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 inspection-targeting rule 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. Beyond food safety, it is also central to regulating—among other things—environmental quality, workplace hazards, international maritime practices, nursing-home standards, and licensed firearm dealers.

With monitoring resources efficiently deployed, a tradeoff exists between enforcement- and noncompliance costs—the sum of which is minimized at the social optimum. Yet, 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 regarding these relationships is sparse and challenging to attain. Variation in monitoring frequency is potentially endogenous to compliance, and even if not, accounting for firms' perceptions is difficult.[2] Finally, even with an exogenous and perceived detection-probability shock, cleanly separating that shock's *deterrence effect*

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

[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.

from its opposing—and often simultaneous—*detection effect*, is seldom feasible.[3] 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 onset, routine health inspections by the Maricopa County Environmental Services Department (MCESD) continued on-site at most permitted food establishments. However, in May 2020 they began conducting these inspections by video-conference for establishments serving vulnerable populations, such as hospitals, nursing homes, and assisted living facilities. 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 policy 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 probabilities 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 tests.[4] Violations of these particular codes will isolate potential anticipation-enabled avoidance, because the virtual format doesn't inhibit their

[3] E.g., 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*).

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

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 last-minute 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. Notably, 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 a substantial decline in compliance effort.

This quasi-experiment also permits examination of a fundamental dilemma: should firms with strong compliance records receive fewer inspections, so that severe violators can receive more? Deterrence-effect heterogeneity supports redirecting some routine inspections away from highly compliant establishments in lower risk classes, and toward establishments in higher risk classes where significant noncompliance has been found. I show that—with existing agency resources—a simple policy of targeted redirection might meaningfully improve compliance.

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.[5] 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 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

[5]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.

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 their 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*.[6] Respectively, establishments in these classes are prescribed 2, 2, 3, and 4, annual routine inspections.

Inspections check health code compliance and violations are specified—from most to least severe—as *priority*, *priority foundation*, and *core*.[7] 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 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

[6] *Class 1* applies only to Micromarket permits, none of which are in the primary estimating samples (see Section 3).

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

"Not Participating" in place of a letter grade. The election is irreversible, and made before the inspection starts.

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.[8]

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.[9] 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 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.[10] In a May 7, 2020 press conference, MCESD Director Darcy Kober explained that, throughout the pandemic, MCESD had continued conducting on-site inspection visits,

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

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

[10]See here.

as many establishments were providing dine-out service.[11] During that time, MCESD recorded many "ineffective visits", where visited establishments were found to be temporarily closed. It's noteworthy that MCESD continued visiting establishments without making status inquiries, as it suggests reluctance to reveal an imminent inspection.

On May 20, 2020, MCESD began conducting what it called "virtual inspections" at establishments with 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 collect 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".[12] In total, 3,489 inspections are tagged as virtual.

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

[12]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.

My interest lies with establishments whose inspections were immediately shifted to remote format at its introduction.[13] As such, establishments are considered treated if their first two inspections following May 19, 2020, are virtual. I further restrict attention to establishments 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.[14] Further, the frequency of these inspections declines beginning 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 due to temporary closure. My primary estimation sample consists of observations 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

[13] 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.

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

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 before the virtual inspection begins. 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

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

$\mathit{Trend}_{i,j}$ 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, $y^a_{i,j}$, in which each core violation adds only 0.25.[15] Appendix Table A2 reports these same estimates using quarterly trends.

To visualize the trend comparison, Figure 1 presents simple quarter-year averages of $y^d_{i,j}$ 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

[15]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.

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, $Treated_{i,j}$, 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. Moreover, between pre- and post-treatment on-site inspections, the estimated difference in detected y^d is relatively small and statistically insignificant; the reduction observed in virtual inspections in no way persists when unscheduled on-site visits resume.

Because establishments are treated on the basis of serving vulnerable populations, a potential concern is that the pandemic may uniquely affect the treated establishments. Between March 9 and April 9, 2020, I observe 116 on-site inspections of treated establishments already exposed to the pandemic. Column (3) of Table 2 adds the interaction of ($Treated_i \times COVID_{i,j}$) as a control, where $COVID_{i,j}$ is a binary variable equaling 1 on and after March 9, 2020. There, $\widehat{\alpha}_2$ represents a 51.9% decrease relative to the pre-treatment average, and the change estimated in pre-treatment pandemic-period inspections is very small and statistically insignificant.

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.[16] 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

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

where $\mathit{Post}_{i,j-1}$ is a one-inspection lag of *Post*. The interaction, $(\mathit{Virtual}_{i,j} \times \mathit{Post}_{i,j-1})$, 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 (4) 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 (5) 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.

Finally, 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;

[16] 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."

and the remainder chose each option at least once.[17] To assess whether participation decisions in virtual inspections are consistent with avoidance behavior, column (6) of Table 2 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. Consistent with opportunistic use of anticipation ability, a 6.6% increase in disclosure participation (relative to a pre-treatment average of 0.801) is estimated in virtual inspections.

