Professionalism, Realignment, and Representation

MORRIS P. FIORINA *Harvard University*

The critique of my 1994 article by Stonecash and Agathangelou reflects a series of misconceptions and misunderstandings—about measures, methods, arguments, and findings. In this rejoinder I attempt to correct these. In addition, I clarify my methods and findings. First, I show that a formal statistical test indicates that limiting the analysis to the northern states is justified. Less formally, the professionalism hypothesis cannot work the same in the South as in the North unless levels of Democratic legislative strength can rise above 100%. Second, although clearly inferior to a pooled analysis, I show that a disaggregated (state-by-state) analysis is far more supportive of the professionalization hypothesis than the flawed results Stonecash and Agathangelou report. Third, despite the repeated assertions of Stonecash and Agathangelou, I demonstrate that there is no evidence that a long-term partisan realignment to the Democrats is occurring, and that, contrary to their methodological recommendations, the variables included in my analysis would capture it if it were. Finally, Stonecash and Agathangelou interpret my research as indicating a lack of relationship between constituencies and who gets elected. That is simply not a correct reading of my article.

My 1994 article (Fiorina 1994) was a narrowly focused piece, and I thought that the arguments, procedures, and results were clearly presented. Apparently, I was wrong. The critique by Stonecash and Agathangelou (1997) reflects a series of misconceptions and misunderstandings—about measures, methods, arguments, and findings. No doubt some of these are shared by others, so, I am grateful to my critics for providing this opportunity for clarification.

Because the various misunderstandings in Stonecash and Agathangelou are intertwined, the starting point for a reply is not obvious. I will first answer the questions they raise about research design, measures, and methods, showing that their criticisms are groundless. Then I address the alternative realignment hypothesis my critics offer, pointing out that there is no evidence to support it. Finally, I reply to the more general philosophical or normative concern that apparently motivates their critique. Here, they wrongly cast me as an adversary, when I am, in fact, on their side.

EXCEPT IN THE SOUTH

Long ago I recall hearing that if there were a first law of U.S. politics, it undoubtedly would be qualified by the phrase "except in the South."[1] Over the years, hundreds of analyses of myriad topics in U.S. politics routinely set aside the South, not because southern politics were inherently uninteresting but because of a general understanding that various dynamics were different there. Such was the case in the research at issue here.

The starting point for my article was the earlier realization that (1) unified government in the American states had greatly decreased in the postwar period, (2) the decrease was a result of a decline in unified Republican states, and (3) the Republican decline reflected a decline in Republican legislatures (Fiorina 1992, 32–6). In addressing the third point I noted that the Republicans had never controlled a southern or border state legislature during the postwar period (my analysis ended with the 1990 elections). Thus, it seemed natural to begin with the question, "What explains the decline in Republican legislative strength in the North?"

Having formulated a hypothesis—that increasing legislative professionalism advantages Democrats—there is no reason in principle not to apply it outside the North.[2] I considered doing so and compiled data for the whole country. Preliminary analysis, however, indicated that the processes at work were very different in the southern and border states. Generational replacement, the out-migration of African-Americans, and the in-migration of people and industry have interacted with changes on the national political scene to produce electoral outcomes distinct from those in the larger nation (Black and Black 1987). Ironically, Stonecash and Agathangelou take me to task for ignoring the possibility of a questionable realignment in northern states, but they estimate equations that make no attempt to account for a known realignment in southern states.

Statistically speaking, the national data do not pool. The equations estimated in Table 1 of my 1994 article (p. 306) cannot even be estimated when the southern and border states are included in the analysis. The estimations fail because levels of Democratic representation in many southern legislatures are virtually constant (at or near 100%) until the mid-1960s or even mid-1970s in some states. As a result, the lagged dependent variable in these states is nearly the same as the state intercept for much of the period.[3] If regional

Morris P. Fiorina is Professor of Government, Harvard University, Cambridge, MA 02138.

I thank James Alt and Gary King for their comments and suggestions.

[1] Like other remarks in a similar vein, this one is often attributed to Ray Wolfinger.

