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Does Country-of-Origin Marketing Matter?

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Abstract

Firms often display product information on their front-of-package labels, with some firms going as far as to make deceptive claims. We study the impact of the "Made in USA" claim - a disclosure not legally required on consumer-packaged goods and yet a claim highlighted by many firms, sometimes deceptively, - on consumer demand. Leveraging the FTC's investigation of four brands that resulted in removal of the claim from product packages, we study the impact such removal had on sales. We find a decline in demand following the removal of the "Made in USA" claim. Second, to ensure complete exogeneous variation, we conduct a field experiment on eBay, where we run over 900 auctions, varying only whether or not a product contains this country-of-origin information. We find consumers are willing to pay 28% more for the product when marketed as "Made in USA." The experiments alongside observational data allow us to rationalize firms' incentives in making deceptive country-of-origin claims.

Keywords: Field Experiments, Natural Experiments, Deceptive Advertising, Country-of-origin, FTC, Public Policy

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1 Introduction

Theory predicts complete and truthful disclosure by firms when consumers can easily verify the information provided. However, when verification is costly and firms believe such information will increase demand, incentives to engage in deceptive disclosure exist. One such setting is the country-of-origin disclosure, where instances of firms making deceptive claims abound. Examples include food products falsely claiming "Made in Italy," and consumer products falsely claiming "Made in USA" to increase demand from the local population. In the US alone, the Federal Trade Commission (FTC) investigated over 150 products for violating the "Made in the USA" policy.

However, little empirical evidence exists that allows us to understand if firm practices to differentiate their products using the country-of-origin claim make economic sense. Taking the case of the "Made in USA" claim, this paper aims to understand if consumers are willing to pay more for products that contain this claim. This paper also aims to quantify the penalties firms should be awarded for violating the FTC's "Made in the USA" policy, an action item cited in the FTC's 2019 "Made in the USA" workshop (FTC 2019a).

In addition, the US has witnessed a move toward Buy American and Hire American under the Trump administration (The White House 2019). Arguments such as more job opportunities for the country's citizens, economic growth, and less dependence on foreign trade have been put forth to incentivize firms to produce goods manufactured in the US. With the recent push toward local manufacturing, this paper aims to understand whether the additional demand associated with the country-of-origin claim justifies the substantial relocation costs and investments in local manufacturing.

We use a two-pronged approach to recover consumers' willingness to pay for the "Made in USA" attribute. First, we utilize the FTC's investigation around four brands that were deceptively making this claim. The investigations led to removal of the claim from the affected products' marketing materials, which was accompanied by the issuance of closing letters by the FTC. We treat the timing of this removal or the issuance of these closing letters as exogenous; that is, the firms did not remove the claim for reasons related to demand, and we examine the sales outcome of affected brands using a difference-in-differences strategy. Second, we run a field experiment on eBay, where we run over 900 auctions over the course of three months, varying only whether a product is advertised with or without the "Made in USA" claim. This experiment is aimed at creating exogenous variation in the claim with everything else held constant, helping recover consumers' valuation of the claim.

In the US, only imported goods are required to have a country of origin marking, for example, "Made in China". Goods made in the US are not legally required to have a country-of-origin marking unless they belong to the automobile, textile, wool, or fur industries. However, a wide variety of consumer packaged goods choose to voluntarily disclose their country of origin when made in the US. Examples include New Balance's Made in the USA sneakers, Crayola crayons, and Pyrex glassware.[1] For such claims, the FTC requires that the product be "all or virtually all" made in the US, with no or negligible foreign content. Nonetheless, deceptive or misleading claims about whether the product is made in the US exist widely. Since 2010, the FTC has investigated more than 150 cases of deceptive or misleading claims about products claiming to be made in the US.

Of these 150 cases, we restrict attention to the four brands that are covered in the Nielsen data[2]: Gorilla Glue, Loctite glue, Gorilla Tape, and Tramontina cookware. In response to the FTC's investigation, these brands removed or rectified deceptive country-of-origin information on product packaging, marketing materials, company websites, and with third-party retailers. Following satisfactory reaction from these firms, the FTC issued dated closing letters on its website. We take the timing of the FTC closing letters as exogenous to products' country-of-origin marketing (i.e., the firms removed these claims because they were ordered to do so by a regulatory authority, and not in response to consumer demand), and examine the resulting sales of the impacted brands, relative to other brands in the same category. This measure likely contains the sole effect of the removal of the "Made in USA" claims, because these investigations received very little news coverage as ascertained by archived news database Factiva. For three of the four brands, we find a negative effect of removing "Made in USA": Gorilla Glue experienced a significant 1.9% decline in weekly store sales; Loctite glue, a 6.1% decline; and Tramontina cookware, a 19.5% decline following the removal of "Made in USA" claims. Gorilla Tape, on the other hand, experienced an increase in sales.

One shortcoming of the event study is that it likely impacts the sales of brands serving as controls (e.g., if consumers switch brands after the removal of the claim), making our measure an upper bound. Another shortcoming of the event study is that the time of treatment is not precisely known: Firms might have conducted their remedial actions anytime (e.g., a week or a month) before the issuance of the closing letter. Finally, the event study covers products making deceptive claims and therefore measures the effect of the removal of the claim, whereas our object of interest is the impact of the claim.

We therefore resort to a field experiment, which allows us to overcome the above shortcomings. In this experiment, we exogenously vary whether or not a product carried the "Made in USA" claim in the product title, description, and pictures, enabling us to directly

[1] The information on brand names was acquired from the respective firm websites, https://www.newbalance.com/made-in-the-usa/, https://www.crayola.com/type:Made%20in%20America, https://pyrex.cmog.org/faq, and not from the Nielsen database.

[2] We exclude, for example, products in the furniture and apparel industry.

recover consumers' valuation of the "Made in USA" claim. In the field experiments, we pick a product category where demand is high on eBay, and both variants of the product, namely, with and without country-of-origin information, are sold in the market. Screen protectors for handheld devices fit these requirements, with the additional benefit of being relatively cheap. A power calculation run before the experiment (pre-registered at AsPredicted #22138) required us to conduct 434 pairs of auctions, or sell 868 units, making the price of procurement a non-trivial decision factor. We therefore sold 912 screen protectors using three-day auctions on eBay. We find the treatment effect of the "Made in USA" claim on auction transaction price is significant and valued at $0.07 for a product with a mean transaction price of $0.26, suggesting a 28% price premium for the "Made in USA" claim.

To understand the drivers of this effect, we turn to the extant behavioral literature which suggests two main sources of the country-of-origin effect: as a symbol of identity and as a predictor of quality. To understand if this effect is driven by identity, which we proxy for by party affiliation (e.g., Republicans might be willing to pay higher prices for the "Made in USA" attribute, because the Trump administration has emphasized Hiring and Buying American) or because the "Made in USA" claim serves as a signal of quality, we supplement our analysis with additional data. We collected voting results of the 2016 presidential election at the precinct level and matched these data to the buyer's ZIP Code information to serve as a proxy of the buyer's party affiliation. Similarly, we used the amount of time spent browsing review websites as a proxy for preference on quality at the ZIP Code level. Using both these supplemental datasets, we obtain party affiliation and quality preferences for each user, extrapolated from the user's ZIP Code. In the scope of our experiment, we find both factors contribute to the "Made in USA" effect and the magnitude of the effect attributable to political affiliation is five times greater than that attributable to quality preference.

Both the observational study and the field experiments conducted on eBay provide empirical evidence for a significant positive impact of "Made in USA" marketing on consumer demand, suggesting that consumers value the country-of-origin attribute and that deceptive country-of-origin marketing hurts consumer welfare. Because the FTC is considering awarding civil penalties in addition to equitable relief to "Made in USA" policy violators, our study provides a way to quantify the magnitude of consumer damage created by such deceptive claims. Our calculations suggest 1.9%-19.5% of firms' revenues, an equivalent of $26,489-$46,602 in monthly revenues, are attributable to the "Made in USA" claim.

From firms' perspective, combining data on profit margins from a publicly listed screen-protector manufacturer and the results of the eBay field experiment, the demand-side effect of "Made in USA" claims would not incentivize firms to move production locations from overseas to the US if the variable production cost is anything more than 1.63 times the cost overseas. As a benchmark, labor costs alone are 9 times more in the US than in China. In the extreme case, where all variable costs are attributable to labor, the demand for "Made in USA" is insufficient to justify a relocation. Knowing the exact breakdown of the cost structure will help shed better light on the feasibility of the relocation. Although firms might not be motivated to relocate, the increased demand for the "Made in USA" claim nevertheless creates incentives to make deceptive country-of-origin marketing claims. We believe this finding further underscores the role of the regulator in enforcing truthful representation of the country-of-origin claim.

Contribution

Whether firms truthfully disclose their true quality has received a lot of theoretical as well as empirical interest. Grossman (1981) and Milgrom (1981) show that if disclosure costs are zero, sellers will reveal their true quality because rational consumers associate non-disclosure with low quality. However, empirical studies have found partial disclosure: Jin (2005) finds incomplete disclosure in the HMO market, and Moorman (1998) and Mathios (2000) find the 1990 Nutrition Labelling and Education Act changed consumer and firm behavior, suggesting disclosure before the Act was incomplete. Our paper adds to this literature by studying firm incentives to disclose country-of-origin, when such disclosure is not legally required.

Although the disclosure literature has limited attention to truthful disclosure, a related stream studies firm incentives to engage in and consumer responses to deceptive advertising. Deception has been studied in lab settings (e.g., Dyer and Kuehl 1978, Olson and Dover 1978, Johar 1995, Darke and Ritchie 2007) and, more recently, in empirical settings with observational data (e.g., Cawley et al. 2013, Mayzlin et al. 2014, Luca and Zervas 2016, Rao and Wang 2017). Nelson (1974) and Rhodes and Wilson (2018) both state the law is an important source of deceptive advertising, arguing that when a law is moderately enforced, consumers will believe disclosure is occurring truthfully, creating incentives for some firms to mislead. Our paper examines this intersection of regulation and deception, where firms have clearly deceived consumers about their country of origin with little penalty, highlighting the importance of understanding consumers' willingness to pay for this attribute.

Our paper also contributes to the vast country-of-origin literature, which has largely relied on stated purchase intentions and perceived quality as outcomes, by providing two empirical methods, both using actual purchase outcomes, to recover the willingness to pay for the "Made in USA" attribute. Peterson and Jolibert (1995), Verlegh and Steenkamp (1999), Pharr (2005), and Wilcox (2015) provide reviews of this literature. Although product perceptions and purchase intentions are relevant outcomes, they do not translate directly to willingness to pay, an important measure for making economically meaningful conclusions.

An exception is Koschate-Fisher et al. (2012), who investigate the country-of-origin effect on consumers' willingness to pay, using Becker-Degroot-Marschak's (1964) procedure in a lab setting. We extend this literature by providing a way to measure willingness to pay in the field in a real-world purchasing context.

Few papers have looked at willingness to pay using actual purchase outcomes. Chu (2013) uses variations in country of origin of PCs resulting from cross-border mergers and acquisitions. Ma and Hartmann (2018) use variations in manufacturing locations of nearly identical automobile offerings by the same parent company. However, these variations might not be completely orthogonal to product quality, confounding the country-of-origin effect with that of unobserved product quality. Our paper adds to the literature by using two new approaches to study the impact of the country-of-origin claim: a quasi-experiment that led to removal of claims for reasons unrelated to demand, and a field experiment that exogenously manipulates the existence of the country-of-origin claim.

This paper is also related to the literature that studies preferences for local goods: Blum and Goldfarb (2006) and Hortacsu et al (2009) show that even for purely digital goods, that is, goods with no shipping or distance-related costs, consumers exhibit a strong preference for trading with nearby or local sellers. The conclusion is that distance proxies for taste, with users living in geographic proximity having similar preferences, as well as trust. On the other extreme, studies have shown consumers exhibit negative preferences for products from certain countries when those countries adopted unpopular policies (e.g., Chavis and Leslie 2009, Pandya and Venkatesan 2016, Bentzen and Smith 2002 and Chen and Zhong 2019).

