## Title

Homophily Amplification: How Proximity and Guns Affect Racialized Residential Preferences


## Abstract

Racial residential segregation persists despite declining overt prejudice. Existing accounts explain this gap through direct racial preferences, discrimination, and neighborhood economic characteristics. We argue these accounts are incomplete. Theoretically, residential cues that increase uncertainty about neighborhood interactions by elevating their frequency or potential consequences are likely to amplify racial preference, and these effects are likely to vary by observer race. Using a set of novel neighborhood grid experiments with a national probability sample (N = 2,176), we test how proximity and gun ownership amplify racial preferences. The findings show that Americans prefer multiracial neighborhoods but favor White neighbors. Proximity amplifies homophily preferences: participants cluster same-race neighbors at adjacent houses, and non-Black participants place Black neighbors at the neighborhood periphery. Gun ownership cues also magnify racial homophily: participants assign guns preferentially to same-race neighbors and assign same-race neighbors to armed houses, although the penalty on Black gun ownership reverses between White and Black participants. These findings indicate that Americans' comfort in multiracial neighborhoods is spatially structured in ways standard preference measures do not capture.

## Introduction

For three-quarters of a century, sociologists have used the neighbor to measure the racial boundaries Americans draw around where they live (Bogardus 1947; Charles 2000; Krysan and Farley 2002). The ordering the measure returns is stable: a neighborhood racial hierarchy in which Americans, particularly White Americans, rate White neighbors and neighborhoods as more desirable than Black ones (Farley et al. 1978), a preference that holds when crime, school quality, and housing values are fixed (Emerson, Yancey, and Chai 2001; Lewis, Emerson, and Klineberg 2011), and even persists after the socioeconomic differences that race is presumed to proxy are stripped away (Harris 1999). While overtly reported prejudice has declined, the formal legal machinery of residential exclusion has weakened, and Black-White segregation has eased slowly (Logan and Stults 2022; Pager and Shepherd 2008), the racial hierarchy has scarcely changed, and racial preferences continue to structure neighbor choice (Charles 2003; Krysan and Crowder 2017).

The current paper argues that the hierarchy's durability is better understood through *homophily amplification mechanisms* than through stable preferences. Specifically, focusing on uncertainty reduction, we theorize the conditions under which a routine preference for similar others becomes a stronger one. People prefer the predictable in their dealings with others, and uncertainty should heighten the appeal of the familiar and the similar (Hogg 2007). We argue that prospective residents treat race as a proxy for predictability; a neighbor of one's own race is presumed to be more likely to share a language, a routine, and norms about social behavior (e.g., about how a street is kept and disagreements are settled) (McPherson, Smith-Lovin, and Cook 2001). Race may be less the object of residential judgment than a hedge against the cost of

misjudging a neighbor. This situates durable preference within a broader family of amplification accounts, in which modest discrepancies in preference and advantage compound into large aggregate disparities (DiMaggio and Garip 2012; DiPrete and Eirich 2006; Schelling 1971), rather than within models that infer residential behavior from stated racial attitudes (e.g., Charles 2000).

Although there is growing evidence within the residential-preferences literature that racial preferences are conditional (Charles 2003; Krysan, Couper, Farley, and Forman 2009; Lewis, Emerson, and Klineberg 2011), the theoretical mechanisms giving rise to such moderation have received scant attention. Additionally, almost all prior studies of moderation have been observational; they establish that preferences are ordered, but not which cues condition them. The few experiments that have been conducted focused on third-party evaluations of neighborhoods—that is, neighborhood designs where the participants were asked to evaluate neighborhoods as outsiders rather than as residents (e.g., Emerson, Yancey, and Chai, 2001; Krysan et al., 2009). Unless participants have a residential stake in the neighborhood (real or hypothetical), the distance between self and neighbor—the original quantity of social distance theory (Bogardus 1947)—goes unmeasured.

To the extent that racial-preference moderation has received theoretical attention in previous studies, the focus has been limited to identifying conditions under which a specific out-group, most often Black Americans, becomes more threatening or less desirable (Blalock 1967; Quillian and Pager 2001). However, such threat-focused theoretical scholarship does not anticipate conditions that intensify every group's preference for its own members. In this paper, we broaden the theoretical lens to two such channels of homophily amplification, physical proximity and consequence. The first, proximity, operates through the *frequency* of interaction.

A neighbor across the yard is encountered more often than one at the end of the block, and each encounter is another occasion of uncertainty—one where a mismatch in habit or expectation may be consequential—and therefore the value of perceived similarity should rise as distance falls (Anicich et al. 2021). Theoretically, physical proximity should intensify same-race preference for every racial/ethnic group of respondents.

The second channel for homophily amplification, consequence, relates to factors that affect uncertainty about the potential severity of interactional outcomes. Although overlooked in past work, gun ownership is a particularly important factor that should affect homophily preferences through the *consequence* of interaction. An armed neighbor's capacity to do harm raises the stakes of any single encounter and may reduce willingness to socialize (Sola and Pickett 2024). However, the same firearm may mark a citizen-protector to one observer and a threat to another (Carlson 2015), and such perceptions may be racialized (Hayes et al. 2021; Higginbotham et al. 2023). Therefore, unlike the proximity channel for homophily amplification, gun effects may fall along racial lines, such that the same gun may be perceived as safe in White hands but threatening in Black ones (Carlson 2015, 2019; Drakulich and Craig 2024).

We test these claims with a nationally representative AmeriSpeak sample of 2,176 U.S. adults and a unique survey-experimental design. Building on the neighborhood showcards used by Farley et al. (1978), Charles (2000), and the 2000 GSS (Smith 2001), we implement an interactive grid that records both neighborhood composition and the location of each household selection, relative to the participant's home. Each participant composed a preferred neighborhood on a 5×3 grid arrayed around their own household, assigning either a race or a gun status to each of 14 neighbors. This design better taps into the self-neighbor distance at the heart of social distance theory, as it varies the distance of neighbors from the participant's home.

By adding experimental cues about the attributes of the neighbor and neighborhood, our design extends the composition preferences that showcard studies have long measured (Charles 2000; Farley et al. 1978; Smith 2001). In particular, the experimental design allows us to estimate how a second cue (e.g., gun ownership) interacts with race within a single judgment. Importantly, the design measures not only which neighbors are preferred but where they are placed in the neighborhood. In this way, the grid experiment externalizes the evaluative work that precedes residential action (Bell 2020), measuring preferred neighborhood structure rather than realized mobility. Preferred structure is arguably more theoretically germane than mobility, as the latter depends on housing opportunities, not just preference, and because Americans have grown less likely to move (Huang and Butts 2023). Where standard accounts treat preference as a disposition, we treat its expression as conditional on the uncertainty of interaction. The contribution is that uncertainty-fueled homophily amplification mechanisms can explain how taste for integration coexists with active quarantining of outgroups in American neighborhoods.

## Theory and Literature

### 1. Neighbor Preferences: Homophily and Integration

Studying residential preferences matters because they aggregate (Schelling 1971) and may help explain racial segregation in the United States (Clark 1991). When Bruch and Mare (2006) estimated preference curves directly from survey responses, the exchange that followed showed that segregation depends on how strongly residential choices depart from chance and on the group-specific racial preferences (see Van de Rijt, Siegel, and Macy 2009; Bruch and Mare 2009:1193). The housing search process is itself racially structured: networks, routines, and knowledge restrict which neighborhoods are considered (Krysan and Crowder 2017), while

stereotypes and discrimination compound across the linked stages of housing exchange (Korver-Glenn 2018). Online information now structures this search as well; small amounts of ostensibly race-neutral listing information can change neighborhood evaluations and reproduce racial-spatial ordering (Besbris, Schachter, and Kuk 2026). Observed homophily can therefore reflect opportunity structure as well as interpersonal choice (McPherson, Smith-Lovin, and Cook 2001; Smith, McPherson, and Smith-Lovin 2014). As Americans grow less mobile, intercounty migration has become both less frequent and more segmented across counties with dissimilar racial, political, and urban contexts (Huang and Butts 2023), so ingroup preference further constrains a narrowed set of choices.

Sociological scholarship indicates that people prefer neighbors who resemble them. There are two primary theoretical accounts as to why. Social-distance research begins from the premise that people vary systematically in the "degrees of intimacy" they will accept in social relations (Park 1924:340). Race, ethnicity, and class shape whether respondents will accept members of a group as coworkers, neighbors, or relatives by marriage (Bogardus 1925a, 1925b). Bogardus (1947:306) understood acceptance and exclusion as satisfying an "urge for security," but distance was measured relationally rather than spatially. Repeated U.S. replications with college-student samples found declining social distance through the early 2000s but a partial reversal by 2011, underscoring the value of contemporary measurement beyond student samples (Parrillo and Donoghue 2005; 2013). Nationally representative evidence is largely limited to the 2000 General Social Survey (GSS). Presented with fixed racial compositions, acceptance varied by both the target and respondent group (Smith 2001; Weaver 2008).

Homophily research accounts for similar findings by arguing that social ties require a common basis of understanding (Lazarsfeld and Merton 1954). Because this common basis

"breeds connection" (McPherson, Smith-Lovin, and Cook 2001:415) and race and ethnicity are among the sharpest discontinuities in U.S. networks, race inflects the formation of connections. Both social distance and homophily research therefore predict that Americans will prefer a plurality of same-race neighbors, but not demand monochromaticity.

Prior showcard studies, which elicited the desirability of different racial compositions or asked respondents to compose preferred neighborhoods, also generally support the same-race core of this prediction, despite having substantial limitations. In particular, they have relied on samples that are decades old and usually drawn from single cities. The first study, Farley et al. (1978), was conducted in 1976 in Detroit, Michigan. They converted attitude items into structured choices using fifteen-house showcards, and found that White discomfort rose sharply with the depicted Black presence, while Black respondents generally preferred mixed areas but anticipated White hostility in all-White ones. Of course, Farley et al.'s (1978) focus on Black-White differences in a specific year and location raises questions about the generalizability of the findings across place, time, and racial/ethnic groups.

Charles (2000) extended the design to a multiethnic ideal-neighborhood task with a 1990s Los Angeles sample. Every group reserved the largest single share for its own race while generally constructing an integrated neighborhood. In-group attachment had little to do with willingness to live with other groups; out-group affect, stereotypes, and, among White respondents, a sense of group position did (Bobo and Zubrinsky 1996). For Black respondents, however, preference for some Black presence was closely tied to anticipated White hostility and discrimination, not simply neutral ingroup attraction (Charles 2003; Krysan and Farley 2002). In other words, Black participants' concerns about White racial hostility increased uncertainty about interactions with White neighbors. An analogous task appeared in the 2000 GSS, where

each group again leaned toward an own-group plurality within an integrated neighborhood (Smith 2001). White respondents showed the strongest same-race preference in both studies; the relative ordering among Black, Hispanic, and Asian respondents varied.

In short, several regularities have emerged in past studies. First, respondents generally reserved the largest single neighborhood share for their own group. Second, their ideal neighborhoods were usually integrated rather than homogeneous. Third, racial composition influenced evaluations even when class, crime, and other neighborhood-quality cues were held constant (Emerson, Yancey, and Chai 2001; Krysan et al. 2009; Lewis, Emerson, and Klineberg 2011). Unfortunately, the extant evidence is city-specific and/or dated. New research is especially important as some scholarship indicates that Barack Obama's presidency may have changed racial attitudes in America (Goldman and Mutz, 2014; Welch and Sigelman, 2011). To advance the literature on residential preferences, we use a contemporary, nationally representative sample to test several preregistered hypotheses. The first two follow:

> **Hypothesis 1 (Racial Homophily):** Participants will allocate the largest share of neighborhood positions to their own racial group. Same-race neighbors will also make a neighborhood more desirable. White participants will exhibit the strongest same-race preference.
>
> **Hypothesis 2 (Integration):** Participants of all races/ethnicities will prefer some integration to homogeneity.

