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Persistence of Political Partisanship: Evidence from 9/11

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The Persistence of Political Partisanship: Evidence from 9/11*

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Abstract

This paper empirically examines whether the act of deciding to support a political party can impact partisan leanings years later. We use the discontinuity in the probability of being registered to vote around the 18th birthday to look at the impact of registration after the 9/11/01 attacks on party of registration. We first show that 9/11 increased Republican registration by approximately 2%. Surprisingly, these differences in registration patterns fully persist over the two year period from 2006 to 2008, even for a group of registrants who moved and changed their registration address. We find full persistence for those registered in zip codes within two miles of a four year university, suggesting that persistence is unlikely to be explained by lack of easy access to or inability to process information. Instead, we suggest an interpretation of our findings based upon either cognitive or social biases.

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1 Introduction

Modern liberal theories of democracy are predicated upon rational discourse and a free flow of information[1]. According to such theories, the importance of discussion is to inform individuals and help them make rational decisions over policy. Rational informed decision-making is critical when voters directly decide on public policy as they do in ballot initiatives. However, it is also important in the choice of representatives, as well as in disciplining representatives to choose reasonable policies. There is a remarkable degree of persistence in political affiliations, which is hard to reconcile with underlying economic incentives, access to information, or cultural background. For instance, the generation which became eligible to vote during the time of the popular Republican President Eisenhower tends to be more pro-Republican even today[2]. In contrast, the generation that came of age during the popular Democratic administrations of Franklin Delano Roosevelt remains to this day unusually pro-Democrat. This difference between these two groups of voters remains despite similarity in underlying economic incentives on policy: almost all voters in both groups are recipients of Social Security and Medicare and most from both groups are retired. Another example is that partisan realignments happen very slowly. When Lyndon Johnson signed the Civil Rights Act, he proclaimed that the Democratic Party had "lost the South for a generation". In fact, it was not until a generation later that the Democrats lost the South. Clear persistent support for the Republican party amongst Southern whites did not appear until 1994 (Black and Black, 2003).

In this paper, we attempt to understand what factors help explain this persistence in political partisanship. In particular, we develop an empirical strategy that allows us to test whether the mere act of registering for a political party today can affect future politics by causing a persistent and enduring support for that party. We analyze whether an individual's choice of political party is consistent with rational learning about the political environment or whether cognitive and social factors shape political preferences. In doing so, we suggest that this persistence may dampen political

[1] The idea that sharing information, promoting discussion and deliberating consequences is key to a successfully functioning democracy goes back at least to the original publication of *The Human Condition* by Hannah Arendt in 1958. The importance of rational communication was expanded upon by Habermas (1987). A good overview of some recent contributions is provided by Elster (1998).

[2] http://www.nytimes.com/imagepages/2006/10/14/weekinreview/15kirk_graphic.ready.html?scp=4&sq=today%27s%20voters:%20how%20generation%20influences%20party&st=cse

competition and be a source of persistence in policy.

Empirically estimating the impact of voter registration on the evolution of voter beliefs is not straightforward. Individuals who register for a party are likely to have personal characteristics which affect their registration decision. High income earners tend to vote Republican (Gelman, 2008); high income earners today are also likely to be high income earners tomorrow. Thus, high income earners are likely to be Republican today and tomorrow. Moreover, the underlying determinants of political partisanship, whether economic or familial, may persist and thus lead to persistence in partisanship. In this paper we argue that the September 11 attacks on the United States provides us with a natural experiment that allows us to test the effect of the initial registration decision on the evolution of political partisanship.

There is a large increase in registration of first time voters around their 18^{th} birthday. This leads to a discontinuity in the probability (a fuzzy discontinuity) of first registering after a given date. In the case of the September 11 attacks, the discontinuity in the probability of first registering to vote after 9/11/01 leads to a discontinuity in the probability of registering initially for the Republican party. Those who, based upon birth date alone, are more likely to register after 9/11/01 are approximately 2% more likely to register Republican. We measure the initial effect using 2006 data. Surprisingly, this gap remains roughly constant over the 2 year time period in our data: from 2006 through to 2008. In fact, we can not reject the initial choice of party due to small random differences in birth date is fully persistent.

We estimate our regression discontinuity using local linear semi-parametric regression. However, our discontinuity design is robust to controlling for linear trends in birth date separately on either side of the discontinuity. Our identifying assumption is that registrants born just before the discontinuity date are not on average different from registrants born just after the discontinuity except in their probability of having registered for the first time after September 11, 2001. Our main identification concern, therefore, is that persistence in political affiliation is due not to the act of voter registration per se, but driven instead by other underlying factors such as future expected income or family background which differ systematically for registrants born on either side of the birth date discontinuity. However, our identifying assumption is supported by several placebo tests. These include tests for balance of covariates, including demographic characteristics, across the threshold and tests for continuity of the density of registrants across the threshold.

Our main result is quite striking. We find that taking a political position such as a decision to register (or not register) for a political party, can in and of itself be a critical determinant of future political identity. Our point estimates are generally very close to full persistence. For our most precise results, results on persistence of Republican registration, we can reject a 50 % reversion over two years. Persistence is not restricted to those with party affiliations but also to independents, suggesting that the electorate may be divided into swing and partisan voters. This suggests a different model of voter behavior from canonical models where the probability of voting for either of two parties is continuously distributed across the population.

In addition to the political findings of our results, we also use our results to provide three possible explanations for our results: costly information acquisition or processing, social biases and cognitive biases. Simple rational Bayesian updating is not consistent with our findings. With costless acquisition and processing of information there should not be any systematic differences in the percent of the population registering Republican across the birth date discontinuity, which is contradicted by our baseline results. Even costly information acquisition or processing models are difficult to reconcile with our results. Political partisanship fully persists even for those who live in zip codes within 2 miles of a four year university where information should be most readily available. Instead, we find a potential role for either or both of two possible mechanisms. First, the act of registering impacts individual or group identity formation which persists even if Bayesian updating is done properly. We call this identity or peer effects. Second, the act of party registration impacts the way that individuals filter news, leading the individual to confirm their original registration decision. We call this confirmatory bias (Rabin and Schrag, 1999).

The contribution of this paper goes beyond the particular interests of social psychologists and behavioral economists to those with a general interest in understanding policy. Our paper contributes to two strands of the political economy literature. One strand has focused on why governments are responsive to the needs of only some sections of the electorate. For instance, Dixit and Londegran (1995, 1996) and Lindbeck and Weibull (1987) emphasize how the policy making process is responsive to the needs of 'swing' electoral groups who have a relatively weak partisan attachment. Another strand in the political economy literature has been preoccupied with the puzzle of the persistence of inefficient economic policies (Fernandez and Rodrik, 1991; Alesina and Drazen, 1991; Majumdar and Mukand, 2004; Coate and Morris, 2000). We unearth a distinct mechanism that simultaneously throws light on both these issues: namely, the persistence in political beliefs. In particular, if voters are unwilling to shift political allegiance in response to new politically relevant information then inefficient policies are more likely to persist. Acemoglu et al (2010) argue that any kind of incumbency advantage results in the persistence of bad governments and policies. We suggest that in as much as incumbency advantage may arise due to the persistence of partisan attachments, we can have bad governments and policies remain in place.

Our estimates suggest that a citizen-voter's initial political affiliation can have important political consequences. This is particularly true since we find evidence to suggest that young voters are particularly impressionable and susceptible to shocks. Consider the implications of our findings when applied to the 2008 US election. In this election according to Pew Research Center, amongst those in the 18-29 category, 66% voted for Obama in 2008 as opposed to 53% in the general population. If we then calculate the vote gap for Democrats between those becoming eligible to vote for the 2008 general election and those becoming eligible in 2000 when the youth gap was zero, our estimates of persistence imply that it will take approximately 26 years to reduce this difference down to a 1% gap.

Finally, our paper makes a contribution to the regression discontinuity literature by introducing a placebo-based inference method for computing standard errors. This method is useful when there is serial correlation in the running variable or in the functional form bias.

In section 2, we discuss relevant literature. In section 3, we describe the data that we use. In section 4, we present our empirical methodology. In section 5, we present our main findings including our estimates of the persistence of political partisanship. In section 6, we quantitatively as well as qualitatively interpret our results; in particular, we reconcile our findings with various theories of belief and preference formation. Finally, in section 7, we conclude.

2 Related Literature

Certainly since the seminal work of Downs (1957), models of political behavior have assumed that citizens are rational actors in their voting decisions. According to this "rational choice" view a citizen's choice of political party is a function of "a running tally of retrospective evaluations of party promises and performance" (Fiorina, 1981). According to this view, a citizen switches his political party affiliation in response to information about the political party and changes in the underlying environment. At first glance, the persistence in a citizen's political affiliation may be considered to be inconsistent with this view. However, Achen (1992) and Gerber and Green (1998), show that a rational Bayesian model is consistent with considerable persistence in a citizen's political party affiliation. Indeed this 'rational choice' view of citizens who process information and update their political choices in a Bayesian manner underlies almost all formal models of the political process in both economics and political science.

Probably the papers most closely related to ours are Alesina and Fuchs-Schudeln (2008) and Giuliano and Spilimbergo (2009). Alesina and Fuchs-Schudeln show that growing up in a Communist political system directly affected ideology, particularly towards redistribution. They show that East Germans are more in favor of redistribution as compared to their West German counterparts. Our paper differs from theirs in three important ways. First, is the difference in identification strategies. Alesina and Fuchs-Schundeln compare the evolution of political ideology between East and West Germans controlling for income and demographic characteristics. In contrast, we compare political affiliations of almost identical cohorts born a few days apart. Second, they do not attempt to throw light on possible mechanisms which may explain the persistent divergence in political opinion. In particular, we show that the mere act of registering for a political party has a persistent impact on political affiliation; moreover, we then interpret our results in light of theories of belief formation. The paper by Giuliano and Spilimbergo examines whether exposure to a recession or a boom when young results has an impact upon beliefs about the role of luck in determining income and upon preferences towards economic redistribution. Their analysis is similar to ours in that it focuses on differences across cohorts (time), while Alesina and Fuchs-Schudeln exploit spatial variation.

Whereas our paper is the first to use the discontinuity in ability to vote at age 18 to estimate persistence of political beliefs, the age-related discontinuity has been used to look at persistence in voting (Meredith, 2009) as well as polarization of ideology (Mullainathan and Washington, 2009). Meredith uses a regression discontinuity in the eligibility to vote in 2000 to estimate the effect of voting in the 2000 election on the probability of voting in 2004. His results are very similar to estimates of persistence in voting from the randomized Get Out The Vote experiments (Gerber, Green and Shachar, 2003). Mullainathan and Washington (2009) was the first paper to use the discontinuity at age 18 to estimate the impact of voting on political preferences. They use survey data to compare political attitudes of 20 and 21 year olds who had voted at least once to attitudes of 18 and 19 year olds who had not yet had the opportunity to vote. Their focus is political polarization. They find that the older cohort of 20 and 21 year olds has more extreme views than the 18 and 19 year old cohort. In contrast to our paper, the differences across cohorts are plausibly due to differences in information. The older cohort on average has more education and may well have also have spent time collecting information when they voted. Even in a world where information collection is costly, opinions should not systematically differ across cohorts where the difference in age is extremely small.

Gerber, Huber and Washington (forthcoming) carry out an interesting field experiment where they examine whether political affiliations (partisanship) affect political attitudes (ideology). They direct a mass mailing to mobilize individuals with 'latent' preferences in favor of a political party to register in the upcoming elections. They survey these individuals subsequently to examine the change in political attitudes over a four month period and find that political partisanship shapes political attitudes.

In many ways, our paper is most closely related to an older tradition in political science that draws on social psychology. In particular, Campbell *et al.* (1960) is an early critic of rational information processing from the social psychology tradition and their classic study highlights "the role of enduring partisan commitments in shaping attitudes towards political objects". Recent expositions of this argument is provided by Bartels (2002) and Green, Palmquist and Schickler (2002). A distinctive aspect of the current paper is that we analyze the role of alternative mechanisms that may result in persistence in political partisanship. These include the behavioral literature on confirmatory bias (Rabin and Schrag, 1999) as well as individual or group identity formation (Akerlof and Kranton, 2010).

Finally, we add to the literature on treatment effects using a regression discontinuity design. We estimate our results using Porter's regression discontinuity estimator (Porter, 2003). We suggest a new placebo-based method for computing standard errors that provides a solution to concerns about testing in the presence of functional form bias (Card and Lee, 2007) as well as serial correlation in the outcome across the running (forcing) variable. As far as we know, serial correlation in the running variable has not been previously been addressed by the regression discontinuity literature.

3 Data

Our main data source is an electronic copy of the universe of voter registration forms for the state of California, collected first in June of 2006 and then again in August of 2008. While examining registration data is interesting in its own right, it would be interesting to also look examine voting data. Not surprisingly, with the ballot being secret such individual level data is not available. Furthermore, aggregate voting data does not contain precise information on individual birth dates, which would prohibit the use the regression discontinuity design for identification. Nevertheless, we observe that when we use precinct-level voting data we are able to show that the precinct-level correlation between registration for a political party and Presidential voting is 0.92 for Democrats and 0.91 for Republicans for 2004. [3] Therefore, registration behavior is a good proxy for voting behavior.

The registration data contains the name of every individual registered in the state, a unique individual numeric identifier which allows for linkage of records over time, the registration date, of birth, sex, and address (including 5-digit zip code). In practice, sometimes the reported date is the date of first registration; other times, it is the date of most recent registration. We see in the data some people who moved and retained their old registration date and others who moved and their registration date changed. This introduces measurement error if we try to use the registration date as the date of first registration.

Between the years 2000 and 2009, California had a closed primary system[4]. In addition, the data reports party of registration for each individual. For most registrants, the party is either Democrat,

[3] Available for download at http://swdb.berkeley.edu/d00/g04.html and http://swdb.berkeley.edu/d00/p04.html respectively.

[4] California recently adopted an open primary system for state-wide offices and for the House of Representatives with the passage of state ballot proposition 14 on June 8, 2010. Party registration would still determine voting rights for President; however, for all other offices, party registration would not determine voting ability.

Republican or not recorded. However, a small percentage of voters also register with other parties.