[2] Indeed, outside the United States for that matter. As noted earlier (Fiorina 1992, 50), a version of the hypothesis was articulated by John Stuart Mill writing about parliamentary compensation in nineteenth-century Britain.

[3] Substantively speaking, these high levels of Democratic legislative strength mean that the experience of most of these states provides little or no usable information, unless we believe that rising compen-

TABLE 1. Explaining Democratic Lower House Seat Shares, 1946–90: Is the Nation Homogeneous?

	Main Effects		Southern Interactions		Border Interactions	
Variable	Coefficient	*t*-value	Coefficient	*t*-value	Coefficient	*t*-value
South	242.120	2.14	—		—	
Border	262.120	1.32	—		—	
Year	.032	.91	−.151	−2.28*	−.142	−1.31
Democrats (t − 1)	.508	20.10**	.435	7.70**	−.067	−.52
Compensation	.007	2.08*	−.005	−1.03	−.007	−.59
Presidential year	−27.079	−11.10**	22.778	6.19**	11.520	1.35
Presidential vote	.498	9.40**	−.452	5.85**	−.234	−1.39
Gubernatorial year	−.18.540	−7.86**	15.111	4.08**	−.440	−.04
Gubernatorial vote	.364	8.10**	−.328	−5.52**	−.044	−.20
Off year	−10.042	−10.62**	6.800	3.80**	2.818	.82
GNP growth	.149	2.74**	−.157	−1.49	−.148	−.79

Note: Thirty-one state intercepts not shown. Coefficients are unstandardized.
n = 1,035, adjusted R^2 = .89, *SEE* = 7.94
*p < .05; **p < .01

dummy variables for the southern and border states are substituted for the individual state intercepts, equations can be estimated. Then, after forming southern and border interactions with all the other right-hand side variables, a standard F-test indicates whether the southern and border states are part of the same sample as the other 31 states.[4] The results indicate that they are not—at well beyond the .01 significance level.[5] As shown in Table 1 above virtually all the southern interactions are highly significant; the border interactions mostly have the same signs but are weaker in magnitude. In the South there is a clear trend away from the Democrats, presidential and gubernatorial coattails are nearly the reverse of the main effects, the midterm penalty is mostly wiped out, the national economy does not matter, and the retention rate of Democrats from the previous legislature is nearly unity (.508 + .435 = .943). Interestingly, the main effect of legislative compensation is still significant in the whole sample, and the net effect in the South (.007–.005) is slightly positive—suggesting that rising compensation has partly dampened the trend toward the Republicans, consistent with the original hypothesis—but I put little confidence in this pooled estimation.

In sum, a formal statistical test provides conclusive grounds for rejecting Stonecash and Agathangelou's argument that my 1994 analysis should have included the entire United States. More data do not always mean a more dependable analysis.[6]

MEASURES

Stonecash and Agathangelou criticize the crude nature of the variables used to proxy legislative professionalization. Everyone who has ever compiled data from the *Book of the States* knows that numerous errors are inevitable. I explicitly listed some of the sources of error in my article (1994, 308).[7] But imperfect data in no way distinguish research on state politics from any other research. If we have to wait for perfect data before doing any empirical analysis, we might as well all become theorists. The alternative I prefer is to recognize the sources of error in the data, use multiple indicators, and proceed carefully. In the case at hand, the errors are in the independent variables and generally work against my hypothesis. Consider the New York example noted by Stonecash and Agathangelou. The error in measuring "days in session" clearly works against the professionalization hypothesis: If the "days" measure exaggerates the burdens of legislative service in New York, then Republicans would not be as discouraged as the measure would predict.

Once again, I find Stonecash and Agathangelou's own procedures perplexing in light of the criticism they level: "Given the clear errors in Fiorina's days variable, as well as the great difficulty of gathering accurate information, we have chosen to focus on salary and annual sessions" (p. 152). But *Annual Sessions* is a *dummy* variable that assigns the same value to New York and to Wyoming. Because there are errors in "days in session" Stonecash and Agathangelou think it preferable to assign only values of zero and one? In the 1989–90 sessions using "days" has the effect of assigning 729 days to New York and 79 to Wyoming.

sation is sufficiently important to push levels of Democratic legislative strength above 100%.

