Division of Economics and Business
Working Paper Series

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Working Paper 2020-01
http://econbus-papers.mines.edu/working-papers/wp202001.pdf

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January 2020

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ABSTRACT
The traditional theory of firm regulation and enforcement examines the interplay of firms and regulator, with citizens as passive consumers of goods or providers of votes. However, in industries such as oil and gas, citizens can play an important role in inspections and enforcement, which we analyze with a novel dataset of Colorado regulatory activities. We find regulators frequently conduct follow-up inspections of citizen complaints, and these citizen-driven inspections are just as likely to lead to regulatory action as "normal" scheduled inspections. However, the evidence is consistent with regulators treating these complaints as "one-offs" — regulators do not increase inspection activity of other wells owned by a firm that was complained about. An inspector conducting a complaint inspection crowds out two regular inspections at the daily level, but we find no evidence of crowd-out at time scales of one month or greater. Finally, heterogeneity across complaint types suggests citizens are particularly adept at identifying nuisance-related violations (e.g. noise, smell), but are less adept at identifying more technical violations.

JEL **classifications: Q58, Q48, K42**
Keywords: enforcement, oil and gas, citizen participation

*We gratefully acknowledge comments by Mary Evans, Laura Grant, Zach Raff, Jay Shimshack, and attendees of the AERE 2019 Summer Conference and 2019 Colorado Energy Camp.

1 Introduction

What role do citizens play in enforcement of environmental regulations? In a typical analysis of environmental regulation, firms maximize profits while the regulator seeks to determine (via monitoring) and enforce (via penalties) compliance with environmental or other regulations. In this framework, citizens simply have demands for goods, suffer harms from bads, or vote reflecting their environmental preferences. This modeling framework accurately represents many industries, particularly those with a limited number of sites. However, this framework is incomplete for other sectors such as oil and gas, where the number of sites may number in the hundreds of thousands, and citizen complaints constitute an important and poorly understood formal part of the regulatory process.

This paper focuses specifically on how citizen complaints affect the regulatory agency's actions: Do citizen complaints lead to additional inspections with greater assessed penalties? Does following up on citizen complaints crowd out regular inspection activity? Do citizen complaints update regulators priors about a firm's compliance? Are citizens good at finding violations? We leverage a rich dataset of all inspections, penalties, and formal complaints by Colorado's oil and gas regulatory agency, the Colorado Oil and Gas Conservation Commission, from 2012 to 2018 totaling 879 complaints and over 100,000 inspections.[1]

The findings from our analysis suggest regulators take a nuanced view of complaints: (1) conditional on firm fixed effects, an inspection following up on a complaint is generally not more or less likely to lead to penalties than a regular inspection, (2) each complaint inspection crowds out two regular inspections at a daily level, but does not crowd out or

[1] Importantly, our results generally rely on observational data with no natural experiments or clearly exogenous sources of independent variation, and while we are able to leverage fixed effects and demographic controls to eliminate many potential confounding variables, our results should be interpreted accordingly.

crowd in regular inspections at longer time scales, (3) citizen complaints are typically about nuisance-related potential violations such as odors and noise, and (4) complaint inspections are more likely lead to penalties when the complaints are about odors, but not noise. The above findings suggest citizen complaints have a real but limited role to play in environmental regulation. On the one hand, citizens are home (and therefore near wells) far more frequently than inspectors are on site, and citizens do seem to be able to effectively identify some classes of violations. Furthermore, fears that complaints crowd out the agency's ability to perform other inspections seem unfounded, though there may be adjustment on unobserved margins such as overtime budgets or inspection intensity. On the other hand, citizen complaints do not seem to identify otherwise unknown (to the regulator) "bad actor" firms or sites, nor do they uncover more technical violations.

