

## **Priming, Projection, or Both?**

### **Reevaluating the Classic Media Priming Hypothesis**

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**Abstract:** This study reevaluates the classic “media priming” hypothesis, which argues that, when news coverage raises an issue’s salience, voters align their overall evaluation of the president with their assessment of him on that issue. Experimental studies of media priming typically show greater correspondence between overall and issue evaluations among subjects exposed to issue-related news. The greater correspondence in the treatment group is identified as priming. However, this phenomenon is also consistent with another explanation. Precisely the opposite, the “projection” hypothesis argues that voters exposed to issue news align their opinion *on the issue* with their assessment of the president’s overall performance. Existing experimental studies cannot rule out this alternative explanation, so we conduct a survey experiment to evaluate the priming and projection hypotheses jointly. Despite recent evidence suggesting that projection is the true underlying effect, our findings support the priming hypothesis. This represents the first unconfounded evidence of media priming.

## Introduction

The media priming hypothesis (Iyengar and Kinder 1987; Iyengar et al. 1984) fundamentally reshaped scholars' understanding of the political power wielded by the media. After decades of research turned up little evidence that mass communication could alter vote choices directly, many researchers concluded that the mass media affect political behavior only at the margins, if at all (e.g. Patterson and McClure 1976).<sup>1</sup> Priming theory challenged this skeptical assessment, contending that the media exert widespread and substantial, though indirect, effects on vote choice. Specifically, the theory holds that the news media, by determining the content of the stories to which we are exposed, alter the criteria we use to evaluate elected officials. Individuals who read about the latest jobs report, for instance, tend to judge the president based on his handling, as they see it, of unemployment.

Early experimental evidence in support of this priming effect (i.e. Iyengar and Kinder 1987) revitalized the study of media's role in politics. These studies showed the correspondence between voters' evaluations of the president on an issue and their overall evaluation of his performance was stronger among subjects randomly exposed to news stories on the issue.<sup>2</sup>

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<sup>1</sup> This is not to say that early studies did not conceive of effects similar to priming. To the contrary, some scholars argued that the media tells voters what to think about, rather than what to think (notably Cohen 1963; Lippmann 1920, 1922, 1925). Although there was some early evidence of this agenda-setting effect (e.g. Erbring et al 1980; McCombs and Shaw 1972), this did not constitute evidence of media priming or a theory of priming.

<sup>2</sup> For a review of the literature on priming, as well as how the priming effect differs from agenda setting and framing, see: Kinder 2003; Scheufele and Tewksbury 2007.

Effects were found for a wide range of issues—including foreign policy (e.g. Krosnick and Kinder 1990), the economy (e.g. Mutz 1998), and racial attitudes (e.g. Valentino et al. 2002).

Despite this evidence, the media priming hypothesis is under fire. The controversy centers on the fact that these studies cannot rule out reverse causation as a plausible account of the findings. Past studies observe that exposure to news on education, for example, increases the *correlation* between a voter's approval of the president on the issue of education (issue approval) and her approval of his overall performance (overall approval). The media priming literature assumes that the increased correlation occurs because the voter shifts her overall approval to reflect her prior issue approval. However, the observed correlation is also consistent with the projection hypothesis, precisely the opposite causal interpretation. In contrast with priming, the projection hypothesis holds that news causes voters to align their overall approval with issue approval. In experimental studies to date, researchers have assumed, perhaps incorrectly, that their findings are a result of changes in overall approval, rather than changes in issue approval.

Unfortunately, in the confines of existing research designs, projection would manifest in precisely the same way as media priming; the two behaviors are observationally equivalent. The equivalence arises because researchers measure both issue approval and overall approval *after* the individual is exposed to the news. It is unclear, therefore, which measure is aligning with which, and extant studies provide no basis for adjudicating between these competing explanations.<sup>3</sup>

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<sup>3</sup>This same critique about potential confounding due to projection can be leveled against existing observational studies of media priming as well. Lenz (2009) provides a detailed critique of this literature. For our purpose here, however, we focus specifically on experimental tests of the media priming hypothesis and our language reflects this choice.

What is more, there is evidence to suggest that projection is the root cause of the effect commonly identified as media priming. Most notably, Lenz's (2009) recent analysis of panel data suggests that, in four cases where an issue became salient between panel waves, news coverage induced subjects to shift their issue preferences, not their overall candidate evaluations—suggesting an effect akin to projection, not priming. These results challenge the entire media priming corpus. So, after almost three decades of inquiry, we are still uncertain about the veracity of the media priming hypothesis.

How, then, can we advance the extant understanding of the indirect effects of news coverage in light of these ambiguous results? The approach we adopt here is to replicate a classic media priming experiment, but to use pretreatment measures of issue and overall approval as baselines for determining whether news induces subjects to align overall approval with pretreatment issue approval, or issue approval with pretreatment overall approval. Thus, our study tests whether the effect commonly identified as “media priming” is actually due to priming, projection, or a combination of the two.

Distinguishing priming and projection is an important enterprise. The priming hypothesis ascribes to the media a significant role in politics. Priming implies the media have the capacity, via issue coverage, to define the terms of presidential support and, consequently, influence both individual-level behavior and aggregate-level outcomes. If projection holds, however, voters are not pushed around in response to changing news coverage. Instead, voters interpret new information so as to remain consistent with prior beliefs. The implications of this difference for the study of media and politics are substantial.

In the next section, we describe the priming and projection hypotheses and the reasons why existing studies of media priming cannot rule out projection as a plausible alternative

explanation for their findings. We then describe our experimental design and the ways in which it corrects the limitations of prior studies. The next sections replicate the conventional (confounded) media priming finding and then present unconfounded tests for both priming and projection. We show that, for the issues we tested, exposure to news induced a priming effect. We find no evidence of projection. Although this study does not rule out the possibility that projection effects exist in some contexts or for some issues, our findings are notably the first unconfounded experimental evidence of media priming. The final section discusses the implications of our findings and suggests avenues for future research.

### **Priming and the Problem of Reverse Causality**

Priming refers to shifts in the criteria individuals use to make political judgments. A media priming effect occurs when, in response to an issue's increasing salience in the news, an individual reevaluates a politician's tenure in office (usually the president's) based on considerations of the now salient issue. That is, the voter shifts her overall approval to align it with her prior issue approval. If news coverage highlights unemployment, for instance, priming holds that citizens who disapprove of the president's economic stewardship will downgrade their overall approval to reflect their negative economic opinion. Alternatively, citizens who approve of the president's handling of the economy will adopt more positive overall assessments when exposed to news on employment.

In the conventional lab-based experiment, the researcher tests the media priming hypothesis by regressing overall approval on issue approval for subjects assigned to the treatment group – who were exposed to news about a given issue – and subjects assigned to the control group – who were not. If the issue weight (i.e. the estimated coefficient for issue

approval) is greater among subjects in the treatment group than among those in the control group, the researcher concludes that the treatment induced priming.

