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Xiang Hui, Meng Liu, Tat Chan

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Targeted Incentives, Broad Impacts: Evidence from an E-commerce Platform

Abstract

Digital platforms sometimes offer incentives to a subset of sellers to nudge behavior, possibly affecting the behavior of all sellers in the equilibrium. In this paper, we study a policy change on a large e-commerce platform that offers financial incentives only to platform-certified sellers when they provide fast handling and generous return policies on their listings. We find that both targeted and non-targeted sellers become more likely to adopt the promoted behavior after the policy change. Exploiting a large number of markets on the platform, we find that in markets with a larger proportion of the targeted population—hence more affected by the policy change—non-targeted sellers are more likely to adopt the promoted behavior and experience a larger increase in sales and equilibrium prices. This finding is consistent with our key insight that a targeted incentive may increase demand for non-targeted sellers when both platform certificates and the promoted behaviors are quality signals. Our results have managerial implications for digital platforms that use targeted incentives.

Keywords: targeted incentives, quality provision, signalling, demand expansion.

Xiang Hui
Washington University
St. Louis / MO / USA
hui@wustl.edu

Meng Liu
Washington University
St. Louis / MO / USA
mengl@wustl.edu

Tat Chan
Washington University
St. Louis / MO / USA
chan@wustl.edu

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1 Introduction

Digital platform designers often use incentives to improve sellers' quality provision. For example, Amazon offers a 75% discount on storage fees to sellers who store their products in its warehouses, and Uber and Lyft give bonuses to drivers who accept consecutive rides during busy hours.[1] It can be costly for platforms to give universal incentives to all sellers, so, to reduce costs, platforms sometimes offer incentives only to a subset of sellers. We refer to this practice as targeted incentives. The trade-off, however, is that targeted incentives may have a limited effect on nudging sellers' quality provision because these incentives do not directly apply to non-targeted sellers. In addition, one can hypothesize that targeted incentives may reduce demand for non-targeted sellers by offering a cost advantage only to targeted sellers.

This paper studies how targeted incentives affect the market equilibrium. We first provide evidence of the effect of targeted incentives on the quality provision of both targeted and non-targeted groups. We then study the effect on market outcomes, such as quantity sold and equilibrium prices, to uncover the mechanisms behind the dynamics of the quality mix in the market as a result of targeted incentives.

Our study draws evidence from eBay, one of the largest e-commerce platforms in the world. We exploit a policy change in March 2012 when eBay started to exclusively provide its certified sellers, namely eBay Top Rated Sellers (eTRS hereafter), a 5% discount on selling fees on their listings if they offered 1-day handling and 14-day returns (premium service, or PS hereafter). The setting is ideal for studying our research questions for three reasons. First, eBay has more than 400 distinct product markets that differ in the share of targeted sellers (i.e., eTRS), which we exploit in the empirical analysis. Second, targeted sellers are chosen based on sellers' certification status, which is determined before the policy change and is relatively fixed in the short run. Lastly, the incentive promotes a specific behavior of sellers, so quality is well-defined and directly observed in the data. This differs from many papers that rely on consumer reports to measure quality. These unique features help us identify the causal effects of the targeted incentive.

Our empirical analyses start with an identification of the policy effect on seller quality provision by leveraging the rich data at the seller-week level. We first adopt an event study approach that is motivated by the sudden announcement and implementation of the policy. We find that the share of PS listings of targeted (i.e., eTRS) sellers increases by 55% after the policy change. The share

[1] https://www.cnet.com/news/to-power-prime-one-day-shipping-amazon-asks-sellers-to-send-it-more-stuff/; https://www.uber.com/blog/los-angeles/consecutive-trips-earnings/; (9/14/2020)

of PS listings of non-targeted (i.e., non-eTRS) sellers also increases by 36%. These findings suggest that the targeted incentive has spillover effects on the non-targeted group, whose quality provision also increases as a result.

To complement the event study that may suffer from identification threats such as seasonality, we leverage the large number of product subcategories on eBay to exploit the varying treatment intensities as defined by the presence of the targeted group (i.e., eTRS sellers) prior to the policy change. The reason is that product markets on eBay differ in the share of certified sellers and the focal policy does not affect a seller's ability to meet the certification criteria in the short run. This feature of eBay then motivates a continuous difference-in-differences (DiD) approach that exploits this cross-market variation and compare the temporal changes in outcomes in "more exposed" markets, where a larger share of transactions comes from the targeted eTRS sellers before the policy change, with the temporal changes in outcomes in "less exposed" markets. That is, we compare the before-after change of sellers that operate in markets more exposed to the policy to that of sellers that operate in markets less exposed to the policy, while controlling for seller-fixed effects and a list of market characteristics interacting the post dummy to account for seller-specific unobservables and alternative policy exposure measures. We find that both targeted and non-targeted sellers that operate in markets more exposed to the policy increase their PS offering more.

We propose a conceptual framework of targeted incentives to explain the spillover effects and guide the empirical tests of the mechanisms. A seller's type (i.e., eTRS status) is fixed in the short run, and the seller's quality provision (i.e., adopting the promoted PS or not) is endogenous. Consumers infer seller quality from two signals, namely eTRS and PS, and they make purchase decisions by comparing four substitute seller types — eTRS–PS, eTRS–non-PS, non-eTRS–PS, and non-eTRS–non-PS. The targeted incentive incentivises eTRS–non-PS sellers to offer PS, leading to an increased supply of eTRS–PS and a decreased supply of eTRS–non-PS (due to a fixed eTRS supply in the short run). The increasing price of eTRS–non-PS (due to its decreased supply) may cause a demand increase for non-eTRS–PS sellers, if consumers value the eTRS status and PS as substitute quality signals. Therefore, at the market equilibrium, (1) there can be an increased PS supply by both eTRS and non-eTRS sellers, and (2) the demand and price of non-eTRS–PS sellers can increase.

The potential increase in demand for non-targeted sellers may seem prima facie counterintuitive: in standard models of *targeted subsidies*, which are monetary transfers to sellers of a given type *regardless of* their behavior, the demand for non-subsidized firms will be unambiguously lower because the prices of their subsidized competitors will decrease.[2] However, our key insight is that this may no longer hold for *targeted incentives*, which are targeted subsidies contingent on behavior, if the targeted types and the promoted behavior are observed and valued by consumers. Specifically, if consumers regard non-eTRS–PS and eTRS–non-PS sellers as closer substitutes than non-eTRS–PS and eTRS–PS sellers, then by steering eTRS sellers towards offering PS, the targeted incentive can actually lessen competition for non-eTRS sellers, leading to a larger residual demand for them.

The finding that non-eTRS sellers also respond positively to the targeted incentive implies that the PS premium, as defined by the marginal effect of offering PS on sales probability, increases for non-targeted sellers as a result of the policy change. We estimate the PS premium by comparing almost identical listings that only differ in whether PS is offered. We find that the PS premium for non-eTRS sellers is indeed greater after the policy change and the increase in PS premium is greater in markets more exposed to the policy.

Motivated by the conceptual framework, we study the policy effect on market equilibrium outcomes for each of the four competing seller types: eTRS–PS, eTRS–non-PS, non-eTRS–PS, non-eTRS–non-PS. We find that the equilibrium changes for the four competing seller types are consistent with our theory prediction that allows for a consumer demand increase for non-targeted sellers conditional on quality provision, as a result of a targeted incentive. Specifically, we find a supply reallocation from eTRS–non-PS to eTRS–PS sellers, where supply is proxied by the number of listings (regardless of whether they sell). The result is an increased equilibrium quantity and decreased equilibrium price among eTRS–PS sellers, while the eTRS–non-PS sellers experience a drop in quantity sold and an increase in equilibrium price. Similar to our finding for eTRS sellers, we find a supply reallocation from non-eTRS–non-PS to non-eTRS–PS sellers. While our model does not unambiguously predict the demand change of non-eTRS–PS sellers, the finding on increasing equilibrium price implies an increasing demand for these sellers, because otherwise the increasing supply would have caused a price drop. Lastly, we find a decreasing supply, a decreasing equilibrium quantity, and an insignificant change in equilibrium price for non-eTRS–non-PS sellers.

Our findings have managerial implications for the use of targeted incentives on digital platforms. In terms of the effectiveness of nudging behavior, targeted incentives can affect the quality provision from both targeted and non-targeted sellers through market forces. A larger targeting size increases the spillover effect on the non-targeted group, causing them to improve quality further. Additionally, targeted incentives may increase demand for non-targeted sellers, especially those

[2] Examples of targeted subsidies are government subsidies for small firms and tax cuts for a sector of the economy.

who offer the promoted behavior, provided that the seller's status and the promoted behavior are quality signals valued by consumers. This finding suggests that, unlike offering targeted subsidies, offering targeted incentives does not necessarily give an unfair advantage to a selected group of sellers—a concern that some platform designers may have.

1.1 Related Literature and Contribution

Our paper contributes to several strands of the literature. First, our study of targeted incentives is related to a large literature on the effectiveness of consumer targeting, such as text message communications (e.g., Ansari and Mela (2003), Luo et al. (2014)), targeted price promotions (e.g., Feinberg et al. (2002), Zhang and Wedel (2009)), targeted online ads (e.g., Goldfarb and Tucker (2011), Lambrecht and Tucker (2013), Bleier and Eisenbeiss (2015)), as well as targeting based on contextual characteristics (e.g., Fong et al. (2015), Andrews et al. (2016)).[3] In particular, our finding that an incentive targeting a specific group of sellers can affect the behaviors of non-targeted sellers is similar to the documented spillover effects of targeted promotions in several recent studies. Fong et al. (2019) shows that targeted promotions can increase sales of promoted categories on an e-book platform while decreasing the sales of non-promoted categories, highlighting the spillover effect of targeted promotions in terms of forgone customer exploration and purchase of non-promoted content. Sahni et al. (2017) find that targeted discount offers on an online ticket resale platform can lead to more spending by the targeted population on both the discount-applicable categories as well as non-applicable categories. Fong (2017) find a similar spillover effect where targeted email offers tend to reduce consumer search of non-promoted products. Zhang et al. (2018) demonstrate that customers who receive promotions tend to become strategic in the long-run by adding more products to cart to induce more shopping-cart promotions and pay lower prices for future transactions, and consumers also exhibit these strategic behavior towards sellers who did not offer such promotions. McGranaghan et al. (2019) find that better lead offers lead to more subsequent consumer search and use of other offers in a coupon website. Liang et al. (2019) find spillover effects to non-featured products when platforms perform curated recommendations of certain products. Unlike the above studies, our focus is on the spillovers among sellers. Similar to the above studies, our findings also highlight the importance of accounting for the spillover effect as a result of such targeted incentives. However, we identify a different mechanism of the spillover effect compared to the literature, where

[3]See Rafieian and Yoganarasimhan (2021) for a discussion of the pros and cons of behavioral and contextual targeting from the perspective of an ad network.

the spillover effect is caused by a demand expansion for the non-targeted group. As we discuss in later sections, this demand expansion is driven by the fact that the targeted group and non-targeted group are competitors to begin with, and the exclusive promotion to the targeted group is conditional on user actions.

Next, our paper contributes to the literature that studies the effect of supply-side incentives.[4] Researchers have shown that financial incentives effectively encourage user contribution in posting reviews (e.g., Cabral and Li (2015), Fradkin et al. (2018), Sun et al. (2017), and Burtch et al. (2018)), in knowledge sharing (e.g., Kuang et al. (2019)), and in participating in open source software communities (e.g., Roberts et al. (2006)). Besides financial incentives, social incentives can also be effective motivators for user behaviors. For example, users post more reviews after being informed of the social norm (e.g., Chen et al. (2010) and Burtch et al. (2018)), and they generate more content when the size of their social network is larger (e.g., Zhang and Zhu (2011) and Shriver et al. (2013)). Ahn et al. (2011) and Kumar et al. (2014) build structural models of content generation in which the social network enters users' utility of content creation. Our paper contributes to this literature by studying the consequences of targeting incentives to only a subset of sellers, highlighting the potential to enhance overall quality at a lower cost.

