Fear in The Neighborhood: The Cost of Racial Prejudice

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Abstract

Residential segregation in the United States is persistent, and even though policymakers tout the benefits of integration, social and economic headwinds limit its progress. These challenges to integration are often a result of fear of interacting with different racial groups. Using a nearest neighbor research design, we show that receipt of a new Black next-door neighbor, relative to receipt of a new White next-door neighbor, leads to an increase in 911 calls auto-dialed by home alarm systems. This effect exists irrespective of the race of the current resident. However, we find no change in either crime related calls or reported crimes after the arrival of new Black neighbors. Taken together, these results suggest the presence of perceived but no actual threat to personal safety from new Black neighbors. More broadly, polices that promote integration may benefit from initially investing in actions that increase social interactions between different racial groups to counteract perceived threats to personal safety driven by racial stereotypes.

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1 Introduction

The US has high levels of residential segregation (Lichter et al., 2016) and Dunsmuir (2013) finds that many Americans do not have any friends from other racial groups. Beyond the implications of segregation on the quality of neighborhoods and schools (Bayer and McMillan, 2005; Echenique et al., 2006), limited interactions with different races is important for racial disparities across a range of outcomes. For example, Billings et al. (2014) , Johnson (2011) and Reber (2011) find that racial integration in schools under Brown vs. Board (1954) and its court ordered end in the 1990s helped narrow the black white test score gap as well as improved criminal outcomes for black youths. The impacts of racial integration extends to political beliefs with exposure to diversity leading to more progressive political beliefs among whites (Billings et al., 2022; Chyn et al., 2022). There is also evidence that exposure to diversity has impacts on generosity, prejudice and views of egalitarianism (Achard et al., 2022; Bursztyn et al., 2021; Rao, 2019). Finally, Ba et al. (2021), Rivera (2022) and Domènech-Arumí (2023) provide evidence that exposure to diversity at work and at home decreases racial disparities in police stops and use of force.

Given all this existing evidence that exposure to diversity can help address systematic racial inequalities, policies to foster integration have largely been undercut by the government and the courts. The end of school desegregation plans in the 1990s, the end of affirmative action in 2022 and upholding exclusionary residential zoning practices has limited the ability of government policies to directly address racial inequalities. Additionally, policies to promote integration have met resistance by the public. Beyond the historic standoff by Governor George Wallace during the integration of the University of Alabama in 1963, there are countless episodes in US history of local government legislatively weakening and public protests to the integration of schools, restaurants, neighborhoods and public transit (Delmont, 2016; Goetz, 2018; Meyer, 2000; O'Rourke and Pace, 2020; Ward, 2011). In the more recent context, there has been a growth in white nationalist sentiment (Fahy and Vilchis, 2023; Katz, 2017; Swain, 2002) in response to protests around racial disparities in policing as well as broader events surrounding immigrants entering the US. Racial rhetoric and fear propagated by politicians even contributes to current racial disparities in policing (Grosjean et al., 2023).

Extant work seeking to better understand the existing hindrances to increasing diversity has generated causal evidence of the effects of racial integration on personal and social conflict by focus-

ing on policies that assigned refugees to certain cities (Alan et al., 2021; Bazzi et al., 2019), large scale migrations (Ajzenman et al., 2023; Derenoncourt, 2022) or the desegregation (and subsequent end of it) of public schools (Billings et al., 2014; Johnson, 2011; Reber, 2011). There are also a number of descriptive papers highlighting that racial stereotypes increase the perception of crime (Hurwitz and Peffley, 1997; Quillian and Pager, 2001) and that segregation increases crime (Breetzke, 2018; Chiricos et al., 1997; Xie and Lauritsen, 2012) as wells as widens gaps in black-white homicide rates (Light and Thomas, 2019). These papers provide evidence of personal conflict and perceived social threats from large shifts in racial composition which may coincide with broader social changes in institutions and economic opportunities.

In this paper, we provide new evidence of how being exposed to individuals of different races impacts actual and perceived threats to personal safety. We examine a context that is a result of idiosyncratic changes to a neighbor's race and not larger changes in racial composition. Specifically, we examine how an existing black or white resident reacts to a new next-door neighbor who is a different race. We focus our attention on 911 calls tied to current residences' addresses.[1] Each 911 call has an associated description of or reason for the call, which allows us to disentangle calls auto-dialed by home alarm systems versus those made by people reporting violent crimes, property crimes, or disturbances. The latter three outcomes measure actual crime while the first indicates perceived threats – sudden spikes in home alarm system 911 calls with no corresponding changes in either 911 calls reporting criminal activity or documented crimes is most consistent with the likely adoption of an alarm system. Indeed, auto-dialed 911 calls from home alarm systems are one of the most common 911 calls and are, in many cases, accidentally triggered by residents learning a new system. Our study area is focused on two cities in North Carolina - Durham and Charlotte - given the need for precise geographical information on 911 calls and sizeable racial diversity.

We adopt two different nearest neighbor designs to highlight the impacts of an individual receiving a new next door neighbor who is of a different race. To control for neighborhood attributes and dynamics, we compare current residents of the same race who live in the same census block group and both receive new neighbors in the same year and quarter. These current residents differ in that one received a new next-door neighbor of the same race and the other of a different race.

[1] Since 911 call data is used for police dispatch purposes, each 911 call is precisely geolocated. Reported crimes, on the other hand, are often noisy due to the locations chosen by the officer taking the report.

The identifying assumption is that new neighbors can sort by property attributes or neighborhood characteristics but that the precise homes new neighbors end up buying within a neighborhood is as good as random. Second, we compare current residents of the same race as the treated current resident but who live slightly farther away. This model leverages the economic importance of next-door neighbors and controls for micro-neighborhood racial trends in new residents. In contrast to previous papers that have used these research designs (Bayer et al., 2022; McCartney et al., 2024) to investigate the move-out decisions of current residents, which are one-time events, the outcomes we investigate in this paper, 911 calls and reported crimes, allow us to conduct event study analyses to confirm the existence of parallel pre-trends.

In order to implement our nearest neighbor research design, we incorporate property level data on housing transactions, 911 calls and voter registration data in order to detail the timing of residential moves and perceived or actual threats to personal safety. This data allows us to determine the race of new and existing residents based on the purchase or sale of a home. Voter registration data provides our measure of race since racial information is provided in North Carolina voting records.[2] We can use the exact timing and location of 911 calls to link them to addresses and then relate this to the timing of new neighbors of different races. Since our measure of residential moves is constructed off property records, our analysis focused on a population that purchases or rents single-family homes. This population is likely important for policy as these households are more likely to be politically active in supporting or protesting policies that impact patterns of racial segregation.

We find compelling evidence that receiving a new black neighbor, relative to a new white neighbor, leads to a 20% to 25% increase in 911 calls from home alarm systems for black and white current residents, respectively. This pattern does not exist for 911 calls related to violent crimes, non-violent crimes or disturbances. The limited effects on 911 calls related to criminal activity is also confirmed by another data set that details reported crimes. Effects for black new neighbors on existing black residents may be surprising, but is consistent with racial stereotypes that perceive greater threat to personal safety from black residents. This belief about new black neighbors may generate fear more broadly than actual racial differences.

We next document heterogeneous treatment effects. Among black current residents, alarm calls

[2]As discussed in Ihlanfeldt et al. (2024), the use of voter registration records (when available) provides a larger sample of households with racial information and better represents Census measures of race than HMDA based racial information.

increase the most following the arrival of new black next-door neighbors in already high crime neighborhoods. In contrast, the treatment effect does not seem to be influenced by the neighborhood income level. Interestingly, among white current residents, these results are flipped. Within this population, the arrival of new black next-door neighbors generates the largest increase in 911 alarm calls in low income neighborhoods, but does not differ in previously low versus high crime areas. These results are remarkably consistent with the income heterogeneity results documented in Bayer et al. (2022) and suggest that racial prejudices may be highest among white households in low income neighborhoods. Consistently, we show that in no types of neighborhoods does the likelihood of a crime related call change upon arrival of a new black neighbor.

