What the Demolition of Public Housing Teaches Us about the Impact of Racial Threat on Political Behavior

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How does the context in which a person lives affect his or her political behavior? I exploit an event in which demographic context was exogenously changed, leading to a significant change in voters' behavior and demonstrating that voters react strongly to changes in an outgroup population. Between 2000 and 2004, the reconstruction of public housing in Chicago caused the displacement of over 25,000 African Americans, many of whom had previously lived in close proximity to white voters. After the removal of their African American neighbors, the white voters' turnout dropped by over 10 percentage points. Consistent with psychological theories of racial threat, their change in behavior was a function of the size and proximity of the outgroup population. Proximity was also related to increased voting for conservative candidates. These findings strongly suggest that racial threat occurs because of attitude change rather than selection.

One of the most significant demographic changes in United States history was the migration of African Americans from the South to northern and western cities in the mid-twentieth century. Scholars of the 1960s have claimed that the political reaction of urban whites to the influx of African Americans was palpable: previously apolitical individuals became politically activated (Edsall and Edsall 1992; Rieder 1985). For example, Edsall and Edsall (1992) documented racially liberal Paul Douglas's losses in white Chicago wards surrounding the expanding black ghetto. The segregationist George Wallace was relatively successful in these same wards. The implication of these observations is that the individual behavior of these white voters was conditioned by the context in which they lived (e.g., Key 1949; Putnam 2007). But how do social scientists know whether the context of surroundings really does condition behavior?

The study of individual behavior and geographic context is difficult for many reasons: inference can be dependent on the choice of unit of measurement, self-selection makes it difficult to identify a causal effect of context on behavior, and, even if a causal connection can be established, there is little agreement about the mechanism underlying this effect. Furthermore, long-standing research on racial politics argues that racial attitudes are highly stable and formed by early-life socialization (Henry and Sears 2002; Kinder and Sanders 1996), making it unlikely that behavior should be a direct function of racial context. In order to discover the influence of geographic context on individual behavior, these challenges should be addressed.

In this article, I exploit a rare event in which context is changed abruptly and exogenously. The demolition of 12 large public housing projects in Chicago, starting around 2000, removed roughly 25,000 people from the Chicago neighborhoods in which they had lived. Notably, nearly all of these families were African American. Several of these demolished housing projects were in close proximity to predominantly white neighborhoods. The demolition of these projects precipitated a large-scale demographic change to the surrounding neighborhoods. Because the decision to demolish these projects was outside the control of those who lived near the projects and because I test for a change in behavior before

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substantial resorting could take place, I am able to separate the behavioral effect of the demographic change from other preexisting influences and from self-selection in or out of the neighborhood. A comparison of voting rates and vote choice before and after the demolition of the projects provides a test of the influence of the housing project and its African American residents on the turnout and vote choice of nearby white voters. When African Americans are removed from the neighborhood, how do white voters respond?

After the demolition, voter turnout dropped by more than 10 percentage points for white voters living nearest to the projects. The change in turnout also varied by the size of the population that had been removed. The turnout of African Americans living nearby did not change. This result is maintained even when a number of alternative tests are considered. I also demonstrate that whites living near the projects had voted more conservatively than whites living farther away and that this difference disappeared after the removal of their African American neighbors. I argue that these results demonstrate that racialized political behaviors are highly context dependent and that white voters respond significantly to changes in the African American population over a period of just 4 years, suggesting that racial threat (Key 1949) findings are not driven by the selection of individuals into contexts. Furthermore, I argue that I isolate an effect of racial threat that is causally identified and is not subject to the usual risks to inference associated with the limited data used to study the effects of context.

Racial Threat and the Study of Context

Key's (1949) findings set the stage for a long line of research on the influence of racial context on behavior. Key found that, at the county level in the American South, white voter turnout and white vote for conservative politicians were correlated with the number of African Americans in the county. Key claimed that whites felt threatened by the presence of African Americans and, therefore, were more politically motivated. This relationship came to be known as racial threat. Since Key's initial study, there have been numerous observational studies by political scientists, economists, sociologists, and psychologists regarding both the attitudinal and behavioral manifestations of racial threat. Scholars have examined the effects of racial threat on voter turnout (Hill and Leighley 1999; Leighley and Vedlitz 1999; Matthews and Prothro 1963), candidate support (Carsey 1995; Enos 2010; Giles and Buckner 1993; Spence and McClerking 2010; Voss 1996), policy support (Glaser 1994; Hopkins 2010), racial attitudes (Bobo and Hutchings 1996; Fitzpatrick and Hwang 1992; Fossett and Kiecolt 1989; Gay 2006; Oliver 2010; Oliver and Mendelberg 2000; Oliver and Wong 2003; Quillian 1995; Sigelman and Welch 1993; Taylor 1998; Welch et al. 2001; Wright 1977), and social capital (Campbell 2006; Putnam 2007; Wright 2011). In short, studies of racial threat now include multiple behavioral outcomes, social groups, time periods, and comparative settings.

However, this long history of scholarship has often led to directly competing claims, often centered around the appropriate use of data and whether mechanisms appropriate for one context can be exported to another. Perhaps the challenges in the study of racial threat are best illustrated by the exchange between Giles and Buckner (1993) and Voss (1996). Giles and Buckner (1993) use aggregate election results and claim that proximity to African Americans at the county level made whites in Louisiana more likely to vote for the openly racist David Duke in the 1992 gubernatorial election because of the stimulation of "old-fashioned" racial stereotypes. Yet Voss (1996), using a different geographic aggregation in the same election, finds no relationship between African American proximity and Duke support. Voss argues that mechanisms of old-fashioned stereotype stimulation were inappropriate in the late twentieth-century South and that Giles and Buckner had aggregated data inappropriately. Other such contradictory findings are common in the literature. As summarized by Oliver (2010), the racial threat literature is characterized by "sharp divergences" (p. 14).

Challenges in the Study of Racial Threat

That it remains largely unsettled whether racial threat influences behavior speaks to the difficulties of studying context in general and racial threat in particular. These include limitations in data, identification, and theory.

The Causal Effect of Context

Sampson (2008) stated that "the specter of 'selection bias' has been raised to cast doubt on almost all observational research" (p. 191). But theories of context, such as racial threat, face particularly acute difficulties with selection. Individuals almost always have some degree of

autonomy about where they choose to be, so it is difficult to separate the causal effect of context from other variables that led the person to that context in the first place. It is easy to imagine individuals selecting where to live based on the demographics of their neighbors, and there are documented cases of this phenomenon (Tam Cho, Gimpel, and Hui 2013). Plausible alternative stories about selection can be mustered to support competing claims about racial threat. Relationships between racial diversity and positive outgroup attitudes can easily be attributed to self-selection because racially liberal individuals might select into racially diverse neighborhoods (Oliver 2010). However, relationships between racial diversity and *negative* outgroup attitudes can suffer from similar problems. For example, prominent theories argue that negative intergroup attitudes are a result of economic threat; that is, economic competition from members of an outgroup can lead to negative attitudes about the entire outgroup (Blumer 1958; Bobo 1983; Gay 2006). So if economically insecure individuals are more likely to select into racially diverse areas, like central cities, or if more economically secure individuals are more likely to select into more racially homogeneous areas, like suburbs (Massey and Denton 1993), then the relationship between a proximate outgroup and negative attitudes toward that outgroup would be spuriously caused by economic competition.

Generally, it is difficult for researchers to link attitudes to changes in a local population without risks of selection bias because it is very difficult to use surveys or other observational data to simultaneously track context and individual attitudes over time. To directly observe attitude change as a function of context, a researcher would have to measure attitudes both before and after population change. Because of these difficulties, researchers have limited knowledge of how sensitive individuals are to a changing demographic environment. In this study, I take a different approach: instead of measuring attitudes directly, I measure two behavioral correlates of attitudes identified in the literature—voter turnout and vote choice—and I link these to a rare large-scale population shift occurring over a short period.

