retailer as a distributor. Of central concern for manufacturers is how to effectively enforce and achieve compliance from online markets, which account for a large fraction of sales.[2]

MAP violations are often attributed to manufacturers' failure to invest in either monitoring or enforcement efforts, which keep them from acquiring detailed information on advertised retail prices. Or, 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, Jensen and Meckling 1976, and others). However, retailers often commit violations even though a MAP policy is a clear legal

[1] I arrived to this conclusion after collecting more than 440 publicly posted online MAP policies and an extensive online search that included press releases, articles, and industry forums.

[2] In the online channel, since MAP policy applies to almost any price that appears on a website (for products with a MAP policy), these policies become essentially a minimum price policy.

document, and despite substantial investments manufacturers make into monitoring and enforcement.

In this paper, I demonstrate that achieving compliance with MAP policies may require changes in channel policies and agreements. Investments in monitoring and enforcement may be insufficient when the channel policies are not aligned with compliance efforts. 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 the punishment.

In online environments, there is a need both for greater transparency in information flow and better segmentation between online versus offline retailers. Customizing policies to the online channel allows a manufacturer to alleviate asymmetric information when contracts are established and thereby improve the manufacturer's ability to select channel partners, thus reducing adverse selection. A credible punishment improves the effectiveness of a contract, which is a well-known moral hazard issue.[3]

I obtain these findings in a field setting in which I observe, over several years, the interactions between a durable goods manufacturer and hundreds of its retail partners in the online channel. The vast distribution through online channels makes it difficult 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 SKUs on each website where its products are sold. In addition, savvy retailers may choose to advertise their products under multiple domain names[4], which the manufacturer is not aware of and may 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.

Monitoring is not enough to achieve compliance, since retailers must believe that the threat of punishment is credible and that the manufacturer will enforce the MAP policy. During my observation period sample, the manufacturer invests in obtaining detailed information on the pricing behavior of

[3] 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.

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

downstream retailers. In addition, a substantial fraction of employee time is spent 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 test period in which notification emails are sent to violating retailers. Among violating retailers, these emails cause a short-term reaction and violations are reduced. 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 has its authorized retailers re-sign these agreements.[5] The revised policies 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 the 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 partners. This change also allowed the manufacturer to correctly identify websites of retailers that do not have a distribution authorization agreement, namely unauthorized retailers. This step improved transparency and the credibility of the manufacturer's ability to monitor MAP.

Second, the manufacturer revises 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.

[5] 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.

Additionally, under the new MAP policy, a violating retailer would receive an email notifying them of a violation. Specifying clear consequences and sending intermediate notification emails allow the manufacturer to credibly signal its commitment to enforce the policy. Importantly, the threat of punishment appears to be more credible after the policy change, as I will show, even though both the original and the updated policy share the same punishment.

The focus of this paper is the effect of the policy change of June 2012 on MAP compliance. Before the policy change, average violation rates in the authorized channel were 8.5%. Using a 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 possible 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 authorized retailers that do not provide services; authorized retailers that are not top sellers in the category; or for products in wide distribution. This suggests that the policy is effective where it matters most – for retailers who are less committed in the first place, and for products with high visibility. At the same time there is almost no reduction in violation rates after the policy change among authorized top retailers, service providers or for low distribution products, although those groups did exhibit higher opportunistic behavior by providing higher discounts when violating MAP.

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. Using an event study approach, I find that within a week of a notification, violations drop by more than 30% among the notified authorized retailers, and the reduction persists for at least four weeks. 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.

Put together, these findings suggest that channel agreements and policies must be aligned with monitoring and enforcement efforts in order to effectively govern a channel policy such as MAP. Investments in monitoring and enforcement alone are insufficient to achieve sustained compliance with the original agreements and policies. New agreements and policies alone are also insufficient during the months with no enforcement emails. Only when monitoring and enforcement efforts are complemented with appropriately designed channel agreements and policies, is the manufacturer able to achieve MAP compliance in the authorized channel. This suggests that the context under which the investments in monitoring and enforcement take place is critical. By modifying the channel agreements and policies, the manufacturer created an environment in which investments in monitoring and enforcement are effective.

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. 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 was able to significantly improve 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,[6] but the prevalence of MAP violations in recent years has become a central concern for manufacturers. Charness and Chen (2002) look at MAP policy design using a controlled laboratory experiment to investigate the question of how to achieve MAP compliance. [7] This paper extends 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[8] 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, this paper uses a quasi-experiment to study how manufacturers can effectively implement MAP policies.

The second literature to which this paper relates is crime and punishment deterrence (Becker 1968 and others), which 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 lean to fewer violations (Nagin 2013). In the setting I studied, the manufacturer revised 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 final punishment did not change, the wording and details of the punishment procedure did, and these changes improved 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

[6] A self-enforcing agreement was first modeled and analyzed by Telser (1980).
[7] The experiment was conducted in Hewlett-Packard (HP) laboratories, to examine specifications for their MAP policy.
[8] 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.

increases compliance, but when risks are low, vagueness is more likely to increase compliance; In my setting, if retailers perceived 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. Finally, the new punishment structure included three strikes, such that the final strike led 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, typically the final punishment is more severe than the original punishment before the policy change.

