Property Values & Political Preferences: Evidence from the Adoption of E-ZPass

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Abstract

This study uses the introduction of E-ZPass in Pennsylvania and New Jersey in order to identify the effect of changing property values on support for US political parties. We show that E-ZPass caused a decline in support for Democratic Presidential candidates. We argue that this effect is not spurious, but rather due to the fact that precincts near E-ZPass toll locations experienced a downward shock to transportation cost and a corresponding rise in average home price. We rule out other explanations dealing with turnout, community change, or rising incomes, and replicate the design in Ohio. By conducting a sensitivity analysis, placebo test, and using multiple dependent variables, we demonstrate robustness.

1 Introduction

This study uses the introduction of E-ZPass in order to identify the effect of changes in property values on support for US political parties. In 2002, E-ZPass was introduced along toll roads in New Jersey and Pennsylvania. Drivers who purchased E-ZPass transponders were able to avoid manual toll collection, while driver who did not purchase the transponder nonetheless benefited from shorter lines at the manual tolls. According to estimates published by the Department of Transportation of New Jersey, the introduction of E-ZPass reduced total delays at toll locations 85% in the year after its adoption, resulting in an average decrease of about 10% in the daily commute of those who used highways with E-ZPasses installed (Authority New Jersey Turnpike 2001). We would expect that, over time, driving behaviors would change as more individuals elect to use the now speedier toll roads. This change might have the effect of reducing traffic on local streets as well. Communities near some sections of highway saw no substantial change in traffic patterns, whereas communities near other sections of highway saw substantial decreases in travel times and less congestion on local roads. As a consequences, we would expect (and find) that communities near newly introduced E-ZPasses receive a positive wealth shock in the form of higher property values. The introduction of E-ZPass is thus fairly unique in that it creates intra-state variation in property values, but without creating the side-effects typically associated with government stimulus programs. We explore the effect of changes in property values on how citizens express their support for political parties at the national level.

Although land values have been widely-recognized as an important variable in macro-economic models and by historical political economy, the topic remains underexplored by scholars of contemporary political behavior. This is surpsing. Most Americans today have invested a plurality of their wealth in their homes,

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with mortgages also constituting the single biggest debt obligation that households face. According to most recently available statistics, the US homeownership rate stood at about 65 percent. Compared to renters, homeowners are more likely to vote, to be conservative, and to hold strong policy preferences (Gilderbloom and Markham, 1995; Kingston, Thompson, Eichar, 1984; Schwartz, 2008). As Lacy and Soskice (2015) write, "the decisive voter in local elections is likely to be a home owner." This group thus constitutes a pivotal constituency in American politics.

One reason that scholars of American politics have not paid sufficient attention to residential property is federalism. In the American system many of the most important decisions facing homeowners - such as zoning, schooling, and policing - are made primarily at the state or local level. Yet many aspects of federal policy do affect home values. Indeed, home owners are directly impacted by federal tax and monetary policy, to say nothing of the myriad federal programs designed to support home ownership, or the bank bailout of 2008. Although historically there has been bipartisan support for programs increasing homeownership, the fact that both parties have supported similar polices does not mean that such policies have benefited each party equally.

Methodologically, we isolate causal effects using a conditional difference-in-difference estimator, using matching as well as bootstrap and block bootstrap standard errors. Instrumental variable (IV) estimates are also presented. Throughout the study, we primarily examine precinct-level election outcomes from 2000 and 2004. In both elections, property and taxation were at the center of the political debate, providing an ideal setting for this analysis. We also provide a detailed portrait of the communities impacted by E-ZPass to confirm that E-ZPass was associated with no other significant socio-economic changes. We areal interpolate census block-group data to provide state-of-the-art estimates of the change in racial and economic makeup of communities near the infrastructure improvement. We find that the introduction of E-ZPass did not significantly impact the socio-economic attributes of citizens. E-ZPass is primarily associated with a steep rise in property values, and also with an increase in conservative political behavior. These empirical results are consistent with a formal model developed in the Appendix describing the relationship between property ownership and voting behaviors. By conducting a sensitivity analysis, placebo test, and using multiple dependent variables, we demonstrate that our results are robust.

2 Relationship to the Literature

At least since the 1940s, social scientists have relied on survey research to understand the factors affecting voting behavior, but only recently have begun to use experimental and observational evidence to gain "causal leverage for analyses of voting behavior" (Bartels 2010). Early studies of voting behavior established that individual vote choice is susceptible to election-year specific shocks, such as an unusually popular presidential candidate. However, these studies also found that the way an individual votes in most elections is largely a function of their personal identity characteristics (Converse 1966). 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 (Lazarsfeld et al. 1948; Berelson et al. 1954; Campbell et al. 1960; Stanley and Niemi 2006). 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 (Tufte 1975; Meltzer and Vellrath 1975; Hibbs 1987). Yet it has been argued that this effect is largely based on "sociotropic" evaluations of the national economy, not personal economic experience. Evidence for the latter's effect on voting behavior has been mixed (Linn et al. 2010). In this way, early survey research argued that long-term voting behavior was mostly reducible to an individual's demographic characteristics, and that, all else equal, increasing an individual's income would not change their vote choice. 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 (e.g., Romer 1975; Roberts 1977; Meltzer and Richard 1981).

