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Do Higher Housing Values Make Communities More Conservative? Evidence from the Introduction of E-ZPass

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Abstract

A rich literature exists on the extent to which homeownership is central to American political attitudes. This paper uses the introduction of E-ZPass in Pennsylvania and New Jersey to identify the effect of traffic-reducing transportation infrastructure on property values and, in turn, political behaviors. We develop a model showing that faster travel times results in individuals preferring a lower tax rate, as those who face the lower travel times are made effectively wealthier. Next, we present empirical evidence consistent with this theoretical result. We show that voting precincts near newly introduced E-ZPass toll plazas experienced a sharp increase in property values relative to similar precincts near non-E-ZPass exists, giving us leverage to identify the causal effect of property value changes on voting. We find that positive shocks in property values are associated with an increase in Republican vote share.

1 Introduction

2 Relationship to the Literature

At least since the 1940s, social scientists have relied on survey research to understand the factors affecting voting behavior, yet only recently have scholars begun to use experimental and observational evidence to gain "causal leverage for analyses of voting behavior" (**Bartels2010**). Early studies of voting behavior established that although individual vote choice is susceptible to election-year specific shocks, such as an unusually popular presidential candidate, the way an individual votes in most elections is for the most part a function of their personal identity characteristics (**Converse1966**). According to this line of research, the most significant determinants of long-term voting behavior include party identification, ethnicity, gender, age, religion, education, and occupation (**Lazarsfeld1948**; **Berelson1954**; **Campbell1960**; **Stanley2006a**). Noteworthy too are the factors these studies did *not* find significant for long-term voting behavior: preferences about political issues, for example, and economic self-interest. This is not to say that financial well-being was ever considered irrelevant for voting behavior; the literature is replete with studies demonstrating that the success of incumbents is tied to that of the economy (**Tufte1975**; **Meltzer1975**; **Hibbs1987**). Yet this effect appears to be based largely on "sociotropic" evaluations of national economic performance, not personal economic experience, and evidence for the latter's effect on voting behavior is at best mixed, even

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in the short term (**Linn2010**). For early survey research, long-term voting behavior was for the most part reducible to an individual's demographic characteristics, and would not predict that increasing an individual's wealth or income would not change their vote choice, all else equal. This view is in considerable tension with formal models whose comparative statics expect such a relationship between an indivdual's economic situation and their voting behavior (**Romer1975**; **Roberts1977**; **Meltzer1981**).

Although the conclusions of survey research just described have been replicated numerous times (**Nie1976**; **Smith989**; **LewisBeck2008**), in the past decade these results have nonetheless faced significant challenges from scholars bringing to bear new data sources and applying new data analysis techniques. In particular, ideology and economic interest are gaining renewed recognition as determinants of life-long voting behavior. **Ansolabehere2008** show that the apparent incoherence of individual issue attitudes may largely be a result of measurement error, and that by averaging multiple question responses one gets a more stable estimate of individual issue attitudes, which become nearly as predictive of voting behavior as partisan identification. **Gelman2007** use multilevel-modeling of exit poll data to establish that income can be an important determiner of individual vote behavior, but its predictive power depends greatly on geography: in poor states like Alabama those who make little vote very differently from those who make a lot, while in rich states like Connecticut the difference is barely present. **Hersh2015** building on the results of **Gelman2007** use highly disaggregated registration, census, and election returns to show that income is a significant determiner of voting behavior only in Congressional districts with large minority populations. **Arunchalam** use height as an instrument for studying the effect of income on support for the Conservative Party in the United Kingdom.

