

The impact of ballot access restrictions on electoral competition: Evidence from a natural experiment

Abstract

Measuring the effect of ballot access restrictions on electoral competition is complicated because the stringency of ballot access regulations cannot be treated as being exogenous to candidates' entry decisions. This paper exploits the 1968 U.S. Supreme Court decision to strike down Ohio's ballot access law as a natural experiment to overcome the endogeneity problem. The evidence from difference-in-difference estimations suggests that the court decision and the accompanying sharp decrease in Ohio's petition requirements resulted in major parties facing a significant increase in competition from third party and independent candidates.

Keywords: Ballot access, petition requirements, electoral competition, natural experiment

JEL Classification: D72, D78

1 Introduction

While it is widely accepted that competition is a salient prerequisite for the efficient functioning of markets for goods and services, the precise role of competition in politics is less evident. Numerous authors like Becker (1983, 1985) and Wittman (1989) have argued that political competition enhances the efficiency of democracies. This view contrasts with the work of Demsetz (1982) and others which stresses potential drawbacks of electoral competition. Lizzeri and Persico (2005), for instance, argue that more competition forces parties to focus on the interests of a narrower constituency thereby strengthening

the influence of special interests.¹ The empirical evidence, however, indicates that political competition is indeed important for mitigating agency problems that are prevalent in politics. Glenn and Choi (2006), for instance, show that congressional legislators tend to vote more in line with their own preferences rather than those of their constituency when barriers to competition are raised. Besley et al. (2005) provide evidence suggesting that a considerable part of economic growth in the U.S. can be explained by variation in political competition. According to their interpretation, political competition improves the quality of candidates, thereby fostering economic growth.²

Our investigation starts from the idea that similar to incumbent firms in oligopolistic markets, incumbent political parties might reduce the effective degree of competition by deterring other parties from participating in political contests. Yet, despite the long-lasting normative debate on the role of political competition, the empirical evidence regarding the effectiveness of such ‘endogenous’ restrictions to political competition is still very limited. To shed light on this issue, we estimate the effect of ballot access requirements on the degree of electoral competition in U.S. House elections as measured by the number of minor party and independent candidates.

Ballot access laws specify the conditions potential candidates need to fulfill in order to be listed on the ballot. In the U.S., these restrictions greatly differ

¹Stigler (1972) provides a more general discussion of the analogies between market competition and political competition.

²Stratmann (2005) discusses a similar argument.

between major and minor parties. For major party candidates, the most common route to get a candidate on the ballot is a primary election. Minor parties and independent candidates, however, commonly need to file a petition signed by a certain number of eligible voters.³ When measuring the effectiveness of such ballot access rules, it needs to be taken into account that the state laws specifying the requirements for minor party and independent candidates are subject to changes by the state legislatures. Moreover, with the state legislatures being dominated by the two major parties, it is likely that the design of the respective state laws reflects the major parties' self-interest to limit competition by minor parties and independent candidates. Consequently, a strong demand for participation in elections by potential third-party and independent candidates might trigger more restrictive ballot access requirements. Of course, if ballot access restrictions are effective in reducing the number of third-party candidates, once these restrictions are in place we will observe low levels of competition. In contrast, if the established parties do not face any competition by candidates of other parties, there is no need to implement particularly restrictive ballot access rules. In such a situation, the stringency of ballot access is endogenously determined, and simple regressions to determine the effectiveness of ballot access requirements in terms of reducing the actual degree of political competition are generally prone to bias. In our example,

³Despite the fact that a majoritarian voting system as in the U.S. promotes the existence of two dominating political parties (Duverger, 1964), third party as well as independent candidates frequently appear on the ballot in state as well as federal elections. During the period considered in our analysis (1952 to 1984), around 17.2% of the races in U.S. House elections saw three candidates, and an additional 8.7% had four or more candidates.

we would observe low levels of competition everywhere, but some states with more and others with less restrictive laws. We may then falsely conclude that ballot access requirements do not affect the degree of political competition. By recognizing that ballot access regulations cannot, in general, be treated as being exogenous to candidates' entry decisions, our study relates to recent work on endogenous political institutions by Aghion et al. (2004) and Trebbi et al. (2007).

