

Long-Term Causes of Partisanship: Evidence from Linked Census and Voterfile Data*

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Abstract

The central role of partisanship in shaping political behavior and attitudes is well-established in research on political behavior in the United States. Partisan identification is highly stable, even in the face of changes to context and individual traits, after adolescence. But what shapes partisanship before it becomes stable? Can early life experiences produce enduring political affiliations? We examine the long-term determinants of partisan identification through an unprecedented individual-level dataset that links contemporary voterfiles to the 1940 U.S. Census. Linking these two datasets allows for novel analysis of the connection between early life experiences and present-day political behavior, providing new insights on classic questions about the nature of early-life socialization and partisanship. We investigate the effect of childhood exposure to people of different races and incomes, exploiting the ordering of households on the 1940 U.S. Census enumeration sheets to construct individual-level measures of racial and inequality exposure. We find that, among whites, early-life exposure to Black neighbors predicts Democratic partisanship over 70 years later. On the other hand, early life exposure to inequality, in the form of a neighbor of a greatly different income, has no long-term effect on partisanship. We discuss the implications of these findings and potential for the broader research paradigm to yield additional insights.

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1 Introduction

Can early life experiences have an enduring effect on political behavior? For example, can parents, neighborhoods, or schools shape the political attitudes of children so deeply that their influence is apparent in adulthood?¹ This is an enduring question, cutting across a number of social science fields and for which there are implications for how we understand socialization and political behavior, but one for which there is surprisingly little high-quality evidence. To date, most studies have had to rely on survey data characterized by measurement error, small samples, and have been limited to quantifying persistence over relatively short time periods (see, for example, [Niemi and Jennings \(1991\)](#)). These issues limit our ability to make inferences about the overall role of political socialization in shaping political behavior.

To help overcome many of these issues, we undertake the first large scale linking of full population, individual-level administrative data on contemporary voterfiles to the recently digitized 1940 U.S. Federal Census. In doing so, we are able to construct a dataset that captures a number of early life experiences such as exposure across race and class early in one's childhood linked to contemporary partisanship over 70 years later. We link voters in three states for which we have data on state of birth (California, North Carolina, and Nebraska) to the 1940 U.S. Decennial Census yielding an overall dataset of over 250,000 individuals.² Though our current approach is limited to three states, we are able to observe a significant amount of variation across racial diversity, income, and urban/rural status.

Building on a rich set of research regarding the role of racial exposure on political behavior, which finds a largely mixed set of results with regard to the impact of exposure to outgroups on political attitudes, we test for the impact on whites of having a black neighbor in 1940. To do so, we follow [Logan and Parman \(2017\)](#) and leverage the ordering of

¹ See [Campbell \(2006\)](#) for an overview of this research.

²We discuss this in more detail below.

the census manuscript to infer the identity of a given household’s neighbor, which (1) overcomes many of the problems of aggregation inherent to this literature and (2) provides a more fine-grained test of racial exposure than existing studies. Using an empirical strategy that uses increasingly fine-grained fixed effects to account for sorting even below the county level, we document strong evidence showing that white voters who had a Black neighbor in 1940 are more likely to be Democrats in 2017 than white voters who did not have a Black neighbor in 1940. With the same empirical strategy, we also benchmark this effect against having neighbors who are either richer or poorer than a linked individual’s family and find a near zero effect of early childhood exposure to different classes on partisanship today. Following recent studies that highlight the importance of the interaction of age and exposure (Chetty, Hendren, and Katz, 2016), we show that the impact of having a Black neighbor among whites is *increasing* in a child’s age—a pattern that is consistent with a socialization theory of political behavior. In short, our results help to establish a considerable role for early childhood exposure to different races, but not different classes on partisanship over the course of 70 years.

Our results provide significant contributions to the study of political socialization with particular regard to racial exposure (Niemi and Jennings, 1991; Jennings, Stoker, and Bowers, 2009). Recent work by Holbein (2017), which also links administrative data, shows that psychosocial skills developed early in childhood affect turnout in adulthood.³ In a similar spirit, our work shows a strong role for early childhood exposure to outgroups on long-run partisanship. While much of the earlier work on political socialization tends to focus on early adulthood (Niemi and Hepburn, 24; Verba and Almond, 1989), our data and results push for a renewed emphasis on socialization *throughout the life-course*.

In addition, our work helps to push long-standing debates about the role of racial expo-

³For more on the role of childhood psychosocial skill development and political participation, see Holbein et al. (Forthcoming).

sure on political behavior forward by introducing the role of exposure to diversity in early childhood (Key, 1949; Allport, 1954; Giles and Buckner, 1993; Oliver, 2001; Hopkins, 2010; Newman, 2013; Enos, 2016, 2017). In a recent study most similar to our own, Goldman and Hopkins (Forthcoming) document that county-level racial diversity in adolescence among whites is positively correlated with more conservative racial attitudes in adulthood. Our study differs, however, in using population-level data and examining the impact of exposure to other races who are *immediate neighbors*. Together, our results suggest the need for future research as to the precise ways in which early childhood exposure to diversity might shape political behavior over the long-run.

As part of a larger project investigating the role of salient early life experiences on political attitudes, we harness data that can, potentially, examine the behavior of every registered voter alive in the United States today who was alive and enumerated in the 1940 United States Census. With this data and other available administrative data, we are able to link early life experiences, such as exposure to neighbors of different social groups, participation in welfare programs, and parental experiences, to downstream political behavior.

As we discuss in greater detail below, we believe this method has the potential for significant contributions to knowledge. To the best of our knowledge, this is the largest long-term study of individual-level political behavior ever conducted. It represents, not only a significant increase in size over previous survey-based studies that have, at most, just a few thousand people (for example Niemi and Jennings (1991)) but also represents a significant increase in time-span over which effects are measured. We observe our sample first as young children in 1940 and then again as much older adults, more than 7 decades later in the voterfiles. The long-term endurance of these effects has important implications: if we can demonstrate that the effects of racial exposure on political behavior persist over seven decades later, especially on an important characteristic such as partisanship, it may be reasonable to assume that the influence of these early-life experiences are also present in intervening

decades, when people are active in politics and the workforce, and that the influence may be present in a range of behaviors (Erikson and Stoker, 2011).

2 Theoretical Background

We connect early life experiences, in particular exposure to diversity of income and racial background, to downstream partisanship. In the United States, because partisanship continues to be the strongest predictor of nearly every political attitude (Achen and Bartels, 2017), this has powerful implications for a range of political outcomes.

Why might these early-life experiences shape partisanship? Partisanship can be characterized as a social identity (Green, Palmquist, and Schickler, 2004), meaning a group membership that is closely tied to a person’s self-image. Other examples of social identities, in the American context, include race and religion. Social identities are powerful predictors of behavior (Fiske, 2000) and, indeed, partisanship has been shown to induce a range of behaviors, including the type of group-based bias that characterizes race-based social identities (Iyengar and Westwood, 2014). Deeply-held identities and the way in which they shape behavior are often described as “symbolic attitudes,” which are attitudes that tend to be stable, determinative of other attitudes, and shaped early in life (Tarman and Sears, 2005). Because they are shaped, or “conditioned,” early in life, such attitudes are particularly prone to strong social influences during key periods of a person’s life. Influential studies have argued that such attitudes are shaped by “classic conditioning” that happens in early years, perhaps pre-adolescence (Campbell et al., 1960; Tarman and Sears, 2005), while other studies have pointed to later adolescence or early adulthood as the time in which political attitudes are stabilized (Sidanius et al., 2008).

The effects of exposure across racial groups has been the subject of a tremendous amount of scholarship, with claims dating back at least as far as Allport (1954), that the type of intimate contact induced by residential proximity can reduce individual prejudice and

improve intergroup relations. While the nuances of the effects of exposure, especially in institutional settings, is still debated (Paluck, Green, and Green, 2017), there is convincing evidence that residential racial context can causally affect political attitudes (Enos, 2017). However, most of this research has been limited to studying the effects of exposure over a fairly short time period, at most over the course of a few years (for example, Sacerdote (2001)), while the research on political socialization argues that the effects of early life conditioning can be enduring. By connecting early life exposure to partisanship decades later, we are, to the best of our knowledge, showing the effects of such exposure over the longest period of time yet recorded—demonstrating the remarkable endurance of socialization and it’s ability to influence decades of political activity. We draw on research from Erikson, MacKuen, and Stimson (2002) and others showing that partisanship is strongly influenced by racial attitudes and predict that, therefore, these early-life experiences of racial exposure may shape partisanship.

By 1940, racial geography was highly politically salient feature of American society (Key, 1949) and, therefore, likely exhibited strong effects on intergroup relations and social behavior. Racial segregation doubled in the United States from 1880 to 1940. This increase in segregation was driven by racial sorting at the household level; Blacks and whites became increasingly less likely to live next to each other and these patterns held in urban and rural areas in the North and South (Logan and Parman, 2017). Consistent with theories of segregation’s negative effects on intergroup relations (Enos, 2017), this sorting also corresponded with interracial violence, as counties with lower levels of whites and Blacks living next to each other had higher levels of white lynching of Blacks (Cook, Logan, and Parman, 2017). We test how individual variation in this salient racial context influenced political behavior downstream.

Similarly, while exposure across income groups is less well-studied than cross-race exposure, there is reason to believe such exposure can affect political behavior. Neighborhood-

level exposure to wealth has been shown to effect lifetime mobility (Chetty, Hendren, and Katz, 2016) and, more directly relevant, Sands (2017) uses a field-experiment to persuasively demonstrate that even fleeting exposure to inequality can affect political behavior. And Mendelberg, McCabe, and Thal (2017) showed that socialization of the wealthy during college affects attitudes about redistribution. While residential economic segregation has risen dramatically in recent decades (Taylor and Fry, 2012), the pre-war centralized nature of cities and the income instability caused by the Great Depression (Piketty and Saez, 2003) means that residential exposure across class was likely not uncommon in the 1930's when the subjects in our study were children. What was the long-term effect of exposure for these subjects?

3 Data

In this section, we describe the linkage process for connecting the present day voterfile to the 1940 U.S. Federal Census and provide basic descriptive statistics on our linked dataset. Linking the voterfile to the 1940 Census provides us with rich information on the early-life environment of Census-matched voters. Below, we describe the details of the linking process and provide some initial descriptive statistics to characterize our sample. The combination of these two datasets massively increases the available data compared to other studies in the field and also has certain measurement advantages that we describe throughout the manuscript.

3.1 Census Data

Data on early-life experiences comes from the 1940 Federal Decennial Census. In 1940, the Census was conducted by enumerators who went door to door, attempting to contact every household in the United States, recording information on name, age, gender, race, years of education, labor earnings, employment status and many other characteristics. Census

manuscripts are housed in the National Archives and are made available for public use in accordance with a statutory 72-year restriction to protect privacy. The 1940 Census was released digitally for research on April 2, 2012, and digital images of census cards can be obtained from the National Archives. The Census has been organized into structured datasets through collaborative efforts between Integrated Public Use Microdata Series at the Minnesota Population Center (IPUMS) and Ancestry.com and FamilySearch. While IPUMS makes anonymized full count 1940 Census data available to the public, we access de-anonymized data through the National Bureau for Economic Research.

3.2 Voterfile data

Our voterfile is obtained from L2, a commercial data vendor working with both major political parties in the United States. The data includes records on every registered voter in the United States as of 2017⁴. L2 and other commercial vendors obtain this data from state governments who either collect it directly or aggregate it from counties for the purposes of administering elections and helping incumbent politicians in their campaigns (Hersh, 2015). The vendors then sell this data to political campaigns for voter targeting. Until very recently when some states introduced automatic registration of citizens, voter registration was voluntary in nearly every state. As such, our data includes about 80% of the voting eligible population, which is about 92% of the approximately 250 million persons in the United States over 18 years old, and does not include non-citizens and people not allowed to vote in certain states because of felony convictions and other reasons. Voter data provided by the states usually includes a large number of records that are invalid because the registrant has died or moved and re-registered. L2 and other vendors attempt to remove these records before selling the data. The commercial vendors also attempt to link people across time,

⁴The precise dates of each voterfile vary by state. The California voterfile used for linking is current as of March 25, 2017, the North Carolina voterfile is dated January 12, 2017, and the Nebraska voterfile is dated January 13, 2017.

preserving parts of their records as they change addresses and/or states delete older data.

When citizens register to vote, they usually provide basic information, including their full name, address, and date of birth. In three states, they provide their state of birth. As we describe below, we leverage the time-invariant variables of name, birth date, and place of birth to match with historical Census data. In most, but not all, states, voters also declare a party affiliation when registering. In certain states in the South, registrants also indicate their race. Most states also record voter participation in individual elections. To provide other information about voters, the commercial vendors link this official self-reported data to commercial data and use proprietary techniques to impute other variables, such as partisanship and race.⁵ As we explain below, we focus on three states, California, North Carolina, and Nebraska, each of which records partisanship at the time of registration.

3.3 Record Linkage

Data-driven record linkage across administrative and survey datasets is becoming increasingly popular across the social sciences. To date, many of these methods rely on non-statistical methods based on comparisons of name, gender, race, and age.⁶ These methods are *non-statistical* in the sense that they do not rely on a probabilistic model. Recent advances in the record linkage literature suggest using probabilistic algorithms such as Support Vector Machines (Goeken et al., 2011), regression (Feigenbaum, 2016), and bayesian inference (Enamorado, Fifield, and Imai, 2017; Abramitzky, Mill, and Perez, 2018). The core advantage of the probabilistic model is that it allows the researcher to directly control the false-positive rate by tuning the acceptance threshold for match probabilities.

For the main set of results, we use the supervised machine learning procedure developed by Feigenbaum (2016). The key strength of this algorithm is that it allows the researcher to take advantage of the value of human coders by generating a hand-coded training set. Census

⁵ See Enos (2016) for an example of such race imputation techniques.

⁶For an overview of record linkage methods, see Bailey et al. (2018).

data was recorded in cursive by enumerators and transcribed 72 years later, introducing many layers of measurement error. Humans are quite good at comparing lists of names and making judgements across imperfect links, implicitly weighing differences in first and last names, as well as differences in year of birth or implied age. The machine learning approach makes those implicit weights on various record link features—how much worse is a one letter mistake in a last name relative to a first name, do the strings sound the same—explicit and then replicably applies those weights to link very large datasets at scale. While the creation of a training set does require additional work, [Feigenbaum \(2016\)](#) shows that the costs are quite minimal and generally require less than ten hours of work. To create the training set, we began by subsetting the voterfile to the states for which we have information on state of birth: California, North Carolina, and Nebraska. We restrict our searches to men since the common practice of surname changes at marriage during the 20th century makes it quite difficult to link women accurately. We then further subsetted the data to examine possible matches who were similar on first name, last name, and age, blocking on state of birth.⁷ Finally, we trained a probit model using a number of constructed variables from the first and last name strings and the years of birth. See appendix for further details on the constructed variables used to train the linkage algorithm.

Overall, our linkage method performed quite well. The match rates—the shares of voter records we are confident in linking to a record in 1940—for California, North Carolina, and Nebraska were 52%, 46%, and 65% respectively. To benchmark this, we compare this to performance by [Enamorado, Fifield, and Imai \(2017\)](#) which attempts to link the 2015 voterfile to the 2014 file. In their study, they achieve an average link rate of about 93%. Since our study attempts to match individuals from 2016 to 1940—a period of over 70 years—we believe that the contemporary voterfile to 1940 Census match rate is quite good. Rates

⁷Specifically, we looked at all men with matching states of birth, born within 5 years of the record in the voterfile, and with a first and last name within 0.3 in Jaro-Winkler string distance.

around 50% are also inline with census match rates in economic history when records are linked by hand (Parman, 2011; Feigenbaum, 2018), as transcription errors and enumerator errors are much more severe in historical data than in contemporary data. Our final linked sample size is 259,762 individuals.

There are, of course, limitations to this data. First, there are likely to be name transcription errors in the 1940 Census. Assuming these transcription errors are driven by arbitrary variation in transcriber skill across the many data enterers used by Ancestry.com, they will not likely be correlated with our variables of interest. Thus, missing matches due to transcription should not substantively affect any of our results. Second, there could be biases resultant from differential mortality rates by observed and unobserved factors. If we are more likely to link individuals who are healthier and health is correlated with partisanship, this would bias our findings. Third and finally, our results are only valid within the sample of males who are registered to vote as of 2017 in three states. We can, of course, relax our linking thresholds to try and include more states and women in the future.

3.4 Descriptive Statistics

Before demonstrating how this data can be used to answer fundamental questions about long-term causes of partisanship, we first provide descriptive statistics on the linked data. Figure 1 is a map of the logged number of linked records by county in 1940. After linking, our data in 2017 consists only of people living in California, Nebraska, and North Carolina today, but these people could have lived anywhere in the United States in 1940.⁸ Note that most of our matched sample still in 2017 still lived in the states in which they were residing in 1940 but, as should be expected, many also lived in high population counties of major metropolitan areas, such as Chicago and Detroit, from which they moved to California, Nebraska, or North Carolina at a later time. Figure 2 displays the state to state flows of our

⁸ Alaska and Hawaii were not yet states in 1940.

linked population from 1940 to 2017.

Figure 3 is the distribution of raw counts in our linked sample of party registration, our key dependent variable. Consistent with current trends in the relationship between partisanship and age, our sample tends to lean Republican though there are still a significant proportion of Democrats as well. We also have a smaller, yet sizable, contingent of individuals who are officially registered as non-partisan.

Our sample is very well-suited to detect the effects of socialization because of their young ages in 1940. In Figure 4, we display the age distribution of our sample of linked men in 1940 (in Appendix A1 we show the age distribution of their fathers). As Figure 4 shows, the plurality of our sample was essentially *just born* in 1940 with the large majority of individuals younger than ten years of age and nearly the entire sample being younger than 20 years old.

Our sample is predominantly white ($\approx 92\%$), as is clear in Figure 5. This is slightly more white than the full US in 1940 (89.8%), likely driven both by the states in our sample and the higher match rates for whites in census linking. We also observe a smaller set of non-whites, predominantly African Americans. Given that our analysis on racial socialization relies on whites, we still maintain sufficient sample size in those analyses below.

Finally to get a sense of our sample's economic backgrounds, we also look at the distribution of father's income in Figure 6 and father's geographic mobility in Figure A2 in the Appendix. While the 1940 Federal Census recorded an individual's wage income, it only records an indicator for whether an enumerated individual received more than \$50 in non-wage income. Thus for many Americans—primarily for farmers who did not earn wage income, but also other business owners and operators—we do not have information on the total wage and non-wage income. We drop any individuals who reported receiving more than \$50 in non-wage income when computing statistics based on wage income. Looking at income in Figure 6, we can see that a significant proportion of fathers are unemployed and receiving zero wage income. This likely reflects the depressed economic activity at the

