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CULTURE WAR IN AMERICA: MYTH OR REALITY?

Hide the Republicans, the Christians, and the Women: A Response to “Politics and Professional Advancement Among College Faculty”

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Hide the Republicans, the Christians, and the Women: A Response to “Politics and Professional Advancement Among College Faculty”*

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Abstract

Do conservatives suffer discrimination in academe? In “Politics and Professional Advancement Among College Faculty,” Rothman, Lichter, and Nevitte argue that “conservatives and Republicans teach at lower quality schools than do liberals and Democrats.” Using a survey of 1643 faculty members from 183 four-year colleges and universities, they conclude that their results are “consistent with the hypothesis that political conservatism confers a disadvantage in the competition for political advancement.” In this response, we show that Rothman, Lichter, and Nevitte’s work is plagued by theoretical and methodological problems that render their conclusions unsustainable by the available evidence. Furthermore, we offer an alternative hypothesis theoretically consistent with their findings. Unfortunately, we were unable to subject our alternative hypothesis to empirical assessment (or even to replicate the initial results of Rothman, Lichter and Nevitte) since they have refused to make their data available to the scientific community.

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In their recent *Forum* article, “Politics and Professional Advancement Among College Faculty,” Rothman, Lichter, and Nevitte (2005; hereafter RLN) seek to evaluate empirically the accusation that political conservatives, Republicans, and practicing Christians suffer professional discrimination in academia (Kimball 1990; Sykes 1990; Horowitz 2002). To that end, RLN claim to find systematic evidence that “confirms” the hypothesis that conservatives and Republicans are “disadvantage[d] in the competition for professional advancement” (2005, 12-13). They base this claim on a national survey of 1643 faculty members from 183 four-year colleges and universities.¹

Questions of intellectual discrimination deserve careful study. The academy is supposed to be characterized by freedom of thought. If intellectual discrimination is indeed occurring, it strikes a blow to the very foundation of higher education. However, a careful reading of the RLN article makes it clear that their “analysis” adds little to the controversy over “politics and professional advancement among college faculty.” From both a theoretical and methodological perspective, RLN’s results are at best inconclusive and at worst misleading.²

Theoretical Problems

In the words of King Keohane, and Verba (1994, 76), “Avoiding causal language when causality is the real subject of investigation either renders the research irrelevant or permits it to remain undisciplined by the rules of scientific inference.” Unfortunately, RLN fall victim to exactly this sin. Their thesis is “that professional advancement is influenced by ideological orientation” (p. 1). Thus, they wish to show that ideological position *causes* lower status academic jobs (their dependent variable). Yet *nowhere do they provide a theoretical justification*

¹ This survey was funded by the Randolph Foundation, which has a reputation as “a right-leaning group that has given grants to such conservative organizations as the Independent Women’s Forum and Americans for Tax Reform” (Kurtz 2005). Interestingly, the authors fail to discuss the source of their funding in their article. While it is anti-intellectual to assume that the results of an allegedly scientific survey are biased simply on the basis of their funding source, we find it curious that the authors are less than forthcoming about their financial support. It is common practice in the social sciences to disclose fully the sources of research funding.

² Originally, we had hoped to replicate RLN’s findings and test some of our theoretically derived alternative hypotheses. However, the authors refused to grant access even to a dataset limited to the variables used in their 2005 article, let alone the full dataset. We were told that the dataset would be made available when RLN wrote “two books and several more articles on the basis of the data” (Rothman 2005). Given that the survey was conducted in 1999, the fact that the first article did not appear until 2005, and the attention this piece has received in the media (e.g., Kurtz 2005), we did not think it prudent to wait before responding.

for this assertion. Without specifying a theoretical reason for including a variable like ideology in the model, it is difficult to know what to make of the statistical significance of the variable.³

It is difficult even to imagine ideological discrimination occurring at the point of hiring. When a typical department offers an applicant an interview, it knows little more than the candidate's gender, educational history, and publication record. After the interview, the department also knows race and perhaps even marital status. But it has *no* idea about ideological affiliation unless the candidate deliberately brings it up in conversation. Granted, one might imagine political scientists and even sociologists engaging in general, scholarly conversations regarding partisanship and/or ideology during the interview process. Are such discussions likely when physicists, musicologists, and professors of hotel/restaurant management are interviewed? *How* could there be a cause-and-effect relationship between ideological affiliation and employment when the "cause" is not likely to be known by those capable of bringing about the "effect"?

