Do Tax Hikes That Are Perceived as Unfair Spur Cheating in Interpersonal Exchange?

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Draft Version: March 08, 2019

Abstract:
Recent experimental economics research has observed that individuals who believe they were treated unfairly in an interaction with one party are more likely to cheat in a subsequent, unrelated exchange with another party. This effect is attributed to unfair treatment eroding one's appreciation for the prosocial norms that discourage cheating and unfairness. Viewing taxpayers as the individuals who believe they were treated unfairly, we examine whether a cigarette tax hike, which surveyed smokers perceive as quite unfair, elicits an increase in cheating in transactions *outside* of the tax compliance decision. We examine this question using detailed data on NYC taxi drivers where we can identify fraudulent overcharging on taxi rides (cheating) and a subsample of smokers (affected taxpayers, identified via tickets for smoking in a cab). After a 160% increase in federal cigarette taxes, we observe that taxi drivers who smoke are about one third more likely to cheat customers relative to expectations from our difference-in-differences models.

JEL Codes: D91, H23, H30, K42

Key Words: taxes; cheating; fairness

Acknowledgements:
For helpful feedback we thank Steve Kaplan and Han Stice.

Introduction

We examine whether sin tax increases spur taxpayers to engage in antisocial cheating behavior. Two potential causes could produce such a reaction. First, being treated unfairly makes cheating behavior easier to rationalize, even when the potential victims of fraud are not the party behind an individual's unfair treatment (Houser et al. 2012). Surveys consistently report that the affected taxpayers view sin tax hikes as unfair (Carlson 2005; Dugan 2014), which suggests that these affected taxpayers may find it easier to rationalize defrauding others in the aftermath of sin tax hikes. Second, sin tax increases can serve to reduce the buying power of targeted taxpayers. Given the importance of incentives in spurring fraud, this reduction in purchasing power could increase an affected taxpayer's marginal value of wealth and, accordingly, their associated incentive to commit fraud.

We test this research question in a sample of taxi drivers, where we can observe both fraud and taxpayers affected by a sin tax hike. We identify frauds as cases of fraudulent overcharging on taxi rides, where drivers charge the "out of town rate" for rides taking place within New York City (as in Paci 2014 and Douthit et al. 2018). We identify taxpayers affected by a sin tax hike by identifying taxi drivers who smoke, via tickets for violating the taxi regulator's prohibition on drivers smoking in cabs. We use this latter data set to define a set of smoker (treatment) and nonsmoker (control) taxi drivers. We then examine how the likelihood of these drivers committing fraud changes around a large (159%) increase in federal excise taxes on cigarettes. The treatment effect odds ratio on the test variable *Smoker x Post Cigarette Tax Hike* in our difference-in-differences tests is consistently around 1.3, suggesting that relative to expected levels, smokers commit about one third again as much fraud in the weeks immediately after the tax hike goes into effect.

Directly, our findings add to the literature that documents a negative relation between tax compliance and perceived tax fairness (e.g., Feld and Frey 2007; Hartner et al. 2008; Alm et al. 1993; van Dijke and Verboon 2010). Building on this established effect of unfair taxes discouraging tax compliance, we document that a secondary effect of unfair taxes is that they can encourage similar antisocial cheating behavior in interpersonal exchanges. That is, unfair taxes not only lead to taxpayers cheating tax authorities, but also to taxpayers cheating counterparties in their interpersonal transactions. We are the first to document this secondary effect of unfair taxes, and we believe that it could be of interest to tax authorities in designing tax systems.

More broadly, our findings of fraud being spurred by perceived unfair tax hikes relate to the growing literature on the high costs of distrust in markets. The type of fraud we study contributes to distrust and discourages trade, which can generate significant efficiency losses when analyzed collectively across an entire market economy (Algan and Cahuc 2010). This tradeoff is perhaps worth considering when policymakers and tax authorities weigh techniques for revenue generation against potential costs (e.g., Guiso et al. 2011; Guiso et al. 2004).

The following sections outline the theory underlying our tests, describe the setting, specify empirical models, report results, and conclude.

Background and Hypothesis Development

Sin taxes are broadly defined as excise taxes on societally discouraged consumption (Lorenzi 2004). In many states and municipalities, these types of taxes are applied to tobacco products, alcohol, and gambling (Johnson and Meier 1990). Other examples include incremental excise taxes on soft drinks (Fletcher et al. 2010), strip clubs services (often referred to as a "pole tax," see Morse 2009), plastic bags (Weinstein 2009), and tanning salon services (Knapp 2011). These taxes are implemented to both discourage the consumption of societally harmful goods and

raise revenue, sometimes with the intent to use said revenue to rectify the harm created by the focal consumption (e.g., funding health initiatives with the proceeds of tobacco taxes, see Jamison et al. 2009).

These excise taxes can produce tax burdens on affected goods that are much higher than otherwise expected. For example, the tax hike we study increased New York City cigarette prices from about $7.50 per pack to about $9 per pack (Jaccarino and Ziemaszko 2009; Lee 2009). This 20% price increase is higher than the actual tax increase (from $0.39 to $1.01 per pack), but retail and wholesale markups are calculated after taxes, which magnifies the effect of the tax hike on retail prices (see Ribisl et al. 2010 and Hanson and Sullivan 2009).[1]

We are not the first to consider the unintended consequences of sin taxes. Chernick and Merriman (2013), for example, examine litter before and after an increase in cigarette taxes, observing that trashed packets of smuggled cigarettes (lacking a tax stamp) are more common after the excise tax increase. Likewise, research into smokers' health observes that tobacco tax hikes contribute to smokers smoking their (more expensive) cigarettes more intensely, which leads to substantial adverse health effects (like cancer at higher rates, see Thun et al. 1997). This behavior is more prevalent among low income smokers, suggesting that working class smokers are attuned to excise tax changes on cigarettes (Adda and Cornaglia 2006). Related work also indicates that while tax hikes of this type lead to smokers smoking fewer cigarettes, affected smokers shift towards cigarettes higher in tar and nicotine, which results in no overall changes in the total consumption of tar and nicotine (Evans and Farrelly 1998; Farrelly et al. 2004).

This literature, while informative, centers almost exclusively on how cigarette taxes, and sin taxes in general, impact the targeted consumption. Our aim in this paper is different. Instead,

[1] That is, federal excise taxes are collected by the manufacturer at the point of sale to distributors.

we focus on examining spillover effects, namely how increases in sin taxes impact the activities of targeted consumers *beyond* issues related to the consumption at hand. That is, we use a cigarette tax hike to examine how tax increases impact the actions of smokers in a domain outside of smoking.

The focal domain we examine is fraud, as recent experimental research has observed that unfair treatment can spur increased cheating behavior. This has been demonstrated in numerous cases when the cheating behavior is directed at the counterparty responsible for an individual's unfair treatment. Several clever field studies explore this relation in observing that workplace theft increases when employers cut wages (Greenberg 1990; Chen and Sandino 2012), and a long history of experimental studies documents similar tit-for-tat-style negative reciprocity. Hoffman et al. (1998) and Camerer and Thaler (1995) provide thorough reviews of this literature, and notable examples include Güth et al. (1982) and Fehr and Gächter (2000). The general consensus from this literature is that self-dealing by a first mover is frequently punished by the second mover, even in one-shot games where said punishment is costly for the second mover.