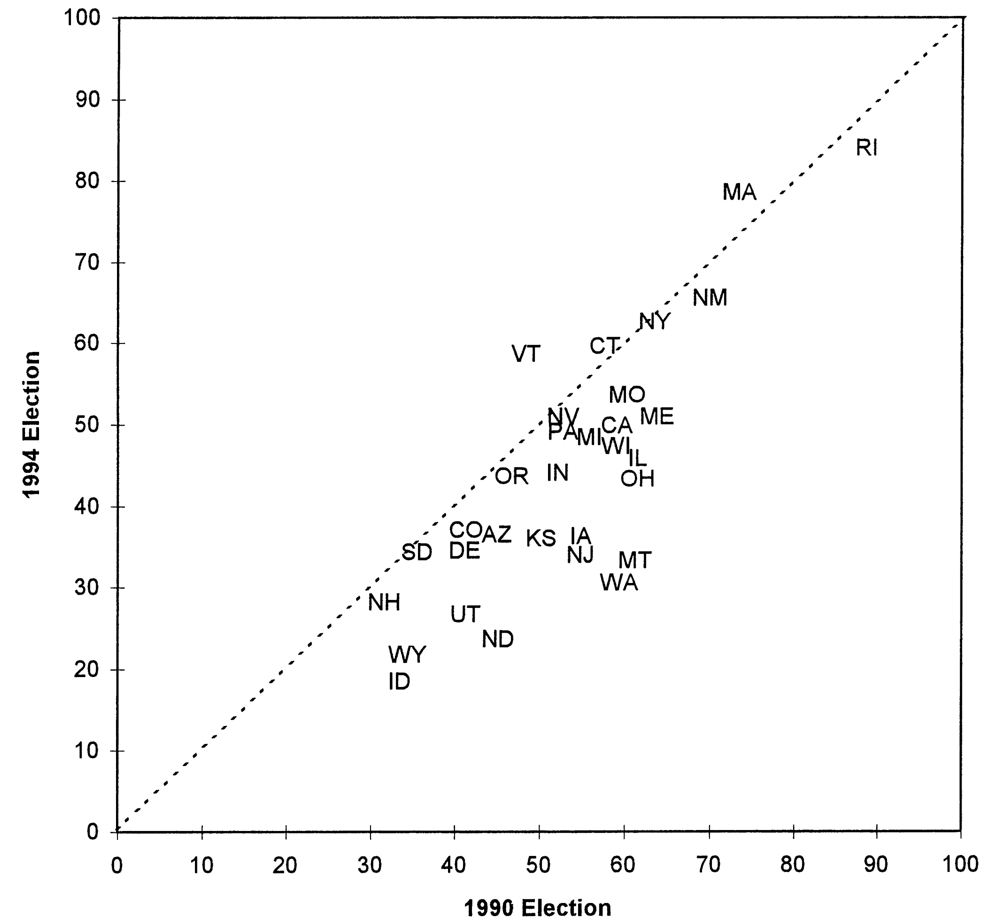
[4] As in the original article nonpartisan Nebraska is omitted, as are Alaska, Hawaii, and Minnesota, which lack data for a significant part of the series.

[5] The sum of squared residuals in the additive model is 74,230, compared to 61,460 in the model with south and border interactions. With 1,035 observations, 42 variables (including 31 state intercepts and south and border dummies) in the additive model, and 60 variables in the interactive model, the standard formula yields an F statistic of 11.24.

[6] I would not defend to the death the decision to omit all the border states, which include West Virginia, a state that nicely fits my hypothesis (i.e., it falls in the top category of Table 2), but since the border category includes states such as Kentucky and Oklahoma that are often categorized as southern (e.g., by Congressional Quarterly), I eliminated all of them rather than be suspected of picking and choosing among them. Incidentally, Stonecash and Agathangelou comment that I coded Tennessee as a border state rather than a southern state. That was a coding error in the data file; it did not affect the analysis because both southern and border states were omitted.

[7] For a recent discussion of some of the questions that arise see Carey, Niemi, and Powell (1996, 25–7).

