Election Cycling:
Quantifying the Impact of Bike Infrastructure on Local Elections and Democracy

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Abstract

While the role and presence of *bike infrastructure* – inclucing bike lanes and bike share systems – is growing rapidly in U.S. cities, literature has not yet considered its quantitative impact in electoral politics. In this thesis, I draw on related literature and propose three hypotheses about what that impact might be: backlash to bike lanes from voters near projects hurts incumbents overseeing bike infrastructure expansion (*H1*), bike lanes boost turnout by increasing local government salience among voters near projects (*H2*), and bike share systems boost turnout by reducing transportation barriers to voting (*H3*).

I use three case studies in the 2010s to test each hypothesis separately. In Portland, OR, I test *H1* with new bike lanes as a geographical treatment in a regression analysis using incumbent mayor re-election vote share. In Seattle, WA, I test *H2* by reusing the bike lane treatment in regression analyses looking at turnout change between local and federal elections. Finally, in Manhattan, New York City, I test *H3* in a multi-part analysis of bike share system ridership on Election Day and in relation to polling place proximity.

In Portland, I find that while electoral backlash to bike lanes may be real, its geographical component is not. This suggests incumbents may disregard electoral pressures regarding the placement of bike infrastructure or similar public goods. In Seattle, I find that new bike lanes are generally associated with lower relative turnout change, but this effect is lower in local elections. This suggests local government should do more to reassure residents that associations between unwanted demographic change and public improvements are not causal if this is the case. In Manhattan, I find that setting up polling places closer to bike share stations has potential to increase turnout. This suggests bike and transportation policies' effectiveness can be improved by precise alignment of stations and destinations.

My thesis is a first-of-its-kind contribution to quantitative understanding of the relationship between bike infrastructure and elections. In addition, I publish original, high quality data sets for future research and describe methods to overcome significant data barriers to electoral analysis.

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Chapter 1

Introduction

Bike infrastructure is on the rise in most U.S. cities, as local policymakers realize its potential in achieving goals of sustainable transportation, safety, and mobility. As a result, political phenomena relating to bike lanes or bike share stations, which were rare a decade ago, are common today. Routine neighborhood backlash against bike lane projects has made the term “bikelash” a staple of local politics. Higher-quality and highly-visible bikeways are more frequently part of local politicians’ infrastructure policy platforms. Bike share systems tout free-fare programs on Election Day meant to ease movement to the polls. In turn, it is reasonable to suspect that these themes – *backlash*, *visibility*, and *movement* – cause bike infrastructure to play a role in *elections*.

Despite this, there is a clear lack of *quantitative* research on the relationship between bike infrastructure and electoral politics. I aim to change this with a first-of-its-kind study. I begin by hypothesizing three mechanisms by which bike infrastructure may shape elections. Then, I identify cities as case studies for testing each hypothesis and quantifying each mechanism. The theoretical and policy takeaways from these tests, original methods I define, and cleaned data I publish lay the groundwork for a new subfield in quantitative studies of local elections. The thesis that follows is divided into five chapters: a literature review, three analytical case studies addressing unique aspects of bike infrastructure and local elections, and a conclusion discussing broader implications of these findings.

Chapter 2 begins by describing bike infrastructure and surveying related literature on political opinion, local elections, and the costs of voting. I propose three hypotheses regarding how bike infrastructure impacts elections in large U.S. cities. First, backlash to bike lanes hurts incumbent re-election in precincts that receive these projects (*H1*). Second, visible neighborhood change via bike lanes increases the salience of local elections and boosts turnout (*H2*). Third, bike share systems (which provide public access to bikes) increase turnout by facilitating transportation to polling places (*H3*). To test each hypothesis separately, I use data on Portland (OR), Seattle, and New York City's Manhattan borough.

Chapter 3 tests *H1* by looking at Portland, OR, where Mayor Ted Wheeler oversaw a significant expansion of the city's bike lane network between his two elections in 2016 and 2020. I use Wheeler's vote share change as an outcome variable, and I introduce two original methods for quantifying bike lanes as a precinct-level treatment (T_{grid} using lane proportion to its street grid versus T_{walk} using areas within a 15-minute walk of lanes). I also introduce an original taxonomy for bike lane quality and publish original data on lane quality for this time period. I run a linear regression model with demographic confounders and test iterations of the bike lane treatments by quality and recency. I find communities associated with bike lane backlash punish Wheeler more, but the *geographical distribution* of lanes has no impact.

Chapter 4 tests *H2* using multiple election cycles in Seattle, WA, overlapping with the city's own bike lane expansion in the 2010s. I use voter turnout change as an outcome variable, introduce an original method for aligning changing precinct shapes and their statistics across years, and reuse my original bike lane treatment method (T_{walk}) and quality taxonomy from the Portland chapter. I similarly publish original bike lane data on timing and quality for this time period. I run linear regression models, test iterations of lane treatments by quality and recency, and compare local to federal election regressions to isolate the impact of bike lanes on *local* turnout. I find communities that received bike lanes saw larger turnout decline, though this was notably smaller in local versus federal elections.

Chapter 5 tests *H3* in Manhattan, where data on its expansive bike share system, *Citi Bike*, allows me to investigate bike ridership patterns and turnout on Election Days in 2014 and 2018. I run three sub-analyses using linear regression models, separately studying bike share station introduction and turnout change (*A1*), system volume on Election Days versus modelled expectation (*A2*), and station volume on Election Day 2018 compared to polling place proximity (*A3*). I find bike share station placement and system use have no impact on turnout, but stations closer to polling places see notably higher Election Day use.

Table 1.1: Summary of analytical chapter metholodogies and takeaways.

Case study	**Methodology**	**Takeaway**
Chapter 3: *Bikelash and incumbency* in Portland, OR	Linear regression model comparing Wheeler vote share change from 2016 to 2020, with bike lane treatment options using length or 15-minute walkshed overlap.	Bikelash may exist among car-commuters or those concerned with gentrification, but voting patterns do not involve proximity to new bike lanes.
Chapter 4: *Bike lane heuristics* in Seattle, WA	Linear regression model comparing turnout change in 2010s elections at several levels, reusing bike lane 15-minute walkshed treatments.	Bike lanes may dampen local turnout decline in the changing neighborhoods where they are commonly placed.
Chapter 5: *Biking to the polls* in Manhattan, NYC	Linear regression analyses looking at new bike share stations and turnout change (*A1*), modelled versus Election Day ridership (*A2*), and polling place proximity effects (*A3*).	Polling place proximity matters in biking to the polls, and bike share systems do not boost turnout as they are not aligned well with polling places.

Chapter 6 concludes by packaging all findings into an initial theory of bike infrastructure's impact on elections. I arrive at three insights: electoral ramifications of bike infrastructure *placement* are minimal, bike infrastructure may dampen local turnout decline in changing neighborhoods, and bike share systems have potential in moving voters if properly aligned with polling places. Not only are these novel contributions to urban political literature, but they also suggest political and policy recommendations for pro-bike infrastructure officials looking to navigate its electoral implications. These recommendations include that elected officials should ignore political pressure incentivizing inefficient bike lane distribution, local governments should work to convince residents that bike lanes do not cause unwanted neighborhood change, and polling places should be moved or added nearer to bike share stations to make voting easier and boost turnout.

This thesis lays the groundwork for future research. Much of my work involved cleaning data related to elections and infrastructure, which are *more accurate* than government-released files, and I published these for public use. I developed methods for data challenges including comparing elections across time by merging precincts, projecting higher-level Census features onto precincts, and converting bike lane shapefile lines to treatments. I hope these resources and concepts encourage future exploration in understanding how bike infrastructure interacts with urban political opinion and behavior.

Chapter 2

Literature Review and Contributions

The contributions this thesis makes to the literature are twofold. First, literature on the interaction between American cities' ever-increasing bike infrastructure and its municipal elections is absent. Political punditry, on the other hand, follows phenomena like local backlash toward bike projects closely. This means societal understanding of this connection is painted by a qualitative, anecdotal brush. These stories are informative, but they may exaggerate or extrapolate effects of bike infrastructure on elections. I bring a quantitative lens to the subject: big data on bike infrastructure, though decentralized and difficult, offers strong research opportunities.

The second contribution is more general. Literature on local elections and infrastructure policy – or infrastructure itself – generally lacks electoral analysis, instead frequently relying on survey or qualitative research. Feedback loops between public goods distribution and incumbent electoral performance or turnout are under-explored.

To begin, I define "bike infrastructure" and outline its growing role in contemporary U.S. cities. Then, I build three qualitative hypotheses for quantitative testing, explore literature related to each, and identify existing gaps. Together, these three hypotheses and the distinct aspects of electoral politics they touch provide exciting potential for understanding how bike infrastructure shapes elections. These also relate to larger questions on the mechanics of urban political opinion, expression, and lifestyle.

The rise of bike infrastructure

Bike infrastructure constitutes anything ranging from bike lanes painted on an existing street to a new bike share station as part of a larger system. These are like most other public goods, part of a universe of budgetary and distributive political decisions. Yet the recent rise of bike infrastructure has been precipitous. The first painted bike lane was installed in Davis, California, in 1967[1]; today, most cities regularly update Bicycle Plans, which imagine vast networks of pristine, grade-separated cycleways making intracity trips convenient and safe on two wheels. Some cities, including the ones I analyze, are closer to that vision than others. Overall, though, United States government spending on bicycling infrastructure increased from a mere $22.9 million in 1992 to $970 million in 2017 (Nunno, 2021) as planners increasingly recognize cycling's importance to goals of health, climate, and mobility.

Those who study urban public goods provision are wary that bike infrastructure's *placement* is solely based on these goals, however, and distributive analyses constitute most research on bike infrastructure. The most holistic American analysis comes from Braun et al. (2019), who found across 22 large U.S. cities that concentration of bike lanes was lower in more Hispanic, lower income Census block groups in the mid-2010s. But this analysis treated all lanes, from cheap paint to high–quality parkways, as the same treatment. Quality is a crucial metric for my analysis, since higher-quality infrastructure is likely more noticeable to voters. Research that distinguishes by quality is lacking in the United States, though international studies from Santiago de Chile (Mora et al., 2021) to London (Tait et al., 2022) find that wealthier areas generally receive higher-quality (safer) bike infrastructure.

Unequal distribution of bike infrastructure is not my primary focus, and including confounders in an electoral analysis helps control for disparities. But these studies highlight the relationship between bike infrastructure and urban demography. This in turn raises three hypotheses about the role of bike infrastructure in urban political participation.

[1] This is widely cited and reinforced by the City of Davis itself: https://www.cityofdavis.org/about-davis/history-symbols/first-bicycle-lanes-in-davis

Bikelash and incumbency

Opposition to bike lanes is generally known as *bikelash*, and its study focuses on protests during planning stages. Scholars of this phenomenon find that bike lanes increasingly clash with two demographically-unique urban interest groups domestically.

First, *anti-gentrification activists* characterize bike lanes as "white lanes of gentrification" (Lubitow, 2016, 250); however, the mechanism by which this infrastructure has become associated with urban displacement is unclear. For instance, in the Shaw / U Street neighborhood in Washington D.C., a historically Black neighborhood that gentrified quickly in the 2000s, Hyra (2015, 1767) describes how new white, wealthy gentrifiers successfully fought for "limited parking, [...] bikes lanes and dog parks, which cater to their tastes and preferences." On the other hand, Betancur (2002, 806) analyzes gentrification in Chicago's West Town and concludes that the government "steered gentrification" to the neighborhood via "major infrastructural improvements." Whether gentrifiers spur infrastructure or planners use infrastructure to attract the so-called "creative class" of young professionals who accelerated downtown revival in the 2000s is unclear. The takeaway is that gentrification and bike amenities are closely linked in the minds of urban residents.

Second, typically older *motorists* see bike infrastructure as a threat to cars' unique cultural dominance in American cities. As the primary trade-off of high-quality bike projects that require expanded right-of-way, parking operates as a convenient microcosm of this culture. For instance, in *Paved Paradise*, Henry Grabar (2023) describes how parking scarcity intensely shapes urban behavior; competition for parking spaces culminates in murder surprisingly often in the United States. More broadly, Wild et al. (2018, 510) include motorist opposition to bike infrastructure under the banner of "conservatism," arguing that value commitments including "the centrality of car travel to notions of family and economic responsibility; [and] the commitment to sub-urbanism [...] all play out in conservative resistance to progressive planning projects." Since bike projects in the 2010s and beyond are motivated by pedestrian safety and climate concerns, it is no surprise that conservatives resist these under the car-dominant regime of post-urban renewal U.S. cities.

Journalism on bikelash reinforces the primary opponents outlined by the literature: consider recent headlines like: "In Mattapan, bike lanes divide the community: 'They're just trying to push us out'" from *The Boston Globe*[2], or: "Bikes or cars? The battle over one Bay Area bridge lane is heating up" from the *San Francisco Chronicle*[3].

Unlike academia, journalism has also focused on bikelash in an electoral setting. Many cities have lobbying and advocacy groups on both sides of the bike debate, whose endorsements candidates seek and advertise. Take Cambridge, MA, as an example: in its 2023 City Council elections, the city's "Cycling Safety Ordinance" requiring a massive bike network expansion emerged as a key issue (Arshad, 2023). Pro-cycling advocacy organization Cambridge Cycling Safety distributed leaflets at bike share stations and knocked on doors; opponents of the ordinance touted their stance in interviews with local newspapers. Similar debates over cycling infrastructure happened in urban contests across over the past year: in Minneapolis, the *Star Tribune*[4] rhetorically asked City Council candidates whether a marginal budgetary dollar should go to "vehicles or [. . .] a bike lane," and in an unexpected Los Angeles culture war, firefighters launched a campaign against a bike and bus lane referendum[5]. The lack of research on electoral bikelash is a significant gap in contemporary urban political studies, in addition to a general lack of quantitative local electoral analysis.

Local political contests are unique: unlike those at higher levels, the vast majority are nonpartisan contests, and turnout is generally lower. As a result, J. Eric Oliver (2012, 179) demonstrates via survey research in *Local Elections and the Politics of Small-Scale Democracy* that the local electorate is "far more interested, engaged, and informed about

[2]https://www.bostonglobe.com/2022/08/01/metro/mattapan-bike-lanes-divide-community-theyre-just-trying-push-us-out/

[3]https://www.sfchronicle.com/bayarea/article/richmond-san-rafael-bridge-bike-lane-future-18444983.php

[4]https://www.startribune.com/2023-city-election-minneapolis-city-council-mayor-jacob-frey-ward-mpls-candidates/600305882/

[5]https://www.latimes.com/california/story/2024-02-14/firefighters-launch-campaign-against-measure-hla

local politics." Across localities of different sizes and features, disapproval of local government performance is correlated with votes for challengers, and relatedly, issue-sharing between a voter and candidate is associated with votes for that candidate. These suggest that engaged anti-gentrification activists and car culture warriors are more likely to vote against an incumbent pushing a cycling agenda.

The question of *geography* remains, and few studies have looked at the place-based impacts of infrastructure policy. Regarding similar policies, however, issue-based activists overwhelmingly emerge from near the site of a controversy. In studying the composition of "NIMBYs," or those who routinely block high-density housing projects, Einstein (2020, 102) found in *Neighborhood Defenders* that "eighty-two percent of commenters live in the same census tract [...] as the housing development" they intend to block with requests for delays or more red tape reviews. NIMBYism is a useful analogy for bikelash, given that the older, home-owning demographic composition of housing opponents parallels the profile of conservative cycling opponents (Wild et al., 2018). While housing developments themselves are largely led by private developers, research focusing on public infrastructure supports the idea that voters punish incumbents for unpopular distribution. Burnett and Kogan (2017), for instance, find that pothole complaints are negatively correlated with mayoral and city council member incumbent vote share.

Thus, a reasonable theory of bikelash suggests pro-bike infrastructure incumbent re-election performance will be worse in areas with higher concentrations of older automobile users and areas at risk of or actively gentrifying. Though literature is not helpful in explaining effects beyond these, reporting on bikelash in elections suggests that substantively, bikelash will be less pronounced or absent elsewhere. Formally:

Hypothesis 1 (H1): Bike lanes are unpopular among (a) older, car-commuting and (b) gentrifying neighborhoods; so they will punish incumbents upon receiving new lanes.

Bike lanes and local government visibility

A defining feature of local elections is their low turnout. In Houston, the nation's fourth-largest city with almost 2.5 million residents, only about 17% of eligible voters cast a ballot to select their mayor in December 2023 (Sledge, 2023). Compare this to the 2020 U.S. presidential election, where 60% of Harris County, TX, voters cast ballots (Kanik, 2022).

Studies on the impacts of low local participation detail many associated effects, including lower minority representation in local government (Hajnal and Trounstine, 2005) and unequal municipal spending incentives (Trounstine, 2013). Oliver (2012), however, argues that low turnout is not an existential problem for localities, as citizen preferences are more homogenous at the local level, and the number of divisive issues are self-evidently fewer at a less powerful level of government. Furthermore, homeownership is strongly correlated with mayoral election turnout (Jiang, 2018), leading Oliver (2012, 84) to argue these voters have "strong emotional and material connections to their communities [...] exemplifying all that we expect in a classical notion of the ideal democratic citizen."

Some may find this problematic beyond class undertones; most large U.S. cities are densifying in large part due to growth in renters. Progressive cities encourage this growth for reasons economic, cultural, or environmental, and they may consider higher renter turnout healthier for local democracy as this population segment grows.

As such, many researchers have turned attention to interventions that increase participation in municipal elections. Perhaps the most common is moving these "on-cycle" with federal elections, as most local contests take place in odd-years (Hajnal et al., 2022). However, local races, particularly in larger cities, often involve early primaries, runoff elections, or ranked-choice ballots; this intervention involves more system upheaval than one might expect. Others note that increasing campaign spending, introducing partisanship, or higher contestation in mayoral elections may increase turnout (Holbrook and Weinschenk, 2014; Marschall and Lappie, 2018). But these changes themselves may bring negative political externalities, and an outside treatment like encouraging higher spending may be difficult.

While literature discusses the above structural reforms or vote choice mechanisms (which treat turnout as constant), few have explored how increased local government action on display, or what I call *visibility*, impacts turnout. Even local politicians are unsure of the relationship between their distributive spending and its mobilization effects (Dynes, 2020). Just as literal local election reminders increase turnout (Hopkins et al., 2023), local government action on display may serve as a constant reminder to vote. In other words, any local government action resulting in visible change – perhaps a street reconstruction – may increase the salience of future local elections for nearby residents. A potential issue may be that citizens are bad at assigning responsibility for government services in other areas like train delays (de Benedictis-Kessner, 2018). But early evidence supports the theory that at least some of the electorate may respond to higher local government visibility: Hajnal and Lewis (2003), for example, demonstrate that cities providing more in-house services like trash collection or libraries, as opposed to outsourcing, see higher turnout.

Bike lanes offer an opportunity to test whether more local government action coincides with greater electoral participation by way of increasing its *visibility* to voters. In the past two decades, many cities have changed their approach to a core service: street maintenance. Mill and overlay projects increasingly incorporate a painted bike lane; full reconstructions more frequently integrate grade-separated bikeways. Streets are perhaps the most visible, ubiquitous element of a city, and one might imagine significant changes to its layout, such as removing parking or lanes of traffic for cyclists, are among the most salient *everyday* evidence of local government in action. In voters' minds, this may up the stakes, or simply serve as a reminder that local government and its elections exist at all. Finally, in a city with stagnant bike ridership across a period of bike lane network growth, one can more confidently isolate the impact of visibility versus changes in urban mobility. Formally:

Hypothesis 2 (H2): Bike lanes amplify local government visibility, increasing the salience of municipal elections and thus increasing local turnout.

Bike share stations and going-to-the-polls

The previous hypothesis focuses on how local government infrastructure may increase the salience of a local election, but since urban transportation infrastructure is usually meant to ease *movement*, another theory suggests bike infrastructure may make the *act of voting* easier in dense cities. The popular rational choice model of turnout represents one's choice to vote as a cost-benefit analysis (Riker and Ordeshook, 1968). Aldrich (1993, 248) notes that costs include those "of obtaining information, processing it, and deciding what to do and the direct costs of registering and going to the polls." But little is known about the "*going*" in "going to the polls," and for those interested in boosting voter turnout, lowering transportation barriers to voting is important.

Despite the traditional American view of voting as being in person, on the first Tuesday following the first Monday of November, it is true that absentee voting or voting by mail has increased significantly in recent cycles. Yet the in-person picture is still largely accurate: even at the height of the 2020 COVID-19 pandemic, 54% of voters cast their ballots in-person, and over half of these were on Election Day[6]. In the 2022 midterm elections, levels of in-person Election Day voting rebounded to nearly half of all votes[7], down from 60% in 2018, but still demonstrating a regression to pre-pandemic patterns of voting.

Literature on geography and costs to participation has focused largely on the placement of polling places. Findings agree that turnout and distance to a polling place are related, but models differ. These range from findings that distances exhibit the largest negative effect at close changes in distance (Haspel and Knotts, 2005), further distances (Burke et al., 2019), or the middle ground of suburbs (Gimpel and Schuknecht, 2003). Survey research finds that transportation is indeed a barrier to voting: nearly 30% of nonvoting respondents in a 2016 survey listed lack of transportation as a reason for skipping the election, though the geography of these nonvoters and their mode of transportation is unknown (Stewart,

[6]https://www.pewresearch.org/politics/2020/11/20/the-voting-experience-in-2020/
[7]https://www.eac.gov/sites/default/files/2023-06/2022_EAVS_Report_508c.pdf

2017). Notably, studies overlook explicitly urban contexts, where urban form, non-motorized mobility, and public transportation may have a greater effect.

Despite a lack of concrete evidence, interventions that lower transportation barriers are common in recent elections. The FTA notes that ten large urban transit systems in America, from Los Angeles to Dallas, launched free-fare services on Election Day in 2020[8]. Some private companies like Lyft continued initiatives offering discounted rides to the polls in 2020[9]. Bike share systems, including Citi Bike in NYC[10], Divvy in Chicago[11], and Blue Bikes in Boston[12], have offered free or lower-cost rides to users for the past three federal elections.

These bike share systems present an intriguing opportunity for studying transportation patterns and turnout on Election Day, especially in the nation's densest neighborhoods. Expanding bike share systems in cities like New York City and Boston have seen corresponding massive increases in ridership, and patterns of traveling to polling places may change when these are located near stations. Furthermore, in dense neighborhoods, higher concentration of bike share stations in election precincts may make transportation to polling places easier, marginally reducing the costs of voting. Bike share ridership data allows for distinguishing this mobility effect from the previous hypothesis on visibility. Formally:

Hypothesis 3 (H3): Bike share infrastructure supports transportation to polling places on Election Day, and in dense neighborhoods, this increases total electoral turnout.

[8] https://www.transit.dot.gov/funding/grants/transit-programs-increase-access-voting

[9] https://www.lyft.com/blog/posts/helping-more-people-get-to-the-polls-on-election-day

[10] https://www.thrillist.com/news/new-york/lyft-offering-off-rides-citi-bikes-election-day

[11] https://www.nbcchicago.com/news/local/chicagoland-divvy-bikes-offers-free-rides-on-election-day/170157/

[12] https://blog.bluebikes.com/blog/bike-to-vote-2020

Conclusion and next steps

In summary, I hypothesize three ways bike infrastructure impacts elections. First, some groups – older car commuters or anti-gentrification activists – oppose bike lanes, hurting incumbent re-election performance as a function of proximity to projects (*H1*). Second, bike lanes serve as a reminder to residents of local government in action, increasing turnout (*H2*). Third, bike share systems make traveling to polling places easier in urban areas, increasing turnout (*H3*). Each requires an independent test, given the diversity of bike infrastructure data, electoral systems, and urban dynamics across different cities.