Lastly, our mechanism on the demand expansion of non-targeted sellers relates to the literature that studies the demand expansion due to increased competition on platforms. In this literature, the increase in competition typically comes from new sellers' entry to the platform. For example, Cennamo et al. (2016) show that in the home-video-game market, competition can induce firms to create new product niches, expanding the product market for future consumers. Li and Agarwal (2017) show that the integration of Instagram on Facebook benefits large third-party applications on Facebook because of a larger customer base, while hurting the small ones because of higher competition. Similarly, Reshef (2019) documents that the entry of new restaurants on a food delivery platform increases the performance of high-quality incumbent businesses, because consumers have more options to choose from on the platform, while hurting low-quality incumbent businesses. Furthermore, Cao et al. (2018) show that entrants expand market demand for incumbents due to

[4] We use "incentive" to specifically mean the incentive payment contingent on seller/supplier behavior. This relates to and slightly contrasts with the literature on the effects of targeted firm subsidies (e.g., Rotemberg (2019)). These subsidies can be in the form of directed lending (e.g., Banerjee and Duflo (2014)), capital subsidies (e.g., Bergström (2000)), access to finance (e.g., Krishnan et al. (2015)), export facilitation (e.g., Hui (2019)), and procurement subsidies (e.g., Marion (2007)), among others. In this paper, we focus on targeted incentives, which are incentives contingent on seller behavior, instead of incentives given to seller just based on target type with no requirement on behavior. As we elaborate in later sections, this difference in incentive eligibility bears important implications on seller quality provision and market equilibrium outcomes. Most notably, we show that unlike targeted subsidies, targeted incentives may expand demand for the non-targeted sellers.

the network effect in the bike-sharing industry in China. Shen and Xiao (2014) and Yang (2019) study the learning effect of observing competitors' entry in the fast food industry. Lastly, there are a set of papers that show how various advertising/marketing activities of the focal brand can affect demand of competitors and/or other brands of the same firm, such as TV advertising (Shapiro (2018), Chesnes and Jin (2019)), direct mail marketing (Anderson and Simester (2013)), sponsored search advertising (Sahni (2016), Simonov et al. (2018)), online display advertising (Lewis and Nguyen (2015)), and word-of-mouth campaigns (Chae et al. (2017)). In this paper, we document a demand expansion effect due to targeted incentives, and importantly, we show that the demand expansion for the non-targeted group can take place even without entry of new market participants, network effects, or learning of the existence or quality of competitors.

2 Background and Data

The eBay Top Rated Seller (eTRS) is eBay's flagship certification program to reduce buyers' asymmetric information about seller quality. Sellers are evaluated on the 20th of each month and are eTRS if they pass a set of requirements, which are based on past sales (at least 100 items and $3,000 in sales in the previous year) and past quality (98% or higher positive consumer feedback, less than 0.5% of 1s and 2s on the 5-point Detailed Seller Ratings, and less than 0.5% or two buyer claims). Sellers who obtain the eTRS status enjoy several benefits. First, certified sellers get an eTRS badge, which is prominently displayed on every listing of the seller. Second, eTRS sellers get a 20% discount on the final value fee (i.e., eBay's commission fee). Lastly, the listings from eTRS sellers can appear higher on the product search results page.

On February 28, 2012, eBay initiated a platform-wide campaign to encourage sellers to offer fast shipping and generous return policies. The campaign was in effect from March 1, 2012, to May 31, 2012. During this three-month period, eBay offered eTRS sellers an additional 5% discount of the final value fee, besides the usual 20% discount, when an eTRS seller offered a 14-day (or more) money-back return policy *and* same-day or one-day handling for a listing (Premium Service, or PS henceforth). Note that the benefit is listing-specific: eTRS sellers get the policy benefit only for listings for which they offer the above-mentioned service. The benefit is also seller-specific — the incentive applies only to eTRS sellers. To offer PS, sellers choose the qualified handling days and return option when they list an item, and eBay automatically detects PS listings and applies the commission discount when these listings sell. While sellers' eTRS status can change over time, offering PS is not a requirement for the eTRS certification. Given this, we choose to define a seller's eTRS status based on her eTRS status from the February 20, 2012, to March 19, 2012, cycle and keep the eTRS status fixed for the same seller in our sample period. As reported above, seller eTRS status is mainly based on cumulative past consumer feedback, which is difficult for sellers to change in the immediate short run. In fact, the share of sellers whose post-policy eTRS status is the same as the pre-status is 88.6% in our sample. Therefore, treating eTRS as a time-invariant seller type is relatively innocuous for our short sample period, and doing so gains us more straightforward empirical analyses.

Consumers can learn about whether PS is offered by reading the information about the return option and handling time on the item listing page. The return specifics are found underneath the price and shipping information, as shown in Appendix Figure A1. To get the information on handling days, consumers need to scroll down the listing page and click on the "Shipping and payments" tab. The seller-specified handling time is listed at the bottom of this section, as exemplified in Figure A1.

To study the effect of the targeted incentive, we use internal data from eBay, which includes detailed listing attributes, transaction outcomes, product characteristics, buyer history, seller history, and feedback and reputation. Our data set covers the period from the 20 weeks before and the 13 weeks after the date of the policy change, March 1, 2012.[5] Besides, we remove extremely small and infrequent sellers from the data and focus on sellers who had sold at least \$5,000 in the year before the beginning of the three-month policy-effective period.

A key feature of the data that enables our identification is the large number of subcategories on eBay. There are 428 subcategories, such as "Household Supplies & Cleaning", "DVDs & Blu-ray Discs", "Men's Clothing", and "Cell Phones & Smartphones". Similar to Hui et al. (2017), we treat each subcategory as a separate market.[6] Markets on eBay differ significantly in size. We drop the smallest markets to prevent extreme values from affecting our results. The remaining 238 markets account for 98% of total gross merchandise value (GMV) on eBay during the 20-week pre-policy period.

In addition, our data allows us to directly observe the quality provision in terms of the promoted

[5] We choose to end the sample period at Week 13 because eBay announced a new policy in Week 14. This policy was designed to further incentivize sellers to improve on shipping and handling. Specifically, eTRS sellers would lose the 20% discount and higher position on the search results page for listings without PS.

[6] Note that eBay has a finer catalog, namely "leaf categories". For example, a leaf category within "Cell Phones & Smartphones" may identify a phone brand. However, this catalog can be too fine for defining markets, as it is unclear whether similar products such as Samsung phones and Google Pixel phones, for example, belong to two separate markets.

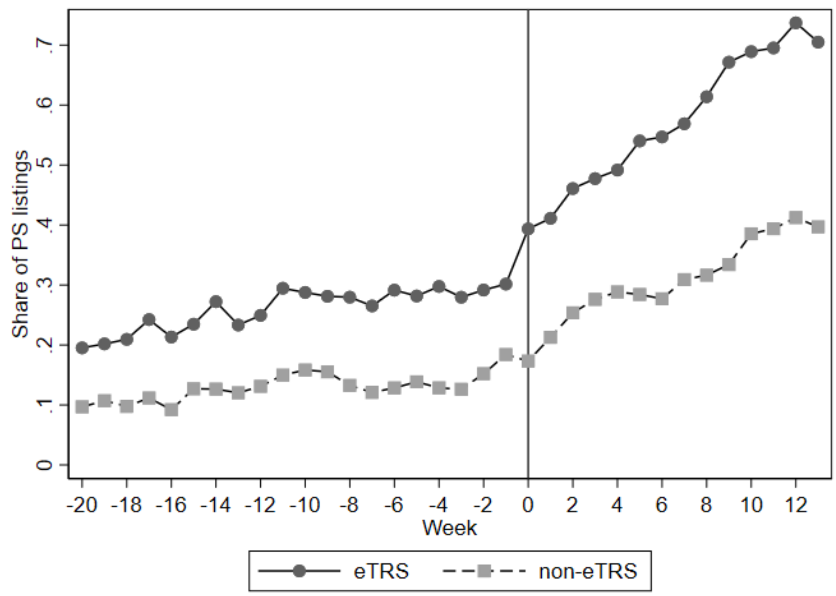


Figure 1: Share of Listings with Premium Service, eTRS vs. Non-eTRS

Notes: Week 0 is the policy week of March 1, 2012. The data consists of sellers who had sold at least $5,000 in the year before the beginning of our sample period.

behavior: for every listing, we observe whether PS is offered. This direct, listing-level measurement contrasts with previous literature which proxies quality provision with feedback and assumes that consumers' rating behavior is unaffected by the policy change. Henceforth, we refer to the listings that offer PS as "PS listings", and to the other listings as "non-PS listings".

In Figure 1, we plot the time series of the share of PS listings in the 20 weeks before and 13 weeks after the policy change. On the X-axis, "0" refers to the policy week and all the other weeks are normalized to this week. We make two main observations based on this graph. First, the share of PS listings is consistently higher for eTRS sellers than for non-eTRS sellers, consistent with the fact that eTRS sellers are of higher quality on average. Second, immediately after the introduction of the incentives targeted to eTRS sellers, both eTRS and non-eTRS sellers increase their PS offering in their listings. The fact that non-eTRS sellers are also more likely to offer PS after the policy change suggests that the targeted incentive has a spillover effect on the non-targeted group.

We report the summary statistics of our sample in Table 1. The sample to generate these summary statistics is at the seller-week level, from the 20 weeks before and the 13 weeks after the policy change. All the values in Table 1 except for Share of PS Listings are normalized with respect to the average of eTRS sellers in the pre-policy period. After the policy change, the number of

Table 1: Summary Statistics

	eTRS Sellers		Non-eTRS Sellers	
	Before	After	Before	After
Num. of Listings	1.00	0.93	0.59	0.54
Quantity Sold	1.00	0.89	0.42	0.38
Revenue	1.00	0.83	0.45	0.40
Share of PS Transactions	22%	52%	10%	25%

Notes: Averages for eTRS and non-eTRS sellers. The sample to generate these summary statistics is at the seller-week level. All values except for Share of PS Transactions are normalized with respect to the average of eTRS sellers in the pre-policy period.

listings decreases by about 7% (i.e., $(0.93-1)/1$) for eTRS sellers, and by about 8% for non-eTRS sellers ($(0.54-0.59)/0.59$). Because of seasonality in sales, one should be cautious of interpreting these as supply decreases due to the policy change. We find that eTRS sellers experience an 11% drop in quantity sold and 17% in revenue after the policy change, and the quantity sold drops by about 10% and revenue drops by about 11% for non-eTRS sellers. In addition, we observe a 136% increase (52%/22% -1) in PS listings among eTRS sellers and a 150% (25%/10% - 1) increase in PS listings among non-eTRS sellers, consistent with the patterns in Figure 1.

3 The Effect of Targeted Incentives on Seller Quality Provision

In this section, we identify the policy effect on seller quality provision as measured by the share of PS listings. Our objective is twofold: we first identify an overall effect of the policy on seller quality provision via an event study; we then study if there is a stronger effect on quality provision for sellers with greater policy exposure via a continuous difference-in-differences (DiD) approach. At the seller level, the event study to identify the overall policy effect essentially compares the seller's average outcome before the treatment and her average outcome after the treatment. Specifically, we adopt the following empirical model:

$$Y_{it} = \beta Post_t + \gamma t + \eta_i + \epsilon_{it}, \tag{1}$$

where Y_{it} denotes the share of PS listings of seller i in week t, and $Post_t$ is a dummy variable that equals 1 after the policy change and 0 otherwise. We use η_i to denote the seller fixed effect that captures any seller-specific unobservables. We include in the specification γt to allow for a linear time trend. We use ϵ_{it} to denote the idiosyncratic error term. Standard errors are clustered at the seller level.

The regression results are reported in column (1) and column (2) of Table 2, respectively, for eTRS sellers and non-eTRS sellers. In column (1), the coefficient estimate shows an increase of the share of PS listings among eTRS sellers of 12.2 percentage points. This effect is highly significant, as evidenced by the standard error. Given that the average PS provision of eTRS sellers before the policy change is 22%, this variation represents an increase of 55%. Similarly, non-eTRS sellers also increase their PS listings after the policy change. The estimate in column (2) suggests an average increase of 3.6 percentage points, which is statistically significant. Averaged at 10% prior to the policy change, the PS share among non-eTRS sellers increases by 36%. These results are consistent with our model-free evidence in Figure 1, and they show a strong policy effect on both eTRS and non-eTRS sellers that is not fully explained by a linear time trend.