FIGURE 1. Comparison of Democratic Proportion of Lower House Seats after the 1990 and 1994 Elections



Inaccurate though it may be, "days" captures the difference between the two state legislatures better than does a dummy variable.

METHODS

With short time series—23 elections in this case—individual state analyses are impractical. The ratio of noise to information is too high. Inclusion of the theoretically hypothesized variables plus relevant control variables eats up too many degrees of freedom to permit confident conclusions. But short time series across similar units can be pooled to increase efficiency and extract the maximum amount of information from the data. When diagnostics revealed no peculiarities in the data (Fiorina 1994, n. 15), I turned to standard estimating techniques. The results were statistically significant and robust across specifications.

Stonecash and Agathangelou reject the advice of the methodologists I consulted and claim that regressions on individual states are the more appropriate statistical technique. Using the data I provided they regress percentage of Democratic seats on the *Annual Sessions* dummy variable and the *Real Compensation* variable and report that only about 60% of the two-variable regressions have the right sign for *Compensation*, the workhorse variable in my analyses. This exercise is misconceived.

Stonecash and Agathangelou claim to get around the degrees-of-freedom problem by including only two right-hand side variables, the dummy variable for annual sessions and the real compensation variable that emerged significant in my pooled analysis. They do not include a lagged variable (percentage of Democratic representatives in the previous legislative session), a statistical necessity.[8] Nor do they include presidential and gubernatorial coattails, national economic conditions, or the midterm penalty, saying that I have "no rationale for including them" beyond the fact that they are "conventional indicators" (p. 152). Following are some rationales:

[8] Stonecash and Agathangelou's omission of the lagged percentage of Democratic legislators ignores problems of autocorrelation, but even more important, the omission of a variable strongly related to the dependent variable results in biased coefficients. In Stonecash and Agathangelou's case, since lagged Democrats is positively correlated with annual sessions and compensation, both those coefficients will be biased downward.

1. Look at Figure 2 (1994, 305) of the original article: Years such as 1958, 1964, and 1974 stand out against the general trend. These are well known as years of electoral turbulence in which strong national tides were running.
2. How do indicators become "conventional"? The main reason is that previous research has clearly established their relevance. In this case, articles on state legislative elections, some of them recently published in this *Review*, find highly significant effects for national and state-level political forces (Campbell 1986; Chubb 1988; Simon, Ostrom, and Marra 1991).
3. Consider the 1994 elections that not only gave control of Congress to the Republicans but also gave them control of more state legislatures than any election since 1968 (Fiorina 1996, 139–40). I have not updated the original data set beyond the 1990 elections, but judging from figures A-1 and A-3 of my article (1994, 313–4) annual sessions and real compensation had leveled off by 1990—all but four states included in the analysis had annual sessions by then, and legislatures obviously were not raising their pay at a time when their state electorates were clamoring for term limits. Thus, Stonecash and Agathangelou's equations that include only these two variables make the out-of-sample prediction that the same level of Democratic legislative strength will exist in 1994 as in 1990. Figure 1 illustrates the inaccuracy of such a prediction. On average the Democrats won 10% fewer lower house seats in 1994 than in 1990; only 3 of 31 states did not see Democratic representation in their lower house fall. This systematic misprediction indicates that Stonecash and Agathangelou's equations omit important features of state legislative elections.

In contrast, the equations in my article make the out-of-sample prediction that the replacement of Republican President Bush by Democratic President Clinton would result in 9–10% fewer Democratic seats in 1994 than in 1990. The coattails of victorious Republican governors (24 of 36 in 1994) would have subtracted a bit more (.4% for every percentage point they gained above 50.5%), while the coattails of victorious Democrats would have dampened the midterm loss by a similar amount (the state that falls farthest above the line is Vermont, where Democratic Governor Dean's landslide victory–69% of the vote—largely offset the Republican tide). The "conventional indicators" that Stonecash and Agathangelou dismiss are in fact the only way of capturing the systematic changes evident in Figure 1, as recognized by virtually all previous research.

