

An Approach to the Question of the Nationalization of Political Action  
from a Vaguely Field-Theoretic Perspective\*

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\* We are grateful to Laura Stoker for discussions about the methodology at the beginning of this project and to Henry Brady of the Survey Research Center for suggestions and support. A previous version was presented at the University of California, Berkeley's Survey Research Center Occasional Research Papers Colloquium; we thank the participants and especially Neil Fligstein and Iris Hui for their comments and criticisms. We are extremely grateful to Stephen Raudenbush for numerous suggestions and clarification regarding hierarchical models.

“All politics is local”

--Tip O’Neill

## THE QUESTION OF NATIONALIZATION

### *The Substantive Question*

How does a national political field emerge? Has the degree of nationalization of American politics increased with changes in communicative media? Have new political alliances at the national level led to a transformation of party loyalties at the local level? Such questions are central to political science and political sociology (e.g., Lipset 1960: 303), and yet they have received only sporadic attention. Further, although substantively we tend to acknowledge the force of O’Neill’s aphorism, we routinely model political behavior using national level surveys and ignore any possible local variation; to the extent that we recognize that our data are in the form of a set of localities, we usually merely inflate our standard errors as a compensation.

At the same time, the formation of a national level party system has been understood as a key theoretical question. At least since the work of Duverger (1954) there has been interest in how different parts of a national party link up. Most recently, Chhibber and Kollman (2004) examine nationalization in terms of the strength of party attachments across regions. Thus in terms of party organization, nationalization refers to an aggregative process whereby local candidates throw in their lot with one another and, crucially, are recognized by voters as doing such.

Nationalization of politics, then, should leave traces in the patterns of party choice across localities. Although one could use party membership or party identification, most analysts have

used voting data, not only because such data are generally easier to acquire, but also because we—and party leaders—are generally more interested in what citizens *do* than in how they describe themselves. Further, although one could (at least in the United States) examine votes for national-level offices such as the presidency, or votes for state-level offices (such as for senator or for governor) most analysts have looked at votes for congressional representative. For it might be that certain national- or state-level contests do cut across the pattern of local politics, while these local divisions and alignments remain strong when the contests are strictly local.<sup>1</sup>

We go on to review approaches to studying the nationalization of American politics using such congressional vote data; we then suggest an alternative conceptualization that leads to a different empirical approach.

### *Previous Approaches*

The first formal attempt to determine the degree of nationalization was by Stokes (1965), who proposed the variance decomposition approach that has guided most subsequent investigations. He envisioned the problem as one of nested levels—forces that led to a certain party preference for all voters in a country, those that led to this preference for all voters within a state (but not across all states), and those which operated within a congressional district (but not across all districts within any state). Given the aggregate data he had at hand, the lowest level in his hierarchical model was the congressional district. He found evidence of substantial localism (around half of the variance seemed to be at the district level). Others working in this tradition include Katz (1973), who allowed state and national forces to affect different districts differently.

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<sup>1</sup>. Finally, such votes seem in many cases more comparable to the votes for parties in parliamentary systems, though one can make a case that single representative districts (such as seen in the U.S. and the U.K.) are hard to compare to more nationalized systems of choice of parliamentary representative.

Claggett, Flanigan and Zingale (1984: 80) pointed out that there was confusion as to whether by nationalization one meant convergence in the degree of support for one party over different areas of the nation, or convergence in response of different units to political forces—by pursuing a decomposition of variance into that associated with an election (as opposed to other elections), that associated with a sub-unit (as opposed to other sub-units), and their interaction, they were able to show a decrease in nationalization over the long nineteenth century mainly due to Southern distinctiveness, and then an (uneven) increase from the 1920s to the 1960s. But they found was consistent evidence of strong localism at all times.

Using these methods, Vertz, Frenreis and Gibson (1987) examined senatorial, gubernatorial and presidential elections in addition to congressional elections and found that while the variance in House elections was primarily at the local level, that for gubernatorial and senatorial elections was primarily at the state level, and that for president at the national level—a rather satisfying finding, all things considered. Morgenstern and Potthoff (2005) later made a more elegant version of this same basic model that included only random effects (as well as restricting the number of levels to two, to facilitate comparisons across nations without a distinct local level below that of the province).

Finally, one very straightforward approach was used by Brady, D’Onofrio and Fiorina (2000: 137), and this was simply to compare the partial correlations between party share for House elections in any mid-term election with, on the one hand, the party’s share in the House vote two years ago, and, on the other hand, the party’s share in the Presidential vote at the same time. Doing so, they found that although the 1954 election was rather nationalized, there was in general an increase in the national component of votes, with a decrease in 1986 and 1990, and then a further increase. The increase in the national component, however, does not necessarily

imply a decrease in the local component; depending on certain analytic decisions, especially whether to treat the incumbency advantage as a “local” effect, the local effect can seem quite stable or fluctuate dramatically.)

### *Paradoxes of Nationalization*

It will be seen that different authors use the concept of nationalization in somewhat different ways. Caramani (2004: 36, 39, 74) distinguishes three senses: first, the territorial homogeneity of support for different parties, second, that political forces originate at the national level, and third, that the response to political forces is uniform. His impressive work emphasizes the first, and he notes that the second cannot really be measured without individual-level data.

Not only are these ways of understanding nationalization different, they can contradict one another, leading to seeming paradoxes, as others have noted. Most obviously, the Stokesian approach sees a set of localities as highly “nationalized” when all seem to behave similarly in terms of fluctuations in vote shares (also see Alemán and Kellam 2008; and see Bartels 1998 for an approach focusing on change). In other words, they use the third of Caramani’s senses. Yet this does not always capture what we think of when we speak of nationalization—it is somewhat similar to saying that a field of daisies is more of a single thing than a tree, because when the wind blows, parts of the tree move differently, while all the daisies move the same way. (Thus Morgenstern and Swindle [2005] term the “local vote” the differential district *change*, and purge the baseline district heterogeneity from their measure of locality. A static system with great regional variation would thus appear highly nationalized.)

But the other senses also have their limitations. For example Jones and Mainwaring (2003: 140) suggest that “by a highly nationalized party system, we mean one in which the major

parties respective vote shares do not differ much from one province to the next”—that is, they adopt the first of Caramani’s senses. Certainly such homogeneity of vote share seems to imply an absence of regionalism, but such an absence of regionalism might just as well be seen as “atomization” as “nationalization,” for if there were no national-level parties whatsoever, but people simply voted at random (perhaps with an incumbent advantage), we would see such homogeneity of vote share.

There are other understandings of nationalism that are compatible with heterogeneity of vote share. For example, regionally based parties that compete for resources at the national level indicate a kind of nationalization compatible with Chhibber and Kollman’s approach—an integrated political system with weighty allocative decisions being made at the national level. Yet the vote shares will differ across areas. Second, if there is an uneven geographic distribution of types of persons differentially appealed to by different parties, we would expect some net regionalism if individuals in different localities responded to the two parties only on the basis of their individual-level characteristics. For example, if unionized factors workers are more likely to vote Democratic than are non-unionized workers in the United States, we might (at some times) expect a greater Democratic vote in the northern Midwest than in the South.

There is no solution to these puzzles, for they have to do with the incomplete connection between labels such as nationalization and our more developed intuitions about political structure and behavior (also see Mughan 1978). Yet there is another way in which we might understand the nationalization of local politics, one that has been largely ignored in part due to the sorts of aggregate data commonly used. This understanding is based, if somewhat loosely, on the idea of a “political field,” a commonly used though rarely defined terms. Here we use the term in the technical sense associated with field theory. Such field theory emerged in the social sciences

largely via social psychology (e.g., Lewin 1951) and was revived by the work of Pierre Bourdieu (1984 [1979]), leading to some Bourdieuan analyses of political fields (e.g., Ray 1999). These analyses tend to emphasize the agonistic sense of field (a field of contestation) at the expense of the notion of a field of forces.

This latter idea was most seriously investigated by Köhler (1920), who drew heavily upon the mathematical field theories of his day, such as classical theories of electro-magnetism. There is a pleasing analogical implication: just as a magnetic fields arise in a metal when local magnetic domains are aligned in parallel, so a social field may emerge when there is some sort of alignment of localities. Such ideas were investigated by organizational theorists regarding the emergence of organizational fields (Emery and Trist 1965).