Unfortunately, this method cannot isolate the priming effect because it cannot eliminate potential bias due to reverse causation. This is because, in existing experimental studies of media priming, both overall approval and issue approval (the key dependent and independent variables) are measured posttreatment.<sup>4,5</sup> Individual responses to either question, or both, might be affected

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<sup>4</sup> Unlike the literature on media priming, some studies of racial priming measure the key independent variable—racial predispositions—immediately prior to treatment (e.g. Mendelberg 2001). However, this may influence responses to the treatment. Banks and Valentino (2012) measure the independent variable, though not the dependent variable, one week in advance. Overall, this literature supports the racial priming hypothesis (alternatively, Huber and Lapinski 2006). Research in psychology also demonstrates that recent activation of a mental schema “primes” that schema so that it is called to mind more readily and used in subsequent judgment tasks (see Fazio, 2001 for a review). Whether the increase in the correlation between issue and overall evaluations of the president after exposure to issue specific news is an example of this psychological process, or is the result of projection remains unclear.

<sup>5</sup> We found two notable exceptions in the media priming literature where issue and overall evaluations were measured before treatment. First, in Iyengar and Kinder’s (1987) Experiment 8, reported in Chapter 7, issue and overall evaluations were taken both before and after treatment, but results provided nonetheless used posttreatment measures. Their appendix says data was also analyzed using the pretreatment measures, but model specifications are not provided, and only one of three confirmatory estimates were provided in the text (p. 147). Second, Brewer, Graf and Willnat (2003) took both pretreatment and posttreatment measures.

by exposure to the news story. Therefore, without suitable pretreatment measures of overall *and* issue approval, priming and projection effects manifest in exactly the same way: the researcher observes that the correlation between issue approval and overall approval is larger among treatment subjects than among control subjects. Advocates of the media priming hypothesis can only assume that the treatment affected responses to overall approval but not issue approval. This problem of observational equivalence is depicted in Figure 1.

### **The Case for Projection**

The projection hypothesis posits that the voter's overall approval of the president affects her impression of the president's performance on salient issues.<sup>6</sup> When news coverage raises the salience of an issue, the voter responds by shifting her approval on that issue to reflect her prior overall approval. If economic news coverage spikes, projection holds that voters with a positive overall evaluation of Obama will bump up their opinion of his economic management to reflect their prior positive overall approval. Similarly, voters with negative overall evaluations will respond to the news by adopting a more negative view of his handling of the economy.

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However, while they include pretreatment overall approval as a control in their specification, they apparently use posttreatment issue approval. What is more, none of their four tests produced evidence of priming, possibly due to limited statistical power.

<sup>6</sup>The notion that voters adopt opinions held by their preferred party is certainly not new to political scientists (e.g. Campbell et al. 1960; Erbring et al. 1980), and this tendency has been documented in a number of studies (e.g. Bartels 2002; Carsey and Layman 2006; Wilcox and Wlezien 1996; Wlezien et al. 1997). The projection hypothesis, however, goes a step further, arguing that this relationship is strengthened by individual-level exposure to news coverage.



The projection effect may arise as result motivated reasoning, a well-studied psychological phenomenon. Motivated reasoning theory (Kunda 1990) posits that cognitive processes can be biased when humans are motivated to arrive at a particular conclusion. This is highly plausible in a partisan environment. When the salience of an issue is raised, the voter may be alerted to an inconsistency between her overall evaluation of the president, and her evaluation of him on the issue (i.e. after hearing news about the war in Afghanistan, she realizes that while she approves of the president's overall performance, she has held a negative opinion of his handling of foreign policy). Because partisans are motivated to believe that officials of their party are successful, however, the increased salience of an issue may cause the voter to focus on positive recollections about a candidate's issue performance and adjust her issue evaluation accordingly—altering her issue evaluation to reflect her prior overall evaluation. Projection, then, is a plausible explanation for the results the literature presents as priming effects.

Recent observational evidence also supports the plausibility of the projection hypothesis. Specifically, Lenz (2009) demonstrates that four cases of apparent priming were actually the result of projection-like effects.<sup>7</sup> In particular, he shows that a real-world increase in the salience of an issue informs voters and then motivates them to adopt their party's position on the issue. These findings are notable because, unlike past studies, Lenz's method addresses the problem of

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<sup>7</sup> These findings, along with two additional cases of apparent priming, are presented in Lenz's (2012) forthcoming book. It is worth noting that Lenz (2012) does find observational evidence that the economy was primed in three cases (see Ch. 2). However, his experimental test fails to support this claim (pp. 273-274). This may reflect his small sample size. Yet, given the inherent vagaries of observational data and the inability to replicate what prior studies have called priming, the need for additional experimental tests of priming vs. projection is clear.

reverse causality through the analysis of panel data. Because panel data records responses to the key dependent and independent variables at multiple waves, it allows the researcher to observe whether issue evaluations or overall approval move over time in response to changes in media coverage.

Lenz's study suggests that projection may be more prevalent than previously understood. Yet additional analyses are needed to evaluate priming and projection as competing theories of media influence. Observational studies are limited in that they cannot raise the salience of news issues in isolation; instead, many other factors may change when the salience of an issue increases in the real world (e.g. when party elites take positions on the issues and begin to make persuasive arguments that put the issues into partisan context). It may be the corollary events, not increased salience per se, that lead to projection-like effects. The experimental setting, on the other hand, provides an opportunity to control issue salience in isolation of other factors.

## **Experimental Design**

In order to provide an unconfounded test of the media priming and projection hypotheses, we conduct an online survey experiment that, as we explain below, corrects for the limitations of prior experiments. Our study was funded through Time Sharing Experiments in the Social Sciences (TESS) and fielded by Knowledge Networks. The 2,085 Knowledge Networks panel members included in our sample have demographic characteristics representative of the adult population of the United States.<sup>8</sup> Figure 2 depicts our study design.

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<sup>8</sup> Knowledge Networks maintains high standards for probability sampling methods to achieve nationally representative samples. While the quality of the samples may help to bolster

The key challenge in discriminating a media priming effect from a projection effect is identifying whether, when the salience of an issue is raised, voters shift their overall approval to reflect their preexisting issue approval, or the reverse—aligning issue approval with preexisting overall approval. Such a judgment requires preexisting (pretreatment) measures of both issue approval and overall approval. This necessitates two separate waves of data collection: one to record baseline measures of overall and issue approval, and a second, conducted at a later date, to administer the treatment and again record overall and issue approval.<sup>9</sup> Fortunately, in order to join the Knowledge Networks panel, participants must complete a questionnaire which, among other things, asks them to rate the president’s overall performance as well as his performance on a number of issues. Thus, suitable pretreatment measures of issue and overall approval are already *on file* for many of the panelists, and our sample was limited to those for whom such measures were available.