Theoretical Challenges

Long-standing theories of racial politics in the U.S. provide good reason to believe that racial attitudes and related behaviors should not be sensitive to context, therefore casting doubt on the causal effect of racial threat and pointing instead to selection. Attitudes about racial groups, particularly white attitudes toward African Americans, are believed to be among the most stable in American politics and to result from early adult socialization (Henry and Sears 2002; Kinder and Sanders 1996). They have been demonstrated to influence a large range of policy attitudes (Gilens 1999; Tesler 2012) and voting preferences (Tesler and Sears 2010). Furthermore, recent research shows that the attitudes of Americans are generally insensitive to their local context, and, even in cases where local context is relevant, the effects are conditioned by an issues's national salience (Hopkins 2010, 2013). These challenges are buttressed by observational research that has explicitly rejected racial threat as a process driving racial attitudes and points instead to a process of residential sorting by education levels (Oliver and Mendelberg 2000).[1]

Nevertheless, some intergroup attitudes have been shown to be sensitive to changes in the levels of an outgroup in the population (Hopkins 2010; Newman 2012), suggesting that intergroup attitudes are malleable in the face of even very minor contextual change (Enos 2014). But it is not clear whether these findings are applicable to white attitudes caused by racial threat from an African American population: the findings of Hopkins (2010), Newman (2012), and Enos (2014) involve measuring responses to growth in Latino immigrant populations rather than attitudes about African Americans. Inferences drawn from changes in the Latino population may be uninformative because of the relative stability of attitudes toward African Americans compared to attitudes toward Latinos (e.g., Sidanius et al. 2008) and because African Americans are a relatively demographically stable population, meaning that the psychologically important condition of changes to a population's status quo is lacking. And, except in the case of a highly localized and short-term field experiment by Enos (2014) and other laboratory experiments (Kurzban, Tooby, and Cosmides 2001), most of these studies are not able to separate attitude change from population replacement as the causal force.

The myriad of proposed mechanisms for racial threat has also created confusion. An incomplete list of proposed mechanisms includes rational responses to material threat (Bobo 1983), competition over descriptive representation (Spence and McClerking 2010), stimulation of old-fashioned racial stereotypes (Giles and Buckner 1993), manipulation of fear by interested elites (Key 1949), and preservation of "white power" (Voss 1996). The mechanisms can broadly be grouped into

[1]Tesler and Sears (2010), in their account of the effect of racial attitudes in the 2008 presidential election, also reject racial threat as "outdated" (p. 170).

instrumental mechanisms, such as competition over representation, and *psychological* mechanisms, such as the stimulation of stereotypes.

Instrumental and psychological mechanisms are not mutually exclusive, and instrumental mechanisms are often consistent explanations for behavioral responses to the presence of an outgroup. However, such mechanisms are sometimes implausible, leaving psychological mechanisms as the best explanation. For example, in large electoral districts, an individual voter is often an inconsequential part of the electorate, which makes voting in response to an outgroup difficult to explain instrumentally (Downs 1957; Olson 1971). In other situations, the outgroup is too small to influence election outcomes. Furthermore, many studies have found that Americans are largely innumerate about the demographics of their community (Alba, Rumbaut, and Marotz 2005; Gallagher 2003; Martinez, Wald, and Craig 2008; Nadeau, Niemi, and Levine 1993; Sigelman and Niemi 2001; Wong 2007), making it difficult for voters to respond instrumentally to outgroup size.

Of course, even when there is no clear individual instrumental motivation, famous examples of racial threat behavior can be found. Perhaps the most prominent example is Key (1949), where whites were thought to be motivated to vote by the threat from African Americans, who were, for all practical purposes, disenfranchised. However, given the limited data and identification strategies available to Key (1949) and other scholars, it is not clear whether behavior in these examples should be attributed to racial threat or to the data limitations I turn to next.

Challenges from Geographic and Aggregate Data

The problem of scale also often threatens inference in studies of racial threat: Researchers sometimes choose geographic units out of convenience because theories of context are often silent with respect to scale. Often, there is only data for administrative units, such as census tracts. These units may have no relevant social or political meaning, and the correlation of an areal unit with individual behavior may change with the unit chosen by the researcher. For example, research on racial homogeneity and voter behavior has been modeled as being dependent on the racial composition of the state (Leighley and Nagler 1992), county (Giles and Buckner 1993), ZIP code (Leighley and Vedlitz 1999), and census tract (Putnam 2007). While these geographies can be important, there is little reason to suppose that living in a diverse state has the same effect as living in a diverse ZIP code. Researchers trying to measure the same effect can therefore reach very different conclusions depending on the choice of unit. A related difficulty is the modifiable areal unit problem (MAUP), that is, the possibility that the sometimes arbitrary boundaries of some areal units, such as census tracts, can greatly affect estimates of underlying population parameters. Both MAUP and the problem of scale lead to the same potential difficulty with inference: When using aggregate data to measure an underlying population parameter, the choice of the areal unit can matter as much as the underlying variation in population characteristics. This is generally a problem for any study of context that uses predefined boundaries. Indeed, it is possible that much of the inconsistency in the previous literature on racial threat is due to variation in scale across authors and to MAUP (see Tam Cho and Baer 2011; Voss 1996).

The Contribution of This Study

Using individual geocoded data measuring behavior at different points in time, this study identifies an effect of racial threat, while being relatively free of the risks to inference from problems of scale or MAUP that are usually associated with aggregate data and also free of the problems with causal identification often associated with observational data. I argue that the effect of racial threat is identified by relying on the exogenous nature of the removal of the African American population caused by the demolition of public housing in Chicago and by testing the difference-in-differences in voter turnout between sets of voters before and after the demolition. These results demonstrate that racial threat, unlike the stability of some intergroup attitudes, is highly context-specific. The substantial change in behavior suggests that racial threat does not arise because of the selection of populations into specific contexts, but rather in direct reaction to living near the outgroup. I also demonstrate a situation in which instrumental incentives are likely not the mechanisms causing racial threat behavior, suggesting that racial threat can result from the psychological salience of the outgroup caused by proximity.

A notable feature of this study's design is that it measures the effects of the removal of a threatening outgroup, rather than the usual design of measuring the difference in levels of the outgroup or increases in the local presence of an outgroup. Do theories of racial threat imply that individuals' response to population decreases should be similar to their response to increases or differences in levels? Scholars have been silent on this explicit question, but there is good reason to believe that the various mechanisms behind racial threat imply that a population

decrease will result in behavioral change. Taking a politically instrumental approach, if the local outgroup population decreases, then that group's local electoral impact is also likely to decrease, thereby decreasing the individual utility of voting motivated by the presence of the outgroup. From a more psychological point of view, in which local outgroup proximity is related to the psychological salience of that group and thereby stimulates behavior, a reduction in the group's local population should also reduce the group's salience. In either case, these theories imply that the reduction in the local outgroup should cause attitudinal and behavioral change.

Chicago Public Housing

Since 1999, the Chicago Housing Authority (CHA) has relocated thousands of families through the process of destroying and reconfiguring its massive system of public housing in the city.

Prior to 2000, when widespread demolition began, the CHA was the second largest public housing agency in the United States, controlling over 2,800 properties.[2] The great majority were considered "scattered-site" units consisting of a single building, unattached to other public housing. However, there were 83 large multibuilding properties that, collectively, housed tens of thousands of families. All of the families in the large mutlibuilding properties were low income, and the overwhelming majority were African American. Of the demolished projects for which data are available, the average racial composition was 99.7% black. Most of the housing was on the city's South and West sides. Chicago being one of the most segregated cities in the United States (Massey and Denton 1993), most of the neighborhoods containing housing projects were overwhelmingly African American or Latino. However, there were projects near predominantly white neighborhoods.

The CHA's 1997 "Plan for Transformation" designated certain low-income housing units as requiring demolition. The guidelines for requiring demolition were set by the U.S. Department of Housing and Urban Development (HUD), in Washington, DC. The process was, more or less, decided exogenously to the particular neighborhood. Rules deciding demolition were based on algorithmic measures of the size of facilities and levels of decay that were outside of the residents' control (CHA 2000). The key assumption in this article is that the choice of units designated for demolition is uncorrelated with the difference in changes in turnout for white and African American voters. In the supporting information, I give more details about the process of selecting projects for demolition, including the balance on pretreatment covariates for the areas surrounding housing that was demolished and housing that was not demolished.

Some housing was reconstituted, and some was entirely demolished. While almost all of the reconstruction required the displacement of residents, the units requiring demolition were overwhelmingly the large high-rise, multibuilding complexes that had become notorious for poor living conditions. The destruction of these units changed the demography and density of their neighborhoods. Twelve CHA projects were completely or partially demolished between 2000 and 2004, displacing over 25,000 people. In some neighborhoods, such as the near Northside neighborhoods surrounding the Cabrini-Green project, this displacement caused an extreme change in the presence of African Americans near white voters because most of the nearby African Americans were concentrated in the projects.[3]

Data

To execute this study, I obtained four atypical data sources. First, I obtained the 2004 Illinois voter file and augmented it with demographic data from the 2000 and 2010 Census counts. Second, I geocoded the residences of the approximately 1.2 million voters in Chicago and determined their distance from each of the demolished public housing projects using a Geographic Information System (GIS). Third, I was able to identify the exact distance of each voter to the edge of a housing project using data on the two-dimensional spatial boundaries of the housing projects. The edges of the projects, rather than just the centroid, are crucial data because some housing projects were very large, covering many city blocks. Fourth, I collected a unique data set of property records, including homeownership data and home values, for all Chicago voters. I also identified each voter's race using a Bayesian process based on the voter's name and location. Election returns and precinct GIS data were also obtained from the Chicago Board of Election Commissioners. In the supporting

[2] Sixty-nine of these units were senior housing.