This work fits in to a larger literature on monitoring and enforcement and determinants of regulatory behavior (surveyed in Gray and Shimshack (2011)). A few papers study the role of private actors. Earnhart (2000) provides evidence Czech water authorities responded to citizen complaints (or "suits") in a rational manner, Buntaine et al. (2019) find a similar program in Uganda did lead to long-term improvements, and Evans and Shimshack (2019) study complaints about industrial air pollution in Texas. Goeschl and Jürgens (2012) provide a theoretical framework highlighting that citizen complaints can improve or harm environmental quality depending on key parameter values. Scott (2018) examines firms and citizens, finding a positive association between citizen complaints and firm's voluntary management practices. Other related work examines how private lawsuits (Langpap and Shimshack 2010; Ashenmiller and Norman 2011) and environmental NGO's (Grant and Grooms 2017; Grant and Langpap 2018) affect environmental outcomes and monitoring activities. An important and distinct feature of citizen complaints is that they are private monitoring, but rely on public enforcement. We outline the conceptual framework underlying our analysis of citizen complaints in the next section.

2 Conceptual framework

Understanding how citizen complaints affect firms' compliance with environmental regulations, and if it would be advisable for regulatory bodies to encourage more citizen complaints, is a challenging problem both from a theoretical and an empirical perspective. On the theoretical side, the interactions between regulators, citizens and firms introduce substantial complications compared to the "simpler" problem of modeling regulation between just a firm and a regulator. Here, the regulator also receives complaints from citizens about the firm (Scott 2018), and they may choose to follow-up on those complaints with inspections.

Counterintuitively, more vigilance and more complaints by citizens can actually lead to worse compliance by firms (Goeschl and Jürgens 2012). Regulatory agencies have finite budgets and responding to citizen complaints requires time and effort; follow-up inspections of citizen complaints could crowd out regular inspections, leading to fewer total inspections of firms, and thus less compliance by firms. While the relationship between firm compliance and actual environmental quality is complex, less compliance by firms would likely degrade environmental quality, which would be exacerbated if citizens are particularly bad at actually identifying violations. On the other hand, if citizens are good at identifying violations, and it is not too costly for inspectors to follow up on complaints, then compliance may be improved by complaints. This would be enhanced if citizen complaints provide additional information regulators can act on by identifying "bad actors" which regulators could target in subsequent inspections.

On the empirical side, many of the features one would like to include in an econometric model are either hard to measure or fundamentally unobservable. Furthermore, natural experiments or clearly exogenous sources of independent variation are not readily available. Nonetheless, our fixed-effects empirical model provides useful insight into the contours of the complex relationship between regulators, firms and citizens. Based on the conceptual model above, we conjecture that citizen complaints are likely good for firm compliance if the following testable predictions hold in our empirical exercise: i) citizens are good at finding violations, ii) complaints do not crowd out regular inspector activity, and iii) complaints identify "bad actors". We test these predictions using Colorado data below.

3 Data

This paper primarily uses two data sets from the Colorado Oil and Gas Conservation Commission (COGCC), the agency with primary oversight of oil and gas production in Colorado. The first dataset describes all inspections of oil and gas wells in Colorado from 2012-2018. This includes the well inspected, the date, and whether the inspection resulted in a "corrective action." We observe 109,517 inspections from 2012-2018.

We also use a database of all complaints filed with COGCC about oil and gas wells. We observe 879 complaints about oil and gas wells from 2012-2018. Each record includes the date of the complaint, which well is complained about, the text of the complaint, and how COGCC categorized the complaint (e.g. noise, odor, lighting). We assume an inspection was a followup to a complaint if it is of the same well within 30 days of the complaint.[2]

We use "corrective actions" to measure penalties. A corrective action is a formal step in which the regulator documents a problem and allows the firm the opportunity to fix it. If the firm does not satisfactorily address the problem, the regulator may then proceed to levy fines. Fines are relatively rare, so corrective actions provide more statistical robustness and are intended to induce firm compliance. If there is a corrective action, we observe the inspector's description of the problem.

We also use three supplemental datasets. Enverus (formerly Drillinginfo) provides detailed well data that we use to construct measures of the number of wells for a firm or inspector area (discussed more in Appendix A), as well as to merge with inspector districts provided by COGCC. COGCC inspectors typically have responsibility for inspections in a defined area (Appendix Figure C1). We do not observe which inspector performed a given inspection, but inspection district fixed effects are a good proxy measure for inspector and may also proxy for geographic variation in culture or other unobserved characteristics. Finally, we use Census-tract level demographic and economic characteristics as control variables (see Appendix A and B).