## 2. Neighbor Preferences: Between Outgroups

The sociological literature discussed above predicts that people will prefer ingroup neighbors (i.e., those who share their race/ethnicity). It does not say whether racial/ethnic

outgroups are interchangeable. In their residential preferences, will Americans treat all outgroups alike or distinguish between them? U.S. racial stratification privileges Whiteness and disadvantages Blackness, while assigning Asian and Hispanic/Latino groups more conditional positions along dimensions of status and belonging (Du Bois 1899; Kim 1999; Bonilla-Silva 2004; Zou and Cheryan 2017).[1] At the neighbor level, the implication is that a racial hierarchy of preference among outgroups may exist and be layered on top of the preference for racial homophily described in H1.

Indeed, Wilkerson (2020, p. 49-50) documents the historical roots of the racial hierarchy. She explains that to get by historically, non-Black minorities have situated themselves above the existing color (or caste) line:

> Thus, each new immigrant—the ancestors of most current-day Americans—walked into a preexisting hierarchy, bipolar in construction, arising from slavery and pitting the extremes in human pigmentation at opposite ends. Each new immigrant had to figure out how and where to position themselves in the hierarchy of their adopted new land ... To gain acceptance, each fresh infusion of immigrants had to enter into a silent, unspoken pact of separating and distancing themselves from the established lowest caste.

Once a racial hierarchy is internalized, a group's position may organize its reactions to outgroups (Blumer 1958; Vogel and Messner 2024). Prejudice is one such "defensive" reaction to positional evaluations: groups defend claims to status and resources, and therefore evaluate other groups through the lens of their own perceived location in a multiracial order (Blumer 1958; Bobo and Hutchings 1996). Consequently, perceived threat may rise with an outgroup's

[1] Du Bois locates the lack of labor opportunities for Black workers in both prejudice and resource threat (1899:129): "To repeat, then, the real motives back of this exclusion are plain: a large part is simple race prejudice, always strong in working classes and intensified by the peculiar history of the Negro in this country. Another part, however, and possibly a more potent part, is the natural spirit of monopoly and the desire to keep up wages. So long as a cry against "Irish" or "foreigners" was able to marshal race prejudice in the service of those who desired to keep those people out of some employments, that cry was sedulously used. So today the workmen plainly see that a large amount of competition can be shut off by taking advantage of public opinion and drawing the color line."

relative size or mobilization (Blalock 1967; Taylor, 1998), and associated perceptions may persist as status beliefs that rank groups asymmetrically (Bobo 1999).

Residential and social-distance measures reveal both common ordering and rater-specific variation. Overall, White households received the largest outgroup share from Black, Hispanic, and Asian respondents in the 2000 GSS allocation task (Smith 2001). However, while ingroup preference was constant, outgroup ordering varied, ruling out a single racial preference hierarchy shared by all Americans (Weaver 2008). For example, Latino immigrants in Durham expressed greater social distance from Black Americans and perceived greater similarity to White Americans (McClain et al. 2006). Outgroups, as they are subject to the rater's own position as well as a racial hierarchy, are unlikely to be interchangeable or ranked identically.

Socioeconomic advantages accrue to those who distance themselves from Blackness. For example, Asian Americans are relatively valorized over Black Americans even as they are ostracized as culturally foreign (Kim 1999; Zou and Cheryan 2017). Asian Americans' position therefore better fits a multidimensional account than a singular rank order (Xu and Lee 2013). As a result, a contested racial stratum emerges between the White and Black poles (Bonilla-Silva 2004; see also Wilkerson 2020).

By extension, the residential-preferences translation of structural theories indicates that racial and ethnic minorities are sorted by place according to their group's relative standing in society, limiting even socially mobile members' access to the neighborhoods of comparable Whites, creating countervailing preferences (Alba and Logan 1993; Charles 2003).[2] At the level of stated residential preference, Charles (2000:386-87; 2003:185) found that White households were the most included outgroup among non-White respondents, while Black households were

[2] Scholars' accounts of the mechanism between structural theories of race and segregated outcomes are often direct: "Whites use segregation to maintain social distance" (Charles 2003:182).

most often excluded; Asian and Latino placement varied by respondent group. These studies demonstrate that Americans overlay hierarchy on racial homophily, but do not imply that every respondent group will reproduce the same four-group ladder. We therefore test the following aggregate expectation:

> **Hypothesis 3:** Americans will exhibit a racial hierarchy beyond same-group homophily (H1) and the preference for some integration (H2). Across respondent groups, White and Asian households will receive a larger combined share of neighborhood positions than Black and Hispanic households.[3]

## 3. Neighbor Preferences: Proximity Effects

Despite decades of research on residential preferences, little work examines *where* respondents prefer different neighbors to be located. Specifically, past research has focused on overall compositional preferences, or the preferred racial/ethnic composition of one's neighborhood. Yet, proximity preferences—that is, preferences for where in one's neighborhood neighbors of different racial/ethnic backgrounds are located—are equally important. Spatial arrangement can reproduce segregation within nominally mixed areas. Grigoryeva and Ruef (2015) show that household-level sequences reveal micro-segregation hidden by larger units, while Grannis (1998) shows that pedestrian street connections organize racial similarity more strongly than simple geographic distance. These studies establish why placement and adjacency matter, but concern realized patterns rather than preference structures. Instead, we ask whether participants reproduce micro-segregation in their preferences.

[3] In the 2000 GSS Asian households were overselected relative to their population share (Smith 2001). H3 is therefore a pooled target contrast rather than a claim that Asian households rank second for every respondent group.

Proximity should matter for many reasons, including anticipation of contact. To begin, the term "neighborhood" is vague and varies in its meaning, such that defining it is "a bedeviling challenge" for scholars (Hipp and Boessen 2013: 287). Because a "neighborhood" is a socio-psychological construct, individuals' conceptualizations of its boundaries will differ (Siordia and Coulton 2015; von Stülpnagel et al. 2019). Consequently, their likelihood of defining someone as a "neighbor" as well as their expected frequency of interacting with them are likely to increase with proximity. Anticipated future interactions, in turn, are likely to increase uncertainty about interactional outcomes. Importantly, uncertainty can heighten identification with similar groups (Hogg 2000; 2007), giving racial commonality additional value. As people are likely to expect more frequent interactions with the households nearest to their own, they may place greater emphasis on satisfying the previously discussed (H1) "urge for security" (Bogardus 1947:306) that racial similarity may provide.

Put simply, adjacent neighbors are encountered more often than neighbors at the end of the block, and each encounter adds another occasion on which a mismatch in norms, habit, or expectation may matter. Although nearby households create more opportunities for routine interaction (Festinger, Schachter, and Back [1950] 1963), which should reduce uncertainty over time, evidence about the effects of *experienced* contact does not fix the direction of *prospective* preference. Intergroup contact under ideal conditions reduces prejudice (Pettigrew and Tropp 2006), but brief, nonvoluntary exposure can increase exclusionary attitudes (Enos 2014). As importantly, intergroup contact studies ask how contact or exposure changes attitudes *after* it occurs. A separate question, and one that is the focus of our experiment, is: how do people from different racial/ethnic backgrounds contemplate, a priori, the increased interaction borne by

proximity and arrange prospective neighbors accordingly? This is an important question because such residential preferences may foreclose opportunities for intergroup contact.

Anicich et al. (2021) provide the closest prior spatial experiment. White participants concentrated White residents around self-relevant locations in a fictional city—such as workplaces or schools where they expected to spend more time—thereby reducing anticipated incidental contact with non-White residents. In contrast, in a set of Dutch probability-panel conjoint experiments (Dederichs et al. 2025), respondents exhibited ingroup sport club preferences, but those preferences did not strengthen as group size decreased. Prior experimental evidence therefore makes proximity amplification plausible but contested: Anicich et al. (2021) document anticipatory spatial sorting, whereas Dederichs et al. (2025) find that expected interaction intensity does not affect ingroup preference. However, neither experiment varied interaction intensity within a neighborhood: Anicich et al. used locations in a fictional city, and Dederichs et al. tested intensity only for sports clubs.

Theoretically, proximity may produce two complementary patterns: own-group neighbors may be placed nearby, whereas especially dispreferred neighbors, if included at all, may be pushed to the most distant points. The grid nature of our experimental design allows us to test both a near to far homophily gradient (moderating H1) and whether there exists a stronger form of exclusion we call quarantining, in which less-preferred attributes are pushed disproportionately to the furthest corner of the neighborhood, where interactions are least likely (extending H3). Such proximity preferences are important, because they may contribute to micro-segregation and accumulate into macro-segregation (Schelling 1971; Bruch and Mare 2009), and because preferred structure grows more consequential as Americans grow less mobile (Huang and Butts 2023). Therefore, the fourth hypothesis that we test is as follows:

**Hypothesis 4:** Participants will prefer to have same-race neighbors nearby and will quarantine less-preferred racial/ethnic groups to the most distant parts of their neighborhood; distance will thereby moderate (dis)preference.

## 4. Neighbor Preferences: Consequence Effects

In the National Book Award-winning novel *Hell of a Book*, author Jason Mott (2021, p. 264) explained why uncertainty about interactional outcomes increases when a neighbor owns guns: "Guns are like pets. Even if you don't own one, it's only a matter of time before your neighbor, friendly or unfriendly, brings one into your life and you have to cross your fingers and hope it's friendly." Theoretically, then, whereas distance governs how often neighbors are encountered, arms govern how consequential encounters with them may be. Indeed, many Americans want the right to know whether their neighbors own guns (Burton et al., 2025). This makes sense, as an armed neighbor's capacity to do harm raises the potential consequence of interaction even when the probability of contact is unchanged. Prior experiments establish that gun cues matter to neighbor choice: Sola and Pickett (2024) find broad aversion to AR-15 ownership and insecure gun storage, but do not test whether those cues are interpreted differently by the neighbor's race.

However, the meaning of gun ownership may be both receiver-dependent and racialized (Higginbotham et al. 2023; Schutten et al. 2022). Hayes et al. (2021, p. 829) find that "many white respondents are much less supportive of the ability of African-Americans to purchase handguns than they are of white Americans." Mencken and Froese (2019) show that firearms anchor identity and empowerment for a subset of owners, especially economically distressed White men. Carlson (2019) shows that police chiefs distinguish criminal gun activity associated

with Black and Brown communities from lawful gun ownership often marked as White and middle class. Drakulich and Craig (2024) connect intersectional threat to gun-policy attitudes. These studies make heterogeneous interpretation plausible, although none directly shows that ordinary respondents evaluate the same armed neighbor differently by race.

Research on neighborhood disorder shows that racial context shapes how ostensibly objective cues (e.g., litter) are perceived (Drakulich 2013; Sampson and Raudenbush 2004). However, guns differ because their valence need not be uniformly negative. Depending on the owner, a firearm may mark a citizen-protector to one observer and a menace to another (Carlson 2015, 2019; Sola and Lee 2026), a duality as old as armed Black self-defense and the reaction it provoked (Williams 1962); as Simon (2010) details, the postwar suburb marketed security through separation, and gun ownership is an extension of this project. The same firearm may therefore lead one neighbor to be perceived as desirable and another as undesirable.