Finally, this paper also draws on literature concerning distribution channel management and coordination. Research on enforcement of manufacturers' contracts and policies has focused on gray markets 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). 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 effect 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 look at 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, this literature relies on self-reported measures for both the dependent and independent variables, while I use observed data.

One of the new features of the policy change I investigate is a notification email that is sent 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 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). I also use an event study approach[9] to examine the change in violation rates surrounding the day a notification email is sent.

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

3. Policy change and Data Description

In recent years manufacturers commonly use MAP in online marketplaces. Yet, tracking and monitoring MAP compliance is difficult due to the broad online distribution and the presence of unauthorized retailers in the channel. Because retailers often sell products on multiple websites and finding those sites is time consuming, manufacturers have difficulties identifying the offending retailers, even when they find a violation. To improve monitoring efforts, therefore, manufacturers typically hire third-party companies to track MAP prices on the Internet. These third party 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. Additionally, manufacturers may 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. 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, 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 their products were available on many more online outlets than they had identified previously. Not only did they discover unauthorized retailers, but they also found out that several of the seemingly unauthorized websites were in fact their 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.

In June 2012, the manufacturer also revised its agreement and policies and had its authorized dealers sign updated agreements. That June 2012 policy change is the focus of this paper (see Exhibit 1 for a timeline of the policy changes). The policy change included 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's goal was 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 agreements require retailers to be preapproved to sell products online, in predetermined website addresses and restrict all ecommerce dealers from advertising products unless they carry a minimum of one-month inventory. Further, retailers are required to commit to a pre-defined 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 reduced information asymmetry and allowed 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 includes 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. 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. There doesn't seem to be a single best practice in the industry. Of 446 MAP policies that were collected in an online search, 41% describe a specific punishment or timeline, and 59% include vague descriptions of potential punishments. 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[10]) 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 this session, the manufacturer explained the reasons and motivation for the channel agreements and policies

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

and reviewed the application procedures in detail. The training process aligned the manufacturer's employees and agents, and the retailers with the new policies and agreements. The new legal documents were effective June 2012, and the notification email system was launched by the end of July 2012.

After the policy change, I observe a natural variation in MAP policy enforcement, which I exploit to examine the persistence of compliance in the absence of notifications. 18 months after the policy change, the manufacturer modified the agreements and policies to significantly reduce the number of authorized online retailers by deciding to approve fewer ecommerce retailers. The manufacturer imposed additional restrictions regarding how its product and intellectual property is displayed in advertising on retailers' websites, and limited the number of allowed website addresses for individual retailers. In addition, the manufacturer increased by 40% the required minimum dollar amount to which the retailer had to pre-commit. The three months prior to these changes was a transition period in which no notification emails were sent, but price monitoring continued. My main analysis examines data before the transition period.

Before the policy change, 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.

The data for this study are provided by Channel IQ, a company that monitors and enforces MAP policies and collects data about online prices for its manufacturer clients, and from one of their manufacturer clients. The data are unique because MAP policies are often confidential, and it is rare to observe communication between manufacturer and retailers.[11]

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[12] 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

[11] 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.

[12] 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.

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.[13] 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.[14] For the event study analysis, where I examine immediate response to notification emails, I use daily observations.

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 violations for that month is 20%. The average percentage of violations in the monthly database is 15.9% (6.8% among authorized, 28.2% among unauthorized), and the average depth of violations is 8.1% (7% among authorized, 9% among unauthorized). I observe violations on 22,657 of the 84,891 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 (22.5 days on average, 23.4 for authorized and 21.4 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 11.2 SKUs each month on average, an authorized retailer has an assortment size of 16.6 on average, and an unauthorized retailer

[13] 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.

[14] 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.

assortment size is 7.8 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 – 19% of retailers have a showroom in addition to their online website (49% of authorized retailers, 11% of unauthorized); 8% of retailers provide an online chat tool (20% of authorized, 4% of unauthorized); and 44% of retailers have a call center (77% of authorized, 36% of unauthorized).[15] In addition, 70% of the top online retailers in terms of sales in this industry[16] sell the manufacturer's product, 93% of which are authorized retailers.

I also obtained detailed manufacturer sales reports that include the purchases of products for each of the retailers between July 2002 and December 2013. I use these 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. My empirical analysis includes three subsections. The goal of the first subsection is to measure the overall effect of the policy change on retailers' violation rates, violation depths, assortment size and duration of product availability. The goal of the second subsection is to measure the direct effect of the notification-email component of the new policy. The goal of the third subsection is to investigate the effect of the policy change on prices and demand as proxied by inventory ordered and dollars spent by retailers. While the third subsection is exploratory in nature, my identification strategy uses a difference-in-difference approach for the first subsection, and an event study approach for the second subsection.

I attempt to measure the overall effect of the policy change on retailers' behavior. One aspect is the rate of violation occurrences, since improving those rates was the main goal of the change. The effect of

[15] These data were collected in September 2015 and are available for 80% of retailers, since some websites have been removed by that time.
[16] I obtain the relevant top retailers list from the relevant trade publications.

