Anticipated Monitoring, Inhibited Detection, and Diminished Deterrence

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Abstract

Monitoring programs—by creating expected costs to regulatory violations—promote compliance through general deterrence, and are essential for regulating firms with potentially hazardous products and imperfectly observable compliance. Yet, evidence on how monitoring deployment affects perceived detection probabilities and—by extension—compliance, is sparse. Beginning in May 2020, pandemic-related protocols in Maricopa County, Arizona, required routine health inspections to occur by video-conference at food establishments with vulnerable populations (e.g., hospitals and nursing homes). Unlike conventional on-site inspections—which continued at most food establishments—these "virtual" inspections were scheduled in advance, and thus, easily anticipated. The virtual format also likely inhibits observation of some violations, further reducing detection probability. Tracking five violations that are detected by tests in both inspection formats, I find evidence of substantial anticipation-enabled detection avoidance. Comparing against contemporaneous on-site inspections, virtual inspections detect 53% fewer of these specific violations relative to pre-treatment levels, and that decrease reverses entirely when treated establishments are subsequently inspected on-site. Detected counts of all violations decrease 39% in virtual inspections. Consistent with general deterrence, this decrease is *more* than offset in establishments' first post-treatment on-site inspections, where detected counts exceed the pre-treatment average by 25%. Deterrence-effect heterogeneity suggests a simple dynamic policy could improve overall compliance with existing agency resources.

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1 Introduction

Programs of routine unannounced inspections are nearly universal in enforcing food-service hygiene and safety regulation. Yet, while entirely preventable, the Centers for Disease Control and Prevention (CDC) estimates that 48 million Americans contract a foodborne illness each year, with an annual economic burden estimated at 15.5 billion dollars (Hoffmann et al., 2015).[1] And from 2017 through 2019, Moritz et al. (2023) report that the CDC was *voluntarily* alerted to 800 foodborne-illness outbreaks involving retail food establishments, by 25 state and local health departments.

Periodic compliance monitoring creates expected costs for regulatory violations—the penalty if detected multiplied by the perceived detection probability—and promotes compliance through general deterrence (Becker, 1968). This enforcement approach has profound reach; it is central to regulating—among other things—environmental quality, workplace hazards, international maritime practices, nursing-home standards, and licensed firearm dealers. In the food-service and retail-food industries, because causality will often be profoundly difficult to establish *ex post* of harms, *ex-ante* regulation of safety through deterrence is particularly important.

With monitoring resources efficiently deployed, a tradeoff exists between enforcement- and noncompliance costs—the sum of which is minimized at the social optimum. Yet, achieving efficient (noncompliance-cost minimizing) deployment of monitoring resources is practically complex, and requires knowledge of: (i) how deployment affects actual, and perceived, detection probabilities; (ii) how perceived detection probabilities affect compliance; and (iii) potential heterogeneity in these effects across regulated entities.

Empirical evidence concerning these relationships is relatively sparse due to several inherent challenges. First, variation in monitoring frequency is often potentially endogenous to compliance. Moreover, perceptions of detection probability may vary across

[1] CDC estimate here; economic burden is estimated in 2013 USD.

similarly-monitored firms.[2] Finally—and most significantly—even with an exogenous detection-probability shock, cleanly separating that shock's *deterrence effect* from its opposing (and typically simultaneous) *detection effect*, is seldom feasible. For example, following an exogenous and perceived detection-probability increase, fewer violations will be committed (the *deterrence effect*), but a greater share of committed violations will be detected (the *detection effect*). Exploiting a regulator's pandemic-induced shift to remote inspections for *some* entities under their jurisdiction, I largely overcome these issues.

From the COVID-19 pandemic's outset, routine health inspections by the Maricopa County Environmental Services Department (MCESD) continued on-site at most permitted food establishments. In May 2020 however, among establishments serving, or located near, populations especially vulnerable to COVID infection (such as hospitals, nursing homes, and assisted living facilities) MCESD began conducting such inspections remotely by video-conference. These "virtual" inspections required advance scheduling with an establishment's person-in-charge, and were thus, easily anticipated. Advance notice of inspections undermines a fundamental aspect of enforcement *via* deterrence: the continual threat of detection and punishment. By knowing in advance when detection will occur, establishments treated with virtual inspections can avoid punishment by correcting violations just prior, and upon recognizing this, may also relax compliance effort. Moreover, the remote format likely inhibits inspector ability to observe some violations, *further* reducing their detection probability.

Using MCESD inspections spanning 2018 through 2022, I leverage this sudden format adjustment as a natural experiment, and test multiple facets of the imperfect-monitoring model. Concurrent on-site inspections at untreated establishments provide control for contemporaneous factors that may have affected compliance generally, and the sudden return of unannounced on-site inspections at treated establishments enables identification of a deterrence effect. In initial post-treatment on-site inspections, actual detection prob-

[2] Across several industries, an initial literature (Gray and Deily, 1996; Laplante and Rilstone, 1996; Eckert, 2004; Telle, 2009) estimates inspection propensity as a function of firm observables, and generally finds positive relationships between predicted probabilities (which proxy for firm perceptions) and compliance. Gray and Shimshack (2011) review the challenges of accounting for perceptions of regulatory stringency and monitoring intensity.

abilities return to pre-treatment levels (removing any detection effect), but compliance efforts are still based on virtual-regime perceptions.

Initially, I track a subset of five MCESD codes where—regardless of inspection mode—compliance is checked through formal tests.[3] Violations of these particular codes will isolate potential anticipation-enabled avoidance, because the virtual format doesn't inhibit their detection. Comparing against contemporaneous (same 14-day period) on-site inspections, and controlling for time-invariant establishment-specific differences, virtual inspections detect about 53% fewer of these "virtually demonstrable" violations. Consistent with opportunistic and short-lived corrections, this decrease reverses entirely in subsequent on-site inspections. Notably, the decrease is almost entirely evident in treated establishments' first virtual inspections, suggesting fairly immediate avoidance behavior.

While advance notice reduces detection probability on any violation capable of quick remedy, those five violations isolate anticipation's effect because, even in virtual inspections, they *will* be detected if not corrected prior. Conversely, violations detected by visually observing premises are presumably less likely to be caught by virtual inspections, even when left uncorrected. Thus, I then expand focus to violations of any MCESD code, and use the return of unannounced on-site inspections at treated establishments to assess how overall compliance responds to the detection-probability shock.

Detected counts of all violations are 39% lower in virtual inspections, relative to the pre-treatment average. However, in establishments' initial post-treatment on-site inspections—when their perceptions of detection probability are likely based on the virtual regime—that decrease is *more* than offset, yielding an estimated net increase that exceeds the pre-treatment average by 25%. Consistent with general deterrence, this suggests the detection-probability decrease caused greater noncompliance.

This quasi-experiment also permits examination of a common enforcement agency dilemma: should firms with strong compliance records receive fewer inspections so that severe violators can receive more? Among higher risk-class establishments that crossed particular noncompliance thresholds in 2018, estimated deterrence effects are especially

[3]These tests involve demonstration of an appropriate holding temperature with a thermometer, or sufficient sanitizer concentration in cleaning solutions with pH test strips.

large, suggesting an explicit policy requiring inspection-frequency increases at such establishments would likely improve compliance among the targeted group. Moreover, among lower risk-class establishments that had perfect compliance records in 2018, the estimated response to the detection-probability decrease is very small and not significantly different from zero. Redirecting some of this group's routine inspections to accommodate such a dynamic enforcement policy, might meaningfully improve overall compliance, and do so with existing agency resources.

My findings build on a nascent literature utilizing field and natural experiments to empirically test enforcement *via* imperfect monitoring. In Florida food-service health inspections, following adoption of handheld devices which reminded inspectors of potential violations, Jin and Lee (2014) find an immediate 11% increase in detected violations; subsequent inspections suggest modest compliance-effort improvements in response. Duflo et al. (2018) study an experimental doubling of environmental-inspection frequency at Indian factories. Treated plants perceive elevated scrutiny, and are more frequently cited for violations, but no effect on average emissions is found. Most closely related to this work, two recent studies draw identifying variation in detection probability from the ability of some entities to anticipate monitoring in advance.

Makofske (2021) examines Las Vegas facilities housing multiple food-service establishments. At such facilities, inspectors often conduct many inspections during one visit, and establishments inspected second or later likely anticipate those inspections in advance. The study finds that detected noncompliance, within establishment, is significantly higher when inspected first—an effect driven by violations capable of quick remedy, suggesting anticipation-enabled avoidance—but is unable to test deterrence.[4] Zou (2021) exploits every-sixth-day pollution monitoring under the Clean Air Act, which the US Environmental Protection Agency (EPA) allows at some monitor sites. Near intermittent sites, Zou (2021) finds satellite pollution measures are 1.6% lower during monitor on-days than off-days, and that air-quality advisories are more likely during on-days, suggesting strategic

[4]Using Los Angeles County health inspections, Makofske (2019) compares detected noncompliance within establishment, across days when receiving the sole inspection, or one of many inspections, at a facility. Significantly more violations are detected on sole-inspection days, when anticipation is less likely.

responses by local governments. Following the retirement of some intermittent monitors, Zou finds that pollution levels significantly increase on what would have been on-days, and change little otherwise, consistent with deterrence.

Makofske (2021) and Zou (2021) use variation in anticipation ability—within-entity and across-entity, respectively—that is due to established institutional features, and present throughout their samples. Here, firms with no prior anticipation ability acquire it from an abrupt and unforeseeable inspection-format change. The immediate response found here suggests practices which inadvertently enable anticipation, even if short-lived, can meaningfully undermine enforcement. Further, observations before and after the virtual regime enable comparisons across inspections where detection probabilities are similar, but perceived to be quite different. This yields an exceptionally clean test of deterrence, and also enables examination of potential policy improvements.

In the space remaining, I detail the MCESD inspection program and the virtual regime begun in 2020. Next, I review the data and estimating sample, explain the methodology employed, and test its underlying assumptions. I then present estimates of anticipation-enabled avoidance and general deterrence. Finally, I examine deterrence-effect heterogeneity, discuss policy implications, and conclude.

2 Background

2.1 Maricopa County Inspection Program

The Maricopa County Environmental Services Department (MCESD) regulates and inspects food service and retail food establishments whom—per the MCESD—receive "required unscheduled food safety inspections." MCESD issues 24 different food establishment permit types which, based on the nature of food and population typically served, are assigned risk classifications (from lowest risk to highest): *class 2*, *class 3*, *class 4*, and *class 5*.[5] Respectively, establishments in these classes are prescribed 2, 2, 3, and 4, annual routine inspections.

[5]There is a *class 1*, which applies only to Micromarket permits, none of which are in the primary estimating samples (see Section 3).

Inspections check health code compliance and violations are specified—from most to least severe—as *priority*, *priority foundation*, and *core*.[6] MCESD supplements inspections with ratings and disclosure. Inspection performances are graded: *A*, *B*, *C*, and *D*, according to the schedule here. A peculiarity of this grading policy is that disclosure participation is voluntary. Prior to every inspection, the establishment's person-in-charge chooses whether they will participate in the grading program for that inspection. If participation is elected, the grade—along with any cited violations—is shared on the county's restaurant ratings page; a grade card is also issued but display of the card is optional. If participation is declined, the inspection report with violations are posted online with "Not Participating" in place of a letter grade. The election is made before the inspection starts, and irreversible.