We use these raw datasets to develop several datasets for our analysis. In Section 4.1, we use all inspections, merged with inspector districts and Census data. In Section 4.2 and 4.3, we collapse our inspection data to the inspection district, firm, and inspection-district

[2] Based on conversations with agency staff, the agency typically follows up within a day of a complaint, and formal inspections are frequently carried out within a few days of a complaint. The unconditional probability of well inspection in a month is 2%, so an inspection shortly after a complaint is unlikely to be happenstance. Nonetheless, we vary this window and find similar results using 7 and 60 day windows. An additional wrinkle is that a complaint may be matched to multiple inspections during the 30 (or 7 or 60) day window. Typically this is because inspectors conduct a formal inspection in response to a complaint, and then follow up shortly thereafter to verify identified problems have been fixed. As such, the number of complaint inspections may exceed the number of complaints filed.

firm levels at various time frequencies. In each, we calculate the number of non-complaint inspections as the number of inspections minus the number of complaint inspections. Finally, in Section 4.4, we examine the raw complaint data to examine what topic the complaint covers (e.g. noise, smells, etc), and the raw inspections data to examine the inspectors' descriptions of problems in the corrective actions.[3]

Table 1 presents simple summary statistics. We observe over 100,000 inspections of about 40,000 unique wells, which is about half of the wells in Colorado. Across the 18 inspector districts, inspectors averaged about 4 inspections per workday for the period of our sample. One quarter of non-complaint inspections result in corrective actions, while one third of complaint inspections do. All regression specifications are described in detail in Appendix A.

4 Results

Our first notable result is that citizen complaints lead to followup inspections by regulators. Half of complaints lead to formal inspections within 30 days, compared to a well's unconditional monthly probability of inspection of approximately 2 percent. However, this followup varies by complaint type - about 85% of complaints about spills and 60% of complaints about water lead to formal inspections, compared to about 40% of complaints about noise and 10% of complaints about odor.

[3] We also used machine learning text processing methods to identify topics of complaints from the text of the complaints. Results matched very closely with agency categorizations, so we use agency categorizations for simplicity.

Table 1: Summary Statistics

	(1) Complaint Inspections	(2) Non-Complaint Inspections	(3) Total
Number of Inspections	1,311	108,206	109,517
P(Corrective Action)	32.6%	23.6%	23.7%
Number of Inspector Districts			18
Number of Wells			40,784

Summary statistics are for the regression sample. It is typically estimated that there are approximately 80,000 wells on Colorado, including abandoned wells.

4.1 Complaint Inspections Lead to Penalties

Table 2 shows the conditional odds ratio that a complaint inspection results in penalty, relative to non-complaint inspections. All specifications include demographic controls as well as month-by-year fixed effects. In the absence of firm or inspection district fixed effects, we find complaint inspections are 1.491 times as likely as non-complaint inspections to lead to a penalty. However, after we include firm fixed effects in Column 2, the odds ratio is substantially lower. This suggests firms' unobserved propensity to be penalized is positively correlated with complaint inspection likelihood.

Examining Columns 3 and 4, we see that inclusion of inspection district fixed effects further reduces the odds ratio. This suggests that spatial (district) or inspector effects are also positively correlated with both penalties and complaints. While we control for some

Table 2: Complaints Lead To Penalties

	(1)	(2)	(3)	(4)
Odds Ratio of Penalty	1.491***	1.145	1.374***	1.013
	(0.168)	(0.107)	(0.139)	(0.102)
Fixed Effects		Firm	Inspection District	Firm x Insp Dist
Observations	93860	93860	79646	79646

We report odds ratios, or exponentiated coefficients from logistic regressions. The null hypothesis is that the Odds Ratio is 1. Control variables include demographic controls as well as month-by-year fixed effects and group fixed effects as labeled. Appendix A describes the full regression model. Robust standard errors are clustered at the level of firm-inspection district in Column (1) and at the level of the fixed effect in Columns (2)-(4). Robust standard errors are clustered at the level of the fixed effect for Columns 2-4. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

demographics, unobserved inspector stringency or regional political power might explain the pattern. In our most exhaustive specification in Column 4, the odds ratio is a precise one, with a 95% confidence interval of 0.8-1.2. This result is consistent with regulators choosing which complaints to follow up on and effectively triaging which complaints to follow up on.

