Agglomeration Over the Long Run: Evidence from County Seat Wars*

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Abstract

Urban areas are shaped by their size, perhaps particularly so over the long run. We study how historical shocks to the size of towns in the American West affected long-run economic outcomes, using elections which determined county seats (capitals) in the 1800s and a regression discontinuity (RD) design. High rates of mobility in the frontier period meant that these elections quintupled population density in winning locations, ultimately determining where 15% of a county resides today. Although the county seat provides relatively few government jobs, we show that the increased town size reshaped their modern local economies in several important ways. First, reported IRS income increases with an elasticity of 0.15 with respect to density. Second, these gains are distributed primarily to the upper end of the income distribution, leading to a 2.9pp increase in the top 5% income share. Finally, spillover effects onto nearby communities are minimal, cutting against notions of positive amenity provision and negative agglomeration shadows.

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1 Introduction

The size and location of urban areas are key factors in the study of economic geography. Spatial equilibrium models emphasize that population density can influence economic outcomes (Davis and Dingel 2020), particularly in the presence of externalities (Desmet and Rossi-Hansberg 2013) and over long timespans (Allen and Donaldson 2020; Kleinman et al. 2021). Empirical studies, however, have often focused on shorter-run difference-in-difference effects (Greenstone et al. 2010) or the longer-run effects from the presence of specific industries (Kline and Moretti 2014; Bleakley and Lin 2012; Hornbeck and Moretti 2018). Since locational suitability contributes significantly to where people live (Ellison and Glaeser 1999) and because spatial equilibria often resist temporary shocks (Davis and Weinstein 2002), finding identifying variation in city size alone is difficult. Therefore, we know less about the role that historical accidents can play in shaping city size and the long-run effects of this general agglomeration (Puga 2010). Additionally, much research on these topics has focused on the role of large cities (Combes et al. 2012; Rosenthal and Strange 2004; Eberts and McMillan 1999), leaving smaller towns understudied.

In this paper, we study the long-run, causal effects of town size in the late 1800s American West. For identification, we exploit close elections which selected the locations of county courthouses in a regression discontinuity (RD) framework. Although courthouses provide only a small amount of employment, this boost along with the prestige conferred by securing a county's "seat of justice" were enough to drive large and broad-based migration in the fluid period of frontier settlement. These migration decisions proved persistent, making this an ideal setting in which to study the long-run dynamics between population agglomeration and economic outcomes.

We find that historical agglomeration substantially reshaped the present-day economies of the victorious towns. Winning towns roughly quadruple their population over their rivals, on net shifting about 15% of each county's population. Just within the close elections of our limited sample, this result implies county seat elections were responsible for the locations of about 4.5 million people. The population increases also led to increased average incomes in winning locations, but these gains are unequal and concentrated in white-collar, education-intense sectors.

Our analysis required extensive collection of new data. We construct an original dataset of county seat election results, drawn from historic newspaper archives and administrative records. Additionally, we geographically locate all towns within the RD bandwidth of our sample, including ones that ceased to exist and do not appear in any available dataset. We link these data to census records on population, post office locations, and geographic characteristics. The result is a detailed political and economic history of our sample counties.

We then study how historical agglomeration of households affected local economies' industry mix and output. Our estimates show that by 2010, winning towns become wealthier, more educated, and more focused in "high-skill" industries. Overall, Zip-level taxable income increases with an elasticity of 0.15 in response to population density, notably higher than the wage elasticity estimate of 0.04 for high-income countries in the literature (Ahlfeldt and Pietrostefani 2019). However, these gains are not distributed evenly, with the top 5% income share increasing 2.9pp. Instead, the increased income seems to reflect a shift toward higher-paying, white collar, and education-intense industries. These results provide causal support for the notion that larger cities' increased income comes at the expense of increased inequality as in (Baum-Snow et al. 2018; Baum-Snow and Pavan 2013; Ahlfeldt and Pietrostefani 2019).

While the direct effect of becoming the county seat lead to the construction of an administrative building (the courthouse) and the creation of corresponding jobs, the main mechanism behind our effects appears to be the agglomeration effects from a long-term increase in the population of winning towns. In the early frontier period, settlers did not have deep-rooted attachments to their current homes (Shortridge 2004). Historical accounts suggest that the elections served as a coordinating mechanism for locating populations, with settlers having a preference of living in larger and more stable communities. In a number of cases a large portion of the losing town's population directly moved into the winning town. Correspondingly, we show that most of the winner's population growth occurs within 20 years of its victory. The population effects are much larger than the number of jobs directly provided the seats. County jobs in legal or administrative fields only account for 0.3% of national employment, far lower than the 15% population moved by the election; jobs in public administration also do not increase detectably. We also directly rule out a number of other mechanisms including pre-election imbalance and increases in government service provision. For the latter, we focus on school district outcomes and can rule out all but small increases in revenues per student. We additionally argue that election winners are not more connected to transportation networks than losers given the size of the population increase.

The population boom from a county seat election, surprisingly, does not seem to have had noticeable long-term effects on nearby communities. In theory, winning towns might attract population into their surroundings by offering employment opportunities and amenities (Rappaport et al. 2005). On the other hand, areas surrounding county seats might lose population as households leave to move to the county seat instead (Cuberes et al. 2019; Beltrán Tapia et al. 2017). While we find increases in rural population on the outskirts of winning locations, effects on other urban populations are small and statistically insignificant.

Overall, towns that won county seat elections agglomerated their population, attracted education-intensive occupations, and positively boosted the density of surrounding rural areas while avoiding some of the negative consequences of agglomeration in large, productive cities such as rising living costs. Our results suggest some increases in inequality, indicating that the gains accruing to county seats might not be shared equally. This result mirrors others in the literature wherein gains in total factor productivity benefit only some residents, e.g. home owners (Hornbeck and Moretti 2020) or highly skilled workers (Qian and Tan 2021) while possibly harming renters or workers without college education.

We contribute to several active strands of the literature in urban economics and economic history. First, we add to our understanding of the effects of agglomeration forces. Why economic activity is geographically concentrated has been studied both across cities (Rosenthal and Strange 2004; Duranton and Puga 2004; Glaeser et al. 1992) and within (Ahlfeldt et al. 2015; Heblich et al. 2020). Additional work (Greenstone et al. 2010; Qian and Tan 2021) also examines the spillovers of economic activity on the surrounding region. Our paper contributes to the relatively less understood spillovers in population and occupational structure of centers of local economic activity (towns) on their immediate surrounding areas. Commonly studied reasons for agglomeration include locational fundamentals and natural advantages, productivity gains from proximity to a higher density of workers and firms (Ahlfeldt et al. 2015; Heblich et al. 2020; Qian and Tan 2021) and knowledge spillovers (Davis and Dingel 2019; Combes et al. 2008; De La Roca and Puga 2017). Here, we consider a novel initial source of agglomeration stemming from the population increase following a county seat election victory. This allows us to credibly isolate the effects of population density on the structure of a nascent economy.

Second, by using historic variation from county seat elections we contribute to the open question of what factors shape the growth and location of cities (Bleakley and Lin 2012; Davis and Weinstein 2002; Harari 2020; Brown and Cuberes 2020). County seat elections provide a clean source of identification of the effects of agglomeration separately from underlying fundamentals of a given location. Using this strategy we are able to study long-run effects of agglomeration, thereby shedding light on the persistence of historic shocks (Allen and Donaldson 2020; Hanlon and Miscio 2017) to productivity and infrastructure. We employ widespread, cross-town variation for large parts of the United States, which broadens the understanding we have from previous literature of agglomeration in geographically or industrially concentrated areas (see Hanlon and Heblich (2020) for an overview).

Our paper also adds to the subset of this literature focused on the role of capital cities in economic development (Bluhm et al. 2021; Campante and Do 2014; Ades and Glaeser 1995; Bai and Jia 2021) which finds that that national or regional capitals induce extra investment as a complement to an expanded public sector presence in capital cities. However, the small-scale nature of local government in our context means our primary mechanisms differ from those researched by this body of work. First, we show in Section 5.1 that close election victory results in a large, but temporary migration influx, inducing an annual growth rate around 20% in the first years after the election, but few changes in population growth afterward. For comparison, Bluhm et al. (2021) report about a 1% growth rate after selection as a regional capital. Second, any increases in government sector employment are too small for us to statistically detect, as shown in Section 5.2. Finally, this literature has pointed to the government's ability to actively direct public investment toward capital regions (Ades and Glaeser 1995). We show in Section 7.2, however, that county governments do not engage in this manipulation, with their school districts performing equivalently or perhaps slightly worse on revenue and teachers per student. Overall, we argue the long-term "county capital" effect here is thus driven by the historical migration effects rather than the political economy elements discussed in this literature.

Third, we contribute to the literature of the historical development of America's frontier and rural areas (Smith 2020; Donaldson and Hornbeck 2016; Hornbeck and Naidu 2014; Acemoglu et al. 2016; Nagy 2020). County seat elections were a ubiquitous and salient feature of American life outside the original colonies and their impact on location choices and local economies is an important study in its own right from a historical perspective.

Finally, our paper also speaks to the literature on effectiveness and persistence of place-based policies such as Ehrlich and Seidel (2018), Kline and Moretti (2013) and Kline and Moretti (2014). Since county seat contests occurred both in densely populated and rural counties, our findings on how a population boost affects these places are helping in understanding the consequences of policies that offer a boost to locations instead of targeting individuals (Glaeser and Gottlieb 2008; Bartik 2020). If agglomeration leads to higher productivity and positive spillovers in low-density regions, there could be welfare gains from such policies though these gains might not be equally distributed (Ehrlich and Seidel 2018) or simply cancel out in aggregate (Kline and Moretti 2014).

The paper proceeds as follows: Section 2 discusses the historical significance of county seat elections, section 3 describes our data collection and assembly process, section 4 introduces our research design, section 5 and section 6 discuss our results on towns and economic geography respectively. Section 7 tests alternative mechanisms and section 8 concludes.

2 Historical Background

> "Lamesa won the county seat election by five votes... A town meeting the next day invited the citizens and merchants of Stemmons to move to the new county seat... The offer was accepted and effected within several days"
>
> —Texas State Historical Association, *Handbook of Texas*

New counties on the American frontier faced the decision of where to conduct official business. Locating the seat of county government had both potential to impact a town through tangible and intangible effects. Tangibly, a county seat required a courthouse, storage of local government records, and some government employment. With respect to the last category, a number of county government positions such as judges, the county clerk (election administration), and the county recorder of deeds (land records) would naturally locate in the county seat. Other local government employees like public school teachers would locate more widely. To contextualize the size of the former group, county government employment in the legal and public administration sectors accounts for about 0.3% of national employment (Census 2012). Intangibly, being named the county seat accorded a large amount of prestige, suggesting a town's premier position within a county. However, in the period of fluid migration of the early frontier, even this prestige could be quite important.

As the opening quote indicates, the location of the county seat was of major importance for the growth of western towns. In the nascent frontier period these few jobs and the prestige conferred by the title could be the difference of life or death for these communities. "Towns desired county seats... because the designation brought increased status for the town" (Nevada Secretary of State 2016; Paher 1969). Losers of these elections could face rapid population loss, particularly in migration to the winner.

The rapid response of early settlers to county seat elections was a general pattern (see Section 5.1) and indicates that the primary effects were broad-based. Although a small number of county officials would have their jobs moved, the vast majority of residents could theoretically have kept their positions without change. The rapid turnover in population documented in the historical accounts and Section 5.1 thus speak to the fluidity of the preferences of a large number of settlers who preferred to live in larger, more prominent towns regardless of their identity. In some sense, the county seat acted as a coordinating device for such settlers and had great potential to shape the long-term growth of the American West's nascent towns. Towns recognized the high stakes of these elections, frequently leading to a contentious atmosphere between close rivals. The bitter competition such elections engendered thus led some observers to refer to them as "county seat wars."

The administrative procedures for each election could differ based on state and time period, but all are amenable to our regression discontinuity strategy. In some instances, elections were "first past the post" where the highest vote earner won; in other instances, a majority was a required leading to a runoff of the top two towns where no majority was achieved. In most states, towns could later challenge an incumbent, though the process was not costless, often requiring a collection of signatures at a minimum and usually a waiting period following the incumbent's victory. For example, Texas required that county seat elections be held only during presidential elections, meaning a minimum four-year waiting period. Other states further raised the bar for unseating incumbents by requiring a supermajority of votes; in later Nebraska elections, for instance, challengers needed to earn a 60% share. Finally, in a few cases, state legislatures attempted to tip the scales toward certain types of towns by requiring supermajorities from towns they deemed undesirable. In Texas, towns far from the center required a 2/3 supermajority to prevail.

As discussed in Section 4, we account for these divergent systems by defining our running variable as the percentage of votes above or below each town's threshold for winning the election. Although in isolation the winner of a county seat was unlikely to be random, particularly given the above considerations, vote percentages are likely to be continuously distributed. Further, pre-specified cutoffs for victory for different towns would not violate the key RD requirement for balance across either side of the threshold. We empirically test these assumptions in Section 4.3 and find no imbalances across pre-election population or geographic characteristics.

3 Data

3.1 County Seat Elections

Our primary contribution in terms of data is assembling an original original dataset of county seat elections. In general, few preexisting comprehensive datasets of such elections exist, even at the state level, and our process thus required a county-by-county search. We draw most extensively from historical newspapers which frequently detailed the election's exact vote totals. We also consult county clerks, county histories, and historical societies for additional data. In the exceptional case of Oklahoma, administrative data was available as part of the Governor Haskell records at the Oklahoma State Archives. Our current data set consists of 897 county seat elections across 637 counties in 38 states. These data are mapped in Figure

1a. Collectively, the counties in our sample were home to 55 million people in 2010. Figure 1b plots the 2010 populations of the census places in our sample, compared to all other census places. In general, while our sample locations tend to be modestly larger, the two distributions are fairly similar. In that sense, the areas we study are fairly representative of towns in the United States.

