Opinion Polarization: Important Contributions, Necessary Limitations

John H. Evans  
*University of California, Los Angeles*

Bethany Bryson  
*University of Virginia*

Paul DiMaggio  
*Princeton University*

In our 1996 article “Have Americans’ Social Attitudes Become More Polarized?” (*AJS* 102 [3]: 690–755) we addressed the substantive question of whether or not polarization on 18 morally charged social issues had increased over a period of approximately two decades. That article examined all of the then-available years of data from the General Social Survey (GSS) and the National Election Study (NES). The approach was necessarily broad, entailing more than 200 separate analyses of polarization within the population as a whole, within subgroups of the population and between groups. Moreover, each examination in turn required the use of three or four statistical models.

Our major conclusion was that little polarization had occurred over these years and that the American public was by many criteria more unified in its views in the 1990s than it had been in the 1970s. The most important exception to this generalization, based on analyses of data from both the NES and the GSS, was that the population’s attitudes toward abortion had been polarizing. The results of Mouw and Sobel’s analysis of the NES data suggest that attitudes toward abortion had not been polarizing, thus challenging the one qualification we made to our central conclusion.

We would like to thank Ted Mouw for sharing the Stata code for his procedure and for discussing it with us. We would also like to thank Rob Mare, Vincent Fu, and Steven Nock for close readings of a previous draft. Direct correspondence to Bethany Bryson, University of Virginia Department of Sociology, Post Office Box 400766, Charlottesville, Virginia 22904-4766. E-mail: bryson@virginia.edu

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argument that polarization had not increased. As we understand it, they did not question that central argument itself.

Other scholars working in parallel have developed analogous methods for measuring attitude polarization in the senses of dispersion (Gay, Ellison, and Powers 1996) and consolidation (Hout 1999). Hoffmann and Miller (1998) expanded on our 1996 article to create a method for adjusting variance and kurtosis measures that would control for changes in demographic variables.

According to Mouw and Sobel, we and other scholars who have examined polarization have erred in treating ordinal scales as if they were interval scales. Statisticians have many concerns about this practice, but the major problem, as Mouw and Sobel note, is that we do not know if the distance between 1 and 2 on the scale is the same as the distance between 3 and 4. Mouw and Sobel offer a solution to this problem for one of the four dimensions of polarization we discussed, dispersion, and analyze one of the 18 issues we examined, abortion (based on one of the two scales we used to examine it). Additionally, they criticize us for combining two different abortion questions in the NES into one time series, and they make the substantive claim that polarization over abortion attitudes in the general population has remained unchanged from 1980 to 1993, the last year in our time series. We respond to their methodological and substantive claims separately.

To anticipate the essence of our comments, we welcome Mouw and Sobel’s approach as a valuable new tool for students of polarization. We raise some cautionary points of interpretation, but even in pointing to limitations in the application of the method we call attention to and provide examples of additional ways in which it can be valuable. At the same time, we emphasize that the method only addresses one of our four indicators of polarization in attitudes toward abortion. Moreover, using Mouw and Sobel’s method and additional years of NES data, we find support for our substantive conclusions about even this one measure of polarization with evidence indicating that polarization in responses to the NES abortion item increased between 1980 and 1998.

THE METHODOLOGICAL CRITIQUE

We begin by emphasizing a significant point of agreement: Mouw and Sobel have made an important methodological contribution to the study of polarization. They propose a method for modeling changes in the variance of categorical data and argue that their method is more appropriate than the ordinary least squares (OLS) method we used. Like many methodological advances, however, theirs entails both costs and benefits. In
Comment on Mouw and Sobel

this section we first address their criticisms and then explore the advantages and limitations of using an ordered heteroscedastic probit analysis to measure attitude polarization. In the process, we discover a benefit of their method that Mouw and Sobel do not discuss: They are able to test for increasing variance in the overall spread of opinion while controlling for some forms of bimodality.2

Abortion Question Wording Change

Before we begin that longer discussion, we will address briefly a less complex methodological issue. Mouw and Sobel argue that we should not have combined the two abortion questions into one time series. Using an appropriate adjustment formula they reveal that our conclusions about the change in the mean and variance are unaffected by the change in wording, but that the decreasing kurtosis identified in our analysis may have been an artifact of the wording change.