Although the conclusions of survey research just described have been replicated numerous times (Nie et al. 1979; Smith 1989; Lewis-Beck et al. 2008), in the past decade, these results have faced significant challenges from scholars bringing to bear new data sources and applying new statistical techniques. In particular, ideology and economic interest are gaining renewed recognition as determinants of life-long voting behavior. Ansolabehere et al. (2008) show that the apparent incoherence of individual issue attitudes may largely be a result of measurement error. If one averages multiple question responses, estimates of individual issue attitudes are stabilized, and become nearly as predictive of voting behavior as partisan identification. Gelman et al. (2007) use multilevel-modeling of exit poll data to establish that income is an important predictor of individual vote behavior, but its predictive power depends greatly on geography: in poor states like Alabama, the poor vote very differently from wealthier citizens, while in rich states like Connecticut the difference is barely present. Hersh and Nall (2015), building on the results of Gelman et al. (2007), use highly disaggregated registration, census, and election returns to show that income significantly influences voting behavior only in Congressional districts with large minority populations. Lastly, Arunchalam and Watson 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 vote choice. Our decision to examine property values rather than income naturally invites comparison with the approach of Ansell (2014). In his 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 political attitudes, 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 conservative political parties.

Our work also implicitly relies on insights developed in the study of urban economics and economic geography. The "monocentric city model" has a long history within economics and seeks to explain how the economy organizes space. First proposed by Thunen (1826) to study crop usage, this model and its refinements remain widely used in the theoretical and empirical literature (Alonso 1964; Fujita et al. 1999; Baum-Snow 2007). For our purposes, an important implication of such models 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. Hence, a change in travel costs should result in a change in property values. The idea of exploiting the introduction of E-ZPass for econometric inference comes from Currie and Walker (2011), who look at the effect of decreased traffic and car polluton on infant health. Considerable evidence supports the conclusion that residential property values are responsive to shocks that affect travel costs (Levkovich et al. 2016), and indeed we confirm that this is true for the E-ZPass introduction. Changes in property values may eventually result in changes to who is able to live in communities, but in the short term, residents will not necessarily be willing or able to sell their homes. In the Appendix, we formalize these notions by developing a two period model: in the first period, individuals choose homes given a personal endowment and assuming they will have to pass a balanced budget to support the provision of public goods. In the second, they 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.

3 Research Design

In order to understand how changes in individual wealth affect voting behavior, we rely on the fact that the introduction of E-ZPass reduced transportation costs for some localities but not others. 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 rise in property values) and those that were not (Donald and Lang 2007). This shock is plausibly exogenous because E-ZPass plazas were not selected strategically at the time of introduction, but replaced already existing toll structures. We also perform a placebo analysis to ensure that the initial (endogenous) placement of the tolls does not confound our inferences.

We rely on a combination of low-level voting, political contribution, census and geographic data. All geographic analysis were conducted using ArcGIS with a national highway map provided by ESRI, the developer of ArcGIS. The location of E-ZPass tolling booths were taken from the replication dataset to Currie and Walker (2011), and this data was replicated and supplemented with data collected from Department of Transportation websites.

We use three measures for Democratic support, our outcome of interest. First, we examine the two-party Democratic vote share in presidential elections. We define this quantity as the total number of votes cast for the Democratic candidate in each election divided by the sum of votes cast for either the Democratic or Republican candidate. Next, we also examine two-party Democratic cash share. This quantity is defined as the total dollar amount of campaign contributions given to the Democratic candidate for President divided by the total dollar amount contributed to either Republican or Democratic candidate. Finally, we also examine the support from stationary campaign contributors in 2000 and 2004. Stationary contributors are defined as citizens residing at the same address between election years, and who contributed to presidential candidates in both years. Here, we proxy for Democratic support in a precinct by dividing the number of stationary contributors supporting the Democratic candidate for President by the total number of stationary contributors there. Ideally, we would use party registration data from New Jersey and Pennsylvania to form a similar proxy of Democratic support. However, the 2000 voter files in these states were not available. 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 geographic boundaries of each precinct, were taken from Ansolabehere et al. (2014). Contribution data collected by the Federal Election Commission (FEC) was reported at the individual level by zipcode, and address information for contributors is available from Bonica (2013) as well.

Census data used for matching and for obtaining home price statistics were taken from the 2000 decenial census and the 2005-2009 American Community Survey (ACS). Our matching variables are precinct-level statistics, and include average income, percentage of the population with a bachelors or professional degree, percentage of the population which identifies as black, percentage of the population which is female, percentage of the population which is over the age of 65, and percentage of the population residing in the same house as in 1995.

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 a voting precinct. Voting precincts are contiguous areas, typically cover about 5 square miles, and are roughly the same size as census block groups, the smallest area at which census data are reported. Zipcodes are usually 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. The intuition 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 Ansolabehere et al. (2014) to be the precinct's location, and we operationalize distance to the highway (or an E-ZPass plaza) to be the minimum network or "over-road distance" to the nearest highway entrance.

Next, this analysis must address a key, if subtle, study design issue. To obtain a causal estimate, it is first necessary to identify treatment and control groups, or at least some axis that is used to establish intensity of treatment and to enable researchers to compare units. 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 criticisms over data mining. As a consequence, this analysis uses two definitions of treated and control groups in order to illustrate that our results are robust to a range of conceptual and empirical specifications.

Our analysis first uses a conditional difference-in-differences (diff-in-diff) model with matching. This method takes as the treated group those precincts that are close to an E-ZPass exit toll plaza, which therefore are more likely to have received an exogenous increase in average home price. Whether "close" should mean 5, 10, or 15 miles is unclear *a priori*, and 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, there are precincts that are both within, say, 10 miles of an E-ZPass highway and 10 miles of a highway without E-ZPass, particularly close to metropolitan areas. In order to get genuine separation of treatment and control groups, we create a rule excluding those precincts that are too close to being in the alternative testing group. The exclusion rule should have a radius at least as big as the inclusion rule to guarantee perfect separation. One should also be concerned that citizens may be willing to drive further to take a non-toll road than one with tolls. 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 larger than the radius of the inclusion rule. As the exclusion radius increases, however, the sample size necessarily decreases. The units most likely to be dropped are those closer to metropolitan areas where different highways intersect. While, in principle, treatment and control groups could each have their own inclusion and exclusion rules, we assume that treatment and control each have the same rule. We conduct our basic analysis including a precinct in a testing group if it is within 12 miles of that testing group's highway, but not within 18 miles of the alternative group's highway. We then replciate the diff-in-diff on a grid of plausible values for the exclusion and inclusion as a sensitivity test.