Our results contribute and are consistent with a cross-disciplinary literature that highlights fear of and actual victimization from other racial groups. For example, Chiricos et al. (1997) surveys adults during a heightened period of crime and finds that perceived racial composition impacts fear among whites, but not black urban residents. Other surveys (Hipp, 2010; Quillian and Pager, 2010) find support for racial stereotypes with respect to crime as well as perceptions of victimization with whites perceiving higher crime. Quillian and Pager (2001) and Drakulich (2012) find evidence of racial stereotypes linking black residents to higher levels of crime among both white and black respondents with larger effects for the former. Rader (2017) highlights a body of evidence that shows perception of crime risk is explained by a number of individual and contextual factors that lead individuals to overstate actual crime risk. Consistent with a mechanism of racial integration, Louie and DeAngelis (2024) finds that increases in black residents leads whites to perceive more crime and danger in the neighborhood. Finally, our main conclusion is consistent with the findings of (Ajzenman et al., 2023) who uses a different research design (a shift share instrument) and survey data to show that the large influx of immigrants in Chili had no impacts on crime victimization but did lead to increases in home and personal security.

The policy implications of our results are important as it appears that perceived threats from certain racial groups is a dimension of any policy that promotes racial integration. Most importantly, these results show that actual safety does not appear to be compromised base on the race of a new neighbor at an observational unit that is quite salient and commonplace. Therefore, policies that promote integration are mostly being opposed based on perception and not actual risk to individual safety. Since the most common prescription to overcome perceived fears in therapeutic settings is

repeated exposure to the source of fear, policies that promote integration may benefit from actions that lead to repeated interaction with specific races even in anticipation of larger policy change. In the context of neighborhoods, one could imagine benefits from community and neighborhood events that increase social interaction prior to the implementation of an integrative school or housing policy.

2 Data Description and Research Design

Identifying the effects of receiving different-race neighbors requires overcoming a severe identification challenge: Households do not randomly choose where to move. Consequently, the characteristics that attract new neighbors to move in might be the same neighborhood features that affect local criminal activity and 911 calls. To overcome this problem, we build a novel, individual-level dataset and use a new next-door neighbor research design to identify whether households' choices are directly linked to the racial identities of their neighbors (Bayer et al., 2022; McCartney et al., 2024).

2.1 Data Sources

Housing Data. We acquire assessor files and county deeds records for the state of North Carolina from CoreLogic Real Estate Solutions. The assessor data describes residential parcels and includes owner name, home size, home age, complete site address, and precise latitude and longitude. The deeds data is a companion data set that describes all real estate transactions. We observe every time a property changes ownership and know the buyers' names, sellers' names, and date of the transaction.

North Carolina Voter Records. Publicly available voter registration records come from the North Carolina State Board of Elections (NCSBE).[3] These data are available as yearly snapshots starting in 2005. Each year, we know the full name, birth date, residential address, and self-reported race for the universe of registered voters. This dataset provides our measure of race and captures a larger share of new and existing residents than alternative datasets. For example, researchers can match Home Mortgage Disclosure Act (HMDA) data to property transactions in order to get a homebuyer measure of race. We later confirm that using either dataset generates comparable results.

911 Calls. We acquire data on 911 calls through a combination of FOIA requests and publicly

[3] Source: https://dl.ncsbe.gov/index.html.

accessed data. These date describe all 911 calls and the nature or description of the call. We classify calls into one of four types as detailed in Online Appendix A. We do not know who called 911, or where they were calling from. Instead, we observe the precise latitude and longitude that the operator assigns to the call. This geolocation could direct officers to, for example, a home being broken into, a car that has been involved in an accident, or the current location of a violent altercation. Our data includes all calls made in Mecklenburg County, North Carolina (home of Charlotte, NC), between 2005 and 2010 and all calls made in Durham County, North Carolina, between 2006 and 2019. The difference in study periods across out two cities is due to the fact that for more recent years, the city of Charlotte introduced noise into their geographical information for 911 calls which makes it impossible to implement our research design since it requires precise geographical information.

Criminal Activity. From the Charlote-Mecklenburg and Durham Police Departments, we observe all criminal incidents that occurred in the county between 1998 and 2018. This dataset describes the nature and location of the criminal activity. But, since many of these records are compiled after the fact and since there is not a pressing need for a precise geolocation, the location data in this dataset is often approximate.

2.2 Sample Creation

The first step in creating our sample dataset is to use the cross-section of real estate transactions from the deeds data and the cross-section of ownership records from the assessor data to create a quarterly panel of parcels and their owners over time. This parcel-by-quarter panel dataset initially includes the precise geolocation of every residential property along with the full name of each its owners.

Our second step uses the algorithm described in Bayer et al. (2022) to identify each parcel's nearest neighbors. We use address numbering conventions and street names along with a number of filters to identify the neighbors immediately next-door to each parcel. Each parcel has a max of two next-door neighbors, one on each side; some parcels have one or zero next-door neighbors. When used together with our panel dataset of parcel ownership, we can identify which current residents received new next-door neighbors and when those new neighbors arrived.

Third, we use the current homeowner's full name and address to merge in demographic information – birth year, race, and sex – from the voter records. In the event that a house has multiple home-

owners, we say that the household is Black if either or both of the owners of the property self-reports their race as Black. There are two potential limitations of using voter registration information to assign race to homeowners. We we do not observe the self-reported race of homeowners who are not registered to vote nor the race of voters who choose note to report their race. In practice, though, the vast majority of homeowners are registered to vote (Hall and Yoder, 2022; McCabe, 2016; Smets and Van Ham, 2013) and, among those who are registered to vote, more than 95% of voters self-report as either White or Black. There are two key advantages of using the voter data. The first is that since both the registry of deeds and the state voter rolls serve official, legal purposes, the quality of the data is phenomenal and the match rate consequently very high. The second is that, since voters are registered to vote just at their primary residence, the merge mechanically drops non-occupant landlords and non-person owners from the data. The output of steps one through three is a quarterly panel dataset that details owner-occupants and their race along with the races of their neighbors and if those neighbors are recent arrivers to the neighborhood.[4]

In our final step, we merge in the 911 calls data. Specifically, we create an indicator variable that we set equal to 1 if there was a 911 call geolocated to within 25 meters of a parcel. We create further subcategory variables that indicate if there was a 911 call within 25 meters of a parcel with a specific nature or description: home alarm system, violent crime, property crime, disturbance. We follow a similar procedure to merge in criminal activity from the incidence data. Lastly, using name and address we merge concealed handgun permit (CHP) application data into our quarterly panel of residential parcels and their owner occupants.

Our final 911 calls dataset covers Mecklenburg County between 2005q1 and 2010q4 and in Durham County between 2006q1 and 2019q4 and details all residential parcels, the names and races of their owner occupants, the names and races of their next-door neighbors and whether these neighbors have recently changed. We know at a quarterly frequency whether there were 911 calls of varying types geolocated to each parcel. The basic analysis in this paper relates the number and type of households' 911 calls at a parcel to the characteristics of those households' nearest neighbors. Before describing the final sample, we explain the research design.

[4]This paper is not the first to merge real estate data from CoreLogic and voter data from the NCSBE and we direct interested readers to Bayer et al. (2022), McCartney (2021) and McCartney et al. (2024) for more details on both the process and the quality of the merged data set.