In **Chapter 3** on Portland, Oregon, a national leader in urban cycling, I test *H1* by comparing mayoral incumbent re-election performance across neighborhoods both altered and untouched by the city's expanding high-quality bike network. My results contradict *H1*: bikelash appears to have no geographic component in elections, although groups associated with bikelash do punish incumbents more.

In **Chapter 4** on Seattle, Washington, another cycling leader with more elected turnover and significantly more precincts, I test *H2* by looking at the association between local electoral turnout and the distribution of bike infrastructure between cycles. My results support *H2*: while areas receiving bike infrastructure generally see turnout decline, this is less pronounced in local elections than in federal ones.

In **Chapter 5** on Manhattan, New York City, I explore *H3* by studying election turnout at several levels, the expansion of bike share system Citi Bike, and ample data on Election Day system ridership. My results somewhat support *H3*: while new bike share stations are not associated with higher turnout change, evidence suggests this may be due to the placement of stations themselves further from polling places.

Together, these amount to *entirely novel* first steps in the study of how bike infrastructure interacts with electoral politics. In turn, my research offers general insights into the dynamics of electoral politics in the first place.

Chapter 3

Electoral bikelash in Portland

This chapter aims to quantify the relationship between the geographic distribution of new, protected bike lanes and the re-election performance of an incumbent overseeing it. Specifically, I test:

> <u>*Hypothesis 1*</u> *(H1): Bike lanes are unpopular among (a) older, car-commuting and (b) gentrifying neighborhoods; so they will punish incumbents upon receiving new lanes.*

I examine Portland, OR, where Mayor Ted Wheeler's vote share change by precinct between the city's 2016 and 2020 primary elections serves as a case study in *electoral bikelash*. I first cleaned precinct and election data, then projected electoral factors, including neighborhood gentrification stage, age, proportion car-commuting, and population density, onto precincts. To test the impact of bike infrastructure, I categorized bike lanes by quality and year using several sources and constructed two treatment options. *Treatment 1* (T_{grid}) uses the proportion of new bike lanes to a precinct's total street grid, since lanes in most cases follow existing roads. *Treatment 2* (T_{walk}) uses precinct percent overlap with 15-minute "walksheds" for points imputed on bike lanes, or shapes representing areas that are a realistic 15-minute walk away from this infrastructure.

My analysis consists of linear regression models with confounders, treatment effects distinguishing by infrastructure implementation and quality, and interactions between these.

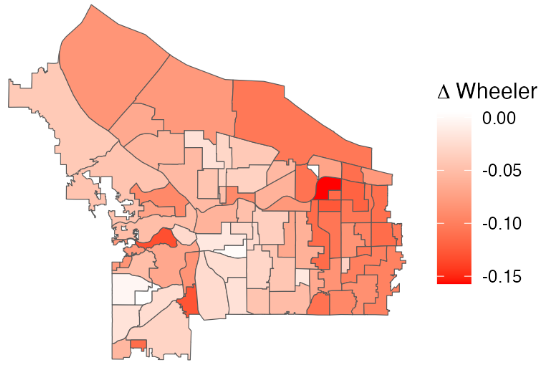


Figure 3.1: Wheeler vote share change, 2016-2020.

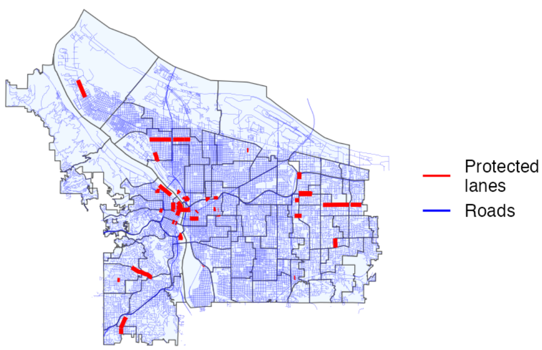


Figure 3.2: Protected bike lanes overlaid on the street network, whose proportion by precinct constitutes *Treatment 1*.

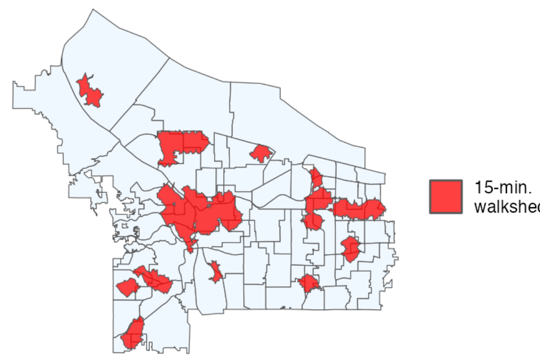


Figure 3.3: Protected bike lane aggregated 15-minute walksheds, whose overlap with precinct areas constitutes *Treatment 2*.

My results somewhat contradict *H1*, indicating that the there is no statistically significant relationship between the *geographical placement* of bike infrastructure and incumbent backlash when controlling for confounders. However, variables associated with groups more likely to exhibit *bikelash* – older car-commuters and those in gentrifying neighborhoods – did punish Wheeler significantly more in his second election. This suggests while *electoral bikelash* is not place-based, it may exist citywide.

I end by discussing the implications of these findings, including that bike-forward politicians concerned with electoral performance need not worry about *where* lanes are distributed; residents far from these projects may punish a pro-cycling agenda regardless.

Why Portland, Oregon?

Portland, Oregon, is among a select few American cities lauded for its Scandinavian-style bike network and corresponding bike-forward political culture. The city reports having "over 400 miles of bikeways, including 100 miles of low-stress neighborhood greenways," thanks to an early start in drafting its first Bicycle Master Plan in 1973[1].

Mayor Ted Wheeler, elected in 2016, bolstered Portland's reputation as a top American biking city in his first term, overseeing a massive expansion of the city's protected cycling network. Urbanist analytics website Walk Score ranked Portland second on its "Bike Score" rankings in 2021[2], and Portland has consistently held its place among national cycling advocate People for Bikes' top five large U.S. biking cities since Wheeler entered office[3].

From a research perspective, three features make Portland the ideal city for studying *electoral bikelash*. First, among top biking cities, Portland is one of few which has seen an incumbent run in a close re-election race. To measure incumbency punishment, it is necessary to have an incumbent's original and re-election years coincide with a period of significant bike network expansion; in Portland, like many cities, this took place in the late 2010s.

Second, Ted Wheeler's elections both occurred at the latest possible four-year window in this decade, 2016 and 2020. Both elections taking place within the same decennial U.S. Census period means that precinct boundaries are largely unchanged in Portland. Furthermore, since precincts are constructed from Census blocks, the latter provide a common, comparable source of demographic information across the election cycle.

Third, and finally, Portland is one of very few cities with accurate, extensive data on a bike lane's most recent upgrade. Other top cycling cities considered for this analysis release only partial or inaccurate data on timing or lanes themselves.

[1]https://www.portland.gov/transportation/walking-biking-transit-safety/biking-portland
[2]https://www.walkscore.com/cities-and-neighborhoods/
[3]Various, from: https://www.peopleforbikes.org

Outcome: Incumbent vote share change

Portland holds nonpartisan mayoral elections in presidential years: a first round primary takes place in May, and a runoff occurs in November if no candidate wins a majority. Ted Wheeler won his first election outright in 2016, so my analysis uses May primary results for 2016 and 2020.

Using May primary results may be the only longitudinally-comparable option in Portland, but it also has some advantages over a higher-turnout November mayoral election in testing *H1*. Both mayoral primaries saw similar turnout around 50%, which is similar to off-year mayoral races common in most large U.S. cities. The May primary also allows the most options for voting against an incumbent, whereas binary-choice November runoffs may force anti-cycling voters to vote for an incumbent against a more aggressively pro-cycling challenger. There is no perfectly generalizable American local election, since U.S. cities hold mayoral elections by a variety of systems taking place throughout the year[4], but this set of elections is a reasonable case study while keeping caveats in mind.

Election precinct shapefiles were crucial for calculating treatment effects and aggregating confounders. These shapefiles for Portland come from GIS analysts Kevin Rancik[5] and Ian R. McDonald[6] for 2016 and 2020, respectively. Both used statewide data to reconstruct these for analysis of presidential results. One cluster of precincts were aggregated in 2020 but not 2016; I combined these and their statistics for both cycles. Precincts visually matched city-released overview maps, and precinct shapes overlapped across years within 1% error, suggesting technical differences and not boundary changes themselves. Chapters 4 and 5 on Seattle and Manhattan elaborate on the process of aligning precincts across years; thankfully for Portland, algorithmically merging precinct shapes and data was unnecessary.

[4]For example: New York City has partisan elections, and given its Democratic bent, the winner is essentially crowned when the Democratic primary race is called in June. Chicago's elections kick off with a first round in February and a runoff in April.

[5]https://www.kevinrancik.com/home

[6]https://www.ianrmcdonald.com

Election returns come from Multnomah[7], Clackamas[8], and Washington Counties[9] in Oregon[10]; the vast majority of Portland and its precincts fall into the former. These were released in PDF document form, meaning some were cleanable using R packages like `tabulizer`, while others required manual entry. I extensively verified the accuracy of these via official summary statistics and randomly-selected precincts.

I then removed all precincts with fewer than 150 votes cast in 2016. These were largely on the perimeter, with small land areas, whose outcome variable was skewed by small changes in net Wheeler votes cast that resulted in larger percentage swings. Removing these also alleviated issues in collecting confounders using block group overlap as outlined later. I selected the cutoff of 150 as it conveniently resolved the several above issues at once, and a steep increase in votes cast begins after this threshold. For i in 78 resulting precincts or combined precincts [Figure 3.4], the outcome variable is defined as[11]:

$$\frac{(\text{2020 Wheeler votes})_i}{(\text{2020 total votes})_i} - \frac{(\text{2016 Wheeler votes})_i}{(\text{2016 total votes})_i} = (\Delta\text{Wheeler})_i$$

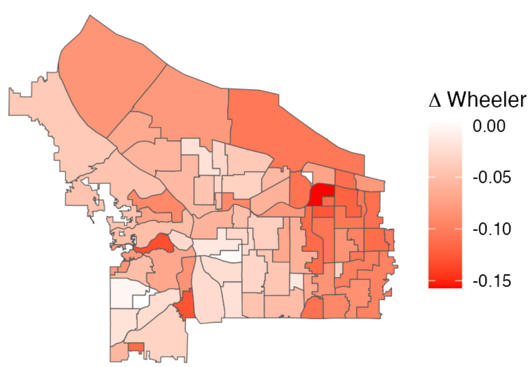


Figure 3.4: Wheeler vote share difference by precinct, 2016-2020.

[7]https://www.multco.us/elections

[8]https://www.clackamas.us/elections

[9]https://www.washingtoncountyor.gov/elections

[10]Portland is a rare large U.S. city with a handful of residents who vote in a separate county, seemingly outside of the city limits. Most of these precincts were small and filtered out in the end.

[11]Note that the result is Δ% Wheeler, not %Δ Wheeler, which is to say I use a simple difference in percents and not the percent change.

Treatment: Protected bike lanes

Since *H1* revolves around bikelash fueled by older motorist conservatism and anti-gentrification sentiment, I chose *bike lanes* as a treatment. These are disruptive for automobile users and the dominant symbol of bike infrastructure as gentrification. While a later chapter treats bike share stations as infrastructure, Portland's own system, BIKETOWN, is less prominent than others nationally and relatively uniformly dispersed across the city[12]; its effect as a treatment is likely minimal across a mere 78 precincts. Additionally, public system data stretches only as far back as September 2020, after elections measured here.

There are many methodological challenges to analyzing bike infrastructure, but three general questions stand out: how to conceptualize the *timing* of an introduction, how to capture *quality*, and how to measure primary *recipients* of a project based on its placement. My analysis explores recency, quality, and geography, so all are important variables. I make several assumptions which simplify the above.

First, on the question of *timing*, I consider a bike lane's "introduction" to be when its construction is completed. It is reasonable to believe that most residents of a given election precinct see bike infrastructure projects as discrete change represented by a "before" and an "after." In the long run, voters, especially long-term residents, will remember a change for these two longer-term states. To be sure, construction is an annoyance, but it happens constantly all around any given city; this analysis seeks to look at bike infrastructure as a treatment, not its implementation process. Additionally, in inspecting all rows in the data individually, I found that most projects are on a relatively quick construction schedule. These projects typically start and end within the six month "warm weather" window and all are complete by Election Day.

Second, I propose categorizing the *quality* of bike infrastructure by how easy it is to ignore, not by the comfort of its use. Many more Portland voters drive cars than bike; the

[12] https://account.biketownpdx.com/map

question of quality is partly how disruptive it is to car users. Additionally, higher quality infrastructure is more visible as a symbol of gentrification to some groups. I divide bike infrastructure into two categories of quality: *painted* and *protected*.

Painted lanes are self-explanatory: these follow a street network and require essentially no construction but technically allocate dedicated space to cyclists [Figure 3.5]. Standard bike lanes typically sit on the shoulder of a road as painted white lines, sometimes between traffic and parking, and provide little protection from drivers. Others, such as those running through downtown Portland, are bright green strips of the road, more visible to non-cyclists yet just as easy to drive over. Buffered bike lanes, which place thicker double-striped lines or patterned neutral space between bike and traffic lanes, legally occupy more of the street but are, again, just as easy to drive over. Installing a painted bike lane changes little about the topography of a street, meaning disgruntled motorists can simply refuse to change bad habits, like failing to look over one's shoulder or speeding through a turn. It is for this reason that some cyclists complain painted bike lanes are merely symbolic. Painted bike lanes are less important for my analysis, and I treat its variants as one category.

Protected bike lanes are more complicated as they vary more widely in convenience to motorists. These are like bike lanes in that they follow a street network, but some form of barrier exists physically preventing collision between cyclists and motorists. In Portland, many forms are categorized under the banner of "protected bike lane." The most common barrier are "flex posts," a type of bollard that emergency vehicles can easily drive over but strongly disincentivizes driving in a bike lane [Figure 3.6]. Others involve small raised curbs, usually laid out in a dashed line with open intervals where driveways exist. Many involve a combination of flex posts and curb sections, particularly when driveways are present along a route [Figure 3.7]. Finally, the highest-quality projects are grade-separated, serving as more of an extension to the sidewalk than something that looks like part of the street [Figure 3.8]. All of these are highly visible and significantly more disruptive to drivers than paint, though construction schedules vary.

Figure 3.5: *Painted* lanes on SE Belmont Street.

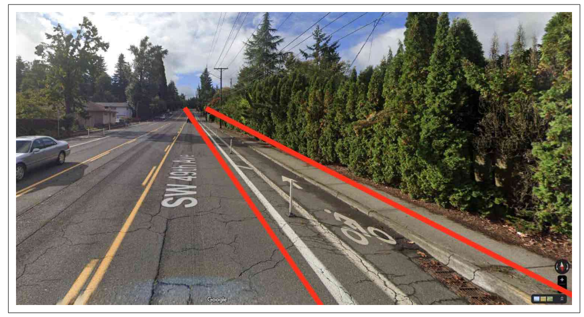


Figure 3.6: *Flex post / bollard* lanes on SW 49^{th} Avenue.

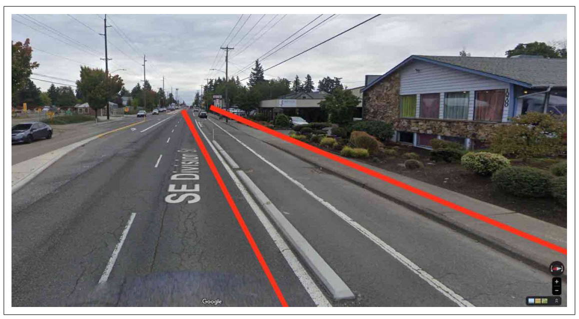


Figure 3.7: *Flex/curb mixed* lanes on SE Division Street.

Figure 3.8: *Fully protected* lanes on SW Capitol Highway.

Figures 3.5 through 3.8 display the four categories of data I used[13]. Bike lane data comes from the City of Portland[14], which maintains shapefiles for all existing bike routes.

I split this data into two subsets. The first encapsulates the *painted* category, or all standard and buffered bike lanes. The second includes all *protected* lanes. Some segments were named, and most routes had an associated year of construction. To verify the segments included in the protected lanes subset across all years, I individually "visited" each one on Google Street View, using historical imagery to find pre- and post-construction years. I saved hyperlinks to images for each of these [see Appendix: Data]. This not only allowed me to confirm all construction years were correct, but it also helped me ensure no recent (post-2020) construction projects were built as lanes of a lower quality in between the 2016 and 2020 elections.

[13] All examples are taken from the final data set of lanes, with images from Google Street View (red lines added to delineate the lane): https://www.google.com/streetview/

[14] https://gis-pdx.opendata.arcgis.com/datasets/PDX::bicycle-network/about

Finally, this allowed me to categorize each protected segment into two types based on protection: *type 1* includes flex post only lanes [Figure 3.6] or those with a combination of flex posts and raised curbs[15] [Figure 3.7], and *type 2* includes grade-separated or fully curb protected lanes [Figure 3.8].

Filtering for projects constructed from 2016 to 2020[16], and converting all to GIS "linestrings," the final protected data set includes 61 shapefile segments, some of which were part of the same project. At the segment level, there were 48 of *type 1* and 13 of *type 2*. I took painted lane installation dates as given upon confirming dates for protected lanes, and I filtered these similarly.

Having defined the *timing* and *quality* components, the final question revolves around *recipients* of bike infrastructure. I propose two treatment methods to quantify geographical proximity to bike lanes, since these should be more salient to those nearer to them.

Treatment 1 ($\mathrm{T}_{\mathrm{grid}}$) is defined as new bike lane length as a proportion of a precinct's total street *grid*. To gather the Portland street network, I used the `tinytiger` package[17], which downloads the U.S. Census Bureau's TIGER/Line shapefiles on roads for selected counties. I calculated the total length of roads within each precinct, as well as the length of all paint and protected projects in that precinct. For protected lanes, I distinguished between construction years to analyze project recency and treatment. For each precinct, I calculated the proportion of painted project lengths to total street grid length, and the total proportions of protected project lengths by year and type (type 1 versus type 2, as defined above) to total street grid length. For i in 78 precincts, and for category $= c \in \{$paint (all); protected (all); protected (subset by type)$\}$ (separately for all years *and* by year), $\mathrm{T}_{\mathrm{grid}}$ is:

$$\frac{(\text{Total bike lane length})_{c,i}}{(\text{Street grid length})_i} = (\mathrm{T}_{\mathrm{grid}})_{c,i}$$

[15] I combined the flex post and flex post / curb combination categories as the latter only had six examples, and these were similarly "disruptive" to the streetscape visually and in construction.

[16] I included 2020 lanes since for all 11 in this subset, installation had occurred. The COVID-19 pandemic and civil unrest that year may have slowed the infrastructure pipeline temporarily.

[17] https://alarm-redist.org/tinytiger/index.html

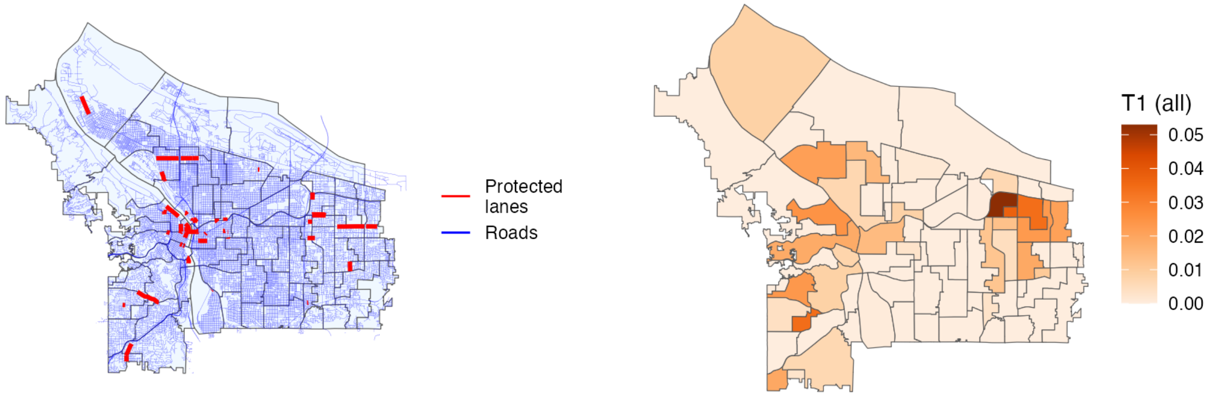
Treatment 2 (T_{walk}) is defined as the proportion of each precinct area within a *15-minute walk* of bike infrastructure. The complexity of this process meant I calculated T_{walk} for protected type 1 and type 2 lanes only, opting to use T_{grid} for painted lanes throughout. I first imputed points on each line at 500 meter intervals, or at the line's centroid if it were sufficiently short. I then used the `r5r` package[18] to compute 15-minute *walksheds*: polygons that represent a 15-minute walk radius from each point, calculated by crawling the sidewalk network. Walksheds are useful because they avoid features like water or highway impasses, more accurately capturing *experienced* proximity to an urban feature. Admittedly, 15 minutes may seem an arbitrary threshold, but this number is commonly cited in urbanist circles as a key metric for one's core neighborhood[19]. For separate analyses, I joined these polygons either by year, or overall: if I were ignoring *timing*, for instance, I first merged all walksheds regardless of implementation year, then calculated overlap. This way, like the set of T_{grid} variables, the range of T_{walk} is zero (no walkshed overlap) to one (full overlap). For i in 78 precincts, and for category $= c \in$ {paint (all); protected (all); protected (subset by type)} (separately for all years *and* by year), T_{walk} is defined as:

$$\frac{(\text{Bike lane walkshed overlap area})_{c,i}}{(\text{Total area})_i} = (T_{\text{walk}})_{c,i}$$

These treatments are imperfect, but their underlying data is high-quality, and they capture different aspects of one's experience with bike infrastructure. T_{grid} captures the potential compounding effects of bike infrastructure, where under T_{walk}, two nearby projects are coded the same as one, since their overlap is a joined walkshed polygon. T_{walk}, however, has the advantage of capturing proximity to bike infrastructure, as walksheds can cross precinct boundaries, such as in the case of a project being near the edge of a precinct. Figure 3.9 illustrates the translation of lanes to precinct-level treatment via T_{grid}; Figure 3.10 illustrates the same for T_{walk}.

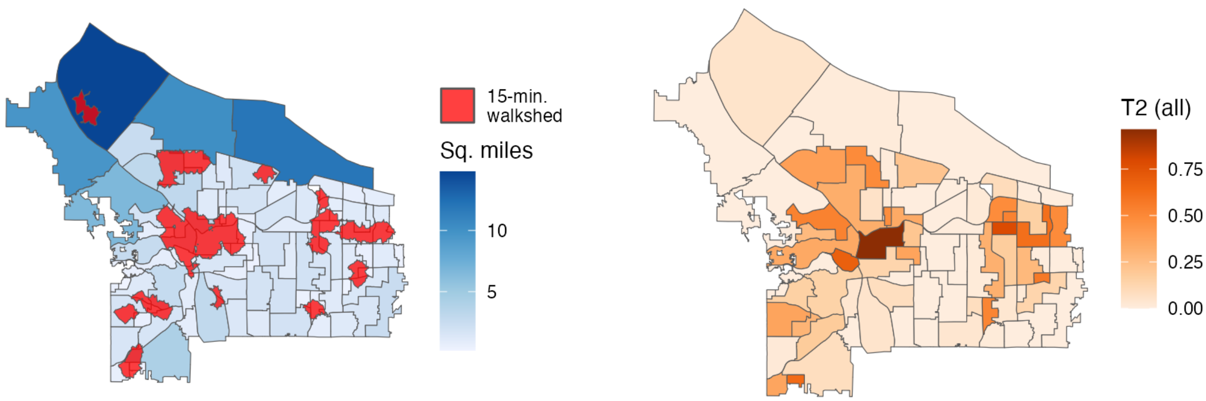
[18]https://ipeagit.github.io/r5r/

[19]Take, for instance, the popular concept of the "15 minute city": https://www.15minutecity.com



(a) Protected lanes overlaid on street grid. (b) $\mathrm{T}_{\mathrm{grid}}$: lane concentration within a precinct.

