The Political Consequences of Spatial Policies: How Interstate Highways Facilitated Geographic Polarization

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Abstract

In the postwar era, Republican voters have become increasingly more likely than Democratic voters to live in non-urban counties, and the two parties distributed across increasingly distinct geographic enclaves. Public policies that shape geographic space have been a major contributor to this *geographic polarization*. This article examines the effect of the Interstate Highway System, the largest public works project in American history, on this phenomenon. Drawing on a historical database of postwar U.S. highway construction since passage of 1956 highway legislation, it shows that suburban counties with Interstate became more Republican than they would have otherwise, primarily in the less urbanized South and where highways were built earlier. Metropolitan areas with denser Interstate networks also became more polarized. Analysis of the Youth-Parent Socialization Panel Study (1965-1997) reveals individual-level mechanisms underlying these changes: suburbs along Interstates facilitated white flight and became home to more affluent residents, reinforcing partisan geographic polarization.
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American partisans are increasingly sorted by population density. While both parties have been suburbanizing for decades, Republicans have become much more likely to live in low-density suburban and exurban areas than Democrats (Schneider, 1992; Gainsborough, 2001). By multiple measures, metropolitan areas have become more polarized along an urban-to-rural continuum, and this *geographic polarization* has grown along with Congressional polarization, growing income inequality, and increasing residential income segregation (McCarty, Poole and Rosenthal, 2006; Reardon and Bischoff, 2011).

Sorting of Democrats and Republicans by residential density has influenced which issues reach the parties' national agendas, how public goods are distributed in metropolitan areas (Einstein, 2011; Gerber and Gibson, 2009), and how votes are translated into seats in state legislatures and Congress (Chen and Rodden, 2011). Politicians in the two parties speak to partisan audiences that have become increasingly sorted not just on policy issues (Levendusky, 2009), but also in their geographic location. Policies related to population density have become increasingly linked to party agendas. In 1968, for example, many white swing voters lived in urban counties, and both party platforms devoted multiple paragraphs to urban policy (Democratic Party, 1968; Republican Party, 1968*a*). By 2012, only the Democratic platform spoke of urban issues at any length, while the Republican platform's sole discussion was to accuse Democrats of "pursuing an exclusively urban vision of dense housing and government transit" (Baker, 2012; Democratic Party, 2012; Republican Party, 1968*b*). The urban-suburban divide propagates into numerous policy disputes, from urban financial control, to transportation financing, to electoral reform laws that disproportionately affect urban minorities.

Though the urban-suburban socioeconomic, racial, and partisan divide has been a persistent feature of American politics, its growth in the last half century has been exceptional. Urban-suburban polarization has doubled since World War II, and has grown monotonically since 1970. Figure 1 presents the difference in the Democratic vote between the county containing a central city and other counties in the same Census 2000 metropolitan statistical areas (MSAs) with a major city of at least 200,000 persons (Leip, 2012), for the country as a whole and for metropolitan areas in and out of the South. Regardless of region, urban-suburban polarization has grown since the mid-1950s. The indices increased faster in the South, possibly as a result of franchise expansion to urban black voters and the substantial partisan realignment of white voters. However, polarization also increased in non-Southern metropolitan areas after 1970. Over the same period, these changes have marched in lockstep with income segregation (Reardon and Bischoff, 2011), income inequality, and Congressional polarization (McCarty, Poole and Rosenthal, 2006).

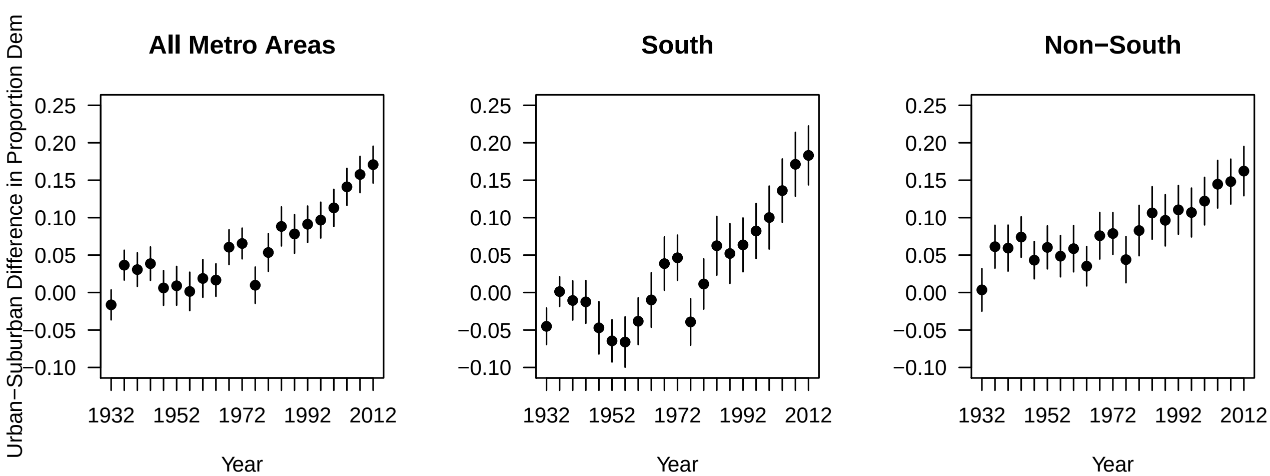

Figure 1: Mean urban-suburban difference in the Democratic presidential vote, 1932-2012, for all counties (left), Southern counties (center), and non-Southern counties (right). 95% confidence intervals for the unweighted metro-level means accompany each estimate.

The urban-suburban political divide has not occurred only by chance or as a result of individual-level residential choices operating in a vacuum. Previous research on segregation has explained how segregation on correlates of partisanship—such as race and income—can be explained by the aggregation of *ex ante* individual-level preferences (e.g., Schelling 1971; Farley 1995) or of local communities trying to lure residents who "vote with their feet" (Tiebout, 1956; Lewis and Neiman, 2009). Others explain segregation as a consequence of exclusionary policies such as zoning, redlining, and racially exclusive covenants that restrict residential opportunities (Levine, 2006; Massey and Denton, 1993). Regardless of which of these approaches explains more of the variation in sorting on partisanship and its demographic correlates, neither individual preferences nor overtly discriminatory policies are sufficient to explain partisan sorting that has been observed. In fact, major public policies that at first glance appear to be universalistic and non-discriminatory exacerbate the urban-suburban divide by selectively facilitating greater residential mobility among some groups than others, resulting in different social, economic, and political geography, than would have existed otherwise, changing the geographic base of the two major parties and the geographic bases of support for public policies. I call this consequence of policies that shape spatial relationships *spatial policy feedback*.

This article considers the political consequences of one of the most important of these "spatial policies": the Federal Aid Highway Act of 1956, colloquially known as the Interstate Highway Act. The largest public works project in American history, the 41,000-mile system conceived in the legislation connected urban, suburban, and rural areas.[1] By stimulating new suburban residential growth—especially in the Sun Belt—highways helped create a new class of suburb reliant on federal housing and transportation policy. Highways facilitated urban-suburban partisan sorting by creating the conditions for whites and middle- and upper-income to move from declining cities into single-family residential neighborhoods built along freeways. The unintended result has been partisan residential sorting and associated geographic polarization, especially in areas of the country, such as the South, in which highways had a significant role in suburban development.

[1]Later highway legislation added 3,000 miles to the system.

This article explicates highways' role in the development of this spatial, political hierarchy, examining two plausible empirical implications of Interstate highways using data on both presidential election returns and Interstate construction records. Previous scholars have shown that highways stimulated suburban growth (Baum-Snow, 2007) and rural development (Chandra and Thompson, 2000), showing that transportation infrastructure can lead to economic and population change. However, no previous studies have rigorously examined the extent to which highways have had a causal effect on what Rae (2001) calls the "viacratic hierarchy": separation of the rich, mobile, suburban (and more Republican) from the poor, immobile, and urban (and Democratic). Highways facilitated the migration of whites and wealthy Americans into new, suburban residential areas along major highways, which not only grew in population, but also became more Republican.

This paper presents two major aggregate-level implications of infrastructure-induced political change, demonstrating that highways changed political geography at county and metropolitan levels. It then examines observational data to ascertain the individual-level mechanisms underlying observed geographic change. First, matching comparable suburban counties with and without Interstates, I show that highways made suburbs in which they were built substantially more Republican, with much of the effect originating in the South and in counties where highways were built earliest. Next, using data on Interstates' overall density within metropolitan areas, and controlling for population and other confounders, I show that metropolitan areas with more Interstates became more polarized over time than those with fewer. Finally, I present observational findings from a restricted version of the Youth-Parent Socialization Panel Study (YPSPS), a panel survey of the high school Class of 1965. Results from this study show that Interstates were central to migration. Republicans were more likely than Democrats to move from urban areas after high school, and when they did were much more likely to settle in zip codes along non-urban Interstate highways. Republicans who started out in non-urban areas were more likely to end up in communities along Interstates than Democrats who also started adulthood outside cities. These differences appear to have been caused by both racial and economic sorting. Collectively, these related findings show that Interstate highways added to urban-suburban polarization by advancing Republican suburbs and speeding white flight from major metropolitan areas.

1 Transportation Infrastructure and Causal Inference

Political scientists have rarely studied transportation infrastructure except as an instrument of "pork-barrel" distributive politics (Golden and Min, 2013), and then they usually treat infrastructure as a dependent variable, not an explanatory variable. Yet infrastructure often has consequences beyond its role in "greasing the wheels" of the legislative process (Evans, 1994). Other scholars, mostly outside of political science, have often noted infrastructure's substantial consequences, from the spread of antislavery and religious ferment in the Erie Canal's "burned over district" (Cross, 1950), to the role of turn-of-the-century transit lines in racial and ethnic segregation (Oliver 2010, 100) or the use of urban infrastructure projects as a method of social control (Scott, 1998). Others have noted the neighborhood-level segregation that arises from placement of transportation infrastructure. Local governments have rerouted highways to separate white and black neighborhoods (Connerly 2002, Chicago Historical Society 2004, Kruse 2005, 86), and major streets and rail lines often become lines of demarcation between neighborhoods (Grannis, 2009; Ananat and Washington, 2009).

While previous research on infrastructure's effects has tended to offer suggestive observations, the Interstate Highway System offers an ideal policy case for studying infrastructure's causal effects on political geography. First, it was both large in scope (41,000 miles in length, as originally planned) and centrally organized, distinguishing it from previous infrastructure programs involving small projects that were easily manipulated by members of Congress or state legislators (Weingast and Wallis, 2005), complicating estimation of their causal effects because political manipulation is difficult to model. By contrast, the Federal-Aid Highway Act of 1956 established a single, nationwide program for highway construction, and the gas-tax-financed Highway Trust Fund covering 90% of the cost. Rather than opening the door to congressional meddling in highway placement, the program gave state and federal highway engineers substantial discretion to identify routes to serve traffic most effectively, discretion that they exercised quite freely through the 1960s (Seely, 1987). The process by which highways were built strategically can therefore be statistically recreated more compellingly for Interstates than for many other policy interventions.[2]

Studies of major infrastructure programs commonly adopt a historical counterfactual approach, making great claims about the sweep of history without quantifying those claims or justifying assumptions. Major suburban histories that mention Interstate highways claim that the program was crucial to suburban growth and changing suburban politics (Jackson 1985, 249-250; Fishman 1989, 190-191). Such claims have face validity but rarely state the strong assumptions necessary to conclude that the public policy was responsible for American suburbanization (and not vice versa). Classic research in economic history has examined the effect of infrastructure policies by constructing counterfactuals rooted in the

[2]The preeminent account of highways' effects on cities notes that state and federal engineers, not local officials, had substantial discretion over highway routing: "Federal funds for an arterial highway within a city were handed not to the city but to the state in which it was located. The state—not the city—had the power to spend those funds. The state had the final say on every detail of the plans and specifications, down to the selection of the specific city streets and lots along which the roads would run" (Caro, 1975, 711). Highway engineers with a "production orientation" regarded highway construction as the obvious technical solution to traffic congestion, and considered placement of highways to serve traffic as a scientific process (Rose and Seely, 1990).

researchers' conception of alternative policy histories. Fogel (1964), for example, estimates the effect of railroads on economic growth by constructing a synthetic historical counterfactual in which canals, rather than railroads, were built across the Great Plains region. Setting aside the extreme counterfactual reasoning implied, Fogel's approach is at least explicit about the proposed counterfactual, a standard to which little work on Interstates' effects has risen.

Instead of attempting to reconstruct the historical counterfactual of a country without Interstate highways, this paper exploits spatial and temporal variation in highways' placement to infer the effects of highway construction on political geography. Throughout, this is done by examining the politics where Interstates were built, compared to a counterfactual inferred from comparable units that had no (or fewer) highways. To causally interpret these results, it is assumed throughout that Interstate highways were assigned "as-if" randomly to places, conditional on relevant measured observables (Dunning, 2012). Past research has tried to justify this assumption several ways. One way (and the least believable) is to assume that highways were placed randomly. Early work used interrupted time series in local case studies highways' effects on local economic development (e.g., Garrison et al. 1959). Other recent work has assumed that the highways were placed to connect cities, and were placed effectively randomly in rural counties (Chandra and Thompson, 2000, 482). Such an approach assumes that reverse causality is not a concern. Concerns about reverse causality are typically addressed through instrumental-variables methods, which exploit seemingly exogenous instruments, including preexisting infrastructure, geography, or planning documents to predict highway placement (Baum-Snow, 2007; Duranton and Turner, 2008), but the exogeneity of such instruments is often questionable.[3]

This article adopts a different approach, using matching and linear regression to account for the "as-

[3] A shortcoming of the instrumental variables approach is that the effect can be estimated only in places in which the instrument induced highway construction.

signment mechanism" by which highway locations were chosen.[4] Compared to other federal policies that enabled suburbanization, the Interstate program fits well with this research design: a well-documented plan was adopted before construction, enabling researchers to reconstruct, and control for, the non-random assignment process. Key criteria used in planning appear in the 1944 *Interregional Highways* report, the first detailed proposal of a national expressway system that was the basis for the present-day Interstate System (United States, Public Roads Administration, N.d.). Throughout the peak period of Interstate building, from the mid-1950s to the late 1960s, highway engineers were given substantial latitude to identify needed routes, using well-documented technical criteria, most of which appeared in the 1944 report (Seely, 1987). This differs substantially from present-day roadbuilding and one-off projects commonly studied in the distributive politics literature (Lee, 2003). Using data on the criteria used in roadbuilding leading into this period, generating an "as-if random" estimate of highways' effects is tenable. Indeed, even if technocratic criteria were used to justify politically motivated decisions *post hoc*, controlling for these factors also accounts for political manipulation.

In the next two sections, I examine highways' effects on the geography of two settings: on the suburban counties in which they were built, and on metropolitan areas.

2 Highways and Suburban Political Development

To begin, I estimate highways' effects on the political composition of suburbs, combining data from a Federal Highway Administration database of Interstate construction through 2008, county-level Census data through 1950 and the Democratic share of the presidential vote from 1948 to 2008.

[4]For a recent example, see Rephann and Isserman (1994).

2.1 Data and Methods

I define suburban counties as those with geographic centroids 20 to 100 kilometers from the center of the 100 most populous cities in 1950.[5] County data permit over-time comparisons over the Interstate highway era that are untenable using more recently available, higher-resolution data, such as precinct voter files (e.g., King and Palmquist 1998; Ansolabehere and Rodden 2012) or contemporary party-identification data from voter. Counties often delimit school districts, public services, and other factors relevant to residential sorting, making them units of interest in their own right. The full suburban county sample of $n = 988$ used in matching and regression analyses appears in Figure 2. Summary statistics for this sample appear in the Supporting Information, Table A-1.[6]

Estimation of highways' effects needs to account for two features of infrastructure: its permanence, and the date of initial construction. First, once an Interstate is built in a county, it is rarely removed, meaning that once a county is "treated" with an Interstate, it is unnecessary to use methods that allow the treatment variable to alternate in value at each time period. Second, it is necessary to account for variation

[5] Any definition of a metropolitan area is sensitive to researcher choice (Rosenbaum, 1999). Fixing the geographic units using selection criteria defined at a single point in time permits over-time comparisons, allowing metropolitan boundaries to vary with changing population density over time (for example, by comparing urban and rural areas defined in the decennial Census). A 100-kilometer radius captures approximately a one-hour commute under typical Interstate highway speeds. Such areas would be most susceptible to highway-induced change. A discussion of the sensitivity to the radius choice and varied density cutoffs appears in the Supporting Information, and is applied to an analysis using a simplified treatment variable (p. 43).

[6] Proximity of each county to the closest major city center is defined by the point location in the StreetMap USA Cities layer, usually located in the central business district (ESRI, 2008).

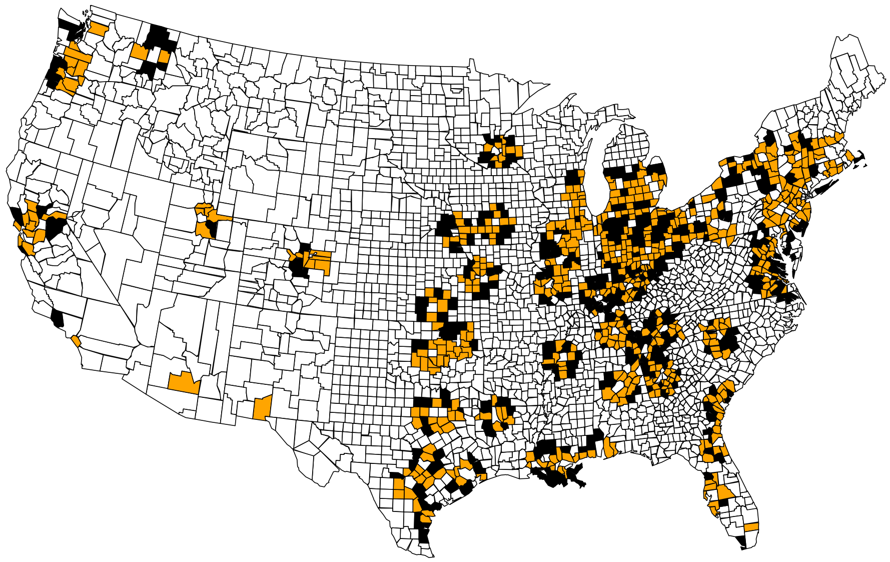

Figure 2: Map of the suburban-county sample. Counties containing an Interstate highway through 1996 are lightly shaded, while those without an Interstate are black.

in the timing of freeway construction, which featured a boom of relatively unrestricted construction until the late 1960s, with diminishing construction as the network was mostly completed by 1980. A present-day county with Interstates built in the late 1950s would have had its development shaped by their presence twice as long as the few counties in which Interstates were built in the late 1970s. By 1965, Interstates had already opened in 51% of suburban counties that would eventually have them. Another 34% of such counties would get Interstates by 1972, and by 1980, another 11%. To consider the variable timing of Interstate construction, all subsequent analyses are divided into three non-overlapping periods: the initial period of highway construction (1956 through 1963), the middle period of highway construction (1964 through 1971), and a late period of highway construction (1972 through 1979).

For each of the three treatment periods, I define a county as "treated" by Interstates if at least one

Interstate opened during the treatment period in question, using the Federal Highway Administration's PR-511 database of highway construction segments as assembled and geocoded by Baum-Snow (2007). I then examine the effect of being treated on the Democratic share of the presidential vote in subsequent elections. To account for regional heterogeneity in effects, this process is repeated for all suburban counties, those in the South, and those outside the South.

To model the effect of the presence of Interstates on each of these cohorts, and to account for factors that led to non-random assignment of highways to counties, I account for potential confounders by matching comparable counties using coarsened exact matching (CEM). For each of the three treatment cohorts, for each region, and for each presidential election year following the treatment cohort, counties with an Interstate are matched to comparable counties in which an Interstate had not yet been built. A least squares regression was then estimated on each of these samples using the same treatment variable and covariates used in matching (Ho et al., 2007; Iacus, King and Porro, 2011*a*,*b*). CEM places observations in multidimensional bins created using coarsened versions of the covariates, then assembles a sample only of the treated counties (those with an Interstate) and untreated counties (those without) that are matched on each multidimensional subclass.[7] Both treated and untreated observations that are not matched in a common bin are discarded.[8]

County-level Census and political variables used in matching reflect criteria for highway placement that appear in the *Interregional Highways* report, or could otherwise act as confounders. Demographic and economic factors include *Log population density*, *Crop value per capita*, and *Percentage urban* in 1950; and the *Number of manufacturing establishments in 1939*, which capture factors mentioned in the

[7]Each covariate is subclassified into at most three coarsened categories.

[8]The result of this subclassification and trimming of the sample yields a local estimate of the average treatment effect on the treated (ATT) for observations that could be well matched.