Figure 1: Residence of Linked Sample in 1940

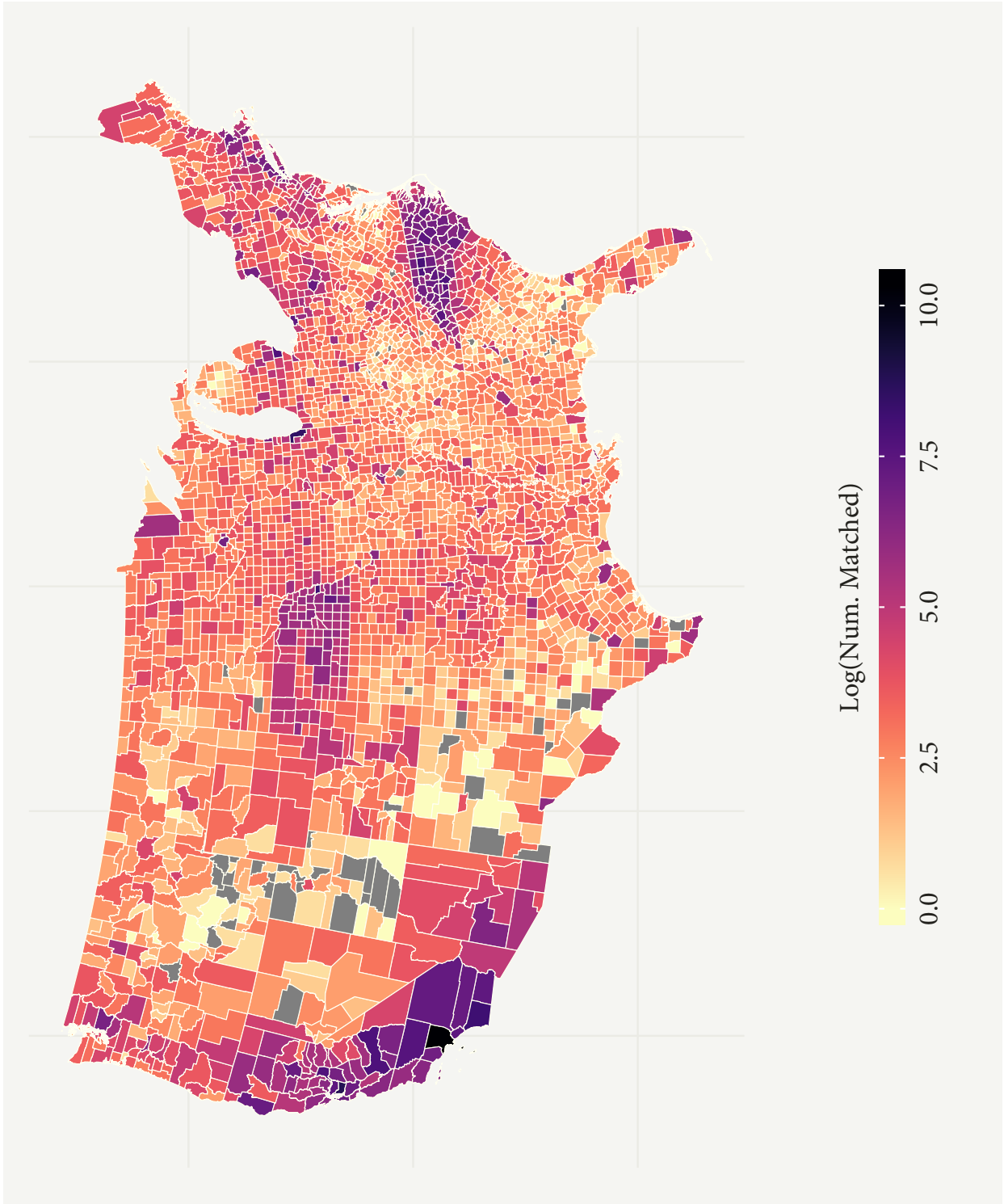


Figure 2: Transition Plot of State of Birth in 1940 to State of Residence Today

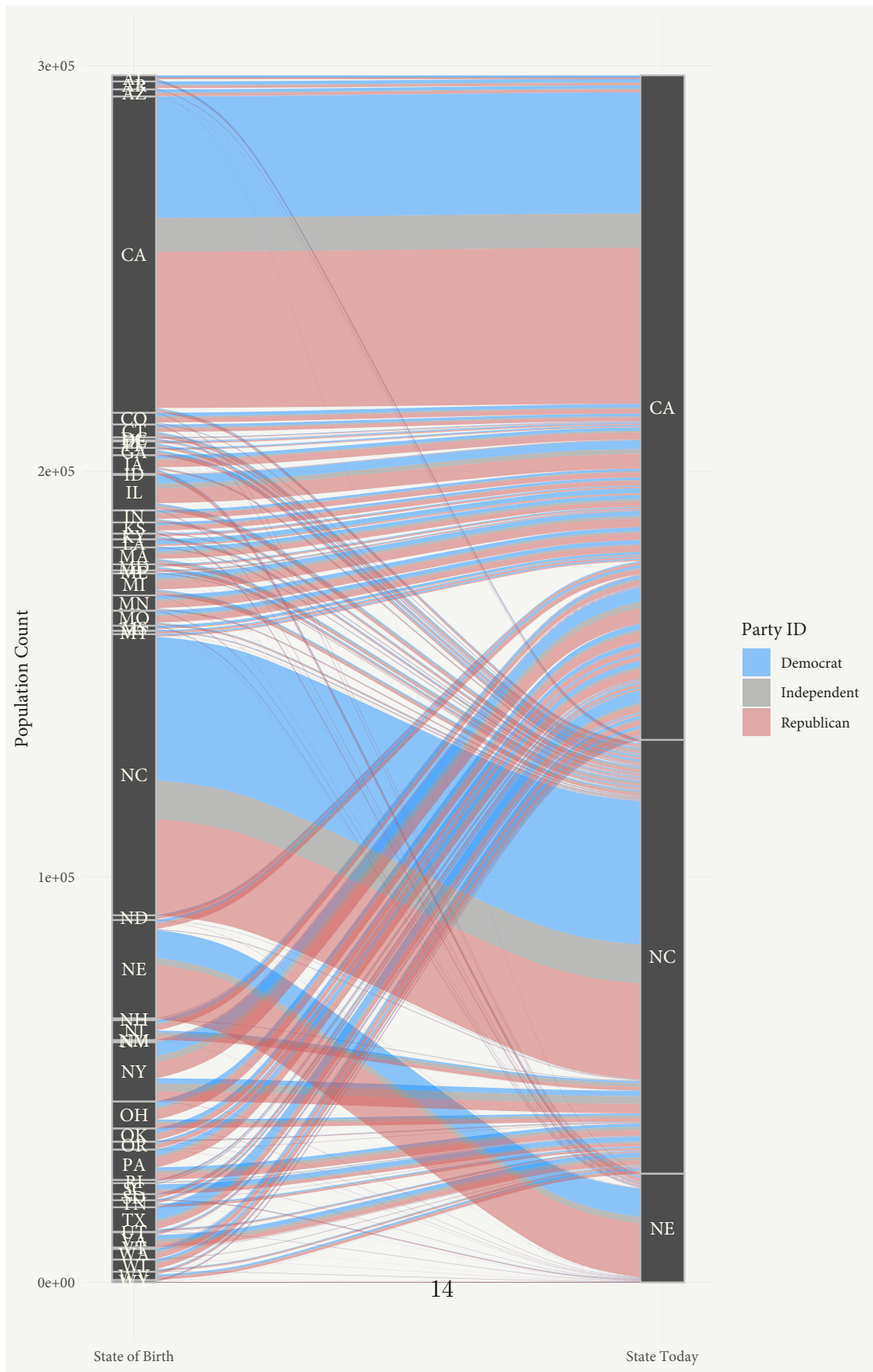


Figure 3: Distribution of Partisanship in 1940 Census Linked Sample

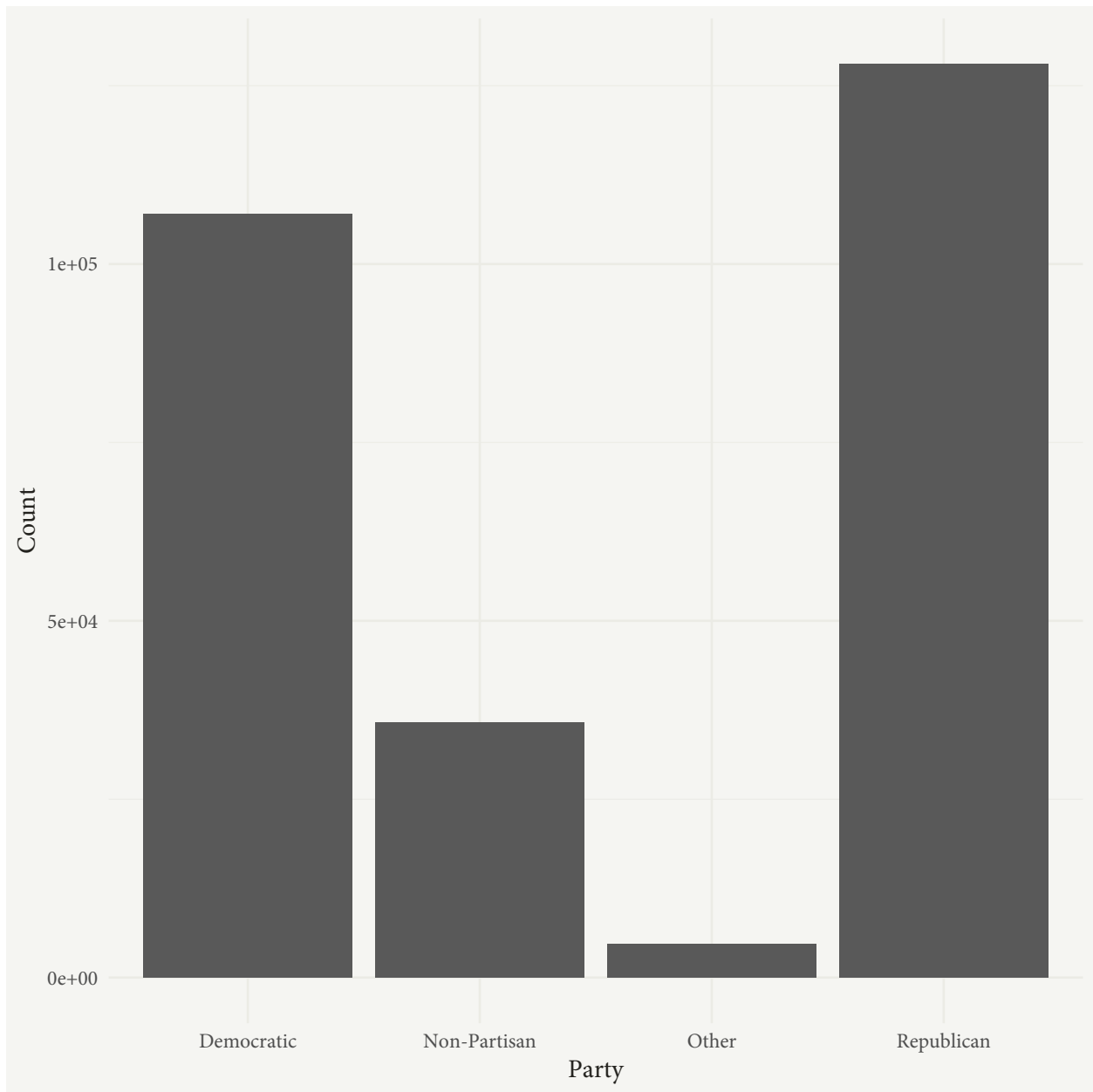


Figure 4: Distribution of Age in 1940 Census Linked Sample

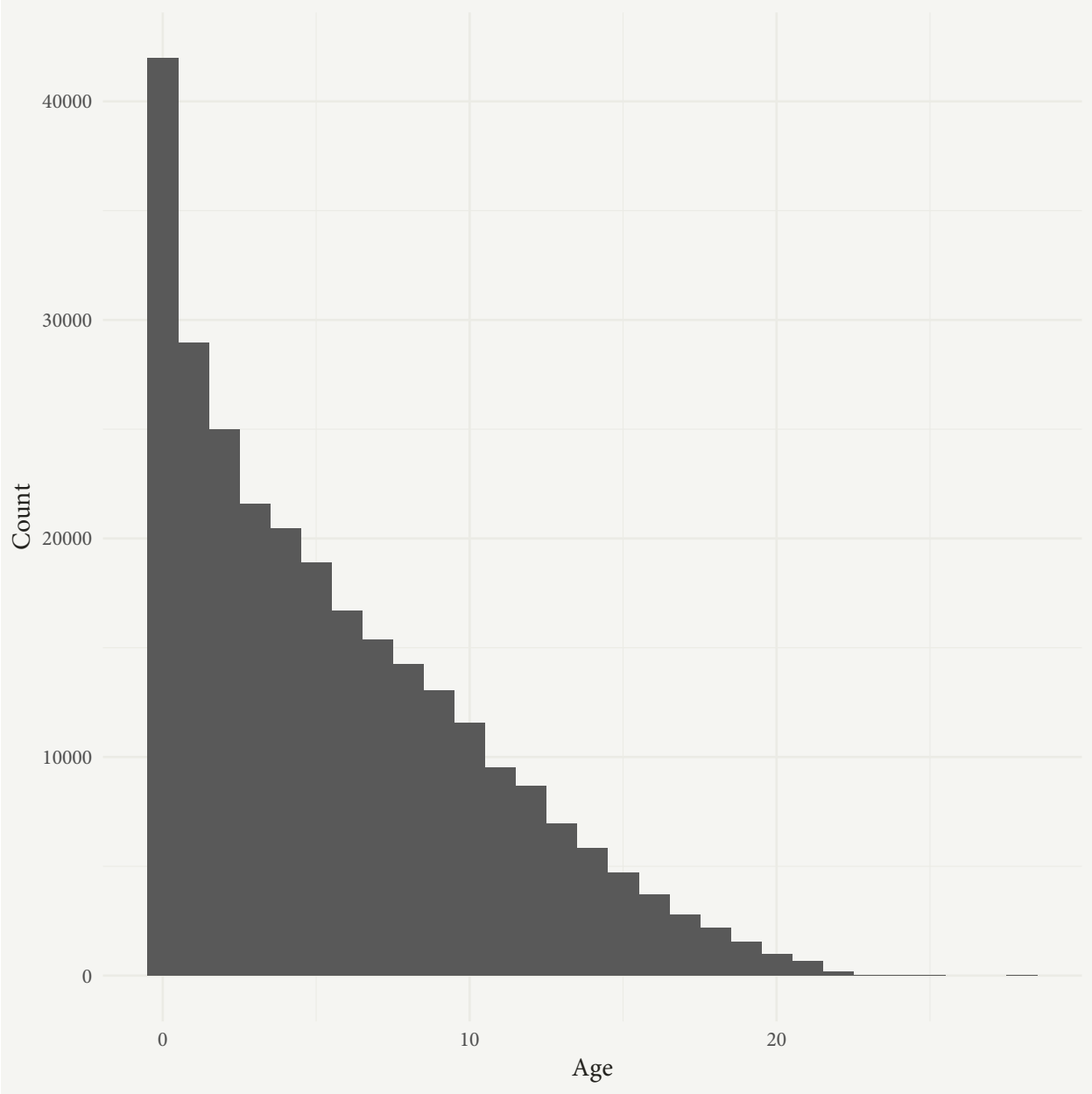


Figure 5: Distribution of Race in 1940 Census Linked Sample

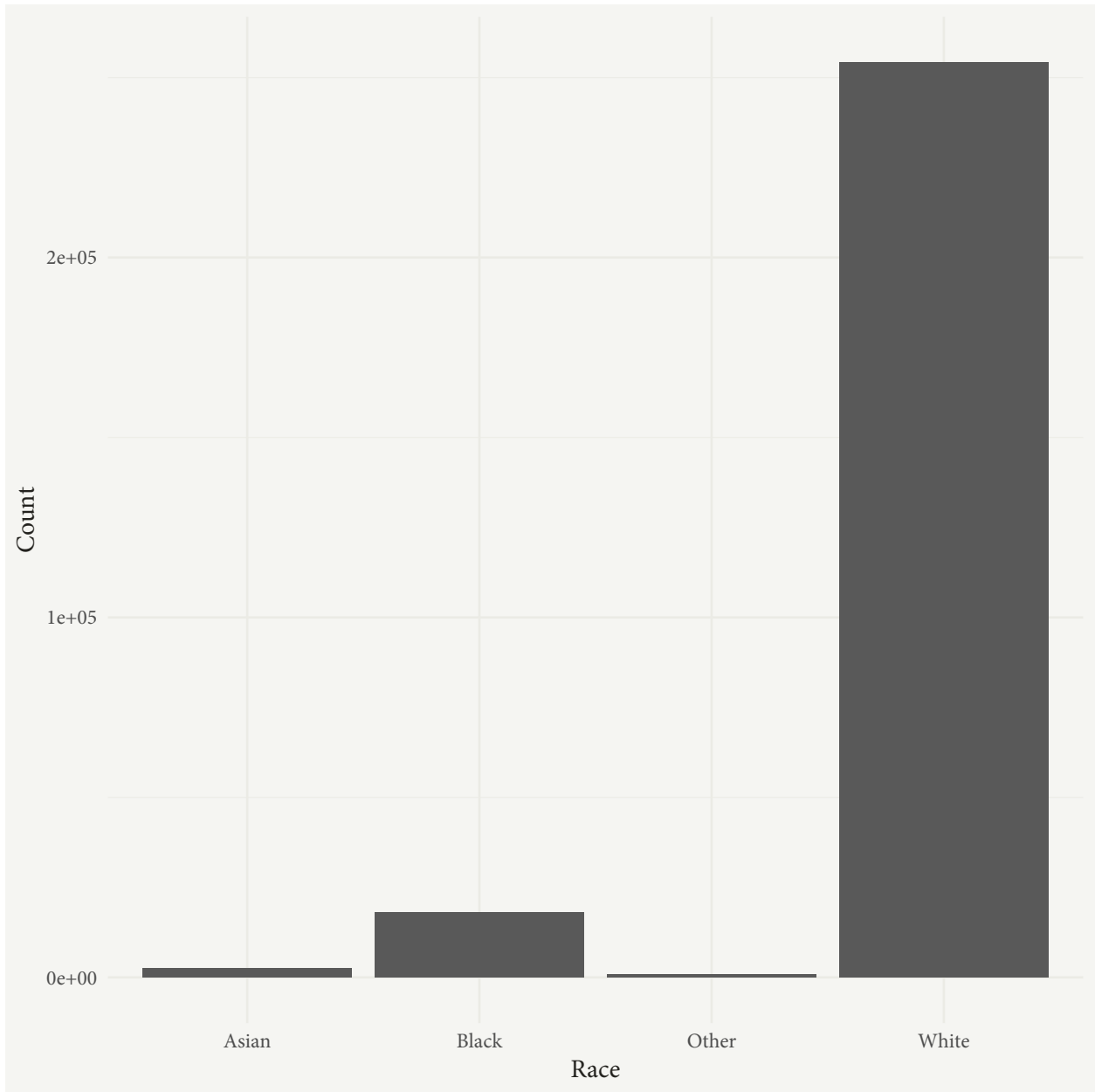
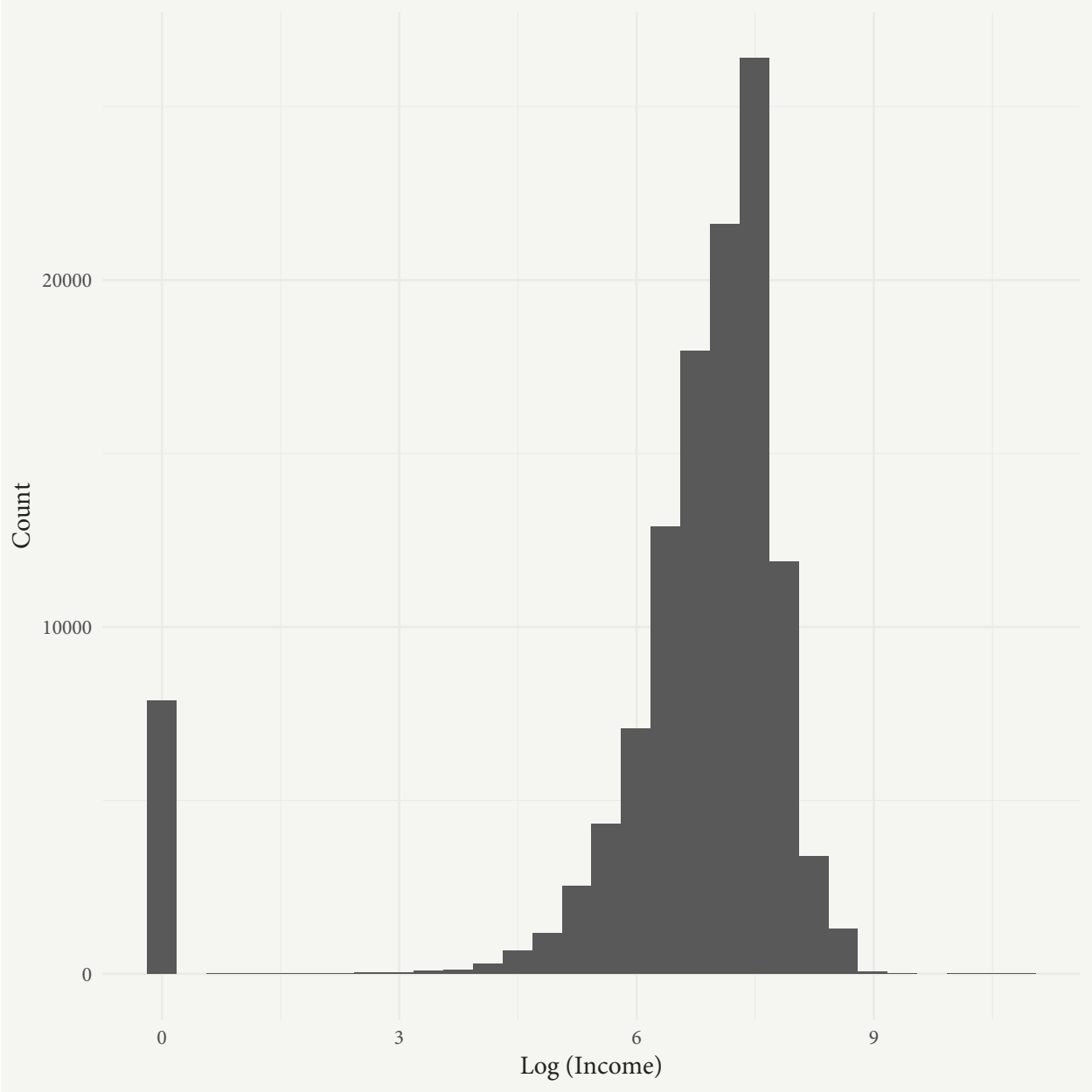


Figure 6: Distribution of Father's Income in 1940 Census Linked Sample



time following the Great Depression, although it might also reflect some measurement error. Figure A2 shows that the plurality of fathers moved within counties between 1935 and 1940. Additionally, a significant proportion of fathers did not move at all. Together, these figures suggest that our sample reflects a fair degree of income inequality while also having relatively little cross-county or state geographic mobility.

4 Results

4.1 Early-Life Exposure to Different Races

Does having a Black next neighbor influence the long-term political attitudes and identities of whites? Through the linking process just describe, we are presented with a unique opportunity to test the downstream political effects of early-life racial contact.

4.1.1 Measuring Racial Exposure

To measure cross-race exposure, we build on methods for measuring segregation developed by Logan and Parman (2017), exploiting the ordering of households on the 1940 U.S. Census enumeration sheets. When compiling the Census, enumerators in 1940 went door to door down the street, recording information for each house. As a result, adjacent households are very likely to appear immediately next to each other on census pages, and we are able to identify the characteristics of an individual's immediate next door neighbor, as well as the characteristics of their neighbors two doors down, three doors down, and so on. With this information, we construct measures of racial exposure for each individual in our dataset, specifically whether each individual had a neighbor of the opposite race, and their relative proximity to said neighbor.

The key assumption for our measures of racial exposure is that proximity in Census enumeration is a proxy for residential proximity in actual geography. This assumption is supported by evidence that census enumerators were exhaustive, in that they visited every

recorded household, and that they visited and recorded households per their geographic ordering (Agresti, 1980; Magnuson and King, 1995; Grigoryeva and Ruef, 2015). Under this assumption, we contend that households geographically next door to each other will be next to each other on census manuscript pages, and that as we move up or down the census reel away from an individual in our dataset we locate increasingly more distant neighbors. We also assume that moving, say, 3 spaces up the Census manuscript page is on average the same relative position as moving 3 spaces down the page. The neighbor who lives 3 doors up the street is not considered more proximate than the neighbor living 3 doors down the street, and vice versa.

To measure racial exposure, we construct an indicator variable representing the 10 most proximate pairs of neighbors for each individual in our dataset. Each indicator incorporates two variables because for any given position (closest neighbor, second closest neighbor, ...) there are two households that are equally proximate to the individual, depending on whether you look up or down the Census page. As such, for $k \in [1, 10]$, each indicator equals 1 if at least 1 of the 2 households located k doors down from the individual are inhabited by a head of the household of a different race than our individual. With this, we can measure the effect on future political behavior of having a neighbor of a different race living a certain number of houses away from you, accounting for the race of your other proximate neighbors. In calculating racial exposure, we drop individuals whose race is not identified in the Census and we estimate the effects below for just whites, leaving us 238,3533 individuals, with slightly smaller sample sizes in the regression analysis due to missingness in some covariates (see below). Figure 7 presents the breakdown of whites in our dataset who have a Black neighbor at each neighbor position versus those who have no Black neighbors at each position. The number of whites with a Black neighbor at each position is low, indicating the low population share of Blacks and the high levels of residential racial segregation in 1940.⁹ Notice too that

⁹ Logan and Parman (2017) calculate county-level rural Black/white Dissimilarity in 1940 to be 0.37 and

levels of exposure are lower at the immediate neighbor level, perhaps reflecting an aversion, among whites, to living immediately next to Blacks.

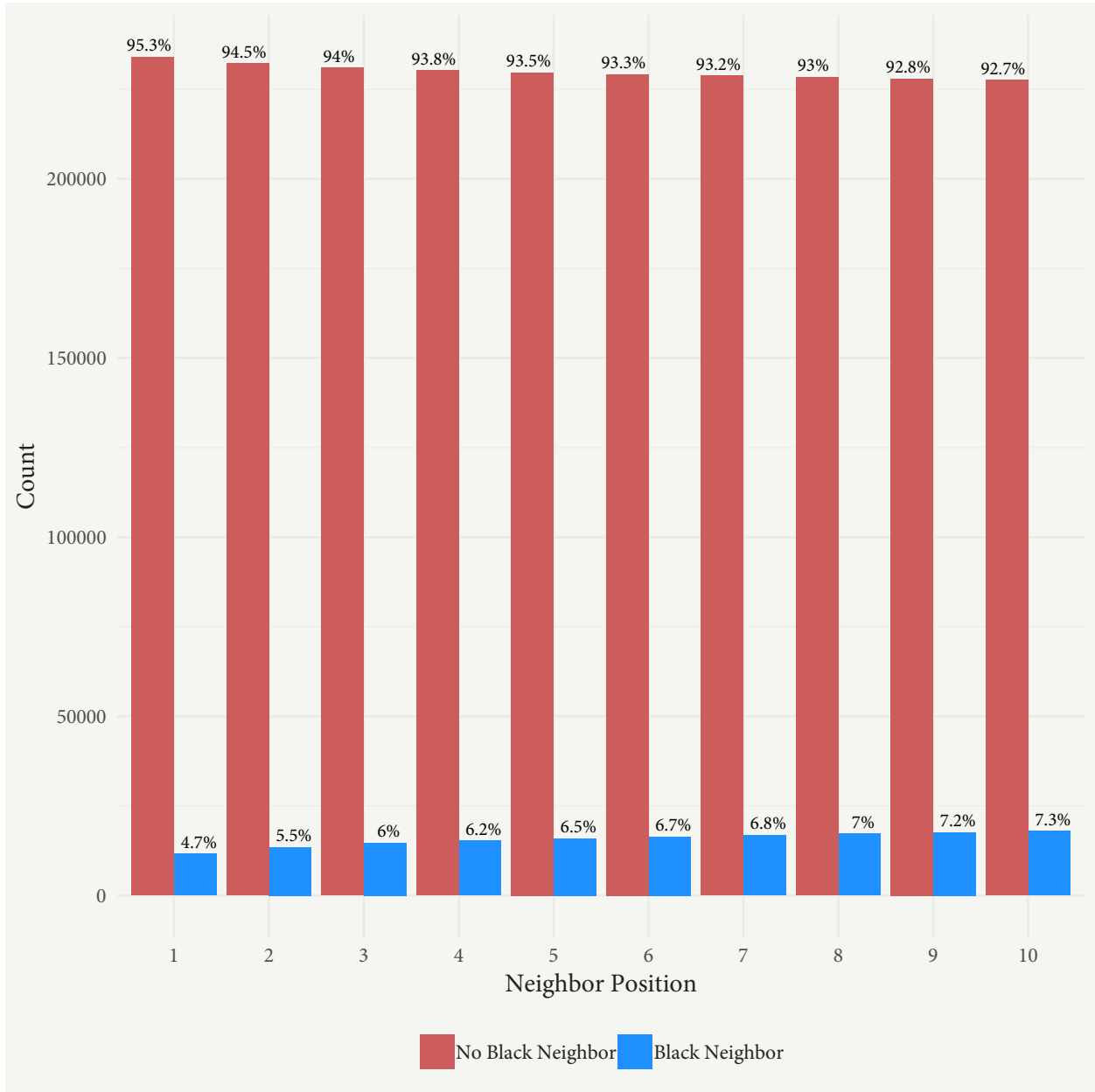
By measuring racial exposure at the individual level, we avoid problems of aggregation and scale common to geographic analysis. Conventional measures of racial segregation rely on aggregated measures of racial dissimilarity or exposure within a defined geographic unit. First, these measures mask variation in spatial location within these areal units, where Blacks and whites could live next door or far apart within the given unit, and the measures cannot distinguish between these patterns (White, 1983). Second, these measures are subject to the Modifiable Areal Unit Problem (Openshaw, 1983), meaning that the measure of segregation is as much a function of variation in the boundaries of geographic units as are the underlying population distributions. We overcome these problems because we determine racial exposure in the context of the individual, who their most proximate neighbors are, and we can test the effect of neighbors of varying distances from the individuals in our sample.

4.1.2 Identification Strategy: Fixed Effects Model

We estimate the percent change in the likelihood of being a Democrat in 2017 associated with having a neighbor of a different race in each of the 10 household proximity pairs. We can infer that this observed effect is causal to the extent that we can assume that there are no omitted variables that might influence both racial exposure and future partisanship. The major threat to inference is that the patterns we observe between 1940 racial exposure and 2017 partisanship is a result of selective sorting in 1940, where more racially-liberal people or more Democratic leaning people choose to live (or do not opt to move away from) neighbors of a different race. To account for this potentiality and to strengthen our causal argument, we estimate a series of linear models with increasingly punitive geographic fixed effects. The idea is that, by limiting the statistical comparison to smaller and smaller geographies, we

urban Black/white Dissimilarity to be 0.67. Dissimilarity ranges from 0 to 1, where 1 is perfectly segregated. According to Logan and Stults (2011), the county-level national average for Black/white Dissimilarity was 0.59.

Figure 7: Whites with Black neighbors by neighbor position



Note: Data taken from the 1940 US Federal Census available from IPUMS.

are comparing increasingly alike individuals, particularly in the context of their residential decisions, who differ only by whether or not they happen to have a neighbor of a different race.

As such, we estimate separate specifications with state, county, enumeration district fixed effects, pre-defined census geographies. Enumeration districts are sub-county geographic units defined as the area for which an enumerator (census taker going house by house) could complete a count of the area's population for a given census year. Enumeration districts are comparable in population but can vary widely in land area, depending on population density. We also estimate models with fixed effects based on Census manuscript aggregations. Each household in the 1940 Census has a reel number and a reel-page number. Based on the assumption that position in the Census manuscript is a proxy for geographic proximity, we estimate models with fixed effects by reel, by page, and by adjacent groups of 2, 5, and 10 pages. We present two versions of paired-page fixed effects, pairing pages up and down the Census manuscript. Table 1 presents descriptive statistics for the different fixed effect levels, ordered by their approximate geographic size, from largest to smallest. The table displays the number of unique blocks for each fixed effect level, as well as the average number of units in each block and the range of units in each block. The percentage of blocks for each fixed effects level that contain 2 or more individuals is also presented. We see that even at the more restrictive fixed effect levels we have sizable blocks in which to make comparisons. Note that a page represents a small level of geography, with approximately 10-20 households on each page, giving us fairly high levels of confidence in our identification strategy.

For each fixed effects level, we estimate two specifications, one with control covariates and one without. We estimate these specifications on the subset of our dataset that are reported as whites in the 1940 Census, testing the effect of Black neighbors on future partisanship. Each specification includes the set of 10 binary variables ($k = 1$ through 10) representing whether a white individual in our dataset has a Black neighbor living at that distance away

Table 1: Fixed effects levels descriptive statistics

	Fixed Effect Level	Count	Mean	Min	Max	% > 2 people
1	State	49	5301	114	58008	100%
2	County	3046	85	1	23085	97.9%
3	Enumdist	3123	83	1	11186	97.95%
4	Reel	4577	57	1	929	99.91%
5	Reel Page 10	127612	2	1	25	36.9%
6	Reel Page 5	158910	2	1	15	30.52%
7	Reel Page 2	197244	1	1	15	21.2%
8	Reel Page 2 Alt	197227	1	1	15	21.14%
9	Reel Page	219404	1	1	15	14.4%

from them on the Census page. In the specifications with covariate controls, we control for an individual’s age and the income of the head of their household. A potential criticism of an observed effect of a Black neighbor on partisanship is that race is serving as a proxy for socio-economic status, so we cannot separate the effect of exposure to Blacks from the effect of exposure to poverty or inequality. To account for this, we include binary variables indicating if one or more of the two households at each position has a head of the household income in the bottom 20th percentile of national head of the household income¹⁰ The specifications are shown below:

$$2017 \text{ Democrat}_{i,g} = \alpha + \sum_{k=1}^{10} \beta_k \text{Black Neighbor}_{k,i} + \lambda_g + \varepsilon_{i,g} \quad (1)$$

¹⁰ The cutoff for the bottom 20th percentile of national head of the household income in 1940 is \$200 dollars.

$$2017 \text{ Democrat}_{i,g} = \alpha + \sum_{k=1}^{10} \beta_k \text{Black Neighbor}_{k,i} + \gamma \text{Age}_i + \delta \text{Income}_i + \sum_{k=1}^{10} \theta_k \text{Poor Neighbor}_{k,i} + \lambda_g + \varepsilon_{i,g} \quad (2)$$

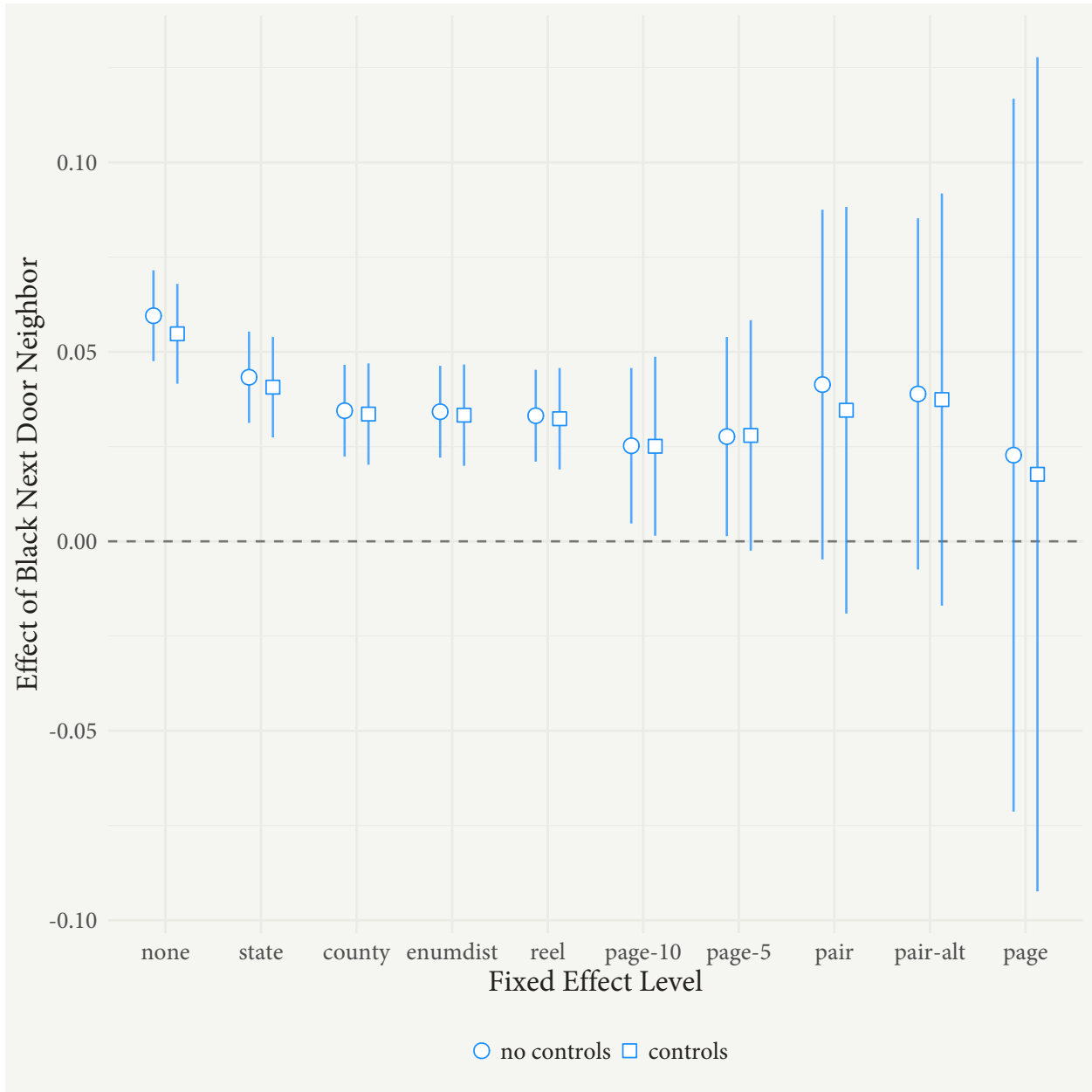
where α is the intercept, λ_g is the respective geographic fixed effects, and $\varepsilon_{i,g}$ is the error term. The coefficient on each Black neighbor indicator variable can be interpreted as the effect of having a Black neighbor living k houses down on the likelihood of being a registered Democrat in 2017, conditional on being alive and registered in 2017.

