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Tax Salience and Attention Variation: Evidence from a Natural Experiment

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Tax Salience and Attention Variation: Evidence from a Natural Experiment*

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Abstract

We examine the salient effects of posting tax-inclusive price tags on demand. We discover attention variation among consumers with only less-educated consumers underreacting to the not-fully-salient tax display. We also find that a change in price tag displays to tax-exclusive prices induces consumers to visit such stores more often than before. More than half of the increase in demand in the stores using the tax-exclusive price tags is accounted for by changing regular stores, which implies that the less salient tax price display influences store patronage. However, our results suggest that the salient effect is a short-lived, not permanent, phenomenon.

JEL Classification: D12; D25; H71

Keywords: consumption tax; rational inattention; store choice; store patronage; tax salience; value-added tax

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1 Introduction

The inattentiveness of consumers to some types of incentives has attracted an increasing number of studies. Although full optimization of incentives by agents is a standard assumption in economics, past studies reject this fully optimizing behavior. Inattention and imperfect optimization are some of the important issues, particularly in the tax system. A seminal paper by Chetty et al. (2009) examines the impact of price display on the demand for healthcare goods and alcohol using retail sales data and illustrates that posting tax-inclusive price tags reduces demand by 8%, which is almost the same rate as the tax rate of 7.375%. Past studies provide supporting evidence of consumers being inattentive to not-fully salient tax (Allcott et al., 2014, 2015; Einav et al., 2014; Feldman and Bradley, 2015; Finkelstein, 2009; Gallagher and Muehlegger, 2011).

However, the evidence of attention variation across consumers is scant. The literature implicitly assumes that the inattentiveness of consumers is homogeneous: each consumer exhibits the same degree of underreaction. Examining attention variation requires data at the individual level, which is not usually available. Given data limitations, past empirical studies provide evidence of how consumers underreact on average by assuming a representative agent committing the "average mistake." However, the reality may differ from the assumption of homogeneity. Complex incentive schemes may entail heterogeneous reactions across consumers.Taubinsky and Rees-Jones (2018) is the only study that provides evidence of attention variation and endorses the intuition that attention variation does exist.[1] They show significant individual differences in the underreaction, revealing a significant positive association between the average mistake and financial literacy, income, and numeracy, and a significant negative association between the average mistake and age.[2]

Furthermore, evidence on whether salience effects are long-lasting is mixed. Salience effects may be expected to disappear in the long run if consumers become more attentive than before or learn that they misoptimize. By contrast, salience effects could be persistent if less salient incentives offer customers the impression of lower-priced goods and result in changing regular stores. The issue is important because the salience of a tax system can influence equilibrium tax rates if the effects persist in the long run.[3] Past studies relying on field

[1]Goldin and Tariana (2013) show income differences in the attentiveness to cigarette taxes, using the survey data of consumer spending on cigarettes. They find that only low income consumers respond to taxes levied at the register, while high income consumers underreact to taxes that are not salient. Although Goldin and Tariana (2013) find the attention gap and long-lasting effects of a tax salience on cigarette demand, their finding reflects the reduction in smoking habit formation of low income consumers rather than salience effects.

[2]Taubinsky and Rees-Jones (2018) find no relationship between the average mistake and sex, marital status, education, and race.

[3]Goldin (2015), Farhi and Gabaix (2020), and Moore and Slemrod (2021) develop a theory of optimal taxation when tax rates are not-fully-salient. Finkelstein (2009) discusses whether the salience of a tax system affects equilibrium tax rates.

experiments (Chetty et al., 2009; Allcott et al., 2015) or laboratory experiments (Taubinsky and Rees-Jones, 2018) cannot identify long-term effects unless researchers conduct follow-up experiments.[4] The literature relying on natural experimental settings provides mixed evidence (Finkelstein, 2009; Gathergood, 2019; Bollinger et al., 2011).Finkelstein (2009) shows that the salience effect of a tax system on demand is long-lasting, while Gathergood (2019) demonstrates that the salient placement of balances on credit cards induces consumers to misallocate repayments and misallocation is highly persistent over time. Bollinger et al. (2011) also illustrate the long-lasting effects of the salience effect by evaluating the effect of calorie labeling on food orders in chain restaurants and revealing that conservation habits are formed and the calorie reduction persists at least for 10 months after the commencement of calorie posting. However, counter-evidence also exists. Kiszko et al. (2014) provide a comprehensive survey on the influence of calorie labeling on food orders and consumption and document that calorie labels do not have the desired effect in reducing total calories. Further evidence is needed on whether salience effects are long-lasting or only short-lived.

In this study, we examine the attention variation and long-term effects of inattentiveness to less salient tax rates. First, we comprehensively measure the impacts of posing tax-inclusive price tags on the demand for a wide variety of commodities, exploiting not only the POS data of retailers on daily commodities but also a nationwide survey on the tax display of retailers. A newly enacted law by the Japanese government allows us to re-examine consumers' inattentiveness to the not-fully-salient tax price display using a natural experiment approach. In October 2013, the Japanese government promulgated the Act Concerning Special Measures for Correcting Practices Impeding Consumption Tax Pass-on (消費税転嫁対策特別措置法) to transfer the consumption tax smoothly and properly. In Japan, a consumption tax was introduced in April 1989. The tax increased by 2% in April 1997 and was gradually raised to 8% in April 2018 and 10% in October 2019.[5] After the enactment of the law, the tax-included price was not required to be presented on goods or services until after a year and a half from the date of the consumption tax hike to 10%. Retailers had to prominently display the (tax added) gross price before October 2013, while they could choose to display the tax-exclusive price tag after that. To evaluate the impact of which price tags are displayed on goods in demand, we conduct a nationwide survey on the tax display of retailers. We ask respondents to answer when and whether they changed the price display after the enactment of the law to

[4]Kiszko et al. (2014) provide a comprehensive survey on the effects of calorie posting on public health and point out that most studies are conducted over a short period and do not capture any potential long-term impacts of menu labeling. Wang (2015) finds that static analyses overestimate the long-run price elasticity as she examines the impact of soda taxes on consumer welfare.

[5]In the Japanese consumption tax system, expenses such as medical treatment fees, housing rent, and tuition fees for schooling are not taxed. In October 2019, a reduced tax rate was introduced. While the standard tax rate is 10%, a levy of 8% is applied to food and newspapers. See more details about the Japanese consumption tax system at the National Tax Agency's site: https://www.nta.go.jp/english/taxes/consumption_tax/index.htm.

identify the timing of the change in price display. We combine the survey data with the sales data of the respondents to examine the impact of a change in the price tag. The matched data allow us to measure the effect of tax salience on the quantity sold.

Second, we examine the variation in underreaction by uncovering who underreacts to taxes that are not salient. We combine the survey data on the tax display of retailers with consumers' expenditure data to further identify who underreacts to taxes that are not salient. The linked data capture when, where, and who purchases what goods and in what quantities, which allows us to examine attention variation by identifying who spent less at stores that use a *tax-inclusive* price display.

Third, we examine whether salience effects are long-lasting. The unique datasets that we use open a new avenue to investigate consumers' store-switching behavior from stores using the tax-inclusive tags to those using the tax-exclusive tags. In addition to analyzing quantity demanded, we focus on the usage history of retail stores that consumers regularly visit. The consumer panel data allow counting how frequently consumers visit which stores and identifying consumers that visit the stores using tax-exclusive price display more often than before. When retail stores change the display from tax-inclusive to tax-exclusive price, misreaction to the tax display may induce consumers to *marginally* change regular stores. In this case, it may increase the sales volume in the stores using the tax-exclusive tax display. Thus, the matched data can provide deeper insights into the relationship between the tax salience and the store-switching behavior of consumers.