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 A3 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 become less compliant. In initial post-treatment on-site inspections, while treated establishments' compliance efforts likely reflect virtual-regime perceptions, actual detection probabilities returns to the pre-treatment levels, thereby removing the detection effect and isolating any deterrence effect.

In assessing the response of compliance effort, 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 ob-

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

served 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 (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, $\alpha_1 > 0$.

Columns (1) and (2) of Table 3 report full-specification estimates for y and y^a. As expected, detected noncompliance is substantially lower in virtual inspections. 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. Further, those decreases are more than offset by the return of unannounced on-site visits. Consistent with general deterrence, establishments' initial post-treatment on-site inspections detect y and y^a exceeding their pre-treatment averages by 25.2% and 14.5%.

The return to on-site inspections was not publicly announced, hence the assumption that establishments' compliance efforts reflected virtual-regime perceptions of detection probabilities; and to the extent that some establishments expected their return to on-site visits, this will attenuate $\widehat{\alpha}_1$. To account for that possibility, columns (3) and (4) report estimates using the same sample, but add an interaction term equaling 1 in initial post-treatment on-site inspections where establishments declined participation in grade disclosure, and 0 otherwise. (Presumably, establishments caught off-guard are more likely to decline participation.)

In column (4), among disclosure participants, y^a (which adjusts $y_{i,j}$ according to the grading scheme) in their first post-treatment on-site inspections is not significantly different from the pre-treatment period. However, in initial post-treatment on-site inspections a statistically significant and large increase in y^a is detected among the establishments declining participation. Those two coefficients combine to equal 0.324 (standard error

[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).

0.140), which represents a 42.6% increase in y^a relative to a pre-treatment average of 0.761 among these 458 establishments.

There are 430 treated establishments observed in a second post-treatment on-site visit in the inspection immediately following their first. Columns (5) and (6) adjust the estimating sample by including their second post-treatment on-site inspections, and excluding all observations from: treated establishments not observed in two consecutive post-treatment on-site inspections, and untreated establishments with fewer than four observed post-period inspections. The estimated specification adds an interaction term equaling 1 in an establishment's second post-treatment on-site inspection, and 0 otherwise.

In column (6), relative to a pre-treatment average of 0.517, y^a is 19.3% higher in initial post-treatment on-site inspections. Further, consistent with incorrectly low detection-probability beliefs driving that difference, a relative decrease occurs in the second post-treatment on-site inspection. The sum of those coefficients yields a small and statistically insignificant difference of 0.030 (standard error 0.040) in y^a between the second consecutive post-treatment on-site inspection and the pre-treatment period.

5.3 Policy Implications

Dynamic enforcement policies raise individual-level expected costs following observed noncompliance, thereby enhancing 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. Here, rather than penalties, I assess the feasibility of raising inspection frequency, and thus, detection probability.

In this setting, uniform deployment of inspections is likely 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 an inspection-targeting rule might be leveraged toward such improvements.

Consider a rule explicitly applied to a set of establishments ($\mathbb{S}$), where $y^a_{i,j} \geq Z$ 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, when $y^a_{i,j} \geq Z$ is realized. Condition (2) suggests the rule will deter violations, because the establishments most at risk of triggering additional scrutiny will likely respond to the threat of it. At the social optimum, the difference in cost between the noncompliance deterred by the rule and caused by its 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

[19] Any specific deterrence generated by the added penalty (as in Makofske, 2020b), reduces the cost of additional redirection, but doesn't alter the fundamental tradeoff.

establishments on whether or not $\max_{i,2018}(y^{a}) \geq Z$, and separately estimate equation (1) within each group, including all controls, with $y_{i,j}^{a}$ as the outcome variable, and using only post-2018 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 $y_{i,j}^{a} \geq Z$ in 2018.[20]

Among establishments with $y_{i,j}^{a} \geq Z$ in 2018, deterrence effect estimates are increasing in Z up to 2.5. Notably, relative to 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 or equal to 2, 2.25, and 2.5, respectively. These findings support such a rule's ability to deter violations, as the establishments apt to trigger additional scrutiny are also responsive to expected costs. Figure A5 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 $y_{i,j}^{a} = 0$ in both 2018 inspections, and 3,327 had $y_{i,j}^{a} = 0$ in both 2019 inspections. Among higher risk-class establishments: $y_{i,j}^{a} \geq 2.5$ occurred 2,510 times in 2018, and 2,479 times in 2019; whereas $y_{i,j}^{a} \geq 2.25$ occurred 3,123 times in 2018, and 3,099 times in 2019. Redirecting from lower risk-class establishments with $y_{i,j}^{a} = 0$ in their two most recent inspections, appears sufficient to target higher risk-class establishments where $y_{i,j}^{a} \geq 2.5$ with an additional inspection over the next year.

6 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. Ex-

[20] Appendix Table A4 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.

ploiting 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 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 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.[21] 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.[22] 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.