4.1.3 Results

Using this strategy, we estimate the model results at the different fixed-effect levels. The coefficient estimate for the effect of living immediately next door to a Black person is consistently significant down to neighborhood level approximation fixed effects, losing significance at the most punitive fixed-effect levels where our effective sample is smallest. As displayed in Figure 8, the point estimate is stable across fixed-effect levels, ranging between 1.8 and 6.0 percentage points, providing strong evidence that there is a causal effect of having an immediate next door Black neighbor on future Democratic partisanship for whites. Table 2, 3, and 4 present the results in greater detail. The coefficient estimates for Black neighbors at positions less proximate than immediately next door are less stable and lose significance as fixed effect levels become more punitive. The lack of evidence for persistent effects of less proximate Black neighbors perhaps indicates that close, if not daily, contact is necessary for racial exposure to influence political attitudes long-term.

The results suggest that whites who live next door to a Black person during their formative years are more likely to identify as Democrats later in life. While we account for an individual's age in 1940 in our main specification, we might expect that the extent to which a cross-race neighbor leaves a lasting impression varies by an individual's age. For example, a

Figure 8: Effect of Black 1940 next door neighbor on 2017 Democratic partisanship



Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level.

Table 2: Effect of Black neighbors on future partisanship

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Black neighbor (k=1)	0.060*** (0.006)	0.055*** (0.006)	0.043*** (0.006)	0.041*** (0.007)	0.034*** (0.006)	0.034*** (0.007)	0.034*** (0.006)	0.033*** (0.007)
Black neighbor (k=2)	0.021*** (0.006)	0.019*** (0.007)	0.010 (0.006)	0.009 (0.007)	0.003 (0.006)	0.004 (0.007)	0.002 (0.006)	0.003 (0.007)
Black neighbor (k=3)	0.020*** (0.006)	0.016** (0.006)	0.011* (0.006)	0.008 (0.007)	0.006 (0.006)	0.004 (0.007)	0.006 (0.006)	0.005 (0.007)
Black neighbor (k=4)	0.016*** (0.006)	0.011 (0.006)	0.007 (0.006)	0.004 (0.007)	0.005 (0.006)	0.003 (0.007)	0.005 (0.006)	0.003 (0.007)
Black neighbor (k=5)	0.022*** (0.006)	0.022*** (0.006)	0.012** (0.006)	0.014** (0.007)	0.009 (0.006)	0.010 (0.007)	0.009 (0.006)	0.009 (0.007)
Black neighbor (k=6)	0.023*** (0.006)	0.017*** (0.006)	0.014** (0.006)	0.010 (0.007)	0.009 (0.006)	0.005 (0.007)	0.009 (0.006)	0.005 (0.007)
Black neighbor (k=7)	0.015*** (0.006)	0.015** (0.006)	0.008 (0.006)	0.010 (0.007)	0.008 (0.006)	0.011 (0.007)	0.008 (0.006)	0.011 (0.007)
Black neighbor (k=8)	0.018*** (0.006)	0.013** (0.006)	0.007 (0.006)	0.005 (0.007)	0.003 (0.006)	0.001 (0.007)	0.003 (0.006)	0.001 (0.007)
Black neighbor (k=9)	0.020*** (0.006)	0.017*** (0.006)	0.009 (0.006)	0.009 (0.007)	0.004 (0.006)	0.005 (0.007)	0.004 (0.006)	0.005 (0.007)
Black neighbor (k=10)	0.024*** (0.005)	0.021*** (0.006)	0.006 (0.006)	0.006 (0.006)	0.002 (0.006)	0.002 (0.006)	0.001 (0.006)	0.002 (0.006)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
FE	None	None	State	State	County	County	Enumdist	Enumdist
N	238,344	215,581	238,344	215,581	238,344	215,581	238,344	215,581

*p < .1; **p < .05; ***p < .01

Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level.

Table 3: Effect of Black neighbors on future partisanship

	(1)	(2)	(3)	(4)	(5)	(6)
Black neighbor (k=1)	0.033*** (0.006)	0.032*** (0.007)	0.025** (0.010)	0.025** (0.012)	0.028** (0.013)	0.028* (0.016)
Black neighbor (k=2)	0.002 (0.006)	0.003 (0.007)	0.002 (0.010)	0.007 (0.012)	0.004 (0.013)	0.005 (0.015)
Black neighbor (k=3)	0.005 (0.006)	0.004 (0.007)	0.008 (0.010)	0.010 (0.012)	0.015 (0.013)	0.018 (0.015)
Black neighbor (k=4)	0.004 (0.006)	0.003 (0.007)	-0.009 (0.010)	-0.010 (0.012)	-0.016 (0.013)	-0.018 (0.015)
Black neighbor (k=5)	0.008 (0.006)	0.010 (0.007)	0.001 (0.010)	0.004 (0.012)	0.001 (0.013)	0.005 (0.015)
Black neighbor (k=6)	0.007 (0.006)	0.004 (0.007)	0.017* (0.010)	0.015 (0.012)	0.018 (0.013)	0.015 (0.015)
Black neighbor (k=7)	0.006 (0.006)	0.009 (0.007)	-0.001 (0.010)	0.002 (0.012)	-0.003 (0.013)	0.002 (0.015)
Black neighbor (k=8)	0.0001 (0.006)	-0.002 (0.007)	0.0003 (0.010)	0.0003 (0.012)	-0.001 (0.013)	-0.005 (0.015)
Black neighbor (k=9)	0.003 (0.006)	0.004 (0.007)	0.004 (0.010)	0.009 (0.012)	0.015 (0.013)	0.018 (0.015)
Black neighbor (k=10)	0.001 (0.006)	0.002 (0.006)	-0.002 (0.010)	-0.002 (0.011)	-0.009 (0.012)	-0.008 (0.014)
Controls	No	Yes	No	Yes	No	Yes
FE	Reel	Reel	Page 10	Page 10	Page 5	Page 5
N	238,344	215,581	238,344	215,581	238,344	215,581

*p < .1; **p < .05; ***p < .01

Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level.

Table 4: Effect of Black neighbors on future partisanship

	(1)	(2)	(3)	(4)	(5)	(6)
Black neighbor (k=1)	0.041* (0.024)	0.035 (0.027)	0.039* (0.024)	0.037 (0.028)	0.023 (0.048)	0.018 (0.056)
Black neighbor (k=2)	-0.002 (0.024)	0.002 (0.027)	0.005 (0.023)	0.007 (0.027)	0.001 (0.046)	0.004 (0.053)
Black neighbor (k=3)	0.013 (0.024)	0.011 (0.028)	0.015 (0.023)	0.015 (0.027)	0.012 (0.045)	0.016 (0.053)
Black neighbor (k=4)	-0.013 (0.023)	-0.005 (0.028)	-0.009 (0.023)	-0.009 (0.027)	-0.004 (0.044)	0.004 (0.052)
Black neighbor (k=5)	0.002 (0.023)	0.003 (0.026)	-0.004 (0.023)	-0.005 (0.027)	-0.001 (0.042)	-0.001 (0.049)
Black neighbor (k=6)	0.011 (0.023)	0.006 (0.027)	0.016 (0.023)	0.019 (0.026)	0.030 (0.042)	0.036 (0.049)
Black neighbor (k=7)	0.004 (0.023)	0.006 (0.027)	0.017 (0.023)	0.011 (0.027)	0.019 (0.044)	0.023 (0.051)
Black neighbor (k=8)	0.001 (0.022)	0.001 (0.026)	0.002 (0.023)	-0.006 (0.026)	0.003 (0.042)	0.003 (0.049)
Black neighbor (k=9)	0.013 (0.022)	0.017 (0.026)	0.004 (0.022)	0.007 (0.026)	0.021 (0.041)	0.020 (0.049)
Black neighbor (k=10)	-0.017 (0.022)	-0.015 (0.025)	-0.013 (0.022)	-0.003 (0.025)	-0.022 (0.041)	-0.019 (0.047)
Controls	No	Yes	No	Yes	No	Yes
FE	Page 2	Page 2	Page 2 Alt	Page 2 Alt	Page	Page
N	238,344	215,581	238,344	215,581	238,344	215,581

*p < .1; **p < .05; ***p < .01

Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level.

person at age 2 is less perceptive of their surroundings, less cognizant of who their neighbors are, than a 7 year old. As such, effects that vary by age will also give us confidence that our results are driven by cross-race exposure and not a spurious variable. To test this, we estimate a model where we interact age with the indicator variable for the immediate next door neighbor. The results of the model are displayed in Figure 5. The coefficient on interaction between age and the next door neighbor indicator is positive and significant across models, save the most restricted fixed effects specifications, where it loses significance. These results indicate that the effect of a Black neighbor is strongly moderated by age, with the long-run effect on partisanship increasing with age in 1940. The effect of a Black neighbor on future partisanship increases by approximately 0.6 to 1 percentage points for each year increase in age. The coefficient on the indicator for a Black next door neighbor is also now effectively 0, meaning that an individual at age 0 is not experiencing an increase in their probability

in being a registered Democrat in 2017. This relationship is demonstrated in Figure 9 for the model with enumeration district fixed-effects, which also plots the support for the age variable in the sample. Figure A6 in the Appendix shows the same plot for the model with county fixed-effects.

The causal process we are testing is one where a young white person grows up living immediately next door to a Black family, potentially interacting with Black children of similar ages. This interaction could shape racial attitudes longterm, thus shaping partisanship. As such, we expect that the results should hold when we look only at Whites in our sample who grew up next to Black families with children. Consistent effects in this comparison will give us further confidence that our results are not from a spurious variable driving geographic sorting and partisanship, but from cross-race exposure and interaction. To test this, we estimate the effect of living next to a household with at least one Black child (at each position K) on Whites likelihood of identifying as a Democrat in 2017. We match on whether a person in our sample lived next to a family with children, comparing Whites who lived next to Black families with children to Whites who lived next to non-Black families with children. We also estimate the effect of living next door to a Black family with a male child. The individuals in our sample are all males, and may have been more likely to play with their next door neighbor if the child was of the same gender. In this analysis, we match based on whether the individuals in our sample lived next to a family with at least one male child. The results of these two analyses are shown in Figures ?? and ??. Up to neighborhood-approximate level fixed effects, effect sizes and significance do not differ from our main specification for both the neighbors with children and neighbors with male children comparison¹¹. At more punitive fixed effect levels, the results are not statistically distinguishable from zero, and the average effect sizes themselves are variable. This imprecision is in part a function of the reduction in power from the matching analysis. Generally, however, the results provide

¹¹The full regressions tables for these specifications are shown in Figures A1 through A6 in the Appendix

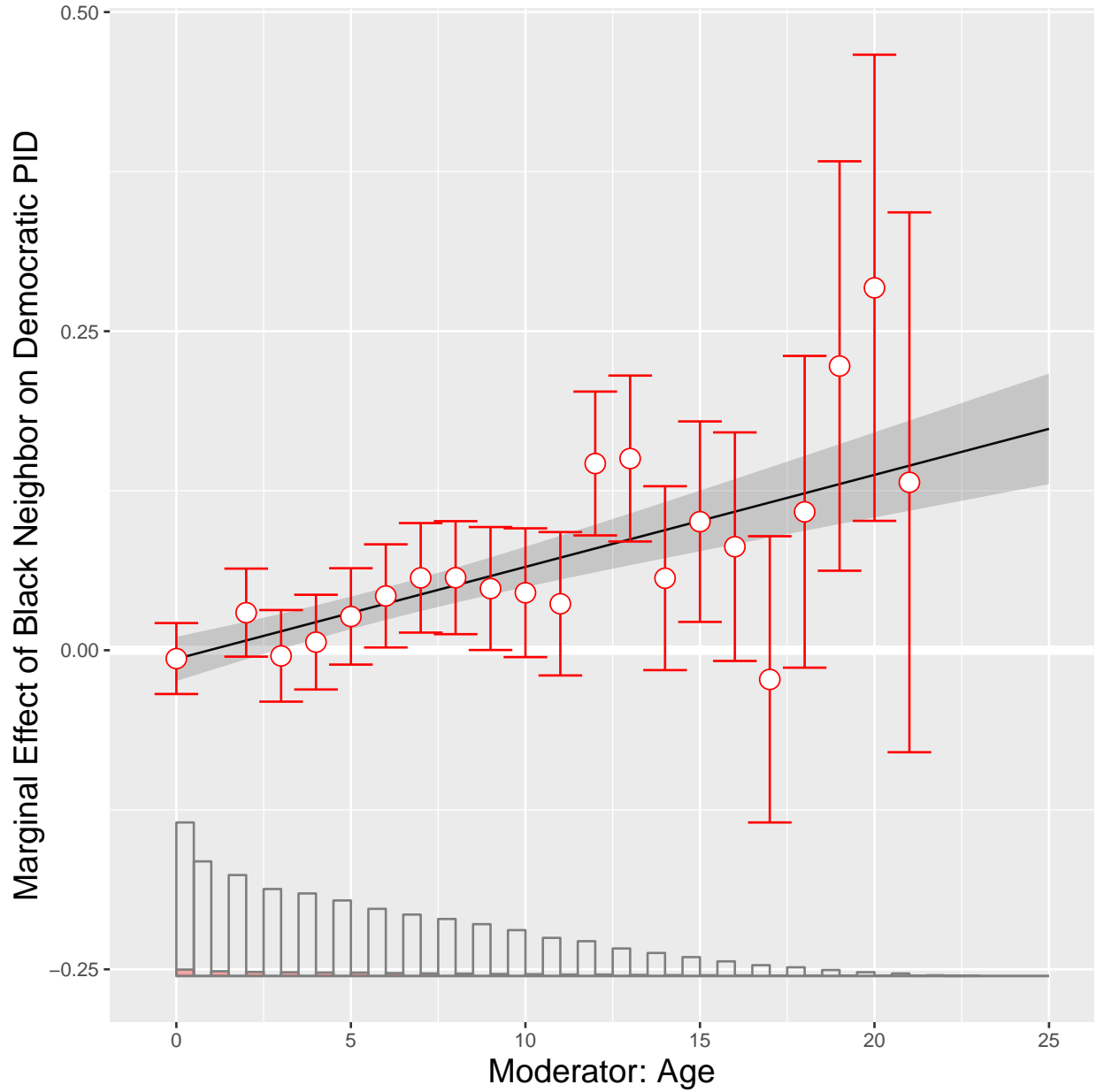
Table 5: Effect of Black neighbors on future partisanship with age interaction

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Black neighbor (k=1)	0.016* (0.009)	0.004 (0.009)	-0.007 (0.009)	-0.007 (0.009)	-0.006 (0.009)	-0.022 (0.016)	-0.016 (0.021)	-0.012 (0.036)	0.019 (0.037)	-0.018 (0.069)
Black neighbor (k=2)	0.019*** (0.007)	0.009 (0.007)	0.004 (0.007)	0.004 (0.007)	0.004 (0.007)	0.007 (0.012)	0.006 (0.015)	0.003 (0.027)	0.007 (0.027)	0.004 (0.053)
Black neighbor (k=3)	0.016** (0.006)	0.008 (0.007)	0.004 (0.007)	0.004 (0.007)	0.004 (0.007)	0.010 (0.012)	0.019 (0.015)	0.011 (0.028)	0.015 (0.027)	0.016 (0.053)
Black neighbor (k=4)	0.011 (0.006)	0.004 (0.007)	0.003 (0.007)	0.003 (0.007)	0.003 (0.007)	-0.010 (0.012)	-0.018 (0.015)	-0.005 (0.028)	-0.009 (0.027)	0.003 (0.053)
Black neighbor (k=5)	0.022*** (0.006)	0.014** (0.007)	0.010 (0.007)	0.010 (0.007)	0.010 (0.007)	0.004 (0.012)	0.005 (0.015)	0.003 (0.026)	-0.005 (0.027)	-0.001 (0.049)
Black neighbor (k=6)	0.017*** (0.006)	0.010 (0.007)	0.005 (0.007)	0.005 (0.007)	0.004 (0.007)	0.015 (0.012)	0.015 (0.015)	0.005 (0.027)	0.019 (0.026)	0.036 (0.049)
Black neighbor (k=7)	0.015** (0.006)	0.010 (0.007)	0.010 (0.007)	0.010 (0.007)	0.009 (0.007)	0.002 (0.012)	0.003 (0.015)	0.006 (0.027)	0.011 (0.027)	0.023 (0.051)
Black neighbor (k=8)	0.014** (0.006)	0.005 (0.007)	0.001 (0.007)	0.001 (0.007)	-0.002 (0.007)	0.001 (0.012)	-0.005 (0.015)	0.001 (0.026)	-0.006 (0.026)	0.002 (0.049)
Black neighbor (k=9)	0.017*** (0.006)	0.009 (0.007)	0.005 (0.007)	0.005 (0.007)	0.004 (0.007)	0.010 (0.012)	0.018 (0.015)	0.018 (0.026)	0.007 (0.026)	0.020 (0.049)
Black neighbor (k=10)	0.020*** (0.006)	0.005 (0.006)	0.002 (0.006)	0.002 (0.006)	0.002 (0.006)	-0.002 (0.011)	-0.009 (0.014)	-0.016 (0.025)	-0.003 (0.025)	-0.019 (0.047)
age:Black neighbor (k=1)	0.007*** (0.001)	0.007*** (0.001)	0.007*** (0.001)	0.007*** (0.001)	0.007*** (0.001)	0.008*** (0.002)	0.008*** (0.003)	0.008*** (0.004)	0.003 (0.004)	0.007 (0.007)
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	
FE	None	State	County	Enumdist	Reel	Page 10	Page 5	Page 2	Page 2 Alt	Page
N	215,581	215,581	215,581	215,581	215,581	215,581	215,581	215,581	215,581	215,581

*p < .1; **p < .05; ***p < .01

Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level.

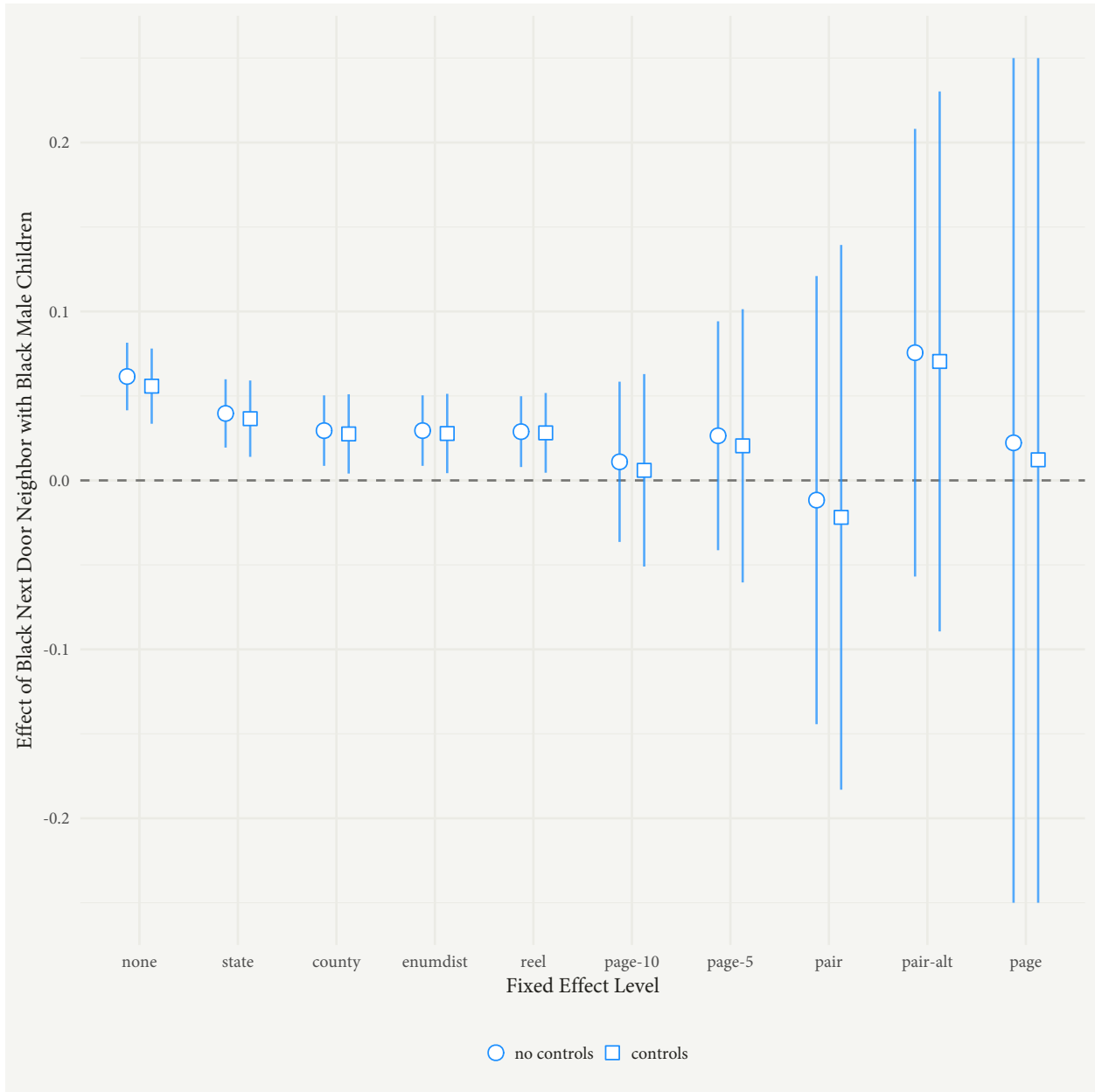
Figure 9: Neighbor effect by age, enumeration district fixed effects



Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level.

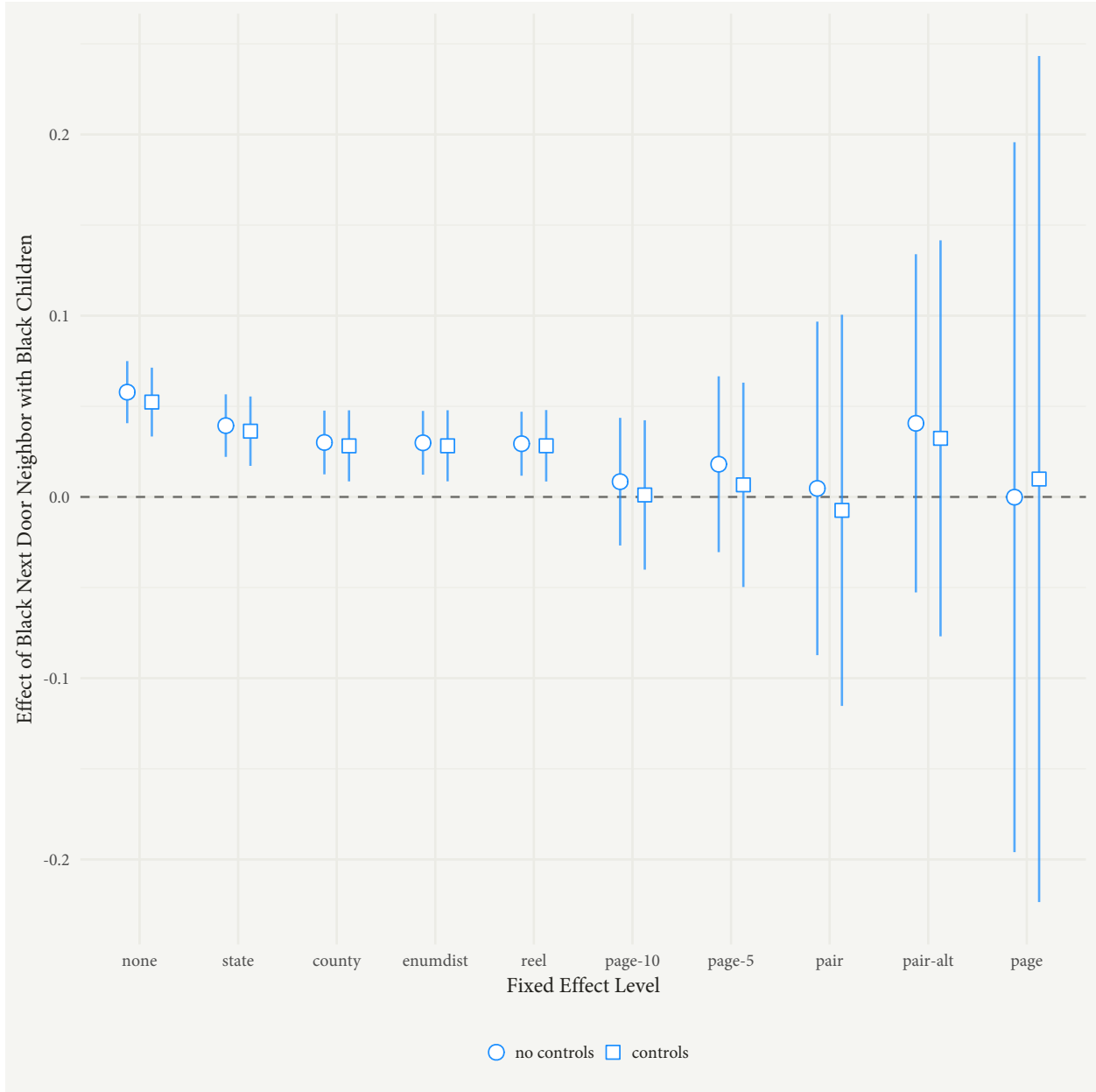
further evidence that cross-race exposure is driving the effects on partisanship.