3.2 Other Data

We combine county seat election data on historical populations with modern population and economic data from the US Census. We base population figures on (Schmidt 2018), supplementing it with additional entries from relevant census tables where it is missing. We draw Census block-level data on job characteristics from the 2010 LODES data. We also make use of subcounty IRS income statistics, reported at the Zip Code level. Finally, other modern data sources include 2010 census-block population totals and 2010-14 ACS characteristics at the place, subcounty, and block group level. Finally, we make use of the Zillow ZTrax sales data[1] on individual, single-family homes to illustrate the effects on land prices.

For other historical data, we turn to a geocoded, panel dataset on US post office locations throughout the nation due to Blevins (2015, 2021). Elevation data are constructed from the SRTM dataset, stream shapefiles are via ESRI. Railroad data are from "Railroads and the Making of Modern America" (1870) and the "National Transportation Atlas Railroads" (modern, distributed by ESRI).

3.3 Linking Data Sources

Some care is required when linking economic data to the towns. In a large majority (82%) of cases relevant to the regression discontinuity,[2] the competing towns exist as US Census "places" and can thus be linked to exact set of census blocks. However, in the remaining cases, the location no longer exists as a town large enough to be mapped by the census.

[1] Our current access limits us to two states, Kansas and Iowa, chosen based on the highest number of elections in our sample.

[2] Within 15 percentage points of victory, on either side.

For towns that no longer exist, we match to characteristics of the rural areas close to the original site. In the case of block-level data, we report data within 1 kilometer of the location. In the case of ACS data, we link to either the subcounty or block-group data, whichever has the lowest portion of its population from census places. In case of ties, we choose the smaller unit. This procedure thus gives the most finely-tuned view of the rural population near the original town site.

Note that this linking procedure requires accurate coding of the locations of abandoned or ghost towns. In most cases, these are unavailable in existing datasets. We extensively reviewed historical county maps and histories to locate these towns, focusing our efforts on those relevant to the regression discontinuity (within 15 percentage points of the victory threshold). In all of these cases we are able to successfully locate the original site. The final baseline sample consists of 1714 unique locations, with 1531 geolocated.

Table 1 presents summary statistics for the locations[3] within our unrestricted sample. The median election occurs in 1882 with a large range of years from 1855 to 1914 within the 10^{th} to 90^{th} percentiles. We geocode over 90% of the locations within our sample, including all locations within a bandwidth of 15pp of election victory. All or a large majority of these have data on the distribution of jobs and IRS income tax information.

4 Econometric approach

4.1 Regression Discontinuity

Our main identification strategy consists of regression discontinuity (RD) analyses. In keeping with the best practices recommended by the literature (Calonico et al. 2014; Gelman and Imbens 2019) we use the specification

$$y_i = \alpha \mathbb{1}\left(\text{Win Election}\right)_i + f(\text{Victory Margin})_i + X_i\beta + \varepsilon_i \tag{1}$$

where f is a local linear function in vote percentages interacted with victory, X_i are controls,

[3] Reflecting their weight in the regression, towns which appear in multiple elections are counted separately each time in the summary statistics.

and ε_i are error terms clustered by election. By comparing towns that narrowly won to those that narrowly lost, α captures the causal effect of a county seat election victory on agglomeration measures. Controls typically include county fixed effects, geographic controls: town latitude and longitude relative to the county center, log distance to the county center, miles to the nearest stream, and mean elevation. In these cases, the county is defined as the pre-election county to remove the possibility of "bad controls" if county boundaries responded to election outcomes. For comparability, we use a single default bandwidth are selected via the Calonico et al. (2014) procedure for log[4] town population with state fixed effects as controls. Section 7.3 shows robustness to bandwidth choice.

4.2 Sample Selection

Our baseline sample includes the first[5] election for each county as well as any elections that occur 25 years or later. We select this sample for two reasons. First, the initial election and its timing are determined before election results are known, thus the sample criteria are exogenous to results. In contrast, a criterion such as selecting only the last recorded election could imbalance the sample in favor of counties which repeatedly contested the seat until they were victorious. Second, although 82% of counties in our sample only hold a single election, a small number of counties held a large number of repeated elections early in their history. Our criteria thus do not overweight these counties. We empirically test for balance in Section 4.3 and find no statistically-detectable violations and explore sample selection choice in Section 7.3.

4.3 First Stage and Balance

As a preliminary step, we confirm that the elections in our sample typically determined the modern county seat and show no empirical signs of imbalance. Figure 2 shows the RD plot and indicating victory leads to an 76 percentage point increase in being the modern county

[4]For this and other outcomes that include zero in their support, "log" denotes $\log(\max(1, x))$, i.e. we bottom-code the logged value at 0. Relative to other transformations such as $\log(1 + x)$ or $\text{asinh}(x)$, this transformation puts no emphasis on changes from 0 to 1 and exactly preserves the logarithm's properties elsewhere.

[5]For this purpose, we include multiple rounds of runoff elections as a single election

seat. We should not expect a 100pp effect here for two reasons. First, most states under some conditions allowed for subsequent elections to reverse a result. Second, when counties were subdivided, losing contestants could successfully become the seat of a new county. Still, the 76 point estimate is quite high and consequently we present many subsequent results as reduced form impacts of victory. This latter procedure would not fundamentally change our results, adjusting the magnitudes upward by about 25%.

In Figure 3, we empirically test for imbalance along a set of pre-election and geographic characteristics and find no statistically detectable imbalances. Hypothetically, such imbalances could have occurred if election results were manipulated or if they biased our data collection process. We consider outcomes on population in the most recent census prior to the election and having a post office in the year prior to the election. For geographic characteristics, we consider elevation, latitude, longitude, and the log distance to the (pre-election) county center, along with several functional form modifications of these. The point estimates are typically small and none approach statistical significance; the highest magnitude among nine z-scores magnitude is 1.2, about what would be expected via chance. In summary, we see little cause to suspect imbalance and we should have confidence in the causal nature of the RD estimates.

5 Results on Towns

Victorious towns changed along a number of dimensions. Most notably, in 2010 these towns become much larger relative to their losing rivals, about a 1.7 log point (5.5 times) increase within 1 km. The size change is economically significant, determining where about 15% of the town's county resides. Within just the close elections in our sample, the results imply the elections determined the residences of 4.5 million people.[6] Winning towns also have more educated populations, higher income, and development of high-skill industries. These results are consistent with theory Kline and Moretti (2014) and shorter-term empirical results (Hornbeck and Moretti 2020). These gains are unevenly distributed as income inequality

[6]We define close elections as those decided by 15 points or less and multiply the modern county population by the treatment effect of winning, adding across all elections in our baseline sample

rises. Finally, the gains are not primarily driven by increases in government jobs as these do not show statistically detectable increases.

5.1 Town Growth

Our results show that historical accidents, in this case close elections, can have long-term consequences on the distribution of population. Victorious towns grew significantly and rapidly as a result of the elections. Figure 4 shows the impact of winning on a town's population as a percent of its county's. The results are both statistically and economically significant, implying that roughly 15% of a county's modern population was determined by the election result.

The town growth result is not at all sensitive to functional form and not driven by a small portion of the sample. Table 2 reports a variety of measures of a town's population, with column (1) replicating Figure 4. Column (2) suggests a 3.1 log point increase in a town's official population , corresponding to a very large 23[7] multiplicative change. Some of this coefficient represents effects from towns which disappear, an effect measured in column (5). However, even with generous bottom coding of values in column (3), the effect remains at a large 1.7 log point (5.3 multiplicative) increase. Column (4) takes a boundary-neutral approach to population, measuring the number of people within 1 kilometer of the town center regardless of official town boundaries and computes a 1.7 log point (5.5 multiplicative) increase. Distributionally, Figure 5 shows that the net effect of election is victory is to roughly take unpopulated or locations with less than 5% of their county's population and increase the fraction of locations with 10% or more of their county's population. Of course, the individual effects may differ substantially from the distributional ones, but the latter serves to illustrate the right-ward shift in the distribution.

It is important to highlight that these impacts are very large, with even the smallest estimate implying a quintupling of a town's population. The changes are also large in an absolute sense, with 15% of the full county's population being affected. Given that government (public administration) jobs on average account for only about 8% of the sample's employment, it is implausible that the subset of those jobs from county government di-

[7] i.e. $e^{3.13}$

rectly account for the changes measured here. As such, population changes are driven by broad-based migration and jobs creation, a topic explored more in Section 5.2.

Migration to victorious towns is quite rapid, consistent with the historical accounts discussed in Section 2. Figure 6 depicts the impact on annual town growth rates per year of treatment. By far the most notable effects are in the early periods, with a 21% annual relative growth rate caused by election victory in the subsequent few years. The second decade's growth increase is also high at roughly 10% annually, but there is a notable decrease going forward. Some caution is warranted in interpreting these figures as US Census procedures for measuring town population were quite variable in newly-settled areas. Although the detailed reports consulted by us and Schmidt (2018) frequently included towns of just several hundred people, in other cases new towns had not officially incorporated and were omitted. This fact likely has the effect of raising some growth rates as the bottom-coded populations may have been higher in actuality. However, this problem would be more severe in later years as populations stabilized and incorporated and were finally included in the censuses. As such, a fuller set of data would likely reinforce the existing pattern by differentially lowering growth rates further out in time.

The timing of the population effects argues for an interpretation of election victory as impacting initial or early-stage migrants to an area. As with the historical accounts in Section 2, populations were very fluid in the early frontier periods and did not have deep attachments to their current locale. In the most extreme cases, victorious towns could entirely absorb the losers, but even the typical case represented large changes in population. County seat elections served as a coordinating device in agglomerating the initial population distribution, one which proved highly persistent over time. In contrast, there seems to be little evidence that the effects stem from later episodes in American history such as the population pressures faced by rural areas in the second half of the 20$^{\text{th}}$ century. With the median election occurring in 1882, the relevant time period of growth to consider are the 50-year effects and later which are quite small and not statistically significant — long-run effects are locked in early.

5.2 Agglomeration and Economic Impact

Over the long run, the massive increase in early population changed the economies of winning towns, increasing their productivity, education levels, and skill mix according to 2010 data. Gains are higher at the top of the income distribution, however, leading to a rise in inequality. For income, we primarily rely on Zip-level statistics of reported taxable income ("adjusted gross income"; AGI) from the IRS. Although Zip Codes need not map neatly onto town borders, these data offer two advantages for our setting. First, there are typically many zip codes even within rural counties, allowing for geographic detail. Second, the administrative nature of the data avoids the problem of sampling rural locations with small populations. With approximately 5% of Americans included in the 5-year ACS, estimates for locations of several hundred people are subject to substantial error.

Table 3 shows results based on the level and composition of IRS income tax data from 2010. A historical election victory and the resulting population influx result in 6.1% higher income, representing an elasticity of 0.15 with respect to Zip-level population density. The primary mechanism for this change is increases in wage and salary income; there is no detectable change in the fraction of income from this source or others such as capital gains. Finally, the fraction of filers who use the public assistance Earned Income Tax Credit (EITC) program does not detectably change. This fact notably suggests that column (1)'s income increases were not shared equally: if they had been, we would expect fewer recipients of the EITC.

Table 4 shows that the income gains demonstrated before occur disproportionately at the top of the income distribution, concording with other research suggesting that urbanization leads to inequality (Baum-Snow and Pavan 2013). Although the Zip-level IRS data do not report a full distribution of income, it does report average income by category allowing for an approximate calculation.[8] Columns (1)-(4) demonstrate that the top 1%, 5%, 10%, and 20% shares of income all increase as a result of historic election victories. These increases are primarily driven by gains among the top 5% as further percentiles show no change in income share. As a final illustration, column (6) shows that the percent of filers reporting

[8]For simplicity, we assume that the average income within each income category is constant and compute shares accordingly.

$100,000 or more in income increases by one percentage point as compared with a sample mean of 8%.

Block-level data on jobs from the 2010 census LODES dataset show that the skill mix in winning locations shifts toward education-intense industries, providing an explanation for the uneven gains in income. Table 5, Panel A presents results of historical election victory on job characteristics. Column (1) shows a 3pp increase in jobs requiring a bachelor's degree, mirroring results on the percentage of degree-holders according to the ACS. Columns (3)-(4) show increases of 3pp and 1pp in the fraction of white collar workers, each using slightly different definitions of the term. The former defines it as industries where 30% or more of workers have bachelor's degrees,[9] the latter uses the "skilled scalable service" industries identified in Eckert et al. (2021) as using highly educated labor and ICT capital. Finally, column (5) shows there is an increase of 3pp in the percentage of jobs in the top tercile[10] of the wage distribution. All these results point to a more white-collar, education-intense economy where the economic gains are concentrated. Panel B provides further support for this view, showing that higher-paying white-collar/bachelors jobs increase as a share of all jobs, but higher-paying, non-white collar jobs do not do so detectably. Lower-wage jobs in some industries fall, though the change is not statistically significant.

Taking stock, historical agglomeration forces led to a more education-intense economies though corresponding gains in pay were not evenly spread. In a similar vein, Ehrlich and Seidel (2018) find that a policy aimed at subsidizing a particular region strongly benefited landowners, Qian and Tan (2021) find that homeowners and highly educated individuals experience welfare gains and renters and less educated individuals welfare losses as a consequence of agglomeration caused by high-tech firm entries. On the other hand, Hornbeck and Moretti (2020) find that local growth in factor productivity reduces inequality. In contrast to these papers, we take a very long-run view of the effects of agglomeration. The relevant counterfactual in our case is changing the initial distribution of population across towns as opposed to established economies receiving a shock to productivity. In that sense, we are

[9] According to the 2010 5-year ACS. Specifically: Education [61], Professional/Scientific/Technical [54], Management [55], Finance/Insurance [52], Information [51], Public Administration [92], Healthcare / Social Assistance [62], and Real Estate [53].

[10] The LODES data does not report detail in wages beyond terciles.

contributing to our understanding of the welfare effects of changing the economic structure of cities and towns.