This article continues in the vein of these most recent papers, using data sources and methods not frequently used in the voting behavior literature to support the claim that economic experience can have a significant effect on which party individuals support. Our decision to examine property values rather than income naturally invites comparison with the approach of **Ansell2014** In that paper, Ansell subsets ANES survey data to Metropolitain Statistical Areas where the Federal Housing Adminstration (FHA) collects data on home values. He uses these data to calculate the effect of home value appreciation on attitudes toward a redistributive program. He finds that increasing property values makes individuals less likely to support redistribution. For robustness, he looks at similar data from the UK as well as cross-country survey data. Ansell's paper nicely compliments ours. His data sources do not readily lend themselves to causal identification strategies, whereas ours do, while our outcome variables of donations and votes are only rough proxies for attitudes toward redistribution, which are better captured by surveys. Together, these papers present consistent evidence of a substantial connection that increasing property values make individuals less supportive of redistribution and more likely to support right-wing political parties.

Our work also implicitly relies on insights developed in the study of urban economics and economic geography."Monocentric city models" that explain how the economy organizes space have a long history in economic thought. First proposed by **Thunen1826** to study crop usage, this model and its refinements remain widely used in the theoretical and empirical literature (**Alonso**; **Fujita1999**; **BaumSnow2007**). The important implication of such models for our purposes is that the price individuals are willing to pay for residential property is inversely related to the costs of getting from that property to the city center. Thus, a change in travel costs should result in a change in property values. Our study exploits a shock to travel-costs identified by **Currie2011a** the introduction of EZ-Pass tolling plazas. According to a study by the New Jersey Turnpike Authority, the introduction of EZ-Pass reduced total delays at toll locations 85% in the year after its adoption, decreasing the amount of time cars were delayed by 1.8 million hours (**NJT2001**). Considerable evidence supports the conclusion that residential property values are responsive to shocks that affect travel costs, and indeed we confirm that this is true for EZ-Pass introduction. Although these changes in property values should eventually result in changes to who lives in communities, in the short term individuals will not necessarily be willing or able to sell their homes. In the Appendix, we formalize these notions by developing a two period model where individuals first choose homes given a personal endowment and assuming they will have to pass a balanced budget to support the provision of public goods, and then in the second period either receive a reducation in travel costs or do not. In line with our informal predictions, the formal model produces comparative statics showing that those who receive a windfall reduction in travel costs are more reluctant to support redistributive programs. W

3 Research Design

Our empirical approach to understanding how changes in individual wealth affect voting behavior relies on the fact that the introduction of E-ZPass served as a shock to transportation costs for some localities but not others. E-ZPass plazas were not selected endogenously at the time of introduction, but replaced already existing toll structures.[1] We propose IV and conditional difference-in-difference estimators to evaluate the change in political attitudes between precincts that were exposed to E-ZPass (and that thus experienced a sharp rise in property values) and those that were not (Donald and Lang 2007).

For this study, we rely on a combination of low-level voting, political contribution, census and geographic data. All geographic analysis, in particular data about the relative distance of two locations, were conducted using ArcGIS with a national highway map provided by the developer of ArcGIS. The location of EZ-Pass tolling booths were taken from the replication dataset to **Currie2011a** and this data was replicated and supplemented with data collected from Department of Transportation websites. Precinct-level data on the number of registered voters and the number of votes received by each party in Presidential elections, as well as shape files detailing the geography location of each precinct, were taken from **Ansolabehere2014** Contribution data collected by the Federal Election Commission (FEC) was reported at the individual level, however this data is reported by zipcode. Census data used for matching and for constructing our instrumental variable were taken from the 2000 decenial census and the 2005-2009 American Community Survey (ACS).

Because these data are reported at different levels of aggregation, substantial effort was required to create a dataset suitable for analysis. Formally, we consider an observational unit in our study to be the voting precinct. Voting precincts are contiguous areas, typically about 5 square miles in area, and are roughly the same size as a census block group, the smallest area at which census data are reported. Zipcodes are usually much larger areas containing multiple precincts or census block groups. Since the boundaries of precincts, census block groups, and zipcodes are generally not identical, we use areal interpolation to impute the data from these other geographic area to the precincts. ArcPython replication code is provided for those interested in seeing exactly how each column in our dataset was constructed, however the basic idea is no more complicated than taking a weighted average, with weights based on the amount of area that overlaps between the precinct and the other geographic area one is interpolating. Also crucial to our analysis is the distance of each precinct to the highway. For this purpose we consider the polling place as coded in **Ansolabehere2014** to be the precinct's location, and we operationalize distance to the highway to be the network or "over-road distance" to the nearest highway entrance.

