How Does Wealth Impact Political Behavior? Evidence from a Government-sponsored Intervention*

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November 15, 2016

Abstract

In this study, we use the introduction of a government-sponsored intervention in Pennsylvania and New Jersey in order to isolate the effect of wealth on voting, which is difficult to identify due to the correlation between affluence and other factors that predict partisanship. We show that the government program caused a downward shock in private transportation costs, and thereby a relative increase in home prices in affected areas. We then use three measures of voting behavior to demonstrate the robustness of subsequent effects on political behavior, one of which is computed at the individual level. We rule out explanations related to community change, turnout, or rising incomes. We also replicate the design in Ohio. Exit poll data suggest that concern over taxes is a primary mechanism explaining the rise of conservative voting in response to the upward wealth shock. *Word count: 9,926*

*The authors thank Jeffry Frieden and Kenneth Shepsle for insightful comments. The authors names' are listed alphabetically. Author contributions: C.J. and B.L. designed the research, performed the research, analyzed data, and wrote the paper. The authors declare no conflict of interest.

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1 Introduction

Since the early days of survey research, scholars have found that individuals with greater education, income, and wealth have tended to support conservative political parties, both in the United States and other advanced democracies. Yet questions about this relationship remain. Affluence may be related to a range of other factors, such as education, that also influence voting. Moreover, the mechanisms behind this relationship have been hard to pin down: are the wealthy more conservative because they are less worried about job loss, or because they oppose the higher taxes that might come about from more redistribution (Ansell 2014; Meltzer and Richard 1981; Rehm 2011; Rehm, Hacker, and Schlesinger 2012)? The importance of answering this question has increased in the past two decades, as new research suggests that disparities in wealth have grown even more quickly than disparities in income and education (Piketty and Saez 2014; Reardon 2011). Within political science, recent work explore these trends and their effects on democracy (Bartels 2008; Gilens and Page 2014; McCarty, Poole, and Rosenthal 2006), yet creative empirical strategies are needed to help understand the mechanisms driving the relationship between wealth and political behavior.

In an ideal setting, we would measure the effect of wealth on voting behavior by manipulating individual wealth directly. This approach would address concerns that changes in wealth may be masking other social or economic fluctuations. Outside the context of lotteries, it is difficult to imagine any entity willing to give away significant wealth on a random basis. Lottery studies do provide valuable evidence on this question, but there are concerns with this approach, especially around the extent to which lottery participants are representative of the population at large, and whether the wealth effect can be distinguished from other factors related to winning a lottery.[1] In addition, it is unclear how this design could be adapted to study other relationships in political economy.

Our study proposes another approach for identifying wealth effects. We focus on a program that predictably affected individual wealth without involving direct cash trans-

[1] It is worth noting that existing lottery studies find effects that are qualitatively similar to the estimates we provide.

fers. The benefits of this design are twofold. First, this design provides estimates with good external validity properties, since our measures come from administrative data on all citizens in our treatment and control sites. Second, this template can be adapted by other researchers who wish to study the effects of wealth on other forms of political behavior, since the design can be replicated whenever it is possible to locate an external shock to transportation costs, or to asset prices more generally.

The program we study is the introduction of E-ZPass in three U.S. states. The E-ZPass intervention reduced traffic congestion on affected highways by 85% in the year after its adoption. The economic geography literature predicts that travel-time reducing shocks should increase home prices—and therefore private wealth—in benefited communities. In what follows, we find this expected effect on property values following the E-ZPass intervention, while finding no demographic shifts in the treated sites. We also find significant changes in three measures of support for the Republican Party. By using multiple dependent variables, replicating the analysis on two geographically and temporally independent interventions, and performing extensive placebo and robustness checks, we hope to address legitimate concerns that this correlation is spurious. It is important to emphasize that one of our measures of political behavior is computed at the contributor level, allowing us to examine within-person variation. Matching, county-level fixed effects, and clustered standard errors are used to address spatial autocorrelation.

