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Policing Gentrification: Stops and Low-Level Arrests during Demographic Change and Real Estate Reinvestment

Brenden Beck*
University of Florida

Does low-level policing increase during gentrification? If so, are police responding to increased crime, increased demand by new residents, or are they attempting to "clean up" neighborhoods marked for economic redevelopment? To address these questions, I construct a longitudinal dataset of New York City neighborhoods from 2009 to 2015. I compile data on neighborhoods' demographics, street stops, low-level arrests, crimes, 311 calls to the police, and—using a novel measure—property values. Maps, spatiotemporal modeling, and fixed effects regressions compare changes in stops and low-level arrests to changes in several measures of gentrification. I find, on average, calls to the police increased after a neighborhood's middle-class population grew. Calls did not translate into more stops or low-level arrests, however. Net of crime and spatial autocorrelation, police made more order-maintenance and proactive arrests following real estate market growth, suggesting development-directed policing. Property value growth in wealthy and already gentrified neighborhoods was not associated with an increase in arrests, underscoring policing's role in early-stage urban "renewal." The article includes an analysis of three sources of property value data.

INTRODUCTION

Does low-level policing intensify during gentrification? There are several reasons to think it will. Each of gentrification's subsidiary elements—new residents and capital reinvestment—could act as a catalyst for heightened law enforcement. White, middle-class people typically have higher opinions of the police and have had few negative interactions with them. So, white, middle-class in-movers might demand more policing than previous residents out of habit or to try and assert control over the norms in their new neighborhoods. Real estate developers and landlords, seeking protection for their properties, might request police department managers deploy more officers to "clean up" neighborhoods undergoing reinvestment. Even absent demands by gentrifiers or real estate elites, police might attempt to deter crime, which some research suggests increases during gentrification, by assertively making discretionary stops and arrests.

If gentrification increases low-level policing, the consequences could be severe. Recent scholarship has demonstrated the far-reaching impacts of stops and low-level arrests for

*Correspondence should be addressed to Brenden Beck, Department of Sociology, Criminology, and Law, University of Florida, 330 Newell Drive, Gainesville, FL 32603; Brenden.Beck@ufl.edu.

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police, the people detained, their families, and their communities. A focus on stops and low-level arrests takes police away from responding to violent crime, and years of studies have shown the costs of misdemeanor-focused policing are not outweighed by the small or null crime deterrence benefits (Gelman, Fagan and Kiss 2007; Greenberg 2013; MacDonald, Fagan and Geller 2016; National Academies of Sciences 2017; Peters and Eure 2016; Weisburd et al. 2015;). Exposure to even one stop or arrest can be traumatic for the person detained. It can threaten their employment, family ties, health, school performance, and immigration status (Fagan et al. 2010; Geller et al. 2014; Legewie and Fagan 2018; National Academies of Sciences 2017). A single arrest increases the likelihood a juvenile arrestee will reoffend (Liberman et al. 2014). Pervasive, low-level enforcement also delegitimizes police among neighborhood residents not detained (Howell 2009; Jashnani et al. 2017; Tyler 2005). If police increase stops and low-level arrests during gentrification, the results could be profound for both individuals and neighborhoods, compounding gentrification's other consequences.

A host of qualitative research has found a policing–gentrification link, while quantitative studies on the topic are less numerous. Studies in New York, Chicago, Los Angeles, San Francisco, Seattle, New Orleans, Vancouver, and Washington DC all describe long-term residents who report police make more stops and arrests during gentrification (Fagan and MacDonald 2012; Freeman 2006; Herbert 2009; Lyons et al. 2017 Maharawal 2017; Parekh 2015; Rai 2011; Saunders and Kirby 2010; Smith 1996). Some studies identified new residents' increased demand for policing as the key mechanism, while others zeroed in on city elites designating certain neighborhoods for upscaling and enforcement. Considering this wealth of qualitative data, there is surprisingly only a trio of quantitative studies explicitly investigating the policing–gentrification relationship. Sharp (2013) analyzed 180 large cities and found cities with more residents employed in "creative class" jobs make more order maintenance arrests, net of crime rates. Beck and Goldstein (2017) also analyzed large cities and found increases in housing prices were associated with increases in municipal spending on police. Laniyonu (2017), the only neighborhood-level study to date, analyzed police stops made in New York City. He used a binary definition of gentrification based on whether an area saw an increase in people with a B.A. degree and an increase in rents reported to the American Community Survey. Places that underwent gentrification by this definition saw a statistically significant increase in police stops in three of the five years studied. With the current project, I extend these studies by using longitudinal data at the neighborhood level to model change over time and I disaggregate gentrification to capture its class, race, and property value components.

I hypothesize policing will intensify in response to two distinct aspects of gentrification: demographic changes and real estate market changes. Demographic change occurs when middle-class and white people move into a previously working-class, predominantly non-white neighborhood. The new residents often demand different retail options like coffee shops, cocktail bars, and clothing boutiques (Ocejo 2014; Papachristos et al. 2011; Zukin et al. 2009). The newcomers organize to change the neighborhood's schools and reshape its atmosphere and amenities (Billingham and Kimelberg 2013; Freeman 2006). In several high-profile cases, white people called the police on nonwhite people for barbequing, selling water, and sleeping in a library (Weaver 2018). Researchers conducting systematic studies have found white people are more likely to call the police than non-white people (Tyler 2005). Thus, I expect white and middle-class in-movers will demand more policing in the form of 311 calls, 911 calls, attendance at police-community meetings, and votes for "law and order" city officials. I further expect police will, in response, make more stops and low-level arrests.

The second aspect of gentrification I measure is real estate market growth. Landlords, real estate speculators, and city officials can spur gentrification by marking neighborhoods for economic redevelopment. During this process, developers invest in neighborhoods previously low in capital. They produce new units and bid up the price of real estate. Meanwhile, city officials contribute to the redevelopment to seek economic growth and its attendant tax revenue. They rezone neighborhoods, designate some houses for historic preservation, and add amenities like bike lanes. As with gentrification's demographic changes, I expect police will respond to changes in real estate markets by attempting to "clean up" neighborhoods marked for upscaling through increased discretionary arrests like those for drug possession. Drawing on theories of urban political economy (Logan and Molotch 1987; Smith 1996), I refer to this as development-directed policing.

To examine low-level policing, I measure changes in three police actions: pedestrian stops, order-maintenance arrests, and proactive arrests. The street stop was a central element in the widely publicized stop-question-frisk strategy the New York Police Department (NYPD) used between 2002 and 2012. Under stop, question, and frisk, police needed "reasonable suspicion" to stop a pedestrian, a lower legal threshold than the "probable cause" needed for an arrest. At its peak in 2011, the NYPD made nearly 700,000 stops, 84 percent of them of black or Latino people (Lenehan 2017). Stop and frisk is sometimes conflated with order-maintenance policing, but the two differ in important respects. Order-maintenance policing (also called "broken windows" policing) focuses on making arrests for quality-of-life offenses like disorderly conduct, property damage, or trespassing (Beck 2017; Braga et al. 2015; Sharp 2013; Weisburd et al. 2015). While the NYPD sharply curtailed their use of stop and frisk in 2012 following public outrage and a federal court decision, they used order-maintenance policing widely through 2015, the last year of this study (see Figure 2 below for graphs of the frequency of each police practice over time).

Proactive arrests are a third distinct aspect of low-level policing. An officer usually witnesses the offending behavior when making an order-maintenance arrest, whereas proactive arrests are for offenses not easily visible like drug possession, weapon possession, and driving while intoxicated (NYPD 2015, footnote 8). Proactive arrests are "proactive" in that they require the police to pursue and search a suspect (National Academies of Sciences 2017). While many policing strategies can be described as "proactive," and policing terminology is notoriously imprecise, I use the term "proactive policing" here to mean arrests for these not-easily-visible, misdemeanor offenses. Proactive arrests often result from street stops, but can also stem from investigations.

These three low-level tactics allow police wide discretion. The courts have given police great latitude in deciding what qualifies, for instance, as "reasonable suspicion" or "disorderly conduct." Police managers are also afforded wide discretion in deciding where to deploy street-level officers and whether to encourage them to make low-level stops and arrests. While police might deploy in areas where residents are requesting they do so, most stops and arrests are made in response to police decisions, not victim complaints. As a result of this discretion, order-maintenance and proactive arrest rates are more volatile over time compared to rates of arrests for violent crime that are driven by victim complaints (Chauhan et al. 2017). Another characteristic shared by street stops, order-maintenance arrests, and proactive arrests is their racial disproportionality. The NYPD targets black and

Latino people at higher rates than their share of the population or their offending rates (Fagan et al. 2010; Gelman, Fagan and Kiss, 2007; Stoudt, Fine and Fox, 2011). Research into the efficacy of order-maintenance policing and stop and frisk has largely found the practices do not reduce crime or their modest crime reductions are not worth their costs (Gelman, Fagan and Kiss, 2007; Greenberg 2013; MacDonald, Fagan and Geller 2016; National Academies of Sciences 2017; Weisburd et al. 2015). Research into the crime-deterring effects of proactive policing is nascent and more mixed (National Academies of Sciences 2017).