The rest of the paper is organized as follows. In section 2, we introduce the empirical setting of "Made in USA" claims as well as our identification strategy using the Nielsen data and eBay field experiments. In section 3, we describe the demand analysis using the Nielsen data. Section 4 describes the eBay field experiment design and discusses the results of the experiment. Section 5 concludes the paper.

2 Empirical Setting and Identification Strategy

The challenge in empirically measuring the impact of country-of-origin claims on consumer demand lies in the endogeneity of country-of-origin marketing. Production-site decisions and, subsequently, marketing decisions of country of origin are equilibrium outcomes jointly determined by both supply and demand functions. We therefore need exogenous variations, either in firms' manufacturing-location decision or in the way firms market their country-of-origin attribute, to measure the effect of country-of-origin claims on consumer demand. We take two approaches to overcome this challenge. First, we exploit the exogenous variation in country-of-origin marketing triggered by the FTC's investigations on firms making deceptive "Made in USA" claims. Second, we employ field experiments on eBay, where we sell products, varying only whether they are advertised with or without the country-of-origin attribute.

We describe below the sources of variation, our identification strategies, and the underlying assumptions in each of the two approaches.

2.1 Identification Using Observational Data

Products sold in the US, except automobiles, textile, wool, and fur products, are not legally required to disclose US contents, making disclosure of the "Made in USA" attribute an endogeneous choice of firms. Products that do choose to market themselves using this label, however, are subject to compliance with the FTC's "Made in USA" policy (FTC 1998), which requires that a product advertised as "Made in USA" be "all or virtually all" made in the US. Without the need for pre-approval from the FTC for a "Made in USA" claim, a company can make such a claim as long as it is truthful and substantiated.

Our empirical strategy exploits the FTC's investigation of a set of products that were directly in violation of the "Made in USA" policy, which led to removal of deceptive country-of-origin claims. We use a difference-in-differences approach, comparing sales of the focal brands with the sales of a set of control brands (that were under no such policy violation) before and after the FTC investigation.

2.1.1 FTC Investigation of Non-compliance with the "Made in USA" Policy

The FTC has investigated more than 150 cases of non-compliance with the "Made in USA" policy as of March 2019. In a typical investigation, the FTC starts by raising concerns about potentially deceptive US-origin claims in marketing material, on the websites, or on product packaging of the company under investigation. In response to the FTC's initial communication, the company is expected to take remediation steps. Depending on the scope of the deceptive claims, remediation actions often include updating country-of-origin information in marketing materials, on the company website, and on product packaging, as well as such information at third-party retailers. When remediation actions are deemed satisfactory, the FTC issues a closing letter on its website.

For example, Tramontina USA Inc, a manufacturer of cookware, cutlery, kitchen accessories, and housewares, was under investigation because it was overstating the extent to which its products were made in the US, when, in reality, some of its products incorporated significant imported content and others were sourced entirely overseas. In response to the investigation, Tramontina updated claims to state that affected products contain imported components, removed from all of its product webpages a banner with the word "USA" superimposed over an image of the flag, revised the website to clarify Tramontina's manufacturing process, and contacted third-party retailers to provide updated claims and product description (FTC 2015). As another example, Gorilla Glue Company, a manufacturer of glue and tape products, removed the "Made in USA" claim from product packaging after the FTC investigation. The change in product packaging is shown in Figure 1.

Figure 1: "Made in USA" claim removal after FTC intervention

Extracted May 2015 (pre-FTC) — Extracted July 2015 (post-FTC)

Highlighted area indicates deceptive "Made in USA" claim on product packaging before the FTC investigation.
Source: https://web.archive.org/web/20150502011256/http://gorillatough.com/
https://web.archive.org/web/20150715153545/http://www.gorillatough.com/

2.1.2 Scope of Brands

The FTC website lists all firms affected by the FTC investigations on the "Made in USA" policy violation (FTC 2019b). As of March 2019, the FTC had issued 141 closing letters and 27 cases. We narrow the scope of affected brands according to the following criteria: (1) The case or closing letter should be dated between 2004 and 2016 to be within the date range of the Nielsen data (4 cases and 92 closing letters remain); (2) the affected brand category should be present in the Nielsen data (0 cases and 12 closing letters remain); (3) the total number of purchase occasions across all Nielsen panelists over the course of the dataset should exceed 500 to ensure statistically meaningful results (6 closing letters remain); and (4) the remediation actions should contain more than just revisions and corrections for online marketing materials (3 closing letters remain).

These criteria leave us with three closing letters for four established brands sold in retail stores: Gorilla Glue, Loctite glue, Gorilla Tape, and Tramontina cookware. Table 1 lists the companies, affected brands and categories, the date of the FTC closing letters, and firms' remediation actions. In all cases, firms overstated the extent to which the products were made in the US on product labels and/or marketing materials.

Identifying Assumptions To believe in the empirical results carried out by the diff-in-diffs strategy, however, we need to make and justify the following identifying assumptions:

First, we assume the timing of the FTC investigation is exogenous, and therefore the subsequent changes in country-of-origin marketing resulting from firms' remediation actions are exogenous to consumer demand. Although the FTC does not disclose how it decides to start an investigation, a reasonable assumption is that companies do not know the FTC's intention to investigate in advance, nor can they negotiate the time of investigation with the FTC in accordance with other supply- and demand-side factors. Companies are unlikely to strategically decide the timing of remediation actions, because timely response prevents an escalation in investigation.

Second, we assume products under investigation remain unchanged in the short interval before and after the FTC investigation; that is, the only changes to the product are related to the country-of-origin labeling and marketing. From the FTC closing letters, we observe that all companies stated changes in either marketing materials, packaging information, website wording, or internal procedure, and none mentioned adjustment in their sourcing or production process. Using Wayback Machine, an online website archive, we further verify that companies were compliant with the remediation actions stated in the closing letter. All firms analyzed in this study made observable changes in product packaging or website marketing.

Third, we claim the FTC investigation itself is unknown to consumers, so that consumers are only treated by changes in country-of-origin marketing and not the negative publicity related to deception. The absence of negative publicity is ascertained by archived news database Factiva, in which we did not find any major news coverage during the time of investigations.

Lastly, although we ensure affected brands and their respective controls share parallel trends prior to the FTC investigation, we acknowledge that they may all be affected by the treatment because they compete in the same product category. If consumers substitute to the competitor brands used as controls, our diff-in-diffs estimator provides an upper bound of the effect size.

2.2 Identification Using Field Experiments

Our second approach relies on field experiments conducted on eBay, where we varied whether otherwise identical products were marketed with or without the "Made in USA" claim, to measure consumers' valuation for this attribute.

Two decisions that had to be taken were the choice of product and the format of sale - we decided to sell screen protectors via a three-day auction format. We describe below the reasons behind the product choice, format of sale, and the chosen length of the auction.

2.2.1 Choice of Product

Our goal was to find a product category where demand is high on eBay, manipulation of country-of-origin information is feasible, and the product is relatively cheap so that we can afford listing hundreds of items to reach enough power. We chose to sell screen protectors for the following reasons:

First, screen protectors are among the most sold items on eBay across all categories in the seven-day period prior to July 25, 2018. High demand for the screen-protector category ensures a thick market with many active buyers and sellers, in which not only does the product have a higher likelihood of sale, but also our experiments are unlikely to distort the behavior of market participants.

Second, both kinds of screen protectors, ones with and without country-of-origin information, exist in the market. Therefore, selling screen protectors, some with the “Made in USA” claim and some without, was realistic. For example, a category containing no “Made in USA” products for sale in the market might have made our manipulation appear suspicious to consumers. Moreover, because some brands are actually made in the US, we could truthfully claim (without deception) they were “Made in USA”. We used the same products in the control condition, but without the “Made in USA” claim.

Third, many screen protectors are sold as generic, unbranded items. Because we wanted to facilitate a clean manipulation of the “Made in USA” attribute, dissociating the country-of-origin feature from the product’s brand name was crucial. We did not want consumers’ knowledge about certain brands, or their ability to conduct a search on the brand and realize it is made in the US to taint the experiment. We therefore stripped away all brand-related information from the description and pictures, selling the screen protectors as generic and unbranded.

Finally, screen protectors are relatively cheap and fit within our research budget. Examples of other “Made in USA” products that could have been auctioned on eBay, include Nylabone dog chew toys ($11), Magpul iPhone case (retail price $17), Princeton Tec LED Flashlight (retail price $25), and Igloo cooler ($15 - $60), which are more expensive than our choice of screen protector (retail price at around $8).[3] Moreover, these other items do not have high demand on eBay, making them less preferred candidates.

Note that although we focus on screen protectors, we cover a wide variety of products ranging from screen protectors for Google’s Pixel phones to screen protectors for Apple’s watches. We were able to identify three brands: Armorsuit, Skinomi and Spectre Shield that sell Made in USA screen protectors. Across these three brands we had a total of 76 distinct

[3]The retail price information is collected from Amazon. It is not acquired from the Nielsen database.

products that we sold on eBay. A complete list of products chosen is presented in Table 2.

2.2.2 Choice of Auction as Format of Sale

We had two options to sell our chosen product: list the product in an auction format or list it for direct sale.

A second-price auction is an efficient way of eliciting and recovering the distribution of consumers' valuation, because the winning bid reveals the willingness to pay of the second-highest bidder in each auction (Vickrey 1961). Moreover, given the number of potential bidders and the auction transaction price (the second-highest-order statistic), one can invert the transaction-price distribution to arrive at the valuation distribution. See Athey and Haile (2002) for a complete discussion about the mapping from empirical bid distritution to consumers' valuation distribution. A Buy/Not Buy setting (e.g., eBay's Buy-It-Now or Amazon's Add to Cart), on the other hand, does not allow us to learn about any order statistic of the valuation distribution, requiring us to sell the same product at different price points to recover the valuation distribution.

eBay auctions use an open, ascending-bid second-price format with a fixed ending time and proxy bidding system. The proxy bidding system is such that a bidder is asked to submit a maximum bid instead of her instant bid amount, and eBay issues a proxy bid equal to the minimum increment over the next highest bid (see Bajari and Hortacsu [2003a, 2003b, 2004] for a detailed explanation of eBay auctions). Assuming symmetric independent private values (IPV), Song (2004) shows in all symmetric Bayesian-Nash equilibria that the two highest-valued potential bidders always bid their valuations before the auction ends. Because we, as researchers and the seller, observe the winning bid, which is a minimum increment above the second-highest bid, we essentially observe the valuation of the second-highest-valued bidder in each auction. This equivalence is given by the relationship of bid and valuation in the open, ascending-bid second-price symmetric IPV auction model.

The observed winning bid in each auction directly helps us estimate the difference in transaction price of auctions for identical products with "Made in USA" claims and those without. Using a simple two-sided t-test on the winning bids of treatment and control listings, we are able to measure the average treatment effect of "Made in USA" claims on transaction price for the product. In addition, we are able to recover the underlying valuation distribution of control and treatment listings by relying on the second-highest-order-statistics nature of the winning bid.

2.2.3 Length of Auction

eBay allows sellers to run auctions that last for 1, 3, 5, 7, or 10-days. We chose the three-day auction to allow enough bidders to arrive at the listings while at the same time keeping the length of the experiment practicable. The current choice of three-day auctions resulted in a three-month-long experiment. A longer auction would have allowed more bidders to arrive but would have significantly increased the length of the experiment.

3 Empirical Analysis with Observational Data

We use the Nielsen HMS data, which contain households' purchases at the UPC-trip level for a panel of approximately 40,000-60,000 US households, the Nielsen RMS data, which contain weekly sales and weekly prices at the UPC-store level for participating retailers, and the Nielsen AdIntel data, which contain national and local TV advertising, to investigate the impact of the removal of "Made in USA" claims on consumer demand.