We derive the following four results. First, we find that the demand for goods sold at stores with a tax-exclusive price display is 3% higher than that at stores with a tax-inclusive price display. This suggests that consumers underreact to less salient taxes. Second, we find that less-educated consumers underreact to non-salient taxes. High school graduates spend 6% more at stores that use a *tax-exclusive* price display than college graduates. Third, we find the store-switching behavior of consumers with low educational attainment. Our estimation results imply a change in price tags to tax-exclusive displays induces consumers to misperceive that products are less expensive, thereby marginally changing regular stores. Fourth, we find evidence that salience effects are not persistent. The salient effects last at least three months before disappearing, suggesting that the salient effect is a short-lived, not permanent, phenomenon.

Our study is related to three strands of literature. First, it is related to examining whether consumer behavior is biased toward the salient and immediately visible. The literature shows the underreaction of consumers to imperfect information that is not salient. Chetty et al. (2009) and Feldman and Bradley (2015) illustrate the salient effects of tax-inclusive prices via experiments and find that the purchase volume of sales decreases significantly under tax-inclusive prices. Einav et al. (2014), Gallagher and Muehlegger (2011), Allcott et al. (2014), and Allcott et al. (2015) also demonstrate the underreaction of consumers to less salient information. Our study contributes to this growing literature stream by exploring the salient effects of information on demand, exploiting a natural experiment based on a nationwide survey on retail sales and consumer expenditure. While results in prior literature are contingent on a field or laboratory experiment, our approach, based on a natural experiment, allows for examining not only variation in underreaction but also how long consumers underreact to imperfect information that is not salient. We provide evidence on not only tax salience at retail stores[6] but also find that the salient effect is not persistent but transitory; it lasts for three months but disappears thereafter, implying that the salient effect does not have a permanent influence on the steady state. As far as we know, this is the first study that documents that misreaction to the partial salient tax display is short-lived.

Second, our approach follows prior studies on the underlying mechanism of consumers' imperfect incentive optimization. The literature suggests that inattention and limited attention can explain the underreaction of economic agents.[7] Caplin and Dean (2015) demonstrate that all patterns of consumer behavioral bias are consistent with a general model of optimal costly information acquisition.[8] Clerides and Courty (2017) and Huang and Bronnenberg (2018) provide empirical evidence that rational inattention explains consumer behavior when acquiring information is costly.[9] Lacetera et al. (2012) and Foellmi et al. (2016) provide evidence of limited attention when economic agents address signals in decision-making. Using wholesale used-car transactions, Lacetera et al. (2012) find that consumers imperfectly process odometer values. Foellmi et al. (2016) document evidence of a limited inattention bias even among professional athletes. Our unique survey data on consumer spending allows us to examine underreaction variation to the less salient tax incentive. Our finding that only consumers with low educational attainment are inattentive to less salient tax displays sheds light on the possibility that the educational gap, which leads to attention variation, is the underlying reason for imperfect optimization.

Third, this study is related to the growing literature examining salience and choice of agents. The literature shows that salient attributes of goods, such as quality and price, influ-

[6]This salient effect is also found in food choices and stock trading decision-making. Bollinger et al. (2011) study the impact of calorie posting on consumers' purchase decisions at coffee shops and find that average calories per transaction fall by 6%. Frydman and Wang (2020) show that a salient shock to a stock's purchase price increases the disposition effect significantly.

[7]Gabaix (2019) provides a comprehensive survey of behavioral inattention. Tiefenbeck et al. (2016) show an effective method to overcome this bias because of inattention and imperfection. Byrne et al. (2011) document how imperfect information generates heterogeneous effects in information treatments with feedback.

[8]Given the evidence that consumers do not consider all feasible alternatives, Masatlioglu et al. (2012) provide a choice theoretical foundation under the limited attention.

[9]Clerides and Courty (2017) show that quantity surcharges, where the same physical product is sold in two packs of different sizes and the large size has a higher price per unit than the small one, has only a slight impact on the sales volume because of rational inattention. Huang and Bronnenberg (2018) also illustrate the rational inattention of consumers in shopping, providing evidence that consumers pay fixed purchasing costs for a wide variety of product choices.

ence consumer choice. Bordalo et al. (2013) present a theory of context-dependent choice in which a consumer's attention is drawn to salient attributes of goods. D'Acunto et al. (2021) provide empirical evidence that consumers rely on salient price changes when forming expectations about aggregate inflation. Dessaint and Matray (2017) illustrate that salient risks influence managers' choices. While the marketing perspective literature investigates the effects of multi-channel retail mix (Melis et al., 2009), assortment (Briesch et al., 2009), and category positioning (Briesch et al., 2013) on store choice, evidence of how the display of incentives influences consumer retail choice is scant. [10]We identify whether consumers change regular stores they frequently use in response to the change in the tax price display and find a store-switching behavior: the change in the tax price display to the less salient tax price may induce less-educated consumers to feel that products sold are less expensive and visit stores slightly more frequently. This study bridges the gap in the literature by providing new evidence of a change in store choices of consumers when the salience of tax price displays is heterogeneous across stores.

The remainder of this paper proceeds as follows. In Section 2, we present the research design to comprehensively evaluate the impacts of tax salience on demand. In Section 3, we identify the types of consumers who underreact to taxes that are not less salient. In Section 4, we examine attention variation across consumers while in Section 5, we investigate whether misoptimization induces consumers to change regular stores. Section 6 concludes.

2 Research design

Do consumers underreact to non-salient taxes? To answer the question, we first estimate the difference in quantity demanded between control and treatment stores when the treatment store changes the tax display from tax-inclusive tags to tax-exclusive tags. The following subsection explains how we identify which retailers use the tax-inclusive/exclusive display.

2.1 Identification of changes in the price display at retailers

To identify which retailers change the tax display from tax-inclusive tags to tax-exclusive tags and the timing of this change, we conducted a questionnaire survey of retailers in April 2019. The survey respondents were chain stores of supermarkets, drug stores, and home centers. We approached 282 firms through telephone, e-mail, or fax to seek information non (1) the price display that they use and (2) whether they changed the price displays to tax-exclusive tags after the promulgation of the Act Concerning Special Measures for Correcting Practices

[10]Pan and Zinkhan (2006) provide a comprehensive survey on the determinants of retail patronage from a meta-analytical perspective.

Impeding Consumption Tax Pass-on.[11] We obtained responses from 207 firms comprising 117 supermarkets, 30 drugstores, and 60 home centers.[12] Thus, the response rate of the survey was approximately 72%.

We identified which stores use what type of price display by directly asking respondents to choose the most similar display from nine choices, as illustrated in Figure 2. The top panel in Figure 2 shows typical patterns of tax-inclusive tags. As taxes are obvious enough to be noticed on the tax-inclusive display, consumers could easily observe how much taxes they should pay at the register.[13] We classify the stores that chose the option from the top panel in Figure 2 as the control group. By contrast, the lower panel in Figure 2 shows depicts patterns of tax-exclusive tags. Although the tax-exclusive display contains information about taxes, the levied taxes do not seem to be completely visible, unlike in the case of the tax-inclusive display. We classify the stores that selected the option from the lower panel in Figure 2 as the treatment group.

