Fear in The Neighborhood: The Cost of Racial Prejudice

Stephen B. Billings, W. Ben McCartney, and Calvin Zhang*

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Abstract

Residential segregation in the United States is persistent, and even though policymakers and researchers 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 the receipt of a new Black next-door neighbor, relative to the receipt of a new White next-door neighbor, leads to an increase in 911 calls auto-dialed by home alarm systems. This effect is greatest for white current residents and consistent with a newly installed alarm system. 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.

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*Billings: University of Colorado - Boulder, stephen.billings@colorado.edu. McCartney: McIntire School of Commerce, University of Virginia, ben.mccartney@virginia.edu. Zhang: Federal Reserve Bank of Philadelphia, calvin.zhang@phil.frb.org. Disclaimer: This paper represents preliminary research that is being circulated for discussion purposes. We are thankful to participants at the Western Economic Association 2024 meeting, Fall 2024 VICE Online Seminar, the Institute of Behavioral Research Seminar Series at University of Colorado - Boulder. The views expressed are solely those of the authors and do not necessarily reflect the views of the Federal Reserve Bank of Philadelphia or the Federal Reserve System. Any errors or omissions are the responsibility of the authors.

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 are important for racial disparities across a variety 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 extend 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) show 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 have limited the ability of government policies to directly address racial inequalities. Additionally, policies to promote integration have met resistance from 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 against 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 contribute 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 well 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 are 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 sizable 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 noisier 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 home purchased within a neighborhood is as good as random. Second, we compare current residents who are two or three homes away and of the same race as the treated current resident. 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 30% increase in 911 calls from home alarm systems for white current residents and even black current residents when focusing on households without previous alarm calls. 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 are also confirmed by another data set that details reported crimes. Effects for black new neighbors on existing black residents may be surprising, but are 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.

[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.

We next document heterogeneous treatment effects. First, we show effects seem to exist irrespective of the current resident moving away, but effects are largest for those residents who move away in the next two years. We also see some evidence that younger new next-door black neighbors (under 40) are driving our results. For white residents, the arrival of new black next-door neighbors generates the largest increase in 911 alarm calls in low-income and high black share neighborhoods. These results are 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 type of neighborhood does the likelihood of a crime-related call change upon the 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 lead 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 are a dimension of any policy that promotes racial integration. Specifically, the presence of racial-based fear incurs private costs in terms of instillation and ongoing alarm system fees as well as public costs due to officer time used to respond to alarm system calls. Most importantly, these results show that actual safety does not appear to be compromised based 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 deed 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.[4].

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

[4] For example, linking Home Mortgage Disclosure Act (HMDA) data to property transactions generates a considerably smaller dataset where we can observe the race of both existing residents and new next-door neighbors

911 Calls. We acquire data on 911 calls through a combination of FOIA requests and publicly accessed data. These data describe all 911 calls and the nature or description of the call. We classify calls into one of four types as detailed in Appendix Table A1. 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 our 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 Charlotte-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 for dispatch, the location data in this dataset represents the location of the crime as reported by the officer.

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 of 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 infor-

mation – birth year, race, and sex – from the voter records. In the event that a house has multiple homeowners, we say that the household is Black if either or both of the owners of the property self-report their race as Black. There are two potential limitations of using voter registration information to assign race to homeowners. We do not observe the self-reported race of homeowners who are not registered to vote nor the race of voters who choose not 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 and the match rate are both quite 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 arrivals to the neighborhood.[5]

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.

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.

[5]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 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 explain 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 we scale to be equal to 100 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 neighbor's 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 to be a stacked difference-in-difference model which addresses concerns about differential timing.

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 in 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 residents 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 are included to help with statistical precision.

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." Experiments 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.[6]

Finally, looking at the counts along the bottom rows of the table, we see that there are 2,768 treated Black residents – Black residents who received new next-door White neighbors. Our research design compares these residents to 5,952 control Black residents who live in 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 53,172. For White current residents, our research design compares 1,721 treated residents to 12,767 control residents with a total final sample of 113,261 White residents.