To overcome the endogeneity problem, this study exploits variation in ballot access rules from a natural experiment. In particular, we make use of the U.S. Supreme Court's decision in *Williams v. Rhodes* from 1968 which struck down the highly stringent ballot access law the state of Ohio had enacted in 1951. The resulting sharp decrease in Ohio's signature requirements provides us with variation in ballot access that comes from an arguably exogenous source. Exploiting this exogenous variation allows us to identify the impact of ballot access restrictions on the entry decisions of third party and independent candidates. We estimate the effectiveness of petition requirements as a measure of the stringency of ballot access with data for U.S. House elections from 1952 to 1984. Using electoral districts in other states with stable ballot access regulations as a control group, we show by means of difference-in-difference estimations that the Supreme Court decision of 1968 resulted in a significant increase in the number of minor party candidates.

The magnitude of our estimates can be interpreted quite generally. Between

1951 and 1968 the petition requirements for third party and independent candidates in Ohio were practically insurmountable. Due to the Supreme Court decision, Ohio had to reduce its signature requirements to a level comparable to the moderate regulations existent in most other states. Thus, our difference-in-difference estimations quantify the potential increase in electoral competition if ballot access restrictions were lowered to moderate levels in the remaining states which practically exclude minor party candidates until today.⁴

Our work is related to a number of earlier contributions. Abramson and Aldrich (1995) and Rosenstone et al. (1996), for example, study the relevance of third party and independent candidates in the history of U.S. presidential elections. Amorim Neto and Cox (1997) discuss the determinants of the number of parties in general and explain it as an outcome of the interaction between the electoral system and the heterogeneity of the society. One of the first contributions discussing entry barriers in politics and the role of rewards of office to foster electoral competition is provided by Tullock (1965). Moreover, there are a few empirical studies analyzing the effectiveness of ballot access laws in deterring minor party candidates. Ansolabehere and Gerber (1996) find that higher filing fees increase the frequency of uncontested races and decrease the frequency of retirements when examining congressional elec-

⁴Georgia, for instance, still requires minor party congressional candidates to file petitions, separately for each congressional district, signed by 5 percent of registered voters eligible to vote in the last election. In order to place its candidates on the ballot in all Georgia districts, a new party would thus have to collect almost 200,000 signatures.

tions from 1984 to 1990. In the same vein, Stratmann (2005) examines the effect of filing fees and signature requirements on the number of candidates in U.S. Lower House elections at the state level in 1998 and 2000. His findings suggest that higher filing fees reduce both the number of major party and minor party candidates. In contrast to Ansolabehere and Gerber (1996) and Stratmann (2005) who do not discuss the problem of endogenous determination of ballot access restrictions and thus rely on the assumption that ballot access is exogenously given, we explicitly address the relevant state laws as being endogenous to the degree of electoral competition. Besides Aghion et al. (2004) and Trebbi et al. (2007), only a few papers explicitly address the potential endogeneity of political institutions. In a companion paper to this study, we demonstrate that ballot access rules in the U.S. should generally be treated as being endogenously determined (Drometer and Rincke, 2008). In fact, the analysis shows that state ballot access laws in the U.S. between 1946 and 1976 have been systematically tightened in response to stronger political competition. In related work dealing with the North Carolina elections in 1992, Hamilton and Lado (1996) find that the design of the ballot may have been chosen strategically by county election boards dominated by the members of one party.

The remainder of the paper is organized as follows. Section 2 provides the historical background of Ohio's ballot access laws. The empirical approach and the data are discussed in Section 3. Thereafter, Section 4 presents our

results, and Section 5 concludes.

2 The history of ballot access in Ohio

The establishment of election rules was left to the states in the U.S. Constitution of 1788 since its framers did not agree on a unitary election law for the new federal government.⁵ Access to the ballot remained entirely unregulated during much of the 19th century, and the candidates themselves were allowed to print and distribute ballot papers. As these practices resulted in polling irregularities and discouraged independent candidates due to the immense costs of providing their own ballots, the state governments in the 1890s gradually adopted the Australian Ballot, prescribing that ballots were to be printed and distributed by the state government (Argersinger, 1980). Consequently, an official nomination procedure for potential candidates had to be established. These ballot access requirements became increasingly more complex and more stringent in the course of the 20th century. Since ballot access requirements are set by the state legislators, the demands vary considerably both in absolute and relative terms.