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.

4.1 Measuring the Overall Effect of the Policy Change on Retailers' Behavior

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.[17] 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 demand shocks that 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. 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.

[17] 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.

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 obtains its inventory through a legitimate authorized retailer, or through the gray market, and competes with both authorized and other unauthorized retailers. Since manufacturers do not hold legitimate power against the unauthorized channel, MAP policies do not apply to them and are therefore unenforceable in that channel. For simplicity, however, I use the term "violation" to indicate cases where unauthorized retailers advertise prices below MAP. Manufacturers can try to identify unauthorized retailers and combat them through trademark or intellectual property related legal suits, which are time-consuming and hard to prove.

I study changes over time (before versus after the policy change) in outcome variables, such as violation rates, between authorized and unauthorized retailers in a difference-in-differences setting. This empirical methodology does *not* assume that the unauthorized group is *ex ante* identical to the treatment group of authorized retailers; indeed authorization is not randomly assigned. The difference-in-differences methodology accounts for the fact that authorized and unauthorized retailers are potentially different in various confounding characteristics. I only assume that the trends in behavior are similar before the policy change. Specifically, the identifying assumption for the difference-in-differences approach to measure the policy change's effect is that the trend in unauthorized retailers is approximately similar to the trend in authorized retailers in absence of the policy change shock. This premise is also confirmed in my data. The difference-in-differences approach captures the effect of the policy change by comparing the violation rates and depths and other retail variables before versus after the changes in agreements and policies (first difference), comparing authorized versus unauthorized retailers (second difference). I then construct a series of robustness tests to validate my difference-in-difference results.

One concern with using the unauthorized group as a control group may be that the trends in behavior of unauthorized retailers are potentially indirectly affected by the policy change. For example, the new policy may 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. Additionally, a concern of reduced supply to the unauthorized channel is not likely to manifest itself immediately after the policy changed, and is 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. Appendix Table A1 reports the estimates of that analysis. Finally, if the policy is effective and authorized retailers raise their prices, it is not clear 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.

4.2 Measuring the Direct Effect of the Email Component of the New Policy

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. I examine the change in violation rates among authorized retailers who 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.

5. Data Analysis

I organize the analysis into three 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 effected. The goal of the second subsection is to isolate the effect of an email notification on violation behavior, using an event study approach. In the third subsection I investigate 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. 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).[18] 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 $Authorized_r xPost_m$ indicate whether month *m* occurs following 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

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

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.

The results of the difference-in-differences analysis are presented in Table 1. 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% 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 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 (Column 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 to 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 to 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.[19]

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 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 to no significant difference obtained in the difference-in-differences analysis. There is no significant difference in assortment size, compared to an increase of 4 products in the difference-in-differences analysis. Finally, there is a reduction of 3.7 days in duration compared to 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 Appendix), vary the definition of the "post" period (Table A4 in the Appendix), run placebo tests (a la Anderson, Fong, Simester, and Tucker 2010, in Table A5 in the Appendix), verify the group composition (Table A6 in the 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 2), control for additional time invariant characteristics (Table A9 in the Appendix), and address concerns of common support on observables (Table A10 in the 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

[19] 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 of 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.

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 service levels, sales levels, and product popularity. To do so, I construct interactions from the type: $AuthorizedxPostxCharacteristic$ and $AuthorizedxPostxNotCharacteristic$ that allow me to examine the treatment effect on the group with the particular characteristic and compare that to the group without that characteristic. I use these in place of the $AuthorizedxPost$ interaction. I use the regressions in columns 1-4 of Table 2 as a baseline for comparison.

I first look at top retailers, who 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 authorized top 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 authorized non-top 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.

Regression analyses (unreported) reveals differences between authorized retailers who have top sales in the industry and those who do not. Prior to the policy change, authorized top retailers' violation rates are lower than authorized non-top retailers, but the depth of violations does not differ between the two groups. Regarding SKUs, authorized top retailers are likely to carry the same number of SKUs as authorized non-top retailers before the policy change, but SKU availability is longer on average. After the policy change, authorized non-top retailers' violation rates reduce by 4.7 percentage points (p-val<0.001), while top-authorized retailers' behavior is not differentially affected by the policy change. Authorized top retailers' violation depth increases by 9 percentage points after the policy change, while authorized non-top retailers violation depth remains as it was before the policy change. Authorized top retailers are marginally likely to carry 4.3 more SKUs after the policy change (p-val=0.082), while authorized non-top retailers do not significantly change their behavior. Finally, authorized top retailers SKU availability increased by 2.3 days (p-val<0.001) compared with 3.3 days for authorized non-top retailers (p-val<0.001). These results are reported in columns 1-4 of Table 3. Overall, there are improvements across both types of retailers – authorized non-top retailers violate less and carry product for a longer time period after the policy change, and top retailers carry more SKUs and make them 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 one if the retailer provides any service (chat, call center or showroom) and equals zero otherwise. The results are robust to different definitions of this variable (for example, an indictor for whether 0, 1, 2, or all services are provided). Similarly to the top-retailer characteristic, a-priori there is no clear prediction of which authorized retailers will be affected more. Those who provide services made more investments and commitments compared to those who did not. Before the policy change, authorized service providers were more likely to violate MAP than non-service providers who are authorized, but there was no significant difference in violation rates. The service providers carried the same number of products as those who do not provide services, for a longer period on average. After the policy change, authorized retailers that do not provide services reduce their violations by 12 percentage points (p-val<0.001) compared to marginally significant 2.4 percentage point reduction (p-val=0.067) for the service provider. That is, those who were less likely to violate reduced their violations even more. Authorized retailers who are non-service providers also carry more products on average (5.4, p-val=0.115 in a low powered test[20]), and for a longer period of time (5.1, p-val<0.001) after the policy change. As for the non-service providers, their SKU availability increases by 2.7 days (p-val<0.001) after the policy change, but when they violate the policy, they increase violation depth by 2.9 percentage points (p-val=0.015). These results are reported in columns 5-8 of Table 3.