All inspection reports are published by Maricopa County in a searchable online database. For each establishment, an initial page provides the cited number of priority violations and hyperlinks to reports of all inspections from the last three years, regardless of grading participation. Inspection results are also incorporated into the consumer-review platform, Yelp. An establishment's Yelp profile (e.g., here) shows their most recent inspection's letter grade or "Not Participating" in the "Amenities and More" section, and a "Health Score" hyperlink leads to a list of *all* recent inspections with violation counts and descriptions.[7]

Detected violations carry other potential costs as well. MCESD inspectors have authority to suspend or revoke operating permits. Following routine inspections, failure to correct any noted violation within the time limit given is cause for suspension of the permit.[8] With priority and priority-foundation violations, if not immediately correctable, a re-inspection within 10 days to verify correction is required. Further, repeating the same priority violation in consecutive inspections requires an additional "Active Managerial Control Intervention plan" visit at the establishment, and a future priority violation of

[6]Severity levels are not specific to the health codes; i.e., a particular health code can be violated to each severity level.

[7]In Louisville, KY, where mandatory on-site disclosure was already in place, Makofske (2020a) finds that posting compliance scores on Yelp caused substantial improvements among independent restaurants.

[8]See Chapter 8.1 of the Maricopa County Environmental Health Code.

that particular code may result in permit suspension.

2.2 COVID-19 Pandemic and Virtual Inspections

On March 19, 2020, Arizona Governor Doug Ducey issued an executive order restricting restaurants in counties with confirmed cases of COVID-19 to offer food for dine-out only. On May 4, 2020, he issued executive orders providing guidance on re-opening of businesses during the COVID-19 pandemic, and allowing resumption of in-person dining on May 11.[9] In a May 7, 2020 press conference, MCESD Director Darcy Kober explained that, throughout the pandemic, MCESD had continued conducting on-site inspection visits, as many establishments were providing dine-out service.[10] During that time, MCESD recorded many "ineffective visits", where visited establishments were found to be temporarily closed.

On May 20, 2020, MCESD began conducting what it called "virtual inspections" at establishments serving, or located near, populations highly vulnerable to COVID-19, such as nursing homes, assisted living facilities, and hospitals. Specifics of the virtual inspection program are detailed in an award application submitted by MCESD. Per that application, virtual inspections were pre-scheduled and establishments were instructed they would need a thermometer and flashlight. Establishments were required to demonstrate appropriate holding temperatures for potentially hazardous foods, and sanitizer concentration for cleaning solutions with pH test strips (which MCESD code requires establishments have at all times), checks normally conducted by inspectors.

3 Data

For all permitted food establishments, Maricopa County's website maintains a list of hyperlinks to inspection-result pages, which contain dates and hyperlinks to reports, for all inspections conducted within the last 3 years. Establishment-page and inspection-result links were first collected on June 5, 2022. For inspections prior to June 5, 2019, I col-

[9] See here.

[10] Video of the press conference is available here.

lect report hyperlinks from separately published weekly inspection summaries. An initial round of collection yielded inspections up to August 2, 2022. Following a subsequent round, data are collected for all routine inspections spanning January 2, 2018 through December 23, 2022.

From inspection reports I collect the health codes and information provided on all cited violations, and all text in the "Inspection Comments" section. In those comments, virtual inspections are typically tagged: "VIRTUAL INSPECTION – COVID-19".[11] In total, 3,489 inspections are tagged as virtual.

My interest lies with establishments whose inspections were immediately shifted to the remote format at its introduction.[12] As such, establishments are considered treated if their first two inspections following May 19, 2020, are virtual. I further restrict attention to those observed in at least two inspections before May 20, 2020. Among these treated establishments, there are 112 inspections lacking the virtual tag, despite the establishment's prior and next inspections being tagged as virtual, suggesting potential misclassification. However, Appendix Figure A1 shows the distribution of these inspections by their month of sample, and 103 occur after April 2021, when COVID-19 vaccines had been widely available in Maricopa County.[13] Also, the frequency of these inspections declines beginning in November 2021, coincident with rising delta-variant infections. Of these untagged inspections, the 105 after 2020 are presumed correctly classified. Yet, the 7 in 2020 are very likely virtual inspections that were erroneously not tagged; I code these as virtual.

Within each MCESD permit type, Appendix Table A1 summarizes the frequency of treated establishments (as defined above), and all untreated establishments observed in at least two inspections before, and two inspections after, May 19, 2020. I exclude 347 establishments (all untreated) that went an entire calendar year without an inspection, presumably due to temporary closure. My primary estimation sample consists of obser-

[11] See, e.g., here. Naturally—as all inspections prior to May 20, 2020 were conducted in person—inspection reports don't explicitly indicate on-site visits.

[12] There are a small number of establishments that, despite primarily receiving on-site inspections, received a single virtual inspection during this time. Ultimately, 53 such establishments are excluded from all analyses.

[13] See https://www.maricopa.gov/5671/Public-Vaccine-Data.

vations from: all such treated establishments, all untreated establishments with the same permit type as a sampled treated establishment, and excludes observations from any establishment with a *Daycare Food Service*, *Food Bank*, *Food Processor*, or *Micromarket* permit, as each category contains one anomalous treated establishment.

As mentioned above, the return of on-site visits was partly interrupted beginning in late 2021, hence the coincident rise in virtual inspection frequency. Among treated establishments, Appendix Figure A2 plots the frequency of virtual inspections within 14-day bins. From May through October of 2022, virtual inspections appear largely discontinued, yet their frequency rises again in late November 2022. As the reasons for that latest surge are unclear, I exclude all virtual inspections after September 8, 2022 from estimation. The primary sample consists of 13,660 untreated, and 619 treated establishments—593 are observed in at least one post-treatment on-site inspection.

4 Methodology

A total of 52 different MCESD code violations are cited within the data, all of which presumably carry lower detection probability in virtual inspections. These detection-probability decreases have two potential sources. First, *inspection anticipation* enables avoidance—committed violations that would have been detected by an unannounced inspection, can be corrected between the scheduling and start of a virtual inspection. Second, detection of some violations may be subject to *format limitations*—inspector difficulty observing certain violations when not physically present. Initially, I seek to isolate changes in detected compliance attributable only to inspection anticipation.

To isolate an effect of anticipation, I track a subset of regulations: (i) "food-contact surfaces: cleaned and sanitized", (ii) "proper cold holding temperatures", (iii) "proper cooling methods used, adequate equipment for temperature control", (iv) "proper cooling time and temperatures", and (v) "proper hot holding temperatures". As in on-site inspections, compliance with these regulations must be demonstrated during virtual inspections *via* thermometer and test-strip readings. As such, the remote format should not inhibit detection of these "virtually demonstrable" violations.

I estimate

$$y^d_{i,j} = \alpha_1 \left[(1 - \mathit{Virtual}_{i,j}) \times \mathit{Post}_{i,j}\right] + \alpha_2 \, \mathit{Virtual}_{i,j} + \mathbf{X}^{'}_{i,j}\boldsymbol{\omega} + a_i + \epsilon_{i,j}, \tag{1}$$

where $y^d_{i,j}$ is the count of virtually demonstrable violations detected in inspection j of establishment i. $\mathit{Virtual}_{i,j}$ indicates that an inspection was virtual, and a_i is an establishment fixed effect. $\mathit{Post}_{i,j}$ equals one if inspection j of establishment i occurs on or after the date of their first virtual inspection, and 0 otherwise. In the primary sample, there are 1,925 on-site inspections of treated establishments, that occur after the establishment has received virtual inspections. In such inspections, $[(1 - \mathit{Virtual}_{i,j}) \times \mathit{Post}_{i,j}] = 1$, which prevents $\widehat{\alpha}_2$ from reflecting comparisons against post-treatment on-site inspections. In the full specification, vector $\mathbf{X}_{i,j}$ contains fixed effects for an inspection's day of week, month of year, and 14-day period of the sample.

In estimating α_2, observably similar and contemporaneous on-site inspections provide a counterfactual estimate for virtual inspections. This counterfactual is valid if, absent the virtual-inspection regime, treated and untreated establishments would have exhibited a common trend in $y^d_{i,j}$ following May 19, 2020. To gauge the plausibility of that assumption, I test whether the two groups exhibit common trends prior to the virtual-inspection period. Using inspections before May 20, 2020, I estimate

$$y^d_{i,j} = \gamma_1 \left(\mathit{Treated}_i \times \mathit{Trend}_{i,j}\right) + \gamma_2 \, \mathit{Trend}_{i,j} + \gamma_3 \, \mathit{Treated}_i + \mathbf{X}^{'}_{i,j}\boldsymbol{\omega} + c_i + \epsilon_{i,j}. \tag{2}$$

$\mathit{Trend}_{i,j}$ is an inspection's month of the sample, and $\mathit{Treated}_i$ indicates that i is a treated establishment. Under common trends prior to the virtual-inspection period, $\gamma_1 = 0$.

Table 1 reports these estimates. In column (1), the vector of controls is empty. In column (2), fixed effects for day-of-week, 14-day period, and establishment are included. Both specifications estimate a very small difference in pre-period trends, with fairly precise null effects—in column (2), the 99-percent confidence interval on $\widehat{\gamma}_1$ is $[-0.004, 0.004]$. Columns (3) and (4) report analogous estimates using the detected count of all violations, $y_{i,j}$, as the dependent variable. In columns (5) and (6), the dependent variable is a severity-adjusted count of all violations, $y_{i,j}^a$, in which each core violation adds only 0.25.[14] Appendix Table A2 reports these same estimates using quarterly trends, and findings are very similar.

To visualize the trend comparison, Figure 1 presents simple quarter-year averages of $y_{i,j}^d$ among untreated establishments (powder-blue diamonds), on-site inspections of treated establishments (solid red circles), and virtual inspections of treated establishments (hollow red circles). Prediction lines for each group are from the simple quarterly-trend estimates reported in column (1) of Table A2. Averages for both groups track closely prior to the virtual inspection period, after which there is a sharp drop among treated establishments, but *only* in virtual inspections; when on-site inspections resume, average y^d returns the levels predicted by their simple pre-period trend.

5 Results

5.1 Anticipation Ability and Detection Avoidance

Columns (1) and (2) of Table 2 report estimates of equation (1). Standard errors, clustered multi-way on establishment and 14-day period, are reported in parentheses. In column (1), 14-day-period fixed effects and the indicator, $Treated_i$, are the only controls; column (2) reports estimates under the full specification. Across both specifications, the estimated effect of anticipation on detected noncompliance is substantial. Among treated establishments, pre-treatment on-site inspections detect 0.269 demonstrable violations on average. Relative to that level, the full specification in column (2) estimates a 53% decrease in virtual inspections.