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 small and insignificant decline in alarm calls in the year following the arrival of a new white next-door neighbor, relative to otherwise

[6] Appendix Figure A1 provides maps of where our sample of experiments occur and highlights a wide spatial distribution of neighborhoods throughout Mecklenburg and Durham counties.

similar current residents who receive new black next-door neighbors. Effect sizes are typically less than 10% and quite noisy for the remaining crime calls. When focusing on white current residents, we see a 1.136 or a 30% increase in calls for service related to alarms. For white residents, we see small and noisy impacts on crime-related calls.[7]

In Figure 1, we estimate dynamics around the arrival of the new neighbors. This Figure highlights the conditional mean values of each outcome by time period with all estimates relative to same-race new next door neighborhoods in the quarter prior to a new neighbor moving in.[8] These figures demonstrate no pre-trends for either new same-race or different-race neighbors. 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 different-race neighbor. This holds for 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. Given our focus on quarterly results and the low frequency of our outcomes, no single period is significant, but jointly the post period is significant for white current residents. Taken together, we see an increase in alarm calls for service when a black 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, we also exhibit similar lack of pre-trends prior to the move-in date. Figure 1 shows that, in contrast to alarm calls, 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's 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. 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 (e.g. suspicious person, noise, solicitation,

[7] As shown in Appendix Table A2 All of our results are robust to a number of sample restrictions we originally imposed upon creating our main sample, including removing outliers in terms of home attributes, population density as well as limiting next door neighbors to be within 25 meters.

[8] These figures provide a more transparent version of a standard single line difference-in-difference figure since it can highlight meaningful trends in the control group.

disturbance) 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 dataset 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, Appendix Table A3 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.[9]

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

[9] It is also unlikely that a next door neighborhood would burglarize a current resident since there would be a high probability of being arrested.

housing market. Estimates from these models are provided in Table 3.

[TABLE 3 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 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 Table 4 based on the individual attributes of current residents or new neighbors. Interestingly, we do see negative significant effects of a new white neighborhood on black current residents in column 1 when we focus on current residents who never had a previous alarm call. This result is consistent with racial stereotypes even among black current residents. When turning to white current residents in Panel B, we again see evidence that those without previous alarm calls experience an increase in alarm calls. Effect sizes are even larger with a new black neighbor generating an increase of 1.595 or 99%. Effects for only those without previous alarm calls highlight that our results are based on new alarm systems and not simply turning on existing alarm systems. Since (Bayer et al., 2022) finds that current white residents are more likely to sell their homes when they get a new black next door neighbor, we bisect our sample based on that outcome. Effects seem to exist irrespective of the current resident moving away, but effects are largest for those residents who move away in the next two years. We also see some evidence that younger new next door black neighbors (under 40) are driving our results for current white residents.

[TABLE 4 HERE]

We focus on three aspects of heterogeneity at the neighborhood level in Appendix Table A4, comparing neighborhoods with above median vs below median income, black share, and 911 call volume. We see no interesting patterns of heterogeneity for black current residents, but white current residents see the largest increase in 911 alarm calls in low income, high black share and low call volume

(i.e. lower crime). 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 the arrival of a new black neighbor.