Faced with the 1980 wording change, we compared responses before and after the change and found (as Mouw and Sobel did) that slightly fewer respondents chose middling responses with the new wording. We had two choices. First, we could shorten the time span of the data series (as Mouw and Sobel do), thus reducing the chance of finding significant results, both by decreasing the number of cases and by reducing the chronological scope of our analysis. Or we could combine the questions and report in our article that the change in question wording could inflate estimates of polarization. We chose the latter course, reporting the limitations of the data and the risk inherent in our decision in table 1 and footnote 13 (pp. 700, 702). If we had been testing polarization on this one issue with one population and one data set, we would have taken Mouw and Sobel’s approach. But ours was a broad analysis of many issues and, because we were skeptical of finding polarization, we tried in every case to give the polarization hypothesis the benefit of the doubt. Moreover, we were confident that analysis of the longer and better-constructed time series on abortion attitudes from the GSS would prevent us from drawing the wrong conclusions about polarization in abortion attitudes. We accept Mouw and Sobel’s point in the context of their single-issue analysis. To test whether there has been polarization over abortion, the best course of action is to separate the two questions.

Much of this section is taken from a longer discussion of Mouw and Sobel’s method (Bryson 2000).
American Journal of Sociology

Is This a Comprehensive Test of Polarization?

Let us now turn to the heart of Mouw and Sobel’s methodological critique and contribution. Our 1996 article identified four dimensions of polarization: dispersion, bimodality, constraint, and consolidation. Mouw and Sobel only attempt to measure dispersion (variance). They dismiss the value of identifying bimodality (kurtosis) on the grounds that their measure of dispersion also taps bimodal trends (see our 1996 article, pp. 695–96, for a critique of this view). Although they do not mention the constraint of opinion across issue domains (Cronbach’s alpha) or the consolidation of opinion parameters with structural parameters (group-level differences in means), they do discuss changes in means for one group over time. Such changes in a single group, however, do not indicate polarization. Finally, although they do not make this argument, Mouw and Sobel’s general critique could extend to our method of measuring consolidation by comparing group-level differences in means calculated from ordinal data.

In recent work on polarization between and within religious traditions in the United States, Evans (2000) used a method that addresses Mouw and Sobel’s implicit critique of the consolidation measure. Instead of estimating the mean for each year for each group under comparison, he used an ordered logistic regression model, with the abortion scale as the dependent variable. A group × year interaction term tested whether changes in response to the abortion question were significantly different for the two groups. This approach also made it possible to control for covariates that may shape opinions over time, as advocated by Hoffman and Miller (1998). He found that polarization in abortion attitudes (in the sense of consolidation) has occurred between mainline and evangelical Protestants since 1972.

The Conflation and Separation of Different Dimensions of Polarization

As Mouw and Sobel note, their method transforms data from the abortion question into a new variable that has more desirable qualities including a normal distribution. Because the kurtosis of a normal distribution is zero however, they are unable to model changes in kurtosis, an indicator of bimodality. This would appear to be a severe limitation on their method, particularly because bimodality (the clustering of opinion into distinct camps) is likely to be more closely related to political conflict than is dispersion. Moreover, by allowing the cut-points to shift, Mouw and Sobel’s method effectively absorbs increasing bimodality. To understand this claim, we must delve a bit farther into their method.

Probit is a regression model designed to work with ordinal or dichot-
Comment on Mouw and Sobel

omous dependent variables by transforming the observed data into a variable that meets the assumptions of regression. Whereas logistic regression—the most commonly used solution to this problem—turns the dependent variable into the log of the odds of an event, probit regression addresses the problem by taking the z-score of the probability to produce an unconstrained continuous variable. (Specifically, it calculates the cumulative normal probability of choosing the next higher category of \( Y \).

It happens that a z-score includes the mean and standard deviation of the (now continuous) variable in question: \( (x - \text{mean})/\text{SD} \). The important point here is that z-scores contain estimates of variance for the new dependent variable. This is the reason to choose probit rather than some other transformation.