Finally, in the discussion, we consider evidence about *why* increasing wealth caused more conservative voting behaviors at treated sites. To gain leverage on this question, we conclude by analyzing precinct-level exit poll data. This analysis suggests that the positive wealth shock did not cause a broad shift in conservative political attitudes or in the fear of job loss, but instead a localized increase in concerns about higher taxes. This result provides evidence that, at least in the regions examined in this study, the salience of taxation served as a primary driver of wealth's effect on voting.

In what follows, Section 2 provides background, Section 3 contains the research design, and Section 4 presents the results. Section 5 discusses mechanisms, while Section 6 concludes.

2 Background

Theories about the origins of voting preferences are legion, and there is insufficient space to do them justice here. For useful reviews, see Bartels (2010) or Flanigan et al. (2015). This section has three aims. First, we review the literature in economics how transportation shocks should affect property values, as our argument critically depends on dynamics predicted by this literature. Second, we selectively review recent work on political behavior and wealth. Finally, we provide details about the US presidential elections of 2000 and 2004, focusing on how the candidates differed on policies relevant to the bottom-line of people who benefited from the wealth shock at issue in this study.

2.1 Transportation Shocks & Property Values

Numerous studies have examined the relationship between traffic congestion and property values. 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."

This relationship was first predicted 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). These models predict 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. Considerable evidence supports this conclusion, and the studies described in the above represent only a sample of the literature (for a review, see Levkovich, Rouwendal, and Marwijk (2016)). In the long run, a shift in the demand curve for property may cause compositional changes in affected sites, but in the short run, turnover is likely to be small due to the stickiness of home ownership decisions. In the Appendix, we formalize these notions by developing a two-period model. In the first period, individuals choose homes given a personal endowment. In the second, they either do or do not receive a reduction in travel costs. The formal model produces comparative statics showing that those who receive a windfall reduction in travel costs are more reluctant to support redistributive programs and higher taxation.

2.2 Wealth & Voting

Prior work in political behavior has established that numerous factors affect voting decisions. These factors include group identity (Converse 1966; Lewis-Beck, Norpoth, and Jacoby 2008), the performance of economy during in inter-election years (Fiorina 1981; Healy and Malhotra 2013), as well as the fit between the bundle of policies that candidates offer and voters' ideological or economic preferences. Although it is hard to establish the direction of causality between affluence and conservative voting behavior, evidence for such an association goes back to the earliest days of survey research and continues into the present. Gelman et al. (2007), for example, used multilevel-modeling of exit poll data to establish that income is an important predictor of individual vote behavior, while Hersh and Nall (2015) use disaggregated registration, census, and election returns to show that income has an impact on voting behavior, especially in Congressional districts with a large minority population.

There are two approaches for understanding why wealth should influence political behavior. Those in one camp would tend to emphasize the role of identity in explaining the relationship between wealth and voting. Identity-based theories would emphasize how rising wealth should have only a muted impact on voting, which is based largely on cultural factors (Luttmer and Singhal 2011) or symbolic identification with a party (Sears et al. 1980). By contrast, the political economy literature would tend to emphasize how wealth shocks create new incentives for the affected citizens, who then respond by adjusting their vote choices. There are several ways of understanding how this process might work. Theories in the tradition of Meltzer and Richard (1981) would suggest that tax policy should drive voting behavior. Others might underline the role of economic risk in shaping political attitudes: these theories suggest that increasing wealth can decrease demand for social insurance, as more affluent individuals face a lower risk of unemployment, or are more able to self-insure (Ansell 2014; Rehm 2011; Rehm, Hacker, and Schlesinger 2012). Finally, retrospective voting theory would suggest that those who experienced a growth in wealth should reward the party that caused the gain (Fiorina 1981).

