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The cost of honesty: Field evidence☆

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ABSTRACT

This paper studies honesty in the face of changing cost. Using field data from a snack delivery company that employs an honesty payment system, the paper presents an event-study to analyze how price increases affect pay rates. The honesty payment system expects customers to pay a listed price for each consumed snack. This allows to measure honesty with the pay rate that compares consumption to payments. The results, which draw on deliveries from several thousand firms, show that price increases that make honest behavior more costly cause more cheating. Price increases of 15% trigger a fall in pay rates of 11%.

1. Introduction

Anecdotal evidence suggests that dishonest behavior is often driven by material motives. Declaring a private dinner as a business expense, underreporting income to save on taxes, and using public funds to build a private palace may differ in scope, but they share the same motivation. Given the large welfare losses induced by fraud, tax evasion, and corruption, a thorough understanding of the effects of material incentives on dishonesty is essential for public policy. Yet despite a recent surge in honesty-related research, the basic relation between material incentives and dishonest behavior is still uncertain. The present study contributes to the effect of material incentives on cheating through original field data and a novel identification strategy.

Traditional economic models predict that individuals cheat when material gains exceed the cost of punishment multiplied by the probability of detection (Becker, 1968). However, after observing that individuals regularly forgo material gains to remain honest even in the absence of punishment, more recent approaches incorporate the concept of psychological lying costs into the standard model (Abeler, Nosenzo, & Raymond, 2019). Lying costs are typically argued to comprise two components. First, the intrinsic component, often referred to as self-image concern, represents the desire to behave honestly before oneself (Abeler et al., 2019). In this sense, lying carries psychological costs because it disregards the self-concept (Mazar, Amir, & Ariely, 2008) or because it violates protected personal values (Gibson, Tanner, & Wagner, 2013). Second, lying costs are argued to consist of an extrinsic social part that reflects a perceived cheating aversion (Dufwenberg & Dufwenberg, 2018). Often described as reputation (Abeler et al., 2019), image (Khalmetski & Sliwka, 2019), or social identity concerns (Gneezy, Kajackaite, & Sobel, 2018), this component considers perceptions of one's dishonest behavior in the eyes of others.

Even when the standard model is extended with lying costs, individuals are expected to cheat when material gains exceed lying costs. Increasing the material incentives to cheat should eventually lead to dishonest behavior. However, despite the intuitive appeal of this prediction, some lab and field evidence have not shown such a clear relation between material incentives and honesty.

In lab experiments, participants are typically asked to perform a task in private and report the outcome to the experimenter. In the original die-roll experiment by Fischbacher & Föllmi-Heusi (2013), the authors observe that participants react to higher incentives within treatments, but not across treatments. In the meta-study by Abeler et al. (2019) that uses results from 90 die-roll experiments, the authors find that "an increase in incentives affects behavior only very little". In matrix tasks (Mazar et al., 2008), larger payoffs have not been found to increase dishonesty. Mazar et al. (2008) and Fischbacher & Föllmi-Heusi (2013) argue that this may be explained by lying costs increasing proportionately to the size of a lie and hence with material incentives.

Field studies provide more often than not indications that changes in material incentives are associated with changes in honesty. Within

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an analysis of donut and bagel deliveries under an honesty payment system, Levitt (2006) examines the effect of price changes on the pay rate. Although his study is purely descriptive, Levitt (2006) notices that higher item prices appear to correlate with a larger propensity to cheat. In a different setting, Berger, Fellner-Röhling, Sausgruber, & Traxler (2016) use state-border differentials in TV license fees to analyze the effect of different incentives on fee evasion rates in Austria. Their results suggest that a 1% increase in license fees corresponds to a 0.3 percentage point larger evasion rate. Sending out letters in a field experiment in Peru, Castillo, Petrie, Torero, & Viceisza (2014) find that fewer letters arrived when the letters contained money. Dugar & Bhattacharya (2017) analyze how sellers misrepresent the weight of fish at a large market in Kolkata. They observe a positive, though non-monotonic relationship between cheating rates and the absolute price of fish. Crossing lab and field evidence, Balafoutas, Czermak, Eulerich, & Fornwagner (2020) show that internal auditors respond to changing incentive schemes: When compensation was modified to depend on other's performance, subjects increasingly under-reported the performance of their peers.[1]

The idea that material incentives can affect dishonesty is further corroborated by a detailed analysis of lab designs. As Kajackaite & Gneezy (2017) point out, researchers regularly find an effect of incentives on dishonesty in experiments that present the option to cheat more explicitly, such as sender-receiver games.[2] They argue that an experimental set-up that eliminates observability further and renders the potential of cheating more salient is likely to suppress social concerns among the participants. Kajackaite & Gneezy (2017) demonstrate that modifying the die-under-cup paradigm to a mind game, whereby it becomes even more difficult to expose individual subjects as liars, in fact restores the relationship between incentives and cheating. Thus, Kajackaite & Gneezy (2017) show that individuals care about being exposed when cheating even in settings that aim to minimize reputational concerns. These residual suspicion may offer an explanation for the previous mixed effects of material incentives on honesty.

This paper investigates honesty and material incentives with field data. Using novel evidence from a company that delivers open boxes filled with snacks under an honesty payment system, this paper is the first to employ a Difference-in-Difference event study to measure the impact of price increases on honesty levels. Making honest behavior more costly, the results show a robust and persistent drop in honesty following a price increase. In the light of previous studies, this decline in honesty is stronger than anticipated: In the main specification, a price increase of around 15% corresponds to a decrease in honesty of 11%. The robustness of the results is validated through a placebo estimation, a detailed analysis of the individual increases, and the use of a fully balanced sample.

This study suggests that price changes can affect cheating behavior in the real world. The magnitude of the results can be interpreted in support of more recent experimental reviews by Kajackaite & Gneezy (2017) and Gneezy, Kajackaite, & Meier (2020), in that incentives may matter more than observed before.

Yet the study also points to a methodological caveat in equating pay rates with honesty: With aggregate data, one needs to acknowledge that the pay rate can be affected by heterogeneous consumption responses that do not reflect changes in honesty. Possibly, a price increase might stop honest office workers from using the service altogether, while the consumption of dishonest individuals could remain stable. In this case the aggregate pay rate could shift while individual honesty stays the same. Although a heterogeneity analysis does not suggest that this applies to the main result of the present study, other research involving aggregate pay rates should be mindful of this limitation.

Further findings indicate that dishonesty takes time, unfolding as it does over multiple deliveries. Being able to track honesty over time, the present results offer another view on previous findings (Abeler et al., 2019), adding to the discussion of the persistence of honesty.

Moreover, the company delivers snacks to offices, which allows to measure honesty at the location of the workplace. With data covering almost half a million deliveries made over 15 years to major branches of the public and the private sector, the study also contributes to the literature on honesty in specific industries, such as finance (Cohn, Fehr, & Marechal, 2014; Huber & Huber, 2020; Rahwan, Yoeli, & Fasolo, 2019) or the public service (Hanna & Wang, 2017; Barfort, Harmon, Hjorth, & Olsen, 2019). Although collected at the workplace, the data does not indicate a distinctive relationship between honesty and the industries studied previously.

2. Method

2.1. Data

The effect of incentives on honesty is studied through data from a company that supplies snacks to office workers. In the pages that follow, the typical operations of the company are explained along with the resulting key variables. Their summary statistics are provided in Table 1.

The items, mostly candy bars, cookies, and crisps, are delivered in small open boxes to offices in two large cities in a northern European country.[3] To process payments, the company uses an honesty payment system. Office workers take snacks out of the box, and are expected to place the money amount equal to the listed price in a locked cash box. Hence, the price information distinguishes the offering from pay-what-you-want schemes (Riener & Traxler, 2012; Gravert, 2017). As a rule of thumb, items are priced below vending machine levels but above grocery-shop prices.

At the initial delivery, a sales representative of the snack company visits the customer's office and explains the scheme to the present staff. Additional, self-explanatory instructions are printed on the box along with the price for one snack. The box is set up on a table or counter, usually in communal spaces such as a tea kitchen, an open office, or a reception. Boxes vary in size, from 52 items in the smallest box to 258 items in the largest (Table 1). Naturally, the composition of the items has been subject to small variations over 15 years. Although the exact arrangement of the items may vary between offices and across time, the basic layout and the nature of the items remain the same. An indicative photograph of a box is provided in Fig. 12 (Appendix A).

Boxes are refilled regularly. The standard refill frequency is determined by consumption levels throughout the first refill deliveries. If the box is emptied earlier, office workers can always request a refill via telephone or more recently, by scanning the QR code on the box. Intervals between refills vary. The mean interval between refill deliveries is 27 working days, so boxes are refilled approximately every five to six calendar weeks. The average customer relationship is durable, as reflected by a mean of around 40 deliveries per office.

When they refill boxes, the employees of the snack company record payments and count the number of remaining snacks to measure consumption and the pay rate. The pay rate constitutes the main variable, and is defined as the actual revenue collected from the cash box divided by the expected revenue from consumed snacks. Thus, the pay rate can be understood as the proportion of the total payments made and the value of the snacks consumed. The pay rate constitutes the measure of honesty for each delivery and office. A pay rate of 1 (or 100%) would indicate complete honesty, while a pay rate of 0 would reflect complete dishonesty.

[1] A comprehensive summary of further field experiments on dishonesty is provided by Gomes, Farrington, Defoe, & Maia (2021).

[2] For example, see Gneezy (2005) or Gibson et al. (2013).

[3] The company wishes to remain anonymous.

Table 1
Summary statistics with full sample (n = 454,173) .