[3] Actually, the demographic change was probably more dramatic than even the 25,000-person displacement because CHA resident counts do not include the thousands of homeless squatters and residents illegally living with legal residents. Some estimates have put the proportion of occupied units that were illegally occupied at as high as 50% (see Cunningham et al. 2005; Kotlowitz 1992).

information, I describe how the data were processed prior to analysis.

These data sources provide many advantages unavailable to previous studies of racial threat. Because I am using a voter file, I can examine the individual voter turnout of the universe of voters in Chicago; thus, my analysis of turnout avoids both the ecological assumptions common to aggregate data and the sampling assumptions common to survey data. Because I can identify the location of each voter, instead of being tied to data from administrative units, I can test for an effect of the treatment using a variety of definitions of the relevant context. This means my analysis is not confined to a certain scale of measurement available in a given survey, such as a congressional district. Finally, I was able to augment the data in ways that greatly helped with inference. Even if a researcher has access to individual-level data from a voter file, the data often lack key variables, such as race and homeownership. Because I imputed race and collected a unique data set of property records for all Chicago voters, I can control for the effect of race and homeownership on voting.

Design

The tearing down of the large-scale public housing projects can be thought of as a quasi-experiment. The treatment is the demolition, and the outcome is the change in white political participation and support for conservative candidates. I measure the effects by measuring changes in presidential election turnout between 2000 and 2004. Isolating the effects on behavior implied by racial threat requires elections in which voting behavior is unlikely to have been motivated by local issues or candidate contests that could turn in part upon the presence (or absence) of the projects or their residents. Because the projects were not at stake in the presidential election, it is unlikely that voters voted with the intention of influencing the future of the housing projects. I then use a series of elections between 1996 and 2008 to measure effects on vote choice.

I will test the following hypotheses derived from theories of racial threat:

> **H1 (Racial Threat and Turnout):** After the demolition of the projects, turnout should decline for white voters close to the projects relative to the rest of the city.
> **H2 (Proximity and Size):** The salience of a group is a strong predictor of intergroup attitudes (Brewer and Miller 1984). Psychologists have empirically demonstrated the intuitive finding that salience can be a function of the size and "immediacy" of an object (Latané 1981; Latané and L'Herrou 1996; Latané et al. 1995; Lewenstein, Nowak, and Latané 1992; Latané and Wolf 1981). This leads me to expect a "dose effect," whereby the treatment should vary with the size and proximity of the treatment. Operationally, the treatment effect should decline as the white voters are farther away from a project and as the population of a project represents a smaller portion of the local outgroup population.[4]
> **H3 (Racial Threat and Vote Choice):** After the demolition of the projects, white voters close to the former projects should experience a decline in racially conservative voting relative to the rest of the city.

Identifying the Race of Voters

A key variable in this analysis is race. I must differentiate between white, black, and other voters. I use a method I developed that is described in Enos (2012). I estimate the race/ethnicity of voters by combining census demographics with name frequencies, which yields a probabilistic estimate of an individual's race. Because Chicago, like most large U.S. cities, is hypersegregated (Massey and Denton 1993)—that is, because so many census blocks are either overwhelmingly black or overwhelmingly white—I can make very certain predictions about the racial identity of many voters. I use the notation $p(race|name)$, where, in this case, *race* is *white* or *black*. This is shorthand for $p(race|\ name\ and\ location)$ because the estimate is a function of both name and location. Details of the estimation process are in the supporting information. An alternative strategy of using only perfectly homogeneous census blocks, so that there is no probabilistic element, yields substantively similar results.

Estimation

I want to measure the change in voter turnout for white voters who were treated by living near demolished public housing projects. The treatment is the demolition and

[4]The size of and distance from a project could also generate predictions via a different mechanism: close proximity and a larger population could lead to more interpersonal interaction, potentially reducing intergroup threat as classically predicted by Allport (1954). If this interaction does occur, it would likely counterbalance some of the effect of increased psychological salience. In the conclusion, I discuss why segregation likely made this sort of mixing unlikely.

the treatment group is white voters who lived nearby, whereas the control group is made up of white voters who lived farther away. In the most simple terms, the effect of the treatment is the difference between the mean turnout at times t and $t-1$ for white voters close to the demolished projects relative to white voters not close to the demolished projects. I have to choose a distance from the housing project for which to measure the treatment effect. Call this distance d^*. If each voter lives d distance away from the nearest demolished project, then white voters for whom $d \leq d^*$ are the treatment group. Voters for whom $d > d^*$ are the control group. By taking the difference in turnout between t and $t-1$ for the control group, I account for the average change in turnout across the city. This is the change in turnout that was experienced, on average, by everyone in the city and is not attributable to racial threat. I subtract this control group difference from the treatment group's difference in turnout at t and $t-1$. This difference-in-differences yields the average treatment effect (ATE). With t as 2004 and $t-1$ as 2000, the equation is

$$ATE = [P(Vote_{2004}|d^* \leq d) - P(Vote_{2000}|d^* \leq d)] - [P(Vote_{2004}|d^* > d) - P(Vote_{2000}|d^* > d)] \quad (1)$$

This is a very straightforward test: Relative to the change in voting in the rest of the city, did white voters close to the projects vote more or less after the projects were demolished? If they voted less, then this might be attributable to a reduction in racial threat. This is a simple, nonparametric test that relies on no modeling assumptions. A second important test, which serves as a placebo, is to see whether African American voters behave similarly. If African American voters changed their voting behavior in a manner similar to white voters, it is unlikely that the behavioral change was due to the removal of racial threat.

Equation (1) is a difference-in-differences estimator. This estimation technique eliminates bias from unobserved differences in treatment and control by differing them away. The implicit assumption is that in the absence of treatment, the unobserved differences between the treatment and control groups would be the same over time. I establish the validity of this assumption by performing a parallel trends test to demonstrate that prior changes in voting were similar for white and black voters in both treatment and control. Furthermore, the difference-in-differences estimator helps eliminate bias from possible sources of measurement error on the dependent variable. In analyzing turnout, I use the same individual voters before and after the housing projects were demolished, only counting voters in the treatment or control group if they were in the group in 2000 and in 2004.[5] However, there is almost certainly error in the records; for example, a voter may move between 2000 and 2004, but her registration at the previous address may not be purged from the voter file, giving the impression that she has stopped voting when, in fact, she has simply moved. The difference-in-differences estimator accounts for this sort of measurement error because for the error to cause bias, it must be correlated with the difference in the change between the treatment and control groups. In the following section, I also estimate a difference-in-differences where the treatment group is white voters living near demolished projects and the control group is black voters living near demolished projects. Racial threat theory predicts that the difference should be more negative for white voters than for black voters. For errors caused by voters who have moved to cause bias, yielding a false positive on racial threat, white voters must be moving away and causing errors on the voter lists at a faster rate in the period between 2000 and 2004 than black voters.[6]

Also, in estimating Equation (1), I have to make two decisions about what data to include: the distance d^* at which to define a control group and how to probabilistically define race based on $p(race|name)$. In both cases, the large amount of data allows for flexibility so that my estimates can be tested across a range of choices. There is no obvious choice of cutoff distance, d^*, so I look at d^* at increasing distances from 100 meters to 1 kilometer from the projects.[7]

[5] This feature of the design also alleviates concerns about bias caused by the type of voter who may have moved close to or away from the demolished projects prior to or after demolition. For example, if relatively young white voters moved into the areas near the demolished projects in anticipation of the "gentrifying" of the area, these young voters may be less likely to vote, and overall turnout would decrease. However, because my comparison is only between voters present in 2000 and 2004, these newly arrived voters do not bias the estimation.

[6] Given that the demolition of the housing projects may have made the surrounding areas more appealing for many white residents, white voters moving away at a greater rate than black voters seems unlikely.

[7] It is my opinion that, in terms of everyday human interactions, 1 kilometer is a considerable distance in an urban area. Looking at a map of Chicago, a relatively high-density city, moving 1 kilometer can take a person through sociologically very different neighborhoods. Generally speaking, it seems that 1 kilometer^2 is beyond what a typical person would consider his or her neighborhood. In fact, political science research often uses geographies like a census tract or block group to approximate a neighborhood. If these are good approximations of a neighborhood, then a 1 kilometer^2 area can be far larger than a neighborhood. In Chicago, moving across 1 kilometer would take a person through several block groups and,

I also have to decide how to define a voter's race based on my probabilistic estimates described above. For example, to identify a voter as white, do I only include voters for whom the estimated $p(white|name) = 1$ (generally voters from perfectly homogeneous census blocks), or do I allow for voters with a lower probability of being white? The lower $p(race|name)$, the larger the sample size, but the greater the chance that my estimates were contaminated by misidentification of the voter's race. I choose a threshold of .975. In the supporting information, I test my findings across a number of probabilities, from .91 to 1, which yields consistent results.

