Appendix A: Expanded Literature Review

Existing work on racial threat has long been beset by conflicting findings. Some scholars find that proximity to outgroups fuels racially threatened attitudes and behaviors (Enos 2017; Tolbert and Grummel 2003; Giles and Hertz 1994), others that it has no effect (Voss 1996; Cain, Citrin, Wong 2000; Campbell et al 2006), and still others that it reduces threat (Fetzer 2000; Fox 2004; Welch et al 2001; Carsey 1995; Oliver and Wong 2003; Voss 1996; see Enos 2016 and Oliver 2010 for excellent reviews of the literature).

A number of reasons have been offered for these conflicting findings, including variation in the groups being studied (Oliver and Wong 2003), socio-economic conditions (Oliver and Mendelberg 2000; Tam Cho and Baer 2011), the levels of segregation of the given contexts (Enos 2017; Rocha and Espino 2009), and the operationalization of threat as standing population versus change in population (Alexseev 2006; Newman 2013; Green et al. 1998; Hopkins 2010; Hopkins 2009; Newman and Velez 2014). While all of these are important factors, two dominant explanations have been offered for conflicting results: residential self-selection and the modifiable areal unit problem (MAUP).

First, contexts aren't randomly assigned (Simpson 2008; Clark 1992). There are a number of reasons we might choose to live in a given community, including our attitudes towards racial and ethnic outgroups (Tam Cho, Gimpel, and Hui 2013; Oliver 2010; though see Mummolo and Nall 2016). The non-random nature of residential self-selection makes it difficult to tease out the causal effect of context on attitudes and behaviors using observational data.

Prior scholarship has attempted to address concerns over residential self-selection by controlling for self-reported neighborhood preferences (Oliver and Wong 2003) or preferences together with the ability to relocate (Enos and Gidron 2016), performing endogeneity tests (Rocha and Espino 2009), demonstrating that racial orientations are not predictive of respondents' racial and ethnic context (Branton and Jones 2005), using instrumental variables (Acharya et al 2016), using field experiments (Enos 2014), and recently, by identifying events where large changes in minority populations have occurred and where characteristics of the event facilitate causal inference, typically by mitigating concerns about selection bias (Hopkins 2012; Enos 2016).

Second, variation in outcomes could be driven by variation in the contextual or areal unit chosen for analysis (Yule and Kendall 1950; Openshaw and Taylor 1979). The relationship between racial composition and behavior, for example, has been examined at the state level (Leighley and Nagler 1992; Hero and Preuhs 2007), county level (Giles and Buckner 1993; Gaines and Tam Cho 2004; Key 1949; Hopkins w.p.), zip code (Leighley and Vedlitz 1999; Hopkins 2010), census tract (Oliver and Wong, 2003; Putnam 2007), and census block group (Gay 2006), among others. Individuals are nested within a number of different administrative geographic boundaries and the covariation of an outcome of interest with some contextual variable of interest can vary drastically depending on the choice of administrative geographic boundary. In fact, Oliver (2010) finds that high levels of racial diversity at the zip code level is correlated with low levels of prejudice among White residents of that "neighborhood," while high levels of racial diversity at the county level is generally correlated with higher level of prejudice.

This issue has frequently been cited as a contributor to the conflicting findings in the extant literature (Tam Cho and Baer 2011; Enos 2016; Oliver 2010; Voss 1996). Researchers generally attempt to bypass issues of MAUP by attempting to theoretically justify their choice of areal unit of analysis and conducting various robustness checks (Oliver 2010). More recently, researchers have attempted to avoid this problem all together by examining a continuous measure of spatial proximity to some "treatment" as their operationalization of threat (Enos 2016; Hopkins w.p.). For a greater discussion of the MAUP and how we address it, see Appendix M.

In sum, while a number of factors have been singled out as culprits in the literature's conflicting findings, residential self-selection and the MAUP are most frequently cited as the chief culprits. Studies of racial threat need to carefully assess how both of these issues may affect the findings and thus the conclusion of the study.

Appendix B: Modifiable Areal Unit Problem (MAUP)

Two potential issues arise with regards to choice of areal unit of analysis (in our case, city), both of which fall under the rubric of the modifiable areal unit problem (MAUP) (Yule and Kendall 1950; Openshaw and Taylor 1979). For a discussion of the MAUP in the racial threat literature, see Appendix A.

The first issue, which is most relevant to the vast majority of studies on contextual effects (see Enos 2016; Oliver 2010; Tam Cho and Baer 2011), is that individuals are nested within a number of different administrative geographic boundaries. This first concern is one that our analysis avoids, as we are not using multilevel data and are not making a choice over a contextual unit to embed our units of analysis (i.e., cities) within. In other words, we are not examining the behavior of our unit of analysis as a function of the demographic composition or change of some researcher-chosen overarching areal unit; rather, like Enos (2016), we examine the behavior of our unit of analysis as a function of its *proximity* to a "treatment" stimulus, which in our case is cities where the Black population grew dramatically between 1940 to 1960. Proximity is a continuous measure and is limited not by administrative boundaries but only by the maximum range of spatial distance between cities in California.

The second issue has to do with the use of aggregate data. Here, the applicable MAUP concerns whether or not the positive and statistically significant relationship we observe between proximity to Black growth cities and support for Proposition 14 would change if we used data aggregated at different levels (e.g., MSA, zip code, census tract, etc.). Our analysis uses aggregations of voters at the city-level, and it is possible that an individual on the eastern-most boundary of a city lives in a different micro-context (e.g., further / closer to a Black growth treatment city) than an individual on the western-most boundary of a city. Without geo-coded survey responses or aggregate data at finer levels of geography, we do not have the ability to assess greater levels of detail. However, there are several reasons why we believe these types of concerns do not overly threaten the inferences we draw from our city-level findings.