Table 2: Share of PS Listings: Event Study

Dependent Variable: Share of Listings with PS		
	(1)	(2)
	eTRS	non-eTRS
Post	0.122***	0.0361***
	(0.000934)	(0.000741)
seller FE	Yes	Yes
linear time trend	Yes	Yes
adj. R-squared	0.652	0.594
observations	2613546	1841535

Sample at the seller-week level. Standard errors are clustered at the seller level.

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

Sellers who sell in a market with many sellers eligible for the incentives may be affected more than sellers in a market with few sellers targeted. To understand how the policy effect varies across sellers of different policy exposure, we leverage a continuous DiD identification that compares temporal changes in outcomes across sellers operating in markets with different target population sizes. Specifically, for each market, we construct the policy exposure measure using the share of transactions from eTRS sellers out of all transactions in the 20 weeks before the policy change.[7]. We then construct the seller-level exposure by averaging across markets the seller operates in,

[7] Using the share of eTRS sellers as an alternative policy exposure measure does not change the results qualitatively

weighted by the share of transactions the seller makes in each market. All data used to construct the seller-level exposure is from the 20-week pre-policy period.

This *ex ante* policy exposure essentially creates a continuum of treatment and control groups at the seller level, which we use to compare the temporal changes in the "more exposed" and "less exposed" seller groups:

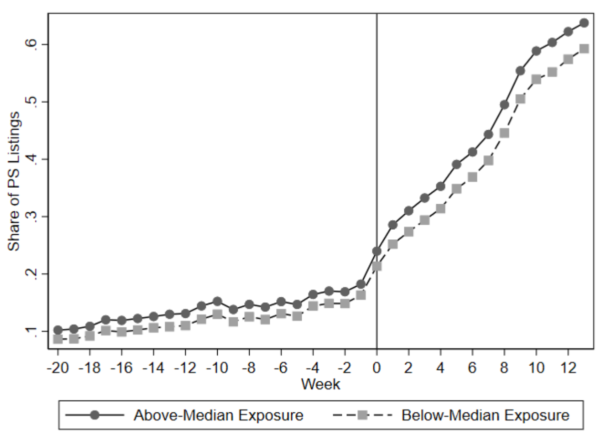
$$Y_{it} = \beta Share_i \times Post_t + \delta X_i \times Post_t + \eta_i + \xi_t + \epsilon_{it}, \tag{2}$$

where Y_{it} is the share of PS listings of seller i in week t; $Share_i$ is the seller-specific policy exposure measure defined above; $Post_t$ is a dummy variable that equals 1 after the policy change and 0 otherwise; η_i is the seller fixed effect that accounts for any seller-specific heterogeneity; ξ_t is the week fixed effect; and ϵ_{it} is the idiosyncratic error term. Our coefficient of interest β measures how the targeted population size affects the impact of targeted incentives. A major benefit of the continuous DiD approach, as opposed to the event study approach, is that the ξ_t term allows us to control for platform-wide time-specific unobservables. Throughout the analysis, the standard errors are clustered at the seller level to account for serial correlations and the heteroskedasticity of outcome variables for a given seller.

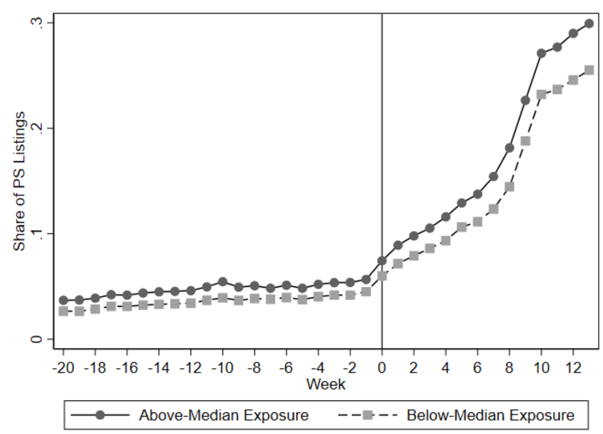
The identification assumption of Equation 2 is that the policy exposure affects seller quality provision only through differences in policy intensity because of the size of the targeted population. One may argue that instead of the target population size, other forces may drive the variation in PS increase and not including them may cause omitted variable bias to the focal intensity measure. For example, our policy exposure can be correlated with the market competition level, the quality distribution of sellers in a market, or the cost of providing PS. To verify that the target population size is indeed the treatment intensity, we include in the regression a set of alternative seller-specific exposure variables interacting with the post dummy, $\delta X_i \times Post_t$. If our policy exposure merely loads up alternative exposure measures, its effect will vanish once we include those alternative measures into the DiD specification.

Because these alternative policy measures should be correlated with our policy measure, we consider the following characteristics (and add their interation with the Post dummy into the DiD regression): the seller's average price, her total GMV, her PS share, her average Effective Percentage Positive (EPP, or the number of positive ratings divided by total transactions as defined in Nosko and Tadelis (2015)), her share of low Detailed Seller Ratings (DSR) (e.g., 1s and 2s on a 5-point scale), her share of consumer claims, her average package size in cubic inches, her average package weight in ounces, and her average postage fees in USD. Note that these measures are all constructed using the seller's pre-policy data, so that they are not affected by the policy. In Appendix Table A1, we report the correlation coefficients between each of these alternative policy exposure and our policy exposure, separately at the market level and at the seller level. We confirm that these characteristics are all somewhat correlated with our policy measure.

Like any difference-in-differences exercises, our approach cannot control for time-varying *and* seller-specific error terms that could be correlated with the policy exposure variable (e.g., sellers with high policy exposure are those that experience a faster growth rate in quality provision). We provide evidence of parallel trends in Figure 2, where we split eTRS and non-eTRS sellers into "more treated" and "less treated" groups by their policy exposure. The pre-policy periods exhibit parallel trends between "more treated" and "less treated" groups and the gap appears to increase only after the policy change, for both eTRS sellers and non-eTRS sellers.



(a) eTRS sellers



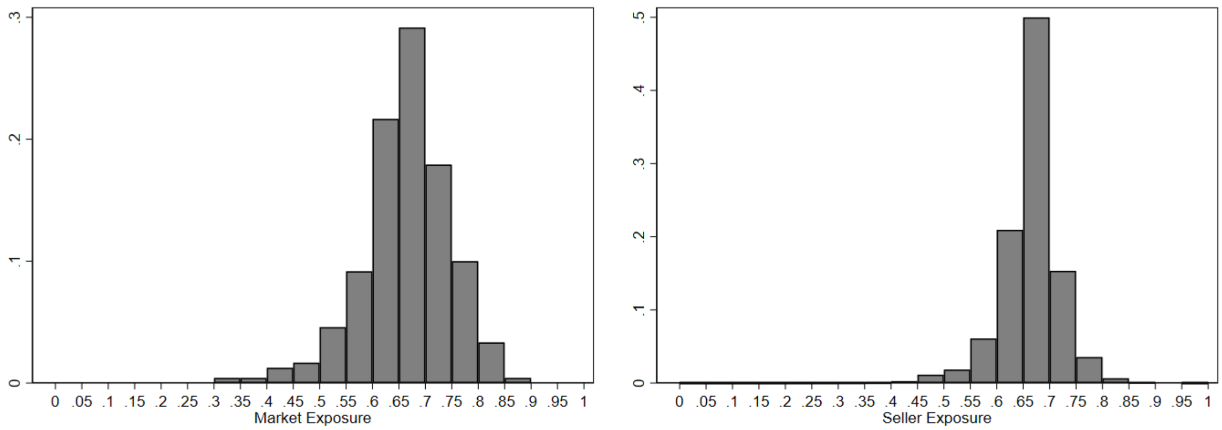
(b) Non-eTRS sellers

Figure 2: Parallel trends

Notes: Week 0 is the policy week of March 1, 2012.

For the continuous DiD to work, we also need sufficient variation in the policy exposure. To confirm this, we plot the distribution of the *ex ante* share of eTRS transactions across 238 markets on eBay in Figure 3a. In most markets, eTRS transactions account for more than half of all transactions, but their share varies significantly across markets. The variation of seller-level policy exposure is shown in Figure 3b. On both levels, we observe a meaningful amount of variation.

The estimation results of Equation 2 are reported in Table 3. In columns (1) and (2), we



(a) Market level (b) Seller level

Figure 3: Distribution of Policy Exposures

Notes: The figures plot the distribution of policy exposure, i.e., the share of transactions from eTRS sellers, across the 238 markets in our sample, using the 20-week pre-policy data. Figure on the left is the distribution across markets. Figure on the right is the distribution across sellers, where the intensity of each seller is weighted by her transactions in each of her markets.

Table 3: Share of PS Listings: DiD

Dependent Variable: Share of Listings with PS				
	DiD		DiD with controls	
	(1)	(2)	(3)	(4)
	eTRS	non-eTRS	eTRS	non-eTRS
Share × Post	0.349***	0.311***	0.316***	0.272***
	(0.0210)	(0.0176)	(0.0212)	(0.0177)
seller FE	Yes	Yes	Yes	Yes
week FE	Yes	Yes	Yes	Yes
adj. R-squared	0.674	0.614	0.676	0.616
observations	2613546	1841535	2613546	1841535

Sample at the seller-week level. Standard errors are clustered at the seller level.
* p<0.10, ** p<0.05, *** p<0.01

estimate Equation 2 without $X_i \times Post_t$. We find that for both eTRS and non-eTRS sellers, sellers with greater policy exposure increase their PS listings more. Specifically, for eTRS sellers, a 10 percentage points increase in policy exposure leads to an increase of 3.5 percentage points in PS listings (0.349 × 0.1), or an increase by 15.9% (= 3.5/22, where 22 is the average PS percentage share among eTRS sellers before the policy change). For non-eTRS sellers, PS listings increase by 3.1 percentage points, or by 31% more (= 3.1/10), when the sellers are exposed to the policy by 10 percentage points more. As discussed earlier, we add $X_i \times Post_t$ to the DiD specification in columns (3) and (4) in order to tease out possible effects from other sources. In Table 3 we report only the variable of interest, $Share_i \times Post_t$, to save space. Detailed estimates of these additional controls are reported in Table A2 in the appendix. We find that the DiD results with additional controls are consistent with the baseline estimates in columns (1) and (2), and the size of the estimates becomes slightly smaller, while the difference is insignificant. The evidence in Table 3 provides strong support for an uneven growth of quality improvement across sellers, where both targeted and non-targeted sellers with greater policy exposure tend to have faster quality improvement.

To check the robustness of our findings, we redo the event study and DiD estimation at the market level to show consistency of results across different levels of observations. Like before, we split sellers into eTRS and non-eTRS and then aggregate the data at the market level. Here, policy exposure is the market-specific share of eTRS transactions in the pre-policy data. For both the event study and the DiD, we control for market fixed effects and week fixed effects, and cluster standard errors at the market level. We find that the parallel pre-trends seem to hold, as reported in Figure A2 in the appendix. Overall, we find that the regression results reported in Table 4 are consistent with the seller-level analysis in Table 3: both eTRS and non-eTRS sellers increase their PS offering and the increase is greater in markets more exposed to the policy. In columns (5) and (6) we include a set of market-specific characteristics interacting with the post dummy. The coefficient estimates of these variables are reported in Table A6 in the appendix, to save space. Specifically, these additional controls are based on the pre-data and include market average price, market total GMV, market PS share, average seller EPP of the market, average share of sellers' low DSR of the market, average share of sellers' consumer claims of the market, HHI of the market, average package size of the market, average package weight of the market, and average postage cost of the market. We find that adding these confounders does not absorb the effect of policy exposure.