Figure 3 illustrates the proportion of firms that use a tax-exclusive price tag. The figure shows that the proportion jumps twice: in October 2013 and April 2014. The Act was promulgated on October 1, 2013, and the consumption tax rate changed from 5% to 8% on April 1, 2014. Our survey indicates that approximately 15% and three-fourths of respondents changed the tax display to a tax-exclusive price tag in October 2013 and April 2014, respectively.

2.2 Data on retail sales

To estimate the impact of a change in posted prices, we combine the survey data with the POS data on each respondent's retail sales. The data originate from SRI and were collated by Intage Inc. SRI is a nationwide retail store panel survey that collects data from approximately 4,000 stores across Japan, including supermarkets, home centers/discount stores, and drug stores. Survey items include the date of sales, volume of sales, and volume of food sales excluding fresh food, beverages, and alcohol at each store using barcode identification. We combine the SRI data on the monthly volume of sales and the unit selling price with data from the survey of the price display. Table 1 presents the basic statistics of SRI for four years from January 2012 to December 2015. The sample covers more than 400 stores among survey respondents. The table indicates that treatment stores record considerably larger sales than the control stores. —sales value (Japanese Yen) and volume in treatment stores are twice those in control stores. Figure 4 shows the logarithm of sales volume and average price (Japanese

[11] A total of 282 firms was surveyed because of the availability of the POS data on their retail sales from a marketing research company, Intage Inc.

[12] According to the 2014 annual report by the National Supermarket Association of Japan, 540 firms are affiliated with industry associations.

[13] We conducted a questionnaire survey of retailers in April 2019. At that time, the consumption tax rate was 8%. Consumers can easily perceive that they should pay ¥8 for the consumption tax when they buy ¥100-priced goods.

Yen) in treatment and control stores in the top and bottom panels, respectively. The figure suggests that the average sales volume and price levels differ between treatment and control stores, although the trends seem to be parallel. To mitigate possible selection bias between treatment and control groups, we control the size effects by matching the propensity scores or by a "triple difference" approach in the subsequent analysis.

3 Do consumers underreact to less salient tax displays?

Do posted price tags influence the quantity demanded? We examine whether the quantity demanded differs between the control and treatment groups. Treatment groups are stores that change the price display from tax-inclusive to tax-exclusive price tags at the end of September 2013, while control groups are stores that do not change the price display until the consumption rate hike in April 2014.[14]

We calculate the effect of posting tax-exclusive prices on demand using a difference-in-difference-in-differences (DDD) approach as in Gruber (1994) and Chetty et al. (2009). First, we detrend the data by calculating the year-on-year growth rates of the quantity sold in each store i to satisfy the parallel trend assumption between the control and treatment groups. Second, we construct a difference-in-difference (DD) estimate by using the difference in the growth rates between the period before and after October 1, 2013. Third, we further take the difference in the DD estimate between the control and treatment groups. Here, we assume that the triple difference allows us to differentiate time trends and control the effect of covariates. The sample period covers six months from July 2013 to December 2013. The baseline and treatment period is set to three months from July to September 2013 (2013:Q3) and three months from October to December 2013 (2013:Q4).[15] The first column in Table 2 summarizes the estimation result. The difference in quantity demanded between the control and treatment groups is estimated to be -2.4%, which is significantly positive and different from zero. The results suggest that a percentage increase in posted prices reduces products sold by 0.54%.[16]

Our estimation results are robust when we employ a simple DD approach or a propensity score analysis. The second column in Table 2 summarizes the results from the double difference approach. The difference between treatment and control groups is -3.6% and differs significantly from zero, similar to that of the DDD estimate. This is the case when we

[14]Note that the Japanese government promulgated the Act Concerning Special Measures for Correcting Practices Impeding Consumption Tax Pass-on in October 2013. Retailers had to prominently display the (tax added) gross price before October 2013, whereas they could choose to display the tax-exclusive price tag thereafter.

[15]We split the sample by the end of 2013:Q3 rather than the end of 2014:Q1 because the subsample with a tax rise fails to identify the degree of underreaction; the tax rate should be constant, as shown in Chetty et al. (2009).

[16]Given that the consumption tax rate was 5% until the fiscal year 2013, the marginal effect of the tax-exclusive price tags on sales is computed as approximately $-0.5\%(= -2.43\%/5\%)$.

conduct the propensity score analysis, which allows us to control the confounding bias on quantity demanded. Specifically, we calculate the difference in the growth rates of the quantity sold between the control and treatment groups and match two covariates that measure the size of sales and the place of the stores.[17] Table 3 summarizes the result from the propensity score analysis. Figure 1 depicts the difference in means and covariate distributions because of matching. Table 1 shows that the sales volume of treatment stores is more than double that of control stores, although the figures suggest that possible bias by confounding variables such as the size of sales are mitigated. The top panel in Figure 1 illustrates that the difference in means between treatment and control groups is almost zero after matching. The bottom panel shows that matching improves the balance for the gross sales amount. The propensity score analysis supports the view that the quantity sold significantly differs between the control and treatment stores; the treatment group's sales were larger than those of the control groups even when the size of sales is controlled.

In sum, a less salient tax price display impacts the quantity demanded—the quantity sold in treatment stores is approximately 3% more than that in control stores. As far as we know, this is the first evidence of underreaction to less salient tax displays from nationwide POS data of retail stores.

4 Attention variation

In the previous section, we indicated the underreaction of consumers to non-salient taxes. However, another question that arises is the variation in the underreaction to not-fully-salient tax price display across consumers. Studies such as Goldin and Tariana (2013), Stango and Zinman (2014), and Taubinsky and Rees-Jones (2018) report variation in underreaction to non-salient taxes. Goldin and Tariana (2013) find evidence that income differences affect attentiveness to cigarette taxes, while Stango and Zinman (2014) find the significant effects of the salience of bank overdraft fees for consumers with lower education. That is, socioeconomic factors, such as income levels and educational attainment, can predict who underreacts to the not-fully-salient incentives.

To uncover the variation in underreaction to less salient displays in daily shopping for purchasing various products, we utilize the survey on consumers' expenditure. We employ panel data (SCI) on the consumption expenditure, collated by a marketing company, Intage. We use the data records of day-to-day shopping information collected on an ongoing basis from more than 50,000 consumers aged 15–79 years all over Japan. The data capture the profile of these consumers in detail, including aspects such as income, education, and financial

[17] We further control covariates of the logarithm of sales and the location dummy of retail stores, which takes a value of one if the store is in Tokyo, Osaka, or Fukuoka prefecture.

assets. We can observe who bought what, when, where, at what price, and in what amount. These data cover items that households purchase frequently, such as food (except for fresh food, prepared food, and lunch boxes), beverages, daily miscellaneous goods, cosmetics, pharmaceutical products, and cigarettes.[18] We combine the price display survey with the consumption expenditure and uncover the variation in underreaction to the less salient display.

