Political Entrepreneurs and Electoral Realignments: Individual Agency in Politics Revisited

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Abstract

The current focus of political science on characterizing equilibria has at times come at the expense of understanding the drivers of political change. But political change is not only relatively frequent, it is also often driven by the capacity of lone individuals to affect deep changes in the central dimensions of political competition. This paper identifies numerous cases of electoral realignments induced by these individual political entrepreneurs in US states over the past 150 years. It does so using a novel empirical strategy with the potential for more general applications. Furthermore, the paper shows that while it may be impossible to fully explain the idiosyncratic timing of the appearance of individual entrepreneurs, as a population, there is evidence that entrepreneurs respond to institutional features that affect the ease of political entry. Using a difference-in-difference empirical strategy that takes advantage of variation in the introduction of primaries at the state level, this paper shows that the introduction of primaries in the United States has lead to an increase in the emergence of political entrepreneurs using both actual and instrumented primary implementation dates.

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Introduction

> *Our knowledge of structure-induced stability has come at the expense of understanding the very struggles and conflicts at the center of political life. [...] the focus on institutions as instruments of stability does not provide many tools for us to grapple with the problem of change. Yet casual political observation confirms that we live in a world of patterned interactions, but one that is also dynamic and subject to periodic events such as new social movements, unexpected electoral outcomes, or dramatic policy initiatives that institutional approaches predicated on stability cannot easily account for.* (Sheingate, 2003)

Few topics have been as understudied in the political science literature as political entrepreneurs. At best, political scientists view political entrepreneurs as an idiosyncratic force best modeled as an exogenous shock, while a worst, most research simply assumes entrepreneurs to be a constant but benign force in politics, continually and quietly arbitraging away small disequilibria. This paper challenges both of these conceptions. First, it shows that while individual entrepreneurs may appear idiosyncratic, as a population, their emergence is systematically shaped by institutional features that create electoral barriers to entry. Second, this paper shows that while entrepreneurs are far from rare, they are hardly a pervasive presence either. Rather from time to time they are responsible for significant and discrete political realignments, creating a pattern of punctuated equilibria in which long periods of stability are occasionally upset by significant changes in voting behavior. And finally, the pattern of these realignments – which are uncorrelated in time across states – suggests that they are not "mechanical" responses to national structural changes but rather the result of significant individual agency.

These conclusions are reached using a novel empirical strategy introduced in this paper for identifying electoral realignments *induced by individual entrepreneurs* that occur at the state level. This strategy represents a major leap forward in the study of political entrepreneurs, who until now have escaped systematic study in large part because of the difficulty of measuring their influence. The measure of entrepreneurs presented here allows for what is – to the best of this author's knowledge – the first systematic study of the emergence of political entrepreneurs across a long period of time and in a set of heterogeneous environments.

Before introducing this measure, it is important to be precise about definitions, as the term "political entrepreneur" evokes very different ideas to different people. In this paper, a political entrepreneur is defined as one who induces electoral realignments. This definition owes much to Riker (1986)'s heresthetic leader, a politician who changes the dimension of political competition to induce realignments. It is broader than this classic definition, however. In addition to capturing politicians who change the dimension of competition (for example from urban-rural issues to

religious issues), it also encapsulates agents of the type characterized by Wilson (1995) who are able to overcome collective action problems and form electoral coalitions that may not have been previously realized (such as the coalition of economic and religious conservatives that make up the current day Republican party). This paper will touch on the question of *how* entrepreneurs induce realignments, but for the purposes of this paper an entrepreneur is defined by what she accomplishes, not by the tools she employs. A systematic study of how entrepreneurs induce realignments is best deferred to future work.

There are two steps to identifying political entrepreneurs systematically. First, elections must be identified in which significant realignments take place. And second, this list must be reduced to elections in which the realignment can be attributed to the agency of an individual campaign. This second step is crucial. The aim of this paper is to identify situations in which individuals exhibit agency and are directly responsible for an electoral realignment, so cases where politicians are simply riding a realignment caused by a structural economic or social shift must be excluded.

This first task – of identifying changes in electoral coalitions – is accomplished by measuring the inter-election correlation (IEC) in county-level Democratic party vote shares from one election to another for state-level electoral offices. Provided there is some degree of heterogeneity across counties, low IECs are indicative of shifts in the types of people voting for the democratic party (i.e. an electoral realignment).

To illustrate the intuition of this model, consider two gubernatorial elections in California, one in 1980 and one in 1984. Assume that in the 1980 election, the primary dimension of political competition is urban-versus-rural issues, with the Democratic candidate being favored by urban voters. In this election, we would expect urban counties to have relatively high Democratic vote shares, and rural counties to have relatively low Democratic vote shares. Now imagine the gubernatorial candidate in 1984 manages to shift the primary dimension of competition to immigration policy, with the Democratic candidate being favored by communities with more immigrants and farmers who employ immigrants voters. In 1984 the Democratic candidate would see an increase in support from rural communities, would gain from southern urban centers, and lose support in urban centers. As a result, the IEC across counties would be low as compared to correlations between previous inter-year comparisons where the dimension upon which candidates were competing remained the same.

This measure has three very appealing properties. First, it is robust to differences in candidate valence. Suppose that the Democratic candidates in 1980 and 1984 in our California example above both drew support from urban centers, but the candidate in 1984 was a much more impressive candidate. This would result in an across the board increase in county-level Democratic vote shares, but should not change the fact that urban counties have higher Democratic vote shares than rural counties, leading to high IECs.

Second, it is agnostic about the nature of a realignment. Many studies of electoral realignments rely on voter surveys in which voters are asked about their views on issues, and these views are compared to voting behavior. But if surveyors fail to ask about something like preferences over mining rights, and a gubernatorial candidate is able to make that a salient election issue, a potential realignment may be overlooked.

Finally, and perhaps most importantly, as this measure is not reliant on voter surveys it is uniquely suited for historical analysis of electoral realignments. Realignments are relatively rare, so they can only be properly studied using long time series. But because voter surveys are a relatively novel innovation, traditional survey methods cannot be used for the study of historic realignments, especially at the sub-national level where data is particularly scarce. The measure employed by this paper has no such limitation, however, making it possible to quantitatively study all US state level elections going back 150 years.

It is not enough to identify inter-election realignments if we wish to study political entrepreneurs, however; it is also necessary to identify realignments that have been *actively induced* by a particular campaign. To this end, this paper employs a difference-in-difference approach to identify cases in which electoral coalitions only change for one of several simultaneous state-level races. In other words, it compares the county-level IECs for multiple state races – namely gubernatorial, presidential, and senatorial races – and looks for cases where only one race shows signs of significant realignment. Cases where realignments are experienced by all campaigns are excluded by this analysis, as these types of "global" realignments are more likely to be induced by structural changes than individual political entrepreneurs. Only cases where a senator and the president are elected by the same coalition that supported them in the previous election, but the governor is elected by a substantially different coalition than his or her predecessor are highlighted.

This difference-in-difference strategy also has the advantage of implicitly controlling for things like inter-county migration, which could potentially cause IECs to fall despite the same types of people voting for each party. Inter-county migration should impact all races equally, thus dropping out in a difference-in-difference analysis.