Overall, I find that the policy change affects those authorized retailers who do not provide services more than those who provide 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, authorized retailers who were top retailers were less likely to violate MAP, and those who provided service were more likely to violate MAP.[21] Interestingly, both the top retailers and the service providers who are authorized increased the depth of violation after the policy change, while not changing their rate of violations. At the same time, the authorized retailers who do not provide service or are not top retailers, reduced their violation rates, but their depth of violations was unaffected.

Finally, I examine the effect of the policy change on more popular versus more niche products. I construct indicators of below and above the median (median=41) distribution and interact those with the *Authorized x Post* indicator. Before the policy change, there were no differences in violation rates, depth, or SKU availability for highly distributed versus low distributed SKUs among authorized retailers. 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. I find that after the policy change, authorized retailers reduce violations on the more popular products (by 4.6 percentage

[20] Tests that include monthly observations and do not limit to one observation before and one after per retailer show significant differences.
[21] The effects cancel each other for those who were top retailers and provided service.

points, p-val<0.001). In addition, their SKU availability increases by 3.4 days (p-val<0.001). At the same time, once they violate MAP on the popular product, violation depths are higher by 2.5 percentage points (p-val=0.035) compared to before the policy change. For low-distribution products, there are no statistically significant changes in violation rates or the SKU availability after the policy change among authorized retailers, but the violation depth is higher by 7.2 percentage points (p-val=0.013). The results are reported in the Appendix Table A11.

Put together, these results suggest that the policy is effective where it matters most – there are bigger reductions in violation rates among those retailers who provide lower services or that have lower overall sales in the product category. Less robustly, the policy improves the level of service provided by these retailers by increasing SKU availability and the assortment size. Improvements in compliance rates and SKU availability are also observed for highly visible products that are available in many retail outlets. For top retailers, retailers that provide services, and products that are distributed in fewer retail outlets, there is almost no reduction in violation rates. At the same time, there is an increase in opportunistic behavior due to the policy among these retailers and 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).

5.2 The Effect of a Violation Notification Email: Event Study

The previous section 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 section, 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, and 4 instances when emails were sent during a test period before the policy change. 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 4 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 to 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, to a similar level of the group of all other authorized retailers.[22]

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

[22] 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%.

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 4 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 4 I observe 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 4 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%.[23,24]

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 4). 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

[23] 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 4.

[24] Panel A 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.

all authorized retailers that did not receive emails did not violate MAP, and those who 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 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.[25]

5.3 The Effect on Manufacturer's Profit: an exploratory comparison

In this section I examine whether MAP enforcement affects dollars spent or quantity ordered from the manufacturer. One of the reasons manufacturers avoid MAP is the 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'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 B.

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

[25] 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.

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 to 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 regards 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. Conclusions

In this paper I investigate a manufacturer's ability to influence compliance rates among authorized retailers 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 and application 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, 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 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 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 non-top retailers, authorized retailers that do not provide services and for highly distributed products. On the other hand, the depth of violations is higher after the policy change among top retailers, service providers, and low distribution products, while their violation rates mostly 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 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 4 weeks following the notification. I attribute the 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 that this manufacturer took, it suggests 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 this problem.