The data are novel because they provide new possibilities to investigate the variation in the salient effects among consumers. First, we can match the consumer panel data with a nationwide survey of retailers on the tax display. Given that the matched data allow us to identify which stores consumers use to purchase daily commodities, it contributes to examining whether the salient effects occur without a field or laboratory experiment. Second, the consumer panel data on expenditure include covariates of the survey respondents, such as sex, age, income level, and educational attainment. This allows identifying who purchases more at stores using the tax-exclusive price display before switching to the tax price display. Thus, we can examine whether socioeconomic factors determine the underreaction to non-salient taxes. Third, the data open a new avenue for investigating the store-switching behavior of consumers from stores using tax-inclusive tags to those using tax-exclusive tags. The consumer panel data allow counting how frequently consumers visit which stores. This implies that we can identify which consumers visit the stores using tax-exclusive price displays more often than before. When retail stores change the price display from tax-inclusive to tax-exclusive, misreaction to the tax display may induce consumers to *marginally* change their regular stores. In this case, misreaction to the tax display may increase the sales volume in the stores using the tax-exclusive tax display. Thus, the matched data can provide a deeper insight into the store-switching behavior of consumers, as shown in Section 5.[19]

In this section, we first examine who underreacts more to non-salient taxes to uncover the underlying mechanism for the sales difference between the stores using the tax-inclusive and tax-exclusive price display. Exploiting the matched data with consumers' expenditure and a survey of retailers for the tax display, we adopt the two types of DID approach: simple and dynamic DID. Here, treatment and control stores are denoted as the stores that use a tax-exclusive and tax-inclusive price display, respectively. For every consumer, the assignment of control and treatment stores is random; each consumer does not predict which retailers change the price display after April 1, 2014. Random assignment of control and treatment stores

[18] As our scanner data cover daily necessities, they do not cover housing, utilities, durables, clothing, and services. That is a caveat in the data on the consumption expenditure. The coverage of the data relative to Japanese households' consumption is not large. While Diamond et al. (2020) use the same survey, they report that the items included in the data cover approximately 30% of the weight of the Japanese Consumer Price Index.

[19] We match the consumer panel data with a survey of retailers for the tax display. We successfully match 11,244 consumers with a survey identifying the patterns of price display. Table 4 presents the basic statistics of the average purchase amount in treatment and control stores. The sample is excluded when control and treatment stores have no purchase record in one month.

allows us to adopt the simple DID method for examining who underreacts to the not-fully-salient tax display. When consumers are fully attentive to posted prices, they do not change their consumption bundle between retailers using a tax-inclusive or tax-exclusive display even after the consumption tax hike. However, when they are inattentive to less salient taxes, the purchase volume at stores that use a tax-exclusive display becomes larger than that at stores that use a tax-inclusive display after the consumption tax hike.

We compare the purchase volume of consumers at treatment stores with that of control stores before and after the consumption tax hike in April 2014. We use the sample before and after April 2014 rather than before and after October 2013 because more than 70% of retailers changed their tax price display from tax-inclusive to tax-exclusive at the timing of the tax hike on April 1, 2014. Less than 15% retailers implemented the same in October 2013. The matched sample before and after October 2013 contains only a small quantity of purchases—the monthly average quantity purchased is less than ten. As more samples are matched with a survey of retailers for the tax display, the subsequent analysis uses the sample period before and after April 2014.

Table 5 illustrates the result from a simple DID approach. The difference in the quantity purchased by consumers between the treatment and baseline periods are 2.2% and -0.4%, respectively. The difference between them is a DID estimator. It is 2.6%, insignificant but similar to that from the retailer's data in Section 2.

To further identify which consumers underreact to the tax-exclusive price display, we split the sample by income level and educational attainment. Table 6 summarizes the results using the subsamples and reports the estimator of the DID approach; that is, the difference in the quantity purchased between the treatment and control stores. The first column in Table 6 shows that the difference is approximately 2.6%. The estimation result is the same as that in Table 5, using the full sample. The second and third columns illustrate the results using the subsample by income level, while the fourth and fifth columns show the results using the subsample by educational attainment. The fourth column suggests that the difference in the quantity purchased between control and treatment groups is significant, using the subsample from low-educated consumers. The DID estimator is 6.0%. The quantity purchased by consumers with low education (relatively) in treatment stores increases 6.0% more than before the tax display changes. The table suggests that only low-educated consumers misoptimize and shows that the quantity purchased by consumers with low income and high education increased by 2.4%, although it is insignificant.[20] It suggests that educational attainment, not income level determines underreaction.

To check the robustness, we use a dynamic DID method. We estimate the following

[20]Even so, the table shows that the quantity purchased by consumers with high income and low education increased by 31.4%; nonetheless, it is insignificant.

equation:

$$\ln\left(q_{i,j,t}\right) = \alpha D^{Treatment} + \sum_{m} \beta_m D^{Month_m} + \sum_{m} \gamma_m D^{Treatment} \times D^{Month_m} + \mathbf{X}\delta + \varepsilon_{i,j,t}, \tag{1}$$

where $q_{i,j,t}$ denotes the purchase volume of consumer i at store j in month t. $D^{Treatment}$ is a dummy variable, taking the value of one when store j uses tax-exclusive price display and zero otherwise. D^{Month_m} is a calendar dummy. $\mathbf{X}$ is a vector of control variables of socioeconomic factors, such as age, gender, income levels, and educational attainment. This estimation strategy allows for checking whether the "trend" of expenditure is parallel among the control and treatment stores and whether the salient effects are persistent. Our interest is in the coefficient γ of the cross term between D^{Month} and $D^{Treatment}$. A positive γ suggests that consumers purchase more products in the tax-exclusive price display stores than in the tax-inclusive price display stores.

Figure 5 shows the development of the coefficient γ using the sample of consumers with low education.[21] First, the coefficient γ is almost zero in the first three months, which guarantees that the trends of the quantity purchased in the control and treatment stores are parallel. This evidence satisfies the assumption for the appropriate DID approach. Second, the coefficient γ becomes significantly positive after April 2014. The impact from April 2014 to June 2014 for three months and April 2014 to September 2014 for six months is 10.7% and 7.2% on average, respectively.[22] It suggests that the salient effects are larger specifically for consumers with low educational attainment. Third, the coefficient becomes negative in July 2014 and is insignificant thereafter. For three months from July 2014 to September 2014, the impact decreases to 3.6% and is insignificant. The decrease in γ suggests that the salient effects are a transitory, not permanent, phenomenon.

In summary, we find a variation in underreaction to the not-fully-salient tax price display. The salient effect is significant only for consumers with low education. The effect is large—the quantity purchased by less-educated consumers is 7.2% more on average for six months of the treatment period. However, the effect is not persistent, suggesting that the salient effect is a short-lived, not permanent, phenomenon.

[21] Table 7 illustrates the estimates of γs. The first and second columns present the results from estimating Equations (1) and (2).

[22] Table 7 indicates that the null hypothesis that the impact over the three months is zero is rejected at the 5% significance level.

5 Store-switching behavior of consumers

We found that relatively less-educated consumers purchase approximately 10% more every month in treatment stores than in control stores. However, a question arises—is the difference in quantity purchased all accounted for by the consumer's inattention to the not-fully-salient tax display? The difference in purchase amounts by less-educated consumers among control and treatment stores could be because of not only their inattention at the moment of purchase but also their store-switching behavior. The store-switching behavior mechanism is as follows. The change in the price display may induce consumers to feel that products sold in the treatment stores are less expensive than in the control stores. In this case, consumers may marginally change regular stores when the stores switch the price display from tax-inclusive to tax-exclusive. Visiting the treatment stores more often than before increases the quantity purchased by consumers in the stores using the tax-exclusive tax display. Thus, the difference in the tax display among stores influences not only the inattention to the not-fully-salient tax display *when* shopping but also the decision of which store they use *before* shopping. If so, a consumer's choice about which store they use changes after the retailers switch the tax display, contributing to a relative increase in the quantity purchased in the treatment stores.