Why would Stonecash and Agathangelou inexplicably dismiss national and state political forces and estimate such misspecified equations? Part of the explanation appears to be that they are under the erroneous impression that I am analyzing "long-term shifts in partisan attachments" (p. 152) for which short-term national and state political developments are irrelevant. This is not so. And that is not the hypothesis they "test" with their simple regressions. My article analyzes

TABLE 2. Relationships Between Real Compensation and Democratic Legislative Strength

Coefficient	States	Kurtz Category[a]
Positive	California	red
($1.73 < t < 2.8$)	Connecticut	white
($p < .05$)*	Illinois	red
	Iowa	white
	Maine	blue
	Michigan	red
	New Jersey	red
	New York	red
	Ohio	red
Positive	Indiana	white
($1.33 < t < 1.73$)	Kansas	white
($p < .10$)	Missouri	white
	North Dakota	blue
	Pennsylvania	red
Positive ($t < 1.33$)	Arizona	white
	Massachusetts	red
	Montana	blue
	South Dakota	blue
	Utah	blue
	Washington	white
	Wisconsin	red
Negative	Colorado	white
	Delaware	white
	Idaho	blue
	Nevada	blue
	New Hampshire	blue
	New Mexico	blue
	Oregon	white
	Rhode Island	blue
	Vermont	blue
	Wyoming	blue

Source: Kurtz 1991.
*one-tailed test, $df = 20$
[a] red = professionalized; blue = nonprofessionalized; white = in-between. See footnote 9 for further detail.

year-to-year variations in Democratic lower house seats; so do Stonecash and Agathangelou.

Still, is it at all disturbing that stronger indications of professionalization effects do not emerge in simple state-by-state regressions? In fact, stronger indications do emerge. In my original article I indicated that while exploring the data set I did some individual state analyses (1994, 309). If the results had been as negative as those Stonecash and Agathangelou achieved, I probably would not have continued the research. Table 2 should allay any suspicions that sleight-of-hand is behind pooled data and multivariate regressions. The table reports estimates of the effect of real compensation on Democratic legislative strength in the 31 states included in my original analysis. Like the equations estimated by Stonecash and Agathangelou, the equations summarized in Table 2 include a constant and real compensation, but they also include the lagged value of the dependent variable. These equations too are misspecified in that they exclude the initial effect of short-term forces that Stonecash and Agathangelou exclude, but the lagged dependent variable does capture the continuing effects of such shocks and greatly improves the fit of the equations.

Nearly 70% of the coefficients on real compensation are positive, and two-thirds of these have p levels of at least .10, even with only 23 observations per regression. States whose electoral histories support the hypothesized relationship span most of the country, from New York to California and Michigan to Missouri. In contrast, only one of the negative coefficients (for Idaho) is larger than its standard error. None of the nine states in Kurtz's (1991) "red" (most professionalized) category has a negative coefficient, and seven of the nine have coefficients with p levels of at least .10. In contrast, states in Kurt's "blue" (least professionalized) category dominate the category of states with negative coefficients.[9] In sum, states whose legislatures professionalized the most experienced significant gains in Democratic legislative strength. States whose legislatures professionalized the least experienced little or no gains in Democratic strength.

I do not wish to make too much of these simple regressions, but at a minimum they suggest that the hypothesized relationship is sufficiently robust to appear even in the absence of important control variables. It is not some sort of statistical artifact teased out of a large number of observations by complex methods and multiple variables. On the contrary, the original pooled analysis only makes it possible to get a statistically more precise and causally purer estimate of the relationship suggested by the individual state analyses.

WHERE IS THE REALIGNMENT?

According to Stonecash and Agathangelou, there was a realignment to the Republicans in the North in the 1890s that eroded in the 1920s and was overturned by the New Deal realignment. We are all aware of these developments, but what is their relevance for my analysis, which covers the 1946–90 period? The 1946 starting point was not chosen just to avoid the war years but also in recognition that the New Deal realignment took some time to work its way through the country (Key 1959). I hoped that by beginning in 1946 I would confine the analysis to the stable phase of a party system, which allows the individual state intercepts to capture the average or historical level of Democratic support in the state during the period (net of the other variables included in the equations).