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

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

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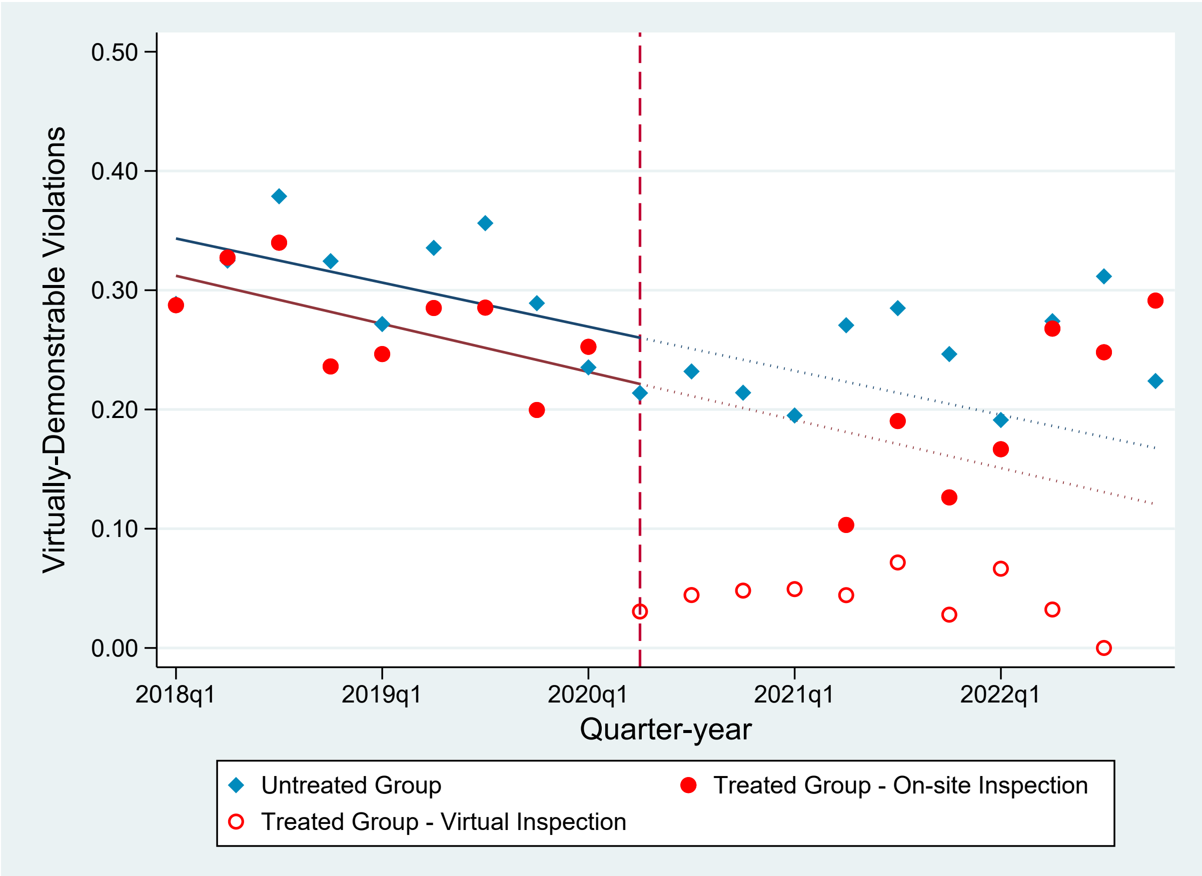
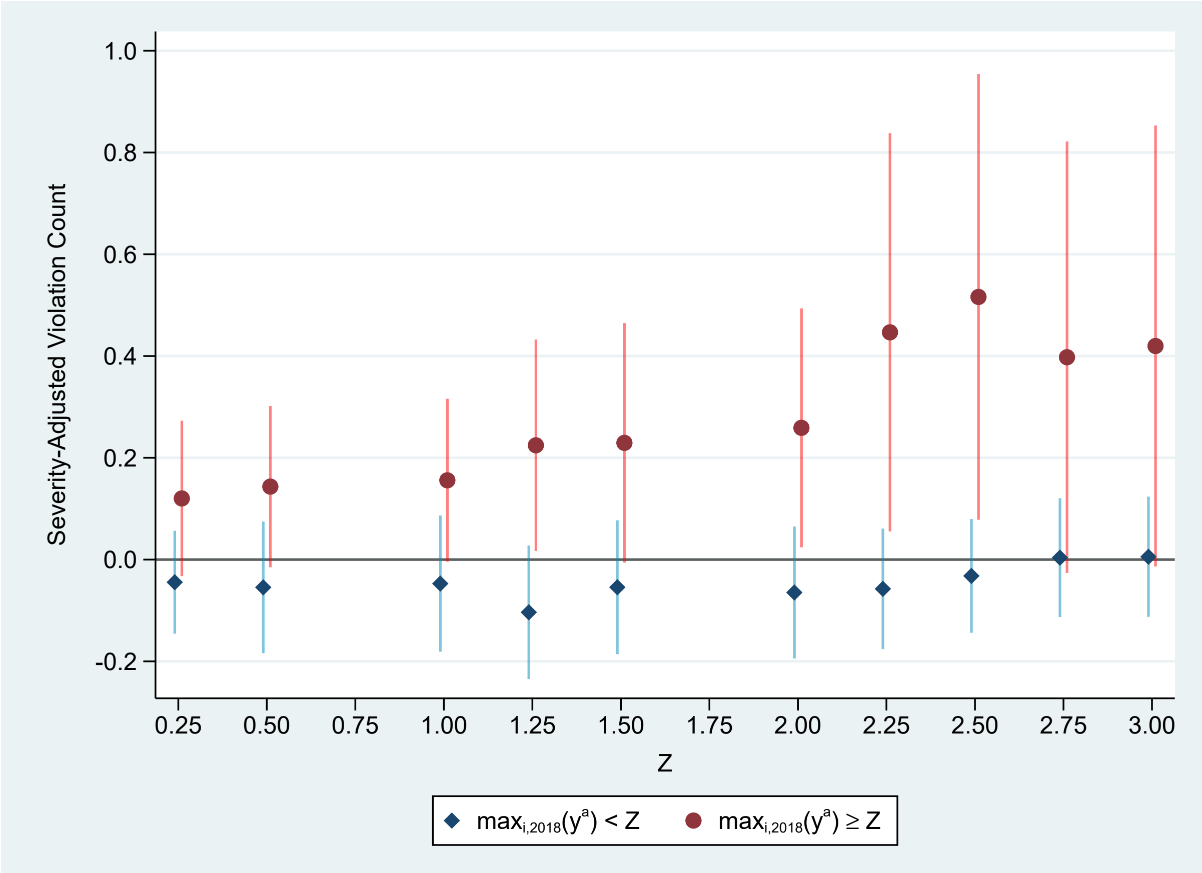
Figure 1: Inspection Format and Detected Violations