2.3 Research Design

Our specific question of interest is how current residents respond to receiving new neighbors whose race is different than their own. To this end, we first consider the sample of current residents who receive new different-race neighbors. We then compare the likelihood of 911 calls being directed to the area around the resident's house in the post-period, after the arrival of the new next-door neighbor, to the likelihood in the pre-period. This simple event study suffers from two shortcomings. The first is that it might be the change in next-door neighbor that matters, not necessarily the race of that new neighbor. This motivates a second difference. We further consider current residents who receive new same-race neighbors and then compare not just pre-period and post-period, but also whether the new neighbor is same-race or different-race. The second potential concern is that some unobserved neighborhood-level changes explains both the arrival of the new neighbors and any change in the rate of 911 calls. To absorb these neighborhood-level changes, we include group-by-quarter fixed effects that, in essence, ensure that both the current residents who get new different-race neighbors and the current residents who get new same-race neighbors are being affected by otherwise changing neighborhoods in the same way. We separately estimate the following equation for the sample of Black residents who received new next-door neighbors and the sample of White residents who received new next-door neighbors:

$$I(\text{911 Call})_{i,t} = \beta \times \text{Treated}_i + \delta \times \text{Post}_t + \gamma \times \text{Treated}_i \times \text{Post}_t + \text{Controls}_i \times \Theta + \eta_{group \times new\ nbr\ quarter} + \psi_{block} + \epsilon_{i,t}, \quad (1)$$

where current resident households are indexed by i and the nine time periods around the arrival of the new neighbor are indexed by t. $I(\text{911 Call})_{i,t}$ is a dummy variable equal to 1 if there is at least one 911 call geolocated to household i in period t. Treated_i is a time-invariant dummy variable at the household level that equals 1 if the current resident received a new next-door neighbor of a different race and 0 if the new resident received a new next-door neighbor of the same race. Post_t is a dummy variable equal to 1 in the quarter the new neighbor arrives and the following four quarters and a 0 in the four quarters prior to the new neighbors arrival. Control variables include home age, home size, current resident's age, and current resident's birth state. We further include census block group

by new neighbor arrival quarter fixed effects. This fixed effect defines the quasi-experiment and means that variation in our treatment-by-post status is a function of only whether the new neighbor received a new different-race or same-race neighbor. The inclusion of this fixed effect also renders our main estimation strategy an, in essence, stacked difference-in-differences.

Our coefficient of interest, γ, estimates the effect of being treated, i.e., receiving a new next-door neighbor of a different race, relative to other same-race current residents on the same block group who also received new next-door neighbors, but whose new neighbors were of the same race. We note that, under our assumption of random assignment to which of the block group's current resident receives the different-race new neighbor and which receives the same-race new neighbor, our inclusion of resident and property controls and block fixed effects is not necessary for identification. But they do help with statistical precision and so we include them in most specifications.

The richness of the data allows us to estimate a dynamic diff-in-diff with multiple leads and lags, instead of the simplified pre vs post analysis described by Equation 1. Specifically, instead of pooling the four pre-periods and five post-periods, we estimate a separate time dummy for each period for both treated and control current residents. We examine both the relative and absolute differences between treated and control current residents in the quarters around the new neighbors' arrivals, by creating eighteen dummies – nine event times by treatment versus control. We can then estimate the number of calls relative to the baseline likelihood of a call being made by control residents in the quarter before the new neighbors arrive.

2.4 Estimation Sample

To conduct our analysis, we focus our attention on the sample of current residents who received new next-door neighbors. Our final sample includes only households that self-report as White or Black. Further, we only include current residents involved in an "experiment." Experiment are conducted at the block group-by-quarter-by-current resident race level and occur when at least one current resident receives a same-race next-door neighbor and another resident receives a different-race next-door neighbor. We describe this main sample in Table 1.

[TABLE 1 HERE]

This summary table highlights the attributes of current residents split by race. We find that both

current Black and White residents live in typical suburban homes - average year built in the 1990s, 2,000+ square feet. The average resident is in their late 40s. We also observe that average Black residents in our sample have higher 911 call volume and live in neighborhoods with lower median income. Given existing patterns of residential segregation, Black residents live in neighborhoods with Black share equal to 48% and White residents live in neighborhoods with Black share equal to 23%. The need for some level of racial mixing means that our sample does not include the most segregated blocks. Neighborhoods that Black or White households never move to are, mechanically, never in our sample.

Finally, looking at the counts along the bottom rows of the table, we see that there are 3,650 treated Black residents – Black residents who received a new next-door White neighbors. Our research design compares these residents to 4,637 control Black residents who live on the same census block group and in the same calendar quarter received new next-door Black neighbors. Current residents can be involved in multiple experiments and, in those cases, our final sample includes duplicates at the resident-by-calendar quarter level (but never the resident-by-calendar quarter-by-experiment level). Since each experiment is nine quarters long, the number of household-quarter observations in our final sample of Black residents is 76,288.

3 Main Results

3.1 The Effect of New Neighbors on 911 Calls

Our nearest neighbor design allows us to identify the relative impacts of racial differences in a new next door neighbor. In our context, we directly estimate Equation 1 to generate Table 2.

[TABLE 2 HERE]

In Table 2, we show that for black current residents, there is a 1.017 percentage point decline (equivalent to a 20% decline) in alarm calls in the year following the arrival a new white next-door neighbor moves in, relative to otherwise similar current residents who receive new black next-door neighbors. Effects sizes are below 10% and quite noisy for the remaining crime calls. When focusing on white current residents, we see a 0.603 or a 25% increase in calls for service related to alarms. For white residents, we again see limited and noisy impacts on crime-related calls.

In Figure 1, we estimate dynamics around the arrival of the new neighbors. This Figure highlights the lack of meaningful pre-trends in our analysis. That is, prior to a new neighbor moving in, we see no relative differences in calls for service of any type between residents who eventually receive a same-race versus difference-race neighbor. This holds for both both black and white current residents. Focusing on the time periods after the new neighbors arrive, we see increases in all calls as well as calls for alarms, which are largely driving the all calls results. Looking at black current residents, we see increases in alarms for getting a same race relative a different race new neighbor. These results reverse when we focus on white current residents. Taken together, we see an increase in alarm calls for service when a black new neighbor moves in next door. This result grows quickly after the move-in date and then tends to flatten out as we look up to a year later.

[FIGURE 1 HERE]

When we turn to other calls for service that are related to any criminal activity. Specifically, we highlight in Figure 1 that calls for service related to violent crime and property crime also appear to exhibit similar trends prior to the move-in date. However, in contrast to alarm call, the post period exhibits no evidence of differences between those who receive new same- versus different-race neighbors. The lack of effects for different crime related calls highlights that overall crime risk is unlikely to have been changed based on the new neighbor race. Since racial tensions or stereotypes may engender fear from a new black neighbor, this would likely, if anything, cause greater usage of 911 calls about issues related to public safety. Therefore, our crime calls may in essence be muted toward zero for black new neighbors. Individuals will likely always call the police for more serious incidents, but for calls with more discretion, there may simply be more calls in the presence of racial tension. Our calls for disturbances should highlight if more discretionary calls are heightened based on racial differences. As shown in Table 2, we find limited effects on racial differences in new next door neighbors for disturbance calls.

We next investigate the possibility that the increased presence of alarm systems could lead to a deterrence effect from would-be burglars. Our findings to this point suggest this is unlikely since we still might have expected to find effects on disturbance calls if this were the case. But, since an actual burglary could trigger an alarm call, we do want to confirm that we do not see an increase in burglaries around the race of our new next door neighbors. We turn to a different data set that

details crimes that generate reports, as is the case for burglaries and break-ins. Using the same research design, but now with an indicator for reported criminal activity as the outcome variable, Table 3 finds no increase in burglaries or theft and if anything a decrease due to the deterrence of alarm systems when a new black neighbor moves in next door to a current white resident.[5] We plot estimates from the event study in Appendix Figure B1.

[TABLE 3 HERE]

These results do highlight a narrative of alarms and not actual crime. The increased prevalence of alarm calls is consistent with two features of our design: the salience of a next door neighbor as well as the frequency of alarm calls especially for new systems where individuals are more likely to accidentally trigger the system and thus could lead to a 911 call. These results support a perceived increase in crime risk but no clear evidence of increased risk of victimization.

4 Robustness and Heterogeneity

4.1 Alternative Research Design

A key threat to the validity of our main research design is that new neighbors are systematically sorting to very localized neighborhoods. We address this concern directly by allowing our research design to further compare current residents of the same race who received the same new neighbors but who lived slightly farther away. Specifically, we compare residents who live right next-door to the new neighbor to those who live two- or three-doors down. This alternative identification strategy solves the problem of within-neighborhood sorting by comparing new neighbors within a few hundred feet of each other, a level at which it is impossible for new neighbors to sort given the thinness of the housing market. Estimates from these models are provided in Table 4. Plotted estimates from the event study version of this design can be found in Figure B2.