This analysis must address a key issue around internal validity. That is, to obtain any causal estimate, it is first necessary to identify treatment and control groups. However, studies that use geographic boundaries as identification mechanisms are open to criticism around how the treatment and control groups are specified. If the treatment and control groups can be defined arbitrarily, then a study may be vulnerable to data mining problems. As a consequence, this analysis uses two definitions of treated and control groups in order to illustrate that our results are robust to these two specifications.

Our analysis first uses a conditional difference-in-differences model with matching. This method takes as the treated group those precincts living corollary close to an E-ZPass exit toll plaza, who therefore are more likely to have received an exogenous increase in their housing price. Whether "close" should mean 5, 10, or 15 miles is unclear *a priori*, thus a sensitivity analysis is required to assess the dependency of the results on how one defines "closeness."" We must also define a reasonable control group that could have received a reduction in traffic but did not. To construct this control group, we examine precincts close to exits on major highways without E-ZPass tolls. However, particularly close to metropolitan areas, there are precincts that are both within 10 miles of an E-ZPass highway and 10 miles of a highway without E-ZPass. In order to get genuine separation of treatment and control groups, we create a rule excluding those precincts that are in one testing group but are on the cusp of being on the other. The exclusion rule should have a radius at least as big as the inclusion rule to guarantee perfect separation. But one should also be concerned that citizens may be willing to drive further to take a non-toll road than one with tolls (i.e. in citizens' everyday experience, perceived "closeness" to an E-ZPass exit may differ from perceived "closeness" to a non-E-ZPass exit). According to this line of reasoning, the radius of the exclusion rule should be a bit larger than the

[1] We later perform a series of robustness checks to ensure that the initial (endogenous) placement of the tolls does not confound our inferences.

radius of the inclusion rule. If the exclusion radius is too large, however, one will exclude many units in metropolitan areas where different highways intersect. While, in principle, treatment and control groups could each have their own inclusion and exclusion rules, we shall assume that treatment and control each have the same rule for inclusion and the same rule for exclusion. We conduct our basic analysis including a precinct in a testing group if it is within 12 miles of the highway it belongs in, but not within 18 miles of a highway it should not belong in. We then replciate the diff-in-diff on a grid of plausible values.

Our analysis next uses an instrumental variables (IV) approach. This method does not posit the existence of a separate treated and control group. Rather, we use distance to an E-ZPass toll plaza as an instrument for housing price appreciation for those precincts within some fixed distance from the toll plaza. This framework depends on two assumptions. First, distance to E-ZPass toll plaza must be correlated with the endogenous explanatory variable (i.e. gain in housing price). This correlation is strong (with a t-statistic of over 20). Next, the instrument cannot be correlated with the error term of our explanatory equation predicting change in Democratic 2-party vote share. This assumption would be violated if distance to E-ZPass toll plazas affected Democratic 2-party vote share even when housing prices are kept constant.

In what follows, we show that our results are robust to the specification of the treated and control groups, as well as to the method of inference. We have a slight preference for the conditional difference-in-difference approach because this specification of treated and control groups seems more natural. Areas 2 vs. 10 miles from an E-ZPass toll plaza may be more systematically different from each other than precincts near E-ZPass toll plazas and those near non-E-ZPass traffic exits. Even so, it is important to give careful thought to the specification of the treated and control groups in geographically-based analyses, and it is best to show the robustness of a result across a range of specifications. We also take care to show that other important predictors of Democratic support do not undergo significant changes following the introduction of E-ZPass.

4 Results

4.1 Conditional DD

Table 1: Pre-treatment balance. Data from the 2000 Census. Sample size: 1324 treated and 1324 control units.