We find firm identity (fixed effect) explains a substantial portion of the variation in penalties and complaints (12% of each), but inspector district fixed effects only explain about 4% of variation in penalties and 1% of variation in complaints. Moreover, the fixed effects from both firm regressions are positively correlated ($p < 0.001$). This suggests some firms are simply more prone to complaint inspections and penalties, perhaps because citizens and the regulator observe the same (lack of) firm compliance effort.

4.2 No Evidence that Complaints Crowd Out Other Inspections at Long Time Scales

We find evidence that complaint inspections crowd out other inspections in the short-term, but not long-term. In Table 3, we report the effect of one more complaint inspection per inspector district on the number of non-complaint inspections over varying time horizons. Column 1 shows one more complaint inspection is associated with two fewer non-complaint inspections on the same day, with a 95% confidence interval excluding both zero and one. While clearly inspectors cannot be in two places at once, this suggests complaint inspections take more time than regular inspections.[4] In Column 2, we see that one additional complaint inspection is associated with about one-third fewer non-complaint inspections at the weekly level. By contrast, in Columns 3-5, one more complaint inspection is not associated with fewer non-complaint inspections at the monthly, quarterly, or yearly level. These results suggest the regulator can adjust on other margins in order to conduct needed inspections, though we do not observe whether those margins are increased overtime, reducing effort per inspection, use of additional floating staffers, etc.

Point estimates in Columns 3-5 are near zero, and the confidence intervals are small - less than 0.05 in absolute magnitude and we can reject the null that one additional complaint inspection leads to as much as one-eighth fewer non-complaint inspections over the course of a month, quarter, or year. While the number of observations is modest, these regressions

[4] The greater time investment may be due to speaking with the complainant, increased travel times, or other logistical factors.

are sufficiently well powered to reject even moderate effect sizes.

Table 3: Change in Regular Inspections by Inspector District

	(1)	(2)	(3)	(4)	(5)
	Daily	Weekly	Monthly	Quarterly	Yearly
Δ Insp per Complaint Insp	-2.057***	-0.326**	0.00434	0.0446	-0.0310
	(0.300)	(0.156)	(0.0594)	(0.0425)	(0.0209)
Observations	13928	5246	1463	503	126

Values describe the change in the expected number of non-complaint inspections by inspector district from one additional complaint inspection. This is calculated from a poisson model which also includes the total number of wells and fixed effects for time step (month in daily specification) and inspector district. Appendix A describes the full regression model. Robust standard errors are clustered at the inspector district level. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

4.3 No Evidence that Complaints Crowd In Other Inspections

If regulators believe firms that received citizen complaints are likely to have other problems ("bad actors"), then complaints may have a broader social benefit through a positive information externality. In this case, we would expect to see complaints lead to increases in inspections of other wells owned by the same firm. However, we find no evidence complaints lead to increased inspections of the same firm. In Table 4, we show the change in noncomplaint inspections at the firm-inspector district level from one additional complaint inspection, and in Table 5 we show results at the firm level (statewide). We find one additional complaint inspection is associated with a small decrease in inspections at the firm-district-daily and firm-district-weekly levels, but with (precisely) no change at other temporal scales. This is consistent with regulators not updating their beliefs about firms in response to complaints. Note, there is some mechanical crowd out at the inspector district-date level - intuitively, we would expect the coefficient to be approximately the point estimate of -2.0 from Column (1) of Table 3 divided by the average number of firms per district (34), or $-2.057/34 \approx -0.0605 \approx -0.0619$. This small effect persists at the weekly level. There is no evidence of crowd-in at statewide scale or longer timescales, and an effect size one-fifth of the daily effect is outside the 95% confidence interval in Columns 3-5 for Tables 4 and 5.