First, by measuring proximity between centroids of cities, we average across all of the proximities for individuals residing at opposite ends of a given city. Thus, even if we were to entertain the possibility of the existence of unobserved heterogeneity in White voter behavior within cities as a function of within-city variation in racial micro-context, the use of city centroids to measure distance between predominately White cities and Black growth cities averages across all of the proximities for White voters residing in varying within-city micro-contexts.

Perhaps more convincingly, variation in the actual geographic scale of the cities in our data makes it possible to assess whether our main findings hold when focusing on cities with smaller total land area, as such cities essentially represent smaller aggregations of White voters across space. Indeed, city land areas in our dataset range between 0.3 and 455 miles (mean=8.15, median=3.4), indicating that our analysis combines cities the size of the average contemporary census tract (in LA county today, 1.73 square miles) and zip code (37 square miles, based upon estimates from the 2016 American Community Survey) with cities approximating the size of the average county in states such as Ohio, Tennessee, and Maryland (450 square miles). When restricting the analysis to 90% White or greater cities with below median total land area, we find that our results hold. We present the results from this analysis in Table B.1. Within the confines of the available data, this analysis essentially illustrates that our results hold when using smaller levels of geographic aggregation, as the land area of this below median subsample of cities is close to the size of an average 2010 census tract in LA county (1.73 square miles).

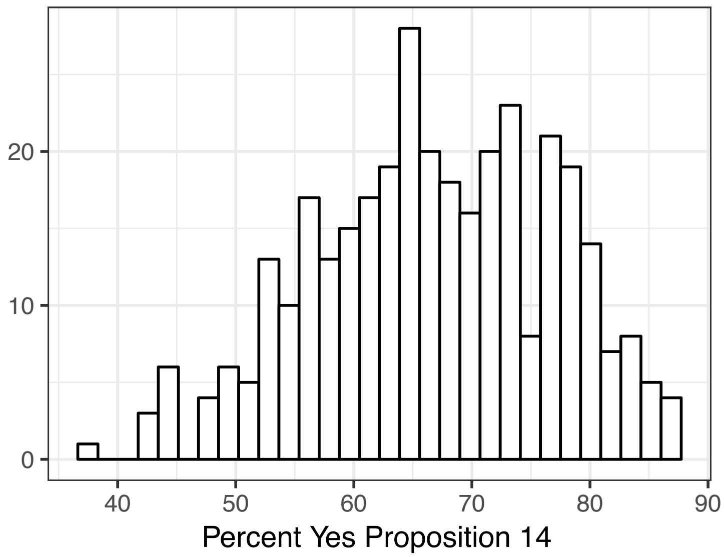
Table B.1: Sample Restrictions by Land Area

	Prop 14, 1964	
	(1)	(2)
Proximity	4.62***	7.97***
	(1.13)	(1.57)
Constant	68.94***	71.79***
	(1.45)	(1.18)
N	149	148
R^2	0.10	0.11
Adjusted R^2	0.09	0.10
Residual Std. Error	8.91 (df = 147)	9.87 (df = 146)
F Statistic	16.39*** (df = 1; 147)	17.73*** (df = 1; 146)

Note: OLS coefficients and heteroskedastic robust standard errors in parentheses. Sample restricted to cities that are 90% and greater White and are below median land area (column 1) and above median land area (column 2). *$p < .1$; **$p < .05$; ***$p < .01$ *(two-tailed).*

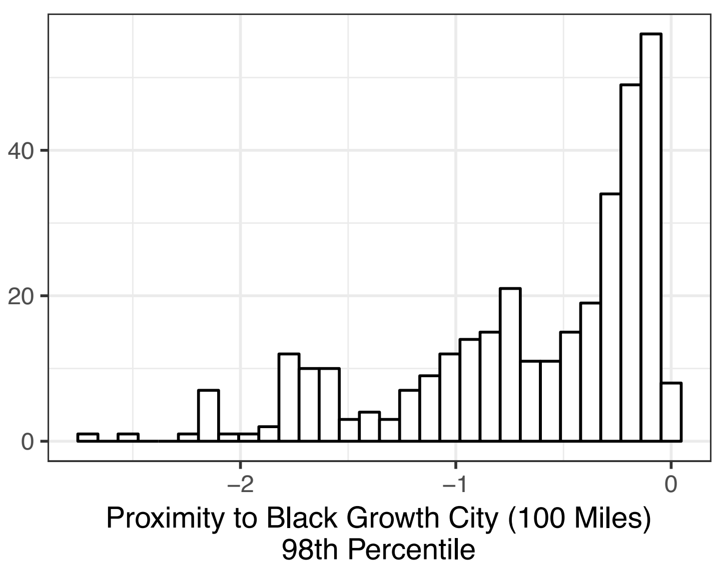
C. Descriptive Statistics

Figure C.1: Distributions of Votes for Proposition 14



Note: Bars indicate distribution of city-level vote for Proposition 14 for 90% or greater White cities.

Figure C.2: Distributions of Proximity to Black Growth Cities



Note: Bars indicate distribution of city proximity to nearest Black growth city in miles.

Table C.1: Descriptive Statistics

	Mean	Standard Deviation
Percent Democrat (1964)	57%	14%
Population Density	30,329	80,695
Owner Occupied Units	61%	15%
Income	$6,694.84	$1570.34
Unemployment	6%	3%

Note: Cells display mean and standard deviation of control variables. For the analysis, we rescale population density and income to 1000s.