Because virtual inspections were implemented based on service of, or proximity to, populations especially vulnerable to COVID, a potential concern is that the pandemic may uniquely affect the treated establishments. For instance, rather than detection avoidance, $\widehat{\alpha}_2$ may reflect actual compliance improvements made in response to the pandemic.

[14] The inspection grade becomes B given one priority violation, one priority foundation violation, or four core violations, hence the weights of 1, 1, and 0.25.

However, in subsequent unannounced on-site visits, there is no evidence to suggest actual improvements. The estimated difference in detected y^d between pre- and post-treatment on-site inspections, $\widehat{\alpha}_1$ is relatively small and statistically insignificant.

Moreover, between March 9 and April 9, 2020, I observe 116 on-site inspections of treated establishments already exposed to the pandemic in its early stages. Columns (3) and (4) of Table 2 report estimates analogous to columns (1) and (2), but include an interaction $(\mathit{Treated}_i \times \mathit{COVID}_{i,j})$, where $\mathit{COVID}_{i,j}$ is a binary variable equaling 1 on and after March 9, 2020. The changes estimated in pre-treatment pandemic-period inspections are very small and statistically insignificant. In column (4), $\widehat{\alpha}_2$ is very similar to the estimate in column (2), and represents a 51.9% decrease relative to the pre-treatment average.

Recall that $y^d_{i,j}$ tracks a subset of violations that are verifiably tested for in virtual inspections, meaning format limitations on detection ability are not likely driving these findings. Further, the 14-day-period fixed effects likely account for any general changes in compliance driven by pandemic-related measures. Yet, a remaining alternative explanation of $\widehat{\alpha}_2$ is that virtual inspections, because they assign a more active role to an establishment's person-in-charge, were educational and thereby caused hygiene improvements. The award application referenced in Section 2.2 suggests MCESD had hoped for this.[15] If $\widehat{\alpha}_2$ reflects an educational effect of treatment, then that effect should persist to some extent in subsequent on-site inspections (which estimates of α_1 contradict), and can only manifest *after* an establishment receives a virtual inspection.

To assess whether the effect estimated by $\widehat{\alpha}_2$ materializes after establishments' first virtual inspections, I estimate

$$\begin{aligned} y^d_{i,j} = {} & \beta_1 \left[(1 - \mathit{Virtual}_{i,j}) \times \mathit{Post}_{i,j}\right] + \beta_2\, \mathit{Virtual}_{i,j} \\ & + \beta_3 \left(\mathit{Virtual}_{i,j} \times \mathit{Post}_{i,j-1}\right) + \mathbf{X}'_{i,j}\boldsymbol{\omega} + a_i + \epsilon_{i,j}, \end{aligned} \tag{3}$$

[15]From that document: "An unexpected bonus of the virtual inspections has been the PIC being put in an active, hands-on role and learning from this. For example, the PIC must calibrate the food thermometer, verify the temperature of foods in hot-holding and/or cold-holding tables, open containers in the walk-in refrigerator and verify cold-holding temperatures, etc."

where $Post_{i,j-1}$ is a one-inspection lag of $Post$. The interaction, $(Virtual_{i,j} \times Post_{i,j-1})$, equals 1 in all virtual inspections that come after an establishment's first virtual inspection. If the effect estimated by equation (1) reflects better hygiene practices learned through virtual inspections, $\beta_2 = 0$.

Column (5) of Table 2 reports estimates of equation (3). The estimated decrease in establishments' first virtual inspections ($\widehat{\beta}_2$) is substantial, and accounts for about 94.1% of the effect estimated among all virtual inspections in column (2). As an additional test, column (6) reports estimates of equation (1) under a sample that ends following: treated establishments' first virtual inspections, or untreated establishments' first inspections after May 19, 2020. This also estimates an effect very similar to column (2), further challenging the plausibility that any educational effect is embedded in α_2 estimates.

Recall that establishments irreversibly chose whether or not to participate in grade disclosure at the start of each inspection. Of the establishments in the primary sample: 1,256 (about 8.8%) never participate; 4,373 (about 30.6%) always participate; and the remainder chose each option at least once.[16] To assess whether participation decisions change with virtual inspections in a manner consistent with avoidance behavior, Table 3 reports estimates of equation (1) using $Disc_{i,j}$—a binary variable indicating that establishment i chose disclosure participation in inspection j—as the outcome.[17]

Columns (1) and (2) report estimates using the full primary sample. Consistent with opportunistic use of anticipation ability, the full-specification in column (2) estimates a 6.6% increase in disclosure participation (relative to a pre-treatment average of 0.801) in virtual inspections. In the full sample, a considerable amount of variation in $Disc_{i,j}$ is absorbed by establishment fixed effects. Column (3) restricts the estimating sample to establishments where $Disc_{i,j} = 0$ is observed at least once prior to May 20, 2020; column (4) restricts the sample to establishments where $Disc_{i,j} = 1$ is observed at least once prior to May 20, 2020; and column (5) restricts the sample to establishments for which both

[16] For comparison, from the grade program's introduction in 2011, through 2013, Bederson et al. (2018) find that only 58% of establishments ever participate.

[17] Appendix Table A3 reports estimates of equation (2) using $Disc_{i,j}$ as the dependent variable. All estimates fail to reject a null hypothesis of common pre-period trends in $Disc_{i,j}$ between treated and untreated establishments.

disclosure choices are observed prior to May 20, 2020. Notably, among establishments exhibiting pre-period variation in $Disc_{i,j}$, column (5) estimates a 26.9% increase in disclosure propensity in virtual inspections, relative to a pre-treatment average of 0.605.

While the primary comparison group consists of untreated establishments with the same permit type as a treated establishment, estimates are robust to an expanded comparison group. Appendix Table A4 reports estimates analogous to Table 2, but with the comparison group expanded to include any permit type. Results are very similar.

5.2 Testing Deterrence

The introduction of virtual inspections causes a sharp drop in detection probability at treated establishments. Deterrence theory suggests that treated establishments—conditional on recognizing this and expecting its continuation—will relax compliance effort and commit more violations on average. In initial post-treatment on-site inspections, while treated establishments' compliance efforts likely reflect virtual-regime perceptions, actual detection probabilities return to the pre-treatment levels, thereby removing the detection effect and isolating a deterrence effect.

In assessing this compliance response, I use an inspection's detected count of all violations, $y_{i,j}$, as well as the severity-adjusted count of all violations, $y^a_{i,j}$ (described in Section 4). Virtual inspections likely lowered detection probabilities for all health-code violations, hence the shift to these broader outcomes. Columns (3), (4), (5), and (6) of Table 1, suggest very similar pre-period trends in $y_{i,j}$ and $y^a_{i,j}$, between treated and untreated establishments.

I test deterrence by estimating equation (1) with y and y^a as dependent variables. Any inspections of treated establishments after their initial post-treatment on-site visits are excluded in estimation, as are all observations from treated establishments not observed in a post-treatment on-site inspection, and any untreated establishments observed in fewer than three inspections following May 19, 2020.[18]

The coefficient of interest, $\widehat{\alpha}_1$, estimates the difference in conditional expectation of y

[18]Treated establishments in this sample are also observed in three or more inspections after May 19, 2020 (at least two virtual, and one post-treatment on-site).

(or y^a) between treated establishments' pre-treatment, and initial post-treatment, on-site inspections. If treated establishments don't respond to the lower detection probability—or do respond, but anticipate the return of on-site visits and adjust back—then $\alpha_1 = 0$. Alternatively, if they respond in a manner consistent with general deterrence, and are caught unawares by the return of on-site inspections, $\alpha_1 > 0$.

Table 4 report these estimates. The count of detected violations is the outcome for columns (1), (2), and (3); severity-adjusted violation count is the outcome for columns (4), (5), and (6). As expected, detected noncompliance is substantially lower in virtual inspections. Under the full specification in columns (2) and (5), detected y and y^a are 38.5% and 38.1% lower in virtual inspections, relative to pre-treatment averages of 0.659 and 0.498, respectively. More importantly, those decreases are more than offset by the return of unannounced on-site visits, consistent with general deterrence. In columns (2) and (5), establishments' initial post-treatment on-site inspections detect y and y^a exceeding their pre-treatment averages by 25.2% and 14.5%, respectively. In columns (3) and (6), virtual inspections are excluded from the estimating sample as they are not ultimately needed to estimate the parameter of interest. With both outcome variables, the sample of on-site inspections only, yields slightly larger deterrence-effect estimates.

The return to on-site inspections was not announced, hence the assumption that establishments' compliance efforts in these inspections reflected virtual-regime perceptions of detection probabilities. Yet, to the extent that some establishments might have expected their return to on-site visits, this will, if anything, attenuate $\widehat{\alpha}_1$. If so, I provide a potential correction by excluding virtual inspections and estimating

$$y^a_{i,j} = \theta_1 \left[(1 - Disc_{i,j}) \times Post_{i,j}\right] + \theta_2 Post_{i,j} + \mathbf{X}'_{i,j}\boldsymbol{\omega} + a_i + \epsilon_{i,j}, \tag{4}$$

which separates the deterrence effect based on establishments' disclosure decisions in their initial post-treatment on-site inspections. Being caught off-guard by the return unscheduled on-site visits should, if anything, reduce participation propensity.

Columns (1) and (2) of Table 5, report estimates of equation (4), as specified above. In initial post-treatment on-site inspections, y^a (which adjusts $y_{i,j}$ according to the grading scheme) among disclosure participants modestly exceeds counterfactual predictions. However, among the 88 establishments that declined disclosure in their initial post-treatment on-site inspection, the difference is considerably larger. In column (2), the sum of $\widehat{\theta}_1$ and $\widehat{\theta}_2$ is 0.287 (standard error 0.103), which represents a 39.4% increase in y^{a} relative to their pre-treatment average of 0.731.

These individual disclosure decisions provide a useful, but also limited, proxy. In this estimating sample, 28 (4.7%) of the treated establishments never participate in disclosure, but 25% of these establishments have perfect (no violations detected) compliance records, and 42.9% have compliance sufficient for A grades in every inspection. I.e., some exceptionally compliant establishments never disclose, perhaps as countersignal (see Bederson et al., 2018), or concern that a current disclosure will commit them to future disclosure when expected performance is more uncertain (see e.g., Grubb, 2011). Similarly, some establishments caught off-guard might always disclose due to company policy, or a sense of commitment from prior participation.

Estimates in columns (3) and (4) of Table 5 replace $(1 - Disc_{i,j})$ with $SwitchOff_{i,j}$, a binary variable indicating that establishment i participated in disclosure in their prior inspection, but switched to non-participation in the current inspection (i.e., $Disc_{i,j} = 0$ and $Disc_{i,j-1} = 1$). At decision time, $Disc_{i,j}$ reflects whether or not the establishment's expected inspection outcome meets their own threshold for participation, both of which are unobservable. Yet, expected performance—being inspection-specific—is presumably more time-variant than the establishment's participation threshold. As such, a switch from participation in the prior inspection, to non-participation in the current inspection, likely reflects expected outcomes lying on opposite sides of an unchanged threshold.