6 Economic Geography

Having established that county seat elections reshaped economic towns' economies through historical agglomeration, we next consider the interconnectedness of population between locations and spillovers in general. Whether growth in one area should have positive or negative spillovers on nearby locations is theoretically unclear. On the one hand, such growth might help neighbors via increased market access and job opportunities in a higher productivity location (Bosker and Buringh 2017; Beltrán Tapia et al. 2017). On the other hand, a town's growth might depopulate its neighbors via Tiebout-style competition, sometimes termed the creation of an "agglomeration shadow." Cuberes et al. (2019) find that which of these effects outweighs the other can to a large extent be explained by transport costs. As such, the actual interconnectedness present in economic geography is an empirical matter which we explore here.

6.1 Sample Considerations

One theoretical difficulty in assessing the impacts on economic geography is that county seat elections plausibly affected both winners and losers. For example, Section 2 discussed historical examples wherein population directly migrated from the losing location to the winning location. While technically this would mean that winning an election had effects outside the victorious location, it more properly represents the direct effects of treatment rather than a spillover. Therefore, we have to take care not to code effects on losing locations as spillovers.

We accomplish the direct vs. spillover distinction in the following manner. First, for any point in space, we attach it to the nearest of the top two competing towns in an election. We thus compute effects at a distance of "X miles" by comparing locations X miles from close winners relative to locations X miles from close losers in an RD framework. Since losing locations are, of course, closest to themselves at 0 miles, this strategy ensures that any impact on losing locations is not mistakenly recorded as a spillover from the winning location.[11] For additional simplicity, we also exclude areas in between the competitors. It is unclear, for example, if a location three miles from a winner and two miles from a loser would expect the same effects as one three miles from a winner but much further from a loser. Figure 7 illustrates this procedure for two towns in the dataset, with 0.1 mile radius circles drawn around each location center.

6.2 Effects on Communities and Population

The large influx of migrants to winning towns did cast an agglomeration shadow on nearby communities, leading to their depopulation or abandonment. The effect is statistically detectable over the long run and economically meaningful in size, albeit modest relative to the changes in competing towns. These effects are compensated by increases in the primary location as well as its rural environs, meaning average changes in total population are consistently positive or zero.

To measure population in a consistent manner over the past two centuries, we turn to the post office data from (Blevins 2015). As pointed out by the latter paper, whereas the census only documented smaller, unincorporated communities infrequently, the US Postal Service typically made sure to have an office even in these areas. Thus, post offices serve as markers for populated communities.

Figure 8 shows the effect of a county seat election victory on the number of post offices at varying distances from the center over time, defining distances as discussed in Section 6.1. To properly understand the effects in communities beyond the competitors, we focus on areas at least one mile from the center. Across a large majority of distance and post-election periods, the point estimates are negative and typically become significant in the long run, 100 years or more after the election date.To avoid inconsistent samples across time periods, we assume that any post office currently open will remain so in the future. After 150 years, victory reduces the number of post offices open between one and ten miles from the competing town

[11] For example, if the winning and losing towns were 3 miles apart, simply comparing all areas 3 miles apart would artificially produce spillovers. Areas 3 miles from the loser would look large due to the winning location and vice versa.

center by 0.23, representing about 15% of the sample mean. Thus, smaller communities were sometimes depopulated by the agglomeration shadow cast by the county seat.

The negative population impacts on smaller communities are canceled by boosts to winning towns and their rural environs over the long run. Figure 10 depicts effects on 2010 populations from census blocks,[12] presented as elasticities with respect to the log population within 1km of the town center.[13] Several results become apparent. First, total population effects are positive with maximum elasticities around 1 and shrink to 0 about five miles from the town center. There are similar effects on the population within towns (census-designated places, CDPs), though with a maximum elasticity of 1.5. The effect on competing town population remains positive, if small, ten miles out, driven by a small number of sprawling cities. Consistent with this longer radius, population in non-competing towns shrinks with elasticities around -0.2. Notably, this graph in isolation would not imply the existence of an agglomeration shadow as it could simply represent areas absorbed into the main urban hub. However, the negative effect is consistent with the post office regressions shown in Figure 8. Rural populations increase roughly one mile from the town center with elasticities shrinking from 0.2 to 0 at 5 miles from the center. We see very similar results using "lights at night" pixel brightness with high effects at the competing centers which decay to approximatley zero after four or five miles.

Overall, these figures provide evidence for spatial diffusion of population effects, including large effects on the winning town itself and a modestly negative agglomeration shadow effect which depopulated smaller communities nearby. This effect is not constant over time and primarily occurs 25 years or more after the election, underscoring the importance of considering events over the long run. While the effect measured by post offices is economically meaningful at 15% of the sample mean, the negative effects are frequently offset by gains to the county seat and its immediate rural environs. Thus, the average effect on population remains positive until around five miles from the town center.

[12] Since blocks may be split among multiple circles, we assume a constant population density within each block to compute population.

[13] We implement this by modifying our main specificiation to a fuzzy regression discontinuity with victory as the instrument and log block population as the independent variable.

6.3 Economic Effects

The primary economic impacts of historical agglomeration occur in the competing towns themselves with the relatively smaller spillover effects leading to few structural changes in the present day. We thus find little evidence for theories where high levels of agglomeration become a hub for growth in a broader area or where such growth merely draws talent from surrounding areas. To examine economic spillovers, we primarily rely on the 2010 Census LODES data on jobs, coded on the basis of worker residence. Since these data are available at the block level, they represent one of very few data sources which can detail economic outcomes flexibly over space. Areas of economic agglomeration and productivity are characterized by high land rents corresponding to high wages to maintain the spatial equilibrium. We use Zillow ZTrax data from 2018-2020 to examine the geographic spillovers in terms of house prices, focusing on transactions of single-family homes. Because Section 6.2 illustrated that urban communities may respond endogenously to the election, we cannot easily pre-select these areas without compromising causal inference. Most other sources of economic data are highly aggregated to at least the subcounty level, making it difficult to detect spillovers within the five- or ten-mile range on which we focus.

Figure 10 examines the spatial distribution of present-day changes in job type and housing prices, looking at bachelor-level work (panel (a)), top tercile wages (panel (b)), and single-family homes sales prices (panel (c)). Although all panels show meaningful gains in these figures within a mile or so of the town center, but no detectable effects at further distances. While panel (b) does show statistically significant effects at 6 miles out, given the lack of other effects at similar distances in either panel, these are more plausibly due to chance. Tracking the pattern of education and high-paying jobs and confirming theoretical intuition, single-family home prices are up to 50% higher within a mile from the town center, but notably decaying to roughly zero about a mile out as with the other outcomes.

Taken together with the moderately-sized population spillovers described in the preceding sections, these results show that the primary economic effects of historical agglomeration occur in the centers of concerned towns rather than as spillovers. The lack of negative spillovers on nearby areas suggests that the gains in well-paying jobs are not zero-sum transfers from nearby, but rather a benefit created by population growth over a long-run period.

7 Alternate Mechanisms

In this section we address potential alternate explanations for our results, including the direct effects of the county government, increased provision of services or public goods, and changes to transportation networks.

7.1 Direct Effects of the County Seat

We first consider that the direct possession of the county seat could boost incomes or employment beyond the historical migration effects that we observe. The present-day effects we estimate would then be more akin to a "capital city" effect with growth due directly to government administrative employment and spending (Bai and Jia 2021) or other unique abilities to attract private investment (Bluhm et al. 2021; Campante and Do 2014). We examine both of these aspects in turn.

7.1.1 Government Sector

The most obvious alternative to our story of long-run agglomeration would be the direct effects of county government jobs provided by the county seat. In this view, the causal effects presented should be primarily understood as the transformation of economies that are governmentally-focused.

A priori, the small size of the relevant governmental sector means its growth is unlikely to drive the population and economic changes we document. Table 2 showed that population density more than quintuples as a result of an election victory. Yet, only 8.2% of the sample works in public administration among victorious locations in our sample. Thus, the vast majority of marginal jobs are created in other industries. Even this figure is a large overstatement as it does not specify whether the federal, state, or county government employs the worker. Among county employees, the largest occupations are police officers and public school teachers rather than the legal or administrative jobs most associated with the county seat.[14] As noted in Section 2, these jobs only account for 0.3% of national employment. Finally, Table 5 is unable to detect a causal effect of election victory on the fraction of public administration jobs, with the point estimate being very close to 0. The relevant governmental sector is overall quite small and unlikely to drive broad economic changes.

Changes in the characteristics of public sector jobs also do not drive our results. In theory, even if the county government sector did not expand, if its wages increased it could increase average incomes. Table 6 considers this question but finds little evidence for it. It replicates the outcomes of wage terciles and white collar industries[15] of Table 5, both among all jobs and among jobs outside public administration. Removing public administration does not substantially affect observable job characteristics: there is a slightly larger shift into white collar industries (3.5pp versus 3.0pp) and a slightly smaller shift into high-paying jobs (2.5pp versus 2.9pp), but these differences are not statistically significant.

7.1.2 Other Unique Aspects of Government

Even if the government sector does not directly matter for our effects, its presence could still affect economic decisions. For example, firms and individuals might find it more convenient to locate near governmental offices.

We show in Figure 11 that county seat effect is primarily focused in elections held before or shortly after county creation. In this period of early settlement, populations were much more transitory, meaning that even small changes in a county's political structure could induce large migration and corresponding agglomeration effects. In contrast, once 30 or so years pass after county incorporation, county seat elections do not detectably change population or IRS-reported income. Although the smaller sample sizes make it difficult to pinpoint an exact year where effects become economically insignificant, the sharp drop in estimates makes it clear that the effect sizes are small after a certain period. In contrast, county seat status reliably changes regardless of time period. These two observations cut against the notion that the effect is due to the seat per se, rather than the large migration impacts observed only in early periods.

[14]The LODES data do not provide information on the identify of government employment or job characteristics beyond 2-digit NAICS codes.

[15]The LODES data does not include cross-tabs of educational requirements with industry.

7.2 Service Provision

Another explanation for our results is that the position of county government enabled towns to obtain a disproportionate share of public goods as discussed in Ades and Glaeser (1995). For example, county officials might have influenced railroad officials to connect their town or prevented the closure of post offices, making use of their special positions to do so. Our data show that this explanation is unlikely, however.

7.2.1 Continuous Measures of Public Goods

We begin by examining cases where the measure of public goods scales naturally with population, turning to Common Core school district data for outcomes. Here we can consider outcomes such as teachers or revenue on a per student basis to determine whether county seats systematically directed public funds toward their populations. As with other measures, the outcomes here are for the modern period of the 2017-18 school year.[16]

Table 7 shows the results. Unsurprisingly, the number of students in the district markedly increases, although by notably less than the population of the winning town; the attenuated coefficient here is not surprising as low enrollment districts would likely be merged together for administrative scale. In terms of the money available for students, the results suggest there is little difference. The point estimates imply winning results in a 1.8% expenditure increase per student but also 1.5% lower revenue. As neither of these are statistically significant and are opposite in sign, they suggest little real difference in school fiscal performance. Except for the subcategory of state revenue, which decreases, all sources of revenue per student are not detectably changed. Finally, column (8) suggests a small but statistically significant decrease in teachers per student with about one fewer teacher per 240 students or 5% of the sample mean. This latter fact may represent a real loss to schools in a district or simply an "integer problem" that in very small classrooms it is impossible to have fewer than one teacher.

The results cut strongly against the notion that county seats manipulated public funding for their schools. The key estimates of revenue and expenditure per student are not

[16]The earliest for which all key data are available.

detectably changed, despite a rise in students. Most major sources of revenue, including property taxes and other local funds, are also not detectably changed. If anything, a small drop in the teacher to student ratio would suggest a slight decrease in school quality, if anything.

7.2.2 Lumpy Public Goods

We also argue there are no notable differences in the presence of lumpy (i.e. discrete) public goods, adjusted for population. Here we study the presence of modern roads, railroads, and post offices. These public goods naturally have some relationship with population, though it is less natural to report the presence of the outcome divided by population as we do in the last subsection.

Figure 12 plots the relationship for the presence of the public good in 2010 with respect to log town population for close[17] election winners and losers in our main sample. Similar relationships hold both unconditionally and when the full sample is residualized on county fixed effects and geographic controls.[18] Conditional on town population, close winners are slightly more likely to have a post office, slightly less likely to have a railroad, and about as likely to have a road connection. The relationship thus shows little evidence of manipulation of public goods toward county seats.

Because we study a single historical shock, it is important to acknowledge the limits of the conditional analyses we can conduct here. Small towns are naturally much less likely than larger towns to have public goods like railroad connections, meaning that the large population increase from an election victory mechanically produces higher provision in an absolute sense. However, such an effect says little about manipulation — a losing town that grew for other reasons would also become connected. On the other hand, Figure 12 is conditioning on a downstream control which can lead to biased estimates. Absent a second population instrument, it is thus not possible to rule out manipulation in a strict statistical sense. But given that the relationship of public goods and population is so similar among close winners and losers, such biases would have to roughly cancel to zero. Such cancellation

[17]Within the default RD bandwidth.

[18]Both the outcome and (logged) town population are residualized. For aesthetic purposes, we shift the residuals by a constant such that the mean of each variable among election losers is preserved.

would also have to hold when residualizing on control variables as this action preserves the similarity in relationships.

7.3 Specification

Our results are robust to a number of different econometric specifications. Although Section 4.3 showed no detectable imbalances in geographic or pre-election characteristics, other arbitrary choices in specification could theoretically drive our results, diminishing their meaning. We thus test for robustness on a number of dimensions.

Figure 13 shows that bandwidth choice does not fundamentally change the results of two of our main variables: population density and (log) income. For the first outcome, decreasing the bandwidth from 20[19] to 5 modestly raises the effect size while leaving all results highly significant. For the second, decreasing the bandwidth again largely leaves the point estimate unchanged, although with the smaller sample size at lower bandwidths, the estimates lose power and do not achieve statistical significance.