The state of Ohio introduced a particularly restrictive ballot access law in 1951. Interestingly, the circumstances of its introduction seem to substantiate the notion that the stringency of entry barriers to political markets can, in

⁵See Bott (1990) for a brief history of ballot access in the United States.

general, not be regarded as exogenously given. Before 1951, Ohio required a third-party candidate to collect the signatures of only one percent of the registered voters, and third-party candidates frequently appeared on the ballot, both in federal and in state elections. In the 1948 presidential election, Henry Wallace from the Progressive Party polled 1.3 percent of the votes, leaving the Republican candidate Harry Truman a margin of only about 7,000 votes to win the state against the Democratic candidate Thomas Dewey. To rule out the possibility that third candidates prevent a clear victory of one of the major parties, the Ohio legislature adopted a new ballot access law in 1951 which practically excluded any third party candidates (Bott, 1990). To be recognized as a political party, Ohio demanded the submission of a petition signed by 15 percent of the last gubernatorial vote, an independent candidate in a state-wide election needed a petition signed by 7 percent of the last gubernatorial vote, and independent presidential candidates were not permitted. As a consequence, only the two major parties, which had to satisfy less strict requirements, were present in Ohio's political arena in the following years. Unsatisfied with their situation, several minor parties challenged the Ohio ballot access laws by appealing to the U.S. Supreme Court. In October 1968 the Supreme Court ruled in *Williams v. Rhodes* that Ohio's regulations violated the 1st and 14th Amendments of the Constitution. In the *Williams* opinion, Justice Black observed that '[t]he state of Ohio in a series of election laws has made it virtually impossible for a new political party, even though it

has hundreds of thousands of member, to be placed on the state ballot [...].⁶

Forced to reduce the barriers to entry for non-major party candidates, in October 1969 the Ohio legislature decided to lower the signature requirements for new parties to 7 percent and for independent candidates to 4 percent of the last gubernatorial vote. Yet even this softened regulation was declared unconstitutional by a three-judge U.S. District Court in July, 1970. Hence, the Ohio legislature took action again and adopted a new law (effective since March 1972) which reduced the requirements to 5,000 signatures for an office voted on statewide (including President) and one percent of the last gubernatorial vote in the respective district for congressional elections.

3 Estimation approach and data

The main purpose of our paper is to provide evidence of the effect of ballot access restrictions on electoral competition. As mentioned in the introduction, the identification of this effect is complicated by the apparent endogeneity of any regulation defining barriers to the entry of new political parties. We present a straightforward approach to solve the identification problem which exploits the Supreme Court decision of 1968 and the resulting significant reduction of ballot access requirements in Ohio as a natural experiment. To qualify as a natural experiment the units of observation need to be affected

⁶Cited in Bott (1990, p. 176).

by a sharp and unexpected change in some key variable of interest (in our case, the restrictiveness of petition requirements for minor party and independent candidates). It is undisputable that the change in Ohio's ballot access laws resulting from the ruling in *Williams v. Rhodes* qualifies as such an event. The state moved from being by far the most restrictive state in terms of petition requirements to a position that was very similar to the rules adopted in most other states. For a number of reasons the Supreme Court decision in *Williams v. Rhodes* can also be regarded as an unexpected event. First of all, when the Supreme Court struck down the law it had already been in place for 17 years. Secondly, *Williams v. Rhodes* was the first ruling against existing ballot access laws in the history of the Supreme Court. Note furthermore that in *Jenness v. Fortson*, a case showing remarkable similarities to *Williams v. Rhodes*, the Court upheld the ballot access law of the state of Georgia in 1971.⁷ Likewise, in a number of court cases in later years, existing ballot access regulations were confirmed.⁸ Thus, the behavior of the Supreme Court was not characterized by a general tendency to rule against pronouncedly restrictive state ballot access laws. Rather, *Williams v. Rhodes* constitutes a unique and unprecedented case in the history of U.S. ballot access laws.

Our estimation approach aims at comparing electoral competition in congressional districts in Ohio between 1952 and 1984 to electoral competition in a control group of congressional districts in other states. The key point about

⁷Winger (2002) provides a detailed discussion of the Supreme Court's ruling in *Jenness v. Fortson*.

