Fear in The Neighborhood: The Cost of Racial Prejudice

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

This Version: *February 23, 2025*
Click here for the latest version

Abstract

Even though policymakers and researchers tout the benefits of integration, fear of interacting with different racial groups may perpetuate segregation. 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. 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.

JEL Classification: J14, K40
Keywords: Racial Integration, Crime

*Billings: University of Colorado - Boulder, stephen.billings@colorado.edu. McCartney: McIntire School of Commerce, University of Virginia, ben.mccartney@virginia.edu. Zhang: University of Oregon, calvinz@uoregon.edu. 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, 2024 North American Urban Economic Association Meetings and the Institute of Behavioral Research Seminar Series at University of Colorado - Boulder. Any errors or omissions are the responsibility of the authors.

1 Introduction

The US has high levels of residential segregation, and many Americans do not have friends from other racial groups (Dunsmuir, 2013; Lichter et al., 2016). This segregation impacts not only the quality of neighborhoods and schools (Bayer and McMillan, 2005; Echenique et al., 2006), but also contributes to racial disparities in various outcomes. Studies (Billings et al., 2014; Johnson, 2011; Reber, 2011) have shown that racial integration in schools can reduce the achievement gap between Black and White students and improve criminal outcomes for Black youth. Additionally, exposure to racial diversity influences political beliefs, leading White households to adopt more progressive views (Billings et al., 2022; Chyn et al., 2022). There is also evidence that diversity can enhance generosity, reduce prejudice, and promote egalitarian views (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.

Despite this existing evidence that exposure to diversity can help address systemic racial inequalities, policies to foster integration have largely been undercut by the government and the courts. Notable setbacks include the termination of school desegregation plans in the 1990s, the end of affirmative action in 2022, as well as the continued practice of exclusionary residential zoning, which has 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 at 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). More recently, there has been an increase in White nationalist sentiment, which has been fueled by protests against racial disparities in policing and broader issues concerning immigration in the US (Fahy and Vilchis, 2023; Katz, 2017; Swain, 2002).

These historic resistances to integration efforts suggest concerns about actual or perceived harm to incumbent groups. Our existing evidence on how racial integration impacts personal and social conflict focuses 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). 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) and 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 changes in institutions and economic opportunities.

In this paper, we provide new evidence of how exposure to individuals of different races impacts actual and perceived threats to personal safety by examining idiosyncratic changes to a neighbor's race and not larger changes in racial composition. We analyze reactions from Black or White residents when a neighbor of another race moves in, focusing specifically on the nature of 911 calls made from the address of the current resident.[1] Each 911 call has an associated description or reason for the call, which allows us to disentangle calls auto-dialed by home alarm systems versus those made by people reporting, for example, 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.

To study the effects of living next to neighbors of a different race, we formed a group that includes all current residents who have new neighbors moving in next door. We divided these residents into two categories: the "treated" group, which includes residents whose new neighbors are of a different race, and the "control" group, consisting of those whose new neighbors are of the same race. To ensure that any differences in outcomes between these two groups are due to the race of the new neighbors and not other factors in the neighborhood, we only compared residents from the same neighborhood who received their new neighbors at the same time. The main assumption in our study is that new neighbors often cannot choose their exact homes within their preferred neighborhood due to the

[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 since the location is chosen by the officer taking the report.

idiosyncratic nature of homes becoming available for sale, thus making the assignment of current residents to either the treatment or control group essentially random.[2] We address the possibility that micro-neighborhood trends explain our results in two ways. First, an advantage of our setting is that, since the outcomes we investigate in this paper are recurrent, we can compare the treated and control group both after and before the new neighbor's arrival. We can therefore explicitly test for differential pre-trends. Second, we implement a modification to our research design where we expand our control group to include not just residents who live immediately next door to the new arrival, but also those who live two or three doors down on the same side of the street. This modification allows us to control for micro-neighborhood trends using neighbors only a couple of homes away from the next door neighbor.

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. Voter registration data provides our measure of race since racial information is provided in North Carolina voting records.[3] The timing and locations of 911 calls are matched with residential addresses, allowing us to examine how the arrival of new neighbors from different racial backgrounds impacts 911 calls. Since our measure of residential moves is constructed off property records, our analysis focuses on homeowners. These homeowners are crucial for policy considerations because they are more likely to participate in political activities that influence racial segregation.

We find that receiving a new Black neighbor, relative to a new White neighbor, leads to an increase in 911 calls from the home alarm systems of White current residents of up to 30%. 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. Additionally, our alternative research design confirms that the increase in alarm-triggered 911 calls is significantly higher when a Black neighbor moves directly next door, rather than two or three houses away, ruling out micro-neighborhood factors as the cause.

[2] We are not the first to use research designs of this nature. Indeed, Bayer et al. (2022) and McCartney et al. (2024) lay much of the groundwork for this paper in their thorough tests of the validity of these research designs. Both of those papers investigated how new neighbors affect the move-out decisions – which are, notably, one-time events – of current residents.

[3] 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 that our main results are driven entirely by current residents who had no 911 calls from home alarm systems in the pre-period. This result is best explained by the home alarm system being newly installed, and thus more likely to generate false alarms by the current resident, upon the receipt of a new Black next-door neighbor. Second, to compare these results to the findings of Bayer et al. (2022), we compare the responses of current residents who move away in the near future. If move-outs and installing home alarm systems are both caused by the same underlying racial prejudice, we would expect to see alarm call responses strongest among residents who eventually move. This is exactly what we find. Although we do also document responses even among current residents who stay. Finally, we present some evidence that younger new next-door Black neighbors (those under 40 years old) are driving our results. Finally, we document that, 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.

Our results contribute to 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 White, 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 White respondents 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. Consistent with a context 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 shift-share instrument and survey data to show that the large influx of immigrants in Chile had no impacts on crime victimization but did lead to increases in home and personal security.

The implications of our results are important as perceived threats from certain racial groups generate private costs in terms of installation 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 is not 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 be-

ing 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 the fact that 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 assemble a number of datasets at the property level and use a new next-door neighbor research design to identify whether households' choices are directly linked to the identities of their neighbors.