[TABLE 4 HERE]

This analysis yields two important takeaways. First, the largest change in alarm calls is for residents who receive a different-race neighbor immediately next-door, especially for white current

[5] The less precise geospatial data for reported crimes makes these results less precise in matching to the home of current residents.

residents. Second, the effect size decays rapidly, consistent with perceived threats to safety being driven by the neighbors nearest to current residents. Overall, the results of this analysis highlight the robustness of our main results.

4.2 Heterogeneity

We provide some analysis of heterogeneity in our pattern of main results in Figure 2. We focus on three aspects of heterogeneity, comparing neighborhoods with above median vs below median income, black share, and 911 call volume.

[FIGURE 2 HERE]

For black current residents, we find that alarm calls increase the most following the arrival of new black next-door neighbors in already high crime neighborhoods. In contrast, the treatment effect does not seem to be influenced by the neighborhood income level. We acknowledge that the results from the heterogeneity with respect to black share tests are hard to interpret. The demands of the research design mean that there are mechanically very few experiments on black current residents in predominately white neighborhoods.

Interestingly, among white current residents, these results are flipped. Within this population, the arrival of new black next-door neighbors generates the largest increase in 911 alarm calls in low income neighborhoods, but does not differ in previously low versus high crime areas. These results are remarkably consistent with the income heterogeneity results documented in Bayer et al. (2022) and suggest that racial prejudices may be highest among white households in low income neighborhoods. Consistently, we show that in no types of neighborhoods does the likelihood of a crime related call change upon arrival of a new black neighbor.

One potentially surprising result is how similar the effects on white residents are in low black share versus high black share neighborhoods. One may have thought that in neighborhoods with sufficiently high black populations, white current residents would not respond to new black neighbors. However, our finding that suggests instead that new neighbors of a different race can still generate effects since the new neighbors themselves are unknown and can therefore engender racial stereotypes.

5 Conclusions

We provide evidence that receiving a new next door black neighbor increases 911 calls for residential alarms, but has no effect of 911 calls related to criminal activity. These results hold irrespective of the race of the existing resident. We even find some evidence that black households are relatively less likely to apply for CHPs when they receive new white neighbors. Together these result provide evidence that racial stereotypes around black households increase perceived, but not actual, threats to personal safety. These results have policy implications for any government intervention that attempts to promote racial integration as incumbents may misperceive the risk to personal safety from these policies. More work is needed to better understand specific interventions that will reduce perceived threats to personal safety in the context of regular social interactions that are common in neighborhood and school settings.

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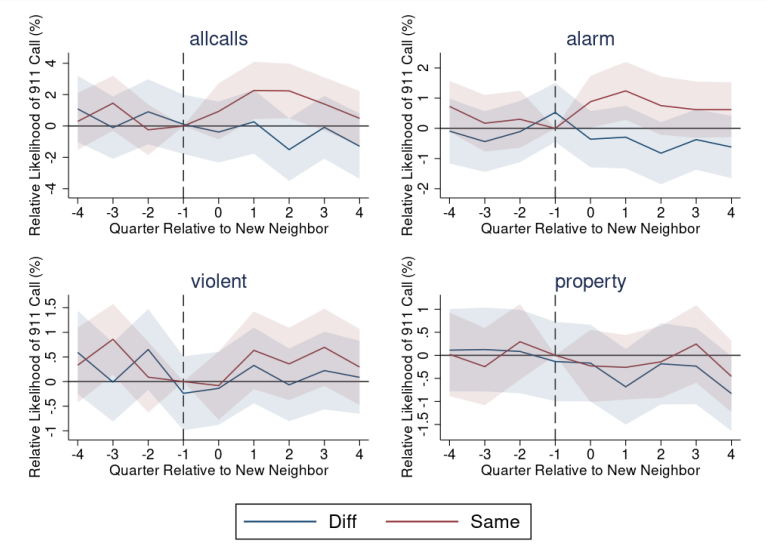
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Figure 1: Event Study Analyses of the Effect of New Next-Door Neighbors on 911 Calls

These event studies plot the estimated effects of receiving a new neighbor of the same race versus a different race who arrives at time t=0. The sample includes all Black and White current residents who received new Black or White next-door neighbors. In the first panel, the outcome variable is dummy equal to 100 if there was a 911 call that quarter coming from the current resident's house for any reason. In the panels that follow, we investigate just calls coming from alarm systems, reporting violent crimes, or reporting property crimes. To create this figure, we regress our outcome of interest on the full set of interacted event time dummies. The omitted group is same-race neighbor at event time -1 (the quarter before they new neighbor arrived). 95% confidence intervals are represented by the shaded regions.

Panel A. Black Current Residents



Panel B. White Current Residents

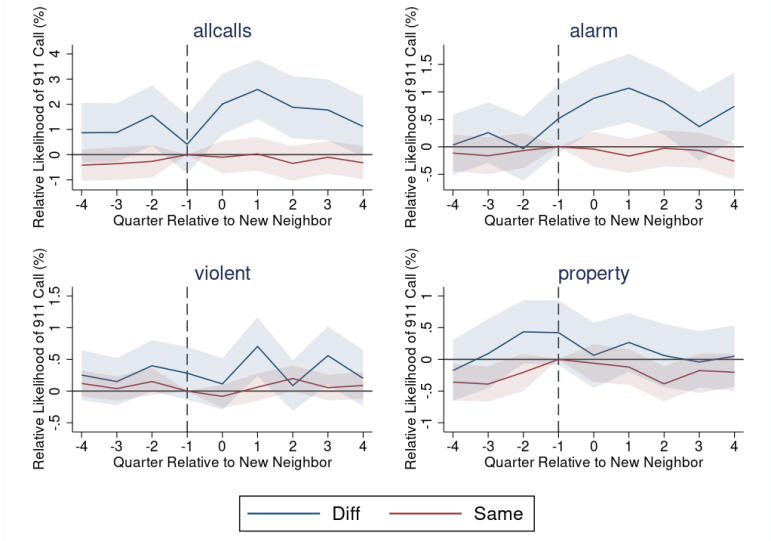
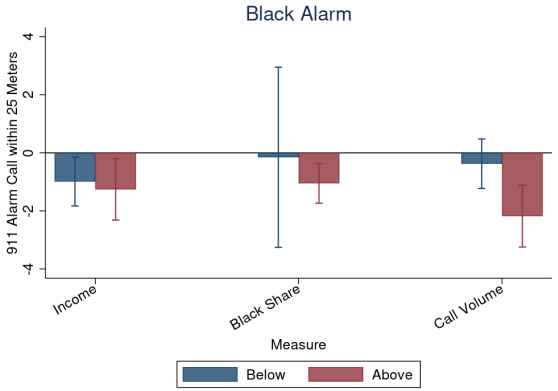


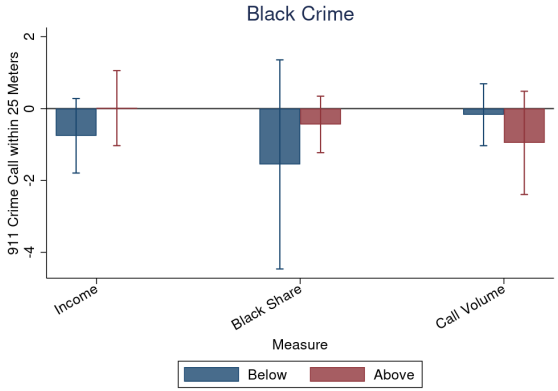
Figure 2: Comparing the Effect of New Different-Race Neighbors as a Function of Neighborhood Attributes

To create this figure, we characterize census block groups on three dimensions: (i) median income, (ii) share of the population that is Black, and (iii) average call volume over the sample period. We then split the sample in half seperately for each characteristic. The bars plot the point estimates produced by our canonical regression estimated on the corresponding subsample. 95% confidence intervals are plotted. The regression estimates can be found in Table B1, Table B2, and Table B3.