Stonecash and Agathangelou repeatedly assert that the period I studied in fact cuts across a secular realignment to the Democrats, but they provide no evidence for their assertions beyond an observation that congressional voting has polarized by income between 1952 and 1980. They conclude that the latter "improved the fortunes of Democrats in the North" (p. 150), a conclusion that is not obvious to me, given declining turnout and the well-known propensity of people with lower incomes to vote at lower rates than do the more affluent. Moreover, their hypothesed pro-Democratic realignment is doubtful on its face. As noted in my 1994 article, the greatest part of the rise in Democratic legislative strength took place between the early 1960s and mid-1970s, a time of severe strain for the Democratic coalition.

The American National Elections Studies (NES) provide more direct evidence. Figure 2 shows the distribution of party identification in the nonsouthern and nonborder states between 1952 and 1992. Evidently, there is no evidence of a secular realignment to the Democrats during this period.

Stonecash and Agathangelou may firmly believe that the increased Democratic strength in northern state legislatures is due to an underlying realignment, but they offer no reason for us to share their belief. In the absence of evidence distinct from the dependent variable itself, claims that realignment explains increasing Democratic legislative strength are tautological. Thus, Bullock (1988) attempts to identify regional realignment by examining trends in partisan control of offices. But if the hypothesized *result* of a realignment is used to indicate its *presence*, we are locked in a circle from which there is no empirical escape.

Of course, future research may produce direct evidence of realignment during the period under consideration. But as I indicated (1994, 311), I am skeptical that the trends in northern state legislatures reflect secular realignment. On the contrary, it seems to me at least as likely that the hypothesis of a secular realignment is a reflection of the observed increase in Democratic state legislative representation that may well have other causes, including professionalization.

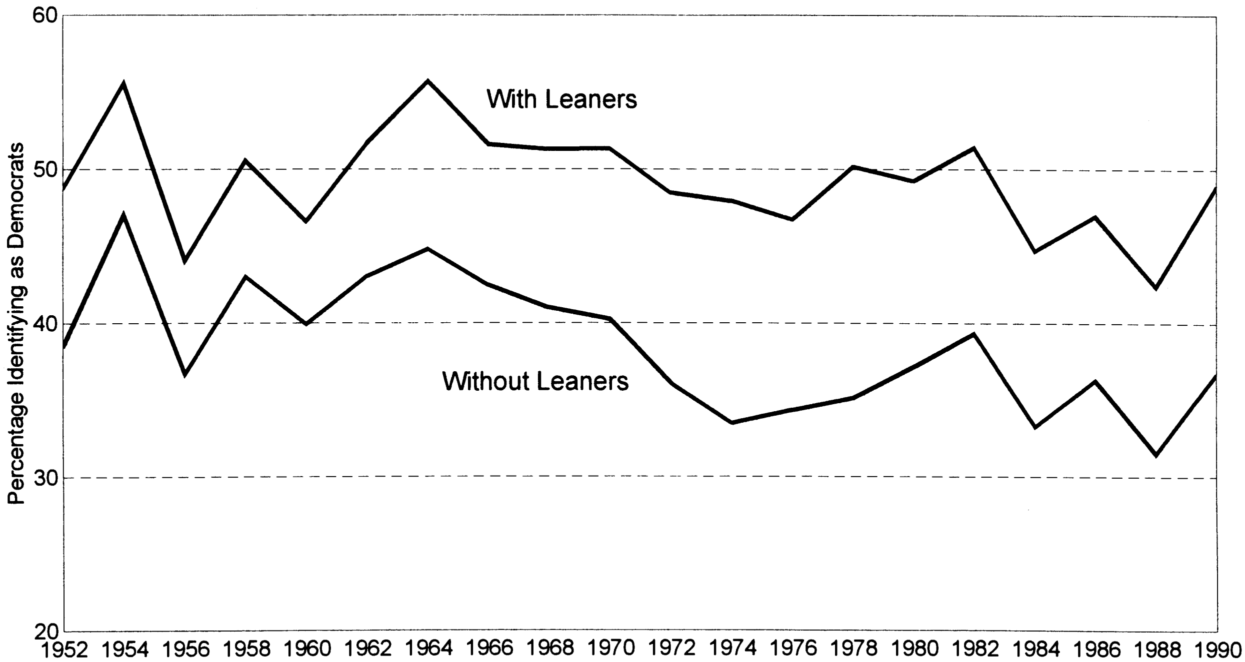
Whether my skepticism is justified is immaterial, however, for contrary to Stonecash and Agathangelou's claim, I do understand the difference between causation and correlation, and the statistical findings I reported do not confuse the two. Rather, my results indicate that *over and above* any realignment that might be occurring, professionalization matters.