Figure 3.9: *Treatment 1* ($\mathrm{T}_{\mathrm{grid}}$), from all protected lanes to street grid proportions.



(a) Protected lanes overlaid on precinct area. (b) $\mathrm{T}_{\mathrm{walk}}$: lane coverage of a precinct.

Figure 3.10: *Treatment 2* ($\mathrm{T}_{\mathrm{walk}}$), from all protected lanes to walkshed overlap proportions.

Again, note the subtle differences between $\mathrm{T}_{\mathrm{grid}}$ and $\mathrm{T}_{\mathrm{walk}}$. In the former, the center downtown area – with visually high bike lane coverage – is not the most "treated" of all precincts, since downtown has a similarly dense street grid. Instead, the more residential northwest precincts are at the $\mathrm{T}_{\mathrm{grid}}$ upper end. In $\mathrm{T}_{\mathrm{walk}}$, the downtown area is the most "treated," since it is almost completely covered by protected lane walksheds. $\mathrm{T}_{\mathrm{grid}}$ is also a much smaller variable numerically, since Portland's street network is universally larger than its bike network. In summary, $\mathrm{T}_{\mathrm{grid}}$ captures concentration, while $\mathrm{T}_{\mathrm{walk}}$ captures coverage.

Confounders: Car-commuting, age, gentrification

For a full picture of electoral bikelash, I considered both the factors cited in *H1* and others that may correlate with changes in incumbent performance. The former includes a measure of gentrification and variables capturing motorist conservatism including car use and age. The latter whittles down to population size and density.

The first confounder crucial to my analysis is gentrification. The City of Portland came under scrutiny in the late 2000s as urban scholars noticed its population growth increasingly amounted to displacement, especially for nonwhite, lower-income residents. In response, the city conducted numerous studies documenting neighborhoods at-risk or actively gentrifying. One such report[20] used pre-treatment data from 2016 and earlier and considers whether a Census tract "has a vulnerable population, has experienced demographic change, and [...] housing market conditions," categorizing some tracts as susceptible or in early gentrification stages and others as actively gentrifying. I recreated this tract index as shapefiles, grouped "early" and "susceptible" areas as *pre-gentrifying*, and kept these as well as actively *gentrifying* areas [see Appendix I for visualization]. Census tracts do not align perfectly with precincts, so I created variables for *percent overlap with pre-gentrification* (pGA) and *gentrification areas* (GA). For i in 78 precincts, these are:

$$\frac{(\text{Pre–gent. overlap area})_i}{(\text{Total area})_i} = (\text{pGA})_i \qquad \frac{(\text{Gent. overlap area})_i}{(\text{Total area})_i} = (\text{GA})_i$$

Next, I wanted to capture the demographic profile of conservative bikelash: older motorists. The American Community Survey (ACS) releases five-year estimates for a number of demographic variables at the block group level, including *percent commuting by automobile* and *median age*. These estimates are averages over a pre-treatment period from 2011-2016.

[20] https://www.portland.gov/sites/default/files/2020-01/gentrification_displacement_typology_analysis_2018_10222018.pdf

Using the `tidycensus`[21] package, I collected several ACS variables to calculate an estimated employed population (population commuting to work plus population working from home), estimated workforce (total population subtracting population under age 16 and non-labor force participating population above age 16), and estimated employment rate (the ratio of the two) by Census block group (BG). I then calculated the percent of a block group's population commuting to work via the following formula:

$$\frac{\text{Pop. auto–commuting}}{\text{Pop. workforce}} \times (\text{Employment rate}) = \%\ \text{auto–commuting}$$

With total proportion commuting by automobile calculated and median age collected for each Census *block group*, I then had to convert these to the precinct level. Importantly, precincts are constructed from Census blocks, as are Census block groups, meaning precinct and block group boundaries often overlap (Amos et al., 2017). My solution to this is what I term *population-weighted block group (pwBG) projection*.

First, I collected each Census block's 2010 populations using the `censable`[22] package. I use 2010 populations, as block-level counts are only available in Census years, 2020 data is post-treatment, and 2020 data accuracy is impacted by differential privacy[23]. Next, I multiplied statistics of interest – here, BG *percent commuting by automobile* and *median age* – by each contained block's 2010 population. This is akin to weighting each one.

Finally, matching each 2010 Census block to the precinct encompassing it, I sum all population-weighted statistics, and divide by the sum of all block populations within the precinct. Formally, to find numerical variable V for precinct i with population P_i and N_i blocks $\{b_1, ..., b_{N_i}\}$, each with corresponding block group V statistics $\{v_{b_1}, ..., v_{b_{N_i}}\}$ and

[21] https://walker-data.com/tidycensus/
[22] https://christophertkenny.com/censable/
[23] https://www.census.gov/content/dam/Census/library/factsheets/2021/differential-privacy-and-the-2020-census.pdf

population $\{p_{b_1}, ..., p_{b_{N_i}}\}$, I used the following function:

$$f_{pwBG}(V, i) = \sum_{j=1}^{N_i} \left(\frac{v_{b_j} \cdot p_{b_j}}{P_i} \right)$$

To find pwBG-projected precinct proportion auto-commuting (Auto) and median age (Age), for i in 78 precincts, these variables are:

$$f_{pwBG}(\text{Prop. auto–commuting}, i) = (\text{Auto})_i \qquad f_{pwBG}(\text{Median age}, i) = (\text{Age})_i$$

The pwBG-projected median age and proportion commuting by automobile variables are averages of estimates, since I project block group estimates onto blocks, and precinct estimates are generated by weighting these by block population. This introduces caveats, but it is reasonable to assume these variables accurately capture precinct characteristics. The accuracy of *projecting* block group features onto blocks relies on the assumption that these features are homogenous within a block, which is reasonable since block groups are in part selected based on common neighborhood features. The "average of medians" variable for age, additionally, was the best option and an accurate one: it captures age across Portland well when compared to Census tracts and other data points[24].

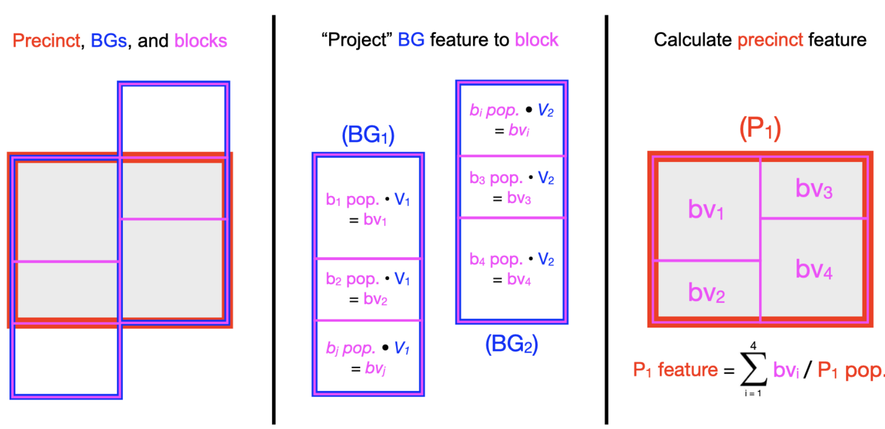


Figure 3.11: The intuition and math behind pwBG-projected variable V.

[24]I spent time verifying precinct maps matched Census tracts colored by age. I also investigated outliers, finding that older precincts contained nursing homes, younger ones were university housing, and more.

Calculating these algorithmically is complicated but made easier using functions from several aforementioned packages. Visualization makes the process intuitive: Figure 3.11 illustrates this formula for one precinct.

Finally, I computed a *population density* (PD) variable for 2020, as denser neighborhoods may be more urbanist and thus more receptive to pro-biking policies, and these same neighborhoods may have higher renting rates and thus more residential turnover not captured in population change. This meant dividing 2010 precinct population from Census blocks by precinct area. For i in 78 precincts:

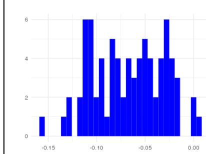
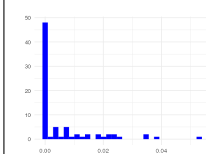
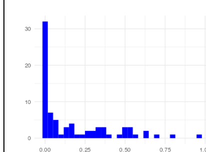
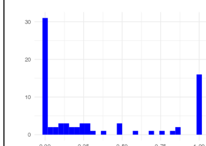
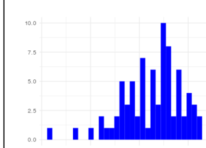
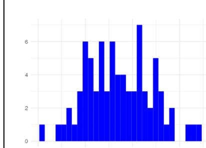
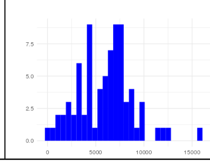
$$\frac{(\text{Population})_i}{(\text{Total area})_i} = (\text{PD})_i$$

I initially gathered variables for race and income for this analysis; however, the gentrification variables largely captured race in Portland, given racial demographics were a major input for their classification. Income, too, correlated strongly with the gentrification metrics and somewhat with age. I was interested in employing a thorough, but simple suite of variables, and decided only those enumerated so far were necessary.

Table 3.1 summarizes the variables I collected, their sources, an overview of the processes by which I cleaned and formatted these, and their histograms. The outcome variable histogram displays a somewhat normal distribution and shows Wheeler performed worse in most precincts in 2020. The majority of precincts have near-zero exposure to T_{grid} or T_{walk} as these are right-skewed[25]. The Auto, Age, and PD variables appear normally distributed as is ideal for linear regression. The gentrification metrics of pGA and GA are bimodal and skewed, respectively, but for sake of interpretation, I did not transform these; pGA functions much like an indicator variable in this way.

[25] I use various iterations of T_{grid} and T_{walk}, aggregating or separating protected lanes by quality or year. I also use T_{grid} for aggregated painted lanes. *Importantly*, the histograms in Table 3.1 show protected lane treatments across all years and both qualities – the subsets have similar distributions.

Table 3.1: Variables for analysis of *electoral bikelash* in Portland, OR.

	Variable[26]	Sources	Process	Histogram
Outcome	**ΔWheeler**: Wheeler vote share change, ('16-'20)	Oregon counties; GIS analysts	Filtering; calculation	
Treatment	$\mathbf{T}_{\text{grid}}$: % new bike lanes to street length	Portland open data; Census GIS via `tinytiger`	Verification; calculation	
	$\mathbf{T}_{\text{walk}}$: % bike lane 15-min. walkshed overlap	Portland open data; walksheds via `r5r`	Verification; imputation; walkshed build; calculation	
Confounder	**pGA**: % overlap with pre-gentrification areas ('16)	Portland open data	Study map build; calculation	
	GA: % overlap with gentrification areas ('16)	Portland open data	Study map build; calculation	
	Auto: *pwBG* % auto commuting ('16)	5-year ACS via `tidycensus`	Calculation; *pwBG* computation	
	Age: *pwBG* median age ('16)	5-year ACS via `tidycensus`	*pwBG* computation	
	PD: Population density ('10) (people/mi^2)	Census via `censable`	Calculation	

[26] In all cases, "%" actually refers to *proportion*; these variables have range [0, 1].

Regression and results

I test several linear regression models that address the nuances in bike infrastructure type, quality, and timing, all of which are measured in Tables 3.2 and 3.3. The baseline model, *Model 1*, includes 2016 pre-gentrifying (pGA) and gentrifying area (GA), 2011-2016 pwBG-projected median age (Age) and percent auto commuting (Auto) (and an interaction variable between these two variables), and 2010 population density (PD)[27].

Models 2, *3*, and *4* add variables for iterations of Treatment 1 (T_{grid}), bike lane projects as a proportion of the street grid, to the baseline model. *Model 2* incorporates total painted projects and total protected projects by precinct across the election time period, as well as interactions between each of these with pGA, GA, and the interaction variable for Age and Auto. *Model 3* acknowledges that different types of protected lanes are more salient than their painted counterparts, and distinguishes between types 1 and 2 lanes (flex posts or flex posts and some curbs versus full curb or grade-separation). Finally, given more recent lanes may have a larger effect on one's perception of an incumbent's bike contribution, I used only projects completed in 2019 or 2020 as separate T_{grid} variables for *Model 4*.

Table 3.2[28] displays these, excluding the numerous interaction variables included in *Models 2*, *3*, and *4*. Arbitrarily few of these have statistically significant[29] associated coefficients. None of the models show statistically significant regression coefficients for bike infrastructure of any kind, though, combined with interaction variables, these are negative.

Models 2, *3*, and *4* provide little evidence that electoral bikelash occurred in Portland in 2020. In *Model 2*, I find regression coefficient 0.116 for the aggregate protected T_{grid}, or proportion of protected bike lane overlap with a precinct's street grid across all years and quality types. This means that Wheeler is expected to have performed 11.6% worse from 2016 to 2020 in a precinct with *no* protected lanes than in one one with protected lanes

[27]Note that in each regression, I divided density units by 10^3, such that a one-point increase in the variable used corresponds to a $1,000$-person increase in population density; this makes coefficients viewable.

[28]All regression tables constructed using the `stargazer` (Hlavac, 2022) package.

[29]I use the significance threshold of $\alpha = 0.05$ unless stated otherwise.

Table 3.2: *Model 1* and T_{grid} *Models 2*, *3*, and *4*.

	Dependent variable: Wheeler vote share change from 2016 to 2020			
	Model 1	*Model 2*	*Model 3*	*Model 4*
Prop. gentrifying area	-0.058^{***}	-0.062^{**}	-0.060^{**}	-0.060^{**}
	(0.020)	(0.024)	(0.024)	(0.024)
Prop. pre-gentrifying area	-0.059^{***}	-0.032^{**}	-0.033^{**}	-0.029^{**}
	(0.010)	(0.012)	(0.013)	(0.013)
Prop. auto commuting (pwBG)	-0.321	-0.734^{**}	-0.891^{**}	-0.897^{**}
	(0.325)	(0.326)	(0.343)	(0.388)
Median age (pwBG)	-0.008	-0.014^{**}	-0.016^{***}	-0.016^{**}
	(0.006)	(0.006)	(0.006)	(0.007)
Auto-age interaction var.	0.010	0.020^{**}	0.023^{**}	0.024^{**}
	(0.009)	(0.008)	(0.009)	(0.010)
Pop. density (10^{-3}/mi.^{2})	0.005^{***}	0.005^{***}	0.005^{***}	0.006^{***}
	(0.002)	(0.002)	(0.002)	(0.002)
Painted projects (as prop. of street grid)		-2.061 (1.329)	-1.815 (1.387)	-2.279 (1.422)
Protected projects (as prop. street grid)		**0.116** (1.898)		
Type 1 protected projects			**1.077**	
			(2.102)	
Type 2 protected projects			**−21.759**	
			(13.977)	
Type 1 protected projects (2019)				**1.436** (3.551)
Type 2 protected projects (2019)				**0.332** (6.082)
Type 1 protected projects (2020)				**−4.037** (15.192)
Type 2 protected projects (2020)				**492.211** (341.441)
Constant	0.206	0.425^{*}	0.511^{**}	0.512^{**}
	(0.218)	(0.215)	(0.223)	(0.253)
Observations	78	78	78	78
R^2	0.414	0.548	0.573	0.600
Adjusted R^2	0.365	0.447	0.443	0.440
Residual Std. Error	0.029 (df = 71)	0.027 (df = 63)	0.027 (df = 59)	0.027 (df = 55)
Note:				*p<0.1; **p<0.05; ***p<0.01

completely covering its street grid, *ceteris paribus.* This goes against *H1*, which suggests the latter would punish Wheeler more, but the coefficient is not significant. When separating this effect by quality (as in *Model 3*) or quality *and* year (as in *Model 4*), I find statistically insignificant coefficients for bike lane treatments, including the painted lane variable.

Instead, incorporating more covariates actually strengthens the argument that confounders are the true determinants of incumbent performance; there is a negative relationship between a precinct's gentrification risk and Ted Wheeler's change in vote share. Additionally, higher median age and auto commuting areas punished Wheeler more in 2020, even when examining the positive interaction variable which dampens but does not cancel out this effect at a reasonable range. Population density, too, is statistically significantly associated with higher Wheeler vote change between precincts.

An ANOVA test on *Models 2*, *3*, and *4* supports that baseline *Model 1*, with only the original confounders, may be a better fit [see Appendix: Data]. Only the F-statistic for measuring *Model 2* against *Model 1* is significant at the 95% confidence level, though since I perform many comparisons, a more conservative p-value than 0.05 may be needed. Thus, I do not reject the null that any model including some form of T_{grid} is better than *Model 1.*

Treatment 2 (T_{walk}), or precinct proportion overlap with protected lane 15-minute walksheds, provides a better metric of voter coverage by bike infrastructure at the expense of measuring its concentration. For the T_{walk} set of models, I take the T_{grid} set and replace only its protected infrastructure variables with their T_{walk} walkshed counterparts. I use the original T_{grid} variable for painted lanes given aforementioned computational issues: painted lanes are so common that their walksheds would cover most precincts completely. *Model 5* replaces the protected variable and interactions from *Model 2* with the T_{walk} walkshed variable for all lanes; *Model 5* is a similar walkshed counterpart to *Model 3*, and *Model 7* is the same to *Model 4.* Table 3.3 displays these, again excluding most interaction variables.

Table 3.3: *Model 1* and T_{walk} *Models 5*, *6*, and *7*.

	Dependent variable: Wheeler vote share change from 2016 to 2020			
	Model 1	*Model 2*	*Model 3*	*Model 4*
Prop. gentrifying area	-0.058^{***}	-0.066^{***}	-0.075^{***}	-0.060^{***}
	(0.020)	(0.023)	(0.023)	(0.022)
Prop. pre-gentrifying area	-0.059^{***}	-0.048^{***}	-0.046^{***}	-0.044^{***}
	(0.010)	(0.013)	(0.013)	(0.012)
Prop. auto commuting (pwBG)	-0.321	-0.531	-0.971^{**}	-0.764^{**}
	(0.325)	(0.373)	(0.382)	(0.378)
Median age (pwBG)	-0.008	-0.011^{*}	-0.018^{***}	-0.015^{**}
	(0.006)	(0.006)	(0.006)	(0.007)
Auto-age interaction var.	0.010	0.015	0.026^{***}	0.023^{**}
	(0.009)	(0.009)	(0.010)	(0.010)
Pop. density (10^{-3}/mi.^{2})	0.005^{***}	0.005^{***}	0.005^{***}	0.007^{***}
	(0.002)	(0.002)	(0.002)	(0.002)
Painted projects (as prop. of street grid)		0.337 (0.218)	0.409^{*} (0.223)	0.436^{*} (0.224)
Protected projects (as prop. walkshed overlap)		**−0.012** (0.058)		
Type 1 protected projects			**0.058**	
			(0.082)	
Type 2 protected projects			**−0.218***	
			(0.115)	
Type 1 protected projects (2019)				**0.431**** (0.186)
Type 2 protected projects (2019)				**−0.288** (2.661)
Type 1 protected projects (2020)				**0.165** (0.241)
Type 2 protected projects (2020)				**−18.144***** (6.799)
Constant	0.206	0.325	0.579^{**}	0.426^{*}
	(0.218)	(0.254)	(0.254)	(0.253)
Observations	78	78	78	78
R^2	0.414	0.467	0.551	0.643
Adjusted R^2	0.365	0.378	0.442	0.509
Residual Std. Error	0.029 (df = 71)	0.029 (df = 66)	0.027 (df = 62)	0.026 (df = 56)
Note:				*p<0.1; **p<0.05; ***p<0.01

Models 5, *6*, and *7* provide contradictory evidence that electoral bikelash occurred in Portland in 2020. In *Model 5*, I find regression coefficient -0.012 for the aggregate protected T_{walk}, or proportion overlap with 15-minute walksheds for protected bike lanes across all years and quality types. This means that Wheeler is expected to have performed 1.6% better from 2016 to 2020 in a precinct with *no* protected lanes within a 15-minute walk than in one one with full 15-minute walking access to protected lanes, *ceteris paribus*. This agrees with *H1*, but the coefficient is not significant. When I separate this effect by quality as in *Model 6* or quality and year as in *Model 7*, I find some statistically significant coefficients for protected bike lane walksheds. For instance, in *Model 7*, Wheeler is expected to have performed 43.1% worse from 2016 to 2020 in a precinct with no type 1 (flex post plus or minus some curb) protected lanes constructed in 2019 within a 15-minute walk than in one one with full 15-minute walking access to type 1 protected lanes constructed in 2019. A result like this goes against *H1*; *Model 6* and *Model 7* suggest that bikelash occurs against type 2 (full curb or grade-separation) bike lanes but the opposite occurs for type 1 lanes.

Interestingly, an ANOVA test finds a statistically significant F-statistic at the 95% confidence level for *Model 7* measured against the baseline *Model 1*, suggesting it may be a better fit. This model finds a positive effect for the former and a negative for the latter, which suggests that some infrastructure may "age well" with voters. However, ANOVA tests on *Models 5* and *6* fail to reject the null hypothesis that *Model 1* is a better fit for the data.

In the end, that *Model 7* produced significant results reveals little, as a lower p-value threshold than 0.05 may be necessary. A very conservative approach is similar to a Bonferroni correction: dividing the original p-value threshold by the number of models. Neither the significant p-value for type 1 protected projects in 2019 nor that for type 2 projects in 2020 meet this low threshold of $p^* = 0.05/6 \approx 0.008$ [see Appendix: Data]. Instead, as in the T_{grid} set, a main takeaway from T_{walk} *Models 5*, *6*, and *7* is that the original set of confounders – gentrification, age, driving – were negatively associated with Wheeler vote change as expected even when controlling for bike lane geography.

Discussion

Overall, I conclude that if any *electoral bikelash* occurred in Portland's 2020 mayoral primary, the geographical placement of infrastructure did not play a role. Treatments as constructed failed to produce convincing evidence that voting behavior in this election had a geographical relationship to bike infrastructure. Null results, however, still offer an interesting story. For one, the variables often associated with communities exhibiting bikelash – those living in gentrifying or risk-of-gentrifying areas, as well as older, driving areas – correlated negatively with change in Wheeler vote share in 2020, with high statistical significance across all models. An alternative hypothesis may be:

<u>Hypothesis 1_A</u> ($H1_A$): Bike lanes are unpopular among (a) older, car-commuting and (b) gentrifying neighborhoods, but they punish incumbents regardless of proximity to new lanes.

$H1_A$ suggests bikelashing communities' disapproval is toward a *citywide*, not hyper-local, bike agenda. It is reasonable to assume in the four years between elections, people gather some information about what is happening elsewhere in a city: through local news (perhaps bikelash reporting), traveling through other neighborhoods, and more. The take-away for pro-cycling politicians like Wheeler may be that incumbents need not worry about the *placement* of bike infrastructure, but rather bike lane *proliferation* and voters' perception of their wider agenda.

