

Service Cynicism: How Civic Disengagement Develops

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Abstract

How does civic disengagement develop? This article examines the theory that the dissatisfaction and disengagement citizens develop toward one government agency can extend to an alternative agency. Leveraging police precinct-level data on 311 calls and criminal complaints from 2004 to 2012 in New York City, it investigates whether government responsiveness to municipal issues predicts citizens' willingness to submit criminal complaints to the police. The study finds that predictors of disengagement with law enforcement extend beyond negative interactions with law enforcement alone. Rather, the time it takes local government officials to fix a 311 request for services, such as filling potholes and abating noise, shapes the likelihood that residents will file misdemeanor criminal complaints. Thus policymakers must account for the policy environment beyond their agency's domain to alleviate citizens' dissatisfaction and disengagement with government overall.

Keywords

civic engagement, participation, bureaucratic governance, service provision, law enforcement

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The basic insight of civic engagement—that government effectiveness requires both horizontal trust (citizen to citizen) and vertical trust (citizen to government)—is a fundamental principle of democratic governance.¹ For policymakers, cultivating vertical trust through repeated successful engagement and positive reciprocal interactions with their constituents is a primary concern: the government's execution of anticipated commitments stimulates citizens' positive disposition toward it and induces greater future reliance. Trust in government wavers, depending in part on citizens' evaluations of government effectiveness, and in theory government responsiveness should encourage positive engagement. However, government is not a singular entity, but instead comprises somewhat independent bureaucratic arms with varying levels of success in invoking citizen participation. Thus, a key question emerges about the nature of citizen engagement across different government bureaucracies: Does the dissatisfaction and disengagement citizens develop toward one agency extend to another, different agency?

This question is particularly salient in sectors of government that directly distribute goods and services for public consumption. Behind every policy decision, the question of how to facilitate the delivery of goods and services looms large. By socially constructing and differentially rewarding distinct target populations, policies stratify society by material goods, political incentives, and perceptions of political efficacy.² Wrong decisions about how to deliver services or the metrics employed to define effectiveness can engender a "confidence gap"³ and breed cynical citizens who believe government is using their authority against them, that they are being misunderstood or ignored by government, and that government is ineffective.⁴

This article argues that citizens evaluate government performance as a whole, and that their satisfaction with one agency informs their views of, and engagement with, other parts of government. As local governments move toward participatory government practices that rely on citizens co-producing services,⁵ the public's willingness to volunteer information and participation becomes even more central.⁶ However, a citizen's willingness to initiate contact with a particular agency is not motivated exclusively by past experiences with that agency, or solely by desires to improve community conditions and enforce social norms.⁷ Instead, we posit that citizens possess a holistic view of government, and that interactions with one agency, whether positive or negative, form the basis of the citizen's willingness to interact with another agency, even in a distinct policy domain.

We test our argument by looking at two avenues of citizen-initiated services—the 311 hotline for public services and criminal complaints to police—as a case study of our theory. We ask: Does government responsiveness to nonemergency, municipal requests for services predict criminal complaints to the police? We choose this pairing because although these two spheres of government and citizen interaction converge in the mechanism for contact, they dramatically diverge in their circumstances. In other words, both policing and municipal service requests require citizen initiation, but whereas 311 calls seek government assistance for mundane services, police complaints alert government to criminal conduct during emergencies. The latter also has particularly strong policy relevance, because citizens' compliance with law enforcement is necessary for public safety and effective governance.

We introduce a novel measure of citizens' perception of government competence—the length of time it takes to resolve 311 requests for services—and find that slower bureaucratic responsiveness to 311 fixes has a negative effect on future levels of misdemeanor criminal complaints. Specifically, citizens are most affected by government efficacy in the most recent past year, but they update their opinions about overall government efficacy over the span of the last three years. These findings indicate that trust in government in one sector (311 fixes) spills over into another sector (law enforcement) if citizens see a short-term improvement in government services; however, this increased trust in government will be most robust with steadily high performance over a longer period of time. It is not the presence of disorder *per se* that matters, but the government's responsiveness in attending to it that shapes residents' willingness to initiate, cooperate, and engage.

These findings have both political and social consequences. Politically, to improve citizen engagement with any one government agency, reform must extend beyond that agency. A policy's intended effect, in other words, may not actualize for reasons independent of the policy's design or execution. When implemented, its effects depend on the actions of other agencies and the multiplicity of policies vying for citizens' attention. Socially, the inability of government to provide services comprehensively across different agencies to the entire population can intensify service cynicism—citizens' dissatisfaction and disengagement with government services—over time. Government services will increasingly be seen as unreliable and eventually treated as an unavailable option.

These political and social consequences reveal policy implications for a key aspect of state governance: citizen and police interactions. A wider scope of reform is needed beyond professional training and convincing police officers to act in ways that are more procedurally just. Each interaction a citizen has with a government agent is an opportunity to improve relations and shape perceptions of the legitimacy of all other government agents, not just police officers. Prompt attention to service requests—even mundane services during nonemergencies—enhances trust among citizens, police, and government overall.

The article first reviews extant research on how citizens evaluate public services, and then presents our research design and data. We next articulate our hypotheses and main results before presenting a placebo test and robustness checks. We conclude with limitations and policy implications.

Citizen Expectations and Evaluations of Government Service Provision

Enduring democratic civil society requires both horizontal and vertical trust.⁸ Horizontally, successful reliance on co-citizens advances solidarity and confidence that issues of public concern are identified and addressed.⁹ Vertically, successful exchange of government services with citizen engagement generates positive evaluations, builds trust and legitimacy, and motivates continued cooperation.¹⁰

The vertical relationship between citizen and government features a basic exchange: taxes for public services. Citizens rely on services for two reasons: (1) to satisfy baseline

needs and (2) to take advantage of promised benefits. Regarding the first, citizens often feel legally entitled to or “due” the services satisfying baseline needs because they pay taxes.¹¹ The second, in contrast, involves discretionary participation in services, and is therefore largely animated by perceptions of the agency’s effectiveness in delivering the promised benefits.¹² Together, government policies determine whether citizens can purchase a home, afford higher education, access healthcare, and other fundamental aspects of modern life. The content and execution of policies provide information to citizens about which courses of action to pursue.

Effective local democratic governance increasingly relies on the co-production of services, that is, citizen participation to initiate, direct, and improve service delivery.¹³ For instance, police departments rely on residents to dial 911 to alert officers to a potential crime, provide context to the scene when officers arrive, and cooperate with the investigation by offering information and leads. Not only do government officials expect civic engagement,¹⁴ participation,¹⁵ and compliance,¹⁶ but citizens, too, expect government responsiveness, or “a relatively specific response” executed “in the very immediate future.”¹⁷ Citizens evaluate both the means and ends of the government response, organize their future conduct accordingly, and in doing so provide feedback to government officials about their performance.