1944 report. Above all, highways were built to serve population centers and industrial and agricultural markets.[9] A dummy variable for *Strategic route* is included to indicate whether a county was on or near a strategic military route as defined in 1941 (United States, Public Roads Administration, N.d., 33). In the absence of county-level vehicle registration data, *Median family income in 1950* is included as a correlate of county-level pre-1956 automobile ownership, suburban residential development, and partisanship. Other included covariates account for additional potential confounders. The *Percentage of households that lived outside the county in 1949* captures pre-existing suburban migration trends. The *Republican presidential vote share in 1948* accounts for partisan interventions in highway planning and to predict unobserved partisan realignment trends. The *Percentage nonwhite in 1950* is a strong correlate of both partisanship and partisan change. To control for pre-treatment trends in the presidential vote and to capture secular trends in partisan change, the county-level *Republican presidential vote share in 1948 and 1952* was included in each regression. In addition, to account for confounding political trends between the time Interstate highways were assigned through legislation and when they were built in each of the three treatment cohorts, a control is included for the presidential vote as of the first year of each treatment cohort.[10] When constructing a national matched sample, a dummy variable for the *South* is included to account for disproportionate Southern legislative influence over highway policy (which may have led to additional highway construction in the South) and because the strong shift of rural and southern suburban whites to the Republican Party coincided with highway-induced suburbanization in that region. For the regional analyses, this dummy variable is used to define separate Southern and non-Southern samples in the regional analyses.

[9]The crop value variable also proxies for land that is suitable for highway construction.

[10]Findings are insensitive to the decision to include lagged presidential results, and these results appear in the Supporting Information.

This matching process substantially reduces imbalance between treated and untreated suburban counties. The standardized difference in means for each treatment cohort, region, and election year appear in the Supporting Information, Figures A-7-A-15. Matching reduces or eliminates imbalance on most covariates in most post-treatment years. This is true even in later years and in the Southern and non-Southern subsamples.[11]

Next, on the matched samples generated for each treatment cohort, post-treatment election year, and region, I estimate Interstates' effect on the Democratic percentage of the two-party vote using least squares regression, starting in the election years immediately after each treatment period (1964, 1972, and 1980) to 2008:

$$\mathbf{Y_t} \;=\; \beta_{0t} + \beta_z \mathbf{z} + \beta_1 \mathbf{x}_1 + \ldots + \beta_k \mathbf{x}_k + \boldsymbol{\epsilon} \qquad (1)$$

where Y_t is the county Democratic presidential vote share in year t, and $\mathbf{z}$ is a dummy variable representing whether an Interstate was built in the county during the specified treatment cohort. The coefficient β_z captures the effect of interest, and coefficients on $\mathbf{x}_1 \ldots \mathbf{x}_k$ (which are left unreported) are included to adjust for any remaining bias not eliminated during the matching process.

To construct accurate confidence intervals, and to account for election-specific deviations from a "normal" party-line vote, a combination of bootstrapping and lowess smoothing to construct accurate confidence intervals and smooth over election-specific (or candidate-specific) effects. For each matched sample in each region-year, one thousand samples were drawn (with replacement) with a probability equal to the CEM matching weights, which are generated to ensure that causal estimates on the matched sample apply to the matched treated units (Iacus, King and Porro, 2011*b*). The linear regression model

[11]While imperfect balance in the matched data requires stronger modeling assumptions at the linear regression stage, but does not imply that treatment effect estimates will be biased.

specified above was estimated on the national and regional matched samples, yielding 1,000 bootstrapped point estimates for the Interstate highway coefficient in each year. These estimates were smoothed by calculating a locally weighted regression (lowess) of each set of annual simulated point estimates against the election year variable.[12] The quantiles of the resulting lowess-smoothed, simulated values were used to construct 95% and 80% confidence intervals.[13]

2.2 Results

Interstate highways made suburban counties in which they were built less Democratic than they would have been otherwise across most of the study period, though these effects vary across both time and region. By far, the largest effects arose from Interstates constructed earlier in the study period, as well as those in the South (Figure 3). On average, building an Interstate in a suburban county between 1956 and 1963 (the initial boom period in highway construction) reduced the Democratic vote share by 2 to 3 points in each year across most of the study period, with diminishing effects in later years. Most of this effect is attributable to effects in Southern counties of anywhere from 5 to 7 percentage points, a steady effect that has been significant at the 5% level since 1972. Effects in non-Southern counties were, as expected, smaller and less persistent, though the Interstate highways built early on still made such counties 1 to 2 points less Democratic than they would have been otherwise between 1980 and 1996. Highways built in later years had smaller effects on partisan composition. Because about half of Interstates were built during the first treatment cohort, smaller sample sizes for the later treatment cohorts reduce power to detect effects. Nevertheless, on average point estimates fall in the expected direction.

Two major observations emerge from these data: that the effects are large in the South and small

[12]This lowess function uses a span of one-third of the data points in the smoothing kernel and three "robustifying iterations" (Becker, Chambers and Wilks, 1988).

[13]Substantive findings are not vulnerable to use of nonparametric smoothing.

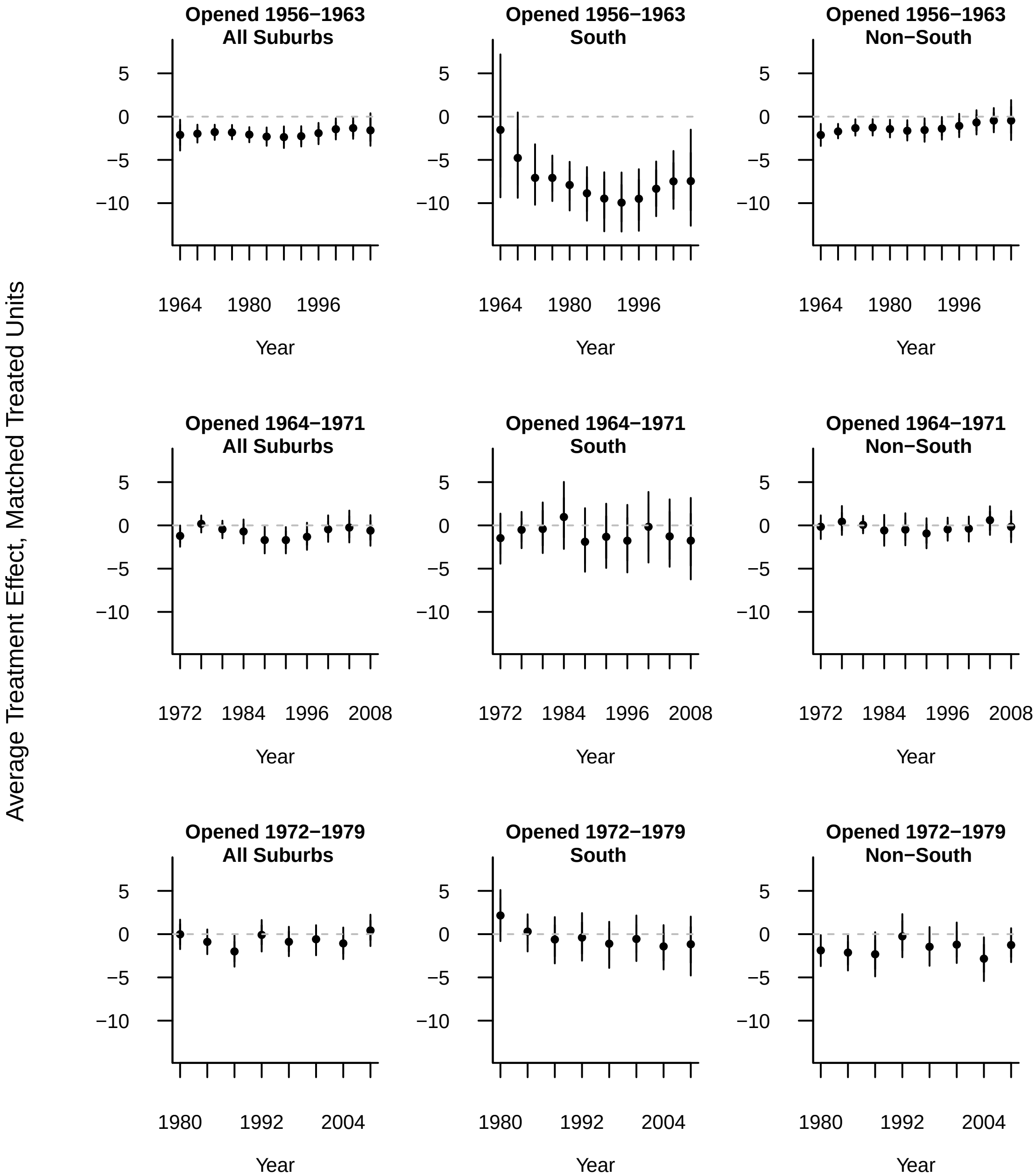

Figure 3: Smoothed OLS estimates of effect of Interstate in a county, using CEM-matched samples. Interstate highways contributed to a decline in the suburban Democratic vote, mostly early on and in the South. 80% (thick line) and 95% (thin line) confidence intervals accompany each estimate. Top row: Interstates opened 1956-1963. Middle row: Interstates opened 1964-1971. Bottom row: Interstates opened 1972-1979.

elsewhere, and highways' effects in suburbs faded out over time. These patterns have likely substantive explanations, as well as a methodological explanation related to the application of causal inference in a geographic context.

While there are many plausible reasons for the larger effects observed in the South, two likely explanations arise. First, while non-Southern suburbs had more developed housing and transportation infrastructure (McShane, 1994), postwar Southern road building was vital to help a laggard region catch up economically (Schulman, 1994, 158). Good roads had long been considered essential to this goal (Ingram, 2014), and were heartily embraced by Southern representatives.[14] Fast-developing, highway-dependent suburbs such as Cobb County, Georgia grew around Interstates, becoming, in journalist Ronald Brownstein's words, "Newtland," the suburban base of the modern Southern Republican Party (Black and Black, 2002, 6-7). By contrast, non-Southern counties built around rail infrastructure and road infrastructure had already suburbanized to a considerable extent compared to Southern counties in which Interstates were built.[15] This result is also consistent with previous findings that suburbanization led to different overall development patterns inside and outside the Sunbelt (Mieszkowski and Mills,

[14]In 1944 hearings on the Interstate program, for example, Hugh Peterson (D-GA) spoke favorably of the ability of highways to reduce juvenile delinquency, crime, and generally "lift the standard of living" in a community, and spoke favorably of highway construction as an alternative to welfare-state approaches to development, such as education spending.

[15]In 1940, the average population density of non-Southern suburban counties was 229 persons per square mile, versus 123 persons per square mile in the South. Twenty-eight percent of residents of non-Southern suburban counties lived in urbanized areas in 1950, versus 20% of residents of Southern suburban counties. On average, non-Southern suburban counties had 86 manufacturing establishments in 1939 to Southern counties' 21 (Fitch and Ruggles, 2003).

1993). Second, Southern whites transitioned *en masse* into the Republican party during this period, and the most remarkable transition was more consistent income-based voting among Southern whites (Shafer and Johnston, 2006). As highways stimulated growth of wealthy suburbs, a corresponding swing to the Republican party would have occurred in such counties.[16]

The observed decline in the effect of highways on suburban counties over time, both with respect to the years they were built and in the number of years post-treatment in several time periods and regions, can be explained by two phenomena. First, in the South, non-urban whites have, over recent years, become consistently more Republican than whites elsewhere (Gelman et al., 2008), almost amounting to a racial voting bloc, and affluent Southerners have substantially shifted to the Republican party (Shafer and Johnston, 2006), and the South is both racially and politically more segregated than the non-South (Einstein, 2011). The second explanation is related to the inference using geographic units. Over time, highways' effects on one county are likely to extend to neighbors. More non-Interstate roads were built to connect to Interstates. While this diffusion of highways' effects across county lines is a violation of statistical independence, the spillover effects are, if anything, likely to work against the hypothesis of highway-induced suburban Republican voting by biasing effects' magnitude downward, especially in later years as the communities built around new Interstates matured and development spread away from the Interstates. Such spillover effects are especially likely in cases where Interstates fall near a county

[16]Some of the differences may also be due to baseline differences in the matched samples. Counties that could be easily matched in the South were different from units that could be matched outside the South. Coarsened exact matching, which by default entails trimming both treated and untreated units, results in estimates applicable to the subset of units that could be well matched. Units that were well matched in the South were more likely than those elsewhere to lie at the periphery of the 100-kilometer radius used to define metropolitan areas.

boundary. While the effect of Interstates on the difference *between* suburbs with and without Interstates would then appear to decline over time, such a mechanism would still, overall, make suburban counties more Republican.

3 Highways and Urban-Suburban Partisan Sorting

Establishing that Interstates had a measurable, localized effect in suburban counties where they were built, I now turn to the question of whether highways increased the urban-suburban gap over time. Highways' estimated effects on suburbs appears to have declined over time, even as overall urban-suburban polarization increased. In this section, I address this apparent paradox, showing that highways had a global influence on urban-suburban polarization. Using historical data on the density of highway exits, a way to measure metropolitan connectivity, I show that metropolitan areas with higher overall highway density became more polarized than they would have been otherwise.

3.1 Data and Methods

I define metropolitan areas as "couplets" formed by aggregating the urban and suburban counties in each metropolitan area. The urban portion of each couplet consists of counties encompassing the 100 most populous cities in 1950, while the suburban portion consists of an aggregation of other counties with centroids within 100 kilometers of the central city (cities). All urban counties are combined to form the urban portion of the metropolitan area, while all other counties in the 100-kilometer catchment area form the suburban portion. This yields 84 urban-suburban couplets including 100 major cities and their hinterlands. The outcome of interest is the difference in the urban-suburban difference in the Democratic vote share, in each couplet, where ($\bar{D}_{iut}$) and $\bar{D}_{ist}$ represent, respectively, the urban and suburban Democratic vote share in the metro area i in year t, and the difference is represented by

$\Delta_{i,t} = \bar{D}_{iut} - \bar{D}_{ist}$.

While in the previous section counties were defined as treated if they had at least one Interstate highway open, in this section, the "treatment" variable consists of the density of *Interstate highway exits* in each metropolitan area. The reasons for using exit density are two-fold. First, unlike highway length, exits provide a measure of the extent to which Interstates are interconnected with the local street network. Economic development is likely to occur around exits, and a limited-access highway is only a mechanism for residential construction and mobility as much as it allows access to local street networks. Most rural and suburban highways in a metropolitan area may do little to facilitate migration and suburban development because there are no access points. Second, at least on Interstates, the number of exits has been surprisingly stable since the Interstates were built, since highway planners explicitly aimed to limit impediments to traffic brought about by excessive exits.[17] To generate a count of historical exits, the Baum-Snow PR-511 database, based on the Federal Highway Administration's records of the Interstate segments' opening date, was merged with the 2008 ESRI shapefile of Interstate exits as of 2008. Using the count of exits from this combined shapefile, I divided the number of exits open in each 100-kilometer catchment area in each year by the combined land area of each metropolitan area to generate the number of exits per square mile in each year.

Because the explanatory variable is continuous and the sample was too small to permit effective matching, I use least-squares regression alone is used to control for non-random factors determining whether a metropolitan area had higher exit density. Relying on criteria mentioned in *Interregional Highways*, the most important of these was population: more highways and exits were built where pre-

[17]A robustness check using historical atlases shows that few exits were added to Interstates after Interstate construction, even in Sun Belt cities where suburban growth occurred and more exits would have been justified. See Supporting Information, Table A-3.

existing population density was already high. *Metropolitan area population density in 1950* captures that highways were built to serve population centers.[18] The *Proportion of counties on a route of strategic military importance in 1941* accounts for the perceived military importance of each metro area. The *Mean number of manufacturing establishments in 1939* accounts for preexisting industrialization, and is also a predictor of Interstate construction. To anticipate future changes in urban suburban political geography, the models include the *Lagged urban-suburban difference in the Democratic presidential vote share* in 1948, 1952, and 1956 (all pre-treatment years). Race's role as one of the strongest correlates of urban-suburban partisan polarization, and its role as a factor in white flight potentially associated with partisan change, is represented by the *Urban percentage non-white minus the suburban percentage non-white in 1950* and the *Mean urban and suburban percentage non-white in 1950.* A dummy variable for the *South* accounts for preexisting regional differences in infrastructure and accounts for political pre-trends leading into the post-Civil Rights realignment. Finally, a metropolitan area's economic prosperity at baseline is accounted for using the *Mean of median family income of counties across the metropolitan area*.[19]

The effect of the highway density variable and its log-transformed version (which implies diminishing marginal impact of increasing highway density) is estimated by least-squares regression:

$$\mathbf{\Delta_t} = \beta_0 + \beta_{z_t}\mathbf{z_t} + \beta_2\mathbf{x}_1 + \ldots + \beta_k\mathbf{x}_k + \boldsymbol{\epsilon} \quad (2)$$

where β_{z_t} represents the effect of a one Interstate-mile-per-square-mile (or the natural logarithm of the

[18]Because metropolitan areas are approximately the same area, overall population density is highly correlated with population itself, making a separate population variable unnecessary.

[19]Within-couplet urban-suburban differences are calculated using population-weighted (for Census variables) or total-voter-weighted (for election variables) means. The within-couplet means are calculated by taking the unweighted mean of the weighted urban and suburban averages, in each couplet.

Interstate mileage per square-mile) difference in exit density at year $t-4$ and $\mathbf{x}_1, \ldots, \mathbf{x}_k$ are included controls. The analysis otherwise follows the same bootstrapping and smoothing procedure discussed in Section 2. One thousand samples were drawn, and β_{z_t} was estimated on each, for each election year t.[20] The bootstrapped point estimates were smoothed by the same lowess-smoothing procedure, yielding a 1000-by-13 matrix of smoothed point estimates. The mean and the relevant quantiles of these data were used to construct 80% and 95% confidence intervals.[21]

3.2 Results

Figure 4 plots predicted first-differences in the urban-suburban Democratic voting gap associated with a typical increase in Interstate exit density (here, a shift from the 25th to 75th percentile, using the 1996 sample quantiles to permit comparability). For display purposes, the estimated first-difference permits a comparison of values across a substantively meaningful and easily compared range (here, the difference in exit density between the 25th and 75th percentile in the year 1996). The left panel displays this effect for the untransformed exit-density variable, the right panel for its log-transformed version. These results show that higher Interstate mileage in a metropolitan area is associated with an increase in urban-suburban polarization in the presidential vote. Point estimates are uniformly positive under both versions of the Interstate-density variable beginning in 1972. In the log-transformed case, 80% confidence intervals permit rejection of the null by 1992, and 95% confidence intervals yield results

[20] A small positive value, 10^{-4}, was added to each value before the logarithmic transformation to permit calculation of the logarithmic transformation. This transformation was only relevant in a few early years of the Interstate program during which a few metropolitan areas had yet to undergo new highway construction.

[21] Region-specific effects were estimated but were insufficiently powered due to limited sample size. The magnitude of these point estimates was larger in the South than elsewhere.

close to significance across the same period.

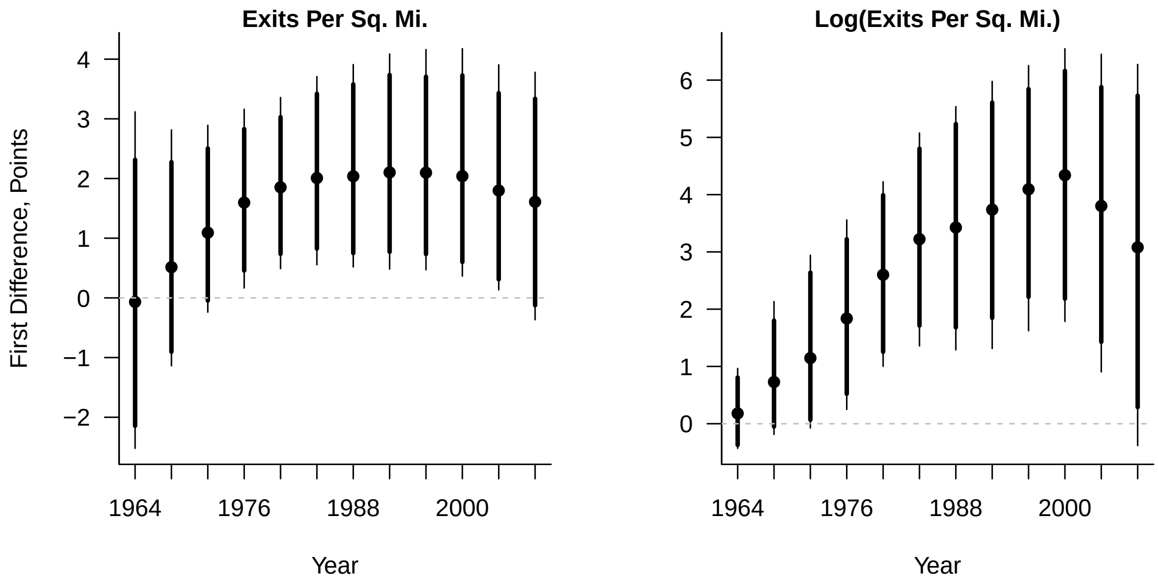

Figure 4: Interstates' predicted effect on the urban-suburban Democratic voting gap. Difference across the interquartile range of highways per square mile in 1996. Left: exit density. Right: log-transformed highway density. Bootstrapped 80% and 95% confidence intervals accompany the estimates.