One concern with estimating persistence using voter registration data is that party registration may persist even if partisanship does not simply because voters don't re-register. Imagine that 9/11 had an impact on party of registration but no one re-registered afterwards. A regression discontinuity would find a persistent impact of 9/11 even if the effect had died out. We therefore estimate our effects both on the universe of voters close to the birth date discontinuity and on a subset of individuals whom we identify in the data as having moved. Voters usually re-register when they move in order to be able to vote near their new address. We can identify movers in our data as registrants whose street number changes between 2006 and 2008. We therefore construct a panel in order to identify individuals who changed their registration because they moved. We use the voter registration identification number to identify individuals. We check to see that individuals are correctly matched. In 100% of cases, the birth date in 2006 matches the birth date in 2008. In 99.91% of cases, the sex also matches. In 17% of cases, individuals changed the numeric portion of their street address. Even in the main sample, we limit registrants to those who are present in both the 2006 data set as well as the 2008 data set. 26.5% of individuals registered in 2008 were not registered in 2006.

For each individual in a given zip code, we extract the average value of census characteristics for their listed zip code, which we use in placebo exercises. We extract the following variables: total population, number living in an urban area, white, black, Native American, Asian, Latino, over 15, median income, number of households, number of families, and number below the poverty line. Except for total population, we express all census variables in per capita terms by dividing by total population in the zip code.

Finally, we use GIS software to create a dummy variable for all zip codes within two miles of a Federal Congressional District boundary. California has 53 Congressional districts. We throw out zip codes which cross Congressional District boundaries. Border zip codes are 44.5% of our sample. We also create a dummy variable for all zip codes within two miles of a four year university. We obtained data from the Thomson Corporation, which publishes the *Peterson's Guide to Four Year Colleges*[5]. There are 260 four year universities in the Peterson's Guide's listings for California.

[5] We thank Devin Pope for providing us with this data.

Registrants living within two miles of a four year university are 56.3% of the sample.

4 Methodology

The key hypothesis examined is whether the mere *act of registering* for a political party increases the probability of registering for that same party even many years later. Identifying such an effect is not straightforward. This is because party registration is correlated with other underlying determinants of partisanship such as income, future expected income and political upbringing. If these other underlying determinants of political partisanship and ideology are persistent over time (which they are likely to be), then we can expect a positive correlation in political partisanship over time. To make matters worse, most of these underlying determinants are not observable to the econometrician.

Accordingly, we address this endogeneity concern using the unforeseen political shock from the September 11, 2001 attacks on the World Trade Centre as a natural experiment. There are two aspects of the experimental design that are key. First, we exploit the fact that a large number of voters typically register for a political party for the first time when they turn eighteen. In fact, in the state of California, any citizen is allowed to register when they will be 18 by the next election. In practice, many act as if they obtain the right to register right after they turn 18. We document this registration behavior in the results section. Second, we argue that political partisanship is randomly and *differentially* assigned between those citizens who were likely to register just before 9/11 and those likely to register immediately after.

Both conditions are necessary for our design. Without a discontinuity in the probability of registering before versus after 9/11/01, there would be no different treatment and control *groups*. Without the 9/11 shocks, there would be no *treatment* differentially across the two groups. Together, this double-discontinuity (in age and in the citizen information sets caused by the 9/11 event) allows us to use a regression discontinuity design in birth date to identify the impact of initial registration on the evolution of political preferences. We note that our approach of comparing individuals with slightly different birth dates and discretely different probabilities of registering for the first time after 9/11 is quite different from comparing those who registered just before versus after 9/11. Whereas there is no fundamental reason why people with small differences in birth dates should register differently, it is quite plausible that 9/11 mobilized Republicans or demobilized Democrats and so that there are underlying differences between those who actually registered before versus after 9/11.

The basic idea of the design is simple. In order to identify the causal effect of voter registration today on the dynamic evolution of political partisanship, we need to consider exogenous differences in voter registration that are unrelated to the other determinants of a citizen's political preferences. Perceived restrictions on age of registration for elections provide one such variable. The cohort of individuals who are likely to register to vote right before 9/11 should be identical to the cohort of individual are likely to register immediately after. Accordingly, we assume that any differences across these two cohorts in the proportion of Republicans, Independents and Democrats can be reasonably attributed to the informational content of (and emotional experience associated with) the 9/11 shock itself.

However, a priori under the assumption that information processing (and re-registration) are relatively costless, we should not expect any systematic differences across these two cohorts in subsequent years. In other words, as citizens update their information sets in response to new information, their political affiliations should change. Accordingly, we should expect there to be complete convergence in the political affiliations of otherwise identical cohorts in subsequent years.

One additional note which is relevant for our estimation design is that individuals are free to register when they choose, as long as they are eligible. Since treatment status is not a deterministic function of age, we cannot use a sharp discontinuity design. Instead, ours is a "fuzzy" regression discontinuity design, where we exploit discontinuities in the probability of treatment conditional on the citizen's date of birth. Though we do not, estimate the impact of registration after 9/11, we in essence use birth date as an instrument for date of registration.

4.1 Estimation Equations

We estimate the impact of birth after the discontinuity birth date in three different ways. In our first set of estimates, we use a linear probability model and regress party of registration dummies on a birth date dummy:

$$P_{it} = a + \beta D_t + \epsilon_{it} \tag{1}$$

where D_{it} is a dummy variable for birth after August 31, 1983 and P_{it} is a dummy variable which either takes on a value of one for Republican and zero otherwise, a value of one for Democratic registrants and zero otherwise, or a value of one for Independents and zero otherwise depending upon the regression. Here i indexes the individual and t the individual's birth date where the discontinuity date is set to $t = 0$. Note that we do not index our variables by that the data was collected (2006 or 2008) because we estimate our results separately for each sample year. We show in the results section that there is a discontinuous jump in the probability of registration after September 11, 2001 for those born after August 31, 1983 versus on or before August 31 1983. We show and discuss this in the results section. August 31, 2001 is the Friday before labor day and 10 days before Tuesday, September 11, 2001. We both show the discontinuity graphically and estimate it formally.

In our second specification, we better control for the functional form of the relationship between the independent variable and the running variable by including linear trends in birth date differentially before and after the discontinuity. We estimate:

$$P_{it} = a + \beta D_t + \gamma_B (1 - D_t)(C - t) + \gamma_A D_t (t - C) + \epsilon_{it} \tag{2}$$

where C is the cutoff birth date. γ_B and γ_A are coefficients for the trends in registration over time before and after the discontinuity date respectively.

Our third specification attempts to improve controlling for the functional form of the relationship between the dependent variable and birth date. We use a semi-parametric estimator due to Porter (2003). Porter's approach is to estimate the coefficient for the jump (dummy) variable controlling for a non-parametric estimate of the outcome variable where the outcome variable is itself a function of the jump variable. The explicit estimation of the jump variable leads to a $\sqrt{N}$ rate of convergence of the discontinuity estimate. This rate of convergence is faster than rates from non-parametric regression discontinuity estimates and thus does not have the same asymptotic bias problems. We estimate:

$$\min_{\beta} \sum_{i=1}^{N} \left[P_{it} - \beta d_{it} - \sum_{j=1}^{N} \frac{K(t_j - t_i)}{\sum_{r=1}^{N} K(t_i - t_r)} (P_{jt} - \beta d_{jt}) \right]^2 \tag{3}$$

where t_i is the birth date of the ith individual and K is an Epanechnikov Kernel function.

We then consider additional checks on our specifications due to remaining concerns about (1.) imperfect ability to capture the functional form, especially given discrete data and (2.) bias in standard errors due to serial correlation both in the underlying data as well as due to functional form error. Card and Lee (2007) assume that error in functional form would induce positively correlated errors at a point in the running variable but serially uncorrelated across the running variable. Their suggestion, in our case, amounts to clustering on date. However, functional form bias can cause serial correlation over time in addition; also, the underlying data can be serially correlated. In this case, the Card and Lee correction will not be sufficient[6]. Therefore, we compute standard errors using a placebo distribution of regression discontinuity estimates. We do this both for the linear trends model detailed in equation (2) and for the Porter estimator detailed in equation (3). We are additionally worried about time varying heteroskedasticity. If day to day shifts in average vote shares for a party is greater in periods right around September 11, 2001, then it is possible that the day to day changes are large due to higher volatility, not a shift in average public opinion. Therefore, we additionally, estimate a distribution of placebo of t-statistics corresponding to the placebo of the mean estimate.

Since our data is discrete (by birth date), we inherently do not have an actual discontinuity. Therefore, the underlying model for regression discontinuity estimation does not exactly match our data. As a robustness check, we therefore also compute the average difference in party registration rates across the days surrounding the discontinuity[7]:

$$\hat{\omega} = \frac{\sum_{i=1}^{N_{t_0+1}} P_{i,t_z+1}}{N_{t_z+1}} - \frac{\sum_{j=1}^{N_{t_0-1}} P_{j,t_z+1}}{N_{t_z-1}} \tag{4}$$

where N_{t_z} is the sample size at date t_z where $t_z = 0$ is the placebo discontinuity date for placebo sample z. We are still concerned with time varying heteroskedasticity. Therefore, we additionally, estimate the standard deviation of daily fluctuations in party registration rates over a twenty day window surrounding the placebo discontinuity dates. We exclude the three dates over which the

[6] In a future version of this paper, we will show this both analytically as well as through simulations.

[7] Note that this simple average could also be estimated using a Porter estimator with a uniform Kernel and bandwidth equal to one.

placebo discontinuity effect, $\hat{\omega}$, is estimated. We report the percentile of the standard deviation for the 9/11 effect relative to the placebo standard deviation distribution.

Our final and main specification is the estimation of persistence. We regress party of registration in 2008 on party of registration in 2006, instrumenting for birth on or after 9/1/83. Note that we have a somewhat more complicated set up than a traditional instrumental variable setup:

$$\begin{aligned} \text{Birth after 9/1/83} \quad &\rightarrow \quad \text{Registration after 9/11/01} \rightarrow \\ \text{Republican Registration in 2006} \quad &\rightarrow \quad \text{Republican Registration in 2008} \end{aligned}$$

Since we are interested in the impact of party of registration in 2006, instrumented by small differences in date of birth, on party of registration in 2008, we bypass the second step and directly estimate registration in 2006 with birth after 9/1/83:

$$\text{Birth after 9/1/83} \rightarrow \text{Republican Registration in 2006} \rightarrow \text{Republican Registration in 2008}$$

We estimate in two different ways: the two stage least squares method with separate linear trends on each side of the discontinuity and the two sample IV method using the Porter estimator. Our two least squares estimation equations are given by:

$$\begin{aligned} (EQ2)\ P_{it}^{2008} &= \alpha + \rho P_{it}^{2006} + \gamma_B^2 (1 - D_t)(C - t) + \gamma_A^2 D_t (t - C) + \epsilon_{it} \\ (EQ1)\ P_{it}^{2006} &= \alpha + \beta D_{it} + \gamma_B^1 (1 - D_t)(C - t) + \gamma_A^1 D_t (t - C) + \nu_{it} \end{aligned} \tag{5}$$

where P_{it}^k is the party of registration for individual i who is born on date t and appears in the voter registration sample from year k.

The main coefficient of interest is ρ. One potential concern with the estimation of ρ is the stationarity of the error term. If, for example, $\epsilon_{it} = \lambda\epsilon_{it-1} + \mu_t$, then standard errors will generally be incorrect if asymptotic convergence is through $T \rightarrow \infty$. Moreover, if $\lambda > 1$, inference is not possible without a transformation of the data. However, we use cross-sectional data to identify a time series parameter: $lim_{N\rightarrow\infty}\, T = 2$. In other words, the number of individuals $I \rightarrow \infty$ as $N \rightarrow \infty$ and since we reasonably assume that ϵ_{it} is identically and independently distributed over individuals i, we can use the central limit theorem to invoke convergence of averages of our residuals to a stable normal distribution. In other words, we invoke the Central Limit Theorem using cross-sectional variation where the error term is stationary and, in fact, distributed i.i.d. If the error term is non-stationary in the time dimension, this does not pose a problem for the computation of our standard errors.

A second potential issue which arises with the estimation of the persistence parameter, ρ, is the difficulty, originally noted by Heckman (1978), in distinguishing state persistence from unobserved hetereogeneity. Without full persistence, this is a problem associated with panels which are small in the time dimension. Individuals with high propensities to be in a given state are also more likely to be in that state initially. In other words, the initial state is not randomly assigned and is in fact correlated with the fixed effect. Thus, the estimate of persistence can be confounded with the estimation of the fixed effect. In a long panel with a stationary degree of persistence, a consistent estimator of the fixed is obtained and the persistence parameter can be properly estimated. Of course, our sample includes only two time periods. Nonetheless, due to the regression discontinuity in birth date, our initial state is quasi-randomly assigned and thus not correlated with the fixed effect. In other words, since we instrument initial party registration with birth date before versus after 9/1/01, initial party affiliation is unlikely to be correlated underlying propensities to affiliate with a given political party. Our estimates thus do not suffer from the traditional bias problems. This remains true even if the process is fully persistent.

We also estimate persistence using the two sample IV estimator introduced by Angrist and Krueger (1995) using estimates of β from the Porter regressions. The two sample IV estimator is the ratio of the first stage divided by the reduced form. In other words it is estimate of the impact of birth after 9/1/83 on registration for a party in 2008 divided by the estimate of the impact of birth after 9/1/83 on registration for a party in 2006:

$$\frac{\beta^{2008}}{\beta^{2006}} \tag{6}$$

We bootstrap the standard errors with 1,000 replications to construct confidence intervals. Different from the two stage least squares IV estimator which is biased in small samples towards the OLS estimator, the two sample IV estimator is biased towards zero.

4.2 Bandwidth Selection

We pick the bandwidth using a cross-validation procedure. We compare our results to those from a four degree polynomial. We estimate separately above and below the discontinuity and within a 100 birth day window on either side of September 1, 1983. We find the minimum mean squared integrable error occurs at a bandwidth of 23 for Republican registration, 36 for Democratic registration, and for independent registration[8]. In all cases, mean integrable squared error seems to follow a quadratic pattern as a function of bandwidth, first falling and then rising. Rather than using different bandwidths for different variables, we choose a uniform bandwidth of 30 which we use to determine the size of our estimation sample in our OLS regression and as the bandwidth in our Porter regressions across all dependent variables. Our results do not qualitatively vary with bandwidth though sometimes significance of variables other than registration after 9/11/01 and Republican registration do depend upon the bandwidth.