This idea requires further testing, and survey or experimental research may fill in the gaps. Polling about important issues in a local contest, which often focuses on standard issues like crime, could gauge whether these communities are truly aware of bike politics regardless of proximity.

The research decisions I made may have impacted the results. I avoided post-treatment variables in these models, but changes within and across precincts that my confounders could not predict certainly took place between elections. Therefore, these models far from capture the entire picture of change that may impact incumbent performance. Perhaps a different experimental design, such as looking at difference-in-differences changes between closer elections (a primary and a general election) may alleviate this. The variables I used present other problems; pwBG-projection leaves perfect accuracy out of the question. The timing of these variables, too, isn't ideal (block population, for instance, is taken from 2010); even a new apartment building in a Census block can mean hundreds of new residents, skewing any statistic.

Portland itself presents further challenges. Ideally, its precincts would be smaller and greater in number (like Seattle or NYC). Yet its ample bike lane data and the timing of Wheeler's elections make this worth accepting. The question of generalizability remains; Portland is more wealthy, younger, less Black, and less Hispanic than the median U.S. city with over 300,000 residents [see Appendix III]. Future research into electoral bikelash may want to investigate cities with various demographic, cultural, and economic characteristics. Still, I believe this is a strong analysis that rose to the challenge of numerous data and conceptual hurdles; it is, to my knowledge, the first attempt to quantify the oft-discussed phenomenon of *bikelash* in an electoral context.

Ted Wheeler advanced to and eventually won the general election in 2020. Bike lanes have not exited the conversation, but they are still wrapped up in bikelash issues revolving around gentrification and cars. For instance, in a gentrifying northeast Portland neighborhood, the city recently removed a bike lane after residents called out insufficient public engagement[30]. Soon-retiring Mayor Wheeler, however, is still moving forward with his cycling agenda, as indicated in a speech presented to bike advocates in September 2023[31].

In the next chapter, I move north to Seattle, WA, where similar bike infrastructure expansion in the 2010s allows me to test *Hypothesis 2* (*H2*) that lanes boost local turnout by increasing the visibility of local government in action.

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[30]https://bikeportland.org/2023/12/18/crews-have-scrubbed-off-ne-33rd-ave-bike-lanes-382601

[31]https://bikeportland.org/2023/09/14/mayor-wheeler-comes-out-at-councils-biggest-advocate-for-cycling-379437

Chapter 4

Bike lane heuristics in Seattle

This chapter aims to quantify the relationship between the geographic distribution of new, protected bike lanes and local turnout via a government *visibility* mechanism; by this, bike lanes are *heuristics* reminding residents of local influence. Specifically, I test:

<u>*Hypothesis 2*</u> *(H2): Bike lanes amplify local government visibility, increasing the salience of municipal elections and thus increasing local turnout.*

I examine change in voter turnout in two Seattle, WA, election cycles: parallel mayoral races in 2013 and 2017 [Figure 4.1], and City Council races in 2015 and 2019 [Figure 4.2]. I first cleaned precinct and election data, algorithmically merging changing precinct shapes across cycles. I then projected electoral factors including race, age, income, and election district fixed effects onto precincts. To test the impact of bike infrastructure, I categorized bike lanes by quality and year using several sources. My treatment (T_{walk}) uses precinct overlap with 15-minute "walksheds" for points imputed on bike lanes, or shapes representing areas that are a realistic 15-minute walk away from this infrastructure [Figure 4.3].

My set of analyses consists of linear regression models including confounders, treatment effects distinguishing by infrastructure implementation and quality, and interactions between these. To test whether whether voters assign bike lanes visibility increases to *local* government, I compare these to corresponding analyses of Seattle turnout change in two federal election cycles: 2014 and 2018 midterms, and 2016 and 2020 presidential elections.

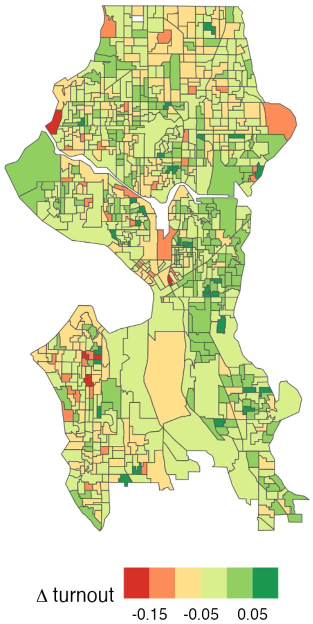

Figure 4.1: Seattle mayoral election turnout change, 2013-2017.

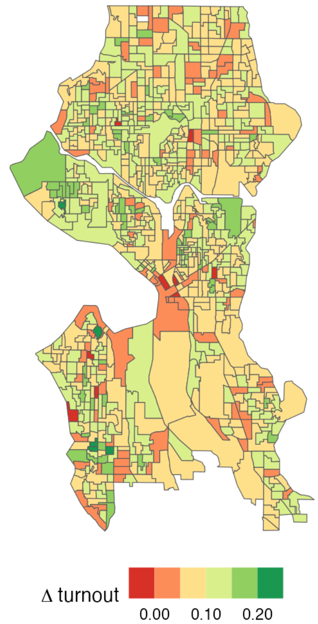

Figure 4.2: Seattle City Council election turnout change, 2015-2019.

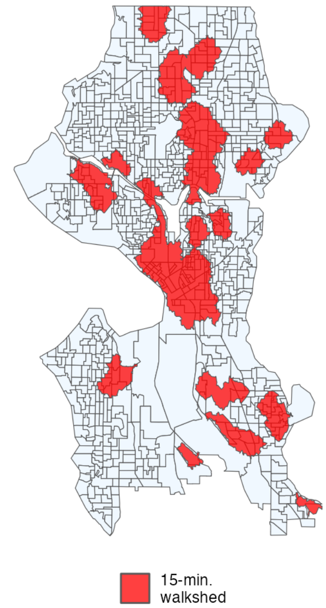

Figure 4.3: Protected bike lane aggregated 15-minute walksheds, all years.

My results may agree with *H2*, as they show that precincts receiving bike lanes saw significantly higher turnout decline in federal elections than in local contests. However, these findings are far from definitive; a different research design may be needed to isolate the effects of bike infrastructure placement and other neighborhood change.

I end by discussing the implications of these findings, including the concerning point that protected bike lane placement correlates with turnout decline in any case. If my alternative hypothesis is the case, future research may be important in reassuring residents concerned with neighborhood change that bike lanes do not *cause* this dilution of democracy, and, if anything, they dampen its effect at the local level.

Why Seattle, Washington?

Seattle, Washington, saw a bike infrastructure boom in the 2010s that arose not only from bike-boosting politicians like former Mayor Mike McGinn[1], but also from grassroots activism and popular concern with rising traffic deaths in the city. *Seattle Bike Blog*[2] founder Tom Fucoloro (2023, 127) describes Seattle's transportation policy shift in the 2010s as being primed by "a decades-long struggle to push back against a cars-first mentality." First, *sharrows* (shared roads) became standard, then came *painted lanes*, and finally, beginning in 2013, *protected bikeways* emerged. The political culture shift was swift: when activists installed an illegal "guerilla" lane on Cherry Street in 2013 using cheap pylons and adhesives, City Traffic Engineer Dongho Chang literally thanked them in a statement[3].

The story of Seattle's bike lane boom illustrates a feedback loop between local activism and increased local government *visibility*. A city with responsive government like Seattle is ideal for analyzing whether citizens respond back – namely, by turning out in local elections.

From a research perspective, Seattle shares many of the features that made Portland ideal for analysis. The city built an impressive network of bike lanes in the 2010s, and it releases accurate shapefiles of the infrastructure, categorized by quality. The city does not release clean lane implementation dates, but these exist elsewhere on the Internet, such as on expired planning pages, blogs, local news, and more.

Seattle also has many election precincts – up to 1,000 by the end of the decade. As a result, when I recycle the 15-minute walkshed treatment method (T_{walk}) used previously, treatment variables function much more like indicator variables, since it takes a smaller walkshed to encompass an entire Seattle precinct, on average. A large sample size and simpler treatment distribution make analysis in Seattle more precise and intuitive.

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[1] https://www.thebicyclestory.com/2014/11/mayor-mike-mcginn/

[2] *Seattle Bike Blog* (https://www.seattlebikeblog.com) is arguably the most prominent source for bike infrastructure news in the city, and I used it extensively in verifying data.

[3] https://www.seattlebikeblog.com/2013/04/04/guerrilla-road-safety-group-politely-installs-illegal-bike-lane-protectors-on-cherry-street/

Outcome: Voter turnout change

Seattle's local elections occur in November in odd years, and turnout fluctuated at around 50% of all registered voters in these from 2013 to 2019. The federal elections couched between these saw turnout around 80% of registered voters, with the exception of 2014 when midterm turnout tanked nationally[4]. I compare general election turnout across two odd-year election cycles, 2013 to 2017 and 2015 to 2019. The former corresponds to mayoral races, while the latter only had City Council candidates at the top of the ticket. This controls for turnout fluctuations based on the stakes of the local race as well as diverging demographic mobilization effects that may occur in different cycles.

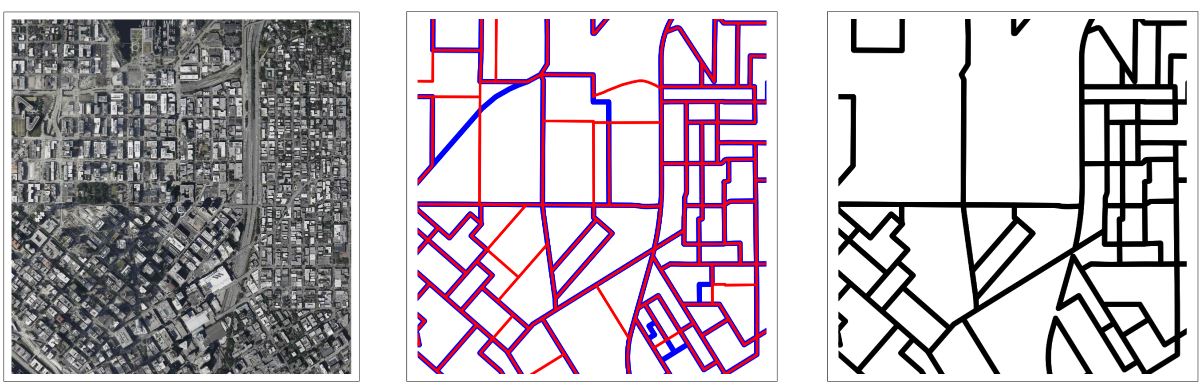
Election data came from King County, Washington[5]. I calculate voter turnout as a fraction of registered voters. Using turnout as a percent of registered voters means the outcome variable is not only impacted by eligible voters' decision to turn out, but also by changes in who registers, how voter lists are managed, and more. However, the net number of registered voters increased smoothly across odd-year elections in the city, from about 410,000 in 2013 to 475,000 in 2019, in line with population growth. The registered voter roll for the average precinct (as I define them next) grew by about 13% between these years; the standard deviation of voter roll growth was about 18%. This suggests a somewhat stable change in voter registration rates across the city.

Inspecting some of the precincts that saw the highest increase in voter registration, I found these are usually large redevelopment areas whose populations increased. Controlling for variables pre-treatment that may correlate with a neighborhood's future redevelopment, as I describe later, may help alleviate these outliers. Overall, this outcome variable calculation is reasonable and common, but keeping in mind the role of registered voter counts as an input is important in selecting confounders and in analysis.

[4] https://www.electproject.org/national-1789-present

[5] https://kingcounty.gov/en/legacy/depts/elections/results

Election precincts for Seattle came from the Washington Secretary of State[6]. These presented a second challenge, since many splits or small border changes occurred between every cycle in the decade. As a result, the number of precincts across the city grew from 942 to 1,000 precincts between 2012 and 2020. Since precincts are constructed from Census blocks, I identified each block with the precinct that encompassed it for each election. Then, I combined precincts that "swapped" Census blocks at any point; these came from the `censable`[7] package. I did this separately for odd-year local elections and even-year federal elections. For instance, if a given Census block was assigned to a hypothetical *Precinct 1* for all odd-year elections but switched to *Precinct 2* in the latest due to population shifts, I combined these over the entire set into a larger *Precinct 1.* The resulting two longitudinally-consistent, citywide datasets contained 833 precincts or combined precincts for odd-year elections and 815 for even-year elections. Figure 4.4 illustrates this process and what these combined precincts look like when focusing on the downtown area from 2013 to 2019.



(a) Downtown Seattle satellite image, from Apple Maps. (b) 2019 (red) precincts overlaid on 2013 (blue) precincts. (c) Data set with precincts merged across all elections.

Figure 4.4: Precincts in the window above split or morphed between 2013 and 2019, and I merged these shapes – along with their features – based on a common outline across all years. Not all changes are visible in this example; in a few cases, precincts in 2013 and 2019 matched, while differences within these appeared in 2015 and 2017 sets.

[6]https://www.sos.wa.gov/elections/data-research/election-data-and-maps/reports-data-and-statistics/precinct-shapefiles

[7]https://christophertkenny.com/censable/

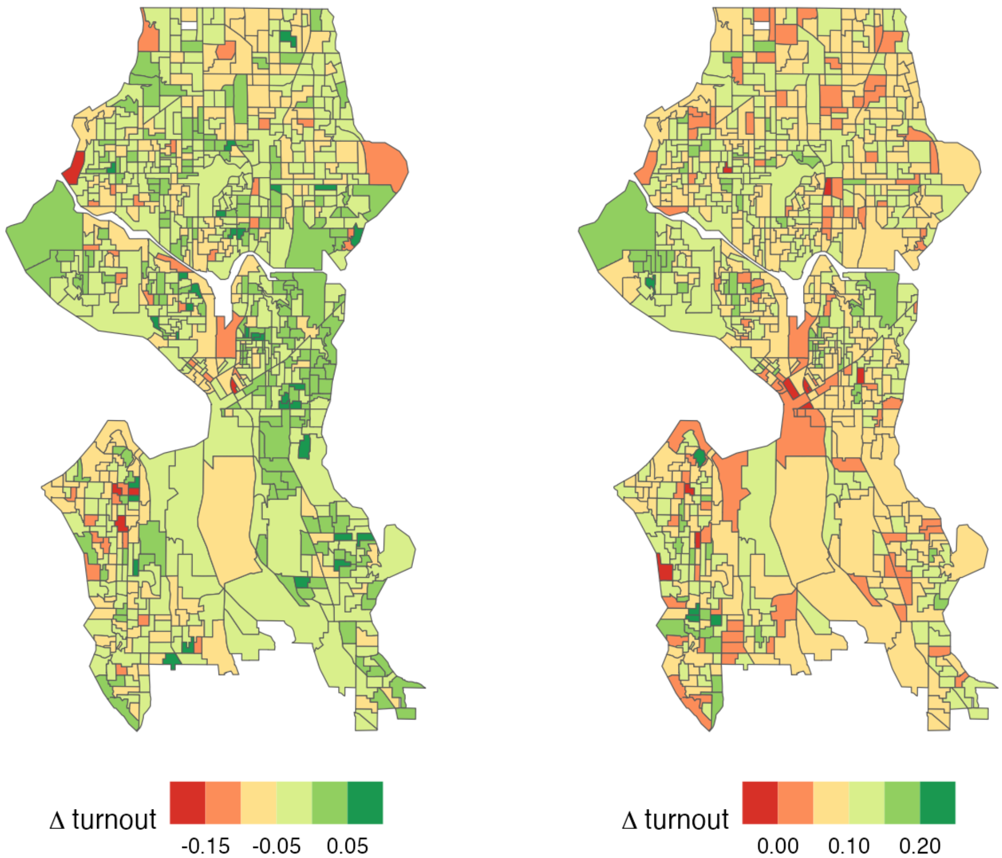
To validate these original algorithms, I inspected the odd-year set and merged precincts manually. This meant creating a *match-overlap* algorithm: I matched election precincts for a given cycle by ID and removed those with 99% overlap[8], which left many splits or precincts with small border changes – the *leftover* set. I then tediously hard-coded a column for joins across election years by inspecting side-by-side maps of leftover sets and the original precinct maps. Finally, I merged precincts until the match-overlap algorithm produced no leftovers.

The algorithmic and validation (semi-algorithmic) processes for odd-year cycles were similar, though the validation method resulted in slightly fewer combined precincts. I determined this is because my algorithm tested for block overlap with precincts by year, and in some cases, a freeway or trivial (empty) block switched precincts. Fixing this in the algorithmic approach by removing trivial blocks in 2010 was an issue, since redevelopment over the decade resulted in some empty blocks gaining residents between cycles[9]. Though validation ended up performing better, this was marginal; I opted for the algorithmic approach for consistency and replicability.

It is worth noting that precincts which split across cycles – or those that I algorithmically merged for the final set of precincts – may have different features than others. In the odd-year set, for instance, there are 86 *merged* precincts, and 747 *original* precincts whose boundaries did not change (many of these are the same in the even-year set). A two-sample t-test finds that in 2010, the merged set were younger, less affluent, and less white than the original set at the 95% statistical significance level; this suggests that precincts with splits changed more over the course of the decade as compared to wealthier, suburban-style neighborhoods. Whether redevelopment growth or gentrification drives this is unclear; these two concepts are intertwined. In any case, this result is not a significant issue, since I control for all three confounders in my analyses.

[8]This was a conservative threshold; some precincts with less than 99% overlap still exhibited miniscule visible change.

[9]This included major roadways converted to developable land: https://wsdot.wa.gov/construction-planning/major-projects/alaskan-way-viaduct-replacement-program



(a) $C_{O,1}$: 2013-2017 (b) $C_{O,2}$: 2015-2019

Figure 4.5: Proportion turnout change for both odd-year election cycles; turnout generally declined from 2013-2017 and generally rose from 2015-2019.

My precinct merge algorithm was not only a novel approach, it also maintained full precinct coverage of the city. In creating it, I avoided the problematic but common choice of simply filtering out mismatched precincts. For the odd-year set, for i in 833 precincts or merged precincts, and cycle $= C_O \in \{2017, 2019\}$[10] corresponding to the mayoral and City Council elections, the outcome variable is defined[11]:

$$\frac{(\text{Counted})_{C_O,i}}{(\text{Registered})_{C_O,i}} - \frac{(\text{Counted})_{C_O-4,i}}{(\text{Registered})_{C_O-4,i}} = (\Delta\text{turnout})_{[C_O-4 \text{ to } C_O],i}$$

[10] Each has first year election $= C_O - 4 \in \{2017, 2019\}$, as used in the formula.

[11] I used the same formula, of course, for the even year elections, with i in 815 precincts or merged precincts and cycle $= C_E \in \{2018, 2020\}$.

Treatment: Protected bike lanes

As in the previous chapter using bike infrastructure in Portland, I am primarily concerned with the geographical placement of *high-quality* lanes or cycleways around the city between each election cycle. These make up my treatment (T_{walk}) in analyzing the effect of bike infrastructure on voter turnout in Seattle. I make a few changes from the Portland analysis, however. First, I do not include painted lanes; this variable was insignificant in Portland, and as I explain, the Seattle data is much less organized at the outset.

Raw bike lane data comes from the City of Seattle[12], which maintains shapefiles covering all bike routes throughout the city, categorized by infrastructure quality. I filtered these for protected bike lanes and buffered bike lanes, as the city describes each, since these covered the high-quality protected lane types I used in the previous chapter. From the previous chapter, I reuse my definitions of *quality* and *timing* of bike lanes. Protected lanes are either of *type 1* (flex post or flex post / curb combination) or *type 2* (full curb or grade-separation). Using Google Street View, I classified all high-quality protected lanes as one of these two quality sub-types.

Since implementation dates, my definition for the "introduction" of lanes, were often missing or inaccurate in the raw data, I hard coded or verified all of these carefully. I used historical Google Street View data to find years before and after the infrastructure was installed [Figures 4.6 and 4.7], and filtered out those installed before late 2013 or after 2020 as evidenced by Street View images. Then, I used historical City of Seattle construction pages or the Wayback Machine[13] to research each lane's year of completion. For years where this was difficult, I was able to find posts on the popular *Seattle Bike Blog*[14] or several other

[12] https://data-seattlecitygis.opendata.arcgis.com/search?tags=bike

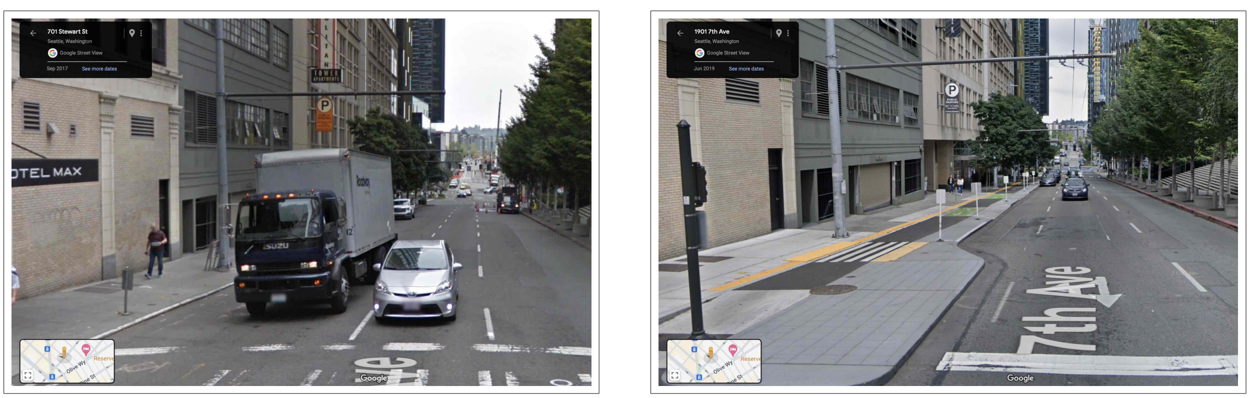
[13] Example: official city data cited 2010 as the installation date for Roosevelt Way NE lanes, but this planning page helped me correct it to 2015: https://www.seattle.gov/Documents/Departments/SeattleBicycleAdvisoryBoard/presentations/SBABBikeMasterPlan6modraftmap.pdf

[14] Example: official city data cited 2011 as the installation year for Mercer St. lanes, but this blog post helped me correct this to 2015: https://www.seattlebikeblog.com/2015/05/05/mercer-street-king-of-the-stroad/

sources (news sites, for instance) documenting the opening dates for remaining lanes. Sifting through planning documents or articles for each lane also allowed me to verify that most were constructed in a few months' time and all were complete before a November general election within its coded year due to seasonal construction timelines. I compiled these sources in an original data set [see Appendix: Data]. At the end of this process, I had a shapefile data set consisting of 108 *type 1* and 27 *type 2* projects constructed between 2013 and 2020 with accurate implementation years.

(a) Before: street parking, faded painted lanes. (b) After: flex post protection, parking removal.

Figure 4.6: Flex post (*type 1*) protected lanes installed on S Myrtle St. in 2019[15].



(a) Before: three traffic lanes, two-side parking. (b) After: two traffic lanes, one-side parking.

Figure 4.7: Grade-separated (*type 2*) protected lanes installed on 7$^{\text{th}}$ Ave. in 2017[16].

