The Impact of a Transportation Intervention on Electoral Politics: Evidence from E-ZPass *

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Abstract

How do transportation policies impact electoral politics? Empirically, the question is difficult to answer because policies that create new transit opportunities may involve a large number of community effects. In this manuscript, we focus on a unique kind of transportation intervention—the replacement of human-operated with electronic tolls on highways—which economic theory predicts should have a concentrated effect on local property values. We find that the shift in local property values following a government program to introduce electronic tolling is correlated with a subsequent change in support for conservative candidates in presidential elections. Geographically-linked exit polling data suggests that concern over taxation is the primary observable mechanisms. To show robustness, we use various measures, one of which is computed at the individual level. We also present placebo analyses to address worries that the correlations are driven by community change, turnout, or rising incomes. Remaining limitations are also considered. *JEL classifications: D72, R41, R21*

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1 Introduction

According to the US Census Bureau, the typical worker spends 7% of their waking time commuting between work and home. Policies which impact the quality of transportation infrastructure should thus have large downstream consequences for society. However, the nature of these effects is difficult to ascertain because a single change in a transportation network can have multiple interacting effects. For example, the creation of a new road may reduce commute times, but may also lead to the creation of new businesses. The road may be funded by some combination of local, state, or federal sources, and money used for the road cannot be used for other public goods. The road might alter the community's social network dynamics as movement and communication patterns evolve. Indeed, transportation policies play an important role shaping social or political developments but precisely because their influence might be so large, they are difficult to study quantitatively.

In this study, we examine a unique transportation intervention which should have a concentrated effect on an important part of Americans' financial life—property values. We then examine downstream effects of the intervention for electoral politics, which in some fundamental sense a conflict over property (especially in questions over taxation and the distribution of wealth (Dougherty 2003; Fischel 2004; Marx 2010; Mayhew 1993)). Using difference-in-difference methods, we find that the transportation intervention is associated with a sizable increase in local property values. This increase in property values is correlated with a subsequent change in support for conservative candidates in presidential elections. Our analysis of a geographically-linked exit poll database allows us to make inferences about the mechanism behind this change. We find that, following the transportation-based shock to property values, voters placed greater emphasis on the importance of taxation. As in all observational studies, we do not have the ability to rule out unobserved confounding variables and conclude by considering what those may be in the context of this study.

2 Relationship to Existing Literature

2.1 Transportation Shocks & Property Values

Our study focuses on the introduction of electronic tolling, its effects on local property values, and subsequent consequences for electoral politics. The first part of this logic stems from an economic argument: when electronic tolling stations replace human-operated booths, local traffic congestion shows a marked reduction. The reduction in commute times for people living near the toll area should imply an increase in their property values as there is higher demand for their homes than there was previously.

The relationship between congestion and property values was first laid out by Thunen (1826). Thunen's "monocentric city model" was developed to explain crop usage patterns and this model was adapted by Alonso (1964) to explain residential housing patterns. The monocentric city model remains widely used in urban economics and economic geography (Baum-Snow 2007; Fujita, Krugman, and Venables 1999). The model predicts that the price people are willing to pay for property is inversely related to the transportation costs they would bear as owners. A change in travel costs should thus result in a change in property values.

Empirical evidence for this relationship is strong. Glen and Nellis (2011), Tang (2016), as well as Zhang and Shing (2006) analyzed the introduction of the London Congestion Charge in 2003, which reduced traffic in central London by about 20% and was associated with a relative increase of 3.68% in nearby property values. Li (2016) found that initiatives to reduce congestion had large effects on home prices in Beijing, and Levkovich, Rouwendal, and Marwijk (2016) showed that homes near gridlocked roads tend to have lower prices than comparable homes elsewhere. In addition, Guttery (2002) found that increased congestion in subdivisions around Dallas were associated with a 5% home price discount. Bateman et al. (2001) analyzed data from Scotland, which revealed a 0.20% decline in property prices for each additional decibel increase in traffic noise. Bagby (1980) arrived at similar conclusions while studying traffic patterns over a 25 year period in Grand Rapids, Michigan. In this

period, property values "exhibit[ed] a surprisingly high elasticity with respect to reductions in traffic flow." The studies described above represent only a sample of the literature (for a review, see Levkovich, Rouwendal, and Marwijk (2016)).