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

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

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

Table 1 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 2 - 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*** (.012)	.011 (.0099)	2.9 (1.9)	-.98*** (.22)	-.15*** (.016)	.0049 (.016)	2.9 (1.9)	-1.8*** (.28)
Post	-.018 (.012)	-.032*** (.0075)	-2.3*** (.8)	-8.9*** (.26)	-.031** (.013)	-.02*** (.0063)	-2.3*** (.8)	-9.2*** (.34)
Authorized x Post	-.039*** (.013)	.02* (.011)	2.3 (1.7)	2.1*** (.34)	-.026* (.013)	.0027 (.011)	2.3 (1.7)	2.6*** (.43)
Assortment Size	-.0029*** (.00024)	.00067*** (.00026)		.072*** (.0057)	-.0018*** (.00034)	.00071* (.0004)		.065*** (.0077)
Charge for Shipping	.093*** (.016)	-.011 (.0088)		3.8*** (.3)	.11*** (.021)	.022 (.014)		4.1*** (.38)
Retailer Shipping	.018* (.011)	.0031 (.0082)	2.3** (1.2)	-2*** (.22)	-.0047 (.015)	-.0087 (.011)	2.3** (1.2)	-1.5*** (.29)
Days SKU offered	-.0034*** (.00055)	-.002*** (.0004)			-.0027*** (.00067)	-.001** (.00046)		
Retailer all Appearances	-.00003 (.00002)	-.00004*** (.00001)	.012*** (.0023)	.0086*** (.0003)	-.00007*** (.00002)	-.00003 (.00002)	.012*** (.0023)	.0094*** (.0004)
Number of Markets	-.0038 (.0056)	.0012 (.0054)	.57 (1.2)	-.28** (.13)	.013* (.0076)	.015 (.01)	.57 (1.2)	-.12 (.16)
Constant	.39*** (.016)	.17*** (.014)	2 (1.4)	15*** (.27)	.4*** (.021)	.11*** (.018)	2 (1.4)	14*** (.34)
R-squared N cases	.16 7910	.18 2931	.28 487	.35 7910	.17 5106	.12 1422	.28 487	.38 5106
SKU Fixed Effects	+	+	-	+	+	+	-	+

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

Table 2 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 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 3: Heterogeneity in response to the policy change

	Top Sellers				**Service Providers**			
	Violation Rate	**Violation Depth**	**Assortment Size**	**SKU Availability**	**Violation Rate**	**Violation Depth**	**Assortment Size**	**SKU Availability**
Authorized	-.096***	.013	1.9	-1.1***	-.095***	.0035	1.6	-1.1***
	(.012)	(.011)	(2.2)	(.24)	(.012)	(.011)	(2.2)	(.24)
Post	-.025**	-.045***	-2.5***	-9.8***	-.024**	-.045***	-2.5***	-9.8***
	(.012)	(.0084)	(.94)	(.27)	(.012)	(.0084)	(.94)	(.27)
Authorized x Post x Yes	-.01	.091***	4.3*	2.3***	-.024*	.029**	2.1	2.7***
	(.016)	(.02)	(2.5)	(.45)	(.013)	(.012)	(2)	(.36)
Authorized x Post X No	-.047***	.0061	2.3	3.3***	-.12***	.028	5.4	5.1***
	(.014)	(.012)	(2.1)	(.38)	(.02)	(.023)	(3.4)	(.6)
Assortment Size	-.0023***	.00096***		.072***	-.0023***	.00092***		.072***
	(.00024)	(.00028)		(.0059)	(.00024)	(.00028)		(.0059)
Charge for Shipping	.06***	-.0071		4***	.059***	-.0059		4***
	(.016)	(.0095)		(.32)	(.016)	(.0097)		(.32)
Retailer Shipping	-.0052	.0038	3.3**	-1.7***	-.0071	.003	3.4***	-1.7***
	(.011)	(.0087)	(1.3)	(.23)	(.011)	(.0087)	(1.3)	(.23)
Days SKU offered	-.0041***	-.0022***			-.004***	-.0022***		
	(.00056)	(.00042)			(.00056)	(.00042)		
Retailer All Appearances	-8.1e-06	-.000042***	.013***	.0084***	-7.3e-06	-.000039***	.013***	.0083***
	(.000018)	(.000013)	(.0024)	(.00032)	(.000018)	(.000013)	(.0024)	(.00032)
Number of Markets	.013**	-.0061	-.5	-.15	.014**	-.0074	-.55	-.18
	(.0059)	(.0062)	(1.3)	(.14)	(.0059)	(.0062)	(1.3)	(.14)
Chat Tool	.049***	-.0016	-.77	-.47**	.048***	.0009	-.66	-.43*
	(.01)	(.01)	(3.2)	(.23)	(.01)	(.01)	(3.2)	(.23)
Call Center	-.092***	-.02**	4.1***	-.63***	-.11***	-.02**	4.4***	-.23
	(.012)	(.0087)	(1.4)	(.22)	(.013)	(.0086)	(1.4)	(.24)
Showroom	-.012	.022***	-2.8	-1***	-.015*	.023***	-2.6	-.91***
	(.0086)	(.0082)	(1.7)	(.2)	(.0086)	(.0083)	(1.8)	(.2)
Top Retailer	-.023*	.016	1.6	.44	-.0084	.052***	2.7	.051
	(.013)	(.015)	(3.9)	(.31)	(.01)	(.013)	(4.5)	(.29)
Constant	.42***	.19***	1.6	16***	.42***	.19***	1.5	15***
	(.019)	(.015)	(1.7)	(.31)	(.019)	(.015)	(1.6)	(.31)
R-squared	.16	.21	.31	.37	.16	.2	.31	.37
N cases	7537	2720	431	7537	7537	2720	431	7537
Retailer X SKU FE	+	+	-	+	+	+	-	+
Retailer 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, 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). 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 4 - Event Study: Changes in Violations Following Notification Email

	1	2	3
Week 1	-.033 (.054)	-.15*** (.037)	-.15*** (.037)
Week 2	-.2*** (.047)	-.29*** (.035)	-.29*** (.037)
Week 3	-.35*** (.037)	-.41*** (.031)	-.37*** (.028)
Week 4	-.4*** (.036)	-.48*** (.032)	-.43*** (.03)
Constant	.53*** (.031)	.41*** (.061)	.52*** (.098)
R-squared N cases	.19 3817	.49 3817	.52 3817
SKU FE	-	+	+
Retailer FE	-	+	+
Month FE	-	-	+
Year FE	-	-	+

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

Table 4 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.