Figure 11: Effect of Black 1940 next door neighbor with male children on 2017 Democratic partisanship



Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level. Control group in this analysis are on Whites with a next door neighbor with non-Black male children.

Figure 10: Effect of Black 1940 next door neighbor with children on 2017 Democratic partisanship



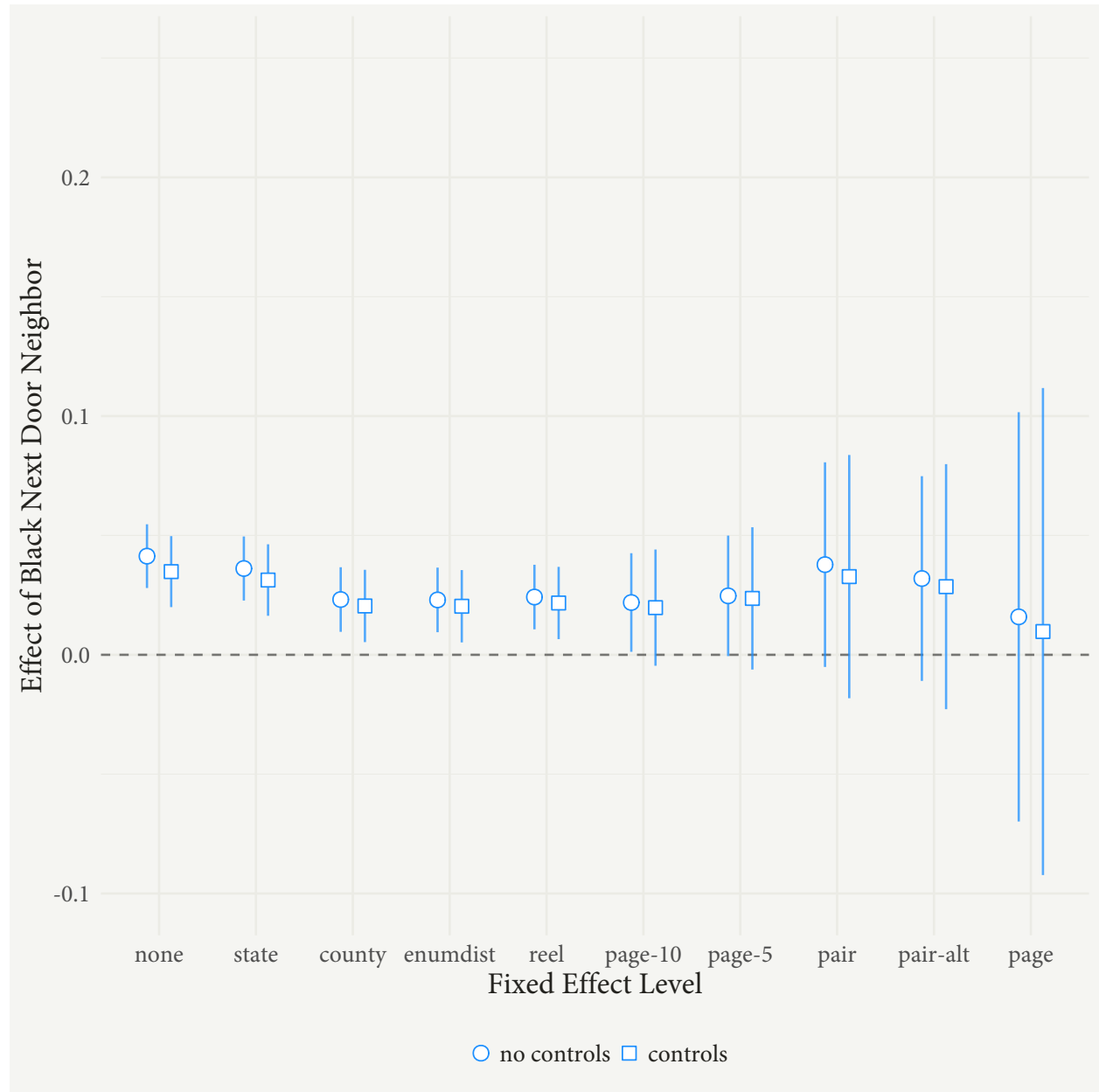
Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level. Control group in this analysis are on Whites with a next door neighbor with non-Black children.

Another threat to inference in our analysis is that the results may be driven by the persistence of Democratic party identification in the southern region of the United States. In the first half of the twentieth century, the South was controlled by racially conservative Whites that comprised a key faction of the Democratic Party (Key, 1949). As the Democratic party outside the South incorporated racial minorities, eventually embracing the Civil Rights Movement, racially conservative White Southerners begin to defect to the Republican Party (Carmines and Stimson, 1990; Schickler, 2016). Despite this partisan transition, Democratic identification in Southern states, particularly among older voters, persisted into the twenty-first century, even if vote choice has shifted (Mickey, 2015). If our results are driven by individuals in our sample who lived in the South in the 1940s, then observed effect on Democratic partisanship may be the result of individuals living in states with high Black populations being more likely to be racially conservative lifelong Democrats who never formally changed their partisan identification. This explanation is directly counter to our model of cross-race exposure causing Whites to become more racially liberal, causing them to identify as Democrats.

To test against this possibility, we estimate the results from the main specification subset by individuals in our sample who live in the South in 1940 and those living outside the South. We use the 1940 Census region definitions, including an individual in the South subset if they are recorded as living in Alabama, Arkansas, Delaware, DC, Florida, Georgia, Kentucky, Louisiana, Maryland, Mississippi, North Carolina, Oklahoma, South Carolina, Tennessee, Texas, Virginia, and West Virginia in the 1940 Census. The effect of living next door to a Black person in 1940 on future Democratic partisanship is shown in Figures ?? and ?? for the South and non-South, respectively. The effect sizes are consistently significant even down to small geographic fixed effect levels in both subsets, but are noticeably larger in the non-South sample. These results suggest that the effects are not driven by racially conservative lifelong Democrats in Southern states, and are even smaller in these states with

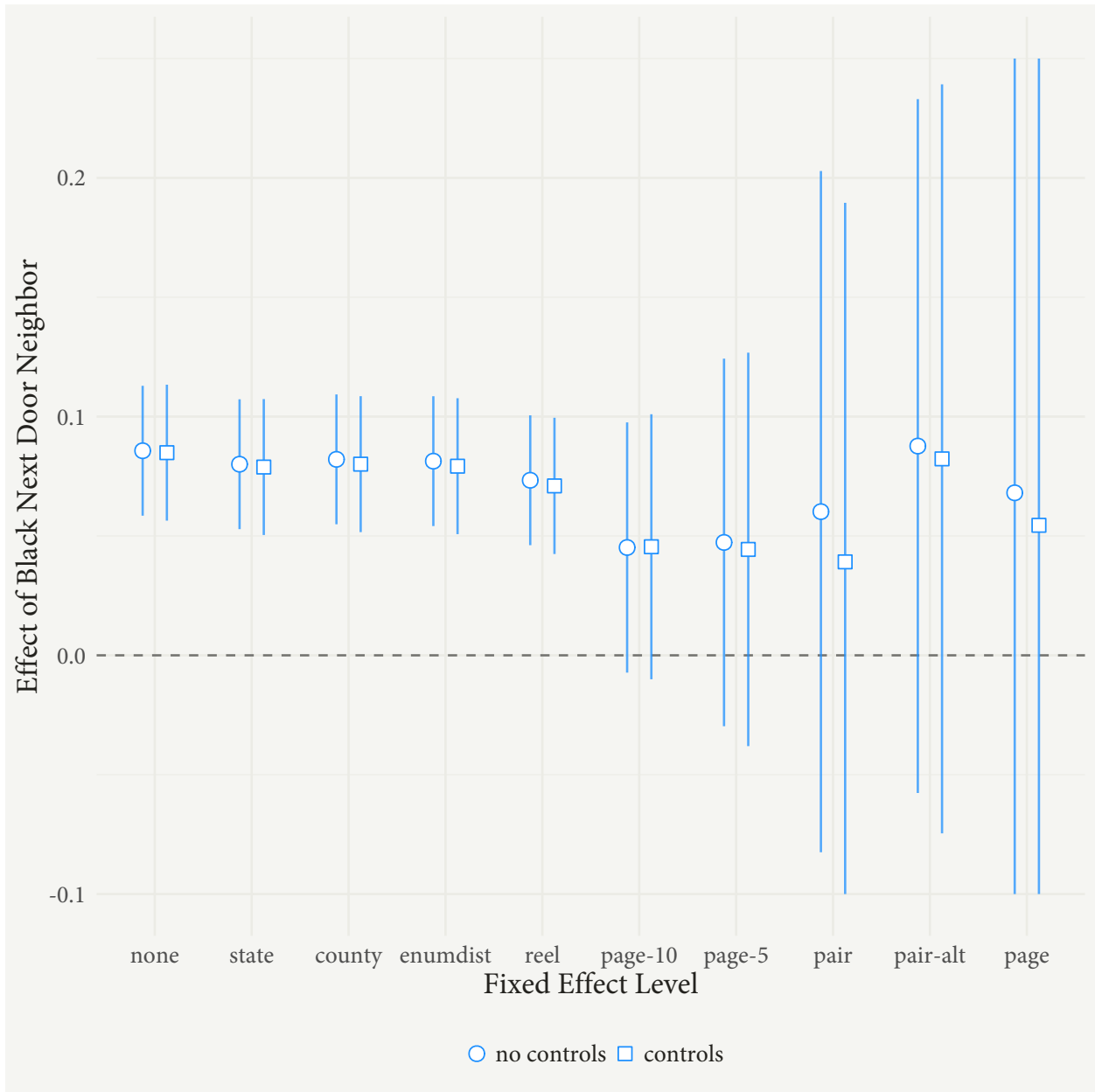
higher levels of modern conservatism and Republican party identification.

Figure 12: Effect of Black 1940 next door neighbor on 2017 Democratic partisanship (South)



Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level. Sample is limited to individuals living in the South in 1940.

Figure 13: Effect of Black 1940 next door neighbor on 2017 Democratic partisanship (Non-South)



Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level. Sample is limited to individuals living outside the South in 1940.

4.2 Early-Life Exposure to Different Classes

Exposure to inequality may produce lasting changes in political attitudes and behavior. Similar to our analysis of racial contact, we measure exposure to neighbors of different incomes or classes in 1940, and estimate its effect on future partisanship.

4.2.1 Measuring Class Exposure

Class exposure is measured similarly to racial exposure, identifying the relative income characteristics of neighbors to each individual in our sample. Income segregation is generally measured similar to racial segregation, measuring income dissimilarity of a geographic unit by comparing the income levels of smaller geographic areas within the unit (Massey and Eggers, 1993; Jargowsky, 1996; Reardon and Bischoff, 2011). These measures can incorporate either continuous or categorical income data, but still suffer from the previously discussed methodological challenges of geographic aggregation and scale. We employ a straightforward approach, comparing the income categories of individuals in our dataset to their neighbors.

While future versions of the project may incorporate continuous measures of income difference between individuals and neighbors, we rely here on comparisons of an individual's income tier to that of their neighbors. We examine extreme cases of class difference between neighbors, looking at cases where someone in the top 20% of the nationwide income distribution and someone in the bottom 20% of the nationwide income distribution are neighbors¹². This represents a strong example of exposure to a much higher or much lower class. If we do not observe an effect of class exposure in this context, we should not expect less stark differences in neighbors' class to exhibit an effect.

Levels of income segregation in 1940 are much lower than racial segregation. Individuals are quite likely to have some neighbors with much higher or much lower incomes, suggesting

¹²The cutoff for the bottom 20th percentile and top 20th percentile of the nationwide head of the household income distribution is \$200 and \$1716, respectively

that exposure to inequality is relatively high at this point in U.S. history. The neighbor breakdowns by neighbor position are displayed in Figures 14 and 15. Even households in the top 20% of the income distribution frequently reside near neighbors in the bottom 20%.

4.2.2 Identification Strategy: Fixed Effects Model

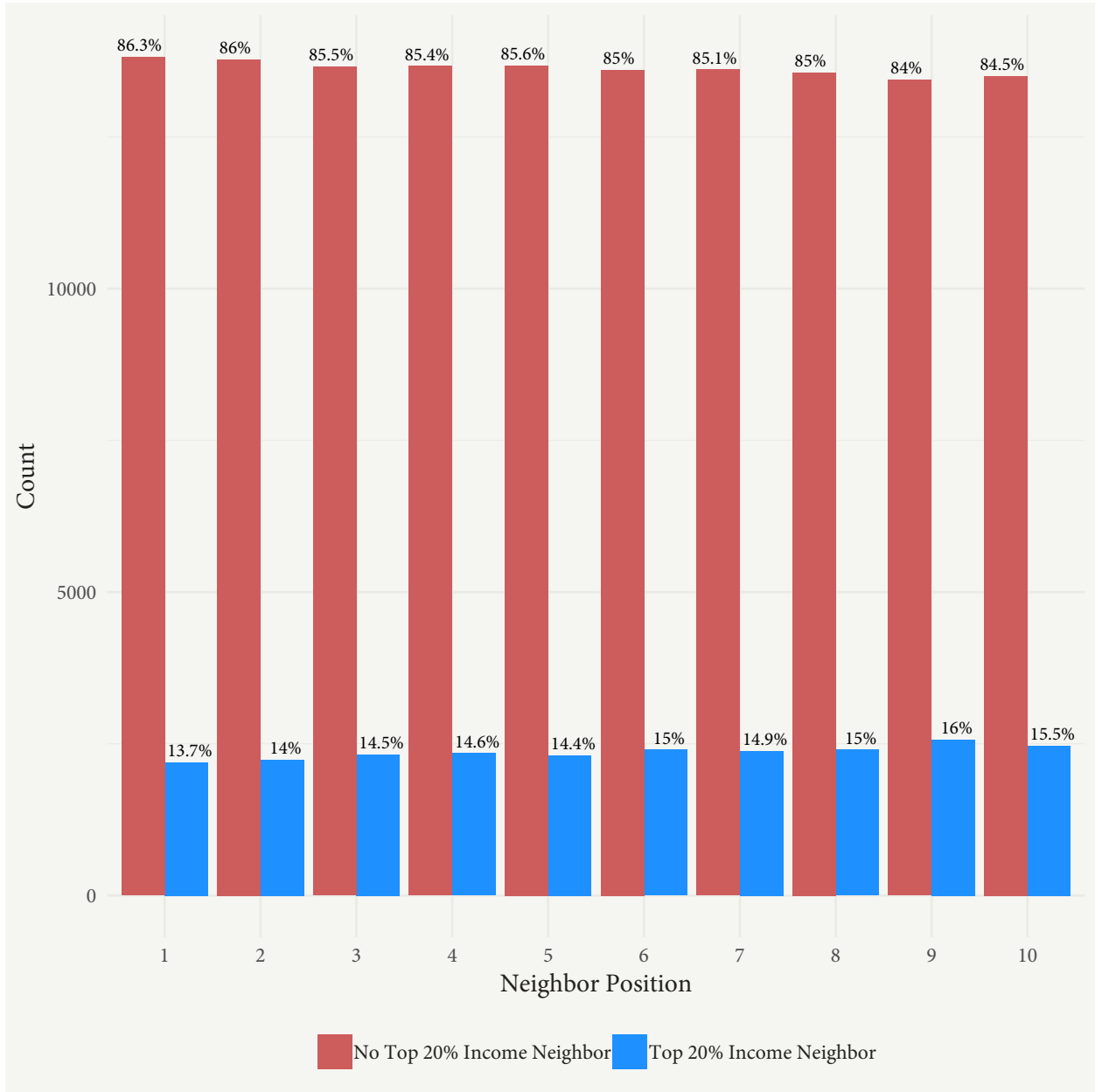
The identification strategy for measuring the effects of class exposure is similar to our approach for racial exposure, using a series of linear models with increasingly restrictive geographic fixed effects. We estimate models with fixed effects at the state, county, enumeration district, reel, ten page grouping, five page grouping, pair page grouping, and single page levels. We also estimate specifications with and without covariate controls, controlling for individual age and head of household income as well as whether the various neighbors are Black. The general specifications are shown below:

$$2017 \text{ Democrat}_{i,g} = \alpha + \sum_{k=1}^{10} \beta_k \text{Different Class Neighbor}_{k,i} + \lambda_g + \varepsilon_{i,g} \tag{3}$$

$$2017 \text{ Democrat}_{i,g} = \alpha + \sum_{k=1}^{10} \beta_k \text{Different Class Neighbor}_{k,i} + \gamma \text{Age}_i + \delta \text{Income}_i + \sum_{k=1}^{10} \theta_k \text{Black Neighbor}_{k,i} + \lambda_g + \varepsilon_{i,g} \tag{4}$$

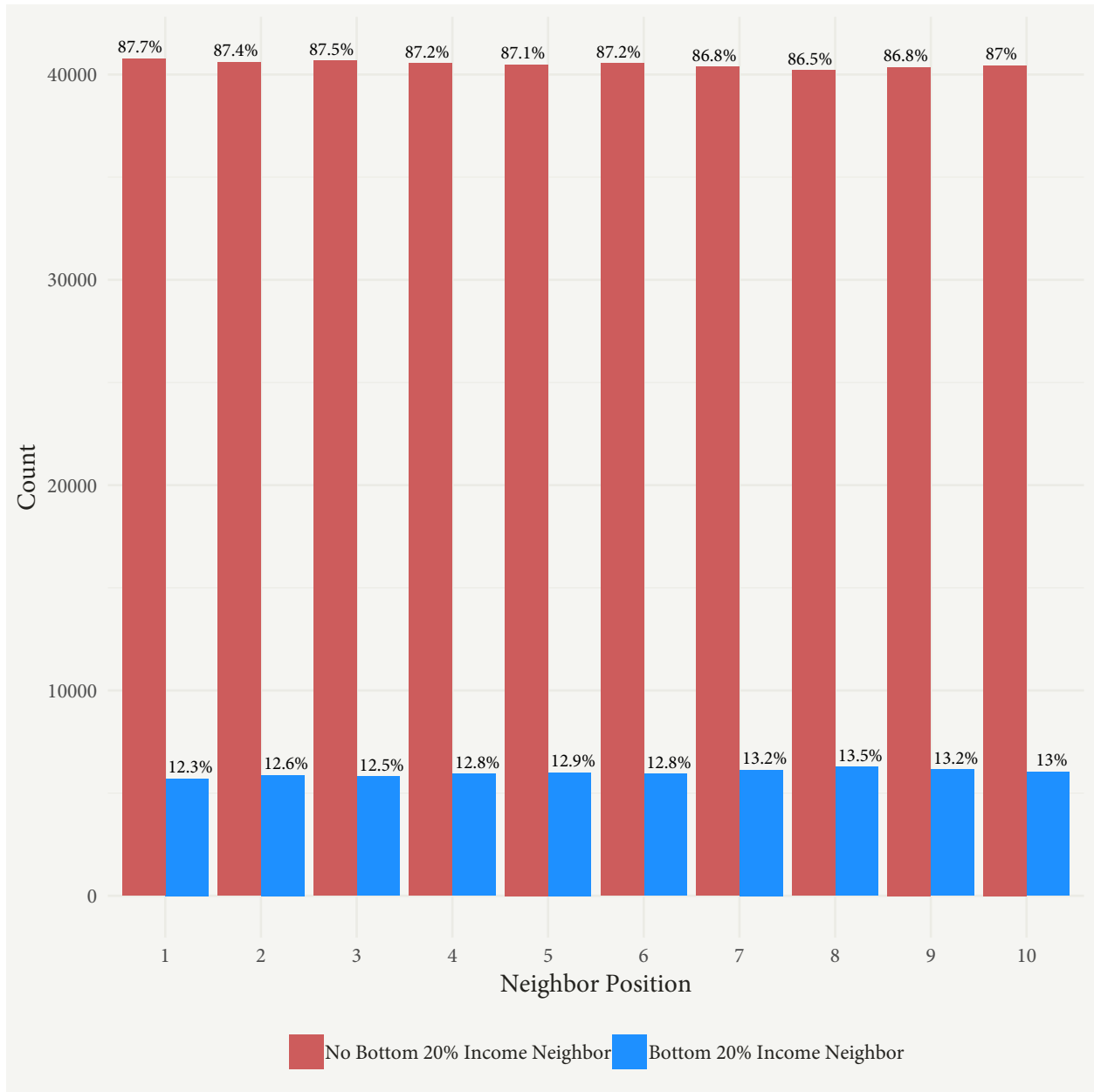
where α is the intercept, λ_g is the respective geographic fixed effects, and $\varepsilon_{i,g}$ is the error term. The coefficient on each class neighbor indicator variable can be interpreted as the effect of having a neighbor of a different income category living k houses down on the likelihood of being a registered Democrat in 2017, conditional on being alive and registered in 2017. Because the effects of cross-class exposure may be different for the rich and poor, we subset our data and estimate the model separately for the richer and poorer group.

Figure 14: Bottom 20% income households with top 20% neighbors



Note: Data taken from the 1940 US Federal Census available from IPUMS.

Figure 15: Top 20% income households with bottom 20% neighbors



Note: Data taken from the 1940 US Federal Census available from IPUMS.

4.2.3 Results

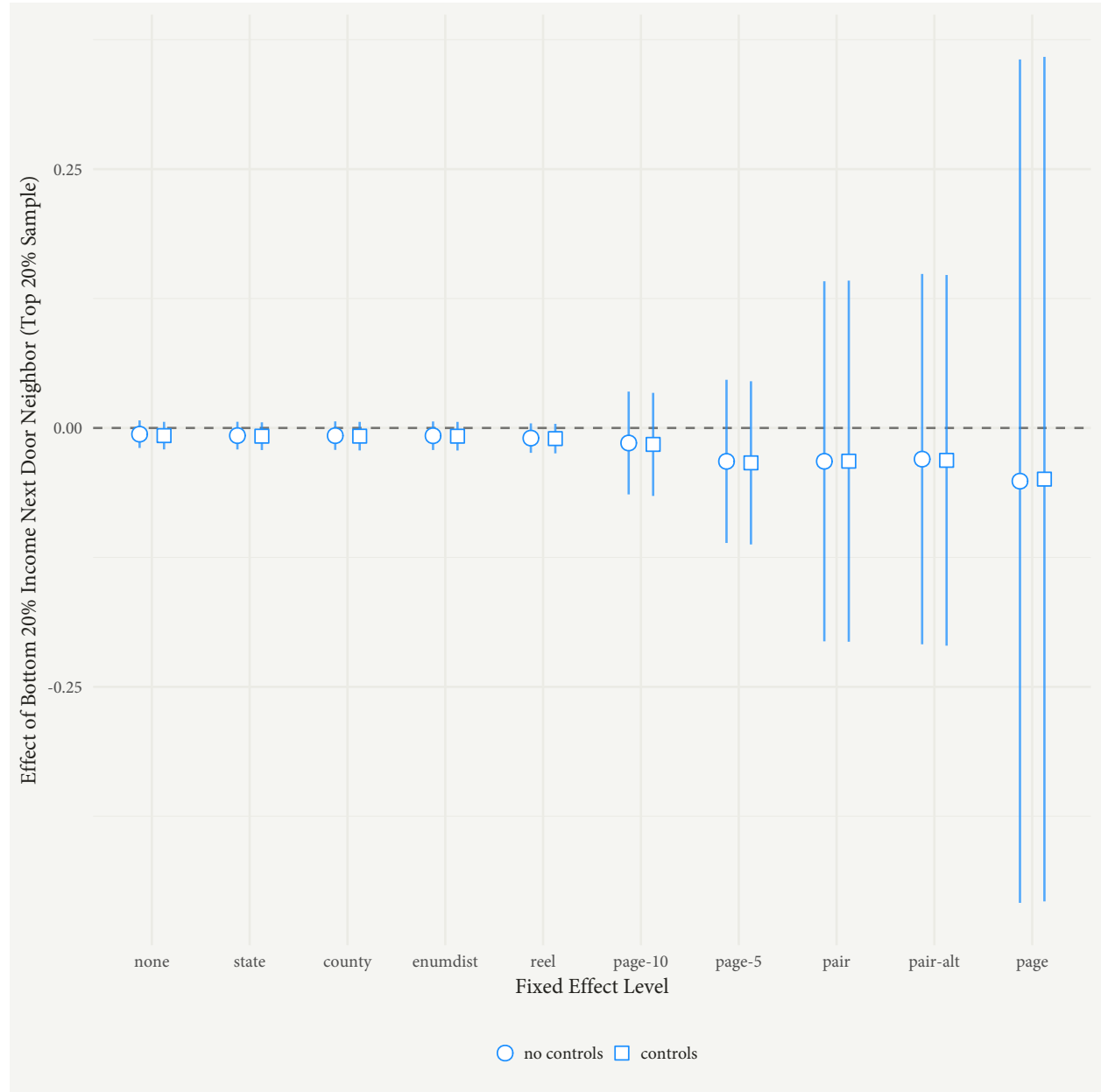
The results indicate that class exposure does not influence long-term partisanship. As shown in Figures 16 and 17 for the wealthy and poor, respectively, the effect of a poor or rich neighbor are consistently indistinguishable from zero across fixed effects specifications. The results are precisely close to zero down to reel fixed effects, and fluctuate slightly but do not with statistical significance at the most restrictive geographic fixed effect levels. The null results are also consistent across specifications with and without covariate controls.

These results are particularly strong evidence that exposure to inequality through one's next door neighbor does not produce strong changes in partisanship. The distance between these two groups is substantial, yet no effect is detectable. In comparison to the strong racial exposure effects, these results are somewhat surprising. Living directly next to a household earning a lot of money, when your household earned no money, does not seem to influence long-term political attitudes. Conversely, living next to an impoverished family when yours is living comfortably does not change future partisanship. One caveat to these findings is that the results may be obscured by the volatility of income. A family earning one income in 1940 may earn a much different level of income in the preceding and ensuing years, while we can be quite confident that race is constant across years. Highly volatile levels of income would make 1940 a less representative snapshot of class exposure, and should drive the results towards zero.