⁸See e.g. Winger (2002) and Bott (1990).

the control-group districts is that *Williams v. Rhodes* did only affect signature requirements in Ohio and not in other states. Hence, comparing the change in the number of third-party and independent candidates in districts in Ohio to the change in districts from other states should identify the true effect of *Williams v. Rhodes* on electoral competition even in the presence of changes in political participation common to all states. Technically, we exploit the Supreme Court decision of 1968 by running difference-in-difference estimations. Our baseline estimation equation takes the form

$$\#MINOR_{it} = \alpha + \beta OHIO_i \times 1970-84_t + \gamma OHIO_i + \tau_t + e_{it}, \quad (1)$$

where $\#MINOR_{it}$ is the total number of minor party and independent candidates that were listed on the ballot in district i in year t , α is a constant, and $OHIO_i \times 1970-84_t$ is the interaction effect of a Ohio state dummy and a dummy for years after 1968, i.e. after the Supreme Court decision on Ohio's ballot access law.⁹ Moreover, τ_t is a year effect and e_{it} is a residual. The coefficient β captures the differential effect of the change in Ohio's ballot access requirements. Note that in some estimations we do not include year effects, but just a single post-shock indicator for years after 1968.

A straightforward alternative to Equation (1) is to allow for a separate inter-

⁹We treat the year 1968 as belonging to the pre-shock period because presumably there was too little time for most potential new candidates to prepare and run a campaign. We present robustness checks for this assignment later on, suggesting that assigning the year 1968 to the post-shock period or dropping observations from 1968 does not affect any of our main results.

action effects for each post-shock year,

$$\begin{aligned} \#MINOR_{it} = & \alpha + \beta_{70} OHIO_i \times 1970_t + \beta_{72} OHIO_i \times 1972_t + \dots \\ & + \beta_{84} OHIO_i \times 1984_t + \gamma OHIO_i + \tau_t + e_{it}, \end{aligned} \quad (2)$$

where $OHIO_i \times 1970_t, \dots, OHIO_i \times 1984_t$ denote interactions of the Ohio state dummy and indicators for observations from 1970, \dots , 1984. Equation (2) is more flexible, as it accounts for different treatment effects across years. We also estimate specifications including a trend specific to congressional districts in Ohio to account for a possible trend in electoral competition in this state. This is potentially important as the estimates of the treatment effects might otherwise pick up an independent state-specific trend, leading to false conclusions regarding the effectiveness of petition requirements in preventing electoral competition.¹⁰ Regarding the residuals in both estimation equations suggested above, note that while we would like to account for unobserved congressional district effects, this is not generally feasible. The reason is that, due to redistricting, the districts change considerably (at least) every 10 years according to the update of population figures by the decennial census. For instance, the districts in the 1972 congressional election do not coincide in general with the districts in the 1968 or 1970 election. We do include, however, a separate indicator for districts in Ohio. Together with the constant, this dummy variable will account for all time-constant characteristics shared

¹⁰Details on the specifications are reported in the tables showing the estimation results.

by districts in Ohio as opposed to control group districts. Furthermore, we allow for heteroscedasticity as well as serial correlation of the residuals by reporting fully robust standard errors in the tables below.

In our analysis, we compare the congressional districts from Ohio to those from Illinois. The latter constitute a well-suited control group because Illinois was not affected by the Supreme Court decision but is very similar to Ohio in many respects. Furthermore, Illinois did not change its ballot access requirements between 1952 and 1984.¹¹ This is important, as such changes would interfere with the differential impact of the Supreme Court decision on Ohio's congressional districts. Illinois' election law constantly demanded a number of signatures equal to a considerable 5 percent of the vote in the last election in the respective district in which a candidate was seeking access to the ballot. Thus, Illinois required a similar number of petitions compared to Ohio before 1968. As Table 1 shows, Ohio and Illinois are also very alike in many other respects. First of all, both comprise a similar number of congressional districts. Ohio has between 21 and 24 districts, while Illinois has 22 to 25. Moreover, both states are demographically and economically very much alike. During the period we consider, both have almost the same population, per capita income, educational attainment and level of urbanization. Finally, Illinois is roughly of the same size and geographically close to Ohio.

Table 2 presents summary statistics of the dependent and the key explanatory

¹¹We checked this by searching over the states' revised statutes for changes in ballot access laws for the whole period considered in our analysis.

Table 1: Characteristics of treatment and control group (means)

Variable	Year	Ohio	Illinois
Number of congressional districts	1952	23	25
	1968	24	24
	1984	21	22
Total population (1,000's)	1952	8299	8986
	1968	10462	10907
	1984	10817	11428
Per capita income	1952	5143	5775
	1968	7018	7680
	1984	8607	9628
Educational attainment	1952	0.375	0.361
	1968	0.509	0.501
	1984	0.705	0.704
Urbanization	1952	0.708	0.782
	1968	0.749	0.827
	1984	0.736	0.838

Data sources: Statistical Abstract of the United States (various years). Per capita income is measured in 1980 dollars. Educational attainment refers to share of total population 25 years and over with a high school diploma or a higher degree. Urbanization is percentage of urban population as defined by the U.S. Census.