We randomly assigned subjects to one of two treatment arms or the control arm. We choose two treatment issues to ensure that our results are not issue specific. In the *environment* (En) arm, respondents read a news article about greenhouse gas regulations and the Environmental Protection Agency. These stories were adapted from published news items to

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arguments for external validity, the basis for statistical inference and internal validity in this study is the randomization of subjects into treatment or control arms.

<sup>9</sup> This could be done in a single wave wherein the researcher measures overall and issue approval, then delivers the treatment, and then again records overall and issue approval. However, we prefer a two-wave design because answering the pretreatment questions could affect responses to the treatment, responses on the posttreatment questionnaire, or both.

maximize believability.<sup>10</sup> In the *education* (Ed) arm, respondents read about student test scores in the United States and abroad. Finally, in the *control* (C) arm, respondents read a news article about male pattern baldness which, presumably, should not affect political considerations. Text of the full stories is provided in Appendix A.

Survey respondents were also randomly divided into two subsamples, A and B. In subsample A, after reading the assigned news article, respondents provided their overall evaluation of the president before providing issue evaluations. Conversely in subsample B, after reading the assigned news article, respondents provided their issue evaluations before providing an overall evaluation. We limit our tests of the priming hypothesis to subsample A, and our tests of the projection hypothesis to subsample B. This avoids any question order effects that might arise from the posttreatment ordering of issue approval and overall approval questions. Nonetheless, we ask both issue and overall evaluations of each subject for two reasons. First, asking both allows us to replicate the classic priming result. Second, it allows us to estimate treatment effects using two stage least squares (2SLS). Although 2SLS may be preferable to some readers, we present ordinary least squares (OLS) regressions using pretreatment variables

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<sup>10</sup> These stories were also chosen because the issues featured are not explicitly politicized and convey little partisan information. This reduces the possibility that our findings are driven by learning effects (see: Gelman and King 1993) rather than increased issue salience. However, to the extent that these stories do include information about the president, his administration, and their policies, Lenz's (2012) findings suggest this should increase the likelihood of observing projection, not priming (see especially his findings from the "SCHIP expansion" experiment: pp. 196-201, 271-274)

in the main text for simplicity and 2SLS estimates in Appendix C. Table 1 provides the scaling for our four key variables of interest.

## **Analysis and Results**

The analysis proceeds in three parts. The first presents the replication of the effect scholars conventionally identify as priming. The second and third sections use pretreatment measures of overall approval and issue approval to evaluate whether this conventional finding is evidence of priming, projection, or a combination of the two.

### Replicating the Conventional “Priming” Result

As our aim is to determine the underlying cause of the effect conventionally identified as media priming, we first replicate the increased correspondence between posttreatment overall and posttreatment issue approval even though this is a flawed analysis. (For the sake of clarity, we refer to this as the “correspondence effect”, differentiating it from a genuine media priming effect.) To do so, we specify the issue weights for education approval and energy approval for the control group,  $\beta_{C,Ed}$  and  $\beta_{C,En}$ , as follows:

$$y_{i,t} = \alpha_C + \beta_{C,Ed}x_{Ed,i,t} + \beta_{C,En}x_{En,i,t} + \rho y_{i,t-1} + u_i, \quad (1)$$

where  $y_{i,t}$  is the posttreatment overall presidential approval of subject  $i$ , who is in the control group;  $x_{Ed,i,t}$  and  $x_{En,i,t}$  are posttreatment issue approval for education and energy, respectively;  $y_{i,t-1}$  is pretreatment overall approval<sup>11</sup>;  $\rho$  is the weight determining how much pretreatment overall approval predict posttreatment overall approval; and  $u_i$  is an error term.

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<sup>11</sup> We include this lagged dependent variable in the model because both the priming and projection hypotheses are about change in approval, not the level of approval.

We specify the issue weights in each of the treatment arms,  $\beta_{T,Ed}$  and  $\beta_{T,En}$ , with the same equation. For the education arm:

$$y_{i,t} = \alpha_{T,Ed} + \beta_{T,Ed} x_{Ed,i,t} + \beta_{C,En} x_{En,i,t} + \rho y_{i,t-1} + u_i, \quad (2)$$

and, for the environment arm:

$$y_{i,t} = \alpha_{T,En} + \beta_{C,Ed} x_{Ed,i,t} + \beta_{T,En} x_{En,i,t} + \rho y_{i,t-1} + u_i. \quad (3)$$

To evaluate whether the treatment stories induced a “correspondence effect,” we test the increase in the issue weights from the control group to the relevant treatment group:

$$\psi_{Ed} = \beta_{T,Ed} - \beta_{C,Ed}, \quad (4)$$

and

$$\psi_{En} = \beta_{T,En} - \beta_{C,En}, \quad (5)$$

estimating the parameters in equations 1, 2, and 3 using OLS. We conduct post hoc tests of significance for 4 and 5 based on these (biased) OLS results. Table 2 presents the estimates of these models.<sup>12</sup>

The results show clear evidence of the correspondence effect for both the energy treatment ( $\psi_{En}=0.046$ ,  $p=0.051$ , 1-tailed) and for the education treatment ( $\psi_{Ed}=0.091$ ,  $p<0.001$ , 1-tailed). Therefore, our study successfully replicated the confounded result that scholars conventionally assume is evidence of media priming. The next sections evaluate whether the treatment stories induce genuine priming or, just the opposite, a projection effect.

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<sup>12</sup> We estimate these models in a single regression by stacking the data, including indicator variables for the treatment arms and the control arm, and interacting these with responses to the issue approval questions. We could also estimate the effect of each treatment in separate regressions (treating subjects in the off-treatment arm as part of the control group). The substantive conclusions remain the same (this is true for analyses in the next section as well).

### An Unconfounded Test of Priming

To test for media priming in a way that eliminates bias due to projection effects, we regress posttreatment overall presidential approval on the pretreatment measure of issue approval. We first specify the issue weights of prior education approval and prior energy approval for those in the control arm,  $b_{C,Ed}$  and  $b_{C,En}$ , in the context of the following model:

$$y_{i,t} = \alpha_C + b_{C,Ed}x_{Ed,i,t-1} + b_{C,En}x_{En,i,t-1} + \rho y_{i,t-1} + u_i, \quad (6)$$

where  $x_{Ed,i,t-1}$  and  $x_{En,i,t-1}$  are pretreatment issue approval for education and energy, respectively. All other terms can be interpreted as in equation 1.<sup>13</sup>

Second, we define the issue weights for education approval and energy approval in each treatment arm as  $b_{T,Ed}$  and  $b_{T,En}$ , respectively. For subjects who were exposed to the education news item, we specify:

$$y_{i,t} = \alpha_{T,Ed} + b_{T,Ed} x_{Ed,i,t-1} + b_{C,En} x_{En,i,t-1} + \rho y_{i,t-1} + u_i \quad (7)$$

Similarly, the model for subjects in the energy arm is

$$y_{i,t} = \alpha_{T,En} + b_{C,Ed} x_{Ed,i,t-1} + b_{T,En} x_{En,i,t-1} + \rho y_{i,t-1} + u_i. \quad (8)$$

Finally, given these specifications, we estimate the models using OLS and evaluate the priming effects as the increase in the issue weights from the control arm to the treatment arm.