Although it is reasonable to think of EZ-Pass introduction as an exogenous shock, independent of changes in Democratic vote share between the 2000 and 2004 elections, it is nonetheless still possible that treated and control precincts systematically differ in terms of relevant background covariates. To address this possibility, which is present even in randomized studies, we match on key covariates which might confound our estimates (Morgan and Rubin 2012). Matching also reduces model sensitivity (Ho et al. 2007), narrowing the range of estimated effects. In the matching routine, we use propensity score matching with a caliper of 0.20. Matching was done without replacement. We also attempted Mahalanobis distance matching and coarsened exact matching, but these approaches gave poorer balance.

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 the nearest E-ZPass toll plaza as an instrument for home price appreciation.[1] This framework depends on two assumptions. First, distance to E-ZPass must be correlated with the endogenous explanatory variable (i.e. gain in home price). This correlation is strong, with a t-statistic of over 10. 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 2-party vote share even when home prices are kept constant.

4 Results

Table 1 on the following page presents summary statistics useful for analyzing covariate balance between matched units, while Figure 1 presents a map showing units by treatment status. After matching, we find that treated and control communities are very similar on background covariates such as average income, percentage of the population over the age of 65, percentage of the population living in the same house as in 1995, percentage of the population which is female, and percentage of the population holding a bachelor's or professional degree.

Although matching has given us a well-balanced sample on most covariates, treated units had an average African-American population of about 6% while control units had an average African-American population of about 4%. Because our n is large, this difference is statistically significant. It is a concern, therefore, 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 decreases 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%

[1] For the purposes of this IV analysis, we examine only those precincts within 20 miles of E-ZPass plazas.

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

Figure 1: *Illustration of matches. The treated precincts are shaded gray, and the control precincts are dotted.*

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

of the black population and 66% of the non-black population voted in 2004. Assuming that these state-wide estimates of turnout and support by race are the same in treated and control units, we can estimate the difference-in-difference in two-party vote share purely due to racial imbalance as being about -0.0007.[3] Looking ahead, we will see that this is two orders of magnitude smaller than the effect we found. Thus, racial imbalance between treated and control does not seem to pose a serious threat to our analysis of voting behavior.

Having established that our treated and control precincts are comparable on observables, we can begin our analysis of the diff-in-diff. First, it is clear that average home price in E-ZPass precincts did increase, as expected. In our baseline model, we calculated the unconditional diff-in-diff for the treated and control communities. We also present the results from a covariate adjusted model. In this adjusted model, we add county-level fixed effects and matching variables as predictor covariates. We then calculate the diff-in-diff

[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 '$0X$. C_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 control units, with C_O^{0X} is defined analogously.

Table 2: *Main difference-in-difference results, comparing treated to control and each outcome variable to its analogue from 2000. Matching/control variables are same as in Table 1.*

Dependent Variable		DiD Estimate	(Bootstrap S.D.)	(Block Bootstrap S.D.)
Average Home Price	Baseline model	\$81,752	(4,949)	(19,926)
	With covariate adjustment	\$79,054	(4,722)	(17,866)
	—			
Dem. Vote Share, 2004	Baseline model	-2.37	(0.2)	(0.97)
	With covariate adjustment	-2.46	(0.24)	(0.98)
	—			
Dem. Vote Share, 2008	Baseline model	-3.1	(0.33)	(1.18)
	With covariate adjustment	-2.6	(0.35)	(1.14)
	—			
Dem. Cash Share, 2004	Baseline model	-7.53	(1.61)	(5.97)
	With covariate adjustment	-7.72	(1.59)	(6.44)
	—			
Dem. Support by Stationary Contributors, 2004	Baseline model	-20.04	(10.25)	(9.68)
	With covariate adjustment	-18.19	(11.16)	(8.43)

estimates using linear regression to increase precision. We find that, according to the baseline model, treated precincts saw an increase in average home price about $\$82,000$ greater than that of the control baseline, well above statistical significance as calculated using both traditional bootstrap and block bootstrap standard errors.[4] The estimated effect of E-ZPass is essentially the same with the covariate adjusted model.

Having seen that E-ZPass precincts received a boost in average home price, we consider the effect of E-ZPass on change in Democratic support in these communities. We present four measures of Democratic support. First, we find that the Democratic presidential vote share between 2000 and 2004 dropped 2 percentage points relative to control. This effect is significant in both the baseline and unadjusted model. If we expand our time horizon and look at the change in Democratic Presidential vote share between 2000 and 2008, we find that this estimated effect increases in magnitude. Next, we can examine changes in the share of campaign contributions going to Democrats. With this proxy for Democratic support, we find a 8 percentage point drop for the Democratic cash share in treated communities relative to control precincts between 2000 and 2004. Finally, using individual contribution data, we identify stationary individuals living in our treated or control precincts who contributed both in 2000 and 2004. If we restrict our analysis to this group only, we find an average drop in Democratic Presidential support of 20 percentage points. Here, we are able to identify a sizable percentage point decline in the likelihood of individual voters contributing to the Democratic Party, allaying concerns that our findings reflect demographic sorting only, and not genuine changes in voting behavior. Table 2 summarizes these results.

The diff-in-diff analysis just presented takes one approach to conceptualizing E-ZPass as a natural experiment, but we can also take a different approach using instrumental variables. In the IV approach, we look at communities within 30 miles of E-ZPass toll plazas, and use proximity to E-ZPass as an instrument for change in average home price. Figure 2 gives justification for this approach. On the left panel, we see the tight relationship between distance from E-ZPass and average change in home price. We find that precincts closest to E-ZPass plazas show the highest change in home price. On the right panel, we see the tight relationship between distance from E-ZPass and average change in Democratic vote share. We find that precincts closest to E-ZPass plazas show the most negative change in Democratic support. The symmetry is striking.