[15]https://www.seattle.gov/transportation/projects-and-programs/programs/maintenance-and-paving/current-paving-projects/swift-myrtle-othello

[16]https://www.seattle.gov/transportation/projects-and-programs/programs/bike-program/protected-bike-lanes/7th-ave-mobility-improvements

In this analysis, I use the walkshed treatment method ($\mathrm{T}_{\mathrm{walk}}$) described in the previous chapter to address who the *recipients* of bike lanes are. For each lane, I imputed points at 500 meter intervals (or centroids if the lanes were less than this threshold), and I used the `r5r` package to find polygons representing the area within a 15-minute walk from each point. This treatment is a more accurate capture of *coverage* of bike infrastructure projects, even though it doesn't capture *concentration* as accurately as the street grid proportion option ($\mathrm{T}_{\mathrm{grid}}$) did in the previous chapter. I use the walkshed approach here primarily because utilizing street grids is less feasible for smaller precincts: an extra block of a protected bike lane could mean an additional 10% of the precinct's street grid is covered, even though the marginal impact of such a bike lane extension may be small in the minds of nearby residents.

Finally, I created variables for whether a lane's implementation falls into each of the four election cycles (two odd- and two even-year). Depending on the election cycle I use for each regression, I group either all that fall between the two election years together, or group them by *type*, and compute the proportion of overlap between the walkshed and precinct area for the selected subset.

For the odd-year set, for i in 833 precincts or merged precincts, cycle $= C_O \in \{2017, 2019\}$ to filter lanes implemented between mayoral or City Council elections[17], and $c \in$ {protected (all); protected (subset by type)}, the treatment $\mathrm{T}_{\mathrm{walk}}$ is:

$$\frac{(\text{Bike lane walkshed overlap area})_{C_O,c,i}}{(\text{Total area})_{C_O,c,i}} = (\mathrm{T}_{\mathrm{walk}})_{C_O,c,i}$$

Figure 4.7 shows walksheds for lanes constructed between the 2013 and 2017 mayoral elections overlaid on precinct area. Next to this, Figure 4.8 shows the treatment variables, or overlap with protected lanes off both types, by precinct for this cycle.

[17] Again, I used the same formula for the even year elections, with i in 815 precincts or merged precincts and cycle $= C_E \in \{2018, 2020\}$.

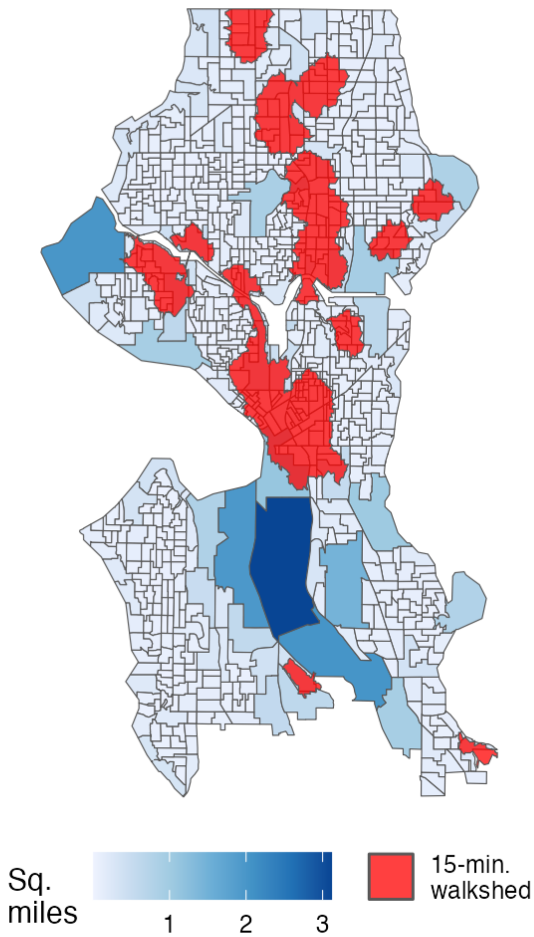


Figure 4.8: Protected bike lane aggregated 15-minute walksheds, 2013-2017, overlaid on precinct area.

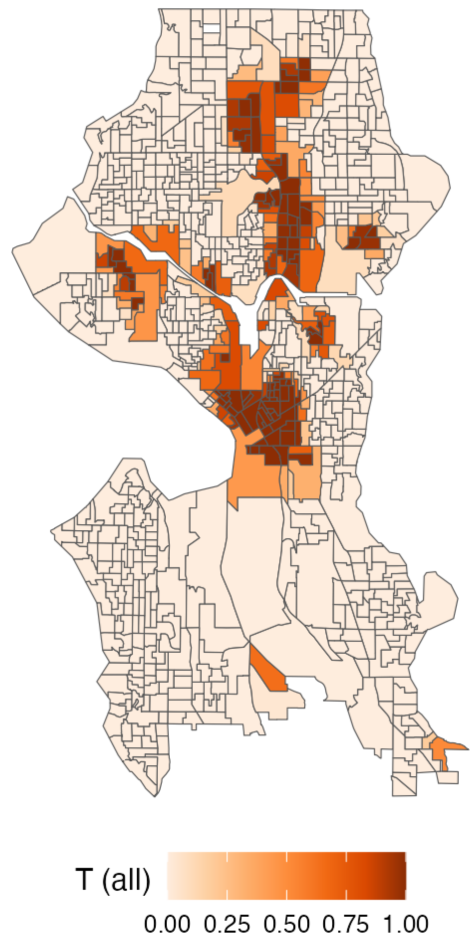


Figure 4.9: Protected bike lane aggregated 15-minute walksheds, 2013-2017, converted to proportion overlap with precinct.

Confounders: Race, age, income

To isolate the effect of *H2* regarding bike lanes increasing turnout (via an increased local government *visibility* mechanism), I control for precursors of precinct change that may affect the turnout calculation: race, income, and age. These double as crucial controls for cyclical election effects; unique groups are affected differently by candidates and issues across cycles. I also included previous turnout and district fixed effects as confounders.

The first confounder is race. Using the `censable` package, I collected population counts by race for each Census block in 2010 and combined these at the precinct level. I found multicollinearity between percent white, percent Black, and percent Hispanic was too high as indicated by these variables' respective variance inflation factors (VIF) in early models, so I included only percent white by precinct as a confounder. The average precinct in my merged data set for odd-year elections is about 70% white, although some are as low as 6% white. The most diverse precincts in the city are concentrated in the south and southeast neighborhoods; Seattle's 2nd City Council district encompassing these was only about 30% white in 2015[18]. Neighborhoods that were more white in 2010 were less at risk of rapid gentrification, given these were many of the lower-density, suburban-style neighborhoods.

I included median income as a second confounder, as lower income neighborhoods are by definition at higher risk of gentrification. I used the *population-weighted block group (pwBG) projection* [Figure 3.11] process outlined in the previous chapter to collect these: I gathered 5-year American Community Survey (ACS) median income estimates for Seattle block groups in 2013, "projected" these medians onto Census blocks (with a homogeneity assumption), weighted these by 2010 block population, and took their precinct-level average[19].

I also gathered median age because younger areas – universities, downtown, and more

[18]Council districts were first drawn in 2015 when the city switched from at-large voting; I do not use post-treatment data from this year, however. For demographics, see: https://projects.seattletimes.com/2015/city-council-districts/

[19]I divide the median income statistic by 10^3 for interpretability in the regression table.

– generally see more turnover in a city like Seattle, and these areas turn out at lower baseline rates regardless of race. This variable was available at the block level in the 2010 Census, although calculating this at the precinct level still meant taking an average of medians.[20]

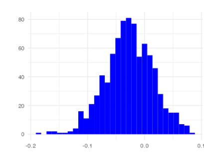
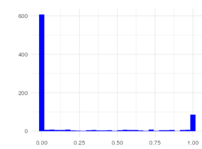
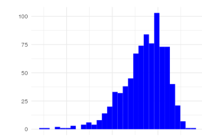
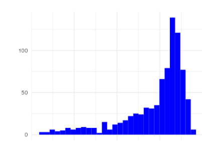
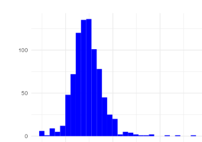
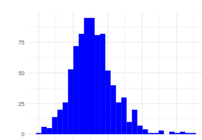
For electoral confounders, I first included the original cycle's turnout. Past turnout is a strong predictor of future turnout, and mathematically, precincts with lower initial turnout have higher turnout ceilings for the next election (and vice versa). *H2* implies that there should be larger changes in precincts with lower initial turnout, since these are precincts where local government is not as relatively salient to its voters before the introduction of infrastructure, and using baseline turnout as a confounder helps isolate this impact.

Finally, for the non-mayoral election years, where the top of the ticket likely varied across the city in terms of candidate quality and campaign efforts, I control for district fixed effects using variables for a precinct's overlap with each of the seven City Council districts. Most are zeros or ones, but a few precinct splits occurred across district borders, so the variable is technically not an indicator. I did the same for the federal midterm election merged precinct set to account for the two congressional districts that bisect Seattle. I constructed these fixed effects variables using the original precinct data.

Table 4.1 summarizes the variables I collected, their sources, an overview of the processes by which I cleaned and formatted these, and their histograms. All correspond to the first local election cycle, or the mayoral races; the "Dist" histogram uses the indicator variable for council district 1. The outcome variable histogram is normally distributed and centered near -5% turnout change from 2013 to 2017. Most precincts have near-zero or zero exposure to the treatment, though a decent fraction have full walkshed overlap; this holds for walkshed subsets by type not shown below. The "Auto" and "Income" variables appear normally distributed as is ideal for linear regression, while the "White" variable is left skewed.

[20]This is the best option and reasonable on its own; Census blocks have similar populations, and blocks grouped into the same precinct often have similar demographic characteristics due to urban segregation.

Table 4.1: Variables for analysis of *bike lane visibility turnout* in Seattle, WA.

	Variable[21]	Sources	Process	Histogram
Outcome	**Δturnout**: Turnout change by cycle	King County; Census GIS	Merging precincts across cycles, calculation	
Treatment	$\mathbf{T}_{\text{walk}}$: % bike lane 15-min. walkshed overlap	Seattle; local news; walksheds via `r5r`	Manual imputation; walkshed build; calculation	
Confounder	**bTO**: Baseline (election year one) turnout proportion	King County	Calculating proportion	
	White: % population white only ('10)	Census via `censable`	Calculating proportion	
	Age: Median age, weighted ('10)	Census via `tidycensus`	Weighted calculation	
	Income: *pwBG* median income ('13)	5-year ACS via `tidycensus`	*pwBG* computation	
	Dist: District fixed effects by cycle	King County	Calculating overlap	

[21]In all cases, "%" refers to *proportion*; these variables have range [0, 1]; in my regression, however, I use percentages with range [0, 100].

[21]Note that the Age variable was not computed via *pwBG* projection; these were available at the block level for 2010 and weighted by population up to the precinct level.

Regression and results

For each election of the two odd-year local election cycles, I test regressions that address nuance in bike infrastructure quality on the outcome variable (Δturnout). The baseline models, *Model 1a* for 2013-2017 and *Model 2a* for 2015-2019, include baseline election year turnout (bTO), 2010 percent white (White), 2010 population-weighted median age (Age), and 2011-2016 pwBG-projected median income (Income). The latter city council cycle also includes district level fixed effects (Dist). Importantly, across all regressions, I multiply Δturnout, bTO, and White by 100, such that these are in percentage point units.

Models 1b / 2b and *Models 1c / 2c* add variables for treatment (T_{walk}) to *Models 1a / 2a*, respectively. *Models 1b / 2b* include aggregated 15-minute walksheds for all infrastructure constructed between election years. *Models 1c / 2c* distinguish these walksheds by type (flex posts or flex posts and some curbs versus full curb or grade-separation).

The results for the *Model 1* set for 2013-2017 and the *Model 2* set for 2015-2019 are displayed in Tables 4.2 and 4.3, respectively.

The confounders are revealing in both sets of elections. All six models find statistically significant[22], positive associations between 2010 median income with turnout change, and a statistically significant, negative relationship between baseline turnout and change in turnout. The former is surprising, suggesting, *ceteris paribus*, areas that were more affluent in 2010 saw turnout rise in local elections over the course of the decade. The latter is unsurprising, since precincts with higher turnout to begin with have a lower ceiling for turnout improvement. Other confounding variable relationships are interesting; for instance, some City Council districts, like District 2, saw significant increases in turnout across the second set of elections, along with precincts that were more white in 2010.

[22]I use the significance threshold of $\alpha = 0.05$ throughout.

Table 4.2: *Models 1a*, *1b*, and *1c* for mayoral cycle 2013-2017.

	Dependent variable: Difference in % turnout, 2013-2017		
	Model 1a	*Model 1b*	*Model 1c*
2013 % turnout	−0.146***	−0.152***	−0.156***
	(0.024)	(0.025)	(0.025)
% white, 2010	−0.010	−0.008	−0.007
	(0.010)	(0.011)	(0.010)
Median income (pwBG) $\times 10^{-3}$	0.051***	0.050***	0.050***
	(0.006)	(0.006)	(0.006)
Median age (pop.-weighted)	−0.018	−0.019	−0.016
	(0.026)	(0.026)	(0.026)
Protected lanes (walkshed overlap divided by area)		**−0.393**	
		(0.440)	
Type 1 protected lanes			**−0.377**
			(0.458)
Type 2 protected lanes			**−1.012**
			(0.977)
Constant	2.641***	2.968***	3.063***
	(0.951)	(1.019)	(1.015)
Observations	833	833	833
R^2	0.108	0.109	0.110
Adjusted R^2	0.104	0.103	0.104
Residual Std. Error	3.991 (df = 828)	3.991 (df = 827)	3.990 (df = 826)
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 4.3: *Models 2a*, *2b*, and *2c* for City Council cycle 2015-2019.

	Dependent variable: Difference in % turnout, 2015-2019		
	Model 1a	*Model 1b*	*Model 1c*
2015 % turnout	-0.151^{***}	-0.166^{***}	-0.169^{***}
	(0.023)	(0.024)	(0.023)
% white, 2010	0.094^{***}	0.099^{***}	0.092^{***}
	(0.013)	(0.014)	(0.014)
Median income (pwBG) $\times 10^{-3}$	0.037^{***}	0.035^{***}	0.033^{***}
	(0.006)	(0.006)	(0.006)
Median age (pop.-weighted)	-0.058^{**}	-0.056^{**}	-0.036
	(0.026)	(0.026)	(0.026)
Council district 1	0.669	0.241	-0.325
	(0.485)	(0.504)	(0.525)
Council district 2	3.050^{***}	3.070^{***}	2.065^{**}
	(0.778)	(0.775)	(0.821)
Council district 3	1.599^{***}	1.653^{***}	1.076^{*}
	(0.547)	(0.544)	(0.563)
Council district 4	-0.609	-0.648	-1.262^{**}
	(0.526)	(0.524)	(0.548)
Council district 5	0.159	-0.171	-0.840
	(0.520)	(0.529)	(0.559)
Council district 6	0.479	0.170	-0.296
	(0.497)	(0.505)	(0.519)
Protected lanes (walkshed overlap divided by area)		$\mathbf{-1.289}^{***}$ (0.436)	
Type 1 protected lanes			$\mathbf{-0.587}$
			(0.472)
Type 2 protected lanes			$\mathbf{-3.404}^{***}$
			(0.945)
Constant	8.623^{***}	9.537^{***}	10.165^{***}
	(1.010)	(1.051)	(1.058)
Observations	833	833	833
R^2	0.144	0.153	0.165
Adjusted R^2	0.133	0.142	0.153
Residual Std. Error	3.739 (df = 822)	3.721 (df = 821)	3.695 (df = 820)
Note:			*p<0.1; **p<0.05; ***p<0.01

More important for my analysis, however, are the protected cycling infrastructure variables. These coefficients – both those for all infrastructure and those split by type – are insignificant for the 2013 to 2017 mayoral cycle. Interestingly, however, for the lower-stakes City Council cycle from 2015 to 2019, this is not the case. In *Model 2b*, I find a statistically significant coefficient for the aggregated treatment variable (all protected infrastructure walkshed overlap) of -1.29. This suggests that, *ceteris paribus*, a precinct with complete overlap with new protected infrastructure walksheds averaged a 1.29 percentage point decrease in turnout in the next election, compared to a precinct with no walkshed overlap. *Model 2c* shows that the coefficient for type 2 protected lane project walksheds is statistically significant, while that for type 1 infrastructure is not, and the magnitude of its negative association with turnout change is even greater. All coefficients for the treatment variables, significant or not, are negative. This contradicts *H2*, which says that proximity to new bike infrastructure increases local turnout.

Next, I run the same regressions on federal elections to examine whether association between bike infrastructure and turnout is truly *local*. Table 4.4 summarizes the baseline models for the federal midterm cycle from 2014 to 2018, while Table 4.5 shows these for the higher-turnout presidential cycle from 2016 to 2020 (*Models 3a* and *4a*, respectively), in addition to corresponding models containing covariates and walkshed treatments, either aggregated (*Models 3b* and *4b*) or separated by type 1 versus type 2 protected infrastructure (*Models 3c* and *4c*) between respective election years. The *Model 3* set also includes a fixed effects indicator for Washington's 7^{th} congressional district in the 2010s, since the city is split by two districts.

Again, the relationship between confounders and the outcome variable, turnout change between cycles, is unsurprising. Older precincts in 2010 saw relatively lower turnout change between the 2014 and 2018 midterms, the latter of which some described as a "blue wave" midterm of young voters. Previous election cycle turnout is strongly predictive of turnout change, likely in part because there's more room to grow for low-turnout precincts.

Table 4.4: *Models 3a*, *3b*, and *3c* for midterm cycle 2014-2018.

	Dependent variable: Difference in % turnout, 2014-2018		
	Model 3a	*Model 3b*	*Model 3c*
2014 % turnout	-0.391^{***}	-0.399^{***}	-0.404^{***}
	(0.021)	(0.021)	(0.022)
% white, 2010	0.153^{***}	0.156^{***}	0.157^{***}
	(0.013)	(0.013)	(0.013)
Median income (pwBG) $\times 10^{-3}$	0.007	0.006	0.005
	(0.005)	(0.005)	(0.005)
Median age (pop.-weighted)	-0.210^{***}	-0.210^{***}	-0.202^{***}
	(0.021)	(0.021)	(0.022)
Congressional district 7	-2.224^{***}	-2.243^{***}	-2.242^{***}
	(0.529)	(0.527)	(0.528)
Protected lanes (walkshed overlap divided by area)		$\mathbf{-0.814}^{**}$ (0.374)	
Type 1 protected lanes			$\mathbf{-0.087}$
			(0.407)
Type 2 protected lanes			$\mathbf{-1.608}^{**}$
			(0.766)
Constant	43.734^{***}	44.237^{***}	44.115^{***}
	(0.884)	(0.912)	(0.908)
Observations	815	815	815
R^2	0.487	0.490	0.491
Adjusted R^2	0.484	0.487	0.486
Residual Std. Error	3.361 (df = 809)	3.353 (df = 808)	3.354 (df = 807)
Note:			*p<0.1; **p<0.05; ***p<0.01

Table 4.5: *Models 4a*, *4b*, and *4c* for presidential cycle 2016-2018.

	Dependent variable: Difference in % turnout, 2016-2020.		
	Model 4a	*Model 4b*	*Model 4c*
2016 % turnout	-0.191^{***}	-0.201^{***}	-0.200^{***}
	(0.021)	(0.021)	(0.021)
% white, 2010	-0.024^{***}	-0.023^{***}	-0.023^{***}
	(0.007)	(0.007)	(0.007)
Median income (pwBG) $\times 10^{-3}$	0.014^{***}	0.012^{***}	0.012^{***}
	(0.003)	(0.003)	(0.003)
Median age (pop.-weighted)	-0.012	-0.014	-0.015
	(0.014)	(0.014)	(0.014)
Protected lanes (walkshed overlap divided by area)		$\mathbf{-1.133}^{***}$ (0.276)	
Type 1 protected lanes			$\mathbf{-1.242}^{***}$
			(0.313)
Type 2 protected lanes			**0.455**
			(0.656)
Constant	21.313^{***}	22.413^{***}	22.418^{***}
	(1.421)	(1.433)	(1.433)
Observations	815	815	815
R^2	0.263	0.278	0.279
Adjusted R^2	0.259	0.274	0.273
Residual Std. Error	2.444 (df = 810)	2.420 (df = 809)	2.421 (df = 808)
Note:			*p<0.1; **p<0.05; ***p<0.01

Interestingly, in federal elections, I find that new high quality bike infrastructure walksheds may be even *more* predictive of turnout change between election cycles, again in the negative direction. Between 2014 and 2018, a precinct having complete overlap with new protected infrastructure walksheds was associated with a statistically significant average decline in turnout by 0.8 percentage points compared to a precinct with no overlap. Between the presidential elections of 2016 and 2020, this significant association was a decline in turnout of about 1.1 percentage points. Separating walksheds by protected infrastructure type indicates this association was only significant for type 2 infrastructure walkshed overlap between midterm elections and type 1 infrastructure between presidential races.

Across all except for the *Model 1* set, an ANOVA test found that at least one of the models with the treatment variables was a statistically significant improvement on the baseline model [see Appendix: Data]. This suggests that proximity to high-quality protected lanes are informative predictors of both local *and* federal election turnout, though in a direction I did not anticipate in developing *H2*.

Discussion

Overall, the results present a perplexing picture of the relationship between proximity to new bike infrastructure and change in turnout, but the disparity between local and federal turnout effects may align with a rephrasing of *H2*, which originally said proximity to new, protected bike lanes would increase turnout between local election cycles in contrast to a smaller or absent effect in federal cycles. In the higher-turnout, federal election cycles, proximity to new bike infrastructure appeared to have a stronger, more negative association with turnout change between election cycles than in lower-turnout local cycles. An alternative hypothesis may be:

Hypothesis 2_A ($H2_A$): Bike lanes amplify local government visibility, increasing the salience of municipal elections and thus dampening changing neighborhoods' local turnout decline.

One possible interpretation of this evidence supports the idea that proximity to bike lanes indeed has a positive, *local* effect on turnout. Generally, I found that treated precincts saw larger relative declines in turnout for federal elections when compared to untreated precincts, whereas in local elections, this was less of the case. The treatment of bike lanes, then, could be interpreted as the *reason* treated precincts saw less of a "turnout hit" in local elections than in federal elections. In other words, features of a precinct I could not measure – post-treatment features touching on demographic change – may have caused its relatively higher overall turnout decline; bike lanes and their contribution to the *visibility* of local government may have been what dampened this turnout decline in local elections.

To investigate this, I gathered 2020 Census block population data, merged this by precinct, and calculated 2013 precinct population change over the decade. I found a statistically significant positive association between precinct population growth and overlap with infrastructure walksheds in the decade [see Appendix II].

In short, treated precincts grew faster; the confounders I included may not have served as ideal precursors to post-treatment change. This or other changes associated with my treatment variable may be causing turnout to fall more, and bike lanes may be working against this in local elections as residents notice neighborhood changes and turn out.

There is a possibility, on the other hand, that bike lanes reduce turnout in federal elections more than local ones by some other mechanism – perhaps those who generally vote in federal elections and not local ones associate lanes with higher levels of government. But I believe an explanation like this is unlikely. Public engagement on these projects is led by local governments on social media, through mailers, and in-person; local candidates, unlike those at the federal level, campaign on bike lane issues.

$H2_A$ requires further testing, and an experimental design may be ideal to isolate neighborhood changes occurring at the same time bike lanes are installed. Identifying demographically-similar precinct pairs that saw similar growth, differing only in one of each pair receiving a bike lane, may be one option for isolating a bike lane effect.