Recent work by Houser et al. (2012) extends this literature on negative reciprocity by examining whether this effect holds even when the party the second mover can "punish" is not the self-interested first mover. That is, if a first mover treats a second mover unfairly, is the second mover more likely to treat a third mover unfairly? Houser et al. (2012) find that this is the case, namely because the first mover's actions violate fairness norms, which suggests to the second mover that such norms are not valued. Accordingly, the types of norms that promote fairness and prosocial cooperation, and discourage cheating and self-dealing, are not weighed as heavily by the second mover when making their own decision on how to treat the third mover (Keizer et al. 2008). As individuals update their expectations for fair treatment by others after being on the receiving

end of unfair treatment, they themselves become less fair in their treatment of others in unrelated interactions.

This type of contagious spread of norms suggests that if smokers feel that they are unfairly treated by tax authorities via a cigarette tax hike, they will be more likely to go on to defraud unrelated third parties in their personal economic exchanges. Gallop regularly polls individuals on the fairness of excise tax increases on cigarettes, and respondents who smoke tend to rate such tax hikes as unfair, and especially so relative to nonsmokers (Carlson 2005; Dugan 2014). This view has been seconded by researchers in law and ethics, though occasionally commentators in those fields view sin taxes as just, even if unfair (given the societal costs imposed by the targeted consumption like alcohol or tobacco, see McLachlan 2002; Reiter 1996; Perkins 2014; Morse 2009; Zimring and Nelson 1995; Remler 2004). These surveys and commentaries suggest that sin tax hikes are perceived as unfair by targeted taxpayers, and that this perception of unfairness has some reasonable basis. Accordingly, we view this setting as one that involves the type of tax hikes likely to spur the erosion of prosocial norms against self-dealing and cheating suggested by Houser et al. (2012).

There is, however, contravening research suggesting that paying taxes promotes prosocial behavior. Harbaugh et al. (2007) manipulate contributions to public goods via two channels, voluntary donation or taxation, and then examine the neural responses of contributor subjects via fMRI (i.e., measuring brain activity via scans that track blood flow). In this setting, even mandatory tax payments towards public goods elicit neural rewards for good behavior, which are predictive of future voluntary contributions. That is, forcing individuals to pay taxes makes them feel good about being prosocial, which then encourages more unforced prosocial actions.

If this result holds with regard to cigarette tax hikes, for example, then perhaps smokers will be encouraged by their forced increased contributions to the public good, which could inspire more prosocial behavior going forward (e.g., less cheating). There is no closely approximate field evidence to support this supposition, though a small literature in tax compliance suggests that complying with taxes promotes patriotism (Konrad and Qari 2012; Slemrod 2007), which could be viewed as a type of affinity for recipients of the public good funded by one's tax dollars (i.e., fellow countrymen who also contribute to and benefit from the public goods you share in).

Both of these fraud predictors, either unfairness spurring more cheating or forced taxation increasing prosocial affinity and prosocial behavior, operate by affecting the ability of taxi drivers to rationalize fraud. That is, higher taxes in these settings either make committing fraud more or less easy, depending on the theory, for cab drivers to psychologically sanction. Beyond rationalization, however, higher taxes likely impact the fraud decision by changing the incentives of drivers.

First, higher payoffs to fraud predict more fraud. This result is robust and well documented in both laboratory experiments studying cheating (e.g., Gill et al. 2013; Kajackaite and Gneezy 2017; Charness et al. 2018; Covey et al. 1989) and empirical work on executive payoffs and corporate fraud (e.g., Ma et al. 2017; Haß et al. 2015; Shi et al. 2016). Increased taxes likely increase the incentives for fraud among affected taxpayers by decreasing their purchasing power. For example, cigarette tax hikes decrease the purchasing power of smokers, which perhaps results in smokers having a higher marginal utility of wealth after cigarette taxes increase, at least relative to nonsmokers. Moreover, smokers are more likely to be in the bottom of the income distribution, so these tax hikes may represent a meaningful decrease in purchasing power.[2] Accordingly, as

[2] By way of comparison, Farrelly et al. (2012) document that low and middle income smokers in New York spend between 5% and 24% of their incomes on cigarettes.

smokers likely place more value on a dollar after cigarette taxes are hiked, incentive effects could motivate more cheating among smokers after the implementation of such a tax hike.

On the other hand, research on how insolvency risk changes firm decisions suggests tax hikes may reduce drivers' incentive to commit fraud. Tax hikes increase costs, which, in turn, increases insolvency risk. One way for drivers to minimize insolvency risk in the face of this cost increase is to reduce rate code fraud. While these frauds have a positive expected value, due to few being detected and punished by regulators (Douthit et al. 2018), financially constrained drivers may choose to cease this cheating behavior to guarantee that they avoid the rare but costly ($350) fine for getting caught. Similar decisions are observed in the human capital and R&D investment patterns of innovative firms facing financial constraints, as these firms tend to cut likely profitable investment spending to reduce insolvency risks (Li 2011; Popov 2014; Mukherjee et al. 2017). For example, Mukherjee et al. (2017) find that corporate tax hikes decrease R&D investments, even in situations where cutting R&D is detrimental to the long-term prospects of the firm. Following this line of research, we may observe smokers cutting back on fraud, relative to non-smokers, after an increase in cigarette taxes.

Given these contradicting lines of research, we refrain from making a directional hypothesis regarding how increased sin taxes will impact fraud, and we instead consider the issue agnostically as an open research question.

Methods and Data

Testing this question requires a setting where researchers can identify (1) cheating behavior, (2) taxpayers targeted by a sin tax, and (3) a shock to said sin tax. Data on New York City taxi drivers and rides provide a sample that fits these criteria.

For every ride given by a licensed NYC taxi, a GPS computer system built into the taximeter collects data on the time and location of pickup and drop-off, time elapsed during the ride, distance covered by the ride, fare, method of payment, tip (when paid by credit card), and the ID number of the cab driver. The NYC taxi regulator, the NYC Taxi and Limousine Commission (TLC), makes this ride-level data available for public download. Importantly, these data link individual rides with a driver identifier, allowing for micro-level tracking of transactions across the same individual through time. These rich data have been used by economists in a variety of studies on labor supply (Farber 2008; Farber 2015; Camerer et al. 1997), learning (Haggag et al. 2017; King and Peters 2012), tipping (Haggag and Paci 2014), and contracting (Schneider 2010; Jackson and Schneider 2011).

A set of recent papers has also used these data to examine fraud (e.g., Paci 2014; Douthit et al. 2018). As described in this literature, fares in NYC cabs are calculated as a linear combination of time elapsed in travel and distance traveled. Accordingly, the correct fare can be closely approximated with the data on travel time and distance travelled provided by the TLC.