5 Conclusions

We provide evidence that receiving a new next door black neighbor increases 911 calls for residential alarms, but has no effect on 911 calls related to criminal activity. Together these results 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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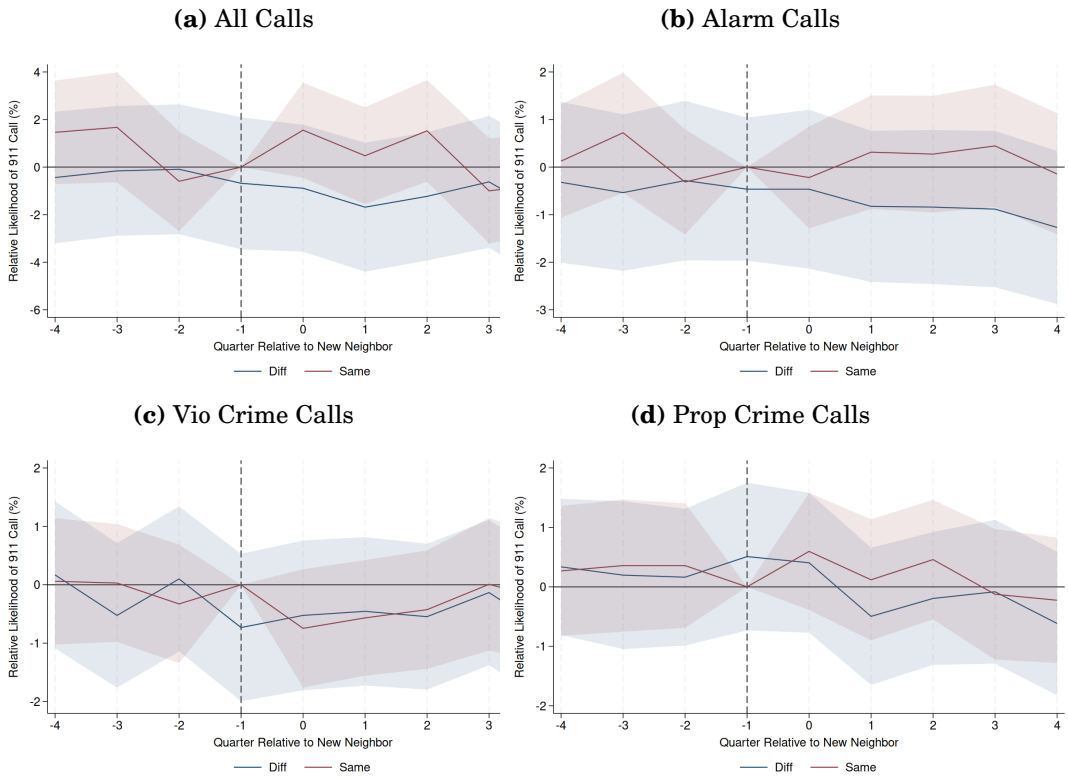
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Figure 1: Event Study Analyses of the Effect of New Next-Door Neighbors on 911 Calls

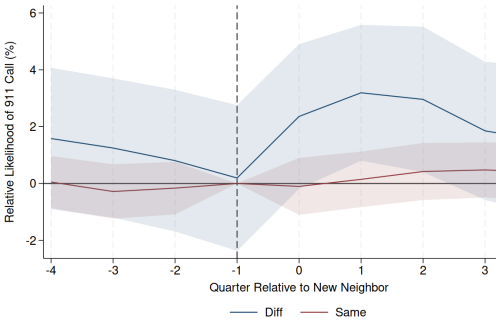
These event studies plot the estimated effects of receiving a new neighbor of the same race versus a different race who arrives at time t=0. The sample includes all Black and White current residents who received new Black or White next-door neighbors. In the first panel, the outcome variable is dummy 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

