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%.

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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, the Maricopa County Environmental Services Department (MCESD) continued conducting routine health inspections on-site at most permitted food establishments. However, in May 2020 they began conducting these inspections by video-conference for establishments serving especially 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, will likely 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 detection avoidance.

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.

Individual-level responses to this shock provide insight on 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 the highest risk class where significant violations have been found. I find that a simple rule would achieve this targeted redirection, and potentially enhance general deterrence in the highest risk classification and reduce social noncompliance costs, with existing inspection 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.[5] Zou (2021) exploits every-sixth-day pollution monitoring under the Clean Air Act, which the US Environmental Protection Agency 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

[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.

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 firm-specific responses allow an examination of potential policy improvements that is not possible in prior work.

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 26 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

[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 code violated; i.e., a particular health code can be violated to each severity level.

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.

Despite the ability to preemptively opt out of grading, detected violations carry potential costs presumed sufficient to motivate avoidance. 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

[8]In Louisville, KY, where mandatory on-site disclosure of a compliance score was already in place, Makofske (2020) 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.

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, 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—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,

[10] See here.

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

virtual inspections are typically tagged: "VIRTUAL INSPECTION – COVID-19".[12]

In total, 3,496 inspections are tagged as virtual. My primary interest lies with establishments whose inspections were immediately and temporarily shifted to remote format at its introduction.[13] As such, establishments are considered treated if they receive at least two consecutive virtual inspections beginning in 2020, and I restrict attention to establishments observed in at least two inspections before May 20, 2020. Among such establishments, there are 118 inspections that, despite the establishment's prior and next inspections being virtual, are not tagged as virtual, raising misclassification concerns. However, Appendix Figure A1 shows the distribution of these inspections by month of sample. Of these inspections, 109 occur in, or after, May 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, explaining the subsequent virtual inspections. Thus, all 111 of these inspections after 2020 are presumed correctly specified. Yet, the 7 of these inspections 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, and two inspections after, May 19, 2020. Observations are excluded from 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*, or *Food Processor* permit, as each category contains one anomalous treated establishment.

[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.

[13]There are a small number of establishments that, despite primarily receiving on-site inspections, do receive a single virtual inspection during this time due to employees' recent COVID exposure. Ultimately, 53 such establishments are excluded from all analyses.

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

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 sanitizer-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 1,055 on-site inspections of treated establishments, that occur after the establishment has received a virtual inspection(s). 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 estimate 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), 14-day period and establishment fixed effects 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 a quarterly trend; all results are very similar.

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 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.

[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.

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 compliance 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 52.5% decrease due to anticipation. 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 affect treated and untreated establishments differently. Between March 9 and April 9, 2020, I observe 115 on-site inspections of treated establishments already exposed to the pandemic (virtual inspections began May 20, 2020). Columns (3) and (4) of Table 2 report estimates analogous to columns (1) and (2), but include an interaction of $(Treated_i \times COVID_{i,j})$, where $COVID$ is a binary variable equaling 1 after March 8, 2020. In column (4), $\widehat{\alpha}_2$ represents a 50.4% 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 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 effect of learning through treatment: (i) that effect would likely persist to some extent in subsequent on-site inspections (which estimates of α_1 contradict), and (ii) that effect 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 97.5% of the effect estimated among all virtual inspections in column (2). As an additional test, column (5) reports estimates of equation (1) under a restricted sample that ends following either: treated establishments' first virtual inspections, or untreated establishments' first inspections after May 19, 2020. This estimates an effect very similar to column (2), and also challenges the plausibility of *any* learning effect in the initial α_2 estimates.

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.

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

[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."

sample: 1,256 (about 8.8%) never participate; 4,367 (about 30.6%) always participate; and the remainder chose each option at least once.[17] To assess whether participation decisions in virtual inspections are consistent with avoidance, I estimate equations (1) and (3), with $Disc_{i,j}$, a binary variable indicating establishment i chose disclosure participation in inspection j, as the outcome. These estimates are reported in Table 3, and suggest a statistically significant increase in disclosure propensity in virtual inspections. Across all virtual inspections, a 6.5% increase is estimated relative to a pre-treatment average of 0.800. While modest in magnitude, the direction of this change is consistent with opportunistic use of anticipation ability.