Average $y_{i,j}^{d}$ by quarter-year of sample. The "treated group" are establishments that received at least two consecutive virtual inspections beginning in 2020, and observed in at least 2 inspections before May 20, 2020 (when virtual inspections began). 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 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 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.

Table 1: 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) $y^a_{i,j}$
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. $y^d_{i,j}$ 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. *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) $Disc_{i,j}$
$(1 - Virtual) \times Post$	-0.006	0.001	0.004	0.004		0.041***
	(0.022)	(0.019)	(0.030)	(0.030)		(0.015)
Virtual	-0.135***	-0.143***	-0.140***	-0.134***	-0.127***	0.053***
	(0.017)	(0.016)	(0.027)	(0.035)	(0.033)	(0.014)
$Virtual \times Post_{j-1}$				-0.007		
				(0.025)		
$Treated \times COVID$			-0.003	-0.003	-0.006	
			(0.030)	(0.030)	(0.028)	
Treated	-0.040**					
	(0.016)					
14-day period FE	✓	✓	✓	✓	✓	✓
Establishment FE		✓	✓	✓	✓	✓
Month-of-year FE		✓	✓	✓	✓	✓
Day-of-week FE		✓	✓	✓	✓	✓
R-squared	0.012	0.271	0.271	0.271	0.340	0.556
N	155,285	155,285	155,285	155,285	88,413	155,285

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

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. $Post_{i,j-1}$ equals 1 in all inspections after an establishment's first virtual inspection, 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: Testing Deterrence

Variable	(1) $y_{i,j}$	(2) $y^a_{i,j}$	(3) $y_{i,j}$	(4) $y^a_{i,j}$	(5) $y_{i,j}$	(6) $y^a_{i,j}$
$(1 - \mathit{Virtual}) \times \mathit{Post} \times D_{i,j}$			0.173	0.296**	-0.088*	-0.070
			(0.153)	(0.140)	(0.050)	(0.042)
$(1 - \mathit{Virtual}) \times \mathit{Post}$	0.166***	0.072**	0.140***	0.028	0.225***	0.100**
	(0.042)	(0.035)	(0.041)	(0.031)	(0.052)	(0.044)
Virtual	-0.254***	-0.190***	-0.254***	-0.190***	-0.283***	-0.212***
	(0.036)	(0.030)	(0.036)	(0.030)	(0.043)	(0.035)
$D_{i,j} = (1 - \mathit{Disc}_{i,j})$			✓	✓		
$D_{i,j} = (1 - \mathit{Virtual}_{i,j-1})$					✓	✓
14-day period FE	✓	✓	✓	✓	✓	✓
Establishment FE	✓	✓	✓	✓	✓	✓
Month-of-year FE	✓	✓	✓	✓	✓	✓
Day-of-week FE	✓	✓	✓	✓	✓	✓
R-squared	0.401	0.370	0.401	0.370	0.399	0.369
N	149,463	149,463	149,463	149,463	142,368	142,368