This measure diverges significantly from the single other attempt to systematically characterize political entrepreneurs in the political science literature – Schneider and Teske (1992). That paper attempts to identify political entrepreneurs by sending questionnaires to out to county clerks in 1400 suburban communities and asking those clerks whether any individuals' "policy proposals and political positions represented a dynamic change from existing procedures." It finds that entrepreneurs are more common in cities with larger tax bases, larger discretionary shares of budgets (both of which the authors argue offer potential for policy "profits" that might induce entrepreneurs), or a greater share of citizens who own their homes (which they argue facilitates collective action (Cox, 1982)). But this innovative attempt to study political entrepreneurs systematically did not spark a

new literature on political entrepreneurs, due perhaps to the lack of time series variation that could be leveraged for causal identification or uncertainty about how clerks interpreted questionnaires when identifying entrepreneurs.

Having now introduced the measure that will be employed in this paper, it is worth pausing to consider what exactly it does and does not capture. First, it is a highly conservative measure in that it limits our attention to individuals who organize novel, cross-cutting coalitions for themselves *but not other contemporaneous campaigns*. In the interest of clean identification, it overlooks anyone who induces a realignment that propagates to all campaigns in a given election. Moreover, because of its dependency on inter-county heterogeneity for identification, in cases of relatively homogeneous counties this measure is likely to systematically underestimate the presence of political entrepreneurs.

Second, while this measure can identify *campaigns* that induce realignments, it cannot identify the individual entrepreneur. Namely, it is unclear whether a realignment is being caused by the winning candidate, a member of his or her staff, or someone on the losing side of the campaign. It can only identify *races* that result in electoral realignments.

And third, it is a measure of the prevalence of *successful* entrepreneurs. As in the economics literature, the assumption of this paper is that entrepreneurs are always present, seeking out disequilibria on which to capitalize. It is only in *some* instances that entrepreneurs are successful – those identified by this measure.

These three limitations make it difficult for these measures to be used to study the characteristics of individual entrepreneurs, but have no impact on the study of how various institutional rules and structures affect the prevalence of successful entrepreneurs, which has not previously been possible. In this sense, this paper embraces the view of Sheingate (2003), who argues:

> Although one might be tempted to examine entrepreneurs themselves, it would be a mistake to divert our attention from the structure of institutions to the personal qualities or characteristics of an individual, for this would limit the utility of the concept to the study of great men. Rather, attention to the institutional factors that contribute to entrepreneurial success and failure may offer new insights into the structure of institutions, sharpen our understanding of the mechanisms of institutional change, and perhaps even suggest how entrepreneurship can address larger questions of American political development.

In that spirit, the focus of this paper is not on entrepreneurs as individuals, but entrepreneurs as a population. When and where do they arise? How is the likelihood of their emergence shaped by electoral rules? And to the degree the data supports conclusions on the topic, are these entrepreneurs exploiting disequilibria, or inducing disequilibria of their own? Albeit preliminary, the

answers this paper provide to these questions are a major first step in answer the call for the systematic study of political agency that have rung out for decades by Colomer (2005); Mair (1999); Przeworski and Sprague (1986); Sartori (1969); Urwin (1973); Zuckerman (1975), among others. This piece is certainly not the last word on the subject, but hopefully it will bring new insight to our understanding of political change and agency, a subject Evans (2010) argues in a recent review article on research into political cleavages is something "we need most urgently to address."

Although this empirical strategy does not require the type of voter opinion survey employed in most cleavage current studies, it does have some restrictive data requirements. In particular it requires electoral returns (a) reported at a sub-constituency level (b) by units that are relatively comparable across time (c) for sequential elections (d) in which multiple offices are contested simultaneously. Moreover, in order to ease analysis the elections should also be (e) first-past-the-post,[1] and (f) be available for a long enough period (due to the relative perceived rarity of electoral realignments). Given that elections generally occur once every two or four years, this final constraint is particularly restrictive.

With that in mind, this paper restricts its attention to electoral contests at the state level in the United States. National races do not provide multiple elections which take place at the same time, and moving to sub-state units (namely Congressional districts) is problematic both due to data constraints and the changing nature of district boundaries over time. In theory this measures could be applied to both Senate and Gubernatorial races, but due to time limitations this paper will focus exclusively on governors, for which more data is available (since the senate only became a publicly elected office in the early 1900s). Extending this measure to other countries is a next logical step, and data collection efforts are already underway for this endeavor.

The remainder of the paper is organized as follows: Section 1 provides a detailed discussion of the methodology employed by this paper. Section 2 provides a descriptive overview of the entrepreneurs identified by this paper – namely when and where they appear. It also provides detailed descriptions of data from a number of states in order to give readers a feel for the underlying data and types of individuals identified by this strategy. Section 3 shows that many of the realignments induced by political entrepreneurs actually propagate to other elections and offices, showing that in many cases entrepreneurs are doing more than gathering a unique coalition for themselves – they are also fundamentally changing the political playing field in their home states. And finally Section 4 takes advantage of variation in the introduction of direct primaries in the United States to show that while individual entrepreneurs may appear idiosyncratic, the probability of their emergence is shaped by institutional features like rules of electoral nomination.

[1]First-past-the-post electoral systems ease analysis dramatically by making it possible to fully characterize competition within a district by the vote share of one of the two parties that inevitably emerge thanks to Duverger's Law. Expanding this measure to proportional systems may be possible, but is beyond the scope of this analysis

1 Methodology

Data

This paper is based primarily on data collected by the Inter-University Consortium for Political and Social Research (ICPSR). County level returns for US Presidental, Senatorial, and Gubernatorial elections from 1824-1968 come from ICPSR (1999), and US Presidental, Senatorial, Gubernatorial, and in some cases other state offices (like Secretary of State, Attorney General, etc.) come from ICPSR (1995).

Estimation of Correlations

IECs are estimated in two ways: simple IECs in Democratic vote shares and Spearman correlations on the ordinal ranking of counties by Democratic vote share. Results in the paper are presented using simple IECs, but are robust to choice of measure.

One unfortunate facet of the American political system is that elections do not always occur simultaneously. To correct for this fact, in most of the following analysis difference-in-difference estimates are computed on the basis of linear interpolations of observations.[2] As with any linear interpolation, this fails to capture the possibility that a change in correlations is not occurring continuously across time. In the case of a discrete shift between elections, this interpolation could either lead to the erroneous identification of an entrepreneur (if the actual discrete shift occurs before the intervening election) or a failure to identify an entrepreneur (if the actual shift occurs after). However this does not appear to be a major driver of results, as will be shown later in this paper both by an examination of the behavior of elections preceding and following "entrepreneurial" swings, and by showing that results are robust to restricting data to simultaneous elections.

Vote Share Estimation

One other challenge in using a relatively long time series is that the modern Democrat-Republican divide was not present in the 1800s to the degree that it is present now. This fact is addressed by the paper in three ways. First, attention is focused on the Democratic vote share, given the relative stability of the Democratic party doing this period. Since all US electoral competitions are first-past-the-post, the are almost never more than two major parties competing (á la Duvreger's

[2]For example, consider a situation in which a presidential election in 1980 has a correlation of 0.8 with the last election, and the subsequent presidential election has a value of 0.2. However, because of state idiosyncrasies, a gubernatorial election occurs in 1982 at a time when no presidential election is actually taking place. To compute the difference-in-difference estimate of the IEC for the 1982 gubernatorial election, it will be assumed that had a presidential election election taken place in 1982, it would have had an IEC of 0.5 (as $\frac{(0.8+0.2)}{\frac{1984-1980}{2}} = 0.5$).