5 Conclusion

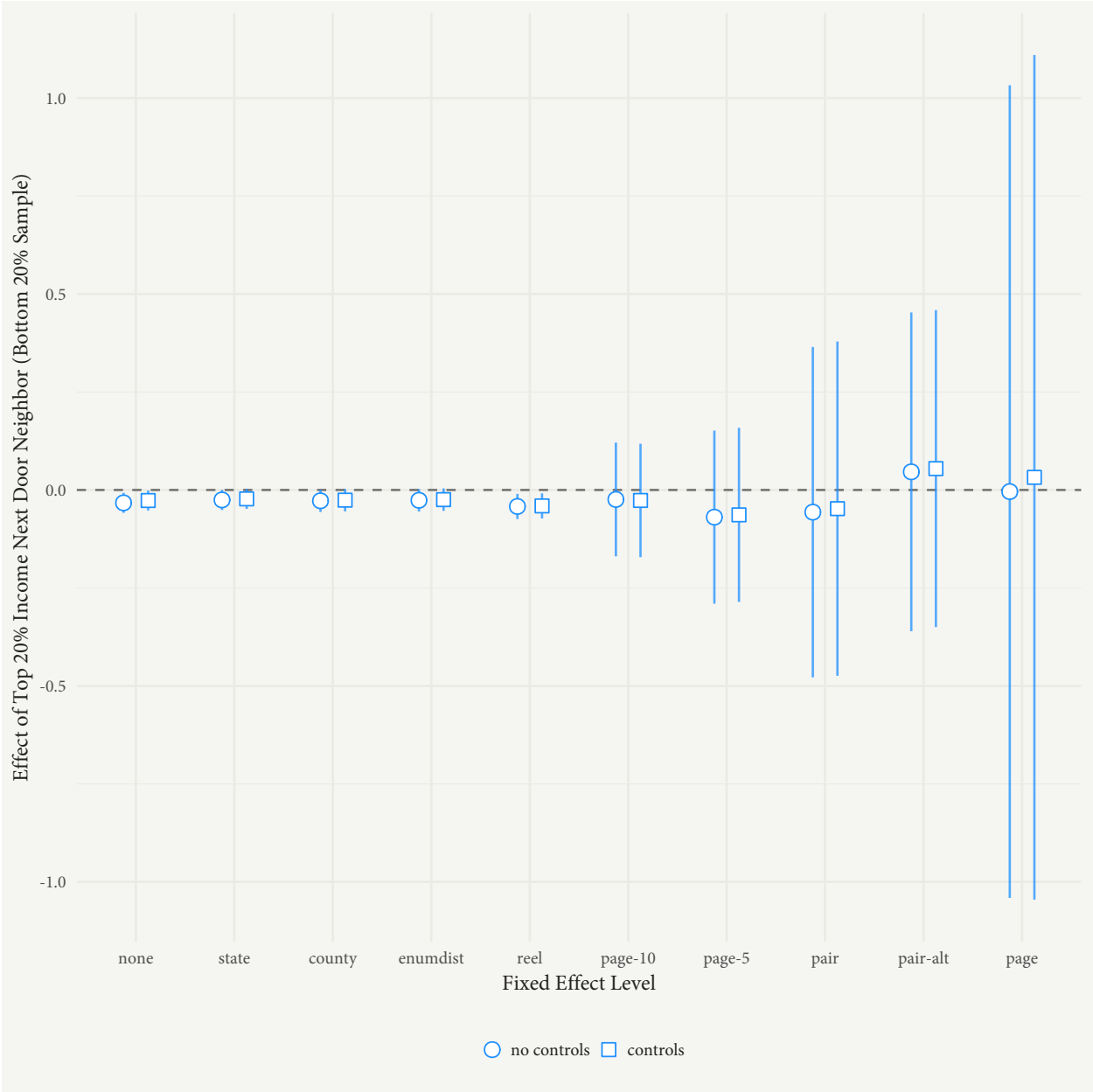
In this manuscript, we have undertaken the first large-scale linking of Census data and voter records, allowing us to examine the effect of early life experiences on political behavior over 70 years later. We have demonstrated, with plausible causal identification, that white voters' early life exposure to Black neighbors is related to later life partisanship. Because these effects persist over such a long time period, we argue that they are plausibly informative

Figure 16: Among wealthy, the effect of 1940 poor next door neighbor on 2017 Democratic partisanship



Note: Coefficient estimates are for models fit to subset of individuals whose head of the household reported earning income in the top 20% of the head of the household income distribution in 1940. Standard errors are clustered at the Census page level.

Figure 17: Among poor, the effect of rich next door neighbor in 1940 on 2017 Democratic partisanship



Note: Coefficient estimates are for models fit to subset of individuals whose head of the household reported earning no income in 1940. Standard errors are clustered at the Census page level.

about effects in intervening time periods, when the effect early-life exposure may have been even stronger. On the other hand, early life exposure to different classes, as measured by income, both rich to poor and poor to rich, is unrelated to partisanship over the lengthy time period. Notably, the extreme shocks of the Great Depression may have made this particular exposure less meaningful than such exposure in other time periods if annual earnings in 1939 were a very noisy measure of a family's usual economic status.

To further test the robustness of the findings in our study and to try and account for survivorship bias, we also linked the 1940 census to older voterfiles, the 2005 California and the 2009 North Carolina voterfiles. We linked to these datasets by the same method as we used to link to the 2017 voterfiles. The resulting sample offers an opportunity to retest our analysis on a much larger sample, as a larger number of voters who were alive in 1940 were also alive in 2005 and 2009 than in 2017. This additional analysis is also a test of the temporal dependency of our results. Studies of the American electorate find that recent White defection to the Republican party is attributable to racial attitudes related to concerns about Hispanic immigration and the reaction to a Black president [Abrajano and Hajnal \(2015\)](#); [Tesler \(2016\)](#). Given these recent trends, we might expect differential effects if we measure partisanship during a time when racial politics were less politically salient. On the other hand, partisan sorting along racial attitudes is a product of trends dating back to the partisan realignment following the Civil Rights movement [Carmines and Stimson \(1990\)](#); [Schickler \(2016\)](#), so liberalizing effect of living next door to a Black next door neighbor in 1940 may have influenced partisanship for individuals in our sample at an earlier point, and persisted through 2005 and 2009 and further to 2017. Details of the 2005 and 2009 voterfile analysis can be found in the Appendix, but the results are strongly consistent, both in estimated effect sizes and significance levels with the 2017 results. The effect of a next door Black neighbor increases the likelihood of being a Democrat in 2005 and 2009 by approximately 2 to 5 percentage points, and the effect of income class exposure

are null.

In moving this project forward, we envision several future additions. First, we can refine our measurement of the traits of neighbors. For example, we can see if the voter and cross-race neighbor were of similar age in 1940, which may have led to greater exposure and stronger effects downstream. We can also exactly match the social class of the neighbor to their race so that class and race are better separated (currently the matching is done in pairs so that we know if, at least, one neighbor is poor and, at least, one is Black, but not whether the poor neighbor and Black neighbor are the same). We can also measure differences in income using a continuous difference between neighbors or using other measures of socioeconomic status in the Census, like occupations or years of education.

Second, we can potentially improve causal identification by looking at length of tenure in the residences of the voters and their neighbors in 1940. If we restrict the sample to white voters who had a Black neighbor who moved next to them, rather than white neighbors who chose to move next to a Black neighbor, we would be removing some threats of selection on our inferences.

Relatedly, and third, we can move back in time to match with the 1930 Federal Census to understand how shocks in income over a time period, for example moving from rich to poor, affect long-term attitudes. This would also allow us to measure how exposure to neighbors of a different status affects long-term behavior in a period with less transitory income variation because the Great Depression was only just beginning in 1930 and—without earnings recorded in 1930—we would rely on other potentially less variable measures of status like occupation.

Fourth, we can explore options for loosening our matching criteria by not using state of birth, which will allow us to expand to other states and to look at effects on women.

Finally, as we discussed in earlier sections, we believe this methodology has considerable promise: most notably, the potential for targeted surveying of this population can yield

important insights about outcomes such as attitudes about redistribution and diversity and can help us to understand how interventions during a unique period of American history have shaped the long-term trajectory of American politics. As debates continue over the proper role of government in social welfare promotion, for example those involving the Affordable Care Act of 2010, and over maintaining diversity in cities with increasingly constricted housing supplies, such findings have important implications for these discussions.

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A Appendix

A.1 Record Linkage

Historical record linkage is a complicated process. One key constraint is that we do not have access to ground truth data. That is, we do not have validated links between the voterfile and the 1940 Census, because no such links exist. Rather than learning to make matches from ground truth, the machine learning algorithm learns from human-made links. We consider human linking to be quite good—perhaps the best we can do with such noisy historical data lacking unique identifiers—but ultimately the links are subject to researcher bias and errors.

There are two main metrics to use to evaluate an automated linking process relative to a human linker. The first is a measure of accuracy. What share of the records linked by the algorithm would a well-trained human have linked? The positive predictive values (PPV) in our three samples are all better than 80%: The PPV for California was 84%, 85% for North Carolina, and 88% for Nebraska. These results are roughly in line with past work using the algorithm ([Feigenbaum, 2016](#)).

The second metric is a measure of efficiency or sensitivity, defined as the true positive rate (TPR). Though the name is a bit of an abuse of notation in this case—true positives are links that agree between the algorithm and the researcher not necessarily with the truth—it measures what share of record links a well-trained and careful researcher makes is the algorithm able to find and make. Again, our results are quite encouraging: we find TPRs of 83% for California, 74% for North Carolina, and 93% for Nebraska. The different demographics and naming patterns in the three states, as well as the very different population sizes in 1940, likely explain the wider range in TPR.

There is a trade off between making accurate links and finding as many links as possible. In future work, we can change two meta-parameters in the linking process to increase PPV at the cost of TPR (or vice versa) to ensure that our results are robust to variation in the linking process.

The record linkage model we currently use relies on a number of features—ideally the same features a human linker uses to decide which links are correct. These features are the Jaro-Winkler string edit distances in first and last name, the difference between the age on the 1940 census and the estimated age in 1940 based on the voterfile birthdate, whether or not the first names could be nicknames or abbreviations (Jake and Jacob for example or Wm and William), the commonness of the name in 1940, agreement on the first and last letters of the first and last names, agreement on the middle initial, and whether or not the soundex phonetic code is the same for the first names or the last names (Lewis and Louis, for example).

A.2 Additional Parental Characteristics

Figure A1: Distribution of Father's Age in 1940 Census Linked Sample

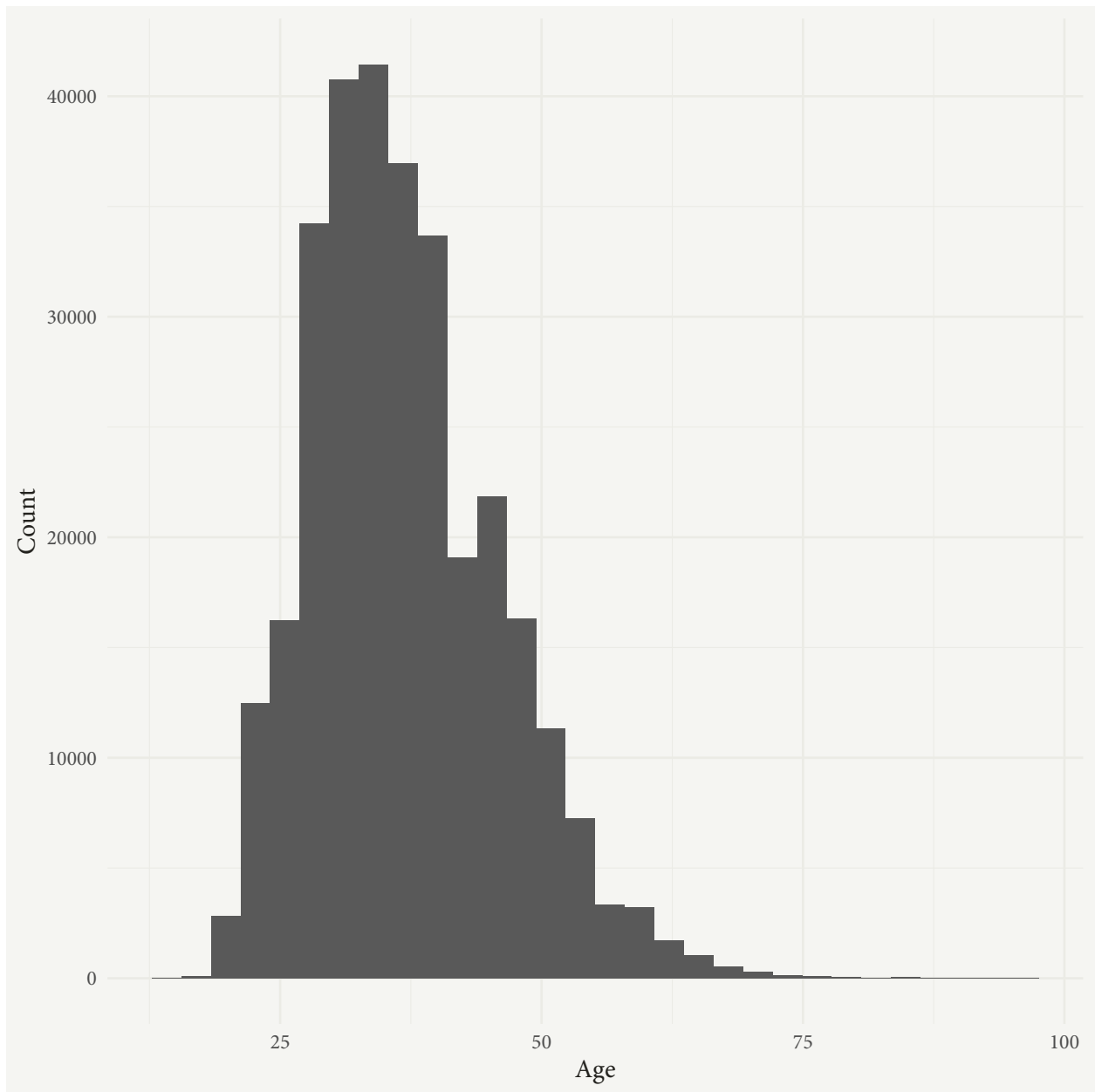


Figure A2: Distribution of Father's Migration Status in 1940 Census Linked Sample

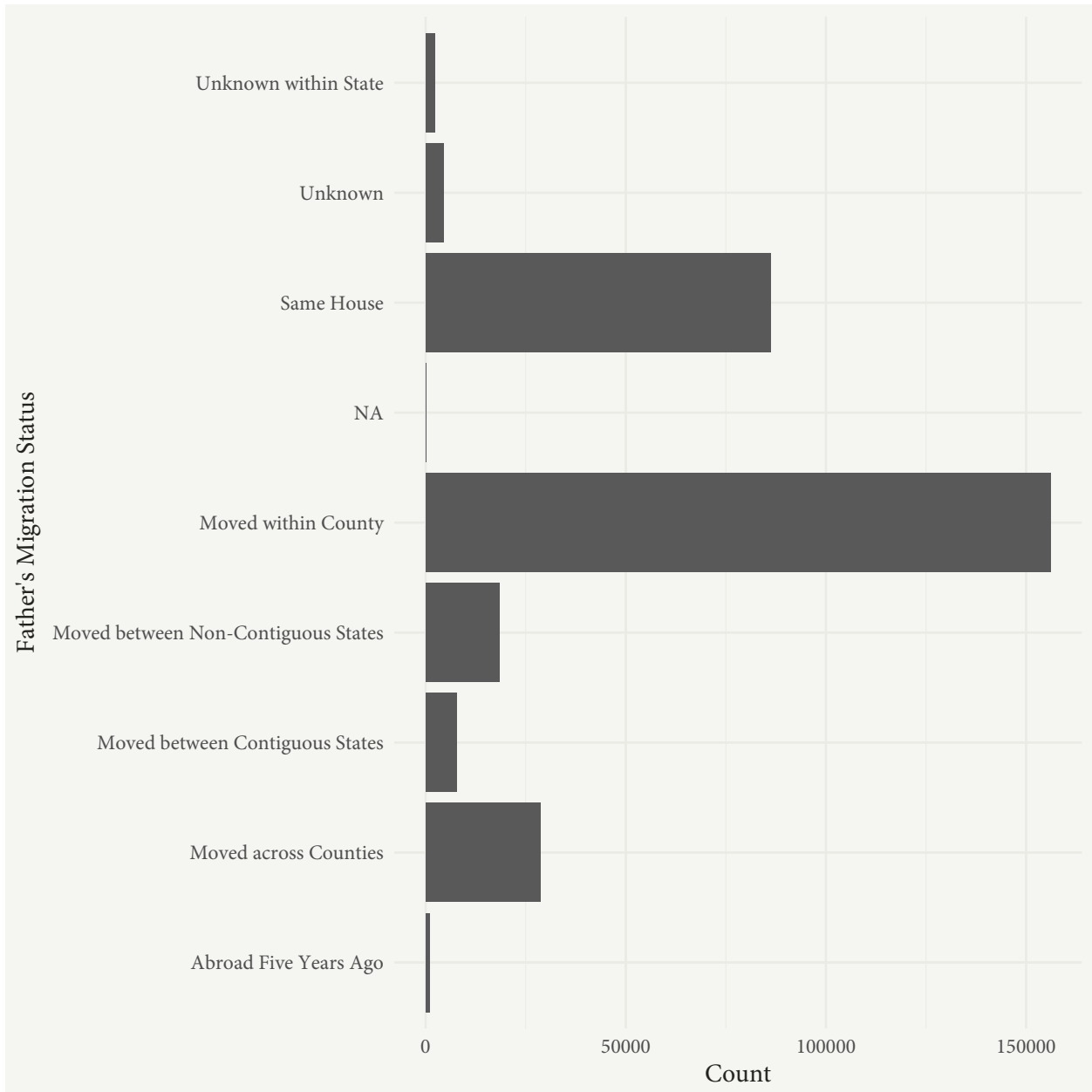


Figure A3: Distribution of Parent's Age in 1940 Census Linked Sample

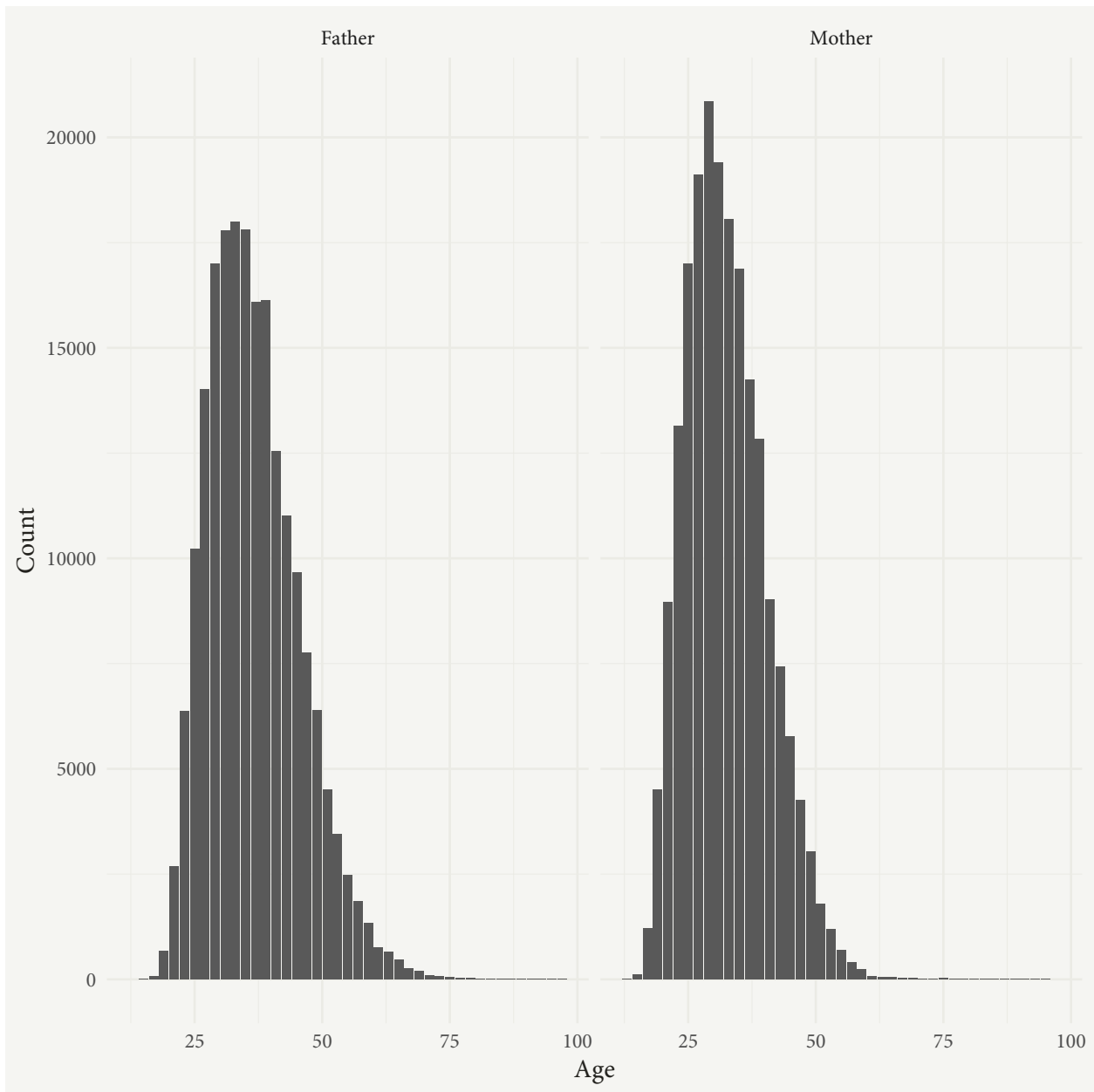


Figure A4: Distribution of Parent's Migration Status in 1940 Census Linked Sample

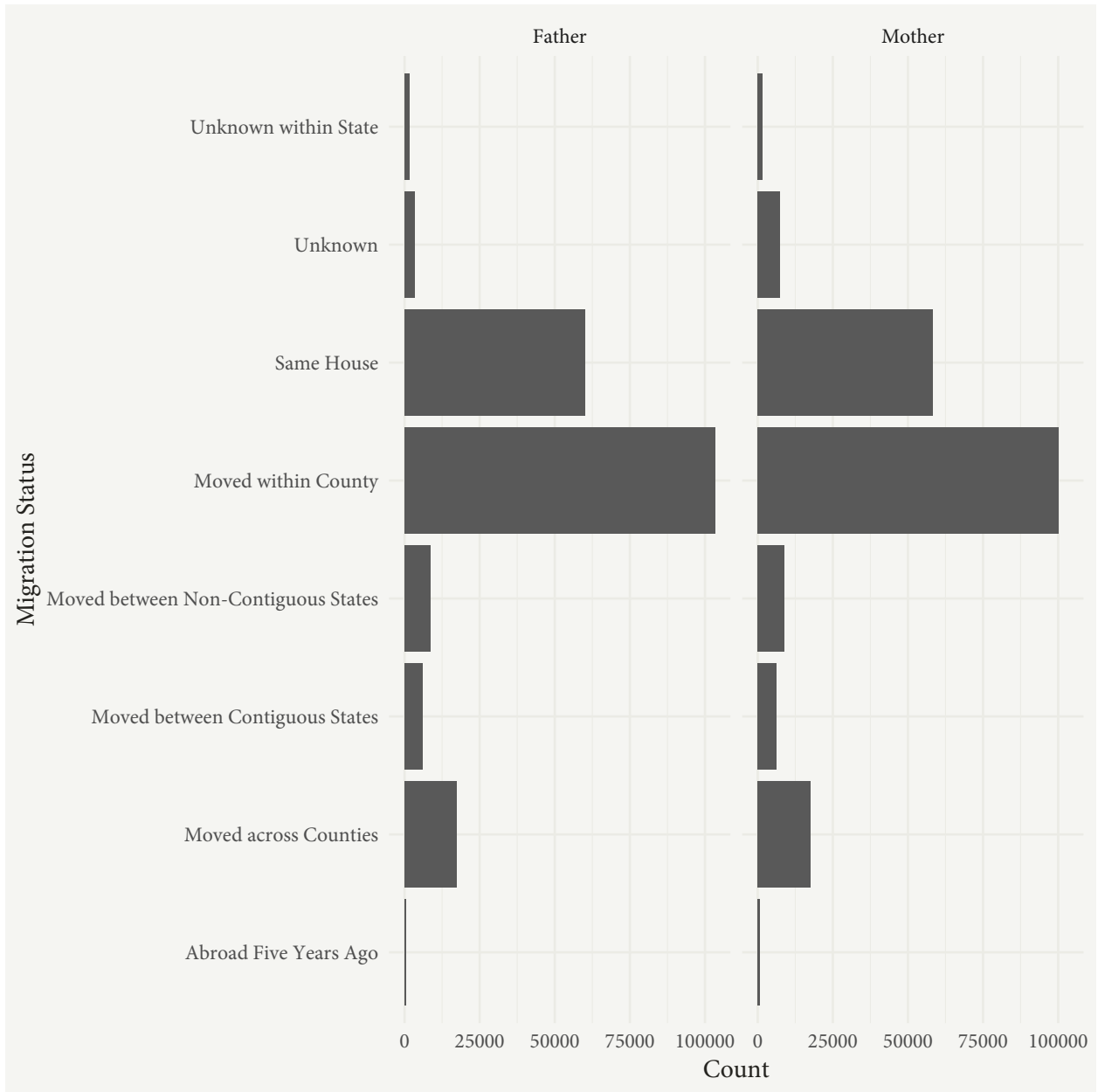


Figure A5: Distribution of Parent's Income in 1940 Census Linked Sample

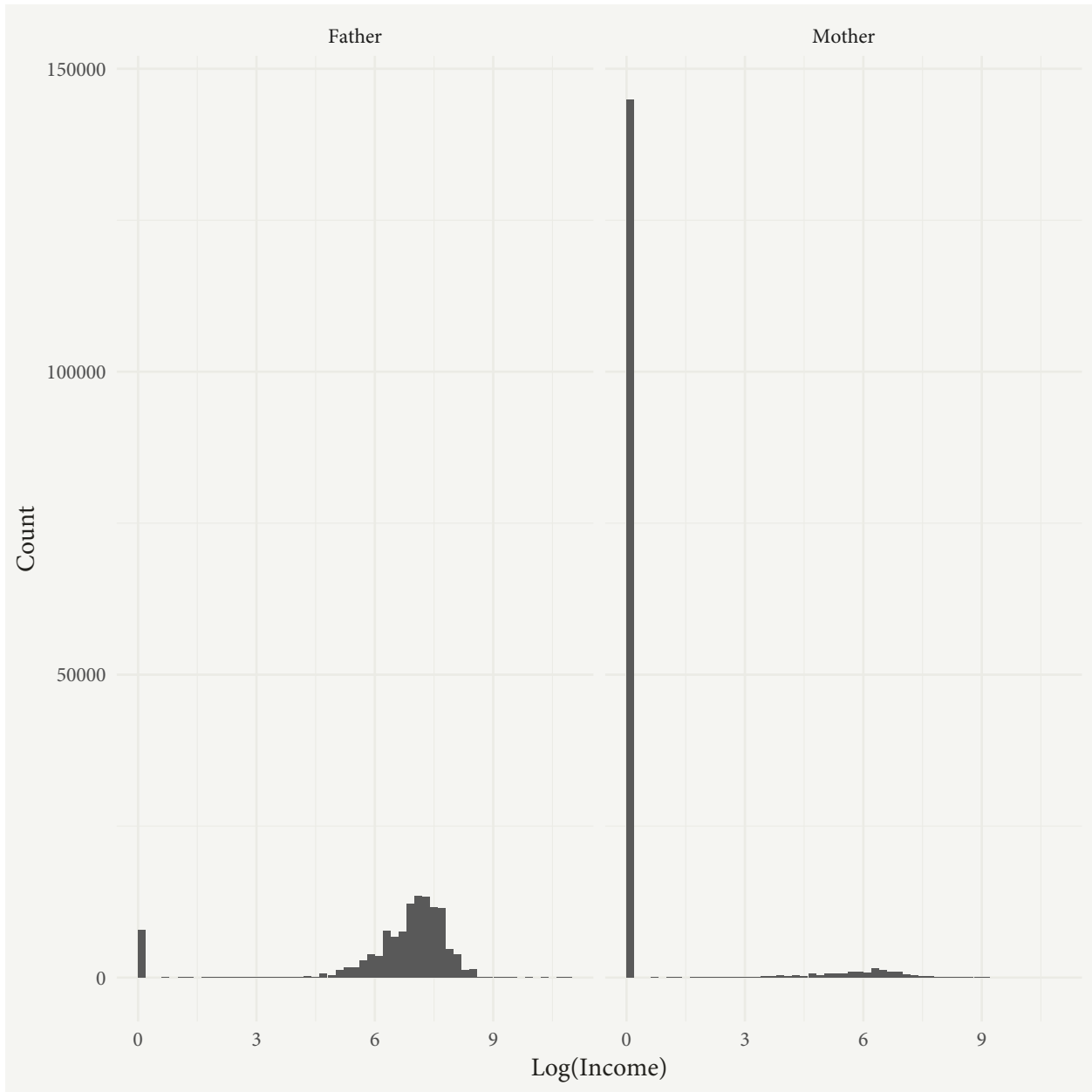


Table A1: Effect of Black neighbors with children on future partisanship

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Black neighbor (k=1)	0.058*** (0.008)	0.052*** (0.009)	0.039*** (0.009)	0.036*** (0.010)	0.030*** (0.009)	0.028*** (0.010)	0.030*** (0.009)	0.028*** (0.010)
Black neighbor (k=2)	0.037*** (0.009)	0.029*** (0.010)	0.025*** (0.009)	0.019* (0.010)	0.015 (0.009)	0.014 (0.011)	0.015 (0.009)	0.013 (0.011)
Black neighbor (k=3)	0.032*** (0.009)	0.030*** (0.010)	0.019** (0.009)	0.019* (0.010)	0.012 (0.009)	0.013 (0.010)	0.012 (0.009)	0.012 (0.010)
Black neighbor (k=4)	0.021** (0.009)	0.017* (0.009)	0.010 (0.009)	0.008 (0.010)	0.002 (0.009)	0.002 (0.010)	0.002 (0.009)	0.001 (0.010)
Black neighbor (k=5)	0.025*** (0.008)	0.017* (0.009)	0.011 (0.009)	0.006 (0.010)	0.002 (0.009)	-0.001 (0.010)	0.002 (0.009)	-0.001 (0.010)
Black neighbor (k=6)	0.018** (0.008)	0.008 (0.009)	0.007 (0.009)	-0.001 (0.010)	-0.001 (0.009)	-0.008 (0.010)	-0.002 (0.009)	-0.008 (0.010)
Black neighbor (k=7)	0.022*** (0.008)	0.023** (0.009)	0.011 (0.009)	0.014 (0.010)	0.007 (0.009)	0.012 (0.010)	0.007 (0.009)	0.012 (0.010)
Black neighbor (k=8)	0.037*** (0.008)	0.033*** (0.009)	0.024*** (0.009)	0.024** (0.010)	0.017* (0.009)	0.017* (0.010)	0.017* (0.009)	0.017* (0.010)
Black neighbor (k=9)	0.014* (0.008)	0.007 (0.009)	0.0004 (0.008)	-0.004 (0.009)	-0.006 (0.009)	-0.009 (0.010)	-0.006 (0.009)	-0.008 (0.010)
Black neighbor (k=10)	0.028*** (0.008)	0.023*** (0.009)	0.009 (0.008)	0.007 (0.009)	0.001 (0.008)	-0.001 (0.009)	0.001 (0.008)	-0.001 (0.009)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
FE	None	None	State	State	County	County	Enumdist	Enumdist
N	112,040	101,049	112,040	101,049	112,040	101,049	112,040	101,049

*p < .1; **p < .05; ***p < .01

Note: Coefficient estimates are from models fitted to subset of whites in the data who live next door to a family with at least one child. Standard errors are clustered at the Census page level.