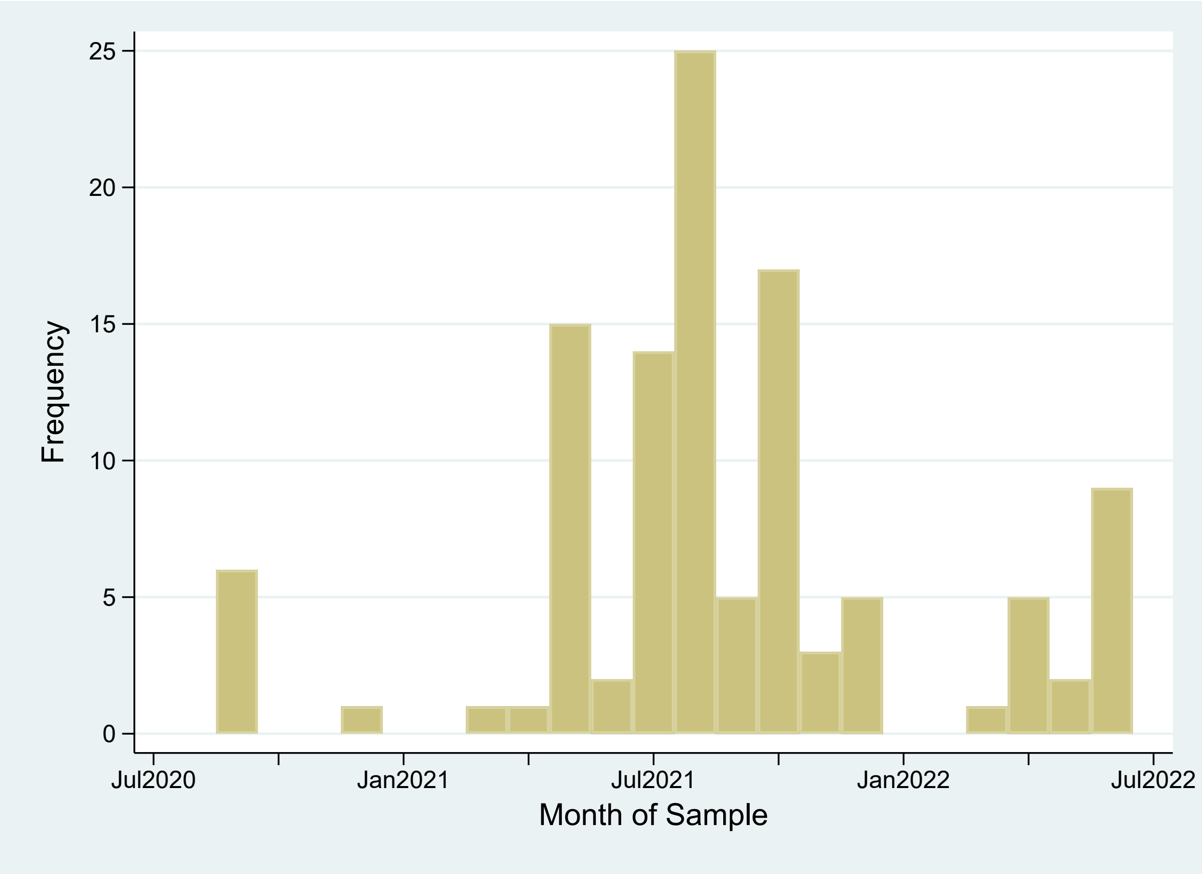
***$p < 0.01$, **$p < 0.05$,*$p < 0.1$

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. $\mathit{Virtual}_{i,j-1} = 1$ if the previous inspection of establishment i was virtual, and 0 otherwise.

Columns (5) and (6) estimating sample: All inspections prior to, and including, a treated establishment's second consecutive post-treatment on-site inspection. Excludes: treated establishments not observed in a second consecutive post-treatment on-site inspection, and untreated establishments observed in fewer than 4 inspections after May 19, 2020.