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. This data details information about owner name, home size, home age, complete site address, and latitude and longitude for each residential parcel. 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).[4] These data are available as yearly snapshots starting in 2005. Each year, we know the full name, age, 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.[5]

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

[4] Source: https://dl.ncsbe.gov/index.html, accessed August 1, 2024.

[5] 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.

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. We do, however, 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 which requires precise geographical information.

Criminal Activity. We collect comprehensive data on all reported criminal incidents from the Charlotte-Mecklenburg and Durham Police Departments. Our dataset covers the period from 1998 to 2018 for Charlotte-Mecklenburg and from the second quarter of 2006 to the third quarter of 2018 for Durham. This dataset describes the nature and location of the criminal activity based on the report filed with the police officer.

2.2 Sample Creation

The first step in creating our sample dataset is to use the complete history of real estate transactions from the deeds data and a cross-section of ownership records from the assessor data to create a quarterly panel of parcels and their owners. To align with our new-neighbor research design, we restrict our sample to parcels where neighboring properties can be clearly identified. As a result, our final sample consists of single-family homes, townhomes, as well as some multiplexes, but does not include apartments or condominiums. This parcel-by-quarter panel dataset includes the geolocation of every residential property along with the full names 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 to identify addresses that are immediately next door to each other. 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 homeowners, we say that the household is Black-owned if either or both of the owners of the property self-report their race as Black. A limitation of using voter data to assign race is that we do not observe the race of non-voters or voters who do not specify 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. First, since the state voter rolls and the registry of deeds serve legal purposes, the quality of the data and the match rate between the two data sets is quite high. Second, 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.[6]

Next, 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, for any reason, geolocated to within 25 meters of a parcel's point location in a given quarter.[7] We further create subcategory variables that indicate the specific nature or description of the call: a call dialed by a home alarm system, a call reporting a violent crime, a call reporting a property crime, or a call reporting a disturbance. We follow a similar procedure to merge in reported crimes. Our full sample is thus a quarterly panel of parcels that describes the occupants and their race, the races of their neighbors, tags for if those neighbors recently moved in, and the nature and probability of 911 calls and crimes assigned to each parcel.

2.3 Estimation Sample

To create the main estimation sample, we restrict the sample described above to current residents who (i) received new next-door neighbors and (ii) whose race and new neighbor's race we observe. We then impose a final set of restrictions to our sample, similar to Bayer et al. (2022). First, to ensure consistency in our housing stock, we drop all homes built before 1900 or with over 6,000 square feet of living area. Second, we keep only block groups where the number of housing units on the block

[6]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 McCartney (2021) and McCartney et al. (2024) for more details on both the process and the quality of the merged data set.

[7]While 911 calls are precisely geolocated, the exact location of the call and the parcel's latitude and longitude in the CoreLogic data may still differ. It is to capture this kind of imprecision that we allow for a 25 meter difference.

is between 20 and 500 and the population density is between 500 and 10,000 persons per square mile based on data from the 2010 census. Third, to focus our sample on those neighborhoods where social interactions between neighbors are most likely to occur, we drop residents whose next-door neighbors live more than 25 meters away.[8] Finally, we require that incumbent households have lived in their home at least a year prior to receiving a new next-door neighbor and that incumbents do not move out in either the quarter the new neighbor arrives or the subsequent one. This filter drops households whose choices were likely determined prior to the new neighbor's arrival and thus unaffected by them. We describe our final sample in Table 1.

This summary table highlights the attributes of current residents split by race. Consistent with existing patterns of residential segregation, we see that Black and White current residents live in neighborhoods (census block groups) with Black share equal to 47% and 21% respectively. Since our research design requires a neighborhood to have some level of racial mixing, our sample mechanically excludes the most segregated blocks.[9] Looking at the counts along the bottom rows of the table helps to clarify how we implement our research design. There are 2,768 "treated" Black residents – Black residents who received new next-door White neighbors. Our research design compares these residents to 3,184 "control" Black residents – Black residents who received new next-door Black neighbors. Since we follow incumbents starting four quarters before the new neighbor's arrival through the quarter of the arrival and then for four more quarters following the arrival, the number of resident-quarter observations in our final sample of Black residents is 53,172.[10] For White current residents, our research design compares 1,721 treated residents to 11,048 control residents with a total final sample of 113,279 White resident-quarters.

2.4 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 might naively consider the sample of current residents who receive new different-race neighbors and compare the likelihood of 911 calls in the quarters after the arrival of the new next-door neighbor to the likelihood in the quarters before.

[8] We confirm the robustness of our results to these choices in the appendix.

[9] 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.

[10] Our final sample of 53,172 observations is slightly less than $(2,768 + 3,184) \times 9 = 53,568$ since some incumbents move out in event times t+2, t+3, or t+4.

This simple event study suffers from two shortcomings. First, it might be the change in next-door neighbor that matters, not necessarily the characteristics of that new neighbor. Second, unobserved neighborhood-level changes might explain both the arrival of the new neighbor and any change in the rate of 911 calls. To address both of these concerns, we use a new next-door neighbor research design which compares current residents who receive new different-race, next-door neighbors (the treated group) to other same-race, same block-group current residents who, at the same time, receive new same-race next-door neighbors (the control group). Specifically, 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:

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

where current resident households are indexed by i and the nine event time periods around the arrival of the new neighbor are indexed by t, where $t \in -4, ..., 4$. $I(\text{911 Call})_{i,t}$ is a dummy variable we scale to be equal to 100 if there is at least one 911 call spatially assigned to household i in event time 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 current 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 (event times $t \in 0, ..., 4$) and a 0 in the four quarters prior to the new neighbor's arrival (event times $t \in -4, ..., -1$). Control variables include home age, home size, current resident's age, and current resident's birth state.