Now, if the exclusion restriction holds, we can proceed with a two-stage least squares analysis. The first stage model uses each precinct's proximity to the nearest E-ZPass plaza as a predictor of change in average home price between 2000 and 2004 after controlling for other background variables. The second stage model uses the predicted change in home price to explain changes in Democratic support, again after controlling for the background variables.

Table 3 presents these IV results. The first stage effect is strong, with a point estimate of 3.76 and a robust standard error of 0.26. Substantively, this first stage indicates that, if we consider areas within 30 miles of the E-ZPass exits, those precincts which were closer to the E-ZPass nexus saw a greater home price increase than precincts farther away. The second stage leads to conclusions consistent with the diff-in-diff analysis. Using

[4]See Bertrand et al. (2004) for why bootstrapping is preferred to traditional standard errors for diff-in-diff estimates.

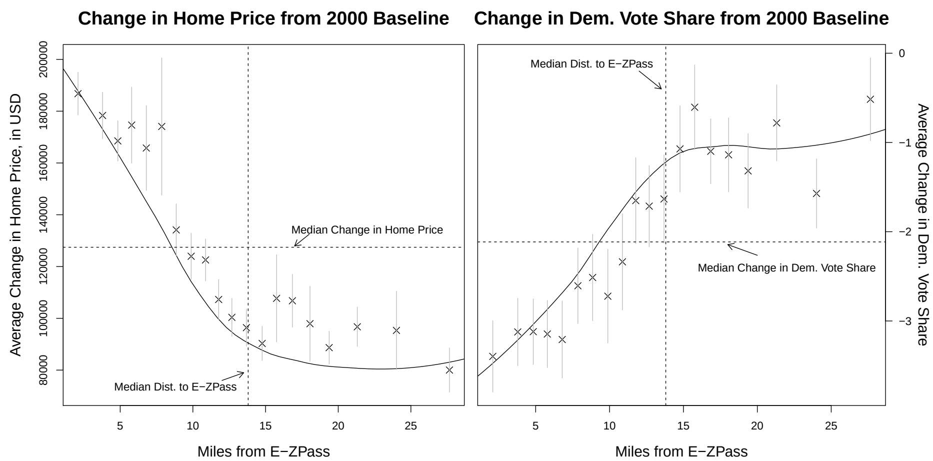


Figure 2: *Distance to E-ZPass and change in average home price and Democratic vote share.*

the same four outcome variables as used before, we find a negative estimated effect of the E-ZPass shock on Democratic support. The largest estimated effect is found for the Democratic support from stationary contributors. According to the IV model, if a stationary contributor received a $50,000 home price increase, we would expect to see their support for Republican presidential candidates increase by about 5 percentage points. The median home price in the US is now about $300,000. Hence, if a precinct were populated only by average homes, and if home prices received an exogenous shock of 16% and increased to $350,000, we would expect stationary contributors from that precinct to flip from 52.5% supporting Democrats in 2000 to 52.5% supporting Republicans in 2004.

The IV and conditional difference-in-difference methods presented so far find a consistent effect over a variety of dependent variables, statistically significant and of similar magnitude. To ensure that these findings are not spurious, we conduct several validity checks.

4.1 Validity Checks

We provide four validity checks for our results. We first show that the previously presented results are not sensitive to the exclusion and inclusion rules. Next, we establish that other important community-level variables are not significantly affected by the introduction of E-ZPass. This fact bolsters our argument that decrease in Democratic vote share is not explained by migration or other community-level changes besides the increase in property values. A placebo test gives evidence that unobserved confounders are not problematic. Finally, we conclude by demonstrating that weather is not a time-varying confounder.

Figure 3 on the next page shows that our main diff-in-diff results are not sensitive to the inclusion

Table 3: *IV analysis. Estimates for demographic control variables are omitted. Control variables include the matching variables previously discussed. Robust standard errors are adjusted for the IV estimation.*

First Stage for Predicting Change in Average Home Price			
Causal variable	Instrument	Estimate	(Robust S.D.)
Change in Home Price from 2000	*Proximity to E-ZPass Plaza*	3.76	0.26

Second Stage for Predicting Change in Dem. Support			
Dependent Variable	Causal variable	IV Estimate	(Robust S.D.)
Change in Dem. vote share, 2004	*Change in average home price*	-0.28	0.09
Change in Dem. vote share, 2008	“ ”	-1.29	0.19
Change in Dem. cash share, 2004	“ ”	-7.83	1.07
Change in Dem. stationary support, 2004	“ ”	-11.50	(3.28)