2.2 Linking National Politics and Local Property Values

The literature on property values and traffic congestion is robust, but what about the one relating property values and political preferences? The political economy literature has long emphasized the role of wealth in explaining political preferences: because most governments tax wealth differently for the rich compared to the poor, the interests of the two groups are in conflict. Whereas those without wealth should support higher taxation and redistribution, the wealthy should do the opposite (Meltzer and Richard 1981; E. Peterson 2016), even if other mediating factors such as economic risk also shape support for redistribution (Rehm, Hacker, and Schlesinger 2012). Although these theories are pitched in terms of wealth, the distinction between property and wealth can be fuzzy. Property (defined as a thing belonging to an agent) serves as the basis for wealth (defined as the degree to which one possesses valuable things or ready money itself). Moreover, it is fairly rare for wealth to be primarily stored in the form of ready money. Indeed, although less than half of citizens have any wealth invested in the stock market, between 60% and 70% of occupied housing units are inhabited by the owner (Kuebler and Rugh 2013). In short, in order to understand the relationship between wealth and preferences for president, we must first and foremost examine those preferences in relation to property: many forms of property are physically rooted to a place.

Our work attempts to distinguish between several mechanisms about how precisely local residential property values, if altered by a transportation intervention, are related to preferences for national policies or for president. Some theories have centered on the ability of citizens to self-insure using their property. For instance, Ansell (2014) uses panel data to show that those who live in areas with rising home prices tend to develop more conservative political attitudes over time, both in the United States and comparatively. Ansell interprets this finding as consistent with

a wealth effect on demand for social insurance mediated by the newfound ability of citizens to self-insure against income loss.

While Ansell (2014) emphasizes the role of self-insurance, other scholars have emphasized how the tax implications of property ownership might structure political preferences. The idea that local property values might affect political attitudes is not new, and was elaborated in Kemeny (1977) and Saunders (1978), who argue that home owners tend to be more conservative than renters, since home owners are more likely to pay capital gains taxes (see also Dietz and Haurin (2003)).

Indeed, a home is a form of capital, and taxes that apply to capital transactions also apply to one's home, with the two most prominent taxes of this kind being the estate and capital gains taxes. The capital gains tax is highly salient for homeowners. IRS Manual 701, *Selling Your Home*, states as its first key point that "[i]f you sell your home at a significant profit (gain), some or all of that gain could be taxable." Tax considerations can have far-reaching consequences for the decisions of homeowners. For example, the capital gains tax is a significant transaction cost that may make citizens less likely to move. In addition, capital gains taxes can themselves influence the price that sellers can get for their homes: a capital gains tax of 22% can reduce the equilibrium price for homes by 8%, as this tax reduces the expected discounted sum of incoming deriving from the home (thus the price of the home itself, see Svensson (2013)).[1] Prior to 2001, the estate tax was also applied to anyone with assets over $625,000, and the tax was a salient political issue. Although the exception is now about $5 million and the top federal estate tax rate is about 40%, the tax is still relevant for wealthy homeowners as they strategize about their end-of-life plans.

The tax system interfaces with local property dynamics in yet other ways. Local property taxes are the only ones directly applied to home values on a yearly basis, but those local taxes are relevant for citizens' federal tax returns. In particular, homeowners can deduct property tax payments and mortgage interest from their federal income tax. The revenue foregone via these deduction is worth roughly three

[1] Anecdotally, it is worth noting that the maximum tax rate on long term gains had been steadily decreasing in the run-up to the American housing bubble.

times the amount the federal government spends on low-income housing (Poterba and Sinai 2008). There are also a number of studies which document how the mortgage interest and property tax deductions aggravate inequality (Seetharaman 1994; Ventry 2010). These deductions become even more important for those in areas experiencing home price appreciation, since this appreciation will cause an increase in their property tax burden and the amount they can deduct from their federal tax bill. Homeowners must thus consider the implications of federal tax policy on their home not only when selling it or passing it on to their descendants, but also on a yearly basis as they calculate their deductions.