Finally, we include census block group-by-new neighbor arrival quarter fixed effects ($\eta_{group \times new\ nbr\ quarter}$). These fixed effects define our quasi-experiments and mean that variation in treatment status is a function of only whether the current resident received a new different-race or new same-race neighbor. This fixed effect is the same for all nine event quarters that we follow the current residents. In other words, it is *not* a block group-by-calendar quarter fixed effect (which would change each quarter). The inclusion of this fixed effect also renders our main estimation strategy a stacked difference-in-differences 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, at the same time, received new next-door neighbors of the *same* race. We note that, under our assumption that which of the block group's current residents receives the different-race versus same-race new neighbor is as if randomly assigned (conditional on the block group-by-arrival quarter fixed effect), our inclusion of resident and property controls and block fixed effects is not necessary for identification. We nevertheless include them to help with statistical precision.

The richness of the data also allows us to estimate a dynamic difference-in-differences with multiple leads and lags, instead of the aggregate 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 relative likelihoods of a call being made in a given quarter compared to the baseline likelihood of a call being made by control residents in the quarter before the new neighbors arrive.

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.

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 similar current residents who receive new Black next-door neighbors. Effect sizes are typically less than 10% and quite noisy for the remaining call types. When focusing on White current residents, in contrast, we see a 1.139 percentage point, or 30%, increase in calls for service coming from home alarm systems. However, inconsistent with the idea that this increase in home alarm system calls is driven by actual increases in criminal activity, we document small effects of new Black neighbors on the likelihood of crime-related calls for service being directed to White homes.[11]

[11] As shown in Appendix Table A2, all of our results are robust to excluding the sample restrictions we originally imposed when creating our main sample.

In Figure 1, we estimate dynamics around the arrival of the new neighbors.[12] We find no pre-trends among those receiving either same-race or different-race new 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 calls for any reason as well as calls for alarms – which are largely driving the all calls results – among White residents receiving new Black neighbors.[13]

When we turn to other calls for service that are related to any criminal activity, we also exhibit a 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 changed based on the new neighbor's race. Since racial tensions or stereotypes may engender fear from a new Black neighbor, this would, if anything, cause greater usage of 911 calls about issues related to public safety. Individuals will almost 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, disturbance) should highlight if more discretionary calls are heightened based on racial differences. As shown in Table 2, we find limited effects of racial differences in new next-door neighbors even for disturbance calls.

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 police reports. 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.

These results 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

[12] These figures provide a more transparent version of a standard single line difference-in-difference figure since they can highlight meaningful trends in both the treatment and control group.

[13] Focusing on quarterly results loses a lot of statistical power and means that no single period's effect is significant. However, when considered jointly, as in Table 2, the post period shows a significant increase in home alarm calls for White current residents.

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 potential threat to the validity of our main research design is that new neighbors are systematically sorting to very localized neighborhoods. In other words, the part of the census block group where the Black arriver moved to is different than the part of the neighborhood where the new White arriver moved to in a way that violates our identifying assumption. 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, instead of comparing one treated group (incumbents who received new different-race next-door neighbors) to one control group (incumbents who received new same-race next-door neighbors), we add two more kinds of current residents. Current residents who live two- or three-doors down from new different-race neighbors and current residents who live two- or three-doors down from new same-race neighbors.

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 even hyper-local amenities and neighborhood changes would surely be shared. That is, if it were the case that the Black arriver was choosing a part of the neighborhood that was changing in a certain way, then we would expect to see that all of the White incumbents were similarly "affected" regardless of whether they lived one-, two-, or three-doors down. However, this is not what we see from results in Table 3.

This analysis yields two important takeaways. First, as before, the largest effect is found in alarm calls, especially among White current residents receiving new Black next-door neighbors. Furthermore, it is among White residents who receive a new Black neighbor immediately next door that the effect is the strongest. This dramatically decaying effect size is most consistent with perceived threats to safety being driven by the neighbors nearest and most salient to current residents.

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.[14] 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 percentage points, 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.

We focus on three aspects of heterogeneity at the neighborhood level in Appendix Table A4, comparing neighborhoods with above median vs below median values for household income, Black share, and 911 call rate (the average number of calls per person per year). We see no interesting patterns of heterogeneity for Black current residents, but White current residents see the largest increase in 911 alarm calls especially in high Black share and low call rate (i.e. lower crime) block groups.

5 Conclusions

We provide evidence that receiving a new next-door Black neighbor increases 911 calls for residential alarms, but has no effect on either 911 calls related to criminal activity or documented crimes. 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 mis-

[14] Arnold et al. (2018) provide evidence of similar racial stereotypes towards minority defendants in bail setting decisions of both minority and non-minority judges.

perceive 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.

References

ACHARD, P., ALBRECHT, S., CETTOLIN, E., GHIDONI, R., AND SUETENS, S. 2022. The effect of exposure to ethnic minorities on ethnic preferences.

AJZENMAN, N., DOMINGUEZ, P., AND UNDURRAGA, R. 2023. Immigration, crime, and crime (mis) perceptions. *American Economic Journal: Applied Economics* 15:142–176.

ALAN, S., BAYSAN, C., GUMREN, M., AND KUBILAY, E. 2021. Building social cohesion in ethnically mixed schools: An intervention on perspective taking. *The Quarterly Journal of Economics* 136:2147–2194.

ARNOLD, D., DOBBIE, W., AND YANG, C. S. 2018. Racial bias in bail decisions. *The Quarterly Journal of Economics* 133:1885–1932.

BA, B. A., KNOX, D., MUMMOLO, J., AND RIVERA, R. 2021. The role of officer race and gender in police-civilian interactions in chicago. *Science* 371:696–702.

BAYER, P., CASEY, M. D., MCCARTNEY, W. B., ORELLANA-LI, J., AND ZHANG, C. S. 2022. Distinguishing causes of neighborhood racial change: A nearest neighbor design. Technical report, National Bureau of Economic Research.

BAYER, P. AND MCMILLAN, R. 2005. Racial sorting and neighborhood quality.

BAZZI, S., GADUH, A., ROTHENBERG, A. D., AND WONG, M. 2019. Unity in diversity? how intergroup contact can foster nation building. *American Economic Review* 109:3978–4025.

BILLINGS, S., CHYN, E., AND HAGGAG, K. 2022. The long-run effects of school racial diversity on political identity. *American Economic Review: Insights* 3:267–284.