Table 2: Summary statistics

Variable	Mean	S.D.	Min	Max
Number of third party and independent cand's	0.123	0.425	0	4
OHIO	0.488	0.500	0	1
1970-84	0.462	0.499	0	1
OHIO× 1970-84	0.227	0.419	0	1

Sample includes all electoral races for the U.S. House of Representatives from 1952 to 1984 in Ohio and Illinois (799 observations). Sources: ICPSR (1994), the Office of the Clerk of the U.S. House of Representatives, Winger (2006) and the revised state codes for the respective states and several years. OHIO is an indicator for the state of Ohio. OHIO× 1970-84 is the interaction between OHIO and an indicator for years after 1968.

variables. The data on election outcomes for U.S. House elections between 1952 and 1984 come from ICPSR (1994). These contain a record for each individual candidate, providing information on the candidate's name, party affiliation, and the number of votes received. From the party code we identified third party and independent candidates. In some rare cases the party codes are missing. Therefore, we checked the party affiliation of all candidates with missing party codes by referring to the official congressional election

statistics.¹² For our analysis we only considered third party and independent candidates that were actually listed on the ballot. Hence, we eliminated write-in candidates from our data set and ignored scattered votes. Moreover, we restrict attention to general elections. The numbers in Table 3 confirm that our sample is almost perfectly balanced between districts belonging to either treatment and control group, and observations from pre- as well as post-shock periods.

4 Results

Before turning to the outcomes of our difference-in-difference estimations, we will first provide an intuition for our results by a comparison of means of the number of third party and independent candidates before and after 1968. As Table 3 shows, the average number of third party and independent candidates on the ballot in Ohio jumped from virtually zero before 1968 to 0.44 on average after 1968. In Illinois, the increase in the number of third party and independent candidates was much less pronounced.

This difference is illustrated in more detail in Figure 1. Between 1952 and 1968, in both states the number of third party and independent candidates was close to zero. While Illinois experienced a moderate increase beginning in 1972, the average number of third party and independent candidates in

¹²These statistics are available from the office of the Clerk of the U.S. House of Representatives at http://clerk.house.gov/member_info/electionInfo/index.html.

Table 3: Average number of Congressional candidates per district

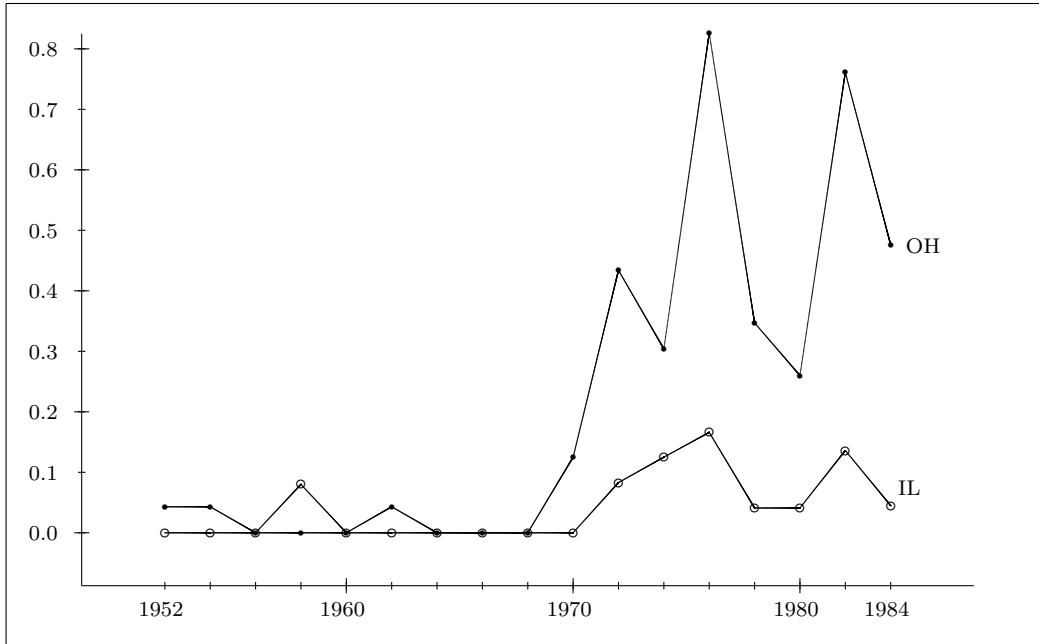
	1952-1968			1970-1984		
	Nob	Mean	S.D.	Nob	Mean	S.D.
OHIO						
Overall number of candidates	209	1.99	0.18	181	2.36	0.81
Major party candidates	209	1.98	0.14	181	1.92	0.27
Third party & independent candidates	209	0.01	0.12	181	0.44	0.75
ILLINOIS						
Overall number of candidates	221	2.00	0.12	188	2.05	0.31
Major party candidates	221	1.99	0.07	188	1.97	0.16
Third party & independent candidates	221	0.01	0.09	188	0.08	0.29