Results

In Figure 1, I display the results of a difference-in-differences test for white treatment groups minus white control groups (white circles) and for black treatment groups minus black control groups (black circles). The differences are the average turnout in 2004 minus the average turnout in 2000. I define the treatment group at increasing distances from the projects (d^*) in 100-meter increments. Vertical lines represent the 95% confidence intervals from bootstrapped standard errors of the difference between treatment and control groups.[8] The number of subjects (N) in each treatment group is in parentheses. The control N is all white (black) voters outside the distances representing the treatment group, so these groups are always quite large, being made up of tens of thousands of voters. Voters in this and subsequent tests are included if $Pr(race|name) > .975$.[9]

The ATE is negative and substantively large for the white treatment group. At $d^* = 100$, the effect is 13.4 percentage points, meaning that after the demolition of the housing projects, the turnout of white voters living near the projects declined by 13.4 percentage points relative to the voters living farther away. This effect becomes smaller as d increases, dropping to 10.7 percentage points when $d^* = 500$, which is consistent with diminished pretreatment salience of the outgroup and, therefore, with a diminished effect of their removal. These results are consistent with Hypothesis 1.

The treatment effect for the black treatment group is relatively small and shows no sensitivity to d. This effect is important for establishing the mechanism of the change in behavior. If the effect were caused by some reason other than the removal of the threatening outgroup, such as a change in the location of a polling place or a reduction in criminal activity associated with the projects, we should expect to see the same effect on African Americans. This test also speaks to alternative explanations about changes in elite behavior causing the observed changes in white turnout: If white voters reduced their turnout because of reduced campaigning, for example, then a similar change should be present for blacks.

In Figure A.2 in the supporting information, I demonstrate that treatment and control groups, both black and white, had parallel trends in turnout between 1996 and 2000, validating the assumptions behind the difference-in-differences estimator.

In Figure A.3 in the supporting information, I display estimates similar to those in Figure 1, but with voters matched on covariates. The process yields results very similar to those in Figure 1.

Robustness Checks

I now undertake robustness checks by defining control groups in such a way that if the mechanism was something other than racial threat, there should be a similar behavioral change in the control group.

Matching with White Voters Near Nondemolished Projects. First, I match the treatment group with a group of white voters living near public housing projects that were not demolished between 2000 and 2004. If the change in behavior was caused by some factor associated with living in areas near housing projects other than the removal of these white voters' African American neighbors, then we should expect similar behavior among white voters for whom the nearby projects were not demolished. White voters living the same distance from nondemolished projects are matched with the treatment group using all variables available in the voter file: gender, age,

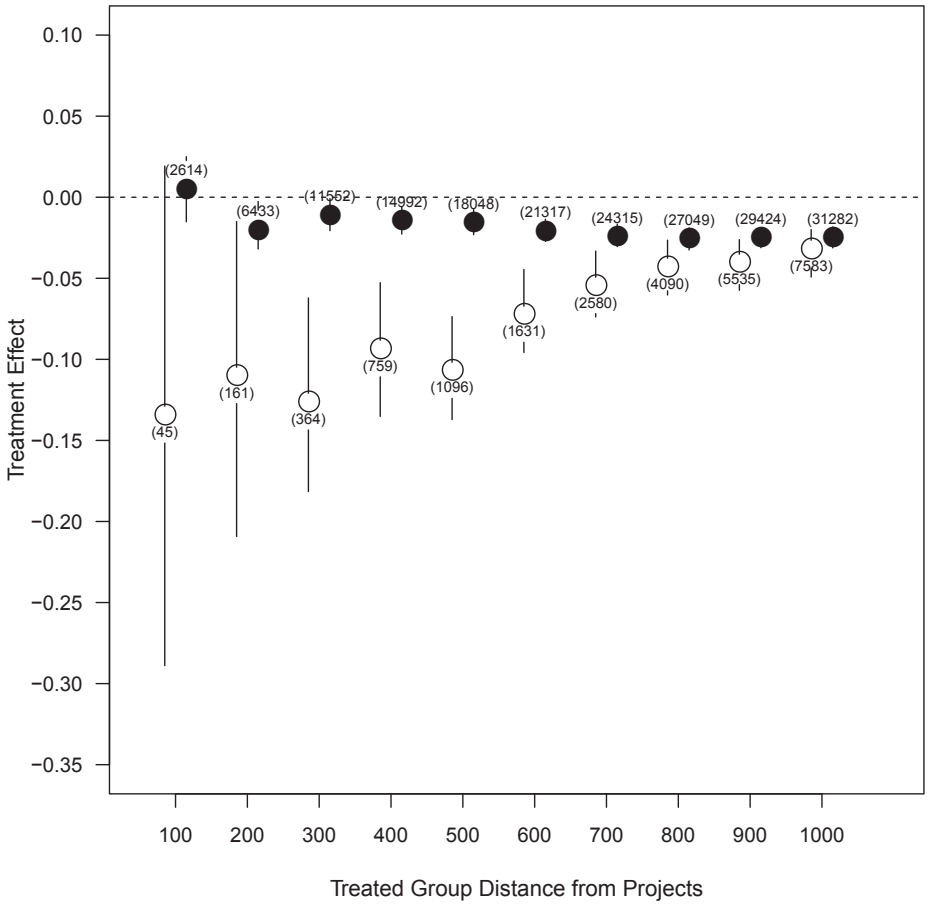
potentially, several census tracts. In this sense, it would not be surprising if the treatment strength varies significantly over 1 kilometer.

Throughout this article, I measure d using the shortest distance between two points, rather than driving or walking distance. This is consistent with other recent studies, where intra-urban distances are used in analysis (Brady and McNulty 2011) and seem to be the most appropriate measure of distance for the study of racial threat when the salience of the outgroup is affecting behavior.

[8]See Bertrand, Duflo, and Mullainathan (2004) for why bootstrapped standard errors are desirable for difference-in-differences estimators.

[9]The number of voters available for this study is 848,066 (see supporting information for a description of data processing). The size of the sample used for each test varies depending on $Pr(race|name)$ and other factors.

FIGURE 1 Treatment Effects



Note: Difference-in-differences results for treatment groups defined by increasing distance from the demolished projects. Differences are for the mean turnout in 2004 minus the mean turnout in 2000 for the treatment group minus the same difference for the control group. White circles represent the mean effect on white voters; black circles represent the mean effect on black voters. The N in each treatment group is in parentheses next to the mean effect. Vertical lines represent the 95% confidence intervals generated by bootstrapped standard errors of the difference between treatment and control.

and party registration (defined as Democrat, Republican, or Independent).[10]

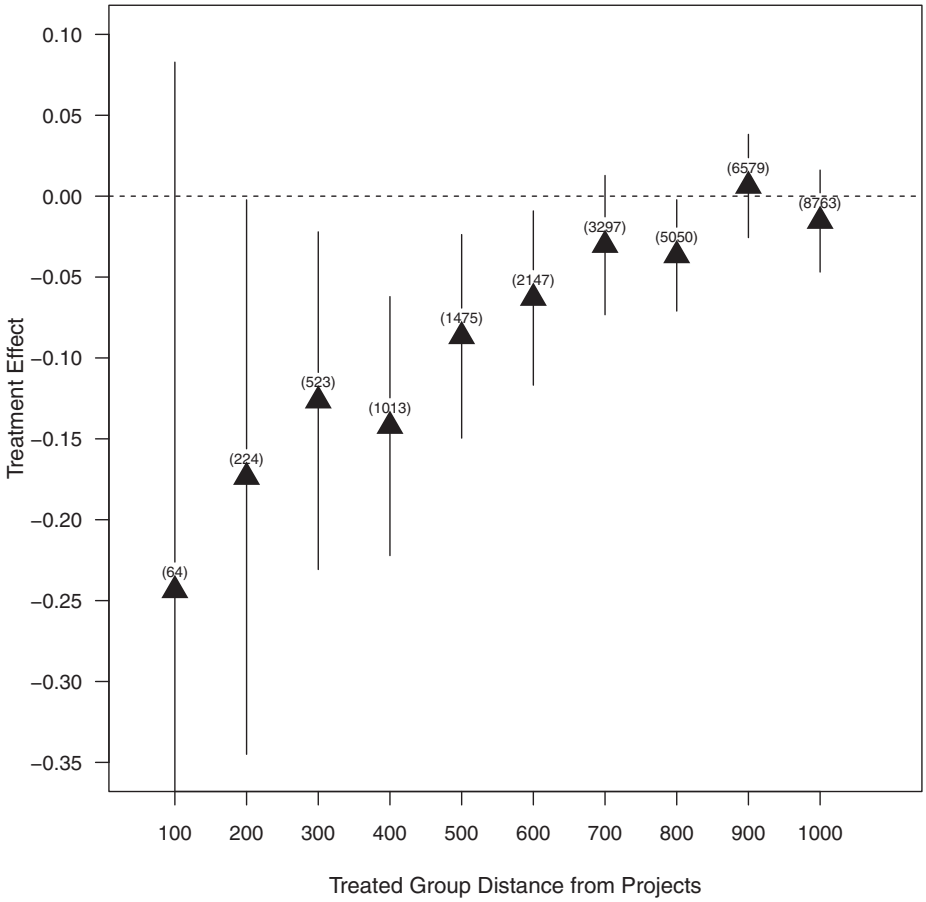
I also match on the income of the voters' census block groups after matching their home addresses with census records. Using nearest neighbor matching yields a control group of the same N as the treatment group. With this test, the white voters in the treatment group are matched with other white voters who are demographically similar and also living near housing projects prior to the treatment, the only difference being that the housing projects were not demolished for the control group.