5 Main Results

5.1 Discontinuity in Registration After 9/11/01

We now turn to the results. We begin by showing that there is a discontinuity in the probability of registration after 9/11/01 for those born on or after 9/1/83, relative to those born just before. We estimate the Regression Discontinuity in three different ways and report each of them in Table II. First we put in a dummy for 18th birthday occurring after 9/1/01. Second, we control for differential linear trends in birth date separately on either side of the discontinuity. Third, we implement the Porter estimator[9]. We estimate the latter two specifications both on the full sample as well as on a sample of movers who changed their registration address between 2006 and 2008. In our OLS estimates, we cluster on county. We also try the Card and Lee (2007) correction, which in our case amounts to clustering on birth date. Though results were similar across the two specifications, standard errors were actually slightly larger when clustering on county. In both cases, standard errors were similar to unclustered standard errors, suggesting that functional form mis-specification main

[8] We thank Douglas Almond, Joseph Doyle, Amanda Kowalski, and Heidi Williams for sharing their Stata code which implements the cross-validation procedure.

[9] We thank Douglass Miller for posting his .do file which implements the Porter estimator online.

not be a concern for our particular regression discontinuity design. We choose the more conservative option of reporting standard errors clustered on county. Given the number of counties, it follows that the number of clusters is 52.

The discontinuity is estimated by the regression with registration after 9/11/01 as the outcome. There is a clear shift at the discontinuity (see Figure II). The shift is somewhere between six and seven percentage points and t-statistics are generally above ten for the main sample (see Table II). There is a slightly smaller discontinuity (between five and six percentage points) in the sample of movers and t-statistics are between four and five. The decrease t-statistic size are mainly caused by a smaller sample size. As we show in Table I, movers make up approximately 17% of total number of registrants in our sample. The OLS linear trend estimates are similar to those using the Porter estimator. We use this discontinuity to look at long run impacts of decisions at the age of political maturity of choice of political party in the short run and the medium run. Discontinuities in registration after a date approximately 18 years after the birthday are common. Moreover, the discontinuities are often, though not always around 10 days after a birth cohort's birth day. We demonstrate this in Figure III where we shift 9/11/01 backwards as well as forwards in time by intervals of 100 days, starting with a -500 day shift and ending with a +500 day shift and then estimate probabilities of registering after the shifted date by birth date.

Democrats and Republicans may be more likely to be born at different times of the year. If in fact partisanship varies systematically by birth date, it can make it problematic to estimate the impact of registration for a party on future registration for that party. We first check to see whether in fact this is a concern. We use a sample of all registered voters in the state of California from 1971 to the present. We regress a dummy for party of registration on year dummies and month dummies. For Democrats, we find four individual month dummies significant at the 5% level and one additional at the 10% level. The winter months are Democratic and the summer Republican. The month dummies are jointly significant at the 0.01% level. The maximum difference in partisanship is from January to May and is 0.59%. For Republicans the results are very similar with a maximum difference of 0.58% between March and November and the month dummies are jointly significant at a 0% level to four digits. We effectively control for monthly differences in demographics which determine political preferences, by looking at small differences in birth dates.

A more serious issue that we face is that voting is optional in the United States, which implies that not everyone registers to vote. Approximately 72% of potential registrants are actually registered[10]. One potential worry with our design is thus that 9/11 mobilized Republicans or demobilized Democrats born after 9/1/83. We address this in multiple ways. First, we show that the density in the number of registrants is continuous around the discontinuity of first registration after 9/1/01. Therefore, there was no net persistent demobilization or mobilization of voters. Formally, we perform the McCrary[11] test for a discontinuity and are unable to reject the null hypothesis of density continuity. The t-statistic for the test is 0.0655 (and 1.1496 for a sample of registrants who moved between 2006 and 2008). We also non-parametrically plot (Figure IV) the density for registration separately by birth date and by registration date. Visually, the density of registrants by birth date looks flat in the vicinity of 9/1/83.

5.2 Estimates of Impact of Registration After 9/11 on Party of Registration

In the next step we use regression discontinuity design to estimate the impact of birth after 9/1/01 on party of registration: in practice we estimate whether the voter is registered as a Republican, Democrat or an Independent. In our analysis we count as an Independent anyone who is not registered as either a Republican or a Democrat. In 2006, approximately 93% of those registered were in one of these three categories; the others were registered with smaller parties such as the Green Party, the American Independent Party and the Libertarian Party. In 2006, 38% of Californians born after 1970 were registered Democrat, 28% as Republican and 34% were not registered with either major party.

We show estimation results in Table II. In general, the results for the simple dummy model (equation (1)) are different from the trend controls (equation (2)) and the Porter model (equation (3)) estimates. However, the trend controls and the Porter model estimates are quite similar to each other, suggesting that a linear trend most likely accounts for most of the endogeneity due to partisanship trends over time. For the full sample, the increase is only significant at a 5% level for effect on Republican registration in the trend control model. Birth after 9/1/83 increases 2006 Republican registration by 2.5 percentage points in the local trend model and 2.2% in the Porter

[10] See http://www.census.gov/prod/2006pubs/p20-556.pdf, p. 2.

[11] We thank Justin McCrary for making his .do file available online.

model. Both are significant at the 5% level though the local trend estimate is also significant at the 1% level. The estimates in the 2008 data sample are very similar in magnitude: 2.3% in the trend control model and 1.9% in the Porter model, with both being significant at the 10% level and the trend control estimate also being significant at the 1% level. The numerical results are confirmed in Figures IIIA (Republicans), IIIB (Democrats), and IIIC (Independents).

The movement towards the Republican party subtracts both from the Democratic Party and from independents. However, neither estimates are significant at conventional levels in 2006. The 2008 impact in 2008 is -1.6% in both the local trend and Porter estimates and *is* significant at the 10% level using the Porter model.

The results for movers are somewhat different from the results for the full sample and in interesting ways. The estimated effects are substantially larger: approximately 6.2% both in 2008 and in 2006 for the Porter model and 6.8% and 6.5% respectively for the trend control model. All estimates are significant at the 5% level and the 2008 estimate using the trend control model is significant at the 1% level. Different from the results for the full sample, the effects are almost of equal size though opposite in sign for the independents. The estimates for independents are -6.0% in the 2006 trend control model and otherwise within 0.2% of -5.0%. All estimates are significant at the 10% level. The point estimates for Democrats are very small and insignificant in all specifications across both samples of Democrats.

What accounts for the differences in magnitudes across movers and the full sample, as well as the differences in effected political parties? Movers are demographically distinct from the full sample. Looking at table V, movers are more likely to be from zip codes with more whites and smaller populations. They also are more likely to have chosen the Republican party as an alternative to registering independent as opposed to registering Democrat. Consistent with Zaller's (1992) hypothesis about who is impacted by the media, those impacted by the 9/11 shock are likely to be somewhat more highly educated whites living in more urban areas. We will return to the issue of who was impacted and why in the section 6.

5.3 Census and Gender Placebos

In addition to running regressions with registration after 9/11/01 and choice of political party as outcome variables, we also run a number of placebos. Even though we find no net mobilization or demobilization of voters born just after 9/1/83, we are still concerned that 9/11 could have mobilized latent Republicans and demobilized latent Democrats born just after 9/1/83 and in roughly equal proportions. To address this concern, we run placebos on the voter characteristics and characteristics of voters' zip codes for those born on either side of the birth date discontinuity.

We test run regressions with 13 placebo outcomes. 12 of the 13 placebo outcomes are census averages for the zip code of the registrant. The other outcome is the registrant's gender. Our census placebo variables include total population, fraction urban, fraction White, fraction Black, fraction Latino, fraction Native American, fraction Asian, fraction over 15, median per capita income, number of households per capita, number of families per capita, and the poverty rate. Table III shows that none of the placebos is significant at the 5% level. Only gender is significant at the 10% level and only in the mover sample. Therefore, 9/11 was unlikely to have permanently mobilized or demobilized voter registration.

5.4 Robustness Checks

We now present our robustness checks to deal with serial correlation across the running variable and specification bias. The first of these is that we break up the running variable into non-intersecting blocks, each 60 days in length. We then estimate placebo regression discontinuities on the placebo days in the middle of the different blocks and report the percentile of the true value in the distribution compromised of the placebo discontinuities and the true value. We estimate 90 placebo discontinuities starting with individuals born in January, 1971. Of course, in the presence of time-varying heteroskedasticity, it is possible that the true discontinuity estimate has a larger effect just due to higher variance in the true discontinuity block. Therefore, we report p-values both for the distribution of estimates and the distribution of t-statistics. For the impact of birth on Republican registration, our true estimate was larger than any of the other 90 estimates. However, one of the 90 placebo blocks did have a larger t-statistic though with a negative mean impact. In addition, the p-value for the effect on Democratic registration was 0.09 though the p-value for the t-statistic distribution was considerably lower at 0.21. We graph the distributions in Figure VI. The impacts on independent registration are not close to significance at any conventional levels. In Figure VII, we also graph the distribution of the census placebos and self-reported gender. We do find two out of 13 significant at a 10% level though none at the 5% level. Also, none of the distributions of the t-statistics are significant at a 10% level.

As an additional check on functional form bias, we estimate the simple difference estimator in equation (4). The benefit of this estimator is that it formulates a hypothesis which conforms to the data and thus does not suffer from functional form bias. However, since the change is estimated using a small number of points, it is a high variance estimator. Since we are again concerned about serial correlation in heteroskedasticity, we break up the running variable into non-intersecting blocks of 20 days and estimate 271 placebo changes in addition to 271 placebo standard deviations of the outcome variable over the placebo windows. We compute the standard deviations excluding the days over which the change is computed. Instead of taking the change between the day and the day before the discontinuity, we look at a two day change. Our results are quantitatively and qualitatively very similar to the one day change. The change for both Republicans and Democrats are significant at the 5% level though not the 1% level. The mean estimate for Republicans is a change of 4.6% and for the Democrats a change of -6.9% putting the changes into the 97.8 percentile and 1.1 percentile of the respective placebo distributions. The standard deviations were at the 63.6 percentile and 19.1 percentile of the Republican and Democratic placebo standard deviation distributions, reinforcing that the increase in Republican voting and decrease in Democratic voting is not due to an increase in cross-day variability in party of registration around 9/11.

5.5 Estimates of Persistence

In table VI, we estimate our main specification: the degree of persistence. We regress party of registration in 2008 on party of registration in 2006, instrumenting party of registration with birth after 9/1/01. We do this with both the full sample and the mover sample and in two ways: with OLS and with the Porter estimator.

Before presenting our instrumental variables estimates, we first provide some evidence whether on our instrument's monotonicity. This is important in order to be able to interpret the IV estimates as a local average treatment effect of people likely to switch party affiliation due to the 9/11 event. We bootstrap our estimates with 1,000 replications. Only three of our 1,000 bootstrap estimates for the impact of birth after 9/1/01 on Republican voting are negative and only 21 for the impact on Democratic registration were positive. This is in contrast to the independents, for whom 48.4% were negative and 51.6% were positive.

For the OLS estimates, we include linear trends in birth date differentially before and after 9/1/01 as an included instrument. Our sole excluded instrument is birth after 9/1/01. In four out of six cases, we can reject the null hypothesis of no persistence but in no cases can we reject the null of full persistence. In the other two cases, the first stage F-statistics are below one. Moreover, our precise estimates are all within 0.1 below one and 0.25 above one. In particular, the estimates for Republicans are 0.938 for the full sample and 1.072 for the movers sample. The Republican full sample estimate is the only one with a first stage F-statistic above 10. Since weak instruments can lead to estimates that are biased towards the OLS estimate and can also lead to incorrect test sizes, we also estimate using the Porter estimator. We use a simple two sample IV, bootstrap the standard errors with 1,000 replications where we report the percentage of bootstrapped estimates below zero as our p-values. The p-values are generally higher with the two sample IV, potentially due to weak instruments and small sample bias. However, in our four precisely estimated cases, the two sample IV estimates look quite similar to the 2SLS IV estimates. All the two sample IV estimates except for the full sample independent voter estimate are within 0.2 of one. The estimates for Republicans are 1.166 for the full sample and 1.013 for the movers sample. The Democrat full sample estimate is 0.935. All three of these are significant at the 5% level. In addition, the Independent voter mover sample's point estimate is 1.020 and is significant at the 10% level.

We do not show results of the impact of 9/11 on party of registration because the impact of birth date on registration after 9/11 is underestimated. Sometimes, when citizens re-register, their registration date remains the same and other times, the registration date changes. Unfortunately, this depends upon policies of local counties. Since a much larger percentage of those born before 9/1/83 registered just before 9/11/01, re-registration lowers the measured effect of birth after 9/1/83 on 9/11/01. This leads to some unreasonable results. We can see this from a two sample IV of the impact of registration after 9/11 on Republican registration. This is obtained by dividing the impact of birth after 9/1/83 on party of registration by the impact of birth after 9/1/83 on registration after 9/11/01. We shows some simple calculation focusing on our benchmark of impact on registration for the Republican party.

Using Porter estimates, which are similar to the results using the linear control model, in 2006, we estimate that registration after 9/11 increases Republican registration by 33%; for movers, the impact is much higher: 113%. Between 2006 and 2008, the estimates for the impact on Republican registration do not change much, consistent with full or near full persistence. For the sample as a whole, the decline in the estimated impact is 14%; for the movers, the estimated change in the impact is 0%. In contrast, there is a marked decline in the measured impact of birth after 9/1/83 on registration after 9/11/01. For the full sample, the point estimate declines by 27% and for the movers, who are more likely to change their registration, the point estimate declines by 47%. 35% of movers in have a registration date in 2007 or 2008 in the 2008 data set. Therefore, the estimates of the impact of registration after 9/11/01 on Republican registration increase dramatically to 40% for the full sample and 214% for movers. So, whereas our design is useful for estimating the persistence of party affiliation, it only provides an upper bound estimate of the impact of registration after 9/11 on party of registration.

6 Interpretations

We now try to better interpret our findings. First, we look at which groups (age and region) responded to the 9/11 shocks. Then, we discuss the quantitative impacts of our findings dynamically over time. Finally, we use our findings to understand models of belief formation.

6.1 Responses to Information

This subsection documents who reacted to the 9/11 political shock. We look across age and partisanship of region of residence. We check whether our results are consistent with Converse's (1976) hypothesis that the young are much more subject to influence by news. Similarly, we also check the consistency of our findings with Zaller's (1992) theory that those who are subject to influence by news are those who pay attention, but are not as partisan.