Because gentrification is characterized by differences over time, cross-sectional data provide a limited window into the process. To account for gentrification's dynamism, I use longitudinal data to measure change over time in each element of gentrification (race, class, and property value) between 2009 and 2015. I compare the gentrification measures to the three measures of low-level policing in fixed effects models to analyze change within neighborhoods, not differences between neighborhoods. Using a panel of 501 census tracts across the seven years, I estimate the relationship between policing and gentrification.

Results indicate every 5 percent growth in the typical eligible-to-gentrify neighborhood's real estate value was associated with 0.2 percent more order-maintenance arrests and 0.3 percent more proactive arrests, net of crime. When more white people moved into a neighborhood, police made more order-maintenance arrests, but fewer stops and proactive arrests. More middle-class people in a neighborhood were related to more 311 calls to the police, but unrelated to more low-level police actions, and 311 calls themselves were unrelated to the three low-level police actions. In the "Data and Methods" section, I contrast the data reliability of three measures of property value and in supplementary models I control for spatial autocorrelation.

CRIME, COMPLAINTS, AND NEW RESIDENTS

During gentrification, crime and demographic shifts are likely two major levers of low-level police changes, and these have been foci of past research. Differential offending theory attributes variation in stops and low-level arrests to variation in crime rates (Engel and Cohen 2014; Gaston 2018). Police do not make more stops and low-level arrests in poor and non-white communities because of racial bias or demands from new middle-class residents, but because there is more crime in those places, according to the theory. Order-maintenance policing and proactive policing are designed to focus on "high-risk people" and "high-risk places" (National Academies of Sciences 2017). So, to differential offending theory, racially disproportionate stop and arrest rates are collateral consequences of targeting crime that is disproportionately committed by non-white people. In the present study, this theory will find support if gentrification is related to a neighborhood's stop and low-level arrest rates only through its relationship to crime.

Past scholarship suggests crime at first increases then modestly decreases during gentrification (Kreager, Lyons and Hays 2011). Gentrification is associated with more nonlethal violent crime like street robberies and aggravated assaults, but associated with fewer homicides (Boggess and Hipp 2014; Papachristos et al. 2011). Studies linking gentrification to increases in crime have largely been unable to identify mechanisms linking the two trends. Perhaps gentrification's displacement of long-term residents aggravates tensions between groups or perhaps new, wealthier residents have more possessions to steal. As for gentrification's suppressive relationship to homicides, research suggests public investment in housing is one mechanism linked to both more gentrification and reductions in murder (Fagan and MacDonald 2012). Based on crime's increase during at least early-stage gentrification, differential offending theory anticipates an associated rise in stops and low-level arrests.

Much has been written on the potential deterrent effects of order-maintenance arrests on crime. The bulk of this research suggests "generalized aggressive use of increased misdemeanor arrests as a means to controlling disorder in a broken windows strategy generates small to null impacts on crime" (National Academies of Sciences 2017:4–44). The current study, however, asks about the opposite causal direction: do low-level arrest rates change in response to changes in crime? Several studies find an effect of crime on arrest rates (e.g., Chappell, MacDonald and Manz 2006; Parker and Maggard 2005). These studies support differential offending theory insofar as the theory expects that, when making stops or arrests, police respond to crime levels. It is important to stress, however, this theory suggests it is *only* crime, and not real estate or demographic changes, to which police respond.

A large body of qualitative research into gentrification's demographic changes finds low-level policing intensifies as more white and middle-class people move into a neighborhood. Studies have found long-term residents describe a policing-gentrification link in places as disparate as New York City, Chicago, Los Angeles, San Francisco, Seattle, New Orleans, Vancouver, and Washington DC (Fagan and MacDonald 2012; Freeman 2006; Herbert 2009; Lyons et al. 2017; Maharawal 2017; Parekh 2015; Rai 2011; Saunders and Kirby 2010 Smith 1996). Two studies have speculated it is new neighbors' demands for police that spur more policing (Freeman 2006; Parekh 2015). One long-term resident of gentrifying Harlem put it this way to sociologist Lance Freeman: "[I]f you sit on the benches the police will come along and point to the no loitering sign and say you can't stay here. [This is] because of new people moving in and putting pressure on the police to make things orderly" (2006:105). Legewie and Schaeffer (2016) found calls to New York's nonemergency 311 hotline increased during gentrification because, the authors theorized, new residents are "contesting boundaries," leading to clashes in competing visions of normative neighborhood behavior and upkeep. Legewie and Schaeffer did not disaggregate 311 calls made to the police versus other city agencies, however. The present study will isolate 311 calls to the police.

While not in the context of gentrification, there is quantitative evidence supporting the qualitative findings that white and middle-class people demand more policing. White people and middle-class people are more likely to be satisfied with the police than non-white and working-class people (Norman 2017; Wu et al. 2009). Whites' stronger trust in policing is explained, in part, by their having reported better past treatment by police and their believing racial profiling and police harassment are rare (Tyler 2005). Black and Latino people, in contrast, will sometimes hesitate to call the police out of fear they will become suspects (Lerman and Weaver 2014). Thus, past research suggests calls to 911 or 311 (New York's nonemergency hotline) and demands from the public during police–community meetings, which every New York City precinct holds monthly, might intensify during gentrification. Even absent resident demand, police might make more stops and low-level arrests in response to white people moving to a neighborhood. There is research suggesting police respond to 911 calls faster when the calls come from white neighborhoods, though the study was conducted by an advocacy group (ACLU Illinois 2014).

One study explicitly measured the relationship between gentrifiers and policing. Sharp (2013) compared the amount of order-maintenance policing across postindustrial cities in 2003. Sharp operationalized "postindustrial" cities as those with relatively larger shares of college-educated service workers, the typical gentrifier. The study measured order-maintenance policing as the share of all arrests for low-level offenses like disorderly conduct, drunkenness, and loitering. In cross-sectional regression models of 180 large U.S. cities, Sharp found more postindustrial workers and more residents with a B.A. were associated with an increased share of order-maintenance arrests. Sharp found support for a class-based gentrification metric relating to more low-level policing. As it is a cross-sectional study, however, it provides a limited view into gentrification as a process over time. Also, gentrification is a neighborhood-level phenomenon and Sharp (2013) used cities as the unit of analysis. Often, cities will experience aggregate declines in white people, middle-class people, and real estate investment while certain neighborhoods within the cities are rapidly gentrifying. The present study uses longitudinal models at the neighborhood level to capture these granular and processional changes.

DEVELOPMENT-DIRECTED POLICING

The research mentioned above addresses the relationship between neighborhood demographic change and policing. There has been less research into the economic development inducements to increased policing during gentrification. Such an approach shifts the focus away from the gentrifiers and toward the real estate market to examine development-directed policing. Such a supply-side theory examines how, during gentrification, landlords and real estate developers work with city government to extract higher rents from both long-term and new residents and thereby generate economic growth (Logan and Molotch 1987; Smith 1996). This theory expects that an "urban growth machine" populated by a coalition of landlords, elected officials, and real estate elites will use police as another mechanism to increase the economic productivity of previously dis-invested neighborhoods. As real estate developers buy properties, refurbish old housing, and build new units, they encourage policing activity with the hope police will remove homeless people, displace poorer tenants, and encourage wealthy in-movers. Order-maintenance policing, a strategy already geared toward removing "disorder," will accelerate as that disorder becomes a threat not just to public safety, but also to economic development and its attendant tax revenue.

Historically, New York City has seen at least two instances of such a police–business alliance. In 1985, mayor Ed Koch and real estate developers were encouraging the "renewal" of the Lower East Side neighborhood of Manhattan (Smith 1996). To further the neighborhood's redevelopment, the NYPD launched "Operation Pressure Point" there, making 1,300 arrests, mostly for drugs, in 18 days (Greer 1985; Smith 1996). An article in the *New York Times* wrote of the police action: "Thanks to Operation Pressure Point, art galleries are replacing shooting galleries," a reference to places where people "shoot" intravenous drugs (Greer 1985). The *Times* reporter noted this was spurring higher rents, writing, "many [Lower East Side] residents fear the Police Department's success may carry with it the price of displacement" (Greer 1985). Similarly, in Manhattan's Times

Square neighborhood in 1994, the city pursued a development agenda that included tax incentives for developers to build there and also included aggressive arrests of people for prostitution, vagrancy, loitering, and other petty crimes (Fagan and MacDonald 2012:29).

Policing to protect private investment is not unique to New York City. The city government of Wichita, Kansas, has been trying, mostly unsuccessfully, to rebrand the city's Skid Row neighborhood as "Old Town" for over 50 years. During times of intense redevelopment, they have directed the police to aggressively enforce loitering and public drinking laws there with the hope of permanently displacing the area's homeless residents (Billingham 2017). Similar development-directed policing projects have been detailed in Los Angeles, Seattle, and Charlotte (Beckett and Herbert 2009; Moore and Poethig 1999; Stuart 2016). Policing for growth is not always initiated by city government. In Wichita in the 1990s, police collaborated with local business owners to lobby the city to tighten the public drinking laws and to prohibit the feeding of homeless people (Billingham 2017). Some police departments have so thoroughly adopted the growth machine ideology they have included their cities' property values as a performance metric alongside crime rates (Moore and Braga 2003). There is little evidence policing can improve property values, but many cities are linking real estate and policing nevertheless.

