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Three Essays On Environmental And Urban Economics

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ABSTRACT

THREE ESSAYS ON ENVIRONMENTAL AND URBAN ECONOMICS

BY

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Committee Chair: Dr. H. Spencer Banzhaf

Major Department: Economics

This dissertation has three chapters in the field of urban and environmental economics. Chapter one studies the relationship between home prices and crime rates, comparing different sources of crime data. Chapter 2, co-authored with Stefano Carattini, Andreas Löschel, and Béla Figge, studies the impact of local building regulations on the adoption of solar photovoltaics in Germany. Chapter 3 studies the relationship between COVID-19 policy stringency and levels of saltwater fishing.

Chapter 1: Hedonic analysis of home prices is a common technique for estimating the value of local public goods such as public safety or environmental quality. In this line of research, measurement error bias is a frequent concern due to the complex, multi-faceted nature of a neighborhood. In this study, I conduct a hedonic analysis measuring the price of public safety by incorporating 2 different sources of crime data from the FBI's Uniform Crime Reporting program: (1) jurisdiction-level crime rates from the SRS, and (2) incident-level, geo-coded crime data from the NIBRS.

Chapter 2: Conflicting societal goals can lead to national and local policies that are at odds with each other. National policies promoting the adoption of solar photovoltaics may be counteracted by local policies defining the aesthetics of the built environment. Using unique data from Germany, a leader in solar adoption, we document the impact that the rise in municipalities amending their building codes to restrict solar installations, often with an eye toward preserving the historical nature of the town, has on solar adoption. We combine a unique survey of municipalities regarding such building codes with administrative data on all solar installations in Germany.

Chapter 3: Governments responded to the Covid-19 pandemic with different policies to curtail

the spread of the virus. We show how sportfishing levels are related to the stringency of Covid-19 policies. Specifically, we relate the total number of resident sportfishing trips taken each month in each of 16 U.S. states to a state-level index of COVID policy stringency. We model the number of recreational fishing trips taken in each state-month using a fixed effect Poisson regression model with state-specific seasonality and time trends.

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A Dissertation Submitted in Partial Fulfillment
of the Requirements for the Degree
of
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ACCEPTANCE

This dissertation was prepared under the direction of the candidate's Dissertation Committee. It has been approved and accepted by all members of that committee, and it has been accepted in partial fulfillment of the requirements for the degree of Doctor of Philosophy in Economics in the Andrew Young School of Policy Studies of Georgia State University.

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Chapter 1

Measuring the Price of Public Safety: Comparing NIBRS and SRS Crime Data

1.1 Introduction

A longstanding literature in economics seeks to infer consumer's values for neighborhood amenities such as parks and safety through analysis of housing price differentials (Rosen, 1974; Taylor, 2017). Typically referred to as property value hedonics, these methods help inform discussions of the value of crime reductions, environmental valuation, and the value of school quality improvements, among others.

In this paper, I address the measurement of the value of crime reductions (or its inverse, the value of public safety). A frequent concern in this line of research is that the econometrician may not be able to properly measure the effect of crime due to the difficulty with measuring crime, and additionally due to the potential confounding of crime with other, unmeasured amenities.

Traditionally, research has relied on the FBI's Summary Reporting System (SRS), a part of the Uniform Crime Reporting (UCR) program, which is only at the jurisdiction level. Research has typically used the jurisdiction-wide average or triangulated on the nearest three jurisdictions (Anselin and Lozano-Gracia, 2008; Banzhaf and Walsh, 2008; Bayer et al., 2016; Bishop and Timmins, 2019; Bishop et al., 2021; Pope and Pope, 2012; Zabel, 2015). These were the only data available for many years and remain so in many US jurisdictions. More recently, data have become available from the National Incident-Based Reporting System (NIBRS), with detailed geolocated data. I compare results from both datasets, to gauge the importance of the new data.

I perform a hedonic analysis of the Atlanta single family home market, focusing on estimating the relationship between property values and neighborhood crime as measured alternately by the NIBRS and SRS. The NIBRS data is substantially more information-rich in that it contains 122,577 crimes reported from 2013 through 2016, with geo-location data for each crime, allowing for the construction of very detailed crime density (crimes per unit area) measures specific to each home in the dataset. In contrast, the SRS data gives crime rates (crimes per unit population) and involves

multiple levels of aggregation and interpolation, which may degrade its usefulness in measuring the price of public safety.

In order to compare the reliability of public safety price estimates based on the NIBRS and SRS data, I also incorporate fine-grained information on how people perceive neighborhoods, based on the Place Pulse online survey developed by Naik et al. (2014) in which people evaluate Google Street View images of the neighborhoods in question, to assess how the inclusion of this information alters estimates of the value of neighborhood crime reductions. Similar data have been considered by Glaeser, Kincaid, and Naik (2018), who find that there is a strong relationship between looks and home value, but also that the marginal predictive power of these characteristics in home price regressions is minimal after conditioning on basic home characteristics and location. In some model specifications I also include non-parametric spatial price effects, specified as a second-degree polynomial of latitude and longitude, to further assess the robustness of estimates from these 2 different datasets.

I also consider a model specification in which the crime regressor is based on a forecast of future crime given information available at the time of purchase, rather than using the current crime at the time of purchase. This study therefore ties together a long running concern about measurement error of neighborhood amenities in hedonic models with a recent strand of literature in which researchers have been developing methods to account for the fact that the home-purchasing decision is a forward-looking decision, in which current amenity values at the time of purchase may not be the best predictors of the discounted future flow of amenity values, which is what is really being purchased (Bishop and Murphy, 2019).

I find that when using the NIBRS data to its fullest potential, by assigning each home multiple different crime density measures for crimes at various distances from the home and therefore non-parametrically estimating the decay function for the impact of distance from the home on the price premium associated with a given crime, controlling for previously unobserved dimensions of neighborhood quality does not have a large impact on the estimates of the implicit price of violent crime, shifting point estimates of the relationship between violent crimes and home prices

by at most 7%. In contrast, using the NIBRS data with more aggregation, such as by lumping together crimes at various distances from the home into a single measure, and failing to distinguish between violent crime and property crime, results in substantially less robust estimates which vary substantially in magnitude and may even flip signs. Similarly, using the heavily interpolated SRS data results in coefficients that vary by over 100%, although sign flipping is not observed in any model specifications.

1.2 Data

This study combines data on the sales prices of single-family homes in Atlanta, Georgia with data on neighborhood characteristics including crime rates, demographics, school quality, and neighborhood aesthetics. The final analysis dataset consists of 7,758 single-family homes within my study area, which transact exactly once between 2013 and 2016. The study area is the intersection of Fulton County (for which I have housing data), the Atlanta Police Department's (APD) jurisdiction (for which I have detailed crime data), and the city perimeter (for which I have street-level neighborhood perceptions data). The years 2013-2016 are chosen so as to align with the years in which the Place Pulse survey responses were recorded. Within the study area, my analysis sample represent a census of all valid, market-rate home sales.

The following subsections describe the sources and characteristics of these datasets.

1.2.1 Home Characteristics and Transactions

Conducting hedonic housing price regressions requires data on both housing characteristics and housing transactions, which I obtain from two separate sources. The housing characteristic data is obtained from CoreLogic[1], a real estate information firm. For each single-family residential property, this data includes information on the finished square footage of the dwelling, lot size, number of bedrooms, number of bathrooms, number of stories, and presence of a swimming pool. The data also includes information on the year the main structure was built, as well as "effective year built,"meaning the last time permitted major improvements were made to the property. Among

[1] formerly known as DataQuick

the properties in my data, I observe all transactions which are recorded by the Fulton County Tax Assessors office. In order to prepare this housing characteristic and housing transaction data for analysis, I make a variety of sample restrictions which are typical to the hedonic housing literature, and which I describe in detail through the remainder of this subsection.

To clean the CoreLogic property characteristic data of anomalous observations, I drop parcels which have values of zero for square footage or lot-size, parcels with no bathroom, and parcels whose square footage divided by the number of stories is greater than the lot size. After cleaning, I am left with 59,374 properties in the study area. Among these properties, there are 32,034 transactions recorded in the Fulton County Assessor's database from 2013 through 2016. For these transactions, I observe the sales price, the name of the buyer, the name of the seller, the date of the transaction, and information about the nature of the sale. In order to restrict my analysis to market-rate transactions, I drop "non arms-length transactions" where either the sales price is equal to zero, or the name of the buyer is the same as the name of the seller, and additionally I drop a variety of transactions for which the tax assessor has recorded a flag that the transaction is not a valid sale[2]. These restrictions collectively drop 21,712 transactions, leaving 10,322 remaining. I then drop any transactions which remain for properties for which I do not have complete amenity data, which includes 88 observations which do not match to Atlanta Public Schools district, which I rely on for measuring school average test scores as a measure of educational quality. I then drop transactions of parcels which are in the top 5% of the transaction frequency distribution, which in this data are parcels that transact more than once between 2013 and 2016. There are a total of 1,085 such properties representing 2,310 transactions, so after dropping these transactions I am left with 7,924 transactions. After 2% alpha trimming (dropping transactions from the top and bottom 1% of the sale price distribution) to remove outliers, I am left with 7,758 transactions for analysis.

Table 1.1 displays summary statistics for the structural characteristics and sale prices of properties in this final analysis sample. Table 1.2 tabulates the homes in the sample by original year

[2]Some of these flags include, for instance, that the transaction was part of a liquidation or foreclosure, the the transaction was for multiple parcels, that the property was sold to a bank, investment firm or government agency, and that the transaction was for less than $1,000

	Mean	SD	Min	Max
Sale Price (USD)	420,713	443,015	7,600	2,450,000
Dwelling Size (sqft)	2,006	1,191	480	16,537
Lot Size (sqft)	14,629	16,626	378	457,380
# of Bedrooms	3.07	0.94	0	9
# of Bathrooms	2.14	1.15	1	9.00
# of Swimming Pools	0.00	0.04	0	3
# of Stories	1.29	0.46	1	3
N	7,758			

Table 1.1: Summary Statistics for Parcel Characteristics of Transactions

	Original year built		Effective year built	
	Frequency	Percentage	Frequency	Percentage
1870	2	0.0258	1	0.0129
1890	13	0.1676	2	0.0258
1900	52	0.6703	6	0.0773
1910	62	0.7992	16	0.2062
1920	1,283	16.5378	368	4.7435
1930	980	12.6321	359	4.6275
1940	1,282	16.5249	554	7.1410
1950	1,516	19.5411	904	11.6525
1960	586	7.5535	1,041	13.4184
1970	127	1.6370	673	8.6749
1980	245	3.1580	1,357	17.4916
1990	362	4.6662	1,075	13.8567
2000	1,242	16.0093	1,397	18.0072
2010	6	0.0773	5	0.0644
N	7,758			

Table 1.2: Tabulation of Parcels by Decade Built

built and effective year built; it is evident from comparing original to effective year built that many homes in the sample have been substantively renovated over their lifetime, as the number of properties built in the 1960's and later increases substantially when considering the effective year built, while the number built in the 1950's and earlier decreases substantially.

Lastly, Table 1.3 includes summary statistics for all of the amenity variables used in the study, tabulated at the transaction level. The first six variables are local crime measures, the next two are violent crime and property crime measures based on FBI data, the next three are Census demographics at the Block Group level, the next two are school quality measures based on statewide

	mean	sd	min	max
APD Violent Crimes, 0-0.1 miles	1.09	1.99	0	24
APD Violent Crimes, 0.1-0.25 miles	6.07	7.86	0	56
APD Violent Crimes, 0.25-0.5 miles	22.0	22.0	0	148
APD Property Crimes, 0-0.1 miles	5.82	7.30	0	153
APD Property Crimes, 0.1-0.25 miles	33.8	36.2	0	427
APD Property Crimes, 0.25-0.5 miles	133	110	0	797
FBI Violent Crime Rate (Imputed)	1050	228	694	1493
FBI Property Crime Rate (Imputed)	7,118	1,759	4,950	11,757
% Black Residents	.446	.393	0	1
% Hispanic Residents	.042	.062	0	.481
Per Capita Income (1,000's 2011 $)	46.0	33.4	4.34	174
5th Grade Reading Score (2014 CRCT)	838.5	19.5	811.1	875.7
5th Grade Math Score (2014 CRCT)	839.5	26.0	794.11	880.33
Wealthy	1226	413	719	1970
Safety	1272	6.2	1264	1284
Lively	1179	36	1101	1255
Depressing	1180	288	620	1550
Boring	1221	6.8	1209	1235
Beautiful	1148	621	341	2375
N	7,758			

Table 1.3: Amenity Summary Statistics

standardized exams, and the remaining six are neighborhood aesthetic measures from the Place Pulse survey.

1.2.2 Crime

I use an administrative dataset of all robberies, murders, aggravated assaults, burglaries, auto thefts, and larcenies reported to APD since 2009. This is the NIBRS data, a data collection program administered by the FBI. Importantly, this data includes longitude and latitude for each crime. From this data I construct crime variables for each property by drawing concentric rings around the property and counting the number of crimes in each ring in the year of the sale. The distances for these rings are 0 to 0.1 miles, 0.1 miles to 0.25 miles, and 0.25 miles to 0.5 miles. Since many properties in my dataset lie near the border of APD's jurisdiction, and thus do not have crime observations in the portion of their rings which lie outside APD's jurisdiction, some imputation is necessary for these properties. I divide the crime measures by the portion of the ring which falls inside APD's

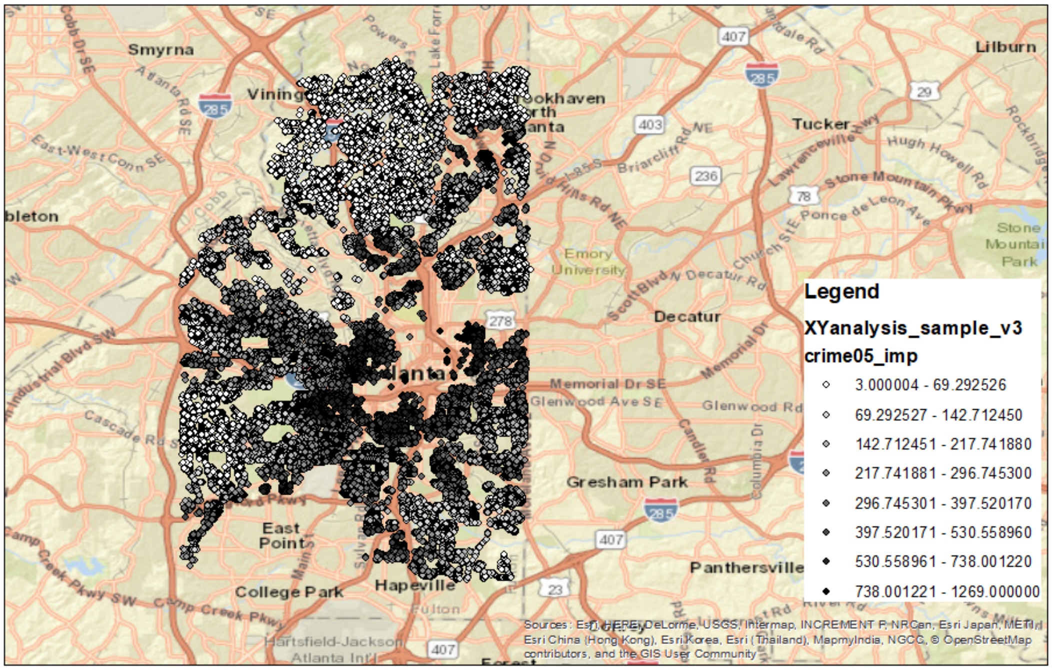


Figure 1.1: Parcels by # of APD Crimes Within 0.5 Miles

jurisdiction in order to obtain a measure which corrects for this missing data problem, assuming that the spatial distribution of crimes outside APD's jurisdiction is similar to the distribution inside. Figure 1.1 displays the spatial distribution of crimes reported to APD by showing parcels in the study area color-coded by imputed number of crimes within 0.5 miles. This map makes clear that crimes in Atlanta are concentrated in the downtown area, and the neighborhoods West and South of downtown, while the neighborhoods to the North and the far West of the city see substantially less crime.

The approach that I take here in using incident-level crime data in a hedonic study is comparable to work by Ihlanfeldt and Mayock (2010), who use similar data for Miami-Dade county in a panel data context. In comparison to their work, my ring-based approach to measuring the crime density at different distances from the home makes more detailed use of this incident-level data, as in that paper they still aggregate crimes to the Census tract level. However, I do choose the crime density as the measure to use, consistent with their finding that crime densities (crimes per unit area) are preferable to crime rates (crimes per unit population) in the measurement of public safety

	mean
% Transactions within 0.1 mi. of APD Boundary	2.7
% Transactions within 0.25 mi. of APD Boundary	7.8
% Transactions within 0.5 mi. of APD Boundary	16.5
% Transactions within 1 mi. of APD Boundary	33.3
% Transactions within 2 mi. of APD Boundary	65.8
N	7,758

Table 1.4: Transactions near APD Jurisdiction Boundary

for hedonic models.

In the hedonic regressions, it will be useful to measure the crime level for a home not only taking into account the distance of the crimes from the home, but also the type of crime. In the data provided by APD, there are a total of 122,577 crimes reported from 2013 through 2016. These crimes can be broken down into violent crimes, of which there are 18,038, and property crimes, of which there are 104,539. The violent and property crime categories can be further broken down into 5 sub-categories each: property crimes include larceny from a vehicle (38,390), larceny not from a vehicle (28,866), residential burglary (16,733), auto theft (16,710), and non-residential burglary (3,840), while violent crimes include aggravated assault (8,862), pedestrian robbery (7,065), commercial robbery (941), residential robbery (801), and homicide (369).

Table 1.4 summarizes the severity of the missing data problem for the transactions in the study area, using dummy variables equal to 1 if the transaction is within a given distance of the APD jurisdiction boundary. Only 2.7% of transactions are within 0.1 miles, and only 16% are within 0.5 miles, but 66% of the observations are within 2 miles of the border. Among the 1,278 transactions that lie within 0.5 miles of the border, the majority (670) have at least 80% of their surrounding 0.5 mile buffer within APD jurisdiction, and less than a quarter (315) have less than 68% of their area outside the jurisdictions, and hence missing data is not a large issue for the vast majority of properties in the sample. I will present some results dropping the 608 transactions that have less than 80% of their surrounding 0.5 miles buffer within APD jurisdiction, for robustness.

The traditional way to measure crime rates is to use data reported by the FBI's SRS. This data is provided as part of the UCR program, the same program from which the NIBRS data comes,

however the less-detailed SRS data has been around for longer than the NIBRS data. Additionally, it remains the only data available in many jurisdictions. Consequently, it has been widely used in the literature (Banzhaf and Walsh, 2008; Anselin and Lozano-Gracia, 2008; Zabel, 2015; Bishop et al., 2021). The SRS is a nationally standardized method for local crime reporting agencies to inform the FBI of the number of violent crimes and property crimes occurring in their jurisdiction, reported as a rate of crimes per 100,000 persons per year. To match these local crime rates to properties, it is necessary to interpolate between the different jurisdictions. The standard approach is to take the inverse-distance-weighted average of the 3 closest crime reporting agencies' crime rates, using the central point of the agency's jurisdiction. I include this method for sake of comparison with the more detailed crime data provided by APD/NIBRS. Figure 1.2 displays the crime reporting agencies that are applicable for the Atlanta area, including Atlanta Police Department, East Point PD and College Park PD to the South, Decatur PD to the East, and Smyrna PD and Chamblee PD to the North. The figure also shows the parcels in the study area, color-coded by IDW estimated violent crime rate.

Comparing Figures 1.1 and 1.2 makes clear the shortcomings of the SRS data. The central point for Atlanta PD's jurisdiction is located in the neighborhood of Bankhead, one of the hotspots for crime in the city. But since Atlanta overall has a lower crime rate than East Point and College Park, parcels in Bankhead are assigned a lower SRS crime rate than parcels towards the Southern edge of APD's jurisdiction, despite the fact that the map of actual APD crime rates clearly shows that the parcels in Bankhead experience higher amounts of crime than their neighbors to the South. The SRS method also underestimates the amount of crime on the East side of the city due to the very low crime rate reported by Decatur PD.