The two main political parties in the United States have sent strong signals to home owners on their positions regarding these tax policies. For example, the preamble to the 2000 Republican Party platform stated: "We cheer [the 1997 Republican Congress's] lowering of the capital gains tax rate and look forward to further reductions that will stimulate property sales." The 2004 Republican platform was similar, emphasizing the idea that "good government is based on a system of limited taxes." It claimed credit for having offered a plan "to lower all tax rates", "phase out the death tax", and lower the capital gains tax to 15%. For their part, Democrats in 2004 released a plan to "roll back" the Bush tax cuts and establish "a tax code that rewards work and creates wealth for more people, not a tax code that hoards wealth for those who already have it." In short, the federal government's role in redistribution through taxing residential property transactions have been salient in national political debates.

Besides through the tax code, federal policy is also indirectly linked with property values because federal and local governments can serve as strategic substitues for the provision of social insurance and local public goods such as police, fire fighters, or schools (Parchet 2014; Volden 2005). Theories of federalism typically suggest that the most efficient allocation of public goods should involve the federal government undertaking policies aimed at redistribution and local governments focusing on development (P. Peterson 2012; Wong 2015). Hence, property values in a com-

munity can be expected to affect the demand for federal assistance in areas of core local government competence. Moreover, an increase in local property values would lead to an increase in the total amount raised by local governments for public goods, which could then reduce demand for the deployment of such goods by the federal government.

To summarize, there are three main mechanisms discussed in the literature explaining why transportation policy may influence preferences for president through its impact on local property values. First, rising property values may allow citizens to use their home to self-insure against income loss, reducing their demand for federal redistribution. Next, not only are property transactions taxable in the federal tax code, but also mortgage interest and property tax payments can be deducted from one's federal income tax. Finally, an increase in local property values increases the resources available to local governments, which could reduce demand for the creation of public goods by the federal government. Our project provides some of the first evidence to assess the relative importance of these mechanisms empirically without relying heavily on self-reported data on political attitudes, which as E. Peterson (2016) points out, might produce biased estimates of wealth's relationship with voting in surveys (as we use an empirical strategy that avoids some of these concerns by using individual-level campaign contribution data).

3 Design

How we use some of the insights from the transportation literature to study property and preferences for president? This section, which outlines our approach towards answering this question, also explains how this method can help us make inferences about the deeper reasons why preferences for president may be influenced by local property values. The design uses the replacement of human-operated with electronic tolls as a key source of variation in local property values. By using multiple dependent variables, replicating the analysis on two geographically and temporally independent interventions, and performing placebo checks, we work to address con-

cerns that this correlation is spurious, and also discuss continuing limitations to our approach. One of our measures of political behavior is computed at the contributor level, allowing us to examine within-person variation and to avoid concerns specific to ecological inference (King 1997). We obtain leverage on why these observed changes may have occurred by analyzing precinct-level exit poll data.

3.1 Electronic Tolling

Between 2000 and 2002, E-ZPass was introduced along toll roads in New Jersey and Pennsylvania.[2] Drivers who purchased E-ZPass transponders were able to avoid manual toll collection, while drivers who did not purchase a transponder benefited from decreased traffic in the manual payment lanes. According to estimates published by the New Jersey Department of Transportation, 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-ZPass installed (New Jersey Turnpike Authority 2001). The introduction of E-ZPass creates intra-state variation in travel time changes, but without creating the side effects associated with government investment projects in infrastructure. Moreover, the E-ZPass plazas were not selected strategically at the time of introduction, but replaced already existing toll structures. In other words, officials did not introduce E-ZPass to areas which were experiencing especially fast growth in transportation demand. Instead, all existing toll plazas received the intervention at once. If the E-ZPass intervention were anticipated by some homeowners, the subsequent effects on housing values would be attenuated towards zero, so this concern would make our reported results statistically conservative.