Citizen evaluations are not necessarily objective assessments. Instead, they often rely on cues and cognitive shortcuts to facilitate understanding and decision making in a society that is both complex and uncertain.¹⁸ By conferring resources such as money and time, or shaping the incentives to mobilize politically, policies often offer direct advantages to targeted populations.¹⁹ Rather than outcomes, evidence from social psychology emphasizes citizens’ prioritization of the process in which services are delivered: when the delivery is deemed fair, citizens are more likely to comply with authorities and to approve of government.²⁰ In addition to outcomes and processes, an underlying prerequisite for citizen evaluations of government performance is the visibility and traceability of the service.²¹ Visibility refers to how discernable the policies’ outcomes are to voters, whereas traceability emphasizes the link between the government action and its outcome. Policies that are more visible and traceable, as opposed to those with low visibility, tend to animate citizens to punish or reward politicians proactively.²²

Citizen evaluations of government are neither static nor dichotomous, but rather subject to constant update. Citizens learn from each interaction with an agency, recalibrate their expectations, and bridge those beliefs from the agency to government in general. If trust is defined as circumstances in which one party expends fewer resources to monitor and enforce compliance from another party,²³ then citizen trust in government wavers when government commitments are interpreted as no longer credible—a situation that leads either to vocal discontent and greater monitoring (voice) or to cynicism and disengagement (exit).²⁴ Just as policy feedback from the politically active motivates policymakers most, the parallel trend of citizen dissatisfaction and disengagement only intensifies over time: policy attention, as well as the advantages conferred, is focused on the former, at the expense of the latter.²⁵

Citizens are less likely, for example, to initiate contact with law enforcement and seek public services following past negative interactions with the police,²⁶ excessive

stop and frisks with no arrests,²⁷ and high profile cases of police violence.²⁸ This line of reasoning comports with Harding's description of a cultural frame he calls "institutional distrust"—a lens developed through negative interactions with police and that colors the interpretation of experiences with other institutions.²⁹ Institutional distrust can translate into what Goffman and Brayne call "system avoidance," or the purposive evasion of record-keeping institutions such as hospitals in an effort to go under the radar of police surveillance.³⁰ Cynical citizens' institutionally distrusting lens and system-avoiding behavior are not agency-specific but apply rather to government in general.

But does dissatisfaction and disengagement developed toward one agency extend beyond to an alternative agency? Although Soss demonstrates that citizens adopt broader political orientations based on interactions with one agency, it is unclear whether such orientations shape perceptions and conduct toward another agency in a separate policy domain.³¹ Similarly, although Weaver and Lerman find that negative interactions with the carceral state socialize citizens to disengage politically, it is unknown whether the reverse is true—that negative interactions with agencies of non-criminal justice shape citizens' willingness to initiate contact with law enforcement.³²

Research Hypotheses

The dissatisfaction and disengagement citizens develop, we argue, is not limited to the agency with which they have had contact. Rather, citizens evaluate the government holistically, and their satisfaction with one agency informs their views of other agencies. Thus past experiences with government service providers can have direct and immediate consequences for whether citizens are willing to initiate contact with other agencies in the future. Negative evaluations of the government's ability to provide timely services aggregate into a general service cynicism, in which citizens become dissatisfied and disengaged with government and are disinclined to initiate contact for future services. This theory represents a more accurate rendering of citizens' lived experiences, as people are subject to multiple policies and agencies simultaneously.

Applying this logic to our substantive case—government responsiveness to 311 calls incentivizing different levels of engagement with police—generates three main predictions. First, we agree with prior research that poor provision of public services through the 311 program can lead to disengagement with the program, as individuals submitting 311 requests update their beliefs about the efficacy of government public goods provision (Hypothesis 1). If the government cannot commit to fixing problems reported through 311 calls, it is unlikely that citizens will think of returning to the 311 program for subsequent issues.

Our theory, however, takes it one step further and offers Hypothesis 2: cynicism about government services, measured by longer response times to fix 311 requests, does not only affect future engagement with the 311 program. We hypothesize that when individuals observe that neighborhood-level or street-level problems are not resolved in a timely manner, they will be less likely to turn to government for other problems. In the present case, we argue that a seemingly unrelated sector of

government—complaints to the police about local crimes—can be affected by how the government responds to 311 calls.

Finally, we make a third hypothesis: repeated learning matters. Hypothesis 3 argues that years of poor service provision, measured by longer 311 response times, can compound cynicism toward government service provision, thereby leading to greater and more permanent disengagement with government and political bodies. Whereas citizens may interpret short-term or sporadic poor responses to service requests as anomalies, repeated government inefficiencies can cement beliefs and increase levels of cynicism. Thus, as response times to 311 calls remain slow for consecutive years, we argue that civilians are less and less motivated to contact police about local crimes.

Research Design

The project takes a micro-level approach to identifying the relationship between government efficacy, trust, and civic engagement by focusing specifically on the five boroughs of New York City as the case study of interest. Zooming in on New York is particularly beneficial for three reasons. First, the city is ideal for studying civilian-police interactions because of its size and high population density. We expect that the population size and large number of police precincts that service the city will give us good leverage over variation in the dependent and independent variables both within and across precincts. Second, given the socioeconomic demographics of New York City and the recent stop-and-frisk program, police-community relations are a particularly salient issue that permeates citizens' everyday lives. Finally, limiting our study to one city allows us to hold state effects constant for the sake of practicality.

We create a time-series cross-sectional data set that allows us to examine changes over time along with differences between precincts. Our unit of analysis is precinct-year, and covers seventy-five precincts in New York from 2004 to 2012.³³ This research design allows us to account for several potential biases that arise from comparing across different areas in the city. Given the wide range of criminal activity and concentrated disadvantage across different neighborhoods, it is important to compare only within each precinct over time. This restriction allows us to establish "baseline" crime levels and propensity to call 311. It also mitigates issues related to redistricting precincts: while precinct redistricting occurred before our study and thus is exogenous to our effects, it is possible that different precincts are correlated with propensity to report crime. The fixed effects estimation allows us to account for such effects.

Data

Dependent Variable: Criminal Complaints

Although much empirical research has used surveys to measure people's self-reported attitudes toward police,³⁴ our study uses a measure of actual behavior—the number of people who file criminal complaints. Criminal complaints comprise all crimes or violations reported to the New York Police Department (NYPD). Complaints, which

formally trigger criminal proceedings, are written accusations charging one or more persons with at least one misdemeanor or felony. Data on criminal complaints made to the police by precinct are publicly available on the NYPD website.³⁵

The complaint data include citizen-initiated complaints (such as 911 calls following a home burglary), police-initiated complaints (such as a patrol officer observing a jewelry store break-in), and cases such as narcotics possession that do not have a specific complainant but rather generally “the People of the State of New York.” Because the criminal complaints data included both citizen-initiated reports and NYPD’s “proactive policing strategies,” there may be confounding factors if the latter are included in our dependent variable measure. To correct for this, we exclude categories in which a significant portion of the misdemeanor complaints is not generated by citizen calls. These categories—as reported by the NYPD—are “misdemeanor possession of stolen property,” “misdemeanor dangerous drugs,” “misdemeanor dangerous weapons,” “intoxicated and impaired driving,” and “criminal trespass.”