Statistic	Mean	St. Dev.	Min	Pctl (5)	Pctl (95)	Max
Pay rate	0.78	0.17	0.00	0.67	0.89	1.80
Price (in cents)	70.88	7.56	60	60	80	80
Delivered volume (number of items)	145.79	39.28	52	85	227	258
Avg. consumption per day and office	6.21	5.35	0	1.14	16	143
Delivery interval (in working days)	27.39	16.60	1	8	54	230
Number of the refill delivery*	46.52	47.63	1	2	141	486
Number of total refill deliveries per office*	39.92	45.62	2	8	56	486

*The number of the refill delivery represents the nth delivery per office and is assigned to each delivery (n = 454,173), while the number of total refill deliveries denotes one total number of deliveries to each office office (n = 11,378).
Note: Each observation represents one delivery.

The average pay rate per delivery is 78%. This finding is consistent with the literature, showing that on average, individuals exhibit fairly honest behavior. The magnitude reflects values previously observed in honesty payment systems (Haan & Kooreman, 2002; Levitt, 2006; Schlüter & Vollan, 2015). However, similar to the measures of honesty in other contexts, the data only allows observation at the aggregate level. Information about the intensive and extensive margins of honesty cannot be provided.

Fig. 1 displays the distribution of pay rates for all deliveries (left) and for all office averages (right). Interestingly, the pay rate follows a smooth distribution, with 6.6% of all observations featuring a pay rate above 1. The method of payment may explain this seeming supra-honesty. Unlike traditional vending machines, the system does not provide change if workers find themselves without the correct sum for their selection. Although workers could ask office colleagues for spare change, individuals might find overpaying more convenient. One can assume that workers who overpay due to not having the correct change would balance their losses out by paying less at the next opportunity. Looking at the distribution of office averages corroborates this conjecture. In the long run, only 1.3% of offices pay more than the listed price.

Fig. 2 shows the time trend of average monthly pay rates of all deliveries throughout the sample period. This graphical evidence indicates that pay rates have declined over time. While pay rates started at 87% in 2004, they had decreased to around 73% 15 years later. The reasons for this decline are not self-evident. Learning or habituation effects can provide one explanation. Once workers become familiar with the honesty payment system, they may start to cheat more. Within-office trends provide partial support to this explanation. Fig. 3 displays the average pay rate for the number of the refill delivery. The decline at each office partially mirrors the declining general trend. The descriptive evidence from Fig. 3 suggests that workers behave less honestly with each delivery. This trend continues until approximately the 100th delivery, when the decline begins to level out.

However, the decline may also be explained by snack prices. Over the 15 years covered by the data, prices increased within two periods.

The 2004 price of 60 cents per item was raised to 70 cents towards the end of 2007 and increased further to 80 cents in 2012.[4] Although the two price increases are clustered around similar periods, they followed a staggered roll-out. This variation allows to identify an effect of the price increase with a Difference-in-Difference event study.

Although prices are presented saliently on the front of the box, the deliveries that immediately followed the price increases featured an additional note that highlighted the price increase. The note was placed right next to the cash box. Thus, offices were informed about the price increase on the first delivery featuring the new price. The price increases largely followed the rate of inflation for general food items throughout the observed period (Fig. 13, Appendix A).

Beyond that, the data offers further details. While honesty payment systems are an established measure of honesty,[5] the unique panel structure of the scheme and its ample capacity stand out: The data cover close to half a million deliveries made over 15 years to 11,378 different offices in various sectors. While the longitudinal data about deliveries permits an analysis of delivery-specific variations, such as price increases, cross-sectional data on the offices holds further information about specific industries.

The boxes are delivered to a wide array of private firms and public institutions. These range from banks, law firms, and post offices to police stations, hospitals, and universities. The company that supplies the service does not discriminate systematically between industries when approaching potential customers. The only sanctioning mechanism the company has is the withdrawal of the service from the office if pay rates become too low. However, this is uncommon. The average pay rate in the last delivery to offices terminating the service is 71%. Comparing this to the mean pay rate of 74% in the year 2013, which represents the mean year of all exits, suggests that dishonesty is not a predominant reason for ending the service. This is further emphasized when looking at the number of offices entering and exiting over time: After acquiring many new offices in the first years of service, the attrition rate appears relatively stable over the rest of the sample period (Fig. 11, Appendix A).

The snack company does not use legal actions to enforce payments. Therefore, the probability of facing external punishment is negligible. The service is focused on offices in two major cities. Naturally, the data is skewed towards city-specific core industries and the service sector in general (Fig. 4). Despite the possibility that particular office types use the service more frequently than others, the diverse spectrum of sectors represented in the data indicates that this impact is not severe. For this study, all offices have been coded on the division level in line with the detailed structure of the NACE Rev. 2 classifications. This allows to study industry specific honesty, the findings thereof are discussed in the Results section.

The underlying data set features several unique properties. Presently, these are summarized to position the article within other lab and field studies of honesty.

First, the box and its honesty payment system are situated at workplaces. Therefore, office workers use the system in a social environment on a day-to-day basis. They can familiarize themselves with the mechanism over several deliveries and over periods: In some cases, boxes have been supplied for over a decade. Given the social element, learning might be facilitated by communication between workers. As a result, individuals are likely to have near-complete information about the system.

Second, social identity concerns are possibly limited: Office workers are unlikely to be exposed as dishonest in front of their peers. Although payments could be processed in front of other coworkers, individuals are likely to find opportunities to be alone in tea kitchens, hallways, and at reception desks. Similarly, the system does not encourage relations between workers and the company or its representatives. Given that refill deliveries only occur every five to six weeks on average and involve no contact with workers, social ties can be considered scarce.

[4] The payment method might induce payment frictions. However, the country studied is cash intensive: An inquiry at the central bank revealed that in 2017 (the only year for which data is available), individuals carried coins worth on average € 6.28, with a mean of 2.06 ten-cent pieces, 1.90 twenty-cent pieces and 1.51 fifty-cent pieces. Bearing in mind that the data is from 2017 and the fact that the past decade saw a generally declining trend in cash payments, coin carriage was likely even higher when the price increases occurred in 2007 and 2012. As a result, the influence of frictions on pay rates appears negligible.

[5] Such as in the context of newspapers (Pruckner & Sausgruber, 2013), food items (Levitt, 2006), and flower fields (Schlüter & Vollan, 2015).

Fig. 12. Standard box. *Note:* Picture of a standard box with the cash box on the right. Prices are listed on the front in the blurred section.

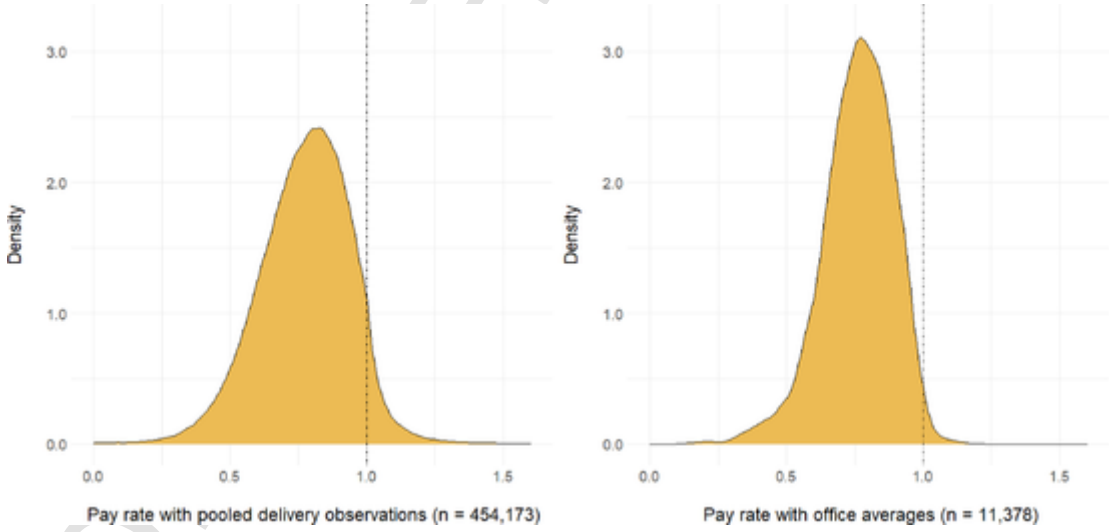

Fig. 1. Density plots of pay rate. *Note:* The left density plot shows the individual pay rates for all 454,173 deliveries, while the right plot illustrates the density of the average pay rates for all 11,738 offices. The dashed reference line for a pay rate of 1 denotes full honesty. Although some office workers overpay in certain deliveries (left), few do so constantly in individual offices (right).

Lastly, cheating entails a strategic tradeoff: If the box is overexploited, the probability of withdrawal increases. However, even the withdrawal of the box could not be attributed to individual behavior.

2.2. Empirical framework

The analysis measures the effect of changing incentives on honesty. Its focus is on the effect of price increases on the pay rate. For this purpose, the paper deploys an event-study design. Specifying the model requires an account of the longitudinal nature of the data set and the characteristics of the price increases.

As Fig. 2 indicates, time trends are relevant. Although deliveries with price increases were not subject to any other changes, general time-specific effects cannot be ruled out. Between 2004 and 2019, not only the prices of the items have changed. Economic events, such as the Great Recession and the Eurozone crisis, may have influenced preferences for honesty. Furthermore, it should be noted that many deliveries that followed the two price increases occurred around the end of the year. For this reason, seasonal effects must be considered alongside exogenous trends. Thus, time fixed effects are introduced that cover the 184 months of the sample.