3.2 Data Sources

In the empirical section of this paper, we rely on a combination of low-level voting, political contribution, census, and exit poll data. The geographic analysis was con-

[2]The idea of using the introduction of E-ZPass for econometric inference comes from Currie and Walker (2011), who look at the effect of decreased traffic and car pollution on infant health.

ducted using ArcGIS with a national highway map provided by ESRI. The location data for E-ZPass sites were taken from the replication dataset to Currie and Walker (2011), and was supplemented with data collected from the Department of Transportation in Ohio, New Jersey, and Pennsylvania. Crucial to our analysis is the distance of each precinct to the intervention sites. For this purpose, we consider the polling place as coded in Ansolabehere, Palmer, and Lee (2014) to be the precinct's location, and we define distance to an E-ZPass plaza to be the minimum network or "over-road distance" to that entity.

Although we use the two-party Democratic vote share in presidential elections as one of our outcome measure, we also focus on changes in individual support from same-address campaign contributors in our study period. Same-address contributors are those individuals with the same name and address in the contributor file in both election years. Ideally, we would also use individual-level party registration data measure support for the Democratic Party. However, the 2000 voter files in Pennsylvania and New Jersey are not available. Precinct-level data on vote share, as well as shape files detailing the geographic boundaries of each precinct, are taken from Ansolabehere, Palmer, and Lee (ibid.). Contribution data are taken from Bonica (2013). Data used for matching and for obtaining home price trends are from the 2000 decenial census and the 2005-2009 American Community Survey (ACS). It is important to note that the home value measurements were estimated using the owner's estimate of the sale price for the property on the current market. Thus, we cannot know for certain whether these numbers accurately reflect the amount that owners would in fact obtain. Moreover, the census only reports the percentage of owners in each area with homes in certain price bins (e.g. below $20,000, between $20,000 and $50,000, and so forth). To compute the average home value for each area, we used a weighted sum (where we multiplied the bin proportion by the price midpoint within the bin and summed across bins). We used the imputation procedure outlined in Armour, Burkhauser, and Larrimore (2013) for estimating the average price for the top bin (for robustness, we truncated the top-bin average at

$10 million).

Table 1: *Data streams. See the Appendix for a short note on how we harmonized these sources.*

Data stream	Original geographic unit	Content
Census data	Census block	Aggregate home price, income, demographic data
Voting returns	Precinct	Aggregate voting returns
Contribution data	Home address	Individual-level contribution data
VNS and NEP exit poll data	Precinct	Individual-level tax/jobs salience data

3.3 Design Logic

One of our empirical specifications is based on a conditional difference-in-differences (diff-in-diff) model, which has been adopted in a growing number of publications to estimate the effect of different policies on home values (see, for examples, Diao, Leonard, and Sing (2017), Ellen et al. (2007), and Levkovic, Rouwendal, and Marwijk (2016)). The parallel trends assumption is important for estimates from this model to be unbiased. This assumption requires that the areas close to E-ZPass exits would have experienced the same changes in home prices (due to, for example, gentrification or neighborhood improvement) as the areas near non-E-ZPass exits. The model only allows us to estimate average effects (whereas, as we later see, areas closer/farther to E-ZPass sites exhibited larger/smaller effects, respectively). Despite this fact, the diff-in-diff is attractive because it is easily interpretable and because we are not required to estimate a possibly complex non-linear relationship between our outcomes and distance to the E-ZPass sites.

In the diff-in-diff model, we define "treated" precincts to be those close to an E-ZPass site, and which are therefore likely to have received an increase in average home price. Whether close should mean 5, 10, or 15 miles is unclear *a priori*. A sensitivity analysis is required to assess the dependency of the results on how one defines closeness. We also must define a reasonable control group that could have received the intervention but did not. To construct this control group, we examine precincts close to exits on major highways without E-ZPass tolls. However, some precincts are both within, say, 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 precincts that are too close to being in both groups. The exclusion rule should have a radius at least as big as the inclusion rule to guarantee perfect separation. In addition, citizens may be willing to drive a greater distance to take a non-toll road than one with tolls, so the radius of the exclusion rule should be 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 main analysis including a precinct in the treatment (control) group if it is within 12 miles of an E-ZPass (non-E-ZPass) exit, but not within 18 miles of a non-E-ZPass (E-ZPass) exit. We then replicate the analysis on a grid of values for the exclusion and inclusion as a sensitivity test.