BILLINGS, S. B., DEMING, D. J., AND ROCKOFF, J. 2014. School segregation, educational attainment, and crime: Evidence from the end of busing in charlotte-mecklenburg. *The Quarterly journal of economics* 129:435–476.

BREETZKE, G. D. 2018. The concentration of urban crime in space by race: Evidence from south africa. *Urban geography* 39:1195–1220.

BURSZTYN, L., CHANEY, T., HASSAN, T. A., AND RAO, A. 2021. The immigrant next door: Exposure, prejudice, and altruism. *NBER Working paper* .

CHIRICOS, T., HOGAN, M., AND GERTZ, M. 1997. Racial composition of neighborhood and fear of crime. *Criminology* 35:107–132.

CHYN, E., HAGGAG, K., AND STUART, B. 2022. The effects of racial segregation on intergenerational mobility: Evidence from historical railroad placement. *Dartmouth University Working Paper* .

DELMONT, M. F. 2016. Why busing failed: Race, media, and the national resistance to school desegregation, volume 42. Univ of California Press.

DERENONCOURT, E. 2022. Can you move to opportunity? evidence from the great migration. *American Economic Review* 112:369–408.

DOMÈNECH-ARUMÍ, G. 2023. Black neighbors matter: Officer neighborhoods and racial differences in policing. *Working Paper* .

DRAKULICH, K. M. 2012. Strangers, neighbors, and race: A contact model of stereotypes and racial anxieties about crime. *Race and justice* 2:322–355.

DUNSMUIR, L. 2013. Many americans have no friends of another race: Poll. *Reuters, August* 8.

ECHENIQUE, F., FRYER JR, R. G., AND KAUFMAN, A. 2006. Is school segregation good or bad? *American Economic Review* 96:265–269.

FAHY, C. AND VILCHIS, R. 2023. Suing. heckling. cursing. n.y.c. protests against migrants escalate. *New York Times* .

GOETZ, E. G. 2018. The one-way street of integration: Fair housing and the pursuit of racial justice in American cities. Cornell University Press.

HALL, A. B. AND YODER, J. 2022. Does homeownership influence political behavior? evidence from administrative data. *The Journal of Politics* 84:351–366.

HIPP, J. R. 2010. Resident perceptions of crime and disorder: How much is "bias", and how much is social environment differences? *Criminology* 48:475–508.

HURWITZ, J. AND PEFFLEY, M. 1997. Public perceptions of race and crime: The role of racial stereotypes. *American journal of political science* pp. 375–401.

IHLANFELDT, K., YANG, C., AND RODGERS, L. P. 2024. Adding race and ethnicity to microeconomic databases: An assessment of alternative options. *Working Paper* .

JOHNSON, R. 2011. Long-run impacts of school desegregation school quality on adult attainments. *NBER Working Paper 16664* .

KATZ, A. 2017. Charlottesville:'unite the right'rally, state of emergency. *Time. Time. Retrieved from* .

LICHTER, D. T., PARISI, D., AND DE VALK, H. 2016. Residential segregation. *Pathways* pp. 65–74.

LIGHT, M. T. AND THOMAS, J. T. 2019. Segregation and violence reconsidered: Do whites benefit from residential segregation? *American sociological review* 84:690–725.

LOUIE, P. AND DEANGELIS, R. T. 2024. Fear of a black neighborhood: Anti-black racism and the health of white americans. *Social Forces* 102:817–838.

MCCABE, B. J. 2016. No place like home: Wealth, community, and the politics of homeownership. Oxford University Press.

MCCARTNEY, W. B. 2021. Does household finance affect the political process? evidence from voter turnout during a housing crisis. *The Review of Financial Studies* 34:949–984.

MCCARTNEY, W. B., ORELLANA-LI, J., AND ZHANG, C. 2024. Political polarization affects households' financial decisions: Evidence from home sales. *Journal of Finance* 79:795–841.

MEYER, S. G. 2000. As long as they don't move next door: Segregation and racial conflict in American neighborhoods. Rowman & Littlefield.

O'ROURKE, S. P. AND PACE, L. K. 2020. Like Wildfire: The Rhetoric of the Civil Rights Sit-Ins. Univ of South Carolina Press.

QUILLIAN, L. AND PAGER, D. 2001. Black neighbors, higher crime? the role of racial stereotypes in evaluations of neighborhood crime. *American journal of sociology* 107:717–767.

QUILLIAN, L. AND PAGER, D. 2010. Estimating risk: Stereotype amplification and the perceived risk of criminal victimization. *Social psychology quarterly* 73:79–104.

RAO, G. 2019. Familiarity does not breed contempt: Generosity, discrimination, and diversity in delhi schools. *American Economic Review* 109:774–809.

REBER, S. 2011. School desegregation and educational attainment for blacks. *Journal of Human Resources* 45:893–914.

RIVERA, R. 2022. The effect of minority peers on future arrest quantity and quality. *Available at SSRN 4067011* .

SMETS, K. AND VAN HAM, C. 2013. The embarrassment of riches? a meta-analysis of individual-level research on voter turnout. *Electoral studies* 32:344–359.

SWAIN, C. M. 2002. The new white nationalism in America: Its challenge to integration. Cambridge University Press.

WARD, J. M. 2011. Defending White Democracy: The Making of a Segregationist Movement and the Remaking of Racial Politics, 1936-1965. Univ of North Carolina Press.

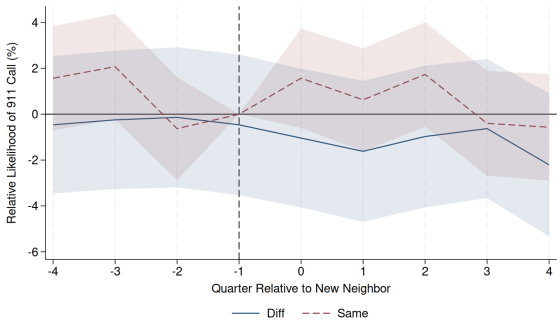
XIE, M. AND LAURITSEN, J. L. 2012. Racial context and crime reporting: A test of black's stratification hypothesis. *Journal of quantitative criminology* 28:265–293.