Mouw and Sobel’s adapted probit model has the useful feature of making it possible to ascertain how well the model fits with prescribed changes in the variance across time \( (x) \). It does not focus our attention, as do most probit analyses, on the effect of \( x \) on \( y \). Instead, Mouw and Sobel assess the fit of a model that posits increasing variance over time, and compares the fit of that model to the fit of other models that hold variance constant. Alvarez and Brehm (1995) use this method (derived from Harvey 1976) to demonstrate that respondents who hold conflicting values on the role of women and the sanctity of human life vary more in their responses to abortion questions than respondents who hold consistent positions on those issues.

Mouw and Sobel estimate several of these probit models in order to test such assumptions as constant variance, increasing variance, increasing mean, and several combinations thereof, on the premise that the model with the best fit describes the data most accurately. If the best model were one that specified a linear change in variance, Mouw and Sobel would conclude that there had been a linear polarizing trend in underlying opinions. To better understand this point, consider the example of the NES abortion scale.

The observed variable, here called ABLAW, has four possible values: 1, 2, 3, and 4. The probit transformation begins with the probability of choosing each response category (represented by the percentage of respondents in each category). For example, the observed proportions of respondents in each category for 1980 were .115, .327, .190, and .368, respectively. To predict responses to multiple categories in one equation, the method standardizes the cumulative probability for each category, in this case, .115, .442, .632, and 1.00. Because such probabilities cannot range below 0 or above 1, probit standardizes the (cumulative) probability for each category using a z-score: \( (x - \text{mean})/\text{SD} \).

Think of the area under a normal curve as a histogram of sorts. A line down the middle of a normal curve would divide the area into two equal
parts (50% in each category). Adding two more lines would divide the
distribution into four categories, but equally spaced lines do not fit the
observed data. The left two categories together only account for 44.2% 
of responses, so the middle line should go to the left of center. Figure 1
represents the 1980 data as areas under a normal curve (mean = 0; SD
= 1). We divide the bell into four unequal pieces of 11.5%, 32.7%, 19.0%,
and 36.8%. To make the proportions match the 1980 data, we must draw
the lines (hereafter, “cutpoints”) dividing that bell in specific locations that
can be mathematically derived using the probability density function. This
is the sense in which the analysis fits parameters to match the data. Every
time the procedure “fits” a parameter, we lose a degree of freedom. In
this example, we specified two parameters (the mean and SD). Those two
parameters defined the scale and shape of the curve and the other three
parameters (the cutpoints) emerged to fit the data as proportional divisions
of the area under the curve. We could, however, choose to specify one
cutpoint and the mean. Then the other three parameters (two cutpoints
and the SD) could be identified from the data. This is the crux of Mouw
and Sobel’s approach. It allows probit to identify a meaningful standard
deviation from the observed data. Together, the five parameters inform
the equation that estimates latent opinion on abortion law.

![Figure 1](image)

**Fig. 1—A normalized histogram**

PROOF 6
Comment on Mouw and Sobel

Shifting Cutpoints Absorb Changes in Bimodality

Probit defines the latent variable as normal, forcing its shape to have zero skew and zero kurtosis. That transformation is usually acceptable, but it is fatal if the goal of an analysis is to identify change in kurtosis over time. The transformation is, furthermore, problematic for cases where the distribution has large tails or where the action occurs in the endpoints of an ordinal scale because the model’s assumptions do not fit the observed data as well as they might otherwise. The result would be a poorly fitting model that cannot identify changes in the skew or bimodality of a distribution across the independent variable. This issue is particularly salient for the survey question that Mouw and Sobel reexamine. Figure 2 presents a histogram of responses to the abortion question in 1980 and again in 1994. This is where Mouw and Sobel’s discussion of cutpoints becomes important.

As indicated above, Mouw and Sobel’s figure 1 can be interpreted as a bell-shaped histogram divided with three “cutpoints” into four portions matching the frequency distribution in the observed variable—11% of the area in the first section, 32% in the second, and so on. Mouw and Sobel’s cutpoints are not evenly spaced and symmetrical because the observed variable does not have a normal distribution. We may think of this procedure as, in effect, absorbing bimodality into a normal curve.