D. Alternate Modeling Specifications

Table D.1: Effect of Proximity to Black Growth Cities on Support for Proposition 14

	Prop 14, 1964
Proximity	0.28***
	(0.08)
Median Income	-0.02
	(0.03)
Unemployment	-0.16
	(1.35)
Homeownership	-0.40
	(0.29)
Partisan Composition (%D)	0.07
	(0.27)
Population Density	0.000
	(0.000)
Constant	1.31***
	(0.30)
N	181
R^2	0.10
Log Likelihood	175.89

Note: Beta regression coefficients with standard errors in parentheses. Sample restricted to 90% and greater White cities. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$ *(two-tailed).*

Table D.2: Reanalysis with County Fixed-Effects

	Prop 14, 1964
Proximity	5.36*
	(2.78)
Median Income	-0.58
	(0.63)
Unemployment	-39.28
	(25.95)
Homeownership	4.42
	(5.53)
Partisan Composition (%D)	-3.16
	(4.91)
Population Density	0.001
	(0.004)
Constant	74.27***
	(5.82)
Fixed effects?	Yes
N	181
R^2	0.69
Adjusted R^2	0.65
Residual Std. Error	5.91 (df = 159)
F Statistic	16.96*** (df = 21; 159)

Note: OLS coefficients with heteroskedastic robust standard errors in parentheses. Sample restricted to 90% and greater White cities. Model includes county fixed effects. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$ *(two-tailed).*

E. Sample Size: Restrictions and Controls

In this section we assess the robustness of the effect of proximity to Black growth cities given differing sample sizes due to (1) the introduction of controls with missing values and (2) restricting our sample to 90% and 95% or greater White cities.

The most straight forward way of determining whether our results are influenced by the sample restrictions is to assess the robustness of the proximity coefficient as we restrict the sample in a variety of ways. First, we assess the bivariate relationship between proximity and city-level vote for Proposition 14 in the full dataset, thus including all n=386 cities for which we have proximity measures and for which data is reported in the Supplement to the Statement of the Vote (column 1), then for the remaining cities that are included in the sample if we were to include controls (column 2), and finally for those cities that were dropped from the regression for missing covariates (column 3). We then look at the coefficient when we include the full set of controls for all cities (column 4), for 90% or greater White cities (column 5) and for 95% or greater White cities (column 6).

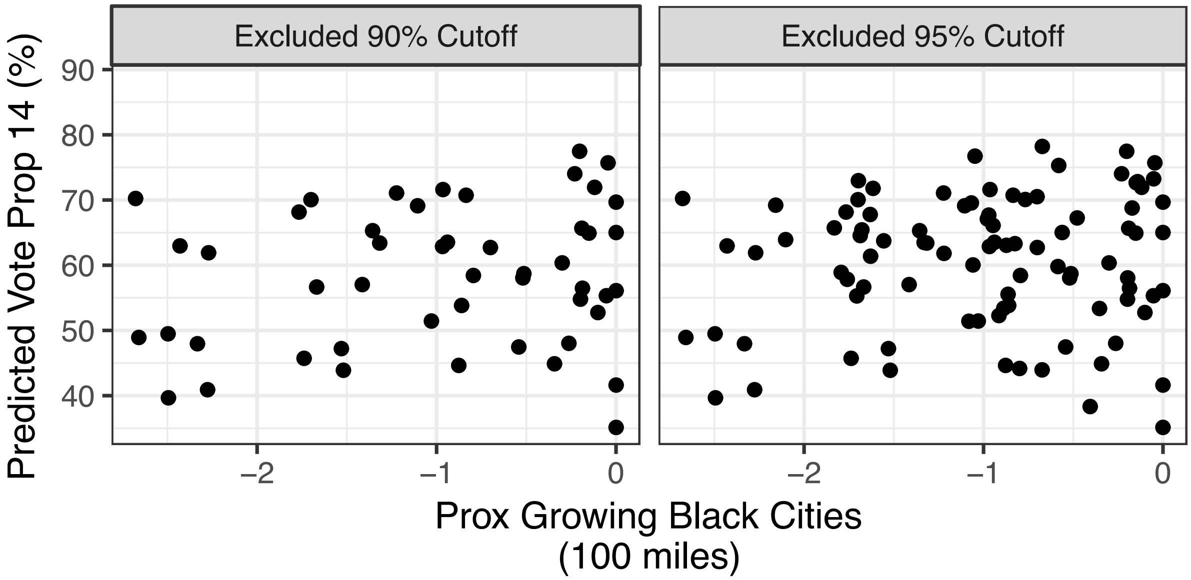
Across all sample restrictions, we see a retention of a positive and statistically significant effect of proximity to Black growth cities on support for Proposition 14, which increases our confidence that the results are not being driven by the exclusion of cities either from sample restrictions or missing data on control variables.

Table E.1: Effect of Sample Restrictions on Relationship Between Proximity and City Vote for Proposition 14