Although theories of wealth and voting have a long history, Ansell (2014) noted how empirical research has focused more attention on the role of income in explaining political behavior. Nevertheless, some progress has been made towards evaluating the wealth-voting relationship empirically. Using panel data, Ansell shows 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. He interprets this finding as consistent with a wealth effect mediated by the newfound ability of citizens to self-insure against income loss. Quasi-experimental research on wealth effects in the US has largely been limited to the lottery studies discussed in the above. Doherty, Gerber, and Green (2006) presents survey evidence comparing attitudes of lottery winners to the general public. They find that lottery winners are more hostile to taxes and redistribution than the general public, without much change in broader political attitudes. These studies provide valuable evidence about wealth and voting, but there are at least two open questions about this literature. First, Peterson (2016) rightly points out that non-response, social desirability pressures, or inaccurate self-reports might produce biased estimates of wealth's relationship with voting. In addition, even studies that use voter registration data may be open to questions around external validity. For example, it is unclear whether lottery recipients are representative of the population at large in how they react to increased wealth. Rubenstein, Scafidi, and Rubinstein (2002) find that lower income and non-white households are more likely to participate in lotteries compared to more affluent and white individuals. Thus, it is possible that lottery studies might overstate the effect of wealth on voting, as this effect is likely strongest for the poor (Gelman et al. 2007). For these reasons, it is important to study the wealth-voting relationship with new methods that possess favorable external validity properties.

2.3 Historical Context

The elections of 2000 and 2004 provide an ideal setting for putting these theories to the test. In both elections, the Republican Party made prominent its position on capital gains and estate taxes, two policies that have significant implications for homeowners and the amount of wealth they can pass to their children.[2] As 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 wealth and residential property were very much on the agenda in the election years of interest in this study.

[2] IRS Manual 701, *Selling Your Home*, states as its first key point that "If you sell your home at a significant profit (gain), some or all of that gain could be taxable." Moreover, prior to 2001, the estate tax was applied to anyone who had assets worth over $625,000, while the tax reform bill passed by President Bush in 2001 raised that threshold to $3,500,000 over the course of the decade, and completely exempted anyone who died after 2010. See https://www.irs.gov/pub/irs-soi/00esart.pdf

3 Research Design

3.1 E-ZPass Description

In 2002, E-ZPass was introduced along toll roads in New Jersey and Pennsylvania.[3] Drivers who purchased E-ZPass transponders were able to avoid manual toll collection, while drivers who did not purchase a transponder benefited from shorter lines at manual tolls. 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-ZPasses installed (New Jersey Turnpike Authority 2001). Over time, it is also possible that more drivers used the less congested toll roads, thereby reducing congestion on local streets as well. As we show in a formal model in the Appendix, shorter travel times should result in private wealth gains for those in nearby communities. 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.

3.2 Data Sources

We rely on a combination of low-level voting, political contribution, census, and exit poll data. The geographic analysis was conducted using ArcGIS with a national highway map provided by ESRI, the developer of ArchGIS. 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.

[3] 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.

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 the highway (or an E-ZPass plaza) to be the minimum network or "over-road distance" to the nearest highway entrance.

We use three measures of support for the Democratic Party. 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 total votes cast for either the Democratic or Republican candidate. Next, we also measure the two-party Democratic cash share. This quantity is defined as the share of campaign contributions going to the Democratic candidate for president. 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 were not available. Instead, we examine individual support from same-address campaign contributors in 2000 and 2004. Same-address contributors are those individuals with the same name and address in the contributor file in both election years. Precinct-level data on vote share, as well as shape files detailing the geographic boundaries of each precinct, were taken from Ansolabehere, Palmer, and Lee (ibid.). Contribution data were taken from Bonica (2013). Data used for matching and for obtaining home price trends were taken from the 2000 decenial census and the 2005-2009 American Community Survey (ACS).

Table 1: *Data streams.*

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

Because these data are reported at different levels of aggregation, 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 of about 5 square miles, and are roughly the same size as census block groups (the smallest unit at which census data are reported). Zipcodes are larger, containing multiple precincts or census block groups. Since the boundaries of precincts, block groups, and zipcodes are not identical, we use areal interpolation to aggregate the data from all geographic units to the precinct level. The intuition for this kind of interpolation 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. Replication code is available for those interested seeing each step required to merge these data streams.