Table 5 - Change in Prices After the Policy Change

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

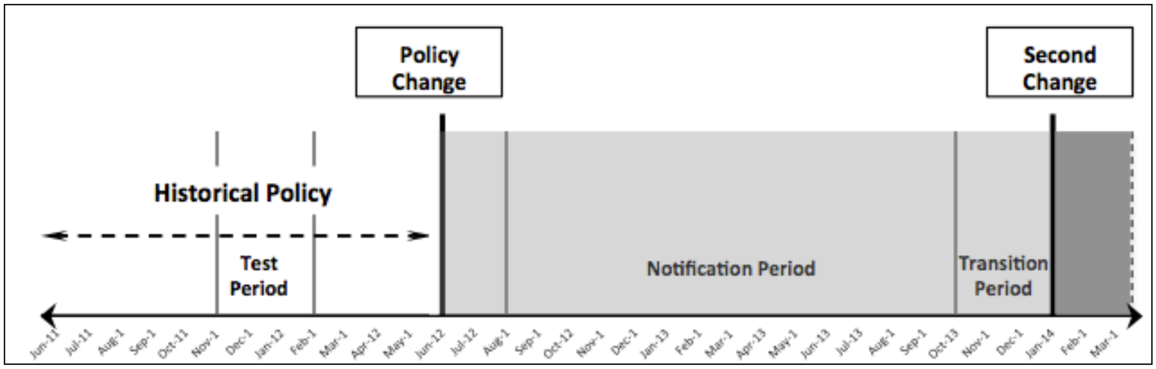


Exhibit 2 - Punishment Descriptions in Sample Policies

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:

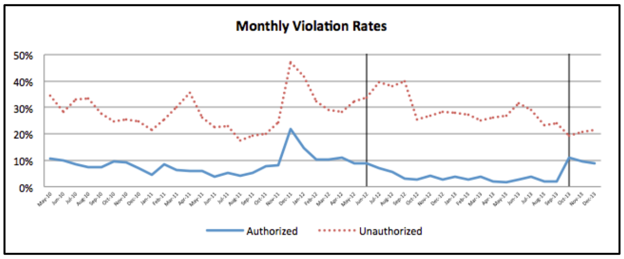
I) In the event a dealer or distributor chooses not to follow the MAP policy, sanctions will be unilaterally imposed by STA. Intentional and/or repeated failure to abide by this policy will result in termination of dealership or distributorship. STA does not intend to do business with dealers and/or 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)

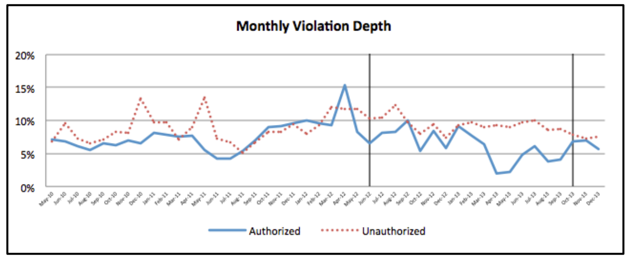
Exhibit 3 - Outcome Variables Trends Charts

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.

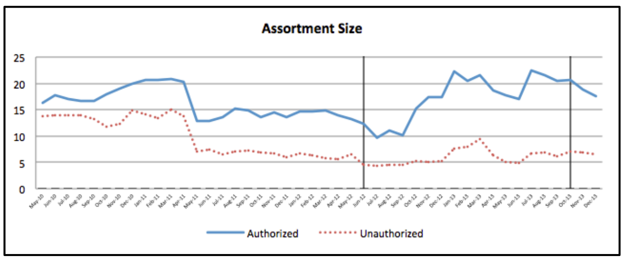
Panel A:



Panel B:



Panel C:



Panel D:

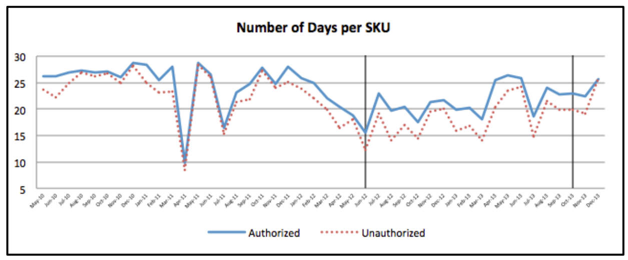
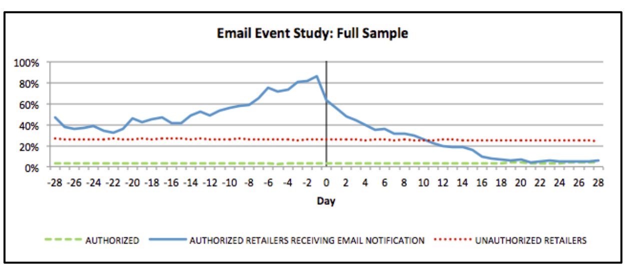


Exhibit 4 - Email Event Study Charts

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.