There is overwhelming evidence that Black Americans are especially likely to be stereotyped as prone to violence and criminality (Pickett et al. 2012)—a stereotype that guns are likely to activate (Ellyson et al., 2023; Oh et al., 2025). Indeed, past experiments show that racial and weapon cues are not processed independently (Todd et al., 2021). Black facial primes increase the speed with which participants identify weapons and, under time pressure, increase weapon-identification errors (Payne 2001). In shoot/don't-shoot simulations, participants respond more quickly to armed Black targets and more quickly withhold fire from unarmed White targets (Correll et al. 2002). Related experiments show reciprocal associations between Blackness and crime-relevant objects more generally (Eberhardt et al. 2004). These studies establish a basis for testing whether gun cues are interpreted differently depending on neighbor

race/ethnicity, and specifically on whether neighbors are Black. Thus, the final hypotheses that we test are as follows:

**Hypothesis 5:** Participants will be less likely to arm racially dissimilar neighbors.

**Hypothesis 6:** Participants will be less likely to arm Black neighbors, regardless of their own race/ethnicity.

## Data and Methods

### 1. Sample

Through the Time-Sharing Experiments for the Social Sciences (TESS) program, we conducted an online survey experiment with a national probability sample of U.S. adults drawn from NORC at the University of Chicago's AmeriSpeak panel. AmeriSpeak recruits households using random address-based sampling and then has them complete computerized questionnaires. This ensures representativeness while allowing for self-administered surveys, yielding "the optimal combination of sample composition accuracy and self-report accuracy" (Chang and Krosnick 2009: 641; see also Yeager et al. 2011). Computerized self-administration not only reduces social desirability bias (Gnambs and Kaspar 2015), but also allows for complex experimental designs with visual and interactive components (Tourangeau et al., 2013). NORC's AmeriSpeak panel is a preferred source of survey data for sociological research (e.g., Barnes, Johfre, and Munsch 2024; Grigoryeva and Robbins, 2024; Kucinskas and Stewart 2022; Small, Brant, and Fekete 2024).

The *Anonymized* Institutional Review Board approved the survey (IRB #24-2973) before it was fielded (between July 25 and August 11, 2025). NORC invited 11,275 panelists; 2,807 began the survey and 2,720 completed it, for a completion rate of 24%. This rate falls within the

normal range for AmeriSpeak surveys (e.g., 26% in Grigoryeva and Robbins 2024; 28% in Kucinskas and Stewart 2022). Because of our experimental design, nonresponse bias is not a threat to the internal validity of findings; it is also unlikely to threaten external validity (Mullinix et al. 2015; Peyton et al. 2022), which would require not just systematic differences between responders and nonresponders, but meaningful interaction effects between those differences and the causal effects of randomized treatments (Coppock et al. 2018; Mize and Manago 2022). Additionally, nearly all the participants (97%) who began the survey (N = 2,807) finished it, meaning that most nonresponse decisions occurred prior to exposure to survey content, and so cannot reflect motivated abstention from racially sensitive questions (Berinsky 1999).

After response-quality checks (speed and item-skipping review), NORC provided a covariate-linked sample of 2,176 participants (Appendix C). Final survey weights account for panel and study selection, adjust for survey nonresponse, and rake the sample to February 2025 Current Population Survey benchmarks. The weighted sample has a mean age of 48.0 years and is 51.3 percent female. It is 59.9 percent non-Hispanic White, 18.4 percent Hispanic, 12.1 percent non-Hispanic Black, 5.5 percent non-Hispanic Asian or Pacific Islander, and 4.2 percent multiracial or another race; 69.4 percent live in owner-occupied housing. Appendix D reports complete weighted and unweighted sample characteristics.

## 2. Design

Our experimental design built on Charles's (2000) showcard study, using a 5×3 grid of houses with the participant's home at the center. Before fielding the TESS study, we piloted the grid with 500 U.S. adults recruited through YouGov. The pilot assessed whether the task functioned across devices, whether nonacademic respondents understood the response options,

and whether the outcomes displayed sufficient variation. The pilot produced only minor design revisions. We then preregistered the study's design, hypotheses, and analyses on the Open Science Framework under the TESS protocol.[4] Appendix A reports the associated power analyses.

In each task, "Your House" occupied the center of the grid and was surrounded by 14 neighboring houses. Participants were instructed that "you will see several houses around **Your House**. Click or tap on those houses to create the neighborhood you would feel most comfortable living in."

Phone users were also instructed to use landscape mode or scroll left and right to view the full neighborhood. Extant evidence generally indicates that surveys taken on smartphones and computers yield similar data quality (Antoun et al., 2017). In our experiment, clicking or tapping a house opened a dropdown menu; once participants chose an option from the dropdown menu, the label they selected appeared inside the house. Participants completed the baseline tasks in randomized order and then the conditional tasks in randomized order. The ordering of response options was also randomized for each task. Participants completed demographic questions after the experimental tasks (Appendix B).

Our experimental grid design extends prior showcard measures by recording both neighborhood composition and the spatial placement of each selected household. It therefore measures not only whom participants select as neighbors, but also where they place them—a dimension absent from prior showcard analyses (Charles 2000; Farley et al. 1978). Manhattan

[4] TESS application preregistration: osf.io/fxyae/overview?view_only=1ddaa7f8d54348349e8b3ffb481f6de1 Further review and focus on racial homophily and proximity effects, rather than on the political and (direct) gun effects, led to the addition of two new hypotheses, and the rewording of existing hypotheses without substantive change in their content. There are no substantial deviations to report from the updated preregistration: https://osf.io/5u8fk/overview?view_only=9643302ca6c643b2b1fa2b7ee82e0bc9

distance from "Your House" divides the surrounding positions into close, middle, and far bands, corresponding to distances of one, two, and three grid steps (Figure 1). This allows us to test whether same-race preference intensifies with proximity and whether less-preferred groups are disproportionately placed at the neighborhood's farthest locations (see Grannis 1998).[5]

*[Figure 1 about here]*

Next, cue-conditioned tasks add reciprocal tests of how race and gun ownership shape neighborhood assignments. Randomly assigned racial labels allow us to test whom participants are willing to arm, while randomly assigned gun-ownership statuses allow us to test which racial groups participants place in armed houses.

In the baseline racial-composition task, participants selected White, Black, Hispanic, or Asian labels for the 14 surrounding houses without any reference to guns. The task screen read:

> Imagine your IDEAL NEIGHBORHOOD in terms of RACE/ETHNICITY. Create the neighborhood you would feel most comfortable living in:

Each house's dropdown menu offered participants four racial/ethnic categories: White, Black, Hispanic, and Asian. Participants' selections provide the preferred racial composition, integration, hierarchy, and placement measures used to test H1-H4.[6]

Next, in the race-cue gun-assignment task, we randomly assigned racial labels to the houses surrounding the participant's home under one of four equally likely racial-prevalence scenarios: nearly all White, majority White, majority Black, or majority Hispanic.[7] Each house's racial label was visible while participants selected one of four gun-ownership statuses (No Gun,

[5] We test an inner-versus-outer column distance coding (Appendix E: Tables E4 and E5) and find similar results.
[6] Participants also completed baseline gun and political tasks that are not analyzed here.
[7] Neighborhood per-house racial prevalence scenarios (equal likelihood of selection):

- Nearly all White condition: 97% White, 1% Black, 1% Hispanic, 1% Asian. In this scenario the neighborhood has a 65% (0.97^14) probability of being entirely White.
- Majority White condition: 70% White, 10% Black, 10% Hispanic, 10% Asian.
- Majority Black condition: 10% White, 70% Black, 10% Hispanic, 10% Asian.
- Majority Hispanic condition: 10% White, 10% Black, 70% Hispanic, 10% Asian.

Hunting Rifle, Pistol, or Pistol & AR-15) for the household. One concern was that participants might infer political differences from the racial character of neighborhoods. Indeed, Americans prefer co-partisan neighbors and infer their neighbors' politics from race (Gimpel and Hui 2015: 139), though effects are limited (Mummolo and Nall 2017). To separate political and racial cues (Dafoe et al. 2018), we randomly varied the neighborhood's political status via a textual description of yard signs or flags.[8] These cues appeared as text above the grid rather than as signs or flags attached to particular houses.

The gun-cue racial-assignment task reversed these roles. We randomly assigned gun-ownership statuses to surrounding houses under one of four equally likely prevalence scenarios ranging from nearly no guns to majority gun ownership.[9] Participants then selected White, Black, Hispanic, or Asian labels for the houses using the same dropdown interface. The task used the same randomized political conditions as the race-cue gun-assignment task.

After each cue-conditioned assignment task, participants advanced to a neighborhood desirability scale ranging from *Very Undesirable* to *Very Desirable* (coded from 1 to 7). The left-to-right ordering of the endpoints was randomly reversed between participants. The item was adapted from the neighborhood-desirability measure used in the 2004 Detroit Area Study

[8] Neighborhood political scenarios (equal likelihood of selection):

- Republican condition: "From the yard signs and flags hanging in the neighborhood, your neighbors appear to be REPUBLICANS."
- Democrat condition: "From the yard signs and flags hanging in the neighborhood, your neighbors appear to be DEMOCRATS."
- No-signs condition: "Your neighbors do not have any political yard signs or flags."
- Control: No political information provided.

[9] Neighborhood per-house gun ownership scenarios (equal likelihood of selection):

1. Nearly no guns condition: 97% No Gun, 1% Pistol & AR-15, 1% Hunting Rifle, 1% Pistol. In this scenario the neighborhood has a 65% (0.97^14) probability of having no guns.
2. Low gun ownership condition: 82% No Gun, 6% Pistol & AR-15, 6% Hunting Rifle, 6% Pistol.
3. Moderate gun ownership condition: 64% No Gun, 12% Pistol & AR-15, 12% Hunting Rifle, 12% Pistol.
4. Majority gun ownership condition: 34% No Gun, 22% Pistol & AR-15, 22% Hunting Rifle, 22% Pistol.

(Krysan et al. 2009; see also Farley et al. 2016). The prompt read, "Overall, how would you rate this neighborhood as a place to live?"[10]

These tasks measure preferred neighborhood composition and spatial placement when race, gun ownership, and neighborhood politics are explicit. They therefore identify responses to legible cues within a hypothetical neighborhood, not the process through which prospective residents infer those attributes or translate preferences into realized residential mobility.

*[Figure 2 about here]*

## 3. Measures and Analytic Strategy

We estimate binary house-level outcomes with linear probability models, per recommendations for experimental data (Gomila, 2021; Huang, 2022),[11] and the seven-point neighborhood-desirability rating with ordinary least squares. House-level models contain repeated observations from each participant and therefore use Bell-McCaffrey CR2 cluster-robust standard errors clustered by participant (Abadie et al. 2023); participant-level models use HC2 heteroskedasticity-robust standard errors (Bell and McCaffrey 2002; MacKinnon and White 1985; Pustejovsky and Tipton 2018). LPM coefficients are multiplied by 100 to be directly interpretable as percentage-point differences in selection probabilities.

The descriptive racial-composition estimates and composition-based tests of H1-H3 are weighted, as are the sample characteristics. All other analyses are unweighted to recover the sample-average treatment effects as survey-experimental guidance recommends (Franco et al.,

[10] The original measure asked participants to "rate the neighborhood you just saw" after viewing a video (Krysan et al. 2009). Because participants in our study viewed an interactive grid, we replaced "the neighborhood you just saw" with "this neighborhood."

[11] Logit refits of the adjusted models in Tables 2, 4, and 5 yield average probability contrasts within 0.1 percentage points of the LPM estimates. An ordered-logit refit of Table 1 yields a similar change in expected desirability (0.026 versus 0.030 points). These checks change no inferences.

