Party Aggregation and Political Consolidation in the American States

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Abstract

Electoral institutions and social heterogeneity have long garnered the most attention from political scientists looking to explain the variation in party systems across different countries. However, less attention has been paid to explaining the overtime variation in the number of parties within political jurisdictions, ignoring the longitudinal variation. Additionally, the study of party systems in the United States has focused mainly on the failure of third parties and the lack of major American socialist party, neglecting the broader party systems literature. This paper proposes to test the party aggregation theory of Chhibber and Kollman which argues that fiscal centralization creates incentives for parties and voters to coordinate that decreases the number of effective parties within a jurisdiction, using the American states as a testing ground. Hopefully, this will provide further evidence for Chhibber and Kollman’s party aggregation thesis, while incorporating more of the broader comparative literature on party systems into the study of American state party systems.

1 Introduction

Electoral institutions and social heterogeneity have long garnered the most attention from political scientists looking to explain the variation in party systems across different countries. However, less attention has been paid to explaining the overtime variation in the number of political parties within political jurisdictions. In short, most research has addressed cross-national rather than longitudinal variation. However, many countries have seen large fluctuations in the strength and number of viable national parties over their history (Chhibber & Kollman 1998; 2004). Institutions and social cleavages, however, rarely change and thus do a poor job of explaining this overtime variation.

This paper will propose a research design to test Chhibber and Kollman’s central thesis that the consolidation of political power by a government provides incentives
for voters to choose broadly competitive parties over local or regional parties and thus drives down the effective number of parties (ENP). While Chhibber and Kollman have shown evidence that fiscal centralization leads to fewer parties at the national level, their theory of *party aggregation* still has not been extensively tested, and the mechanism has never been studied at the subnational level, in this case the American states. This project proposes an empirical test of the theory on a selection of five American states to see if greater political centralization affects party aggregation at the state as well as the national level. In short, does the theory of party aggregation by political centralization explain the variation within the American states as well as at the national level?

2 Literature Review

As mentioned above, institutions and social cleavages have received the bulk of the consideration from political scientists. Most modern institutional research on party systems was kick-started by Duverger’s famous law that plurality electoral systems and single-member districts (SMD) favor the emergence of two-party systems (1954). Obviously, however, the empirical reality did not conform entirely to Duverger’s expectations. Several states have displayed a stubborn tendency not to develop nationwide two party systems. India is an often cited example (Riker 1982, Chhibber and Petrocik 1989; Lijphart 1994). Other primarily English-based systems like Canada and Britain have historically also seen divergence from the Duvergerian equilibrium in their national legislatures, even though most of their legislative elections have used
SMD’s and plurality voting (Chhibber and Kollman 2004).

Since its publication, Duverger’s originally theory has been refined significantly. As many scholars have noted, it is not a strictly determinative law (Ware 1996). The most important refinement of Duverger is almost certainly Cox’s seminal work *Making Votes Count* (1997). Here Cox emphatically argues that Duverger’s law is an explicitly district-level phenomenon. To illustrate, Cox argues that Duverger’s law really only ensures that a country, or other district-divided political jurisdiction, will have *no more* than twice the number of parties as electoral districts. The two-party equilibrium is an institutional function of each individual district. In other words, if a political jurisdiction has \(X\) districts, Duverger’s law only sets a theoretical maximum of \(2X\) parties for the jurisdiction overall.

Therefore, another process, something other than simple mechanical institutionalism, is needed to go from district-level bipartisanship to national-level bipartisanship. Cox refers to this process as ‘linkage’ or ‘cross-district coordination’ and argues that it is one of the most understudied areas in the party system literature (Cox 1999). In fact, in his review of the electoral rules and coordination literature he only finds two articles (Chhibber and Kollman 1998; Samuels 1998) that address this issue directly. What is missing is some sort mechanism that drives the effective number of parties at the national level towards the number of parties at the district level. In short, an intervening variable or process is needed to bring the overall political jurisdiction’s party system inline with the Duvergerian bipartisanism that develops at the district-level in plurality electoral systems.

A number of different theories have been proposed to explain linkage based on co-
oordination of candidates in particular. Cox argues that the existence of an upper-tier legislative chamber (such as the U.S. Senate which is state, rather than district-based) or a presidential system can incentivize candidates to coordinate in order secure control of the central government (Cox 1997, 1999). Aldrich presents a similar argument that elite coordination within the legislature helped establish national party labels in the early U.S. legislature (1995). In both cases, parties were established by elites to overcome collective action problems so as to better control public resources.