3.3 Design Logic

Studies that use geographic features as identification mechanisms may be open to criticisms about how the treatment and control groups are specified. To respond to such worries, this analysis uses two definitions of treatment and control groups in order to illustrate that our results are robust to distinct conceptual and empirical specifications.

The analysis first uses an instrumental variables (IV) approach. This method does not sharply distinguish between treated and control groups. Rather, it uses proximity to the nearest E-ZPass toll plaza as a way of measuring the extent to which a precinct should be encouraged to see home price appreciation. This framework depends on two assumptions. First, proximity to E-ZPass must be correlated with the endogenous explanatory variable (i.e. gain in home price). This correlation is strong, with a Student's 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, which is hard to test directly, would be violated if distance to E-ZPass toll plazas affected 2-party vote share even when home prices were kept constant. For the IV analysis, we focus in on precincts within 20 miles of E-ZPass plazas.

The second conceptual specification is based on a conditional difference-in-differences (diff-in-diff) model. This specification defines 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 reasonable to think of the intervention as an exogenous shock, it is 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 tried using Mahalanobis distance matching and coarsened exact matching, but these approaches gave poorer balance. 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.[4]

[4] Some readers may suggest that 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. It would also be preferable to match on the percentage of rentiers in each precinct, but this

3.4 Standard Errors

Because of the complex nature of our data, it is important to explicitly discuss how we estimate the uncertainty underlying our measured effects. Spatial autocorrelation occurs when subjects who are close to each other geographically are more likely to have similar outcomes than subjects who are far apart. Spatial correlation violates assumptions underlying Ordinary Least Squares (OLS) models, leading in many cases to overstated statistical significance. We use correlation-adjusted standard errors to address this possibility.

For all our estimates, we first report block bootstrap standard errors. The block bootstrap is a non-parametric technique for improving the quality of standard errors in spatially dependent data. In the traditional bootstrap, we resample observations with replacement, estimate the target quantity, and quantify the uncertainty around this estimate by taking the standard deviation of simulated values. Under certain conditions, bootstrap standard errors converge to the true standard errors as the sample size grows (Efron and Tibshirani 1986). Block bootstrapping is a more general method.[5] Instead of resampling individual observations, we resample geographic units and all units therein. In our case, the block bootstrap is performed by county (or by metropolitan region for the exit poll data). This approach accounts for geographic correlations and thus is known to de-bias standard errors in dependent data (Hall et al. 1995). For the IV analysis, we also report clustered standard errors.[6]

data is not available for both years. However, this omission would bias our estimates towards 0, since rentiers would be expected to have the opposite reaction to higher home values compared to home owners.

[5]Abadie and Imbens (2008) show that the classical bootstrap fails when matching with replacement is used, but Austin and Small (2014) provide evidence that, when matching without replacement, the classical bootstrap performs well. We use matching without replacement in this analysis. In addition, there is controversy about whether the matching should be done within the bootstrap routine, or beforehand. Austin and Small (ibid.) also suggest that the latter approach is preferable, so our reported standard errors follow this framework, but we re-ran the entire analysis using the alternative method and found that this choice does not affect our results. See also Bertrand, Duflo, and Mullainathan (2004).

[6]Clustered standard errors analytically adjust for correlations between observations in the same geographic area. As a result, they are useful for measuring uncertainty in spatial data. We cluster by county.

4 Results

We first present results from the IV analysis. In the IV approach, we look at communities within 20 miles of E-ZPass toll plazas, and use proximity to E-ZPass as an instrument for change in average home price. Figure 1 gives justification for the IV. 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 the intervention show the sharpest rise in home price. On the right panel, we see the relationship between distance from E-ZPass and average change in Democratic vote share. We find that precincts closest to the intervention show the largest fall in Democratic support. The symmetry is encouraging.

With this in mind, we proceed to the 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.