Law), and so the vote share of a single stable party is a sufficient statistic for characterizing party cleavages in most situations. Second, this analysis is restricted to post-1850 elections in which the Democratic party was relatively stable (despite the availability of data going back to 1824). And third, in cases where an easy mapping from the Democratic party or the Republican party to an antecedent party of a different name is possible, these corrections are made.

Vote shares are also calculated both as Democratic party share of total votes, and Democratic party share of two-party votes, where the Democratic vote share is divided by the sum of the vote shares for the two dominant parties (usually Democrats and Republicans). Results will be presented in terms of two-party vote shares, which are more robust to the presence of third party candidates, but all results are robust to difference choice of measures. In cases where multiple candidates from the same party run simultaneously (which occurs with some frequency prior to the introduction of primaries in the early 1900s), votes for all candidates of the same party are pooled before calculating vote shares.

Cross-State Comparisons

The underlying heterogeneity of counties within a state has a significant impact on how a given change in electoral behavior will map into changes a in state's IECs. For example, in a state where all counties have Democratic vote shares of 49% or 51%, a relatively small change in electoral coalitions may result in a full reversal of the ordering of counties, leading to an IEC of -1. However, in a state in which there is greater heterogeneity across counties – say where all counties have Democratic vote shares of 100% or 0% – it would take am much larger actual change in voting behavior to induce a full reordering of counties and an IEC of -1. Thus the magnitude of all IECs are normalized at the state level with a mean of 0 and a standard deviation of 1 when comparisons are made between states.

Corrections for Joint Estimation

The question of how to evaluate the differenced IECs produced by this strategy is not as straightforward as it might initially appear. This paper is explicitly searching for unusual elections, an approach which usually requires corrections for the fact that while "digging" for unusual observations, many are likely to emerge randomly. This would suggest the need to employ some type of correction for joint hypothesis testing, like choosing a Familywise Error Rate.

It is not clear that a frequentist paradigm is appropriate in this setting, however. ICPSR data is based on vote *totals*, which means no stochasticity is being introduced by sampling. Moreover, while it seems reasonable to assume that there is a stochastic aspect individual voting decisions (as embodied in the common use of random utility functions to model voting decisions), if we

assume that these random utility functions are subject to uncorrelated shocks, then they should cancel out in large populations. To induce meaningful stochasticity in the difference-in-difference measure employed in this paper, it would have to be the case that voter utility functions are subject to shocks that are not only positively correlated across individuals, *but also negatively correlated across campaigns*. In such a case, it is unclear how to interpret these *campaign specific* shocks except as the result of active campaigning.

With these considerations in mind, each election will be treated as a non-stochastic observation in this analysis, particularly in the informal analysis presented in Section 2.

2 Raw Results

This section provides an overview of the raw IECs generated using the method described above. This presentation is meant to serve two purposes. First, as with any new measure it is important to give readers an unvarnished feel for the data to ensure it behaves in a manner that is not radically out of line with our political intuition before reducing them to a few coefficients in a regression table – the so called "laugh test." But second, given the scarcity of *even the most basic of summary statistics* on the existence of entrepreneurs, the facts presented by this paper offer the *first systematic description* of who political entrepreneurs are, and when and where they have emerged. These summary statistics are an absolutely essential first step to understanding some of our most basic and long outstanding questions about political agency in American electoral politics.

Getting a Feel for the Data

In Figures 1- 3 below, raw IECs are presented for three different states that are representative of extremes in the overall data. Along with raw IECs for Presidential, Gubernatorial, and Senatorial races graphed on the left, difference-in-difference estimates of gubernatorial IECs are presented on the right. Difference-in-differences estimates that yield correlations *above* zero are truncated at zero in these graphs. This is because in most cases the magnitude of positive differences between gubernatorial IECs and those of other simultaneous elections says more about the other races than the gubernatorial campaign itself.

Figure 1 presents the results for Kentucky. These results indicate the remarkable inter-election stability for all but the last gubernatorial election presented. As shown in the left graph, while Kentucky did experience some realignments during the 1860s, these realignments were party wide, affecting voting for both the Governor and the President. As a result, when a difference-in-difference estimate is computed, there is little or no residual evidence of an entrepreneur, as shown in the figure on the right.