Similarly, we propose to approach the nationalization question as one pertaining to the alignment of local politics into a national level field. (Although he used a different methodological approach, Caramani's [2004: 1] theoretical discussion follows parallel lines.) More specifically, what we propose should be aligned when a national field develops is the set of oppositions in the electorate that fuel party competition—what Manza and Brooks (1999) have called cleavages. Thus we consider a political field to form when the oppositions in different localities are similarly aligned with party divisions. If, say, better educated voters tended towards the Republican side in some districts but towards the Democratic side in other districts, we would say that such non-alignment of local oppositions indicated that the national-level field was weak.

Most directly, we would say that local oppositions were perfectly aligned if a completely specified model for vote choice yielded the same coefficients across different localities. Of course, there may still be variation in the vote share parties receive in different districts after

including these individual-level covariates. Such residual vote share might be understood to correspond to the “district effects” discussed by some of the previous authors. One might propose to examine the variation in such residual effects as a way of getting at the regional appeal of different parties. That is, one might imagine partitioning district-level variation into that associated with *compositional* differences across districts (differences in the distribution of individuals on key independent variables), *regional* differences (differences in constants once covariates are taken into account) and *structural* differences (how cleavages are incompletely aligned; that is, how covariates matter differently). We note below that actually it is nearly impossible to make this sort of partition without making very strong assumptions; our theoretical interest is in the last of these and we choose data and methods to carry out this particular investigation.

It is this conception of the alignment of political cleavages that we go on to study over the second half of the twentieth century. We seek to describe the dispersion of local effects of certain individual-level covariates and how these have changed over time, as well as to quantify the degree of localism of political behavior.

## DATA

We seek a set of data on voting behavior of individuals including covariates that has the greatest time span. The natural choice is the *American National Election Study* (ANES). Here we use the data from 1952 to 2004. We begin with 1952 because previous years did not use a probability sampling frame. Those who have used these data are aware of some issues pertaining to changes in the sampling frame, oversampling in certain years, and so on. In particular, in some years, the primary sampling units were congressional districts; this has some advantages

but actually decreases the power of our tests regarding congressional district level variation (for when PSUs are not equivalent to districts, we generally include more districts) (Stoker and Bowers 2002). Such issues can make a comparison of any two years somewhat problematic; our findings, however, cannot be attributed to such changes.<sup>2</sup>

We use data on congressional vote choice reported after the election; we eliminate respondents who did not report a vote for either the Republican or the Democratic candidate; given the generally spotty turnout in American elections, this is on average 37.9% of any year's respondents. The distribution of respondents across some of our key independent variables changed over the half-century examined; as a result, we have transformed our variables to ensure maximum comparability. Family income has been transformed to a Z-score; the much lumpier measure of years of education was transformed into a percentile. Race is a dummy variable that is 1 if the respondent is black and 0 otherwise; sex is coded 1 for female.

Finally, work status dummies indicate whether the respondent was unemployed, retired, a homemaker, or working (full or part time?), the omitted category. We do not incorporate these dummies in our final models because of the difficulty of estimating so many random slopes; however, initial results demonstrated that the income, age and gender coefficients did not change greatly depending on the presence or absence of the work status terms.

For most years, the NES sampling frame does not require weights; for these analyses, we do not weight cases. We have or will have \* replicated all analyses using case weights for the years in which they may be applied.

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<sup>2</sup>. The years in which congressional districts were the sampling units were 1978 and 1980. Inspection of our results gives us no reason to believe that they are due to these changes, or other changes in the number of congressional districts represented or the average number of persons in each. Other changes in the sampling frame occurred at 1964, 1972, 1984 and 1996. Again, we have no reason to believe that these changes affect our results.

In contrast to many analyses, we do not examine party identification or ideology as predictors of choice, for we consider them too proximate and likely to lead to confusing results. Instead, we examine these as alternate dependent variables (NOTE: these analyses will possibly be incorporated in a later version of this paper).

## METHODS

Given that we have individual-level data, we are able to employ a hierarchical linear modeling (HLM) approach. In many cases, such an approach allows for a direct decomposition of variance; however, because voting is a dichotomous outcome and our models logistic ones, we cannot pursue this. Instead, we examine the dispersion in slopes across congressional districts.

That is, given  $y_{ijt}$  as the vote of person  $i$  in district  $j$  at time  $t$ , with  $K$  independent variables  $x_{kijt}$ , written in matrix form as  $\mathbf{X}$ , we carry out an independent analysis for each time  $t$ . (Here we blend the notation of Raudenbush and Bryk [2002: 304-306] with that of Snijders and Bosker [2000: 216] for the case of the logistic multilevel model.)

Let  $p_{ijt} = \text{Prob}[y_{ijt} = 1]$ ; then we write

$$\text{logit}(p_{ijt}) = \beta_{0jt} + \sum_{k=1}^K \beta_{kjt} x_{kijt} \quad (1)$$

$$\beta_{0jt} = \gamma_{00t} + u_{0jt} \quad (2)$$

$$\beta_{kjt} = \gamma_{k0t} + u_{kjt} \quad (3)$$

where

$$u_{0jt} \sim N(0, \tau_{00t}) \quad (4)$$

$$u_{kjt} \sim N(0, \tau_{kktt}) \quad (5)$$

That is to say,  $u_{0jt}$  is a random effect for the  $j^{\text{th}}$  district's deviation from the mean intercept, and  $u_{kjt}$  is a random effect for the  $j^{\text{th}}$  district's deviation from the mean slope for the  $k^{\text{th}}$  coefficient.

We make no constraints on the covariance ( $\mathbf{T}$ ) matrix (such as to force some covariances to be zero.) For any model,  $t$  is fixed (that is, we run all years separately).

Different algorithms to fit such multilevel logistic models can lead to different results for the random slopes (Snijders and Bosker 2000: 219). We use the PQL method implemented in HLM, though we have replicated using the Laplace method.<sup>3</sup> In such models, there are some issues regarding interpretation: first, the inclusion of the random effects seems to increase slope coefficients and hence it may be that parameters (both slopes and standard deviations of random effects) should be examined in the form of their ratio to the level-one variance (in the form of a standard error) (Snijders and Bosker 2000: 228). However, this is generally an issue when one is comparing nested models to the same data; here we compare the same model to multiple data sets and there is no reason to imagine that our conclusions are biased.<sup>4</sup>

We here make no assumptions, and allow all slopes to vary across all congressional districts.<sup>5</sup> Such an approach has recently been used by Gelman, Shor, Bafumi and Park (2007) to

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<sup>3</sup>. Although the Laplace method may avoid the downward biases of the marginal quasi-likelihood and predictive quasi-likelihood methods, it does not produce the information matrix we need for later analyses, and its behavior is perhaps less well understood.

<sup>4</sup>. This is for three reasons. First, the total variance (that might be used to standardize coefficients) is largely driven by the residual variance that is implicit in the logistic model, and is of an order of magnitude greater than the model variances. The *changes* in these model variances are of an order of magnitude less than this, and so there is no reason to think that such changes drive our results. Second, this would only be a practical issue if our variances tended to rise and fall together, and we had to make sure that we were not inflating our estimate of one on the basis of another. But we find that, as might be inherent in the nature of the HLM model, years when variance in one parameter is large may be years when the variance in another is small. Third, given that our interest is in the variances, we need to avoid the paradoxical position of declaring that a variance is not as big as it seems, because it is precisely as big as it is! But this is the logic of deflating it by including it in the denominator of an adjustment.

<sup>5</sup>. It might seem simpler just to carry out independent analyses across all districts and then describe the results. We attempted this, but the nature of the distribution of the observations across districts coupled with a logistic model led to extreme parameter estimates (as it is not hard for a logistic model to perfectly fit certain extreme cases). To carry out comparisons one would either have to make arbitrary recodes of large values, or impose some sort of distributional assumption on the slopes. We chose the latter, and use

examine the specific question of state-level effects of the relation between income and presidential vote choice over time. They also use the ANES data, but supplement this with data from exit polls that allow for a complete representation of all states.

In our case, we hope to use the results of the model to see if there are trends that could be interpreted as evidence of increasing nationalization of local politics. The key issue is how to do this. When one has an interval-level dependent variable, it is possible to partition the variance and determine to what extent the variation is due to structural differences across districts. However, given that we treat vote choice (and party identification) as dichotomous outcomes, such a partitioning is not possible.<sup>6</sup>

Our chief interest is in the variation of the slopes for our independent variables across districts. However, we might imagine that it is difficult to simply compare variances, because (on the one hand) it seems intuitive that as the mean slope gets further from zero, the variance might also increase. On the other hand, it is not obvious that if we have a mean that changes from, say, .5 to -.5 over 50 years, with a constant variance of 1.0, that we would want to automatically adjust for the change in mean and conclude that rather than a constant dispersion over time, the dispersion first increased dramatically and then decreased again. To best present the information without making problematic assumptions, we graph the change in the central 50% of cases for each of our key independent variables.