Table 8 explores robustness to various election sample procedures. Each column shows the effects on log population density within 0.5 miles, remaining careful to choose only criteria determined at the time of the first election. All samples include the first election results and successively include more elections based on years since the first election, with column (3) representing the baseline sample. Panel (a) presents results as fuzzy RDs with the independent variable being whether the location is the modern seat; panel (b) presents reduced form estimates of victory. Keeping in mind that sample changes change the population underlying the LATE and thus we should not expect identical coefficients, the estimates are fairly similar. In panel (a), modern seat effects range from 2.1 to 2.3 log point effects on density, all highly statistically significant.

Our estimates are also not sensitive to the inclusion of controls. Table 9 panel (a) shows three of our main outcomes, comparing results with and without county fixed effects and geographic controls. Control variables do not appreciably change any control estimate, at

[19]Note that a small number of communities are not geolocated outside of the 15-point bandwidth, meaning that their outcomes are generally missing. Since this missingness may be endogenous to the election result, we prefer bandwidths within 15 points but include up to 20 points for completeness.

most moving it by 0.3 standard deviations. Control variables do make the estimates more precise, however, and in the case of income make the estimate statistically significant (though while barely changing the point estimate).

These properties are not shared by simple OLS estimates of our primary relationships. Using the same sample as the RD estimates, including controls in the OLS estimates typically changes estimates by an average of 1.8 standard errors. As such, all of the sign, significance, and absolute value of OLS estimates are quite sensitive to controls, indicating significant bias from omitted variables. When population has accumulated more in places that have characteristics making them (unobservably) better at economic production today, we expect a positive bias. When population has accumulated in areas (unobservably) worse at economic production today, we expect a negative bias. For example, Bleakley and Lin (2012) notes that US population historically grew at key sites for water navigation and persisted, even though such advantages have become obsolete. This situation is most applicable for comparing our RD and OLS estimates, with the latter showing a more positive relationship between density and income or high-education jobs.

8 Conclusion

This paper studies the long-run causal effects of historical agglomeration using a universal event occurring on the American Frontier, namely the county seat election. County seat elections determined the location of county government and gave winning towns a reputational boost that sufficed as a coordinating device to cleanly agglomerate population.

Establishing causality for historical shifts in population is difficult and requires variation in agglomeration that is independent of underlying geographic fundamentals. We employ a close election regression discontinuity design, comparing towns that narrowly won the county seat election to those that narrowly lost. We assemble a novel data set featuring county seat vote outcomes that we link to census records. We find that historic county seat victories overwhelmingly determine the location of county government in the present day and that winning towns increase their population fourfold and about 15% of a county population's location is a response to county seat elections. This increase is much larger than the directly expected increase in population following the location of jobs in county administration.

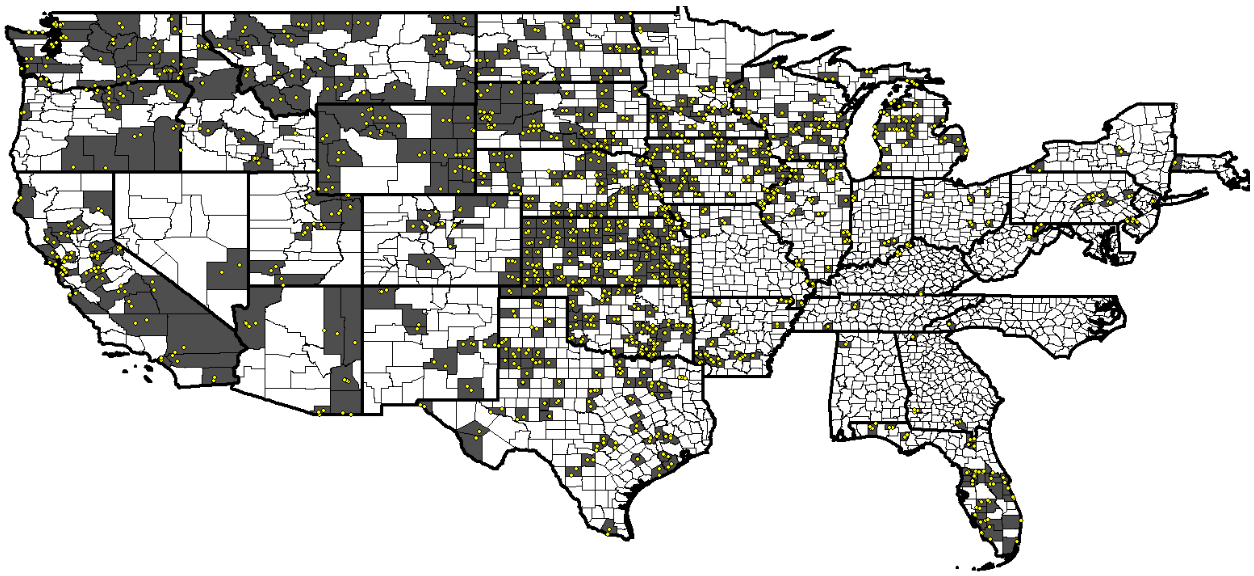
Next, we study how this sizable population increase impacted the structure of winning towns over the long run. Our results imply that winning towns experienced increases in income with their economies shifted towards education-intense industries. These gains are not distributed evenly, however, and occur disproportionately among upper incomes, leading to increased inequality. We rule out that our results are driven by other potential mechanisms such as government service provision or imbalances in the sample prior to the elections.

Our data and setting also allow us to examine the spatial spillovers of county seats on their surrounding areas. Communities near county seats suffer from depopulation over the long run, as measured by the presence of post offices. However, this effect represents a moderate drop of 15% of the sample mean and does not appear to result in job structure in these areas. Average population effects are also positive for around five miles from the competing town center, primarily driven by growth in this town and its immediate rural environs.

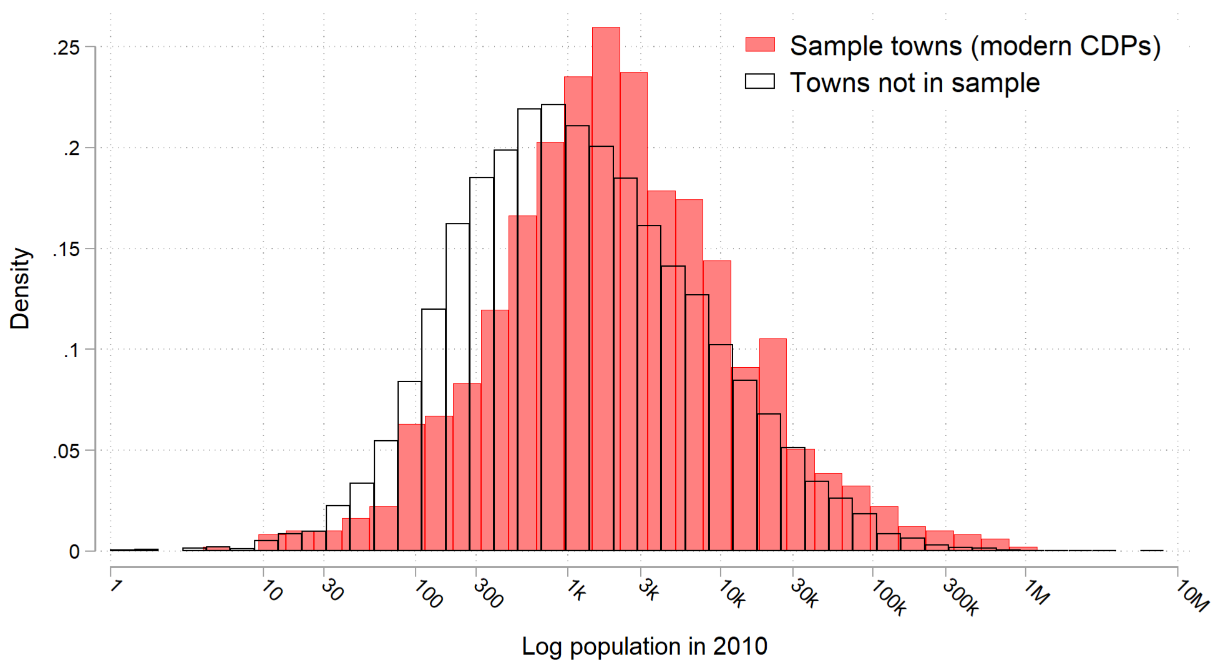
Overall, our results suggest clear benefits to historical agglomeration in terms of income and high-skilled jobs for individual towns and surprisingly moderate spillovers elsewhere. We contribute to a large literature this topic both with our focus on long-run, causal estimates and on expanding the focus to towns smaller than those typically studied. Roughly 40% of America's population lives in rural areas or towns under 10,000 people and expanding the study of agglomeration to these areas is important in its own right. Our results make clear that historical migration shaped the long-term economic and population structures in these parts American West in ways that are still apparent in the 21^{st} century.