In the proceeding analysis, we define the focal brand as the brand investigated by the FTC for violating the "Made in USA" policy. Treatment is defined as the removal of the deceptive "Made in USA" claims from the product's label, packaging, and/or other marketing materials. The actual time when a brand received treatment should be the date when such changes were made, but it is not precisely observable to us. We therefore adopt the date of the FTC closing letter as a proxy for the date when the focal brands receive treatment.

3.1 Difference-in-Differences Estimator

We use both the Nielsen HMS data and the Nielsen RMS data for the analysis. In the Nielsen HMS data, we aggregate household-level purchases across all panelists to the brand-week level. In the Nielsen RMS data, we aggregate UPC-level sales within a brand to form store-brand-week level observations.

We restrict our attention to a local time window around the FTC closing-letter date, including only observations eight weeks prior to and after the FTC closing-letter date. By focusing on the short-term response in aggregate sales, we exclude other confounding factors that might have occurred far away from the treatment, that also affected sales but are unrelated to the treatment.

3.1.1 Regression Specification – Nielsen HMS

$$\log(sales_{jt}) = \beta_1 \mathbb{1}\{j = j_{FTC}, t \geq \tau_{FTC}\} + \beta_2 \bar{p}_{jt} + \beta_3 A_{jt} + \eta_j + \lambda_t + \epsilon_{jt} \tag{1}$$

Equation 1 describes a standard diff-in-diffs specification with two-way fixed effects, where the outcome variable is the log aggregated sales for brand j in week t. The coefficient of interest, β_1, measures the causal impact of removing the "Made in USA" label on aggregate sales under our empirical setting. The indicator variable is 1 when j is the focal brand and t is any week after the FTC closing-letter date. Week fixed effects λ_t control for inter-temporal shocks common to all brands, such as time trends and seasonalities. Brand fixed effects η_j absorb the time-invariant differences across brands. The two-way fixed effects specification forms the basis of our diff-in-diffs comparison. We also include controls for concurrent changes in price and advertising levels. More specifically, $\bar{p}_{jt}$, the brand-week-level price index, is the national sales-weighted average price across all UPCs for a given brand in a week, constructed from the Nielsen RMS data, and A_{jt} is the total duration of advertisements, including both national TV and spot TV advertising, for brand j in week t.

3.1.2 Regression Specification – Nielsen RMS

$$\log(sales_{sjt}) = \beta_1 \mathbb{1}\{j = j_{FTC}, t \geq \tau_{FTC}\} + \beta_2 \bar{p}_{sjt} + \beta_3 A_{sjt} + \eta_{sj} + \lambda_{st} + \epsilon_{sjt} \tag{2}$$

We apply the diff-in-diffs specification with two-way fixed effects again to the Nielsen RMS data with store-brand week-level observations, as specified in equation 2. The outcome variable is the log aggregate sales for brand j at store s in week t. The coefficient of interest, β_1, measures the causal impact of removing the "Made in USA" label on aggregate sales under our empirical setting. The indicator variable is 1 when j is the focal brand and t is later than the FTC closing letter date. Similarly, store-brand fixed effects η_{sj} absorb time-invariant differences across brands at a store, and store-week fixed effects λ_{st} absorb store-specific time trends and seasonality, while still allowing for identifying treatment effects.

Regressions are separately run for each brand category. As before, we include additional controls for average price and advertising to account for concurrent changes in the supply side. $\bar{p}_{sjt}$, the brand-week ZIP-Code-level price index, is the sales-weighted average price across all UPCs for a given brand in a week in the three-digit ZIP Code area in which store s is located, and A_{sjt} is the duration of advertisements, including both national TV and spot TV advertising, for brand j in week t aired in the DMA to which store s belongs. Whenever store ZIP Code or store DMA is not available, we use the national-level price index or national TV ad duration instead.

3.1.3 Control-Group Selection

In the diff-in-diffs specification, we include observations of the focal brand as well as a set of control brands around the FTC-closing letter date. The set of control brands are chosen so as to satisfy the parallel-trends assumption and serve as a benchmark of potential outcomes had the focal brand not received the treatment. Nonetheless, we acknowledge that control brands may also be affected by the treatment, because they compete with the focal brand in the same market. The diff-in-diffs estimator therefore provides an upper bound of the effect size.

For the Nielsen HMS and RMS datasets, we separately implement the following selection criteria. We first include the nine brands with the highest market share in each category (other than the focal brand) as control-brand candidates. If the focal brand ranks lower than 10, we include the top 14 brands to account for competing brands with smaller market share. This approach gives us nine control-brand candidates for Gorilla Glue and Loctite glue, nine control-brand candidates for Gorilla Tape, and 14 control-brand candidates for Tramontina cookware. Among the 14 control brand candidates for Tramontina, some mainly sell aluminum foil, which is distinct from the cookware product line for Tramontina. Therefore, we aggregated the aluminum foil brands into a composite brand, thus leaving Tramontina with nine control-brand candidates.

We plot weekly log aggregate sales for the focal brand and all control-brand candidates. We then hand pick control-brand candidates that share a similar sales trend with the focal brand from January 2014 until the FTC closing-letter date (see Figures 2 and 3). This approach leaves us with two control-brand candidates for each brand category in the HMS data, two for Gorilla Glue, two for Loctite glue, three for Gorilla Tape, and four for Tramontina in the RMS data. Notice that Gorilla Glue and Loctite glue appear to be each other's control-group candidate, and both brands received treatment in 2015 (Gorilla was treated in June and Loctite was treated in September). Therefore, we exclude Gorilla from Loctite's control group, because by the time Loctite glue was treated, Gorilla Glue has already been treated and can no longer serve as an untreated control brand.

These steps eventually leave us with the list of brands, shown in Table 3, as control groups.

3.1.4 Results

Tables 4 and 5 report results using the Nielsen HMS and RMS data respectively. Table 4 shows that in the immediate eight-week window around the FTC closing date, three of the four affected products show a decline, although insignificant, in sales using the HMS data, but Gorilla Glue shows a significant increase in sales. The effects are robust after controlling for average price index and advertisement duration.

Using the RMS data, Table 5 Panel A shows the removal of "Made in USA" claims negatively affected three of the four brands. These effects are significant and directionally consistent with the results from the HMS data, suggesting the small number of observations (16 weeks $\times$ 3 brands) in the HMS data might contribute to the lower level of statistical significance. The effects remain robust after controlling for average price index and advertisement duration (Table 5 Panel B), where the treatment effect is -1.9% for Gorilla Glue, -6.1% for Loctite glue, and -19.5% for Tramontina cookware, but is positive at 13.3% for Gorilla Tape. Note that we treat price and advertising as controls, and therefore interpret the price and advertising coefficients as correlations. The endogeneity of price and advertising does not affect our main coefficient of interest β_1, because the exogeneity of the treatment still holds conditional on price and advertising.[4]

The positive effects on Gorilla Tape in both the HMS and RMS data seem puzzling - noise in the data (because we use the FTC closing-letter date to proxy for the exact time of treatment) or other unobserved shocks to Gorilla Tape not captured by the control group could contribute to some of this effect. To make up for any shortcomings associated with the observational data, in the next section, we measure the effect by running field experiments on eBay where we sell identical products with and without the "Made in USA" claims.

3.2 Economic Significance

Our study of the four investigated brands speaks to the potential damage deceptive country-of-origin claims had on consumer welfare.

Among the three brands that exhibit a decline in sales post FTC investigation, our RMS diff-in-diffs estimates translate to \$26,489 lost in monthly sales (-1.9%) nationwide in the RMS data for Gorilla Glue, \$46,602 lost in sales (-6.1%) for Loctite glue, and \$29,689 lost in sales (-19.5%) for Tramontina cookware had they not made deceptive claims. This back-of-the-envelope calculation uses the average nationwide weekly sales prior to the FTC investigation as a baseline and applies the percentage sales decline estimated by the RMS diff-in-diffs regression to obtain the loss in sales.

At present, the remedies that the FTC seeks against companies that make deceptive "Made in USA" claims only involve information correction. Our study suggests that although information correction is necessary, the FTC may consider awarding civil penalties to the "Made in USA" policy violators because consumers do put a non-neglibile value on the

[4]Endogeneity concerns typically stem from omitted variable bias. Such a bias is less of a concern here, especially in the local window around the FTC investigation, because we directly observe the timing of treatment and control for it. If firms and consumers respond to the removal of this claim, this is explicitly controlled for in our estimation. Supply side responses, beyond price and advertising, will be part of β_1. For example, if firms responded with other marketing activities designed to increase demand after the FTC investigation, the magnitude of our estimate will be biased downwards, i.e., in the absence of these other marketing activities the effect would be more negative.

"Made in USA" attribute. With the categories studied in this paper as an example, the value associated with the "Made in USA" attribute ranges from 1.9% to 19.5% using the RMS data.

4 Empirical Analysis with Field Experiment Data

Although the FTC's investigations into deceptive "Made in USA" claims provide a source of exogenous variation in the presence of country-of-origin claims, they still present challenges to the identification due to the nature of the treatment and the proxies we use for the time of treatment. Therefore, we proceeded to conduct field experiments on eBay to recover consumers' valuation for the country-of-origin feature through a cleaner manipulation in an experimental setting.

eBay provides a good setting to run an experiment: the large volume of buyers it attracts helps us receive multiple bidders per listing and the large number of sellers on the platform implies our experiments do not distort the behavior of market participants. Other papers that have used eBay as an experimental setting include Resnick et al. (2006) who manipulate whether a seller is established or not and Cabral and Li (2015) who manipulate whether buyers are rewarded for providing feedback. In our setting, we manipulate whether the same product is shown with or without country-of-origin information to measure consumers' valuation for the country-of-origin attribute.

4.1 Experiment Design

As highlighted in Section 2.2, we sell pairs of identical screen protectors via three-day auctions on eBay while varying only the presence of country-of-origin information between products in a pair. The experiment is pre-registered at AsPredicted #22138.

In the following sections we delineate our implementation of the treatment and the power calculation done before the experiment to compute the total number of required auctions. We also describe how we prevent consumers from inferring the control product's true country of origin and other steps that help ensure a clean manipulation.

4.1.1 Treatment

An auction listing contains information in the product's title, picture, description, and a drop-down menu of product specifics. We took product descriptions and pictures from the actual marketing materials used by the screen-protector brands. We digitally edited out all brand-relevant information to prevent consumers from identifying the true country of origin from branding information. For the treatment listings, we digitally added "Made in USA" labels to the product pictures, inserted "Made in USA" to the product's title on eBay, highlighted "Made in USA" in bold font and highlighted color at the beginning of the product's description, and indicated the US as the manufacturing region in the product specifics. None of these country-of-origin cues appeared in the control listings. The product was marketed in the same way but without any indication of its origin. Figure 4 shows an example of a pair of listings, Figure 5 shows an example of visual cues of "Made in USA" in product pictures, and Figure 6 shows an example of the product description.

4.1.2 Power Calculation

We determined the required number of auctions, N, using a two-group independent sample t-test with equal variance given by Equation 3:

$$N = 2(t_{\frac{\alpha}{2}} + t_{\beta})^2 (\frac{\sigma}{\delta})^2 \tag{3}$$

where α is the Type-I error rate that determines significance and β is the Type-II error rate that determines statistical power, and $t_{\frac{\alpha}{2}}$ and t_{β} are the corresponding t-statistics. Two inputs required are an estimate of the effect size δ and an estimate of the variance of the outcomes σ.

To estimate the variance of outcomes σ, we conducted a pilot experiment of 10 three-day auctions in the control condition. The resulting variance of transaction prices, the outcome of interest, was \$0.788.

To estimate the effect size δ, we conducted an individual-level demand estimation using the Nielsen HMS data, on households' purchases in categories where the focal products underwent FTC investigation. The estimated effect sizes δ for Gorilla Glue, Loctite glue, Gorilla Tape, and Tramontina cookware were \$0.42, \$0.26, \$0.06, and \$2.79, respectively. Appendix A presents details of the estimation and the results. We chose to use an effect size of \$0.15, which is in between the second-lowest effect size (Loctite glue) and the lowest effect size (Gorilla Tape). We do not use the smallest effect size of \$0.06, because it would require 2,708 auctions per group, making the experiment almost infeasible to run due to the large amount of time (nearly 16 months) and budget required. We therefore choose a more practiable estimate which requires a reasonable time and monetary budget for the experiment.