	Prop 14, 1964					
	Full	Full Control Cities	Full Excluded Cities	Full Controls	90% Controls	95% Controls
	(1)	(2)	(3)	(4)	(5)	(6)
Proximity	5.686***	3.988***	4.474***	4.891***	5.882***	6.179***
	(0.782)	(1.352)	(1.098)	(1.647)	(1.463)	(2.068)
Median Income				-0.885	-0.724	-0.814
				(0.862)	(0.901)	(0.961)
Unemployment				-11.435	-6.528	-4.123
				(31.433)	(31.610)	(33.907)
Homeownership				0.246	-7.143	-5.223
				(7.039)	(6.973)	(7.770)
Partisan Composition (%D)				-3.042	1.487	1.286
				(5.986)	(5.863)	(6.417)
Population Density				-0.003***	0.009	0.023
				(0.001)	(0.007)	(0.016)
Constant	69.671***	70.445***	66.888***	79.486***	81.059***	80.350***
	(0.829)	(1.070)	(1.365)	(7.898)	(7.806)	(8.633)
N	386	199	187	199	181	161
R^2	0.118	0.029	0.100	0.064	0.083	0.090
Adjusted R^2	0.116	0.024	0.095	0.035	0.052	0.054
Residual Std. Error	9.938 (df = 384)	10.507 (df = 197)	9.015 (df = 185)	10.448 (df = 192)	9.744 (df = 174)	10.080 (df = 154)
F Statistic	51.619***(df = 1; 384)	5.935**(df = 1; 197)	20.632***(df = 1; 185)	2.202**(df = 6; 192)	2.629**(df = 6; 174)	2.535**(df = 6; 154)

Note: OLS coefficients and heteroskedastic robust standard errors in parentheses. Column 1 displays bivariate relationship between proximity and Proposition 14 vote for the full sample. In Column 2 we show the bivariate relationship for just the cities that remain once we introduce controls. In Column 3 we show the bivariate relationship for those cities that are excluded when controls are introduced. Columns 4, 5, and 6 show results of the relationship with controls in the full sample, in 90% or greater White cities, and 95% or greater White cities. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$ *(two-tailed).*

Figure E.1: Proximity and Vote of Excluded Cities Under Sample Restrictions



Note: bivariate relationship between proximity and city-level vote for Proposition 14 for cities excluded when we restrict the sample to 90% (panel 1) and 95% White (panel 2).

F. Ecological Inference

In an effort to address ecological inference issues when using aggregate data, we run an ecological inference (EI) analysis for our cities to obtain more precise estimates of White and Nonwhite behavior across all cities. We use King's (1997) EI package to infer the proportion of White and non-White voters who supported Proposition 14 within each city given vectors of (1) support for Proposition 14, (2) the White and Non-White population within cities, and (3) the total population within cities. We find that the results closely correspond to Field Poll estimates from the 1960s (as cited in HoSang 2010) with an average White support of 68% and Black support of only 10%. We then re-estimated our main model (Table F.1) substituting out the official tally of city-level vote for Proposition 14 with our EI estimated White support for Proposition 14. We find that the proximity coefficient remains essentially unchanged by this substitution, increasing our confidence that our method of sample restriction is appropriate.

Table F.1: Effect of Proximity on Estimated (EI) White Prop 14 Vote

	Prop 14, 1964
Proximity	4.83***
	(1.51)
Median Income	-0.76
	(0.90)
Unemployment	0.27
	(31.27)
Homeownership	-6.70
	(6.49)
Partisan Composition (%D)	0.53
	(5.66)
Population Density	-0.001**
	(0.001)
Constant	82.35***
	(7.84)
N	199
R^2	0.06
Adjusted R^2	0.03
Residual Std. Error	9.80 (df = 192)
F Statistic	1.93* (df = 6; 192)

Note: OLS coefficients and heteroskedastic robust standard errors in parentheses. Outcome is EI-estimated city-level White vote for Proposition 14. Data is not restricted to 90% or greater White cities. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$ *(two-tailed).*

G. Proximity Measures

In this section we assess the robustness of the effect of proximity to Black growth cities to different operationalizations of proximity both with regards to (1) our choice of treatment cities and (2) the choice of Euclidean distance between city centroids as our measure of proximity.

Choice of 'Treatment Cities'

We conduct robustness checks using additional cut points to define "Black growth cities," including 95th, 90th, and 85th percentiles. We choose the 98th percentile for our models but assess the robustness of our choice in the Table G.1. We find that the relationship is similar, and indeed strengthens, regardless of how we define a Black growth city.

Table G.1: Alternative Choices of Treatment Cities

	Prop 14, 1964			
	(1)	(2)	(3)	(4)
Proximity 98	5.882***			
	(1.463)			
Proximity 95		5.455**		
		(2.566)		
Proximity 90			8.926**	
			(3.674)	
Proximity 85				12.190**
				(5.091)
Median Income	-0.724	-0.520	-0.390	-0.375
	(0.901)	(0.840)	(0.829)	(0.837)
Unemployment	-6.528	-34.750	-37.902	-39.711
	(31.610)	(31.252)	(30.400)	(30.150)
Homeownership	-7.143	-7.413	-8.257	-8.558
	(6.973)	(7.320)	(7.364)	(7.366)
Partisan Composition	1.487	3.443	4.661	5.262
	(5.863)	(5.945)	(6.031)	(6.037)
Population Density	0.009	0.010	0.007	0.007
	(0.007)	(0.008)	(0.008)	(0.008)
Constant	81.059***	78.910***	78.276***	78.389***
	(7.806)	(7.433)	(7.039)	(7.048)
N	181	181	181	181
R^2	0.083	0.041	0.050	0.062
Adjusted R^2	0.052	0.008	0.017	0.030

Residual Std. Error (df = 174)	9.744	9.965	9.918	9.855
F Statistic (df = 6; 174)	2.629**	1.239	1.527	1.920*

Note: OLS coefficients and heteroskedastic robust standard errors in parentheses. Sample is restricted to 90% or greater White cities. Each model defines definition of Black growth city as proximity to nearest city that experienced greater than or equal to the n-th percentile of Black population growth between 1940 and 1960. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$ (two-tailed).

Choice of Euclidean Distance

The earliest available city shapefiles were from the 1980 decennial census. While it is true that California cities likely annexed unincorporated territory in the sixteen years following the 1964 election, the centroids of the cities do not change dramatically over time.