Figure 1: Kentucky: An Example of a Stable State

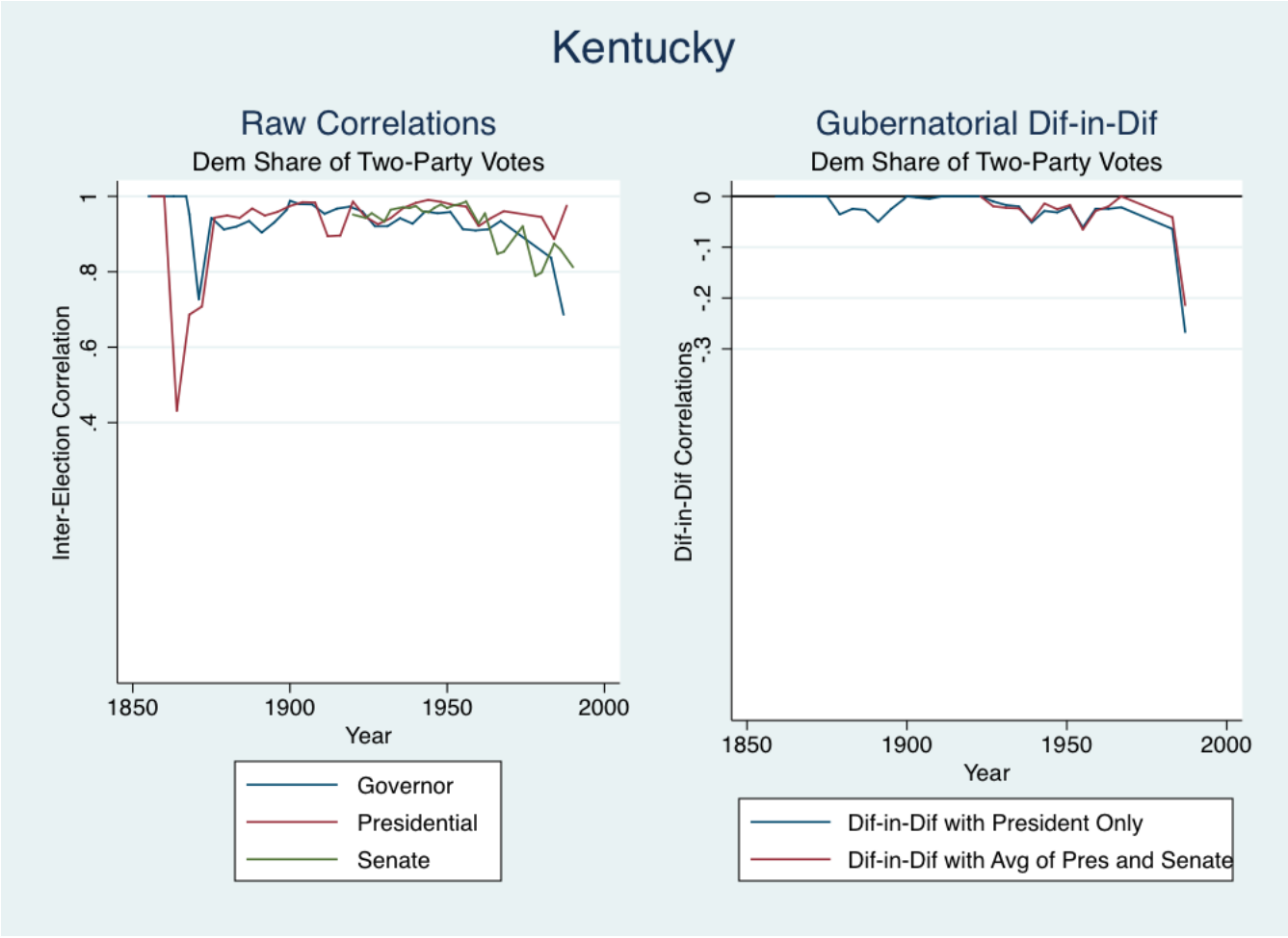
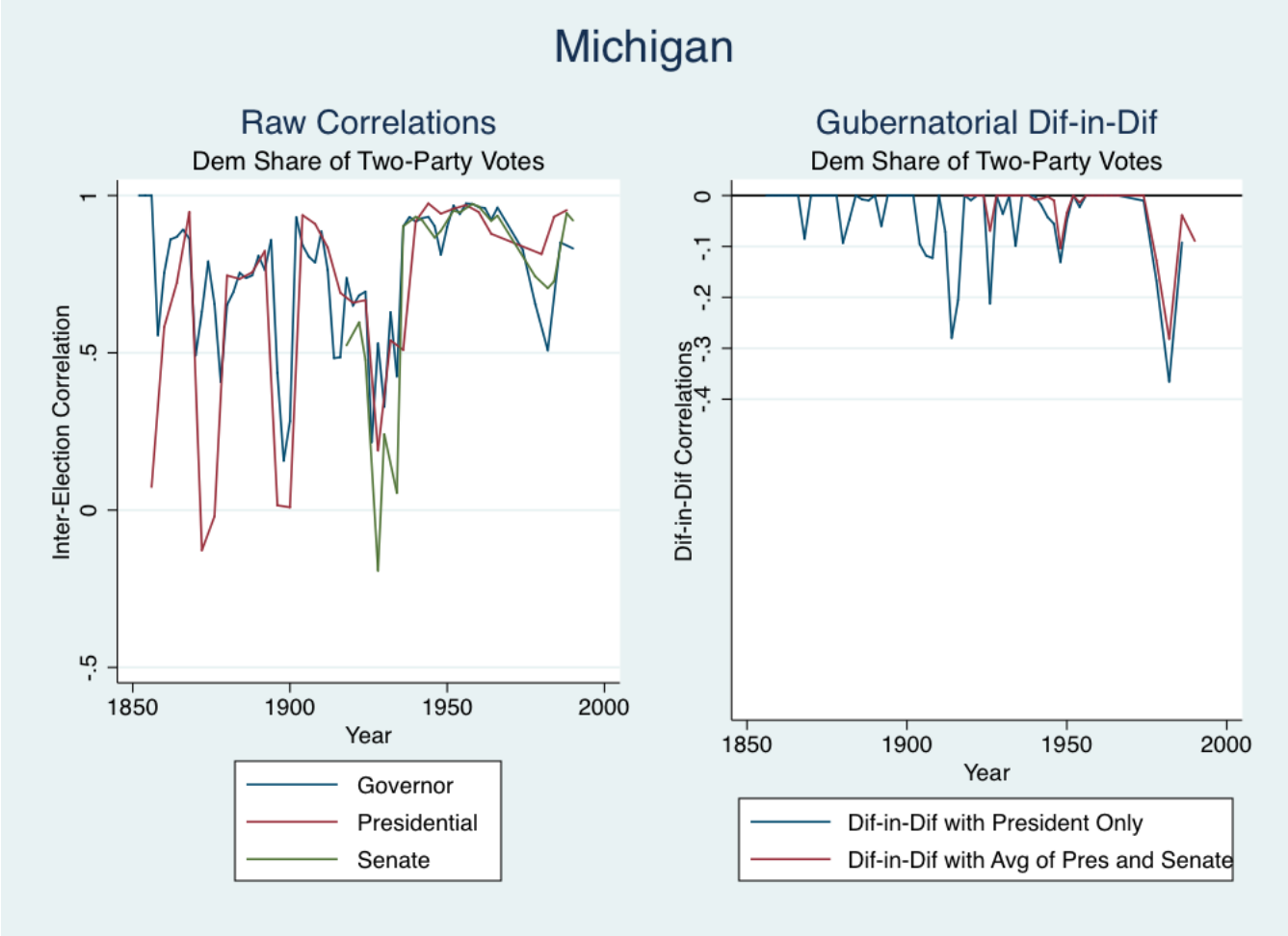


Figure 2: Michigan: High Volatility, but no Standout Entrepreneurs



In Michigan, by contrast, we find significant inter-election variation in county-level voting. As shown in the right hand graph of Figure 2, however, these realignments are almost never office specific. Michigan thus presents a perfect example of the analytic power gained by the difference-in-difference strategy employed in this paper – a simple undifferenced examination of IECs might suggest that Michigan experiences regular realignments and is thus full of entrepreneurs. After making a difference-in-difference adjustment, however, it is clear that most of these shifts are party level shifts, and thus do not necessarily represent evidence of a political entrepreneur.

The preceding two figures provided a sense of the "baseline" stability that appears in many states after making difference-in-difference adjustments. But Figure 3 clearly illustrates the type of dynamic electoral realignment that can be induced by a single individual, in this case Perry Swisher, a Republican senator from Pocatello who in 1967 ran as an independent for governor, initiating a

major split of the state Republican party. Like many conservative states at the time, the primary wing of the Republican party stood for small government. But as budget pressures increasingly squeezed the state and the Republican party campaigned to block a measure to raise sales taxes to support higher education, Swisher decided to step into the race as an advocate for a more moderate form of conservativism which wanted small but not nonexistent government. In explaining his decision, he said that "Someone must go into the campaign and defend the good things the sales tax is paying for and speak for the new faith in Idaho's resources." Stapilus (2009)

Swisher lost as an independent, but he picked up more than 12% of the vote and also began a broad discussion about the nature of conservatism in the state. The conversation he began eventually led to a significant wing of the state party turning on the then-governor, the extremely conservative Don Samuelson, and as a result in 1971 – along the same cleavage lines established in the 1967 election – a Democratic governor was elected for the first time in 24 years in Idaho.

Swisher's story is not only remarkable for having established a new political cleavage in his own election, but for inducing a realignment that propagated to the subsequent senate race. In other words, while his realignment began as a campaign-specific shift, in later years it spilled over to other offices, as can be seen (informally) in the low inter-election senate correlations in 1970 and 1972 (this spillover is examined more rigorously in Section 3). Moreover Swisher's story draws attention to the complexity of political entrepreneurism. As noted before the measure used in this paper identifies significant campaigns, not individuals, and the entrepreneur in a campaign may not always be the winning candidate. But in first identifying exceptional elections, the measure presented here is a crucial first step to developing a greater understanding of political entrepreneurship.