6.1.1 Age

We examine whether the 9/11 shock had a much larger impact on the political registration behavior of 18 year olds than those in the age group 25 to 60. We mainly focus on the younger age group for two reasons. The first and most important is that there is a discontinuity in registration probability when people turn 18. This is critical for our identification strategy because it randomizes, within the group of 18 year olds, those who register first after 9/11/01 (and thus are more likely to initially register Republican) and those who register first before 9/11/01 (and thus are initially more likely to register Democrat). However, another benefit of looking at 18 year olds is that individuals at this age are particularly open to political persuasion. There is a large literature which says that political ideas are formed in early adulthood and afterwards ideas stabilize and become harder to change (Converse, 1976). Therefore, older adults may, in general, be less likely to be influenced by political events. Because they have lower variance political priors, they may be less likely to react to a political shock in the short or the long run. More recent research has challenged this view (Green, Palmquist and Schickler, 2002). To look at this issue, we run additional specifications, concentrating on people who were between 25 and 60 years old on 9/11/01, and who re-registered to vote within a 30 day window of September 11, 2001.

We run the Porter regression of Republican registration, Democratic registration and independent registration on date of registration with a discontinuity date of 9/11/01. We only report the results in the text. We do find an impact on Republican registration,which is remarkably similar magnitude to our benchmark estimate of 18 year olds: 2.2%. The effect is significant at the 1% level. Whereas we see a small and statistically insignificant impact on Democratic registration (-0.05%), the impact on registration as an independent is -1.7% and significant at a 5% level. However, we have to be careful in comparing these estimates to our estimates for 18 year olds. The estimates for 18 year olds were estimates of the impact of *birth date*, not registration date on Republican registration. As discussed in Section 5.5, our estimates for the impact of *registration* after 9/11 on Republican registration is 33%; it is estimated to be 113% for the movers subsample. Of course, these estimates have an upward bias due to a downward bias in the measurement of the impact of birth after 8/31/83 on registration after 9/11/01. However, even for the full sample of 18 year olds, the estimated impact would have to fall by an order of magnitude in order to be even close to the impact for those older than 25. This would imply an impact of birth after 8/31/83 on registration after 9/11/01 of over 50%, which is implausible.

Moreover, we are concerned about the endogeneity of our findings for the older population. Whereas Republican 18 year olds can not shift their birth date in response to 9/11, Republicans can differentially re-register after 9/11. To check for potential endogeneity, we run a Porter regression on sex and year of birth. Those registering just after 9/11/01 are 0.6 years older on average, and the effect is significant at even a 0.1% level of confidence. Additionally, we perform the McCrary test for continuity of registration around 9/11 and are able to reject at a 1% level of confidence. Accordingly, we conclude that the impact of the 9/11 shock seems to have had a much larger impact on 18 year olds than on those between 25 and 60, consistent with Converse (1976).

6.1.2 Partisanship By Region

Where information is more available, we expect less persistence. Information availability depends upon both the demand side and the supply side. Later, we will look at the demand side (zip codes near universities). We now look at the supply side. We look at more partisan and less partisan districts. We examine the impact of 9/11 in pro-Democratic, Republican and swing districts. Accordingly, we split our sample into the top third most Democratic, the top third most Republican and the remaining third in the middle according to aggregate Congressional vote share in the Congressional District. The Democratic districts have Republican vote shares of less than 32.5%. The Republican districts have a Republican vote share of greater than 64%. The swing districts then have Republican vote share between 32.5% and 64.0%. Table VIII shows that in all three types of districts, there is a large and significant impact of birth on or after 9/1/83 on registration after 9/11/01. The magnitude is around 6-7% in all cases for 2006. The largest impact on the Republican vote share is in the Democratic districts. It is 4.5%, and significant at a 95% level of confidence. The effect in the Republican districts is slightly smaller at 3.9%, but is not significant at even a 90% confidence level, partially due to the smaller sized effect and partially due to higher standard errors. Note that the sample size for the sum of three types of districts is significantly smaller than the overall sample size. This is due to zip codes which cross Congressional District boundaries and thus get dropped. In the swing districts there is not much movement in vote share from 9/11. Also note that the effect on Democrat vote share is larger in magnitude than the effect on Republican vote share in the Democratic areas. People who otherwise would have registered Democrat instead register Independent as well as Republican. This is different from the Republican areas, where there was a movement towards the Republican party from both the Democrats and the Independents. We note that where we have significant effects (the Democratic districts) in 2006, we have significant effects of a similar magnitude in 2008, once again confirming full, or close to, full persistence across the population.

Since Republican, Democratic and swing potentially attract very different types of individuals, we restrict our sample to people registered in zip codes within 2 miles of a Congressional District boundary. We then break up our sample into zip codes in a district which had a close House of Representatives election in 2002 or 2004. We compare our results in these competitive districts to similar districts within 2 miles of a Congressional District boundary in lop-sided races. We define close races as races where the two party vote share was less than 55% and greater than 45% for the two parties. All other races we define to be lop-sided. Figure VII is a map of the state of California with electoral district boundaries in red and zip codes within two miles of a Congressional District boundary in blue. We throw out zip codes which cross Congressional District Boundaries. The estimation results are shown in Table VII. We only find an impact of 9/11 in the lop-sided districts where political parties are the least active. However, the null effect in close election areas is somewhat illusory. We do *not* find an impact of birth after 9/1/83 on registration after 9/11/01. This could be in part due to competitiveness of political parties in reaching out to 18 year olds and mobilizing them to register right at their birth day. We find an equally large effect of birth after 9/1/83 on registration in the non-border areas as in the close election border areas. However, there is no significant impact of 9/11 on Republican registration in 2006. The point estimates are 0.014, as opposed to 0.043 in the lop-sided districts. Border zip codes are more rural, wealthy, and ethnically white (Table VI). Many of the non-border areas are in close election districts, potentially explaining the lack of a significant effect in those areas. This lack of persistence of the 9/11 shock in competitive districts suggests that political persistence is not driven by costly information processing.

Unfortunately, more independent areas did not react to the 9/11 shocks and so we can not use differential response by degree of party competition to make inference about the role of information availability in generating persistence. However, the fact that there was an impact of 9/11 in both Democratic and Republican areas but not in areas which are more mixed is itself quite interesting. It suggests either of two possible interpretations: (1.) those in the middle are less interested in politics and thus do not react to new information or (2.) they react less because both parties are active in filtering information which moderates the impact of news. This latter interpretation is consistent to Zaller's (1992) theory that individuals who are knowledgeable are less influenced by news. Zaller focussed on demand-side determinants of knowledge. However, political parties may play a role as well on the supply side. This would be an interesting area for future research.

6.2 Magnitudes

We showed that an individual's initial choice of political party persists and we can not reject that it persists indefinitely - at least for the set of voters whose political affiliations were affected by the September 11 attacks.[12] We should point out that our estimates cannot rule out the possibility that there is heterogeneity in persistence in which some voters persist indefinitely and others persist a sufficiently small amount of time that the switchers had already switched by 2006 when we first collected registration data. There is a large long run difference between full persistence horizon and 90% persistence over two years much less 60% over 2 years. Whereas all of the confidence intervals for our persistence estimates include full persistence (a coefficient of 1.0), they all also include a two year persistence between 0.8 and 0.9. We should point out that heterogeneity in persistence is consistent with our findings. For example, it is possible that preference shocks persist indefinitely for some members of the electorate while for others, persistence is so small that the switchers had already switched by 2006 when we first collected registration data.[13]

The range of numbers within our 95% confidence interval. There is a large long run difference between full persistence over a two year horizon and 90% persistence over two years much less 60%. Our analysis suggests that the length of time required to reduce the average partisan gap by an

[12] Furthermore, the degree of persistence in political partisanship is even more striking if we account for the possibility that the group of registrants affected by 9/11 were if anything, less ideologically partisan before their birth date.

[13] We should point out that our estimates cannot rule out the possibility that there is heterogeneity in persistence and that some voters persist indefinitely and others persist a sufficiently small amount of time that the switchers had already switched by 2006 when we first collected registration data.

order of magnitude (to 10 percent of the initial gap) is quite long. In particular, a 2 year persistence coefficient of 0.6 implies that it takes 10 years, a coefficient of 0.8 implies 22 years, 44 years with a coefficient of 0.9, and 460 years with a coefficient of 0.99. On average, convergence never happens with a coefficient of 1.0.

Given this persistence, it is possible that a president who is popular with the young can have a large impact on election outcomes years into the future. For instance, according to Pew Research Center, amongst those in the 18-29 category, 66% voted for Obama in 2008 as opposed to 53% in the general population[14]. In contrast, since 1980, the youth gap had never been higher than 6%. Moreover, 18-29 year olds accounted for 17% of the electorate. That is roughly a 2.3% impact on the vote share. Vote share differences in US Presidential elections have been below 2.3% almost once a decade: 4 times since 1960. In 1960, the Kennedy defeated Nixon by 0.1%; then in 1968, Nixon defeated Humphrey by 0.7%. In 1976, Ford lost to Carter by 2.1%; finally, in 2000, Bush won over Gore with an aggregate vote share differential of -0.5% (though a majority of the electoral votes). Moreover, if we take our lowest point estimate for Republican persistence (the party for which we have the greatest precision), it is 0.938. If we then calculate the vote gap for Democrats between those becoming eligible to vote for the 2008 general election and those becoming eligible in 2000 when the youth gap was zero, it will take approximately 26 years to reduce down to a 1% gap.

Estimates of full persistence do not at all preclude switching. In fact, in our main sample of people born within 30 days of 9/1/83, 2.6% switch from Republican to Democrat and 4.3% switch from Democrat to Republican. Unsurprisingly, the numbers are larger in magnitude for movers. 7.9% of movers switch from Republican to Democrat and 12.1% switch from Democrat to Republican[15]. How are these results of substantial switching reconcilable with our findings of full or near full persistence? The answer is that they measure very different things. Lets examine the case of full persistence. People born just after 8/31/83 are more likely to have initially registered Republican than people born just before. Individuals may switch from Republican to Democrat or vice versa. However,

[14] See http://pewresearch.org/pubs/1031/young-voters-in-the-2008-election.

[15] Alternatively, we could have estimated hazard models. However, given the measurement error in registration date, the results would not have been terribly meaningful. Therefore, we could have only reasonably estimated hazard rates off of the difference between the 2006 data sample and the 2008 data sample. Reporting simple transition probabilities seemed a more parsimonious approach given our data limitations.

this will not happen differentially across the birth date threshold. Therefore, the average differences between those born on either side of 9/1/01 will remain despite random individual switching. In other words, there will be no mean reversion from the 9/11 shock in the aggregate population.

6.3 The Persistence of Political Partisanship: Possible Mechanisms

We now use our estimates of persistence to help us better understand models of belief formation. A range of potential theories may help explain aspects of persistence of partisanship. We discuss each of them in turn, be it costly information acquisition or processing, social effects or cognitive biases.

1. *Bayesian Learning & Persuasion*: Our persistence results are incompatible with models of strict Bayesian learning. Any differences in beliefs across Bayesian learners must be due either to differences in priors (Acemoglu, Chernozhukov and Yildiz; 2009) or due to differences in information. However our RD design allows us to rule out that differences in priors is the culprit. On average, we should not expect priors to differ across groups of people who are born a couple of days apart. Could differences in information at the time of initial registration account for long run differences in partisanship? 9/11 was a large enough political event that rational Bayesian individuals registering differently because they were born a few days apart should not have different posteriors about politics after 9/11. Moreover, they also should not systematically seek different information sources.

Nevertheless, since parties send out information to their registered voters, it is possible that the two different groups could passively receive different information. Kamenica and Gentzkow (forthcoming) show that a biased source can manipulate a rational Bayesian updater's decision by choosing what massages to send. In particular, they show that it is possible for a Democratic news source to persuade rational Bayesian updaters to register Democrat by sending signals which most of the time convince the voter that the Democratic candidate is better and a small percentage of the time convince the voter that the Republican candidate is much better. In a new working paper, Kamenika and Gentzkow (2010) show that media consumers become fully informed when two media sources with opposing viewpoints both send messages. So their argument relies on large costs of information acquisition. We turn to that particular mechanism next.

2. Costly Information: If information collection or processing costs are an important source for persistence, we should expect greater political persistence amongst those individuals who find it costlier to collect or process information. For instance, better educated individuals, or those who are located where education levels are higher are likely to have lower costs of processing politically relevant information. We examine this by looking at zip codes within 2 miles of a 4 year university. These zip codes are enclosed by red lines in the map of the state of California shown in Figure VI. University zip codes tend to be somewhat poorer, less likely to have people under 15, are more urban, and have a higher density of African Americans (Table VI). Somewhat surprisingly, there are similar impacts of birth on or after 9/1/01 on registration after 9/11/01 in the university and non-university areas (Table VII). However, there is only an impact on party of registration in the university zip codes. The impact on Republican registration is 3.1% and significant at the 5% level, even with the smaller sample size. There are no significant impacts on either registration as an independent or as a Democrat. The point estimates are negative for impacts on both Democrats and independents and roughly equal in size but they are not significant in either case. Moreover, the point estimate for Republicans is even larger in 2008 than in 2006, suggesting full persistence. Since we find full persistence even in university areas, we are inclined to disfavor channels that emphasize costly information collection or costly information processing.

3. Social Effects and Identity: One possibility is that an individual's pre-existing set of friends strongly influences political party choice. Our experimental design rules out this type of simple peer effect. It is unlikely that prior to registering for a political party, individuals systematically preferred to associate with only those others who were born on either side of an arbitrarily chosen date. This is especially true given that we are looking at such small differences in the date of birth. In contrast, registering for a political party may help crystallize an 'identity'. This may result in political persistence if this 'identity' results in an 'identity cost' of switching political parties (Akerlof and Kranton, 2010).

4. Cognitive Biases: Cognitive factors may also help explain political persistence. In particular, our findings are consistent with confirmatory bias (Rabin and Schrag, 1999) where an individual pays selective attention or interprets information so as to confirm her initial priors. First impressions may affect memory recall or alter subsequent information filtering. This is closely related to the idea of cognitive dissonance - where registering for a political party may make the individual psychologically vested in that decision. The individual will, then, process information that favors his initial choice of party - resulting in political persistence.