The interaction term, $SwitchOff_{i,j} \times Post_{i,j}$, isolates treated establishments that declined disclosure in their first post-treatment on-site inspection, after having participated in disclosure in their virtual inspection immediately prior. The deterrence effect estimated among this group, reported in columns (3) and (4) of 5, is substantial. In column (4), with all controls included, the sum of $\widehat{\theta}_1$ and $\widehat{\theta}_2$ is 0.625 (standard error 0.248), which represents a 74.5% increase in y^{a} relative to their pre-treatment average of 0.839. The correlations between disclosure decisions and $\widehat{\theta}_2$ evident in Table 5 also further support deterrence as explaining the broader findings reported in Table 4.

6 Policy Implications

Dynamic enforcement policies prescribe expected-cost increases for regulated entities that cross specified noncompliance thresholds, and thereby enhance deterrence—the threat of greater penalties or scrutiny in the future, further deters violations at present. Blundell (2020) and Blundell et al. (2020) examine EPA enforcement of air quality regulation, where current noncompliance raises future fines. Notably, Blundell et al. (2020) estimate that this dynamic enforcement scheme reduces pollution substantially. Given the quasi-experimental shock to detection probability available here, rather than prescribed penalties, I assess the feasibility of raising inspection frequency—and thus, detection probability—conditional on detection of noncompliance beyond some threshold.

In this setting, uniform annual deployment of inspections across establishments would presumably be inefficient. Establishments differ in their propensities for noncompliance, the potential costs posed by their noncompliance, and the sensitivity of their compliance effort to detection probability. While MCESD's allocation of annual inspections by risk classification accounts for differences along the first two dimensions, this natural experiment enables assessments along the third. If inspection costs are similar across establishments, then redirecting an inspection—away from an original designee, and toward a targeted establishment—is an improvement so long as it causes noncompliance costs to decrease at the target by more than they increase at the original designee. Examining heterogeneous responses to the virtual regime's detection-probability shock, I evaluate whether a dynamic enforcement policy might be leveraged toward such improvements, given existing agency resources.

Consider a rule explicitly applied to a set of establishments ($\mathbb{S}$), where $y_{i,j}^a \geq Z$ triggers one additional inspection over a specified time frame. Such a rule's efficacy is facilitated if: (1) some establishments are highly compliant for reasons outside the inspection program, and (2) compliance efforts among the relatively less compliant establishments in $\mathbb{S}$ are responsive to expected costs. Condition (1) provides establishments whose inspections can be redirected at little noncompliance cost as needed. Condition (2) suggests the rule will deter violations, because the establishments most likely to trigger additional scrutiny are responsive to the threat of it. Optimally, the noncompliance cost deterred by the rule, minus the noncompliance cost caused by the requisite redirected inspections, is maximized. *Cet. par.*, expanding $\mathbb{S}$ or reducing Z, will deter more noncompliance, but also require redirection of more inspections.[19]

The sample used to test deterrence contains 10,134 establishments (434 treated and 9,700 untreated) observed in all prescribed inspections for 2018. Using them, I evaluate potential threshold values by examining correlation between 2018 inspection outcomes and deterrence-effect estimates. Of these establishments, 6,677 (176 treated and 6,501 untreated) are prescribed 2 inspections per year, and 3,457 (258 treated and 3,199 untreated) are higher risk-class establishments requiring 3 or 4 inspections per year. As their noncompliance likely poses greater social costs, *cet. par.*, suppose initially that $\mathbb{S}$ consists of higher risk-class establishments only.

Figure A4 summarizes each higher risk-class treated establishment's maximum $y^{\,a}$ in 2018, denoted $\max_{i,2018}(y^{\,a})$. For $Z \in \{0.25, 0.5, 1, 1.25, 1.5, 2, 2.25, 2.5, 2.75, 3\}$, I divide establishments on whether or not $\max_{i,2018}(y^{\,a}) \geq Z$, and separately estimate equation (1) within each group, including all controls, with $y^{a}_{i,j}$ as the outcome variable, and using only post-2018 on-site inspections. Figure 2 presents $\widehat{\alpha}_1$ and 95% confidence intervals from these regressions. Red dots (blue diamonds) correspond to establishments that did (did not) have $y^{a}_{i,j} \geq Z$ in 2018.[20]

Among establishments with $y^{a}_{i,j} \geq Z$ in 2018, deterrence effect estimates are increasing in Z up to 2.5. Relative to their post-2018 pre-treatment averages, $\widehat{\alpha}_1$ represents 31.4%, 44.7%, and 46.4% increases for establishments with $\max_{i,2018}(y^{\,a})$ greater than

[19] When triggered, the added inspection may generate specific deterrence (as in Makofske, 2020b). While this could offset costs from additional redirection, it wouldn't remove that tradeoff.

[20] Appendix Table A5 reports the corresponding α_1 estimates, standard errors, and sample sizes. Table A6 reports corresponding equation (2) estimates; within each grouping, estimated differences in pre-period trends are very small and statistically insignificant.

or equal to 2, 2.25, and 2.5, respectively. Moreover, the responses estimated within this group are quite large relative to the full-sample estimate. For instance, $\widehat{\alpha}_1$ among those with $\max_{i,2018}(y^a) \geq 2$, is 0.259, nearly 3 times as large as the full-sample estimate of 0.089.[21] These findings support such a rule's ability to further, and meaningfully, deter noncompliance, as the establishments most susceptible to triggering additional scrutiny are also responsive to these expected costs. Figure 3 presents analogous estimates generated among lower risk-class establishments. These establishments are less prone to severe noncompliance in 2018, and deterrence effects estimates suggest little benefit to their inclusion in $\mathbb{S}$.

For treated establishments with perfect observed compliance in 2018, $\widehat{\alpha}_1$ is small and statistically insignificant among both the higher and lower risk-class groups. This suggests the existence of highly compliant establishments where compliance effort is relatively insensitive to the inspection program's expected costs. Among all lower risk-class establishments, 2,785 had $y^a_{i,j} = 0$ in both 2018 inspections, and 3,327 had $y^a_{i,j} = 0$ in both 2019 inspections. Among higher risk-class establishments: $y^a_{i,j} \geq 2.5$ occurred 2,510 times in 2018, and 2,479 times in 2019; whereas $y^a_{i,j} \geq 2.25$ occurred 3,123 times in 2018, and 3,099 times in 2019. Redirecting from lower risk-class establishments with $y^a_{i,j} = 0$ in their two most recent inspections, appears sufficient to at least target higher risk-class establishments where $y^a_{i,j} \geq 2.5$ with an additional inspection over the next year.

7 Concluding Remarks

General deterrence through imperfect monitoring is essential to enforcing a profound body of regulation. Yet deterrence is, by nature, difficult to empirically evaluate. Exploiting MCESD's temporary adoption of virtual compliance inspections among some establishments, I largely overcome the typical obstacles.

I find that establishments exploit inspection anticipation to avoid detection of noncompliance. This contributes to recent work (Makofske, 2019, 2021; Zou, 2021) demonstrating the detrimental effect of anticipation ability on monitoring programs. Here, anticipation

[21]See column (6) of Table 4.

ability does not stem from long-standing practices. Rather, establishments with no prior history of anticipation ability suddenly acquire it. I find that avoidance behavior is immediate, suggesting that even sporadic anticipation ability can undermine enforcement.

Compliance efforts respond to perceived detection probabilities in a manner consistent with general deterrence. In establishments' initial post-treatment on-site inspections, detected violations exceed pre-treatment levels by 25%. Deterrence effect heterogeneity supports a dynamic policy of targeting higher risk-class establishments with additional inspections following severe noncompliance. Lower risk-class establishments that were highly compliant in pre-treatment inspections are also unresponsive to the detection probability shock; redirecting some inspections away from this group could support such a policy and improve overall compliance with existing agency resources.

Finally, note that MCESD was hardly alone in adopting virtual inspections; many agencies utilized the remote format during the COVID-19 pandemic, and some did so for all food establishments in their jurisdictions.[22] This point is particularly important because presently—as with other activities that migrated to remote format during the pandemic—debate exists over whether virtual food-safety inspections should continue in some capacity.[23] While no doubt less costly, my results demonstrate that in this regulatory setting—or any where compliance status can change in the time between a virtual inspection's start and its requisite advance scheduling—remote inspections are a remarkably poor substitute for unannounced on-site visits.

[22]See `https://www.astho.org/topic/brief/virtual-food-safety-inspections-during-the-covid-19-pandemic/`.

[23]See, e.g., here or here.