However, NYC cabs are allowed to charge double fares on trips taken outside of New York City's five boroughs, such as rides to suburbs in Long Island or Westchester county. This "double fare" feature is enabled by the push of a button on the taximeter that selects the "out-of'town" rate code. In 2010 it came to light that some cabbies were taking advantage of this functionality to fraudulently increase their earnings on in-town rides, even though charging double for these rides is illegal. This cheating behavior was uncovered after TLC staff closely examined the digital data reported by taximeters after being tipped off by a physician who noticed an unusually high charge on a cab ride he made regularly from his home to the hospital where he worked (Cartwright 2010). Further investigations reported that somewhere around 70% of all drivers engaged in this cheating

to some degree, with at least 10% being regular offenders (O'Connor 2010; Barbaro 2010). Ultimately, thousands of NYC cabbies were fined for these frauds, and dozens of the worst offenders were prosecuted under a state felony statute for "first-degree scheme to defraud" (NY Penal Law § 190.65). In sum, the TLC estimates that these frauds generated about $8 million in illicit gains for NYC taxi drivers in 2009 and 2010 (Barbaro 2010).

Paci (2014) was the first to study this type of fraud using the publicly available data on NYC taxi rides provided by the TLC. His study examines whether a driver's relative shift performance predicts rate code frauds, and he defines these frauds as those that exceed a conservatively (upwardly biased) estimated formulaic prediction of what the accurate fare should be. Douthit et al. (2018) employ the same method in examining whether sanctions for this type of fraud dissuades fraud going forward. We follow these two papers in defining rate code fraud. Specifically, NYC taxi fares during the regime we study (2009) included a base fare of $2.50 in addition to $2 per mile when moving at least 6 miles per hour or $0.40 per minute when in slower traffic. These rates are automatically accrued unless the driver intervenes to engage the "out-of-town rate" that doubles fares. We cannot observe speed at every point of the trip, so we follow Paci (2014) in defining drivers as committing rate code fraud in cases where the true fare exceeds what would be charged if *both* the $2 per mile (rate accruing in fast-moving traffic) and the $0.40 per minute (rate accruing in slow moving traffic) were simultaneously charged. Note that when honest in-town fares are being billed, only one of these charges is ever accruing at one time. When actual fares exceed what would be charged if *both* fares were accruing, a fraudulently higher rate was applied for at least part of the trip.[3]

[3] In some cases, certain surcharges or MTA taxes are applied to taxi rides. These are tacked on after a fare is calculated as a function of time and distance and, importantly for our setting, these incremental charges are also separately recorded in the TLC data. This allows us to exclude these charges and avoid any complications that could arise in determining fraudulent overcharging relative to rides with higher costs due to surcharges or taxes.

To limit our data to rides where charging this higher rate is explicitly illegal, we omit all trips taking place outside of Manhattan (following Paci 2014 and Douthit et al. 2018). Trips between any two locations in Manhattan should not pass through any area where a more expensive rate could accrue, so it is unlikely that these trips involved a "shortcut" from one side of Manhattan to the other that also crossed through Westchester county, for example. About 80% of all NYC taxi rides take place within Manhattan, so this design choice removes only a small portion of the overall population of rides.

Table 1 provides a preliminary test to highlight the effects of this rate code fraud. Model 1 predicts taxi fare in our primary sample of taxi rides, described in the following paragraphs, as a function of travel time, distance, and other factors that could inhibit the flow of traffic (time of day, weekends, etc.). Given the formulaic nature of taxi fares, these covariates explain almost all of the variation in fare amounts (R^2=95%). Model 2 adds to this baseline model an indicator variable for rate code fraud, which loads with a coefficient of 3.06 ($p<0.001$). This suggests that, controlling for ride characteristics, the mean incidence of rate code fraud generates fraudulent gains of about $3, or about 35% of the mean fare ($8.80). Notably, taxi drivers in NYC are almost exclusively residual claimants via either owning the medallion and cab, owning the cab and leasing the medallion, or leasing both the cab and medallion (which is by far the most popular arrangement, see Schneider 2010). Accordingly, this fraudulently obtained $3 accrues to the driver committing fraud and provides incentive for said action (as opposed to these drivers working for an hourly wage wherein the gains for this cheating behavior accrue to some separate principal).

To test whether unfair tax hikes increase these incidences of fraud, we identify taxpayers targeted by sin taxes using ticketing data provided by an open records request we submitted to the TLC. One of the responsibilities of the TLC is to enforce the regulations governing cab driver

conduct. During the regime we study, Rule §54-15(c) of the TLC Rule Book prohibited drivers' smoking in NYC taxicabs, and defined the penalty for violating this rule as a $150 fine (NYC TLC 2010). Our open records request to the TLC is for all tickets written under this rule over the 2010–2015 period, and we use drivers included in these data to define our sample of "smokers".[4] Nonsmokers are defined as cab drivers who did not receive a ticket for smoking in this data. This control sample of nonsmokers likely includes many smokers who did not receive a ticket for smoking in a taxicab, but we can at least be confident that the ratio of smokers to nonsmokers is likely lower in this control sample than in the treatment sample (where all the drivers have a ticket for smoking). Table 2 reports that 972 unique cab drivers received a ticket for smoking during this window.[5]

The sin tax hike we focus on is the increase in U.S. federal excise taxes on cigarettes from $0.39 per pack to $1.01 per pack that occurred on April 1, 2009, as part of President Obama's effort to fund a public health insurance program for poor children (Jamison et al. 2009).[6] We focus on taxi rides in the month before and month after the tax hike goes into effect. This leaves us with a sample stretching from March 1, 2009 to April 30, 2009. As we mention above, we only include

[4] We focus on a window after the implementation of the cigarette excise tax hike at the center of our study to ensure that we focus on taxpayers who bear the tax, as opposed to those who instead give up smoking in response to the tax hike and avoid explicit financial costs. That is, the drivers we identify as smokers were caught smoking after the passage of the excise tax, and were not dissuaded from their habit by higher taxes (e.g., Farrelly et al. 2012; DeCicca and McLeod 2008).

[5] Matching these tickets into the broader sample of taxi rides by driver entails some atypical methodology. In particular, the ride-level data released by the TLC uses an anonymized code as a driver identifier, which does not immediately permit matching into the data set of disciplinary sanctions, like tickets for smoking, where drivers are identified via actual taxi driver license numbers. However, researchers have recently cracked the anonymization procedure used by the TLC, and related research has used this incident as an instructional case of potential mistakes made in data security (Pandurangan 2014; Sharad 2016). We exploit this flaw, de-anonymize the ride-level data released by the TLC, and use this new de-anonymized data to identify drivers who have received a ticket for smoking in a New York City taxi cab. Using public data in this manner is not illegal (Michael 2015; Rubinstein and Hartzog 2016), though admittedly some observers do find this methodology distasteful (Egelman et al. 2012 and Metcalf and Crawford 2016 provide insightful commentaries, as does Michael 2015).

[6] Later, more dramatic increases in local cigarette excise taxes would perhaps make for a better research setting. Unfortunately, later years of the NYC taxi data are anonymized in a way that precludes identifying individual drivers with tickets for smoking.

within-Manhattan rides in the sample. To further ensure that unusual outliers do not influence the result, we also restrict the sample to rides that last between 5 and 120 minutes and cover less than 20 miles. For comparison, the entire borough of Manhattan is 13 miles long and 2.3 miles wide at its widest. We further exclude drivers that do not have at least one ride in both the pre- (March) and post-tax-hike period (April).