With an effect size of \$0.15 (δ), outcome variance of \$0.788 (σ), we need 434 pairs of auctions (434 in control group and 434 in treatment group) to achieve a 5% significance level (α) and 80% power ($1 - \beta$). We decided to run 456 pairs of auctions because it is the minimum number needed to make all 76 products have equal number of matching pairs.

We ran eBay auctions on 76 products over a three-month period starting from April 15, 2019. Altogether, we listed 912 auctions over the span of the experiment. The experiment covered 76 products over three months, where in each month, we listed every product in both the treatment and control listings, and each product was in one condition twice in the same week. Using this approach, we achieved 76 product $\times$ 3 month $\times$ 2 condition/month $\times$ 2 time/condition = 912 auctions.[5] Tables 6 and 7 show a detailed experiment schedule. Within any given pair, we counterbalance the timing of the treatment and control listings in a given month, so we do not, for example, always have the treatment listings of that pair in the beginning of a month.

4.1.3 Prevention of Treatment Spillover

We grouped the 76 products into three screen-protector brand groups, corresponding to the three brands Armorsuit, Skinomi, and Spectre Shield. We assigned the treatment condition over groups, meaning a "Made in USA" Armorsuit iPhone XR screen protector and a non "Made in USA" Skinomi Samsung Galaxy S9 Plus screen protector could show up at the same time. But all Armorsuit-branded products were in the same condition at one time, as were the other two groups. We purposely set the grouping in this way to prevent spillover of treatment across models of the same screen-protector brand through similarities in product pictures and descriptions. This setting helps avoid the situation where a consumer searches for screen protectors, sees an Armorsuit iPhone in the treatment condition and an Armorsuit iPad in the control condition and infers both must be made in the US. Note that because we stripped away all brand-related information, users cannot make inferences based on brand names. This measure is out of an abundance of caution, to avoid situations where users infer similarities just based on pictures and/or descriptions.

Within a pair of auctions, we listed the treatment and control auctions in two different weeks with a blank week in between. The blank week prevented spillover of treatment within a pair of auctions. In the current setting, only if a consumer browsed the category for three consecutive weeks would she have the opportunity to discover a product's true country of

[5] Taking as an example, the "iPhone XR Screen Protector" and "iPhone XR Screen Protector Made in USA" and focusing on the first month of the experiment, each product within the pair was listed twice in the same week. The two listings in the same condition were spread out over two three-day windows in a week, for example "iPhone XR Screen Protector Made in USA" (Monday-Thursday in week 1) and "iPhone XR Screen Protector Made in USA" (Thursday-Sunday in week 1). They were paired with the two control listings in week 3, that is, "iPhone XR Screen Protector" (Monday-Thursday in week 3) and "iPhone XR Screen Protector" (Thursday-Sunday in week 3). All products were scheduled in a similar fashion, so that in each month, we spent one week's time listing two three-day treatment auctions starting from Monday and Thursday, and one week's time listing two three-day control auctions. Over the course of the experiment, we counterbalanced the order of which condition to run first.

origin after seeing both listings in a pair. The probability of such lengthy search happening was quite low given the low price of screen protectors.

We operated four seller accounts to run the auctions. Two accounts specialized in selling treatment auctions, and the other two specialized in selling the controls. All seller accounts were similar in terms of selling history and feedback scores (with no more than 42 positive feedbacks at the time of auctions, and no more than 4 negative feedbacks per account). We designed the specialization to prevent consumers from inferring product origin by looking at a seller's concurrent auctions. The seller's historical sales were not observable to consumers. In addition, we asked winning bidders not to leave feedback for the transaction, so that changes in the seller's ratings were minimal. Although we still received scattered feedbacks from buyers - 123 positive, 7 negative and one neutral in total - none revealed the product's country of origin in the feedback.

4.1.4 Other Experiment Logistics

The reservation price for all auctions was set at $0.01 to ensure minimum left censoring of bids. We offered free shipping and a three-business day handling time - a usual practice of other eBay screen protector sellers. This offer helped increase the likelihood of a sale and also helped mask the experimental nature of our listings. We sent debriefing messages to winning bidders after the conclusion of the experiment. An example of debriefing message is presented in Figure 7.

4.2 Data

Data collected from the experiment include three parts, namely, (1) orders report, (2) auction bidding history, and (3) auction page-views history. Whereas the orders report and the auction bidding history were directly provided by eBay, the auction page-views history was scraped from eBay.

The orders report records transaction information for all auctions that were sold, including the transaction price, the buyer's eBay ID, the buyer's name and address (anonymized per IRB's requirement), postal code, and the payment method. The auction bidding history documents the detailed information of each bid for an auction, including bidder ID, bid amount, time of bidding, and the bidder's postal code.

We also collected each bidder's feedback score and her tenure at eBay. An eBay user collects +1 point for each positive rating, no points for each neutral rating, and -1 point for each negative rating over her entire history (Source: eBay). Both buyers and sellers can be rated by each other. Most of our buyers had received 100% positive ratings. Although not every transaction results in a feedback score, assuming the review rate is identical across all buyers, the feedback score proxies for a user's total number of past transactions, and thus her experience with eBay.

The auction page-views history records the cumulative page views of an auction listing every 30 minutes. Page views is defined as the number of unique visitors to an auction listing page per day. We collect this additional information to proxy for the number of potential bidders, required to invert the bid distribution and obtain the valuation distribution.

Table 8 presents summary statistics of the data.

4.3 Results

4.3.1 Transaction Prices

A simple two-sided t-test shows the difference in transaction price between the treatment auctions and control auctions is \$0.067 and significant at the 10% level (t=1.70, p-value=0.090). Equation 4 below specifies our estimation equation:

$$p_i = \beta \cdot MadeInUSA_i + \delta \cdot BuyerExp_i + Weekend_i + Week_i + Device_i + \epsilon_i, \qquad (4)$$

where p_i is the transaction price of auction i. $MadeInUSA_i$ indicates whether the product in that auction was in the treatment condition. $BuyerExp_i$ controls for the buyer's experience with eBay. We use the buyer's feedback score as a proxy for her experience on eBay. In a robustness check, we use her tenure on eBay, and find the results hold. $Weekend_i$ dummy is one when auction i is run in the three-day window from Thursday to Sunday, and zero otherwise. $Week_i$ and $Device_i$ are the week, and device fixed effects. β is the treatment effect that measures the additional willingness to pay for the "Made in USA" attribute.

Table 9 presents results from the regression controlling for (1) whether or not the listing was in the treatment condition, (2) adding week fixed effects and controlling for whether the auction was listed over a weekend or not, (3) adding controls for the buyer's experience with eBay as well as device-model fixed effects, and (4) removing outliers from the data.

The baseline regression shows the average treatment effect on the auction transaction price is \$0.067 and significant at the 10% level, replicating the results of the two-sided t-test. The effect is robust when adding control variables $Weekend_i$ for the two auction windows (Monday through Wednesday and Thursday through Sunday), $Week_i$ for week fixed effects during rotation of treatment, $Device_i$ for device models, and $BuyerExp_i$.

With the full set of controls, the average treatment effect is \$0.092 and is statistically significant. The treatment effect remains robust and statistically significant at \$0.073 when we further drop the 1% extreme bids at the right tail (column (4) in Table 9), assuring that the effect is not solely driven by extreme bids.[6]

We proceed with the specification in the last column of Table 9. Although \$0.073 is small in absolute value, the average transaction price for control auctions is \$0.26 (the constant term in Column (1) Table 9). The estimated treatment effect is economically significant and represents a sizable increase in transaction price compared to the baseline transaction price of \$0.26, representing a 28% increase.

We note that the average transaction price for control auctions, \$0.26, is low, especially when compared to the retail price of the screen protectors. However, many factors may have contributed to the low transaction price. Firstly, we strip away brand information of the screen protectors, making them generics rather than branded items. Generic screen protectors are typically sold at around or below \$1 through eBay auctions. Secondly, buyers in the eBay auction market are likely to be different from the buyers who purchase from retailers, such as Amazon and Apple, in terms of their price sensitivity. Lastly, the uncertainty about transaction fulfillment and seller quality in the eBay auction market may also contribute to the low price point.

In Appendix B, we recover the valuation distribution from the transaction price distribution. The resulting distribution confirms our main finding, that on average, consumers value the Made-in-USA product more than the identical product without the Made-in-USA claims. We now turn to understanding the mechanism through which country-of-origin marketing has a positive effect on consumer valuation.

4.3.2 Discussion of Potential Mechanisms

The literature has documented two main sources as contributing to the country-of-origin effect: as a symbol of identity, and as a predictor of quality. Although the data are not well suited to identify the mechanism, we present directional evidence for each of the accounts[7].

Symbol of Identity The "Made in USA" claim might resonate with consumers' political identity. Because President Trump strongly advocates for American-made products, Republicans may be willing to pay more for otherwise identical but marketed as "Made in USA" products to express their political opinion. Our approach to investigate this mechanism is

[6]This result is robust when we included the ratings our seller accounts received over time, as an additional control.

[7]A third possibility is that the treatment effect only exists among buyers with little experience on eBay, because they may naively believe sellers' claims, whereas experienced buyers are more skeptical about such claims. We check the interaction effect of treatment with buyer experience, $BuyerExp_i$, to evaluate this possibility, finding no significant effect.

to examine the interactive effects between treatment and proxies of political identity.

More specifically, we use the precinct voting result during the 2016 US Presidential Election to represent party affiliation. In each auction, we observed the buyer's 5-digit ZIP Code and obtained the Republican voting share, Rep_i, at the ZIP Code level from the New York Times' precinct voting result (NYTimes 2018).

We regress transaction prices on the treatment variable interacted with the Republican share, Rep_i, while controlling for ZIP-Code-level median income, ZIP-Code-level percentage of population native born, buyer experience, as well as weekend, week, and device model fixed effects. We find a 10% decrease in the Republican share is correlated with a \$0.043 increase in the treatment effect. In other words, the treatment effect significantly increases with the Democratic share of votes in the ZIP Code (column (2) Table 10).

One interpretation of this result is that Democrats value "Made in USA" more than Republicans do. Anecdotal evidence might support this interpretation: The FTC's two Democratic appointees voted for imposing financial penalties on companies violating the "Made in USA" policy, whereas the three Republican commissioners appointed by President Trump voted against it (NYTimes 2019).

Another possibility is that the interactive effect might be driven by the degree of political polarization - in majority-Democrat ZIP Codes, Republicans become more expressive of their stance on American-made products. To examine this account, we construct a political polarization index, namely, $Polar_i = |Rep_i(1 - Rep_i) - 0.25|$. This index measures the degree of deviation from a 50-50 split of voting shares between Democratic and Republican candidates. We regress transaction prices on the treatment variable interacted with both Rep_i and the polarization index $Polar_i$, while controlling for ZIP-Code-level median income, ZIP-Code-level percentage of population native born, as well as buyer experience and weekend, week, and device-model fixed effects. We found the interaction effect on the Republican share remains significantly negative and the interaction effect on polarization is positive but insignificant (Table 10 column (3)). We conclude that positive valuation for "Made in USA" products can be explained by political identity, where Democrats value American-made products more than Republicans do, and the degree of political polarization increases the effect, although insignificantly.

Predictor of Quality The second explanation is that "Made in USA" claims signal higher quality and so consumers are willing to pay a premium for perceived quality improvement. We investigate this explanation by looking into whether the treatment effect is higher among consumers who care more about product quality.