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 probability returns to the pre-treatment level, 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 inspections are excluded in estimation, as are all inspections from treated establishments not observed in a post-treatment on-site inspection. The coefficient of interest, $\widehat{\alpha}_1$, estimates

[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.

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, $\alpha_1 > 0$.

These estimates are reported in Table 4. As expected, detected-violation counts are substantially lower in virtual inspections. In column (3), with all controls included, relative to the pre-treatment average of 0.660, a 38.6% decrease in detected violations is estimated. Further, that decrease is more than offset by the return of unannounced on-site visits. Consistent with general deterrence, establishments' initial post-treatment on-site inspections detect violation counts that exceed the pre-treatment average by 25%. Columns (4) and (5) of Table 4 report similar estimates using the severity-adjusted count of violations, $y^a_{i,j}$ as the dependent variable. With all controls included, severity-adjusted violation counts in establishments' initial post-treatment on-site inspections are 14.3% higher than the pre-treatment average, although that difference is only weakly significant. Finally, Appendix Table A4 reports the same estimates but with virtual inspections dropped in estimation. In all cases, estimates of α_1 are very similar to those in Table 4.

6 Concluding Remarks

General deterrence through imperfect monitoring is essential to enforcing a profound body of regulation. Yet, the theory of general 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 empirical 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, establishments with no prior history of anticipation ability suddenly acquire it, as opposed to prior work where anticipation ability stems from long-standing practices. I find that avoidance behavior is immediate, suggesting that even sporadic provision of anticipation ability might significantly undermine enforcement of food-safety regulation.

I also find that 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%. Moreover, considerable heterogeneity underlies this average effect. Notably, establishments that were highly compliant in observed pre-treatment inspections, and with permit types in lower risk classes, appear largely unresponsive to the reduction in detection probability and expected cost. Redirecting some inspections away from these establishments, and toward targets in the highest risk class, could significantly improve how inspections are allocated. Moreover, if targeting is explicitly tied to detected noncompliance, beyond improving inspection allocation, enhanced general deterrence should further reduce noncompliance costs. Existing MCESD inspection resources appear sufficient to comfortably accomplish this through a straightforward dynamic-enforcement policy.

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.[18] 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.[19] 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.