These effects are substantively comparable to other observed effects of distributive politics on spending. The difference in urban-suburban polarization associated with an increase in exit density across the interquartile range is about one-fourth as large as the average urban-suburban gap during the study period. The effects are similar in magnitude to other observational estimates on the effects of public policies on aggregate-level voting in presidential and congressional elections. For example, Levitt and Snyder (1995) find that an increase in federal non-transfer spending of $100 per capita, "approximately $50 million" per house district, boosts incumbent House member vote share by about 2 percentage points. Each presidential disaster declaration adopted in a state increases the incumbent president's vote share by 1 point (Reeves, 2011, 1150). The magnitude of these effects is notable because Interstates' effects appear to be long-term, and have persisted years after the initial expenditure. Infrastructure may "lock in" long-term effects in a way that other public policies do not.

4 Individual-Level Mechanisms

Thus far, all results presented have been aggregate geographic outcomes. These aggregate results allude to a causal mechanism that is some combination of partisan conversion, especially in the South during the post-Civil-Rights-Era realignment, and partisan sorting, in which Republicans (or the predominantly white voters likely to become Republican) moved to suburban areas along Interstates. The geographic results reported up to this point can only suggest the underlying mechanisms. While a representative sample of all voters is not available for this period, the Youth-Parent Socialization Panel Study (YPSPS) tracks a sample of $n = 935$ Class of 1965 high school students, and followed up with respondents in 1973, 1982, and 1997 (Stoker, Jennings and Bowers, 2009; Jennings et al., 2005). The study captures the life course of Americans who came of age and were making residential decisions right after most Interstates had been built. Along with extensive data on political attitudes and partisan identification over the life course, the study collected respondent addresses, and the restricted-use data provide zip codes in 1965 (for the high school address), 1982, and 1997. I merged these data with a shapefile of 2004 zip code polygons, which closely approximate zip code boundaries to which respondents were assigned.[22] These data were then spatially joined with the PR-511 data in ArcGIS to calculate each zip code's proximity to the nearest Interstate at each year, and the proximity to the nearest city listed among the top 100 cities as of 1950. From these data, I categorize self-identified Democrats and Republicans

[22]While the particular zip code database used in restricted-use database is not documented, checking the data against historical zip code directories from 1967 and 1982 indicates that the zip codes used were from late in the study period, justifying the use of 2004 zip code boundary files.

(including independent leaners) as of each year according to place of residence: urban (within 10 miles of one of the 100 largest cities as of 1950), more than 10 miles from such a city and within 10 miles of an Interstate, or more than 10 miles from both a major city and Interstate. The proportion of respondents in each of these categories was calculated separately for who graduated from both urban and non-urban high schools, for self-identified partisans, for each of the three panel waves.

Figure 5 displays the changing geographic location of Democrats and Republicans over the study period, revealing a mass migration of Republicans into zip codes along Interstates. The top row of the figure shows the changing geographic distribution of members of the 1965 high school class who started in non-urban high schools. In this group, Republican identifiers in this group were more likely, over time, to live in zip code along non-urban Interstates, and Democratic identifiers were slightly more likely to move to cities. The difference is greater among graduates of urban high schools. By 1997, almost half of the Republicans self-identifying in that year lived along non-urban Interstates, compared to slightly more than 30% of Democrats. By 1997, Democratic urban-high-school graduates were 25 percentage points more likely than the Republicans to still live near a central city. These results are consistent with the geographic mechanisms hypothesized earlier: Republicans moved into suburban and rural areas along Interstates at a much higher rate than Democrats who started out in similar geographic circumstances.[23]

A regression model offers insights into the demographic explanations for Republicans' sorting into freeway-based suburbs. Table 1 presents descriptive least squares regression models of 1965 high school graduates' probability of living in a non-urban zip code within 10 miles of an Interstate highway in 1997, as a function of racial, class, and economic variables. The include a dummy variable for *White* race, the respondent's *Father's occupational prestige score* (on a ten-point scale), a three-category *Family income in 1973* variable (low (base category), middle, and high), and and a three-category *Party identification*

[23]This analysis is repeated for the South and non-South in the Supporting Information, Figure A-24.

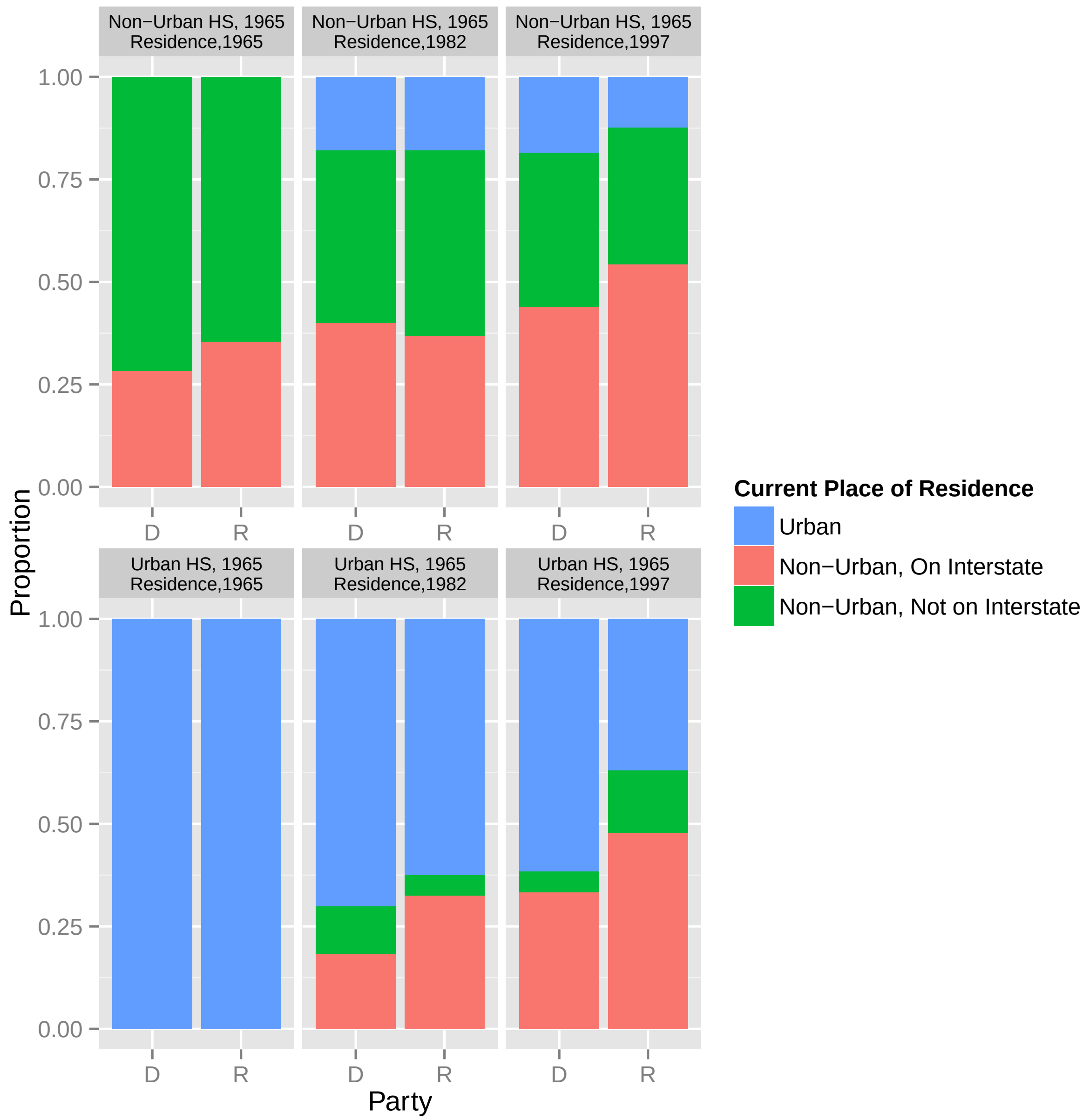

Figure 5: Barplot of distribution of residential type of place, by party and urban/non-urban location of respondent's high school in 1965. The partisan identification variable is allowed to vary with each time period.

variable (Democrat (base category), Independent, and Republican). All three models convey that race is a key predictor of the move from the city to the suburbs. Urban whites in the Class of 1965 were anywhere from 24 to 26 points more likely than non-whites to move to zip codes near rural or suburban Interstates, a result that holds up whether or not party identification as of 1973 is included in the model. Among all respondents, the coefficient on Republican (versus Democratic) party identification is in the expected direction, but not distinguishable from zero at the $\alpha = 0.05$ level. Neither income nor party appears as a significant predictor in Model 3, which features only white respondents. Models 4 through 6 present the same models, but for graduates from a high school that was at least 10 miles from a major city center. This sample is more homogeneously white. Race is not a significant predictor, but the three models show that communities around non-urban Interstates have attracted and retained the middle class and wealthy. After controlling for other factors, those with high incomes in 1973 were 22 to 24 points more likely than the poor, and those with middle incomes 16 to 17 points more likely, to live along non-urban Interstates. Controlling for white race, income, and class background, Independents and Republicans are about 11 to 12 points more likely than Democrats to live along non-urban Interstates, though these estimates are not uniformly significant at the $\alpha = 0.05$ level.

While not providing definitive causal effects, these results suggest two socioeconomic mechanisms by which Interstates shape migration and partisan geographic change. Those who started adult life outside of cities were, on average, less racially diverse, but were more likely to segregate according to income, with the poor either more likely to live in rural areas away from Interstates or migrating into cities. By contrast, race is a dominant factor predicting movement in and out of cities. These findings are consistent with aggregate-level analyses using Census economic and racial data, which show that Interstates contributed consistently to urban-suburban racial segregation, but less clear effects socioeconomic differences. Analysis of suburban county data found that Interstates made suburban counties wealthier, but had no discernable effect on suburban counties' racial composition. (See Supporting Information, Figures A-16 and A-19.)

Table 1: Least Squares Regression of Socioeconomic, Racial, and Political Predictors of Migration to Interstate-Highway Suburbs

	Urban H.S. Grads (1965)			Non-Urban H.S. Grads (1965)		
	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6
(Intercept)	0.22	0.20	0.49*	0.32*	0.26*	0.16
	(0.17)	(0.17)	(0.21)	(0.12)	(0.12)	(0.09)
White	0.26*	0.24*		−0.11	−0.15	
	(0.12)	(0.12)		(0.10)	(0.10)	
Father's Prestige	−0.01	−0.01	−0.01	0.02*	0.02*	0.01
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Middle-Income (1973)	−0.05	−0.06	−0.07	0.16*	0.17*	0.16*
	(0.12)	(0.12)	(0.15)	(0.07)	(0.07)	(0.07)
High-Income (1973)	0.01	−0.00	0.00	0.22*	0.23*	0.24*
	(0.13)	(0.13)	(0.16)	(0.08)	(0.08)	(0.08)
Party: Independent		0.08	0.08		0.12*	0.12
		(0.08)	(0.09)		(0.06)	(0.06)
Party: Republican		0.15	0.11		0.11	0.11
		(0.12)	(0.13)		(0.07)	(0.07)
N	175	175	152	383	382	355

Standard errors in parentheses

* indicates significance at $p < 0.05$

Individual results also suggest that differences in migration behavior alone are not responsible for the larger effects observed in the South. Migration from urban core to periphery does not vary dramatically across regions. By 1997, Republicans who had graduated from Southern non-urban high schools were as likely as Southern Democrats to live along a non-urban Interstate. Republican alumni of Southern urban high schools were 10 points more likely to live along non-urban Interstates than Southern Democrats from urban high schools, not much different than outside the South. Few substantive interregional differences in partisan urban-suburban migration can be detected from the YPSPS data. Even in the South there was substantial white flight from cities, and this flight culminated in more Republicans living in suburbs built around Interstates (Kruse, 2005). The large effect observed in Southern suburbs may, therefore, be a result of the interaction of both high migration rates and Southern whites' more substantial shift to Republican party identification relative to their non-Southern counterparts.

5 Discussion and Conclusion

For the past half century, Democrats and Republicans have organized themselves on an urban-to-suburban continuum. This article has demonstrated that American transportation policy has been central to these changes. Interstate highways shaped numerous aspects of the geography of American metropolitan areas, including their politics. Like any national policy intervention, highways interacted with different on-the-ground conditions. Their effects on politics were contingent on both the baseline partisan geography where they were built and pre-existing sorting trends. Built at a point of rapid social and economic change during the postwar era, Interstates facilitated ongoing suburbanization and white flight, contributing to a larger urban-suburban split where they were built. Unsurprisingly, highways had a strong effect in the South, where counties with Interstates became 5 points less Democratic than they would have been otherwise. Over time, differences across suburbia faded, even as the urban-suburban gap increased.

Metro areas with Interstate density at the 75th percentile became, over time, four points more polarized than if their highway density had been at the 25th percentile (using 1996 values). These findings hold even after accounting for baseline population density, racial polarization, political polarization, and political confounding variables. A descriptive individual-level analysis provides insights into the mechanisms underlying these changes. Among individuals who started adulthood in non-urban zip codes, income and class are the dominant factor in residential migration. Among those who began in urban zip codes, white flight seems to be a major factor driving migration into outlying neighborhoods along Interstates.

Given individual-level findings, one may ask if the findings I present are merely a second-order effect of racial and economic segregation. Clearly, the partisan geography observed in this analysis is not primarily a result of preferences for the partisanship of one's neighbors, but to create partisan segregation the sorting need occur only on correlates of partisanship. Highways enabled an automobile-centered lifestyle among individuals who could afford to (and were permitted to) live in suburbs. Studies of household transportation uses show that the poor are much more likely to rely on walking and transit (Pucher and Renne, 2003) and this is believed to contribute to poor Americans' preference to remain in cities (Glaeser, Kahn and Rappaport, 2007). As Interstate suburbs became wealthier, and also were more likely to have higher rates of solo commuting to work outside county from 1970 to 2000, than comparable non-Interstate suburbs (Figure A-16).

One might also ask why this article has not attempted to isolate the contextual effect of highways on individual attitudes and behavior. Much research on suburbanization and suburban politics aims to estimate exogenous contextual or neighborhood effects. This study is deliberately agnostic on the question, because individual-level contextual effects need not translate into different political geography, and political geography need not induce contextual effects to be meaningful. One might, of course, expect that the geographic polarization described here leads to a political "echo chamber" leading to more extreme opinions, as lab-based studies have suggested (Mutz, 2006; Sunstein, 2007). While evidence suggests that one's literal neighbors may not act as an "echo chamber" (Abrams and Fiorina, 2012), the types of political discussion that occur in a community at large, and issues that appear on a local agenda, are almost certainly be tied to the partisan composition of the community. Such a question is outside the scope of this study, but is worthy of future research.

The findings here suggest that scholarship on policy effects and "policy feedback" should account more seriously for geographic mechanisms. Most scholarship on public policy's political effects has focused on policies' feedback effects as operating primarily through individual-level behavioral effects (Campbell, 2003; Mettler, 2002; Soss, 2000). Spatial policy feedback mechanisms, however, can change politics not just by changing individual attitudes and behavior, but by changing individuals' location and producing different geographic aggregations with different preferences. One of these potential mechanisms is different, geographically defined political economies (Costa and Kahn, 2010). For example, low-density Republican areas are almost wholly dependent on private automobiles, while only very high-density, predominantly Democratic areas use public transit to any meaningful degree (Pucher and Renne, 2003). Republicans' preference for sprawl-like neighborhoods is consistent with divergent consumption preferences and issue attitudes (Belden, Russonello & Stewart, 2011). A potential implication is that partisan issue sorting (Levendusky, 2009) combined with geographic sorting of Democrats and Republicans, should lead to a stronger linkage between spatial location, partisanship, and issue attitudes. Thus, changes in political geography can change politics even if they have no "contextual effect" on individual behavior.

While numerous public policies have influenced the growth of suburbia and the political geography of the two parties, Interstate highways have been central to the development of geographic polarization. Interstate highways enabled mobile residents to express residential preferences linked to their partisanship. An unintended consequence for American politics was the growth of suburban Republican enclaves where highways were built, and a greater partisan gap between cities and their periphery.

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Supporting Information

A County-Level Analysis

A.1 Summary Statistics for Full Suburban-County Sample

A.2 Defining the Suburban County Sample

This section addresses one of the basic concerns that arises in the study of American metropolitan areas: the potential sensitivity of findings to rule-based sample definitions. The results presented in the body of the paper define suburban counties by including only counties more than 20 kilometers from the central city and less than 100 kilometers from the central city. Findings are robust to both selection of the inner and outer radius and to a sampling frame based on population density. For purposes of brevity, these sensitivity analyses were conducted using a simplified version of the dummy variable for presence of an Interstate in a county, coded 1 if an Interstate had opened in the county at least four years earlier.

First, findings are robust to selection of inner and outer radii. Results for the full suburban county sample, excluding pre-1956 freeway adopters and applying coarsened exact matching, appear in Figures A-1 and A-2. To begin, the full sample, excluding early-adopter states, is subsampled using a grid-search procedure. This grid search covers three different values of the inner radius (10, 20, and 30 kilometers) and six values of outer radius (40, 50, 60, 70, 80, 90, and 100 kilometers). This yields a total of 21 different subsamples. On each of these samples, coarsened exact matching and least squares regression are used to estimate the average treatment effect of highways on the suburban county vote, following the procedure described in the text. For all samples with an outer radius greater than 50 kilometers, the results corroborate those presented in the main text.

Figure A-3 displays a similar grid search over sample definition rules. Instead of defining the suburban sample for each metropolitan county in terms of radius, it defines the sample in terms of county

Variable	Mean	SD	Min.	Max.
Treatment Variables				
Earliest Interstate Opening, (Treated Counties)	1965	6.717	1942	1992
Interstate Highway Built At Any Time	0.554	0.497	0	1
Covariates				
Republican Presidential Vote, 1948	0.413	0.192	0.006	0.841
Republican Presidential Vote, 1952	0.548	0.152	0.081	0.930
Republican Presidential Vote, 1956	0.556	0.154	0.095	0.926
Median Family Income, $, 1949 (9-category)	5.16	1.524	2	9
Proportion Non-White, 1950	0.110	0.164	0	0.818
Log(Persons Per Acre, 1950)	-2.54	0.872	-7.26	0.2825
Proportion in Urban Residence, 1950 (10-category)	2.60	2.231	0	9
Crop Value Per Capita, 1950 ($)	112	133	0	1619
Mfg. Establishments, 1939	36	51	0	457
On Strategic Route, 1941	0.657	0.475	0	1
Proportion Non-Resident One Year Earlier, 1950	0.013	0.13	0.000	0.192
Region=South	0.42	0.49	0	1

Table A-1: Summary statistics for suburban county sample ($n = 988$).

population density. Each sample is defined by selecting different minimum and maximum county population densities, using sample quantiles. This grid search applies the same matched sampling and regression methods to samples defined by shrinking the density window progressively. Samples that include low-population-density counties yield results closer to those obtained in the published analysis.

A.3 Simple Difference in Means

One may be concerned that the estimates presented here are a result of specification hunting. One way to allay these concerns is to present simple difference-in-means estimates for each of the analyses. Figure A-4 presents the difference in the mean Democratic vote (in percentage points) for counties with and without Interstates at least four years earlier. A simple scatterplot of urban-suburban polarization against exits per square mile as of each year is presented in Figure A-5. In both cases, the difference in means is in the expected direction.

A.4 Robustness of Findings to Inclusion of Lagged Presidential Vote as a Matching Variable

In this Figure A-6, I present results analogous to Figure 3, but excluding the lagged presidential vote from the first year for each of the three treatment cohorts (i.e., 1956, 1964, and 1972). Such a lagged variable was included to reduce biases associated with varying secular trends over time. Since key decisions about where to locate Interstate highways were made years earlier, a possible concern is that controlling for these later election results introduces the potential for a version of post-treatment bias, even though outcomes are not measured for 8 years after the included presidential results. To address this concern, I present results for models that are identical to those in the text except for their exclusion of the lagged Democratic presidential vote proportion.

A.5 Balance Checks

In this section, I present results of balance checks used to generate the matched samples, for each region, Interstate highway construction cohort, and year, presented in Figure 3. As discussed in the text, to generate the largest possible matched data sets while preserving balance, the Interstate treatment in each year was matched against all counties in which an Interstate had not yet been built, for each election year following the treatment cohort in question. For example, when creating a matched samples for the 1956-1963 treatment cohort, the matched control group for estimating Interstates' effects was assembled from counties in which an Interstate had not been opened as of 1964, while the matched sample for year t was assembled from the observations that had yet to be treated in year t. This means that the size and composition of the matched samples for each treatment cohort varies slightly over time. The matched samples are typically smaller in later election years, when the most comparable control units have, themselves, had Interstates open.