Over the course of the survey period, however, the proportion of responses in category four increased, making the observed distribution even less appropriate for standardization. Mouw and Sobel’s method accommodates this change by allowing the third cutpoint to move toward the center of the distribution after 1990—making room in the right tail for more pro-choice responses in the later part of the time period and thereby absorbing the change that produced decreasing kurtosis (increased bimodality) in our 1996 models. Indeed, the large improvement in fit that Mouw and Sobel report as a result of allowing cutpoint 3 to move toward the mean constitutes evidence of this polarization. Following the logic that an improvement in fit indicates a significant effect of changes in the model’s parameters, we conclude that there has been a significant shift in abortion opinion from category 3 (a middling opinion) to category 4 (an endpoint). Given that the leftmost category has not lost ground, we argue that the shift toward category 4 constitutes polarization of the sort measured by kurtosis.

When the leftmost or rightmost cutpoint moves toward the center, an endpoint of that distribution increases, and polarization may result if the opposite pole maintains its strength. If both tails increase, we can be more certain that the shift is not a shift toward agreement on a polar position (as would be the case for opinion on women’s rights over time; see fig. 3
Fig. 2.—Opinion on abortion law, 1980 and 1994
Comment on Mouw and Sobel

in our 1996 article). If both tails increase at the same rate, however, there may be no need to allow cutpoints to shift. A small or moderate evenly balanced shift toward the tails would merely produce increasing variance and can be modeled that way with Mouw and Sobel’s method. Unfortunately, a strong polarizing trend has the potential to empty the middle beyond the constraints of normality. That is, there could be so few responses in the middle category that 68% could not lie within one standard deviation of the mean at the same time that only 95% lie within two standard deviations of the mean. Therefore, the result of a strong trend toward the tails of a distribution would be a poorly fitting model rather than a significant increase in variance. This is not the case for the example at hand, but it is an unfortunate characteristic of the method that reduces its usefulness as a measure of polarization.

Although scholars of the pro-choice movement might debate the merits of Mouw and Sobel’s historical claim that a new abortion discourse appeared in the early 1990s, the observed shift in survey responses does appear suddenly and in conjunction with political changes rather than in a long gradual trend of eroding middle ground. It appears, then, that Mouw and Sobel have documented the same shift that we found in our 1996 article while offering an alternative explanation that fits the evidence more closely than does the linear view implicit in the culture wars hypothesis.

On the other hand, we cannot know whether their interpretation is correct. Mouw and Sobel state that it would be inappropriate to allow for a cutpoint shift unless the researcher is fairly certain that there has been a shift (using the current example) in the way respondents interpret the question. Their decision to allow a cutpoint shift thus assumes that the hypothetical respondents who would answer category 3 in earlier years but switch to category 4 after 1992 would do so not because their underlying opinion had changed but because their interpretation of the question had changed such that they felt a different category better matched their unchanging opinion. In short, they argue that the relevant indicators changed because people changed the way they expressed a given attitude without necessarily changing the attitude itself.

For our question, however, the theoretical interpretation of that change is irrelevant. We did not ask whether interpretations had changed over time, we asked whether the pattern of responses had changed, regardless of the meaning one might attach to survey questions or their response categories. It is entirely possible that political contexts changed the way respondents interpreted questions about abortion, producing an “artificial” polarization. But it is also possible that people changed their opinions and that a deeper moral rift appeared in attitudes toward abortion law. The data and methods before us cannot determine whether either of these
assumptions is true. We can only know that more people chose category 4 in later years than in earlier years and that there had not been any erosion in the popularity of the opposite pole.

We suggest, however, that there is an important unremarked benefit to modeling cutpoint shifts and changes in the mean of a dependent variable as Mouw and Sobel do. If cutpoint changes can be interpreted as indicators of polarization, Mouw and Sobel’s method offers the useful feature of making it possible to distinguish the trend in dispersion from the trends in both means and kurtosis we observed in our 1996 article. In their analysis, an improved fit with changing cutpoints indicates and, in effect, controls for the change that produced decreasing kurtosis. From this perspective, then, the comparison of their $M_3$ and $M_4$ demonstrates that defining a linear change in variance does not significantly improve the fit of the model, when controlling for the shift from category 3 to category 4 (e.g., an increase in bimodality). This method, furthermore, allows the researcher to test for changes in variance under other assumptions such as constant, fitted, or linear trends in the mean (another byproduct of the increased size of category 4). Because the increasing proportion of respondents selecting the pro-choice category is clearly specified, tested and controlled, the result is a clearer view of changes in the NES measure of opinion toward abortion law between 1980 and 1994 than the one we presented in “Have Americans’ Attitudes Become More Polarized?”