The case studies discussed above center on high-profile redevelopment projects, usually in cities' central business districts. There are fewer quantitative studies that might illuminate whether policing to protect private investment extends into neighborhood renewal projects outside downtowns. The few extant studies suggest it does. At the city level, Beck and Goldstein (2017) investigated whether cities relying heavily on their real estate sector for economic growth during the 1990–2008 housing bubble exhibited correspondingly larger police budgets. Using two measures of housing market reliance (mortgage originations and a housing price index) across 171 cities, they found an association between larger housing bubbles and larger police budgets. They also present quotes from police chiefs and mayors crediting police with growing home prices. Because gentrification occurs at the neighborhood-level, however, this city-level finding is merely suggestive in the context of gentrification.

Laniyonu's study (2017) is the most robust quantitative analysis of the police-gentrification dynamic to date. Laniyonu found, in repeated cross-sectional models, that gentrification in New York City was associated with more police stops in three of the five years analyzed. The study used a binary measure of gentrification derived from changes in residents' education levels and changes in rents. Laniyonu also accounted for spillover correlation effects by including spatially weighted variables. This spatial analysis required the regressions be performed on all tracts in New York City, not only those eligible for gentrification. This approach has two drawbacks. First, the flattening of the gentrification variable into a binary one obscures which aspect of gentrification is interacting with increased police stops. Second, it limits analysis of covariates (like 311 calls and percent black) to the city writ large, not gentrifying tracts. Building on Laniyonu, I disaggregate gentrification's demographic and real estate market elements using more precise continuous measures. I use longitudinal analysis to capture the processional nature of gentrification. And, I use distance decay spatial weights which allow me to analyze spillover effects in tracts eligible for gentrification separately from those too wealthy to gentrify. I also analyze low-level arrests in addition to stops.

A theory of policing focused on demographic change highlights new residents as the levers of change. A theory of real estate market change highlights landlords and city elites as the levers of change. Both such demand- and supply-side theories expect policing will intensify during gentrification but differ as to why. The two are not mutually exclusive. It is possible that, in the context of landlords maximizing growth potential, new residents will demand more policing. Differential offending theory is less compatible with these other explanations. It explicitly disavows any relationship between policing and demographics or real estate investment. I will test which, if any, of these approaches finds support in the data.

DATA AND METHODS

I tested these theories using a longitudinal dataset of 2,038 New York City Census tracts from 2009 to 2015. Availability of Census data dictated the time period. Census tract boundaries changed in 2010, so I harmonized data from 2009 into the new boundaries using the Longitudinal Tract Database (Logan et al. 2014). I define "neighborhood" as "tract" and use the terms interchangeably.

When researching gentrification, it is important not to compare tracts that were eligible for gentrification to those that were already gentrified or too wealthy to be gentrified. So, I distinguish two subsamples: tracts eligible for gentrification in 2009 and tracts not eligible. To determine eligibility, I use Freeman's (2005) criteria: at the start of the study period, 2009, eligible tracts had a median household income below the city median and had a percentage of housing units built since 1990 below the city median. There were 501 tracts eligible to gentrify in 2009. Figure 1 is a map of eligible tracts. The unit of analysis is the tract-year.

OUTCOME VARIABLES

To operationalize low-level policing, I use three annual measures: *street stops, order maintenance arrests,* and *proactive arrests*. Each is a rate per 1,000 residents and each is publicly accessible through the New York City Open Data Portal. I placed the incident-level data in their Census tracts using the 2010 Census TIGER/Line Shapefile tract map with Stata's "geoinpoly" and "shp2dta" operations (Picard 2015). I log these and all variables to reduce outliers and induce normality.

Stops data come from the NYPD's "Stop, Question, and Frisk" dataset. The stop, question, and frisk policing strategy that motivated the NYPD's widespread use of street stops was sharply curtailed in 2012 after a federal court found the strategy as practiced was unconstitutional and after considerable public outcry. See Figure 2 for a graph of the practice's sharp decline. Not only did the quantity of street stops drop off following 2012, the accuracy of the stop data diminished as well. A report by a federal court monitor found police failed to record 30 percent of stops they made in 2015 (Baker 2016). So, in line with other recent studies, I limit the models of stops to pre-2013 data (see, e.g., Goel, Rao and Shroff, 2016; Legewie and Fagan 2018; Rosenfeld and Fornango 2017).

Order maintenance arrests are from the NYPD's Historic Arrest Database. The NYPD classify these data differently than the FBI does in the national UCR dataset. So, I cannot perfectly match previous, national studies, but the categories I classify as order maintenance arrests resemble past research operationalizations as closely as possible (see, e.g., Beck 2017; National Academies of Sciences 2017; Sharp 2013). The NYPD offense categories

FIG. 1. Map of gentrification eligibility, 2009.

that I aggregate into *order maintenance arrests* are: prostitution, criminal mischief, offenses against the public order and public sensibilities, disorderly conduct, and theft of services. Criminal mischief is a category including property destruction and vandalism charges. Offenses against the public order include the vague offense "aggravated harassment in the second degree" that has largely replaced disorderly conduct in New York. Theft of services, or turnstile jumping, is most often riding public transit without paying a fare.

The *proactive arrests* data also come from the NYPD Historic Arrest Database. The NYPD defines proactive arrests as those where "a substantial portion" of crimes were identified as a result of the implementation of proactive policing strategies rather than complaints by victims (NYPD 2015, footnote 8). They are arrests for trespassing, drug possession, weapon possession, intoxicated driving, and possession of stolen property (NYPD 2015, footnotes 1, 5, and 8). Relying on practitioner definitions runs the risk of accepting, rather than showing, a phenomenon. So, here, proactive arrests serve as a compliment to order-maintenance arrests, not as a replacement. There are no arrest types that overlap the order-maintenance and proactive arrest variables. Order maintenance arrests were 13 percent of all arrests in 2015 and proactive arrests were 8 percent.

When using any police-generated data on low-level crimes, caution is needed because of frequent measurement error (Berg and Lauritsen 2016; Lauritsen, Rezey and Heimer 2016). This is less of a concern here because I am using low-level arrests as a measure of police activity rather than as a measure of crime. Nevertheless, to test the validity of the two arrest measures, I also constructed a three-year average of each variable to smooth the year-over-year measurement variation and I reran the models. The results were substantively the same. I use the single year figures here to preserve the 2009 and 2015 values which the averaging process truncates. Measuring order maintenance and proactive arrests rather than all arrests has the added benefit of isolating police activity from underlying crime. Because police officers have discretion in whether to make an order maintenance or proactive arrest, these rates reflect police decisions and enforcement priorities more than actually existing amounts of crime.

To measure resident demand for low-level policing, I use the number of calls to New York City's nonemergency, 311 hotline per 1,000 residents. The 311 hotline gives residents an alternative to 911 that they can choose to call to complain about city conditions like building code violations, uncollected trash, and other quality-of-life offenses. From the universe of all 311 calls, I create *311 calls to the police* by counting all calls that residents directed to the NYPD and all calls reporting crimes that callers directed to another agency. The most common such complaints were those for: noise, parking violations, violations of park rules, unleashed dogs, smoking, illegal animal—sold/kept, and graffiti. The ideal dataset would also include 911 emergency calls, but New York City does not publicly release those. Not having 911 call data means this variable does not include reports of moderately serious offenses like drug selling and trespassing that might also increase as new residents move into a neighborhood. However, 311 data will well-capture demand for order maintenance policing as it collects complaints about disorder. It is also likely residents who call 311 are more likely to call 911. As an outcome variable, *311 calls to the police* will indicate whether an influx of gentrifiers increases demand for police. For the models of stops and low-level arrests, *311 calls to the police* will also be used as an explanatory variable to capture demand by new residents.

EXPLANATORY VARIABLES

To assess the relationship between crime and arrests, I use each neighborhood's *violent crime rate*. It is an annual rate per 1,000 people of murder, manslaughter, robbery, and felony assault crimes made known to the police. It comes from the NYPD Historic Complaint Database of all crimes made known to the police. The measure excludes rapes because they are not attached to location data to preserve anonymity of victims. I use the violent crime rate instead of the total crime rate to avoid, as much as possible, subjective classification decisions by police. Researchers frequently use the number of murders as a proxy for objective levels of crime since homicides rarely suffer from under- or over-reporting or from misclassification (Lauritsen, Rezey and Heimer 2016). Many tracts in this study had zero murders, however, so I use the nearest option without zeros, the measure of violent crime. I run alternative models, presented below, with property crime and robbery as additional crime measures.

There are many ways to measure gentrification, and I try to capture several different approaches. Some scholars foreground race in their definitions. They study the increase in white people to a neighborhood (e.g., Maharawal 2017; Vigdor 2002). Others ignore racial changes, measuring changes in neighborhoods' average income (e.g., Furman 2016; Landis 2016), usually combined with changes in educational attainment and/or occupational status (e.g., Freeman 2005; Hwang and Lin 2016; Timberlake and Johns-Wolfe 2017). Following the split in the literature about whether to include ethnoracial measures I measure race and class separately. To measure an influx of new, white residents, I use the *percent non-Hispanic white* of a neighborhood's population.

To measure middle-class in-movers, I construct an *index of class demographics* using factor analysis of four key measures: a tract's median household income, the percent of the tract with a B.A. degree or higher, the percent employed as professionals or managers, and the percent of families not in poverty. These conditions are highly correlated and load on the same factor. With an eigenvalue of 2.7, the factor contains high loadings (0.72–0.85) for each component variable. I calculated a factor regression score that weights each variable by its factor loading and joined this measure to the associated Census tract-year. Class and race data come from the Census Bureau's American Community Survey's 5-year estimates.