We have focused on the share of listings that offer PS in the above analyses. Another related outcome is the timing of PS adoption. A more exposed market is likely to have not only more sellers adopting PS but also at a faster pace. Sellers on eBay usually sell in multiple markets, and a median seller sells in seven different product markets. To provide more visibility into sellers' decisions on quality provision, we analyze the timing of PS adoption across the markets a given seller operates in. In particular, we look into whether a seller adopts PS earlier on in her product markets with greater policy exposure. This boils down to a regression analysis at the seller-market level where the dependent variable is the first week of PS adoption and the key explanatory variable

Table 4: Share of PS listings: Event Study and DiD at the Market Level

Dependent Variable: Share of PS listings						
	Event study		DiD		DiD with controls	
	(1)	(2)	(3)	(4)	(5)	(6)
	eTRS	non-eTRS	eTRS	non-eTRS	eTRS	non-eTRS
Post	0.134***	0.0642***				
	(0.00651)	(0.00461)				
Share × Post			0.350***	0.278***	0.538***	0.478***
			(0.103)	(0.0670)	(0.152)	(0.111)
Market FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	No	No	Yes	Yes	Yes	Yes
linear time trend	Yes	Yes	No	No	No	No
adj R-squared	0.785	0.714	0.834	0.762	0.848	0.770
observations	8092	8092	8092	8092	8092	8092

Sample at the market-week level. Standard errors are clustered at the market level.
* p<0.10, ** p<0.05, *** p<0.01

is the policy exposure of the market, while controlling for seller fixed effects. In doing this, we also control for whether the product market is the top product market of the seller by GMV, under the intuition that sellers may "feel the market" earlier in their main markets. In addition, we control for a set of market-specific characteristics that potentially confounds the focal relationship, which includes the average price, total GMV, PS share, average seller EPP, share of low DSR, share of claims, the HHI of the market, averge package size, average package weight, and the average postage. The sample consists of the top 10 markets among sellers who sell in at least 10 markets based on pre-policy data; the sample represents 40% of all unique sellers in our main data set. In Table A3 of the appendix, we report qualitatively similar results on the sample of the top three markets among sellers who sell in at least three markets; that sample covers 75% of all unique sellers.

The regression results are presented in Table 5. For eTRS sellers, the coefficient estimate of *Share* is -0.85, suggesting that on average an eTRS seller adopts PS 0.85 weeks earlier in the most exposed than in the least exposed market she operates in. We also find that eTRS sellers tend to offer PS 6.29 weeks earlier in their top product markets. In column (2), we allow for the interaction of policy exposure and top product dummy, whose coefficient estimate is negative and significant. In particular, the estimate suggests that for the top product market, an eTRS seller is expected to offer PS 5.60 weeks earlier ($= -0.70 - 4.90$) when the market has complete policy exposure than when it has none. As reported in columns (3) and (4), we find that the above-mentioned correlations are also present among non-eTRS sellers. In particular, both policy exposure and top product correlate with earlier PS adoption. In addition, the correlation with policy exposure is much stronger in the top product market, indicating an early PS adoption by 3.42 weeks ($= 3.13 + 0.29$) when the top product market has complete policy exposure than when it has none. For all specifications, we include a set of market-specific attributes (i.e., the average price, total GMV, PS share, average seller EPP, share of low DSR, share of claims, the HHI of the market, average package size, average package weight, and average postage) and report their coefficient estimates in Table A4 of the appendix, to save space. As an alternative specification, we also control for product rank, an integer variable that ranges from 1 to 10, interacting with policy exposure and find qualitatively similar results (reported in Table A5 in the appendix). In summary, we find strong evidence that both eTRS and non-eTRS sellers start offering PS earlier in markets more exposed to the policy, and this effect is much more pronounced in their top product markets. We interpret these patterns as consistent with and complementary to the DiD results by leveraging the within-seller variation across markets that differ in policy exposure.

Table 5: Sellers' Timing of PS Adoption Across Their Product Markets

Dependent Variable: First Week Offering PS	eTRS Sellers		Non-eTRS Sellers	
	(1)	(2)	(3)	(4)
Share	-0.852***	-0.701***	-0.408***	-0.285***
	(0.0782)	(0.0778)	(0.0584)	(0.0578)
Top Product (0/1)	-6.287***	-2.982***	-2.215***	-0.138
	(0.0273)	(0.295)	(0.0192)	(0.180)
Share × Top Product		-4.898***		-3.129***
		(0.436)		(0.272)
Seller FE	Yes	Yes	Yes	Yes
adj R-squared	0.575	0.575	0.577	0.577
observations	2120574	2120574	1728383	1728383

Sample at the seller-market level. Standard errors are clustered at the seller level.
* p<0.10, ** p<0.05, *** p<0.01

Why do non-eTRS sellers improve quality even though they do not get the monetary incentive per the policy? In addition, why do non-eTRS sellers increase quality more in markets with more eTRS presence to begin with? In the following sections, we investigate the underlying mechanisms of the observed quality provision of non-eTRS sellers. We first provide a conceptual framework and discuss its predictions in Section 4. We then study the PS premium for non-eTRS sellers in Section 5. Finally, we study how targeted incentives affect the market equilibrium in Section 6.

4 A Conceptual Framework

In this section, we discuss a simple conceptual framework to explain the previous empirical findings and guide the subsequent empirical analyses on the mechanisms. In this framework, a seller's target type (i.e., eTRS status) is fixed and the seller's quality provision (i.e., offering PS or not) is endogenous.[8] Consumers differentially value seller quality but do not observe it. Instead, they rely on signals of seller quality, namely sellers' eTRS status and whether they offer PS, to make purchase decisions among four competing types of sellers—eTRS–PS, non-eTRS–PS, eTRS–non-PS, and non-eTRS–non-PS.

For clarity of illustration, we develop the framework in three steps. First, we provide a benchmark model of targeted subsidies. Next, we modify it to study targeted incentives, in which targeted sellers receive compensation *only if* they adopt the promoted behavior. Lastly, we further modify the model to study how quality provision and market outcomes are affected by different treatment intensities in terms of the ex ante eTRS presence.

Starting with the first step, suppose eBay offers a targeted subsidy to eTRS sellers. This decreases their production cost and therefore increases their supply (i.e., an outward shift of their supply curve). Demand for non-eTRS sellers decreases (i.e., an inward shift of their demand curve) because the price of their subsidized competitors decreases. Therefore, targeted subsidies unambiguously reduce demand for non-targeted sellers.

In the second step, consider the case of a targeted incentive and suppose that eBay compensates eTRS sellers *only if* they adopt PS. The direct effect of this targeted incentive is to encourage eTRS sellers to adopt PS, leading to an increase in supply among eTRS–PS sellers and a decrease in supply among eTRS–non-PS sellers. This reallocation in eTRS sellers' supply decreases the equilibrium price among eTRS–PS sellers but *increases* the equilibrium price among eTRS–non-PS sellers.[9] What is the effect on demand for non-eTRS sellers in equilibrium? There can be two cases. First,

[8] We do not allow for entry and exit in this model, to keep it tractable. We provide empirical support to this assumption by observing no significant increase or decrease in the number of eTRS or non-eTRS sellers as a result of the policy change. Regression results are reported in Table A10 and a discussion is provided in Section 6.

[9] It is assumed that the policy does not directly affect the demand curves for eTRS sellers.

if consumers treat the eTRS status and PS as two independent seller characteristics, as in the standard choice model, then non-eTRS–PS sellers are more substitutable with eTRS–PS sellers than with eTRS–non-PS sellers. In this case, the residual demand for non-eTRS–PS sellers will decrease because the price of their closest substitute decreases, leading to lower price and quantity sold. As the residual demand for non-eTRS-PS sellers decreases, in the new market equilibrium non-eTRS sellers will reduce PS offering, and therefore the share of non-eTRS-PS will decrease after the policy change. However, this is inconsistent with the empirical results presented in Section 6.

In the second case, assuming buyers have asymmetric information about seller quality in markets, they could view eTRS and PS as similarly good signals for seller quality. Then the substitutability between eTRS–non-PS and non-eTRS–PS sellers will be higher (than that between eTRS–PS and non-eTRS–PS sellers) because both have one quality signal. In this case, the residual demand for non-eTRS–PS sellers will *increase* because the price of their closest substitute increases, leading to higher price and quantity sold. The residual demand for non-eTRS–non-PS sellers may or may not increase, because their substitutability with eTRS–non-PS sellers is smaller than that with non-eTRS–PS sellers.[10] In summary, non-eTRS sellers will increase PS provision in the new market equilibrium, and this is consistent with the results in Section 6.

There could be other mechanisms that explain the increase in the share and adoption timing of non-eTRS-PS sellers in Section 3. For example, it could be driven by non-eTRS sellers following eTRS sellers after observing more of these sellers offering PS. We argue what drives the increase in share and adoption is that the residual demand for non-eTRS-PS sellers has increased after the policy change. To find evidence to support that argument, we define "PS premium" as the difference in the sales probability between a product with PS and an otherwise identical product without PS. In the first case discussed above, demand for non-eTRS–PS sellers decreases, while demand for non-eTRS–non-PS sellers increases, which leads to a decreasing PS premium among non-eTRS sellers. Given this, non-eTRS sellers will shift their supply from PS to non-PS. In the second case, however, demand for non-eTRS–PS sellers increases, while for non-eTRS–non-PS sellers it increases less. This implies an increasing PS premium among non-eTRS sellers, and, as a result, non-eTRS sellers will shift their supply from non-PS to PS. In this case, the targeted incentive creates a positive spillover effect on non-targeted sellers both in terms of incentivizing

[10] This can also be interpreted through a standard vertical differentiation model. Suppose there are three quality levels: high (eTRS–PS), medium (eTRS–non-PS, non-eTRS–PS), and low (non-eTRS–non-PS). When there are more eTRS–PS sellers and fewer eTRS–non-PS sellers, competition becomes less fierce for both types of non-eTRS sellers, but its extent is larger for non-eTRS–PS sellers, as they are closer to eTRS–non-PS sellers on the vertical line.

their quality provision and increasing their residual demand.

In the last step, we expand this framework to discuss how the impacts of the targeted incentive on market equilibrium vary across markets that differ in *ex ante* eTRS presence. This will not only provide evidence on the proposed mechanisms, but also enhance managerial implications. We argue that the equilibrium changes in quality provision and market outcomes laid out above are more salient in markets with greater *ex ante* eTRS presence. This is because the initial supply increase in eTRS-PS, as induced by the targeted incentive, is stronger in markets with greater eTRS presence to begin with. This stronger initial exposure to incentives will enable a stronger series of changes, as laid out above.

5 The Effect of Targeted Incentives on the PS Premium

Our conceptual framework implies that the changes in non-eTRS sellers' quality provision across markets should be consistent with the changes in the PS premium — the increased sales probability due to PS. For example, if non-eTRS sellers offer more PS listings in markets with high policy exposure, it must be because the PS premium for non-eTRS sellers becomes larger in these markets. To estimate the PS premium, we use an identification strategy that matches listings which can be considered identical except for whether the PS is offered. We construct a strict matching of listings. This is necessary because PS listings and non-PS listings possibly differ in ways that correlate with both demand and the propensity of offering PS. For example, the decision of whether to offer PS may depend on the price and shipping cost of the item, which affect demand. Seller heterogeneity could also confound the estimate if high-quality sellers are more likely to offer PS than lower-quality sellers are, while demand is presumably higher for high-quality sellers. Lastly, time-varying market conditions can also affect consumer demand.

To mitigate the above-mentioned concerns, we match the listings in several key components to control for product-, seller-, and market-level heterogeneity as much as we can. Following Elfenbein et al. (2012)) and Einav et al. (2015), we match the listings based on the following variables: seller identity, item listing title, item listing subtitle, item's leaf category on eBay, sales price, and listing start week. In addition, these matched listings must have variation in whether PS is offered. As argued by these authors, matched listings can be thought of as instances where eBay sellers experiment with sales parameters (i.e., whether to offer PS in this case) to understand consumers' preferences. Having constructed these matched sets of listings, we then exploit the within-match variation in whether PS is offered to identify its effect on the sales probability using the following equation:

$$Success_{ij} = \gamma PS_{ij} + \mu_i + \nu_{ij}, \tag{3}$$

where $Success_{ij}$ is an indicator for whether listing j within the matched set i is sold; PS_{ij} is the dummy variable for whether listing j in matched set i offers PS; μ_i is the fixed effects for the matched set of listings; and ν_{ij} is random error. Our coefficient of interest is γ, which measures the premium that consumers attach to PS for otherwise identical listings. We estimate the equation separately for the pre-policy period and the post-policy period.