Finally, this analysis also provides subtle nuance to *H1* on *bikelash.* Seattle was the fastest growing U.S. city with a population over 300,000 in the 2010s [see Appendix III], adding over 150,000 new residents in this period; its growth rate eclipsed even those of Austin, Fort Worth, and Denver at the time. *Change* is likely on the minds of its residents, particularly those more likely to engage in bikelash. That bike lanes, population growth, and turnout decline are all related, regardless of causal direction if any exists, helps explain why residents may resist bike infrastructure improvements in the first place.

Seattle's growth has since slowed while its bike expansion continues in full force. But more research on the relationship between bike lanes and turnout is important, particularly when residents may be justified in pointing to a negative association between the two at several election levels.

In the next chapter, I cross the country to Manhattan, New York City, where a vast network of bike share stations allows me to test *Hypothesis 3* that bike infrastructure improves democracy by making the act of voting itself easier.

Chapter 5

Biking to the polls in Manhattan

This chapter aims to quantify the relationship between the geographic distribution of bike share stations – or hubs with publicly available bicycles – and election turnout via a *mobility* mechanism; by this, bike share stations make traveling to the polls less costly. Specifically, I test:

> *Hypothesis 3 (H3): Bike share infrastructure supports transportation to polling places on Election Day, and in dense neighborhoods, this increases total electoral turnout.*

I examine ridership data from New York City's *Citi Bike* system using three sub-analyses focusing on its Manhattan borough between 2014 and 2018; these explore the relationship between Citi Bikes and Election Day sequentially via unique methods and data.

In *Analysis 1 (A1)*, I use a linear regression with several confounders to test whether Citi Bike station *introduction* to a precinct leads to higher turnout change between the 2014 and 2018 New York gubernatorial elections.

In *Analysis 2 (A2)*, I explore whether 2014 and 2018 Election Day Citi Bike usage is disproportionately higher than expected in the first place, building a weather-weekday model to predict station volume and comparing Election Day counts to these predictions.

In *Analysis 3 (A3)*, I look at whether results in the previous analyses may be impacted by the system layout itself, measuring Election Day station ridership against two treatment options capturing proximity to 2018 polling places.

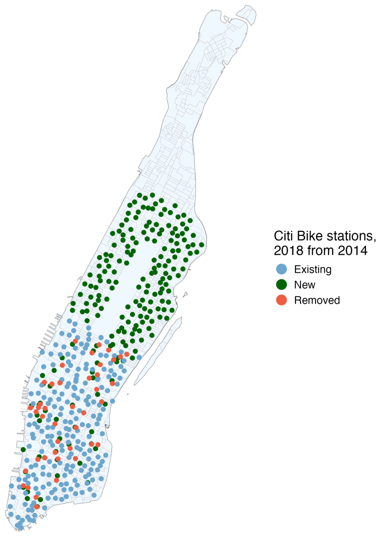


Figure 5.1: *A1* tests whether Citi Bike station introduction to a precinct leads to higher turnout increases between 2014 and 2018.

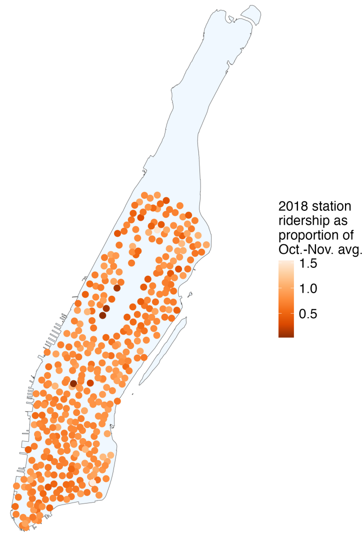


Figure 5.2: *A2* explores whether Citi Bike usage is higher than modelled expectation on 2014 and 2018 Election Days.

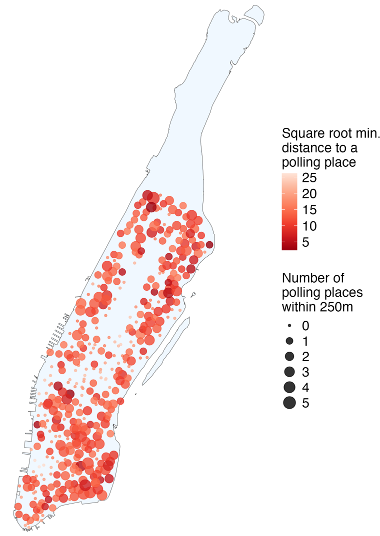


Figure 5.3: *A3* looks at whether Citi Bike station proximity to polling places in 2018 is related to higher Election Day usage.

In *A1*, I find that there is no significant relationship between Citi Bike stations and voter turnout changes between 2014 and 2018, even when controlling for confounders. In *A2*, I find that Election Day Citi Bike ridership is not higher than modelled expectation, meaning the Citi Bike system is unlikely to play a big role in voting transportation. In *A3*, I find a statistically significant relationship between Citi Bike station proximity to polling places and Election Day usage of that station.

I combine these findings to explain why the results may still support *H3*. Working backward: the Citi Bike network is not optimally designed to serve polling places, which may lead to average or below-average Election Day usage, which explains the lack of association between station presence and turnout. These analyses are far from definitive; other methods like Citi Bike user surveys may be helpful in identifying why people don't seem to bike to the polls in higher numbers despite growing bike infrastructure in cities. To boost biking to the polls, policymakers should consider a simple reform: place polling places near bike share stations.

Why Manhattan, New York City?

Manhattan, New York City, is home to the most expansive and popular bike share system in the nation: Citi Bike. What began in 2013 as 332 stations and 6,000 publicly-available bikes doubled to twice that size by 2017[1]; in August 2023, the system surpassed four million rides in a single month[2]. Citi Bike is still growing in popularity and has big expansion plans to corners of the region far beyond its roots in Lower Manhattan.

New York City is arguably the nation's most urbanist city: it has the most prominent public transportation system, the lowest car ownership[3], and is among the most walkable[4]. Urbanists, as such, see it as a bellwether for pro-cycling policies given not only its expanding Citi Bike system and bike lane network, but also high rates of bicycle use as a mode share. However, New York City is not a recent leader in voter turnout. In 2014, New York statewide turnout was the second-lowest in the nation[5]. This improved in 2018, but the state and city still had strict voting laws surrounding mail-in voting until recently.

Ironically, this chaotically varying turnout and higher in-person voting make Manhattan ideal for looking at cycling and going-to-the-polls in the 2010s. More importantly, though, Citi Bike releases incredibly valuable data documenting *every single* ride using the system, perfect for exploring bike movement on and around Election Day.

Finally, New York City leaders are concerned with the paradox of vibrant urban lifestyle and imperfect local democracy. Some solutions, like Citi Bike offering free rides on past Election Days[6], are unproven. As such, this chapter ultimately seeks ways to improve local democracy *using* bikes; if New York City can achieve this, the nation will follow.

[1] https://ny.curbed.com/2016/11/28/13770004/citi-bike-expansion-public-funding-possibility
[2] https://twitter.com/CitiBikeNYC/status/1697652570087375122
[3] https://www.nyc.gov/html/dot/downloads/pdf/nycdot-citywide-mobility-survey-report-2018.pdf
[4] https://www.walkscore.com/cities-and-neighborhoods/
[5] https://www.timesunion.com/tuplus-local/article/New-York-s-2014-voter-turnout-49th-best-in-the-US-6146753.php
[6] https://donyc.com/p/citi-bike-for-free-on-election-day

Citi Bike station data and analyses

Citi Bike data for New York City is extensive, stretching back as far as 2013, and files are managed by system operator Lyft Bikes and Scooters, LLC[7]. Publicly available annual or monthly files contain *every single Citi Bike ride*: from the moment a rider "unlocks" their bike to the moment they dock it. I refer these as the ride *start* and *stop*.

Citi Bike data is a trove of information on movement in New York City. A single row, which represents a Citi Bike ride, includes not only exact start and stop date and timestamps, start and stop stations, and coordinates for each station, but also sparse demographic information on the rider. For many rows, date of birth and gender are available. This data is available under Lyft's commitment[8] to "supporting bicycling as an alternative transportation option," but that the data is released in raw, non-differentially private form is surprising. Presumably, it would be possible to identify an individual by the available information and their commuting patterns. Lyft may want to consider adding noise in the future to protect privacy. That said, Citi Bike data has incredible research potential.

In examining *H3*, I am concerned with three aspects of the Citi Bike data; correspondingly, I performed three analyses utilizing it in different forms. For all analyses, however, I use a subset of Citi Bike data centered on Election Day, the first Tuesday after the first Monday of the month, in the years from 2014 to 2018. I filtered the data for rides up to two weeks before Election Week to rides up to two weeks after it. I also filtered out weekends, since leisure ridership is significantly higher on these days, and I am focused on mid-week Election Days. In summary, the data I used included 25 weekdays, or five weeks, for each year from 2014 to 2018. Though my five weeks do not span all of October and November, I refer to this as "Oct.-Nov." ridership data throughout. Since I examine Citi Bike Election Day ridership, which I hypothesize may be relating to voting, I filtered all rides ending between 5:30 AM and 9:00 PM (30 minutes before polls open to poll close).

[7]https://citibikenyc.com/system-data
[8]https://ride.citibikenyc.com/data-sharing-policy

Additionally, in my third analysis, I am concerned with rides *to* the polls on Election Day; I group all data by day, year, and trip *stop* station (where the bike was docked to end a trip), and count the total number of rides[9]. This gives me my final, overarching subset of Citi Bike ridership data: the total count of rides between 5:30 AM to 9:00 PM by station, date, and year, for weekdays in five weeks centered on Election Week, from 2014 to 2018.[10]

I next prepared the Citi Bike data subset for each of three analyses exploring unique ways in which an expansive bike share system may relate to voting: new station access increasing turnout (*A1*), system use above weekday-weather modelled expectation on Election Day (*A2*), and higher station use on Election Day when located closer to a polling place (*A3*). I first describe the Citi Bike data preparation for each and elaborate on these later.

First, in *Analysis 1* (*A1*), I am interested in exploring whether there is an association between turnout change and new Citi Bike stations in a precinct. *H3* suggests that this association would be positive, or Citi Bike stations would increase turnout as getting to the polls via bicycle is easier. Since I use the 2014 and 2018 elections in my analysis, I defined my treatment ($\mathrm{T}_{\mathrm{Citi}}$) as *whether a precinct had a new station in 2018 and no station in 2014*. This gives a simply metric of whether a precinct's voters had increased access to Citi Bikes. Note that a polling place is not always located *in* a precinct, but my grouping of the subsetted Citi Bike data here includes all existing stations, so $\mathrm{T}_{\mathrm{Citi}}$ still measures geographical access to a bike – regardless of whether one uses it to leave *for* or arrive home *from* the polls. To prepare Citi Bike data for computing $\mathrm{T}_{\mathrm{Citi}}$, I simply created two data sets of points: coordinates for all 2018 stations and the same for all 2014 stations. The regression using $\mathrm{T}_{\mathrm{Citi}}$ as a treatment includes turnout change from 2014 to 2018 as an outcome variable with demographic confounding variables and electoral fixed effects.

Second, in *Analysis 2* (*A2*), I am interested in exploring whether there is an association

[9] In *Analyses 1* and *2*, I explain, this grouping by trip end is less important; I use station *presence* (where ridership count is unimportant) or *direction-independent volume* on Election Day (where I am not concerned with whether one is arriving or departing from a station, only whether total usage is higher), respectively.

[10] Since my analysis applies to Manhattan, New York City, this data is also filtered to stop stations within New York County.

between expected system use (controlling for weather and day of week) and actual system use on Election Day. While unrelated directly to *H3*, this helps answer the *extent* to which bikes are used for transportation in traveling to the polls, and thus helps explain the results of *A1*. For this analysis, I first calculated *average* stops by station for each year in the Oct.-Nov. period. I group by year because system ridership increased overall annually as the network expanded, and taking averages by year accounts for this annual fixed effect. Next, for each station and day, I compute the proportion of total stops to Oct.-Nov. annual average stops; this gives a relative metric of that station's use each day. This metric, *the proportion of daily station stops to annual Oct.-Nov. average* (P_S), is my outcome variable in a regression I run across all days, stations, and their proportions, using weather conditions and weekday as predictors. From this model, I calculate what the *expected* proportion of Election Day rides in 2014 and 2018 should be, and compare the actual proportion to this.

Third, in *Analysis 3* (*A3*), I am interested in exploring whether there is a relationship between station *proportion of Election Day station stops to Oct.-Nov. average* (P_S) and proximity to polling places in 2018. *H3* suggests that there will be, since it rests on the idea that transportation, including walking, is a barrier to voting, and station use may be lower on Election Day if it is less convenient to access a polling place by docking at it. I use two treatments to calculate distance: square root of distance from a station to the nearest polling place (T_{dist}), and the count of polling places within a 250m radius (T_{count}). I chose the 250m cutoff as it seems realistic; polling places within this are very convenient to access, about a 2-minute walk from a station, meaning these stations may be Election Day "hotspots." The only Citi Bike data preparation this section required was creating the 250m buffers around each station for T_{count}.

These three analyses – which respectively address new station access and turnout, Election Day system use compared to modeled volume, and Election Day station use and proximity to polling places – are summarized in Table 5.1 below. The following sections describe additional data required for each and present their results.

Table 5.1: *Analyses 1*, *2*, and *3*, applying Citi Bike station data to various aspects of 2014 and 2018 elections in Manhattan.

Description	Sources	Data preparation[11]
Analysis 1 (*A1*): Linear regression, comparing merged precinct turnout change between 2014 and 2018 elections against the presence of new bike share stations (T_{Citi}) and demographic confounders	State of NY; VEST; Census via `censable`; 5-year ACS via RDH and `tidycensus`	Precinct merges across 2014 and 2018; Confounding variable collection (*pwBG*, etc.); Treatment calculation (*new* station presence with *no existing* stations)
Analysis 2 (*A2*): Investigating Election Day turnout as proportion of Oct.-Nov. mean by station and year; comparing value to predicted proportion via weather/weekday annual model	NWS Central Park historical data via Kaggle	Calculating average station stops, Oct.-Nov. by year; Calculating proportion daily stops to average; Regressing proportion on weather and weekday; Calculating predicted Election Day proportion
Analysis 3 (*A3*): Linear regression, comparing 2018 Election Day turnout as proportion of Oct.-Nov. mean by station in against two treatment options: square root distance to nearest polling place (T_{dist}) and count of polling places within a 250m radius (T_{count})	NYC Open Data; city addresses via `tidygeocoder`; Google Earth	Imputing missing polling place coordinates via OSM data or manually; Identifying nearest polling place for each station and computing distance; Computing 250m buffers for each station and calculating number of polling places within (T_{count})

[11] Not including initial preparation of Citi Bike station data.

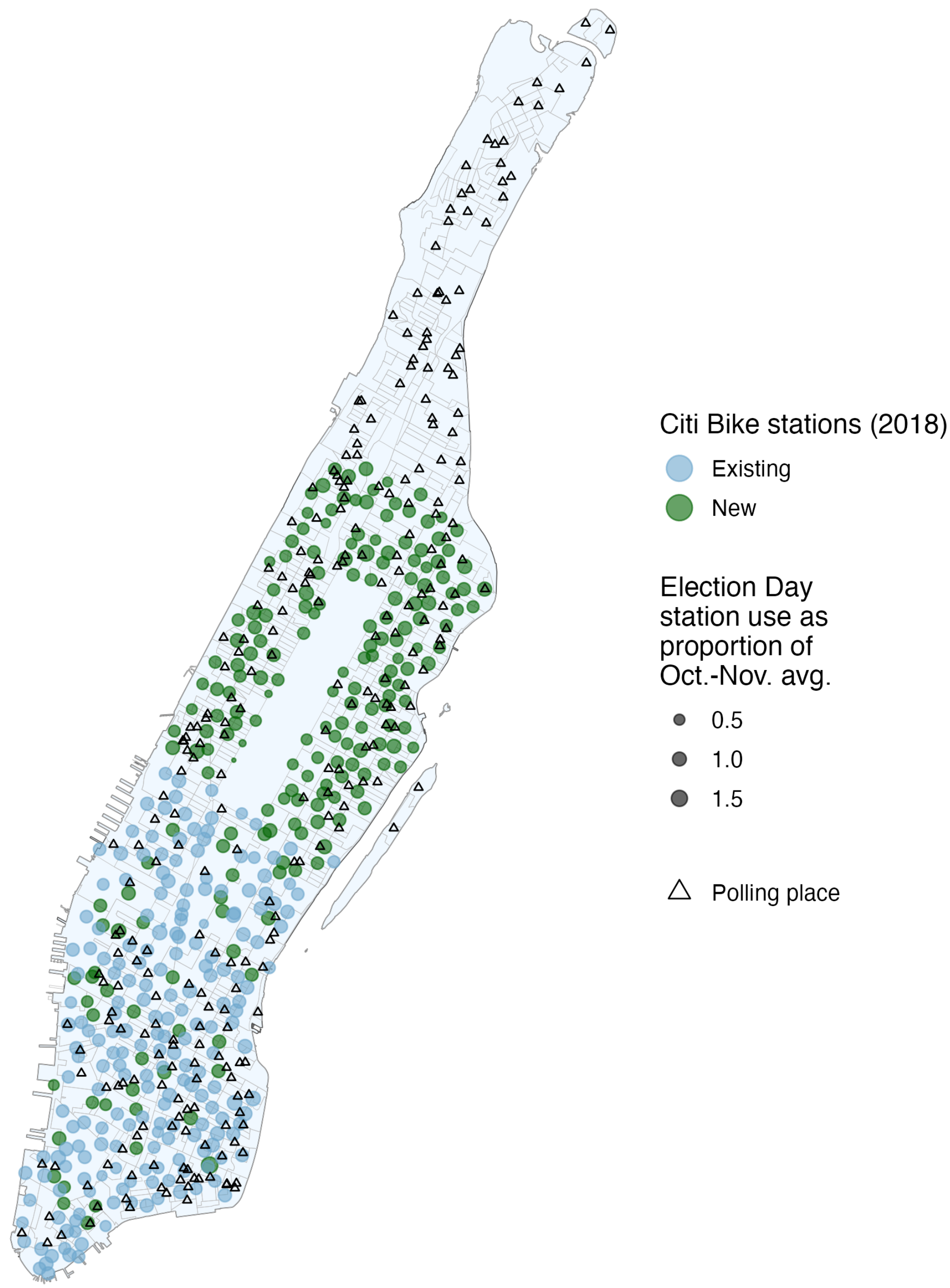


Figure 5.4: Map of Manhattan including variables relating to each of my three analyses: precincts and new stations for *A1*, station volume for *A2* and *A3*, and polling place locations for *A3*.

Turnout effect of new Citi Bike stations (*A1*)

In *Analysis 1* (*A1*), I begin by testing the impact of new Citi Bike stations within a precinct on its voting turnout change from 2014 to 2018. *H3* expects this relationship to be positive; that is, a new station makes it easier to get to the polls, thereby making voting less costly and increasing turnout.

Data required for this analysis, aside from Citi Bike station data for 2014 and 2018, includes election precinct shapefiles and returns for these years. For 2014, I gathered precincts from cartographer Nathaniel Kelso[12] and results from OpenElections[13]. For 2018, both precincts and results came from the University of Florida Voting and Science Election Team (VEST)[14] for 2018. I filtered both for New York County, or Manhattan.

The initial Manhattan election precinct shapes were difficult to clean and visually nonsensical. Some precincts were disjoint shapes; several snaked through neighborhoods as if they were gerrymandered. And, of course, many precinct shapes did not match across the years, as election officials modified borders to serve changing populations. As in the previous chapter (where I describe the precinct merge process more extensively [Figure 4.4]), I identified the Census blocks that constituted each of the 2014 and 2018 precincts[15] and merged precinct shapes that shared Census blocks across the two data sets. The process worked slightly less smoothly than in the previous chapter, and a handful of precincts – including those encompassing parkland or whose shapes erroneously cut through block borders – did need to be removed due to lack of block alignment or incorrect raw data[16]. However, these were few in number, and the resulting longitudinally-consistent data set covered over 95% of all populated Manhattan Census blocks. The final data set contained 775 original or combined precincts.

[12]https://github.com/nvkelso/election-geodata/tree/master

[13]https://github.com/openelections

[14]https://election.lab.ufl.edu/data-archive/

[15]Again, using blocks from `censable`: https://christophertkenny.com/censable/

[16]There appeared to be no solution to this, as blocks covering these showed no demographic information. Issues like this are common in New York election data, but the source of the problem is unknown here.

To build the treatment for my *A1* regression ($\mathrm{T}_{\mathrm{Citi}}$), I found whether each precinct contained at least one 2018 Citi Bike station and whether each contained at least one 2014 Citi Bike station, using the data described earlier. Then, if the former was true and the latter was not, I coded the precinct as treated, since my treatment is *whether a precinct had a new station in 2018 and no station in 2014*. Figure 5.6 visualizes that most Citi Bike expansion during this time period occured along Central Park on the Upper East and West Sides. Expressed technically with indicator variables where $I = 1$ if true and 0 otherwise, and $n_{y,i}$ equalling the number of stations within precinct i in year y, for i in 775 precincts, the indicator treatment variable $\mathrm{T}_{\mathrm{Citi},i}$ is:

$$I\big(n_{2018,i} > 0\big) \cdot \Big[I\big(n_{2018,i} > 0\big) - I\big(n_{2014,i} > 0\big)\Big] = \mathrm{T}_{\mathrm{Citi},i}$$

As in the previous chapter, I computed turnout by dividing the number of votes at the highest common level of office, or the gubernatorial election, by the number of active registered voters. Change is computed as the difference between this statistic for 2018 minus that for 2014. Figure 5.5 shows relatively uniform turnout increase across Manhattan. For i in 775 precincts, the outcome variable is:

$$\frac{\big(\text{Counted}\big)_{2018,i}}{\big(\text{Registered}\big)_{2018,i}} - \frac{\big(\text{Counted}\big)_{2014,i}}{\big(\text{Registered}\big)_{2014,i}} = (\Delta\text{turnout})_{[2014\ \text{to}\ 2018],i}$$

Finally, I included many of the same confounding variables as the previous chapter: demographic characteristics were race, income, and age, and electoral controls were baseline year turnout and Congressional district fixed effects. The former are meant to control for fluctuations in voter mobilization from external factors, including salient political issues, outreach, and more. The latter control for either technical aspects of the variable construction (baseline lower-turnout precincts have more "room for improvement," mathematically) or more election cycle effects like campaign effectiveness that may differ, for instance, by competitiveness of a congressional seat. I gathered median income and median age via *population-weighted block group-projection* (pwBG) using 5-year ACS data from 2009-2014 from `tidycensus`[17] (see the Portland chapter for an in-depth description of pwBG [Figure 3.11]). I included 2010 percent white, Black, and Hispanic in this model given the complex diversity of Manhattan and VIF results indicating this does not cause an overfit baseline model; these variables came from underlying Census blocks used to combine precincts. Finally, Congressional district shapefiles came from the Redistricting Data Hub[18]; their units are proportion overlap with each precinct, which most cases was 0 or 1.

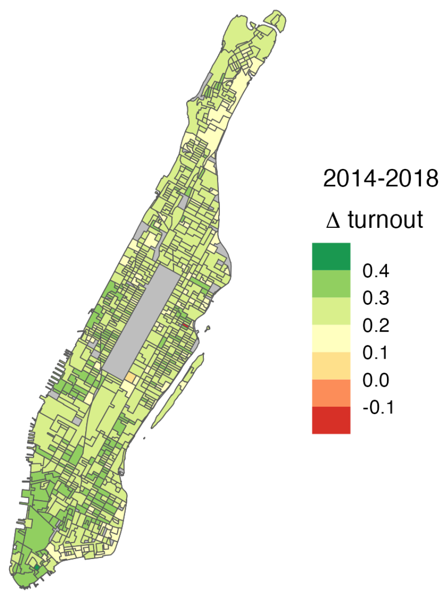


Figure 5.5: *A1* outcome variable.