Figures A-7 through A-15 present the standardized imbalance in the full ("F") and matched ("M") samples, for each variable included in the matching exercise, for both pre-treatment and post-treatment election years. These results find that imbalance between treatment and control counties is substantially reduced by the coarsened exact matching procedure in almost all cohorts, regions, and years, though not completely eliminated. In cases where imbalance is not completely eliminated, the results depend more on the adequacy of the linear regression model (Ho et al., 2007).

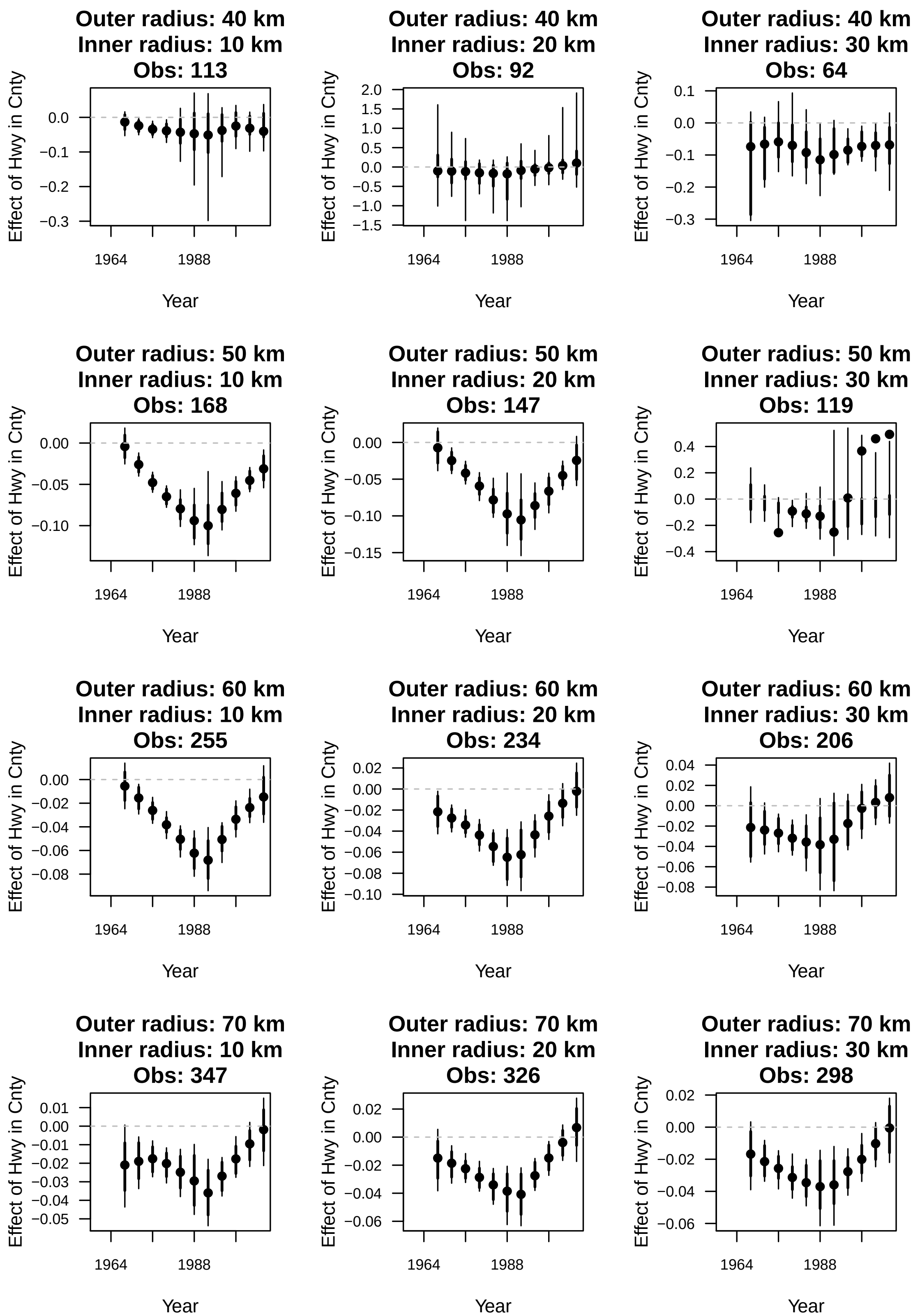

Figure A-1: Robustness of findings to definition of a suburban county sample based on the inner and outer radius defining rings drawn around the metropolitan central city. Outer radii of 40 to 70 kilometers.

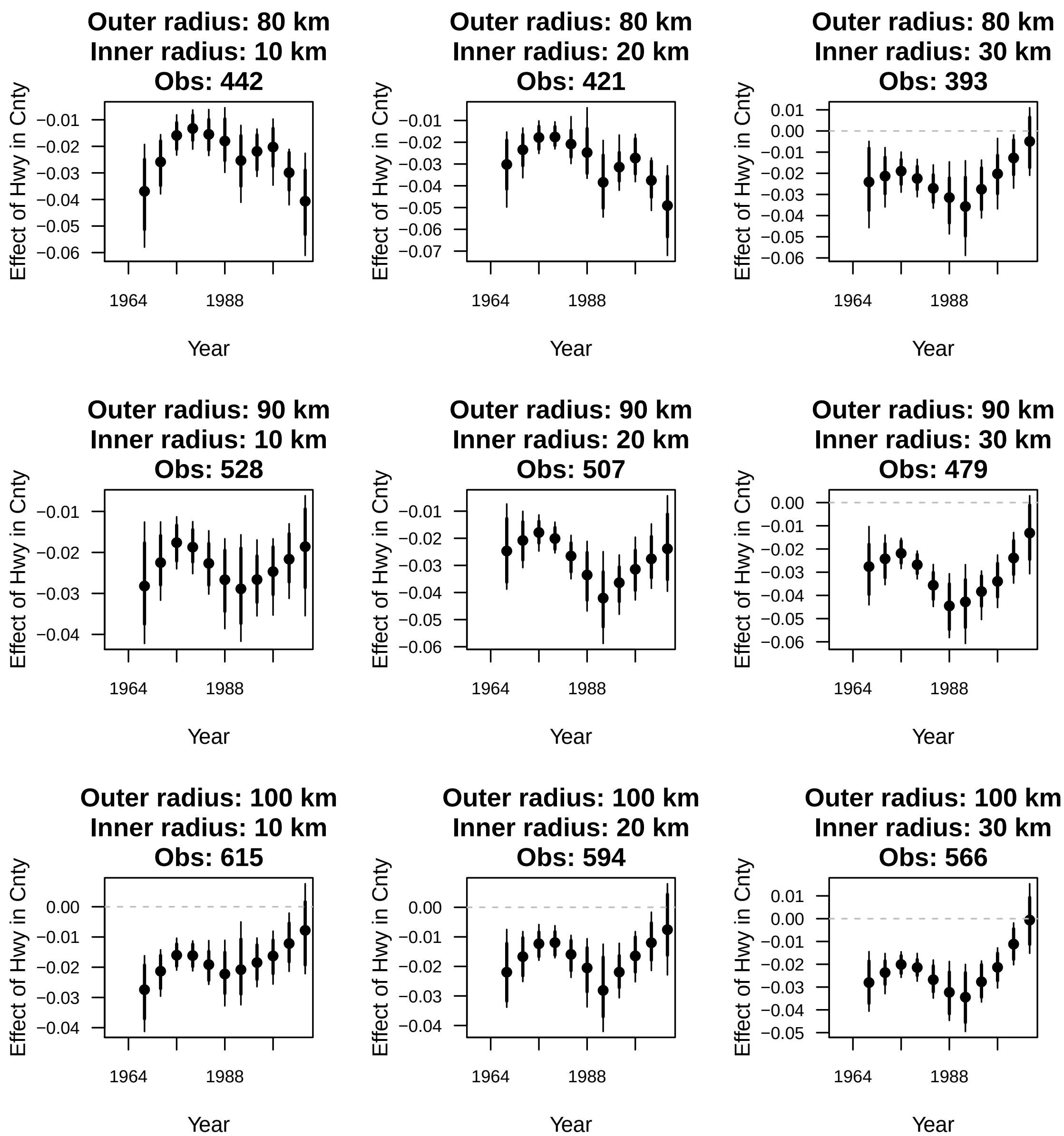

Figure A-2: Robustness of findings to definition of a suburban county sample based on the inner and outer radius defining rings drawn around the metropolitan central city. Outer radii of 80 to 100 kilometers.

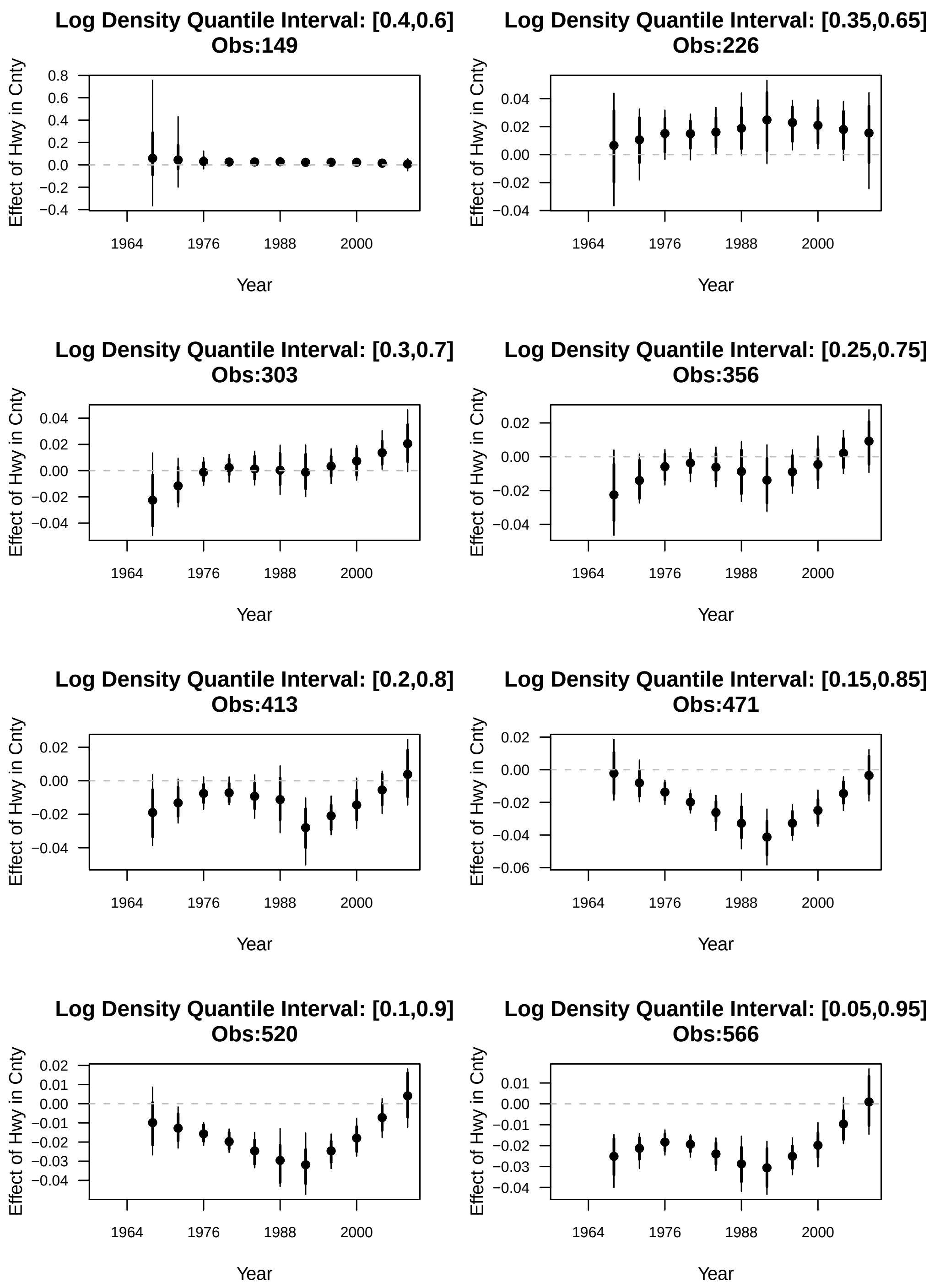

Figure A-3: Robustness of findings to definition of suburban county sample by population density.

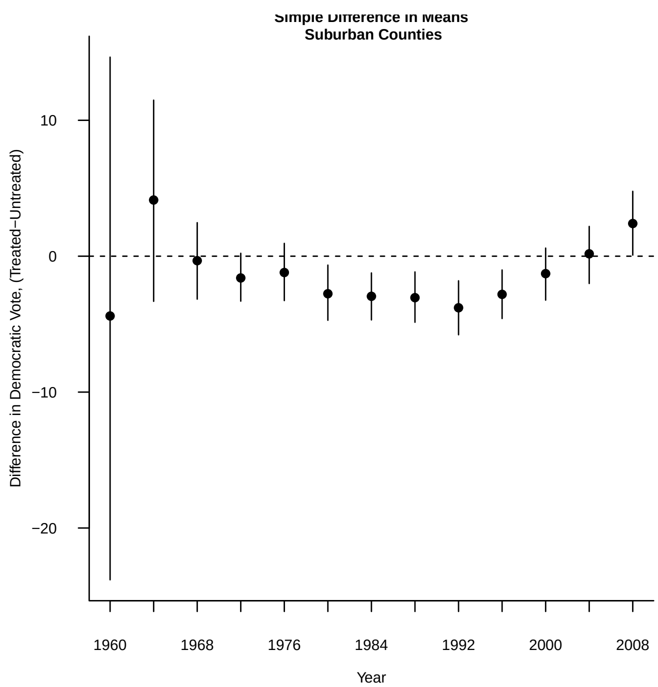

Figure A-4: Difference in mean Democratic vote share between counties with and without an Interstate at year $t-4$. Counties in early-adopter states excluded.

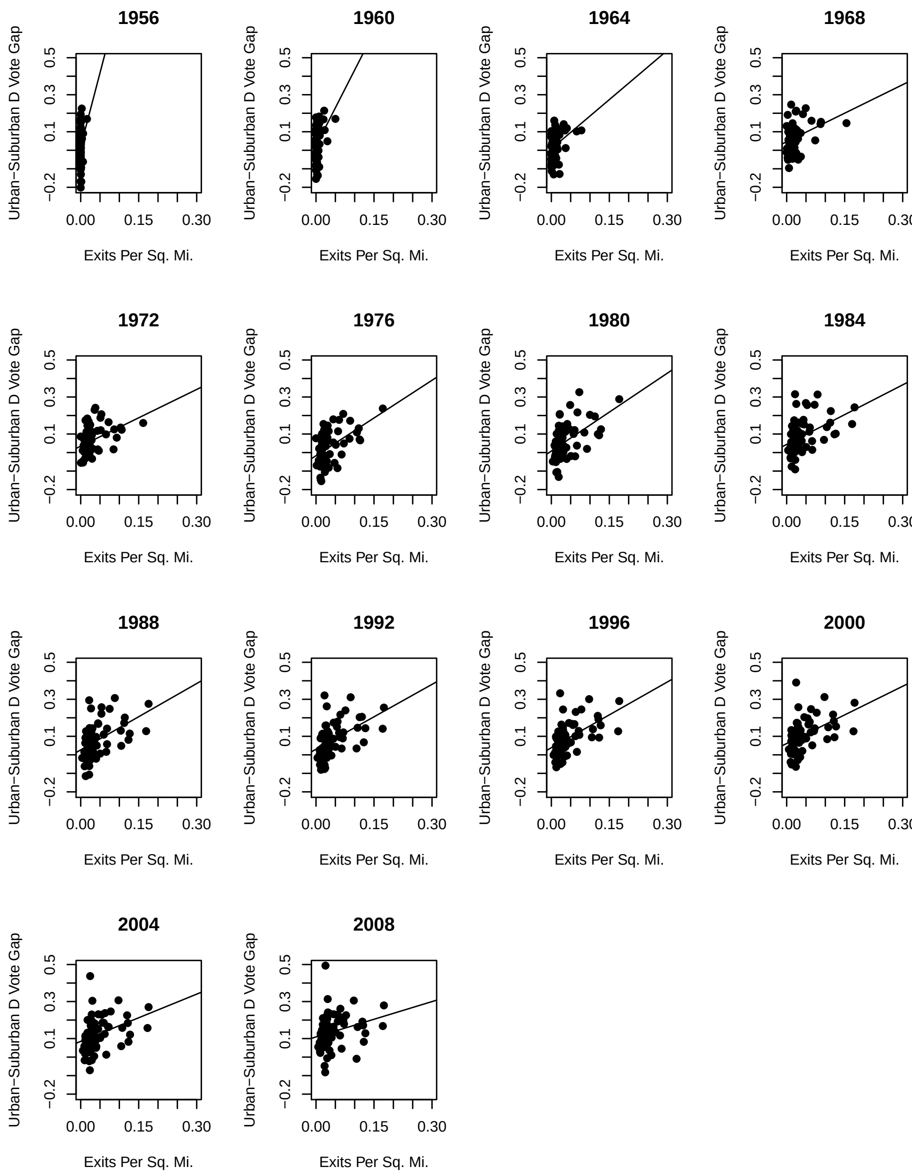

Figure A-5: Simple bivariate plots of the urban-suburban Democratic voting gap, by year, against metro-area exits per square mile.

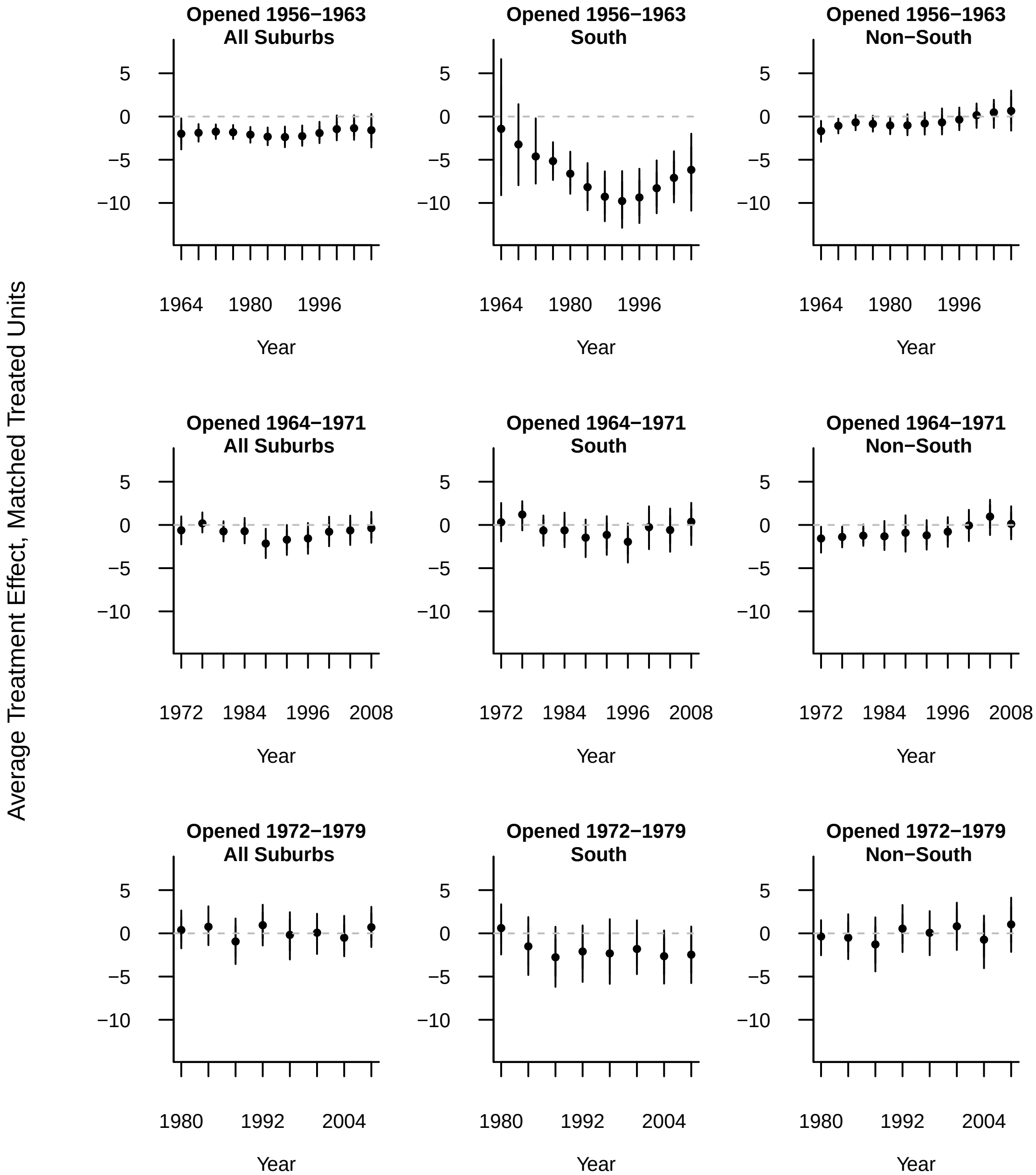

Figure A-6: OLS estimates of effect of Interstate in a county, using CEM-matched samples. Interstate highways contributed to a decline in the suburban Democratic vote, mostly early on and in the South, omitting Democratic vote at baseline. 80% (thick) and 95% (thin) confidence intervals accompany each estimate. Top row: Interstates opened 1956-1963. Middle row: Interstates opened 1964-1971. Bottom row: Interstates opened 1972-1979.