We also present variation in the intercepts over time. This might be understood to correspond most closely to the previous understandings of nationalization, in which we are

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the HLM approach to do this. Further, the random effects approach of HLM is theoretically preferable, given that our congressional districts are a sample from a larger population of districts.

<sup>6</sup>. There are some techniques for a pseudo-partitioning for logistic regression models, but none appropriate for the questions at hand.

concerned with the distribution of party share. This method might be understood as pursuing this previous interest, only purging the effects of the independent variables. That is, variation in the intercepts can be understood as what the spatial variation in party share would look like, were there no compositional differences in age, income, race, education, sex and work status across congressional districts.

However, the measured variance of the intercepts, unlike the variance of the slopes, is dependent on the centering of the independent variables. Here we have centered our variables at the overall sample mean for each;<sup>7</sup> this seems to lead to the most interpretable results, yet caution should still be used in making strong inferences from comparisons across samples, as we do here (Snijders and Bosker 2000: 69f, 81f).

## FINDINGS

### *Descriptive Findings and The Influence of Presidential Elections*

Figure 1 charts typical output from our model (here this is for the effect of income): we have a set of three lines, the center, the average slope (the fixed effect or  $\gamma_{k0t}$ , for  $t = 1956$  to  $2004$ ). The upper and lower lines indicate the boundaries of the central 50% of slopes. (These are computed as follows; given  $\sigma_{kt}$  as the standard deviation of the  $u_{kjt}$  terms, the 50% bounds are  $\gamma_{k0t} \pm (2^{1/2})\sigma_{kt}$ .) We see some sort of pulse, as the means tend to go up and down, and the bounds in and out, like an accordion. This is clearly due to the difference between presidential and mid-term elections. A presidential election brings less interested voters to the polls, who also vote for congress. Further, even interested voters are likely to link their vote for congress with their

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<sup>7</sup>. This also may facilitate convergence in some programs.

presidential vote. Hence it is reasonable that we find that there is less variance across localities for congressional vote in presidential years than there is in mid-term years.

<<Figure 1 about here>>

Figure 2 separates these two sets of years; we see evidence of a change in the average slope over time, but not a clear trend. It might be that for presidential years there has been a decreased variation over time in congressional vote, but even this is uncertain. Figure 3 gives a similar separate presentation of trends in education. Here again we see little evidence of increased nationalization. Finally, Figure 4 presents the effects of race. Here there *does* seem to be evidence of decreased national variation over time, especially in presidential years.

<<Figures 2-4 about here>>

It is reasonable to imagine that this is due to changes in Southern politics, as the white “solid south” support for the Democratic party changed over the years. In terms of elections for the House of Representatives, there was a notable increase in the proportion of Southern seats going to Republicans between 1962 and 1968, and again between 1992 and 1996 (Jacobson 2000: 17, 21f, 29, 32).<sup>8</sup> This meant that there was increased nationalization in the sense that local votes (for congress) were, after 1994, more closely correlated with national votes (for President)—while for quite some time, a substantially greater proportion of Southern whites had voted for Democratic representatives than voted for Democratic presidential candidates, by 1994 this difference had closed to zero (Bullock 2000: 39, 41, 52).

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<sup>8</sup>. This change also led to fewer uncontested seats (which until 1992 were generally those of Democratic incumbents), which may also affect our results in ways we haven’t yet thought about. (That is, we need to see how HLM deals with clusters with a uniform dependent variable; it certainly throws them out for some things.) We would need to replicate removing uncontested districts. Related issues involve redistricting that, for some time, made districts with extremely high concentrations of blacks, later deemed to be unconstitutional.

Unfortunately, because we have so few observations of blacks in the South voting republican, we are unable to simply take South into account as a level-2 variable. Instead, we here replicate the analysis, but only looking at congressional districts other than those in the previous confederacy. Figure 5 shows the replication for the race effect in the non-South. It does not appear that this decreased variation was simply due to the South converging with the North, for we see it in the North as well. (This surprising indifference of the result to the exclusion of the South was also found by Brady, D’Onofrio and Fiorina [2000: 145] in their study of nationalization using different methods). Thus it seems that there has been an increased understanding of the racial division between parties in the US over this time.

<<Figure 5 about here>>

Finally, Figure 6 graphs the variance in the constants, and Figure 7 replicates this for the Non-South districts. Recall that these constants might be, with suitable caution, interpreted as reflecting the degree of homogenization in vote shares adjusting for compositional differences across localities. Overall, there seems to no tendency towards increased nationalization—rather, there is a de-nationalization in the 1980s.

<<Figures 6, 7 about here>>

These diagrams give us an intuitive understanding of the basic model and its results, but they do not provide a clear test of nationalization. We go on to use a modified HLM framework to provide such a test.

### *Simplified Presentation.*

HLM creates the random slopes through a set of parameters giving the variance/covariance matrix of the random effects, the tau matrix, defined as follows:

$$\tau_{ab} = \text{cov}(u_{aj}, u_{bj}) \quad (6)$$

where  $a, b \in \{0 \dots Q\}$ , where  $Q$  is the number of random slopes (note that we here suppress the time subscript for ease of expression). Hence

$$\tau_{aa} = \text{var}(u_{aj}) \quad (7)$$

It is important to emphasize that this variance is not empirically observed, but rather is a parameter estimated from the data in order to best re-create observed patterns. Thus there is a Fisher information matrix pertaining to the way in which the pseudolikelihood responds to joint changes in these different parameters.<sup>9</sup> Taking the inverse of this matrix leads to a new covariance matrix,  $I$ , as follows:

$$I_{ab,cd} = \text{cov}(\tau_{ab}, \tau_{cd}) \quad (8)$$

Thus the standard error for any variance can be computed from this matrix, returned by HLM.

For the sake of parsimony, we will notate the standard error for the  $k^{\text{th}}$  variable at the  $t^{\text{th}}$  time as  $s_{kt}$ .

$$s_{kt} = \sqrt{I_{kk,kk}} \quad (9)$$

where the  $t$  subscript is suppressed on the right hand side for consistency with previous expressions.

Because  $\tau$  is likely to be skewed, we take its logarithm, which, for the  $k^{\text{th}}$  variable at the  $t^{\text{th}}$  time we denote  $d_{kt}$ . (As we are now only looking at the variances for a single item as opposed to covariances, we henceforward subscript our  $\tau$ 's with a single subscript for the item and a time

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<sup>9</sup>. Because the Laplace method does not use the information matrix, HLM does not produce this output from the Laplace estimates; accordingly, we use the information matrix produced by the penalized quasi-likelihood (PQL) method, which produced estimates closely agreeing with those derived from the Laplace method.

subscript, hence  $\tau_{kt}$ ). We are then interested in the variance of this transformed parameter, which is

$$V_{kt} = \left( \frac{s_{kt}}{\tau_{kt}} \right)^2 \quad (10)$$

(This is because if  $y = \log(x)$ , then  $\text{var}(y) = \text{var}(x)/x^2$ .)

We use HLM 6.0 to estimate a model in which we use these  $V_{kt}$ 's to supply a known level-1 variance (see Raudenbush, Bryk, Cheong, Congdon and du Toit 2004: 189). Our level 1 model takes the form of

$$d_{kt} = q_{kt} + e_{kt} \quad (11)$$

$$e_{kt} \sim N(0, V_{kt}) \quad (12)$$

where  $q_{kt}$  is our key level-2 expression which we model as follows:

$$q_{kt} = \alpha_{0kt} + \beta_{kt}P_t + \varphi_k t + u_{kt} \quad (13)$$

$$u_{kt} \sim N(0, \tau_{kt}) \quad (14)$$

where  $P_t$  is an indicator variable = 1 if  $t$  was a presidential year and 0 otherwise and  $\phi_k$  is a parameter indicating the time trend. The statistical significance of  $\phi_k$  can be used as a test of the thesis of nationalization; if  $\phi_k$  is significantly less than zero, then we must conclude that we find increasing national consensus regarding the implications of the  $k^{\text{th}}$  cleavage for political sides. Of course, we can also add quadratic and cubic time trends for a greater fit if we so choose.