The priming effect for education is written

$$\theta_{Ed} = b_{T,Ed} - b_{C,Ed}, \quad (9)$$

while the priming effect for energy is written

$$\theta_{En} = b_{T,En} - b_{C,En}. \quad (10)$$

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<sup>13</sup> We also ran these models with an additional term that interacted  $y_{i,t-1}$  with treatment.

Substantive results are the same.

Ordinary least squares results for these unconfounded tests of the priming hypothesis are provided in Table 3. The results demonstrate priming effects for both the energy treatment ( $\theta_{En}=0.047$ ,  $p=0.014$ , 1-tailed) and for the education treatment ( $\theta_{Ed}=0.065$ ,  $p=0.041$ , 1-tailed). This is clear evidence that exposure to issue news caused subjects to shift their overall evaluation of the president's tenure in office to reflect their prior opinion of his handling of education or the environment.

In appendix B, two-stage least squares estimates (2SLS) are provided, with  $x_{i,t-1}$  instrumenting for  $x_{i,t}$ , the posttreatment measure of issue approval which may include endogenous components induced by treatment.<sup>14</sup> In all cases, p-values differ very little and substantive conclusions differ not at all.

#### An Unconfounded Test of Projection

To test whether our experimental treatments also induced a projection effect (i.e. causing subjects to shift their issue approval to reflect their prior overall approval), we reverse the models

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<sup>14</sup> Because 2SLS corrects for measurement error as well as endogeneity, its point estimates may be preferable to OLS estimates. That said, both OLS (which corrects for treatment-induced endogeneity by using pretreatment measures in the model) and 2SLS (which corrects for treatment-induced endogeneity *and measurement error* by instrumenting for the posttreatment measures with the pretreatment measures) provide the basis for valid hypothesis tests. Consequently, both arrive at similar p-values in this study, supporting identical substantive conclusions. To avoid a lengthy discussion of the merits of 2SLS in correcting for measurement error, however, we place those estimates in the appendix for interested readers. In the body of the text, we provide OLS point estimates in keeping with custom even though these estimates are biased by measurement error, but focus on the hypothesis tests which are, nonetheless, valid.



for priming, estimating the effect of overall approval on issue approval. For respondents in the environment treatment group, we specify the issue weight for prior overall approval,  $\delta_{T,En}$ , in the context of the following model:

$$x_{En,i} = \gamma_{T,En} + \delta_{T,En} y_{i,t-1} + \varphi_{En} x_{En,i,t-1} + e_i, \quad (11)$$

where  $x_{En,i}$  is posttreatment issue approval for the  $i^{\text{th}}$  individual who is in the energy treatment group;  $\gamma_{T,En}$  is the intercept for the energy treatment group;  $y_{i,t-1}$  is the pretreatment measure of overall presidential approval;  $\varphi_{En}$  is a weight determining how much prior energy approval predicts posttreatment energy approval;  $x_{En,i,t-1}$  is pretreatment energy approval; and  $e_i$  is an error term. For the control group (which includes subjects assigned to the control arm and those assigned to the education arm),<sup>15</sup> we specify the effect of prior overall approval,  $\delta_{C,En}$ , on energy approval in the context of the model:

$$x_{En,i,t} = \gamma_{C,En} + \delta_{C,En} y_{i,t-1} + \varphi_{En} x_{En,i,t-1} + e_i. \quad (12)$$

Based on these models, we estimate the energy projection effect,  $\Delta_{En}$ , as the increase in the issue weight for prior overall approval:

$$\Delta_{En} = \delta_{T,En} - \delta_{C,En}. \quad (13)$$

We use the same approach to estimate the effect of the education treatment story. For subjects in the education arm, we specify the issue weight for overall approval,  $\delta_{T,Ed}$ , in the context of the model:

$$x_{Ed,i,t} = \gamma_{T,Ed} + \delta_{T,Ed} y_{i,t-1} + \varphi_{Ed} x_{Ed,i,t-1} + e_i. \quad (14)$$

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<sup>15</sup> If we limit our test to those in the control arm (i.e. excluding subjects in the education arm), our conclusions are unchanged.

Similarly, we specify the issue weight for those not in the education arm,  $\delta_{C,En}$ , in the context of the model:

$$x_{Ed,i,t} = \gamma_{C,Ed} + \delta_{C,Ed} y_{i,t-1} + \varphi_{Ed} x_{Ed,i,t-1} + e_i. \quad (15)$$

Finally, the projection effect for education equals the difference in issue weights:

$$\Delta_{Ed} = \delta_{T,Ed} - \delta_{C,Ed}. \quad (16)$$

The OLS estimates of these models are presented in Tables 4 and 5. In Table 4, the test of projection for the environmental issue fails to achieve statistical significance ( $\Delta_{En} = -0.057$ ,  $p=0.9225$ , 1-tailed) and the sign on the estimated effect is actually opposite the direction predicted by the hypothesis. In Table 5 the test of the projection hypothesis using the education issue also fails to achieve statistical significance ( $\Delta_{Ed}=0.023$ ,  $p=0.2895$ , 1-tailed). Our data, therefore, provide no evidence that issue news induced a projection effect.<sup>16</sup>

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<sup>16</sup> In both Tables 4 and 5, the coefficients on pretreatment overall approval are large. In fact, they appear to dwarf the coefficients on pretreatment issue approval. This might lead to the counterintuitive interpretation that pretreatment overall approval is a better predictor of issue approval than pretreatment issue approval; however, the large disparity in coefficient sizes is driven by the different scaling of the two pretreatment measures. Recall that pretreatment issue approval is binary while pretreatment overall approval is ordinal (see Table 1). When overall approval is artificially dichotomized the disparity disappears. (Results available upon request.) However, we cannot rule out the possibility that the observed correspondence between pretreatment overall approval and issue approval is a result of projection that occurred before our experiment. Our contention is simply that we find no evidence that projection occurred as a result of exposure to our treatments.

## Discussion

Together, these results clearly demonstrate the capacity of news coverage to change individual-level evaluations of the president through priming. For the two issues we tested, exposure to news caused voters to align their overall approval with their prior issue approval. Alternatively, we found no evidence of the reverse—that treatment news stories caused voters to shift their issue approval to reflect their prior overall approval. This is consistent with the predictions of priming theory and, notably, is the first unconfounded evidence of media priming.

What do these results mean for the extant media priming literature? One could conclude that our findings validate those of past studies and that we need not worry that estimates from prior studies are biased by reverse causality. Yet, Lenz's (2009) findings complicate this interpretation. Lenz's study provides credible evidence that there are instances in which exposure to news induces projection-like effects and not priming. Since projection seems to occur in at least some contexts, it seems unwise to rule it out as a possible factor in past, confounded, experimental results, at least until the research community has a better understanding of the critical contextual factors. Instead, a new wave of experiments that avoid the same methodological mistakes would help to better adjudicate the meaning of past studies.