COMPARING MEASURES OF PROPERTY VALUE

Following Hwang's (2016) call to build better quantitative measures of gentrification, I use property tax data to measure *property value per unit*. This is among the first studies to use this measure of gentrification, and I believe it is a substantial improvement on Census surveys and sales data. A colleague and I gathered tax assessment data by requesting the yearly property tax rolls from the New York City Department of Finance (DOF) for 2008–2016. The data include the assessed market value for every building in New York before and after exemptions. To avoid the subjective influence of housing policy and exemption laws, I use the DOF's full market value, the value before any exemptions or adjustments. The DOF uses historical pricing data, new sales, and assessor evaluations in regression models to estimate the value of every property in the city. This includes incorporating recent sale prices to update the values of nearby properties that have not been recently sold. The DOF omits any non-arms-length, below-market-value transactions between family members (Rolandi 2017). I adjusted the raw data from fiscal to calendar year. I geocoded each property using the New York City PLUTO dataset. I used Texas A&M's Geocoding Services for the addresses that PLUTO could not code. As with the arrest and crime data, I joined each property with its census tract using Stata's "geoinpoly" operation. This created the *property value per unit* variable. It includes both commercial and residential properties and excludes government-owned property.

One potential downside of tax assessment data is that it is a lagging indicator of market value. This can be corrected, at least in New York City, where every building is reassessed every year, incorporating new transactions data. I confirmed with the City Assessor that sales data are incorporated into tax assessments (both for sold properties and to update the values of nearby properties) before the release of the next year's rolls (Rolandi 2017). The tax data reliably follow sales data by one year, then. To adjust for this, I lagged the property tax data one year, synchronizing it with the other variables.

Sale price data might be more contemporaneous and might more accurately reflect the market because it would not be filtered through assessors. I gathered DOF real estate transactions data and adjusted them for inflation to compare them to the tax assessment data and investigate their reliability. Surprisingly, sales data and property tax assessment data are very weakly correlated ($r = 0.08$ in 2015). This might be partly attributable to the infrequency of sales at the granular tract level. In 2015, the average New York City tract eligible for gentrification saw 13 of its properties sold, 0.8 percent of its stock. Seven percent of tracts had zero transactions. As one would expect with few data points and 7 percent missing data, sales data are volatile, with large year-to-year variation. The within-tract, across-year standard deviation of sale price is four times its mean. The same tax assessment standard deviation is only twice its mean. This volatility is further exacerbated by sales not coming from a representative sample of the tract's properties. Sales data are also degraded by below-market-rate sales, so-called "non-arms-length" sales, between family members. Thirty percent of property sales were for $0 in 2015, and it is likely more sales than that were for deflated costs. Indeed, even excluding $0 sales, the average sale price ($1,406,433) was about half the average tax assessment value ($2,486,587). If a lot of non-arms-length transfers occur during gentrification, the average sale price would appear to decline when actual prices are rising. While assessor fallibility might introduce some error, assessors and data staff at the DOF also use their expertise to purge below-market-rate sales from the assessment data. Transactions data are more quickly available than property tax data, and they might be accurate at large geographic scales, but tax assessment data outperform then in several respects. Tax data incorporate transactions data for properties sold and nearby properties, cover all properties in the city, are more consistent, and their noncontemporaneousness is correctable.

The most common measure of real estate price in gentrification research is the Census Bureau's survey questions on home price (used in, e.g., Boggess and Hipp 2014; Freeman 2005; Hwang and Sampson 2014). This measure is also susceptible to error. Hwang notes Census data on housing is lacking because it surveys single-family homeowners, missing buildings owned by large-scale developers and commercial businesses (2016, 228). The property tax assessment measure includes commercial properties that allow it to capture the business upscaling that is integral to gentrification like the expansion of coffee shops, boutiques, and cocktail bars (Ocejo 2014; Papachristos et al. 2011; Zukin et al. 2009). To Hwang's critique I would add three other shortcomings of the Census measure. First, the Census housing price variable measures owner-occupied, residential properties, which covered only 29 percent of units in New York City in 2015. Second, the Census measure is susceptible to survey completers not knowing, inaccurately remembering, or knowingly misreporting their home value. Finally, the Census measure top-codes home prices at $1 million, obscuring the top of New York City's housing market (6 percent of owner-occupied units in the city in 2015 were over $1 million). Property tax data better capture the top part of the market, are not susceptible to reporter error, and capture commercially owned properties and other nonowner-occupied buildings.

CONTROL VARIABLES

Because young men are the group most targeted by police, I control for the percent of a neighborhood that is *18–34 years old* and the *percent male*. These control data come from the Census Bureau's American Community Survey. The *year* variable is the number of years since 2009 and controls for macro, secular changes affecting the whole city such as rising awareness of the 311 system or mayoral directives.

All the variables from the American Community Survey are those from its five-year estimates. I treat the five-year estimate's final year as its representative year. For example, I treat the 2005–2009 figure as occurring in 2009. While this practice is not as precise as using an annual measure, annual measures for these variables are not available at the tract level. I use the five-year estimates as their final year because this positions them like a lagged independent variable. Each measurement occurred before the dependent variables were measured.

For *median income* and *property tax value*, I adjust for inflation using the consumer price index (CPI) for the New York consolidated metropolitan statistical area (MSA) to convert them to constant, 2015 dollars. I use the MSA CPI because using the national CPI would overdeflate the median income measure as New York underwent a relatively smaller economic recession than the country at large during the study years. For *median income,* I adjust using the CPI from the Bureau of Labor Statistics (BLS). For the property tax assessment, I adjust using the "CPI less shelter" figure provided by the Federal Reserve Bank of St. Louis. Because the BLS's CPI adjusts for changes in housing prices, using this full CPI when deflating property values would control away some of the real estate market fluctuation I am hoping to capture. So, I use the CPI without the shelter (housing) items included.

DESCRIPTIVE RESULTS

Table 1 reports the means, standard deviations, and percent changes for each of the above variables. I report the descriptive statistics separately for each subgroup: tracts eligible for gentrification and those ineligible.

Figure 2 graphs trends in the four dependent variables (311 calls to the police, street stops, order maintenance arrests, and proactive arrests) in New York City between 2009 and 2015 and compares them across the two neighborhood types. The criterion for gentrification eligibility is the same as that described above: those tracts with below-city-median income and below-city-median recent building construction in 2009 (Freeman 2005). Calls to the police through 311 increased sharply during the study period, as awareness of the relatively new service spread. Stops rose until 2011 and then dropped off sharply, for the political reasons described above. Stops were much more frequent in neighborhoods eligible for gentrification than wealthy neighborhoods during the pre-2012 era. Order maintenance arrests declined steadily and show much more similarity between the neighborhood types. Proactive arrests also declined over the study period everywhere. Proactive arrests resemble stops in that there is a large spread between eligible and ineligible neighborhoods, perhaps because proactive arrests sometimes result from street stops.

Figure 3 is a map of per capita order maintenance arrest intensity in 2015. Police made the most arrests in the Bronx, upper Manhattan, central Brooklyn, the north shore of Staten Island, and Jamaica, Queens. Arrests also concentrated in mid-town Manhattan near office buildings and transit hubs. Maps for proactive arrests and stops were largely

TABLE 1. Variable Means and Standard Deviations, Over Time

		Eligible-to-Gentrify Tracts, N = 501			Not Eligible-to-Gentrify, N = 1,537		
		2009	2015	Percent Change	2009	2015	Percent Change
Stops per 1,000 (through 2012)	**Mean**	85.9	73.8	−13.3	65.0	60.5	−6.9
	SD	118.6	83.6		83.0	79.7	
Proactive arrests per 1,000		8.4	4.9	−41.7	6.3	3.9	−38.1
		11.4	5.7		9.2	5.4	
Calls to police per 1,000		24.8	80.6	225.0	27.8	79.4	185.6
		16.8	43.8		23.7	55.3	
Violent crimes per 1,000		5.0	5.1	2.0	4.2	4.3	2.4
		4.4	4.2		4.2	4.3	
Class index		1.6	1.9	18.8	2.1	2.3	9.5
		0.5	0.6		0.8	0.8	
Percent BA or greater		21.0	24.5	16.1	32.8	35.7	8.8
		11.2	12.5		20.9	21.5	
Median household income		$39,750	$42,086	5.9	$65,839	$64,419	−2.2
		$10,432	$13,749		$29,594	$30,579	
Percent professionals or managers		23.7	28.1	18.6	34.6	39.1	13.0
		10.5	11.5		17.2	18.4	
Percent families not in poverty		79.0	77.5	−1.9	86.8	85.3	−1.7
		11.5	11.7		12.5	12.5	
Percent white		24.6	22.5	−8.9	38.7	35.9	−7.2
		28.0	25.3		32.9	30.9	
Property value per unit		$2,201,376	$2,314,426	5.1	$3,235,958	$3,243,783	0.2
		$11,374,129	$10,632,706		$35,604,366	$36,657,651	
Properties per 1,000		411.3	434.6	5.6	470.0	466.1	−0.8
		177.7	273.5		442.9	279.7	
Percent aged 18–34		25.6	27.4	7.0	25.7	26.8	4.3
		6.3	6.6		8.3	8.5	
Population density		26.0	26.3	1.2	18.0	18.3	1.7
		14.1	13.7		13.1	13.1	
Population		4,461	4,537	1.7	3,826	3,879	1.4
		2,333	2,361		2,064	2,056	

The last year for street stops is 2012.

similar. Comparing Figure 3 to Figure 1, the map of eligible-to-gentrify neighborhoods, some, though not all, of the eligible tracts are also tracts with a high arrest intensity.