Because the complaint data are disaggregated into different crimes, we separate them into three categories—misdemeanors, felonies, and violations. We argue that an increase in the duration of 311 fixes should lead to a decrease in the (population-normalized) number of misdemeanor reports. Misdemeanors are relatively small crimes (at the magnitude of petty theft, disorderly conduct, vandalism, etc.) that citizens may or may not choose to report to the police; the same cannot be said for violations or felonies. Whereas violations tend to be overly small transgressions, felonies include serious crimes that are very likely to be reported regardless of civic engagement levels, such as armed robberies or murders. We thus use misdemeanors as our main dependent variable, and felonies as our placebo dependent variable. For comparability across precincts, we divide the total number of criminal complaints per precinct by the precinct’s total population.

Independent Variable: Duration of 311 Fix

The growing availability of 311 service call data represents a novel way to study questions that have long interested political and social science studies of urban life. Minkoff distinguishes between two facets of 311 that shape call volume: condition and contacting propensity.³⁶ The former refers to the (subjective) awareness of a condition that requires fixing, whereas the latter refers to awareness of the 311 program and motivation to dial. Although studies on “condition” have leveraged 311 as an indicator for private neglect and public denigration³⁷ or the quality of city-provided public goods,³⁸ most focus on “contacting propensity” and theorize 311 as residents’ custodianship and care for physical disorders in public spaces,³⁹ the presence of ethno-racial neighborhood conflict,⁴⁰ or as a proxy for political participation and community engagement.⁴¹

There are multiple advantages to using 311 data. First, unlike observational data,⁴² 311 complaints about disorder are reported by actual residents of the community. These residents can discern and dial in the disorder 24 hours a day throughout seasonal changes.⁴³ Unlike survey data, which necessarily select certain community spokespersons and

prioritize their specific standard for disorder to represent those of the entire community, 311 is more equal access. In fact, the annual growing volume of 311 calls, as described below, indicates that more and more residents are actually using this service. These data account for people's perception of disorder, rather than "objective" signs researchers may have measured by systematic observation of streets and coded as visible cues of disorder, such as litter.⁴⁴ Separate hotlines for 311 and 911 complaints may also help disentangle what exactly residents view as crimes and what they view simply as disorder.⁴⁵ And finally, 311 data are incredibly specific, containing information about when the complaint was registered (a proxy for when the issue first began), when it was fixed, the nature of the disorder, and precise geographic coordinates of where the disorder is located.

New York City launched the "nation's largest and most comprehensive government information and services center" in 2003.⁴⁶ We leverage the city's 311 data as our independent variable, which measures the *duration* between complaint and fix for 311 calls as a proxy for government responsiveness. This duration statistic measures the number of days between a request's creation and closure.⁴⁷ The 311 operators record the status of the request as "open," "assigned," "pending," or "closed." A request is deemed closed after it has been assigned to an agency and the agency has responded to the request. For instance, one request for "derelict vehicles" was deemed closed after "the Department of Sanitation removed the items." If the caller remains unsatisfied with the "fix" even though the request has been deemed "closed," then the caller is encouraged to contact the agency directly or submit another request.

By conceptualizing the duration of 311 fixes as a proxy for government responsiveness, we avoid the pitfalls of employing 311 data as a measure of civic or political participation.⁴⁸ However, by looking at the 311 data in totality, we include a large proportion of calls that could be considered about private matters, such as issues with landlords or buildings. Although we argue that such incidents also shape citizen beliefs, public matters may cause a larger effect as they are observed by a larger number of people. For example, if a traffic light malfunction is quickly resolved, every person who has driven past the traffic light may update his or her beliefs about government responsiveness. We therefore further disaggregate the 311 data to look at two subcategories—noise complaints and street level complaints (see Appendix Table A1 for the types of calls included in these categories)—as further tests.

Control Variables

We hypothesize that a decrease in government efficacy in responding to 311 requests negatively affects people's attitudes toward the police, which in turn affects the likelihood of contacting the police when a citizen witnesses a crime. Identifying the impact of 311 requests on criminal complaints presents significant challenges because of potential endogeneity and a number of unobserved variables that may affect both the dependent and the main explanatory variable.

Particularly problematic is the fact that we are attempting to measure changes in citizens' propensity to report misdemeanors, when in fact the underlying true levels of crime—beyond what has been reported—are unknown. We account for this problem in three ways. First, our fixed-effects model allows us to estimate effects within units over

time, and thus avoid obviously different levels of crime between different precincts. Second, we control for population-normalized arrest numbers, which (although skewed by willingness to call) does provide us with some information about crime levels in a particular precinct each year. Finally, we account for within-precinct changes in crime level over the nine-year period by adding an interacted time trend. This time trend aims to capture both the citywide decrease in crime levels—as reported by the FBI Uniform Crime Reporting statistics⁴⁹—as well as recent research suggesting that neighborhoods with greater levels of *initial* concentrated disadvantage have seen greater reductions in crime levels.⁵⁰ We operationalize initial concentrated disadvantage as the population-normalized stop and frisks made by police in 2003 since (a) stop and frisks, unlike arrests or 911 calls, do not rely at all on citizen reports; (b) 2003 is the year prior to the start of our study; and (c) police began the stop and frisk program by targeting the most disadvantaged and troubled neighborhoods. We interact stop and frisk with time period, which allows crime rates to decrease more quickly for disadvantaged neighborhoods in comparison to neighborhoods that are more well-off.

We break the remainder of the control variables into three subcategories.

1. **Demographic:** We argue that a precinct's demographic data affects the number of police calls as well as 311 calls. We thus control for a variety of individual-level data aggregated to the precinct level, such as indicators for age groups, household income measures, race, and level of education measures (see Appendix Table A2 for summary statistics of controls). We also control for percent vacant buildings and percent homeowners within a precinct.
2. **Interactions with police:** Clearly, interactions with police may affect both 311 calls⁵¹ and subsequent disposition toward the police. We use one variable—frisk but no arrest—to control for interactions with the police. This variable captures the number of times a stop and frisk episode did not lead to an arrest, normalized by the precinct's total population. This variable aims to capture the level of citizens' sense of injustice toward law enforcement.
3. **Call data and lag reports:** Finally, we control for the number of 311 calls per person within each precinct, as well as the lagged dependent variable where applicable, since we expect that reports to the police are highly dependent on willingness to report in the previous year.