1.2.3 Street-Level Neighborhood Perceptions

Markets respond to participants' perception of conditions, not necessarily to objective measures of conditions. Information on neighborhood perceptions at the street-level is obtained from the Place Pulse survey. This online survey asks participants to compare two images of urban landscapes from

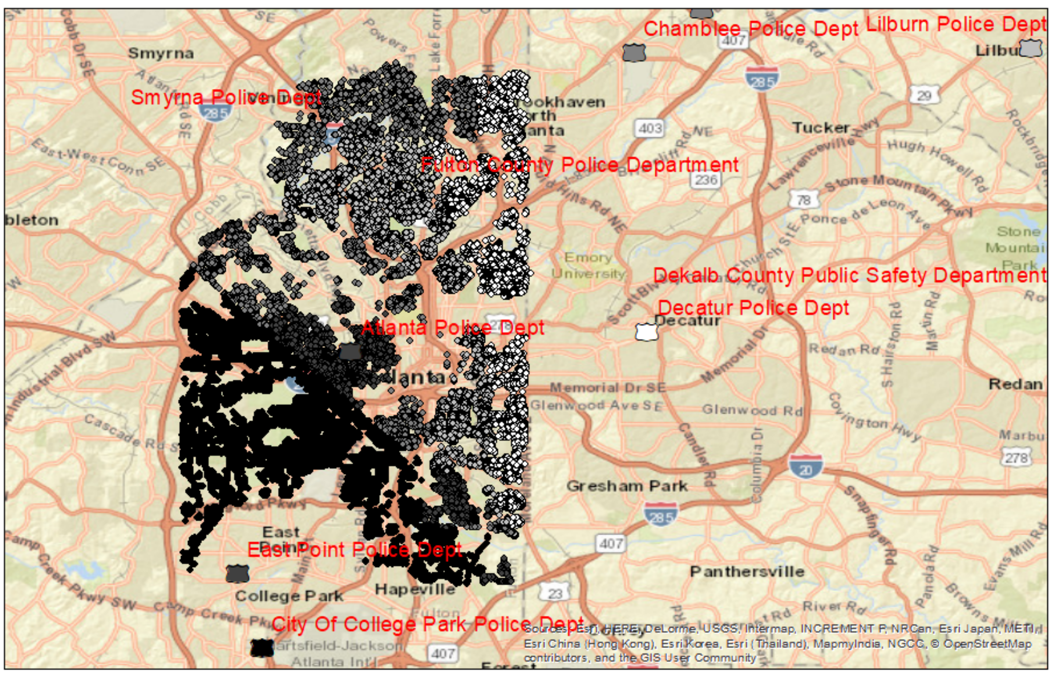


Figure 1.2: Parcels by Interpolated FBI SRS Crime Rate (IDW-3NN Interpolation)

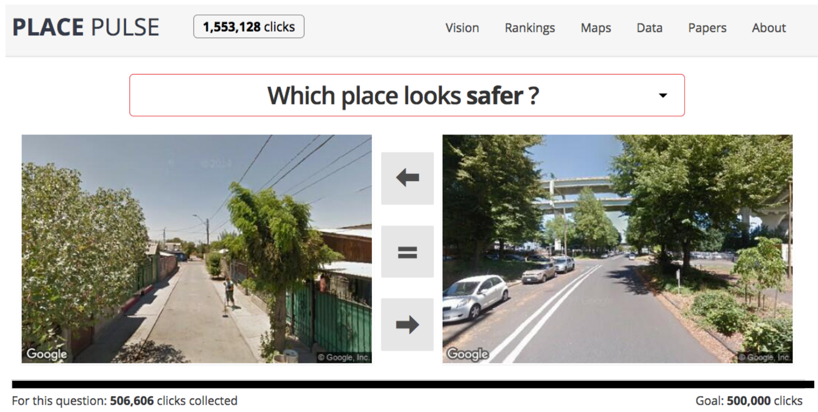


Figure 1.3: Place Pulse Survey Form

Google Street View and click on the image they think answers the question "Which place looks safer." Additionally, the survey asks about other perceptions which are useful controls, including: "more beautiful," "more boring," "wealthier," "livelier," and "more depressing." The survey form is illustrated in Figure 1.3. The comparisons use over 111,391 locations from 56 cities across the globe, including 4,059 images from the city of Atlanta. From 2013 to 2016, this survey amassed over 1 million responses (clicks),[3] each representing a comparison of 2 randomly selected images. These binary comparisons can be used to construct image ratings in a manner analogous to the process used to rate chess players based off of their wins and losses, as described in section 1.3.3.

1.2.4 School Quality

To measure the quality of neighborhood schools, I match each property to its 5th grade public school attendance boundary, which for the properties in the study area includes 42 different elementary schools from the Atlanta Public Schools system. The school quality measures I use are drawn from a statewide standardized test, the Criterion Referenced Competency Test (CRCT). I use the school-wide average on the 5th grade reading CRCT, and the 5th grade math CRCT, for the 2013-2014 school year. Values from the reading test are mapped in Figure 1.4.

[3] 152,241 clicks for "wealthier", 368,926 for "safer", 267,292 for "livelier", 132,467 for "more depressing", 127,362 for "more boring", and 175,361 for "more beautiful"

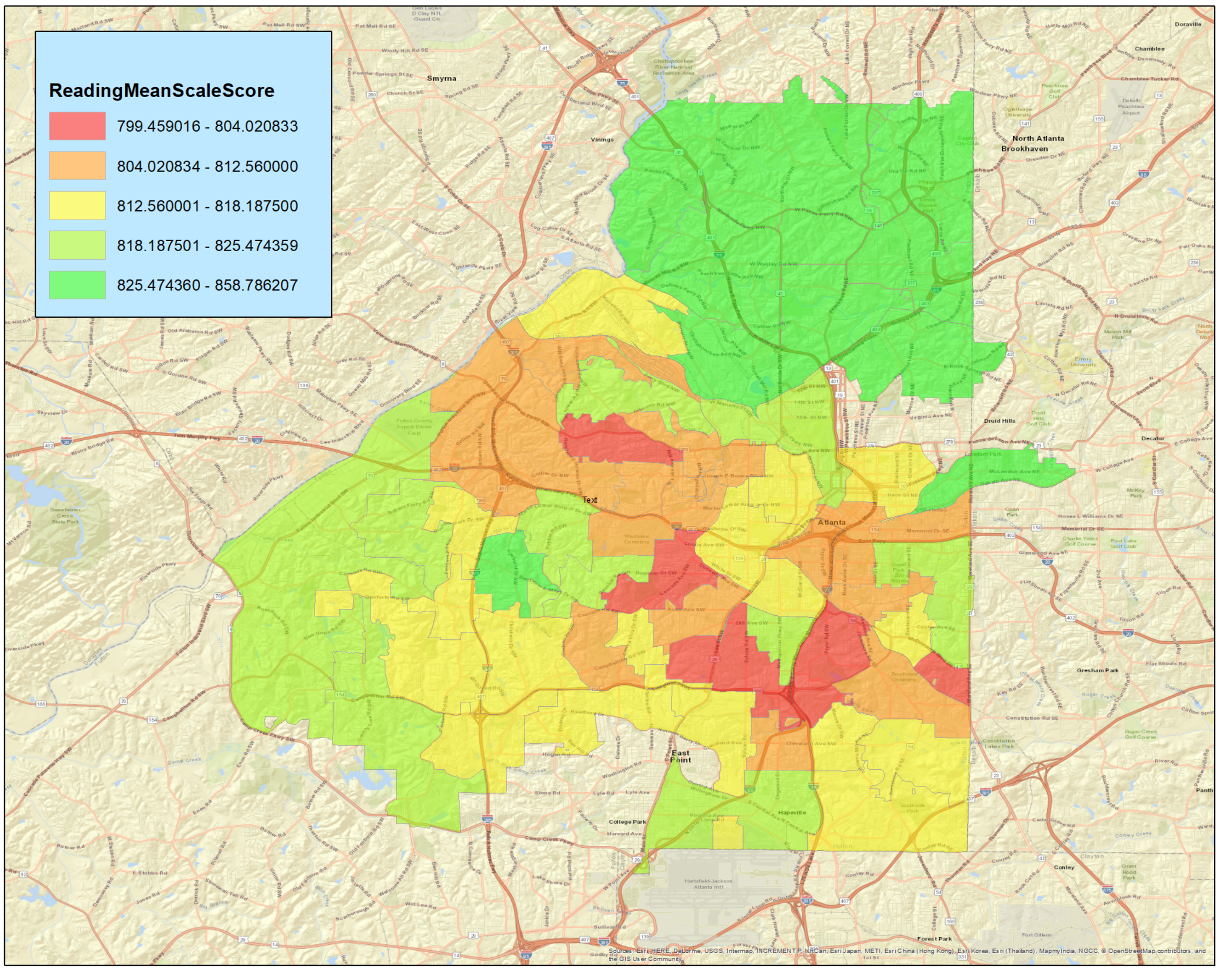


Figure 1.4: 5th Grade Reading Scores for Atlanta Elementary Schools

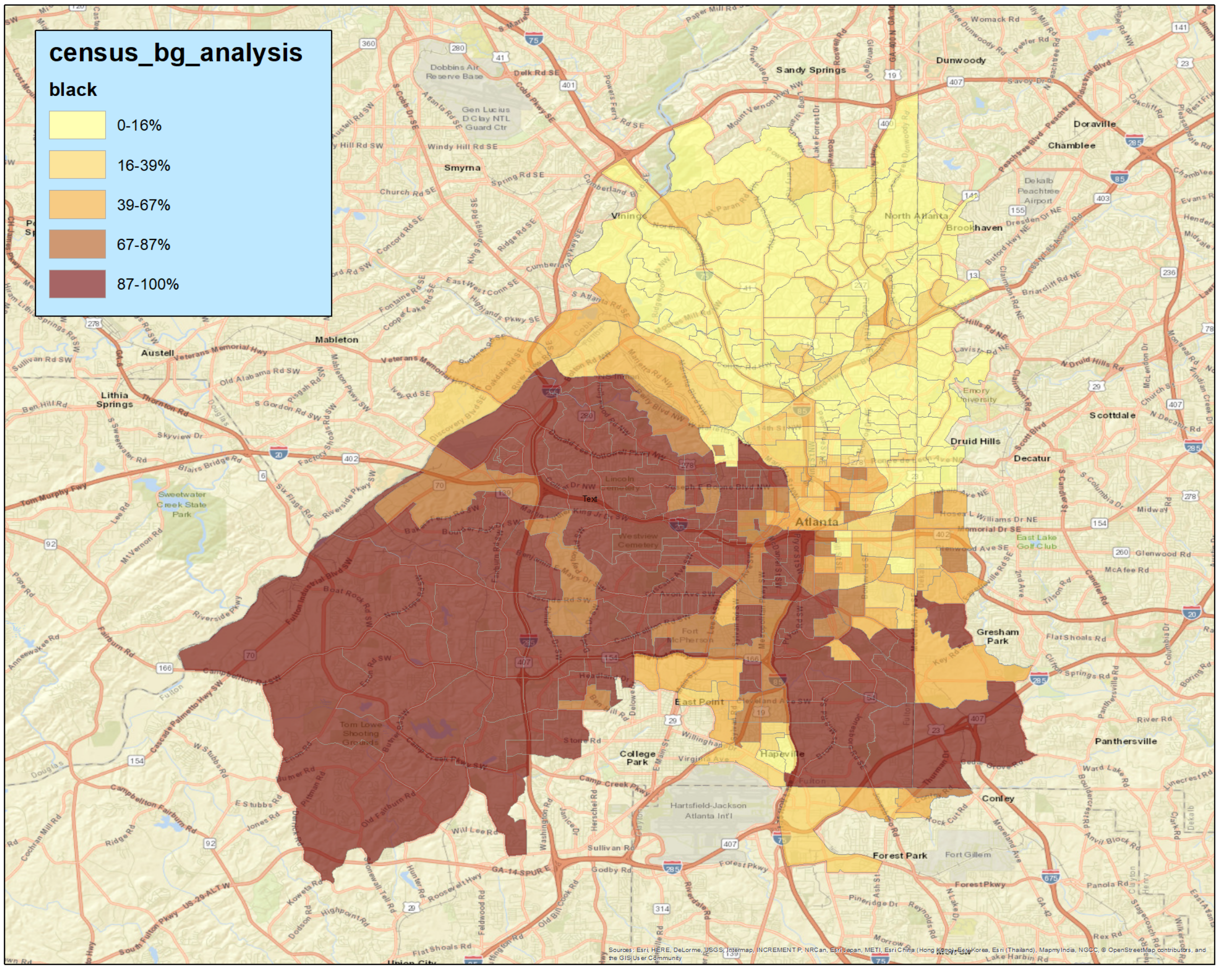


Figure 1.5: % Black, 2011 ACS Demographics

1.2.5 Census Demographics

I include demographic variables for each property based on the Census' 2011 5-year ACS estimates. I use block group level estimates and match each property to the block group in which it resides. The variables I use include percentage black population, percentage Hispanic population, per capita income, and population density. Values for the black population and per capita income variables are mapped in Figures 1.5 and 1.6.

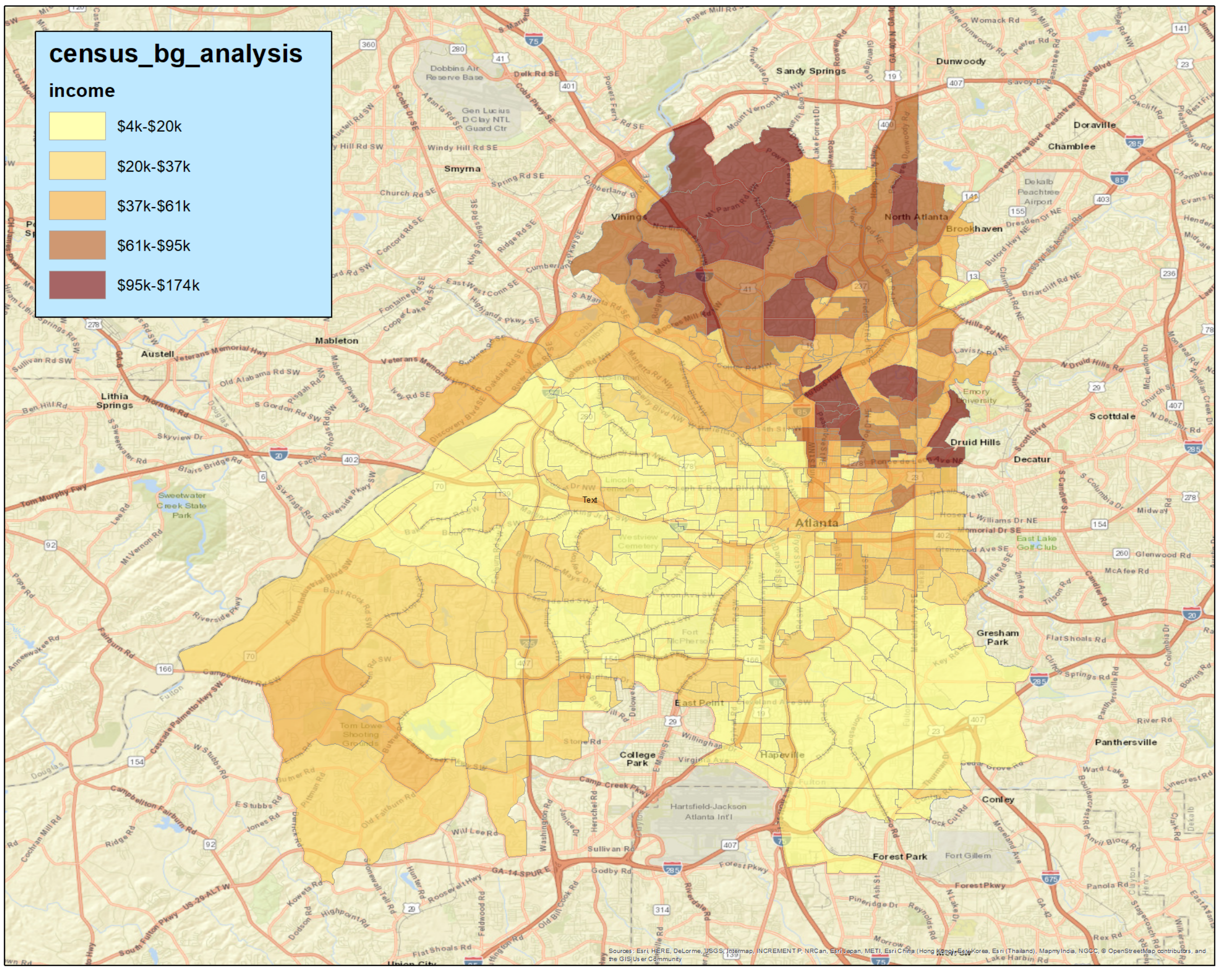


Figure 1.6: Per Capita Income, 2011 ACS Demographics

1.3 Methods

1.3.1 Hedonic Regressions

I estimate semi-log hedonic models of the form

$$log(p_i) = \alpha_t + \gamma X_i + \beta Z_i + \epsilon_i,$$

where p_i is the price observed for transaction i, α_t is a fixed effect for quarter-year t, X_i is a vector of home characteristics including square footage, lot size, number of bedrooms, number of bathrooms, and decade of construction, and Z_i is a vector of neighborhood characteristics which can include reading and math scores, crime rates, Census demographics, and the Place Pulse measures. In some specifications, I also control for unobserved spatial effects in the form of a second degree polynomial of latitude and longitude. This model specification permits interpretation of the regression coefficients as percentage changes in home values associated with a unit change in the neighborhood characteristics of interest. Model coefficients are estimated via Ordinary Least Squares regression, and variance estimation is performed via the Huber-White "sandwich" method.

1.3.2 Crime Forecasting

Crime is not constant over time, especially in a city such as Atlanta which experienced considerable growth and development during the 2010's, the decade my data is drawn from. Homebuyers likely are aware of this, and when they purchase a a home they are not purchasing a place to live given the current or past levels of crime, but rather given their expectations of future crime. Bishop and Murphy (2019) introduce a simple approach for estimating hedonic regressions based on forward-looking estimates of crime, and study the conditions under which it is important to consider such forward-looking behavior. I implement a version of their estimation approach which is appropriate and feasible given the data available in my application. The APD crime data begins in January 2009 and continues through 2019, so while analyzing transactions that occur between 2013 and 2015, I can use crime data from 2009 until the date of the sale to construct forecasts of expected

future crime from the time of sale until T years after the sale.[4]

Figures 1.7 and 1.8 illustrate the value of a forward-looking approach. In both figures, the dots plot the number of property crimes within a half mile radius of a home from 2009 to 2019, and the trend line shows a forecast based on the data from 2009 through 2014. Thus, the value of the trend line from 2015 on represents the expectations about future crime which may be held by homebuyers considering the two parcels in 2014. Both parcels see similar levels of crime in the year 2014, and so a myopic model of consumer behavior might treat them as essentially similar in regards to the crime rate. However, looking at the full history of crime makes it clear that the parcel in Figure 1.7 had been on a downward trend that could have been reasonably expected to continue, whereas the parcel in Figure 1.8 was on a flat trajectory. It is therefore appropriate to assign the former a lower value of expected future crime than the latter.

To construct these forecasts, I perform a least squares regression of the crime rate on a quadratic function of time, constrained to have a negative first derivative and a positive second derivative from 2009 until T years after the sale. These constraints reflect the typical pattern of crime rates in Atlanta during my sample period, while keeping the degrees of freedom low, in order to handle the bias-variance tradeoff given the need to construct forecasts with as little as five years of data. However, it is worth noting that for the few neighborhoods where crime has been increasing, the estimator collapses to the average of crime from 2009 until the year of the sale.

1.3.3 Place Pulse Elo Algorithm and Interpolation

To turn the votes from the Place Pulse dataset into measures, I use an algorithm known as the Elo system. This system estimates a scalar-valued 'score' for each image in the data, with respect to each of the 6 characteristics studied. It begins by assigning uniform Elo scores of 1200 to all images. Then, in a contest between two images with current scores Elo^0_{winner} and Elo^0_{loser}, we update:

[4]Following Bishop and Murphy (2019), I set $T = 7$, reflecting the fact that 7 years is the approximate average household tenure, which is the time period of interest if we assume consumers can re-optimize by moving to a new location.