In other words, we know from the result of our previous model what the variance is on our estimates of the slope variances, and we say that the true value of the logged variance is some unobserved quantity  $q_{kt}$ , plus the error (of which we already know the mean and variance as this was part of our fitted model). This allows us to present a smoothed plot that includes presidential and non-presidential years together.

Finally, we noted above that there might be reason to imagine that the variance would be related to the mean slope, although our preliminary graphs did not suggest that this was likely to be a problem. We can attempt to control for the effects of changes in the mean by fitting the model:

$$q_{kt} = \alpha_{0kt} + \beta_{kt}P_t + \rho_{kt}\ln(\gamma_{k0t}) + \varphi_k t + u_{kt} \quad (15)$$

$$u_{kt} \sim N(0, \tau_{kt}) \quad (16)$$

where  $\gamma_{k0t}$  is the mean slope from equations (1-3) and  $\rho_{kt}$  is an estimated parameter tapping the relationship between mean slope and variance.

**Note: These analyses are in progress. Sorry that we made you read through all those equations, but they'll be here by Monday!** Figure 8 (which does not yet exist) shows the trends in the variances coming from equations (not sure yet) which adjust for presidential year (AND POSSIBLY MEAN). We see LITTLE EVIDENCE OF A TREND / SUGGESTIVE EVIDENCE OF A TREND / CLEAR EVIDNCE OF A TREND. Table 1 (also not yet existent) contains models that incorporate a specified time trend. (This will have two models for every dependent variable, most probably.) As we can see, the time trend (linear or quadratic) IS / IS NOT statistically significant.

<<Figure 8, Table 1 about here>>

In sum, we find only moderate evidence of an increasing trend towards nationalization, and this may be in regard to race increasingly making the same difference across localities. But even here, the temporal story is uneven and possibly restricted to the most recent periods.

HOW LOCAL IS POLITICS?

We have not found evidence of an increasing nationalization of politics. It might be that there is no trend towards nationalization: politics is and always was a local affair. Certainly the received wisdom was that over the 20<sup>th</sup> century House elections were increasingly driven by local factors—especially incumbency and personality effects (see Brady, D’Onofrio and Fiorina 2000: 131). In the above analyses, we allowed for all slopes to vary across districts—we did not test whether or not such a relaxation was even necessary. The conventional approach to testing the need for such a relaxation would be to fit the restricted model that constrains the slopes to be equal, and then compare the degree of fit, using a chi-square statistic. However, a peculiarity of HLM models is that there is unlikely to be a great difference in overall fit between the two models (Snijders and Bosker 2000:104f). Testing for random slopes in logistic models is even more tenuous (Snijders and Bosker 2000: 221). So we go on to conduct a different sort of analysis.

For any year, the overall sample  $N$  can be decomposed into  $M$  congressional districts, each with a sample size of  $N_j$ . First, we run our logistic regression in each of the  $M$  districts. Then, for each of our  $K$  independent variables, we compute the divergence of slopes across districts. Here we take as our measure the range of the central 50% of observations; because logistic regression parameters can easily go towards infinity in small samples, the extreme values lead to incomparability if we were to use the standard deviation.<sup>10</sup> Keeping track of these observed dispersions, we then place them in a constructed probability distribution.

To make this distribution, we take our complete sample, and randomly sort the respondents into artificial districts, so that we end up with  $M$  imaginary congressional districts

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<sup>10</sup>. Further, we also throw out the single largest value for any variable; this was a choice made after inspection of the results of various trials, some of which could have extreme values in the central 50% of cases. This had the effect of stabilizing the estimates without throwing out too much of the data. Our results are not dependent on this choice, however.

with the same distribution of  $N_j$  as in our observed data. We then carry out the same procedure to determine the dispersion that we had previously done for the observed data, and keep track of this value for each of our independent variables. We then go on to construct another such artificial data set, and another, and another. With 500 data sets, we have a good idea of where our observed dispersion sits on this probability distribution. If the observed dispersion is at the tail of this distribution, we conclude that it is very unlikely that we would have observed a dispersion this large or larger were we actually sampling from a homogeneous national population. But if the observed value sits squarely within this distribution, we cannot reject the hypothesis that politics is *not* local.

Figure 9 gives an example of the output for one year for four variables (education, age, race and gender). The vertical line is the observed value, and the histogram indicates the constructed distribution. In this example, we see that there is strong evidence that the variation in what gender meant across district was greater than would be expected under chance, but for the other variables, there is little evidence of localism.

<<Figure 9 about here>>

Figure 10 gives a simple linear average of these numbers over the coefficients for gender, age, race, income and education. Although this number may not have a rigorous interpretation, as a rule of thumb we see, first, no clear trend, and second, that although the values tend to be greater than .5 as opposed to less than .5, and hence are on the higher end of the distribution, they are generally far from tail (here .95 would indicate that the observed values are greater than 95% of the permuted distribution).

<<Figure 10 about here>>

Another way of looking at these results is to simply count the number of times, for any variable, the observed dispersion was at some level of significance. Figure 11 puts a mark for each variable any time its dispersion was greater than 90% of that expected by chance. It might seem interesting that four times, income was outside this expected range (and hence seems to be significant), but given 23 trials (our number of elections), one would expect 4 or more “hits” given a .10 probability around 19% of the time, so this is not very impressive. Only race demonstrates a clear local organization beyond what is expected by chance (the probability of 11 significant findings by chance is .000004). But it is worth emphasizing that these coefficients for race were the most difficult to estimate, given the skewed nature of the independent variable and its strong association with the dependent variable. Finally, in terms of a time trend, there is no evidence that the most recent periods are the ones in which there is less evidence of local voting.

<<Figures 11, 12 about here>>

Figure 12 replicates this analysis, but for presidential vote (and hence has fewer years). In this case, there is evidence that income is the variable we would accept as being non-local: although both education and race have three hits, this is expected around 13.4% of time, while income’s five hits would only occur 0.65% of the time by chance. Of course, we have done multiple tests, so we must be somewhat cautious in any particular interpretation. But it seems plausible that income is the effect most likely to vary across place—an interesting confirmation of Gellman’s recent arguments.

Finally, we have replicated these analyses using political ideology (liberal-conservative) as our dependent variable, and again find little evidence of localism. (There is frequently greater-than-expected dispersion of race coefficients; even though these are OLS regressions and hence should be less problematic than logistic regressions, we find that the observed coefficients,

though usually much more dispersed than expected, are sometimes much *less* dispersed than expected, and hence we are not convinced that this is a strong finding). It is not, then, that we have failed to find evidence of increasing nationalization over the second half of the twentieth century because politics always was, always will be, and always is local. Instead, it appears that by the start of our period American politics was already strongly nationalized. The fundamental political cleavages associated with the master statuses of education, income and race were pretty much the same everywhere—even in the South.

## CONCLUSIONS

In the second half of the twentieth century, American society underwent incredible upheavals. The rise of the cold war led to an intertwining of domestic and international politics. The breakup of the once “solid South” led to the final demise of the last clear-cut regional basis for national politics. The spread of televisions with networks airing the same programs and the increasing extinction of local newspapers led to an increasingly shared basis for political information across the country. And so on.

Yet American politics did not, over this period, dramatically increase in its national orientation of cleavages, and this is not because our data or measures are too crude to pick up any changes that might have occurred. It is because American politics was already nationalized. Of course, there were—and still are—exceptions: there are, as inhabitants of many university towns will tell you, places in which increasing income makes you more, not less, likely to vote Democratic. But there are actually no more of these exceptions than one would expect, given the size of the country. Overall, the pattern of social cleavages that we have examined has been fully nationalized for over half a century. It is interesting that this is in line with Caramani’s

(2004: 81, 230) arguments regarding European politics: contrary to what many others imagined, by the first world war, political systems were high nationalized, even if the parties were not yet mass parties.

This finding may seem to be quite at odds with Gelman et al's (2007) recent work; it is worth emphasizing the differences in question that they asked and we asked. First, they examined presidential voting—our interest in the possibility of local politics has led us, like others examining nationalization of politics, to examine congressional elections. Second, they relied on exit poll data in addition to the NES, giving them far more data points and increased sensitivity to small effects. That said, the trends they identified were not small in substantive terms. Third, their approach is deliberately descriptive, focusing on income. By looking at income and education in the same analysis, it is possible that our analyses of the same data would diverge due to changes in the relationship between income and education over time: this is especially weighty given the increasing education and labor force participation of women over this period—depending on how one treats the income of non-working married women, one can come to very different results.