	Overall		*Treated*		*Controls*		*Difference*	
	—		—		—		—	
	Mean	(S.D.)	Mean	(S.D.)	Mean	(S.D.)	Dif.	(S.D)
Average income	\$61030.94	(34783.57)	\$61497.61	(30444.52)	\$60564.26	(38642.63)	\$933.36	(1235.67)
% bachelors	16.3	(9.3)	16.37	(8.5)	16.23	(10.03)	0.14	(0.33)
% black	4.86	(11.43)	5.76	(12.45)	3.95	(10.23)	1.81	(0.4)
% professional degree	51.49	(3.03)	51.57	(2.69)	51.41	(3.34)	0.16	(0.11)
% female	15	(6.94)	14.98	(7.67)	15.03	(6.13)	-0.05	(0.25)
% of pop. over 65	2.2	(2.53)	2.21	(2.68)	2.2	(2.37)	0.01	(0.09)
% in same house as in '95	62.66	(11.73)	62.67	(11.56)	62.65	(11.91)	0.03	(0.42)

Table 2: Difference-in-difference results for change in Democratic vote share. Matching/control variables include precinct-level covariates on average income, percent of residents living in the same house as in 1995, percent of residents who are black, percent of residents who are over the age of 65, percent of residents with a bachelor's degree. All matching/control data are from the US census.

	Point Estimate	(Bootstrap S.D.)	(Block Bootstrap S.D.)
Baseline model	-2.37	(0.20)	(0.99)
With covariate adjustment	-1.93	(0.25)	(0.76)

Table 3: Difference-in-difference results for change in average home price. Matching/control variables include precinct-level covariates on average income, percent of residents living in the same house as in 1995, percent of residents who are black, percent of residents who are over the age of 65, percent of residents with a bachelor's degree. All matching/control data are from the US census.

	Point Estimate	(Bootstrap S.D.)	(Block Bootstrap S.D.)
Baseline model	\$81,785	(4,868)	(17,580)
With covariate adjustment	\$68,237	(5,039)	(12,485)

4.2 Validity Checks

Although our matching methods acheived balance on almost all covariates the literature has generally considered significant at predicting long-term voting behavior, treated units had an average African-American population of about 6% while control units had an average African-American population of about 4%, and this difference was statistically significant. One concern is that changes in racial voting behavior between the 2000 and 2004 election could explain some of our results. The fact that the black population is so small in both groups decrease this possibility, however as a precaution we provide here a brief analysis of exit poll and turnout data for each state and each election year, in order to estimate how changes in African-American and non-African American voting behavior might effect our results. According to these exit polls, Gore was supported by 90.5% of black voters and 51.4% of other voters, while Kerry was suppoted by only 83.4% of black voters and 47.3% of other voters.[2] At the same time, about 53% of the black population and 56% of the non-black population voted in 2000, while 61% of the black population and 66% of the non-black population voted in 2004. Assuming that these estimates of average turnout and support by race are the same in treated and control units, we can estimate the difference-in-difference purely due to racial imbalance as -0.0007, two orders of magnitude smaller than our effect. [3] Thus, racial imbalance between treated and control does not seem to pose a serious threat to our analysis of voting behavior. One final threat to inferential validity is the possibility of unobserved confounders, a particularly serious problem when dealing with data that has a high degree of geographic correlation. While one cannot possibly hope to exhaust all such possible confounders, we do think it appropriate to consider one obvious confounder: weather. **Gomez2007** presents regression estimates of the effect of rain and snow on turnout and propoensity to vote Repbulican. They find that for every inch of rain above the normal amount of rain a place gets, there is on average about a 0.83% in turnout and about a 2% increase in Republican vote. Since weather is also spatially correlated, it has the potential to confound the estimates for our vote-share dependent variable, although not for our dependent variables based on donations. The perfect storm, so to speak, would be if it rained heavily along I-76 and I-95 (Southern Pennsylvania and Western New Jersey, respectively) in 2004, but nowhere else in 2004, and everywhere else in 2000. Fortunately, the perfect storm did not happen. Figure 2 presents precipitation maps from November 7th, 2000 and Nov 2nd, 2004 as reported by the National Oceanic and Atmospheric Administration (NOAA). According to these maps, on election day 2000 there was about 1/100th an inch of precipitation in Western Pennsylvania, while on election day 2004 there was about 1/4th of an inch of rain in Western Pennsylvania, about 1/10th of an inch in Central Pennsylvania, and a touch of rain around New York City. This is not a great deal of rain. Historical data available through Weather Underground characterizes conditions in the apparent epicenter, Pittsburgh, as "light rain" for most of the afternoon, cloudy in the morning and evening, with some additional rain around 9 PM. If one accepts the estimates in **Gomez2007**