To examine whether consumers change which stores they use after the tax display switching, we first conduct a simple DID. We compare the frequency of visiting treatment and control stores before and after the consumption tax hike in April 2014. If consumers feel that products sold in the treatment stores are less expensive than in the control stores, consumers may marginally change regular stores. Table 8 illustrates the result from a simple DID approach. The growth rates of the frequency of visiting stores are 0.1% and -0.6% in the treated and control groups, respectively. This implies that the difference between them is almost zero, that is, the store-switching behavior of consumers using the full sample is not evidenced.

Next, we split the sample by income level and educational attainment. Table 9 summarizes the results using the subsamples. The table reports the estimator of the DID approach, that is, the difference in the frequency of visiting treatment and control stores. The first column shows the difference is approximately 0.6%. The estimation result is the same as that in Table 8 using the entire sample. The second and third columns indicate the results using the subsample by income level, while the fourth and fifth columns show the results using the subsample by educational attainment. The fourth column, which uses the subsample from low-educated consumers, suggests that the difference in the quantity purchased between the control and treatment groups is 3.3%. The p-value is 10.2%; the null hypothesis that the difference is zero is almost rejected at the 10% significance level. When using the subsample from low-educated consumers, the DID estimator is 3.3%; the frequency of visiting treatment stores by consumers with low education (relatively) is 3.3% more compared with that before the tax display changes. Remember that in the previous section, we demonstrated that only less-educated consumers increase the quantity purchased in treatment stores—by 6.0% more than that before the tax display changes. The simple DID analysis of the frequency of visiting stores suggests that more frequent visits to treatment stores by less-educated consumers account for half of the difference in the quantity purchased in treatment stores.[23]

To check the robustness, we estimate using a dynamic DID method as follows:

$$\ln\left(visit_{i,j,t}\right) = \alpha D^{Treatment} + \sum_{m} \beta_m D^{Month_m} + \sum_{m} \gamma_m D^{Treatment} \times D^{Month_m} + \mathbf{X}\delta + \varepsilon_{i,j,t}, \tag{2}$$

where $visit_{i,j,t}$ denotes the frequency of visiting store j by consumer i in month t. Our interest is in the coefficient γ of the cross term between D^{Month} and $D^{Treatment}$. A positive γ suggests that consumers visit more "tax-exclusive" stores than "tax-inclusive" stores.

Figure 6 illustrates the development of the coefficient γ using the sample from consumers with low education. First, the coefficient γ is insignificant in the first three months, which guarantees that the trends of the frequency of visiting the control and treatment stores are parallel. This evidence satisfies the assumption for the appropriate DID approach. Second, the coefficient γ becomes significantly positive after April 2014. The impact is approximately 8% every month on average from April 2014 to June 2014 for three months and half of that, approximately 4%, from April 2014 to September 2014 for six months. This suggests that the store-switching behavior is significant for consumers with low educational attainment. Third, the coefficient turns to almost zero in July 2014 and is insignificant thereafter. The decrease in γ suggests that the effect of the tax display on the decision of which store they use is short-lived.

The above estimation results allow us to disentangle the inattention of consumers with low education to the not-fully-salient tax display and their store-switching behavior. The store-switching behavior accounts for more than half of the (relative) increase in the quantity purchased by consumers with low education in the treatment stores. In the previous section, we estimated that the difference in the quantity purchased between the treatment and control stores using a simple DID (a dynamic DID) is 6.0% (5.0%) every month for six months after April 2014. By contrast, the effect of the tax display on store-switching behavior using a simple DID (a dynamic DID) is estimated to be 3.3% (4.0%) during the same period. These results suggest that the behavior of less-educated consumers matters when explaining why the sales volume in the stores using tax-exclusive price displays increases relatively more than in those using tax-inclusive price displays. The effects of store-switching can account for 55% to 80% of the increase in sales volume in the treatment stores. Our finding implies that the not-fully tax salient display influences consumers' decision-making about not only purchases

[23] The sixth column in Table 9 shows that consumers with high income and low education visit treatment stores 31.4% more frequently than control stores. While the sample size is not as large, the evidence that low-educated consumers visit treatment stores more often than before is robust.

at the moment when they add products to their shopping basket but also which stores they use before they travel for shopping. Although the store-switching behavior of less-educated consumers in the treatment stores is marginal and transitory, our estimation results imply that less-educated consumers misperceive that the products sold in the treatment store are less expensive.

6 Conclusion

In this study, we aimed to evaluate the impact of which price tags are displayed on goods in demand by conducting a nationwide survey on the tax display of retailers in Japan. We asked retailers to answer when and whether they changed the price display after the enactment of the law to identify the timing of a change in the price display. We combined the survey data with the sales data of the respondents to examine the impact of a change in the price tag. The unique data allowed us to measure the effect of tax salience not only on the demand for various products but also on a nationwide scale. To further examine the impact, we also combined the survey data on the tax display of retailers with the consumers' expenditure data, which captures when, where, and who purchases what goods and in what quantities. This novel data allowed us to identify which types of consumers underreact to non-salient taxes. Furthermore, the unique data sets open a new avenue to investigate consumers' store-switching behavior, from stores using tax-inclusive tags to those using tax-exclusive tags. The consumer panel data allowed counting how frequently consumers visit which stores and identifying which consumers visit the stores using tax-exclusive price display more often than before. When the retail stores change displays from tax-inclusive to tax-exclusive prices, misreaction to the tax display may induce consumers to *marginally* change regular stores. Thus, the matched data can provide deeper insights into the relationship between the tax salience and the store-switching behavior of consumers.

We can draw four major inferences from our study. First, we revealed that the demand for products sold at stores with a tax-exclusive price display is 3% higher than that at stores with a tax-inclusive price display. This may be because consumers underreact to less salient taxes. Second, we found evidence of a variation in underreaction: only consumers with low educational attainment underreacted to the less salient tax display. Third, we discovered that the less salient tax price display influenced store patronage. The estimation results suggest that consumers visit the store using the tax-exclusive price tags more frequently than before the change in the tags. The evidence that consumers change their regular stores in response to a change in the price tags implies that the salient effects influence consumers' decision-making about momentary purchases when adding products to the shopping basket as well as which stores to use before shopping. Fourth, we found evidence that salience effects are not persistent. The salient effects last at least for three months but disappear thereafter, which suggests that the salient effect is a short-lived, not permanent, phenomenon.

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Table 1: Summary statistics of the data on retail sales from January 2012 to December 2015

Control stores	Sales (Japanese Yen)	Average price (Japanese Yen)	Volume of sales
Mean	20,404,462	203.03	122,893
Median	9,229,173	176.20	48,580
Standard deviation	25,514,899	82.98	158,539
Observations	6,096	6,096	6,096
Treatment stores	**Sales (Japanese Yen)**	**Average price (Japanese Yen)**	**Volume of sales**
Mean	47,623,352	168.69	282,315
Median	39,072,381	166.52	238,444
Standard deviation	37,004,457	23.43	209,475
Observations	17,161	17,161	17,161

Notes: The data covers four years from January 2012 to December 2015. Treatment stores change the displays from tax-inclusive to tax-exclusive prices after October 2013, while control stores use the tax-inclusive price displays over the sample period. The number of treatment and control stores are 127 and 367, respectively.