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

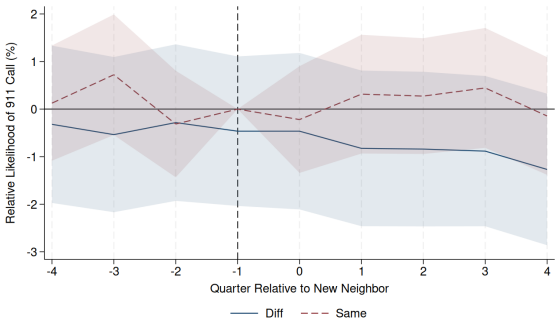
These event studies plot the estimated effects of receiving a new neighbor of the same race (control) versus a new neighbor of a different race (treated). The sample is described in Table 1. New neighbors arrive at time t=0. The sample includes all Black and White current residents who received new Black 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 spatially assigned to the 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

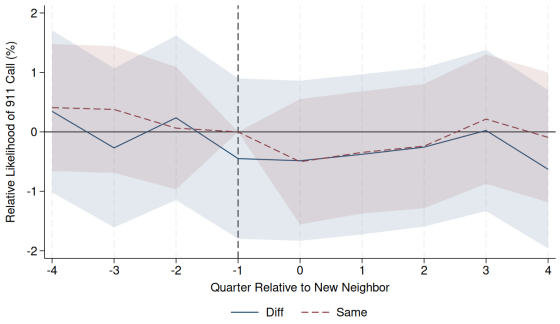
(a) Calls for Any Reason



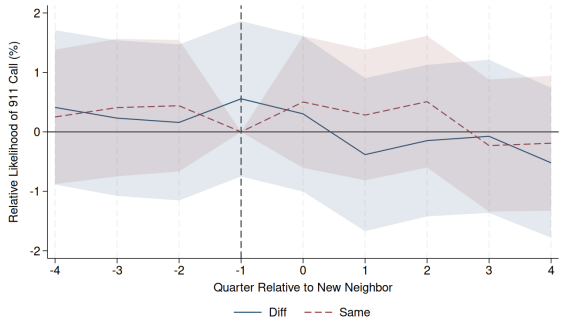
(b) Home Alarm Calls



(c) Violent Crime Calls

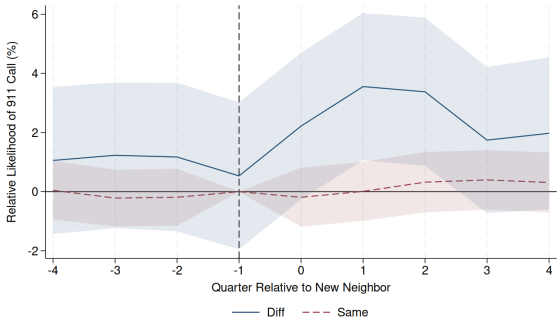


(d) Property Crime Calls

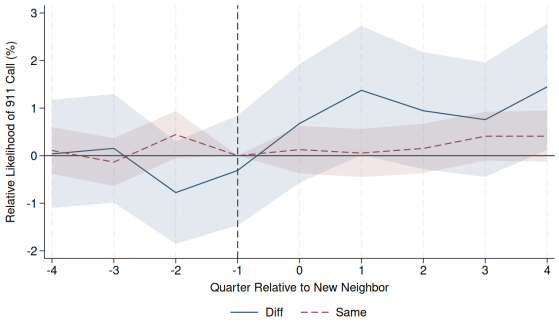


Panel B. White Current Residents

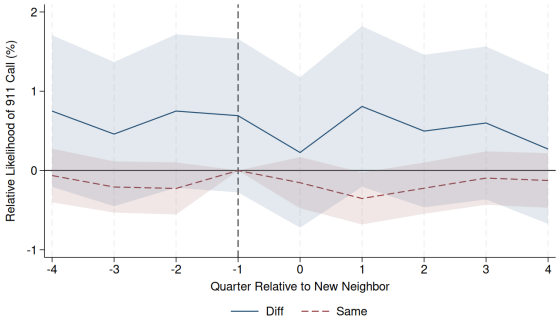
(a) Calls for Any Reason



(b) Home Alarm Calls



(c) Violent Crime Calls



(d) Property Crime Calls

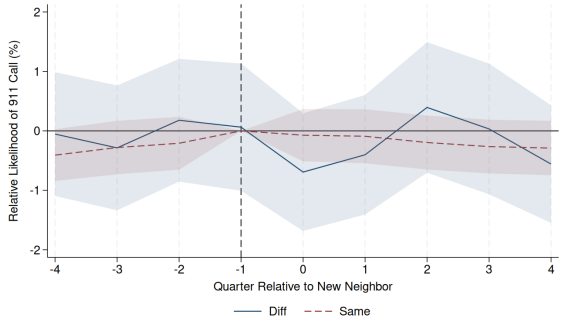


Table 1: Summary Statistics

This table describes our estimation sample. The sample includes all current residents who received new next-door neighbors. Each time a current resident receives a new next-door neighbor, that individual is in the sample up to nine quarters – the four prior to the arrival of the new neighbor (event times t-4 to t-1), the quarter when the new neighbor arrives (event time t=0), and the four following the new neighbor's arrival (event times t+1 to t+4). The dateset covers new neighbor arrivals that occurred in Durham between 2007q2 and 2017q3 and in Charlotte between 2006q1 and 2009q4. Sample restrictions used to create the final data set can be found in section 2.3. Our outcome variables are dummy variables (=100) if there is a 911 call for a given reason spatially assigned to a given address. New Neighbor Different Race is an indicator variable equal to 1 if the new neighbor that arrives at event time t=0 has a different race than the incumbent. Rate of 911 calls is defined as the block group's calls per person per year.