2017; Miratrix et al. 2018). We report unadjusted and adjusted estimates (Lin, 2013). When doing so, Model 1 is unadjusted and Model 2 adjusts for co-occurring experimental conditions (where applicable) and for participant covariates including age in years, household size, 2024 presidential turnout, household income, education, party identification, ideology, housing tenure, race,[12] sex, religion, home type, Census division, metropolitan status, internet access, media trust, and religious attendance. Ordinal covariates—household income, education, party identification, ideology, media trust, and religious attendance—enter as integer scores in scale order; the remaining categorical covariates enter as unordered factors.

For analyses of randomly assigned racial and gun cues, Model 1 estimates the unadjusted experimental contrast, while Model 2 absorbs residual variance and accounts for chance covariate imbalance without changing the source of identification (Gerber and Green 2012; Lin 2013). For analyses of participants' own racial assignments, the same specifications instead provide unadjusted and covariate-adjusted preference contrasts. In the gun analyses, Hunting Rifle, Pistol, and Pistol & AR-15 are combined into an Any-Gun contrasted with No Gun.

Observed choices from partially completed neighborhoods are retained and clustered by participant. Neighborhood-level outcomes are constructed separately, as described with the relevant results. As with missing outcome data, missing covariates are handled through listwise deletion, producing model-specific sample sizes reported in each table. Unadjusted models retain usable task responses without NORC covariates when those covariates are not required. For coefficient interpretability, each table first reports a model-implied reference value for the focal reference category, averaged over the analytic sample, followed by the relevant contrasts. Standard errors for these reference values are calculated as linear combinations of fitted

[12] Adjusted models include participant race as a covariate except in specifications already stratified by participant race, where it is constant by construction.

coefficients using the same CR2 or HC2 variance-covariance matrix as the reported contrasts. As Models 1 and 2 have different sample inclusion rules and covariate profiles, reference values are specification-specific. Therefore focal contrasts are more directly comparable between models.

## Results

### 1. Do Americans prefer a plurality of same-race neighbors?

Figure 3 reports participants' preferred neighborhood racial composition, based on their housing choices in the baseline task. Every participant group (White, Black, Asian, or Hispanic) preferred a plurality of same-race neighbors. That is, each demonstrated a same-race modal preference, giving more houses in the neighborhood to its own racial group (same race) than to any of the specific racial outgroups: White participants chose 44 percent same-race neighbors, Black participants chose 39 percent, Asian participants chose 36 percent, and Hispanic participants chose 31 percent. These differences only reached statistical significance consistently for White and Black participants. Although the preference among Hispanic and Asian participants for same-race neighbors over White neighbors was not significant, their preference for same-race neighbors over those from other non-White groups was significant. As hypothesized, White participants' preference for same-race neighbors exceeded that among non-White participants (pooled) by 9.9 percentage points (95 percent CI: 7.3, 12.6; $p < .001$), and it also exceeded that in each of the specific non-White participant groups (Black, Hispanic, and Asian). This finding provides support for H1's prediction that the preference for neighborhood homophily would be strongest among White participants (see Appendix E, Table E1).

*[Insert Figure 3 about here]*

The next portion of the analysis uses data from a second experimental task to test H1's same-race desirability prediction. In this task, participants viewed a neighborhood where the racial composition was randomly assigned and they rated the neighborhood's desirability on a seven-point scale. The desirability rating is the outcome variable and the independent variable is the randomized racial composition—specifically, the number of randomly assigned same-race households out of the total of 14. The results, which are presented in Table 1, show that racial composition exerted a statistically significant effect on neighborhood desirability. Specifically, each additional same-race neighbor that was randomly assigned to the neighborhood increased its desirability by approximately 0.03 points, supporting H1's desirability prediction.

*[Insert Table 1 about here]*

**2. Do Americans prefer racial integration to homogeneity?**

To test whether participants preferred neighborhoods with some racial integration (H2), we again use data from the baseline task, where participants chose their neighbors' race. We classify a neighborhood as integrated when participants chose neighbors from at least two racial groups. Our analytic sample was composed of the 2,064 participants who selected a racial label for all 14 houses. Of these completed grids, 1,841 (89 percent) were integrated and 223 (11 percent) were homogeneous. When weighted, these results show that 89 percent (95 percent CI: 87, 91) of the participants preferred a neighborhood with at least some racial integration.

The preference for integration was evident in every participant group: 87 percent of White participants preferred integration, 89 percent of Black participants did, 95 percent of Asian participants did, and 93 percent of Hispanic participants did. Compared with White participants, Asian participants were 7.7 percentage points (95 percent CI: 2.3, 13.0; $p < .01$) and

Hispanic participants were 6.1 points (95 percent CI: 1.8, 10.4; $p < .01$) more likely to construct an integrated neighborhood. A majority of every racial group of participants designed a neighborhood with two or more racial groups (all $p < .001$; Appendix E, Table E2), supporting H2. Preferences for multiracial neighborhoods (H2) therefore coexist with the same-race pluralities described above and predicted in H1.

**3. Are residential preferences hierarchical?**

Attention turns now to H3, which predicts that Americans will prefer more White and Asian neighbors to Black and Hispanic neighbors due to the historical racial hierarchy in the United States and associated racial stereotypes. Figure 3A shows the overall weighted composition. The findings are complex and surprising. Overall, participants preferred 39 percent of their neighbors to be White, 22 percent to be Black, 20 percent to be Hispanic, and 19 percent to be Asian. The combined White-Asian share was 57 percent, compared with 43 percent for Black-Hispanic households. The paired difference was 14.6 percentage points (95 percent CI: 12.3, 17.0; $p < .001$), supporting the aggregate contrast in H3.

Even among Black, Hispanic, and Asian participants, White neighbors were the most preferred racial outgroup (Figure 3B), consistent with earlier showcard findings (Charles 2000; Smith 2001). Otherwise, outgroup rankings varied. White participants chose Black neighbors more often than Hispanic or Asian neighbors. Among non-Asian participants, Asian neighbors were the least preferred outgroup. In short, the evidence supports H3's specified pooled contrast and the centrality of White neighbors, but it does not support a common non-White outgroup ranking shared by all participant groups.

**4. Does proximity amplify homophily preferences?**

We next examine whether participants tended to allocate same-race neighbors to houses closer to their own and place racially dissimilar neighbors in houses that are further away (H4). To do this, we use data from the baseline task and compare the middle and far distance bands with the four closest positions, evaluating racial homophily overall and then within each respondent racial group.

*[Insert Table 2 about here]*

The findings show that, as hypothesized, participants tended to place same-race neighbors nearer to their own homes. Whereas the probability of selecting a same-race neighbor was 46 percent at close positions, it was 5.9 percentage points lower at middle positions, and 6.0 points lower at the far corners (both $p < .001$; see Model 2 in Table 2 and Figure 4). The far-minus-close contrast was negative in every respondent group, indicating a generalized proximity effect: -4.6 points among White participants, -6.5 among Black participants, -8.8 among Hispanic participants, and -14.4 among Asian participants (all $p < .05$). Middle-distance contrasts were also negative in all groups, although the Asian estimate was not significant (Table 2).

*[Insert Figure 4 about here]*

The pooled pattern is principally an in-group advantage for adjacent neighbors. Participants made little distinction between middle and far positions (difference = -0.1 points, $p = .882$). An alternative inner-versus-outer-column coding revealed a similar proximity effect, with the probability of same-race selection being higher at nearer houses than at more distant ones (-3.9 points, $p < .001$; Appendix E, Table E4; see also Appendix E, Table E5).

To test the theorized possibility that participants were quarantining Black neighbors, we estimate separate house-level outcomes for selecting each racial label. Participants tended to

place White households at closer positions than at middle and far positions, but they tended to do the opposite with Black households: selection rose by 1.3 points at middle positions and 2.8 points at the far corners. The direct far-minus-middle contrast was 1.6 points (95 percent CI: 0.4, 2.8; $p = .011$), supporting H4's prediction of concentration of Black neighbors at the most distant neighborhood positions. Among non-Black participants the far-corner increase was 4.0 points (95 percent CI: 2.5, 5.4; $p < .001$; Appendix E, Table E5, Section B). Hispanic placements showed no clear distance pattern, while Asian placements were slightly more common in the middle distance (see Table 3).

*[Insert Table 3 about here]*

**5. Do guns amplify homophily preferences?**

To explore how guns and race intersect to shape racial and residential preferences (H5), we first examine whether participants were more likely to arm same-race neighbors (race-cue task). Then, we test whether participants are more likely to place same-race neighbors in houses with guns (gun-cue task). We begin with the former, the race-cue task, wherein the neighbor's race is randomly assigned, and the outcome variable is whether the participant prefers them to be armed or not. The independent variable is the neighbor's randomly assigned race—specifically, whether it matched the participant's or differed.

[Insert Table 4 about here]

The findings show that, consistent with our fifth hypothesis, participants were more likely to arm same-race neighbors than different-race neighbors. The difference was 7.4 percentage points in Model 1 and 7.9 points in Model 2 (95 percent CI: 5.1, 10.6; $p < .001$), supporting H5.

The adjusted probability of gun allocation rose from 38.8 percent for different-race neighbors to 46.7 percent for same-race neighbors, which is a sizable increase (more than 20%) (Table 4).

The gun-cue task provides a reciprocal test: if participants knew a house would have a gun, were they more likely to put same-race neighbors in it? Here the house's gun status was randomly assigned, and participants selected the race of the neighbor who would live there. Compared with a No Gun cue, a gun cue increased participants' probability of selecting a same-race neighbor by 5.9 points in Model 1 and 6.6 points in Model 2 (95 percent CI: 4.6, 8.5; $p < .001$). The adjusted probability of choosing a same-race neighbor rose from 39.4 to 46.0 percent when that neighbor would be armed. Racial similarity and gun ownership were therefore preferentially paired in both task directions, indicating that homophily preferences are amplified in the presence of guns. Both effects persist in gun-owning and non-gun-owning households.

**6. Are Americans most averse to gun ownership by Black neighbors?**

Our final hypothesis (H6) predicts that participants, regardless of their own race, will be less likely to arm Black than White neighbors. To test it, we broaden the analytic lens beyond same- vs. different race neighbors to focus on each of the four randomly assigned neighbor races: White, Black, Hispanic, or Asian. We test how each influenced participants' willingness to arm the neighbor. The findings are shown in Table 5. The unadjusted Black-White difference was -3.1 percentage points ($p = .079$) in Model 1, but -6.3 points in Model 2 (95 percent CI: -8.8, -3.9; $p < .001$); Model 2 holds constant the prevalence scenario within which labels were drawn. Hispanic labels also received fewer gun selections than White labels (-3.7 points, $p < .01$), while the Asian-White difference was not significant.

[Insert Table 5 about here]

The subgroup estimates show that while White participants were substantially and significantly less likely to arm Black neighbors (-9.8 percentage points), Black participants were 13.0 percentage points more likely to arm Black neighbors (both $p < .001$). On the other hand, the Black–White contrasts were not significant among Hispanic and Asian participants (Appendix E, Table E3). A pooled interaction model rejected equality of the racial-label contrasts across participant groups (Wald $\chi^2(9) = 38.46$, $p < .001$), indicating that participant race moderated the effect of neighbor race on gun allocation. H6's support is therefore partial: the Black gun ownership penalty is clear among White participants, not significant among Hispanic and Asian participants, and reversed among Black participants.