A.3 Additional Figures

A.4 Older Voterfile Results

We also linked the 1940 census to older voterfiles, the 2005 California and the 2009 North Carolina voterfile. Overall, our linkage method performed comparably to the 2017 linkage. The match rates—the shares of voter records we are confident in linking to a record in 1940—for California and North Carolina were 59.8% and 60.7%, with true positive rates of 83.4% and 80.6%, respectively. Our final linked sample size is 509,871 individuals in California and 162,461 in North Carolina. The positive predictive values are 90.6% and 81.1%.

We estimate the effect cross-race and cross-class exposure on this sample to re-test our results in a different political context and on a much larger sample. Since the two voterfiles are different years, we estimate the results separately on each linked state sample. The

Figure A6: Neighbor effect by age, county fixed effects

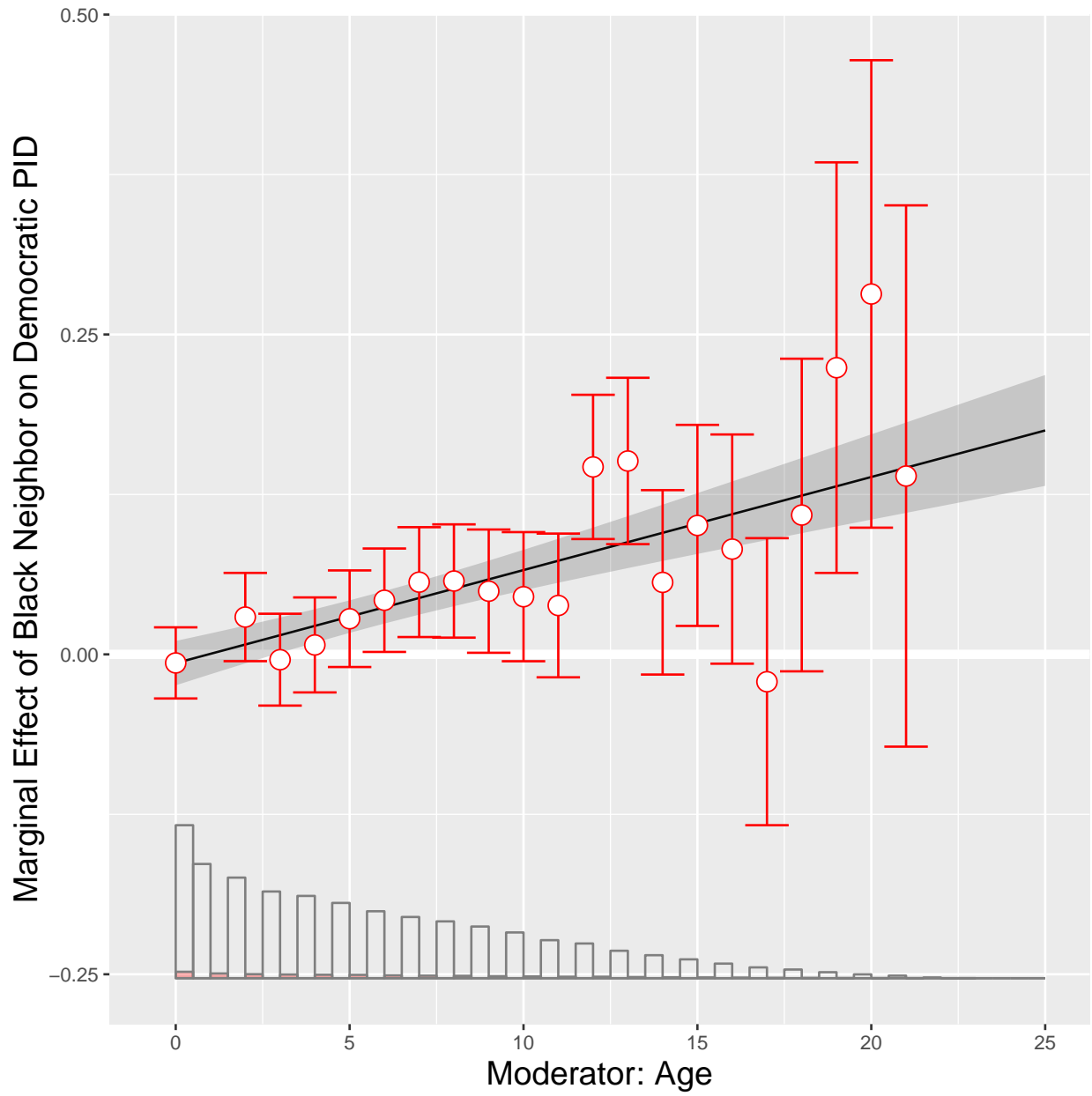


Table A2: Effect of Black neighbors with children on future partisanship

	(1)	(2)	(3)	(4)	(5)	(6)
Black neighbor (k=1)	0.029*** (0.009)	0.028*** (0.010)	0.008 (0.018)	0.001 (0.021)	0.018 (0.025)	0.007 (0.029)
Black neighbor (k=2)	0.015 (0.009)	0.014 (0.011)	0.037** (0.019)	0.037* (0.022)	0.024 (0.026)	0.014 (0.030)
Black neighbor (k=3)	0.012 (0.009)	0.014 (0.010)	0.008 (0.019)	0.009 (0.022)	0.010 (0.026)	0.006 (0.030)
Black neighbor (k=4)	0.002 (0.009)	0.003 (0.010)	-0.012 (0.018)	-0.021 (0.021)	-0.009 (0.025)	0.002 (0.029)
Black neighbor (k=5)	0.002 (0.009)	-0.0001 (0.010)	0.0001 (0.018)	0.004 (0.021)	-0.00004 (0.024)	0.004 (0.028)
Black neighbor (k=6)	-0.001 (0.009)	-0.008 (0.010)	0.0004 (0.018)	-0.011 (0.021)	0.012 (0.024)	-0.007 (0.028)
Black neighbor (k=7)	0.007 (0.009)	0.012 (0.010)	-0.001 (0.018)	0.015 (0.021)	-0.007 (0.024)	0.018 (0.029)
Black neighbor (k=8)	0.014 (0.009)	0.013 (0.010)	0.024 (0.018)	0.023 (0.021)	0.004 (0.024)	-0.009 (0.028)
Black neighbor (k=9)	-0.008 (0.009)	-0.009 (0.010)	-0.008 (0.017)	-0.008 (0.020)	-0.014 (0.024)	-0.013 (0.029)
Black neighbor (k=10)	-0.0002 (0.008)	-0.001 (0.009)	0.005 (0.017)	0.008 (0.019)	0.006 (0.023)	0.006 (0.027)
Controls	No	Yes	No	Yes	No	Yes
FE	Reel	Reel	Page 10	Page 10	Page 5	Page 5
N	112,040	101,049	112,040	101,049	112,040	101,049

*p < .1; **p < .05; ***p < .01

Note: Coefficient estimates are from models fitted to subset of whites in the data who live next door to a family with at least one child. Standard errors are clustered at the Census page level.

Table A3: Effect of Black neighbors with children on future partisanship

	(1)	(2)	(3)	(4)	(5)	(6)
Black neighbor (k=1)	0.005 (0.047)	-0.007 (0.055)	0.041 (0.048)	0.032 (0.056)	-0.0001 (0.100)	0.010 (0.119)
Black neighbor (k=2)	0.022 (0.049)	0.014 (0.059)	0.032 (0.049)	0.034 (0.057)	0.014 (0.097)	0.033 (0.113)
Black neighbor (k=3)	-0.005 (0.049)	-0.013 (0.059)	0.006 (0.049)	-0.008 (0.058)	-0.022 (0.095)	-0.025 (0.115)
Black neighbor (k=4)	-0.006 (0.046)	0.0003 (0.056)	0.005 (0.047)	0.019 (0.057)	0.008 (0.088)	0.041 (0.109)
Black neighbor (k=5)	0.004 (0.047)	0.003 (0.055)	0.009 (0.045)	0.019 (0.053)	-0.009 (0.085)	0.020 (0.101)
Black neighbor (k=6)	-0.006 (0.044)	-0.040 (0.052)	0.004 (0.043)	-0.014 (0.051)	0.015 (0.080)	-0.001 (0.093)
Black neighbor (k=7)	0.013 (0.044)	0.039 (0.053)	0.010 (0.046)	0.026 (0.054)	0.001 (0.084)	0.046 (0.099)
Black neighbor (k=8)	0.025 (0.043)	0.012 (0.051)	0.019 (0.043)	-0.003 (0.051)	0.029 (0.078)	0.024 (0.090)
Black neighbor (k=9)	-0.034 (0.045)	-0.038 (0.054)	-0.024 (0.043)	-0.018 (0.051)	-0.049 (0.081)	-0.037 (0.096)
Black neighbor (k=10)	0.023 (0.042)	0.020 (0.050)	0.007 (0.041)	0.014 (0.048)	-0.0005 (0.076)	0.006 (0.090)
Controls	No	Yes	No	Yes	No	Yes
FE	Page 2	Page 2	Page 2 Alt	Page 2 Alt	Page	Page
N	112,040	101,049	112,040	101,049	112,040	101,049

*p < .1; **p < .05; ***p < .01

Note: Coefficient estimates are from models fitted to subset of whites in the data who live next door to a family with at least one child. Standard errors are clustered at the Census page level.

Table A4: Effect of Black neighbors with male children on future partisanship

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Black neighbor (k=1)	0.062*** (0.010)	0.056*** (0.011)	0.040*** (0.010)	0.037*** (0.012)	0.029*** (0.011)	0.028** (0.012)	0.029*** (0.011)	0.028** (0.012)
Black neighbor (k=2)	0.047*** (0.011)	0.038*** (0.012)	0.034*** (0.011)	0.027** (0.013)	0.024** (0.012)	0.020 (0.013)	0.024** (0.012)	0.020 (0.013)
Black neighbor (k=3)	0.020* (0.011)	0.015 (0.012)	0.006 (0.011)	0.004 (0.012)	0.001 (0.011)	-0.0001 (0.013)	0.001 (0.011)	-0.0005 (0.013)
Black neighbor (k=4)	0.041*** (0.010)	0.042*** (0.012)	0.029*** (0.011)	0.033*** (0.012)	0.019* (0.011)	0.026** (0.013)	0.018 (0.011)	0.026** (0.013)
Black neighbor (k=5)	0.024** (0.010)	0.015 (0.012)	0.010 (0.011)	0.004 (0.012)	-0.003 (0.011)	-0.008 (0.013)	-0.003 (0.011)	-0.008 (0.013)
Black neighbor (k=6)	0.017* (0.010)	0.005 (0.011)	0.006 (0.011)	-0.005 (0.012)	-0.008 (0.011)	-0.015 (0.012)	-0.008 (0.011)	-0.015 (0.012)
Black neighbor (k=7)	0.018* (0.010)	0.018 (0.011)	0.005 (0.011)	0.008 (0.012)	-0.001 (0.011)	0.005 (0.012)	-0.001 (0.011)	0.004 (0.012)
Black neighbor (k=8)	0.039*** (0.010)	0.037*** (0.011)	0.027*** (0.010)	0.028** (0.012)	0.020* (0.011)	0.018 (0.012)	0.020* (0.011)	0.018 (0.012)
Black neighbor (k=9)	0.010 (0.010)	0.0002 (0.011)	-0.005 (0.010)	-0.012 (0.012)	-0.011 (0.011)	-0.014 (0.012)	-0.011 (0.011)	-0.014 (0.012)
Black neighbor (k=10)	0.027*** (0.010)	0.020* (0.011)	0.009 (0.010)	0.004 (0.011)	-0.001 (0.010)	-0.005 (0.011)	-0.001 (0.010)	-0.006 (0.012)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
FE	None	None	State	State	County	County	Enumdist	Enumdist
N	70,761	63,483	70,761	63,483	70,761	63,483	70,761	63,483

*p < .1; **p < .05; ***p < .01

Note: Coefficient estimates are from models fitted to subset of whites in the data who live next door to a family with at least one male child. Standard errors are clustered at the Census page level.

Table A5: Effect of Black neighbors with male children on future partisanship

	(1)	(2)	(3)	(4)	(5)	(6)
Black neighbor (k=1)	0.029*** (0.011)	0.028** (0.012)	0.011 (0.024)	0.006 (0.029)	0.026 (0.035)	0.020 (0.041)
Black neighbor (k=2)	0.025** (0.012)	0.023* (0.013)	0.061** (0.026)	0.064** (0.031)	0.053 (0.037)	0.035 (0.044)
Black neighbor (k=3)	0.001 (0.011)	0.001 (0.013)	-0.001 (0.025)	-0.003 (0.031)	-0.009 (0.037)	-0.010 (0.044)
Black neighbor (k=4)	0.019* (0.011)	0.028** (0.013)	0.005 (0.025)	0.018 (0.030)	0.008 (0.036)	0.022 (0.043)
Black neighbor (k=5)	-0.003 (0.011)	-0.007 (0.013)	-0.006 (0.025)	0.008 (0.030)	-0.030 (0.035)	-0.021 (0.043)
Black neighbor (k=6)	-0.004 (0.011)	-0.012 (0.012)	0.001 (0.024)	-0.005 (0.029)	0.015 (0.034)	0.007 (0.041)
Black neighbor (k=7)	-0.001 (0.011)	0.005 (0.012)	0.006 (0.024)	0.016 (0.029)	-0.005 (0.035)	0.013 (0.041)
Black neighbor (k=8)	0.016 (0.011)	0.014 (0.012)	0.025 (0.024)	0.021 (0.029)	0.024 (0.034)	-0.007 (0.041)
Black neighbor (k=9)	-0.012 (0.011)	-0.016 (0.012)	-0.017 (0.024)	-0.014 (0.029)	-0.007 (0.035)	-0.003 (0.041)
Black neighbor (k=10)	-0.001 (0.010)	-0.004 (0.012)	-0.012 (0.023)	-0.007 (0.027)	0.013 (0.034)	0.010 (0.039)
Controls	No	Yes	No	Yes	No	Yes
FE	Reel	Reel	Page 10	Page 10	Page 5	Page 5
N	70,761	63,483	70,761	63,483	70,761	63,483

*p < .1; **p < .05; ***p < .01

Note: Coefficient estimates are from models fitted to subset of whites in the data who live next door to a family with at least one male child. Standard errors are clustered at the Census page level.

Table A6: Effect of Black neighbors with male children on future partisanship

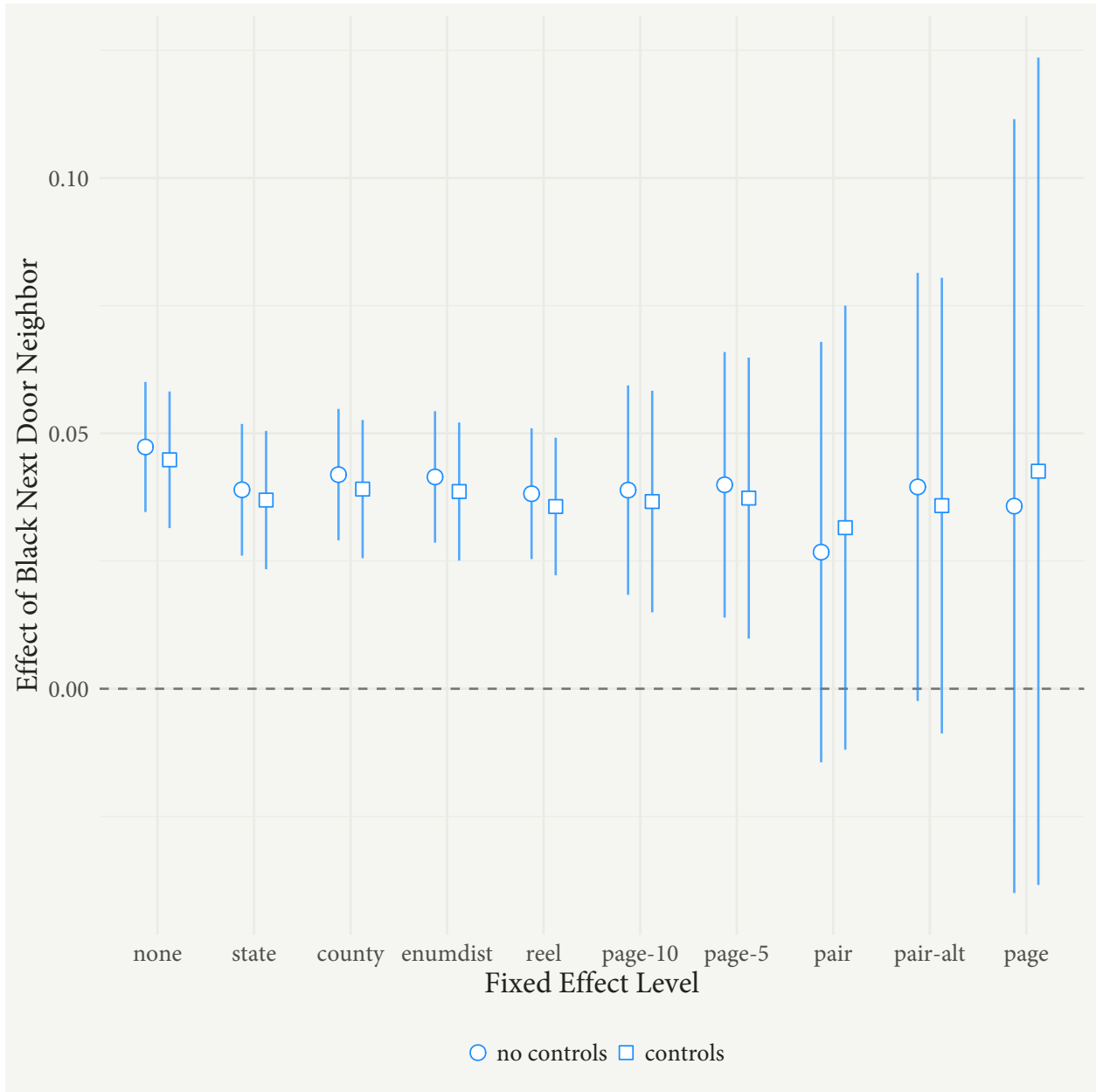
	(1)	(2)	(3)	(4)	(5)	(6)
Black neighbor (k=1)	-0.012 (0.068)	-0.022 (0.082)	0.076 (0.068)	0.070 (0.082)	0.022 (0.142)	0.012 (0.177)
Black neighbor (k=2)	0.050 (0.070)	0.043 (0.085)	0.056 (0.067)	0.056 (0.079)	0.046 (0.132)	0.069 (0.158)
Black neighbor (k=3)	-0.063 (0.068)	-0.074 (0.085)	-0.002 (0.067)	-0.018 (0.081)	-0.056 (0.130)	-0.078 (0.165)
Black neighbor (k=4)	0.011 (0.065)	0.021 (0.081)	0.039 (0.068)	0.062 (0.084)	0.042 (0.122)	0.081 (0.155)
Black neighbor (k=5)	-0.037 (0.068)	-0.036 (0.083)	-0.017 (0.062)	0.001 (0.076)	-0.040 (0.114)	-0.018 (0.141)
Black neighbor (k=6)	-0.005 (0.063)	-0.023 (0.075)	0.014 (0.062)	0.005 (0.074)	0.005 (0.112)	0.011 (0.136)
Black neighbor (k=7)	-0.020 (0.061)	-0.002 (0.073)	0.008 (0.064)	0.023 (0.076)	0.012 (0.114)	0.041 (0.135)
Black neighbor (k=8)	0.024 (0.061)	-0.0004 (0.074)	0.053 (0.061)	0.017 (0.075)	0.054 (0.109)	0.018 (0.131)
Black neighbor (k=9)	-0.040 (0.064)	-0.050 (0.077)	-0.035 (0.062)	-0.034 (0.073)	-0.051 (0.112)	-0.046 (0.133)
Black neighbor (k=10)	0.024 (0.060)	0.022 (0.071)	0.015 (0.059)	0.025 (0.071)	-0.012 (0.103)	-0.024 (0.123)
Controls	No	Yes	No	Yes	No	Yes
FE	Page 2	Page 2	Page 2 Alt	Page 2 Alt	Page	Page
N	70,761	63,483	70,761	63,483	70,761	63,483

*p < .1; **p < .05; ***p < .01

Note: Coefficient estimates are from models fitted to subset of whites in the data who live next door to a family with at least one male child. Standard errors are clustered at the Census page level.

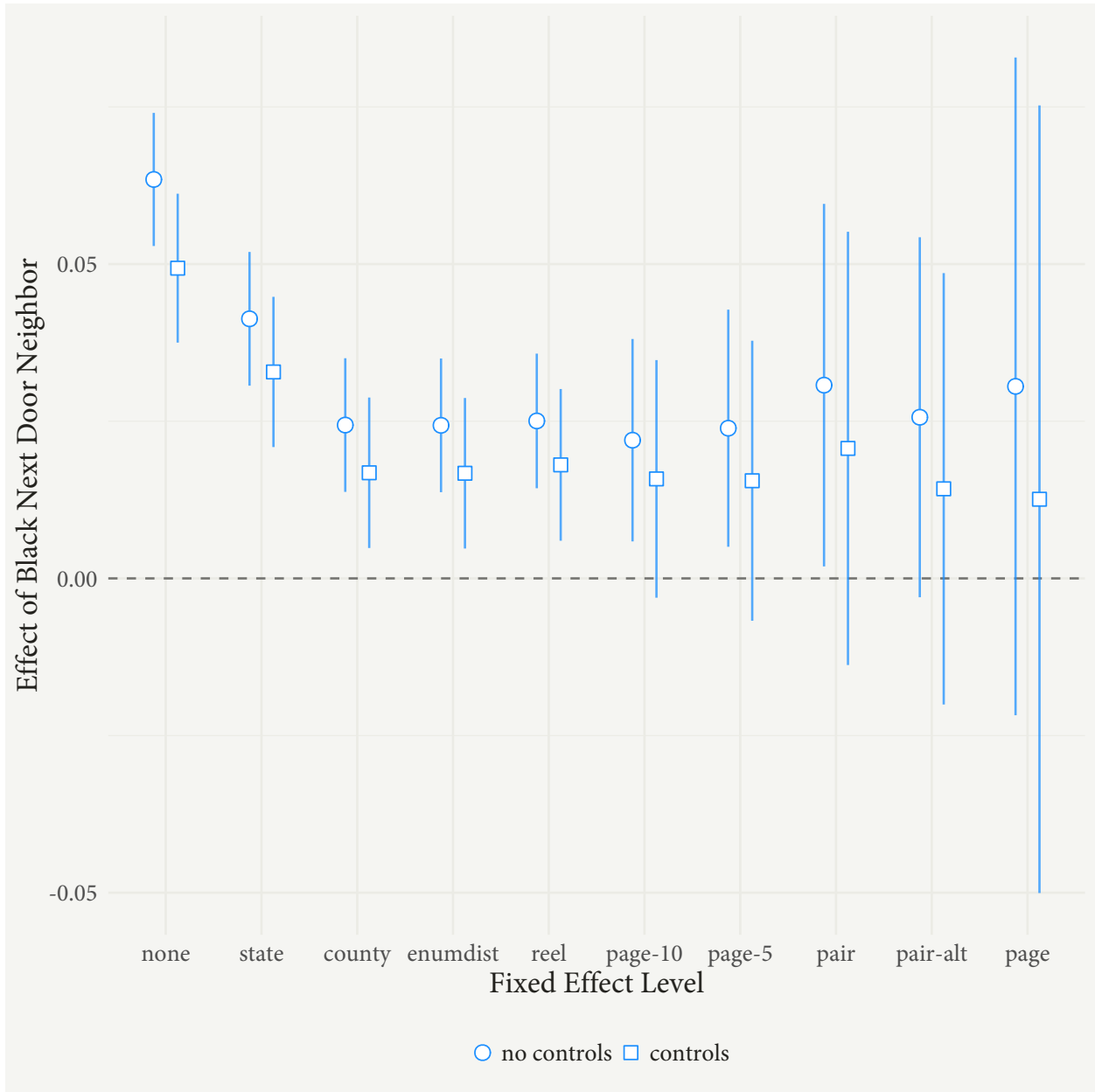
results are strongly consistent with the 2017 results, and are displayed in the figures and tables below.

