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Abstract

In the first chapter, I contribute to the literature that studies academic impacts of disruptions due to weather or natural disasters. Such studies so far have mostly focused on younger children and evidence for older students is still limited. Furthermore, the wildfire landscape in California is characterized by frequent fires that burn close to the state's residential areas and their schools. I combine locations of the entire population of in-state wildfires with administrative school-level data to document the detrimental effects of wildfire exposure for older student academic achievement on standardized exams in public schools in California. I provide novel results for older students and I estimate implications of the physical presence of local wildfires, rather than smoke attributable to all wildfires in the U.S. as wildfire literature has done in the past. I find that local presence of large wildfires reduced mean test scores of boys by 0.05 standard deviations across all schools and by up to 0.15 standard deviations in rural socio-economically disadvantaged schools.

In the second chapter, we provide evidence to the discussion about the effect out-of-state university students have on potential in-state students. Despite paying a premium to attend state universities, researchers argue that out-of-state students may come at a cost to in-state students by negatively affecting academic quality or by crowding out in-state students. To study this relationship, we examine the effect of a 2016 policy at a highly ranked state flagship university that removed the limit on how many out-of-state students it could enroll. We find the policy caused an increase in out-of-state enrollment by around 29 percent and increased tuition revenue collected by the university by 47 percent. We argue that this revenue was used to fund increases in financial aid disbursed at the university, particularly to students from low-income households, indicating that out-of-state students cross-subsidize lower income students. We also fail to find evidence that this increase in out-of-state students had any effect on several measures of academic

quality.

In the final chapter, we consider the early-1990s land protections covering tens of millions of acres of old-growth forest in the Northern Spotted Owl habitat in the U.S. Pacific Northwest and Northern California. In the intervening period, wildfire regimes in this region have become significantly more frequent, larger, and more severe. We find that these restrictions on timber harvesting lead to two outcomes. First, they caused an increase in the share of low-intensity wildfire ignitions by enhancing the natural shady and cool conditions of old-growth forests and their extensive tree canopies. At the same time, they ultimately greatly increased areas of wildfire perimeters that burned at high-severity in the protected forests—almost certainly because the logging restrictions encouraged accumulation of vegetation fuels. Severe wildfires often greatly harm affected ecosystems, and impose substantial economic costs on humans. We argue that qualified logging operations could serve a beneficial, complementary role to prescribed burns in forest management plans that aim to reduce wildfire severity.

Contents

Exposure to Wildfires and Standardized Testing of High School Students at Public Schools in California

Natalia Orlova*

Abstract

Literature that studies academic impacts of disruptions due to weather or natural disasters has focused on younger children and evidence for older students is still limited. Furthermore, the wildfire landscape in California is characterized by frequent fires that burn close to the state's residential areas and their schools. I combine locations of the entire population of in-state wildfires with administrative school-level data to document the detrimental effects of wildfire exposure for older student academic achievement on standardized exams in public schools in California. I provide novel results for older students and I estimate implications of the physical presence of local wildfires, rather than smoke attributable to all wildfires in the U.S. as wildfire literature has done in the past. I find that local presence of large wildfires reduced mean test scores of boys by 0.05 standard deviations across all schools and by up to 0.15 standard deviations in rural socio-economically disadvantaged schools.

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1 Introduction

Wildfire frequency, size, damage, and costs in the U.S. have been on the rise and are projected to keep escalating, at least in the near future.[1] In 2018, a particularly devastating year for wildfires and the last year considered in this paper, direct wildfire suppression and infrastructure damage costs alone amounted to almost 0.1 percent of the U.S. GDP.[2] California consistently ranks as one of the most wildfire-prone states in the U.S. Wang et al. (2021) further estimate that the 2018 wildfires in California caused a broad scope of damages that amounted to 1.5 percent of the state's GDP in that year. Their calculations show that the indirect costs, like supply-chain disruptions to a variety of industry sectors, appear to be even greater than the more direct costs, such as health problems due to wildfire smoke or damages and repairs of infrastructure.

This paper contributes to the estimation of wildfire costs beyond the publicly available suppression costs and damages to infrastructure, as the less direct burdens have remained largely unexplored.[3] Yet, they are crucial for setting the optimal wildfire suppression and management policies, as well as for preventative practices, such as forest management. This is especially important in California, as the state currently has a highly aggressive wildfire suppression policy, which has not been the case historically nor is it believed to be sustainable in the future. There have been numerous calls to scale back aggressive suppression of lower-threat fires and to invest more into fire and forest management practices to decrease the likelihood of extreme wildfires in the future. Suppression and management require a different skill set and economic incentives will need to be aligned if we want to shift resources into more passive wildfire mitigation practices. This shift will also have implications for other

[1]See Iglesias, Balch and Travis (2022); NIFC External Affairs Office (2023); NOAA National Centers for Environmental Information (2023); Liu, Goodrick and Stanturf (2012); Kennedy et al. (2021), among many others.

[2]Calculations are based on suppression costs reported by NIFC External Affairs Office (2023) and infrastructure damages reported by NOAA National Centers for Environmental Information (2023).

[3]St. Denis et al. (2023) share a database of wildfire attributes, including suppression resource costs and damages to infrastructure, for a subset of fires in the U.S. Aggregate data on suppression costs for all U.S. wildfires is published annually by NIFC External Affairs Office (2023).

fields, like forestry, logging, or even individual landowner responsibilities for people who own forested properties. Documenting the less direct costs of wildfires also informs broader issues, particularly climate change-related policy.

There is a recent wave of research that explores the consequences of wildfires on health (Reid et al., 2016; Heft-Neal et al., 2023; Miller, Molitor and Zou, 2017; To, Eboreime and Agyapong, 2021), labor markets (Borgschulte and Zou, 2022), household finances (An, Gabriel and Tzur-Ilan, 2023; Biswas, Hossain and Zink, 2023), migration and home ownership (McConnell et al., 2021), and educational outcomes (Wen and Burke, 2022; Wu, 2022; Gibbs et al., 2019). Most papers focus on the effects of poor air quality attributable to wildfire smoke, therefore considering only one channel, no doubt an extremely important one, of how fires may harm individuals and environments around them.[4] Identification relies on the exogeneity of wildfire smoke, as it can travel great distances and even cross country borders. Research that does incorporate source wildfire locations tends to only include a subset of particularly destructive wildfires.

The extent of what is known about the implications of wildfire exposure for academic outcomes also primarily comes from air quality research that focuses on younger students. Aguilar-Gomez et al. (2022) review the literature on the impacts of general air pollution on various non-health outcomes, including cognitive and academic tests. They cite a growing literature that links exposure to air pollution to worse performance on a variety of such tests. The consensus is that performance on verbal tests, rather than non-verbal, is most affected by pollution. The effects are also particularly strong for men.[5] Two papers study the implications of wildfire-attributable air pollution for district-level elementary and middle

[4]Burke et al. (2021) estimate that up to 25 percent of fine particle pollution is attributable to wildfire smoke. This number goes up as high as 50 percent in some western U.S. regions.

[5]Zhang, Chen and Zhang (2018) speculate that this pattern is due to the white-gray matter balance in our central nervous system. One of the documented health impacts of air pollution is a reduction of the density of white matter in the brain. Men already have a much smaller amount of activated white matter during cognitive tests compared to women, so male performance is more affected. Language skills mainly depend on white matter, compared to math, so we see greater impacts in verbal testing than in math.

school standardized test scores in the U.S. [6] Wen and Burke (2022) and Wu (2022) find that exposure to wildfire smoke during the school year produces adverse effects for math and ELA student achievement.[7] Both studies find more negative effects on the youngest students. Wen and Burke test whether their estimates are driven by wildfire smoke or by direct contact with wildfires by iteratively excluding school districts that are a certain distance away from any wildfire perimeter. The authors conclude that wildfire smoke is the main culprit.

In Australia, another country where fires occur regularly, Gibbs et al. (2019) classify primary schools into low, moderate, or high affectedness by the 2009 Black Saturday bushfires based on loss of life and property, rather than exposure to wildfire smoke.[8] The authors find that students in schools that were highly affected by the bushfires saw reduced performance on their reading and numeracy assessments.[9] They further highlight that the negative effects persist for up to 4 years after the bushfires. In summary, there is evidence of negative consequences from general or wildfire-attributable air pollution and from direct presence of extreme fires for academic performance of young students. In wildfire-specific research, between Wen and Burke (2022), Wu (2022), and Gibbs et al. (2019), there is no consensus on the heterogeneity of wildfire exposure implications by gender, racial, or economic disadvantage groups and no results for older students.

It makes sense that disruptions to learning due to wildfires should cause students to fall behind on completing their school curriculum. Yet, there is research suggesting that it is

[6] The authors' test score data source (Stanford Education Data Archive) excludes a non-trivial number of school districts in California. Furthermore, both papers use a time interval that covers structural changes to California's standardized testing system between 2012 and 2014.

[7] The source's (Stanford Education Data Archive) test scores are reported as standardized scores relative to the National Assessment of Educational Progress (NAEP). For example, a score of 0.25 means that an average student in that district performed 0.25 of a standard deviation higher than the reference cohort that took the NAEP. This makes the actual magnitude of authors' estimates hard to compare directly to my findings.

[8] The 2009 Black Saturday bushfire series were some of the most destructive disasters in Australia.

[9] The authors use student-level test score data from the annual Standardized National Assessment Program - Literacy and Numeracy (NAPLAN) for non-urban schools.

not coordinated disruptions that are detrimental to student achievement (Goodman, 2014). Teachers are generally able to adjust instructional pace and bring students up to speed when everyone falls behind on the same material, so coordinated disruptions and absenteeism may not necessarily lead to major implications for test scores. Instead, it is individual student absenteeism that truly undermines academic achievement.

Wildfire impact studies have not commonly incorporated fire location and perimeter data, which is surprising given the local nature of wildfire policy. About 70 percent of my sample of wildfires in California described in the next section falls under the jurisdiction of its state agency—the Department of Forestry and Fire Protection (CAL FIRE). Even federal agencies involved in wildfire response have vastly different approaches to fire suppression and management practices across different states. Using the population of all local wildfires and documenting their impacts helps states carry out targeted wildfire policy interventions. There are also implications specific to the presence and the risk of proximate fires. Such wildfires cause disturbances to nearby community infrastructure and resources, such as preemptive electricity shutoffs, water supply disruptions, or physical damage to buildings and roads. In extreme cases, evacuation orders may be put in place. There is increased financial pressure on residents of wildfire-prone regions, particularly those living in the wildland-urban interface, for example, in the form of higher fire insurance premiums. Despite the country-wide scope of wildfire smoke movements, the populations living close to burning wildfires are likely to be exposed to more concentrated pollutants. Finally, the magnitude of mental health impacts of local and distant wildfires is unlikely to be the same.

In this paper, I provide evidence on the costs that in-state wildfires impose on academic achievement of older students at public schools in California. Young adults in the U.S. take a variety of standardized exams in high school (AP, SAT, and ACT tests) and are faced with critical decisions about their academic future, so it is important to document any disturbances to their learning. I use administrative school-level records on mean math and ELA test scores for students in grade 11 in academic years 2014-15 through 2018-19. I also obtain information on student counts in various test score-based achievement groups: students who did not meet, nearly met, met, or exceeded the state testing standard. This allows me to capture more subtle shifts in student learning outcomes that average test scores may not pick up. Finally, I use locations of the population of in-state wildfires, rather than wildfire smoke data or the presence of only highly destructive fires, to aid wildfire policy conclusions. I find that local presence of large wildfires reduced mean test scores of boys by 0.05 standard deviations across all schools and by up to 0.15 standard deviations in rural socio-economically disadvantaged schools. Disruptions from the entire population of wildfires decreased student exam passing rate by up to 2 percentage points, with greatest effects in rural schools with high levels of student poverty. Both sets of estimates are substantial in the context of similar literature.

The remainder of the paper is structured as follows. Section 2 goes into detail on data sources and sample construction. Section 3 defines measures of wildfire exposure. Section 4 describes the econometric model. Section 5 interprets the results. Section 6 concludes with a discussion of results.

2 Data and Descriptive Results

2.1 Schools

I subset the directory of all California public schools provided by the CDE to traditional schools that are currently active. The traditional school restriction drops from sample institutions like adult education centers or special education schools. About 5 percent of active traditional schools opened after the 2013-14 academic year. I drop them to get a balanced sample of 8,271 schools. Figure 1 maps the 1,406 schools that include high school students.

The CDE further reports information on student socio-economic characteristics and academic outcomes. I use data on California Assessment of Student Performance and Progress (CAASPP) Smarter Balanced math and ELA standardized test scores, enrollments, and Free or Reduced-Price Meals (FRPM) eligibility.[10] Most of this information can be accessed at a gender or racial subgroup level. Data on CAASPP test scores is only available starting in 2014-15, as the CAASPP testing system was then introduced to replace the older Standardized Testing and Reporting (STAR) exam. CAASPP tests are administered every spring to grades 3-8 and grade 11. For my outcome of interest, I focus on CAASPP test results for students in grade 11 (juniors) to capture the older public school student population. The exact scoring scales differ slightly over time but are constant between 2014-15 and 2018-19 academic years. Students are also assigned into 4 categories based on their scores: state testing standard not met, nearly met, met, or exceeded.[11]

The test timing is loosely defined as in spring. For each school, local education agencies, such as school districts or county offices of education, set an individual testing window that they commit to by the end of the first half of the academic year. There is no centralized information on CAASPP test administration dates for each school, but regulations call for the testing window to open when at least 66 percent of instructional days have been completed. In general, most schools do not test at the very end of the academic year and prefer to leave time for students who missed the window due to emergencies or special circumstances to make up the exam.[12]

Figure 2 provides a sense of the spatial distribution of junior students' CAASPP testing results in 2018-19. The top rows show results for math and the bottom rows for ELA exams. Panels (a) and (c), or the first column, map mean scores by school, while panels (b) and

[10]California also administers CAASPP Alternate Assessments math and ELA, science, and Spanish standardized exams. Data on science assessments starts in 2018-19. Another recent CDE data provides detailed absenteeism statistics starting in 2017–18. These two sources could provide insightful information for researchers looking at more recent time periods.

[11]Scores that define these groups can be found on the CAASPP website: https://caaspp-elpac.ets.org/caaspp/ScaleScoreRangesSB

[12]This information is based on correspondence with CAASPP administrators.

(d) plot the percentage of students who met or exceeded the state testing standard. We see a gap between math and ELA test results, with math performance substantially lagging. Math outcomes also appear to be more dispersed compared to ELA. The most prominent observation about the geographic distribution of testing outcomes is that high-achieving schools are concentrated in urban areas. Furthermore, this distribution is granular with different schools producing a variety of testing outcomes within small geographic areas.

2.2 Wildfires

Wildfire management and suppression responsibility is split between local, state (CAL FIRE), and federal government agencies. CAL FIRE assumes responsibility over most local wildfires through local government contracts, so the share of truly locally managed wildfires is small and is not the focus of this paper. A variety of federal agencies participate in wildfire suppression, with main ones being the U.S. Forest Service (FS), the Bureau of Land Management, the Bureau of Indian Affairs, and the National Park Service. Supplemental figure A.1 maps government responsibility areas and federal land ownership to provide a sense of this complex structure.

Information on state-controlled wildfires and their ignition point coordinates comes from the California Incident Data and Statistics Program—CAL FIRE's emergency incident data collection program (CAL FIRE Office of the State Fire Marshal, 2023). The database also contains fire attributes such as start and end date, acreage burned, and estimated damage to property and its contents. Although federal agencies lack a centralized source of historic incident information, Short (2022) compiled and shared a unique database of wildfires with a similar set of attributes reported by individual federal and state agencies. I use her data for federally-controlled incidents and their ignition point locations. Since some incidents are multijurisdictional, I further check for duplicate reporting of incidents between the two datasets before combining the two sources.[13]

[13] I check for duplicates in a number of ways. CAL FIRE and the U.S. Forest Service provide a unique

An overwhelming number of wildfires burns in California. Figure 3 shows ignition point locations of all incidents 10 acres and above stratified by control agency for each year in my sample. The key takeaway is that wildfires close to more densely populated areas are under state jurisdiction, while federal agencies control wildfires in more remote rural locations. Data on incident start date and acreage burned is the most complete of all attributes and I use it to eliminate fires unlikely to be threatening to nearby communities. Figure 4 presents histograms of wildfire acreage burned across all sample years. As this distribution is highly right-skewed with bunching at relatively small sizes, I plot uncensored log of wildfire size in panel (a) and right-censor the level at 500 acres in panel (b). Even the logged distribution shows signs of an impressive right tail, where an occasional wildfire blows up to an extreme size. The median and mean of the level distribution are 43 and 2,161 acres. Panel (b) displays an abrupt drop in fire count over 100 acres. This most likely corresponds to a rounding error of visual estimation of wildfire size for small fires. For my main specification of the population of in-state wildfires, I use this break in the data and discard all fires 100 acres or smaller.[14]

I further drop fires that ignited outside of California's wildfire season (May through November) but keep those that started in December, primarily to account for multiple large wildfires that broke out in southern California in December 2017. Off-season fires are generally unlikely to cause major disruptions. In figure 5, we see that even the incidence of fires in January-April pales in comparison to that of May-December fires. An additional reason for this omission is to avoid a measurement timing conflict with the outcome. Since I do not observe each school's test administration dates, I cannot know if the fire occurred before or after the students were tested. Following similar logic, I also exclude fires that ignited in May

inter-agency identifier for their incidents. I use this identifier to merge the two data sources. To identify duplicates between CAL FIRE and other federal agency incidents, I run a fuzzy merge on incident name using Levenshtein distance and start date. As a final check, I merge state and federal incident data on start date and examine if any ignition points are located too close together and are unlikely to be separate incidents.

[14]The median wildfire in this group was contained within the same day and the 90th percentile fire in 5 days, although containment date information is not complete for all wildfires.

and June. Based on the discussion in the previous section, it is likely that most schools finish administering the tests by June, and possibly by May. Even though May marks the start of California's wildfire season, it rarely sees significant wildfire activity. To further alleviate concerns of confounding by spring fires, figure 5 shows that these fires are geographically spread out, so they do not systematically affect schools in any given region. The final sample of wildfires, those that burned more than 100 acres between July and December, is plotted in red.

I supplement data on ignition point coordinates with spatial data on California fire perimeters provided by multiple agencies and hosted by CAL FIRE.[15] This allows me to more precisely calculate the key relationship of interest, proximity of schools to wildfires, and serves as a consistency check of ignition point location data. While perimeter data is not available for the entire population of ignitions, it covers 79 percent of my sample described above and does include nearly all large fires. For the unmerged ignitions in my sample, I approximate fire boundaries by assuming that they spread radially from their ignition points and use acreage burned to calculate the radius of spread.[16] Figure 6 mimics figure 5, but plots perimeters instead of ignition points. Compared to the densely packed ignition point maps, there is only a handful of wildfires that truly stand out on the map due to their size, which makes sense given the highly right-skewed distribution of acreage. Only a couple of such wildfires started in months that I do not include in my analysis (January-June).

[15] I merge my database of all incident ignitions with perimeters on unique inter-agency identifier where available. For the rest, I run a fuzzy merge on incident name using Levenshtein distance and start date. I also perform consistency checks on ignition point and perimeter GIS information, such as making sure ignition locations fall within perimeters or are at least not a significant distance outside, to catch merging errors.

[16] Maximum radius of fires without perimeter information is about 3 miles and the mean is 1, so measurement error of school distance to fire due to this imputation is minimal.

3 Measures of Wildfire Exposure

I combine all data described above to quantify schools' exposure to wildfires. I explore a number of measures based on local ignition point and wildfire burn area densities, school proximity to wildfires, and fire size. My most plausibly exogenous measure is a count of all wildfire ignitions within a certain number of miles of a school. I consider distances from 10 to 50 miles. Impacts of more distant fires are primarily channelled through air pollution. Despite potential measurement error from omitting wildfire smoke as a treatment, this omission should not affect my outcomes in a systematic way as the smoke is generally accepted to spread in an exogenous manner. I also calculate separate wildfire frequencies by aggregating over fires of a certain minimum size. For larger fires, instead of a count, I use an indicator for any fire presence.

Next, I incorporate wildfire burn areas matched to the corresponding ignition locations and control for a count or an indicator of wildfire burn areas within a certain distance of a school. A potential concern is that while wildfire ignition is exogenous, how a fire spreads may not be. However, particularly for larger fires, ignitions provide a noisy estimate of proximity to wildfires. The trade-off between potential endogeneity of wildfire perimeters and measurement error of ignition point locations is something to keep in mind.

Figure 7 presents histograms of two key fire exposure measures. In panel (a) we see that most schools experience at least one fire igniting within a 50 mile radius during the first half of an academic year. The typical distance to the closest ignition is 25 miles. Panel (b) further shows that local wildfires are a common occurrence for California schools. We see means of about 5 fire ignitions within 50 miles and a small share of schools that get hit especially frequently by 10 or more wildfires. Figures 8 and 9 map fire exposure variation by year. Generally, these figures show significant spatial and temporal variation. However, there is also evidence of persistence of fire exposure over time for some regions. For example, schools in the Los Angeles area tend to have very local fires (figure 8), while North California shows persistence of high-frequency exposure to fires (figure 9). [17]

I do not control for other wildfire attributes in my analysis. This is partially due to a non-trivial amount of missing values (except for acreage and start date) and partially due to endogeneity concerns. Wildfire management and suppression decisions, evacuation orders, resource order costs, and sometimes even fire duration and size involve human decision making in response to fire behavior. Apart from the human factor, the way a wildfire behaves once ignited is driven by local topography, vegetation, and weather. To control for schools' different underlying probabilities of experiencing a nearby fire, I use CAL FIRE's data on fire threat mapped for the state. Supplemental figure A.3 displays this original map. The five threat classes, ranging from low to extreme, are calculated based on measures of fuel conditions and fire potential in the ecosystem. For each school, I extract distance to the closest area of either a very high or extreme fire threat. Figure A.2 maps this distance for all schools. The two figures show that the bulk of the school density is surrounded by very high or extreme fire threat areas, which translates into short distances in figure A.2. Once I control for this distance and for local (school district or school) fixed effects, most of the leftover treatment variation is due to random weather events.

4 Methodology

In line with similar literature, I use a two-way fixed effects (TWFE) model:

$$y_{it} = \beta d_{it} + \gamma^g X_i^g + \gamma^s X_{it_0}^s + \alpha_d + \delta_t + \varepsilon_{it}$$

I run a separate set of regressions for each measure of wildfire exposure, d_{it}. I focus on two size-based samples of wildfires: (1) extreme fires that burned 2,000 or more acres and (2) all fires more than 100 acres in size. The latter sample can be thought of as a population of wildfires in California that excludes small fires, a lot of which are close to 0 in size. For

[17] I omit burn area-based histograms and maps as they look very similar to those for ignition points.

large wildfires, d_{it} is an indicator for any such fires occurring within a specified distance of a school. For wildfire population regressions, d_{it} is a count of fires burning within a certain distance. Distance to wildfire is the mileage to the closest fire perimeter point in main results and to fire ignition point location in one set of robustness checks. In regression results, I refer to wildfire sample (1) d_{it} as local large wildfire presence and to wildfire sample (2) d_{it} as local wildfire burn area count.

For each treatment, the key outcomes (y_{it}) are standardized school-level mean test scores and percent of students who fall into each score-based achievement group: students who met or exceeded the state testing standard, those who nearly met it, and those who did not meet it. I repeat this analysis for math and ELA tests administered to students in grade 11. All outcomes are measured in spring and wildfire exposure variables are based on wildfires in July-December, so these models estimate immediate same-academic year effects on student learning.

I control for distance to very high or extreme fire threat areas (X_i^g) as explained in the previous section. Finally, I include a set of baseline school-level characteristics in 2013-14 ($X_{it_0}^s$): enrollment share breakdowns by gender, race, and FRPM eligibility. In my main specification, I include school district (α_d) and year (δ_t) fixed effects and run school and year FE models in robustness checks. I choose not to weight my regressions by school enrollment and allow each school to contribute equally to the results, as I do not want my estimates to mainly represent the Los Angeles area. Inference is based on Conley standard errors with a cutoff at 50 miles and all confidence intervals are drawn at the 95 percent level.

5 Results

5.1 All Wildfires by Size

I start my analysis by looking at the impacts of the count of wildfires within 50 miles of a school plotted for various fire size cutoffs in figure 10. I use these plots mainly to motivate my next steps. Panels (a) through (f) display estimates for standardized mean test scores of all students and separately by gender. The shape of the graph is intuitive. Aggregating over all wildfires that burned 10 acres or more yields negligible incremental effects, while only counting wildfires measured in thousands of acres leads to point estimates that are up to 10 times greater. It is also clear that the effects are more persistent and significant for boys, especially for ELA exams, which is consistent with literature on education and air pollution.

Various agencies responsible for wildfire suppression or data maintenance use different criteria for a significant wildfire. The definition is commonly based on wildfire size, fuel, or the type of incident management team assigned to it. Even though greater size does not always lead to higher fire damages and costs, based on the shape of these plots, in the next section I look more closely at wildfires 2,000 acres or greater in size to gauge the effects of more extreme fires. I also consider the previously defined sample of fires greater than 100 acres to account for effects of the nearly entire population of in-state wildfires.

5.2 Large Wildfires by Distance

Figures 11 and 12 present estimates of the impacts of large 2,000-acre or more wildfires on mean test scores of boys and girls. As there is no agreed-upon distance cutoff for such effects, I plot them for a wildfire distance range of 10 to 50 miles from a school. I further stratify the estimates by school location (rural or urban) and low student body socio-economic status approximated by a higher than sample average share of student FRPM eligibility (60 percent). Large wildfires are infrequent—fires that burned 2,000 or more acres make up 10 percent of my sample, so the treatment measure here is whether any large fire burned within the specified distance of a school. Generally, the point estimates increase marginally as we consider shorter distance, but the effects are hard to gauge very close to a school due to their noise. For junior boys, the academic effects of exposure to any local large wildfires hover around a 0.05 standard deviation decrease in math and ELA test scores in all locations—the rural and urban school distinction does not appear to matter for large fires. Having a higher than average share of disadvantaged students, however, matters a great deal in rural locations and doubles (0.1 standard deviation decrease in math) or triples (0.15 standard deviation decrease in ELA) the point estimates, despite somewhat noisier results due to sample size reduction. [18] By contrast, figure 12 shows no discernible effects of exposure to large wildfires on mean test scores for girls. Table 1 summarizes key takeaways for boys using a wildfire exposure radius of 30 miles.

To put these estimates into context, in student absenteeism literature, Goodman (2014) estimates that one student absence is associated with a 0.008 standard deviation decrease in math scores and each student absence induced by a snow day reduces math scores by 0.05 standard deviations. He notes that a typical student in the U.S. is absent more than two weeks during the academic year. Air quality literature finds that exposure to various increments of pollution (one standard deviation increase in fine particle pollution, a 10 unit increase in the air pollution index, a shift from no air pollution to its mean value) reduces test scores by 0.4 to 3 percent of a standard deviation and by up to 8 percent for long term multi-year exposure (Aguilar-Gomez et al., 2022; Wen and Burke, 2022). Finally, Gomez and Yoshikawa (2017) find that children who experienced a severe earthquake in Chile saw their test scores drop by 20 percent of a standard deviation.

[18] 22.5 percent of the 1,406 public schools are classified as rural. The percentage of socio-economically disadvantaged schools is around 50 percent by construction and it is very similar among rural and urban schools. Figures for urban and urban disadvantaged schools have been omitted due to their similarity to estimates for all locations.

5.3 All Wildfires by Distance

To account for a more representative distribution of wildfires in California, rather than just the top decile by size, I repeat the analysis from the previous section for all fires greater than 100 acres. Figure 13 summarizes the resulting estimates of the effects of the count of these wildfires burning within a certain distance of a school. I focus on boys and omit the discussion for girls due to the lack of effects found previously. Figure 13 does not produce much support for a robust story of general wildfire disruptions to student learning. The overall effects are small and, even if scaled by the typical wildfire frequency, they are noisy. There is some minor evidence of rural and disadvantaged rural schools bearing a disproportionate burden from these disruptions based on marginal significance and much higher magnitudes of point estimates, but it is tenuous.

Figure 14 provides an additional breakdown of wildfire exposure effects on male students in various test performance categories and gives more insight into student shifts between these groups. There are significant negative impacts on the percentage of boys who met or exceeded the state testing standard across all samples. Raw point estimates of an additional wildfire burning within a given radius decrease with distance and can be as high as a 2 percentage point drop for very proximate fires. Again, we see evidence of deeper detrimental effects of wildfire disruptions to learning in rural and socio-economically disadvantaged rural schools—the estimates are at least doubled compared to all schools (note that the first two plots are set to a different scale for visibility reasons). When scaled by typical wildfire frequency, the distribution of point estimates over distance is relatively flat. For the ELA test, the mean scaled effects over distance average out to -0.77, -1.68, and -2.32 for all, rural, and disadvantaged rural schools. For the math exam, the corresponding numbers are -0.49, -1.31, and -0.94. Table 2 summarizes the marginal estimates on boys' passing rate using a wildfire exposure radius of 30 miles.

We further see that a reduction in students meeting proficiency standards is generally accompanied by a comparable increase in the group of students who did not meet the state testing standard, while the estimate for the nearly passing group is not statistically or academically significant. This potentially tells a story of a small share of previously passing students falling substantially far behind in material comprehension, rather than just crossing into the adjacent nearly passing category.

In summary, the reduction in student passing rates is not enough to accurately measure the corresponding drop in mean test scores, but these extensive margin estimates are still non-trivial. For reference, between academic years 2020-21 and 2021-22, the proportion of junior students meeting or exceeding the state testing standard dropped by 5 and 8 percentage points for ELA and math CAASPP exams. This drop has been largely attributed to the COVID-19 pandemic. I find that when a school is exposed to the average wildfire frequency, relative to an academic year without wildfires, typical student passing rates can decrease by up to 2 percentage points depending on location and poverty profile of the school. Given close proximity to a school, even one additional wildfire, not necessarily a large one, can have the same effect.

5.4 Robustness Checks

I run a couple of robustness checks for significant results for boys. I do this for large wildfire models and for entire wildfire population effects on the percent of passing students. First, I try an alternative TWFE model and control for school-level and year fixed effects. Figures A.4 and A.5 show that this change tells the same story as my main specification results. Next, I consider ignition point locations of wildfires, rather than burn areas, when calculating fire proximity to schools. Burn perimeters account for the way wildfires spread from their ignitions but are less plausibly exogenous due to wildfire management and suppression practices, particularly when low-intensity, minimal-threat fires are allowed to spread intentionally. For large wildfire results, using ignitions yields similar point estimates but much noisier inference, as we see in figure A.6. This makes sense, since large wildfire presence approximated by just its ignition location can add up to significant measurement discrepancies. Wildfire population results in figure A.7, however, are still very similar to the main specification. This similarity reflects the fact that most wildfires in California are not large enough for the measurement error to accumulate (recall figure 6). The general distance-to-wildfire calculations based on ignition points will still be relatively precise for areas where perimeter data is not available.

6 Discussion and Conclusion

This paper provides novel evidence on the detrimental effects of wildfire exposure for older student academic achievement on standardized test scores in grade 11 in California. The state has an unusually dense distribution of wildfires igniting and burning close to communities and their public schools. I use locations of the entire population of in-state wildfires and a subset of larger fires to document effects on student learning due to local presence of typical and more extreme wildfires. I find evidence of disruptions to boys' learning but I only see smaller and less precisely estimated results for girls. For boys, local presence of extreme fires yields large and precise negative effects on school-level mean test scores, especially in rural schools with high student poverty. The effects are comparable with estimates from literature on air quality, student absences, and natural disaster disruptions to learning.