Some of RLN's other findings also contradict their inference that the correlation between certain political identifications and the quality of institutional affiliation is a function of discrimination. First, RLN note that women also appear to be disproportionately underplaced in academia. Of course, applicant gender is known throughout the hiring process. Are the authors ready to ascribe this relationship to bias as well? If so, the same liberals denying Republicans and conservatives employment at top-tier research universities are also biased against women. Given that gender equality is a fundamental tenet of liberalism and the "political correctness" RLN decry, such a charge defies reason. Something else must be going on.⁴

RLN confidently state that, although a few more conservatives find homes in academia as one moves down the ladder of institutional reputation, the "predominance of liberal and Democratic perspectives is not limited to particular types of institutions." If so, then hiring committees must be dominated by liberals regardless of institutional reputation. Thus, in order to buy RLN's discrimination hypothesis, one has to believe that liberal majorities at national research universities systematically engage in *greater* intellectual discrimination than do

³ It should be noted that none of the reported relationships pertaining to ideology and institutional rank stand out as particularly strong in a substantive sense.

⁴ By the same token, we suspect that the well-known conservative characteristic of the officer ranks of the armed forces and the managerial ranks of Fortune 500 companies is not the product of discrimination but of some other process. Just as self selection into the armed forces helps drive the dominance of political conservatism in the military, self selection into academic careers (while individuals with similar advanced degrees and training are commanding much higher salaries in the private sector) may drive the relative dominance of political liberalism in academe.

liberal majorities at liberal arts colleges and regional universities. The absurdity of this conclusion points to the likelihood that some mechanism other than intellectual discrimination explains the observed relationships that RLN report.

An Alternative Hypothesis

We offer self-selection as the likely culprit. First, there may be a rural/urban divide driving the relationship. Conservatives may want to live in communities whose ideological climate is more consistent with their own belief structure. Thus, given the strong correlation observable between the metropolitan density of a particular county and the mean conservatism of its citizens (e.g. Sperling 2004; White 2002), it would not be surprising if conservatives, academic or otherwise, *prefer* to work in smaller, more rural areas. Given the large number of small, liberal arts colleges in these areas (which show up as “lower-tier” institutions in RLN’s analysis even though the qualifications of both the student body and the faculty at such institutions are often of the highest quality), this selection effect could bias the results. Of course, we do not *know* if this is the case, because RLN include no variable in their models measuring community size. Given that this threatens the validity of their study, they clearly should have explored it.

Second, *regional* selection affects hiring, particularly at smaller institutions that are unable to fly in (any or many) applicants for interviews. Easterners stay in the East; Southerners stay in the South; Midwesterners stay in the Midwest. It is no secret that Midwesterners and, especially, Southerners are more conservative, more religious, and less Jewish than Northeasterners. Because the South and Midwest also contain proportionately fewer elite universities and colleges, regional selection effects would produce exactly the effects RLN find – but without any active discrimination at all.

Third, many conservatives may deliberately choose not to seek employment at top-tier research universities because they object, on philosophical grounds, to one of the fundamental tenets undergirding such institutions: the scientific method. As a great deal of scholarship has demonstrated, party identification and voting behavior are now driven much more by religio-cultural predispositions than by fiscal attitudes or orientations toward the New Deal (e.g. Layman 2001; Legee et al. 2002; Abramowitz & Saunders 1998). Furthermore, cultural conservatism, as revealed in antipathy toward gay rights, the women’s movement, and abortion rights (among other things), has been shown to stem in large part from an embrace of Christian fundamentalism as a dominant worldview (e.g. Legee & Kellstedt 1993; Altemeyer 1996; Layman & Carsey 1998). Fundamentalism, by definition, is an absolutist, “faith-based” allegiance to a particular dogma, the veracity of which is considered beyond question or

argument.⁵ Such worldviews are (again, by definition) antithetical to the philosophy of science, which promotes reason and evidence as the determinants of truth. Challenging entrenched dogma is the essence of science. Indeed, many scholars consider this distinction – between “faith-based” reasoning and “scientific reasoning” – to be the essential dichotomy underlying the so-called “culture war” between “red” and “blue” Americans in the 21st century. As James Davison Hunter puts it: “the politically relevant world-historical event (separating contemporary liberals from conservatives) . . . is now the secular Enlightenment of the eighteenth century and its philosophical aftermath” (Hunter 1992: 136). In other words, the faith-based reasoning of Christian fundamentalism (and by extension, of most socio-cultural conservatives) is essentially incompatible with the mission of contemporary research universities.⁶ So, in sum, we are suggesting that the relationships RLN identify might be a spurious function of self-selection based on a fundamentalist antipathy toward the scientific method and other approaches to revealed “truth” – precisely the business of “top-tier” research universities.⁷ We suspect that because of this, many fundamentalist academics (who also happen to identify as conservatives and “practicing Christians”) prefer to work in institutions emphasizing teaching or research less reliant on the scientific method.