Although it is tempting to argue that the intervention serves as an exogenous shock to the local housing market, it is possible that treated and control precincts differ systematically in terms of relevant background covariates. To address this possibility, which is present in observational and experimental studies alike, we match on key covariates which might confound our estimates (Ho et al. 2007; Morgan and Rubin 2012). In the matching algorithm, we use propensity score matching with a caliper of 0.20. Matching was done without replacement. Our precinct-level matching variables 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.[3]

We must carefully consider how to model uncertainty in this research design. Because our data is spatial, autocorrelation presents a serious threat. With auto-

[3]Should we should also enforce balance on the 2000 level of home values in treated and control sites? For reasons we explain in the Appendix, we argue that this approach would bias our estimates.

correlation, there is a higher probability of underestimating standard errors. In order to address this issue, we take three precautions. First, we report block bootstrapped standard errors (Abadie and Imbens 2008; Austin and Small 2014; Efron and Tibshirani 1986; Hall et al. 1995). Next, we use county-level fixed effects. Finally, for our individual-level analysis, we report county-clustered standard errors.

4 Results

First, Table 2 presents summary statistics useful for analyzing covariate balance between matched units used in the diff-in-diff, while the Appendix presents a map showing units by treatment status. After matching, we find that treated and control communities are similar on nearly all background covariates.[4]

Table 2: *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	$61,030.94	(34,783.57)	$61,497.61	(30,444.52)	$60,564.26	(38,642.63)	$933.36	(1,235.67)
% 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)

Figure 1 gives initial evidence for our argument that electronic tolling (a) increases local property values and (b) decreases support for the Democratic candidate for president. On the left panel, we see that precincts closest to the intervention experienced the sharpest rise in home price. On the right panel, we see that precincts closest to the intervention show the largest fall in Democratic support. The light gray line interspersed with "C"'s shows the same trends for precincts in the control areas near non-E-ZPass exits. These control areas show a much less striking pattern

[4]There were no significant differences for 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. The only significant difference is that treated units had an average African-American population of about 6% while control units had an average African-American population of about 4%. Using ecological arguments, we find that this difference could only account for a small fraction of the observed effect. For details, see the Appendix.

of increase/decrease close to the intervention sites.

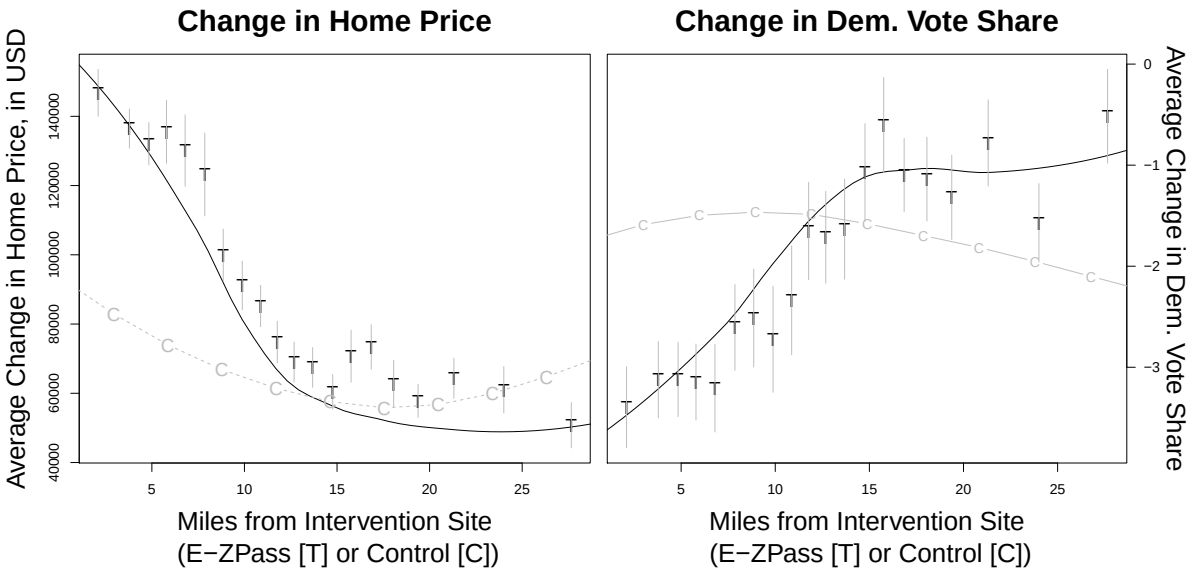

Figure 1: *Distance to E-ZPass ("T") and similar non-E-ZPass exits ("C") against inflation-adjusted change in average home price and against change in Democratic vote share.*