Figures 4–6 are maps of percent changes in each of the three measures of gentrification between 2009 and 2015. Each shade of gray represents a decile in the change variable, so the middle values straddle the median change and the zero point is different for each map. Figure 4 shows that the class index (which combines income, education, poverty, and occupational status) rose the most in Manhattan below 92nd street and in the Queens and Brooklyn neighborhoods close to Manhattan. Some tracts in the outer sections of the Bronx and Queens also saw an increase in upper-class residents. Interestingly, comparing this map to Figure 1, neighborhoods that were eligible to gentrify appear to have lower class index scores in 2015 than they did in 2009, though there are some high-increase tracts intermixed.

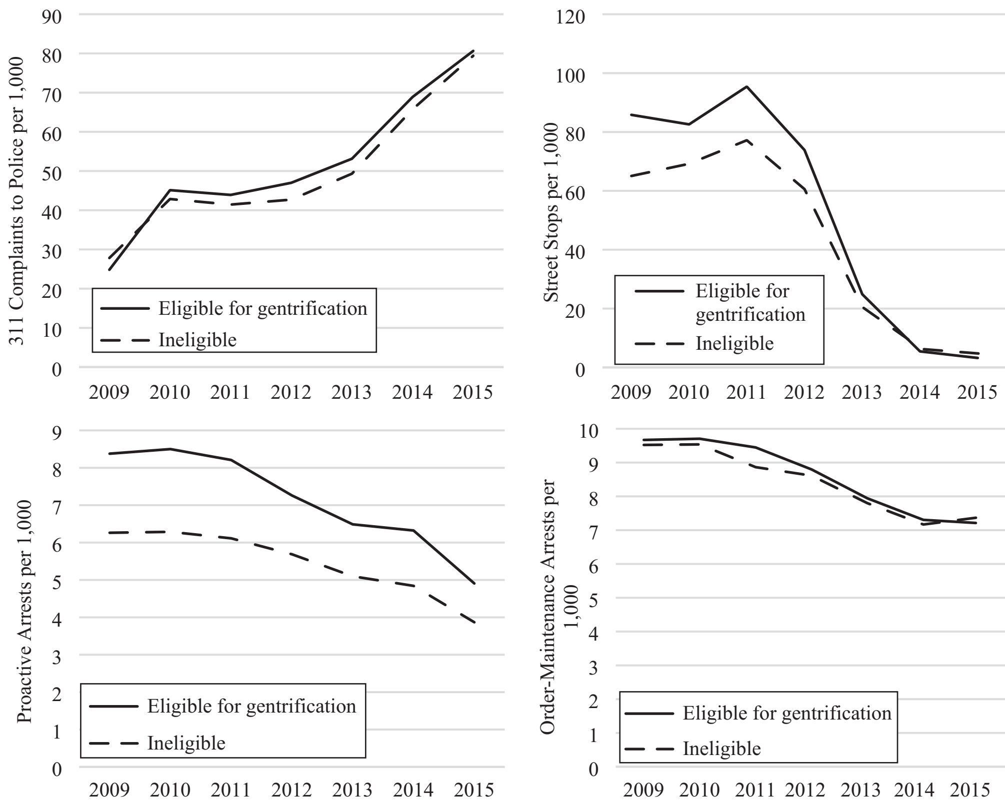

FIG. 2. Dependent variables over time, two neighborhood types.

Figure 5 shows the change in the percent of tracts that were white. The percent white map is the highest contrast of the three gentrification measure maps. Central Brooklyn, upper Manhattan, and some parts of the South Bronx display clear and clustered gentrification as measured by increases in their white populations. Notably, these neighborhoods abut white flight neighborhoods in outer Brooklyn, the Bronx, and Queens. Manhattan below 92nd Street, south Brooklyn, and Staten Island had more stable white populations.

Figure 6 shows the change in the property values of New York City's census tracts. This map displays considerable heterogeneity. Few patterns emerge, with high-increase tracts next to high-decrease tracts. Tracts further from Manhattan appear to be more stable.

These maps provide some descriptive support for the theories that discretionary, low-level policing is more intense in gentrifying neighborhoods. Some areas of intense low-level policing and areas of high increases on the gentrification measures appear to overlap. The three elements of gentrification evince somewhat different spatial patterns, suggesting they might not always co-occur. The maps do not, of course, control for variability in crime rates, 311 calls to the police, or age and gender demographics. Nor do they test the variables simultaneously or examine change over time in individual tracts. The multivariate model results below extend the analysis to address these lacks.

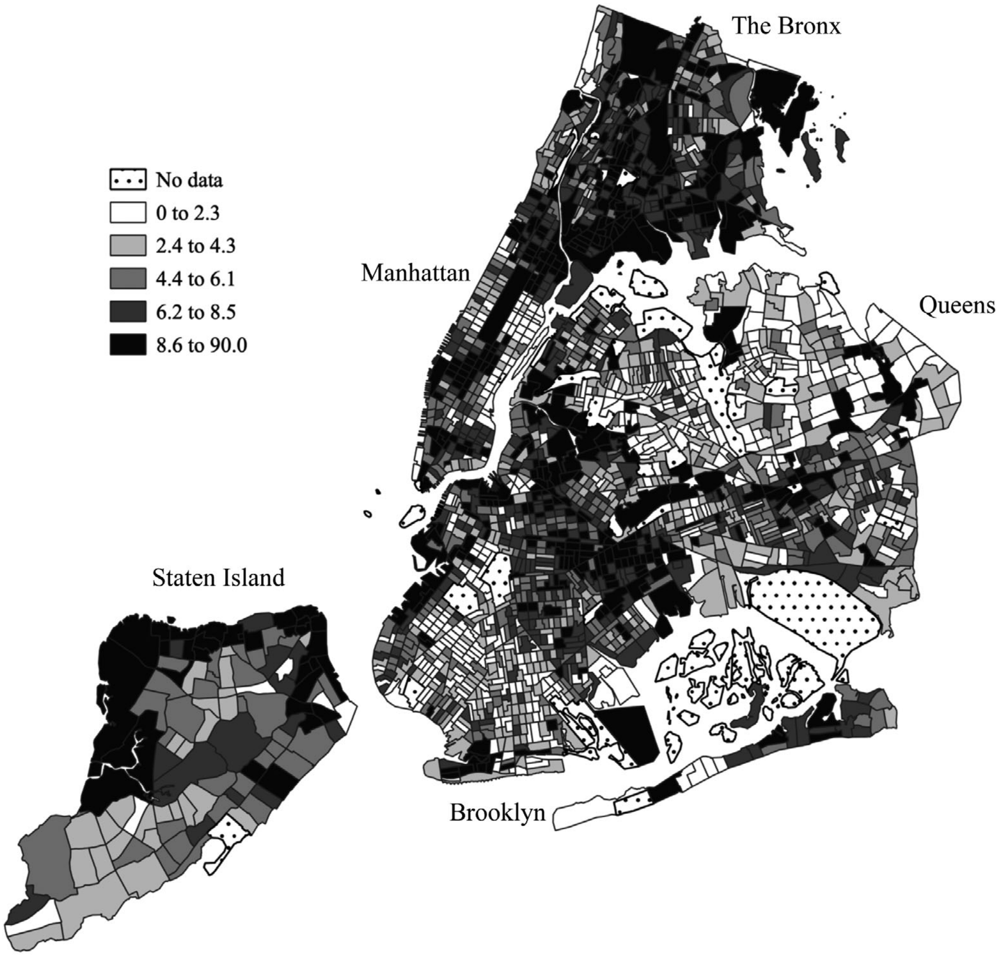

FIG. 3. Order maintenance arrests per 1,000 people, 2015.

ANALYTIC STRATEGY

To estimate the four dependent variables, I use linear, log-log regression panel models with tract and year fixed effects. Tract and year fixed effects control for all unobserved, time-invariant differences in cases (Vaisey and Miles 2017; Allison 2009). For instance, a neighborhood's proximity to the city center, its land area, and its police precinct's idiosyncrasies are controlled for in this model, provided they did not change between 2009 and 2015. A Hausman test produced a chi-squared score of 0.000, indicating a fixed effects model, and not a random effects model, is appropriate. To purge serial autocorrelation, I use a variance–covariance estimator that clusters standard errors by tract.