9 Tables and Figures

Figure 1: Sample Locations and Modern Town Populations

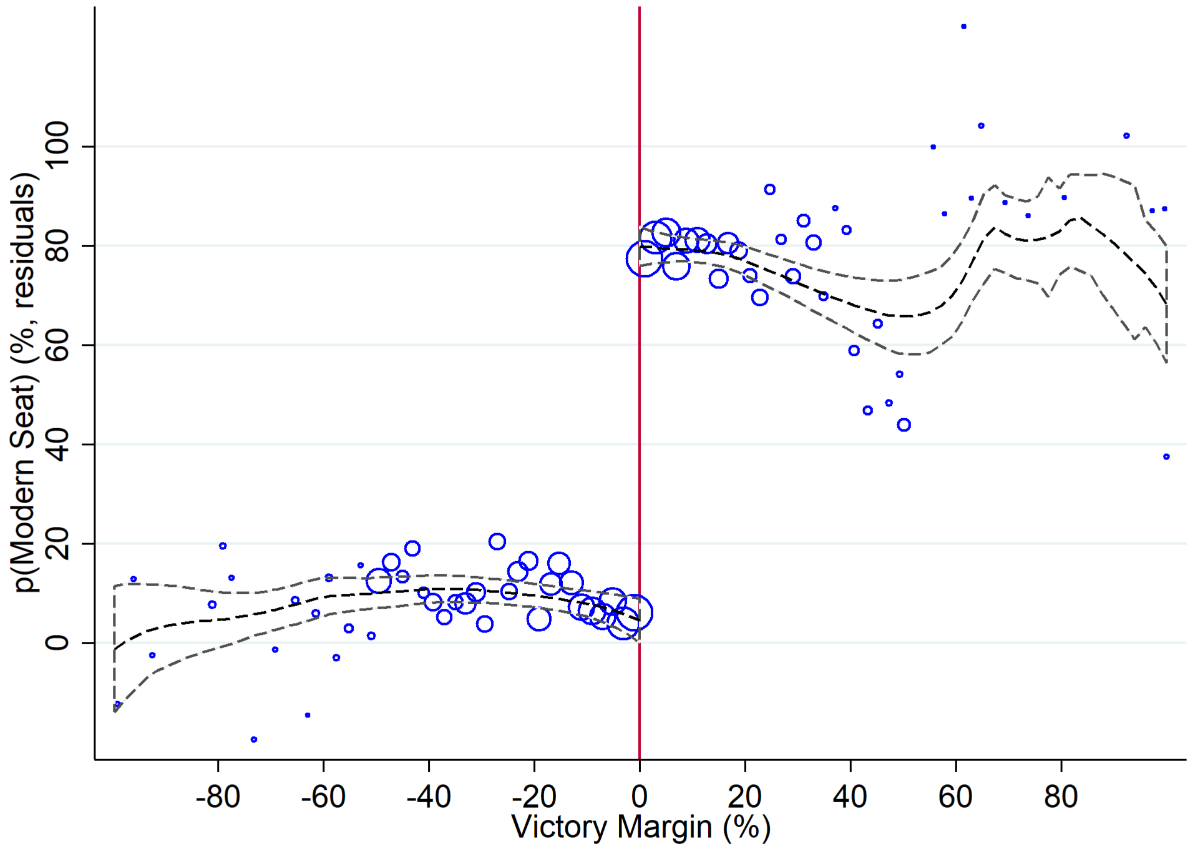


(a) Sample Counties and Towns



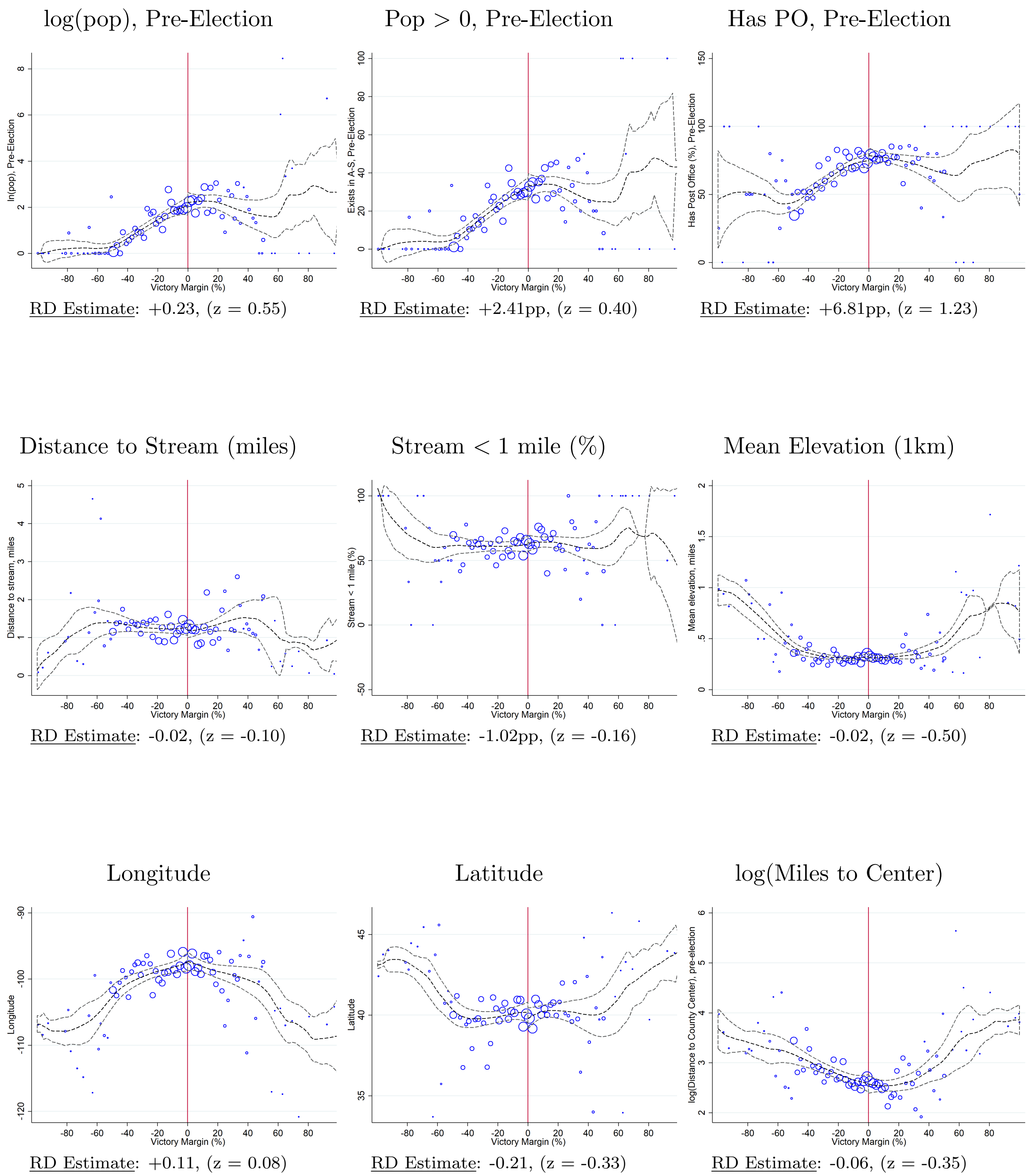
(b) Sample and non-Sample Town Size

Figure 2: Impact on Being the Modern County Seat



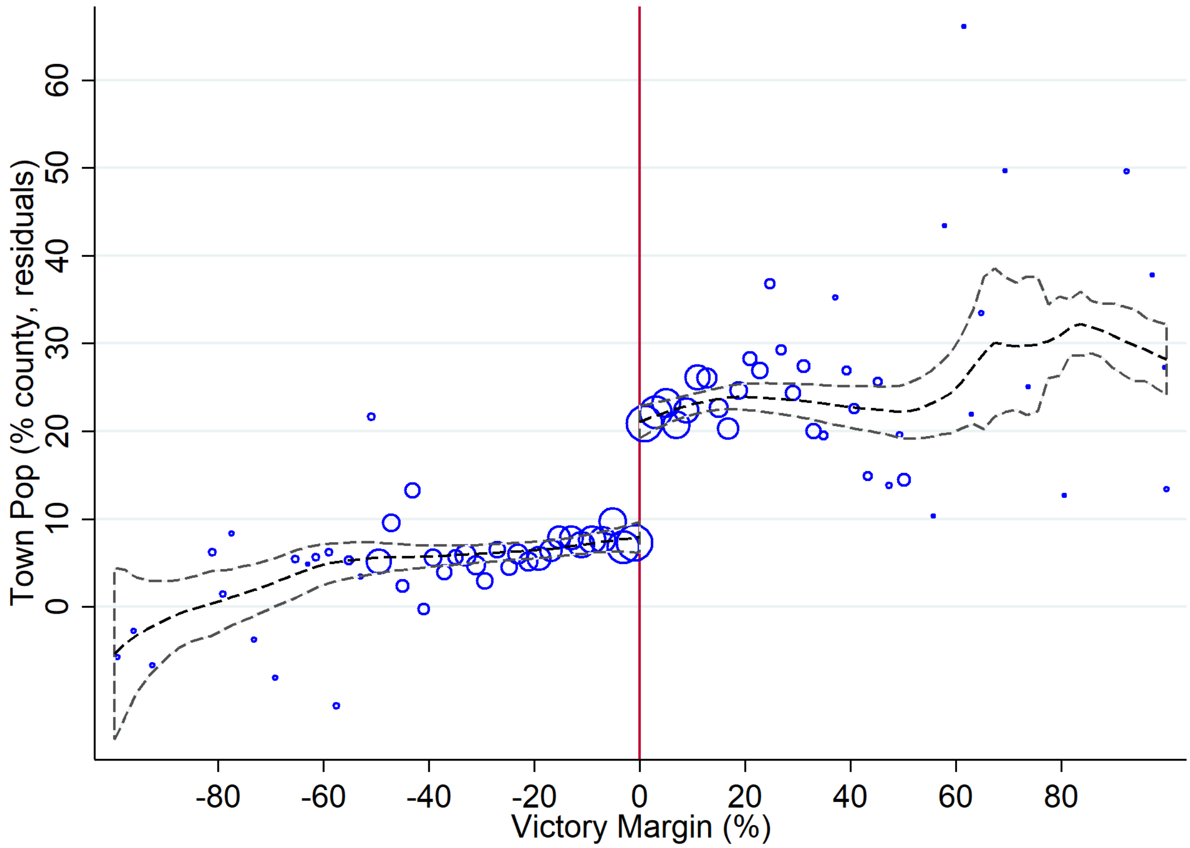
Notes: Binned scatter plot and local polynomial fit on being the modern county seat. Y axis plots residuals after county fixed effects and geographic controls as discussed in Section 4. RD Estimate: +76.06pp, (z = 18.79)***

Figure 3: RD Balance Tests — Multiple Outcomes



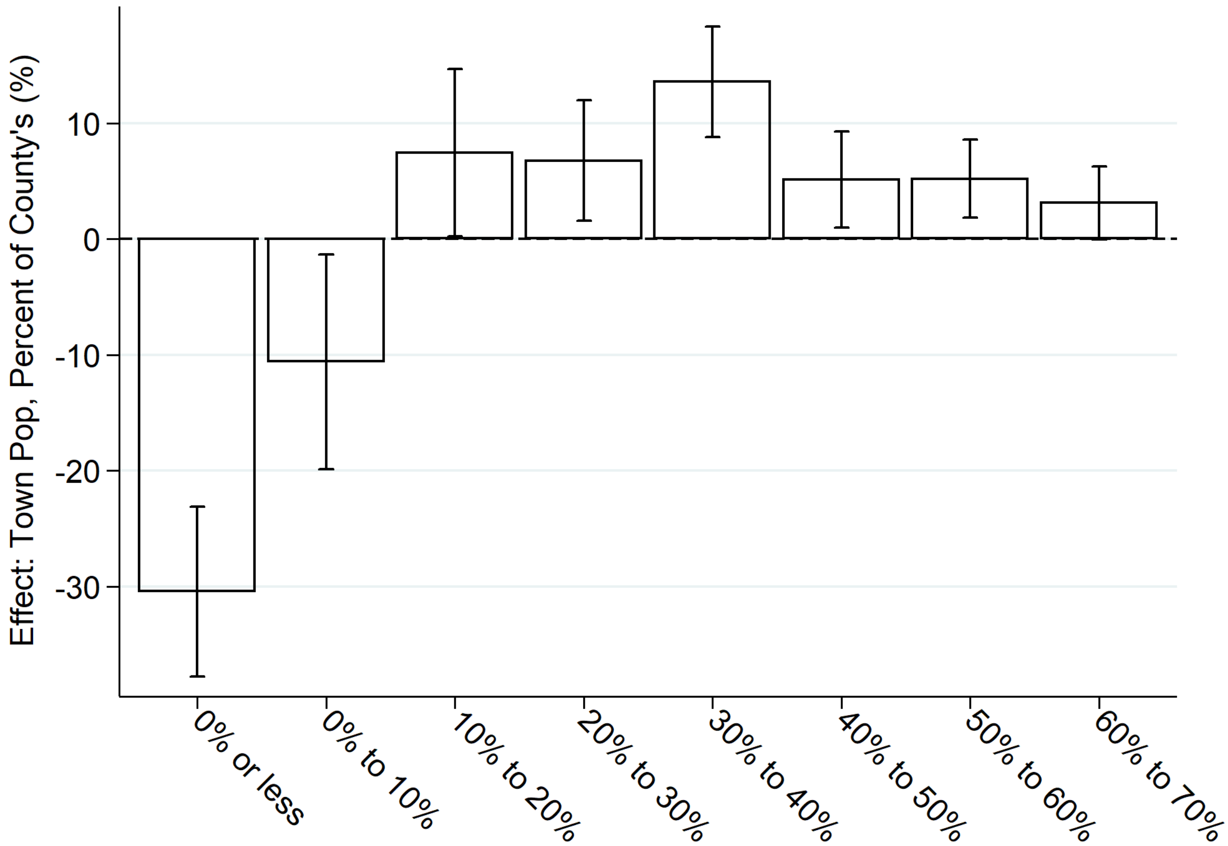
Notes: Binned scatter plot and local polynomial fit for multiple pre-period or geographic characteristics. RD estimates and z-scores are presented below as text.

Figure 4: Impact on Town Population (% County)



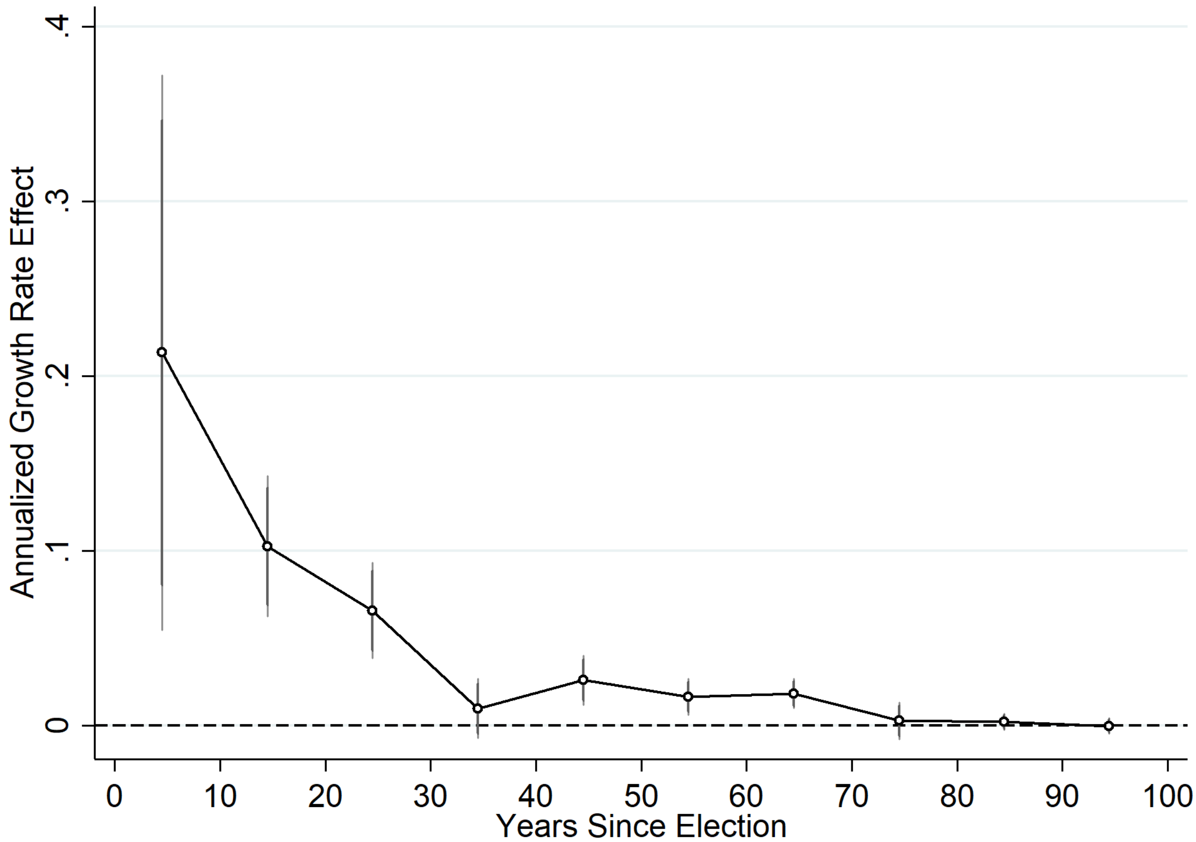
Notes: Binned scatter plot and local polynomial fit on a town's population in the 2010 census as a percent of its modern county's population. Y axis plots residuals after county fixed effects and geographic controls as discussed in Section 4. RD Estimate: +14.58pp, (z = 8.33)***

Figure 5: Impact on the Distribution of Town Population (% County)



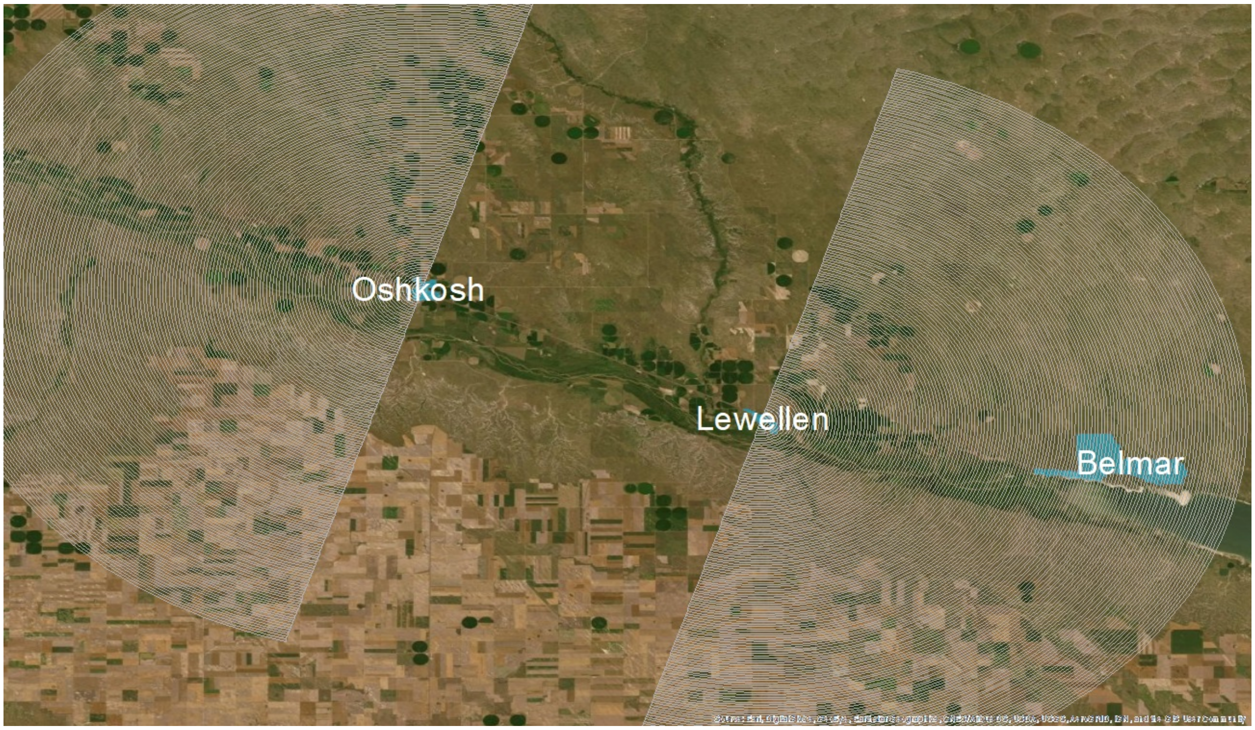
Notes: This figure plots RD estimates on whether a town's population falls within a specific range of a percent of a county's total. County fixed effects and geographic controls are included as discussed in Section 4.