Figure A7: Effect of Black 1940 next door neighbor on 2005 Democratic partisanship – CA '05 sample



Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level.

Figure A8: Effect of Black 1940 next door neighbor on 2009 Democratic partisanship – NC '09 sample



Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level.

Table A7: Effect of Black neighbors on future partisanship – CA '05 sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Black neighbor (k=1)	0.047*** (0.006)	0.045*** (0.007)	0.039*** (0.007)	0.037*** (0.007)	0.042*** (0.007)	0.039*** (0.007)	0.041*** (0.007)	0.039*** (0.007)
Black neighbor (k=2)	0.023*** (0.006)	0.021*** (0.007)	0.017** (0.007)	0.016** (0.007)	0.017** (0.007)	0.016** (0.007)	0.017** (0.007)	0.015** (0.007)
Black neighbor (k=3)	0.027*** (0.006)	0.024*** (0.007)	0.021*** (0.007)	0.019*** (0.007)	0.022*** (0.007)	0.019*** (0.007)	0.022*** (0.007)	0.019*** (0.007)
Black neighbor (k=4)	0.023*** (0.006)	0.020*** (0.007)	0.017*** (0.007)	0.015** (0.007)	0.018*** (0.006)	0.016** (0.007)	0.017*** (0.006)	0.016** (0.007)
Black neighbor (k=5)	0.022*** (0.006)	0.015** (0.007)	0.017*** (0.006)	0.011 (0.007)	0.020*** (0.006)	0.015** (0.007)	0.020*** (0.006)	0.014** (0.007)
Black neighbor (k=6)	0.018*** (0.006)	0.017*** (0.006)	0.013** (0.006)	0.013* (0.007)	0.012* (0.006)	0.012* (0.007)	0.012* (0.006)	0.011* (0.007)
Black neighbor (k=7)	0.015** (0.006)	0.013** (0.006)	0.008 (0.006)	0.008 (0.007)	0.010 (0.006)	0.009 (0.007)	0.010 (0.006)	0.009 (0.007)
Black neighbor (k=8)	0.022*** (0.006)	0.019*** (0.006)	0.016** (0.006)	0.014** (0.007)	0.017*** (0.006)	0.015** (0.007)	0.017*** (0.006)	0.014** (0.007)
Black neighbor (k=9)	0.020*** (0.006)	0.018*** (0.006)	0.014** (0.006)	0.013* (0.007)	0.015** (0.006)	0.014** (0.007)	0.015** (0.006)	0.013** (0.007)
Black neighbor (k=10)	0.025*** (0.006)	0.023*** (0.006)	0.014** (0.006)	0.014** (0.006)	0.017*** (0.006)	0.017*** (0.006)	0.017*** (0.006)	0.016*** (0.006)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
FE	None	None	State	State	County	County	Enumdist	Enumdist
N	478,762	441,315	478,762	441,315	478,762	441,315	478,762	441,315

*p < .1; **p < .05; ***p < .01

Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level.

Table A8: Effect of Black neighbors on future partisanship – CA '05 sample

	(1)	(2)	(3)	(4)	(5)	(6)
Black neighbor (k=1)	0.038*** (0.007)	0.036*** (0.007)	0.039*** (0.010)	0.037*** (0.011)	0.040*** (0.013)	0.037*** (0.014)
Black neighbor (k=2)	0.013* (0.007)	0.012* (0.007)	0.019* (0.010)	0.015 (0.011)	0.019 (0.013)	0.016 (0.014)
Black neighbor (k=3)	0.020*** (0.007)	0.018*** (0.007)	0.014 (0.010)	0.013 (0.011)	0.011 (0.013)	0.008 (0.014)
Black neighbor (k=4)	0.014** (0.006)	0.013* (0.007)	0.005 (0.010)	0.007 (0.011)	0.003 (0.013)	0.005 (0.014)
Black neighbor (k=5)	0.015** (0.006)	0.010 (0.007)	0.012 (0.010)	0.007 (0.011)	0.011 (0.013)	0.007 (0.014)
Black neighbor (k=6)	0.011* (0.006)	0.012* (0.007)	0.022** (0.010)	0.019* (0.011)	0.016 (0.013)	0.011 (0.014)
Black neighbor (k=7)	0.006 (0.006)	0.006 (0.007)	0.005 (0.010)	0.003 (0.011)	-0.001 (0.013)	-0.0002 (0.014)
Black neighbor (k=8)	0.013** (0.006)	0.011* (0.007)	0.010 (0.010)	0.010 (0.011)	0.006 (0.013)	-0.001 (0.013)
Black neighbor (k=9)	0.012* (0.006)	0.011* (0.007)	0.005 (0.010)	0.006 (0.011)	0.012 (0.013)	0.017 (0.014)
Black neighbor (k=10)	0.011* (0.006)	0.012* (0.006)	0.001 (0.009)	0.005 (0.010)	0.002 (0.012)	0.002 (0.013)
Controls	No	Yes	No	Yes	No	Yes
FE	Reel	Reel	Page 10	Page 10	Page 5	Page 5
N	478,762	441,315	478,762	441,315	478,762	441,315

*p < .1; **p < .05; ***p < .01

Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level.

Table A9: Effect of Black neighbors on future partisanship – CA '05 sample

	(1)	(2)	(3)	(4)	(5)	(6)
Black neighbor (k=1)	0.027 (0.021)	0.032 (0.022)	0.040* (0.021)	0.036 (0.023)	0.036 (0.039)	0.043 (0.041)
Black neighbor (k=2)	0.011 (0.021)	0.005 (0.022)	0.011 (0.021)	0.005 (0.023)	0.020 (0.037)	0.015 (0.040)
Black neighbor (k=3)	-0.006 (0.021)	-0.010 (0.022)	0.001 (0.021)	-0.004 (0.022)	-0.014 (0.036)	-0.017 (0.039)
Black neighbor (k=4)	-0.002 (0.020)	0.0004 (0.021)	0.020 (0.020)	0.025 (0.021)	0.020 (0.035)	0.027 (0.038)
Black neighbor (k=5)	0.002 (0.021)	-0.003 (0.022)	0.001 (0.020)	-0.003 (0.021)	0.006 (0.035)	0.004 (0.038)
Black neighbor (k=6)	0.019 (0.020)	0.013 (0.021)	0.016 (0.020)	0.014 (0.021)	0.029 (0.034)	0.024 (0.036)
Black neighbor (k=7)	0.006 (0.021)	0.001 (0.022)	0.006 (0.021)	0.004 (0.022)	0.028 (0.035)	0.023 (0.038)
Black neighbor (k=8)	-0.001 (0.020)	-0.0004 (0.021)	0.001 (0.020)	-0.006 (0.021)	-0.016 (0.033)	-0.020 (0.036)
Black neighbor (k=9)	0.024 (0.021)	0.023 (0.022)	0.028 (0.020)	0.028 (0.022)	0.033 (0.035)	0.031 (0.037)
Black neighbor (k=10)	-0.013 (0.020)	-0.012 (0.021)	-0.012 (0.020)	-0.010 (0.021)	-0.017 (0.033)	-0.013 (0.036)
Controls	No	Yes	No	Yes	No	Yes
FE	Page 2	Page 2	Page 2 Alt	Page 2 Alt	Page	Page
N	478,762	441,315	478,762	441,315	478,762	441,315

*p < .1; **p < .05; ***p < .01

Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level.

Table A10: Effect of Black neighbors on future partisanship – NC '09 sample

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Black neighbor (k=1)	0.063*** (0.005)	0.049*** (0.006)	0.041*** (0.005)	0.033*** (0.006)	0.024*** (0.005)	0.017*** (0.006)	0.024*** (0.005)	0.017*** (0.006)
Black neighbor (k=2)	0.026*** (0.005)	0.023*** (0.006)	0.010* (0.006)	0.012* (0.006)	-0.002 (0.005)	0.001 (0.006)	-0.002 (0.005)	0.001 (0.006)
Black neighbor (k=3)	0.028*** (0.005)	0.025*** (0.006)	0.013** (0.006)	0.014** (0.006)	0.005 (0.005)	0.005 (0.006)	0.005 (0.005)	0.005 (0.006)
Black neighbor (k=4)	0.022*** (0.005)	0.017*** (0.006)	0.011* (0.006)	0.009 (0.006)	0.004 (0.006)	0.004 (0.006)	0.004 (0.006)	0.004 (0.006)
Black neighbor (k=5)	0.028*** (0.005)	0.022*** (0.006)	0.014*** (0.006)	0.011* (0.006)	0.008 (0.005)	0.005 (0.006)	0.008 (0.005)	0.005 (0.006)
Black neighbor (k=6)	0.025*** (0.005)	0.021*** (0.006)	0.012** (0.006)	0.011* (0.006)	0.007 (0.005)	0.007 (0.006)	0.007 (0.005)	0.007 (0.006)
Black neighbor (k=7)	0.019*** (0.005)	0.018*** (0.006)	0.006 (0.006)	0.008 (0.006)	0.002 (0.006)	0.005 (0.006)	0.002 (0.006)	0.005 (0.006)
Black neighbor (k=8)	0.024*** (0.005)	0.019*** (0.006)	0.007 (0.006)	0.006 (0.006)	-0.005 (0.005)	-0.005 (0.006)	-0.005 (0.005)	-0.005 (0.006)
Black neighbor (k=9)	0.035*** (0.005)	0.028*** (0.006)	0.019*** (0.005)	0.015** (0.006)	0.011** (0.005)	0.008 (0.006)	0.011** (0.005)	0.008 (0.006)
Black neighbor (k=10)	0.034*** (0.005)	0.029*** (0.006)	0.009* (0.005)	0.008 (0.006)	-0.004 (0.005)	-0.003 (0.006)	-0.004 (0.005)	-0.003 (0.006)
Controls	No	Yes	No	Yes	No	Yes	No	Yes
FE	None	None	State	State	County	County	Enumdist	Enumdist
N	139,949	121,971	139,949	121,971	139,949	121,971	139,949	121,971

*p < .1; **p < .05; ***p < .01

Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level.

Table A11: Effect of Black neighbors on future partisanship – NC '09 sample

	(1)	(2)	(3)	(4)	(5)	(6)
Black neighbor (k=1)	0.025*** (0.005)	0.018*** (0.006)	0.022*** (0.008)	0.016 (0.010)	0.024** (0.010)	0.016 (0.011)
Black neighbor (k=2)	0.001 (0.006)	0.004 (0.006)	-0.007 (0.008)	-0.007 (0.010)	-0.005 (0.010)	-0.007 (0.011)
Black neighbor (k=3)	0.005 (0.006)	0.006 (0.006)	0.005 (0.008)	0.007 (0.010)	0.004 (0.010)	0.008 (0.011)
Black neighbor (k=4)	0.004 (0.006)	0.005 (0.006)	-0.001 (0.008)	0.0003 (0.010)	-0.001 (0.010)	-0.001 (0.011)
Black neighbor (k=5)	0.009* (0.006)	0.007 (0.006)	-0.0001 (0.008)	-0.004 (0.010)	-0.0001 (0.010)	-0.002 (0.011)
Black neighbor (k=6)	0.007 (0.006)	0.007 (0.006)	0.008 (0.008)	0.004 (0.010)	0.007 (0.010)	0.007 (0.011)
Black neighbor (k=7)	0.002 (0.006)	0.005 (0.006)	0.001 (0.008)	0.006 (0.010)	-0.001 (0.010)	0.002 (0.011)
Black neighbor (k=8)	-0.004 (0.006)	-0.005 (0.006)	-0.005 (0.008)	-0.005 (0.010)	-0.005 (0.010)	-0.001 (0.011)
Black neighbor (k=9)	0.011* (0.005)	0.008 (0.006)	0.014* (0.008)	0.011 (0.010)	0.019* (0.010)	0.013 (0.011)
Black neighbor (k=10)	-0.002 (0.005)	-0.002 (0.006)	-0.005 (0.008)	-0.005 (0.009)	-0.005 (0.009)	-0.006 (0.011)
Controls	No	Yes	No	Yes	No	Yes
FE	Reel	Reel	Page 10	Page 10	Page 5	Page 5
N	139,949	121,971	139,949	121,971	139,949	121,971

*p < .1; **p < .05; ***p < .01

Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level.

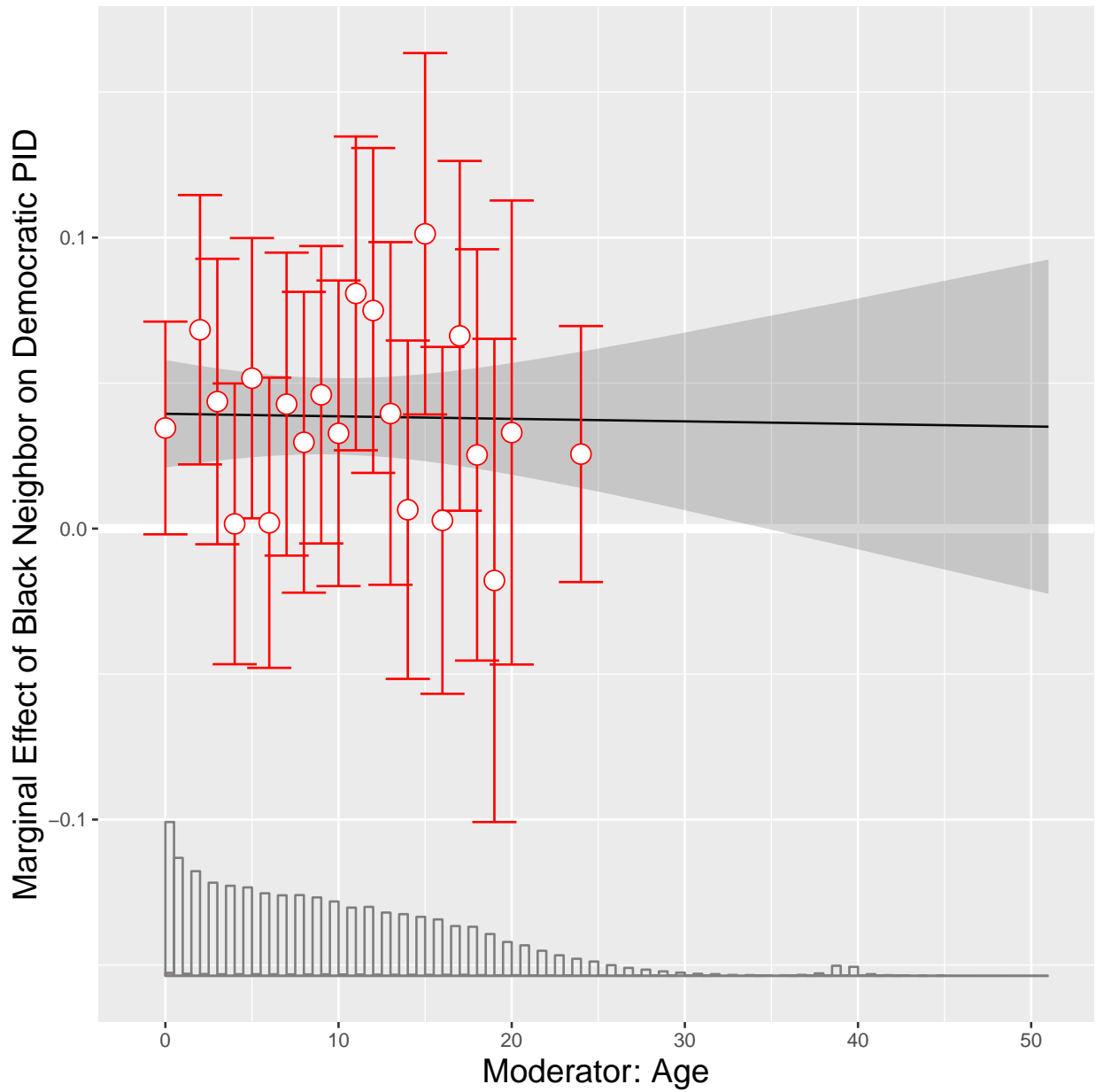
Table A12: Effect of Black neighbors on future partisanship – NC '09 sample

	(1)	(2)	(3)	(4)	(5)	(6)
Black neighbor (k=1)	0.031** (0.015)	0.021 (0.018)	0.026* (0.015)	0.014 (0.018)	0.031 (0.027)	0.013 (0.032)
Black neighbor (k=2)	-0.006 (0.014)	-0.006 (0.017)	-0.005 (0.014)	-0.007 (0.017)	-0.002 (0.026)	0.0003 (0.031)
Black neighbor (k=3)	0.003 (0.014)	0.007 (0.017)	0.014 (0.014)	0.022 (0.017)	0.005 (0.025)	0.026 (0.030)
Black neighbor (k=4)	0.010 (0.014)	0.011 (0.017)	-0.010 (0.014)	-0.016 (0.017)	-0.007 (0.025)	-0.014 (0.030)
Black neighbor (k=5)	0.0003 (0.014)	-0.005 (0.017)	-0.010 (0.014)	-0.007 (0.017)	-0.006 (0.024)	-0.003 (0.028)
Black neighbor (k=6)	0.007 (0.014)	0.006 (0.017)	0.010 (0.014)	0.010 (0.017)	0.004 (0.024)	0.009 (0.029)
Black neighbor (k=7)	0.004 (0.014)	0.007 (0.017)	0.005 (0.014)	0.002 (0.017)	-0.004 (0.024)	-0.006 (0.029)
Black neighbor (k=8)	0.0004 (0.014)	-0.001 (0.017)	-0.007 (0.014)	-0.001 (0.017)	-0.001 (0.024)	0.004 (0.028)
Black neighbor (k=9)	0.015 (0.014)	0.009 (0.017)	0.011 (0.014)	0.008 (0.017)	0.011 (0.024)	0.001 (0.029)
Black neighbor (k=10)	-0.008 (0.014)	-0.012 (0.016)	0.002 (0.013)	-0.003 (0.016)	-0.007 (0.023)	-0.014 (0.028)
Controls	No	Yes	No	Yes	No	Yes
FE	Page 2	Page 2	Page 2 Alt	Page 2 Alt	Page	Page
N	139,949	121,971	139,949	121,971	139,949	121,971

*p < .1; **p < .05; ***p < .01

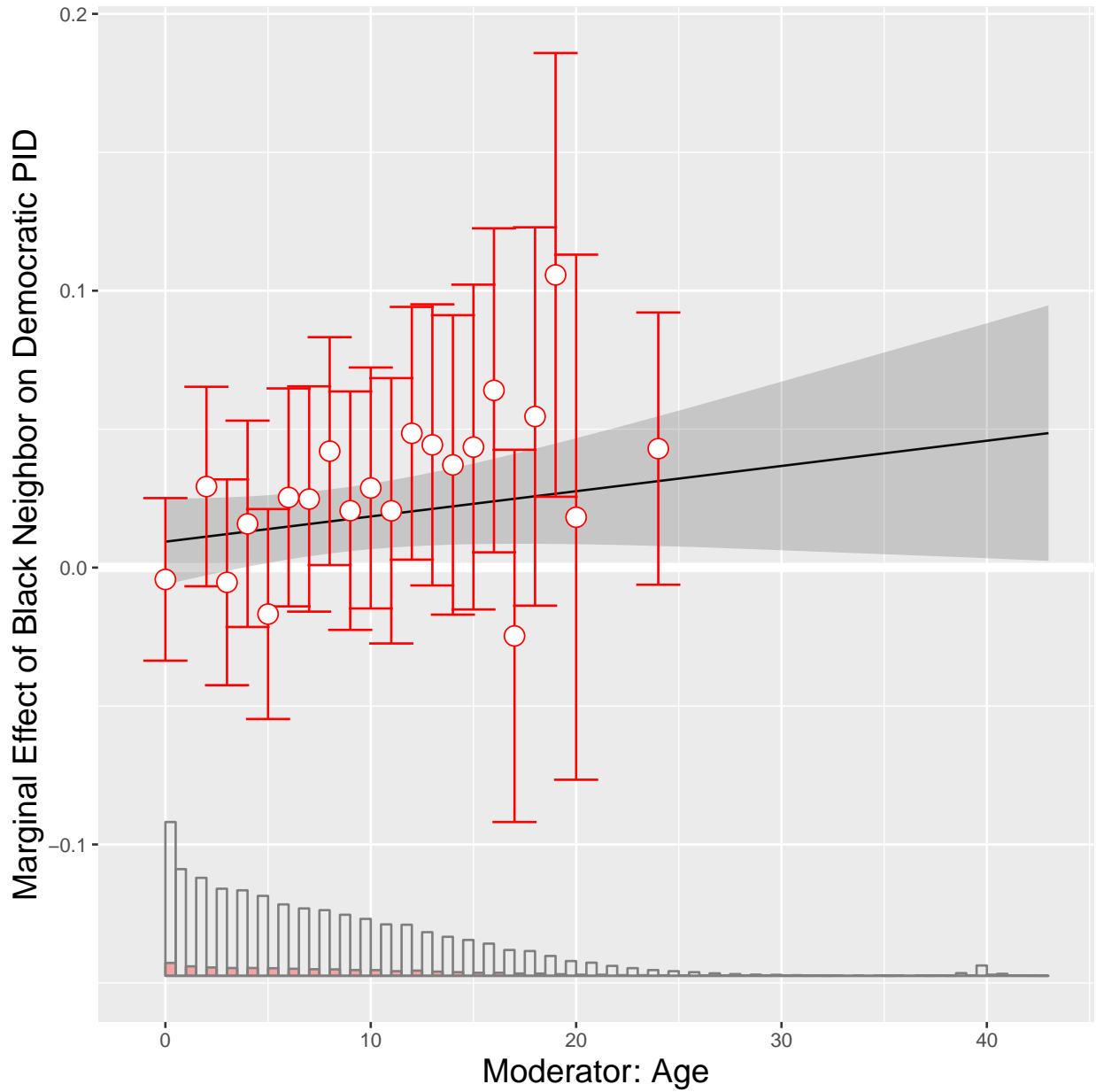
Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level.

Figure A9: Neighbor effect by age, enumeration district fixed effects – CA '05 sample



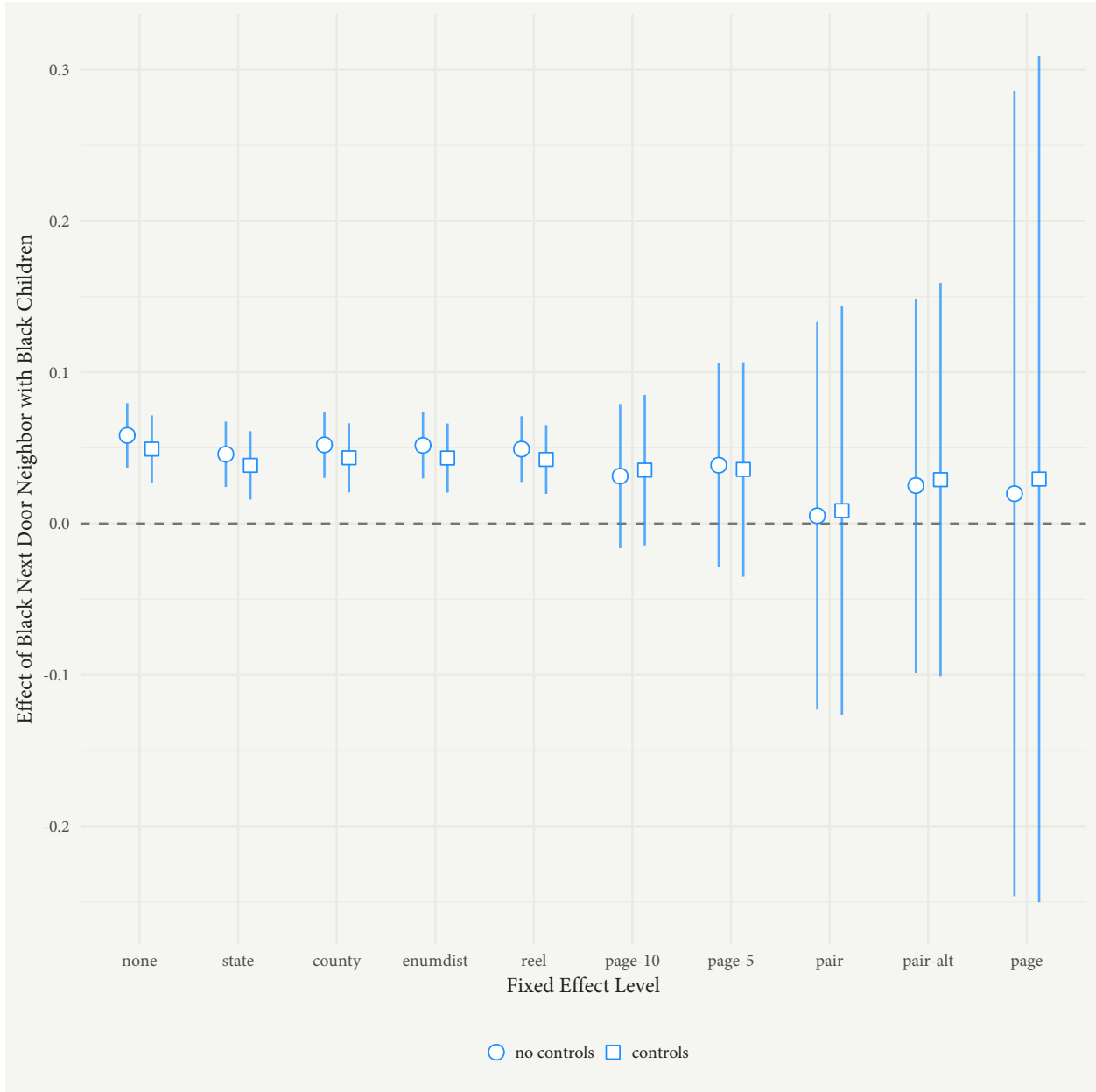
Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level.