	Black Current Residents	White Current Residents
	Mean	Mean
911 Call that Quarter (= 100)		
Reason: Any	29.58	18.40
Reason: Alarm System	5.83	3.88
Reason: Violent Crime	4.14	1.66
Reason: Property Crime	4.51	2.67
Reason: Disturbance	6.03	3.66
Treatment Assignment		
New Neighbor Different Race	0.46	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
Block Group Characteristics		
Black Share	0.47	0.21
Rate of 911 Calls	0.51	0.41
Median Income	$58,998	$74,013
Counts		
Observations	53,172	113,279
Treated Residents	2,768	1,721
Control Residents	3,184	11,048

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

This table presents estimates of the effect of an incumbent household receiving a new next-door neighbor on 911 calls directed to an incumbent's property. The sample is described in Table 1. We consider five different outcome variables: calls for any reason (column 1), calls made by a home alarm system (column 2), calls reporting a violent crime (column 3), calls reporting a property crime (column 4), and calls reporting a disturbance (column 5). Each is an indicator variable (=100) if there is a 911 call directed to a given property. New Nbr Diff Race 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 new neighbor arrives and the four subsequent quarters. Control variables include home age, home size, resident age, and resident birth state. Census block group by new-neighbor arrival quarter fixed effects and separate block fixed effects are also included. Standard errors, adjusted for clustering at the block group-by-arrival year quarter level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Black Current Residents

Dependent Variable:	Any	Alarm	Violent	Property	Disturbance
	(1)	(2)	(3)	(4)	(5)
New Nbr Diff Race × Post	-0.811	-0.452	0.096	-0.404	0.408
	(0.813)	(0.440)	(0.373)	(0.376)	(0.426)
Post	-0.151	0.000	-0.407	-0.097	-0.537*
	(0.596)	(0.321)	(0.284)	(0.280)	(0.319)
New Nbr Diff Race	-1.079	-0.535	-0.246	0.065	-0.255
	(1.187)	(0.645)	(0.529)	(0.455)	(0.514)
Controls	X	X	X	X	X
Fixed Effects:					
Group × Arrival Quarter	X	X	X	X	X
Census 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

Dependent Variable:	Any	Alarm	Violent	Property	Disturbance
	(1)	(2)	(3)	(4)	(5)
New Nbr Diff Race × Post	1.333*	1.139***	-0.113	-0.267	-0.188
	(0.783)	(0.417)	(0.307)	(0.320)	(0.378)
Post	0.254	0.122	-0.068	0.044	0.072
	(0.253)	(0.137)	(0.079)	(0.108)	(0.127)
New Nbr Diff Race	1.090	-0.328	0.788**	0.199	0.501
	(0.905)	(0.388)	(0.312)	(0.331)	(0.386)
Controls	X	X	X	X	X
Fixed Effects:					
Group × Arrival Quarter	X	X	X	X	X
Census Block	X	X	X	X	X
N	113,279	113,279	113,279	113,279	113,279
Dependent Variable Mean	18.40	3.88	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 neighbor within three doors. We consider five different outcome variables: calls for any reason (column 1), calls made by a home alarm system (column 2), calls reporting a violent crime (column 3), calls reporting a property crime (column 4), and calls reporting a disturbance (column 5). Each is an indicator variable (=100) if there is a 911 call directed to a geolocation for that reason within 25 meters of the property. The new neighbor is either the same race or a different race as the incumbent 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 new neighbor arrives and the four subsequent quarters. Control variables include home age, home size, resident age, and resident birth state. Census block group by new-neighbor arrival quarter fixed effects and separate block fixed effects are also included. Standard errors, adjusted for clustering at the block group-by-arrival year quarter level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Black Current Residents

Dependent Variable:	Any	Alarm	Violent	Property	Disturbance
	(1)	(2)	(3)	(4)	(5)
New Nbr Diff Race 1-Dr × Post	-0.948*	-0.443	-0.311	-0.503**	-0.134
	(0.562)	(0.304)	(0.234)	(0.248)	(0.279)
New Nbr Same Race 1-Dr × Post	-0.176	-0.007	-0.419	-0.096	-0.549*
	(0.590)	(0.318)	(0.282)	(0.278)	(0.317)
New Nbr Diff Race 2/3-Dr × Post	0.040	-0.165	0.179	-0.102	0.114
	(0.349)	(0.178)	(0.143)	(0.144)	(0.173)
New Nbr Diff Same 2/3-Dr × Post	0.147	-0.166	-0.498**	-0.213	0.520**
	(0.404)	(0.215)	(0.190)	(0.187)	(0.213)
Controls	X	X	X	X	X
Fixed Effects:					
Group × Arrival Quarter	X	X	X	X	X
Census Block	X	X	X	X	X
N	170,778	170,778	170,778	170,778	170,778
Dependent Variable Mean	26.73	5.11	3.75	3.87	5.30

Panel B. White Current Residents

Dependent Variable:	Any	Alarm	Violent	Property	Disturbance
	(1)	(2)	(3)	(4)	(5)
New Nbr Diff Race 1-Dr × Post	1.575**	1.239***	-0.180	-0.221	-0.119
	(0.737)	(0.391)	(0.295)	(0.302)	(0.352)
New Nbr Same Race 1-Dr × Post	0.262	0.120	-0.067	0.046	0.073
	(0.252)	(0.136)	(0.078)	(0.107)	(0.127)
New Nbr Diff Race 2/3-Dr × Post	0.113	-0.011	-0.065	-0.014	0.251
	(0.417)	(0.206)	(0.146)	(0.168)	(0.199)
New Nbr Diff Same 2/3-Dr × Post	-0.167	-0.162**	-0.014	0.025	0.025
	(0.130)	(0.065)	(0.039)	(0.053)	(0.063)
Controls	X	X	X	X	X
Fixed Effects:					
Group × Arrival Quarter	X	X	X	X	X
Census Block	X	X	X	X	X
N	450,598	450,598	450,598	450,598	450,598
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 911 calls split by various individual characteristics, including: whether the incumbent had previously experienced an alarm related 911 call; whether the incumbent sells their home in the following two years; or whether the new neighbor is younger than 40-years-old. The sample is similar than the one described in Table 1. The outcome variable is an indicator variable (=100) if there is an alarm-triggered 911 call spatially assigned to the property. The new neighbor is either the same race or a different race as the incumbent 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 new neighbor arrives and the four subsequent quarters. Control variables include home age, home size, resident age, and resident birth state. Census block group by new-neighbor arrival quarter fixed effects and separate block fixed effects are also included. Standard errors, adjusted for clustering at the block group-by-arrival year quarter level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Black Current Residents