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

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

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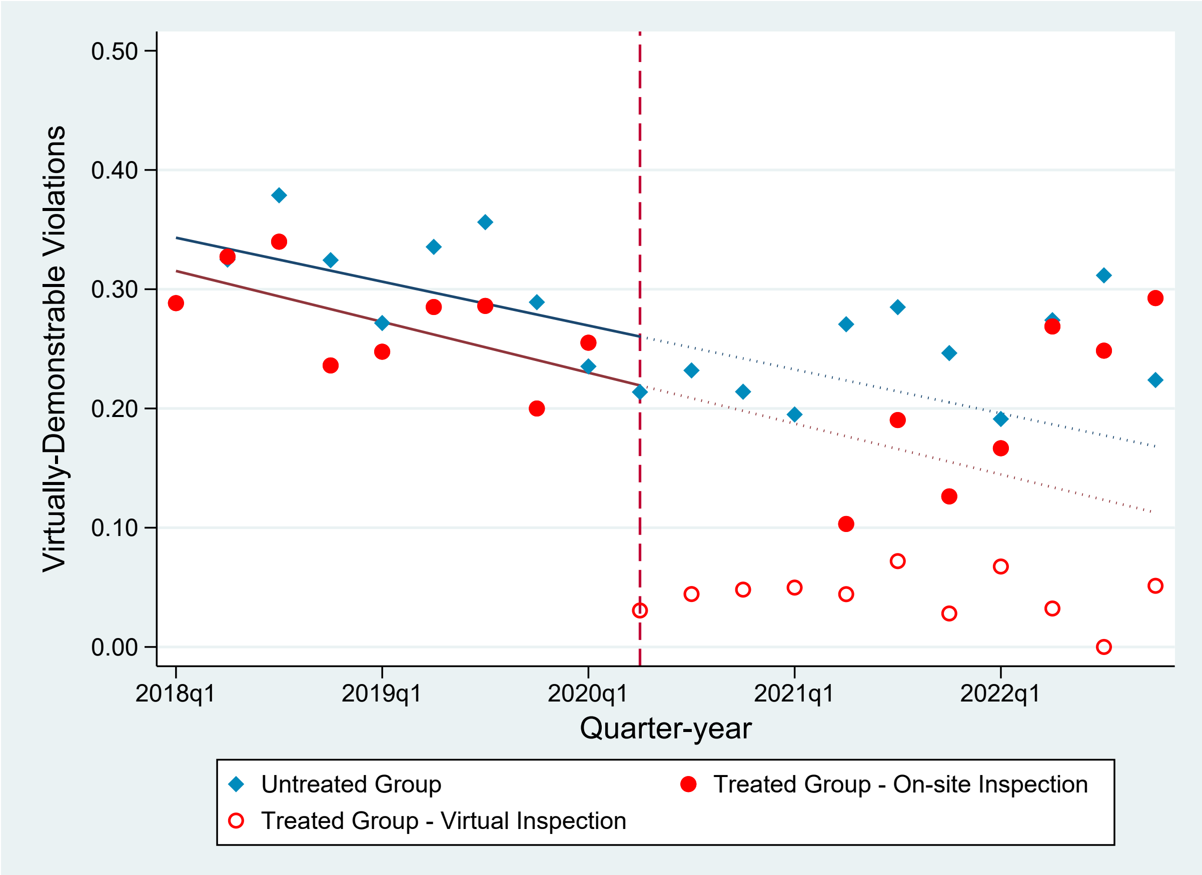
Figure 1: Inspection Format and Detected Violations



Average $y^d_{i,j}$ by quarter-year of sample. The "treated group" are establishments 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—26 and 9, respectively, whereas there were 126 and 289 such inspections in 2021q2 and 2021q3.

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	0.001	0.000
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.003)
Trend	-0.003***	0.012	-0.010***	0.025	-0.008***	0.022
	(0.001)	(0.010)	(0.002)	(0.019)	(0.002)	(0.015)
Treated	-0.032		-0.411***		-0.237***	
	(0.031)		(0.064)		(0.049)	
14-day period FE		✓		✓		✓
Establishment FE		✓		✓		✓
R-squared	0.002	0.368	0.006	0.502	0.005	0.470
N	74,113	74,113	73,974	73,974	73,974	73,974

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

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 and equals 1 in January 2018. Estimating sample in columns (3), (4), (5), and (6), excludes treated establishments that are not observed in a post-treatment on-site inspection.

Table 2: ANTICIPATION ABILITY AND DETECTED COMPLIANCE

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) $y_{i,j}^d$
$(1 - \textit{Virtual}) \times \textit{Post}$	-0.006	0.001	0.031	0.001	0.001	
	(0.022)	(0.021)	(0.041)	(0.031)	(0.031)	
Virtual	-0.136***	-0.142***	-0.099**	-0.142***	-0.138***	-0.131***
	(0.017)	(0.017)	(0.038)	(0.028)	(0.036)	(0.036)
$\textit{Virtual} \times \textit{Post}_{j-1}$					-0.005	
					(0.026)	
$(\textit{Treated} \times \textit{COVID})$			-0.039	0.000	0.000	-0.002
			(0.040)	(0.031)	(0.031)	(0.031)
Treated	-0.039**		-0.038**			
	(0.016)		(0.017)			
14-day period FE	✓	✓	✓	✓	✓	✓
Establishment FE		✓		✓	✓	✓
Month-of-year FE		✓		✓	✓	✓
Day-of-week FE		✓		✓	✓	✓
R-squared	0.013	0.271	0.013	0.271	0.271	0.340
N	155,362	155,362	155,362	155,362	155,362	88,388