SUBSTANTIVE CRITIQUE OF FINDINGS ON POLARIZATION OVER ABORTION

Beyond their methodological innovation, Mouw and Sobel also offer a substantive intervention into debates about polarization in the United States, joining a large number of scholars who have worked on this very question (DiMaggio et al. 1996; Hunter 1991, 1994; Williams 1997; Evans 1996, 1997; DiMaggio and Bryson, in press; Hoffmann and Miller 1998; Gay et al. 1996; Hout 1999; Davis and Robinson 1996). Although we welcome the methodological innovations that lead to this conclusion, and although Mouw and Sobel’s no-polarization conclusions, if accurate, would strengthen the underlying argument of our 1996 article as a whole, we cannot accept that conclusion for several reasons.

First, as Mouw and Sobel note, their method has not yet been extended to deal with additive scales of the kind derived from the GSS. Our 1996 conclusions about polarization of abortion attitudes relied primarily on GSS results, and are therefore in no way undermined by Mouw and Sobel’s analysis.

Mouw and Sobel do imply that our GSS results are incorrect because
the NES variable is an ordinal measure and the GSS scale may fail to meet even this standard. The problem of using what appears to be an ordinal dependent variable is resolved in the operationalization used in our article. Rather than treating response categories as meaningful positions “in relation to some theoretical maxim” (p. 693), we asked whether the distribution of responses had changed over time. For that purpose, meaningful estimates of mean, variance, and kurtosis only require that the data be unidimensional. Although we did not consider the meanings that survey respondents may have attached to response categories, scholars who wish to do so would find our methods inappropriate, as Mouw and Sobel indicate, which is one of many reasons that we consider their approach a useful complement to our own.

Furthermore, there is a long and well-documented debate over whether the Rossi abortion scale in the GSS is bidimensional (e.g. whether three questions represent a latent attitude toward “hard” cases, and three toward “easy” cases of abortion morality decisions; see, e.g., Clogg and Sawyer 1981; Duncan, Sloane, and Brody 1982; Muthen 1982; Gillespie, Vergert, and Kingma 1987, 1988). Whereas in the past many scholars used the six questions to create a Guttman scale, more recently researchers have created additive scales as we did (see, e.g., Hout 1999). Adjudicating this debate over scaling goes beyond the scope of this comment, but the research indicates that the Rossi scale is indeed unidimensional and, while not a Likert scale, that it is appropriate for use in the conventional manner (Gillespie et al. 1987, 1988). In other words, the most important evidence that we presented for the polarization of abortion attitudes, evidence from the GSS, remains compelling.

Second, Mouw and Sobel have not challenged our conclusions about two of the four dimensions on which we measured (and found) opinion polarization over abortion. We have demonstrated, as have other researchers (Hout 1999), that intergroup differences in attitudes toward abortion have increased over time, indicating a form of polarization we called consolidation. Similarly, we found a significant increase in ideological cohesion among the items used to form the GSS abortion scale, indicating an increase in the aspect of polarization we called constraint.

Third, although we accept Mouw and Sobel’s conclusion that there was no linear trend in the dispersion of NES abortion attitudes from 1980 to 1994, we have applied Mouw and Sobel’s method to the NES abortion question from 1980 to 1998, bringing the data series up to date and separating the effects of changes in variance from the pro-choice shift that produced declining kurtosis in our article. Because Ted Mouw graciously shared his Stata code with us, we were able to apply their method precisely and find evidence of an increase in variance even when controlling for the cutpoint shift.
We first ran a model (EBD1, reported in table 1) with constant variance, constant cut points, and fitted means as a baseline comparison. This model does not fit the data well. What about change in dispersion (variance) without a shifting cutpoint? We fit EBD2, which holds the cutpoints constant, but allows the variance to change linearly (their $M_2$). EBD2 fits much better than EBD1, but not well enough for further consideration. We concur with Mouw and Sobel that there does not appear to be any linear increase in variance without also modeling a change in cut point.