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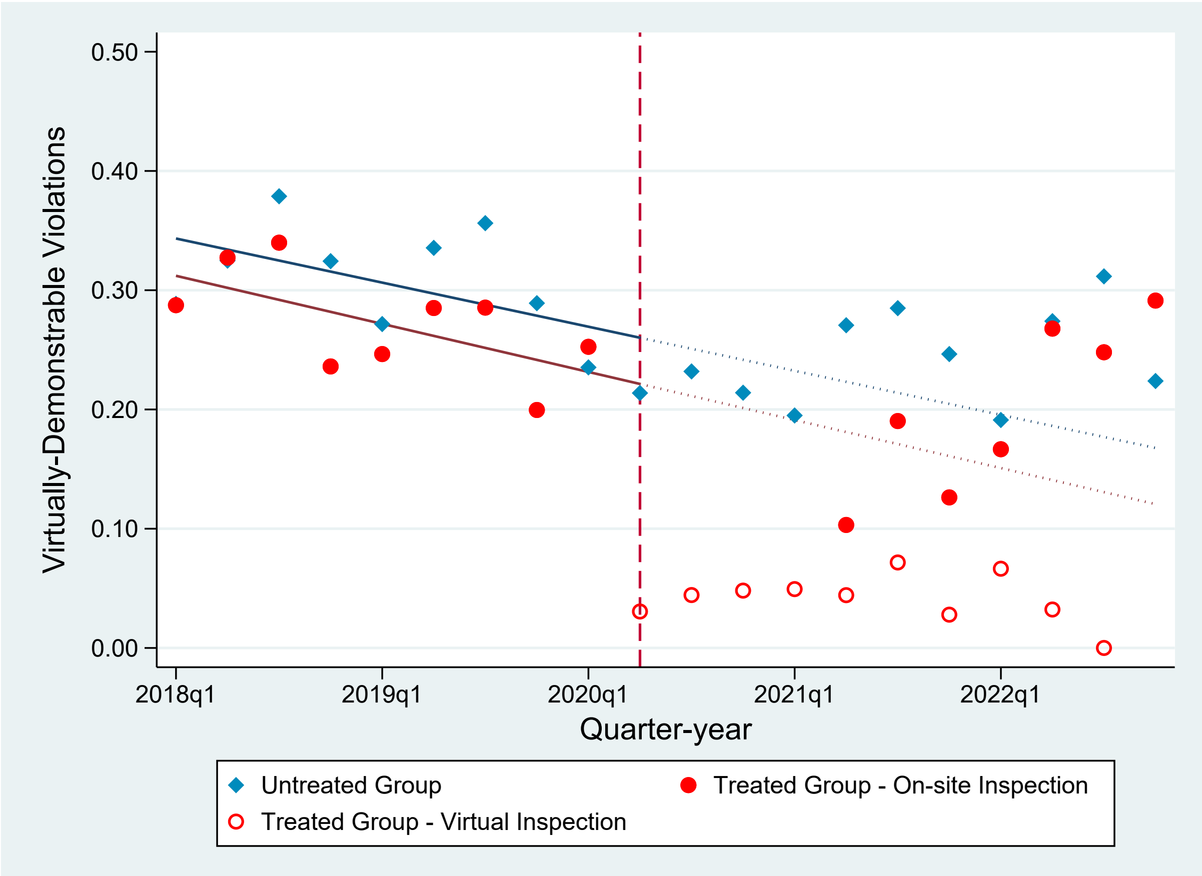
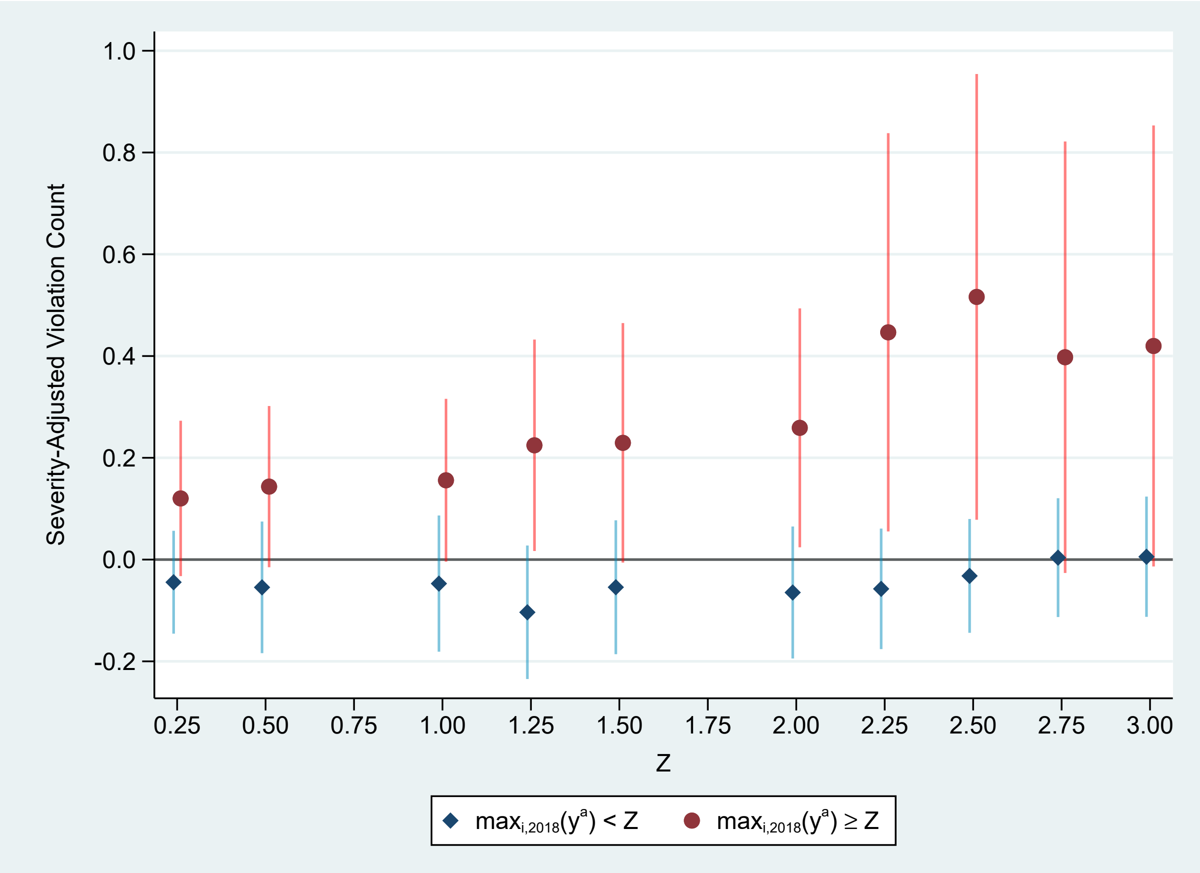
Figure 1: Inspection Format and Detected Violations



Average $y^d_{i,j}$ by quarter-year of sample. The "treated group" are establishments observed in at least 2 inspections before May 20, 2020, and that received at least 2 consecutive virtual inspections thereafter. The "untreated group" are establishments with the same permit type as a treated establishment that: never received a virtual inspection, and are observed in at 2 inspections before, and at least 2 inspections on or after, May 20, 2020. Prediction lines (navy for untreated, maroon for treated) are simple quarterly trend estimates from observations before May 20, 2020. Treated group averages from on-site inspections are suppressed for 2020q2 and 2021q1, due to few observations—27 and 9, respectively, compared to 126 and 289 such inspections in 2021q2 and 2021q3.

Figure 2: Deterrence Effect Estimates: Higher Risk-Class Establishments

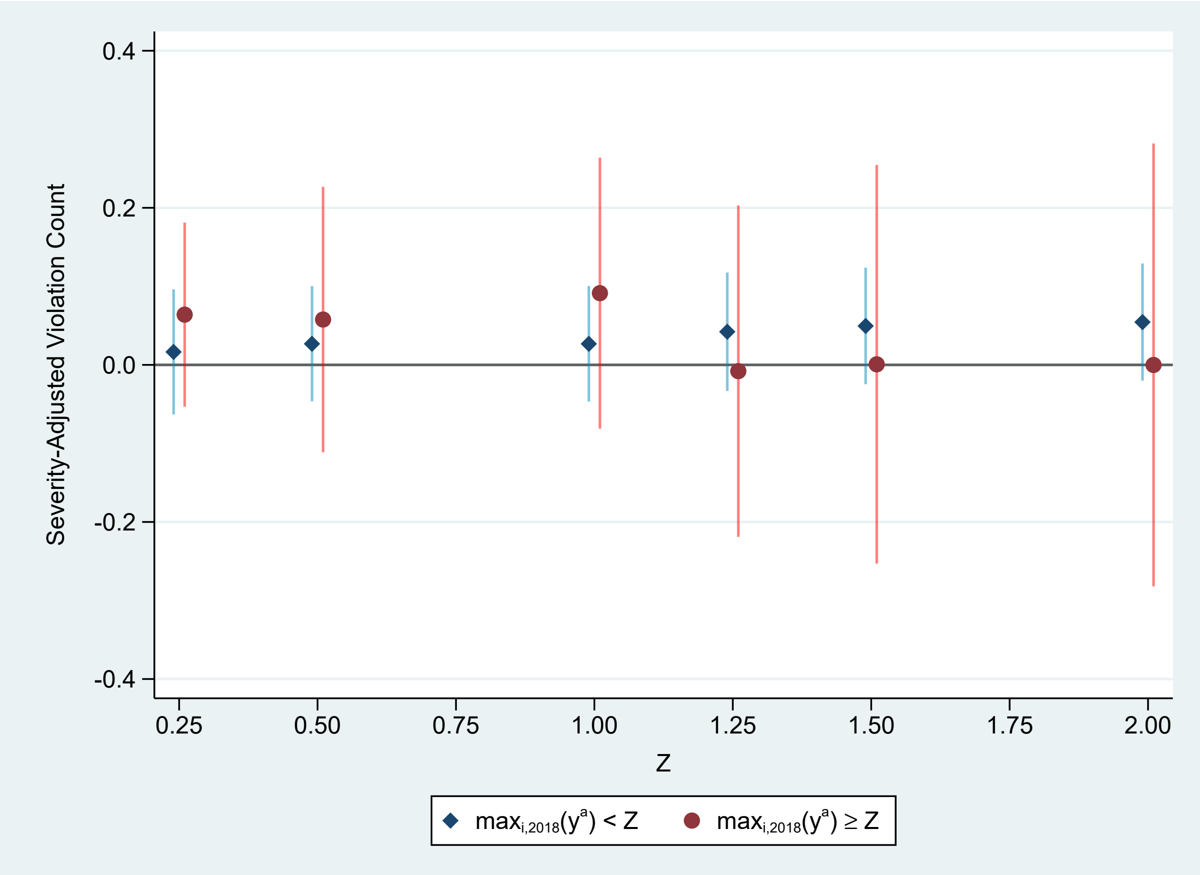


Coefficients from OLS estimates of equation (1) with: y^a as the outcome, all controls included, and only higher risk-class establishments fully observed in 2018.

Estimating samples: All post-2018 inspections for untreated establishments, and all post-2018 inspections prior to, and including, initial post-treatment on-site inspections for treated establishments.

$\max_{i,2018}(y^a)$ denotes establishment i's maximum observed y^a in 2018. Blue diamonds and bands mark $\widehat{\alpha}_1$ and 95% confidence intervals, among establishments with $\max_{i,2018}(y^a) < Z$. Red circles and bands mark $\widehat{\alpha}_1$ and 95% confidence intervals, among establishments with establishments with $\max_{i,2018}(y^a) \geq Z$. Standard errors for confidence intervals are clustered two-way on establishment and 14-day period.

Figure 3: Deterrence Effect Estimates: Lower Risk Class Establishments



Coefficients from OLS estimates of equation (1) with: $y^{\,a}$ as the outcome, all controls included, and only lower risk-class establishments fully observed in 2018.

Estimating samples: All post-2018 inspections for untreated establishments, and all post-2018 inspections prior to, and including, initial post-treatment on-site inspections for treated establishments.

$\max_{i,2018}(y^a)$ denotes establishment i's maximum observed $y^{\,a}$ in 2018. Blue diamonds and bands mark $\widehat{\alpha}_1$ and 95% confidence intervals, among establishments with $\max_{i,2018}(y^a) < Z$. Red circles and bands mark $\widehat{\alpha}_1$ and 95% confidence intervals, among establishments with establishments with $\max_{i,2018}(y^a) \geq Z$. Standard errors for confidence intervals are clustered two-way on establishment and 14-day period.

Table 1: Assessing Pre-period Trends

Variable	(1) $y_{i,j}^d$	(2) $y_{i,j}^d$	(3) $y_{i,j}$	(4) $y_{i,j}$	(5) $y_{i,j}^a$	(6) $y_{i,j}^a$
Trend × *Treated*	0.000	-0.000	0.004	0.002	0.002	0.000
	(0.002)	(0.001)	(0.003)	(0.003)	(0.003)	(0.002)
Trend	-0.003***	0.018*	-0.010***	0.042**	-0.008***	0.036***
	(0.001)	(0.009)	(0.002)	(0.017)	(0.002)	(0.014)
Treated	-0.035		-0.419***		-0.246***	
	(0.029)		(0.062)		(0.048)	
14-day period FE		✓		✓		✓
Establishment FE		✓		✓		✓
Day-of-week FE		✓		✓		✓
R-squared	0.002	0.368	0.006	0.501	0.005	0.470
N	74,134	74,134	71,249	71,249	71,249	71,249

***$p < 0.01$, **$p < 0.05$,*$p < 0.1$

OLS estimates of equation (2) from inspections prior to May 20, 2020. Standard errors, clustered two-way on establishment and 14-day period, are reported in parentheses. $y_{i,j}^d$ is an inspection's detected count of demonstrable violations. $y_{i,j}$ is an inspection's detected count of all violations. $y_{i,j}^a$ is a severity-adjusted count of all violations in which each core violation adds only 0.25. *Trend* is the month of sample.