Dependent Variable:	Home Alarm Call (=100)					
Subsample	No Previous Alarm	Previous Alarm	Does not Sell In Next 2 Yrs	Sells in Next 2 Yrs	New Neighbor Younger than 40	New Neighbor 40 or Older
	(1)	(2)	(3)	(4)	(5)	(6)
New Nbr Diff Race × Post	-1.723***	-1.364	-0.431	-0.820	-1.217*	0.805
	(0.342)	(1.218)	(0.454)	(1.774)	(0.635)	(0.719)
Controls	X	X	X	X	X	X
Fixed Effects:						
Group × Arrival Quarter	X	X	X	X	X	X
Census 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

Dependent Variable:	Alarm Call (=100)					
Subsample	No Previous Alarm	Previous Alarm	Does not Sell In Next 2 Yrs	Sells in Next 2 Yrs	New Neighbor Younger than 40	New Neighbor 40 or Older
	(1)	(2)	(3)	(4)	(5)	(6)
New Nbr Diff Race × Post	1.595***	-0.253	0.883**	3.036***	1.344**	0.822
	(0.321)	(1.648)	(0.444)	(1.119)	(0.611)	(0.548)
Controls	X	X	X	X	X	X
Fixed Effects:						
Group × Arrival Quarter	X	X	X	X	X	X
Census Block	X	X	X	X	X	X
N	99,964	13,315	100,098	13,181	64,599	42,654
Dependent Variable Mean	1.60	20.99	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

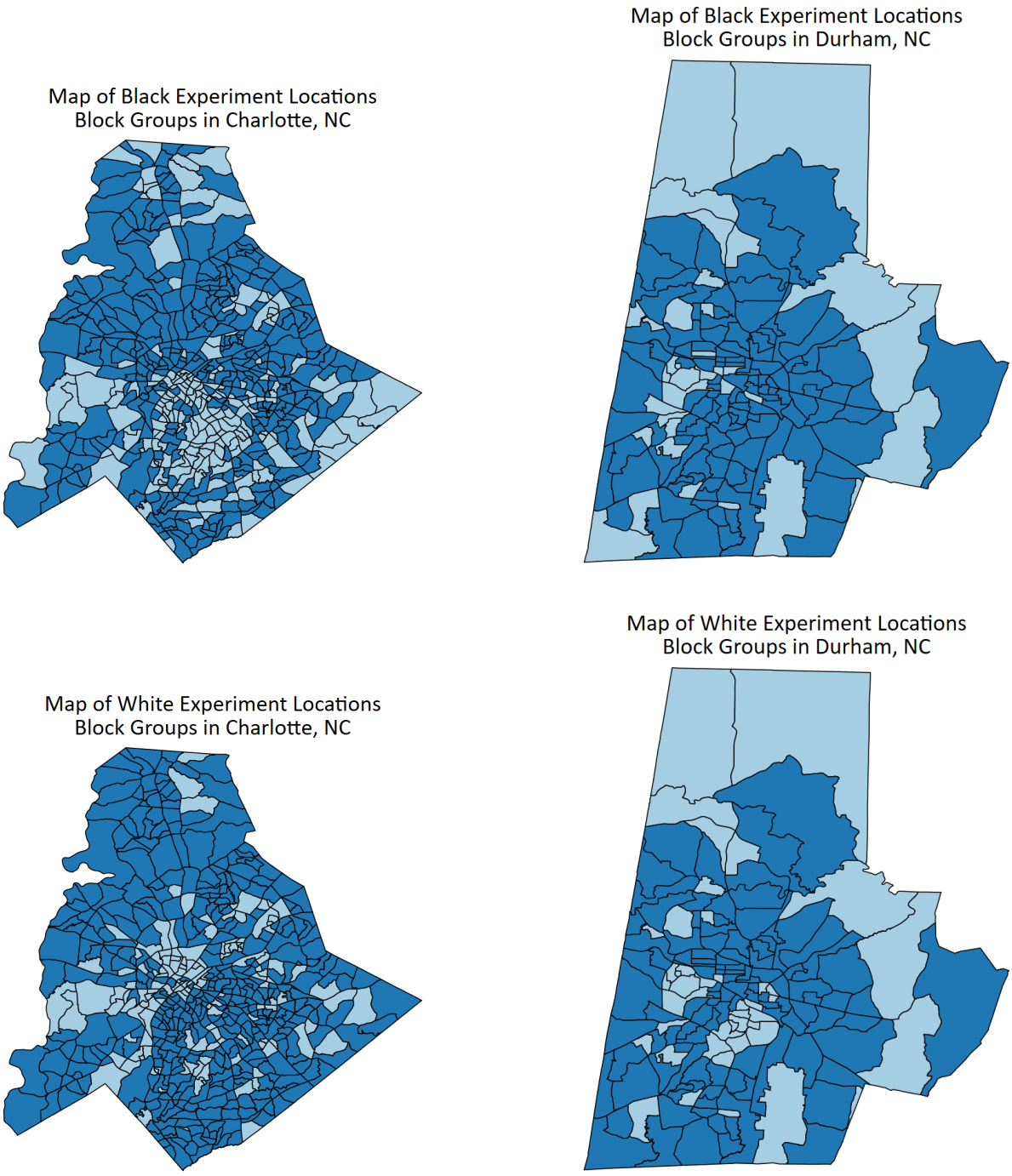


Figure A1: Geographic Coverage of the Sample

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

Table A1: 911 Call Shares

This table details the classification of the reasons for a 911 call in our data, along with the share of each type of call. This table is created using all 911 calls from the same set of block group and year quarters from Table 1

.