Most importantly, they examined the state level, while we examined the congressional district. They were interested in determining whether certain types of states operated in certain ways; we have been examining the total degree of localism. It is entirely possible for there to be no localism in politics, while cleavages in presidential voting vary by state. Indeed, this seems to be what the evidence tells us; although it is not entirely clear what this means, we can expect future studies to examine how congressional districts are embedded in states, and states in the nation.

Our results also differ from those that find that there has been a recent re-nationalization associated with the Republican take-over of the House of Representatives in 1994, whereby districts that tended to vote Republican at the Presidential level but Democrat at the Representative level now voted a straight Republican ticket (for example, a number of districts in the South) (here see Jacobson 2000). Our results do not contradict these, but they show a more fundamental continuity in the alignment of cleavages than we might imagine. And of course, our approach to nationalization—one that focuses on whether local cleavages are aligned in the same way—is different from those that examine spatial heterogeneity of party support or of changes in party support.

At the same time, our results are important for understanding nationalization in these other senses—for there can be local variation in party support while there is nationalization of alignments if different types of persons are differentially distributed across localities. Indeed, perhaps one of the interesting questions in the current political landscape is whether the nationalization of political alignments—the universal understanding of what education-net-of-income implies, say—is making possible a new type of geographical separation (see, e.g., Tiebout [1956] for the basic idea of preferential sorting; Bellah et al. [1985: 71-75] for a critique; Clark [2004] for a reconsideration). Our results suggest that the *structural* uniformity of American politics—the alignment of cleavages across locales—means that future net heterogeneity will depend on the distribution of persons who are disproportionately likely to be attracted to one party as opposed to the other. This is hardly revolutionary, yet it is not obviously the case.

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Table 1: Results from Model for Variances

To be added

Figure 1: Effect of Income

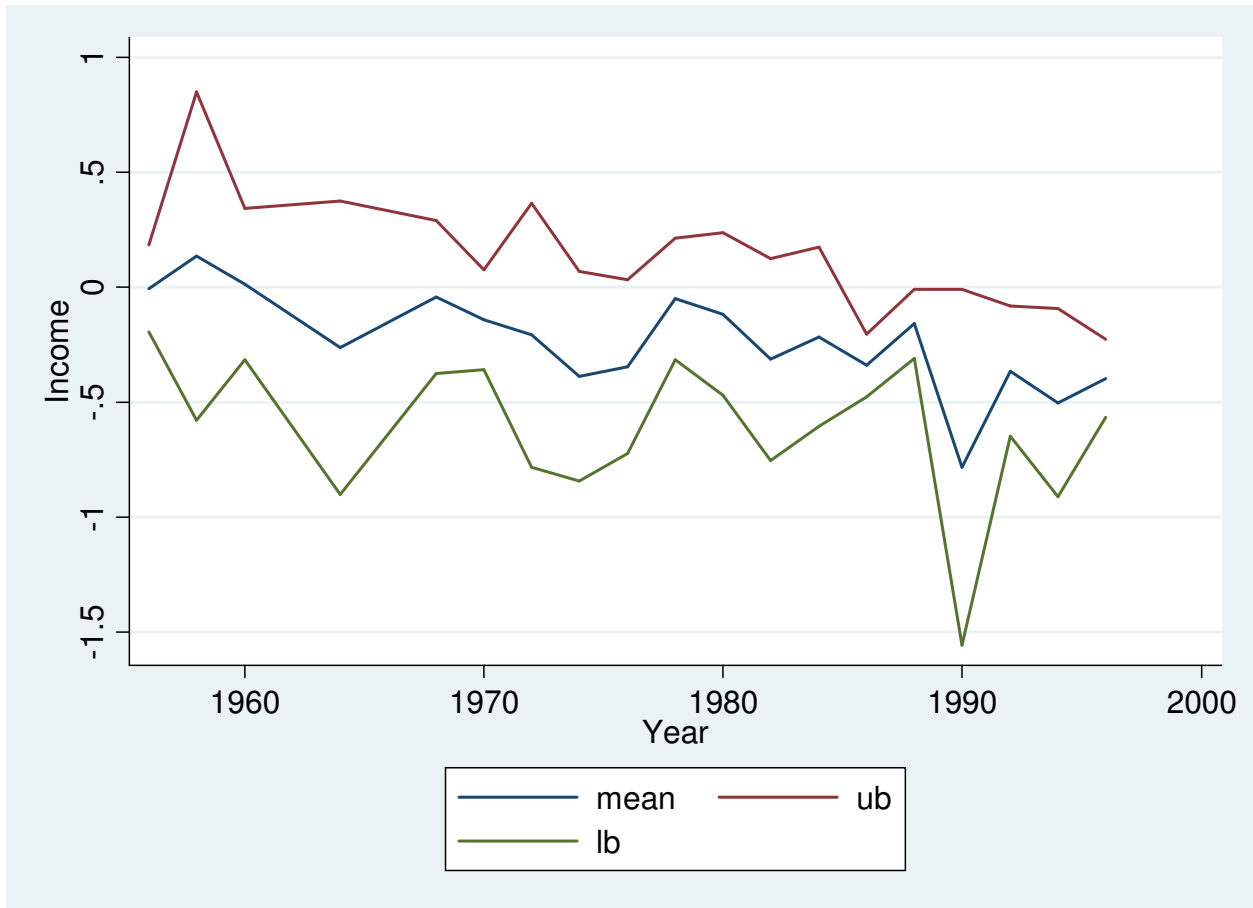


Figure 2: Income Effect Separated by Presidential and Non Presidential Years.