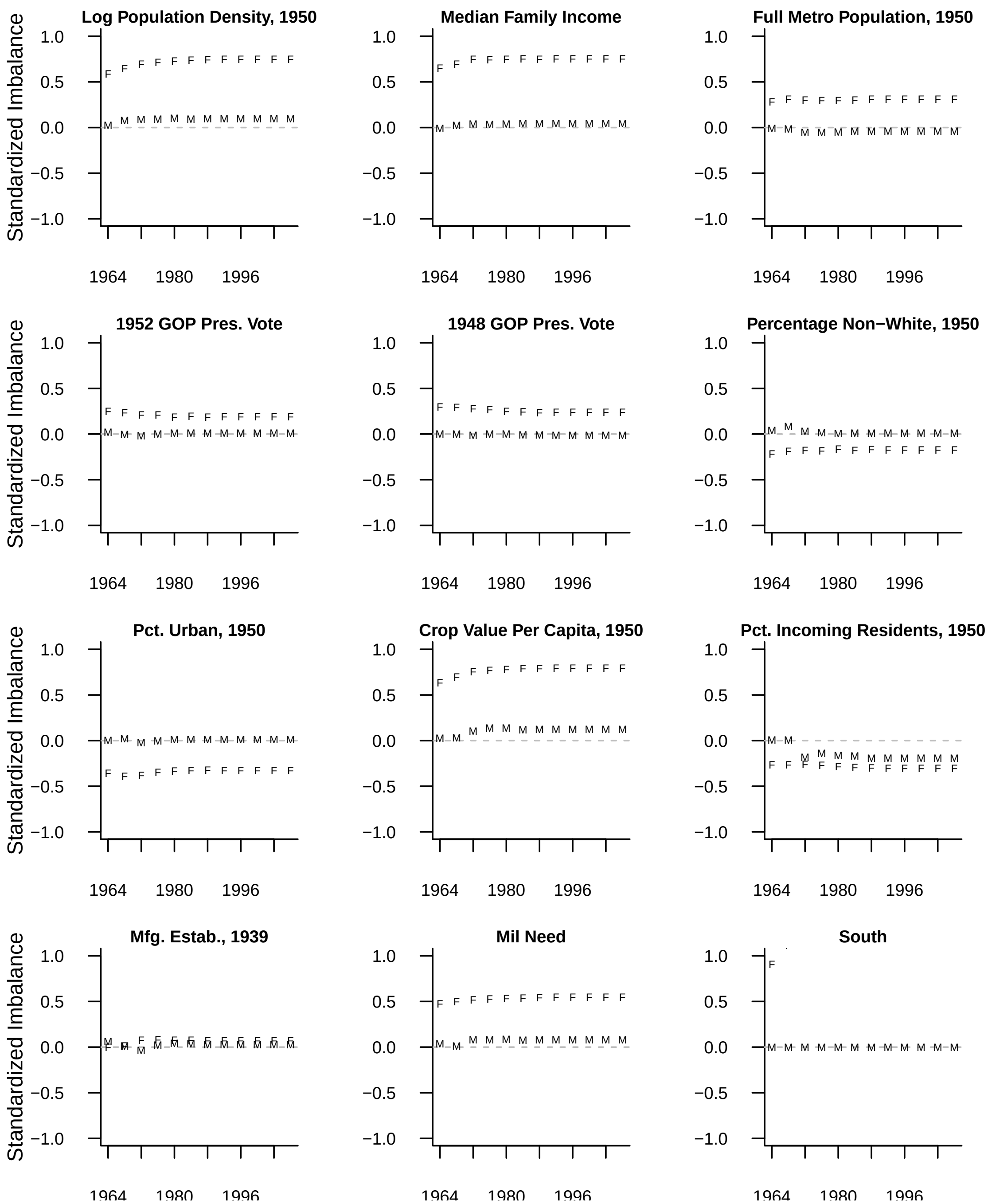

Figure A-7: Standardized imbalance, 1956-1963 treatment cohort, all suburban counties. Difference in means for the treated and control group, divided by the standard deviation in the unmatched control group, for the [F]ull available sample and the [M]atched sample.

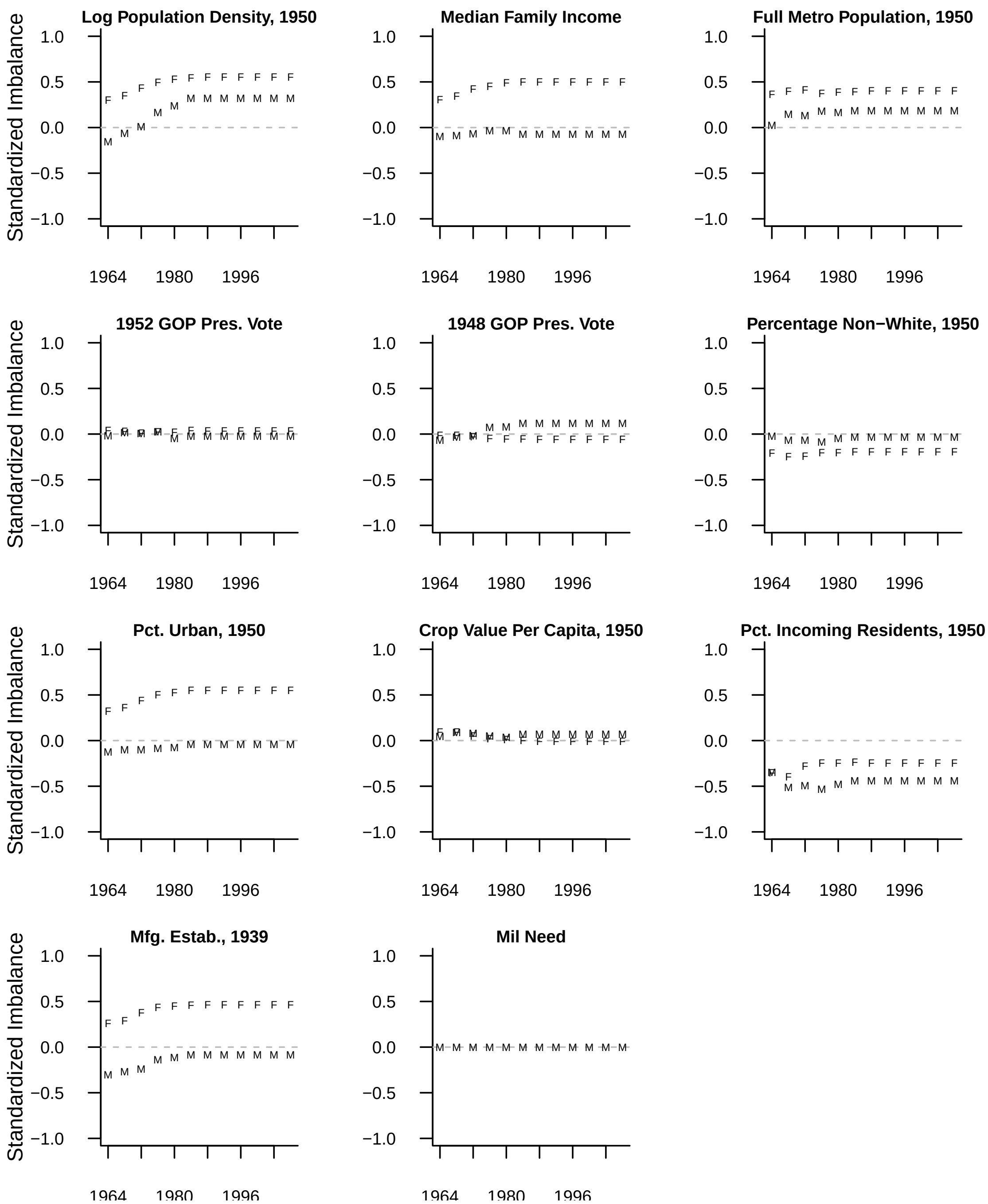

Figure A-8: Standardized imbalance, 1956-1963 treatment cohort, Southern suburban counties. Difference in means for the treated and control group, divided by the standard deviation in the unmatched control group, for the [F]ull available sample and the [M]atched sample.

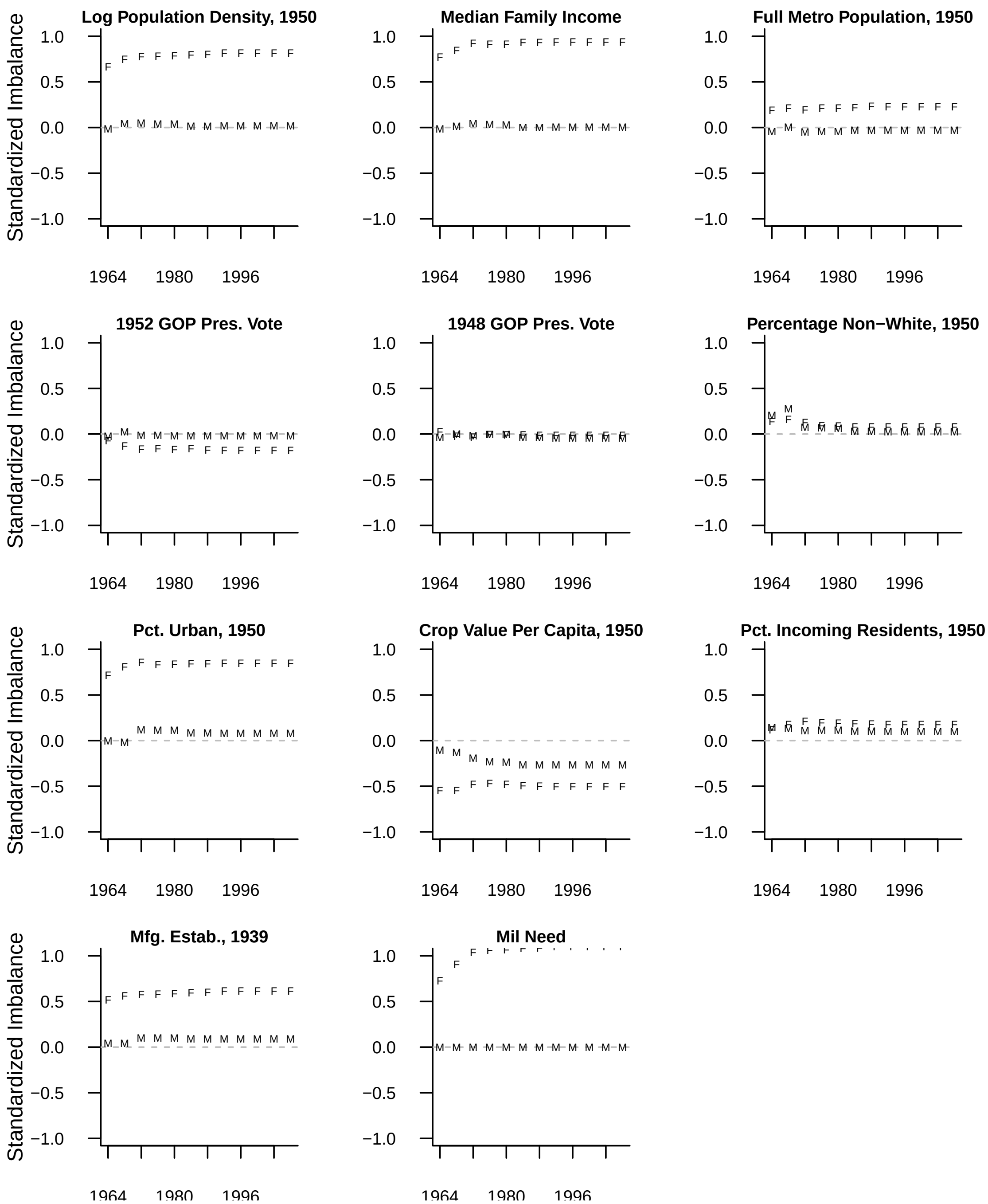

Figure A-9: Standardized imbalance, 1956-1963 treatment cohort, Non-Southern suburban counties. Difference in means for the treated and control group, divided by the standard deviation in the unmatched control group, for the [F]ull available sample and the [M]atched sample.

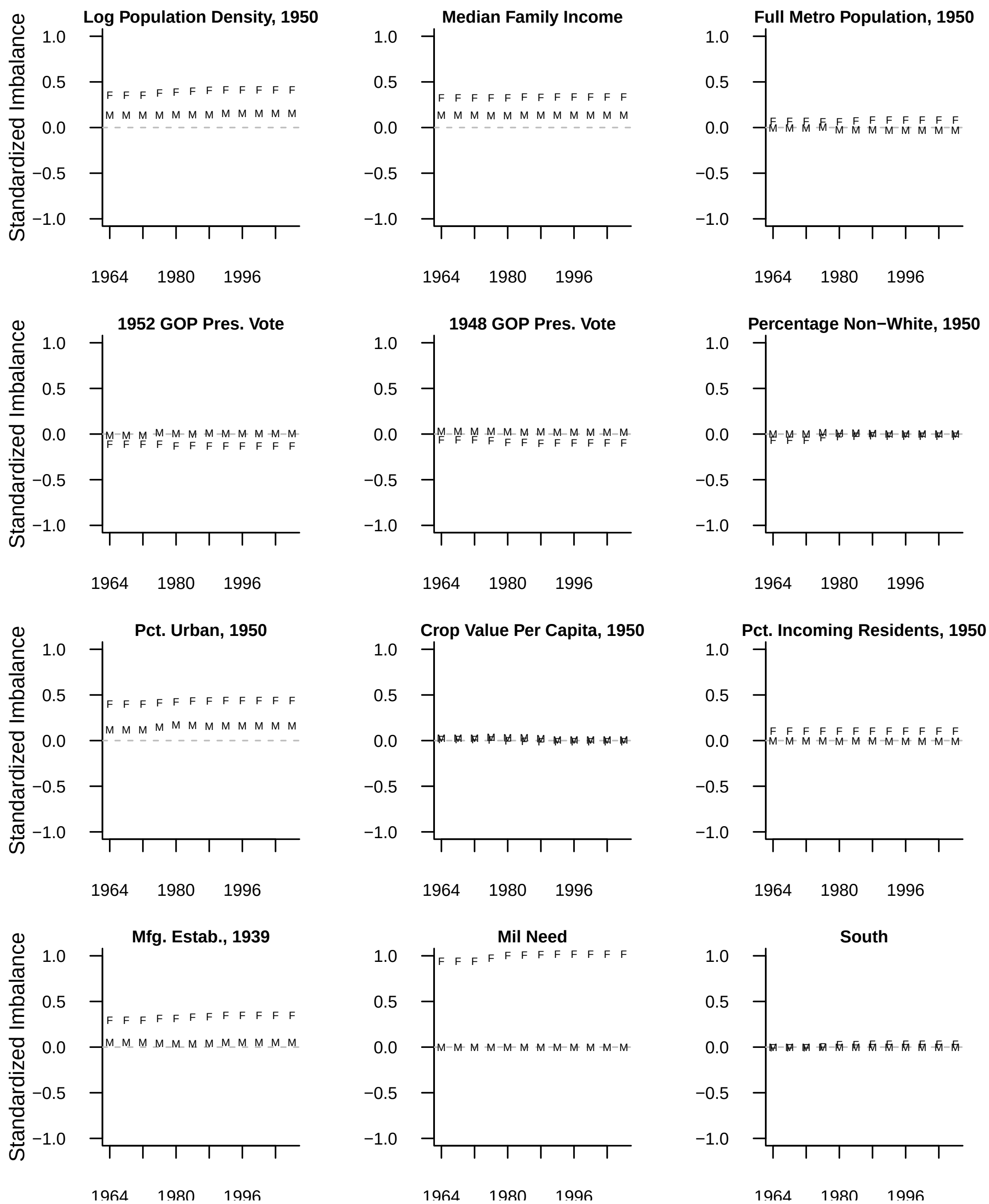

Figure A-10: Standardized imbalance, 1964-1971 treatment cohort, all suburban counties. Difference in means for the treated and control group, divided by the standard deviation in the unmatched control group, for the [F]ull available sample and the [M]atched sample.

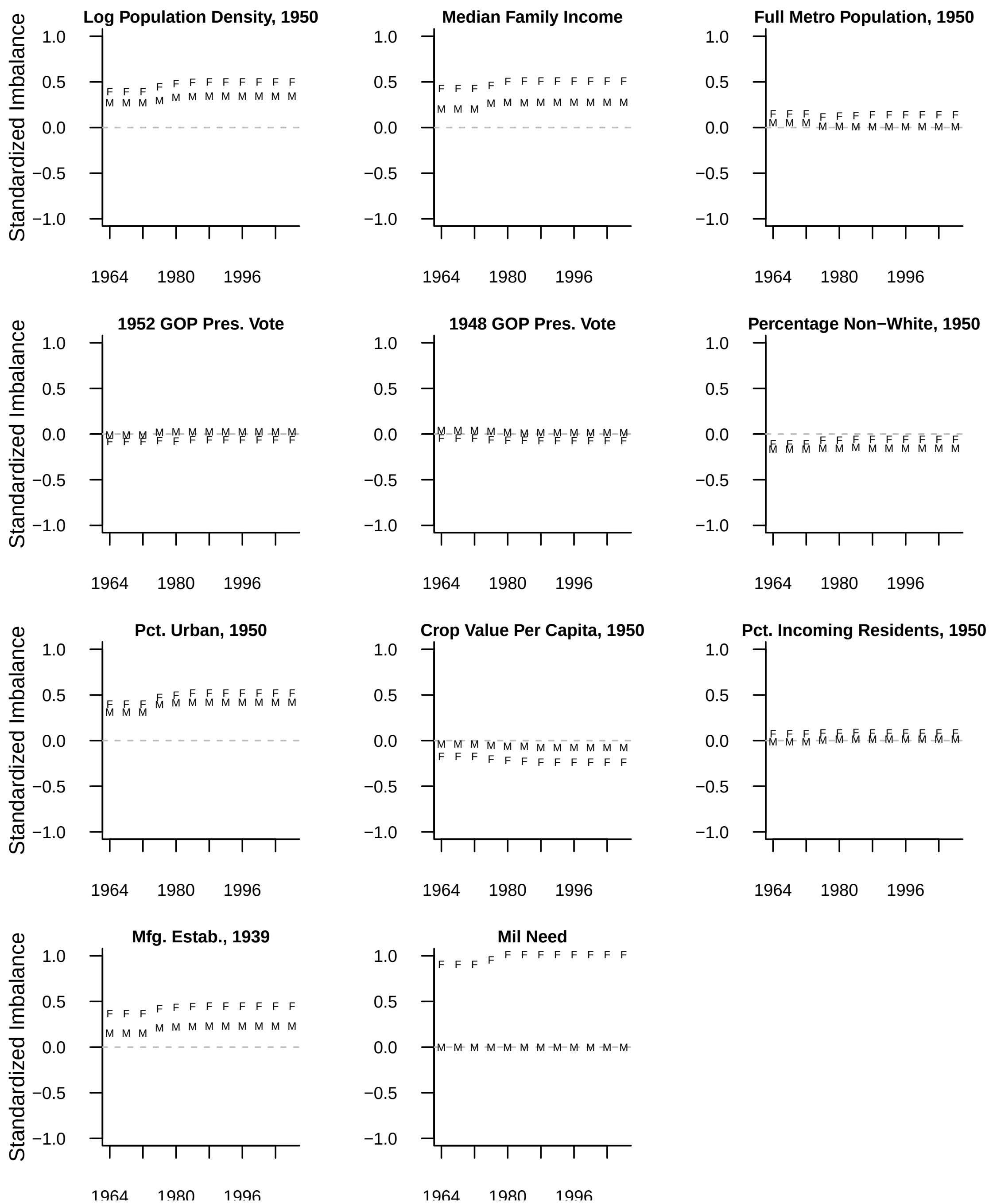

Figure A-11: Standardized imbalance, 1964-1971 treatment cohort, Southern suburban counties. Difference in means for the treated and control group, divided by the standard deviation in the unmatched control group, for the [F]ull available sample and the [M]atched sample.