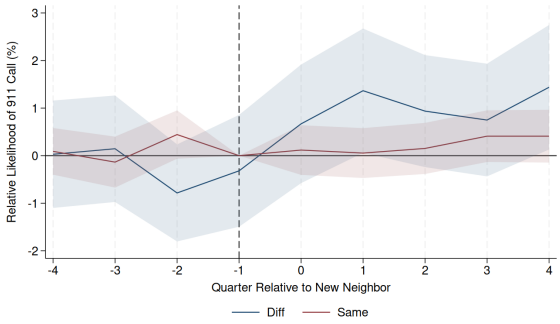


Panel B. White Current Residents

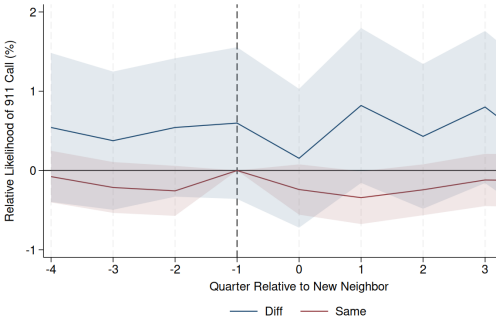
(a) All Calls



(b) Alarm Calls



(c) Vio Crime Calls



(d) Prop Crime Calls

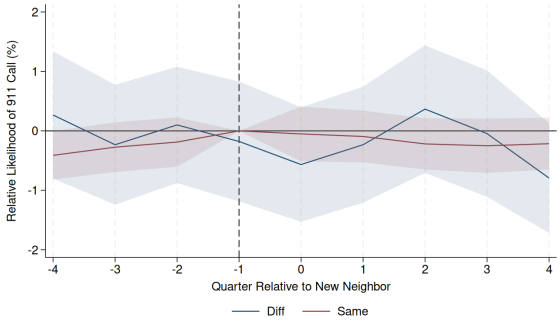


Table 1: Summary Statistics

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	29.58	18.40
Nature: Alarm	5.83	3.87
Nature: Violent	4.14	1.66
Nature: Property	4.51	2.67
Nature: Disturbance	6.03	3.66
Treatment Assignment		
New Neighbor Different Race	0.49	0.13
Property Characteristics		
Year Built	1993	1987
Building Sq Ft	2,049	2,235
Current Resident Demographics		
Race: Black	1.00	0.00
Race: White	0.00	1.00
Homeowner Age	46.22	47.84
Born in NC	0.41	0.21
Neighborhood Characteristics		
Black Share	0.47	0.21
Rate of Total Calls	0.51	0.41
Block Group Median Income	58,997.95	74,014.84
Counts		
Observations	53,172	113,261
Treated Residents	2,768	1,721
Current Residents	5,952	12,767

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

	All Calls	Alarm	Violent	Property	Disturbance
	(1)	(2)	(3)	(4)	(5)
post_new_nbr_diff_race_1dr	-0.811	-0.452	0.096	-0.404	0.408
	(0.851)	(0.461)	(0.354)	(0.386)	(0.429)
post	-0.151	0.000	-0.407	-0.097	-0.537*
	(0.626)	(0.351)	(0.278)	(0.284)	(0.324)
new_nbr_diff_race_1dr	-1.079	-0.535	-0.246	0.065	-0.255
	(1.134)	(0.664)	(0.515)	(0.454)	(0.498)
Controls	X	X	X	X	X
Fixed Effects:					
Group by Qtr	X	X	X	X	X
Block	X	X	X	X	X
N	53,172	53,172	53,172	53,172	53,172
Dependent Variable Mean	29.58	5.83	4.14	4.51	6.03

Panel B. White Current Residents

	All Calls	Alarm	Violent	Property	Disturbance
	(1)	(2)	(3)	(4)	(5)
post_new_nbr_diff_race_1dr	1.328*	1.136***	-0.113	-0.267	-0.188
	(0.754)	(0.392)	(0.304)	(0.319)	(0.386)
post	0.259	0.125	-0.068	0.044	0.072
	(0.267)	(0.145)	(0.080)	(0.111)	(0.134)
new_nbr_diff_race_1dr	1.092	-0.331	0.787**	0.201	0.500
	(0.924)	(0.377)	(0.320)	(0.342)	(0.403)
Controls	X	X	X	X	X
Fixed Effects:					
Group by Qtr	X	X	X	X	X
Block	X	X	X	X	X
N	113,261	113,261	113,261	113,261	113,261
Dependent Variable Mean	18.40	3.87	1.66	2.67	3.66

Table 3: 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

	All Calls	Alarm	Violent	Property	Disturbance
	(1)	(2)	(3)	(4)	(5)
post_new_nbr_diff_race_1dr	-0.948	-0.443	-0.311	-0.503**	-0.134
	(0.594)	(0.300)	(0.237)	(0.250)	(0.285)
post_new_nbr_same_race_1dr	-0.176	-0.007	-0.419	-0.096	-0.549*
	(0.619)	(0.347)	(0.276)	(0.281)	(0.321)
post_new_nbr_diff_race_2_3dr	0.028	-0.169	0.178	-0.106	0.110
	(0.338)	(0.180)	(0.147)	(0.144)	(0.179)
post_new_nbr_same_race_2_3dr	0.148	-0.163	-0.506**	-0.217	0.520**
	(0.418)	(0.213)	(0.204)	(0.193)	(0.225)
new_nbr_diff_race_1dr	-0.215	-0.084	-0.256	0.025	0.533
	(0.743)	(0.395)	(0.285)	(0.271)	(0.344)
new_nbr_same_race_1dr	1.538***	0.139	0.227	0.241	1.068***
	(0.516)	(0.284)	(0.258)	(0.229)	(0.263)
new_nbr_diff_race_2_3dr	-1.087*	-0.267	-0.173	-0.428*	0.272
	(0.631)	(0.324)	(0.248)	(0.235)	(0.296)
Controls	X	X	X	X	X
Fixed Effects:					
Group by Qtr	X	X	X	X	X
Block	X	X	X	X	X
N	170,805	170,805	170,805	170,805	170,805
Dependent Variable Mean	26.73	5.11	3.75	3.87	5.30