Panel A:



Panel B:

ONLINE APPENDIX

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 4 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 Table A4.

Recall that the manufacturer conducts a test at the end of November 2011. During this test, the manufacturer sends 4 emails 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 Table A4.

Overall, the results are directionally consistent with the treatment effects of Table 1. There are some differences in terms of effect size and statistical significance mainly for the violation rate and depth. For violation rate, Table 1 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 4.3 percentage points in violation rate. As illustrated in Exhibit 3 Panel A, the reason for the smaller reduction when the time periods 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).

3. Placebo Tests

I repeat the analysis with two "placebo interventions," similarly to the robustness test in Anderson, Fong, Simester, and Tucker (2010).[26] 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 1 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 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

[26] 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.

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 1), the effect on assortment size is 3.6 SKUs (compared to 4 in Table 1) and the effect on the duration a SKU is available is about 2 days (compared to 1.2 in Table 1).

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.[27]

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

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

assortment 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 2. 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.

APPENDIX B: 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 at most twice).[28] $ViolationRate_{rst}$ 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.

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[28] 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.

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	+	+	+	+	-	-	+	+

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

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

* 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

	“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***	.0073	2.6	-.68***	-.15***	.0027	2.3	-.78***
	(.012)	(.0051)	(1.6)	(.14)	(.013)	(.0054)	(1.8)	(.15)
Authorized x Post	-.018	-.01	4.3**	.94***	-.043***	.0018	3.2**	.97***
	(.013)	(.0064)	(1.7)	(.24)	(.014)	(.0061)	(1.5)	(.21)
Assortment Size	-.0011***	-.00053***		.0013	-.0011***	-.00055***		.0023
	(.00022)	(.000087)		(.003)	(.00023)	(.00009)		(.003)
Charge for Shipping	.072***	.004		2.7***	.072***	.0044		2.8***
	(.011)	(.0044)		(.15)	(.011)	(.0046)		(.15)
Retailer Shipping	-.0057	.017***		-1.4***	-.0069	.018***		-1.4***
	(.0099)	(.0044)		(.14)	(.01)	(.0046)		(.14)
Days SKU offered	-.0029***	-.0012***	.66		-.0031***	-.0013***	.54	
	(.00021)	(.00012)	(1.1)		(.00022)	(.00013)	(1.1)	
Retailer all Appearances	-.00005***	-.00002***		.0074***	-.00006***	-.00002***		.0074***
	(.000017)	(6.0e-06)		(.00018)	(.000018)	(6.4e-06)		(.00018)
Number of Markets	.0024	.0029	.0089***	-.56***	.0045	.0024	.009***	-.6***
	(.0049)	(.003)	(.0021)	(.09)	(.0051)	(.0032)	(.0021)	(.094)
Constant	.43***	.099***	.23	21***	.43***	.1***	.2	21***
	(.013)	(.0065)	(1)	(.26)	(.014)	(.0069)	(1)	(.27)
R-squared	.14	.14	.21	.27	.15	.14	.21	.28
N cases	84981	22657	7617	84981	80064	21337	7187	80064
SKU FE	+	+	-	+	+	+	-	+

* 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). 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 when the test period began. 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	+	+	-	+	+	+	-	+

* 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 Fixed Effects	+	+	-	+	+	+	-	+

* 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	+	+	-	+	+	+	-	+

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

* 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 1 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	.13	.15	.12	.14	.15	.21	.23	.28
N cases	80656	80656	21507	21507	7202	7202	80656	80656
SKU Fixed Effects	+	+	-	+	+	+	-	+

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

* 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 1 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**
Post	-.015	-.024**	-2.2*	-8.9***	-.015	-.024***	-2.2*	-8.9***
	(.021)	(.012)	(1.1)	(.56)	(.017)	(.0085)	(1.1)	(.45)
Authorized x Post	-.04*	-.002	2.3	1.5**	-.04**	-.002	2.3	1.5**
	(.022)	(.022)	(2.4)	(.73)	(.018)	(.016)	(2.4)	(.59)
Assortment Size	-.00091	.0015		.22***	-.00091	.0015*		.22***
	(.00076)	(.0012)		(.03)	(.00061)	(.00085)		(.024)
Charge for Shipping	.013	.042		.15	.013	.042		.15
	(.047)	(.058)		(1.5)	(.038)	(.041)		(1.2)
Days SKU offered	-.0013	-.0016			-.0013	-.0016**		
	(.0011)	(.0011)			(.00085)	(.00074)		
Constant	.23***	.12***	12***	16***	.23***	.11***	12***	17***
	(.026)	(.031)	(.51)	(.79)	(.021)	(.021)	(.51)	(.59)
R-squared	.87	.92	.81	.79	.8	.79	.81	.69
N cases	7910	2931	487	7910	5106	1422	487	5106
Retailer X SKU FE	+	+	-	+	+	+	-	+
Retailer Fixed Effects	-	-	+	-	-	-	+	-