Apart from stop and frisk data, which are recorded by precinct, other control variables are taken from the American Community Survey (ACS)⁵² and thus required some further data manipulation. Since the ACS releases precise data in five-year increments beginning in 2005, we use ACS 2005–9 data for years 2004–9, and the ACS 2009–13 data for years 2001–12. The unit of analysis in the ACS is census tract; for 2005–9 data, Infoshare Online⁵³ provides all the ACS 2005–9 data aggregated to the precinct level. We aggregate the ACS 2009–13 data by weighting each census tract according to population share, and then summing all the values:

$$precinct_{i,t} = \sum_{i=1}^N \frac{\text{census tract pop}_{i,t}}{\text{precinct pop}_{i,t}} \times ACS_{i,t}$$

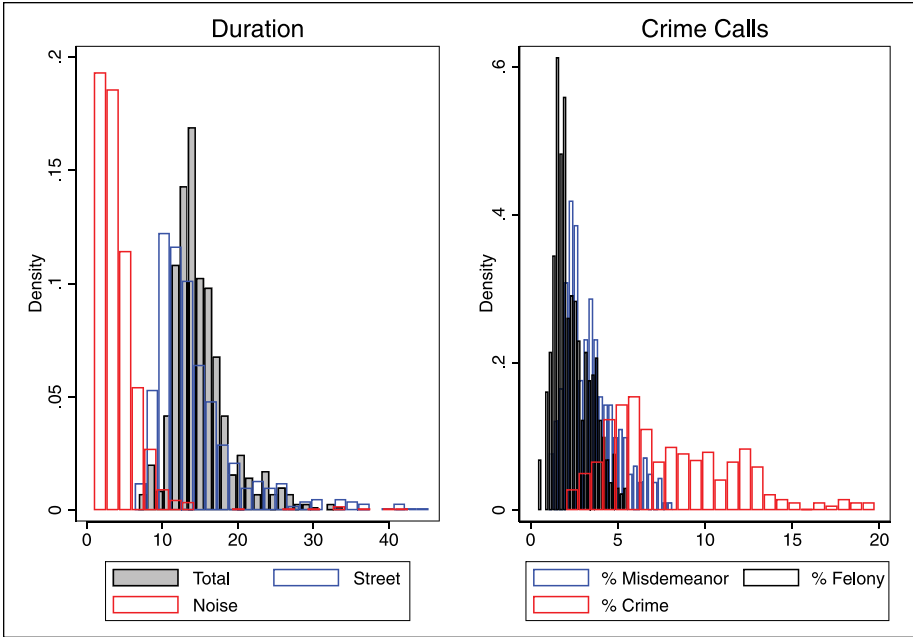


Figure I. Duration and Criminal Complaints Distribution.

Note: The average duration of 311 fixes is depicted by the figure on the left, and the percentage of criminal complaints is depicted by the figure on the right. See online version of this article to view figure in color.

Source: 311 duration from New York City Open Data⁵⁴; criminal complaints from NYPD website.

Descriptive Statistics

Figure 1 displays the distributions of the main dependent variables (percentages misdemeanor, felony, and crime) and the main independent variables (311 duration, noise duration, and street duration). Table 1 provides summary statistics for these same variables at the precinct level. Notably, 311 responses within a precinct-year can take from an average of seven days to forty days. Over the course of the nine years in our sample, the precinct with the lowest 311 response duration is around nine days in Staten Island, while the highest average duration is nineteen days in Washington Heights.

Results

Our theory argues: (1) poor provision of public services through slower 311 responses can produce cynicism in the efficacy of government (Hypothesis 1); (2) this cynicism can translate into different sectors of government such as the police (Hypothesis 2); and (3) years of poor service provision compounds this cynicism (Hypothesis 3). We test the first step of this argument by examining whether changes in 311 fix duration encourage people within a precinct to place more calls to 311 in the future.

Table 1. Descriptive Statistics.

	Count	Mean	SD	Min	Max
Mean total duration	675	15.076	3.837	6.887	33.922
Mean noise duration	675	4.449	4.63	0.921	68.28
Mean street duration	675	14.399	5.902	6.341	45.267
Percentage crime	675	9.149	5.833	2.042	39.498
Percentage misdemeanors	675	3.953	2.642	1.047	16.749
Percentage felonies	675	2.697	1.845	0.424	17.365
Percentage calls	675	19.766	9.138	5.658	82.613

Source: New York City (NYC) Open Data; NYPD website; American Community Survey (ACS).

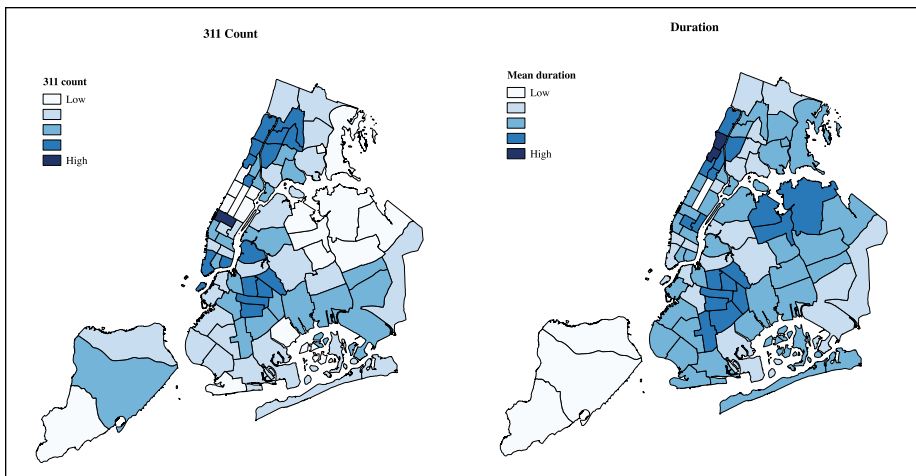


Figure 2. Police Precinct Map of Total 311 Count and Duration.

Note: The figure on the left depicts the population-normalized total number of 311 calls in each NYPD precinct. The figure on the right shows the average 311 fix duration in each NYPD precinct. See online version of this article to view figure in color.

Source: 311 duration from NYC Open Data; criminal complaints from NYPD website; precinct shapefiles from NYC Department of City Planning.⁵⁵

Figure 2 illustrates a New York City map with the total count of 311 calls and the duration of fix per district side by side. In general, the total count of calls and duration appear inversely related. In particular, precincts with fewer calls correlate with longer duration.

Table 2 provides a formal test of Hypothesis 1 using a fixed effects regression with the full set of controls. As we can see, a positive change in duration (longer fix time) leads to a negative change in the number of calls placed in the following year. This effect persists not just from the year before, but also from two years prior. The mean change in 311 count is -356.9 , so a one-day increase in 311 fix time decreases the

Table 2. Effect of Duration on Change in Counts.

	(1)
	Δ 311 count
Δ duration (lag 1)	-136.433***
	-38.799
Δ duration (lag 2)	-79.945***
	-21.279
Observations	450
R ²	0.502
Dep var mean	-356.89

Note: Standard errors in parentheses.

Source: NYC Open Data; NYPD; ACS.

* $p < .10$, ** $p < .05$, *** $p < .01$.

number of calls in the subsequent year by more than 38 percent of the mean value, and decreases the number of calls in two years by more than 22 percent. This suggests that citizens do adjust their engagement based on the rate at which the government provides services to the community.