To explain, recall that the regressions in my article (1994, Table 2) include the percentage of Democrats in the preceding legislature, presidential and gubernatorial fortunes in the state, and (in equation 1) a time trend. The regression coefficients on these variables would not provide direct estimates of realignment (except perhaps for that on the time trend), but if any secular realignment were going on, it would be picked up by these variables. Stonecash and Agathangelou recognize that, but regard this as a further reason to leave such variables out of their regressions, since they would be "theoretically and statistically redundant (p. 152).[10] Redundant in what sense? These variables might be redundant if Stonecash and Agathangelou had a measure of partisan realignment, but they do not, and in the absence of state-by-state, year-by-year data on

[9] Kurtz (1991) measures professionalization with three indicators: length of session, legislator compensation, and size of staff. He categorizes nine legislatures as "red" or professionalized, eighteen as "blue" or nonprofessionalized, and the remainder as "white," or in between.

[10] Stonecash and Agathangelou seem to have discovered the one source (Gordon 1968) published in the past thirty years that advocates omitting important variables from equations.

FIGURE 2. Democratic Party Identification, 1952–90, outside of Southern and Border States



Source: National Election Studies.
Note: Entries are percentage Democrats of all respondents.

party identification, party registration, or other proxies for underlying partisan sentiments, these so-called redundant variables are in fact the only way to pick up any realignment that might be occurring.

What my statistical results show is that over and above any secular realignment that would be incorporated in the various control variables, and over and above short-term forces incorporated in presidential and gubernatorial coattails, the national economy, and midterm effects, there is a statistically significant effect of legislator compensation, a professionalization variable. Moreover, the effect is substantively significant—the direct effect cumulated over the 1946–90 period averages falls in the 5 to 10% range in the more professionalized states.[11]

LARGER IMPLICATIONS

Stonecash and Agathangelou (p. 153) conclude with what appears to be the deeper basis for their critique of my article:

> Empirical analyses have implications, intended or not. What is perhaps most important about Fiorina's research is its underlying implication . . . . His argument is similar to that made by opponents of political "machines," which implies that the connection between constituents and legislators is tenuous . . . . The suggestion is that the election of Democrats does not reflect support from specific constituencies.

Such an implication is entirely in the eyes of my critics. If Stonecash and Agathangelou's charge were correct, my article would represent a complete repudiation of my previous work. In *Representatives, Roll Calls, and Constituencies* (1974) I questioned the then-prevailing view that representatives were largely unresponsive to largely uninformed constituents and suggested theoretical rationales for how responsiveness could coexist with the known empirical facts. During the mid-1970s, culminating in *Retrospective Voting in American National Elections* (1981), I challenged the prevailing view that national elections were largely reflections of arational social attachments and the personalities of the individual nominees, arguing for a mechanism through which relatively uninformed voters could enforce an acceptable degree of electoral accountability. In *Congress—Keystone of the Washington Establishment* (1977), I addressed aspects of representation that legislative scholars had overlooked and expressed a concern that assiduous representation on these dimensions could lead to an erosion of representation on more traditional policy dimensions. Finally, in *Divided Government* (1992, 1995), I took exception to the argument that divided government was an accident of congressional incumbency and suggested that there might be some policy basis for the divided government condition in which the country found itself.[12] Now, in 1997, Stonecash and Agathangelou accuse me of suggesting that electoral outcomes are

[11] There is also an indirect effect incorporated in the generally rising values of the lagged Democratic seats variable.