After imposing these restrictions on the TLC data, we are left with a sample of 15,925,145 rides served by 29,622 cab drivers. Of these drivers, 411 are in our treatment sample of smokers, leaving 29,211 control drivers in the nonsmoker sample. Overall, smokers make up about 1.39% of total drivers and conduct about 1.39% of total rides. Table 3 details these statistics.

Next, we report summary statistics for these rides in Table 4. The dependent variable of interest in our study, *Rate Code Fraud*, is a dummy variable that equals one in cases where cab drivers fraudulently overcharge their passenger, as described above. Similar to related research on this type of fraud, we observe that these instances are relatively uncommon, constituting only about 0.4% of all rides (analogous fraud rates are documented in Paci 2014 and Douthit et al. 2018).

Table 4 also includes summary statistics for the control variables that we include in our models to control for other predictors of drivers' propensity to cheat. First, we include dummy and continuous variables that proxy for whether or not the driver in question committed fraud in the prior month. These variables are defined as labeled, *Rate Code Fraud in Prior Month (dummy)* and *Rate Code Frauds per 1,000 rides* (in the prior month). A consistent line of research has documented that prior fraud is an influential predictor of contemporaneous fraud, which suggests that these variables may have substantial predictive power (e.g., Douthit et al. 2018; Rajgopal and White 2016; Millar et al. 2018). Defining this variable requires processing February, 2009 TLC

data to document rate code frauds in the month prior to the start of our sample period in March, 2009.

Beyond measures of prior fraud, we also include a method of payment dummy variable (*Credit Card Payment*). In a clever field study, Gino and Pierce (2010) find that working class service providers are less likely to cheat working class customers. The cab drivers in our sample fall into most reasonable definitions of working class (with an average annual income in New York City of about $35,000), and since credit card access skews to high-earners, customers paying with plastic may be a more common target for rate code fraud (Cohen-Cole 2011).

Another interesting field study, Balafoutas et al. (2013), documents that cab drivers more readily defraud out-of-town visitors, as these customers are less likely to become suspicious of unusually high taxi fares. To control for this relation, we include a dummy variable (*Nearby Hotel*) that equals one in cases where the pickup or drop-off point of the taxi ride is within 100 meters of a Manhattan hotel. Similar to Rajgopal and White (2016) and Douthit et al. (2018), we identify these hotel locations via municipal zoning data from New York City (see New York City Department of City Planning 2016).

Paci (2014) suggests another potentially important covariate, in that he finds low earnings predict more fraud. Accordingly, we include two dummy variables to control for this occurrence. We define *Low Earnings on Shift* to equal 1 in cases where a driver's earnings are in the lowest tercile compared to other drivers in the same decile of time on shift. We identify the start of a shift as any trip where at least 5 hours have transpired since the conclusion of the driver's last ride. We also include a dummy variable *Low Earnings in prior month*, which we set equal to 1 for drivers who have earnings in the lowest tercile compared to all other drivers in the prior month.

Beyond these covariates, we also include date- and time-level dummies (*Late Night*, *Weekday Rush Hour*, *Weekend*) as well as trip-level covariates (*Trip Time* and *Trip Distance*). *Late Night* is an indicator that equals one if the ride begins between 11PM and 4AM. *Weekday Rush Hour* is an indicator that equals one if the ride begins between 7AM and 9AM or 5PM and 7PM on a weekday. Similarly, *Weekend* is an indicator variable for rides that begin on Saturday and Sunday.

Univariate statistics for these variables, and a few others included to engender better understanding of our data, are reported in Table 4. The median ride in our sample lasts 10 minutes, covers 1.76 miles, and sees a fare of $7.70. The driver on this median ride has been on shift for about 4 hours, provided 12 prior rides, and generated about $113 in revenues.

Along with these controls, we also report summary statistics for the test variables necessary for the difference-in-differences (diff-in-diff) models we estimate. Specifically, we include a treatment dummy (*Smoker*), a post-period dummy (*Post Tax Hike*), and an interaction term that we will interpret as the treatment effect (*Smoker x Post Tax Hike*).

We estimate difference-in-differences logistic regressions predicting rate code fraud as function of these test variables and the control variables discussed previously. We cluster standard errors at the driver-date level.

Results

Table 5 reports the results of these logit models using the full sample (n=15,925,145). Model 1 is a baseline model excluding the diff-in-diff test variables, but including the controls. In this model we see that prior rate code fraud is a positive and powerful predictor of fraud on the current ride. Fraud is also more common among drivers with low past earnings (as measured in

the prior month, but not on the current shift), on weekends, during rush hours (on weekdays), and when passengers pay by credit card.

Model 2 in Table 5 incorporates the *Smoker* and *Post Tax Hike* dummy variables, both of which load negatively. This suggests that overall, smokers commit less fraud than nonsmokers, and all drivers commit less fraud in April relative to March. This first result could be motivated by the fact that smokers tend to use smoking to lower stress, when facing disappointing earnings for example, whereas nonsmokers without a potential relaxing agent like tobacco may be more tempted to turn to fraud (Pomerleau and Pomerleau 1987). More fraud in March relative to April may stem from the fact that better weather improves moods and increases prosocial behavior (Keller et al. 2005; Cunningham 1979).

Model 3 in Table 5 reports the full specification, which includes all covariates and test variables. The interaction term of interest, *Smoker x Post Tax Hike*, loads with a positive and significant coefficient ($p<0.01$). The odds ratio on this treatment variable is about 1.37, suggesting that after controlling for ride characteristics and prior fraud activity, smokers are about one third again more likely to defraud riders after the tax hike relative to non-smokers.

Next, we test this result for robustness using a matched sample design. Specifically, we use one-to-one Coarsened Exact Matching (CEM) to match pre tax hike rides with analogous post tax hike rides provided by the same driver (Berta et al. 2017; Blackwell et al. 2009; Iacus et al. 2011). Our goal in this analysis is to ensure that the treatment effect we observe in Model 3 of Table 5 is not a relic of different ride types being correlated with the treatment (e.g., smokers conducting more rides on weekends or near hotels after the cigarette tax hike is enforced).

We match on observable ride characteristics including trip time (coarsened into 3 minute buckets), distance (coarsened into quarter-mile buckets), date/time (matching on *Weekend*,

Weekday Rush Hour, and *Late Night*), earnings (matching on *Low Earnings on Shift* and *Low Earnings in prior month*), and the presence of hotel near the pickup or drop-off points. Few data points are lost in this match, and the descriptive statistics for this new sample of 15,092,886 taxi rides are reported in Table 6. These rides are largely similar to those in the broader, unrestricted sample.

Table 7 reports the covariate balance for this matched sample. Rubin (2001) suggests that well-matched samples should have variance ratios between 0.5 and 2, as well as standardized differences in means below 0.25 in absolute value. The standardized differences and variance ratios we report in Table 7 fall within these acceptable bounds for all of the variables in our analysis, suggesting that our match is successful in identifying control and treatment samples that do not differ on observables.