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

OLS estimates of equations (1) and (3). Standard errors, clustered two-way on establishment and 14-day period, are reported in parentheses. $y_{i,j}^d$ 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: ANTICIPATION ABILITY AND DISCLOSURE DECISIONS

Variable	(1) $Disc_{i,j}$	(2) $Disc_{i,j}$	(3) $Disc_{i,j}$	(4) $Disc_{i,j}$
$(1 - Virtual) \times Post$	0.042**	0.042**	0.143***	0.144***
	(0.016)	(0.016)	(0.041)	(0.041)
$Virtual$	0.053***	0.052***	0.154***	0.128***
	(0.014)	(0.014)	(0.040)	(0.040)
$(Treated \times COVID)$			-0.105***	-0.105***
			(0.040)	(0.040)
14-day period FE	✓	✓	✓	✓
Establishment FE	✓	✓	✓	✓
Month-of-Year FE		✓		✓
Day-of-Week FE		✓		✓
R-squared	0.556	0.556	0.556	0.556
N	155,352	155,352	155,352	155,352

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

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. Column (4) restricts the sample of treated establishments to those observed in a pre-treatment COVID-period on-site inspection.

Table 4: Testing Deterrence

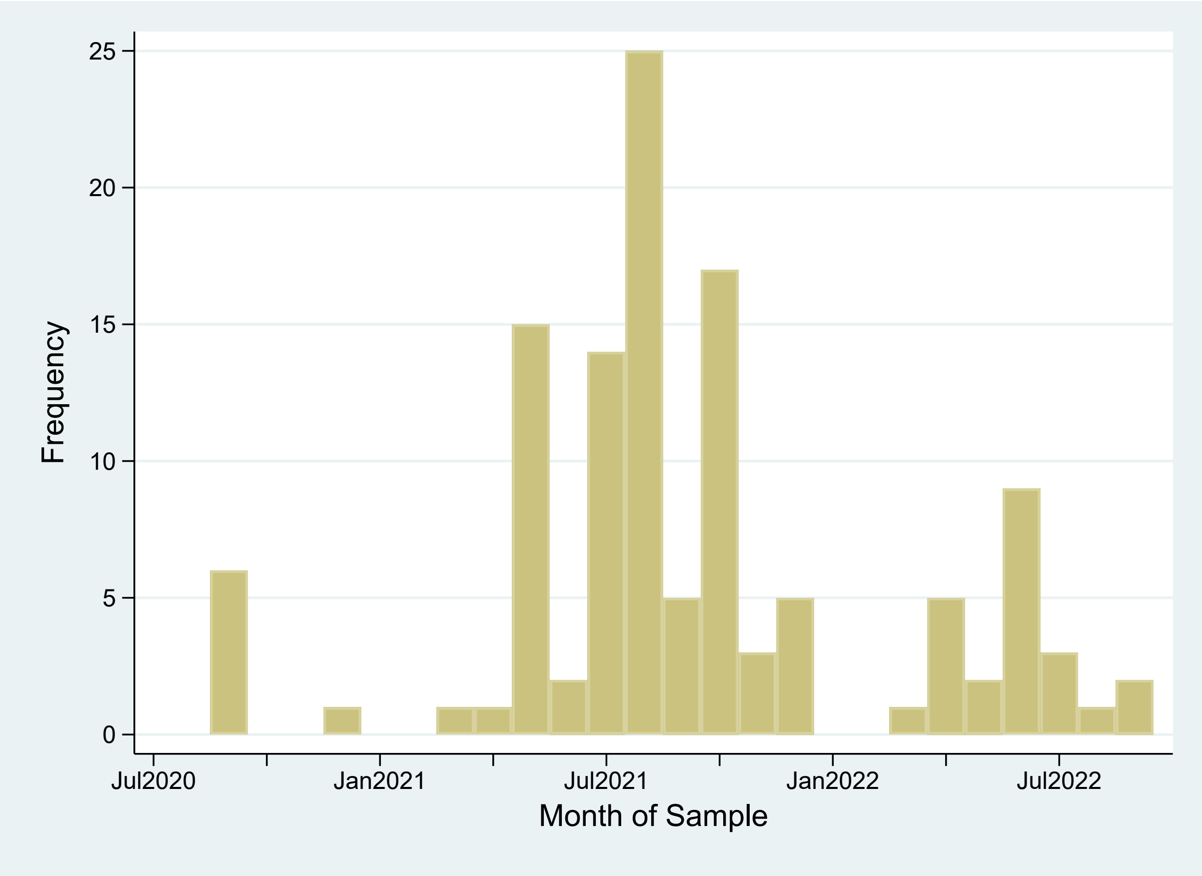
Variable	(1) $y_{i,j}$	(2) $y_{i,j}$	(3) $y_{i,j}$	(4) $y^a_{i,j}$	(5) $y^a_{i,j}$
$(1 - \mathit{Virtual}) \times \mathit{Post}$	0.121**	0.162***	0.165***	0.069*	0.072*
	(0.058)	(0.045)	(0.045)	(0.037)	(0.037)
Virtual	-0.249***	-0.258***	-0.255***	-0.195***	-0.192***
	(0.040)	(0.039)	(0.039)	(0.032)	(0.032)
Treated	-0.379***				
	(0.039)				
14-day period FE	✓	✓	✓	✓	✓
Establishment FE		✓	✓	✓	✓
Month-of-year FE			✓		✓
Day-of-week FE			✓		✓
R-squared	0.022	0.403	0.403	0.372	0.372
N	153,430	153,430	153,430	153,430	153,430