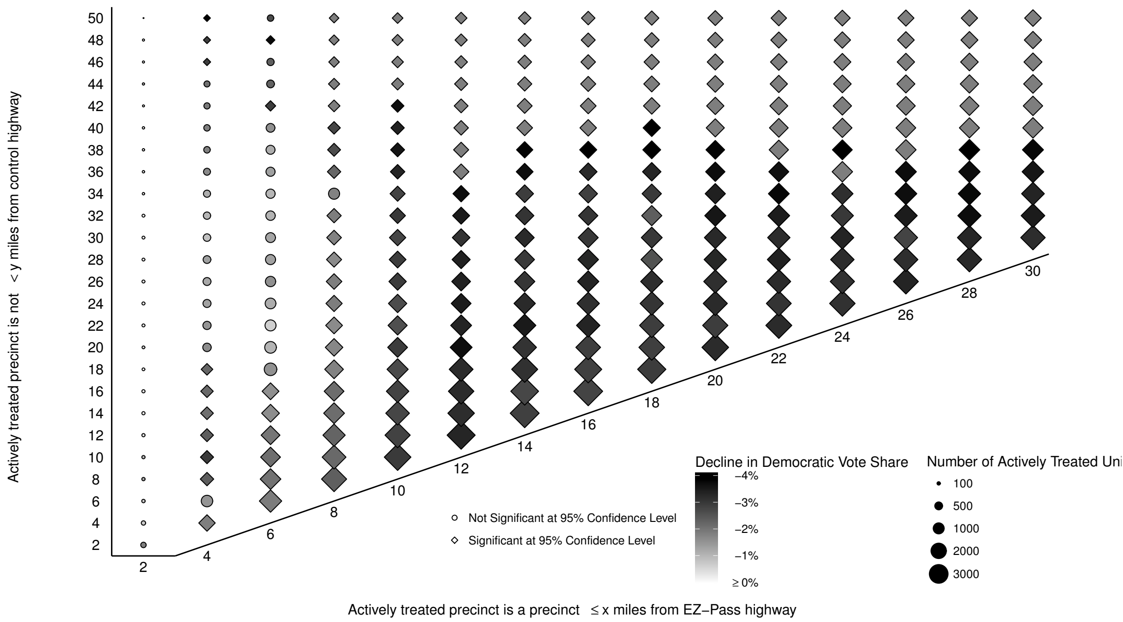


Figure 3: *Sensitivity Analysis. X-Y position gives the conditional difference in difference for a given combination of inclusion rule and exclusion rule. For example, at (12,18), one sees the effect of saying treated precincts are those within 12 miles of an E-ZPass highway but more than 18 miles from 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.*

or exclusion parameters. On this graph, the X-Y position gives the conditional diff-in-diff for a given combination of inclusion and exclusion rule. For example, at (12,18), one sees the estimated effect when treated precincts are defined as those within 12 miles of an E-ZPass plaza but more than 18 miles from a non-EZ-Pass highway. Effect size is indicated by the darkness of the shape, the 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, and changes in effect magnitude are gradual.

Any diff-in-diff analysis is also threatened by the presence of time-varying confounders. The effect of the E-ZPass intervention might be correlated with other changes occurring in treated or control communities. Although we have little leverage in directly identifying unobserved confounding, we can establish empirically whether key observed confounders might be changing along with our outcomes of interest. In particular, we can identify such effects by completing the same diff-in-diff procedure for other factors that are important for predicting Democratic vote share. We find that no key predictors of Democratic vote share underwent changes associated with the introduction of the E-ZPass.

Table 4 shows that average incomes seem to have remained stable in treated E-ZPass precincts relative to their control counterparts. The diff-in-diff estimate ranges from -\$613 to -\$1,450, but are statistically insignificant. We might have been concerned that E-ZPass might have pushed out poor residents and led to an influx of richer ones. However, this worry seems unfounded. Indeed, the sign suggests that incomes fell in E-ZPass precincts relative to control areas. Next, since E-ZPass precincts are now more desirable, it could be that these communities experienced an influx of new, potentially more conservative residents. However, we find that, on average, E-ZPass communities did not see a population change compared to the control areas. Changes in turnout might be another confounding variable. If E-ZPass significantly increase or decreased turnout, inferences about the changes in the average Democratic vote share might be suspect. Here too, we find no effect of E-ZPass. Finally, we might wonder whether other characteristics of the E-ZPass

Table 4: *Diff-in-diff results for other key variables.*

	Baseline model —		*With covariate adjustment* —	
Dependent variable	DiD Estimate	(Block Bootstrap S.D.)	DiD Estimate	(Block Bootstrap S.D.)
Average income	-613.81	(2488.12)	-1830.39	(2572.56)
Population	30.88	(30.17)	15.27	(32.61)
Turnout	0.72	(0.79)	0.61	(0.82)
Percent with bachelors	0.64	(0.37)	0.56	(0.43)
Percent black	0.47	(0.35)	0.59	(0.33)

communities were affected by the introduction of the electronic tolls. Could it be that E-ZPass communities experienced changes in racial or educational composition? No, neither the percentage of black residents nor the percentage of residents with a bachelor's degree changed in treated relative to control precincts.

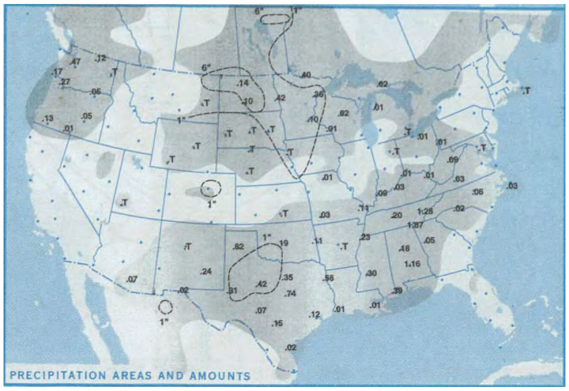
A second threat to inference comes from time-varying *unobserved* confounders. Although it is impossible to disprove the presence of these confounders, we can nevertheless gain some leverage on the problem by considering the placebo case of Ohio. Ohio did not replace its toll structures with E-ZPass electronic tolls until 2008. Thus, we can use Ohio as a placebo case to help identify whether areas near toll exits underwent a unique process of political change relative to non-toll areas. We know that there should be no estimated effect of E-ZPass in this period. If we were to identify such an effect, we would have evidence that time-varying unobserved factors are making precincts near toll exits more conservative than those near non-toll exits. However, using the same matching algorithm, inclusion/exclusion rule, and modeling approach, we find that no such effect is present. We find that precincts near exits that would adopt E-ZPass saw a negative change in their average housing values, and a positive (but insignificant) change in Democratic vote share. These factors support the contention that it is the E-ZPass alone (not time-varying unobserved confounders) which is accounting for the decrease in Democratic vote share.