[2]Here all figures correspond to two-party vote, consistent with the approach taken throught the paper. Individuals who say they supported Nader or some other candidate are therefore dropped in our analysis of these exit polls.

[3]Formally, we use the following equation:

$$\left(\frac{T_B^{04}D_B^{04}+T_O^{04}D_O^{04}}{T_B^{04}+T_O^{04}}-\frac{T_B^{00}D_B^{00}+T_O^{00}D_O^{00}}{T_B^{00}+T_B^{00}}\right)-\left(\frac{C_B^{04}D_B^{04}+C_O^{04}D_O^{04}}{C_B^{04}+C_B^{04}}-\frac{C_B^{00}D_B^{00}+C_O^{00}D_O^{00}}{C_B^{00}+C_B^{00}}\right)$$

Here, T_B^{0X} is the fraction of the black population that voted in the year $200X$ multiplied by the fraction of the population that is black in the treated units, while T_O^{0X} indicates the fraction of the population that voted among other races times the fraction of the population that is not-black. D_B^{0X}, D_O^{0X} indicates the proportion of blacks and non-blacks who supported the democratic Presidential candidate in year XX. C_B^{XX} is the fraction of the black population that voted in the year $200X$ multiplied by the fraction of the population that is black in the control units, with C_O^{XX} is defined analogously.