Because perceived product quality is not observed in the data, we use internet browsing behavior in the comScore data to construct a measure for quality sought out by consumers. More specifically, we calculate the average browsing duration on product-review websites per capita by ZIP Code. We also examine alternative measures of quality pursuit, for example, average number of web browsing sessions and average number of pages viewed. We examine website browsing at seven popular product review websites[8] in 2014. If a consumer cares a lot about product quality, she might spend more time browsing product-review websites. Therefore, if the "Made in USA" effect is driven by the perception that the label is perceived to be a signal of quality, we would expect a positive interaction effect between treatment and the browsing duration on product-review websites.

We regress transaction prices on the treatment variable interacted with the quality index, $Qual_i$, while controlling for ZIP-Code-level median income, ZIP-Code-level percentage of population native born, as well as buyer experience, and weekend, week, and device-model fixed effects. We find a marginally significant positive interaction effect of treatment and the browsing duration on product-review websites, that is, a 10-minute increase in browsing duration on product-review websites is correlated with an additional \$0.03 valuation for "Made in USA" products. The effect is robust when using the number of web browsing sessions or number of pages viewed (see Table 11).

Discussion We present initial evidence that suggests the "Made in USA" treatment effect could be driven by expression of one's political beliefs and by serving as a signal of product quality. We show the buyer's sophistication in terms of her experience with eBay does not have much explanatory power. To quantify the relative impact of symbol of identity and predictor of quality, we estimate a specification that includes both effects. Table 12 presents the results of this regression. Using results from column (4), for an average ZIP Code in the experiment with a 37% Trump vote share and nine minutes spent browsing product-review websites, the treatment effect attributable to the political identity is five times greater in magnitude than that attributable to the predictor-of-quality mechanism. As noted at the beginning of this section, these results serve as directional evidence and should not be interpreted causally.

4.4 Economic Significance

Given the consumer-valuation difference estimated from the eBay field experiment, would moving its production site from overseas to the US and claiming its products as "Made in USA" make economic sense for a firm? Although we do not observe supply-side factors, a

[8] Domain names include cnet.com, consumersearch.com, consumerreports.org, viewpoints.com, tomsguide.com, thewirecutter.com, and slant.co.

back-of-the-envelope analysis based on the demand-side effects sheds light on this question. Suppose a hypothetical firm producing screen protectors overseas sets the retail price at \$0.26 (the average transaction price of control listings in the experiment). If the screen protector had been produced in the US and marketed as "Made in USA," consumers' willingness to pay for this product would have increased by \$0.07. Therefore, if moving production to the US raises the variable cost by more than \$0.07, the firm's profit margins would get hurt. Borrowing the gross profit margin of a leading screen-protector producer, we infer the variable cost of production of screen protectors to be 43%.[9] A firm can therefore bear up to a 0.07/(0.26*0.43)=63% increase in variable cost without having its profit margin hurt. That is, if the variable production cost in the US is 1.63 times more than the cost overseas, moving the production location from overseas to the US would not be justified. As a benchmark of comparison, the manufacturing hourly compensation in the US is 9 times more than that in China (The Conference Board 2018). In the extreme case, where all variable costs are attributable to labor, the demand for "Made in USA" is insufficient to justify a relocation. Knowing the exact breakdown of the cost structure will help shed better light on the feasibility of the relocation.

5 Conclusion

This paper aims to understand firms' incentives to make deceptive country-of-origin claims, and finds evidence of an increase in demand for products marketed as "Made in USA." Using both an observational study that exploits removal of the "Made in USA" claim resulting from a regulatory investigation, as well as a field experiment conducted on eBay where the presence of the claim was exogeneously varied, we find evidence that the "Made in USA" claim has a positive impact on demand.

Although our field experiment was limited to screen protectors, we find that even in this category that some might argue is commoditized, consumers are willing to pay 28% more for the "Made in USA" feature, suggesting this effect might be more prominent in other categories. We hope future research documents the presence and magnitude of this effect across different categories. Furthermore, testing differences across different countries of origin would also shed light on the value of different country-of-origin attributes.

Our back-of-the envelope calculations suggest the increase in demand associated with the "Made in USA" attribute might be insufficient to justify moving production locations from overseas to the US. Nevertheless, the increased demand creates incentives for firms to make

[9]Per 10-K filing of ZAGG, a publicly listed screen protector producer, in fiscal year 2008, the gross profit margin of screen protectors is 57%. The variable cost of production is therefore 100%-57%=43%.

deceptive country-of-origin marketing claims. Although there are no penalties associated with such deceptive practices currently, our calculations suggest consumers place a value ranging from 1.9% to 19.5% (using the Nielsen data) and 28% (using the eBay field experiment) on the "Made in USA" attribute. This estimate provides a benchmark for awarding civil penalties to firms making deceptive claims. Moreover, regulatory authorities can use the methods in this paper to identify a case-specific estimate for such penalties.

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Tables

Table 1: Investigated Companies and Their Remediation Actions

Brand	Category	Date	Remediations
Tramontina	Cookware	13-Nov-15	(1) updating claims to state affected products contain imported components (2) removing from all of its product webpages a banner containing the word “USA” superimposed over an image of the flag (3) revising the website to clarify Tramontina’s manufacturing process (4) contacting third-party retailers to provide updated claims and product descriptions.
Loctite	Glue	18-Sep-15	updating labels to state “US Made: US/Foreign Materials”
Gorilla	Tape, Glue	5-Jun-15	(1) updating claims to state “Made in USA with Domestic and Imported Materials” or “Made in Germany. Bottled in USA,” as appropriate (2) revising product packaging to make qualifications more clear and prominent (3) updating product descriptions and photos on company website and social media (4) contacting third-party retailers to provide updated product claims, descriptions, and images.

Source:
https://www.ftc.gov/tips-advice/business-center/legal-resources?type=closing_letter&field_consumer_protection_topics_tid=234.
The product manufacturer information is not acquired from the Nielsen database.

Table 2: Products Sold in eBay Field Experiments

Model		**Retail Price**	**Model**		**Retail Price**
Device	Brand		Device	Brand	
iPhone Xs Max	Armorsuit	$7.85	Samsung Galaxy S9 Plus	Armorsuit	$7.85
	Skinomi	$7.85		Skinomi	$7.85
	Spectre Shield	$7.82		Spectre Shield	$7.82
iPhone XR	Armorsuit	$7.85	Samsung Galaxy S9 / S8	Armorsuit	$7.85
	Skinomi	$7.85		Skinomi	$7.85
	Spectre Shield	$7.82		Spectre Shield	$7.82
iPhone XS / X	Armorsuit	$7.85	Samsung Galaxy S7	Armorsuit	$7.85
	Skinomi	$7.85		Skinomi	$7.85
	Spectre Shield	$7.82		Spectre Shield	$7.82
iPhone 8 Plus / 7 Plus	Armorsuit	$7.85	Samsung Galaxy S7 Edge	Armorsuit	$7.85
	Skinomi	$7.85		Skinomi	$7.85
	Spectre Shield	$7.82		Spectre Shield	$7.82
iPhone 8 / 7	Armorsuit	$7.85	Samsung Galaxy Note 9	Armorsuit	$7.85
	Skinomi	$7.85		Skinomi	$7.85
	Spectre Shield	$7.82		Spectre Shield	$7.82
iPhone SE	Armorsuit	$7.85	Samsung Galaxy Note 8	Armorsuit	$7.85
	Skinomi	$7.85		Skinomi	$7.85
	Spectre Shield	$4.48		Spectre Shield	$7.82
Apple Watch 44mm	Armorsuit	$7.85	Google Pixel 3 XL	Armorsuit	$7.80
	Skinomi	$7.85		Skinomi	$7.85
	Spectre Shield	$7.82		Spectre Shield	$7.82
Apple Watch 42mm	Armorsuit	$7.85	Google Pixel 2 XL	Armorsuit	$7.85
	Skinomi	$7.85		Skinomi	$7.85
	Spectre Shield	$7.82		Spectre Shield	$7.82
Apple Watch 40mm	Armorsuit	$8.95	Google Pixel XL	Armorsuit	$7.85
	Skinomi	$7.85		Skinomi	$7.85
	Spectre Shield	$7.82		Spectre Shield	$7.82
Apple Watch 38mm	Armorsuit	$7.85	Google Pixel 3	Armorsuit	$7.86
	Skinomi	$7.85		Skinomi	$7.85
	Spectre Shield	$7.82		Spectre Shield	$7.82
iPad Pro 9.7”	Armorsuit	$8.95	Google Pixel 2	Armorsuit	$7.85
	Skinomi	$8.95		Skinomi	$7.85
iPad Pro 10.5”	Armorsuit	$9.95		Spectre Shield	$7.82
	Skinomi	$11.95	Google Pixel	Armorsuit	$7.85
iPad Pro 11”	Armorsuit	$8.95		Skinomi	$7.85
	Skinomi	$11.95		Spectre Shield	$7.82
iPad Pro 12.9”	Armorsuit	$11.85			
	Skinomi	$9.95			
iPad mini 4	Armorsuit	$8.95			
	Skinomi	$10.00			

Note: The retail price information is collected from Amazon. It is not acquired from the Nielsen database.

Table 3: Control Brands for Diff-in-Diffs Analysis

	Gorilla (Glue)	Loctite (Glue)	Gorilla (Tape)	Tramontina (Cookware)
Panel A. HMS	Loctite	Krazy Glue	Seal-it One Arm Bandit	Lodge Logic
	Krazy Glue	Super Glue	Shurtape Frogtape	Farberware
Panel B. RMS	Loctite	Krazy Glue	Seal-it One Arm Bandit	Kitchen Essentials
	Krazy Glue	Duro	Seal-it	The Original Green Pan
			Duck	Lodge Logic
				Kitchen Aid

Table 4: Diff-in-Diffs with HMS Data

	Dependent variable:			
	Log Weekly Sales, Aggregate			
	Gorilla (Glue)	Loctite (Glue)	Gorilla (Tape)	Tramontina (Cookware)
	(1)	(2)	(3)	(4)
Panel A. Diff-in-Diffs baseline				
Treatment	**-0.041**	**-0.118**	**0.544^{***}**	**-0.039**
	(0.094)	**(0.114)**	**(0.142)**	**(0.326)**
Panel B. Diff-in-Diffs with advertising duration and price controls				
Treatment	**-0.030**	**-0.132**	**0.602^{***}**	**-0.007**
	(0.131)	**(0.116)**	**(0.204)**	**(0.357)**
Advertising Duration	-0.004	-0.045^{**}	0.020	
	(0.012)	(0.021)	(0.025)	
Price	-0.992	1.179	3.853^{**}	-0.483
	(1.229)	(1.644)	(1.441)	(1.963)
Observations	48	48	48	48

Note: *p<0.1; **p<0.05; ***p<0.01

The regressions use a ± 8-week window of Nielsen HMS data around the FTC closing-letter date of each affected brand. Brand fixed effects and week fixed effects are included.

Table 5: Diff-in-Diffs with RMS Data

	Dependent variable:			
	Log Weekly Sales, Store Level			
	Gorilla (Glue)	Loctite (Glue)	Gorilla (Tape)	Tramontina (Cookware)
	(1)	(2)	(3)	(4)
Panel A. Diff-in-Diffs baseline				
Treatment	**−0.067*****	**−0.059*****	**0.134*****	**−0.154*****
	(0.006)	**(0.005)**	**(0.006)**	**(0.032)**
Panel B. Diff-in-Diffs with advertising duration and price controls				
Treatment	**−0.019*****	**−0.061*****	**0.133*****	**−0.195*****
	(0.007)	**(0.005)**	**(0.009)**	**(0.033)**
Advertising Duration	0.010***	0.006***	−0.0003	−0.009*
	(0.001)	(0.002)	(0.001)	(0.005)
Price	0.039***	0.093***	0.142***	0.199***
	(0.014)	(0.019)	(0.015)	(0.032)
Observations	1,439,200	1,308,352	1,144,928	183,135

Note: *p$<$0.1; **p$<$0.05; ***p$<$0.01

The regressions use a ± 8-week window of Nielsen RMS data around the FTC closing-letter date of each affected brand. Store-brand fixed effects and store-week fixed effects are included. Standard errors clustered at store level.