Panel B. White Current Residents

	All Calls	Alarm	Violent	Property	Disturbance
	(1)	(2)	(3)	(4)	(5)
post_new_nbr_diff_race_1dr	1.575**	1.239***	-0.180	-0.221	-0.119
	(0.705)	(0.363)	(0.290)	(0.299)	(0.357)
post_new_nbr_same_race_1dr	0.267	0.123	-0.067	0.046	0.073
	(0.265)	(0.145)	(0.080)	(0.110)	(0.133)
post_new_nbr_diff_race_2_3dr	0.113	-0.011	-0.065	-0.014	0.251
	(0.435)	(0.222)	(0.153)	(0.171)	(0.194)
post_new_nbr_same_race_2_3dr	-0.168	-0.162**	-0.013	0.025	0.025
	(0.136)	(0.067)	(0.040)	(0.059)	(0.062)
new_nbr_diff_race_1dr	2.333***	0.071	0.819***	0.680***	0.724**
	(0.649)	(0.308)	(0.251)	(0.243)	(0.309)
new_nbr_same_race_1dr	0.687***	0.192*	0.099	0.206**	0.090
	(0.222)	(0.109)	(0.067)	(0.082)	(0.105)
new_nbr_diff_race_2_3dr	0.485	0.029	0.097	0.200	-0.141
	(0.454)	(0.215)	(0.132)	(0.164)	(0.179)
Controls	X	X	X	X	X
Fixed Effects:					
Group by Qtr	X	X	X	X	X
Block	X	X	X	X	X
N	450,553	450,553	450,553	450,553	450,553
Dependent Variable Mean	14.44	2.98	1.21	2.01	2.84

Table 4: Heterogeneity in the Effects of New Next Door Neighborhood by Individual Attributes

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. 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

	(1)	(2)	(3)	(4)	(5)	(6)
post_new_nbr_diff_race_1dr	-1.723***	-1.364	-0.431	-0.820	-1.217*	0.805
	(0.356)	(1.269)	(0.480)	(1.706)	(0.674)	(0.743)
Sample	No Prev Alarm	Prev Alarm	Don't Sell Next 2 Yr	Sell Next 2 Yr	Nbr Under 40	Nbr 40 and Over
Controls	X	X	X	X	X	X
Fixed Effects:						
Group by Qtr	X	X	X	X	X	X
Block	X	X	X	X	X	X
N	43,312	9,860	49,428	3,744	29,965	20,350
Dependent Variable Mean	2.51	20.43	5.82	5.96	6.17	5.06

Panel B. White Current Residents

	(1)	(2)	(3)	(4)	(5)	(6)
post_new_nbr_diff_race_1dr	1.595***	-0.246	0.880**	3.036***	1.340**	0.822
	(0.329)	(1.686)	(0.428)	(1.060)	(0.576)	(0.568)
Sample	No Prev Alarm	Prev Alarm	Don't Sell Next 2 Yr	Sell Next 2 Yr	Nbr Under 40	Nbr 40 and Over
Controls	X	X	X	X	X	X
Fixed Effects:						
Group by Qtr	X	X	X	X	X	X
Block	X	X	X	X	X	X
N	99,964	13,297	100,080	13,181	64,581	42,654
Dependent Variable Mean	1.60	21.00	3.89	3.76	4.01	3.54