Violent	
Assault	10.53%
Domestic Disturbance/Violence	60.08%
Fight	5.78%
Gunshot	18.34%
Rape/Sexual Assault	1.40%
Robbery	3.61%
Stabbing	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 under the removal of different sample restrictions. The sample is similar to the one described in Table 1, except we remove a sample restriction in each column. The outcome variable is an indicator variable (=100) if there is an alarm-triggered 911 call directed to a given property. The new neighbor is either the same race or a different race as the incumbent 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 new neighbor arrives and the four subsequent quarters. Control variables include home age, home size, resident age, and resident birth state. Census block group by new-neighbor arrival quarter fixed effects and separate block fixed effects are also included. Standard errors, adjusted for clustering at the block group-by-arrival year quarter level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Black Current Residents

Dependent Variable:	Home Alarm (=100)			
Sample Change:	None	No Prop Char Reqs	No Density Reqs	No Max Nbr Dist
	(1)	(2)	(3)	(4)
New Nbr Diff Race × Post	-0.452	-0.468	-0.320	-0.354
	(0.440)	(0.440)	(0.391)	(0.308)
Controls	X	X	X	X
Fixed Effects:				
Group × Arrival Quarter	X	X	X	X
Census Block	X	X	X	X
N	53,172	53,235	68,048	85,325
Dependent Variable Mean	5.83	5.83	5.95	4.75

Panel B. White Current Residents

Dependent Variable:	Home Alarm (=100)			
Sample Change:	None	No Prop Char Reqs	No Density Reqs	No Max Nbr Dist
	(1)	(2)	(3)	(4)
New Nbr Diff Race × Post	1.139***	1.136***	0.997***	0.760***
	(0.417)	(0.417)	(0.371)	(0.243)
Controls	X	X	X	X
Fixed Effects:				
Group × Arrival Quarter	X	X	X	X
Census Block	X	X	X	X
N	113,279	113,698	146,166	253,035
Dependent Variable Mean	3.88	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. We consider five different outcome variables: any type of crime (column 1), violent crime (column 2), theft (column 3), vandalism (column 4), and burglary (column 5). Each is an indicator variable (=100) if there is a crime reported by the officer at a geolocation for that reason within 25 meters of the property. New Nbr Diff Race 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 new neighbor arrives and the four subsequent quarters. Control variables include home age, home size, resident age, and resident birth state. Census block group by new-neighbor arrival quarter fixed effects and separate block fixed effects are also included. Standard errors, adjusted for clustering at the block group-by-arrival year quarter level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Black Current Residents

Dependent Variable:	Any Crime	Violent	Theft	Vandalism	Burglary
	(1)	(2)	(3)	(4)	(5)
New Nbr Diff Race × Post	-0.184	-0.287*	0.092	-0.139	0.054
	(0.461)	(0.174)	(0.327)	(0.096)	(0.227)
Post	-0.268	0.033	-0.315	0.037	-0.322*
	(0.344)	(0.135)	(0.242)	(0.073)	(0.180)
New Nbr Diff Race	0.423	0.218	0.091	0.025	-0.201
	(0.566)	(0.222)	(0.382)	(0.110)	(0.287)
Controls	X	X	X	X	X
Fixed Effects:					
Group × Arrival Quarter	X	X	X	X	X
Census 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

Dependent Variable:	Any Crime	Violent	Theft	Vandalism	Burglary
	(1)	(2)	(3)	(4)	(5)
New Nbr Diff Race × Post	-0.383	-0.190	-0.276	-0.028	-0.137
	(0.407)	(0.152)	(0.303)	(0.062)	(0.208)
Post	0.104	-0.059	0.087	0.006	0.008
	(0.128)	(0.036)	(0.096)	(0.021)	(0.058)
New Nbr Diff Race	0.420	0.441***	0.300	-0.041	0.190
	(0.393)	(0.143)	(0.290)	(0.056)	(0.182)
Controls	X	X	X	X	X
Fixed Effects:					
Group × Arrival Quarter	X	X	X	X	X
Census Block	X	X	X	X	X
N	113,279	113,279	113,279	113,279	113,279
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 911 calls split by various neighborhood characteristics, including: block group income, block group Black share, and block group 911 call rate. The sample is similar than the one described in Table 1. The outcome variable is an indicator variable (=100) if there is an alarm-triggered 911 call assigned to a given property. The new neighbor is either the same race or a different race as the incumbent 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 new neighbor arrives and the four subsequent quarters. Control variables include home age, home size, resident age, and resident birth state. Census block group by new-neighbor arrival quarter fixed effects and separate block fixed effects are also included. Standard errors, adjusted for clustering at the block group-by-arrival year quarter level, are reported in parentheses. *, **, and *** denote statistical significance at the 10%, 5%, and 1% level, respectively.

Panel A. Black Current Residents

Dependent Variable:	Home Alarm Call (=100)					
Subsample	Low Block Group Income	High Block Group Income	Low Block Group Black Share	High Block Group Black Share	Low Block Group Call Rate	High Block Group Call Rate
	(1)	(2)	(3)	(4)	(5)	(6)
New Nbr Diff Race × Post	-0.526	-0.698	-3.177	-0.489	-0.209	-0.854
	(0.531)	(0.852)	(2.411)	(0.470)	(0.580)	(0.696)
Controls	X	X	X	X	X	X
Fixed Effects:						
Group × Arrival Quarter	X	X	X	X	X	X
Census 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

Dependent Variable:	Home Alarm Call (=100)					
Subsample	Low Block Group Income	High Block Group Income	Low Block Group Black Share	High Block Group Black Share	Low Block Group Call Rate	High Block Group Call Rate
	(1)	(2)	(3)	(4)	(5)	(6)
New Nbr Diff Race × Post	1.183**	1.166*	0.513	1.642***	1.635***	0.144
	(0.535)	(0.665)	(0.951)	(0.472)	(0.485)	(0.785)
Controls	X	X	X	X	X	X
Fixed Effects:						
Group × Arrival Quarter	X	X	X	X	X	X
Census Block	X	X	X	X	X	X
N	56,101	57,178	54,641	58,638	77,772	35,507
Dependent Variable Mean	3.74	4.01	4.05	3.72	3.44	4.82