Table 4: Change in Regular Inspections by Firm-Inspector District

	(1)	(2)	(3)	(4)	(5)
	Daily	Weekly	Monthly	Quarterly	Yearly
Δ Insp per Complaint Insp	-0.0619***	-0.0242***	-0.00192	0.000601	-0.000311
	(0.00984)	(0.00555)	(0.00243)	(0.00180)	(0.000967)
Observations	24593	17412	10663	6457	2860

Values describe the change in the expected number of non-complaint inspections by inspector district-firm from one additional complaint inspection. This is calculated from a poisson model which also includes the total number of wells and fixed effects for time step (month in daily and weekly specifications) and inspector district-firm. Appendix A describes the full regression model. Robust standard errors are clustered at the firm-inspector district level. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

These results are consistent with the an explanation that regulators do not view complaints as providing important new information about firms. Viewed in conjunction with the results of Section 4.1, which find firms' complaint rates and penalty rates are correlated, this

Table 5: Change in Regular Inspections by Firm

	(1) Daily	(2) Weekly	(3) Monthly	(4) Quarterly	(5) Yearly
Δ Insp per Complaint Insp	-0.0608***	-0.0104**	0.00210	0.000973	0.0000939
	(0.0173)	(0.00469)	(0.00167)	(0.00151)	(0.000845)
Observations	21316	13286	7265	4105	1688

Values describe the change in the expected number of non-complaint inspections by firm from one additional complaint inspection. This is calculated from a poisson model which also includes the total number of wells and fixed effects for time step (month in daily specification) and firm. Appendix A describes the full regression model. Robust standard errors are clustered at the firm level. $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$

suggests citizens may be equally good as the regulator at identifying "bad actor" problem firms, but the regulator already has appropriate beliefs about the firms and does not further update those beliefs.

4.4 Nuisances Are Important

Complaints are primarily about nuisance-related phenomena - for example, approximately 40% of complaints are about noise or odor, while fewer than 1% of corrective actions mention noise or odor. Conversely, 16% of corrective actions focus on signs or placards at the wellhead (which can be important for emergency responders), 7% mention contaminated soil at the wellsite (typically from spills), and 2% mention violations of well pressure. These problems seem to be unimportant or unobservable to citizens, as only three complaints are about signage, 2% are about on-site soil contamination, and none are about well pressure.[5]

It may be difficult for citizens to know whether what they observe is in violation of the law, instead of merely being unpleasant. If a nearby resident can smell odors from a drilling operation, those chemical concentrations are likely illegal.[6] By contrast, the noise limit can be as high as 90 decibels - comparable to a motorcycle at 25 feet - potentially far above the level which could annoy a neighbor.[7] Controlling for year, month of year, and inspector district, odor complaints are 3.3 times as likely as other complaints to result in penalties ($p < 0.05$) while noise complaints are slightly less likely to result in penalties ($p \approx 0.75$).

5 Conclusion

Examining a novel dataset of Colorado citizen complaints and regulatory activities related to oil and gas wells, we find regulators frequently conduct follow-up inspections of citizen complaints. However, the evidence is consistent with regulators treating these complaints as "one-offs". Importantly, we find no evidence of crowd-out from citizen inspections at time scales of one month or greater. Taken together, this suggests complaints have a real but limited role to play in environmental regulation.

There are important caveats to this analysis. Methodologically, the relative rarity of complaints raises concerns about statistical power, though our estimates are reasonably precise.

[5] Two of the complaints about signage were from local government officials. Less than 2% of all complaints are by government officials - typically representatives of county or municipal governments.

[6] Per Colorado Department of Public Health and Environment Regulation 7.IV.B, VOCs must be below 10,000 ppm at the source. At 500 feet away, if evenly dispersed this would equate to much less than 0.0001 ppm, below the level that people can typically smell. https://www.colorado.gov/pacific/cdphe/aqcc-regs, retrieved 08/15/2019.

[7] COGCC Rule 802.b, https://cogcc.state.co.us/documents/reg/Rules/LATEST/800Series.pdf, retrieved Aug 15, 2019

Additionally, our results generally rely on observational data with no natural experiments or clearly exogenous sources of independent variation, though we control for observable demographics and a variety of fixed effects. Stepping back, these results describe a single agency regulating a single sector, and results may vary in other settings.