Table 2: Effect of posting tax-inclusive prices on sales

Approach	Triple difference (DDD) approach	Double difference (DD) approach
Estimates	-0.0243***	-0.0355***
	(0.0018)	(0.0000)
Number of stores	531	621

Notes: The difference-in-difference-in-differences (DDD) estimate is calculated by using the triple difference: we (i) compute the year-on-year log difference of the quantity sold per store, (ii) construct a difference-in-differences (DD) estimate by taking the difference in the year-on-year growth rates between quantities sold before and after October 1, 2013, and (iii) take the difference in the DD estimate between the control and treatment groups. The DD estimate is calculated by taking the double difference: we (ii) take difference in quantities sold before and after October 1, 2013, and (ii) take the difference between the control and treatment groups. Standard errors are in parentheses. *, **, and *** indicate 10%, 5%, and 1% significance level, respectively. The sample spans six months from July 2013 to December 2013.

Table 3: Propensity score analysis from July 2013 to December 2013: changes in differences and distributions after matching the gross sales amount

	Log difference	Standard error	p-value
Difference-in-differences	0.027	0.012	0.029

Notes: We match two covariates: the logarithm of sales and the location dummy of retail stores, which takes a value of one if the store is in Tokyo, Osaka, or Fukuoka prefecture.

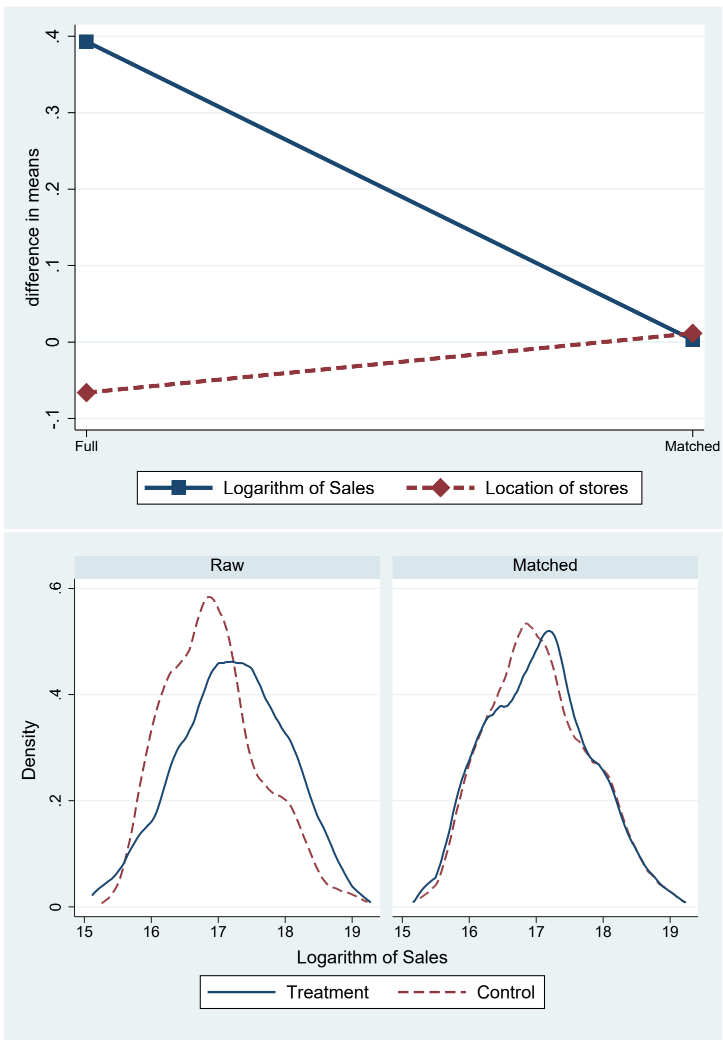

Figure 1: Differences in means and distributions after matching

Table 4: Summary statistics of the monthly data on consumer expenditure from January 2014 to September 2014

Unit			
All	All-Store	Treatment Store	Control-Store
Mean	26.920	14.530	12.390
Median	16.000	5.000	4.000
Standard deviation	31.438	24.519	22.591
Observations	78,185	78,185	78,185
High school grad.	All-Store	Treatment Store	Control-Store
Mean	29.553	16.908	13.245
Median	18.000	6.000	4.000
Standard deviation	32.891	25.578	24.261
Observations	21,621	21,621	21,621
College grad.	All-Store	Treatment Store	Control-Store
Mean	25.913	13.850	12.064
Median	15.000	4.000	4.000
Standard deviation	30.806	24.067	21.910
Observations	56,564	56,564	56,564

Visit			
All	All-Store	Treatment Store	Control-Store
Mean	4.449	2.555	1.893
Median	3.000	1.000	1.000
Standard deviation	4.314	3.609	2.998
Observations	78,185	78,185	78,185
High school grad.	All-Store	Treatment Store	Control-Store
Mean	4.592	2.676	1.916
Median	3.000	1.000	1.000
Standard deviation	4.418	3.667	3.084
Observations	21,621	21,621	21,621
College grad.	All-Store	Treatment Store	Control-Store
Mean	4.394	2.509	1.885
Median	3.000	1.000	1.000
Standard deviation	4.273	3.586	2.964
Observations	56,564	56,564	56,564

Notes: Each cell shows the (log) mean of monthly quantity purchased per respondent per month, for various subsets of the sample. Standard errors (clustered by month) are presented in parentheses and the number of observations in square brackets. Treatment period spans 6 months from April 2014 to September 2014, which covers the period immediately after the consumption tax hike on April 1, 2014.

Table 5: Who underreacts to the not-fully-salient display? A simple DID of the quantity purchased by consumers from January 2014 to September 2014 using the various subsamples

Period	Treated stores	Control stores	Difference
Baseline (2014:01–2014:03)	2.421	2.425	−0.004
	(1.234)	(1.165)	(0.013)
	[17,913]	[16,670]	[34,583]
Treatment (2014:04–2014:09)	2.372	2.350	0.022
	(1.233)	(1.149)	(0.009)
	[35,101]	[31,516]	[66,617]
Difference over time	−0.049	−0.075	0.026
	—	—	(0.016)
	—	—	[101,200]

Notes: Each cell shows the (log) mean of monthly quantity purchased per respondent per month, for various subsets of the sample. Standard errors (clustered by month) are presented in parentheses and the number of observations in square brackets. Treatment period spans 6 months from April 2014 to September 2014, which covers the period immediately after the consumption tax hike on April 1, 2014.

Table 6: Who underreacts to the not-fully-salient display? A simple DID of the quantity purchased by consumers from January 2014 to September 2014 using the various subsamples

Sample	All	Income level		Education attainment		High income and low education	Low income and high education
		Low	High	Low	High		
Difference over time	0.0256	0.00925	−0.00479	0.0602**	0.0125	0.314	0.0240
	(0.0159)	(0.0308)	(0.0713)	(0.0299)	(0.0187)	(0.198)	(0.0222)
	[101,200]	[26,443]	[5,185]	[28,163]	[73,037]	[736]	[51,523]

Notes: Each cell shows the(log) mean of monthly quantity purchased per respondent per month, for various subsets of the sample. Standard errors (clustered by month) are presented in parentheses and the number of observations in square brackets. Consumers with high and low income are defined as those who annually earn 7 million yen or above and less than 4 million yen, respectively. Consumers with high and low education are defined as those who graduated college and high school, respectively. The baseline and treatment periods span 3 months from January 2014 to March 2014 and 6 months from April 2014 to September 2014, respectively. The treatment period covers the time immediately after the consumption tax hike on April 1, 2014. Here, ** indicates 5% significance level.