Figure 6: Impact on Town Annual Growth Rates Over Time



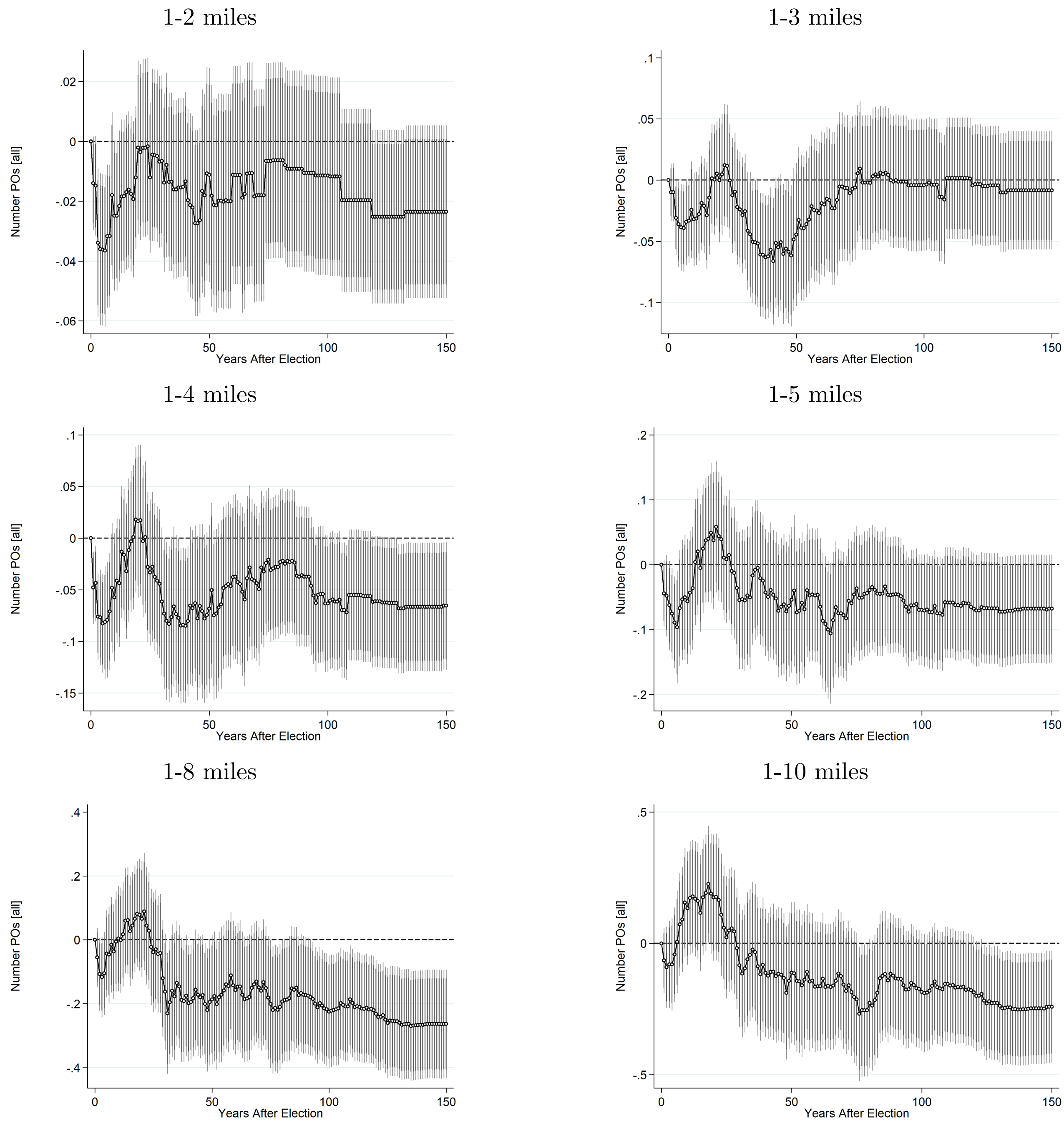
Notes: This figure plots RD estimates on a town's annual growth rate according to census figures. Growth rate treatment effects are defined as the the difference in $\log(max(1, \text{pop}))$ divided by years of treatment, with the election year itself counted as a treatment year. Effects are depicted by decadal bins, reflecting the decadal nature of census data. County fixed effects and are included.

Figure 7: Separating Space



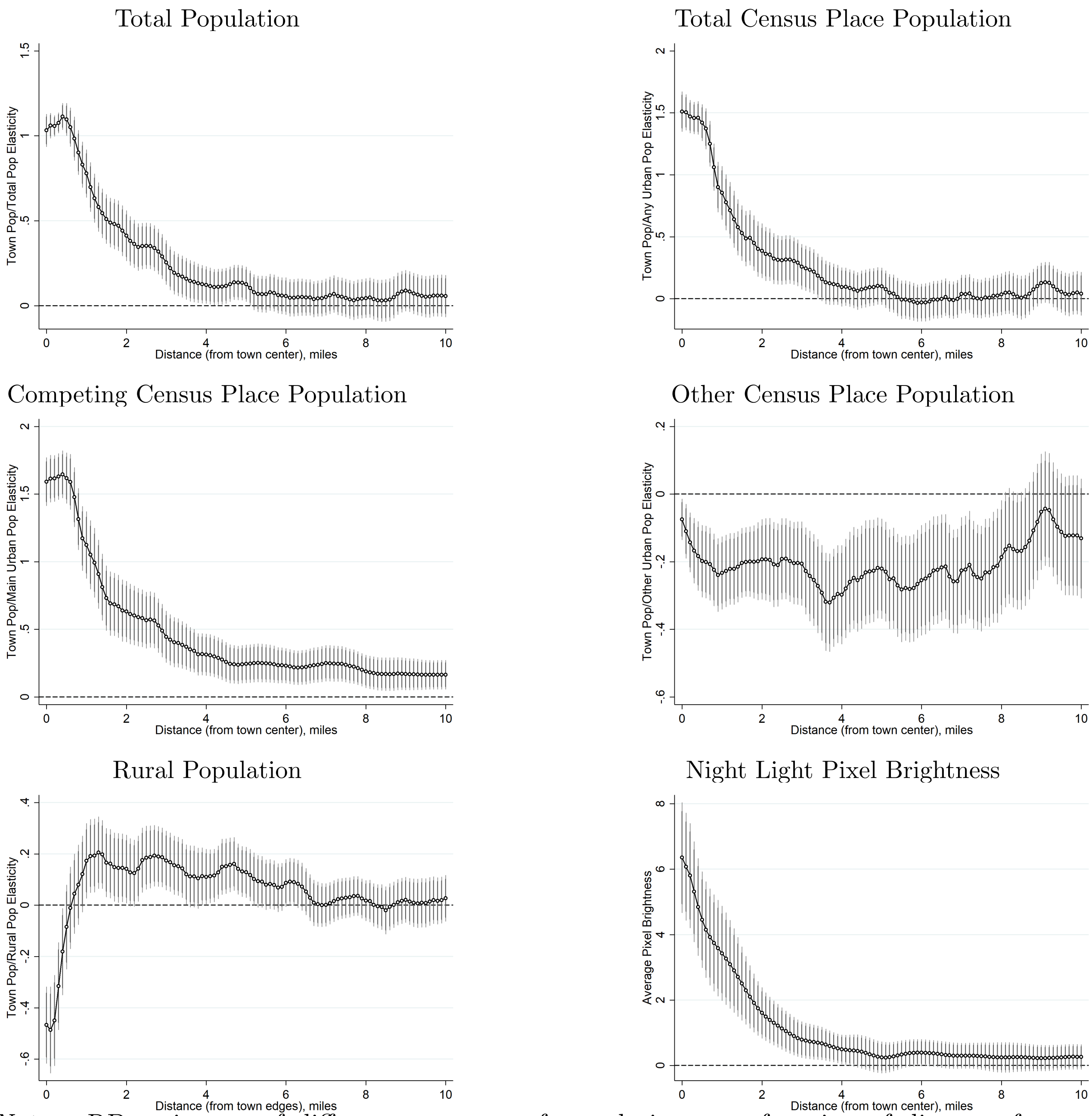
Notes: This figure illustrates the attachment described in Section 6.1. Panel (a) shows the election between Oshkosh and Lewellen; rings indicate areas of fixed distance ranges from the town centers. Areas are attached based on the closest town. Area between the two towns is excluded.

Figure 8: Spillover Effects on USPOs, Distance to Center



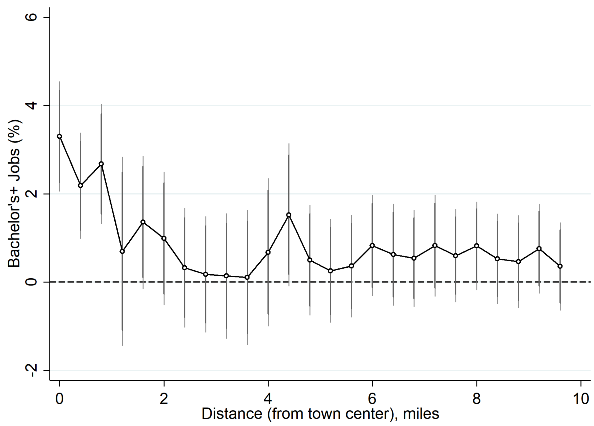
Notes: RD estimates on the number of post offices within specified distance range of competing town centers. Number of years after the election shown on the x-axis. Confidence intervals of 90% (thick line) and 95% (thin line) shown over each estimate. Controls include the geographic characteristics listed in Section 4, county fixed effects, the number of pre-election post offices open at each mile interval from the town, and the area of each mile distance segment.

Figure 9: Spillover Effects by Population Category (Elasticities)

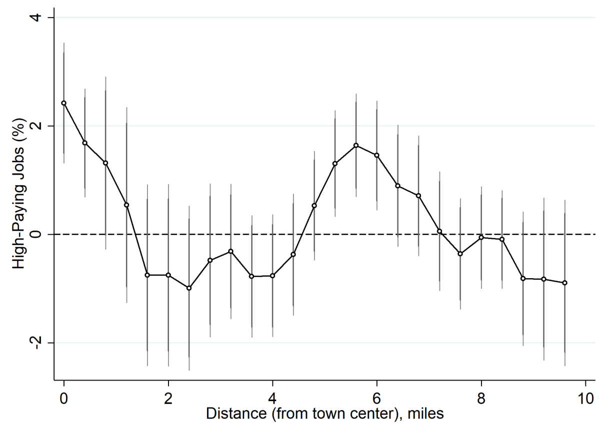


Notes: RD estimates of different measures of population as a function of distance from competing town centers. Elasticities with respect to the population within 0.5 miles of the town center are computed using a fuzzy RD. Since outcomes can include 0, we bottom-code population values at 1. We categorize populations based on whether the block is coded as a Census-Designated Place (CDP) and whether the CDP is part of the competing town. For this purposes, we consider all contiguous CDPs to be a single unit. The final graph shows the average pixel brightness of lights at night in reduced form (non-elasiticity). Confidence intervals of 90% (thick line) and 95% (thin line) shown over each estimate. Controls include the geographic characteristics listed in Section 4 and county fixed effects.