Table 6: eBay Field-Experiments Schedule

Week	Brand	Mon - Thu	Thu - Sun	Seller Account
1	Armorsuit	T (#27)	T (#27)	reiltas
	Spectre Shield	T (#22)	T (#22)	reiltas
	Skinomi	C (#27)	C (#27)	viottis
2	-	-	-	-
3	Armorsuit	C (#27)	C (#27)	viottis
	Spectre Shield	C (#22)	C (#22)	viottis
	Skinomi	T (#27)	T (#27)	reiltas
4	-	-	-	-

Notes: The table shows experiment schedule in weeks 1-4. In weeks 5-12, we counterbalanced the order of treatment; that is, Armorsuit and Spectre Shield started with control (treatment) and Skinomi started with treatment (control) in week 5 (9). We used seller accounts reiltas (T) and viottis (C) in weeks 1-4, olielli (T) and chillpam (C) in weeks 5-8, and reiltas (T) and viottis (C) in weeks 9-12. Seller ratings accumulated over time, but remained stable over a four-week period.

Table 7: Experiment Scehdule

Autction End Date	Days of Week	Week	Treatment	Brand (N)
2019-04-18	Mon-Thu	1	0	Skinomi (27)
2019-04-18	Mon-Thu	1	1	Armorsuit (27) + Spectre Shield (22)
2019-04-21	Thu-Sun	1	0	Skinomi (27)
2019-04-21	Thu-Sun	1	1	Armorsuit (27) + Spectre Shield (22)
2019-05-02	Mon-Thu	3	0	Armorsuit (27) + Spectre Shield (22)
2019-05-02	Mon-Thu	3	1	Skinomi (27)
2019-05-05	Thu-Sun	3	0	Armorsuit (27) + Spectre Shield (22)
2019-05-05	Thu-Sun	3	1	Skinomi (27)
2019-05-16	Mon-Thu	5	0	Armorsuit (27) + Spectre Shield (22)
2019-05-16	Mon-Thu	5	1	Skinomi (27)
2019-05-19	Thu-Sun	5	0	Armorsuit (27) + Spectre Shield (22)
2019-05-19	Thu-Sun	5	1	Skinomi (27)
2019-05-30	Mon-Thu	7	0	Skinomi (27)
2019-05-30	Mon-Thu	7	1	Armorsuit (27) + Spectre Shield (22)
2019-06-02	Thu-Sun	7	0	Skinomi (27)
2019-06-02	Thu-Sun	7	1	Armorsuit (27) + Spectre Shield (22)
2019-06-13	Mon-Thu	9	0	Skinomi (27)
2019-06-13	Mon-Thu	9	1	Armorsuit (27) + Spectre Shield (22)
2019-06-16	Thu-Sun	9	0	Skinomi (27)
2019-06-16	Thu-Sun	9	1	Armorsuit (27) + Spectre Shield (22)
2019-06-27	Mon-Thu	11	0	Armorsuit (27) + Spectre Shield (22)
2019-06-27	Mon-Thu	11	1	Skinomi (27)
2019-06-30	Thu-Sun	11	0	Armorsuit (27) + Spectre Shield (22)
2019-06-30	Thu-Sun	11	1	Skinomi (27)

Note: The last column shows the number of unique products (N) listed in that three-day period.

Table 8: Summary Statistics of eBay Experiment Auctions

	Treatment	Control
Number of Observations	456	456
Number of Unsold Auctions	48	28
Transaction Price	0.33	0.26
	(0.66)	(0.53)
Page Views	9.57	9.36
	(6.29)	(5.89)
Number of Bidders	2.05	2.08
	(1.19)	(1.04)

Table 9: eBay Field-Experiment Transaction Price

	Dependent variable:			
	Transaction Price			
	(1)	(2)	(3)	(4)
Made in USA	**0.067***	**0.080****	**0.092****	**0.073*****
	(0.040)	**(0.038)**	**(0.042)**	**(0.023)**
Buyer Experience			-0.00001^{**}	-0.00001^{***}
			(0.00001)	(0.00000)
Weekend		-0.018	-0.039	-0.035
		(0.036)	(0.041)	(0.023)
Constant	0.261***	0.220**	0.251**	0.276***
	(0.028)	(0.105)	(0.113)	(0.062)
Drop 1% Right Tail				Y
Week FE		Y	Y	Y
Device Model FE		Y	Y	Y
Observations	912	912	825	811
R^2	0.003	0.193	0.187	0.310
Adjusted R^2	0.002	0.163	0.152	0.280
Residual Std. Error	0.599 (df = 910)	0.549 (df = 878)	0.573 (df = 790)	0.317 (df = 776)
F Statistic	2.882* (df = 1; 910)	6.374*** (df = 33; 878)	5.354*** (df = 34; 790)	10.246*** (df = 34; 776)
Note:				*p<0.1; **p<0.05; ***p<0.01

Table 10: Made in USA and Symbol of Identity

	Dependent variable:		
	Transaction Price		
	(1)	(2)	(3)
Made in USA	0.073^{***}	0.258^{***}	0.221^{**}
	(0.023)	(0.053)	(0.101)
Republican Share		0.052	0.011
		(0.094)	(0.133)
Polarization			−0.185
			(0.430)
Made in USA x Republican Share		**−0.430*****	**−0.372****
		(0.124)	**(0.183)**
Made in USA x Polarization			**0.272**
			(0.635)
log(Income)		-0.098^{**}	-0.099^{**}
		(0.039)	(0.039)
Native-born		0.035	0.035
		(0.110)	(0.110)
Constant	0.276^{***}	1.328^{***}	1.361^{***}
	(0.062)	(0.450)	(0.463)
Buyer Experience		Y	Y
Observations	811	705	705
R^2	0.310	0.324	0.324
Adjusted R^2	0.280	0.286	0.284
Residual Std. Error	0.317 (df = 776)	0.316 (df = 666)	0.316 (df = 664)
F Statistic	10.246^{***} (df = 34; 776)	8.408^{***} (df = 38; 666)	7.972^{***} (df = 40; 664)
Note:			*p<0.1; **p<0.05; ***p<0.01

The sample excludes observations in the 1% right tail. Weekend, week, and device-model fixed effects are controlled for. Covariate $log(Income)$ controls for the median level of income of buyer's ZIP Code and covariate Native-born controls for the percentage of population born native in the buyer's ZIP Code.

Table 11: Made in USA and Predictor of Quality

	Dependent variable:			
	Transaction Price			
	(1)	(2)	(3)	(4)
Made in USA	0.073***	0.064**	0.067***	0.054**
	(0.023)	(0.025)	(0.026)	(0.026)
Browsing Duration		−0.001		
		(0.001)		
Browsing Session				−0.004
				(0.003)
Pages Viewed			−0.001	
			(0.001)	
Made in USA x Browsing Duration		**0.003****		
		(0.001)		
Made in USA x Pages Viewed			**0.003***	
			(0.001)	
Made in USA x Browsing Session				**0.011*****
				(0.004)
log(Income)		−0.107***	−0.103***	−0.111***
		(0.037)	(0.037)	(0.037)
Native-born		−0.119	−0.122	−0.115
		(0.082)	(0.082)	(0.081)
Constant	0.276***	1.579***	1.540***	1.616***
	(0.062)	(0.430)	(0.431)	(0.429)
Buyer Experience		Y	Y	Y
Observations	811	784	784	784
R^2	0.310	0.322	0.317	0.326
Adjusted R^2	0.280	0.288	0.282	0.291
Residual Std. Error	0.317 (df = 776)	0.305 (df = 745)	0.306 (df = 745)	0.304 (df = 745)
F Statistic	10.246*** (df = 34; 776)	9.328*** (df = 38; 745)	9.085*** (df = 38; 745)	9.466*** (df = 38; 745)
Note:				*p<0.1; **p<0.05; ***p<0.01

The sample excludes observations in the 1% right tail. Weekend, week, and device-model fixed effects are controlled for. Covariate $log(Income)$ controls for the median level of income of buyer's ZIP Code and covariate Native-born controls for the percentage of population born native in the buyer's ZIP Code.

Table 12: Mechanisms of "Made in USA" Effect

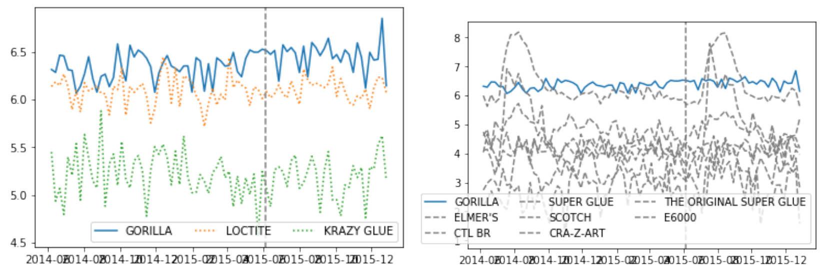
	Dependent variable:			
	Transaction Price			
	(1)	(2)	(3)	(4)
Made in USA	0.073***	0.221**	0.064**	0.182*
	(0.023)	(0.101)	(0.025)	(0.102)
Republican Share		0.011		−0.007
		(0.133)		(0.134)
Polarization		−0.185		−0.180
		(0.430)		(0.428)
Browsing Duration			−0.001	−0.001
			(0.001)	(0.001)
Made in USA x Republican Share		**−0.372****		**−0.338***
		(0.183)		**(0.183)**
Made in USA x Polarization		**0.272**		**0.367**
		(0.635)		**(0.632)**
Made in USA x Browsing Duration			**0.003****	**0.003****
			(0.001)	**(0.001)**
log(Income)		−0.099**	−0.107***	−0.102***
		(0.039)	(0.037)	(0.039)
Native-born		0.035	−0.119	0.036
		(0.110)	(0.082)	(0.110)
Constant	0.276***	1.361***	1.579***	1.397***
	(0.062)	(0.463)	(0.430)	(0.462)
Observations	811	705	784	705
R^2	0.310	0.324	0.322	0.334
Adjusted R^2	0.280	0.284	0.288	0.292
Residual Std. Error	0.317 (df = 776)	0.316 (df = 664)	0.305 (df = 745)	0.315 (df = 662)
F Statistic	10.246*** (df = 34; 776)	7.972*** (df = 40; 664)	9.328*** (df = 38; 745)	7.904*** (df = 42; 662)
Note:				*p<0.1; **p<0.05; ***p<0.01

The sample excludes observations in the 1% right tail. Weekend, week, and device-model fixed effects are controlled for. Covariate $log(Income)$ controls for the median level of income of buyer's ZIP Code and covariate Native-born controls for the percentage of population born native in the buyer's ZIP Code.