Table 8 replicates the logistic models of Table 5 using this matched sample. Briefly, the treatment effects we document in the unrestricted sample continue to hold at statistically significant levels, as the *Smoker x Post Tax Hike* treatment variable in Model 3 is a positive and statistically significant ($p<0.05$) predictor of rate code fraud. In Model 3, the odds ratio on this treatment effect is 1.25, suggesting that smokers are about one quarter more likely to commit rate code fraud after the cigarette tax hike goes into effect, relative to predicted levels.

We report a similar analysis in Table 9 that also uses this matched sample. However, unlike the Table 8 analysis, Table 9 weights each driver equally in a weighted logistic regression. For example, if Driver 1 accounts for 4 rides in our sample and Driver 2 accounts for 10 rides, in Table 9 we weight the 4 rides of Driver 1 by 0.25 and the 10 rides of Driver 2 by 0.1. We view this test as important, because we want to ensure that our results are not driven by a small number of smokers (or nonsmokers) who drastically change their conduct around the enforcement of the tax

increase. Table 9 resolves those concerns, as our treatment effect holds (and is actually stronger) in a sample that weighs each driver equally. The odds ratio on the *Smoker x Post Tax Hike* treatment variable in Model 3 is 1.6 ($p<0.0001$). This indicates that relative to their own baseline cheating levels and their nonsmoking peers, cab drivers who smoke are more than half again as likely to commit rate code fraud after a sizable increase in the excise taxes on cigarettes.

Next, we confirm our results in rare event logistic regressions. These models are optimized for data where the dependent variable deviates from the mode in less than 5% of cases, as distributions of this type generally yield upwardly biased coefficients when using conventional logistic regression models (King and Zeng 2001). Our data falls into this category, as *Rate Code Fraud* equals 1 in only about 0.4% of cases. Tomz et al. (2003) and King and Zeng (2001) introduce a logistic model corrected for rare event analysis, and we use this corrected model in Table 10 (for other examples of papers in finance and accounting applying this method, see Grabner and Moers 2013; Cain et al. 2017; Bhattacharya and Marshall 2012). Model 1 re-estimates our full model in the full sample, and Model 2 uses the matched sample. In both cases, we observe a statistically significant treatment effect ($p < 0.05$), with an odds ratio between 1.25 and 1.4. In line with our prior findings, these corrected models confirm that relative to non-smokers, smokers tend to ramp up their fraud (by about one third) after a large hike in cigarette taxes.

As a final robustness check, we execute a placebo/randomization test via Monte Carlo analysis to ensure that our observed treatment effect is meaningful. This is particularly important in large samples like ours. As we use more than 15 million observations, conventionally estimated p-values are almost always statistically significant. Lin et al. (2013) describe such econometric scenarios as ones where typical regressions are “too big to fail.”

To determine whether our observed difference-in-differences treatment effect (*Smoker x Post Tax Hike*) is actually meaningful, as opposed to just statistically significant along with everything else in a model with 15 million observations, we calculate Monte Carlo p-values as originally suggested by Fisher (1935). To do so, we randomize the treatment group by selecting 411 placebo smokers from our sample of drivers, as this is the same number of ticketed smokers in our actual sample (see Table 3). We then re-run our primary regression (identical to model 3 in Table 5, with 15,925,145 observations each) with this new randomized treatment sample and record the coefficient on the treatment effect (*Smoker x Post Tax Hike*) using the randomized sample of smokers. We execute this analysis 250 times to build a randomized distribution of our variable of interest, which we can then use to calculate a Fisher Monte Carlo p-value (beyond Fisher 1935, discussions and examples of this method are presented in Moir 1998; Brown et al. 2015; Davison and Hinkley 1997; North et al. 2002). Specifically, Fisher Monte Carlo p-values are calculated as:

$$\text{Fisher Monte Carlo p} - \text{value} = \frac{1 + \#\ randomizations\ where\ treat.effect > than\ actual\ treat.effect}{1 + total\ \#\ randomizations}$$

In our analysis, we observe that only 23 of the randomizations using randomly assigned treatment samples include a treatment effect (*Smoker x Post Cigarette Tax Hike*) that is larger than that observed in our actual data (0.3128). This generates a Fisher Monte Carlo p-value for our observed result (p=0.096) that is at least marginally significant, indicating that the treatment effect we observe is meaningful and unlikely to arise from random noise.

Discussion and Conclusion

We document that, relative to nonsmokers, smokers engage in more fraud in their interpersonal economic exchanges after an increase in cigarette taxes. Relative to base rates, this

increase in fraud is somewhere on the order of one quarter to one half. Two different forces predict this relation, rationalization and incentives.

First, being treated unfairly makes cheating others easier to rationalize. Houser et al. (2012) document that after being treated unfairly by a third party authority, experimental participants were more likely to lie and cheat in an effort to increase their own earnings, even when the counterparty they cheat is not the counterparty responsible for their own original unfair treatment. Houser et al. (2012) suggest that these effects likely stem from the original unfair treatment putting participants in a self-interested frame of mind, rather than a prosocial frame of mind, which then contributes to them making more self-interested decisions. Sin taxes, like the cigarette tax hike we examine, likely elicit similar behavioral reactions, as survey evidence suggests that sin tax increases are perceived as unfair by affected taxpayers (Carlson 2005; Dugan 2014).

Second, smokers likely have higher incentives to commit fraud after an increase in cigarette taxes, as these taxes contribute to smokers facing higher costs. As a result, smokers should have a higher marginal utility of wealth after the implementation of a cigarette tax hike, which, in expectation, will increase their payoffs to committing fraud.

Is our result a function of this unfair treatment spurring more fraud or simply an incentive effect? We cannot disentangle these two explanations. It is likely that doing so will require an experimental manipulation, as observational data permitting such analysis is difficult to envision. Accordingly, we cannot say with confidence whether our result is specific to only sin tax hikes and other tax hikes perceived as unfair (and motivated by the behavioral pressures documented in Houser et al. 2012), or whether our result is driven by incentive effects and generalizes to all tax hikes and cost increases. Most likely, both of these pressures contributed somewhat to our

observed treatment effects, but future research would be helpful in disentangling the relative contributions of these fraud motivators.

Our findings may inform several streams of literature. First, we extend research on the responses of taxpayers to taxes that are perceived as unfair. Prior studies in this area have identified that tax evasion is higher when taxes are perceived as unfair (e.g., Feld and Frey 2007; Hartner et al. 2008; Alm et al. 1993; van Dijke and Verboon 2010). We add to this existing work in documenting that other types of cheating are also more prevalent in the face of unfair taxation. In doing so, we also provide real-world support for recent behavioral economics research that documents that a first mover's unfair treatment of a second mover contributes to degrading the second mover's appreciation for prosocial norms, which leads to the second mover being more likely to cheat a third mover (Houser et al. 2012). Our study observes similar effects, with a tax authority in the role of the first mover, a taxpayer in the role of the second mover, and the focal taxpayer's customers in the role of the third mover.