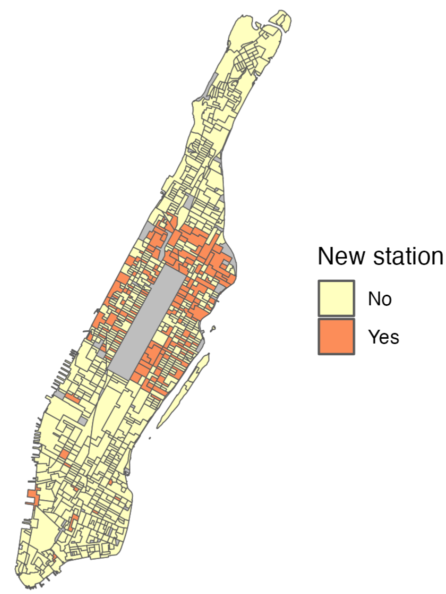


Figure 5.6: *A1* treatment variable.

Table 5.2 displays the results of baseline regression *Model 1a* with only confounders and *Model 1b* including the Citi Bike station treatment variable. *Model 1b* shows there is no statistically significant relationship between the presence of a new Citi Bike station and voter turnout change between these election years. Instead, some confounders help explain fluctuations in turnout: precincts that were older or more Hispanic saw statistically significant, relatively higher turnout declines, all else held equal, and those that were more white in 2010 saw a significant turnout increase.

[17]https://walker-data.com/tidycensus/
[18]https://redistrictingdatahub.org

Table 5.2: *Models 1a* and *1b*.

	Difference in % turnout, 2014-2018	
	Model 1a	*Model 1b*
2014 % turnout	-0.241^{***}	-0.244^{***}
	(0.029)	(0.029)
% white, 2010	0.100^{***}	0.102^{***}
	(0.020)	(0.020)
% Black, 2010	0.002	0.004
	(0.020)	(0.020)
% Hispanic, 2010	-0.071^{***}	-0.071^{***}
	(0.017)	(0.017)
Median income (pwBG) $\times 10^{-4}$	-0.003	-0.003
	(0.005)	(0.005)
Median age (pwBG)	-0.198^{***}	-0.196^{***}
	(0.027)	(0.027)
CD-07[19]	-65.056	-63.127
	(136.959)	(136.840)
CD-10	-61.895	-59.922
	(136.962)	(136.843)
CD-12	-63.751	-61.816
	(136.965)	(136.846)
CD-13	-60.611	-58.589
	(136.967)	(136.849)
New Citi Bike station		**-0.597**
		(0.386)
Constant	99.194	97.233
	(136.969)	(136.850)
Observations	775	775
R^2	0.399	0.401
Adjusted R^2	0.391	0.392
Residual Std. Error	0.040 (df = 764)	0.040 (df = 763)
Note:		*p<0.1; **p<0.05; ***p<0.01

Given there is no significant relationship between the placement of new Citi Bike stations and turnout, I now turn to *Analyses 2* and *3*, which uniquely examine Citi Bike stop volume on Election Day to test the potential role of bikes in going to the polls.

[19]Baseline turnout, race variables, and the outcome variable are multiplied by 100, so their units are percentages with theoretical range [0, 100], not proportions. Median income is divided by 10^4 for interpretability.

Modelled system use versus Election Day (*A2*)

In *Analysis 2* (*A2*), I test for differences between expected Citi Bike system volume on Election Day and expected volume on an ordinary day in Manhattan. This analysis is related to *H3* in that it is meant to explain the mechanism behind *A1*: it unpacks the extent to which bikes play a role in transportation on Election Day in the first place.

Election Day ridership is impacted by several variables, so a model predicting *expected* stop volume must include important factors of *weather* and *weekday*. Poor weather makes biking unpleasant and may suppress ridership; the day of week may explain work week-based cyclical commuting patterns (for instance, people may have more energy to bike to work on a Monday). Before controlling for these, comparing average Oct.-Nov. turnout to Election Day turnout for 2014 and 2018 yields contradictory results; in 2014, stations saw above-average ridership on Election Day, while the opposite was true for 2018.

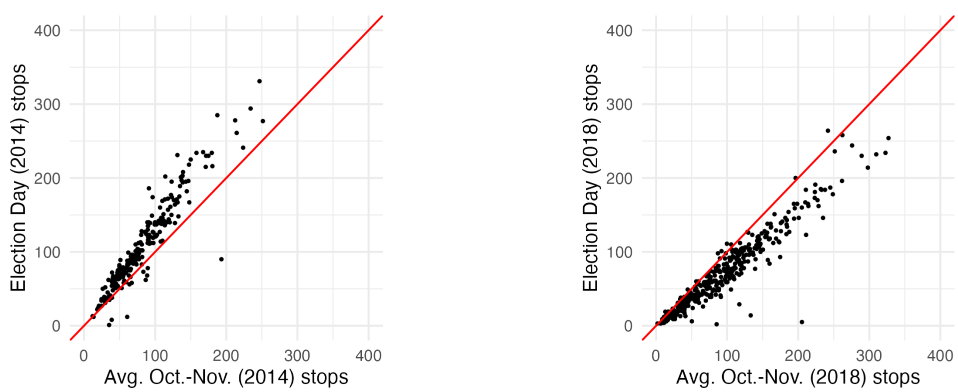


Figure 5.7: Average Oct.-Nov. turnout compared to Election Day turnout, by station and election year, with red $y = x$ line.

Weather data comes from the National Weather Service (NWS) via Kaggle[20]; this includes daily precipitation, snowfall, and snow depth in inches, as well as high and low temperatures, measured from Central Park since 1869. Weekday was already included in my cleaned Citi Bike data set.

[20]https://www.kaggle.com/datasets/danbraswell/new-york-city-weather-18692022

As described, for each station, day, and year, I calculated the *proportion of Election Day station stops to that year's Oct.-Nov. average* (P_S) as my outcome variable, a relative measure of ridership. Regressing on this, then, for a training set including all days *except for annual Election Days* each year from 2014 to 2018, produces a model explaining why station ridership is higher or lower than its average based on weather and day of week.

Table 5.4 shows my *weather-weekday model*. Rain and ridership were negatively correlated, while minimum and maximum temperatures are associated with higher volume. Station ridership is was higher earlier in the week. The positive association for snowfall is confusing[21], but the regression otherwise holds logically.

Table 5.3: *Weather/weekday model.*

	Prop. daily stops to annual Oct.-Nov. avg.
Precipitation (in.)	-0.453^{***}
	(0.004)
Snowfall (in.)	0.050^{***}
	(0.002)
Snow depth (in.)	-0.002
	(0.003)
Min. temperature (°F)	0.005^{***}
	(0.0003)
Max. temperature (°F)	0.013^{***}
	(0.0003)
Monday	0.040^{***}
	(0.004)
Tuesday	0.074^{***}
	(0.004)
Wednesday	0.064^{***}
	(0.004)
Thursday	-0.033^{***}
	(0.004)
Constant	0.079^{***}
	(0.008)
Observations	39,362
R^2	0.445
Adjusted R^2	0.445
Residual Std. Error	0.259 (df = 39352)
Note:	$^{*}p<0.1$; $^{**}p<0.05$; $^{***}p<0.01$

[21] The snowfall variable isn't cancelled out by snow depth, since the former is of higher magnitude and the latter is usually first measured after a snow day. Perhaps leisure ridership is higher on these days, or this effect is "cancelled out" by much lower temperatures and their variable effects.

Next, I use the *weather-weekday model* to predict ridership proportion to annual average for Election Day on 2014 and 2018. The former saw clear skies and a high temperature near 70°F, and the model predicts the proportion of Election Day 2014 ridership compared to 2014 Oct.-Nov. average to be 1.29, or 29% above-average ridership. For Election Day 2018, with weather in the fifties and almost an inch of rain, this proportion is predicted to be about 0.85, or 85% of average 2018 Oct.-Nov. ridership.

Finally, I look at the distribution of the proportion of Election Day rides to average Oct.-Nov. rides for 2014 and 2018 at all stations. These are shown below in Figure 5.8, where the red vertical line is the modelled proportion. A 95% confidence interval for the center of the 2014 distribution is $\left[1.273, 1.332\right]$, which contains the modelled proportion of 1.29. On the other hand, a 95% confidence interval for the center of the 2014 distribution is $\left[0.731, 0.764\right]$, which does not contain the modelled proportion of 0.85, meaning ridership on Election Day in 2018 was statistically significantly *lower* than expected. This is clear in the histogram, which shows a 2018 distribution shifted left of the red line.

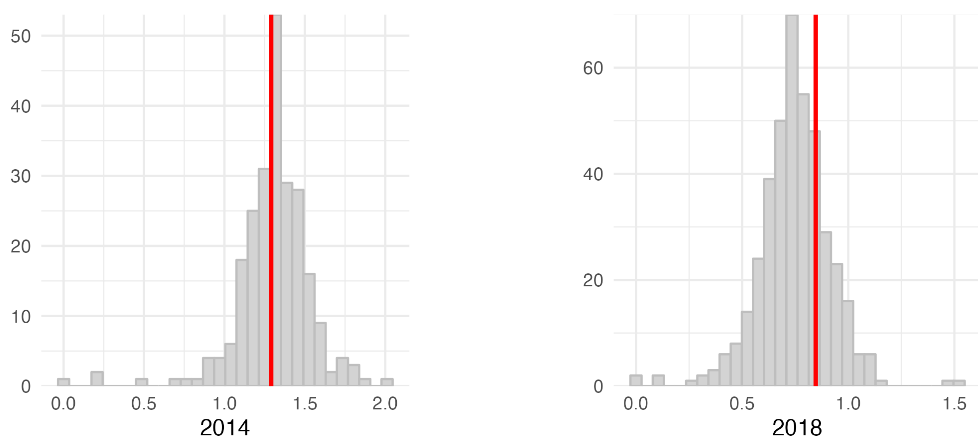


Figure 5.8: Election Day 2014 and 2018 ridership as a proportion of average annual Oct.-Nov. ridership by station, with red line *weather-weekday modelled* proportion.

This result suggests that Election Day Citi Bike ridership is at or lower than ordinary expectations. To see whether the distribution of Citi Bike stations relates to this, *Analysis 3* explores the relationship between stations proximity to polling places and ridership volume.

Election Day station use & polling place proximity (*A3*)

In *Analysis 3* (*A3*), I test for a relationship between Citi Bike station ridership as a *proportion of Oct.-Nov. average* (P_S) and proximity to polling places in 2018. This analysis helps explain the results of the previous two which contradict *H3*, or why I find that Citi Bike stations don't seem to boost turnout, nor do people use them disproportionately more on Election Day.

To do this, I gathered polling place data from NYC Open Data[22], which was only available for 2018. The data includes variables for the polling station name and address, but for about a sixth of the locations, the latitude and longitude variables were missing. To find missing coordinates, I used `tidygeocoder`[23] and polling place addresses, which recovered around half of the missing data. Using polling place names, I manually collected the remaining coordinates using Google Maps; these street addresses matched those in the data. Polling places were most frequently set up in schools, community centers, and large apartment buildings or complexes. The final data set has 274 polling places.

Next, I construct two treatment options that capture a Citi Bike station's *proximity* to a polling place. For the first treatment option (T_{dist}), I find the minimum *distance* (in meters) to a polling place from each Citi Bike station and take its square root[24]. For the second treatment option (T_{count}), I construct a 250m buffer around each Citi Bike station – which represents a few minutes in walking time – and *count* the number of polling places within this buffer. Note that the latter of these two, T_{count}, is similar to the walkshed method (T_{walk}) used in previous chapters as a measure of proximity. Here, though, I use buffers since Manhattan sits on a consistent street grid, so shapes are similar anyway. I use a several-minute walk instead of fifteen, as I am looking at the *convenience* of getting to a polling place by docking a Citi Bike nearby, and a fifteen minute walk is potentially costly

[22] https://data.cityofnewyork.us/City-Government/Voting-Poll-Sites/mifw-tguq/about_data

[23] https://jessecambon.github.io/tidygeocoder/

[24] I do this as the non-transformed statistic is right-skewed; using the right-skewed statistic, however, does not change the high-level results nor their statistical significance.

to voters. T_{dist} can be thought of as capturing the *maximum* convenience of a station, while T_{count} comes closer to capturing its *aggregate* convenience on Election Day in getting to a polling place. I reuse the 2018 Citi Bike station data set from *A1* to compute these. Figure 5.9 shows the distribution of polling places overlaid on Citi Bike stations, and Figure 5.10 shows Citi Bike stations colored and sized based on T_{dist} and T_{count}, respectively.

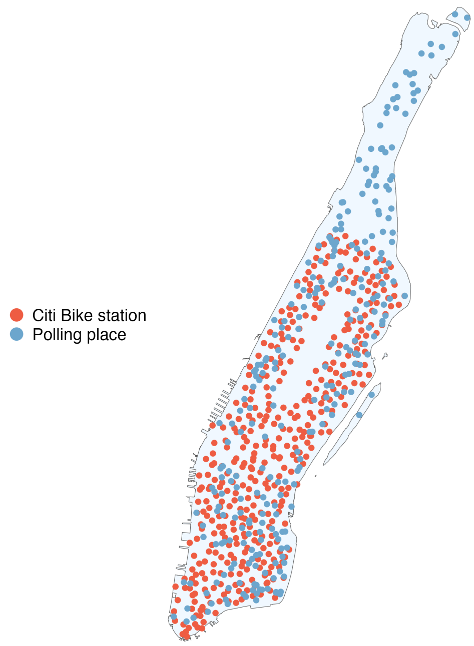


Figure 5.9: Polling places overlaid on Citi Bike stations, 2018, used to construct treament options T_{dist} and T_{count}.

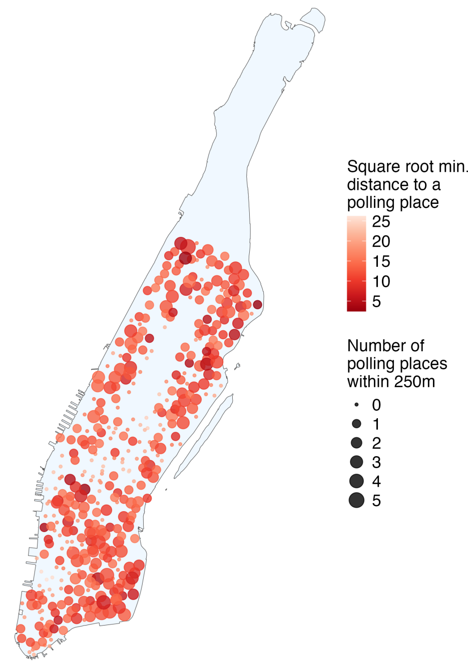


Figure 5.10: *Min. distance to* (T_{dist}) and *nearby concentration of* (T_{count}) 2018 polling places from stations.

Finally, I run a simple regression on Election Day station ridership as a proportion of 2018 Oct.-Nov. average ridership using each treatment: *Model 3a* uses the minimum distance option (T_{dist}) while *Model 3b* uses the nearby polling places option (T_{count}). Table 5.4 shows the results of these models below.

Table 5.4: *Models 3a* and *3b*.

	Prop. E-Day stops to annual Oct.-Nov. avg.	
	Model 3a	*Model 3b*
[Min. distance to a polling station (m)]$^{\mathbf{0.5}}$	$\mathbf{-0.007}^{***}$	
	(0.002)	
Number of polling places within 250m radius		$\mathbf{0.032}^{***}$
		(0.008)
Constant	0.844^{***}	0.713^{***}
	(0.028)	(0.012)
Observations	407	407
R^2	0.031	0.035
Adjusted R^2	0.028	0.033
Residual Std. Error (df = 405)	0.168	0.168
Note:		*p<0.1; **p<0.05; ***p<0.01

In *Model 3a*, I find a statistically significant negative relationship between square root minimum distance to the nearest polling place (T_{dist}) from a station and P_S, its stop volume as a proportion of the Oct.-Nov. average. The coefficient of -0.007 is very small, but it suggests that for a 100-meter increase in a station's minimum distance to a polling place – about a one-minute walk – the proportion of Election Day ridership to Oct.-Nov. average is expected to fall by about 0.07; if Election Day ridership matches the average, this change amounts to a 7% decrease. In *Model 3b*, I find a similar result: the coefficient for the number of nearby polling places to a station (T_{dist}) is 0.032, suggesting that an additional nearby polling place amounts to an increase of Election Day ridership of about 3% if it initially matched the Oct.-Nov. average [see Appendix IV for visualization].

These results offer some insight into why *A1* and *A2* produced little evidence regarding the impact or use of Citi Bikes on Election Day. Proximity to polling places matters in a station's Election Day ridership, as *Models 3a* and *3b* find that closer distance to polling places and more polling places nearby are associated with higher ridership. This suggests the Citi Bike system may not be ideally set up to facilitate transportation to the polls. As such, *H3* is difficult to measure, even using the nation's most prominent bike share system.

Discussion

The results of the three analyses from this chapter suggest that today, bikes play a small role in the act of voting itself in Manhattan, but policy options exist for bolstering that role. *Analysis 1* shows that turnout changes between 2014 and 2018 were not significantly associated with new Citi Bike stations. While this contradicts my original *H3* – that new bike share stations would boost turnout by making transportation to the polls easier – I find in *Analysis 2* that system volume on Election Day was near modelled expectation for a similar, ordinary day. This suggests that the Citi Bike system may be imperfectly poised to move voters, and *Analysis 3* supports this, demonstrating that stations see higher above-expected volume on Election Day when they are located closer to polling places. An alternative hypothesis may be:

Hypothesis 3_A ($H3_A$): Bike share stations support transportation to the polls on Election Day when these are close, but overall inefficient placement masks any turnout effect.

Citi Bike stations are deployed at busy intersections, commuting hotspots, and entertainment venues; polling places are set up in schools, community centers, and churches. That the two align imperfectly on Manhattan's grid is unsurprising, but policymakers should be aware of this dynamic. If New York City is honest in its goals of boosting turnout, it should consider making polling places even more convenient for Citi Bike users. This may amount to increasing the *number* of polling places: even after merging precincts, there were about three for every polling place. Balancing this ratio by setting up "bike-oriented" polling places may be doubly beneficial in promoting sustainable, convenient, and healthy transportation to the polls while also boosting turnout. Future research to investigate these claims and $H3_A$ may be led by the city itself: in an election cycle, officials could pilot more polling places easily convenient to Citi Bike users at popular nodes, reusing analyses similar to mine to identify whether these relate to higher turnout or Election Day ridership.

As in previous chapters, data quality and availability are consistent challenges in researching local elections. In *A1*, the best option for utilizing confounding variables were pre-treatment projected "estimates" from the block group level, while in *A2* and *A3*, my models for station volume were built without unavailable data that may also cause fluctuations use by station (such as entertainment events or nearby demographics). Partially overcoming data barriers to research is possible using the methods I develop across all chapters, but this may only go so far. Citi Bike, however, sets an impressive standard: its simple, comprehensive, and publicly available data makes analysis easier for academics studying urban politics and elections.

Still, the results I find, both in this chapter and previous ones, send strong preliminary signals about how bike infrastructure interacts with local elections. In Manhattan, I find that the nation's largest bike share system doesn't help move people to the polls on a significant scale. But more strategic Citi Bike station placement and expansion may play a role in changing this. Additionally, this may shape the trajectory of urban cycling in New York City as a whole. If proximity to polling places boosts station use on Election Day, one may assume the same applies to other destinations. The difference between placing future stations near grocery stores versus Madison Square Garden may also be the difference between a system catering to daily lifestyle versus basketball fandom. The former is certainly a wiser choice in tailoring a bike share system to *all* residents of the city it serves.

In the next, final chapter, I review and tie my findings together. Results from Manhattan and chapters on Portland and Seattle speak to previously unknown questions on the interaction of bike infrastructure with various aspects of elections. But since these cities are seen as leaders in pro-cycling policy, they also present lessons on the future role of bike infrastructure and local democracy across the country.

Chapter 6

Conclusion

Bike infrastructure's role in U.S. cities is set to grow immensely in the coming decades, and this growth is not only in cities leading the current cycling rankings. Even smaller localities are taking on the challenge of making transportation greener, reducing pedestrian deaths, and improving health. Portland, Maine, mirroring its Pacific Northwest namesake, has several high-quality bikeway projects in the pipeline and launched its own bikeshare system nearly two years ago[1]. Meanwhile, Manhattan, Kansas, with a population less than one-thirtieth of the size of the New York City borough, recently adopted a 20-year plan to expand its fledgling bike and pedestrian network[2].

The rise of bike infrastructure in U.S. cities will only make its role in urban electoral politics more important, but quantitative literature addressing this role in elections is currently absent. I offer first steps in closing those gaps. Additionally, I provide a guide for city leaders who may wonder about the implications of the bike infrastructure on their next election or local democracy itself. For instance, transportation planners may be interested in improving voting *and* mobility inequities at the same time, and they might have questions about bike lanes as a solution to this. A mayor might receive federal funding to build out a city's bike network but assume placing lanes in car-dominated neighborhoods means electoral defeat. My hope is this work, in addition to its academic contributions, helps the bike infrastructure movement by offering clarity beyond existing sparse, anecdotal evidence

[1]https://www.portlandmaine.gov/699/Bicycle-Pedestrian-Planning
[2]https://cityofmhk.com/1070/Bicycle-and-Pedestrian

on the electoral impacts of bike infrastructure.

Motivated by this, I first drew on related literature to formulate three hypotheses about how bike infrastructure interacts with elections. *H1* stated that backlash to bike lanes hurts incumbent re-election performance in precincts that receive these projects. *H2* said bike lanes make local government action more visible, increasing the salience of local elections and boosting turnout. Finally, *H3* asserted that bike share systems increase turnout by making transportation to polling places easier.

By looking at Portland, OR, Mayor Ted Wheeler's re-election performance using two bike lane treatment options, I found that <u>*H1* is not entirely true</u>. Older, car-commuting and gentrifying communities punished Wheeler at relatively higher rates, but this potential *bikelash* did not have a geographic component to it. I conclude that electoral bikelash may be real, but it is citywide, as people might become aware of a mayor's *overarching* platform and vote according to this information.

Through investigating turnout fluctuations in various elections in Seattle, WA, in the 2010s compared to a similar bike lane treatment method, I found that <u>*H2* generally holds</u>. While turnout change between cycles was relatively lower in precincts that received new bike infrastructure, this effect was weaker or insignificant in local contests. This suggests the bike lanes themselves may have encouraged higher engagement with local elections in areas that received them.

Finally, using Citi Bike system data in Manhattan, New York City, to study aspects of turnout and Election Day ridership, I found <u>*H3* may still be true, but is unrealized</u>. On one hand, new Citi Bike stations as a precinct treatment did not increase relative turnout change, and system ridership was not notably higher on Election Day. However, I show that a station's proximity to polling places is strongly associated with higher Election Day ridership. This hints at the distribution of stations themselves as the issue; if polling places and bike share stations were more closely aligned, evidence suggests biking to the polls would be higher.

The conclusions I reach in each analytical chapter lead to three respective policy recommendations. These are summarized in Table 6.1. I end with technical suggestions for cities regarding easing data challenges to studying public policy.

Table 6.1: Summary of analytical chapter takeaways and policy recommendations.