Main Model

Having ascertained that citizens are in fact less likely to call for services when government response rates are slower, our main model tests for Hypothesis 2 of our argument—that this civic disengagement can extend into a different sector, specifically in providing information to the police about misdemeanor crimes occurring within each precinct. Our baseline dependent variable here is the number of misdemeanor complaints divided by the total population of the precinct. We argue that an increase in the duration of 311 fixes should lead to a decrease in the (population-normalized) number of misdemeanor complaints.⁵⁶

Model 1 in Table 3 provides the simplest formulation of our hypothesis with only fixed effects and none of the control variables. We use a lagged measure of duration, which not only aids our claims of exogeneity, but is also theoretically sound: if there is indeed an effect from 311 fixes on calls to the police, we should expect that effect to manifest after the 311 calls are made. In other words, past experiences with 311 fix duration would inform present willingness to submit criminal complaints—hence the lagged measure of 311 fix duration. We find a negative relationship between a one-year lag of 311 fix duration and the number of misdemeanors that get reported by citizens: when the mean fix duration increases by one day, misdemeanor reports decrease by about 0.02 percent. That means that misdemeanor reports decrease by more than 0.076 percent per standard deviation increase in 311 fix duration, or almost a 2 percent decrease of the sample mean. Model 2 adds control variables, including a one-year lag in misdemeanor calls. While the relationship remains negative, the coefficient is no longer statistically significant; we suspect that this is not because of dependence on the

Table 3. Effect of 311 Fix Duration on Percentage Misdemeanor Reports.

	Percentage Misdemeanor					Δ Percentage Misdemeanor
	(1)	(2)	(3)	(4)	(5)	(6)
Mean total duration (lag 1)	-0.020** (0.008)	-0.003 (0.006)			-0.018* (0.009)	
Mean total duration (lag 2)					-0.005 (0.007)	
Mean total duration (lag 3)					-0.003 (0.005)	
Two-period mean duration			-0.009 (0.007)			
Three-period mean duration				-0.037*** (0.012)		
Change in duration						-0.022* (0.011)
Lagged percentage misdemeanors		0.190*** (0.057)	0.013 (0.069)	-0.102 (0.163)	0.021 (0.065)	
Observations	600	600	450	375	450	525
R ²	0.985	0.992	0.995	0.995	0.995	0.216
Dep var mean	3.953	3.953	3.992	4.009	3.992	0.038
Likelihood ratio (LR) test (lag 1 + lag 2 + lag 3)					0.000	
Lag 1 + lag 2 = 0					-0.023**	
Lag 1 + lag 2 + lag 3 = 0					-0.026**	
Controls	No	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses.

Source: NYC Open Data; NYPD; ACS.

p* < .10, *p* < .05, ****p* < .01.

prior year (the newly included lagged misdemeanor reports variable), but rather because of the large number of demographic controls (fifty-two in total) we included, which likely overinflated the standard errors. In a separate model (not reported), we find that the results continue to be statistically significant if we include full fixed effects and a lagged misdemeanor reports variable, but none of the demographic controls.

In Models 3 and 4, we take an alternative approach. While it is plausible that people are affected by the duration of 311 fixes in the previous year, could behavior be informed by several years of slow government response to neighborhood needs? We create two additional variables—two-year mean duration and three-year mean duration—which is simply the mean duration of last year and two years ago (for two-year

mean duration), and the mean duration of last year, two years ago, and three years ago (for three-year mean duration). The drawback of this liberal use of lags is that it reduces the number of years we can use, and thus drops our number of observations to 525 and 450, respectively. Despite the large number of controls in our model, however, we do find a statistically significant negative effect in the three-year mean duration model—about a 3.6 percent decrease in misdemeanor reports per standard deviation increase in 311 fix duration, relative to the sample mean—which suggests that people’s trust in government is certainly informed by years of repeated learning about government efficacy.

Model 5 features a distributed lag model with three lag periods. This model answers the question, Does any particular year have a greater effect on future behavior? For example, if citizens have “short memories,” the effect of poor service for several years in a row can be mitigated by a year of suddenly good service. Alternatively, if citizens “never forget,” poor service three years ago will continue to inform beliefs about government efficacy, regardless of more recent improvements. Results in Model 5 suggest that “short memories” is a more likely explanation: the coefficient on total mean duration (lag 1), which looks only at last year’s 311 fix durations, is statistically significant at the $p < 0.10$ level, whereas the other two years exhibit a negative but insignificant relationship.

However, as noted in Hypothesis 3, our argument suggests the effect of poor service provision ought to be cumulative, such that citizens develop cynicism toward the government as time goes on. To test this, we run tests for joint significance in Model 5. The likelihood ratio test suggests that the joint inclusion of the three duration variables provides a significantly better fit to the data than a baseline model of only demographic variables and previous year’s misdemeanor reports. We further test for the linear combination of $\text{lag1} + \text{lag2}$ as well as for $\text{lag1} + \text{lag2} + \text{lag3}$. The linear combination tests determine whether, even though a specific year may not be statistically significant, poor government response to 311 calls piled on over two or three years could lead to disillusionment with the government. Results indicate that the sum of these variables do indeed have an effect on subsequent misdemeanor calls. Results of the linear combination tests increase in effect size as more years are added, providing strong evidence that citizens do indeed develop cynicism of the government when years of poor service delivery accumulate.

Finally, in Model 6, we look simply at whether the change in duration between two years ago and one year ago affects the change in misdemeanor calls between last year and this year. This first-differences model is meant to explain the same question as Model 2, except with an arguably cleaner and more intuitive structure. We find a statistically significant effect of long duration on lower crime calls: For every one day increase in 311 fix duration, we find an associated 0.022 percent decrease in misdemeanor calls. The sample mean is a 0.038 percent increase in misdemeanor calls from year to year—a baseline value. However, if a 311 call is resolved one day longer than average, our results suggests that this baseline 0.038 percent increase in year-to-year misdemeanor calls drops to only 0.016 percent, which represents approximately a 58 percent decrease from the original number. This is further exacerbated as the duration

of 311 fix increases: when a 311 fix duration increases by two or more days, the year-to-year baseline increase in misdemeanor calls turns into a decrease in misdemeanor calls from year to year. Given that the standard deviation change in duration is 2.66 days, this suggests that while the average precinct sees 0.038 percent more criminal complaints to police year to year, those that receive one standard deviation slower services from 311 calls see 0.021 percent fewer misdemeanor calls per person—leading to a 0.059 percent gap. In the long run, this suggests a greater gap in cooperation with law enforcement in communities that receive government services more slowly in comparison to areas that receive timely services.

Placebo

In the previous section, we found that citizens do in fact update their opinions about government efficacy based on the length of 311 fix durations, and that these opinions later affect the number of misdemeanor reports that citizens submit. However, could these relationships be a result of any omitted variables that link fix durations and criminal complaints? In this section, we run the exact same tests on a placebo condition—namely, reports on felonies—as a test of our measurement about trust in government. As we previously argued, we do not expect the same relationship between 311 fix duration and felonies since the criminal seriousness of felonies suggests that any citizen would report them, regardless of trust in government. For example, a citizen who witnesses an armed robbery or a murder is likely to report it, in contrast to a citizen who ignores a petty theft.