Sample: Congressional districts of Ohio and Illinois in Congressional election years from 1952 to 1984 (799 observations).

Ohio quickly soared to more than 0.8 in 1976. This number went down to values around 0.3 in 1978/80 and raised again to a value somewhat below 0.8 in 1982. In all years beginning with 1970, the number of third party and independent candidates in Ohio was higher than in Illinois. The numbers in Table 3 and the graphs in Figure 1 give a first impression of the effectiveness of Ohio’s ballot access laws before 1968. The evidence suggests that the Supreme Court decision had a strong and immediate impact on electoral competition. However, we need a more technical approach to substantiate the descriptive evidence. In particular, we would like to check the statistical significance of the effects suggested by the descriptive analysis.

We now turn to the results of our difference-in-difference estimation approach. Table 4 displays a first set of results. The dependent variable is the number of third party and independent candidates. Our units of observation are 799 electoral races for the U.S. House of Representatives in Ohio and Illinois between 1952 and 1984. Note that for all regressions we report standard errors

Figure 1: Minor party and independent candidates, 1952-1984



Graphs show, separately for Ohio and Illinois, average number of third party and independent candidates per district in U.S. House elections, 1952-1984.

that account for clustering on congressional districts. Since districts boundaries change regularly due to redistricting, we form district-specific clusters for the periods 1952-1960, 1962-1970, 1972-1980, and 1982-1984, giving a total number of 186 clusters.

Column 1 shows the results for a baseline specification of our difference-in-difference model. Besides the interaction term $\text{Ohio} \times 1970-84$, it accounts only for an Ohio state effect and an indicator for post-shock periods. The increase in the number of third party and independent candidates in Ohio (relative to congressional districts in Illinois) resulting from the Supreme Court decision is estimated to be 0.35 and is significant at the 1% level. The magnitude of the treatment effect is notable (recall that the average number of minor party

Table 4: Effectiveness of ballot access rules, difference-in-difference estimations (OH vs IL)

Dependent variable: Number of third party and independent candidates							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
OHIO	0.005 (0.012)	0.005 (0.012)	0.005 (0.012)	0.021 (0.036)	0.023 (0.040)	0.009 (0.008)	0.038 (0.039)
1970-84	0.070 *** (0.023)	-	-	-	-	0.094 ** (0.036)	-
Ohio time trend	-	-	-	-0.003 (0.005)	-0.003 (0.006)	-	-0.004 (0.004)
OHIO×1970-84	0.351 *** (0.067)	0.352 *** (0.068)	-	-	-	0.317 *** (0.085)	-
OHIO×1970	-	-	0.119 * (0.070)	0.134 * (0.071)	0.139 * (0.075)	-	0.129 * (0.069)
OHIO×1972	-	-	0.345 ** (0.162)	0.364 ** (0.163)	0.369 ** (0.165)	-	0.360 ** (0.162)
OHIO×1974	-	-	0.173 (0.194)	0.195 (0.196)	0.201 (0.198)	-	0.192 (0.195)
OHIO×1976	-	-	0.653 *** (0.199)	0.678 *** (0.201)	0.685 *** (0.204)	-	0.677 *** (0.200)
OHIO×1978	-	-	0.300 ** (0.141)	0.327 ** (0.145)	0.335 ** (0.150)	-	-
OHIO×1980	-	-	0.213 (0.135)	0.243 * (0.141)	0.252 * (0.147)	-	-
OHIO×1982	-	-	0.620 *** (0.207)	0.653 *** (0.212)	0.662 *** (0.216)	-	-
OHIO×1984	-	-	0.425 ** (0.169)	0.461 *** (0.177)	0.471 ** (0.183)	-	-
Adjusted R^2	0.159	0.191	0.206	0.205	0.201	0.148	0.215
Years	1952-84	1952-84	1952-84	1952-84	1952-84 ^a	1960-76	1960-76
Sample size	799	799	799	799	751	427	427
Year effects	No	Yes	Yes	Yes	Yes	No	Yes

^a Observations from 1968 omitted. All regressions include a constant. Standard errors (robust to heteroscedasticity and clustering on districts) in parentheses. ‘OHIO’ is an indicator for districts located in Ohio. ‘1970-84’ is an indicator for post-shock periods, 1970-1984. Significance levels: * 10%; ** 5%; *** 1%.

candidates is only 0.12 in our sample).