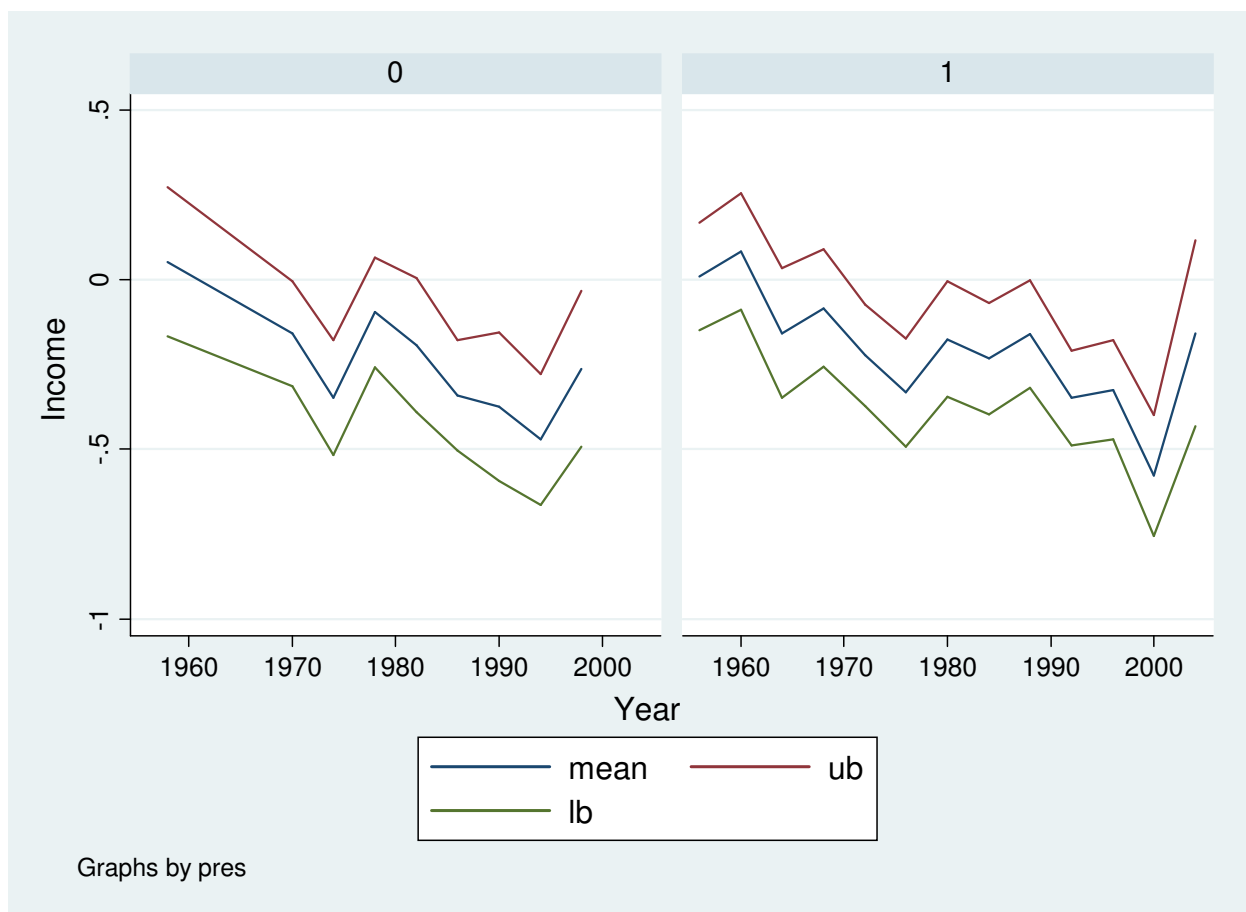


Figure 3: Effects of Education

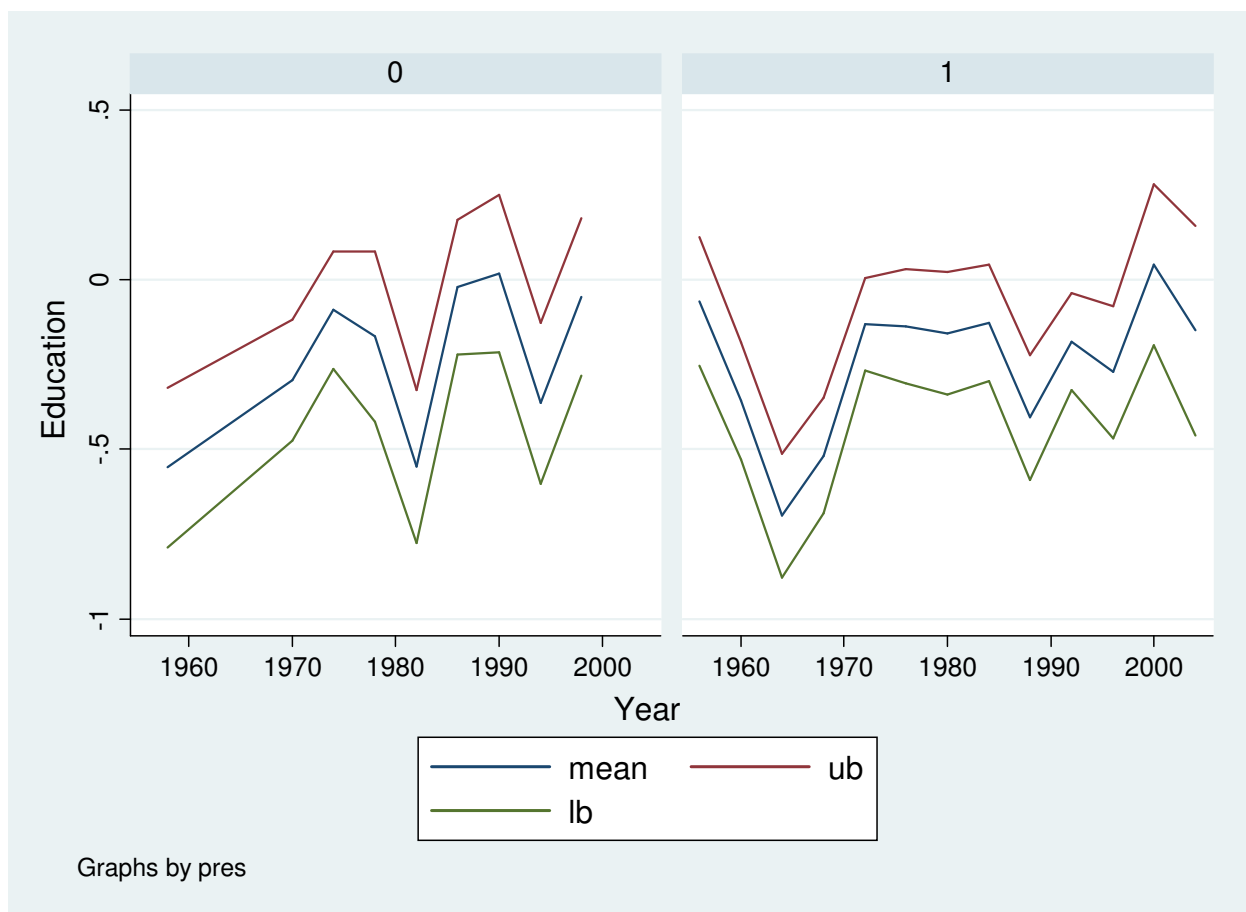


Figure 4: Effects of Race

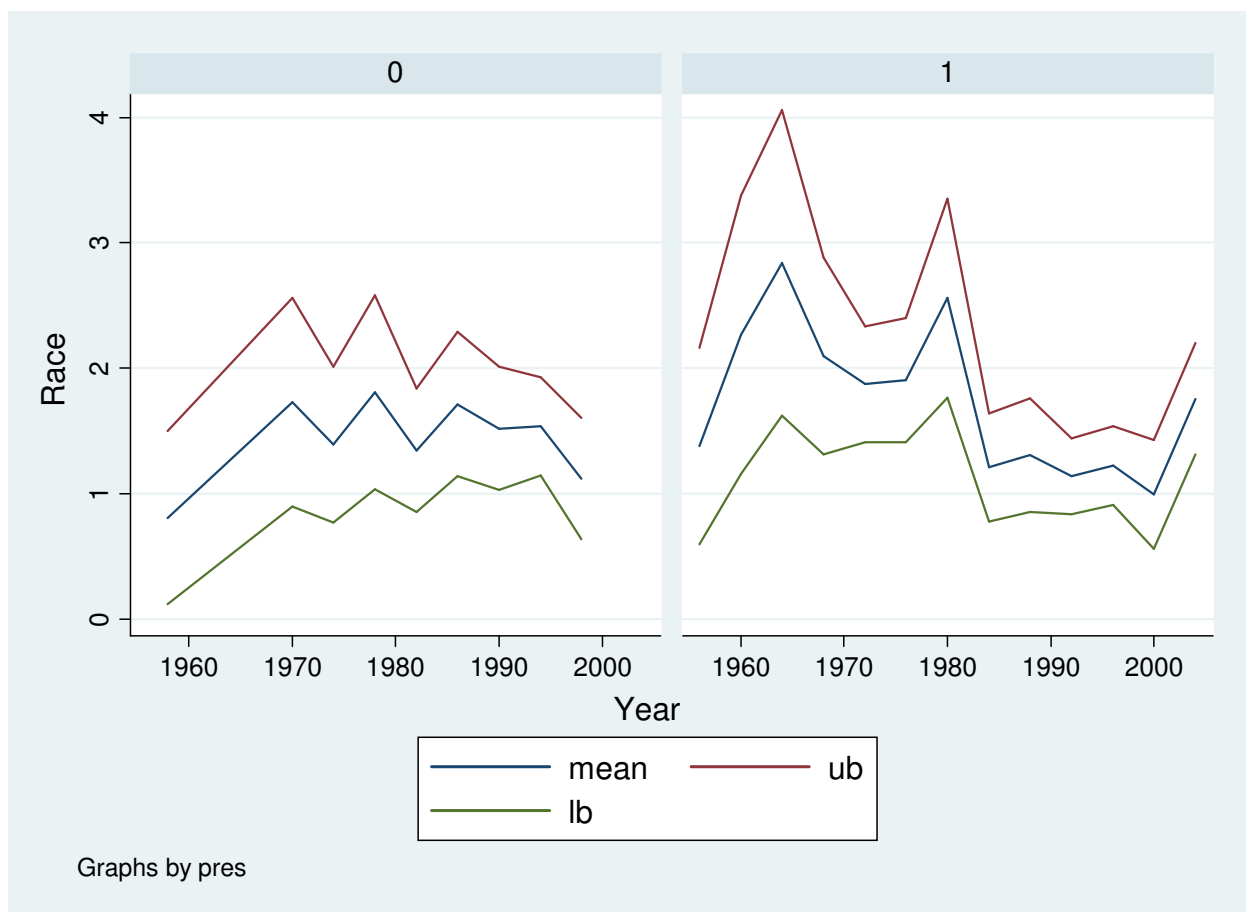


Figure 5: Effects of Race, Non-South Only