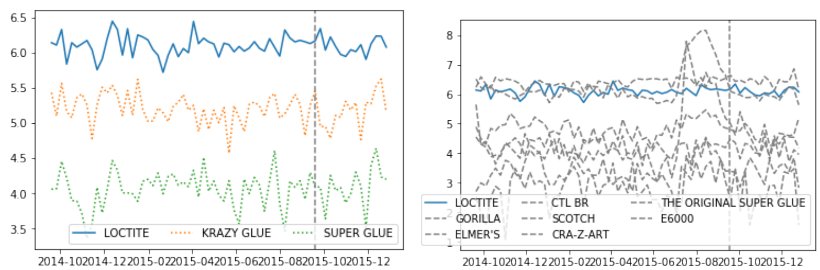
Figures

Figure 2: Parallel Sales Trends of Control Brands and Non-parallel Sales Trends of Excluded Brands - HMS

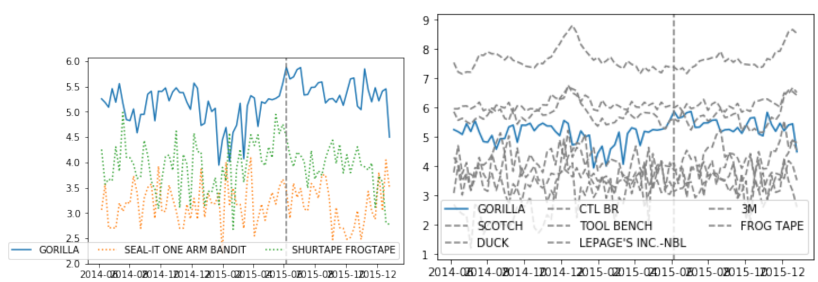
(a) Gorilla Glue



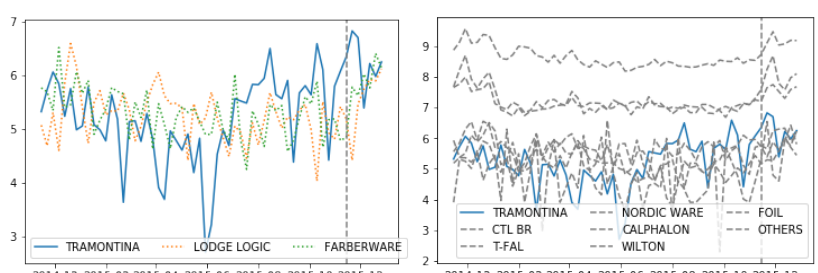
(b) Loctite Glue



(c) Gorilla Tape



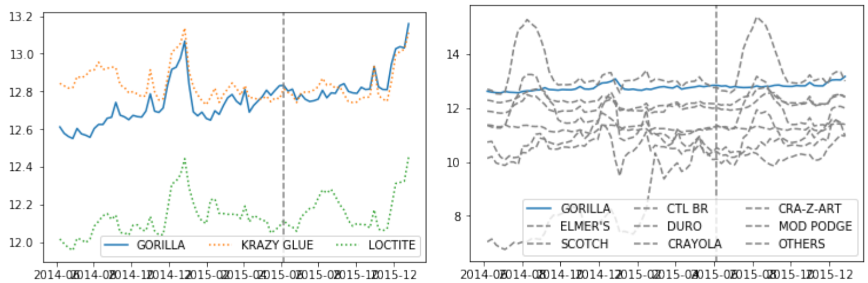
(d) Tramontina Cookware



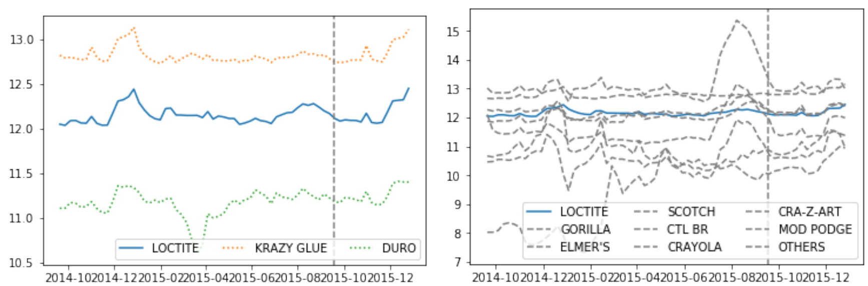
Note: This figure shows sales trend of focal brands and control-brand candidates prior to the FTC investigation in the Nielsen HMS data. Figures in the left panel contain focal brands and the top-selling brands included in the control group, whereas figures in the right panel contain the focal brand and the other top-selling brands excluded from control group due to non-parallel sales trends.

Figure 3: Parallel Sales Trends of Control Brands and Non-parallel Sales Trends of Excluded Brands - RMS

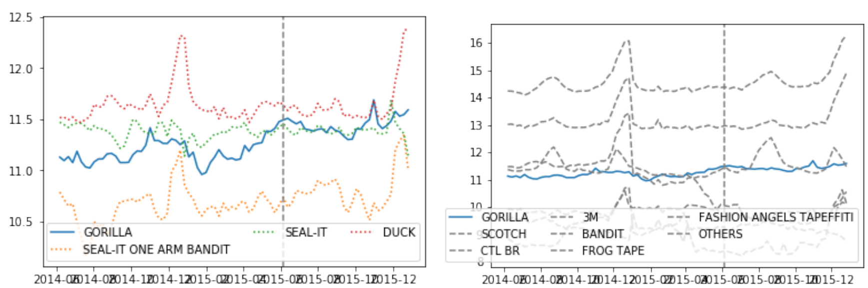
(a) Gorilla Glue



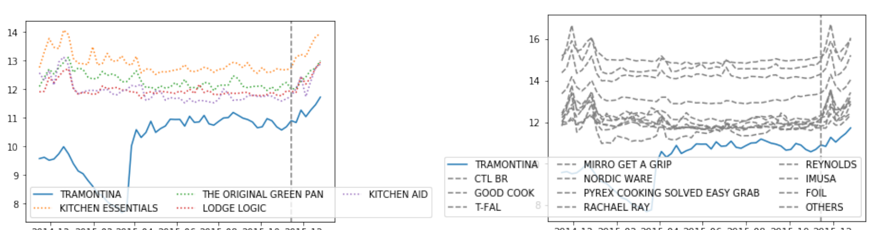
(b) Loctite Glue



(c) Gorilla Tape



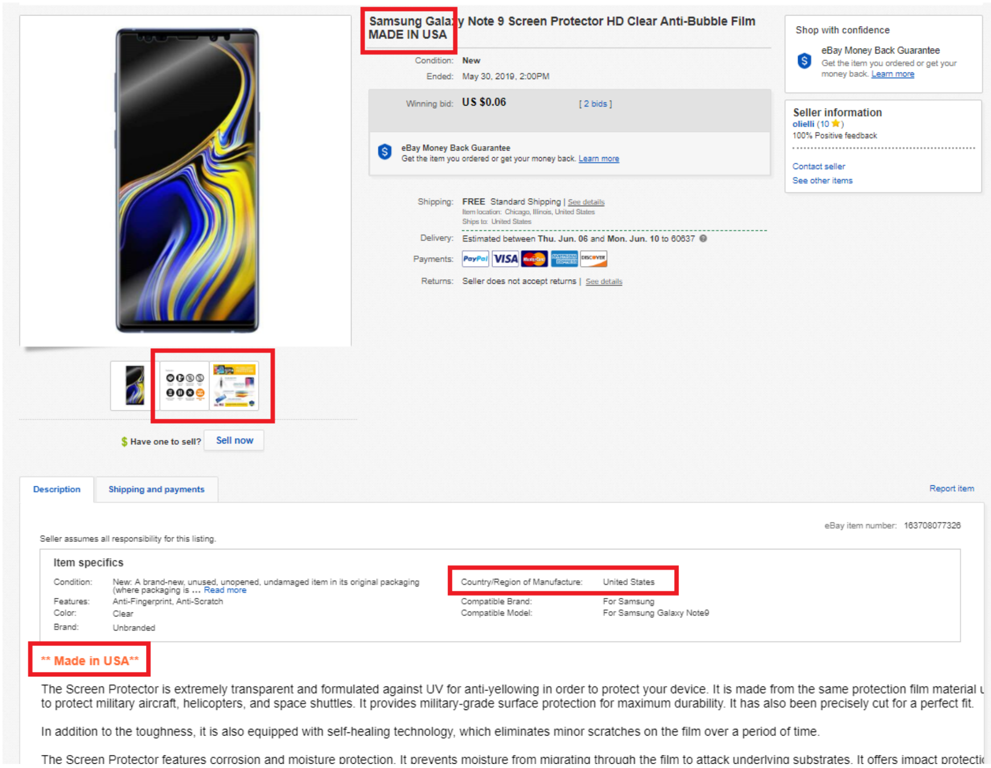
(d) Tramontina Cookware



Note: This figure shows sales trend of focal brands and control-brand candidates prior to the FTC investigation in the Nielsen RMS data. Figures in the left panel contain focal brands and the top-selling brands included in the control group, whereas figures in the right panel contain focal brands and the other top-selling brands excluded from control group due to non-parallel sales trends.

Figure 4: An Example of Treatment and Control Listings

(a) Treatment Listing



(b) Control Listing

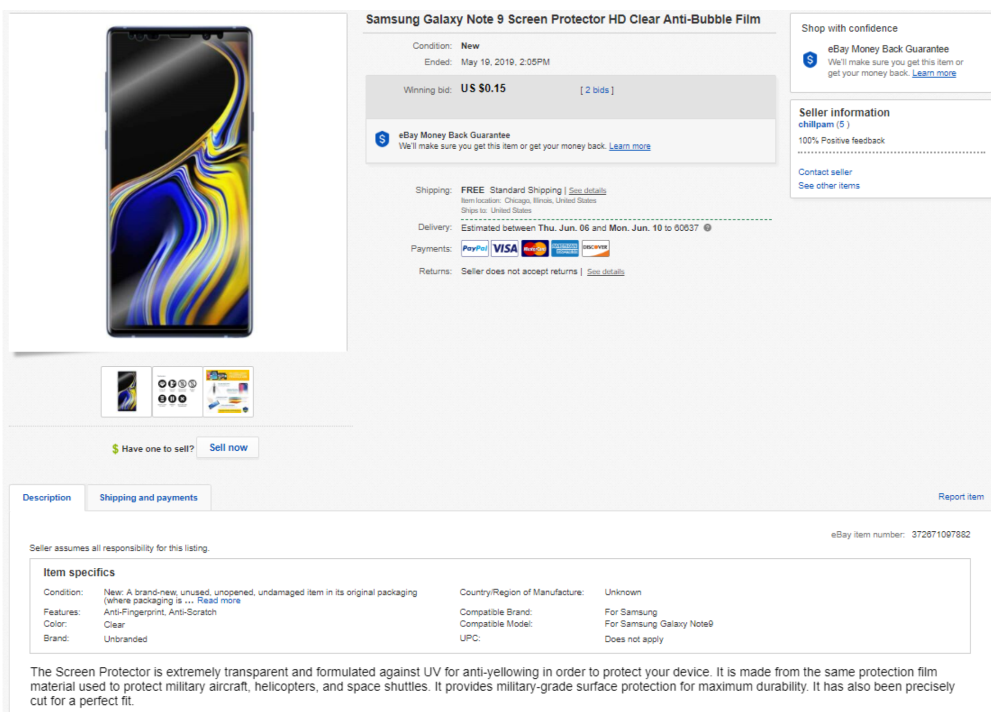


Figure 5: “Made in USA” Cues in Product Picture

(a) Treatment Listing

(b) Control Listing

Notes: This figure provides an example of product pictures of Armorsuit’s Samsung Galaxy Note 9 screen protector in treatment and control listings. In both treatment and control listings, the picture of a cellphone is the main photo that shows up in the search result. In treatment listings, ”Made in USA” cues show up in the form of a highlighted feature, as well as a flag of the USA.

Figure 6: An Example of Product Description

**** Made in USA****

The Screen Protector is extremely transparent and formulated against UV for anti-yellowing in order to protect your device. It is made from the same protection film material used to protect military aircraft, helicopters, and space shuttles. It provides military-grade surface protection for maximum durability. It has also been precisely cut for a perfect fit.

In addition to the toughness, it is also equipped with self-healing technology, which eliminates minor scratches on the film over a period of time.
The Screen Protector features corrosion and moisture protection. It prevents moisture from migrating through the film to attack underlying substrates. It offers impact protection against plastics, metals, and composites from sand, rock, rain, and debris. It protects against general wear, rubbing, chaffing, abrading, scraping, etc. It is easy to install and provides long term edge sealing protection.

With the Screen Protector, you can finally relax and stop worrying about permanent scratches!

Package Contents:

- Samsung Galaxy Note 9 Case-Friendly Screen Protector
- 1 Installation Squeegee
- 1 Spray Bottle Solution
- 1 Microfiber Cloth
- Installation Instructions

Notes: This is an example of product description of Armorsuit Samsung Galaxy Note 9 screen protector in treatment listings. The first line, i.e. the ”Made-in-USA” disclaimer, does not appear in the control listing.