A key discovery of this analysis is that Perry Swisher is not at all alone. Political entrepreneurs appear regularly across both time and space, as shown in Figure 4 below.[3] Not only does this finding provide important evidence that political entrepreneurs are not a minor and rare occurrence – they are a fundamental part of the political landscape.

Moreover, this data provides initial guidance into answering some of the stickiest and most persistent questions about political entrepreneurs. First, the heterogeneity of entrepreneur emergence across states is highly consistent with the common presumption that the emergence of political entrepreneurs is likely influenced by institutional rules (Deegan-Krause and Enyedi, 2010; Schneider and Teske, 1992; Sheingate, 2003) (a finding which is further substantiated by the following analysis of the impact of state primaries in Section 4).

Second, the lack of significant temporal "bunching" provides support for the argument that

[3]To provide a better sense of the distribution of entrepreneurs, the difference-in-difference corrected IECs from each state are normalized so as to facilitated inter-state comparisons – see Section 1 for a more detailed discussion of this – and individuals are marked as "entrepreneurs" if their differenced IECs are more than two standard deviations from the state mean.

Figure 3: Idaho: A Clear Entrepreneur!

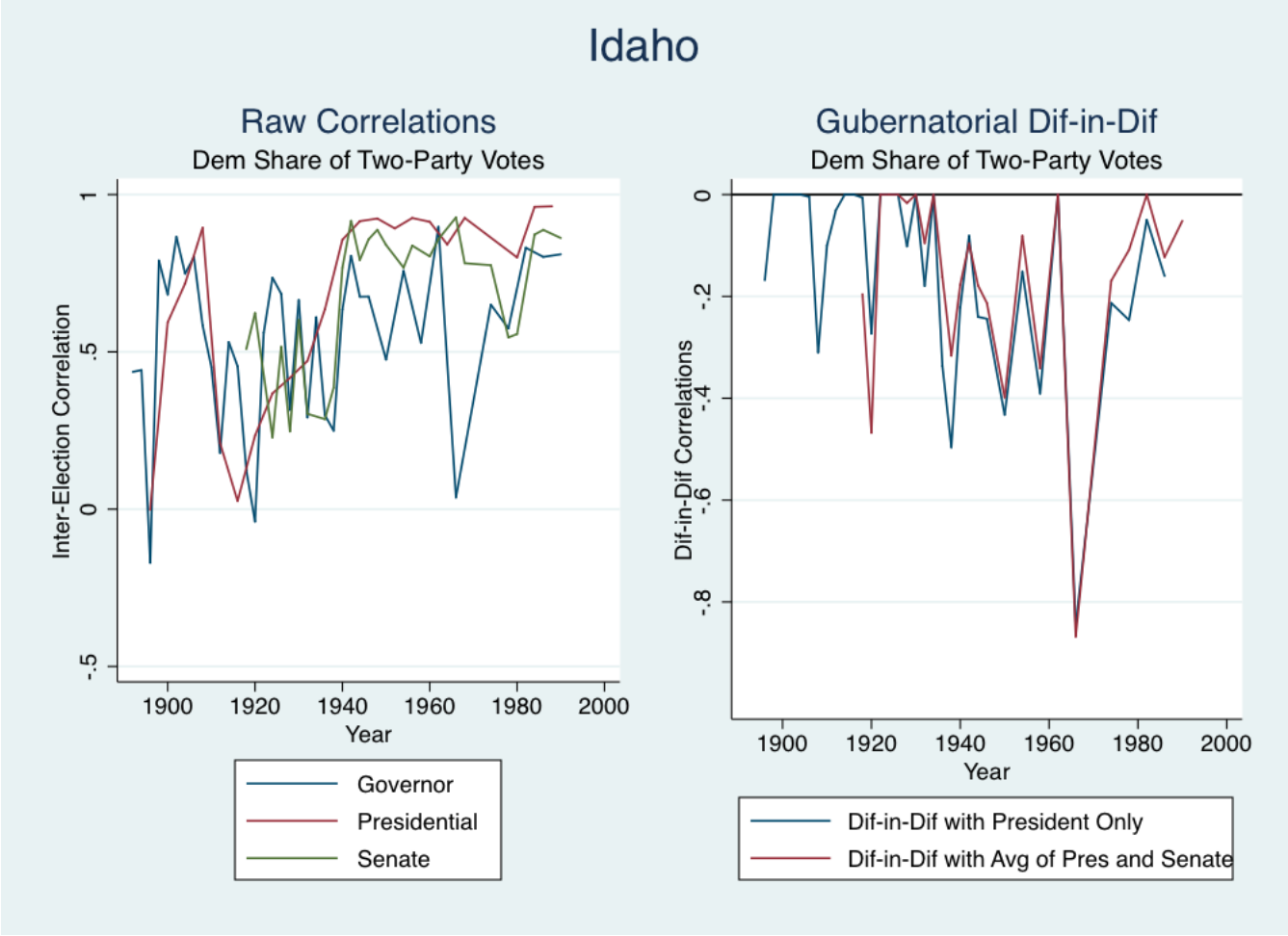
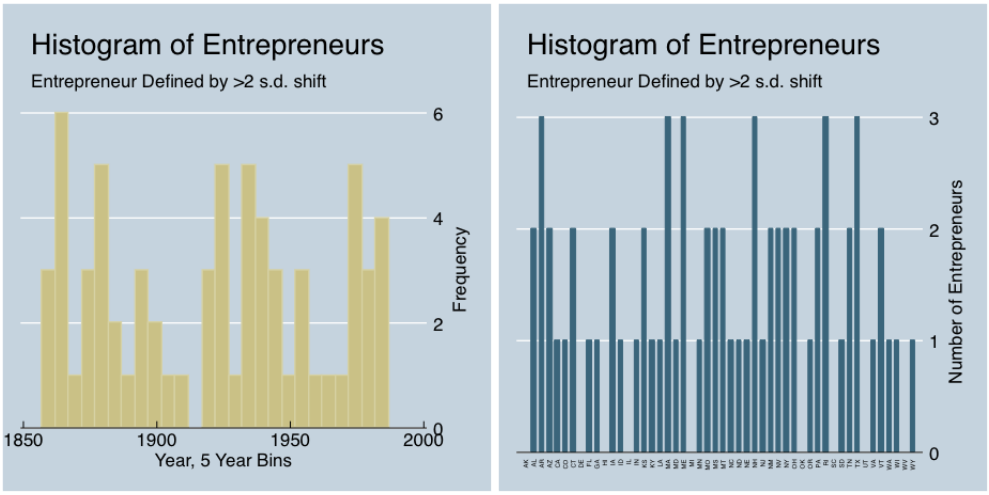


Figure 4: Distribution of Entrepreneurs Across Time and Space



political entrepreneurs have significant individual agency. If political entrepreneurs were simply responding to disequilibria induced by structural economic or social changes the moment they emerged, we would see political entrepreneurs appearing in bunches as everyone seeks to take advantage of the same disequilibria. Moreover, we would likely see more small shifts and fewer discrete realignments than appear in the data. The lack of inter-temporal correlation and presence of discrete realignments suggests that while entrepreneurs may be taking advantage of existing disequilibria rather than inducing them themselves, in most cases these disequilibria likely existed for some time before an exceptional politician came along who was able to capitalize on the opportunity. In other words, this data is consistent with the idea of occasional exceptional entrepreneurs, not a steady stream of interchangeable individuals always marginally arbitraging disequilibria as they appear.