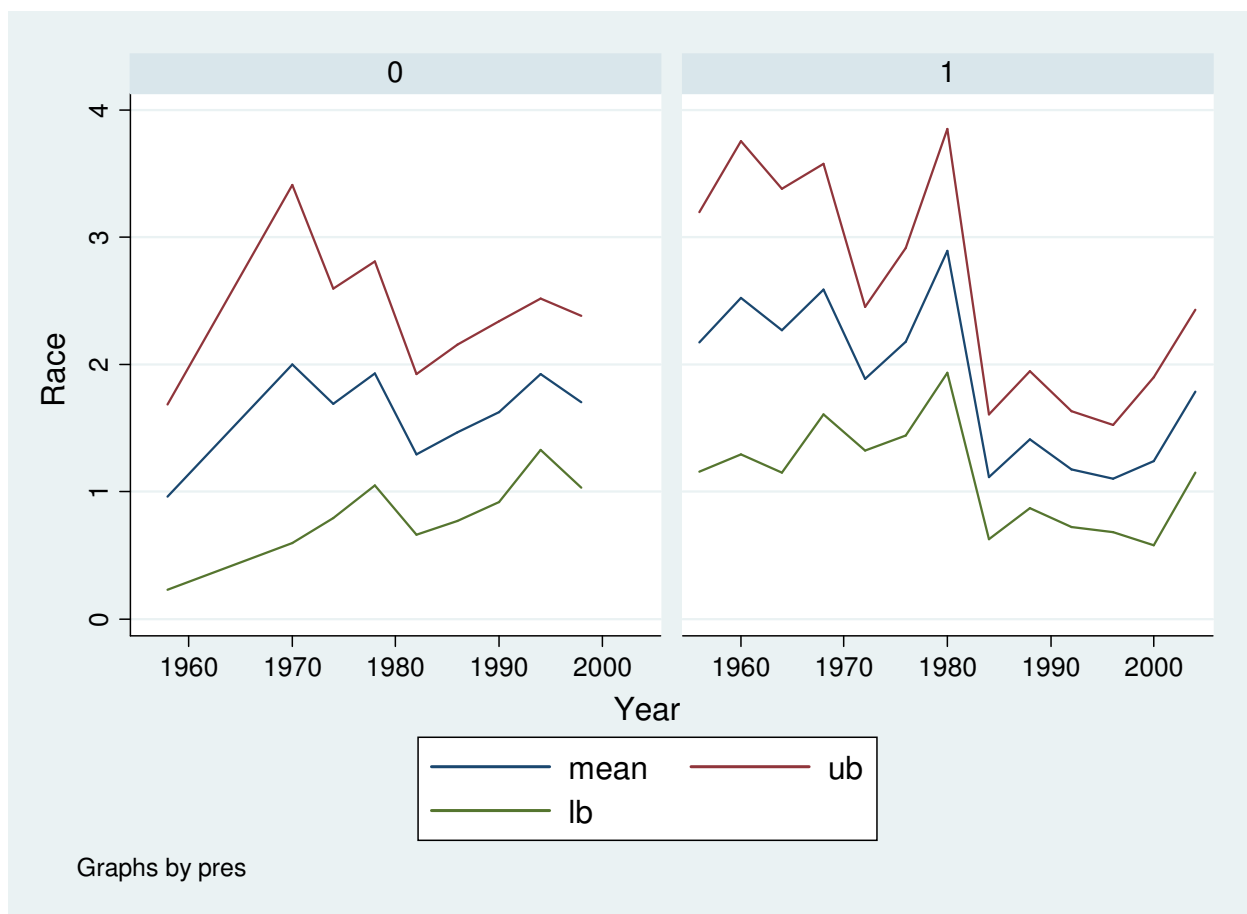


Figure 6 Dispersion of Constants (Residual District-Level Variation)



Figure 7 Dispersion of Constants, Non-South Only

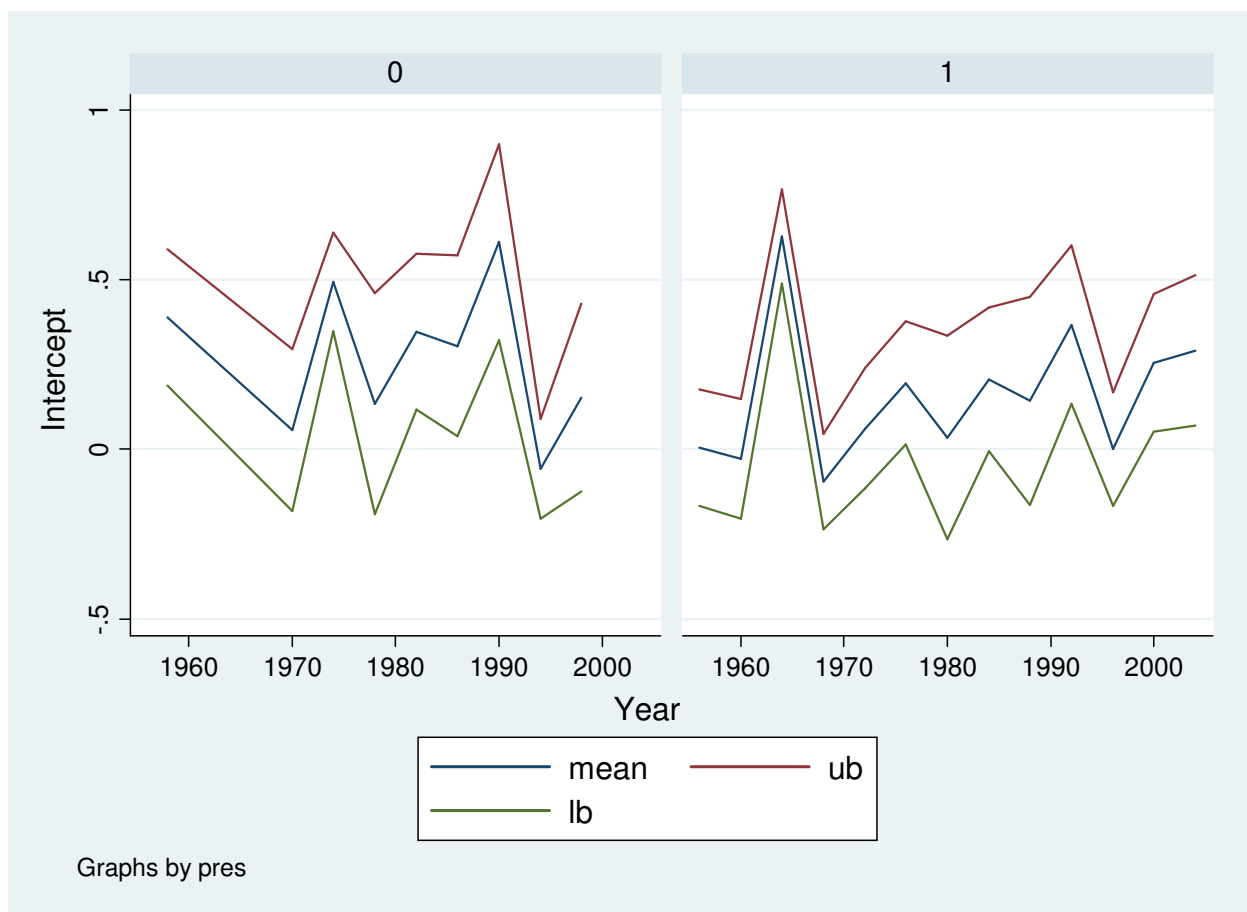


Figure 8: Trends in Variances

To be added.