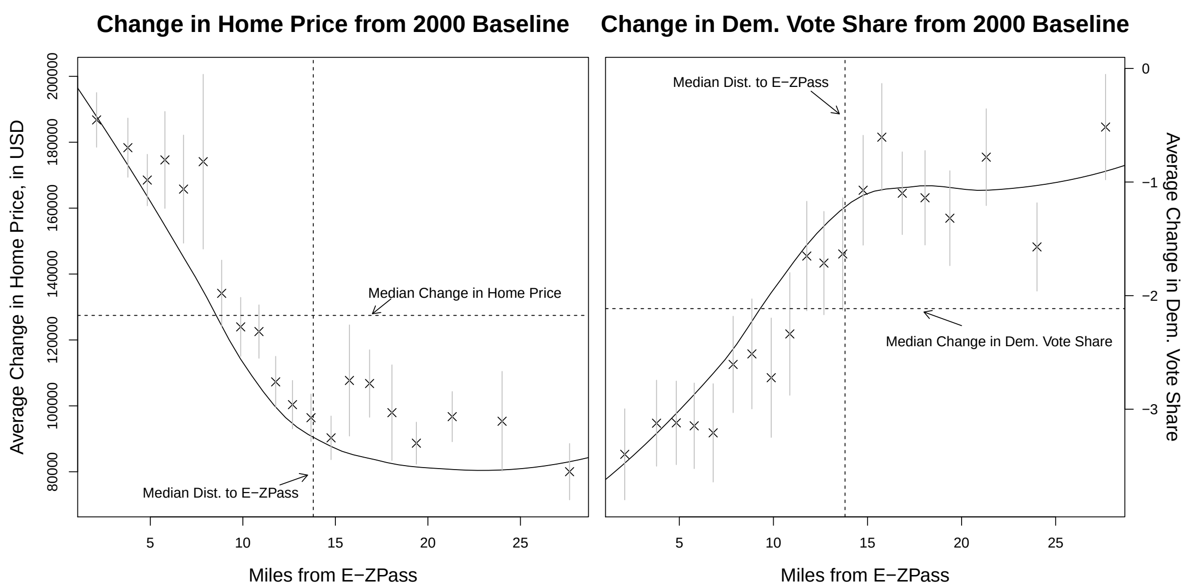

Figure 1: *Distance to E-ZPass and change in average home price and Democratic vote share.*

Table 2 presents these IV results. The first stage effect is strong, with a point estimate of 2.83 and a clustered standard error of 0.83. This first stage gives evidence that those precincts which were closer to the E-ZPass shock saw a greater home price appreciation than precincts farther away. It is possible that there were anticipatory effects associated with the introduction of E-ZPass, and that property values rose after the 1998 announcement of the program. However, this possibility would mean that our estimated effects are conservatively biased towards 0.

Next, using different measurement strategies, we consistently find a negative effect of the intervention on Democratic support, with the largest effects being found for the individual-level outcome. If a precinct were populated only by same-address contributors in average homes, and if home prices received a shock of 16% (increasing from the US average of $300,000 to $350,000), then this precinct would to flip from 52.5% supporting Democrats in 2000 to 52.5% supporting Republicans in 2004.

Table 2: *IV analysis. Estimates for demographic control variables are omitted. Control variables include the matching variables previously discussed, as well as county fixed effects. Clustered standard errors are adjusted for the IV estimation.*

First Stage for Predicting Change in Average Home Price				
Causal variable	Instrument	Estimate	(Clustered S.D.)	(Block bootstrap S.D.)
Δ in Home Price from 2000	*Proximity to E-ZPass Plaza*	2.83	(1.38)	(1.31)

Second Stage for Predicting Change in Dem. Support				
Dependent Variable	Causal variable	IV Estimate	(Clustered S.D.)	(Block bootstrap S.D.)
Δ in Dem. vote share, 2000-2004	*Change in average home price*	-1.35	(0.66)	(0.89)
Δ in Dem. vote share, 2004-2008 (Placebo outcome)	" "	0.1	(0.44)	(0.92)
Δ in Dem. cash share, 2000-2004	" "	-8.24	(3.03)	(3.94)
Δ in Dem. support from same-address contributors, 2000-2004	" "	-12.65	(3.98)	(3.87)