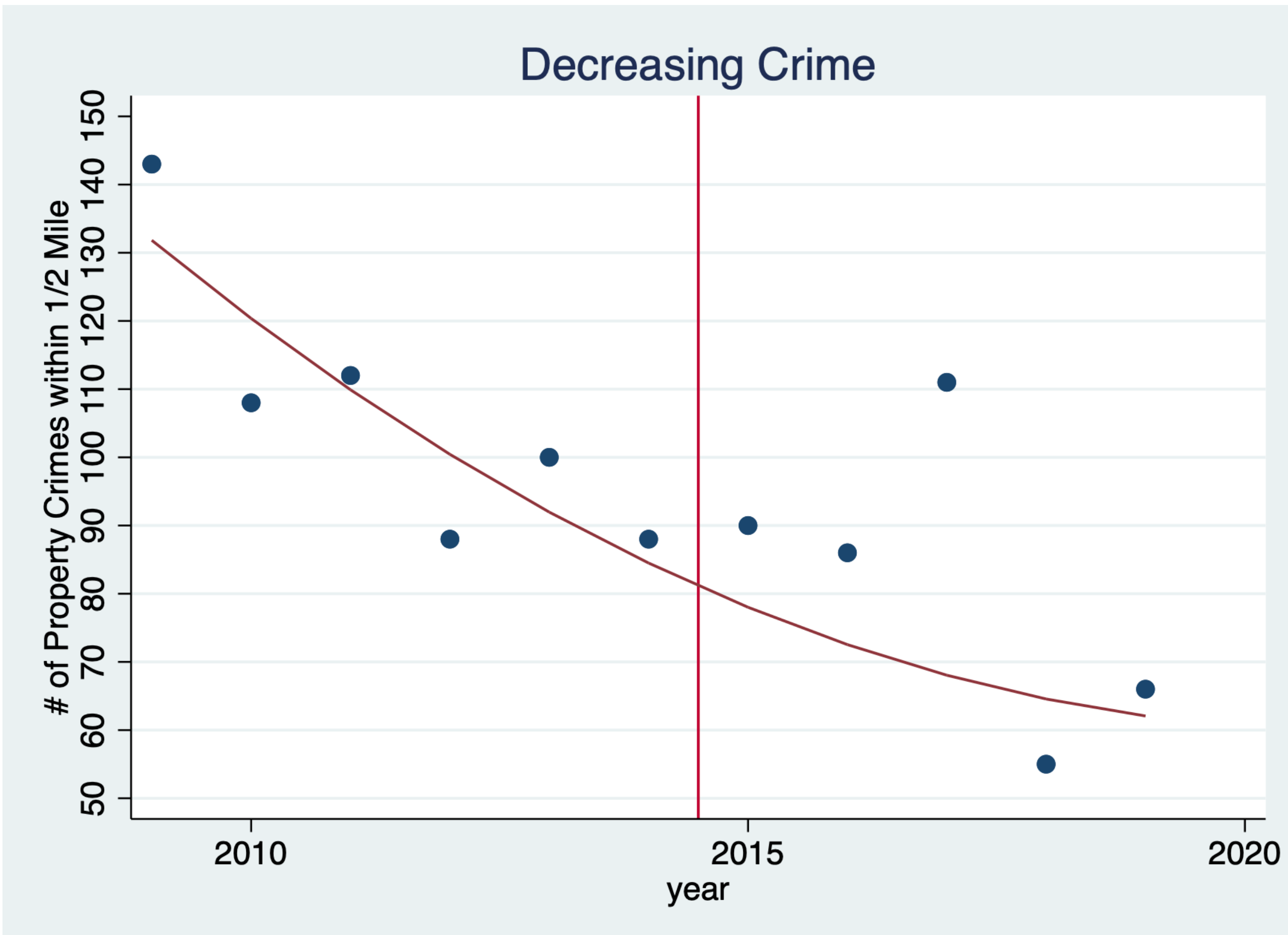


Figure 1.7: Property Crime Forecast With Decreasing Crime Rate

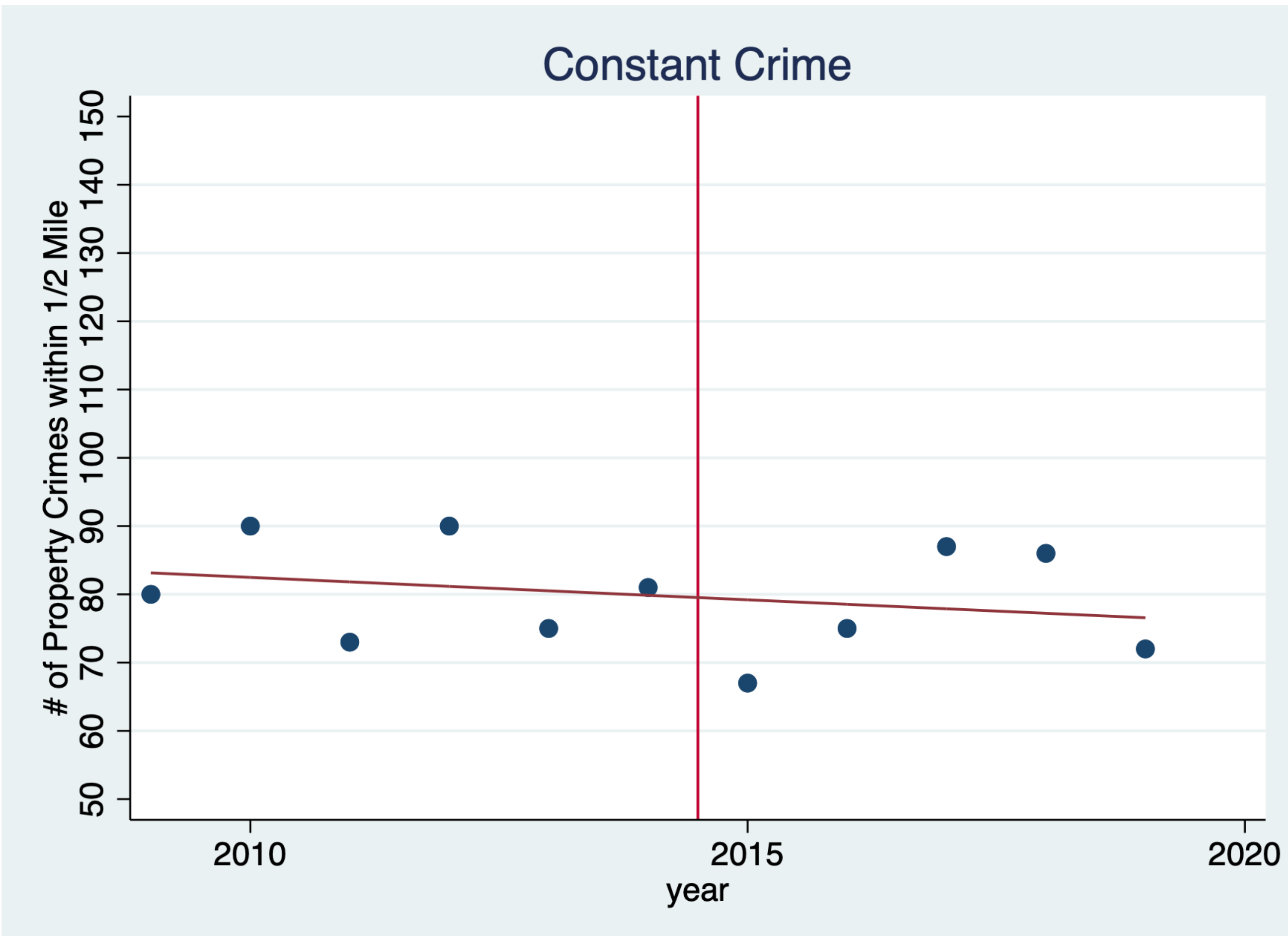


Figure 1.8: Property Crime Forecast With Constant Crime Rate

$$Elo^1_{winner} \leftarrow Elo^0_{winner} + K * f(Elo^0_{loser} - Elo^0_{winner})$$

$$Elo^1_{loser} \leftarrow Elo^0_{loser} - K * f(Elo^0_{loser} - Elo^0_{winner}),$$

where K represents the maximum number of points which can be won or lost in a single contest, and $f(x) = \frac{1}{1+e^{\frac{-x}{s}}}$ is the logistic cdf with scale parameter s. Intuitively, the number of points gained by the winner should be larger when it pulls off an upset than when it beats an easy opponent. Hence, when the loser has a much higher Elo score than the winner, $f(\cdot) \approx 1$, and the maximum number of points K are awarded to the winner and taken from the loser. When the winner is a heavy favorite, $f(\cdot) \approx 0$, and hardly any points are transferred.

The update rule form of the Elo system lends itself well to updating the rankings of players or teams as new match results come in. However, since I am interested in static rankings for my images based on the full length of the Place Pulse dataset, I use an algorithm which repeats the Elo update process many times to obtain a stationary distribution of Elo rankings. Specifically, the algorithm is:

1. Each image is assigned an initial Elo score of 1200
2. For each comparison in the dataset, the Elo scores of the participating images are updated according to the rule above.
3. The order of the comparisons is shuffled, and step 2 is repeated until the updated Elo scores converge to a stable distribution.

I calculate Elo scores separately for each of the 6 categories in the Place Pulse dataset, yielding 6 distinct measures of survey-taker's perceptions of places in Atlanta.

Once Elo scores are calculated for the images in the Place Pulse dataset, we must then assign Elo scores to the properties in the study area. This is an exercise in spatial interpolation. That is, we have a dataset $\{x_i, y_i, z_i\}_{i=1,\ldots,N}$ where i indexes images, x is the longitude of the image, y is the latitude of the image, and z is the estimated Elo value of the image, and we are interested in estimating a function $z = g(x, y) + \epsilon$ that captures the underlying spatial distribution of Elo score.

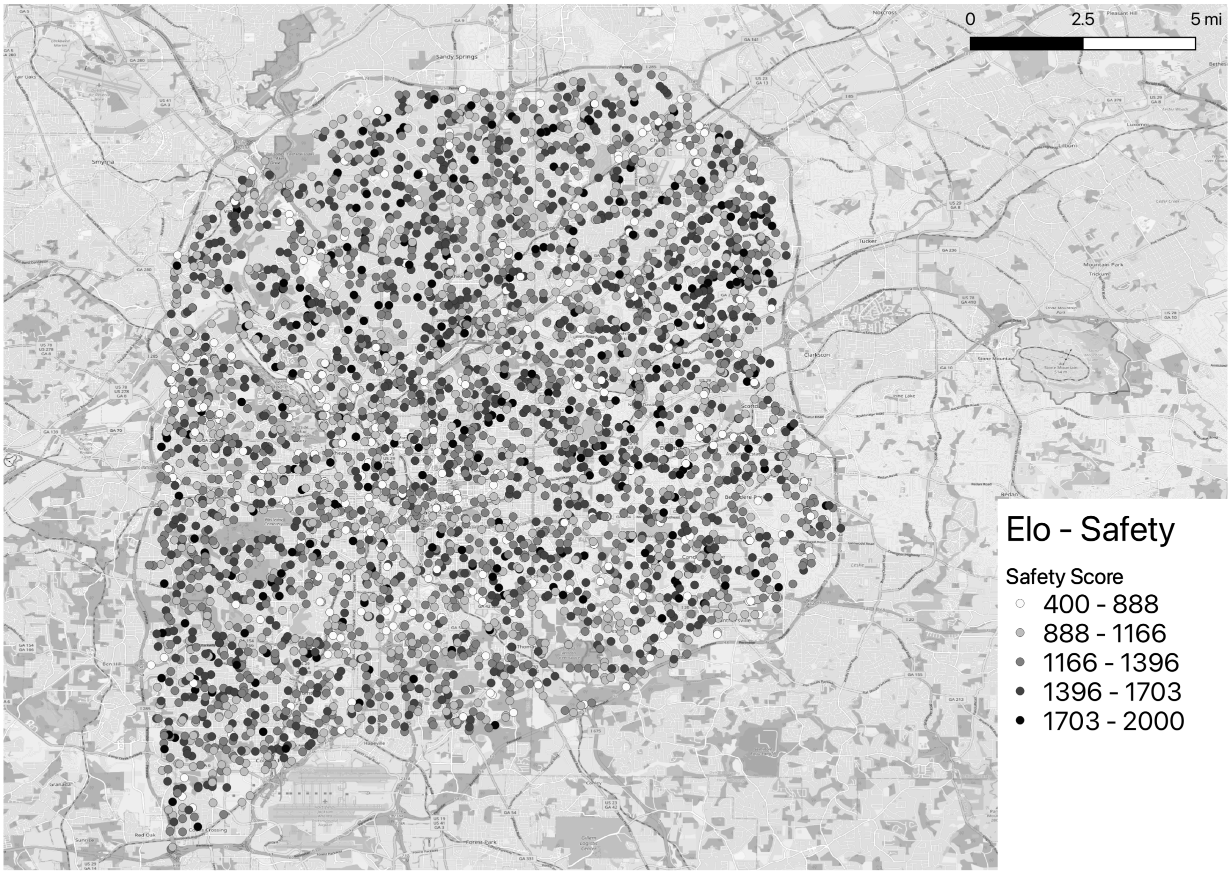


Figure 1.9: Safety Elo for Place Pulse Images in Atlanta

Our choice of method for estimating $\hat{g}$ reflects our assumptions about the data generating process. For example, if we believe that we have measured Elo with precision, we may desire an interpolation method that gives $z_i = \hat{g}(x_i, y_i), \forall i$, as is the case with inverse-distance-weighted nearest neighbors interpolation, or with any form of regression-based interpolation where the model is given enough degrees of freedom to fit the data perfectly. In our case, Elo scores have a highly variable spatial distribution, with many instances where an image in the bottom 10% of the Elo distribution lies close to a point in the top 10%. This can be seen in Figure 1.9, which shows the distribution of Safety Elo for the images in Atlanta. In this Figure, each dot is placed at the location of the Google Street View image used in the survey, and the color of the dot displays the decile of the image's rating.

If we believe that this distribution reflects meaningful intra-neighborhood variation in aesthetics, we may desire a technique such as inverse-distance-weighted nearest neighbors interpolation,

which would preserve this intra-neighborhood variation. But if we believe that this reflects the role of random noise in the data generating process, such as the possibility that the weather was gloomy when a certain area's Street View photo was taken, then we may prefer an interpolation method that smoothes out these random errors. To smooth out errors from the Elo estimation process, I choose a kernel smoothing regression as implemented by the STATA package `krls` (Ferwerda, Hainmueller, and Hazlett, 2015). [5]

Figures 1.10 through 1.15 present maps of the kernel smoothed version of each of our 6 Place Pulse measures, color coded by decile of the within-Atlanta smoothed Elo distribution. Comparing Figure 1.14 to Figure 1.9, it is much easier to see the overall spatial trend in perceived Safety in Figure 1.14. It is reassuring that the spatial distribution of perceived wealth in Figure 1.15 tracks well with the measured household income displayed in Figure 1.6. One particularly interesting comparison is between Figures 1.13 and 1.14, for the characteristics Lively and Safety. These characteristics have broadly similar spatial distributions, which is sensible in light of the traditional wisdom from urban studies and criminology that a more lively neighborhood has more eyes on the street, which deters crime (Jacobs, 1961). However, these characteristics differ informatively around the core downtown area and some of the neighborhoods Southwest of downtown. These areas are firmly in the bottom tercile of Safety, but in the middle tercile of Lively. This makes sense for a downtown area that sees a lot of commercial activity, but is also subject to a lot of crime, as demonstrated by Figure 1.1. An alternate method for transforming the Place Pulse voting data into scores for the images in our dataset would be to use a discrete choice model, such as a logistic regression. In this approach, each image in the dataset would have an associated dummy variable equal to 1 when considering a vote pertaining to that image. The estimated coefficients for each image could then be used in a manner similar to how the Elo scores are used here. The problem with this approach in my application is that I would need to perform a logistic regression

[5]This package differs from the "local-linear" regression methods such as that implemented by STATA's `lpoly` command in that it does not calculate multiple different linear regressions at different points in covariate space, but the regression surface estimated by krls still has smooth first derivatives. Additionally, `krls` allows for multiple regressors, which is needed in this cases as I use both longitude and latitude as predictors for the smoothed Elo scores, while `lpoly` allows only for a single regressor.

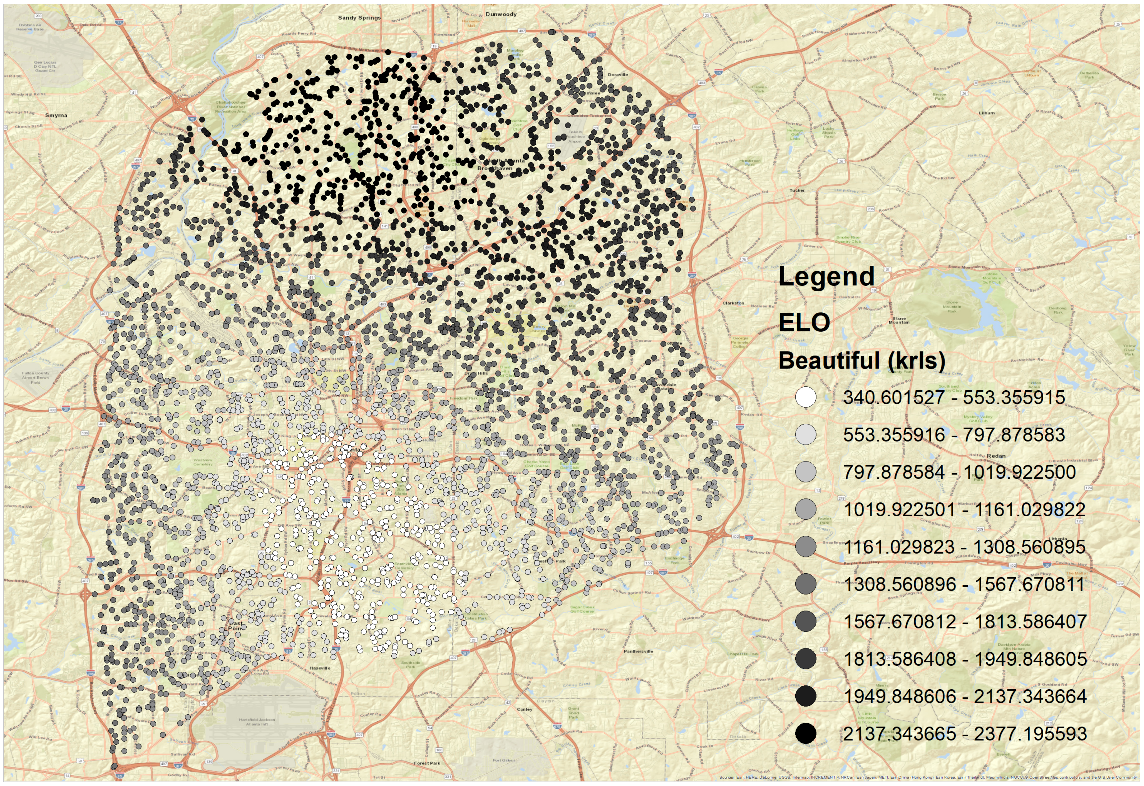


Figure 1.10: Kernel Smoothed Elo Scores - Beautiful

with 111,391 dummy variables.

1.4 Results

Before presenting the results of the hedonic regressions, I first present simple correlations and reverse regressions which show the Place Pulse measures and crime rates regressed on other amenity measures. These results provide intuition about the equilibrium relationship between the amenities studied, and the extent to which the crime variables are proxies for one another

Reverse Regressions and Correlations Table 1.5 presents pairwise correlation coefficients for a subset of the amenity variables, including (in the order the appear in the table): all NIBRS violent crimes within a half mile, all NIBRS property crimes within a half mile, SRS violent crimes, SRS property crimes, the census demographics, reading test scores, and Place Pulse Safety. The