How does this visual evidence hold up? Average home prices near the intervention sites increased markedly. According to the baseline model, treated precincts saw an inflation-adjusted increase in average home price of $50,000 relative to the control baseline, above statistical significance as calculated using both clustered and block bootstrap standard errors. This value represents about 35% of the baseline average home value of $130,000—the same order of magnitude as the 20% price increase found in Zhang and Shing (2006)'s study of the London Congestion Charge. The estimated effect of E-ZPass is similar with or without covariate adjustment.

We next consider the effect of the intervention on change in 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 evolution in Democratic presidential vote share between 2000 and 2008, we find that this estimated effect increases in magnitude.

Using individual contribution data, we identify same-address individuals living

Table 3: *Main difference-in-difference results. Matching/control variables are same as in Table 2.*

Dependent Variable		DiD Estimate	(Block Bootstrap S.D.)
Δ *Average Home Price*	Baseline model	$50,185	(22,627)
	With covariate adjustment	$47,399	(17,062)
Δ *Dem. Vote Share, 2000-2004*	Baseline model	-2.37	(0.99)
	With covariate adjustment	-2.46	(0.96)
Δ *Dem. Vote Share, 2000-2008*	Baseline model	-3.11	(1.42)
	With covariate adjustment	-3.32	(1.55)
Δ *Dem. Vote Share, 2004-2008 (Placebo Analysis)*	Baseline model	-0.73	(0.94)
	With covariate adjustment	-0.97	(1.00)

in the treated or control precincts who contributed both in 2000 and 2004. This analysis is important because it allows us to address worries that our conclusions are driven by ecological inference issues. In the individual-level regression model, we predict changes in support for the Democratic candidate using distance to E-ZPass as a key covariate (see Table 4). Contributors closer to E-ZPass experienced a relative increase in home values in their communities and were more likely to support the conservative candidate for president. This analysis uses within-person variation, reducing (but not eliminating) concerns that our findings reflect demographic change, and not genuine shifts in political behavior. Tables 3 and 4 summarize these results.

Table 4: *Effect on individual-level change in support for the Democratic presidential candidate. The outcome variable is calculated by measuring the share of each contributor's donations for president going to the Democratic candidate, and taking the difference in this variable between 2000 and 2004. Individual-level controls include gender, latitude, and longitude. Precinct-level control variables include average home price in 2000, Democratic vote share in 2000, change in Democratic vote share between 2000 and 2004, average income gain score, state, % of families below the federal poverty line, and black Americans as a percentage of the precinct population.*

Predictor	Effect on Individual Δ in Dem. Support	(Cluster-Robust S.D.)	(Block Bootstrap S.D.)
2000-2004	-10.33	(4.03)	(4.87)
2004-2008 (Ohio Replication Analysis)	-1.18	(0.35)	(0.61)
2000-2004 (Ohio Placebo Analysis)	-1.01	(0.93)	(1.30)
2004-2008 (Placebo Analysis)	1.2	(0.80)	(1.05)

4.1 Validity Checks

The methods presented so far find consistent results. We next conduct additional robustness checks. In addition to these checks, the Appendix also shows that our results are insensitive to the choice of inclusion/exclusion hyperparameters and that observable weather patterns in treated and control sites do not muddle the findings.

A major threat to inference comes from time-varying unobserved confounders. We investigate this concern in various ways. First, we re-ran the analysis for the change in Democratic vote share between 2004 and 2008 in New Jersey and Pennsylvania (where no effect should be found). We correctly arrive at null estimates in both the aggregate and individual-level analyses (see Table 3 and 4).