**Discussion**

Neighborhood racial hierarchy realized as segregation forecloses opportunities for intergroup contact and for wealth accumulation (Korver-Glenn 2018). And segmented immobility further limits exchange between racially dissimilar counties (Huang and Butts 2023). We began this study by observing that neighborhood racial hierarchy is durable even as the formal machinery of residential segregation has weakened and Americans' overt racial prejudices have softened (Logan and Stults 2022; see also Bonilla-Silva 2022: 44-47). The field's answers to this discrepancy point to persistent direct discrimination (Charles 2003; Korver-Glenn 2018; Pager and Shepherd 2008) and the networks, routines, and knowledge that narrow which neighborhoods are considered at all (Krysan and Crowder 2017). If declining direct prejudice does not materially weaken racial hierarchy, then standard measures may be eliciting preference where it is weakest.

We proposed *homophily amplification*, whereby same-race preferences are conditionally activated rather than constant, as another account of the durability of racial hierarchy. We linked social distance scholarship (Bogardus 1947), homophily research (McPherson, Smith-Lovin, and Cook 2001), and uncertainty reduction (Hogg 2007) to argue that Americans may hedge against the cost of misjudging a neighbor in uncertain contexts by increasing their preference for same-race neighbors, who are presumed to be more likely to share important norms. Homophily amplification may sustain reliance on race when proximity makes encounters more frequent or gun ownership makes their consequences more serious. Therefore, we hypothesized that while Americans would prefer multiracial neighborhoods, they would still want their nearest or armed neighbors to be of the same race.

To test this, we used a novel survey-experimental design. Our first contribution is the replication of past findings with a large 2025 nationally-representative probability sample. We find that in some ways, Americans' current residential preferences resemble those of past decades. Every participant group allocated its largest average share to same-race neighbors, with White participants assigning the largest share to White households, confirming our first hypothesis. Yet, 89 percent of participants constructed neighborhoods containing at least two racial groups, also confirming our second hypothesis that Americans prefer integration to uniformity. Black participants were second-most likely to construct a racially homogeneous neighborhood (11 percent). These patterns extend earlier composition studies (Charles 2000; Smith 2001), and do not support a single racial ordering. White participants selected Black neighbors more often than Hispanic or Asian neighbors. Moreover, Hispanic and Asian participants' same-race shares did not significantly exceed their White-neighbor shares, as in the 2000 GSS (Smith 2001). The persistence of White neighbors' appeal therefore coexists with

variation in other groups' relative appeal, consistent with social-distance research that distinguishes outgroup rankings by the rater's own group (Weaver 2008).

We predicted in hypothesis 3 that Asian and White households would receive a larger combined share of neighborhood selections than Black and Hispanic households. This hypothesis was confirmed: White households' selection was co-equal or second-highest among non-White participants, but Asian household selection was last among non-Asian participants, leading to a combined share that still exceeded that of selected Black and Hispanic neighbors. White household selection affirms earlier work on racial hierarchy (Charles 2000, 2003; Smith 2001). Our interpretation of Asian household selection's lag echoes Zou and Cheryan's (2017) distinction between perceived inferiority and cultural foreignness: Asian Americans' relatively high socioeconomic standing did not translate as much as we expected into greater appeal as a neighbor. Asian neighbors' low selection among non-Asian participants is therefore compatible with a perception of unfamiliar norms.

We predicted in hypothesis 4 that participants would prefer same-race neighbors nearby and would quarantine less-preferred outgroups to the most distant parts of their neighborhood. Indeed, we find that participants concentrated same-race neighbors near their own homes, with selection significantly higher for households bordering theirs. Proximity's homophily amplification was universal, though its magnitude varied across participant groups. This furthers scholarship on street connectivity (Grannis 1998) and household-level micro-segregation (Grigoryeva and Ruef 2015) by showing that preferred arrangements also exhibit racialization. Anicich et al. (2021) found that White participants placed White residents nearer to their own hypothetical home. Our extension is to use a nationally representative sample that is racially diverse, finding that proximity amplifies homophily *generally*, not just among White Americans.

Dederichs et al.'s (2025) Dutch-sample finding of no proximity gradient in educational ingroup preferences contrasts with our finding that proximity amplifies Americans racial preferences. Race may convey a stronger normative signal than education does.

Furthermore, we found that participants were most comfortable with Black households at the far corners of their neighborhoods. Neighborhood inclusion and immediate proximity are therefore distinct preferences. Even if a neighborhood is multiracial, a preference to quarantine Black Americans into corners may foreclose opportunities for the types of contact that reduce prejudice (Pettigrew and Tropp 2006) while brief exposures can increase exclusionary attitudes (Enos 2014).

A second proposed homophily amplification mechanism is consequence, examined here through gun ownership. Prior work had shown that guns are a cue whose meaning depends on both the owner and the observer (Carlson 2019; Hayes et al. 2021; Higginbotham et al. 2023). Therefore, we hypothesized that participants would perceive gun-owning neighbors to be less risky when of the same race, and would therefore align self-same race and gun ownership in their preferred neighborhoods.

Indeed, not only does a neighbor's racial similarity condition participants' assignment of gun ownership, but a neighbor's gun ownership also conditions participants' assignment of race. When household racial labels were randomized, participants were 7.9 percentage points more likely to select guns for same-race neighbors. When household gun ownership was randomized, participants were 6.6 points more likely to select same-race neighbors for armed houses. This extends work that finds an aversion to some types of gun ownership (Sola and Pickett 2024) by showing how homophily amplification can render gun ownership more or less attractive based

on racial (dis)similarity. Aversion to particular gun practices coexists with a racial preference about which neighbors should possess guns.

We also predicted that participants, regardless of their own race, would be less willing to arm Black neighbors. This prediction drew on racialized perceptions of neighborhood crime (Quillian and Pager 2001) and experiments showing that racial cues affect weapon identification and decisions to shoot (Correll et al. 2002; Payne 2001). While there is a pooled penalty to Black gun ownership, we find heterogeneous effects rather than the hypothesized homogeneous penalty. White participants were 9.8 percentage points less likely to arm Black than White neighbors, but Black participants were 13.0 points *more* likely to do so. Hispanic and Asian participants' Black-White contrasts were not statistically significant.

Perhaps, rather than a universal hierarchy, position-dependent threat better explains these mixed findings (Blumer 1958; Bobo and Hutchings 1996). In Appendix E, Table E3, White participants assigned fewer guns to every non-White label. On the other hand, Black participants assigned more guns to Black, Hispanic, and Asian neighbors than to White neighbors. This pattern suggests comparatively greater reluctance among Black participants to arm White neighbors, not simply an armed-ingroup preference. This may be in anticipation of White hostility in residential choices (Bobo and Hutchings 1996; Krysan and Farley 2002), or reflect a view that having more owners of color is preferable given the predominance of White owners.

Two theoretical implications follow. First, hierarchy is not adequately described by a single ranking of preferred neighbors. The appeal of White neighbors in composition, the peripheral placement of Black households in pooled choices, and observer-specific gun allocations reveal distinct dimensions of racialized preference. Second, amplification need not take the same form across conditions. Proximity was associated with stronger same-race

preference in every participant group; willingness to arm Black versus White neighbors instead depended on the participant's race. This combination fits the broader distinction between frequency and consequence, while qualifying our more specific prediction of a common aversion to arming Black neighbors.

The conditional strengthening measured here may contribute to, but is distinct from, cumulative amplification through networks and over time (DiMaggio and Garip 2012; DiPrete and Eirich 2006; Schelling 1971). Simulations that incorporate spatially conditional preferences and housing constraints may help resolve the Bruch and Mare debate (Van de Rijt, Siegel, and Macy 2009; Bruch and Mare 2009) and may help link preferred racial micro-segregation to observed segregation.

This work opens new research avenues for homophily amplification: what dampens reliance on race? Disambiguation, by displacing racial assumptions with credible information, may reduce uncertainty and racial homophily. If so, which cues are most and least effective at that disambiguation? Crime and disorder are candidates, but their interpretation is already racialized (Sampson and Raudenbush 2004). Class signifiers affect neighborhood evaluations, but race still affects valuations even when displayed class conditions are held constant (Krysan et al. 2009). However, those data were collected 20 years ago, so there may be an opportunity to leverage class signifiers in future homophily research. Further work can link the process of the housing search (drawing on Korver-Glenn 2018) with exploration of how ostensibly race-neutral listing information (see Besbris, Schachter, and Kuk 2026) conditions homophily amplification.

Our findings have limitations. Racial and gun-ownership labels were explicit, whereas information about these attributes may be incomplete in actual housing searches. And these labels may evoke expectations about class, crime, or social behavior. Our estimands deliberately

include these informative associations as they are constitutive of racial perception rather than separable contaminants (Hu and Kohler-Hausmann 2024). The four broad racial categories limit within-group distinctions and exclude some participants from direct same-race comparisons. Finally, the small Asian subsample also warrants interpretive caution.

Direct preferences for neighborhood integration provide an incomplete description of preferred residential life. People may welcome a multiracial neighborhood while drawing sharper distinctions about who lives next door and which neighbors should be armed. The prevalence of desire for integration invites more research on the two questions: which conditions permit that openness to extend to immediate neighbors, and which amplify racial boundaries within otherwise mixed places? Clear answers can inform us on how to build communities that cohere regardless of shared race.

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## Appendix A. Power benchmarks

The planning calculations assumed 2,000 participants, approximately 500 per prevalence scenario. With anticipated racial proportions of 65 percent White, 18 percent Hispanic, and 14 percent Black, the original two-group comparisons yield the benchmarks in Table A1. Calculations use 80 percent power and a two-sided .05 significance level. They describe preregistered design-stage benchmarks rather than post hoc power for the observed effects.

**Table A1. Original two-group power benchmarks**

| Comparison | Group 1 n | Group 2 n | Cohen's d |
|---|---|---|---|
| Two scenarios | 500 | 500 | 0.18 |
| White versus non-White | 325 | 175 | 0.26 |
| White versus Black | 325 | 70 | 0.37 |
| White versus Hispanic | 325 | 90 | 0.33 |
| Hispanic versus Black | 90 | 70 | 0.45 |

## Appendix B. Supplementary instrument details

The main text reproduces the racial-composition prompt, response options, cue conditions, and desirability item. Participants also completed baseline gun-composition and political-composition tasks that are not analyzed in this article.

Participants were prompted to complete all houses but could proceed after acknowledging a skipped-selection warning. The reported analyses distinguish completed baseline grids, used for the integration classification, from available house choices, used for composition and placement estimates.

After the client-hosted experimental tasks, NORC asked, “Do you or does anyone in your household own a gun?” The options, as recommended by Schutten et al. (2024), were “I personally own a gun”; “I do not personally own a gun, but someone in my household owns a gun”; and “No one in my household owns a gun.” Demographic covariates came from panel profiles collected before this survey. The task data and profile data are linked by participant identifier.

## Appendix C. Analytic samples and neighborhood completion

Analytic samples follow the information required for each outcome and specification. Household models exclude unanswered choices while retaining completed choices within partial neighborhoods. Unadjusted models that do not require participant characteristics also include usable responses without NORC covariates. Same-race comparisons require a participant racial category that matches one of the four selectable labels. Desirability uses observed neighborhood ratings and all 14 displayed racial cues, independent of gun-choice completion.