Typical disruptive effects averaged out over the entire wildfire population also yield evidence on negative consequences to student learning. Wildfire presence leads to a drop in the percentage of boys who met or exceeded the state testing standard. Again, the effects are much higher for rural schools with high levels of student poverty. This drop is non-trivial in magnitude when compared to the post-COVID annual reduction in passing rates of older students on the same exam. Though the passing rate changes are not enough to accurately estimate effects on mean test scores, they are accompanied by comparable increases in the lowest non-passing score-based student category (rather than the intermediate nearly passing). This trend suggests that it may worthwhile to further investigate wildfire disturbances to the entire distribution of student learning outcomes using individual-level academic data to gain a better picture of test score shifts that are more subtle than fluctuations in means.

In line with literature on the impacts of poor air quality on academic achievement, the estimates are significant and larger in magnitude for boys, particularly for ELA exams. This pattern suggests that increased air pollution is still a key channel of proximate wildfire effects. This is further supported by the fact that the exact school-to-wildfire distance (10 to 50 miles) does not produce drastically different estimates when they are appropriately scaled. However, coefficients calculated based on the entire wildfire population show that students in rural schools see their performance drop disproportionately more. The magnitude of estimates also more than doubles when we look at rural socio-economically disadvantaged schools. By contrast, poverty does not matter at urban schools. This can reflect the fact that rural and particularly socio-economically disadvantaged schools are smaller and not as well-equipped to help students catch up on disrupted learning. Alternatively, this can imply that other channels of wildfire exposure consequences are also involved in addition to air pollution, as wildfire smoke is believed to spread in a random way and would not systematically target schools with certain characteristics. The pattern can potentially be interpreted as evidence of schooling disruptions from direct infrastructural damage, preemptive measures to mitigate local wildfire risk, or from evacuation orders in remote disadvantaged communities living in wildfire-prone regions.

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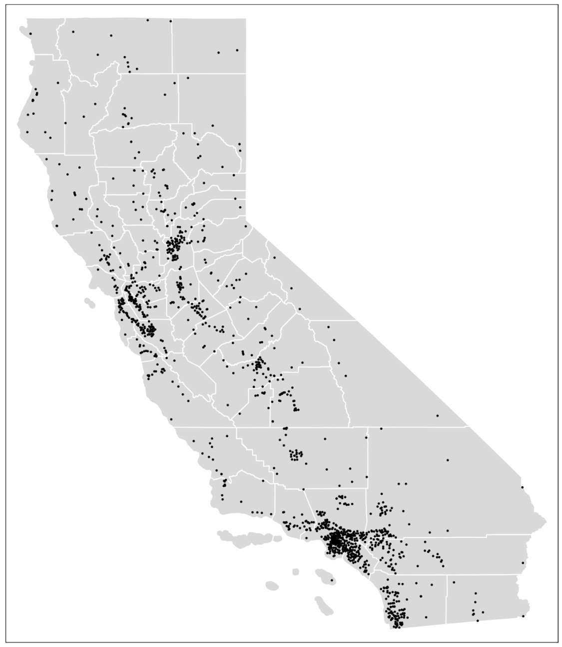
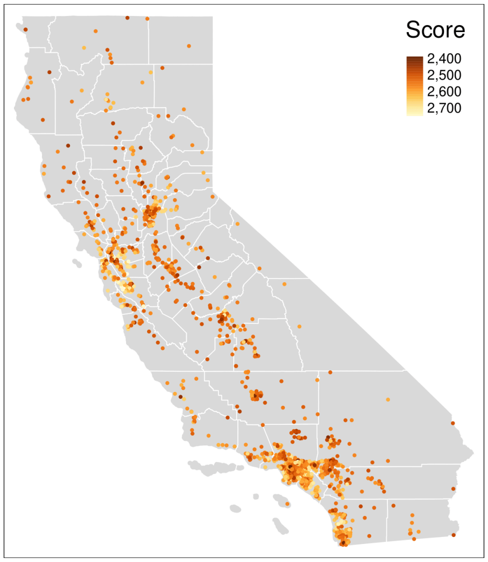
Figures

Figure 1: Locations of Traditional Public High Schools

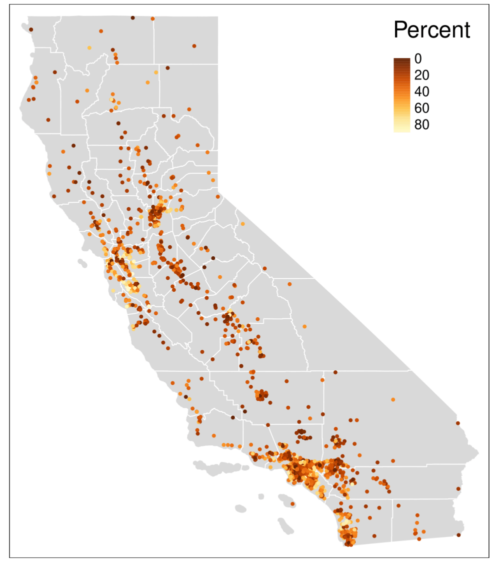


Notes: **Mapped schools are currently active and opened in 2013-2014 academic year or earlier.**

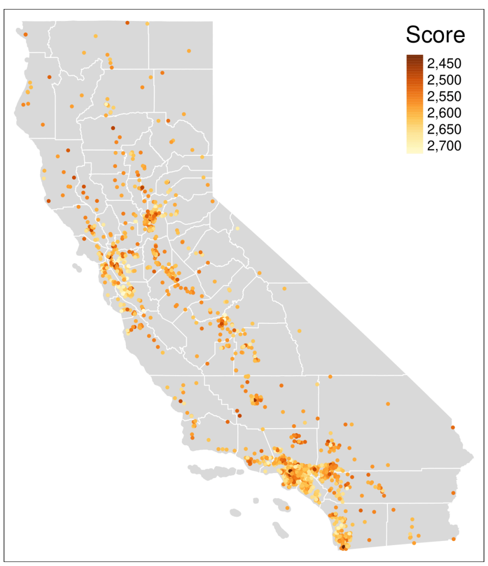
Figure 2: CAASPP Test Results in 2018-19, Grade 11



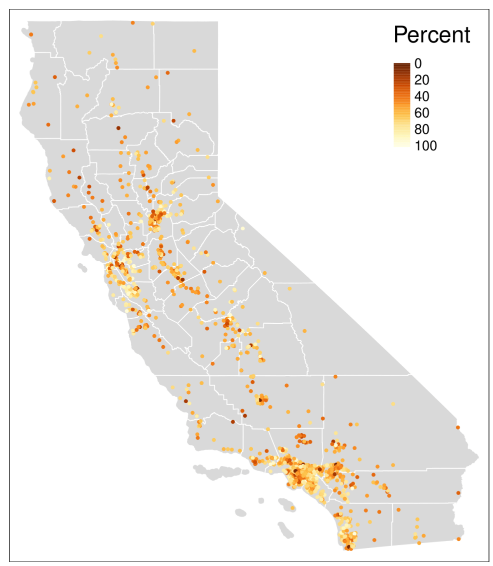
(a) Mean math scores



(b) % passed the math standard



(c) Mean ELA scores



(d) % passed the ELA standard

Notes: **CAASPP math and ELA exams are scored on scales of 2280-2862 and 2299-2795, respectively. Students are further assigned into 4 categories based on their scores: state testing standard not met (2280–2542 and 2299–2492), nearly met (2543–2627 and 2493–2582), met (2628–2717 and 2583–2681), or exceeded (2718–2862 and 2682–2795).**

Figure 3: Ignition Points of Wildfires 10 Acres or Greater, By Responsible Agency

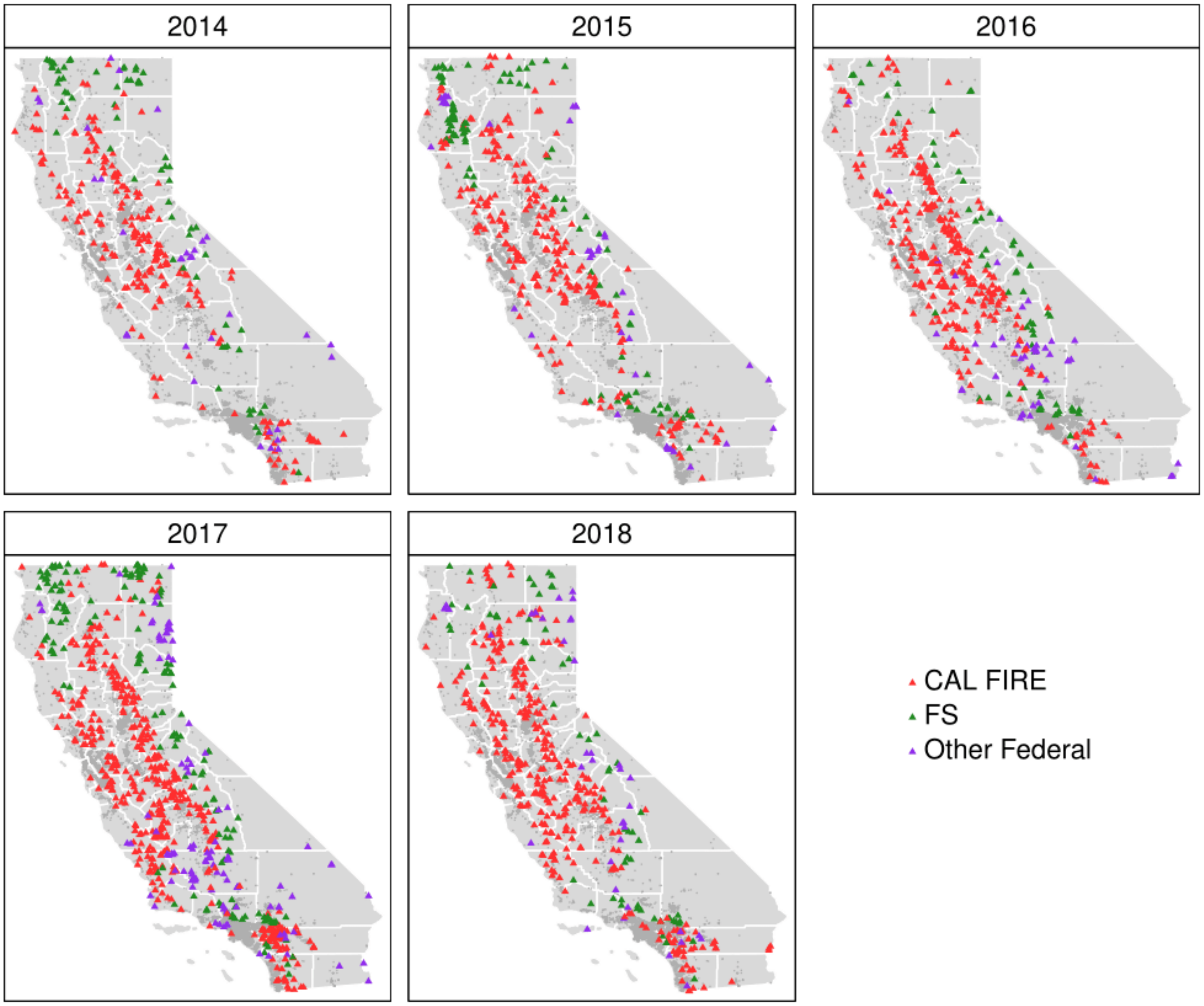
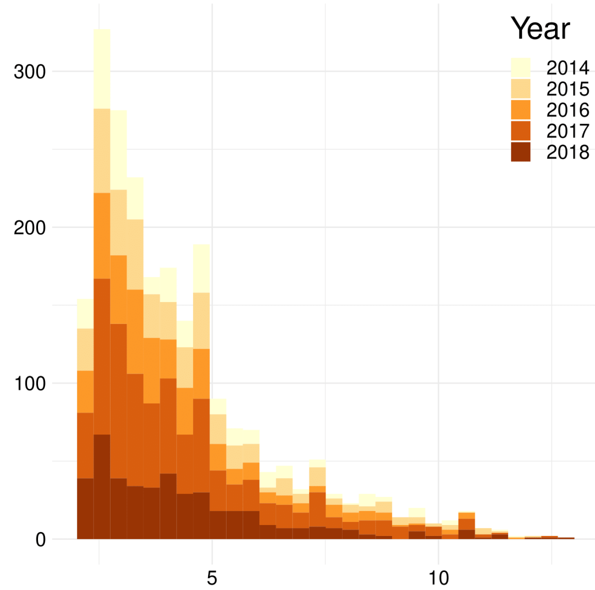
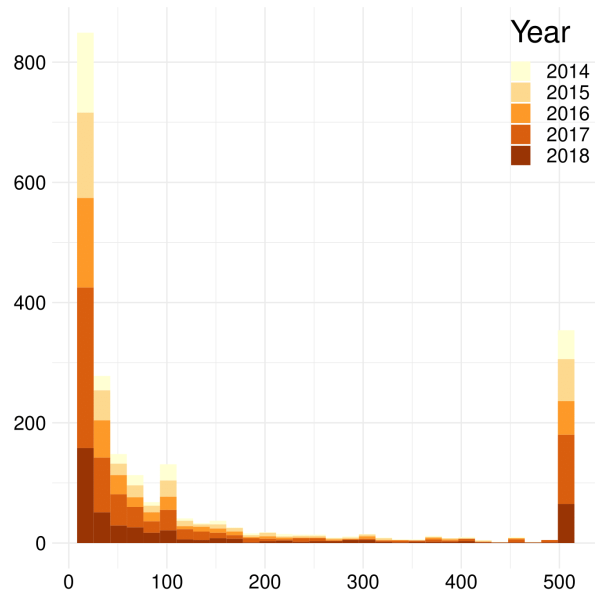


Figure 4: Distribution of Wildfire Size, by Year



(a) Log of acres



(b) Acres, right-censored at 500

Notes: **Data excludes any fires that burned less than 10 acres.**

Figure 5: Ignition Points of Wildfires Over 100 Acres, By Start Month

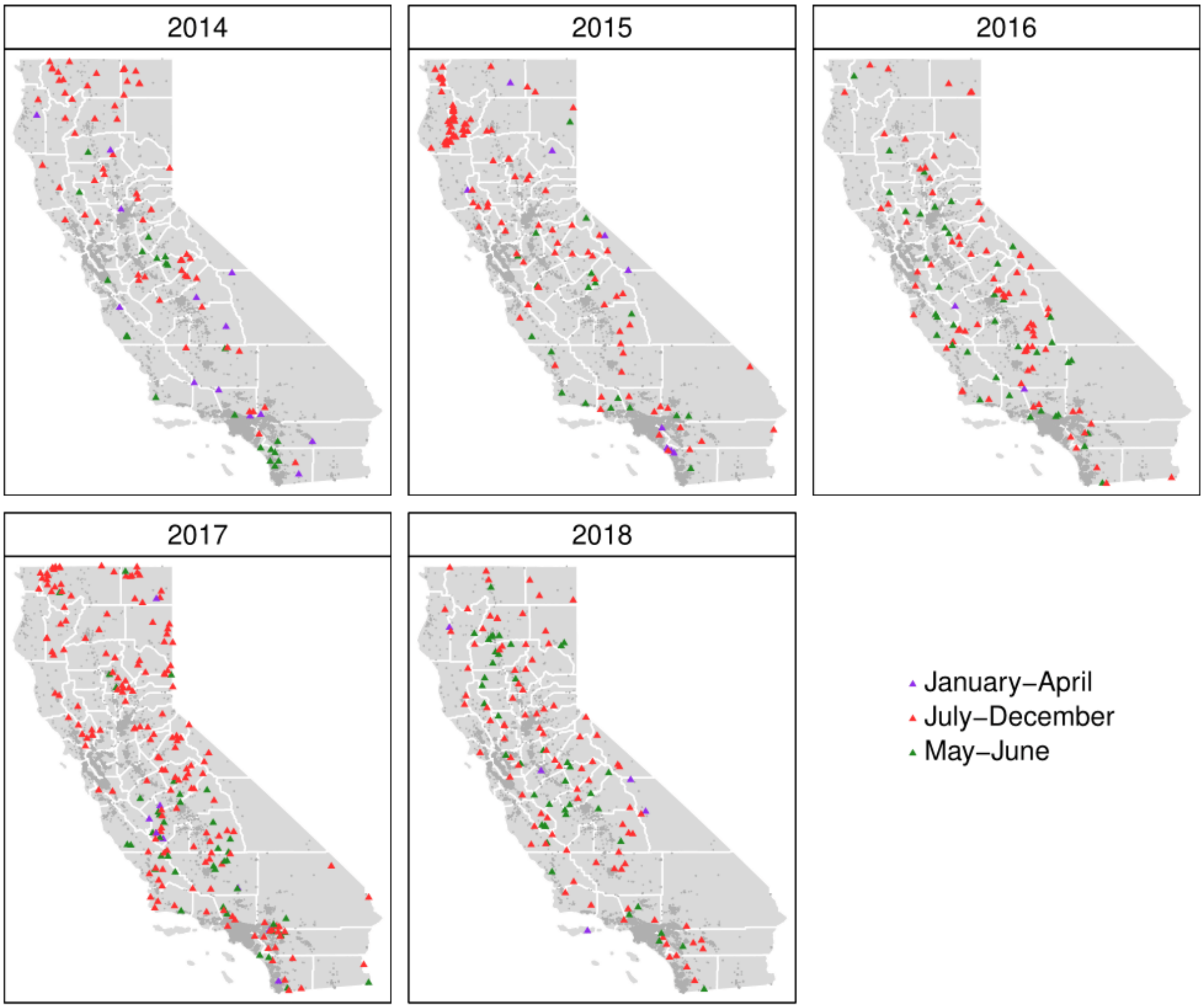


Figure 6: Perimeters of Wildfires Over 100 Acres, By Start Month

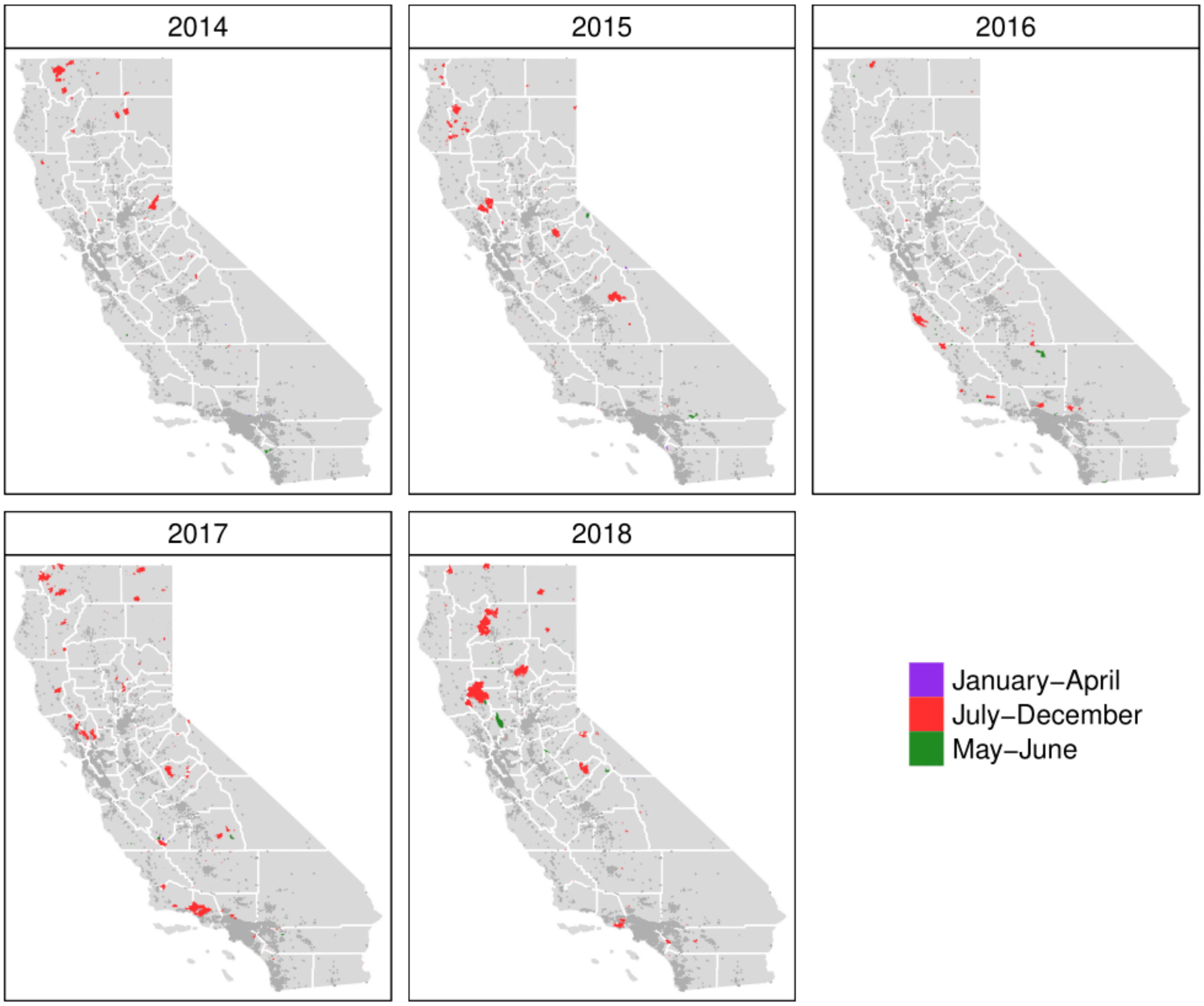
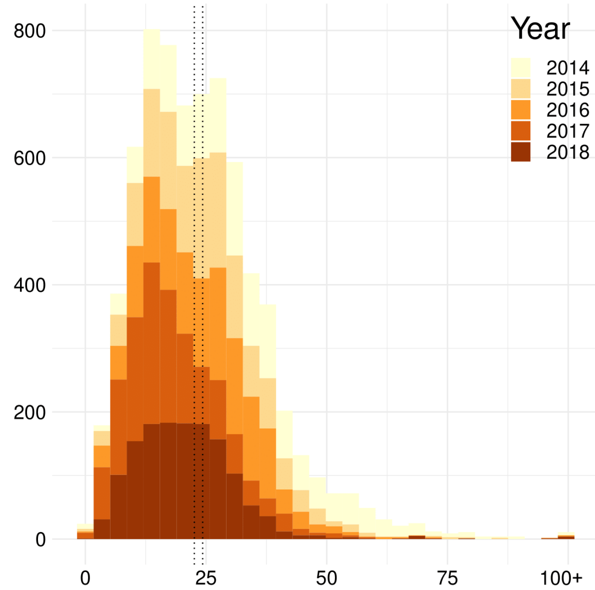
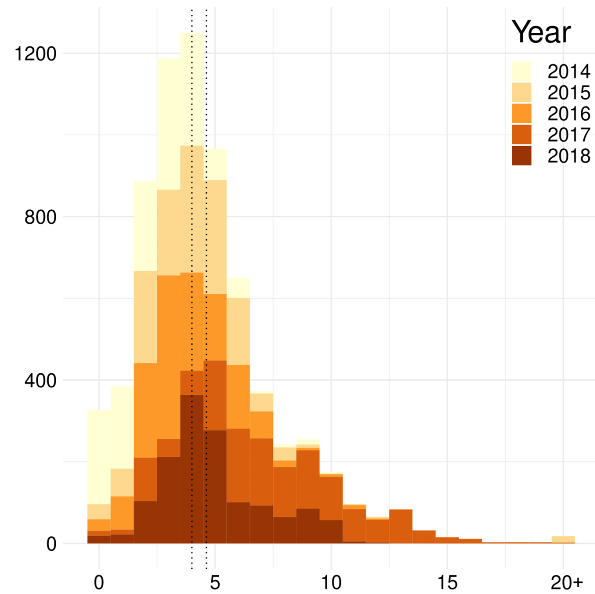


Figure 7: Distributions of Wildfire Ignition Proximity and Frequency



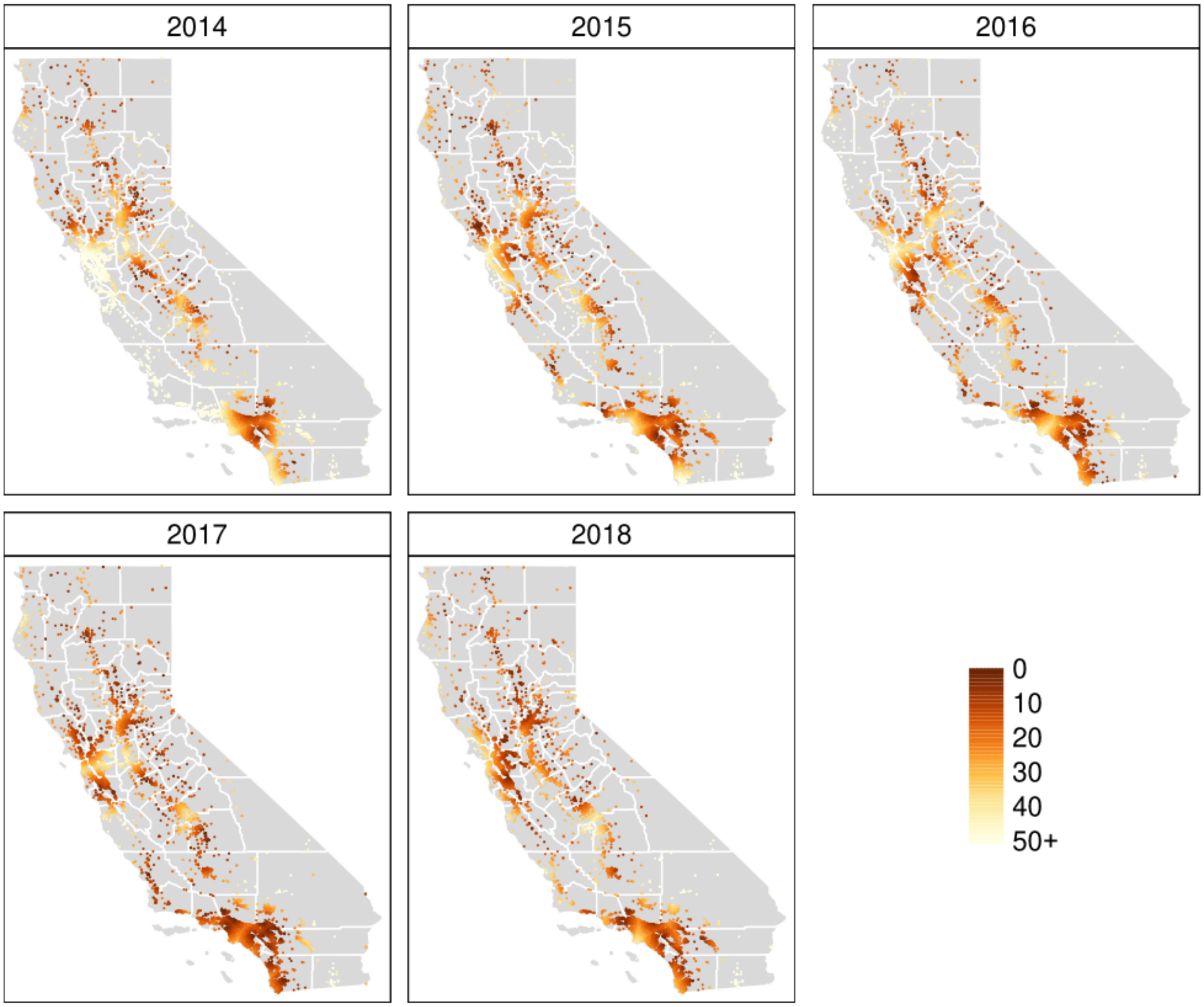
(a) Miles to closest ignition



(b) Frequency within 50 miles

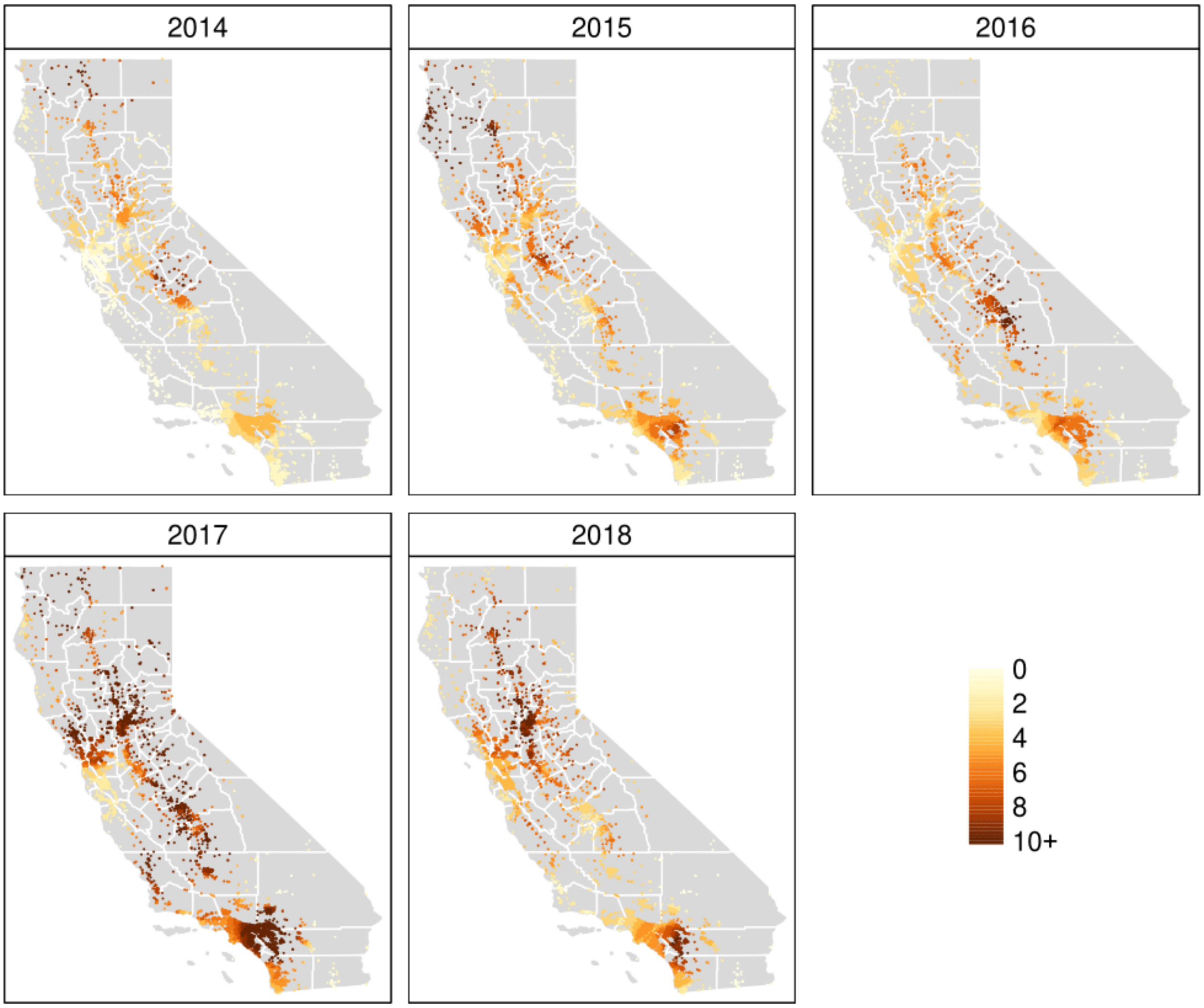
Notes: **The dotted lines show mean and median values. Ignitions and burn areas include wildfires over 100 acres in size that burned in July through December.**

Figure 8: Spatial Variation of Distance to Closest Wildfire Ignition (Miles)



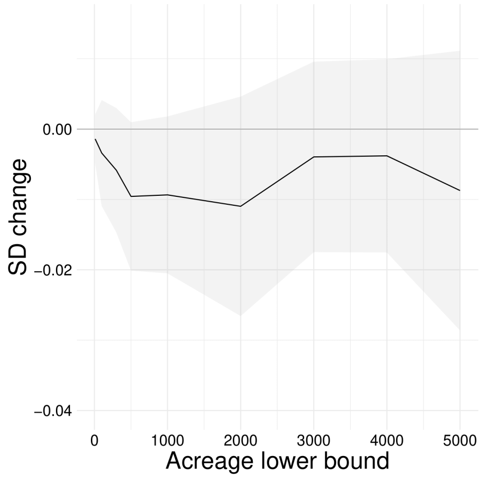
Notes: **Calculations are based on wildfires over 100 acres in size that burned in July through December.**