3 Propagation of Realignments

The focus of this section is understanding whether the realignments induced by political entrepreneurs represent unique coalitions that rally around a specific candidate but not others, or whether the realignments induced by entrepreneurs have spillover effects on other politicians in subsequent elections and different offices. This section shows that the realignments induced by a great many entrepreneurs *do* spillover, showing the entrepreneurs are not just idiosyncratic individuals, but rather agents who change the political landscape for everyone. Moreover, this result emerges despite the fact the measure used in this paper overlooks the most obvious potential cases of spillover in the interest of clean identification – elections in which multiple offices face simultaneous realignments. In other words, despite throwing out any elections in which an entrepreneur may be causing a simultaneous realignment for multiple offices and restricting out attention to cases where there is no *immediate* spillover, we still find numerous cases of spillover in subsequent elections!

To test these spillover effects, the list of gubernatorial political entrepreneurs identified in the preceding section using a "two standard deviation" criteria is further restricted to include only those entrepreneurs whose realignment appeared in an election with a concurrent senate election (to eliminate any concern results are driven by linear interpolation). We call this initial election $t = 0$. Then the county level Democratic vote shares from the subsequent election (let us call it election $t = 1$) are regressed against both the senate Democratic vote shares *and* gubernatorial Democratic vote shares from the $t = 0$ election in which the entrepreneur's realignment first appeared. If it is the case that the gubernatorial realignment has propagated to subsequent senate races, then the coefficient on the *gubernatorial* county Democratic vote share should be a strong predictor of $t = 1$ vote shares even controlling for *senate* $t = 0$ vote shares.

As shown in Figure 5 below, the realignments of 9 of the 24 entrepreneurs who appear in years

with simultaneous senate and gubernatorial elections are indeed significant and positive predictors of senate voting behavior in the subsequent senate election, suggesting that in many cases the realignments induced by political entrepreneurs *are* propagating to other candidates, offices, and elections. The results for entrepreneurs who appear in elections that do not occur simultaneously with senate elections but make use of senate IEC interpolations to compute differenced gubernatorial IECs (presented in Figure 7 in Appendix A) are even stronger: the county level returns of 9 of 16 gubernatorial entrepreneurs have positive and statistical significant predictive power over subsequent senate election voting even when controlling for actual vote shares in the first previous (non-interpolated) senate election.

4 Primary Rules and the Emergence of Political Entrepreneurs

One of the greatest challenges facing the study of political entrepreneurs is the need to move beyond the study of exceptional individuals to begin understanding the institutional features that drive the emergence of political entrepreneurs. This section takes a first significant step in that direction by examining how the emergence of political entrepreneurs has been shaped by the introduction of direct primaries in the United States.

Direct primaries were introduced in the United States in the early 1900s in response to two pressures. The first was the Progressive Movement, which argued at the turn of the century that US parties had become too powerful, and that the introduction of primaries was necessary to lessen their grip on nominations. But the second influence, and the factor that made the implementation of primaries political feasible, was that the parties themselves were looking for a way to simultaneously better aggregate preferences across ever growing populations to maintain competitiveness, as well as consolidate intra-party power at the state level. At the turn of the century, political parties in most non-Southern US states were struggling to control sub-state party officials who, as the size of state populations grew, were exerting ever greater independence. (The adoption of primaries in the some southern states was driven by very different processes, and so may southern states are excluded from the analysis in this section following Ware (2009)). Thus, while it was the case that primaries were expected to lessen total party control over nominations (and indeed did, as shown by Ansolabehere (2004) party discipline within congress did decline in states that implemented primaries), in most states the implementation of primaries was actually supported by the party (Ware, 2009).

With this in mind, this section presents two different approaches to estimating the effect of primaries on entrepreneur emergence. In the first approach, this paper mimics the empirical strategy of Ansolabehere (2004) and treats the timing of primary introduction as exogenous in light of Ware

Figure 5: Realignment Propagation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
State	**AZ**	**CO**	**CT**	**IA**	**ID**	**IL**	**MA**	**MD**	**MD**	**MI**	**MN**	**MN**
Year	**1934**	**1978**	**1974**	**1918**	**1966**	**1928**	**1978**	**1974**	**1986**	**1982**	**1918**	**1922**
Senate Race Dem	0.363	0.477***	1.396	0.695***	0.488***	0.586***	-0.148	0.340**	0.692***	-0.0104	.	0.569***
Vote Share	(0.313)	(0.0735)	(1.240)	(0.0672)	(0.133)	(0.141)	(0.253)	(0.133)	(0.0961)	(0.0751)	.	(0.165)
Gubernatorial Race	0.152	0.482***	-0.524	-0.0571	0.251*	0.0793	0.269	0.120	-0.183	1.111***	0.568***	0.396***
Dem Vote Share	(0.234)	(0.0934)	(0.711)	(0.0728)	(0.129)	(0.148)	(0.200)	(0.126)	(0.163)	(0.0692)	(0.0528)	(0.133)
Observations	14	63	8	99	44	102	14	24	24	83	86	86
R-squared	0.255	0.728	0.203	0.591	0.644	0.629	0.179	0.358	0.773	0.888	0.579	0.732

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

	(13)	(14)	(15)	(16)	(17)	(18)	(19)	(20)	(21)	(22)	(23)	(24)
State	**MO**	**NH**	**NV**	**NY**	**OH**	**PA**	**RI**	**TN**	**TN**	**TX**	**TX**	**WI**
Year	**1980**	**1974**	**1938**	**1974**	**1932**	**1922**	**1982**	**1954**	**1958**	**1924**	**1934**	**1932**
Senate Race Dem	0.135*	0.698	0.649**	1.017***	0.970***	-0.462**	0.668**	0.811***	0.655***	0.544***	0.158	1.617***
Vote Share	(0.0733)	(0.380)	(0.248)	(0.252)	(0.161)	(0.185)	(0.0885)	(0.0273)	(0.119)	(0.0555)	(0.232)	(0.266)
Gubernatorial Race	0.699***	-0.010	0.226	0.0827	-0.286*	1.191***	-0.279	-0.296**	0.123	0.177***	1.187***	-0.764***
Dem Vote Share	(0.0961)	(0.350)	(0.189)	(0.188)	(0.154)	(0.207)	(0.224)	(0.135)	(0.176)	(0.0397)	(0.225)	(0.285)
Observations	115	10	17	62	88	67	5	95	95	241	241	71
R-squared	0.609	0.568	0.796	0.691	0.509	0.541	0.993	0.906	0.645	0.458	0.597	0.527

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

(2009)'s argument that party strength did not drive adoption timing. Then using the variation in the timing of primary implementation shown in Table 1 below, a difference-in-difference strategy is used to estimate the impact of primaries.