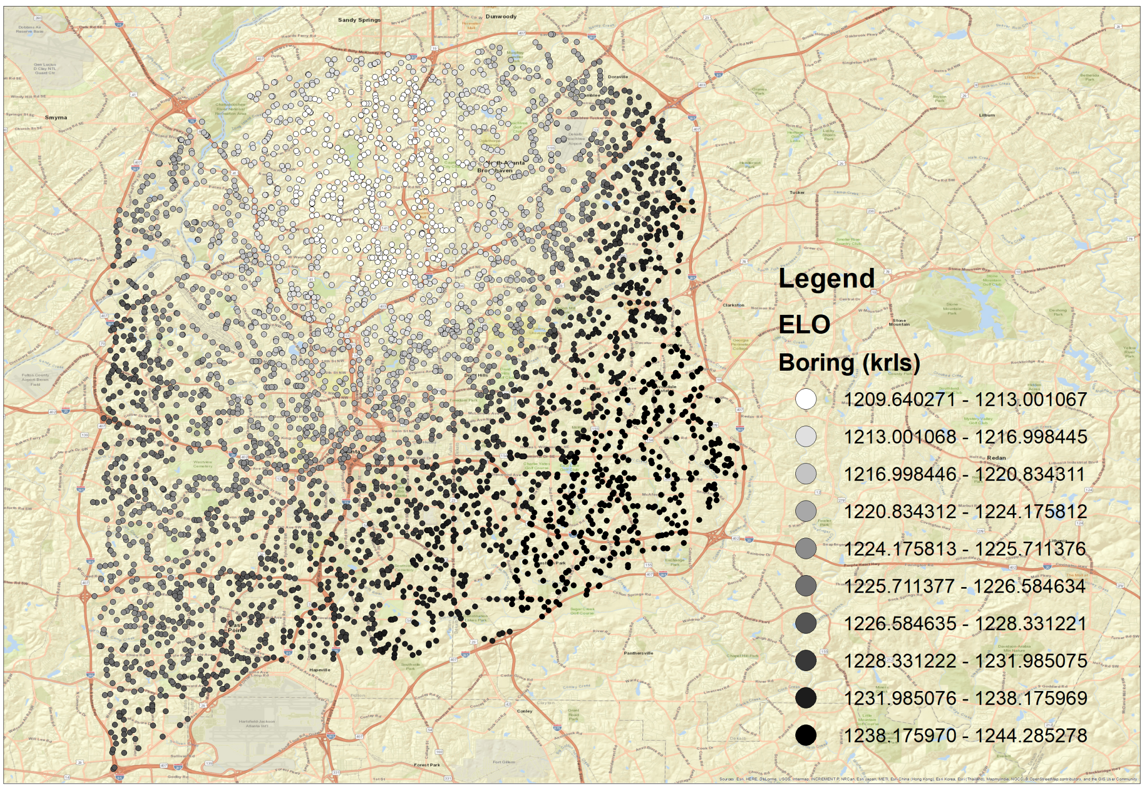


Figure 1.11: Kernel Smoothed Elo Scores - Boring

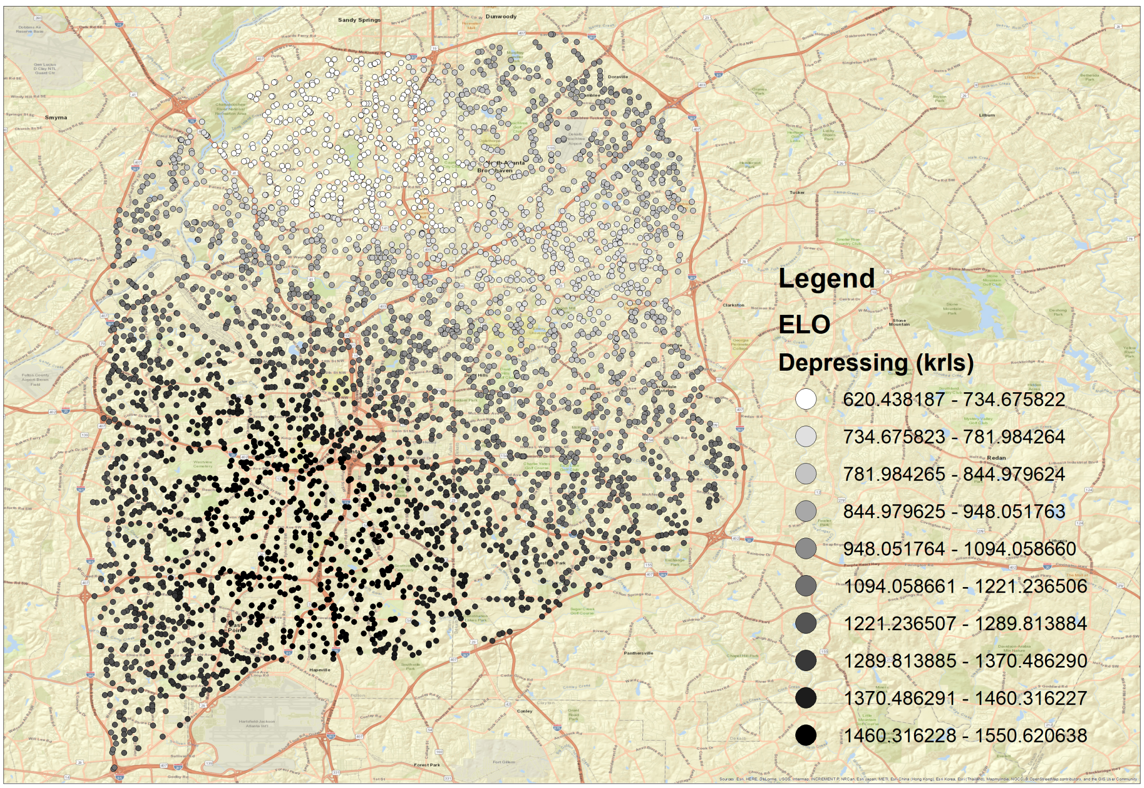


Figure 1.12: Kernel Smoothed Elo Scores - Depressing

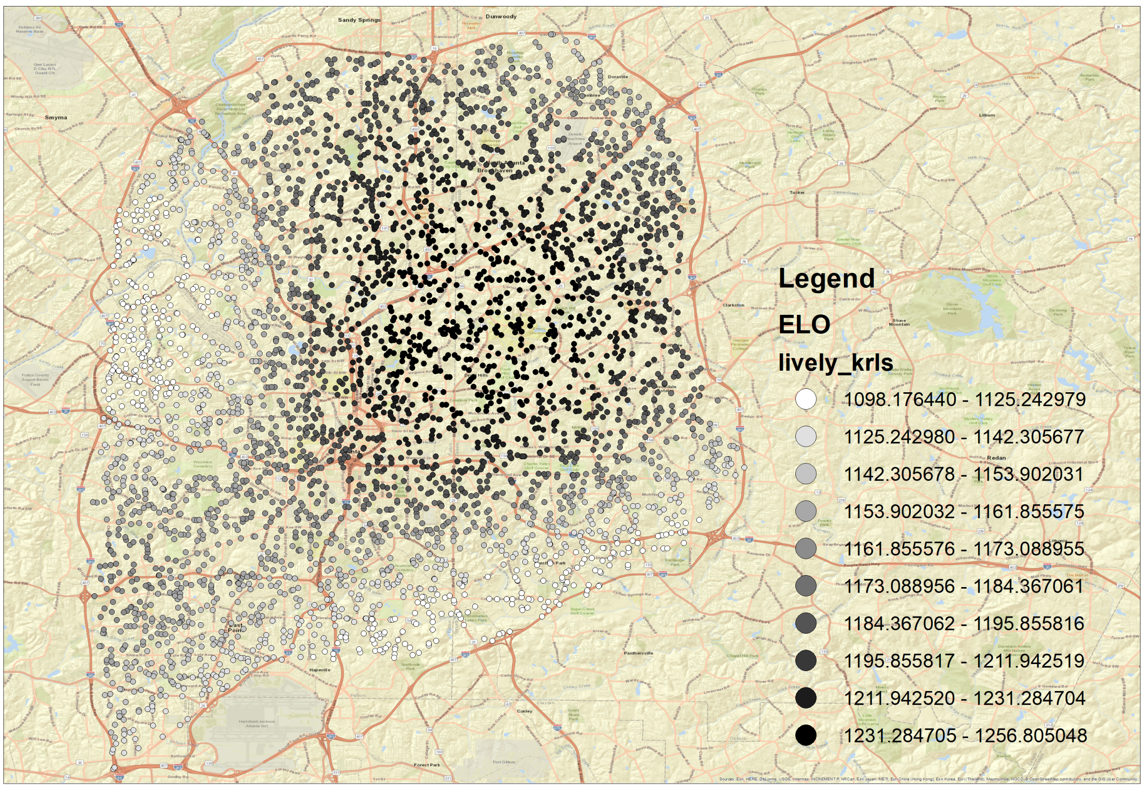


Figure 1.13: Kernel Smoothed Elo Scores - Lively

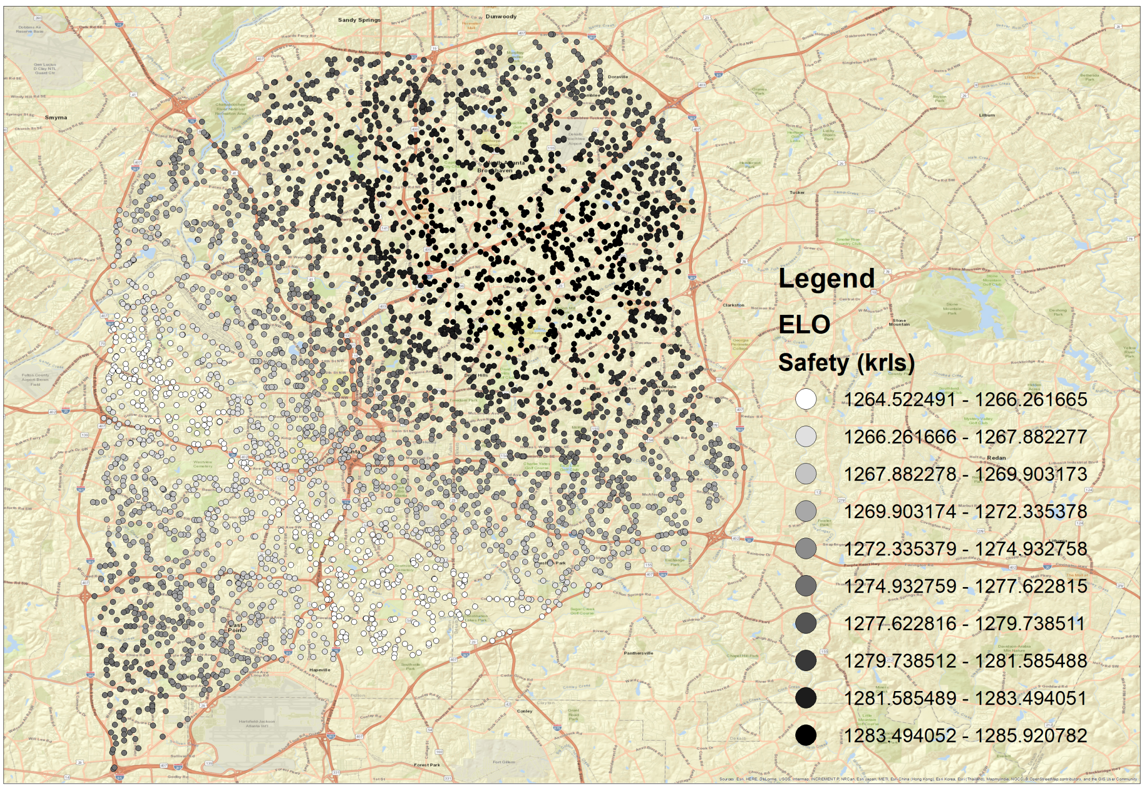


Figure 1.14: Kernel Smoothed Elo Scores - Safety

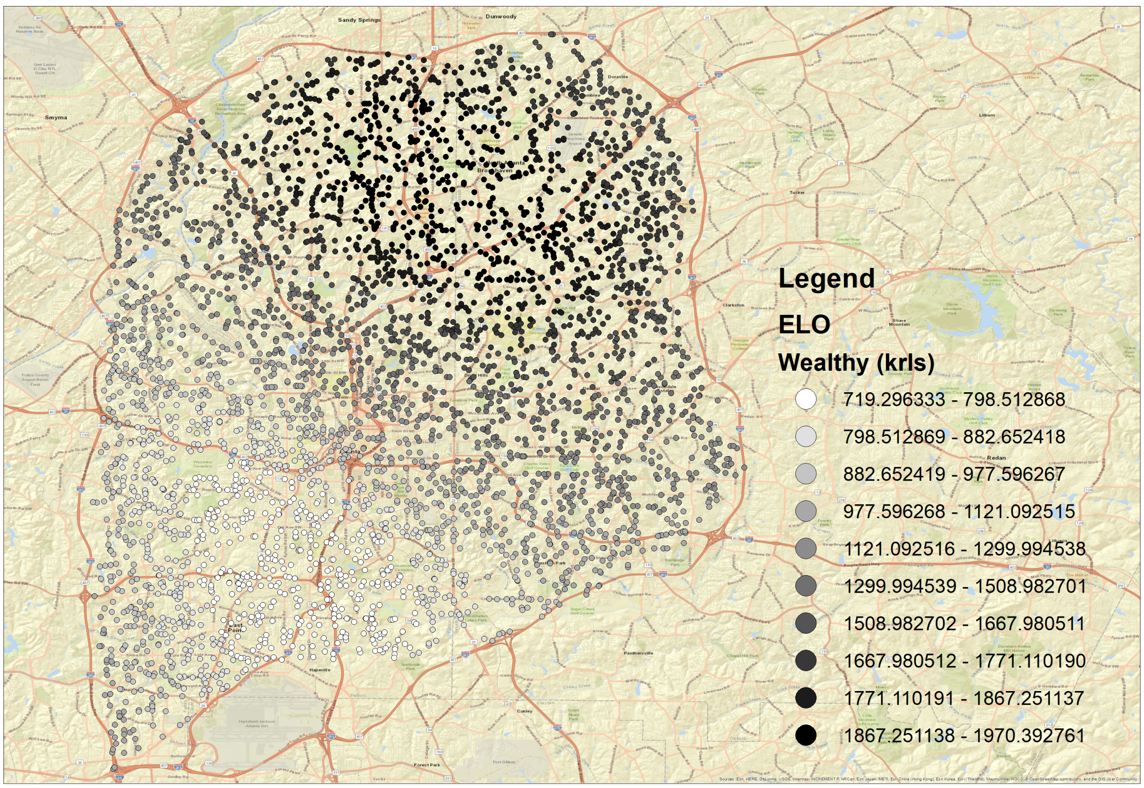


Figure 1.15: Kernel Smoothed Elo Scores - Wealthy

SRS and NIBRS measures of violent crime have a correlation of only 0.47, and the respective correlation for property crime is only 0.08. This suggest that the SRS is not a good proxy for the NIBRS. Both actually have negative correlations with the Place Pulse measure of safety, indicating that subjective measures - at least those based on photographs - are very different from data-driven objective measures. Comparing the first 2 columns we can see that NIBRS violent crimes are much more strongly correlated with the other amenity measures than are property crimes, which suggests that violent crimes are more systematically related to neighborhood quality than property crimes, as will also be seen in the hedonic regression results. Other correlations in the table are largely as expected, for instance that neighborhoods with higher income have better test scores and are perceived as safer.

In order to look in greater detail at the relationship among the amenity variables I study, Table 1.6 shows reverse regressions of the kernel-smoothed neighborhood aesthetics measure for Safety on the other amenity measures. In this table, the dependent variable has been standardized to have a mean of 0 and standard deviation of 1, in order to facilitate interpretability.

In Table 1.6, we see that, except when controlling for spatial effects with a polynomial of latitude and longitude, the R^2 is only about 0.6 or 0.7, suggesting additional information is added by . Moreover, even with spatial controls, columns (3) and (6) show that there is a strong association between perceived safety and all other amenities I measure. The relationships are in the expected directions: safer-looking neighborhoods are associated with higher-performing schools, lower proportions of black and hispanic residents and higher incomes, and lower levels of nearby crimes, with the exception of the most distant crimes in column (6). Peculiarly, there is a positive coefficient for distant (0.25-0.5 mile) crimes. This result recalls findings of a non-monotonic relationship between housing values and transit access (Bowes and Ihlanfeldt (2001)). One explanation for this result is that there tend to be strings of crime near commercial centers such as retail and entertainment developments. Being close to such developments is an amenity however, since people value the reduced travel time for shopping and entertainment trips, and so these areas may attract people who maintain safer looking neighborhoods.

Variables	VC	PC	SRS VC	SRS PC	% Black	% Hispanic	Income	Reading	Safety
VC	1.000								
PC	0.645	1.000							
SRS VC	0.469	0.088	1.000						
SRS PC	0.431	0.083	0.943	1.000					
% Black	0.660	0.219	0.721	0.732	1.000				
% Hispanic	-0.071	0.061	-0.185	-0.167	-0.220	1.000			
Income	-0.607	-0.353	-0.527	-0.559	-0.797	-0.098	1.000		
Reading	-0.611	-0.320	-0.590	-0.608	-0.819	-0.003	0.786	1.000	
Safety	-0.541	-0.190	-0.673	-0.652	-0.798	0.026	0.761	0.766	1.000

Table 1.5: Amenity Correlations

	(1)	(2)	(3)	(4)	(5)	(6)
Reading	0.0195***	0.0119***	0.00394***	0.0203***	0.0126***	0.00424***
	(0.00138)	(0.00121)	(0.000530)	(0.00135)	(0.00120)	(0.000524)
Math	0.0164***	0.00102	0.00243***	0.0143***	0.000706	0.00230***
	(0.00104)	(0.000948)	(0.000414)	(0.00102)	(0.000940)	(0.000409)
Crimes, 0-0.5 mi.	0.000310***	0.000561***	-0.0000272			
	(0.0000461)	(0.0000406)	(0.0000211)			
Crimes, 0-0.1 mi.				-0.0131***	-0.00655***	-0.00440***
				(0.000940)	(0.000835)	(0.000365)
Crimes, 0.1-0.25 mi.				-0.000628***	-0.0000116	-0.000142
				(0.000191)	(0.000167)	(0.0000725)
Crimes, 0.25-0.5 mi.				0.00103***	0.000952***	0.000128***
				(0.0000633)	(0.0000562)	(0.0000264)
% Black		-1.092***	-0.443***		-1.022***	-0.386***
		(0.0363)	(0.0188)		(0.0365)	(0.0191)
% Hispanic		-0.716***	-0.237***		-0.720***	-0.227***
		(0.117)	(0.0522)		(0.116)	(0.0516)
Income		0.00779***	0.00171***		0.00765***	0.00170***
		(0.000369)	(0.000176)		(0.000366)	(0.000174)
spatial effects	No	No	Yes	No	No	Yes
N	7758	7758	7758	7758	7758	7758
R^2	0.602	0.705	0.946	0.620	0.710	0.947

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.6: Safety Regressed on other Amenity Measures

Table 1.7 presents regressions of the number of crimes within a 0.5 mile radius on other amenities, including all 6 kernel-smoothed Place Pulse measures. The demographic and school measures look as expected here, with better schools having less crime, and higher hispanic and black populations and less money having more crime. Including all Place Pulse measures independently produces some predictable results, with more boring and more beautiful areas seeing less crime. Perhaps surprisingly, areas perceived as wealthier tend to have lower levels of crime, despite the greater potential upside to crime in visibly wealthier areas. The results for the coefficient on perceived safety are not as stable as the coefficients for the other characteristics. In columns (4) and (5), this coefficient is negative, as would be expected if perceptions of safety from the Place Pulse survey takers accurately reflect actual local safety. But including spatial effects in column (6) causes this sign to flip, which may indicate that these survey-based perception measures are not very good measures of actual local safety - an issue I consider further below in the discussion of the results of the hedonic regressions.

It is worth considering what the results of these reverse regressions mean for the hedonic regressions which will follow. To this end, we may look at the relative predictive power of the various regression models. The models in Table 1.6 have substantially higher R^2 values than do the models with crime as the dependent variable in Table 1.7. This tells us that the NIBRS crime data is especially information-rich, in that it retains substantial independent variation even after conditioning on all of the other amenities which will be included in the hedonic regressions. This suggests that the neighborhood perception measures would be poor proxies for the actual local crime densities in contexts where these data are not available.

Hedonic Regressions Tables 1.8 through 1.13 present the results of hedonic housing regressions under a variety of specifications, all using the log of the sales price as the dependent variable. All specifications include at least one measure of crime, along with property characteristics and other neighborhood amenities. Each table uses a different measure of crime: Table 1.8 relies on the Inverse-Distance-Weighted FBI crime data, Table 1.9 uses all APD crimes within a half mile

	(1)	(2)	(3)	(4)	(5)	(6)
Reading	-3.162***	-2.546***	-2.641***	-4.679***	-3.896***	-5.505***
	(0.339)	(0.337)	(0.284)	(0.285)	(0.283)	(0.285)
Math	-0.956***	-0.301	0.0627	0.756***	0.959***	2.254***
	(0.255)	(0.265)	(0.223)	(0.216)	(0.215)	(0.219)
Wealthy				-0.182*	-0.365***	-0.270
				(0.0722)	(0.0715)	(0.206)
Safety				-29.38***	-29.01***	41.67***
				(3.194)	(3.190)	(7.635)
Lively				3.994***	4.050***	1.295*
				(0.268)	(0.269)	(0.516)
Depressing				-0.590***	-1.117***	0.0272
				(0.111)	(0.115)	(0.237)
Boring				-22.24***	-27.84***	-15.60***
				(1.419)	(1.446)	(4.363)
Beautiful				-0.291***	-0.428***	-0.653***
				(0.0341)	(0.0364)	(0.0677)
% Black		-70.28***	112.7***		69.50***	99.11***
		(10.11)	(10.06)		(9.929)	(10.27)
% Hispanic		-163.8***	247.4***		216.3***	159.6***
		(32.64)	(27.98)		(26.38)	(25.90)
Income		-1.947***	-0.718***		-0.967***	-0.941***
		(0.101)	(0.0943)		(0.0882)	(0.0894)
spatial effects	No	No	Yes	No	No	Yes
N	7758	7758	7758	7758	7758	7758
R^2	0.234	0.270	0.502	0.533	0.552	0.580

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.7: Crime (0-0.5 miles) Regressed on other Amenity Measures

of the home, Table 1.10 uses the ring-based measures of both property and violent APD crimes, and Table 1.11 uses a forward-looking projection of property crime within a half mile. In each table, 4 columns are presented, each showing the results from a different regression specification: specification (1) includes the property characteristics, the measure of crime for the given table, 5th grade reading and writing test scores, proportion of black and hispanic population, per capita income, and quarter by year fixed effects to non-parametrically control for time trends; specification (2) adds non-parametric spatial effects through a 2nd degree polynomial of longitude and latitude; specification (3) adds the Place Pulse survey measures; and (4) adds both the non-parametric spatial effects and the Place Pulse measures. The coefficients of parcel characteristics, spatial effects, and quarter-by-year fixed effects have been suppressed to focus on the estimates of interest.

To interpret the coefficients on crime, consider first Table 1.8, for the SRS crime measure. In column (1), we see a coefficient of -0.0007 on violent crime. This means that a 1 unit increase in the annual rate of violent crimes per 100,000 people is associated with a 0.07% decrease in home values. The standard deviation of this crime measure is 228, so this translates to a 16% decrease in home values for a 1 standard deviation increase in crime. Evaluated at the mean home price of $420,713, this is a $67,000 willingness to pay for a 1 SD reduction in the crime rate. Notably, using the smaller coefficient from column (4) gives the willingness to pay as only $31,500.

We can similarly calculate the willingness to pay for crime reductions using the coefficients from the NIBRS data in Table 1.10. In column (1), the coefficient on crimes 0-0.1 miles away is -0.04, the coefficient for crimes 0.1-0.25 miles is -0.01, and the coefficient for crimes 0.25-0.5 miles is -0.008. Multiplying these coefficients by the relevant standard deviations of 2, 8 and 22 gives percentage changes of 8%, 8%, and 17.6% reductions in home values associated with a 1 standard deviation increase in the various crime densities. At the mean home value of $420,713, this translates to a $33,600 willingness to pay for a 1 SD decrease in crimes closer to the home (either 0-0.1 or 0.1-0.25 miles), or a $74,000 willingness to pay for a 1 SD decrease in crimes farther from the home (0.25-0.5). Interpretation of these facts should bear in mind that the absolute number of crimes is far larger for the crimes farther from the home, owing to the much greater area

	(1)	(2)	(3)	(4)
FBI Violent Crime	-0.000705***	-0.000383***	-0.000556***	-0.000329**
	(0.0000500)	(0.0000904)	(0.0000846)	(0.000101)
Reading	-0.00141	-0.00480***	-0.0112***	-0.0138***
	(0.00118)	(0.00119)	(0.00122)	(0.00126)
Math	0.00868***	0.0104***	0.0124***	0.0146***
	(0.00105)	(0.00103)	(0.00105)	(0.00108)
% Black	-1.770***	-1.837***	-1.811***	-1.733***
	(0.0446)	(0.0499)	(0.0515)	(0.0533)
% Hispanic	-0.819***	-0.759***	-0.748***	-0.766***
	(0.112)	(0.114)	(0.110)	(0.110)
Income	0.00296***	0.00323***	0.00183***	0.00211***
	(0.000336)	(0.000359)	(0.000358)	(0.000354)
Wealthy			-0.00355***	-0.00559***
			(0.000337)	(0.000866)
Safety			0.0230	0.193***
			(0.0157)	(0.0337)
Lively			0.00820***	-0.00261
			(0.00134)	(0.00257)
Depressing			-0.00458***	-0.00553***
			(0.000503)	(0.000683)
Boring			-0.0224**	-0.0336**
			(0.00694)	(0.0122)
Beautiful			-0.000278	-0.00128***
			(0.000178)	(0.000231)
property characteristics	Yes	Yes	Yes	Yes
spatial effects	No	Yes	No	Yes
quarter-year fixed effects	Yes	Yes	Yes	Yes
N	7758	7758	7758	7758
R^2	0.839	0.845	0.851	0.852

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.8: Hedonic Regressions Using SRS Violent Crime

	(1)	(2)	(3)	(4)
Crimes, 0-0.5 mi.	0.000114*	-0.0000618	-0.000186***	-0.000281***
	(0.0000469)	(0.0000490)	(0.0000496)	(0.0000506)
Reading	0.000340	-0.00551***	-0.0122***	-0.0157***
	(0.00120)	(0.00118)	(0.00125)	(0.00128)
Math	0.00826***	0.0109***	0.0131***	0.0156***
	(0.00108)	(0.00102)	(0.00106)	(0.00107)
% Black	-2.065***	-1.862***	-1.834***	-1.697***
	(0.0389)	(0.0489)	(0.0504)	(0.0536)
% Hispanic	-0.794***	-0.750***	-0.736***	-0.714***
	(0.115)	(0.114)	(0.110)	(0.110)
Income	0.00298***	0.00318***	0.00144***	0.00193***
	(0.000342)	(0.000362)	(0.000363)	(0.000355)
Wealthy			-0.00273***	-0.00589***
			(0.000315)	(0.000857)
Safety			0.00662	0.229***
			(0.0164)	(0.0322)
Lively			0.00948***	-0.00467
			(0.00139)	(0.00240)
Depressing			-0.00483***	-0.00616***
			(0.000516)	(0.000678)
Boring			-0.00754	-0.0364**
			(0.00652)	(0.0123)
Beautiful			-0.000560**	-0.00176***
			(0.000180)	(0.000230)
property characteristics	Yes	Yes	Yes	Yes
spatial effects	No	Yes	No	Yes
quarter-year fixed effects	Yes	Yes	Yes	Yes
N	7758	7758	7758	7758
R^2	0.833	0.844	0.850	0.852

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.9: Hedonic Regressions Using APD Crime Data Aggregated to 0.5 Mile Radius