**Table C1. Main analytic samples**

| | *Participants* | | *Household Choices* | |
|---|---|---|---|---|
| **Analysis** | **M1** | **M2** | **M1** | **M2** |
| Weighted composition: all participants | 2,133 | — | — | — |
| Weighted composition: four racial groups | 2,038 | — | — | — |
| Weighted integration: complete grids | 2,064 | — | — | — |
| Neighborhood desirability | 2,078 | 2,057 | — | — |
| Same-race placement | 2,038 | 2,020 | 28,220 | 27,969 |
| Target-race placement | 2,372 | 2,115 | 32,188 | 29,293 |
| Race cues → gun selection | 2,049 | 2,028 | 28,543 | 28,257 |
| Gun cues → racial selection | 2,016 | 1,997 | 27,958 | 27,692 |
| Racial cues → gun selection, all participants | 2,221 | 2,121 | 30,538 | 29,559 |

Note: Models 1 and 2 are unadjusted and adjusted, respectively. Participant-level outcomes have no household-choice count. “Four racial groups” denotes non-Hispanic White, non-Hispanic Black, Hispanic, and non-Hispanic Asian/Pacific Islander participants.

## Appendix D. Sample characteristics

### Table D1. Participant characteristics

| Characteristic / category | Unweighted | Weighted |
|---|---|---|
| **Age (years) (n = 2,176)** | | |
| Mean (SD) | 48.5 (17.7) | 48.0 (18.3) |
| **Sex (n = 2,176)** | | |
| Female | 52.0% | 51.3% |
| Male | 48.0% | 48.7% |
| **Race/ethnicity (n = 2,176)** | | |
| 2+, non-Hispanic | 3.0% | 3.2% |
| Asian-Pacific Islander, non-Hispanic | 3.9% | 5.5% |
| Black, non-Hispanic | 11.1% | 12.1% |
| Hispanic | 17.8% | 18.4% |
| Other, non-Hispanic | 1.4% | 1.0% |
| White, non-Hispanic | 62.8% | 59.9% |
| **Household size (n = 2,176)** | | |
| Mean (SD) | 3.0 (1.5) | 3.1 (1.6) |
| **Education (n = 2,176)** | | |
| Less than HS | 5.1% | 8.8% |
| HS graduate or equivalent | 18.2% | 28.5% |
| Some college/ associates degree | 39.0% | 26.1% |
| Bachelor's degree | 22.6% | 23.0% |
| Post grad study/professional degree | 15.1% | 13.7% |
| **Household income (n = 2,176)** | | |
| Less than $5,000 | 1.6% | 2.1% |
| $5,000 to $9,999 | 2.0% | 1.8% |
| $10,000 to $14,999 | 2.7% | 2.9% |
| $15,000 to $19,999 | 2.6% | 3.0% |
| $20,000 to $24,999 | 4.1% | 3.9% |
| $25,000 to $29,999 | 4.0% | 4.2% |
| $30,000 to $34,999 | 3.9% | 4.2% |
| $35,000 to $39,999 | 3.3% | 3.4% |
| $40,000 to $49,999 | 6.4% | 6.8% |
| $50,000 to $59,999 | 8.0% | 7.9% |
| $60,000 to $74,999 | 10.9% | 10.1% |

| Characteristic / category | Unweighted | Weighted |
|---|---|---|
| $75,000 to $84,999 | 5.0% | 4.6% |
| $85,000 to $99,999 | 9.5% | 9.7% |
| $100,000 to $124,999 | 10.8% | 9.9% |
| $125,000 to $149,999 | 7.3% | 7.7% |
| $150,000 to $174,999 | 6.4% | 5.9% |
| $175,000 to $199,999 | 3.2% | 3.6% |
| $200,000 or more | 8.3% | 8.4% |
| **Housing tenure (n = 2,176)** | | |
| Occupied without payment of cash rent | 3.2% | 3.3% |
| Owned or being bought by you or someone in your household | 67.8% | 69.4% |
| Rented for cash | 29.0% | 27.3% |
| **Home type (n = 2,176)** | | |
| A building with 2 or more apartments | 19.5% | 20.0% |
| A mobile home or trailer | 4.0% | 4.5% |
| A one-family house attached to one or more houses | 8.2% | 7.8% |
| A one-family house detached from any other house | 67.9% | 67.3% |
| Boat, RV, van, etc | 0.4% | 0.5% |
| **Metropolitan status (n = 2,176)** | | |
| Metro Area | 86.1% | 87.2% |
| Non-Metro Area | 13.9% | 12.8% |
| **Census division (n = 2,176)** | | |
| East North Central | 19.4% | 14.0% |
| East South Central | 6.0% | 5.8% |
| Mid-Atlantic | 10.9% | 12.7% |
| Mountain | 7.4% | 7.7% |
| New England | 3.1% | 4.7% |
| Pacific | 15.6% | 16.0% |
| South Atlantic | 19.1% | 20.6% |
| West North Central | 8.4% | 6.4% |
| West South Central | 10.1% | 12.3% |
| **Religion (n = 2,176)** | | |
| Agnostic (not sure if there is a God) | 7.3% | 6.6% |
| Atheist (do not believe in God) | 7.0% | 6.5% |
| Buddhist | 1.0% | 0.8% |
| Hindu (Hinduism) | 0.5% | 1.0% |
| Jewish (Judaism) | 1.7% | 1.7% |

| Characteristic / category | Unweighted | Weighted |
|---|---|---|
| Just Christian | 19.8% | 20.1% |
| Mormon (Church of Jesus Christ of Latter-day Saints/LDS) | 1.7% | 1.6% |
| Muslim (Islam) | 1.1% | 0.8% |
| Nothing in particular | 16.4% | 17.5% |
| Orthodox (Greek, Russian, or some other orthodox church) | 0.6% | 0.8% |
| Protestant | 23.5% | 22.8% |
| Roman Catholic (Catholic) | 17.5% | 17.9% |
| Something else, please specify: | 1.1% | 0.9% |
| Unitarian (Universalist) | 0.9% | 0.9% |
| **Religious attendance (n = 2,175)** | | |
| Never | 32.0% | 31.2% |
| Less than once per year | 17.0% | 17.3% |
| About once or twice a year | 12.2% | 12.6% |
| Several times a year | 8.2% | 8.2% |
| About once a month | 3.6% | 4.3% |
| 2-3 times a month | 5.1% | 4.6% |
| Nearly every week | 6.2% | 6.5% |
| Every week | 12.6% | 12.6% |
| Several times a week | 3.1% | 2.7% |
| **Party identification (n = 2,174)** | | |
| Strong Democrat | 20.0% | 19.7% |
| Not so strong Democrat | 15.1% | 15.1% |
| Lean Democrat | 11.5% | 11.7% |
| Don't Lean/Independent/None | 17.5% | 18.3% |
| Lean Republican | 8.6% | 8.1% |
| Not so Strong Republican | 12.0% | 12.0% |
| Strong Republican | 15.2% | 15.1% |
| **Ideology (n = 2,158)** | | |
| Very liberal | 13.1% | 13.1% |
| Somewhat liberal | 12.3% | 12.5% |
| Moderate | 48.7% | 48.2% |
| Somewhat conservative | 14.4% | 14.6% |
| Very conservative | 11.6% | 11.7% |
| **2024 presidential turnout (n = 2,174)** | | |
| I'm sure I voted in the November 2024 General Election. | 82.1% | 79.6% |
| I did not vote in the November 2024 General Election. | 13.2% | 15.5% |

| Characteristic / category | Unweighted | Weighted |
|---|---|---|
| I thought about voting in the November 2024 General Election, but I did not. | 2.5% | 3.0% |
| I usually vote, but I did not in the November 2024 General Election. | 2.2% | 1.8% |
| **Trust in media (n = 2,171)** | | |
| None | 17.5% | 16.4% |
| A little | 33.0% | 32.3% |
| A moderate amount | 38.2% | 39.9% |
| A lot | 8.7% | 8.4% |
| A great deal | 2.6% | 3.0% |
| **Internet access (n = 2,176)** | | |
| Internet Household | 92.6% | 92.6% |
| Non-internet household | 7.4% | 7.4% |
| **Household gun ownership (n = 2,162)** | | |
| I do not personally own a gun, but someone in my household owns a gun. | 16.4% | 16.6% |
| I personally own a gun. | 29.6% | 29.3% |
| No one in my household owns a gun. | 53.9% | 54.0% |

Note: Values are percentages or means (SDs) among non-missing responses. Sample sizes are shown for each characteristic. Weighted estimates use the final NORC AmeriSpeak survey weight.

## Appendix E. Detailed estimates and alternative distance coding

### Table E1. Paired tests of same-race preference (H1)

| Contrast | Difference [95% CI] | p |
|---|---|---|
| White: own minus Black | 24.5 [22.1, 26.9] | < .001 |
| White: own minus Hispanic | 26.4 [24.0, 28.8] | < .001 |
| White: own minus Asian | 26.6 [24.2, 29.0] | < .001 |
| Black: own minus White | 14.1 [7.8, 20.4] | < .001 |
| Black: own minus Hispanic | 18.8 [12.9, 24.7] | < .001 |
| Black: own minus Asian | 23.3 [17.4, 29.1] | < .001 |
| Hispanic: own minus White | 0.6 [-3.9, 5.1] | .791 |
| Hispanic: own minus Black | 10.1 [6.5, 13.7] | < .001 |
| Hispanic: own minus Asian | 13.7 [10.6, 16.8] | < .001 |
| Asian: own minus White | 2.5 [-6.5, 11.6] | .583 |
| Asian: own minus Black | 19.9 [13.3, 26.5] | < .001 |
| Asian: own minus Hispanic | 20.2 [13.5, 27.0] | < .001 |
| White own minus Black own | 5.3 [0.9, 9.7] | .017 |
| White own minus Hispanic own | 13.3 [10.2, 16.3] | < .001 |
| White own minus Asian own | 8.7 [3.6, 13.8] | < .001 |
| White own minus pooled non-White own | 9.9 [7.3, 12.6] | < .001 |

Note: Weighted percentage-point differences in racial selection shares. Paired and between-group contrasts use participant-clustered CR2 standard errors; the White-pooled-non-White contrast uses HC2 standard errors. Brackets contain 95% confidence intervals; p-values are two-tailed.

**Table E2. Integrated baseline neighborhoods (H2)**

| Participant group | Complete grids | Integrated grids | Weighted % [95% CI] |
|---|---|---|---|
| All | 2,064 | 1,841 | 88.7 [86.9, 90.5] |
| White | 1,295 | 1,137 | 87.0 [84.7, 89.4] |
| Black | 230 | 203 | 89.1 [84.0, 94.2] |
| Hispanic | 370 | 346 | 93.1 [89.5, 96.7] |
| Asian | 77 | 71 | 94.7 [89.8, 99.6] |

Note: Integrated neighborhoods contain at least two racial groups among 14 completed choices. Percentages are survey-weighted; brackets contain HC2-based 95% confidence intervals. The overall sample includes all racial categories.