Figure 9: Spatial Variation of Count of Wildfire Ignitions Within 50 Miles

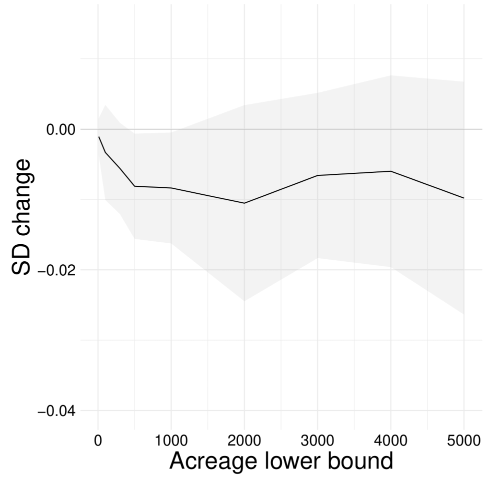


Notes: **Calculations are based on wildfires over 100 acres in size that burned in July through December.**

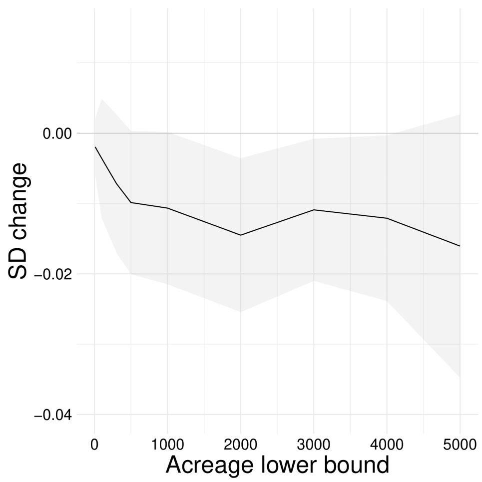
Figure 10: Effects of Wildfire Count within 50 Miles on Mean Test Scores, by Fire Size



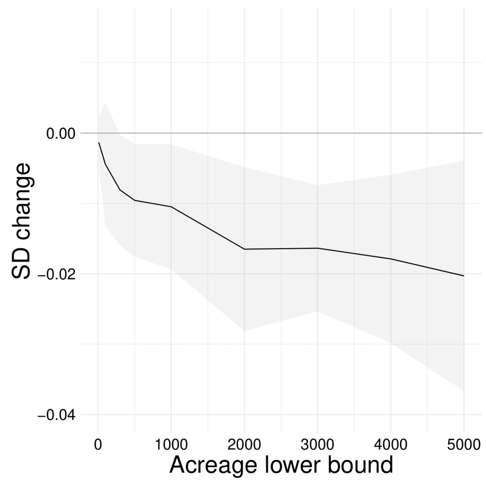
(a) Math, all students



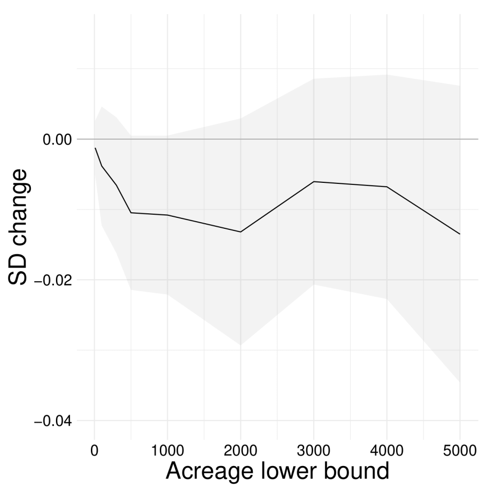
(b) ELA, all students



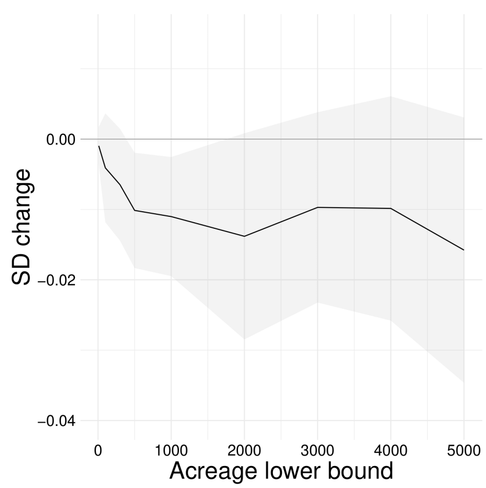
(c) Math, boys



(d) ELA, boys

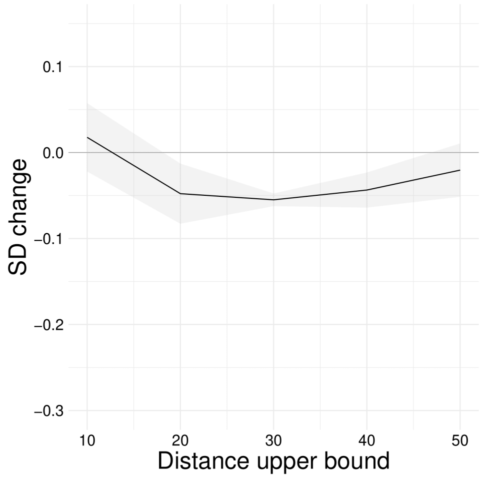


(e) Math, girls

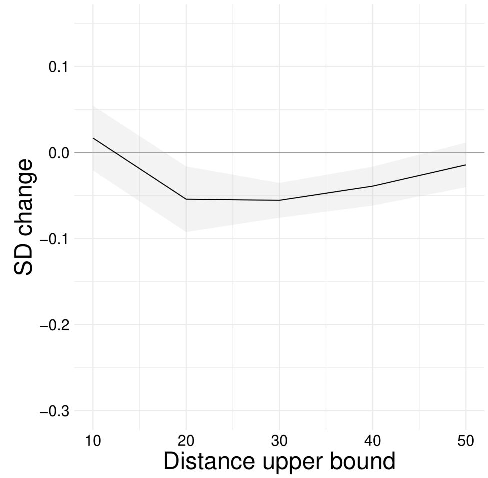


(f) ELA, girls

Figure 11: Effects of Local Large Wildfire Presence on Boys' Mean Test Scores, by Distance (Miles)



(a) Math, all schools



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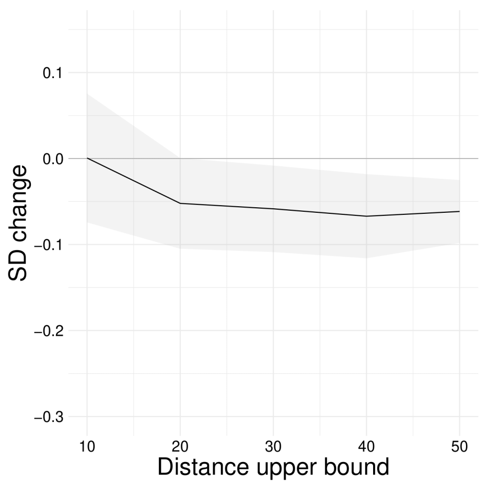
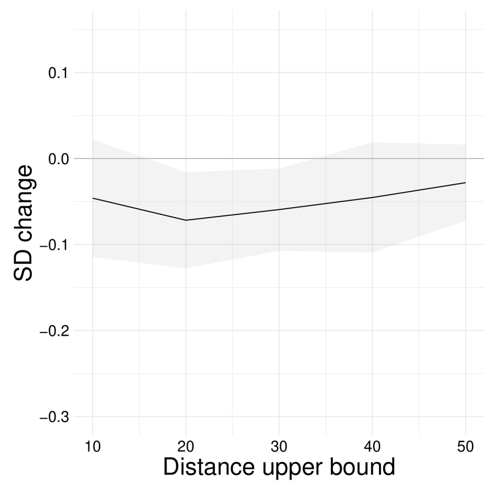
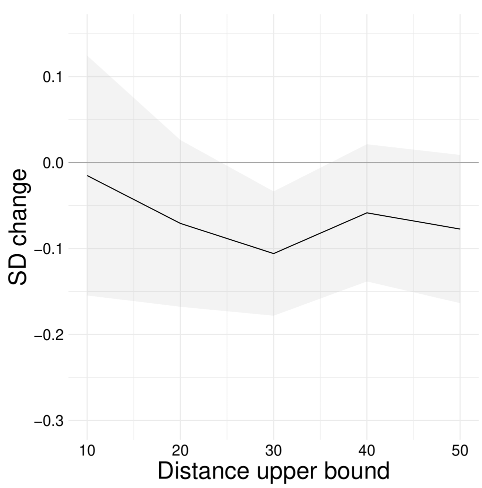


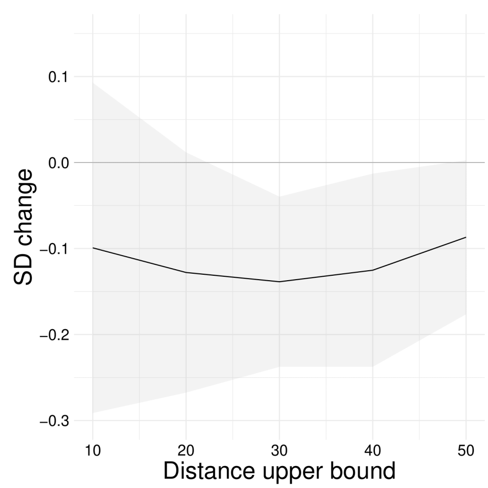
(c) Math, rural schools



(d) ELA, rural schools

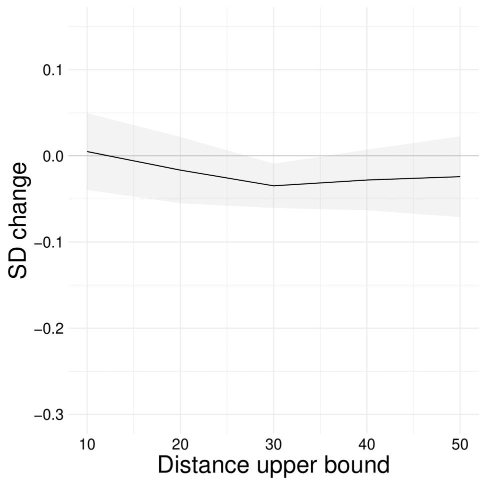


(e) Math, rural and FRPM>60%

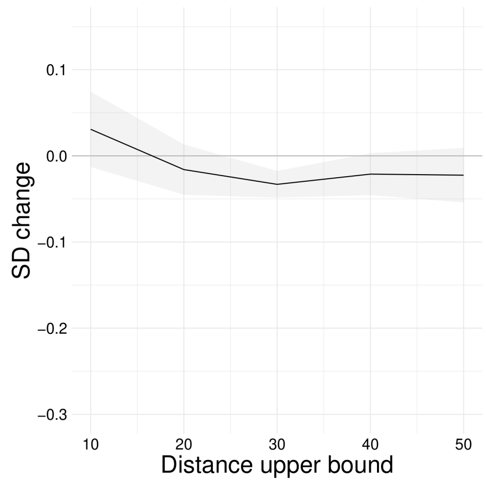


(f) ELA, rural and FRPM>60%

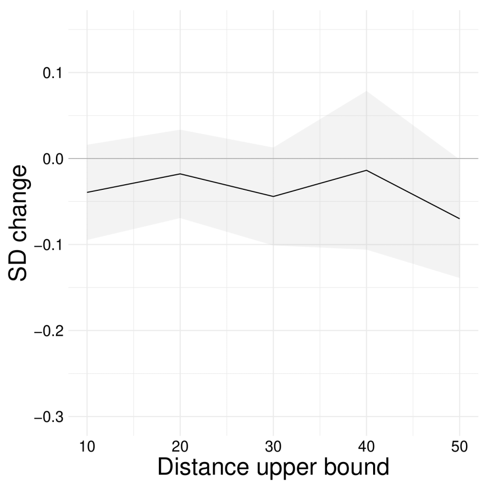
Figure 12: Effects of Local Large Wildfire Presence on Girls' Mean Test Scores, by Distance (Miles)



(a) Math, all schools



(b) ELA, all



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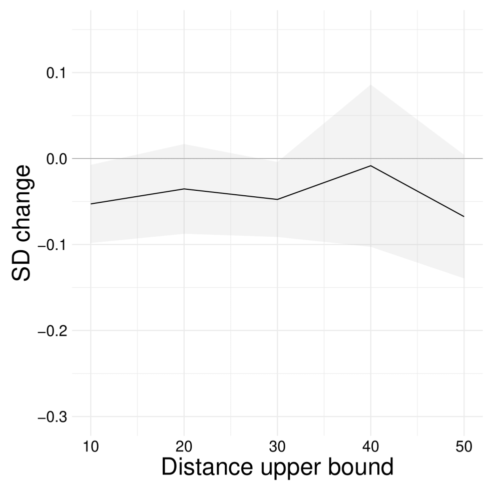
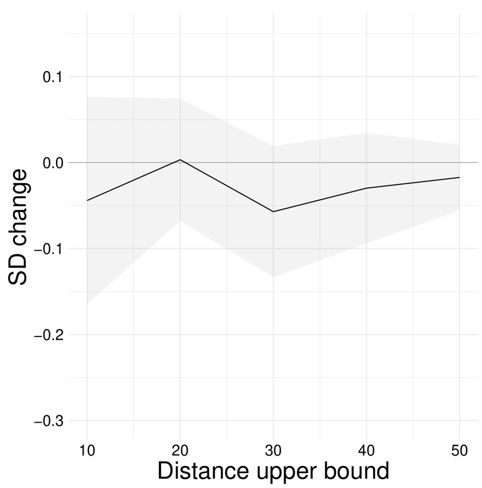
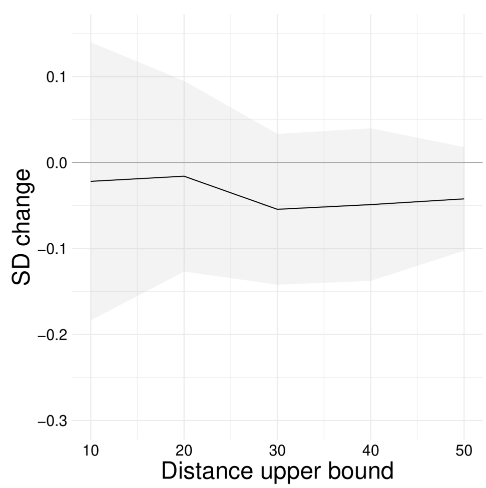


(d) ELA, rural schools

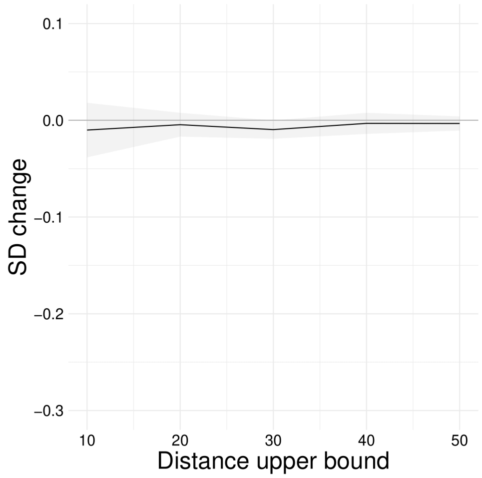


(e) Math, rural and FRPM>60%

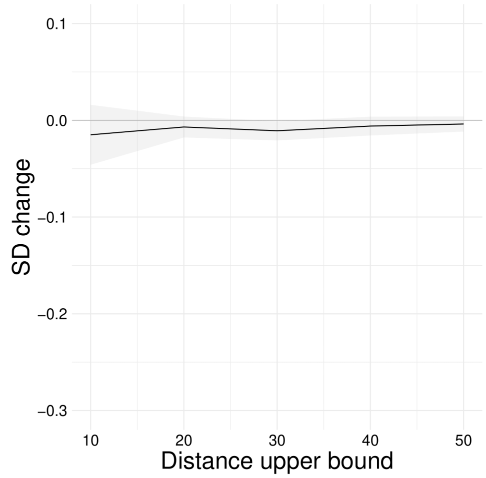


(f) ELA, rural and FRPM>60%

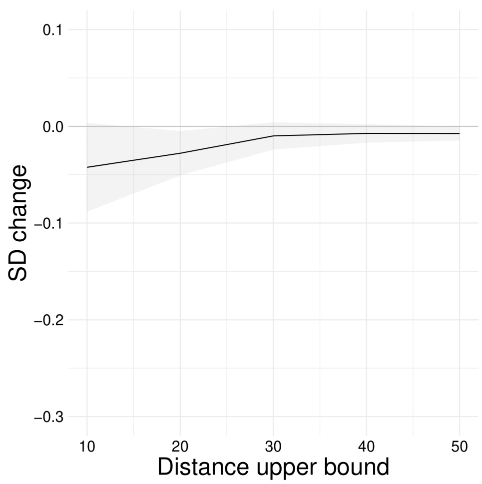
Figure 13: Effects of Local Wildfire Burn Area Count on Boys' Mean Test Scores, by Distance (Miles)



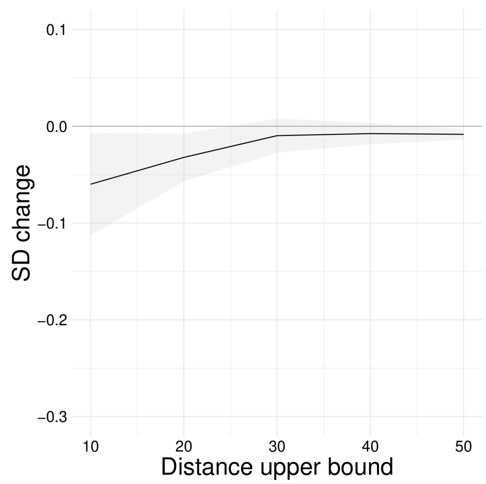
(a) Math, all schools



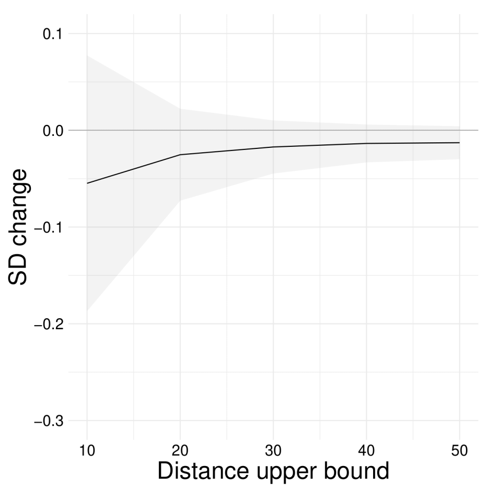
(b) ELA, all schools



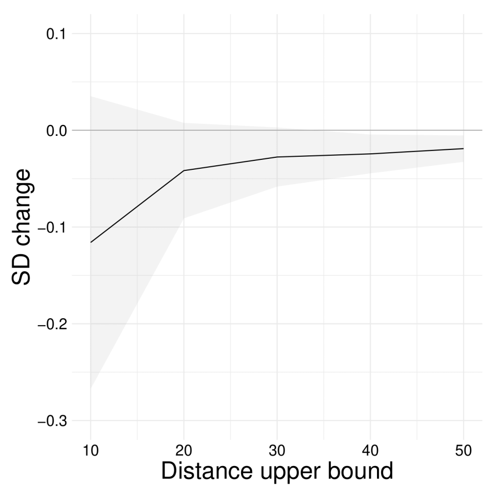
(c) Math, rural schools



(d) ELA, rural schools

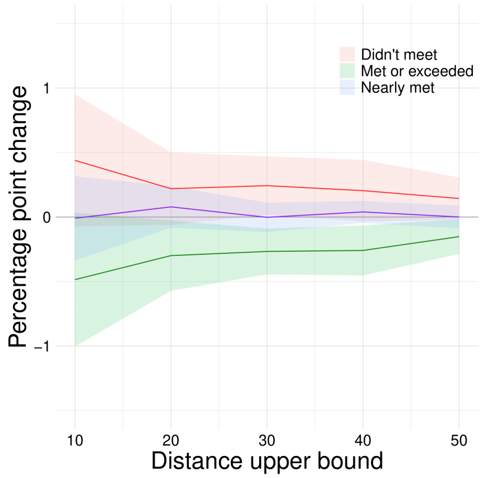


(e) Math, rural and FRPM>60%

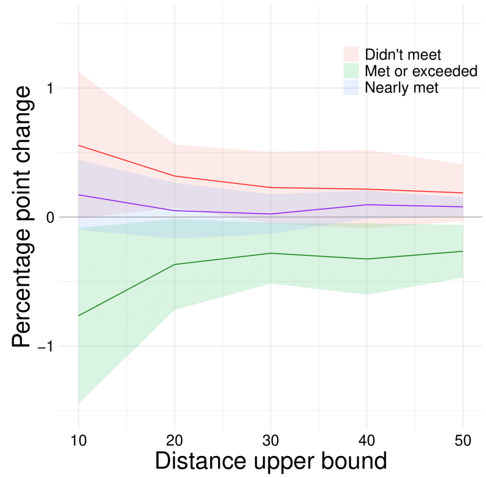


(f) ELA, rural and FRPM>60%

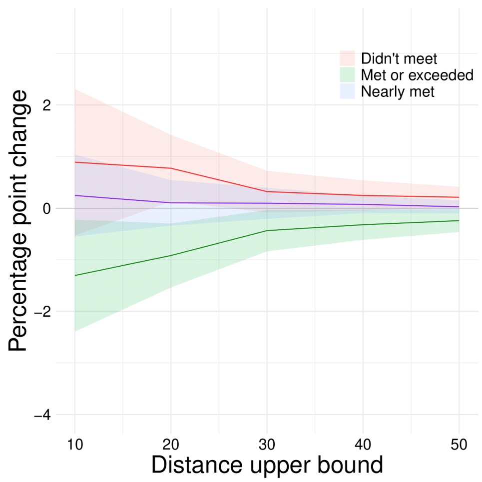
Figure 14: Effects of Local Wildfire Burn Area Count on Boys' Achievement Groups Relative to the State Testing Standard, by Distance (Miles)



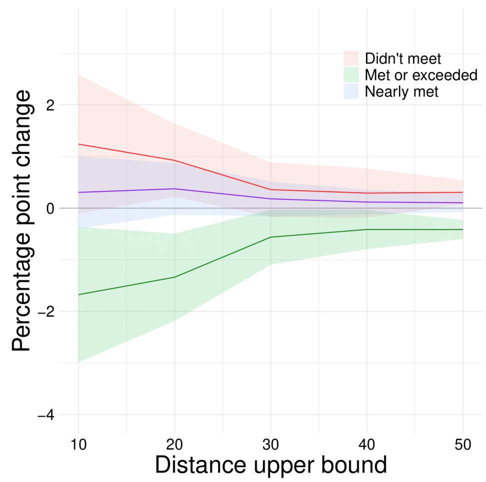
(a) Math, all schools



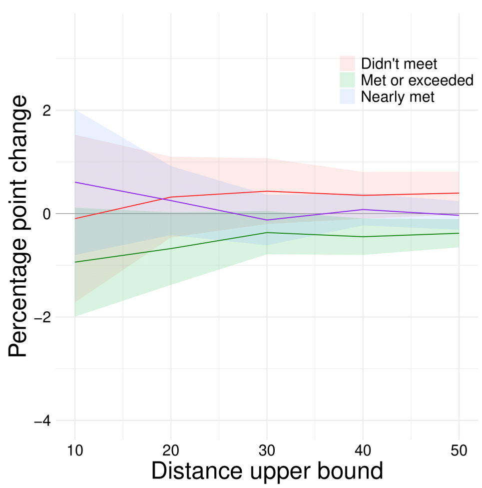
(b) ELA, all schools



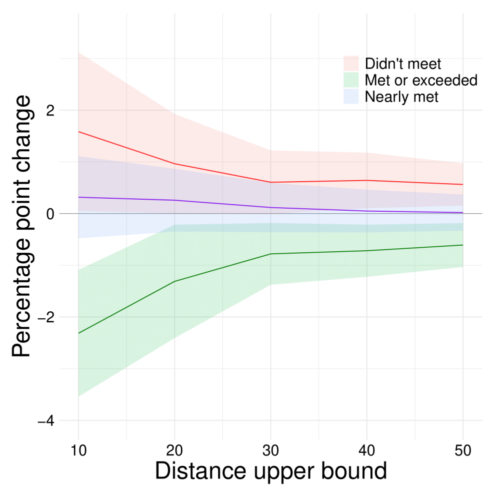
(c) Math, rural schools



(d) ELA, rural schools



(e) Math, rural and FRPM>60%



(f) ELA, rural and FRPM>60%

Tables

Table 1: Effects of Local Large Wildfire Presence on Boys' Standardized Mean Test Scores

	All Schools		Rural		Rural, FRPM>60%	
Dependent Variables:	Math	ELA	Math	ELA	Math	ELA
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Any fires within 30 mi	-0.055**	-0.056***	-0.058**	-0.059**	-0.106**	-0.139***
	(0.024)	(0.021)	(0.026)	(0.026)	(0.041)	(0.053)
Fit statistics						
Observations	6,315	6,321	1,241	1,243	555	556
R^2	0.475	0.398	0.621	0.573	0.5756	0.548
Within R^2	0.178	0.142	0.161	0.139	0.025	0.018

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

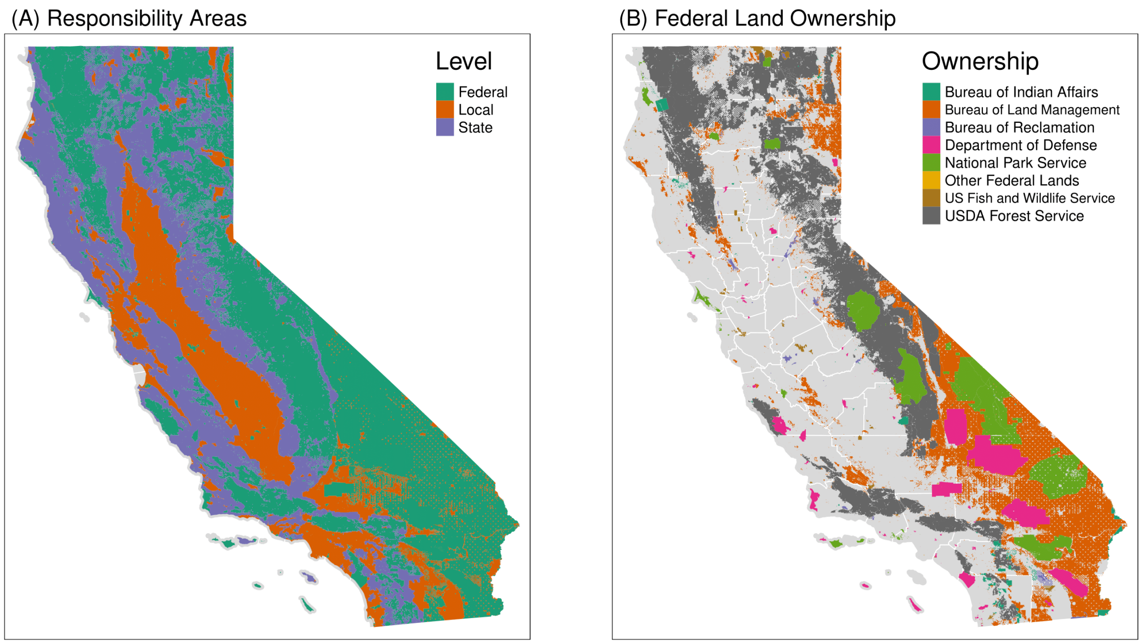
Table 2: Effects of Local Wildfire Burn Area Count on Percent of Boys Who Met or Exceeded the State Testing Standard

	All Schools		Rural		Rural, FRPM>60%	
Dependent Variables:	Math	ELA	Math	ELA	Math	ELA
Model:	(1)	(2)	(3)	(4)	(5)	(6)
Variables						
Fire count within 30 mi	-0.267***	-0.281**	-0.436**	-0.562**	-0.368	-0.780***
	(0.091)	(0.113)	(0.195)	(0.240)	(0.234)	(0.300)
Fit statistics						
Observations	6,315	6,321	1,241	1,243	555	556
R^2	0.715	0.575	0.681	0.622	0.644	0.618
Within R^2	0.411	0.293	0.247	0.228	0.056	0.048

*Signif. Codes: ***: 0.01, **: 0.05, *: 0.1*

A Supplementary Figures

Figure A.1: Wildfire Responsibility Areas and Federal Land Ownership



Notes: **On some territories, land owning agency may be different from the agency in charge of wildfire response, as defined by Direct Protection Areas. Source: CAL FIRE, Fire and Resource Assessment Program.**

Figure A.2: Distance to Very High or Extreme Fire Threat Area (Miles)

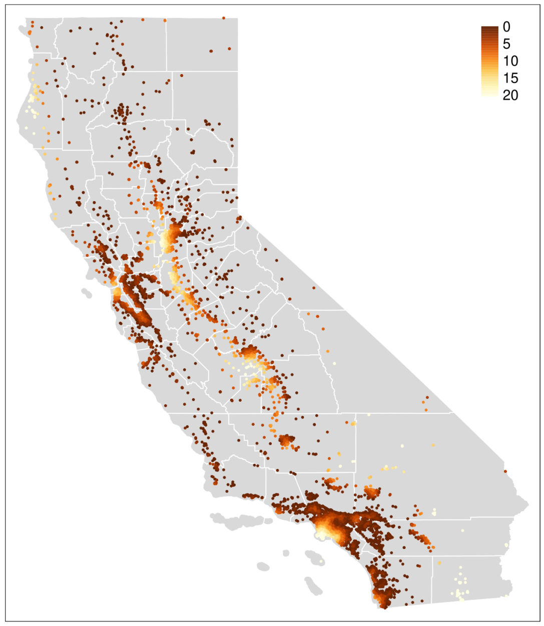
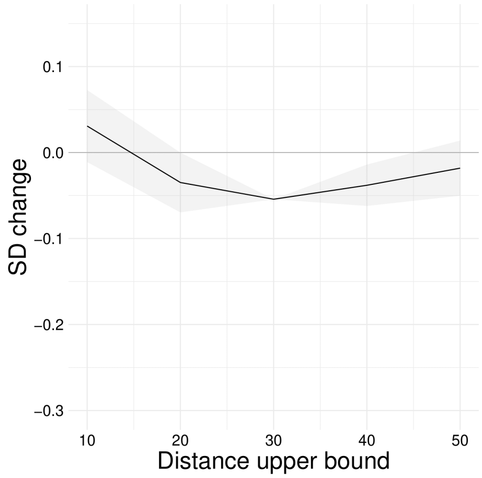


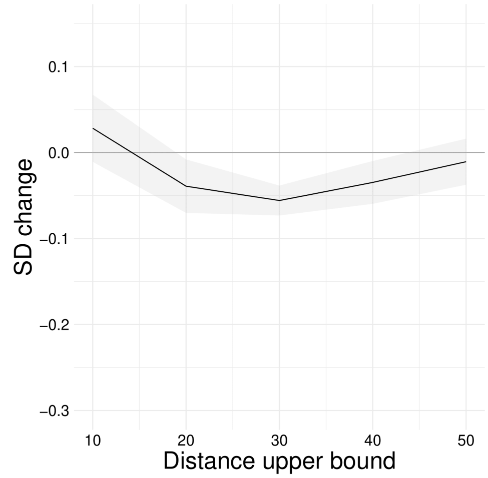
Figure A.3: CAL FIRE Fire Threat Areas by Level, 2019

Notes: **Source: CAL FIRE, Fire and Resource Assessment Program**

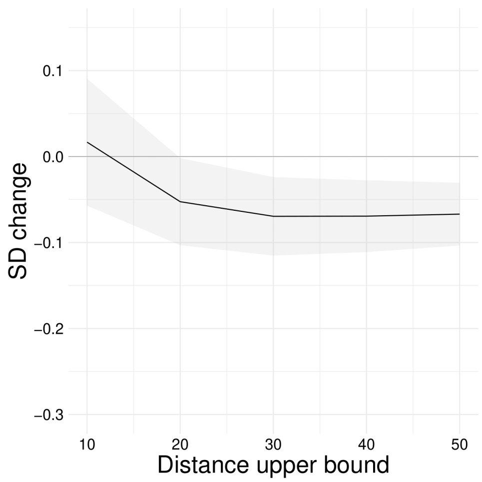
Figure A.4: Effects of Local Large Wildfire Presence on Boys' Mean Test Scores, by Distance (Miles), School-Level Fixed Effects