It’s important to note that this is an primarily bottom-up theory of party system formation, which argues implicitly that parties initially form at the district/local level but some other incentive needs to exist to cause candidates and parties to coordinate across districts. However, it is also important to point out that voters also need some incentives to identify with a particular party brand name as well, and an even further incentive to choose a nationally competitive party over a favored regionally competitive one (Chhibber and Kollman 2004). In short, explaining how parties aggregate (or coordinate, or link) across districts to form broader-based political parties requires theorizing about the incentives of both candidates to coordinate and voters to prefer nationally competitive parties to local ones that might have policy preferences closer to their ideal.

Chhibber and Kollman’s own theory, the obvious inspiration for this paper, argue that fiscal and governmental centralization drives party aggregation by affecting the incentives of both candidates and voters (1998, 2004). Each group must adjust its strategic calculations to accommodate a more active and involved central government. Candidate cross-district coordination incentives can be a reaction to either
voter preferences or the desire to influence national policies. The later reason is fundamentally the same as the Cox and Aldrich arguments, but the former brings in the desires of the citizenry. The important development in the Chhibber and Kollman argument is that “under a system of central authority, voters see themselves as either benefiting or losing from national policies (1998, pg. 335).” In this cases, voters have strategic reasons to choose a nationally competitive party over a preferred local or regionally competitive one. Basically, with a stronger centralized government, access to public resources depends more on the actions of the national (or state) government than the local one. In short, the stakes are too high for voters to continue to support non-nationally competitive parties. This has the effect of driving down the number of parties. Empirical evidence for this phenomenon has been shown in the U.S. and India (Chhibber and Kollman 1998) and Brazil (Samuels 1998) as well as Canada and the Britain (Chhibber and Kollman 2004).

To be fair, it is necessary to briefly mention some research on the effect of social cleavages on linkage or party aggregation. While the Chhibber and Kollman thesis owes its intellectual heritage primarily to the institutional literature, there is some evidence that cleavages can affect linkage as well, although primarily in a negative sense. Simply put, social and ethnic heterogeneity can increase the ‘chances of malcoordination (Cox 1999, pg. 159)’ and therefore inflate the effective number of parties in a jurisdiction. Social heterogeneity can intervene in the linkage process, causing variations in the effectiveness of party aggregation. While research on the effect of various types of social heterogeneity on party systems is extensive (Lipset and Rokkan 1967; Lijphart 1979; Kalyvas 1998), very little has been done on the effect
of social cleavages specifically on the process of linkage. Would voters of a broadly
diverse jurisdiction - ethnically, linguistically, racially, etc. - find it more difficult to
coordinate for political gain than voters in a more homogeneous jurisdiction when
faced with increased in political centralization?

Generally speaking, party aggregation theory is based on rationality and relies
on strategic actors, both candidates and voters. However, many authors have ar-
gued that the outcome of the party system is best shown as an interactive process
between social heterogeneity and electoral institutions (Cox 1999) indicating that
social heterogeneity may impact the variation in the ENP seen at various levels. For
the purposes of this study, as you can see below, ethnic heterogeneity is primarily
included as a control in order to more clearly see the effects of political consolidation,
and thus is of less concern to the overall empirical research question.

Finally, while linkage has been only minimally studied, there has been a small,
if interesting, set of research on the electoral support for so-called third parties in
the United States (Hirano and Snyder 2007). However, this literature has been
motivated differently than the literature on linkage and party aggregation. Funda-
mentally, both are interested in explaining the seeming stability of the U.S. two party
system. However, the U.S. third-party literature has been motivated by the absence
of a major socialist party in the U.S. (Lipset 1977; Lipset and Marks 2000) rather
than how the overall party system itself emerged. Because of this, there is a major
methodological difference between the two literatures. While the party systems and
electoral institutions research focuses on the number of parties as a measure of the
party system, the third-party literature typically uses electoral support rather than
the effective number of parties as a measure of the dependent variable.

Because of this, the American-focused third-party literature has developed a different series of explanations for the stability of the U.S. party system. Coincidentally, some authors have come back to an explanation of the ineffectiveness of third-parties based primarily on the difficulties of overcoming SMD’s and plurality voting (Rosenstone et al. 1996). However, as mentioned above, this relatively constant institutional rule still doesn’t explain the overtime variation in the number of parties nationally. Additionally, other institutional changes, specifically the adoption of the direct primary and the Australian ballot, have been cited as explanations for the overall lack of third-party voting in the United States (Epstein 1986; Ware 2002). However, tests of these institutional variables on electoral support for third parties have seen inconsistent results (Hirano and Synder 2007; Crespin 2004).