However, it is worth speculating how one might reconcile our results in light of Lenz's, apparently contradictory, finding in favor of projection-like effects. One possibility is that exposure to mass media induces priming in some cases and projection in others. In support of the contingency of media effects, Lenz argues that priming may be most likely to occur on valence issues, which are "generally easier for citizens to understand" (p. 834), as opposed to policy positions. For the policy issues he studied, for example, voters may not have sophisticated preferences over the policy options, so they may be more prone to take cues from party elites. By

contrast, citizens, through their everyday conversations and experiences, may have a relatively fixed opinion about presidential performance on the issues we tested. The evidence presented in Lenz's forthcoming book provides initial support for this claim. Specifically, while he finds evidence of projection and not priming for seven salient policy issues, he also finds that economic considerations were primed in three elections (see: Chapters 2 and 8).<sup>17</sup>

Another possibility concerns differences in the stimulus to which individuals were exposed. The treatment in our experiment—a single news item—differed a good deal from the “treatments” in Lenz's observational analyses—large overall shifts in real-world issue coverage due to events occurring between survey waves. While a single news article may be a relatively weak treatment, the experimental context also has the virtue of being more controllable, particularly with respect to individual-level exposure to issue coverage. Large shifts in media focus, by contrast, may induce a variety of downstream consequences such as elite position taking. As a story unfolds, these downstream effects may induce projection.

Thinking through this possibility inevitably pushes us away from a methodological discussion and toward a more difficult question about the factors that mediate the priming and

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<sup>17</sup> Again, however, Lenz (2012) fails to find evidence of economic priming in his experimental tests (pp 271-274). Moreover, in considering the possibility that priming may be most likely on issues like education and the environment, one might wonder how similar these valence issues are to retrospective economic evaluations. While the latter taps into respondents' evaluations of the nation's economic performance in the past year, the former taps into their approval of the president's handling of a particular issue, not the state of the issue (i.e. has educational attainment gotten better or worse in the last year?). We leave this question open for future researchers.

projection relationships. Specifically, the discrepancy between our findings in support of media priming and Lenz's findings in support of projection may indicate that these effects are driven by different mechanisms. If media priming, like racial priming and psychological priming more generally, is mediated strictly by automatic accessibility, it may be only a temporary effect, potentially counteracted in the medium or long term by exposure to other news items or elite position taking.<sup>18</sup> If so, media priming is likely to be observed when overall approval is measured immediately or shortly after individuals are exposed to news coverage and when the news coverage does not cue a partisan response.

If projection, by contrast, is driven by a mechanism like motivated reasoning, then it is likely to be observed when news items put an explicitly political spin on the issue at hand or when elites provide political commentary.<sup>19</sup> In an observational study where a dramatic shift in media attention to an issue between survey waves, projection may be more likely to be observed precisely because politicians are a source of commentary on the news, interpreting it for political gain. Over a longer time frame, salience-driven effects, like priming, may be overshadowed by projection-like effects as political elites respond to shifts in news coverage, taking sides and arguing over issue framing. By putting a political spin on salient issues and cueing partisanship,

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<sup>18</sup> Iyengar and Kinder (1987, p. 24-26) find that the agenda-setting effect persists for one week, but they do not evaluate the persistence of priming.

<sup>19</sup> This does not denigrate the importance of accessibility for the projection effect. In fact, becoming alerted to an inconsistency depends on the issue being accessible in active memory. However, projection may also rely on a conscious or semi-conscious motivation to hold positions consistent with those of a political party.

political elites may inform citizens of the party's positions and motivate them to follow, evoking a projection effect.

It is interesting to note how our study differed from earlier, experimental studies of media priming. First, in their seminal work, Iyengar and Kinder (1987) attempted to obscure experimental manipulations by assembling half-hour evening news programs, commercials and all, and presenting them as if they were the usual (unaltered) evening news. The goal was to increase the plausibility of the treatment and reduce the chance that subjects suspected the hand of the researcher in manipulating the media content. By contrast, we made no attempt to construct a naturalistic setting, and subjects may have suspected that the particular news article was curated to a particular end.

Additionally, Iyengar and Kinder presented subjects with evening news for up to five days. Their treatment had the potential for greater impact. We on the other hand could not count on repetition for our treatment effect, and increased our study size accordingly. Our study included over two thousand subjects while experiments in *News that Matters* ranged in size from 28 subjects to 140 subjects. In spite of these differences, our results are not qualitatively different; however, our treatment appears to have been less impactful, as evidenced by the just-significant p-values. To obtain greater treatment effects and increased sensitivity then, future research may need to return to laboratory studies.

We are left to conclude that we still have much to learn about how voters will respond to particular changes in news coverage. By unequivocally demonstrating the existence of media priming in an experimental setting, our results represent a significant step towards an accurate understanding of the indirect effect of media exposure. Yet the need for additional analyses of the priming and projection hypotheses is immediately evident by the contradiction between these

findings and those of Lenz (2009). Equally evident is the necessity of collecting pretreatment measures of both issue approval and overall approval in all future studies of priming and projection. Regrettably, this requires a more complicated and more costly method of data collection. However, further reliance on (or replications of) the original, flawed methodology will not advance scholarship in a meaningful way.

## **Conclusion**

This study reevaluated the classic media priming hypothesis. Past studies have been taken as evidence of priming but failed to rule out projection as an alternative explanation. We conducted an online survey experiment that corrected for the limitations of previous studies. Specifically, we eliminated the threat of reverse causality by obtaining pretreatment measures of our key independent and dependent variables: overall presidential approval and approval of the president's handling of specific issues. For the news issues we tested in our study (education and the environment), the results provided strong support for the priming hypothesis. Despite recent evidence in support of projection-like effects, we found no evidence that exposure to news stories induces projection.

Although we present the first unconfounded experimental evidence of media priming, we do not take our results to be the final word in the priming versus projection debate. Nor do we necessarily take them to be a validation of conclusions drawn in prior, confounded tests of the media priming hypothesis. Instead, our results demonstrate that the mass media, at least in some cases, systematically alters support for elected officials through priming. They also highlight the necessity of collecting pretreatment measures of both issue approval and overall approval in future priming studies.