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

Stephen Billings, Ben McCartney, and Calvin Zhang

Online Appendix

A Supplemental Figures and Tables

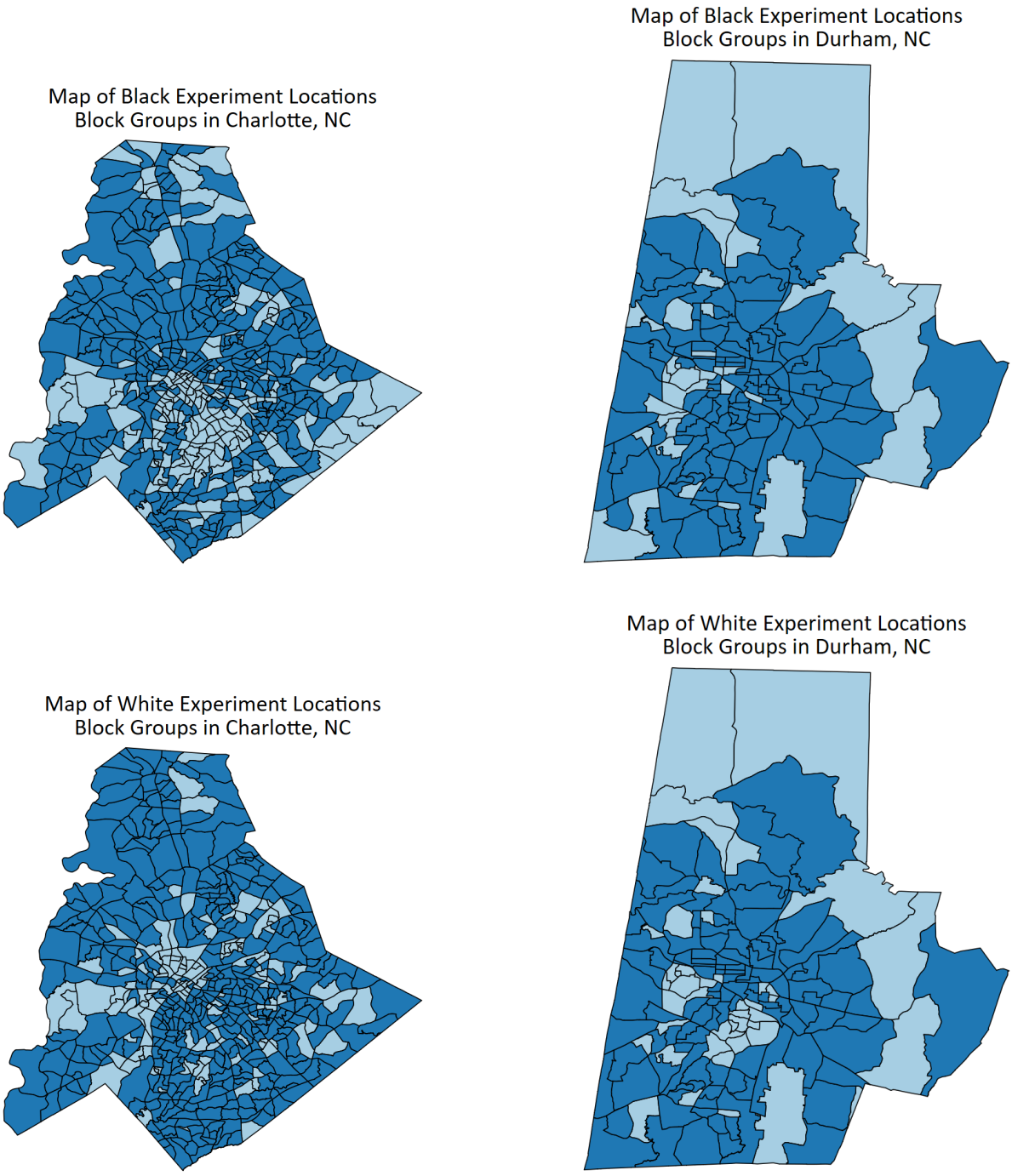


Figure A1: Geographic Coverage of the Sample

Notes: This figure shades in dark blue the US census block groups where our "experiments," as defined in the text and described in Table 1, occur.

Table A1: Crime Counts

Violent	
Assault	10.53%
Domestic Disturbance/Violence	60.08%
Fight	5.78%
Gunshot	18.34%
Rape/Sexual Assault	1.40%
Robbery	3.61%
Stab	0.26%
Property	
Break and Enter	27.39%
Drug	8.73%
Intoxication	2.92%
Larceny	44.45%
Loitering	5.51%
Trespassing	10.65%
Vandalism	0.36%
Disturbance	
Welfare/Property Check	13.18%
Disturbance	24.99%
Harassment	1.18%
Attempt to Locate	12.83%
Noise Complaint	8.70%
Solicitation	1.85%
Suspicious Person	33.12%
Threat	4.14%

Table A2: Robustness to Sample Restrictions

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, except we remove different sample restrictions. 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