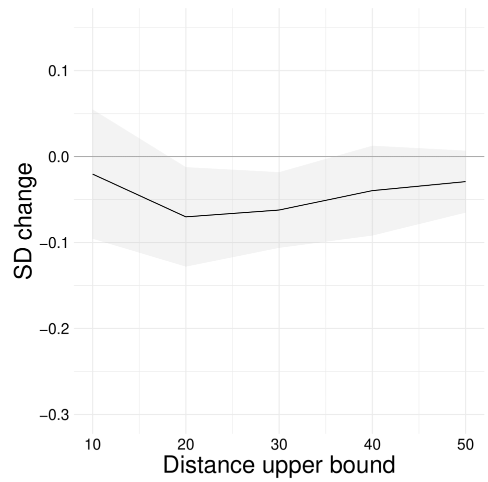
(a) Math, all schools



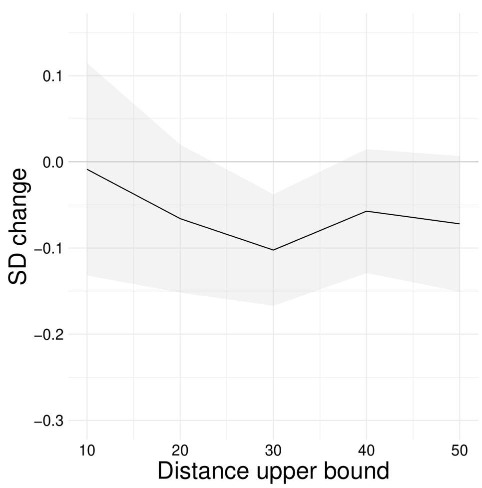
(b) ELA, all schools



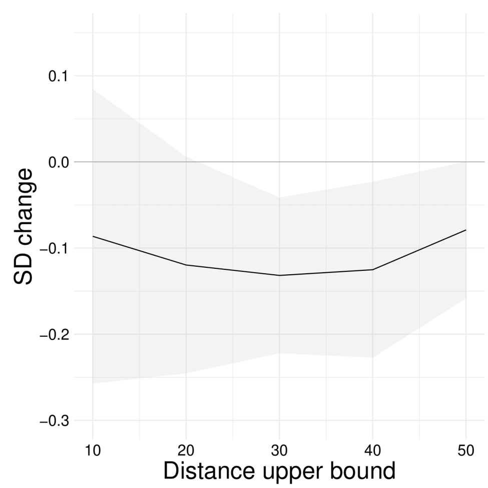
(c) Math, rural schools



(d) ELA, rural schools

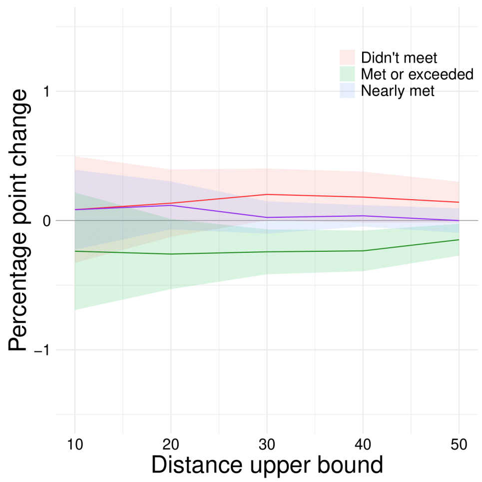


(e) Math, rural and FRPM>60%

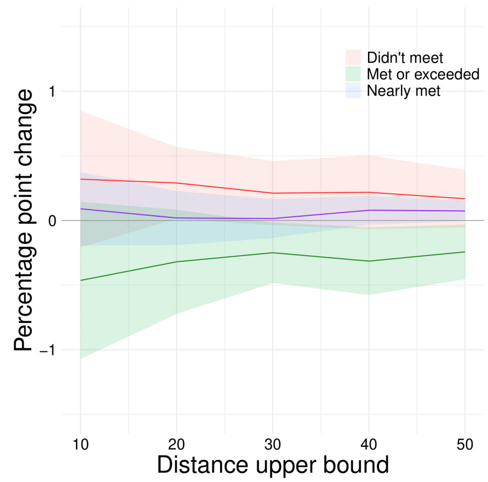


(f) ELA, rural and FRPM>60%

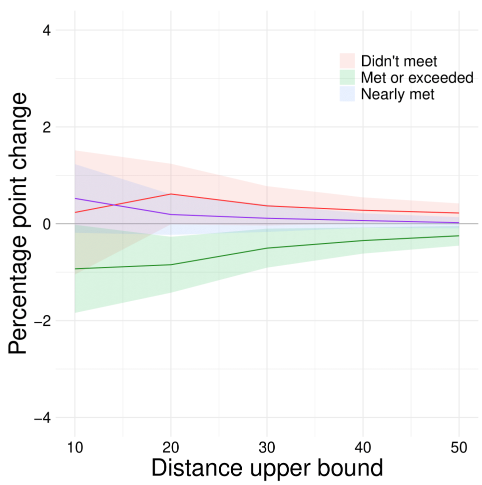
Figure A.5: Effects of Local Wildfire Burn Area Count on Boys' Student Achievement Groups Relative to the State Testing Standard, by Distance (Miles), School-Level Fixed Effects



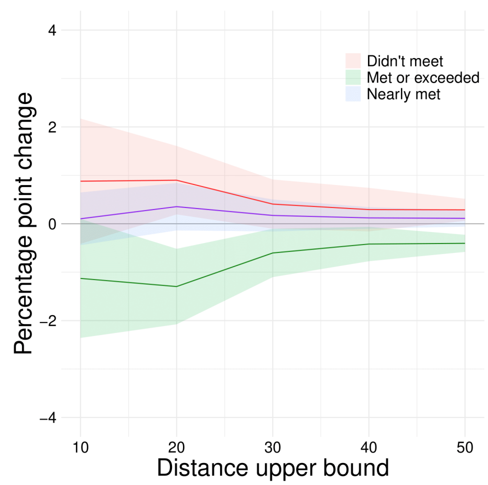
(a) Math, all schools



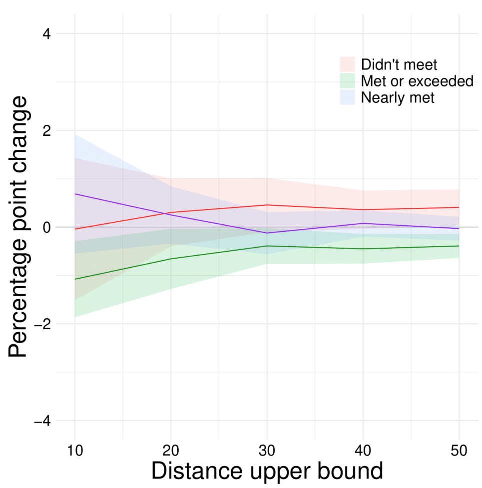
(b) ELA, all schools



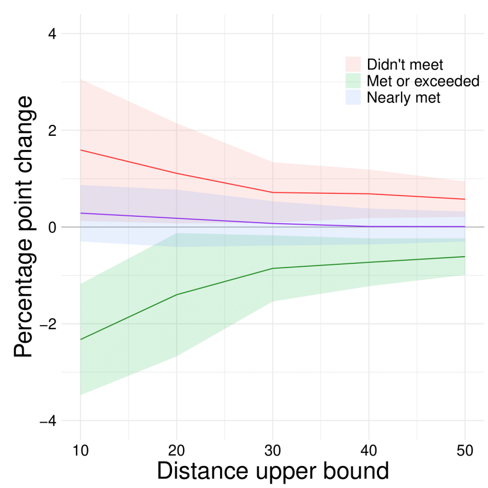
(c) Math, rural schools



(d) ELA, rural schools

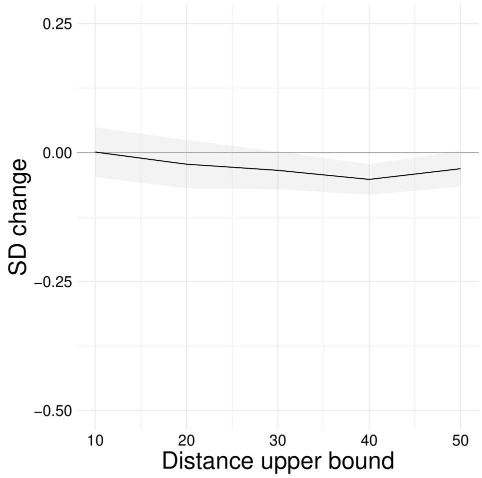


(e) Math, rural and FRPM>60%

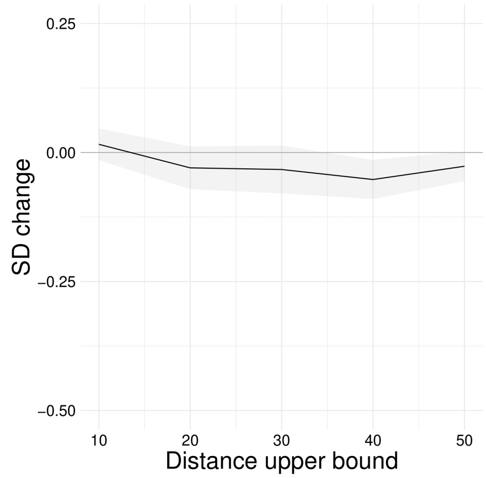


(f) ELA, rural and FRPM>60%

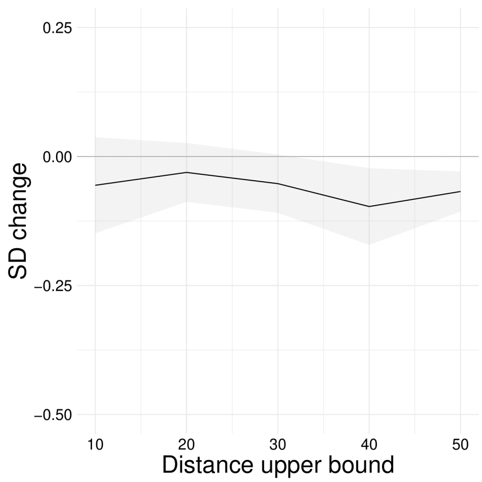
Figure A.6: Effects of Local Large Wildfire Presence on Boys' Mean Test Scores, by Distance (Miles), Wildfire Ignitions



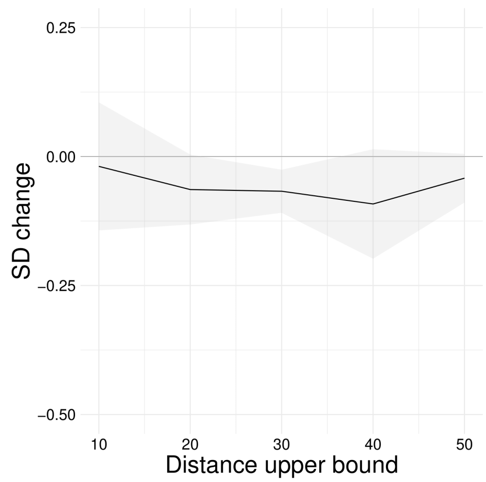
(a) Math, all schools



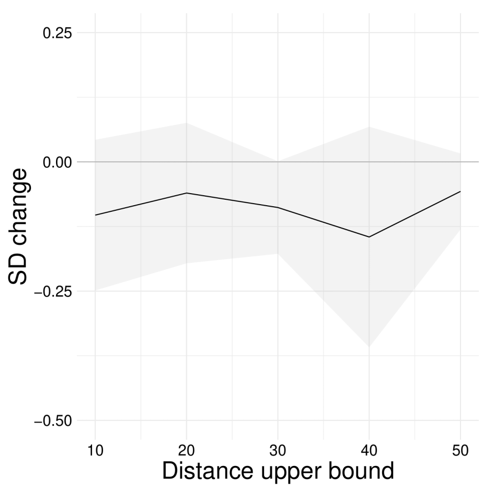
(b) ELA, all



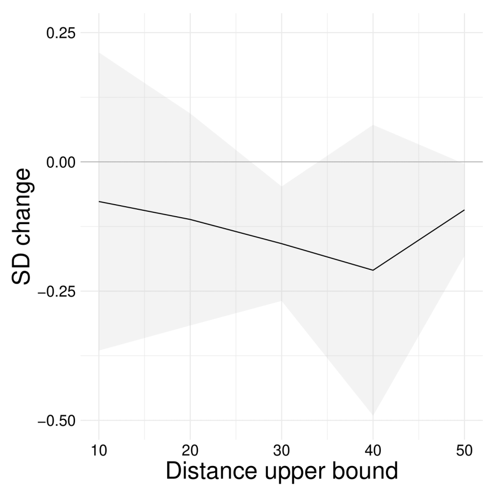
(c) Math, rural schools



(d) ELA, rural schools

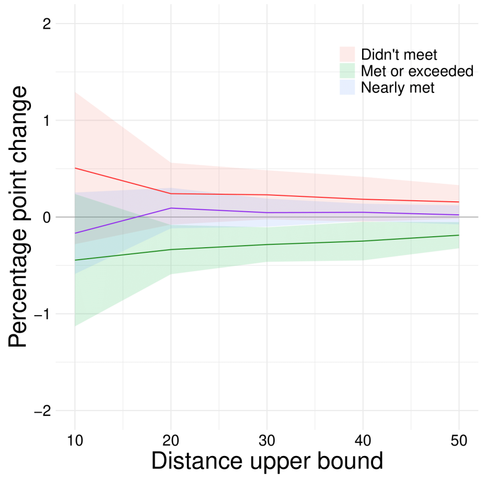


(e) Math, rural and FRPM>60%

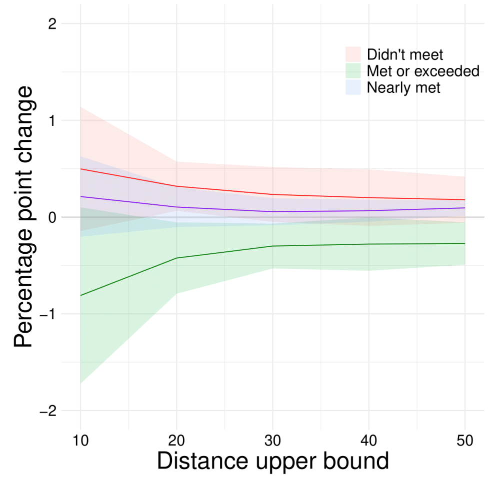


(f) ELA, rural and FRPM>60%

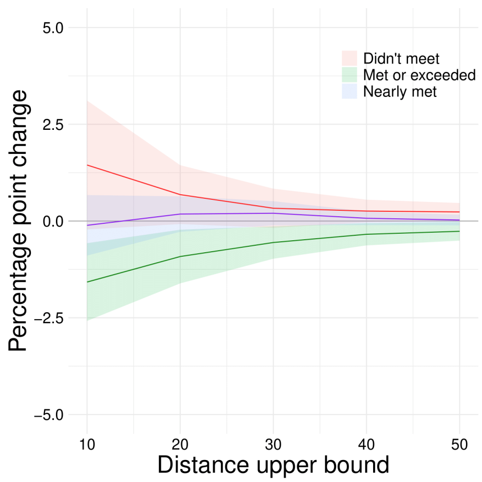
Figure A.7: Effects of Local Wildfire Ignition Count on Boys' Achievement Groups Relative to the State Testing Standard, by Distance (Miles)



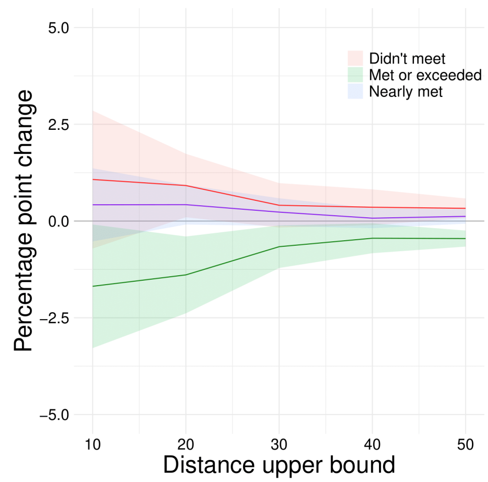
(a) Math, all schools



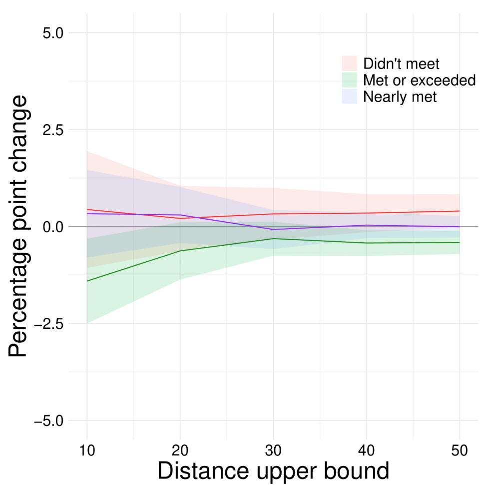
(b) ELA, all schools



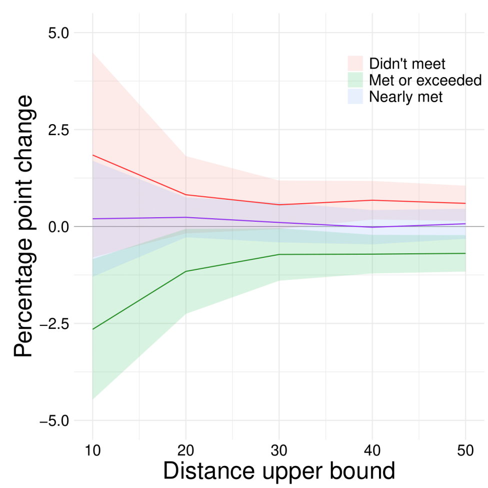
(c) Math, rural schools



(d) ELA, rural schools



(e) Math, rural and FRPM>60%



(f) ELA, rural and FRPM>60%

Out-of-State Enrollment, Financial Aid and Academic Outcomes: Evidence from Wisconsin

Natalia Orlova[†] Derek Rury[‡] Justin C. Wiltshire[§]

Abstract

Scholars disagree about the effect out-of-state university students have on potential in-state students. Despite paying a premium to attend state universities, researchers argue that out-of-state students may come at a cost to in-state students by negatively affecting academic quality or by crowding out in-state students. To study this relationship, we examine the effect of a 2016 policy at a highly ranked state flagship university that removed the limit on how many out-of-state students it could enroll. We find the policy caused an increase in out-of-state enrollment by around 29 percent and increased tuition revenue collected by the university by 47 percent. We argue that this revenue was used to fund increases in financial aid disbursed at the university, particularly to students from low-income households, indicating that out-of-state students cross-subsidize lower income students. We also fail to find evidence that this increase in out-of-state students had any effect on several measures of academic quality.

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1 Introduction

It is commonplace in the United States to attend college outside of your home state. According to results from the Current Population Survey, nearly one third of all students travel to another state for college. For many state universities, this implies that they face high demand from out-of-state students (OSS). In the face of decreasing funding from state government sources, these out-of-state students provide key resources that can fund educational expenditures (e.g financial aid), as they pay a high premium to attend public universities relative to in-state students.

Yet many state universities also face restrictions on the number of OSS they can enroll.[1] Politicians have also emphasized that public universities are meant to serve in-state, rather that out-of-state students.[2] Similarly, some scholars believe that enrolling more out-of-state students may come at a cost to in-state students, either through a crowding-out mechanism or a reduction in academic quality (Jacquette, 2017; Jacquette, Curs, and Posselt, 2016; Mathias, 2020). It is therefore unclear whether greater enrollment of out-of-state students will have a net positive or negative impact on in-state students at public universities.

In this paper, we study how changes in undergraduate OSS enrollment affect financial aid and academic outcomes. To study this, we exploit a 2016 policy change at a highly ranked state flagship university that removed the existing limit on out-of-state student enrollment—the University of Wisconsin, Madison (WM). To estimate the causal effect of the policy on several key outcomes, we employ a bias-corrected synthetic control estimating strategy. In this procedure, our comparison unit consists of an optimally-weighted average of control universities that are selected to best match WM. For our analysis, we use data from the IPEDS survey, which collects annual data on enrollments and several other variables from virtually every university in the country.

[1]These include the University of North Carolina and the University of California systems, along with others including (until 2016) the University of Wisconsin, Madison.

[2]Hilary Clinton declared during the 2016 presidential race, "We have got to get back to using public colleges and university for what they were intended. If it is in California, for the children in California. If it is in New York, for the children in New York."

We find that lifting the cap resulted in greater OSS enrollment, which provided sharply higher tuition revenues that were used to increase funding for in-state low-income students. This was achieved without any clear negative effect on academic outcomes. Specifically, we find that the policy increased out-of-state enrollment by nearly 29 percent four years after implementation. As a result, overall tuition revenue increased by 46.8 percent over that same time period. We fail to find any significant effects on either in-state or out-of-state tuition rates, indicating that this increase in revenue came from these newly admitted out-of-state students.[3] As a result of the increase in tuition revenue, we find WM spent 29.4 percent more on student grant aid, particularly for in-state students from families that report making $30,000 or less.

When we explore other potential effects of increased OSS enrollment, we do not find evidence of a negative impact on the academic quality of the university. We estimate that student-to-faculty ratios, retention and four year graduation rates are not impacted negatively, implying that WM may have increased spending on educational inputs (Deming and Walters, 2017). These results are robust to the use of different estimators and methods of inference, including placebo tests on the donor pools and on the timing of the policy. Our findings are also likely generalizable to most institutions that wish to increase OSS enrollment, as we demonstrate that most comparable institutions have excess capacity, potentially due to declining domestic enrollment over the past decade.

This paper contributes to two literatures. First, it contributes to work studying how out-of-state students affect the academic quality of universities and impact in-state students more broadly. Previous research has documented that as state appropriations to higher education have fallen, universities have increased the number of out-of-state students they enroll to make up the difference (Curs and Jacquette, 2017). These studies indicate that this increase in out-of-state students places academic burdens on universities (Jacquette, 2017; Jacquette, Curs, and Posselt, 2016; Curs and

[3] In the IPEDS data, tuition and fees is reported as a single variable. We use this variable in our main analysis. In reality, tuition and fee amounts are different for in-state and out-of-state students on average. In this paper, "tuition" is used to refer to the tuition and fees variable.

Jacquette, 2017), making the decision to enroll more out-of-state students more challenging. Further research has found that increases in OSS may also crowd-out in-state student enrollment (Curs and Jacquette, 2017), arguing that attendance at public universities is a zero-sum game between in-state and OSS. Our paper contributes to this literature by documenting that concerns about increases in out-of-state enrollments at state universities may be incorrect. While we estimate small negative effects on in-state enrollment, these results are non-significant, indicating no compelling evidence that greater OSS enrollment crowds-out in-state students. When we look at measures of academic quality, we also fail to detect significant negative effects.[4]

Second, this paper also contributes to the literature studying out-of-state student enrollment and higher education finance. Measuring the effect of increased OSS enrollment on financial outcomes is difficult as these outcomes are all likely related to other potentially endogenous factors such as student applications and enrollment, or how generous a university's or state's financial aid packages are (Winters V., 2012; Curs and Singell, 2002; Kerkvliet and Nowell, 2012). While most other papers in this literature use a fixed effects approach to study the relationship between OSS and university finances (Jaquette and Curs, 2015), our design leverages a policy that exogenously changes how many OSS the University of Wisconsin, Madison could admit. Moreover, this policy was decided by the Wisconsin Board of Regents, which operates under the approval of the governor, not the university itself. Recent work studying the relationship between states and universities finds that they are often optimizing different objective functions (Groen and White, 2004), reducing concerns that the policy choice we study was endogenous to the university.

Similar to research studying increases in international student enrollment (Shih, 2017; Chen, 2021), we find strong evidence that tuition revenue from out-of-state students was used to subsidize in-state low-income students, ultimately increasing the resources available to Wisconsin residents. Focusing on how changes in revenue influence university behavior, Miller and Park (2022) find that

[4] Our estimates studying student-to-faculty ratios and 4-year graduation rates actually indicate a potential positive effect of the 2016 policy, although our estimates studying these outcomes are noisy.

tuition freezes and caps at public intuitions limit universities' financial aid generosity, influencing where students who depend on financial aid go to college. We find that increases in OSS act as a counter-weight to such effects, essentially increasing the university's budget constraint. Lastly, our results are consistent with findings in Cook and Turner (2022), who document an increase in price discrimination at universities, where students who have a higher ability to pay are charged more than low-income students. We argue that the significant premia paid by out-of-state students plays an important role in universities' ability to support low-income students.

The paper is structured as follows: section 2 describes the University of Wisconsin–Madison and the policy change which removed the limit on out-of-state student enrollment; section 3 provides more details on the data and presents descriptive results; section 4 discusses our synthetic control estimation procedure; section 5 presents our main findings and considers the robustness of our results; and section 6 concludes.

2 Policy Change Details

The University of Wisconsin, Madison is a large public research university located in southern Wisconsin. As of 2022, it's undergraduate enrollment totalled 37,235, with an average GPA and SAT score for incoming freshman of 3.86 and 1,390 respectively, making admissions to WM competitive. WM is also part of the University of Wisconsin system, which consists of WM and 12 other universities. The University of Wisconsin system is overseen by a governing body known as the board of regents, which is comprised of 18 members, 16 of which are appointed by the governor of Wisconsin. Board members are subject to approval by the state senate and serve for two-year terms. The board of regents decide levels of public funding from the state for each university and dictate other features of the University of Wisconsin system, such as enrollment levels.

In an exercise of it's authority, in 2012 the Wisconsin board of regents decided to limit the out-of-state share of students which WM could enroll, to 27.5%.[5] Exemptions from this policy included students coming from Minnesota, who were for the purposes of the university, in-state students. The rationale for this decision was to prioritize students who were most likely to live in Wisconsin after graduation. According to documents presented at the board of regents meeting, over 75% of WM students from Wisconsin live in the state after graduation, whereas only 15% of OSS do so.

In October 2015, however, the university of Wisconsin board of regents voted to remove the limit on out-of-state enrollments at WM the next year. The purpose of this removal was to counteract decreasing high school graduation trends in Wisconsin.[6] In addition to this rationale, previous work has documented that OSS can also add a significant boost to revenue, as they often pay high premiums to attend out-of-state universities. Specifically, at WM, in-state students pay $10,720 per year, while OSS pay $39,427 per year for the 2022-2023 school year. The OSS enrollment policy took effect in the fall of 2016. [7] This policy removed any constraint on the number of OSS WM enrolled each year. We use this policy change to study three research questions; 1) whether it caused an increase in out-of-state enrollment, 2) what were the financial impacts of this policy and, 3) what academic impacts did this policy have on the WM student population?

3 Data, Sample, and Descriptive Results

3.1 Data

To study OSS enrollment and university outcomes, we use the Integrated Postsecondary Education Data System (IPEDS). IPEDS is the primary source of information on postsecondary institutions in the U.S. It includes a wealth of information on institutional characteristics, the student body, and

[5] In 2012, the regents voted to increase the share from 25 to 27.5%

[6] Details on the rationale can be found in the meeting minutes for the October 2015 board of regents meeting, found here: https://www.wisconsin.edu/regents/download/meeting_materials/2015/october_2015/October-2015-Education-Committee-pdf-corrected-1007.pdf

[7] As part of the policy, the board of regents required that at least 3,500 students from the state of Wisconsin be admitted each year.

school and student finances. We use data on basic institutional characteristics, such as type (e.g. public or private) and level of degrees offered, admission considerations, enrollments, retention and graduation rates, financial aid distribution, and school revenues and expenditures. The timing of data collection for some variables is not straightforward and we describe it in more detail below. Additionally, whenever we refer to an academic year as a single year rather than a range, we're referencing the academic calendar start year (e.g. 2015-2016 would be described as 2015, except as indicated below).

Postsecondary institutions collect fall enrollment information on October 15 or on the institution's official fall reporting date. IPEDS provides breakdowns of enrollment statistics both at the total undergraduate level and at the first-time degree/certificate-seeking undergraduate student level. We refer to the latter group as freshmen throughout the paper. Enrollment breakdowns are available by gender, race, and age group (under 25 or 25 and older) at the total undergraduate level. Enrollments by country (U.S. or non-U.S. only) and by the U.S. state of residence when the student was admitted are available for freshmen. The OSS group includes either international students or individuals from the U.S. state that is different from the state of the institution they enrolled in. For WM, we define students coming from both Wisconsin and Minnesota as in-state. Institutional reporting of this data to IPEDS is only mandatory in even years. In odd years, a lot of schools choose not to submit this data, so we see a lot of missing values. Retention rates are based on fall enrollment counts of returning full-time freshman undergraduates. [8] Student-to-faculty ratio is the count of total undergraduate full-time equivalent students divided by the count of full-time equivalent instructional staff not teaching in graduate programs.

IPEDS reports graduation rates for student cohorts who entered the institution six years prior to current academic year. We use three sets of graduation rates - for students who completed their undergraduate degrees in four, five, and six years. IPEDS measurement timing means that the

[8] This variable only measures transitions from first-year to second-year, which is only one measure of total retention. The freshman retention rate is often used as a valuable proxy for important outcomes such as graduation.

rate of students graduating in four years is based on completion counts two years prior to current academic year, the rate of students graduating in five years is based on completion counts from the previous year, and the rate of students graduating in six years is based on current year completions.

Some measures of school finances reflect statistics for the fiscal year that ended before October 1 of the current academic year. The reporting period varies slightly across institutions (fiscal year end dates in our sample range from May 31 to August 31) but can be roughly thought of as the previous academic year. Current academic year school finance variables are published tuition and fees. Fiscal year variables are revenue shares and student financial aid. Measures of student financial aid, such as Pell Grant recipient counts and average amount of aid per student, only include full-time freshmen. Student financial aid by household income is further restricted to full-time freshmen paying in-state tuition who were awarded any grant or student loan aid.

3.2 Primary Sample and Descriptive Results

For our primary analysis we restrict our sampling frame to public 4-year land-grant institutions.[9] We further restrict the donor pool to ensure we have a consistent sample for all outcomes over time. This requires a complete panel for all outcome variables and also for our covariates during the pre-treatment period (so they can all be matched on for each pre-treatment year specified). Figure A.1 maps our final primary donor pool institutions (listed in Table A.1). Most of the sample loss is due to the voluntary nature of reporting of enrollments by student residence in odd years, as discussed in the the previous section.

Figure 1 provides visual evidence of the WM policy effect on in- and out-of-state enrollment levels and logged enrollments. In panels A and C, we see that the in-state enrollment did not change

[9] A U.S. land-grant college or university is an institution that has been designated by its state legislature or Congress to receive the benefits of the Morrill Acts of 1862, 1890, and 1994. The original designation of these institutions reflected a growing demand for agricultural and technical education in the U.S. and was intended to provide a broad segment of the population with a practical education. In section 5.2 we relax this restriction on the sample of untreated institutions.

across the policy threshold for WM. It also remained constant or increased slightly over time for donor pool schools. Panels B and D show OSS enrollment trends. In 2015, the number of OSS at WM was just above 2,000. This number increased by 50% to over 3,000 by 2019. There is some evidence of OSS enrollments trending upwards over time for donor pool schools, but nothing as drastic as seen at WM.