Figure A10: Neighbor effect by age, enumeration district fixed effects – NC '09 sample



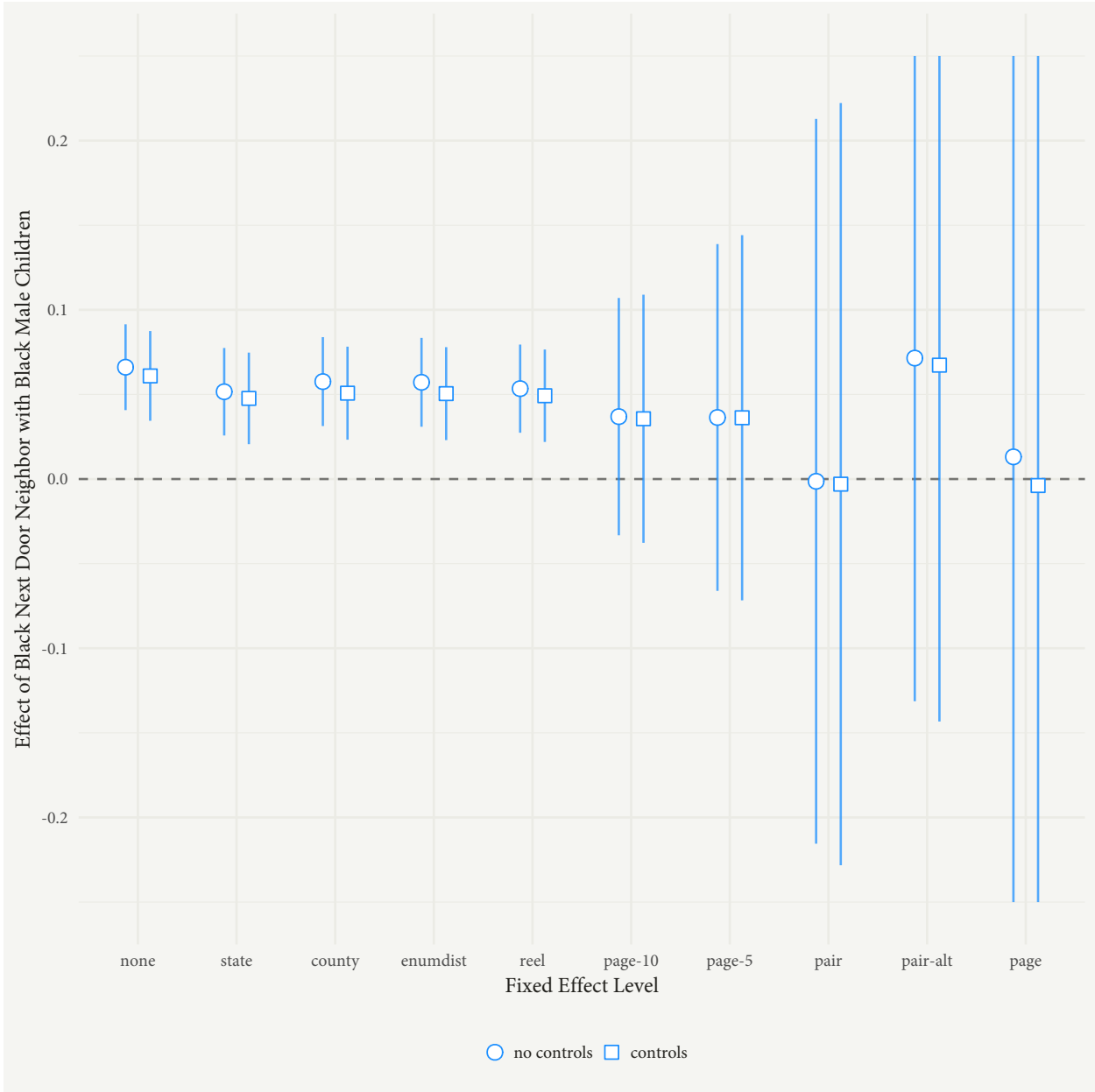
Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level.

Figure A11: Effect of Black 1940 next door neighbor with children on 2005 Democratic partisanship – CA '05 sample



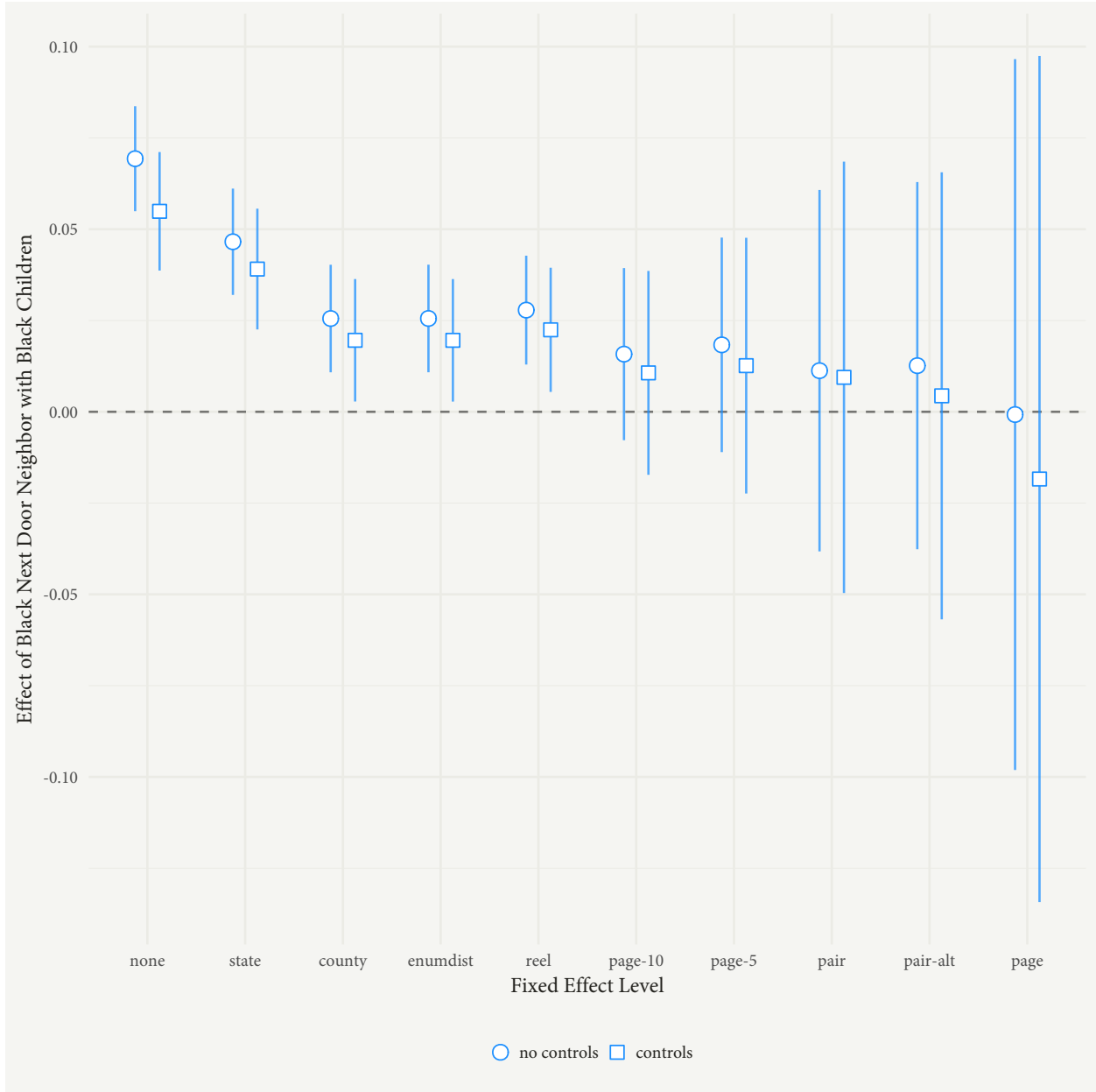
Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level. Control group in this analysis are on Whites with a next door neighbor with non-Black children.

Figure A12: Effect of Black 1940 next door neighbor with male children on 2005 Democratic partisanship – CA '05 sample



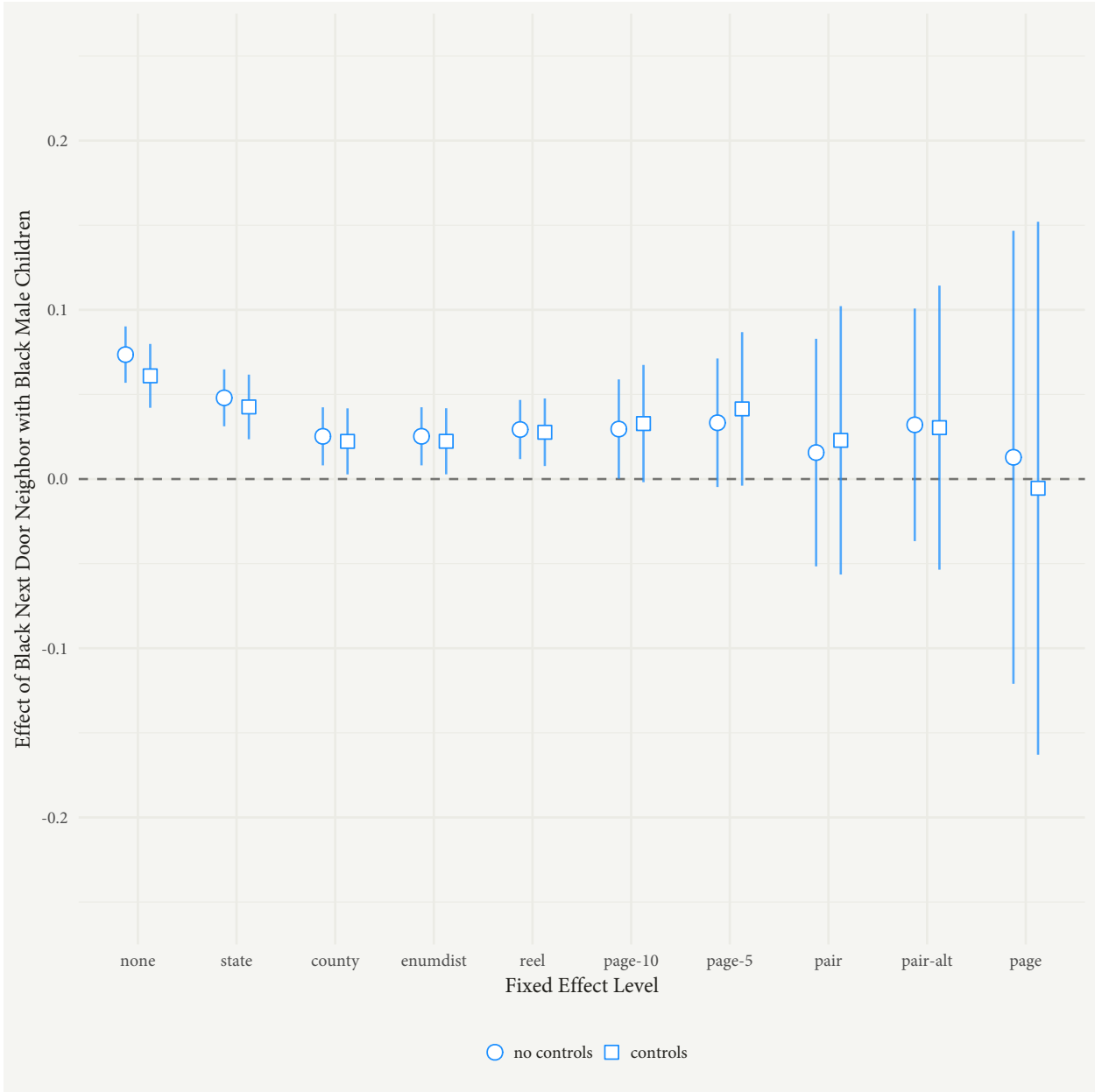
Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level. Control group in this analysis are on Whites with a next door neighbor with non-Black male children.

Figure A13: Effect of Black 1940 next door neighbor with children on 2009 Democratic partisanship – NC '09 sample



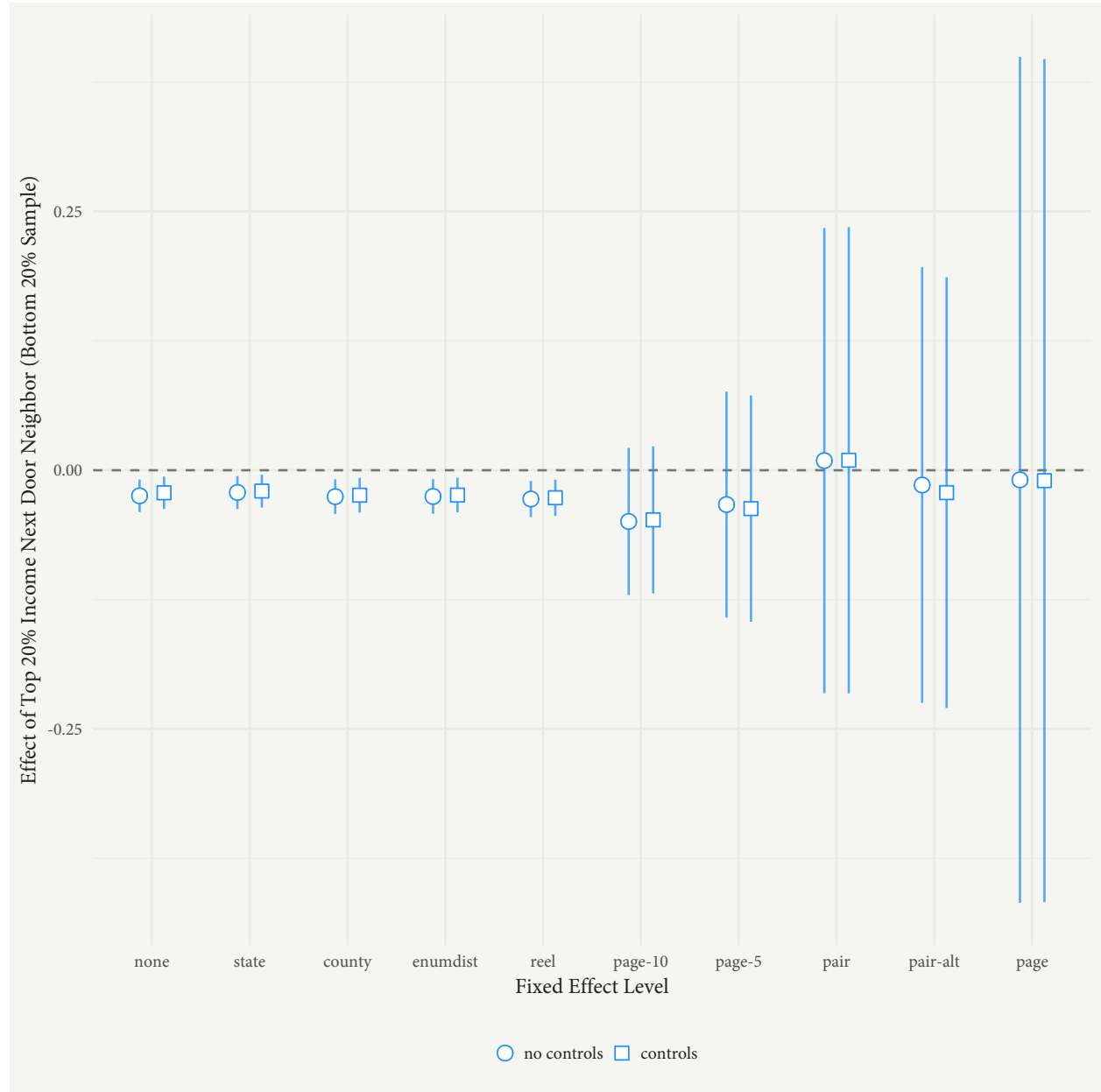
Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level. Control group in this analysis are on Whites with a next door neighbor with non-Black children.

Figure A14: Effect of Black 1940 next door neighbor with male children on 2009 Democratic partisanship – NC '09 sample



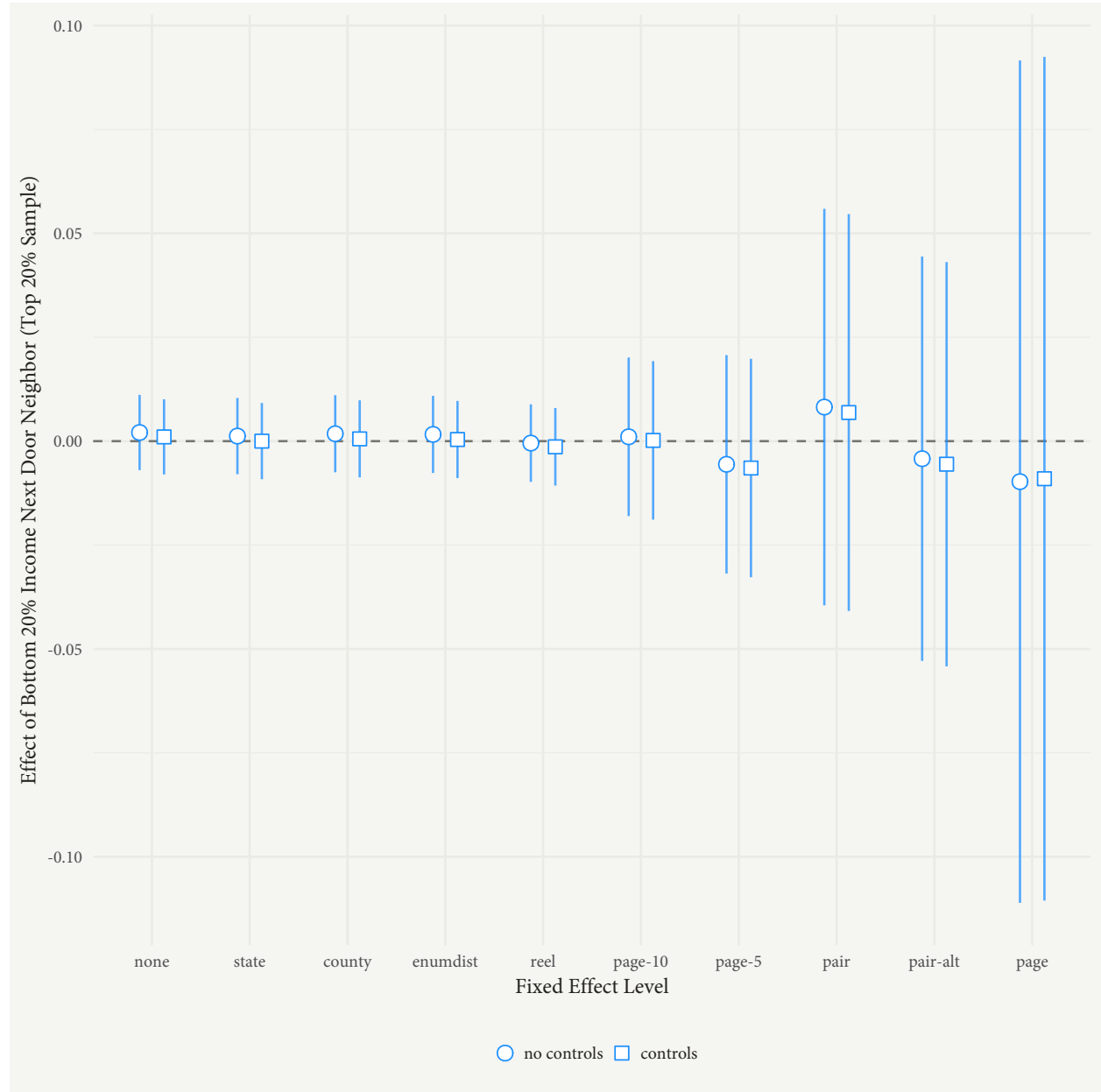
Note: Coefficient estimates are from models fitted to subset of whites in the data. Standard errors are clustered at the Census page level. Control group in this analysis are on Whites with a next door neighbor with non-Black male children.

Figure A15: Among wealthy, the effect of 1940 poor next door neighbor on 2005 Democratic partisanship – CA '05 sample



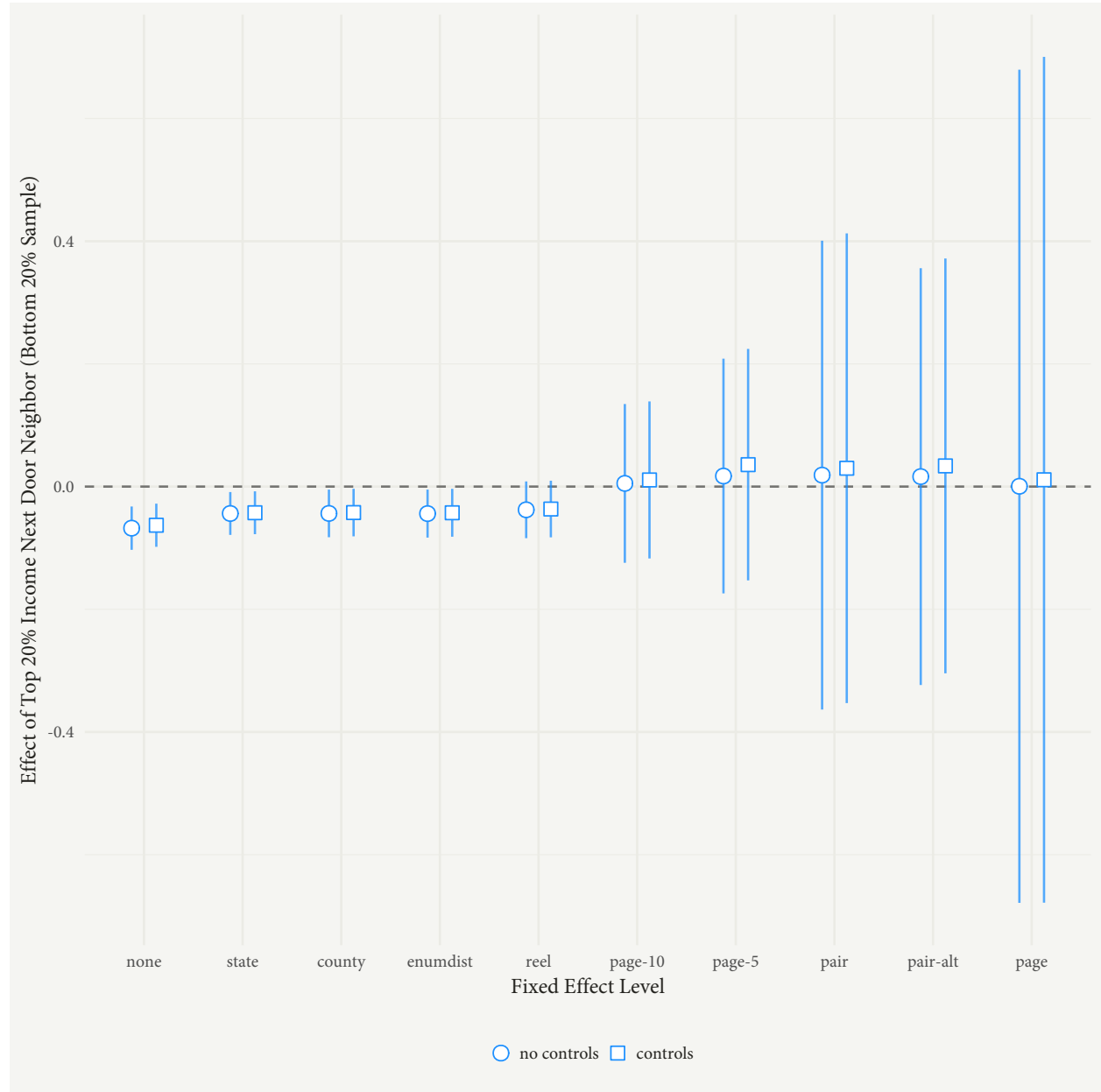
Note: Coefficient estimates are for models fit to subset of individuals whose head of the household reported earning income in the top 20% of the head of the household income distribution in 1940. Standard errors are clustered at the Census page level.

Figure A16: Among poor, the effect of rich next door neighbor in 1940 on 2005 Democratic partisanship – CA '05 sample



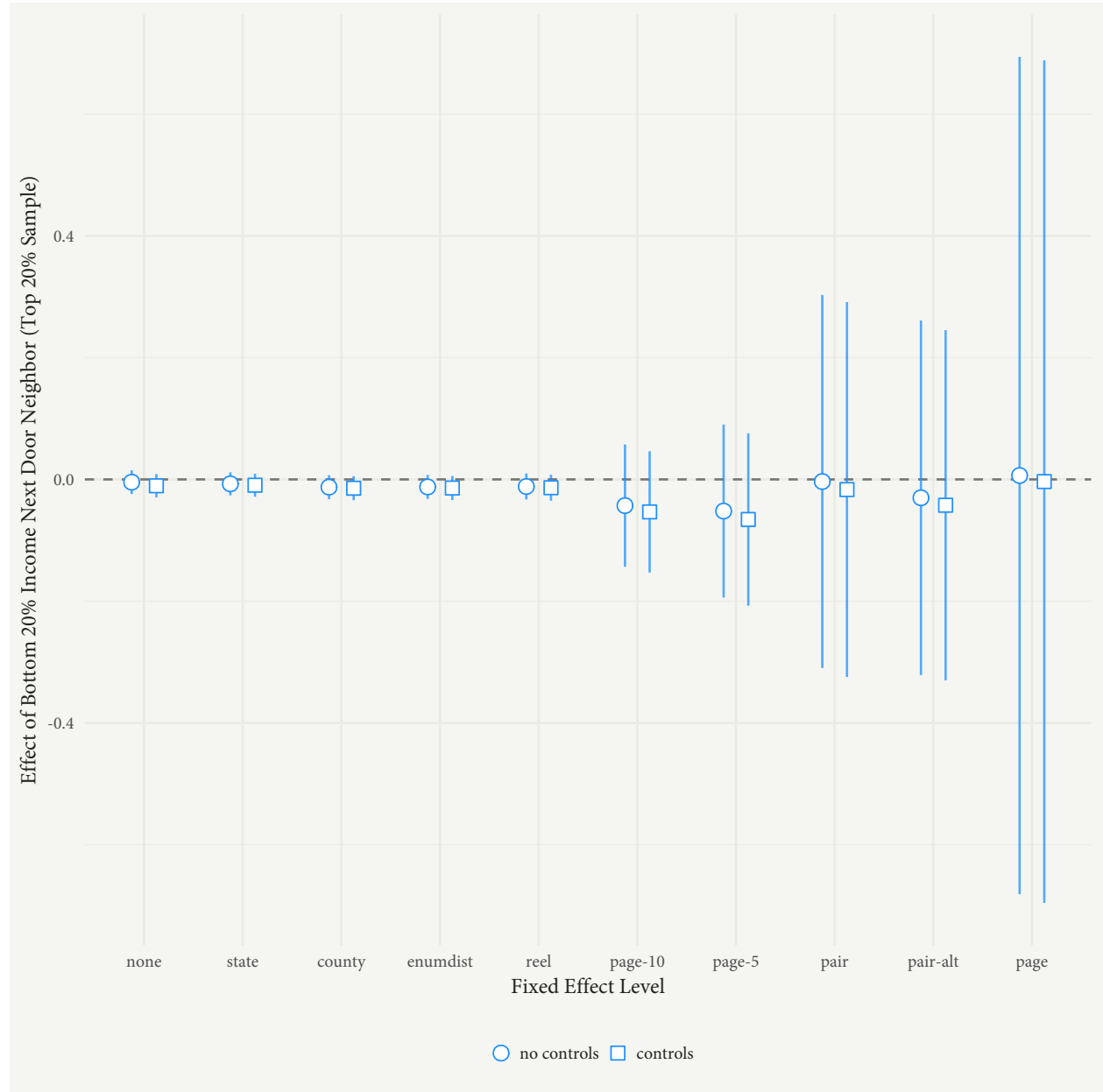
Note: Coefficient estimates are for models fit to subset of individuals whose head of the household reported earning no income in 1940. Standard errors are clustered at the Census page level.

Figure A17: Among wealthy, the effect of 1940 poor next door neighbor on 2009 Democratic partisanship – NC '09 sample



Note: Coefficient estimates are for models fit to subset of individuals whose head of the household reported earning income in the top 20% of the head of the household income distribution in 1940. Standard errors are clustered at the Census page level.

Figure A18: Among poor, the effect of rich next door neighbor in 1940 on 2009 Democratic partisanship – NC '09 sample



Note: Coefficient estimates are for models fit to subset of individuals whose head of the household reported earning no income in 1940. Standard errors are clustered at the Census page level.