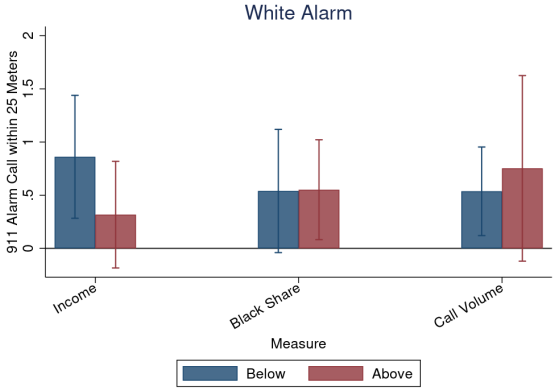
Panel A. Black Current Residents - Alarms



Panel B. Black Current Residents - Crime Calls



Panel C. White Current Residents - Alarms



Panel D. White Current Residents - Crime Calls

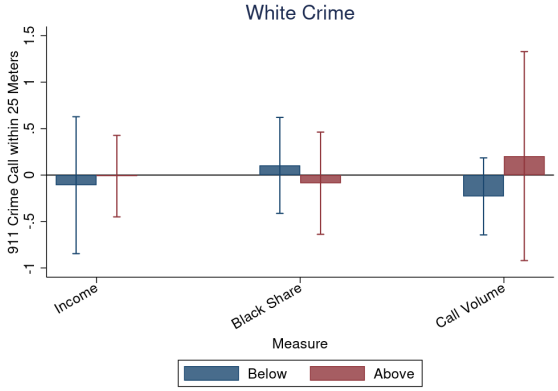


Table 1: Summary Statistics

This table describes our estimation sample. The sample includes all current residents who were involved in an "experiment." A valid "experiment" requires at least two current residents of the same race to each receive a new next-door neighbor in the same quarter and for at least one current resident to be treated (receive a new neighbor of a different race) and at least to be a control (receive a new next-door neighbor the same race). Each current resident is in the sample up to nine quarters – the four prior to the arrival of the new neighbor, the quarter when the new neighbor arrives, and the four following the new neighbor's arrival. The dateset covers experiments in Durham between 2007 and 2018 and experiments in Charlotte between 2006 and 2009. Our outcome variables are dummy variables (=100) if there is a 911 call in that quarter for a given reason within 25 meters of the parcel's geolocation.

	Black Current Residents	White Current Residents
	Mean	Mean
911 Call that Quarter (= 100)		
Nature: All	23.62	11.88
Nature: Alarm	5.20	2.39
Nature: Violent	3.03	1.06
Nature: Property	3.39	1.66
Nature: Disturbance	4.87	2.47
Treatment Assignment		
New Neighbor Different Race	0.45	0.24
Property Characteristics		
Year Built	1995	1991
Building Sq Ft	2,241	2,628
Current Resident Demographics		
Race: Black	1.00	0.00
Race: White	0.00	1.00
Homeowner Age	46.58	48.75
Born in NC	0.41	0.22
Neighborhood Characteristics		
Black Share	0.48	0.23
Total Call Volume	10,250.62	6,973.02
Block Group Median Income	62,758.61	77,936.29
Counts		
Observations	76,288	191,013
Treated Residents	3,650	5,146
Current Residents	4,637	15,467

Table 2: The Effect of Receiving New Next-Door Neighbors on 911 Calls

This table presents estimates of the effect of receiving new next-door neighbors on 911 calls. The sample is described in Table 1. The outcome variable is an indicator variable (=100) if there is a 911 call in the quarter. Treated is a dummy equal to 1 if the new next-door neighbor the current resident receives is a different race. Post is a dummy equal to 0 for the four quarters before the new neighbor arrives and 1 for the quarter the number arrives and the four subsequent quarters. Control variables include home age, home size, resident age, and resident birth state. Group × Arrival Quarter fixed effects represent experiments. Standard errors, adjusted for clustering at the tract-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Black Current Residents

Dependent Variable:	911 Call (=100)				
Nature of 911 Call:	All	Alarm	Violent	Property	Disturbance
	(1)	(2)	(3)	(4)	(5)
Treated × Post	-2.197***	-1.017***	-0.226	-0.270	-0.060
	(0.650)	(0.337)	(0.272)	(0.286)	(0.317)
Post	1.112**	0.540**	0.073	-0.190	0.009
	(0.511)	(0.252)	(0.196)	(0.202)	(0.238)
Treated	0.133	-0.300	-0.068	0.021	-0.114
	(0.683)	(0.357)	(0.259)	(0.269)	(0.304)
Control Variables	X	X	X	X	X
Fixed Effects:					
Group × Arrival Quarter	X	X	X	X	X
Block	X	X	X	X	X
N	76,288	76,288	76,288	76,288	76,288
Dependent Variable Mean	23.62	5.20	3.03	3.39	4.87

Panel B. White Current Residents

Dependent Variable:	911 Call (=100)				
Nature of 911 Call:	All	Alarm	Violent	Property	Disturbance
	(1)	(2)	(3)	(4)	(5)
Treated × Post	0.876**	0.603***	0.073	-0.170	-0.076
	(0.393)	(0.188)	(0.139)	(0.172)	(0.196)
Post	0.087	-0.028	-0.014	0.044	0.118
	(0.173)	(0.080)	(0.054)	(0.068)	(0.083)
Treated	1.176***	0.286	0.197*	0.438***	0.512***
	(0.362)	(0.178)	(0.111)	(0.139)	(0.177)
Controls	X	X	X	X	X
Fixed Effects:					
Group × Arrival Quarterr	X	X	X	X	X
Block	X	X	X	X	X
N	191,013	191,013	191,013	191,013	191,013
Dependent Variable Mean	11.88	2.39	1.06	1.66	2.47

Table 3: The Effect of Receiving New Next-Door Neighbors on Criminal Activity

This table presents estimates of the effect of receiving new next-door neighbors on criminal activity. The sample is similar than the one described in Table 1 but only covers Mecklenburg County since we do not have access to criminal activity data for Duhram County. The outcome variable is an indicator variable (=100) if there is a reported crime in the quarter. Treated is a dummy equal to 1 if the new next-door neighbor the current resident receives is a different race. Post is a dummy equal to 0 for the four quarters before the new neighbor arrives and 1 for the quarter the number arrives and the four subsequent quarters. Control variables include home age, home size, resident age, and resident birth state. Group × Arrival Quarter fixed effects represent experiments. Standard errors, adjusted for clustering at the tract-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Black Current Residents

Dependent Variable:	Criminal Activity (=100)				
Nature of Crime:	All	Violent	Theft	Vandalism	Burglary
	(1)	(2)	(3)	(4)	(5)
Treated × Post	-0.747	-0.736*	-0.086	-0.130	-0.072
	(0.512)	(0.439)	(0.379)	(0.190)	(0.281)
Post	0.359	0.358	-0.120	0.133	-0.186
	(0.370)	(0.294)	(0.268)	(0.119)	(0.213)
Treated	0.263	-0.299	-0.170	0.146	-0.214
	(0.501)	(0.460)	(0.365)	(0.160)	(0.256)
Controls	X	X	X	X	X
Fixed Effects:					
Group × Arrival Quarter	X	X	X	X	X
Block	X	X	X	X	X
N	41,191	41,191	41,191	41,191	41,191
Dependent Variable Mean	6.85	4.47	3.14	0.88	1.63

Panel B. White Current Residents

Dependent Variable:	Criminal Activity (=100)				
Nature of Crime:	All	Violent	Theft	Vandalism	Burglary
	(1)	(2)	(3)	(4)	(5)
Treated × Post	-0.290	0.219	-0.330*	0.136	-0.228*
	(0.296)	(0.211)	(0.197)	(0.118)	(0.131)
Post	-0.116	-0.081	0.053	-0.091**	-0.014
	(0.111)	(0.079)	(0.077)	(0.042)	(0.045)
Treated	0.762***	0.238	0.430**	0.031	0.315***
	(0.253)	(0.174)	(0.174)	(0.097)	(0.111)
Controls	X	X	X	X	X
Fixed Effects:					
Group × Arrival Quarter	X	X	X	X	X
Block	X	X	X	X	X
N	115,831	115,831	115,831	115,831	115,831
Dependent Variable Mean	2.75	1.36	1.34	0.39	0.51