We can gain additional insight into the problem of unobserved confounders by considering the case of Ohio. Ohio did not replace its toll structures with E-ZPass plazas until 2008. We can thus use Ohio as a placebo case. We know that there is no effect of the E-ZPass intervention between 2000 and 2004. If we were to measure 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, after using the same matching algorithm, exclusion/inclusion rule, and modeling approach as before, we correctly arrive at null results in both the aggregate and individual-level models (see Table 5 and 4). Ohio precincts near exits that would later adopt E-ZPass saw no change in average home value or in Democratic vote share between 2000 and 2004. This placebo analysis supports the conclusion that it is the intervention alone which is accounting for the increase in Republican vote share. In addition, the effect of the intervention is significant after the tolls are actually replaced by E-ZPass in 2008 (see the Appendix for more details).

4.2 Disentangling Mechanisms

It is tempting to interpret the results of the previous sections as demonstrating that the positive shock in property values, induced by the introduction of electronic tolling, may have led to a shift in support away from Democratic candidates for

president and towards Republicans. Yet, without understanding the origins of this change, the results of the preceding analysis are of limited significance. We must therefore examine the evidence as to why the observed changes in political behavior may have occurred.

First, we consider community turnover as an explanation for the change in partisan behavior. Our analysis of contribution data shows that there was a shift in donation patterns among those who resided at the same address during the 2000 and 2004 elections. This individual-level change could conceivably be driven by community-level factors. For example, the racial threat hypothesis could be relevant here (Behrens, Uggen, and Manza 2003; Giles et al. 1994). Same-address residents may have adopted more conservative political attitudes following the intervention if it led to an influx of minority residents or led to other compositional changes.

We can test for community-level changes as a way of evaluating this kind of reasoning. To do so, we use the same procedure as before, but changing the outcome variable to include other factors predictive of Democratic vote share. After performing this analysis, we find that no factors related to racial threat underwent substantial movement following the intervention. As shown in Table 6, there are null effects for changes in income, population, turnout, the percentage of each precinct with a bachelor's degree, and blacks as a percentage of the precinct population.

We next turn to evidence about the mechanisms related to taxation, self-insurance,

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 such as 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.*

Dependent variable		DiD Estimate	(Block Bootstrap S.D.)
Δ *Average Home Price (Ohio Placebo)*	Baseline model	$-997	(5,965)
	With covariate adjustment	-$3,573	(5,296)
Δ *Democratic Vote Share (Ohio Placebo)*	Baseline model	1.02	(0.79)
	With covariate adjustment	1.11	(1.03)

Table 6: *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.)
Δ *Average income*	-102.10	(1,910.66)	-1,097.22	(3,503.09)
Δ *Population*	30.88	(30.17)	15.27	(36.01)
Δ *Turnout*	0.72	(0.85)	0.61	(1.18)
Δ *Percent with bachelors*	0.64	(0.47)	0.56	(0.61)
Δ *Percent black*	0.47	(0.37)	0.54	(0.56)

and public goods provision. Here, the best evidence we have comes from exit poll data measured at the precinct level following the elections of 2000 and 2004. We linked this poll data to our geographic database, and were able to analyze the responses of voters in the vicinity of the intervention. The data suggest that the transportation/housing shock is not associated with a broad shift in conservative political attitudes. The shock is also not associated with a reduction in concern about the economy or citizens' job prospects. Instead, it is only associated with an increase in the probability of a respondent expressing concern about taxation. This regression analysis provides support for the idea that the expansion of Republican support occurred due to the growing salience of taxation in neighborhoods experiencing the property shock.

Overall, our results support the idea that, in this context, the transportation-induced changes to local property values are better explained by growing concern over taxes than voter's newfound ability to self-insure using their home equity.