Table 7: Response of quantity purchased and frequency of visiting to a change in the tax display using the subsample from consumers who graduated high school

		Quantity purchased	Frequency of visiting
γ_1:	January (Base month)	0.000	0.000
		(—)	(—)
γ_2:	February	0.033	0.029
		(0.059)	(0.041)
γ_3:	March	0.000	0.026
		(0.059)	(0.041)
γ_4:	April	0.117*	0.079*
		(0.060)	(0.042)
γ_5:	May	0.116*	0.068
		(0.060)	(0.042)
γ_6:	June	0.088	0.078*
		(0.060)	(0.042)
γ_7:	July	−0.022	0.004
		(0.060)	(0.042)
γ_8:	August	0.062	0.046
		(0.059)	(0.041)
γ_9:	September	0.069	0.025
		(0.059)	(0.042)
Observations		28,127	28,127
Wald test ($H_0 : \gamma_4 + \gamma_5 + \gamma_6 = 0$)		4.86**	4.93**

Notes: Each cell shows the coefficients of the cross term γs in Equation (2) The baseline and treatment periods span 3 months from January 2014 to March 2014 and 6 months from April 2014 to September 2014, respectively. Here, *, **, and *** indicate 10%, 5% and 1% significance level, respectively.

Table 8: Effect of posting tax-inclusive prices on the frequency of visiting the treatment and control stores from January 2014 to September 2014: a difference-in-difference approach

Period	Treated stores	Control stores	Difference
Baseline (2014:01–2014:03)	0.942	0.767	0.175
	(0.844)	(0.785)	(0.009)
	[17,913]	[16,670]	[34,583]
Treatment (2014:04–2014:09)	0.943	0.761	0.181
	(0.850)	(0.787)	(0.005)
	[35,101]	[31,516]	[66,617]
Difference over time	0.001	−0.006	0.006
	—	—	(0.011)
	—	—	[101,200]

Notes: Each cell shows the (log) mean of monthly quantity purchased per respondent per month, for various subsets of the sample. Standard errors (clustered by month) are presented in parentheses and the number of observations in square brackets. Treatment period spans 6 months from April 2014 to September 2014, which covers the period immediately after the consumption tax hike on April 1, 2014.

Table 9: Who underreacts to the not-fully-salient display? A simple DID using the frequency of visiting stores by consumers

Sample	All	Income level		Education attainment		High income and low education	Low income and high education
		Low	High	Low	High		
Difference over time	0.00638	0.00600	0.0100	0.0334†	−0.00374	0.217*	0.000
	(0.0109)	(0.0213)	(0.0475)	(0.0208)	(0.0127)	(0.129)	(0.0151)
	[101,200]	[26,443]	[5,185]	[28,163]	[73,037]	[736]	[51,523]

Notes: Each cell shows the (log) mean of monthly quantity purchased per respondent per month, for various subsets of the sample. Standard errors (clustered by month) are presented in parentheses and the number of observations in square brackets. Consumers with high and low income are defined as those who earn 7 million yen or above and less than 4 million yen, respectively. Consumers with high and low education are defined as those who graduated college and high school, respectively. The baseline and treatment periods span 3 months from January 2014 to March 2014 and 6 months from April 2014 to September 2014, respectively. The treatment period covers the time immediately after the consumption tax hike on April 1, 2014. Here, * indicates 10% significance level and † indicates that the p-value is 10.2%.

Figure 2: Tax display. The top panel from one to five shows salient taxes, while the lower panel from six to nine shows non-salient taxes.

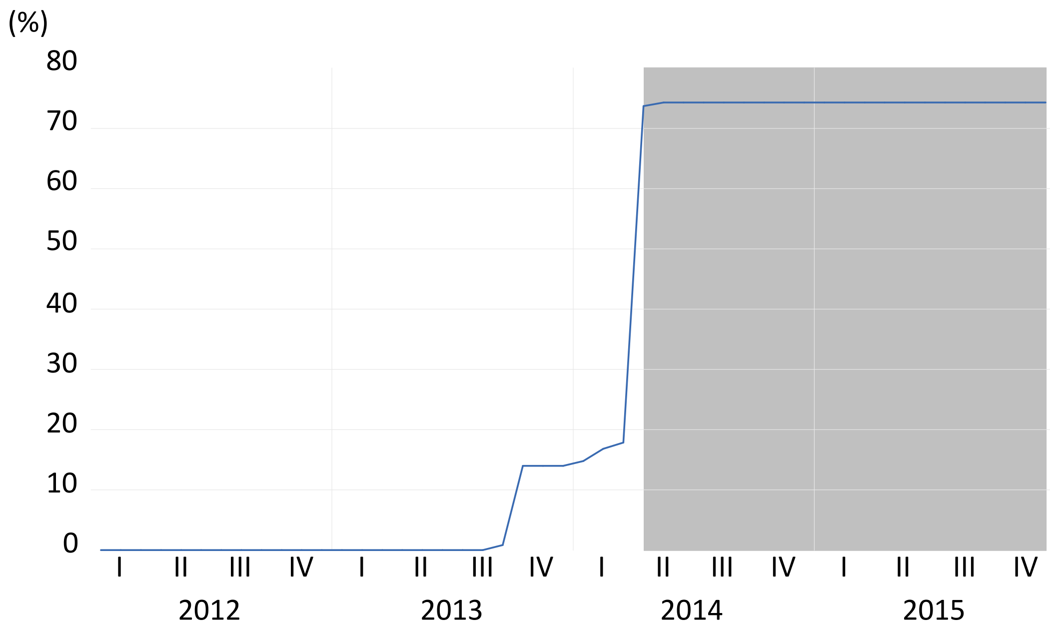

Figure 3: Proportion of the number of firms that use tax-exclusive price tags. The period after the consumption tax rate hike from 5% to 8% in April 2014 is shaded.