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A Econometric Models

A.1 Model for Section 4.1

Section 4.1 uses logit and fixed-effects logit models. Column 1 is based on a logit model of the form

$$P(CA_i = 1) = \frac{exp(\beta IsComplaint_i + \alpha X_t + \gamma_m)}{1 + exp(\beta IsComplaint_i + \alpha X_t + \gamma_m)} \tag{1}$$

where $CA_i = 1$ if and only if there was a corrective action issued at inspection i, $IsComplaint_i = 1$ if and only if inspection i resulted from a complaint, X_t is a set of tract-level demographic control variables, and γ_m is a vector of month-by-year dummies. We then report the estimated odds-ratio for $IsComplaint_i$, or $e^{\hat{\beta}}$. Robust standard errors are clustered at the level of the fixed effect (if any). For columns 2-4, we add fixed effects for firm, inspection district, and firm-inspection district pairs. Our regression equation becomes

$$P(CA_i = 1) = \frac{exp(\beta IsComplaint_i + \alpha X_t + \gamma_m + \alpha_j)}{1 + exp(\beta IsComplaint_i + \alpha X_t + \gamma_{my} + \alpha_j)} \tag{2}$$

where j indexes fixed effects at the respective levels. Again we report odds ratios. Robust standard errors are clustered at the level of the fixed effect for fixed effects specifications, and at the firm-inspection district level in specifications without fixed effects.

These regressions control for socioeconomic status and demographic measures for the Census tract containing the well being inspected. Control variables X_t are tract-level per capita income (in case high income households are able to more effectively access the regulatory system), population density (as a proxy for potential harms from incidents), fraction of households with children under 18 (as a proxy for potential harms from incidents), and fraction of households speaking English as a primary language (based on Vissing (2015)'s finding that non-English speaking households experience discrimination in oil and gas leasing). We use the 2012 5-year ACS sample. We use controls from the beginning of our study period instead of yearly controls in case these control variables are changing in response to the industry, though we expect changes over our relatively short study period are likely limited.

A.2 Model for Sections 4.2

Section 4.2 uses fixed-effect poisson models of the form

$$E[insp_{dt}] = exp(\beta ComplaintInsp_{dt} + \delta Wells_{dt} + \gamma_t + \alpha_d) \tag{3}$$

where d indexes inspection districts and t indexes time. The outcome variable $insp_{dt}$ is a count of the number of non-complaint inspections in district d at time t, $ComplaintInsp_{dt}$ is the number of complaint inspections, the control variable $Wells_{dt}$ is the number of existing wells (we should expect more inspections in districts with more wells), and γ_t is a time fixed effect - month for daily, weekly, and monthly regressions, quarterly and yearly for quarterly and yearly regressions. Robust standard errors are clustered at the inspector district level.

Poisson coefficients are approximately percentage changes (as they are changes in log points). We multiply by the average value of $insp_{dt}$ to get an (approximation of) $\frac{\Delta E[insp_{dt}]}{\Delta ComplaintInsp_{dt}}$.

A.3 Model for Section 4.3

Section 4.3 also uses fixed-effect poisson models, but uses the firm or firm-district as the unit of analysis. Allowing f to index firms, the regression equations become

$$E[insp_{fdt}] = exp(\beta ComplaintInsp_{fdt} + \delta Wells_{fdt} + \gamma_t + \alpha_{fd}) \tag{4}$$

and

$$E[insp_{ft}] = exp(\beta ComplaintInsp_{ft} + \delta Wells_{ft} + \gamma_t + \alpha_f) \quad (5)$$

Otherwise the analyses follow those of Section 4.2, with robust standard errors clustered at the firm-inspector district and firm level respectively.