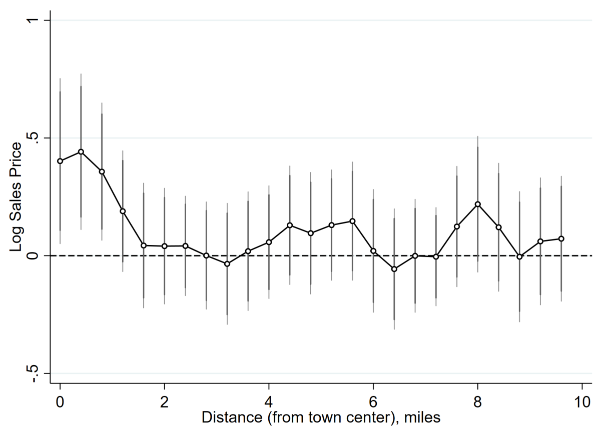
Figure 10: Economic Spillovers Across Space



(a) Jobs Requiring a Bachelor's Degree



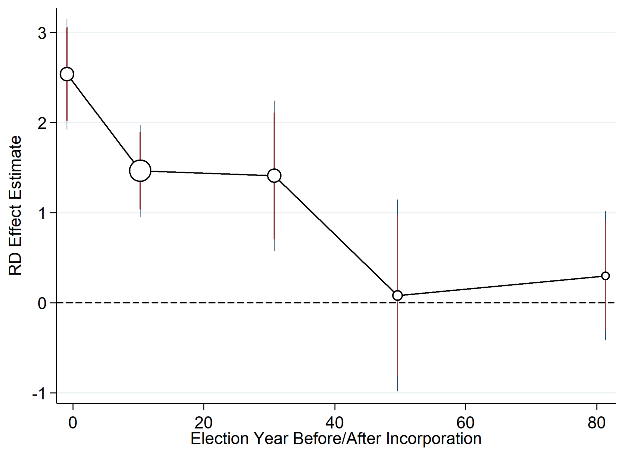
(b) Top Tercile Wage Jobs



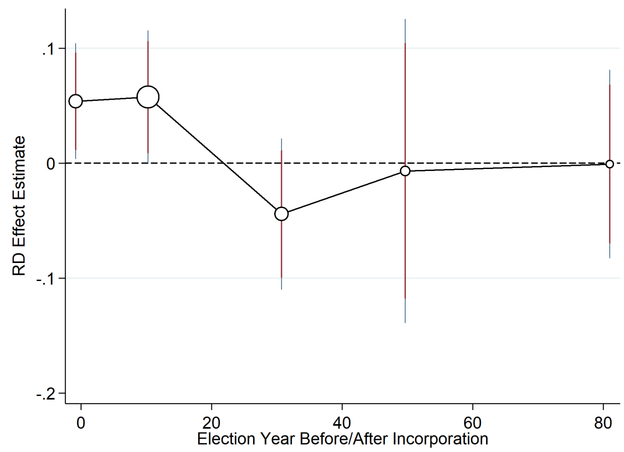
(c) Single-family Home Sales Prices

Notes: RD estimates of the percent of jobs requiring a bachelor's (panel (a)) and with a top tercile salary (panel (b)) as a function of distance from competing town centers. Panel (c) shows RD estimates of the log of sales prices (from Zillow ZTrax data) of single-family homes from 2018-2020 as a function of distance from competing town centers. Reflecting data limitations, sales observations are restricted to Kansas and Iowa only. Confidence intervals of 90% (thick line) and 95% (thin line) shown over each estimate. Controls include the geographic characteristics listed in Section 4 and county fixed effects.

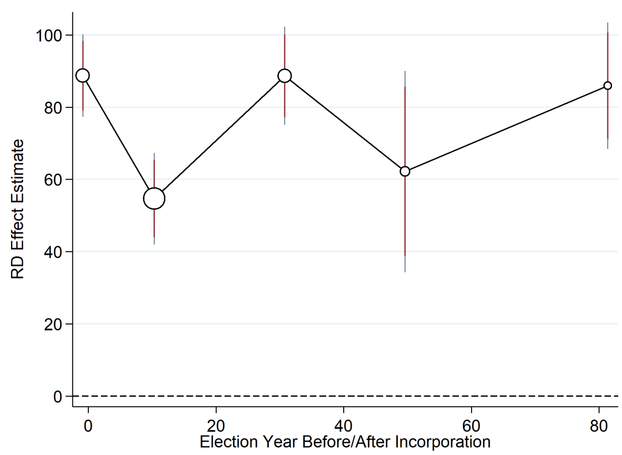
Figure 11: Timing of Effects Relative to County Creation



(a) (log) Population Density within 0.5 miles



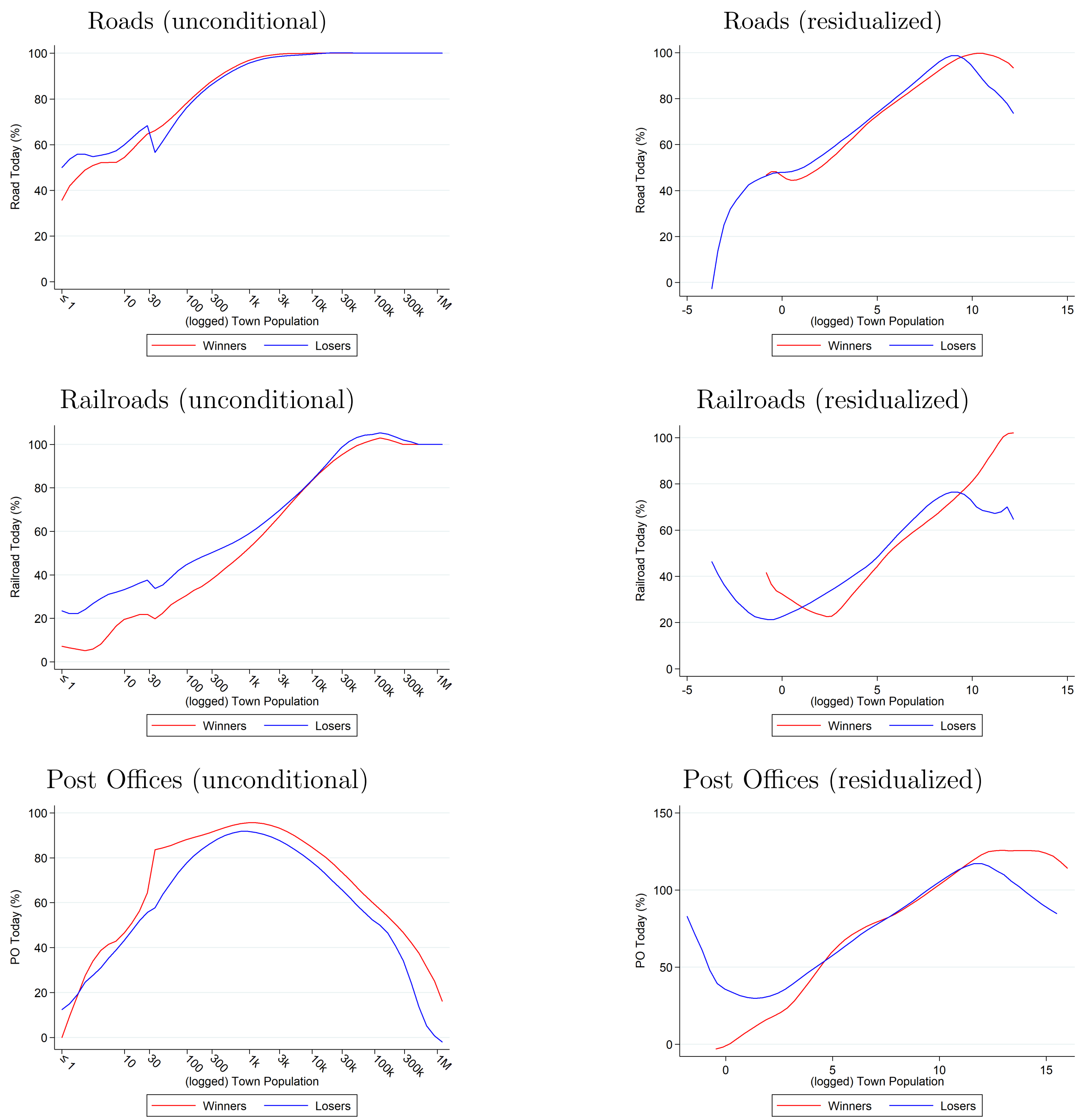
(b) (log) Adjusted Gross Income (IRS Zip-level)



(c) Is Modern Seat (%)

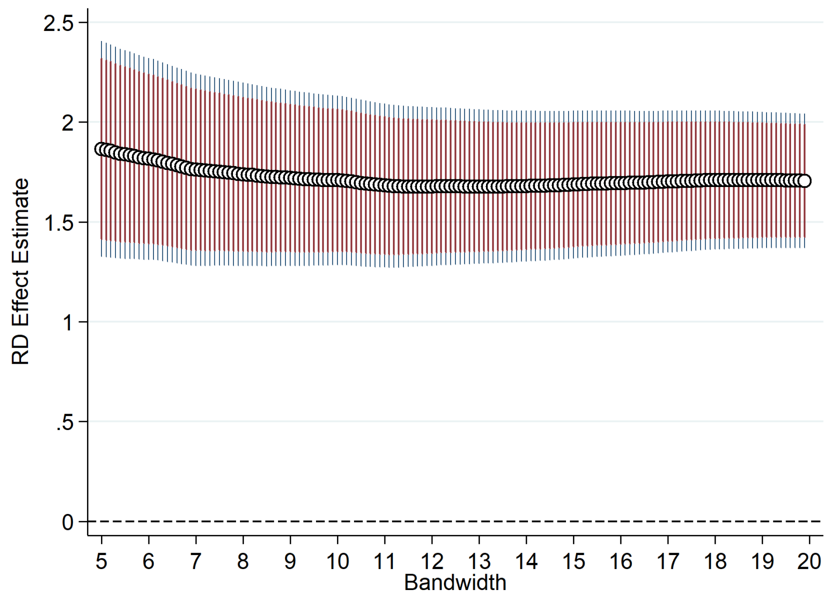
Notes: RD estimates on log population of census blocks within 0.5 miles of town centers (panel (a)), log adjusted gross income (AGI) at the Zip-level (panel (b)), and the identity of the modern seat (panel (c)). Each point represents a regression on a subset of elections based on their year relative to county incorporation. x-axis values represent the average relative year of the election within the bin; marker size reflects the number of observations within the bandwidth of the regression. The first bin contains all elections that occur within one year of county creation or earlier. Subsequent bins are grouped in 20-year intervals, with those over 60 years after county incorporation being grouped together. Confidence intervals of 90% (thick line) and 95% (thin line) shown over each estimate. Controls include the geographic characteristics listed in Section 4 and county fixed effects.

Figure 12: Public Goods versus Population, Election Winners and Losers

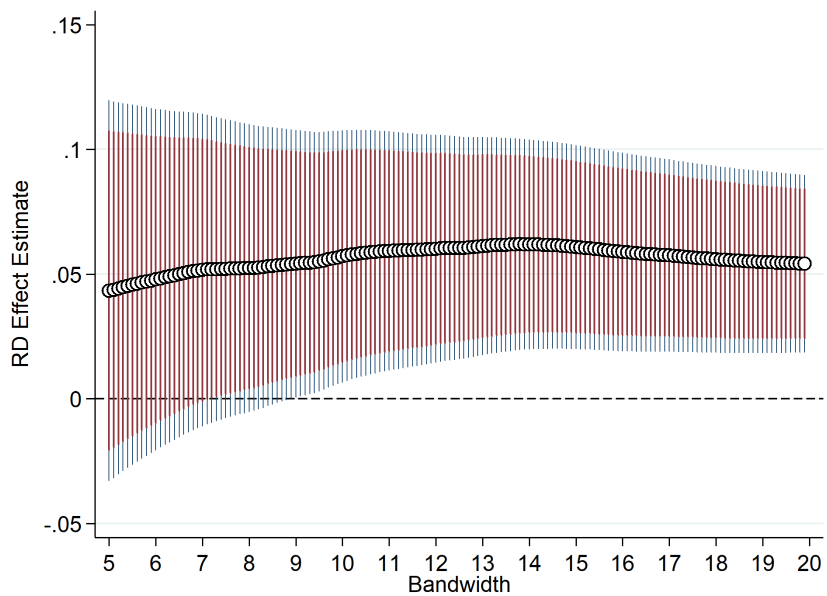


Notes: Relationship between various measures of public goods and population for close (within the defaul RD bandwidth) election winners and losers. Each line represents a local linear regression of the presence of the feature on log formal town population. The outcomes are respectively a road or railroad in the modern CDP or within one mile of the location; a post office within one mile of the center. The left graphs present the unconditional relationship, the right graphs show the relationship on residuals after regressing on the geographic characteristics listed in Section 4 and county fixed effects.

Figure 13: Bandwidth Selection



(a) (log) Population Density within 0.5 miles



(b) (log) Adjusted Gross Income (IRS Zip-level)

Notes: RD estimates on log population of census blocks within 0.5 miles of town centers (panel (a)) and log adjusted gross income (AGI) at the Zip-level (panel (b)). Each point represents a regression at the specified bandwidth. Confidence intervals of 90% (thick line) and 95% (thin line) shown over each estimate. Controls include the geographic characteristics listed in Section 4 and county fixed effects.

Table 1: Location-Level Summary Statistics

	N	Mean	SD	p(10)	p(25)	p(50)	p(75)	p(90)
Modern Seat (%)	2,094	38.2	48.6	0	0	0	100	100
Town Pop 2010	2,094	9061.2	52401.9	0	0	869.5	3329	12883
Pop < 0.5 mile	1,910	1588.0	2144.8	10.3	148.6	1010.0	2201.5	3883.4
Election Year	2,094	1883.8	23.7	1855	1868	1882	1902	1914
Election Number	2,094	1.34	0.78	1	1	1	1	2
Vote Margin (%)	2,094	-9.53	25.4	-46.9	-24.7	-4.81	4.86	17.0
Longitude	1,910	-98.7	10.2	-116.1	-102.2	-97.0	-92.8	-86.6
Latitude	1,910	40.2	4.74	33.8	37.6	40.7	43.8	46.1
Zip Income ($)	1,820	48166.3	20534.5	36322.0	40271.0	45238.2	50966.0	58727.4
Top 5% Share	1,820	20.7	6.20	14.1	17.1	20.3	23.8	27.1
Top 10% Share	1,820	33.5	6.76	26.9	30.3	33.3	36.7	39.8
White Collar Jobs (%)	1,892	44.3	12.7	33.3	39.0	44.1	48.7	55.2
Bachelors Jobs (%)	1,892	16.8	7.91	10	13.9	16.4	19.2	23.6
Top $\frac{1}{3}$ Wage (%)	1,892	28.5	11.9	17.0	22.6	27.6	33.4	41.4
Jobs in Public Admin (%)	1,892	7.88	7.08	1.13	4.32	6.57	9.87	14
Miles to County Center	1,910	37.2	93.6	2.82	5.58	11.7	29.0	64.8
Miles to Steam	1,910	1.21	1.58	0.16	0.34	0.67	1.45	2.85
Avg Elevation (km) < 0.5 miles	1,910	0.32	0.28	0.059	0.14	0.22	0.42	0.71

Notes: Summary statistics on location-level data. Shown respectively are the non-missing count, the arithmetic mean, standard deviation, and 10^{th}, 25^{th}, 50^{th}, 75^{th}, and 90^{th} percentile values.