Table 4 provides the results for this placebo measure, run on the same tests as the main model using misdemeanors. As we can see, the negative relationship between the duration of 311 fixes and crime reporting is now gone; instead, we have a fairly robust positive relationship in Models 1 to 5, suggesting that there is an increase in major felony complaints in precincts where the government has not been responsive to neighborhood complaints. This provides direct evidence that our main models in the previous section are plausible: for events where citizens may or may not choose to comply with law enforcement, trust in government efficacy is key.

How might we interpret the positive coefficients in the placebo table? Since we argue that any citizen who sees a felony is most likely going to report it regardless of their feelings toward government, felony reports more closely track actual felony levels in a precinct. We might thus interpret the positive relationship as: within a neighborhood, in years with longer 311 fix duration, the number of felonies in subsequent years increase. This is broadly consistent with the “broken windows” argument, in which neighborhoods with higher and persistent signs of disorder, such as vandalism or empty buildings, become attractive targets for criminal behavior (though in this case, residents are in fact reporting signs of disorder to 311). Given our fixed effects structure (comparing precincts only to themselves in previous years), the number of controls (controlling for slow demographic changes within the precinct), and the duration lags, we are confident that this finding is not affected by other confounding factors.

Table 4. Effect of 311 Fix Duration on Percentage Felony Reports.

	Percentage Felony			Δ Percentage Felony		
	(1)	(2)	(3)	(4)	(5)	(6)
Mean total duration (lag 1)	0.047*** (0.008)	0.008** (0.003)			0.002 (0.006)	
Mean total duration (lag 2)					0.001 (0.008)	
Mean total duration (lag 3)					0.014*** (0.004)	
Two-year mean duration			0.019*** (0.007)			
Three-year mean duration				0.027*** (0.009)		
Δ Duration						-0.001 (0.004)
Lagged percentage felonies		0.556*** (0.061)	0.329*** (0.086)	0.291*** (0.062)	0.326*** (0.085)	
Observations	600	600	450	375	450	525
R ²	0.967	0.991	0.992	0.994	0.992	0.349
Dep var mean	2.650	2.650	2.557	2.504	2.557	-0.073
LR test (lag 1 + lag 2 + lag 3)					0.000	
Lag 1 + lag 2 = 0					0.004	
Lag 1 + lag 2 + lag 3 = 0					0.018*	
Controls	No	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses.

Source: NYC Open Data; NYPD; ACS.

* $p < .10$, ** $p < .05$, *** $p < .01$.

As an alternative placebo test for confounding factors, we check to see if changes in future 311 fix duration is significantly correlated with percent misdemeanor reports by citizens. While we have argued that increased efficacy with regards to 311 calls will lead to increased engagement with the police department, this relationship should only hold for 311 fixes that happened in previous years—future 311 fix duration should have no impact on current misdemeanor reports unless (1) there are confounding variables missing from our main specification, or (2) the relationship between duration and misdemeanor reporting is endogenous.

Table 5 presents regression estimates checking for the effects of future 311 fix duration on current misdemeanor reports. While our main models report one-, two-, and three-year lags, we restrict the placebo to one-year lags to preserve the number of usable observations. As the table shows, the point estimates for both the future mean duration and the change in duration from a future period do not significantly differ from zero. We

Table 5. Future 311 Duration on Misdemeanor Reports.

	Percentage Misdemeanor	Δ Percentage Misdemeanor
	(1)	(2)
Future mean duration	0.013 -0.008	
Δ Future duration		0.006 -0.006
Lagged percentage misdemeanor	0.174*** -0.06	
Observations	525	525
R ²	0.993	0.266
Dep var mean	3.931	0.002
Controls	Yes	Yes
Fixed effects	Yes	Yes

Note: Standard errors in parentheses.

Source: NYC Open Data; NYPD; ACS.

* $p < .10$, ** $p < .05$, *** $p < .01$.

take this as an indication that our main specification does correctly and convincingly capture our hypothesized relationship between past 311 fix duration and citizens' subsequent engagement with law enforcement with regards to reporting misdemeanors.

Robustness Check: Disaggregating 311 Calls

Having established the negative relationship between misdemeanors and 311 fix duration, we look into disaggregating the 311 calls into specific categories. One counterargument to our main specification may be that the types of 311 calls made to the city government are quite varied, and may be private rather than public. For example, the fact that someone calls 311 to fix heating issues within a private building is not visible to others in the community and thus assesses poorly how citizens in a neighborhood are affected by government efficacy. Although we argue that this is not necessarily problematic—our estimations are within precincts, and the types of problems that people have within a precinct should not differ considerably from year to year—this section looks specifically at street complaints and noise complaints to assess the robustness of our estimates. We argue that neighborhoods in every precinct should have complaints about street-level problems as well as noise complaints, and that these issues are noticeable to the community when present (and thus noticeable by the community when fixed). The types of complaints that fall under “street” and “noise” are presented in Appendix A1.⁵⁷

Table 6 provides our main model, looking at only noise and street-level complaints and the duration of response to these requests. In Models 3, 4, and 5, our estimation is in fact even more robust than the main model, with larger coefficients and greater

Table 6. Effect of Street and Noise 311 Duration Fixes on Percentage Misdemeanor Reports.

	Percentage Misdemeanor			Δ Percentage Misdemeanor		
	(1)	(2)	(3)	(4)	(5)	(6)
Street and noise mean duration (lag 1)	-0.015* (0.008)	-0.002 (0.006)			-0.007 (0.008)	
Street and noise mean duration (lag 2)					-0.014** (0.007)	
Street and noise mean duration (lag 3)					-0.009* (0.005)	
Two-year street and noise mean duration			-0.026*** (0.008)			
Three-year street and noise mean duration				-0.052*** (0.012)		
Δ Street and noise mean duration						-0.003 (0.009)
Lagged percentage misdemeanor		0.189*** (0.058)	0.015 (0.067)	-0.118 (0.159)	0.024 (0.064)	
Observations	600	600	450	375	450	525
R ²	0.984	0.992	0.995	0.995	0.995	0.198
Dep var mean	3.953	3.953	3.992	4.009	3.992	0.038
LR test (lag 1 + lag 2 + lag 3)					0.000	
Lag 1 + lag 2 = 0					-0.022**	
Lag 1 + lag 2 + lag 3 = 0					-0.031***	
Controls	No	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses.

Source: NYC Open Data; NYPD; ACS.

* $p < .10$, ** $p < .05$, *** $p < .01$.

statistical significance. We also see greater statistical significance in the linear combination test in Model 5. We therefore find that street and noise 311 calls may be more sensitive to longer duration, which makes intuitive sense: a noise complaint should be resolved within the same day, and many street complaints that include sanitation issues and broken traffic lights should also receive quick responses. Thus, a slow response to these issues will be noticeable to residents in the community, and should have a more pronounced effect on willingness to initiate contact with law enforcement with regards to reporting misdemeanors.