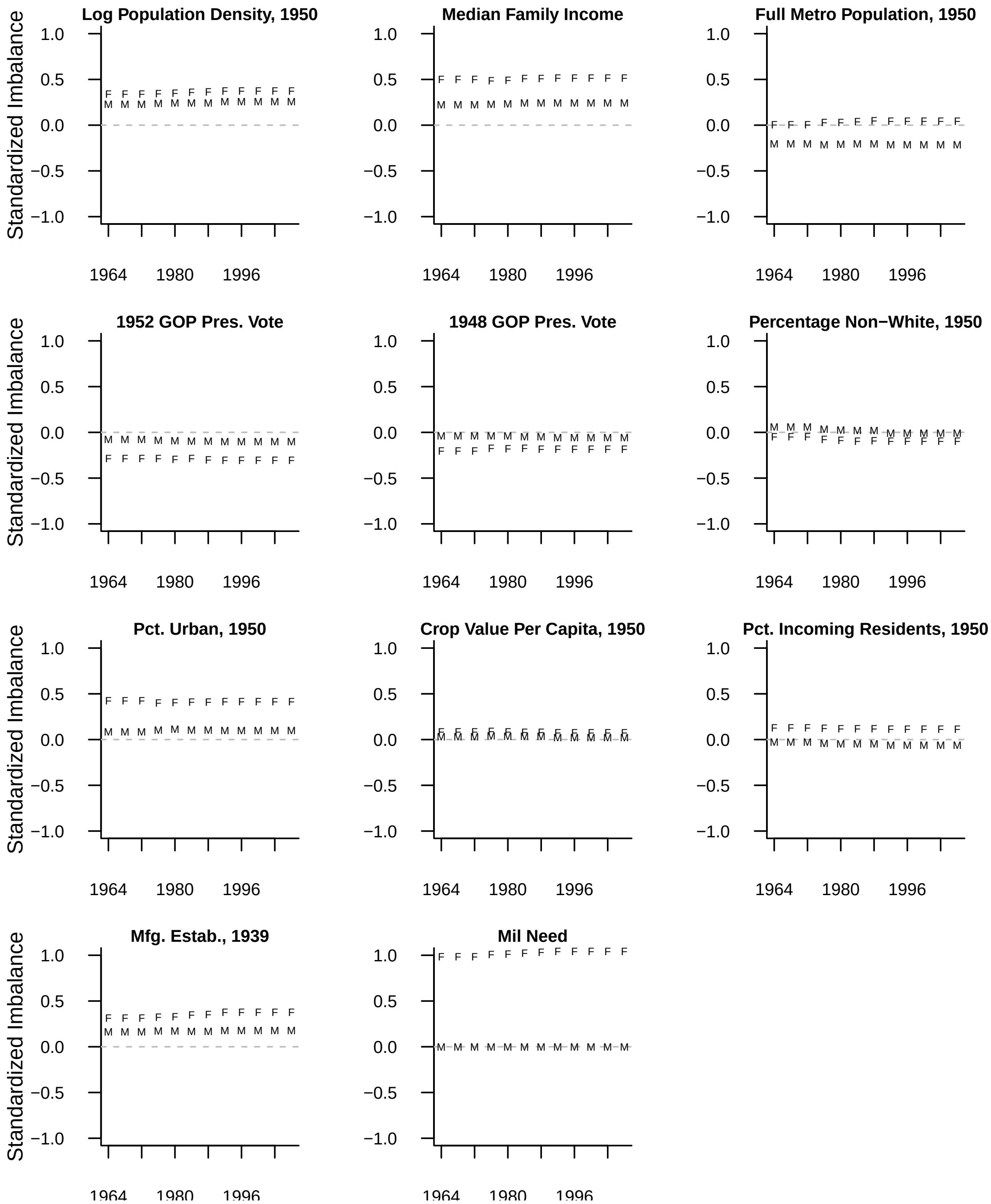

Figure A-12: Standardized imbalance, 1964-1971 treatment cohort, Non-Southern suburban counties. Difference in means for the treated and control group, divided by the standard deviation in the unmatched control group, for the [F]ull available sample and the [M]atched sample.

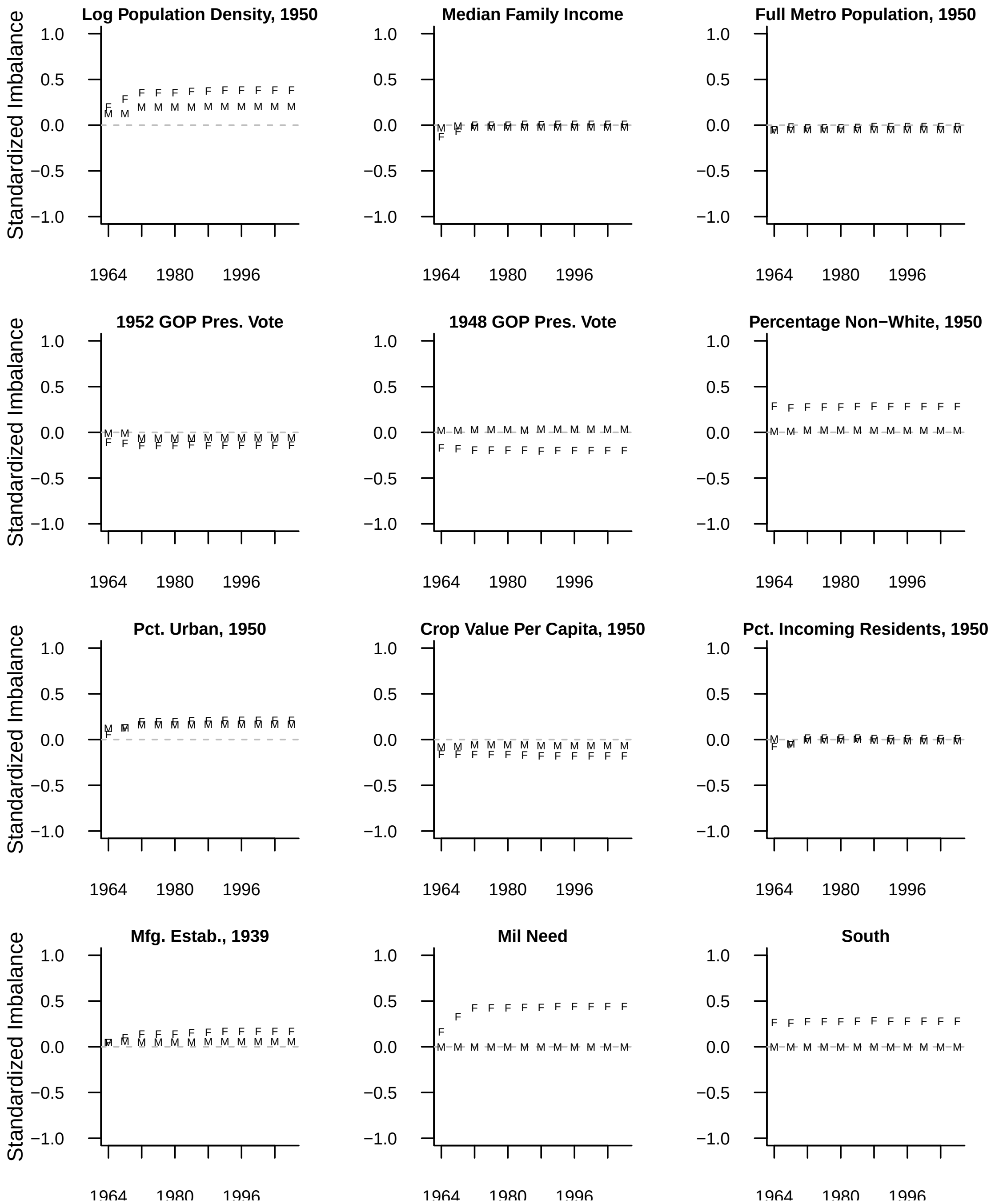

Figure A-13: Standardized imbalance, 1972-1979 treatment cohort, all suburban counties. Difference in means for the treated and control group, divided by the standard deviation in the unmatched control group, for the [F]ull available sample and the [M]atched sample.

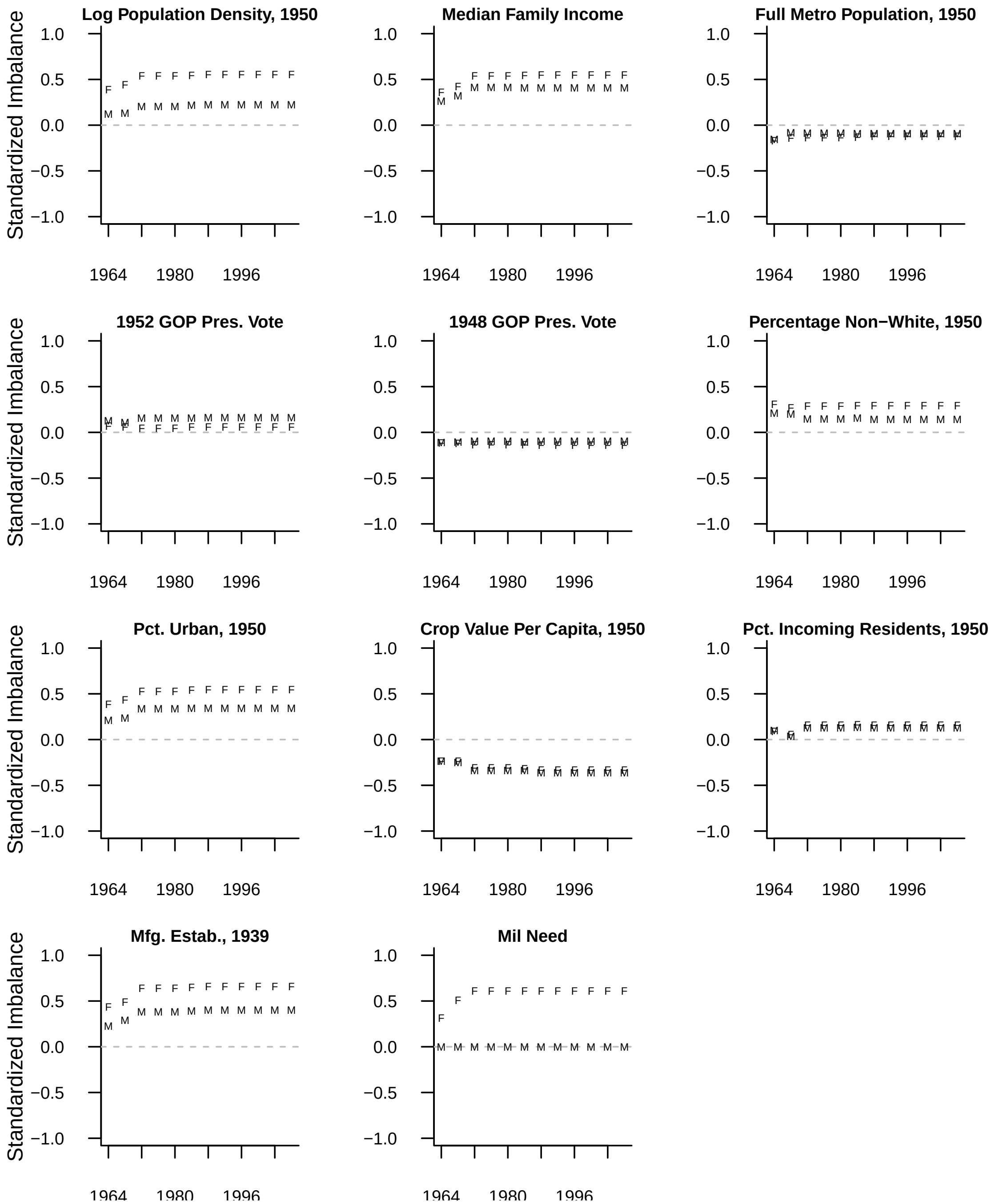

Figure A-14: Standardized imbalance, 1972-1979 treatment cohort, Southern suburban counties. Difference in means for the treated and control group, divided by the standard deviation in the unmatched control group, for the [F]ull available sample and the [M]atched sample.

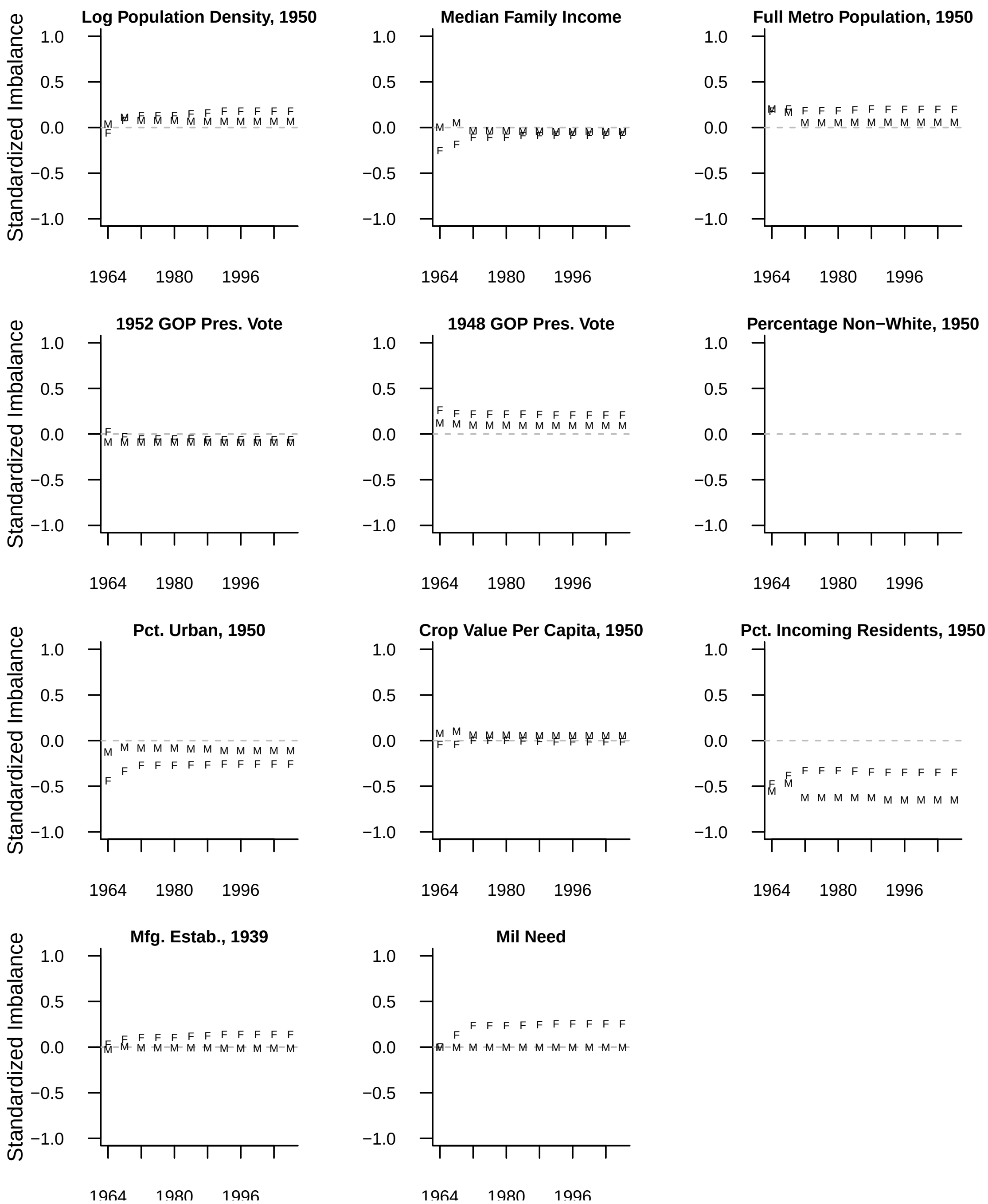

Figure A-15: Standardized imbalance, 1972-1979 treatment cohort, Non-Southern suburban counties. Difference in means for the treated and control group, divided by the standard deviation in the unmatched control group, for the [F]ull available sample and the [M]atched sample.

A.6 Highway-Induced Changes in Suburban Correlates of Partisanship

Partisan change in suburban counties and across metropolitan areas did not occur in a controlled environment. Highways had multiple effects, including changing the demographic and economic composition of communities, and, while we cannot completely disentangle sorting and compositional effects, we would expect highway-induced influences on both the political and demographic makeup of communities. In the case of the suburban-county vote, highways may have aided the sorting of *ex ante* Democratic and Republican identifiers, or placed voters in a context in which they are more likely to vote for one of the major parties. We could attempt causal mediation analysis to engage these demographic changes' relative influence over the total effect of highways on partisanship, but even in an experimental setting such an analysis would require strong assumptions (Imai et al., 2011). As an alternative, we can estimate highways' effect on a range of correlates of partisanship to demonstrate the aggregate-level outcomes that were most affected by the presence of highways. Available evidence from both the suburban counties and urban-suburban couplets during time period in question (using county-level Census data from 1970 to 2000) suggests that in the suburban comparative analysis, highways' most pronounced effect was to aid construction of wealthier commuter neighborhoods in suburban counties in which they were built. Within urban-suburban couplets, it appears that racial change was a more important consequence of highway construction.

The analyses presented here present compositional changes brought about by highway construction. Estimated effects on five correlates of partisan voting behavior are presented. For simplicity, a dummy treatment variable is coded "1" if an Interstate opened in the county at least four years earlier, and zero otherwise. Four of these are socioeconomic correlates of partisanship, measured using data from the National Historical GIS (Fitch and Ruggles, 2003): *Average home value (2010 dollars)*, *Per capita in-*

come (2010 dollars), *Percentage of workers over 16 working outside the county* (a measure of a county's commuter status), and the *Percentage of homes built in the previous decade*. To test for highways' effect on racial compositional change, I estimate the effect on the *Nonwhite percentage of the population*.

The first of these findings applies the methods for the suburban-county analysis as presented in Section 2. Results of this analysis (Figure A-16) suggest that partisan differences between suburban counties historically arose from highway-induced changes in counties' economic composition. Specifically, counties with Interstate highways have had higher income, higher home values, and a higher out-of-county commuter rate than comparable counties without them. These results are consistent with the model of residential sorting laid out earlier: highways enable creation of new, upper-middle-class and wealthy commuter suburbs in previously rural areas. They do not show that highways create large racial differences between suburban counties, though this is unsurprising given the overwhelmingly white population in the suburban sample.

Even in the South, differences in racial composition between suburban counties with and without Interstates are insubstantial. The non-white percentage of the population in suburban control counties (those that never had an Interstate) and suburban treated counties (those that had an Interstate built at some point during the study period) show little difference between the groups and little divergence over time as a function of Interstate status. By the 1990s, Southern suburban counties with and without Interstates had become more diverse, and developed suburbs with Interstates had become slightly more diverse. These results appear in Figure A-17.

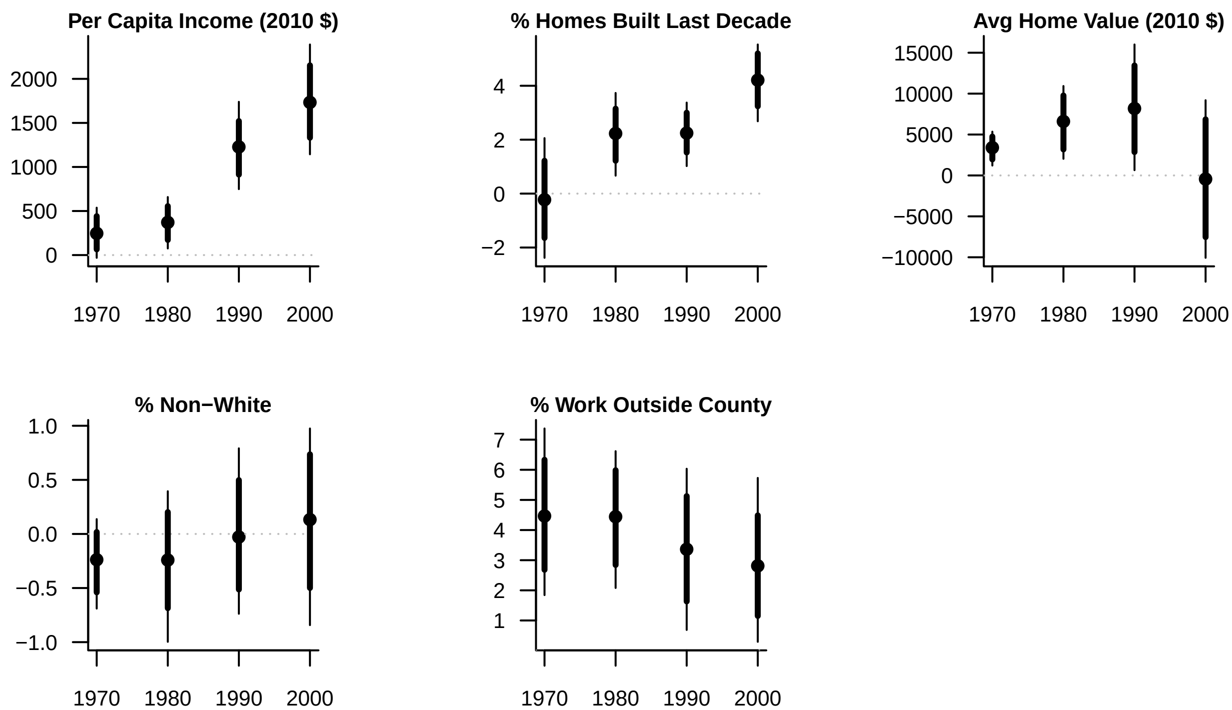

Figure A-16: Interstate highways' impact on economic and racial correlates of partisanship in suburban counties (average treatment effect on the treated in the population represented by the matched sample).