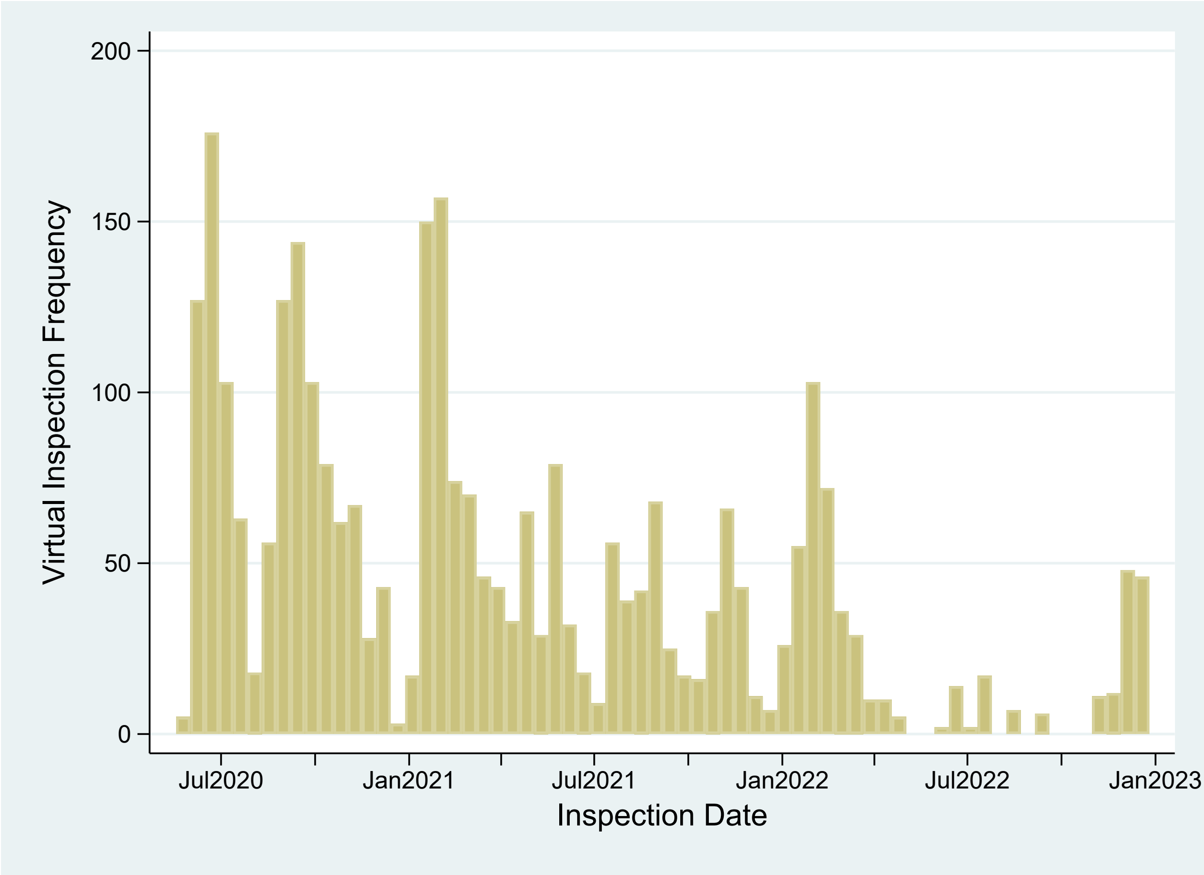
A1 Appendix

Figure A1: Frequency of Flagged Inspections



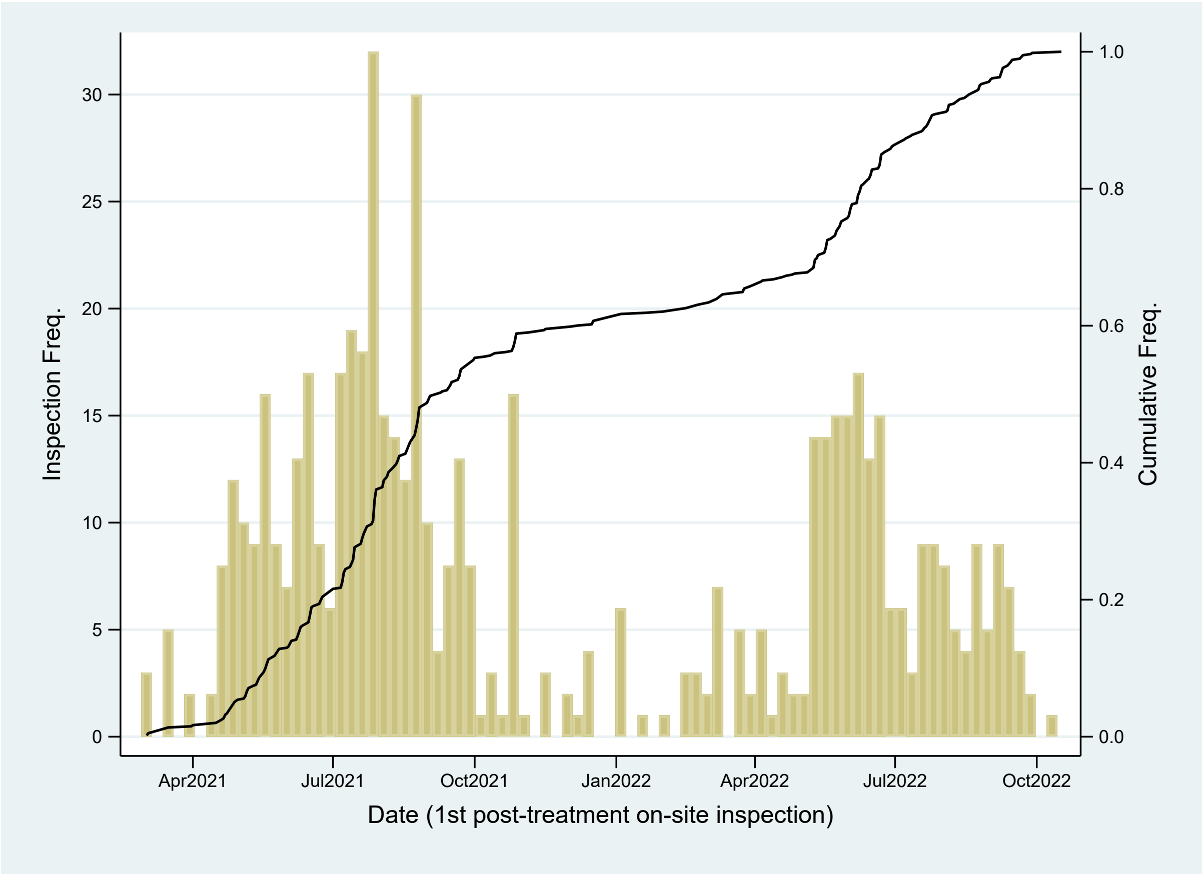
Frequency distribution of the 118 inspections that are not indicated as being virtual, but that occur in between virtual inspections of a treated establishment.