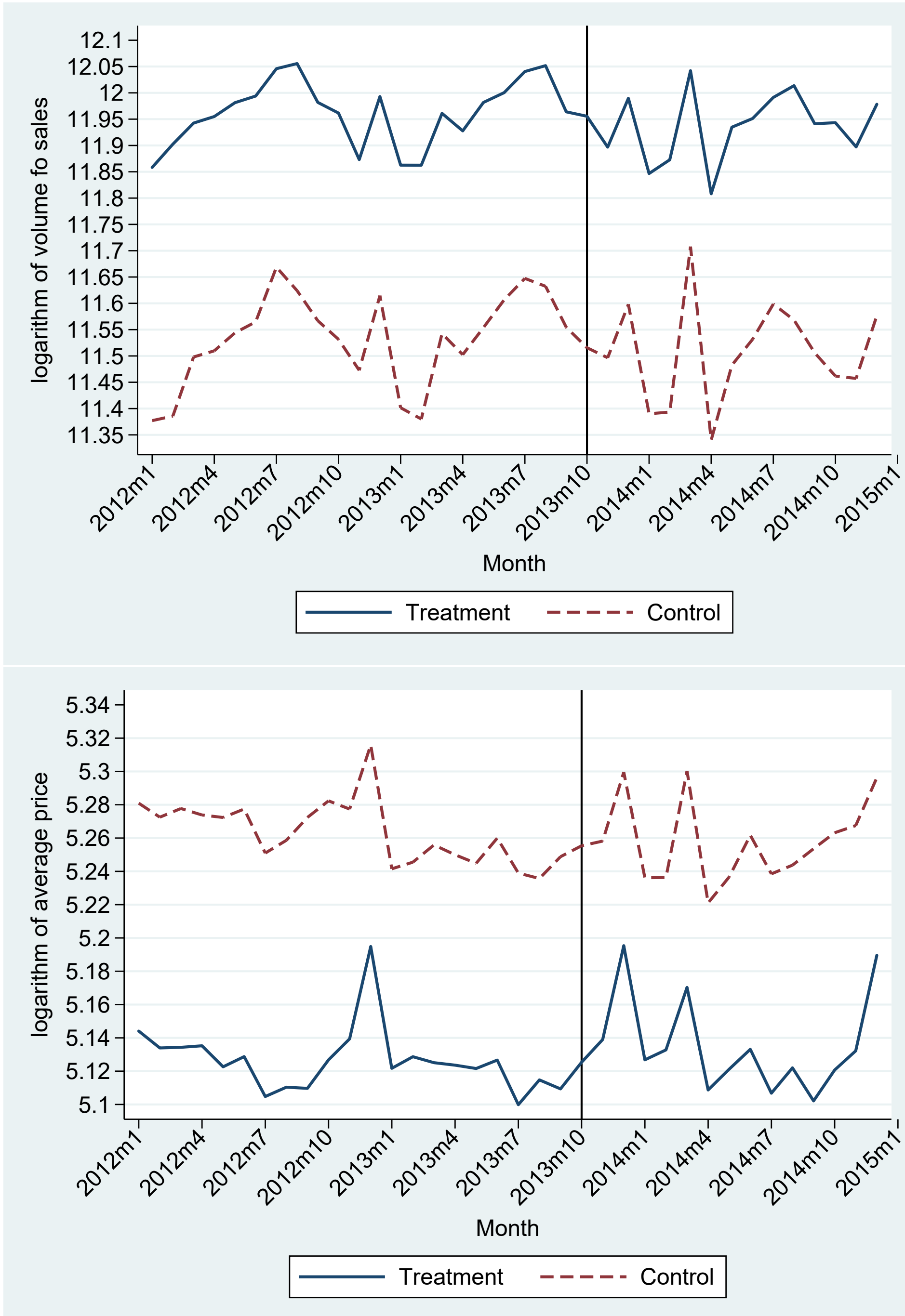

Figure 4: Development of the logarithm of sales volume (in top panel) and average price (in bottom panel). The vertical line is anchored on October 2013, when the Japanese government promulgated the Act Concerning Special Measures for Correcting Practices Impeding Consumption Tax Pass-on to transfer the consumption tax smoothly and properly. The law allows retailers to switch from a tax-inclusive to a tax-exclusive tag.

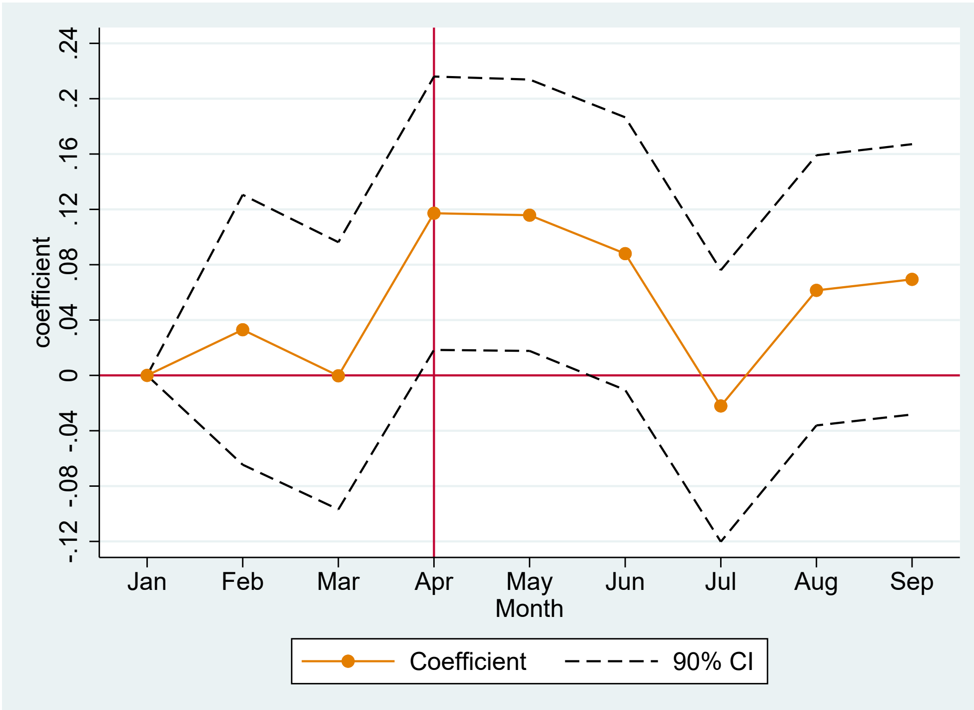

Figure 5: Development of the relative quantity purchased by those who graduated high school: A dynamic DID approach. The figure reports the coefficients γs from estimating Equation (1). The dotted lines represent the 95% confidence interval bands. The vertical line is constructed on April 2014 when the consumption tax rate increased to 8%.

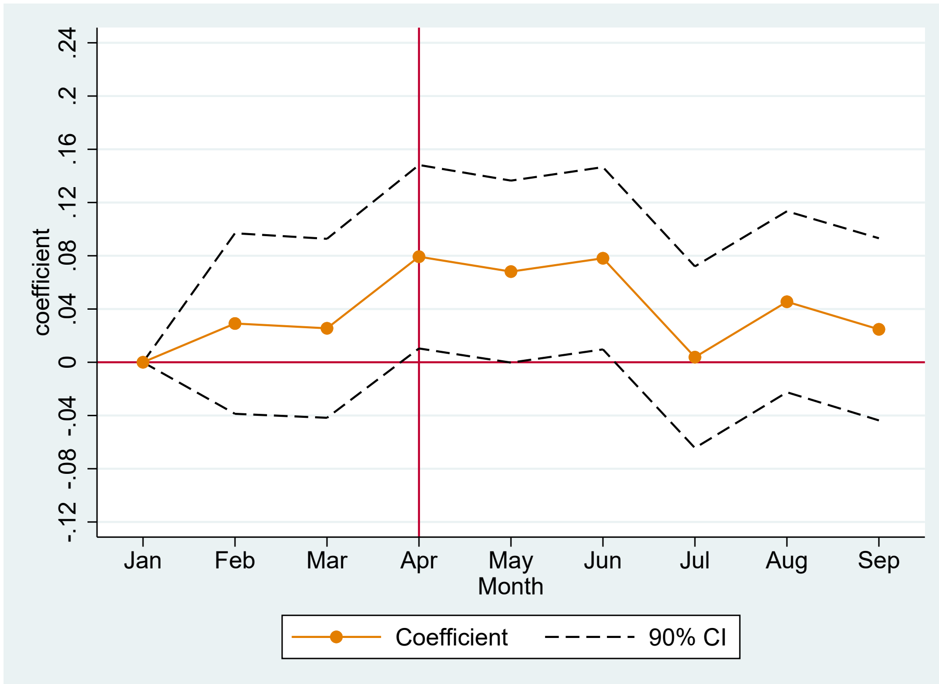

Figure 6: Development of the (relative) frequency of visiting treatment stores by those who graduated high school: A dynamic DID approach. The figure reports the coefficients γs from estimating Equation (2). The dotted lines represent the 95% confidence interval bands. The vertical line is anchored on April 2014 when the consumption tax rate increased to 8%.