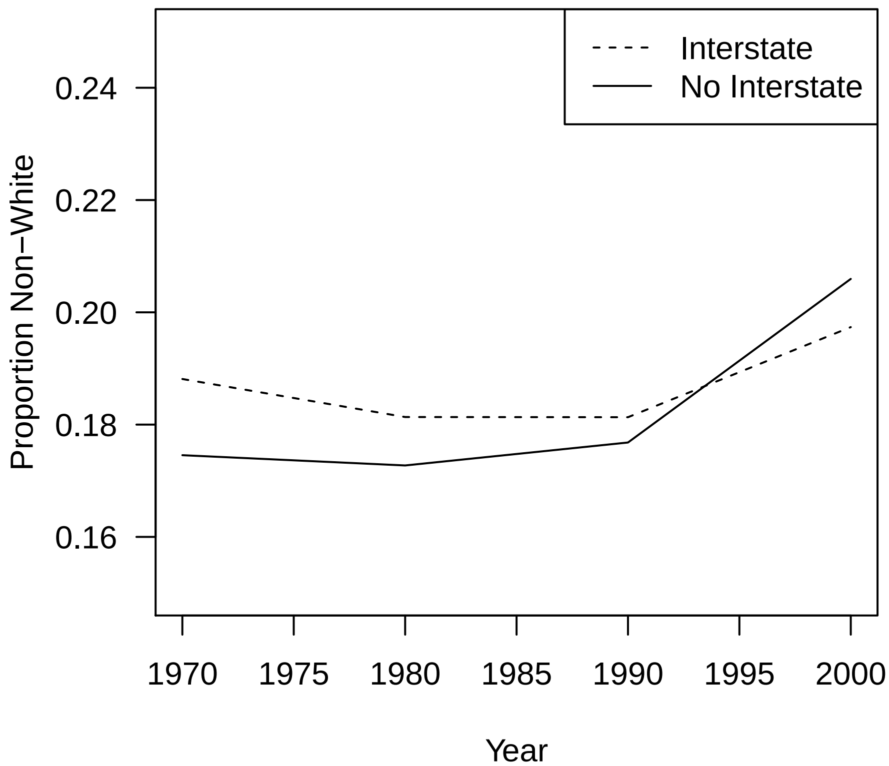

Figure A-17: Average proportion non-white residents in Southern counties with and without Interstates as of 1996.

A.7 Sensitivity Analysis

B Urban-Suburban Analysis

B.1 Summary Statistics for Urban-Suburban Couplet Sample

B.2 Sensitivity of Urban-Suburban Findings to Choice of Outer Radius

The sensitivity of the effect of the log-transformed variable is assessed by varying the outer radius used to define each metropolitan area. The outer radius of each metropolitan area is varied and the effects estimated by the same methods used on p. 20. These results, presented in Figure A-18, show that the direction of point estimates is insensitive to choice of outer radius.

B.3 Highway Effects on Urban-Suburban Gap in Correlates of Partisanship

One may speculate about the degree to which the urban-suburban divide is similarly tied to correlates of partisanship influenced by highway-induced residential sorting. As with the analysis of the urban-suburban Democratic gap, estimates are likely to be imprecise. Here, we can estimate the effect of highway density on the urban-suburban gap in per capita income, age of housing stock, home values, percent non-white, and the percentage of residents working outside the county. Lowess-smoothed estimates of highway density's effects on urban-suburban differences in selected Census variables are presented in Figure A-19. These are estimated using the same linear regression and smoothing process (without matching) that appears in the main text, p. 20. These estimates control for the same baseline covariates included in the text.

Across all categories, the estimates of the differences are in the expected direction. A larger positive value indicates that the urban-minus-suburban gap in the outcome is larger. The negative estimates

Variable	Mean	SD	Min.	Max.
Exits Open Per Square Mile, 1956	0.005	0.008	0	0.050
Exits Open Per Square Mile, 1964	0.023	0.027	0	0.155
Exits Open Per Square Mile, 1968	0.032	0.033	0	0.162
Exits Open Per Square Mile, 1972	0.038	0.038	0	0.197
Exits Open Per Square Mile, 1976	0.041	0.040	0.004	0.214
Exits Open Per Square Mile, 1980	0.044	0.042	0.004	0.214
Exits Open Per Square Mile, 1984	0.045	0.042	0.004	0.214
Exits Open Per Square Mile, 1988	0.046	0.042	0.004	0.217
Exits Open Per Square Mile, 1992	0.047	0.043	0.006	0.220
Exits Open Per Square Mile, 1996	0.049	0.043	0.006	0.220
Urb-Sub Diff. in Dem. Pres. Vote, 1948	0.034	0.086	-0.201	0.210
Urb-Sub Diff. in Dem. Pres. Vote, 1952	0.033	0.093	-0.169	0.197
Urb-Sub Diff. in Dem. Pres. Vote, 1956	0.024	0.092	-0.202	0.226
Urb-Sub Diff. in Proportion Nonwhite, 1950	0.026	0.056	-0.132	0.185
Urb-Sub Mean, Proportion Nonwhite, 1950	0.098	0.112	0.003	0.440
Urb-Sub Mean, Mfg. Estabs., 1939	801	1804	0	14320
Urb-Sub Mean, Co. on Strategic Route	0.83	0.19	0.1	1
Log Persons/Sq. Mi., 1950	5.10	0.93	3.74	8.013
South	0.275	0.449	0	1

Table A-2: Summary statistics for explanatory variables and covariates for urban-suburban couplets (n=83).

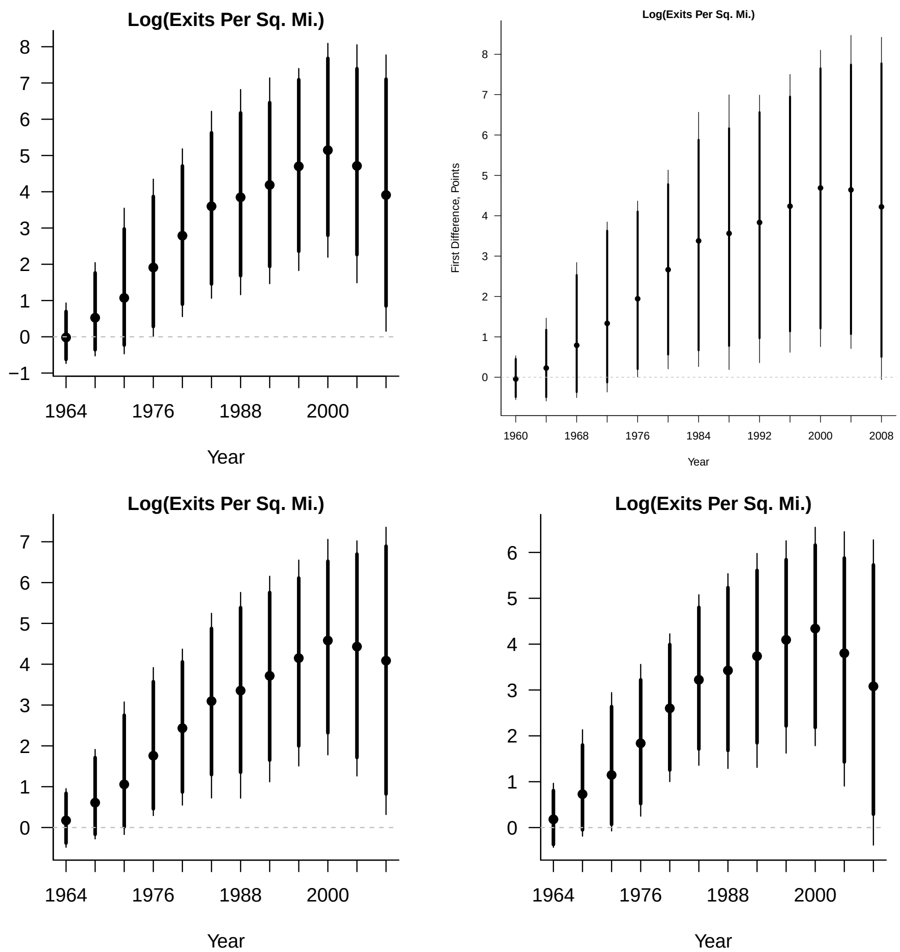

Figure A-18: Highways' effect on urban-suburban polarization across interquartile range of logged exits, as a function of outer radius used to define the metropolitan area. Top left: 50 kilometers; top right: 70 kilometers; bottom left: 80 kilometers, bottom right: 100 kilometers.

obtained for the effect on per capita income, for example, indicate that the income gap between suburbs and cities is larger in metropolitan areas with more highways. The large positive estimate for non-white racial composition indicates that metro areas with more highways tend, on average, to have a greater urban-suburban racial segregation.

B.4 Exogeneity of Exits

One concern raised about the exits analysis is that exit construction may be endogenous to local demands for highway construction. That is, as highways are built, they induce demand for more highways, and may also induce demand for exits. As discussed in the text, the construction of and placement of exits on Interstate highways in order to avoid excessive traffic jams was a key concern of highway planners. The merging delays and congestion now known to occur in urbanized areas with many highway ramps was a concern that prompted highway planners to place restrictions on exit construction. While there were key exceptions to the rule, most exits along the Interstate system are original to the Interstates, and few additional exits were built after initial construction. To demonstrate the stability of the number of exits over time, and how exits follow new highway construction and are not simply a response to new traffic demands, I present historical exit counts from three Sun Belt cities: Phoenix, Houston, and Atlanta. These three cities were selected because they were among the fastest growing cities over the study period, and because highways were essential to their outward growth. Thus, they constitute a "hard case" for my claim that exits were placed exogenously. Four quantities are presented from metropolitan-area maps from 1965, 1975, and 1985 Rand McNally Road Atlases: total exits on Interstates, total exits added in the previous decade, the proportion of exits that were inserted (i.e., that were added after a highway segment had been completed, rather than with new construction) and the percentage of exits that were these "inserted" exits. Of the three cities, Houston appears to be the worst-case scenario,

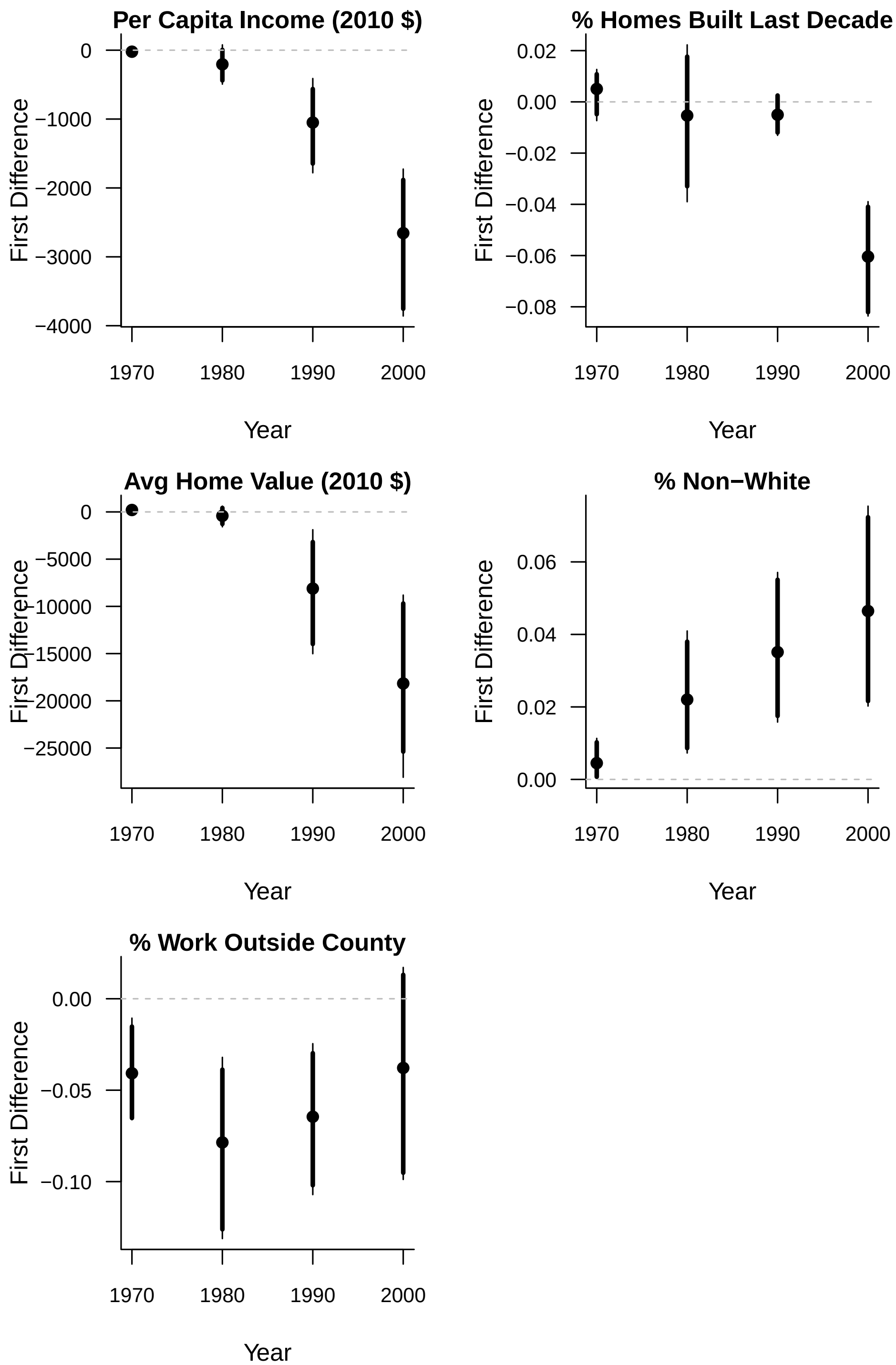

Figure A-19: Highways and urban-suburban polarization on correlates of partisanship. Lowess-smoothed estimate of the first difference in urban-suburban polarization in five key Census variables for the 25th to 75th percentile of the logged exit-density variable, without matching. Bootstrapped 80% and 95% confidence intervals accompany the point estimates.

City	Year	Interstate Exits	Exits on New Segments	Inserted Exits	% Inserts
Atlanta	1965	39	-	-	0%
Atlanta	1975	66	27	0	0
Atlanta	1985	83	14	3	3.6
Houston	1965	37	-	-	0%
Houston	1975	77	40	3	3.9
Houston	1985	115	38	14	12.2
Phoenix	1965	16	-	-	0%
Phoenix	1975	26	10	0	0
Phoenix	1985	40	4	0	0

Table A-3: The exogeneity of exits in three Sun Belt cities in 1965, 1975, and 1985.

with 12.2% of exits qualifying as "inserted" exits in the year 1985. The number of freeways added post-construction is minimal in the other two cities.

C Instrumental Variables Estimation

Among the alternatives to the matching and regression framework applied in the paper is an instrumental variables framework, in which an exogenous instrument is used to predict an endogenous treatment variable. One reason to seek out and use such an instrument is that it helps to address the widely held concern that once highways were built, they created traffic and, in turn, induced additional highway construction. We would want to identify an exogenous instrument that effectively predicts highway construction. Previous scholarship has used highways plans as instruments for eventual highway construction. Using two-stage least squares or other instrumental variables methods, we can then estimate the effect of highways only in counties whose highway construction can reasonably be construed to be induced by the original plan. If the instrument is uncorrelated with the outcome variables except through the endogenous regressor, we can interpret such an estimate as an unbiased local effect among those units whose construction occurred because of their presence on the highway plan.

Instrumental variables analysis requires strong assumptions that are difficult to satisfy in this setting. The first is that the instrument must be a strong predictor of where highways were built. In this respect, the highway plan is a strong instrument for Interstate construction, as noted in the paper. Across most of the period, very few Interstates were built that did not appear on original plans. A second requirement is that any instrument must satisfy the exclusion restriction: after conditioning on shared predictors of the instrument and the outcome, the instrument must be "uncorrelated with any other determinants of the dependent variables" (Angrist and Pischke, 2009, 116-117). With respect to this project, the highway-plan instrument must not just act upon the outcome, but must be correlated with the outcome only by way of the endogenous regressor (whether a highway was built).

Do highway plans that pre-date highway construction satisfy these requirements? It is clear that this instrument was not randomized. As noted in the text, the factors considered in drawing the plan are also factors that anticipated future changes in metropolitan areas. Thus, to use highways as an instrument, we must depend very heavily on the other covariates included in the first-stage and second-stage regressions. Thus, the primary advantage of the instrumental variables approach is to eliminate highways that were built "off the plan" from the analysis.

The results of two instrumental variables (two-stage least squares) regressions presented in Figures A-20 and A-22 offer a robustness check on the findings presented in the body of the paper. The top figure is a check on the suburban-county analysis. In lieu of matching, an instrumental variables analysis was applied to all suburban counties in states where highways were built no earlier than 1956. We are interested in the second-stage coefficient estimate on the interstate highway variable (an indicator that an Interstate was open in a county at least four years earlier). The instrumental variable used in the analysis is an indicator variable indicating whether a county was included in the 1944 highway plan, represented by georeferencing the 1944 Interregional Highways report, tracing the lines from the report, then adding a five-mile-wide buffer to account for imprecision in the original map and in coding. The 2SLS regression includes the following county-level covariates in both the first and second stage: median family income in 1950, log population density in 1950, percent non-white in 1950, percent urban in 1950, crop value per capita in 1950, percentage of residents who did not live in the county in 1949, and the number of manufacturing establishments that appeared in the 1939 manufacturing census. The outcome variable used in the second-stage estimate is the gain score measure. The bootstrap/lowess smoothing procedure described in the text is used. These instrumental variables regressions are run separately on the full set of suburban counties, Southern counties, and non-Southern counties.

The second analysis is a similar type of design, but applies to the urban-suburban analysis, and more closely follows the analyses that appear in the literature. The exogenous instrument is the metropolitan-level number of "rays," a count of the number of radial highways, on a 1947 highway plan, while the variable being instrumented is the number of radial highways built as of the beginning of each decade preceding highway construction, from 1950 through 1990 (Baum-Snow, 2007). The first and second stage regressions each include the following covariates: the urban-suburban difference in the 1948, 1952, and 1956 presidential votes, the urban-suburban difference in the nonwhite percentage of the population in 1950, the unweighted urban-suburban mean of the percentage nonwhite in 1950, a dummy variable for the South, and 1950 log population density. This analysis is run only on the full sample and the bootstrap/lowess smoothing procedure described in the text is used.

The results from both of these analyses do nothing to contradict the findings in the main text, and even suggest that the regional effect heterogeneity observed in the South and non-South may be overstated. Across all suburban counties, counties whose highways were induced by the 1944 highway plan were, across most of the study period starting around 1976, about 4 points less likely to vote for the Democratic presidential candidate than comparable counties without highways. While regional effects are estimated with less precision, across much of this period the effect sizes in the South were about twice as large: around 6 to 8 points, though most of these estimates only border on statistical significance at customary levels. The same analysis was conducted with the same gain-score outcome but without including pre-treatment covariates in the system of equations. These results (Figure A-21) are substantially similar.

The rays analysis also yields effects that are qualitatively similar to those presented using exits. The plotted estimates represent the effect of a shift from the 25th percentile to the 75th percentile in the number of rays on the planned map. The dots on each plot represent the unsmoothed point estimates, while the blue line and dashed and full line represent the lowess-smoothed point estimates and 80% and 95% confidence intervals.

The rays analysis also yields effects that are qualitatively similar to those presented using exits. The plotted estimates represent the effect of a shift from the 25th percentile to the 75th percentile in the number of rays on the planned map. The dots on each plot represent the unsmoothed point estimates, while the blue line and dashed and full line represent the lowess-smoothed point estimates and 80% and 95% confidence intervals.