Likely the most popular explanation for the failure of third parties in the United States is co-optation, specifically by the Democratic party (Oestreicher 1988). This theory argues that American third parties are primarily a left-wing phenomenon. Third parties like the Populists and the Progressives account for most of the third-party movements, and the Democratic party has routinely adopted the platforms of these parties as their own, most noticeably during the New Deal but also with the free silver issue and the presidential nomination of William Jennings Bryan in 1896. However, the empirical evidence for this is incomplete and consists mainly of evidence that the Democratic Party moved to the left ideologically, previously third-party affiliated candidates switched primarily to the Democrats, and counties that had previously had high third party vote totals became more Democratic afterward.
(Hirano and Synder 2007).

Overall though, the American third-parties literature suffers from a lack of generality. First, it focuses on the party system in a single nation. Secondly, because it is puzzled by the lack of a major socialist movement in the U.S., it is preoccupied with explaining the actions of parties of the left and not the development of the U.S. party system more generally. Finally, there is nothing in the empirical evidence presented by Hirano and Synder (2007) that is incompatible with the thesis of party aggregation by fiscal centralization or political consolidation. They acknowledge that the New Deal spelled the death knell for third party voting, a finding equally compatible with the party aggregation theory. In addition, party aggregation is likely to result in both candidates and voters abandoning non-competitive regional parties for strategic reasons. Basically, while the mechanism, co-optation versus political consolidation, is different between the two theories, the empirical evidence presented to support the co-optation argument equally supports the party aggregation theory.

3 Research Design

In short, two main theories were presented above. The first, party aggregation by political consolidation, predicts that the effective number of parties will decline as the government becomes more centralized and begins to play a larger role in the lives of its citizens. Alternately, the co-optation argument states that third party electoral support declined as a result of the leftward ideological shift of the Democratic Party, particularly after the New Deal. The primary difference between the two is the
mechanism of the theory, ideological movement or fiscal centralization. Additionally, the co-optation argument is not necessarily incompatible with the fiscal centralization argument but mistakes an effect for a cause. If the party aggregation theory of Chhibber and Kollman is correct, political consolidation should precede the party aggregation, and the ideological shift should be an effect of new party members affecting the internal deliberations of the party, pushing them ideologically. Even a strategic move meant to garner votes by party elites is only an effective co-optation tactic if the voters have an incentive to shift to a party with less commitment to their issues.

This design does not propose to offer a definitive test of the two theories. It simply proposes to test the fiscal centralization argument at the state level using five American states. According to Chhibber and Kollman, “the logic for party aggregation is independent of the level of government...these effects may occur at the state level as well (1998, pg. 340).” By using multiple states (instead of a single country) as the units of analysis, we can test the strength of the party aggregation theory in multiple jurisdictions, with considerable variation in the centralization of their governments and the expansion of the government service sector both cross-sectionally and overtime. Also, according to the co-optation argument, the decline of third parties was primarily a top-down, national phenomenon and a result of elite strategic calculations. However, if the results from this design show a variation in the decline of the ENP across different states at different times, related to government centralization, then that will indicate that political consolidation and strategic behavior on the part of both parties and voters are the underlying cause of the stable
two-party system in the United States.

As mentioned above, the units of analysis for this design will be a selection of American states. Ideally, all the states would be included in the study and combined into a time-series, cross-sectional design that would account for their time of entry into the dataset based on when they joined the union. Unfortunately, there are a couple of issues with this approach. First, there is the issue of the South and its political history of *de facto* one-party government as well as how to account for the Civil War period. Additionally, several states have used, and continue to use, some form of multi-member districts to elect legislators. While evidence indicates that party aggregation due to fiscal centralization may not be a phenomenon entirely limited to single-member district plurality systems (Samuels 1998), including these states would unnecessarily complicate the design. In short, accounting for factors like region and the effects of non-SMD districts is a subject for a separate paper. No to mention that acquiring the district-level and state-legislative results for all states over an *extremely* long time series would require a massive data collection effort.

Given the above, five states have been selected to include in the design: Pennsylvania, New York, Virginia, Michigan, and California. The first three have been states since 1790 and can be analyzed over a time series similar to that used by Chhibber and Kollman for the overall United States (1998). Michigan and California obviously were admitted to the union later, 1837 and 1850 respectively, and offer the opportunity to examine whether states joining the union later followed a differ-

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1See Maryland, Illinois pre-1980, Massachusetts, Ohio, etc. Some states have used non-partisan systems as well, such as Minnesota between 1917 and 1976, which would also complicate the design. For a brief overview, see Hamm & Moncrief 2007.
ent party aggregation process. You’ll notice that one southern state, Virginia, was included, which will hopefully shed some light on the party aggregation processes in the southern states. However, the eventual results for Virginia should be viewed separately from the other four states.