Our findings, are also consistent with explanations which are mixtures of both cognitive and social biases. We give a couple of examples. First, there are peer group effects. People choose to form friendships based at least in part upon politics. Gentzkow and Shapiro (2010) show, using survey data, that whereas media exposure is not very segregated across Democrats and Republicans, friendships are much more segregated; moreover, political discussions mostly happen between people with very similar politics. People may retain the same public political partisan leanings because doing otherwise would be socially costly. However, this is not enough to explain registration behavior. People who change their partisanship could lie to their pre-existing friends. However, cognitive dissonance combined with peer group effects could easily explain persistence. Another possible channel is what DeMarzo, Vayanos and Zwiebel (2003) call "persuasion bias" - where individuals fail to account and adjust for repetition of information that they may receive. For example, individuals may belong to and socially interact with a Democratic group in university. However, information processing will be biased if an individual treats information received from his endogenously chosen social network as independent. However this cognitive bias is again not enough in and of itself. Individuals must first form social networks with people of similar partisan leanings.

Accordingly, we argue that the balance of the evidence seems to suggest that the persistence in political partisanship is driven mainly by some combination of social identity effects and cognitive biases. There is much less support for rational Bayesian arguments, information processing arguments or pre-existing social networks.

7 Conclusion

Does the mere act of affiliating with a political party increase chances of affiliating with that same party in the future? If so, how long does the impact of original affiliation persist? We answer these questions by looking at differences in initial party registration behavior of 18 year olds who happened to register to vote for the first time just before versus after September 11, 2001 due to small differences in birth date. The 9/11 attacks were on the one hand, a large enough political event to have had an impact and, on the other hand, unforeseen so that those registering just before 9/11 were not a select group. In particular, individuals registering in the state of California for the first time before

9/11/01 did not know about the bombings when they registered and those registering immediately after did. Our results are quite striking. We find that on average the 9/11 attacks moved registrants towards the Republican party by over two percentage points. Moreover, the initial choice of political party persists and we can not reject the hypothesis that it persists indefinitely. This persistence in political partisanship is surprisingly extreme, especially accounting for the fact that most likely, relatively less partisan sets of individuals had their politics affected by 9/11. Our findings have important implications for the study of political economy. They provide another channel for policy persistence; policies may persist simply because support for a party persists.

We explore possible reasons to account for this political persistence. We find full persistence for those registered in zip codes within two miles of a four year university - suggesting that information availability is unlikely to be a factor. We do find, however, no impact of 9/11 in highly politically competitive areas, consistent with Zaller's (1992) notion that the informed are not influenced by news. We also find full persistence for independents, suggesting that the electorate is potentially broken into groups of partisans and non-partisans. This finding has strong implications for models of voting behavior such as the probabilistic voting model. Whereas our findings are difficult to reconcile with Bayesian models of political inference, including ones allowing for costly information acquisition or costly cognitive computation, we find our results consistent both with theories of confirmation bias and political identity formation.

We hope that future work will be better able to identify the groups who are particularly subject to political news shocks. Are the elderly less susceptible to influence than the young? What role does education play? We would also like to see if taking a political stance leads to persistence for the entire population instead of just those who are susceptible to news shocks. In addition, we hope to see further clarification on whether persistence in political beliefs is due more to individual political identity formation, social group pressure or whether it is more for cognitive reasons. Understanding persistence is important not only for understanding human behavior and belief formation but also for understanding how public policy evolves.

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Table I
Numbers of Registrants Over Varying Birth Windows

	Num. Born Before 9/1/01	Num. Born After 9/1/01	Total Born in Window	Share Born Before	Num. Reg. Around 9/1/01	% Reg Around 9/1/01	Mover Share
Full Sample							
10 Days	5503	5508	11574	49.98%	538	4.65%	
20 Days	11159	11279	23001	49.73%	1607	6.99%	
30 Days	16743	16978	34284	49.65%	3144	9.17%	
40 Days	22113	22617	45293	49.44%	4956	10.94%	
50 Days	27778	27958	56299	49.84%	6838	12.15%	
Mover Sample							
10 Days	976	930	2000	51.21%	76	3.80%	17.28%
20 Days	1936	1917	3947	50.25%	221	5.60%	17.16%
30 Days	2889	2828	5811	50.53%	440	7.57%	16.95%
40 Days	3837	3790	7721	50.31%	695	9.00%	17.05%
50 Days	4841	4667	9602	50.92%	970	10.10%	17.06%

Notes: (1.) Columns list numbers of registrants born just before and after 9/1/01 for various window sizes (there is a discontinuity in probability of registration after 9/11/01 at 9/1/01), (2.) Share Born Before is the share of those born before 9/1/01 in the sum of those born before 9/1/01 and those born after 9/1/01, (3.) The total window also includes those born on 9/1/01, (4.) Movers restricts the sample to registrants who changed their recorded address between 2006 and 2008.

Table II
Impact of Birth After 9/11/83 on Registration Date and Party of Registration

Outcome	Year	Dummy	Trend Controls	Movers	Local Polynomials	Local Poly.: Movers
Reg. After 9/11	2006	0.103***	0.067***	0.058***	0.066***	0.055***
		(0.0064)	(0.0052)	(0.0117)	(0.0065)	(0.0150)
Reg. After 9/11	2008	0.073***	0.048***	0.029***	0.048***	0.029***
		(0.0040)	(0.0035)	(0.0078)	(0.0054)	(0.0094)
Republican	2006	0.014***	0.025***	0.065**	0.022**	0.062**
		(0.0035)	(0.0063)	(0.0258)	(0.0107)	(0.0260)
Democrat	2006	-0.012*	-0.012	-0.005	-0.015	-0.012
		(0.0051)	(0.0090)	(0.0193)	(0.0113)	(0.0272)
Independent	2006	-0.002	-0.013	-0.060*	-0.007	-0.051*
		(0.0051)	(0.0092)	(0.0232)	(0.0114)	(0.0275)
Republican	2008	0.010***	0.023***	0.068***	0.019*	0.062**
		(0.0036)	(0.0068)	(0.0250)	(0.0106)	(0.0252)
Democrat	2008	-0.009*	-0.016	-0.016	-0.016*	-0.012
		(0.0052)	(0.0095)	(0.0203)	(0.0114)	(0.0279)
Independent	2008	-0.001	-0.007	-0.052*	-0.003	-0.050*
		(0.0048)	(0.0097)	(0.0202)	(0.0113)	(0.0272)
N		34,288	34,288	5,801	34,288	5,801
OLS		Y	Y	Y	N	N
Trend Controls		N	Y	Y	N	N
Mover Sample		N	N	Y	N	Y
Clustered on County		Y	Y	Y	N	N

Notes: (1.) All regressions are in a 30 day window surround the birthdate 9/1/01, (2.) Dummy is a simple regression of the outcome variable on a dummy for birth after 9/1/01, (3.) Trend controls allows for linear controls in time separately before and after 9/1/01, (4.) Movers restricts the sample to registrants who changed their recorded address between 2006 and 2008, (5.) The final two columns show the results of the Porter Estimator for the full sample and for movers respectively, (6.) Standard errors are clustered on county for the OLS regressions, (7.) Year refers to the year of the sample, (8.) Reg. After 9/11 is a dummy variable which takes on one if the voter's recorded day of registration is after 9/11/01, (9.) Republican refers to registration of the Republican Party, Democrat refers to registration for the Democrat party, and Independent refers to registration for neither of the two major parties, (10.) *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table III
Census Placebos

Census Variables	OLS w/ Trends	OLS: Movers	Local Polynomials	Local Poly. : Movers
Total Population	-566	-1,047	-566	-1047
	(605.8)	(754.1)	-492.5	-1111.7
Gender	0.012	0.051	0.012	-0.055*
	(0.0126)	(0.0374)	(0.0139)	(0.0327)
Urban	0.005	0.004	0.005	0.004
	(0.0042)	(0.0067)	(0.0034)	(0.0077)
White	0.007	0.004	0.007	0.004
	(0.0079)	(0.0126)	(0.0061)	(0.0137)
Black	-0.003	0.001	-0.003	0.001
	(0.0038)	(0.0030)	(0.0024)	(0.0049)
Native American	0.000	0.000	0.000	0.000
	(0.0005)	(0.0004)	(0.0003)	(0.0004)
Asian	0.002	0.000	0.002	0.000
	(0.0021)	(0.0041)	(0.0027)	(0.0057)
Latino	-0.006	-0.005	-0.006	-0.005
	(0.0039)	(0.0110)	(0.0055)	(0.0118)
Over 15	0.001	0.000	0.001	0.000
	(0.0011)	(0.0027)	(0.0014)	(0.0036)
Median Income	-0.59	0.513	-0.590	0.513
	(0.6270)	(0.9880)	(0.6827)	(0.7995)
Num. Households	0.000	0.000	0.000	0.000
	(0.0014)	(0.0034)	(0.002)	(0.004)
Num. Families	0.000	0.001	0.000	0.001
	(0.0007)	(0.0018)	(0.0008)	(0.0020)
Poverty	-0.003	0.001	-0.003	0.001
	(0.0025)	(0.0053)	(0.0021)	(0.0048)
N	34,288	5,801	34,288	5,801

Notes: (1.) All regressions are in a 30 day window surround the birthdate, (2.) OLS regressions allow for linear controls in time separately before and after 9/1/01, (3.) Movers restricts the sample to registrants who changed their recorded address between 2006 and 2008, (4.) The final two columns show the results of the Porter Estimator for the full sample and for movers respectively, (5.) Standard errors are clustered on county for the OLS regressions, (6.) LHS variables are census averages of demographics at the county level except for Gender which is from the voter's registration form, (7.) *** p<0.01, ** p<0.05, * p<0.1

Table IV
Inference Using Running Variable Placebos

	Summary Statistics	Total	9/11 Change	Rank	Percentile
Mean (Republican Change)	-0.001	272	0.046**	6	97.79%
Mean (Democrat Change)	0.000	272	-0.069**	269	1.10%
Mean (Independent Change)	0.001	272	0.023	48	82.35%
SD (Rep. Change)	0.105	272	0.106	99	63.60%
SD (Dem. Change)	0.140	272	0.132	220	19.12%
SD (Ind. Change)	0.126	272	0.133	69	74.63%

Notes: (1.) Mean is the average change over two days for the number of voters registering Democrat, Republican and Independent over 272 different pairs of days from 1970 to 2006; the days are themselves spaced 20 days apart, (2.) SD is the mean of the standard deviation of the variable over the 20 day period omitting the days over which the variable change is computed, (3.) 9/11 Change is the change from August 31 to Sept 2, (4.) Rank is the rank of the change around 9/11 compared compared with the other 271 date pairs, (5.) Percentile is the percentile rank of the 9/11 change, (6.) *** $p<0.01$, ** $p<0.05$, * $p<0.1$

Table V
Persistence Estimates

Outcome	Republican Full Sample	Republican Movers	Democrat Full Sample	Democrat Movers	Independent Full Sample	Independent Movers
IV: 2SLS	0.938***	1.072***	1.222***	3.518	0.409	0.900***
	(0.1514)	(0.2615)	(0.3185)	(14.7076)	(0.8415)	(0.2535)
1st Stage F Stat.	12.5513	4.8911	2.9378	0.0375	0.6879	5.7105
IV: Porter	1.166**	1.013**	0.935**	0.986	2.369	1.020*
P-Value	0.010	0.032	0.026	0.282	0.256	0.078

Notes: (1.) All regressions are in a 30 day window surround the birthdate 9/1/01, (2.) All 2SLS regressions regress the dummy for party (Republican, Democrat and Independent) membership in 2008 sample on the party dummy from the 2006 sample with two included instruments (linear trends differentially before and after 9/1/01) and one excluded instrument (birth after 8/31/01), (3.) Porter IV estimates are the ratio of the 2008 Porter estimates for a party divided by the 2006 Porter estimates (i.e. a two sample IV estimate), (4.) Standard errors for the Porter IV estimates are bootstrapped with 1,000 replications, (5.) P-values are the bootstrapped estimates below zero, (6.) Standard errors are clustered on county for the OLS regressions, (7.) Movers estimates restrict the sample to registrants who changed their recorded address between 2006 and 2008, (8.) *** p<0.01, ** p<0.05, * p<0.1

Table VI
Mean Comparisons

	Moved	Univ.	Border	Close Race		Moved	Univ.	Border	Close Race
Repub.	0.008	-0.091***	0.021	0.098**	Total Pop.	-2,026***	4,433***	-5,944***	-4178
(2006)	(0.0125)	(0.0206)	(0.0191)	(0.0465)		(723)	(1647)	(1696)	(4410)
Dem	-0.011	0.053***	-0.007	-0.004	Gender	-0.093***	-0.014***	0.009	0.028*
(2006)	(0.0082)	(0.0192)	(0.0184)	(0.0365)		(0.0062)	(0.0049)	(0.0060)	(0.0155)
Indep.	-0.010	0.030*	-0.012	-0.076**	Urban	-0.0023	0.064***	-0.031***	-0.073*
(2006)	(0.0139)	(0.0155)	(0.0107)	(0.0377)		(0.0059)	(0.0136)	(0.0081)	(0.0423)
Repub.	-0.015	-0.088***	0.018	0.086*	White	0.060***	-0.043	0.074**	0.011
(2008)	(0.0121)	(0.0196)	(0.0186)	(0.0456)		(0.0171)	(0.0363)	(0.0292)	(0.0853)
Dem	0.025**	0.058***	-0.006	-0.010	Black	-0.005	0.033*	-0.011*	-0.024
(2008)	(0.0108)	(0.0182)	(0.0190)	(0.0375)		(0.0049)	(0.0197)	(0.0060)	(0.0177)
Indep.	-0.010	0.030*	-0.012	-0.076**	Native	0.001***	-0.002***	0.002**	0.001
(2008)	(0.0139)	(0.0155)	(0.0107)	(0.0377)	American	(0.0003)	(0.0004)	(0.0007)	(0.0014)
					Asian	-0.014***	0.036**	-0.026***	-0.014
						(0.0027)	(0.0164)	(0.0057)	(0.0252)
					Latino	-0.044***	-0.026	-0.040	0.023
						(0.0133)	(0.0306)	(0.0272)	(0.0628)
					Over 15	0.011***	0.021**	0.015***	-0.031***
						(0.0037)	(0.0079)	(0.0040)	-0.0106
					Median	-0.097	-1.708**	2.115**	4.349
					Income	(0.4384)	(0.7676)	(0.8296)	(5.4961)
					Num	0.021***	0.018**	0.018***	-0.019
					House	(0.0043)	(0.0069)	(0.0046)	(0.0126)
					Num	-0.001	-0.012***	-0.001	0.013***
					Families	(0.0015)	(0.0029)	(0.0028)	(0.0047)
					Poverty	-0.001	0.009	-0.005	0.024
						(0.0043)	(0.0160)	(0.0113)	(0.0181)
Observ.	34,228	34,228	34,228	15,242		34,228	34,228	34,228	15,242