Table 2: Effects on 2010 Population

	(1)	(2)	(3)	(4)	(5)
	Pop (% County)	ln(Pop)	ln(Pop) [Min: 100]	ln(Pop) [< 0.5 miles]	Exists Today (%)
Win	14.6***	3.07***	1.65***	1.68***	30.5***
	(1.75)	(0.31)	(0.16)	(0.19)	(3.73)
County FEs	Y	Y	Y	Y	Y
Geo	Y	Y	Y	Y	Y
SEs / Clusters	Election	Election	Election	Election	Election
BW (pp)	14.5	14.5	14.5	14.5	14.5
N	841	841	841	841	841
N (clusters)	448	448	448	448	448
$E[y]$	17%	11,177	11,177	1,771	83%

Notes: This table presents RD estimates on measures of town population in 2010. Column (1) measures town population as a percent of the county's. Column (2) considers log population. Column (3) uses log population but bottom codes values at 100. Column (4) uses a boundary-neutral definition of population, considering the total number of persons in census blocks within 0.5 miles from the town center. Column (5) is a binary variable for whether the town exists as a distinct entity today. Populations below 1 are bottom-coded to 1 and geographic controls as discussed in Section 4.

Table 3: Effects on 2010 Zip-Level Income

	Income			Composition		
	(1)	(2)	(3)	(4)	(5)	(6)
	log(AGI)	Elasticity AGI/Density	log(Wage & Salary)	Wage & Sal. (% AGI)	Cap Gains (% AGI)	EITC (%)
Win	0.062***	0.15**	0.045***	-0.91	0.46	0.11
	(0.021)	(0.065)	(0.014)	(0.66)	(0.42)	(0.47)
County FEs	Y	Y	Y	Y	Y	Y
Geo	Y	Y	Y	Y	Y	Y
SEs / Clusters	Election	Election	Election	Election	Election	Election
BW (pp)	14.5	14.5	14.5	14.5	14.5	14.5
N	814	814	814	814	814	814
N (clusters)	444	444	444	444	444	444
$E[y]$	\$47,885	\$47,885	\$15,205	66%	3.8%	19%

Notes: Impacts on income according to IRS tax filings at the zip-code level. Column (1) reports the log average adjusted gross income (AGI). Column (2) reports this latter figure as an elasticity with respect to Zip-level population density, using a fuzzy RD specification. Column (3) reports impacts on the log average wage/salary income. Column (4) reports impacts on the fraction of income that is wage or salary. Column (5) uses the fraction of income from capital gains. Column (6) uses the percentage of filers reporting the Earned Income Tax Credit (EITC). Geographic controls as listed in Section 4.

Table 4: Effects on 2010 Top Income Shares / Inequality

	(1) Top 1% IRS	(2) Top 5% IRS	(3) Top 10% IRS	(4) Top 20% IRS	(5) Top 5-10% IRS	(6) \$100k+ Filers (%)
Win	1.64***	2.86***	2.70***	2.02***	-0.15	1.13***
	(0.34)	(0.51)	(0.58)	(0.50)	(0.23)	(0.34)
County FEs	Y	Y	Y	Y	Y	Y
Geo	Y	Y	Y	Y	Y	Y
SEs / Clusters	Election	Election	Election	Election	Election	Election
BW (pp)	14.5	14.5	14.5	14.5	14.5	14.5
N	814	814	814	814	814	814
N (clusters)	444	444	444	444	444	444
$E[y]$	6.3%	21%	21%	21%	13%	8.1%

Notes: Impacts on top income shares in competing town zip codes as recorded by the IRS. Top income shares are computed under the assumption that incomes are constant within IRS Zip-level reporting ranges. Column (5) reports estimates on the income share of filers between the top 5% and top 10% of incomes. Column (6) reports on the percentage of filers earning \$100,000 or more in adjusted gross income (AGI). Geographic controls as listed in Section 4.

Table 5: Effects on 2010 Industry, Job, and Skill Mix

	Panel A: Job Characteristics					
	(1) Bach. (LODES, %)	(2) Bach. (ACS, %)	(3) High Ed Industry (%)	(4) Skilled [SSS] Industry (%)	(5) Top $\frac{1}{3}$ Wage (%)	(6) Public Admin (%) [All]
Win	3.01***	1.00	2.95***	1.06**	3.23***	-0.070
	(0.59)	(0.72)	(0.97)	(0.51)	(0.77)	(0.60)
County FEs	Y	Y	Y	Y	Y	Y
Geo	Y	Y	Y	Y	Y	Y
SEs / Clusters	Election	Election	Election	Election	Election	Election
BW (pp)	14.5	14.5	14.5	14.5	14.5	14.5
N	833	841	833	833	833	833
N (clusters)	448	448	448	448	448	448
$E[y]$	17%	18%	44%	8.9%	28%	8%

	Panel B: Job Composition					
	Top $\frac{1}{3}$ Wage			Bottom $\frac{1}{3}$ Wage		
	(1) White Collar	(2) Bachelors	(3) Non-White Collar	(4) White Collar	(5) Bachelors	(6) Non-White Collar
Win	2.35***	1.96***	0.89	-0.98	0.18	-1.31*
	(0.52)	(0.33)	(0.62)	(0.77)	(0.20)	(0.71)
County FEs	Y	Y	Y	Y	Y	Y
Geo	Y	Y	Y	Y	Y	Y
SEs / Clusters	Election	Election	Election	Election	Election	Election
BW (pp)	14.5	14.5	14.5	14.5	14.5	14.5
N	833	833	833	833	833	833
N (clusters)	448	448	448	448	448	448
$E[y]$	14%	8.4%	14%	12%	3.2%	18%

Notes: RD estimates of impacts of county seat victory on skill and job mix. Competing towns are linked to census blocks (for LODES) or ACS-reporting units as described in Section 3.3. Panel (a) reports on job and worker characteristics: column (1) on the percentage of jobs requiring a bachelor's; column (2) on the percentage of residents with a bachelor's; column (3) on the fraction of jobs in "white collar" industries, where nationally 30% or more workers have a bachelor's; column (4) on the fraction of jobs in "skilled scalable service" industries identified in Eckert et al. (2021); column (5) on the fraction of jobs in the top wage tercile; column (6) on the fraction of jobs in public administration. Panel (b) reports on the relationship between wages and industries. All results are reported as a fraction of all jobs. Columns (1)-(3) consider jobs in the top wage tercile, columns (4)-(6) those in the bottom tercile. Columns (1), (3), (4), (6) define white collar as in panel (a), column (3). Geographic controls as listed in Section 4.

Table 6: Job Characteristics and Government Employment

	All Jobs			Without Public Admin		
	(1) White Collar	(2) Top $\frac{1}{3}$ Wage	(3) Bottom $\frac{1}{3}$ Wage	(4) White Collar	(5) Top $\frac{1}{3}$ Wage	(6) Bottom $\frac{1}{3}$ Wage
Win	2.95***	3.23***	-2.29***	3.36***	2.83***	-2.37***
	(0.97)	(0.77)	(0.88)	(0.93)	(0.76)	(0.86)
County FEs	Y	Y	Y	Y	Y	Y
Geo	Y	Y	Y	Y	Y	Y
SEs / Clusters	Election	Election	Election	Election	Election	Election
BW (pp)	14.5	14.5	14.5	14.5	14.5	14.5
N	833	833	833	833	833	833
N (clusters)	448	448	448	448	448	448
$E[y]$	44%	28%	30%	39%	28%	31%

Notes: RD estimates of impacts of county seat victory on skill and job mix. Competing towns are linked to census blocks as described in Section 3.3. Each column reports on the percentage of specified jobs with the column characteristics. For columns (1)-(3), the denominator is all jobs; for columns (4)-(6) the denominator is all jobs except those in public administration. "White collar" industries are defined as those where nationally 30% or more workers have a bachelor's. Geographic controls as listed in Section 4.

Table 7: School District Characteristics

	(1) log Students	(2) log Exp / Student	(3) log Rev / Student	(4) log Fed $ / Student	(5) log State $ / Student	(6) log Local $ / Student	(7) log Prop Tax $ / Student	(8) Teachers / Student
Win	0.32***	0.018	-0.015	-0.033	-0.15**	0.027	0.033	-0.0042***
	(0.068)	(0.019)	(0.015)	(0.034)	(0.066)	(0.032)	(0.037)	(0.0013)
County FEs	Y	Y	Y	Y	Y	Y	Y	Y
Geo	Y	Y	Y	Y	Y	Y	Y	Y
SEs / Clusters	Election	Election	Election	Election	Election	Election	Election	Election
BW (pp)	14.4	14.4	14.4	14.4	14.4	14.4	14.4	14.4
N	837	837	837	837	837	837	826	836
N (clusters)	447	447	447	447	447	447	442	447
$E[y]$	4,184	$14,944	$15,022	$1,092	$7,003	$6,926	$5,322	.077

Notes: RD estimates of impacts of county seat victory on locations' school districts. Column (1) reports on log students. Column (2) reports on log expenditure per student. Columns (3)-(7) report on log revenue per student from the following sources: total, federal, state, local, and property taxes. Column (8) reports on teachers per student. Columns (2)-(8) are top-coded at the 1% level to remove outliers with few students. Column (8) is bottom-coded at the 1% level to account for districts with zero listed teachers. Geographic controls as listed in Section 4.

Table 8: Election Sample Robustness

	Panel A: log(Density) from Modern Seat (Fuzzy RD)					
	(1) First Election	(2) First or $\geq$ 50 years	(3) First or $\geq$ 25 years	(4) First or $\geq$ 20 years	(5) First or $\geq$ 10 years	(6) All Elections
Modern Seat	2.12***	2.11***	2.21***	2.24***	2.21***	2.05***
	(0.23)	(0.23)	(0.22)	(0.22)	(0.22)	(0.21)
County FEs	Y	Y	Y	Y	Y	Y
Geo	Y	Y	Y	Y	Y	Y
SEs / Clusters	Election	Election	Election	Election	Election	Election
BW (pp)	14.5	14.5	14.5	14.5	14.5	14.5
N	788	798	841	859	929	1085
N (clusters)	421	426	448	457	494	576
$E[y]$	1,793	1,789	1,771	1,771	1,760	1,699

	Panel B: log(Density) from Victory (reduced form RD)					
	(1) First Election	(2) First or $\geq$ 50 years	(3) First or $\geq$ 25 years	(4) First or $\geq$ 20 years	(5) First or $\geq$ 10 years	(6) All Elections
Win	1.59***	1.60***	1.68***	1.72***	1.64***	1.46***
	(0.19)	(0.19)	(0.19)	(0.19)	(0.18)	(0.17)
County FEs	Y	Y	Y	Y	Y	Y
Geo	Y	Y	Y	Y	Y	Y
SEs / Clusters	Election	Election	Election	Election	Election	Election
BW (pp)	14.5	14.5	14.5	14.5	14.5	14.5
N	788	798	841	859	929	1085
N (clusters)	421	426	448	457	494	576
$E[y]$	1,793	1,789	1,771	1,771	1,760	1,699

Notes: Estimates of the impact on log population density within 0.5 miles of town center using different samples of elections. Each sample includes the first recorded election and subsequently adds more based on the number of years elapsed since the year of the first election. Panel (a) presents fuzzy RD estimates based on whether the location is the modern-day county seat. Panel (b) presents reduced form estimates. Geographic controls as listed in Section 4.

Table 9: Specification and Method Robustness

	Panel A: RD Estimates					
	(1) White Collar (%)	(2) White Collar (%)	(3) Bach. (LODES, %)	(4) Bach. (LODES, %)	(5) log(AGI)	(6) log(AGI)
log(Density)	2.10**	1.80***	1.81***	1.84***	0.15	0.15**
	(0.91)	(0.62)	(0.50)	(0.39)	(0.15)	(0.065)
Pop. Var	0.5 mile	0.5 mile	0.5 mile	0.5 mile	Zip	Zip
County FEs		Y		Y		Y
Geo		Y		Y		Y
SEs / Clusters	Election	Election	Election	Election	Election	Election
BW (pp)	14.5	14.5	14.5	14.5	14.5	14.5
N	833	833	833	833	814	814
N (clusters)	448	448	448	448	444	444
$E[y]$	44%	44%	17%	17%	$47,885	$47,885

	Panel B: OLS Estimates					
	(1) White Collar (%)	(2) White Collar (%)	(3) Bach. (LODES, %)	(4) Bach. (LODES, %)	(5) log(AGI)	(6) log(AGI)
log(Density)	-0.21	0.26	0.53**	0.22	0.016*	-0.0082
	(0.38)	(0.54)	(0.22)	(0.32)	(0.0085)	(0.016)
Pop. Var	0.5 mile	0.5 mile	0.5 mile	0.5 mile	Zip	Zip
County FEs		Y		Y		Y
Geo		Y		Y		Y
SEs / Clusters	Election	Election	Election	Election	Election	Election
N	833	833	833	833	814	814
N (clusters)	448	448	448	448	444	444
$E[y]$	44%	44%	17%	17%	$47,885	$47,885

Notes: Estimates of density's impact on the fraction of jobs requiring a bachelor's degree (cols (1)-(2)), jobs in white collar industries (cols (3)-(4)), and log adjusted gross income (AGI) (cols (5)-(6)). Columns (1)-(4) measure the log population density of census blocks within 0.5 miles; columns (5)-(6) use the zip code level. Panel (a) uses the primary fuzzy RD methodology of close county seat elections. Panel (b) uses OLS. For sample consistency, panel (b) considers only locations within the base election sample and within the default RD bandwidth. Geographic controls as listed in Section 4.

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