In Column 2 we replace the indicator for post-shock periods by a full series of year effects. The results are very close to those obtained in Column 1. In Column 3 we split the single treatment effect employed in the first two specifications into a full series of interaction effects, giving a separate treatment effect for each post-shock year. Column 3 thus gives a much more detailed picture of the effects of the change in ballot access on the entry of third party and independent candidates. We note that the estimates for all year-specific treatment effects are positive, with six out of eight estimated coefficients being

statistically different from zero at least at the 10% level. The treatment effect is strongest in 1976, indicating that in this particular election the reduction in ballot access requirements is responsible for 0.65 additional congressional candidates in Ohio relative to districts in Illinois.

As mentioned above, the difference-in-difference approach is generally prone to bias in the presence of a treatment-group specific trend which is not properly accounted for in the model. To hedge against such a potential bias, we allow for a time trend specific to districts in Ohio. As the results reported in Column 4 show, the coefficient of the trend itself is not statistically different from zero, and the general picture regarding the treatment effects is unchanged. Note, however, that now seven out of eight year-specific treatment effects are estimated to be statistically different from zero.

So far we have presented results with observations from the year 1968, i.e. the year of the Supreme Court decision, assigned to the pre-shock period. In order to check the robustness of our findings with respect to this assignment, we excluded these observations and re-estimated the model with the full series of year-specific treatment effects and including the Ohio time trend. The results, displayed in Column 5, are almost identical to those obtained with observations from 1968 assigned to the pre-shock period. We conclude that the assignment of these observations does not critically affect our results.

A well-known further criticism of difference-in-difference estimations is that with many time periods in the sample and significantly serially correlated

observations, the approach may overstate the true effects. To account for this objection, we re-estimated both the baseline specification (Column 6) and the model with a full series of year effects, the Ohio time trend and year-specific treatment effects (Column 7) using only observations from the period 1960-1976. This reduces the number of observations to 427. Again, our results prove to be highly robust. In particular, the coefficient estimates of the year-specific treatment effects in Column 7 are very close to their counterparts in Column 5.

The bottom line from the series of difference-in-difference estimations reported in Table 4 is that the preliminary findings from the descriptive analysis are confirmed. In particular, we note that the strong effect of the Supreme Court decision on electoral competition is statistically significant and robust to various and substantial changes in specification.

A potential objection to our identification approach might be the low variation in the number of third party and independent candidates in Illinois. Based on the descriptive evidence reported in Table 3 and Figure 1, one could argue that our approach comes close to comparing the variation in the number of candidates in the treatment districts to a variable which is constant over time in most districts belonging to the control group. In that case the difference-in-difference approach could be misleading, as the variation in electoral competition in Illinois might simply be suppressed by the very special circumstances of this particular state, namely its highly restrictive ballot ac-

Table 5: Average number of Congressional candidates per district (extended control group)

	1952-1968			1970-1984		
	Nob	Mean	S.D.	Nob	Mean	S.D.
IL, IN, KY & NJ						
Overall number of candidates	518	2.35	0.83	400	2.53	0.99
Major party candidates	518	1.96	0.17	400	1.98	0.15
Third party & independent candidates	518	0.38	0.80	400	0.55	0.98

Sample includes electoral races for the U.S. House of Representatives from Ohio (1952-1984), Illinois (1952-1984), Indiana (1952-1980), Kentucky (1952-1976) and New Jersey (1952-1984).

cess law. To address this point, we extend our analysis by expanding the control group by congressional districts from three additional states: Indiana, Kentucky and New Jersey. We selected these states based on two criteria: Firstly, we can only make use of districts from states that did not change their ballot access laws during at least a substantial part of the time period under consideration. Secondly, in order to induce more substantial variation in electoral competition within the control group, the ballot access laws of the additional states should be significantly less restrictive than those in Illinois.