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

OLS estimates of equation (1). 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. Estimating sample: for untreated establishments, all inspections; for treated establishments, all inspections prior to, and including, their first post-treatment on-site inspection. Treated establishments with no observed post-treatment on-site inspections are excluded.

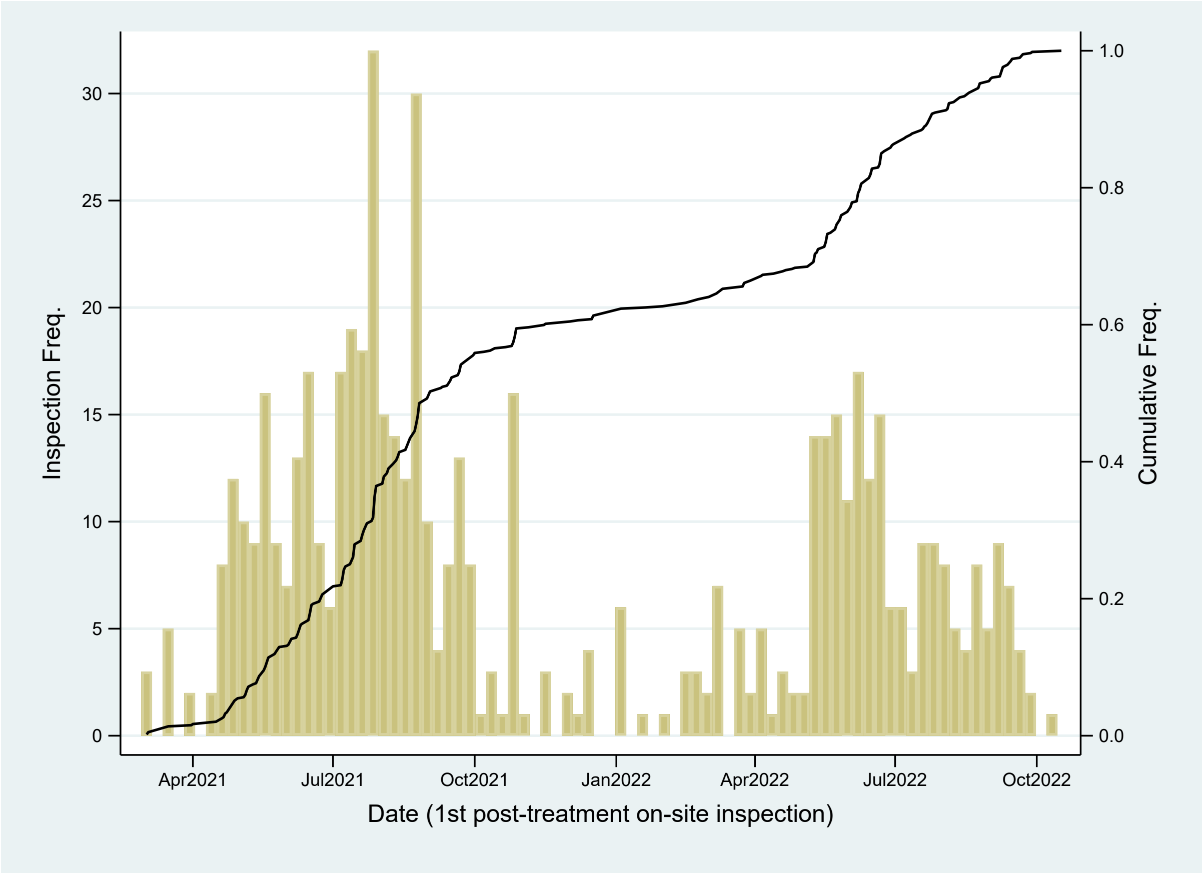
A1 Appendix

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 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).

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	133
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	0
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	166

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 COMMON TRENDS ASSUMPTION

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	-0.000	0.010	0.007	0.002	0.001
	(0.005)	(0.005)	(0.010)	(0.009)	(0.008)	(0.008)
Quarterly Trend	-0.009***	0.014	-0.031***	-0.000	-0.024***	0.007
	(0.003)	(0.015)	(0.007)	(0.038)	(0.005)	(0.027)
Treated	-0.026		-0.409***		-0.232***	
	(0.032)		(0.068)		(0.052)	
14-day period FE		✓		✓		✓
Establishment FE		✓		✓		✓
R-squared	0.002	0.368	0.006	0.502	0.005	0.470
N	74,113	74,113	73,974	73,974	73,974	73,974

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

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) $y_{i,j}^d$	(2) $y_{i,j}^d$	(3) $y_{i,j}^d$	(4) $y_{i,j}^d$	(5) $y_{i,j}^d$	(6) $y_{i,j}^d$