Table 7: *Exit poll results. There are 3,013 respondents used in this analysis. As per Shao and Sitter (1996), we calculate block bootstrap standard errors by resampling data by the geographic region, performing multiple imputation, and then fitting a logistic regression model to predict whether respondents consider (a) taxes or (b) jobs and the economy to be the most important issue, or whether respondents identify with a liberal political philosophy. In line with the literature, the control variables include Income, Income^2, Age, Age^2, Race, Education, and Gender.*

Dependent Variable	Estimated effect of intervention	(Block Bootstrap S.D.)
Tax Salience	0.51	(0.18)
Economy/Jobs Salience	0.28	(0.21)
Identification with Liberal Philosophy	−0.04	(0.27)

4.3 Other explanations

Although we have evidence that the changing salience of taxes explains the rise in support for the Republican Party, we cannot be conclusive about this finding due to the possibility of unobserved confounders that cannot be evaluated by our placebo analyses. Because we cannot be certain about deeper mechanisms, we also must address the possibility that unobserved dynamics might be explaining the relationship we see between the intervention and the change in propensity to support the Republican Party. We consider two possible explanations that seem well-motivated by the social science literature: decreased out-group contact and changes in an individual's emotional environment.

One possible explanation is that the intervention decreased contact with out-groups, since it incentivize people to drive over taking public transportation. Indeed, we found some decrease in reported use of public transportation. Although shy of significance, there may be something to this explanation, but we have some hesitations. For this explanation to bear out, out-group contact would have to generate support for the Democratic Party, but some studies suggests that out-group contact actually provokes backlash (Enos 2014). Decreased out-group contact could then expected to be favorable for the Democratic candidate, yet we find that the intervention decreases such support. Nevertheless, changes in demand for public transportation could conceivably contribute to the observed effects on political behavior.

It is plausible that the intervention brought about changes in citizens' emotional environment by causing a decrease in traffic-induced stress. The ramifications of these emotional effects are hard to assess, and it is possible that they may explain a portion of the observed changes. However, Banks and Valentino (2012) present experimental evidence showing that anger serves as an emotional trigger for negative racial attitudes among white conservatives. Thus, by reducing the traffic burden, the intervention would be predicted to increase support for the Democratic Party, but we find the opposite effect. While we are open to the idea that there are psychological

explanations we are observing, we do not have strong evidence for them.

There are a few final factors which could complicate interpretation of these results. While we do have individual-level evidence from stationary campaign contributors, we have no such evidence for home values, for voting behavior, or on political opinion. Second, we do not have direct evidence that homeowners were aware of trends in their local property values. The home price data are derived from owners' assessments of their current market price, so may reflect their subjective sense in addition to fluctuations in the true market price. Many voters are not homeowners, so the E-ZPass intervention could not affect them through its impact on property values. Moreover, New Jersey, Pennsylvania, and Ohio all experienced Republican control of the governorship and the legislature when E-ZPass was first implemented. Voters may have causally connected their rising property values to E-ZPass and in turn to Republican leadership in their state. This could then have translated into support for the Republican presidential candidate. However, this narrative is somewhat more complicated in New Jersey: there, the Republican-led rollout of E-ZPass was hampered by corruption charges, high costs, and other implementation problems which led to well-received reforms starting in 2002 by the newly elected Democratic governor, Jim McGreevey.

In the end, these alternative explanations are interesting and we cannot rule them out. Nonetheless, the explanation that seems best motivated by the existing literature and best supported by our data is that the E-ZPass transportation shock increased local property values, which led to a change in voting behavior through the effect of property values on tax considerations.

5 Conclusion

This study attempts to unravel the complex relationship between transportation policies and other social dynamics, in particular those surrounding electoral politics. Although we must treat the results with skepticism as we did not have the ability to randomize the E-ZPass intervention, we find that the replacement of human-

operated tolls with electronic stations was associated with a large increase in property values in the areas tightly clustered around the tolling center.

We then presented evidence that these communities experienced a significant rise in support for the conservative candidate in US presidential elections. This relationship held for aggregate and individual-level data. As a robustness check, we replicated the study design in a geographically and temporally distinct setting, and arrived at similar estimates. Placebo checks correctly arrived at null results. Finally, exit poll data collected by precinct demonstrated that the government intervention did not cause a broad shift in political attitudes, but rather an increase in the perceived importance of taxation. The salience of issues besides taxation did not appear to have been significantly affected. We interpret this as providing support for the idea that, in the United States, concerns about federal tax policy is perhaps the most important reason why preference in national elections are responsive to transportation-induced changes in local property values.

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