After matching, I use ordinary least squares (OLS) to regress change in turnout on a dummy variable for treatment (living near a demolished project) and the variables used in matching. Figure 2 is a display of the treatment effect when estimated using these two matched groups. The coefficients estimated by OLS regressions of turnout on treatment and control variables are represented by triangles. These estimates again show a substantively large ATE and the same pattern of decreasing effects with increasing d.[11]

[10]Party on the Illinois voter file represents whether the voter previously participated in a party primary. If a voter has not voted in a Democratic or Republican primary election, he or she is an Independent. Of course, these variables could also be controlled in a regression, but matching reduces reliance on modeling assumptions. I use the software and method described in Ho et al. (2007).

[11]Matching black subjects living near demolished projects with black subjects living near nondemolished projects yields differences in turnout close to zero, as would be expected if the black voters were unaffected by the demolition of the projects.

FIGURE 2 Treatment Effects Using Matched White Voters Near Nondemolished Projects for Control Group



Note: Coefficients on treatment as defined by increasing distance from the demolished projects from OLS regressions on change in turnout from 2000 to 2004 (triangles). N for the regression using matched groups is next to the point representing the coefficient. The treatment group is matched to a control group of white voters living near projects that were not demolished, using nearest neighbor matching. Regressions include variables used in matching as controls. Vertical lines represent the 95% confidence intervals generated by bootstrapped standard errors on the treatment coefficient.

In the supporting information, I display a number of variations on this same test, all of which yield similar results. Results are substantively the same when estimated using a difference-in-differences estimator, an OLS regression with no control variables, or a logit estimator. This is also true of all subsequent analyses in this article.

A Matched Black Control Group. I also match the white treatment group with a black control group. This tests whether black voters living near the housing projects, even if demographically similar to whites, reacted differently than white voters did to the removal of their black neighbors.

A concern with using the same matching algorithm used to match white voters with other white voters is that using census block group income data as a proxy for individual income is inadequate for measuring the differences in income between whites and blacks. One reason for this might be racial disparities in wealth, associated with—among other factors—disparities in homeownership rates between whites and blacks with similar incomes (Oliver and Shapiro 2006). It is particularly important in this study to account for differences in homeownership because homeownership and home values have been shown to be related to voting behavior (Fischel 2001). If the demolition of the projects affected home values, that could have led to a change in voting behavior through mechanisms other than the reduction in the salience of the outgroup.

In order to control for this possibility, I collected data on homeownership and home values (in dollars) from the Cook County [Illinois] Registrar. Names and addresses of

voters on the voter file were matched with deeds and data on homeowner tax exemptions. Details on this process are in the supporting information. The data on voting and homeownership come from two different administrative sources, so there is error in matching records and the N is reduced.[12]

With this process, I have a group of black voters who are nearly identical to the white voters in terms of party identification, gender, age, homeownership status, value of their property, and (importantly) location. In a sense, these white voters are being matched with their geographically nearest black neighbors, who are also voters, share similar demographic characteristics, and have a similar value invested in their homes.

In Figure 3, I display the results of coefficients from OLS regressions with a treatment group defined by a dummy variable for a white voter. The same basic pattern remains in the data, although at $d^* = 100$, with reduced N (an equal number of white and black voters), the point estimates (triangles) are smaller.

These coefficient estimates may be considered estimates of the effect of racial threat on voting.[13] Presumably, the African American voters were not racially threatened by other African Americans, so the change in their turnout between 2000 and 2004 is what the secular change in the matched white sample would have been, if it were not for the treatment of the removal of the threatening outgroup. These coefficient estimates are quite large, the effect being over 15 percentage points even at $d^* = 500$, and can be quite confidently separated from a null effect. This means that for white voters within 500 meters of a housing project, had the projects not been demolished and the threatening outgroup removed, the turnout would have been 15 percentage points higher in 2004.

In the supporting information, I again display a number of variations on this same test, all of which yield similar results.

Estimating the Impact of Size and Distance on Racial Threat

I have established that the basic difference-in-differences estimate between white treatment and control is strongly negative and that this estimate is different from the difference-in-differences estimate for African Americans. The estimate is robust to matching with other whites close to nondemolished projects and to matching with African Americans. This all points to a strong effect of the removal of the outgroup and subsequent diminished threat. With this established, I now test Hypothesis 2 by estimating the effects of the size and proximity of the outgroup on turnout. These two variables are predicted to affect the psychological salience of the outgroup and thereby stimulate racial threat.

Using white voters with $Pr(race|name) > .975$ yields N = 113, 850 subjects. I regress turnout in 2004 on $log(distance)$ from the demolished projects, the logged percent of the local African American population living in the demolished projects,[14] and individual turnout in 2000. The coefficients estimated by OLS are displayed in Table 1.[15] Using these coefficients, I simulate the probability of voting in 2004, conditional on having voted in 2000, as the distance from the project becomes larger and the percent of the local African American population in the project becomes larger. The prediction from racial threat theory is that the size and proximity of the outgroup should have been significant motivators for white voters prior to the demolition, so after the demolition there should be a significant decrease in participation for

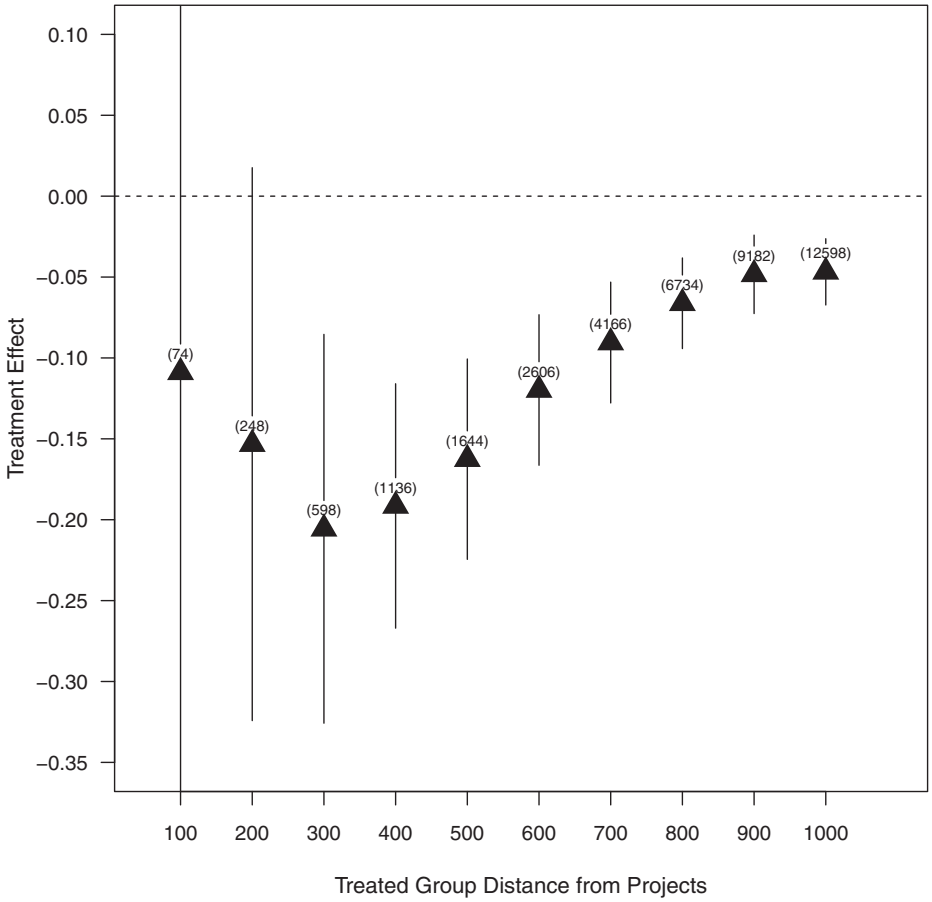
[12]Homeownership and home values can be defined in different ways, described in the supporting information, and the results are robust to using all alternatives. All results presented in this article are also robust to an inclusion of ownership and property value variables, although, in every case, the sample N is reduced by discarding cases that cannot be matched between the voter file and registrar data. In many cases, the ATEs are larger than when homeownership is not included. I present these results in the supporting information.