**Table E3. Gun selection by racial label and participant race (H6)**

| Participant group | Model | White reference (%) | Black - White | Hispanic - White | Asian - White | Participants / house choices |
|---|---|---|---|---|---|---|
| All | M1 | 43.7<br>(1.2) | -3.1<br>(1.8) | -3.4<br>(1.7) | -0.2<br>(1.6) | 2,221 /<br>30,538 |
| All | M2 | 44.6<br>(0.9) | -6.3***<br>(1.2) | -3.7**<br>(1.2) | -2.1<br>(1.3) | 2,121 /<br>29,559 |
| White | M1 | 47.5<br>(1.5) | -5.9**<br>(2.3) | -7.5***<br>(2.2) | -1.0<br>(2.0) | 1,350 /<br>18,814 |
| White | M2 | 48.6<br>(1.1) | -9.8***<br>(1.5) | -7.7***<br>(1.4) | -4.4**<br>(1.6) | 1,344 /<br>18,730 |
| Black | M1 | 29.8<br>(3.1) | 12.6**<br>(4.8) | 15.9**<br>(5.1) | 11.3*<br>(4.6) | 236 /<br>3,282 |
| Black | M2 | 30.2<br>(2.6) | 13.0***<br>(3.7) | 12.8***<br>(3.7) | 11.3**<br>(4.2) | 227 /<br>3,164 |
| Hispanic | M1 | 42.0<br>(2.8) | -2.4<br>(4.3) | 2.3<br>(4.2) | 1.0<br>(3.7) | 379 /<br>5,282 |
| Hispanic | M2 | 42.8<br>(2.2) | -5.6<br>(3.0) | 1.9<br>(2.8) | -3.0<br>(3.0) | 374 /<br>5,212 |
| Asian | M1 | 15.3<br>(4.3) | 7.4<br>(8.0) | -2.7<br>(5.6) | 0.5<br>(5.5) | 84 /<br>1,165 |
| Asian | M2 | 15.9<br>(2.3) | 1.9<br>(2.6) | 0.3<br>(2.6) | 2.4<br>(3.8) | 83 /<br>1,151 |

Note: Outcome is any-gun selection (versus No Gun). Reference values are percentages; contrasts are percentage points. Unweighted LPMs; participant-clustered CR2 standard errors in parentheses. * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed).

A pooled model interacting racial labels with participant race, with the same covariate adjustment, rejects equality of the racial-label contrasts across groups (Wald $\chi^2(9) = 38.46$, $p < .001$; 2,028 participants and 28,257 choices). The omnibus test covers all nine label-by-participant-race interaction coefficients.

**Table E4. Inner-column versus outer-column distance coding**

The alternative coding compares the eight neighboring houses in the inner three columns with the six houses in the two outer columns. Table E4 repeats the same-race models under the alternative coding; Table E5 compares the two codings in a stacked target-race model.

| Participant group | Inner reference (%) | Outer - inner | Participants / choices |
|---|---|---|---|
| All | 43.1<br>(0.6) | -3.9***<br>(0.7) | 2,020 / 27,969 |
| White | 45.4<br>(0.8) | -3.0***<br>(0.8) | 1,332 / 18,436 |
| Black | 43.7<br>(1.9) | -3.6<br>(2.1) | 230 / 3,176 |
| Hispanic | 34.5<br>(1.1) | -5.6***<br>(1.5) | 378 / 5,252 |
| Asian | 43.8<br>(3.0) | -11.3**<br>(3.7) | 80 / 1,105 |

Note: Outcome is a same-race selection. Inner values are percentages; outer-minus-inner contrasts are percentage points. Covariate-adjusted, unweighted LPMs with participant-clustered CR2 standard errors in parentheses. * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed).

**Table E5. Target-race contrasts under both distance codings**

| Selected race | Coding | Contrast | Difference (SE) |
|---|---|---|---|
| | | A. All participants | |
| White | Three bands | Middle minus close | -2.7*** (0.7) |
| Black | Three bands | Middle minus close | 1.3* (0.6) |
| Hispanic | Three bands | Middle minus close | 0.1 (0.6) |
| Asian | Three bands | Middle minus close | 1.3* (0.6) |
| White | Three bands | Far minus close | -2.4** (0.8) |
| Black | Three bands | Far minus close | 2.8*** (0.7) |
| Hispanic | Three bands | Far minus close | -0.6 (0.7) |
| Asian | Three bands | Far minus close | 0.2 (0.7) |
| White | Inner / outer | Outer minus inner | -1.9** (0.6) |
| Black | Inner / outer | Outer minus inner | 2.2*** (0.5) |
| Hispanic | Inner / outer | Outer minus inner | -0.6 (0.5) |
| Asian | Inner / outer | Outer minus inner | 0.3 (0.5) |
| | | B. Non-Black participants | |
| Black | Three bands | Middle minus close | 2.7*** (0.6) |
| Black | Three bands | Far minus close | 4.0*** (0.7) |
| Black | Inner / outer | Outer minus inner | 2.9*** (0.6) |

Note: Outcomes indicate selection of each racial label. Adjusted, unweighted stacked LPMs; Section A uses 29,293 choices from 2,115 participants (117,172 stacked rows), and Section B refits the same models to non-Black participants (26,117 choices from 1,885 participants; 104,468 stacked rows). Contrasts are percentage points; participant-clustered CR2 standard errors appear in parentheses. * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed).

**Figure 1. Neighborhood layout and Manhattan distance bands**

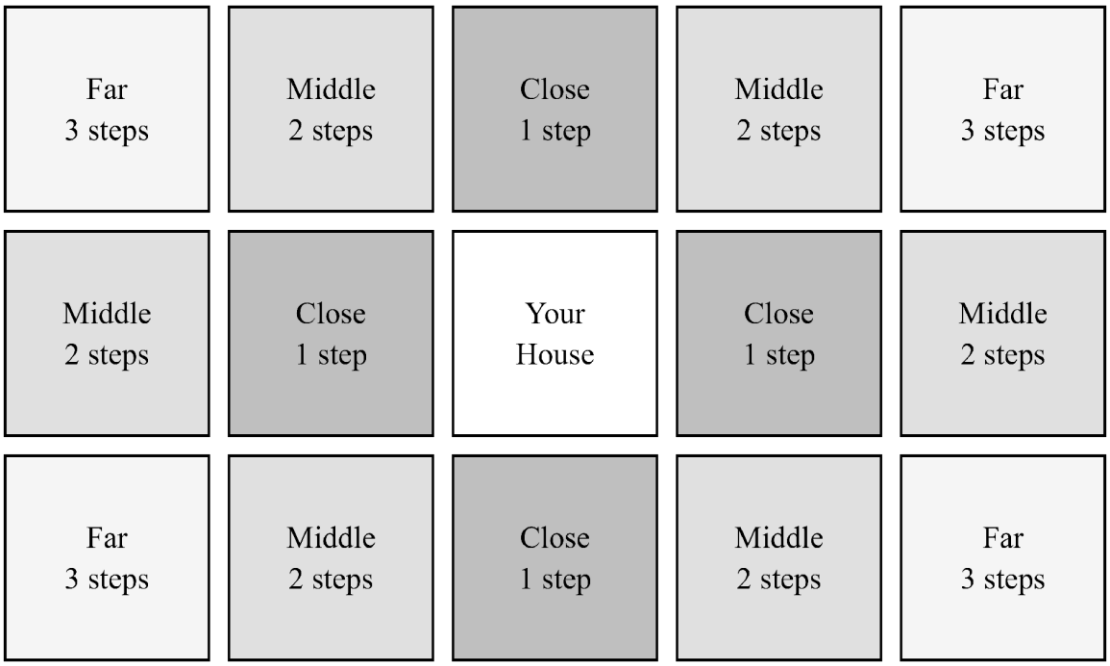
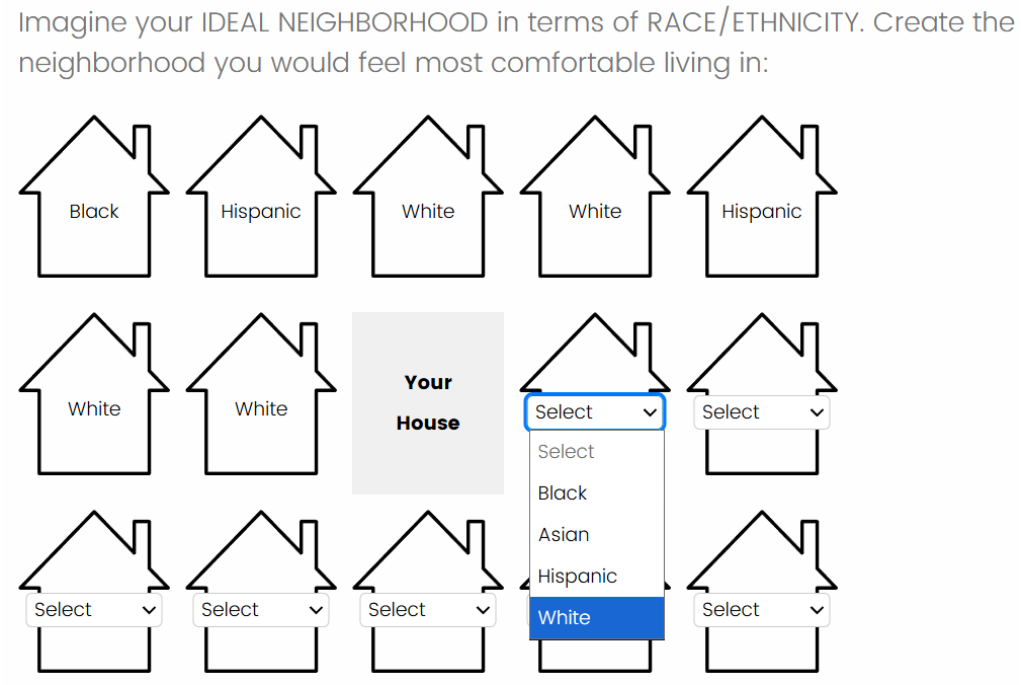