To test the party aggregation theory, it’s necessary to calculate two different measures for each state. The first is the ENP at the state-level; the second is the average ENP per district. The later should stay near two, while ENP at the state-level should be larger and fluctuate overtime. The average ENP per district for the lower chamber of each state’s legislature is calculated using the same process and equation described in Chhibber and Kollman (1998, pg. 331), which is based off an older measure (Laakso and Taagepera 1979). The ENP is calculated for each electoral district using the proportion of the popular vote gained by each candidate in that district and then averaged across each state, to create a measure of the effective number of parties for each state at the district level which is designated $D_N$. The ENP at the state-level, designated $S_N$, is also calculated according to the Chhibber/Kollman method, except that the vote totals will obviously be aggregated within the states for the lower chamber elections than at the national level.

The data for calculating these measures will come largely from a dataset available from the ICPSR, Partisan Division of American State Government (Burnham 1987). This data contains information on state legislative elections (among other contests) from 1834-1985. While not complete - some years are missing and some elections are missing party labels - the data for the five states mentioned above is relatively complete and any missing data shouldn’t affect the results of the statistical and
graphical analysis. Obviously, independent data collection will be necessary to extend the dataset back to 1790 for Virginia, Pennsylvania, and New York. This will likely require archival research or at least a direct request to the secretary of state’s office for each state, since the data does not seem to be available online. This is yet another reason to limit this initial design to a select, manageable number of states.

Obviously, at this point in the final paper, it will be necessary to present graphs similar to Figure 1 in the Chhibber and Kollman article (see pg. 331, 1998). If we don’t see the same sort of separation between $D_N$ and $S_N$ when graphed overtime, then the theory fails to jump an initial hurdle, whether there actually exists any separation between the average district level ENP and the state-level ENP. If the two measures fail to show significant difference overtime, then there is little reason to continue trying to explain something that is not there.

However, assuming that a similar difference exists between $D_N$ and $S_N$ when graphed for each state overtime, the analysis can continue. While interesting, the true measure of the ‘success’ of party aggregation is the difference between $D_N$ and $S_N$ (Chhibber and Kollman 1998, pg. 332; Cox 1999). A difference approaching zero indicates considerable aggregation/linkage and that the number of parties in each state approaches the number of parties in each district. Larger values of the $S_N$ minus $D_N$ - hereafter designated as $D_{S-D}$ - indicate that aggregation is not occurring and that local or regional parties are receiving considerable vote share in many state legislative districts. At this point, I would like to produce graph of $D$ for each state, similar to Figures 3A & 3B in Chhibber and Kollman (1998, pg. 333). Obviously, my charts would span the total available set of years available for each state, graphed
separately.

However, to complete those graphs, it would be necessary to have some sort of measure of the political consolidation and fiscal centralization of each state overtime. Unfortunately, the proportion of national to state/local (nondefense) spending used by Chhibber and Kollman is unlikely to work at the state level. Their measure combines state and local spending relative to national spending. While this is fine for their design since they are concerned with the ENP at the national level, obviously separating state and local spending is important for this design. However, local spending data is unlikely to be easily available or easily comparable across jurisdictions for much of this time period. That said, there are a couple of ways to address measuring state fiscal centralization, basically the primary explanatory variable in the graphical analysis and later in the statistical analysis as well.

First, it is important to determine exactly what type of spending would incentivize voters and candidates to start coordinating across districts within states. In the case of state governments, there are two broad types of spending that, in my opinion, would serve as acceptable proxies for overall fiscal centralization: social welfare spending and education spending. Early in U.S. history, these two issues tended to be handled at the local level by cities or private charity organizations. Then, beginning during the mid-1800's, states started to become seriously involved in social welfare and education, drastically increasing their investment in these programs, extending their involvement in the day-to-day lives of their citizens, and centralizing their control over spending and policymaking (Fishback and Thomasson 2006). Additionally, since it would be difficult, if not impossible, to get a reliable measure of
overall local or charity spending relative to state spending on social welfare and education, the measure of $D_{S-D}$ for each state will be graphed separately against three measures: state spending on social welfare per capita, state spending on education per capita, and overall state spending on both per capita.

While not a relative measure of state versus local spending on education and welfare, which would be ideal, measuring per capita state spending should effectively operationalize increased state involvement in policymaking and the centralization of politics within the state. The implicit assumption in using the per capita measures, apart from the assumption that these are the correct measures of spending to be using in the first place, is that the general trend overtime is for a relative increase in state involvement, which I believe is defensible from the historical record (Fishback and Thomasson 2006). This measure will allow the presentation of graphical analysis of the relationship between spending and party aggregation in each state, and the data to construct these measures is available from the National Historical Geographic Information Systems (NHGIS) at the Minnesota Population Research Center combined with the Historical Statistics of the United States (HSUS) Millennial Edition available online from the Cambridge University Press (MPC 2004; HSUS 2006).