Has there been a nonlinear change in variance? One of the important innovations in Mouw and Sobel’s method is the ease with which nonlinear changes in variance can be modeled. In our article, we hypothesized increasing polarization as linear and monotonic not for any theoretical reason, but due to limitations of available data and methods. After rejecting the hypothesis of a linear change in variance, Mouw and Sobel examined the variance in each year individually in their model 1 ($M_1$), which suggested that there were three distinct variances: $\sigma^2 = 1980, 1984, \text{and } 1988; \sigma^2 = 1986; \text{and } \sigma^2 = 1990, 1992, \text{and } 1994$. The result was their $M_{10}$, where the variance from 1990 to 1994 differed from the variance in the 1980s (excepting 1986). Mouw and Sobel reported that while the coefficient for the difference in variance in the 1990s is highly significant, suggesting polarization, they dismiss the model because it fits the data poorly.

Adding the 1996 and 1998 data to the series changes the perception of where the distinct variance eras begin and end. Our equivalent to Mouw

<table>
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<th>$P$</th>
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and Sobel’s $M_1$ (available upon request) suggested that the discontinuous change in variance occurred in 1992, not 1990. We ran model EBD3 to determine whether there was a nonlinear increase in variance when not modeling the shifting cutpoint. Although the model improved upon EBD1, it does not fit within conventional standards. Thus, our findings support Mouw and Sobel’s conclusion that there has been no overall trend in dispersion without modeling the change in cutpoint.

However, another strength of their method is the ability to disentangle generalized dispersion from a specific move toward one end of a distribution. Assuming that Mouw and Sobel are correct that the cutpoint shift occurred in 1990 we fit the equivalent of their model $M_3$ (EBD4), which allows for a change in cutpoint after 1990 but holds variance constant. This fit significantly better than EBD1, suggesting that after 1990 there was a move from the center to the most liberal category. This finding is also consistent with that of Mouw and Sobel.

To approximate the linear methods that we used in our 1996 article, we can also ask whether variance increases in a linear fashion after modeling, and thus controlling for, the shifting cutpoint. Examining EBD5 reveals that the answer is no.

Finally, we ask whether or not there is a nonlinear change in variance, given the change in cut point. We created a model (EBD6) with fitted means, a cutpoint shift in 1990, and a nonlinear increase in variance from

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1992 on. The data fits the model adequately, using the same fit standard as Mouw and Sobel, and the change in variance is almost significant at the .10 level we used in our original paper ($P = .105$) (see table 2). This is weak evidence of increased dispersion in attitudes over abortion from 1992 to present, assuming a change in cutpoint after 1990.

In an effort to be as fair as possible to our earlier thesis, Mouw and Sobel created a model with the slightly weaker claim that dispersion is greater after 1990 than in earlier years except 1986 ($M_{10}$). When we control for 1986 in EBD6, the result is EBD7 (see table 3). The model fits the data quite well: the individual coefficient for the variances after 1992 is significant ($P < .05$) and the conditional likelihood test of difference between EBD7 and EBD4 is also significant ($P < .01$). Thus, if we too make concessions to 1986 as an exception in the earlier period, there is evidence that attitudes over abortion are more dispersed from 1992 to present than in previous years, assuming a change in cutpoint after 1990. This is suggestive of increased dispersion in addition to the shift toward the pro-choice pole that we measured with kurtosis in our earlier paper. In sum, even when the change modeled by the shifting cutpoint is controlled, there seems to have been increased dispersion in abortion between 1980 and 1998.
Comment on Mouw and Sobel

CONCLUSION

In sum, we welcome the advance in understanding that Mouw and Sobel’s innovations provide. Using their method to model the shift to one end of the distribution captured by the kurtosis statistic in our 1996 article, as well as the variance—and extending the data series—we find that there has been a demonstrable increase in the dispersion of opinion toward abortion law, as well as a shift toward the liberal end of the distribution with the other end holding steady. While Mouw and Sobel’s method does not test for all four dimensions of polarization, it does improve upon our tests of dispersion by (a) solving the problem posed by ordinal data and (b) allowing the researcher to avoid the risk of conflating changes in mean, variance, and some types of bimodality. Although we highlight some limitations of their method and question some of their substantive conclusions, we hail their paper as an important advance toward a generalized method for examining polarization.

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