Lastly, to study how the PS premium varies across markets with different policy exposure, we modify equation 3 to

$$Success_{ij} = \alpha PS_{ij} + \lambda PS_{ij} \times Share_m + \mu_i + \nu_{ij}, \tag{4}$$

where $Share_m$ is the policy exposure of the market in which the product is listed. Our coefficient of interest is λ, which captures the varying consumer demand for PS across markets of varying policy exposure. We estimate the equation separately for the pre-policy period and the post-policy period.

Table 6: PS Premium

	(1) Pre	(2) Post	(3) Pre	(4) Post
PS	0.0243*** (0.000392)	0.0310*** (0.00116)	0.0319*** (0.00421)	-0.0855*** (0.0124)
Share × PS			-0.0109* (0.00596)	0.170*** (0.0181)
Match FE	Yes	Yes	Yes	Yes
$H_0 : \Delta_{Pre} = \Delta_{Post}$	Coefficient on PS		Coefficient on Share × PS	
F-test (p-value)	<0.001		<0.001	
adj R-squared	0.000525	0.00166	0.000525	0.00180
observations	6060038	703611	6060038	703611
number of clusters	392752	84748	392752	84748

Notes: One observation is a listing. Standard errors clustered at the match level.
* p<0.10, ** p<0.05, *** p<0.01

We use matched listings of non-eTRS sellers from the 20 weeks before and 13 weeks after the policy change to estimate Equation 3 and Equation 4. Column (1) in Table 6 shows that before the policy change, offering PS increases the sales probability by 2.4 percentage points for non-eTRS sellers. This is equivalent to an 18% increase from the baseline sales probability of 13.3%. As column (2) shows, the PS premium increases to 3.1 percentage points after the policy change. To test the null hypothesis that the PS premium is the same for the pre and the post periods, we use an F-test for the two coefficients on "PS" for the two periods. We reject the null hypothesis at the 1% significance level (p-value<0.001). Next, we study how the PS premium differs across markets with varying policy exposure and report the results in columns (3) and (4). Starting with column (4), we find that in markets with 10 percentage points more ex-ante eTRS transactions, the PS premium for non-eTRS sellers is 1.7 percentage points higher. Note that estimates in column (4) implies that, for an average market with "Share" around 68%, the PS premium would be around 0.03 ($= -0.0855 + 0.170 * 0.68$), which is broadly consistent with the estimate in column (2). The results in column (4) shows a larger PS premium for non-eTRS sellers in markets more affected by the policy, which suggests a larger demand expansion for these sellers after the policy change. However, the results in column (3) show an opposite but small across-market difference in the PS premium before the policy change: the PS premium is 0.1 percentage point lower in markets with 10 percentage points larger ex-ante share of eTRS transactions, and the result is only borderline significant. Note again that the estimates in column (3) indicates a PS premium of around 0.024 ($= 0.0319 - 0.0109 * 0.68$) for an average market, consistent with the estimate in column (1). The two sets of results in columns (3) and (4) show that the across-market pattern meaningfully exists only *after* the policy change (F-test of the equality of the two coefficients on "Share PS" yields a p-value less than 0.001), suggesting that the result is not driven by omitted variables that correlate with both policy exposure and PS premium across markets.

As a robustness check, we repeat the matched listing analysis using only the four weeks before and four weeks after the policy change. We use a shorter time period to capture the exogenous shock to the market around the policy cutoff. The results, reported in Table A7 in the online appendix, show qualitatively similar results on changes in the PS premium, both overall and across markets.

6 The Effect of Targeted Incentives on Aggregate Market Outcomes

We have so far shown the effects of the policy change at the seller and listing levels. In this section, we study the aggregate market-level changes across the four seller types that are substitutes from the consumers' perspective, namely eTRS–PS, eTRS–non-PS, non-eTRS–PS, and non-eTRS–non-PS, following the conceptual framework we discussed before. Specifically, we study the effect of the policy change on the supply, as measured by the number of listings, quantity sold, and average price, of each type of sellers in each market. Changes of these aggregate outcomes can help us infer how the demand shifted in each market at the new equilibrium. As for the analysis in Section 3, we leverage the continuous DiD to estimate the policy effect on the market equilibrium as evidenced by differential changes across product markets of varying policy exposure. Specifically, we adopt the following DiD specification at the market-week level:

$$ln(Y_{mt}) = \beta Share_m \times Post_t + \eta_m + \xi_t + \epsilon_{mt}, \tag{5}$$

where Y_{mt} is the market outcome of market m in week t; $Share_m$ is the market-specific policy exposure measure defined previously; $Post_t$ is a dummy variable that equals 1 after the policy change and 0 otherwise; η_m is the market fixed effect that accounts for any market-specific heterogeneity; ξ_t is the week fixed effect; and ϵ_{mt} is the idiosyncratic error term. Our coefficient of interest β measures how the market equilibrium responds to varying treatment intensities across markets. The leads-and-lags analysis results, reported in Table A8 and Table A9 in the appendix, suggest that the effect mainly happens after the policy.

We first discuss the market outcome changes of eTRS–PS and eTRS–non-PS, as reported in Table 7. We start with eTRS–PS sellers, and in column (1) we report the estimation results on the differential changes in supply as measured by the logged new listings in a week. The estimate is positive and statistically significant, indicating that the percentage change in supply is higher in markets with greater policy exposure. Specifically, the coefficient estimate suggests an increased supply of eTRS–PS listings by 0.56% if the policy exposure of the market increases by 10%. Similarly, column (2) shows that the percentage change in quantity sold is higher in markets more exposed to the policy. In addition, we find that the average sales price drops in markets more exposed to the policy after the policy change. These results are consistent with our conceptual framework: in markets with greater policy exposure, the targeted incentive causes a larger supply increase among eTRS–PS sellers, leading to a larger increase in the equilibrium quantity and a larger decrease in the equilibrium price.

Column (4) shows that for eTRS–non-PS sellers, in a market with 10% higher policy exposure, the supply decreases by 0.96%. This finding is consistent with the fact that more eTRS sellers start providing PS in markets with a larger targeted population, which naturally leads to a reduction in eTRS–non-PS listings. Similarly, column (5) shows that the equilibrium quantity of eTRS–non-PS sellers also decreases more in markets more exposed to the policy. Lastly, column (6) shows that the average sales price increases by 0.73% more in markets with 10% higher policy exposure. The results for eTRS–non-PS sellers are consistent with a larger supply decrease, and thus a greater price increase in markets more exposed to the policy. The price result among eTRS–non-PS sellers is critical, because it is a necessary condition for a potential demand increase in non-eTRS markets, for which we report evidence in the following discussions.

Table 7: Market Equilibrium: eTRS Sellers

		PS = 1			*PS = 0*	
	(1) ln(num. listings)	(2) ln(quantity sold)	(3) ln(sales price)	(4) ln(num. listings)	(5) ln(quantity sold)	(6) ln(sales price)
Share × Post	0.0556***	0.0528***	-0.0739***	-0.0956***	-0.0777***	0.0733***
	(0.00580)	(0.00702)	(0.00819)	(0.00593)	(0.00704)	(0.00802)
Market FE	Yes	Yes	Yes	Yes	Yes	Yes
week FE	Yes	Yes	Yes	Yes	Yes	Yes
adj R-squared	0.384	0.357	0.587	0.776	0.724	0.726
observations	8092	8092	8092	8092	8092	8092

Sample at the market-week level. Standard errors are clustered at the market level.
* p<0.10, ** p<0.05, *** p<0.01

We now study the policy effect on aggregate outcomes among non-eTRS–PS sellers. Given the higher equilibrium price for eTRS–non-PS sellers and consequently larger PS premium for non-eTRS sellers in markets more affected by the policy, our conceptual framework predicts that non-eTRS sellers should offer more PS in these markets. The regression results are reported in Table 8. Columns (1) and (2) show that among non-eTRS–PS sellers, both supply and equilibrium quantity increase more in markets with greater policy exposure. Column (3) shows evidence of increasing average sales price in markets more exposed to the policy. The evidence suggests that

Table 8: Market Equilibrium: Non-eTRS Sellers

	PS = 1			PS = 0		
	(1) ln(num. listings)	(2) ln(quantity sold)	(3) ln(sales price)	(4) ln(num. listings)	(5) ln(quantity sold)	(6) ln(sales price)
Share × Post	0.125***	0.0998***	0.0232*	-0.0778***	-0.0659***	-0.00770
	(0.00971)	(0.00850)	(0.0137)	(0.00550)	(0.00485)	(0.0138)
Market FE	Yes	Yes	Yes	Yes	Yes	Yes
week FE	Yes	Yes	Yes	Yes	Yes	Yes
adj R-squared	0.486	0.498	0.524	0.702	0.663	0.405
observations	8092	8092	8092	8092	8092	8092

Sample at the market-week level. Standard errors are clustered at the market level.

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

demand increases for non-eTRS–PS sellers, because otherwise we would observe a price drop in markets more exposed to the policy, instead of a price increase. This rising demand is consistent with a larger PS premium for non-eTRS sellers, as we find in Section 5. Moreover, it is also consistent with the finding that the price of non-eTRS–PS sellers' closest substitute (i.e., eTRS–non-PS sellers) increases more in markets more affected by the policy, as shown in column (6) in Table 7.

Next, we study the changes in equilibrium outcomes among non-eTRS–non-PS sellers. As column (4) shows, the supply decreases in markets more exposed to the policy, which is consistent with the increasing supply of non-eTRS–PS sellers in markets more exposed to the policy. Similarly, column (5) shows a decreasing aggregate quantity sold in markets more exposed to the policy. We find that the policy effect on the average sales price, as reported in column (6), is slightly negative and insignificant. A decreasing equilibrium quality with a slight or no change in price, coupled with a decreasing supply, implies a somewhat moderate demand decrease. This is because (1) if the demand increased, the equilibrium price should increase significantly; (2) if the demand decreased substantially, the equilibrium price should drop significantly. Therefore, the results for non-eTRS–non-PS sellers can be interpreted as supply-driven, where the supply decrease dominates the changes in demand. These results are consistent with our conceptual framework. Lastly, we show that the changes in equilibrium outcomes are unlikely driven by entry and exit of eTRS and non-eTRS sellers. Specifically, the number of eTRS and non-eTRS sellers that have at least one listing do not change significantly across markets (results reported in Table A10).

In Section 3, we discuss the potential identification threat to the DiD specification from factors correlated with our policy exposure that can explain the results. The same critique applies to the identification of this section as well. Therefore, We provide robustness by including the same set of controls interacting the post dummy in Equation 5 and regression results are reported in Table A11 and Table A12. Overall, we find that the estimates in Table 7 and Table 8 remain qualitatively the same when additional controls are accounted for.

The most striking finding in this section is that non-eTRS–PS sellers are able to sell more at a higher price as a result of the policy change. This finding highlights a key insight that the demand for non-targeted sellers who adopt the promoted behavior can in fact *increase* even though the policy's monetary incentive is offered only to targeted sellers. However, this result critically depends on how consumers value the promoted behavior as a signal of seller quality vis-à-vis the existing certification. If consumers regard PS as a strong quality signal, then non-targeted sellers can attract more demand by adopting the promoted behavior. This finding is in contrast with an unambiguous decrease in demand for non-targeted sellers, as a targeted subsidy model would predict.

Lastly, we provide evidence on the total supply and total demand at the platform level as a result of the policy change. We do this by running our main DiD regression on the market-week sample, with the outcome being market-week-specific supply (as measured by the logged listings), logged quantity sold, and logged GMV. Results are reported in Table 9 and the robustness check with additional controls interacting the post dummy is provided in Table A13. We observed that the policy had a positive effect on the number of listings. Specifically, a market with a 10 percentage points higher policy exposure is expected to have 0.6% increase in listings, other things held constant. Given that we find no evidence of seller entry or exit as a result of the policy (Table A10), the increased listings suggest that *existing* sellers on eBay are more motivated after the policy and increase their supply. The increase in total quantity sold (Column (2)) is similar in size as the effect on supply. However, the increase in total GMV (Column (3)) is not as pronounced as the increase in supply or quantity sold, indicating a decreased average price in the presence of increased supply. Evidence here suggests a greater overall demand expansion in more exposed markets. In response to that and also the incentive offered by the platform, the number of listings has also increased more in more exposed markets.