	(1)	(2)	(3)	(4)
Violent, 0-0.1 miles	-0.0409***	-0.0428***	-0.0415***	-0.0410***
	(0.00553)	(0.00551)	(0.00542)	(0.00548)
Violent, 0.1-0.25 miles	-0.0103***	-0.0100***	-0.00983***	-0.00975***
	(0.00184)	(0.00176)	(0.00168)	(0.00168)
Violent, 0.25-0.5 miles	-0.00785***	-0.00803***	-0.00814***	-0.00843***
	(0.000645)	(0.000642)	(0.000625)	(0.000634)
Property, 0-0.1 miles	0.00537***	0.00506***	0.00422***	0.00356**
	(0.00126)	(0.00123)	(0.00126)	(0.00129)
Property, 0.1-0.25 miles	0.00103***	0.000955***	0.000813***	0.000638**
	(0.000267)	(0.000243)	(0.000223)	(0.000223)
Property, 0.25-0.5 miles	0.00128***	0.000984***	0.000843***	0.000738***
	(0.0000903)	(0.0000859)	(0.0000806)	(0.0000791)
Reading	-0.000683	-0.00491***	-0.0109***	-0.0145***
	(0.00118)	(0.00117)	(0.00121)	(0.00125)
Math	0.00944***	0.0110***	0.0130***	0.0156***
	(0.00104)	(0.00101)	(0.00102)	(0.00105)
% Black	-1.572***	-1.339***	-1.325***	-1.251***
	(0.0427)	(0.0528)	(0.0533)	(0.0546)
% Hispanic	-0.606***	-0.397***	-0.395***	-0.441***
	(0.114)	(0.113)	(0.108)	(0.109)
Income	0.00364***	0.00340***	0.00216***	0.00214***
	(0.000336)	(0.000350)	(0.000352)	(0.000344)
Wealthy			-0.00211***	-0.00361***
			(0.000314)	(0.000845)
Safety			-0.0151	0.154***
			(0.0160)	(0.0317)
Lively			0.00971***	0.000881
			(0.00134)	(0.00236)
Depressing			-0.00445***	-0.00492***
			(0.000506)	(0.000661)
Boring			-0.0200**	-0.0238*
			(0.00630)	(0.0119)
Beautiful			-0.000746***	-0.00198***
			(0.000173)	(0.000220)
property characteristics	Yes	Yes	Yes	Yes
spatial effects	No	Yes	No	Yes
quarter-year fixed effects	Yes	Yes	Yes	Yes
N	7758	7758	7758	7758
R^2	0.848	0.857	0.862	0.864

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.10: Hedonic Regressions Using Disaggregated APD Crime Data

	(1)	(2)	(3)	(4)
Property, 0-0.5 miles	0.000164*	0.000106	-0.0000244	-0.0000749
	(0.0000649)	(0.0000642)	(0.0000613)	(0.0000637)
Reading	-0.000305	-0.00721***	-0.0155***	-0.0176***
	(0.00133)	(0.00129)	(0.00133)	(0.00140)
Math	0.00895***	0.0110***	0.0136***	0.0154***
	(0.00127)	(0.00119)	(0.00119)	(0.00125)
% Black	-2.139***	-1.886***	-1.622***	-1.577***
	(0.0449)	(0.0564)	(0.0605)	(0.0613)
% Hispanic	-0.771***	-0.749***	-0.500***	-0.483***
	(0.129)	(0.130)	(0.126)	(0.124)
Income	0.00178***	0.00326***	0.00300***	0.00295***
	(0.000445)	(0.000464)	(0.000463)	(0.000452)
Wealthy			-0.00334***	-0.00820***
			(0.000351)	(0.00124)
Safety			-0.00825	0.168***
			(0.0160)	(0.0377)
Lively			0.0152***	0.00365
			(0.00143)	(0.00294)
Depressing			-0.00573***	-0.00839***
			(0.000611)	(0.000887)
Boring			-0.00297	-0.0640**
			(0.00695)	(0.0195)
Beautiful			-0.000528**	-0.00112***
			(0.000188)	(0.000266)
property characteristics	Yes	Yes	Yes	Yes
spatial effects	No	Yes	No	Yes
quarter-year fiexed effects	Yes	Yes	Yes	Yes
N	7758	7758	7758	7758
R^2	0.827	0.838	0.847	0.848

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.11: Hedonic Regressions Using Forward-Looking Crime

	(1)	(2)	(3)	(4)
Crimes, 0-0.5 mi.	0.0000869	-0.0000893	-0.000176***	-0.000266***
	(0.0000501)	(0.0000518)	(0.0000533)	(0.0000545)
Reading	-0.000306	-0.00762***	-0.0122***	-0.0159***
	(0.00126)	(0.00124)	(0.00133)	(0.00136)
Math	0.00772***	0.0117***	0.0129***	0.0153***
	(0.00113)	(0.00109)	(0.00115)	(0.00115)
% Black	-2.121***	-1.867***	-1.865***	-1.749***
	(0.0410)	(0.0544)	(0.0554)	(0.0586)
% Hispanic	-0.665***	-0.672***	-0.697***	-0.651***
	(0.151)	(0.145)	(0.142)	(0.140)
Income	0.00251***	0.00288***	0.000862*	0.00158***
	(0.000340)	(0.000347)	(0.000361)	(0.000354)
Wealthy			-0.00252***	-0.00390***
			(0.000394)	(0.00110)
Safety			-0.000776	0.192***
			(0.0207)	(0.0396)
Lively			0.0102***	-0.00157
			(0.00171)	(0.00292)
Depressing			-0.00447***	-0.00436***
			(0.000628)	(0.000862)
Boring			-0.000787	-0.000780
			(0.00789)	(0.0162)
Beautiful			-0.000420	-0.00153***
			(0.000216)	(0.000272)
property characteristics	Yes	Yes	Yes	Yes
spatial effects	No	Yes	No	Yes
N	7150	7150	7150	7150
R^2	0.816	0.829	0.832	0.835

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.12: Hedonic Regressions Dropping Parcels with less than 80% of Land in a 0.5-Mile Radius within APD Jurisdiction, Using All APD Crimes 0-0.5 Miles

	(1)	(2)	(3)	(4)
Violent, 0-0.1 miles	-0.0433***	-0.0441***	-0.0436***	-0.0435***
	(0.00580)	(0.00577)	(0.00568)	(0.00573)
Violent, 0.1-0.25 miles	-0.0103***	-0.00970***	-0.00966***	-0.00948***
	(0.00192)	(0.00182)	(0.00175)	(0.00175)
Violent, 0.25-0.5 miles	-0.00837***	-0.00840***	-0.00876***	-0.00893***
	(0.000679)	(0.000670)	(0.000664)	(0.000672)
Property, 0-0.1 miles	0.00551***	0.00497***	0.00386**	0.00341*
	(0.00131)	(0.00127)	(0.00133)	(0.00135)
Property, 0.1-0.25 miles	0.00122***	0.00106***	0.000916***	0.000766***
	(0.000282)	(0.000251)	(0.000231)	(0.000231)
Property, 0.25-0.5 miles	0.00129***	0.000948***	0.000877***	0.000776***
	(0.0000931)	(0.0000869)	(0.0000820)	(0.0000816)
Reading	-0.00166	-0.00694***	-0.0117***	-0.0150***
	(0.00120)	(0.00118)	(0.00124)	(0.00128)
Math	0.00934***	0.0121***	0.0136***	0.0157***
	(0.00106)	(0.00102)	(0.00105)	(0.00106)
% Black	-1.627***	-1.312***	-1.315***	-1.276***
	(0.0441)	(0.0567)	(0.0564)	(0.0575)
% Hispanic	-0.644***	-0.525***	-0.537***	-0.543***
	(0.140)	(0.135)	(0.131)	(0.131)
Income	0.00320***	0.00308***	0.00154***	0.00171***
	(0.000330)	(0.000338)	(0.000344)	(0.000343)
Wealthy			-0.00184***	-0.00160
			(0.000384)	(0.00104)
Safety			-0.0473*	0.0865*
			(0.0197)	(0.0373)
Lively			0.0123***	0.00623*
			(0.00161)	(0.00273)
Depressing			-0.00451***	-0.00366***
			(0.000598)	(0.000810)
Boring			-0.0150*	0.00873
			(0.00742)	(0.0150)
Beautiful			-0.000675***	-0.00178***
			(0.000199)	(0.000249)
property characteristics	Yes	Yes	Yes	Yes
spatial effects	No	Yes	No	Yes
quarter-year fixed effects	Yes	Yes	Yes	Yes
N	7150	7150	7150	7150
R^2	0.848	0.858	0.862	0.864

Standard errors in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Table 1.13: Hedonic Regressions Dropping Parcels with less than 80% of Land in a 0.5-Mile Radius within APD Jurisdiction, Using Disaggregated APD Crime Data

covered, and it is still the case the the coefficients on crimes close to the home imply a much greater willingness to pay per crime for closer crimes.

Comparing tables 1.8, 1.9 and 1.10, we see that it is only table 1.10, making full use of the fine spatial resolution of the APD/NIBRS crime micro-data, which has stable point estimates for the crime - home price relationship, especially for violent crimes. For violent crime, the largest deviation in coefficient estimates across the 4 specifications is for the estimate on violent crimes a quarter to a half mile from the home, which has a minimum estimate of -0.00785 in specification (1), and maximum of -0.00843 in specification (4), a 7% difference in the estimated coefficients. In general, for both property and violent crime coefficient estimates in Table 1.10, it is the case that the coefficients on crimes farther from the home are less stable across specifications. For the property crime coefficient on crimes a quarter to a half mile from the home, there is substantially greater variation, from a 0.00128 estimate in specification (1) to 0.000638 in specification (4), a difference of 100%. This is more comparable with the variation in estimates from Tables 1.8 and 1.9, where in Table 1.8 the coefficient on FBI Violent Crime varies from a low of -0.000324 in specification (4) to -0.000682 in specification (1), indicating the inclusion of spatial effects and the Place Pulse survey measures can cause this estimated relationship to vary by over 100%. In Table 1.9, using the APD data but at a high level of aggregation so that all crimes within a half mile of the home are counted together, the coefficient actually flips signs between specifications (1) and (4). This shows how measurement error in measuring crime can substantially reduce the robustness of the estimated price of public safety.

Table 1.10 notably shows positive coefficients for the property crime - housing price relationship. The clearest interpretation of this fact is that many reported property crimes occur at commercial or retail locations, parking lots and roads, and thus these estimates confound the disamenity value of property crimes with the amenity value of being located close to shopping, offices, and other commercial opportunities. To be specific, there are a total of 104,539 property crimes in the data. 67,256, or 64%, of these are larcenies. Only 13,002 of those larcenies occur at a residential location, meaning the remaining 54,254 larcenies, or a majority of all property crimes, occur at

retail/commercial locations, in parking lots, etc.

Table 1.11 presents the results of a model specification using forward-looking projections of crime at the time of purchase. While this approach should more closely reflect the actual decision-making process of homebuyers, the results as presented are currently limited by the use of aggregated crime data, using all property crimes within a half mile of the home as the forecasted variable. Future research should attempt to implement this approach with the disaggregated crime data used for the results in Table 1.10.

Furthermore, columns (3) and (4) of each regression table show consistency with Glaeser, Kincaid, and Naik (2018) in that while these characteristics based on neighborhood aesthetics have a large relationship with property values, they do not explain much of the residual variation in home values after conditioning on basic structural and location variables, as reflected by the small 1.7% increase in R^2 when the Place Pulse measures are included in table 1.10.

Regarding the estimated coefficients for other amenities included in the models, I find that there is consistently a positive coefficient on math test scores and a negative coefficient on reading test scores. In specifications without math scores, the coefficient on reading scores is positive. The coefficients on proportion of black and hispanic populations are consistently negative, while the coefficients on income are consistently positive. Coefficients on the Place Pulse measures are consistently negative for the Wealthy, Depressing, Boring and Beautiful characteristics, while the Safety and Lively characteristics flip signs across some specifications but have positive signs more often than not. While it may be possible to interpret these results in a clever fashion (ie. the counter-intuitive negative coefficient on Wealthy may be explained by arguing that conditional on property characteristics, school quality, etc. a property that "looks wealthier" may be seen as gaudy and therefore receive a discount), I think that given the limited predictive power of these characteristics and the discussion of the next paragraph, it does not seem fruitful to attempt to divine meaning from the signs of these coefficients.

One interesting question to explore in this study is the relative importance of actual neighborhood safety, as measured by crime rates, and perceived safety, as measured by the Place Pulse sur-

vey. Some ideas in economics about decision making with limited cognitive capacity, such as the availability heuristic (Tversky and Kahneman, 1973; Simon, 2013), would suggest that perceived safety should be more strongly associated with property values, since the association between safety and visual information such as the presence of litter or dilapidated homes is more cognitively available than the association between numeric crime rates and safety. However, Columns (3) and (4) of all three tables show that perceived safety does not retain a significant association with home values when we also control for actual safety, as measured by each different measure of crime. One lens through which we can understand this result is to recall Hayek's observations on the importance of local knowledge in market processes (Hayek, 1945). The Place Pulse measures are based on data from anonymous survey takers from across the internet, who are provided no local context when making image comparisons. Conversely, the crime measures provide information on events occurring in the neighborhoods that homeowners are looking to live in, events that are discussed in conversation and in the local media, and therefore more accurately reflect the considerations of homebuyers.

Tables 1.12 and 1.13 repeat the specifications from Tables 1.9 and 1.10 respectively, but dropping 608 homes especially close to the border of APD's jurisdiction, which thus have relatively high levels of data imputation. These sensitivity analyses show that there is relatively little impact of this data imputation on the main regression coefficients. For instance, the coefficient on violent crimes at 0 to 0.1 miles in column (4) changes from -0.0410 in Table 1.10 to -0.0435 in Table 1.13, a 6% difference in the magnitude of the coefficients.

1.5 Conclusion

In this paper I have constructed a housing hedonics dataset for the Atlanta single-family home market from 2013-2016, measuring several commonly studied amenities including school test scores, Census Bureau demographics, and crime rates as measured by both the FBI's SRS program and newer, completely disaggregated data collected under the NIBRS program; I have also measured 6 different dimensions of neighborhood aesthetic quality, using information from the Place Pulse

survey. To create sets of hedonic attributes from disaggregated NIBRS data, I use ring-based measures of the number of crimes within a given distance of a house for the incident-level crime data, while for the SRS data I rely on an inverse-distance-weighted 3-nearest-neighbors interpolation approach to create a measure of the crime rates for each house based on the 77 police jurisdictions reporting to the SRS near Atlanta adjacent to my study area.

Analysis of this dataset revealed that the crime rates from the SRS are a poor proxy for more finely-measured crime densities from the NIBRS, and that using more finely measured crime densities produces a more predictive hedonic model with stable estimates for the implicit price of neighborhood safety. I also find that it is critical to make full use of the fine spatial resolution of the NIBRS data, and that when applying too much aggregation to the NIBRS data it can in fact be worse than the SRS data, with some model specifications showing a positive price premium associated with higher crime when using the over-aggregated NIBRS data.

Chapter 2

Municipal Building Codes and the Adoption of Solar Photovoltaics

2.1 Introduction

Competing societal goals give rise to policy trade-offs. For instance, policy goals defined at the national, or even global level, may conflict with policy goals at the very local level. Local policies that challenge national policy are sometimes referred to as NIMBYism, from ‘not in my backyard.’ In this paper, we show that municipal policies reduce the pace of solar capacity expansion.

Many governments recognize the necessity of expanding renewable energy to tackle climate change, as well as to ensure energy security in an age of renewed geopolitical uncertainty. Renewable energy targets are often determined by carbon emission reductions goals, ‘Nationally Determined Contributions,’ under the Paris Agreement. The share of renewable electricity has been increasing substantially across the globe, yet most countries are relatively far from reaching their goals. On a positive note, the widespread use of subsidies to renewable energy has contributed to a decrease in the price of solar installations, exceeding expectations (Creutzig et al., 2017; REN21, 2022). As solar energy reaches grid parity in many countries, governments are phasing out the subsidy schemes used to promote the installation of solar photovoltaics (PV), which are often expensive and regressive (Marcantonini and Ellerman, 2015; Borenstein, 2017). This new era comes with new challenges for academics and policymakers. Assessing the role of non-price obstacles to the adoption of solar PV represents, arguably, the new frontier in research and policymaking. Here, we focus on the role of local policies, whose aims conflict with the national goal of spurring the adoption of solar PV.

Our paper identifies trade-offs between municipal building code requirements and polices aimed at defining the aesthetics of German towns, in particular with an eye toward historical preservation, and the adoption of solar PV. To do so, we combine geolocalized data on the universe of solar installations in Germany, with a unique survey on municipalities’ current and past building codes affecting the adoption of solar PV. While technological advances in the market for solar PV

have been consistently improving the aesthetics of solar installations, we observe that German municipalities have become increasingly restrictive in regulating the installation of solar PV. Hence, while from a technological perspective such trade-offs may be in the process of becoming obsolete, from a policy perspective analyzing the role of building codes on the adoption of solar PV, an aspect largely neglected so far, seems to be more relevant than ever.

Germany, one of the countries in the world with the highest penetration of solar energy and one of the most mature markets for solar PV, is an ideal place to assess the role of building codes in preventing the adoption of solar PV. Additionally, Germany is particularly well suited to this inquiry because the country has a decentralized administrative structure, which gives municipalities substantial leeway beyond the federal and state building codes. A significant share of German municipalities have implemented building codes that explicitly or implicitly regulate the installation of solar panels on buildings, with this share increasing over time.

To date, no comprehensive registry of municipal solar policies exists. A major contribution of our study is to create such a registry based on survey responses from municipal officials. In this survey, delivered to all municipalities in Germany, we ask for information about how the local building code treats the installation of solar panels. Regulations of solar installations in some cases include explicit bans in certain areas or the entirety of the municipality. Some other municipalities have more subtle provisions, for example, such that solar installations cannot be visible from the street. We obtained information on when municipal policies became effective, as well as on past policies no longer in effect. We match this information to federal data resulting from the mandatory reporting of the location and technical specification of solar panels connected to the electric grid and municipal-level demographic and electoral statistics.

Municipalities do not randomly implement solar policies. First, our study explores the motivation for, and nature of, municipal solar policies. Second, we want to understand the causal effects of municipal policies on the adoption of solar photovoltaics. To this end, we adopt a matched difference-in-difference approach, which also takes into account lessons from the recent advances in the microeconometric literature (see Abadie and Spiess, 2021; Baker, Larcker, and Wang, 2022;

Roth et al., 2022).

We find that a significant portion (15.1%) of the municipalities in our sample have one or more of the local solar policies that we study. Overall, we find that municipalities that implement any type of policy have 8.9 percent fewer solar photovoltaic installations and a 10.4 percent smaller solar capacity, effects driven mostly by small to medium-sized installations of 5-10 kW, consistent with the policy goals of shaping the urban built environment. The larger effect on capacity suggests that municipal policies are effective on both the extensive and intensive margins, leading to less adoption as well as smaller installations conditional on adoption.

This paper contributes to several strands of literature. First, an established literature on NIMBYism, including in relation to energy and environmental issues (e.g. Smith and Desvousges, 1986; Frey and Oberholzer-Gee, 1997; Levinson, 1999; Fischel, 2001; Feinerman, Finkelshtain, and Kan, 2004; Krekel and Zerrahn, 2017). Second, a growing literature on the economics and policy of solar adoption (e.g. Borenstein, 2017; Crago and Chernyakhovskiy, 2017; Gerarden, 2018; De Groote and Verboven, 2019; Gillingham and Tsvetanov, 2019). Third, a broader literature on the role of building codes in the transition towards a greener economy (e.g. Aroonruengsawat, Auffhammer, and Sanstad, 2012; Jacobsen and Kotchen, 2013; Levinson, 2016; Kotchen, 2017). Fourth, a complementary strand of literature analyzing the role of building codes in shaping urban environments and preserving the cultural and historical heritage of towns (e.g. Been et al., 2016; Zhou, 2021).

In terms of policy implications, our paper confirms and quantifies the trade-off between national and global climate mitigation goals and local historical preservation. While our analysis is positive and thus agnostic on whether historical preservation should prime over cost-effectiveness considerations related to the transition to a cleaner economy, we do note that the rapid technological evolution in solar photovoltaic technology has not only led to lower prices for solar panels but also to more options in terms of quality, in particular with respect to how 'invasive' solar panels may be. Going forward, such solutions may relax the trade-off that this paper analyzes, making some of the regulations that we cover obsolete or amendable. 'Invisible' solar installations could

indeed often be compatible with the aesthetics of historical towns, increasing the potential for solar energy wherever a conflict arises between renewable energy goals and local preservation. Making solar 'invisible', either through regulations prescribing where solar panels can be located in historical districts or by prescribing the use of photovoltaic roof tiles, may still limit adoption indirectly through peer effects, which the literature finds to depend on an installation's visibility (see Carattini, Levin, and Tavoni, 2019 for a review). Yet, the direct effect of allowing for photovoltaic roof tiles, even if more expensive than conventional solar installations, could already substantially expand solar capacity in historical towns and other areas where similar aesthetic considerations apply.

The remainder of the paper is organized as follows. Section 2.2 presents the economic and policy background. Section 2.3 describes our data and methodology. Section 2.4 discusses the identification strategy and estimation. Section 2.5 presents our empirical results. Section 2.6 concludes.

2.2 Background

As part of the European Union's commitment to the Paris Agreement, Germany strives to become carbon neutral by 2045. Achieving carbon neutrality implies boosting considerably the uptake of renewable energy in the country, as does increasing energy security and reducing reliance on energy imports from third countries. Both energy security and reliance on third countries are issues that are back at the forefront of policymaking in recent times amid renewed geopolitical uncertainty. The primary policy instrument that has been used over the last three decades to promote renewable electricity is a feed-in tariff scheme (FIT) that guarantees a fixed price for renewable energy. Germany first implemented this type of subsidy for electricity production from all renewable energy sources in 1991, as part of the Electricity Feed-in Law, or Stromeinspeisungsgesetz (SEG) (Stromeinspeisungsgesetz, 1990). The SEG required grid operators to purchase electricity produced by solar photovoltaics at a price equalling 90 percent of the average consumer price per kilowatt hour. In 1995, this corresponded to 8 cent/kWh, which did not cover the cost of electricity production

from solar photovoltaics (Beste and Kälke, 2013). Hence, in 2000, the Renewable Energy Sources Act ("Erneuerbare-Energien-Gesetz" or EEG) replaced the SEG (Erneuerbare-Energien-Gesetz, 2000). Under the EEG, FITs are differentiated by energy source to offset technology-specific cost disadvantages compared to conventional power generation (Böhringer et al., 2017).