Figure A2: Dates of Virtual Inspections



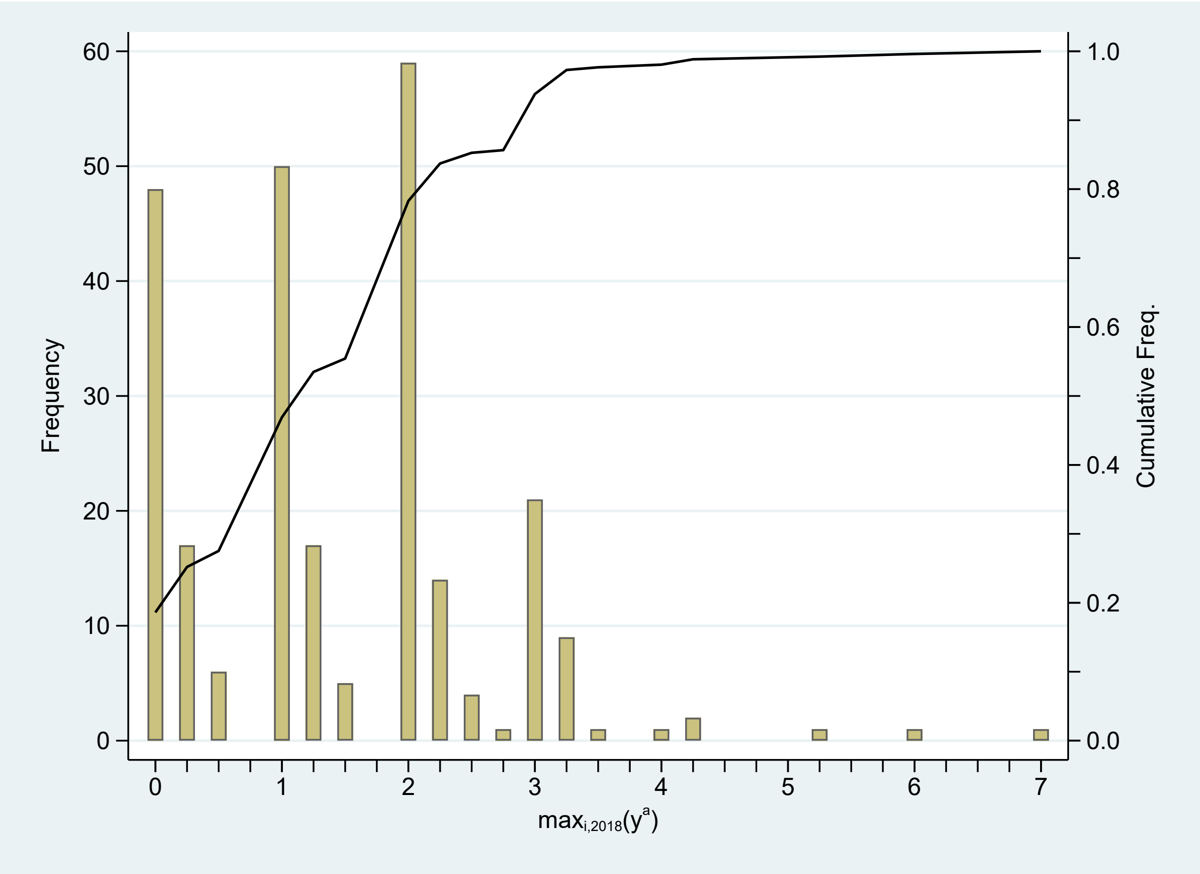
Among treated establishments, beige bars mark the frequency of virtual inspections within 14-day bins.