Case study	Takeaway	Policy recommendation
Chapter 3: *Bikelash and incumbency* in Portland, OR	Bikelash may exist among car-commuters or those concerned with gentrification, but voting patterns do not involve proximity to new bike lanes.	Elected officials should ignore political pressure incentivizing inefficient bike lane distribution.
Chapter 4: *Bike lane heuristics* in Seattle, WA	Bike lanes may dampen local turnout decline in the changing neighborhoods where they are commonly placed.	Local governments should work to convince residents that bike lanes do not cause unwanted neighborhood demographic change.
Chapter 5: *Biking to the polls* in Manhattan, NYC	Polling place proximity matters in biking to the polls, and bike share systems do not boost turnout as they are not aligned well with polling places.	Polling places should be moved or added nearer to bike share stations to make voting easier and boost turnout

First, elected officials should ignore political pressure incentivizing inefficient bike lane distribution. This may be tempting given the intimidating presence of bikelash. For instance, in 2017, a group of Minneapolis protesters were so angry about new painted bike lanes that they marched down the designated bicycle space (ironically safe from traffic) branding signs decrying these as "Nazi lanes"[3]. But my results from studying bikelash in Portland suggests these activists would vote against a pro-cycling incumbent even if those lanes were placed elsewhere. As such, loud voices like these should not prevent politicians from placing bike lanes where they will be most efficient, given they intend to push pro-cycling policies in the first place. This speaks to the larger phenomenon of NIMBYism, or loud pushback against

[3]https://www.bloomberg.com/news/articles/2017-10-18/how-a-minneapolis-bike-lane-satire-turned-into-reality

things like housing or transportation projects, and suggests that the role of geography in electoral backlash may be overstated among these groups.

Second, local governments should work to convince residents that bike lanes do not cause unwanted neighborhood change. The results from studying bike lane heuristics in Seattle relate to turnout, but on the surface they validate certain concerns of those engaging in bikelash. Namely, precincts receiving infrastructure saw larger relative turnout declines; some may see this and erroneously point to bike lanes as diluting neighborhood democracy (when I conclude the opposite). Local governments should address these concerns with care, working hard to convince residents of the good of bike lanes – while acknowledging the association of these with unwanted changes. This speaks to a larger tension between public neighborhood improvements and demographic change: governments should be honest with residents about connections between public goods and neighborhood change but work to reassure them, if it is the case, that this relationship is not causal. Rebuilding trust is important in continuing to provide important public infrastructure to wary communities.

Third, polling places should be moved or added closer to bike share stations to make voting easier and boost turnout. I found in Manhattan that Citi Bike stations closer to polling places saw significantly higher relative Election Day ridership, suggesting *demand exists* for biking to the polls. Cities should try to meet that demand in setting up polling places, which change regularly, near bike share stations (or vice versa in future expansion). This offers potential for lowering mobility barriers to turnout and thus boosting turnout. This also speaks to the nature of transportation projects themselves, and that improving efficiency relates to interventions as simple as better alignment of stations and destinations.

These results are far from conclusive, given the many caveats enumerated throughout the chapters. The variables I use are imperfect: at times, I rely on projected estimates as confounders, while at other times, my variables are artificial in construction. Complicated changes in cities across the time periods I investigate, both demographic and political, are difficult to account for by using pre-treatment variables. That said, *these are strong,* *reasonable analyses*, and I employ intuitive methods for addressing data gaps or challenges. Hopefully, research at the intersection of bike infrastructure and elections will shed more light on these subjects and whether my early findings are generalizable.

Original, high quality *data and methods* are my closing contributions to this subfield. I extensively organized, merged, or completely created data sets, and I made these publicly available for future researchers. I also describe novel approaches to data issues like using bike lane lines as treatments, merging precincts longitudinally, or projecting block group features onto precincts. These will be helpful beyond just the study of elections and bike infrastructure. For instance, if political scientists are interested in studying partisan swings in a city that releases precinct shapefiles, they can use my proposed Census block-based precinct merging method to do so without having to filter those with unchanged borders. If researchers are interested in studying the impact of bike lanes on local businesses in Seattle, they can employ my original data set with accurate implementation dates and quality metrics for all Seattle protected lanes from 2013 to 2020. The original data sets I utilize are available in the Appendix, and source code for functions I describe is available upon request[4].

Better data is also a matter of public policy. A major takeaway from writing this thesis is that local governments should improve data maintenance regarding important public policy like bike infrastructure. Citi Bike sets a "gold standard" for this, though its public system data is *so* comprehensive it may actually pose a threat to user privacy. On the other end of the spectrum was Seattle bike data, which took nearly a week to clean by manually coding implementation dates and lane quality. Improving data quality is important, but other changes may assist research, too. Election administrators may want to draw precincts that follow Census block group lines; this would allow for more accurate demographic information at the smallest voting district unit. If local governments make researchers' jobs easier by improving data practices, researchers will make their jobs easier by spending more time analyzing public policy – and less time cleaning data.

[4]In addition, I intend to publish functions in a forthcoming `R` package.

In political science, some argue cities are limited entities with incentives related to inter-city competition, not goals or programs aimed at improving life in a sweeping way (Peterson, 1981). Bike infrastructure, however, changed my life. Growing up, parkways and bike lanes allowed me to move freely through the Twin Cities of Minnesota, exploring its beautiful parks and rich urban form. Later, these routes inspired my passion for running; their expansion led to my interest in local politics and decision to study it in college. Today, Boston's Bluebikes system has become an integral component of my daily routine.

There is some truth to the "limited city" theory, but localities do have underrated power. The policy recommendations I make, for instance, are an example of how cities can influence democracy itself. But when something as simple as *biking* can become a powerful piece of one's life, it becomes clear that the ultimate power of local government is its ability to give residents the *joy of a bike ride*.

Chapter 7

Data and Appendix

Data

All data used to run models or design figures and tables (with few exceptions) is available on my Github at: https://github.com/lkolar24/election_cycling

Here, I publish original data sets with variable descriptions and information, along with replicable code for running the analyses I use here. This includes code for viewing ANOVA and other results cited but not elaborated throughout.

The data provided here offers enormous potential for analyses beyond those I perform. It includes longitudinally-consistent election precincts with demographic variables across several cycles in three cities, bike shapefile data with original quality and installation year variables, and more.

My full code used for cleaning data is not yet available (as file sizes are too large), but functions and methods I employed will be included in a forthcoming `R` package. This is also available upon request per information at the URL provided.

I. Portland Gentrification index

My Portland gentrification index map was reconstructed from the city's "2018 Gentrification and Displacement Neighborhood Typology Assessment"[1].

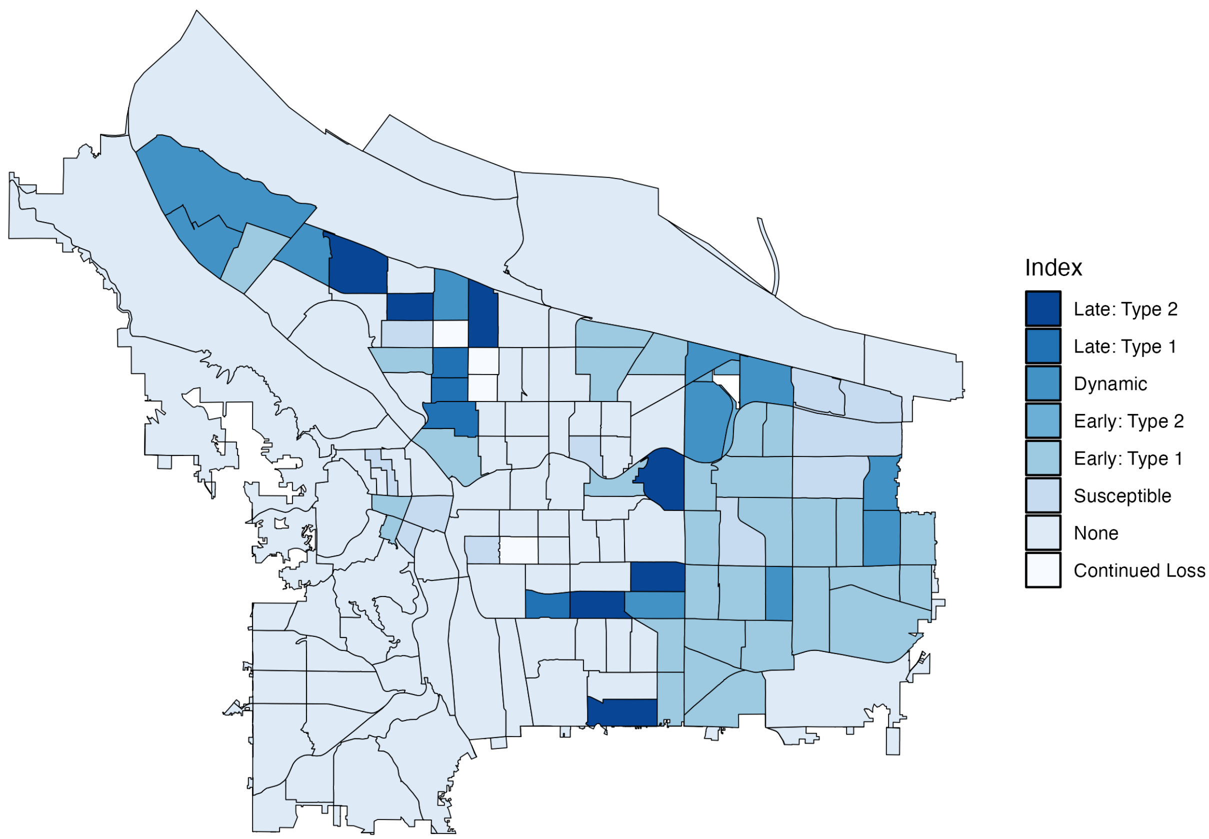


Figure 7.1: Portland gentrification index by tract.

[1] https://www.portland.gov/sites/default/files/2020-01/gentrification_displacement_typology_analysis_2018_10222018.pdf

II. Seattle population change

Seattle population change in the 2010s by merged voting precincts exhibits strong correlation with protected bike infrastructure treatment (T_{walk}).

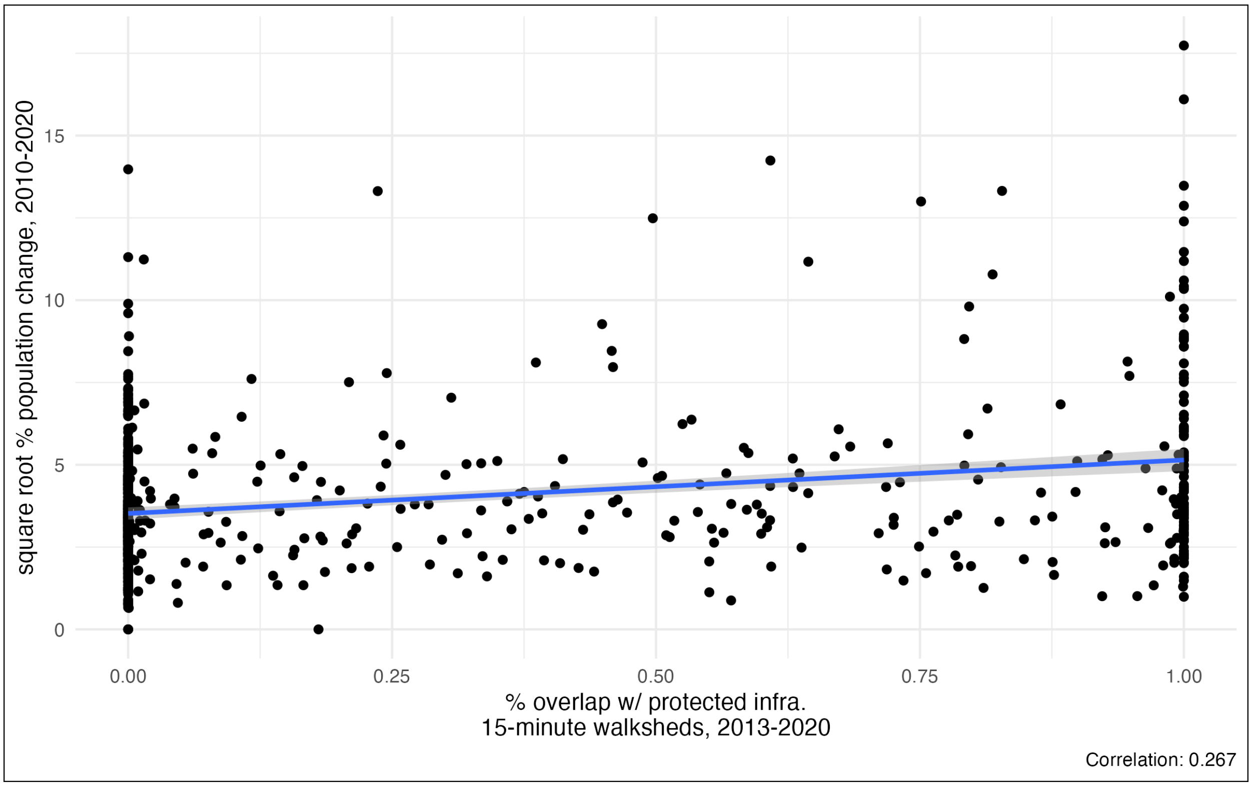


Figure 7.2: Seattle T_{walk} and 2010s population change by merged precinct.

III. City comparisons

Portland and Seattle are unique from the typical U.S. city in many ways. I used data on race from the U.S. Census Bureau[2] and filtered cities with a population greater than 300,000 in 2015. I used data on population change also via the U.S. Census Bureau[3] and filtered for similar cities. Finally, I used data on income across the 55 largest metros in 2022 from the BEA[4].

The plots show show median statistics across large cities in red, and the city in question (Portland or Seattle) in blue.

First, note how **Portland** differs from other cities in race, income, population change.

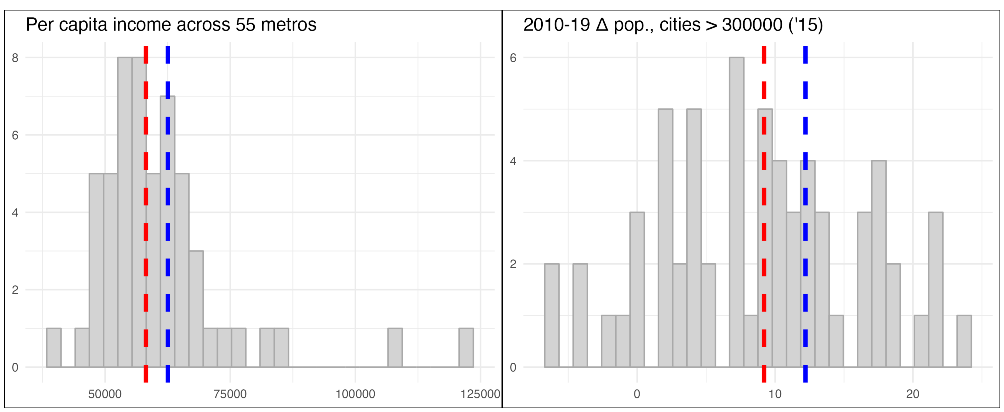


Figure 7.3: Portland income and population change, in blue, versus the median city, in red.

[2]https://public.opendatasoft.com/explore/dataset/us-cities-demographics/information/?flg=en-us
[3]https://www.census.gov/data/tables/time-series/demo/popest/2010s-total-cities-and-towns.html
[4]https://www.bea.gov/data/income-saving/personal-income-county-metro-and-other-areas

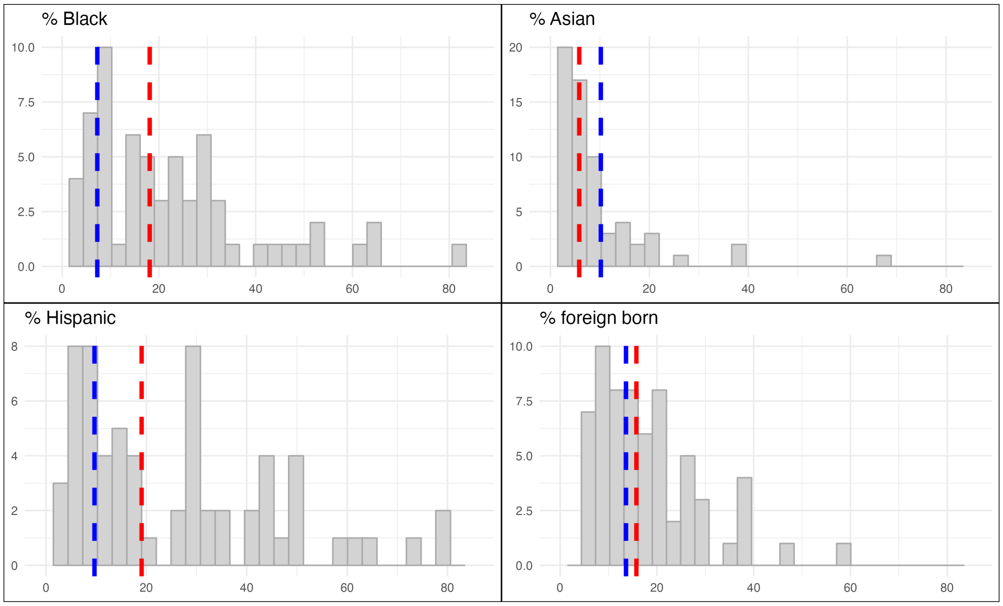


Figure 7.4: Portland racial demographics, in blue, versus the median city, in red.

Seattle is similarly less Black or Hispanic, more Asian, and richer than most cities. It was also incredibly fast-growing.

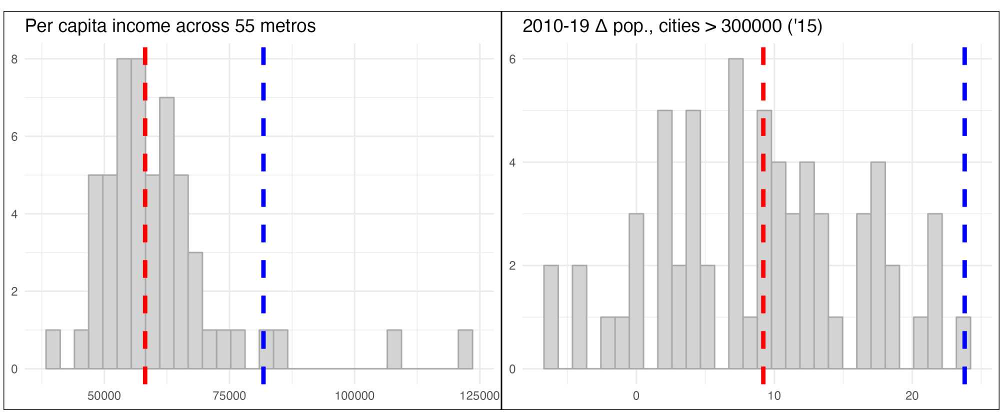


Figure 7.5: Seattle income and population change, in blue, versus the median city, in red.

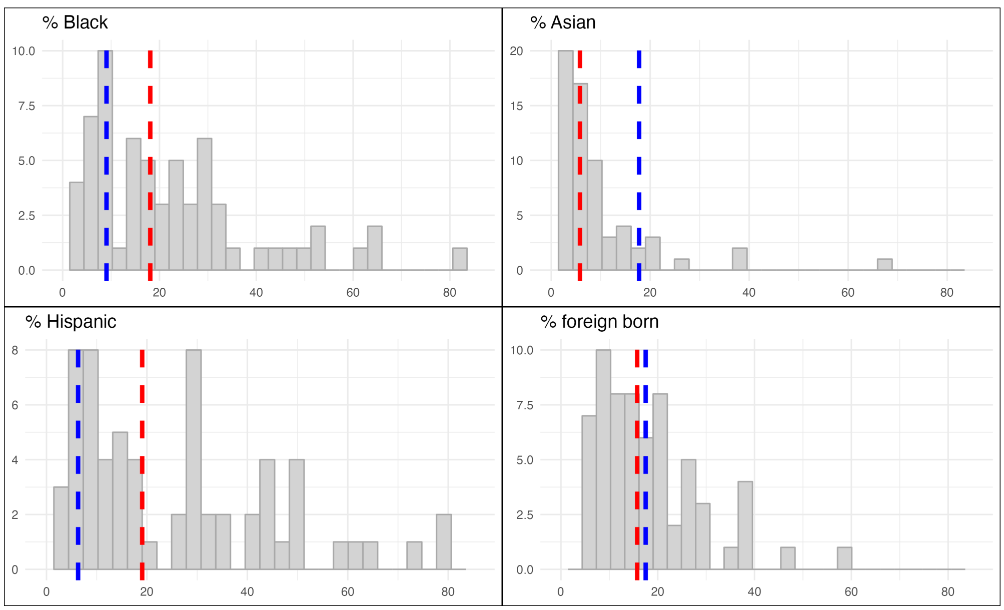


Figure 7.6: Seattle racial demographics, in blue, versus the median city, in red.

IV. Citi Bike station / polling place proximity

The plots below show Citi Bike station polling place proximity treatments' (T_{dist} and T_{count}) strong association with to Election Day stops a proportion of Oct.-Nov. annual average (P_S).

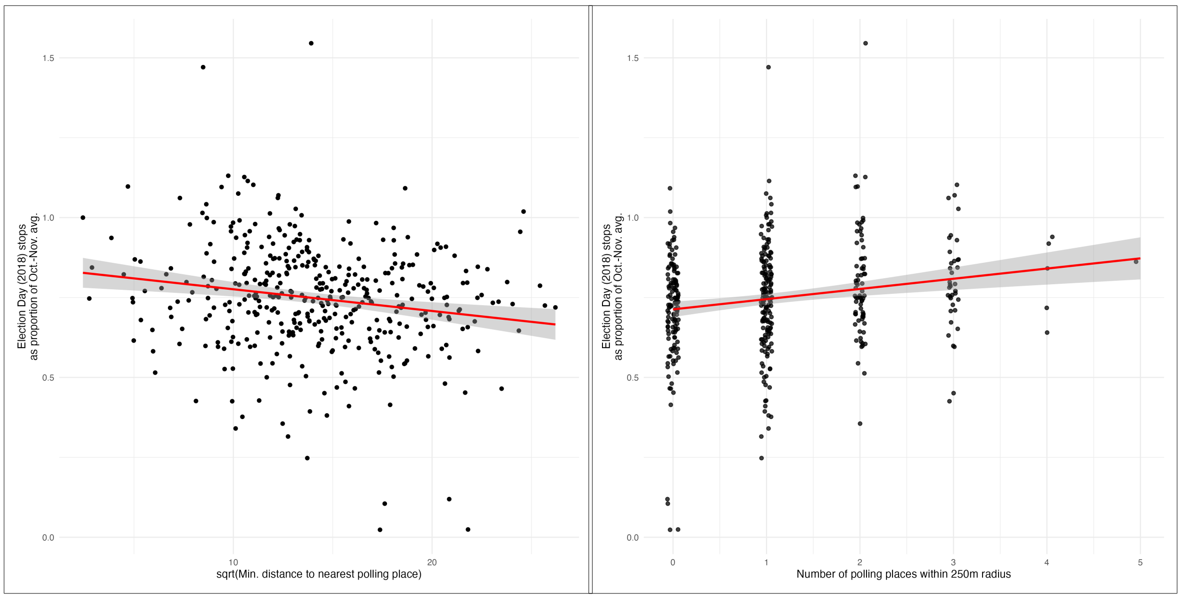


Figure 7.7: T_{dist} (left) and T_{count} (right) relationships with P_S.

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