The number of refill deliveries made to each office deserves further consideration. Some offices stop using the service after a handful of de-

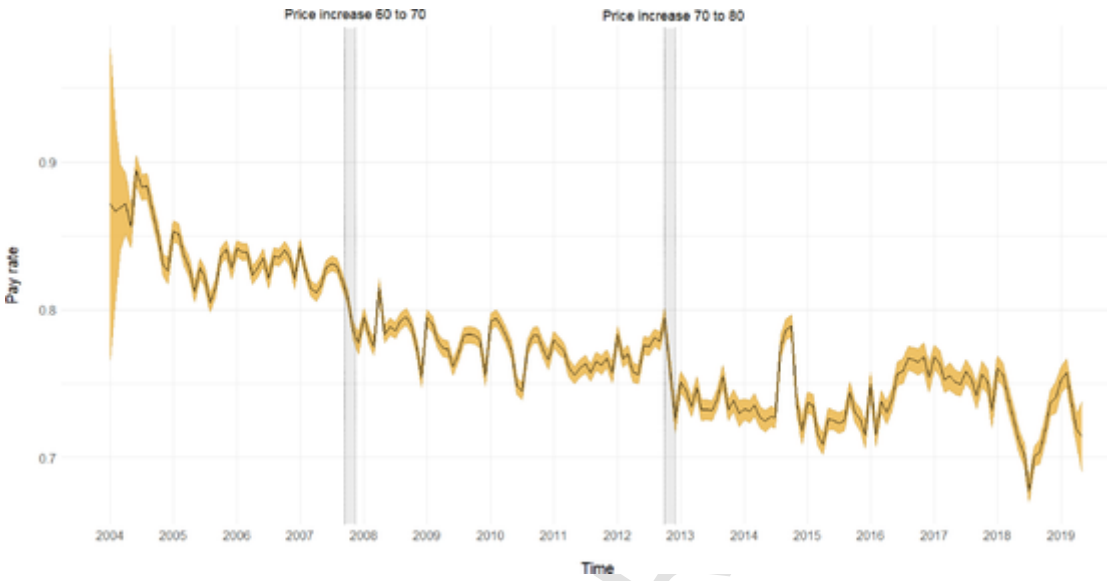

Fig. 2. General time trend of average pay rate per month. *Note:* Average pay rate per month with both price increases highlighted (grey) and 95% confidence intervals. The overall trends indicates declining pay rates as substantiated later in the regression model. However, short term changes need to be interpreted carefully: Sudden jumps may reflect phases of intensified acquisition of new customers, which tend to have higher pay rates in the beginning.

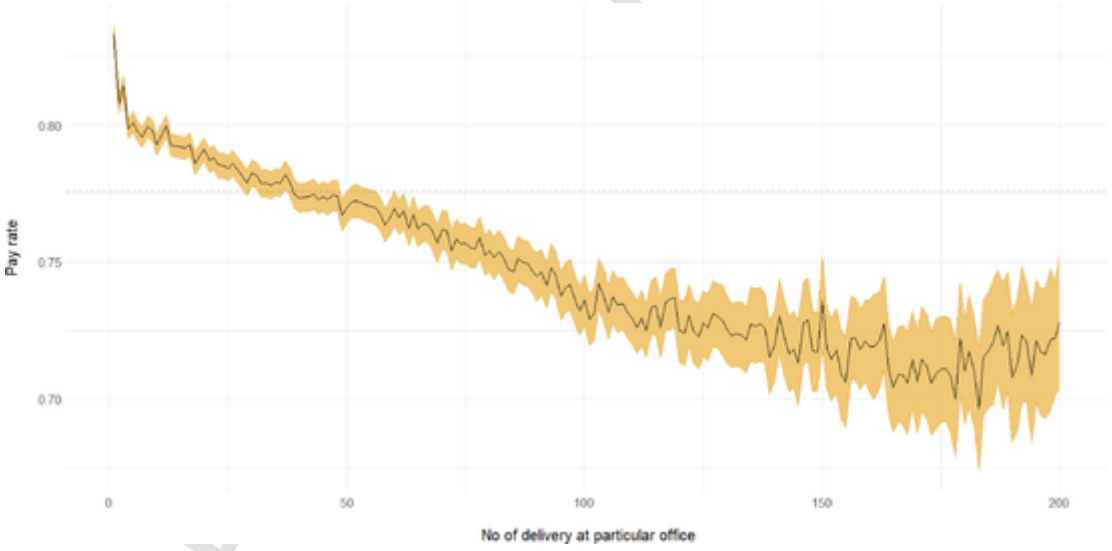

Fig. 3. Within office trend of average pay rate. *Note:* Average pay rate for each refill delivery to the same office with 95% confidence intervals. The dashed horizontal line depicts average pay rates of the entire sample. In each of the first 100 refill deliveries, office workers at the same office appear to behave less honestly.

liveries, while others continue using it for hundreds of refills. As a result, the total number of deliveries made to each office varies considerably and, potentially, endogenously. It cannot be ruled out that the accumulation of experience with the service influences the behavior of individuals. Fig. 3 corroborates this notion, indicating that honesty declines with increased experience. One interpretation is that people adapt to dishonest behavior with each new refill delivery. To account for these possible adaptation effects, the model controls for the number of a delivery to a particular office. Given that the pattern is potentially non-linear (Fig. 3), a quadratic term is added. The main specification uses an event-study estimation with dummy variables for deliveries before and after price-increase events. Formally, the estimation takes the form described in Eq. (1),

$$\log\left(PayRate_{it}\right) = \sum_{k=-5}^{-1} \delta_k D_k + \sum_{k=1}^{6} \delta_k D_k + \alpha X_{it} + \gamma_i + \gamma_t + \epsilon_{it} \quad (1)$$

where the main coefficients of interest δ_k estimate the effect of the delivery dummies D_k around the price increase D_1. The delivery right before the increase D_0 serves as a reference category. The vector X_{it} represents delivery-specific controls, such as the (squared) number of deliveries and the interval between refill deliveries. Furthermore, γ_i indicates office fixed effects and γ_t depicts monthly fixed effects. Given the potentially clustered sampling mechanism, all models employ robust standard errors that are clustered at the office level.

3. Results

The presentation of the results begins with the main findings on the effect of price increases on honesty. These are accompanied by several robustness checks that demonstrate the models' validity. A general discussion of other findings follows. For ease of interpretation, Tables 2

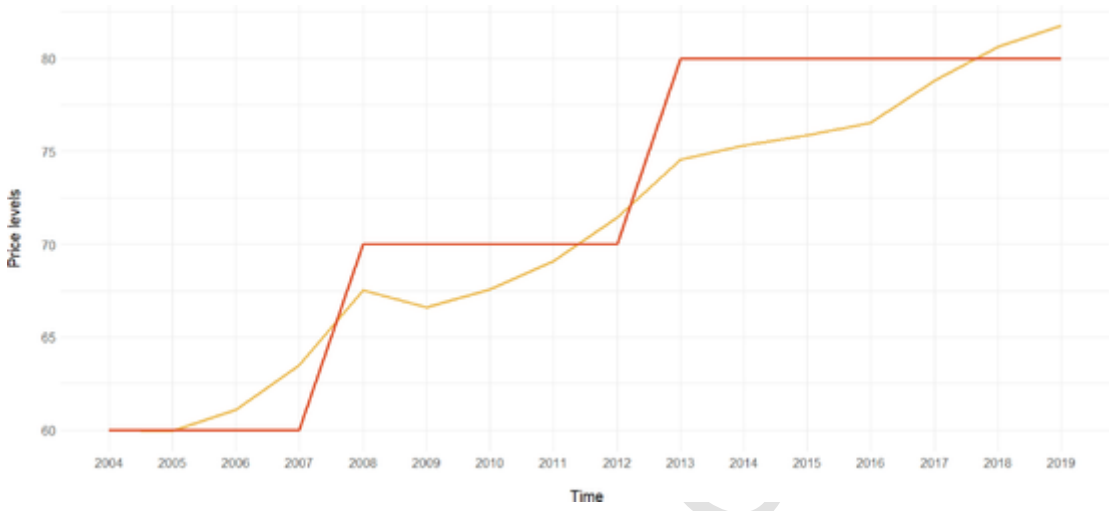

Fig. 13. Inflation and price levels. *Note:* The black line shows inflation for general food items in the sample country throughout the period studied. The red line depicts the listed price of a snack. We see that price increases follow the dimensions of general inflation. (For interpretation of the references to colour in this figure legend, the reader is referred to the web version of this article.)