Figure 9: Example of Permutation Results

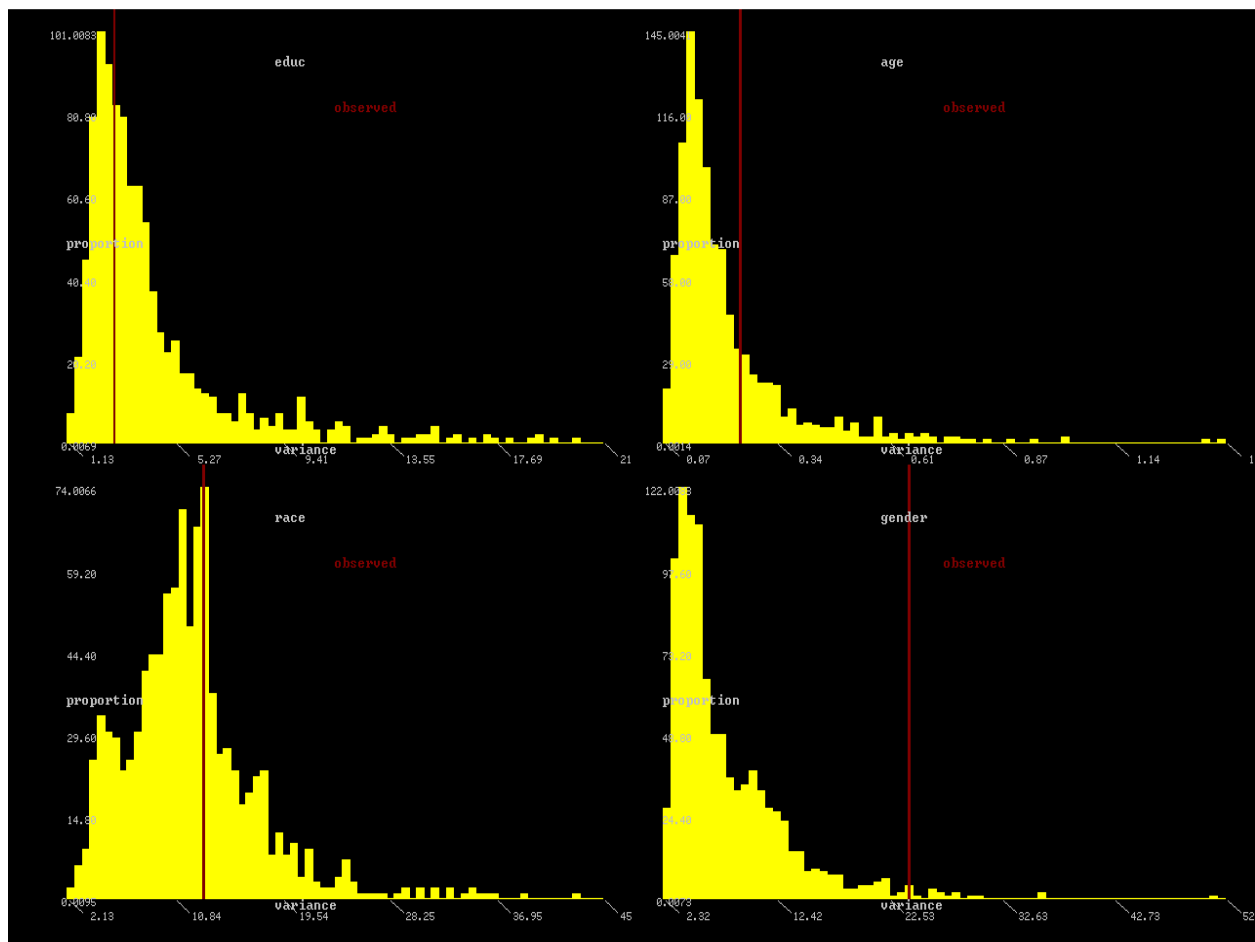


Figure 10: Average Position of Variables on Generated Probability Distribution

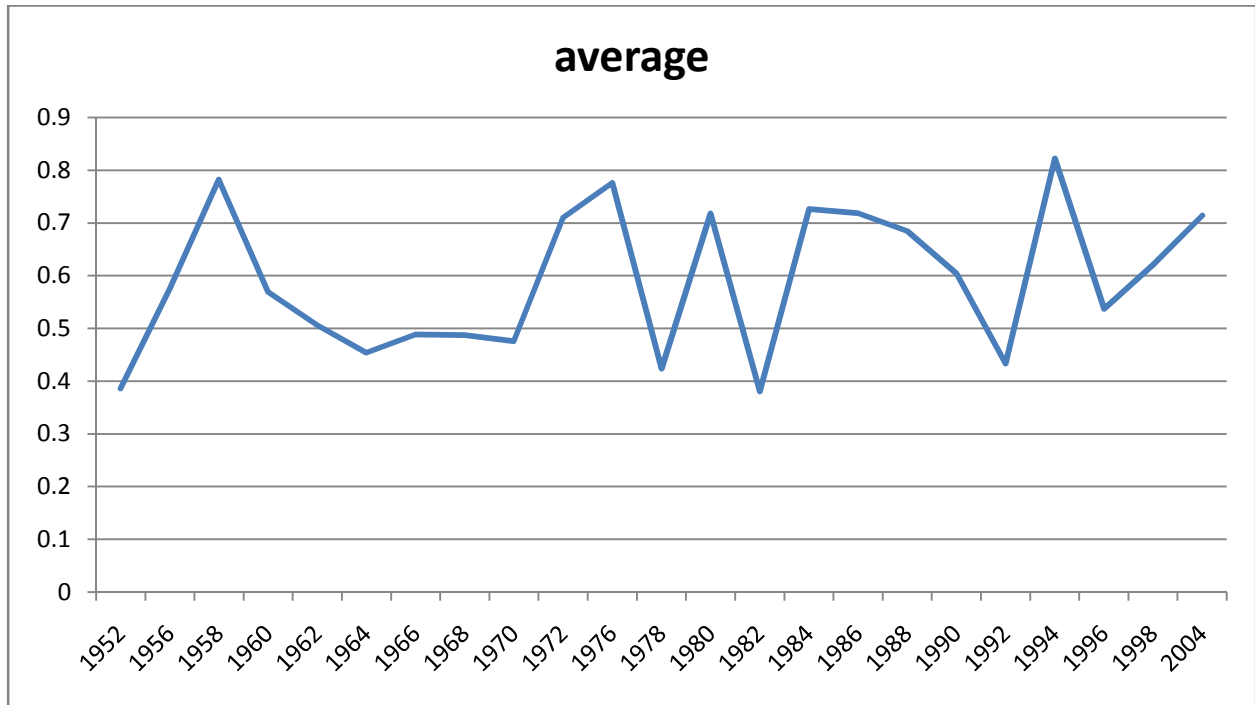


Figure 11: Evidence of Significant Localism, Congressional Vote

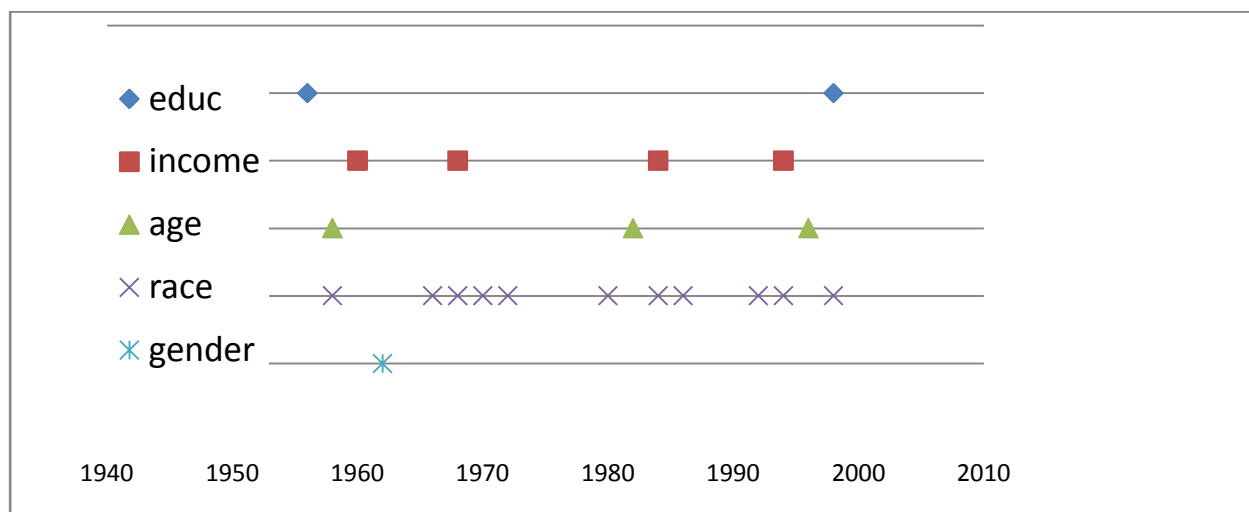


Figure 12: Evidence of Significant Localism, Presidential Vote