Columns (3), (4), (5), and (6) estimating sample: excludes treated establishments that are not observed in a post-treatment on-site inspection, and untreated establishments observed in fewer than 3 inspections after May 19, 2020.

Table 2: Anticipation Ability and Detected Compliance

Variable	(1) $y^d_{i,j}$	(2) $y^d_{i,j}$	(3) $y^d_{i,j}$	(4) $y^d_{i,j}$	(5) $y^d_{i,j}$	(6) $y^d_{i,j}$
$(1 - \textit{Virtual}) \times \textit{Post}$	-0.006	0.001	0.032	0.004	0.004	
	(0.022)	(0.019)	(0.041)	(0.030)	(0.030)	
Virtual	-0.135***	-0.143***	-0.098**	-0.140***	-0.134***	-0.127***
	(0.017)	(0.016)	(0.038)	(0.027)	(0.035)	(0.033)
$\textit{Virtual} \times \textit{Post}_{j-1}$					-0.007	
					(0.025)	
$\textit{Treated} \times \textit{COVID}$			-0.039	-0.003	-0.003	-0.006
			(0.040)	(0.030)	(0.030)	(0.028)
Treated	-0.040**		-0.039**			
	(0.016)		(0.016)			
14-day period FE	✓	✓	✓	✓	✓	✓
Establishment FE		✓		✓	✓	✓
Month-of-year FE		✓		✓	✓	✓
Day-of-week FE		✓		✓	✓	✓
R-squared	0.012	0.271	0.012	0.271	0.271	0.340
N	155,285	155,285	155,285	155,285	155,285	88,413

***$p < 0.01$, **$p < 0.05$,*$p < 0.1$

OLS estimates of equations (1) and (3). Standard errors, clustered two-way on establishment and 14-day period, are reported in parentheses. $y^d_{i,j}$ is an inspection's detected count of demonstrable violations. $\textit{Disc}_{i,j}$ is a binary variable, indicating that establishment i participated in grade disclosure in inspection j. $\textit{Post}_{i,j-1}$ equals 1 in all inspections after an establishment's first virtual inspection, and 0 otherwise. *COVID* equals 1 on and after March 9, 2020, and 0 otherwise.

Column (5) estimating sample: Treated establishments dropped following first treated inspection; untreated establishments dropped following first inspection after May 19, 2020.

Table 3: ANTICIPATION ABILITY AND DISCLOSURE DECISIONS

VARIABLE	(1) $Disc_{i,j}$	(2) $Disc_{i,j}$	(3) $Disc_{i,j}$	(4) $Disc_{i,j}$	(5) $Disc_{i,j}$
$(1 - Virtual) \times Post$	0.042***	0.041***	0.151***	0.060***	0.166***
	(0.015)	(0.015)	(0.029)	(0.014)	(0.028)
$Virtual$	0.054***	0.053***	0.136***	0.071***	0.163***
	(0.014)	(0.014)	(0.027)	(0.013)	(0.028)
$\overline{Disc}_{i,0} < 1$			✓		✓
$\overline{Disc}_{i,0} > 0$				✓	✓
14-day period FE	✓	✓	✓	✓	✓
Establishment FE	✓	✓	✓	✓	✓
Month-of-Year FE		✓	✓	✓	✓
Day-of-Week FE		✓	✓	✓	✓
R-squared	0.556	0.556	0.421	0.435	0.307
N	155,285	155,285	93,750	130,083	68,548

***$p < 0.01$, **$p < 0.05$,*$p < 0.1$

OLS estimates. Standard errors, clustered two-way on establishment and 14-day period, are reported in parentheses. $Disc_{i,j}$ is a binary variable, indicating that establishment i participated in grading in inspection j. $Post_{i,j-1}$ equals 1 in all inspections after an establishment's first virtual inspection, and 0 otherwise. $\overline{Disc}_{i,0}$ denotes average $Disc_{i,j}$ for establishment i in the pre-period (prior to May 20, 2020).

Table 4: TESTING DETERRENCE

VARIABLE	(1) $y_{i,j}$	(2) $y_{i,j}$	(3) $y_{i,j}$	(4) $y^a_{i,j}$	(5) $y^a_{i,j}$	(6) $y^a_{i,j}$
$(1 - Virtual) \times Post$	0.163***	0.166***	0.189***	0.070**	0.072**	0.089***
	(0.042)	(0.042)	(0.042)	(0.035)	(0.035)	(0.034)
$Virtual$	-0.256***	-0.254***		-0.192***	-0.190***	
	(0.036)	(0.036)		(0.030)	(0.030)	
14-day period FE	✓	✓	✓	✓	✓	✓
Establishment FE	✓	✓	✓	✓	✓	✓
Month-of-year FE		✓	✓		✓	✓
Day-of-week FE		✓	✓		✓	✓
R-squared	0.401	0.401	0.400	0.370	0.370	0.370
N	149,463	149,463	146,962	149,463	149,463	146,962

***$p < 0.01$, **$p < 0.05$,*$p < 0.1$

OLS estimates. Standard errors, clustered two-way on establishment and 14-day period, are reported in parentheses. $y_{i,j}$ is an inspection's detected count of all violations. $y^a_{i,j}$ is a severity-adjusted count of all violations in which each core violation adds only 0.25. In all columns, observations of treated establishments end with their initial post-treatment on-site inspection.

Columns (3) and (6) estimating sample: excludes virtual inspections.

Table 5: Deterrence Estimates and Disclosure Decisions

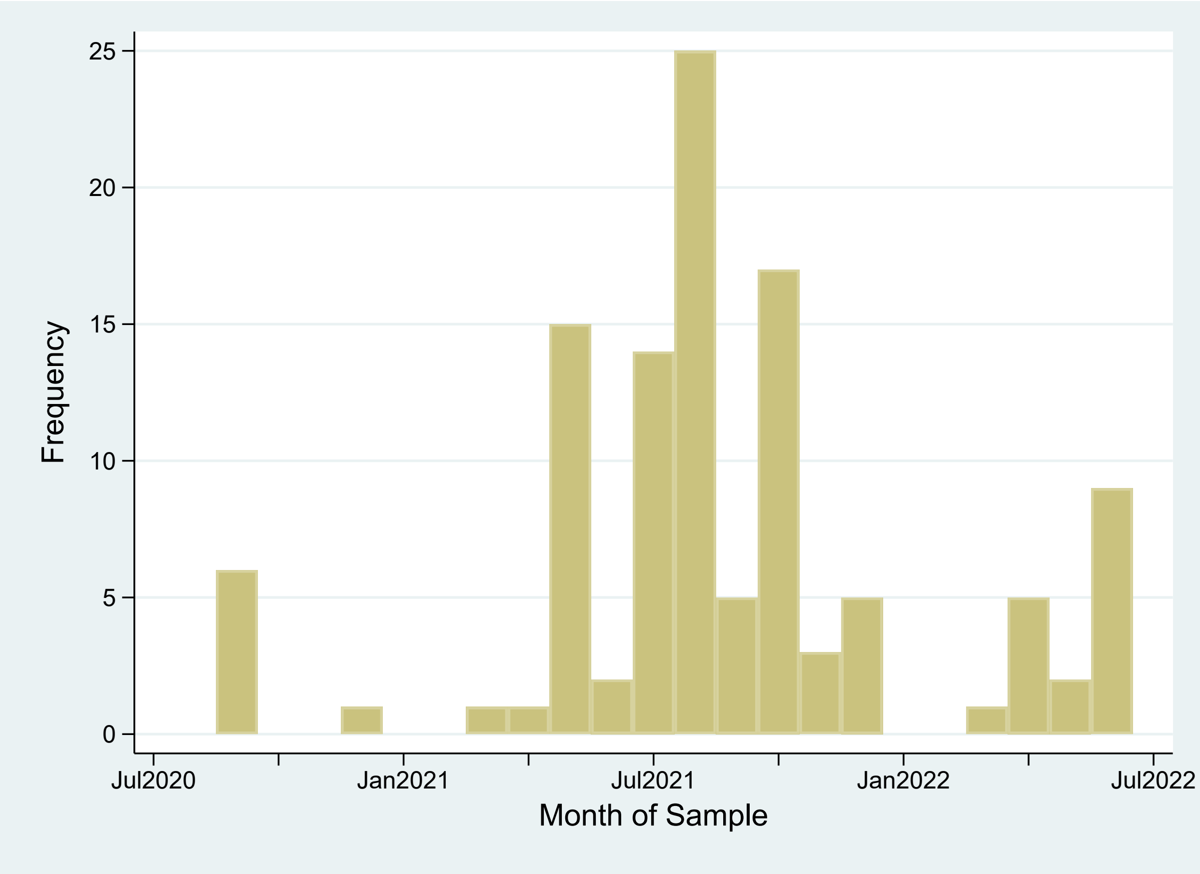
Variable	(1) $y^a_{i,j}$	(2) $y^a_{i,j}$	(3) $y^a_{i,j}$	(4) $y^a_{i,j}$
$\mathit{SwitchOff}_{i,j} \times \mathit{Post}$			0.568**	0.567**
			(0.248)	(0.248)
$(1 - \mathit{Disc}_{i,j}) \times \mathit{Post}$	0.233	0.233		
	(0.146)	(0.145)		
Post	0.052*	0.054*	0.056*	0.058*
	(0.030)	(0.030)	(0.030)	(0.031)
14-day period FE	✓	✓	✓	✓
Establishment FE	✓	✓	✓	✓
Month-of-Year FE		✓		✓
Day-of-Week FE		✓		✓
R-squared	0.369	0.370	0.369	0.370
N	146,962	146,962	146,962	146,962

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

OLS estimates. Standard errors, clustered two-way on establishment and 14-day period, are reported in parentheses. $\mathit{SwitchOff}_{i,j}$ is a binary variable indicating $\mathit{Disc}_{i,j} = 0$ and $\mathit{Disc}_{i,j-1} = 1$. In all columns, observations of treated establishments end with their initial post-treatment on-site inspection. Virtual inspections are excluded.