The models take the following form:

$$y_{it} = \alpha_i + \beta x_{it} + \upsilon_i + \varepsilon_{it},$$

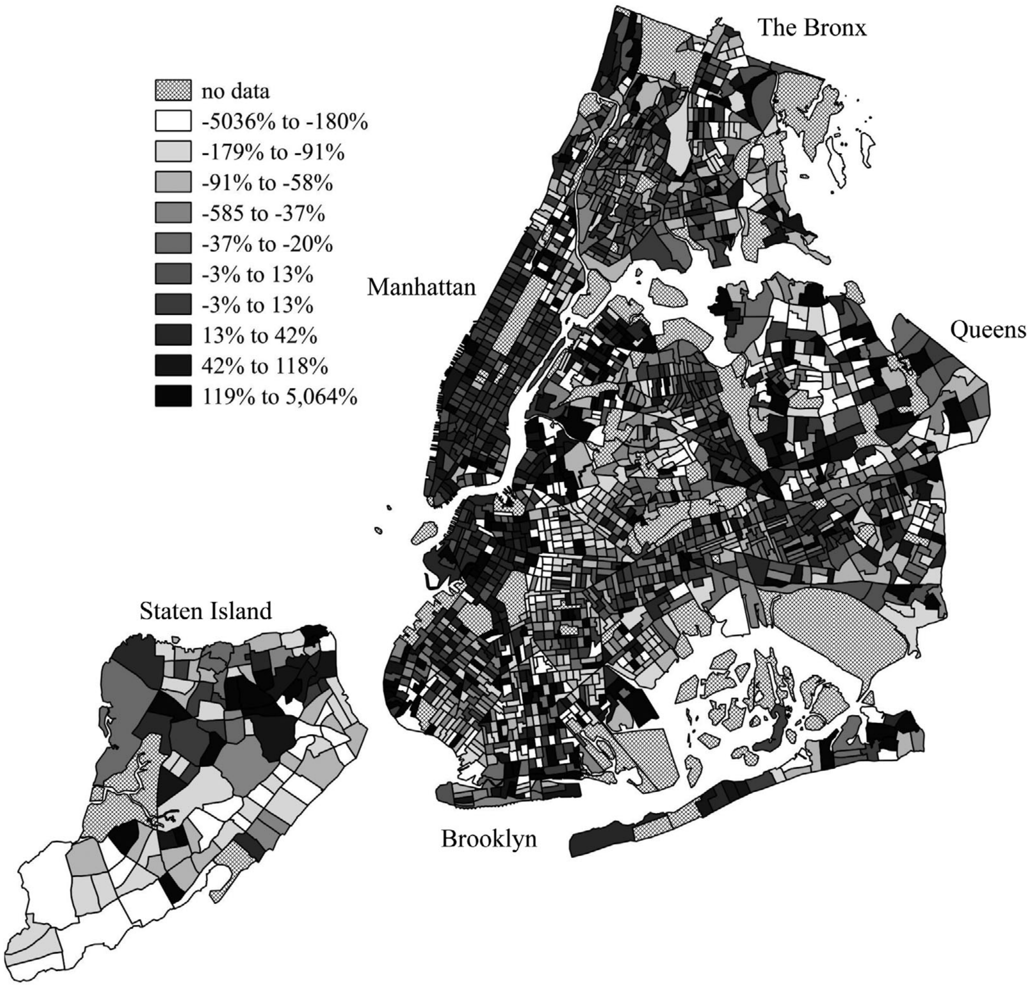

FIG. 4. Change in class index, 2009–2015.

where y_{it} is the logged dependent variable in tract i at time t, x_{it} is a vector of logged covariates, υ_i is a place-specific fixed effect, and ε_{it} is the idiosyncratic error.

As the maps above make clear, some of the gentrification trends cluster spatially. It is possible tracts influence one another across tract boundaries, inducing spatial correlation and biasing the results. The analytic strategy describe above will account for some spatial correlation. The tract fixed effects will control away any of the time-invariant influence of neighboring tracts, but any spatial influence that changes during the study period will not be accounted for. In alternate models, presented below, I run a check for such time-varying spatial dependence. I generated a spectral-normalized, inverse-distance weighting matrix among the eligible-to-gentrify tracts. I modeled the correlation of neighboring tracts on the gentrification variables by adding spatially weighted independent variables and including a spatial lag of the error term. There was minor spatial correlation

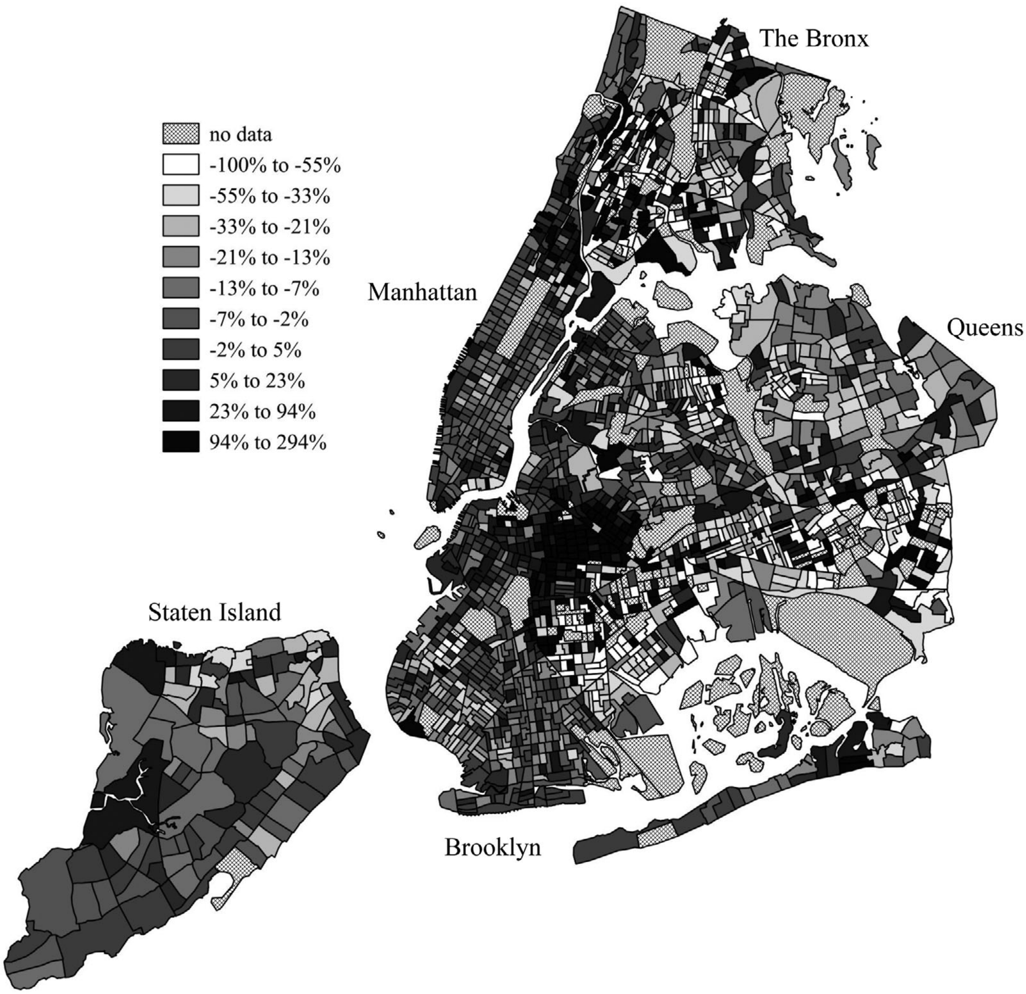

FIG. 5. Change in percent white, 2009–2015.

among the independent variables, and more significant dependence in the error term. Unfortunately, adjusting the error term for spatial correlation precludes adjustment for serial autocorrelation. A single model cannot account for both. So, I compared the spatially adjusted models to those with serially adjusted error terms and compared both to an unadjusted model. The spatially adjusted model more closely resembled the unadjusted one. As one might expect, serial autocorrelation that comes from within tracts and cannot be even partially purged by tract fixed effects is the bigger threat to model accuracy. So, I chose the more conservative modeling strategy, the one controlling for serial autocorrelation. Fortunately, in all three models, the results were substantively unchanged.

With seven years of data across 501 tracts eligible for gentrification, and using listwise deletion to address missing data, the N for the eligible-tract models of 311 calls and of low-level arrests is 3,498 tract-years. There were 1,537 ineligible tracts, creating an N of