B Demographic Selection

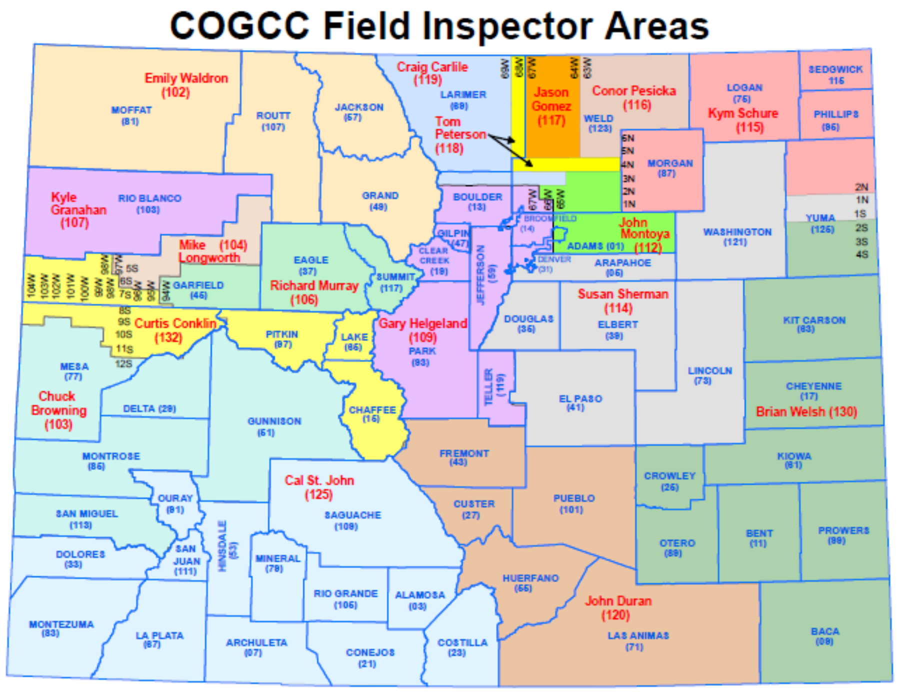
This section provides some evidence on the importance of local socioeconomic and demographic characteristics in our study. In Table C1, we report results of models analysing whether local characteristics affect the probability that a complaint is followed up on with a formal inspection (Columns 1 and 2), and of whether the probability that a formal complaint inspection leads to a violation (Columns 3 and 4). We include local characteristics at the tract level, based on the 2012 5-year ACS Census sample. Columns 2 and 4 include fixed effects for inspector district.

In Table C2, we report results from regressing indicators for complaint type on local socioeconomic and demographic characteristics.

We see that generally there is only weak evidence for these socioeconomic characteristics mattering, especially when conditioning on fixed effects. In particular, fixed effects largely control for selection into inspection based on income and presence of children. This suggests that omitted variable bias in our core regressions is likely modest, conditional on fixed effects. The important exception is that we find some evidence that the local fraction of households speaking English in the home is associated with a different set of complaint topics and a higher probability that complaints lead to violations. This merits further study, particularly in light of Vissing (2015)'s finding of discrimination in oil and gas leasing mediated by language.

C Appendix Figures and Tables

Figure C1: Inspection District Map



Map from COGCC as of May 2017

Table C1: Demographic Controls, Inspections, and Violations

	(1)	(2)	(3)	(4)
	Is Insp	Is Insp	Is Vio	Is Vio
Inc per capita	-0.0253***	-0.000849	0.000526	-0.00511
	(0.00409)	(0.00324)	(0.00451)	(0.00815)
English Speakers	-0.197	-0.256	1.145***	1.775***
	(0.288)	(0.211)	(0.294)	(0.555)
Pop Density	-0.134	-0.0531	-0.0880	0.176
	(0.0936)	(0.0628)	(0.150)	(0.160)
Have Children	1.338***	0.658*	0.945	-0.543
	(0.478)	(0.387)	(0.578)	(0.841)
Observations	692	609	295	226

Fixed effect specifications include inspection district and month-of-sample fixed effects. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table C2: Demographics and Complaint Type

	(1)	(2)	(3)	(4)	(5)	(6)
	Noise	Noise	Water	Water	Odor	Odor
Inc per capita	0.00261	-0.00357	0.0162*	0.00270	-0.00160	0.00625**
	(0.00290)	(0.00283)	(0.00939)	(0.00868)	(0.00251)	(0.00298)
English Speakers	-0.164	0.880***	2.759***	0.397**	-0.430***	-0.680***
	(0.157)	(0.286)	(0.815)	(0.186)	(0.164)	(0.214)
Pop Density	-0.0579*	-0.0879**	0.161	0.0629	-0.0760	-0.0449**
	(0.0327)	(0.0364)	(0.210)	(0.0537)	(0.201)	(0.0214)
Have Children	0.826	0.357	-1.119	0.292	-0.558*	0.830***
	(0.810)	(0.851)	(0.877)	(0.823)	(0.306)	(0.201)
Fixed Effects		Y		Y		Y
Observations	683	560	683	333	683	352

Fixed effect specifications include inspection district and month-of-sample fixed effects. Standard errors in parentheses.

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