Figure 7: An Example of Debriefing Message

Dear [user id],

Thank you for completing the auction with us. The item you bid for has been shipped. You should expect to receive it by YYMMDD.

We'd like to let you know that this auction is a part of an academic research study at <Institute>. The goal of our research is to measure how does country-of-origin claims affect consumer's valuation of the product. We want to see how much does consumer value differently for the same product when it is marketed with or without a "made in usa" claim. Only the time and value of your bid will be used for future data analysis. None of your identifiable buyer information, e.g. buyer id and delivery address, will be disclosed in the study.

Please do not leave any comments for us, as this could affect the results of the study. If you would like to receive a copy of the final report of this study (or a summary of findings) when it is completed, please feel free to contact us. If you have any questions or concerns regarding this study, its purpose or procedures, or if you have a research-related problem, please feel free to contact us as well. If you have questions or concerns about your rights as a participant in this research study, you can contact the <Institute> IRB by email at <email> or by telephone at <phone>.

Please keep a copy of this message for your future reference. Once again, thank you for your participation in this study!

Best regards,

<Contact Name>
<Institute>
<Email>

Notes: Information in the "<>" brackets is anonymized for blind review. Actual information provided to participants will be disclosed if required.

A Individual-level Demand Estimation

To the extent that purchase choice is made at the household level, we are also interested in how individual households respond to changes in country-of-origin claims. By modeling consumer demand at the individual level, we can include the price paid by that household and also account for the outside option in the demand estimation.

We include Nielsen HMS households that purchased any products in the focal product module at least once in the year of and the year after the FTC case (i.e., 2015 and 2016 for all three brands). This selection trades in a relevant market for each brand for a national representative panel.

We retain all trips for each panelist regardless of whether a purchase in the focal product module is made. When panelists do not purchase products in the focal product module in a trip, we record it as choosing the outside option. We account for multiple-unit purchases in one trip by treating each additional unit as a separate observation: When a household purchases more than one unit of a product, we replicate those observations N times, where N is the number of units purchased of that product on that purchase occasion.

Within a product module, we assume a household makes a choice from J+1 options in every trip. We include no purchase, focal brand, as well as J-1 other brands (control brands in diff-in-diffs estimation and a composite brand including all brands not in the control group) as choice alternatives.

The price of the chosen option is directly observed from the HMS data, the price for options not purchased are imputed from Nielsen RMS data, and the advertising duration is matched with Nielsen AdIntel data. When store ZIP Code information is available for a trip, we match prices of unpurchased alternatives with market-brand-week-level prices calculated from Nielsen RMS data. When store ZIP Code is missing, we use nation-brand-week-level price instead. All prices are standardized as per-unit price (e.g., price per ounce for glue, and price per count for tape and cookware). When imputing price from Nielsen RMS data, we define markets to be 3-digit ZIP Code areas. Market-brand-week prices are a sales weighted average of standardized price across all UPCs for a given brand in a week in a 3-digit ZIP Code area. Nation-brand-week prices are a sales weighted average of standardized price across all UPCs and ZIP Codes for a brand in a week. Similarly, we construct total advertising duration at DMA-brand-week level from national and spot TV advertising in the Nielsen AdIntel data and match it with DMA of the panelists. When panelist DMA is not available, we use national advertising instead.

A.1 Utility Specification

We specify an individual household i's utility for product j in trip t as

$$
\begin{aligned}
u_{ijt}(\theta) &= \alpha_j + \beta \mathbb{1}_{\{j=j_{FTC}, t \geq \tau_{FTC}\}} + \tau \mathbb{1}_{\{j \neq 0, t \geq \tau_{FTC}\}} + \gamma p_{ijt} + \phi Ad_{ijt} + \epsilon_{ijt} \\
u_{i0t}(\theta) &= \epsilon_{i0t},
\end{aligned}
$$

where α_j is the brand preference before changing the "Made in USA" claim, τ measures the common time trend for inside options, γ is the price coefficient, ϕ is the advertising coefficient, and ϵ_{ijt} is a T1EV random utility shock. The dummy variable $\mathbb{1}\{j = j_{FTC}, t > \tau_{FTC}\}$ indicates whether "Made in USA" claims were removed (due to the FTC investigation), and so β measures the consumer utility from a product's US-origin attribute. p_{ijt} is the price of product j for household i in trip t, and Ad_{ijt} is the advertising duration of product j in household i's DMA at the time of trip t. The utility for no purchase option ($j = 0$) is normalized to ϵ_{i0t}.

The willingness to pay for the "Made in USA" attribute is given by $\delta = |\frac{\beta}{\gamma}|$, which is used as an estimate of the effect size to compute the required number of auctions in Section 4.1.2.

The probability of household i choosing brand j at time t is $P_{ijt}(\theta) = \frac{\exp(u_{ijt}(\theta))}{\sum_{k=0}^{J} \exp(u_{ikt}(\theta))}$. The log-likelihood function is

$$
L(\theta) = \sum_i \sum_{t_i} \sum_j y_{ijt} \log P_{ijt}(\theta)
$$

where y_{ijt} is a binary variable that is 1 when brand j is purchased at time t by household i.

A.2 Results

Individual demand-estimation results are presented in Table 13. The estimation results show that consumers place a significantly positive value on "Made in USA" attribute for Gorilla Glue and Loctite glue, an insignificant value for Gorilla Tape, and a significantly negative value for Tramontina cookware. The Tramontina cookware results are inconsistent with the HMS diff-in-diffs regression. To understand the reason behind this inconsistency, we include only the controls used in the aggregate diff-in-diffs analysis, and find our results to be consistent. We therefore conclude that the inconsistency is driven by the inclusion of the outside option and the existence of non-control brands (e.g. Others) in the category.

Table 13: Individual Demand Estimation with HMS Data

		(1)		(2)		(3)		(4)	
		Gorilla glue		Loctite glue		Gorilla Tape		Tramontina cookware	
ΔMade in USA	β		**-0.322**		**-0.212**		**0.129**		**2.051**
			(0.062)		**(0.075)**		**(0.118)**		**(0.145)**
Price	γ		-0.764		-0.804		-2.292		-0.736
			(0.004)		(0.004)		(0.010)		(0.002)
Ad Duration	δ		-0.0001		-0.00004		-0.00003		-0.00002
			(0.00000)		(0.00000)		(0.00001)		(0.00000)
Common time trend	τ		0.422		0.594		-0.059		0.164
			(0.029)		0.022		(0.045)		(0.014)
Brand Intercept	α_j								
		Gorilla	-0.496	Loctite	6.084	Gorilla	6.285	Tramontina	-0.454
			(0.046)		(0.068)		(0.096)		(0.110)
		Loctite	5.281	Gorilla	-0.106	Seal-it	-2.483	Lodge Logic	4.518
			(0.061)		(0.039)		(0.089)		(0.114)
		Krazy Glue	9.749	Krazy Glue	10.371	Shurtape	8.418	Farberware	-2.033
			(0.077)		(0.078)		(0.158)		(0.065)
		Super Glue	-1.273	Super Glue	-1.502	Others	2.817	Others	1.345
			(0.046)		(0.045)		(0.035)		(0.017)
		Others	-0.845	Others	-1.309				
			(0.025)		(0.024)				
# Observations			1,987,480		1,910,313		1,978,315		2,119,021
R^2			0.289		0.235		0.297		0.469
Log-Likelihood			-87,632.15		-110,506.70		-61,077.06		-98,580.75
LR Test			71,283.39 (df=9)		67,831.53 (df=9)		51,610.82 (df=8)		174,144.60 (df=8)

Note: The demand estimation uses ± 8 weeks of Nielsen HMS data around the FTC closing letter date of each affected brand.

B Non-parametric Estimation of Valuation Distribution

The transaction prices, used in the regression anlaysis, are the second-highest order statistics of the underlying consumer valuation. Following Athey and Haile (2006), we recover the valuation distribution from transaction price distribution in a non-parametric manner.

In a symmetric IPV second price ascending auction model, the valuation distribution $F(v)$ is identified from the transaction-price distribution $G(b;n)$, because,

$$\begin{aligned} G(b;n) &= F^{(n-1):n}(v) \\ &= nF^{n-1}(v) - (n-1)F^n(v) \end{aligned} \tag{5}$$

where b is the transaction price among auctions with n bidders.

Inverting Equation 5 gives us the valuation distribution $F(v)$. The non-parametric estimate of $F(v)$, can then be given by $\hat{F}(v)$, through the following equation,

$$\hat{F}(v) = \phi^{-1}(\hat{G}(b;n);n)$$

where $\hat{G}(b;n)$ is the empirical CDF of transaction prices estimated from observed transaction prices, and $\phi^{-1}(x)$ is the inverse function of $\phi(x;n) = nx^{n-1} - (n-1)x^n$.

We estimate $\hat{G}(b;n)$ with a kernel estimator, where Gaussian kernels are used in both n and b, and the bandwidth is selected following Hall, Racine and Li (2004). We use the number of page views for a given auction to proxy for the number of bidders n. Page views are defined as the total number of unique visitors per day: multiple visits in a day are counted only once.

While not a perfect proxy for the number of bidders, (a potential bidder can search but not click on the link underestimating our estimate, or she can repeatedly visit the listing on various days of an auction overestimating the number of bidders), it is the best proxy we can get from the information provided by eBay. [10]

Figure 8 shows the $\hat{F}(v)$ inverted from $\hat{G}(b;n)$ of control and treatment groups, respectively. Figure 9 shows a smoothed cubic spline of $\hat{F}(v)$ in control and treatment condition, as well as an estimated empircal CDF of $\hat{G}(b|n=10)$. The CDF of the valuation distri-

[10] Note that the above identification assumes researchers observe n, the number of potential bidders. Song (2004) proposes an alternative identification when the number of potential bidders is unobserved, but the second- and third-highest bids are observed. We choose to assume knowledge of the number of potential bidders, because 68% of the auctions have only two or fewer bidders making the identification proposed by Song (2004) infeasible.

bution of treatment auctions stochastically dominates that of control auctions, implying on average, consumers value Made-in-USA product more than the identical product without Made-in-USA claims.

Figure 8: Inverted $\hat{F}(v)$ from $\hat{G}(b;n)$

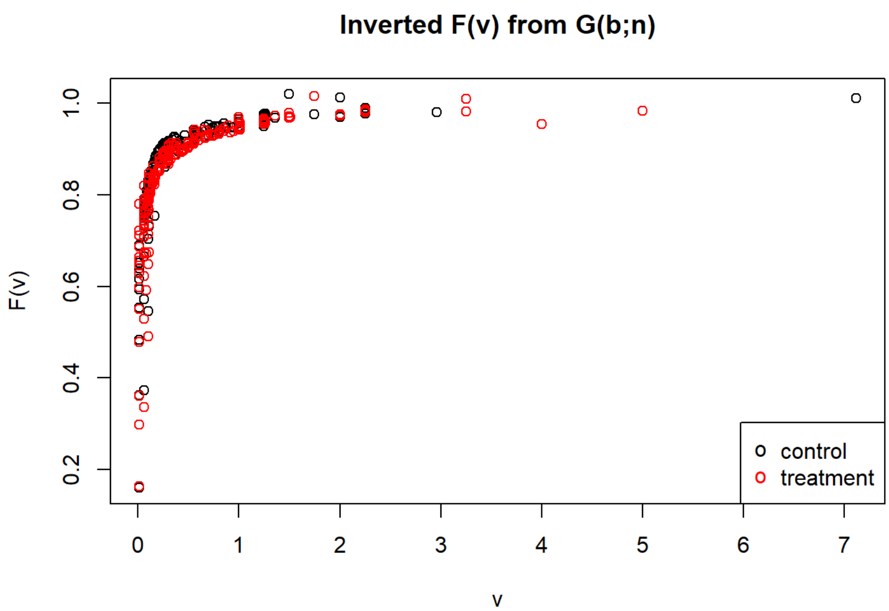


Figure 9: Spline Smoothed $\hat{F}(v)$ and Empirical CDF $\hat{G}(b;n=10)$