Table 4: The Effect of Receiving New Neighbors on 911 calls

This table presents estimates of the effect of receiving new next-door neighbors on 911 calls. The sample is similar to the one described in Table 1 but is expanded to include all current residents who received a new new neighbor in a home three-doors down or closer. The outcome variable is an indicator variable (=100) if there is a 911 call in the quarter. The new neighbor is either the same race or a different race and is either immediately next-door or two- or three-doors down. Post is a dummy equal to 0 for the four quarters before the new neighbor arrives and 1 for the quarter the number arrives and the four subsequent quarters. Control variables include home age, home size, resident age, and resident birth state. Group × Arrival Quarter fixed effects represent experiments. Standard errors, adjusted for clustering at the tract-year level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Black Current Residents

Dependent Variable:	911 Call (=100)				
Nature of 911 Call:	All	Alarm	Violent	Property	Disturbance
	(1)	(2)	(3)	(4)	(5)
New Next-Door Diff-Race Nbr × Post	-1.100***	-0.481**	-0.293*	-0.396**	-0.002
	(0.407)	(0.235)	(0.169)	(0.182)	(0.199)
New Next-Door Same-Race Nbr × Post	0.963**	0.546**	0.081	-0.296	-0.013
	(0.461)	(0.237)	(0.188)	(0.182)	(0.219)
New 2/3-Door Diff-Race Nbr × Post	-0.133	-0.231	0.177	-0.452***	-0.035
	(0.370)	(0.192)	(0.146)	(0.161)	(0.197)
New 2/3-Door Same-Race Nbr × Post	-0.257	-0.029	-0.194	-0.646***	0.359*
	(0.366)	(0.192)	(0.164)	(0.184)	(0.202)
New Next-Door Diff-Race Nbr	-0.587	-0.139	-0.067	-0.243	-0.058
	(0.522)	(0.278)	(0.206)	(0.211)	(0.254)
New Next-Door Same-Race Nbr	-0.687	0.007	-0.140	-0.148	0.169
	(0.440)	(0.220)	(0.188)	(0.187)	(0.204)
New 2/3-Door Diff-Race Nbr	-0.507	0.063	-0.057	-0.272	-0.051
	(0.517)	(0.270)	(0.203)	(0.194)	(0.244)
Controls	X	X	X	X	X
Fixed Effects:					
Group × Arrival Quarter	X	X	X	X	X
Block	X	X	X	X	X
N	221,565	221,565	221,565	221,565	221,565

Panel B. White Current Residents

Dependent Variable:	911 Call (=100)				
Nature of 911 Call:	All	Alarm	Violent	Property	Disturbance
	(1)	(2)	(3)	(4)	(5)
New Next-Door Diff-Race Nbr × Post	0.931***	0.621***	0.082	-0.153	0.029
	(0.343)	(0.163)	(0.122)	(0.155)	(0.173)
New Next-Door Same-Race Nbr × Post	0.064	-0.013	-0.012	0.008	0.163**
	(0.160)	(0.075)	(0.048)	(0.066)	(0.077)
New 2/3-Door Diff-Race Nbr × Post	0.002	0.102	0.082	-0.108	-0.030
	(0.328)	(0.158)	(0.117)	(0.140)	(0.168)
New 2/3-Door Same-Race Nbr × Post	-0.014	-0.147**	-0.002	-0.099	0.131*
	(0.158)	(0.073)	(0.049)	(0.069)	(0.079)
New Next-Door Diff-Race Nbr	0.791**	0.011	0.125	0.364***	0.342*
	(0.330)	(0.159)	(0.110)	(0.131)	(0.175)
New Next-Door Same-Race Nbr	-0.184	-0.124*	-0.065	-0.024	-0.109
	(0.157)	(0.074)	(0.052)	(0.059)	(0.070)
New 2/3-Door Diff-Race Nbr	1.279***	0.181	0.047	0.347***	0.443***
	(0.313)	(0.148)	(0.100)	(0.127)	(0.151)
Controls	X	X	X	X	X
Fixed Effects:					
Group × Arrival Quarter	X	X	X	X	X
Block	X	X	X	X	X
N	511,804	511,804	511,804	511,804	511,804

Online Appendix: Fear in The Neighborhood: The Costs of Racial Integration

Stephen Billings, Ben McCartney, and Calvin Zhang

Online Appendix

Appendix A – Classifying 911 Calls

Appendix B – Supplemental Tables

A Classifying 911 Calls

We classify 911 calls as follows:

Alarm

```
   gen alarm = inlist(Description, "ALARM-RESIDENTIAL-PRIORITY JST-OCC JUST", "ALARM-RESIDENTIAL
JST-OCC JUST")
   replace alarm = 1 if inlist(nature, "ALARM")
```

Violent Crime

```
   gen violent = inlist(Description, "ASSAULT- PHYSICAL ONLY", "DISCHARGING A FIREARM")
   replace violent = 1 if Description == "DOMESTIC DISTURBANCE JST-OCC" | Description == "DOMESTIC
VIOLENCE-PHYSICAL ASSAULT"
   replace violent = 1 if substr(Description,1,5) == "FIGHT"
   replace violent = 1 if substr(Description,1,4) == "RAPE"
   replace violent = 1 if inlist(Description, "ROBBERY FROM PERSON", "ROBBERY FROM PERSON-ARMED")
   replace violent = 1 if substr(nature,1,7) == "ASSAULT"
   replace violent = 1 if nature == "COMMON LAW ROBBERY"
   replace violent = 1 if substr(nature,1,8) == "DOMESTIC"
   replace violent = 1 if nature == "FIGHT"
   replace violent = 1 if substr(nature,1,7) == "GUNSHOT"
   replace violent = 1 if substr(nature,1,4) == "RAPE"
   replace violent = 1 if substr(nature,1,7) == "ROBBERY"
   replace violent = 1 if nature == "SEXUAL ASSAULT"
   replace violent = 1 if nature == "SOUND OF SHOTS"
   replace violent = 1 if substr(nature,1,4) == "STAB"
```

Property Crime

```
   gen nonviolent = inlist(Description, "BREAK/ENTER RESIDENTIAL-OCCUPIED JST-OCC", "BREAK/ENTER
RESIDENTIAL-UNOCCUPIED JST-OCC")
   replace nonviolent = 1 if substr(Description,1,4) == "DRUG"
   replace nonviolent = 1 if inlist(Description, "LARCENY FROM VEHICLE", "LARCENY JST-OCC JUST")
   replace nonviolent = 1 if substr(Description,1,9) == "LOITERING"
```

```
   replace nonviolent = 1 if Description == "SUBSTANCE/PARAPHERNALIA JST-OCC JUST"
   replace nonviolent = 1 if Description == "TRESPASS JST-OCC JUST"
   replace nonviolent = 1 if substr(nature,1,8) == "BREAK IN"
   replace nonviolent = 1 if substr(nature,1,4) == "DRUG"
   replace nonviolent = 1 if substr(nature,1,11) == "INTOXICATED"
   replace nonviolent = 1 if substr(nature,1,7) == "LARCENY"
   replace nonviolent = 1 if substr(nature,1,8) == "TRESPASS"
   replace nonviolent = 1 if nature == "VANDALISM"
```