Notes: (1.) Reported coefficients are from an OLS regression of an outcome variable (rows) regressed on a dummy variable (columns), (2.) Moved is a dummy for the sample of mover across zip codes; Univ.is a dummy for voters registered in zip codes within 2 miles of a university; Border is a dummy for voters registered within 2 miles of a Congressional District Border; Close Race is a dummy for the subsample of border zip codes where the House Race was won by less than 5% points, (3.) Variables on the Right Panel are census averages for a registrant's zip code with the exception of Gender which is the registrant's actual gender, (4.) Standard errors are clustered on county, (5.) *** p<0.01, ** p<0.05, * p<0.1

Table VII

Heterogeneity of Effects: Competitive Electoral Districts, Universities, and District Partisanship

Outcome	Non-Border Zip Codes	Lop-Sided Elections	Close Elections	University Zip Codes	Non-Univ. Zip Codes
Reg. > 9/11	0.045***	0.049***	0.007	0.046***	0.048***
(2006)	(0.0066)	(0.0077)	(0.0356)	(0.0065)	(0.0076)
Reg. > 9/11	0.045***	0.049***	0.007	0.046***	0.048***
(2008)	(0.0066)	(0.0077)	(0.0356)	(0.0065)	(0.0076)
Republican	0.014	0.043**	-0.012	0.031**	0.010
(2006)	(0.0142)	(0.0173)	(0.0662)	(0.0137)	(0.0168)
Democrat	-0.004	-0.038**	0.020	-0.015	-0.013
(2006)	(0.0151)	(0.0184)	(0.0648)	(0.0155)	(0.0166)
Independent	-0.010	-0.005	-0.008	-0.015	0.003
(2006)	(0.0152)	(0.0186)	(0.0518)	(0.0154)	(0.0168)
Republican	0.012	0.043**	-0.004	0.034**	-0.001
(2008)	(0.0141)	(0.0172)	(0.0658)	(0.0136)	(0.0167)
Democrat	-0.009	-0.033*	0.018	-0.023	-0.006
(2008)	(0.0152)	(0.0186)	(0.0652)	(0.0156)	(0.0167)
Independent	-0.003	-0.010	-0.014	-0.011	0.007
(2008)	(0.0151)	(0.0185)	(0.0524)	(0.0153)	(0.0167)
N	19,046	14,168	1,074	19,296	14,992

Notes: (1.) All regressions use the full sample of registrants born within a 30 day window surrounding the birthdate 9/1/01, (2.) All regressions use the Porter Estimator, (3.) Non-border zip codes refers to the sample of all registrants in zip codes which are not within two miles of a Congressional District boundary; Zip codes within two miles of a Congressional District boundary are split into Lop-Sided Elections, where vote margins of victory are less than 10% and Close Elections where vote margins of victory were less than 10%, (4.) University Zip Codes are zip codes within two miles of a 4 year university; Non-University Zip Codes are zip codes within two miles of a university, (5.) Zip codes for estimates by partisan leaning which cross Congressional District boundaries are dropped, (6.) Year refers to the year of the sample, (7.) Reg. After 9/11 is a dummy variable which takes on one if the voter's recorded day of registration is after 9/11/01, (8.) Republican refers to registration of the Republican Party, Democrat refers to registration for the Democrat party, and Independent refers to registration for neither of the two major parties, (9.) *** p<0.01, ** p<0.05, * p<0.1

Table VIII

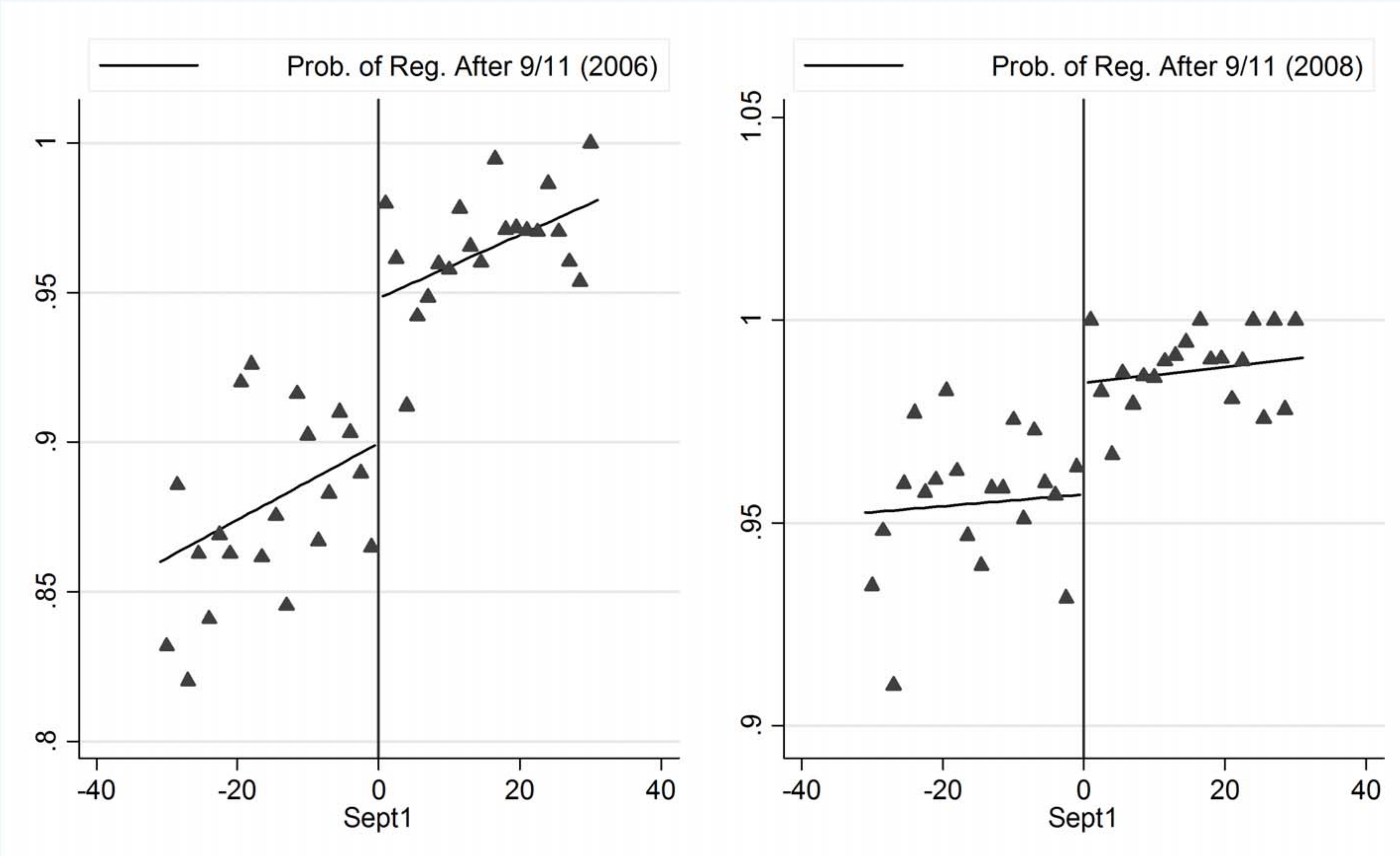
Heterogeneity Across Democratic, Swing and Republican Districts

Outcome	Year	Dem. Dist. Full Sample	Swing Dist. Full Sample	Rep. Dist. Full Sample	Dem. Dist. Movers	Swing Dist. Movers	Rep. Dist. Movers
Reg. After 9/11	2006	0.063***	0.067***	0.059***	0.031	0.020	0.107***
		(0.0174)	(0.0185)	(0.0156)	(0.0354)	(0.0447)	(0.0311)
Reg. After 9/11	2008	0.043***	0.047***	0.041***	0.013	0.024	0.061***
		(0.0144)	(0.0152)	(0.0136)	(0.0202)	(0.0276)	(0.0211)
Republican	2006	0.045**	0.013	0.039	-0.015	-0.059	0.135**
		(0.0226)	(0.0299)	(0.0297)	(0.0523)	(0.0728)	(0.0642)
Democrat	2006	-0.069**	-0.014	-0.029	-0.009	0.099	-0.007
		(0.0311)	0.0295	(0.0275)	(0.0719)	0.0718	(0.0601)
Independent	2006	0.024	0.002	-0.010	0.024	-0.040	-0.142**
		(0.0309)	(0.0286)	(0.0292)	(0.0718)	(0.0702)	(0.0637)
Republican	2008	0.047**	-0.003	0.025	-0.005	-0.065	0.089
		(0.0225)	(0.0297)	(0.0295)	(0.0497)	(0.0705)	(0.0625)
Democrat	2008	-0.072**	-0.004	-0.028	0.010	0.146**	0.041
		(0.0312)	(0.0297)	(0.0279)	(0.0728)	(0.0733)	(0.0622)
Independent	2008	0.025	-0.006	0.002	-0.006	-0.081	-0.129**
		(0.0307)	0.0285	(0.0291)	(0.0707)	(0.0706)	(0.0640)
N		4,988	5,041	5,216	900	873	1,095

Notes: (1.) All regressions are in a 30 day window surround the birthdate 9/1/01, (2.) All regressions use the Porter estimator, (3.) Democratic Districts are the third most Democratic Congressional Districts; they are districts with Republican vote share less than 32.5%; Swing Districts have in between 32.% Republican vote share and 64.0% Republican vote share; Republican Districts are the most Republican districts and have Republican votes share greater than 64.0%, (4.) Zip codes which cross Congressional District boundaries are dropped, (5.) Year is the year of the sample, (6.) Reg. After 9/11 is a dummy variable which takes on one if the voter's recorded day of registration is after 9/11/01, (7.) Republican refers to registration of the Republican Party, Democrat refers to registration for the Democrat party, and Independent refers to registration for neither of the two major parties, (8.) *** p<0.01, ** p<0.05, * p<0.1