Particularly for solar photovoltaics, the EEG dramatically increased feed-in-tariffs compared to the preceding scheme under the SEG. The EEG guarantees producers a fixed above-market price for renewable energy for 20 years from the date of installation. The guaranteed rate for electricity from solar photovoltaics was 50.6 cent/kWh in 2000, and has since dropped to 8.2 cent/kWh in 2020.[1] The EEG prescribed a steady decline in the FIT in anticipation of falling renewable energy generation cost. Any difference between feed-in tariffs paid by the grid operators and the market price is passed on to electricity consumers as a surcharge on the electricity bill. This framework and the structure of the FIT has been retained in a series of revisions of the EEG in 2004, 2009, 2012, 2014, 2017, and 2022. The subsidies fueled the growth in renewable energy production in Germany. The share of renewable energy in gross electricity consumption increased from 3.4% in 1990 to 6.2% in 2000 and to 41.1% in 2021, with solar energy accounting for approximately 20% of all renewable electricity in 2021 (AGEE-Stat, 2022).

The SEG and EEG are federal policies, but lower administrative units -- states, districts, and municipalities -- can alter their impact. Germany has a federal system of government that is shaped by the principle of subsidiarity, which holds that policy issues should be addressed, wherever possible, at the most immediate level. The lowest administrative units are the municipality ("Gemeinde") and collective municipality ("Gemeindeverband"), superseded by districts, governmental districts, and the 16 states ("Länder"). Each state has their own building code that provides a framework for policies at the municipal level. With respect to solar photovoltaics, there are only very minor differences in policy across states. Most importantly, no state requires an application or permit to install solar panels, though municipalities are free to implement regulations beyond

[1]This fixed rate has been offered to owners of solar installations with a capacity under 30 kW, with slightly lower rates for higher capacity solar installations.

the state building code.[2] As of June 2019, there were, for the purpose of this study, 4,691 independent municipalities and 758 collective municipalities.[3] Collective municipalities are a union of at least two municipalities with the purpose of shared governance, administration and policy, with the constituting municipalities retaining some degree of autonomy. Commonly, collective municipalities are governed by one council and a first mayor. Municipal development, taxes and fees, statutes, ordinances, building codes, and municipal services are typically under the purview of the collective administration, though the degree of integration varies.[4] In sum, both independent and collective municipalities have far-reaching authority to enact building regulations that may affect solar adoption.

In this study, including in the survey to municipalities that we describe in the next section, we distinguish between four types of municipal solar policies: bans, permit requirements, regulations, as well as policies promoting solar adoption. Bans, permit requirements, and other types of regulation are often implemented to preserve the appearance of historical buildings and districts. In our sample, which we describe in the following section, 34% percent of municipalities that implement solar policies are historical towns (see Table 2.4).[5] The definition of "historical town" stems from a report, which was commissioned by the German Federal Ministry for Transport, Building and Urban Development (Vereinigung der Landesdenkmalpfleger, 2010). In our paper, we follow the categorization provided in the report.

Of the four policy types that we study, bans are straightforward and prohibit homeowners from installing solar panels. Permit requirements are municipal policies that mandate homeowners to either apply for permission to install solar panels or submit building plans to obtain planning permission. Solar regulations cover all other types of policies that municipalities may implement to

[2]One exception to this structure are independent cities, which are districts in their own right. Three cities (Berlin, Bremen, and Hamburg) are states in their own right. Hence, they are excluded from our study.

[3]Unless the distinction is critical, we refer to both independent and collective municipalities simply as "municipalities."

[4]In particular, there are four states where the individual municipalities may maintain a greater than usual degree of autonomy: Saxony, Baden-Wurttemberg, Bavaria, and Thuringia. This heterogeneity across states had implications for the distribution of our survey, as discussed in Appendix **??**.

[5]Nationwide, some 1,900 municipalities (approximately 17 percent) are designated as historical municipalities (Vereinigung der Landesdenkmalpfleger, 2010).

regulate solar installations. In our study, we request that municipalities explain what their regulation entails. There are three prevalent types of solar regulation that we ask about. These common regulations mandate (1) that solar panels are not visible from the street, (2) do not reflect light on other buildings or the street, and/or (3) that solar panels are integrated in the walls or roof of a building. Finally, we also gather information on the promotion of solar photovoltaics, which refers to municipal-level financial incentives, i.e. tax rebates, for homeowners to install solar panels on existing homes or include solar panels in the construction of a new building. As determined by our study, this latter type of municipal policy is relatively rare with respect to the overall penetration of policies limiting the adoption of solar photovoltaics, and is thus not the main focus our study. In sum, bans are the most restrictive solar policy that municipalities can impose, followed by permits and regulations. Across all policies, we distinguish between policies that apply to the entire municipality and policies that only regulate an area, for example, a historical district.

2.3 Data

2.3.1 Data Sources

We use three sets of data in our analysis. The first dataset is a registry of municipal building policies relating to the adoption of solar photovoltaics, which we created by conducting a survey sent to the building code offices of all municipalities in Germany.[6] To our knowledge, this is the first such registry, worldwide. The second dataset is the Marktstammdatenregister (MaStR), which contains data on the generating capacity of all solar power plants in Germany for the years 1991 to 2019, and is provided by the Federal Energy Agency (Bundesnetzagentur). The third dataset contains socioeconomic characteristics of municipalities and is sourced from the Federal Statistical Office of Germany (Destatis, Statistisches Bundesamt) and the Federal Employment Agency (Bundesagentur für Arbeit).

[6]The survey was complemented with a manual search for all municipalities that did not provide the requested information through the survey, as detailed in Section 2.3.1.1.

2.3.1.1 Building Codes. Municipal regulations of solar installations are typically found either in zoning documents (Bauleitpläne) or in statutes (Satzungen). Many municipalities with substantial historical building stocks have dedicated building statutes (Gestaltungssatzungen) intended to regulate and protect historical buildings and the overall appearance of a municipal district. Our registry of municipal solar policies is primarily based on survey responses from municipal officials. In the survey, sent to all municipalities in Germany, we asked about policies that explicitly concern the installation of solar panels, following the classification described in Section 2.2 We sequentially asked for information on both current and past municipal policies. Municipalities and collective municipalities (as described in Section 2.2) received identical surveys, with one exception: collective municipalities could indicate to which constituting municipalities the reported policies apply. To this end, we included an interactive checklist of (sub-)municipalities in the survey sent to collective municipalities. The survey starts with an overview of policies and asks the municipality to indicate which ones are present, based on the options described in Table 2.1.

As mentioned, the survey asks whether a policy applies to the entire municipality or one or more geographic areas within the municipality. If the policy applies to an area within the municipality we asked to be provided a map, a shapefile with geocoded areas, or a precise description (i.e. cross streets). We also ask that the municipality report the zoning designation (e.g. mixed residential) of the area and whether it is considered an area of historical significance.

The survey was available to municipalities between September 2019 and December 2020. In order to contact all municipalities in Germany, we obtained contact lists from each German state, excluding the three city-states of Berlin, Bremen, and Hamburg. We conducted a small trial in early September 2019, where we sent the survey to 43 independent municipalities, and 23 collective municipalities, allowing us to inspect responses and obtain feedback to make adjustments before scaling up. In October 2019, we started administering the survey to all remaining municipalities. We randomly assigned all municipalities to 1 of 6 waves, and staggered the survey rollout by wave to allow us to provide municipalities with a timely response to questions or to follow up rapidly via email or phone calls in case some fields had been left incomplete. Each municipality received

an initial invitation to participate via email. The initial invitation was directed, whenever possible, to the building department within the municipality. The email provided a brief introduction to the research project and a link to the online survey. By the end of November 2019, all municipalities had been invited to participate in the survey.

During the remaining months through December 2020, continuing work on the survey primarily consisted of 3 tasks: (1) corresponding with municipal officials who submitted incomplete survey responses, or who reached out with questions about how to complete the survey; (2) obtaining updated contact information to re-send the survey when it was discovered that the state-provided contact lists gave deprecated or inappropriate email addresses; and (3) sending periodic reminders to municipalities which had not yet completed the survey.

In the case of municipalities that opted out of survey participation or never completed the survey, we supplement the dataset with information from publicly available municipal documents. We searched municipalities' websites to collect the same set of information that we required the municipalities to fill in the survey. In order to standardize the data collection as much as possible, we limited the search to certain types of official documents and searched the documents using a pre-defined set of keywords (see Table 2.2).

In total, we contacted 4,678 independent municipalities and 756 collective municipalities, representing all municipalities in Germany save for a small handful for which we were unable to obtain contact information, and the abovementioned three city states. The survey response rate is 49.3% among municipalities (2,305 responses) and 32.3% among collective municipalities (244 responses), for an average response rate of 46.9%. Some of the responses are for various reasons not usable, for instance if the respondent failed to provide the start date of a policy, or otherwise left the survey incomplete. As a result, we have 1,102 complete responses for the municipalities, and 103 complete responses for the collective municipalities, implying completion rates of 48% and 42% respectively, and effective response rates of 24% and 14%. To these complete survey responses we add the entries from our manual search process, yielding 172 entries for the municipalities and 26 for the collective municipalities, bringing the total sample to 1,274 and 129,

respectively. Notably, the 129 collective municipality responses translate to a higher number of observations in our main dataset, because our unit of observation is the individual constituent municipalities within the collective. Thus, the 129 responses from collective municipalities imply 600 total responses at the municipality level, bringing the total number of municipalities for which we have solar building code data to 1,874.

In Table 2.3 we present the balance of covariates across survey respondents and municipalities overall. These summary statistics are shown separately for independent and collective municipalities. Collective municipalities are typically smaller than independent municipalities because their constituting municipalities are very small. Overall, municipalities with greater population were more likely to respond to our survey. However, differences between respondents and non-respondents across other observable municipality characteristics are not of meaningful size. Most notably, per capita measures of the number of solar installations added per year and the added annual capacity are not different across survey respondents and non-respondents.

2.3.1.2 Solar Photovoltaics. Our outcomes of interest are the number of solar installations and solar capacity in each municipality and year. In 2019, the federal government made these data on energy market participants available to the public via the MaStR database. The German electricity market has many small producers, including more than 2 million solar installations, implying an overall gross production capacity of 59 GW at the end of 2021. Solar photovoltaics provided 9.1 percent of gross electricity consumption in 2021 (Fraunhofer, Umweltbundesamt, 2022). MaStR was created to provide reliable data on market actors in the energy sector, at a time when the German energy sector was liberalized and the transition to renewable energy was well under way (Bundesnetzagentur, 2018).[7] The MaStR registry contains installation and plant names, name of the owner if not an individual, addresses, type of energy source, and production capacity. Plant owners report the in-service date when the plant installation is completed.[8] All energy market par-

[7] The Federal Energy Agency (Bundesnetzagentur) provides these data to the public in accordance with the federal code that regulates the energy sector (§ 111e and § 111f of the Energiewirtschaftsgesetz).

[8] MaStR also records a reporting date, which refers to the date that the plant information was entered in the database. New plants need to be registered within one month of the in-service date. Late registration can result in fines and the

ticipants are required by law to report any new or existing plant to the Federal Energy Agency and keep this information up to date, regardless of whether they are receiving energy generation subsidies. This rule applies to both conventional and renewable energy generation. The information is verified by the grid operator serving the electricity producer entering the information. Registration of a plant is required in order to receive federal subsidies or tax benefits. Failure to register can result in fines. While information about the postal code where the installation or plant is based is public, the street address is not public information for solar plants generating less than 30 kW.

2.3.1.3 Control Variables. Our control variables, including demographic and socioeconomic characteristics, are provided by the Federal Statistical Office. These data are provided annually back to 2008, and are tabulated at the level of individual municipalities. In particular, even for municipalities which belong to a collective for administrative purposes and thus receive the collective version of the survey, the control data are still tabulated at the level of the individual municipalities. The specific variables that we include in the analyses are: population, share of males in the population, share of children in the population, the green party vote share, and, at the district level, average household income and unemployment rate.

2.3.2 Descriptive Statistics

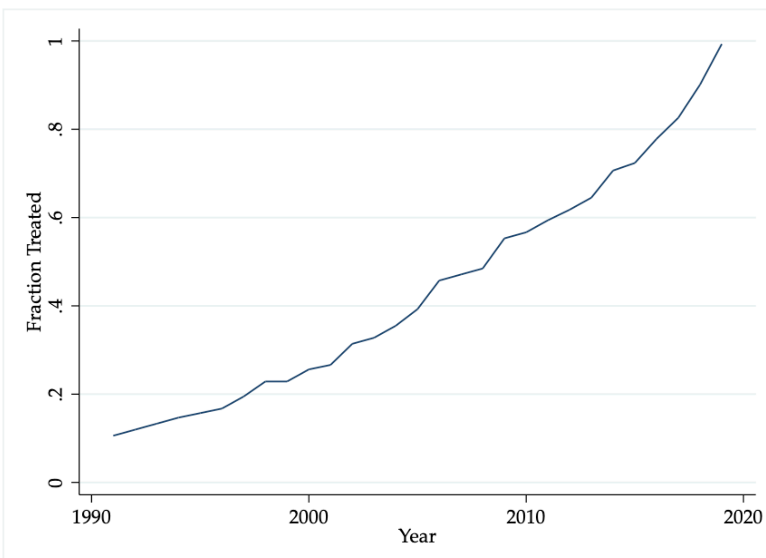
Our final dataset is a yearly panel of 1,874 municipalities from 1991 through 2019. This panel includes data on municipal solar building policies, adoption of solar PV, and time-varying socioeconomic characteristics of the municipalities. Recall that we distinguish between four types of policies: bans, permits, regulations, and policies that promote solar expansion. In our sample, 294 municipalities have one or more of these policies in place. 153 municipalities regulate the installation of solar panels, 44 require a permit, 22 impose a ban on solar panels, and 33 promote the installation of solar photovoltaics (in addition to existing federal subsidies). Within our dataset, it is necessary to define treatment and control groups based on the solar building policy data that we

loss of subsidies. Existing plants that were registered with the Federal Energy Agency prior to the introduction of the MaStR database in 2019 were required to be re-registered with MaStR by the end of January 2021.

collected. The treatment group is defined as the 252 municipalities whose treatment status may turn positive sometime between 1991 and 2019. The control group consists of all 1,579 municipalities which never receive treatment.[9]

Figure 2.1 presents the cumulative adoption of municipal solar building policies in Germany from 1991 through 2019. At each point in time it presents the proportion of municipalities which have implemented solar policies, as a fraction of all municipalities which at some point adopt a policy that we a priori expect to have a negative impact on solar adoption and which are the main focus of this study: bans, permits, and regulations. About 10% of these municipalities report that their policies were in place prior to 1991, and therefore for the purpose of our study do not provide any useful variation in policy. From 1991 through 2019, the adoption of solar policies then occurs at a moderately increasing rate, with only roughly 25% of municipalities treated by 2000, and then nearly 60% treated by 2010.

Figure 2.1: Cumulative Share of Municipalities that Have Solar Policies



The boom in solar photovoltaics followed the major increase in the feed-in-tariff rate in the year 2000. We found that 94 municipalities in the sample introduced policies that directly (or indirectly)

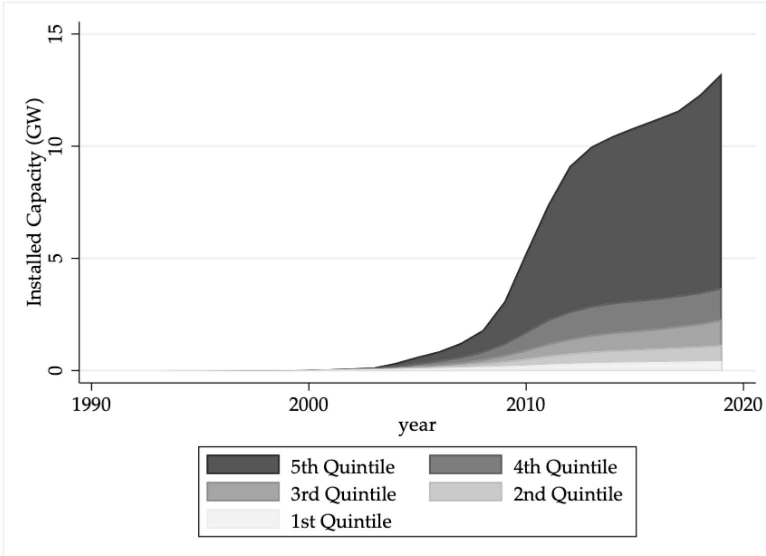
[9] We see that historical towns implemented solar policies at much higher rates than towns without historical districts. In the control group, 12 percent of municipalities are historical towns. In contrast, 34 percent of municipalities that implemented solar policies have historical districts. As described in the following section, the empirical approach takes care of these differences.

define rules concerning the installation of solar photovoltaics. Prior to 2000, the average number of solar installations that existed before the implementation of a relevant building code is only 0.17. The average installed capacity is 0.66 kW per year. Hence, these building codes were written at a time when only a handful of solar PVs existed in these municipalities. Typically, pre-2000 building codes explicitly mention solar PVs in passing, for example, alongside antennas and satellite dish regulations. That is, while these building codes impact the adoption of solar photovoltaics, they were not necessarily written with widespread solar adoption in mind.

Since our data on solar installations include a wide range of installations, ranging from small rooftop installations to massive solar fields, it is useful to construct measures of solar adoption that allow us to focus on different categories of installations. In particular, since the urban policies we study should be expected to have implications only for panels within the urban area, we should not expect effects for particularly large installations. To this end, we define outcome variables that separate solar installations into 5 categories, corresponding to the quintiles of the overall gross capacity distribution for all installations from all municipalities and years. Specifically, this procedure results in separate outcomes for PV installations in the capacity ranges of 0-5 kW, 5-7.44 kW, 7.44-10.5 kW, 10.5-22.1 kW, and 22.1 kW and above. Figure 2.2 shows the cumulative amount of solar capacity installed among each of these separate categories of PV installations from 1991 to 2019, for the 1,874 municipalities in our dataset. From this figure it is evident that while the majority of the 13 GW of capacity installed come from the largest category of installations, there is also a total of 3.7 GW of capacity installed from the 4 smaller categories of installations, and 2.3 GW of capacity from the 3 smallest categories, where we expect the impact of the building policies that we study to be concentrated.

Table 2.4 provides balances of covariates measuring possible selection across treatment status, using control data for the years 1991 to 1999, prior to any significant installation of solar capacity in the country. From Table 2.4, we can see that treated municipalities tend to have greater land area, higher population, less installed PV per capita, and are more likely to be a historical town. For this reason, as described in detail in Section 2.4, we implement a matching approach that significantly

Figure 2.2: Evolution of Solar Capacity



reduces the observable differences between treated and control municipalities.

2.4 Empirical Strategy

2.4.1 Nearest Neighbor Matching

Our goal is the estimation of the causal effects of municipal policies on the adoption of solar photovoltaics. We examine the effect of all policies on our outcome of interest and also study separately the impact of regulations and permit requirements, given their frequency in the data.

Municipalities do not randomly implement solar policies. Therefore, the main challenge in the estimation of causal effects stems from the fact that municipalities choose to implement solar policies.

The descriptive statistics in Table 2.4 tell us that treatment status may in part be correlated with observable municipality characteristics. Hence, to reduce observable differences between municipalities, we use matching as a pre-processing step, followed by regression analysis. More precisely, we implement one-to-many nearest-neighbor matching without replacement. Abadie and Spiess, 2021 show that matching as a pre-processing step to estimation yields valid regression standard errors if matching is done without replacement and standard errors are clustered at the level of the match. We follow this approach.

The set of matching variables is chosen to produce a comparison municipality that has similar characteristics to a treatment municipality, while maximizing the number of successful matches. For the main estimations, the matching variables are, at the municipality level, population, share of women, share of children, land area, green party vote share, and at the district level, the unemployment rate and household income. In our main specification, we match on the average value of these variables for the years 1991 to 1999. Municipalities in both treatment and control group have some existing solar capacity in the late 1990s. However, the timeframe is chosen such that the positive effect of the 2000 renewable energy legislation on solar adoption does not play a role. As we can see in Figure 2.2, almost all the solar capacity installation occurred after the year 2000. Approximately one third of municipalities in the main estimation sample implemented solar policies prior to 2000, suggesting that in those cases, the policy decision was not driven by existing solar capacity.

As we can see in Table 2.4, matching reduces observable differences between treated and control municipalities but does not completely eliminate them—though an exact match is not a necessary condition for identification.

2.4.2 Main Empirical Specification

Once we obtain the matched sample, we use a two-way fixed effects estimator to identify the average treatment effect on the treated (ATET) of municipal solar policies on installed solar capacity. Since municipalities implement solar policies in staggered fashion, our analysis departs from the canonical difference-in-difference design. Roth et al. (2022), in a review of the recent difference-in-difference literature, show that a standard two-way fixed effects approach yields the ATET of a staggered policy if the treatment effect is homogenous and not dynamic. We account for these insights in two ways. First, by relying on one of the recent estimators covered in Roth et al. (2022), as described below. Second, to assuage potential concerns that when using a two-way fixed effects estimator our estimate may be biased due to treatment effect heterogeneity, we separately analyze different types of policies, and quintiles of solar capacity. Hence, we estimate regression equations

of the form:

$$Y_{it} = \beta * Treated_{it} + \gamma Z_{it} + \alpha_i + \alpha_t + \epsilon_{it} \tag{2.1}$$

Where $Treated_{it}$ is a binary variable indicating whether a given municipality i has adopted the policy under consideration. Y_{it} is one of twelve outcome variables measuring the yearly flow of new solar in a municipality, with one outcome variable for the total, and one variable for each of the five quintiles defined in Section 2.3.2, measured in either (natural) log of the number of installations or log of total gross capacity installed.[10] Z_{it} is a vector of time-varying control variables that mirrors the covariates used in the matching procedure: (log) population, the share of males in the population, the share of children in the population, the share of green party voters, household income, and the unemployment rate. The α's are municipality and year fixed effects, and ϵ is an error term.