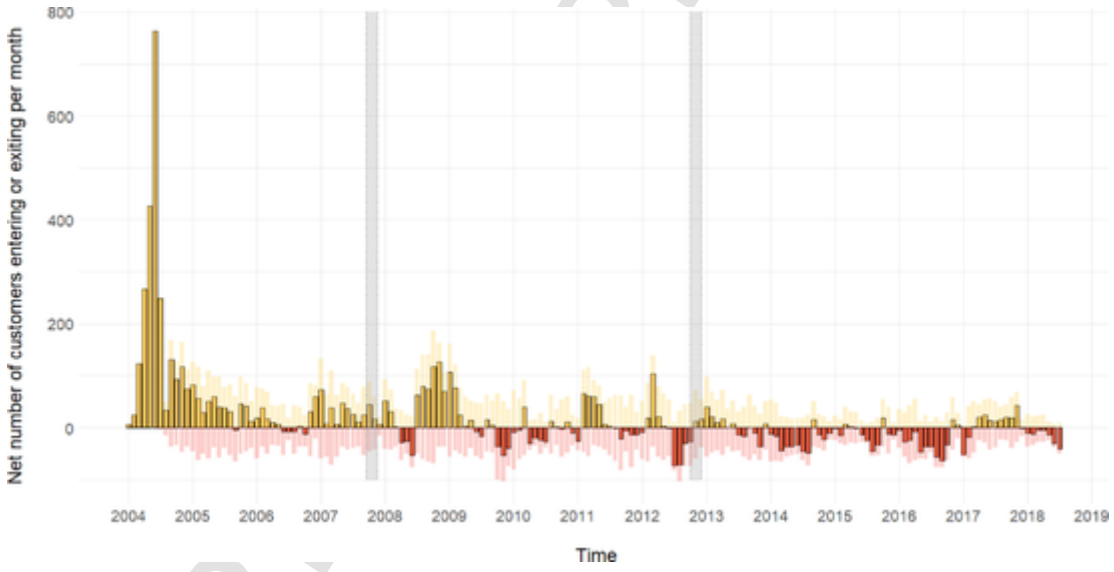

Fig. 11. Number of customer offices entering and exiting per month over time. *Note:* Net (filled bars) and absolute (transparent bars) number of customers entering and exiting per month over the sample from 2004 to 2019 with price increases highlighted (grey). Periods before and after price increases do not show any notable number of offices entering or exiting the service that would corroborate influential selection effects around price increases.

and 3 present the main results with the logarithmized[6] pay rate as the dependent variable that measures honesty and its elasticity.

3.1. Price

To identify a causal effect of changing incentives on honesty, the main model regresses the pay rate over event dummies, as specified in Eq. (1). Therefore, the main model cover the six deliveries before and after a price increase. Given the mean delivery intervals of around 4–6 weeks, this ensures that each estimation sample covers on average one year.

The sample includes only observations that fall within one of the 12 deliveries to establish the delivery right before the price increase (d-0) as reference category. The first array of results is provided in Table 2. For ease of presentation, all event estimates from Model (1) are illustrated additionally in Fig. 5.

The main model shows that increases in prices have a strong negative effect on honesty. While all deliveries prior to the price increase do not produce any effects, the first delivery with increased prices $(d+1)$ coincides with a sudden drop of 7.6% in the pay rate.[7] This effect is significant and increases further over subsequent deliveries up to −10.5% in the sixth delivery after an increase. Bearing in mind that prices were raised by around 15% at each increase, this corresponds to an elasticity of around −0.7 for the later deliveries.

The trajectory of the effect is worth highlighting. Although price increases are displayed saliently on the box, one might argue that some workers fail to notice the price increases at the first opportunity. If this were correct, the negative effect on the pay rate should likely rebound over subsequent deliveries. However, the opposite appears to occur:

[6] The nature of the results remains qualitatively unaffected by the log transformation.

[7] Note that the true percentage change is given by $100 * (e^{\delta_k} - 1)$.

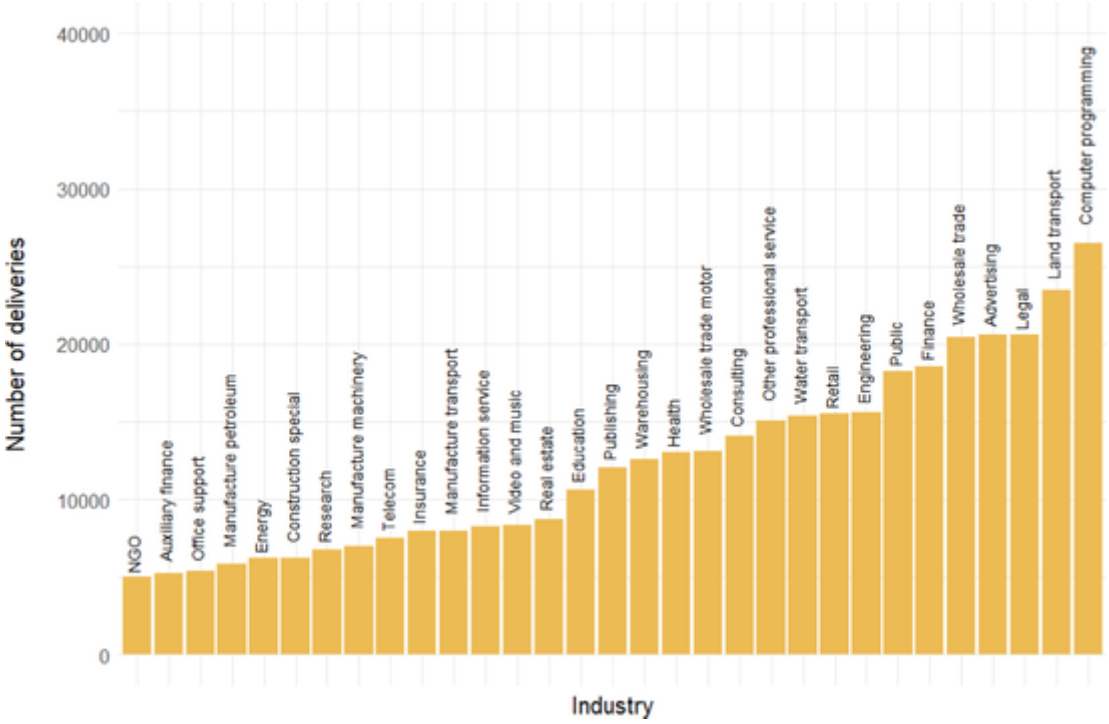

Fig. 4. Distribution of industries. *Note:* Only industries with more than 5.000 deliveries are depicted. Since the business targets offices in two large cities, the sample contains an above-average number of offices in service industries.

The pay rate decreases even more, suggesting that the drop is unlikely driven by the workers' failure to spot the new prices.

Before discussing the negative effect of price increases on honesty further, it is important to validate the approach through a series of robustness checks. Models (2) to (6) are deployed for this purpose. The first point they consider is the timing of the price increases. Even though the price increases from 60 to 70 cents and from 70 to 80 cents occurred five years apart, many of the increases came into effect towards the end of the year for both increases. Despite applying monthly fixed effects, seasonal patterns may still influence the estimates. The placebo Model (2) verifies that the basic event specification in Model (1) does not randomly pick up trends in honesty.

To this end, placebo price increases are simulated that index deliveries one year after the true increase. When no delivery to a particular office occurred in the corresponding month of the following year, the next or previous months were tried. If no box is delivered in any of the three months, the respective placebo increase is discarded. A significant estimate in one of the placebo event dummies would indicate that the results are influenced by seasonal effects.

Model (2) then re-estimates the equation of Model (1) using the placebo price increases. Notably, all event estimates in placebo Model (2) are not statistically different from 0. Fig. 6 highlights this finding by illustrating the estimates. The result suggests that the event design laid out in Model (1) is undisturbed by seasonal effects that could influence honesty.

To verify that Model (1) is not driven by any peculiarities of either one of the two price increases, the further robustness checks investigate each increase separately in Models (3) and (4). These models split the sample used for Model (1) to differentiate between the price increase from 60 to 70 cents (Model 3) and that from 70 to 80 cents (Model 4). Otherwise, the models are structurally equivalent to Model (1). Both specifications corroborate the previous findings. The first increase, from 60 to 70 cents, shows a 5.3% decrease in the pay rate in the delivery with the initial increase ($d+1$). In the following deliveries, there is a moderately declining trend up to delivery $d+6$, when the pay rate drops by almost 9% compared to the pay rates before the price increase.

Model (4) covers the second increase, from 70 to 80 cents. The decrease in $d+1$ starts at 12.7%. This sharp drop grows only marginally to 14.6% in the sixth delivery after the increase. Hence, the fall in the pay rate that follows the second price increase appears stronger than the one that follows the first increase. The patterns from the event estimates from Models (3) and (4) are summarized in Fig. 7.

Considering that the price changes from 60 to 70 cents and from 70 to 80 cents correspond to increases of 17% and 14% respectively, one would expect the first price increase to produce a stronger negative effect on the pay rate. However, an explanation for this disparity may be provided by the different composition of the two subsamples. The difference in effects may have been driven by offices that were not yet being supplied by the service at the time of the first increase in 2007.

Consequently, the final robustness check uses a fully balanced sample. The sample is refined to cover only offices that undergo both price increases and that feature observations of all six deliveries before and after each increase, covering 1215 offices. Estimates for both price increases that use this fully balanced sample are presented in Model (5) and (6) in Table 3, and depicted in Fig. 8.[8]

The first price increase that is estimated through the balanced sample in Model (5) yields coefficients that differ slightly from those of the unbalanced model. The balanced Model (5) shows an initial decrease of 7.9% ($d+1$), which is almost 3 percentage points larger than that in the unbalanced sample. The decrease in $d+6$ is also more apparent, with a total decline in the pay rate of 15%. This decline is approximately 6 percentage points larger than the corresponding decrease given by Model (3). The analysis of the second price increase through the balanced sample in Model (6) does not reveal statistically relevant differences in comparison to the estimation of the unbalanced sample of Model (4).

The results corroborate the conjecture that the difference between the two price increases is likely rooted in a slightly different composi-

[8] As each model only features the 12 consecutive deliveries around a price increase, the application of controls for the number of the delivery would cause collinearity with the event controls. Hence, Models (5) and (6) discard them from the standard event equation.

Table 2
Regression results (Part I).