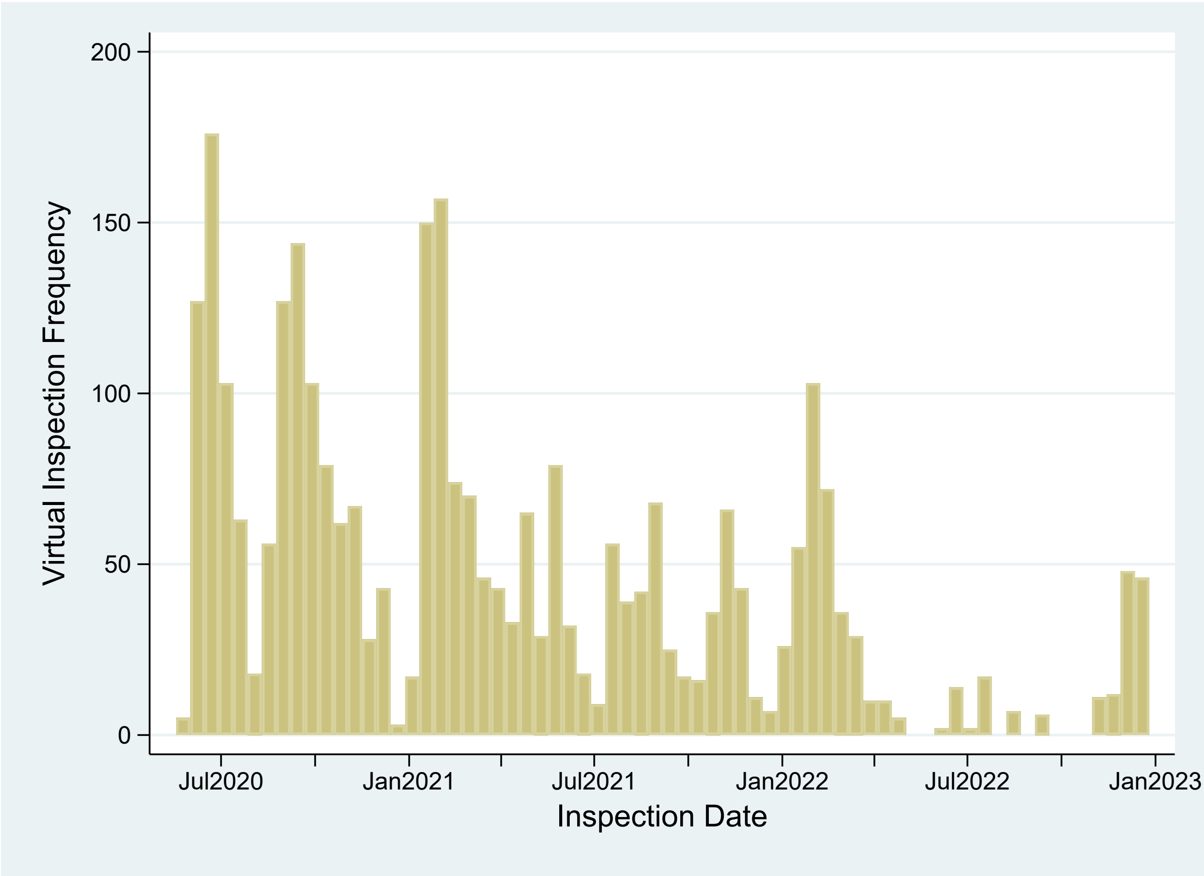
A1 Appendix

Figure A1: Frequency of Flagged Inspections



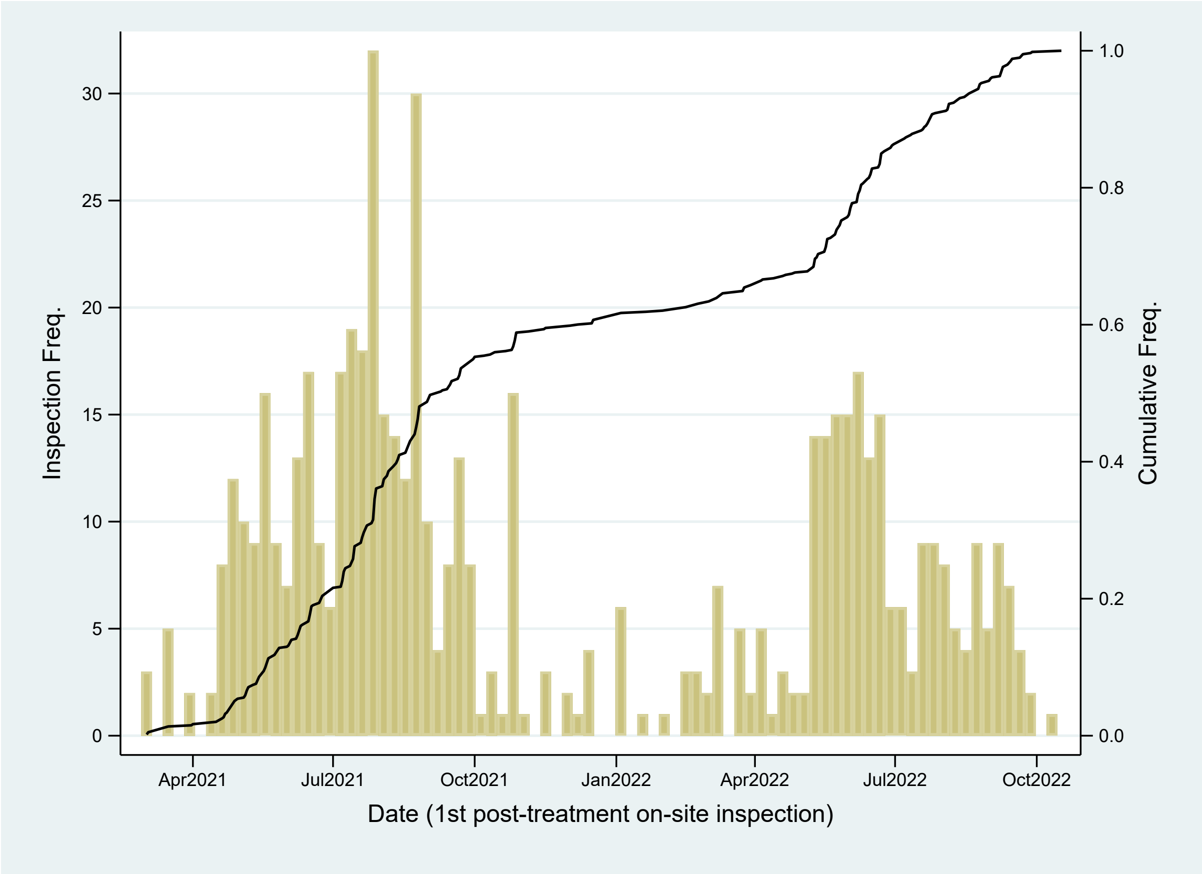
Frequency distribution of the 112 inspections that are not indicated as being virtual, but that occur in between virtual inspections of a treated establishment.

Figure A2: Dates of Virtual Inspections



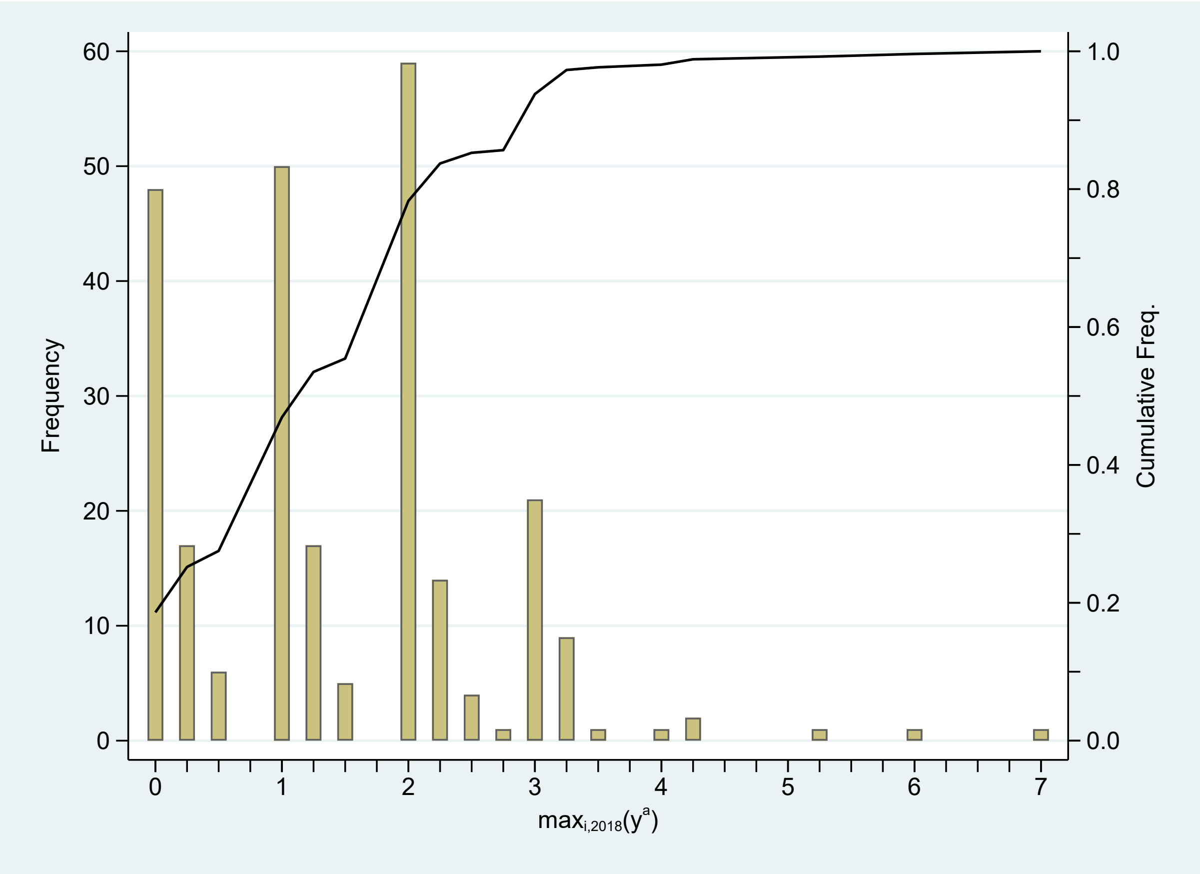
Among treated establishments, beige bars mark the frequency of virtual inspections within 14-day bins.

Figure A3: Dates of Initial Post-treatment On-site Inspections



Beige bars mark the frequency distribution of the estimating sample's 587 initial post-treatment on-site inspection dates (corresponding y-axis: left). The black line marks the cumulative frequency of initial post-treatment on-site inspection dates (corresponding y-axis: right).

Figure A4: 2018 Noncompliance Maxima: Higher Risk-class Treated Establishments



Beige bars mark the frequency distribution of $\max_{i,2018}(y^a)$ among the 258 higher risk-class treated establishments fully observed in 2018 (corresponding y-axis: left). The black line marks the cumulative frequency of $\max_{i,2018}(y^a)$ among them (corresponding y-axis: right).

Table A1: Establishment Types

	Treated with Virtual Inspection	
	No	Yes
Permit Type	Number of Establishments	
Adult Daycare	1	2
Adventure Food Service	1	0
Assisted Living	0	163
Bakery	467	0
Boarding Home	34	0
Bottled Water & Beverage	39	0
Damaged Foods	6	0
Daycare Food Service	307	1
Eating & Drinking	10,527	135
Food Bank	38	1
Food Catering	503	7
Food Jobber	239	0
Food Processor	430	1
Hospital Food Service	1	59
Ice Manufacturing	6	0
Jail Food Service	2	0
Meat Market	606	0
Micromarket	53	1
Nursing Home	0	79
Refrigeration Warehouse	4	0
Retail Food Establishment	2,450	3
School Food Service	852	0
Senior Food Service	3	1
Service Kitchen	175	170

Count of different permit types among: untreated establishments observed in at least two inspections before, and at least two inspections after May 19, 2020; and treated establishments observed in at least two inspections before May 20, 2020. Excluded are 347 untreated establishments not inspected for an entire calendar-year due to temporary closure.