Any generalized difference-in-difference approach requires careful discussion of the parallel trend assumption. In our case, we assume that the newly installed (log) solar capacity (or total number of installations) in treated municipalities would have followed the same trajectory (in the absence of solar policies) as newly installed (log) solar capacity (or total number of installations) in control municipalities. The parallel trend assumption tends to be sensitive to the functional form of the estimated model (Roth and Sant'Anna, 2022). We choose the (natural) log of capacity and log of installations as our outcome variables because in the absence of a solar policy it is plausible that solar capacity in a treated municipality would have increased by a constant proportion.

[10]Note that to retain in the estimation sample the municipality-year observations that have zero solar photovoltaic installations (and thus capacity as well), we define our outcome as log(1+x). We confirm the robustness of this transformation by re-estimating the main model using the inverse hyperbolic sine (IHS) transform of capacity and installations as outcome variable for all our main estimations. Estimates are robust also for the remaining robustness tests and alternative specifications.

2.5 Empirical Results

2.5.1 Main Regressions

Tables 2.5 and 2.6 present the main results of estimating a number of regression equations of the form specified in Section 2.4, equation 2.1; each cell of these tables provides a different estimate $\hat{\beta}$ from a different specification estimated on data from 1991 through 2019. Table 2.5 shows the treatment effect estimates for all solar policies. Table 2.6 shows the treatment effect estimates for permits and regulations. The rows of each table correspond to different outcome variables, so the first row presents results based on the total capacity installed per year, followed by the smallest solar installations (<5 kW) and the bottom row presents results on the largest installations (>22.1 kW). The first column shows the results for (log) capacity, the second column shows the results for the (log) number of installations.

$\hat{\beta}$ should be understood as the average difference between observed solar installations (or capacity) per year in a municipality which has adopted a given policy, and a counterfactual estimate of the solar installations the municipality would have seen if it had not adopted the policy. The counterfactual is informed by the national time trend in solar adoption (year fixed effects), translated up or down to match the overall level of solar adoption in the municipality (municipality fixed effect), and allowed to accommodate differential trends across municipalities based on evolution in the municipality's demographic and economic characteristics (time-varying controls).

In Table 2.5 we see that solar photovoltaic policies reduce the number of installations and solar photovoltaic capacity. Overall, municipal solar policies reduce the number of installations by 8.9 percent and reduce capacity by 10.4 percent. That is, we find a larger effect on capacity than on installations, pointing to the ability of municipal policies to influence both the extensive and intensive margins, leading to fewer installations as well as smaller installations conditional on adoption. We confirm that the difference in coefficients between installations and capacity is statistically significant at conventional levels for all quintiles except the top two quintiles.

The policy effect on the number of installations is smaller for higher capacity solar photovoltaics. The reduction in capacity is most pronounced, and precisely estimated, for installations

between the 20^{th} and 60^{th} percentile of capacity. This corresponds to installations between 5.0 kW and 10.5 kW of capacity, a typical size for single family rooftop solar photovoltaics. It makes also sense that the quintile with the smallest installations may be less affected, as small installations generally tend to be less invasive.

In Table 2.6 we see that the overall effect of the most common types of policies, regulations and permit requirements, reduce capacity by 18.0 percent and the number of installations by 16.1 percent. For these two most common types of policy we also see the largest reduction for installations between the 20^{th} and 60^{th} percentile of capacity. Once more, we see that both the extensive and intensive margins are affected, with the coefficients for capacity being larger than the coefficients for installations, significantly so in several cases, confirming the pattern at which Table 2.5 hinted.

In order to interpret these results at the aggregate level, it is useful to begin by considering the aggregate impact among all municipalities in our sample. One simple way to estimate this impact is to calculate the total amount of solar capacity installed in treatment municipalities during treatment years, and then multiply it by the obtained regression coefficients. For simplicity, in the following calculations we consider only the effects for the 3 bottom quintiles of the capacity distribution, since this is where we find most action to take place. The total amount of solar capacity for all treatment years within treated municipalities is 54 MW for the smallest installations, 91 MW for the next capacity class, and 154 MW for the middle quintile. Multiplying by the Table 2.5 coefficients of 9.0%, 14.1%, and 16.6%, respectively, the estimated impact of solar policies on solar adoption in each size class is 5 MW, 13 MW, and 26 MW, for a total of 44 MW. The total amount of solar capacity installed among the municipalities in our sample over the entire period is 2,276 MW, so the aggregate impact is about a 2% effect. Extrapolating to the national level, a 2% effect would represent a loss of 160 MW of solar, as there is a total of 7,500 MW of installed capacity among the three size categories throughout Germany.

In these calculations, we only focus on overall solar capacity. That is, when deriving policy implications, we assume that every solar installation counts the same. This is likely to be largely the case as in the European context the development of the electricity grid is relatively advanced. Re-

call that the goal of our study, in general, is to illustrate and quantify the trade-off between national (and global) goals of energy security and climate change mitigation and local goals of preservation. If, following our study, municipalities would reconsider their policies and subject them to a more careful analysis of costs and benefits, acknowledging that not all benefits may accrue locally, we do consider important for them to also account for municipality-specific features with respect to grid congestion and connectedness. They could do so building on a growing literature in this area (e.g. Fell, Kaffine, and Novan, 2021; Gonzales, Ito, and Reguant, 2022 for related studies), although we expect these features to be largely similar across municipalities for most contexts.

2.6 Conclusions

Since the 1990s, subsidies to renewable energy, and to solar PV in particular, have been effective in promoting the adoption of new energy sources and spurring innovation in the renewable sector. However, at the very same time, local policies might have counteracted the adoption of solar energy. As we assess in this paper, a substantial share of German municipalities have over time amended their building codes to place restrictions on the adoption of solar PV, often with the aim of preserving the historical aesthetic of the town. With the cumulative innovation and economies of scale that have been achieved in solar PV energy over the past three decades resulting in grid parity, large subsidy programs are being gradually discontinued, making remaining non-pecuniary barriers to solar PV adoption an important topic for empirical research and policymaking.

We document the spread of municipal policies that restrict the adoption of solar PV by means of administering a survey regarding such policies to all German municipalities. Additionally, our survey distinguishes between several varieties of policies which are adopted by municipalities, and we find that while outright bans of solar PV are relatively rare, there are a larger share of municipalities which require residents to go through a permitting process before installing solar, and a still larger share that regulate the precise manner in which solar can be installed, for instance requiring that they be installed on a portion of the roof such that they not be visible from the street.

We also combine these data on policies with comprehensive data on all solar installations con-

nected to the German power grid, to assess the degree to which municipalities that adopt these policies see a reduced rate of solar adoption, both in terms of aver of installations and their size. Using several empirical strategies, we find that these solar policies affect both the intensive and extensive margins of adoption, leading to an aggregate reduction of approximately 160 MW of installed solar capacity at the national level, or approximately 2% of the 7,500 MW of capacity installed nationally in the three quantiles of the capacity distribution in which we find an effect of the policies.

We shed light on this trade-off between local and national (and even global) goals, a so far under-explored case of NIMBYism with very important implications as countries strive to accelerate their transition towards a cleaner economy as well as to minimize their dependence on imports of energy from foreign countries at a time of renewed geopolitical uncertainty, potentially highlighting the need for additional scrutiny on some of the policies that we study, in Germany as elsewhere, accounting also for the evolution in the solar PV technology.

Table 2.1: Municipal Solar Policies Considered in the Survey

Type	Policy	Coverage
Building codes	Ban on solar PV	
		In the entire municipality
		In a part of the municipality
	Regulation of solar PV	
	Street visibility	In the entire municipality
		In a part of the municipality
	Light reflection	In the entire municipality
		In a part of the municipality
	Wall/roof integration	In the entire municipality
		In a part of the municipality
	Solar PV is promoted by the municipality	
		In the entire municipality
		In a part of the municipality
Permits	Specific permits for solar PV	
		In the entire municipality
		In a part of the municipality

Table 2.2: Keywords for Manual Search of Policies

Search Term	Translation
Bauordnung	Building code
Bausatzung	Building statutes
Bauleitplan	Zoning plan
Gestaltungssatzung	Design statutes
Gestaltungsrichtlinie	Design guidelines
Gestaltungsleitfaden	Design guidelines
Baugestaltungsordnung	Building design code
Stadtbildsatzung	Cityscape statutes
Ortsgestaltungssatzung	(place) Design statutes
Abstandsflaechensatzung	Clearance area statutes
Aussenbereichssatzung	Outskirt / exterior statutes

Table 2.3: Municipality Characteristics: All vs. Surveyed (1991-1999 Avg.)

	Collective Municipality			Independent Municipality		
	All Mean/SD	Survey Mean/SD	Diff.	All Mean/SD	Survey Mean/SD	Diff.
Municipal Population	1,169	2,048	-879.21***	9,235	22,045	-12810.50***
	(1,580)	(2,244)	(-14.57)	(19,840)	(60,837)	(-10.36)
Share of Males	0.50	0.50	-0.00**	0.49	0.49	0.00*
	(0.02)	(0.02)	(-2.70)	(0.01)	(0.01)	(2.33)
Pop. Share of Children	0.07	0.10	-0.02***	0.07	0.08	-0.00*
	(0.04)	(0.06)	(-15.68)	(0.03)	(0.04)	(-2.11)
Unemployed Rate	0.03	0.02	0.00***	0.02	0.02	0.00
	(0.02)	(0.01)	(3.34)	(0.01)	(0.01)	(1.58)
Green Party Vote Share	0.05	0.05	-0.00	0.06	0.06	-0.00***
	(0.03)	(0.02)	(-0.53)	(0.03)	(0.03)	(-6.55)
PV Capacity (KW)	0.04	0.05	-0.01	0.33	1.18	-0.85***
	(0.44)	(0.22)	(-0.62)	(2.92)	(8.29)	(-4.96)
No. of PV Installations (KW)	0.01	0.02	-0.01**	0.08	0.30	-0.22***
	(0.05)	(0.08)	(-2.87)	(0.42)	(1.78)	(-6.35)
Installed KW's per km^2	0.00	0.00	-0.00	0.01	0.02	-0.01***
	(0.01)	(0.02)	(-0.96)	(0.05)	(0.06)	(-4.46)
Solar Installations per km^2	0.00	0.00	-0.00	0.00	0.00	-0.00***
	(0.00)	(0.01)	(-0.92)	(0.01)	(0.02)	(-6.41)
Installed KW's per 1000 Pop.	0.01	0.01	0.00	0.01	0.01	-0.00
	(0.05)	(0.06)	(0.23)	(0.04)	(0.03)	(-0.69)
Solar Installations per 1000 Pop.	0.00	0.00	0.00	0.00	0.00	-0.00
	(0.02)	(0.02)	(0.58)	(0.01)	(0.01)	(-1.75)
N	4,997	948	5,945	2,821	1,832	4,653

Note: The no. of obs. in the "Collective Municipalities" columns reflects the no. of constituent municipalities.

Table 2.4: Municipality Characteristics (Averages Over 1991-1999): Unmatched vs. Matched Set

	All			Matched		
	Control Mean/SD	Treated Mean/SD	Pre-Match Diff.	Control Mean/SD	Treated Mean/SD	Post-Match Diff.
Municipal Population	10,430	18,837	-8,406.87***	9,608	11,514	-1,906.23*
	(21,855)	(65,284)	(-4.04)	(12,584)	(14,395)	(-2.01)
Share of Males	0.50	0.49	0.00***	0.50	0.49	0.00
	(0.01)	(0.01)	(3.32)	(0.01)	(0.01)	(1.70)
Pop. Share of Children	0.07	0.06	0.01***	0.06	0.06	-0.00
	(0.03)	(0.02)	(4.08)	(0.01)	(0.02)	(-0.69)
Unemployed Rate	0.04	0.05	-0.00	0.04	0.05	-0.00
	(0.03)	(0.03)	(-1.72)	(0.03)	(0.03)	(-1.73)
Household Income	21,279.48	21,351.65	-72.17	21,263.13	21,218.58	44.54
	(3,821.43)	(4,542.32)	(-0.29)	(3,743.72)	(4,286.20)	(0.16)
Green Party Vote Share	0.06	0.07	-0.01***	0.06	0.06	-0.00
	(0.03)	(0.03)	(-3.81)	(0.02)	(0.03)	(-1.90)
Historical Municipality	0.12	0.33	-0.21***	0.14	0.31	-0.17***
	(0.32)	(0.47)	(-9.59)	(0.34)	(0.46)	(-6.32)
PV Capacity (KW)	234.22	295.88	-61.66**	249.42	275.10	-25.68
	(324.58)	(402.09)	(-2.87)	(318.80)	(363.50)	(-1.07)
No. of PV Installations (KW)	9.72	13.47	-3.75***	10.42	11.93	-1.51*
	(11.52)	(17.83)	(-4.63)	(9.83)	(11.46)	(-2.03)
Installed KW's per km^2	5.64	5.65	-0.01	6.20	5.65	0.55
	(6.50)	(5.98)	(-0.02)	(6.63)	(5.03)	(1.18)
Solar Installations per km^2	0.26	0.28	-0.02	0.29	0.29	0.00
	(0.21)	(0.24)	(-1.74)	(0.21)	(0.22)	(0.20)
Installed KW's per 1000 Pop.	48.31	44.46	3.85	48.39	49.47	-1.08
	(78.16)	(115.16)	(0.71)	(72.02)	(128.28)	(-0.17)
Solar Installations per 1000 Pop.	1.89	1.65	0.24*	1.96	1.77	0.19
	(1.85)	(1.68)	(2.05)	(1.84)	(1.72)	(1.40)
N	1,581	293	1,874	1,061	226	1,287

Table 2.5: Effect of Any Policy on Solar Adoption

	(1) Log Capacity	(2) Log Installs
All installations	-0.104	-0.0892
	(0.0965)	(0.0580)
1 quintile	-0.0897	-0.0529
	(0.0602)	(0.0403)
2 quintile	-0.141*	-0.0829*
	(0.0755)	(0.0471)
3 quintile	-0.166**	-0.0867*
	(0.0830)	(0.0489)
4 quintile	-0.131	-0.0640
	(0.0829)	(0.0400)
5 quintile	-0.129	-0.0621
	(0.126)	(0.0452)
Time-varying controls	Yes	Yes
N	33169	33169
Year FE	Yes	Yes
Municipality FE	Yes	Yes
Adj. R^2	0.616	0.642
Adj. within R^2	0.00514	0.00848

Standard errors clustered at the municipality-level.

Significance levels: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$.

Table 2.6: Effect of Permits & Regulations on Solar Adoption

	(1) Log Capacity	(2) Log Installs
All installations	-0.180	-0.161**
	(0.117)	(0.0692)
1 quintile	-0.160**	-0.101**
	(0.0717)	(0.0478)
2 quintile	-0.201**	-0.117**
	(0.0909)	(0.0557)
3 quintile	-0.264***	-0.142**
	(0.0968)	(0.0552)
4 quintile	-0.239**	-0.117**
	(0.100)	(0.0461)
5 quintile	-0.162	-0.0731
	(0.144)	(0.0500)
Time-varying controls	Yes	Yes
N	31378	31378
Year FE	Yes	Yes
Municipality FE	Yes	Yes
Adj. R^2	0.628	0.656
Adj. within R^2	0.00769	0.0125

Standard errors clustered at the municipality-level.

Significance levels: * $p < 0.10$ ** $p < 0.05$ *** $p < 0.01$.

Chapter 3

COVID-19 Policy Stringency and Outdoor Recreation: The Case of Resident Marine Sportfishing in the United States

3.1 Introduction

Saltwater angling is a major outdoor activity in the U.S., with expenditures on fishing trips totaling more than $10 billion annually as of 2017 (Lovell et al., 2020). Roughly 4% of Americans participate in saltwater fishing compared with around around 20% for running, the most common outdoor recreation activity (Foundation, 2019). Furthermore, in some marine fisheries, recreational angling is a substantial component of harvest, especially in the Southeastern US, where sportfishing can account for a large share of the harvest of commercially and culturally significant species (Lewin et al., 2019).

The COVID-19 pandemic and ensuing policy responses had far-reaching social and economic implications, and marine angling is no exception. Landry et al. (2021) summarize the potential pathways through which COVID-19 and related policy responses may affect outdoor recreation. For recreational fishing the potential pathways are as follows:

1. Site closures may force anglers to cancel trips if, for example, boat ramps or piers were closed.

2. Concern about infection risk may have led people to cancel planned trips or cease to plan new trips.

3. People may have more leisure time due to work-from-home flexibility, reduced commuting time, or job loss.

4. A decrease in substitute leisure activities, especially indoor activities, may have made angling relatively more attractive as a way to spend one's leisure time (Midway et al., 2021; Morales-Nin, Arlinghaus, and Alós, 2021)

The first two pathways imply a decrease in fishing effort while the latter two imply an increase. Link et al. (2021) documents interruptions in U.S. marine fishery data collections due to COVID-19 policies such as lockdowns. Many of these interruptions also affected anglers' access to fishing launch sites.[1]. In a national survey of U.S. anglers, most respondents experienced some access restrictions, but nearly all respondents reported that they did not think that recreational fishing was unsafe during the early stages of the pandemic in 2020 (Midway et al., 2021). The same study found that anglers reported very little change in the number of trips they took relative to the same period in years past. However, this is self-reported changes for Spring 2020 averaged over various levels of COVID-19 policy stringencies. Actual changes in effort over a longer period of time and range of COVID-19 policy stringencies may be different.

The net effect of the four pathways depends on prevailing conditions at the time effort is measured. For example, early in the response to the pandemic when lockdowns where common, the effect of site closures and concerns about infection risk would dominate and effort should be relatively low. At other times, however, as lockdowns are relaxed, but restrictions on indoor gatherings and leisure travel remain, effort could increase due the effects of increased leisure time and lack of substitute activities for that leisure time. A similar type of variation in trip taking behavior could happen over space, i.e. the different levels of COVID-19 policy stringency in different jurisdictions (e.g., states) could contribute to variations in angler trip taking behavior across jurisdictions.

We use data on the estimated monthly number of saltwater fishing trips from states on the U.S. east coast along with a monthly, state-level measure of COVID-19 policy stringency to examine the relationship between the stringency of COVID-19 policy and the level of marine angling activity. Our results suggest that the relationship is non-monotonic, whereby the number of fishing trips increases at moderate levels of COVID-19 policy, but declines at higher levels. This finding is important to understanding how people respond to measures aimed at containing virus spread and can help in planning during future pandemics.

[1] Our focus is on saltwater anglers, but Paradis et al. (2021) found that around ninety percent of freshwater fishing jurisdictions in North America were able to keep fishing open during spring of 2020

3.2 Methods

3.2.1 Data

To conduct our analysis, we construct a panel dataset at the state-month level spanning 2017 through 2021 for 16 US states, by merging two primary data sources: (1) the Marine Recreational Information Program (MRIP), which provides the necessary angler survey data to construct monthly estimates of the fishing activity in each state, providing our dependent variable, and (2) the Oxford COVID Government Response Tracker (OxCGRT), which provides fine-grained (daily) variation in the level of COVID policy stringency in each of the US states, which we collapse to the monthly level to provide our key independent variable. Below, we describe in greater detail these two major data sources, as well as state population data used in the analysis, and our methodology for processing these data sources to produce our panel dataset.

3.2.1.1 MRIP. The MRIP program oversees survey research to monitor the level of marine angling activity in a consistent fashion throughout US fisheries (we discuss the structure of these surveys in greater detail below). The program consists of 2 main data collections. The first data collection we use from MRIP is the Fishing Effort Survey (FES), a mail survey which asks anglers to recall how many trips they have been on recently, in order to get information on the total level of fishing activity. The other data collection is the Access Point Angler Intercept Survey (APAIS), an intercept survey that captures trip-level data about catch, angler attributes, and trip details such as area fished.

We use the MRIP data for the 16 states along the Gulf and Atlantic coasts in which it is available.[2] The MRIP program publishes estimates of fishing activity for 2-month periods referred to as waves, but rather than use these estimates we use the publicly-available microdata files[3] to estimate total trips in each state-month. These microdata are anonymized versions of the completed APAIS data and are thus trip-level data, and they are augmented with survey weights derived from the FES

[2]Specifically, the states are: AL, CT, DE, FL, GA, ME, MD, MA, MS, NH, NJ, NY, NC, RI, SC and VA

[3]The MRIP data files are maintained by NOAA fisheries at the following URL: https://www.fisheries.noaa.gov/recreational-fishing-data/recreational-fishing-data-downloads

data that allow for the construction of estimates that are representative of all trips covered by the MRIP program. The primary unit of measurement for our aggregated summary of the MRIP data is the angler-trip (hereafter abbreviated simply to trip), representing the total number of distinct trips multiplied by the average party size of those trips.

Creating our own estimates also allows us to customize the estimates, and we choose to specifically create estimates of the number of trips taken by in-state residents. Considering only resident fishing trips is a simple way to achieve our goal of estimating the relationship between trips taken and COVID stringency. In the appendix, we also consider trips taken across state lines, where for out-of-state trips the relevant independent variables include the stringency level prevailing at the trip-taker's origin as well as at the destination.

We assemble monthly observations from 2017 through 2021 for three different modes of fishing: private boats, charter boats, and shore fishing. With 5 years, 12 months in each year and 16 states, the data set for each mode could potentially have 960 observations. However, the data collection is not conducted for certain state-months for which there is known to be minimal fishing activity for a given mode, which are typically the winter months in colder states.[4] The only states for which data is collected in all months for all modes are Florida, Alabama, and North Carolina. There are a total of 35 state-months for which no data is collected for the private boat and shore modes, so data for these modes includes only 785 observations, and 50 state-months have no data collection for the charter mode, leading to 735 observations.

The MRIP data collection process was partially impacted by COVID conditions, however, the main data that we rely on was able to continue unhindered. Specifically, the FES mail survey was able to continue, which is what is necessary for the estimation of effort (trips) in each state-month. The APAIS however was impacted by COVID in that many access points were closed and the survey was unable to be conducted during March and April of 2020. If it were not for this fact, it

[4]The MRIP Data User Handbook explains this in Table 1: many states are not sampled during January and February, and Maine for instance is not sampled in March or April either. In the MRIP Survey Design Manual, Figure 6 shows the state-month's which do not have coverage in white, along with detail of the 2020 data quality for state-months which are covered. Since these observations are non-randomly missing, there is no reason to estimate a hurdle or 2-stage model, and we simply drop these state-months from our estimation samples.

would be possible to investigate other outcomes such as the mean hours fished on a trip, the average distance traveled to the fishing site, or other attributes of trips which come from the APAIS data.