Finally, because our data has a high degree of geographic correlation, it is important to consider other time-varying factors that might be confounding the analysis. While one cannot possibly hope to exhaust all such possible confounders, we do think it appropriate to consider one obvious confounder: weather. Gomez et al. (2007) 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% decrease 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.

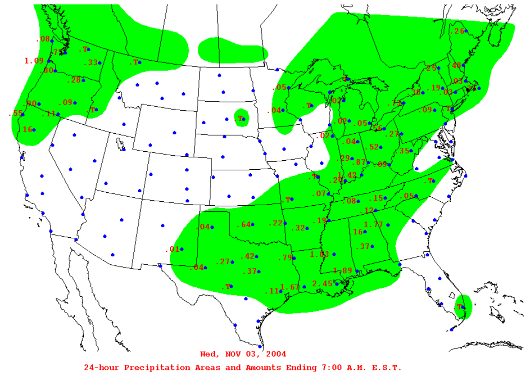
Fortunately, the perfect storm did not happen. Figure 4 presents precipitation maps from November 7th, 2000 and Nov 2nd, 2004 as reported by the National Oceanic and Atmospheric Administration (NOAA). According to these maps, on election day 2000 there was about $1/100^{\text{th}}$ an inch of precipitation in Western

Dependent variable		DiD Estimate	(Bootstrap S.D.)	(Block Bootstrap S.D.)
Change in Average Home Price	Baseline model	$-997	(4,557)	(5,379)
	With covariate adjustment	$3,358	(4,382)	(5,038)
Change in Democratic Vote Share	Baseline model	1.39	(0.53)	(0.68)
	With covariate adjustment	0.74	(0.87)	(0.8)

Table 5: *Placebo analysis. Diff-in-diff results for change in average home price and Democratic vote share for Ohio. 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 Bureau.*



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



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

Figure 4: *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.*

Pennsylvania, while on election day 2004 there was about $1/4^{\text{th}}$ of an inch of rain in Western Pennsylvania, about $1/10^{\text{th}}$ 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 Gomez et al. (2007), the rain differential between 2004 and 2000 is not enough to make a significant dent in our estimates, even in a worst case analysis.[5] 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 with our inference, we estimate the effect on turnout due to E-ZPass. 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 Discussion

We interpret the results of the previous section as demonstrating that increasing property values caused a shift in support away from Democrats and toward Republicans. More work is required to justify our interpretation, and in this section we present our reasoning. Taking the estimates as given, one must address the possibility that what we observe happening to our dependent variables actually represents something else besides a change in propensity to support the Republican Party. For example, a turnout or incumbency effect might be observationally equivalent. In this section, we first argue that the observed change most likely denotes a shift in support for the Republican Party. We then consider whether this shift might have causes other than the rise in property values. E-ZPass alters the environment in many ways, yet only a small subset of these are politically significant. Electronic tolls use radio-frequency identification (RFID) technology, and therefore increase individual exposure to radio waves. However, no one could reasonably believe that electromagnetic fields are making people conservative. We consider a few other possible explanations that seem well-motivated by the political science literature: decreased out-group contact, changes in an individual's emotional environment, and changes in costs of mobilizing constituencies.

First, we consider the possibility that the decline in support for Democratic candidates is based on something besides a change in political attitudes. On its face, it is plausible that a policy related to travel cost like E-ZPass could effect turnout, and ipso facto two-party vote share. We tested for a turnout effect,

[5] 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.

but found none. It is possible that E-ZPass changes electorate composition without changing the overall amount of voting that happens. Yet, reforms like E-ZPass, which make voting easier, tend to help Democrats. More importantly, changes in turnout patterns cannot explain the changes in our dependent variables based on campaign contributions, where we do find significant effects in the same direction as as our vote share variable. Another way that vote choice might change without a shift in underlying political attitudes might be if E-ZPass somehow enhanced the incumbency advantage. Our additional analyses suggest that this unlikely: we replicated the results in 2008 in Pennsylvania and New Jersey, when the election was for an open Presidential seat, and also in 2012 in Ohio, when a Democratic President was the incumbent. All this suggests that the effect is not about incumbency. However, a change in propensity to support the Republicans would explain all these results.

Second, we consider whether other causes besides the rise in property values might explain the increase in propensity to support the Republicans. One possible explanation is that the Republican Party was better able to exploit the changes in transportation cost that E-ZPass brought about. The problem with this explanation is that, if true, one would expect to see a change in vote share *and* turnout, but we only see the former.

Another possible explanation is that E-ZPass decreases contact with out-groups, since individuals are incentivized to drive over taking public transportation. We found some decrease in reported use of public transportation, about 2%. Although shy of significance, this change is on the same order of magnitude as our effect, and we think there might be something to this explanation. Yet, we are hesitant to attribute causal significance to this correlation. For this explanation to bear out, out-group contact would have to absolutely determine support for the Democratic party. We do not think that this is plausible. Indeed, evidence suggests that out-group contact actually provokes backlash (Enos 2014). Decreased out-group contact could then be expected to be favorable for the Democrats, yet we find that E-ZPass decreases their support.

Finally, we do think it is plausible that E-ZPass brings about changes in an individual's emotional environment, in particular by causing a relative decrease in stress levels due to time spent idling in traffic. The ramifications of these emotional effects are hard to assess, and we think it is plausible that these might also explain our results. Banks and Valentino (2012) present experimental evidence showing that anger serves as an "emotional trigger" for negative racial attitudes among "white racial conservatives." The effect one would expect from that paper goes in the wrong direction toward explaining the results of our paper, since decreased anger should mean these emotional triggers are less likely to be set off. Thus, while we are sympathetic to the idea that there are psychological explanations for what we are observing, we do not have any theory or evidence to use to assess this possibility.