Table A2: Assessing Pre-period Trends

Variable	(1) $y^d_{i,j}$	(2) $y^d_{i,j}$	(3) $y_{i,j}$	(4) $y_{i,j}$	(5) $y^a_{i,j}$	(6) $y^a_{i,j}$
Quarterly Trend × *Treated*	-0.001	-0.001	0.011	0.006	0.003	0.001
	(0.005)	(0.004)	(0.010)	(0.008)	(0.008)	(0.007)
Quarterly Trend	-0.009***	0.018	-0.031***	0.005	-0.024***	0.016
	(0.003)	(0.013)	(0.007)	(0.035)	(0.005)	(0.026)
Treated	-0.030		-0.418***		-0.242***	
	(0.031)		(0.066)		(0.050)	
14-day period FE		✓		✓		✓
Establishment FE		✓		✓		✓
Day-of-week FE		✓		✓		✓
R-squared	0.002	0.368	0.006	0.501	0.005	0.470
N	74,134	74,134	71,249	71,249	71,249	71,249

***$p < 0.01$, **$p < 0.05$,*$p < 0.1$

OLS estimates from inspections prior to May 20, 2020. Standard errors, clustered two-way on establishment and 14-day period, are reported in parentheses. Estimating sample in columns (3), (4), (5), and (6), excludes treated establishments that are not observed in a post-treatment on-site inspection. *Quarterly Trend* is the quarter-year of the sample, equal to 1 for January-March 2018. $y^d_{i,j}$ is an inspection's detected count of demonstrable violations. $y_{i,j}$ is an inspection's detected count of all violations. $y^a_{i,j}$ is a severity-adjusted count of all violations in which each core violation adds only 0.25.

Table A3: Pre-period Trends in Disclosure Decisions

Variable	(1) $Disc_{i,j}$	(2) $Disc_{i,j}$	(3) $Disc_{i,j}$	(4) $Disc_{i,j}$	(5) $Disc_{i,j}$	(6) $Disc_{i,j}$
Trend × *Treated*	-0.002	-0.001	-0.003	-0.006	-0.003	-0.010
	(0.001)	(0.001)	(0.003)	(0.004)	(0.003)	(0.008)
Trend	-0.000	0.012**	0.001*	0.000	0.004	0.002*
	(0.000)	(0.005)	(0.000)	(0.001)	(0.009)	(0.001)
Treated	0.204***		0.115**	0.208***		0.127**
	(0.024)		(0.050)	(0.025)		(0.051)
Monthly Trend	✓	✓	✓			
Quarterly Trend				✓	✓	✓
$0 < \overline{Disc}_{i,0} < 1$			✓			✓
14-day period FE		✓			✓	
Establishment FE		✓			✓	
Day-of-week FE		✓			✓	
R-squared	0.007	0.637	0.001	0.007	0.637	0.001
N	74,134	74,134	33,131	74,134	74,134	33,131

$^{***}p < 0.01$, $^{**}p < 0.05$,$^{*}p < 0.1$

OLS estimates of equation (2) from primary-sample inspections prior to May 20, 2020. Standard errors, clustered two-way on establishment and 14-day period, are reported in parentheses. $Disc_{i,j}$ is a binary variable, indicating that establishment i participated in grading for inspection j. $\overline{Disc}_{i,0}$ denotes average $Disc_{i,j}$ for establishment i in the pre-period (prior to May 20, 2020).

Table A4: ROBUSTNESS TO EXPANDED COMPARISON GROUP

VARIABLE	(1) $y_{i,j}^d$	(2) $y_{i,j}^d$	(3) $y_{i,j}^d$	(4) $y_{i,j}^d$	(5) $y_{i,j}^d$	(6) $y_{i,j}^d$
$(1 - Virtual) \times Post$	-0.007	-0.002	0.055	0.018	0.018	
	(0.021)	(0.019)	(0.039)	(0.031)	(0.031)	
Virtual	-0.142***	-0.150***	-0.080**	-0.130***	-0.125***	-0.119***
	(0.017)	(0.016)	(0.037)	(0.028)	(0.035)	(0.032)
$Virtual \times Post_{j-1}$					-0.007	
					(0.023)	
$Treated \times COVID$			-0.064*	-0.021	-0.021	-0.021
			(0.038)	(0.031)	(0.031)	(0.028)
Treated	-0.003		-0.001			
	(0.016)		(0.016)			
14-day period FE	✓	✓	✓	✓	✓	✓
Establishment FE		✓	✓	✓	✓	✓
Month-of-year FE		✓	✓	✓	✓	✓
Day-of-week FE		✓	✓	✓	✓	✓
R-squared	0.011	0.280	0.011	0.280	0.280	0.347
N	186,032	186,032	186,032	186,032	186,032	106,055

***$p < 0.01$, **$p < 0.05$,*$p < 0.1$

OLS estimates from expanded sample including establishments of any type, with at least two inspections before, and at least one inspection on or after May 20, 2020. Standard errors, clustered two-way on establishment and 14-day period, are reported in parentheses. $y_{i,j}^d$ is an inspection's detected count of demonstrable violations. $Disc_{i,j}$ is a binary variable, indicating that establishment i participated in grade disclosure in inspection j. $Post_{i,j-1}$ equals 1 in all inspections after an establishment's first virtual inspection, and 0 otherwise. *COVID* equals 1 on and after March 9, 2020, and 0 otherwise.

Column (6) estimating sample: Treated establishments dropped following first treated inspection; untreated establishments dropped following first inspection after May 19, 2020.

Table A5: Deterrence Effect Estimates: Higher Risk-class Establishments

	$\max_{i,2018}(y^a) \geq Z$			$\max_{i,2018}(y^a) < Z$		
Z	$\widehat{\alpha}_1$		N	$\widehat{\alpha}_1$		N
0.25	0.120	(0.077)	34,563	-0.045	(0.051)	6,050
0.50	0.143*	(0.080)	31,606	-0.055	(0.065)	9,007
1.00	0.156*	(0.081)	29,829	-0.047	(0.067)	10,784
1.25	0.225**	(0.105)	24,706	-0.104	(0.066)	15,907
1.50	0.229*	(0.119)	21,830	-0.054	(0.066)	18,783
2.00	0.259**	(0.118)	20,608	-0.065	(0.065)	20,005
2.25	0.447**	(0.197)	14,765	-0.058	(0.060)	25,848
2.50	0.516**	(0.221)	12,301	-0.032	(0.056)	28,312
2.75	0.398*	(0.214)	11,306	0.004	(0.059)	29,307
3.00	0.420*	(0.219)	10,944	0.006	(0.060)	29,669

$^{***}p < 0.01$, $^{**}p < 0.05$,$^{*}p < 0.1$

Coefficients, standard errors, and sample sizes, corresponding to Figure 2. All estimates include fixed effects for: establishment, 14-day period, month-of-year, and day-of-week. Standard errors, clustered two-way on establishment and 14-day period, are reported in parentheses.

Table A6: Pre-period Trends: Higher Risk-class Establishment Groups

	$\max_{i,2018}(y^a) \geq Z$			$\max_{i,2018}(y^a) < Z$		
Z	$\widehat{\gamma}_1$		N	$\widehat{\gamma}_1$		N
0.25	0.004	(0.006)	12,448	-0.009	(0.007)	2,229
0.50	0.004	(0.007)	11,372	-0.001	(0.006)	3,305
1.00	0.004	(0.007)	10,731	0.002	(0.007)	3,946
1.25	0.003	(0.011)	8,849	0.002	(0.007)	5,828
1.50	0.004	(0.011)	7,805	0.002	(0.006)	6,872
2.00	0.005	(0.011)	7,376	0.002	(0.006)	7,301
2.25	0.004	(0.019)	5,233	-0.002	(0.005)	9,444
2.50	0.003	(0.023)	4,357	-0.002	(0.005)	10,320
2.75	-0.007	(0.024)	4,009	0.001	(0.005)	10,668
3.00	-0.003	(0.024)	3,880	-0.000	(0.005)	10,797

$^{***}p < 0.01$, $^{**}p < 0.05$,$^{*}p < 0.1$

OLS estimates of equation (2), using higher risk-class establishments fully observed in 2018. Estimating samples exclude all inspections conducted during 2018, or after May 19, 2020. All estimates include fixed effects for: establishment, 14-day period, and day-of-week. Standard errors, clustered two-way on establishment and 14-day period, are reported in parentheses.

Table A7: Deterrence Effect Estimates: Lower Risk-class Establishments

	$\max_{i,2018}(y^a) \geq Z$			$\max_{i,2018}(y^a) < Z$		
Z	$\widehat{\alpha}_1$		N	$\widehat{\alpha}_1$		N
0.25	0.064	(0.059)	30,561	0.016	(0.040)	21,770
0.50	0.058	(0.085)	24,926	0.027	(0.037)	27,405
1.00	0.091	(0.087)	22,036	0.027	(0.037)	30,295
1.25	-0.008	(0.106)	14,966	0.042	(0.038)	37,365
1.50	0.001	(0.128)	11,522	0.050	(0.037)	40,809
2.00	-0.000	(0.142)	10,208	0.055	(0.038)	42,123

$^{***}p < 0.01$, $^{**}p < 0.05$,$^{*}p < 0.1$

Coefficients, standard errors, and sample sizes, corresponding to Figure 3. All estimates include fixed effects for: establishment, 14-day period, month-of-year, and day-of-week. Standard errors, clustered two-way on establishment and 14-day period, are reported in parentheses.

Table A8: Pre-period Trends: Lower Risk-class Establishment Groups

	$\max_{i,2018}(y^{a}) \geq Z$			$\max_{i,2018}(y^{a}) < Z$		
Z	$\widehat{\gamma}_1$		N	$\widehat{\gamma}_1$		N
0.25	0.011	(0.012)	11,322	-0.004	(0.006)	8,041
0.50	0.016	(0.016)	9,235	-0.004	(0.005)	10,128
1.00	0.009	(0.016)	8,172	-0.000	(0.005)	11,191
1.25	0.008	(0.021)	5,548	-0.001	(0.006)	13,815
1.50	-0.003	(0.021)	4,267	0.001	(0.006)	15,096
2.00	-0.011	(0.023)	3,784	0.003	(0.006)	15,579

$^{***}p < 0.01$, $^{**}p < 0.05$,$^{*}p < 0.1$

OLS estimates of equation (2), using lower risk-class establishments fully observed in 2018. Estimating samples exclude all inspections conducted during 2018, or after May 19, 2020. All estimates include fixed effects for: establishment, 14-day period, and day-of-week. Standard errors, clustered two-way on establishment and 14-day period, are reported in parentheses.