3.2.1.2 COVID Stringency Index. Our key independent variable, on state-month level COVID stringency, comes from the Oxford COVID Government Response Tracker project. This data, as described by Hale et al. (2021), is a global dataset tracking government response to COVID across 19 distinct policy indicators such as school- and work-closures, stay-at-home-orders, and movement restrictions. These data are tracked at the daily level, and for the US are available by state. The data have been continuously updated since February 2020 by a team of volunteers who parse government reports, news, and other sources of info to create a standardized set of indicators which are comparable across the globe and across time. The full dataset includes indicators for economic response and health policy, as well as the closure indicators mentioned previously. The data product we use is the Stringency Index, which combines all 8 closure indicators, as well as a variable indicating the presence of public information campaigns, such as the "flatten the curve" campaign which was common throughout the US circa March and April 2020. All indicators in this index are recorded on discrete, ordinal scales with either 3, 4 or 5 ordinal levels.

Hallas, Hatibie, and Koch (2021) present additional information pertinent to the US state-level data, as well as the stylized facts in the data, such as the multiple cycles of stringency at first tightening, followed by loosening policy until a new COVID wave and increased community transmission results in a re-tightening of COVID stringency. Additionally, over time the political gap in Stringency widened with democratic-led states having progressively tighter COVID policy compared to conservative states.

We take the daily data for each state and collapse it to a state-month level dataset by taking the average value of the Stringency index for each day in the month. This data is then merged with the MRIP data for analysis.

3.2.1.3 Population Data. In order to place the MRIP data on a comparable per-capita basis across states, we include information on state populations using annual population estimates for each state in the sample. For 2017-2019, these are estimates from the American Community Survey, and for 2020 and 2021 we use figures from the Decennial Census.

3.2.2 Descriptive Statistics

Figure 3.1 displays seasonal trends in the level of trips across the 16 states in the dataset during 2019, for each mode of fishing. Each grey line plots the seasonal pattern for one state, with the monthly values for each state displayed as a fraction of the annual total. The black line provides a simple average of these relative values across the 16 states within each month, so that the black line represents the composite national-level seasonality trend in fishing trips among these 16 states. July is both the modal peak month among the states, and the peak month for the national trend, for all 3 modes of fishing. There are many state-months in which 0 trips are recorded, and in particular for the private mode there are only 4 states with positive trips in January: Florida, North Carolina, Alabama, and Mississippi.

We can see from Figure 3.1 that fishing activity for the private mode is in most states fairly broadly distributed from April through November, with only 10 state-months accounting for more than 20% of the annual trips in a state, and a maximum of 35% for Maine in June. All but 1 of the 10 state-months with more than 20% of annual trips come from the Northern states of Maine, New Hampshire, Massachusetts, Delaware and Rhode Island, with July in Mississippi as the exception. While the pattern for the shore mode is quite similar to private mode, we can see that the charter mode has substantially more concentration in the Summer, reflecting the fact that those fishing with their own equipment are more likely to get use out of that equipment throughout the year, and vice versa.

Figure 3.2 shows the evolution of COVID policy stringency over the course of 2020 and 2021 for each of the 16 states in our analysis. Following the initial uniform jumps in stringency during the March/April 2020 lockdowns, there is substantial variation between the states in how COVID

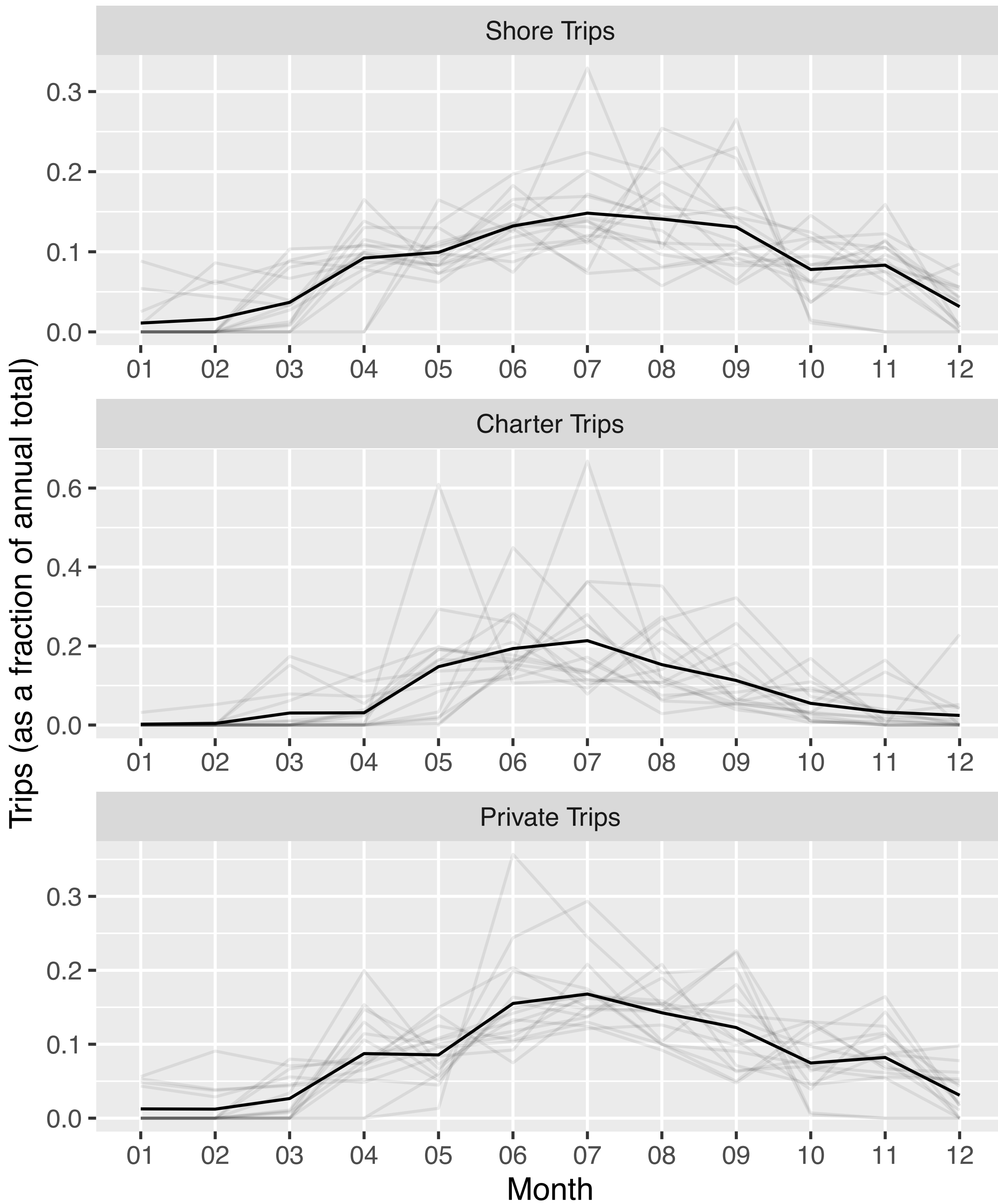


Figure 3.1: 2019 Monthly Marine Sportfishing Trips by State, Relative to Annual Total.

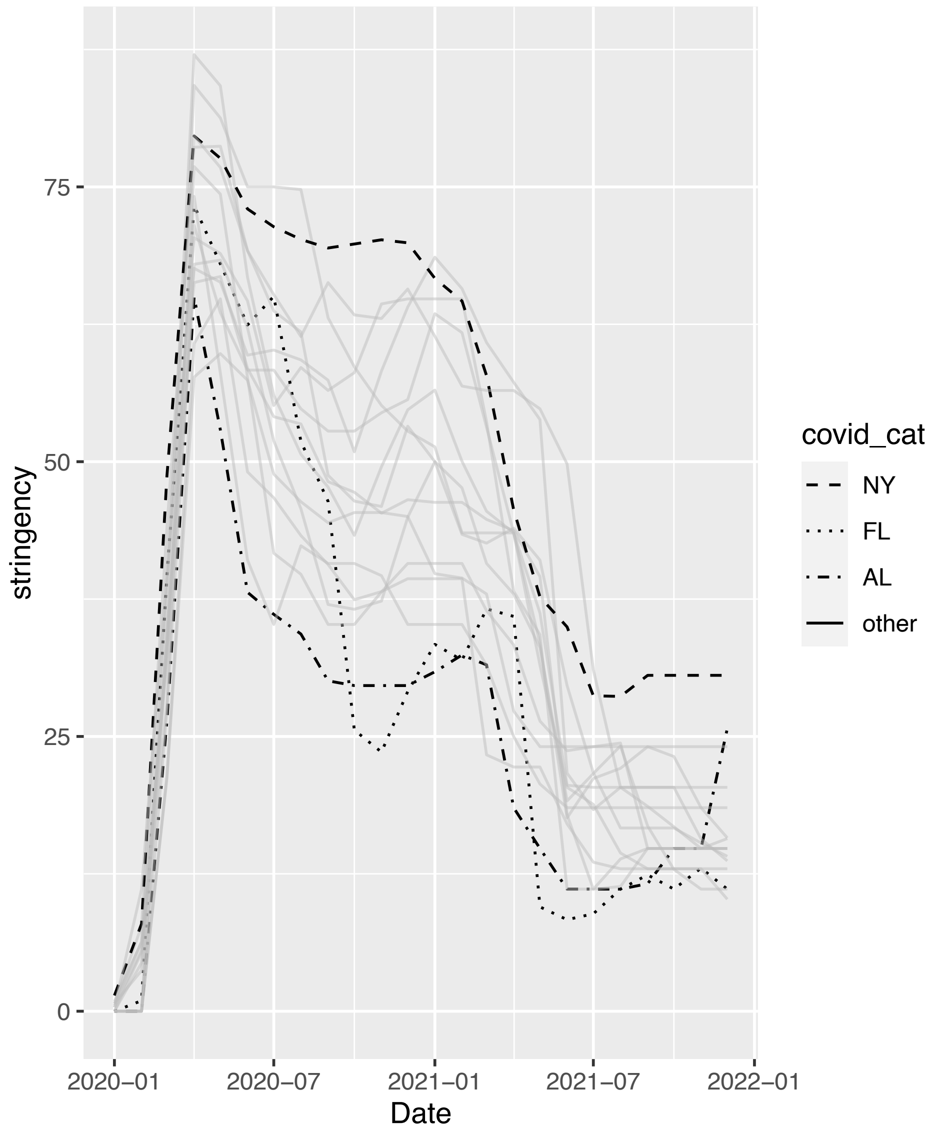


Figure 3.2: Monthly 2020 Stringency Index by State

policy evolved. Here we have highlighted 3 states showing archetypal cases: (1) New York, which had consistently stringent COVID policy, (2) Alabama, where after April COVID policy was loosened substantially, and stayed loose, and (3) Florida, which displays the most variation in COVID policy over time. The substantial independent variation in these series affords the opportunity to examine the relationship between COVID policy stringency and fishing activity.

3.2.3 Modeling Approach

We model the relationship between the aggregate monthly number of saltwater fishing trips trips taken in each state and the monthly COVID policy stringency using a fixed effect Poisson regression model with the following conditional mean:

$$\mu_{imy} = W_{iy} * \exp(\beta_1 S_{imy} + \beta_2 S_{imy}^2 + \alpha_{im} + \gamma_i y)$$

where for state i during month m and year y, μ_{imy} is the expected aggregate trips, W_{iy} is the state population, and S_{imy} is the monthly average COVID policy stringency. The coefficients β_1 and β_2 capture the aggregate relationship between stringency and trips taken, while the term α_{im} represents state-month fixed effects which allow for state-specific seasonality trends in our model, and γ_i is a state-specific linear time trend. We choose a Poisson regression model to estimate this conditional mean function, following the advice of Wooldridge (2010) who notes that estimating a Poisson model is the most efficient method which is consistent when only the conditional mean needs to be correctly specified, without needing to also correctly specify the conditional variance.

We calculate variance estimates clustered at the state level, so as to account for unmodeled correlation between the number of trips taken in different periods within a state, as may be the case for instance if unfavorable weather in a given month causes some trips to be postponed to the following month. Furthermore, our clustered variance estimates are adjusted for the fact that the number of clusters is small (16), and therefore typical asymptotically-consistent variance estimators may have a downward bias. Specifically, our variance estimates are calculated using a bias-reduced linearization method proposed by Bell and McCaffrey (2002). We estimate the Poisson regression model using the `fixest` package in `R` (Bergé, 2018), and variance estimation is conducted through the `clubSandwich` package in `R` by Pustejovsky and Tipton (2016).

3.3 Results

Table 3.1 presents the results of our estimated fixed effects Poisson regression models, with the state-specific time trends and seasonality fixed effects suppressed to focus on the coefficients of

Table 3.1: Poisson Fixed Effect Regression of Trips on Covid-19 Stringency, by Mode

	Private	Charter	Shore
Stringency	0.0135^{***}	0.0236	0.0172^{*}
	(0.0020)	(0.0125)	(0.0071)
Stringency^2	-0.0002^{***}	-0.0004	-0.0002
	(0.0000)	(0.0002)	(0.0001)
Num. obs.	785	735	785
Pseudo R^2	0.9578	0.8939	0.9356

$^{***}p < 0.001$; $^{**}p < 0.01$; $^{*}p < 0.05$. SEs clustered by state with small-sample adjustment.

interest. In each of the 3 fitted models for the different fishing modes, the first order relationship is an increase in fishing trips at moderate levels of policy stringency, with a second order effect dampening or even reversing this relationship at higher levels of stringency. The first order relationships are significant at the 1% level for private mode trips, and at the 5% level for shore mode, while the coefficients for the charter mode are significant only at the 10% level. The second order coefficient is significant only for the private mode, though it has a negative sign in all 3 independent models.

Figure 3.3 represents these same relationships graphically, by plotting the estimated model's predictions for the relative change in resident fishing trips at the various possible levels of COVID stringency. The solid line provides the point estimates for predicted percentage change in trips, while the shaded areas plot the 95% confidence intervals around these predictions.

It is important to note that these results for the impact of COVID stringency on fishing trips represent a composite effect of factors correlated with COVID stringency. For instance, to the degree that traffic lessened in proportion to stringency, the travel cost to visit fishing sites was reduced and therefore our results are partially driven by that change.

In interpreting these results, one consideration is that the estimated increase in trips at moderate levels of policy stringency may reflect the dynamic behavior of anglers. That is, since the periods of moderate stringency were immediately preceded by periods of high stringency, the increase in trips may be due to anglers saving up time and money for going on trips when stringency was high,

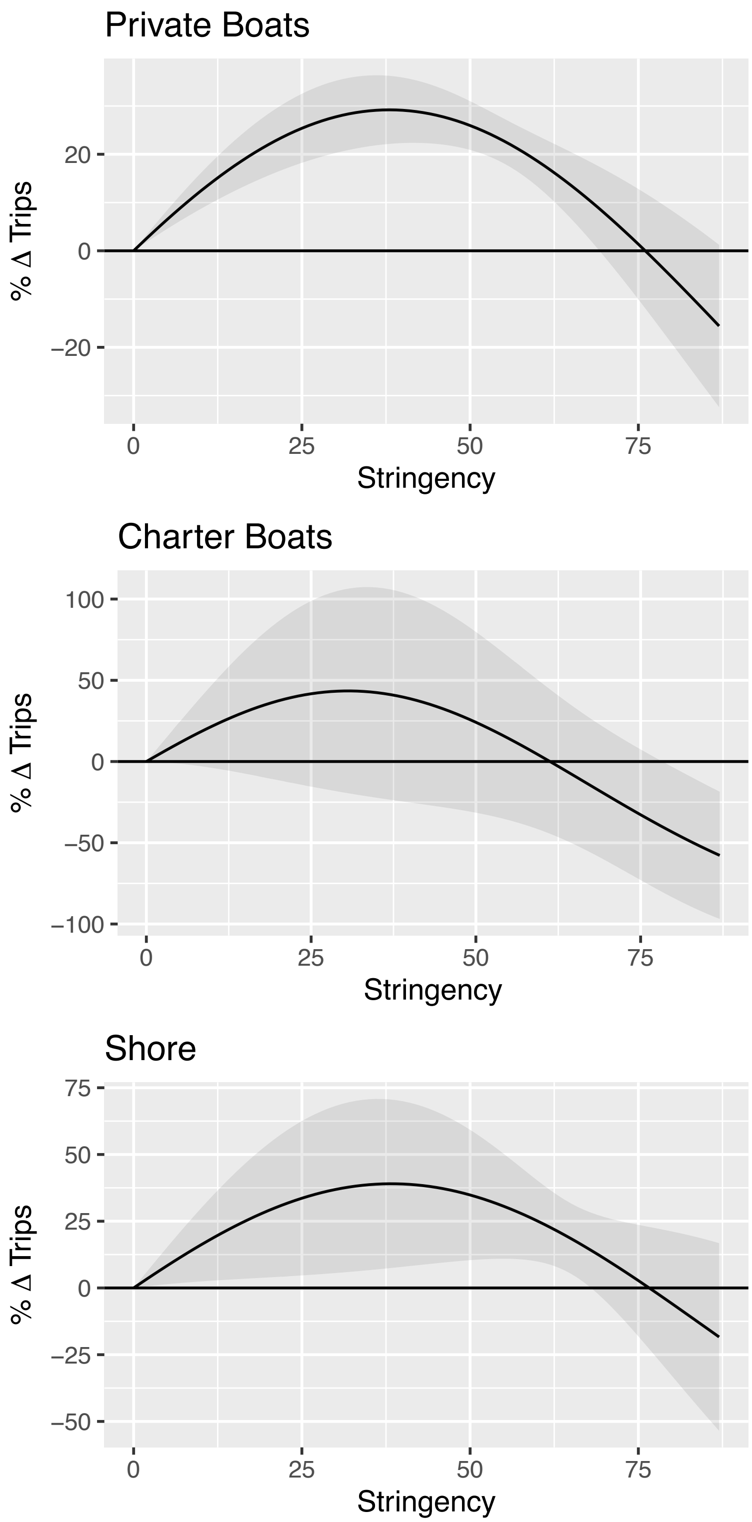


Figure 3.3: Predicted Percentage Change in Trips as a Function of the Stringency Index

and additionally having an increased desire to go fishing after having been unable or unwilling to go fishing during the high stringency period. Our data do not provide sufficient resolution to explicitly model such intertemporal substitution dynamics, which would be best modeled at the level of individual anglers if individual-level panel data on fishing activity were available, but nonetheless these dynamics are a plausible explanation for the trend observed in the aggregate data.

3.4 Conclusion

In this paper, we measure the net effect of COVID policy stringency on recreational marine fishing effort, among 16 states in the eastern U.S. To do so, we rely on data from the FES to measure the total number of angler-trips in each state-month, and on the OxCGRT dataset to measure the average level of COVID policy stringency in each state-month. We estimate a fixed effect Poisson model to estimate this non-monotonic relationship, allowing for state-specific seasonality trends and time trends in the number of fishing trips.

Our results represent a composite effect of the actual local COVID policies, as well as localized attitudes, risk tolerances, and beliefs about COVID-19 which are correlated with COVID stringency. In particular, we see the inverse-U relationship as the outcome of countervailing forces: (1) stringency reduces available substitutes of indoor or high-density outdoor activities, increasing the attractiveness of fishing, which is the dominant force on the low-stringency side of the curve, and (2) COVID risk makes even a relatively distanced activity, such as fishing, less appealing compared to entirely in-home activities such as cooking, backyard games, video games, etc., which in addition to site closures is the dominating force on the high-stringency side of the curve.

Overall, our results for the relationship between COVID policy stringency and total angler-trips taken, which show an approximately 20% increase in private boat trips taken at the levels of COVID stringency which predominated in most states by late 2020 into 2021, suggest that marine fishing activity was actually relatively stable throughout 2020, at least in comparison with socioeconomic indicators like unemployment, GDP, etc. which saw 3- or 4-sigma changes, sometimes

the largest month-to-month changes since these statistics have been recorded.

This paper has considered specifically the impact of state-level COVID stringency on in-state marine fishing trips taken by residents. Future research may also consider the question of cross-state substitution of fishing trips, a question which can not be answered sufficiently with the MRIP data we rely on in this paper. Such cross-state substitution may be due to anglers seeking places with lower COVID stringency to vacation, or perhaps also by an increase in mobility due to the rise of remote work arrangements. One data source which may be appropriate to addressing this question is the National Saltwater Angler Registry (NSAR), a database of all marine fishing license sales for states covered by the MRIP program. The NSAR database contains sufficient information to uniquely identify anglers, and could therefore be used to measure not only the rate of sales of out-of-state temporary licenses to anglers from various origin states, but also the rate of migration of anglers from one state to another based on where they have purchased in-state licenses. Such analysis would likely show an increase in angler migration to major destinations like Florida during COVID, but what is less clear is whether a general pattern of migration toward states with lower COVID stringency would be observed, such as from New York to New Hampshire.

In future research, it may be possible to assess the impact of COVID on fishing activity in different ways. For instance, one data source that could be utilized is foot traffic data based on cellphone GPS tracking. In this way it would be possible to measure the level of activity near the coastline on a much finer spatial and temporal scale than is possible with the survey data we rely on in this paper. It would be a challenge to distinguish between activities such as beach going and other uses of the coastline versus fishing-specific activity with this data source, and additionally it would likely not be possible to assess boat-based activity, however the advantages in terms of data resolution may still make it an attractive option.

In regards to policy implications, our results may be interpreted in a few ways. For instance, to the extent that our results reflect the increased internal migration within the US, perhaps certain states in the Southeast will need to take account of persistent increased fishing pressure, while other states that have lost population will not. Or, to the extent that the results are driven by

new participants in fishing, it will have to be seen whether these new participants continue to use the fishery before we know if there will be long-run ramifications for management. Relatedly, if increases in congestion (eg. boat ramps being busier, not only due to increased fishing activity but also increased boating in general) have effectively increased the cost of fishing trips, then angler welfare per trip may be reduced, which would have implications for the management of the fishery. Future research may seek to better understand the determinants of the change in effort, as well as the implications of these changes for angler welfare and net benefits from the fishery.

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