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



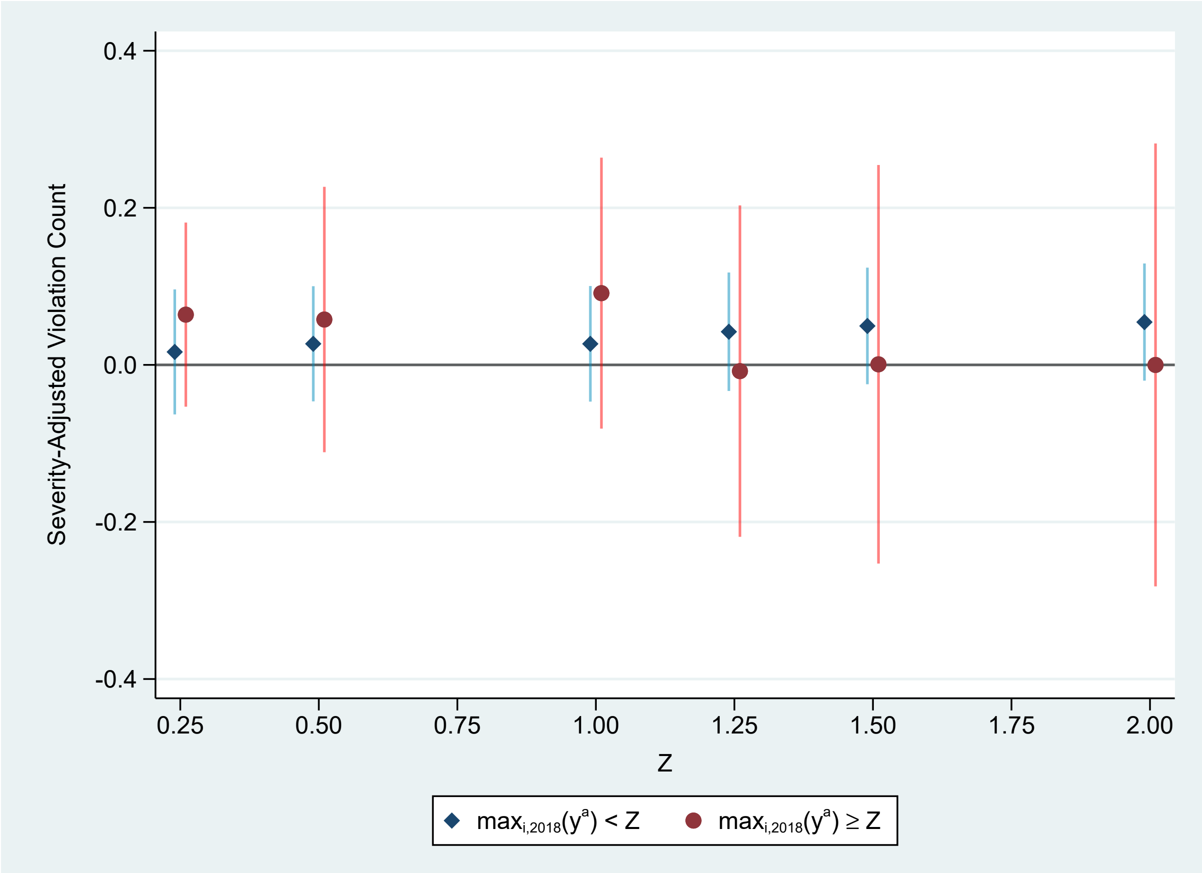
Beige bars mark the frequency distribution of the estimating sample's 587 initial post-treatment on-site inspection dates (corresponding y-axis: left). The black line marks the cumulative frequency of initial post-treatment on-site inspection dates (corresponding y-axis: right).

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).

Figure A5: 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 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) $y_{i,j}^{d}$	(2) $y_{i,j}^{d}$	(3) $y_{i,j}$	(4) $y_{i,j}$	(5) $y_{i,j}^{a}$	(6) $y_{i,j}^{a}$
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. $y_{i,j}^{d}$ 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.

Table A3: Robustness to Expanded Comparison Group

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) $Disc_{i,j}$
$(1 - Virtual) \times Post$	-0.007	-0.002	0.018	0.018		0.038**
	(0.021)	(0.019)	(0.031)	(0.031)		(0.015)
$Virtual$	-0.142***	-0.150***	-0.130***	-0.125***	-0.119***	0.054***
	(0.017)	(0.016)	(0.028)	(0.035)	(0.032)	(0.014)
$Virtual \times Post_{j-1}$				-0.007		
				(0.023)		
$Treated \times COVID$			-0.021	-0.021	-0.021	
			(0.031)	(0.031)	(0.028)	
$Treated$	-0.003					
	(0.016)					
14-day period FE	✓	✓	✓	✓	✓	✓
Establishment FE		✓	✓	✓	✓	✓
Month-of-year FE		✓	✓	✓	✓	✓
Day-of-week FE		✓	✓	✓	✓	✓
R-squared	0.011	0.280	0.280	0.280	0.347	0.572
N	186,032	186,032	186,032	186,032	106,055	186,032

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

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. $y^d_{i,j}$ is an inspection's detected count of demonstrable violations. $Post_{i,j-1}$ equals 1 in all inspections after an establishment's first virtual inspection, 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 A4: 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 A5: 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 A5. 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: 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.