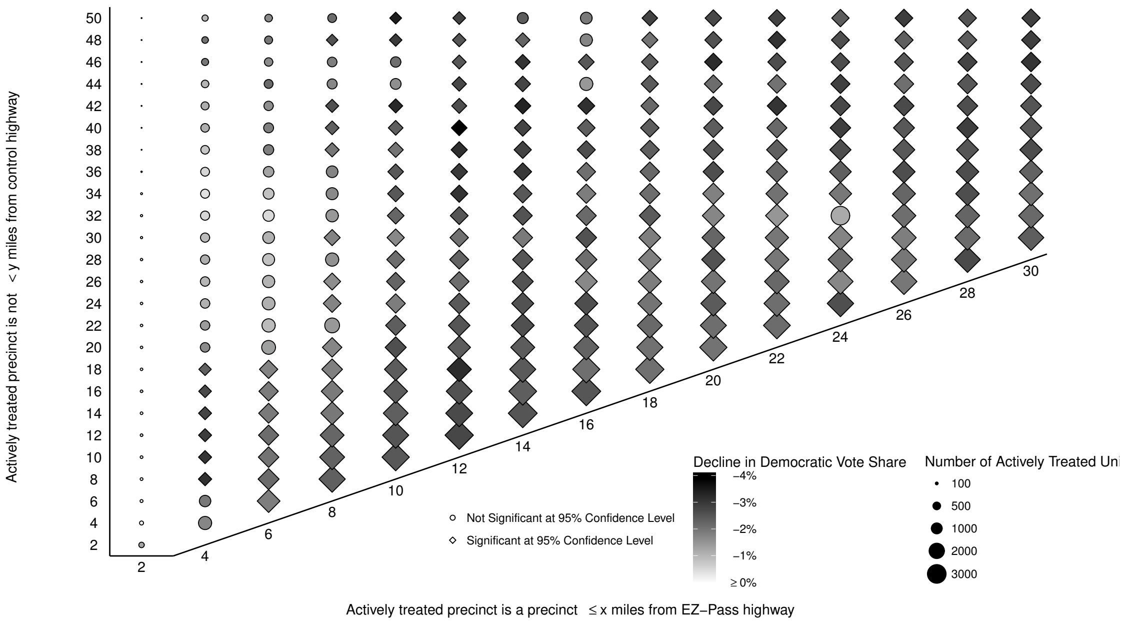


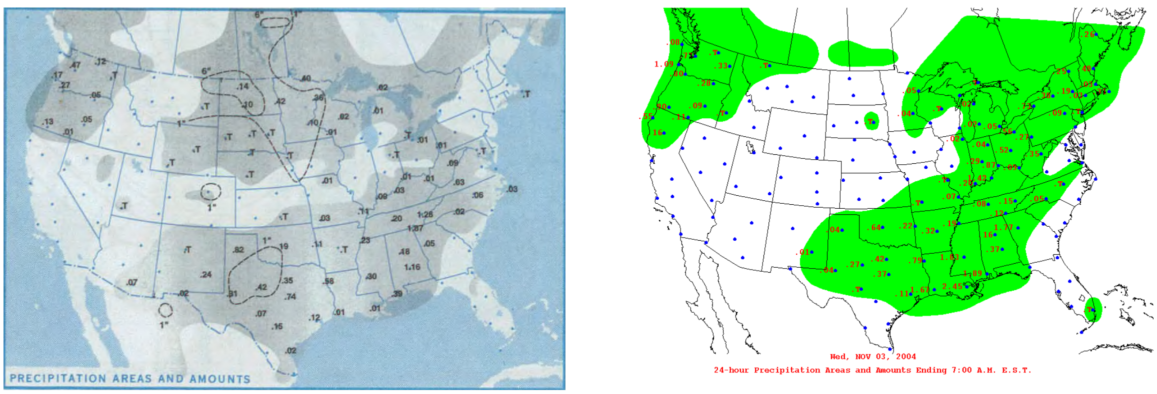
Figure 1: Sensitivity Analysis. X-Y position gives the conditional difference in difference for a given combination of inclusion rule and exclusion rule. For example, at (12,18), one sees the effect of saying treated precincts are those within 12 miles of an EZ-Pass highway but more than 18 miles from a non EZ-Pass highway, and similarly for control units. Effect size is indicated by the darkness of the shape, number of matched units is indicated by the area of the point, and results that are significant at the conventional 95% threshhold are indicated by diamonds. Generally, effect magnitudes and significance are not sensitive to choice of rule, changes in effect magnitude are gradual.

the rain differential between 2004 and 2000 is not enough to make a significant dent in our estimates, even in a worst case analysis.[4] Moreover, the rain appears to affect treated and control regions evenly in 2004, if anything control regions were hit harder. To be especially sure that rain has not interfered our inference, we estimate the effect on turnout due to EZ-Pass. We find that there was no significant effect on turnout, which one would expect if rain was a serious confounder, and indeed the sign goes the opposite direction.

5 Robustness Check

6 Conclusion

[4]The normal rainfall in Pennsylvania is about 0.1 inches, so a worst case analysis that assumes 0.3 inches of rain in every treated unit but a normal amount of rain in every control would still only explain a 0.4% increase in Republican vote. The effect we found was an order of magnitude larger.



(a) 7AM Nov. 7 - 7AM Nov. 8, 2000

(b) 7AM Nov. 2 - 7 AM Nov. 8, 2000

Figure 2: National weather maps from the two election days used in our study (Source: National Oceanic and Atmospheric Administration Central Library Data Imaging Project). The chart shows areas that had precipitation during the 24 hour period starting at 7 AM EST the day indicated until 7AM EST the next day. All numbers are reported in inches rounded to the nearest $^1/_{100^{\text{th}}}$, except for **.T** which refers to trace amounts of precipitation.