To alleviate concerns about a changing distribution of incoming freshmen as a result of the WM policy, we present figures 2 and 3. Figure 2 addresses the scenario of a potential drop in academic quality of students as more out-of-state students flood the school. The figure plots math and verbal SAT scores of incoming freshmen at the 75$^{\text{th}}$ (panels A and B) and 25$^{\text{th}}$ (panels C and D) percentiles. For both WM and almost all donor pool schools, and particularly for the verbal exam, there is an abrupt increase in student scores between 2016 and 2017. This is most likely an artificial result that has nothing to do with student quality, as the SAT had undergone structural changes in March 2016. Despite the changes, there is otherwise no clear sign of a drop in academic student quality at WM compared to other institutions. Past 2017, trends remain mostly flat (verbal) or even slightly positive (math). It appears that WM has the reputation to keep attracting high-quality students from outside the state. Figure 3 shows statistics for the share of freshmen awarded Pell Grants. The Federal Pell Grant Program is need-based, rather than merit-based, and program participation is not related to individual schools' assessment of student financial need. Again, there is no evidence of a changing student composition based on family financial need in response to the WM policy.

Appendix figure A.2 further reports summary graphs on our model covariates and here we see that in the years before the WM policy, our treated unit generally falls within the support of the donor pool. Finally, Figures A.3–A.5 show that the same is true for our outcome variables: OSS enrollment, revenue from tuition and fees, financial aid, and academic quality outcomes.

One final potential issue worth considering is how much capacity universities, including WM, have to enroll additional students. To measure capacity, we note that IPEDS records the maximum number of students universities can supply with on- or off-campus residential facilities. Using the difference between this statistic and total enrollment as a measure of extra enrollment capacity, we find that in our primary sample of donors 94 percent of schools have the capacity to enroll more students. For supplemental donor pool samples, between 80 (public 4-year) and 90 (R1+R2) percent of universities have excess capacity to enroll students. Among schools able to enroll more students, the average excess capacity by donor pool sample ranges from 1,500 to 3,100. This is perhaps unsurprising as domestic enrollment in the U.S. has slightly decreased over the past decade, essentially leaving space for new OSS students at universities facing OSS limits.[10]

4 Methodology

Our preferred estimation strategy is a bias-corrected synthetic control method (SCM). As a robustness check we also present results using a synthetic difference-in-differences (SDiD) estimator (Arkhangelsky et al., 2021), and juxtapose both sets of estimates against those from a two way fixed effects (TWFE) estimator with a difference-in-differences research design.

Synthetic control methods (SCMs) (Abadie and Gardeazabal, 2003; Abadie, Diamond, and Hainmueller, 2010, 2015) are widely-used in applied research to estimate the effects of policy interventions in cases with few or even one treated unit(s), when many regression-based approaches may be inappropriate. Unlike difference-in-differences research designs, SCMs do not rely on a parallel pre-trends assumption; and they are explicit about the contribution of each untreated unit to the counterfactual estimates, making those estimates transparent and easily interpretable (Abadie, 2021). For these reasons, we argue that a synthetic control estimating strategy is ideal for estimating the effects of increasing out-of-state enrollment.[11]

[10]Results on enrollments are collected by the National Center for Education Statistics and can be accessed at https://nces.ed.gov/fastfacts/display.asp?id=98

[11]Our synthetic control method applies a procedure to address bias resulting from pairwise matching matching discrepancies among predictor variables.

The idea underlying SCMs is that, for any "treated" unit (affected by a policy intervention), the effects of treatment can best be estimated by comparing the evolution of an outcome of interest to the combined evolution of that outcome in otherwise-similar but untreated "donor pool" units. Given a set of specified "predictors" of the outcome of interest during the pre-treatment period, SCMs estimate positive weights for a subset of donor pool units, such that the evolution/trajectory of the weighted average of untreated-unit outcome values (the "synthetic control") will be nearly identical to that of the associated treated unit during the pre-treatment period.

Under fairly general assumptions and a good pre-treatment fit, the synthetic control trajectory serves as a plausible estimate of the counterfactual trajectory for the treated unit during the post-treatment period. The difference between the trajectories of the treated unit and its synthetic control in a given post-treatment period is the estimated effect of the policy intervention. Causal inference can be conducted by permuting treatment across the donor pool units and comparing the trajectory of the estimated effect to the distribution of placebo treatment effects. We point interested readers to Abadie (2021) for a formal exposition of the synthetic control method (and to Wiltshire (2022) for practical details on implementation of the bias-correction procedure).

We normalize our outcome variables to 100 in 2015, the final pre-treatment year. To ensure our estimated synthetic controls are similar to University of Wisconsin, Madison, we include as covariates the non-normalized 2015 values of freshman out-of-state enrollment, institutional grant aid, financial aid received by full-time in-state freshmen from households earnings under $30,000/year, and the level and share of full-time freshmen receiving Pell Grants, and for each outcome include several (normalized) pre-treatment year values of the outcome as predictors. In our preferred specification, and to capture potentially important variation in student demographics and international student enrollment, we additionally include as covariates the 2015 shares of undergraduates who are male, of undergraduates who are under 25 years old, and of undergraduates who are Asian (we also present estimates without these covariates as a robustness check on our results).[12]

[12]Including the share that are Asian may also help capture differences in the size of the international student body,

We follow Abadie, Diamond, and Hainmueller (2010) and estimate the synthetic control weights, $w_2,...,w_{J+1}$, on our J untreated/donor universities to minimize the distance between the synthetic control values of the specified predictors and the predictor values at the University of Wisconsin, Madison, given a separate set of weights, $v_1^i,...,v_k^i$, that determine the relative importance of the predictors. We impose $w_j \geq 0$ and $\sum_{j=2}^{J+1} w_j = 1 \ \forall \ j \in \{2,...,J+1\}$, which are standard restrictions in most synthetic control applications.

We then apply the synthetic control bias-correction proposed by Abadie and L'Hour (2021) and Ben-Michael, Feller, and Rothstein (2021), to mitigate potential bias resulting from differences in predictor variable values between the University of Wisconsin, Madison and its synthetic control donors. This bias-correction procedure is detailed in Wiltshire (2022), which describes the Stata package, `allsynth`, that we use to estimate our synthetic control results.

The most widely examined and adopted inferential approach for synthetic controls, developed in Abadie, Diamond, and Hainmueller (2010, 2015), generates p-values based on distributions of the ratios of the mean squared prediction error (RMSPE) calculated by permuting treatment across untreated units and estimating placebo treatment effects. We primarily adopt this inferential approach, and to ensure our p-values are conservative, we do not remove any donor pool units with a poor pre-treatment fit. Given this choice, and given we have a single treated university and just 39 donor pool universities, our tests are underpowered. To help mitigate this issue, where appropriate we adopt one-sided tests which can substantially increase the statistical power (Abadie, 2021). Specifically, we posit that any detectable effect of the policy on OSS enrollment, tuition revenues, and financial aid awarded will be positive, and conduct one-sided tests for those outcomes. We are agnostic about the sign of any effect on the remaining outcomes of interest, and so conduct two-sided tests for those outcomes. In all cases we view an RMSPE p-value of ≤ 0.1 as indicative of statistical significance given the relatively few donor pool units and given we construct our p-values to be conservative and never approach zero (e.g. even when WM has the largest RMSPE relative to

which may also have implications for tuition revenues and student outcomes.

the empirical distribution of placebos, the associated p-value will be $rank(RMSPE_{WM})/N = \frac{1}{N} > 0$).

We also present estimates of the treatment effects on the (normalized) outcomes of interest using the Synthetic Difference-in-differences (SDiD) estimator (Arkhangelsky et al., 2021) along with p-values from the prescribed placebo variance procedure, implemented using the `sdid` Stata package (Clarke et al., 2023). Tests are again one- or two-sided as with the synthetic control results. The synthetic control covariates are included but have little effect on the SDiD estimates as they are pre-treatment averages observed in each institution, and as such are effectively controlled for by unit fixed effects.[13]

Finally, for comparative purposes, we use OLS to estimate the model:

$$Y_{it} = \gamma_i + \lambda_t + \sum_{s=2016}^{2019} \beta_s \mathbb{1}[s = t] \times D_i + \varepsilon_{it}$$

where Y_{it} is the (normalized) outcome value of interest for institution i at time t, γ_i and λ_t are respectively institution and year fixed effects, D_i is a dummy indicating whether i is the University of Wisconsin, Madison, treated in $t = 2016$, and β_s are the coefficients of interest we present.[14] We note that, with a single treated unit, OLS is not consistent for the β_s (Conley and Taber, 2011). Thus while we present the associated asymptotic p-values for reference, they should be interpreted with caution.[15]

[13] This selection of covariates maintains consistency with the synthetic control specifications and also ensures there is no bias from using "bad controls".

[14] The inclusion of the synthetic control covariates makes no difference to our coefficients of interest as they are pre-treatment averages observed in each institution, and are thus effectively controlled for by the unit fixed effects.

[15] We again conduct one- or two-sided tests consistent with our approach for our other estimators.

5 Results

To study the causal effect of the 2016 policy, we present results from each of our estimators—including two synthetic control specifications, three different donor pool samples, and several post-treatment years—for each of our outcomes of interest.

5.1 Main Results

Our preferred estimates are the synthetic control estimates using the complete set of matching covariates, presented in Table 1 and Figures 4–6. We examine the effect of the policy on 2019 outcomes.[16] We first present results on our "first-stage" outcomes, including out-of-state student enrollment and tuition revenues, in Figure 4 and section 1 of Table 1. This allows us to check whether the policy had the intended effect of increasing institutional revenues by increasing OSS enrollment. In Figure 5 and section 2 of Table 1 we then examine effects on financial outcomes, including institutional grant aid, financial aid to students from households earning $\leq \$30,000$, and published tuition fees for in-state and OSS, separately. This serves to check whether any effects seen in our first-stage resulted in other policy changes that would have directly affected students financially. Finally, in Figure 6 and section 3 of Table 1 we consider the effects on academic outcomes including in-state freshman enrollment, the retention rate of full-time students, the student-to-faculty ratio, and the 4-year graduation rate. This serves to check whether any first-stage effects had consequences for academic policies that could impact access or academic quality for students.

Figure 4 shows the evolution of synthetic control "gaps" for both OSS enrollment (Panel A) and overall tuition revenue (Panel B), for WM (in blue) and all donor schools (in gray) in each post-treatment year. Both graphs show large increases beginning in 2016, the year the policy took effect. Table 1 quantifies the magnitude of those increases in 2019, showing the policy caused

[16] This year was chosen because it offers us the most recent effect of the policy, while remaining free from the effects of the COVID-19 pandemic. All enrollment decisions were made in the fall 2019 term. We have confidence that measurement of our outcome variables was unaffected by the pandemic. However, as one of several robustness checks we also present estimates focusing on the 2018-2019 year in the robustness section.

an increase in OSS enrollment of 28.9 percent (RMSPE p = 0.05) and increase in overall tuition revenue collected by WM of 46.8 percent (RMSPE p = 0.075).[17]

To examine whether this increase in tuition comes from changes at the intensive or extensive margin, we then examine the impact of the policy on both in and out-of-state published tuition fees. Panels A and B of Figure 5 present SC gaps for these outcomes. We see that there is no published impact on in-state tuition, although there visually appears to be a modest increase in published out-of-state tuition. Table 1 confirms there no significant impact on either, with a point estimate of -0.52 percent on in-state tuition fees (RMSPE p-value of 0.775). The estimated impact on OSS tuition is 11.6 percent, but RMSPE p-value of 0.25 indicates this estimate is not statistically significant. This leads us to conclude that increases in tuition revenue which we estimate were caused by the 2016 policy come from changes in OSS enrollment.

We next study how this increase in revenue impacted the amount of financial aid dispersed at WM. Previous work has shown that public universities use increases in tuition to subsidize low-income students (Shih, 2017; Cook and Turner, 2022). Figure 5 presents the synthetic control gaps for institution grant aid disbursed, with Panel C showing overall institutional grant amounts awarded to freshman and Panel D showing financial aid awarded to low-income students whose families earn less than \$30,000 a year. Both panels capture a large, distinct increase after the 2016 policy. Table 1 shows significant treatment effects of 29.4 percent (RMSPE p = 0.075) and 24.1 percent (RMPSE p = 0.10), respectively. We therefore conclude that the the increase in tuition allowed WM to support more students, and particularly to provide more financial support to low-income students.

Previous work has reported that OSS students place a burden on universities' academic quality, negatively impacting in-state students (Jacquette, 2017). We thus next examine effects on outcomes that might be impacted by increased OSS enrollment, with a focus on measures that capture

[17]The across-the-board zero gaps in 2012 and 2013 for all results in Figures 4–6 follow mechanically from the bias-correction procedure given the inclusion of outcomes in those years as predictors. See Wiltshire (2022).

elements of academic quality. While we admit that these variables are very coarse and may not represent perfectly accurate measures of academic life at the university, we view them as important proxies of academic quality during this period.

Figure 6 presents the SC gaps for each of our academic outcomes. We see no indication that the 2016 policy impacted retention at WM (Panel B). The plots for in-state enrollment (Panel A), student-to-faculty ratios (Panel C), and 4-year graduation rate (Panel D) all show some mild movement post-treatment. However, Table 1 shows that the 2019 estimated effects of -2.6 percent (RMSPE p = 0.275), -9.7 percent (RMSPE p = 0.85), and 7.4 percent (RMSPE p = 0.45), respectively, make clear that none of these estimates are statistically significant.[18] Panel B of Figure 6 and the values in Table 1 (-0.2 percent, RMSPE p-value of 0.45) show no change in the retention rate. If we focus particularly on the plot for in-state enrollment, we can see a small u-shape in the post-treatment period, but in fact none of these point estimates from any year are statistically significant. In summary, we conclude that the 2016 policy did not place a negative burden on WM or on its students' academic outcomes.

5.2 Robustness

To test the robustness of our main findings, in Tables A.2–A.4 we present results based on various tests and alternative specifications and donor pool samples. These include: re-running our synthetic control estimation for 2019, excluding the covariates for student sex and age and international enrollment (column 1 of Tables A.2–A.4); estimating treatment effects in 2018 to demonstrate that our main findings are not contingent on selecting 2019 to measure our outcomes (column 2); changing our donor pool sample to R1 and R2 universities (column 3) and all public four-

[18] A negative point estimate on the student-to-faculty ratio may sound counterintuitive, given the dramatic increase in out-of-state freshmen. We attribute this to two causes. First, though not as drastic, there was a simultaneous increase in instructional, research and public service staff employed at WM. Second, the student-to-faculty ratio is based on the count of all undergraduates, and not just freshmen. Given our positive point estimate on the 4-year graduation rate, it is possible that there was a decrease in senior student enrollment through faster graduation (unless the counterfactual was to drop out altogether, rather than take longer to graduate).

year universities (column 4), both subject to the restriction that a complete panel is observed for all included institutions; using a two way fixed effects (TWFE) estimator (column 5); and using a synthetic difference-in-differences (SDiD) design (column 6). We note that while we present TWFE estimates for completeness, the validity of the standard errors for these treatment effects assumes homoskedasticity across units and normality of the estimand (Arkhangelsky et al., 2021), and therefore should be interpreted with a degree of caution. For the SDiD estimates, we present p-values estimated using the placebo variance (Arkhangelsky et al., 2021).

Looking at our first stage outcomes in Table A.2, the magnitudes and significance levels appear similar to those from our primary specification. The point estimates generally grow larger as we expand the donor pool to R1 and R2 universities and to all public four-year universities, though we note that for the R1+R2 analysis, only, the estimates for OSS enrollment is no longer significant (p = 0.362). When we expand the donor pool even more, to include all public four-year universities, the point estimates for OSS enrollment grow even larger and regain significance (p = 0.01). The estimated effects on tuition revenues are significant across the board. Looking at our financial outcomes in Table A.3, we find a similar pattern: most point estimates resemble those from our primary specification. The estimated effect on average institutional grant aid awarded loses significance for the R1+R2 analysis, only (p = 0.246) and regains significance for the analysis using all public four-year universities in the donor pool (p = 0.02). The point estimates on average student aid awarded to students from low-income households are all similar in size to our primary estimates, though they are somewhat noisier, with the p-values in columns (1) and (4) slipping to p = 0.13 and p=0.12, respectively, and that for the SDiD estimate reaching p = 0.29, suggesting a degree of caution is in order. Additionally, the SDiD estimated effect on out-of-state tuition and fees positive and marginally significant despite being smaller than our preferred estimate. Lastly, all of the estimates looking at our academic outcomes, presented in Table A.4, are similar to those from our primary specification.

6 Discussion and Conclusion

In this paper, we investigate a 2016 policy at the University of Wisconsin, Madison that removed the limit on the number of out-of-state students it could enroll. Using a synthetic control approach, we estimate the policy led to a significant increase in the proportion of out-of-state students admitted to the university by 28.9 percent as well as an increase in tuition revenue by nearly 50 percent. We also find an increase in the amount of financial aid distributed to students—importantly for those whose families earn less than $30,000 a year—by 29.4 and 24.1 percent, respectively. We fail to detect significant effects on several educational outcomes that measure academic quality at universities, including retention, student-to-faculty ratios and graduation rates. Given these results, we view concerns about the negative impacts on academic quality from out-of-state students as misplaced.

Furthermore, under increasingly tight budget constraints experienced by universities, we see these results as confirmation that out-of-state students represent a much needed financial resource for universities that have the capacity to enroll them without displacing in-state students. This is especially true if universities wish to fund higher education for low-income students (Cook and Turner, 2022) but are unable to to because of constraints, such as caps or freezes, on tuition levels (Miller and Park, 2022).

A clear policy implication from this study is that universities should reconsider limits on out-of-state enrollment, especially for institutions facing decreases in state support for higher education. One limitation of this study is that we cannot observe these outcomes in a scenario where limits on out-of-state enrollment are removed from each university. While previous research has found that this would be efficient from a national perspective (Knight and Schiff, 2019), public universities exist for the benefit of individual states and treatment effects may look different under a system where limits on OSS are removed entirely.

Another concern is that universities might not have capacity for new OSS. Yet we find that universities comparable to WM have the average capacity to enroll between 1,500 and 3,100 additional students—possibly due to declining domestic enrollment in the U.S. over the past decade. While our results are limited to those institutions with excess capacity, the evidence suggests this concern does not restrict the generalizability of our results when considering these universities.

A further limitation of this study is that it focused on a selective public university in the Midwest. It may be the case that demand for university admission at the University of Wisconsin, Madison is higher than most other public universities both in the Midwest and other regions. While increasing out-of-state enrollment is likely to increase revenue, as out-of-state students are often charged more than in-state students, it is unclear whether this increase in out-of-state students would impact academic quality at other universities. Therefore, we caution our results against extrapolation to other settings. This warrants further research to estimate impacts on universities with different characteristics.

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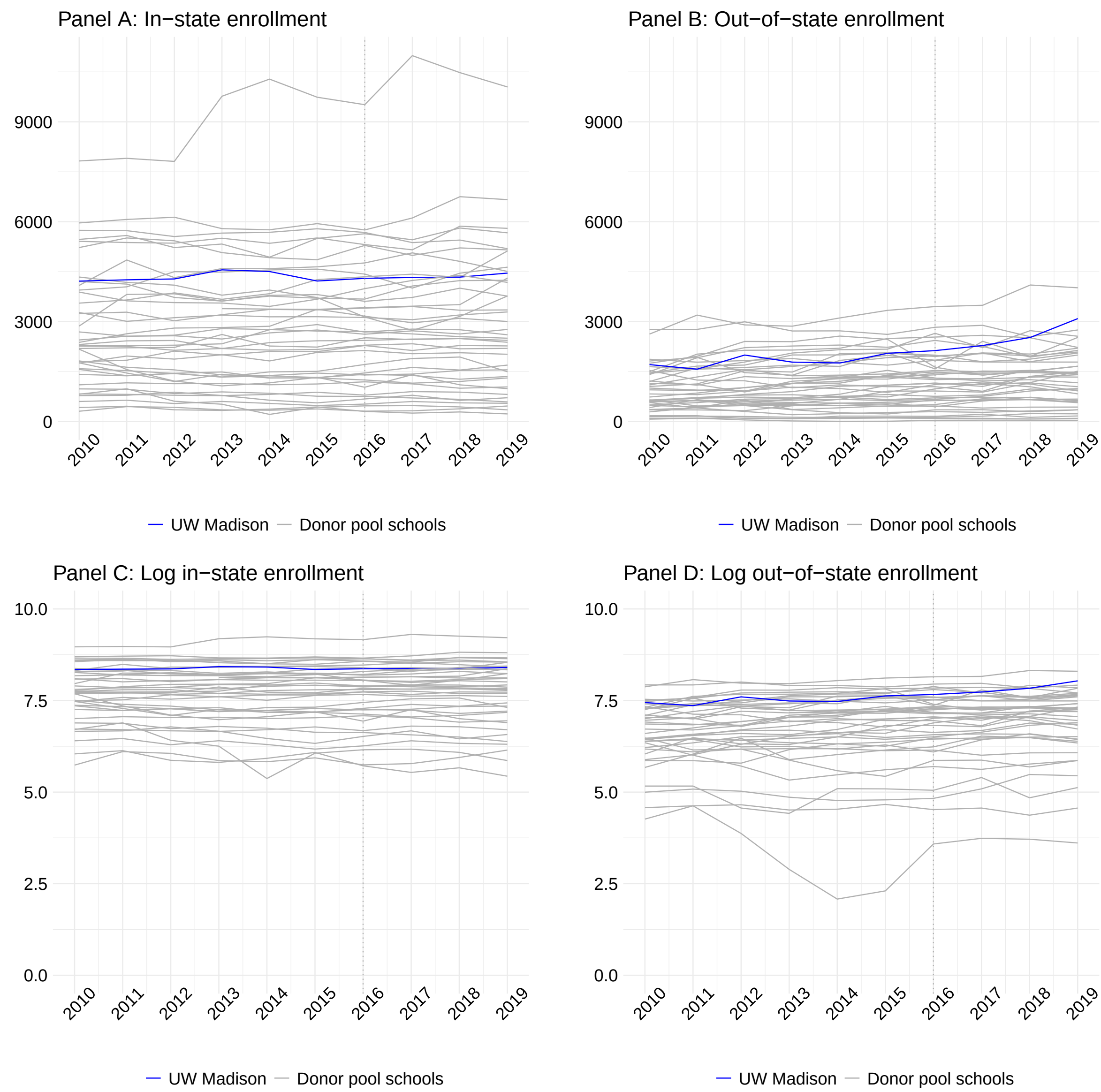
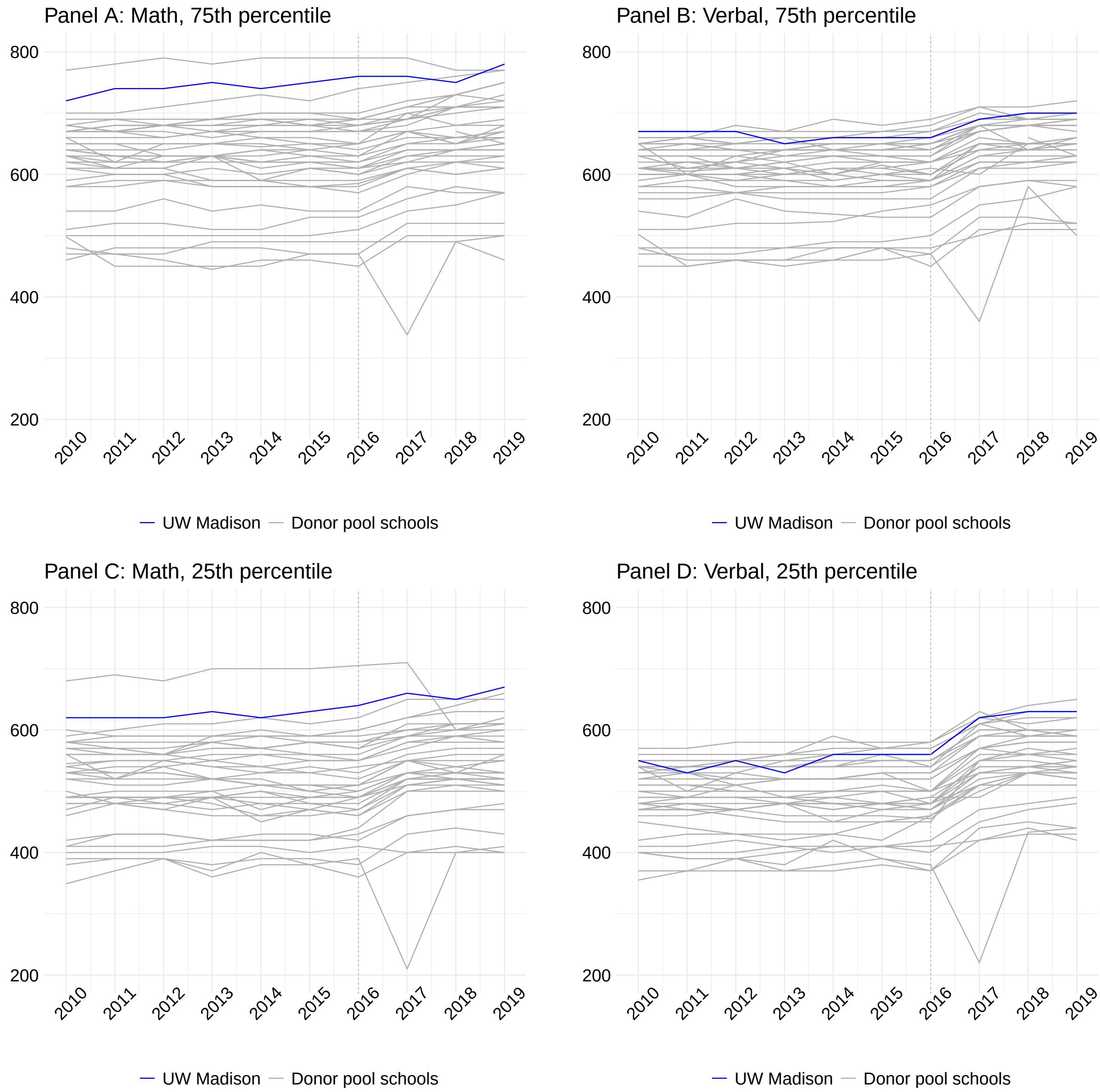
Graphs and Tables

Figure 1: In- and Out-of-State Freshman Enrollment



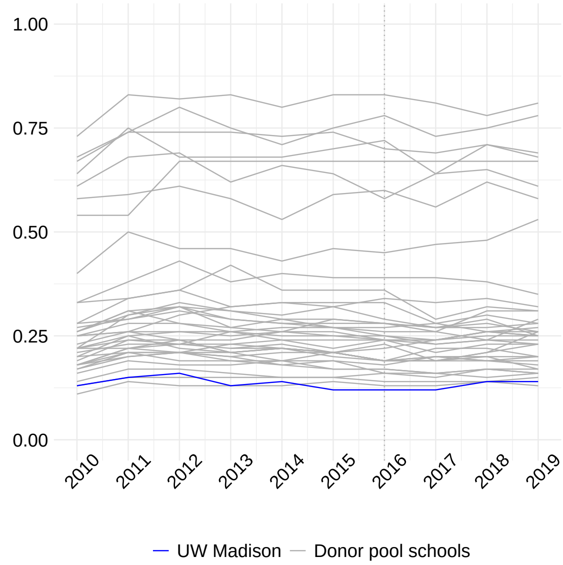
Note: The dotted vertical line shows 2016, the first year of treatment. Values calculated using data from IPEDS. The primary donor pool consists of the 39 land-grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010–2019.

Figure 2: Freshman SAT Scores, by Percentile



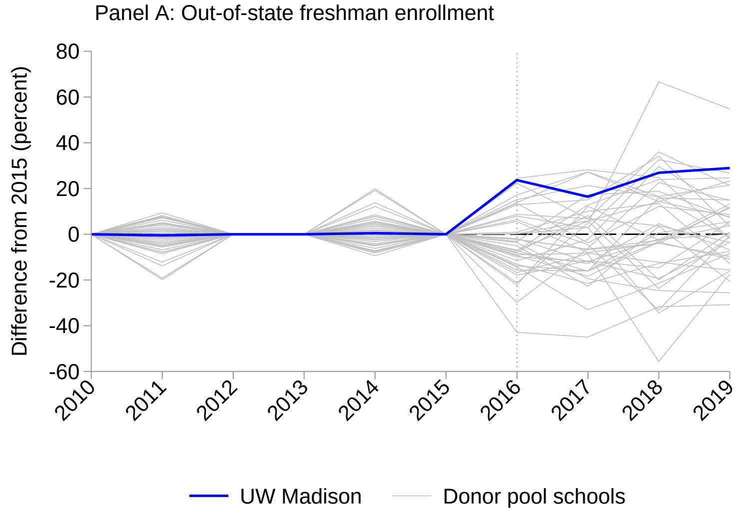
Note: The dotted vertical line shows 2016, the first year of treatment. Values calculated using data from IPEDS. The primary donor pool consists of the 39 land-grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010–2019. SAT score data is missing for some donor pool schools as not all require students to submit these scores upon application. Score trends between 2016 and 2017 should be interpreted with caution due to the structural changes to the exam starting in March 2016.

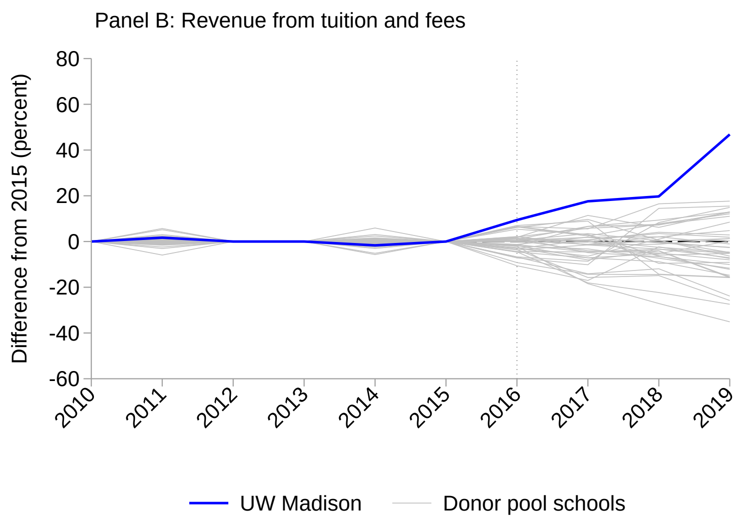
Figure 3: Share of Freshmen Receiving Pell Grants



Note: The dotted vertical line shows 2016, the first year of treatment. Values calculated using data from IPEDS. The primary donor pool consists of the 39 land-grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010–2019.