Table 9: Policy Effect on Market Totals

	(1) ln(num. listings)	(2) ln(quantity sold)	(3) ln(sales in USD)
Share × Post	0.0631*** (0.00584)	0.0670*** (0.00634)	0.0280*** (0.00611)
Market FE	Yes	Yes	Yes
week FE	Yes	Yes	Yes
adj R-squared	0.525	0.525	0.520
observations	8092	8092	8092

Notes: Sample at the market-week level. Standard errors are clustered at the market level.

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

7 Conclusion

In this paper, we study the effects of targeted incentives. We leverage a platform-wide campaign on eBay that targets financial incentives to platform-certified sellers who provide fast shipping and generous return policies. We find that besides targeted sellers, non-targeted sellers are also more likely to adopt the promoted behavior. In particular, when the targeted incentive is offered to more sellers in a market, non-targeted sellers will also respond by adopting the promoted behavior to a larger extent, resulting in a more pronounced effect in this market. Consistent with the increased quality provision of non-targeted sellers, we find that non-targeted sellers experience a demand expansion when they adopt the promoted behavior. Our results on the demand expansion for non-targeted sellers in the presence of a targeted incentive imply that consumers value the targeted type and the promoted behavior as substitute quality signals of sellers.

One limitation of our work is that in our setting the targeted sellers are always platform-certified; thus, they are of higher quality and are essentially market leaders. This fact could make the spillover effect more salient, because an average seller may closely observe the market leaders when they sell. One should keep this caveat in mind when interpreting the results: if an incentive is targeted to non-certified sellers instead, we may not observe a spillover effect of a similar magnitude on certified sellers; in addition, we may not observe a demand expansion effect for certified sellers, because they already have the quality certification from the platform. Another limitation is that we cannot study the effects of targeted incentives in the long run, allowing for entry and exit of sellers, because of eBay's subsequent policy changes.

Our findings have managerial implications for digital platforms that use or consider using targeted incentives. To determine the optimal targeting size, a platform may want to estimate both targeted and non-targeted sellers' elasticity of adopting the promoted behavior with respect to the incentive, using methods such as the one in this paper or using field experiments. Based on these estimates, the platform can determine the optimal targeting size weighing the benefit and cost of the promoted behavior. In addition, platforms may want to make the promoted behavior a salient quality signal to consumers, to take advantage of the potential demand expansion effect for non-targeted sellers.

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Online Appendix

A Figures

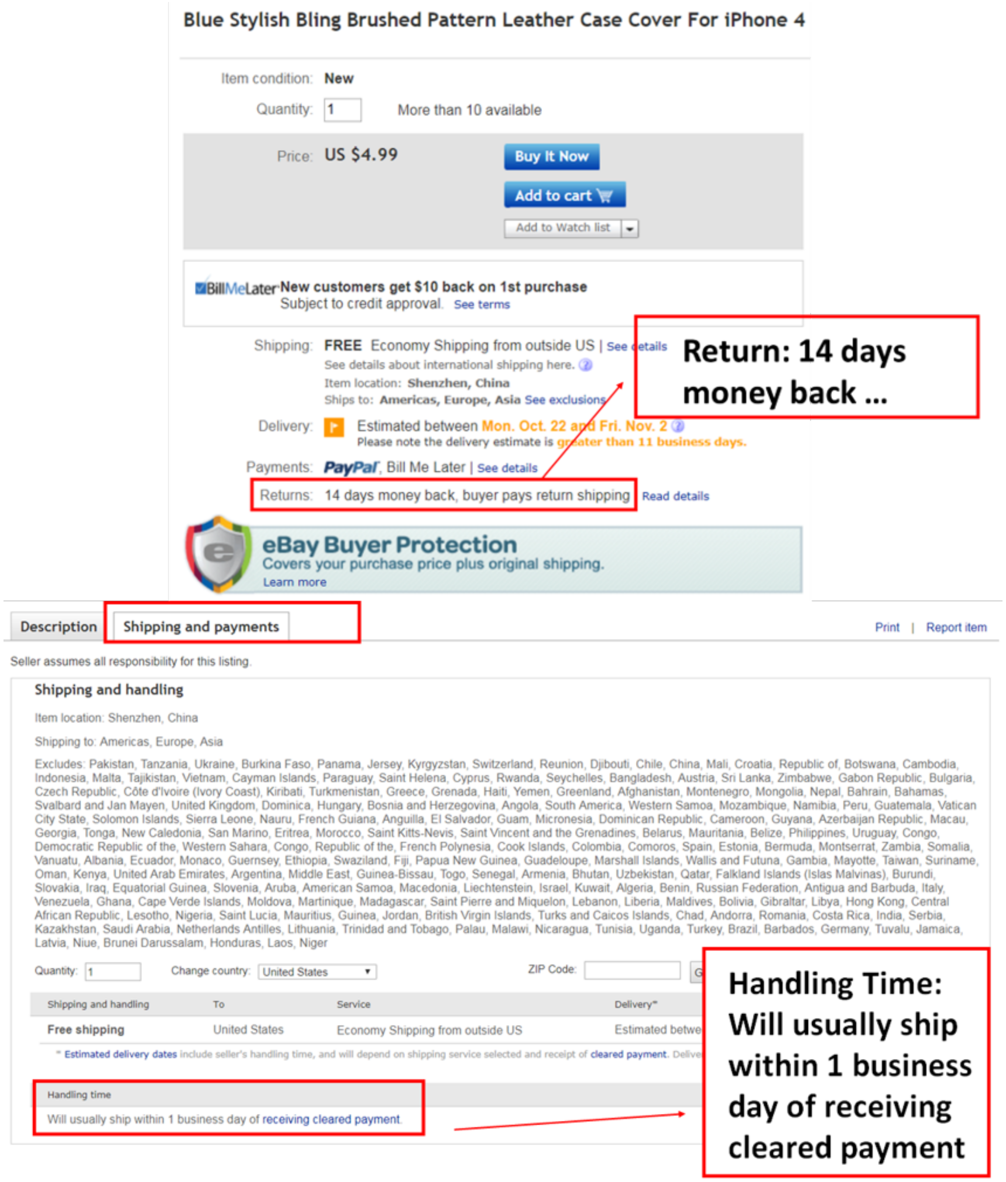

Figure A1: Information on Return Specifics and Handling Time

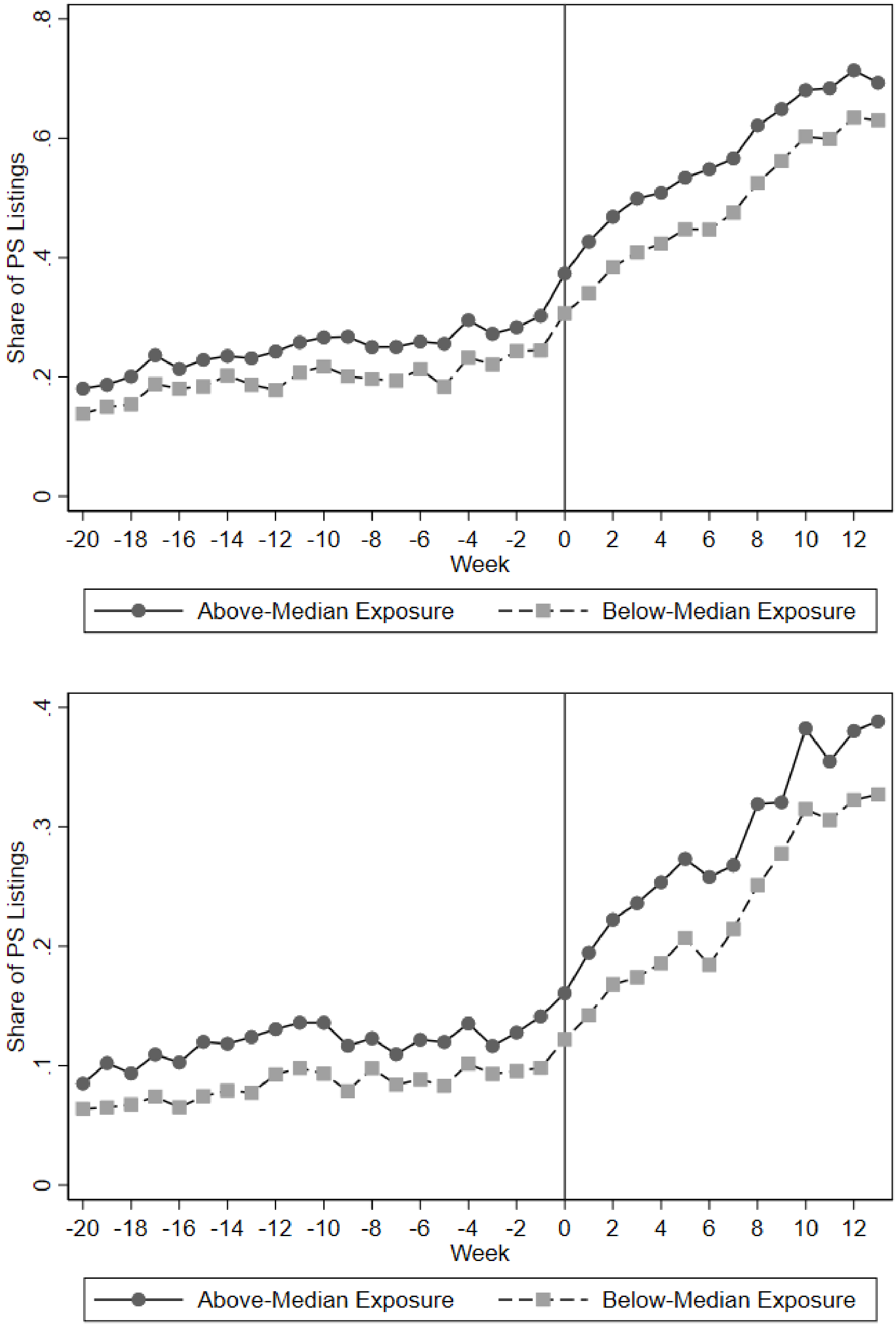

Figure A2: Market-level Parallel Trends

Notes: Week 0 is the policy week of March 1, 2012.

B Tables

Table A1: Correlation between the Policy Exposure and Various Market and Seller Characteristics

Panel A. Market exposure, correlation with market characteristics				
Average price	Revenue	PS share	EPP	Share low DSR
-0.342	0.091	0.360	0.425	-0.485
Share claims	HHI	Package size	Package weight	Postage
-0.491	-0.090	-0.145	-0.141	-0.143
Panel B. Seller exposure, correlation with seller characteristics				
Average price	Revenue	PS share	EPP	Share low DSR
-0.168	0.059	0.176	0.052	-0.114
Share claims	Package size	Package weight	Postage	
-0.114	-0.086	-0.085	-0.108	

Sample in Panel A at the market level. Sample in Panel B at the seller level. See variable definitions under Equation 2 .

Table A2: Share of Transactions with PS: DiD — Full Table

Dependent Variable: Share of transactions with PS	DiD		DiD with controls	
	(1)	(2)	(3)	(4)
	eTRS	non-eTRS	eTRS	non-eTRS
Share × Post	0.349***	0.311***	0.316***	0.272***
	(0.0210)	(0.0176)	(0.0212)	(0.0177)
Average Price × Post			-0.0000505***	-0.0000199***
			(0.00000457)	(0.00000155)
Revenue × Post			2.44e-08*	8.17e-08***
			(1.37e-08)	(1.87e-08)
Ex-ante PS share× Post			-0.00211***	0.00164***
			(0.0000734)	(0.000130)
EPP × Post			0.000592***	0.000129***
			(0.0000685)	(0.0000395)
Share low DSR × Post			-0.00596***	-0.00166***
			(0.000767)	(0.000154)
Share claims × Post			-0.00180**	-0.00134***
			(0.000721)	(0.000131)
Package size × Post			-0.00000594	-0.0000112***
			(0.00000383)	(0.00000266)
Package weight × Post			-0.0000122	0.0000998***
			(0.0000494)	(0.0000361)
Postage cost × Post			-0.00293***	-0.00164***
			(0.000461)	(0.000354)
seller FE	Yes	Yes	Yes	Yes
week FE	Yes	Yes	Yes	Yes
adj R-squared	0.674	0.614	0.676	0.619
observations	2613546	1841535	2613546	1841535

Notes: This table reports the full regression results of Equation 2 that include $X_i \times Post_t$ in columns (3) and (4). For completeness, we also report the DiD results without $X_i \times Post_t$ in columns (1) and (2). Sample at the seller-week level. Standard errors are clustered at the seller level.