In order to address the robustness of this choice, we conducted three additional analyses. First, following Nall, Schneer, and Carpenter (2018), we estimate driving distance between cities in the United States using an OpenStreetMap protocol API, an open source mapping software. Second, we used the same software to calculate drive times, using current driving conditions (not adjusting for traffic). Third, because we were concerned that contemporary driving distances and times may not be good proxies for driving times and distances in the 1960s, given the different infrastructure landscape at the time, we acquired a copy of Rand McNally's Standard Highway Mileage Guide from 1966 (which is the legal standard for driving distances under 28 U.S.C. 1821 for driving mile reimbursement, https://www.law.cornell.edu/cfr/text/28/21.5.) The guide includes driving distances between 31 different cities in California (for a total of 961 unique distance pairs). These distances were hand-keyed into a distance matrix and then compared to present day driving distances.

We find that contemporary driving distance, drive time, and Euclidean distances all correlate at greater than 0.98. Substituting driving distance and drive time in the model as a proximity measure does not change the substantive relationship between proximity and voting for Proposition 14, as we show in Table G.2. Finally, 1966 and contemporary driving distances correlate at 0.998, signaling to us that driving distances have not changed dramatically over the last 50 years and therefore current driving distances are a good proxy for 1960s driving distances.

Table G.2: Operationalizing Proximity as Driving Distance and Travel Time

	Vote to Prop 14		
	(1)	(2)	(3)
Proximity	5.882***		
	(1.463)		
Driving Distance		4.952***	
		(1.274)	
Travel Time			3.000***
			(0.810)
Median Income	1.487	1.482	1.263
	(5.863)	(5.914)	(5.917)
Unemployment	0.009	0.008	0.008
	(0.007)	(0.007)	(0.007)
Homeownership	-7.143	-7.077	-7.108
	(6.973)	(6.948)	(6.950)
Partisan Composition (%D)	-0.724	-0.700	-0.683
	(0.901)	(0.891)	(0.889)
Population Density	-6.528	-8.246	-9.355
	(31.610)	(31.891)	(31.723)
Constant	81.059***	81.051***	81.652***
	(7.806)	(7.751)	(7.845)
N	181	181	181
R^2	0.083	0.081	0.075
Adjusted R^2	0.052	0.049	0.043
Residual Std. Error (df = 174)	9.744	9.757	9.786
F Statistic (df = 6; 174)	2.629**	2.544**	2.360**

Note: OLS coefficients with heteroskedastic robust standard errors in parentheses. Sample is restricted to 90% or greater White cities. Column 1 displays our primary analysis using proximity to growing Black cities as the primary independent variable using Euclidean distance between the centroids of cities as our operationalization of proximity. For comparison, in Column 2, we run the same model but operationalize proximity as driving distance to growing Black cities. Driving distance was calculated using the OpenStreetMap protocol API. The third column uses estimated drive time instead of proximity using the same OpenStreetMap protocol API. The differences in effect of proximity using any of these measures are trivial. *$p < .1$; **$p < .05$; ***$p < .01$ *(two-tailed).*

H. 1940 Covariates

An issue worth addressing is the non-random settlement pattern of African Americans arriving into California. One possibility is that "redlining" (Rothstein 2017) led African Americans to settle in neighborhoods deemed less desirable, which may have housed lower socioeconomic status, and consequently, more racially conservative Whites. Such possibility could explain the negative relationship we observe between Black growth and White support for Proposition 14. This possibility, however, is not suggested by the data, as proximity to Black growth cities among mostly (90% or greater) White cities is not strongly correlated with pretreatment (i.e., pre-SGM) 1940 median home values (r = .10), homeownership rates (r = .15), or 1940 unemployment rates (r = .01). Nonetheless, as an additional robustness check, and to account for the possibility that the 1960 controls are post-treatment, we re-estimated the model presented in Table 1 including pretreatment controls for 1940 median home values (median household income is not available in the 1940 census track file; given this, we use median home value as a proxy for the level of wealth of a city in 1940) , 1940 unemployment, 1940 homeownership, and 1940 population density. When replacing the 1960 covariates with 1940 covariates, we find that the effect of proximity remains positive and statistically significant (p=0.035). Full model results are below in Table H.1.

Table H.1: Effect of Proximity on Voting 1940 Covariates

	Prop 14, 1964
Proximity	23.370**
	(10.879)
Partisan Composition	-19.567***
	(6.923)
Population Density	0.141*
	(0.071)
Homeownership	0.129
	(0.088)
Median Home Values	-0.366**
	(0.173)
Unemployment	140.154
	(112.375)
Constant	80.796***
	(7.641)
N	69
R^2	0.194
Adjusted R^2	0.116
Residual Std. Error	8.047 (df = 62)
F Statistic	2.486** (df = 6; 62)

Note: OLS regression coefficients with standard errors in parentheses. Sample restricted to 90% and greater White cities. All controls, with the exception of partisan composition were from the 1940 census. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$

I. Placebo Tests: Voting for Non-Racial Propositions

We include a number of placebo tests to ensure that our measure of proximity was not correlated with non-racial ballot propositions. While city-level vote tabulations for other ballot propositions in 1964 were not available in the Supplement to the Statement of the Vote, we did locate city-level tabulations for two other non-racial ballot propositions in 1966.