None of these alternative explanation seem particularly plausible for explaining the rise in conservative sentiment. By contrast, we do observe a statistically and substantively significant bump in residential property values in response to E-ZPass, and indeed it is the only such change that our census data could find. The explanation for why this upswing happened and why it should effect voting behavior is intuitive, supported by formal modeling, and consistent with prior research on the relationship between political attitudes and property values (Ansell 2014). Thus, we interpet our results as showing that increasing property values cause a higher propensity to support the Republican party.

6 Conclusion

This study has found that the introduction of E-ZPass brought about a significant rise in property values, and was also associated with a significant decrease in various measures of support for Democratic Presidential candidates. These relationships were estimated using conditional diff-in-diff and instrumental variables, techniques not frequently used in the study of political behavior. We have argued that this relationship between property values and support for Democratic candidates is causal, since EZ-Pass has not altered the environment in other ways that are significant enough to explain our results.

Although our study has only looked at the introduction of EZ-Pass in the Eastern United States, the design is easily transported to other geographical contexts, both within the US and abroad. One interesting question for future research is whether the strength of the property-voting relationship depends on a community's wealth level. We have no real evidence to answer this question, but by analogy to income (Gelman et al. 2007), we might expect the effect to be stronger in political communities that are poorer.

Another important question is *why* property values cause this effect. Ansell (2014) argues that a rise in property values gives individuals more "permanent income" that can be converted via sale or borrowing, decreasing demand for social insurance. Others might argue that the effect is mostly due to the increased salience of property taxes. One could imagine a design that sought to sort out these explanations by using survey data rather than vote outcomes. The primary challenge for such a study is finding individuals who live close enough to a highway and therefore can be considered as participating in the study; if it were easy to find enough individuals using existing survey data, we would have done so. Yet with advance planning, such a study clearly could be done, since govermental entities typically announce EZ-Pass well in advance of installation. Such a panel survey might give political science scholarship additional causal leverage on the questions of how individual economic circumstances affect their attitudes.

More broadly, we see our work as helping build an explanation for political polarization in the United States based on patterns of internal migration. Internal migration increases property values in states that gain citizens and decreases property values in states that lose them. The migrants themselves experience little change in individual wealth, at least in the short-term. However, those who already own homes in net recipient states should receive a windfall in their individual wealth. In the United States, migrants are predominantly leaving blue states and going to red states. Thus, internal migration has the effect of gradually making red states purple via a composition change, while pushing the already conservative home owners in red states even further to the right. More works needs to be done to evaluate such a theory, which we can only sketch in brief here.

In sum, this work has used a novel identification strategy to address an important question at the intersection of political behavior and political economy. It suggests that scholars and political observers should consider citizens' entire economic portfolio, and not just income, in analyzing American politics. □

7 Appendix

7.1 Formal Model

We herein sketch a simple model that formalizes some of our intuitions about the relationship between commuting distance, property values, and voting behavior. It builds on Alonso's model of commuting behavior and the Meltzer-Richard model of voting behavior. In our model, individuals may purchase two commodities: a bundle of consumer goods and a housing unit some distance from the city center. They derive utility from these two goods and also from leisure. Individuals earn income by working at the city center and also receive lump-sump transfers from the government. Individuals are differentiated from each other purely based on their productivity, which we model following Persson and Tabellini (2000) by endowing each individual with a unique amount of time. The resources individuals have available to acquire consumer goods are limited, however, by the cost of purchasing a housing unit and also by the cost of commuting to work. Moreover, individuals have only a limited amount of time in the day, which they must fill with work, leisure, and commuting.

We solve the model in two stages. First, taking the amount of taxes and welfare-subsidies as fixed, individuals choose a place to live, an amount to work, and thereby necessarily determine an amount of income they will spend on consumer goods and an amount of time they will spend at leisure. Then, treating their amount of time spent working and the location of their home as constants, individuals choose a preferred tax rate. Since we assume the government's budget must be balanced, it follows that choosing the amount of taxes necessarily determines the amount of welfare as well.

More formally, we assume that individuals have the following quasi-linear utility function

$$U(c, l) = c + h(l)$$

where c indicates the amount of consumer good purchased, l indicates the amount of time spent at leisure, and h is a concave function with analytically "nice" properties. We assume that individuals all receive the same direct utility from having a house regardless of its distance from the city center and homelessness is not an option, therefore house choice only enters the utility function as a constant which may be dropped without loss of generality. It would be wrong to think that how far one's house is from downtown (d) is irrelevant, however, since it indirectly affects individuals utility through the budget and time constraints.

Of the two constraints, the time constraint is the more straightforward. Each individual is endowed with a certain amount of time α, with the average individual getting $\bar{\alpha}$. Formally, α comes from a well-behaved distributed F. As individuals spend all their time working, at leisure, or commuting, the time constraint is given as

$$\alpha = l + n + bd$$

Meanwhile, the budget constraint is given by

$$(1-\tau)n + w = ad + R(d) + c$$

The left hand side of this equation gives the amount of disposable income and the right hand side gives the costs. The tax rate is given by τ and the amount of welfare per individual is w. Commuting costs a fixed amount per distance, a, and the cost of a mortgage/rent is given by $R(d)$. We treat all other consumer goods as the numeraire and therefore the cost of purchasing c units of these goods is c.