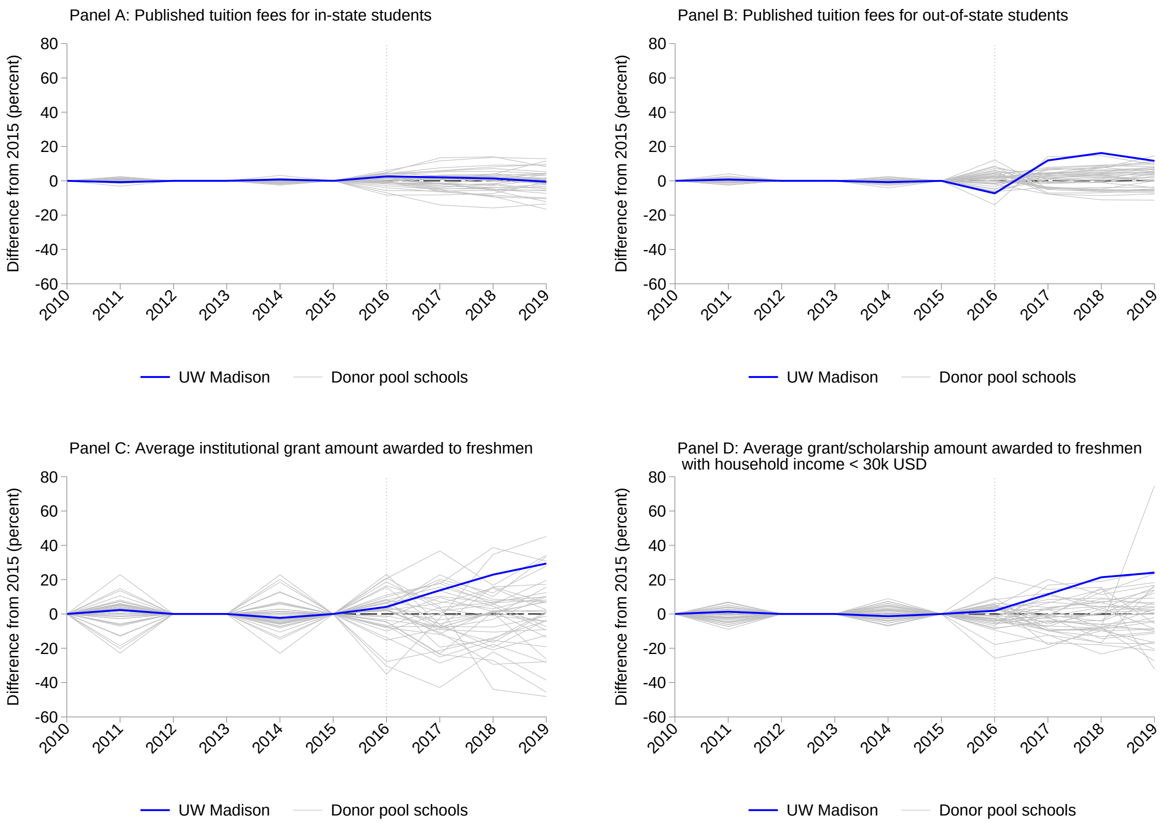
Figure 4: First Stage Results





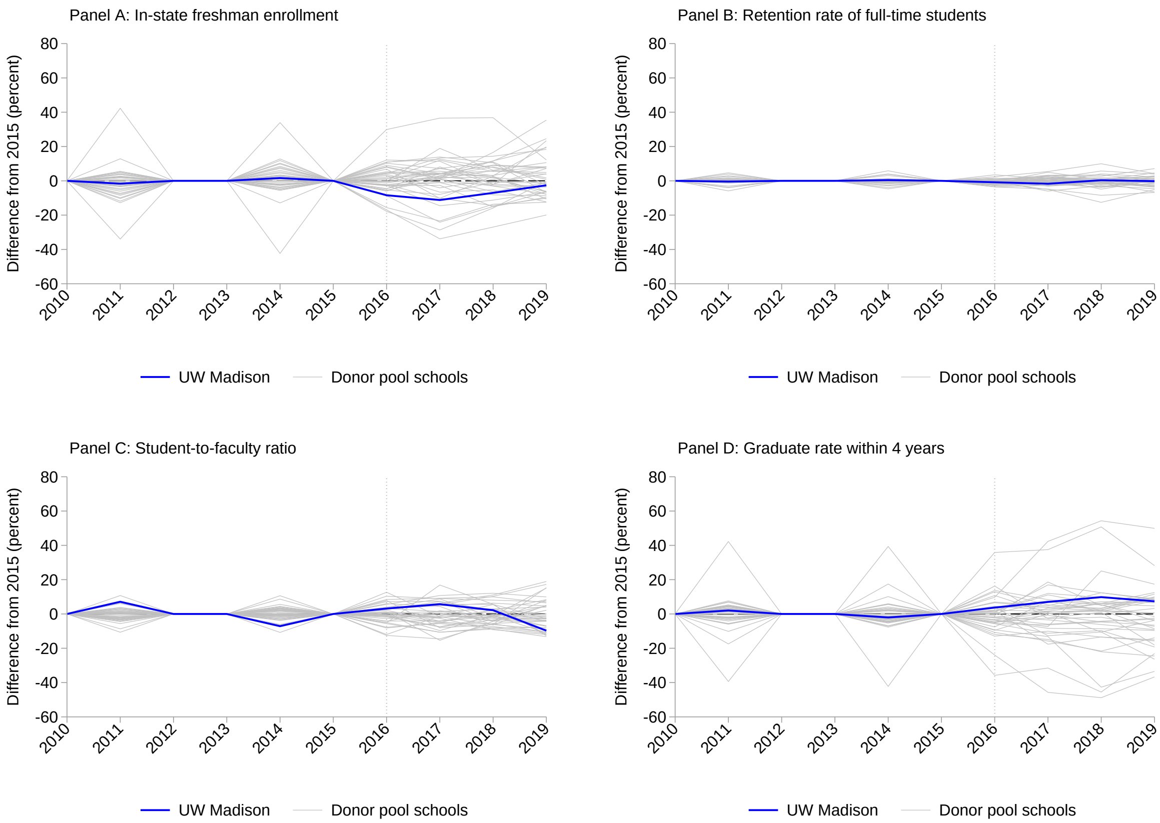
Note: Estimated effects using data from IPEDS. The primary donor pool consists of the 39 land-grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010–2019. The y-axis shows the percent difference between the outcome value and the associated estimated synthetic control, both normalized to 2015. The solid blue line shows the estimated effect at the University of Wisconsin, Madison. The dark grey lines show the 39 placebo treatment effects, estimated by permuting treatment "in space", across the donor pool universities, then taking the difference between the outcome values of the placebo treated unit and those of its synthetic control. The vertical dotted line shows 2016, the first year of treatment. The results are corrected for bias from matching discrepancies.

Figure 5: Financial Outcomes Results



Note: Estimated effects using data from IPEDS. The primary donor pool consists of the 39 land-grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010–2019. The y-axis shows the percent difference between the outcome value and the associated estimated synthetic control, both normalized to 2015. The solid blue line shows the estimated effect at the University of Wisconsin, Madison. The dark grey lines show the 39 placebo treatment effects, estimated by permuting treatment "in space", across the donor pool universities, then taking the difference between the outcome values of the placebo treated unit and those of its synthetic control. The vertical dotted line shows 2016, the first year of treatment. The results are corrected for bias from matching discrepancies.

Figure 6: Academic Outcomes Results



Note: Estimated effects using data from IPEDS. The primary donor pool consists of the 39 land-grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010–2019. The y-axis shows the percent difference between the outcome value and the associated estimated synthetic control, both normalized to 2015. The solid blue line shows the estimated effect at the University of Wisconsin, Madison. The dark grey lines show the 39 placebo treatment effects, estimated by permuting treatment "in space", across the donor pool universities, then taking the difference between the outcome values of the placebo treated unit and those of its synthetic control. The vertical dotted line shows 2016, the first year of treatment. The results are corrected for bias from matching discrepancies.

Table 1: Main results

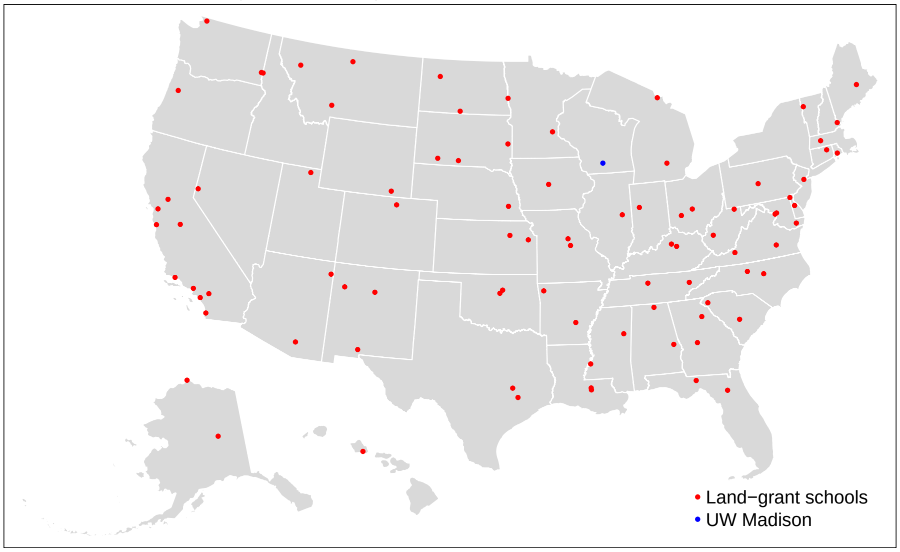
	Estimates
1. First Stage Outcomes	
Out-of-state Freshman Enrollment	
Treatment Effect (%)	28.945
Ranked-RMSPE-based p-value*	0.050
Revenue from Tuition and Fees	
Treatment Effect (%)	46.820
Ranked-RMSPE-based p-value	0.075
2. Financial Outcomes	
Published In-state Tuition Fees	
Treatment Effect (%)	-0.521
Ranked-RMSPE-based p-value	0.775
Published Out-of-state Tuition Fees	
Treatment Effect (%)	11.636
Ranked-RMSPE-based p-value	0.250
Average Institutional Grant Awarded	
Treatment Effect (%)	29.407
Ranked-RMSPE-based p-value*	0.075
Average Financial Aid Awarded to Students from Households Earning $< \$30k$	
Treatment Effect (%)	24.063
Ranked-RMSPE-based p-value*	0.100
3. Academic Outcomes	
In-state Freshman Enrollment	
Treatment Effect (%)	-2.584
Ranked-RMSPE-based p-value	0.275
Full-time Retention Rate	
Treatment Effect (%)	-0.192
Ranked-RMSPE-based p-value	0.450
Student-to-faculty Ratio	
Treatment Effect (%)	-9.737
Ranked-RMSPE-based p-value	0.850
4-year Graduation Rate	
Treatment Effect (%)	7.379
Ranked-RMSPE-based p-value	0.450
N	40

Note: Estimated effects in 2019 using data from IPEDS, with the set of control universities restricted to those with a complete panel for the full set of covariates. Section 1 contains first stage outcomes. Section 2 contains financial outcomes. Section 3 contains academic outcomes. Column (1) presents our preferred estimates—the bias-corrected synthetic control estimates using the full set of covariates. For each outcome, Row (1) presents estimated treatment effects and Row (2) presents p-values from ranking the RMSPEs of the empirical distribution of in-space placebo treatment effects through 2019 (for the synthetic control estimates). p-values marked with a * are one-sided; the remainder are two-sided.

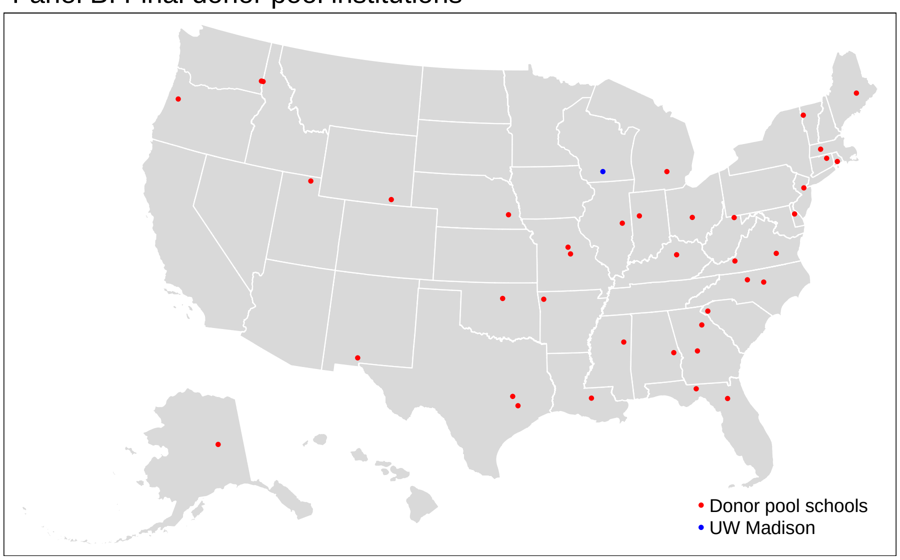
Supplementary Figures

Figure A.1: Institution Locations (Primary Sample)

Panel A: Public 4−year land−grant institutions

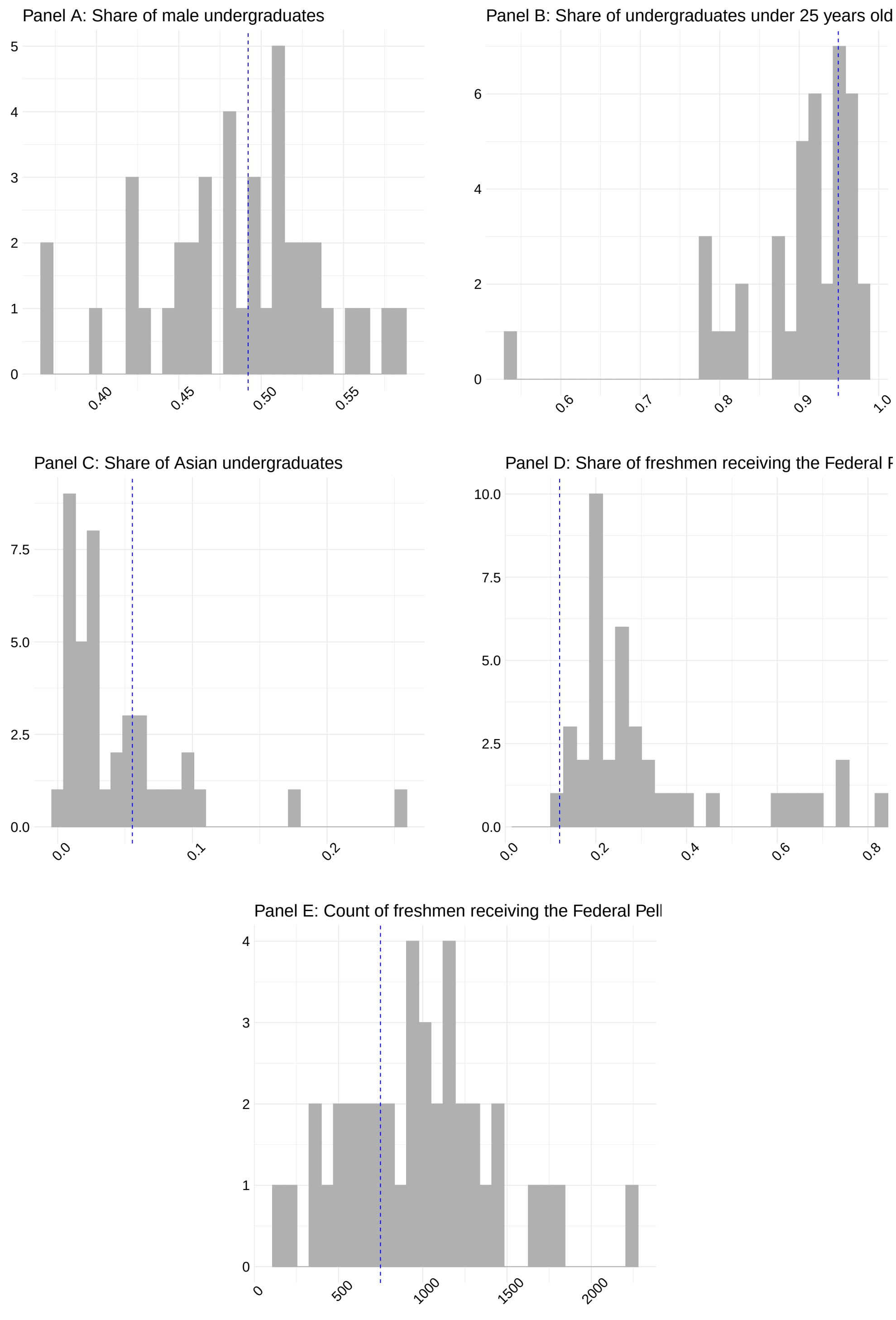


Panel B: Final donor pool institutions



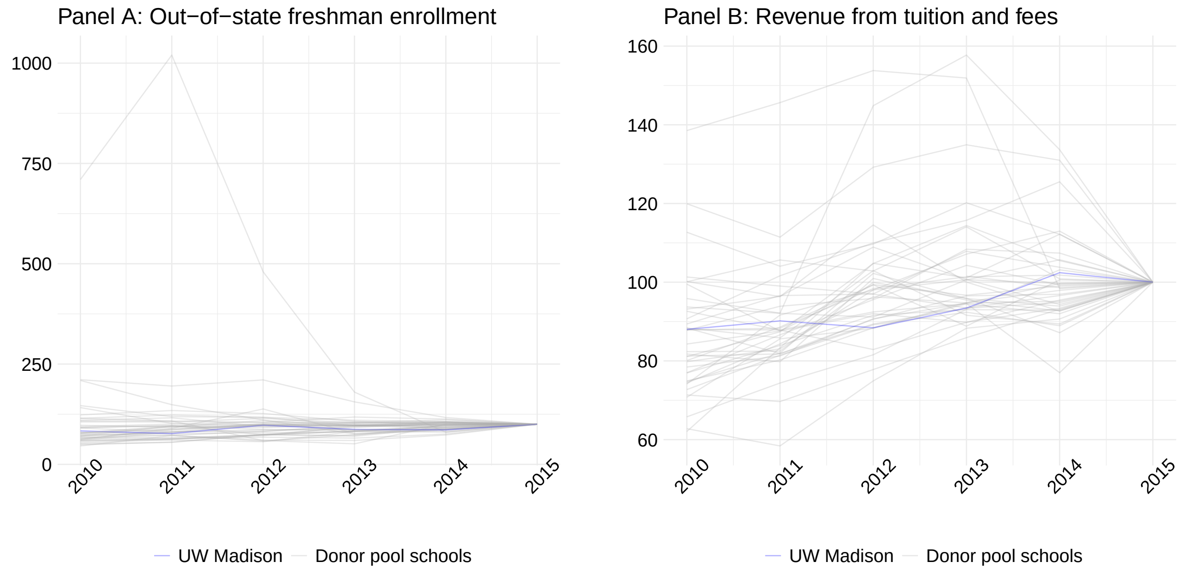
Note: Institution locations as reported in IPEDS. The primary donor pool consists of the 39 land-grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010–2019.

Figure A.2: Control Variables Summary Statistics, 2015



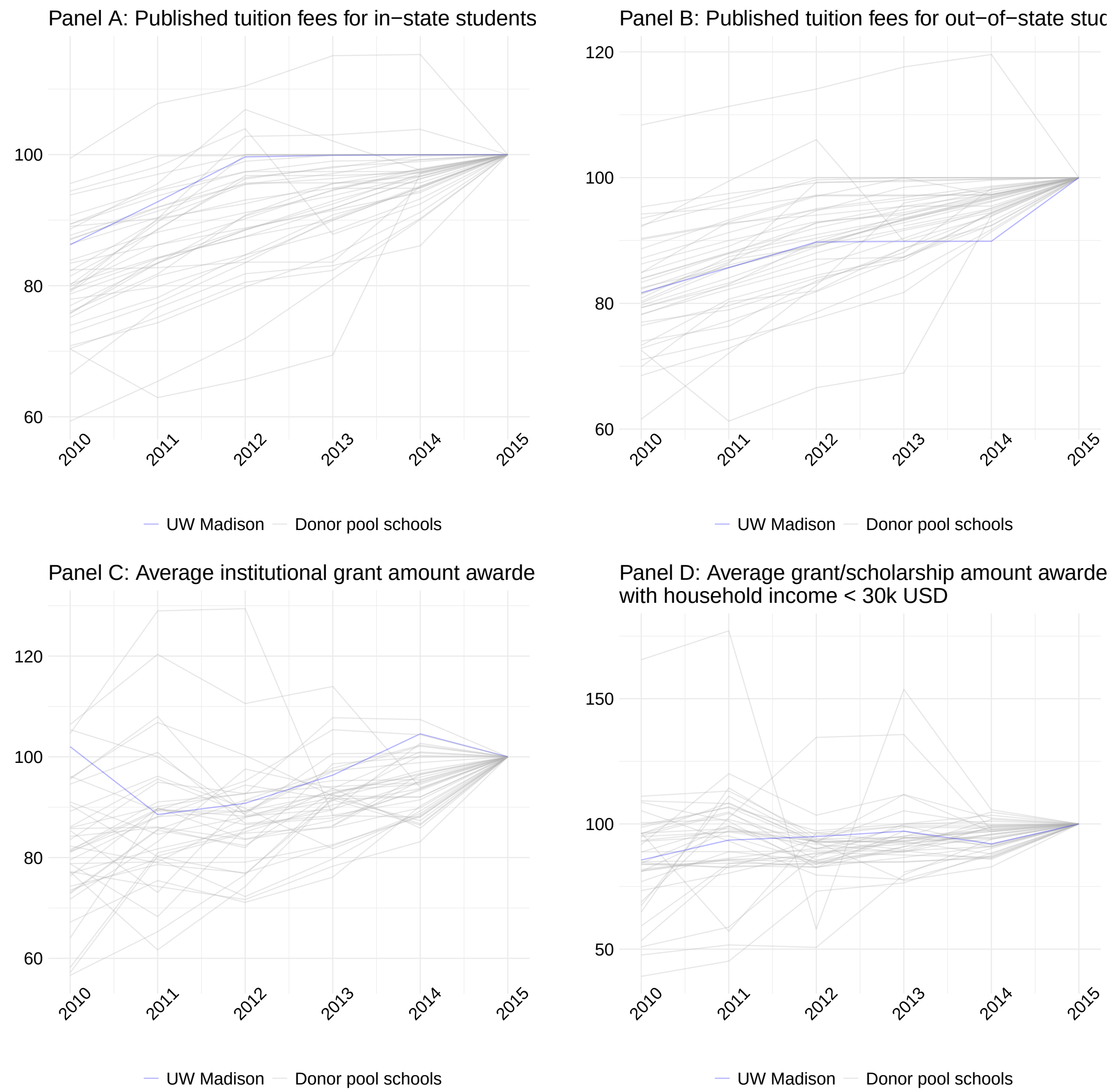
Note: Values calculated using data from IPEDS. The dashed blue line shows the value for the University of Wisconsin, Madison. The dark grey bars show the distribution for the primary donor pool institutions. The primary donor pool consists of the 39 land-grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010–2019.

Figure A.3: First Stage Summary Statistics



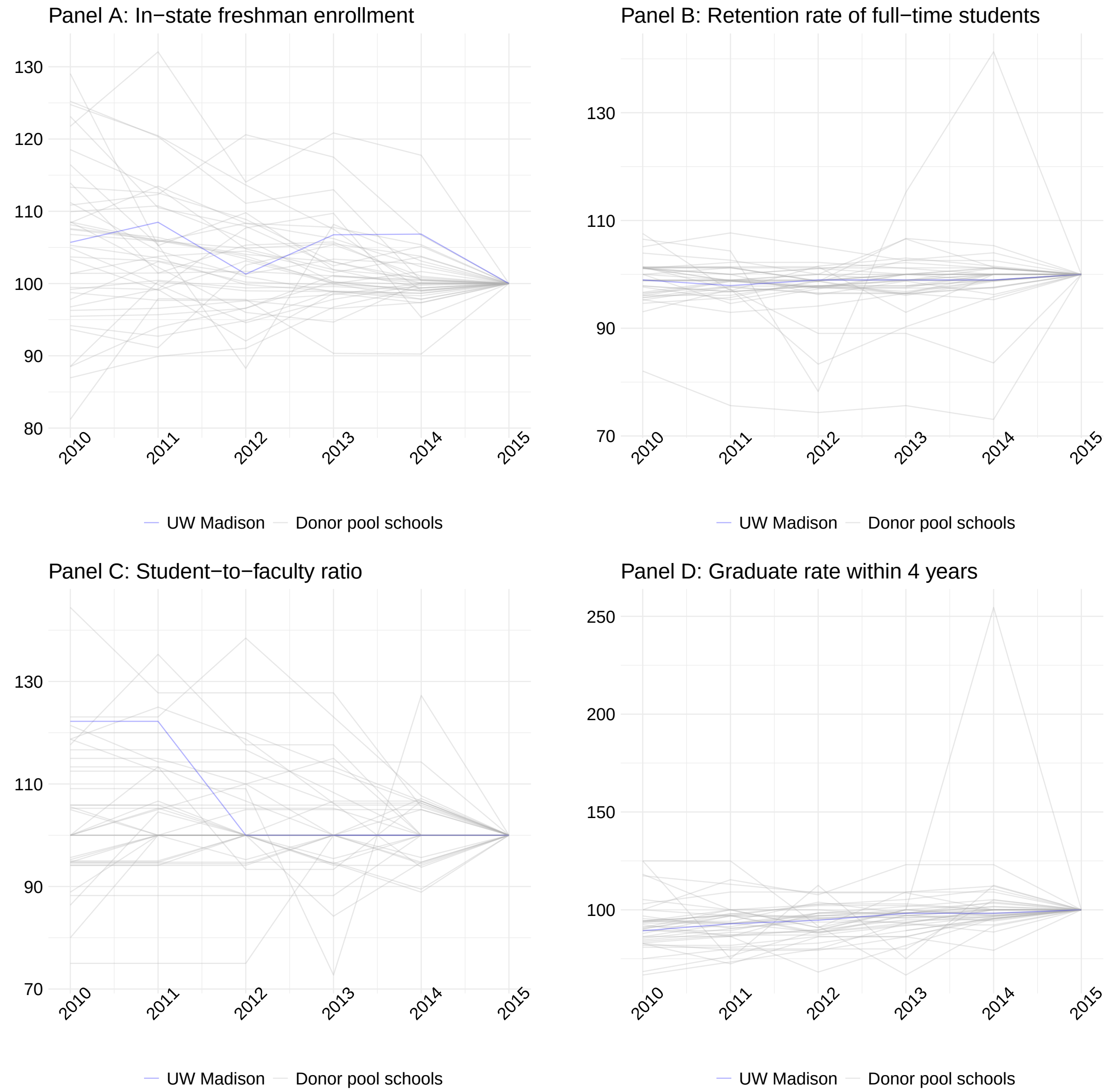
Note: Values calculated using data from IPEDS and normalized to 100 in 2015. The primary donor pool consists of the 39 land-grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010–2019.

Figure A.4: Financial Outcomes Summary Statistics



Note: Values calculated using data from IPEDS and normalized to 100 in 2015. The primary donor pool consists of the 39 land-grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010–2019.

Figure A.5: Academic Outcomes Summary Statistics



Note: Values calculated using data from IPEDS and normalized to 100 in 2015. The primary donor pool consists of the 39 land-grant universities outside of Wisconsin and Minnesota for which we have observations for all outcome variables and covariates from 2010–2019.

Table A.1 Primary Control Group/Donor Pool Institutions

Auburn University	Clemson University
Delaware State University	Florida A & M University
Fort Valley State University	Lincoln University
Louisiana State University and A&M	Michigan State University
Mississippi State University	New Mexico State University-Main Campus
North Carolina A & T State University	North Carolina State University at Raleigh
Ohio State University-Main Campus	Oklahoma State University-Main Campus
Oregon State University	Prairie View A & M University
Purdue University-Main Campus	Rutgers University-New Brunswick
Texas A & M University-College Station	University of Alaska Fairbanks
University of Arkansas	University of Connecticut
University of Florida	University of Georgia
University of Idaho	University of Illinois at Urbana-Champaign
University of Kentucky	University of Maine
University of Massachusetts-Amherst	University of Missouri-Columbia
University of Nebraska-Lincoln	University of Rhode Island
University of Vermont	University of Wyoming
Utah State University	Virginia Polytechnic Institute and State University
Virginia State University	Washington State University
West Virginia University	

Note: List of institutions in our primary control group/donor pool. Includes land grant universities outside Wisconsin and Minnesota that have observations for each of our covariates from 2010–2019 in IPEDS.

Table A.2 First Stage (robustness checks)

Outcome	Synthetic Control (1)	Synthetic Control (2)	Synthetic Control (3)	Synthetic Control (4)	TWFE (5)	Synthetic DiD (6)
Out-of-state Freshman Enrollment						
Treatment Effect (%)	30.601	26.894	31.001	49.829	44.800	39.810
Ranked-RMSPE-based *p*-value	0.025	0.075	0.362	0.011		
Asymptotic *p*-value					0.000	
Placebo-variance-based *p*-value						0.034
Revenue from Tuition and Fees						
Treatment Effect (%)	45.177	19.722	52.563	43.810	38.575	45.974
Ranked-RMSPE-based *p*-value	0.050	0.075	0.072	0.058		
Asymptotic *p*-value					0.000	
Placebo-variance-based *p*-value						0.000
Covariates	Restricted	All	All	All	All*	All*
Year of estimate	2019	2018	2019	2019	2019	2019
Control universities	Land grant	Land grant	R1+R2	Pub. 4-yr	Land grant	Land grant
N	40	40	69	190	40	40

Note: Estimated effects using data from IPEDS. Column (1) presents the bias-corrected synthetic control estimates for 2019 using the restricted set of covariates, with the set of control universities restricted to land grant institutions with a complete panel for the full set of covariates (our "preferred" sample of control universities). Column (2) presents the bias-corrected synthetic control estimates for 2018 using the full set of covariates and the preferred sample of controls, Column (3) presents the bias-corrected synthetic control estimates for 2019, with the set of control universities restricted to R1 and R2 institutions with a complete panel for the full set of covariates. Column (4) presents the bias-corrected synthetic control estimates for 2019, with the set of control universities restricted to public, four-year-degree-granting institutions with a complete panel for the full set of covariates. Column (5) presents the TWFE estimates for 2019 using the preferred sample of controls, and Column (6) presents the synthetic difference-in-differences estimates for 2019 using the preferred sample of controls. For each outcome, Row (1) presents estimated treatment effects, Row (2) presents *p*-values from ranking the RMSPEs of the empirical distribution of in-space placebo treatment effects through the year of estimation (for the synthetic control estimates), Row (3) presents the large-sample asymptotic *p*-values resulting from the event study estimate for 2019 with standard errors clustered by state (for the TWFE estimates), and Row (4) presents estimated *p*-values from the variance of the empirical distribution of in-space placebo treatment effects in 2019 (for the SDiD estimates). All *p*-values for *Out-of-state Freshman Enrollment*, *Domestic Out-of-state Freshman Enrollment* and *Revenue from Tuition and Fees* are one-sided. * Including the synthetic control covariates makes no difference to the TWFE or SDiD estimated coefficients of interest, as the covariates are pre-treatment averages observed in each institution and are effectively controlled for by the unit fixed effects. This selection of covariates maintains consistency with the synthetic control specifications and also ensures there is no bias from using "bad controls".

Table A.3 Financial Outcomes (robustness checks)

Outcome	Synthetic Control (1)	Synthetic Control (2)	Synthetic Control (3)	Synthetic Control (4)	TWFE (5)	Synthetic DiD (6)
Published In-state Tuition Fees						
Treatment Effect (%)	-2.396	1.383	-3.811	0.132	-13.323	-5.288
Ranked-RMSPE-based p-value	0.450	0.675	0.551	0.695		
Asymptotic p-value					0.000	
Placebo-variance-based p-value						0.298
Published Out-of-state Tuition Fees						
Treatment Effect (%)	8.747	16.217	2.094	10.216	16.325	10.377
Ranked-RMSPE-based p-value	0.525	0.225	0.942	0.811		
Asymptotic p-value					0.000	
Placebo-variance-based p-value						0.060
Average Institutional Grant Awarded						
Treatment Effect (%)	26.121	22.931	22.528	29.262	31.441	30.287
Ranked-RMSPE-based p-value	0.025	0.100	0.246	0.016		
Asymptotic p-value					0.000	
Placebo-variance-based p-value						0.067
Average Fin. Aid Awarded to Students from Households Earning $< \$30k$						
Treatment Effect (%)	33.470	21.382	19.983	28.071	14.405	17.438
Ranked-RMSPE-based p-value	0.125	0.100	0.072	0.121		
Asymptotic p-value					0.001	
Placebo-variance-based p-value						0.292
Covariates	Restricted	All	All	All	All*	All*
Year of estimate	2019	2018	2019	2019	2019	2019
Control universities	Land grant	Land grant	R1+R2	Pub. 4-yr	Land grant	Land grant
N	40	40	69	190	40	40

Note: Estimated effects using data from IPEDS. Column (1) presents the bias-corrected synthetic control estimates for 2019 using the restricted set of covariates, with the set of control universities restricted to land grant institutions with a complete panel for the full set of covariates (our "preferred" sample of control universities). Column (2) presents the bias-corrected synthetic control estimates for 2018 using the full set of covariates and the preferred sample of controls, Column (3) presents the bias-corrected synthetic control estimates for 2019, with the set of control universities restricted to R1 and R2 institutions with a complete panel for the full set of covariates. Column (4) presents the bias-corrected synthetic control estimates for 2019, with the set of control universities restricted to public, four-year-degree-granting institutions with a complete panel for the full set of covariates. Column (5) presents the TWFE estimates for 2019 using the preferred sample of controls, and Column (6) presents the synthetic difference-in-differences estimates for 2019 using the preferred sample of controls. For each outcome, Row (1) presents estimated treatment effects, Row (2) presents p-values from ranking the RMSPEs of the empirical distribution of in-space placebo treatment effects through the year of estimation (for the synthetic control estimates), Row (3) presents the large-sample asymptotic p-values resulting from the event study estimate for 2019 with standard errors clustered by state (for the TWFE estimates), and Row (4) presents estimated p-values from the variance of the empirical distribution of in-space placebo treatment effects in 2019 (for the SDiD estimates). All p-values for *Average Institutional Grant Awarded* and *Average Financial Aid Awarded to Students from Households earnings* $< \$30k$ are one-sided. All p-values for *In-state Tuition Fees* and *Out-of-state Tuition Fees* are two-sided. * Including the synthetic control covariates makes no difference to the TWFE or SDiD estimated coefficients of interest, as the covariates are pre-treatment averages observed in each institution and are effectively controlled for by the unit fixed effects. This selection of covariates maintains consistency with the synthetic control specifications and also ensures there is no bias from using "bad controls".