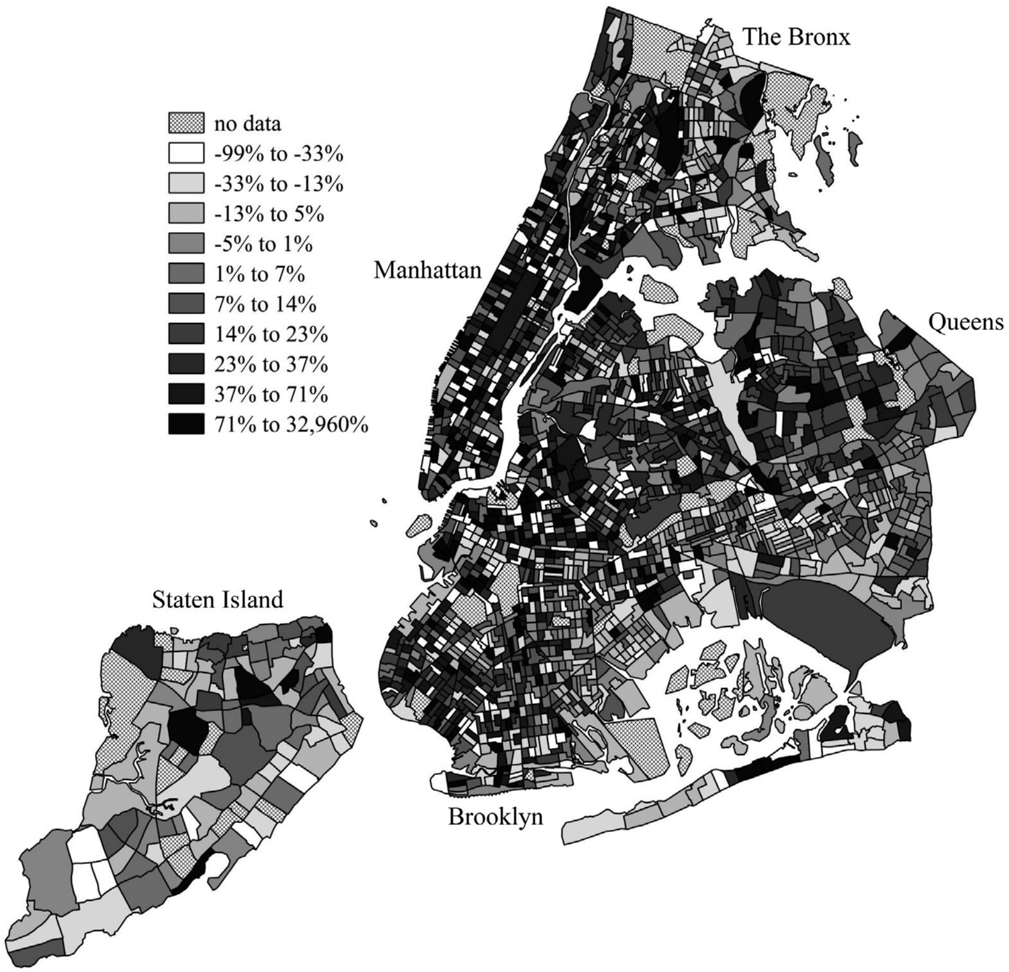

FIG. 6. Change in property values, 2009–2015.

10,754 tract-years for the ineligible-tract models. The models of street stops include data only through 2012, so had Ns of 1,999 and 6,138, respectively. I limit all samples to tract with more than 1,000 residents in 2009.

MULTIVARIATE RESULTS

To estimate changes in demand for policing associated with gentrification, Model 1 in Table 2 regresses 311 calls to the police on the demographic and control variables. Model 1 was run on only the tracts eligible for gentrification. Because both the explanatory and outcome variables are logged, the coefficients are elasticities, and can be interpreted as the percent increase in the outcome variables associated with a 1 percent increase in the independent variable.

TABLE 2. Primary Coefficients (and Standard Errors) of Linear, Log-Log Regression Models with Tract and Year Fixed Effects

	Tracts Eligible to Gentrify				Tracts Ineligible to Gentrify			
	Model 1 311 Calls to the Police	Model 2 Street Stops	Model 3 Order Maint. Arrests	Model 4 Proactive Arrests	Model 5 311 Calls to the Police	Model 6 Street Stops	Model 7 Order Maint. Arrests	Model 8 Proactive Arrests
Violent crime rate	0.02	0.07	0.08***	0.01	−0.00	0.08***	0.06***	0.04*
	(0.02)	(0.04)	(0.02)	(0.03)	(0.01)	(0.02)	(0.01)	(0.02)
Class index	0.22*	0.17	0.09	−0.06	0.18***	0.05	−0.06	−0.17*
	(0.09)	(0.15)	(0.07)	(0.13)	(0.05)	(0.09)	(0.04)	(0.07)
Percent white	0.04	−0.09*	0.05**	−0.11***	0.02	−0.03	0.00	−0.11***
	(0.03)	(0.04)	(0.02)	(0.04)	(0.01)	(0.02)	(0.01)	(0.02)
Population density	−1.30***	−1.20***	−0.66**	−0.90***	−1.05***	−0.74***	−0.69***	−0.82***
	(0.10)	(0.21)	(0.08)	(0.15)	(0.05)	(0.16)	(0.08)	(0.12)
Percent 18–34	0.13*	−0.16	0.00	0.02	0.03	0.09	0.02	0.06
	(0.06)	(0.09)	(0.05)	(0.07)	(0.03)	(0.05)	(0.03)	(0.04)
Percent male	−0.09	0.29	0.08	0.36*	0.04	−0.18	−0.03	−0.32***
	(0.13)	(0.18)	(0.09)	(0.16)	(0.08)	(0.12)	(0.06)	(0.10)
311 calls to the police		−0.02	0.02	−0.00		0.10***	0.02*	−0.03*
		(0.03)	(0.01)	(0.02)		(0.02)	(0.01)	(0.02)
Property value		0.01	0.03*	0.06*		0.00	0.00	−0.01
		(0.03)	(0.01)	(0.02)		(0.01)	(0.01)	(0.01)
Properties		−0.20	−0.02	−0.10		0.11	0.12	−0.03
		(0.14)	(0.04)	(0.08)		(0.12)	(0.06)	(0.09)
Year	0.17***	−0.02*	−0.06***	−0.04***	0.15***	−0.02***	−0.05***	−0.03***
	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.01)	(0.00)	(0.00)
Constant	6.60***	8.24***	3.53***	3.46***	5.33***	5.04***	3.41***	5.76***
	(0.61)	(1.42)	(0.55)	(1.00)	(0.36)	(1.10)	(0.56)	(0.82)
N (tract-years)	3,498	1,999	3,498	3,498	10,754	6,138	10,754	10,754

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

Model 1 indicates 311 calls to the police increase as a neighborhood's middle-class population increases, net of crime and the other controls. The average eligible-to-gentrify tract saw its middle-class population increase by 18.8 percent during the study period (see Table 1). Model 1, then, suggests the average eligible-to-gentrify tract would experience an associated 4.1 percent increase (0.22×18.8) in 311 calls to the police. More white people moving into a tract was not related to more 311 calls to the police, contrary to my hypothesis. So, either the class variable is capturing all the variation of the percent white variable because the two co-occur, or both white and non-white gentrifiers make more 311 calls to the police. The relationship between middle-class in-movers and 311 calls was not unique to gentrifying neighborhoods. Model 5, which repeated Model 1 on a set of wealthy tracts ineligible for gentrification, found the same result. I made 311 calls an independent variable in Models 2–4, and such calls were not associated with changes in low-level policing, contra expectations that resident demand would spur low-level police activity.

Models 2–4 regress the three low-level policing variables on the explanatory and control variables. The violent crime rate had positive coefficients for all three low-level policing measures, though only in the model of order maintenance arrests was the coefficient statistically significant. Alternate models, discussed below, add property crime and robbery as controls. Crime was a strong correlate of order-maintenance arrests, as differential offending theory predicted, but it was not related to stops or proactive arrests. It is important to note that differential offending theory expected crime would be the *only* influence on policing, and as the other statistically significant coefficients in the table make clear, that is not the case.

The associations between racial gentrification and low-level police actions were mixed. When more white people moved into the typical gentrifiable neighborhood, police made fewer street stops and fewer proactive arrests, but more order maintenance arrests. While the typical eligible tract saw its white population decline during the study period, as Table 1 shows, some tracts saw their white populations increase by as much as 100 percent. These models suggest that during such stark racial gentrification, police made 9 percent *fewer* street stops (-0.09×100), 11% *fewer* proactive arrests (-0.11×100), and 5% *more* order maintenance arrests (0.05×100). The different coefficient directions for percent white in the order maintenance and proactive arrest models were not anticipated by the theory. In alternate models, available upon request, I disaggregated stops of white people from stops of black and Latino people. When an eligible tract's white population increased, stops of white people decreased and stops of black and Latino people remained the same. The arrest variables unfortunately cannot be disaggregated by race, but the decrease in proactive arrests associated with more white people is likely also driven by a decrease in white arrests, as many proactive arrests stem from stops.

The measure of gentrification's supply-side dynamics, property value, had a clear, consistent, and positive relationship with low-level policing. An increase in property value in neighborhoods eligible for gentrification was statistically significantly related to more order maintenance arrests and proactive arrests. The average tract in the sample saw its property value increase by 5.1 percent between 2009 and 2015 (Table 1). During such reinvestment, the typical tract experienced an associated 0.2 percent increase in order maintenance arrests (0.03×5.1) and a 0.3 percent increase in proactive arrests (0.06×5.1). Neighborhoods in the top 10 percent of property value appreciation saw 71 percent growth or more, as shown in the top decile of Figure 6. Such rapid real estate market gentrification would, on average, co-occur with an increase in order maintenance arrests of 2.1 percent (0.03×71) and an increase in proactive arrests of 4.3 percent (0.06×71). Property value increases were also related to more stops, though not at a statistically significant level. Notably, the property value–arrest relationship was not demonstrated in neighborhoods ineligible for gentrification. In Models 6 through 8, the coefficient for property value is nonsignificant, underscoring that the real estate investment-policing dynamic is one unique to gentrifying neighborhoods.

The year variable was negatively related to the low-level policing outcomes because the three outcomes were, in aggregate, declining city-wide over the study period. Year was positively related to 311 calls to the police because they were increasing city-wide.