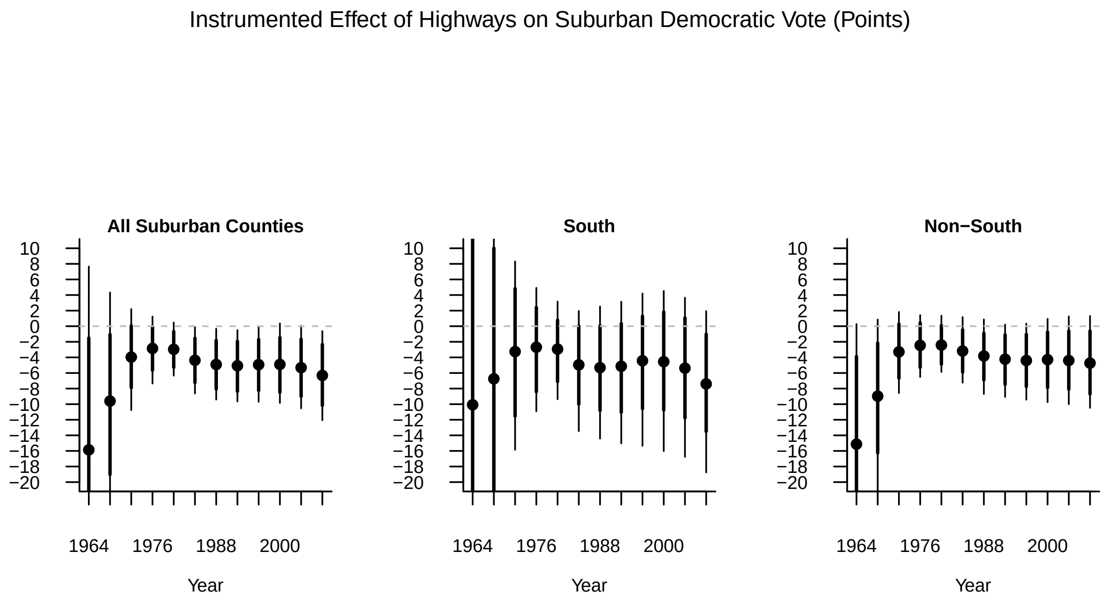

Figure A-20: Instrumental variables estimates using early highway plans as an instrument for eventual adoption. Effect of highways on suburban Democratic vote. Full set of pretreatment covariates included in estimation.

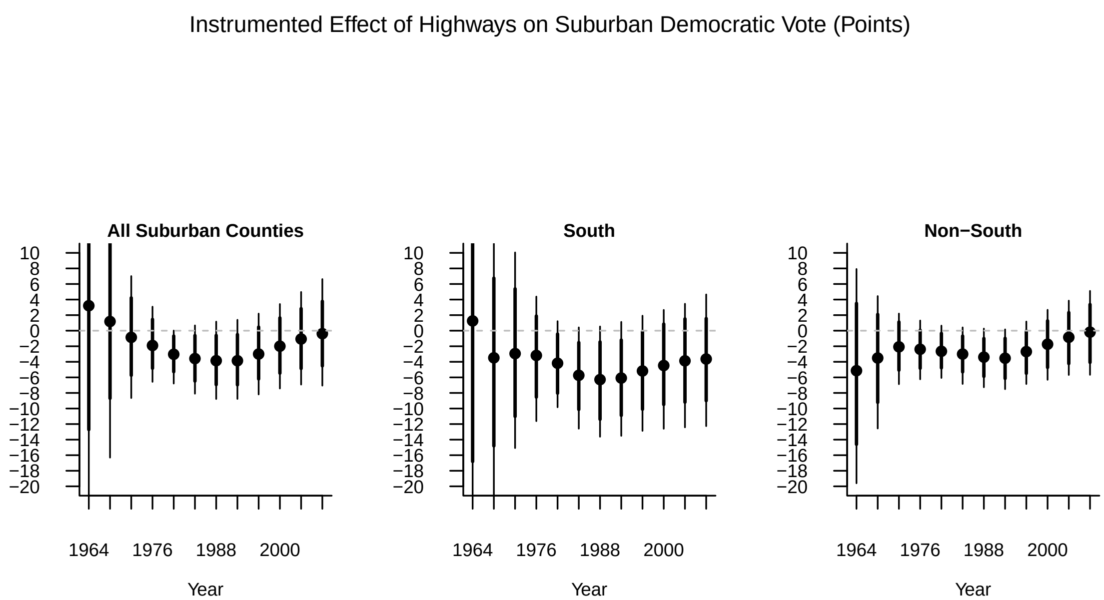

Figure A-21: Instrumental variables estimates using early highway plans as an instrument for eventual adoption. Effect of highways on suburban Democratic vote. No covariates included in estimation.

D Addressing Regional Heterogeneity

D.1 Differences and Similarities Between the South and Non-South

Infrastructure was able to have a larger influence in the South because metropolitan areas in the region had lower density suburban regions than the non-South before the construction of the Interstate Highway System. At the same time, the differences between the South and other states were small enough to be addressed by including controls that represent the different starting points for suburbanization in the two regions. The scale of these differences is presented in the Figure A-23, which captures these suburban pre-trends using two variables whose effect was accounted for in matching and linear regression. The horizontal axis of each graph represents the distance of each county from the nearest central city in each metropolitan area, while the vertical axis represents the county-level mean. These graphs demonstrate that counties on the outskirts of Southern cities were slightly lower-density than suburban counties elsewhere, and such counties also had a slightly higher proportion of new residents. Both of these are consistent with the hypothesized mechanisms and the larger effect sizes observed in the South.

While these analyses are presented as examples of regional differences, the differences observed here are addressed in the analysis in several respects. Both of the variables presented here are included as linear covariates, so their linear additive influence over the results is accounted for in the models. If that proves insufficient to account for the differences presented here, a region fixed effect is included, which encompasses any non-time-varying differences between the South and non-South not accounted for in the other covariates.

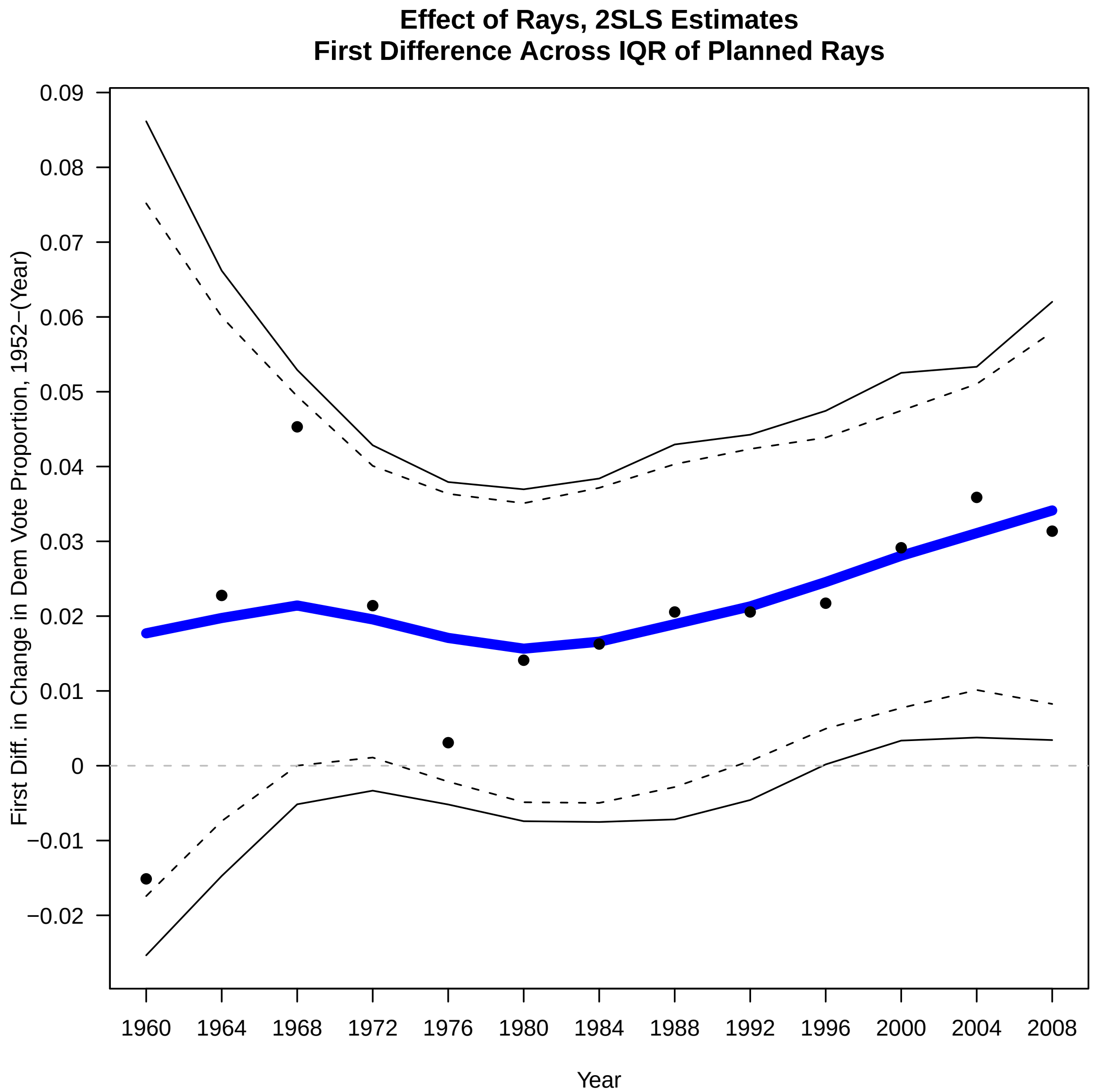

Figure A-22: Instrumental variables estimates using early highway plans as an instrument for eventual adoption. Effect of radial highways on the urban-suburban Democratic gap.

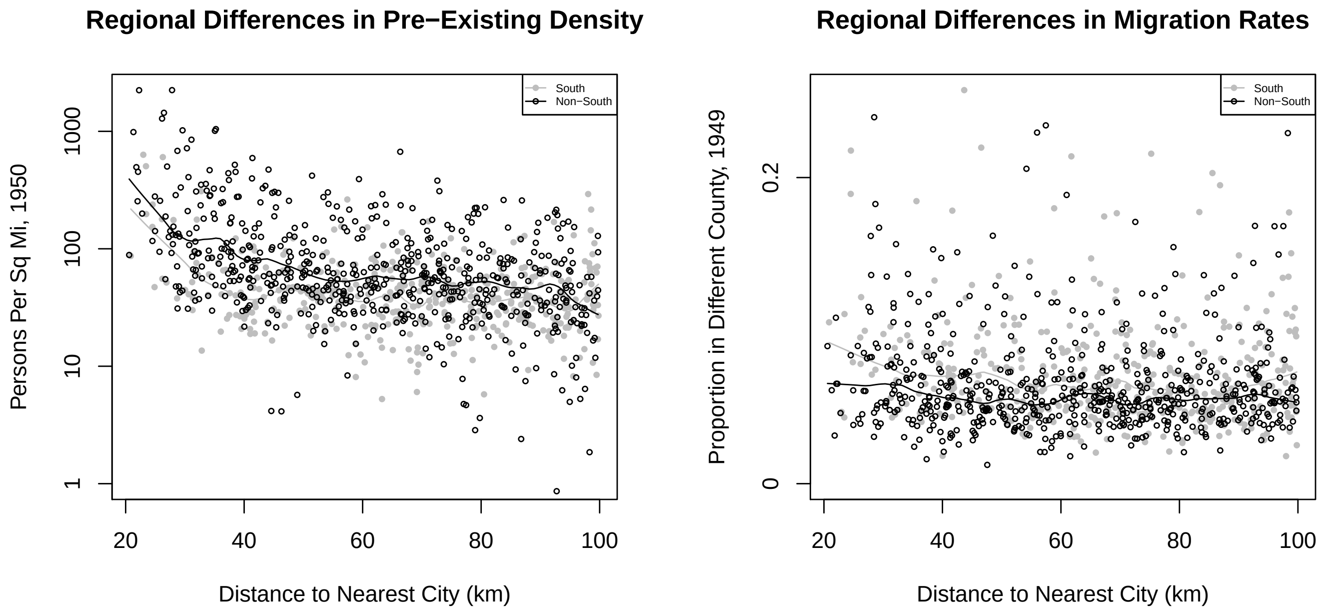

Figure A-23: Southern suburban counties were only slightly lower density than non-Southern suburban counties in 1950, and residents in Southern counties were slightly more likely to have lived in a different county a year earlier.

Another concern about the South is the extent to which conversion in place, rather than geographic mobility. While this distinction is not important to the estimating highways' effect on geographic change (since highways could act by indirect means to help induce partisan change at higher rates in counties with highways), descriptive statistics from historical Censuses (Fitch and Ruggles, 2003) indicate a larger role for geographic migration in suburbs in the South, and a larger difference associated with highways, than outside the South. Figure A-23 presents the proportion of migrants who lived outside the county five years earlier. The regional averages for the South and non-South are presented, along with averages for counties that had an Interstate highway five years earlier and those that did not. While this figure does not provide evidence of a causal effect, about 25% of residents of counties with Interstates were newcomers, while just around 20 percent of residents in counties without highways were newcomers. The effect is also in the expected direction (though, unsurprisingly, smaller in magnitude) across this period. In all Censuses, in both regions, the average proportion of new arrivals in counties with highways exceeds the proportion in counties without highways. Also consistent with a model of sorting in which highways have larger effects when they are built in new areas, the highway-related gap grows smaller over time as more areas have Interstates for longer periods. Some of this convergence of places with and without Interstates may be due to the diffusion of residential growth from counties with highways into those without them.

D.2 Barplots of Migration by Party ID, South and Non-South

Appendix References

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Fitch, Catherine A and Steven Ruggles. 2003. "Building the National Historical Geographic Information System." *Historical Methods* 36(1):41–51.

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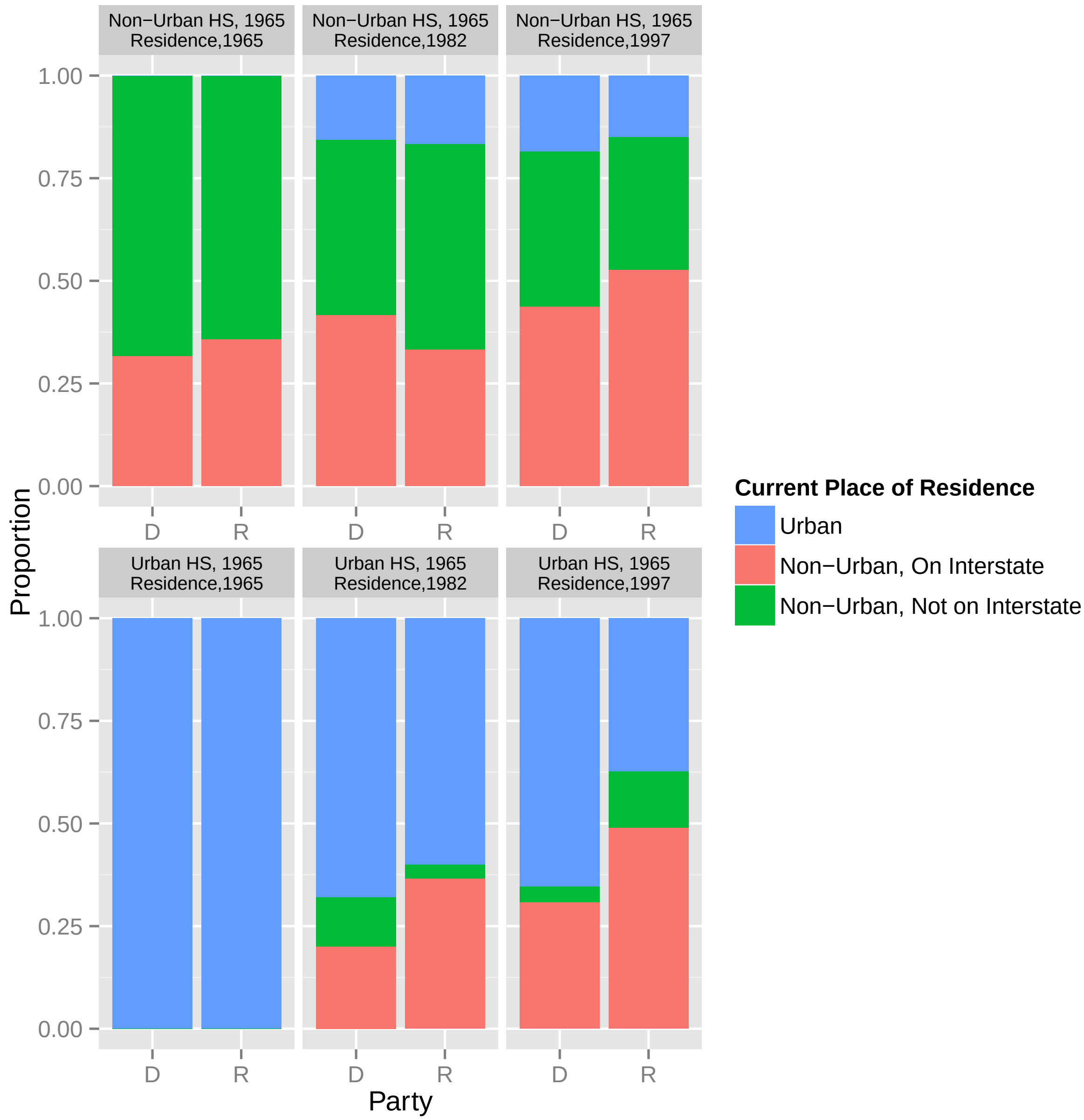

Figure A-24: Barplot of distribution of residential type of place, by party and urban/non-urban location of respondent's high school in 1965, Southern high school graduates only. The partisan identification variable is allowed to vary with each time period.

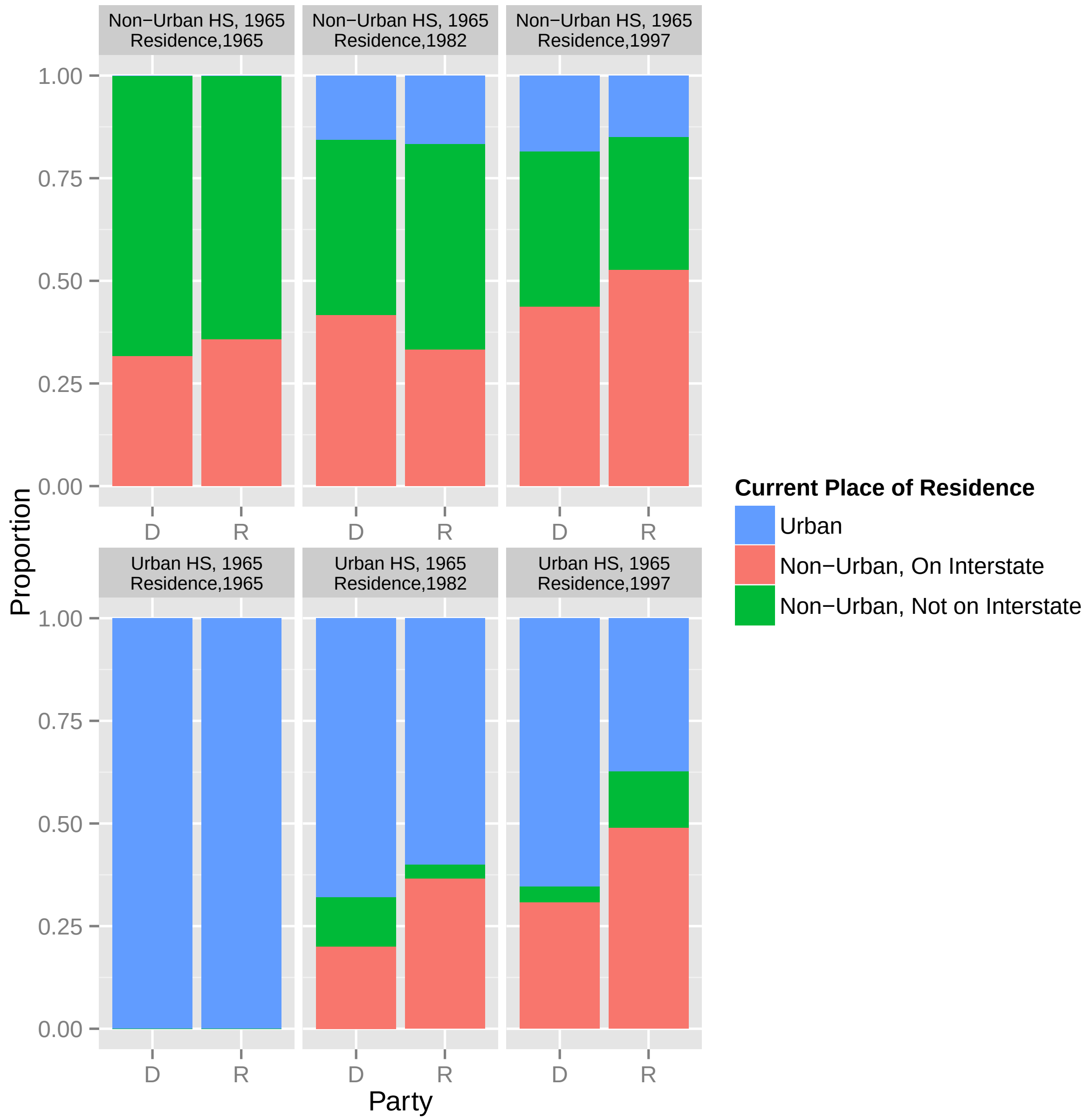

Figure A-25: Barplot of distribution of residential type of place, by party and urban/non-urban location of respondent's high school in 1965, Southern high school graduates only. The partisan identification variable is allowed to vary with each time period.