Table A.4 Academic Outcomes (robustness checks)

Outcome	Synthetic Control (1)	Synthetic Control (2)	Synthetic Control (3)	Synthetic Control (4)	TWFE (5)	Synthetic DiD (6)
In-state Freshman Enrollment						
Treatment Effect (%)	-2.007	-7.030	1.347	-2.721	2.475	-1.203
Ranked-RMSPE-based p-value	0.525	0.200	0.884	0.826		
Asymptotic p-value					0.439	
Placebo-variance-based p-value						0.936
Full-time Retention Rate						
Treatment Effect (%)	-0.415	0.365	-0.750	-1.350	-1.442	-1.005
Ranked-RMSPE-based p-value	0.275	0.425	0.348	0.332		
Asymptotic p-value					0.031	
Placebo-variance-based p-value						0.645
Student-to-faculty Ratio						
Treatment Effect (%)	-13.076	2.189	1.959	0.854	-7.663	-10.344
Ranked-RMSPE-based p-value	0.750	0.900	0.870	1.000		
Asymptotic p-value					0.000	
Placebo-variance-based p-value						0.180
4-year Graduation Rate						
Treatment Effect (%)	8.316	9.830	3.766	2.289	-4.960	-6.251
Ranked-RMSPE-based p-value	0.150	0.375	0.609	0.789		
Asymptotic p-value					0.211	
Placebo-variance-based p-value						0.839
Covariates	Restricted	All	All	All	All*	All*
Year of estimate	2019	2018	2019	2019	2019	2019
Control universities	Land grant	Land grant	R1+R2	Pub. 4-yr	Land grant	Land grant
N	40	40	69	190	40	40

Note: Estimated effects using data from IPEDS. Column (1) presents the bias-corrected synthetic control estimates for 2019 using the restricted set of covariates, with the set of control universities restricted to land grant institutions with a complete panel for the full set of covariates (our "preferred" sample of control universities). Column (2) presents the bias-corrected synthetic control estimates for 2018 using the full set of covariates and the preferred sample of controls, Column (3) presents the bias-corrected synthetic control estimates for 2019, with the set of control universities restricted to R1 and R2 institutions with a complete panel for the full set of covariates. Column (4) presents the bias-corrected synthetic control estimates for 2019, with the set of control universities restricted to public, four-year-degree-granting institutions with a complete panel for the full set of covariates. Column (5) presents the TWFE estimates for 2019 using the preferred sample of controls, and Column (6) presents the synthetic difference-in-differences estimates for 2019 using the preferred sample of controls. For each outcome, Row (1) presents estimated treatment effects, Row (2) presents p-values from ranking the RMSPEs of the empirical distribution of in-space placebo treatment effects through the year of estimation (for the synthetic control estimates), Row (3) presents the large-sample asymptotic p-values resulting from the event study estimate for 2019 with standard errors clustered by state (for the TWFE estimates), and Row (4) presents estimated p-values from the variance of the empirical distribution of in-space placebo treatment effects in 2019 (for the SDiD estimates). All p-values for *In-state Freshman Enrollment*, *Full-time Retention Rate*, *Student-to-faculty Ratio*, and *4-year Graduation Rate* are two-sided. * Including the synthetic control covariates makes no difference to the TWFE or SDiD estimated coefficients of interest, as the covariates are pre-treatment averages observed in each institution and are effectively controlled for by the unit fixed effects. This selection of covariates maintains consistency with the synthetic control specifications and also ensures there is no bias from using "bad controls".

The Unintended Consequences of Logging Restrictions in the Northern Spotted Owl Habitat

Natalia Orlova† Justin C. Wiltshire‡ Derek Rury§

Abstract

We consider the early-1990s land protections covering tens of millions of acres of old-growth forest in the Northern Spotted Owl habitat in the U.S. Pacific Northwest and Northern California. In the intervening period, wildfire regimes in this region have become significantly more frequent, larger, and more severe. We find that these restrictions on timber harvesting lead to two outcomes. First, they caused an increase in the share of low-intensity wildfire ignitions by enhancing the natural shady and cool conditions of old-growth forests and their extensive tree canopies. At the same time, they ultimately greatly increased areas of wildfire perimeters that burned at high-severity in the protected forests—almost certainly because the logging restrictions encouraged accumulation of vegetation fuels. Severe wildfires often greatly harm affected ecosystems, and impose substantial economic costs on humans. We argue that qualified logging operations could serve a beneficial, complementary role to prescribed burns in forest management plans that aim to reduce wildfire severity.

JEL Codes: Q23, Q28, Q57

Key words: land management, forestry, wildfire

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1 Introduction

Land protections have long been advocated by environmental activists as necessary for ensuring the preservation of threatened ecosystems. Perhaps nowhere were these efforts so prominent or so contentious as in the Pacific Northwest and Northern California regions of the United States in the early 1990s. The preservation of the ecosystems in these protected areas—particularly the habitat of the Northern Spotted Owl (NSO), widely considered an "indicator species" for the health of these ecosystems—prompted court-imposed logging restrictions on millions of acres of forest in 1990. It was the primary motivation behind the 1990 listing of the NSO as threatened under the Endangered Species Act in 1990, the 1992 Recovery Plan for the Northern Spotted Owl, and the 1994 Northwest Forest Plan (NWFP), which further enshrined and extended these land protections to cover tens of millions of acres containing old-growth forests on federally-managed lands.

While the listing of the NSO and the subsequent land protections were successful in greatly reducing timber harvesting in old-growth forests (contributing to a decline in related employment, as in Ferris and Frank (2021)), they have been less successful in preserving many of the target species, including the Northern Spotted Owl (Spies et al., 2019), the population of which declined by nearly 62% on federal lands between 1993 and 2017 (Davis et al., 2022b). While some studies have suggested this is largely attributable to increased competition with Barred owls (Davis et al., 2022b; Spies et al., 2019; Thomas et al., 2006), others suggest that a dramatic increase in wildfire extent and severity may also play a critical role (Clark, Anthony, and Andrews, 2011; Ganey et al., 2017), with high-severity fires posing an especial threat to spotted owl populations through their impact on forest canopy cover (Durboraw et al., 2022). More generally, while the deleterious impact of wildfires on old-growth forests in protected areas has been in line with predictions, official monitoring programs note that an increase in the extent and frequency of wildfires pose a concerning threat to these forests and the ecosystems they nurture (Davis et al., 2022a).

Not all wildfires pose a great threat. Some are even desirable (Haugo et al., 2019; Pausas and Keeley, 2019) and are intentionally allowed to spread by firefighters for forest management practices.

Fire behavior is described by a number of factors, such as how it ignites, how hot it burns, its rate of spread, and the fuel loads that sustain it. Post-containment, wildfire damage can be estimated based on aerial photos. Extreme wildfires rank high on these measures and are dangerous, unpredictable, and highly damaging to their surrounding ecosystems. In this paper, we investigate whether forest protection policies alter extreme fire behavior through changing fuels and forest weather (temperature, wind patterns, and moisture) by focusing on wildfire intensity and burn severity. Formally, *intensity* is a measure of energy released during various phases of a fire, and *burn severity* is the change in organic matter above or below the ground and a measure of site disruption (Eidenshink et al., 2007; Keeley, 2009). Less formally, intensity tells us how hot a fire burns while severity captures post-fire damage. In other words, burn severity is a consequence of fire intensity. We examine the impact of the 1990 listing of the NSO as threatened and the logging bans that it triggered on wildfire ignition intensity and burn severity over the next 30 years. We find that these wide-reaching land regulations were responsible for reducing the share of ignition points which were high-intensity, but conversely also caused wildfires in the protected areas to become more severe.

The ecology literature broadly agrees that wildfire extent and severity in the western United States have both increased in recent decades (Cansler and McKenzie, 2014; Ganey et al., 2017; Stephens and Ruth, 2005; Wasserman and Mueller, 2023),[1]. Haugo et al. (2019) argue that this recent higher frequency of wildfires may not be unusual compared to pre-European settlement historic norms, but nonetheless demonstrate that the severity distribution has changed dramatically, with 36% of recent dry forest fires burning with high severity compared to a historic norm below 10%.

While the literature broadly agrees that mild and moderate fires can have both beneficial and detrimental ecological impacts, there is less agreement on the impact of severe fires. Some authors argue that severe wildfire plays a maintenance role that has been and remains necessary for some flora and fauna to flourish (e.g. Baker (2015); Bond et al. (2012); Hutto et al. (2016)); yet most agree that uncharacteristically severe fires pose a threat to vegetation distribution and forest con-

[1] Baker (2015) is a notable exception to this consensus.

ditions within affected ecosystems (e.g. Ganey et al. (2017); Haugo et al. (2019); Stephens and Ruth (2005); Wasserman and Mueller (2023)), and generally advocate for policies—including fuel reduction—that will reduce the share and/or frequency of high-severity wildfires, in line with the objectives of the Collaborative Forest Landscape Restoration Program established by Congress in 2009 (Schultz, Jedd, and Beam, 2012). While the impact of wildfires on Northern Spotted Owl populations remains uncertain, researchers in this area generally conclude it is likely that severe fires in mature and older forests have contributed to the continuing decline of the NSO population (Clark, Anthony, and Andrews, 2011; Ganey et al., 2017)—a perhaps-tragic irony, given our own findings on the impact of this policy purportedly intended to arrest the decline of NSO populations.

One thing that is near-universally agreed upon in this literature is that highly-severe fires pose a substantial threat and cost to humans, including through property risk, risk to human life, and increased suppression costs (also see Mattioli et al. (2022) and National Interagency Fire Center (2023)). The economics literature concurs, broadly finding negative consequences of nearby wildfires, including increased health costs (Moeltner et al., 2013), reduced output (Bayham et al., 2022; Meier, Elliott, and Strobl, 2023), and lower house prices in the years after a nearby fire (Boustan et al., 2020; Kiel and Matheson, 2018; Mueller, Loomis, and González-Cabán, 2009).

For all these reasons, our results suggest that forest protections associated with the listing of the NSO as threatened may have not only failed to achieve many of their stated goals—among others, to protect especially the oldest-growth forests and wildlife including the NSO, and to provide a predictable and sustainable timber harvest (Davis et al., 2022b; Spies et al., 2019)—but they may perversely have exacerbated several of the issues they were intended to address, at substantial indirect cost to humans.

In addition to documenting general adverse effects of land protection policies and wildlife conservation efforts on wildfire behavior, our research contributes to another, more targeted line of literature—the role of forest management activities, namely logging, in mitigating the costs and damages of wildfires. Fuels reduction practices, such as thinning and prescribed burning, are documented to have positive effects on forest resilience and prevention of extreme wildfires. However, they are frequently riddled with logistical, financial, and sometimes political hurdles, which results in these tools being severely underemployed (Mathews, 2020). Logging is a tempting alternative, or at least a complement, to fuels reduction activities due to its established history and profitability. However, there is no consensus on the direction of its effects on wildfires (DellaSala et al., 2022a; Hanson et al., 2009). This paper adds to that literature by providing evidence that logging operations can play a beneficial role in reducing reducing wildfire severity.

The remainder of our paper is structured as follows. Section 2 summarizes the policy environment around the time of the NSO listing. Section 3 goes into detail on data sources and sample construction. Section 4 describes the econometric model. Section 5 interprets the results. Section 6 concludes with a discussion of results.

2 Policy Environment and NSO Habitat Background

There are several reasons why state actors enact land protection policies, including the promotion of agriculture, human development, and conservation. Forests, in particular, are considered high-value targets for conservation efforts as they are often complex ecosystem that host significant biodiversity. Throughout the 18th, 19th and 20th centuries, in large part because of human interaction with forests, several species found in forests have either faced extinction or become extinct. Environmentalists have since argued that these species are worth protecting per se or that the survival and protection of those species is in the public good. The endangered species act was therefore signed into law by Richard Nixon in 1973. The act was designed to protect and conserve species facing dire threats to their survival. Over the years, many species have been placed on the endangered species list, prompting authorities to either halt or suspend economic activity, creating tension between conservation and commercial interests.

Perhaps the most famous case of this tension is the Northern Spotted Owl (NSO). There has been much controversy surrounding policies meant to protect the habitat of the NSO. The NSO habitat spans across three U.S. states, namely Washington, Oregon and California. Its population has decreased severely over the past several decades. One leading reason for this cause is that human logging is disruptive to its survival in old-growth forests, which represent a crucial habitat for the NSO. Accordingly, in the 1980s and 1990s the fate of the NSO became a famous, nation-wide dispute between those involved in the logging industry and the forests in which the logging took place.

To help protect NSO populations from further decline, environmental groups lobbied to protect NSO habitat from the timber industry. Following two lawsuits, the NSO was listed as threatened under the Endangered Species Act in 1990 (U.S. Fish and Wildlife Service, 1990). A district court subsequently restricted logging on 10 million acres within the NSO habitat. The first Recovery Plan for Northern Spotted Owls focused on providing adequate amounts of suitable forest cover to sustain this subspecies (Spies et al., 2018). Thus in 1992 6.9 million acres were designated as "critical habitat" by the U.S. Fish and Wildlife Service, further protecting the NSO and restricting logging. Shortly thereafter, in 1994, the Clinton administration developed the Northwest Forest Plan (NWFP, or plan) to further enshrine the NSO habitat as protected from human activities that could threaten its survival. We use the protection of the NSO and the restriction on logging that resulted as a natural experiment to study how this land-use policy impacted wildfire behavior on protected lands.

Panel (a) of Figure 1 presents the current NSO habitat range and its critical habitat areas. The entire habitat range boundary serves to identify lands where we suspect the NSO may be present. The critical habitat, or the lands formally listed under protectionist policies, appear to be evenly distributed within entire range. We suspect that reduced logging operations and conservationist mentality extended to the entire habitat range, so we use both of these regions in our analysis. Panels (b)-(d) of Figure 1 address the possibility of changing habitat boundaries over time, given that our analysis spans almost 30 years. We see that not much has changed since 1992.

A unique feature of the NSO habitat is its extensive coverage by mature and old-growth forests.

Figure 2 maps all such vegetation in the U.S. West. While exact definitions of old-growth forests differ slightly by region and researcher, the forests are generally characterized by complex multi-layer canopies, cooler temperatures, moist air and soil, and hardier moss-covered trees (Lesmeister et al., 2019; Wohlleben, 2015). Historically, old-growth forests in the U.S. West consisted of large, fire-resistant species growing at low densities. They were frequented by low-intensity fires, which prevented accumulations of fuels and reduced the probability of extreme wildfires. This natural mechanism was somewhat lost with the advent of increased fire suppression activities during the 20th century. As a result, fire-excluded, heavily ingrown, and denser old forests are no longer as resilient and low-maintenance as they once were (Mathews, 2020). Still, Lesmeister et al. (2019) find that even today old-growth forests tend to burn at a much lower severity than their younger counterparts.[2]

For many years prior and even during the mounting concerns about the effects of logging on old-forest ecosystems, timber harvesting was extensive on these lands (Spies et al., 2018). Panel (a) of Figure 3 plots volume of timber cut across all national forests in California, Oregon, and Washington for the years in our sample. We see that the listing of the NSO as threatened in 1990 marked an irreversible change in the logging industry in the U.S. West. There was an incredible drop in the amount of cut timber on these lands. Cut timber volume kept dropping dramatically throughout the subsequent few years of increased legal challenges to the timber industry that the NSO listing precipitated, eventually flattening out. Panel (b) of Figure 3 zooms in on the policy shift period. We split up the national forests into those affected by the listing and those lying outside of the NSO habitat range. Among the forests that house the NSO, there is again an abrupt decrease in cut timber starting in 1990. The control forests show signs of a smaller reduction of logging operations as well, likely due to spillover effects, but it is much more gradual.[3]

[2]The authors define severity based on the percentage of trees lost.

[3]The two groups in Figure 3 panel (b) do not add up to totals in panel (a). We exclude south California forests and national forests with significant amount of land both in and outside of the NSO habitat boundary. The omitted forests are Angeles, Cleveland, Los Padres, San Bernardino, Mendocino, Deschutes, Okanogan & Wenatchee National Forests.

3 Data and Descriptive Results

We focus on fires that burned in California, Oregon, or Washington, which are the states that are home to the NSO and were affected by the land protection policies. A variety of federal and state agencies are responsible for wildfire suppression and management across the Western U.S. We restrict our analysis to wildfires under the jurisdiction of the U.S. Forest Service (USFS).[4] We define treated wildfires as falling either within the general NSO habitat range or its critical habitat, depending on the specification. The two sources of wildfire data, described in more detail below, map wildfire ignition locations and entire fire perimeters. For ignition points, we require that they fall within the habitat boundaries to be considered treated. For perimeters, 80 percent of the fire area must lie within the habitat. For both treatment specifications, our control group is the same. Wildfires assigned into the control group burn on other federal national forest territories, therefore avoiding comparisons to private lands or to non-timberlands. There are a couple of forests that border the NSO habitat. To create a buffer between treated and control wildfires, we drop all control fires that burned within 10 miles of the NSO habitat border.[5] We further exclude 4 national forests in south California due to their lack of old-growth vegetation coverage (refer to Figure 2).[6]

The FIRESTAT Fire Occurrence data collects information on burn intensity (how hot a fire burns), ignition point locations, size, start date, and other attributes for all wildfires that started on USFS lands since the middle of the 20th century. There is an abrupt drop in the records of fire occurrence in 1985 and earlier, so we start our analysis in 1986. We also subset our sample to wildfires 10 acres or greater in size to reduce noise from extremely small fires, as these are very common in the West. From the attribute data, we obtain outcomes that measure total acreage burned by the fire and burn intensity around its ignition point. Ignition burn intensity is approximated by the average flame height observed at the head of the fire during the initial suppression. Following the USFS classification, we define ignitions as high intensity if their flame height was over 8 feet tall. We

[4]We do this both due to historic data limitations and because most of the wildfire activity within the NSO habitat occurs on U.S. Forest Service lands.

[5]Formally, within 10 miles of the convex hull of the NSO habitat border, since the boundary is highly complex.

[6]Angeles, Cleveland, Los Padres, and San Bernardino National Forests

classify ignitions as low intensity if the flames were 4 feet or lower and as moderate intensity for all other fires. Figure 4 maps ignition locations of all fires that started in or outside of the entire and critical NSO habitats across all years following the treatment and control group definitions described above.

While wildfire intensity captures its potential for destruction, fire severity actually quantifies the damage done to the surrounding ecosystems. To measure fire severity, we use data published by the Monitoring Trends in Burn Severity (MTBS) program. The data uses satellite imagery of wildfire burn areas to evaluate how severely a given fire burned based on the degree of disruption of above-ground biomass (vegetation and soil) within its perimeter starting in 1985. This information is only mapped for large wildfires 1,000 acres or greater in size. For outcomes, we use the existing MTBS classification of fire burn patches into three categories: unburned or low severity, moderate severity, and high severity. We then look at the share of total burn area that falls into each damage category (instead of classifying wildfires into groups, like with ignition point data). We also look at average severity within the entire burn area—the average is taken over the four categories measured on a scale of 1 through 4. Figure 5 plots perimeters of fires that meet the above definitions of treatment and control groups and Figure 6 provides an example of what the raw severity data looks like for a wildfire.

We plot key statistics over time (aggregated into 5-year bins) in Figures 7-10. The top rows of figures 7 and 8 give an overview of the frequency of all and larger wildfires that ignited on our lands of interest. Frequency data is characterized by high volatility, which is typical for wildfire data. More fires ignite on control group lands, but the treated and control trends appear to converge over time. Though the larger fires only make up 19 percent of our sample, they account for 96 percent of total acreage burned, so we are not losing a lot of data in severity analysis. The bottom rows of figures 7 and 8 show that post-treatment, the ratio of the count of ignitions in treated to control lands increases for all treatment and wildfire size specification combinations. Figures 9 and 10 provide similar plots for regression outcomes. Generally, most wildfires ignite at lower temperatures and most of their areas burn with minimal damage to the surrounding environment. We see suggestive evidence of more fires burning at low-temperatures in their early stage but at the same time an increase in highly-damaging burn areas on treated lands after the conservation initiative.

4 Methodology

To examine the impact of land restrictions on wildfire behavior, for each outcome of interest we estimate four difference-in-differences (DD) model specifications. The timing of the policy shift is extended, as the listing of the NSO precipitated a number of subsequent relevant policy actions. However, the 1990 listing was the first event that had immediate legal implications for timber harvesting and, as figure 2 showed, it marked the beginning of a dramatic decline in timber cut on protected lands. We hence run a standard DD model with a policy break in 1990 using both the critical and the entire range of the NSO habitat as treated areas:

$$y_{it} = \beta \mathbb{1}\{t >= 1990\} \times Habitat_{it} + \gamma X_{it} + \lambda Habitat_{it} + \alpha_c + \delta_t + \varepsilon_{it}$$

We provide two key sets of results. In the first, y_{it} is an identifier for high, moderate, or low burn intensity of wildfire at its ignition location. We run separate models for the three binary outcomes. In the second set of results, y_{it} measures the share of total fire burn area classified into high, moderate, or low severity based on its damage to above-ground biomass. We also include a measure of average severity over the entire burn perimeter and run separate regressions for the four outcomes.

The key treatment variable $Habitat_{it}$ is equal to one for any wildfire within the NSO habitat. We control for wildfire behavior predictors: terrain elevation, slope, and wildfire start month (X_{it}), and include fire start year (δ_t) and county fixed effects (α_c).[7] Regression results for wildfire severity are weighted by total acreage burned by the fire. Standard errors are clustered three-way on fire

[7] County is taken to be the county of ignition point location and the county of fire perimeter centroid for intensity and severity results, respectively.

start month, year, and county. We stop our analysis after 2019. Finally, to gauge how long it takes for the effects of the logging ban to kick in, we also break up all post-treatment years into 10 year bins and rerun the DD model.

5 Results

Burn Intensity of Ignitions

Our results for all models show an increase in the share of wildfires that start burning at low intensity in areas affected by logging restrictions. Tables 1 and 2 report the main estimates. Quantitatively, this result is true when we consider treated ignitions on the entire or just on the critical NSO habitat, but results are statistically significant only for the critical habitat. The proportion of low-intensity ignitions increases by 0.169 due to conservation efforts within the critical habitat area. At the same time, there is a comparable decrease (by at 0.142) in the share of fires that ignited at high intensity in this area following the conservation initiative. In our sample, 41 percent of ignitions are low-intensity and 25 percent are high-intensity, so these are both substantial changes. By contrast, we only see noisy estimates of any changes in the share of moderate intensity fires.

To check whether this increase in low-intensity ignitions is driven by smaller wildfires, we restrict our sample to fires that ultimately grow to burn 1,000 or more acres. Tables 3 and 4 show a similar pattern for large fires. They burn cooler at an early stage—the share of fires in the low-intensity category grows by at least 0.154. Finally, table 7 breaks down post-treatment effects by decade for all wildfires in the critical NSO habitat. We don't see any significant effects within the first decade after the policy change. The effects peak in 2000-2009 and then start decreasing both in magnitude and significance in 2010-2017. We conclude that our results in this section are driven by the natural characteristics of old-growth forests. We know that these forests burn much cooler than younger forests due to their extensive canopies, plenty of shade, and moisture. A ban on logging only enhances these characteristics and reduces the probability of elevated fire temperatures upon ignition.

Burn Severity

To measure the actual, rather than potential, damage to the surrounding ecosystems due to conservation efforts and logging restrictions, we next look at burn severity results. Tables 5 and 6 show consistent evidence of more damaging fires occurring within the NSO habitat territory after the policy change. Most wildfires that burned within the entire NSO range also overlapped with the critical habitat, so there is not as much discrepancy in results between the two treatment definitions. We see that average burn severity measured over the entire fire perimeter increases by 0.258-0.388 post treatment. Compared to the sample mean of 2.324, this is a sizeable gain.

The lower bound estimates for the increase in highly-damaging and the decrease in unburned or low severity fire perimeter area shares are 0.080 and 0.110, respectively. Moderate-severity burn patches also show some evidence of growth. To put these estimates into context, even though almost all wildfires in our sample have at least one high-severity burn patch, the average area share that burned severely is only 16 percent. The corresponding number for low-severity areas is 61 percent. The magnitude of the high-severity estimate is particularly significant given these statistics—it is a 50 percent increase from the mean. When we look at post-treatment effects by decade in tables 8 and 9, we see that it takes some time for these adverse effects of logging restrictions to kick in. For wildfires within the critical habitat, it takes a decade for these restrictions to translate into tangible damages and the damaging effects intensify in the decade after. For wildfires within the entire NSO habitat, it takes even longer.

Evidence on increased wildfire damage somewhat contrasts the previous section that finds cooler wildfire ignitions on conserved lands after the policy change. However, the two patterns are not mutually exclusive. We are trying to reconcile measurements taken at an early stage of a wildfire and over its entire lifetime. It is possible that due to the natural features of old-growth forests (shade and moisture), it takes time for wildfires to pick up in intensity. Once they spread from their ignitions and find their way to accumulations of extra vegetation, increased damages ensue.

6 Discussion and Conclusion

We find that forest protection practices, through logging restrictions, lead to wildfires that do more damage to their surrounding ecosystems despite a cooler start. We focus on fires in California, Oregon, and Washington, where wide-ranging land protections imposed in the 1990s brought a halt to timber harvesting in large swaths of western old-growth forests. These restrictions were initially proposed to protect the habitat of the Northern Spotted Owl, which was widely viewed by many as an "indicator species" for the overall health of these late-stage, old-growth forests.

We draw two key findings. First, we find that these restrictions on timber harvesting resulted in an increase in low-intensity fires during the initial wildfire suppression stage—that is, in fires that are more likely to burn at low temperatures upon ignition. This is not entirely surprising, as old-growth forests already contain more shade and moisture, and thus burn cooler, compared to younger forests (we refer to these as weather elements). Other things equal, a drop in timber harvesting leaves more standing trees with extensive canopies, enhancing these characteristics and making extreme wildfire behavior less likely. Second, when incorporating information on wildfires' entire lifespan and burn footprint, we find that even lower-intensity ignitions progress to wildfires with a larger area share of high-severity burn patches following the withdrawal of the logging industry.

Our results support a story of a complex interaction of factors affecting wildfire behavior. In our context, it appears that weather elements are more important in the earlier stages of a wildfire, and their impact was amplified by these timber-harvesting restrictions to reduce extreme fire behavior. However, the logging bans also directly contributed to a steady build-up of extra vegetation fuels that feed severe wildfire regimes. The fuels-accumulation factor eventually outweighs the weather-amplification factor in importance, causing more damage, and leading us to conclude that these logging restrictions ultimately harmed the target forests through an increase in high-severity burns.

We further interpret our results as suggestive evidence of old-growth forests' loss of natural resilience to extreme wildfires. Despite the established threat of logging practices to the Northern Spotted Owl's habitat, and their contribution to the eradication of old-growth forests in general, our research finds that an abrupt halt of timber harvesting for species conservation motives may not be an appropriate solution. Leaving old-growth forests to fend for themselves may have worked in the past, as they have historically been self-sufficient in being able to withstand major wildfire damage. However, we have heavily tampered with these forests' landscape for decades and their current natural state is different from their pre-settlement conditions. Even if other forest management practices, such as prescribed burns, are ongoing, they tend to be underutilized and fraught with logistical and financial issues. A qualified version of logging operations may after all be a beneficial supplement to wildfire suppression and the allowed forest management activities on these environmentally important lands.