Finally, following the graphical analysis, I propose to present a very basic statistical analysis, comparable to Table 1 in Chhibber and Kollman (1998, pg. 337). Basically, this simple statistical analysis of the data regresses the national ENP in a given year on their measure of fiscal centralization as well as a one-year lag of the dependent variable. My analysis would be similar, simply substituting the state ENP in a given year ($S_{N,t}$) for their national measure and regressing it on one or more
of the state per capita spending measures as well as a one-year lag ($S_{N}^{t-1}$). Exactly which measure of state per capita spending would depend on the results from the graphical analysis; however, we can assume that the overall per capita measures of both welfare and education spending can be used.

However, the original Chhibber and Kollman statistical analysis fails to control for social heterogeneity and the possible ‘malcoordination’ effects that it could have on party aggregation. Therefore, I propose to extend their analysis by also including a measure of social heterogeneity to control for at least one possible alternative explanation. Choosing a measure of social heterogeneity that is able to be calculated across the time series proposed (approximately 1790-1980) is difficult. While the NHGIS site does have all historical Census data available for download by state for the entire time period of interest, obviously what data is available changes over-time (MPC 2004). Given the sparse information that was available from the early Censuses, I propose to include a measure of based on the ‘birthplace index’ of ethnic fragmentation (see Costa and Kahn 2003, pg. 105). The birthplace index is based on the Gini coefficient and ranges from 0 to 1, with 0 meaning a completely ethnically homogeneous state and a 1 equalling complete ethnic heterogeneity and fragmentation. Higher measures should indicate greater social heterogeneity and increase the difficulty of coordinating across districts, having a negative effect on party aggregation. Additionally, the birthplace index is useful because nativity and place of birth information have been collected since the early Censuses, and, since the measure is categorically based, it can accommodate the expansion of racial and ethnic Census designations since the 18th century.
4 Conclusion

To conclude, hopefully this design will offer further support to the Chhibber and Kollman thesis that political consolidation, as measured by fiscal centralization, can drive party aggregation at the state level as well as the national level. Previous explanations for the failure of third parties and socialist parties in the United States have failed to incorporate the broader party systems literature and test for a more general explanation of the U.S. case. Additionally, this design will hopefully advance research on linkage and party aggregation, helping to close the gap between the institutional and social foundations of party systems and the final empirical output.

In addition, hopefully this design will spur further research on other questions related to linkage. For example, what types of government spending are important for party aggregation? Chhibber and Kollman only used a broad measure of non-defense spending (1998). This design only proposes using social welfare spending and education spending, but, in my opinion, the relevant characteristic of the spending should likely be penetration. In other words, how deeply does the spending impact the citizenry? How many people are directly impacted by the spending and how much benefit do they derive from it? This is also another reason why the per capita measures used above are appealing, since the measure captures, at least somewhat, how much benefit each citizen receives from different types of spending.

For example, in the admittedly broad category of ‘social welfare’ spending, most of the original spending was for health clinics, food, and other care primarily directed at the indigent. In contrast, public education was broadly available and had an impact across all social classes. I would argue that the education spending might show
more penetration and therefore have an even greater impact on party aggregation than other types of spending. In short, voters are more likely to pay attention to state politics when the state is providing funding and dictating policy to the schools that are educating their children.

Finally, testing these theories out at the state-level allows for far more degrees of freedom and the ability to include more control variables and overall get a clearer picture of the process of linkage and party aggregation. Specifically, expanding this pilot study to include all states, possibly excluding the South, would offer the opportunity to test the party aggregation theory in non-SMD electoral systems. As I mentioned above, many states have used some form of MMD to elect their legislators at various times throughout their history. This practice has declined over the last several decades, largely do to accusations that the districts can be discriminatory, but historical state elections data would offer an excellent opportunity to test the effects of SMD’s vs. MMD’s on party aggregation.

In the end, institutions and social heterogeneity may be the foundation of party systems, but party systems express themselves as they do because of intervening variables that create linkage between broad influences and empirical outcomes. As I hope the eventual analysis shows, political consolidation plays a major role in creating that linkage and motivating party aggregation. State party systems come to resemble their local components because of the role of government in the lives of its citizens. Increase that role, and voters will respond by turning to parties that have the broad-based appeal necessary to govern and address those concerns.
References