[13]However, it should also be noted that homeownership might be considered posttreatment because, arguably, race often affects homeownership. Strictly speaking, in such a framework, the effect of race and of the demolition of the housing project on voting is biased if income is in the model. It is notable that, as demonstrated in the basic difference-in-differences design, the different behavior of black and white voters is robust to inclusion or exclusion of homeownership in a model. Also, in this study, I am less interested in estimating the causal effect of the racial identity of the voter and focus rather on black voters as a plausible and important control group with which to compare the white treatment group of interest.

[14]The local African American population is represented by the total number of African Americans within1 kilometer of the project in 2000. The population of the housing projects is represented by the African American population of the census blocks containing the projects (these census blocks are usually contiguous with the projects). The percent of the local African American population living in the demolished projects, is these two quantities expressed as a proportion. For demolished projects this variable is $mean = .16, median = .08, minimum = .004, maximum = .85$. Representing the local African American population using a smaller area (< 1 km) yields similar results. Using non-log-transformed variables also yields similar results. An alternative measure using the physical size of the demolished projects as a proxy for population size yields similar results.

[15]These estimates are substantially unchanged by including controls for property, distance from nondemolished projects, and percent of local black population in nondemolished projects, and fixed effects for the closest project.

FIGURE 3 Treatment Effects Using Matched Black Control Group and Controlling for Homeownership



Note: Coefficients on treatment as defined by increasing distance from the demolished projects from OLS regressions on change in turnout from 2004 to 2000 (triangles). N for the regression using matched groups is next to the point representing the coefficient. The white treatment group is matched to a black control group of the same N using nearest neighbor matching and including variables on homeownership and home value. Regressions include variables used in matching as controls. Vertical lines represent the 95% confidence intervals generated by bootstrapped standard errors on the treatment coefficient.

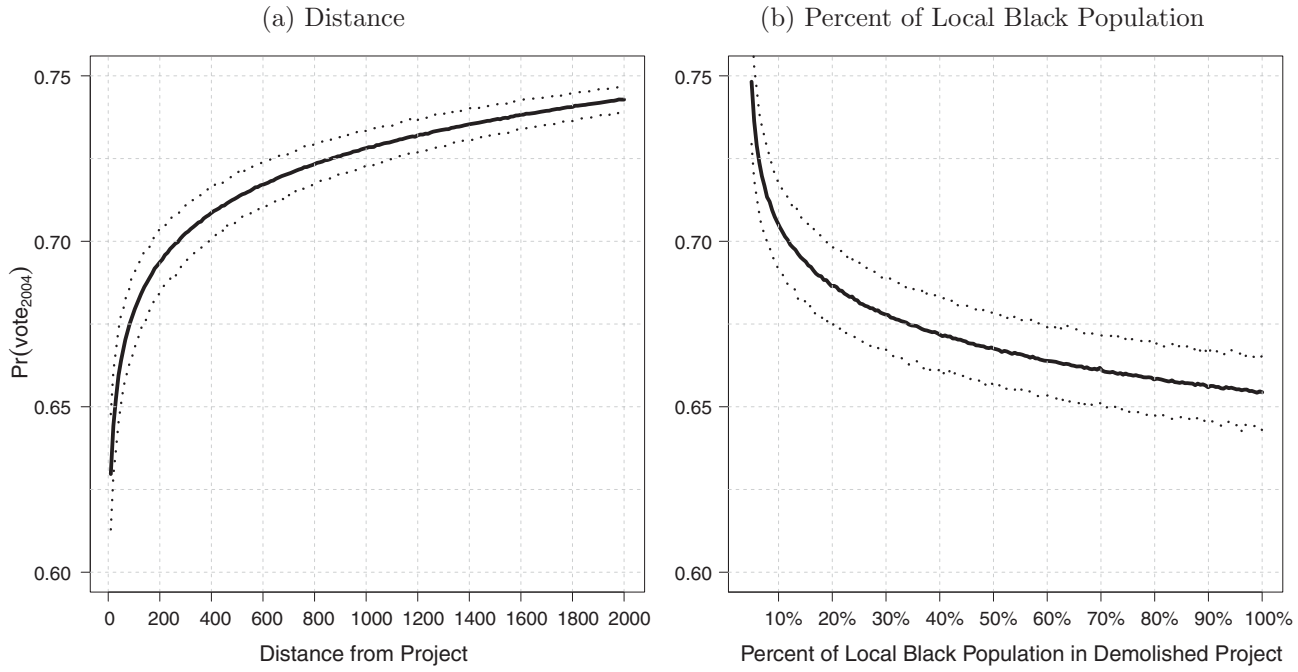
voters living close to the demolished projects and near demolished projects with a relatively large population.

The predicted effects of distance and population size on voting are displayed in Figure 4. These simulations of voter turnout in 2004 based on size and distance from the demolished projects, conditional on having voted in 2000, support Hypothesis 2. Between 2000 and 2004, turnout for the average white registered voter in Chicago declined (see Figure A.2 in the supporting information). Figure 4 reflects voters near the demolished projects decreasing their turnout at a faster rate. Figure 4(a) demonstrates that for a person already inclined to vote, the probability of voting increases with distance: by almost 10 percentage points when moving 500 meters away from the demolished projects. This indicates that persons living near projects were significantly motivated by their proximity to the projects when the projects were still standing. Similarly, Figure 4 (b) shows that as a person already inclined to vote moves from living near projects representing a small portion of the local population to living near projects representing a large portion of the local population, her probability of voting decreases, indicating that, for white voters living nearby, the relative size of the local outgroup had a significant effect on voter turnout.

Effects on Vote Choice

Scholars have observed correlations in a variety of settings between proximate outgroups and voting for racially

FIGURE 4 Effects of Distance and Size of Projects



Note: Predicted effects generated from $vote_{2004} = \beta_0 + \beta_1(log(distance)) + \beta_2(log(localpercent)) + vote_{2000}$, with white voters. Figure 4(a) is the predicted probability that a person who voted in 2000 will vote in 2004 with increasing distance, while holding size at its mean. Figure 4(b) is the predicted probability that a person who voted in 2000 will vote in 2004, with increasing outgroup population size, with *distance* = 100. Dotted lines represent 95% confidence intervals generated by bootstrapped standard errors.

conservative candidates or against candidates perceived as representing the outgroup (Carsey 1995; Enos 2010; Giles and Buckner 1993; Key 1949; Spence and McClerking 2010). An observable implication of my claims about the effect of racial threat on voting is that the removal of the outgroup might produce changes in voters' propensity to vote Republican. With this claim, I am relying on substantial evidence that more racially conservative voters prefer Republican candidates (e.g., Tesler and Sears 2010). My prediction in Hypothesis 3 is that the demolition of the housing projects should lower the proportion of white voters living near the projects who vote for Republican candidates.