The 1966 Supplement contained tabulated city-level results for two prominent race-neutral ballot measures: Proposition 1, which authorized the investment of public pension or retirement funds in the stock market, and Proposition 16, which enhanced the prohibition on the production, distribution, sale, and possession of obscene materials. The argument written in favor of Proposition 1 concerned the outdated law that prevented public employee retirement fund managers from investing in common stocks which impeded a business-like approach to the management of the funds, whereas the argument written against Proposition 1 argued that the stock market was simply too risky given the speculative nature and fluctuations of the stock market. The argument written in favor of Proposition 16 concerned the necessity of protecting teenagers and young children from smut publishers, whereas the argument against concerned the constitutionality of California trying to censor art and literature. Voter information guides from these and other past California General Elections are archived at UC Hastings College of the Law (https://repository.uchastings.edu/ca_ballot_pamphlets/).

In Table I.1 we display the results of placebo tests estimating the effect of proximity to Black growth cities on support for these two race-neutral propositions. The results from the placebo tests indicate that proximity to Black growth cities is only associated with a statistically significant increase in voter support in the case of Proposition 14. In the case of Propositions 1 and 16 in 1966, the effect of proximity to Black growth cities is statistically indiscernible from zero. These findings increase our confidence that our measure of proximity is tapping into racial threat and not a different underlying phenomenon.

Table I.1: Placebo Tests – Effect of Proximity on Voting for Non-Racial Propositions

	Prop 14, 1964	Prop 1, 1966	Prop 16, 1966
	(1)	(2)	(3)
Proximity	5.88***	0.79	-0.19
	(1.46)	(1.19)	(1.14)
Median Income	-0.72	1.14***	-1.35*
	(0.90)	(0.43)	(0.82)
Unemployment	-6.53	-18.71	23.78
	(31.61)	(26.09)	(27.46)
Homeownership	-7.14	0.25	-1.76
	(6.97)	(3.64)	(4.98)
Partisan Composition (%D)	1.49	-14.41***	6.14
	(5.86)	(4.69)	(4.68)
Population Density	0.01	0.003	-0.001
	(0.01)	(0.002)	(0.004)
Constant	81.06***	59.87***	51.02***
	(7.81)	(5.33)	(7.27)
N	181	181	181
R^2	0.08	0.29	0.18
Adjusted R^2	0.05	0.27	0.15
Residual Std. Error (df = 174)	9.74	6.24	7.42
F Statistic (df = 6; 174)	2.63**	12.13***	6.37***

Note: OLS coefficients and heteroskedastic robust standard errors in parentheses with sample restriction to 90% or greater White cities. Column 1 displays our main model result for reference. Column 2 displays model results for 1966 CA Proposition 1. Proposition 1 allowed public pension funds to invest in equities, lifting the requirement that these funds only invest in bonds. Column 3 displays results for 1966 Proposition 16, which was a prohibition on obscene materials. We find that these non-racial propositions are not positively correlated with proximity to growing Black communities. $^{*}p < .1$; $^{**}p < .05$; $^{***}p < .01$ *(two-tailed).*

J. Estimating Individual-Level Level Support for Proposition 14

Another way we can operationalize proximity, which hews more closely to existing literature on racial threat, is to look at racial threat as a function of growing Black population within an individual's county. Fortunately, three surveys in 1964 asked California respondents how they intended to vote on Proposition 14.

With this data, we can bypass the issue of discerning individual-level White behavior from aggregate data and instead examine individual level attitudes as a function of proximal growing Black population. To do this, we downloaded and pooled California Field Poll data from 1964 on support for Proposition 14 (survey 6405, n=1128, fielded 8/31/64-9/5/64, survey 6406, n=1193, fielded 10/2/64-10/7/64, survey 6407, n=1148, fielded 10/23/64-10/28/64). In Model 1 in Table J.1 we display individual-level White support for Proposition 14, controlling for county and individual level demographics, as a function of county-level Black population growth. We find that this county-level demographic change is correlated with support for Proposition 14, consistent with our findings on aggregate vote results for Proposition 14. This finding provides additional evidence that our results are robust to a more traditional operationalization of racial threat in California.

While this type of analysis is comparable to what typically is done in the extant literature on racial threat, this type of analysis is also highly vulnerable to the modifiable areal unit problem (MAUP) (Enos 2016). In the end, the value of this analysis is to demonstrate that the results from the aggregate city-level analyses presented in the main manuscript hold when utilizing an alternative analytic strategy employing individual-level survey data and conducting contextual analysis (i.e., on nested or multilevel data). The consistency of the results increase our confidence that White support for Proposition 14 derived from racial threat from growing Black populations.

Table J.1: Individual-Level White Opposition to Rumford Act 1964

	Prop 14, 1964	
	Logit	OLS
	(1)	(2)
Growth in Black Pop 1940-1960	6.82***	1.65***
	(1.91)	(0.45)
Pop Density County 1940	-0.04**	-0.01**
	(0.02)	(0.004)
Unemployed County 1940	3.63**	0.88**
	(1.68)	(0.40)
Female	-0.19**	-0.05**
	(0.09)	(0.02)
Age 30-39	-0.06	-0.01
	(0.14)	(0.03)
Age 40-49	-0.28**	-0.07**
	(0.14)	(0.03)
Age 50-59	-0.06	-0.01
	(0.15)	(0.04)
Age 60-69	-0.22	-0.05
	(0.16)	(0.04)
Age Over 70	-0.27	-0.07
	(0.18)	(0.04)
Homeowner	-0.25**	-0.06**
	(0.10)	(0.02)
College	-0.27**	-0.07**
	(0.11)	(0.03)
Income	-0.06**	-0.01**
	(0.03)	(0.01)
Survey 6406	-0.02	-0.004
	(0.12)	(0.03)
Survey 6407	0.44***	0.11***
	(0.10)	(0.02)
Constant	0.02	0.50***
	(0.29)	(0.07)
N	2,630	2,630

Note: Logistic regression (column 1) and OLS coefficients (column 2) with heteroskedastic robust standard errors clustered at the county level in parentheses. Regression analysis uses survey weights. Growth in Black population measured as percent change in Black population at the county level between 1940 and 1960. *p < .1; **p < .05; ***p < .01 (two-tailed).