As robustness checks, I ran alternate models accounting for spatial correlation and alternate models including different measures of crime. Table 3 presents the results of these models. To address concern that tracts' gentrification might spill-over, affecting nearby tracts, I created a distance-based weights matrix and multiplied the gentrification variables (class index, percent white, and property value) by the matrix to create spatially

TABLE 3. Regression Coefficients, Tracts Eligible for Gentrification

	Spatial Lags Included				Alternate Crime Variables Included			
	Model 9 311 Calls to the Police	Model 10 Street Stops	Model 11 Order Maint. Arrests	Model 12 Proactive Arrests	Model 13 311 Calls to the Police	Model 14 Street Stops	Model 15 Order Maint. Arrests	Model 16 Proactive Arrests
Violent crime rate	0.01	0.08	0.07***	0.03	−0.02	0.10	0.08***	0.07
Class index	0.20**	0.15	0.08	−0.07	0.21*	0.17	0.09	−0.06
Percent white	0.03	−0.07*	0.04	−0.07**	0.04	−0.09*	0.04*	−0.11**
Population density	−1.22***	−1.23***	−0.68***	−0.78***	−1.26***	−1.16***	−0.55***	−0.86***
Percent 18 to 34	0.01	−0.17*	−0.01	0.01	0.12*	−0.16	−0.00	0.02
Percent male	−0.06	0.22	0.07	0.44***	−0.08	0.29	0.10	0.37*
311 calls to police		−0.03	0.03*	−0.01		−0.02	0.02	0.00
Property value		0.01	0.03*	0.06***		0.01	0.03*	0.06*
Properties		−0.21***	−0.03	−0.06		−0.20	−0.02	−0.10
Spatially lagged class index	0.58*	0.51	0.09	−0.23				
Spatially lagged percent white	0.03	−0.08	0.01	−0.36***				
Spatially lagged property value	−0.02	−0.06	0.02	−0.05				
Spatial error term	0.48***	0.76***	0.44***	0.60***				
Property crime rate					0.03	0.05	0.12***	0.06
Robbery rate					0.05	−0.03	−0.02	−0.07*
Year	0.16***	−0.03	−0.06***	−0.04***	0.17***	−0.02*	−0.06***	−0.04***
Constant	0.30***	0.27***	0.22***	0.33***	6.40***	7.97***	2.82***	3.14**
N (tract-years)	3,563	1,999	3,563	3,563	3,561	1,999	3,561	3,561

*$p < 0.05$; **$p < 0.01$; ***$p < 0.001$.

lagged variables. Most of the spatially weighted variables were not significant in most of the models, suggesting there is either not much spillover of the gentrification variables or that the spillover is time-invariant and thus controlled away by the fixed effects. There appears to be some positive, indirect cross-tract influence of the class index in the models of 311 calls (Model 9) and negative, indirect cross-tract influence of percent white in the proactive arrest model (Model 13), but neither changes the primary coefficients. The spatial error term, on the other hand, is statistically significant in every model, demonstrating positive spillover associations via the error term. Accounting for both types of spatial correlation does not change the coefficients for the primary variables, except that the coefficient for percent white in the model of order maintenance arrests drops out of statistical significance. Models 13–16 include the property crime and robbery rates as a sensitivity check to the choice of the violent crime variable. The results of these models are substantively identical to the main models. The models in Table 3 demonstrate that the main findings are robust to controls for spatial correlation and alternative measures of crime.

DISCUSSION AND CONCLUSION

This project set out to answer whether low-level policing intensifies during gentrification. I distinguished between two aspects of gentrification: the influx of new residents and the tightening of real estate markets, hypothesizing both would be associated with more low-level policing. I expected police would increase discretionary stops and low-level arrests in response to increases in demands from new white and middle-class residents to a neighborhood, and I expected real estate interest groups would work with city officials to direct more policing to neighborhoods marked for redevelopment. I collected longitudinal, neighborhood-level data to model the relationship between gentrification and low-level policing in linear regression models with place and year fixed effects.

I drew on the theories of Logan and Molotch (1987) and Neil Smith (1996) to demonstrate how real estate developers and city elites mark neighborhoods for reinvestment during gentrification to create growth and expand the city's tax base. This supply-side account of gentrification lead me to expect government officials and police performance metrics would motivate police to try and "clean up" neighborhoods undergoing upscaling. This study provides evidence New York City practiced such development-directed, low-level policing between 2009 and 2015 in gentrifying neighborhoods. In the average gentrifiable neighborhood, police increased low-level arrests following reinvestment. Neighborhoods saw between 0.2 percent and 0.3 percent more discretionary arrests with every 5% increase in their property values. Neighborhoods that were too wealthy or recently developed to be eligible for gentrification did not experience such a trend, underscoring development-directed policing's concentration in neighborhoods undergoing "urban renewal." When New York's Times Square and Lower East Side neighborhoods were redeveloped in the 1980s and 1990s, there was considerable press and scholarly attention to the role police played in making arrests to clear those neighborhoods of their previous hustlers, drug dealers, and homeless denizens. This study's findings suggest policing for gentrification was still occurring throughout New York City's poorer, less-developed neighborhoods in the early 2010s.

Demand-side accounts expected the demographic change during gentrification to be related to more 311 calls to the police and more low-level policing. In support of the class-focused version of such theories, I found the typical neighborhood saw an increase in 311 calls to the police during an influx of middle-class people. That increased demand did not translate into more arrests, however, as the number of 311 calls from a neighborhood was not statistically significantly related to stops or low-level arrest rates there. Further, an increase in middle-class residents was not at all related to the three policing outcomes, undermining class-based accounts of low-level policing during gentrification and confounding my hypothesis.

Gentrification's racial dynamics have a complicated and sometimes contradictory relationship to low-level policing, this study found. My hypotheses and considerable qualitative evidence expected new white residents would motivate intensified policing. Contradicting this, these models indicate neighborhood whitening was unrelated to 311 calls to the police and related to *fewer* street stops and proactive arrests. This aggregate decrease was likely driven by fewer stops and arrests of white people and stability in stops and arrests of black and Latino people. Supporting my hypothesis and previous research was the finding that neighborhood whitening was associated with more order maintenance arrests.

Differential offending theory expected police arrest rates would react only to crime rates. Consistent with the theory, an increase in crime was accompanied by an intensification in order maintenance arrests. But, police did change alongside changes in a neighborhood's racial makeup and increases in real estate prices net of any changes in crime rates. While low-level police actions seem to be partly motivated by crime rates, public safety is clearly not the sole correlate of police stops and low-level arrests.

Police responded differently to gentrification's demographic changes and its real estate market changes. This has implications for how we should measure gentrification and at what scale we should analyze it. The divergence in findings for gentrification's three components (race, class, and property value) suggests studies of gentrification's impacts need to disaggregate its subsidiary elements. That each policing outcome varied as neighborhoods got whiter suggests police are responsive to street-level changes. The race of residents is easy to observe, and it is possible beat officers changing their behavior in response to observing more white people. In addition to these street-level dynamics, my findings suggest police respond with more low-level arrests to meso-level demands from interest groups like real estate developers. It is likely police department managers, not rank and file cops, are initiating such development-directed policing. The mechanism here is hard to observe. Researchers are unlikely to be allowed into meetings where city officials and police captains are discussing enforcement priorities. Also, instructions might not be as overt as requests from city elites that police intensify enforcement in neighborhoods marked for upscaling. It could be that police managers take their own initiative absent any explicit demand once they understand the city's development plans. Future gentrification research and policing research would do well to attend to the various scales at which police and place interact and disaggregate measures of gentrification.

This study is limited in several respects. Limited data availability confined the time window to seven years, a short horizon in the life cycle of a neighborhood. Gentrification can happen over a long stretch, and areas marked for redevelopment might experience years of demographic change before reinvestment arrives. Future research might more explicitly engage with various time frames and sequences of gentrification to better pattern the relationship.

A second limitation of this project is its case selection. New York City has a unique economy, it is much larger than other U.S. cities, and it has a unique cultural status that might attract particularly persistent gentrification. Especially relevant to this project, New York City had much higher rates of order maintenance and proactive arrests compared to the national average during the study years. Qualitative research found the policing-gentrification relationship to be strong in cities of many types, so research might yet reveal similar findings in mid-sized cities. But, this study is likely more applicable to other global cities like London, Tokyo, or San Francisco, and the findings might have limited generalizability to smaller U.S. cities less integrated in global markets.

A third limitation might be the choice of real estate property value metric. As I wrote in the "Comparing Measures of Property Value" section, I think the property tax assessment data is likely the most accurate at the tract level. But, these findings might be sensitive to the type of real estate data used and the geographic level at which it is aggregated. Studies of higher level units might incorporate sales and Census data to test this sensitivity.

Stops and arrests for low-level offenses can have lasting consequences not just for those detained, but also for their families and communities (Fagan et al. 2010; Geller et al. 2014; Howell 2009; Jashnani et al. 2017; National Academies of Sciences 2017). Similarly, gentrification affects not just those squeezed by higher rents, but also those left behind in changing neighborhoods. Any tendency to dismiss either low-level policing or gentrification as epiphenomenal must first contend with these ripple effects. The present study merges insights from urban sociology and the sociology of police and suggests low-level policing intensified following real estate reinvestment in New York City's eligible-to-gentrify neighborhoods between 2009 and 2015. The relationship was not simple, however. Gentrification's different facets—here class, race, and real estate markets—each related to policing differently. Such relationships should be considered when assessing the impacts of both gentrification and policing for city life.

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