Beyond our contribution to research in tax compliance and behavioral economics, our findings may be of interest to policymakers concerned with taxes and the efficiency of economic exchange. Specifically, self-interested cheating in interpersonal exchange, such as the rate code frauds we study, leads to increased distrust in markets. This distrust tends to inhibit economic transactions, as almost all transactions require at least some degree of trust (Arrow 1972). When distrust is rife, only exchanges with large gains from trade are completed, as potential transactions with small gains tend to be precluded by the necessity of price protecting against self-dealing counterparties (Chen et al. 2016). The associated deadweight efficiency losses of distrust are striking. Algan and Cahuc (2010), for example, estimate that residents of low-trust countries like

Mexico, Yugoslavia, and Russia could nearly double their per capita income if their exchanges involved levels of trust seen in high-trust countries like Sweden, Canada, and Belgium.

It is unlikely that taxi drivers cheating their customers in Manhattan will lead to an erosion of trust that weakens New York City's economy to the point where it is comparable to developing nations. However, this literature on the role of trust in economic growth underscores the high cost of distrust in a number of clever studies (a review is provided by Guiso et al. 2011), and policy makers would be prudent to consider this risk when contemplating decisions to impose taxes, or other policy interventions, that may be perceived as unfair.

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Table 1

Table 1 reports OLS models predicting taxi fare as a function of ride characteristics and rate code fraud in a sample of taxi rides in Manhattan in March and April of 2009, around the April 1, 2009 increase in federal cigarette taxes. Standard errors are clustered at the driver-date level, and * indicates statistical significance at the p<0.05 level.

OLS Models: Taxi Ride Fare = $\alpha + \beta_1$ x Rate Code Fraud + Σ(controls)				
	Model 1		Model 2	
Variable	Coefficient	[t-statistic]	Coefficient	[t-statistic]
Rate Code Fraud			3.06*	[235.13]
Trip time (minutes)	0.25*	[467.22]	0.25*	[470.21]
Trip distance (miles)	1.60*	[1049.72]	1.62*	[1081.87]
Weekday Rush Hour (dummy)	0.09*	[137.3]	0.08*	[131.72]
Weekend (dummy)	-0.05*	[-50.93]	-0.05*	[-58.76]
Late Night (dummy)	0.03*	[42.84]	0.03*	[45.09]
Intercept	2.45*	[819.2]	2.42*	[818.93]
Observations	15,925,145		15,925,145	
R^2	94.75%		95.07%	

Table 2

Table 2 reports the distribution of cab drivers with a ticket for smoking in the full sample of taxi cab ticketing infractions and those that also appear in the test sample of rides we analyze that take place in Manhattan in March and April of 2009, around the April 1, 2009 increase in federal cigarette taxes.

Drivers with Smoking Tickets	Cab Drivers in Sample
in Smoking Ticket Sample (2010-2015)	972
in Tax Hike Sample (March-April 2009)	411

Table 3

Table 3 reports the distribution of cab drivers with a ticket for smoking relative to the universe of drivers that appear in the test sample of rides we analyze that take place in Manhattan in March and April of 2009, around the April 1, 2009 increase in federal cigarette taxes.

Subsample Analysis	Drivers	Rides
Total in Full Sample:	29,622	15,925,145
Smokers:	411	221,973
Non-smokers:	29,211	15,703,172

Table 4

Table 4 reports the summary statistics for the test sample of taxi rides used in the main analysis. These rides take place in Manhattan in March and April of 2009, around the April 1, 2009 increase in federal cigarette taxes.

Variable	n	Mean	Std. Dev.	5th %	25th %	50th %	75th %	95th %
Smoker x Post Tax Hike (dummy)	15,925,145	0.007	0.083	0.000	0.000	0.000	0.000	0.000
Smoker (dummy)	15,925,145	0.014	0.117	0.000	0.000	0.000	0.000	0.000
Post Tax Hike (dummy)	15,925,145	0.503	0.500	0.000	0.000	1.000	1.000	1.000
Rate Code Fraud (dummy)	15,925,145	0.004	0.066	0.000	0.000	0.000	0.000	0.000
Rate Code Fraud in Prior Month (dummy)	15,925,145	0.467	0.499	0.000	0.000	0.000	1.000	1.000
Rate Code Fraud Cases in Prior Month	15,925,145	2.302	5.406	0.000	0.000	0.000	2.981	9.856
Rides in Prior Month	15,925,145	552.867	193.841	199.000	439.000	565.000	675.000	852.000
Revenue in Prior Month ($)	15,925,145	5,552.350	1,874.150	2,090.160	4,490.970	5,660.440	6,720.740	8,420.250
Cumulative Revenue on Shift ($)	15,925,145	126.670	86.609	15.600	58.100	112.660	178.930	286.120
Time on Shift (minutes)	15,925,145	265.168	201.180	12.217	102.067	229.567	390.917	629.000
Number of Rides on Shift	15,925,145	13.091	8.996	2.000	6.000	12.000	19.000	30.000
Nearby Hotel (dummy)	15,925,145	0.651	0.477	0.000	0.000	1.000	1.000	1.000
Credit Card Payment (dummy)	15,925,145	0.254	0.435	0.000	0.000	0.000	1.000	1.000
Weekday Rush Hour (dummy)	15,925,145	0.260	0.439	0.000	0.000	0.000	1.000	1.000
Weekend (dummy)	15,925,145	0.270	0.444	0.000	0.000	0.000	1.000	1.000
Late Night (dummy)	15,925,145	0.164	0.370	0.000	0.000	0.000	0.000	1.000
Low Earnings on shift (dummy)	15,925,145	0.330	0.470	0.000	0.000	0.000	1.000	1.000
Low Earnings in prior month (dummy)	15,925,145	0.173	0.378	0.000	0.000	0.000	0.000	1.000
Trip Fare ($)	15,925,145	8.798	3.610	4.900	6.200	7.700	10.200	16.100
Trip time (minutes)	15,925,145	11.578	5.938	5.050	7.050	10.000	14.100	22.867
Trip distance (miles)	15,925,145	2.169	1.469	0.700	1.200	1.760	2.700	5.100

Table 5

Table 5 reports logistic models predicting the occurrence of rate code fraud in a sample of taxi rides in Manhattan in March and April of 2009, around the April 1, 2009 increase in federal cigarette taxes. Standard errors are clustered at the driver-date level, and * indicates statistical significance at the p<0.05 level.