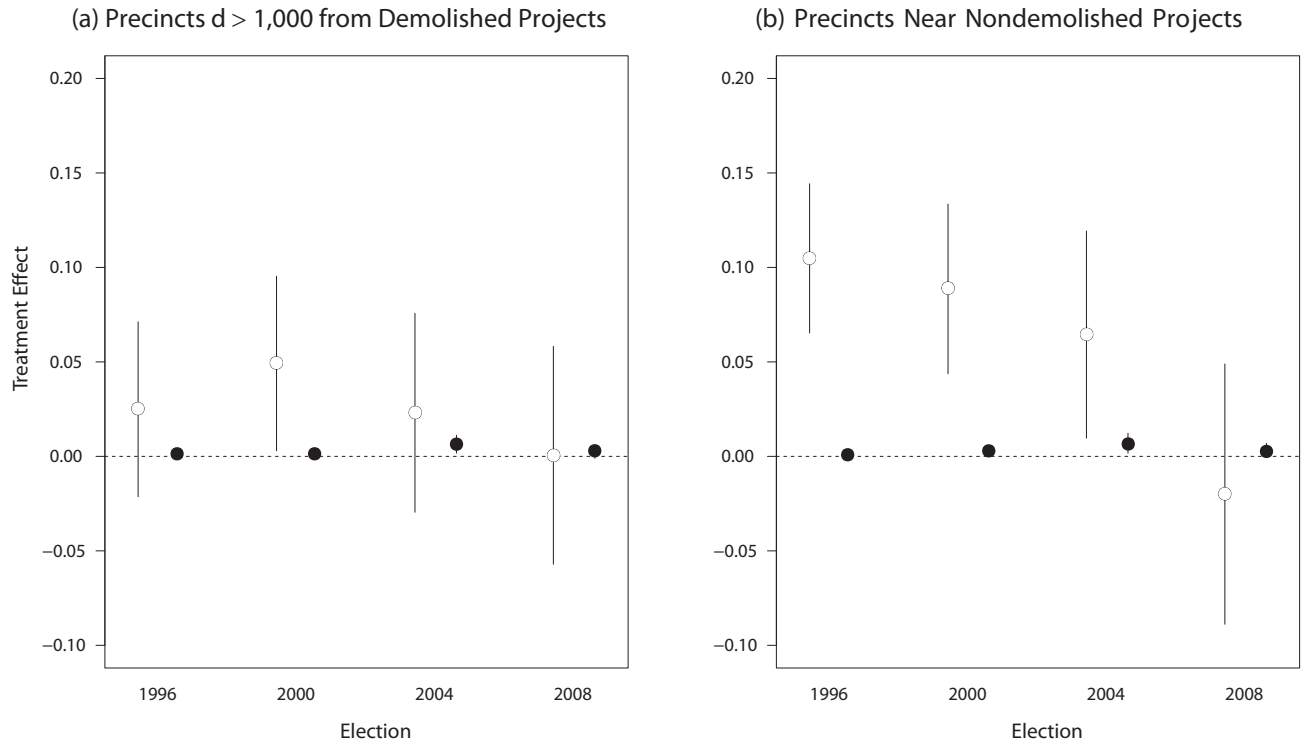
To test this, I estimate the vote for Republican presidential candidates at the precinct level among white and black voters from 1996 to 2008, using King's method of ecological inference (King 1997). I then take the precincts within 1,000 meters of the demolished projects and use census income for whites and blacks to match each precinct with a similar precinct more than 1,000 meters from the projects. Matching is done separately for whites and blacks, so that I am left with a white treatment group for which $d \leq 1{,}000$, a white control group, matched on income, for which $d > 1{,}000$, and complementary treatment and control groups of black voters. This yields 102 white precincts and 150 black precincts.[16] I then separately calculate a difference of means between treatment and control for whites and blacks, weighted by the population of the group in each precinct. The black voters again serve as a placebo: if white behavior is modified by racial threat, we should not see similar behavior in blacks.

Because the unit of analysis is the precinct rather than the individual, this analysis of vote choice has a number of limitations relative to the analysis of voter turnout: the baseline vote percentages are ecological estimates, the N is smaller, and, importantly, the precincts were redistricted between the 2000 and 2004 elections, so a difference-in-differences between elections is not possible. However, keeping these caveats in mind, a comparison of aggregate evidence across elections is still informative when examined in conjunction with the individual-level turnout data.

In Figure 5(a), I display these differences of means for treatment and control for whites (white circles) and blacks (black circles) in the 1996, 2000, 2004, and 2008 presidential elections. The quantity of interest here is the probability of voting for the Republican candidate, so if

[16]There are more black precincts because there are precincts that are entirely black.

FIGURE 5 Difference in Republican Vote for Matched Precincts



Note: Figure 5(a) shows differences in weighted mean Republican vote for precincts with $d \leq 1{,}000$ and matched precincts with $d > 1{,}000$ for white voters (white circles) and black voters (black circles). Figure 5(b) shows differences in weighted mean Republican vote for white voters and black voters matched with precincts with $d \leq 1{,}000$ from nondemolished projects.

TABLE 1 Regression of Turnout on Distance and Population Size

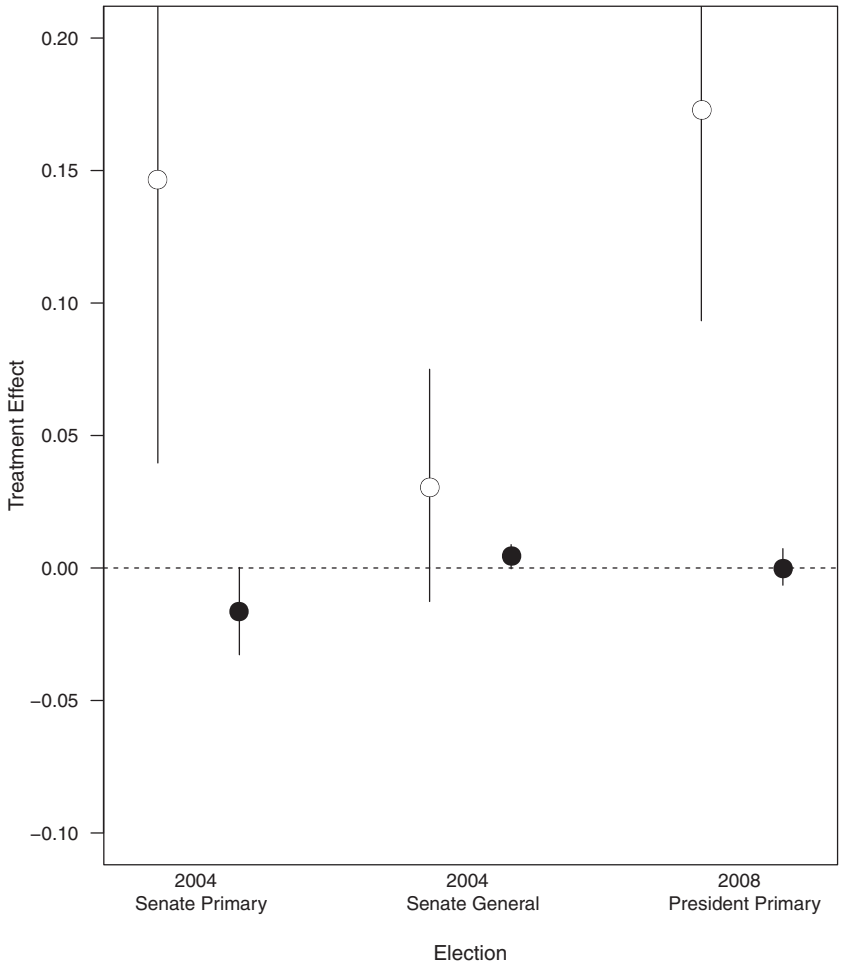
Variable	Coef. (Std. Err.)
Log(distance)	0.021
	(0.001)
Log(percent of local black population)	–0.018
	(0.002)
2000 turnout	0.419
	(0.003)
Intercept	0.136
	(0.012)
Degrees of freedom	113,847
Adjusted R-squared	0.119

Notes: OLS regression of 2004 voter turnout on listed variables for white voters. The local African American population is represented by the total number of African Americans within 1 kilometer of the project. The population of the housing projects is represented by the African American population of the census blocks containing the projects. The percent of the local African American population living in the demolished projects is these two quantities expressed as a proportion. Standard errors are listed in parentheses. All coefficients are significant at $p < .0001$.

the precincts close to the projects are voting more conservatively, the estimates should be greater than zero in 1996 and 2000 and should be reduced in 2004 and 2008 after the demolition of the housing projects. Black voters living near the projects showed almost no difference from blacks living farther from the projects in the propensity to vote Republican, and this remained unchanged before and after the demolition of the projects. On the other hand, white voters near the projects voted for Republican candidates at a higher rate than white voters farther away in 1996 and 2000. In 2000, this difference was statistically significantly different from zero. By 2004, however, after the demolition of the projects, the difference in vote for the Republican candidate becomes smaller and not statistically different from zero. This decline is striking since the same Republican candidate, George W. Bush, ran in 2000 and 2004, adding an extra level of control. By 2008, the difference between treatment and control diminishes to zero.

The change in voter behavior is even more striking in the difference between white voters near demolished projects and near nondemolished projects. In Figure 5(b), I display these differences for white voters

FIGURE 6 Difference in Obama Vote for Matched Precincts



Note: Differences in weighted mean Obama vote for precincts with $d \leq 1{,}000$ for demolished projects and matched precincts with $d \leq 1{,}000$ for nondemolished projects for white voters (white circles) and black voters (black circles).