$(1 - \mathit{Virtual}) \times \mathit{Post}$	-0.007	-0.002	0.055	0.015	0.015	
	(0.021)	(0.020)	(0.039)	(0.032)	(0.032)	
Virtual	-0.143***	-0.150***	-0.081**	-0.133***	-0.129***	-0.122***
	(0.017)	(0.017)	(0.037)	(0.029)	(0.036)	(0.035)
$\mathit{Virtual} \times \mathit{Post}_{j-1}$					-0.005	
					(0.025)	
$(\mathit{Treated} \times \mathit{COVID})$			-0.064*	-0.018	-0.018	-0.018
			(0.038)	(0.033)	(0.033)	(0.031)
Treated	-0.002		0.000			
	(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	186,104	186,104	186,104	186,104	186,104	106,024

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

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. $\mathit{Post}_{i,j-1}$ equals 1 in all inspections after an establishment's first virtual inspection, and 0 otherwise.

Table A4: Testing Deterrence: Virtual Inspections Excluded

Variable	(1) $y_{i,j}$	(2) $y_{i,j}$	(3) $y_{i,j}$	(4) $y_{i,j}^a$	(5) $y_{i,j}^a$
$(1 - \mathit{Virtual}) \times \mathit{Post}$	0.120**	0.185***	0.188***	0.087**	0.089**
	(0.058)	(0.045)	(0.045)	(0.036)	(0.036)
Treated	-0.379***				
	(0.039)				
14-day period FE	✓	✓	✓	✓	✓
Establishment FE		✓	✓	✓	✓
Month-of-year FE			✓		✓
Day-of-week FE			✓		✓
R-squared	0.016	0.401	0.402	0.371	0.371
N	150,941	150,941	150,941	150,941	150,941

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

OLS estimates of equation (3), but with all virtual inspections excluded in estimation. 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. $y_{i,j}^a$ is a severity-adjusted count of all violations in which each core violation adds only 0.25.