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

Table A3: Sellers' Timing of PS Adoption Across Their Product Markets: Full Table

Dependent Variable: First Week Offering PS	eTRS Sellers		Non-eTRS Sellers	
	(1)	(2)	(3)	(4)
Share	-0.852***	-0.701***	-0.408***	-0.285***
	(0.0782)	(0.0778)	(0.0584)	(0.0578)
Top Product (0/1)	-6.287***	-2.982***	-2.215***	-0.138
	(0.0273)	(0.295)	(0.0192)	(0.180)
Share $\times$ Top Product		-4.898***		-3.129***
		(0.436)		(0.272)
Average Price	0.00367***	0.00368***	0.00144***	0.00142***
	(0.000115)	(0.000115)	(0.0000780)	(0.0000781)
Revenue	-5.22e-09***	-5.26e-09***	-3.58e-09***	-3.62e-09***
	(2.49e-10)	(2.49e-10)	(1.77e-10)	(1.77e-10)
EPP	-0.0344***	-0.0343***	-0.0110***	-0.0107***
	(0.000918)	(0.000918)	(0.000682)	(0.000682)
HHI	0.00173***	0.00173***	0.000788***	0.000785***
	(0.0000404)	(0.0000404)	(0.0000309)	(0.0000309)
Package Size	-0.0000127**	-0.0000119*	-0.00000652	-0.00000538
	(0.00000634)	(0.00000634)	(0.00000421)	(0.00000421)
Package Weight	0.00364***	0.00370***	0.00133***	0.00140***
	(0.000432)	(0.000432)	(0.000302)	(0.000302)
Postage	-0.0375***	-0.0380***	-0.0212***	-0.0215***
	(0.00457)	(0.00457)	(0.00336)	(0.00336)
Seller FE	Yes	Yes	Yes	Yes
adj R-squared	0.575	0.575	0.577	0.577
observations	2120574	2120574	1728383	1728383

Notes: This table reports the full regression results of the timing of PS adoption across the markets of a given seller. Sample at the seller-market level. Standard errors are clustered at the seller level.

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

Table A4: Sellers' Timing of PS Adoption Across Their Product Markets: Top 3 Products

Dependent Variable: First Week Offering PS	eTRS Sellers		Non-eTRS Sellers	
	(1)	(2)	(3)	(4)
Share	-0.902***	-0.481***	-0.441***	-0.247***
	(0.0772)	(0.0772)	(0.0581)	(0.0584)
Top 3 Product (0/1)	-5.091***	-2.243***	-1.947***	-0.920***
	(0.0232)	(0.171)	(0.0174)	(0.102)
Share × Top 3 Product		-4.246***		-1.548***
		(0.253)		(0.153)
Average Price	0.00361***	0.00361***	0.00146***	0.00142***
	(0.000113)	(0.000113)	(0.0000774)	(0.0000779)
Revenue	-3.53e-09***	-3.63e-09***	-2.84e-09***	-2.87e-09***
	(2.46e-10)	(2.46e-10)	(1.74e-10)	(1.74e-10)
EPP	-0.0288***	-0.0287***	-0.00874***	-0.00857***
	(0.000904)	(0.000904)	(0.000675)	(0.000676)
HHI	0.00157***	0.00156***	0.000732***	0.000727***
	(0.0000386)	(0.0000384)	(0.0000302)	(0.0000301)
Package Size	-0.00000883	-0.00000836	-0.00000747*	-0.00000664
	(0.00000629)	(0.00000629)	(0.00000419)	(0.00000419)
Package Weight	0.00288***	0.00297***	0.00103***	0.00109***
	(0.000426)	(0.000426)	(0.000300)	(0.000300)
Postage	-0.0381***	-0.0383***	-0.0214***	-0.0214***
	(0.00449)	(0.00449)	(0.00333)	(0.00333)
Seller FE	Yes	Yes	Yes	Yes
adj R-squared	0.582	0.582	0.580	0.581
observations	2120574	2120574	1728383	1728383

Notes: This table reports qualitatively similar results to those in Table 5 on the sample of top three markets among sellers who sell in at least three markets; the sample covers 75% of all unique sellers. Sample at the seller-market level. Standard errors are clustered at the seller level.

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

Table A5: PS Adoption Across Markets: Continuous Product Rank

Dependent Variable: First Week Offering PS	eTRS Sellers		Non-eTRS Sellers	
	(1)	(2)	(3)	(4)
Share	-0.881***	-2.389***	-0.333***	-0.647***
	(0.0761)	(0.110)	(0.0573)	(0.0762)
Product Rank	0.0785***	0.0472***	0.0360***	0.0295***
	(0.000875)	(0.00174)	(0.000762)	(0.00130)
Share $\times$ Product Rank		0.0471***		0.00977***
		(0.00230)		(0.00145)
Average Price	0.00159***	0.00159***	0.000681***	0.000655***
	(0.000108)	(0.000108)	(0.0000726)	(0.0000729)
Revenue	-1.43e-09***	-1.54e-09***	-1.69e-09***	-1.70e-09***
	(2.37e-10)	(2.37e-10)	(1.64e-10)	(1.64e-10)
EPP	-0.0203***	-0.0202***	-0.00488***	-0.00484***
	(0.000897)	(0.000897)	(0.000671)	(0.000670)
HHI	0.000937***	0.000929***	0.000383***	0.000380***
	(0.0000327)	(0.0000326)	(0.0000253)	(0.0000253)
Package Size	0.0000160**	0.0000160**	0.00000919**	0.00000908**
	(0.00000628)	(0.00000626)	(0.00000416)	(0.00000416)
Package Weight	-0.00395***	-0.00366***	-0.00160***	-0.00153***
	(0.000418)	(0.000419)	(0.000294)	(0.000295)
Postage	0.0437***	0.0416***	0.0133***	0.0130***
	(0.00445)	(0.00446)	(0.00327)	(0.00327)
Seller FE	Yes	Yes	Yes	Yes
adj R-squared	0.584	0.584	0.585	0.585
observations	2120574	2120574	1728383	1728383

Notes: This table reports qualitatively similar results to those in Table 5 where we control for product rank interacting with policy exposure. Sample at the seller-market level. Standard errors are clustered at the seller level.

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

Table A6: Share of Transactions with PS: Event study and DiD — Market-level Analysis

Dependent Variable: Share of transactions with PS	Event study		DiD		DiD with controls	
	(1)	(2)	(3)	(4)	(5)	(6)
	eTRS	non-eTRS	eTRS	non-eTRS	eTRS	non-eTRS
Post	0.134***	0.0642***				
	(0.00651)	(0.00461)				
Share × Post			0.350***	0.278***	0.538***	0.478***
			(0.103)	(0.0670)	(0.152)	(0.111)
Average Price × Post					-0.000224**	-0.000169
					(0.000112)	(0.000106)
Revenue × Post					5.43e-10	2.61e-10
					(4.21e-10)	(3.13e-10)
Ex-ante PS share× Post					-0.00622***	-0.00321**
					(0.00162)	(0.00126)
EPP × Post					0.00226**	-0.000730
					(0.00107)	(0.000786)
Share low DSR × Post					0.136***	0.102**
					(0.0517)	(0.0412)
Share claims × Post					-0.0177	-0.00781
					(0.0208)	(0.0160)
HHI × Post					-0.0000678	-0.0000133
					(0.0000585)	(0.0000448)
Package size × Post					-0.0000138***	0.00000835**
					(0.00000444)	(0.00000325)
Package weight × Post					0.0000986	-0.000874***
					(0.000579)	(0.000327)
Postage cost × Post					-0.00151	0.00399
					(0.00568)	(0.00383)
Market FE	Yes	Yes	Yes	Yes	Yes	Yes
Week FE	No	No	Yes	Yes	Yes	Yes
linear time trend	Yes	Yes	No	No	No	No
adj R-squared	0.785	0.714	0.834	0.762	0.847	0.767
observations	8092	8092	8092	8092	8092	8092

Notes: This table reports the full regression results on quality provision at the market level in columns (5) and (6) that include a set of market-specific characteristics interacting with the post dummy. These controls are market average price, market total GMV, market *ex ante* PS share, average seller EPP of the market, average share of sellers' low DSR of the market, and average share of sellers' consumer claims of the market. For completeness, we also report the event study results in columns (1) and (2), and DiD results without these characteristics interacting with the post dummy in columns (3) and (4). Sample at the market-week level. Standard errors are clustered at the market level.

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

Table A7: PS Premium: Four Weeks Before and After Policy Change

	(1) Pre	(2) Post	(3) Pre	(4) Post
PS	0.0259***	0.0325***	0.0272*	-0.0750***
	(0.00134)	(0.00151)	(0.0145)	(0.0164)
Share × PS			-0.00199	0.156***
			(0.0209)	(0.0237)
Match FE	Yes	Yes	Yes	Yes
$H_0 : \Delta_{Pre} = \Delta_{Post}$	Coefficient on PS		Coefficient on Share × PS	
F-test (p-value)	<0.001		<0.001	
adj R-squared	0.000565	0.000797	0.000562	0.000913
observations	484084	409492	484084	409492

Notes: This table reports the robustness check of the PS premium analysis in Table 6 by focusing on the four weeks before and four weeks after the policy change. One observation is a listing. Standard errors clustered at the match level.

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

Table A8: Market Equilibrium for eTRS Sellers: Leads-and-Lags Analysis

	PS = 1			*PS = 0*		
	(1) ln(num. listings)	(2) ln(quantity sold)	(3) ln(sales price)	(4) ln(num. listings)	(5) ln(quantity sold)	(6) ln(sales price)
Month=-4 × Share	0.00907***	0.00667*	0.00198	-0.00222	-0.00302	-0.00571
	(0.00290)	(0.00365)	(0.00910)	(0.00208)	(0.00254)	(0.00597)
Month=-3 × Share	0.00289	-0.00144	-0.0123	-0.00192	-0.00131	-0.00918
	(0.00364)	(0.00448)	(0.0104)	(0.00278)	(0.00299)	(0.00894)
Month=-2 × Share	0.00199	-0.00431	-0.0223*	-0.00212	-0.0136***	-0.00925
	(0.00411)	(0.00474)	(0.0126)	(0.00292)	(0.00358)	(0.00971)
Month=-1 × Share	0.00451	0.00141	-0.0129	-0.00154	-0.00804	-0.00575
	(0.00427)	(0.00605)	(0.0149)	(0.00312)	(0.00490)	(0.0102)
Month=0 × Share	0.0422***	0.0371***	-0.0790***	-0.0975***	-0.0848***	0.0623***
	(0.00882)	(0.00993)	(0.0144)	(0.00664)	(0.00847)	(0.0111)
Month=1 × Share	0.0651***	0.0616***	-0.0829***	-0.0991***	-0.0838***	0.0675***
	(0.00976)	(0.0104)	(0.0127)	(0.00649)	(0.00866)	(0.0113)
Month=2 × Share	0.0616***	0.0539***	-0.0835***	-0.0950***	-0.0805***	0.0731***
	(0.00801)	(0.00924)	(0.0133)	(0.00627)	(0.00848)	(0.0122)
Month=3 × Share	0.0771***	0.0678***	-0.0906***	-0.0968***	-0.0818***	0.0656***
	(0.0124)	(0.0131)	(0.0133)	(0.00643)	(0.00870)	(0.0121)
Market FE	Yes	Yes	Yes	Yes	Yes	Yes
week FE	Yes	Yes	Yes	Yes	Yes	Yes
adj R-squared	0.385	0.358	0.587	0.776	0.724	0.726
observations	8092	8092	8092	8092	8092	8092

Notes: This table reports the leads-and-lags analysis of Equation 5 for eTRS sellers only. We control for policy exposure interacting with all monthly dummies except the first month of the sample period. Month 0 is March 1, 2012, to March 31, 2012. Sample at the market-week level. Standard errors are clustered at the market level.