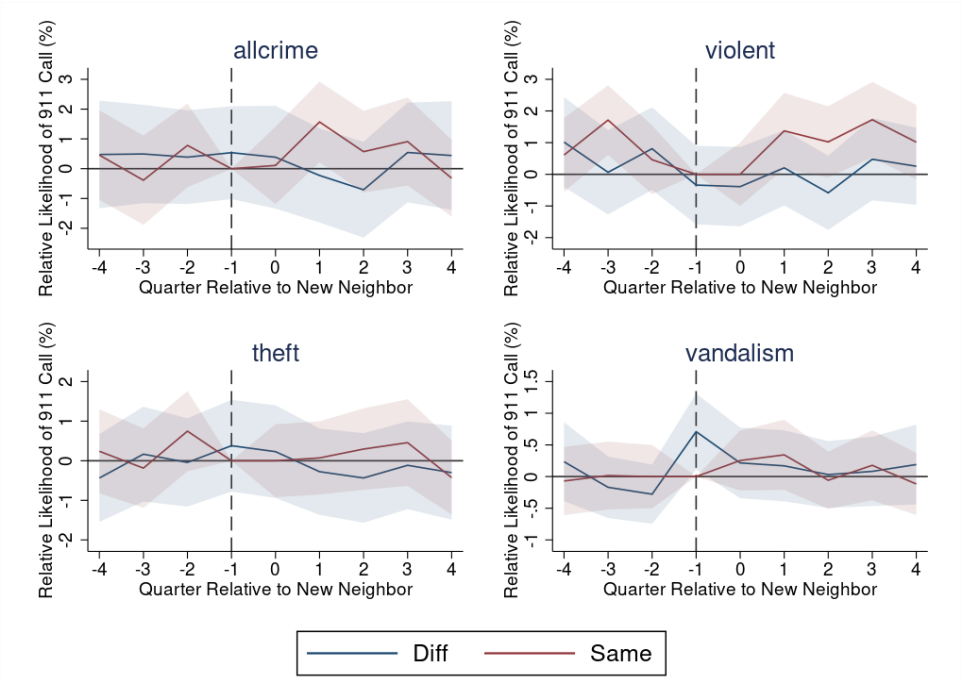
Disturbance

```
   gen disturbance = Description == "CHECK THE WELFARE"
   replace disturbance = 1 if inlist(Description, "COMMUNICATING THREATS-OTHER REPORT",
"COMMUNICATING THREATS-PERSON JST-OCC")
   replace disturbance = 1 if substr(Description,1,15) == "NOISE COMPLAINT"
   replace disturbance = 1 if Description == "SUSPICIOUS PERSON/PROWLER DRTODR"
   replace disturbance = 1 if nature == "ATTEMPT TO LOCATE"
   replace disturbance = 1 if substr(nature,1,11) == "DISTURBANCE"
   replace disturbance = 1 if nature == "HARASSMENT"
   replace disturbance = 1 if substr(nature,1,15) == "NOISE COMPLAINT"
   replace disturbance = 1 if nature == "PANHANDLING OR SOLICITATION"
   replace disturbance = 1 if nature == "PROPERTY CHECK"
   replace disturbance = 1 if substr(nature,1,10) == "SUSPICIOUS"
   replace disturbance = 1 if substr(nature,1,6) == "THREAT"
   replace disturbance = 1 if nature == "WELFARE CHECK"
```

B Supplemental Figures and Tables

Figure B1: Crime Reports

Panel A. Black Current Residents



Panel B. White Current Residents

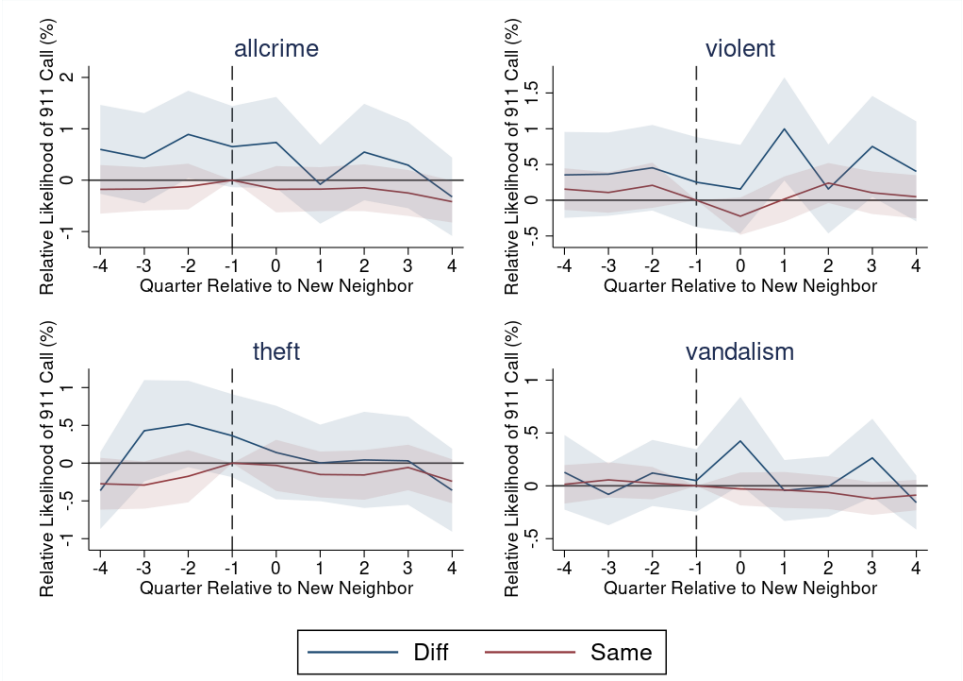
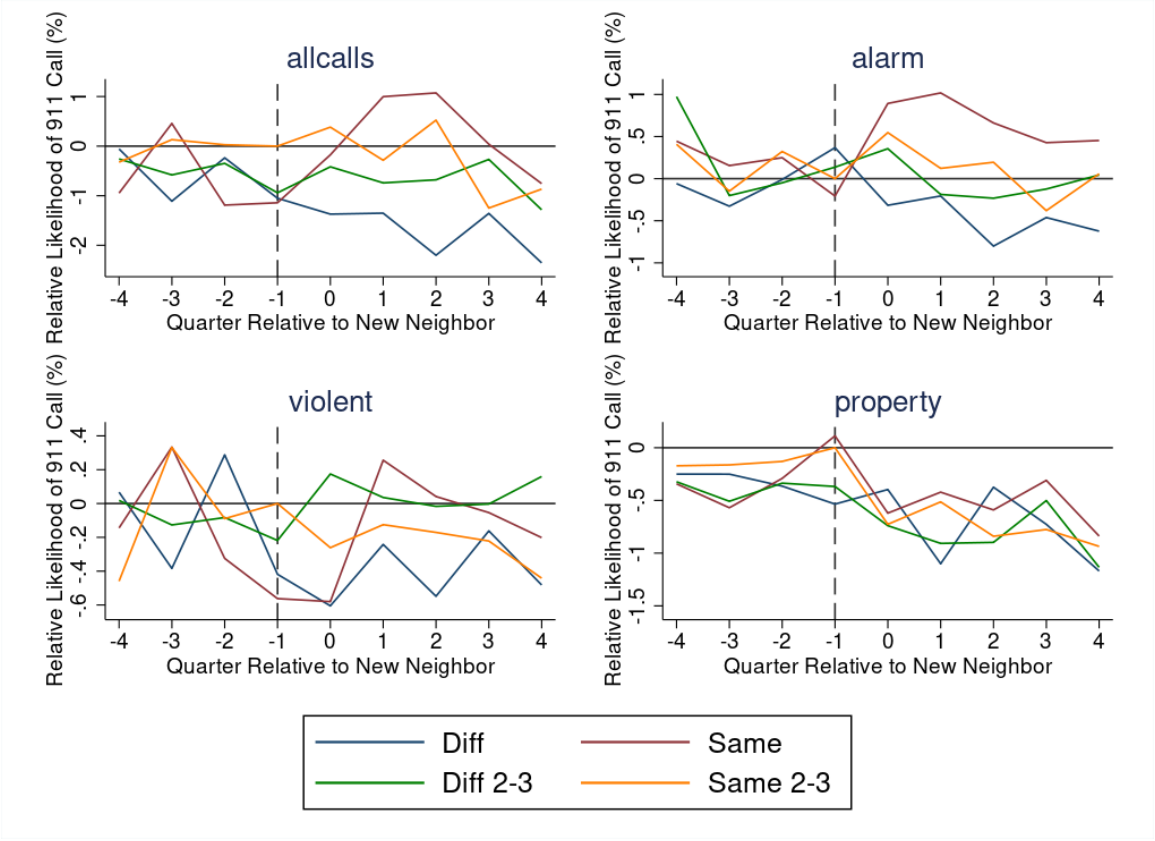


Figure B2: 911 Calls

These event studies plot the estimated effects of receiving a new neighbor at time t=0 on 911 calls. The new neighbor is either the same race or a different race and is either immediately next-door or two- or three-doors down. In the first panel, the outcome variable is dummy equal to 100 if there was a 911 call that quarter coming from the current resident's house for any reason. In the panels that follow, we investigate just calls coming from alarm systems, reporting violent crimes, or reporting property crimes. To create this figure, we regress our outcome of interest on the full set of interacted event time dummies (9 event times × 4 treatment arms). The omitted group is same-race neighbor two- or three-doors down at event time -1 (the quarter before they new neighbor arrived). 95% confidence intervals are represented by the shaded regions.