	(1)	(2)	(3)	(4)
post_new_nbr_diff_race_1dr	-0.452	-0.468	-0.320	-0.354
	(0.461)	(0.461)	(0.406)	(0.334)
Sample Restrictions	All	No Home Char	No Density	No Nbr Dist
Controls	X	X	X	X
Fixed Effects:				
Group by Qtr	X	X	X	X
Block	X	X	X	X
N	53,172	53,235	68,039	85,325
Dependent Variable Mean	5.83	5.83	5.95	4.75

Panel B. White Current Residents

	(1)	(2)	(3)	(4)
post_new_nbr_diff_race_1dr	1.136***	1.133***	0.995***	0.758***
	(0.392)	(0.392)	(0.353)	(0.231)
Sample Restrictions	All	No Home Char	No Density	No Nbr Dist
Controls	X	X	X	X
Fixed Effects:				
Group by Qtr	X	X	X	X
Block	X	X	X	X
N	113,261	113,680	146,148	253,017
Dependent Variable Mean	3.87	3.88	3.93	2.57

Table A3: 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. 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

	All Crime	Violent	Theft	Vandalism	Burglary
	(1)	(2)	(3)	(4)	(5)
post_new_nbr_diff_race_1dr	-0.184	-0.287	0.092	-0.139	0.054
	(0.477)	(0.186)	(0.326)	(0.089)	(0.222)
post	-0.268	0.033	-0.315	0.037	-0.322*
	(0.361)	(0.141)	(0.237)	(0.065)	(0.169)
new_nbr_diff_race_1dr	0.423	0.218	0.091	0.025	-0.201
	(0.519)	(0.220)	(0.377)	(0.096)	(0.282)
Controls	X	X	X	X	X
Fixed Effects:					
Group by Qtr	X	X	X	X	X
Block	X	X	X	X	X
N	53,172	53,172	53,172	53,172	53,172
Dependent Variable Mean	6.89	0.94	3.41	0.27	1.58

Panel B. White Current Residents

	All Crime	Violent	Theft	Vandalism	Burglary
	(1)	(2)	(3)	(4)	(5)
post_new_nbr_diff_race_1dr	-0.383	-0.190	-0.276	-0.028	-0.137
	(0.418)	(0.159)	(0.301)	(0.062)	(0.203)
post	0.104	-0.059	0.087	0.006	0.008
	(0.130)	(0.037)	(0.096)	(0.021)	(0.059)
new_nbr_diff_race_1dr	0.421	0.441***	0.301	-0.041	0.190
	(0.408)	(0.151)	(0.290)	(0.057)	(0.175)
Controls	X	X	X	X	X
Fixed Effects:					
Group by Qtr	X	X	X	X	X
Block	X	X	X	X	X
N	113,261	113,261	113,261	113,261	113,261
Dependent Variable Mean	3.77	0.32	2.14	0.10	0.80

Table A4: Heterogeneity in the Effects of New Next Door Neighborhood by Neighborhood Attributes

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. 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

	(1)	(2)	(3)	(4)	(5)	(6)
post_new_nbr_diff_race_1dr	-0.526	-0.698	-3.177	-0.489	-0.209	-0.854
	(0.529)	(0.883)	(2.528)	(0.486)	(0.627)	(0.693)
Sample	Low Block Group Income	High Block Group Income	Low Black Share	High Black Share	Low Call Volume	High Call Volume
Controls	X	X	X	X	X	X
Fixed Effects:						
Group by Qtr	X	X	X	X	X	X
Block	X	X	X	X	X	X
N	38,344	14,828	4,873	48,299	29,528	23,644
Dependent Variable Mean	6.08	5.17	4.37	5.98	5.39	6.37

Panel B. White Current Residents

	(1)	(2)	(3)	(4)	(5)	(6)
post_new_nbr_diff_race_1dr	1.177**	1.166*	0.513	1.636***	1.631***	0.144
	(0.501)	(0.625)	(0.940)	(0.446)	(0.463)	(0.744)
Sample	Low Block Group Income	High Block Group Income	Low Black Share	High Black Share	Low Call Volume	High Call Volume
Controls	X	X	X	X	X	X
Fixed Effects:						
Group by Qtr	X	X	X	X	X	X
Block	X	X	X	X	X	X
N	56,083	57,178	54,641	58,620	77,754	35,507
Dependent Variable Mean	3.74	4.01	4.05	3.71	3.44	4.82