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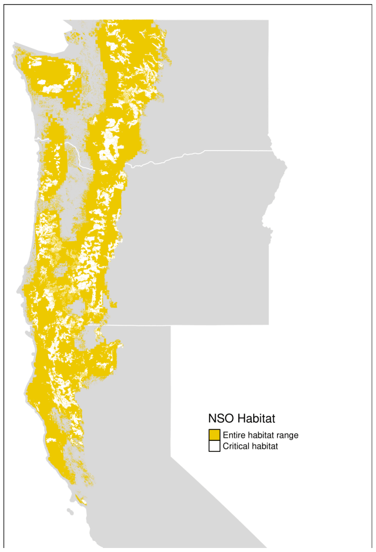
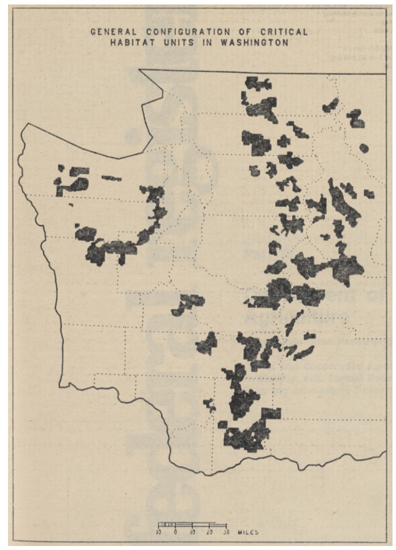
Figures

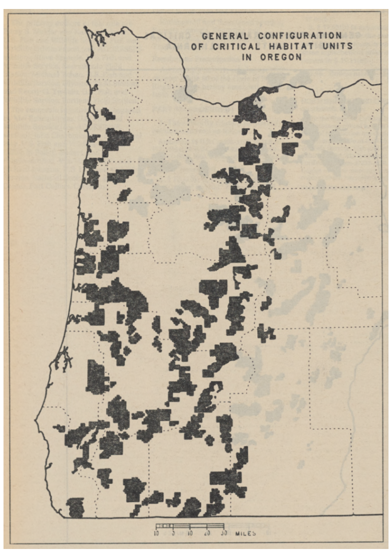
Figure 1: Current and Historic Northern Spotted Owl Habitat



(a) Current habitat



(b) 1992 critical habitat boundary: Washington

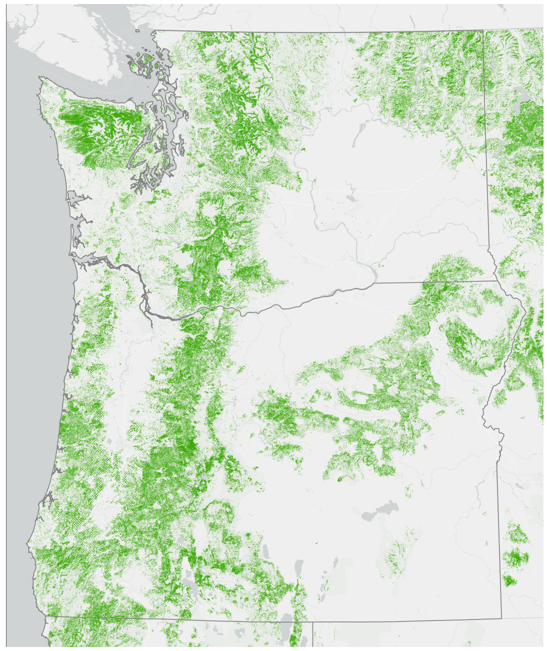


(c) 1992 critical habitat boundary: Oregon

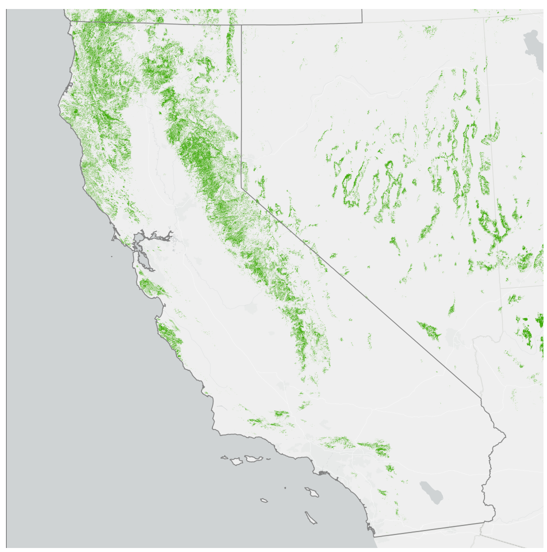
(d) 1992 critical habitat boundary: California

Notes: Source for current habitat map: U.S. Fish and Wildlife Service, https://ecos.fws.gov/ecp/species/1123. Source for historic maps: U.S. Fish and Wildlife Service (1992)

Figure 2: Mature and Old Forests in the U.S. West



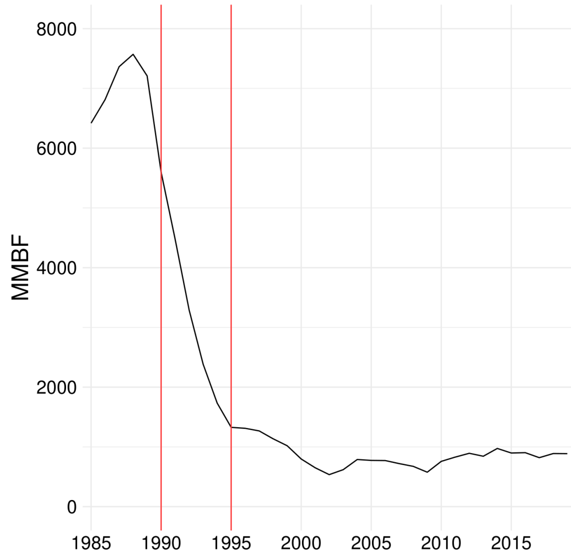
(a) Oregon and Washington



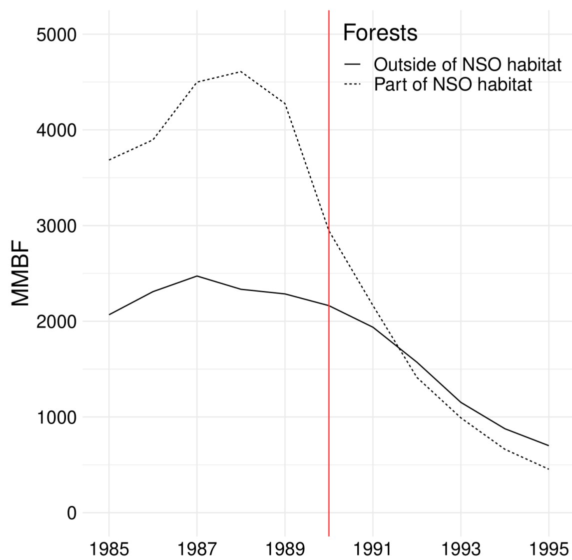
(b) California

Notes: Maps are based on data from DellaSala et al. (2022b). Mature and old-growth forests are defined by measures of canopy height, canopy cover, and aboveground living biomass.

Figure 3: Volume of Timber Cut on National Forests in the U.S. West



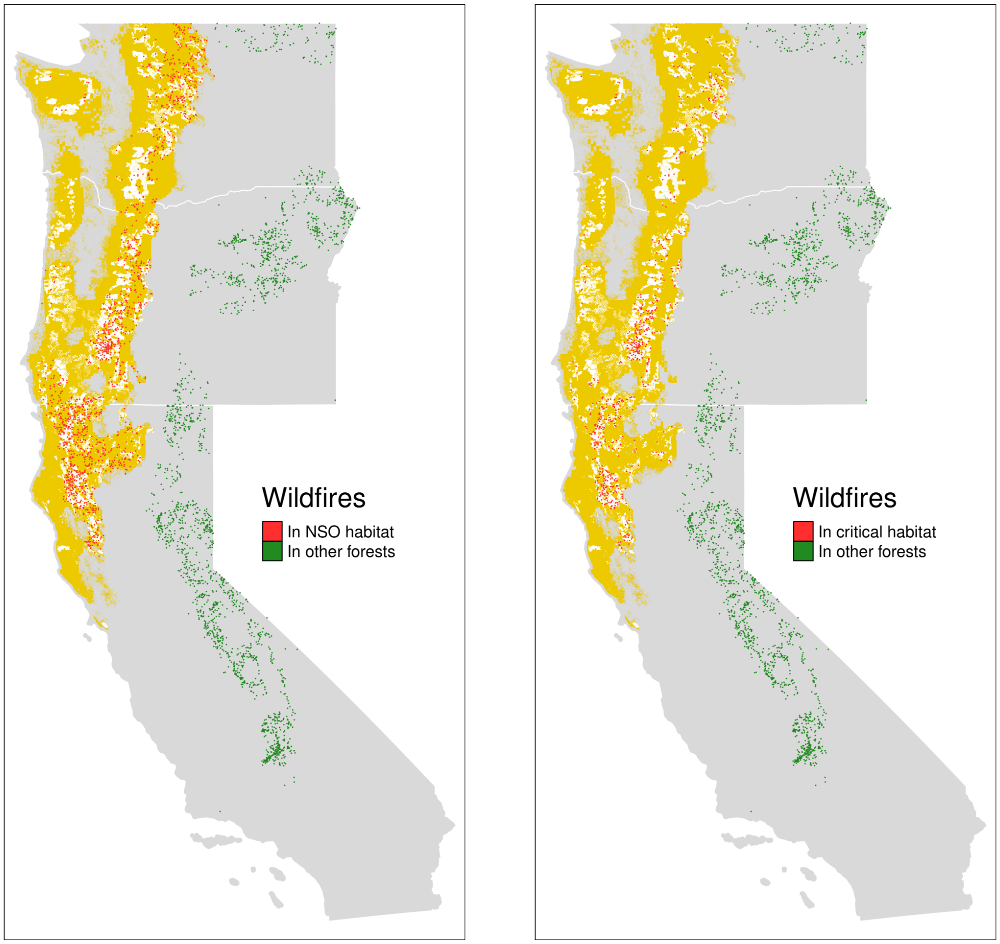
(a) California, Oregon, and Washington total



(b) Volume by national forest group

Notes: MMBF is a measure of volume of cut wood equal to 1000,000 board feet. One board foot gives the volume of a board that is one foot long, one foot wide, and one inch thick. Source: USFS Forest Products Cut and Sold reports: https://www.fs.usda.gov/forestmanagement/products/cut-sold/.

Figure 4: Ignition Locations of Treated and Control Wildfires, 1986-2019

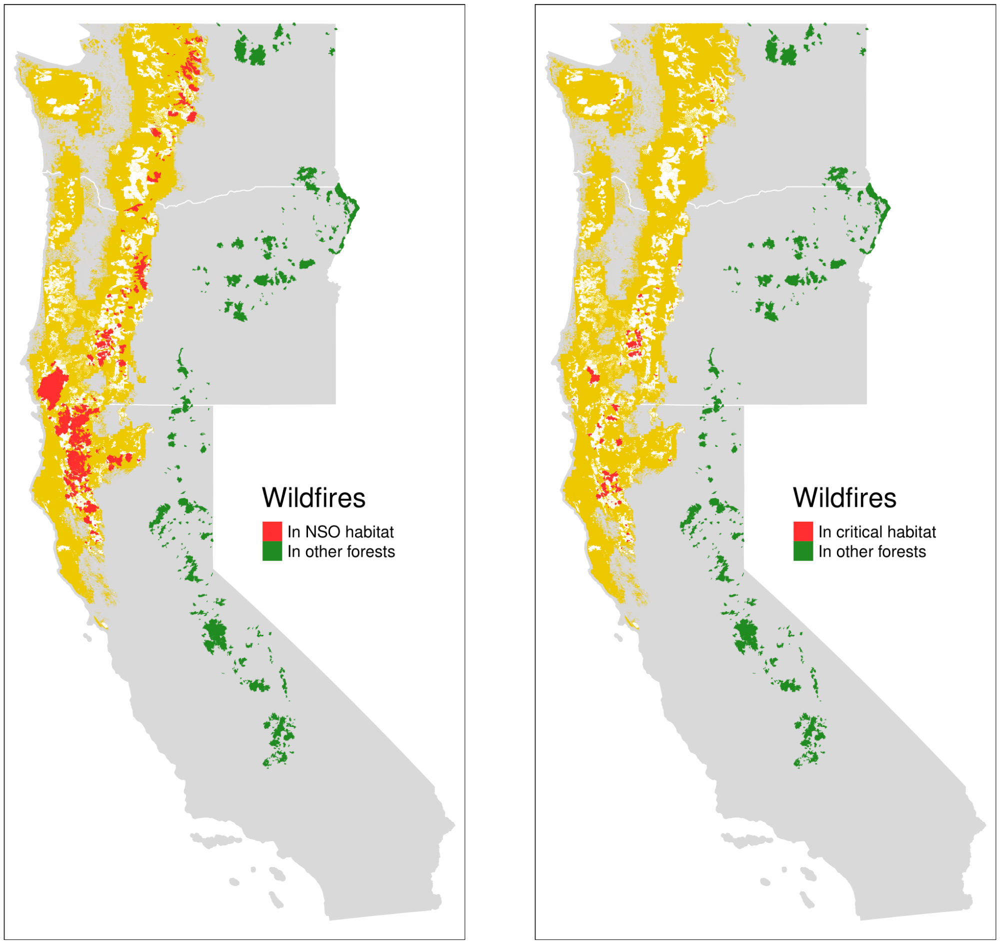


(a) Treatment specification 1

(b) Treatment specification 2

Notes: Authors' calculations based on the FIRESTAT Fire Occurrence data: https://data.fs.usda.gov/geodata/edw/datasets.php

Figure 5: Burn Perimeters of Treated and Control Wildfires, 1985-2019

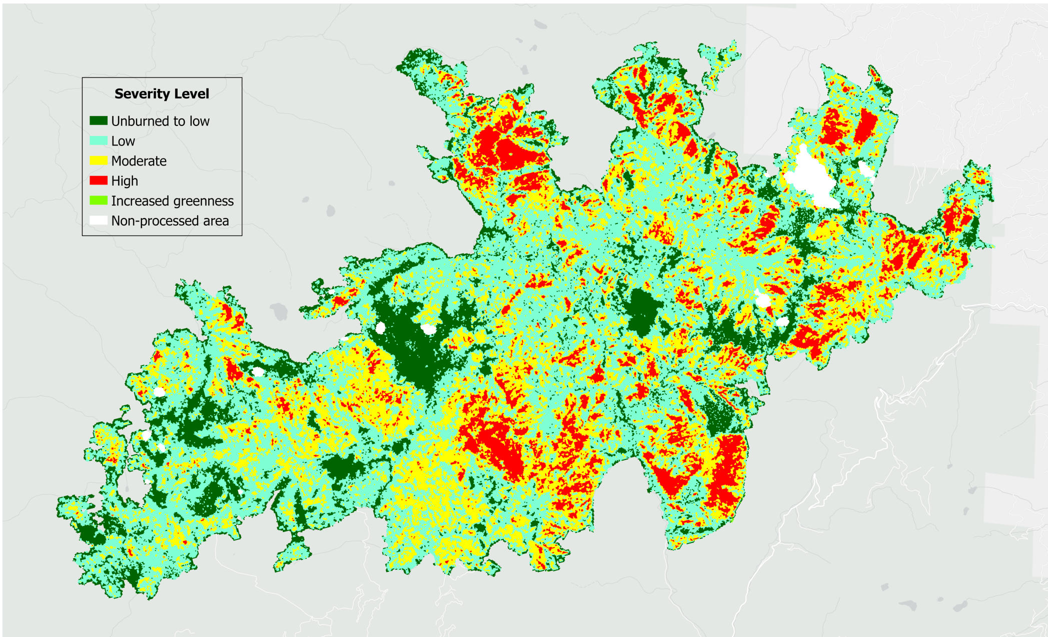


(a) Treatment specification 1

(b) Treatment specification 2

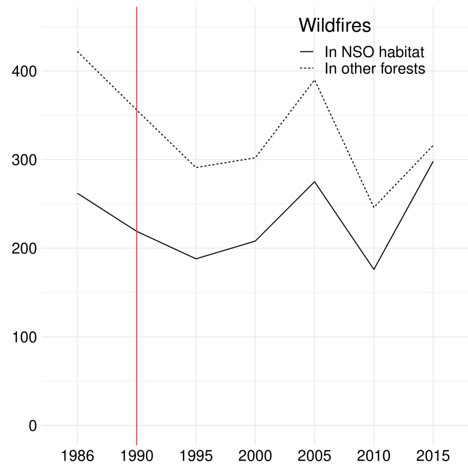
Notes: Authors' calculations based on the Monitoring Trends in Burn Severity (MTBS) data: https://www.mtbs.gov/

Figure 6: An Example of a Burn Severity Map for the 2017 Island Fire near Etna, CA

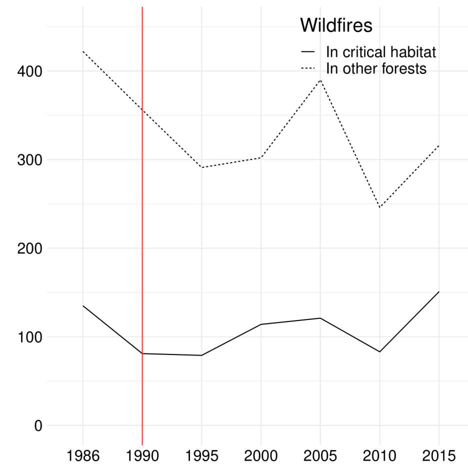


Notes: Source: Monitoring Trends in Burn Severity (MTBS) data: https://www.mtbs.gov/

Figure 7: Frequency of Treated and Control Wildfires 10+ Acres Over Time

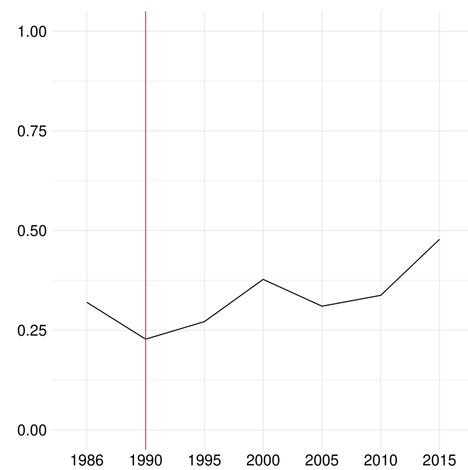


(a) Treatment specification 1, all fires by group



(b) Treatment specification 2, all fires by group

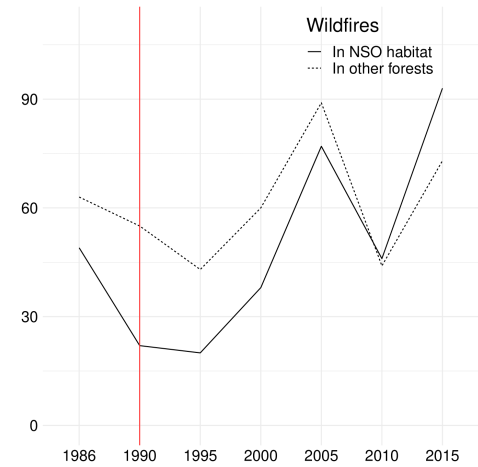
(c) Treatment specification 1, ratio of treatment to control fires



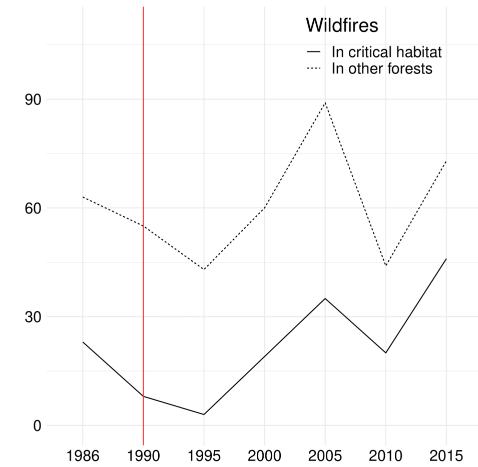
(d) Treatment specification 2, ratio of treatment to control fires

Notes: Data is aggregated into 5-year bins (4-year bin for pre-treatment period). 1986 represents data for 1986-1989, 1990 represents data for 1990-1994, and so on.

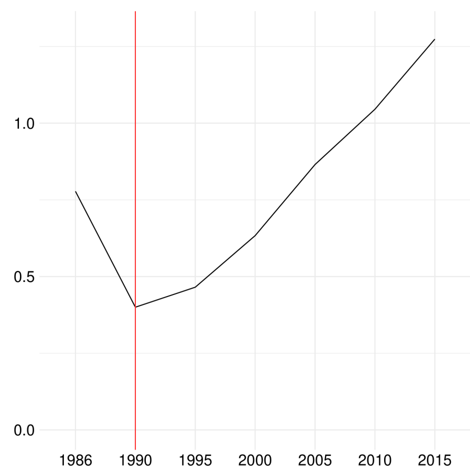
Figure 8: Frequency of Treated and Control Wildfires 1000+ Acres Over Time



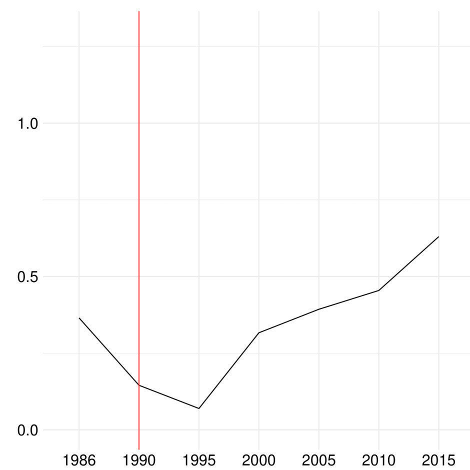
(a) Treatment specification 1, all fires by group



(b) Treatment specification 2, all fires by group



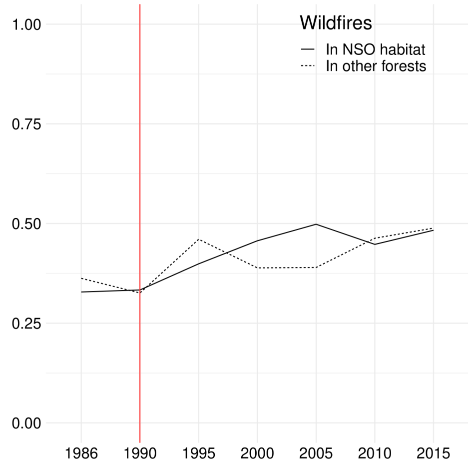
(c) Treatment specification 1, ratio of treatment to control fires



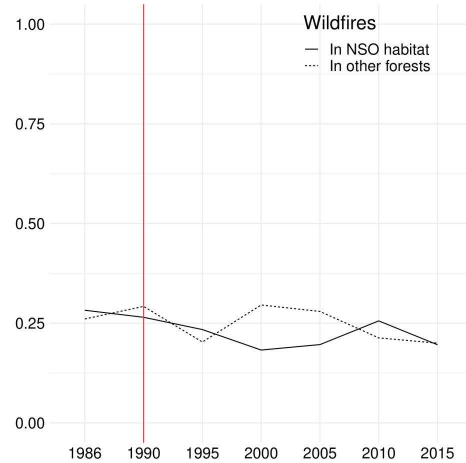
(d) Treatment specification 2, ratio of treatment to control fires

Notes: Data is aggregated into 5-year bins (4-year bin for pre-treatment period). 1986 represents data for 1986-1989, 1990 represents data for 1990-1994, and so on.

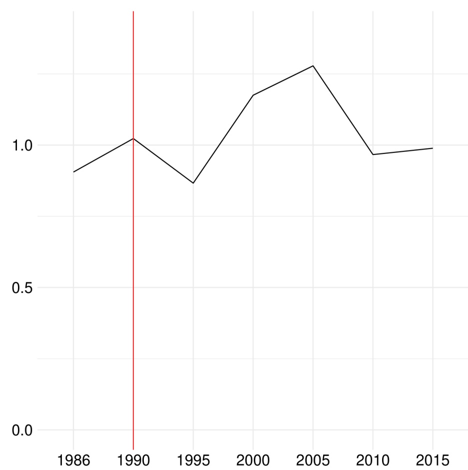
Figure 9: Sample Share of Low- and High-Intensity Wildfire Ignitions Over Time



(a) Low-intensity ignitions, all fires by group



(b) High-intensity ignitions, all fires by group

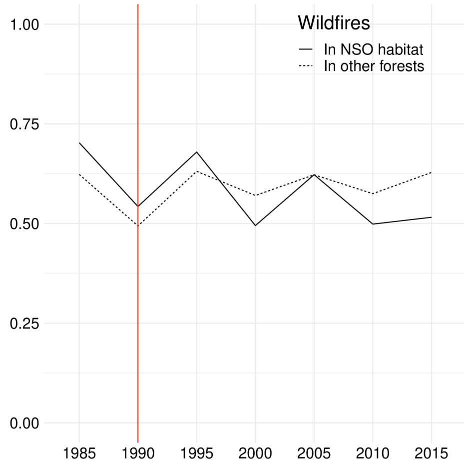


(c) Low-intensity ignitions, ratio of treatment to control fires

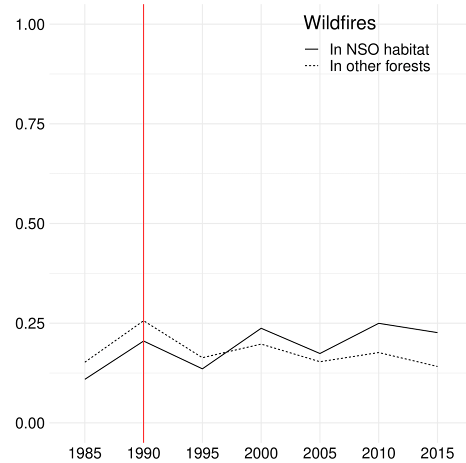
(d) High-intensity ignitions, ratio of treatment to control fires

Notes: Data is aggregated into 5-year bins (4-year bin for pre-treatment period). 1986 represents data for 1986-1989, 1990 represents data for 1990-1994, and so on.

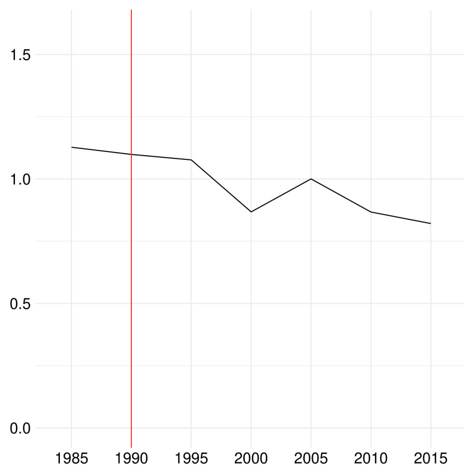
Figure 10: Mean Share of Low- and High-Severity Wildfire Burn Area Over Time



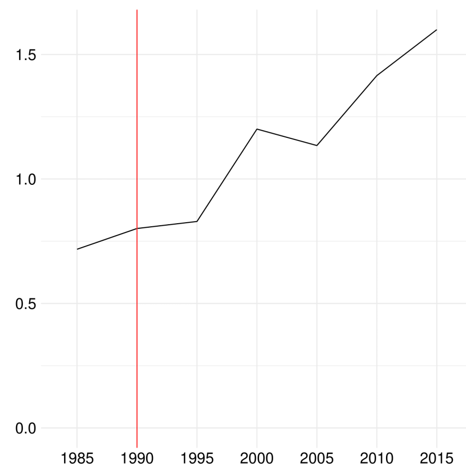
(a) Low-severity burn area share, all fires by group



(b) High-severity burn area share, all fires by group



(c) Low-severity burn area share, ratio of treatment to control fires



(d) High-severity burn area share, ratio of treatment to control fires

Notes: Data is aggregated into 5-year bins. 1985 represents data for 1985-1989, 1990 represents data for 1990-1994, and so on.

Tables

Table 1: DD Regression Results for Wildfire Ignitions, All Wildfires 10+ Acres, Treated Fires are in Entire NSO Habitat Range

Dependent Variables:	Low Intensity	Moderate Intensity	High Intensity
Model:	(1)	(2)	(3)
In habitat $\times$ Year $>=$ 1990	0.102*	-0.035	-0.068
	(0.057)	(0.058)	(0.047)
Fixed-effects			
Year	Yes	Yes	Yes
Month	Yes	Yes	Yes
County of ignition	Yes	Yes	Yes
Fit statistics			
Observations	3,598	3,598	3,598
R^2	0.12851	0.06058	0.10918
Within R^2	0.00685	0.00274	0.00498

Clustered (three-way on fire start month, year, and county) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 2: DD Regression Results for Wildfire Ignitions, All Wildfires 10+ Acres, Treated Fires are in Critical NSO Habitat

Dependent Variables:	Low Intensity	Moderate Intensity	High Intensity
Model:	(1)	(2)	(3)
In habitat × Year >= 1990	0.169**	-0.027	-0.142**
	(0.081)	(0.065)	(0.061)
Fixed-effects			
Year	Yes	Yes	Yes
Month	Yes	Yes	Yes
County of ignition	Yes	Yes	Yes
Fit statistics			
Observations	2,823	2,823	2,823
R^2	0.15307	0.06482	0.13104
Within R^2	0.00741	0.00090	0.00741

Clustered (three-way on fire start month, year, and county) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 3: DD Regression Results for Wildfire Ignitions, Large Wildfires 1000+ Acres, Treated Fires are in Entire NSO Habitat Range

Dependent Variables:	Low intensity	Moderate intensity	High intensity
Model:	(1)	(2)	(3)
In habitat × Year >= 1990	0.220***	-0.195*	-0.025
	(0.027)	(0.115)	(0.115)
Fixed-effects			
Year	Yes	Yes	Yes
Month	Yes	Yes	Yes
County of ignition	Yes	Yes	Yes
Fit statistics			
Observations	678	678	678
R^2	0.25740	0.21603	0.28821
Within R^2	0.02706	0.02135	0.00066

Clustered (three-way on fire start month, year, and county) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 4: DD Regression Results for Wildfire Ignitions, Large Wildfires 1000+ Acres, Treated Fires are in Critical NSO Habitat

Dependent Variables:	Low intensity	Moderate intensity	High intensity
Model:	(1)	(2)	(3)
In habitat × Year >= 1990	0.154***	-0.030	-0.123
	(0.054)	(0.142)	(0.129)
Fixed-effects			
Year	Yes	Yes	Yes
Month	Yes	Yes	Yes
County of ignition	Yes	Yes	Yes
Fit statistics			
Observations	514	514	514
R^2	0.31470	0.29583	0.36835
Within R^2	0.00897	0.01821	0.01058

Clustered (three-way on fire start month, year, and county) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 5: DD Regression Results for Wildfire Burn Areas, Treated Fires are in Entire NSO Habitat Range

Dependent Variables:	Average Severity	Low Severity	Moderate Severity	High Severity
Model:	(1)	(2)	(3)	(4)
In habitat × Year >= 1990	0.258**	-0.110**	0.030	0.080***
	(0.105)	(0.042)	(0.019)	(0.030)
Fixed-effects				
Year	Yes	Yes	Yes	Yes
Month	Yes	Yes	Yes	Yes
County of perimeter centroid	Yes	Yes	Yes	Yes
Fit statistics				
Observations	729	729	729	729
R^2	0.57727	0.53547	0.36746	0.56694
Within R^2	0.02635	0.02798	0.00551	0.05311

Clustered (three-way on fire start month, year, and county) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 6: DD Regression Results for Wildfire Burn Areas, Treated Fires are in Critical NSO Habitat

Dependent Variables:	Average Severity	Low Severity	Moderate Severity	High Severity
Model:	(1)	(2)	(3)	(4)
In habitat × Year >= 1990	0.388***	-0.169***	0.063***	0.106***
	(0.102)	(0.042)	(0.011)	(0.035)
Fixed-effects				
Year	Yes	Yes	Yes	Yes
Month	Yes	Yes	Yes	Yes
County of perimeter centroid	Yes	Yes	Yes	Yes
Fit statistics				
Observations	507	507	507	507
R^2	0.58129	0.53129	0.39260	0.57216
Within R^2	0.02355	0.01811	0.00806	0.01817

Clustered (three-way on fire start month, year, and county) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 7: DD Regression Results for Wildfire Ignitions, All Wildfires 10+ Acres, Treated Fires are in Critical NSO Habitat

Dependent Variables:	Low Intensity	Moderate Intensity	High Intensity
Model:	(1)	(2)	(3)
In habitat × Year 1990-1999	0.125	-0.037	-0.088
	(0.089)	(0.084)	(0.072)
In habitat × Year 2000-2009	0.202**	-0.008	-0.195***
	(0.090)	(0.073)	(0.062)
In habitat × Year 2010-2017	0.168*	-0.047	-0.121
	(0.100)	(0.067)	(0.085)
Fixed-effects			
Year	Yes	Yes	Yes
Month	Yes	Yes	Yes
County of ignition	Yes	Yes	Yes
Fit statistics			
Observations	2,823	2,823	2,823
R^2	0.15363	0.06498	0.13248
Within R^2	0.00806	0.00108	0.00905

Clustered (three-way on fire start month, year, and county) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 8: DD Regression Results for Wildfire Burn Areas, Treated Fires are in Entire NSO Habitat Range

Dependent Variables:	Average Severity	High Severity	Moderate Severity	High Severity
Model:	(1)	(2)	(3)	(4)
In habitat × Year 1990-1999	0.231	-0.101	0.009	0.092
	(0.142)	(0.064)	(0.023)	(0.055)
In habitat × Year 2000-2009	0.092	-0.041	0.015	0.026
	(0.079)	(0.027)	(0.012)	(0.029)
In habitat × Year 2010-2017	0.350***	-0.149***	0.042*	0.107***
	(0.128)	(0.053)	(0.024)	(0.036)
Fixed-effects				
Year	Yes	Yes	Yes	Yes
Month	Yes	Yes	Yes	Yes
County of perimeter centroid	Yes	Yes	Yes	Yes
Fit statistics				
Observations	729	729	729	729
R^2	0.59138	0.54771	0.37211	0.57922
Within R^2	0.05885	0.05360	0.01281	0.07997

Clustered (three-way on fire start month, year, and county) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1

Table 9: DD Regression Results for Wildfire Burn Areas, Treated Fires are in Critical NSO Habitat

Dependent Variables:	Average Severity	High Severity	Moderate Severity	High Severity
Model:	(1)	(2)	(3)	(4)
In habitat × Year 1990-1999	-0.221	0.051	0.026	-0.077
	(0.576)	(0.241)	(0.065)	(0.180)
In habitat × Year 2000-2009	0.239***	-0.118***	0.042***	0.076***
	(0.078)	(0.027)	(0.004)	(0.028)
In habitat × Year 2010-2017	0.470***	-0.197***	0.074***	0.122***
	(0.131)	(0.059)	(0.018)	(0.045)
Fixed-effects				
Year	Yes	Yes	Yes	Yes
Month	Yes	Yes	Yes	Yes
County of perimeter centroid	Yes	Yes	Yes	Yes
Fit statistics				
Observations	507	507	507	507
R^2	0.58784	0.53497	0.39479	0.57509
Within R^2	0.03884	0.02582	0.01164	0.02490

Clustered (three-way on fire start month, year, and county) standard-errors in parentheses
Signif. Codes: ***: 0.01, **: 0.05, *: 0.1