Figure I: Birth After 8/31/83 on Registration After 9/11

Porter Estimates



Note: Share of Voters Registering Before 9/11 by Birth Date

Figure II: Kernel Density Estimates
of Registration and Birthdates Around 9/11

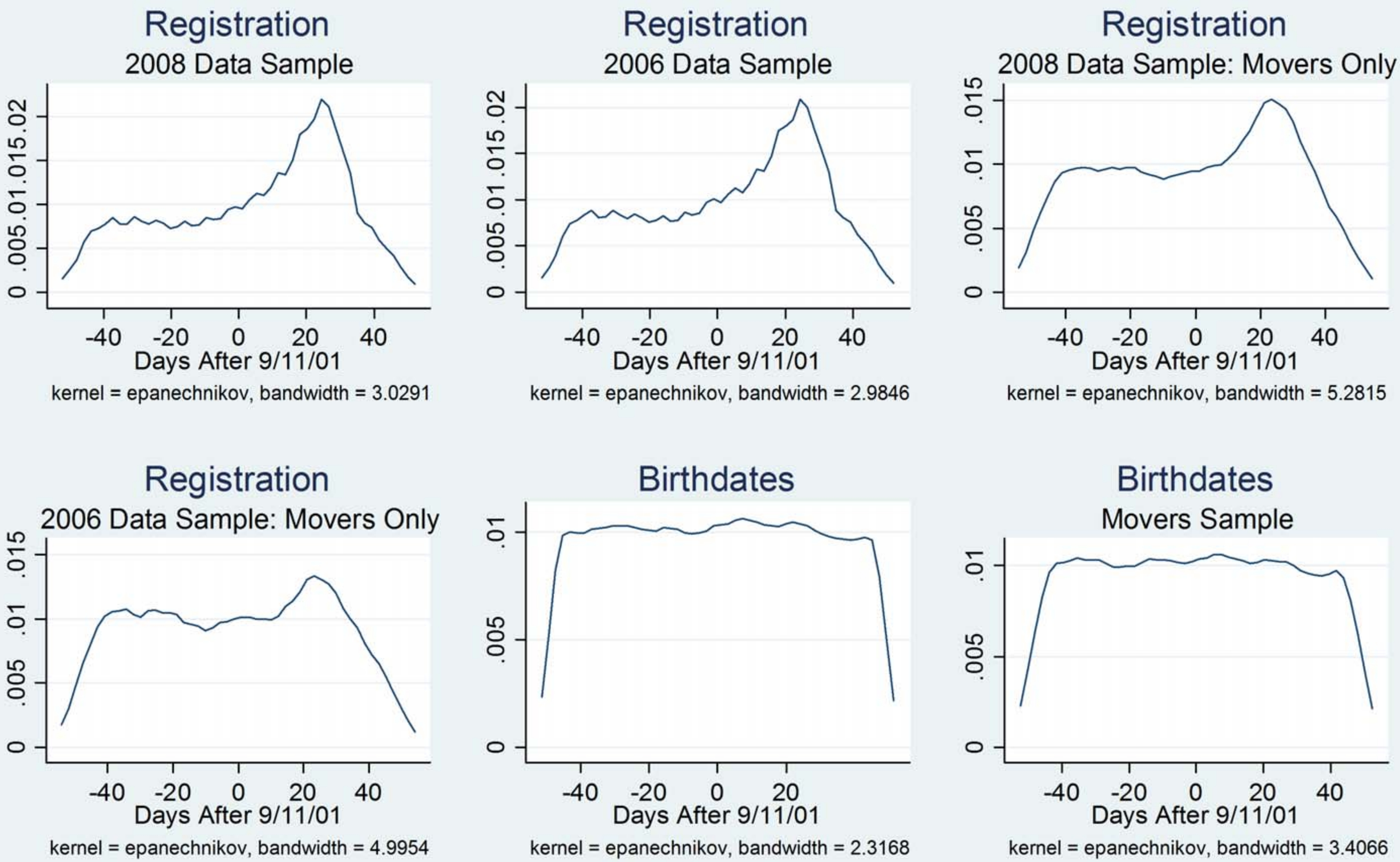
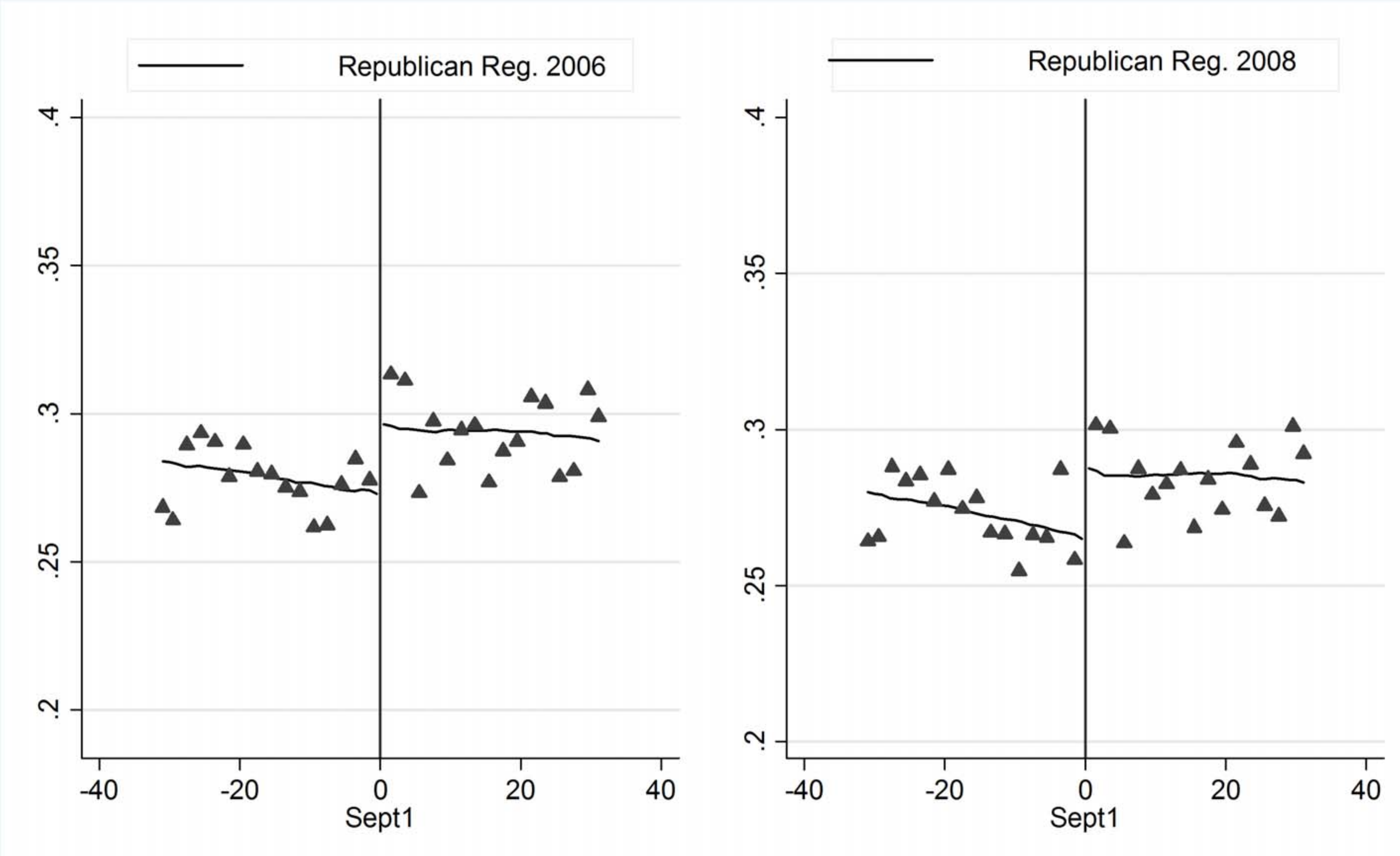


Figure IIIA: Republican Registration by Birth Date

Porter Estimates



Note: Share of Voters Registering with Republicans on Y-Axis

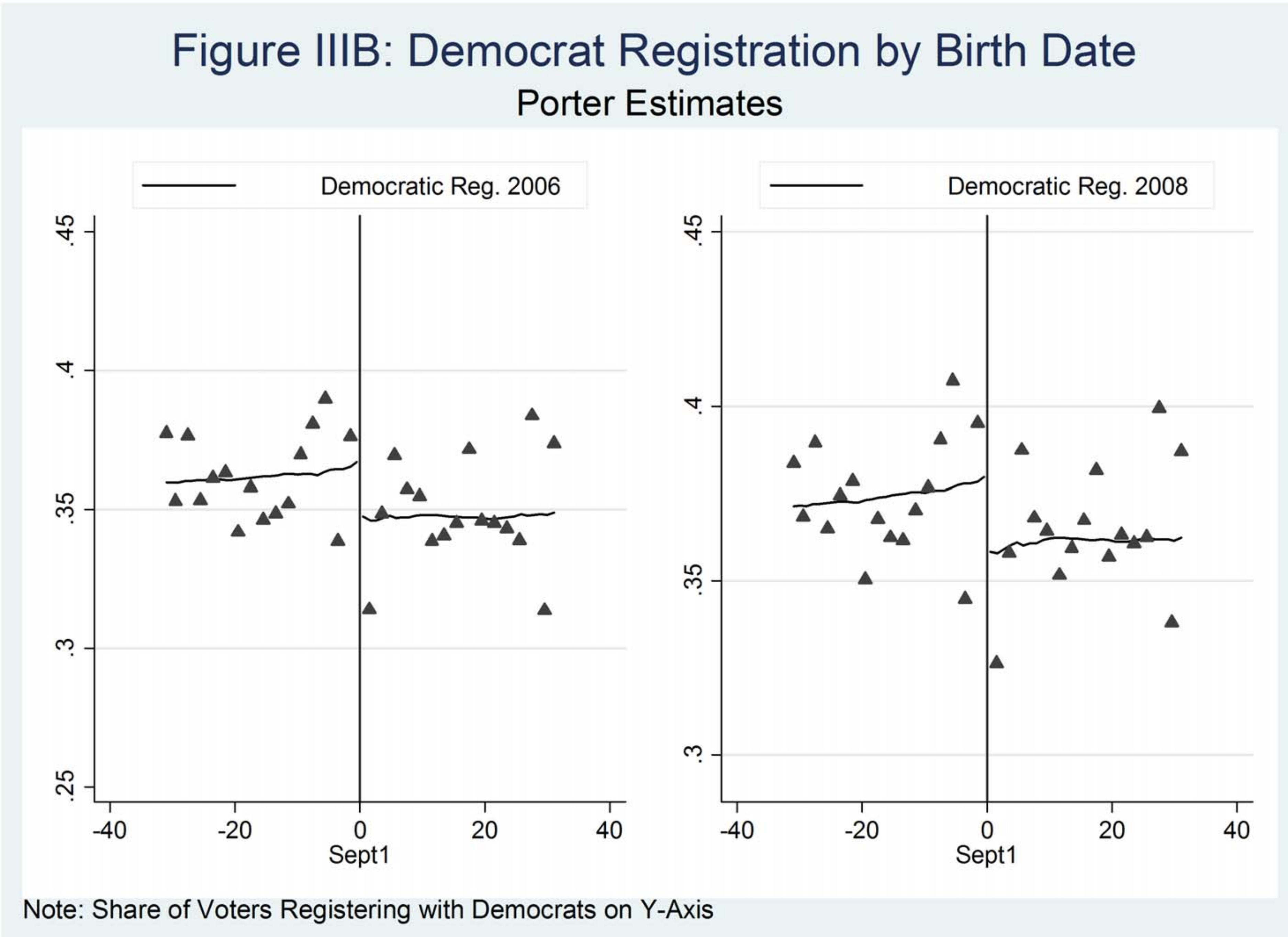
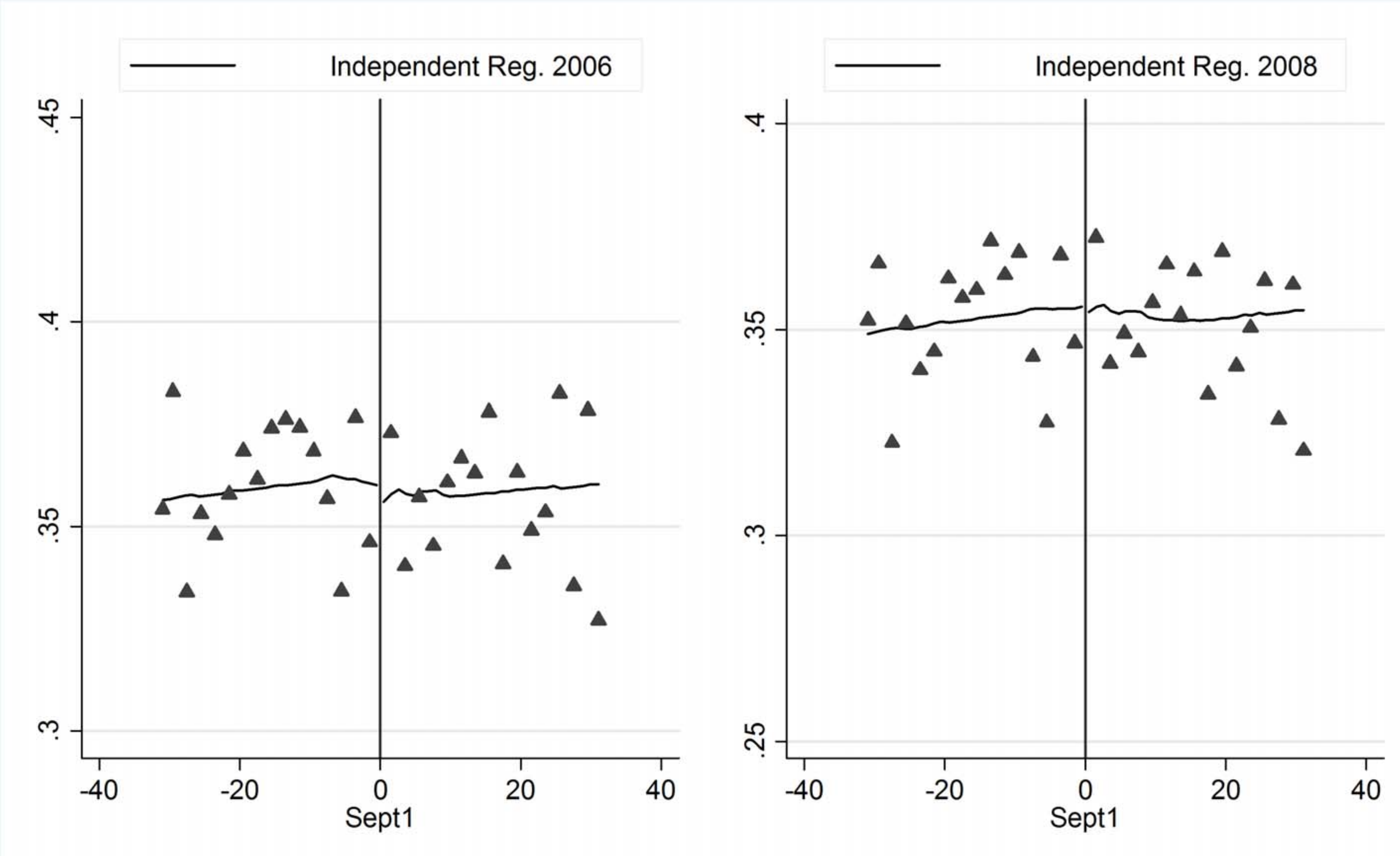


Figure IIIB: Democrat Registration by Birth Date
Porter Estimates
Democratic Reg. 2006
Democratic Reg. 2008
.25
.3
.35
.4
.45
-40
-20
0
20
40
Sept1
Note: Share of Voters Registering with Democrats on Y-Axis

Figure IIIC: Independent Registration by Birth Date

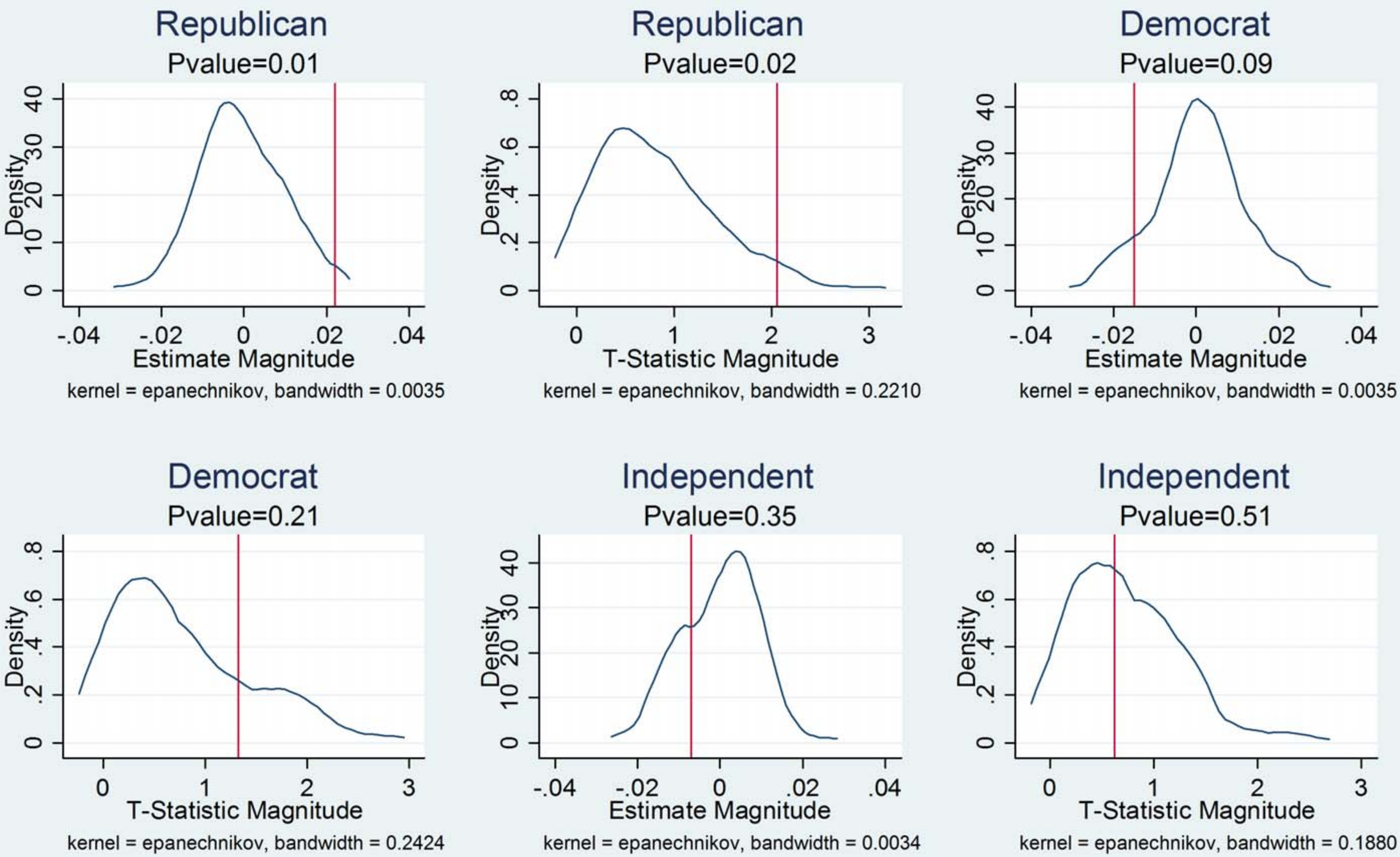
Porter Estimates



Note: Share of Voters Registering Independent on Y-Axis

Figure IV: Distributions of Placebo Estimates (Main Vars)