K. Robustness Residential Tenure, White Growth, and Housing Markets

Table K.1: Controlling for Residential Tenure, White Growth, and Housing Markets

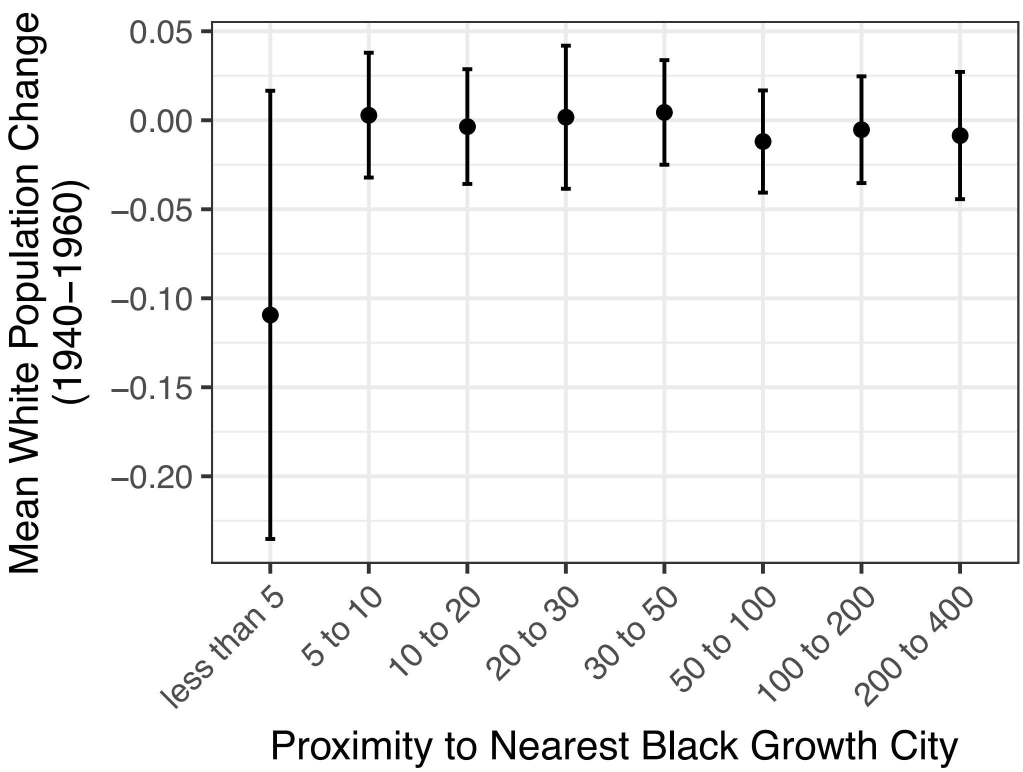
	Prop 14, 1964					
	< Median Res Tenure	> Median Res Tenure	< Median White Grow	> Median White Grow	Prox Contracting Housing	Prox Rising Home Values
	(1)	(2)	(3)	(4)	(5)	(6)
Proximity	4.51*	5.92***	6.94***	4.24*	4.42***	4.42***
	(2.59)	(1.61)	(1.97)	(2.45)	(1.36)	(1.31)
Contracting Housing					4.97***	
					(0.50)	
Home Values						4.27***
						(0.43)
Median Income	-5.06***	-0.29	-2.37**	0.11	-0.33	-0.25
	(1.23)	(0.97)	(0.95)	(1.00)	(0.64)	(0.63)
Unemployment	14.58	-12.65	-62.40*	110.62**	-23.11	-21.56
	(58.71)	(34.04)	(36.04)	(49.59)	(25.46)	(25.26)
Homeownership	12.52	-17.11*	-18.79**	1.73	3.76	2.12
	(9.28)	(9.10)	(8.59)	(11.56)	(5.88)	(5.86)
Median Home Values	-29.07***	5.90	18.40*	-11.60	4.73	5.03
	(7.85)	(7.33)	(9.55)	(8.86)	(5.01)	(5.01)
Unemployment	0.13***	0.005	0.001	0.10***	0.002	0.003
	(0.03)	(0.01)	(0.004)	(0.03)	(0.004)	(0.004)
Homeownership	113.84***	81.00***	92.22***	67.65***	77.31***	76.73***
	(11.72)	(8.81)	(7.70)	(10.29)	(5.39)	(5.33)
N	53	128	64	84	181	181
R^2	0.45	0.11	0.27	0.15	0.47	0.46
Adjusted R^2	0.38	0.07	0.20	0.09	0.44	0.44
Residual Std. Error	6.32 (df = 46)	10.29 (df = 121)	8.55 (df = 57)	9.83 (df = 77)	7.46 (df = 173)	7.50 (df = 173)
F Statistic	6.24*** (df = 6; 46)	2.50** (df = 6; 121)	3.59*** (df = 6; 57)	2.30** (df = 6; 77)	21.53*** (df = 7; 173)	21.02*** (df = 7; 173)

*Note: OLS regression coefficients and heteroskedastic robust standard errors for fully specified models in 90% White or greater cities with samples split at median residential tenure before 1940 (columns 1 and 2), median White population growth (columns 3 and 4), and controlling for proximity to cities with the most rapidly contracting housing markets and cities with the fastest growing cost of living (columns 5 and 6). *p < 0.1; ** p<0.05;***p<0.01 (two-tailed)*

L. White Flight

To explore the possibility that racially threatened White residents fled further away to outer ring suburbs and rural areas, leaving behind some mixture of presumably racially tolerant (i.e., liberal) Whites and those unable to relocate, we estimate the mean level of change in White population at various binned distances from Black growth cities. We find that almost all of the White flight, to the extent that it occurred, was concentrated in the first five miles.