Panel A. Black Current Residents



Panel B. White Current Residents

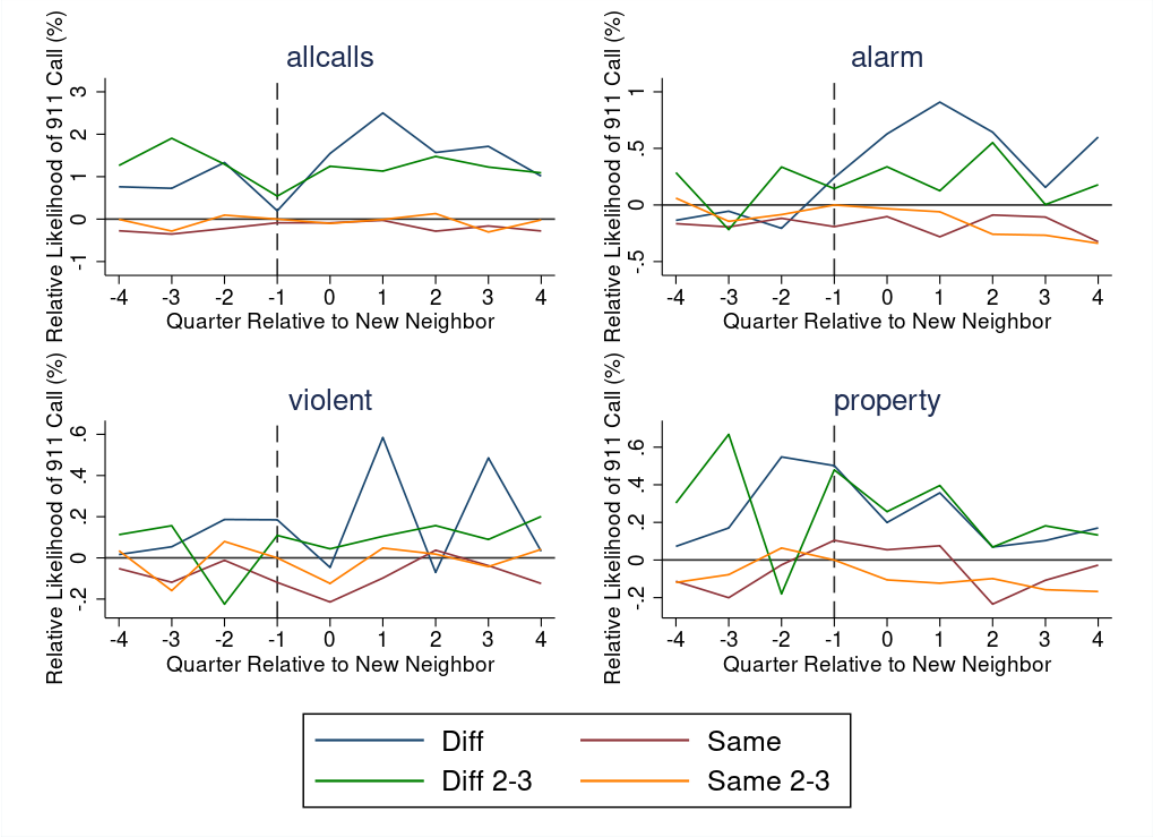


Table B1: Heterogeneity with respect to Neighborhood Income

Comparing the effects of getting different race neighbors in low income vs high income neighborhoods.

All Current Residents

	Alarm	Alarm	Alarm	Alarm	Crime	Crime	Crime	Crime
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
post_have_diff_race_1dr	-0.990**	-1.258**	0.861***	0.317	-0.756	0.013	-0.109	-0.011
	(0.429)	(0.539)	(0.295)	(0.256)	(0.529)	(0.533)	(0.376)	(0.224)
post	0.309	1.080**	-0.018	-0.035	-0.176	-0.164	0.013	0.068
	(0.299)	(0.456)	(0.146)	(0.094)	(0.358)	(0.437)	(0.176)	(0.083)
have_diff_race_1dr	-0.479	0.089	0.414	0.215	-0.089	0.077	1.056***	0.143
	(0.512)	(0.461)	(0.273)	(0.235)	(0.522)	(0.515)	(0.300)	(0.210)
Controls	X	X	X	X	X	X	X	X
Race	Black	Black	White	White	Black	Black	White	White
Income	Under 70K	Over 70K	Under 70K	Over 70K	Under 70K	Over 70K	Under 70K	Over 70K
Fixed Effects:								
Group by Qtr	X	X	X	X	X	X	X	X
Block	X	X	X	X	X	X	X	X
N	49,915	26,373	77,819	113,194	49,915	26,373	77,819	113,194
Dependent Variable Mean	5.68	4.28	2.78	2.12	7.53	3.56	4.02	1.70

Table B2: Heterogeneity with respect to Neighborhood Black Share

Comparing the effects of getting different race neighbors in low vs high black share neighborhoods.

All Current Residents

	Alarm	Alarm	Alarm	Alarm	Crime	Crime	Crime	Crime
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
post_have_diff_race_1dr	-0.152	-1.051***	0.540*	0.552**	-1.553	-0.440	0.104	-0.088
	(1.584)	(0.348)	(0.296)	(0.240)	(1.484)	(0.401)	(0.264)	(0.281)
post	0.340	0.544**	-0.180	0.094	1.757	-0.214	0.144	-0.030
	(1.421)	(0.257)	(0.113)	(0.109)	(1.146)	(0.292)	(0.089)	(0.131)
have_diff_race_1dr	-1.769	-0.275	-0.120	0.504**	-2.172	0.004	0.235	0.666***
	(2.443)	(0.363)	(0.320)	(0.210)	(2.093)	(0.383)	(0.251)	(0.229)
Controls	X	X	X	X	X	X	X	X
Race	Black	Black	White	White	Black	Black	White	White
Block Group Black Share	Under 15%	Over 15%	Under 15%	Over 15%	Under 15%	Over 15%	Under 15%	Over 15%
Fixed Effects:								
Group by Qtr	X	X	X	X	X	X	X	X
Block	X	X	X	X	X	X	X	X
N	2,491	73,797	76,320	114,693	2,491	73,797	76,320	114,693
Dependent Variable Mean	2.61	5.29	1.93	2.70	3.01	6.27	1.32	3.52

Table B3: Heterogeneity with respect to 911 Call Volume

Comparing the effects of getting different race neighbors in low income vs high call volume neighborhoods.

All Current Residents

	Alarm	Alarm	Alarm	Alarm	Crime	Crime	Crime	Crime
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
post_have_diff_race_1dr	-0.375	-2.179***	0.537**	0.753*	-0.170	-0.954	-0.229	0.204
	(0.434)	(0.544)	(0.213)	(0.445)	(0.440)	(0.733)	(0.212)	(0.574)
post	0.284	0.901**	-0.037	0.013	-0.254	-0.062	-0.007	0.311
	(0.330)	(0.384)	(0.080)	(0.261)	(0.327)	(0.515)	(0.079)	(0.286)
have_diff_race_1dr	-0.466	-0.048	0.304	0.263	0.184	-0.580	0.539***	0.834*
	(0.396)	(0.784)	(0.196)	(0.402)	(0.406)	(0.791)	(0.187)	(0.494)
Controls	X	X	X	X	X	X	X	X
Race	Black	Black	White	White	Black	Black	White	White
Block Group 911 Call Volume	Low	High	Low	High	Low	High	Low	High
Fixed Effects:								
Group by Qtr	X	X	X	X	X	X	X	X
Block	X	X	X	X	X	X	X	X
N	48,016	28,272	153,140	37,873	48,016	28,272	153,140	37,873
Dependent Variable Mean	4.72	6.02	2.15	3.35	4.44	9.08	2.01	5.19