Porter Estimator



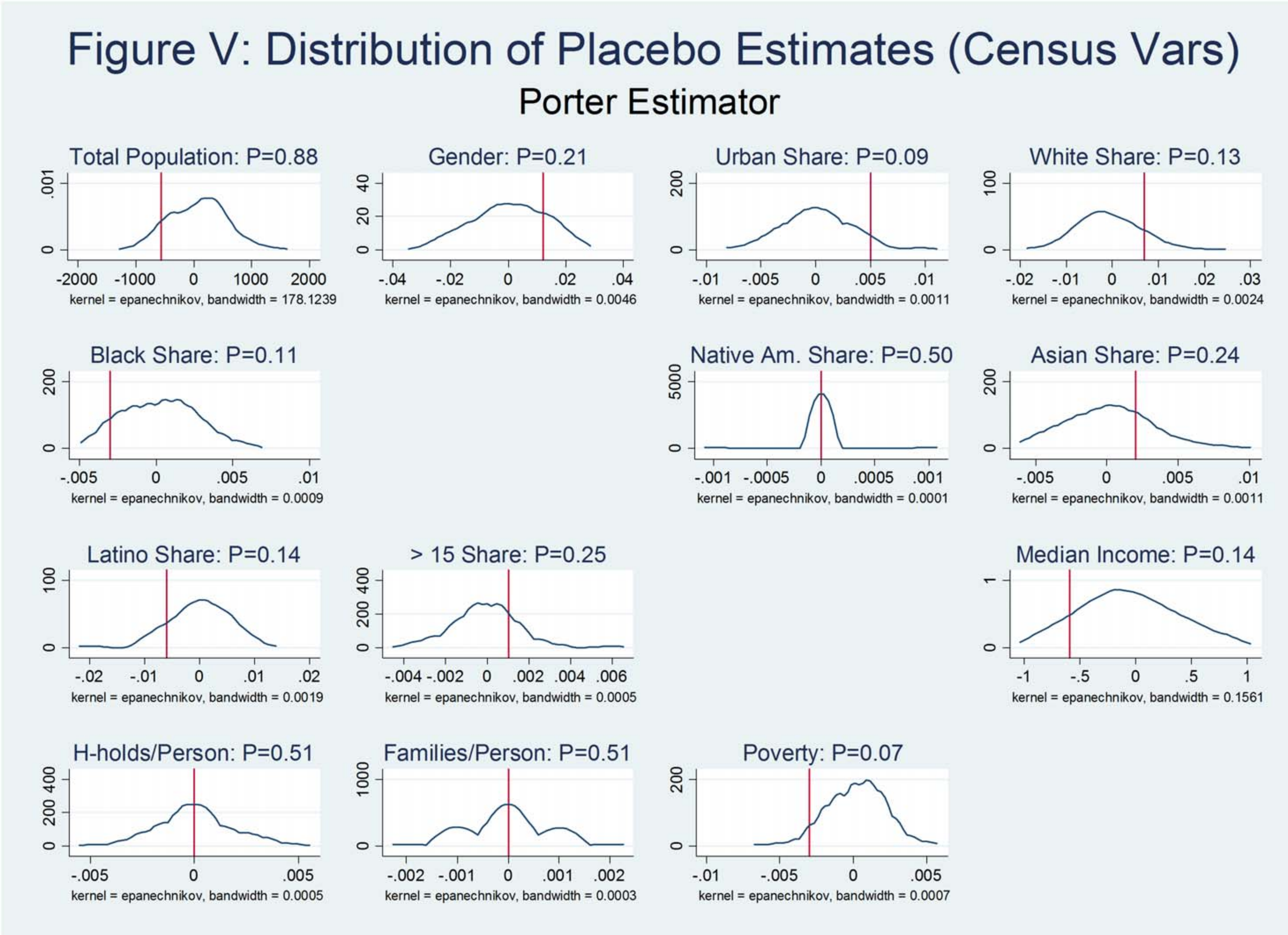


Figure V: Distribution of Placebo Estimates (Census Vars)
Porter Estimator
Total Population: P=0.88
kernel = epanechnikov, bandwidth = 178.1239
Gender: P=0.21
kernel = epanechnikov, bandwidth = 0.0046
Urban Share: P=0.09
kernel = epanechnikov, bandwidth = 0.0011
White Share: P=0.13
kernel = epanechnikov, bandwidth = 0.0024
Black Share: P=0.11
kernel = epanechnikov, bandwidth = 0.0009
Native Am. Share: P=0.50
kernel = epanechnikov, bandwidth = 0.0001
Asian Share: P=0.24
kernel = epanechnikov, bandwidth = 0.0011
Latino Share: P=0.14
kernel = epanechnikov, bandwidth = 0.0019
> 15 Share: P=0.25
kernel = epanechnikov, bandwidth = 0.0005
Median Income: P=0.14
kernel = epanechnikov, bandwidth = 0.1561
H-holds/Person: P=0.51
kernel = epanechnikov, bandwidth = 0.0005
Families/Person: P=0.51
kernel = epanechnikov, bandwidth = 0.0003
Poverty: P=0.07
kernel = epanechnikov, bandwidth = 0.0007

Figure VI: Zip Codes Within 2 Miles of a Congressional District Boundary

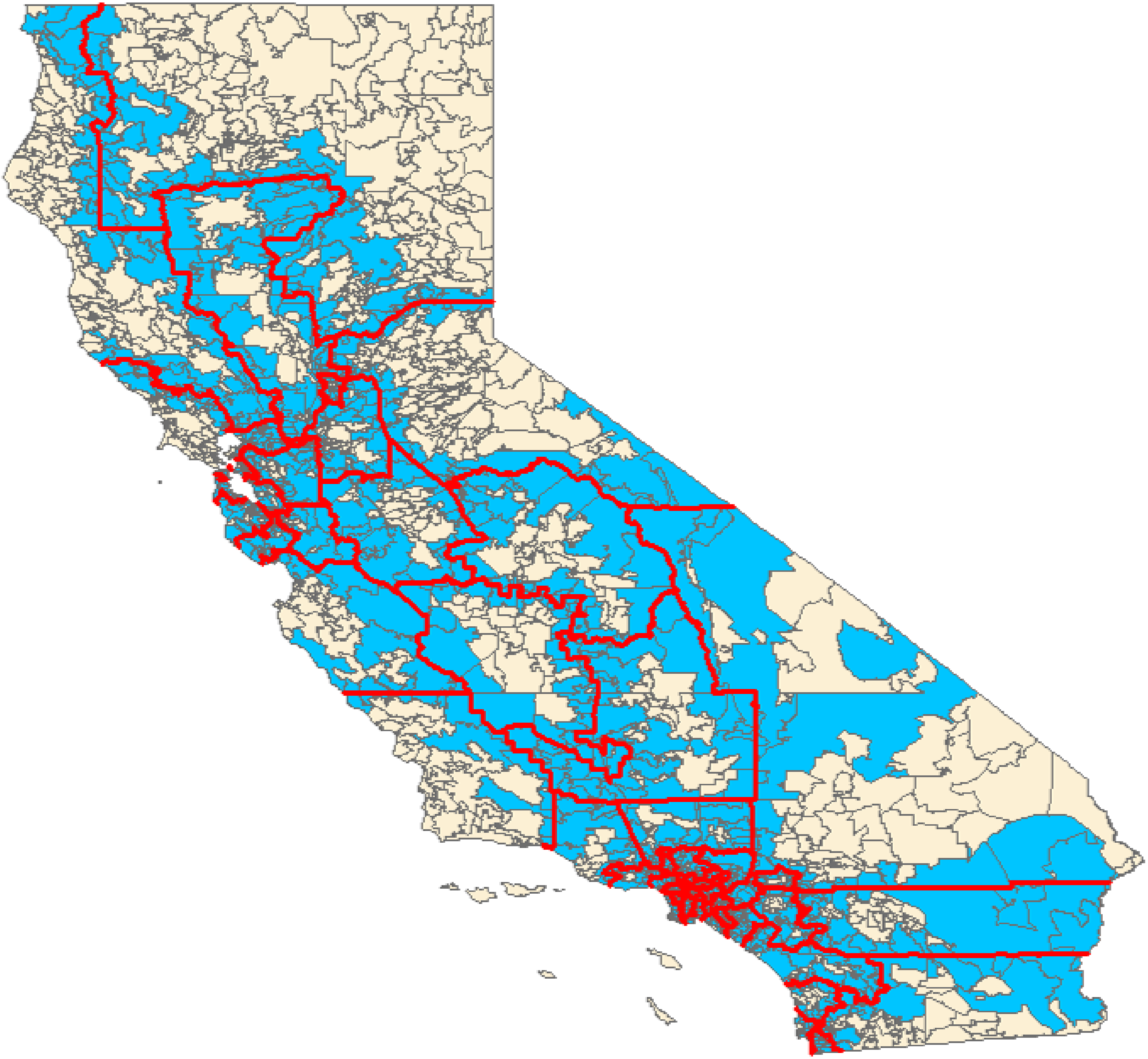
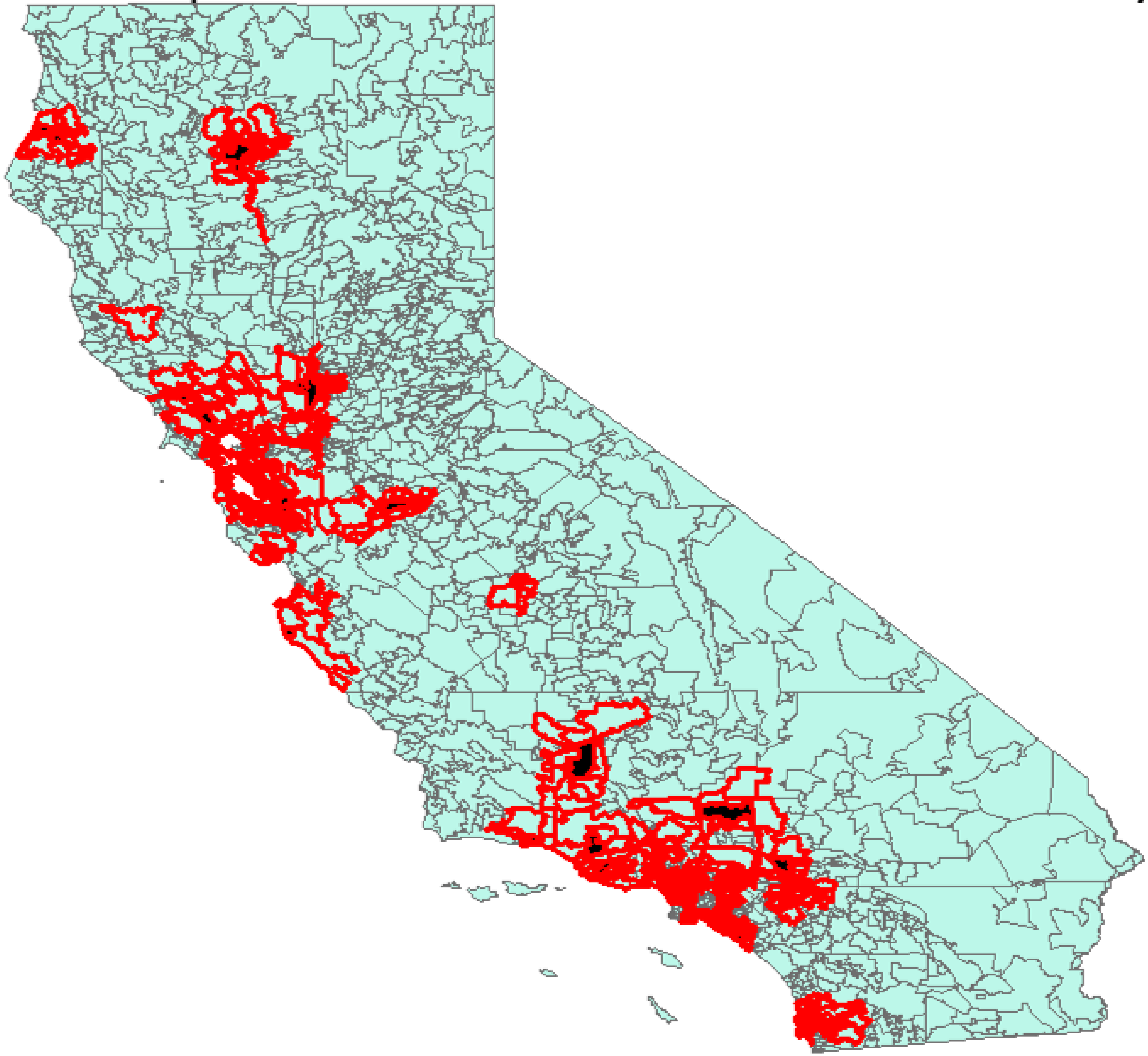


Figure VII: Zip Codes Wtihin 2 Miles of a 4 Year University.



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