The IV analysis conceptualizes the intervention in one way, but we can also take a different perspective using the diff-in-diff. Table 3 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 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 fairly large, this

Table 3: *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)

difference is statistically significant. A concern would be 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 briefly review exit poll and turnout data for each state and election year in order to estimate how changes in African-American and non-African American voting behavior might have bearing on our conclusions. According to exit polls, Gore was supported by 90.5% of black voters and 51.4% of others, while Kerry was supported by only 83.4% of black voters and 47.3% of others.[7] At the same time, about 53% of the black population and 56% of the non-black population voted in 2000, while 61% of the black population and 66% of the non-black population voted in 2004. Assuming that these state-wide estimates of turnout and support by race are the same in treated and control units, we can estimate the diff-in-diff in two-party vote share purely due to racial imbalance as being about -0.0007.[8] 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, especially because one of

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

[8]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 precincts, while T_O^{0X} indicates the fraction of the population that voted among other groups times the fraction of the population that is not black. D_B^{0X} and D_O^{0X} indicate the proportion of blacks and non-blacks who supported the Democratic presidential candidate in year 200X. 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.

our measures uses within-person variation.

Having examined the balance of treated and control sites, we now present the diff-in-diff results. As expected, average home prices near the intervention sites increased markedly. According to the baseline model, treated precincts saw an increase in average home price of about $82,000 relative to the control baseline, above statistical significance as calculated using both clustered and block bootstrap standard errors. The estimated effect of E-ZPass is similar with or without covariate adjustment.

Table 4: *Main difference-in-difference results. Matching/control variables are same as in Table 3.*

Dependent Variable		DiD Estimate	(Block Bootstrap S.D.)
Δ *Average Home Price*	Baseline model	$81,752	(20,658)
	With covariate adjustment	$79,054	(17,270)
—			
Δ *Dem. Vote Share, 2000-2004*	Baseline model	-2.37	(0.99)
	With covariate adjustment	-2.46	(0.96)
—			
Δ *Dem. Vote Share, 2004-2008 (Placebo Outcome)*	Baseline model	-0.73	(0.94)
	With covariate adjustment	-0.97	(1.00)
—			
Δ *Dem. Vote Share, 2000-2008*	Baseline model	-3.11	(1.42)
	With covariate adjustment	-3.32	(1.55)
—			
Δ *Dem. Cash Share, 2000-2004*	Baseline model	-7.59	(6.21)
	With covariate adjustment	-7.45	(6.11)
—			
Δ *Dem. Support from Same-address Contributors, 2000-2004*	Baseline model	-7.53	(3.37)
	With covariate adjustment	-8.27	(3.30)

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 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 an 8 percentage point decline in the Democratic cash share when we compare treated to control sites between 2000 and 2004. Finally, using individual contribution data, we identify same-address 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 decline in Democratic support of 8 percentage points if we aggregate up by precinct, and similar effects if we fit an individual-level regression model to predict changes in support for the Democratic candidate (see Table 5). This last analysis uses within-person variation, allaying concerns that our findings reflect demographic sorting only, and not genuine changes in political behavior. Table 4 and 5 summarize these results.

Table 5: *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. The proximity variable is defined as the negative of distance to E-ZPass to preserve the interpretation of the coefficient sign.*

Predictor	Effect on Individual Δ in Dem. Support	(Cluster-Robust S.D.)	(Block Bootstrap S.D.)
E-ZPass Indicator	-10.33	(4.03)	(4.87)
Proximity to E-ZPass (in units of 10 miles)	-1.45	(0.54)	(0.60)

4.1 Validity Checks

The IV and conditional difference-in-difference methods presented so far find a consistent effect over a variety of dependent variables, and we next conduct several robustness checks. We first show that the previous results are not sensitive to the exclusion and inclusion rules for the diff-in-diff. We then conduct two placebo tests and replicate the study design in a geographically and temporally separate case. Finally, in the Appendix, we demonstrate that weather is not a time-varying confounder, and show that our diff-in-diff results are not sensitive to the inclusion or exclusion parameters.