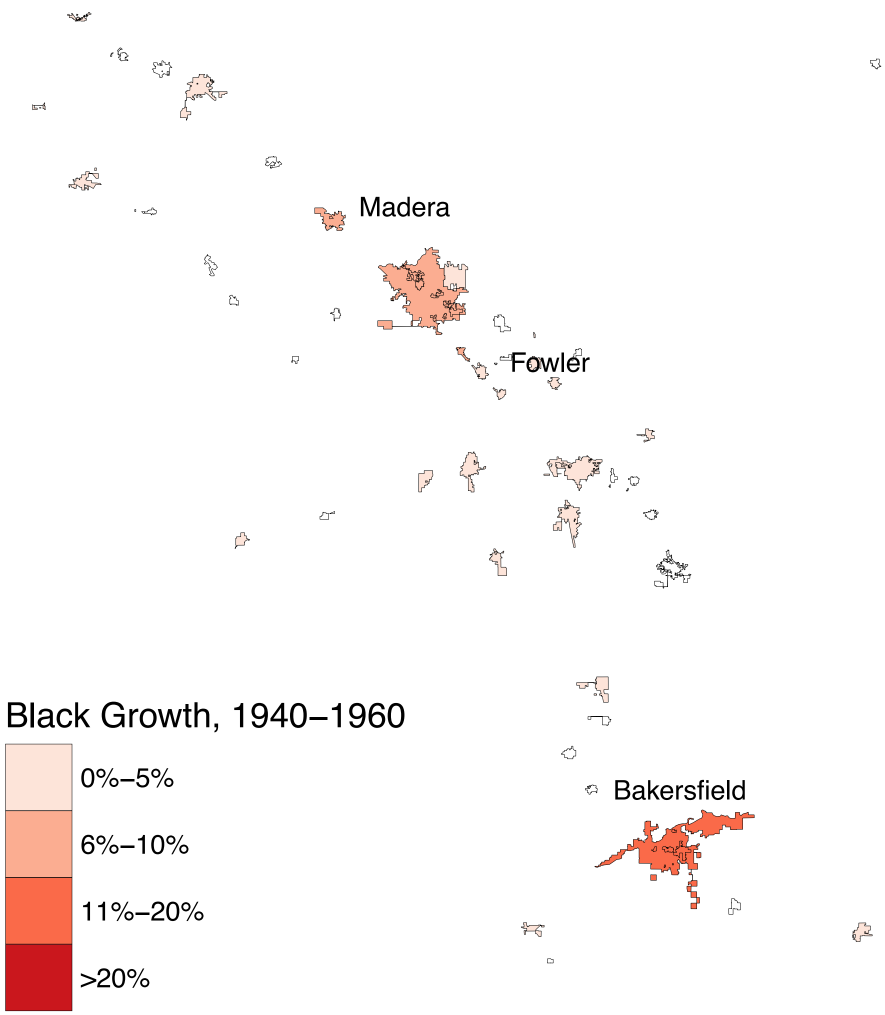
Figure L.1: Mean Levels of White Population Change As a Function of Proximity to Black Growth Cities



Note: Mean percentage point change in White population 1940-1960 conditional on proximity to Black growth city (98th percentile). White flight appears to largely be restricted to cities less than five miles from Black growth cities.

M. Additional Maps

Figure M.1: Map of Central Valley Black Growth Cities



Note: 95th percentile growth cities of Bakersfield, Fowler, and Madera in Central Valley.

Appendix N: Analysis with Various City Outlier Restrictions

Table N.1: Sample Restrictions by Varying Proximities

	Prop 14, 1964			
	(1)	(2)	(3)	(4)
Proximity	6.76***	13.12***	18.67***	34.43***
	(1.68)	(3.63)	(6.38)	(8.76)
Median Income	-0.83	-1.17	-1.30	-0.89
	(0.94)	(1.10)	(1.34)	(1.25)
Unemployment	-1.14	15.99	40.38	129.89
	(31.87)	(54.31)	(59.28)	(82.16)
Homeownership	-6.60	-4.63	-1.44	0.99
	(7.03)	(7.56)	(8.34)	(8.89)
Partisan Composition (%D)	0.04	-2.18	-5.82	-15.57*
	(6.04)	(6.62)	(8.02)	(9.11)
Population Density	0.01	0.01*	0.02	0.02
	(0.01)	(0.01)	(0.01)	(0.01)
Constant	82.28***	85.18***	85.61***	84.69***
	(8.10)	(10.76)	(12.78)	(12.50)
N	179	158	148	134
R^2	0.09	0.11	0.13	0.18
Adjusted R^2	0.06	0.07	0.09	0.14
Residual Std. Error	9.77 (df = 172)	9.88 (df = 151)	10.03 (df = 141)	9.78 (df = 127)
F Statistic	2.76** (df = 6; 172)	2.99*** (df = 6; 151)	3.38*** (df = 6; 141)	4.69*** (df = 6; 127)

Note: OLS coefficients and heteroskedastic robust standard errors in parentheses. All samples restricted to 90% or greater White cities. Results of further sample restrictions where we drop cities that are more than 200 miles away (column 1), 100 miles away (column 2), 75 miles away (column 3) and 50 miles away (column 4) from the nearest Black growth city. *$p < .1$; **$p < .05$; ***$p < .01$ *(two-tailed).*

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