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**Table 1: Variable Coding Schemes**

	<b>Pretreatment Overall Approval (<math>y_{i,t-1}</math>)</b>	<b>Posttreatment Overall Approval (<math>y_i</math>)</b>	<b>Pretreatment Issue Approval (<math>x_{Ed,i,t-1},</math> <math>x_{En,i,t-1}</math>)</b>	<b>Posttreatment Issue Approval (<math>x_{Ed,i}, x_{En,i}</math>)</b>
strongly approve	1	1		1
approve	0.5	0.5	1	0.5
neither approve nor disapprove	0	0		0
disapprove	-0.5	-0.5	-1	-0.5
strongly disapprove	-1	-1		-1

*Note: Pretreatment Issue Approval and Pretreatment Overall Approval were collected by Knowledge Networks during prior surveys and were on file before the commencement of our experiment. Pretreatment Issue Approval was administered using a two-option forced-choice format, and differs from the other 5-point Likert items.*

**Table 2: Replication of the Conventional (Confounded) Test for Media Priming**

<b>Parameter</b>	<b>Estimates</b>	<b>Difference</b>
Intercept, control arm ( $\alpha_C$ )	-0.050 (0.012)	
Intercept, environment arm ( $\alpha_{T,En}$ )	-0.041 (0.012)	
Intercept, education arm ( $\alpha_{T,Ed}$ )	-0.045 (0.012)	
Posttreatment environment approval, control ( $\beta_{C,En}$ )	0.277 (0.022)	
Posttreatment environment approval, treatment ( $\beta_{T,En}$ )	0.323 (0.028)	
<b>Correspondence effect, env.</b> ( $\psi_{En} = \beta_{T,En} - \beta_{C,En}$ )		0.046 (0.028) p=0.051
Posttreatment education approval, control ( $\beta_{C,Ed}$ )	0.224 (0.021)	
Posttreatment education approval, treatment ( $\beta_{T,Ed}$ )	0.314 (0.027)	
<b>Correspondence effect, edu.</b> ( $\psi_{Ed} = \beta_{T,Ed} - \beta_{C,Ed}$ )		0.091 (0.028) p<0.001
Pretreatment approval ( $\rho$ )	0.485 0.016	
Observations	2,085	
R-squared	0.783	

*Note: Estimates and standard errors (in parentheses) are from OLS regression on overall approval measured posttreatment. Difference statistics in the second column provide the basis of conventional tests (1-tailed) of the priming hypothesis, which we refer to here as the “correspondence effect.” In the leftmost column, parameters being estimated are in parentheses.*

**Table 3: Unconfounded Test of the Priming Hypothesis**

Parameter	Estimates	Difference
Intercept, control arm ( $\alpha_C$ )	-0.110 (0.020)	
Intercept, energy arm ( $\alpha_{T,En}$ )	-0.122 (0.019)	
Intercept, education arm ( $\alpha_{T,Ed}$ )	-0.106 (0.019)	
Pretreatment energy approval, control ( $b_{C,En}$ )	0.031 (0.023)	
Pretreatment energy approval, treatment ( $b_{T,En}$ )	0.078 (0.036)	
<b>Priming effect, energy</b> ( $\theta_{En} = b_{T,En} - b_{C,En}$ )		0.047 (0.027) p=0.041
Pretreatment education approval, control ( $b_{C,Ed}$ )	0.014 (0.030)	
Pretreatment education approval, treatment ( $b_{T,Ed}$ )	0.079 (0.022)	
<b>Priming effect, education</b> ( $\theta_{Ed} = b_{T,Ed} - b_{C,Ed}$ )		0.065 (0.030) p=0.014
Pretreatment approval ( $\rho$ )	0.743 (0.022)	
Observations	1,019	
R-squared	0.716	

*Note: Estimates and standard errors (in parentheses) are from OLS regression on overall approval measured posttreatment. Difference statistics in the second column provide the basis of unconfounded tests of the priming hypothesis. In the leftmost column, parameters being estimated are in parentheses.*

**Table 4: Unconfounded Test of the Projection Hypothesis, Environment**

Parameters	Estimates	Difference
Intercept, control arm ( $\gamma_{C,En}$ )	-0.087 (0.015)	
Intercept, env. arm ( $\gamma_{T,En}$ )	-0.079 (0.021)	
Pretreatment overall approval, control ( $\delta_{C,En}$ )	0.563 (0.028)	
Pretreatment overall approval, treatment ( $\delta_{T,En}$ )	0.506 (0.036)	
<b>Projection effect, env.</b> ( $\Delta_{En} = \delta_{T,En} - \delta_{C,En}$ )		-0.057 (0.040) p=0.9225
Pretreatment energy approval ( $\phi_{En}$ )	0.097 (0.015)	
Observations	1,066	
R-squared	0.560	

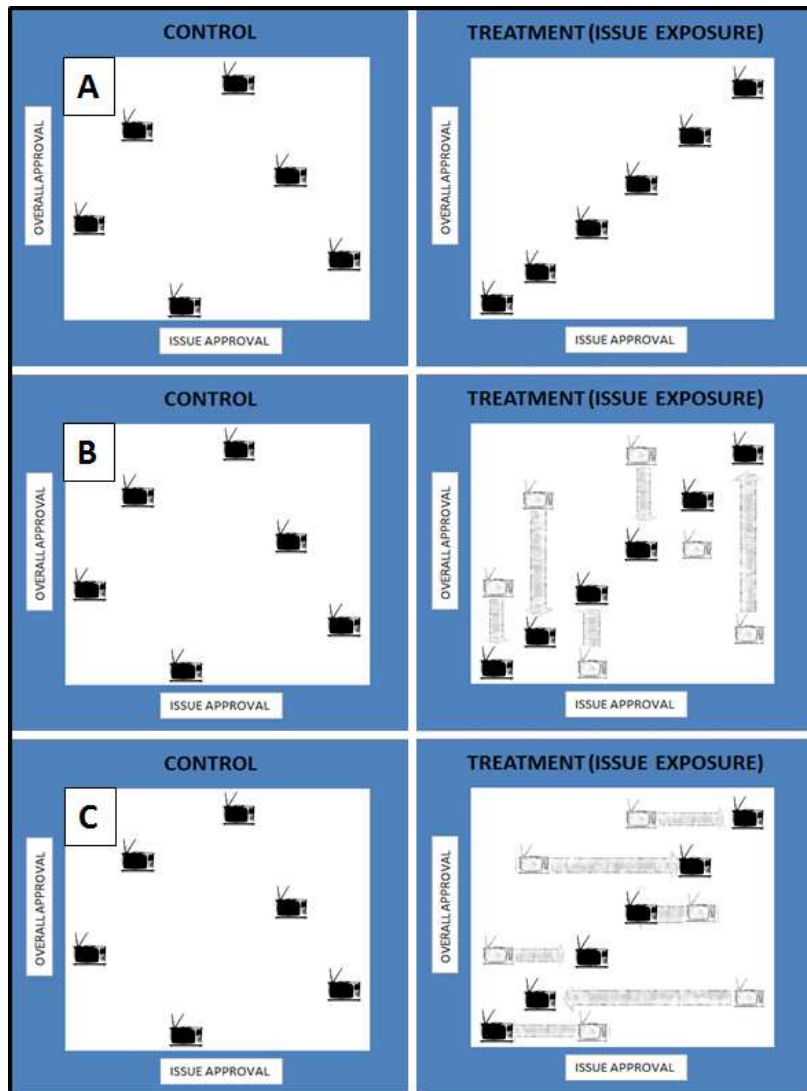
*Note: Estimates and standard errors (in parentheses) are from OLS regression on energy approval measured posttreatment. Difference statistics in the second column provide the basis of unconfounded tests of the priming hypothesis. In the leftmost column, parameters being estimated are in parentheses.*