Although a major threat to inference comes from time-varying unobserved confounders, we can gain some leverage on the problem 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 to help discern whether areas near toll exits underwent a separate process of political change relative to non-toll areas between 2000 and 2004. In other words, 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, after using the same matching algorithm, exclusion/inclusion rule, and modeling approach as before, we correctly arrive at null results (see Table 6). 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 Appendix).

Table 6: *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)

In a related vein, we can also consider another robustness check, which involves manipulating precincts' distance to E-ZPass and re-running the main analysis exactly as before. In particular, we created a synthetic dataset in which each precinct was moved k miles closer to E-ZPass, where $k = 0, 1, ..., 10$. When the true distance minus k is less than 0, we gave this precinct the maximum distance from E-ZPass minus k. After altering these distances, we carry out the same matching and statistical procedures as in the main analysis. We would be concerned if our estimated effects remain large even after we alter the distances. Ideally, the estimated effects should decay towards 0 as k gets larger. Figure 2 gives evidence for the second possibility. The placebo estimates decay to 0 when the placebo manipulation gets more pronounced. In a final placebo test, we re-ran the analysis for the change in Democratic vote share between 2004 and 2008 in New Jersey and Pennsylvania (where again no effect should be found). Whether we use the diff-in-diff (see Table 4), or the IV method (see Table 2), we again correctly arrive at null estimates.

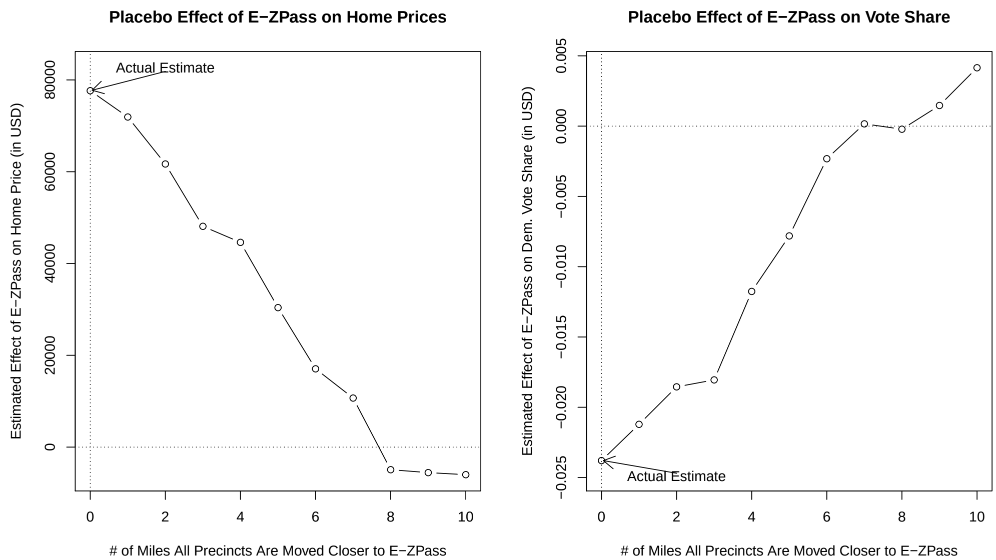

Figure 2: *Another placebo check. In this analysis, we manipulated the distance of precincts to E-ZPass plazas by moving all precincts* k *miles closer to E-ZPass, where* $k = 0, 1, ..., 10$. *When the true distance minus* k *miles was less than 0, we moved this precinct to the maximum distance away from E-ZPass minus* k. *As desired, the estimates decay to 0 as the placebo manipulation gets larger.*

5 Discussion

We interpret the results of the previous sections as demonstrating that the positive shock in property values led to a shift in support away from Democrats and towards Republicans. In this section, we consider the mechanisms that might be underlying this observation.