For those who are unconvinced by the exogeneity of primary introduction timing, however, this section also estimates the effect of primaries by first instrumenting for the date of primary introduction using the size of states. As previously noted, one of the major motivators for party support for primary introduction was the need to better address organizational challenges arising for increasing population sizes. As a result, the primary was adopted more quickly in states with more congressional districts, as Ware (2009) shows in a simple cross-tabulation, as presented in Figure 6 below. Instrumenting off this variation allows for a second set of estimates of the effects of primaries to be estimated, which are broadly consistent with the non-instrumented results.

Table 1: **Share of States with Primaries**

YearInterval	mean
1900	0
1905	.26
1910	.72
1915	.89
1920	.89
1925	.89
1930	.89
1935	.89
1940	.95
1945	.97
1950	.97
1955	1

Source: Data.dta

Difference in Difference Approach

This section uses data on primary implementation from Ware (2009) – who provides estimates of the date at which most state offices within a state were first chosen by primary – to conduct a difference-in-difference estimation of the effect of primaries according to the following specification:

$$
\begin{aligned}
y_{t,s} =& \alpha + \beta_1 * PRIMARY_{t,s} + \beta_2 * STATE_s \\
&+ \beta_3 * FIVE\_YEAR\_DUMMIES_t \\
&+ \beta_4 * PRIMARY * FIVE\_YEAR\_DUMMIES \\
&+ \epsilon
\end{aligned}
$$

Where:

$y_{i,s}$	is the measure of entrepreneurship in state s at time t,
$PRIMARY_t$	is a dummy for whether state s had a Primary for majority of five year window t,
$STATE$	is a vector of state fixed effects,
$FIVE\_YEAR\_DUMMIES$	is a vector of dummies for each five year period,
ϵ	is a stochastic error term.

And the marginal effect during any given period t is given by $\beta_1 + \beta_4$.

Two measures of entrepreneurship are used for this estimation. The first is the difference-in-difference corrected IEC measure for governors. This measure uses only the presidential IECs when computing the differenced gubernatorial IECs. This is because senate data is not available before the introduction of primaries, and introducing a systematic change in how the dependent variable is measured simultaneously with a change in institutional setting could lead to obvious erroneous inferences. To facilitate cross-state comparisons, this measure is normalized at the state level to have a mean of 0 and a standard deviation of 1. Differenced gubernatorial IECs above 0 (i.e. periods in which the IEC of the governor is *greater* than those of the president) are then truncated at zero, since inferences cannot be made about the meaning of the positive magnitude of this difference[4]. As previously noted, in most cases where gubernatorial IECs exceed presidential IECs say more about exceptional presidential elections than the relevant gubernatorial election. When using this measure, the unit of observation is a gubernatorial election.

The second measure is the share of elections with political entrepreneurs within a five year period (with each five year period entering regressions once). Entrepreneurs are defined in a binary manner as governors whose differenced IEC is more than one standard deviation from the state mean. Note that this differs from the preceding section in which a cutoff of two standard deviations was employed – this choice was made due to the relative infrequency with which two standard deviation entrepreneurs are observed.

[4]This truncation obviously requires some additional econometric attention, which it will receive in future drafts

Difference in Difference Results

The results of the difference in difference estimates of the effect of primaries are presented here in two tables. First, Table 4 below presents initial regression results. Second, Table 3 shows estimates of the marginal effect of primaries for each five year period for both measures of entrepreneurship ($\beta_1 + \beta_4$ from the preceding specification), along with p-statistics. The levels of statistical significance vary from period to period, but the results are generally consistent with the theory that primaries reduced barriers to entry for entrepreneurs: the implementation of primaries drives down gubernatorial IECs (suggesting greater plasticity in electoral cleavage structures) and increases the frequency with which one-standard-deviation-entrepreneurs appear.

Instrumental Estimation of Primary Implementation

This section presents estimates of the effect of primary implementation based on instrumented timing of implementation. The working assumption of this section is that (a) the timing of primary adopting was driven in part by the desire of political parties to deal with the increasing difficulty of managing internal party conflicts during a period of rapid population growth, and (b) that greater party interest in primaries lead to early primary adoption. This assumption is substantiated by the work of Ware (2009), who in addition to making the case for this dynamic on the basis of a detailed study of historical sources, also presents the following Figure 6, which can be thought of as a stylized version of the first stage estimates to follow.

Of course, one may find the use of state size as an instrument objectionable – it is not difficult to come up with a story for how state size might be related to the entrepreneurial potential of a state. Larger states may have more people who could become entrepreneurs, for example, or may have more cross-cutting cleavages on which entrepreneurs could capitalize. In this setting, however, it is not necessary that state size be unrelated to a state's *level* of entrepreneurs, only the state's entrepreneurial *time trend*. For example, if larger states were seeing a high rate of growth in political entrepreneurialism at the turn of the century, then by estimating earlier primary implementation dates for large states, this strategy would systematically associate higher rates of entrepreneurialism with periods in which states have primaries. There is no clear reason to think state size has any such effect on the time trend of entrepreneurialism within states, however, suggesting that this is in fact a valid instrument for this empirical strategy.

As shown in the first stage estimation in Table 4 below, larger states (as measured by log census population in 1900) adopted primaries at earlier dates than smaller states controlling for the census region of a state, as predicted by theory and the work of Ware (2009). Moreover, this effect is quite

Table 2: Primary Rules and Entrepreneur Emergence

	(1) Gov Corrs	(2) Pct with Entrep.
Has Primary	0.28**	-0.26***
	(3.52)	(-4.75)
Primary * 1905 Dummy	-0.066	0.15
	(-0.43)	(1.91)
Primary * 1910 Dummy	-0.012	0.066
	(-0.07)	(0.52)
Primary * 1915 Dummy	-0.44***	0.30***
	(-5.72)	(4.32)
Primary * 1920 Dummy	-0.36	0.16
	(-1.22)	(0.73)
Primary * 1925 Dummy	-0.48***	0.33***
	(-6.39)	(4.86)
Primary * 1930 Dummy	-0.70***	0.40***
	(-4.67)	(4.05)
Primary * 1935 Dummy	-0.46**	0.37***
	(-3.41)	(4.40)
Primary * 1940 Dummy	-0.68***	0.46***
	(-4.76)	(5.65)
Primary * 1945 Dummy	0	0
	.	.
Primary * 1950 Dummy	-0.59***	0.44***
	(-6.13)	(7.20)
Constant	-1.02***	0.044
	(-5.53)	(0.64)
Observations	1659	876

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

State and Five-Year-Period fixed effects omitted from table.

Dependent variable in Column 1 is inter-election gubernatorial dif-in-dif corrected correlations.

Dependent variable in Column 2 is share of elections entrepreneur, defined as more than 1 standard deviation negative swing in inter-election correlations

Clustered on states.