**Table 5: Unconfounded Test of the Projection Hypothesis, Education**

Parameters	Estimates	Difference
Intercept, control arm ( $\gamma_{C,Ed}$ )	-0.039 (0.016)	
Intercept, education arm ( $\gamma_{T,Ed}$ )	-0.108 (0.022)	
Pretreatment overall approval, control ( $\delta_{C,Ed}$ )	0.497 (0.028)	
Pretreatment overall approval, treatment ( $\delta_{T,Ed}$ )	0.520 (0.036)	
<b>Projection effect, education</b> ( $\Delta_{Ed} = \delta_{T,Ed} - \delta_{C,Ed}$ )		0.023 (0.041) p=0.2895
Pretreatment education approval ( $\varphi_{Ed}$ )	0.109 (0.016)	
Observations	1,066	
R-squared	0.516	

*Note: Estimates and standard errors (in parentheses) are from OLS regression on education approval measured posttreatment. Difference statistics in the second column provide the basis of unconfounded tests of the priming hypothesis. In the leftmost column, parameters being estimated are in parentheses.*

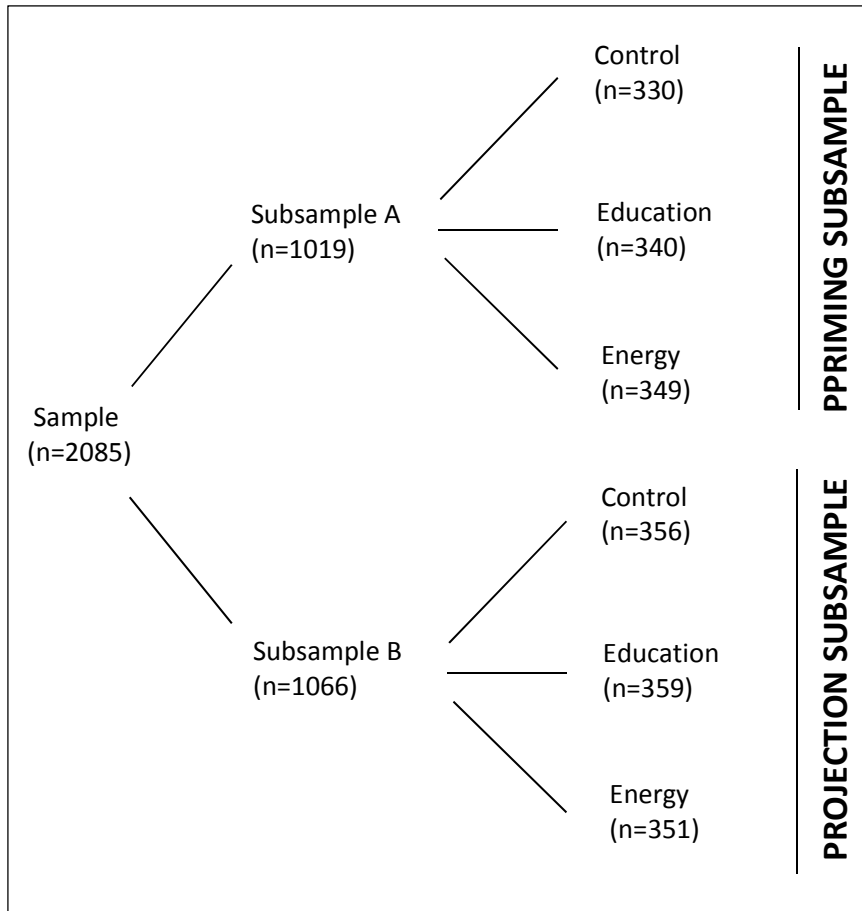
**Figure 1: Projection and Priming**



*Note: Panel A illustrates results from a hypothetical priming experiment; subjects exposed to issue news show greater association between issue and overall approval. Priming explains the outcome by arguing that voters align their overall approval with their issue approval. Panel B illustrates priming; voters move vertically on the plot. Projection explains the observed outcome by arguing that voters align their issue approval with their overall approval. Panel C illustrates a projection effect; voters move horizontally on the plot. Without observing pretreatment coordinates (grey televisions), the two effects are observationally equivalent.*



**Figure 2: Research Design**



*Note: Tests of priming are limited to Subsample A because, after treatment, subjects in this subsample answered the overall evaluation question (the left-hand side variable in the test of priming) before the issue evaluation questions. Tests of projection are limited to Subsample B because, after treatment, subjects in this subsample answered issue evaluation questions (the left-hand side variables in the test of projection) before the overall evaluation. Partitioning the sample in this fashion allows us to eliminate posttreatment question order as a potential confounder.*

## **Appendix A. Experimental Treatments**

Participants in our study were randomly assigned to read one of the following news items, each adapted from stories that appeared in a national newspaper.

### *Environment Story*

#### **E.P.A. Says It Will Press on With Greenhouse Gas Regulation**

The Environmental Protection Agency announced a timetable on Thursday for issuing rules limiting greenhouse gas emissions from power plants and oil refineries, signaling a resolve to press ahead on such regulation even as it faces stiffening opposition in Congress.

The agency said it would propose performance standards for new and refurbished power plants and oil refineries in the coming year. But the E.P.A. was vague on how stringent the rules would be and how deep a reduction in carbon dioxide emissions would result. Gina McCarthy, the assistant administrator for air and radiation, said the rules would be “cost-effective” but the agency declined to be more specific, saying only that the agency would consider the costs and benefits of available control technologies.

Power plants and refineries are the nation’s top emitters of carbon dioxide, a greenhouse gas that has been linked to global warming. Having declared greenhouse gases to be a threat to public health last year, the agency began regulating those emissions on Jan. 2 under the Clean Air Act.

The rules for new power plants and refineries are certain to be challenged by industry, some states, and many in the House of Representatives who have vowed to limit the agency’s regulatory powers.

On one level, the E.P.A. seemed to be flexing its muscle after drawing criticism from environmental groups for recently deciding to delay issuing standards on conventional pollutants from industrial boilers. But by isolating only power plants and refineries, the agency also seemed to signal that for now, at least, it will go after only big industrial sources.

Coal-fired power plants already face a cascade of new regulations scheduled to take effect in coming months covering their emissions of sulfur dioxide and nitrogen oxides, mercury and other pollutants. By putting utilities on notice that it is adding carbon dioxide to the pollutant list, the E.P.A. is increasing pressure on utilities to shut down older coal-fired plants.

Jeffrey R. Holmstead, who held Ms. McCarthy’s post under President George W. Bush, noted that the agency was “studious in avoiding” a definition of “cost-effective.”