	Dependent variable: log(Pay rate)			
	Sample restricted to event obs.	Sample restricted to event obs. (placebo)	Sample restricted to 60 to 70 increase	Sample restricted to 70 to 80 increase
	(1)	(2)	(3)	(4)
Price increase $d-5$	0.005	−0.001	−0.013	0.005
	(0.007)	(0.006)	(0.012)	(0.014)
Price increase $d-4$	−0.0006	0.0003	−0.013	−0.005
	(0.006)	(0.006)	(0.010)	(0.012)
Price increase $d-3$	−0.002	−0.002	−0.007	−0.009
	(0.006)	(0.005)	(0.008)	(0.011)
Price increase $d-2$	−0.004	0.001	−0.006	−0.011
	(0.005)	(0.005)	(0.007)	(0.009)
Price increase $d-1$	−0.006	−0.0007	−0.003	−0.012
	(0.004)	(0.004)	(0.005)	(0.007)
Price increase $d+1$	−0.079***	−0.004	−0.053***	−0.136***
	(0.006)	(0.005)	(0.007)	(0.014)
Price increase $d+2$	−0.084***	−0.003	−0.070***	−0.131***
	(0.008)	(0.005)	(0.011)	(0.017)
Price increase $d+3$	−0.091***	−0.001	−0.076***	−0.138***
	(0.008)	(0.006)	(0.012)	(0.017)
Price increase $d+4$	−0.102***	−0.003	−0.083***	−0.152***
	(0.009)	(0.007)	(0.013)	(0.019)
Price increase $d+5$	−0.102***	0.005	−0.086***	−0.148***
	(0.009)	(0.007)	(0.013)	(0.020)
Price increase $d+6$	−0.111***	0.002	−0.093***	−0.158***
	(0.010)	(0.007)	(0.015)	(0.021)
No of delivery	−0.001**	−0.001***	−0.008***	−0.003*
	(0.0003)	(0.0004)	(0.002)	(0.002)
No of delivery^2	2.654×10^{-6} **	2.604×10^{-6} **	4.883×10^{-5} ***	1.637×10^{-5} ***
	(1.003×10^{-6})	(9.2×10^{-7})	(1.209×10^{-5})	(4.054×10^{-6})
Interval between refills (working days)	0.001***	0.001***	0.001**	0.001***
	(0.0002)	(0.0001)	(0.0004)	(0.0002)
Office level FE	Yes	Yes	Yes	Yes
Delivery specific FE	Yes	Yes	Yes	Yes
Time FE (monthly)	Yes	Yes	Yes	Yes
Observations	76,098	64,742	35,215	40,883
Adjusted R^2	0.379	0.394	0.332	0.434

Office clustered standard-errors in parentheses.
Signif. Codes: ***: 0.001, **: 0.01, *: 0.05.
Notes: Delivery specific fixed effects include delivered volume and box type. The reference delivery for all models depicted is the last delivery prior to the price increase, d-0.

tion of the samples. Once a balanced sample is used, the magnitude of the estimates becomes comparable. Still, it should be noted that the progress of the fall in the pay rate varies slightly even when a balanced sample is employed. The drop in honesty following the first price increase appears to unfold less abruptly than the one which follows the second price increase. Since the two increases are separated by five years, it is not inconceivable that the increases were accompanied by unobserved differences in the sample composition. Considering the structure of firms entering and exiting the service (Fig. 11, Appendix A), the sample of the first increase is likely to consist of more offices that were among the early adopters of the service. Possibly, these offices may have had closer ties to the company's founder or were more interested in the service than offices that entered the sample later.

Table 3
Regression results (Part II) .

	Dependent variable: log(Pay rate)		
	Balanced sample 60 to 70 increase	Balanced sample 70 to 80 increase	Full sample NACE estimates
	(5)	(6)	(7)
Price increase $d-5$	0.004	0.024	
	(0.009)	(0.018)	
Price increase $d-4$	0.006	0.023	
	(0.009)	(0.017)	
Price increase $d-3$	0.012	0.019	
	(0.008)	(0.015)	
Price increase $d-2$	−0.003	0.004	
	(0.007)	(0.014)	
Price increase $d-1$	0.001	0.003	
	(0.006)	(0.010)	
Price increase $d+1$	−0.082***	−0.155***	
	(0.007)	(0.019)	
Price increase $d+2$	−0.119***	−0.155***	
	(0.011)	(0.021)	
Price increase $d+3$	−0.130***	−0.159***	
	(0.012)	(0.022)	
Price increase $d+4$	−0.146***	−0.171***	
	(0.013)	(0.024)	
Price increase $d+5$	−0.149***	−0.164***	
	(0.014)	(0.025)	
Price increase $d+6$	−0.163***	−0.176***	
	(0.014)	(0.026)	
Interval between refills (working days)	0.002***	0.001***	0.0008***
	(0.0003)	(0.0003)	(7.392×10^{-5})
Price = 70			−0.090***
			(0.011)
Price = 80			−0.173***
			(0.017)
NACE FE	No	No	Yes
Office level FE	Yes	Yes	No
Delivery specific FE	Yes	Yes	Yes
Time FE (monthly)	Yes	Yes	Yes
Observations	14,580	14,580	454,173
Adjusted R^2	0.343	0.448	0.145

Office clustered standard-errors in parentheses.
Signif. Codes: ***: 0.001, **: 0.01, *: 0.05.
Notes: Delivery specific fixed effects include delivered volume and box type. The reference delivery for Models (5) and (6) is the last delivery prior to the price increase, d-0. is the Price increase in d-0. The relevant reference categories for Model (7) are a price of 60 cents and the NACE division "53-Postal services". Model (7) does not control for office level fixed-effects due to multicollinearity with industries that comprise of few offices. However, even when including office level fixed-effects, estimates and standard errors for the remaining industries remain virtually unaffected.

Yet the moderately different estimates between the price increases do not affect the main result qualitatively: Changed incentives, in the form of higher prices, cause individuals to behave less honestly. In contrast to previous findings, these effects are sizeable. The different robustness checks corroborate the findings from the main model. The results show that price increases have a substantial and persistent effect on the pay rate. The main Model (1) implies an elasticity of around −0.7, indicating that a large fraction of the price increase is offset by the spike in dishonesty. The results of the balanced-sample specification suggest that honesty is almost unit elastic: Price increases of 17% and 14% correspond to decreases in the pay rate of around 15% and 16% in the later deliveries.

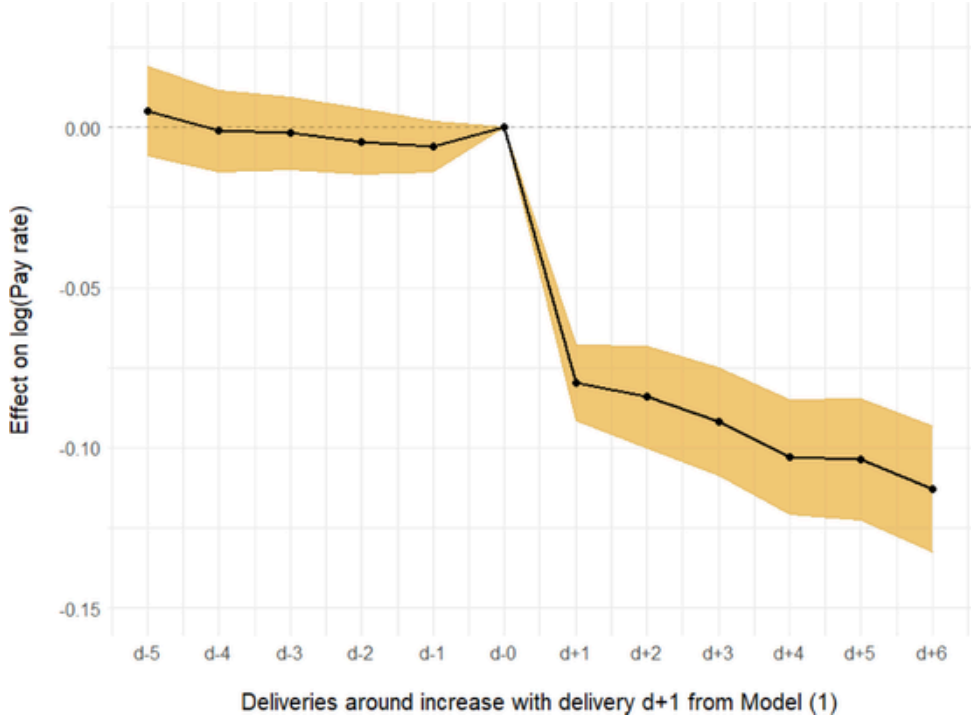

Fig. 5. Estimates of deliveries around price increase from Model (1). *Note:* Error bars show 95% confidence intervals. The estimates of the pay rate reveal a sudden drop in the first delivery after a price increase ($d + 1$).