Table 3: Primary Effects

	GovCorrMarginalEffect	P-Value	EntrepreneurMarginalEffect	P-Value
Year1905	0.216	0.149	-0.104	0.137
Year1910	0.269	0.117	-0.189	0.109
Year1915	-0.154	0.049	0.044	0.324
Year1920	-0.079	0.792	-0.091	0.687
Year1925	-0.203	0.002	0.073	0.129
Year1930	-0.416	0.009	0.141	0.136
Year1935	-0.182	0.153	0.118	0.093
Year1940	-0.395	0.006	0.209	0.002
Year1945	0.281	0.001	-0.255	0.000
Year1950	-0.313	0.000	0.180	0.000

Figure 6: State Size and Primary Adoption (*Source:* Ware (2009))

	Five or Fewer Congressional Districts	Six to Eleven Congressional Districts	Twelve or More Congressional Districts
Provision for Presidential Primary in 1920	Montana New Hampshire North Dakota Oregon South Dakota Vermont	California Maryland Nebraska West Virginia Wisconsin	Illinois Indiana Massachusetts Michigan New Jersey New York Ohio Pennsylvania
No Provision for Presidential Primary in 1920	Arizona Colorado Connecticut Delaware Idaho Maine Nevada New Mexico Rhode Island Utah Washington Wyoming	Iowa Kansas Kentucky Minnesota Oklahoma	Missouri

Table 4: First Stage Estimation of Primary Implementation

	(1)
	Date of Primary Implementation
Log 1900 Population	-3.09
	(-1.27)
South	-7.86
	(-1.03)
Midwest	-9.35
	(-1.45)
West	-2.87
	(-0.39)
Observations	39

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

Omitted region is Northeast

strong.

To compute the "instrumented" effect of primary implementation, the preceding first stage regression is used to predict a most likely implementation date for each state, which are presented in Table 5 below. These dates are then substituted for actual dates of primary implementation, and the same difference-in-difference estimation presented above is replicated. These results are presented below in Table 6 and Table 7.[5]

Table 5: **Share of States with Primaries using IV Implementation**

YearInterval	**mean**
1900	0
1905	.088
1910	.33
1915	.54
1920	.68
1925	.97
1930	1

Source: Data.dta

[5]Please note that this section is a relatively recent addition to the paper, and I have not yet had time to think seriously about how to properly estimate standard errors in this unorthodox two-stage least squares setting. The standard errors presented are the uncorrected standard errors that emerged from the second stage regression and do not take the sequential nature of estimation into account.

Table 6: IV Primary Rules and Entrepreneur Emergence

	(1) Gov Corrs	(2) Pct with Entrep.
IV Primary Dummy	-0.16**	0.045
	(-3.18)	(0.48)
IVPrimary * 1905 Dummy	-0.35	0
	(-1.51)	.
IVPrimary * 1910 Dummy	0.26*	-0.14
	(2.31)	(-1.24)
IVPrimary * 1915 Dummy	0.026	0.015
	(0.30)	(0.13)
IVPrimary * 1920 Dummy	-0.038	0.015
	(-0.26)	(0.10)
IVPrimary * 1925 Dummy	0	-0.0013
	.	(-0.01)
IVPrimary * 1930 Dummy	0.58*	0.052
	(2.57)	(0.48)
Constant	-0.58**	0.043
	(-3.04)	(0.62)
Observations	1659	876

t statistics in parentheses

* $p < 0.05$, ** $p < 0.01$, *** $p < 0.001$

State and Five-Year-Period fixed effects omitted from table.

Dependent variable in Column 1 is inter-election gubernatorial dif-in-dif corrected correlations.

Dependent variable in Column 2 is share of elections entrepreneur, defined as more than 1 standard deviation negative swing in inter-election correlations

Clustered on states.

Table 7: IV Primary Effects

	GovCorrMarginalEffect	P-Value	EntrepreneurMarginalEffect	P-Value
Year1905	-0.512	0.026	0.045	0.631
Year1910	0.099	0.417	-0.098	0.177
Year1915	-0.135	0.196	0.060	0.192
Year1920	-0.199	0.172	0.060	0.547
Year1925	-0.161	0.003	0.043	0.176
Year1930	0.421	0.092	0.096	0.195

The results of these tables are generally consistent with the previous non-instrumented results: instrumented introduction of primaries has a negative and sometimes statistically significant effect on differenced gubernatorial IECs – again suggested primaries increased cleavage plasticity. And instrumented primaries have a generally positive (albeit not generally statistically significant) impact on the emergence of one-standard-deviation-entrepreneurs. The point estimates for both dependent variables are roughly similar to those from the non-instrumented regressions. Thus taken as a whole, this results are broadly consistent with the non-instrumented estimates, and supportive with the argument that primaries reduced barriers to entrepreneurial entry, leader to more frequent electoral realignments.

5 Conclusion

The results presented here clearly contradict the common notion that political entrepreneurs cannot be studied systematically in a way that yields interesting and exciting insights. The results show first that while individuals may be idiosyncratic, as a population, entrepreneurs respond to institutional forces, and as such are hardly above systematic study. Furthermore, the results show that entrepreneurs are far from a quiet and omnipresent feature of the political landscape. Political entrepreneurs are *not* mere arbitrageurs who make constant marginal adjustments to maintain equilibria. Rather, exceptional entrepreneurs do emerge on far from rare occasions, upsetting periods of stability, and creating realignments that effect not only the political fortunes of the entrepreneur himself, but also the broader political landscape. Much work remains to be done elucidating the causal mechanisms facilitating entrepreneurs to induce realignments, but the empirical strategy of this paper provides a unique foundation for further study, not merely at the state level in the United States, but potentially in many other settings as well.

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A Robustness

Figure 7: Realignment Propagation – Elections with Interpolated Dif-in-Difs

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
State	**AR**	**AZ**	**CO**	**CT**	**FL**	**IN**	**KY**	**LA**
Year	**1940**	**1936**	**1928**	**1978**	**1978**	**1984**	**1987**	**1964**
Senate Race Dem Vote Share	0	0.416	0.485***	0.0425	0.418***	0.704***	0.706***	0
	(0)	(0.266)	(0.101)	(0.708)	(0.120)	(0.0634)	(0.0730)	(0)
Gubernatorial Race Dem Vote Share	0	0.198	-0.0494	2.013**	0.357**	0.165**	0.118	0
	(0)	(0.224)	(0.0920)	(0.726)	(0.139)	(0.0803)	(0.0727)	(0)
Observations	75	14	63	8	67	92	120	64
R-squared		0.277	0.280	0.652	0.648	0.823	0.667	1

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1

	(9)	(10)	(11)	(12)	(13)	(14)	(15)	(16)
State	**MA**	**MN**	**MS**	**ND**	**NM**	**RI**	**TX**	**VA**
Year	**1986**	**1920**	**1983**	**1924**	**1968**	**1956**	**1932**	**1981**
Senate Race Dem Vote Share	0.0658	.	0.427***	0.0868*	0.0676	0.838***	0.348***	0.152
	(0.109)	.	(0.106)	(0.0444)	(0.270)	(0.0598)	(0.0187)	(0.112)
Gubernatorial Race Dem Vote Share	0.701***	0.252***	0.583***	0.120***	1.132***	0.404**	0.0113	0.920***
	(0.101)	(0.0872)	(0.106)	(0.0367)	(0.395)	(0.0576)	(0.0129)	(0.101)
Observations	14	86	82	52	32	5	227	136
R-squared	0.917	0.090	0.439	0.282	0.362	0.996	0.614	0.768

Standard errors in parentheses
*** p<0.01, ** p<0.05, * p<0.1