We further include robustness checks on just street-level calls, just noise calls, and total crime (sum of violations, misdemeanors, and felonies) in Appendix Tables

A3–A5. Results remain robust for street-level calls but less so for noise calls. Results are also robust for total crime, although we suspect that this result is being driven by the large number of violations and misdemeanors (less serious crimes) and not felonies (more serious crimes), given our placebo result of a strong positive relationship between felonies and 311 fix durations.

Conclusion and Policy Implications

Intended policy feedback effects are not guaranteed, but rather subject to the content and execution of policies by government agencies even in unrelated policy domains. Sources of policy change are often difficult to discern: some sources are “hidden”⁵⁸ whereas others are part of the “submerged state.”⁵⁹ We highlight another source of policy feedback not readily discernable: policies from intergovernmental agencies.

The findings above support the theory (1) that efficient provision of public services through the 311 program can enhance trust in the efficacy of government and (2) that this trust can translate into different government agencies. Citizens’ reflections on three years of 311 responsiveness shape their trust in local government, as indicated by their willingness to participate in another sector—in this case, submitting misdemeanor complaints to law enforcement. Thus, this theory captures a more holistic and accurate picture of government and of the compounding presence of policies enacted across government agencies.

Given that programs should efficiently allocate resources, improving police and citizen interaction in communities with pervasive distrust must focus on providing comprehensive services across government agencies. Although it is important to reexamine each police and citizen exchange, local government must also improve citizens’ experiences with the sanitation official plowing snow, the transportation official repairing streetlights, and all other service providers who are agents of local government. Policymakers may counter that civil servants cannot be incentivized to care about the effect of their work on other branches of government. Yet it is important to remember that incentives are aligned: not only does greater responsiveness to 311 calls improve citizens’ attitudes toward, and participation in, the 311 program itself, but the benefits spill over into the other government agencies as a beneficial by-product. The social costs of the alternative are high. If government and its citizens are mutually dependent, then citizens can punish government agencies by withholding their trust and the cooperation, compliance, and information that accompany it.

However, this study is not without limitations. First, starting in 2004, New York City established the Street Conditions Observation Unit (SCOUT) where inspectors drive through every city street once per month and report conditions negatively affecting quality of life to 311. Thus 311 operators receive SCOUT calls the same way they receive calls from ordinary citizens. Because these reports are *not* citizen-initiated, they may inflate the number of 311 calls. However, because (a) we are interested in fix duration rather than total number of calls and (b) the number of SCOUT incidents represents only a minority fraction of total 311 calls, we are confident that the results remain robust.

Second, 311 callers may be different individuals from those submitting criminal complaints—a theoretically significant distinction, because only the 311 caller directly experiences the length of time it takes for a request to be fixed. Data availability prevents us from accounting for this difference directly, but previous research suggests that people who engage socially and politically in one domain are more likely to exhibit pro-social tendencies in other domains.⁶⁰ Further, the fact that researchers tend to measure social and political engagement by a wide variety of engagement methods (e.g., voting, contacting local government, volunteering) suggests a pervasive understanding that individuals who are pro-social in certain aspects are more likely to be pro-social in other aspects.⁶¹ We can test this empirically in New York by using the Pew Research Center’s 2012 Civic Engagement Survey to examine the correlation between a series of activities that require active participation or initiative.⁶² We calculate the Cronbach’s alpha, which measures internal consistency, or the extent to which a series of variables are asking the same underlying question. We find a scale reliability coefficient of 0.71, which is within the range of acceptable alpha values (0.7–0.9).

Notwithstanding these data limitations and concerns with the 311 program itself, 311 represents a novel tool to facilitate direct citizen engagement. The implications of 311 participation, however, extend beyond the particular 311 request. This unexplored mechanism of interagency transmission of dissatisfaction and disengagement highlights the importance of considering the design, content, and execution of policies from all government agencies. Policymakers must approach governance holistically, carefully accounting for the unintended consequences the actions of one agency can have on another.

Appendix

Table A1. Street and Noise Categorizations.

Coding Street:

```
replace street = 1 if strpos(complaint, "street") ///
| strpos(complaint, "highway") ///
| strpos(complaint, "construction") ///
| strpos(complaint, "graffiti") ///
| strpos(complaint, "litter") ///
| strpos(complaint, "sanitation") ///
| strpos(complaint, "bus") ///
| strpos(complaint, "crane") ///
| strpos(complaint, "bridge") ///
| strpos(complaint, "dirty conditions") ///
| strpos(complaint, "traffic") ///
| strpos(complaint, "sidewalk") ///
| strpos(complaint, "sewer")
```

(continued)

Table A1. (continued)

Coding Noise:

```
replace noise = 1 if strpos(complaint, "noise") ///
                | strpos(complaint, "loud")
```

Source: Authors' data design.**Table A2.** Summary Statistics for Controls.

	Count	Mean	SD	Min	Max
Percentage calls	675	0.198	0.091	0.057	0.826
Percentage arrests	675	0.049	0.039	0.004	0.209
Percentage frisked, no arrest	675	0.034	0.029	0.001	0.151
Logged population	675	11.491	0.518	9.617	12.437
Percentage households making 30k or less	675	0.345	0.132	0.125	0.654
Share of household heads living below poverty	675	0.197	0.103	0.046	0.464
Share age < 5	675	0.066	0.017	0.029	0.113
Share age, 5–9	675	0.057	0.018	0.015	0.098
Share age, 10–14	675	0.058	0.020	0.008	0.099
Share age, 15–17	675	0.037	0.013	0.004	0.067
Share age, 18–19	675	0.026	0.011	0.005	0.095
Share age, 20	675	0.015	0.006	0.002	0.043
Share age, 21	675	0.014	0.005	0.003	0.043
Share age, 22–24	675	0.044	0.011	0.019	0.093
Share age, 25–29	675	0.093	0.027	0.048	0.176
Share age, 30–34	675	0.085	0.023	0.050	0.160
Share age, 35–39	675	0.076	0.014	0.050	0.123
Share age, 40–44	675	0.073	0.010	0.049	0.110
Share age, 45–49	675	0.070	0.008	0.046	0.089
Share age, 50–54	675	0.064	0.009	0.042	0.084
Share age, 55–59	675	0.057	0.009	0.036	0.078
Share age, 60–61	675	0.021	0.004	0.011	0.033
Share age, 62–64	675	0.027	0.006	0.016	0.042
Share age, 65–66	675	0.015	0.004	0.008	0.029
Share age, 67–69	675	0.020	0.005	0.010	0.040
Share age, 70–74	675	0.028	0.008	0.013	0.063
Share age, 75–79	675	0.022	0.008	0.008	0.054
Share age, 80–84	675	0.018	0.007	0.005	0.042
Share age ≥ 85	675	0.017	0.007	0.005	0.040
Share of households on public assistance	675	0.046	0.033	0.004	0.147
Percentage unemployed	675	0.046	0.013	0.023	0.090
Percentage dependents	675	0.352	0.070	0.212	0.726
Percentage with middle school education	675	0.075	0.044	0.008	0.255