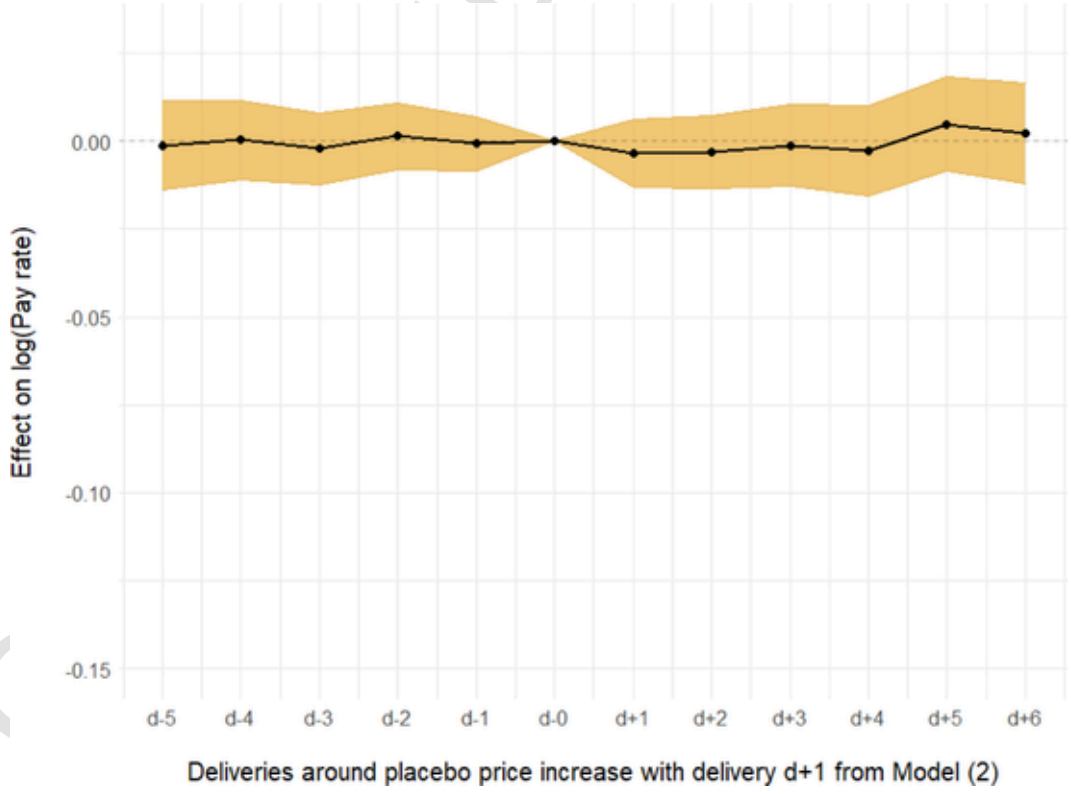

Fig. 6. Estimates around placebo price increase from Model (2). *Note:* Error bars show 95% confidence intervals. The placebo estimation confirms that honesty is not disturbed by seasonal effects.

One has to acknowledge that honesty as measured by the pay rate may be affected by different honesty types (Gibson et al., 2013). Individuals that differ systematically in their honesty could respond to the price increase at different margins. Assuming that honest office workers would change consumption after a price increase while dishonest workers may not alter their consumption levels, the pay rate could be affected despite honesty staying the same. Although the aggregate nature of the office-level data limits the study of heterogeneous types, observing honesty over a longer time period allows to approximate the prevalence of honesty types among each office. Based on the average pay

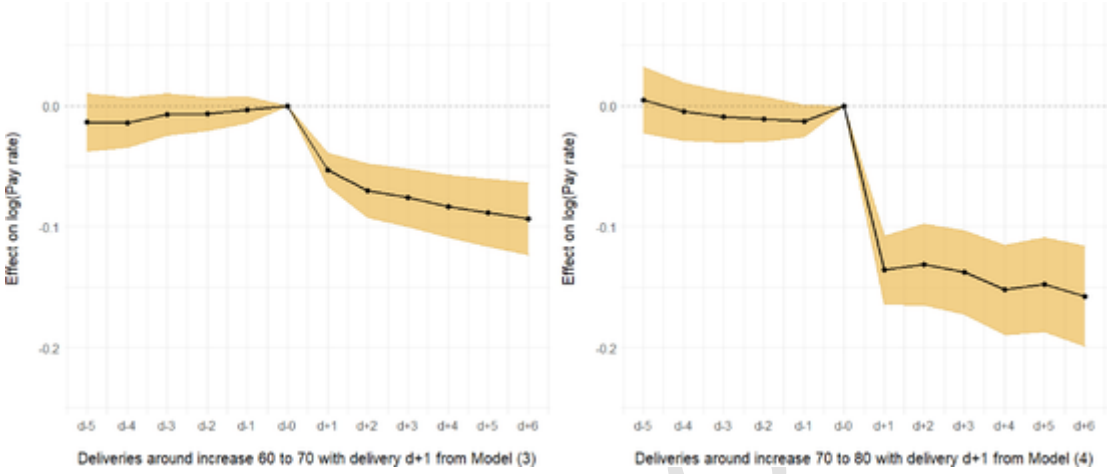

Fig. 7. Estimates of deliveries around a price increase from Models (3) and (4). *Note:* Error bars show 95% confidence intervals. When price increases are examined individually, estimates and trajectories show slight differences.

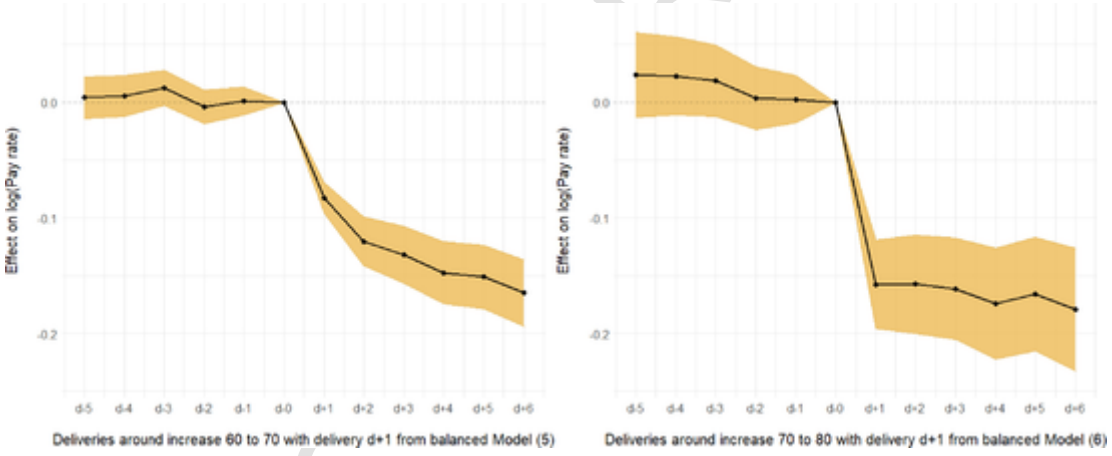

Fig. 8. Estimates of deliveries around a price increase from balanced Models (5) and (6). *Note:* Error bars show 95% confidence intervals. The balanced sample is restricted to offices that were being supplied at both price increases. As a result, estimates and trajectories become more alike.

rates of deliveries prior to the price increases, I identify the most and least honest offices. Using a fully balanced sample to minimize selection effects, I compare consumption after a price increase of offices in the top quartile to those in the bottom quartile of the pay rate distribution.

This exercise demonstrates modest heterogeneity in consumption responses: Surprisingly, while general consumption appears to increase by around 3 percent after a price increase (Table 4, Appendix A), this effect seems to be driven by the least honest offices (Table 5, Appendix A). Office workers who were predominantly honest before a price increase hardly change consumption levels. Thus, a slight share of the observed drop in the pay rate following the price increases appears to be driven by dishonest individuals increasing their overall consumption. These individuals responding to a price increase stronger at the consumption margin would be in line with the general finding of this paper: Increasing consumption while not increasing payments to the same extend can be described as increasing dishonesty. However, these inferences need to be interpreted with care given the limited information on individual honesty types and their inclination for corresponding behavioral responses.

3.2. General variables

Findings other than the effect of price increases also deserve attention. Model (7) controls for the division levels of the NACE Rev. 2 classification to explore the relation between specific industries and honesty. To use the entire sample, Model (7) replaces the event controls with

Table 4
Regression results for consumption from balanced sample .

	Dependent variable: log(Consumption)			
Model:	(1)	(2)	(3)	(4)
Price increase in past 3 months	0.021**			
	(0.007)			
Price increase in past 6 months		0.028***		
		(0.007)		
Price increase in past 9 months			0.021**	
			(0.007)	
Price increase in past 12 months				0.035***
				(0.008)
Office level FE	Yes	Yes	Yes	Yes
Delivery specific FE	Yes	Yes	Yes	Yes
Time FE (monthly)	Yes	Yes	Yes	Yes
Observations	29,160	29,160	29,160	29,160
Adjusted R^2	0.68	0.68	0.68	0.68

Office clustered standard-errors in parentheses.
Signif. Codes: ***: 0.001, **: 0.01, *: 0.05.
Notes: Delivery specific fixed effects here include No of delivery, delivered volume and box type. The dummy variable "Price increase" marks a delivery if it took place within the specified period after a price increase.

Table 5
Regression results for consumption from balanced sample split by honesty quartile .

	Dependent Variable: log(Consumption)							
	Top quartile				Bottom quartile			
Model:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Price increase	0.040*				0.014			
in past 3 months	(0.016)				(0.012)			
Price increase		0.025				0.038**		
in past 6 months		(0.014)				(0.014)		
Price increase			0.014				0.039*	
in past 9 months			(0.015)				(0.015)	
Price increase				0.029				0.050**
in past 12 months				(0.017)				(0.016)
Office level FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Delivery specific FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Time FE (monthly)	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,272	7,272	7,272	7,272	7,296	7,296	7,296	7,296
Adjusted R^2	0.63	0.62	0.62	0.63	0.69	0.69	0.69	0.69

Office clustered standard-errors in parentheses.
Signif. Codes: ***: 0.001, **: 0.01, *: 0.05.
Notes: Delivery specific fixed effects here include No of delivery, delivered volume and box type. The dummy variable "Price increase" marks a delivery if it took place within the specified period after a price increase.

general price levels. The resulting estimates from the NACE controls are depicted in Fig. 9. Although all sectors are controlled for, only the estimates for divisions which account for at least 100 unique offices are displayed. The results suggest some heterogeneity with industry honesty, yet almost none that is statistically significant. The utmost effects are observed in machine manufacturing with a pay rate that is 3.6% higher, and car dealerships, where the pay rate is 5.6% lower.