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 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 engaged in more conservative behaviors following the intervention if our identification mechanism led to an influx of minority residents. Even if individuals adjusted their behavior, this adjustment may have been brought about by compositional changes in treated sites.

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 predictors of Democratic support underwent substantial movement following the intervention. As shown in Table 7, 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.

Table 7: *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)

We next turn to evidence about individual-specific mechanisms. 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 wealth shock. The data suggest that the 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 associated with an increase in the proportion of people expressing concerns about taxation. Together, this analysis gives evidence that the reasons we see the expansion of Republican support following the wealth shock is due in part to the growing salience of taxation.

Table 8: *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)

One of the reasons we cannot be more conclusive about deeper mechanisms is that we use previously collected survey data intended for a different purpose. On the one hand, our research design requires data gathered at a low level of aggregation, which is why we use exit polls that are collected by precinct. On the other hand, a downside of using these polls is that they ask relatively few questions and these vary from year to year. Yet, a design based on geographically differentiated government programs can provide opportunities for forward-looking scholars. Government programs such as the intervention we studied are usually announced in advance of the implementation date. If future scholars were to plan a survey following the announcement of a similar program, they could obtain a more detailed portrait of political attitudes both before and after the expected wealth effects are observed.

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 political science literature: decreased out-group contact and changes in an individual's emotional environment.

One possible explanation is that the intervention decreased contact with outgroups, 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, we think there may be something to this explanation, but are hesitant to place too much weight on the theory. For this explanation to bear out, outgroup contact would have to generate support for the Democratic Party, even as evidence suggests that outgroup contact actually provokes backlash (Enos 2014). Decreased outgroup contact could then expected to be favorable for the Democrats, yet we find that the intervention decreases support for this party.

We also think 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 plausible that they may explain our results. 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 traffic stress, the intervention would be predicted to increase support for the Democratic Party, but we find the opposite effect. While we are sympathetic to the idea that there are psychological explanations we are observing, we do not have evidence for them.

In the end, the two alternative theories seem less compelling than the story about taxes and property values. This explanation is intuitive, supported formal modeling, and consistent with prior research on the relationship between political attitudes and property values (Ansell 2014). One caveat to mention is that the intervention changed individual wealth by affecting the value of their property, so we are not able to identify whether the observed changes are caused specifically by a change in property values or more generally by a change in wealth. Nevertheless, the two are interrelated, as the largest asset for the plurality of Americans is their home.

6 Conclusion

This study has demonstrated the existence of a robust relationship between a government-sponsored program stimulating private wealth and increased support for the Republican Party. This relationship held regardless of whether we look at vote or donations data, whether we use difference-in-differences or instrumental variables to estimate the effect, and whether we only examine individuals who remained at the same address during our study period. 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 economic issues besides taxation does not appear to have been significantly affected. We interpret this as providing limited support for Meltzer-Richard type theories of voting behavior.

Although our substantive findings are significant, one of the most important aspects of our design is that it elaborates upon an alternative way of quantifying the effect of wealth on politics. To study wealth and voting, previous quasi-experimental designs (such as lottery studies) primarily used a cash-transfer programs carried out on a random basis. However, wealth can also be studied in another fashion. That is, a citizen's assets are worth only as much as another entity is willing to pay for them. If researchers can identify external sources of variation in these market prices, it is possible to isolate the impact of wealth shocks on political behavior. In this study, transportation shocks provided this source of external variation, but future research could examine a range of other interventions.

In the end, there are many questions left unanswered by this study about how wealth affects political decision-making. For example, how much does context matter? Scholars studying income have found that socio-economic circumstances seems to matter a great deal, with income effects nearly disappearing in more affluent and racially homogenous areas. Does the same hold for wealth? More broadly, we think there is substantial opportunity for comparativists to build upon these results, exploring the mediating role of national institutions or culture. Globalization and technological change are causing disparities in wealth to grow at an accelerating rate, and we must think creatively about how to identify the consequences for politics. □

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