**Figure 2. Neighborhood tasks**

A. Baseline racial-composition selection

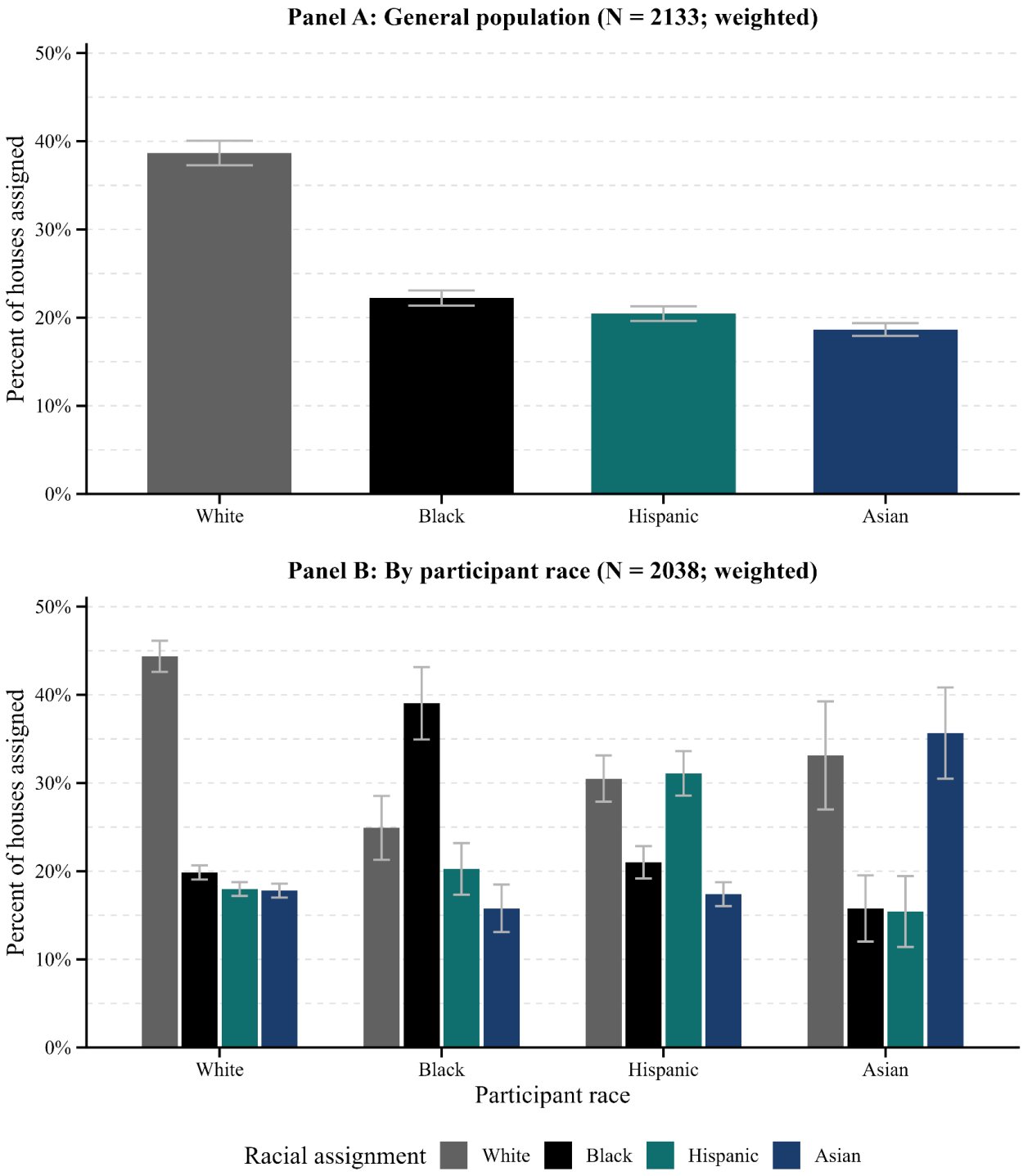



B. Gun selection under randomized racial composition and political context



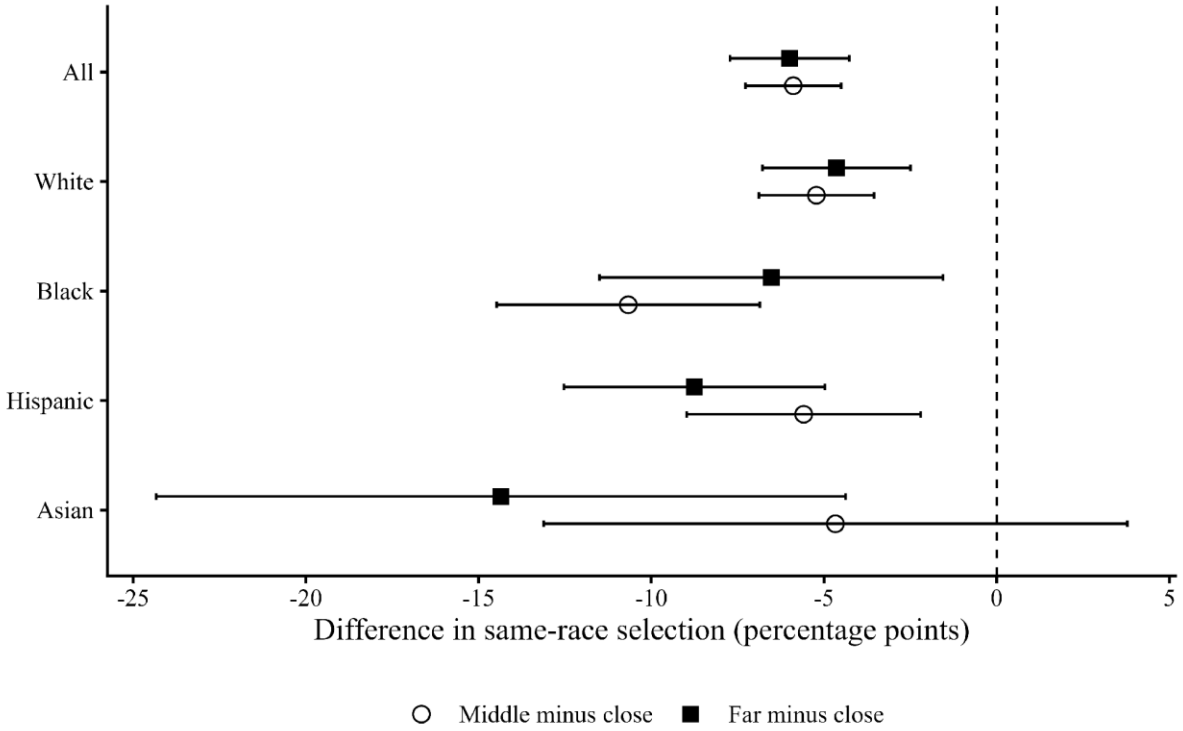

Note: Partially completed task screens. Participants selected racial labels in Panel A and gun statuses in Panel B; Panel B's political scenario, racial scenario, and racial labels were randomly assigned.

**Figure 3. Preferred racial composition**




Note: Weighted neighbor racial selection shares. Error bars show 95% confidence intervals using participant-clustered CR2 standard errors. Panel A: all weighted respondents (n = 2,133); Panel B: four respondent groups (n = 2,038).

**Figure 4. Same-race selection by distance and participant race**




Note: Outcome is a same-race selection. Points show adjusted, unweighted percentage-point differences from close positions; bars show 95% confidence intervals using participant-clustered CR2 standard errors. See Table 2.

**Table 1. Same-race neighbors and neighborhood desirability**

| | Model 1 | Model 2 |
|---|---|---|
| Reference rating: no same-race neighbors | 4.89<br>(0.06) | 4.88<br>(0.06) |
| Change per same-race neighbor | 0.029***<br>(0.008) | 0.030***<br>(0.008) |
| Participant covariates | No | Yes |
| Political condition | No | Yes |
| Participants | 2,078 | 2,057 |

Note: Outcome is neighborhood desirability (1-7). Unweighted OLS estimates with HC2 robust standard errors in parentheses. * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed).

**Table 2. Distance and Racial Homophily, by participant race**

| Participant group | Model | Close (%) | Middle Δ | Far Δ | Participants / house choices |
|---|---|---|---|---|---|
| Combined | M1 | 45.7<br>(0.8) | -5.9***<br>(0.7) | -5.9***<br>(0.9) | 2,038 / 28,220 |
| Combined | M2 | 45.7<br>(0.7) | -5.9***<br>(0.7) | -6.0***<br>(0.9) | 2,020 / 27,969 |
| White | M1 | 47.7<br>(0.9) | -5.2***<br>(0.8) | -4.6***<br>(1.1) | 1,336 / 18,491 |
| White | M2 | 47.7<br>(0.9) | -5.2***<br>(0.9) | -4.6***<br>(1.1) | 1,332 / 18,436 |
| Black | M1 | 48.5<br>(2.1) | -10.2***<br>(1.9) | -5.8*<br>(2.5) | 239 / 3,302 |
| Black | M2 | 48.6<br>(2.1) | -10.7***<br>(1.9) | -6.5*<br>(2.5) | 230 / 3,176 |
| Hispanic | M1 | 36.9<br>(1.5) | -5.6**<br>(1.7) | -8.5***<br>(1.9) | 382 / 5,308 |
| Hispanic | M2 | 37.0<br>(1.5) | -5.6**<br>(1.7) | -8.8***<br>(1.9) | 378 / 5,252 |
| Asian | M1 | 45.1<br>(4.0) | -5.0<br>(4.3) | -14.4**<br>(5.0) | 81 / 1,119 |
| Asian | M2 | 45.0<br>(3.9) | -4.7<br>(4.3) | -14.4**<br>(5.1) | 80 / 1,105 |

Note: Outcome is a same-race selection (1 = yes). Combined models include White, Black, Hispanic, and Asian participants. Adjusted subgroup models omit participant race because it is constant within each group. Close values are percentages; Middle Δ and Far Δ values are percentage points contrasts with the close value. Unweighted LPMs; participant-clustered CR2 standard errors in parentheses. * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed).

**Table 3. Distance as a moderator of racial assignment (unadjusted and adjusted LPMs)**

| Selected race | Model | Close (%) | Middle Δ | Far Δ |
|---|---|---|---|---|
| White | M1 | 40.1<br>(0.7) | -2.7***<br>(0.6) | -2.5**<br>(0.8) |
| White | M2 | 40.2<br>(0.7) | -2.7***<br>(0.7) | -2.4**<br>(0.8) |
| Black | M1 | 21.1<br>(0.5) | 1.2*<br>(0.6) | 2.8***<br>(0.7) |
| Black | M2 | 21.1<br>(0.5) | 1.3*<br>(0.6) | 2.8***<br>(0.7) |
| Hispanic | M1 | 21.0<br>(0.5) | -0.1<br>(0.6) | -0.7<br>(0.7) |
| Hispanic | M2 | 20.7<br>(0.5) | 0.1<br>(0.6) | -0.6<br>(0.7) |
| Asian | M1 | 17.8<br>(0.5) | 1.6**<br>(0.5) | 0.4<br>(0.6) |
| Asian | M2 | 18.0<br>(0.5) | 1.3*<br>(0.6) | 0.2<br>(0.7) |

Note: Outcome is selection of the indicated racial label at a house (1 = yes); contrasts are by Manhattan distance band (see Figure 1). Close values are percentages; Middle Δ and Far Δ values are percentage-points contrasts with the close value. Unweighted LPMs; participant-clustered CR2 standard errors in parentheses. Model 1: 2,372 participants, 32,188 choices; Model 2: 2,115 participants, 29,293 choices. * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed).

**Table 4. Reciprocal effects of racial and gun-ownership cues**

| | Model 1 | Model 2 |
|---|---|---|
| *Effect of a same-race cue on gun selection* | | |
| Reference condition: a different-race neighbor | 39.1<br>(1.1) | 38.8<br>(1.0) |
| Δ probability of gun selection for a same-race neighbor | 7.4***<br>(1.5) | 7.9***<br>(1.4) |
| Participants | 2,049 | 2,028 |
| House choices | 28,543 | 28,257 |
| *Effect of a gun cue on same-race selection* | | |
| Reference condition: a no-gun neighbor | 39.6<br>(0.7) | 39.4<br>(0.6) |
| Δ probability of same-race selection for a gun-owning neighbor | 5.9***<br>(1.0) | 6.6***<br>(1.0) |
| Participants | 2,016 | 1,997 |
| House choices | 27,958 | 27,692 |

Note: Outcomes are gun selection (race-cue task) and same-race selection (gun-cue task). Gun selection is any gun versus No Gun. Model 2 adjusts for political condition, prevalence scenario, and participant covariates. Reference values are percentages; Δ values are percentage-point contrasts with the reference value. Unweighted LPMs; participant-clustered CR2 standard errors in parentheses. * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed).

**Table 5. Racial cues and selection of gun-owning neighbors**

| | Model 1 | Model 2 |
|---|---|---|
| Reference: White-labeled house | 43.7<br>(1.2) | 44.6<br>(0.9) |
| Δ probability of gun selection for a Black household | -3.1<br>(1.8) | -6.3***<br>(1.2) |
| Δ probability of gun selection for a Hispanic household | -3.4<br>(1.7) | -3.7**<br>(1.2) |
| Δ probability of gun selection for an Asian household | -0.2<br>(1.6) | -2.1<br>(1.3) |
| Participant covariates | No | Yes |
| Political and racial-prevalence conditions | No | Yes |
| Participants | 2,221 | 2,121 |
| House choices | 30,538 | 29,559 |

Note: Outcome is any-gun selection (versus No Gun). Reference values are percentages; Δ values are percentage-point contrasts with the reference value. Unweighted LPMs; participant-clustered CR2 standard errors in parentheses. * $p < .05$; ** $p < .01$; *** $p < .001$ (two-tailed).