⁵ Christian fundamentalism specifically refers to an unwavering belief in the absolute authority and inerrance of the traditional interpretation of the Bible, as it pertains even to matters of science, history, and the future. According to the 2000 National Election Study, approximately 35% of all Americans believe that the Bible is the literal Word of God. This is, of course, not to be equated with practicing Christian religiosity. According to the same survey, approximately 50% of the U.S. public professes an active Christian religiosity but rejects fundamentalism.

⁶ It should be noted that we are not suggesting that fundamentalist Christians have less intellectual acumen than non-fundamentalist Christians or non-Christians. We merely note that fundamentalism is, by definition, anti-intellectual in the scientific sense.

⁷ Indeed, the 2000 National Election Study, conducted by the Survey Research Center at the University of Michigan, reveals that people who believe the Bible is the literal word of God were substantially more likely than the rest of the population (about half of which also considers itself practicing Christians) to report “preferring simple problems to complex ones” and to “dislike thinking.” Furthermore, they were much more inclined to believe that morality is absolute and that different visions of morality should not be tolerated. Finally, they tended to express much greater conservatism on cultural issues such as gay rights and abortion, but this was not necessarily the case with regard to social welfare issues (Burns et al. 2001).

Research Design and Methodology

We have two primary methodological objections to the RLN study: (1) the validity of their chosen dependent variable, and (2) the specification of their statistical models. These weaknesses are important primarily because they fail to ward off spuriousness attributable to selection bias, as we discussed above. If the analysis had been carried out more carefully, the discrimination hypothesis and self-selection hypothesis could have been tested side by side. Given our arguments above, it would be extremely surprising if the discrimination hypothesis stands up to careful empirical scrutiny.

Choice of dependent variable

First, RLN are unclear as to whether they are measuring “professional success” (page 3), “professional advancement” (page 13), or “quality of institutional affiliation” (page 9). We believe that these terms connote rather distinct concepts, but RLN treat them as one and the same. Given that RLN ultimately seem to focus on advancement, several dependent variables would have been plausible. They could, for example, have looked at the proportions of liberals and conservatives granted (or denied) tenure. They could have examined the average length of time before liberals and conservatives win promotion to full professor. They might have explored the relative likelihood that conservatives become department chairs, deans, journal editors, or leaders of professional disciplinary organizations. Alternatively, since the one benchmark by which all academics are evaluated is publication record, the authors might have searched for evidence of ideological discrimination in that realm. After all, if a conservative feels underplaced, the best way to “move up” is to conduct rigorous scientific research and publish it in the top journals. If RLN had discovered that quality scholarship by political conservatives is disproportionately denied space in the top professional journals, that would qualify as compelling evidence of bias. However, RLN chose to look at none of these measures of “professional advancement,” focusing instead on the “quality” of faculty members’ academic institutions. This is a poor measure of professional advancement.

Note that this measure conflates current *placement* with *advancement*. It does not distinguish between institutional affiliation at the beginning of a scholar’s career versus the end. It assumes, rather, that employment at top-ranked universities is the exclusive result of professional ambition as opposed to demonstrated excellence at the point of initial hire. Given that the authors’ model fails to control for faculty members’ years in the profession or the reputation of the institution/faculty from which professors earn their Ph.D.’s, (or, for that matter, whether the faculty even have Ph.D.’s), there is simply no way that they

can plausibly make this distinction. This problem of measurement strikes a significant blow to the discrimination hypothesis, because it is difficult to imagine how discrimination could occur at the point of initial placement, before scholars have had a chance to develop a reputation for anything, let alone for being conservative.

Second, it is unclear whether RLN's measure compares academic institutions within tiers or merely across tiers. If discrimination is really occurring, we would expect to find the negative relationship between faculty conservatism and institutional prestige both within and across tiers. However, if this relationship can only be observed across tiers, it is more likely to be a function of self selection, given that institutions differ more across tiers than within tiers in terms of mission, emphasis, and scholarly approach.