(continued)

Table A2. (continued)

	Count	Mean	SD	Min	Max
Percentage with some high school education	675	0.086	0.041	0.012	0.246
Percentage with high school degree	675	0.188	0.058	0.056	0.297
Percentage with some college education	675	0.127	0.026	0.070	0.200
Percentage with associate's degree	675	0.044	0.014	0.018	0.087
Percentage with bachelor's degree	675	0.158	0.092	0.029	0.427
Percentage with graduate degree	675	0.105	0.090	0.009	0.365
Percentage males	675	0.475	0.022	0.423	0.549
Percentage African Americans	675	0.265	0.257	0.010	0.910
Percentage white	675	0.445	0.259	0.022	0.931
Percentage Native Americans	675	0.004	0.002	0.000	0.013
Percentage Asians	675	0.110	0.120	0.005	0.718
Percentage Pacific Islanders	675	0.000	0.000	0.000	0.003
Percentage other race	675	0.149	0.142	0.008	0.505
Percentage persons with mixed race origin	675	0.025	0.015	0.005	0.149
Percentage vacant housing units	675	0.076	0.025	0.041	0.147
Percentage housing units occupied by homeowners	675	0.281	0.174	0.053	0.799
Year	675	5.000	2.584	1.000	9.000
Percentage stopped and frisked in 2003	675	0.023	0.017	0.003	0.107
Year × Percentage stopped and frisked in 2003	675	0.115	0.115	0.003	0.966

Source: American Community Survey (ACS).

Table A3. Effect of 311 Duration Fixes on Percentage Crime Reports.

	Percentage Crime					Δ Percentage Crime
	(1)	(2)	(3)	(4)	(5)	(6)
Mean total duration (lag 1)	0.014 (0.015)	-0.003 (0.007)			-0.021* (0.011)	
Mean total duration (lag 2)					-0.015 (0.009)	
Mean total duration (lag 3)					0.006 (0.006)	
Two-year mean duration			-0.004 (0.009)			
Three-year mean duration				-0.021 (0.013)		
Δ Duration						-0.034** (0.014)

(continued)

Table A3. (continued)

	Percentage Crime					Δ Percentage Crime
	(1)	(2)	(3)	(4)	(5)	(6)
Lagged percent crime		0.181*** (0.057)	0.037 (0.035)	-0.000 (0.050)	0.036 (0.035)	
Observations	600	600	450	375	450	525
R ²	0.990	0.997	0.998	0.999	0.998	0.313
Dep var mean	9.110	9.110	9.089	9.043	9.089	-0.038
LR test (lag 1 + lag 2 + lag 3)					0.000	
Lag 1 + lag 2 = 0					-0.035**	
Lag 1 + lag 2 + lag 3 = 0					-0.029*	
Controls	No	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses.

Source: NYC Open Data; NYPD; ACS.

p* < .10, *p* < .05, ****p* < .01.

Table A4. Effect of Street 311 Duration Fixes on Percentage Misdemeanor Reports.

	Percentage Misdemeanor					Δ Percentage Misdemeanor
	(1)	(2)	(3)	(4)	(5)	(6)
Mean street duration (lag 1)	-0.011* (0.006)	0.001 (0.005)			-0.008 (0.008)	
Mean street duration (lag 2)					-0.010* (0.005)	
Mean street duration (lag 3)					-0.006 (0.005)	
Two-year duration mean			-0.018*** (0.006)			
Three-year duration mean				-0.031*** (0.009)		
Δ Street duration						-0.003 (0.009)
Lagged percentage misdemeanors		0.185*** (0.056)	0.013 (0.070)	-0.112 (0.162)	0.022 (0.065)	
Observations	600	600	450	375	450	525
R ²	0.984	0.992	0.995	0.995	0.995	0.198
Dep var mean	3.953	3.953	3.992	4.009	3.992	0.038

(continued)

Table A4. (continued)

	Percentage Misdemeanor					Δ Percentage Misdemeanor
	(1)	(2)	(3)	(4)	(5)	(6)
LR test (lag 1 + lag 2 + lag 3)					0.000	
Lag 1 + lag 2 = 0					-0.017*	
Lag 1 + lag 2 + lag 3 = 0					-0.023**	
Controls	No	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses.

Source: NYC Open Data; NYPD; ACS.

* $p < .10$, ** $p < .05$, *** $p < .01$.

Table A5. Effect of Noise 311 Duration Fixes on Percentage Misdemeanor Reports.

	Percentage Misdemeanor					Δ Percentage Misdemeanor
	(1)	(2)	(3)	(4)	(5)	(6)
Mean noise duration (lag 1)	-0.010** (0.005)	-0.010* (0.005)			0.006 (0.006)	
Mean noise duration (lag 2)					-0.020** (0.008)	
Mean noise duration (lag 3)					-0.002 (0.005)	
Two-year duration mean			-0.013 (0.010)			
Three-year duration mean				-0.036 (0.023)		
Δ Noise duration						-0.008 (0.007)
Lagged percentage misdemeanors		0.191*** (0.059)	0.014 (0.068)	-0.100 (0.163)	0.015 (0.069)	
Observations	600	600	450	375	450	525
R ²	0.984	0.993	0.995	0.995	0.995	0.200
Dep var mean	3.953	3.953	3.992	4.009	3.992	0.038
LR test (lag 1 + lag 2 + lag 3)					0.000	
Lag 1 + lag 2 = 0					-0.014	
Lag 1 + lag 2 + lag 3 = 0					-0.016	

(continued)

Table A5. (continued)

	Percentage Misdemeanor					Δ Percentage Misdemeanor
	(1)	(2)	(3)	(4)	(5)	(6)
Controls	No	Yes	Yes	Yes	Yes	Yes
Fixed effects	Yes	Yes	Yes	Yes	Yes	Yes

Note: Standard errors in parentheses.

Source: NYC Open Data; NYPD; ACS.

* $p < .10$, ** $p < .05$, *** $p < .01$.

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56. Misdemeanors are relatively small crimes (at the magnitude of petty theft, assault, etc.), which citizens may or may not choose to report to the police, but the same cannot be said for *violations* or for *felonies*. While *violations* tend to be overly small transgressions that are not criminal and come with a warning (such as disorderly conduct and harassment), *felonies* on the other hand include serious crimes that are very likely to be reported regardless of civic engagement levels, such as armed robberies or murders.
57. It is possible that some 311 noise complaints evolved into misdemeanors and are thus counted both as 311 complaints and misdemeanors. However, results should not be biased since the independent variable is duration of 311 call rather than total count. As long as we do not expect that a 311 fix duration of a noise complaint filed last year affects misdemeanor complaints this year, then these results should not be affected by the overlap between noise and misdemeanor complaints.
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