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

Table A9: Market Equilibrium for eTRS Sellers: Leads-and-Lags Analysis

	PS = 1			*PS = 0*		
	(1)	(2)	(3)	(4)	(5)	(6)
	ln(num. listings)	ln(quantity sold)	ln(sales price)	ln(num. listings)	ln(quantity sold)	ln(sales price)
Month=-4 × Share	0.000201	0.00324	0.00595	0.00750*	0.00414	-0.00694
	(0.00222)	(0.00317)	(0.00906)	(0.00433)	(0.00427)	(0.0149)
Month=-3 × Share	-0.000301	0.00532	-0.00486	0.0133***	0.0129**	-0.0246
	(0.00253)	(0.00384)	(0.0149)	(0.00496)	(0.00515)	(0.0169)
Month=-2 × Share	-0.00406	0.00238	-0.00248	-0.000745	0.000939	-0.0149
	(0.00319)	(0.00418)	(0.0188)	(0.00440)	(0.00504)	(0.0158)
Month=-1 × Share	-0.00570	-0.0000314	-0.000714	-0.000727	0.000467	-0.00104
	(0.00378)	(0.00489)	(0.0161)	(0.00480)	(0.00489)	(0.0178)
Month=0 × Share	0.107***	0.0905***	0.0129	-0.0708***	-0.0572***	-0.0262
	(0.0114)	(0.00961)	(0.0177)	(0.00701)	(0.00634)	(0.0199)
Month=1 × Share	0.124***	0.102***	0.0265	-0.0776***	-0.0640***	-0.00902
	(0.0110)	(0.00913)	(0.0213)	(0.00733)	(0.00655)	(0.0200)
Month=2 × Share	0.132***	0.109***	0.0300	-0.0727***	-0.0622***	-0.0134
	(0.0114)	(0.0101)	(0.0201)	(0.00693)	(0.00718)	(0.0226)
Month=3 × Share	0.133***	0.110***	0.0202	-0.0752***	-0.0685***	-0.0232
	(0.0122)	(0.0108)	(0.0196)	(0.00694)	(0.00724)	(0.0238)
Market FE	Yes	Yes	Yes	Yes	Yes	Yes
week FE	Yes	Yes	Yes	Yes	Yes	Yes
adj R-squared	0.486	0.498	0.524	0.703	0.664	0.406
observations	8092	8092	8092	8092	8092	8092

Notes: This table reports the leads-and-lags analysis of Equation 5 for non-eTRS sellers only. We control for policy exposure interacting with all monthly dummies except the first month of the sample period. Month 0 is March 1, 2012, to March 31, 2012. Sample at the market-week level. Standard errors are clustered at the market level.

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

Table A10: Number of eTRS and non-eTRS Sellers with Additional Controls

	(1) eTRS	(2) Non-eTRS	(1) eTRS	(2) Non-eTRS
Share × Post	0.00107	-0.0106	-0.0122	-0.0223
	(0.0277)	(0.0179)	(0.0490)	(0.0332)
Average Price × Post			0.00311	-0.00106
			(0.00273)	(0.00214)
Revenue × Post			-9.20e-09	-1.32e-08
			(1.53e-08)	(1.26e-08)
Ex-ante PS share× Post			0.0325	0.0221
			(0.0405)	(0.0261)
EPP × Post			-0.00563	0.0126
			(0.0343)	(0.0237)
Share low DSR × Post			-0.00583	-0.00114
			(0.0318)	(0.0393)
Share claims × Post			-0.485	0.183
			(0.830)	(0.372)
HHI × Post			-0.000790	-0.000459
			(0.00159)	(0.000768)
Package size × Post			-0.000108	0.0000217
			(0.000166)	(0.0000805)
Package weight × Post			0.00437	0.0000162
			(0.0170)	(0.0142)
Postage cost × Post			-0.0323	-0.0119
			(0.203)	(0.125)
Market FE	Yes	Yes	Yes	Yes
week FE	Yes	Yes	Yes	Yes
adj R-squared	0.767	0.559	0.767	0.559
observations	8092	8092	8092	8092

Notes: Sample at the market-week level. Outcome variables are number of eTRS and non-eTRS sellers with at least one listing in each market-week pair. Standard errors are clustered at the market level.

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

Table A11: Market Equilibrium for eTRS Sellers with Additional Controls

		PS = 1			*PS = 0*	
	(1) ln(num. listings)	(2) ln(quantity sold)	(3) ln(sales price)	(4) ln(num. listings)	(5) ln(quantity sold)	(6) ln(sal price
Share × Post	0.0619***	0.0652***	-0.0695***	-0.108***	-0.0946***	0.0711
	(0.00882)	(0.0105)	(0.0123)	(0.00832)	(0.00961)	(0.011
Average Price × Post	0.000634	0.00119**	0.000271	-0.00127	-0.000881	0.0003
	(0.000566)	(0.000571)	(0.000952)	(0.000780)	(0.000651)	(0.0007
Revenue × Post	3.30e-10	-2.70e-09	2.71e-09	-4.12e-09	-3.50e-09	-1.50e-0
	(2.75e-09)	(3.34e-09)	(4.22e-09)	(2.78e-09)	(3.23e-09)	(4.26e-
Ex-ante PS share× Post	-0.0110	-0.0142	0.0106	0.0202**	0.0276**	0.008
	(0.0113)	(0.0205)	(0.0186)	(0.00951)	(0.0115)	(0.013
EPP × Post	0.0134*	0.00990	-0.0245**	0.00111	0.00275	-0.002
	(0.00737)	(0.00783)	(0.0120)	(0.00713)	(0.00791)	(0.010
Share low DSR × Post	0.0279***	0.0514***	-0.0210	-0.00790	-0.0178*	0.0427
	(0.00705)	(0.0101)	(0.0172)	(0.00902)	(0.00977)	(0.015
Share claims × Post	0.137	0.188*	-0.0298	-0.0531	-0.0989	-0.02
	(0.1000)	(0.110)	(0.162)	(0.115)	(0.106)	(0.16
HHI × Post	-0.000573	-0.000646	0.00142**	0.000453*	0.000629	-0.000
	(0.000347)	(0.000436)	(0.000560)	(0.000256)	(0.000449)	(0.0003
Package size × Post	0.0000496**	0.0000611**	-0.0000787**	-0.0000743***	-0.0000722***	0.00005
	(0.0000223)	(0.0000265)	(0.0000319)	(0.0000205)	(0.0000190)	(0.0000
Package weight × Post	0.00365	0.00185	-0.00648	-0.000515	0.00144	-0.002
	(0.00389)	(0.00383)	(0.00471)	(0.00296)	(0.00288)	(0.004
Postage cost × Post	-0.0469	-0.0299	0.0999*	0.0558*	0.0242	0.002
	(0.0437)	(0.0434)	(0.0556)	(0.0323)	(0.0310)	(0.049
Market FE	Yes	Yes	Yes	Yes	Yes	Yes
week FE	Yes	Yes	Yes	Yes	Yes	Yes
adj R-squared	0.387	0.361	0.593	0.784	0.734	0.73
observations	8092	8092	8092	8092	8092	809

Notes: Sample at the market-week level. Standard errors are clustered at the market level.

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

Table A12: Market Equilibrium for non-eTRS Sellers with Additional Controls

	PS = 1			*PS = 0*		
	(1) ln(num. listings)	(2) ln(quantity sold)	(3) ln(sales price)	(4) ln(num. listings)	(5) ln(quantity sold)	(6) ln(sales price)
Share × Post	0.144***	0.111***	0.000290	-0.0838***	-0.0729***	0.00290
	(0.0145)	(0.0118)	(0.0183)	(0.00839)	(0.00762)	(0.0215)
Average Price × Post	0.00187	0.00132	-0.00139	-0.000144	-0.0000232	-0.00219
	(0.00116)	(0.000963)	(0.00153)	(0.000590)	(0.000557)	(0.00173)
Revenue × Post	6.30e-09	1.90e-09	4.73e-09	-9.25e-09***	-8.22e-09***	2.52e-10
	(5.07e-09)	(4.48e-09)	(4.97e-09)	(2.39e-09)	(1.94e-09)	(4.90e-09)
Ex-ante PS share× Post	-0.0324*	-0.0173	0.00226	0.0203**	0.0198**	-0.0139
	(0.0191)	(0.0157)	(0.0232)	(0.00956)	(0.00888)	(0.0217)
EPP × Post	0.0178	0.0196*	-0.0279	-0.00229	0.00359	-0.00276
	(0.0133)	(0.0113)	(0.0183)	(0.00695)	(0.00593)	(0.0190)
Share low DSR × Post	0.00359	0.0146	0.00485	0.00779	0.00441	-0.0315*
	(0.0164)	(0.0168)	(0.0161)	(0.00698)	(0.00602)	(0.0187)
Share claims × Post	0.175	0.139	-0.687**	-0.0182	-0.0280	0.309
	(0.184)	(0.157)	(0.296)	(0.126)	(0.0991)	(0.239)
HHI × Post	-0.00111**	-0.00101**	-0.000840	0.000572**	0.000352	-0.0000803
	(0.000485)	(0.000513)	(0.000532)	(0.000272)	(0.000255)	(0.000422)
Package size × Post	0.0000913**	0.0000617	-0.0000235	-0.0000215	-0.0000141	-0.00000552
	(0.0000375)	(0.0000381)	(0.000157)	(0.0000217)	(0.0000224)	(0.0000586)
Package weight × Post	0.00333	0.00213	-0.00714	-0.00326	-0.00243	-0.000807
	(0.00524)	(0.00408)	(0.00999)	(0.00302)	(0.00235)	(0.00593)
Postage cost × Post	-0.0778	-0.0387	-0.0154	0.0645*	0.0544*	0.0557
	(0.0565)	(0.0457)	(0.0935)	(0.0330)	(0.0276)	(0.0707)
Market FE	Yes	Yes	Yes	Yes	Yes	Yes
week FE	Yes	Yes	Yes	Yes	Yes	Yes
adj R-squared	0.496	0.504	0.537	0.714	0.672	0.407
observations	8092	8092	8092	8092	8092	8092

Notes: Sample at the market-week level. Standard errors are clustered at the market level.

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

Table A13: Policy Effect on Market Totals: with Additional Controls

	(1) ln(num. listings)	(2) ln(quantity sold)	(3) ln(sales in USD)
Share × Post	0.0685***	0.0717***	0.0240**
	(0.00770)	(0.00789)	(0.00979)
Average Price × Post	0.00156***	0.00166***	-0.000599
	(0.000573)	(0.000614)	(0.000754)
Revenue × Post	-8.11e-11	-2.57e-09	-2.50e-09
	(2.49e-09)	(2.61e-09)	(3.64e-09)
Ex-ante PS share× Post	-0.00110	0.00308	0.0105
	(0.00850)	(0.0106)	(0.0140)
EPP × Post	0.0170**	0.0163**	-0.00728
	(0.00750)	(0.00777)	(0.00898)
Share low DSR × Post	0.00471	0.0153**	0.00342
	(0.00685)	(0.00694)	(0.0139)
Share claims × Post	0.130	0.141	-0.0128
	(0.0891)	(0.114)	(0.136)
HHI × Post	-0.000498	-0.000628*	-0.000196
	(0.000317)	(0.000369)	(0.000261)
Package size × Post	0.0000515**	0.0000497*	0.0000124
	(0.0000246)	(0.0000253)	(0.0000237)
Package weight × Post	0.00129	0.00151	-0.00275
	(0.00299)	(0.00302)	(0.00405)
Postage cost × Post	-0.0327	-0.0323	0.0228
	(0.0329)	(0.0342)	(0.0426)
Market FE	Yes	Yes	Yes
week FE	Yes	Yes	Yes
adj R-squared	0.527	0.528	0.520
observations	8092	8092	8092

Notes: Sample at the market-week level. Standard errors are clustered at the market level.

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