When interpreting these results, it is important to bear in mind that they are limited to correlational evidence and primarily focus on office workers in all industries. This limitation is further aggravated by the fact that different industries might also differ with respect to other characteristics. Honesty is likely affected by the exact location of the box within the office premises that could systematically differ across industry. In a similar fashion, accessibility to the public and employee turnover may be specific to industries.

Special work environments are a commonality among the industries that exhibit the most pronounced effects. Manufacturing of machinery (28), which is at the top end of the scale, or car dealerships (45), at the bottom, are likely to operate from premises that differ from the more conventional office sites that are most common in the sample. In industries with more similar environments, the honesty of office workers exhibits no exceptional disparities. Interestingly, the finance division appears better than its reputation (Cohn et al., 2014). The results indicate a slight positive estimate for finance workers (64), which is almost identical to the estimate for public officials (84). It follows that findings do not corroborate existing studies which argue that dishonest individuals may self-select into jobs where their dishonesty can be used to extract the highest rents (Hanna & Wang, 2017; Barfort et al., 2019). The relative indifference of honesty levels is echoed in other industries. Even when comparing the most extreme estimates for divisions with plausibly similar work environments, such as contrasting insurance (65) and auxiliary finance (66), the difference is only around 4 percentage points. The large standard errors corroborate these weak results. In addition, the analysis offers insights beyond industries.

First, the interval between refill deliveries shows a small but robust effect across all model specifications. The addition of 10 working days between refills translates to an increase in the pay rate of around 1%. The direction of the effect is surprising, as one might expect that individuals would become more dishonest as supplies are depleted, increasing the chance that their favorite snack might become unavailable. However, a reduced selection does not intensify the propensity to behave dishonestly. Gino & Pierce (2009) provide a different perspective on this effect. They observe that unethical behavior increases with abundance. A filled box may signal abundance, potentially tempting workers to behave dishonestly. Still, this effect indicates little. The interval between deliveries is likely to reflect office size, which might also influence preferences for honesty.

Second, service durability appears to affect the pay rate. The regression results for the number of refill deliveries corroborate the visual evidence from Fig. 3, showing that, on average, pay rates decline with each delivery. The estimates from Model (1) show that the pay rate declines by around 10% between the first and the 100th delivery. This decrease suggests some form of adaptation effect. Put differently, people may need practice to become accustomed to cheating. This supports the original observation by Fischbacher & Föllmi-Heusi (2013), who show that individuals who are asked to participate in a die-roll task for a second time cheat significantly more. However, the nature of this trend can be manifold. For example, the decrease could represent a learning effect of the external structure, whereby individuals realize that they can intensify their cheating behavior upon familiarizing themselves with the foreign mechanism and observing that smaller underpayments go unpunished. Alternatively, the decline may represent a form of internal habituation, where individuals adjust mentally to accepting smaller lies, irrespective of their understanding of all the external aspects of the honesty system.[9]

4. Discussion

Working with field data, the present paper complements the literature on incentives and dishonesty. In contrast to the generally modest effects of incentives on dishonesty observed in lab environments, my results demonstrate a strong effect in a field setting. Making an honest choice more costly induces more cheating. The different results could be explained by the different environment.

Naturally, the present field context differs in several dimensions from traditional lab experiments studying honesty. Subjects can interact with the scheme repeatedly, probe the mechanism over years, and discuss implications with coworkers. However, both contexts ultimately study similar behavior: Individuals have the opportunity to

[9] Neurological research supports this notion. For example, Garrett, Lazzaro, Ariely, & Sharot (2016) show that brain responses adapt to self-serving dishonesty.

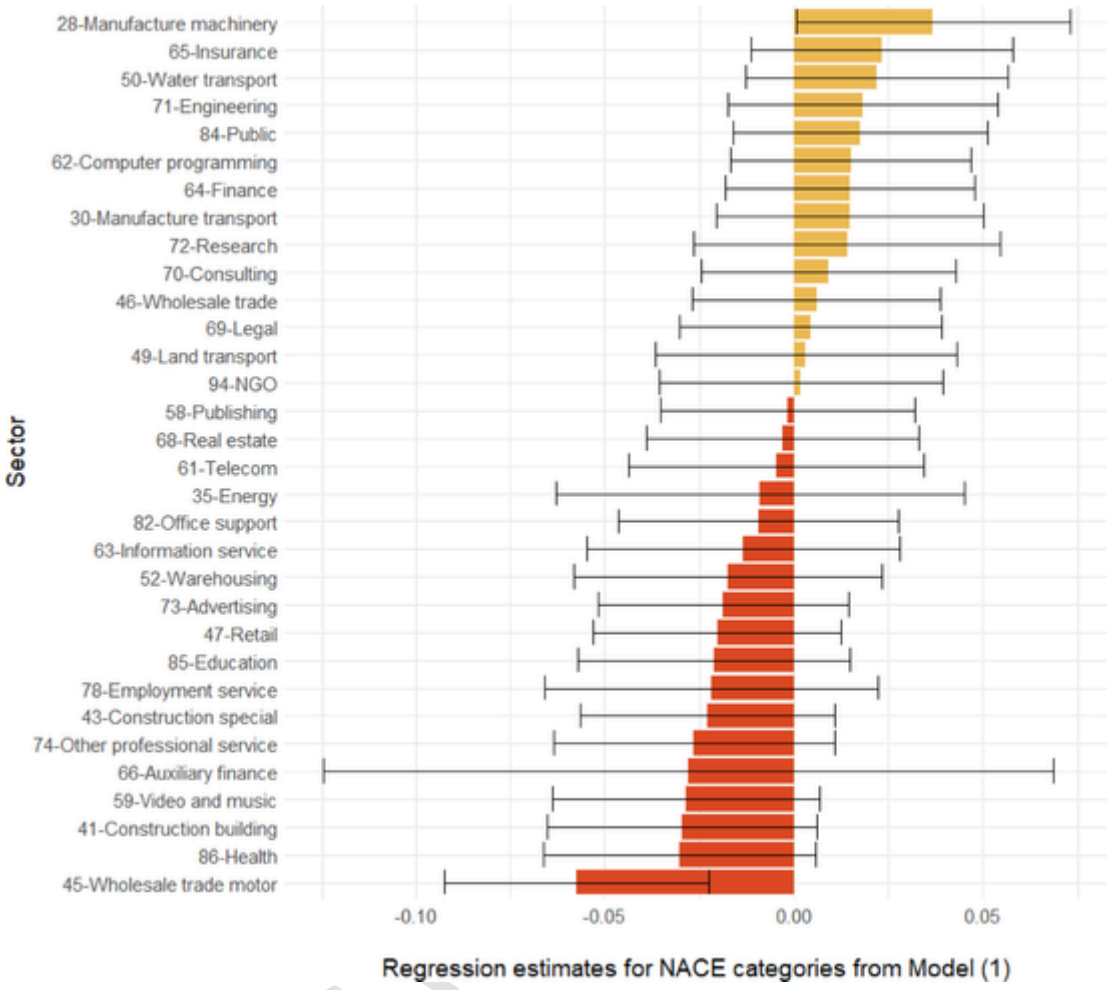

Fig. 9. Regression estimates for NACE divisions from Model (1). *Note:* Only estimates for divisions with more than 100 different offices are displayed. The reference category is "53-Postal services". Error bars depict 95% confidence intervals. Most industries exhibit only small and statistically minor differences in honesty.

cheat for a defined monetary gain at the expense of a remote third party.

An explanation may rest in the perception of anonymity. Kajackaite & Gneezy (2017) show that a slight variation of anonymity in an already anonymous honesty game leads to changes in concerns of being exposed as a liar. Showing that changes in the perceived anonymity alters the results on incentives and cheating in a lab environment highlights the delicacy of measuring a mechanism such as dishonesty, a behavior that most people wish to hide.

The long time horizon studied may corroborate this notion. While many lab experiments use one-shot games, the present study observes honesty over several years. Even with some employee turnover, most workers can familiarize with the mechanism repeatedly. One interpretation of the different results could be that individuals need to familiarize and verify cheating opportunities socially before escalating dishonest behavior. Similarly, long-term honesty may be affected by selective mental accounting. Feeling good from overpaying on one occasion may be more memorable than any negative emotions from cheating at another opportunity, leading to more dishonesty over time. Considering the often long term nature of contexts such as corruption, fraud, and tax evasion, the possibility to familiarize with the situation and engage in social learning should receive further attention for the study of honesty.

Thus, the partially different results observed in this study encourage a closer focus on complementing possibilities and limitations of lab and field environments. As previous research that linked lab and field settings was integral to show that more cheating in the lab predicts more cheating in the field (Potters & Stoop, 2016; Dai, Galeotti, & Villeval, 2018), additional research may help to verify if this applies to all specific mechanisms that underlie cheating behavior, such as the relation between material incentives and cheating.

Methodological discussions aside, the results add to theory. That the company which supplies the boxes can look back on perennial service relationships supports the notion of lying costs (e.g. Abeler, Becker, & Falk, 2014). People refrain from lying maximally although the probability of detection is negligible and individual sanctions are absent. However, the results do not support the theories that assume marginal lying costs to increase proportionally to material gains (Mazar et al., 2008; Fischbacher & Föllmi-Heusi, 2013). Instead, the findings suggest that material incentives should not be discarded as an important factor to explain dishonest behavior. A relatively modest price increase leads to more dishonesty, which aligns with empirical work from other domains, such as taxation (Berger et al., 2016). However, the results and their implications should be interpreted in the light of the present study's limitations.