* 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 (.017)	.0065 (.017)	.081* (.041)	.065 (.04)
Authorized x Post	-.04** (.018)	-.084*** (.019)	-.15*** (.038)	-.14*** (.038)
Assortment Size	-.00091 (.00061)	-.0011 (.00064)	-.0017** (.00071)	-.002** (.0008)
Charge for Shipping	.013 (.038)	.014 (.046)	-.053 (.091)	.0075 (.12)
Days SKU offered	-.0013 (.00085)	-.00086 (.0009)	.00065 (.0016)	-.0011 (.0014)
Constant	.23*** (.021)	.22*** (.021)	.15*** (.044)	.19*** (.032)
R-squared N cases	.8 5106	.79 4274	.66 4772	.67 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: Heterogeneity in distribution in response to the policy change

	Violation Rate	Violation Depth	SKU Availability
Authorized	-.1*** (.012)	.0035 (.011)	-1*** (.24)
Post	-.022* (.012)	-.044*** (.0085)	-9.9*** (.27)
Authorized x Post x High Distribution	-.046*** (.013)	.025** (.012)	3.4*** (.36)
Authorized x Post X Low Distribution	.026 (.027)	.072** (.029)	.42 (.64)
Assortment Size	-.0024*** (.00024)	.00087*** (.00028)	.073*** (.0059)
Charge for Shipping	.058*** (.016)	-.0079 (.0098)	4*** (.31)
Retailer Shipping	-.0035 (.011)	.0042 (.0088)	-1.8*** (.23)
Days SKU offered	-.004*** (.00056)	-.0022*** (.00042)	
Retailer All Appearances	-8.0e-06 (.000018)	-.000039*** (.000013)	.0083*** (.00032)
Number of Markets	.012** (.0059)	-.0075 (.0061)	-.14 (.14)
Chat Tool	.41*** (.019)	.19*** (.015)	16*** (.31)
Call Center	.048*** (.01)	.00096 (.01)	-.43* (.23)
Showroom	-.091*** (.012)	-.019** (.0087)	-.67*** (.22)
Top Retailer	-.012 (.0085)	.023*** (.0082)	-.98*** (.2)
Constant	-.0038 (.01)	.051*** (.013)	-.049 (.29)
R-squared N cases	.16 7537	.21 2720	.37 7537
Retailer X SKU FE	+	+	+
Retailer Fixed Effects	-	-	-

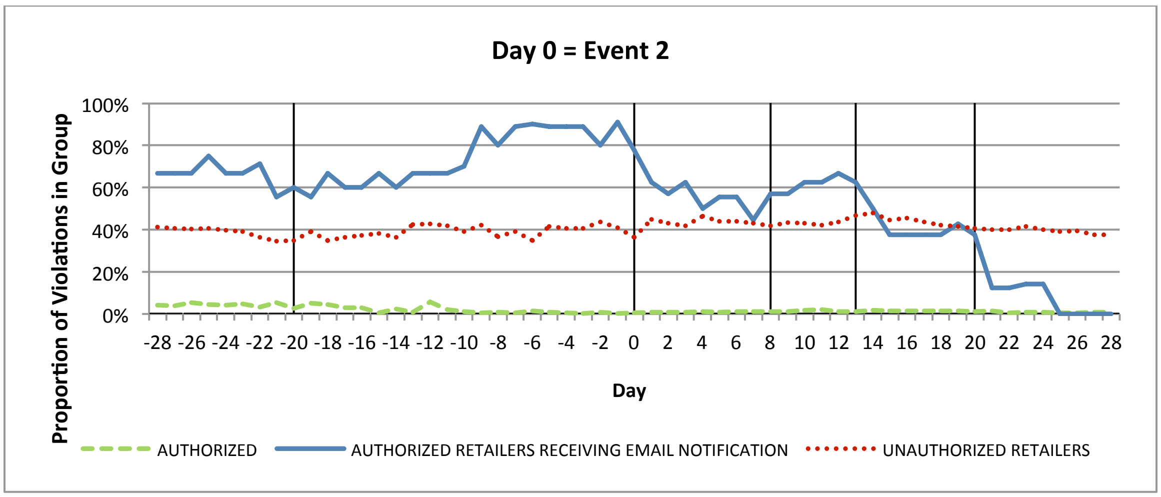
* p<0.10, ** p<0.05, *** p<0.01

Table A11 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 high versus low distribution of an SKU, for three 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), the average monthly violation depth (column 2), and the number of appearances of a SKU in a month (column 3). In columns 1-3 I use any SKU for a retailer that appeared both before and after 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 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 3, standard errors are clustered by retailer X SKU, and there are retailer X SKU fixed effects.

Exhibit A1 - Email Event Study Charts

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. Panel A the second event and illustrates events 1 through 5 for the group which were contacted in event 2, and Panel B presents the events of the test period.

Panel A – Event 2 Group



Panel B – Test Period