(white circles) and for black voters (black circles) near demolished projects matched with voters near nondemolished projects (precincts are again matched on income). Points greater than zero mean that the voters near the demolished projects voted more conservatively than voters near projects that were not demolished. In every election between 1996 and 2008, the vote-choice behavior of black voters near projects that were not demolished and the behavior of black voters near projects that were not demolished was nearly identical, indicated by the points near zero. In 1996 and 2000, white voters in precincts near projects that were eventually demolished were more likely to vote for Republican candidates than white voters near projects that were never demolished. In 2004, after the demolition of the projects, this difference declined, indicating that the pro-Republican leanings of voters near projects that were eventually demolished were partially driven by the presence of their African American neighbors. Strikingly, by 2008, when an African American candidate, Barack Obama, was running for the Democrats, white voters living near the intact projects were actually slightly *more likely* to vote for the white Republican candidate than the white voters living near the demolished projects (the far right, white circle), suggesting that racial threat from their African American neighbors was compelling these voters to vote Republican.[17]

[17]Prior to 2000, white voters near projects that were eventually demolished voted more conservatively than white voters near projects that were never demolished. If voters had been randomly assigned to live near identical projects prior to 2000, we would expect this difference in Republican voting to be zero. In Table A.1 in the supporting information, I establish that precincts near the demolished and nondemolished projects were largely demographically similar prior to demolition. However, the projects to be demolished were not randomly assigned, so there were differences between

Obama's candidacies present another opportunity to examine the effect of outgroup threat on vote choice. I assume that, if racial threat affects vote choice, white voters near projects should favor a white candidate over an African American candidate. Obama appeared as a citywide candidate three times before running in the presidential general election in 2008. Unfortunately, none of these elections—the 2004 Democratic Senate primary, the 2004 Senate general, or the 2008 Democratic presidential primary—occurred before the demolition of the projects, so no before-and-after comparison is possible. However, some information can be gained from the difference between white voters near the demolished projects and white voters near projects that were still intact.

In Figure 6, I display the differences between white voters near demolished projects and matched white voters near nondemolished projects (white circles) and the same differences for black voters (black circles). Here positive numbers mean that the voters near the demolished projects were *more* likely to vote for Obama than voters near the projects still standing. For white voters, positive numbers are consistent with reduced racial threat. Once again, the behavior of black voters is nearly identical near demolished and nondemolished projects. However, notice that in the 2004 primary, white voters near projects that had been demolished were significantly more likely to vote for Obama than white voters living near projects that were still intact, which is consistent with white voters near intact projects favoring a white candidate over a black candidate. However, in the 2004 general election, when Obama was competing against Republican Alan Keyes, also African American, there is little difference in vote choice for those near nondemolished projects and those near demolished projects. This perhaps indicates that racial threat had little influence on vote choice when the contest was between two African American candidates. In the 2008 primary, when Obama was competing against a white candidate, this difference between voters near demolished and nondemolished dramatically returned—despite his being a native son of Illinois—which is consistent with the black outgroup still being salient to white voters near intact projects and, as noted by other scholars (Tesler and Sears 2010), the remarkably racialized nature of the 2008 Democratic primary.

these projects, some of which are consistent with a difference in conservative voting as a result of racial threat. Most prominently, the population of the eventually demolished projects, on average, represented a larger portion of the local African American population (16.0%) than did the population for nondemolished projects (9.3%). As demonstrated with voter turnout in the previous section, this population size difference alone is expected to create a difference in threat.

The Electoral Context in Chicago and the Psychology of Racial Threat

The reduction in turnout and racially conservative voting after the removal of the outgroup is consistent with theories of racial threat. However, as discussed in the opening of this article, racial threat findings have been attributed to a host of mechanisms ranging from the instrumental to the psychological, so it is useful to consider what mechanism may be driving the behavior of whites in Chicago. It appears that the threat they perceived was largely psychological, having little politically or economically instrumental motivation.

The electoral context in Chicago around the time of the demolitions makes instrumental behavior an unlikely mechanism. I measure the treatment effect on turnout by comparing turnout in the 2000 and 2004 general elections. Presidential elections point to a psychological mechanism for racial threat because their national focus means that local issues, for which a voter could rationally hope to influence outcomes, are not at stake. These elections were also locally uncompetitive at subnational levels, such as congressional races; recent general elections in Chicago have yielded overwhelming Democratic victories. It is notable that these elections did not have races for alderman or any other citywide offices for which local factional politics would be more salient.

Geography also makes an instrumental explanation unlikely. The voters in question and their neighbors in the housing projects were usually separated into different electoral districts by district lines drawn close to the boundaries of the housing projects. Local electoral districts, such as aldermanic wards, were also constructed so that residents of housing projects were separated from the voters analyzed in this study (see Figures A.13 and A.14 in the supporting information).[18]

Furthermore, I demonstrated that the strength of the effect in Chicago varied with the size and proximity of the outgroup. This "dose effect" is consistent with psychological mechanisms positing that psychological impact is a function of salience (see Hypothesis 2). However, variation with distance is not directly attributable to instrumental behavior: electoral boundaries were not crossed as the voters' distance from the projects increased,

[18]Only 22% of the voters used in this study lived in the same aldermanic wards as a housing project. If voters living in the same wards as the housing projects are excluded, all analysis presented in this article is substantively unchanged and larger average treatment effects are sometimes obtained. Similarly, while 57% of the voters used were in the same congressional district as a housing project, using only those voters not in the same congressional district as a housing project yields the same substantive results.

meaning that increased distance created no obvious change in a voter's instrumental incentives.

While mechanisms are often difficult to establish and can vary across contexts, the situation in Chicago suggests that voters were not reacting instrumentally to the presence of the outgroup. This causally identified effect supports classic formulations of racial threat, such as Key's assertion that racial threat in the 1930s South was based on the "symbolic potency" of the presence of African Americans because, in fact, "in no state would Negro voting produce 'black supremacy'" (Key, 1949, 646).

Conclusion

I have presented evidence for racial threat that is relatively free of questions of endogenous confounding. The exogenous intervention in the residents' racial context caused a significant change in their voting behavior, indicating that racial threat likely arises from attitude change rather than the selection of individuals into contexts. Additionally, the strength of the effect decreased with distance from the project and increased with the size of the outgroup. The estimated effect of racial threat, over 10 percentage points, is substantively large in many elections.

This finding does not parse out how much of the effect of the project residents on their white neighbors was due to race and how much was due to other dimensions of difference, like poverty. Would the effect have been different had the residents of the housing projects (implausibly) not been poor? The effect I measure operates exclusively on whites and, even when wealth is controlled for, makes it more likely that race is the most important factor. If it were an outgroup defined by poverty, then we might expect to see the same effect on African Americans who were not poor.

However, racial threat can be extended to the impact of the proximity of any meaningfully defined outgroup. The strength of the effect may be smaller when the outgroup is not defined racially, especially since race is such an important social categorization in the United States. An extension of this finding would be to test the strength of the effect when an outgroup is spatially separated in a similar manner, but its difference is a matter of, say, class, religion, or sexual orientation.

The normative implications of my findings can be troubling. It may be tempting to say that racial integration leads to hostility. This could be viewed as evidence against the long-standing and controversial contact theory (Allport 1954), which argued that contact between groups leads to reduced hostility under certain conditions. However, it is important to note that the populations in this study were probably not meaningfully integrated. The white voters in Chicago were threatened by a spatially proximate, yet segregated, outgroup. It is doubtful that these whites and African Americans thought of themselves as part of the same neighborhood or community. I doubt that the residents of the affluent Gold Coast referred to themselves as "living near Cabrini-Green," a nearby project. In the case of some Southside projects, the white and black populations were separated by a literal barrier in the form of the Dan Ryan Expressway.[19] We therefore do not know how whites would have reacted to their African American neighbors had they been meaningfully integrated. This speaks to the importance of considering proximity and segregation, in addition to size, in the relationships of groups. The effects of distance, demonstrated at the individual level, also demonstrate the complexity of measuring social interactions. Previous studies have used proximity as a proxy for social interaction (Welch et al. 2001), arguing that it reduces intergroup conflict. My findings suggest that proximity is often not a valid measure of social interaction and that, in segregated contexts, proximity may increase conflict. Rather, as many scholars have increasingly argued, explicit measurement of segregation should be considered when modeling intergroup contact (Baybeck 2006; Enos and Gidron 2014; Uslaner 2012; Zingher and Steen 2014).

It seems that the ideal conditions for racial and other group-based threat may be when groups are separated but closely proximate. Urban planners have largely reversed the planning philosophy that produced distinct segregation by race and class, and, as a result, the new face of public housing in the United States is that of mixed-income, public/private ventures. Future research should continue to find points of leverage for studying attitudes and behaviors in these newly integrated populations.

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[19] Urban historians have argued that freeway construction was often a deliberate means of racial segregation (Keating 2001).

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Supporting Information

Additional Supporting Information may be found in the online version of this article at the publisher's website:

Figure A.1. Treatment Effects with Different $Pr(race|name)$