	Logit Model: Pr(Rate Code Fraud) = f(Smoker x Post Tax Hike, controls)					
	Model 1		Model 2		Model 3	
Variable	Coefficient	[z-statistic]	Coefficient	[z-statistic]	Coefficient	[z-statistic]
Smoker x Post Tax Hike (dummy)					0.31*	[2.69]
Smoker (dummy)			-0.16*	[-2.48]	-0.34*	[-4.96]
Post Tax Hike (dummy)			-0.08*	[-4.01]	-0.08*	[-4.17]
Nearby Hotel (dummy)	-0.08*	[-8.58]	-0.08*	[-8.63]	-0.08*	[-8.62]
Credit Card Payment (dummy)	0.07*	[6.06]	0.07*	[6.19]	0.07*	[6.18]
Rate Code Fraud in Prior Month (dummy)	1.49*	[46.51]	1.49*	[46.09]	1.49*	[46.07]
Rate Code Frauds per 1,000 rides	0.04*	[67.44]	0.04*	[67.26]	0.04*	[67.3]
Weekday Rush Hour (dummy)	0.61*	[49.3]	0.61*	[49.31]	0.61*	[49.31]
Weekend (dummy)	0.17*	[6.08]	0.16*	[6.01]	0.16*	[6.01]
Late Night (dummy)	-0.03*	[-2.08]	-0.04*	[-2.14]	-0.04*	[-2.15]
Low Earnings on shift (dummy)	-0.27*	[-18.1]	-0.27*	[-18.22]	-0.27*	[-18.21]
Low Earnings in prior month (dummy)	0.49*	[20.32]	0.49*	[20.37]	0.49*	[20.34]
Trip time (minutes)	0.05*	[61.51]	0.05*	[61.67]	0.05*	[61.68]
Trip distance (miles)	-3.36*	[-69.12]	-3.37*	[-68.92]	-3.37*	[-68.93]
Intercept	-3.50*	[-59.12]	-3.45*	[-55.98]	-3.45*	[-55.95]
Observations	15,925,145		15,925,145		15,925,145	
Pseudo R^2	36.415%		36.427%		36.429%	

Table 6

Table 6 reports the summary statistics for the matched sample of taxi rides used in the matched sample robustness check. These rides take place in Manhattan in March and April of 2009, around the April 1, 2009 increase in federal cigarette taxes.

Variable	n	Mean	Std. Dev.	5th %	25th %	50th %	75th %	95th %
Smoker x Post Tax Hike (dummy)	15,092,886	0.007	0.083	0.000	0.000	0.000	0.000	0.000
Smoker (dummy)	15,092,886	0.014	0.117	0.000	0.000	0.000	0.000	0.000
Post Tax Hike (dummy)	15,092,886	0.500	0.500	0.000	0.000	0.500	1.000	1.000
Rate Code Fraud (dummy)	15,092,886	0.004	0.065	0.000	0.000	0.000	0.000	0.000
Rate Code Fraud in Prior Month (dummy)	15,092,886	0.469	0.499	0.000	0.000	0.000	1.000	1.000
Rate Code Fraud Cases in Prior Month	15,092,886	2.316	5.425	0.000	0.000	0.000	2.982	9.859
Rides in Prior Month	15,092,886	555.133	192.756	204.000	443.000	567.000	676.000	853.000
Revenue in Prior Month ($)	15,092,886	5,574.640	1,862.750	2,138.880	4,523.180	5,676.420	6,732.820	8,433.400
Cumulative Revenue on Shift ($)	15,092,886	126.542	86.661	15.400	57.900	112.500	178.810	286.160
Time on Shift (minutes)	15,092,886	264.775	201.248	12.167	102.000	229.000	390.000	629.000
Number of Rides on Shift	15,092,886	13.095	9.004	2.000	6.000	12.000	19.000	30.000
Nearby Hotel (dummy)	15,092,886	0.653	0.476	0.000	0.000	1.000	1.000	1.000
Credit Card Payment (dummy)	15,092,886	0.253	0.435	0.000	0.000	0.000	1.000	1.000
Weekday Rush Hour (dummy)	15,092,886	0.263	0.440	0.000	0.000	0.000	1.000	1.000
Weekend (dummy)	15,092,886	0.266	0.442	0.000	0.000	0.000	1.000	1.000
Late Night (dummy)	15,092,886	0.160	0.367	0.000	0.000	0.000	0.000	1.000
Low Earnings on shift (dummy)	15,092,886	0.329	0.470	0.000	0.000	0.000	1.000	1.000
Low Earnings in prior month (dummy)	15,092,886	0.167	0.373	0.000	0.000	0.000	0.000	1.000
Trip Fare ($)	15,092,886	8.669	3.409	4.900	6.100	7.700	10.100	15.700
Trip time (minutes)	15,092,886	11.325	5.442	5.017	7.000	10.000	14.000	22.000
Trip distance (miles)	15,092,886	2.126	1.389	0.700	1.200	1.730	2.620	4.920

Table 7

Table 7 reports the covariate balance for the matched sample of taxi rides used in the matched sample robustness check. These rides take place in Manhattan in March and April of 2009, around the April 1, 2009 increase in federal cigarette taxes. Matching is done via coarsened exact matching (CEM). Rides given in the pre tax hike period are matched to rides in the post tax hike period by driver, travel time, travel distance, shift earnings, monthly earnings, time (late night, weekend, weekday rush hour), and the presence of a nearby hotel.

	Post Tax Hike (Obs. = 7,546,443)			Pre Tax Hike (Obs. = 7,546,443)			Balance	
Variable	Mean	Variance	Skewness	Mean	Variance	Skewness	Stand. Diff.	Var. Ratio
Rate Code Fraud (dummy)	0.004	0.004	16.004	0.005	0.005	14.567	-0.01	0.83
Rate Code Fraud in Prior Month (dummy)	0.430	0.245	0.283	0.507	0.250	-0.030	-0.16	0.98
Rate Code Fraud Cases in Prior Month	2.240	30.962	13.338	2.393	27.883	13.555	-0.03	1.11
Rides in Prior Month	571	39,228	-0.183	540	34,598	-0.129	0.16	1.13
Revenue in Prior Month ($)	5,795	3,707,655	-0.264	5,354	3,134,967	-0.198	0.24	1.18
Cumulative Revenue on Shift ($)	127.237	7,608.617	0.949	125.847	7,410.749	0.952	0.02	1.03
Time on Shift (minutes)	264.35	40,179.38	0.93	265.20	40,821.85	0.935	0.00	0.98
Number of Rides on Shift	13.167	82.124	0.972	13.022	80.016	0.973	0.02	1.03
Smoker (dummy)	0.014	0.014	8.264	0.014	0.014	8.311	0.00	1.01
Nearby Hotel (dummy)	0.653	0.227	-0.644	0.653	0.226	-0.644	0.00	1.00
Credit Card Payment (dummy)	0.253	0.189	1.137	0.254	0.189	1.133	0.00	1.00
Weekday Rush Hour (dummy)	0.263	0.194	1.077	0.263	0.194	1.077	0.00	1.00
Weekend (dummy)	0.266	0.195	1.061	0.266	0.195	1.061	0.00	1.00
Late Night (dummy)	0.160	0.135	1.850	0.160	0.135	1.850	0.00	1.00
Low Earnings on shift (dummy)	0.329	0.221	0.727	0.329	0.221	0.727	0.00	1.00
Low Earnings in prior month (dummy)	0.167	0.139	1.787	0.167	0.139	1.787	0.00	1.00
Trip time (minutes)	11.344	29.656	1.476	11.305	29.566	1.484	0.01	1.00
Trip distance (miles)	2.124	1.928	1.931	2.128	1.929	1.934	0.00	1.00

Table 8

Table 8 reports logistic models predicting the occurrence of rate code fraud in a matched sample of taxi rides in Manhattan in March and April of 2009, around the April 1, 2009 increase in federal cigarette taxes. Rides given in the pre tax hike period are matched to rides in the post tax hike period by driver, travel time, travel distance, shift earnings, monthly earnings, time (late night, weekend, weekday rush hour), and the presence of a nearby hotel. Standard errors are clustered at the driver-date level, and * indicates statistical significance at the p<0.05 level.