First, the main measurement of honesty cannot identify individual behavior. Since each observation represents an entire office, no inferences can be drawn about the distribution of honest behavior. Two observations may have equally low pay rates, yet in one office this may be caused by many small-scale cheaters while the pay rate for the other may be driven by one particularly dishonest individual. Thus, the effect of changing incentives on honesty cannot be specified along the intensive and extensive margin. Further interpretations and policy responses need to be mindful of the possible heterogeneity of individual cheating behavior (Gibson et al., 2013).

Second, only two price increases are studied, covering a specific range of incentives. Prices were increased at 10-cent increments for an affordable product. Conclusions about the monotonicity of the relationship between other levels of incentives and cheating are not possible. In this context, the study cannot show with certainty that the measured drop in honesty was caused entirely by monetary incentives. The cost of honesty may have been in part raised by concerns about fairness (Houser, Vetter, & Winter, 2012) or negative reciprocity. However, the persistence of the negative effects after price increases limits this argument. Given typical refill intervals of 20–40 workings days, the results would imply that office workers punish the 10-cent price increase consistently for close to 12 calendar months. Considering that the service merely offers non-essential snacks that can be easily substituted, such a prolonged response out of negative reciprocity appears improbable. As the interaction is impersonal and the price increases align well with inflation (Fig. 13, Appendix A), the increases are unlikely to stand out as hostile compared to other price increases observable in grocery stores.

Third, the underlying data were not collected in a closed environment by trained researchers. Instead, they were gathered by the employees of the company as part of their daily work routine. Even though the company is obliged to maintain accurate records of financial data, including the pay rate, for tax reasons, it is possible that the other variables were not recorded with such rigor. Still, the scope of the data ensures that the impact of any noise emerging from the field nature of the study is limited.

Overall, the contrast with the results of most lab experiments deserves further attention. Rather than deriving ever more detailed assumptions from the same experimental games, this study encourages an expansion of methods and data sources to test more general presumptions first. This includes an appeal to embed findings on honesty from closely related fields, such as tax compliance and corruption. A more comprehensive overview could contribute to honesty models that predict behavior outside of the lab.

5. Conclusion

This study uses a novel data set to review the effect of material incentives and other factors on cheating. Collected over 15 years by a company that delivers boxes filled with snacks to various offices, the data permitted honesty to be measured by comparing consumption to payments. The paper, which is the first event-study of this domain, focuses on honesty before and after price increases. The main finding is that individuals act less honestly once prices, and thus incentives to cheat, are raised. Supplemented by a placebo estimation and other robustness checks, the identified effects are strong and persistent. Generally, the findings are consistent with lying-costs theories which posit that people do not always lie as much as they can.

However, the observed effect of incentives on cheating reemphasizes the importance of incentives to honesty. Contrasting the context of the present findings with the characteristics of previous evidence reveals other potentially relevant dimensions of dishonesty, such as social concerns, experience, and long-term behavior. Given that material incentives, in the form of financial sanctions, are a cornerstone of most policy mechanisms that aim to mitigate dishonest behaviors such as fraud, corruption, and tax evasion, a comprehensive understanding of incentives and dishonesty is critical.

The study also offers a unique opportunity for studying differences in honesty across industries. In contrast to previous findings, the results here indicate that workers in the finance industry rank among the most honest. Hence, the findings dovetail with recent scepticism of Rahwan et al. (2019) and Huber & Huber (2020) about particularly lax honesty norms in the banking industry (Cohn et al., 2014). The overall differences between industries are small and not statistically significant. Additionally, declining pay rates over multiple deliveries suggest that dishonesty is subject to learning or habituation effects. Since little attention has been devoted to honesty and adaptation, this is another potential avenue for future research.

In conclusion, the study shows that material incentives matter for honesty. Conflicting evidence on incentives and cheating underlines the necessity of regularly reviewing the context of how these findings are generated.

However, the same critique applies to the setup of the present study. Purchasing snacks under an honesty payment system is not representative of every economic opportunity in which the returns from dishonest behavior are positive. Given that investigating honesty and its mechanisms is fraught with difficulty, more versatile research is needed. Only by considering many settings instead of a few artificial situations can the true nature of honest behavior be brought to light.

Declaration of Competing Interest

Author declares that he has no conflict of interest.

Appendix A

The descriptive evidence does not indicate any remarkable deviations in consumption behavior for periods after a price increase (Fig. 10). However, the identification of consumption following a price increase is not as straightforward as that of honesty, given that consumption is heavily influenced by seasonal effects.

Inference is complicated further by the fact that offices with less consumption receive refills less frequently. This leads to low consumption offices receiving the delivery with a price increase usually later throughout the year than high consumption offices. As a result, the event-study approach cannot be extended safely to studying consumption. To overcome this challenge, an additional estimation is provided which alters the viewpoint of the analysis from deliveries to time periods in Tables 4 and 5. There, deliveries following a price increase are marked based on the bygone period between the increase and the subsequent delivery to circumvent the problem arising from the endogeneity of consumption and refill intervals. To mitigate selection bias as much as possible, I focus on the balanced sample for the examination of consumption effects after price increases.

Similar to the descriptive evidence in Fig. 10, this analysis does not suggest outstanding changes in consumption behavior following a price increase. Interestingly, there is a small increase in consumption following a price increase. This increase may reflect a heightened temptation from the prospect of consuming a good that has become more valuable. Alternatively, the increase may have sparked discussions among coworkers that promoted awareness of the snacks in general.

When comparing offices of the top quartile of the pay rate prior to price increases to the bottom quartile (Table 5), the increase in consumption appears to be mainly driven by the least honest offices (Tables 6 and 7).

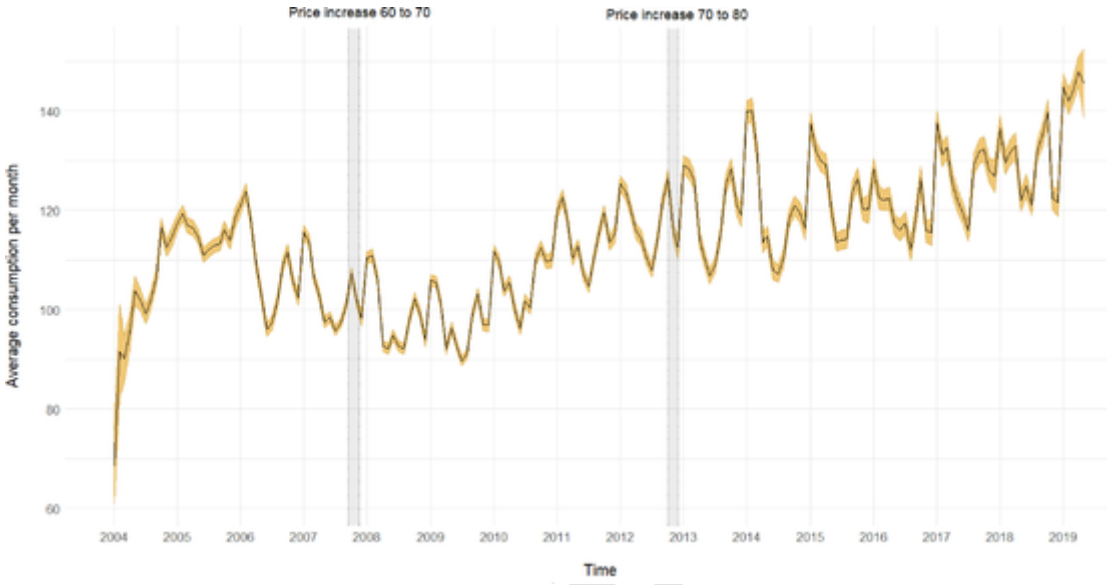

Fig. 10. Time trend of consumption per month. *Note:* Average consumption per month of entire sample from 2004 to 2019 with price increases highlighted (grey). Periods after price increases do not display any unusual change in consumption behavior.

Table 6
Summary statistics for event sample with price increase 60 to 70 cents .

	Normal Sample (Model 3)			Balanced Sample (Model 5)		
Statistic	Mean	Min	Max	Mean	Min	Max
Pay ratio	0.82	0.0002	1.77	0.83	0.03	1.76
Delivered volume	128.51	56	237	130.53	70	258
Avg. consumption/day	5.24	0.00	70.00	5.87	0.32	47.33
Duration of display	26.88	2	160	24.05	2	124
No of refill delivery*	26.23	1	176	31.27	1	176
Number of total refill deliveries per office*	78.20	3	486	124.30	37	486

*The number of the refill delivery represents the *n*th delivery per office and is assigned to each delivery, while the number of total refill deliveries denotes one total number of deliveries to each office office.
Note: Each observation represents one delivery.

Table 7
Summary statistics for event sample with price increase 70 to 80 cents .

	Normal Sample (Model 4)			Balanced Sample (Model 6)		
Statistic	Mean	Min	Max	Mean	Min	Max
Pay ratio	0.76	0.002	1.78	0.76	0.01	1.77
Delivered volume	151.83	68	258	159.76	74	258
Avg. consumption/day	4.24	0.00	53.33	4.74	0.00	49.67
Duration of display	35.39	1	214	34.09	2	214
No of refill delivery*	50.18	1	440	89.09	17	440
Number of total refill deliveries per office*	78.09	3	486	124.30	37	486

*The number of the refill delivery represents the *n*th delivery per office and is assigned to each delivery, while the number of total refill deliveries denotes one total number of deliveries to each office office.
Note: Each observation represents one delivery.

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