In the first part of our solution concept, individuals maximize utility treating its choice variables as c, l, n, d. We formally require that all these quantities be non-negative, although for the analysis presented here we shall assume an interior solution. Substituting the constraints into the utility function gives the following unconstrained maximization problem

$$\max_{n,d} (1-\tau)n + w - ad - R(d) + h(\alpha - n - bd)$$

Which leads to the following first order conditions

$$\begin{aligned} h'(\alpha - n - bd) &= (1-\tau) && (1) \\ h'(\alpha - n - bd) &= \frac{-a - R'(d)}{b} && (2) \end{aligned}$$

In equilibrium, individuals choose levels of labor and home distance (n^*, d^*) that together constitute a local extrema for this function. Concavity of h implies that this extrema is indeed a local maximum. Since equilibrium is defined by the simultaneous solution to these equations, we may combine the two equations and get the following expression.

$$b(1-\tau) + a = -R'(d^*)$$

This equation leads to our first important inference. Since all the terms on the left-hand side are positive, we must have that $R'(d)$ is negative, so that the cost of properties decreases as one goes further from the city. This is as one should expect since being closer to the city gives individuals more time and allows them to spend less commuting. If we assume that R is invertible we get a closed-form expression for the ideal house location. Residually, we determine the value of all the choice variables as follows either through the first order conditions or the constraint equations.

$$\begin{aligned} d^* &= R'^{-1}(-a - b(1-\tau)) && (3) \\ n^* &= \alpha - h'^{-1}(1-\tau) - bd^* && (4) \\ l^* &= h'^{-1}((1-\tau)) && (5) \\ c^* &= (1-\tau)n^* + w - ad^* - R(d^*) && (6) \end{aligned}$$

With closed form solutions for the individual's choice variables, we can begin to understand their voting preferences. Note, however, that we assume individuals cannot in the short run change where they live when solving the tax rate problem. One subtle, but crucial consequence of this is that the utility maximization problem changes and therefore we must solve again the problem of optimal labor.

$$\max_{n} (1-\tau)n + w - ad - R + h(\alpha - n - bd)$$

This gives the first order condition

$$0 = (1-\tau) - h'(\alpha - n - bd)$$

In response to a given tax rate τ, the voter will provide $n^* = \alpha - bd - h'^{-1}(1-\tau)$. Note that to get the indirect utility of a given tax policy we need to understand how much government revenue there is to be redistributed as welfare. This amount is $\tau\bar{n}$, where $\bar{n}$ equals average labor. If we let $N(\tau) = \bar{\alpha}+b\bar{d}-h^{-1}(1-\tau)$ then we can rewrite n^* as

$$n^* = N(\tau) + \alpha - \bar{\alpha} - b(d+\bar{d})$$

If we assumed a finite population then it follows that $\bar{n^*} = N(\tau)$. In the literature we typically assume that there is a continuum of voters with unit mass, and we will follow this assumption as well, however the result is the same as if there was a finite population. Thus, $\bar{n}(\tau) = N(\tau) = \bar{\alpha} + b\bar{d} - h'^{-1}(1-\tau)$. Since transfers are residually determined by the tax rate and the amount of labor, we can state the utility maximization problem that defines voter preferences.

$$\max_{\tau}(1-\tau)n(\tau) + \tau N(\tau) - ad - R(d) + h(\alpha - n(\tau) - bd) \tag{7}$$

Rewriting $n(\tau)$ in terms of $N(\tau)$ on the left yields the following first order condition

$$N_\tau(\tau)[1 - h'(\bar{\alpha} - N(\tau) - b\bar{d})] - (\alpha - \bar{\alpha} - bd - b\bar{d}) = 0 \tag{8}$$

Now we apply the envelope theorem to note that at the maximum of the indirect utility function the choice of n is also optimized. Solving for optimal n in (7) we arrive at

$$\tau = 1 - h'(\alpha - n(\tau) - bd)$$

But then note that at the optimum $\alpha - n(\tau) - bd = \bar{\alpha} - N(\tau) - b\bar{d}$ and so $\tau = 1 - h'(\bar{\alpha} - N(\tau) - b\bar{d})$. Note that the term on the right hand side of the last inline equation is the same as the coefficient of $N_\tau(\tau)$ in (8). Substitution of this expression with τ in (8) results in the following

$$\tau = \frac{\alpha - \bar{\alpha} - bd - b\bar{d}}{N_\tau(\tau)} \tag{9}$$

Note that $N_\tau(\tau)$ is negative. Intuitively, this is the case because the average amount of labor supplied should go down when taxes go up, but we can show it formally as well. Using the definition of $N(\tau)$ and applying the inverse function theorem yields:

$$N_\tau(\tau) = -(1/h'(h^{-1}(1-\tau))$$

But h is concave increasing, so the denominator is positive and we get the expected effect. Thus, returning to (9), we can see that the effect of an increase in the speed of travel b on the ideal tax rate is.

$$\frac{\partial\tau}{\partial b} = \frac{d+\bar{d}}{N_\tau(\tau)}$$

Which is negative by the negativity of $N_\tau(\tau)$. Thus, faster travel times result in individuals preferring a lower tax rate, as those who face the lower travel times are made effectively wealthier.

7.2 Tables

Table 6: *Pre-treatment balance for Ohio. Data from the 2008 Census. Sample size: 136 treated and 136 control units.*

	Overall		*Treated*		*Controls*		*Difference*	
	Mean	(S.D.)	Mean	(S.D.)	Mean	(S.D.)	Dif.	(S.D)
Average income	$51035.38	(12522.88)	$51297.74	(11913.69)	$50769.16	(13151.03)	$528.58	(1516.64)
% bachelors	9.86	(4.8)	9.9	(5.07)	9.81	(4.53)	0.08	(0.58)
% black	8.32	(13.3)	8.35	(10.59)	8.29	(15.62)	0.06	(1.61)
% female	50.98	(3.76)	51.32	(3.32)	50.63	(4.15)	0.7	(0.45)
% of pop. over 65	14.16	(5.79)	14.46	(5.77)	13.85	(5.82)	0.61	(0.7)
% professional degree	0.82	(0.91)	0.89	(0.94)	0.76	(0.87)	0.13	(0.11)

Table 7: *Difference-in-difference results for Ohio, comparing treated to control and each outcome variable to its analogue from 2008. Matching/control variables are same as in Table 6.*

Dependent variable		DiD Estimate	(Bootstrap S.D.)	(Block Bootstrap S.D.)
Average Democratic Vote Share	Baseline model	-1.58	(0.98)	(1.03)