	Logit Model: Pr(Rate Code Fraud) = f(Smoker x Post Tax Hike, controls)					
	Model 1		Model 2		Model 3	
Variable	Coefficient	[z-statistic]	Coefficient	[z-statistic]	Coefficient	[z-statistic]
Smoker x Post Tax Hike (dummy)					0.23*	[2.02]
Smoker (dummy)			-0.19*	[-3.22]	-0.31*	[-4.34]
Post Tax Hike (dummy)			-0.17*	[-8.71]	-0.17*	[-8.77]
Nearby Hotel (dummy)	-0.09*	[-9.17]	-0.09*	[-9.18]	-0.09*	[-9.18]
Credit Card Payment (dummy)	0.04*	[3.11]	0.04*	[3.31]	0.04*	[3.31]
Rate Code Fraud in Prior Month (dummy)	1.49*	[47.26]	1.48*	[46.75]	1.48*	[46.74]
Rate Code Frauds per 1,000 rides	0.04*	[69.0]	0.04*	[68.68]	0.04*	[68.7]
Weekday Rush Hour (dummy)	0.67*	[52.03]	0.67*	[52.06]	0.67*	[52.06]
Weekend (dummy)	0.18*	[6.87]	0.18*	[6.89]	0.18*	[6.89]
Late Night (dummy)	-0.04*	[-2.45]	-0.04*	[-2.51]	-0.04*	[-2.52]
Low Earnings on shift (dummy)	-0.25*	[-17.64]	-0.26*	[-17.77]	-0.26*	[-17.77]
Low Earnings in prior month (dummy)	0.43*	[18.4]	0.43*	[18.43]	0.43*	[18.41]
Trip time (minutes)	0.10*	[56.94]	0.10*	[57.16]	0.10*	[57.16]
Trip distance (miles)	-3.75*	[-81.69]	-3.76*	[-81.43]	-3.76*	[-81.44]
Intercept	-3.58*	[-65.38]	-3.49*	[-60.93]	-3.49*	[-60.91]
Observations	15,092,886		15,092,886		15,092,886	
Pseudo R^2	38.7349%		38.7795%		38.7804%	

Table 9

Table 9 reports logistic models predicting the occurrence of rate code fraud in a matched sample of taxi rides in Manhattan in March and April of 2009, around the April 1, 2009 increase in federal cigarette taxes. Rides given in the pre tax hike period are matched to rides in the post tax hike period by driver, travel time, travel distance, shift earnings, monthly earnings, time (late night, weekend, weekday rush hour), and the presence of a nearby hotel. These models are also equally weighted by cab driver, so that each individual driver affects the model in equal proportion. Standard errors are clustered at the driver-date level, and * indicates statistical significance at the p<0.05 level.

	Logit Model: Pr(Rate Code Fraud) = f(Smoker x Post Tax Hike, controls)					
	Model 1		Model 2		Model 3	
Variable	Coefficient	[z-statistic]	Coefficient	[z-statistic]	Coefficient	[z-statistic]
Smoker x Post Tax Hike (dummy)					0.49*	[3.52]
Smoker (dummy)			-0.15	[-1.87]	-0.44*	[-4.76]
Post Tax Hike (dummy)			-0.11*	[-3.24]	-0.11*	[-3.4]
Nearby Hotel (dummy)	-0.09*	[-6.78]	-0.13*	[-6.78]	-0.13*	[-6.78]
Credit Card Payment (dummy)	0.04*	[2.55]	0.05*	[2.61]	0.05*	[2.6]
Rate Code Fraud in Prior Month (dummy)	1.49*	[26.61]	1.39*	[26.25]	1.39*	[26.24]
Rate Code Frauds per 1,000 rides	0.04*	[42.82]	0.04*	[42.16]	0.04*	[42.18]
Weekday Rush Hour (dummy)	0.67*	[27.19]	0.59*	[27.17]	0.59*	[27.16]
Weekend (dummy)	0.18*	[4.74]	0.23*	[4.73]	0.23*	[4.72]
Late Night (dummy)	0.07	[1.86]	0.07	[1.84]	0.07	[1.83]
Low Earnings on shift (dummy)	-0.25*	[-14.14]	-0.3*	[-14.19]	-0.30*	[-14.17]
Low Earnings in prior month (dummy)	0.43*	[15.45]	0.52*	[15.39]	0.52*	[15.35]
Trip time (minutes)	0.10*	[22.91]	0.08*	[22.91]	0.08*	[22.91]
Trip distance (miles)	-3.75*	[-44.5]	-3.54*	[-44.26]	-3.54*	[-44.26]
Intercept	-3.58*	[-37.27]	-3.37*	[-34.45]	-3.37*	[-34.42]
Observations	15,092,886		15,092,886		15,092,886	
Pseudo R^2	39.03%		39.04%		39.05%	

Table 10

Table 10 reports rare event logistic models, as described by King and Zeng (2001) and Tomz et al. (2003), predicting the occurrence of rate code fraud in a sample of taxi rides in Manhattan in March and April of 2009, around the April 1, 2009 increase in federal cigarette taxes. Model 1 uses the full sample described in Table 4, and Model 2 uses the matched sample described in Table 6. Standard errors are clustered at the driver-date level, and * indicates statistical significance at the p<0.05 level.

Rare Event Logit Model: Pr(Rate Code Fraud) = f(Smoker x Post Tax Hike, controls)				
	Model 1		Model 2	
Variable	Coefficient	[z-statistic]	Coefficient	[z-statistic]
Smoker x Post Tax Hike (dummy)	0.31*	2.68	0.23*	2.01
Smoker (dummy)	-0.33*	-4.94	-0.30*	-4.31
Post Tax Hike (dummy)	-0.08*	-4.17	-0.17*	-8.77
Nearby Hotel (dummy)	-0.08*	-8.62	-0.09*	-9.18
Credit Card Payment (dummy)	0.07*	6.18	0.04*	3.32
Rate Code Fraud in Prior Month (dummy)	1.48*	46.07	1.48*	46.73
Rate Code Frauds per 1,000 rides	0.04*	67.3	0.04*	68.7
Weekday Rush Hour (dummy)	0.61*	49.31	0.67*	52.05
Weekend (dummy)	0.16*	6.01	0.18*	6.89
Late Night (dummy)	-0.04*	-2.14	-0.04*	-2.52
Low Earnings on shift (dummy)	-0.27*	-18.21	-0.26*	-17.77
Low Earnings in prior month (dummy)	0.49*	20.34	0.43*	18.41
Trip time (minutes)	0.05*	61.67	0.10*	57.16
Trip distance (miles)	-3.37*	-68.93	-3.76*	-81.43
Intercept	-3.45*	-55.94	-3.49*	-60.9
Observations	15,925,145		15,092,886	
Pseudo R^2	36.43%		37.78%	