Model Specification

A similar conceptual murkiness plagues "political ideology" – their central independent variable of interest. RLN report that the measure of political ideology derived from respondent self-placement fails to achieve statistical significance in their models (though they do not allow readers to compare the results across these models by presenting the model containing the self-placement measure). Instead, the measure of political ideology that does achieve statistical significance is an additive index of six attitudes. Of these, three (attitudes toward the acceptability of homosexuality, abortion rights, and cohabitation) clearly measure moral tolerance at least as much as political ideology (particularly attitudes toward cohabitation). Two of the other three attitudes (toward government job guarantees and calculated economic redistribution) tap attitudes toward ideals of socialism more than contemporary liberalism, which, in terms of social welfare, focuses more on expanding equality of opportunity through health care, child care and education than on the guaranteed outcomes on which RLN's measure focuses. The final item in the index is an oft-used measure of environmentalism (asking respondents to confront the potential tradeoff between environmental protection and jobs). To be sure, there is much more to political ideology than what these six items capture. But putting this concern aside for the moment, note that this measure clearly captures at least two distinct dimensions of ideology: moral traditionalism and social welfare attitudes. Indeed, RLN report that a factor analysis of these items reveals just that (p. 7). It is curious to us, then, why the authors chose to treat these two dimensions as if they were one. We suspect that the moral traditionalism dimension of the index is driving the relationship

between ideology and institutional prestige.⁸ This principle also applies to the model substituting party identification for ideology, given that, as we mentioned earlier, party identification is increasingly driven by attitudes toward social/cultural lifestyle rather than by economic attitudes. If our suspicions are correct, then the alternative hypothesis of self selection arising from Christian fundamentalism is more plausible than a hypothesis positing discrimination, given that opposition to abortion rights, homosexuality and cohabitation is largely driven by fundamentalist worldviews (again, see Hunter 1992; Legee & Kellstedt 1993; Layman 2001).

This problem would be less serious had RLN adequately measured religiosity. Rather than measuring fundamentalism, or even distinguishing between Catholics, Evangelical Protestants and Mainline Protestants, RLN chose to measure religiosity in a very crude way. First, they lump all “Christians” together, as though Congregationalists, Episcopalians, Southern Baptists, and Pentecostals are all cut from the same cloth. Second, they conflate religious identity with religiosity, multiplying self-identification as a Christian with a measure of whether a person attends church at least once or twice a month. Again, putting aside the statistical problems associated with including interaction terms without each of their requisite components⁹, the lack of precision associated with this variable means that it fails to control adequately for Christian fundamentalism. As King, Keohane and Verba point out, the failure to control for other relevant variables in a model “will bias our estimate (or perception) of the effect of the included variable[s]” (1994: 170). As such, much of the variance that should be captured in the religiosity measure is likely picked up in the ideology and party ID measures, which again points to the plausibility of the selection bias hypothesis.

Our final point regards RLN’s measure of academic achievement. While we are not surprised that the variable is statistically significant, their operationalization of “achievement” is problematic and biased. RLN operationalize the concept of “achievement” by counting the number of books, articles, and book chapters (in the past five years), service on editorial boards or as journal editors, attendance at international meetings, and what “percentage of your working time would you say you spend on research?” (p. 14). Simply put, one cannot weigh equally peer-reviewed articles, book chapters, attendance at (not participation in) conferences, and the self-reported amount of time spent on research. We all know that quality matters at least as much as, if not more than, quantity. In political science, one article in the *American Political Science Review*

⁸ This may also explain why the liberal-conservative self-description failed to achieve statistical significance in the multivariate model (p. 12).

⁹ The interpretation of an interaction term is conditional on its component parts. Failing to include the component parts renders interpretation of the interaction term nonsensical (Friedrich 1982).

is normally worth multiple book chapters. But, as RLN measure achievement, a scholar writing five book chapters and attending two international meetings will have a higher score than one publishing three *APSR* articles over the same five-year span. Measuring achievement in this way fails the test of face validity, and we wager that no reputable departments evaluate faculty in the way proffered by RLN. We are not sure what concept RLN's index measures, but we are quite sure it is not academic achievement.

Conclusion

The issue of potential bias (whether on the basis of ideology, gender, or religiosity) strikes at the heart of the academic enterprise. If scholars face discrimination on the basis of ideology, gender, religiosity, or any other characteristic not directly related to the profession, this would constitute a major problem, a problem requiring immediate attention and rectification.

However, the analysis by Rothman, Lichter and Nevitte adds little to this debate and provides no clear evidence that discrimination indeed occurs. For both theoretical and methodological reasons, RLN's findings are inconclusive, misleading, or false. This exchange has once again shown the value of theory, measurement, and model specification. Without specifying a causal theory, properly operationalizing key concepts, and specifying the model based on the theory, one has nothing more than coefficients and asterisks, not an explanation or understanding.

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