Table 5 displays descriptive statistics of electoral competition as measured by the number of minor party candidates within the extended control group. A quick inspection reveals that the number of third party and independent candidates now exhibits substantial variation both before and after 1968. Difference-in-difference estimations based on the extended sample should therefore provide us with a valid point of reference for the results discussed above. Note that we cannot employ all observations in our estimations since Indiana and Kentucky altered their ballot access rules towards the end of the period considered. We therefore restrict attention to the period from 1952 to 1976.

Table 6: Difference-in-difference estimations (OH vs IL, IN, KY, NJ)

Dependent variable: Number of third party and independent candidates				
	(1)	(2)	(3)	(4)
OHIO	-1.21 *** (0.105)	-1.21 *** (0.105)	-1.21 *** (0.105)	-1.24 *** (0.138)
ILLINOIS	-1.22 *** (0.106)	-1.22 *** (0.105)	-1.22 *** (0.105)	-1.22 *** (0.106)
INDIANA	-0.951 *** (0.120)	-0.952 *** (0.118)	-0.952 *** (0.118)	-0.952 *** (0.118)
KENTUCKY	-1.05 *** (0.118)	-1.05 *** (0.116)	-1.05 *** (0.117)	-1.06 *** (0.117)
1970-1976	0.071 (0.063)	-	-	-
Ohio time trend	-	-	-	0.007 (0.013)
OHIO×1970-76	0.318 *** (0.105)	0.321 *** (0.102)	-	-
OHIO×1970	-	-	0.317 *** (0.099)	0.284 *** (0.110)
OHIO×1972	-	-	0.486 *** (0.177)	0.446 ** (0.188)
OHIO×1974	-	-	0.058 (0.217)	0.011 (0.229)
OHIO×1976	-	-	0.421 ** (0.213)	0.368 (0.231)
Adjusted R^2	0.345	0.371	0.372	0.371
Years	1952-76	1952-76	1952-76	1952-76
Nob	1048	1048	1048	1048
Year effects	No	Yes	Yes	Yes

All regressions include a constant. Standard errors (robust to heteroscedasticity and clustering on districts) in parentheses. ‘OHIO’, ‘ILLINOIS’, ‘INDIANA’ and ‘KENTUCKY’ are indicators for districts located in the respective state. ‘1970-76’ is an indicator for post-shock periods, 1970-1976. Significance levels: ** 5%; *** 1%.

The results based on the extended sample are reported in Table 6. Column 1 again shows the baseline specification including a full series of state dummies, while Column 2 repeats the estimation with a full series of year effects instead of a single indicator for post-shock-periods. We obtain highly significant estimates of the treatment effect in both cases, with slightly lower point estimates compared to the estimations with only districts from Illinois forming the control group. Following the example of the results presented in Table 4, Column 3 replaces the single treatment effect by year specific interactions. Again, we find all estimated parameters to be positive, and three out of four effects are significant at least at the 5% level. Finally, Column 4 demonstrates

that adding a time trend specific to districts in Ohio does nothing to our main results.

We conclude that adding congressional districts from three additional states to extend the control group and to induce higher variation in electoral competition within this subsample confirms the results derived from estimations where the control group comprises only districts from Illinois.

5 Conclusion

This paper investigates the effect of barriers to entry on electoral competition. In particular, we provide evidence on the impact of ballot access restrictions on entry decisions of third party and independent candidates. We note that earlier studies have failed to take account of the likely endogeneity of ballot access requirements. Building on recent contributions by Aghion et al. (2004) and Trebbi et al. (2007) claiming that political institutions should generally be treated as endogenous, we suggest an identification approach that exploits exogenous variation in the stringency of ballot access requirements. The identification rests on the idea to utilize the U.S. Supreme Court decision of 1968 to strike down Ohio's ballot access law and the resulting sharp decrease in signature requirements for third party and independent candidates in Ohio as a natural experiment. By means of difference-in-difference estimations using observations from electoral races in U.S. House elections from 1952 to 1984,

we identify the effectiveness of ballot access restrictions in terms of reduced electoral competition.

Our results indicate that ballot access requirements as used in most U.S. states can be highly effective in reducing the degree of electoral competition faced by major party candidates. This finding, in turn, suggests to take serious the potential endogeneity of political institutions in empirical applications, in particular in cases where it is plausible to assume that stakes of powerful players in the political arena are directly affected by the design of the institutional environment. Hence, the empirical evidence presented in this study strongly suggests to understand political institutions as being subject to strategic choice of influential actors such as governments and legislatures.

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