[12] My fellow political scientists have been slow to recognize the empirical veracity of this argument, but it now appears to have gained the status of conventional wisdom: In the closing days of the 1996 campaign, the Republican National Committee put millions of dollars into ads urging voters to elect Republicans to Congress to check and balance President Clinton.

unrelated to the interests and preferences of the constituencies who determine them.

My critics do not seem to understand the nature of a marginal effects argument with its "all other things being equal" assumption. *All else being equal*, the move from an amateur to a professional legislature advantages Democrats relative to Republicans. That does not imply that the Democrats are overrepresented, let alone that there is no relationship between constituencies and those who represent them. In the Conclusion of my article (1994, 312–3) I explicitly state that my results cannot support any judgment of whether Democrats today are overrepresented in state legislatures. There are many sources of advantage and disadvantage. Perhaps the party based in poorer socioeconomic strata is always disadvantaged to begin with (in resources, for example), so that the shift from an amateur to professional legislature helps redress the balance. Or, to mention another obvious factor, analysts have found that the postwar electoral system was biased toward the Republicans (King and Gelman 1991), so the shift to professional legislatures would have helped offset this bias.

More than many colleagues I am convinced that U.S. politics is sufficiently responsive and accountable that in the long run in most places there are reasonably close links between constituencies, those they elect, and the policies the elected produce. But at some times and in some places the links between constituencies and elected officials can be weakened by any number of factors. Districting arrangements, campaign resources, and differential participation are factors that have received study. My 1994 article only called attention to the possibility that features of legislative life might also affect the linkage. It was a piece that invited further research not a veiled indictment of American democracy.

REFERENCES

Black, Earl, and Merle Black 1987. *Politics and Society*. Cambridge, MA: Harvard University Press.

Bullock, Charles S., III. 1988. "Regional Realignment from an Officeholding Perspective." *Journal of Politics* 50 (August):553–74.

Campbell, James E. 1986. "Presidential Coattails and Midterm Losses in State Legislative Elections." *American Political Science Review* 80 (March):45–63.

Carey, John M., Richard G. Niemi, and Lynda W. Powell. 1996. "The Effects of Term Limits on State Legislatures." Presented at the annual meeting of the American Political Science Association, San Francisco.

Chubb, John E. 1988. "Institutions, the Economy and the Dynamics of State Elections." *American Political Science Review* 82 (March): 133–54.

Fiorina, Morris P. 1977. *Congress: Keystone of the Washington Establishment*. 1st ed. New Haven, CT: Yale University Press.

Fiorina, Morris P. 1974. *Representatives, Roll Calls, and Constituencies*. Lexington, MA: D. C. Heath.

Fiorina, Morris P. 1981. *Retrospective Voting in American National Elections*. New Haven, CT: Yale University Press.

Fiorina, Morris P. 1992. *Divided Government*. New York: Macmillan.

Fiorina, Morris P. 1994. "Divided Government in the American States: A Byproduct of Legislative Professionalism?" *American Political Science Review* 88 (June):304–16.

Fiorina, Morris P. 1996. *Divided Government*. 2d ed. Needham Heights, MA: Allyn & Bacon.

Gordon, Robert A. 1968. "Issues in Multiple Regression." *American Journal of Sociology* 73 (2):592–616.

Key, V. O., Jr. 1959. "Secular Realignment and the Party System. *The Journal of Politics* 21 (May):198–210.

King, Gary, and Andrew Gelman. 1991. "Systematic Consequences of Incumbency Advantage in U.S. House Elections." *American Journal of Political Science* 35 (March):110–38.

Kurtz, Karl T. 1991. "Understanding the Diversity of State Legislatures: The Red, White, and Blue Legislatures." Denver: National Conference of State Legislatures.

Simon, Dennis M., Charles W. Ostrom, Jr., and Robin F. Marra. 1991. "The President, Referendum Voting, and Subnational Elections in the United States." *American Political Science Review* 85 (December):1177–92.

Stonecash, Jeffrey M., and Anna M. Agathangelou. 1997. "Trends in the Partisan Composition of State Legislatures: A Response to Fiorina." *American Political Science Review* 91 (March):148–61.