“I think it’s just their way of saying, what we intend to do will be reasonable,” he said. The E.P.A., he said, has intermittently talked about reductions in carbon dioxide emissions that would pay for themselves because they resulted from improvements in energy efficiency, for example.

But unless the agency demands a fairly high expenditure on the kind of the technology that would be needed to avoid a ton of emissions, reductions will not be achieved, Mr. Holmstead said. “They’re not going to have it both ways for much longer,” he said.

### **International test score data show U.S. firmly mid-pack**

After a decade of intensive efforts to improve its schools, the United States posted these results in a new global survey of 15-year-old student achievement: average in reading, average in science and slightly below average in math. Those middling scores lagged significantly behind results from several countries in Europe and Asia in Tuesday's report from the Organization for Economic Cooperation and Development.

U.S. officials said the results show that the nation is slipping further behind its competitors despite years spent seeking to raise performance in reading and math through the 2002 No Child Left Behind law and a host of other reforms.

"For me, it's a massive wake-up call," Education Secretary Arne Duncan said Monday. "Have we ever been satisfied as Americans being average in anything? Is that our aspiration? Our goal should be absolutely to lead the world in education."

The Obama administration is likely to use the results to press Congress to rewrite the federal education law to prod states to do more to help the lowest-performing schools. On Monday, President Obama warned that the United States faces a "Sputnik moment," needing innovations akin to the effort to put a man on the moon after the Soviet Union launched the first satellite into orbit in 1957.

The report released Tuesday focused on reading ability and found that more than a dozen countries, from Korea to Poland, performed significantly better than the organization's statistical average in that area. The United States did not.

The U.S. scores of 500 in reading and 502 in science, on a 1,000-point scale, were about the organization's average, according to the report. The U.S. math score of 487 was below the average of 496.

Among the other key findings of the study:

-- Countries with similar levels of economic prosperity can yield widely varying academic results. Korea, the strongest performer among the group's member nations, has a lower gross domestic product per capita than the organization's average. So does Shanghai.

-- U.S. math results were up since 2006 but not measurably different than scores in 2003, the earliest year in which comparisons were possible. U.S. science scores were up since 2006, a bright spot in the results.

### **Male pattern baldness: What causes it?**

Why do so many men go bald? What exactly changes on their heads? Hot off the lab bench: Men go bald because the follicles from which their hairs sprout run out of special progenitor cells with which to make the hair.

Normally, inside hair follicles a region called “the bulge” contains a packet of adult stem cells from which the hair is replenished. Scientists have theorized that these stem cells might simply run out in those prone to male-pattern baldness.

To test this, a team of researchers at the University of Pennsylvania (and, it seems, a few other places) looked at hair follicles from discarded bits of scalp from 54 men seeking hair transplants. Comparing the follicles from still-hairy samples of these scalps with non-hairy samples, the researchers found:

- 1) The hair follicle stem cells were still there
- 2) Another set of cells — known as hair progenitor stem cells — were depleted.

The scientists concluded that somehow, for some reason, the stem cells don’t transform into progenitor cells anymore. That makes male-pattern baldness similar to alopecia areata, a reversible kind of hair loss.

All well and good, but what does that mean for a person who has lost his hair and wants it back? Maybe nothing right now, but the scientists do note that the results suggest “potential reversibility of this condition.” And, they add, these and their other findings suggest the hair follicle is a fairly complicated place. The new info should help them develop therapies down the road for a range of hair and skin disorders.

## Appendix B. 2SLS Estimates

**Table B1: Unconfounded Test of the Priming Hypothesis Using 2SLS**

Parameter	Estimates	Difference
Intercept, control arm ( $\alpha_C$ )	-0.077 (0.020)	
Intercept, energy arm ( $\alpha_{T,En}$ )	-0.025 (0.024)	
Intercept, education arm ( $\alpha_{T,Ed}$ )	-0.037 (0.022)	
Pretreatment energy approval, control ( $b_{C,En}$ )	0.177 (0.263)	
Pretreatment energy approval, treatment ( $b_{T,En}$ )	0.344 (0.277)	
<b>Priming effect, energy</b> ( $\theta_{En} = b_{T,En} - b_{C,En}$ )		0.167 (0.084) p=0.023
Pretreatment education approval, control ( $b_{C,Ed}$ )	0.491 (0.246)	
Pretreatment education approval, treatment ( $b_{T,Ed}$ )	0.598 (0.249)	
<b>Priming effect, education</b> ( $\theta_{Ed} = b_{T,Ed} - b_{C,Ed}$ )		0.107 (0.082) p=0.097
Pretreatment approval ( $\rho$ )	0.350 (0.087)	
Observations	1,019	
R-squared	0.738	

*Note: Estimates and standard errors (in parentheses) are from 2SLS regression on overall approval measured posttreatment. Difference statistics in the second column provide the basis of unconfounded tests of the priming hypothesis. In the leftmost column, parameters being estimated are in parentheses.*

**Table B2: Unconfounded Test of the Projection Hypothesis Using 2SLS, Environment**

Parameters	Estimates	Difference
Intercept, control arm ( $\gamma_{C,En}$ )	-0.037 (0.014)	
Intercept, env. arm ( $\gamma_{T,En}$ )	-0.050 (0.020)	
Pretreatment overall approval, control ( $\delta_{C,En}$ )	0.706 (0.031)	
Pretreatment overall approval, treatment ( $\delta_{T,En}$ )	0.673 (0.042)	
<b>Projection effect, env.</b> ( $\Delta_{En} = \delta_{T,En} - \delta_{C,En}$ )		-0.033 (0.045) p=0.773
Pretreatment energy approval ( $\varphi_{En}$ )	0.062 (0.015)	
Observations	1,066	
R-squared	0.607	

*Note: Estimates and standard errors (in parentheses) are from 2SLS regression on energy approval measured posttreatment. Difference statistics in the second column provide the basis of unconfounded tests of the priming hypothesis. In the leftmost column, parameters being estimated are in parentheses.*

**Table B3: Unconfounded Test of the Projection Hypothesis Using 2SLS, Education**

Parameters	Estimates	Difference
Intercept, control arm ( $\gamma_{C,Ed}$ )	-0.006 (0.015)	
Intercept, education arm ( $\gamma_{T,Ed}$ )	-0.044 (0.021)	
Pretreatment overall approval, control ( $\delta_{C,Ed}$ )	0.653 (0.033)	
Pretreatment overall approval, treatment ( $\delta_{T,Ed}$ )	0.639 (0.039)	
<b>Projection effect, education</b> ( $\Delta_{Ed} = \delta_{T,Ed} - \delta_{C,Ed}$ )		-0.014 (0.043) p=0.506
Pretreatment education approval ( $\varphi_{Ed}$ )	0.074 (0.015)	
Observations	1,066	
R-squared	0.592	

*Note: Estimates and standard errors (in parentheses) are from 2SLS regression on education approval measured posttreatment. Difference statistics in the second column provide the basis of unconfounded tests of the priming hypothesis. In the leftmost column, parameters being estimated are in parentheses.*